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Mandatory Schooling of Girls Improved Their Children's Health: Evidence from Turkey's 1997 Education Reform

Bahadır Dursun

Resul Cesur

Inas R. Kelly

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Abstract

This study examines the impact of mandatory maternal education on child health in Turkey, where a non-trivial fraction of families restricted their daughters' schooling due to social and cultural barriers. The analysis employs two large data sets and exploits a quasi-experiment involving an education reform that increased compulsory schooling. Results show that an increase in mother's schooling improves child health at birth (as measured by factors such as low birth weight and premature birth) and lowers child mortality. The current study on the intergenerational benefits of compulsory schooling arguably provides the strongest evidence supporting the argument that mandatory female education has substantial nonpecuniary benefits in terms of the health of the offspring in societies where female education is stigmatized. The implications of this research extend beyond girls' schooling and suggest that compulsory human capital investments in children can correct market failures when families underinvest in their children because of social or cultural barriers.

INTRODUCTION

Investing in maternal education, particularly in developing countries where female schooling has been discouraged, may have long-run implications for the country's future generations. A large body of literature also documents that educated individuals are healthier.[1] In addition to an individual's own schooling, parental education, particularly that of the mother, is a strong predictor of health outcomes (Currie & Moretti, 2003). While several papers have investigated the effects of women's

[1] See Grossman (2006) for a comprehensive overview of this literature.

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schooling on child health in developing societies by employing different sources of identifying variation, the extant literature offers fairly limited information on specific policy implications of the local average treatment effect (LATE) of alternative education reforms among different populations.[2] Some quasi-experiments enable researchers to tease out the impact of increased educational attainment among people with a high interest in receiving more education (such as increasing school availability or eliminating fees when those who desire to attend school cannot afford it). Conversely, compulsory schooling laws can allow one to uncover the causal effect of education in a setting with a low societal interest in female schooling. Given that many developing nations have weakly enforced compulsory schooling laws and usually require relatively few years of education (United Nations, 2015), separating the impact of extra schooling among individuals with a low tendency to attend school from that of increases in the educational attainment of those with a greater propensity to attend school may have important policy implications. Equally important, we know very little about the causal impact of compulsory schooling when such policies enforce the enrollment of girls against the preferences of their parents, who would choose to limit their daughters' education due to cultural reasons.

The policy experiments employed by the notable contributions in the literature generate LATEs, in general, representing the impact of extended schooling among women from families with a high interest in their children's education. This is because the incentives provided by the exogenous sources of variation that these studies relied on either reduced school fees or increased school availability for individuals who could not get more education mainly due to financial constraints. Exploiting variation in primary school construction between regions and birth cohorts in Indonesia, Breierova and Duflo (2004) estimate the effect of mother's and father's formal schooling on fertility and child health. They find that parental education has negative effects on child mortality. Using data from Taiwan, Chou et al. (2010) utilize a compulsory school reform jointly with a school construction program to examine the impact of parental education on birth outcomes and child mortality and find that an increase in years of parental schooling lowers the likelihood of low birth weight and child mortality.[3] Grépin and Bharadwaj (2015) employ Zimbabwe's 1980 education reforms, which removed apartheid-style policies against Blacks, and show that the mother's extended schooling causes a decline in child mortality. Makate and Makate (2016) estimate the causal effect of primary schooling on child health by using the elimination of tuition fees as the source of identifying variation in Malawi and find that an increase in maternal education lowers child mortality.

Moreover, the literature thus far has provided incomplete evidence on the impact of mother's schooling on offspring's health in terms of infant health at birth and child mortality. Although there is research showing that maternal education has a favorable impact on child health (Breierova & Duflo, 2004; Chou et al., 2010; Grépin & Bharadwaj, 2015; Güneş, 2015;[4] Makate & Makate, 2016), there is also evidence

[2] Needless to say, descriptive evidence consistently shows that maternal education is favorably associated with improved child health (Currie & Moretti, 2003; Cutler, Deaton, & Lleras-Muney, 2006; Grossman, 2006; Thomas, Strauss, & Henriques, 1991).

[3] Taiwan's reform in 1968 enables one to study the combined impact of compulsory schooling and reduction in the cost of educational attainment. As noted by Chou et al. (2010), in addition to an increase in the duration of mandatory years of minimum schooling, the education reform in Taiwan dramatically reduced the cost of attending school for a large number of individuals who wanted to continue their schooling beyond a six-year basic education.

[4] Utilizing the 1997 education reform, Güneş (2015) examined the effects of maternal education on child health using a convenience sample from the Turkish Demographic and Health Surveys (TDHS). This study relies on regional differences in schooling capacity (measured by the number of teachers at the province level), combined with the 1997 education reform as the source of exogenous variation in

suggesting that mother's schooling has little effect on birth outcomes and infant mortality (Keats, 2018; Zhang, 2012).[5] Hence, the current incomplete state of the literature on maternal education and child health awaits compelling answers to various important questions (Grossman, 2015), including whether and to what extent requiring girls whose parents oppose their formal education beyond a certain age to attend school produces intergenerational benefits.

This study examines the causal effect of mandatory maternal education on infant health at birth and child mortality. Our birth outcomes data were obtained from the Turkish Ministry of Health (MHBOD). We use data on child mortality from the Turkish Statistical Institute's Population and Housing Census 2011 (PHC). We exploit exogenous variation in formal education induced by the 1997 compulsory schooling reform. The law, enacted in 1997, went into effect immediately and extended the duration of minimum years of compulsory schooling from five to eight years. Individuals born after 1986 were bound by the schooling law to acquire at least eight years of primary education. Thus, we use exposure to the reform as an instrumental variable to estimate the impact of mother's likelihood of holding at least a middle school diploma on birth outcomes and child mortality.

The Republic of Turkey provides an excellent environment to study the intergenerational effects of compulsory schooling among women from families stigmatizing female education. Because of the social and cultural barriers to female education, gender inequality in education is exceptionally high in Turkey, with women lagging behind men in education even though it was classified as a middle-income country during the 1990s (Otaran et al., 2003). However, the 1997 compulsory schooling reform substantially increased female junior high school completion rates for girls who had to discontinue their formal education at age 11 due to parental opposition (Dulger, 2004). This property of the 1997 education reform makes it a unique policy experiment as it enables one to examine the implications of extended schooling due to the mandate of the law for females against the wishes of their families.

Results indicate that mothers who completed at least eight years of schooling are less likely to deliver babies with very low birth weight, low birth weight, and high birth weight, and they are less likely to have a child deceased before age five. Furthermore, obtaining at least a middle school degree extends gestational age and lowers the propensity of delivering premature babies. Our effect sizes are comparable to those produced by other studies, such as Grépin and Bharadwaj (2015) in Zimbabwe and Chou et al. (2010) in Taiwan, using different policy experiments to estimate the impact of parental education on child health.

From a theoretical standpoint, parental education may impact child health via different mechanisms, including productive and allocative efficiency (Grossman, 1972; Kenkel, 1991), income and occupation (Card, 2001; Oreopoulos, 2006), assortative mating (Behrman & Rosenzweig, 2002), and time preferences (Becker & Mulligan, 1997). Improved production efficiency, induced by extended education, may allow mothers to have healthier children with a given set of healthcare and non-healthcare resources. Meanwhile, improvements in allocative efficiency may increase health knowledge and incentivize parents to undertake health behaviors that enhance child health. For instance, more educated mothers may be more likely to seek preventive

schooling. Despite limited statistical power (i.e., a small sample, and relatively low first stage F-tests, with less than four years of schooling specifications), this study finds suggestive evidence that maternal schooling may improve child health.

[5] Research focused on affluent societies offers conflicting evidence on whether maternal education causally impacts child health. While some studies document that maternal education improves child health (Currie & Moretti, 2003; Grytten, Skau, & Sørensen, 2014), a handful of others do not find a statistically significant relationship between the two (Lindeboom, Llena-Nozal, & van der Klaauw, 2009; McCrary & Royer 2011).

care and avoid unhealthy dietary practices, and they may be less likely to smoke. Educated individuals may also enjoy a wage premium in the labor market or match with more educated, healthier, and more affluent partners, which may, in turn, improve the health of their children. Finally, educated individuals may invest more in their children if they benefit from their future well-being.

Our study supports the view that educated mothers exhibit health behaviors conducive to child health. To explore the potential mechanisms explaining our findings using the available data, we examined the impact of extended schooling on maternal outcomes, including the method of birth delivery and the propensity to smoke. We find that additional education increases the likelihood of natural birth delivery and lowers maternal smoking.

Our investigation combined with the available evidence in the literature also suggests that delaying the age of birth delivery partly explains our results. We find that extended schooling reduces the number of births.[6] However, as the women we study are relatively young, our findings are unlikely to represent the impact of education on total lifetime fertility. In addition, Kirdar, Dayıoğlu, and Koç (2018), who study the same reform we undertake, show that education reduces teenage marriage and increases the age of first birth delivery in Turkey.[7] Consequently, we conclude that the avoidance of teen pregnancy, which can impose a substantial risk to the child's health (Ashcraft, Fernández-Val, & Lang, 2013), may constitute another mechanism explaining our findings.

This research contributes to the maternal education and child health literature in developing countries in important ways. First, our findings provide robust evidence in favor of the view that maternal education improves child health. Nevertheless, the major contribution of the current study comes from the LATE of the 1997 education reform, allowing one to approximate the impact of formal schooling among women previously constrained by their families.[8] Our findings arguably provide the strongest evidence supporting the view that mandatory female education can have substantial intergenerational benefits in terms of the offspring's health in social settings where demand for daughters' schooling is low because of familial preferences. That is, enacting and enforcing compulsory schooling laws to increase the educational attainment of females may have significant benefits in terms of children's health in places where societal interest in female schooling is low.

Finally, the implications of this research reach beyond the education policy discussion regarding female schooling and suggest that compulsory human capital investments in children can correct market failures when families underinvest in their children because of social or cultural barriers.

IDENTIFICATION

The 1997 Education Reform

In 1997, the Turkish Parliament increased the duration of compulsory schooling from five to eight years. Known as the "eight-year uninterrupted education reform," law number 4306 required children to continue primary schooling until they earn a middle school degree, which corresponds to at least eight years of schooling

[6] Several studies also examine the causal impact of women's education on fertility using similar identification strategies in different countries (Andalón, Williams, & Grossman, 2014; Fort, Schneeweis, & Winter-Ebmer 2016; Geruso & Royer, 2018; Güneş 2016; Lavy & Zablotsky, 2015; Osili & Long, 2008).
[7] This result is consistent with the literature finding that extra schooling delays the age of birth delivery without necessarily impacting total fertility (Ali & Gurmu, 2018; Miller, 2010).
[8] We thank an anonymous reviewer for their helpful tips in articulating our key contribution.

(Dulger, 2004). The reform became effective in the 1998/1999 academic year, combining the first two stages (i.e., elementary and lower secondary) of schooling under the umbrella of *Primary School* (*İlköğretim Okulu*). The new legislation meant that children who completed fifth grade in 1998 were obliged to continue their education until they earned a middle school degree. Therefore, those who started elementary school in 1993/1994 were the first to be impacted by the law (Dursun & Cesur, 2016).

