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IZA DP No. 12332

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ABSTRACT

The Effect of Education on Health: Evidence from the 1997 Compulsory Schooling Reform in Turkey

This paper analyzes the relationship between education and health outcomes using a natural experiment in Turkey. The compulsory schooling increased from 5 to 8 years in 1997. This increase was accompanied by a massive construction of classrooms and recruitment of teachers in a differential rate across regions. As in previous studies, we confirm that the 1997 reform substantially increased education in Turkey. Using the number of new middle school class openings per 1000 children as an intensity measure for the 1997 reform, we find that, on average, one additional middle school class increases the probability of completion of 8 years or more of schooling by about 7.1 percentage points. We use this exogenous increase in the educational attainment to investigate the impact of education on body mass index, obesity, smoking behavior, and self-rated health, as well as the effect of maternal education on the infant's well-being. Using ordinary least squares, we find that there is a statistically significant favorable effect of education on health outcomes and behavior. However, this relationship becomes insignificant when we account for the endogeneity of education and health by instrumenting education with exogenous variations generated by the 1997 reform and the accompanying middle school class openings. The insignificance of the health effect may be due to lack of statistical power in our data, or to the fact that this policy affects only relatively low levels of schooling and the health effects of education need to be examined at higher levels of schooling.

JEL Classification: C26, I12, I21, I28

Keywords: health, education, compulsory schooling, body mass index, obesity, smoking, selfrated health, maternal education, infant's well-being, Turkey

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The Effect of Education on Health: Evidence from the 1997 Compulsory Schooling Reform in Turkey

1. Introduction

The causal relationship between the level of education of an individual and his or her subsequent health is an important topic in economics. One reason for its importance is that, if a strong positive causal relationship between education and health exists, then educational policies have the potential to also address health challenges experienced by the population. Indeed, observers have suggested that policies aimed at increasing education have the potential to generate considerable health payoffs that may rival or exceed those of policies aimed at improving health care (e.g., Deaton, 2002).

Grossman (1972) formalizes a model of the demand for health based on the theory of allocation of time by Becker (1965). According to this model, more schooling allows individuals to produce better health, as individuals with more education accumulate more knowledge. In fact, it is argued that schooling would have a direct effect on health through either increasing efficiency of inputs in the process of "health production" (termed as "productive efficiency effect") or choosing a better input mix that leads to a better health outcome (termed as "allocative efficiency effect"). A third mechanism via which formal schooling may affect health is changing time preference of individuals (see Grossman, 2006, 2015 for a comprehensive review). In fact, Becker and Mulligan (1997) point out that schooling lowers the discount rate for the future. That is to say, an increase in schooling makes an individual more future-oriented which in turn affects this individuals' investment in better health. Alternatively, Heckman (2007) proposes a model in which early-life interventions—such as education—affecting developmental factors, can affect the stock of individual health and in turn generate larger health returns later in life.[1] Examples of such developmental factors are cognitive and non-cognitive skills (e.g., personality traits). This alternative model provides a competing rationale to the model of Grossman for why increases in schooling may causally result in health effects (e.g., through cognitive skills or improved personality traits). In our study, we employ a reduced-form instrumental variables approach based on a natural experiment in Turkey to examine the causal effect of education on health. Unfortunately, the data available to us does not allow parsing out the relative importance of factors stemming from competing explanations for the potential relationship between education and health.

Empirically, the endogeneity between education and health at the individual level poses challenges in the estimation of the potential health-improving direct effect of schooling. For this reason, the empirical evidence on the relationship between education and health is highly debated in the literature, and offers mixed conclusions (e.g., Grossman, 2006, 2015). The majority of the evidence on the causal relationship between education and health has been produced for developed countries (e.g., Currie and Moretti, 2003; Cutler and Lleras-Muney, 2006; Grossman, 2015), with the corresponding evidence for developing countries lagging. Some notable exceptions are Breierova and

[1] In particular, the model in Heckman (2007) allows for skills acquired early in life to augment the production of skills at later stages in life. It also allows for the acquired skills to raise the productivity of investment in skills later in life. These two features imply that early-life acquisition of skills is more impactful relative to those acquired later in life.

Duflo (2004) who analyze Indonesia, Osili and Long (2008) who analyze Nigeria, Chou et al. (2010) who analyze Taiwan, and Dinçer et al. (2014), Güneş (2015) and Cesur et al. (2014) who analyze Turkey.

This paper contributes to the growing evidence on the causal effect of education on health for developing countries by focusing on Turkey. This large Middle-Eastern country faces various public health problems. For example, consider obesity and cigarette smoking. According to the 2014 OECD health statistics, 24.5% of the Turkish female population is obese. This places Turkey in second place after the USA among OECD countries in female obesity. In contrast, the male obesity rate is below the OECD average. The same OECD statistics report that 41.8% of Turkish men are daily smokers—the highest rate among OECD countries. The smoking rate for females is 13.1%—one of the lowest among the same group of countries. Overall, the obesity rate increased from 12% in 2003 to 19.9% in 2014, while the population smoking rate declined from 43.6% in 1995 to 27.3% in 2014 (OECD, 2017). In principle, given the low average educational attainment in Turkey, there would seem to be room for improving public health via interventions in schooling.

To address the problem of endogeneity between education and health, we use an instrumental variables approach in the context of a natural experiment. The Turkish government increased compulsory schooling from 5 to 8 years in 1997. This mainly politically-motivated compulsory schooling law was enacted to help Turkey in its negotiation for European Union membership, see Dulger (2004). The timing and implementation of the 1997 Basic Education Law provide a natural experiment to investigate the effect of education on health outcomes. There was another increase in compulsory education from 8 to 12 years in 2012, but it is too early to investigate the effect of the 2012 reform.

Changes in compulsory schooling are widely used as instruments in an attempt to circumvent the endogeneity of schooling or to control for time preference and other omitted variables problems in the estimation of a wide range of health outcomes (Grossman, 2006). Health outcomes considered include mortality (e.g., Lleras-Muney, 2005; Mazumder, 2008; Van Kippersluis et al., 2011), self-rated health (e.g., Adams, 2002; Clark and Royer, 2013), the prevalence of illnesses (e.g., Kemptner et al., 2011), fertility (e.g., Breierova and Duflo, 2004; Dinçer et al., 2014), and health risk factors; the prevalence of obesity and smoking (e.g., Kenkel et al., 2006; de Walque, 2007; Jurges et al., 2011; Cesur et al., 2014). Also, infant birth outcomes, such as birth weight, infant mortality, etc. (e.g., Currie and Moretti, 2003; Chou et al., 2010; Breierova and Duflo, 2004; Dinçer et al., 2014) are used to study the relationship between maternal education and infant health.

At the same time, instrumental variables estimates present mixed evidence based on the country being studied and the instrument employed, which in most cases is based on policy interventions affecting educational attainment. Estimates using US data with instruments based on higher levels of education find significant positive effects of education on health outcomes (e.g., Lleras-Muney, 2005; Kenkel et al., 2006; Currie and Moretti, 2003; Adams, 2002; de Walque, 2007). However, studies that estimate the effect of schooling on own-health using an instrument based on interventions targeting the lower end of the education distribution find either small or insignificant impacts of schooling on health. For example, Clark and Royer (2013) for the UK, Arendt (2005) for Denmark, Kemptner et al. (2011) for Germany, and Park and Kang (2008) for South Korea.

The compulsory schooling changes that have occurred in Turkey have recently been used to estimate the effect of education on own-health. Cesur et al. (2014) use the 1997 Basic Education Law as an instrument for schooling to estimate the effect of education on self-rated health, BMI, obesity, and smoking behavior. They use a treatment dummy for the law change as an instrument and find that an increase in schooling increases men's BMI and the probability of being obese while it has no statistically significant effect on women's BMI and the probability of being obese or overweight. They also find no statistically significant effect of education on self-rated health and smoking behavior. Tansel and Karaoglan (2016) use some changes in the education system which mainly affected primary school (5-year) education in the early 1960's by comparing cohorts who are affected by the new system with those that were not. They find that there were no statistically significant effects of education on the probability of smoking and exercising. Their IV results show that one additional year of schooling increased BMI by 0.26 kg/m^2. This was statistically significant only at the 10 percent level. However, their identification strategy was based on a comparison of two very different cohorts. The treatment group contained individuals born between 1952 and 1987, while the control group included all individuals born before 1952.[2]

Besides the impact of education on own-health, the effect of maternal education on the infant's well-being has been examined using natural experiments. For example, Currie and Moretti (2003) use college availability in the county of residence of the mothers when they were 17 years of age as an instrument for mother's schooling. They find that increasing maternal schooling statistically significantly reduces the probability of low-birthweight and preterm delivery. Moreover, Breierova and Duflo (2004) and Chou et al. (2010) use massive school constructions in Indonesia and Taiwan, respectively, and find that better parental education causes better health of infants, especially in terms of birth weight.

The effect of maternal education on infants' health has also been examined in Turkey using the 1997 Basic education law change and its regional variation by Dinçer et. al. (2014) and Güneş (2015). Dinçer et al. (2014) use the 1997 compulsory schooling change and the accompanying variation in teacher recruitment across regions over birth cohorts to estimate the effect of female's education on her fertility and attitude toward gender equality. They used the Turkey Demographic and Health Survey (TDHS) of 2003 and 2008. They instrumented female's education by the intensity of the reform which was defined as the number of teachers (1st-8th grades) as a proportion of the number of primary education school children aged (6-13 years). Their IV results showed that there is a statistically significant negative relationship between education and the number of pregnancies and the average number of children to ever-married women. In fact, they find that a 10-percentage point increase in the proportion of women that completed at least 8 years of schooling is associated with 0.13-0.16 less children per woman. They also find weak evidence on the impact of mother's education on child mortality, and no evidence that it changed attitudes toward gender inequality.[3] Güneş (2015) estimates the effect of maternal education on fertility and infant's health

[2] As an informal check of Tansel and Karaoglan's (2016) identification strategy, we used a different cut-off birth year chosen randomly before 1952. This gave similar and even better strength and power in the first stage. This suggests that their identification strategy is not robust and may not be valid in the estimation of the effect of education on health in Turkey.

[3] The identification in the paper is based on cohorts that are affected by the reform and the intensity of the treatment, which is defined as the teacher-child ratio. They define the first cohort affected by the law as the 1986 birth cohort. However, the school starting age is 6 in Turkey (Resmi Gazete, August 7, 1992). Thus, the 1986 birth cohort is expected to complete 5 years of primary school in June 1997 which is right before the law change. Hence, this cohort should not be affected by the law. Moreover, a small portion of the 1986 birth cohort that deferred entrance to the primary school or repeated any grade in primary school and consequently may be in the 5th grade in 1997/1998 will be affected by the law. Since there is no way to distinguish between treated and

using the 1997 compulsory schooling law exposure and regional variation in the additional classrooms constructed. She uses additional classrooms built between the 1996/1997 and 1997/1998 school years to construct an intensity measure for the exposure to the reform. The main data used were the TDHS of 2008. She finds that the mothers' completion of 8 years of schooling improved infant's health, reduced fertility, reduced smoking, and increased the age at first birth.

Along with the 1997 Basic Education Law, we exploit the accompanying increase in the number of classes offered in grades 6th through 8th that vary across regions and birth cohorts as a measure of treatment intensity. Our health outcomes are BMI, obesity, smoking behavior, and self-rated health. We also investigate the effect of women's education on their infant's well-being. The data we employ comes from two sources that offer different advantages: the 2008 and 2013 Turkey Demographic and Health Survey (TDHS) and from several years of the Turkey Health Survey (THS).

This paper has a number of distinctive features relative to previous studies focusing on Turkey. First, the effect of schooling on own-health outcomes is estimated by employing an instrumental variables method based on the intensity of the 1997 Basic education law, instead of using a treatment indicator as an instrument (e.g., Cesur et.al., 2014). Second, we estimate effects on BMI and the propensities to be obese or overweight for females using measured BMI, instead of using self-reported height and weight (Cesur et al., 2014; Tansel and Karaoglan, 2016) which can be reported with error. Third, we estimate the effect of maternal education on infants' well-being using a refined measure of intensity of the 1997 reform as an instrument (which we discuss in the next section). We analyze those outcomes measuring the intensity of the reform using the expansion in the number of classes only in the affected grade levels (grades 6th through 8th) instead of the expansion in the number of teachers (Dinçer et al., 2014) or the expansion in the constructions of classrooms (Güneş, 2015) for the grades 1st through 8th. Fourth, we use the most recent TDHS data from 2008 and 2013 to estimate the effect of maternal education on infants' health, while Dinçer et al. (2014) and Güneş (2015) employ TDHS data from 2003 and 2008 and from 2008 only, respectively. This data difference is likely important as in 2008 the affected group is still young such that they may not have completed their education nor have children yet. Lastly, our analysis combines the strengths of the TDHS with those of the THS. We discuss their relative advantages in the next section.

