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ABSTRACT

We use the 1997 Education Law in Turkey that increased compulsory formal schooling from five to eight years to study the effect of women's education on a range of outcomes relating to women's fertility, their children's health and measures of empowerment. We apply an instrumental variables methodology and find that a 10 percentage point increase in the proportion of ever married women with eight years of schooling lowered number of pregnancies per woman by 0.13 and number of children per women by 0.11. There is also some evidence of a decline in child mortality, caused by mother's education, but effects turn statistically insignificant in our preferred models. We also find that a 10 percentage point increase in the proportion with eight years of schooling raised the proportion of women using modern family planning methods by eight to nine percent and the proportion of women with knowledge of their ovulation cycle by five to seven percent. However, we find little evidence that schooling changed women's attitudes towards gender equality.

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Introduction

The impacts of women's education on their wellbeing and the wellbeing of their children have been widely documented.[1] But almost all of the research is based on non-Middle Eastern countries. Cultural norms and social environments in Middle Eastern societies discriminate against women, limit their economic and educational opportunities, and relegate them to a lesser status than men (UNDP, 2005). The returns to women's education may be different in a social and cultural environment that discriminates against them than in societies that accord them a more equal status. Despite its significance, there is limited empirical research to estimate the effects of women's education within the social and cultural settings of a Middle Eastern country. Such research is critical in light of recent papers that cast doubt on previous findings of positive effects of maternal education on fertility and infant health (Lindeboom, Llena-Nozal, and Der Klaauw, 2009; McCrary & Royer, 2011; Zhang, 2011).

In this paper, we take advantage of Turkey's Compulsory Education Law, and variation in the intensity of its implementation across regions in Turkey, to study the effect of women's formal years of schooling on a range of measures that capture women's fertility, empowerment, and child mortality. Turkey is the largest economy in the Middle East and by many measures, a relatively modern society. Despite its growing economic and geopolitical influence, the position of women in Turkey continues to be defined along traditional lines: In 2010, only 27% of women (versus 47% of men) had a secondary or higher education and a mere 24% worked for wages (versus 70% men) – a proportion that declined from 32% in 1990 (UNDP, 2011). Surveys indicate that a third of women in Turkey have been exposed to physical violence at home

[1] See literature reviewed in Black and Devereux (2011), Card (1999), Grossman (2000, 2006), and Kaushal (forthcoming).

(Altinay & Arat, 2007). In 2011, Turkey was ranked 122nd (out of 135 countries) in the gender equality index of the World Economic Forum (Hausmann, Tyson, & Zahidi, 2012).

In 1997, Turkey passed the Compulsory Education Law that increased mandatory formal schooling from five to eight years. Individuals born after 1985 (who were 11 or less in 1997) were the target of the Compulsory Education Law. Its primary objective was to prepare Turkey's entry into the European Union by increasing educational attainment and reducing geographic and gender-specific educational disparity. Access to education has been widely acknowledged by the EU as a means of enhancing economic and social development in Turkey as well as in bringing economic and social cohesion across its eastern and western regions. To accommodate the expected increase in enrollment, the government devoted additional resources on school infrastructure and in hiring new teachers leading to a 36% increase in primary school teachers during 1996-2003 (Dulger, 2004; State Institute of Statistics, 1999; Turkish Statistical Institute, 2006).

We capitalize on the 1997 compulsory school reform legislation to estimate the causal effect of women's schooling on a range of outcomes relating to child mortality, women's reproductive health, and measures of empowerment, including age at first marriage, age at first child birth, use of contraceptives, fertility outcomes, and attitudes towards gender equality. We form a treatment group of women who were born between 1986 and 1990 and were affected by the legislation and a corresponding comparison group of women who were born between 1979 and 1985 and were not affected. Investment in new teachers varied across the sub-regions of Turkey. Within each sub-region, we exploit variations across cohorts in the number of primary school teachers in the sub-region of residence at age 11 to construct an instrument to predict the educational attainment of young women. The predicted education variable is then used to

estimate the effect of education on a variety of outcomes experienced by the treatment group of women and their offspring from information obtained when the treatment cohort was between the ages of 18 and 22 and the comparison cohort was between the ages of 23 and 29.

Review of Literature

Economists argue that more educated individuals are more efficient producers of health and more educated parents are more efficient in producing healthy children (Grossman, 2006). Knowledge helps parents make informed decisions on their children's nutrition and healthcare. It influences health-related behaviors (such as smoking, drug abuse, binge drinking) and lifestyles (e.g. physical exercise), and parents', in particular mother's, health behavior and lifestyle impact child health (e.g. birth weight). Parental education is also the most basic component of socio-economic status, which according to epidemiologists is the key determinant of own and child health (Adler & Newman, 2002). Further, education may affect attitudes towards gender equality empowering women (Mocan & Cannonier, 2012). Because mothers are often the primary caregiver for infants and young children, their empowerment is likely to channel family resources towards mother- and child-wellbeing.

There is extensive empirical evidence of the association between parental education and child health (see Grossman, 2006, for a detailed review of this literature). Because genetic endowments are a key determinant of a child's health, it is challenging to provide convincing evidence that the correlation between parental education and child health implies causality: that parental education improves child health. Arguably, heritable ability may result in more able women seeking higher education and having more able children who have better health (Behrman & Rosenzweig, 2002). Further, future orientation may cause mothers to acquire more

education and invest in their children's health (Fuchs, 1982). In short, an unobserved third factor may be causing both higher education among women and better health of their children.

Several researchers have applied increases in parental education resulting from compulsory education laws in different countries to study the effect of an exogenous increase in parental education on the health outcomes of their children (Breierova & Duflo, 2004; Chou, Grossman, Liu, & Joyce, 2010; and Osili & Long, 2008). These studies exploit changes in compulsory education laws in Indonesia, Taiwan, and Nigeria and conclude that parent's education has a negative effect on child and infant mortality. In contrast to the findings of these investigations, a recent innovative study that uses the decline in maternal education triggered by high-school closures during the Cultural Revolution in China from 1977 to 1984 finds that women who completed high-school were more likely to use prenatal care and were more likely to work off-farm, but their high-school completion had no effect on premature-births, low-birth weight, neonatal mortality, and infant mortality (Zhang, 2011).[2] Two other studies, one based on U.S. data and the other on British data, reached similar conclusions. McCrary and Royer (2011) used school entry policies in the US to identify the effect of mother's education on fertility and infant health and found these effects of be small and possibly heterogeneous. Lindeboom, Llena-Nozal, and Der Klaauw (2009) used British compulsory schooling laws and found that postponing the school leaving age of parents by one year had little effect on the health of their children. These findings thus cast doubt on previous research on the effects of mother's education on child health and its applicability across diverse cultural and institutional settings.

[2] Similarly, researchers have applied different models to reach different conclusions. Kan and Lee (2012) apply a regression discontinuity model and find that while the change in compulsory education law in Taiwan increased women's education, it had no effect on their fertility and child mortality. However, they do not use the extensive school intensity data employed by Chou and colleagues (2010). Further, Kan and Lee's treatment groups omit a substantial number of women who were affected by the law and include some women who were not affected by it.

Researchers have also investigated the effect of education on early marriage and childbearing in adolescence – both are known to have adverse consequences on mother and child health (WHO, 1995). This is an important issue in many Middle Eastern countries where marriage and child bearing in adolescence are high. For instance, approximately 17% of ever-married women aged 20-45 in Turkey are married before the age of 16 and 13% have a child before they turn 17.[3] A reduction in childbearing in adolescence is likely to improve birth outcomes and mother's and child's health. Becker's human capital model, for instance, predicts that education results in a quantity-quality trade off in fertility: more educated parents opting for fewer children of higher quality – e.g. better health (Becker & Lewis, 1973).

Empirical studies also suggest that more educated couples have wider knowledge, and make more efficient use of contraceptive methods (Rosenzweig and Schultz, 1989; Breierova and Duflo, 2004). If mothers' education causes a reduction in early marriage and childbearing and improves fertility outcomes, it will improve mother- and child-health. Establishing causality between mother's education and early marriage, early child-bearing, and fertility outcomes is also a challenge because low level of empowerment and high dependency may result in women marrying early and having children thus forgoing education. While this phenomenon may be more prevalent in Middle Eastern countries, in western societies too, teenage pregnancy may limit the options of young mothers and interrupt their schooling.[4] In this context, fertility will be an endogenous variable affecting schooling (Angrist & Evans, 1998). In general, the observed association between low-education and early marriage and fertility could simply be on account

[3]Authors' computations based on the 2008 Turkey Demographic and Health Survey.
[4] In some industrialized societies, where fertility has been declining and secondary education is almost universal among women, researchers have been asking a somewhat different question: Is education or higher education an obstacle to women's fertility (Cohen, Øystein, & Keilman, 2011).

of reverse causality or an unobserved third factor causing both low education and early childbearing.

