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Does Maternal Education Affect Childhood Immunization Rates? Evidence from Turkey

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Abstract

We study the causal effect of maternal education on childhood immunization rates. We use the Compulsory Education Law (CEL) of 1997, and the differentiation in its implementation across regions, as instruments for schooling of young mothers in Turkey. The CEL increased the compulsory years of schooling of those born after 1986 from 5 to 8 years. We find that education of mothers increases the probability of completing the full course of DPT and Hepatitis B vaccinations for their children. Furthermore, education increases the age of first marriage and birth, changes women`s and their spouse's labour market status, and significantly affects women`s attitude towards spousal violence against women and gender discrimination in a manner that empowers women.

Keywords: DPT (diphtheria, pertussis and tetanus), Hepatitis B, Maternal Education, Autonomy of Women, Fertility, Difference-in-Difference-in-Differences, Instrumental Variable

JEL Codes: H51; H52; I12

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1. Introduction

Vaccination can save millions of lives. According to a report by the World Health Organization (WHO) and the United Nations Children's Funds (UNICEF)[1], some 8 million children under five die annually, 17 percent of those deaths could have been prevented if the children were vaccinated. The same report estimates that two to three million possible deaths from measles and diphtheria, pertussis (whooping cough) and tetanus (DPT) are prevented by vaccination each year. Children aged under 6 are most likely to perish or to develop chronic conditions when they fall ill. These deaths and complications could be easily prevented by timely vaccination. The potential gains from increased vaccination coverage are particularly large in less developed countries, where vaccination rates remain relatively low, especially in rural areas.

In Turkey, the percentage of children who are fully vaccinated increased from 46 percent in 1998 to 81 percent in 2008 (Table 1). The likelihood that a child will be vaccinated is closely correlated with the mother's level of education.

Table 1 Trend of full vaccination in Turkey

Mother's education	1998	2003	2008
No education/Primary incomplete	28.5	26.1	64.9
Primary school/First level	48.0	60.9	81.6
Primary school/Second level	64.0	61.2	84.4
Secondary and higher school		68.5	87.8
Total	45.7	54.2	80.5

Source: Author`s own calculation based on TDHS-1998, TDHS-2003, TDHS-2008

The role of maternal education thus seems to be an important determinant of whether infants complete the required vaccinations, as has been highlighted also in the previous

[1] World Health Organization and UNICEF: Global Immunization Data; July 2014. Accessible at http://www.who.int/immunization/monitoring_surveillance/global_immunization_data.pdf [cited on 24/07/2015].

literature.[2] However, all of these studies ignore the likely endogeneity of maternal education due to omitted variables bias, and any estimation not correcting for it is likely to be biased. The instrumental variable method can be used to overcome the endogeneity of schooling when assessing the market and nonmarket returns of education including child health (e.g. Breierova & Duflo, 2004; Chou, Liu, Grossman, & Joyce, 2010; Currie & Moretti, 2003; Dinçer, Kaushal, & Grossman, 2014; Güneş, 2015). Changes in the length of compulsory education can be used as an instrument for the endogeneity of education.

Turkey expanded its compulsory education from 5 to 8 years in 1996-97: this reform affected children aged ten or less in 1996-97. The extension in compulsory education in turn generated an urgent need for the construction of new classrooms and employment of new teachers, which presented an additional demand on the government budget. For example, the reform increased the budget for primary school construction by 33 percent from 1996 to 1997 (see Figure 1). With this additional budget, 58 thousand new classrooms were opened between the academic years 1996-97 and 1997-98 (Güneş, 2015), an almost 30 percent increase in the overall number of classrooms. However, the additional expenditure was not distributed equally across the country. We can therefore exploit the differences in the additional budget spending for classroom construction across regions and across birth cohorts in an instrumental variable (IV) framework to estimate the effect of maternal schooling on children's vaccination rates.

We contribute to the literature in several respects. Firstly, to the best of our knowledge, this is the first study exploring the causal relationship between maternal education and children's vaccination rates. Secondly, education might have an impact on vaccination through different channels, and our understanding of these channels is limited. We

[2] See literature review in the next section.

explore several potential causal pathways affecting the childhood immunization: mother's age at first birth, her age at marriage, employment status of mother and father, household size, wealth, health knowledge, autonomy of women, and women`s attitude towards gender inequality and spousal violence against women.

The findings suggest that maternal education, measured by years of education and as completion of 8 years of education, significantly improves the take-up of the last doses of Hepatitis B and DPT immunisations, even after controlling for gender and birth order of the children. It is also found that education increases the age at marriage and at first birth, and has a statistically significant effect on women`s attitudes towards spouse violence against women and gender discrimination, and on the employment status of women and their husbands. While we do not explicitly test the impact of these various channels on vaccination outcomes, it is still possible to draw some inferences. In particular, it seems fair to suggest that the increase in the childhood immunization rates might be partly attributable to the changes in women`s age of first marriage and birth, women`s and their husbands` employment status, and women's attitudes towards domestic violence and gender inequality.

The next section discusses the previous literature. Section 3 gives a brief background to the 1996-1997 Compulsory Education Reform. Section 4 is outlines the data and empirical strategy used for this study. Section 5 presents the findings of the research. Finally, section 6 gives a summary of the findings.

2. Literature Review

Earlier studies show that maternal education is positively correlated with the mothers` wellbeing and the health of their children alike (see Adler & Newman, 2002; Frost, Forste, & Haas, 2005; Glewwe, 1999; Grossman, 2006; Mistry, Galal, & Lu, 2009;

Muthayya, 2009; Schultz, 2002; Vaahtera et al., 2001). A number of recent studies find maternal education to be positively associated with the complete-vaccination status of infants, even after controlling for various individual and community-level variables such as age of mother, income, ethnicity, socioeconomic status, parity, residence and religion (Abuya, Onsomu, Kimani, & Moore, 2011; Altınkaynak, Ertekin, Güraksın, & Kılıç, 2004; Fatiregun & Okoro, 2012; Schoeps et al., 2013; Singh, Haney, & Olorunsaiye, 2013; Streatfield, Singarimbun, & Diamond, 1990; Vikram, Vanneman, & Desai, 2012). This result is important because vaccination is accepted as the most cost-effective and efficient way to reduce child mortality and morbidity (Breiman et al., 2004; Maurice & Davey, 2009; Rainey et al., 2011).

Starting with knowledge as a potential channel, Streatfield et al. (1990) found, in Indonesian context, that mothers' knowledge regarding the benefits of vaccination was positively correlated with formal education. Children of more educated women, in turn, benefited slightly more from immunisation programs.

Previous research also suggests that formal education of mothers increases their age at first birth, their autonomy, and changes their attitude towards gender inequality and violence against women, which ultimately increases their well-being and that of their children (leading to better health outcomes and promoting longer years of schooling for their children) and, finally, decreases the chance of death at birth for themselves and their baby (UNICEF, 2006). Similarly, formal education has been found to increase women's decision-making autonomy (Babalola, 2009; Kritz and Makinwa-Adebusoye, 1999; Singh et al., 2013; Vikram et al., 2012. When the mother rather than the father is in control of the household budget, more resources tend to go towards family health, in particular, children's health (Thomas, 1990; Shroff et al., 2011; Mistry et al., 2009).