We show that the 1997 reform substantially increased education in Turkey, in line with the previous literature. Using the new class openings in the 6th through 8th grades as an intensity measure of the 1997 reform we find that, on average, one additional middle-school class increased the probability of completion of 8 years or more of schooling by 7.1 percentage points. We also find that the new class openings were more effective in increasing the schooling of women relative to men. Using ordinary least squares, we find a statistically significant effect of education on health outcomes/health behavior in Turkey. This does not account for the endogeneity between education and health. When we account for this endogeneity using an instrumental variables approach (based on the natural experiment provided by the 1997 Basic Education Law and its implementation), we find no statistically significant relationship between higher educational attainment and BMI, obesity, smoking behavior, and self-rated health. This result holds for both females and males. We

non-treated individuals in the 1986 cohort, inclusion of the 1986 cohort in the treated group might be problematic. Cesur et al. (2014) also noted this fact and excluded the 1986 cohort from their regressions.

also find no statistically significant effect of maternal education on infant well-being. We note that our IV results represent a local average treatment effect (LATE) which reflects effects for individuals whose education would be changed by the 1997 reform (mainly completion of middle school). This LATE may not reflect the effects of an increase in education on health in other segments of the educational distribution. We conjecture that plausible reasons for the statistically insignificant effect of education on own-health or infant well-being that we find are that this policy affects only relatively low levels of schooling and this question needs to be examined at higher levels of schooling, or that in our data there is simply not enough statistical power to precisely estimate those effects.

The rest of the paper is organized as follows. Section 2 describes the data sets used in this paper. The methodology is explained in section 3. The effect of the 1997 reform and new class openings on educational attainment is analyzed in section 4. The effect of education on several health outcomes is examined in section 5, while section 6 concludes the paper.

2. Data

The individual-level data sets used in this study are the Turkey Demographic and Health Survey (TDHS) for the years 2008 and 2013 and the Turkish Health Survey (THS) for the years 2008, 2010, 2012, and 2014. Both surveys are nationally representative, and each has strengths and weaknesses.

The Turkey Demographic and Health Survey (TDHS) has been carried out by Hacettepe University Institute of Population Studies (HUIPS) since 1968. It is done every five years and collects data on socio-demographic characteristics of households such as age, education, marital status, and region of birth of each member of the household. The main focus of the survey is on ever-married women aged 15-49 for which very detailed information on individual characteristics as well as marriage, fertility, migration, and health of mothers and their children are obtained. Relative to other nationally representative surveys, the TDHS provides very detailed information on individuals' education, age, birth year, region of birth, region of residence, region of childhood, and migration history. Moreover, the TDHS reports women's measured height and weight as well as very detailed information on marriage, fertility, use of birth control methods, and their children's health. These data have been used in prior studies analyzing the health effects of increased education in Turkey, particularly the 2003 and 2008 years (Dinçer et al., 2014) and the 2008 year (Güneş, 2015). We take advantage of the most recent year of the survey, 2013, where the health outcomes of individuals exposed to the natural experiment we employ for identification are measured when they are older. In contrast, we do not employ the 2003 year of the TDHS since the corresponding individuals are relatively young and little time has passed since these individuals were exposed to the reform.

The Turkish Health Survey (THS) is conducted by the Turkish Statistical Institute (TURKSTAT) biennially, since 2008. The THS is a nationally representative survey which aims at providing health indicators such as infant and child health conditions, adults' self-reported health, height and weight, smoking behavior, and utilization of health services covering all settlements in the territory of Turkey except for the institutionalized population. The THS is a self-reported survey, unlike the TDHS. In contrast, the THS covers all men and women in Turkey, while the TDHS covers ever-married women aged 15-49. Thus, the THS gives us the opportunity to examine the health effects of increased education for males and females regardless of marital status.

Schooling is given as years of schooling and completed level of education in the TDHS. However, only completed level of education is available in the THS. We imputed years of education in the THS data by the years of schooling required to complete each level of education.[4] Note that some birth cohorts in the treated group are quite young in the survey years and thus might not have completed their education. Hence, comparing years of education, especially using THS data, might be problematic. For this reason, we define another schooling variable that is a dummy variable which takes the value 1 if an individual completed at least middle school (8^{th} grade) and zero otherwise. The results presented below use "at least middle school completion" as the schooling variable, but we provide estimates using years of schooling in the Appendix (Part A).

We restrict our attention to the 1982-1991 birth cohorts from both surveys.[5] We excluded the 1986 and 1987 birth cohorts in our analysis because we are unable to distinguish between those affected and those not affected by the law. We will discuss this in more detail in the next section. We further exclude those individuals born outside Turkey since they may obtain their education in another country.[6] Moreover, observations with missing values in our outcome and education variables are excluded from our analysis.[7] Summary statistics of selected variables are presented in Table 1 for both the THS (Panel A) and the TDHS (Panel B) data.

We used regional schooling statistics extracted from the National Education Statistics in order to construct our intensity measure of the reform. National Education Statistics provide us with information on the number of students, number of teachers, number of classes in each grade, and the number of schools in each level of education at the beginning of each school year, disaggregated at the 81 province levels. TURKSTAT releases these data by the level of education on its website. The statistics for primary school (1^{st} -5^{th} grades) and middle school (6^{th} -8^{th} grades) are provided separately before 1997; however, due to the 1997 Basic Education Law, the statistics for the basic primary education (1^{st} -8^{th} grades) are provided jointly since 1997. Since our intensity measure is based on the increase in the number of classes for the 6^{th}-8^{th} grades, we extracted the number of classes for the 6^{th}, 7^{th}, and 8^{th} grades after 1996 to determine the number of new class openings in each region from the National Education Statistics books for the years 1991-2003. The number of classes reported by the National Education Statistics are the number of classes at the beginning of that education year. Thus, the number of classes in the grades of interest (6^{th} to 8^{th}) at the beginning of the 1997-1998 education year is used as the base number and the increase in this number represents the new class openings in each region and is used to construct the intensity measure of the reform.

[4] Individuals who completed some years in any level of education but did not finish that level, are assigned the years of education required for the last completed education level. For example, a person who left school after the 10th grade, completed middle school (basic primary education) but not high school. This individual will be assigned 8 years of schooling.

[5] We also considered robustness of our results below to the inclusion of the 1981 and 1992 cohorts.

[6] We dropped 127 observations in the THS data corresponding to individuals born in another country and 49 observations in the TDHS corresponding to individuals who spent most of their time in a foreign country until the age of 12.

[7] In the THS data, 645 observations contained missing values on either height or weight information, 2 observations on health status ("good health") and 11 observations on whether or not the respondent had any health problem in the last six months. These were dropped from the analysis. In the TDHS data, 388 observations had missing BMI information and were also dropped from the analysis. Table 1 shows the number of observations used for each dependent variable.

Prior work in Dinçer et al. (2014) and Güneş (2015) have exploited different measures of intensity of the same reform. In particular, Dinçer et al. (2014) employ the expansion in the number of teachers after 1997. However, their measure is for the combination of grades 1 through 8, thus likely containing variation corresponding to changes unrelated to the reform intended to affect grades 6 through 8. Güneş (2015) instead employs the number of classrooms constructed between 1996 and 1997 also for grades 1 through 8, resulting in a similar potential inaccuracy. Moreover, there is evidence that after the reform some schools increased double-shifts in order to meet the educational demand (e.g., Dulger, 2004 reports an increase in double shifts from 16% in 1996 to 22% in 2003). This factor likely impacts the previously employed measures of intensity of the reform if some of the same teachers or classrooms are employed in double-shifts. In contrast, the measure based on the number of classes is less affected by this feature.

3. Methodology

3.1. Empirical Strategy

Following the previous literature, the effect of schooling on health behaviors and health outcomes is estimated by adopting the following equation:

$$H_{ijkt} = \beta_1 S_{ijkt} + \beta_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + u_{ijkt} \quad (1)$$

where H_{ijkt} is a health outcome or a health behavior for individual i in region j who is in birth cohort k and observed in survey year t. This is a function of schooling S_{ijkt}, and individual characteristics X_{ijkt}; such as gender and marital status. Region fixed effects, cohort fixed effects, and survey year fixed effects are represented by μ_j, θ_k, λ_t, respectively.[8]

In this model, schooling might affect health through increasing the efficiency of the production of health outcomes or through enabling individuals to choose a better input mix to produce better health (Grossman, 1972, 2006). These mechanisms are termed as "productive efficiency" and "allocative efficiency", respectively. Schooling is likely endogenous since an individual with poor health will have low schooling. Moreover, there might be some omitted variables, such as ability or time preference that would affect the individuals' decisions on both health and education. For instance, Fuchs (1982) points out that more future-oriented individuals invest more, both in their schooling and health. Thus, any estimation based on equation (1) would likely suffer from endogeneity.

There might also be an indirect effect of education on health through various channels such as income, occupation, or cognitive/non-cognitive skills. It is well established that schooling increases individual wages and the probability of having better occupational opportunities (e.g. Psacharopoulos and Patrinos, 2004; Card,1999; Duflo, 2001). Consequently, individuals with higher income and in safer occupations might have better health. However, it is also possible that higher income may have a negative effect on health outcomes by allowing individuals to afford harmful substances or too much food (Grossman, 2006). Another important set of indirect channels

[8] We use a quadratic polynomial in age instead of cohort fixed effects in our IV estimations that use the reform dummy as an instrument.

via which education may affect health is through changes in cognitive and non-cognitive skills due to schooling, such as time preference, self-motivation, patience etc. In particular, the model in Heckman (2007) indicates that changes in those skills early in life can have substantial health consequences later in life. Regardless of the specific channels through which education may impact the health outcomes in our analysis, which we cannot explicitly assess, it is plausible that increased middle school education in Turkey can have future health effects, and the direction of those potential effects is a-priori ambiguous.

We tackle the likely endogeneity of schooling by using instrumental variables based on the 1997 compulsory schooling reform. In first instance, we may consider the following equation for the first stage estimation;

$$S_{ijt} = \gamma_1 T_i + \gamma_2 X_{ijt} + \mu_j + \lambda_t + v_{ijt} \quad (2a)$$

where S_{ijt} is schooling of individual i in region j at time t. Schooling is represented by a dummy variable indicating whether an individual attained 8 years or more of schooling. T_i is a dummy variable which takes a value of 1 if an individual was born after 1987 (i.e., was exposed to the reform). X_{ijt} includes control variables such as gender, age, age square, and marital status. μ_j represent the region fixed effects, while λ_t represents the year fixed effects.

Instead of using a binary indicator of exposure to the reform as the instrument, we follow Duflo (2001) and Chou et al. (2010) and consider an intensity measure of the reform that exploits its variation across birth cohorts and regions:

$$S_{ijkt} = \gamma_1 (P_{jk} * T_i) + \gamma_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + v_{ijkt} \quad (2b)$$

where T_i is the same dummy variable that is used in equation (2a). Note that equations (2a) and (2b) differ in the variable representing the change in compulsory schooling. Here, P_{jk} is the intensity measure for cohort k in region j, which represents the number of new middle school class openings in region j at the time cohort k starts 6^{th} grade. X_{ijt} includes control variables such as gender and marital status. We controlled for birth cohort fixed effects, θ_k, the region fixed effects, μ_j, and the year fixed effects, λ_t. Moreover, an interaction term of the treatment dummy with middle school enrollment rate in 1996 is included in the estimation in order to control for the pre-reform regional differences in education.

3.2. Identification Strategy

The identification strategy is based on the Basic Education Law enacted in 1997. This mandate creates an exogenous source of variation in the schooling of individuals based on their birth year. The government constructed new classrooms and recruited new teachers to accommodate the expected enrollment increase. There was considerable variation across regions based on their pre-reform enrollment rate. We take advantage of this exogenous variation in education to estimate the impact of education on health. There were no changes to the curriculum, see Dulger (2004) for a detailed discussion of the Turkish Education System and the Basic Education law.