Here again researchers have used "natural experiments" to determine the direction of causality between education and marriage and education and teenage fertility. Currie and Moretti (2003) use data on opening of two and four year colleges between 1940 and 1990 in the US as an instrument to predict maternal education to study the effect of the predicted education variable on mother's marriage, infant health, use of prenatal care, and smoking and find that mother's education has a positive influence on these outcomes. Similarly, Osili and Long (2008) exploited the Universal Primary Education Program introduced in Nigeria in 1976 and exposure to this program by age and region to study the effect of women's education on their fertility and found that increasing female education by one year reduced early fertility by 0.26 births. Using the extension of compulsory education from 6^{th} to 9^{th} grade in Mexico in 1993, Andalon, Grossman, & Williams (2013) find that raising women's education beyond 6^{th} grade improved their knowledge and use of contraception. Again, whether findings from these studies can be generalized across cultural and institutional settings is an empirical issue and we investigate that in the context of a Middle Eastern country.

Previous Research on Effects of Education in Turkey

There is no published research on of the effects of mother's formal education on child health and mortality in Turkey. Two working papers have studied the effects of Turkey's Compulsory Education Law on marriage and fertility (Kırdar, Dayıoğlu Tayfur, & Koç, 2011) and on religious tolerance and attitudes towards women's empowerment (Gulesci & Meyersson, 2012). Both studies fail to adjust for confounding factors correlated with the policy as they are

based on changes in outcomes from the pre- to post-policy periods. There were several economic and social factors and policy changes coinciding with the education reform that could potentially confound these estimates. For instance, this is a period of a steady decline in women's employment in Turkey.[5] The decline is often attributed to urbanization and shifts in family activities away from agriculture to sectors where women's participation is relatively low (World Bank, 2009). Further, the 1990s is a decade of financial instability in Turkey that culminated in the 2001 financial crisis (Görmez and Yiğit, 2010). Such national trends in women's employment, urbanization, and overall economic growth are likely to confound estimates of the effect of education reform in a research design that is based on pre- to post- policy changes in outcomes.

In addition, changes in social policy may also have a confounding effect. For instance, in 2002, a change in the Civil Code raised the minimum marriage age of women in Turkey from 15 to 17 years, making it equal to the minimum marriage age of men. Nationwide women aged 11 or less in 1997, the target of the Compulsory Education law, are also affected by the change in the Civil Code since they were all less than 17 in 2002. Indeed, any methodology based on comparisons of outcomes of women born before and after 1986 will not be able to distinguish the effect of the Compulsory Education Law from the effect of the change in legal minimum marriage age.

Gulesci and Meyersson use a regression discontinuity model and assume that women born in or after September 1986 are bound by the Compulsory Schooling Law to acquire 8 years of schooling. The Bylaw of Primary Education in Turkey, however, counts age by calendar year,

[5] Women's employment declined from 34% in 1988 to 22% in 2008 (The World Bank, 2009).

and not school year.[6] Thus it is likely that Gulesci and Meyersson measured the target group of the Compulsory School Law with some degree of error. [7]

In our analysis, we assume that schools in Turkey follow the Bylaw of Primary Educations Institutions for admission and in the implementation of Compulsory Schooling Law. Individuals born in 1986 or later are considered the target of education reform. Further, we apply changes in Compulsory Education Law and geographic differences in the intensity of its implementation, described in detail below, to identify the effects of ever-married women's education on child health and a range of measures capturing the fertility and empowerment of ever-married women. This methodology allows us to control for, in a parsimonious manner, unobserved national economic and social trends and policy changes that are correlated with education reform.

Turkey's Compulsory Education Law

In 1996, Turkey entered the European Union customs union and began preparing for full membership in future. Within the broader context of lowering economic and social disparities, in 1997, the Turkish government launched the Rapid Coverage of Compulsory Education Program that increased years of compulsory schooling from five to eight. To meet the expected increase in enrollment, during 1996-2003, the government built 80,000 new classrooms, a 41% increase over the 1996 base, and hired 103,000 additional primary school teachers, which was a 36%

[6] The Bylaw of Primary Education Institutions published in Official Gazette number 21308 on 7 August 1992 regulates school starting age in Article 14 as follows: "A child who completed 72 months at the end of the calendar year should enroll in primary school." (Authors' translation)

[7] To investigate how strictly schools adhere to the provisions of the Bylaw, we used the 1999 Trends in International Mathematics and Science Study (TIMSS) for Turkey on eighth graders in school year 1998-1999 and found that 22% were born in 1984 and 65% in 1985. Further, we find that the number of eighth graders born in January 1985is three-times the number born in December 1984, whereas the number of eighth graders born in September 1985 is somewhat less than those born in August 1985. Thus even in practice age at entry is counted as of the start of the calendar year (January 1st) and not the start of the school year (September 1st). Note that these students were in grade four in 1994-1995.

increase over the 1996 base (Dulger 2004; State Institute of Statistics, 1999; Turkish Statistical Institute, 2006; also see Figures 1a and 1b). Further, investment in new teachers and infrastructure varied across regions with the aim of devoting more resources to regions with low enrollment among primary school age students.

Primary school enrollment (grades 1-8) rose rapidly in the first four years of the reform: from 9.1 million in 1997 to 10.5 million in 2000 (Turkish Statistical Institute, 2006). Figure 2 presents the trend in gross enrollment in grades 6-8 during 1989-2000, covering eight years prior to the implementation of the Compulsory Education Law and eight years of the post-implementation period. There is a modestly upward trend in enrollment in grades 6-8 during 1989-1993, followed by a leveling off during 1993-1997. Enrollment begins to rise steadily after 1997, with the implementation of the Compulsory Education Law, as cohorts mandated to remain in school reach grade 6 or progress from grades 6 to 8, reaching a plateau about four years after the policy change. Overall, between 1996 and 2000 enrollment in grades 6-8 increased 1.1 million or 42 percent; and gross enrollment rate increased from 66% to 93%. Further, the gap in enrollment across the more developed western regions (e.g. Istanbul, West Marmara and East Marmara) and less developed eastern regions (e.g. Northeast Anatolia, Central East Anatolia, and Southeast Anatolia) declined during this period.

Data

The primary data used in this study come from the Turkey Demographic and Health Surveys (TDHS) of 2003 and 2008. The TDHSs collect data on demographic characteristics of each household member, including their age, sex, region and province of birth, birth place type (rural/urban), completed years of schooling, and current region and province of residence. For a

nationally representative sample of ever–married women aged 15 to 49, the TDHSs collect data on respondent's parents' education, mother tongue[8], and region[9] and province of residence in childhood.[10] Our analysis is based on the ever married sample and we focus on women aged 18 to 29 at the time of the survey. The variables on region of residence in childhood and cohort of childhood (when turned 11) are used to match TDHS individual level data with the administrative data on primary school teachers (teachers for grades 1-8) per child (aged 6 to 13) by region and year, henceforth referred to as teacher-child ratio, for convenience. The data on province of residence in childhood is used to construct 20 sub-regions of childhood.[11] Further, the variable on sub-region of residence in childhood and cohort of childhood (when turned 11) are used to match TDHS individual level data with the administrative data on teacher-child ratio by sub-region and year. The source of administrative data on primary school teachers, by sub-region (and by region) and year, is MONE's National Education Statistics. Estimates of number of children aged 6-13 by sub-region (and region) and year, come from Census 1985, 1990, and 2000; for inter-census years, these data are interpolated assuming a linear trend.

The ever married sample provides retrospective data on birth and fertility histories, including number of pregnancies, number of children born, first birth interval (age at first birth minus age at first marriage, measured in months), number of children who died before age 1, number of children who died before age 5, ever had a stillbirth, ever had a miscarriage, ever had induced abortion, and whether the respondent uses a modern family planning method. We use

[8] The responses to the question are: Turkish, Kurdish, Arabic, and other.
[9] There are 12 regions in Turkey: Istanbul, West Marmara, Aegean, East Marmara, West Anatolia, Mediterranean, Central Anatolia, West Black Sea, East Black Sea, North-eastern Anatolia, Central-eastern Anatolia, and South-eastern Anatolia.
[10] The respondents are asked the region where they spent most of the time until they were 12.
[11] Turkstat divides the country in 26 sub-regions. We merge 6 small sub-regions with other geographically contiguous sub-regions within the same region to ensure that all sub-regions have at least 100 observations. The 20 sub-regions we use are: Istanbul, West Marmara, İzmir & Aydın, Manisa, Bursa, Kocaeli, West Anatolia, Antalya, Adana, Hatay, Central Anatolia, Zongulda & Kastamonu, Samsun, Trabzon, North-eastern Anatolia, Malatya, Van, Gaziantep, Şanlıurfa, and Mardin.

data on marriage and fertility histories to construct outcome variables on age at first marriage and age at first birth and we use data on birth and fertility histories to create the outcome variables relating to child mortality and mother's fertility outcomes. Specifically, we study the following measures of child mortality: number of children deceased before the first month after birth, number of children deceased during 1-12 months after birth, number of children deceased before age 1, and number of children deceased before age 5; and the following measures of women's fertility: number of total births, number of pregnancies, use of contraceptives, and whether the woman has knowledge of her ovulation cycle.

We use responses to the following questions in the ever-married sample to study the effect of education on attitudes towards women's empowerment and gender equality: Does the respondent agree/disagree that men are wiser? Does the respondent agree/disagree that a boy's education is preferable to a girl's education? Does the respondent agree/disagree if all family decisions should be made by men?[12] And finally, we study responses to a set of questions on whether wife beating is justified if she (i) wastes money, (ii) neglects children, (iii) argues with husband, and (iv) refuses sex. Appendix 1 provides means of these variables for ever married women aged 18-22 and 23-29 in 2003 and 2008.

