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Omer Bahadir Dursun
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ESSAYS ON THE IMPACT OF EDUCATION ON ECONOMIC OUTCOMES IN A DEVELOPING COUNTRY

A Dissertation

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor in Philosophy

in

The Department of Economics

by
Ömer Bahadır Dursun
B.S., Marmara University, 2008
M.S., Georgia Institute of Technology, 2010
M.S., University of Mannheim, 2014
August 2017

This dissertation is dedicated to my wife Elif, and my parents Serap and Şevket Dursun…

ACKNOWLEDGEMENTS

This journey would not be possible without the support and encouragement of a group of individuals. First, I would like to express my sincere gratitude to my advisor Dr. Naci Mocan for being a great advisor and his endless support. Dr. Mocan played a significant role on shaping my academic thinking and stance toward life. I will always be grateful to him.

I would also like to thank my dissertation committee members Dr, Robert Newman and Dr. Louis-Philippe Beland. I benefited from their insightful comments during my studies in LSU. I am grateful to Dr. Areendam Chanda, Dr. Bulent Unel, and Mr. Charles Roussel for intellectual discussions and support.

I must express my special gratitude to my co-author Dr. Resul Cesur for his excellent comments, encouragement, and motivation throughout this journey. He never hesitated to share his knowledge with me.

During my graduate studies, I also had chance to discuss my research with a great group of colleagues and friends: Ekrem Cunedioglu, Satadru Das, Sukriye Elif Filiz, Tatyana Gileva, Duygu Guner, Sujana Kabiraj, Masayuki Onda, Sara Oloomi and Luiza Pogorelova, I would like to thank them all for their valuable inputs to my research.

Members of the Tayfa-ul Muallimin, Serdar Kural, and Mehmet Ozgur, provided endless support for every single obstacle that I faced during these years.

I would like to thank my parents Serap and Sevket Dursun, and my brother Mehmet Akif Dursun. They have always supported my decisions from the very beginning, and have not hesitated to make sacrifices for me, while I was trying to reach my dreams. In this respect, I would also like to thank Ozben and Guran Yet (my second family), and Dr. David Goldsman for their trust and encouragement, I will always be indebted to them.

Last but not the least, I would like to thank my better half Elif, for her unconditional love and faith in me. Even in the darkest moments of this journey she was always there for me. If it weren't for her, I would not be able to come this far.

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ABSTRACT

In this dissertation, I present three distinct essays in economics of education and health economics that can be read independently from one another. These studies investigate the non-pecuniary benefits of extended primary schooling in a developing country setting. I exploit the 1997 education reform in Republic of Turkey, which extended the duration of mandatory schooling from 5 to 8 years, to address the endogeneity of educational attainment levels of individuals. A unique feature of the schooling reform of 1997 is that, in a developing country, it arguably provides one of the most suitable empirical frameworks to identify the local average treatment effect of compulsory education among individuals with a low tendency to extend their schooling beyond five years of elementary school. Chapter 2 provides robust evidence in favor of the argument that increasing the duration of mandatory primary education among women who have a low interest in receiving more schooling may have substantial non-pecuniary benefits in terms of the health of their offspring measured by birth weight outcomes and child mortality in developing country setting. In Chapter 3, I examine the impact of mandatory extended primary schooling on happiness of young adults. My analysis reveals that, for females, obtaining at least a middle school degree increases the likelihood of being happy and propensity of being satisfied with various life domains. Descriptive analysis suggests that being hopeful about one's future plays an important role behind this finding. For the case of males, although relatively imprecisely estimated, I find evidence that obtaining at least a middle school degree leads to a decline in happiness. Auxiliary analysis documents the imbalance between aspirations and attainments may be the reason behind this finding among men. In Chapter 4, I examine the impact of extended primary schooling on a set of health indicators among young individuals. In this study I document that; extending schooling may impact women and men differentially, and education does not necessarily promote health and health behaviors. More specifically, while increased education increases male body weight indicators (i.e., overweight, obese), it lowers the propensity of being overweight among women. Findings of this study also indicate that while the effect of extending primary schooling on smoking is positive among females, it's negative for the case of males. Further investigation suggests peer effects, and time use play important roles in explaining these findings.

CHAPTER 1. INTRODUCTION

This dissertation consists of three distinct essays within the fields of economics of education and health economics. More specifically, I focus on the field of human capital formation and its effects on health outcomes and various aspects of individual well-being, by concentrating on the implications of a compulsory schooling reform in a developing country.

Empirically it's hard to disentangle the causal relationship between an individuals' education level and the outcomes of interest. This is because educational attainment level of an individual is potentially correlated with some unobservable third party characteristics, which can be determinant of the outcomes of interest. To tease out the causal impact of education on the outcomes of interest, we need an exogenous event which allows us to predict the educational attainment level of individuals, and potentially not correlated with the outcomes of interest. Luckily, 1997 education reform enacted in the Republic of Turkey provides an excellent opportunity to overcome the problem summarized above.

Prior to the 1997 compulsory education reform, the Turkish Education system was comprised of three stages (i.e., 5 year of basic education, 3 year of middle school education and 3 year of high school). While primary school completion (5 years) was mandatory, the subsequent levels were voluntary. By the law number 4306, the Turkish Government increased compulsory education in Turkey from 5 years of basic education to 8 years, by merging the primary and the middle schools under the same entity.

Accordingly, individuals born after 1986 were exposed to the mandate of the reform and had to complete at least a middle school, while those who were born before 1986 were exempt from the mandate of the reform, and subsequent educational attainment levels after elementary school were voluntary.

Following features of the 1997 education reform provide an ideal natural experiment to study the impact of extended primary schooling on a number of outcomes. First, the reform focuses mainly on increasing the enrollment; it did not make any curricular or compositional changes for pupils (Dulger 2004; Cesur and Mocan 2017), which means 1985 cohort (just missed being exposed to reform) and 1987 cohort (first fully affected cohort) were contingent upon the same classes and regulations. Second, due to the political and institutional power struggle between the military and the elected government, the passage of the reform was substantially exogenous (European Union membership negotiations played a significant role on quick passage of the law as well). Third, while similar

reforms in other developing countries enable researchers to tease out the impact of increased educational attainment among people with a high interest in receiving more education (such as through increased school availability and/or the elimination of school fees when those who desire to attend school cannot afford it), the exogenous variation in education induced by 1997 compulsory schooling reform allows one to examine the impact of education among those with a low tendency to obtain additional years of schooling. Given that many developing nations have weakly enforced compulsory schooling laws and usually require relatively few years of education (UN 2015), separating the impact of extra schooling among individuals with a low tendency to attend school from that of increases in educational attainment of those with a greater propensity to get education may have important policy implications.

Overall, in all three essays forming this dissertation, I exploit the 1997 compulsory education reform as the source of identifying variation to address the endogenous nature of the educational attainment levels of individuals, and investigate the impact of obtaining at least a middle school degree, induced by the 1997 education reform, on a host of different outcomes. More specifically, in Chapter 2, I examine the impact of mother's extended primary schooling on their children's birth outcomes, (i.e., very low birth weight, low birth weight, and high birth weight categories, premature births, and child mortality) using two large datasets from the Republic of Turkey. In the following chapter, I study the impact of mandatory extended primary schooling, on happiness of young adults. In Chapter 4, I investigate the impact of obtaining at least a middle school degree on health indicators including body weight measures (i.e., underweight, overweight, obese), and smoking behaviors among young adults. Finally, Chapter 5 summarizes the findings of these three studies.

CHAPTER 2: THE VALUE OF MANDATING MATERNAL EDUCATION IN A DEVELOPING COUNTRY

2.1 Introduction

Exploiting different natural experiments, a number of studies have analyzed the causal effect of maternal education on child outcomes in developing countries. The literature thus far, though, 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 and Duflo 2004; Chou et al. 2010; Grépin and Bharadwaj 2015; Günes 2015; Makate and Makate 2016), there is also evidence suggesting that mother's schooling has little effect on birth outcomes and infant mortality (Keats 2014; Zhang 2014).[1]

Aside from the non-uniformity on whether parental schooling causally impacts health outcomes of children, existing literature offers fairly limited information on specific policy implications of the local average treatment effects (LATE) of alternative education reforms among different types of populations. For instance, some natural experiments enable researchers to tease out the impact of increased educational attainment among people with a high interest in receiving more education (such as through increased school availability and/or the elimination of school fees when those who desire to attend school cannot afford it). Conversely, exogenous variation in education induced by compulsory schooling laws allows one to examine the impact of education among those with a low tendency to obtain additional years of schooling. Given that many developing nations have weakly enforced compulsory schooling laws and usually require relatively few years of education (UN 2015), separating the impact of extra schooling among individuals with a low tendency to attend school from that of increases in educational attainment of those with a greater propensity to get education may have important policy implications.

Additionally, most studies coming from developing countries are hindered by limited statistical power due to small sample sizes and/or a lack of strong instrumental variables to predict statistically and economically significant changes in schooling. Hence, the current incomplete state of the literature on maternal education and child health warrants more research (Grossman 2015).

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

Using two large data sets from the Republic of Turkey, the current study examines the causal effect of mandatory maternal education on birth outcomes and child mortality by utilizing the 1997 education reform, which extended the duration of minimum years of compulsory schooling from 5 to 8 years, as the source of exogenous variation in mother's education. The analysis exploits the fact that those born after 1986 are bound by the 1997 schooling law to acquire at least eight years of basic education. Thus, this study uses 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 1997 compulsory schooling reform, for the most part, caused an increase in the educational attainment of girls with a relatively low interest in attending middle school because of a high degree of stigma associated with female schooling in Turkey (Dulger 2004). Although Turkey was classified as a middle-income country during the 1990s, gender inequality in education was exceptionally high, with women lagging behind men in education (Otaran et al. 2003). As this study later discusses in greater detail, for a developing country, this aspect of the 1997 education reform makes it one of the rare cases where one can examine what happens when the government forces girls to obtain extra schooling at the lower tail of the educational attainment distribution.

Child health outcomes analyzed in this paper include very low birth weight (< 1500 grams), low birth weight (< 2500 grams), and high birth weight (> 4500 grams); child mortality (deceased before age five); gestation; and premature birth (gestation < 37 weeks). In addition, maternal outcomes analyzed include the method of birth delivery and propensity to smoke. 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. Lastly, we also find evidence suggesting that additional education, through exposure to the reform, increases the likelihood of a vaginal delivery and lowers maternal smoking.

This research contributes to the maternal education and child health literature in developing countries in a number of ways. First, findings presented in this study provide robust evidence in favor of the view that maternal education improves child health. Second, these results support the argument that mandated education may be instrumental in improving public health in developing countries. Thus, in addition to increasing resources for primary and secondary education in developing societies, enacting compulsory schooling laws, which require a

minimum number of years of completed schooling, and enforcing them to increase educational attainment of females with a low interest in attending school may have significant benefits in terms of children's health. This study also shows evidence that without a sufficiently large sample, the instrumental variable estimates of the impact of the treatment variable (e.g., maternal education) on outcome measures (such as child health at birth and child mortality) may be biased when (i) only a relatively small fraction of those in the treatment group are impacted by the instrumental variable at hand, and (ii) the mean of the dependent variable is fairly small.

The rest of the article is organized as follows. The next section describes the 1997 education reform. Section 2.3 discusses the conceptual framework and reviews the relevant literature. Section 2.4 presents the data and measures. In Section 2.5, the econometric methodology is laid out. Section 2.6 displays the main estimates. Section 2.7 performs robustness checks, explores potential heterogeneity, and implements a two-sample instrumental variable estimation strategy for birth outcomes. Section 2.8 examines the impact of additional education on maternal behaviors. Finally, Section 2.9 concludes.

2.2 Identification: The 1997 Education Reform

Until mid-1990s, the Turkish primary and secondary education system was comprised of three stages: elementary school (5 years), junior high school (3 years), and high school (3 years). Of these, completing the first stage (i.e., 5-year elementary school) was compulsory, and whether a child continues schooling after earning an elementary school diploma was left to the discretion of families. Non-compliance is subject to monetary fines in Turkey even though they are not always strictly imposed, and with the exception of books, supplies, school uniforms, and commuting costs, all public primary and secondary schools are free of charge in Turkey.[2]

In 1997, the Turkish Parliament increased the duration of compulsory schooling from 5 to 8 years. Known as the "eight-year uninterrupted education reform," law number 4306 required children to continue primary schooling until earning a middle school degree, which corresponds to at least 8 years of schooling (Dulger 2004). Chief motivators behind the enactment of the 1997 education reform were the efforts towards endorsing Turkey's accession to the European Union and curbing the rise of Political Islam.[3] The reform became effective in the 1998-

[2] See http://spm.ku.edu.tr/wp-content/uploads/pdf/okulterk.pdf and http://mevzuat.meb.gov.tr/html/24.html for more detail.
[3] See Cakir, Bozan, and Talu (2004) and Cesur and Mocan (2013) for a detailed discussion of the political developments leading to the legislation of the 1997 education reform.

99 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. In other words, those who started elementary school in the 1993-94 academic year were the first cohort to be impacted by the law (Dursun and 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, and Grossman 2014), children who were born in 1987 constitute the first fully affected birth cohort.[4] Note that as parents (and school administrators) can exert a considerable degree of discretion with respect to school starting age, some children can start elementary school a year earlier than their own birth cohort and vice versa (Torun 2015). Therefore, while the majority of children born in 1986 were likely to start elementary school in 1992 and not bound by the reform, some of them possibly delayed it to 1993 and were impacted by the reform. This aspect of the education system in Turkey imposes some ambiguity with respect to whether children born in 1986 were affected by the compulsory schooling reform of 1997. Therefore, following the approach taken by Battistin et al. (2009), Cesur and Mocan (2013), Fort, Schneeweis, and Winter-Ebmer (2016), and Dursun and Cesur (2016) in examining the implications of similar schooling reforms, including the current one, I exclude the 1986 birth cohort from main analysis to get a handle around this issue.[5]

Importantly, the reform mainly focused on increasing educational attainment and made no curricular or compositional changes (Dulger 2004). Therefore, the 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 who were born after 1986. This type of an instrumental variable is consistent with the fuzzy regression discontinuity design (Oreopoulos 2006; Lee and Lemieux 2010; Brunello, Fabbri, and Fort 2013; Clark and Royer 2013).

To supplement the mandate of the reform, which aimed to increase enrollment especially among females, the Ministry of Education made a number of changes, including adding new classrooms to existing schools, hiring new teachers, incorporating a bus system to carry students from rural areas to urban schools, reorganizing the geographic distribution of public schools, and providing free books and lunch to poor children (Turkish Ministry of National Education (MONE) 2001; Dulger 2004). Notably, the reform increased classroom capacity at existing

[4] Resmi Gazete; Friday, 7 August 1992, Section 14.
[5] However, including the 1986 cohort in the analysis sample has no bearing on estimates presented in this study.

schools as opposed to constructing new schools in different localities. As shown in Appendix Table A1, the total number of *Primary Schools*, which were established after the reform by uniting the *Elementary* and *Middle Schools*, is lower than the number of *Elementary School*s prior to the enactment of the 1997 education reform. This is because the classroom capacity of urban public schools was extended at the expense of closed elementary schools in rural areas (Dulger 2004). In addition, in a number of rural areas, schools were consolidated into a single school for clusters of villages (Dulger 2004). To compensate for school closures in non-urban areas, children in villages without *Primary Schools* were transported by bus to the nearest available school; hence, the number of students who were bussed increased significantly shortly after the reform (Erçelebi 2000; Dulger 2004). Lastly, in central locations, the capacity of boarding schools was expanded after the reform to catch up with increased enrollment (Dulger 2004; World Bank 2005). Consequently, as shown in Appendix Figure A1, trends in the percent of women holding at least a middle school degree exhibited a secular jump among women who were bound by the reform.[6]

Predictably, using the "eight-year uninterrupted education reform" as the source of identifying variation, a number of studies have examined the causal impact of extended primary schooling on different outcomes, including earnings, subjective well-being, religiosity, marital status, fertility, health outcomes, gender gap in educational attainment, and domestic violence (Cesur, Dursun, and Mocan 2014; Mocan 2014; Dinçer, Kaushal, and Grossman 2014; Güneş 2015, 2016; Torun 2015; Dursun and Cesur 2016; Erten and Keskin forthcoming).

Secondary school attainment could have increased after the 1997 education reform among two different types of female populations: (i) girls with a fairly low interest in 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 and/or reduction in cost of education.[7] 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 1991-92 to 1997-98), and

[6] As the focus of the current study is examining the impact of maternal education on child health, this study displays the trends in educational attainment of women who gave birth to at least one child. However, using different data sets, a number of studies show that the 1997 compulsory schooling reform caused a significant increase in the lower secondary schooling of both men and women in all parts of the country regardless of urban versus rural residential status or of birth of children (Cesur and Mocan 2013; Mocan 2014; Torun 2015; Dursun and Cesur 2016).

[7] Note that the decision to attend middle school upon completing a five-year elementary school degree is made when the child is 11 years old. Hence, while in some cases the decision surrounding whether to attend middle school may be solely decided by the child, families play a major role in the schooling decision of their children. However, for ease of expression, children's propensity to attend middle school is used interchangeably (and/or jointly) with the interest of their families.

no student could be denied enrollment (Turkish Constitution Article 42, and Ministry of National Education Legislation).[8] As discussed above, the geographical convenience of schools did not increase due to the 1997 reform. On the contrary, schools became more concentrated after the reform and students were transported from rural areas to the nearest schools using the bussing system, and the capacity of boarding schools in central areas was expanded to compensate for school closures and accommodate increased enrollment. Therefore, investments in school capacity did not improve the convenience of attending nearby schools. However, the school bus system and boarding schools could have lowered the cost of commuting to schools for students residing in rural areas.

All in all, the 1997 education reform targeted those with a low propensity to attend middle school, especially among females (Dulger 2004; Mocan 2014). As supported by the available evidence, in this study I argue that even for those living in rural areas, the schooling mandate played a much bigger role in increasing educational attainment. To begin with, the reform had a greater impact on female education compared to that of males (Mocan 2014; Torun 2015; Dursun and Cesur 2016). It is well known that there has been a considerably high degree of stigma attached to girls attending secondary schools in Turkey (TDHS 1998; Aytaç and Rankin 2004; Rankin and Aytaç 2006; Unicef 2007). If there is stigma pertaining to female schooling, it stands to reason that girls are less likely to continue their education upon earning a five-year elementary school diploma in the absence of mandatory schooling; thus, the enactment of a compulsory education law may have a larger impact on female schooling. Therefore, the differential positive effect of the reform on male and female schooling (in favor of women) supports the view that the compulsory nature of the reform played a key role in increasing educational attainment. To gain further insight into the reasons for why females did not pursue secondary schooling prior to the 1997 education reform, this study uses 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 5 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 the ones who were 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 Appendix Table A2, show that a sizable 40 percent of women who dropped out of or did not go to

[8] While I 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.

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 not being able to afford 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, this study concludes that the mandate of the law was the major reason behind the jump in educational attainment due to the 1997 compulsory schooling reform while I acknowledge that one cannot fully dismiss the potential role of a reduction in the cost of attending school. This particular property of the natural experiment at hand has a major implication. That is, the LATE obtained from this policy change primarily corresponds to the impact of extended primary education among individuals with a low tendency to continue their schooling beyond five years of basic education. To the extent of my knowledge, this aspect of the "eight-year uninterrupted education reform" makes it the closest natural experiment that approximates the impact of compulsory schooling among women with a relatively low interest in receiving additional education in a developing country. All other reforms in developing countries that have been utilized in the literature either enable researchers to estimate the impact of extra schooling among those who desire to receive more education (Indonesia 1973-78, China 1977-83, Zimbabwe 1980, Malawi 1994, Uganda 1997) or allow one to study the joint effect of extended schooling among those with a high interest in more education in combination with a compulsory schooling reform (Taiwan 1968). Thus, knowledge gained from this reform may have important policy implications for other developing societies that consider increasing the duration of mandatory schooling.

2.3 Background

A large body of literature documents that educated individuals are found to be healthier.[9] In addition to an individual's own schooling, parental education, particularly that of the mother's, has been shown to be a strong predictor of health outcomes (Currie et al. 2010). 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 and Rosenzweig 2002), and time preferences (Becker and Mulligan 1997). Efficiency in production, induced by extended education, may allow

[9] See Grossman (2006) for a nice overview of this literature.

mothers to have healthier children with a given set of both 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 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, if more educated individuals gain more utility from their children's future wellbeing, they may invest more in their children as well.

Research focused on showing correlations finds a strong positive association between mother's schooling and good child health in both affluent and developing societies (Thomas, Strauss, and Henriques 1991; Grossman 2006). Children of more educated mothers are found to be healthier at birth as maternal schooling is inversely associated with premature births and low birth weight (Currie and Moretti 2003). In addition, those with more educated mothers are less likely to die during childhood (Cutler, Deaton, and Lleras-Muney 2006).

Although descriptive evidence consistently shows that maternal education is favorably associated with improved child health, findings based on studies addressing the endogeneity of maternal schooling are not uniform. In developed countries, quasi-experimental evidence offers mixed results. Using college openings in the United States as an instrument for women's schooling, Currie and Moretti (2003) find that higher maternal education promotes child health at birth as measured by gestational age and birth weight. They also document that increased perinatal care, reduced smoking, increased likelihood of mother being married, and reduced parity due to college education may serve as potential mediators. Exploiting variation in the implementation of a compulsory schooling reform between municipalities and over time, Grytten, Skau, and Sørensen (2014) examine the impact of mother's education on child health in Norway. They conclude that increased maternal schooling reduces the likelihood of low birth weight even in a country with universal healthcare coverage. On the other hand, using the 1947 reform in the United Kingdom as the source of identifying variation, Lindeboom, Llena-Nozal, and van der Klaauw (2009) find little evidence that extended parental education improves child health in terms of birth weight, likelihood of illness at birth, having a chronic condition, having a mental condition, and body mass index. In addition, McCrary and Royer (2011) implement two regression discontinuity designs (RDD), enabled by age-at-school-entry policies in California and Texas, to estimate the effect of women's education on fertility and the health of their children.

Although they find that increased educational attainment of women has a small positive effect on mate quality, their results show little evidence supporting the argument that maternal education improves child health.

Literature investigating the role of parental education in the production of child health by using natural experiments in developing countries has yet to settle as different studies reach different conclusions. 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 in a working paper. They find that both maternal and paternal education have negative effects on child mortality. Using data from Taiwan, Chou et al. (2010) utilize a compulsory school reform joint with a school construction program to examine the impact of parental education on birth outcomes and child mortality. The Taiwanese government increased the duration of mandatory primary schooling from 6 to 9 years in 1968, and a major school construction program has been implemented to improve school availability and accommodate increased enrollment. Prior to the reform, public secondary schools were available only for students who did well in a highly competitive national exam, and attending private schools was only possible for those who could afford expensive private school fees. Consequently, the reform enabled a large number of students to enroll in secondary schools that could not afford it otherwise (Chou et al. 2010). They obtain exogenous variation in parental education by exploiting differential exposure to the school construction program at the county level in combination with the change in the minimum years of compulsory schooling. Using data on all the births that took place between 1978 and 1999 in Taiwan, Chou et al. (2010) show that an increase in years of parental schooling lowers the likelihood of low birth weight and child mortality. Grépin and Bharadwaj (2015) employ Zimbabwe's 1980 education reforms to examine the impact of maternal schooling on child mortality using data from the DHS of Zimbabwe. To increase primary and secondary schooling, the government undertook a number of changes, including constructing new schools and the introduction of free and compulsory primary education for all. Exploiting age specific exposure to Zimbabwe's education reform of 1980 (i.e., being younger than 15 in 1980) as an instrument for years of schooling (as well as having some secondary education), Grépin and Bharadwaj (2015) find that mother's extended schooling causes a decline in child mortality. Lastly, 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. They find that an increase in maternal schooling lowers child mortality.

Even though the aforementioned studies show favorable effects of maternal education on child health in developing nations, a few recent articles, making use of natural experiments, fail to show that maternal education favorably impacts child health. Keats (2014) exploits the elimination of primary school fees beginning 1997 in Uganda to estimate the impact of women's education on child health. Using data from DHS Uganda, this study finds little evidence that maternal education lowers child mortality while it shows that an increase in mother's schooling has a positive effect on offspring's likelihood of receiving immunizations and preventive care. Zhang (2014) uses high school closures as a predictor of female education to examine the effect of maternal schooling on child health in China. She does not find much evidence that mother's high school education reduces the probability of prematurity, low birth weight, neonatal mortality, and infant mortality.

Besides the lack of consensus on how maternal schooling impacts child health, different natural experiments used in the literature are only able to approximate the impact of a specific LATE, corresponding to the impact of education among a particular subsample of the population (Angrist and Krueger 1991; Imbens and Angrist 1994). Distinguishing how different types of education reforms shape the relationship between schooling and health among different groups of individuals is particularly important in developing countries as such knowledge may be critically important in designing educational policies (Kremer 2003; Glewwe and Kremer 2006; Glewwe and Muralidharan 2015). The LATEs stemming from most education reforms, employed as the source of identification in studies using data from developing countries, correspond to the impact of education among people with a relatively high tendency to increase their schooling. That is, as summarized in Appendix Table A3, with the exception of Taiwan's education reform of 1968 studied by Chou et al. (2010), all of the natural experiments employed by the above studies generate LATEs which mainly represent the impact of extending schooling among those who had a high desire to receive more education because the incentives provided by these reforms either reduced school fees and/or increased school availability for individuals who could not get more education mainly due to financial constraints. 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. Consequently, there is a gap in the literature and thus a need for studies identifying the impact of mandatory maternal schooling on child health using data from a developing country.

Furthermore, many of the available studies suffer from limited statistical power because of small sample sizes, as well as a lack of statistically or economically significant relationships between the associated natural experiment and the endogenous schooling variable. For example, although the results of Breierova and Duflo (2004) are intuitive, the first stage estimates of the impact of school construction on years of education do not seem to meet the power requirements for an ideal instrumental variable as shown by the values of the first-stage F-statistics (i.e., these values are usually less than 3 rather than the preferred minimum value of 10). Hence, the authors treat their findings as preliminary. Non-availability of large data sets is a particular limitation in the literature examining the relationship between parental schooling and child wellbeing in developing countries as well. That is, because the mean values of key outcome variables (e.g., low birth weight and child mortality) are usually 5 to 7 percent or lower and the natural experiments at hand only affect a fraction of individuals who are in the treatment group (e.g., typically less than 25 percent), even in a study with 8,000 to 10,000 observations, the results may be driven by a handful of observations.[10] Therefore, estimates based on such small (or even moderately large) sample sizes should be treated with caution as they may be biased.[11] Appendix Table A3, summarizes the existing studies providing causal evidence on the relationship between parental schooling and child health in developing societies. Excluding Chou et al. (2010), most studies rely on either moderate (Zhang 2014; Grépin and Bharadwaj 2015) or considerably small (Güneş 2015; Keats 2014) sample sizes, or a weak first stage relationship between the associated natural experiments and formal education (Breierova and Duflo, 2004).

Note that 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, expressed in terms of number of teachers at the province level, in combination with the 1997 education reform, which increased the duration of mandatory basic education from 5

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[10] For instance, let us assume a case where the sample has 8,000 observations with an equal number of individuals in the treatment and control groups, the mean of the binary dependent variable is 5 percent, and the associated compulsory schooling reform affected 20 percent of the individuals in the treatment group. Therefore, out of 4000 people in the treatment group, there are 800 (=4000*0.20) individuals who are impacted by the schooling reform, and of those there are 40 (=800*0.05) individuals for whom the binary dependent variable equals one. Note that these figures get much smaller as the number of observations in the treatment group gets smaller. Consequently, a handful of observations (usually 2 or 3 out of 40) may dictate results that may be biased.

[11] Because Chou et al. (2010) use aggregate data at the county by year of birth level, this limitation does not apply to them given that their analysis relies on information gathered from a large number of observations (i.e., the full population).

to 8 years, as the source of exogenous variation in schooling.[12] Güneş (2015) explores the impact of maternal education on child health outcomes and maternal healthcare utilization using data from the TDHS 2008. She finds that maternal education has a favorable impact on child health (in terms of height-for-age and weight-for-age z-scores and very low birth weight) and mother's likelihood of utilizing healthcare services that may enhance child health. She also estimates the impact of maternal schooling on the likelihood that a child is born with a birth weight below 1500 grams and finds that mother's schooling has a negative effect, which is significant at the 10 percent level.

Even though a data set with a relatively small number of observations may be suitable to obtain population estimates as well as to study the impact of women's education on outcomes with considerably large mean values, the TDHS offers a fairly small sample to study the impact of maternal education on variables that have low average values, such as child mortality and low birth weight. Indeed, in the robustness section of this paper formally tests the importance of using a sufficiently large sample to estimate the causal effect of parental education on child health outcomes that have small mean values when employing an instrumental variables estimation strategy. This exercise, which is performed in Section VI, documents that, in a small sample (e.g., data from DHS), the instrumental variable estimates of the effect of maternal education on birth outcomes and child mortality may produce biased results. I must stress that this research does not invalidate the contributions of previous studies. To the contrary, this study attempts to address the challenges faced by the broad literature that employs data from developing nations to estimate the causal impact of maternal schooling on child health. Therefore, motivated to fill the gaps in the literature, this study examines the causal effect of maternal education on birth outcomes and child mortality by utilizing two large data sets from Turkey that grant us sufficient statistical power.[13]

[12] I prefer to employ a fuzzy-RDD type empirical estimation strategy as I believe that it is more appropriate for a number of reasons. First, as illustrated in Figure 2.1, the implementation of the education reform was completed quickly (i.e., within two years) after the enactment. This identification strategy also relies on much weaker assumptions in comparison to a difference-in-differences estimation strategy. In addition, this study argues in Section 2.2 that the primary driver of the jump in educational attainment was not the increased school availability, but the mandate of the reform. Therefore, as preferred by a number of other researchers, including Erten and Keskin (forthcoming), Cesur and Mocan (2013), Mocan (2014), Torun (2015), and Dursun and Cesur (2016), I consider that an instrumental variables estimation method enabled by a fuzzy-RDD framework at hand is the most appropriate empirical strategy to study the implications of extended schooling due to the 1997 education reform. Lastly, the estimates are robust to accounting for potential regional and province level differences in the implementation of the reform (in addition to addressing potential convergence in child health at the province level) by controlling for province specific birth cohort trends.

[13] Several studies also examine the causal impact of women's education on fertility using similar identification strategies in different countries (Osili and Long 2008; Andalón, Williams, and Grossman 2014; Geruso and Royer 2014; Lavy and Zablotsky 2015; Fort, Schneeweis and Winter-Ebmer 2016; Güneş 2016). As the focus of this study

2.4 Data

This study uses two data sets in analysis: Birth outcomes data from the Turkish Ministry of Health, and child mortality data from the Turkish Statistical Institute's Population and Housing Census 2011.

Information on birth outcomes pertaining to birth weight, gestational age, method of birth delivery, and maternal smoking come from the Ministry of Health. Historically, birth records have not been systematically recorded in Turkey.[14] With the aim of gaining accurate information on women's and children's health, in 2008 the Ministry of Health initiated a collection of data on birth and maternal outcomes through the Family Medicine Program (FMP), which provides universal primary care to all the citizens at Family Health Centers (FHC) regardless of their financial wellbeing.[15] The Ministry of Health provided the MHBOD for my use at the child level.[16] The data also provide information on maternal educational attainment categories of each child.[17] These categories include: no education, elementary school, middle school, high school, college, and graduate degree.[18] Because the 1997 education reform (i.e., the instrumental variable I employ in the analysis) obliged individuals to earn at least a middle school degree (i.e., at least 8 years of schooling), the variable *Middle School* coded 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, this study restricts the estimation sample to children whose mothers were born 5 years before and after 1986.[19]

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is on the relationship between maternal schooling and child health, it also abstains from delving into the literature examining the impact of women's education on fertility.

[14] Consequently, there are relatively few articles exploring the determinants of child health, including birth outcomes, in Turkey. More importantly, existing studies rely on small samples, which result in limited statistical power. Indeed, the Turkish Demographic and Health Survey (TDHS), conducted every 5th year since 1988 with a relatively small sample size, has been the major data source for both academic studies and population estimates for a number of health indicators, including fertility, birth outcomes, and child mortality (Celik and Hotchkiss 2000; Hizel and Coskun 2000; Erci 2003; Ergin et al. 2010; Dinçer, Kaushal, and Grossman 2014; Güneş 2015, 2016).

[15] See Cesur et al. (2017) for a detailed description of the FMP.

[16] Unfortunately, the MHBOD do not include information on maternal birth history, such as birth order.

[17] While it would have been informative for my purposes, data on paternal education are not available.

[18] 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 and Cesur 2016). Because the instrumental variable, the 1997 education reform, specifically mandates earning at least a middle school diploma, this is not an issue particularly to the extent that sheepskin effects, where degrees obtained are more relevant to returns to education than actual years of education (Hungerford and Solon 1987), apply to nonpecuniary benefits.

[19] I also test the robustness of findings to alternate bandwidth selections (i.e., from ± 6, ± 4, and ± 3 years). As shown in Table 2.6, this exercise did not cause any appreciable change in main findings.

Using the available information provided by the MHBOD, I created measures of child health at birth based on birth weight in grams and gestational age. Following the existing literature (Currie and Moretti 2003), I construct two binary low birth weight indicators *Low Birthweight* and *Very Low Birthweight*, besides using natural log of birth weight, *Log of Birthweight*, as a measure of child health at birth. *Low Birthweight* is set equal to one for children who weigh less than 2500 grams at birth, and it is set equal to zero otherwise. *Very Low Birthweight* is constructed analogously using 1500 grams as the cut-off 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; Zhang et al. 2008; Cnattingius et al. 2012; Sparano et al. 2013), type 2 diabetes (Wei et al. 2003; Whincup et al. 2008), cardiovascular and metabolic complications (Walsh and McAuliffe 2012), and reduced cognitive outcomes (Cesur and Kelly 2010). Similarly, maternal diabetes, glucose intolerance, maternal overweight prior to pregnancy, gestational weight gain, gestational age, previous history of macrosomic birth, maternal age (>30) (Sparano et al. 2013; Walsh and McAuliffe 2012; Koyanagi et al. 2013; Retnakaran et al. 2012), and gender of the child (Walsh and McAuliffe 2012) are listed as determinants of high birth weight. In order to investigate the effect of maternal education on high birth weight we generated a *High Birthweight* variable, equal to one if the infant weighs more than 4500 grams, and zero otherwise.

Gestational age in weeks is calculated by taking the difference between the date of birth and the date of last menstruation. Using the standard definition from the World Health Organization, *Premature* is defined as being equal to one if a child's gestational age is less than 37 weeks, and it is coded as zero otherwise.[20] We also employ 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 are limited by the reach of the FMP. Note that 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 prior to 2011 even though

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

the data collection effort was initiated in 2008. Second, although the FMP aims to cover everyone in the country regardless of income and socio-economic status, not all citizens will choose to receive the services offered by the FMP. Lastly, MHBOD are collected by healthcare professionals employed 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 on data collection by the Ministry of Health in addition to staffing shortages at FHCs, which hinder data collection efforts. Nevertheless, despite the aforementioned limitations, the MHBOD is ideal for my purposes and provides us with a large sample to analyze the impact of maternal education on child health. To begin with, depending on the birth outcome, the data provide information on about 82 percent of all the births that took place since 2011, the year at which the FMP was in full effect in all provinces. Furthermore, we also formally test whether the data availability for birth weight, method of birth delivery, 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 health and maternal outcome defined above. If being bound by the compulsory schooling reform impacts the likelihood of reporting information on birth and maternal outcomes, the instrumental variable estimates may be biased. Therefore, this test is particularly important for my purposes. Results presented in Appendix 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. All in all, these exercises increase my confidence in the suitability of the MHBOD for examining the impact of maternal education on child outcomes.

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 on a regular basis.[21] 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 on an annual basis. While the

[21] 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.

ABPC provides detailed information on these measures, it does not collect some internationally recommended and nationally needed data on a number of social, economic, demographic, and housing characteristics of the population.

Therefore, the TurkStat decided to implement a *mini* census in order 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. As we noted previously, we limit the analysis sample and focus on women who were born between 1981 and 1991.

Like the MHBOD, instead of years of completed schooling, the PHC provides us with educational attainment categories, which include no education, elementary school, middle school, high school, college and graduate school. Thus, *Middle School*, which is the key independent variable in the PHC data as well, is coded as one for women who earned at least a middle school degree, and zero otherwise.

Any Child Died takes the value equal to 1 if the respondent mother had at least one child deceased before the age of five, and it is equated to 0 if she had no children passed away before the age 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 mortality rate of infants was 11.7 in a 1000, the mortality rate among ages 1 to 5 was less than 1 in a 1000. 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.

2.5 Econometric Methodology

Using the MHBOD at the child level, equation (2.1) estimates the relationship between maternal schooling and birth outcomes:

$$BO_i = \gamma_0 + \gamma_1 (Middle\ School)_i + \boldsymbol{X_i'\gamma} + u_i \tag{2.1}$$

where *BO* is child i's associated birth outcome, including birth weight, gestational age, and premature birth status. Using the MHBOD, I also estimate the impact of women's education on method of 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, which are province fixed effects, mother's month of birth indicators, child's birth year fixed effects, and linear approximations of mother's birth year-month on each side of the pivotal birth cohort of 1986, thus controlling for maternal age at the birth of the child. 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. Maternal month-of-birth fixed effects account for seasonality in mother's birth (Torun and Tumen 2015).

Equation (2.2), uses data at the mother level from the PHC to examine the relationship between mother's education and child mortality:

$$CM_m = \beta_0 + \beta_1 (Middle\ School)_m + \boldsymbol{X_m'\theta} + \varepsilon_m \qquad (2.2)$$

where *CM* represents whether mother m had at least one child died by the age of 5. The remaining covariates of equation (2.2) are constructed analogously to equation (2.1).

Estimating the parameters of equations (2.1) and (2.2) 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 advantages leading to biased estimates of the effect of maternal education on child outcomes.

In order to estimate the causal impact of maternal education on child outcomes, this study makes use of a two-stage least squares estimation strategy. In doing so, I instrument mother's educational attainment on the 1997 education reform, which is used as the source of exogenous variation in maternal education. Equations (2.3) and (2.4) 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:

$$Middle\ School_i = \alpha_0 + \alpha_1 Reform_i + \boldsymbol{X_i'\alpha} + \emptyset_i \quad (2.3)$$

$$Middle\ School_m = \pi_0 + \pi_1 Reform_m + \boldsymbol{X_m'}¥ + \Omega_i \quad (2.4)$$

where the outcome is *Middle School*. $Reform$ is a dichotomous variable which 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 (2.1) and (2.2). The second stage uses *Reform* as an instrument for *Middle School* and estimate 2SLS models to approximate the causal impact of maternal middle school education on child health.

The 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 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 and Lemieux, 2010). Therefore, this study limits the estimation sample to individuals who were born 5 years before and after 1986. Nevertheless, to ensure the robustness of findings to using different data intervals, I also report results using bandwidths of ∓6, ∓4, and ∓3 years around the cutoff birth cohort. As pointed out earlier, because the treatment status of the 1986 cohort is ambiguous, the 1986 cohort dropped from main analysis.[22] In all specifications, standard errors clustered at the mother's birth year by province level, leading to 810 (=81*10) cluster units. Birth outcome models are weighted using province level number of women in the age of fertility (i.e., ages 15 to 49). The weights provided by the PHC used in child mortality specifications.

2.6. Results

Table 2.1 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 the 1987 to 1991 birth cohorts form the treatment group. Panel A in Table 2.1 highlights the

[22] Eliminating the 1986 birth cohort also accounts for the potential spillover effects and the possible lag between the timing of legislation and implementation as well. Moreover, according to data from the World Bank, approximately 5% 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 2.5, inclusion of the 1986 birth cohort with alternative treatment values (such as Reform=0.25) does not alter main findings.

difference in education levels between the treatment and control groups in birth weight data; while 57% of mothers obtained at least a middle school diploma in the treatment group, this percentage is 49.5% in the control group. Mean birth weight is 3,246 grams. *Very Low Birthweight* and *High Birthweight* categories make up 1% of birth weight sample, while six percent of women gave birth to low birth weight babies (i.e., under 2500 grams). As expected, the prevalence of very low birth weight, low birth weight, and high birth weight babies are all lower in the treatment group compared to the control group. The average gestational age is around 38 weeks; as expected, there were fewer premature births in the treatment group (11%) compared to the control group (12%).