Education also has a host of other potential benefits besides the positive correlation with the take-up of vaccination. Education, along with family wealth and husband's education, determines women's socioeconomic status) which in turn improves child and maternal health significantly (Adler & Newman, 2002; Braveman, Cubbin, Egerter, Williams, & Pamuk, 2010). Behrman and Rosenzweig (2002) find that a woman's formal education is positively correlated with her chances of earning higher income, marrying a more educated husband, and having a husband with a higher income. Educated women are inclined to have a lower fertility preference because of the quality-quantity trade-off, and, therefore, usually have fewer children with a higher level of wellbeing and better health per child (Becker & Lewis, 1973).

Although the above mentioned studies find that improved maternal education is positively correlated with childhood vaccination rates, this relationship is not necessarily causal. None of these studies address the endogeneity of maternal education, and any estimation not correcting for it is likely to be biased. This bias can occur for a number of reasons. Firstly, both maternal education and vaccination take up might be driven by (household) income (Behrman & Rosenzweig, 2002). Secondly, the relationship between education and any outcome of interest may be distorted by the 'ability bias` (Griliches, 1977; Card, 1999). Behrman and Rosenzweig (2002) argue that a woman with higher ability not only would tend to complete more years of formal education, but also marry a more educated husband and raise healthier children. Thirdly, a reverse causality problem might also exist between education and fertility choice because a woman`s education is likely to be affected by the timing of her fertility choices such as timing and number of children (Angrist & Evans, 1998; Jensen & Thornton, 2003).

The research on the causal influences of education on child health, fertility choices, and the usage of prenatal health services frequently utilizes exogenous variation in availability and length of education. Currie and Moretti (2003) use the availability of two-year and four-year colleges in the US state where women lived at the age of 17 as an instrument for mothers' schooling.. They find that maternal education improves children's health and prenatal care use and decreases smoking and improves fertility. Lindeboom, Llena-Nozal, & van Der Klaauw (2009) use an extra year spent at school due to the compulsory schooling law change in the UK in 1947 as an instrument for paternal and maternal education. In contrast to Currie and Moretti (2003), their results indicate only a small impact of parents' education on various children's and parental health measures. Similarly, McCrary and Royer (2011) find limited impact of education on fertility and infant health in the US.

A number of papers follow a strategy similar to that used in current paper, whereby the exposure to education reform by date of birth and the differentiation in its implementation across regions serves as an instrument for schooling of mothers. Breierova and Duflo (2004) in Indonesia and Chou et al. (2010) in Taiwan use regional variation in the number of schools constructed as a measure of the intensity of the education reform. Both find a negative correlation between maternal education and fertility and child health (mortality rates). Osili and Long (2008) use the cross-state differences in government spending for primary school construction after the education reform in Nigeria in 1976. Their findings suggest that an additional year of female education decreases fertility. In the context of Turkey, Güneş (2015) exploits the variation in the number of classrooms constructed across regions, whereas Dinçer et al. (2014) use the variation in the number of teachers recruited after the change in compulsory education year in Turkey in 1997 as the measure of program intensity. They confirm a causal impact of maternal education on children's mortality, birth weight, height and weight for age, prenatal care utilisation.

Finally, the impact of education on the possible causal channels (e.g. health knowledge and empowerment of women) has also received attention. Dinçer et al. (2014) find that education increases women`s knowledge of the ovulation cycle and encourages women to reject spousal violence. Mocan and Connanier (2012) exploit the variation in program intensity across regions and in exposure to the education program by year of birth in Sierra Leone to find that increases in education of women improve women`s awareness of risky health behaviour and spouse violence against women in a manner that empowers women. Johnston, Lordan, Shields, and Suziedelyte (2015) use the UK`s compulsory education reforms, which changed the school dropout age from 14 to 15 in 1947 and 15 to 16 in 1972, to examine the effect of education on the knowledge regarding determinants of common health problems, such as stomach ulcers, migraine, stroke, depression, high blood pressure. Unlike Dinçer, they find that education does not have a significant impact on health knowledge variables considered. Samarakoon and Pariduri (2015) use exogenous variation in education due to a longer education year in Indonesia in 1978 to estimate the effects of education with a fuzzy regression discontinuity design. They find no causal relationship between women`s education and their decision-making power (except saving), ownership of the assets (except jewellery and household appliances) and participation in the community (except visiting community-weighing post).

3. 1997 Compulsory Education Reform in Turkey

The December 1995 election in Turkey resulted in the victory of a religious Welfare Party, which became the senior party in a coalition government in 1996. On 28

February 1997, the National Security Council, dominated by the military, forced the government to resign. The reason behind this was the government's religious orientation, which the military saw as a threat to democracy and secularism in the Turkish Republic. The same meeting of the National Security Council decided to increase compulsory education from 5 to 8 years, which was implemented by the Turkish Parliament in August 1997.

The increase in compulsory education was intended as an important step in the fight against the expansion of religious thinking because it enabled the closing down of the lower secondary (6-8 grades) parts of religious schools (which were not in compliance with the state curriculum). Additionally, university choice was restricted in technical and religious schools. For instance, a person who graduated from a religious school could only go to a university department related to religion.

As a result of the legislative change, the Turkish Government allocated a significant amount of additional resources for investment in school facilities, such as the construction of new classrooms and the employment of new teachers. To be precise, 81,500 new primary-school classrooms were constructed between 1997 and 2002 which corresponds to an almost 40 percent capacity increase (World Bank, 2005), and 70 thousand new teachers were employed (Dulger, 2004). Lastly, the government sought to equalise the enrolment rates between rural and urban areas. To this effect, it consolidated village schools, opened new boarding schools and used school buses to carry children from villages to city centres. All of this necessitated increasing the government's education budget in 1997. Overall, as shown in Figure 1, the reform increased the budget for primary school construction by 30 percent between 1996-97 and 1997-98.[3]