Research Design

Effect of Compulsory Education on Schooling

Our objective is to study the effect of mother's schooling on a range of outcomes measuring child mortality and women's empowerment. Education is endogenous to these outcomes and we use an instrumental variables methodology to address this issue. Equation (1)

[12] There is a difference in the language used for this question in the 2003 and 2008 surveys. The question in the 2003 TDHS is: "Important family decisions should be made by men" and in 2008: "All important family decisions should be made only by men."

describes the baseline first stage regression model to be estimated on a sample of ever married women aged 18-29 using the 2008 TDHS data:

(1) $Edu_i = \eta_c + \eta_j + \lambda_T Teacher_{j\tau} * Young + \lambda Teacher_{j\tau} + \Delta X + \rho * P_{j1996} * Cohort + e_{ijt}$

Edu_i denotes the education of woman i. We use two separate measures of education: a continuous variable indicating the respondent's years of schooling and a dichotomous variable indicating whether she has eight or more years of schooling. Edu_i is modeled as a function of the respondent's cohort of childhood (η_c - a dummy variable for the year respondent turned 11), region of childhood (η_j region where the respondent lived at age 11), and family endowments (X) namely parental education[13], mother-tongue (dummy variables indicating whether the mother-tongue is: Turkish, Kurdish, Arabic, and other), and whether she lived in a rural area in childhood (at age 11). The argument for including parents' schooling is that they may be affect own schooling and be correlated with program intensity. The argument for excluding parents' schooling is that they may be correlated with the some unobservable factor that is correlated with own schooling. In the empirical analysis, we run models with and without these controls. Estimates were similar from the two sets of models, and for brevity, we have opted to present findings with the controls.[14]

The variable Young is equal to 1 if the respondent was born during 1986-1990, and therefore was bound by the Compulsory Education Law to complete eight years of mandatory schooling, otherwise 0. Women born during 1979-1985 are the category of comparison. $Teacher_{j\tau}$ denotes the number of primary school teachers as a proportion to primary school age

[13] Seven dummy variables, for each parent, indicating the following categories: no education, primary drop out, primary (5 years of schooling), middle school (5-<8 years of schooling), lower secondary (8-<11 years), upper secondary (11 years), >11 years of schooling, and unknown schooling.
[14] Estimates without controls for parents' schooling are available upon request.

children (aged 6-13) in the region of childhood j in year τ (τ = year of birth + 11), and is a measure of the intensity of education reform by region and year.

The cohort-of-childhood dummy variables control for the national trends in schooling not related to the 1997 education reform and the region-of-childhood dummy variables control for cohort invariant region-specific unmeasured factors affecting the schooling outcome (e.g. differences across regions due to social and economic development). Parameter λ estimates the association between $Teacher_{j\tau}$ and schooling for the comparison group and $\lambda + \lambda_T$ measures the same for the treatment group (Young). The comparison group was not subject to the Compulsory Education Law, therefore, λ measures the effect on education of other time-varying factors correlated with the reform intensity variable - $Teacher_{j\tau}$. Assuming that these other time-varying factors correlated with the reform intensity variable had the same effect on the treatment and comparison groups, λ_T (= $\lambda + \lambda_T - \lambda$) would estimate the effect of the Compulsory Education Law on the schooling outcome of the treatment group (Young).

Equation (1) also includes a full set of interactions of the cohort of birth dummy variables with the gross enrollment rate in grades 6-8 (P_{j1996}) in 1996, a year prior to the implementation of the Education Reform Law. The intensity of reform was likely to be greater in regions that were lagging in education in the pre-reform period (e.g. Northeast Anatolia, Central East Anatolia, and Southeast Anatolia). Inclusion of the interaction term allows us to explicitly control for variation in intensity of the reform associated with enrollment in the pre-reform period.

The outcome variables in equation (1) are measured as of 2008. Thus one source of difference between the outcomes for the treatment and the comparison groups could be the difference in their ages. The identifying assumption for equation (1) is that in the absence of

education reform, time-varying factors correlated with the variable $Teacher_{j\tau}$ would have the same effect on the treatment and comparison groups. Because the outcome variables are sensitive to age, this is a limiting assumption and the resulting estimates are likely to be biased. We adopt a difference-in-difference methodology to address this issue. To implement this strategy, the first stage regression is estimated on a combined sample of ever married women aged 18-29 in the TDHS 2003 and 2008 data using the following model:

$$(2)\quad \begin{aligned} Edu_{ijt} = {} & \widetilde{\eta}_a + \widetilde{\eta}_c + \widetilde{\eta}_j + \widetilde{\lambda}_T\, Teacher_{j\tau} * Young * Yr2008 + \widetilde{\lambda}_y\, Teacher_{j\tau} * Young + \\ & \widetilde{\lambda}\, Teacher_{j\tau} + \widetilde{\rho} * P_{j1996} * Cohort + \widetilde{\Delta}\ X\ + \widetilde{e}_{ijt} \end{aligned}$$

In equation (2), the symbol ~ is used to distinguish the parameters from equation (1). Equation (2) differs from equation (1) in two respects. First, the former equation controls for a full set of age effects, denoted by $\widetilde{\eta}_a$, a dummy variable for each year of age. Equation (2) also includes cohort fixed effects. Thus, age-fixed effects control for nationwide trends in schooling and cohort effects control for nationwide changes specific to cohorts (e.g. the 2002 change in civil code that raised minimum marriage age for women and affected all cohorts born after 1985). Note that the 1981-1985 cohorts-of-birth appear both in the 2003 and 2008 data. These women were not covered by education reform and thus provide the counter-factual: changes in educational attainment (in the absence of education reform) during the study period.[15]

Second, equation (2) includes a three-way interaction term between $Teacher_{j\tau}$, *Young* and a dummy variable for TDHS 2008. Thus, inclusion of the 2003 data allows estimating the effect of education reforms after controlling for age-specific (young versus older women) time-varying factors that may be correlated with the reform intensity variable. In equation (2), $\widetilde{\lambda}$ captures the effect of time-varying factors correlated with the intensity of the reform on

[15] Note that these controls are critical for the second stage outcomes discussed in the next section.

schooling and $\widetilde{\lambda}_y$ allows these effects to be different for the younger and older cohorts. The parameter of interest is $\widetilde{\lambda}_T$ that estimates the effect of an exogenous increase in investment in primary school teachers resulting from the Compulsory Education Law on the schooling variables of Young ever-married women.

We estimate equation (2) with two alternative definitions of the geographical unit. The first is the region of childhood as specified above and the second is the sub-region of childhood. In the latter case $Teacher_{j\tau}$ is measured at the sub-region of childhood residence, P_{j1996}, is gross enrollment rate in grades 6-8 at the sub-regional level in 1996,[16] and the analysis includes a full set of dummy variables for the 20 sub-regions. The advantage of the regional specification is that estimates are based on a fairly large number of children in each region. The disadvantage is that there are only 12 regions. Our design requires standard errors of regression coefficients to be obtained by clustering on the geographical unit of residence (Huber, 1967). Standard errors tend to be understated if the number of clusters on which they are based is as small as 12 (Angrist & Pischke, 2009).

The advantage of the sub-regional specification is that the number of clusters increases from 12 to 20. Betrand, Duflo, and Mullainathan (2004) and Angrist & Pischke (2009) argue that between 42 and 50 clusters are required to obtain consistent estimates of standard errors that account for clustering. But Bertrand and her colleagues and Cameron, Gelbach, and Miller (2008) actually show that the rate of rejecting the null hypothesis when it is true is smaller when there are 20 clusters than when there are 50. That suggests that 20 clusters, rather than 42 or 50,

[16] Gross enrollment rate in grades 6-8 is defined as number of students enrolled in grades 6-8 in 1996 in region (or sub-region) of childhood divided by the total population of children aged 11-13 in the region (sub-region). In additional work (not presented, but available upon request), we also did the analysis with P_{j1996} defined as primary school (1-8 grades) enrollment rate and the estimates were similar to those reported.

are sufficient. In our case, the disadvantage of the sub-regional model is that the estimates are based on smaller sample sizes in the geographical units than in the regional model.

Mother's Education and Child Mortality

To study the effect of mother's schooling on child mortality, we adopt an equation similar to (2), with two modifications: one, now the dependent variable is a measure of child mortality[17] , and two, predicted value of schooling from equation (2) replaces the three-way interaction term $Teacher_{j\tau} * Young * Yr2008$. The second stage regression has all the other controls of the first stage and thus the identification of the coefficient for predicted schooling depends entirely on the exclusion of the interaction term from the second-stage regression. As long as primary school teacher-child ratio ($Teacher_{j\tau}$) does not affect child health except through its effect on schooling, the IV estimate will provide the causal effect of schooling on child mortality. We estimate the structural form equations using ordinary least squares and two-stage least squares models. Throughout, standard errors are obtained by clustering on region of residence in the region level analysis (Huber 1967). In the sub-regional analysis, standard errors are obtained by clustering on sub-region of residence.