Because the Turkish law allows pupils who are 72 months old by the end of the calendar year to begin elementary schooling in the corresponding academic year (Dinçer, Kaushal, & Grossman, 2014), children born in 1987 constitute the first fully affected birth cohort.[9] However, as parents (and school administrators) can exert considerable discretion concerning the school starting age, some children can start elementary school a year earlier or later than their own birth cohort (Torun, 2015). Therefore, while the majority of children born in 1986 were likely to start elementary school in 1992 and were not bound by the reform, some of them possibly delayed it to 1993 and were impacted by the reform.

Therefore, the policy experiment at hand provides an excellent instrumental variable to identify the effects of education by comparing the outcomes of those born a few years before and after the 1986 birth cohort. Following the approach taken by Battistin et al. (2009), Cesur and Mocan (2018), Dursun and Cesur (2016), Dursun, Cesur, and Mocan (2018), and Fort, Schneeweis, and Winter-Ebmer (2016), we exclude the 1986 birth cohort from the sample to get a handle around this issue.[10]

Notably, the reform mainly focused on increasing educational attainment and made no curricular or compositional changes (Dulger, 2004). Therefore, our identification strategy relies on the following assumption: Once the trends at the birth cohort level are accounted for, those born before 1986 should constitute an ideal comparison group for individuals born after 1986. As shown in Figure 2, trends in the percent of mothers holding at least a middle school degree exhibited a secular jump among those bound by the reform.

The Local Average Treatment Effect of the 1997 Education Reform

Secondary school attainment could have increased after the 1997 education reform among two different types of female populations: (i) girls facing social barriers to receiving more education may have increased their educational attainment due to the mandate of the reform; and (ii) those with a high zeal to attend middle school could have enrolled in a school because of increased school availability or reductions in the cost of education.[11] Even before the reform, school availability was not an obstacle to enrollment in urban areas because nearly all urban localities had a middle school (according to the Ministry of National Education Yearbooks 1991/1992 to 1997/1998), and no student could be denied enrollment (Turkish Constitution Article 42, and Ministry of National Education Legislation).[12] As we discuss in the Appendix,[13] the geographical convenience of schools did not increase due to the

[9] Resmi Gazete; Friday, 7 August 1992, Section 14.
[10] The inclusion of the potentially partially impacted 1986 birth cohort in the treatment group can violate the monotonicity assumption, which dictates that the control group must not be affected by the instrumental variable.
[11] For ease of expression, children's propensity to attend middle school is used interchangeably (or jointly) with the interest of their families.
[12] While we acknowledge the possibility that in some cases a student might have been denied enrollment due to classroom capacity constraints, it was extremely rare and temporary even before the 1997 education reform was in effect.
[13] The Appendix is available at the end of this article as it appears on JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 1. Main reasons for not attending school among ever-married women who were not bound by the 1997 education reform.

Reasons for Dropping Out School	Total	Urban	Rural
His/her family doesn't permit child to go to school	0.401	0.362	0.446
Did not like school/ Did not pass exams	0.195	0.239	0.150
Cannot afford schooling expenses	0.056	0.074	0.036
School not accessible	0.055	0.037	0.075
Got married	0.021	0.023	0.020
Family needed help	0.016	0.012	0.020
Needed to earn money	0.014	0.021	0.006
Had enough schooling	0.007	0.006	0.007
Take care of children	0.001	0.001	0.001
Other	0.204	0.191	0.220
Don't Know/ Missing	0.023	0.017	0.028
Number of Observations	1,563	829	734

Notes: The data come from the Ever-Married Module of the Turkish Demographic and Health Survey collected in 1998. The table shows the percentage distribution of women aged 15 to 24 who stopped attending school upon earning a five-year elementary school diploma for the reasons listed for not continuing their education. Women who were between 15 and 24 were not affected by the 1997 education reform.

1997 reform. Table A1 shows that the number of schools did not increase after the 1997 reform. Thus, the schooling mandate was the primary reason behind increased educational attainment, even in rural areas.

As there has been a considerably high degree of stigma attached to girls attending secondary schools in Turkey (Aytaç & Rankin, 2004; UNICEF, 2007), the 1997 education reform targeted children, especially girls, from families with a low interest in middle school attendance (Dulger, 2004). To gain further insight into the reasons as to why females did not pursue secondary schooling before the 1997 education reform, we use data from the Turkish Demographic and Health Surveys (TDHS) collected in 1998. Specifically, among those who earned at least a primary school diploma (i.e., a minimum of five years of schooling), TDHS 1998 asks ever-married women between the ages 15 to 24 (i.e., born between 1974 and 1983) why they quit or did not attend school at all. As these women are only a few years older than those bound by the 1997 education reform, knowing why they stopped attending school after receiving five years of basic education can provide helpful information on how the 1997 education reform induced females to obtain more schooling. Results, presented in Table 1, show that a sizable 40 percent of women who dropped out of or did not go to middle school at all declared that their family did not allow them to continue their education, and roughly 20 percent reported that they did not like school or could not pass the exams. On the other hand, 5.5 and 5.6 percent of them attribute it to the lack of a suitable nearby educational institution and financial resources to cover school expenses, respectively. Moreover, only 1.4 and 1.6 percent declared that they needed to earn money and help their families, in that order.

Therefore, we conclude that the law mandate was the key driving force behind the jump in educational attainment. This particular property of the quasi-experiment at hand implies that the LATE obtained from this policy change mainly corresponds to the impact of primary schooling among females whose educational attainment is restricted by their own families. To the extent of our knowledge, this aspect of the "eight-year uninterrupted education reform" makes it the closest quasi-experiment that approximates the impact of compulsory schooling among girls who would otherwise not be allowed by their families or seek additional education due to social and cultural barriers to female schooling.

Other reforms in developing countries that have been utilized in the literature either enable researchers to estimate the impact of extra schooling among those for whom the financial cost of school reduced attendance, such as in Indonesia 1973 to 1978, China 1977 to 1983, Zimbabwe 1980, Malawi 1994, Uganda 1997, or allow one to study the combined effect of extended schooling among those in a setting with a higher societal interest in more education in combination with a compulsory schooling reform (Taiwan 1968). Thus, the knowledge gained from this reform may have important policy implications for other developing societies that consider increasing the duration of mandatory schooling, especially when demand for education is low due to social and cultural barriers.

DATA

We use two data sets in our analysis: Birth outcomes data from the Turkish Ministry of Health (MHBOD) and child mortality data from the Turkish Statistical Institute's Population and Housing Census 2011 (PHC).

Ministry of Health Birth Outcomes Data (MHBOD)

Information on birth outcomes, including birth weight, gestational age, birth delivery method, and maternal smoking, comes from the Ministry of Health. Historically, birth records have not been systematically recorded in Turkey. To gain accurate information on women's and children's health, the Ministry of Health initiated a data collection effort on birth outcomes and mothers in 2008. These data were collected through two sources. Information on children's birth weight was obtained from public hospitals. Data on gestational age, the method of birth delivery, and maternal education were collected by healthcare professionals through the Family Medicine Program (FMP), which provides universal primary care to all the citizens at Family Health Centers (FHC) regardless of their financial well-being.[14] The Ministry of Health provided the MHBOD for our use at the child level.[15] The data also provide information on maternal educational attainment categories for each child. These categories include no education, elementary school, middle school, high school, college, and graduate degree.[16]

Because the 1997 education reform (i.e., the instrumental variable we employ in the analysis) obliged individuals to earn at least a middle school degree (i.e., at least eight years of schooling), we coded *Middle School* equal to one for children whose mothers earned at least a middle school degree, and it is set equal to zero otherwise. Similar to Brunello, Fabbri, and Fort (2013), Dursun and Cesur (2016), and Fort, Schneeweis, and Winter-Ebmer (2016), which limit the analysis sample to birth cohorts who were born a few years before and after the pivotal birth cohort, we restrict the estimation sample to children whose mothers were born five years before and after 1986.

[14] As discussed in detail by Cesur et al. (2017, 2021), the FMP provides a broad set of primary care services through walk-in clinics, the Family Health Centers (FHC). A unique aspect of the FMP, especially in the developing world, including middle-income nations, is that it is not a means-tested program. Instead, the FMP covers 100 percent of the citizens. While higher-income individuals are less likely to utilize the services offered by the FMP, it reaches a high share of the population via the FHCs (Cesur et al., 2017, 2021).

[15] Therefore, the data do not include information on maternal birth history, such as birth order.

[16] As is the case in nearly all data sets collected by governmental organizations in Turkey, the MHBOD does not include information on years of schooling (Dursun & Cesur, 2016).

Using the available information provided by the MHBOD, we created measures of child health at birth based on birth weight in grams and gestational age. Following the existing literature (Currie & Moretti, 2003), we construct two binary low birth weight indicators, *Low Birth Weight*, and *Very Low Birth Weight*, besides using the natural log of birth weight, *Log of Birth Weight*, as a measure of child health at birth. *Low Birth Weight* is set equal to one for children who weigh less than 2,500 grams at birth, and it is set equal to zero otherwise. *Very Low Birth Weight* is constructed analogously using 1,500 grams as the cutoff birth weight.

While low birth weight is usually the main concern and more prevalent than high birth weight, a sizable literature in obstetrics, epidemiology, and medicine associates high birth weight with not only short-run maternal and child health complications (i.e., birth delivery problems, newborn morbidity, birth injuries), but also long-run health ailments including overweight/obesity in late childhood (Boulet et al., 2003; Cnattingius et al., 2012; Sparano et al., 2013), type 2 diabetes (Wei et al., 2003; Whincup et al., 2008), cardiovascular and metabolic complications (Walsh & McAuliffe, 2012), and reduced cognitive outcomes (Cesur & Kelly, 2010). To investigate the effect of maternal education on high birth weight, we generated *High Birth Weight*, set equal to one if the infant weighs more than 4,500 grams, and zero otherwise.

Gestational age in weeks is calculated by taking the difference between birth date and the last menstruation date. Using the standard definition from the World Health Organization (WHO), *Premature* is equated to one if a child's gestational age is less than 37 weeks, and it is coded as zero otherwise.[17] We also employ the *Natural Log of Gestational Age* as an outcome variable.

While the Ministry of Health aims to collect data on all births in the country, the extent of the MHBOD is limited by the reach of the Ministry of Health via hospitals and the FMP. The FMP started as a pilot program in 2005 in one province, and it was expanded to all of the 81 provinces in Turkey by 2010. As discussed extensively by Cesur et al. (2017), the purpose of the introduction of the FMP was to provide universal primary healthcare services to all citizens free of charge. Therefore, the MHBOD may have some limitations as well. First, because the nationwide expansion of the FMP was completed during 2010, the MHBOD could only be collected in a subsample of provinces before 2011, even though the data collection effort was initiated in 2008. Second, although the FMP aims to cover everyone in the country regardless of income and socioeconomic status (SES), not all citizens will choose to receive the services offered by the FMP. Lastly, MHBOD is collected by healthcare professionals at family health centers (FHC), through which the FMP program is administered. Therefore, while the MHBOD provides information on a large number of births between 2008 and 2016, it does not include all of them due to weak enforcement of data collection by the Ministry of Health and staffing shortages at FHCs and hospitals.