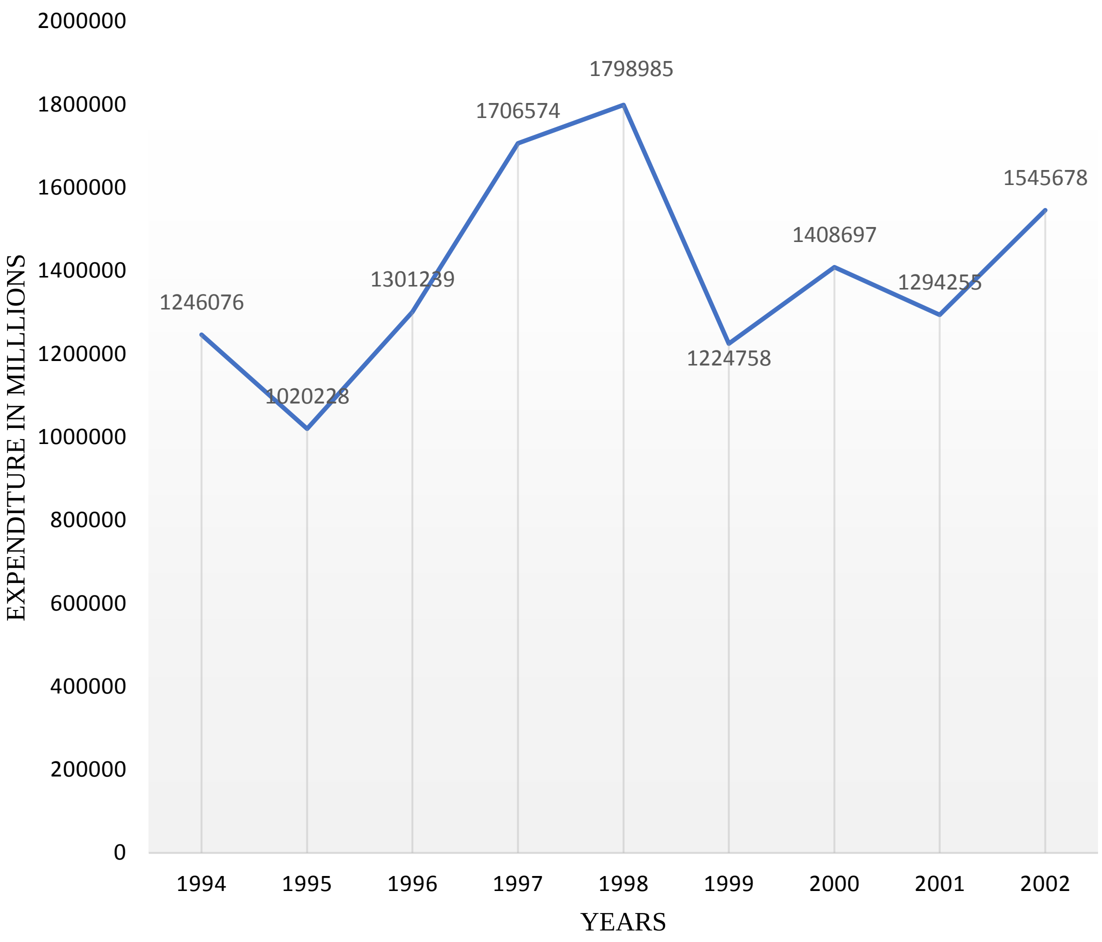
Education reform led to a substantial increase in overall enrolment: the number of students enrolled in grade six increased from 866 thousand in 1996-97 to 1.227 million in the 1997-98 academic year, a growth of more than 30 %. Moreover, in 1997-98, the enrolment rates for girls in the sixth grade in provinces with the highest gender inequalities, which are generally more conservative places, was 162 percent greater than in 1996-67. Finally, it is worth noting that the goal of CEL was to increase enrolment rates and there was almost no change in the curriculum or in other aspects of the quality of education (Dulger, 2004).

4. Data and Empirical Framework

The study’s data mainly come from the last two rounds of the Turkey Demographic and Health Survey (TDHS-2003 and TDHS-2008). The survey aims to provide information on trends and levels of fertility, infant and child mortality, family planning, maternal and child health including preventive health measures (e.g. the childhood vaccination status) of ever-married women.[4] The TDHS surveys also include a wide range of information on women’s socioeconomic characteristics, such as education (completed years of schooling and highest level of education attained), parents’ education, employment status, ethnicity, women’s status in the family etc. The survey also features demographic questions including age, gender, type of birth place (rural/urban), a region of birth, and the region of residence during childhood.

[3] This growth rate is based on the Ministry of Development's Investment Program statistics for 1996 and 1997, and is adjusted for inflation. For comparison, the overall public investment grew by 21 percent over the same period.
[4] The TDHS collects data on maternal health care utilisation from mothers who gave birth in the five years before the interview date. However, for the information regarding the vaccination status of their babies, mothers who gave birth in the three years before the interview date is used.

Figure 1. The Trend in the Public Expenditure on classroom construction in millions (1997 Prices) in Turkey



Source: Statistical Yearbook on Public Expenditure from 1994 to 2002, Turkish Ministry of Development. The years are academic, so that the 1996 figure corresponds to the 1996/97 academic year.

The women`s region of residence until the age of 12 allows us to identify the impact of the compulsory schooling reform on education of women, as it links the respondent's individual data with regional administrative data. Another two regional variables are instrumental for the analysis and the regional gross enrolment rates for junior high school.[5]

[5] The Turkish Statistical Institute divides Turkey into 26 sub-regions at Statistical Regional Classification Unit level (or level 2) (see Turkish Statistical Institute website: http://www.turkstat.gov.tr/Start.do). The

As childbearing outside of marriage is uncommon in Turkey, we only consider married women included in the TDHS. Our final sample consists of young mothers between the ages between 18 and 29 in the 2003 and 2008 TDHS. The mothers aged 18 to 21 form the treatment group and those aged 22 to 29 are the control group. After these adjustments, our final dataset has 3331 to 3382 observations.

4.1 Dependent variables

Two dichotomous variables are used to measure the completion of immunisation for children aged over six months, taking the value of one, if the children received the third and final dose of DPT (diphtheria, pertussis and tetanus) and Hepatitis B vaccines.[6]

In addition to the vaccination status of children, the channels potentially influencing the vaccination status of children by empowering women were measured using a number of dichotomous variables. Three variables measure the autonomy of women: (i) whether she is responsible for planning and controlling the household budget, (ii) whether she pays the bills and does other official work, and (iii) whether she could choose her husband with complete freedom or whether other family members selected her husband. If a woman responds that she performs such works, or freely chose her husband, she is considered autonomous for the purposes of this study.

Next, two indexes were constructed to measure women`s attitude towards gender norms. The first consisted of five survey questions on whether spousal violence against

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public expenditure figures are based on information from the Turkish Ministry of Development`s 1996 and 1997 statistics yearbooks. The Turkish Statistical Institute's 1990 and 2000 census statistics were used to estimate the population aged 6-13 in 1996 and 1997, with missing data estimated using the exponential function method. Gross enrolment rate in junior high school (JHS), i.e. grade 6-8, is calculated by dividing the number of children who are enrolled in JHS in 1996 in the childhood region of children by the population of children aged 11-13 in the same region and year. The number of JHS students, i.e. grades 6 to 8, was obtained from the Ministry of National Education’s National Education Statistics. The school-aged population, i.e. 11 to13 in 1996 was based on the censuses conducted by the Turkish Statistical Institute in 1990 and 2000.

[6] Both vaccines are administered during the first six months after birth. Therefore, we only consider women with children older than six months. The third dose completes the required course of these vaccinations.

women was justified if a woman: (i) wastes money, (ii) neglects children, (iii) refuses sex, (iv) burns food, and (v) argues with her husband. The index is constructed to range between 0 and 1, with 0.2 given for each affirmative answer to any of the aforementioned questions. Four questions are used to construct an index on gender inequality attitudes. These record the woman's opinion on the position of women within the household: (i) family decisions should be made by men, (ii) men are wiser, (iii) women should not argue or speak their mind, and (iv) educating men is more important than educating women. Again, the index ranges between 0 and 1, with 0.25 assigned for every affirmative answer.