As displayed in Panel B of Table 2.1, we observe a jump in the proportion of women who hold at least a middle school degree in the treatment group compared to the control group for the PHC data as well. Panel B of Table 2.1 also shows that 1.9% of women in the treatment group, and 2.9% of the women in the control group, declare that at least one of their children died before the age of five.

Table 2.2 shows the associations between child health and 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 0.50 percent higher birth weight, and 0.48 and 0.30 percentage-point less likely to weigh below 2500 and above 4500 grams, respectively. Table 2.2 also shows that maternal education is associated with a 0.24 percent decrease in the length of gestation and 0.95 percentage point decrease in the likelihood of premature births. Results does not indicate a statistically significant relationship between parental education and head circumference at birth. Panel B of Table 2.2 indicates that children of mothers who hold at least a middle diploma are 1.9 percentage points less likely to have died by age 5. While the OLS estimates are helpful in observing 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 observable and unobservable common determinants of maternal schooling and child outcomes, including income, genetic makeup of the mother, and access to medical care, may bias the estimates of the coefficient on schooling. To get a handle around the potential endogeneity of maternal education, I next employ an instrumental variable (IV) estimation strategy, in which *Middle School* is regressed on the 1997 education reform. As noted above, the 1997 education reform obliged individuals born after 1986 to obtain at least a middle school degree. Figure 2.1 provides visual illustrations of the impact of *Reform* on

Table 2.1 Descriptive Statistics

	All	Control	Treatment
Panel A: Birth Outcomes			
Middle School	0.5315	0.4951	0.5719
	(0.499)	(0.500)	(0.4948)
Birth Weight	3245.947	3260.277	3229.235
	(502.2491)	(511.5284)	(490.6807)
Very Low Birthweight	0.0055	0.0061	0.0049
	(0.0742)	(0.0778)	(0.0698)
Low Birthweight	0.0601	0.0607	0.0595
	(0.2378)	(0.2388)	(0.2366)
High Birthweight	0.0047	0.0058	0.0034
	(0.0686)	(0.0761)	(0.0585)
Age	26.8181	29.4389	23.7617
	(3.2559)	(1.6383)	(1.5753)
	[186,840]	[96,874]	[89,966]
Gestational Age	38.4173	38.3058	38.541
	(2.1752)	(2.1361)	(2.2111)
Preterm<37 weeks	0.1169	0.1212	0.1121
	(0.3213)	(0.3264)	(0.3155)
	[1,486,353]	[756,929]	[729,424]
Head Circumference	34.5653	34.5975	34.5278
	(1.732)	(1.749)	(1.7113)
	[186,505]	[96,689]	[89,816]
Panel B: Child Mortality			
Middle School	0.6215	0.5438	0.8026
	(0.485)	(0.4981)	(0.3981)
Any Child Died	0.026	0.0291	0.0188
	(0.1591)	(0.1681)	(0.1359)
	[340,091]	[237,964]	[102,128]

The means in Panel A are generated using data from the Ministry of Health Birth Outcomes Data (MHBOD), and in Panel B, we used Turkish Statistical Institute's Population and Housing Census 2011 (PHC). Individuals born between 1987 and 1991 constitute the treatment group and those who were born between 1981 and 1985 form the control group. Standard deviations are in parentheses. Variables in Panel A 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 Panel B. Number of observations are in square brackets [].

Middle School by the estimation sample.[23] Accordingly, one can observe an unambiguous jump in the proportion of mothers with at least a middle school diploma among those who were bound by the compulsory schooling reform, regardless of the estimation sample. Appendix Figures A2 and A3 provide additional evidence on validating

[23] More specifically, we visualize the impact of being exposed to reform on completing at least a middle school degree by netting out the influence of pre-determined variables and plotting the residuals for pre-and post-reform cohorts based on Equations (2.3) and (2.4) (Lee and Lemiux 2010; Brunello, Fabbri, and Fort 2013).

Table 2.2
OLS Estimates of Child Health and Maternal Health Outcomes on Maternal Education

Panel A: Birth Outcomes	
Dependent Variables	
Log Birthweight	0.0050***
	(0.0016)
Very Low Birthweight	-0.0004
	(0.0007)
Low Birthweight	-0.0048**
	(0.0021)
High Birthweight	-0.003*
	(0.0007)
Observations	[186,840]
Log Gestational Age	-0.0024***
	(0.0002)
Preterm<37 weeks	-0.0095***
	(0.001)
Observations	[1,486,353]
Log Head Circumference	-0.0006
	(0.0004)
Observations	[186,505]
Panel B: Child Mortality	
Any Child Died	-0.019***
	(0.001)
Observations	[340,091]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Number of observations are in square brackets [].

empirical design as suggested by Lee and Lemieux (2010). First, as a standard procedure of empirical design we use, I show that some of the potential predetermined variables are balanced across the 1986 cutoff. While the data used in this study do not provide us with a rich set of predetermined variables, I am able to employ disability status and cohort size variables, which are not expected to be impacted by exposure to the reform, to descriptively test the "balanced covariates" assumption. Evidently, as Appendix Figure A2 shows, there is no significant jump across the 1986 cutoff. Second, Appendix Figure A3 presents the visual representation of the McCrary test, which provides

evidence that the non-random sorting of the running variable, month of birth, across the 1986 threshold is rejected and is continuous around the cutoff.[24]

The strength of instrumental variable is formally tested in Table 2.3, where I analyze the effect of exposure to the education reform on the likelihood of earning a middle school diploma at the minimum. Results suggest that the reform increased middle 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 one percent level. While these first stage effect sizes are in the range of other studies examining the impact of the 1997 education reform on a number of outcomes (Cesur and Mocan 2013; Torun 2015; Dursun and Cesur 2016), the impact of the reform on educational attainment is greater in the PHC sample in comparison to the MHBOD sample. This point revisited and addressed in the robustness section below by performing a two sample instrumental variables estimation strategy.

Baseline IV estimates of the impact of maternal educational attainment on birth outcomes, and child mortality are presented in Table 2.4. The first stage F statistics shown here are well above the recommended value of 10, revealing the strength of IV.

In Panel A, *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 4.7, and 7.4 percentage points, respectively. Results also show that mothers who hold at least a middle school diploma are 2.7 percentage points less likely to have a baby with a high birth weight (> 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 Birthweight* is positive but small (3.7 percent) and marginally statistically significant. Next, I show that maternal education increases the duration of gestation by 2.3 percent, and it lowers the likelihood of premature births by 5 percentage points. Note that the IV estimate switches the sign on log gestation age, which suggests that the observed negative relationship in OLS specifications may be due to selection bias.[25] Mother's middle school education also increases child's head circumference at birth by (a statistically insignificant) 1.3 percent. When these results jointly evaluated, I reach the conclusion that compulsory maternal schooling has a causal favorable effect on child health at birth.

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[24] The p-values of the estimated jumps are 0.312 and 0.202 for the birth weight sample and the mortality sample, respectively.

[25] For example, the negative sign on OLS estimates presented in Table 2.2 may be due to C-section birth deliveries being performed around 38-39 weeks of pregnancy.

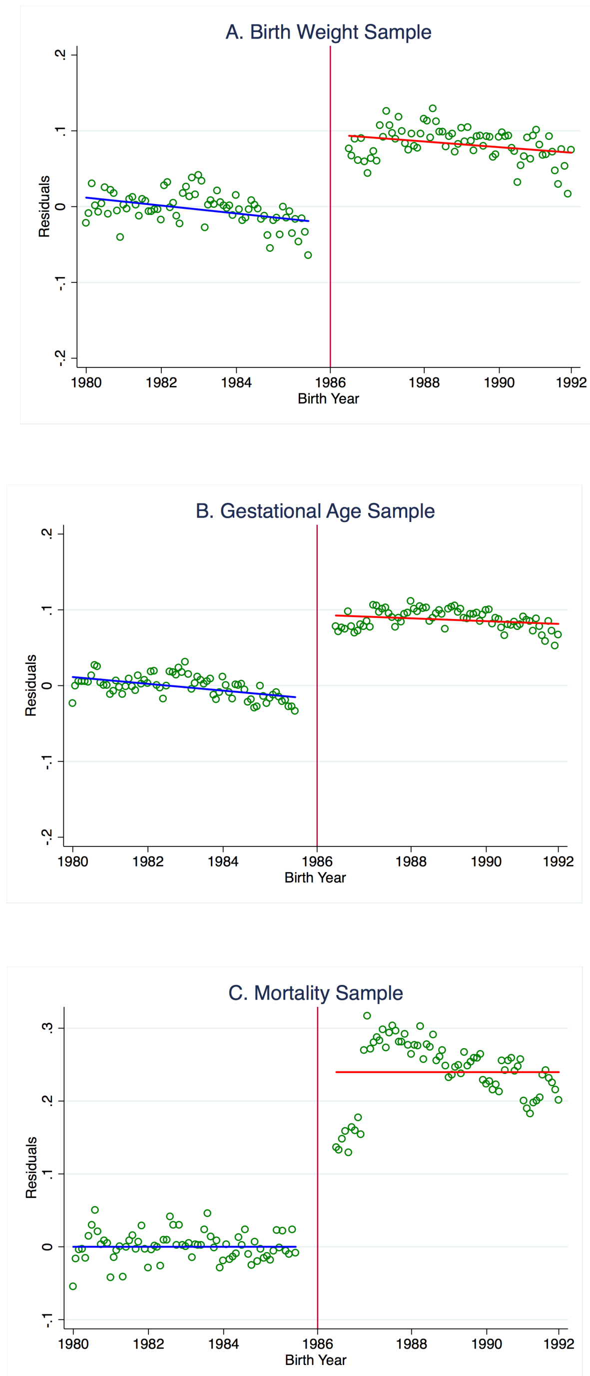

Figure 2.1: The Impact of the 1997 Reform on Educational Attainment

Table 2.3
The Impact of the 1997 Education Reform on Obtaining at Least a Middle School Degree

Panel A: Birth Outcomes	
Birth Weight Sample	
Education Reform	0.0757***
	(0.0099)
	[186,840]
Gestational Age Sample	
Education Reform	0.0834***
	(0.0097)
	[1,486,353]
Panel B: Child Mortality	
Education Reform	0.228***
	(0.014)
	[340,091]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Number of observations are in square brackets [].

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 2.4 (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 5.

2.7 Robustness, Heterogeneity, and Two Sample IV Estimates

This section performs several robustness checks to test the sensitivity of baseline estimates. As a first robustness exercise column (2) of Table 2.5 presents the reduced form estimates of the impact of maternal exposure to the 1997 education reform on child outcomes.[26] These results are fully in line with the 2SLS estimates. Furthermore, the reduced form estimates are more precisely estimated and statistically significant for each specification.[27] As there is some uncertainty with respect to whether those born in 1986 were bound by the compulsory schooling reform, 1986 birth cohort excluded from benchmark analysis. In column (3), I assigned an

[26] To simplify comparison, we also present the baseline IV estimates in the first column of Table 2.5.
[27] The IV estimates can be obtained by dividing these intent-to-treat estimates of the impact of the education reform by the associated first stage estimate.

Table 2.4
Instrumental Variable Estimates of the Impact of Maternal Education on Child Health: Baseline Results

Panel A: Birth Outcomes	
Dependent Variables	
Log Birthweight	0.037*
	(0.022)
Very Low Birthweight	-0.047***
	(0.012)
Low Birthweight	-0.074**
	(0.031)
High Birthweight	-0.027**
	(0.011)
1st Stage F-test	{36.835}
	[186,840]
Log Gestational Age	0.023***
	(0.004)
Preterm<37 weeks	-0.050***
	(0.014)
1st Stage F-test	{74.464}
	[1,486,353]
Log Head Circumference	0.013
	(0.008)
1st Stage F-test	{37.341}
	[186,505]
Panel B: Child Mortality	
Any Child Died	-0.018**
	(0.007)
1st Stage F-test	{232.883}
	[340,091]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

alternate treatment value to (i.e., Reform=0.25) with respect to exposure to the 1997 education reform for those who were born in 1986. [28] These estimates are very similar to baseline findings. In main specifications, my analysis accounted for birth cohort trends by controlling for maternal birth month-year differentiated by exposure to the education reform status. This section checks if the baseline estimates change when maternal birth month-year quadratically specified. As seen in column (4) of Table 2.5, these results are nearly identical to main findings.

Given that the empirical methodology followed in this study is consistent with a fuzzy regression discontinuity design (Lee and Lemieux 2010), I next examine if the results are robust to undertaking the formal RD estimation procedure using local linear regressions. In doing so, I estimate the models with the procedure described by Calonico, Cattaneo, and Titiunik (2014) and Calonico et al. (2016a,b).[29] As presented in column (5) of Table 2.5, the linear RD IV estimates are qualitatively and quantitatively similar to the baseline findings.

Next exercise tests the robustness of the baseline estimates to account for the potential regional heterogeneity in how extended maternal schooling due to the reform impacted child health. To do so, I interact binary province indicators with women's birth month-year and specify them in the regressions. Controlling for province specific birth cohort trends also accounts for any bias that may stem from convergence in child outcomes between provinces (and regions) over the years. Because developing countries have exhibited relatively fast improvements in health outcomes in the past few decades, one may question whether the findings are driven by potential convergence in child outcomes between different regions. Thus, ruling out the role of convergence in this framework is particularly informative. Results presented in column (6) show that when province specific trends controlled, the estimated effect of extended primary schooling is estimated with greater precision. These estimates show that the findings are not driven by convergence in child outcomes by maternal birth year between regions. Instead, this exercise suggests that failing to control for province specific trends may lead to imprecise estimates of the impact of maternal education on child outcomes. An important issue in statistical inference for regression analysis is the possibility that standard errors are correlated within clusters. In such cases, standard errors may be estimated with bias, which in turn may lead to false conclusions (Cameron, Gelbach, and Miller 2015). As

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[28] Assigning alternate treatment values for those born in 1986 (e.g., Reform=0, and Reform=0.33) produces very similar results.

[29] Moreover, instead of five-year static bandwidths, we estimated models with optimal bandwidths obtained by the default MSE-optimal bandwidth selector introduced by Calonico et al. (2014, 2016a,b). The estimates we obtained from this procedure are completely in line with benchmark findings, and are available upon request from the authors.

Table 2.5
The Impact of Maternal Education on Child Health, Robustness Checks

	(1) Baseline Estimates	(2) Intent to Treat Estimates	(3) Alt. Treatment for 1986 Birth Cohort T==0.25	(4) Quadratic BC Control Function	(5) Local Linear IV with rdrobust command	(6) With Province Specific Birth Cohort Trends
Panel A: Birth Outcomes						
Dependent Variables						
Log Birthweight	0.037*	0.00287**	0.046**	0.038	0.033**	0.038**
	(0.022)	(0.00123)	(0.023)	(0.026	(0.015	(0.019)
Very Low Birthweight	-0.047***	-0.00355***	-0.039***	-0.046***	-0.041***	-0.047***
	(0.012)	(0.00056)	(0.011)	(0.011)	(0.01)	(0.011)
Low Birthweight	-0.074**	-0.00563***	-0.077**	-0.073**	-0.06***	-0.074**
	(0.031)	(0.00171)	(0.03)	(0.033)	(0.023)	(0.029)
High Birthweight	-0.027**	-0.00200***	-0.027***	-0.026**	-0.026***	-0.026***
	(0.011)	(0.00073)	(0.01)	(0.011)	(0.01)	(0.01)
1st Stage F-test	{36.835}	---	{43.903}	{36.538}		{58.66}
	[186,840]	[186,840]	[208,424]	[186,840]	[186,840]	[186,840]
Log Gestational Age	0.023***	0.00190***	0.021***	0.023***	0.020***	0.023***
	(0.004)	(0.0002)	(0.004)	(0.004)	(0.004)	(0.003)
Preterm<37 weeks	-0.050***	-0.00414***	-0.057***	-0.052***	-0.037***	-0.050***
	(0.014)	(0.00116)	(0.015)	(0.016)	(0.013)	(0.014)
1st Stage F-test	{74.464}	---	{77.535}	{72.681}		{147.562}
	[1,486,353]	[1,486,353]	[1,654,931]	[1,486,353]	[1,486,353]	[1,486,353]

Table 2.5 (Continued)

	(1) Baseline Estimates	(2) Intent to Treat Estimates	(3) Alt. Treatment for 1986 Birth Cohort T==0.25	(4) Quadratic BC Control Function	(5) Local Linear IV with rdrobust command	(6) With Province Specific Birth Cohort Trends
Panel A: Birth Outcomes						
Dependent Variables						
Log Head Circumference	0.013	0.00100*	0.016*	0.013	0.011	0.013*
	(0.008)	(0.00053)	(0.009)	(0.009)	(0.008)	(0.008)
1st Stage F-test	{37.341}	---	{44.374}	{37.078}		{59.373}
	[186,505]	[186,505]	[208,043]	[186,505]	[186,505]	[186,505]
Panel B: Child Mortality						
Any Child Died	-0.018**	-0.004**	-0.018***	-0.019***	-0.017**	-0.018***
	(0.007)	(0.001)	(0.007)	(0.006)	(0.008)	(0.006)
1st Stage F-test	{232.883}	---	{218.5}	{232.198}		{374.9}
	[340,091]	[340,091]	[375,433]	[340,091]	[340,091]	[340,091]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

previously discussed, school starting age in Turkey is primarily determined by a child's birth year. In addition, the mandate of the reform may have differential effects on educational attainments of individuals residing in different parts of the country. Therefore, in line with Agüero and Bharadwaj (2014), Cesur and Mocan (2013), and Grépin and Bharadwaj (2015), I adjust the standard errors at the mother's birth year by province level. Next, I explore if the statistical inference in the main specifications are robust to adjusting the standard errors at other cluster units. These estimates are presented in Appendix Table A5. Specifically, I cluster the standard errors on the province (in column 1), birth year-month (column 2), birth year-month by province (column 3), birth year by region (column 4), and birth year (column 5), respectively. While a large number of cluster units in columns (1) to (5) employed, standard errors are adjusted for 10 cluster units in column (5). To address the potential bias that may be induced by employing few cluster units when adjusting the standard errors on the year of birth, I perform a wild-cluster bootstrapping method as suggested by Cameron, Gelbach, and Miller (2008). As shown in column (6), this exercise has no bearing on the main conclusions drawn in this study.[30]

In this study, the main analysis conducted by using the sample of individuals born 5 years before and after 1986, which is the pivotal birth cohort. While employing a wider estimation-window boosts statistical power via increasing the sample size, the possibility that the identifying variation comes from individuals who are further away from the discontinuity may threaten the validity of the estimates. Therefore, Table 2.6 assesses whether the preferred estimates (which include province specific birth cohort trends, as shown in column 6 of Table 2.5) hold when the data estimation window changed. In particular, I estimate the baseline specifications using the sample of individuals born within 6±, 5±, 4±, and 3± years of the pivotal birth cohort of 1986 in columns (1) to (4), respectively. Results displayed in Table 2.6 document that findings are robust to the alternative bandwidth selection.

The next robustness exercise presents the graphical illustrations of the effect of the 1997 education reform on outcomes of interest. Such visual summaries are particularly important when the identifying variation in the endogenous variable is obtained from an exogenous discontinuity (Imbens and Lemieux 2008; Lee and Lemieux 2010; Clark and Royer 2013).

[30] We conducted all the empirical analysis of the PHC data in October 2015 in the Data Analysis Center of the TurkStat in Ankara, Turkey. Therefore, in Panel B of Appendix Table A5, I am only able to report standard errors clustered on the province, and birth year-month levels in columns (1) and (2). Nevertheless, the pattern of results presented in Panel A of Appendix Table A5 strongly suggest that the statistical inference for the PHC specifications should be robust to clustering at other levels.

Table 2.6
Robustness of the IV Estimates of the Impact of Maternal Education on Child Health to Using Alternative Bandwidths

	(1)	(2)	(3)	(4)
	80-92	81-91	82-90	83-89
Panel A: Birth Outcomes				
Dependent Variables				
Log Birthweight	0.028*	0.038**	0.030*	0.037**
	(0.017)	(0.019)	(0.017)	(0.016)
Very Low Birthweight	-0.030***	-0.047***	-0.044***	-0.045***
	(0.011)	(0.011)	(0.011)	(0.010)
Low Birthweight	-0.055**	-0.074**	-0.065**	-0.050**
	(0.025)	(0.029)	(0.026)	(0.023)
High Birthweight	-0.027***	-0.026***	-0.030***	-0.019
	(0.009)	(0.010)	(0.010)	(0.012)
1st Stage F-test	{66.797}	{58.660}	{82.271}	{176.673}
	[214,185]	[186,840]	[154,533]	[119,450]
Log Gestational Age	0.021***	0.023***	0.020***	0.018***
	(0.003)	(0.003)	(0.003)	(0.004)
Preterm< 37 weeks	-0.036***	-0.050***	-0.027***	-0.042***
	(0.012)	(0.014)	(0.010)	(0.014)
1st Stage F-test	{155.373}	{147.562}	{203.758}	{279.901}
	[1,710,454]	[1,486,353]	[1,227,026]	[947,857]
Log Head Circumference	0.012*	0.013*	0.008	0.014*
	(0.007)	(0.008)	(0.008)	(0.008)
1st Stage F-test	{67.835}	{59.373}	{82.894}	{177.573}
	[213,807]	[186,505]	[154,257]	[119,231]
Panel B: Child Mortality				
Any Child Died	-0.015**	-0.018***	-0.018**	-0.017*
	(0.006)	(0.006)	(0.007)	(0.009)
1st Stage F-test	{407.0}	{374.9}	{354.0}	{342.5}
	[406,046]	[340,091]	[269,958]	[204,549]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects and province specific maternal birth month-year trends. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Column headings pertains to data estimation intervals based on year of birth year of the mother. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Figures 2.2A-2.2K display the reduced form graphs of the effect of the 1997 compulsory school reform on child health at birth and child mortality. Consistent with the results shown in Table 2.6, these graphs support the argument that rather than a sporadic jump in the outcome variables, the IV estimates are driven by systematic shifts in child health at birth and child mortality due to mother's extended primary schooling.

Note that most natural experiments employed in the literature usually influence the schooling behavior of about 25 percent or a smaller fraction of the individuals in the treatment group, and the mean values for child mortality and low birth weight are usually about 0.10 or lower. Therefore, as argued above that the IV estimates of the impact of maternal education on child health indicators with low mean values (e.g., low birth weight and child mortality) may be biased without a sufficiently large sample.

To test the validity of this assertion, I use data from the three available waves of the Turkish Demographic and Health Survey (TDHS) collected in 2003, 2008, and 2013 and re-estimate the most comprehensive specifications (shown in Table 2.6). To do so, I constructed indicators for birth outcomes, which are child mortality, and maternal smoking using the TDHS data in an identical fashion with the variables used in main analysis. Appendix Table A6 employs the sample of mothers born within 6±, 5±, 4±, and 3± years of the pivotal birth cohort in columns (1) through (4). In the TDHS sample, the estimates of birth outcomes, maternal smoking, and child mortality are conducted with 3,365; 6,564; and 4,692 observations in the largest data interval (column 1) for birth outcomes, maternal smoking and child mortality samples, respectively. Of these observations, 23 to 33 percent are in the treatment group, and about 67 to 77 percent constitute the control group. As displayed in columns (1) to (4) of Appendix Table A6, models using the TDHS are imprecisely estimated in all but one of the cases.

More importantly, coefficients have the "wrong sign" for three of four birth weight indicators (i.e., *Log Birthweight*, *Very Low Birthweight*, and *Low Birthweight*) in most cases, and estimates of *Very Low Birthweight* and *Any Child Died* on *Middle School* change signs as I change the data estimation interval. To gain additional statistical power, in column (5) of Appendix Table A6, I estimated the specifications in the TDHS using the sample of individuals born 10 years before and after the pivotal birth cohort.

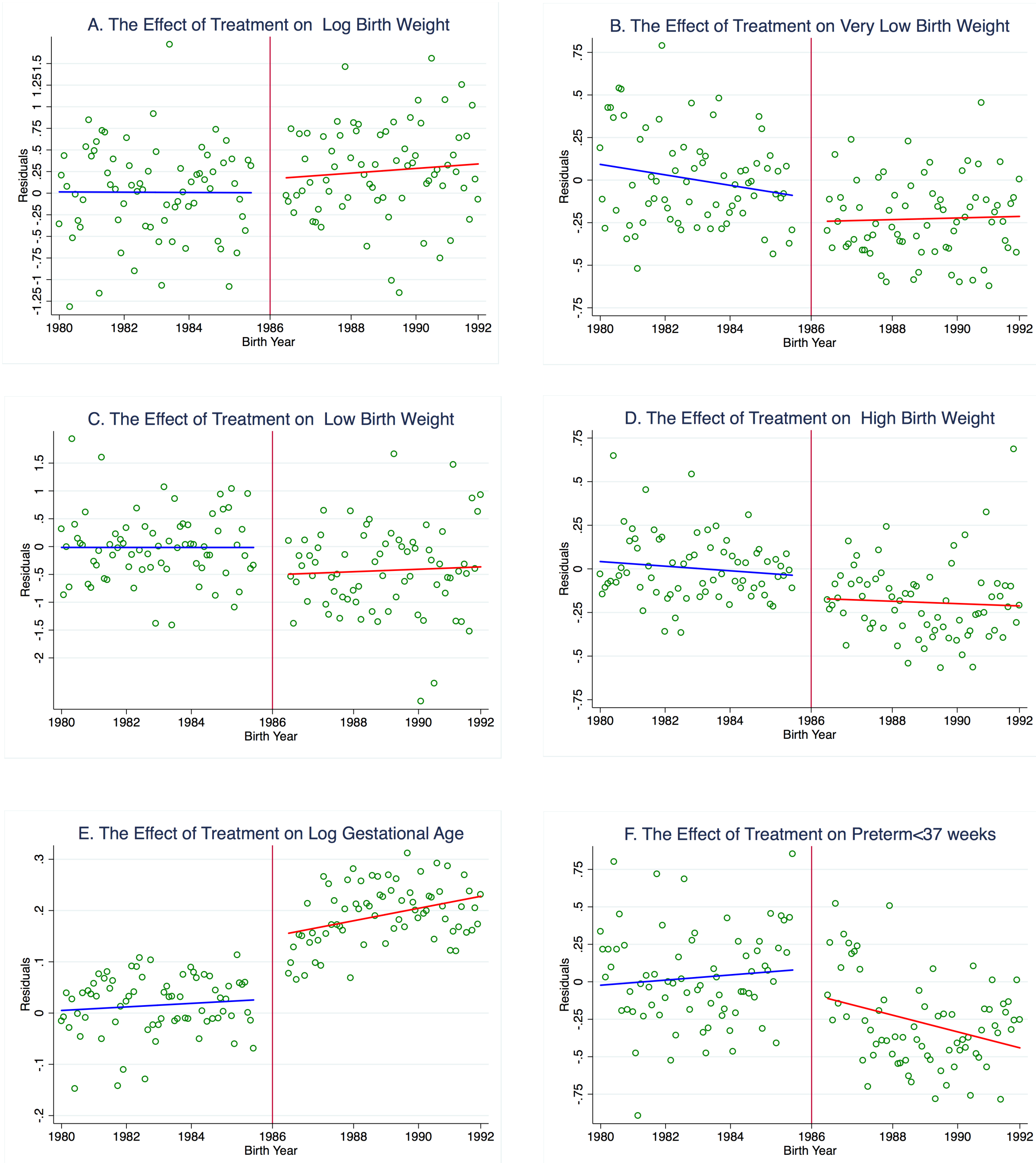

Figure 2.2: The Impact of the 1997 Education Reform on Outcome Variables

Even though the sample sizes increased by 52 to 70 percent, none of the specifications presented in column (5) is statistically significant although they now carry the expected signs. Consequently, these results provide evidence in favor of the argument that without a sufficiently large estimation sample, IV estimates of the impact of maternal education on child health may be imprecisely estimated and/or biased.

To the degree that the datasets used in this study allow, I also undertake a few exercises to explore the potential heterogeneity. Note that the 1997 education reform aimed to increase the years of schooling of those with a low tendency to extend their schooling beyond five years of basic education. How extended education impacts child outcomes among women who are at the lower tail of the educational attainment distribution is particularly important from the viewpoint of a policymaker. Albeit imperfect, to provide an answer to this question, I reproduce the main models by limiting the estimation sample to those with at most a middle school degree. In Table 2.7, despite reduced sample sizes, estimates of the impact of maternal education on child outcomes are precisely estimated.

It is commonly observed in developing countries that families may provide less care to their daughters because of son preference (Oster 2009; Bharadwaj and Lakdawala 2013). That is, if parents receive greater utility from their daughters or sons, they may discriminate care in favor of the preferred gender when providing antenatal or postnatal care. Therefore, Table 2.8 explores whether the effect of maternal education on child health differs by the gender of the offspring. These estimates show that the gender specific estimates 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. Extended maternal schooling may have potentially differing effects on child health in different regions. On the one hand, women's education in poorer or more rural regions may be more effective in improving child health if the marginal effect of maternal behavioral modifications on child outcomes is greater especially when formal healthcare is not readily available. healthcare is not readily available. For example, such behavioral modifications may become more important if infectious diseases and accidents that are the major causes of child mortality (Chou et al. 2010). On the other hand, educating women with a low schooling tendency may improve their productive efficiency when they have easier access to medical care in more developed or urban areas. Therefore, I also explore if the impact of maternal education on child outcomes differ by region of residence.

Table 2.7
Instrumental Variable Estimates of the Impact of Maternal Education on Child Health, at Most Middle School Sample

Panel A: Birth Outcomes	
Dependent Variables	
Log Birthweight	0.020
	(0.024)
Very Low Birthweight	-0.028***
	(0.007)
Low Birthweight	-0.055**
	(0.025)
High Birthweight	-0.024**
	(0.011)
1st Stage F-test	{127.320}
	[116,184]
Log Gestational Age	0.016***
	(0.003)
Preterm<37 weeks	-0.020
	(0.014)
1st Stage F-test	{223.940}
	[961,594]
Log Head Circumference	0.011
	(0.007)
1st Stage F-test	{125.249}
	[115,978]
Panel B: Child Mortality	
Any Child Died	-0.020***
	(0.006)
1st Stage F-test	{430.5}
	[234,084]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects and province specific maternal birth month-year trends. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations.

Although relatively less precisely estimated, region specific estimates, which are available upon request, show that the impact of maternal education on child outcomes are similar across different geographic areas. Lastly, while it would have been informative to explore the heterogeneous effects of maternal compulsory schooling on birth outcomes and child mortality in urban versus rural areas, I am unable to run these

models as the data sets do not provide information on whether the respondents live in urban or rural localities.

Table 2.8
Gender Specific IV Estimates of the Impact of Maternal Education on Child Health

	(1) Daughter Sample	(2) Son Sample
Panel A: Birth Outcomes		
Dependent Variables		
Log Birthweight	0.032	0.045*
	(0.029)	(0.026)
Very Low Birthweight	-0.060***	-0.032***
	(0.016)	(0.011)
Low Birthweight	-0.078	-0.070***
	(0.049)	(0.026)
High Birthweight	-0.030***	-0.023
	(0.01)	(0.018)
1st Stage F-test	{74.157}	{23.479}
	[90,985]	[95,855]
Log Gestational Age	0.024***	0.021***
	(0.004)	(0.005)
Preterm<37 weeks	-0.069***	-0.031*
	(0.024)	(0.018)
1st Stage F-test	{179.517}	{113.265}
	[722,387]	[763,966]
Log Head Circumference	0.01	0.017**
	(0.012)	(0.008)
1st Stage F-test	{75.606}	{23.67
	[90,801]	[95,704]
Panel B: Child Mortality		
Any Child Died	-0.012*	-0.014*
	(0.007)	(0.007)
1st Stage F-test	{374.9}	{374.9}
	[221,061]	[236,081]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects and province specific maternal birth month-year trends. Birth outcome models also control for child's birth year fixed effects and male child dummy.

The first stage estimates presented in Table 2.3 show a discrepancy between two data sources employed. That is, while the reform has a 22.8 percentage point effect on middle school graduation in the PHC data, it is roughly 8 percentage points in the MHBOD sample. To understand why this difference exists, I reached out to a few family physicians, who are in charge of data collection at the Family Health

Centers. Accordingly, they reported that family physicians access the date of birth information of mothers and children from the Census Bureau via a computer software. Information on child outcomes, maternal behaviors, and education are reported by mothers. As the main objective of the data collection efforts through the family health centers is to get accurate data on health outcomes, it is very likely that health information is collected with high accuracy.

However, because the terms to describe 5 years of schooling (*İlkokul)* prior to the reform and 8 years of schooling (*İlköğretim*) after the reform are not only phonetically similar but also have similar meanings in Turkish language, it is possible that those who were bound by the reform may have inadvertently misreported their educational attainment status. That is, in the Turkish language, the meaning of the word *İlkokul* is elementary school, and the word *İlköğretim* means primary education. Recall that the reform united elementary school (*İlkokul*) and middle school (*Orta Okul*) under the roof of primary school (*İlköğretim*). Therefore, the educational attainment status of those who hold an 8-year primary school diploma may be miscoded as 5-year elementary school diploma due to reporting error.[31] If the potential measurement error in *Middle School* variable in the MHBOD is correlated with the 1997 education reform, the IV estimates of γ_1 may be biased.

The standard method to tackle this problem in the literature is to implement a two sample instrumental variables (TSIV) estimation strategy (Angrist and Krueger 1992; Inoue and Solon 2010). In doing so, I used two different data sources to obtain the first stage effect of the law on *Middle School*. First, I used the first stage from the PHC data which is presented in Panel B of Table 2.3. Second, the impact of the 1997 reform on the likelihood of earning at least a middle school diploma among ever married women estimated using data from the Household Labour Force Survey (HLFS) for the period 2008-14.[32] The estimate of the impact of *Education Reform* on *Middle School* in the HLFS is displayed in column (1) of Table 2.9. Accordingly, those who are bound by the reform are 24.5 percentage points more likely to earn at least a middle degree. For ease of presentation, the first stage estimate from the PHC data presented in column (2).

[31] This issue should be of a much smaller concern in the PHC data since the TurkStat carefully trains its survey personnel to minimize potential errors in the TurkStat surveys.
[32] The HLFS has been conducted since 1988 by the Turkish Statistical Institute and is the main data source for employment, economic activity, occupation, and other labor market indicators. See http://www.turkstat.gov.tr/ for more information.

Table 2.9
Two Sample Instrumental Variable Estimates

	(1) First Stage HLFS	(2) First Stage PHC	(3) Reduced Form	(4) 2 Sample IV HLFS	(5) 2 Sample IV PHC
Dependent Variables					
Log Birthweight	0.245***	0.227***	0.00287**	0.0100*	0.013**
	(0.015)	(0.012)	(0.00123)	(0.005)	(0.005)
Very Low Birthweight	0.245***	0.227***	-0.00355***	-0.014***	-0.016***
	(0.015)	(0.012)	(0.00056)	(0.002)	(0.002)
Low Birthweight	0.245***	0.227***	-0.00563***	-0.022***	-0.025***
	(0.015)	(0.012)	(0.00171)	(0.007)	(0.007)
High Birthweight	0.245***	0.227***	-0.00200***	-0.008***	-0.009***
	(0.015)	(0.012)	(0.00073)	(0.003)	(0.003)
	[154,160]	[340,091]	[186,840]	[186,840]	[186,840]
Log Gestational Age	0.245***	0.227***	0.00190***	0.008***	0.008 ***
	(0.015)	(0.012)	(0.0002)	(0.001)	(0.001)
Preterm	0.245***	0.227***	-0.00414***	-0.017***	-0.018***
	(0.015)	(0.012)	(0.00116)	(0.005)	(0.000)
	[154,160]	[340,091]	[1,486,353]	[1,486,353]	[1,486,353]
Log Head Circumference	0.245***	0.227***	0.00100*	0.00373*	0.00441*
	(0.015)	(0.012)	(0.00053)	(0.00211)	(0.00234)
	[154,160]	[340,091]	[186,505]	[186,505]	[186,505]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, child's birth year fixed effects, province fixed effects, and province specific maternal birth month-year trends. 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 and HLFS are used in corresponding estimations. Column headings pertains to data used in estimations. Number of observations are in square brackets [].

It is comforting that the first stage estimate of the effect of law on educational attainment in the HLFS is fairly close to that in the PHC data. Column (3) presents the reduced form impact of the 1997 reform on child health outcomes. Then, columns (4) and (5) display the TSIV using the first stage estimates from the HLFS and PHC, respectively. The TSIV coefficients obtained from this exercise is equal to the ratio of the reduced from and the associated first stage coefficient. I use the delta method to calculate the standard errors for these coefficients as summarized in Inoue and Solon (2010).[33] The estimates presented

[33] More specifically, following Inoue and Solon (2010), and Mocan, Rashke and Unel (2015), I use the delta method to calculate the standard error of the estimated TSIV coefficient, γ_1, as: $var(\gamma_1) = \left(\frac{1}{\alpha_1^2}\right)\left[Var(\delta_1) + \frac{\delta_1^2}{\alpha_1^2}\, Var(\alpha_1)\right]$, where α_1 is the first stage coefficient in Equation 2.3 and 2.4, and, δ_1 is the corresponding reduced form coefficient.

in columns (4) and (5) are very similar to each other and precisely estimated. Furthermore, as implied by the change in the magnitude of the associated first stage estimate, coefficients obtained from the TSIV estimation method are roughly one third of those presented in previous tables. An additional advantage of the TSIV exercise is that it also allows to correctly evaluate the effect sizes.

Column (5), which provides us with the unbiased effect sizes for the IV estimates in the MHBOD, indicates that earning at least a middle school diploma leads to a 1.3 percent increase in birth weight of the offspring. *Middle School* lowers the likelihood of *Very Low Birthweight*, *Low Birthweight*, and *High Birthweight* by 1.6, 2.5, and 0.9 percentage points, respectively. Furthermore, mothers who possess at least eight years of schooling have a 1.8 percentage point lower likelihood of delivering preterm babies, and experience a 1.5, and 0.4 percent increase in gestational length and head circumference, respectively.

I also undertook a quick and dirty approach to circumvent the potential issue of mismeasurement of maternal schooling in the MHBOD. Accordingly, I assigned different probabilities to the likelihood of middle school graduation rates of women who were born after 1986 and declared that their highest level of completed schooling corresponds to a five-year elementary school degree. Appendix Table A7, estimates the impact of maternal schooling on birth outcomes by assigning the values of 1, 0.75, 0.5, and 0.25 to the likelihood of earning at least a middle school diploma for those who were bound by the reform and reported their highest level of educational attainment as five years of elementary school diploma in columns (1) to (4), respectively. This exercise produces coefficients which are similar to those obtained from the TSIV estimates and lead to greater precision in the estimated effect of maternal schooling on outcomes of health. More importantly, in column (2), in which the first stage estimate is very similar to those in the HLFS and PHC, the coefficients on Middle School are qualitatively and qualitatively very similar to those obtained from the TSIV.

2.8 The Impact of Maternal Education on Maternal Behaviors

Does extended primary schooling influence maternal attitudes? While the MHBOD do not contain extensive information on maternal behaviors, the data provide information on method of birth delivery and maternal smoking.

If education impacts women's behaviors during pregnancy in terms of seeking preventive care and/or adherence to recommendations by healthcare professionals, the likelihood of experiencing health complications during child delivery may be reduced (Currie and Moretti 2003; Cutler and Lleras-Muney 2006; McCrary and 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 and Arulkumaran 2003). 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 (Blustein and Liu 2015; Black et al. 2016). Therefore, women who are more knowledgeable about the subsequent health effects of C-sections as a consequence of extended schooling may consider attempting to avoid C-section birth deliveries. *Normal Birth*, which may capture child health at birth or maternal investment in child health, is a binary variable equal to 1 if the birth delivery was vaginal, and 0 otherwise. Lastly, I construct two maternal smoking measures that may capture 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 smoker at the time of data collection, respectively.[34] Descriptive statistics, displayed in Appendix Table A8, show that 46% of women experienced a normal delivery, and this rate is significantly higher in the treatment group (52%) compared to the control group (42%).[35] Women in the treatment group are less likely to report they have ever smoked or are smoking at the time of the data collection in comparison to women in the control group.