Table 2 presents summary statistics for the MHBOD (in panel A) and PHC (in panel B) data sets by exposure to the 1997 education reform status. Children whose mothers were born between 1981 and 1985 constitute the control group and those born between 1987 and 1991 form the treatment group. Panel A highlights the difference in education levels between the treatment and control groups in birth weight data; while 57 percent of mothers obtained at least a middle school diploma in the treatment group, this percentage is 49 percent in the control group. The mean birth weight is 3,246 grams. *Very Low Birth Weight* and *High Birth Weight* categories make up 1 percent of our birth weight sample, while 6 percent of women gave birth to low-birth weight babies (i.e., under 2,500 grams). The prevalence of very low birth

[17] See http://www.who.int/mediacentre/factsheets/fs363/en/.

Table 2. Summary statistics.

	All	Control	Treatment
Panel A: Birth Outcomes			
Middle School	0.532	0.495	0.572
	(0.499)	(0.500)	(0.495)
Birth Weight	3246	3260	3229
	(502.249)	(511.528)	(490.681)
Very Low Birth Weight	0.006	0.006	0.005
	(0.074)	(0.078)	(0.070)
Low Birth Weight	0.060	0.061	0.060
	(0.238)	(0.239)	(0.237)
High Birth Weight	0.005	0.006	0.003
	(0.069)	(0.076)	(0.059)
Age	26.818	29.439	23.762
	(3.256)	(1.638)	(1.575)
	[186,840]	[96,874]	[89,966]
Gestational Age	38.417	38.306	38.541
	(2.175)	(2.136)	(2.211)
Preterm<37 weeks	0.117	0.121	0.112
	(0.321)	(0.326)	(0.316)
	[1,486,353]	[756,929]	[729,424]
Panel B: Child Mortality			
Middle School	0.622	0.544	0.803
	(0.485)	(0.498)	(0.398)
Any Child Died	0.026	0.029	0.019
	(0.159)	(0.168)	(0.136)
	[340,091]	[237,964]	[102,128]

Notes: Mean values in panels A and B are generated using data from the Ministry of Health Birth Outcomes Data (MHBOD) and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC), respectively. Individuals born between 1987 and 1991 constitute the treatment group, and those who were born between 1981 and 1985 form the control group. The 1986 cohort is excluded from the analysis. Standard deviations are in parentheses. Variables in panel A are weighted by the number of women in fertility age (ages 15 to 49) in the province. Survey weights provided by the PHC are used in panel B. The number of observations is in square brackets [].

weight, low birth weight, and high birth weight babies is lower in the treatment group compared to the control group. The average gestational age is around 38 weeks, and there were fewer premature births in the treatment group (11 percent) compared to the control group (12 percent).

Before presenting descriptive statistics, we discussed that the MHBOD data collection effort faced some challenges. The limitations mentioned above can pose a threat to the credibility of our estimates due to the potential sample selection bias at two different levels. First, if selection into the MHBOD is a function of the 1997 education reform, our estimates could be biased. A similar problem may emerge if exposure to the reform influences the likelihood of selection into the estimation samples within the MHBOD. We undertook a few strategies to investigate whether and to what extent selection into data sets can plague our estimates.

First, to understand whether the data on birth outcomes are representative, we compare the mean values in the MHBOD data to those obtained from the 2008 and 2013 waves of the TDHS, aiming to provide the nationally representative estimates of maternal and child health. As shown in the Appendix,[18] this exercise shows that the mean values and the distribution of birth weight in the MHBOD are considerably similar to those in the DHS (Table A2). This result suggests that estimates obtained from the MHBOD data are nationally representative.

Second, if the differential rollout of the FMP affected people's selection to the MHBOD, our findings may be questionable. To address this concern, we reestimate our main findings in Table A3 by excluding the years before 2012. Given that the expansion of the FMP was completed by 2011, this exercise should address the potential bias that may be created by differential exposure to the reform. This finding implies that the differential rollout of the FMP program is orthogonal to exposure to the education reform and does not pose a threat to our findings.

Third, we test whether the data availability for birth weight, birth delivery method, and gestational length is a function of the instrumental variable we employ. Accordingly, we estimated the effect of exposure to the 1997 education reform on the likelihood of having missing information for each child's health and maternal outcome defined above. If being bound by the reform impacts the likelihood of reporting information on birth and maternal outcomes, our instrumental variable (IV) estimates may be biased. Therefore, this test is particularly important for our purposes. Results presented in Table A4 show that the estimated coefficients on education reform are both very small in magnitude and statistically insignificant; thus, the likelihood of having missing information on outcome measures is unrelated to exposure to the 1997 education reform. In particular, this exercise implies that selection into different samples within the MHBOD is not a function of exposure to the education reform.

Population and Housing Census (PHC) 2011 Data

Data on child mortality are obtained from the PHC, conducted by the Turkish Statistical Institute (TurkStat). Until 2000, a general population census, which collected information from all the residents, was conducted regularly.[19] To enhance the provision of public goods and services via more accurate planning, the TurkStat established the Address Based Population Census (ABPC) in 2007, which also replaced the regular census. Since the establishment of ABPC, demographic characteristics, including population, age and gender structure, marital status, educational attainment, place of registration, within-country migration, and nationality of individuals, have been closely monitored through administrative sources and updated annually. While the ABPC provides detailed information on these measures, it does not collect some internationally recommended and nationally needed data on several social, economic, demographic, and housing characteristics of the population. Therefore, the TurkStat decided to implement a *mini* census to collect information unavailable in the ABPC, such as labor force, employment, unemployment, reasons for migration, disability, and building and dwelling characteristics. The PHC was conducted by face-to-face interviews of more than two million households and about nine million people in 2011.

Like the MHBOD, instead of years of completed schooling, the PHC provides us with educational attainment categories, including no education, elementary school, middle school, high school, college, and graduate school. Thus, *Middle School*, which

[18] The Appendix is available at the end of this article as it appears on JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[19] The first census was conducted in 1927, four years after the establishment of the Republic of Turkey. Then starting in 1935, censuses were organized every five years until 1990, and the last census was conducted in 2000.

is the key independent variable in the PHC data, is coded as one for women who earned at least a middle school degree, and zero otherwise.

We set *Any Child Died* equal to one if the respondent mother had at least one child deceased before the age of five, and it is equated to zero if she had no children who passed away before the age of five. Although it would be informative to examine the impact of maternal schooling on neonatal (i.e., death in the first four weeks of life) and infant (i.e., death in the first year of life) mortality, one limitation of the PHC data set is that it only provides information on child mortality by age five. While the lack of information on infant mortality per se is a disadvantage, child mortality statistics released by the TurkStat suggest that the vast majority of child deaths in Turkey occur in the first year of life. More specifically, in 2011, even though the infant mortality rate was 11.7 per thousand, the mortality rate among ages one to five was less than one per thousand. Therefore, for the most part, the child mortality information provided by the PHC is likely to be driven by and thus also reflect infant mortality.

ECONOMETRIC METHODOLOGY

Using the MHBOD at the child level, equation (1) estimates the relationship between maternal schooling and birth outcomes:

$$BO_i = \gamma_0 + \gamma_1(Middle\ School)_i + X_i^{'}\gamma + u_i, \tag{1}$$

where *BO* is child *i*'s associated birth outcome, including birth weight, gestational age, and premature birth status. Using the MHBOD, we also estimate the impact of women's education on birth delivery and maternal smoking. The key independent variable, *Middle School*, is a dichotomous indicator representing whether child *i*'s mother earned at least a middle school degree. The vector X contains predetermined covariates: province fixed effects, mother's month of birth indicators, and linear approximations of mother's birth year-month on each side of the pivotal birth cohort of 1986. The idiosyncratic error term is denoted by u. Controlling for province fixed effects accounts for all the unobserved determinants of female education and the outcome of interest common to everyone living in the same province. Binary indicators for child's year of birth accounts for national trends and shocks in birth outcomes. Finally, maternal month-of-birth fixed effects account for seasonality in the mother's birth.

In equation (2), we use data at the mother level from the PHC to examine the relationship between mother's education and child mortality:

$$CM_m = \beta_0 + \beta_1(Middle\ School)_m + X_m^{'}\theta + \varepsilon_m, \tag{2}$$

where *CM* represents whether mother m had at least one child who died by the age of five. The remaining covariates of equation (2) are constructed analogously to equation (1).[20]

Estimating the parameters of equations (1) and (2) using ordinary least squares (OLS) may not necessarily correspond to the causal effect of maternal schooling on child health given that unobservable determinants of maternal education may also determine birth outcomes and child mortality. For instance, more educated mothers may come from high SES families and possess both genetic and behavioral

[20] Equation (2) does not control for year fixed effects given that the PHC is a cross-sectional data set.

advantages leading to biased estimates of the effect of maternal education on child outcomes.

To estimate maternal education's causal impact on child outcomes, we use a two-stage least squares (2SLS) estimation strategy. In doing so, we instrument mother's educational attainment on the 1997 education reform. Equations (3) and (4) correspond to the first stage estimates of the impact of the 1997 education reform on the likelihood of maternal middle school completion for the child-level birth outcomes, and mother-level child mortality specifications, respectively:

$$Middle\ School_i = \alpha_0 + \alpha_1 Reform_i + X_i'\alpha + \text{Ø}_i \tag{3}$$

$$Middle\ School_m = \pi_0 + \pi_1 Reform_m + X_m'Y + \Omega_i, \tag{4}$$

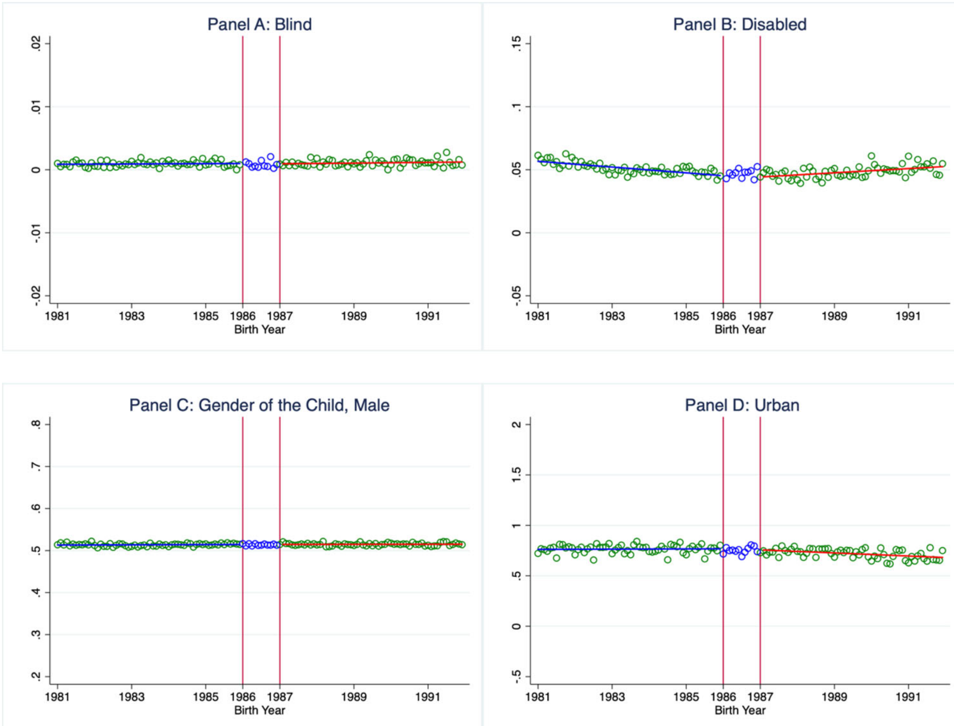
where the outcome is *Middle School*. *Reform* is a dichotomous variable that captures whether the mother was exposed to the reform (i.e., it is set equal to one if she was born after 1986, and zero otherwise). All other variables mimic those specified in equations (1) and (2). In the second stage, we use *Reform* as an instrument for *Middle School* and estimate 2SLS models to approximate the causal impact of maternal middle school education on child health. For our two sample IV (TSIV) results, outlined in detail in the Results section, we use PHC data to estimate equation (4) in the first stage, combined with MHBOD data to estimate birth outcomes as modeled by equation (1) in the second stage.