In addition to the above-mentioned variables, the age at marriage and first birth, household size, wealth index of the family and several other dichotomous variables are explained in descriptive statistics (Table 2). In short, if a woman answered yes to the question, they were coded one and otherwise zero

4.2 Independent Variables

We construct two education variables: years of education, as a continuous variable, and a dummy variable capturing whether the woman completed 8 years of schooling.[7] Descriptive statistics regarding education variables allow us to make a few important observations (see Table 2). The difference between the control groups in 2003 and 2008 is negligible: 0.3 years and 5 percentage points for the years of education and completion of at least 8 years of schooling, respectively. In contrast, the 2008 treatment group obtained significantly more education than the treatment group in the 2003 sample (difference of 1.5 years and 30 percentage points).

[7] The women in our sample no longer remain in education. This means educational data obtained from TDHS represents the final education level of women.

Table 2 Descriptive Statistics

	TDHS 2003				TDHS 2008			
	Control aged 22-29		**Treatment** aged 18-21		**Control** aged 22-29		**Treatment** aged 18-21	
Description of the Variables	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean
Dependent Variables								
Children are Immunized against DPT3	1918	0.624	316	0.520	952	0.821	194	0.810
Children are immunized against for Hepatitis3	1918	0.491	316	0.362	953	0.788	194	0.772
Age at first marriage	1918	18.956	316	16.471	954	19.680	194	16.641
Age at first birth	1918	22.968	316	18.329	954	23.960	194	18.345
Women in the workforce	1918	0.322	316	0.290	954	0.208	194	0.134
Women`s spouses in the workforce	1886	0.471	307	0.376	947	0.596	191	0.532
Owns flushing toilet	1916	0.699	316	0.650	944	0.776	192	0.727
Wealth index of the family	1918	-0.102	316	-0.270	954	-0.076	194	-0.371 .
Household size	1918	5.682	316	6.023	954	5.474	194	5.788
Knowledge of the ovulation cycle	1917	0.252	316	0.134	952	0.206	194	0.111
Responsible for family budget	1918	0.185	316	0.092	952	0.139	194	0.058
Responsible for bills and dealing with authorities	1918	0.120	315	0.055	954	0.134	194	0.049
Woman and her husband arranged marriage	1918	0.494	316	0.404	954	0.466	194	0.422
Woman against violence against women	1918	0.574	316	0.438	954	0.756	194	0.773
Woman against gender inequality	1918	0.448	316	0.275	954	0.477	194	0.378
Independent Variables								
Years of schooling	1918	5.831	316	4.906	954	6.146	194	6.384
Completing 8 years of schooling	1918	0.276	316	0.173	954	0.298	194	0.571
Ethnicity	1918		316		954		194	
Turkish		0.746		0.672		0.738		0.736
Kurdish		0.216		0.290		0.231		0.248
Others		0.038		0.038		0.031		0.027

Rural/urban status during childhood	1883		312		944		193	
Rural		0.510		0.528		0.451		0.400
Urban		0.490		0.472		0.549		0.600
Child-gender dummy	1918		316		954		194	
Male		0.522		0.511		0.532		0.460
Female		0.478		0.489		0.468		0.540
The birth order dummies of children	1918		316		954		194	
First child		0.435		0.781		0.424		0.776
Second child		0.342		0.188		0.332		0.237
Third child		0.133		0.028		0.140		0.089
Fourth child		0.089		0.003		0.104		0.003
Pre-determined Factors								
Mother literate	1908	0.435	314	0.381		0.469	190	0.478
Father literate	1913	0.841	316	0.859		0.869	183	0.856
Mother with 8 years of education	1886	0.035	311	0.027		0.040	194	0.060
Father with 8 years of education	1815	0.140	294	0.124		0.174	194	0.209

Apart from the above explanatory variables, a dummy variable representing the child's gender is included in the regression to control for the impact of gender on the vaccination status of children. Finally, we include dummies for the baby's birth order to account for the fixed effects of the mother's previous birth experiences about vaccination.

To control for the unobserved time-invariant effect of the childhood environment (disparities in socio-economic developments among regions, inequalities in school and teacher quality and their availability in the pre-reform period) on schooling outcomes, we control for the childhood region, and place of residence (rural/urban) in which women spent most of their childhood until the age of 12. Ethnicity is also included. The fixed effects for the mother`s year of birth are used to account for the impact of various government programs and policies, as well as changes in the utilisation of healthcare services and education preferences among different cohorts, which were unrelated to the CEL but occurred within the same period.

A problem arises if the regional intensity of public spending on classroom construction is not arbitrary but is higher in regions with lower pre-reform enrolment rates in grades 6-8. To deal with this issue, the interaction of the year of birth fixed effect with the gross enrolment rate in 1996-1997 in the childhood region prior to education reform is used. This controls for the differentiation in the intensity of the implementation of the education reform linked with the enrolment rates before the reform at junior high schools (grades 6-8) and other unobservable factors related to these enrolment rates.

4.3 Empirical Strategy

4.3.1 Difference in Difference in Differences

Turkish children used to have to attend primary school for at least 5 years. However, in August 1997, compulsory schooling increased from 5 to 8 years. The first cohort affected by this change were the children who started the fifth grade in the 1997-98 academic year. School enrolment in Turkey is determined according to calendar years, rather than schooling years.[11] Therefore, women who were born in or after 1987 (aged 10 or less in 1997) were affected by the education reform whereas the older women (aged 11 or more) were not.[12] Therefore, the identification strategy has to be based on the fact that the education reform sorts individuals into treatment and control groups according to their year of birth.

In this paper, we use the three-year exogenous change in educational attainment triggered by the timing of the education reform as an instrument for education. One of the requirements of a valid instrument is that it should not have any impact on outcome variable other than its influence through schooling. We believe that the education law reform meets this condition. Firstly, the compulsory schooling reform was caused by political events in 1997, so that it has no link with the outcome variables. Secondly, the factors typically blamed for causing endogeneity of education, such as ability bias and other background characteristics, are not likely to be linked to the year of birth.

[11] The law states that "A child who has completed 72 months by the end of the calendar year can be registered to the first degree of primary school", according to the law published in edition No. 21308 of the official newspaper of Turkish Republic on Friday, 7 August 1992.

[12] However, the implementation of the age cut off is not strict: some children who were born in early 1986 might start school in September 1991 instead of September 1992, while some other children might start school in September 1993. This means some of the pupils who were born in 1986 could have been subject to the education reform. This could contaminate the results, and therefore the 1986 cohort was excluded from the estimation as a robustness check. Excluding this cohort yielded results which were not materially different.