The Basic Education Law (Law No: 4306) was enacted on August 16, 1997 and the first group affected by the law were those in the 5th grade in the 1997/1998 school year. Therefore, the birth year and school starting age play a crucial role in the determination of those who are affected and those who just missed the reform. Those who started the 1st grade in 1993 were in the 5th grade in the 1997/1998 school year. In this case, the 1987 birth cohort should have started the 1st grade in 1993 and would be the first cohort affected by the reform. Furthermore, some children born in 1986, who deferred primary school entrance one year, were also affected by the reform. There is no way to distinguish between the affected and unaffected groups in the 1986 birth cohort because of the exception in the bylaw of Ministry of National Education (MONE).[9] We therefore exclude the 1986 birth cohort from our analysis. Furthermore, since the THS has no birth year variable and we are forced to obtain it using the survey year and the completed age; the THS 1987 birth cohort contains some people whose actual birth year is 1986 as well as others whose actual birth year is 1987. Thus, we also exclude the 1987 birth cohort from our analysis.[10] In order to obtain similar treatment and control groups, we define the treatment group as the 1988-1991 birth cohorts, while the control group as 1982-1985 birth cohorts.

The approach of instrumenting education with a dummy variable for the law change has been employed by Lleras-Muney (2005), Angrist and Krueger (1992), and Cesur et al. (2014), among others. Here, the identification of the schooling effect on pecuniary or health outcomes is achieved by comparison of two groups for which different compulsory schooling regimes are mandated. However, it has been shown that failing to control for the regional differential effects may bias the estimated effects (e.g., Stephens and Yang, 2014; Mazumder, 2008). Thus, using a treatment dummy as an instrument might be problematic since the timing of the policy coincides with a time at which various national policy changes occurred that could affect health outcomes.

The Turkish Health system has undergone several changes since 2003 with the introduction of the Health Transformation Program (HTP). Prior to 2003, there was a fragmented social security and health insurance system. There were three social security institutions: Social Insurances Organization (SSK) which covered wage-workers and their dependents; the Social Insurance Agency for Merchants, Artisans, Self-Employed (Bağ-Kur) which covered merchants, artisans, and self-employed and their dependents; and the Government Employees' Retirement Fund (Emekli Sandığı) which covered retired civil servants and their dependents. Moreover, the health care of non-covered and poor citizens was partly financed by the government through the "Green Card" system which was initiated in 1992 (OECD, 2007). There were a small minority who could afford to purchase private health care. The health care provision was also fragmented. There were Ministry of Health (MoH) hospitals, university hospitals, SSK Hospitals, and private hospitals. People under different social security coverage could be admitted to certain hospitals. For example, a worker covered by SSK could receive service only in the SSK hospitals unless referred to other hospitals (MoH, 2012). After 2003, the social security and health care provision went through a gradual transformation in order to create a more equitable health care system for all citizens (see MoH, 2012 for an extensive assessment report on HTP). All citizens have been allowed to be admitted

[9] The enrollment rule is determined by the bylaw published August 7, 1992 in the Official Gazette of the Republic of Turkey, which states: "All children who completed 72 months at the end of the calendar year shall be enrolled in the 1st grade. Those who are required to be enrolled but physically underdeveloped can continue pre-school education one more year with the written consent of the parents." (Resmi Gazete, August 7, 1992).

[10] We checked the robustness of our results to the inclusion of the 1987 and/or 1986 birth cohorts. Our conclusions remain the same when including those cohorts.

by private hospitals using their health insurance in 2003. All public hospitals have been unified under a single roof and Green Card holders have been enabled to receive health services like insured ones in 2005 (MoH, 2012). The Social Insurance and General Health Insurance law (Law No. 5510) which unified the social security funds and created a universal health care system was enacted in 2006 (Resmi Gazete, 2006). Family Medicine, which was introduced in 2005 as a pilot implementation in Düzce province, became countrywide in 2010 (MoH, 2012). Furthermore, the first anti-tobacco law was amended in 1996 and extended its coverage prohibiting all indoor smoking in public places in 2008 (WHO, 2014). If these major health care-related reforms affected differentially the treated and control cohorts defined by the binary reform exposure indicator, then the identification of the effect of schooling on health based on such binary reform indicator would be jeopardized. For this reason, we employ the additional variation available in our instrument capturing the intensity of the reform.

Our main identification strategy exploits the regional and temporal variation in the educational supply investments that occurred with the compulsory schooling reform. This approach is commonly used in the literature as exogenous variation in educational attainment to identify the effect of education on various outcomes, such as wages, infant mortality, smoking, and marriage decisions. Currie and Moretti (2003) use the variation in the availability of colleges in a person's residence county in the US. Duflo (2001) and Breierova and Duflo (2004) use the primary school construction boom in Indonesia. Chou et al. (2010) use the middle school construction variation along with a compulsory schooling change in Taiwan. Dinçer et al. (2014) use the regional variation in the number of primary-school (1^{st}-8^{th}) teachers to instrument educational attainment in the context of the same reform we use.

While the 1997 reform was universal across the country, the intensity of the reform varied across regions and over time. The investments in constructing new classrooms, repairing existing ones and recruiting new teachers to meet the expected increase in the 6^{th}-8^{th} grades varied across regions based on their pre-reform endowments. One of the main objectives of the reform was referred to as the inclusion of those "hard-to-reach" groups of students who lived in rural areas and had low socio-economic background (Dulger, 2004). The larger the intensity of the reform, the larger is the expected effect of the reform on educational attainment in a given region (Chou et al., 2010). Thus, accounting for the intensity of the reform provides us with another source of variation across regions and cohorts, in addition to the one-time variation across cohorts. Next, we present graphical evidence on the variation in the timing of the law change and its regional intensity.

Figure 1(a) shows that the middle school completion rate increased from 0.65 for the last cohort of the control group (1985) to 0.85 for the first cohort of the treatment group (1988). This shows graphically that the 1997 education reform had a sizeable jump on educational attainment in Turkey. Figure 1(b) shows the middle school completion rate by birth cohort for males and females separately. It is clear that the middle school completion rate is higher for males than for females both in the control and treatment groups. Also, the jump in the middle school completion rate corresponding to the 1997 reform is higher for females than for males.

The regional variation in the percentage increase in the number of middle school classes for the school years 1998-2003 are shown in Figure 2. The maps show that the increase varies substantially across regions and the year at which a birth cohort starts 6^{th} grade. It is shown that the highest rate of middle school class openings is experienced by the southeastern regions of Turkey which were historically lagging behind in schooling attainment. Furthermore, the regional variation in the percentage increase in the middle school classes per 1000 children aged 11-13, which we call class-to-child ratio, for the school years 1998-2003 are shown in Figure 3. This increase also varies considerably across regions and the year at which a birth cohort starts the 6th grade.

Table 2 shows the disaggregated class-to-child ratio by region and school year. The rate of increase in the class-to-child ratio was similar across regions in the pre-reform period; however, it varied between 34% (TR1-Istanbul) and 117% (TRC-Southeast Anatolia) after the reform. The 12 (NUTS-1) regions are grouped as TR-Low and TR-High, based on their class-to-child ratio in 1997. Regions with a class-to-child ratio that is lower than the average ratio of the whole country (TRA, TRB, TRC) are represented by "Turkey-Low" and the rest of the regions are represented by "Turkey-High". Note that the average rate of increase in the class-to-student ratio is somewhat higher in TR-High regions in the pre-reform period; however, the average increase in the TR-Low regions is steeper in the post-reform period. Table 2 also shows that there is a sizable increase in the number of middle classes after the 1997 reform.

Based on the new number of middle-school classes per 1000 children (aged 11-13) across regions and cohorts, we create an intensity measure for the 12 NUTS-1 regions following Duflo (2001) and Chou et al. (2010). Different intensity measures of the same reform were used by Dinçer et al. (2014) and Güneş (2015), however, as discussed before, the intensity measure based on number of classes better reflects the regional variation in the schooling investments related to the 1997 compulsory schooling reform.

4. The Schooling Effect of the 1997 Reform and New Class Openings

It is important to determine the region where the individual was educated in order to gauge the effect of the new class openings associated with the 1997 reform. The estimated effect might be biased if individuals who receive their education in one region migrate to another region and we do not account for that. THS reports the region of residence only, while TDHS reports information on region of birth, region of residence, and the region in which the individual lived until the age of 12. In this section, we estimate the effect of the intensity of the reform on educational attainment using the THS data. Using the TDHS data, we check the robustness of the intensity measure making use of the information on the region in which the individual lived until the age of 12. Furthermore, we test whether there is a spillover effect of the reform and its intensity on education levels beyond middle school using the actual years of schooling of ever-married women in the TDHS.

We first estimate the effect of the 1997 compulsory schooling law on educational attainment using the treatment dummy in equation (2a). Panel A of Table 3 shows the OLS results using equation (2a) for all individuals as well as for females and males separately. These results in Panel A show that the 1997 reform was effective in increasing the educational attainment in Turkey. The compulsory education law increased the completion of at least middle school by 16.1 percentage points. The effect is found to be higher for females than for males, 22 percentage points versus 10.4 percentage points. Comparing our results with Cesur et al.(2014), we find very similar effects for females and males. Furthermore, the first three columns show the effect of the reform on years of schooling. It is found that the compulsory schooling law increased years of schooling by 0.74 years. The effect is slightly higher for females than for males.

Moving on to the effect of the intensity of the 1997 reform on schooling, we present the OLS estimates for γ_1 in equation (2b) in Panel B of Table 3. The results show that one additional new middle-school class opening per 1000 children, on average, increases the treated group's probability of completing at least middle school by 2.3 percentage points. These estimates are 3 percentage points for females and a statistically insignificant 1.3 percentage points for males. Also, the first 3 columns show the estimates using years of education as the schooling variable. It is found that one additional new middle-school class increases years of schooling by 0.08 years. The effect is found to be higher for females than for males but these estimates are statistically insignificant. This finding gives another reason to focus our main analysis using the completion of at least middle school treatment variable, in addition to the previously stated argument that our treated group is still young and some individuals might not have completed their schooling yet.

Recall that our estimates using the THS data are based on the use of region of residence as a proxy for the region of education. Thus, migration could bias those estimates. We take advantage of the TDHS data in which females were asked about their province of birth and the province that they spent most of their time until the age of 12, which we call the region of childhood. In order to check the robustness of these results to possible migration, we replicate Table 3 using the TDHS data.

The TDHS data shows that around 22 percent of the sample of individuals who were born between 1982 and 1991 (the 1986 and 1987 birth cohorts excluded) reside in a different region from their region of childhood.[11] Table 4 shows the OLS estimates of γ_1 in equation (2b) using the TDHS data for different sub-samples in which we matched the intensity measure based on the region of childhood defined above. Panel A of Table 4 shows the effect of 1997 reform on schooling. The last 4 columns show that the 1997 reform increased at least middle school completion by 27 percentage points. It is also found that the reform was more effective on the female's middle school or higher education than male's. Furthermore, using years of education, we find that the 1997 reform increased schooling by 1.48 years. This is higher than previously found in Table 3 which is consistent with our concern about imputing years of schooling for the relatively young sample in the THS data.

We further show the impact of new middle-school classes that accompanied the 1997 reform on schooling in Panel B of Table 4. The first 4 columns of Table 4 show that, on average, one additional middle-school class opening per 1000 children increases the years of schooling of the treatment group by 0.28 years. The corresponding figure for females is 0.24 years. The third and fourth columns show the OLS estimates for the ever-married sample that allows comparing females and males in the TDHS data. The impact is higher for husbands than for wives, 0.34 versus 0.26 years, with the coefficient for the husbands statistically significant only at the 10 percent level, consistent with prior results in Table 3. Comparing our estimates for the ever-married female sample to other studies using the same data, Güneş (2015) finds that one additional classroom increases the schooling by 0.04 years, while Dinçer et al. (2014) finds that one percentage point increase in the teacher-child ratio increases schooling by about 0.7-0.8 years.

[11] The mover rate is slightly higher for the control group than for the treated group, 22.2 % and 20.6%, respectively.