Our identification strategy hinges upon the assumption that pre- and post-reform cohorts (i.e., control and treatment groups) have similar observable and unobservable characteristics except for their extended primary schooling induced by exposure to the 1997 reform. This premise becomes more convincing as the estimation window around the cutoff birth year gets smaller (Lee & Lemieux, 2010). We estimate our main specifications using those born five years before and after 1986 because this time interval provides enough statistical power without depending on variation coming from the tails in the data period. Then, we show that our estimates are robust to limiting our sample to those born six, four, three, and two years before and after the pivotal year. As pointed out earlier, because the treatment status of the 1986 cohort is ambiguous, the correct specification is the one where the 1986 cohort is dropped from our main analysis.[21]

Following Cesur and Mocan (2018), Dursun and Cesur (2016), and Dursun, Cesur, and Mocan (2018), standard errors are clustered at the mother's birth year by province level, leading to 810 (= 81*10) cluster units. We cluster our main specifications at the province by the mother's birth year level for the following reason. While the 1997 education reform was implemented at the national level, regional factors can influence the implementation of the reform, women's education, and child health. Therefore, we not only account for the potential influence of location-specific factors by controlling for province fixed effects but also adjust the standard errors at the province-by-year-level in the main analysis.[22] Birth outcome models are

21 Eliminating the 1986 birth cohort also accounts for the potential spillover effects and the possible lag between the timing of legislation and implementation. Moreover, according to data from the World Bank, approximately 5 percent of primary school students repeated a grade in the mid-1990s; excluding 1986 may also help in accounting for this group. However, as presented in Table 5, inclusion of the 1986 birth cohort with alternative treatment values (such as Reform = 0, and Reform = 0.25) does not alter our main findings.

22 We test the resilience of our standard errors to employing alternative cluster units in the Robustness section and show that inference based on employing different clustering units leads to identical conclusions.



Notes: Figures plot the predetermined indicators by month-year of birth bins for women born up to five years before and five years after the pivotal cohort of 1986. Solid lines represent the linear fit for the corresponding cohorts. The data source is the Turkish Statistical Institute's Population and Housing Census 2011 (PHC) for panels A and B, the Ministry of Health Birth Outcomes Data (MHBOD) for panel C, and Ever Married Women Sample of the Turkish Statistical Institute's Household Labour Force Survey Data (2008 to 2014) for panel D, respectively.

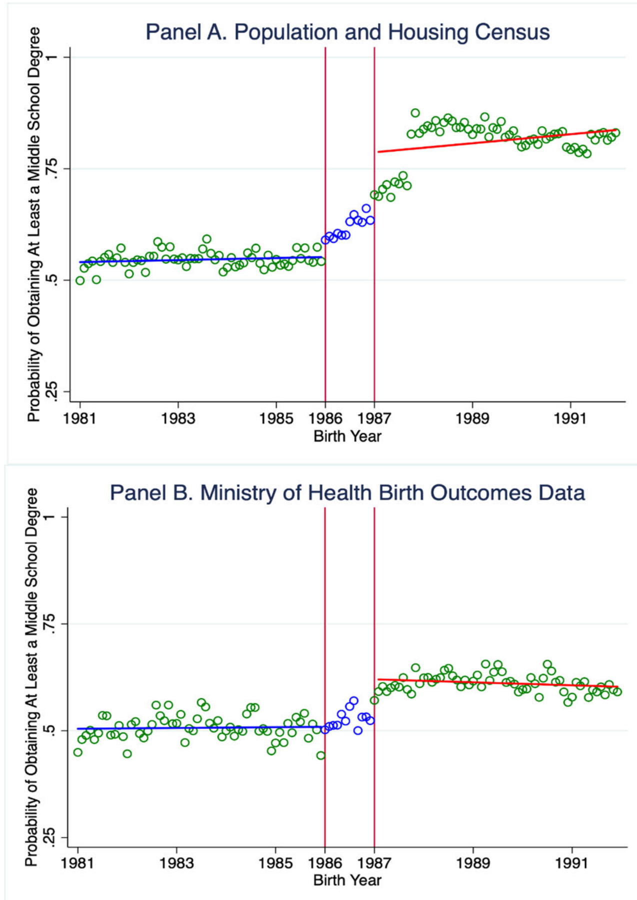
Figure 1. Balanced Covariates.
[Color figure can be viewed at wileyonlinelibrary.com]

weighted using the province level number of women in the age of fertility (i.e., ages 15 to 49). We use the weights provided by the PHC in child mortality specifications.

RESULTS

First Stage: The Impact of the 1997 Education Reform on Maternal Schooling

We begin the first stage analysis by investigating the validity of our empirical approach. As our instrumental variable resembles a fuzzy-RD design, we perform the specification checks suggested by Lee and Lemieux (2010). First, as a standard procedure of our empirical design, we located four predetermined variables in our data to test if there are systemic differences between control and treatment groups in exogenous characteristics. The blindness and disability status indicators come from the PHC data. The gender of the child is obtained from the MHBOD. Finally, we use urban versus rural living status in the Household Labor Force Survey (HLFS). As displayed in Figure 1, in testing the "balanced covariates" assumption, we observe no significant jump across the 1986 cutoff. We also present the regression version of the balanced covariates in Table A5 and confirm the findings displayed in Figure 1.



Notes: The figure plots the probability of obtaining at least a middle school degree by month-year of birth bins for women born up to five years before and five years after the pivotal cohort of 1986. Solid lines represent the linear fit for the corresponding cohorts. The data source is the Turkish Statistical Institute's Population and Housing Census 2011 (PHC) for panel A, and Ministry of Health Birth Outcomes Data for panel B, respectively.

Figure 2. Trends in Middle School Completion Rates by Birth Cohort. [Color figure can be viewed at wileyonlinelibrary.com]

Second, we perform the McCrary test to explore to possibility of nonrandom sorting around the cutoff. Figure A1[23] provides additional support for the exogeneity of our IV by showing evidence that the running variable is smooth around the 1986 threshold.[24] We also present the augmented version of McCrary tests for discrete variables developed by Frandsen (2017) in Figure A2. This exercise leads to identical conclusions.

23 The Appendix is available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.
24 The *p*-values of the estimated jumps are .312, .433, and .202, for the birth weight sample, gestational age, and mortality samples, respectively.

Table 3. The impact of the 1997 education reform on middle school graduation.

Panel A: Birth Outcomes	
Birth Weight Sample	
Education Reform	.076***
	(0.012)
	{0.505}
	[186,840]
Gestational Age Sample	
Education Reform	.083***
	(0.010)
	{0.474}
	[1,486,353]
Panel B: Child Mortality	
Education Reform	.228***
	(0.014)
	{0.544}
	[340,091]

Notes: The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. The dependent variable is obtaining at least a middle school degree for both panels. Individuals born between 1987 and 1991 constitute the treatment group, and those who were born between 1981 and 1985 form the control group. The 1986 cohort is excluded from the analysis. Education Reform is an indicator variable that takes the value of one if the individual is exposed to the education reform (born in 1987 or later), and zero otherwise. All models include the re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, child year of birth dummies, and province fixed effects. Birth outcome models also control for male child dummy. Data in panels A and B come from the Ministry of Health Birth Outcomes Data (MHBOD), and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC). Birth outcome models are weighted by the number of women in fertility age (ages 15 to 49) in the province. Survey weights provided by the PHC are used in panel B. The number of observations is in square brackets []. Outcome variable means of the control group are in curly brackets {}. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$.

A notable pattern in Figures A1 and A2 is the jump in the number of observations in the month of January for each year regardless of the sample undertaken. The disproportionately high number of births in January is due to the misreporting of birthdays in Turkey (Akyol & Mocan, 2020) and this pattern is observed in other developing countries as well.[25] Hence, this is a general pattern in Turkey independent of the 1997 compulsory schooling reform and does not constitute a threat to our identification strategy.

Upon providing evidence supporting the argument that our IV is plausibly exogenous, we explore the impact of the compulsory schooling reform on women's education. As displayed in panel A of Figure 2, we observe a dramatic jump in the proportion of women holding at least a middle school degree among women born after 1986 compared to those born before (80.3 percent versus 54.4 percent) in the PHC data. We also see an unambiguous leap in the likelihood of obtaining at least a middle school diploma, although the magnitude of the jump is noticeably smaller in the MHBOD (49.5 percent versus 57.2 percent).

Then, the strength of our IV is formally tested in Table 3, where we analyze the effect of exposure to the education reform on the likelihood of earning a middle school diploma at the minimum. Results show that the reform increased middle

25 See https://www.npr.org/2019/12/29/792146707/why-so-many-immigrants-have-birthdays-on-jan-1.

school graduation between 8 to 23 percentage points, depending on the estimation sample. In each case, the estimated coefficient on *Reform* is statistically significant at the 1 percent level.

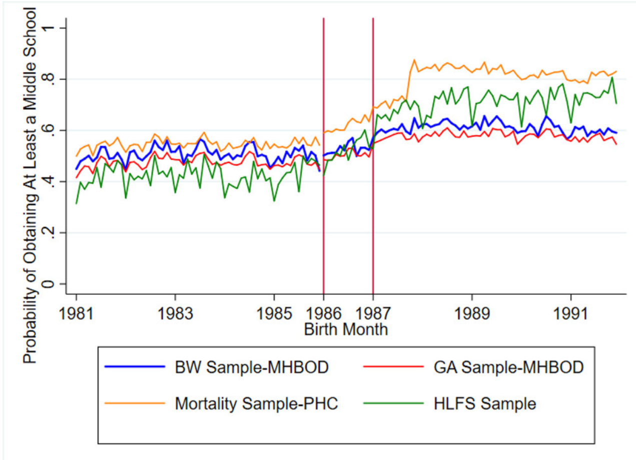
The fact that the reform's impact on educational attainment is much higher in the PHC sample than it is in the MHBOD sample deserves attention. To understand why this is the case, we reached out to a few family physicians who are knowledgeable about the data collection process at the FHCs. Different family physicians suggested that, as the FHC personnel did not prioritize collecting data on non-health outcomes, the discrepancy (likely) caused by the similarities in words that describe educational terms in Turkish. In particular, the terms to describe five years of elementary schooling (İlkokul) before the reform and eight years of primary education (İlköğretim) after the reform are not only phonetically similar but also have remarkably similar meanings in Turkish.

Specifically, in Turkish, the word İlkokul combines words ilk (first) and okul (school) and literally means *the first school*. It has two meanings as an educational term: (1) elementary school; and (2) elementary school degree. For example, in Turkish, "Atatürk İlkokulu" means Atatürk Elementary School, and "ilkokul diploması" means elementary school diploma. Recall that the reform united the five-year elementary school (İlkokul) and three-year middle school (Orta Okul) under the sample umbrella and named it as İlköğretim. The literal meaning of the word İlköğretim, which combines İlk (first) and öğretim (education) is *first education*. Because of the phonetic and literal similarities between the two, the terms İlkokul and İlköğretim have long been used interchangeably. In fact, the law number 222 of 1961, which mandated five years of compulsory schooling and was in effect until 1997, was named "İlköğretim ve Eğitim Yasasi" (The Primary Schooling and Education Law).