Relying solely on the variation in the birth year cohorts might lead to bias in the estimations since there might be other unobserved events taking place at the same time as the education reform. Therefore, we utilise the fact that this reform generated an urgent need for new schools and classrooms. We define the intensity of education reform as the difference in additional expenditure on classroom construction per 1000 children between 1997 and 1996 in the childhood region of the mother.[13]

Some earlier studies have used the actual number of additional classrooms provided for junior high school education (e.g. Güneş, 2015) and teachers hired (e.g. Dinçer et al., 2014) as measures of intensity of Turkey`s compulsory education reform. We use monetary spending instead. Construction costs should be higher in densely-populated regions than regions with low population density because of higher price levels. Additional classrooms built in densely populated regions are more likely to be used fully because of the higher population there, so they are more likely to make a difference. For this reason, it can be suggested that the choice of construction expenditure as a program intensity will explain more than the change in the actual number of teachers employed and classrooms constructed due to the education reform.[14]

Figure 2 shows that there is little correlation between the enrolment rates in 1996 education year and the additional expenditures on classroom constructions: the allocation of additional funds for classroom construction appears as good as random, making it, in combination with the year of birth, a good measure of the reform impact (see Duflo, 2001). Since the identification of the instrument comes from the fact that individuals affected by the education reform experienced a higher intensity of construction expenditures, the intensity is required to be conditionally random. At first

[13] The estimations were made also using the difference between 1998 and 1996 as a robustness check. The change in the measure of intensity does not have an impact on the outcomes of interests.
[14] We address the relevance of the instrument further below in 5.3.2.

glance, the condition for the identification assumption seems to be satisfied as shown in Figure 2. To account for the potential unobserved time-invariant impact of childhood environment on the distribution of additional spending on classroom construction across regions, we account for the childhood region of residence and rural/urban characteristic. Moreover, we control for the interaction of year of birth fixed effect with the gross enrolment rate in 1996-1997 education year in the childhood region prior to the education reform. This accounts for the differentiation in the intensity of the compulsory education reform correlated with the enrolment rates before the reform at junior high school (grade 6-8) and other unobservable factors related to these enrolment rates across cohorts.

Women born between 1987 and 1990, who were affected by the education reform, therefore form the treatment group, and those born between 1979 and 1986 are in the control group.[15] The schooling decision of the individuals can be estimated with Model 1 as follows:[16]

$$S_{ijt} = \mu + \beta_l + \gamma_j + \theta_t\left(treated * intensity_j\right) + \theta intensity + X_{ijt}\pi + \varepsilon_{ijt} \quad (1)$$

S_{ijt} denotes the educational attainment of mother *i* who lived in the childhood region *j* in treatment group *t*. As indicated previously, there are two education variables, years of education and a dummy for completing 8 years of formal education. The "$treated$" variable equals one for the treatment group, and zero for the control group. "$intensity$" shows the regional variation in the intensity of education reform in the childhood region of women, which explain the effect of other determinants related to the CEL. β_l indicates the year of birth fixed effect; γ_j is the childhood region fixed effect, which is the region where women lived most of their lives until age 12; and finally, the

[15] See Table 4 for empirical identification of treatment and control groups.
[16] Model 1 was constructed similarly as in Duflo (2001).

remaining control variables are represented by X_{ijt}. These are ethnicity, the interaction of year of birth with gross enrolment rate in 1996-97, and two dummies: (i) the birth order of the baby and (ii) the gender of the baby.

The correlation between schooling and the reform for the treatment group is estimated by $\theta_t + \theta$ whereas the same relationship for the control group is represented by θ. Therefore, θ_t captures the impact of the compulsory education reform on the formal schooling of the treatment group, if the control and treatment groups are equally influenced by the other determinants associated with the intensity variable. Assuming the reform was exogenous, θ_t measures the impact of the reform intensity variable on the schooling of treated mothers.

Up to now, the discussion has focused on the assumption that the exposure of women to the CEL is jointly determined by year of birth and region of childhood. This assumption implies that factors related to the intensity of public investment on classroom construction have identical influence on mothers in the treatment and control groups. However, if mothers' outcomes such as their use of preventive health measures for their children (e.g. vaccination) vary by age, any method that does not compare women in the same age group might be biased. The difference in difference methodology (DD) cannot account for the impact of age on the outcome of interest. For that reason, we use the Difference and Difference in Differences (DDD) strategy. This methodology assumes that the education choices of individuals are a function of the date of birth, additional government spending on classroom construction in the region of childhood and age. The DDD strategy thus can control for both year of birth fixed effects and age fixed effects whereas DD only controls for the former (see Dinçer et al., 2014). In order to use the

DDD, a sample of young mothers[17] between the ages of 18 and 29 is formed whereby the 2003 and 2008 TDHS cross section data are combined.[18] As a result, the DDD methodology proposed by Dinçer et al. (2014) could be applied in this setting with the combination of 2003 and 2008 TDHS so that Model 2 is structured as follows:

$$S_{ijt} = \propto + \omega_l + \beta_j + \beta_a + \theta_t(treated * intensity_j * 2008) + \theta_y(treated * intensity_j) + \theta intensity_j + X_{ijt}\pi + \varepsilon_{ijt} \quad (2)$$

Note that, different symbols were used to differentiate variables of Models 1 and 2. In the above regression, β_a stands for the age fixed effect; ω_l is the year of birth fixed effect; β_j is the region of childhood fixed effect and 2008 is a dummy for the TDHS 2008 cross section. The year of birth fixed effect controls for general trends in the outcome of interest caused by other changes specific to age cohorts.[19] The age fixed effect, on the other hand, controls for the impact of the age on the outcome of interest.