Table 2.10 presents the estimates of the relationship between women's education and maternal behaviors. The OLS estimates in column (1) reveal that women with at least eight years of schooling are 11.7 percentage points less likely to deliver babies via natural birth delivery. Given that C-sections constitute a

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

[35] Turkey has a disproportionately high rate of C-sections compared to the world average (Akarsu and Mucuk 2014; Zeldin 2012; Seckiner, Tezcan, and Tunckanat 2010). 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 and Mucuk 2014).

large fraction of non-normal birth delivery methods (53.5 percent in my sample), the negative association between maternal education and *Normal Birth* is consistent with the argument that C-section birth deliveries are preferred by women who earned at least a middle school degree.[36] This could be due to the potential positive association between women's level of education / age and experiencing complications when giving birth. Meanwhile, it is also possible that as more educated women can financially afford C-sections, perceived pain avoidance during delivery may partially explain the observed positive association. High prevalence of C-section may also explain the negative association between maternal education and gestational age as C-section surgeries are usually performed at around 38-39 weeks of gestation (Davidoff et al. 2006; Wilmink et al. 2010). In column (1), I 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. My analysis reveal that women who hold at least a middle school diploma are 2 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 and 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. Column (2) presents the IV estimates of the impact of female schooling on maternal behaviors. Children with mothers who earned at least a middle school diploma are 24.8 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 reports that maternal education was negatively correlated with the likelihood of a normal birth delivery. Different factors can explain why an increase in maternal educational attainment has a positive impact on normal 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, I already showed that maternal schooling has a causal negative impact on high birth weight, which is a well-known reason to employ non-normal birth delivery methods. Moreover, if more educated mothers are more aware of the

[36] 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.

Table 2.10
Instrumental Variable Estimates of the Impact of Maternal Education on Maternal Behaviors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	Intent to Treat Estimates	First Stage HLFS	First Stage PHC	2 Sample IV HLFS	2 Sample IV PHC
Dependent Variables							
Normal Birth	-0.11650***	0.266***	0.02289***	0.245***	0.227***	0.093***	0.100***
	(0.00393)	(0.039)	(0.00238)	(0.015)	(0.012)	(0.014)	(0.012)
1st Stage F-test		{167.822}					
	[1,594,793]	[1,594,793]	[1,594,793]	[154,160]	[340,091]	[1,594,793]	[1,594,793]
Ever Smoked	0.01900***	-0.084***	-0.00834***	0.245***	0.227***	-0.034***	-0.037***
	(0.0012)	(0.023)	(0.00192)	(0.015)	(0.012)	(0.009)	(0.009)
Current Smoker	0.00863***	-0.034***	-0.00341***	0.245***	0.227***	-0.014***	-0.015***
	(0.00137)	(0.013)	(0.00122)	(0.015)	(0.012)	(0.005)	(0.005)
1st Stage F-test		{222.429}					
	[550,001]	[550,001]	[550,001]	[154,160]	[340,091]	[550,001]	[550,001]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects, province specific maternal birth month-year trends, child's birth year fixed effects, and male child dummy. Models are weighted by number of women in fertility age (ages 15 to 49) in the province. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

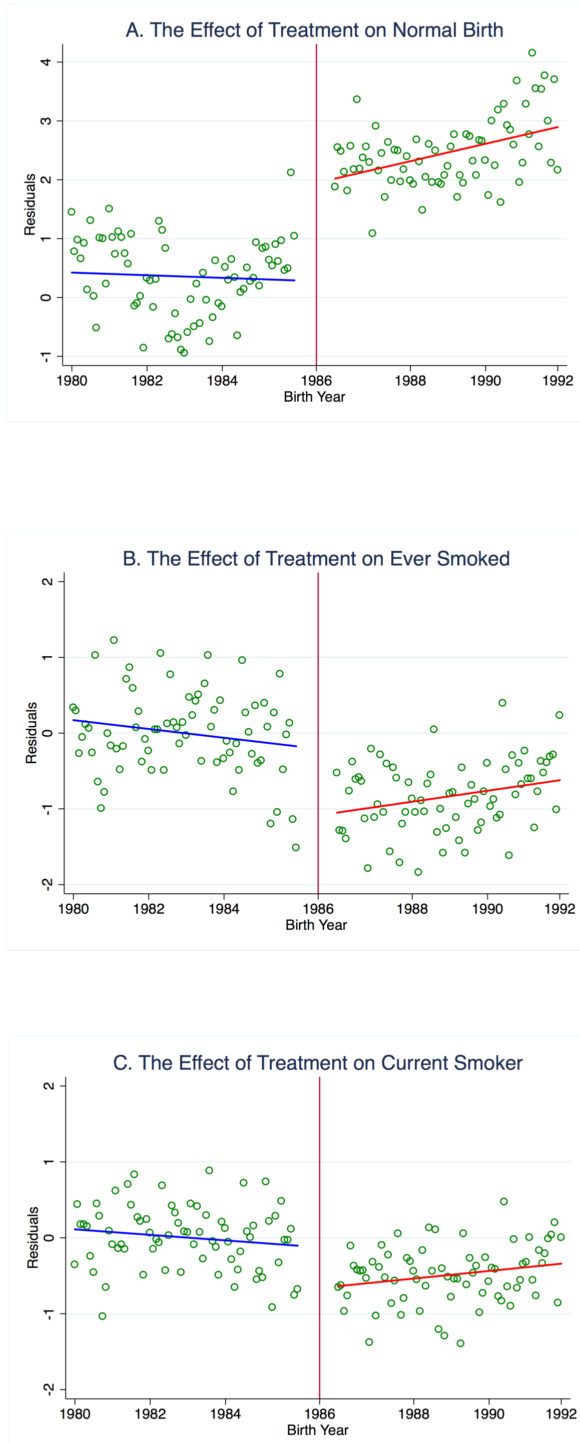

Figure 2.3: The Impact of the 1997 Education Reform on Maternal Behaviors

child health benefits of a vaginal birth delivery, they may choose to avoid unnecessary C-sections. In terms of maternal smoking, presented in column (2), I find that receiving at least eight years of schooling lowers the likelihood of ever smoking regularly and being a current smoker by 8.4 and 3.4 percentage points, respectively. Given that women's smoking is found to have negative effects on child health, these results suggest that the avoidance of risky health behaviors by mothers may mediate the relationship between maternal education and child health.[37] Columns (3) to (7) display the reduced form effect of education reform on maternal attitudes and perform the TSIV estimates. Then, I repeat the robustness and sensitivity exercises performed for child health outcomes in Figures 2.3A-2.3C, and Appendix Tables A9, A10 and A11. These exercises produce a very similar pattern to that of birth outcomes and child mortality. These estimates suggest increased schooling among women improves their behaviors towards enhancing the health of their offspring's. Altogether, these findings boost my confidence of the findings pertaining to the main focus of this study, which show that extended maternal schooling improves child health at birth and lower child mortality.

2.9 Discussion and Conclusion

A number of researchers in economics have explored whether parental (particularly maternal) schooling has a causal effect on child health in developing nations. Although some of these studies document that extending mother's years of schooling improves child health at birth and lowers child mortality, a set of others does not find a statistically or economically significant relationship between the two. A closer look at these studies reveals that a key shortcoming of many of the studies in this literature is the lack of adequate statistical power. Even when the source of identifying variation in maternal education comes from a credible natural experiment, without a sufficiently large estimation sample, the results may be biased especially when the mean of the dependent variable is small. Furthermore, previous literature offers little knowledge on the impact of forcing those with a little interest to pursue additional schooling to receive extra education in developing countries.

[37] Nevertheless, if extra schooling causes women to under-report their smoking to healthcare professionals especially, the estimates may be biased away from zero (or from a potential positive impact of education on smoking). Therefore, caution should be exhibited in interpreting the impact of women's education on maternal smoking.

To fill the void in the literature, I analyze the effect of maternal compulsory schooling on child health using two large data sets from a developing country, the Republic of Turkey. This study exploits the 1997 compulsory education reform as the source of exogenous variation in maternal schooling to address the endogenous nature of mother's education. I analyze a host of outcomes, including birth weight, gestation, head circumference, child mortality, method of birth delivery, and maternal smoking.

The 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, and high birth weight. They are less likely to have a child died by age five. Additionally, obtaining increased schooling extends gestational age and lowers the propensity of delivering premature babies. Finally, I also find evidence suggesting that additional extended primary education increases the likelihood of normal births and decreases maternal smoking.

To put the estimates in context and be able to compare to other studies examining the relationship between maternal education and child health in developing countries, I calculated the impact of at least one additional year of maternal education, which is obtained by dividing the 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. The results imply that at least one more year of maternal education lowers the probability of very low birthweight by 0.53 percentage points, low birth weight by 0.83 percentage points, and high birthweight 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. I also find evidence that it increases the probability of a normal birth by 3.3 percentage points and lowers the probability of maternal smoking by 1.33 percentage points. The results survive a number of robustness checks.[38] These effect sizes are similar to those reported by Grépin and Bharadwaj (2015) and Chou et al. (2010).

The careful analysis addresses potential concerns surrounding the validity of the instrumental variable. As illustrated by graphical analysis, figures reveal a discontinuity around the reform for my outcome variables yet no discontinuity for the balancing covariates, lending further support for the strength of the instrument in the first stage and the validity of exclusion restrictions. The consistency of my results across the estimates of various models, including different econometric specifications, functional forms, choice of bandwidth, different samples, as well as

[38] These effect sizes on birth outcomes and maternal smoking are obtained using the estimates presented in column 5 of Table 2.9. The effect size on child mortality is shown in Table 2.5 (column 6) and also in Table 2.6 (column 2).

the inclusion of province specific birth cohort trends, among other robustness checks, suggests that the findings represent the causal effect of having obtained a middle school degree on child health outcomes.

Importantly, the estimates correspond to the LATE of maternal education on birth outcomes and child mortality among mothers with a low tendency to extend their education 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 for the reason that it allows one to estimate the policy implications of compulsory schooling at the lower end of the educational attainment distribution. Note that, as 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 with no interest in additional schooling. That being said, to the best of my knowledge, this reform provides the closest natural experiment that enables one to study the impact of mandated schooling in a developing country.

While the efforts towards accomplishing universal primary schooling in poorer societies of the world lead to significant progress, many developing countries have relatively few years of mandated education. Furthermore, those with compulsory schooling laws oftentimes fail to successfully enforce the rule, especially for females. Taken together, these 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. 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.

CHAPTER 3: TRANSFORMING LIVES: THE IMPACT OF COMPULSORY SCHOOLING ON HOPE AND HAPPINESS[39]

"The pursuit of happiness is serious business. Happiness for the entire human family is one of the main goals of the United Nations."

-- Ban Ki-moon, United Nations-Secretary-General, 2015

3.1 Introduction

Given that standard economic indicators, such as per capita GDP, are considered insufficient in representing human welfare (Fleurbaey 2009; Stiglitz, Sen and Fitoussi 2009) and the ultimate goal of an individual is "the pursuit of happiness" (Frey and Stutzer 2002a), both politicians and academics have deservedly given subjective well being increased attention. Prominent politicians, including David Cameron and Nicolas Sarkozy, have similarly expressed their concerns about the inadequacy of standard economic indicators in capturing individual well being, and vocalized their support for the construction of better welfare measures.[40],[41] Thus, not surprisingly, the United Nations (UN) announced the celebration of the International Day of Happiness, on the 20th of March since 2012, to endorse efforts supporting recognition of the pursuit of happiness as a fundamental human goal; the Organization for Economic Co-operation and Development (OECD) established the Better Life Index with the motto: There is more to life than the cold numbers of GDP.[42] At the same time, a host of economists as well as researchers from other disciplines have shown interest in the study of subjective well being (hereafter SWB) (Easterlin 1974; Clark and Oswald 1994; Argyle 1999; Kahneman, Deiner, and Schwarz 1999; Van Praag, Frijters and Ferrer-i-Carbonell 2003; Ferrer-i Carbonell 2005; Kahneman and Krueger 2006; Clark, Frijters and Shields 2008; Deaton 2008; Dunn et al. 2008; Yang 2008; Bernanke 2010; Benjamin et al. 2014).

[39] This chapter was originally published in J Popul Econ, Transforming Lives: The Impact of Compulsory Schooling on Hope and Happiness, 29: 911-956 (2016). Bahadir Dursun, Resul Cesur and hereby reprinted with Permission of Springer.
[40] Prime Minister David Cameron emphasized that governments should focus not only on GDP but GWP (general well-being) (Benjamin et al. 2014). Similarly, with President Sarkozy's efforts, the Commission on the Measurement of Economic Performance and Social Progress was established in France with a broad support from economists, including Nobel Economics Prize Laureates, to draw attention on shifting emphasis from measuring economic production to measuring people's wellbeing. For details see (Stiglitz et al. 2010) and http://www.stiglitz-sen-fitoussi.fr/en/index.htm.
[41] "The pursuit of happiness" is also highlighted in the U.S. Declaration of Independence in 1776 as an "unalienable right" akin to life and liberty.
[42] For details, see http://www.oecdbetterlifeindex.org/

There is now a rich literature looking to understand the drives and importance of happiness in human welfare (Easterlin 2001; Alesina, Di Tella, and MacCulloch 2004; Di Tella and MacCulloch 2006; Layard 2006, 2010; Rayo and Becker 2007; Stevenson and Wolfers 2009; Guriev and Zhuracskaya 2009; Oswald and Wu 2010; Benjamin et al. 2012).[43],[44] Accordingly, relying on different identification strategies, recent studies in economics have examined potential causal determinants of SWB, including the impact of income (Frijters, Haisken-DeNew, and Shields 2004a, 2004b and 2006; Luttmer 2005; Gardner and Oswald 2007; Bayer and Juessen, 2015), unemployment (Kassenboehmer and Haisken-DeNew 2009), welfare transfers (Herbst 2013), religious participation (Cohen-Zada and Sander 2011; Campante and Yanagizawa-Drott 2015), housing quality (Cattaneo et al. 2009), access to clean water (Devoto et al. 2012), frequency of having sex (Loewenstein et al. 2015), air quality (Ferreira et al. 2013), natural disasters (Goebel et al. 2015), changes in spatial concentrations of immigrants (Akay et al. 2014), and cultural assimilation of immigrants (Angelini, Casi, and Corazzini 2015).

But many outstanding cause-and-effect questions remain sparsely explored in the emerging literature on economics of happiness (Fleurbaey 2009; Stutzer and Frey 2012; Layard et al. 2014). Among these, notably, is the role of formal schooling. Although education is taken as one of the most important socioeconomic determinants of happiness (Frey and Stutzer 2002b), a sizable literature investigates the relationship between education and various SWB measures, most of this work is in descriptive nature.[45] Moreover, the findings in this literature often conflict (Blanchflower and Oswald 2004; Graham, Eggers and Sukhtankar 2004 vs. Clark and Oswald 1996; Clark 2003). Therefore, whether correlations documented between schooling and SWB indicators correspond to causal or spurious relationships is uncertain. That is, if unobserved precursors of schooling also impact the SWB of an individual, the estimates of the link between happiness and education may be biased.

Motivated by these gaps in the economics of happiness literature that ought to be addressed through causal inference methods, this study investigates causal effects of education on happiness of young adults. To do so, I exploit an exogenous variation in educational attainment resulting from a schooling reform, which increased the duration of compulsory basic education from 5 to 8 years in Turkey.

[43] Following upon Easterlin (2006), this study uses the terms “happiness”, “life satisfaction” and “subjective wellbeing” interchangeably throughout the article.
[44] Relatedly, the reliability of SWB measures as welfare indicators has been questioned as well. For evidence on the suitability of SWB measures see: Krueger and Schkade (2008), Oswald and Wu (2010).
[45] One exception is Oreopoulos (2007), which examines the impact of dropping out of high school on lifetime wealth, health, and happiness.

Using exposure to the 1997 education reform as an instrumental variable to predict the probability of holding at least a middle school diploma, this study finds that extended primary schooling increases the happiness of young women. Furthermore, middle school education has a direct impact on the likelihood of being satisfied with different aspects of their lives. Supplementary analysis suggests that being hopeful about one's own future well being seems to partially explain the impact of education on happiness among women.

I do not find evidence that extended schooling improves male SWB. To the contrary, although relatively imprecisely estimated, my analysis document that for males holding at least a middle school degree lowers the probability of being happy as well as the likelihood of being satisfied with various life domains. Further analysis shows that, among men, holding at least a middle school degree causes a decrease in the perceived relative economic standing in society as well as the probability of being hopeful about future. A plausible explanation behind these counterintuitive findings for men may be that improved educational attainment caused a mismatch between aspirations and attainments.[46] For instance, although two different articles show that the education reform at hand did not cause a reduction in male wages (Torun 2013; Mocan 2014), this study finds that holding at least a middle school degree has a negative and statistically significant impact on the probability of being satisfied with earnings among men. Thus, if extended schooling caused an increase in well being expectations with no corresponding increases in the standard of living, the overall effect of additional schooling on SWB may be negative.

This article contributes to several strands of literatures in economics. First, to the best of my knowledge, this is the first study providing causal evidence on the relationship between education and happiness of young adults (ages 18 to 34); hence, it adds to the literature studying the causal determinants of SWB. Second, despite the fact that gender specific characteristics may play a significant role in this context (Ross and Mirowski 2006; Stevenson and Wolfers 2009), these differences have not received much attention.[47] The results presented in this study support the view that extended schooling may have greater benefits for women than for men (Ross and Mirowski 2006). Relatedly, these findings favor the argument that women's empowerment through education may boost the well being of women (Kandpal, Baylis, and Arends-Kuenning 2012; Mocan and Cannonier 2012). Finally, this study

[46] As discussed later, a line of literature in psychology and other fields defines happiness as the balance between a person's aspirations and attainments (de la Croix 1998; March and Simon 1968; Solberg et al. 2002).
[47] Powdthavee, Lekfuangfu and Wooden (2015) is one of the few studies accounting gender differences. Using a simultaneous equation model (SEM), they analyze the direct and indirect associations between education and life satisfaction through: income, employment, marriage, children and health channels among Australians.

adds to the literature in economics of education looking at populations of developing countries making use of causal identification methods as well as supplementing the literature aiming to uncover the causal effects of schooling on non-pecuniary outcomes (Grossman 2006; Berhman 2010; Oreopoulos and Salvanes 2011).

The remainder of the article is organized as follows. Section 3.2 summarizes the relevant literature and illustrates the conceptual framework. Sections 3.3 and 3.4 introduce details of the 1997 compulsory schooling reform in Turkey and data used in the analysis. Section 3.5 lays out the empirical methodology; Section 3.6 presents main findings. Section 3.7 evaluates sensitivity and robustness; Section 3.8 explores potential mechanisms linking education to SWB. Finally, section 3.9 concludes.

3.2 Literature Review

The argument about the relationship between schooling and happiness across different countries, education levels, and time frames is far from settled. Although a number of studies find a positive association between education and happiness (Hartog and Oosterbeek 1998; Di Tella, MacCulloch and Oswald 2001; Easterlin 2001; Blanchflower and Oswald 2004; Graham, Eggers and Sukhtankar 2004; Cunado and Perez de Gracia 2012), a handful of articles claim that the relationship between the two is negative or inconclusive (Clark and Oswald 1996; Clark 2003). Furthermore, descriptive evidence on which the existing literature draws does not necessarily imply a causal relationship between education and happiness because unobserved individual, family, and environmental characteristics (e.g., motivation, intelligence, parenting, and neighborhood conditions during childhood) are likely to influence schooling and SWB simultaneously (Frey and Stutzer 2002b; Dolan et al. 2008). While evidence on the relationship between education and happiness, for the most part, relies on studies summarizing correlations, one study examined the causal effect of education on happiness. Oreopoulos (2007), using data drawn from the British Eurobarometer Surveys, exploits the change in British minimum school leaving age laws to estimate the causal impact of formal schooling on health, lifetime wealth, employment status, and the probability of being happy. His analysis indicates that extended schooling increases the likelihood of being happy.[48]

Formal schooling may impact an individual's well being through different financial and non-financial channels including earnings, occupation, self-esteem, and health (Oreopoulos and Salvanes 2011). Relying on

[48] In a review article, Oreopoulos and Salvanes (2011) repeat the same exercise for 25-65 year olds using the same data and get similar results.

causal identification techniques, a number of studies show that education impacts labor market outcomes (e.g., wages and job quality) (Angrist and Krueger 1991; Oreopoulos 2006; Brunello, Fort, and Weber 2009), cognitive skills, health, marital status, and fertility (Brunello, Fabbri, and Fort 2013; Fort, Schneeweis, and Winter-Ebmer 2014; Schneeweis, Skirbekk, and Winter-Ebmer 2014; Carlsson et al. 2015; Lavy and Zablotsky 2015). Studies also found education associated with increased self-esteem, social status, social support that individuals receive, as well as relationship stability (Ross and Van Willigen 1997; Argyle 1999).

Existing literature also reveals strong associations between SWB measures and correlates of education (e.g., wages, occupation, and health) (Frey and Stutzer 2002b; Layard et al. 2012). Consequently, it stands to reason that education may affect a person's SWB through different and possibly conflicting mechanisms.

Besides the relatively extensively examined outcomes of education, there may be alternate potential pathways linking education to SWB. For example, in explaining the psychological mechanisms generating happiness, Frey and Stutzer (2002b) emphasize that adaptation and coping skills, which might be transmitted to individuals via education, should be taken into account. Evidence also supports the link between happiness and being hopeful about future well being (Foster, Frijters and Johnston 2012; Senik 2014). Considering these mechanisms, being hopeful might be a mediator between schooling and happiness through the effect of education on developing goal-directed determination and adaptive coping skills, being more flexible, and having positive thoughts (Snyder et al. 1997; Irving, Snyder, and Crowson 1998).

Nevertheless, the relationship between education and SWB may not be monotonic. A strand of literature in psychology as well as other fields, including economics, describes the concept of SWB of an individual as a balance between her/his aspirations and attainments (de la Croix 1998; March and Simon 1968; Solberg et al. 2002). In addition, increased stress levels and decreased leisure time may be inevitable consequences of increased education (Oreopoulos and Salvanes, 2011). Hence, while education may promote happiness to the degree that it enables people to achieve a balance between their aspirations and attainments, as well as avoiding stress, if more schooling causes an imbalance between people's aspirations and achievements (e.g., in terms of income, occupation, and marriage), or increases stress levels, extended education may have negative effects on people's happiness as well (Clark and Oswald 1996; Frey and Stutzer 2002b; Stutzer 2004; Dolan, Peasgood, and White 2008).

A further complicating element in the study of the impact of education on happiness is that increased formal schooling does not necessarily lead to uniform outcomes; thus, major debates in the literature continue on if and how much education influences a number of outcomes for different populations.[49]

Relatedly, whether and to what degree the effects of formal schooling on different outcomes vary by gender may also impact male and female SWB differentially.[50] Moreover, the Resource Substitution Theory (RST) conjectures that individuals with few alternative resources may be more dependent on education than the ones with more resources because resources can be substituted for one another to improve an individual's outcomes (Ross and Mirowski 2006, 2010 and 2011; Van de Velde Bracke and Levecque 2010; Ross, Masters and Hummer, 2012). Accordingly, in articulating gender specific health effects of education, Ross and Mirowski (2006) argue that education is more vital to the well being of females resulting from various disadvantages women often experience in society. In addition, Plagnol and Easterlin (2008) document important differences in male and female SWB levels over the life cycle. That is, while women are more likely to fulfill their family life and material good aspirations at early ages and therefore be happier than men, these gender differences are reversed at later ages, meaning men become happier than women at older ages (Plagnol and Easterlin 2008). Furthermore, especially in developing countries, education may be instrumental in promoting the empowerment of women (Kandpal, Baylis, and Arends-Kuenning 2012; Mocan and Cannonier 2012). Thus increased educational attainment may also impact female SWB through the empowerment channel.

All in all, the net effect of education on SWB depends on the relative magnitudes of the numerous and potentially conflicting mechanisms that have briefly mentioned. For instance, while greater income and better health may improve happiness, higher aspirations to material consumption levels may have the opposite effect. Therefore, as the cumulative impact of education on happiness of young adults (as well as other populations) is ex-

[49] For instance, while Black, Devereux and Salvanes (2005), Oreopoulos (2007), and Devereux and Hart (2010) find that schooling has a positive impact on earnings in Norway, Canada, US, and England, other studies, Pischke and von Wathcer (2008), Grenet (2013), and Stephens and Yang (2014), report zero returns to schooling in Germany, France and US. Similarly, Fort, Schneeweis, and Winter-Ebmer (2014) find conflicting results for the effect of education on fertility in Continental Europe and England.

[50] For example, Brunello, Fabri, and Fort (2013) document differential effects of education on BMI by gender. They also show that education leads to a larger employment probability for women compared to men in several European countries. Stephens and Yang (2014) find differential results by gender on how education impacts divorce in the US. Brunello, Fort, and Weber (2009) show that education has a greater positive effect on the earnings of women in comparison to men in a sample of 12 European countries. Similarly, exploiting the same reform that this study uses, Torun (2013) and Mocan (2014) provide evidence from Turkey that having at least a middle school degree leads to an increase in female wages, but extra schooling has no effect on the earnings of men.

ante uncertain, how formal schooling affects happiness is a novel empirical question, waiting for compelling answers.

3.3 The 1997 Education Reform

Prior to the compulsory education reform of 1997, the Turkish primary and secondary education system had three stages: a 5-year elementary school, a 3-year middle school, and a 3-year high school. While elementary school completion was mandatory, subsequent levels were voluntary. With law number 4306, the Grand National Assembly of Turkey (GNAT) raised compulsory education from 5 years to 8 years, merging the elementary and middle schools under the title *Primary School (i.e.,* ilköğretim okulu). The reform went into effect in the 1998-99 academic year and required students who completed fifth grade in 1998 to continue schooling through the eighth grade (Dulger 2004). Stated differently, those who started elementary schooling in 1993/4 academic year were subject to the new regulation. In Turkey, children 72 months old by the end of the calendar year are eligible to start elementary schooling in the corresponding fall semester (Cesur and Mocan 2013; Cesur, Dursun and Mocan 2014; Dincer, Kaushal, and Grossman 2014).[51] However, school starting age cut-off rules have been loosely enforced in Turkey; parents and school administrators could exercise a significant degree of freedom in deciding whether children below a certain age can start schooling in a given academic year (Torun 2013). Consequently, some children in the same birth cohort may delay starting school, or may start schooling earlier than their peers. This feature of the Turkish education system creates some ambiguity of who is in each school year cohort. Following up on the existing literature, I take the 1987 birth cohort as the first fully affected cohort, excluding the 1986 cohort from the main analyses, thus eliminating the potential of including students not subject to the 1997 reform because of the loose implementation of the school starting age rule (Battistin et al. 2009; Torun 2013; Mocan 2014; Fort, Schneeweis and Winter-Ebmer 2014).[52] In addition to loose implementation of the school starting age rule, parental reactions to the reform may have influenced schooling decisions for some individuals born in 1986, even if they were not bound by the law. That is, as the reform applies to children who began primary schooling in 1993, individuals who graduated from elementary school in 1997 were not obligated to continue schooling. However, it is

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

[52] As shown in Appendix Table B4, adding the 1986 birth cohort to the analysis by assigning different treatment values pertaining to exposure to the education reform status of those who were born in 1986 produces results that are very similar the main estimates.

possible that the compulsory schooling reform may have had spillover effects on some individuals who were not bound by the reform. That is, the parents of some children, who completed elementary school in 1997 that would not continue their education at a middle school in the absence of the compulsory schooling reform, may have reacted to the law change and decided that their children would continue schooling, even though it was not mandatory for them to attend middle school. This aspect of the compulsory schooling reform provides further justification for not including the 1986 birth cohort in the benchmark analysis; it is uncertain whether the education reform impacted those born in 1986.[53]

The 1997 education reform provides an ideal instrumental variable to study the impact of extended primary schooling on a number of outcomes as well as SWB. First, the reform focuses mainly on increasing enrollment rates; it made no curricular or compositional changes for pupils (Dulger 2004), which means the 1985 birth cohort (who just missed being exposed to the reform) and the 1987 birth cohort (who was the first directly affected cohort) experienced the same courses and regulations. Second, other than long run trends at the birth cohort level, there is no reason to suspect that those born a few years before or after 1986 differ from each other.[54] Therefore, not surprisingly, relying on this education reform, several recent studies have examined the effect of schooling on a number of outcomes, including health (Cesur, Dursun and Mocan 2014), earnings (Torun 2013; Mocan 2014), child health and fertility (Gunes 2015, 2016; Dursun and Cesur 2016), religiosity (Cesur and Mocan 2013), women's empowerment (Gulesci and Meyersson 2015), fertility (Dincer, Kaushal, and Grossman 2014), and gender gap in educational attainment (Kirdar, Dayioglu, and Koc 2015).

3.4 Data

This study uses data drawn from the Turkish Statistical Institute's Life Satisfaction Survey (hereafter TLSS), a nationally representative survey of life satisfaction among Turkish citizens, for 2009 to 2014. The TLSS aims to measure and track changes in the SWB of Turkish citizens. The TLSS includes household members at least

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[53] I acknowledge the possibility that the parents of children who were born prior to 1986, such as those who were born in 1985 and 1984, may have reacted to the education reform and decided to enroll their children to a middle school even though they were not required to do so. However, both the trends in holding at least a middle school diploma, presented in Figure 3.1, as well as the anecdotal evidence based on our observations, and conversations with educators and researchers in Turkey suggest that such a reaction by the parents of individuals who were born prior to 1986 did not happen.

[54] Because this study uses repeated cross sections of life satisfaction surveys for the 2009-14 period, by controlling for birth cohort trends and survey year dummies (as well as binary age indicators in robustness tests), our empirical specification allows us to fully account for the age effects.

18 years of age at the time of the survey. These data also contain basic demographic information such as educational attainment, marital status, age, and gender. Analysis sample includes the birth cohorts 1980 to 1992; they were between 18 to 34 years old at the time of the survey. Those born after 1986 constitute the treatment group; individuals born before 1986 are the control group.

The TLSS does not provide information on years of schooling of respondents. Instead, one can observe the highest attained level of education of survey participants. In particular, answers to the educational attainment question are categorized as: "No education; less than elementary school degree (corresponds to less than 5 years of formal schooling); elementary school degree (corresponds to 5 years of education); middle school degree (corresponds to 8 years of education); high school degree; and college or higher levels of education." Because the 1997 education reform mandated obtaining at least a middle school degree, the variable of interest, *Middle School Diploma,* takes the value of one if a respondent has at a least a middle school degree, zero otherwise.

The key outcome variable, *Happy,* is constructed based on the following question: "When you consider your entire life, how happy are you?" Possible answers are: 5 "very happy"; 4 "happy"; 3 "neither happy, nor unhappy"; 2 "unhappy"; 1 "very unhappy." *Happy* is set equal to one if a respondent declared her/his happiness level as "very happy" or "happy", zero otherwise.

To articulate components of SWB, Van Praag, Frijters and Ferrer-i-Carbonell (2003) proposed that overall life satisfaction of an individual could be explained by aggregating his/her satisfaction levels with various aspects of her/his life. This "bottom up" approach relies on how satisfied people are with different life domains, including finances, marriage, health, social life, and job. The literature has confirmed that this method is highly informative and useful (Van Praag, Frijters and Ferrer-i-Carbonell 2003; Easterlin 2006; Easterlin and Sawangfa 2007; Stevenson and Wolfers 2009).[55] Therefore, incorporating domains of life satisfaction measures in main analyses may be beneficial for at least two purposes. First, this exercise allows us to test the sensitivity of the findings to using alternate measures of SWB. Second, and more importantly, exploring the impact of extended primary

[55] Although the "bottom up" approach summarized by Van Praag, Frijters and Ferrer-i-Carbonell (2003) employs these domains of life satisfaction indicators as precursor of happiness, there is no consensus in the literature weighting which life domains are more important in determining overall happiness. Nevertheless, almost all the studies examining the role of different life domains in predicting happiness do agree on the importance of economic conditions, family circumstances, health and workplace conditions (Van Praag, Frijters and Ferrer-i-Carbonell 2003; Easterlin 2006; Easterlin and Sawangfa 2007).

schooling on the probability of being satisfied with various life domains may help uncover mechanisms that link education to SWB.

The TLSS contains life satisfaction questions about how satisfied survey respondents are with different aspects of their lives, including their financial well being (earnings and household income), job, marriage, health, friendship, neighborhood, housing quality, and family relationship. For each of these life domain satisfaction question, possible answers are: 5 "very satisfied"; 4 "satisfied"; 3 "neither satisfied, nor unsatisfied"; 2 "unsatisfied"; 1 "very unsatisfied." Using these categories, I created binary indicators reflecting being satisfied with earnings, household income, marriage, health, relationships with friends, job, neighborhood, housing, and relationships with family members and relatives. I coded each life domain satisfaction-dummy as one for survey participants answering to the associated question "very satisfied" or "satisfied", zero otherwise.

To obtain a more comprehensive measure of life satisfaction, previous researchers also constructed different SWB indices, including Satisfaction with Life Scale (SWLS) (Diener et al. 1985), Life Satisfaction Index-Adults (LSIA) (Neugarten et al. 1961), and Personal Well Being Index-Adults (PWI-A) (Cummins et al. 2003). In a similar spirit, I constructed a continuous life satisfaction measure, the *Composite Life Satisfaction Index (*hereafter *CLSI),* an abridged version the PWI-A.[56] I generate the *CLSI* based on the following life domain satisfaction indicators: household income, health, relationships with friends, housing, relationships with family members and relatives.[57] Using these life domain satisfaction indicators, I create a 25-point scale. Then, to simplify the interpretation of the *CLSI*, normalized it to mean 0 and standard deviation of 1.[58]

Table 3.1 displays descriptive statistics by exposure to the education reform for females (columns 1 to 3) and males (columns 4 to 6). While 77% of women in the treatment group, those born between 1987 and 1992, hold

[56] PWI-A consists of satisfaction indicators for the following life domains: Standard of living, health, achievement, relationships, personal safety, community, religion, and future security. For each life domain, the potential answers vary between 0 (no satisfaction at all) and 10 (complete satisfaction). The *Personal Well Being Index for Adults*, representing subjective wellbeing of an individual, is obtained by adding up these domain scores (International Wellbeing Group, 2013).

[57] Because the TLSS data do not provide all the questions used by the PWI-A, our life satisfaction index measure attempts to approximate it. In doing so, I experimented with different combinations of the life domain satisfaction variables available in the TLSS. Our results did not differ regardless of whether I use all or a subset of the available life domain satisfaction measures. Furthermore, I assigned equal weights to different life domains in constructing the *CLSI*. However, as suggested by Campbell et al. (1976) and Guardiola and Picazo-Tadeo (2014), I also experimented with assigning alternative weights to different life satisfaction measures in constructing our composite index. The results, which are available upon request from the authors, yield very similar estimates to the ones presented in this article.

[58] This type of a normalization of scale variables has been utilized by a number of previous studies, including Fryer and Levitt (2004), Kling, Liebman, and Katz (2007), and Liebman and Luttmer (2015).

at least a middle school diploma, it is 48% for females who are in the control group, individuals born between 1980 and 1985. The corresponding rates for male respondents in the treatment and control groups are 93% and 71%, respectively. Prevalence of happiness in the control and treatment groups for women are 65% and 69%, in that

Table 3.1 Descriptive Statistics

	Female All	Female Control	Female Treatment	Male All	Male Control	Male Treatment
Middle School Diploma	0.62	0.48	0.77	0.81	0.71	0.93
	(0.49)	(0.50)	(0.42)	(0.39)	(0.46)	(0.26)
Happy	0.67	0.65	0.69	0.57	0.57	0.58
	(0.47)	(0.48)	(0.46)	(0.49)	(0.50)	(0.49)
Satisfied with Earnings	0.42	0.41	0.44	0.41	0.38	0.45
	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)
Satisfied with Household Income	0.41	0.38	0.45	0.41	0.37	0.45
	(0.49)	(0.48)	(0.50)	(0.49)	(0.48)	(0.50)
Satisfied with Marriage	0.93	0.92	0.95	0.97	0.97	0.96
	(0.26)	(0.27)	(0.22)	(0.18)	(0.17)	(0.18)
Satisfied with Health	0.81	0.78	0.85	0.87	0.86	0.88
	(0.39)	(0.42)	(0.36)	(0.33)	(0.34)	(0.32)
Satisfied with Friends	0.91	0.91	0.92	0.91	0.90	0.92
	(0.28)	(0.29)	(0.28)	(0.28)	(0.30)	(0.27)
Satisfied with Job	0.70	0.71	0.69	0.70	0.70	0.71
	(0.46)	(0.45)	(0.46)	(0.46)	(0.46)	(0.45)
Satisfied with Neighborhood	0.76	0.76	0.76	0.78	0.78	0.78
	(0.43)	(0.43)	(0.43)	(0.42)	(0.42)	(0.42)
Satisfied with Housing	0.72	0.69	0.74	0.77	0.76	0.79
	(0.45)	(0.46)	(0.44)	(0.42)	(0.43)	(0.41)
Satisfied with Family Relations	0.83	0.83	0.83	0.80	0.80	0.80
	(0.38)	(0.38)	(0.37)	(0.40)	(0.40)	(0.40)
Composite Life Satisfaction Index	-0.05	-0.12	0.03	0.04	-0.01	0.11
	(1.03)	(1.03)	(1.03)	(0.97)	(0.98)	(0.96)
Hopeful	0.76	0.73	0.80	0.75	0.73	0.77
	(0.43)	(0.45)	(0.40)	(0.44)	(0.45)	(0.42)
Can Make Ends Meet	0.13	0.14	0.12	0.14	0.16	0.12
	(0.34)	(0.35)	(0.32)	(0.35)	(0.37)	(0.33)
Standardized Household Income	-0.15	-0.13	-0.18	-0.01	0.01	-0.03
	(0.87)	(0.90)	(0.84)	(0.91)	(0.93)	(0.88)
Standardized Subjective Economic Ladder	0.09	0.08	0.11	0.05	0.03	0.07
	(0.94)	(0.94)	(0.94)	(0.96)	(0.95)	(0.97)
Expects to be Better off Next Year	0.41	0.38	0.43	0.42	0.39	0.45
	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)
Labor Force Participation	0.33	0.34	0.32	0.86	0.96	0.74
	(0.46)	(0.47)	(0.46)	(0.35)	(0.19)	(0.44)
Married	0.67	0.83	0.48	0.38	0.60	0.12
	(0.47)	(0.38)	(0.50)	(0.48)	(0.49)	(0.32)
Age	25.51	28.72	21.82	25.57	28.71	21.90
	(4.09)	(2.28)	(2.12)	(4.08)	(2.28)	(2.22)
Observations	33,231	18,356	14,875	22,222	12,253	9,969

Notes: Means are generated using data from 2009-2014 rounds of TLSS.

order. Among men, 57% of the control group and 58% of the treatment group identified themselves as happy. Summary statistics for domains of life satisfaction indicators as well as the *CLSI* exhibit a similar pattern with that of *Happy*.

To assess whether the happiness data collected from Turkish citizens exhibit similar patterns to those obtained from other populations around the world, Appendix Figure B1 displays the age gradient of happiness by gender using the TLSS data. The U-shaped (first decreasing, then increasing) relationship between age and happiness in the sample corresponds to the commonly observed standard pattern around the globe (Cheng, Powdthavee and Oswald 2015). This consistency argues that the overall quality of the TLSS data is similar to that collected in other parts of the world, including the U.S. and Europe.[59]

Table 3.1 also shows the mean values for covariates that may potentially mediate the relationship between education and happiness as well as age. These mediators, which are *Household Income*, *Can Make Ends Meet, Married, Subjective Economic Ladder, Expects to be Better off in Near Future, Labor Force Participation, and Hopeful* are used to examine the potential channels through which education may impact happiness.[60]

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[59] The few existing articles studying SWB in Turkey focus on documenting the correlates of happiness (Selim 2008; Dumludag 2012; Caner 2014).

[60] While earnings would be an ideal measure of financial wellbeing, the TLSS does not collect wage data. Instead I use household income, which is normalized to mean zero and standard error of one. The second financial well being indicator, *Can Make Ends Meet,* takes the value of one if the respondent declares that his or her family managed to make ends meet in the previous month, zero otherwise. The binary marital status indicator, *Married*, is used to capture the role of marriage as a mediator between education and happiness. I employ the *Subjective Economic Ladder* (SEL) as a proxy of an individual's perception of relative economic standing in the society. Our short-run well being expectation variable, *Expects to be Better off Next Year,* takes the value of one if the respondent expects an improvement in material wellbeing next year, zero otherwise. Finally, our longer-run welfare expectation measure, *Hopeful*, is generated using the following survey question: "How hopeful are you about your future?" with potential answers ranging from 4 "very hopeful" to 1 " not hopeful at all." *Hopeful* is set equal to one for those who are very hopeful or hopeful about their futures, zero otherwise. Table 3.1 reveals that 80% of the women in the treatment group identify themselves as hopeful, while this rate is about 73% in the control group. For men, the rates of being hopeful are 77% and 73% in treatment and control groups, respectively. The proportions of individuals who managed to make ends meet in the previous month are slightly lower among the treated individuals compared to those who are in the control group for both genders (i.e., for females 12% vs. 14%, and for males 12% vs. 16%). While 43% of the treated women report that they expect to be better off next year, it is 38% for females who are in the control group. The corresponding rates for males who are in the treatment and control groups are 45% and 39%, respectively. Table 3.1 also shows that 33% of the women and 86% of the men are in the labor force. Among women, labor force participation rates for control and treatment groups are 32% and 34%, respectively. For men, these rates are 74% and 96% for treatment and control groups, in that order. Similar to CSLI, I standardized the household income and subjective economic ladder variables for ease of interpretation.