Education, Age at Marriage and First Birth, Fertility Behaviors and Gender Equality

Our second objective is to study the effect of women's education on their age at marriage, fertility and attitudes towards gender equality. A methodology similar to that discussed for child mortality outcomes is applied here. We study the following outcomes: (i) age at first marriage and age at first birth, (ii) fertility behavior such as number of pregnancies, number of children

[17] We have used the term child mortality for convenience. The actual measures are number of deceased children per ever married women with at least one child birth.

born, knowledge and utilization of family planning methods, and (iii) attitudes towards gender equality. Our research methodology does not allow us to test if education affected child mortality through any of these channels e.g. via its impact on age at first marriage, age at child birth, knowledge and use of family planning methods or attitudes towards gender equality. However, we can, and do, test if education influenced any of the aforementioned outcomes and draw inferences about the possible existence of these channels. For instance, if mother's education did not alter attitudes towards gender equality that would be evidence that changes in child outcomes that we find could not be due to changes in attitudes towards gender equality. Or if we did not find that education affected use of contraceptives, it would be unlikely that education affected the health outcomes of children, in our data, via improvement in use of contraceptives.

Results

Effect of Compulsory Education Reform on Schooling

We begin the analysis by examining the descriptive data on the educational outcomes of the younger (aged 18-22) and older (aged 23-29) cohorts of the ever married sample of women in 2003 and 2008 (Table 1). In 2008, 34% of the older cohort (aged 23-29) had at least eight years of schooling. The members of the older cohort were born in 1985 or earlier and therefore they did not have to comply with the law on mandatory eight years of schooling. Among the younger cohort (aged 18-22), who had to comply with mandatory increase in schooling, 53% had eight years of schooling in 2008. The 19 percentage-points difference in the schooling variable for the two cohorts could be due to the Compulsory Schooling Law or due to other factors that caused the educational attainment of the younger and older cohorts to be different.

For a rough and crude estimate of the effect of these other factors, we compare the educational attainment of similar cohorts in the pre-policy period: in 2003, 27% of the younger cohort and 32% of the older cohort had eight years of schooling. Assuming that in the absence of the compulsory schooling law, the gap in the education outcome of the younger and older cohorts in 2008 would have been the same as in 2003, we subtract the difference in the schooling outcome between the younger and older cohorts in 2003 from the difference for the two groups in 2008 to arrive at a crude difference-in-difference estimate of the effect of the Compulsory Education Law on the education outcome of the younger cohort. Our crude difference-in-difference estimate is a 24 percentage-point increase in the proportion of ever married mothers with eight years of schooling. Table 1 also computes the difference-in-difference estimate of years of schooling, which is 0.83 years or a 15% increase over the mean for the younger group in 2003, the pre-policy period.

These crude difference-in-difference estimates assume that any increase in years of schooling of the younger cohort over-and-above the difference between the younger and older cohorts in 2003 is on account of the Compulsory Education Law. This may not be true. Indeed, there were social and economic trends correlated with the reform that may have caused increased schooling of the younger cohort. In the regression analysis that follows we control for these factors by estimating the effect of the intensity of the education reform, which differed across regions (and sub-regions) and years as specified in equation (2) and include a complete set of cohort-of-childhood dummy variables and age effects. These estimates are presented in Table 2.

Panel 1 presents the analysis at the regional level and panel 2 at the sub-regional level. The intensity of the policy is measured by the number of teachers as a proportion to the number of children aged 6-13 (or teacher-child ratio) by region in panel 1 and by sub-region in panel 2.

All models, in panel 1, control for respondent's age (a dummy variable for each year of age), region of residence in childhood, and cohort of childhood effects, cohort of childhood interacted with the gross enrollment rate in grades 6-8 in the region of residence in childhood in1996, year of observation, teacher-child ratio at age 11, the interaction of the Young dummy variable with teacher-child ratio at 11, and the triple interaction term of the Young dummy variable, teacher-child ratio and the dummy variable for year 2008. Further controls are added sequentially in Models 2-4: Model 2 includes additional controls for the education of respondents' parents, Model 3 adds respondent's mother tongue, and Model 4 adds mother tongue and a variable for whether the respondent lived in a rural area during childhood, in addition to the controls in Model 2. Panel 2 has all the controls of panel 1, but the geographic variables and controls are at the sub-regional level. Standard errors in parenthesis adjust for clustering by region where the respondent lived in childhood in panel 1 and by the sub-region of residence in childhood in the panel 2.

Estimates in Table 2, model 1 suggest that a 1 percentage point increase in teacher-to child ratio resulted in a 16% percentage-point increase in the proportion of younger ever married women with eight or more years of schooling. Additional controls in Models 2-4 yield similar results: a 1 percentage point increase in teacher-to-child ratio raised the proportion with at least eight years of schooling by 17 percentage points (Model 4, panel 1). In the first five years after the policy reform, the period relevant for this study, nationally the average teacher-child ratio increased by 0.8 percentage points. Thus, on average, the Compulsory Schooling Law raised the proportion of ever married women with at least eight years of schooling by 14 (=17 multiplied with 0.8) percentage points. This is a 41% increase over the mean for the younger cohort in 2003, before the Compulsory Education law was passed. The partial F-stat for the three-way

interaction term (instrumental variable) is 14 for Model 4 (panel 1). Estimates in the bottom panel are similar, but somewhat less precisely estimated. Here the F-stat for the three-way interaction term is 8.53 (Model 4, panel 2). Estimates in Panel 2 (model 4) indicate that the Compulsory Schooling law raised the proportion of ever married women with at least eight years of schooling by 11 percentage points (=13.3 multiplied by 0.8).

Table 2 also presents the first stage estimates with years of schooling as the dependent variable. Estimated effects are positive and mostly statistically significant. These estimates suggest that a 1 percentage point increase in teacher-child ratio resulted in an increase in schooling by approximately 0.7 to 0.8 years. The partial F-stat for the instrumental variable for Model 4 is 7. Comparing the results from Tables 1 and 2 reveals that the simple difference-in-difference estimate that does not control for national trends in schooling over-estimates the effect of the Compulsory Schooling Law on both the schooling outcomes. Further, it appears from the F-stat estimates that we have a much better chance of detecting the effect of the dichotomous variable on eight years of schooling than the effect of the continuous variable on years of schooling on the second stage outcomes. Therefore, henceforth the analysis is restricted to estimating the effect of schooling measured as a dichotomous variable.

Next, we estimate equation 2 with 1 modification: the three way interaction term between Young, teacher-child ratio and year 2008 is replaced by 12 different interactions replacing Young with a dummy variable indicating the age of the respondent for the 12 age categories. In Figures 3a and 3b, we plot the estimated coefficients from the 12 three-way interactions. The point estimates are close to 0 for women ages 23-29 and turn positive and statistically significant for women ages 18-22. These results provide empirical justification for our construction of the treatment (Young) and comparison (Older) groups.

To test the validity of the instrument, following Duflo (2001) and Chou et al. (2010), we run regressions using equation (2) on a set of dichotomous dependent variables defined as equal to 1 if the respondent has at least "m" years of schooling, otherwise 0. In all, we run Model 4 in Table 2 for 16 different outcomes allowing "m" to vary from 1 to 16. The sample of analysis is ever married women aged 18-29, the same as in Tables 1 and 2. Figure 4a presents the point estimates with regional level analysis and Figure 4b presents point estimates with analysis at the sub-regional level.

The point estimates and the 95th confidence intervals of the three way interaction term, displayed in Figures 4a and 4b, show a sharp increase in the proportion of ever married women with six, seven, and eight years of schooling. As expected, the Compulsory Schooling Law has no effect on the proportion of women with five or fewer years of schooling; estimated coefficients for dependent variables with 9 or more years of schooling to 16 or more years of schooling are also modest and statistically insignificant. This is evidence that our instrumental variable, teacher-child ratio, is not capturing the effect of factors that raised education at all levels. If that were the case, the estimated coefficients for middle to secondary education or 1-5 years of schooling would have been positive and statistically significant. That the estimated effects on higher and lower levels of education are modest and statistically insignificant provides some evidence that our instrumental variable is valid.

Effect of Compulsory Education Reform on Fertility and Child Mortality

Table 3 presents the ordinary least squares (OLS) and the two stage least squares (IV) estimates of the effect of mother's education on fertility and child mortality and the reduced form estimates of the effect of the teacher-child ratio on these outcomes. Estimates in panel 1 are

based on region level analysis and estimates in panel 2 are from sub-regional analysis. The OLS estimates show that ever married women aged 18 to 22 with at least eight years of schooling had 0.5 fewer pregnancies, 0.4 fewer children, and 0.03 fewer deceased children (under age 5) than women with less than eight years of schooling. Further, eight years of schooling is also associated with a reduced risk of child mortality based on the following measures: number of deceased children in the first month after birth, number of deceased children during 1-12 months after birth, and before age 1.

The IV estimates are larger (in absolute levels) than the OLS estimates and suggest that a 10 percentage point increase in proportion with eight years of schooling lowers number of pregnancies per ever-married woman by approximately 0.13 and number of children by between 0.11 and 0.16. Further, it lowers number of deceased children before age 5 by 0.02 (24% decline over the mean of 0.07) and most of the decline is in the first month after child-birth - however, for these measures the estimates are statistically insignificant in the sub-region-level analysis. Thus, it is likely that our analyses with fewer clusters (12 clusters in the regional analysis) under-estimate the standard errors and our sub-regional analysis (with 20 clusters) is more reliable (Angrisr & Pischke, 2009; Cameron, Gelbach, & Miller, 2008). Reduced form estimates also lead to the same conclusion that increased schooling resulting from the Compulsory Education Law caused a decline in fertility and lowered child mortality.