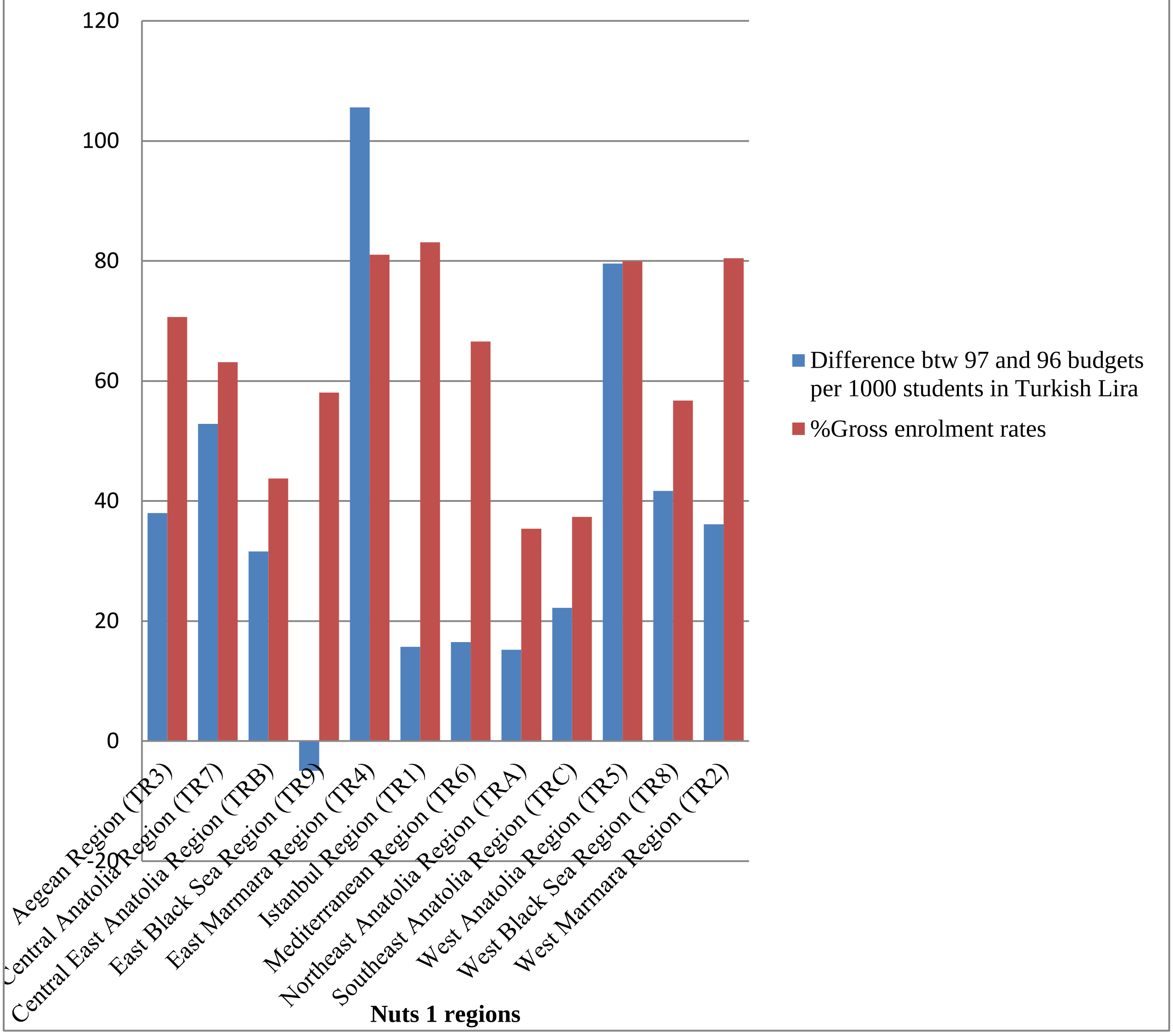
θ_y, measures how the impact of the intensity of public spending on classroom construction varies between the young (aged 18-21) and old (aged 22-29) women who participated in the 2003 wave of the survey. On the other hand, θ_t denotes the impact of the reform intensity on the education of young mothers aged 18 to 21 who participated in TDHS 2008.

[17] If education causes births and teenage marriage to fall, using only ever-married woman may lead to sample selection bias. However, Kirdar, Tayfur, and Koç (2011) found that while CEL reduced childbearing and marriage for women aged 17 and less, no effect was observed for women aged 18 and over. These results make sense because with CEL, the school exit age rises from 11 to 14.

[18] The inclusion of age fixed effects is also important for the outcome measures estimated at the second stage. That is because like other outcome variables, the variation in the utilisation of vaccination as a preventive health care measure by mothers for their children can mainly be explained by their age difference. However, the DDD method comes with an additional assumption. Specifically, it assumes that mothers between the ages of 18 and 21 in TDHS 2003 and 2008 have identical trends related to educational attainment, utilisation of immunisation services for their babies and potential mechanisms affecting vaccination usage in the absence of education reform.

[19] For instance, the minimum age for marriage was raised from 15 to 17 in 2002 affecting women born after 1985. Moreover, due to a transformation in the health program passed by the government in 2003 and initiated in 2004, the amount of government expenditure on the National Immunization Program increased from 18 million TL in 2003 to 286 million in 2008 (Ceyhan, 2010).

Figure 2 The variation in gross enrolment rates and investments in infrastructres



Source: Authors' calculation from the Turkish Republic`s Ministry of Development`s 1996 and 1997 Statistical Yearbooks on Public Expenditure, Ministry of National Education (MONE) 1996 dated yearbook and population statistics of Turkish Statistical Institute.

In all regressions, standard errors are clustered for the 26 regions of childhood. The problem with this might be that if the number of clusters is less than around 42-50, the null hypothesis may be rejected even when it is true (Angrist & Pischke, 2009; Bertrand, Duflo, & Mullainathan, 2004). On the contrary to the above suggestion, Cameron, Gelbach, and Miller (2008) reach a more optimistic conclusion regarding the consistency of standard errors with few clusters. They argue that the null hypothesis is less likely to be rejected when it is true if the number of clusters is around 20 than when it is 50. Moreover, even with as few as six clusters, the rejection rates coming from the theory are very similar to empirical values (Cameron et al., 2008).

4.3.2 Ordinary Least Square and Instrumental Variable

Maternal Education and Childhood Immunization Rates

The effects of maternal education on childhood immunisation in an Ordinary Least Square (OLS) setting could be restated by modifying the above difference in difference in differences strategy as follows (Model 3):

$$Y_{ijt} = \propto + \omega_l + \beta_j + \beta_a + \delta S_{ijt} + \delta_y \left(treated * intensity_j \right) + \theta intensity_j + X_{ijt}\pi + \varepsilon_{ijt} \quad (3)$$

Where Y_{ijt} is a dummy variable which equals 1 if children were vaccinated against DPT3 or Hepatitis B3 and 0 otherwise. However, OLS estimates of δ might be biased because it is possible that schooling is correlated with the error term (ε_{ijt}). For instance, a possible bias of OLS estimates occurs if unobserved characteristics of women similarly influence both their educational attainment and their behaviour related to the vaccination of their children.

On the other hand, if the reform only affects the outcome of interest through education, i.e. the reform has no direct effect on the dependent variable (vaccination rates), then, the results of difference in difference in differences (DDD) estimates in Model 2 capture the effects of the CEL on maternal education. In other words, Model 2 can be used as the first stage of Instrumental Variable (IV) estimation. More specifically, the triple interaction term "$treated * intensity * 2008$" in Model 2 can be used as the instrument for the schooling of mothers, so as to obtain, we can obtain unbiased estimates of the effect of education on the outcome considered.

Model 4 then is as follows:

$$Y_{ijt} = \propto + \omega_l + \beta_j + \beta_a + \delta \widehat{S_{ijt}} + \delta_y (treated * intensity_j) + \theta intensity_j + X_{ijt}\pi + \varepsilon_{ijt} \quad (4)$$

Note that education was replaced by $\widehat{S_{ijt}}$, the predicted value of education. It is also important to note that instead of IV-Probit or Logit, this study uses conventional 2SLS estimation technique as suggested by Angrist (1991) and Angrist (2001) since the dependent and endogenous variables, as well as the instrument, are dichotomous. Under this condition, 2SLS estimates identify marginal treatment effect irrespective of the fact that the dependent variables are binary or continuous (Angrist & Pischke, 2009). There are plenty of examples of the usage of 2SLS estimates instead of IV-Probit or Logit in the previous literature (e.g. Breviero & Duflo, 2004; Chou et al., 2010; Mocan & Cannonier, 2012; Osili & Long, 2008).