3.5 Empirical Methodology

I employ the following econometric specification to estimate the relationship between education and happiness:

$$H_{ict} = \beta_0 + \beta_1 T \times rcby + \beta_2 (1 - T) \times rcby + \beta_3 E_{ict} + \delta_t + \varepsilon_{ict} \qquad (3.1)$$

where H is the associated SWB measure (*Happy*, or one of the domains of life satisfaction indicators) for individual i, birth cohort c, in survey year t. The variable of interest, E_{ict}, represents *Middle School Diploma*. T is a binary variable which takes the value of one if the individual belongs to a post reform cohort, those born after 1986, zero otherwise. As previously discussed, it is not clear whether the 1986 cohort should be in the treatment or control group; hence, I omit the 1986 cohort from the benchmark analysis. Re-centered birth year, $rcby$, reflects respondents' birth year distance to 1986, which takes positive values for cohorts born after 1986, negative values for cohorts born before 1986, and δ_t is the set of survey year dummies. Finally, ε_{ict} is the error term. OLS estimates of equation (3.1) may produce biased results because of the unobserved factors that may impact both schooling and SWB (Card 2001).

To address potential endogeneity of education to SWB, this study exploits the exogenous variation in middle school graduation resulting from the 1997 education reform. More specifically, while the 1997 education reform in Turkey obliged individuals born after 1986 to complete at least 8 years of education, for those born before 1986, schooling beyond 5 years of elementary school was optional. This study uses being born after 1986 as an instrument for middle school completion in the 2SLS estimation. The first stage regression equation below captures the impact of education reform on middle school graduation:

$$E_{ict} = \omega_1 T + \omega_2 T \times rcby + \omega_3 (1 - T) \times rcby + \delta_t + \eta_{ict} \qquad (3.2)$$

where the dependent variable is *Middle School Diploma* and η_{ict} is the idiosyncratic error term. The remainder of equation (3.2) is specified analogously to equation (3.1). Parameter of interest, ω_1 captures the impact of compulsory schooling reform on holding at least a middle school degree. To account for birth cohort trends in SWB as well as schooling, I use linear approximations on both sides of 1986, which are the distance in year to cut-off birth year with respect to exposure to the education reform status. Controlling for re-centered birth year variables, $T \times rcby$ and $(1 - T) \times rcby$, should capture the trends in educational attainment and happiness at the birth cohort level (Fort, Schneeweis and Winter- Ebmer 2014).

This study uses survey weights provided by the TLSS in the analysis. Standard errors are clustered at the birth cohort level, as recommended by Lee and Card (2008) and implemented by McCrary and Royer (2011) and Fort, Schneeweis and Winter- Ebmer (2014).

Individuals born 6 years before and 6 years after the pivotal birth cohort constitutes the estimation sample. To test whether main findings are robust to data interval selection, I also present results from the estimation samples that include survey respondents who were born ∓ 5, ∓ 4, and ∓ 3 years on each side of the pivotal birth cohort of 1986. Note that the instrumental variables strategy used in this study mimics a fuzzy Regression Discontinuity Design (fuzzy RDD), where the associated discontinuity affects only a subset of individuals in the treatment group (Lee and Lemieux 2010). Hence, I also employed Imbens Kalyanaraman (IK) procedure to obtain the optimal bandwidths (Imbens and Kalyanaraman 2012).[61] As displayed in Appendix Table B2, the optimal bandwidth for *Happy* is about 6 years, and the average optimal bandwidth value for 11 SWB measures is 6.2 years for females, and 6.5 years for males, respectively.[62] Therefore, the data estimation interval windows employed in this study are consistent with the IK procedure.

The local average treatment effect (LATE) estimated in this study corresponds to the effect of holding at least a middle school diploma on SWB of individuals who would not have received this level of education in the absence of mandatory schooling reform. This effect might be different from the impact of extending schooling at different points of education distribution (e.g., high school, or college) (Clark and Royer 2013). These estimates may also differ from the effect of education on outcome measures of students who would voluntarily continue their middle school education regardless of being exposed to the education reform (Oreopoulos 2007; Clark and Royer 2013).

While the identification strategy is more suitable for an instrumental variables approach (Imbens and Lemiux 2008; Lee and Lemiux 2010), for the sake of completeness, I also present the reduced form (Intent-to-treat, ITT) estimates using the following model:

$$H_{ict} = \gamma_0 + \gamma_1 T + \gamma_2 T \times rcby + \gamma_3 (1 - T) \times rcby + \delta_t + \epsilon_{ict} \qquad (3.3)$$

where, γ_1 represents the impact of the 1997 education reform (i.e., intent-to-treat effect), ϵ_{ict} stands for the error term, and all other covariates are defined as previously introduced.

[61] Optimal bandwidth selection is accomplished using the rdbwselect Stata command described in Calonico, Cattaneo and Titiunik (2014).

[62] Because birth date information is only available in integer values (i.e., birth year), data estimation windows are defined accordingly.

Table 3.2

The OLS Estimates of Happiness and Domains of Life Satisfaction Indicators on Having at Least a Middle School Degree

	(1) Happy	(2) Satisfied With Earnings	(3) Satisfied With HH Income	(4) Satisfied With Marriage	(5) Satisfied With Health	(6) Satisfied With Friends	(7) Satisfied With Job	(8) Satisfied With Neighborhood	(9) Satisfied With Housing	(10) Satisfied With Family Rel.	(11) Composite LS Index
Panel A: Females											
Middle School	0.045***	0.090***	0.120***	0.030**	0.055***	0.016	0.098***	-0.039***	0.079***	-0.026*	0.228***
Diploma	(0.011)	(0.025)	(0.01)	(0.01)	(0.014)	(0.009)	(0.031)	(0.011)	(0.011)	(0.012)	(0.026)
Observations	33,231	7,356	33,231	23,954	33,231	33,231	7,852	33,231	33,231	33,231	33,231
Panel B: Males											
Middle School	0.072**	0.094***	0.148***	0.004	0.026	0.014*	0.065***	-0.032	0.082***	-0.031*	0.232***
Diploma	(0.023)	(0.021)	(0.019)	(0.008)	(0.016)	(0.007)	(0.021)	(0.018)	(0.023)	(0.017)	(0.035)
Observations	22,222	16,299	22,222	11,327	22,222	22,222	16500	22,222	22,222	22,222	22,222

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

3.6 Main Results

Table 3.2 presents the OLS estimates of happiness and the measures for domains of life satisfaction on having at least a middle school degree. In column (1) of Panel A, female respondents who hold at least a middle school degree are 4.5-percentage points more likely to be happy than those who do not have a middle school degree. In Panel B, males who have at least a middle school degree are 7.2-percentage points more likely to be happy in comparison to those who do not have a middle school diploma. I observe a similar pattern of results for the majority of the domains of life satisfaction measures. The coefficient on middle school diploma is positive for 9 out of 11 life domain satisfaction indicators for both genders. Furthermore, holding at least a middle school diploma is associated with a 0.228 and 0.232 standard deviations increase in the *CLSI* for females and males, respectively.

These estimates show that having at least 8 years of schooling is associated with increased happiness and life satisfaction for both genders. While these results are intuitive, they may be biased due to omitted factors that may simultaneously determine education and SWB. Therefore, the current study employs an instrumental variables estimation strategy to address the endogeneity of schooling to SWB.

Figure 3.1shows trends in holding at least a middle school degree for those born in the period 1980-92 for females and males, respectively. These figures reveal trends in middle school completion rates enjoyed an unambiguous jump among the birth cohorts who were born after 1986. More specifically, for women who were born between 1980 and 1985, having at least a middle school degree reaches the 50% level in 1985, and then jumps to 70% in 1987, the year in which the first directly affected individuals were born. A similar pattern can also be seen for males in Figure 1B. While the proportion of men having at a least a middle school degree is about 75% for the 1985 birth cohort, this ratio climbs up to 90% for males who were born in 1987 and reaches the 95% level for the 1992 birth cohort. The data thus argue strongly that the reform (i.e., being born after 1986) drove an increase in middle school graduation rates, especially for women.[63]

As noted before, the validity of the empirical design hinges upon the assumption that individuals who were born a few years before and after the 1986 cutoff are comparable to each other. While the TLSS data do not include an exhaustively rich set of information on predetermined observable characteristics, following upon Clark and Royer

[63] Note that these compliance rates are similar to the ones observed in a number of European countries (Brunello, Fabbri, and Fort 2013; Fort, Schneeweis and Winter-Ebmer 2014).

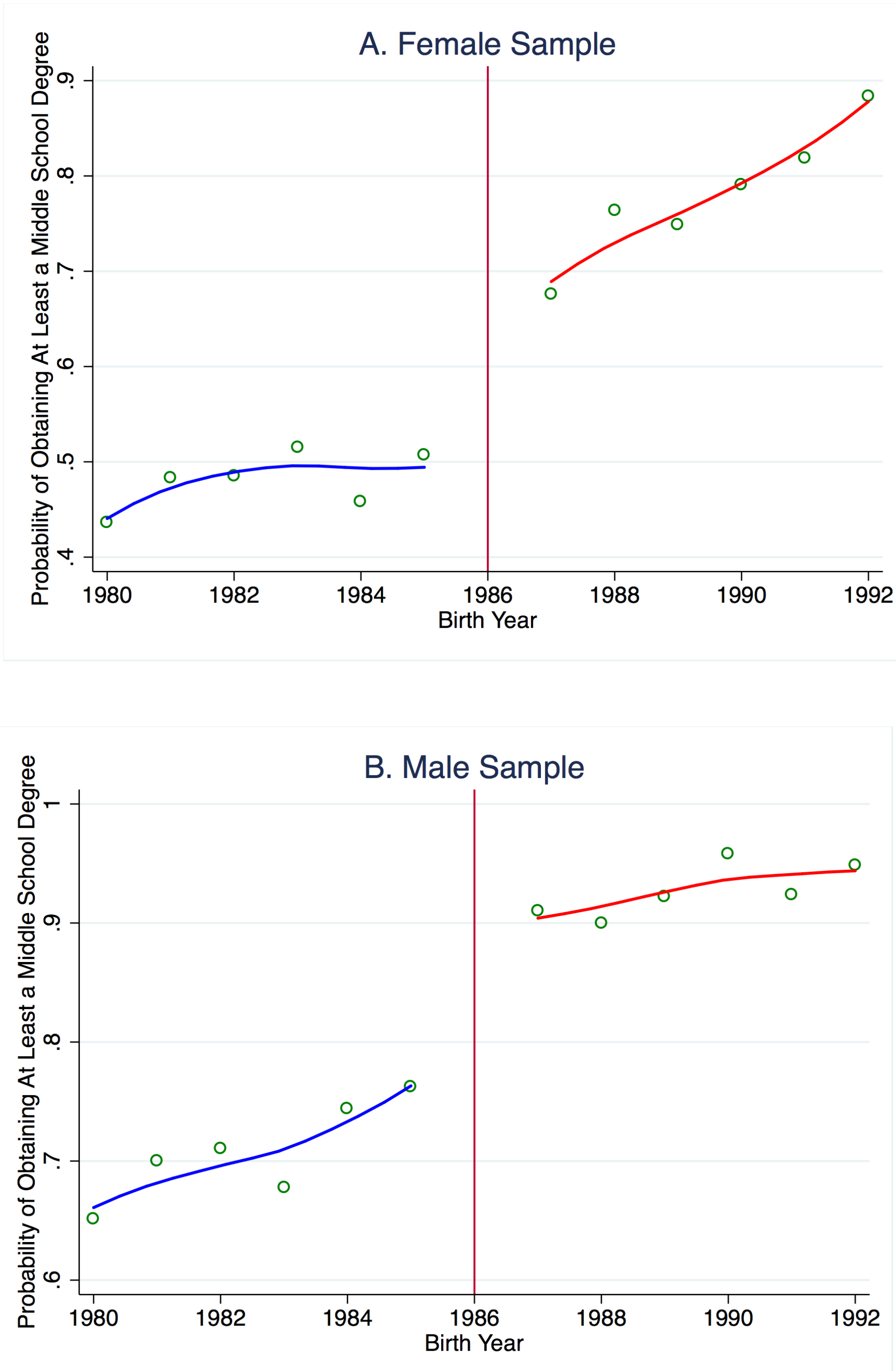

Figure 3.1: Middle School Completion Rates by Birth Cohorts and Gender

Table 3.3
The Impact of Reform on Having at Least a Middle School Degree

	(1) 80-92	(2) 81-91	(3) 82-90	(4) 83-89
		Panel A: Females		
Reform	0.147***	0.172***	0.170***	0.171***
	(0.031)	(0.035)	(0.038)	(0.045)
Observations	33,231	27,870	22,207	16,756
		Panel B: Males		
Reform	0.124***	0.129***	0.104***	0.090***
	(0.016)	(0.018)	(0.021)	(0.02)
Observations	22,222	18,639	14,685	11,090

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

(2013) I employ cohort size and urban variables to test the validity of this assumption. As Appendix Figure B2 shows, there are no apparent jumps in the averages of these predetermined characteristics by birth year for women, and men. Moreover, I implement the McCrary test (McCrary 2008) to see if the density of the running variable is continuous at the 1986 cutoff. Appendix Figure B3 presents the visual representation of this test, which yields an insignificant jump at the 1986 cutoff.[64] These figures provide evidence reinforcing the validity of the identification strategy.

Table 3.3, which displays the estimates of equation (3.2), formally evaluates the effect of compulsory schooling reform on the likelihood of holding at least a middle school diploma for women (Panel A) and for men (Panel B). Among women, the effect of being subject to the education reform on middle school completion varies between 0.147 (column 1) and 0.171 (column 4). The corresponding effect sizes for men range between 0.124 (column 1) and 0.09 (column 4).[65] More specifically, in column (1), being exposed to the education reform of 1997 causes a 14.7 (Panel A) and 12.4 (Panel B) percentage points increase in the probability of holding at least a middle school diploma for women and men, respectively.[66]

[64] The p-value of the estimated discontinuity is 0.206.
[65] These first stage estimates are consistent with previous research studying the effects of the 1997 education reform in Turkey (Torun 2013; Cesur and Mocan 2013; Cesur, Dursun and Mocan 2014; Gunes 2015, 2016).
[66] In Appendix Table B1, I also examine whether the 1997 education reform induced individuals to continue their schooling beyond middle school. For females, the results presented in columns (1) to (4) show that the education reform has a positive impact on the likelihood of holding at least a high school degree (Panel A), and holding at least

Table 3.4
The Impact of Having at Least a Middle School Degree on Being Happy
Instrumental Variable Estimates

	(1)	(2)	(3)	(4)
	80-92	81-91	82-90	83-89
	Panel A: Females			
Middle School	0.281***	0.246**	0.427***	0.289***
Diploma	(0.069)	(0.097)	(0.04)	(0.027)
Observations	33,231	27,870	22,207	16,756
First Stage F-test	22.83	24.4	20.14	14.72
	Panel B: Males			
Middle School	-0.275	-0.163	-0.299	-0.578
Diploma	(0.38)	(0.365)	(0.484)	(0.491)
Observations	22,222	18,639	14,685	11,090
First Stage F-test	62.59	49.96	25.32	19.6

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Figures 2A and B display the visual descriptions of the link between the 1997 education reform and the likelihood of being happy for the female, and male samples, respectively. Figure 3.2A reveals an unambiguous increase in the propensity of happiness among women who were born after 1986. Nevertheless, Figure 3.2B reveals that there is no similar discontinuity in male happiness rates. Panels A and B of Table 3.4 show the IV estimates of the impact of holding at least a middle school degree on happiness of women and men, in that order. Note that the instrumental variable employed in this study satisfies power requirements because the first-stage F-statistic is at least 14 and greater than 20 in most cases.

Among women, column (1), in the largest data interval period, including birth cohorts 1980 to 1992, holding at least a middle school diploma leads to a 28.1-percentage point increase in the probability of being happy. When estimation window narrowed down to ∓5, ∓ 4, and ∓ 3 years on each side of the pivotal birth cohort, the estimated coefficients are 0.246, 0.427, and 0.289, shown in columns (2) to (4).

a college degree (Panel B). However, the education reform does not seem to impact the educational attainment of males (columns 5 to 8) beyond holding a middle school diploma. These patterns of results are consistent with a number of other compulsory schooling reforms in different countries, which cause a greater increase in women's schooling in comparison to that of men (Brunello, Fort and Weber 2009; Devereux and Hart 2010; Brunello, Fabbri and Fort 2013; Gathmann, Jurges, and Reinhold 2014).

Figure 3.2: The Impact of Reform on Probability of Being Happy

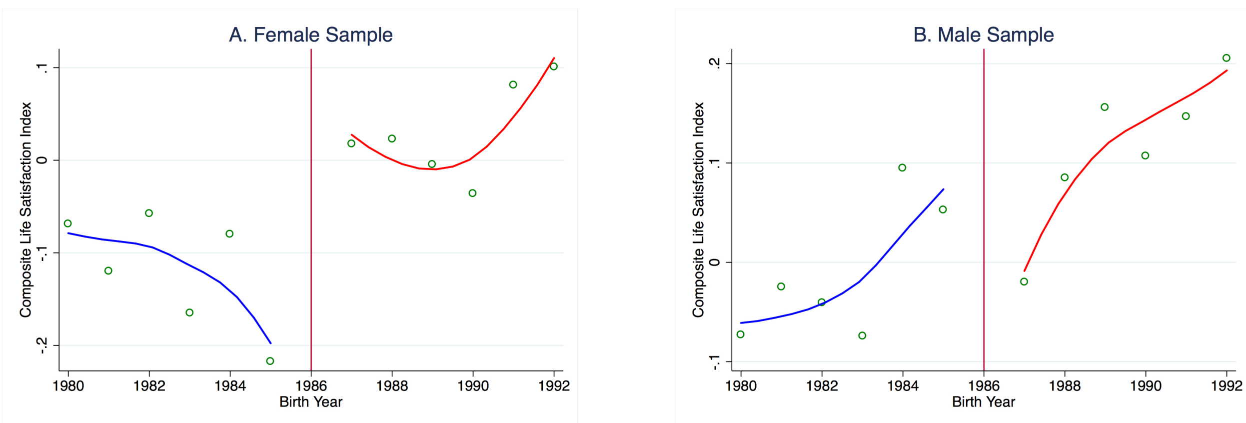

Figure 3.3: The Impact of Reform on Composite Life Satisfaction Index

Table 3.5
The Impact of Having at Least a Middle School Degree on Domains of Life Satisfaction
Instrumental Variable Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Satisfied With Earnings	Satisfied With HH Income	Satisfied With Marriage	Satisfied With Health	Satisfied With Friends	Satisfied With Job	Satisfied With Neighborhood	Satisfied With Housing	Satisfied With Family Rel.	Composite LS Index
Panel A: Females										
Middle School	0.888***	0.202	0.271**	0.286**	0.225	0.578**	0.327	0.382***	0.225**	1.148**
Diploma	(0.294)	(0.342)	(0.114)	(0.144)	(0.162)	(0.252)	(0.259)	(0.106)	(0.097)	(0.511)
Observations	7,356	33,231	23,954	33,231	33,231	7,852	33,231	33,231	33,231	33,231
First Stage F-test	8.756	22.83	18.4	22.83	22.83	15.07	22.83	22.83	22.83	22.83
Panel B: Males										
Middle School	-0.485*	-0.304	-0.212***	-0.278**	-0.333***	0.176*	-0.087	-0.512***	0.319**	-0.830*
Diploma	(0.286)	(0.228)	(0.056)	(0.115)	(0.077)	(0.106)	(0.138)	(0.129)	(0.143)	(0.442)
Observations	16,299	22,222	11,327	22,222	22,222	16,500	22,222	22,222	22,222	22,222
First Stage F-test	47.37	62.59	73.02	62.59	62.59	48.95	62.59	62.59	62.59	62.59

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

For males, Panel B of Table 3.4 demonstrates that the effect of obtaining at least a middle school degree on happiness is negative, but statistically insignificant for all estimation intervals. Nevertheless, while these coefficients estimates are not precise, they are relatively large. For instance, in column (1), holding at least a junior school diploma leads to a (statistically insignificant) 27.5 percentage point decline in the likelihood of being happy.

Next, I estimate the impact of middle school education on the probability of being satisfied with various life domains as well the *CLSI*. Figure 3.3A graphically illustrates the relationship between the 1997 schooling reform and the *CLSI* and shows that exposure to the reform is positively related to overall female life satisfaction. Conversely, Figure 3.3B reveals that men who were born after 1986 experience a negative shock to their overall life satisfaction. Table 3.5 presents instrumental variables estimates of the impact of holding at least a middle school diploma on the probability of being satisfied with various life domains and the *CLSI*. To economize on space, starting with Table 3.5, I present the estimates using the sample of survey respondents who were born 6 years before and after the pivotal birth cohort of 1986.[67]

Panel A of Table 3.5 confirms women who have at least a middle school degree are more likely to be satisfied with every life domain at hand; estimated coefficients are statistically significant at the 5 percent level in 7 out of 10 cases. Specifically, *Middle School Diploma* causes women to be more satisfied with their earnings, marriage, health, job, house, and the relationships with their family. Column (10) presents the overall effect of middle school education on the *CLSI*. Women who hold at least a middle school degree experience a 1.148 standard deviation increase in the *CLSI*. In contrast, Panel B of Table 3.5, for males, reveals that holding at least a middle school diploma has a negative and statistically significant impact on 6 of the 10 life satisfaction measures. While the estimated impacts of *Middle School Diploma* on job satisfaction, and relationships with family and relatives is positive, the effect of middle school education on overall male life satisfaction is negative and statistically significant. That is, as shown in column (10), having at least a middle school degree lowers the *CLSI* by a 0.83 standard deviation.

Note that the IV estimates of SWB measures on middle school diploma are larger than the OLS estimates. This is consistent with the view that the local average treatment effect estimated in this study may correspond to the

[67] As shown in Appendix Table B10, results using all estimation intervals for the remaining tables are very similar to the 6-year before/after estimation window.

impact of additional schooling on happiness among people with the lowest propensity to attend middle school (Card 2001).

3.7 Robustness

This section performs several robustness checks to test the sensitivity of main findings. First, I estimate the impact of the 1997 mandatory education reform on SWB measures in Table 3.6. These intent-to-treat (i.e., reduced form) estimates imply a similar relationship between *Middle School Diploma* and SWB indicators with those documented by the structural parameters (i.e., the instrumental variable estimates).[68, 69]

As previously stated, the 1997 education reform provides an empirical framework, consistent with a fuzzy RDD estimation strategy (Lee and Lemieux 2010). Therefore, in Appendix Table B2, I test whether the findings are robust to employing a more "formal" computation procedure developed by Nichols (2007) using the optimal bandwidths suggested by Imbens and Kalyanaraman (2012). As shown in Appendix Table B2, both the linear RD instrumental variable and the local polynomial RD results, obtained using the estimation procedure offered by Nichols (2007), are qualitatively and quantitatively similar to baseline findings. Next, to test whether main findings hold when I use an alternative specification, I employed an instrumental variables ordered-probit method, which uses maximum likelihood estimation techniques. Again, these estimates, presented in Appendix Table B3, produce results consistent with baseline findings.

Because it is not clear whether individuals born in 1986 were fully exposed to the 1997 education reform, I excluded them from baseline estimates. Panel A of Appendix Table B4 investigates the sensitivity of the effect of holding at least a middle school degree on SWB measures to inclusion of the 1986 birth cohort by assigning it a 25% chance of receiving treatment for females (in columns 1 to 4) and males (in columns 5 to 8). These findings are similar to baseline estimates. In unreported specifications that are available from the authors upon request, I also tested whether main findings change when alternative treatment values assigned for the birth cohort of 1986 resulting from exposure to the education reform status (such as 0, 0.33, and 0.50). These exercises did not change the main findings.

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[68] Dividing the reduced form effect of the 1997 education reform by the corresponding first stage impact produces identical coefficients to the ones resulted from the IV estimates, which are shown in Tables 5 and 6.

[69] A glance over the results shown in Tables 2, 5, and 6 suggest that the *CLSI* is a good proxy measure for overall life satisfaction. Hence, in the remaining tables, we only show estimates of the *CLSI* with the interest of saving space. The results using individual domains of life satisfaction indicators lead to similar conclusions and are available upon request.

Table 3.6
The Impact of Reform on Being Happy and Domains of Life Satisfaction
Reduced Form Estimates

	(1) Happy	(2) Satisfied With Earnings	(3) Satisfied With HH Income	(4) Satisfied With Marriage	(5) Satisfied With Health	(6) Satisfied With Friends	(7) Satisfied With Job	(8) Satisfied With Neighborhood	(9) Satisfied With Housing	(10) Satisfied With Family Rel.	(11) Composite LS Index
Panel A: Females											
Reform	0.041**	0.091***	0.03	0.046**	0.042**	0.033	0.075**	0.048	0.056***	0.033***	0.169**
	(0.016)	(0.019)	(0.056)	(0.015)	(0.015)	(0.02)	(0.033)	(0.032)	(0.008)	(0.01)	(0.055)
Observations	33,231	7,356	33,231	23,954	33,231	33,231	7,852	33,231	33,231	33,231	33,231
Panel B: Males											
Reform	-0.034	-0.064*	-0.038	-0.033***	-0.035**	-0.041***	0.023	-0.011	-0.064***	0.040*	-0.103*
	(0.052)	(0.035)	(0.03)	(0.009)	(0.015)	(0.007)	(0.015)	(0.017)	(0.012)	(0.019)	(0.054)
Observations	22,222	16,299	22,222	11,327	22,222	22,222	16,500	22,222	22,222	22,222	22,222

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

As happiness of young individuals may exhibit dramatic changes within just a few years, failing to separate the age effects from trends in SWB at the birth cohort level may bias estimates. Although jointly controlling for birth cohort trends and survey year dummies should capture age effects, we further test if this possibility causes a bias in estimates of happiness on middle school education in Panel B of Appendix Table B4 by controlling for binary age indicators in addition to the baseline control variables. Controlling for age dummies does not cause a change in our main findings. We also test if estimates of SWB indicators are sensitive to controlling for quadratic birth cohort trends instead of specifying differential linear birth year trends on each side of the pivotal birth cohort. As displayed in Panel C of Appendix Table B4, these results simply mimic baseline findings.

Because TLSS interviews individuals at least 18 years of age, surveys conducted between 2003 and 2008 have a relatively small number of individuals who are in the treatment group (i.e., born after 1986). Therefore, I prefer to limit our estimation sample to the period 2009-14, which generates a similar number of individuals in the control and treatment groups, respectively. As Panel D of Appendix Table B4 shows, including all available rounds of the TLSS data in our estimation sample does not change our baseline findings.

Note that the 1997 education reform increased the minimum years of schooling from 5 to 8 years. Although it is possible that being exposed to the education reform may increase some people's schooling beyond the mandated middle school completion levels, the education reform mainly targeted populations who would stop schooling upon completing a 5-year elementary school. The estimates, shown in Appendix Table B1, provide evidence consistent with this view. I find that the reform induced women to continue their schooling beyond middle school. Therefore, I explore whether and to what degree the baseline estimates of SWB measures differ when the estimation sample is limited to those with at most a middle school diploma in Panel E of Appendix Table B4. This exercise may reveal the impact of middle school education on happiness of the marginal individual who would not otherwise pursue a middle school education. Despite significant reductions in sample sizes, the estimates obtained from the at-most-middle-school sample are statistically significant, not only for the female sample but also for the male sample and larger in magnitude. These findings suggest that the baseline findings are mainly driven by the effects of middle school education among individuals who would have terminated their formal education after receiving an elementary school diploma in the absence of the 1997 education reform. In Appendix Table B5, I repeat the sensitivity tests shown in Appendix Table B4 for the *CLSI* instead of *Happy*. These robustness checks show that estimates of *CLSI* are robust to the use of different subsamples and alternative control variables as well.

Furthermore, in the at-most-middle-school sample, the point estimates are greater for both genders in comparison to baseline estimates and statistically significant in all cases.

I constructed the estimation samples in the main analysis by placing restrictions on the birth year of respondents. Other studies used age restrictions in choosing their estimation samples in studying the impact of education on wages (Torun 2013; Mocan 2014) and child health (Gunes 2015) by relying on the same compulsory schooling reform used in this study. Therefore, Appendix Table B6, re-estimates the happiness and continuous life satisfaction index models with samples, based on different age limitations, including ages 18 to 34, 19 to 33, 20 to 32, 21 to 31, 22 to 30, and 23 to 29. The estimates of *Happy* and the *CLSI* obtained from the samples based on different age limitations are similar to the benchmark estimates.

Although clustering at the region-by-birth-year level may be more preferred because it would allow eliminating the province-by-birth-cohort specific spatial correlations, I am not able to cluster at the region-by-birth-year because such information is not available in the TLSS survey. Therefore, clustering at the birth-year level may lead to biased estimates of standard errors due to few cluster units. As Cameron, Gelbach and Miller (2008) suggest, I perform a wild-cluster bootstrapping method to test the sensitivity of the results. Appendix Table B7 reports the p-values obtained from wild bootstrapping exercises for both the reduced form and instrumental variable regressions.[70] The estimates indicate that inference based on these standard errors lead to conclusions similar to the main estimates: precisely estimated effects among women and imprecisely estimated coefficients among men.

In addition to testing the robustness of the main findings, it would be informative to examine the effects of extended primary schooling among different subgroups, including different ethnicities, as well as different geographic regions. Because the TLSS does not ask the ethnicity of survey respondents, and geographic identifiers are not available except in the 2013 survey, I am not able to examine the potential subsample heterogeneity based on ethnicity and geographic location. Nevertheless, the TLSS provides information on urban versus rural living status of survey respondents prior to 2013. Hence, this study separately estimates the impact of *Middle School Diploma* on SWB measures for urban and rural samples using data for the period 2009 and 2012. The results, shown in Appendix Table B8, are qualitatively similar to baseline estimates even though they are less precisely estimated

[70] We follow Zimmerman, (2014) for obtaining the p-values for instrumental variable estimates. I use Rademacher weights and perform 1999 repetitions in analysis.

because of dramatic reductions in sample sizes. Furthermore, estimates of the impact of *Middle School Diploma* on SWB measures are usually similar in urban and rural subsamples.

3.8 Mechanisms

Next, I explore the potential mechanisms that link extended primary schooling to SWB. Existing literature offers a number of pathways through which extended primary schooling may impact happiness, including financial wellbeing, marital status, perceived relative economic standing in the society, labor force participation, and expectations pertaining to future well being.

I begin by examining the impact of holding at least a middle school diploma on a number of potential mediators available in the TLSS to test whether middle school education affects the potential sources of happiness. Panel A of Table 3.7 displays the instrumental variables estimates of the impact of holding at least a middle school diploma on these potential mediators for women. Middle school completion leads to a statistically significant increase in standardized household income, the likelihood of being able to meet the ends, labor force participation, and probability of being hopeful. Middle school education also lowers the probability of being married for females. Among males, in Panel B, middle school education has a negative and statistically significant effect on perceived relative economic standing in the society, the likelihood of being married, and the probability of being hopeful about future well being. It is worth mentioning that the results presented in Table 3.7 are in line with recent literature testing effects of extended primary schooling on different outcomes using the 1997 Education Reform as the source of identifying variation. That is, extended schooling due to this reform in general has beneficial effects on women's outcomes while I am not aware of any studies documenting favorable effects of holding at least a middle school diploma among males.[71] I descriptively explore whether and to what degree these covariates mediate the impact of middle school education on *Happy* and the *CLSI* in Table 3.8. In row (1) Panel A, among women, controlling for *Hope* reduces the coefficient on *Middle School Diploma* by roughly 27 percent. On the other hand, controlling for

[71] For instance, while holding at least a middle school diploma causes an increase in female wages, it has no effect on male earnings (Torun 2013; Mocan 2014). Cesur and Mocan (2013) show that holding a middle school diploma causes a decline in female religiosity while it has no effect on male religiosity. Dursun and Cesur (2016) show that mother's extended primary schooling has a negative effect on child mortality. Additionally, Gunes (2015) finds that increased maternal schooling has favorable effects on the health of their children. Finally, Cesur, Dursun and Mocan (2014) show that holding at least a middle school diploma caused an increase in the likelihood of being overweight among men.

Table 3.7
The Impact of Having at Least a Middle School Degree on Potential Mediators
Instrumental Variable Regressions

	(1) Standardized Household Income	(2) Can Make Ends Meet	(3) Married	(4) Standardized Subjective Econ. Ladder	(5) Expects to be Better off Next Year	(6) Labor Force Participation	(7) Hopeful
Panel A: Females							
Middle School Diploma	0.768*** (0.192)	0.241*** (0.091)	-0.375* (0.194)	-0.074 (0.275)	0.045 (0.16)	0.272*** (0.067)	0.222*** (0.083)
Observations	32,149	32,149	33,231	33,231	33,231	33,231	33,231
First Stage F-test	18.18	18.18	22.83	22.83	22.83	22.83	22.83
Panel B: Males							
Middle School Diploma	-0.384 (0.268)	-0.087 (0.182)	-0.501*** (0.19)	-1.209** (0.47)	0.021 (0.152)	0.232 (0.284)	-0.478** (0.207)
Observations	21,357	21,357	22,221	22,222	22,222	22,222	22,222
First Stage F-test	49.54	49.54	61.56	62.59	62.59	62.59	62.59

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

being currently married and labor force participation leads to a 30 percent and 13 percent increase in the magnitude of the estimated coefficient, respectively. I observe a similar pattern of results when *Happy* replaced with the *CLSI* in row (2). For young women, these results suggest that while being hopeful about one's own future well being due to extended schooling favorably impacts SWB, a reduction in the probability of being married as well as an increase in labor force participation rate seem to have the opposite effect on happiness.

Panel B of Table 3.8 repeats the same exercise for the male sample. Although the effect of middle school completion on *Happy* is not statistically significant for males, I examine how the point estimates change when potential mediators controlled. In row (1), results show that controlling for marital status, standardized perceived economic standing in society, and hope causes a reduction in the coefficient on *Middle School Diploma* by roughly 29, 52, and 66 percent, respectively. Row (2), explores the role of these potential mediators between middle school education and *CLSI*. When I control for *SEL* (in column 5) and *Hopeful* (in column 7), the effect of having at least a middle school diploma is reduced by 48 to 40 percent, and the estimated coefficients become statistically insignificant.[72]

[72] In Appendix Table B9, we explore the role of potential mediators between middle school education and SWB for the at most middle school sample. This exercise produces a pattern of results similar to those Table 3.8 presents.

Table 3.8
Controlling for Potential Mediators
Instrumental Variable Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Females								
	Estimates of Happy							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School	0.281***	0.287***	0.256***	0.364***	0.285***	0.276***	0.319***	0.205***
Diploma	(0.069)	(0.084)	(0.075)	(0.095)	(0.058)	(0.065)	(0.079)	(0.063)
First Stage F-test	22.83	23.98	22.73	23.13	26.32	25.52	22.72	22.79
Observations	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]
	Estimates of Composite Life Satisfaction Indicator							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School	1.148**	1.131*	1.074**	1.229**	1.161**	1.138**	1.242**	0.987*
Diploma	(0.511)	(0.59)	(0.521)	(0.533)	(0.507)	(0.536)	(0.542)	(0.512)
First Stage F-test	22.83	23.98	22.73	23.13	26.32	25.52	22.72	22.79
Observations	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]	[33,231]
Panel B: Males								
	Estimates of Happy							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School	-0.275	-0.24	-0.252	-0.195	-0.131	-0.279	-0.269	-0.093
Diploma	(0.38)	(0.354)	(0.361)	(0.415)	(0.381)	(0.363)	(0.381)	(0.332)
First Stage F-test	62.59	68.29	59	52.24	95.22	59.56	72.69	67.64
Observations	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]
	Estimates of Composite Life Satisfaction Indicator							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School	-0.830*	-0.722*	-0.771*	-0.837*	-0.429	-0.838*	-0.807*	-0.497
Diploma	(0.442)	(0.388)	(0.440)	(0.446)	(0.366)	(0.432)	(0.450)	(0.315)
First Stage F-test	62.59	68.29	59	52.24	95.22	59.56	72.69	67.64
Observations	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]	[22,222]
Controls For								
Standardized Household Income		Yes	No	No	No	No	No	No
Can Make Ends Meet		No	Yes	No	No	No	No	No
Married		No	No	Yes	No	No	No	No
Standardized Subjective Econ. Ladder		No	No	No	Yes	No	No	No
Expects to be Better off Next Year		No	No	No	No	Yes	No	No
Labor Force Participation		No	No	No	No	No	Yes	No
Hopeful		No	No	No	No	No	No	Yes

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

3.9 Discussion and Conclusion

A number of researchers in economics as well as other fields in social sciences have recently become more interested in subjective well being to address inadequacies in standard economic indicators as measures of human welfare (Frey and Stutzer 2002a; Fleurbaey 2009; Stiglitz, Sen and Fitoussi 2009).

This article contributes to the emerging literature on economics of happiness by providing the first causal estimates of the effect of extended primary schooling on happiness among young adults (ages 18 to 34), using data for the period 2009 to 2014 drawn from Life Satisfaction Surveys the Turkish Statistical Institute conducted.

The ordinary least squares estimates show that men and women who hold at least a middle school diploma are more likely to declare themselves as happy, and be more satisfied with various aspects of their lives in comparison to those whose educational attainment level is below a middle school degree.

To test whether the observed associations between happiness and middle school education is causal or associational, I make use of an education reform, enacted in the Republic of Turkey. The compulsory schooling reform of 1997 increased the minimum educational attainment requirement from completing a 5-year elementary school to an 8-year middle school for the birth cohorts born after 1986. I document that the requirements of the schooling reform led to sizable increases in the middle school completion probabilities of both genders.

The instrumental variables estimates show that holding at least a middle school diploma leads to a significant increase in happiness for young women. Furthermore, this study finds that for women middle school education increases the chances of being satisfied with various aspects of their lives. To understand the nature of the effect of extending formal schooling on SWB, I descriptively explore potential pathways between the two. This analysis reveals informative patterns. That is, while the overall effect of holding at least a middle school diploma on female happiness is positive, middle school education seems to impact happiness through multiple and possibly conflicting mechanisms. For instance, although middle school education impacts happiness positively via an increase in being hopeful of one's own future well being, it seems to have a negative effect on SWB through a reduction in the probability of being married. I also find the overall positive effect among women intuitive because, using the education reform I employ as the source of exogenous variation in educational attainment, a number of studies document that middle school education has favorable effects on women. For example, holding at least a middle school degree causes a wage premium (Torun 2013; Mocan 2014), an increase in the probability of having children with better health outcomes (Gunes 2015; Dursun and Cesur 2016), a decline in religiosity, and the

tendency to vote for political parties with religious affiliations (Cesur and Mocan 2013), and an increase in women's empowerment (Gulesci and Meyersson 2015). Furthermore, the finding that extending primary schooling imposes a negative pressure on SWB through lowered marriage probability is in line with a number of studies documenting that married individuals are more likely to be happy (Frey and Stutzer 2002b; Blanchflower and Oswald 2004; Stevenson and Wolfers 2009). These findings also support the view that education may impact women's happiness through empowerment. The fact that the results seem to be driven by the effects of increased educational attainment among those having the lowest likelihood of attending middle school in the absence of the schooling reform of 1997 provides further support for this argument.

On the other hand, I do not find evidence supporting the view that extended primary schooling improves the SWB of young men. Indeed, the estimates show that holding at least a middle school diploma has an imprecisely estimated negative impact on the likelihood of being happy. Furthermore, middle school completion has a negative and statistically significant effect on the probability of being satisfied with different aspects of their lives as well as the *CLSI*. Supplemental analyses reveal that a reduction in the probability of being married, decline in perceived economic status in society, and lowered hopes pertaining to longer-run well being explain 29, 52, and 66 percent of the variation between education and SWB for men. These results are consistent with literature defining happiness as a balance between aspirations and attainments (de la Croix 1998; March and Simon 1968; Solberg et al. 2002). More specifically, although holding a middle school degree does not affect the wages of males (Torun 2013; Mocan 2014), I show that it leads to a lower likelihood of being content with earnings. In light of these findings, I conjecture that the likely divergence between achievements and aspirations may be the reason behind the negative impact of middle school education on male subjective well being.