The last four columns of Table 4 show the coefficients of the intensity on the probability of completing at least middle school. The estimated coefficient of interest (γ_1) is statistically significant in all samples. The fifth column of Table 4 shows that, on average, one additional middle-school class opening associated with the 1997 reform increases the probability of completing at least 8 years of schooling by 7.1 percentage points which is almost triple the size found in Table 3. The sixth column shows that the impact on all females is 7.1 percentage points. For the ever-married sample, the middle-school class openings increased the ever-married women's probability of completing middle school or higher by 8.4 percentage points. The corresponding figure for ever-married men is 7.2 percentage points.

Table 4 reveals that new middle-school class openings per 1000 children associated with the 1997 reform is effective in increasing educational attainment in Turkey, and the size of the effect is higher than that found in Table 3, suggesting that the effect of the policy on schooling attainment is not robust to migration. Specifically, migration not accounted for in the THS yields considerably smaller effects of the policy on schooling attainment.

Next, we examine the effect of the reform and the intensity of the reform on different levels of education following Duflo (2001). We do this using equations (2a) and (2b) with the TDHS data. We define "m", for m= 1 to 17, dummy variables indicating whether an individual completed at least "m" years of education or not. Then, the effects of the reform (using equation (2a)) and the intensity of the reform (using equation (2b)) are estimated for each dummy variable using a linear probability model. The estimated coefficients and the corresponding 95% confidence intervals are plotted in Figure 4. Figure 4 (a) shows the estimated effects of the reform on each level of education. It indicates that the 1997 reform was effective in increasing the ever-married women's probability of completing middle school (6^{th}-8^{th} grades) with a magnitude of around 30 percentage points. There are some spillovers beyond middle school; however, the effects become insignificant beyond the 12^{th} grade. Moreover, Figure 4 (b) shows the estimated effects of the intensity of the reform on each level of education using equation (2b). Figure 4 (b) shows that one additional middle-school class opening per 1000 children increases the ever-married women's propensity of completing middle school (6^{th}-8^{th} grades) by around 8 percentage points. There are no significant spillover effects beyond the 8th grade.

Overall, we find that the 1997 reform and the new middle-school class openings per 1000 children at a differential rate across regions increased the educational attainment significantly. The impact is more pronounced for female's educational attainment than for male's. The reform and the intensity of the reform were mainly effective in increasing middle school (6^{th}-8^{th} grades) education. Also, we find that the use of the region of residence as a proxy for the region where education was attained results in considerable attenuation of the effect of the policy on schooling attainment due to migration.

5. The Effect of Education on Health Behavior and Health Outcomes

Having shown that the 1997 Basic Education Law and the variation in the implementation of the reform across regions increased the educational attainment, we analyze the effect of education on self-rated health, smoking, BMI, obesity, and being overweight. Using equation (1), the coefficient of interest β_1 is estimated using OLS and instrumental variables (IV), and these estimates are presented for each health outcome in Tables 5-9. The estimates in the previous section based on equations (2a) and (2b), serve as first stage estimates in our IV estimation. The schooling variable used in our estimates is a dummy variable indicating whether an individual completed middle school or higher. For reference, we provide estimates using years of schooling in the Appendix (Part A) despite the problems indicated with this variable. The IV estimates, for each dependent variable considered, are presented separately for instruments T_i (treatment dummy) and $(P_{jk} * T_i)$ (intensity of the reform) for middle-school completion. When we use the intensity of the reform, we control for the pre-reform regional differences in education by including an interaction term of the treatment dummy with middle school enrollment rate in 1996. Furthermore, in the Appendix (Part B) we show that the estimates presented in the balance of this section are all robust to the inclusion of region-specific time trends.

5.1. BMI, Obesity and being Overweight

According to the OECD Health Statistics 2014, the obesity rate for Turkish women ranked 2nd place among OECD countries, while the obesity rate for men is below the OECD average. Figures 5-7 show the average BMI and the proportion of obese and overweight by birth cohort for low-intensity and high-intensity regions, by gender, using self-reported height and weight (THS data) and females' measured height and weight (THDS data). The figures indicate that the prevalence of being obese and overweight is lower among younger cohorts for both males and females. However, there is no systematic jump between the treated and control groups. The only exception is females' being overweight in low-intensity regions using the THS data (Figure 6(b)). There is a jump around the discontinuity showing that the treated women in the low-intensity region have a lower rate of being overweight. However, using measured BMI, Figure 7(b) shows that there is no jump in the rate of being obese between the treated and the control groups using the TDHS data. This suggests that it is important to consider measured BMI instead of self-reported BMI.

Table 5 shows the OLS and IV results for equation (1) with the natural logarithm of self-reported BMI, being obese, and being overweight as the dependent variables. This uses the THS data for the whole sample (Panel A) as well as for females (Panel B) and males (Panel C). The schooling variable measures completion of at least middle school. All standard errors are clustered by birth cohort and region. The first column of Table 5 shows the OLS results for log BMI, implying that having at least middle school education decreases the body mass index by 2 percent for the whole sample. Middle school completion decreases the BMI of women by 4 percent but is not statistically significant for men. We define dummy variables for being obese ($BMI \geq 30\ kg/m^2$) and being overweight ($30\frac{kg}{m^2} > BMI \geq 25\frac{kg}{m^2}$). Columns (4) and (7) show the OLS results for the dependent variable being obese and being overweight, respectively. We find that having at least middle school education decreases the probability of being obese and being overweight for the whole sample by 2 percentage points for each of the dependent variables. Also, education is more effective in decreasing the propensity of being obese or overweight for females rather than males. Nevertheless, these OLS estimates do not address the endogeneity of schooling. These favorable effects of educational attainment on weight problems for Turkey were documented by other studies (see Cesur et al., 2014; and Tansel and Karaoglan, 2016).

We now present the IV estimates that address endogeneity using two different instruments which we defined earlier: a dummy variable for being treated by the 1997 reform (IV-1), and an intensity measure of the reform (IV-2). Columns (2), (5), and (8) in Table 5 show the IV results of equation (1) instrumenting the middle school completion by the treatment dummy, while columns (3), (6), and (9) show the IV results instrumenting the schooling variable by the intensity of the 1997 reform. For the whole sample, the F-statistics of the excluded instruments in the first stage (shown in brackets) are all greater than 10, indicating that both instruments are not weak (following the rule of thumb suggested by Staiger and Stock, 1997). For the female sample, the intensity measure of the 1997 reform is found to be weak, while for the male sample both instruments are weak.

The IV results in the second column of Table 5 show that instrumenting middle school completion by the treatment dummy results in a positive effect of education on BMI but this is only statistically significant at the 10% level. The third column shows that instrumenting schooling by the intensity of reform results in a 5-percent reduction in the BMI for the whole sample. However, it is not statistically significant at conventional levels. In fact, all of the IV estimates are statistically insignificant at the 5% level, finding that the increase in education has no effect on BMI, obesity or being overweight for the whole sample as well as for males and females, separately. We also present the corresponding results using years of schooling in the Appendix, Table A1. The instruments are found to be weak in most cases and the conclusions remain the same—no statistically significant effects.

We take advantage of the TDHS data to address the concern in the THS that we need to assume that the region of residence is the same as the childhood region, which is relevant to the use of the intensity of the reform as an instrument. The TDHS has information on women's childhood region. Moreover, the TDHS provides measured height and weight of women, unlike the self-reported height and weight information in the THS. Table 6 shows the OLS and IV results for ever-married women (the target population in the TDHS). We find that the OLS results are statistically significant for BMI and obesity but statistically insignificant for being overweight. The size of the effects is larger than those found in Table 5. Using the childhood region as the region of education shows that the intensity of the 1997 reform has higher F-statistics for the excluded instruments in the first stage than the treatment dummy. Whether using the treatment dummy as an instrument for the middle school completion, or the intensity of the reform as an instrument, the effects of education on BMI, being obese or overweight remain statistically insignificant, increasing the confidence on the results reported in Table 5.

Overall, we find no statistically significant effects of education on BMI, obesity, and being overweight based on the estimated effects in Tables 5 and Table 6. These findings are in line with most of the recent literature for other countries (e.g., Kenkel et al., 2006; Arendt, 2005; Clark and Royer, 2013). Our IV results for men contrast somewhat with Cesur et al. (2014). They find a large positive effect of education on men's BMI and the probability of being obese.[12] They also find that completion of middle school has no statistically significant effect on female’s BMI and the probability of being overweight/obese. However, they advise the reader of a possible misreport in female weight which could have biased their estimates. The results in Table 6 are based on measured height and weight of women and show no statistically significant effect. Interestingly, Cutler and

[12] This difference may be driven by the fact that Cesur et al. (2014) include the 1987 birth cohort and do not use sampling weights provided in the dataset.

Lleras-Muney (2010) show that the effect of education on obesity is greater at higher levels of education, particularly beyond 12 years of schooling. Figure 4 shows that the reform and the intensity of the reform increased the average years of education in Turkey mainly by increasing middle school education (6^{th}-8^{th} grades) completion. Indeed, note that our IV results estimate local average treatment effects for those individuals that attained more schooling due to the reform at that margin of education (6^{th}-8^{th} grades) rather than the average treatment effect on the population (which would include effects at other education levels). Thus, our IV findings indicate that there is no statistically significant effect of education on weight problems on the middle school education margin in Turkey. This does not rule out the possibility of an effect on BMI/obesity at other levels of education.

5.2. Smoking

Turkey is ranked first among OECD countries in terms of men's smoking rate, while one of the lowest in terms of women's smoking rate. Figure 8 shows the percentage of people who have ever smoked regularly as well as the percentage of those who quit smoking using the THS data (for males and females in low-intensity and high-intensity regions, by birth cohorts). It shows that the prevalence of smoking is higher among males than females. The smoking rate becomes lower for younger cohorts of males and females. There is no clear systematic jump between the treated and the control groups. The probability of smoking cessation is somewhat similar for males and females.

Table 7 shows the OLS and IV results on the probability of having been a regular smoker, the probability of cessation of smoking conditional on ever smoked regularly, and the number of cigarettes smoked daily. All standard errors are clustered by region and birth cohort. The THS data does not ask questions related to smoking in the 2008 survey. Thus, the results in Table 7 are obtained using the 2010, 2012, and 2014 THS surveys. Column (1) of Table 7 shows that females with at least 8 years of schooling are 3 percentage points more likely to smoke regularly relative to those females with less than 8 years of schooling. This is statistically significant at the 5% level. However, there is no statistically significant effect for males. The probability of quitting smoking conditional on having smoked regularly increases for those who attain at least 8 years of education relative to those who had less than 8 years of schooling for the whole sample. This is also true for males, the increase is about 5 percentage points. There is no significant effect of education on the quitting behavior of females. Column (7) shows that the completion of middle school or higher leads to a decrease in the number of cigarettes consumed per day of 1.26 for the whole sample and of 1.84 for males compared to those who did not complete at least middle school. There is a statistically insignificant small negative effect for women.

Table 7, columns (2), (5), and (8) employ the instrumental variables approach using the treatment dummy instrument, while columns (3), (6), and (9) employ the intensity of the 1997 reform as instrument. In most cases, the first stage F-statistics show that the intensity of the reform is a weak instrument while the treatment dummy is not as weak. Overall, we find no statistically significant effect of education on smoking behavior based on the IV results in Table 7. These findings are in line with those found in Cesur et al. (2014) and Tansel and Karaoglan (2016). Moreover, Cesur et al. (2014) test their results against a possible differential effect of the anti-smoking law on the treated and the control groups' smoking behavior that may introduce a bias. They find that the results are robust to controlling for the anti-smoking law. This is consistent with the corresponding estimates in Appendix Table B3 in which we find that including region-specific trends does not change our results. Furthermore, Appendix Table A3 replicates Table 7 using years of education, where it can be seen that the corresponding instruments are weaker and the estimates remain statistically insignificant.