Table 3 also reports the Wooldridge test statistic for the test of endogeneity. We report the Wooldridge test statistic, instead of the Wu-Hausman test, as the former is recommended for TSLS estimates with the cluster robust option for computing standard errors. The Wooldridge test statistic is often statistically significant for the region-level analysis suggesting that the OLS

estimator of the same equation would yield inconsistent estimates and that the instrumental variables technique is required.

The combined evidence from the regional and sub-regional level analyses thus suggests that a 10 percentage-points increase in the proportion of ever married women with eight years of schooling lowered number of pregnancies per ever-married woman by approximately 0.13 and number of children per ever-married woman by between 0.11 to 0.16, and there is some weak evidence that mother's education lowered child mortality.

Effect of Compulsory Education Reform on Age at First Marriage, Age at First Birth, and Fertility Behaviors

Next, we investigate the effect of women's schooling on age at first marriage and age at first birth. Models similar to those in Table 3 are applied and the results are presented in Table 4. The OLS estimates show that a 10 percentage points increase in the proportion of women with eight years of schooling raised age at first marriage and age at first birth by about 2 months (0.15*12). The IV estimates are similar sized and statistically significant in panel 1 but statistically insignificant in panel 2.

The last two columns of Table 4 show the effect of women's education on use of family planning methods and knowledge of their ovulation cycle. The two outcomes we study are: whether the respondent uses modern family planning methods and whether she understands her ovulation cycle. In 2003, only 27% of the young ever-married women aged 18-22 used modern family planning methods and an even smaller proportion, 18%, had knowledge of their ovulation cycle. Thus, one potential channel through which education could affect fertility and child mortality is via educating women on family planning.

The OLS estimates show that a 10% increase in the proportion of women with eight years of schooling raised the proportion that used modern family planning methods by 0.5% and of those with knowledge of their ovulation cycle by 1.6%. The IV estimates are much larger and suggest that a 10% increase in the proportion with eight years of schooling raised the proportion that used modern family planning methods by eight to nine percent and those with the knowledge of their ovulation cycle by five to seven percent. Reduced form estimates are also large and statistically significant. The Wooldridge test statistic is statistically insignificant except for the last outcome (dichotomous variable indicating whether the respondent has knowledge of the ovulation cycle) indicating the OLS estimate is consistent for the other three outcomes, but the IV technique is required for the last measure.

Effect of Compulsory Education Reform on Women's Attitudes towards Gender Equality

Our final objective is to study if increase in education resulting from the Compulsory Education Law changed women's attitudes towards gender equality. Models similar to those in Tables 3 and 4 are applied and results are presented in Table 5. The OLS estimates show that more educated women are more likely to believe in women's equality on all measures. However, the IV estimates are all statistically insignificant. The Wooldridge test statistic is statistically insignificant except for the last outcome (dichotomous variable indicating that the respondent disagrees that wife beating is justified if she refuses sex indicating the OLS estimate is consistent for most outcomes.

The reduced form estimates are also modest and statistically insignificant. This suggests that increases in schooling did not change women's attitudes towards gender equality in any substantial manner during our study period. From this we infer that the decline in fertility and

child mortality observed in Table 3 could not be on account of a change in women's attitudes towards gender inequality. On the other hand, estimates in Table 4 suggest an increase in women's education caused an increase in age at marriage and age at first birth. We also find that increased schooling improved women's use of modern contraceptive methods as well as knowledge of their ovulation cycle. Thus it is more likely that the decline in fertility and child mortality that we observe are on account of these factors along with increase in age at first marriage and the related age at first childbirth.

Conclusion

In this paper, we have used a natural experiment in Turkey to study the effect of education on women's fertility, empowerment, and their children's health. In 1997, Turkey passed the Compulsory Education Law that increased compulsory formal schooling from five to eight years. Individuals born after 1985 (who were 11 or less in 1997) were the target of the law. To accommodate the expected increase in enrollment, the government devoted additional resources on school infrastructure and in hiring new teachers and these investments varied across the sub-regions (and regions) of Turkey. We use the number of primary school teachers per child (aged 6-13) in the sub-region (or region) of childhood at age 11 as an instrumental variable to predict the educational attainment of young women. The predicted education variable is then used to estimate the causal effect of education on a range of outcomes relating to child mortality, women's reproductive health, and measures of empowerment, including age at first marriage, age at first child birth, use of contraceptives, fertility outcomes, and attitudes towards gender equality.

First stage regressions, from the sub-regional analysis, show that the Compulsory Schooling Law raised the proportion of ever married women with at least eight years of schooling by 11 percentage points, which is a 32% increase over the mean in the pre-education reform period.

Estimates from the instrumental variables analysis suggest that a 10 percentage point increase in proportion of ever married women with eight years of schooling lowered number of pregnancies per ever-married woman by approximately 0.13 and number of children by 0.11. There is also some evidence of a decline in child mortality, caused by mother's education, but estimated effects turn statistically insignificant in our preferred models (sub-regional analysis). Further, our analysis shows that a 10 percentage-points increase in the proportion with eight years of schooling raised the proportion of women using modern family planning methods by eight to nine percent and the proportion of women with knowledge of their ovulation cycle by five to seven percent. However, we find little evidence that schooling changed women's attitudes towards gender equality.

From this we infer that the decline in fertility and child mortality (in some models) that we observe could not be on account of changes in women's attitudes towards gender inequality resulting from increased education. It is more likely that the decline in fertility and child mortality that we observe are on account increases in age at first marriage, age at first childbirth or increased use of contraceptive methods and improvements in women's understanding of their ovulation cycle. It might be that attitudes are slow to change and that the content of education in classrooms is an important factor when it comes to changing attitudes.

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Figure 1a:Number of Primary School (grade 1-8) Teachers

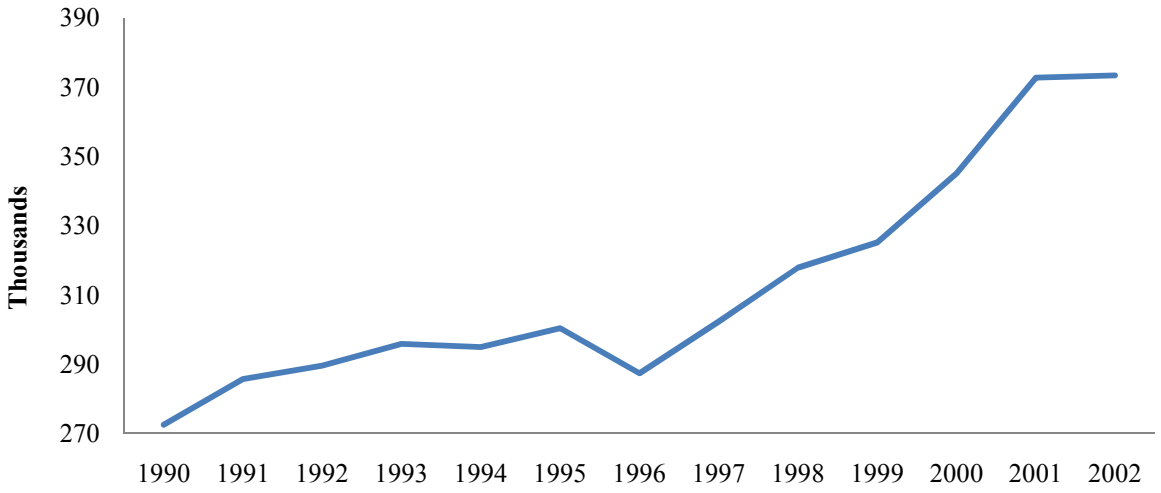

Figure 1b: Primary School Teachers per Child
(Number of grade 1-8 teachers divided by number of children aged 6-13)

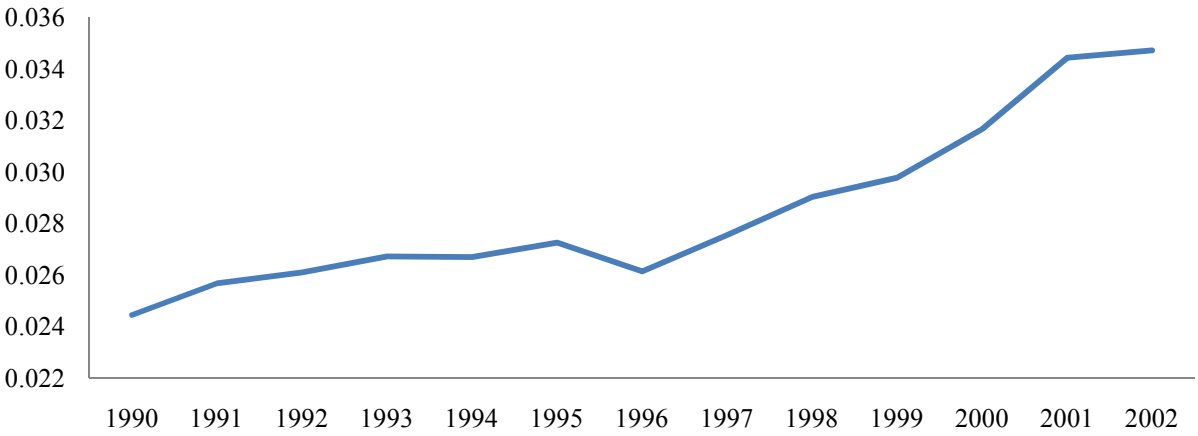

Figure 2: Gross Enrollment Rate in Grades 6-8
(Number of students enrolled in grades 6-8 divided by population of children aged 11-13)