The gender specific differential findings in favor of women also support predictions of the Resource Substitution Theory (RST), which hypothesizes individuals with few alternative resources (i.e., women) may benefit more from extended schooling in comparison to those who have access to greater resources (i.e., men) (Ross and Mirowski 2006, 2010, and 2011; Van de Velde Bracke and Levecque 2010; Ross, Masters and Hummer 2012).

These findings also document that favorable future well being expectations, i.e., hope, may have a direct causal effect on current SWB for both genders (Snyder et al. 2002; Bailey et al. 2007), which is also consistent with the notion that individuals have low long-run discount rates (Giglio et al. 2015).

There may be several limitations to the current study. For instance, these findings may not be applicable to the effects of extended schooling at different stages of educational attainment (e.g., elementary school or college). Additionally, the observed effects among young adults may not necessarily persist into older age; thus, future work examining the effects of holding a middle school diploma on SWB of older individuals may produce valuable information.

What educational policy implications do these estimates have? The findings for women have a clear policy conclusion: benefits of policies that aim to increase women's schooling, especially at the lower tail of educational attainment distribution, may extend beyond the expected monetary pay-offs; thus, policy makers should incorporate consideration of non-pecuniary benefits of women's education in deciding schooling investment. Nevertheless, the estimates presented for men raise a number of questions. Although it is beyond the scope of this study, the fact that men with the lowest propensity of extending their primary schooling seem to bear the misery of holding at least a middle school diploma requires further explanations, and that, extended schooling, resulting from the 1997 education reform, has no beneficial impacts among males as a group. There may be several explanations behind these contradictory findings, including the possibility that an increase in the supply of educated women may crowd-out males from employment in formal sector. Hence, future work explaining why middle school education, induced by the 1997 education reform, does not have any favorable effects among men may produce valuable insights into social and economic dynamics.

Finally, are these estimates relevant to those who live in other developing countries? All in all, the results imply that increased educational attainment may promote the happiness of individuals living in other countries to the degree that it favorably affects the potential sources of happiness.

CHAPTER 4: THE IMPACT OF EDUCATION ON HEALTH AND HEALTH BEHAVIORS IN A MIDDLE-INCOME, LOW-EDUCATION COUNTRY

4.1 Introduction

Formal schooling may influence a person's health trough different channels. According to Grossman's health capital model, which created the theoretical foundation for the analysis of the demand for health (Grossman 1972a, 1972b), education may impact a person's health through productive and allocative efficiency. While productive efficiency explains an individual's ability to "produce" better health with a given set of health inputs, allocative efficiency implies that more educated people produce better health outcomes because they choose optimal input allocations in comparison to those who are less educated. Specifically, education allows a person to acquire more information about the impacts of health inputs (medical care, cigarettes, exercise, and so on), altering the consumption of these inputs and health behaviors, which ultimately may affect health. Education can also determine health via long-term orientation (Becker and Mulligan 1997), earnings (Card 2001, Oreopoulos 2006, Mocan 2014), and occupational choice (Kelly et al. 2011). If more schooling lowers people's discount rates, educated individuals may invest more in their health as they gain greater utility from future consumption. Even though higher earnings may enable one to afford health enhancing goods and services (e.g., organic foods, and gym membership) it may also increase the demand for health depreciating normal goods (e.g., smoking and drinking). Likewise, while educated individuals may afford avoiding being employed in occupations that put extreme strain on a person's health, they may be more likely to take jobs, which induce a sedentary life style. Extended schooling may also delay the age of labor market entry, which may in turn have direct health effects (Brunello et al. 2009; Boccanfuso et al. 2015).

In addition to health knowledge and economic outcomes, a change in the composition of peers, both during schooling and after graduation, because of extended formal schooling, may have potential effects on health and health behaviors (Akerlof and Kranton, 2002; Jensen and Lleras-Muney, 2012). If the new set of peers and or influence groups (e.g., schoolmates, colleagues, and friends after completing school) undertake health-enhancing behaviors (e.g., improved diet), education may have a favorable effect on health and health promoting behaviors, and vice versa. Finally, education can affect a person's health via time use as it may impact how she allocates her time between various activities (Oreopoulos and Salvanes 2011). Therefore, given that schooling may impact health

and health behaviors through multiple and potentially contradictory pathways, the net effect of education on health may be ex-ante uncertain.

Literature based on correlational evidence provides a well-documented positive relationship between education and health outcomes, regardless of whether the focus is on individuals (using data on self-reported health, sick days, etc.) or aggregate units (such as country or state level mortality or morbidity rates) (Grossman 2006). Identification of the causal impact of education on health, however, is complicated because unobservable attributes of individuals may jointly influence their schooling, and health related behaviors and outcomes. Examples of such difficult-to-observe attributes include genetic make-up, socio-economic status, time preferences and intelligence. A further complication arises because of potential reverse causality, i.e., poor health may cause lower educational attainment, producing a positive correlation between education and health.

Therefore, a number of papers exploited different natural experiments to investigate the extent to which education has a causal impact on own health both in developed and developing countries.[73] However, the literature on whether education causally impacts own health around the globe is far from being settled. Evidence from developed countries offers mixed findings on how education impacts health. For example, de Walque (2007) used an instrumental variable approach based on the idea that college attendance could have served as a strategy for draft avoidance in the U.S. during the Vietnam War. His results showed that more education, due to exemption from military service, had a negative impact on smoking. Using exposure to compulsory education laws in the United States from 1915 to 1939, Lleras-Muney (2005) reported a negative impact of education on mortality. Exploiting sibling pairs data from the Wisconsin Longitudinal Study (WLS) for the years 1957 to 1993, Kim (2016) documents that the negative effect of education on obesity persists in the long run. Brunello, Fabbri and Fort (2013) analyzed data on males (females) who live in seven (nine) European countries that increased compulsory years of education. Instrumental variables regressions showed that additional schooling, triggered by the education reforms, had a negative impact on the body mass index in case of females.

Some other studies, however, reported weak or no effect of education on various health outcomes using similar empirical designs. For example, Kemptner, Jürges and Reinhold (2011) employed an instrumental

[73] Making use of natural experiments in different countries, several studies also examined the causal impact of parental schooling on child health and women's fertility both in affluent and emerging societies (Currie and Moretti, 2003; Osili and Long, 2008; Lindeboom et al. 2009; Chou, Grossman, and Joyce, 2010; McCrary and Royer, 2011; Cannonier and Mocan, 2012; Dincer et al. 2014; Fort et al. 2014; Geruso and Royer, 2014; Keats, 2014; Zhang, 2014; Grepin and Bharadwaj, 2015; Gunes 2015; Dursun et al., 2017).

variables strategy using the compulsory education reforms implemented in West Germany between 1949 and 1969. They found that education had a negative impact on long-term illness for men, but not for women, and that education had little impact on having weight problems and had no impact on smoking. Clark and Royer (2013) exploited two changes to British compulsory schooling laws that increased school-leaving-age from 14 to 15 in 1947 and from 15 to 16 in 1972. They could not find a significant impact of education on various health outcomes such as mortality or self-reported health, or on health behaviors such as smoking and drinking. Albouy and Lequien (2009) could not detect a causal impact of education on mortality in their analysis of a French panel data set using two identifying changes to the school-leaving-age. Meghir, Palme and Simeonova (2012) analyzed Swedish cohorts born between 1946 and 1957 that were impacted by education reforms implemented in 1949 and 1962, which increased the compulsory years of schooling. The authors could not detect significant effects of education on mortality up to age 60. Palme and Simeonova (2015) found that an exogenous increase in education, due to the same increase in compulsory education in Sweden, has increased women's risk of breast cancer diagnosis and lowered the breast cancer survival probability.

In developing countries, the impact of education on health is even more important for economic policy. A distinguishing feature of developing countries is that their education levels are low and health outcomes are worse in comparison to those of developed countries. For example, among the 180 countries around the world with available data, the median years of education is 8.2. Dividing countries into two groups as those with average education greater than 8.2 years, and those with average education fewer than 8.2 years, one finds that life expectancy at birth is 65.5 years in the group of countries with low education, while it is 72.8 years among the high-education countries. Similarly, the infant mortality rate is 19.6 deaths per 1,000 live births in the former group, while it is 39 deaths in the latter. The tuberculosis rate is 90 cases per 100,000 residents in high-education countries, while the rate is more than twice as high (193 cases per 100,000 residents) in low-education countries.[74] Thus, if education has a causal impact on health in developing countries, the importance of education as a policy lever is magnified.[75]

[74] Country level education data are obtained from Barro and Lee (2010). Infant mortality, tuberculosis and life expectancy data are from World Bank's World Development Indicators.

[75] A related question is the direct impact of health on economic development, the evidence on which is not clear-cut. For example, Bleakley (2007) analyzes the impact of the eradication of hookworm disease from the Southern states of the U.S. in the early 20th century, and finds that cohorts that benefitted from the hookworm eradication program made substantial income gains. Lorentzen, McMillan and Wacziarg (2008) use cross-national and sub-national data. Their cross-sectional instrumental variables regressions show that high adult mortality promotes higher levels of

Additionally, the developing world is facing steep public health challenges resulting from poor health behaviors. About 1.5 billion people around the world were estimated to be overweight or obese in 2008 (Popkin et al. 2012). Furthermore, as indicated by the World Health Organization, "Almost 6 million people die from tobacco use each year, both from direct tobacco use and second-hand smoke. By 2020, this number will increase to 7.5 million, accounting for 10 percent of all deaths. Smoking is estimated to cause about 71 percent of lung cancer, 42 percent of chronic respiratory disease and nearly 10 percent of cardiovascular disease. The highest incidence of smoking among men is in lower-middle-income countries" (WHO 2010). Consequently, obesity and smoking are listed among the most important public health challenges faced by developing countries, and the reports prepared by the World Health Organization (WHO) and the World Bank (WB) frequently stress the importance of education in promoting health and health behaviors in these countries (WHO, 2009; WB, 2011).[76]

As displayed in Appendix Table C1, there are, however, perhaps due to data limitations and lack of exogenous variation in schooling, only a few studies investigating the causal impact of education on body weight and smoking in developing countries. Huang (2015) shows increased education, due to 1986 compulsory schooling law in China, notably decreases the rates of self-reported poor health, being underweight, and smoking of the working age population (i.e., ages 18 and 50 and birth cohorts of 1955-1993). Conversely, exploiting the same reform in China, Xie and Mo 2014, find that there is no causal effect of education on perceived health and anthropometric health. Behrman et al. (2015) rely on within-monozygotic twins fixed effects and examine the effect of schooling on health. They show that education improves mental health and reduces smoking. Exploiting a randomized controlled experiment in Dominican-Republic, Jensen and Lleras-Muney (2012) show that informing male students about returns to education leads them to stay in school longer, and enhances pro-health behaviors (i.e., decrease in smoking, delayed onset of drinking) by the age of 18.[77] Note that three of the four studies use data from China.

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risky behavior and lower investment in physical capital, and that adult mortality is a substantial determinant of economic growth. On the other hand, Acemoglu and Robinson (2007) fail to detect an impact of a country's per capita GDP and its life expectancy. Similarly, using a simulation model Ashraf, Lester and Weil (2008) find that large improvements in health lead to small gains in GDP per capita.

[76] The Director-General of the WHO, Dr. Margaret Chan, started her speech in the 2010 United Nations Summit on the Millennium Development Goals (MDGs) by stating "Education and health go hand in hand. The evidence demonstrating the links is overwhelming." http://www.who.int/dg/speeches/2010/educationandhealth_20100920/en. Education for All campaign of the United Nations repeatedly refers the potential health improvements as one of the key external benefits of extending primary and secondary education (UNESCO, 2000).

[77] A number of studies also examined the causal effect of education on risky sexual behavior in Sub-Saharan African countries as HIV epidemic has been a widespread public health problem in the region. Alsan and Cutler (2013),

Therefore, to fill the gap in the literature, this article uses data from the Republic of Turkey, based on nation-wide repeated cross-sectional survey conducted by the Turkish Statistical Institute in 2008, 2010, 2012 and 2014, to estimate the causal impact of extended primary schooling on health and behaviors, including self-reported health status, measures of body weight, and tobacco use indicators. This study also analyzes such health behaviors as consumption of fruit, consumption of vegetables, and flu vaccinations, although the primary focus is on smoking and obesity because these are two major preventable causes of death around the globe (Mokdad et al. 2004). Note that Turkey is a middle-income country with per capita income of $10,515 in 2014. Average years of education is low: 6.5 years of schooling among those who are 25 years of age or older. Also, the Turkey is governed by a secular democratic political system in which citizens exhibit a good of freedom pertaining to their health behaviors. Hence, information obtained from the Republic of Turkey may constitute a reasonable comparison for a large group of countries, in which the level of educational attainment is typically low and per capita incomes are growing relatively rapidly, around the world.

To tease out the causal impact of education, I utilize an education reform, enacted in 1997, increased the compulsory years of schooling from 5 to 8 in Turkey, i.e., requiring everyone to continue schooling until earning at least a middle school diploma. This study uses exposure to this reform as an instrument for completed schooling to identify the impact of education on various health outcomes and the consumption of health inputs.[78]

Results show that education impacts the health outcomes and behaviors of young males and females differentially. Holding at least a middle school diploma, which corresponds to at least eight years of schooling,

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instrumented secondary school enrollment with distance to school, and show that secondary school enrolment increases the propensity of abstaining from sex, which decreases the HIV risk among girls in Uganda. Agüero and Bharadwaj (2014), using Zimbabwe's 1980 compulsory schooling reform as an instrumental variable, show that women with extended schooling are more knowledgeable about how HIV spreads, and they engage less risky sexual behaviors by having fewer sexual partners. A handful other studies with similar identification strategies document that additional education led to a significant reduction in HIV infection in Botswana, Malawi and Uganda (Behrman 2015a; De Neve et al. 2015). In a randomized experiment in Kenya, Duflo et al. 2015 investigate the effect of two different programs (i.e., an educational subsidy program, which provides free school uniforms to students, and HIV focused curriculum) on prevalence of sexually transmitted infections (STIs), and find that, when implemented alone neither of the program reduce the prevalence of STIs. However, when both of these policies are implemented jointly, STI prevalence among girls decreases significantly. Cannonier and Mocan (2012) exploit age and region specific exposure to the Free Primary Education program (FPE) in Sierra Leone, and document that increased educational attainment increased the propensity of being tested for HIV and modern contraceptive usage.

[78] Some recent papers also employed instrumental-variables regressions using the 1997 compulsory schooling reform to examine the impact of education on religiosity (Cesur and Mocan, 2013), child health (Dursun et al. 2017; Dincer et al. 2014; Gunes 2015), fertility (Gunes 2016), domestic violence (Erten and Keskin 2016), and subjective well-being (Dursun and Cesur 2016).

has a positive impact on male bodyweight, and it has a negative impact on their smoking. For women, extended primary schooling lowers the likelihood of being overweight, and it increases the chances of smoking. Descriptive tests suggest that labor force participation, occupational choice, and family income do not seem to play a central role in mediating the relationship between education and health indicators. Supplemental analyses imply that rather than the mechanisms, which operate via economic outcomes, peer effects, and time use may be the primary factors explaining the findings of this study.

The current study reveals important patterns on the relationship between compulsory education and health. First, the findings of this study shows that primary education "on itself" may not be sufficient as a policy tool to curb the public health costs, which are stemming from preventable causes across the globe. Second, schooling may have differential health and health behavioral effects on males and females. Third, consistent with the findings of Jensen and Lleras-Muney (2012), the results presented in this study point to the importance of peer effects on how schooling shapes health behaviors. Altogether, these results invite educational policy makers to consider various patterns through which formal education may shape health behaviors in designing school curriculums.

Section 4.2 provides the institutional details of the 1997 education reform. Section 4.3 constructs the empirical framework, and Section 4.4 introduces the data. Section 4.5 presents the results and robustness. I explore the mechanism in Section 4.6; Section 4.7 concludes.

4.2 The 1997 Compulsory Schooling Reform in Turkey as the Source of Identification

In August 1997, the Turkish Parliament passed a law to modify the duration of secular mandatory schooling. Prior to this education reform, mandatory education was limited to five years of elementary education. The reform increased the mandated years of education to eight years by combining the elementary and middle schools. The reform, however, did not involve any changes in curriculum; neither the course contents nor the composition of courses are affected by the reform (Dulger 2004).

The law went into effect in the Fall of 1998, and the students who were enrolled in the fifth grade in Fall 1997 were required to continue their schooling until the end of the eighth grade. On the other hand, students who had already completed the fifth grade by the Summer 1997 were exempt from the mandate. The law was enacted very quickly, for political reasons. During the time period when the law was enacted, Turkey was involved in

negotiations for the European Union membership and the government was concerned that European Union negotiations would not proceed without the implementation of a reform that increased the level of education in Turkey (Dulger 2004). The law was also an attempt to limit the extent of religious education. Details on this point and the political landscape in Turkey in 1997 can be found in Cesur and Mocan (2013).

The relevant Turkish law states that a child may start the first grade in the Fall if he/she is 72 months old at the end of that calendar year (Dincer et al. 2014; Dursun and Cesur 2016; Erten and Keskin, 2016).[79] This means that especially those who are born towards the end of 1986 could start school in 1992. At the same time, the age cut-off is loosely enforced and children may be allowed to start school even if they are a few months younger than the 72-month cut-off. Thus, those who are born in early 1986 would start the first grade in Fall 1991, rather than Fall 1992. Put differently, although most children who were born in 1986 would have enrolled in the first grade in 1992 and therefore have completed the fifth grade in Summer 1997 (thus, being exempt from the mandate of the law), some other children who were also born in 1986 have completed only the fourth grade and these children were impacted by the reform. Furthermore, due to a one-year delay between the enactment of the reform in 1997 and its implementation in 1998, some families whose children were exempt from the law (i.e., born in 1986 and completed the fifth grade in 1997) may have decided in favor continuing the education in response to the education reform. Therefore, in the benchmark models, I exclude those born in 1986, although as I show later that including them does not alter the results.

I visualize the influence of the 1997 education reform on educational attainment in Appendix Figure C1, which display the proportion of males and females who have at least 8 years of education (Middle School Diploma) by birth year. These figures documents two salient patterns. First, there is an upward trend in middle school completion rates of men and women at the birth cohort level. For example, 40 percent of women and 57 percent of men born in 1976 have a middle school education or higher, whereas the corresponding rates are 91 percent and 95 percent, respectively, among those born in 1996. More importantly, trends in holding at least a middle school diploma rates exhibit an unambiguous jump starting with 1987, which is the first fully impacted birth cohort by the education reform. The proportion of individuals with at least a middle school diploma jumps to 75 percent (from 57 percent) for women and to almost 91 percent (from 76 percent) for men who are in the first fully impacted birth cohort (i.e., born in 1987) in comparison to those born in 1985. These patterns are very similar to those reported by

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

other researchers who analyzed the implications of the same education reform but using different data sets (Cesur and Mocan 2013, Mocan 2014).[80] Note that the compliance rates for the 1997 education reform are highly similar to those observed in a number of countries across the world including the western countries (e.g., European Union and the U.S.).[81]

As the compulsory schooling reform at hand offers an ideal quasi-experimental research design, a number of studies employed the 1997 education reform as an instrumental-variable to examine the causal effect of formal schooling on a number of outcomes, including religiosity (Cesur and Mocan, 2013), fertility and child health (Dincer et al., 2014; Dursun et al., 2017; Gunes 2015, 2016), subjective well-being (Dursun and Cesur, 2016), earnings (Torun 2013; Mocan 2014), and domestic violence (Erten and Keskin 2016).

4.3 Empirical Framework

The relationship between health outcomes and education at the individual level is depicted by equation (1) below, where H stands for a particular health outcome or behavior, e.g. self- reported health status, BMI as well as measures of tobacco consumption. In addition, we also investigate the consumption of other health inputs such as fruits, vegetables, and flu vaccinations. Middle School represents a binary indicator for holding at least a middle school degree.[82]

$$\mathrm{H}_i^s = \propto + \beta(\text{Middle School})_i^s + \Psi \mathrm{Z}_i^s + \varepsilon_i^s \tag{4.1}$$

Z is a (nxk) vector of control variables, including year of birth differentiated by exposure to the education reform status (i.e., jointly specified re-centered birth year, and its interaction with the binary Reform indicator, which is set equal to one for those who were born after 1986), survey year fixed effects, and region of residence fixed-effects for the 12 regions of the country. To account for potentially different trends in health by region and the differential

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[80] Mandatory schooling is free in Turkey and non-attendance is subject to fines (http://mevzuat.meb.gov.tr/html/24.html) although these fines are not strictly enforced (http://spm.ku.edu.tr/wp-content/uploads/pdf/okulterk.pdf). Consequently, even though the middle school completion rates increased above the 90 percent level after the reform was in effect, perfect compliance could not be achieved.

[81] Brunello et al. 2013, Fort et al. 2014, and Stephens and Yang 2014 report similar compliance rates for several European countries and the US.

[82] The HS surveys do not include information on years of schooling. Instead, information on the literacy status and educational attainment of survey participant were obtained based on the following options: literate, less than basic 5-year education, basic 5-year diploma, middle school diploma, high school diploma, and college education.

impact of compulsory schooling reform on educational attainment, I also allow birth year trends differ by region (i.e., controlling for interactions between region fixed effects and year of birth in addition to other controls).[83] Standard errors are clustered at the region-by-birth cohort level. As explained in the data section, this study uses four cross-sectional surveys, registered in four waves, in two-year intervals between 2008 and 2014. The superscript s identifies the wave of the survey; i.e. s in {1, 2, 3, 4}.

Estimating equation (1) by OLS may produce biased estimates of β because educational attainment is likely to be correlated with a number of unobservable factors that can also impact health outcomes. Alternatively, reverse causality may exist and health outcomes may impact the level of education. Therefore, to address the endogeneity of educational attainment, I exploit the exogenous variation in the propensity of middle school graduation induced by the education reform of 1997. Note that birth cohorts born after 1986 were mandated to complete middle school (i.e., 8 years of schooling) while those who were born prior to 1986 were not bound by the law. I employ an instrumental variables strategy and use exposure to the education reform as an instrument for having middle school diploma, as depicted by equation (4.2).

$$\text{Middle School}_i^s = \pi + \gamma \text{Reform}_i^s + \Omega Z_i^s + \mu_i^s \qquad (4.2)$$

where Reform is a binary indicator that takes the value of one for those who were born after 1986, and it takes the value of zero for those who were born before 1986. As it is uncertain whether those who were born in 1986 were affected by the reform, the birth cohort of 1986 is omitted from the main regression specifications.[84] By design, those who are younger in a particular survey year are more likely to have been exposed to the reform and those who are older are likely to have missed the reform.[85]

Equation (4.3) estimates the intent-to-treat (i.e., reduced form) impact of education reform on the outcomes of interest:

[83] We know the birth year of the respondents, but not their exact birthday. This is not an issue for this study because exact birthday (i.e., the moth of birth) does not decide the year at which a child starts schooling (Dursun and Cesur, 2016).
[84] I also estimated all models by coding the *Reform* variable in two alternate ways: Reform =0.33 if birth year=1986; and Reform=0.5 if birth year=1986. The results did not change.
[85] For example, people who were born in 1985 are 25 years old in 2010, and those who are born in 1987 are 23 years old in that same year. The former group is not treated by the reform, while the latter group was exposed to it.

$$H_i^s = \delta + \lambda \text{Reform}_i^s + \Phi X_i^s + e_i^s \quad (4.3)$$

where λ represents the reduced form effect of the reform.

Finally, similar to most research designs enabled by compulsory schooling reforms (Brunello et al. 2009, 2013; Clark and Royer 2013; Stephens and Yang 2014), my empirical framework allows us to estimate the impact of extended primary education among those who would not continue schooling in the absence of a compulsory schooling reform; therefore, the estimates presented in this study correspond to the local average treatment effect (LATE) among individuals with a relatively low propensity to continue schooling beyond basic education.

4.4 Data

I use data drawn from the Turkish Statistical Institute's Health Survey (HS). The HS is a cross sectional survey, which has four waves registered in 2008, 2010, 2012, and 2014 up to date. The 2008, 2010, 2012 rounds include 20,624; 20,200; and 37,979 participants, respectively; and the 2014 survey contains 26,075 individuals. The goal of the HS is to provide nationally representative estimates of health indicators in Turkey. Three separate survey questionnaires are provided for the age groups 0-6, 7-14, and 15 and older.[86] For those who are 15 and older, the HS collects detailed health information including self-reported health status, smoking habits, chronic diseases, height and weight. The HS also contain personal background characteristics, including age, gender and the education level of survey respondents. [87]

The instrumental variable is consistent with a fuzzy regression discontinuity design (Lee and Lemiux 2010). In that regard, deciding the estimation interval before and after the, the 1997 education reform, i.e., bandwidth selection around discontinuity, is critically important for myanalysis. While increasing the bandwidth would allow us to estimate the empirical model with larger sample sizes, variation obtained from individuals who are far from the pivotal birth cohort may produce biased results. On the other hand, shortening the bandwidth would lead to noisy estimates of the treatment effect, i.e., precision-bias tradeoff, (Lee and Lemiux 2010). In the analysis, I restrict the analysis sample to those who were born between 1983 and 1989. This means that this study utilizes three

[86] For those respondents who are younger than 15, the parent or guardian completed the survey.
[87] We also obtained information on geographic identifiers at the regional level, which allows us to control for region fixed effects. However, while we can control for unobservable regional characteristics, the region identifier in the HS does not provide the name of the region of residence.

cohorts that are exposed to the education reform (those born between 1987-89) and three cohorts that missed the reform (born between 1983 and 85). The resulting age range of the sample is 19-31. I also employed MSE-optimal bandwidth selection method introduced by Calonico et al. (2016a) to obtain the optimal bandwidths for my estimations.[88] Appendix Table C2 shows that optimal bandwidths range between 2 to 4 years depending on the estimation sample; hence, the 3-year estimation window is consistent with the optimal bandwidths suggested by the MSE-optimal bandwidth selection method.[89]

Table 4.1- Descriptive Statistics

		(1)	(2)	(3)	(4)	(5)	(6)
		Males			Females		
Variable Name	Variable Description	All	Control	Treatment	All	Control	Treatment
A. Descriptive Statistics for Self Reported Health							
Middle School	= 1 if completed Middle School	0.82	0.73	0.93	0.66	0.54	0.80
Diploma	= 0 otherwise	(0.38)	(0.44)	(0.26)	(0.47)	(0.50)	(0.40)
Good Health	= 1 if health is good, excellent	0.86	0.86	0.87	0.80	0.79	0.82
	= 0 otherwise	(0.34)	(0.35)	(0.33)	(0.40)	(0.41)	(0.38)
Excellent Health	= 1 if health is excellent	0.22	0.20	0.25	0.16	0.14	0.18
	= 0 otherwise	(0.42)	(0.40)	(0.43)	(0.37)	(0.34)	(0.39)
Observations		3,531	1,865	1,666	4,504	2,359	2,145
B. Descriptive Statistics for Body Weight Measures							
Middle School	= 1 if completed Middle School	0.83	0.73	0.93	0.69	0.57	0.82
Diploma	= 0 otherwise	(0.38)	(0.44)	(0.26)	(0.46)	(0.50)	(0.38)
BMI	Body Mass Index	24.38	24.75	23.97	23.27	23.75	22.75
		(3.58)	(3.53)	(3.59)	(4.21)	(4.31)	(4.03)
Underweight	= 1 if BMI<18.5,	0.03	0.02	0.03	0.09	0.07	0.10
	= 0 otherwise	(0.16)	(0.14)	(0.18)	(0.28)	(0.26)	(0.30)
Overweight/Obese	= 1 if BMI>=25,	0.38	0.43	0.32	0.28	0.32	0.23
	= 0 otherwise	(0.48)	(0.49)	(0.47)	(0.45)	(0.47)	(0.42)
Obese	= 1 if BMI>=30,	0.07	0.07	0.06	0.07	0.08	0.06
	= 0 otherwise	(0.25)	(0.26)	(0.24)	(0.26)	(0.28)	(0.24)
Age	Age in years	25.65	27.41	23.64	25.52	27.45	23.4
		(2.95)	(2.31)	(2.23)	(3.04)	(2.24)	(2.30)
Observations		3,430	1,825	1,605	4,165	2,186	1,979

[88] Optimal bandwidth selection is carried out with rdbwselect command introduced by Calonico et al. (2016a). For technical and methodological details please see Calonico et al. 2014, 2016b.
[89] For the sake of completeness, in addition to the main estimation sample, I also use the sample of individuals who are born in the periods 1982-90 (i.e., 4-years) and 1984-88 (i.e., 2-years) to examine the sensitivity of the main findings to the choice of estimation sample. These results, shown in Appendix Table C3, are similar to the estimates obtained from the sample of individuals who were born in the 1983-89 period.

Table 4.1- Continued

Variable Name	Variable Description	(1)	(2)	(3)	(4)	(5)	(6)
		Males			Females		
		All	Control	Treatment	All	Control	Treatment
	C. Descriptive Statistics for Smoking Indicators						
Middle School	= 1 if completed Middle School	0.83	0.75	0.93	0.68	0.57	0.81
Diploma	= 0 otherwise	(0.37)	(0.43)	(0.26)	(0.47)	(0.50)	(0.39)
Ever Smoked	= 1 if ever smoked	0.63	0.65	0.61	0.31	0.35	0.27
	= 0 otherwise	(0.48)	(0.48)	(0.49)	(0.46)	(0.48)	(0.45)
Current Smoker	= 1 if currently smokes,	0.52	0.54	0.51	0.20	0.22	0.18
	= 0 otherwise	(0.50)	(0.50)	(0.50)	(0.40)	(0.42)	(0.38)
Ever Smoked	= 1 if ever smoked regularly	0.55	0.58	0.52	0.21	0.23	0.18
Regularly	= 0 otherwise	(0.50)	(0.49)	(0.50)	(0.41)	(0.42)	(0.38)
Age	Age in years	26.37	28.25	24.36	26.28	28.14	24.18
		(2.54)	(1.70)	(1.54)	(2.60)	(1.70)	(1.68)
Observations		2,870	1,486	1,384	3,578	1,898	1,680
	D. Descriptive Statistics for Health Inputs						
Middle School	= 1 if completed Middle School	0.82	0.73	0.93	0.66	0.53	0.80
Diploma	= 0 otherwise	(0.38)	(0.45)	(0.26)	(0.47)	(0.50)	(0.40)
Daily Fruit	= 1 if consumes fruit daily	0.47	0.47	0.47	0.54	0.55	0.52
	= 0 otherwise	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Daily Vegetable	= 1 if consumes vegetable daily	0.58	0.59	0.57	0.66	0.67	0.64
	= 0 otherwise	(0.49)	(0.49)	(0.50)	(0.47)	(0.47)	(0.48)
Flu-Shot Ever	= 1 if ever had flu-shot	0.16	0.16	0.17	0.11	0.11	0.12
	= 0 otherwise	(0.37)	(0.37)	(0.38)	(0.32)	(0.31)	(0.32)
Observations		4,255	2,276	1,979	5,582	2,969	2,613

The data is from the 2008, 2010, 2012, and 2014 rounds of TurkStat's Health Surveys. Smoking Indicators are only available in 2010, 2012, and 2014 waves of the HS. Standard deviations are in parentheses. The treatment group consists of those who were born in 1987-1989, and the 1983-85 birth cohorts constitute the control group.

The key independent variable, Middle School Diploma is a binary indicator, which takes the value of one if the individual holds a middle school or a higher degree, and zero otherwise. The surveys do not include information on years of schooling. Instead, individuals were classified as being literate, having less than elementary school degree, elementary school degree (at least 5 years of schooling), middle school degree, high school degree, and college education. Middle School Diploma corresponds to at least 8 years of education. The mandate of the schooling reform was to provide at least a middle school education, impacting the children who were born in 1987 or after.

Summary statistics by exposure to the 1997 education reform status for males are presented in columns (1) to (3), and for females in columns (4) to (6), respectively. Note that the treatment group includes

individuals born after 1986, and the control group contains those born before 1986. In the self-reported health sample, the proportion of males with middle school diploma is 73, and 93 percent for the control and treatment groups, respectively. The corresponding middle school completion rate for females is 55, and 81 percent in the control and treatment samples, in that order.

I construct two self-reported health status measures based on the survey question "How is your health in general?" Potential answers are; very good, good, average, bad, and very bad. Good Health takes the value of 1 is the respondent declared his/her health status as good or very good, and zero otherwise. The second self-reported health indicator Excellent Health takes the value of 1 is the respondent declared his/her health status as very good, and it is coded as zero otherwise. Summary statistics for self-reported health indicators are shown in Panel A of Table 4.1. Eighty-six percent of men and 80 percent of women declare that their general health is good or excellent. Those in the treatment group have slightly better self-declared health.

Using Self-reported height and weight, we calculate body mass index (BMI) as well as three binary variables to indicate whether the person is underweight, overweight or obese.[90] Specifically, Underweight takes the value of 1 if the BMI score is less than 18.5, and zero otherwise. Overweight is equal to 1 for respondents with a BMI greater than or equal to 25, and zero otherwise. Obese is coded as 1 if BMI is at least 30, and it is coded zero otherwise. As Panel B of Table 4.1 shows, the average BMI in the male sample is 24.38 for men and 23.27 for women. Twenty-eight percent of women and 38 percent of men are either overweight or obese. If women are more concerned than men about their weight, the tendency to underreport weight in the survey could be higher for women than men; and this could be one reason for the lower overweight/obesity rates among women. In the analysis, however, I will divide the sample by sex and investigate the impact of education on BMI within each sex.

Questions on tobacco use were asked in the 2010, 2012 and 2014 surveys. We construct three dichotomous indicators representing smoking habits. My first dichotomous smoking indicator Ever Smoked is set equal to 1 for survey respondents who declared they have used tobacco products at least once. Ever Smoked

[90] BMI = [(Weight in Kilograms)/(Height in Meters Squared)]. BMI does not differentiate between the proportion of fat and the proportion of muscle/bone in total weight. This could be especially important in determining the impact of weight and body composition on outcomes such as wages (Burkhauser and Cawley 2008)

Regularly is a binary variable which takes the value of 1 for those responding to the question "Have you ever regularly (at least 100 units per year) consumed tobacco products?" affirmatively. Finally, the variable Current Smoker takes the value of 1 if the respondent currently consumes tobacco products (at any frequency; i.e., every day, or once-in-a- while), and zero otherwise.[91] As Panel C of Table 4.1 shows, 63 percent of men, and 31 percent of women report ever using tobacco products in the estimation sample. Proportion of men and women who ever smoked regularly are 55, and 21, respectively. Fifty-two percent of men and 20 percent of women, who are between the ages 19 to 31, reported themselves current smokers. All three measures of smoking behaviors indicate that the propensity to smoke is lower in the treatment group.

I also construct additional health behavior indicators to capture dietary habits, such as fruits and vegetables intake, and a health risk avoidance measure, i.e. the propensity to have a flu shot. Daily Fruit takes the value of one if the respondent indicates that he/she eats fruit at least once a day. Daily Vegetable is constructed analogously to daily fruit consumption. Flu Shot-Ever is a dichotomous variable to indicate if the person has ever received a flu shot. Fifty-four (47) percent of women (men) report consuming fruits on a daily basis. Daily vegetable consumption is 66 percent for women and 58 percent for men. Sixteen percent of males and 11 percent of females indicated that they have received at least one flu shot during their lives.

4.5 Results

Table 4.2 presents the ordinary least squares (OLS) estimates of measures of health and health behaviors on having at least a middle school diploma. While the OLS models are likely to be biased due to potential endogeneity of education, it may still be interesting to compare the OLS estimates to those obtained from the instrumental variables regressions. All the estimates use the sample of individuals who were born between 1983 and 1989, unless otherwise indicated.

Among males, in Panel A, OLS estimates show that education is favorably associated with all of the health outcomes and behaviors except body mass. That is, having a middle school diploma (as opposed to having an elementary school degree or no degree) is directly associated with self-reported Good Health and Excellent

[91] While the questions refer to the use of tobacco products, cigarette consumption is by far the largest item in tobacco consumption in Turkey. Consumption of other tobacco items such as chewing tobacco, smoking cigar, pipe or hukka are trivial in comparison to cigarettes. Among those who indicated that they used tobacco products, 98 percent smoke cigarettes.

Health. Furthermore, middle school education is inversely related to measures of smoking. As noted, among males who are in the age range of 19 to 31, at least eight years of schooling does not seem to be correlated with measures of body weight.

In Panel B of Table 4.2, for females, holding at least a middle school diploma is directly associated with self-reported health indicators (i.e., Good Health, and Excellent Health), measures of smoking (i.e., Ever Smoked, Ever Smoked Regularly, and Current Smoker), and it is inversely associated with measures of body mass.[92] The counterintuitive positive association between schooling and tobacco use among women is consistent with the observed education and smoking relationship in countries at different stages of development as shown by Cutler and Llreas-Muney (2012). That is, while the association between education and smoking is negative for affluent societies, it is found to be positive for developing countries. This finding is attributed to the likelihood that female smoking may be perceived as an indicator for women's empowerment in developing societies (Schaap et al. 2009; Hitchman and Fong 2011; Amos et al. 2012; Cutler and Lleras Muney 2012).

Because the OLS results are not necessarily credible, I next report the instrumental variables estimates, in which having at least a middle school degree is instrumented by exposure to the education reform. Table 4.3 demonstrates that being bound by the 1997 education reform has a large positive impact on the propensity to earn at least a middle school degree both for males and females, regardless of the estimation sample employed, classified by health outcome indicators, i.e., self-reported health, BMI, tobacco use, and health inputs samples. The impact of being exposed to the schooling reform on Middle School Diploma ranges between 11.2-12.6 percentage points for men, and 11.9-16.2 percentage points for women. These estimates are statistically significant at the 1-percent level in each case.[93] Figure 4.1 visualizes the results provided in Table 4.3 by purifying the influence of pre-determined variables and potential cohort effects on obtaining at least a middle school degree variable, and plot the residuals for pre-reform and post-reform cohorts based on Equation (2) for males and females respectively (Lee and Lemiux 2010; Brunello et al. 2013). The graphical illustration of Equation (2) is assuring that the 1997 education reform led to a significant jump in middle school graduation rates.

[92] Tansel and Karaoglan (2014) use the same data set as this study uses, to explore the associations between education and health (i.e., consider education as exogenous). They estimate their models using the combined sample of men and women who are older than 25 years of age. I find very similar results when I employ the estimation sample of Tansel and Karaoglan (2014) and use OLS as they do.

[93] Mocan (2014) who used much larger sample sizes (about 75,000 females and 69,000 males) but reported an impact of the reform on the probability of having a middle school diploma by very similar magnitudes: 17 percentage points for women and 13 percentage points for men.

Table 4.2
OLS Estimates of Health Outcomes and Health Behaviors on Education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
Panel A: Males									
Middle School Diploma	0.118***	0.063***	-0.114	0.010	0.001	0.016	-0.056**	-0.093***	-0.077***
	(0.022)	(0.017)	(0.196)	(0.006)	(0.025)	(0.013)	(0.025)	(0.028)	(0.022)
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
Panel B: Females									
Middle School Diploma	0.108***	0.045***	-1.141***	0.035***	-0.123***	-0.037***	0.047***	0.028*	0.038***
	(0.014)	(0.013)	(0.161)	(0.009)	(0.016)	(0.010)	(0.015)	(0.016)	(0.014)
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

Table 4.3
The Impact of the 1997 Education Reform on Earning at Least a Middle School Diploma

	(1) Males	(2) Females
A. Self Reported Health Sample		
Reform	0.123*** (0.023)	0.141*** (0.030)
Observations	3,531	4,504
Mean	[0.823]	[0.663]
B. BMI Sample		
Reform	0.127*** (0.021)	0.129*** (0.029)
Observations	3,430	4,165
Mean	[0.825]	[0.690]
C. Smoking Sample		
Reform	0.136*** (0.022)	0.185*** (0.028)
Observations	2,870	3,578
Mean	[0.834]	[0.683]
D. Health Inputs Sample		
Reform	0.123*** (0.023)	0.140*** (0.030)
Observations	3,527	4,499
Mean	[0.823]	[0.663]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

The validity of the empirical design hinges upon the assumption that individuals who were born in close neighborhoods around the pivotal cohort have similar predetermined characteristics except the emerged discontinuity of having at least middle school degree. Using the available information in the HS, I employ cohort size (i.e., number of individuals born in each birth year), and height of the respondents to check this assumption. Appendix Figure C2 displays the averages of these predetermined characteristics by birth cohort for men and women, respectively. As there are no apparent leaps in these outcomes due to exposure to the education reform (i.e., being born after 1986), these graphs reinforce the validity of the empirical design. Moreover, I also formally implement the McCrary test if the density of year of birth is

Figure 4.1: The Impact of Reform on Obtaining at least a Middle School Degree

continuous around the 1986 threshold (McCrary 2008). Visual inspection of the McCrary test provided in Appendix Figure C3 affirms that the density of year of birth does not exhibit a break around the pivotal birth cohort.[94] Altogether, these tests suggest that the variation in schooling around the cut-off birth cohort is orthogonal to individual characteristics; thus, the instrumental variable, the 1997 education reform, satisfies the exclusion restriction.