These results might be driven by the fact that the sample is quite young. In fact, the Global Adult Tobacco Survey in Turkey 2012 shows that around 80 percent of those who smoke regularly start smoking before the age of 20 with an average of 16.6-years-old among men and 17.8-years-old among women (WHO, 2014). Therefore, if there is any effect of education on the decision to smoke, we should have been able to detect it at least in the first three columns of Table 7 (unless our sample size is not enough to achieve acceptable precision). These findings are in line with studies in which the instruments are based on interventions at lower levels of education (e.g., Clark and Royer, 2013; Park and Kang, 2008; Kemptner et al., 2011). On the other hand, de Walque (2007) finds a statistically significant effect of university education on smoking behavior using the Vietnam War lottery draft as an instrument. Cutler and Lleras-Muney (2010) show that the favorable effect of education on smoking behavior is more pronounced at higher levels of education than at lower levels. Since the instruments we used are based on an intervention that affected mostly the completion of 8 years of schooling, this could be the driving factor behind the lack of statistical evidence of an effect of education on smoking behavior. Also, we note that the instruments are found to be weak in most of the specifications for these outcomes.

5.3. Self-Rated Health

The THS data includes a self-rated health question in which individuals are asked to assess their general health status. The respondents grade their overall health as very poor, poor, fair, good, and excellent. Self-rated health is considered a good indicator for mortality and morbidity (Idler and Benyamini, 1997) or a measure for general health status (Arendt, 2005). We define a dummy variable, "good health", which takes a value of 1 if an individual assesses his/her health as good or excellent, and 0 otherwise. The THS data also asks whether the individual has had any health problems in the last 6 months. We use these two variables to examine the relationship between self-rated health and education. Figure 9 presents the percentage of individuals who report that they have good health (Figure 9(a)) or had a health problem in the last 6 months (Figure 9(b)) by birth cohorts, for males and females, in low and high intensity regions. The graphs show that the probability of reporting good health increases, while having any health problem decreases for the younger cohorts. While there is no systematic difference in reporting good health between the treated and the control groups by gender, we do see small jumps in having health problems in the last 6 months.

Table 8 reports the OLS and IV estimates of equation (3) with the dependent variables "good health" and "had any health problems in the last 6 months". The OLS results show that there is a statistically significant positive correlation between self-rated health and the completion of at least 8 years of schooling, with an estimated coefficient of 10 percentage points. However, instrumenting middle school completion by the treatment dummy or intensity of the reform leads to statistically insignificant estimates of the relationship between education and self-rated health. We also find that those with at least middle-school are 5 percentage points less likely to have any health problems in the last 6 months relative to those with less than middle-school education based on the OLS estimation results. Again, the IV estimates show that there is no statistically significant effect of education on having had any health problems in the last 6 months. In most of the cases, the first-stage F-statistics for the instruments suggest they are not weak. The statistically insignificant IV results are in line with the findings of Cesur et al. (2014). The Appendix Table A4 shows that using years of education) results in estimates that lead to the same conclusions.

5.4. Well-being of Infants

Studies relating maternal education and the infant's birth outcomes in different countries include Currie and Moretti (2003), Chou et al. (2010) and Güneş (2015), to mention a few. We use TDHS data for the years 2008 and 2013 to analyze the effect of maternal education on the infant's birth-weight. Figure 10 shows the mean birth weight of infants by their mothers birth year for low- and high-intensity regions. Figure 10(a) shows that there is no significant jump around the discontinuity. Table 9 shows the OLS and IV estimates of the effect of mothers' education on the log of infants' weight at birth. We also test whether the effect differs for the birth weight of the first child of a mother, and consider a dummy variable for low birth weight which takes the value 1 if the birth-weight is lower than 2.5 kg and 0 otherwise. Figure 10(b) shows the percentage of low-birth-weighted infants by mothers' birth year, where there seems to be a jump around the discontinuity. The OLS estimates in column (1) show that mothers' completion of at least middle school increases the birth weight of infants by 2 percent. However, it is statistically insignificant. Column (4) shows that the estimated coefficient is almost the same for the first birth as that for all births. Column (7) shows that the mother's completion of at least middle school lowers the probability of giving birth to a low-weight infant by 2 percentage points relative to the mothers with less than middle school education, but it is statistically insignificant at conventional levels.

We also examined if education has a significant effect on infant birth outcomes by employing instrumental variables. The first-stage F-statistics for the treatment dummy are over 10 (except one) which suggests it is not a weak instrument, while the intensity of the reform appears to be a weak instrument. The IV estimates indicate that none of the effects are statistically significant at conventional levels, regardless of the instrument. This is contrary to the findings of Güneş (2015). Furthermore, Appendix Table A5 shows the results using years of schooling. The first stage F-statistics indicate that the instruments are weaker, while the statistical insignificance of the IV effects remain.

Overall, the OLS estimates suggest that infants' birth weight is not improved by the increase in maternal education; correspondingly, the instrumental variable results also show that there is no statistically significant effect of maternal education on the infant's birth-weight outcome.

6. Conclusion

The effect of educational attainment on own-health behavior and outcomes, as well as maternal education on infant birth weight, are examined using a unique natural experiment in Turkey in which compulsory schooling increased from 5 to 8 years in 1997. The timing and implementation of the reform created an exogenous shock on individuals' educational attainment. The variation in the implementation across regions and birth cohorts enabled us to examine the relationship between education and health outcomes employing an instrumental variable approach.

We first examined the effect of the reform on educational attainment in Turkey by defining a treatment dummy for those who are affected by the reform. Our findings corroborate previous literature including Dinçer et al. (2014), Cesur et al. (2014), Güneş (2015), and Tansel and Karaoglan (2016), that find that the reform increased the educational attainment significantly. We subsequently define another instrument, the intensity of the 1997 reform, which takes into account the regional and cohort variation in the new middle-school class openings per 1000 middle-school-aged children. We find that one additional middle-school class opening (per 1000 children) is (causally) associated with the 1997 reform increasing the rate of completion of at least middle school by around 7 percentage points, on average. The effects are more pronounced for Turkish females than for males. Taking advantage of the detailed information on the region of childhood, birth, and residence in the TDHS data, we show that use of the region of residence as a proxy for region in which an individual attained his/her middle-school education considerably mitigates the estimated effect of middle-school class openings related to the 1997 reform.

We then analyzed the effect of education on various health measures, including BMI, obesity, smoking behavior, self-rated health, and the impact of maternal education on the birth weight outcome of infants. The OLS estimates for weight problems corroborate the findings in the literature for the whole sample as well as females, showing that there is a negative significant association between education and BMI, obesity, and being overweight. Using an instrumental variables approach, however, we find very weak evidence that middle school completion increases the BMI (calculated from self-reported height and weight) for the whole sample, instrumenting education with the policy treatment dummy. We find no evidence of a statistically significant effect using the intensity of the reform as an instrument. We also considered explicitly measured height and weight for females in the TDHS data and found no evidence of a statistically significant relationship between education and weight problems. This is in agreement with similar findings by Cesur et al. (2014) and Tansel and Karaoglan (2016) on BMI and obesity.

The effect of education on smoking behavior was also examined. OLS estimates show that middle school completion is associated with a higher probability of quitting smoking and lowers the number of cigarettes smoked per day conditional on being a current smoker. Addressing the endogeneity of education, we find no statistically significant effect of education on smoking behavior. We also find that completing middle school is associated with a higher probability of reporting good health and a lower probability of having had any health problems in the last 6 months, compared to those who have less than middle school. However, there is again no statistically significant effect detected using instrumental variables. This is also in agreement with similar findings by Cesur et al. (2014) and Tansel and Karaoglan (2016) on smoking and self-rated health variables.

Lastly, we considered the effect of maternal education on infant birth weight. We showed that completing at least middle school is (weakly) associated with higher birth weight and lower probability of giving birth to a low-birth-weight baby, compared to females who had less than middle-school. However, we found no statistically significant relationship between maternal education and birth weight outcomes of infants once we instrument middle school completion by either the treatment dummy or the intensity measure of the 1997 reform. The OLS results are in agreement with the findings of Güneş (2015) on the effect of maternal education on infant's health, but not the instrumental variable results.

In sum, we found no significant relationship between education and health outcomes for Turkey. As one of our referees suggest: "Some of the IV estimates discussed are non-significant but larger in absolute value than the OLS estimates suggesting that there may be sizable effects of education on health behaviors that the data do not allow one to precisely estimate. In other words many of the "zero" results presented are not precise zeros. As highlighted by Van Kipperluis et al. (2011) one of the main limitations of previous studies analyzing the effects of education on health or mortality is the reliance on small sample sizes and thus the limited identification power." As our data is based on representative surveys and not administrative data (as in Van Kipperluis et al., 2011), this is one possible reason for our lack of detection of statistically significant IV effects.
It should also be noted that our identification strategy is based on an intervention that primarily affected the middle school level. We showed that the reform and the intensity of the reform increased the average education in Turkey by increasing years of education in the 6^{th}-8^{th} grades. Since we employ this reform as an instrumental variable, our IV results represent a local average treatment effect (LATE) for those individuals that attained higher schooling (mainly at that margin) due to the reform (the compliers). This local effect may differ considerably from the (population) average treatment effect that will encompass the effects of an increase in education on health at other segments of the educational attainment distribution. Therefore, our estimates primarily indicate that there is no statistically significant effect of middle school education on health outcomes. However, this does not rule out an effect at other levels of education (e.g., high school and college). As one referee suggests, this is not in agreement with Heckman's (2007) "discussion about the role of education on health, and why education at *early* stages might matter (while at later stages it probably does not have an effect unless it provides *specific* knowledge"). Future research can shed more light on these conjectures using the 2012 reform, which further increased the compulsory schooling from 8 to 12 years in Turkey.

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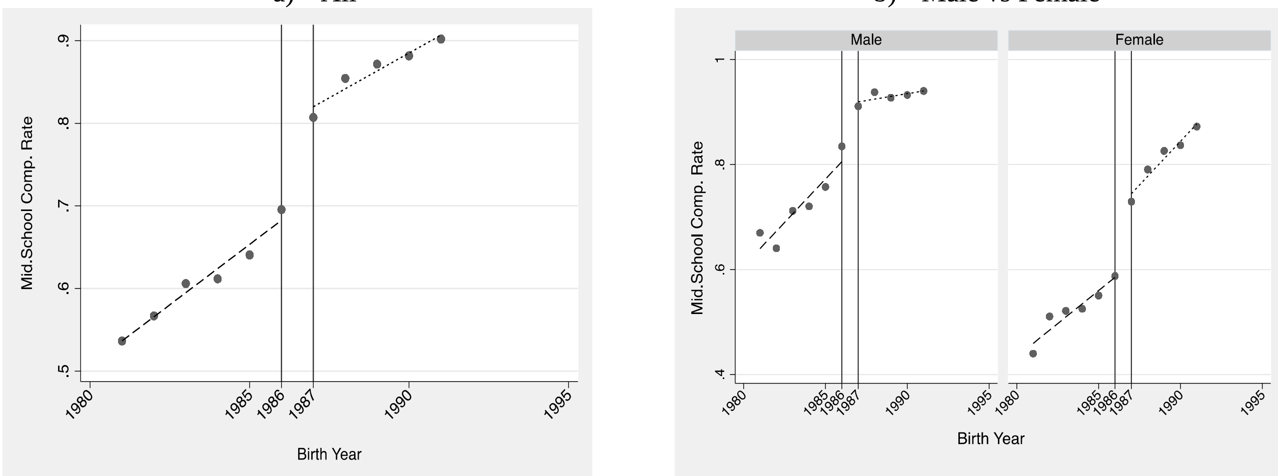
Figures and Tables:

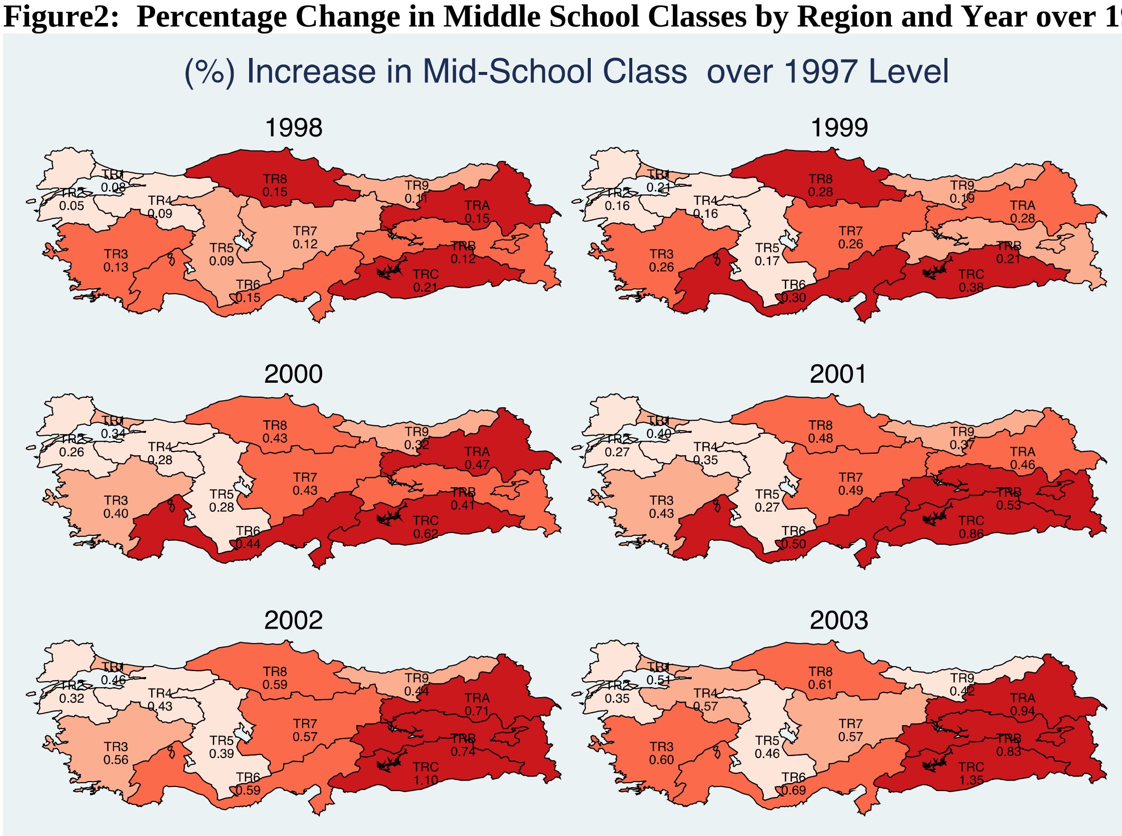
Figure 1: Middle School Completion Rate by Birth Year-THS Data:

a) All b) Male vs Female



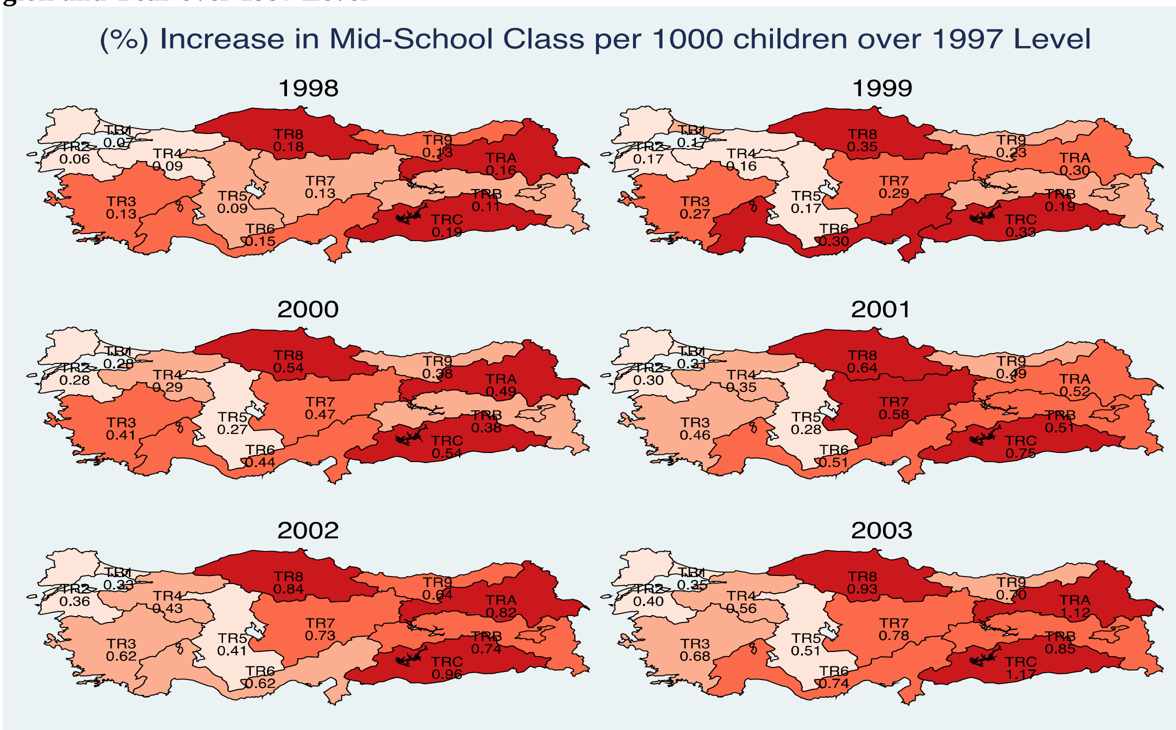
Source: Authors' calculation based on 2008, 2010, 2012, 2014 Turkish Health Survey (THS)

Figure2: Percentage Change in Middle School Classes by Region and Year over 1997 Level



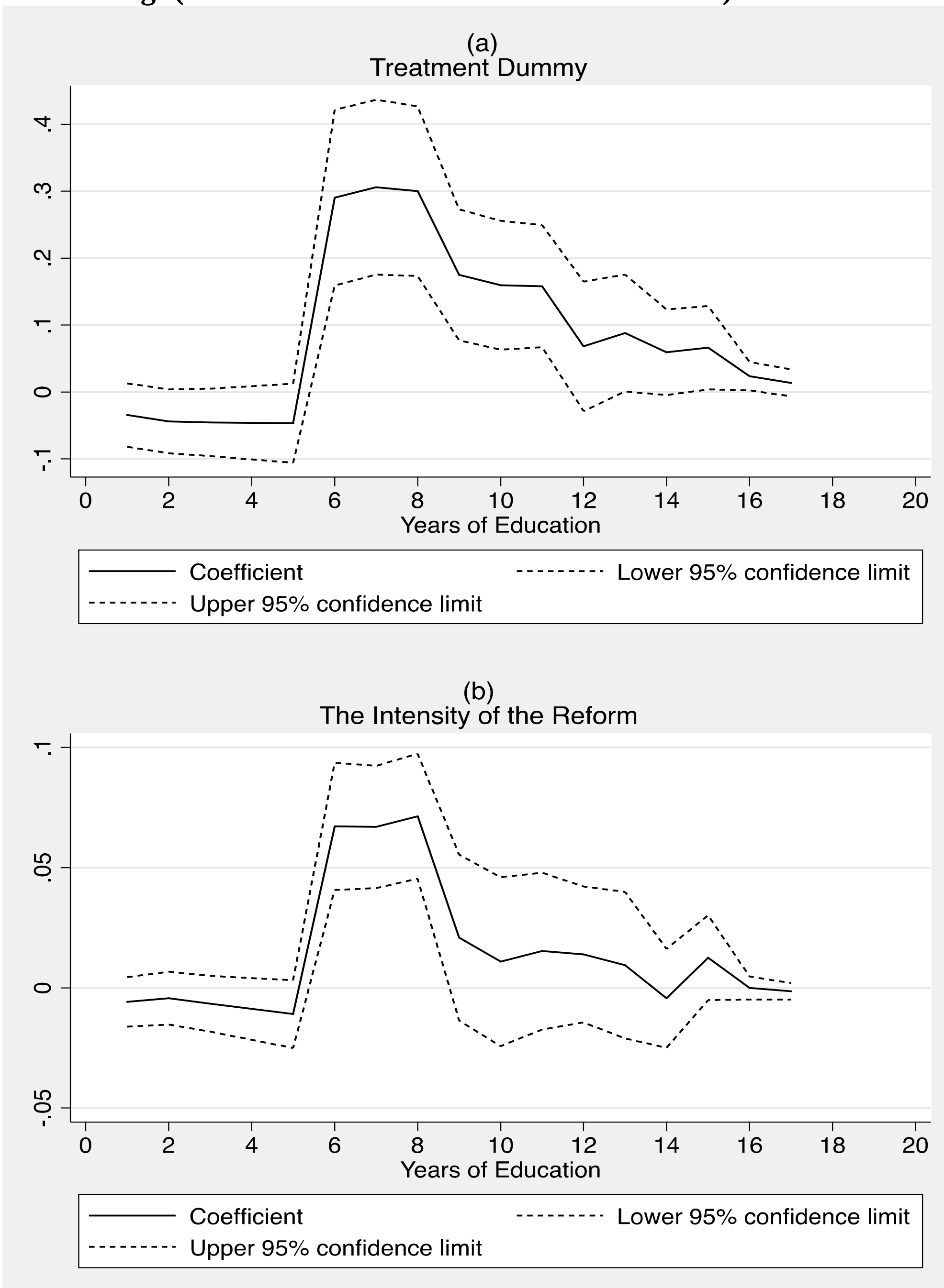
Source: Authors' Calculation MONE's National Education Statistics 1992-2003.

Figure 3: Percentage Change in the Number of Middle School Classes per 1000 Children by Region and Year over 1997 Level



Source: Authors' Calculation based on 1990, 2000, 2007 Population Censuses of Turkey and MONE's National Education Statistics 1992-2003.

Figure 4: The Effect of the Instruments on the Probability of Completing at Least 'm' Years of Schooling- (Ever-married Females in the TDHS Data)



Note: All coefficients and 95% CI's are estimated using linear probability models with the ever-married females sample of the 2008 and 2013 TDHS data.

Figure 5: Average BMI by Birth Cohort for High- and Low-Intensity Regions

Figure 5(a): Self-reported BMI

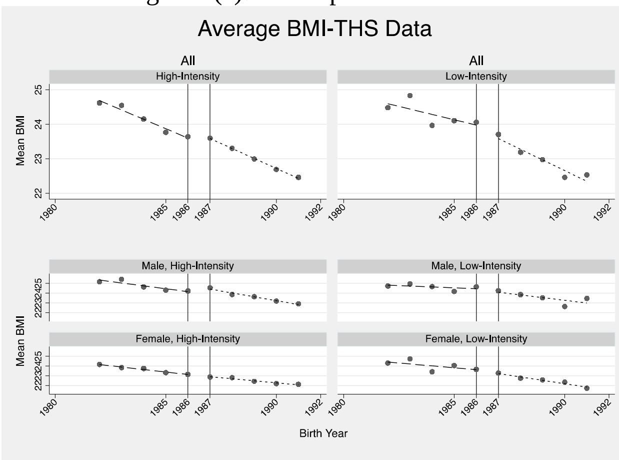
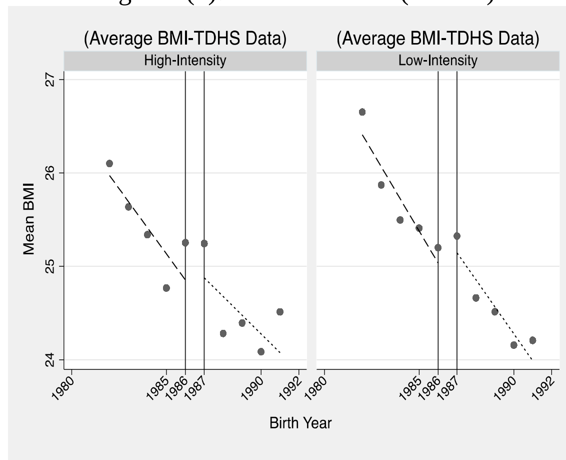


Figure 5(b): Measured BMI (Female)



Notes: (a) Authors' calculation based on the THS for the years 2008-2014. BMI is calculated using self-reported height and weight of individuals (b) Authors' calculation based on the TDHS for the years 2008,2013. BMI is calculated using measured height and weight of women. This information is only available for females in the TDHS data.