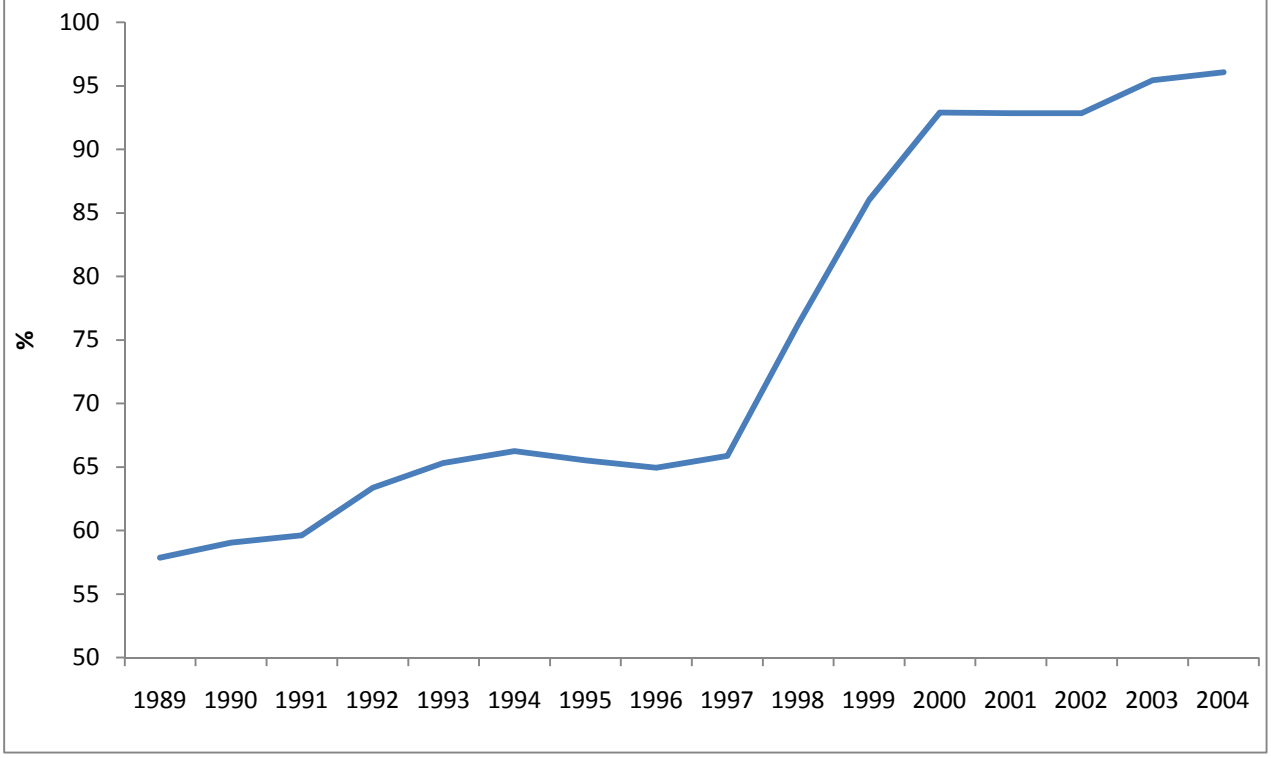

Figure 3a: Coefficients of the Interactions between Age, Program Intensity in the Region of Childhood at age 11, and DHS 2008 (Analysis based on 12-regions of residence in childhood)

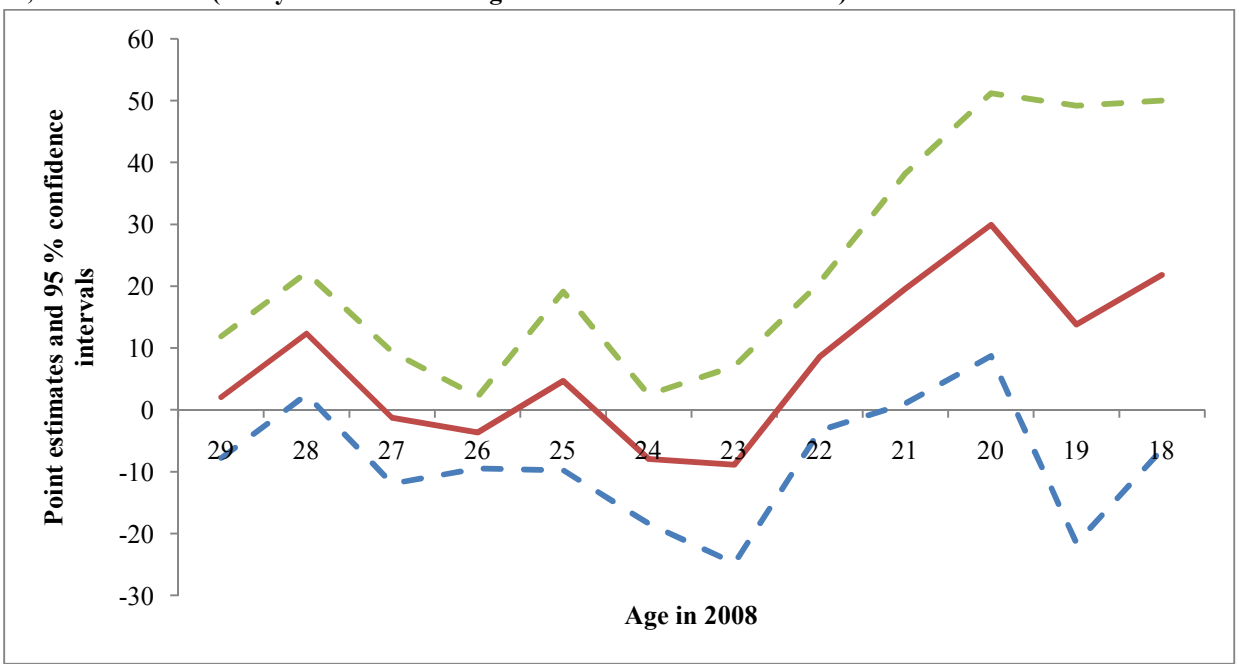

Figure 3b: Coefficients of the Interactions between Age, Program Intensity in the Region of Childhood at Age 11, and DHS 2008 (Analysis based on 20 sub-regions of residence in childhood)

Figure 4a: Association between the Probability of Completing at least "m" years of Schooling and Turkey's 1997 Compulsory Education Law
(Analysis based on 12-regions of residence in childhood)

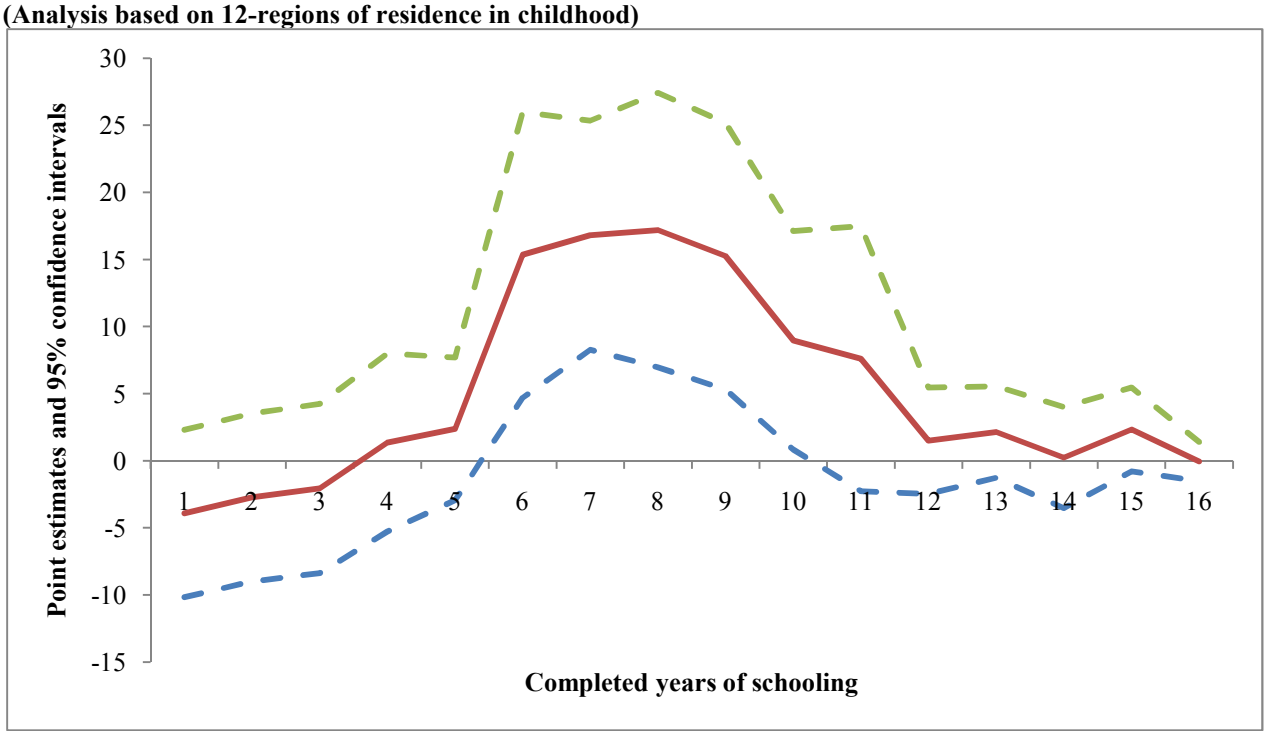

Figure 4b: Association between the Probability of Completing at least "m" years of Schooling and Turkey's 1997 Compulsory Education Law
(Analysis based on 20 sub-regions of residence in childhood)