Table 4.4 presents the instrumental variable estimates of the impact of earning at least a middle school diploma on health outcomes and behaviors for males (Panel A), and females (Panel B). As pointed out by the first-stage F-test statistic values, ranging from 19.1 to 44, depending on the sample, first-stage regressions show that the instrumental variable at hand exceeds the power requirements.

As displayed in columns (1) and (2) of Table 4.4, while the impact of Middle School on self-reported health indicators is positive for both genders, it is only statistically significant for Excellent Health in the case of females. In particular, holding at least a middle school degree increases the likelihood of being in excellent health category by 33.2 percentage-points.

Columns (3) to (6) of Panel A in Table 4.4 show that middle school diploma has a positive and significant impact on male body mass. Having a middle school diploma, which is associated with an extra three years of schooling in comparison to the control group, increases men's BMI by about 8.5 points from a base of about 24.3.

Similarly, the results columns (4) to (6) indicate middle school education has a positive impact on men's propensity to be overweight, and obese. On the other hand, the estimates show that middle school education has a nonlinear impact on female BMI. More specifically, earning at least a middle school degree lowers the likelihood of being underweight, and overweight by 36.5, and 35 percentage points respectively; hence, these results suggest that extended schooling allows women to maintain a healthy body weight.[95] Columns (7) to (9) reveal that at least eight years of schooling has differential effects on male and female tobacco use. Because tobacco use questions were not asked in 2008, I utilize three available waves of the HS survey for these specifications. Among men,

[94] The p-value of the estimated discontinuity is 0.29.
[95] Indeed, the IV estimate of the impact of Middle School on Normal Weight (i.e., 25> BMI>18.5) is 0.738 and it is statistically significant at the 5 percent level.

Table 4.4
Instrumental Variable Estimates of the Impact of Education on Health Outcomes and Health Behaviors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
				Panel A: Males					
Middle School	0.254	0.294	7.311***	-0.109*	0.655***	0.400***	-0.508**	-0.481**	-0.347
Diploma	(0.213)	(0.29)	(1.946)	(0.057)	(0.175)	(0.14)	(0.231)	(0.233)	(0.264)
First Stage F-test	29.399	29.399	34.838	34.838	34.838	34.838	37.088	37.088	37.088
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
				Panel B: Females					
Middle School	0.199	0.332***	0.406	-0.365**	-0.348*	0.098	0.440***	0.286*	0.279***
Diploma	(0.155)	(0.094)	(1.575)	(0.145)	(0.197)	(0.089)	(0.112)	(0.164)	(0.107)
First Stage F-test	21.493	21.493	19.165	19.165	19.165	19.165	44.822	44.822	44.822
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

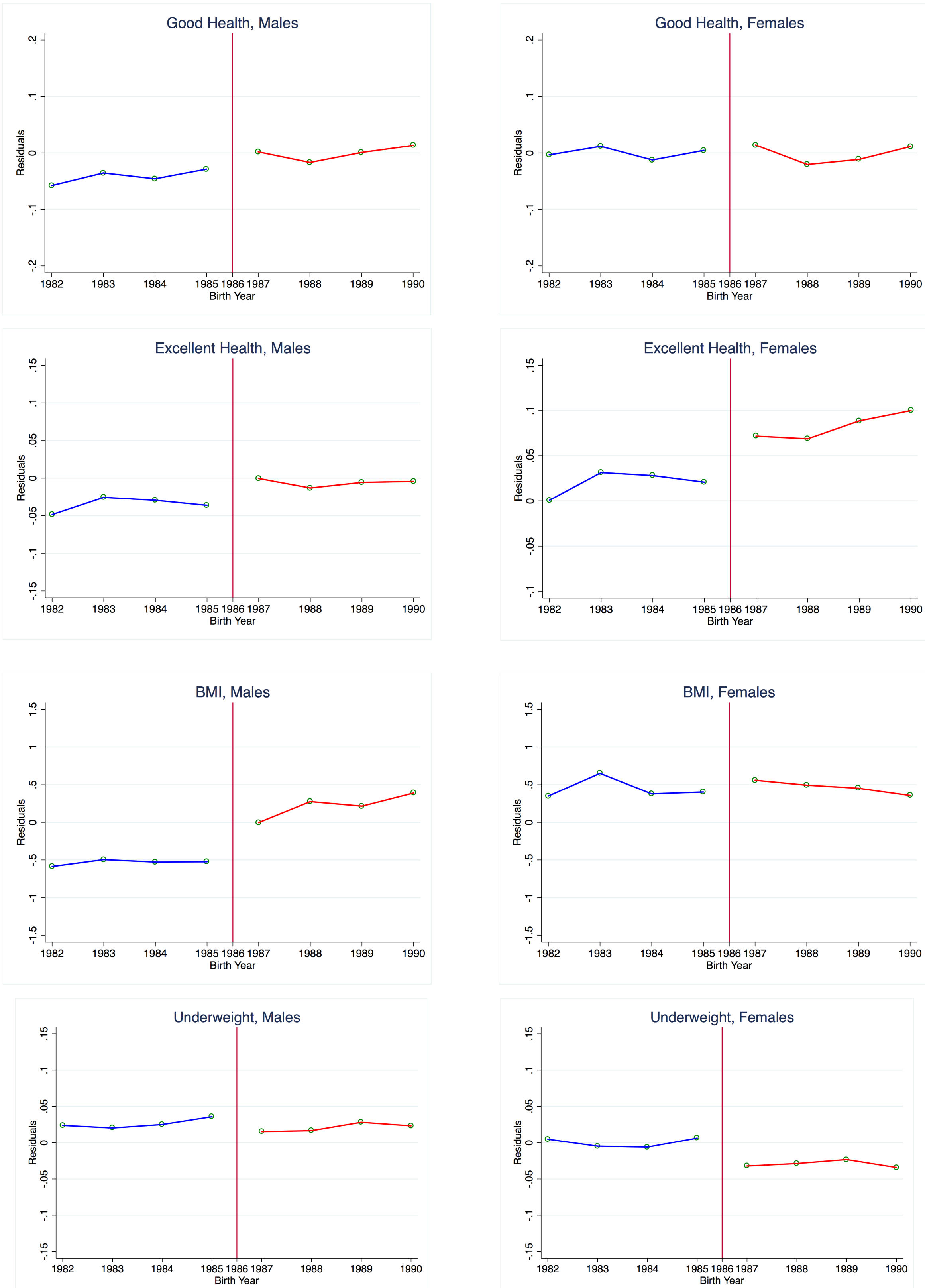


Figure 4.2: The Impact of Reform on Health Outcomes

Figure 4.2 (Continued)

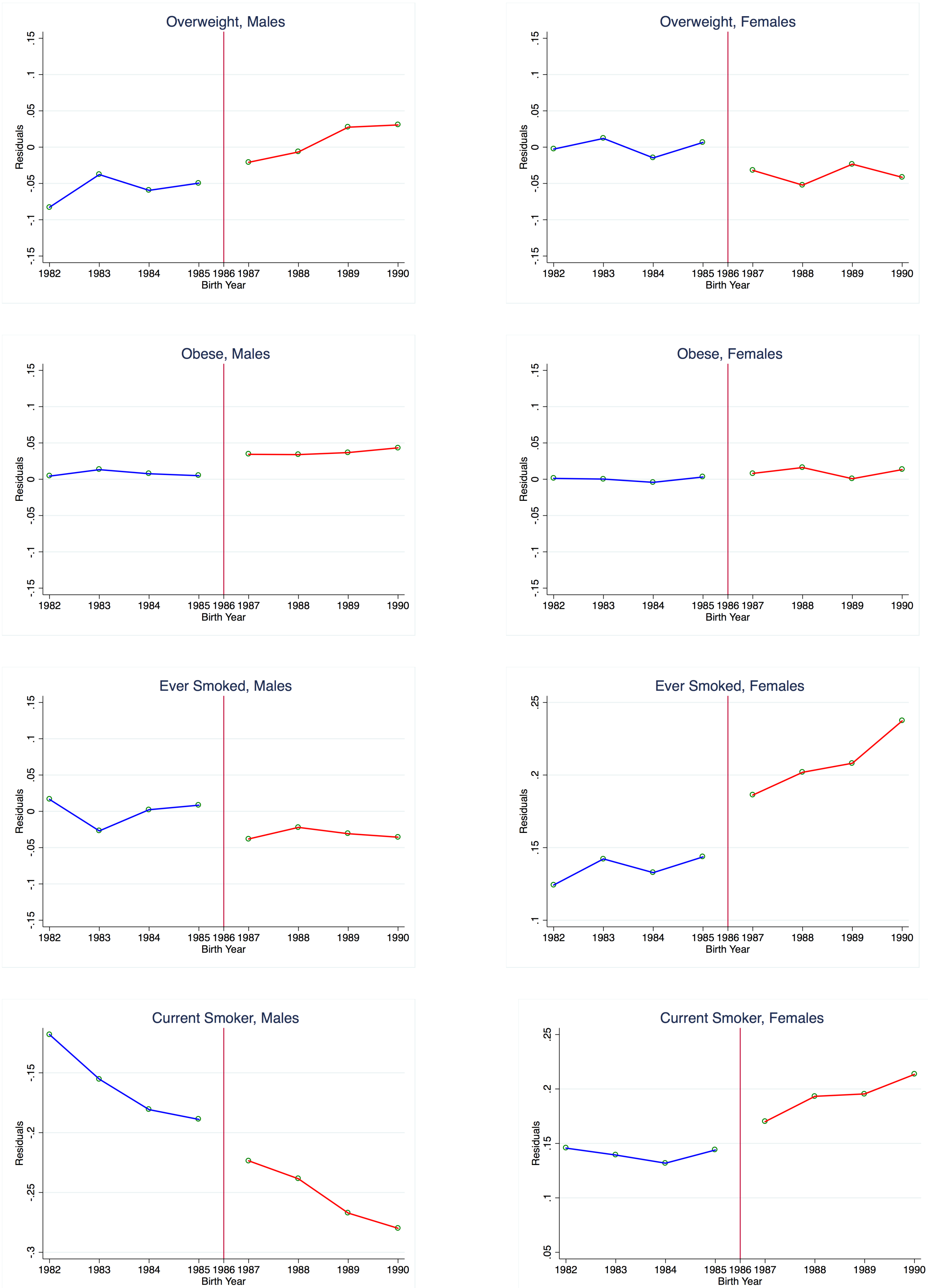
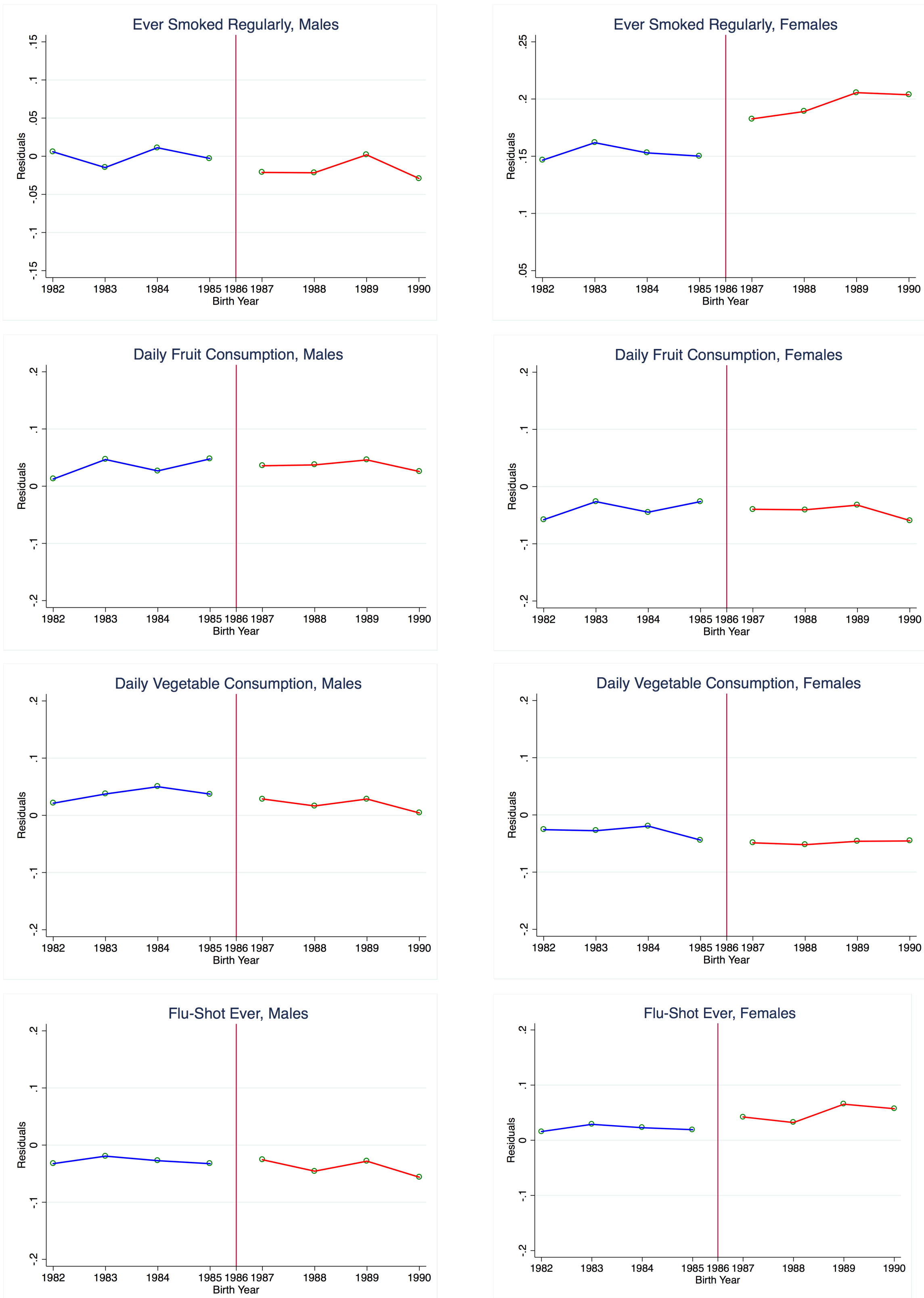


Figure 4.2 (Continued)



middle school education reduces the likelihood of ever using tobacco products and being a current smoker by 51, and 48 percentage points, respectively. For women, Middle School has a positive and statistically significant impact on all three measures of smoking. Accordingly, those who earned at least a middle school degree are 44, 28.6, and 27.9 percentage points more likely to ever use tobacco, be a current smoker, and ever smoke regularly, in that order.

It can be argued that there might be measurement error in reported smoking among females if there is stigma attached to reporting of female smoking in a developing country.[96] Non-systematic measurement error in reported smoking does not produce a bias in the estimates given that smoking is the dependent variable. If the more educated have a tendency to underreport their true smoking propensity because they are aware of the consequences of their unhealthy behavior and do not want to report that they smoke in a health survey, one would obtain a larger negative impact of education than the true impact. Given that I find that increased education has a positive impact on female smoking, this should not be a concern for this case.

Lastly, in Table 4.5, I examine whether the likelihood of eating fruits and vegetables on a daily basis as well as the possibility of flu-shot intake change in response to three additional years of schooling. The OLS estimates, IV estimates, and intent to treat estimates are presented from columns (1) to (3), respectively. I estimate the impact of educational attainment on these measures to further explore the link between schooling and health behaviors. Although the OLS estimates are positive and statistically significant for males and females, in none of the cases, the IV estimates are statistically significant for these additional health inputs.

4.6 Robustness

To investigate the robustness of the results, I tried a number of sensitivity tests. I begin with the robustness checks by estimating the reduced-form effect of the 1997 education reform on measures of health and health behaviors, as depicted by Equation (3). These intent-to-treat estimates, displayed in Table 4.6, represent the impact of having been part of the cohorts that were exposed to the reform. As expected, the reduced form estimates are fully consistent with the IV specifications shown in the previous table.[97]

[96] I suspect no reason for a stigma among males pertaining to reporting their smoking habits.
[97] The IV-coefficients reported in Table 4.4, can also be recovered by taking the ratio of the reduced-form coefficients in Appendix Table C2 and the first-stage coefficients in Table 4.3.

Table 4.5
The Impact of Education on Other Health Inputs

	(1)	(2)	(3)
	OLS	IV	Intent to Treat Estimates
Dependent Variables			
	Panel A: Males		
Daily Fruit	0.090***	-0.197	-0.024
	(0.022)	(0.222)	(0.026)
1st Stage F-test		30.11	
Observations	3,527	3,527	3,527
Daily Vegetable	0.054**	-0.263	-0.032
	(0.023)	(0.297)	(0.035)
1st Stage F-test		29.874	
Observations	3,527	3,527	3,527
Flu-Shot Ever	0.063***	-0.016	-0.002
	(0.016)	(0.226)	(0.028)
1st Stage F-test		31.619	
Observations	3,505	3,505	3,505
	Panel B: Females		
Daily Fruit	0.128***	-0.085	-0.012
	(0.018)	(0.226)	(0.031)
1st Stage F-test		21.5	
Observations	4,498	4,498	4,498
Daily Vegetable	0.090***	-0.078	-0.011
	(0.02)	(0.143)	(0.02)
1st Stage F-test		21.967	
Observations	4,499	4,499	4,499
Flu-Shot Ever	0.070***	0.125	0.018
	(0.011)	(0.126)	(0.018)
1st Stage F-test		21.607	
Observations	4,484	4,484	4,484

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

The visual illustrations of intent-to-treat estimates of the impact of reform on outcome measures are shown in Figure 4.2, which plots the residuals net of exogenous controls from Equation (3). These figures show the observed differences in outcomes measures between the treatment (i.e., born after 1986) and control (born before 1986) groups are not driven by sporadic “bumps.” Instead, these graphs show that the 1997 education reform led to systematic changes in health and health behaviors of young males and females.

Table 4.7A undertakes a number of specification checks for the male sample, and I repeat these exercises for the female sample in Table 4.7B. In Panel A of Table 4.7A, for males, I test whether these findings differ when I control for birth year in a quadratic functional form (i.e., birth year and birth year squared specified jointly) instead of using birth year trends differentiated by exposure to the education reform. Results obtained from this exercise are qualitatively and quantitatively similar to the main findings, which are shown in Table 4.4. In my main specifications, the birth cohort of 1986 was excluded from the analysis sample. This is because, as discussed in Section II, by Turkish law, it is unclear whether those who are born in 1986 are impacted by the reform. Nevertheless, I investigated the sensitivity of the main findings to the inclusion of the 1986 birth cohort. In order to approximate the probability of treatment for that group I assigned alternate values for exposure to the reform (i.e., Reform=0.33)[98]. These estimates, shown in Panel B of Table 4.7A for the male sample, are very similar to the baseline findings. I cluster the standard errors at the region-by-birth-year level in the main specifications. As a robustness check, I next estimate the standard errors using an alternative cluster unit, i.e., year of birth. Clustering at the birth year level lead to smaller standard errors possibly due to a smaller number of cluster units. To get a handle around the issue of small number of cluster units when clustering at the birth year level, I implemented wild-bootstrap clustering procedure as suggested by Cameron et al. 2008.[99] These estimates, shown in Panel C of Table 4.7A, indicate that inference based on bootstrapped standard errors, which are clustered at the birth year level, are very similar to the baseline results.

The main analyses include both non-students and students even though current students include a relatively small portion of the sample (9 percent). It is possible that the impact of education on health and health behaviors while being a student, and after completing formal schooling may differ. Therefore, in Panel D of Table 4.7A, I estimate the impact of Middle School Diploma on health behaviors and outcomes by limiting the analysis sample to males who were not students at the time of interview. This exercise does not cause change in the main findings. Table 4.7B employs the female sample to mimic the specification checks performed in Table 4.7A. Similar to those observed for the male sample, these exercises are fully in line with baseline results. Therefore, I conclude that the findings hold among those who completed their schooling.

[98] Assigning alternative treatment values to 1986 cohort (e.g., 0.25, 0.33), which are available upon request from the authors, lead very similar results presented in Table 4.4.
[99] The corresponding p-values obtained with the wild-bootstrapped standard errors presented in the {curly brackets}, which are obtained by using the boottest Stata command introduced by Roodman (2016).

As previously discussed, I conducted the main analysis using the sample of survey participants born three years before and after the pivotal birth cohort of 1986 (i.e., 1983 to 1999). I next reproduce the IV estimates for the estimation intervals including those born 4 to 2 years before and after 1986 to test the robustness of the main findings to bandwidth selection. As presented in Appendix Table C3, the baseline results are robust to employing estimation samples using different bandwidths.

4.7 Potential Mechanisms

As briefly discussed in the introductory section, allocative and productive efficiency proposed by Grossman (1972), time preferences (Becker and Mulligan 1997; Perez-Arce 2011), labor market effects (Angrist and Krueger 1991; Oreopoulos 2006; Brunello et al. 2009), delayed labor market entry (Brunello et al. 2009) and peer effects (Nakajima 2007; Sacardote 2011; Cawley 2015; Fortin and Yazbeck 2015) are among the usual suspects that may mediate the relationship between extended primary schooling and health behaviors and outcomes among young adults.

Using the available information provided by the HS, I first test whether labor force participation status, occupation, marital status, and household income explain the findings.[100], [101] Table 4.8A and 4.8B explore whether and how much the baseline estimates change when I control for each of these covariates for males and females, respectively. Results show that LFP, occupation dummies, and income at the household level play relatively small roles in explaining the relationship between schooling and health outcome measures at hand.[102]

[100] I am not able to test the role of personal earnings in explaining the relationship between schooling and health outcome because the HS does not provide information on individual earnings.
[101] The occupation categories are: Member of Armed Forces; Legislator, Senior Official, or Managers; Professional; Technician or Associate Professional; Clerk; Service or Shop and Sale Market Worker; Skilled Agricultural or Fishery Worker; Craft and Related Trades Worker; Plant and Machine operator or Assembler; Elementary Occupation Holder. The left-out category includes students and those who are not in the labor force.
[102] A reason why these potential mediators do not explain the link between education and health outcomes and behaviors may be the measurement error in their construction. To descriptively explore this possibility, in Appendix Table C4, I present the OLS estimates of health outcomes on *Middle School* with and without specifying for LFP, occupation, marital status, and household income. These results show that controlling for these covariates explains roughly 10 to 67 percent of the descriptive association between holding at least a middle school degree and health outcomes. Therefore, this exercise increases the confidence in the implications of the findings shown Table 4.8A and B, suggesting that LFP, occupation, marital status, and household income do not play a key role in connecting education to health outcomes among young adults.

However, I cannot deny the possibility that wages may serve as an important mediator between formal schooling and health outcomes given that I am not able to formally test the role of personal earnings due to data unavailability. Torun (2013) and Mocan (2014) use the 1997 education to examine the impact if education on earnings of young adults. Both studies document that while the extended primary schooling has a positive impact on wages of females, it has no effect on male earnings. Their findings suggest that labor market income may play a role in linking education to health among women. For instance, to the degree that cigarettes are normal goods, increased earnings due to more schooling may explain the positive impact of women's education on smoking.

In addition to the effects of education that could be transmitted through the economic outcomes, previous research demonstrates that peer effects due to schooling may play a key role in the formation of health behaviors (Sacerdote 2011). Relying on randomized college roommate assignment as the source of identifying variation, Yakusheva et al. (2014) show that while females are subject to weight gain/loss due to peer effects, they find no evidence of peer effects for male BMI. Using different identification techniques, a handful of other studies document that bodyweight of peers is strongly linked to the BMI of adolescents (Trogdon et al. 2008; Cawley 2015; Fortin and Yazbeck 2015). Christakis et al. (2008) use a longitudinal data set, Framingham Heart Study, which tracks a cohort of individuals repeatedly between 1971 and 2003, to explore whether and to what degree smoking behavior spreads from one person to another. They find that there is a strong negative relationship between a person's quitting decision and his or her friend's smoking; thus, they conclude that social networks play a central role in people's smoking behavior. The argument that tobacco use is a function of smoking behavior of peer and reference groups is supported by a number of studies as well (Powel et al. 2005; Nakajima, 2007; Card and Giuliano, 2013; Eisenberg et al. 2014).

Exploiting a randomized intervention in Dominican Republic, Jensen and Lleras-Muney (2012) find that extended schooling led to a reduction in smoking among males. They find no evidence of a direct effect of education on attitudes towards drinking and smoking, as well as time preferences. Their analysis, instead, show that change in income, exposure to a different set of peers after graduation are the primary mechanisms that connect education to smoking. Hence, schooling induced peer effects in health behaviors may stem not only from schoolmates, but also from colleagues and friends after graduation to the extent that formal education affects the composition a person's workmates and friends in this case as well. Relatedly,

Table 4.6
Reduced Form Estimates of the Impact of the 1997 Education Reform on Health Outcomes and Health Behaviors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
Panel A: Males									
Middle School Diploma	0.031	0.036	0.925***	-0.014*	0.083***	0.051***	-0.069**	-0.065*	-0.047
	(0.025)	(0.036)	(0.191)	(0.007)	(0.021)	(0.015)	(0.033)	(0.033)	(0.038)
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
Panel B: Females									
Middle School Diploma	0.028	0.047***	0.052	-0.047***	-0.045*	0.013	0.081***	0.053*	0.052***
	(0.021)	(0.010)	(0.203)	(0.016)	(0.023)	(0.011)	(0.020)	(0.031)	(0.018)
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends

Table 4.7a

Robustness Checks, Instrumental Variable Estimates, Males

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
Panel A: Quadratic Birth Cohort Function									
Middle School	0.252	0.293	7.288***	-0.109*	0.656***	0.399***	-0.501**	-0.479**	-0.345
Diploma	(0.213)	(0.291)	(1.918)	(0.057)	(0.176)	(0.139)	(0.231)	(0.234)	(0.263)
First Stage F-test	29.531	29.531	35.021	35.021	35.021	35.021	37.791	37.791	37.791
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
Panel B: Alternative Treatment Values for 1986 Cohort (T==0.50)									
Middle School	0.257	0.294	7.261***	-0.119*	0.652**	0.396**	-0.499**	-0.491*	-0.356
Diploma	(0.241)	(0.302)	(2.619)	(0.067)	(0.26)	(0.177)	(0.25)	(0.256)	(0.276)
First Stage F-test	18.517	18.517	20.548	20.548	20.548	20.548	22.931	22.931	22.931
Observations	4,129	4,129	4,018	4,018	4,018	4,018	3,339	3,339	3,339
Mean	[0.863]	[0.222]	[24.372]	[0.025]	[0.372]	[0.067]	[0.632]	[0.521]	[0.550]
Panel C: Clustering at the Birth Year Level									
Middle School	0.254***	0.294***	7.311***	-0.109***	0.655***	0.400***	-0.508***	-0.481***	-0.347***
Diploma	(0.074)	(0.104)	(1.066)	(0.023)	(0.069)	(0.121)	(0.074)	(0.061)	(0.091)
P-values	{0.123}	{0.223}	{0.027**}	{0.011**}	{0.013**}	{0.145}	{0.020**}	{0.022**}	{0.012**}
First Stage F-test	74.413	74.413	72.268	72.268	72.268	72.268	95.9	95.9	95.9
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Panel D: Excluding Students									
Middle School	0.238	0.537*	7.448***	-0.052	0.654***	0.450***	-0.620***	-0.567**	-0.490*
Diploma	(0.228)	(0.313)	(1.915)	(0.061)	(0.174)	(0.161)	(0.214)	(0.232)	(0.267)
First Stage F-test	24.04	24.04	28.777	28.777	28.777	28.777	32.811	32.811	32.811
Observations	3,153	3,153	3,069	3,069	3,069	3,069	2,655	2,655	2,655
Mean	[0.862]	[0.221]	[24.500]	[0.023]	[0.391]	[0.070]	[0.651]	[0.539]	[0.573]

In Panels A, B and D robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. In Panel C, clustering unit is birth year. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

Table 4.7b
Robustness Checks, Instrumental Variable Estimates, Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
Panel A: Quadratic Birth Cohort Function									
Middle School	0.191	0.330***	0.365	-0.366**	-0.360*	0.099	0.437***	0.286*	0.277**
Diploma	(0.144)	(0.092)	(1.555)	(0.145)	(0.191)	(0.089)	(0.115)	(0.165)	(0.109)
First Stage F-test	22.085	22.085	19.7	19.7	19.7	19.7	45.534	45.534	45.534
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]
Panel B: Alternative Treatment Values for 1986 Cohort (T==0.50)									
Middle School	0.191	0.307**	0.621	-0.372**	-0.331	0.106	0.438***	0.286*	0.288**
Diploma	(0.171)	(0.137)	(2.287)	(0.187)	(0.253)	(0.114)	(0.124)	(0.172)	(0.123)
First Stage F-test	20.131	20.131	17.606	17.606	17.606	17.606	36.531	36.531	36.531
Observations	5,275	5,275	4,882	4,882	4,882	4,882	4,186	4,186	4,186
Mean	[0.803]	[0.154]	[23.276]	[0.084]	[0.278]	[0.072]	[0.316]	[0.202]	[0.209]
Panel C: Clustering at the Birth Year Level									
Middle School	0.199	0.332***	0.406	-0.365***	-0.416**	0.098*	0.440***	0.286***	0.279***
Diploma	(0.129)	(0.058)	(0.99)	(0.065)	(0.043)	(0.05)	(0.049)	(0.02)	(0.034)
P-values	{0.244}	{0.018**}	{0.726}	{0.030**}	{0.027**}	{0.027**}	{0.021**}	{0.012**}	{0.014**}
First Stage F-test	46.361	46.361	44.853	44.853	44.853	44.853	47.883	47.883	47.883
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Panel D: Excluding Students									
Middle School	0.158	0.316***	-0.234	-0.322**	-0.390*	0.068	0.463***	0.295*	0.269**
Diploma	(0.16)	(0.088)	(1.624)	(0.161)	(0.204)	(0.092)	(0.111)	(0.155)	(0.105)
First Stage F-test	19.865	19.865	17.739	17.739	17.739	17.739	41.049	41.049	41.049
Observations	4,140	4,140	3,819	3,819	3,819	3,819	3,355	3,355	3,355
Mean	[0.794]	[0.152]	[23.485]	[0.075]	[0.295]	[0.078]	[0.320]	[0.205]	[0.214]

In Panels A, B and D robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. In Panel C, clustering unit is birth year. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

Table 4.8A
Controlling Potential Mediators IV Estimates, Males

	(1)	(2)	(3)	(4)	(5)	(6)
			Controlling for			
	Baseline Estimates	Labor Force Participation	Occupation	Marital Status	HH Income Categories	All Controls
Good Health	0.254	0.279	0.262	0.256	0.253	0.281
	(0.213)	(0.216)	(0.211)	(0.228)	(0.206)	(0.223)
Excellent Health	0.294	0.301	0.295	0.3	0.307	0.322
	(0.29)	(0.294)	(0.29)	(0.308)	(0.28)	(0.313)
First Stage F-test	29.399	29.758	29.644	25.968	32.416	25.225
Observations	3,531	3,531	3,531	3,531	3,531	3,531
BMI	7.311***	7.437***	7.350***	8.154***	7.109***	8.161***
	(1.946)	(1.953)	(1.92)	(2.11)	(1.825)	(2.100)
Underweight	-0.109*	-0.118**	-0.113**	-0.124**	-0.098*	-0.114*
	(0.057)	(0.058)	(0.057)	(0.062)	(0.055)	(0.063)
Overweight	0.655***	0.663***	0.658***	0.738***	0.651***	0.752***
	(0.175)	(0.175)	(0.173)	(0.196)	(0.17)	(0.204)
Obese	0.400***	0.407***	0.401***	0.445***	0.385***	0.441***
	(0.14)	(0.143)	(0.14)	(0.147)	(0.13)	(0.148)
First Stage F-test	34.838	35.395	34.955	31.089	36.21	29.213
Observations	3,430	3,430	3,430	3,430	3,430	3,430
Ever Smoked	-0.508**	-0.476**	-0.519**	-0.492**	-0.498**	-0.476*
	(0.231)	(0.232)	(0.234)	(0.238)	(0.234)	(0.263)
Current Smoker	-0.481**	-0.448*	-0.490**	-0.457*	-0.476**	-0.410
	(0.233)	(0.237)	(0.234)	(0.239)	(0.236)	(0.258)
Ever Smoked	-0.347	-0.315	-0.356	-0.322	-0.335	-0.273
Regularly	(0.264)	(0.263)	(0.261)	(0.274)	(0.266)	(0.290)
First Stage F-test	37.088	37.545	36.743	34.446	39.76	31.661
Observations	2,870	2,870	2,870	2,870	2,870	2870

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

the social context of health behaviors may be instrumental in shaping the nature of peer effects. As previously discussed, Cutler and Llreas-Muney (2012) find non-linear patterns between education and health behaviors at different stages of economic development.[103] More specifically, they find that while education.

[103] For similar findings please see: Strauss and Thomas 1998.

is inversely associated with smoking in developed societies, individuals with higher education levels have lower smoking rates in developing nations Therefore, if a person with more schooling has a greater tendency to interact with individuals who are more educated, where smoking is more prevalent among those with more schooling, it is possible that she may also smoke due to peer effects. To picture the health behaviors of individuals with whom our treated individuals (i.e., those who extend their primary schooling due to the 1997 education reform) are likely to interact due to increased educational attainment, in Appendix Table C5, I present the estimates from univariate

Table 4.8B
Controlling Potential Mediators IV Estimates, Females

	(1)	(2)	(3)	(4)	(5)	(6)
	Controlling for					
	Baseline Estimates	Labor Force Participation	Occupation	Marital Status	HH Income Categories	All Controls
Good Health	0.199	0.202	0.216	0.199	0.199	0.220
	(0.155)	(0.167)	(0.165)	(0.159)	(0.161)	(0.160)
Excellent Health	0.332***	0.344***	0.356***	0.328***	0.335***	0.329***
	(0.094)	(0.102)	(0.101)	(0.097)	(0.095)	(0.091)
First Stage F-test	21.493	19.534	20.809	22.492	24.554	25.287
Observations	4,504	4,504	4,504	4,504	4,504	4,504
BMI	0.406	0.934	0.733	0.534	0.564	0.571
	(1.575)	(1.692)	(1.688)	(1.598)	(1.704)	(1.729)
Underweight	-0.365**	-0.400**	-0.402**	-0.365***	-0.369**	-0.364***
	(0.145)	(0.157)	(0.163)	(0.132)	(0.149)	(0.133)
Overweight	-0.348*	-0.327	-0.352	-0.336*	-0.336*	-0.343*
	(0.197)	(0.21)	(0.217)	(0.201)	(0.204)	(0.207)
Obese	0.098	0.119	0.113	0.1	0.107	0.108
	(0.089)	(0.101)	(0.096)	(0.093)	(0.099)	(0.099)
First Stage F-test	19.165	17.312	18.006	21.669	20.151	22.597
Observations	4,165	4,165	4,165	4,165	4,165	4,165
Ever Smoked	0.440***	0.461***	0.435***	0.432***	0.460***	0.447***
	(0.112)	(0.123)	(0.12)	(0.115)	(0.118)	(0.119)
Current Smoker	0.286*	0.292*	0.28	0.271	0.306*	0.292*
	(0.164)	(0.176)	(0.174)	(0.166)	(0.168)	(0.162)
Ever Smoked Regularly	0.279***	0.288**	0.274**	0.263**	0.289**	0.263**
	(0.107)	(0.116)	(0.113)	(0.111)	(0.114)	(0.113)
First Stage F-test	44.822	40.128	40.39	43.854	50.014	39.989
Observations	3,578	3,578	3,578	3,578	3,578	3,578

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

regressions of measures of body weight and smoking indicators on Middle School Diploma. In this exercise, I limit the analysis sample to those born in the period 1971-80, who are outside of the sample and on average about 10 years older than my main sample, because the health behaviors of these birth cohorts are likely to resemble the health behaviors of potential peer and reference groups.[104] As the relevant literature suggests that observed peer effects are more likely to be gender specific (Nakajima 2007; Card and Giulianao 2013; Cawley 2015), I present these estimates by gender. In Panel A, among males, Middle School Diploma is positively associated with body weight and there is a negative relationship between earning at least a middle school degree and measures of smoking. Panel B finds that women who completed at least eight years of schooling have lower body weight, and they are more likely to smoke.

When I evaluate the findings in light of the nature of the health behaviors of potential peers and or influence groups, shown in Appendix Table C5, and the existing literature documenting the significance of peer effects in bodyweight and smoking, I interpret that the bodyweight and smoking estimates for both genders may at least in part be driven by peer effects. More specifically, if more schooling induces young women to interact with other educated females who are less likely to be overweight but more likely to smoke, my findings on bodyweight and smoking habits among females may at least partially be driven by peer influences. Similarly, the joint reduction in male smoking and the increase in male body weight may be driven by the fact that men who hold at least a middle school diploma are more likely to interact with more educated men who smoke less and more likely to be overweight.

There is also evidence that female smoking may be perceived as an indicator for women's empowerment in developing societies (Schaap et al. 2009; Hitchman and Fong 2011; Amos et al. 2012; Cutler and Lleras Muney 2012). Consistent with this view, Cesur and Mocan (2013), who use the 1997 education reform to examine the effect of education on religiosity, find that extended primary schooling leads to a reduction in the propensity to wear a head cover, and decline in

[104] Using the sample of individuals born in the period 1971-85 and 1976-85 produce estimates similar to those presented in Appendix Table C5 and available from the authors upon request.

declared religiosity. They note that being exposed to a different set of a friendship network due to extended secular education may have liberating effects on women. Therefore, in this context, the net effect of education on female smoking maybe positive if the liberation and social status aspects of education dominates the health concerns channel.

Furthermore, the findings among women may also be explained by the perceived interplay between female beauty, bodyweight, and smoking. It is well known that bodyweight is a key beauty indicator especially for females who are found to be more concerned of their looks compared to males (Averett and Korenman 1996; Brunello and D'Hombres 2007; Oreffice and Quintana-Domeque 2016). Research also shows that smoking is inversely associated with bodyweight (Pinkowish 1999; Rashad et al. 2006; Baum 2009; Aubin et al. 2012; Fletcher 2014; Courtemanche et al. 2016). Cawley et al. (2004), and Rees and Sabia (2010) document a positive association between body weight and smoking initiation among young women, which support the argument that young women smoke to get slimmer. In contrast, these studies do not find a relationship between measures of bodyweight and smoking initiation among males. If educated women are more knowledgeable about the smoking and bodyweight relationship, they may be more likely to smoke get slimmer. Therefore, these findings among women (i.e., negative effect on bodyweight and positive effect on smoking) are also consistent with this view. All in all, the BMI and tobacco use results among women are consistent with arguments emphasizing the role of social interaction in shaping health behaviors.

Time use may serve as a potential pathway between schooling and body weight. If education induces people to be physically more active in their daily lives, it may cause them to lose weight and vice versa. Education may influence how individuals allocate their times among various daily activities both at work and outside of professional life. Although testing whether and to what degree educational attainment impacts time use in a detailed fashion may produce valuable information for my purposes, I am not able to do it because the data lack sufficient information on time use. Nevertheless, using data from the Information and Communication Technology Usage for Households and Individuals (ICT) surveys collected by the TurkStat for the period 2011 and 2015, I estimate the impact of Middle School on measures of computer use and social media participation,

which may serve as reasonable proxies for time use. I construct three technology related time use measures, which represent daily computer, daily internet use, and social media participation (i.e., Daily Computer Use, Daily Internet Use, Social Media Participation).[105] Although I cannot separate whether these covariates represent technology use in professional or private life, they can still inform us on how educational attainment may impact total time use.