Therefore, the educational attainment information of individuals who were born after 1997 and completed eight years of primary schooling (İlköğretim) could easily be miscoded as if they received five years of elementary education (İlkokul). However, the extent of this type of confusion leading to miscoding of the educational attainment levels of those born before 1997 would be much smaller. As such, because the terms used to describe five years of schooling (ilkokul) and eight years of education (orta okul) are distinct, people would be much less likely to confuse them.

To explore the validity of this line of reasoning, in Figure 3, we compare the trends in having at least eight years of schooling in the data collected by healthcare workers in FHCs to those collected by the TurkStat in the PHC. Consistent with our expectation, the difference in means between the two samples widens for individuals born after 1986. This pattern does not change regardless of which sample from the MHBOD we employ. For example, the gestational age sample, which may be more representative due to its large sample size, follows an identical pattern. Consequently, if our conjecture is correct, this type of measurement error in maternal education is likely to inflate the 2SLS estimates of the coefficient on maternal education.

Another possibility is that this discrepancy could be due to potential sample selection bias induced by increased educational attainment. In particular, if the likelihood of being in the MHBOD is a function of the reform, our findings may be biased. It is worth elaborating on how it may impact our findings. Before delving into this thought exercise, we first compare our estimates of the impact of reform on middle school completion rates of women to the one obtained from an extensive nationally representative data set, the HLFS, collected by the TurkStat, the leading statistics and data collection body in the Republic of Turkey. As displayed in Table A6, being bound by the law increases the middle school completion rates among ever-married women in the HLFS by 24 percentage points. This effect size is qualitatively and quantitatively similar to the estimate in the PHC data. This finding suggests that when the data are collected by the TurkStat, which carefully codes educational attainment, the effect sizes are nearly identical and likely to correspond to the true



Notes: The figure plots the probability of obtaining at least a middle school degree by month-year of birth bins for women born up to five years before and five years after the pivotal cohort of 1986. Data come from the Turkish Statistical Institute's Population and Housing Census 2011 (PHC), the Ministry of Health Birth Outcomes Data (MHBOD), and the Ever-Married Women Sample of the Turkish Statistical Institute's Household Labour Force Survey (HLFS) Data (2008 to 2014).

Figure 3. Trends in Obtaining At Least Middle School Degree, Comparing Trends Across Data Sets and Samples.
[Color figure can be viewed at wileyonlinelibrary.com]

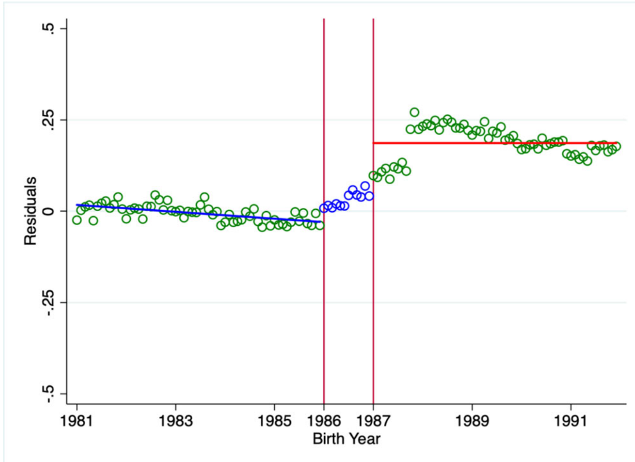
effect of the reform on schooling. Therefore, we are confident that the magnitude of the first stage effect of education reform on *Middle School* in the PHC is the true effect size (i.e., about 23 to 24 percentage points).[26]

As shown in Table 3, the effect of reform on junior high school completion rates in the MHBOD is about 15 percentage points smaller than the reform's true impact. Suppose this difference is due to the possibility that educated mothers are less likely to be in the estimation sample. In that case, the instrumental variables specifications should underestimate the impact of maternal education on the offspring's health, assuming that maternal education has favorable effects on child health.

We find it improbable that exposure to the reform will lower the likelihood of a pregnant woman utilizing free primary care at FHCs. While compulsory schooling leads to an increase in women's wages (Torun, 2015), it is hard to imagine that the associated wage effect is high enough to induce women to forgo free primary care, especially in a country where other forms of health insurance are nearly nonexistent. In fact, the FMP covers all private and public workforce for primary healthcare services in Turkey through social security. Hence, we do not expect that extended primary schooling significantly lowers the utilization of free primary care.

Hence, we conclude that the miscoding of maternal education by medical employees is much more likely to be the case than the possibility of selection into MHBOD

26 While the 1997 education reform does not approximate the impact of holding at least a middle school diploma for the average female in the country, it has a large (i.e., 23 percentage points) positive effect on the educational attainment of females in Turkey. Therefore, our study does not suffer from Oreopoulos' (2006) criticism that compulsory education reforms change the behavior of very few individuals.



Notes: The figure presents the visual illustration of the impact of 1997 Education Reform on the probability of obtaining at least a middle school degree for women born up to five years before and five years after the pivotal cohort of 1986 using residuals from equation (3) net of exogenous controls. Data come from the Turkish Statistical Institute's Population and Housing Census 2011 (PHC).

Figure 4. The Impact of the 1997 Reform on the Likelihood of Obtaining At Least a Middle School Degree. [Color figure can be viewed at wileyonlinelibrary.com]

data sets due to exposure to the reform. The standard method to tackle this problem is implementing a two-sample instrumental variables (TSIV) estimation strategy (Angrist & Krueger, 1992; Inoue & Solon, 2010). We therefore employ a TSIV estimation strategy in the MHBOD to obtain accurate effect sizes. In the TSIV specifications, we use the first stage from the PHC data to maintain consistency.

We display the visual illustrations of the 1997 education reform's impact on junior high school completion rates in the PHC data before moving to the analysis of the impact of maternal education on child health. Figure 4, depicting the residuals obtained from equation (3), assures that exposure to the 1997 education reform dramatically increased the likelihood of obtaining a junior high school diploma. It shows that there is an unambiguous jump in the educational attainment of those born after 1986.

The Impact of Maternal Schooling on Child Health

In column (1) of Table 4, we present the OLS estimates of child health on mother's schooling. In panel A, maternal education is positively associated with a healthy birth weight. Specifically, infants whose mothers earned at least a middle school degree have a 0.5 percent higher birth weight and are 0.5 and 0.1 percentage points less likely to weigh below 2,500 and above 4,500 grams, respectively. We also find that maternal education is associated with a 0.30 percent decrease in the length of gestation and a one percentage point decrease in the likelihood of premature births. In panel B, we find that children of mothers who hold at least a middle school diploma are 1.9 percentage points less likely to have died by age five.

While the OLS estimates help observe the descriptive nature of the relationship between maternal education and the health of the offspring, whether these results reflect the causal effect of education on child health is debatable. Numerous

Table 4. The impact of maternal education on child health, OLS, instrumental variables, and intent to treat estimates.

Panel A: Birth Outcomes	(1) OLS	(2) Instrumental Variables Estimates	(3) Intent to Treat Estimates
Dependent Variables			
Very Low Birth Weight	$-.001$	$-.016^{***}$	$-.004^{***}$
	(0.001)	(0.003)	(0.001)
	{0.006}	{0.006}	{0.006}
Low Birth Weight	$-.005^{**}$	$-.024^{***}$	$-.006^{***}$
	(0.002)	(0.009)	(0.002)
	{0.058}	{0.058}	{0.058}
High Birth Weight	$-.001^{*}$	$-.009^{***}$	$-.002^{***}$
	(0.001)	(0.003)	(0.001)
	{0.006}	{0.006}	{0.006}
Log Birth Weight	$.005^{***}$	$.012^{*}$	$.003^{*}$
	(0.002)	(0.007)	(0.002)
	{8.074}	{8.074}	{8.074}
Observations	[186,840]	[186,840]	[186,840]
Log Gestational Age	$-.003^{***}$	$.008^{***}$	$.002^{***}$
	(0.000)	(0.001)	(0.000)
	{3.643}	{3.643}	{3.643}
Preterm<37 weeks	$-.010^{***}$	$-.018^{***}$	$-.004^{***}$
	(0.001)	(0.005)	(0.001)
	{0.127}	{0.127}	{0.127}
Observations	[1,486,353]	[1,486,353]	[1,486,353]
Panel B: Child Mortality			
Any Child Died	$-.019^{***}$	$-.018^{**}$	$-.004^{***}$
	(0.001)	(0.007)	(0.001)
	{0.029}	{0.029}	{0.029}
Observations	[340,091]	[340,091]	[340,091]

Notes: The reported coefficients in column 1 are OLS estimates of obtaining at least a middle school degree on corresponding outcome variables, column 2 are the Two-Sample Instrumental Variables estimates of obtaining at least a middle school degree on the corresponding outcome variables, and column 3 are the Intent-to-Treat estimates. The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. All models include the re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, child year of birth dummies, and province fixed effects. Birth outcome models also control for male child dummy. Data in panels A and B come from the Ministry of Health Birth Outcomes Data (MHBOD) and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC). Birth outcome models are weighted by the number of women in fertility age (ages 15 to 49) in the province. Survey weights provided by the PHC are used in mortality estimations. Individuals born between 1987 and 1991 constitute the treatment group, and those who were born between 1981 and 1985 form the control group. The 1986 cohort is excluded from the analysis. The number of observations is in square brackets []. Outcome variable means of the control group are in curly brackets {}. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$.

observable and unobservable common determinants of maternal schooling and child outcomes, including income, the genetic makeup of the mother, and access to medical care, may bias the estimates of the coefficient on schooling. For example, the negative sign on gestational length may be due to C-section birth deliveries being performed around 38 to 39 weeks of pregnancy. Potential measurement error may also bias the estimates. While the mismeasurement of the independent variable attenuates the estimated coefficient towards zero, the emergence of the same issue on the dependent variable inflates the standard errors.

To get a handle around the potential endogeneity of maternal education, we next employ an instrumental variable (IV) estimation strategy in which *Middle School* is regressed on the 1997 education reform. The IV estimates of the impact of maternal educational attainment on birth outcomes and child mortality are presented in the second column of Table 4. The first stage F statistics shown here are well above the recommended value of 10, supporting the strength of our IV.

In the MHBOD, in panel A, we present the TSIV coefficients, which are equal to the ratio of the reduced form and associated first stage coefficient. We use the delta method to calculate the standard errors for these coefficients, as summarized in Inoue and Solon (2010).[27] Results show that *Middle School* causes a decline in the probability that a child is born with a birth weight of below 1,500 and 2,500 grams by 1.6 and 2.4 percentage points, respectively. Results also show that mothers who hold at least a middle school diploma are 0.9 percentage points less likely to have a baby with a high birth weight (over 4,500 grams). These findings show that maternal education increases the likelihood that a child is born with a healthy birth weight, which also explains why the effect of *Middle School* on *Log Birth Weight* is modest, 1.2, and relatively imprecisely estimated.