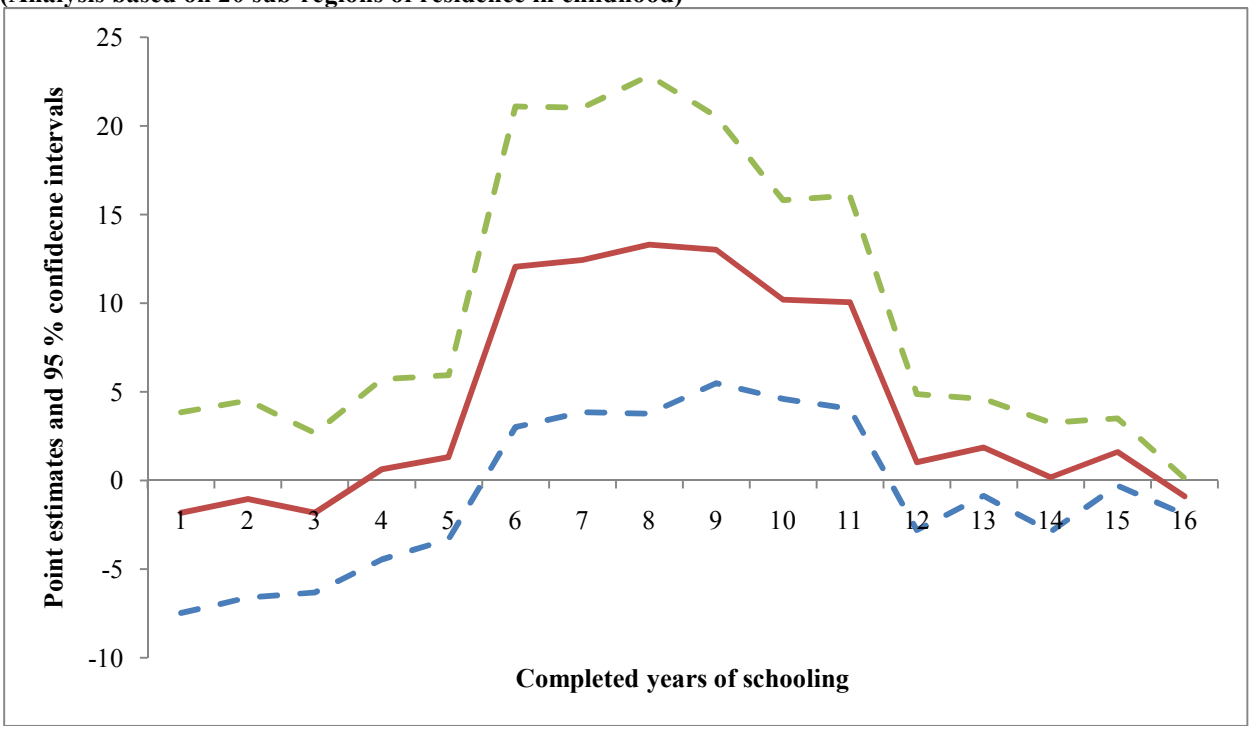

Table 1. Difference-in-differences Estimates of the Association between Compulsory Education Law and Schooling of Ever Married Mothers

	2008			2003			Difference-in-difference
	Young	Older	Difference 1	Young	Older	Difference 2	
At least eight years of schooling	0.53	0.34	0.19***	0.27	0.32	-0.05**	0.24***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)
Years of schooling	6.70	6.68	0.02	5.65	6.46	-0.81***	0.83***
	(0.16)	(0.09)	(0.18)	(0.13)	(0.08)	(0.16)	(0.24)
N	611	1794	2405	795	2033	2828	5233

Note: The sample of analysis is ever married women aged 18-29 in the 2003 and 2008 Turkey Demographic and Health Surveys. Women aged 18-22 are defined as Young and women aged 23-29 are defined as Older. *$0.05 < p \leq 0.1$, **$0.01 < p \leq 0.05$, ***$p \leq 0.01$.

Table 2: Estimates of the Effect of Compulsory years of Schooling on Ever Married Women's Education

	Schooling= Eight years or more				Years of schooling			
Panel 1:Analysis at the level of 12 regions	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Young*Teachers to child ratio (at age 11)* Year 2008	16.33**	16.68***	17.62***	17.19***	62.88	70.47*	85.52**	82.36**
	(5.96)	(4.80)	(4.80)	(4.65)	(41.42)	(33.86)	(31.74)	(31.08)
F-ratio	7.51	12.08	13.48	13.67	2.30	4.33	7.26	7.02
Panel 2: Analysis at the level of 20 sub-regions								
Young*Teachers to child ratio (at age 11)* Year 2008	15.63***	12.81***	13.83***	13.30***	82.93**	59.10**	75.50***	71.74**
	(4.22)	(4.37)	(4.39)	(4.55)	(30.66)	(27.39)	(26.30)	(26.82)
F-ratio	13.69	8.58	9.92	8.53	7.29	4.67	8.24	7.13
Mean of the dependent variable	0.34	0.34	0.34	0.34	6.44	6.44	6.44	6.44
Mean of teachers child ratio (at age 11)	0.026	0.026	0.026	0.026	0.026	0.026	0.026	0.026
Model controls for :								
Mother's and father's education	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Mother tongue	No	No	Yes	Yes	No	No	Yes	Yes
Childhood place of residence (rural/urban)	No	No	No	Yes	No	No	No	Yes
N	5,161	5,149	5,149	5,147	5,161	5,149	5,149	5,147

Notes: Each cell in panels 1 and 2 is based on a separate regression with respondent's schooling outcome listed in the column heading as the dependent variable. The sample of analysis is ever married women aged 18-29 in the 2003 and 2008 TDHS. Women aged 18-22 are defined as Young. Standard errors clustered on the region of childhood residence are in parenthesis in Panel 1 and on the sub-region of childhood residence are in parenthesis in Panel 2. F-ratios are F-statistics of the test of significance of the excluded instrument (the three-way interaction terms listed in table). In addition to the controls listed in the table, each regression controls for respondent's age (a dummy variable for each year of age), respondent's year of birth, teachers to child ratio at age 11, region of residence at age 11, year of childhood (age 11) interacted with 6-8 grade gross enrollment rate in 1996 in the region of childhood, and an interaction of the Young dummy variable with teachers to child ratio in region of residence at age 11. Regressions in Panel 2 are the same as in Panel 1 with one difference: all geographic variables (and controls) are measured at the sub-regional level. Teachers to child ratio is measured by region of childhood in panel 1 and by sub-region of childhood in panel 2. Means of the teachers-child ratio at regional and sub-regional levels are the same at 3 decimal digits.
*$0.05 < p \leq 0.1$, **$0.01 < p \leq 0.05$, ***$p \leq 0.01$.

Table 3: Estimates of the Effect of Mother's Education on Fertility and Child Mortality

	Number of pregnancies	Number of children born	Number of children deceased before age 1	Number of children deceased before age 5	Number of children deceased in 1st month after birth	Number of children deceased in 1-12 months after birth
Panel 1: Analysis at the level of 12 regions						
OLS Estimates						
Schooling=8 years or more	-0.52***	-0.43***	-0.03**	-0.03**	-0.01	-0.01***
	(0.05)	(0.03)	(0.01)	(0.01)	(0.01)	(0.00)
Reduced form estimates						
Young*Teachers to child	-23.25***	-27.13***	-2.02	-3.16**	-2.24**	0.67
ratio (at age 11)* Year 2008	(5.46)	(6.35)	(1.15)	(1.35)	(0.83)	(0.90)
IV estimates						
Schooling=8 years or more	-1.35***	-1.58***	-0.11	-0.17*	-0.12*	0.04
	(0.46)	(0.50)	(0.07)	(0.10)	(0.07)	(0.06)
Wooldridge test statistic	5.92**	9.84***	1.25	2.82	3.81*	1.14
Panel 2: Analysis at the level of 20 sub-regions						
OLS Estimates						
Schooling=8 years or more	-0.51***	-0.43***	-0.03*	-0.03*	-0.01	-0.01***
	(0.04)	(0.04)	(0.01)	(0.02)	(0.01)	(0.00)
Reduced form estimates						
Young*Teachers to child	-17.28**	-14.61*	-1.92	-3.07	-1.91*	0.47
ratio (at age 11)* Year 2008	(7.45)	(7.57)	(1.70)	(2.00)	(0.91)	(1.19)
IV estimates						
Schooling=8 years or more	-1.30**	-1.10**	-0.13	-0.21	-0.13	0.03
	(0.63)	(0.50)	(0.12)	(0.16)	(0.09)	(0.08)
Wooldridge test statistic	1.96	1.61	0.77	1.75	3.52*	0.33
Mean of the dependent variable	1.81	1.52	0.06	0.07	0.03	0.02
N	5,147	5,147	4,228	4,228	4,228	4,228

Notes: Figures in each cell of Panels 1 and 2 are based on separate regressions with column heading as the dependent variable. Standard errors clustered on the region of childhood residence are in parenthesis in Panel 1 and on the sub-region of childhood residence are in parenthesis in Panel 2. The sample of analysis is ever married women aged 18-29 in the 2003 and 2008 TDHS. Women aged 18-22 are defined as Young. All regressions in panel 1 control for respondent's age (a dummy variable for each year of age), respondent's year of birth, region of residence at age 11, parents' education, respondent's mother tongue and place of residence (urban/rural) at age 11, year of childhood interacted with 6-8 grade gross enrollment in the region of residence in childhood in1996, teachers-to-child ratio, and an interaction of the Young dummy variable with teachers to child ratio. The instrumental variables in the IV estimates is the three way interaction between Young*Teachers to child ratio (at age 11)* Year 2008. Regressions in Panel 2 are the same as in Panel 1 with one difference: all geographic variables (and controls) are measured at the sub-regional level. *$0.05 < p \leq 0.1$, **$0.01 < p \leq 0.05$, ***$p \leq 0.01$.