Technology use estimates are presented in Appendix Table C6. Results show that completing at least eight years of schooling has a positive and statistically significant impact on measures of technology use and social media participation among males. Meanwhile, extended primary schooling has no effect on computer use, internet use, and taking part in social media for women. Assuming that computer usage and physical activity are inversely associated, these results imply that some of the observed positive association between Middle School and male body weight may be explained by reduced physical activity.

4.8 Conclusion

Although theoretically well-determined, the causal impact of education on consumption of health inputs, and health outcomes is difficult to establish empirically. This is because education is correlated with unobservable attributes of individuals that can also impact health behaviors and health outcomes. To tease out the causal impact of education on health, recent research employed identification strategies that involved employing instrumental variables, which moved individuals' education levels, independent of their health outcomes. A widely-used strategy is to rely on education reforms that changed the mandatory years of education of particular cohorts in a country, without altering the minimum years of education mandated for older cohorts in that same country. Papers that used this identification strategy analyzed data from developed countries, and reported

[105] *Daily Computer Use* is coded as one for those who report using computers on a daily basis, and it is coded as zero otherwise. *Daily Internet Use*, which represent accessing internet on a daily basis, constructed analogously to the computer use variable. My final technology use indicator *Social Media Participation* takes the value of one for the survey respondents who report that they participate social networks (create user profile, post messages or other contributions to Facebook, Twitter etc.), and it is set equal to zero otherwise.

conflicting findings as to the impact of education on health. While some research revealed a positive impact of education on health outcomes (e.g. Brunello et al. 2013, Chou et al. 2010, Lleras-Muney, 2005), others could not find a significant impact (e.g. Clark and Royer, 2013, Meghir, Palme and Simeonova 2012, Lindeboom et al. 2009). Inference from developing countries, based on exogenous variation in education, is limited (Alsan and Cutler 2013; Agüero and Bharadwaj 2014; Cannonier and Mocan 2014; Behrman 2015a; Behrman et al. 2015; De Neve et al. 2015; Duflo et. al 2015; Huang 2015).

This paper examines the impact of educational attainment on health and health behaviors using data from the Republic of Turkey, which is a middle-income, low education country with per capita income of less than 11,000 in 2014, and average education of 6.5 years among those who are 25 years or older. This study exploits an education reform that increased the mandatory years of education from an elementary school diploma (5 years of schooling) to a middle school diploma (8 years of schooling). Accordingly, cohorts born after 1986 were exposed to the mandate of the law and had to complete eight years of schooling, while those who were born before 1986 were exempt from the mandate.

I use a large nationwide health survey conducted by the Turkish Statistical Institute in 2008, 2010, 2012 and 2014 to identify the causal impact of education on self-reported health, body weight, smoking, fruit and vegetable consumption, as well as the likelihood of receiving a flu shot. I focus on individuals who are between the ages of 19 and 31 at the time of the survey (born between 1983 and 1989).

Exposure to the reform is used as an instrument for educational attainment. Consistent with other studies that exploited the same Turkish education reform, but used different data sets (Cesur and Mocan 2013, Mocan 2014, Kirdar et. al. 2014, Dusun and Cesur 2016; Keskin and Erten 2016), I find that the reform had a significant impact on educational attainment of both men and women.

The results show that an increase in educational attainment has contrasting effects on health behaviors and outcomes of male and female young adults. For males, I find that earning at least a middle school degree increases body weight, and reduces the likelihood of using tobacco

products. To the contrary, among women, at least eight years of schooling has a negative impact on the likelihood of being overweight and it has a positive effect on chances of smoking. I do not find a statistically significant relationship between earning a middle school degree and daily fruit and vegetable consumption, as well as flu shot intake. A variety of robustness analyses do not alter the results.

Supplemental analyses provide little evidence for the argument that economic outcomes, drive my findings. In particular, I find that controlling for labor force participation, occupational dummies, and household income explain only a small fraction of impact of education on health and health behaviors. Nevertheless, because the data I use do not contain information on labor market income, I cannot fully rule out the possibility that earnings may still explain part of relationship between education and health.

Given that conventional economic measures do not seem to be the major actors in explaining the findings of this study, I explored the role of peer effects and time use, which are among the potential mechanisms that may link education to health behaviors and outcomes.

A number of studies suggest that schooling induced peer effects may play a central role in determining health behaviors in terms of body weight and tobacco use. Therefore, I explored the associations between education and health behaviors of individuals who were born between 1971 and 1980 to have an idea on the nature of health behaviors and education relationship. I pick these birth cohorts because they may serve as potential peer and influence groups for the treated individuals in the main estimation sample. Among these individuals (i.e., those born in 1970s), I observed that while more educated males are less likely to smoke and score higher in BMI, women with more schooling are more likely to smoke and score lower in BMI. All in all, when I jointly evaluate the results on BMI and tobacco use the associated literature on peer effects and my descriptive exploration of health behaviors of potential peer and reference groups, I conclude that at least part of the variation between extended primary schooling, and body weight and smoking may be attributed to peer effects.

Even though the data do not allow us to investigate the impact of earning at least a middle school diploma on how survey respondents allocate their time among different activities, I use data

from the ICT to examine how Middle School Diploma affects computer use and the likelihood of social network participation. These estimates document that holding at least a middle school diploma caused men to significantly increase their likelihood of daily computer and internet use in addition to increasing their likelihood of social media participation. I did not find a relationship between education and computer and social media use among women. Hence, I conclude that a decrease in physical activity because of extended primary schooling may have caused men to gain weight.

Given that extended schooling may impact different populations differentially, it may be worthwhile to explore whether and to what degree these estimates hold in different subsamples, such as urban versus rural residence, high versus low income and parental educational attainment status. Unfortunately, as the HS does not provide information on many of these covariates, I am not able to investigate the potential heterogeneity of the impact of education on outcomes of interest by different subsamples.

In conclusion, the findings of this study imply that compulsory education on itself may not be sufficient as a policy tool to improve public health. That is, although schooling may induce some populations to exhibit health promoting behaviors, it may lead to an increase it health depreciating behaviors for some others.

CHAPTER 5. CONCLUSION

5.1 The Value of Mandating Maternal Education in a Developing Country

This study investigates the causal impact of mandatory maternal education on child health, measured by birth weight and child mortality, in a developing country setting. Although several studies examine the aforementioned relationship using quasi-experimental identification strategies, the existing literature on causal relationship between maternal education and child health measures is not complete: First, there is no uniformity on the findings of the literature on whether maternal schooling improves the health outcomes of the children. Second, the local average treatment effects of maternal education, induced by different types of natural experiments on child health are not well-distinguished. Third, almost all the existing studies suffer from limited statistical power, due to small sample sizes, or weak first stages.

In order to close these gaps in the literature, I employ two large datasets from Republic of Turkey and exploit the 1997 education reform, which increased the compulsory years of schooling from 5 to 8 years, as a source of identifying variation. More specifically, the reform forced individuals born after 1986 to obtain at least eight years of education. On the other hand, for the individuals who were born before 1986 schooling decision after five years of elementary school left to the discretion of families. This unique characteristic of the 1997 reform in Turkey, enables me to identify the local average treatment effect of compulsory education among women at the lowest end of the educational attainment distribution.

Using exposure to the reform as an instrumental variable to obtain at least a middle school degree, the current study shows that mothers who earned at least a middle school degree are less likely to deliver babies in very low birth weight (less than 1500 grams), low birth weight (less than 2500 grams), and high birth weight (more than 4500 grams) categories, as opposed to the mothers who have an elementary school degree, or less. Moreover, my analysis indicates that these mothers are less likely to have a child deceased by age five, and have lower propensity to deliver premature babies. I also find evidence suggesting that additional three years of schooling, due to the exposure to the 1997 education reform, increases the propensity of normal births, and decreases the likelihood of maternal smoking.

These findings have very important policy implications: First and foremost, these results indicate that increase in maternal education has very important benefits in terms of child health. Second, from policy makers' point of view, these results provide robust evidence that enacting compulsory schooling

laws and enforcing them in a developing country settings has very important implications on improving child health. Consequently, besides of allocating more resources to primary and secondary education in developing countries, policy makers can enact and enforce compulsory schooling laws.

5.2 Transforming Lives: The Impact of Compulsory Schooling on Hope and Happiness.

Subjective well-being has become a popular topic among researchers, due to the potential problems in standard economic indicators of welfare. There is a growing literature to understand the causal determinants of happiness. Third chapter of this dissertation examines the causal impact of education on various subjective well-being measures. Using data drawn from Turkish Statistical Institute's Life Satisfaction Surveys (2009-2014), this study presents the first causal estimates of the effect of extended primary education on happiness among young adults. I use plausibly quick and unexpected 1997 education reform to identify the causal effect of education on subjective well-being measures. By merging elementary and secondary schools under the same entity, the reform increased the mandatory years of schooling from 5 to 8 years for birth cohorts who were subject to reform (1987 is the first fully affected cohort), and had no impact on older cohorts. Using this exogenous variation, I first document that the enforcement of the 1997 reform led to substantial increase in middle school completion rates for females and males, respectively.

The instrumental variable approach reveals that obtaining at least a middle school degree leads to an increase in happiness of young women. Moreover, my analysis indicates that additional years of schooling also increase the probability of being satisfied with various life domains for the case of women. To the contrary, I do not find evidence that additional years of education enhances the subjective well-being of young men. Moreover, although relatively imprecise, I document that obtaining at least a middle school degree decreases the propensity of being happy for the case of males. These gender specific results are consistent with Resource Substitution Theory (RST), which foresees individuals with few alternative resources (i.e., women) may benefit more from additional schooling in comparison to individuals who have access to greater resources (i.e., men).

Auxiliary analysis reveals that being hopeful about one's own future well-being explains the impact of education on happiness among women. On the other hand, I find evidence that the potential

divergence between achievements and aspirations may be the reason behind the findings for the case of young men.

5.3 The Impact of Education on Health and Health Behaviors in a Middle-Income Low-Education Country

The theoretical foundations of the impact of education on health behaviors is well-established. Nevertheless, the vast majority of the empirical studies investigating the health returns to education use data from developed countries. This study investigates the impact of education on health and health behaviors using data from Republic of Turkey. To reveal the causal impact of education on health outcomes, I rely on 1997 education reform which increased the mandatory education from elementary school to middle school level. Therefore, the 1997 reform obliged individuals born after 1986 to complete at least a middle school degree, while the older cohorts were exempt from the mandate of the reform.

Using exposure to the 1997 reform as an instrumental variable for educational attainment, I find that additional years of schooling, due to reform, leads higher body weight and lower propensity to smoke among males. For females, while obtaining at least a middle school degree decreases the probability of being overweight, it has a positive effect of likelihood of smoking.

Our supplementary analyses reveal that "the usual suspects" labor force participation, occupation, marital status, and household income does not explain our findings. [106]Nevertheless, in this study I find evidence that schooling induced peer effects, daily computer and internet use play important roles in explaining the findings of this study.

[106] However, we cannot deny the possibility that personal earnings may serve as an important mediator between schooling and health outcomes given that we are not able to test the role of personal earnings due to data unavailability.

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APPENDIX A: SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 2

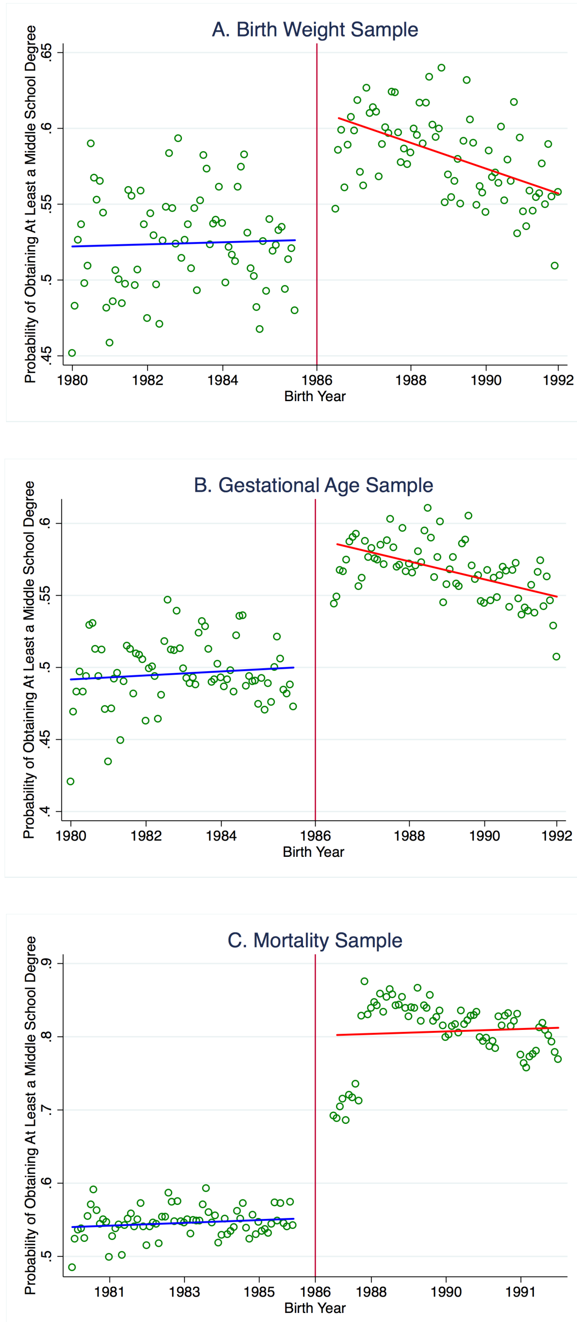


Figure A.1: Trends in Educational Attainment by Birth Cohort

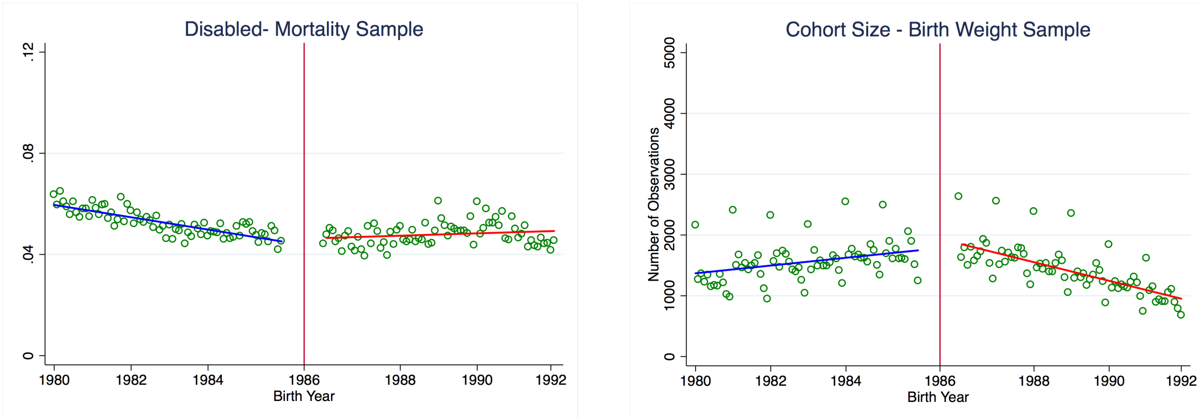


Figure A.2: Balanced Covariates

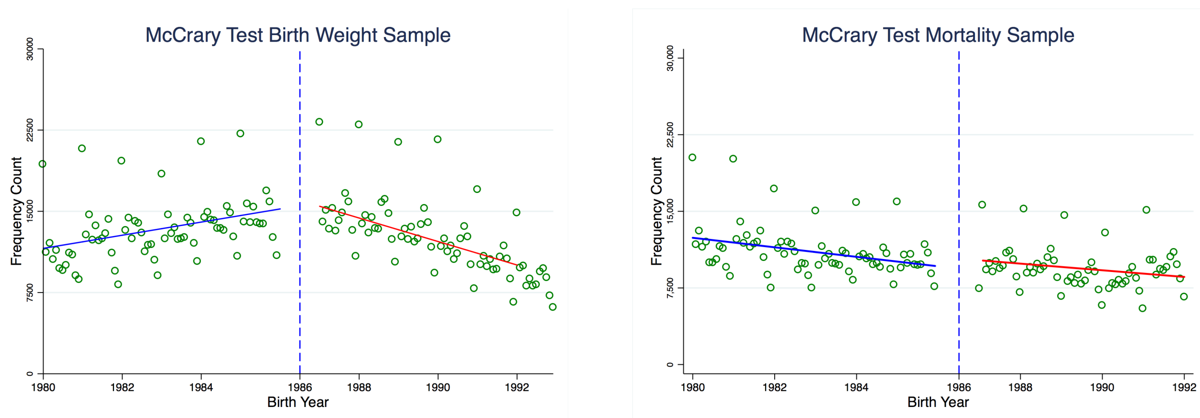


Figure A.3: McCrary Test

Table A.1
Trends in the Number of Elementary, Middle, and Primary Schools in 1990s

	Total Number of Schools		
Academic Year	Elementary School	Middle School	Primary School
1991-92	50,701	7,025	N/A
1992-93	49,974	7,544	N/A
1993-94	49,599	8,318	N/A
1994-95	48,429	8,897	N/A
1995-96	49,240	9,385	N/A
1996-97	47,313	9,830	N/A
1997-98	N/A	N/A	46,801
1998-99	N/A	N/A	44,433
1999-00	N/A	N/A	32,634
2000-01	N/A	N/A	35,356

The data come from the Ministry of National Education's 2000-01 Yearbook.

Table A.2
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
School Not Accessible	0.055	0.037	0.075
Did not like school/ did not pass exams	0.195	0.239	0.150
Cannot afford schooling expenses	0.056	0.074	0.036
Had enough schooling	0.007	0.006	0.007
Needed to earn money	0.014	0.021	0.006
Family needed help	0.016	0.012	0.020
Take care of children	0.001	0.001	0.001
Got married	0.021	0.023	0.020
His/her family doesn’t permit child to go to school	0.401	0.362	0.446
Other	0.204	0.191	0.220
Don’t Know/ Missing	0.023	0.017	0.028
Number of Observations	1,563	829	734

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 15-24 who stopped attending school upon earning a 5-year elementary school diploma by reasons for not continuing their education. Women who were between 15 and 24 were not affected by the 1997 education reform.

Table A.3
Studies Using Natural Experiments to Estimate the Effect of Maternal Education on Child Health in Developing Countries

Study	Country	Identification	Outcomes Coinciding With This Study	Impact of Additional Year of Schooling	No. of Observations
Breierova and Duflo (2004)	Indonesia	Age and region specific exposure to the school construction program (INPRES) that took place between 1973 and 1978.	Total Number of Children Died	45% decrease in the total number of children died	122,818
Chou et al. (2010)	Taiwan	Age and region specific exposure to the school construction program joint with compulsory schooling reform in 1968-69.	Birth weight, Child mortality	Lowers the incidence of low birth weight by 25%.	7,853*
Grépin and Bharadwaj (2015)	Zimbabwe	Age specific exposure to education reform followed by independence in 1980.	Child mortality	Lowers the child mortality by 21%.	7813**
Günes (2015)	Turkey	Age specific exposure to the 1997 education reform and the intensity of the additional classrooms constructed in mother's birth province due to the reform	Birth weight, height/weight for age for age z-scores	Decreases very low birth weight by 85%.	1677
Keats (2014)	Uganda	Exposure to a national reform that eliminates primary school fees in 1997	Preventative care and chronic malnutrition	Null effect for mortality. Decreases stunting by 18% and anemia by 14%.	8,348
Makate and Makate (2016)	Malawi	Elimination of schooling fees due to the 1994 Universal Primary Schooling Program.	Under 5 Mortality Infant Mortality	Lowers mortality by 6.48%.	40,540
Zhang (2014)	China	Mothers' exposure to high school closures after the Cultural Revolution	Birth weight, Child mortality	Null effect.	22,444

* Chou et al. (2010) use data aggregated at the parental county of by birth year level.
** The number of mothers in the sample

Table A.4
The Impact of Being Exposed to the 1997 Education Reform on the Likelihood of Providing Missing Information on Outcome Measures and Maternal Education

Panel A. Missing Birth weight and Mothers Education on Reform	
Education Reform	0.000197
	(0.00069)
	[2,943,692]
Panel B. Missing Gestational Age and Mothers Education on Reform	
Education Reform	0.001056
	(0.00125)
	[2,943,692]
Panel C. Missing Birth Method and Mothers Education on Reform	
Education Reform	0.000066
	(0.001325)
	[2,943,692]
Panel D. Missing Smoking Behavior and Mothers Education on Reform	
Education Reform	-0.001275
	(0.00148)
	[2,943,692]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth month differentiated by exposure to the 1997 reform status, mother's month of birth dummies, province fixed effects, child's birth year fixed effects, and male child dummy. Models are weighted by number of women in fertility age (ages 15 to 49) in the province. Number of observations are in square brackets [].

Table A.5
Instrumental Variable Estimates with Alternative Cluster Levels

	(1) Cluster by Province Level	(2) Cluster by Birth Month Level	(3) Cluster by Birth Month by Province	(4) Cluster by Birth Year by NUTS1	(5) Cluster by Birth Year Level	(6) Wild Bootstrap P-Values
			Panel A: Birth Outcomes			
Dependent Variables						
Log Birthweight	0.038***	0.038	0.038	0.038**	0.038**	0.040
	(0.008)	(0.032)	(0.03)	(0.019)	(0.015)	
Very Low Birthweight	-0.047***	-0.047***	-0.047***	-0.047***	-0.047***	0.015
	(0.011)	(0.016)	(0.016)	(0.012)	(0.012)	
Low Birthweight	-0.074**	-0.074*	-0.074*	-0.074**	-0.074***	0.005
	(0.035)	(0.043)	(0.044)	(0.029)	(0.027)	
High Birthweight	-0.026*	-0.026**	-0.026**	-0.026***	-0.026***	0.055
	(0.016)	(0.011)	(0.013)	(0.010)	(0.007)	
1st Stage F-test	{31.689}	{81.28}	{101.742}	{53.474}	{26.857}	
	[186,840]	[186,840]	[186,840]	[186,840]	[186,840]	
Log Gestational Age	0.023***	0.023***	0.023***	0.023***	0.023***	0.005
	(0.006)	(0.003)	(0.004)	(0.004)	(0.003)	
Preterm<37 weeks	-0.050***	-0.050***	-0.050***	-0.050***	-0.050***	0.020
	(0.018)	(0.018)	(0.019)	(0.014)	(0.013)	
1st Stage F-test	{73.456}	{314.821}	{441.995}	{127.103}	{65.52}	
	[1,486,353	[1,486,353]	[1,486,353]	[1,486,353]	[1,486,353]	
Log Head Circumference	0.013	0.013	0.013	0.013*	0.013**	0.141
	(0.009)	(0.01)	(0.01)	(0.008)	(0.007)	
1st Stage F-test	{31.953}	{81.238}	{101.947}	{54.068}	{27.103}	
	[186,505]	[186,505]	[186,505]	[186,505]	[186,505]	
Any Child Died	-0.018**	-0.018***				
	(0.008)	(0.006)	N/A	N/A	N/A	N/A
1st Stage F-test	{205.309}	{374.9}				
	[340,091]	[340,091]				

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, province fixed effects, and province specific maternal birth month-year trends. Birth outcome models also control for child's birth year fixed effects and 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 mortality estimations. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Table A.6
Instrumental Variable Estimates of the Impact of Maternal Education on Child Health
Using the Turkish Demographic and Health Surveys

	(1) 80-92	(2) 81-91	(3) 82-90	(4) 83-89	(5) 76-96
Panel A: Birth Outcomes					
Dependent Variables					
Log Birthweight	-0.068	-0.012	-0.025	-0.173	0.089
	(0.136)	(0.13)	(0.148)	(0.306)	(0.124)
Very Low Birthweight	0.033	-0.023	-0.011	0.198	-0.004
	(0.051)	(0.046)	(0.049)	(0.164)	(0.052)
Low Birthweight	0.116	0.068	0.141	0.084	-0.012
	(0.196)	(0.192)	(0.222)	(0.447)	(0.165)
High Birthweight	-0.035	-0.044	-0.04	-0.015	-0.014
	(0.067)	(0.066)	(0.08)	(0.152)	(0.054)
1st Stage F-test	{13.432}	{13.884}	{10.053}	{2.637}	{17.665}
	[3,365]	[2,808]	[2,211]	[1,702]	[5,111]
Current Smoker	-0.173	-0.114	-0.274	-0.214	-0.117
	(0.163)	(0.159)	(0.196)	(0.28)	(0.093)
1st Stage F-test	{42.531}	{44.738}	{36.788}	{21.549}	{87.729}
	[6,564]	[5,391]	[4,229]	[3,144]	[11,175]
Panel B: Child Mortality					
Any Child Died	0.026	-0.051	-0.14	-0.370*	-0.024
	(0.081)	(0.079)	(0.095)	(0.205)	(0.071)
1st Stage F-test	{16.133}	{15.642}	{12.543}	{5.071}	{20.974}
	[4,692]	[3,837]	[2,976]	[2,207]	[7,930]

These models estimated using data are from all available rounds of the Turkish Demographic Health Surveys collected in 2003, 2008, and 2013. The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth-date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects and province specific maternal birth month-year trends. Survey weights provided by the DHS are used in estimations. Column headings pertains to data estimation intervals based on year of birth year of the mother. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Table A.7
Instrumental Variable Estimates
Assigning Alternative Probabilities to the Likelihood of Earning At least a Middle School Diploma Among Those Who Were Bound by the Reform but Reported Their Highest Level of Educational Attainment as Elementary School Diploma in the MHBOD

	(1)	(2)	(3)	(4)
	Assigned Probability of Obtaining At Least a Middle School Degree Among Individuals Who Were Bound by the Reform but Reported Their Highest Level of Educational Attainment as Elementary School Diploma (i.e., 5-Years of Basic Education)			
Dependent Variables	(P)=1	(P)=0.75	(P)=0.50	(P)=0.25
Log Birthweight	0.010**	0.012**	0.015**	0.022**
	(0.004)	(0.005)	(0.007)	(0.010)
Very Low Birthweight	-0.012***	-0.014***	-0.019***	-0.027***
	(0.002)	(0.003)	(0.003)	(0.005)
Low Birthweight	-0.019***	-0.023***	-0.030***	-0.043***
	(0.006)	(0.008)	(0.010)	(0.015)
High Birthweight	-0.007***	-0.008***	-0.011***	-0.015***
	(0.002)	(0.003)	(0.004)	(0.006)
1st Stage Estimates	0.302***	0.245***	0.189***	0.132***
	(0.01005)	(0.00973)	(0.00960)	(0.00964)
1st Stage F-test	{899.901}	{633.945}	{386.378}	{187.712}
	[186,840]	[186,840]	[186,840]	[186,840]
Log Gestational Age	0.007***	0.008***	0.010***	0.014***
	(0.001)	(0.001)	(0.001)	(0.002)
Preterm<37 weeks	-0.015***	-0.018***	-0.023***	-0.031***
	(0.004)	(0.005)	(0.006)	(0.009)
1st Stage Estimates	0.283***	0.233***	0.183***	0.133***
	(0.008)	(0.008)	(0.007)	(0.007)
1st Stage F-test	{1175.917}	{912.288}	{630.541}	{361.894}
	[1,486,353]	[1,486,353]	[1,486,353]	[1,486,353]
Log Head Circumference	0.003*	0.004*	0.005*	0.008*
	(0.002)	(0.002)	(0.003)	(0.004)
1st Stage Estimates	0.302***	0.246***	0.189***	0.132***
	(0.010)	(0.009)	(0.009)	(0.009)
1st Stage F-test	{909.440}	{640.618}	{390.408}	{189.704}
	[186,505]	[186,505]	[186,505]	[186,505]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects, province specific maternal birth month-year trends, child's birth year fixed effects, and male child dummy. Birth outcome models are weighted by number of women in fertility age (ages 15 to 49) in the province. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Table A.8
Summary Statistics for Maternal Behaviors

	All	Control	Treatment
Normal Birth	0.4644	0.4166	0.5174
	(0.4987)	(0.493)	(0.4997)
	[1,594,793]	[812,985]	[781,808]
Ever Smoked	0.1025	0.1203	0.0823
	(0.3032)	(0.3253)	(0.2748)
Current Smoker	0.0683	0.0792	0.056
	(0.2522)	(0.27)	(0.2299)
	[550,001]	[281,422]	[268,579]

The means are generated using data from the Ministry of Health Birth Outcomes Data (MHBOD). Individuals born between 1987 and 1991 constitute the treatment group and those who were born between 1981 and 1985 form the control group. Standard deviations are in parentheses. Variables in Panel A are weighted by number of women in fertility age (ages 15 to 49) in the province. Number of observations are in square brackets [].

Table A.9
The Impact of Maternal Education on Maternal Behaviors, Robustness Checks

	(1) Baseline Estimates	(2) Intent to Treat Estimates	(3) Alt. Treatment for 1986 Birth Cohort T==0.25	(4) Quadratic BC Control Function	(5) Local Linear IV with rdrobust command	(6) With Province Specific Birth Cohort Trends
Dependent Variables						
Normal Birth	0.269***	0.02289***	0.214***	0.262***	0.237***	0.266***
	(0.063)	(0.00238)	(0.058)	(0.058)	(0.044)	(0.039)
1st Stage F-test	{76.495}	---	{80.488}	{75.112}		{167.822}
	[1,594,793]	[1,594,793]	[1,775,812]	[1,594,793]	[1,594,793]	[1,594,793]
Ever Smoked	-0.084***	-0.00834***	-0.068**	-0.083***	-0.067**	-0.084***
	(0.026)	(0.00192)	(0.028)	(0.024)	(0.028)	(0.023)
Current Smoker	-0.034***	-0.00341***	-0.030**	-0.034***	-0.029**	-0.034***
	(0.013)	(0.00122)	(0.014)	(0.012)	(0.013)	(0.013)
1st Stage F-test	{98.002}	---	{107.353}	{99.921}		{222.429}
	[550,001]	[550,001]	[612,733]	[550,001]	[550,001]	[550,001]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, province fixed effects, child's birth year fixed effects, and male child dummy. Birth outcome models are weighted by number of women in fertility age (ages 15 to 49) in the province. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Table A.10
Robustness of the IV Estimates of the Impact of Maternal Education on Maternal Behaviors to Using Alternative Bandwidths

	(1) 80-92	(2) 81-91	(3) 82-90	(4) 83-89
Dependent Variables				
Normal Birth	0.242***	0.266***	0.245***	0.208***
	(0.043)	(0.039)	(0.031)	(0.023)
1st Stage F-test	{175.861}	{167.822}	{221.386}	{319.168}
	[1,834,716]	[1,594,793]	[1,316,711]	[1,017,506]
Ever Smoked	-0.083***	-0.084***	-0.057***	-0.056**
	(0.018)	(0.023)	(0.022)	(0.026)
Current Smoker	-0.049***	-0.034***	-0.019	-0.030***
	(0.012)	(0.013)	(0.012)	(0.012)
1st Stage F-test	{231.656}	{222.429}	{228.806}	{251.143}
	[631,108]	[550,001]	[455,856]	[353,401]

The entries in parentheses are standard errors of the estimated coefficients, clustered at the birth year by province level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, and province fixed effects, province specific maternal birth month-year trends, child's birth year fixed effects, and male child dummy. Models are weighted by number of women in fertility age (ages 15 to 49) in the province. Column headings pertain to data estimation intervals based on year of birth year of the mother. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

Table A.11
Instrumental Variable Estimates with Alternative Cluster Levels- Maternal Behaviors

	(1) Cluster by Province Level	(2) Cluster by Birth Month Level	(3) Cluster by Birth Month by Province	(4) Cluster by Birth Year by NUTS1	(5) Cluster by Birth Year by NUTS2	(6) Cluster by Birth Year Level	(7) Wild Bootstrap P-Values
Dependent Variables							
Normal Birth	0.266***	0.266***	0.266***	0.266***	0.266***	0.266***	0.000
	(0.045)	(0.041)	(0.039)	(0.041)	(0.039)	(0.058)	
1st Stage F-test	{68.826}	{351.223}	{488.548}	{140.562}	{162.72}	{77.787}	
	[1,594,793]	[1,594,793]	[1,594,793]	[1,594,793]	[1,594,793	[1,594,793]	
Ever Smoked	-0.084***	-0.084***	-0.084***	-0.084***	-0.084***	-0.084***	0.005
	(0.023)	(0.027)	(0.03)	(0.024)	(0.023)	(0.02)	
Current Smoker	-0.034***	-0.034*	-0.034*	-0.034**	-0.034***	-0.034***	0.090
	(0.007)	(0.02)	(0.02)	(0.014)	(0.013)	(0.012)	
1st Stage F-test	{56.732}	{281.553}	{329.173}	{188.578}	{218.393}	{147.67}	
	[550,001]	[550,001]	[550,001]	[550,001]	[550,001]	[550,001]	

The entries in parentheses are standard errors of the estimated coefficients. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models include re-centered birth date in months differentiated by exposure to the 1997 reform status, mother's month of birth dummies, province fixed effects, province specific maternal birth month-year trends, child's birth year fixed effects, and male child dummy. Models are weighted by number of women in fertility age (ages 15 to 49) in the province. Column headings pertain to alternative cluster levels. Number of observations are in square brackets []. 1st Stage F-Statistics are provided in curly brackets {}.

APPENDIX B: SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 3

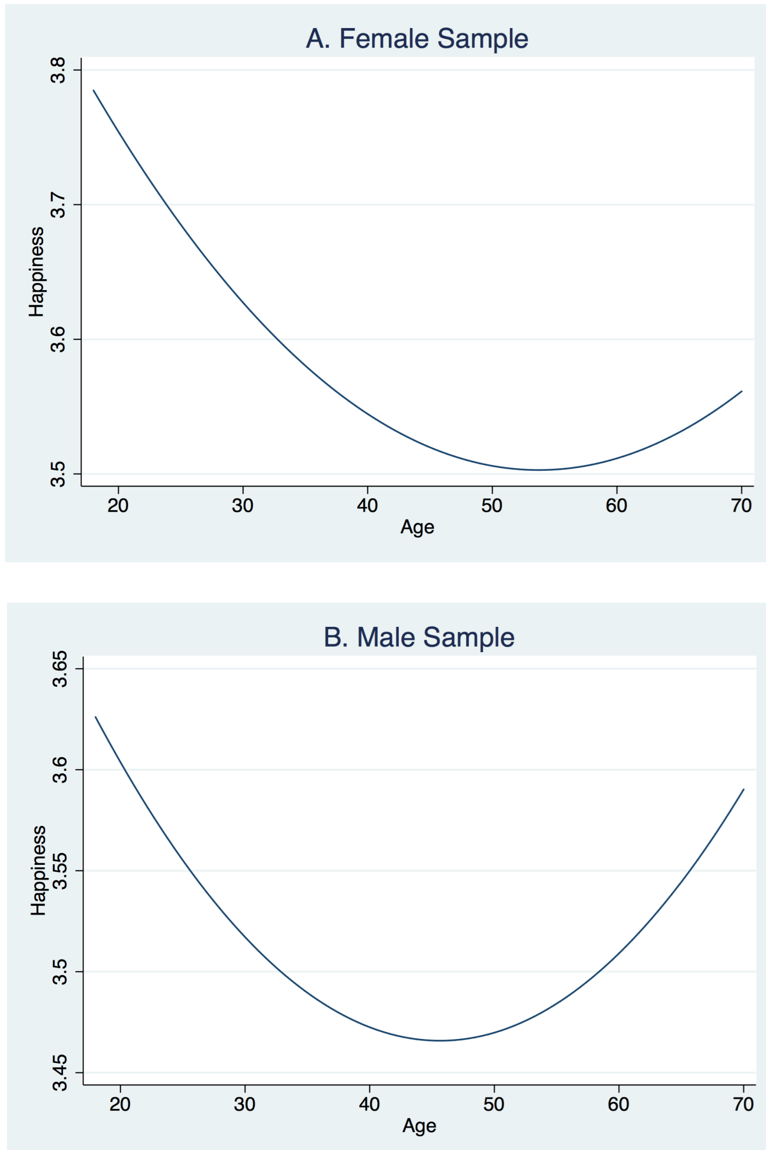


Figure B.1: Age Gradient of Happiness among Turkish Citizens

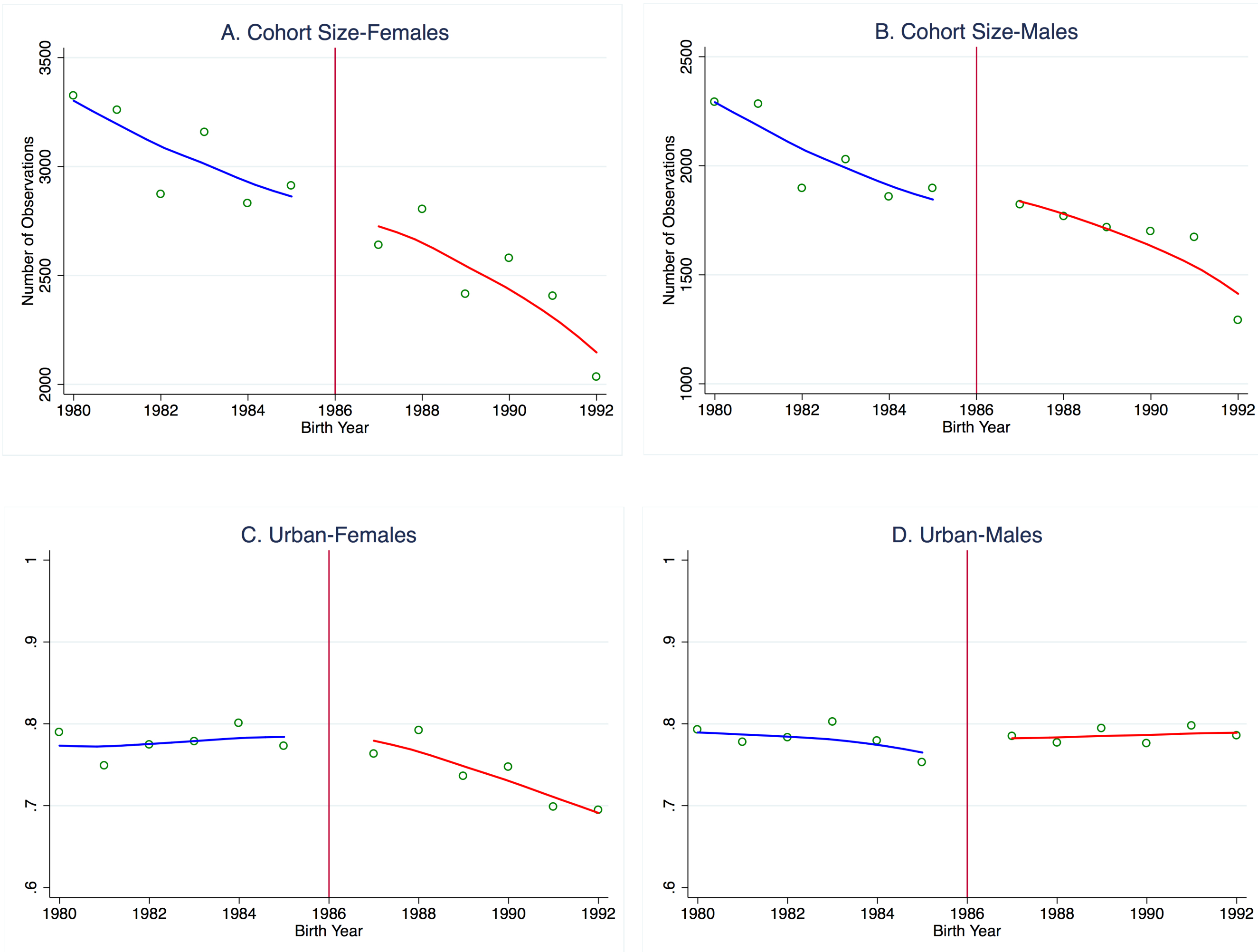


Figure B.2: Balanced Covariates

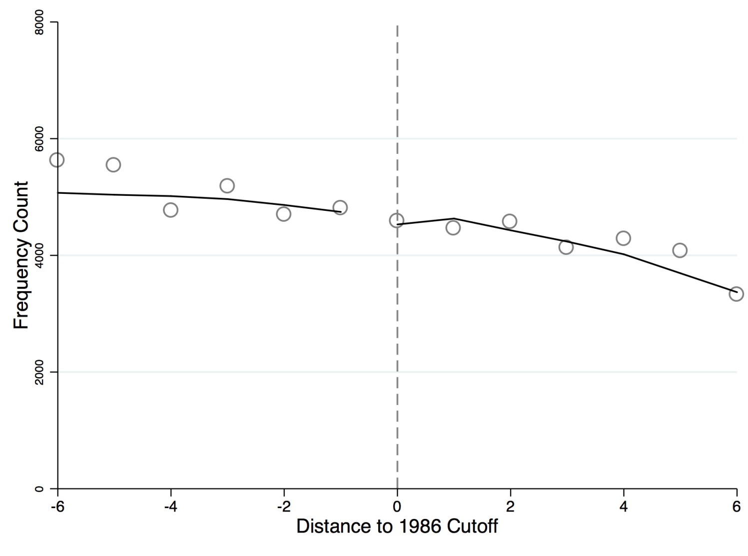


Figure B.3: McCrary Test

Table B.1
The Impact of Reform on High School and College Education

	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	80-92	81-91	82-90	83-89	80-92	81-91	82-90	83-89
Panel A: The Impact of Reform on Holding at Least a High School Degree								
Reform	0.109***	0.113**	0.077**	0.092***	0.073*	0.069	-0.009	-0.042
	(0.034)	(0.038)	(0.023)	(0.021)	(0.036)	(0.039)	(0.01)	(0.021)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
Panel B: The Impact of Reform on Holding at Least a College Degree								
Reform	0.050*	0.055**	0.065***	0.067***	0.001	0.016	-0.008	-0.024
	(0.023)	(0.02)	(0.011)	(0.012)	(0.017)	(0.014)	(0.01)	(0.013)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.2
Linear-RD Instrumental Variables and RD Estimates Obtained from the Computational Procedure Offered by Nichols (2007)