Next, we show that maternal education increases gestation duration by 0.8 percent, and it lowers the likelihood of premature births by 1.8 percentage points. Note that the IV estimate switches the sign on *Log Gestational Age*, which suggests that the observed negative relationship in OLS specifications may be due to selection bias. Evaluating these results jointly, we conclude that compulsory maternal schooling favorably impacts child health at birth.

In panel B, we estimate the causal effect of extended maternal primary schooling on child mortality. In line with the evidence presented in panel A of Table 4 (i.e., improved child health at birth), we find that children of mothers who earned at least a middle school degree are 1.8 percentage points less likely to have died by age five.

The IV estimates in the MHBOD are considerably larger than the OLS estimates. This is largely due to phonetic similarities between terms describing five years of basic schooling before the 1997 mandatory education reform and successfully completing at least eight years of education after the reform, which can easily be confused. Therefore, in this sample, many women, bound by the law change, were coded as holding a basic five-year elementary school diploma although they successfully earned an eight-year middle school diploma. It is straightforward that this type of measurement error in maternal education should attenuate the OLS coefficient on schooling. Indeed, this was the reason we employed a TSIV, where we obtained the first-stage estimates from another data set, PHC. Accordingly, we infer that the dramatic difference we observe between the OLS and IV estimates in the MHBOD in

[27] More specifically, following Inoue and Solon (2010), and Mocan, Rashke and Unel (2015), we use the delta method to calculate the standard error of the estimated TSIV coefficient, γ_1, as: $var(\gamma_1) = (\frac{1}{\alpha_1^2})[Var(\delta_1) + \frac{\delta_1^2}{\alpha_1^2} Var(\alpha_1)]$, where α_1 is the first stage coefficient in equations (3) and (4), and δ_1 is the corresponding reduced form coefficient.

panel A is likely due to the mismeasurement of the educational attainment of those who were bound by the reform.[28,29]

In column (3), we display the reduced form impact of the education reform on the outcomes of interest. These findings are consistent with our IV estimates. Next, we present the graphical illustrations of the reduced form effect of the 1997 education reform on Low Birth Weight, Gestational Age, and Any Child Died in Figure 5. [30] These figures support the argument that rather than a sporadic jump in the outcome variables, our IV estimates are driven by systematic shifts in child health at birth and child mortality due to the mother's extended primary schooling.

ROBUSTNESS

We present our main robustness tests in Table 5. An important issue in statistical inference for regression analysis is the possibility that standard errors are correlated within clusters (Cameron, Gelbach, & Miller, 2008; Cameron & Miller, 2015). As we previously discussed, the school starting age in Turkey is primarily determined by a child's birth year. Nevertheless, the mandate of the reform may have differential effects on the educational attainments of individuals residing in different parts of the country. Therefore, in line with Agüero and Bharadwaj (2014), Cesur and Mocan (2018), and Grépin and Bharadwaj (2015), we adjust the standard errors at the mother's birth year by province level. In column (1) of Table 5, we present the robustness of our standard errors to the use of an alternative clustering unit, the mother's month-year date of birth. We supplement this analysis in Table A7 by clustering the standard errors on the province (column 2), the mother's month-year date of birth by province (column 4), region by birth year (column 5), and birth year (column 6).[31] Inference based on adjusting standard errors at different levels does not alter our conclusions.

As there is some uncertainty on whether those born in 1986 were bound by the compulsory schooling reform, we excluded the 1986 birth cohort from our benchmark analysis. In columns (2) and (3), we assigned alternate treatment values to (Reform = 0) and (Reform = 0.25) concerning exposure to the 1997 education reform for those who were born in 1986, in that order. These estimates are very similar to our baseline findings.

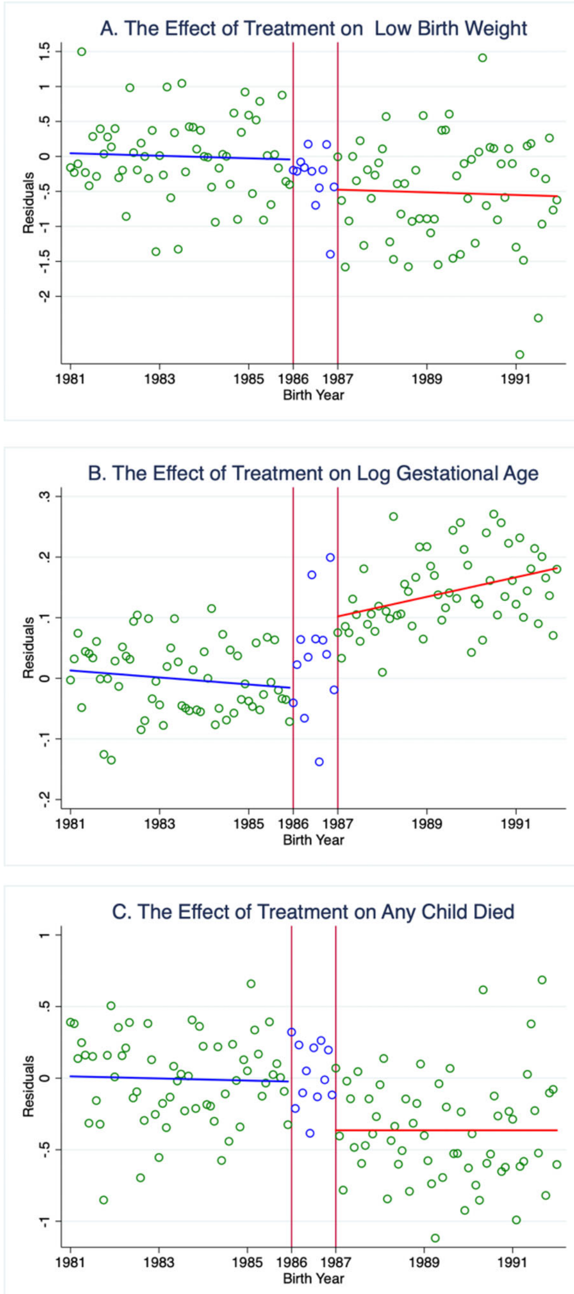
Recall that, in our main specifications, we accounted for birth cohort trends by controlling for maternal birth month-year differentiated by exposure to the education reform status. We check if our baseline estimates change when we specify

[28] Although the potential violation of the exclusion restriction could theoretically be another reason behind the large difference between the OLS and IV coefficients in favor of the latter, we do not worry about this possibility. If our instrument were to fail the exclusion restriction, we would have observed the same pattern for child mortality estimates as well. As the results in panel B display, the difference between the OLS (column 1) and IV (column 2) estimates is small when educational attainment is recorded correctly by the TurkStat surveys. Therefore, we conclude that the possible violation of the exclusion restriction is of minimal concern for our study.

[29] While the magnitude of the difference between the OLS and IV estimates may be related to the size of the LATE among the compliers of the current education reform, the comparison of OLS versus structural coefficients is not appropriate for this purpose. Because it is theoretically impossible to determine the size (and even the direction) of the bias of the OLS estimates, we defer the evaluation of the magnitude of our LATE to the discussion section, where we compare it to other causal estimates.

[30] The graphical illustrations of the reduced form effect of the 1997 education reform on *Log Birth Weight*, *Very Low Birth Weight*, *High Birth Weight*, and Premature are shown in Figure A3. The Appendix is available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[31] In these specifications, we use the reduced form estimates to conduct our robustness to clustering at different units.



Notes: Each figure visualizes the reduced-form impact of being exposed to the 1997 Education Reform on child health at birth and child mortality outcomes for women born up to five years before and after the pivotal cohort of 1986 using residuals net of exogenous controls described in the text. The data source is the Ministry of Health Birth Outcomes Data (MHBOD).

Figure 5. The Impact of the 1997 Education Reform on Outcome Variables. [Color figure can be viewed at wileyonlinelibrary.com]

Table 5. The impact of maternal education on child health, robustness checks.

	(1) Cluster at Birth Month	(2) Alt. Treatment for 1986 Birth Cohort T = 0	(3) Alt. Treatment for 1986 Birth Cohort T = 0.25	(4) Quadratic BC Control Function	(5) Cubic BC Control Function	(6) With Province Specific Birth Cohort Trends	(7) At Most Middle School Sample
Very Low Birth Weight	$-.016^{***}$	$-.012^{**}$	$-.014^{***}$	$-.015^{***}$	$-.013^{***}$	$-.016^{***}$	$-.011^{***}$
	(0.005)	(0.006)	(0.004)	(0.002)	(0.004)	(0.003)	(0.003)
Low Birth Weight	$-.024^{*}$	$-.029^{***}$	$-.027^{***}$	$-.024^{***}$	$-.020^{*}$	$-.025^{***}$	$-.022^{**}$
	(0.014)	(0.010)	(0.010)	(0.009)	(0.012)	(0.008)	(0.011)
High Birth Weight	$-.009^{**}$	$-.009^{***}$	$-.009^{***}$	$-.009^{***}$	$-.011^{**}$	$-.009^{***}$	$-.009^{**}$
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Log Birth Weight	.012	$.025^{**}$	$.016^{**}$	.013	.004	$.013^{**}$	.008
	(0.010)	(0.008)	(0.008)	(0.008)	(0.010)	(0.005)	(0.011)
Observations	[186,840]	[208,424]	[208,424]	[186,840]	[186,840]	[186,840]	[116,184]
Log Gestational Age	$.008^{***}$	$.003^{**}$	$.008^{***}$	$.008^{***}$	$.007^{***}$	$.008^{***}$	$.006^{***}$
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Preterm<37 weeks	$-.018^{***}$	$-.011^{*}$	$-.021^{***}$	$-.019^{***}$	$-.007$	$-.018^{***}$	$-.007$
	(0.007)	(0.006)	(0.006)	(0.005)	(0.008)	(0.005)	(0.005)
Observations	[1,486,353]	[1,654,931]	[1,654,931]	[1,486,353]	[1,486,353]	[1,486,353]	[961,594]
Any Child Died	$-.018^{***}$	$-.020^{***}$	$-.019^{***}$	$-.019^{***}$	$-.018^{*}$	$-.018^{***}$	$-.020^{***}$
	(0.006)	(0.007)	(0.007)	(0.006)	(0.009)	(0.006)	(0.006)
Observations	[340,091]	[375,433]	[375,433]	[340,091]	[340,091]	[340,091]	[234,084]

Notes: The reported coefficients are the Two Sample Instrumental Variables estimates of obtaining at least a middle school degree on the corresponding outcome variables (except column 1). The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level and calculated by delta method. In column 2, we cluster the standard errors at the birth month level. Individuals born between 1987 and 1991 constitute the treatment group and those who were born between 1981 and 1985 form the control group. Columns 3 and 4 include the 1986 cohort. In all other columns, the 1986 cohort is excluded from the analysis. The Ministry of Health Birth Outcomes Data (MHBOD) and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC) are used in estimations. Models include re-centered birth date in months differentiated by exposure to the 1997 reform status (columns 5 and 6 include quadratic and cubic birth cohort control functions, respectively), mother's month of birth dummies, child year of birth dummies, and province fixed effects. Column 7 additionally controls for province-specific linear time trends. Birth outcome models also control for male child dummy. Birth outcome models are weighted by number of women in fertility age (ages 15 to 49) in the province. Survey weights provided by the PHC are used in corresponding estimations. The number of observations is in square brackets []. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$.

maternal birth month-year quadratically and cubically. As seen in columns (4) and (5) of Table 5, these results are similar to our main findings.