OLS and 2SLS strategies are thus used to estimate structural equations in this study. All remaining explanatory variables, except the triple interaction term, are included in Model 4. Moreover, in the same way as with DDD estimates, in all regressions, standard errors are clustered at the level of regions of childhood for 26 regions.

Maternal Education and Channels Affecting Childhood Immunization Rates

The second objective of this paper is to study the effect of education on the various channels potentially affecting immunisation. We employ a similar strategy to that mentioned for vaccination outcomes.[20] If education empowers women, this can serve as a channel through which education affects vaccination rates.

4.3.3. Reduced Form

A modification of Model 2 (the first stage regression) results in a reduced form (RF) estimates. To do this, the outcome of interest in the first stage is replaced with vaccination status and other relevant outcomes. Model 5 is thus as follows:

$$Y_{ijt} = \propto + \omega_l + \beta_j + \beta_a + \theta_t(treated * intensity_j * 2008) + \theta_y(treated * intensity_j) + \theta intensity_j + X_{ijt}\pi + \varepsilon_{ijt} \quad (5)$$

The reason for employing RF estimates is that the results of the estimation may differ because of the systematic difference between young and old cohorts or because of higher education as a result of the CEL. The RF model thus provides information related to the variation in the vaccination status of children and other outcomes due to the exogenous rise in public spending on classroom construction because of the CEL experienced by the treatment group (young) in 2008 (i.e. $treated * intensity * 2008$). The same control variables are used for RF estimates of childhood vaccination status. However, similarly with IV and OLS estimates of channels affecting immunisation, RF of these outcomes variables does not account for the gender and birth order dummies.

5 Results and Discussion

5.1 Difference in Difference in Differences Estimation Result

The results of the DDD analysis are presented in Table 3. Firstly, all DDD coefficients are positive and statistically significant as expected. More importantly, the F-statistics test of the joint significance of the triple interaction term (namely the instrument) is

[20] However, OLS and IV estimates do not control for gender and birth dummies of children for the outcome measures considered in this subsection.

more than 10 for almost all specifications. This indicates that the instrument is strong (Staiger & Stock, 1997). It is worth noting that the instruments used in the previous studies on Turkey were weak for years of education in all model specifications, and therefore the analyses were restricted to the effect of completing 8 years of education, on the outcome of interest (see Dinçer et al., 2014; Güneş, 2015).

The CEL has a strongly positive effect on education in all specification. The effect is significant not only statistically but also economically. Considering column 8, every additional 1 Turkish Lira (TL) of public spending per 1000 children raised primary school completion by 0.3 percentage points. The average increase in public expenditure on education was 40.36 TL. Each additional TL spent led to an increase in the probability of completing at least 8 years education by 12.1 percentage points (i.e. 0.3 multiplied by 40.36). Given that 17 percent of women attained 8 or more years of education in 2003, this would represent approximately a 70 percent increase in the share of women who completed primary school and above. Similarly, one additional Turkish Lira (TL) spent per 1000 children increases education by 0.011 years (column 4). As before, the average additional public expenditure on education is 40.36. Therefore, the education reform caused an increase in years of education by about 0.44 years (162 days). The average length of schooling for the young cohorts in 2003 is 4.91 years. The CEL thus lead to a 9 % increase in the years of education of the treatment group in 2003. To sum up, the education reform had a significant influence on the schooling of treated mothers in 2008.

Table 3 The impact of the Compulsory Education Law on Formal Schooling-DDD analysis (The first stage of IV regression)

Dependent Variable:	**Years of schooling**				**Completing 8 years of schooling**			
	Column1	**Column2**	**Column3**	**Column4**	**Column5**	**Column6**	**Column7**	**Column8**
treatment*intensity*2008	0.014***	0.009***	0.009***	0.011***	0.003***	0.002***	0.002***	0.003***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)
Controls								
Ethnicity	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Rural/urban status during childhood	No	Yes	Yes	Yes	No	Yes	Yes	Yes
A child-gender dummy	No	No	Yes	Yes	No	No	Yes	Yes
The birth order dummies	No	No	No	Yes	No	No	No	Yes
R-squared	0.779	0.810	0.810	0.826	0.370	0.436	0.436	0.464
F-statistics	14.97	7.00	7.24	12.40	44.96	34.84	35.33	39.34
Observations	3,339	3,327	3,327	3,327	3,339	3,327	3,327	3,327

Note: Women aged 18-29 in 2003 and 2008 form the sample of analysis. Women aged 18-21 form the treatment group. The intensity is the difference between the 1997 and 1996 government funds distributed for primary school construction at the region of childhood. Robust standard errors in parentheses cluster at the region of the childhood. F-statistics are the test of the joint significance of the triple interaction term (treatment*intensity*2008). The baseline Models 1 and 5 include no control variable. In addition to the controls given in the table, all models include ethnicity, the urban/rural status of the region of residence in childhood (except Models 1 and 5), the region of childhood, year of birth and age of respondent fixed effects, the intensity variable, the interaction of year of birth with gross enrolment rate in the region of childhood and the interaction of treatment and intensity variables. *** p<0.01, ** p<0.05, * p<0.1

5.2 Validation of treatment and control groups

As explained before, the treatment group is formed by young mothers between the ages of 18 and 21 in the 2003 and 2008 TDHS, whereas those in the age group of 22-29 in the same surveys constitute the control group for the sake of the analysis. Consistent with the strategy of previous studies, we test the validity of the treatment and control groups in this section (e.g. Güneş, 2015; Osili & Long, 2008). To do this, the three-way interaction terms in Model 2 is replaced by 12 separate interaction terms. This means that the treatment variable is turned into 12 dummies, one for each year of age. As expected, the estimates of the coefficients for mothers aged 22-29 are close to zero and statistically insignificant for both years of education and primary school completion (see Table 2.4). However, the coefficients are statistically significant and positive for women aged 18-21. This provides evidence supporting the construction of treatment and control groups.

5.3. Discussion of DDD Estimation Results

5.3.1 Parallel Path Assumption

A crucial assumption in the above DDD estimation is that of "Parallel Paths". It states that the average differentiation in the control group shows the counterfactual differences in the treated group if the treatment group were not treated. However, the treatment group prior to treatment (i.e. young women in 2003 data) cannot be a priori assumed to be a true counterfactual. The data sets of this paper are cross sections collected in every 5 years. Therefore, there is an implicit 5-year shift forward (i.e. time trend) that might affect outcomes.