Table 4: Estimates of the Effect of Mother's Education on Age at First Marriage, Age at First birth and Knowledge/ Use of Family Planning Methods

	Age at first marriage	Age at first birth	Use of modern family planning methods	Knowledge of ovulation cycle
Panel 1: Analysis at the level of 12 regions				
OLS Estimates				
Schooling=8 years or more	1.49***	1.40***	0.05**	0.16***
	(0.11)	(0.11)	(0.02)	(0.02)
Reduced form estimates				
Young*Teachers to child	26.90	29.35**	13.92*	8.39**
ratio (at age 11)* Year 2008	(17.41)	(12.72)	(7.45)	(2.97)
IV estimates				
Schooling=8 years or more	1.56**	1.61***	0.81**	0.48**
	(0.75)	(0.59)	(0.36)	(0.20)
Wooldridge test statistic	0.01	0.13	3.19	3.22
Panel 2: Analysis at the level of 20 sub- regions				
OLS Estimates				
Schooling=8 years or more	1.49***	1.40***	0.05***	0.16***
	(0.10)	(0.09)	(0.02)	(0.02)
Reduced form estimates				
Young*Teachers to child	27.53	19.72	12.53	9.51***
ratio (at age 11)* Year 2008	(19.13)	(17.94)	(7.40)	(2.85)
IV estimates				
Schooling=8 years or more	2.07	1.33	0.94*	0.71**
	(1.27)	(1.07)	(0.49)	(0.29)
Wooldridge test statistic	0.19	0.005	2.67	6.00**
Mean of the dependent variable	19.59	20.64	0.39	0.25
N	5,147	4,228	5,147	5,142

Notes: Figures in each cell of Panels 1 and 2 are based on separate regressions with column heading as the dependent variable. Standard errors clustered on the region of childhood residence are in parenthesis in Panel 1 and on the sub-region of childhood residence are in parenthesis in Panel 2. The sample of analysis is ever married women aged 18-29 in the 2003 and 2008 TDHS. Women aged 18-22 are defined as Young. All regressions in panel 1 control for respondent's age (a dummy variable for each year of age), respondent's year of birth, region of residence at age 11, parents' education, respondent's mother tongue and place of residence (urban/rural) at age 11, year of childhood interacted with 6-8 grade gross enrollment in the region of residence in childhood in1996, teachers-to-child ratio, and an interaction of the Young dummy variable with teachers to child ratio. The instrumental variables in the IV estimates is the three way interaction between Young*Teachers to child ratio (at age 11)* Year 2008. Regressions in Panel 2 are the same as in Panel 1 with one difference: all geographic variables (and controls) are measured at the sub-regional level. *$0.05 < p \leq 0.1$, **$0.01 < p \leq 0.05$, ***$p \leq 0.01$.

Table 5: Estimates of the Effect of Women's Education on Her Attitudes Towards Gender Equality

	Disagrees: men are wiser than women	Disagrees: boys education is more important than girls	Disagrees: men should make decisions	Disagrees: wife beating is justified if she wastes money	Disagrees: wife beating is justified if she neglects children	Disagrees: wife beating is justified if she argues with husband	Disagrees: wife beating is justified if she refuses sex
Panel 1: Analysis at the level of 12 regions							
OLS Estimates							
Schooling=8 years or more	0.15***	0.10***	0.16***	0.12***	0.10***	0.11***	0.06***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reduced form estimates							
Young*Teachers to child	1.82	4.65	5.33	0.86	5.28	4.34	-4.76
ratio (at age 11)* Year 2008	(5.77)	(5.42)	(5.82)	(3.58)	(4.42)	(2.78)	(2.88)
IV estimates							
Schooling=8 years or more	0.11	0.27	0.31	0.05	0.31	0.25	-0.28
	(0.31)	(0.26)	(0.31)	(0.19)	(0.23)	(0.16)	(0.22)
Wooldridge test statistic	0.01	0.33	0.20	0.13	0.74	0.75	4.53*
Panel 2: Analysis at the level of 20 sub- regions							
OLS Estimates							
Schooling=8 years or more	0.15***	0.10***	0.16***	0.11***	0.09***	0.11***	0.06***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reduced form estimates							
Young*Teachers to child	3.35	6.07	5.63	-1.95	0.23	3.01	-2.85
ratio (at age 11)* Year 2008	(5.56)	(4.24)	(5.12)	(4.58)	(3.44)	(2.46)	(2.03)
IV estimates							
Schooling=8 years or more	0.25	0.46	0.42	-0.15	0.02	0.23	-0.21
	(0.37)	(0.30)	(0.37)	(0.36)	(0.25)	(0.17)	(0.20)
Wooldridge test statistic	0.07	1.33	0.48	0.63	0.10	0.43	3.66*
Mean of the dependent variable	0.81	0.86	0.76	0.79	0.81	0.78	0.90
N	5,139	5,141	5,142	5,142	5,142	5,140	5,140

Notes: Figures in each cell of Panels 1 and 2 are based on separate regressions with column heading as the dependent variable. Standard errors clustered on the region of childhood residence are in parenthesis in Panel 1 and on the sub-region of childhood residence are in parenthesis in Panel 2. The sample of analysis is ever married women aged 18-29 in the 2003 and 2008 TDHS. Women aged 18-22 are defined as Young. All regressions in panel 1 control for respondent's age (a dummy variable for each year of age), respondent's year of birth, region of residence at age 11, parents' education, respondent's mother tongue and place of residence (urban/rural) at age 11, year of childhood interacted with 6-8 grade gross enrollment in the region of residence in childhood in1996, teachers-to-child ratio, and an interaction of the Young dummy variable with teachers to child ratio. The instrumental variables in the IV estimates is the three way interaction between Young*Teachers to child ratio (at age 11)* Year 2008. Regressions in Panel 2 are the same as in Panel 1 with one difference: all geographic variables (and controls) are measured at the sub-regional level. *$0.05 < p \leq 0.1$, **$0.01 < p \leq 0.05$, ***$p \leq 0.01$.

Appendix Table 1: Descriptive Statistics

	TDHS 2003				TDHS 2008			
	Older cohort Age=23-29		Younger cohort Age=18-22		Older cohort Age=23-29		Younger cohort Age=18-22	
	N	Mean	N.	Mean	N.	Mean	N	Mean
Age at first marriage	2033	19.81	795	18.19	1794	20.40	611	18.21
Age at first birth	1805	20.97	540	18.96	1550	21.25	402	18.98
Number of pregnancies	2033	2.17	795	1.17	1794	1.97	611	0.98
Number of children born	2033	1.81	795	0.99	1794	1.64	611	0.84
Number of children deceased before age 1	1805	0.07	540	0.06	1550	0.05	402	0.01
Number of children deceased before age 5	1805	0.08	540	0.07	1550	0.06	402	0.01
Number of children deceased in first month after birth	1805	0.04	540	0.03	1550	0.03	402	0.01
Number of children deceased in 1-12 months after birth	1805	0.03	540	0.03	1550	0.02	402	0.00
Use of modern family planning methods	2033	0.43	795	0.27	1794	0.44	611	0.30
Knowledge of ovulatory cycle	2032	0.29	795	0.18	1791	0.27	609	0.18
Disagrees: men are wiser than women	2030	0.78	795	0.74	1790	0.86	610	0.85
Disagrees: boys education is more important than girls	2030	0.85	795	0.80	1792	0.91	610	0.87
Disagrees: men should make decisions	2030	0.72	795	0.60	1793	0.85	610	0.81
Disagrees: wife beating is justified if she wastes money	2030	0.76	795	0.67	1793	0.88	610	0.83
Disagrees: wife beating is justified if she neglects children	2030	0.78	795	0.70	1793	0.87	610	0.86
Disagrees: wife beating is justified if she argues with husband	2029	0.73	795	0.60	1793	0.89	609	0.86
Disagrees: wife beating is justified if she refuses sex	2030	0.86	795	0.82	1791	0.95	610	0.95