If there is convergence in child health outcomes between different regions by birth cohort, our results may be biased. Therefore, in column (6), we test our estimates' resilience to controlling for region-specific maternal birth year trends. To do so, we interact binary province indicators with women's birth month-year and specify them in our regressions.[32] Results show that when we control for province-specific trends, the estimated effect of extended primary schooling is estimated with greater precision. These estimates show that our findings are not driven by convergence in child outcomes by maternal birth year between regions.

We discussed that the 1997 education reform aimed to increase schooling of girls who come from families with a low interest in their daughters' formal education. We also argued that our key contribution is the effect of schooling among these populations and noted that how extended maternal education impacts child outcomes among women at the lower tail of the educational attainment distribution is particularly important from a policymaker's viewpoint. While our data do not allow us to identify women whose families would prevent them from attending school without compulsory schooling, we undertake a second-best approach to tackle these questions.

Accordingly, we reproduce our main models by limiting the estimation sample to those with at most a middle school degree in the final column of Table 5. In these specifications, we assume that the law mandate would not substantially increase the educational attainment of populations who already have a low interest in extra schooling beyond the mandated levels. If our conjecture is correct, most of the impact of maternal education on child health we find should come from this sample and vice versa. Consistent with this prediction, estimates of the impact of maternal education on child health in the (at most) middle school diploma sample are highly similar to the ones presented in Table 5 despite significantly reduced sample sizes. These results offer further support that our findings are driven by the impact of extra schooling in the lower tail of educational attainment distribution.

We conducted our main analysis using the sample of individuals born five years before and after 1986, the pivotal birth cohort. While employing a wider estimation window boosts statistical power by increasing the sample size, the possibility that identifying variation comes from individuals who are further away from the discontinuity may threaten our estimates' validity. Therefore, we assess whether our preferred estimates hold when we change the data estimation window. However, before showing these estimates, we examine if our results are robust to undertaking the formal regression discontinuity (RD) estimation procedure using local linear regressions given that our empirical methodology is consistent with a fuzzy RD design (Lee & Lemieux, 2010). In doing so, we estimate our models with the procedure described by Calonico, Cattaneo, and Titiunik (2014). As presented in column (1) of Table 6, the local linear RD IV estimates, using the optimal bandwidth, produce similar results to our baseline findings. Then, in columns (2) to (6) of Table 6, we estimate our baseline specifications using the sample of individuals born within 6±, 5±, 4±, 3±, and 2± years of the pivotal birth cohort of 1986, respectively.[33]

[32] This also accounts for any bias that may stem from convergence in child outcomes between provinces (and regions) over the years.

[33] Table A8 shows the estimates of the impact of the 1997 education reform on the Middle School using the sample of individuals born within 6±, 5±, 4±, 3±, and 2± years of the pivotal birth cohort of 1986, respectively, in the PHC data. The Appendix is available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 6. The impact of maternal education on child health, robustness alternative data intervals.

	Local Linear RD (1)	80–92 (2)	81–91 (3)	82–90 (4)	83–89 (5)	84–88 (6)
Panel A: Birth Outcomes						
Dependent Variables						
Very Low BirthWeight	−.019***	−.010***	−.016***	−.016***	−.020***	−.017***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)
Opt. BW./Observations	{38/125,378}					
Low Birth Weight	−.023**	−.019***	−.025***	−.024***	−.022**	−.023
	(0.012)	(0.007)	(0.008)	(0.008)	(0.010)	(0.017)
Opt. BW./Observations	{38/125,378}					
High Birth Weight	−.008**	−.009***	−.009***	−.011***	−.008*	−.018***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)
Opt. BW./Observations	{35/116,228}					
Log Birth Weight	.018*	.009*	.013**	.011*	.016**	.025
	(0.009)	(0.005)	(0.005)	(0.006)	(0.007)	(0.018)
Opt. BW./Observations	{33/110,196}					
Observations		[214,185]	[186,840]	[154,533]	[119,450]	[82,427]
Log Gestational Age	.008***	.007***	.008***	.008***	.008***	.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Opt. BW./Observations	{33/873,787}					
Preterm< 37 weeks	−.016***	−.013***	−.018***	−.011***	−.020***	−.021***
	(0.008)	(0.005)	(0.005)	(0.004)	(0.006)	(0.009)
Opt. BW./Observations	{33/873,787}					
Observations		[1,710,454]	[1,486,353]	[1,227,026]	[947,857]	[650,923]
Panel B: Child Mortality						
Any Child Died	−.015*	−.015**	−.018***	−.018**	−.017*	−.044***
	(0.009)	(0.006)	(0.006)	(0.007)	(0.009)	(0.016)
Opt. BW./Observations	{42/236,313}					
Observations		[406,046]	[340,091]	[269,958]	[204,549]	[138,150]

Notes: The reported coefficients in column (1) are the Local Linear Regression Discontinuity IV estimates of obtaining at least a middle school degree on corresponding outcomes. Optimal bandwidths presented in curly brackets {} are calculated using one common MSE-optimal bandwidth selection procedure (Calonico, Cattaneo, & Titiunik, 2014). Point estimates presented in columns (2) to (6) are the Two-Sample Instrumental Variables estimates of obtaining at least a middle school degree on the outcome variables using the sample of individuals born within 6±, 5±, 4±, 3±, and 2± years of the pivotal birth cohort of 1986, respectively. The 1986 cohort is excluded from the analysis. Column headings pertain to data estimation intervals based on the year of birth of the mother. The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level and calculated by the delta method. The Ministry of Health Birth Outcomes Data (MHBOD) and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC) are used in estimations. Models include the re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, child year of birth dummies, province fixed effects, and province-specific linear time trends. Birth outcome models are weighted by the number of women in fertility age (ages 15 to 49) in the province. Survey weights provided by the PHC are used in corresponding estimations. The number of observations is in square brackets []. ***$p < .01$; **$p < .05$; *$p < .1$.

These results document that our findings are robust to employing alternative data intervals.

It is commonly observed in developing countries that families may provide less care to their daughters because of son preference (Bharadwaj & Lakdawala, 2013; Oster, 2009). We explore this possibility (whether the effect of maternal education on child health differs by the gender of the offspring) and find that estimates from these models are similar to those using the full sample; hence, the impact of mother's schooling on child health does not seem to depend on child gender (see Table A9 for details).

We continue our robustness tests by undertaking a placebo analysis, similar to the one adopted by Wherry et al., 2018. Accordingly, we placed an artificial cutoff for each month between January 1975 to January 1981.[34] Then, we reproduced our models for each outcome using the identical specifications we employed in our MHBOD. For each outcome, this gave us 84 placebo treatments. Then, we plot the distribution of the placebo estimates and compare them to our main findings in Figure A4(a)–A4(b). The *p*-values correspond to the share of placebo estimates greater in magnitude than the "true effect" we present in Table 5. The *p*-values are less than .05 for Very Low Birth Weight and Gestational Age and less than .1 percent for Low Birth Weight and Premature birth specifications. For Log of Birth Weight and High Birth Weight specifications, the *p*-values are .116 and .121, respectively. These placebo exercises suggest that four out of six of our estimates are below the threshold of .1 and are not likely to be driven by random factors. The likelihood that the remaining two (Log of Birth Weight and High Birth Weight) are not driven by random factors is 88.4 and 87.9 percent, respectively.

Next, we replicated our main estimates by restricting the estimation sample to children for whom we have complete data on both birth weight and gestational age. Estimates provided in Table A10 show that our results are robust to limiting our estimation sample to non-missing birth weight and gestational age observations. Finally, considering the fact that we estimate the impact of obtaining at least a middle school degree on a number of outcomes, we performed a multiple hypothesis testing exercise to ensure that our inference is robust to adjusted standard errors (Simes, 1986). Table A11 presents the corresponding *p*-values, which suggest that our findings are robust to adjustment for multiple hypothesis testing.

MATERNAL BEHAVIORS AS POTENTIAL MECHANISMS

What may be the mechanisms connecting maternal education to child health? Unfortunately, we lack access to sufficiently large data sets providing rich information on potential mediating pathways. Having said that, while the MHBOD does not contain extensive information on maternal behaviors, the data provide information on the method of birth delivery and maternal smoking.

If education impacts women's behaviors during pregnancy in terms of seeking preventive care and adherence to healthcare professionals' recommendations, the likelihood of experiencing health complications during child delivery may be reduced (Currie & Moretti, 2003; Cutler & Lleras-Muney, 2006; McCrary & Royer, 2011). Furthermore, while the primary purpose of utilizing non-natural childbirth delivery methods (e.g., C-section, vacuum extraction, forceps delivery) is to prevent potential health complications that may impact the mother and child, C-sections are also preferred by some women due to perceived pain avoidance during birth delivery (Penna & Arulkumaran, 2003), in spite of the increased recovery time. Studies have shown that children who are not exposed to maternal bowel flora during birth due to C-section birth delivery may be more likely to face adverse health consequences later in life, including increased risk of obesity, asthma, and energy uptake from the gut and immune development (Black et al., 2016; Blustein & Liu, 2015). Therefore, women who are more knowledgeable about the subsequent health effects of C-sections due to extended schooling may consider attempting to avoid C-section

[34] Because our sample comprises women who gave birth and has few women above the age of 40, moving the arbitrary treatment timing before 1975 leads to small cell sizes, producing sporadic estimates.

Table 7. Estimates of the impact of education on maternal behaviors.

Dependent Variables	(1) OLS	(2) IV Estimates	(3) Intent-to- Treat Estimates
Normal Birth	-0.118^{***}	0.101^{***}	0.023^{***}
	(0.004)	(0.016)	(0.002)
	{0.431}	{0.431}	{0.431}
Observations	[1,485,732]	[1,594,793]	[1,594,793]
Ever Smoked	0.019^{***}	-0.037^{***}	-0.008^{***}
	(0.001)	(0.009)	(0.002)
	{0.108}	{0.107}	{0.107}
Current Smoker	0.009^{***}	-0.015^{***}	-0.003^{***}
	(0.001)	(0.005)	(0.001)
	{0.072}	{0.072}	{0.072}
Observations	[550,001]	[550,001]	[550,001]

Notes: The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. Individuals born between 1987 and 1991 constitute the treatment group and those who were born between 1981 and 1985 form the control group. The 1986 cohort is excluded from the analysis. The Ministry of Health Birth Outcomes Data (MHBOD) and the Turkish Statistical Institute's Population and Housing Census 2011 (PHC) are used in estimations. All models include the re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, child year of birth dummies, province fixed effects, and male child dummy. The number of women in fertility age (ages 15 to 49) in the province and survey weights provided by the PHC are used in the corresponding estimations. The number of observations is in square brackets []. Outcome variable means of the control group are in curly brackets {}. $^{***}p < 0.01$; $^{**}p<0.05$; $^{*}p<0.1$.

birth deliveries. *Normal Birth*, which may capture child health at birth or maternal investment in child health, is a binary variable equal to one if the birth delivery was vaginal, and zero otherwise. Lastly, we construct two maternal smoking measures that may capture the mother's health behaviors during pregnancy. *Ever Smoked* and *Current Smoker* are dichotomous covariates representing whether the mother ever smoked regularly and whether she was a smoker at the time of data collection, respectively.[35] Descriptive statistics, displayed in Table A12, show that 46 percent of women experienced natural birth delivery, and this rate is significantly higher in the treatment group (52 percent) compared to the control group (42 percent).[36] Women in the treatment group are less likely to report they have ever smoked or are smoking at the time of the data collection than those in the control group.