	(1) Happy	(2) Satisfied With Earnings	(3) Satisfied With HH Income	(4) Satisfied With Marriage	(5) Satisfied With Health	(6) Satisfied With Friends	(7) Satisfied With Job	(8) Satisfied With Neighborhood	(9) Satisfied With Housing	(10) Satisfied With Family Rel.	(11) Composite LS Index
Panel A: Females											
Middle School	0.279***	0.811***	0.121	0.465***	0.252*	0.371**	0.395**	0.387	0.385***	0.297***	1.752***
Diploma	(0.068)	(0.165)	(0.333)	(0.160)	(0.141)	(0.159)	(0.172)	(0.250)	(0.097)	(0.104)	(0.527)
First Stage F-test	28.16	17.39	27.67	12.72	28.05	25.94	34.37	28.05	28.15	25.94	25.94
Reform	0.047***	0.094***	0.019	0.071***	0.040**	0.058***	0.062**	0.062**	0.061***	0.046***	0.274***
	(0.018)	(0.013)	(0.054)	(0.009)	(0.016)	(0.015)	(0.030)	(0.029)	(0.006)	(0.011)	(0.043)
Observations	33,231	9,355	38,519	16,364	43,738	27,870	7,852	43,738	33,231	27,870	27,870
Optimal Bandwidth	6.34	7.84	6.53	4.45	8.42	4.52	5.74	8.37	6.45	4.53	5.49
Panel B: Males											
Middle School	-0.300	-0.941***	-0.400**	-0.210***	-0.369***	-0.256***	0.138	-0.144*	-0.540***	0.123	-1.027***
Diploma	(0.402)	(0.181)	(0.165)	(0.057)	(0.102)	(0.093)	(0.086)	(0.076)	(0.104)	(0.093)	(0.375)
First Stage F-test	72.76	41.67	72.76	135.26	88.59	93.04	82.01	72.76	36.98	72.76	88.59
Reform	-0.035	-0.092***	-0.047**	-0.038***	-0.044***	-0.030***	0.018	-0.017*	-0.055***	0.014	-0.123***
	(0.050)	(0.019)	(0.022)	(0.009)	(0.013)	(0.009)	(0.011)	(0.009)	(0.006)	(0.010)	(0.046)
Observations	22,222	11,084	22,222	15,175	25,543	34,804	18,811	22,222	18,639	22,222	25,543
Optimal Bandwidth	5.67	4.22	5.65	8.00	7.15	9.51	6.78	6.18	4.73	5.71	7.40

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.3
Instrumental Variable Ordered Probit Estimates

	1980-92	1981-91	1982-90	1983-89
Panel A: Females				
IV Ordered Probit Estimate	0.474**	0.644***	1.063***	0.626***
	(0.203)	(0.174)	(0.127)	(0.074)
Marginal Effects				
Very Unhappy	-0.022*	-0.032**	-0.078***	-0.029***
	(0.012)	(0.013)	(0.023)	(0.008)
Unhappy	-0.047**	-0.069***	-0.125***	-0.066***
	(0.022)	(0.022)	(0.017)	(0.008)
Neither Happy Nor Unhappy	-0.104***	-0.135***	-0.185***	-0.133***
	(0.04)	(0.029)	(0.007)	(0.012)
Happy	0.076**	0.106***	0.162***	0.099***
	(0.035)	(0.027)	(0.014)	(0.011)
Very Happy	0.097**	0.131***	0.226***	0.128***
	(0.04)	(0.036)	(0.031)	(0.015)
Observations	33,231	27,870	22,207	16,756
Panel B: Males				
IV Ordered Probit Estimate	-0.867**	-0.478	-0.778	-1.207***
	(0.412)	(0.452)	(0.5)	(0.421)
Marginal Effects				
Very Unhappy	0.035*	0.018	0.03	0.058*
	(0.021)	(0.016)	(0.022)	(0.033)
Unhappy	0.104**	0.063	0.097**	0.133**
	(0.041)	(0.051)	(0.048)	(0.032)
Neither Happy Nor Unhappy	0.166**	0.097	0.151*	0.207***
	(0.064)	(0.091)	(0.087)	(0.044)
Happy	-0.087***	-0.078*	-0.092***	-0.053
	(0.018)	(0.042)	(0.008)	(0.07)
Very Happy	-0.218	-0.1	-0.187	-0.347*
	(0.144)	(0.117)	(0.166)	(0.177)
Observations	22,222	18,639	14,685	11,090

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. First rows of each panel report IV Ordered Probit Estimates. Rows 2 to 5 in each panel indicate the categorical marginal effects. The cmp command in Stata used for all the estimations. For details see Roodman (2011). Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations

Table B.4
Robustness Checks for Instrumental Variable Estimates of Being Happy

	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	80-92	81-91	82-90	83-89	80-92	81-91	82-90	83-89
Panel A: Estimation with Coding Treatment = 0.25 for the 1986 Birth Cohort								
IV Estimates	0.279***	0.256***	0.401***	0.235***	-0.337	-0.279	-0.47	-0.869
Diploma	(0.067)	(0.084)	(0.056)	(0.054)	(0.432)	(0.448)	(0.614)	(0.627)
First Stage F-test	20.09	19.12	15.1	10.24	70.66	60.12	24.27	33.94
Observations	35,995	30,634	24,971	19,520	24,043	20,460	16,506	12,911
Panel B: Estimation with Controlling for Full Set of Age Dummies								
IV Estimates	0.255***	0.228***	0.357***	0.235***	-0.078	0.045	-0.201	-0.659
	(0.061)	(0.073)	(0.048)	(0.027)	(0.474)	(0.451)	(0.534)	(0.809)
First Stage F-test	40.09	33.98	25.61	14.62	133.1	128.2	36.92	9.495
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
Panel C: Estimation with Quadratic Birth Cohort Control Function								
IV Estimates	0.284***	0.249***	0.427***	0.289***	-0.269	-0.163	-0.298	-0.575
	(0.062)	(0.085)	(0.04)	(0.027)	(0.41)	(0.402)	(0.539)	(0.555)
First Stage F-test	19.10	21.81	18.84	14.69	54.51	41.74	20.29	20.69
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
Panel D: Estimation with TLSS Survey Years 2003 to 2014								
Middle School	0.257***	0.342***	0.623***	0.336**	-0.237	-0.392	-0.439	-0.598
Diploma	(0.099)	(0.117)	(0.175)	(0.164)	(0.313)	(0.323)	(0.401)	(0.585)
Observations	36,950	31,037	24,845	18,853	24,943	20,930	16,558	12,528
1st F-test	20.08	17.06	13.13	13.31	33.81	33.07	18.18	12.14
Panel E: Estimation with At Most Middle School Sample								
IV Estimates	0.418**	0.461**	0.580***	0.589***	-0.424***	-0.381**	-0.449***	-0.299
	(0.192)	(0.208)	(0.109)	(0.066)	(0.142)	(0.176)	(0.163)	(0.25)
First Stage F-test	12.49	14.44	13.74	9.955	56.31	47.78	23.15	64.33
Observations	19,847	16,516	13,034	9,859	9,383	7,770	6,002	4,535

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.5
Robustness Checks for Instrumental Variable Estimates of Composite Life Satisfaction Index

	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	80-92	81-91	82-90	83-89	80-92	81-91	82-90	83-89
Panel A: Estimation with Coding Treatment = 0.25 for the 1986 Birth Cohort								
Middle School	1.159**	1.103**	1.619***	1.394**	-0.822*	-0.839*	-1.534***	-2.897***
Diploma	(0.514)	(0.503)	(0.516)	(0.616)	(0.445)	(0.433)	(0.503)	(1.114)
Observations	35,995	30,634	24,971	19,520	24,043	20,460	16,506	12,911
First Stage F-test	23.61	25.27	21.01	15.51	56.76	45.98	23.49	11.69
Panel B: Estimation with Controlling for Full Set of Age Dummies								
Middle School	1.126**	1.139**	1.461***	1.046	-0.493	-0.527	-1.079*	-2.930*
Diploma	(0.496)	(0.502)	(0.535)	(0.685)	(0.488)	(0.479)	(0.609)	(1.525)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	40.09	33.98	25.61	14.62	133.1	128.2	36.92	9.495
Panel C: Estimation with Quadratic Birth Cohort Control Function								
Middle School	1.141**	1.099**	1.620***	1.402**	-0.828*	-0.841*	-1.532***	-2.881**
Diploma	(0.510)	(0.497)	(0.506)	(0.633)	(0.437)	(0.434)	(0.501)	(1.122)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	19.10	21.81	18.84	14.69	54.51	41.74	20.29	20.69
Panel D: Estimation with TLSS Survey Years 2003 to 2014								
Middle School	0.799***	0.717**	1.366***	0.927**	-0.210	-0.211	-0.532	-0.811
Diploma	(0.264)	(0.295)	(0.268)	(0.362)	(0.215)	(0.246)	(0.352)	(0.726)
Observations	36,942	31,029	24,837	18,847	24,940	20,927	16,555	12,527
1st F-test	19.94	17.03	13.11	13.25	32.91	32.23	18.05	11.83
Panel E: Estimation with At Most Middle School Sample								
Middle School	1.348*	1.300**	1.537**	1.620*	-0.991***	-1.207***	-1.884***	-2.040***
Diploma	(0.708)	(0.628)	(0.614)	(0.856)	(0.372)	(0.362)	(0.432)	(0.569)
Observations	19,847	16,516	13,034	9,859	9,383	7,770	6,002	4,535
First Stage F-test	13.49	15.63	13.94	9.786	48.93	40.02	19.37	70.04

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.6
Estimates Obtained from Samples Based on Age Limitations
Instrumental Variable Estimates

	(1) 18-34	(2) 19-33	(3) 20-32	(4) 21-31	(5) 22-30	(6) 23-29
Panel A: Female Sample						
Happy	0.236***	0.198***	0.301***	0.328***	0.466***	0.395***
	(0.074)	(0.069)	(0.063)	(0.059)	(0.124)	(0.104)
Observations	43,402	38,120	32,722	26,968	21,951	16,285
1st F-test	29.14	26.35	28.61	26.04	31.02	23.18
CLSI	0.790**	0.824**	0.940**	1.384***	1.762***	1.637**
	(0.379)	(0.416)	(0.429)	(0.486)	(0.596)	(0.657)
Observations	43,402	38,120	32,722	26,968	21,951	16,285
1st F-test	29.14	26.35	28.61	26.04	31.02	23.18
Panel B: Male Sample						
Happy	-0.174	-0.194	-0.243	-0.323	-0.281	-0.424
	(0.304)	(0.327)	(0.346)	(0.348)	(0.370)	(0.387)
Observations	29,127	25,377	21,502	17,956	14,682	10,885
1st F-test	67.73	62.96	57.37	50.80	52.75	64.52
CLSI	-0.456	-0.446	-0.387	-0.614	-0.912*	-0.986**
	(0.409)	(0.448)	(0.504)	(0.488)	(0.534)	(0.445)
Observations	29,127	25,377	21,502	17,956	14,682	10,885
1st F-test	67.73	62.96	57.37	50.80	52.75	64.52

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.7
Wild-Cluster Bootstrapping for Standard Errors

	80-92	81-91	82-90	83-89
Panel A. Female Sample				
Happy				
P-Values for IV	0.00	0.06	0.00	0.00
P-Values for RF	0.05	0.14	0.00	0.00
Composite Life Satisfaction Index				
P-Values for IV	0.09	0.10	0.00	0.00
P-Values for RF	0.09	0.10	0.05	0.36
Panel B. Male Sample				
Happy				
P-Values for IV	0.71	0.80	0.66	0.61
P-Values for RF	0.72	0.80	0.68	0.60
Composite Life Satisfaction Index				
P-Values for IV	0.34	0.41	0.17	0.27
P-Values for RF	0.36	0.42	0.18	0.06

Notes: Each entry represents the associated p-values from the reduced form and instrumental variable regressions. Rademacher weights are used in the analysis with 1999 repetitions. All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Survey weights are used in all estimations.

Table B.8
The Impact of Having at Least a Middle School Degree on Being Happy and Domains of Life Satisfaction
Instrumental Variable Estimates: Subsample Heterogeneity by Urban versus Rural Residency Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Happy	Satisfied With Earnings	Satisfied With HH Income	Satisfied With Marriage	Satisfied With Health	Satisfied With Friends	Satisfied With Job	Satisfied With Neighborhood	Satisfied With Housing	Satisfied With Family Rel.	Composite LS Index
Panel A: Females											
Urban											
Middle School Diploma	0.282	1.741**	0.456	0.269*	0.301	0.345	0.818	0.505	0.824**	0.055	1.665*
	(0.274)	(0.792)	(0.568)	(0.144)	(0.282)	(0.301)	(1.104)	(0.425)	(0.340)	(0.152)	(0.893)
Observations	3,065	772	3,065	1,921	3,065	3,065	799	3,065	3,065	3,065	3,065
1st F-Test	8.606	10.49	8.606	10.01	8.606	8.606	10.91	8.606	8.606	8.606	8.606
Rural											
Middle School Diploma	0.873	0.416	-0.083	1.624	0.818	0.515	0.545	0.505	-0.262	1.217	1.971
	(1.019)	(0.445)	(0.445)	(2.930)	(0.903)	(0.516)	(0.553)	(0.944)	(0.307)	(1.074)	(2.014)
Observations	952	154	952	602	952	952	279	952	952	952	952
1st F-Test	2.349	15.45	2.349	0.299	2.349	2.349	7.319	2.349	2.349	2.349	2.349
Panel B: Males											
Urban											
Middle School Diploma	-0.040	-0.901***	-0.431	-0.436***	-0.451**	-0.328**	0.142	-0.339***	-0.544***	0.398	-0.862
	(0.424)	(0.275)	(0.317)	(0.167)	(0.190)	(0.131)	(0.189)	(0.114)	(0.173)	(0.274)	(0.793)
Observations	2,243	1,619	2,243	891	2,243	2,243	1,656	2,243	2,243	2,243	2,243
1st F-Test	45.62	44.96	45.62	12.16	45.62	45.62	51.40	45.62	45.62	45.62	45.62
Rural											
Middle School Diploma	-3.344	0.774	-0.316	-0.363	0.872	-1.634	2.102	1.044	-2.271	1.075	-3.606
	(3.634)	(4.273)	(0.761)	(0.222)	(1.294)	(1.958)	(5.848)	(1.805)	(3.189)	(1.737)	(5.700)
Observations	620	382	620	256	620	620	450	620	620	620	620
1st F-Test	0.572	0.134	0.572	0.754	0.572	0.572	0.163	0.572	0.572	0.572	0.572

Notes: Data is from 2009-2012 rounds of the TLSS. All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.9

Instrumental Variables Estimates Controlling for Potential Mediators At Most Middle School Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Females								
	Estimates of Happy							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School Diploma	0.42**	0.42**	0.38**	0.42**	0.41**	0.42**	0.41**	0.35*
	(0.19)	(0.20)	(0.19)	(0.20)	(0.18)	(0.20)	(0.19)	(0.18)
First Stage F-test	12.49	12.9	12.5	12.5	12.21	13.46	13.17	12.76
Observations	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]
	Estimates of Composite Life Satisfaction Indicator							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School Diploma	1.39*	1.35*	1.25*	1.39*	1.34*	1.40*	1.37*	1.22*
	(0.73)	(0.76)	(0.76)	(0.73)	(0.70)	(0.76)	(0.70)	(0.73)
First Stage F-test	12.49	12.9	12.15	12.5	12.21	13.46	13.17	12.76
Observations	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]	[19,847]
Panel B: Males								
	Estimates of Happy							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School Diploma	-0.42***	-0.42***	-0.46***	-0.43***	-0.26*	-0.42***	-0.41***	-0.29***
	(0.14)	(0.13)	(0.13)	(0.16)	(0.15)	(0.14)	(0.14)	(0.11)
First Stage F-test	56.31	47.79	48.4	48.41	56.8	51.75	55.47	58.3
Observations	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]
	Estimates of Composite Life Satisfaction Indicator							
	Baseline Estimate	Controlling for Potential Mediators						
Middle School Diploma	-1.02***	-1.02***	-1.08***	-0.96***	-0.53*	-1.01**	-1.05***	-0.74*
	(0.39)	(0.38)	(0.39)	(0.36)	(0.31)	(0.41)	(0.38)	(0.38)
First Stage F-test	56.31	47.79	48.4	48.41	56.8	51.75	55.47	58.3
Observations	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]	[9,383]
Controls For								
Standardized Household Income		Yes	No	No	No	No	No	No
Can Make Ends Meet		No	Yes	No	No	No	No	No
Married		No	No	Yes	No	No	No	No
Standardized Subjective Econ. Ladder		No	No	No	Yes	No	No	No
Expects to be Better off Next Year		No	No	No	No	Yes	No	No
Labor Force Participation		No	No	No	No	No	Yes	No
Hopeful		No	No	No	No	No	No	Yes

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.10
The Impact of Having at Least a Middle School Degree on Domains of Life Satisfaction
Instrumental Variable Estimates Using the Full Set of Data Interval Windows

	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	80-92	81-91	82-90	83-89	80-92	81-91	82-90	83-89
Panel A: Satisfied with Earnings								
Middle School	0.89***	0.66***	0.90***	0.43***	-0.49*	-0.55*	-0.59***	-1.01***
Diploma	(0.29)	(0.15)	(0.35)	(0.09)	(0.26)	(0.29)	(0.21)	(0.22)
Observations	7,356	6,248	5,091	3,877	16,299	13,824	11,084	8,504
First Stage F-test	8.756	15.8	9.198	744.7	47.37	52.17	53.61	12.37
Panel B: Satisfied with Household Income								
Middle School	0.20	0.12	0.17	0.03	-0.30	-0.47***	-0.48***	-0.63**
Diploma	(0.34)	(0.31)	(0.32)	(0.26)	(0.23)	(0.16)	(0.18)	(0.29)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel C: Satisfied with Marriage								
Middle School	0.27**	0.31***	0.33***	0.35***	-0.21***	-0.25***	-0.30***	-0.43***
Diploma	(0.11)	(0.10)	(0.10)	(0.11)	(0.06)	(0.06)	(0.07)	(0.04)
Observations	23,954	20,258	16,364	12,571	11,327	9,324	7,317	5,573
First Stage F-test	18.4	22.5	18.63	14.49	73.02	82.8	161.4	34.43
Panel D: Satisfied with Health								
Middle School	0.29**	0.24*	0.37**	0.35**	-0.28**	-0.35***	-0.51***	-0.77***
Diploma	(0.14)	(0.14)	(0.15)	(0.18)	(0.12)	(0.11)	(0.12)	(0.28)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel E: Satisfied with Friends								
Middle School	0.23	0.15	0.29*	0.37**	-0.33***	-0.29***	-0.42***	-0.67***
Diploma	(0.16)	(0.16)	(0.16)	(0.14)	(0.08)	(0.09)	(0.09)	(0.14)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel F: Satisfied with Job								
Middle School	0.58**	0.27	0.54***	0.49***	0.18*	0.10	-0.10	-0.25
Diploma	(0.25)	(0.19)	(0.13)	(0.15)	(0.11)	(0.10)	(0.11)	(0.18)
Observations	7,852	6,655	5,416	4,113	16,500	13,998	11,218	8,593
First Stage F-test	15.07	34.96	17.98	110.2	48.95	48.95	29.41	11.55

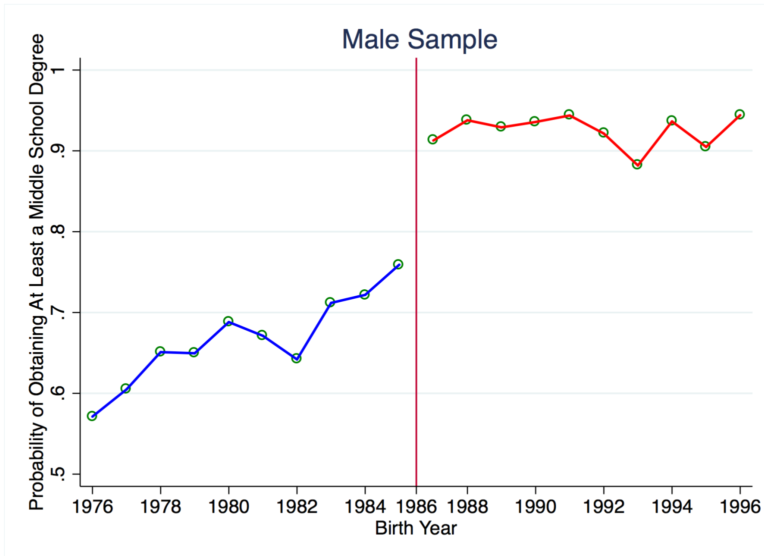
Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

Table B.10 Continued
The Impact of Having at Least a Middle School Degree on Domains of Life Satisfaction
Instrumental Variable Estimates Using the Full Set of Data Interval Windows

	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	80-92	81-91	82-90	83-89	80-92	81-91	82-90	83-89
Panel G: Satisfied with Neighborhood								
Middle School	0.327	0.366	0.558**	0.539*	-0.087	-0.107	-0.244***	-0.451**
Diploma	(0.259)	(0.236)	(0.248)	(0.283)	(0.138)	(0.081)	(0.087)	(0.18)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel H: Satisfied with Housing								
Middle School	0.382***	0.327***	0.382***	0.350***	-0.512***	-0.512***	-0.597***	-0.598***
Diploma	(0.106)	(0.097)	(0.096)	(0.109)	(0.129)	(0.155)	(0.157)	(0.168)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel I: Satisfied with Family Relations								
Middle School	0.225**	0.190*	0.287***	0.220**	0.319**	0.128	0.262	-0.089
Diploma	(0.097)	(0.099)	(0.107)	(0.11)	(0.143)	(0.084)	(0.174)	(0.131)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6
Panel J: Composite Life Satisfaction Index								
Middle School	1.148**	1.099**	1.621***	1.397**	-0.830*	-0.841*	-1.532***	-2.890**
Diploma	(0.511)	(0.499)	(0.516)	(0.628)	(0.442)	(0.433)	(0.505)	(1.147)
Observations	33,231	27,870	22,207	16,756	22,222	18,639	14,685	11,090
First Stage F-test	22.83	24.4	20.14	14.72	62.59	49.96	25.32	19.6

Notes: All models control for re-centered birth year differentiated by exposure to the education reform status on each side of the pivotal birth cohort and survey year fixed effects. Standard errors corrected for clustering on the birth cohort are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Survey weights are used in all estimations.

APPENDIX C: SUPPLEMENTARY FIGURES AND TABLES FOR CHAPTER 4



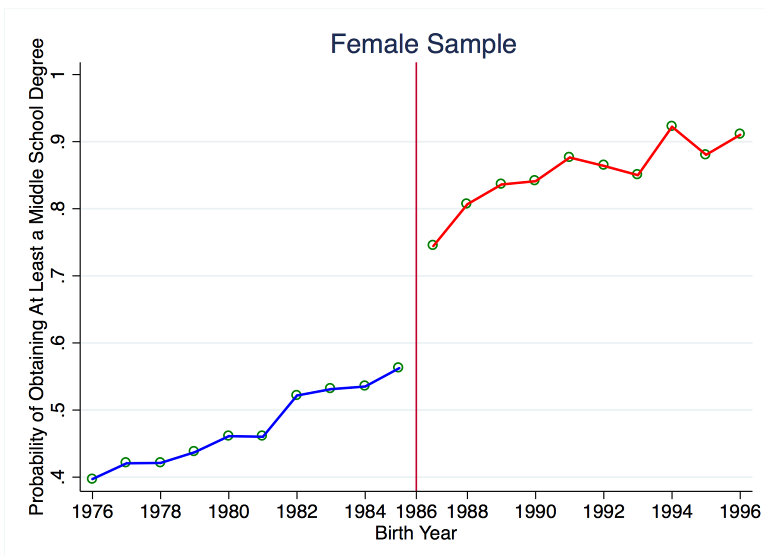


Figure C.1
The Impact of Reform on Obtaining at Least a Middle School Degree-Long Run

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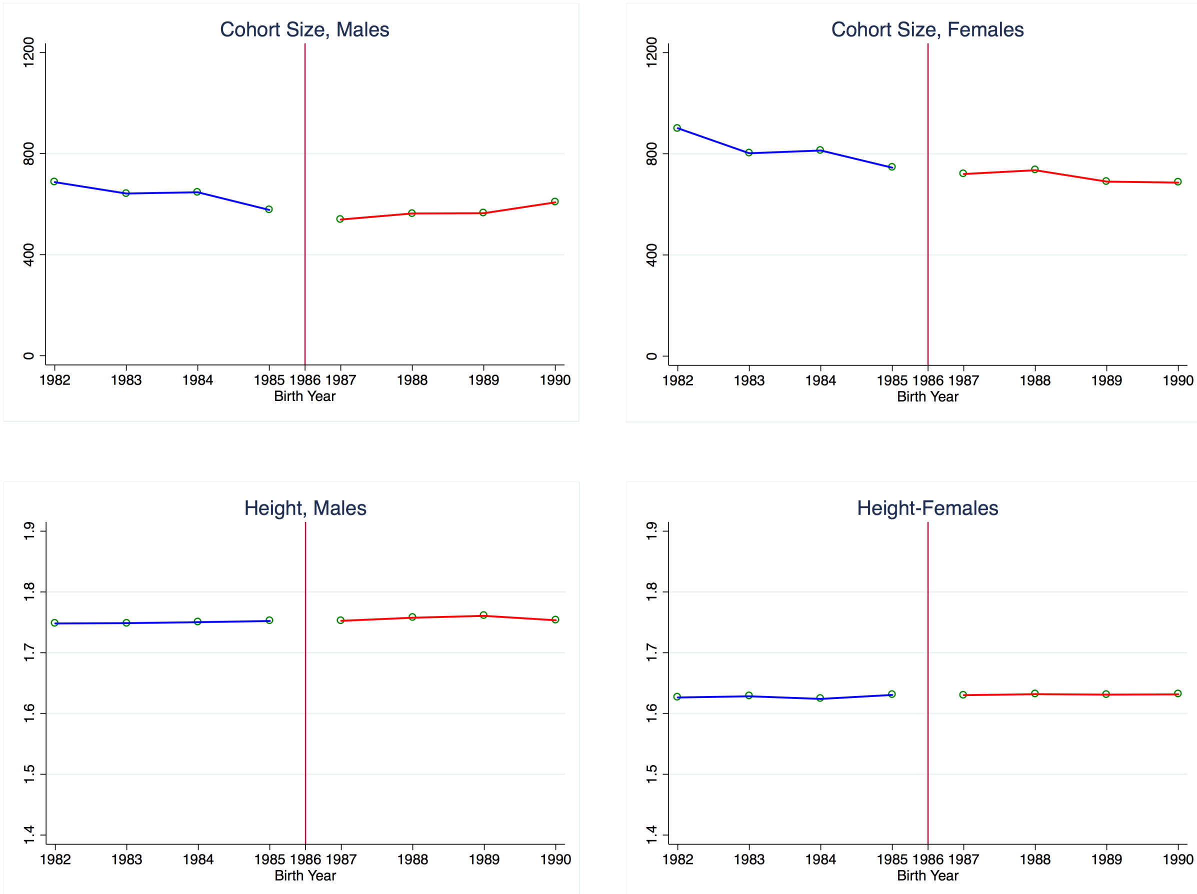

Figure C.2
Balanced Covariates

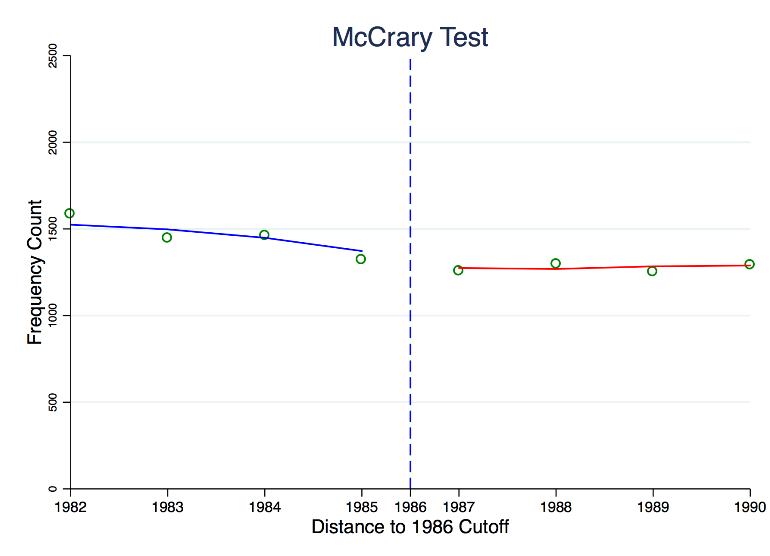

Figure C.3
McCrary Test

Table C.1

Studies Using Causal Identification Methods in Developing Countries

Study	Country	Outcomes	Identification	Results	Gender Specific
Panel A: The Impact of Education on Mental Health, Chronic Disease, Self-Reported Health, Obesity, and Smoking in Developing Countries					
Behrman et al. (2015)	China	Mental Health, Smoking Chronic Diseases	Monozygotic twins FE	(+) Mental Health (-) Smoking	Not gender Specific
Huang (2015)	China	Perceived Health Underweight Smoking	Compulsory Schooling Reform	(-) Poor Health (-) Underweight, Smoking (+) Cognitive abilities.	Not Gender Specific
Jensen and Lleras-Muney (2012)	Dominican Republic	Smoking, Drinking	Randomized intervention, which provides information on returns to education.	(-) Smoking at age 18 (-) Onset of daily or regular drinking	Men
Xie and Mo (2014)	China	Perceived Health, Anthropometric Health Smoking, Overweight	Compulsory Schooling Reform	No effect on self-reported health and anthropometric measures	Gender Specific
Panel B: The Impact of Education on Risky Sexual Behaviors in Sub-Saharan Africa					
Aguero and Bharadwaj (2014)	Zimbabwe	HIV Knowledge	Education Reform	(+) Knowledge of HIV	Women only
Alsan and Cutler (2013)	Uganda	HIV risk	Distance to school as instrument for enrollment.	(-) HIV risk by it's effect on virginity	Women only
Behrman (2015)	Malawi and Uganda	HIV status	Exposure to Universal Primary Education Policies (UPE).	(-) HIV risk	Women only
Cannonier and Mocan (2012)	Sierra Leone	Modern Contraceptives, Test for AIDS	Free Primary education program (FPE)	(-) Number of desired children (+) HIV Testing (+) Contraceptive usage	Women only
De Neve et al. (2015)	Botswana	HIV Status	Exposure to the grade structure reform	(-) HIV risk .	Gender Specific
Duflo et al. (2015)	Kenya	STI Prevalence, Fertility	Randomized interventions: Education Subsidies, and HIV curriculum	Neither the Education subsidies, nor the HIV curriculum have effects on STI prevalence alone	Gender Specific

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Table C.2
Optimal Bandwidths

	(1)	(2)
Dependent Variables	Males	Females
Good Health	3	3
Excellent Health	3	3
BMI	3	3
Underweight	3	3
Overweight/Obese	3	4
Obese	2	3
Ever Smoked	4	2
Current Smoker	4	3
Ever Smoked Regularly	4	3
Daily Fruit Consumption	3	3
Daily Vegetable consumption	3	3
Flu-Shot Ever	3	3

Optimal bandwith selection procedure carried out with Stata's build in command rdbwselect, which is introduced by Calonico et al. 2016a. For technical details please see: Callaneo 2014; 2016a, b.

Table C.3
Instrumental Variable Estimates of the Impact of Education on Health Outcomes and Health Behaviors-Using Alternative Bandwidths

	(1) 4 YEARS	(2) 3 YEARS	(3) 2 YEARS	(4) 4 YEARS	(5) 3 YEARS	(6) 2 YEARS
		Males			Females	
A. The Impact of Education on Self-Reported Health						
Good Health	0.210	0.254	0.567	-0.010	0.199	0.359
	(0.198)	(0.213)	(0.452)	(0.113)	(0.155)	(0.346)
	[0.866]	[0.865]	[0.859]	[0.804]	[0.802]	[0.800]
Excellent Health	0.251	0.294	0.802	0.113	0.332***	0.594**
	(0.287)	(0.290)	(0.56)	(0.074)	(0.094)	(0.249)
	[0.221]	[0.224]	[0.218]	[0.156]	[0.158]	[0.159]
First Stage F-test	29.962	29.399	7.714	39.498	21.493	5.396
Observations	4,825	3,531	2,325	6,091	4,504	3,012
B. The Impact of Education on Body Weight						
BMI	5.642***	7.311***	10.354***	-0.013	0.406	-1.527
	(1.898)	(1.946)	(3.966)	(1.360)	(1.575)	(1.84)
	[24.313]	[24.381]	[24.317]	[23.294]	[23.274]	[23.246]
Underweight	-0.155**	-0.109*	-0.07	-0.184*	-0.365**	-0.229
	(0.073)	(0.057)	(0.113)	(0.101)	(0.145)	(0.141)
	[0.029]	[0.027]	[0.026]	[0.087]	[0.085]	[0.083]
Overweight/Obese	0.420**	0.655***	0.749***	-0.232	-0.348*	-0.283
	(0.202)	(0.175)	(0.211)	(0.147)	(0.197)	(0.208)
	[0.371]	[0.378]	[0.367]	[0.281]	[0.277]	[0.268]
Obese	0.319**	0.400***	0.983**	0.060	0.098	-0.042
	(0.140)	(0.140)	(0.39)	(0.072)	(0.089)	(0.117)
	[0.064]	[0.067]	[0.064]	[0.074]	[0.073]	[0.074]
First Stage F-test	31.480	34.838	9.544	39.016	19.165	20.951
Observations	4,678	3,430	2,267	5,613	4,165	2,786
C. The Impact of Education on Smoking						
Ever Smoked	-0.301	-0.508**	-0.588**	0.274***	0.440***	0.422**
	(0.248)	(0.231)	(0.293)	(0.104)	(0.112)	(0.175)
	[0.637]	[0.633]	[0.639]	[0.309]	[0.313]	[0.31]
Current Smoker	-0.077	-0.481**	-0.719**	0.217	0.286*	0.133
	(0.260)	(0.233)	(0.317)	(0.137)	(0.164)	(0.303)
	[0.528]	[0.522]	[0.523]	[0.199]	[0.201]	[0.203]
Ever Smoked Regularly	-0.169	-0.347	0.056	0.218**	0.279***	0.12
	(0.287)	(0.264)	(0.401)	(0.096)	(0.107)	(0.165)
	[0.555]	[0.552]	[0.549]	[0.207]	[0.208]	[0.204]
First Stage F-test	26.410	37.088	14.358	56.555	44.822	14.507
Observations	3,861	2,870	1,921	4,800	3,578	2,408

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

Table C.4
OLS Estimates Controlling for Potential Mediators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Good Health	Excellent Health	BMI	Underweight	Overweight/ Obese	Obese	Ever Smoked	Current Smoker	Ever Smoked Reg.
Panel A: Males									
Middle School	0.118***	0.063***	-0.114	0.01	0.001	0.016	-0.056**	-0.093***	-0.077***
Diploma	(0.022)	(0.017)	(0.196)	(0.006)	(0.025)	(0.013)	(0.025)	(0.028)	(0.022)
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
Controlling for Potential Mediators									
Middle School	0.089***	0.036*	-0.252	0.009	-0.019	0.018	-0.026	-0.057*	-0.045*
Diploma	(0.022)	(0.019)	(0.200)	(0.007)	(0.026)	(0.015)	(0.027)	(0.031)	(0.024)
Observations	3,531	3,531	3,430	3,430	3,430	3,430	2,870	2,870	2,870
Mean	[0.865]	[0.224]	[24.381]	[0.027]	[0.378]	[0.067]	[0.633]	[0.522]	[0.552]
Panel B: Females									
Middle School	0.108***	0.045***	-1.141***	0.035***	-0.123***	-0.037***	0.047***	0.028*	0.038***
Diploma	(0.014)	(0.013)	(0.161)	(0.009)	(0.016)	(0.01)	(0.015)	(0.016)	(0.014)
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]
Controlling for Potential Mediators									
Middle School	0.077***	0.017	-0.399**	0.021**	-0.060***	-0.016	0.020	0.009	0.014
Diploma	(0.016)	(0.014)	(0.170)	(0.010)	(0.018)	(0.010)	(0.017)	(0.017)	(0.013)
Observations	4,504	4,504	4,165	4,165	4,165	4,165	3,578	3,578	3,578
Mean	[0.802]	[0.158]	[23.274]	[0.085]	[0.277]	[0.073]	[0.313]	[0.201]	[0.208]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

Table C.5
The Univariate Associations between Middle School Diploma and Health Behaviors in Terms of Body Weight and Smoking

VARIABLES	(1) BMI	(2) Underweight	(3) Overweight/ Obese	(4) Obese	(5) Ever Smoked	(6) Current Smoker	(7) Ever Smoked Regularly
			Panel A: Males				
Middle School Diploma	0.242**	-0.007***	0.044***	-0.014	-0.065***	-0.092***	-0.077***
	(0.110)	(0.002)	(0.013)	(0.010)	(0.011)	(0.014)	(0.014)
Observations	5,956	5,956	5,956	5,956	4,900	4,900	4,900
			Panel B: Females				
Middle School Diploma	-2.153***	0.011**	-0.204***	-0.126***	0.123***	0.088***	0.115***
	(0.117)	(0.004)	(0.013)	(0.008)	(0.013)	(0.011)	(0.010)
Observations	7,139	7,139	7,139	7,139	6,243	6,243	6,243

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table C.6

Instrumental Variable Estimates of the Impact of Education on Computer Use and Social Media Participation

	Daily Computer Use	Daily Internet Use	Social Media Participation
	Males		
Middle School	0.454*	0.558***	0.427**
Diploma	(0.233)	(0.214)	(0.214)
First Stage F-test	42.338	42.338	42.338
Observations	11,814	11,814	11,814
Mean	[0.504]	[0.559]	[0.583]
	Females		
Middle School	0.044	0.058	0.076
Diploma	(0.272)	(0.262)	(0.281)
First Stage F-test	15.999	15.999	15.999
Observations	13,080	13,080	13,080
Mean	[0.356]	[0.388]	[0.372]

Robust standard errors, corrected for clustering on region by year-of-birth, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control year of birth differentiated by exposure to the education reform status, survey year fixed effects, region of residence fixed effects, and region specific birth year trends.

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VITA

Ömer Bahadır Dursun was born in Artvin, Turkey. He obtained his Bachelor's of Arts degree in Business Administration and Finance from the Marmara University, Istanbul, Turkey in 2008. Later, he obtained M.S degrees in Economics from Georgia Institute of Technology in 2010, and University of Mannheim, 2014.

Bahadır is expected to receive the degree of Doctor of Philosophy in Economics at Louisiana State University in August 2017. His advisor during his doctoral studies was Dr. Naci Mocan. He is the recipient of the 2016 Excellence in Teaching Award in the Graduate Student Category from the Department of Economics at LSU, and 2017 Outstanding Graduate Teaching Assistant Award in E. J. Ourso College of Business at LSU. His research interests are Health Economics, Labor Economics.