Figure 6: Obesity and Overweight Rate by Birth Cohort for Low- and High-Intensity Regions (THS Data)

Figure 6(a)

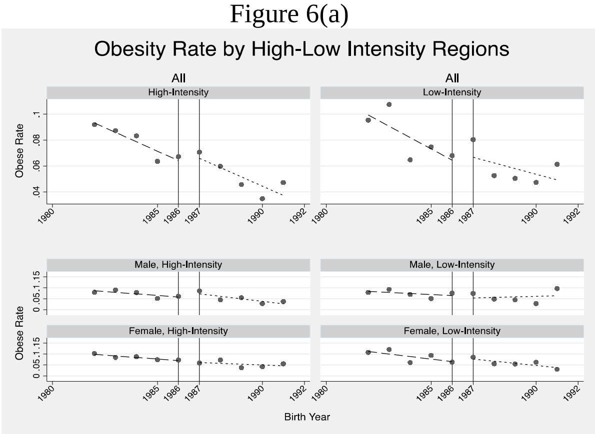
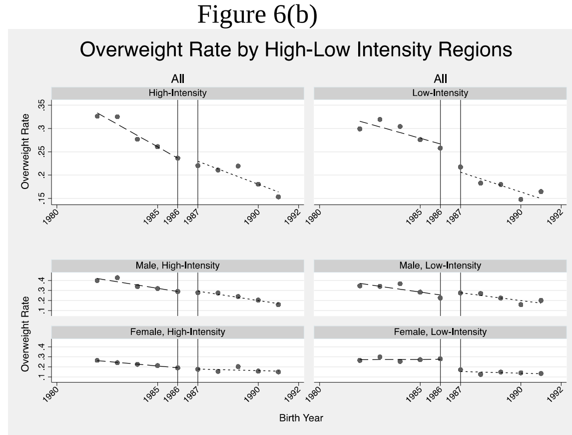


Figure 6(b)



Source: Authors' calculation based on 2008, 2010, 2012, 2014 Turkish Health Survey (THS). BMI is calculated using self-reported height and weight of individuals.

Figure 7: Obesity and Overweight Rate by Birth Cohort for Low- and High-Intensity Regions (TDHS Data)

Figure 7(a)

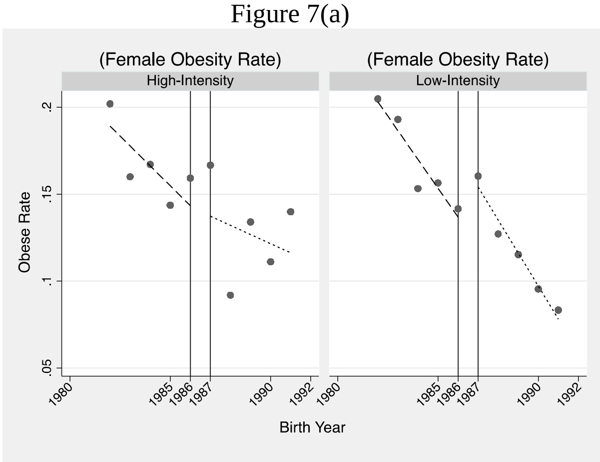
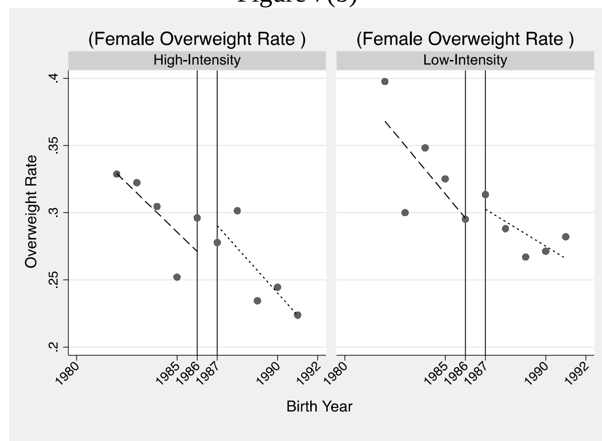


Figure 7(b)



Notes: Authors' calculation based on 2008, 2013 Turkey Demographic and Health Survey (TDHS). BMI is calculated using <u>measured</u> height and weight of women.

Figure 8: Smoking Behavior by Birth Cohort for Low- and High-Intensity Regions (THS Data)

Figure 8(a) Ever Smoked (%)

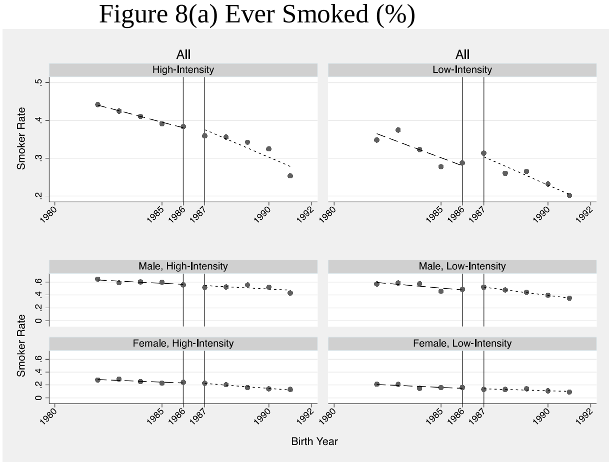
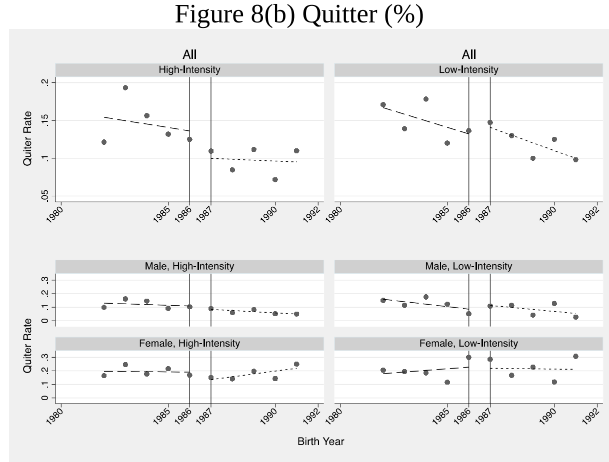


Figure 8(b) Quitter (%)



Notes: Authors' calculation based on the THS for the years 2010-2014. (a) "Ever Smoked" includes those who were classified as "Ever Smoked" regularly. (b) "Quitter" includes those who had smoked regularly and quitted smoking.

Figure 9: Self-Rated Health by Birth Cohort for Low- and High-Intensity Regions (THS Data)

Figure 9(a) Having Good Health (%)

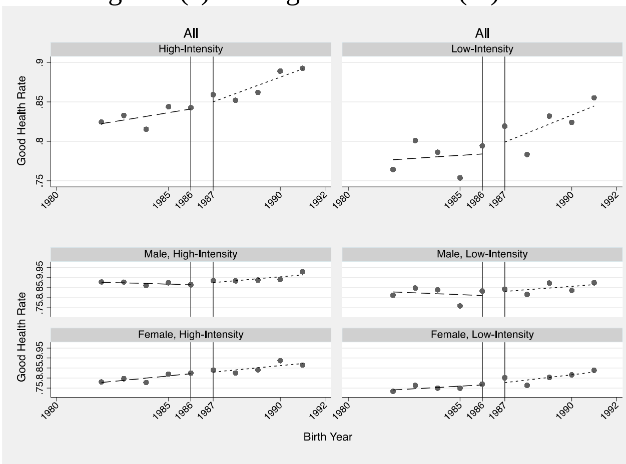
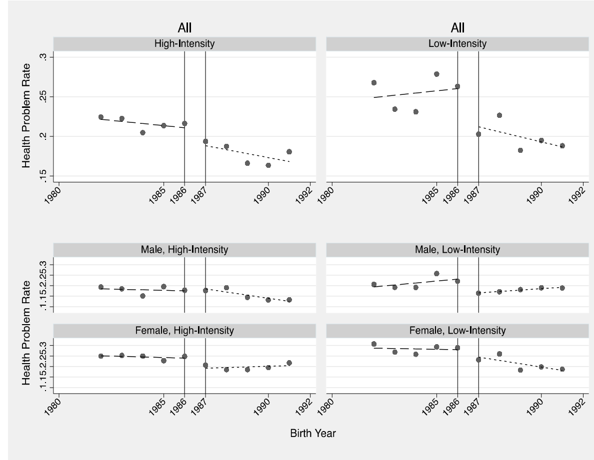


Figure 9(b) Health Problem in the Last 6 months (%)



Notes: Authors' calculation based on the THS for the years 2010-2014.

Figure 10: Infant Birth Weight by Mothers' Birth Year for Low-and High-Intensity Regions (TDHS Data)

Figure 10(a) Infants' Mean Birth Weight

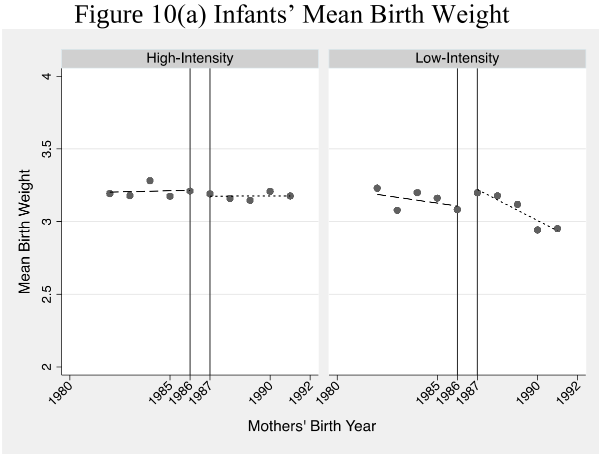
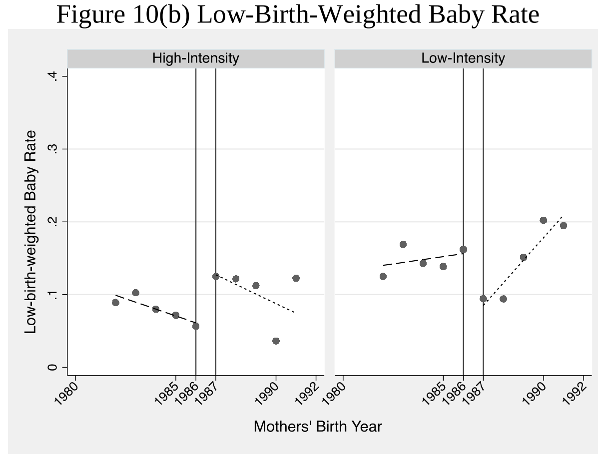


Figure 10(b) Low-Birth-Weighted Baby Rate



Notes: Authors' calculation based on 2008, 2013 Turkey Demographic and Health Survey (TDHS). The calculations are based on infants' measured weight in kg. Babies with birth weight lower than 2.5 kg are considered as low-birth-weighted.

Table 1: Descriptive Statistics

	Control			Treated		
VARIABLES	# Obs.	Mean	Std. Dev.	# Obs.	Mean	Std. Dev.
Panel A: The Turkish Health Survey (THS)						
Age	5,814	27.90	2.404	5,100	21.73	2.521
Female*	5,814	0.561	0.496	5,100	0.552	0.497
Years of Education	5,814	8.941	4.222	5,100	9.602	3.270
At Least Middle School*	5,814	0.605	0.489	5,100	0.877	0.328
Body Mass Index (BMI)	5,509	24.30	4.052	4,760	22.84	3.803
Obese*	5,509	0.0831	0.276	4,760	0.0485	0.215
Overweight*	5,509	0.299	0.458	4,760	0.185	0.388
Ever Smoked Regularly*	4,634	0.395	0.489	3,897	0.298	0.457
Quit Smoking*	1,832	0.151	0.358	1,160	0.0983	0.298
Number of Daily Cigarettes	1,434	13.08	9.629	973	12.93	9.576
Good Health*	5,812	0.815	0.389	5,100	0.859	0.348
Health Problem in the last 6 Months	5,810	0.226	0.418	5,093	0.182	0.386
Panel B: The Turkey Demographic and Health Survey (TDHS)-Female						
Age	2,125	27.37	2.708	1,431	22.54	2.185
Years of Education	2,125	6.742	4.298	1,431	8.365	4.453
At Least Middle School*	2,125	0.371	0.483	1,431	0.696	0.460
Body Mass Index(BMI)	1,891	26.04	4.893	1,277	24.41	4.695
Obese*	1,891	0.194	0.395	1,277	0.120	0.325
Overweight*	1,891	0.338	0.473	1,277	0.255	0.436

Notes: 1- Treated: 1988-1991 birth cohorts. Control: 1982-1985 birth cohorts. * Dichotomous variables. 2- Variables related to smoking are based on the 2010, 2012, and 2014 THS.