In Table 7, we present the estimates of the relationship between women's education and maternal behaviors. In column (1), the OLS estimates reveal that women with at least eight years of schooling are 12 percentage points less likely to deliver babies via natural birth delivery. Given that C-sections constitute a large fraction of non-normal birth delivery methods (53.5 percent in our sample), the negative association between maternal education and *Normal Birth* is consistent with the argu-

35 While it would have been informative to know the status of women's tobacco use during pregnancy, our data do not allow us to make such a distinction.

36 Turkey has a disproportionately high rate of C-sections compared to the world average (Akarsu & Mucuk, 2014; Zeldin, 2012). C-sections are an important option, particularly when the mother or infant is facing a life-threatening situation during labor or delivery. Yet it is riskier than vaginal delivery and has a longer recovery period. In spite of this, many Turkish women prefer C-sections to vaginal births, partly due to "labor pain and fear" (Akarsu & Mucuk, 2014).

ment that women with more schooling in Turkey prefer C-section birth deliveries.[37] It is possible that as more educated women can financially afford C-sections, pain avoidance during delivery may partially explain the observed positive association. The high prevalence of C-sections may also explain the negative association between maternal education and gestational age as C-section surgeries are usually performed at around 38 to 39 weeks of gestation (Davidoff et al., 2006).

In column (1), we also explore the associations between women's education and smoking behavior. This can be a mechanism through which maternal education is linked to child health. We find that women who hold at least a middle school diploma are 1.9 and 0.9 percentage points more likely to have ever smoked cigarettes and smoke at the time of data collection, respectively. This finding is consistent with the observed positive association between education and female smoking in middle-income countries (Cutler & Lleras-Muney, 2012). As discussed by Cutler and Lleras-Muney (2012), this pattern is likely to represent the social context of female smoking in such countries, including women's empowerment and social acceptability.

In column (2), we present the IV estimates of the impact of female schooling on maternal behaviors. Children with mothers who earned at least a middle school diploma are 10.1 percentage points more likely to be delivered via normal birth. This finding is in stark contrast to the OLS estimate presented in column (1), in which we found that maternal education was negatively correlated with the likelihood of natural birth delivery. Different factors can explain why an increase in maternal educational attainment positively impacts natural birth delivery. First, likely due to improvements in allocative efficiency, access to maternal care, and increased income, if maternal education induces mothers to exhibit health-improving behaviors during pregnancy, the chances of experiencing health complications during birth delivery may diminish. For instance, we have already shown that maternal schooling has a negative impact on high birth weight, a first-order determinant of C-section birth deliveries. Moreover, if more educated mothers are more aware of the child health benefits of vaginal birth delivery, they may choose to avoid unnecessary C-sections.

When we turn our attention to maternal smoking, in column (2), we find that receiving at least eight years of schooling lowers the likelihood of ever smoking regularly and being a current smoker by 3.7 and 1.5 percentage points, respectively. Given that women's smoking is found to have adverse effects on child health, these results suggest that mothers' avoidance of risky health behaviors may mediate the relationship between maternal education and child health. In column (3), we display the reduced form effect of education reform on maternal attitudes. These exercises produce estimates that are in line with the IV estimates presented in the previous column.

The results presented in Table 7 suggest that increased schooling among women improves their behaviors towards enhancing their offspring's health. This conclusion boosts our confidence in our findings, showing that extended maternal schooling improves child health at birth and lowers child mortality. As far as we know, one study, Dursun, Cesur, and Mocan (2018), has examined the impact of junior high school completion on health outcomes exploiting the 1997 education reform as the source of identifying variation. They find that holding at least a middle school diploma has some favorable effects on women's health measured by self-reported excellent health and maintaining healthy body weight. Nevertheless, because smoking data are self-reported and collected by healthcare professionals in less than ideal

[37] In an unreported specification, we also estimated the likelihood of C-section on *Middle School*. In line with the OLS estimate of *Normal Birth*, we find that women who completed at least eight years of schooling are 12 percentage points more likely to use C-section as the method of birth delivery.

settings (such as in the presence of other family members), caution should be exhibited as it is possible that women's educational schooling may also influence their truthful reporting of health behaviors in surveys (Dursun, Cesur, & Mocan, 2018).

In Table A13, we estimate the impact of holding at least a middle school diploma on the number of live births delivered by the mother. The results show that junior high school completion reduces the number of births by 0.97. Because the women in our sample are fairly young, these findings are unlikely to correspond to the impact of education on complete fertility. Related literature finds that while schooling delays the age of birth delivery, it does not necessarily affect total fertility (Ali & Gurmu, 2018; Miller, 2010). In line with this view, Kirdar, Dayıoğlu, and Koç (2018), exploiting the 1997 education reform in Turkey, find that education increases the age of marriage and birth delivery. Consequently, our estimates and those of Kirdar, Dayıoğlu, and Koç (2018) imply that the avoidance of teen pregnancy, which can impose a substantial risk to the health of the child (Ashcraft, Fernández-Val, & Lang, 2013), may serve as a mediating pathway between maternal education and child health at birth and infant mortality.

DISCUSSION

This research examines whether maternal education improves child health by exploiting a compulsory schooling reform in Turkey, where a significant share of families prevented their daughters from attending formal schooling beyond five years of basic education. To study this question, we use the 1997 compulsory schooling reform, which extended mandatory primary schooling from five to eight years, as an instrumental variable. The fact that previous literature offers little knowledge on the impact of receiving additional schooling for females facing substantial social barriers to education magnifies the extent of our contribution. Besides, although there exists credible evidence documenting the favorable effects of education in developing societies, the extant literature has not sufficiently addressed the effects of maternal schooling on child health, and the subject invites additional work. Finally, many of the studies using data from developing societies lack adequate statistical power.

Using two large data sets, we first establish that the reform has a massive (i.e., 23 percentage points) impact on female middle school completion rates. We analyze a host of outcomes, including birth weight, gestation, child mortality, birth delivery method, and maternal smoking. Our findings show that mothers who earned at least a middle school diploma are less likely to give birth to babies with very low birth weight, low birth weight, or high birth weight. They are less likely to have a child die by age five. Additionally, obtaining increased schooling extends gestational age and lowers the propensity of delivering premature babies. Finally, we also find evidence suggesting that additional extended primary education increases the likelihood of normal births and decreases maternal smoking. These results survive an extensive set of robustness checks.

The effect sizes found in this study are not trivial. We find that, compared to primary school completion, middle school completion leads to a 2.5 percentage point decrease in the probability of low birth weight. Amarante et al. (2016) find that a generous conditional cash transfer (CCT) in Uruguay resulted in a comparable 1.9 to 2.4 percentage point decrease in low birth weight. We find that middle school completion increases child birth weight by 4.1 percent, from a mean of 3,246, implying a 133-gram increase. Oportunidades beneficiary status is associated with 127.3 g higher birth weight in Mexico (Barber & Gertler, 2008), and Profamilia, a family planning program in Colombia aimed directly at helping mothers, is associated with a 400-gram increase in birth weight (Fiszbein et al., 2009).

To put the estimates in context and enable comparison with other studies examining the relationship between maternal education and child health in developing countries, we calculated the impact of at least one additional year of maternal education, which is obtained by dividing our estimates by three given that the 1997 education reform led to an increase of at least three years of education among those who are impacted by the law. Our results imply that at least one more year of maternal education lowers the probability of very low birth weight by 0.52 percentage points, low birth weight by 0.83 percentage points (a 13.8 percent reduction from a mean of 6.01 percent for a fixed population of births), and high birth weight by 0.3 percentage points. It lowers the probability of premature birth by 0.6 percentage points and the probability of child mortality by 0.6 percentage points (approximately 23 percent from a mean of 2.6 percent). These effect sizes are similar to those reported by Grépin and Bharadwaj (2015), who find that child mortality goes down by 21 percent in Zimbabwe following a schooling reform, and Chou et al. (2010), who find a 19 percent reduction in the number of post-neonatal deaths following a schooling reform in Taiwan.

While we find evidence suggesting that maternal behaviors, such as smoking, could be instrumental in explaining the relationship between mother's education and child health, lack of data on an extensive set of maternal outcomes and behaviors (e.g., healthcare utilization, income, match quality, and diet) deter us from an in-depth exploration of potential mediators. Using the 1997 education reform, Torun (2015) finds that extended primary schooling increases women's wages. Combined with our findings on smoking, these results suggest education can impact maternal behaviors to improve the well-being of their children in addition to being able to afford child health-enhancing goods and services. Cesur and Mocan (2018) find that more educated women exhibit lower religiosity. This result provides indirect evidence for the likelihood that educated women may be more likely to utilize modern medicine and put less weight on prayer and alternative medicine. Furthermore, improvements in marital match quality due to education (Behrman & Rosenzweig, 2002) and friendship networks through school (Aizer & Stroud, 2010; Lochner, 2011) may improve child health.

Importantly, our estimates correspond to the LATE of maternal education on birth outcomes and child mortality among mothers facing social barriers to having their education extended beyond five years of basic schooling in the absence of the mandate of the law. The 1997 education reform in Turkey is a unique one in developing countries because it allows one to estimate the policy implications of compulsory schooling at the lower end of the educational attainment distribution. Note that, as we previously discussed, some aspects of the 1997 reform may have also reduced the cost of educational attainment and enabled some individuals to afford schooling; thus, the LATE of this study does not perfectly represent the pure effect of extended education among those facing substantial social obstacles to additional schooling. That being said, to the best of our knowledge, this reform provides the closest quasi-experiment that enables one to study the impact of mandated schooling in a developing country among girls whose families restrict their educational opportunity.

While the efforts toward accomplishing universal primary schooling in poorer societies lead to significant progress, many developing countries have relatively few years of mandated education. Furthermore, those with compulsory schooling laws often fail to enforce the rule, especially for females. Taken together, our findings underscore the role of positive health spillovers of women's education in developing countries in particular, where rates of female educational attainment are significantly lower due to social and cultural restrictions. Therefore, this research provides strong support for the argument that enacting compulsory schooling laws and enforcing them in developing countries may have large positive externalities in terms of child health.

BAHADIR DURSUN is a Lecturer (Assistant Professor) of Economics at Newcastle University and Newcastle University Business School and a Research Affiliate at the Institute of Labor Economics (IZA), 5 Barrack Road, Newcastle Upon Tyne NE1 4SE, United Kingdom (e-mail: Bahadir.Dursun@newcastle.ac.uk). He is the Corresponding Author for this article.

RESUL CESUR is an Associate Professor of Healthcare Economics at the University of Connecticut School of Business, Finance Department, a Research Fellow at the Institute of Labor Economics (IZA), and a Research Associate at the National Bureau of Economic Research (NBER), 2100 Hillside Road, Unit 1041, Storrs, CT 06269-1041 (e-mail: cesur@uconn.edu).

INAS R. KELLY is a Professor of Economics at Loyola Marymount University, Department of Economics, and a Research Associate at the National Bureau of Economic Research (NBER), University Hall 4200, 1 LMU Drive, Los Angeles, CA 90045 (e-mail: inas.kelly@lmu.edu).

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