Table 2: Number of Middle School Classes (6th-8th) per 1000 Children Aged 11-13 by NUTS-1 Region and School Years

NUTS1	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
TR1	15.53	16.59	17.29	17.63	17.77	18.58	19.85	21.80	23.82	24.29	24.73	25.06
TR2	21.20	22.34	23.17	23.75	24.16	25.48	27.01	29.85	32.70	33.18	34.66	35.68
TR3	16.13	17.15	18.15	18.68	19.12	20.41	23.16	25.99	28.82	29.84	33.10	34.34
TR4	16.90	18.38	19.44	20.32	20.55	21.90	23.80	25.49	28.20	29.50	31.34	34.19
TR5	17.61	18.31	19.09	19.61	19.93	20.68	22.56	24.14	26.37	26.48	29.24	31.17
TR6	13.48	14.41	15.20	15.79	16.38	17.34	19.86	22.47	25.00	26.16	28.12	30.18
TR7	14.56	15.29	15.91	16.33	16.81	17.68	19.96	22.72	26.04	27.97	30.56	31.49
TR8	13.76	14.62	15.51	15.70	16.74	17.68	20.89	23.85	27.25	29.08	32.45	34.06
TR9	14.93	15.76	16.27	16.57	17.05	17.70	19.99	21.75	24.36	26.42	29.11	30.07
TRA	10.41	10.76	10.93	11.31	11.39	12.27	14.25	15.89	18.30	18.65	22.36	26.02
TRB	11.23	11.47	11.48	11.21	11.49	12.03	13.39	14.27	16.58	18.20	20.92	22.29
TRC	8.11	8.59	8.70	8.94	8.91	9.56	11.36	12.75	14.72	16.72	18.74	20.78
Turkey	14.24	15.07	15.71	16.09	16.44	17.34	19.41	21.47	23.97	25.13	27.35	28.91
TR-Low	9.92	10.27	10.37	10.48	10.60	11.28	13.00	14.30	16.53	17.86	20.67	23.03
TR-High	16.01	16.98	17.78	18.26	18.72	19.72	21.90	24.23	26.95	28.10	30.37	31.80

Notes: 1- The "Turkey" row shows the number of middle classes per 1000 children for the whole country.
2- "TR-Low" row shows the average number of middle school classes for those regions whose average was lower than for the whole country.
3- "TR-High" row shows the average number of middle school classes for those regions whose average was higher than for the whole country.

Table 3: The Effect of the 1997 Reform and Its Intensity on Education-The THS Data-OLS

	Years of Education			Middle School Completion		
VARIABLES	All	Female	Male	All	Female	Male
			Panel A			
Treatment Dummy	0.737***	0.781**	0.724**	0.161***	0.220***	0.104***
	(0.201)	(0.304)	(0.304)	(0.0222)	(0.0337)	(0.0318)
Observations	10,914	6,076	4,838	10,914	6,076	4,838
R-squared	0.144	0.175	0.080	0.190	0.210	0.127
			Panel B			
Intensity	0.0830*	0.112	0.0289	0.0230***	0.0298***	0.0126
	(0.0477)	(0.0686)	(0.0741)	(0.00568)	(0.00880)	(0.00868)
Observations	10,914	6,076	4,838	10,914	6,076	4,838
R-squared	0.144	0.174	0.083	0.189	0.209	0.128

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
3- Columns 1-3 control for age, age square, marital status, 12 NUTS-1 regions and year fixed effects. Columns 4-6 control for marital status and birth year, 12 NUTS-1 regions, and year fixed effects as well as an interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 4: The Effect of the Intensity of the 1997 Reform on Education-The TDHS data-OLS

	Years of Education				Middle School Completion			
		All	Ever-married			All	Ever-married	
VARIABLES	All	Female	Female	Male	All	Female	Female	Male
				Panel A				
Treatment Dummy	1.481***	1.493***	0.938**	1.056**	0.271***	0.301***	0.301***	0.169**
	(0.375)	(0.421)	(0.439)	(0.488)	(0.0539)	(0.0638)	(0.0712)	(0.0773)
Observations	5,309	3,556	2,938	1,753	5,309	3,556	2,938	1,753
R-squared	0.173	0.232	0.189	0.054	0.182	0.239	0.191	0.065
				Panel B				
Intensity	0.281**	0.241**	0.265**	0.343*	0.0709***	0.0711***	0.0837***	0.0721***
	(0.112)	(0.120)	(0.130)	(0.174)	(0.0113)	(0.0131)	(0.0182)	(0.0210)
Observations	5,309	3,556	2,938	1,753	5,309	3,556	2,938	1,753
R-squared	0.177	0.234	0.191	0.057	0.194	0.252	0.207	0.072

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1
2- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
3- All regressions control for birth year, the 12 NUTS-1 of childhood region (where an individual spent most of his/her time until the age of 12), and year fixed effects as well as an interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 5: The Effect of at Least Middle School Completion on BMI/Obesity (OLS and IV)

	Dep. Var.: log(BMI)			Dep. Var.: Obese			Dep. Var.: Overweight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Whole Sample					
Middle School Completion	-0.02***	0.09*	-0.05	-0.02***	0.03	0.13	-0.02**	0.06	-0.12
	(0.00)	(0.05)	(0.11)	(0.01)	(0.05)	(0.16)	(0.01)	(0.14)	(0.33)
1st Stage F-stat		[52.94]	[10.36]		[52.94]	[10.36]		[52.94]	[10.36]
# of Obs.	10,269	10,269	10,269	10,269	10,269	10,269	10,269	10,269	10,269
				Panel B: Female					
Middle School Completion	-0.04***	0.05	0.06	-0.04***	0.06	0.45	-0.05***	-0.23	-0.23
	(0.01)	(0.06)	(0.13)	(0.01)	(0.10)	(0.28)	(0.01)	(0.14)	(0.31)
1st Stage F-stat		[46.77]	[8.524]		[46.77]	[8.524]		[46.77]	[8.524]
# of Obs.	5,590	5,590	5,590	5,590	5,590	5,590	5,590	5,590	5,590
				Panel C: Male					
Middle School Completion	0.003	0.17	-0.45	0.01	-0.03	-0.95	0.01	0.72	-0.11
	(0.01)	(0.11)	(0.72)	(0.01)	(0.18)	(1.47)	(0.02)	(0.46)	(1.33)
1st Stage F-stat		[9.639]	[0.640]		[9.639]	[0.640]		[9.639]	[0.640]
# of Obs.	4,679	4,679	4,679	4,679	4,679	4,679	4,679	4,679	4,679

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics for the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity of the 1997 reform is the instrument.
4- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
5- All regressions control for, marital status, age and age square (birth year fixed effect in columns (3), (6), and (9)), 12 NUTS-1 regions, year fixed effects. Also, columns (3), (6), and (9) include interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 6: The Effect of Schooling on <u>Measured</u> BMI/Obesity (OLS and IV) – *(The TDHS data Ever-Married Female Sample)*

	Dep. Var.: log(BMI)			Dep. Var.: Obese			Dep. Var.: Overweight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
Middle School Completion	-0.06*** (0.01)	-0.00 (0.08)	0.09 (0.09)	-0.09*** (0.02)	0.003 (0.16)	0.10 (0.18)	-0.005 (0.02)	-0.09 (0.17)	0.26 (0.18)
1st Stage F-stat		[17.88]	[19.76]		[17.88]	[19.76]		[17.88]	[19.76]
# of Obs.	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics for the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity of the 1997 reform is the instrument.
4- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
5- BMI is based on measured heights and weights of ever-married women in the TDHS.
6- All regressions control for age and age square (birth year fixed effect in columns (3), (6), and (9)), 12 NUTS-1 of the childhood region (where an individual spent most of his/her time until the age of 12), and year fixed effects. Also, columns (3), (6), and (9) include interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 7: The Effect of at Least Middle School Completion on Smoking Behavior (OLS and IV)

	Dep. Var. : Ever Smoked			Dep. Var.: Quit Smoking			Dep. Var.: Num. of Cigarette		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Whole Sample					
Middle School Completion	0.00 (0.01)	0.19 (0.18)	0.59 (0.57)	0.05*** (0.01)	-0.05 (0.35)	0.70 (0.71)	-1.26** (0.55)	-15.99 (10.60)	20.91 (29.73)
1st Stage F-stat		[39.95]	[5.904]		[9.816]	[2.046]		[13.70]	[1.457]
# of Obs.	8,531	8,531	8,531	2,992	2,992	2,992	2,407	2,407	2,407
				Panel B: Female					
Middle School Completion	0.03** (0.01)	0.14 (0.14)	0.52 (0.38)	0.04 (0.03)	-0.29 (0.85)	0.27 (0.47)	-0.08 (0.66)	1.41 (14.82)	-2.95 (11.99)
1st Stage F-stat		[33.51]	[5.209]		[1.680]	[4.205]		[1.297]	[2.273]
# of Obs.	4,769	4,769	4,769	944	944	944	675	675	675
				Panel C: Male					
Middle School Completion	-0.04 (0.02)	0.27 (0.54)	0.87 (2.31)	0.05*** (0.01)	0.13 (0.35)	1.79 (4.56)	-1.84*** (0.70)	-21.17 (14.86)	56.32 (137.09)
1st Stage F-stat		[7.089]	[0.538]		[4.631]	[0.146]		[6.653]	[0.200]
# of Obs.	3,762	3,762	3,762	2,048	2,048	2,048	1,732	1,732	1,732

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics for the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity of the 1997 reform is the instrument.
4- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
5- All regressions control for, marital status, age and age square (birth year fixed effect in columns (3), (6), and (9)), 12 NUTS-1 regions, year fixed effects. Also, columns (3), (6), and (9) include interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 8: The Effect of at least Middle School Completion on Self-Reported Health (OLS and IV)

	Dep. Var.: Good Health			Dep. Var.: Health Problem in the last 6 Months		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2
Panel A: Whole Sample						
Middle School Completion	0.10*** (0.01)	-0.13 (0.15)	0.09 (0.22)	-0.05*** (0.01)	-0.11 (0.13)	0.02 (0.22)
1st Stage F-stat		[52.14]	[16.33]		[53.19]	[16.41]
# of Obs.	10,912	10,912	10,912	10,903	10,903	10,903
Panel B: Female						
Middle School Completion	0.09*** (0.01)	-0.06 (0.14)	0.21 (0.24)	-0.05*** (0.01)	-0.18 (0.14)	0.08 (0.25)
1st Stage F-stat		[42.22]	[11.53]		[42.29]	[12.03]
# of Obs.	6,075	6,075	6,075	6,071	6,071	6,071
Panel C: Male						
Middle School Completion	0.10*** (0.02)	-0.25 (0.27)	-0.25 (0.55)	-0.06*** (0.02)	0.06 (0.32)	-0.10 (0.71)
1st Stage F-stat		[10.50]	[2.013]		[12.24]	[1.974]
# of Obs.	4,837	4,837	4,837	4,832	4,832	4,832

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics for the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity of the 1997 reform is the instrument.
4- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
5- All regressions control for, marital status, age and age square (birth year fixed effect in columns (3) and (6)), 12 NUTS-1 regions, year fixed effects. Also, columns (3), (6), and (9) include interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 9: The Effect of Maternal Education on Infant Birth Weight (OLS and IV)

	Birth Weight (Log)			1st Child Birth Weight (Log)			Low Birth Weight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
Middle School Completion	0.02 (0.01)	0.04 (0.12)	-0.06 (0.16)	0.02 (0.01)	0.10 (0.14)	-0.19 (0.20)	-0.02 (0.02)	0.02 (0.16)	-0.10 (0.22)
1st Stage F-stat		[13.57]	[8.570]		[5.751]	[5.273]		[13.57]	[8.570]
# of Obs.	2,360	2,360	2,360	1,115	1,115	1,115	2,360	2,360	2,360

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics for the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity of the 1997 reform is the instrument.
4- Treatment group 1988-1991 birth cohort vs Control group 1982-1985.
5- BMI is based on measured heights and weights of ever-married women in the TDHS.
6- All regressions control for age and age square (birth year fixed effect in columns (3), (6), and (9)), 12 NUTS-1 of the childhood region (where an individual spent most of his/her time until the age of 12), and year fixed effects. Also, columns (3), (6), and (9) include interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.