Table 4 The impact of CEL on the schooling of each age separately

	Dependent Variables	
Age in 2008	Completing 8 years of education	Years of education
18	0.004***	0.027***
	(0.001)	(0.008)
19	0.004***	0.029***
	(0.001)	(0.005)
20	0.004***	0.024***
	(0.001)	(0.006)
21	0.003***	0.009**
	(0.000)	(0.004)
22	0.000	0.004
	(0.000)	(0.004)
23	0.000	-0.003
	(0.001)	(0.004)
24	0.000	-0.002
	(0.000)	(0.004)
25	-0.000	-0.002
	(0.000)	(0.002)
26	0.000	0.001
	(0.001)	(0.004)
27	0.000	0.004
	(0.000)	(0.003)
28	0.000	0.001
	(0.000)	(0.003)
Observations	3,327	3,327

Table 2.4 shows the impact of CEL on primary school completion rates and single years of education for each age. The estimation sample covers mothers aged 18-29 at the time of the surveys. The interaction term is the interaction of age*intensity*2008 for each age. Robust standard errors are in parenthesis. Standard errors are clustered at the region of childhood. *** p<0.01, ** p<0.05, * p<0.1

Comparison of pre-treatment characteristics can be used to ensure comparability. Table 2 reports summary statistics for the dependent variables, selected independent variables, and some family-related characteristics. Some of these variables could be seen as pre-determined factors. Comparing the average change in control and treatment group related to pre-determined factors should indicate potential violation or satisfaction of the parallel path assumption. The ethnicity of a woman, residence status during childhood, the literacy of a woman`s mother and father are classified as pre-determined factors.

We perform the t-tests to examine the hypothesis that the means of treatment group in 2003 and 2008 are equal for these pre-determined factors. The result suggest that there is no significant difference in the averages of these two groups. We also test whether the averages of the control group in 2003 are different from those in 2008. The t-tests again show no significant differentiation across groups, with the exception of urban-rural status. The control group in 2008 data are more likely to live in urban areas at one percent significance level. Overall, the t-tests show that some characteristics are largely pre-determined and similar across cohorts. [21]

This implies that even though there might be an implicit time trend in the economy and preferences because of 5-year shift forward, it seems that DDD results are valid as the differences in the pre-determined factors are small. We always control for fixed effects of a woman`s year of birth to account for the impact of various government programs and policies, as well as changes in the utilization of health care services, education and other factors across cohorts, which could be unrelated to the compulsory education reform but occurred within the same period, and, therefore, might cause a time trend. Therefore, it can be concluded that comparison of pre-determined characteristics and accounting for the year of birth fixed effects indicate that the treatment group of 2003

[21] The results of t-tests are available upon request.

TDHS can be used as counterfactual and, therefore, parallel path assumption is likely to hold in the construction of DDD estimation for this study.

5.3.2 Discussion of the First Stage F-statistics

This paper obtains larger first stage F-statistics for the years of education and completion of 8 years of education variables than the previous studies on Turkey such as Dinçer et al. (2014), who use a similar estimation technique to investigate the impact of the reform on education outcomes for Turkey. There could be four possible reasons behind our larger first stage F-statistics: (i) using a different measure of the regional intensity variable of the compulsory education reform to construct the instrument, (ii) the usage of different sub-samples, (iii) defining treated population differently, and (iv) using slightly different covariates in the estimated model.

To test whether our F-statistics are lower because of employing a monetary measure of the regional intensity variable of the education reform, or different sub-samples for the analysis, we replaced the construction expenditures with the the number of teachers employed after the reform as in Dinçer et al. (2014). Then, we re-estimated the models in column 4 and 8 in Table 3 with this intensity measure. We found that the value of the F statistics is 9.10 and 31.11 for the years of education and completing 8 years of education outcomes, respectively, as illustrated in the first row of Table 5. However, when the intensity of construction expenditure is used as an intensity measure, the F-statistics are 12.40 and 39.40 for the years of education and completing 8 years of education, respectively, as reported in the second row of Table 5. These F-statistics are greater than the values when teacher recruitment rates are exploited as a measure of the reform intensity. However, they are greater than those reported in Dinçer et al. (2014), as shown in the last row of Table 5.

Thirdly, Dinçer et al. (2014) also differ to this study as they define the treatment group as women aged 18 to 22 whereas only women aged 18-21 are included in this study. In other words, individuals who were born in 1986 (i.e. aged 22) are placed in the treatment group in their study, but not in this study. According to the compulsory education reform, students who started fifth grade in September 1997 were subjected to the education reform whereas individuals who started sixth grade in September 1997 are born in 1986 or aged 11 and were not bound by the reform. Having said that, as explained earlier some of the individuals who were born in 1986 might be still affected by the reform as the age cut off for registering the first grade of primary school was not strict.[22] Table 4 shows that the impact of the education reform on individuals aged 22 is statistically insignificant. Therefore, it is quite likely that including this age cohort in the treatment group instead of the control group could cause an important reduction in the F-statistics.

The third and fourth row of Table 5 illustrate the first stage F-statistics for both intensity variables with the data set of this study when individuals aged 22 are in the treatment group. The fifth row of Table 5 gives the F-statistics of Dinçer et al. (2014). When we define affected individuals as in Dinçer et al. (2014) and estimate the models in column 4 and 8 in Table 3 with their intensity measure as done in the previous paragraph, the F-statistics for both education variables drop significantly. If we go back to our intensity variable and estimate the models in column 4 and 8 of Table 3 with this modified treatment group, the results of the F-statistics are also quite similar to Dinçer et al. (2014).

[22] This was explained in greater details in 4.3 Empirical Strategy Section.

Table 5 Variation in the first stage F-statistics

	Intensity variables	F-statistics Years of education	 Completing 8 years of education	 Observation
Women aged 18-21 form the treatment group as in Table 3	*Construction expenditure*	12.4	39.34	3327
	Teacher	9.1	31.11	3327
Women aged 18-22 form treatment group as in Dinçer et al. (2014)	*Construction expenditure*	4.37	14.1	3327
	Teacher	7.93	14.48	3327
Dinçer et al. (2014)	*Teacher*	6.51	13.42	5147

Note: "Construction expenditure" variable is measured as the difference between the 1997 and 1996 government funds distributed for primary school construction at the region of childhood because of the education reform. Similarly, teacher variable is defined as in Dinçer et al. (2014) and, therefore, measured as the additional teacher employed due to education reform. F-statistics are the test of the joint significance of the triple interaction term (treatment*intensity*2008). Models in column 4 and 8 in Table 2.3 are estimated for comparisons. When teacher variable is used as an intensity measure, the intensity of construction expenditure variable is replaced with teacher variable. Also, models have been modified accordingly. The last row of the table reports F-statistics of Dinçer et al. (2014)` original study to make a comparison of the results reported in this table with their study.

Small differences in the F-statistics could also be attributed to using slightly different covariates and difference in clustering robust standard errors. It can be seen from Table 3 that adding covariates changes the magnitude of F-statistics for both education variable,. Also, the clustering in this study is done with respect to 26 sub-regions of Turkey, but they clustered for 20 regions of Turkey which might influence the standard errors and, therefore, F-statistics. However, in light of the above discussion, it is fair to argue that differentiation in the definition of the treated population and using a different measure of the treatment intensity seems to explain the largest part of the differences in F-statistics between Dinçer et al. (2014) and this study.[23]

5.4. Effects of maternal education on DPT and Hepatitis B vaccination

The previous part discussed the first stage of the IV estimation, that is the correlation between the Compulsory Education Law (CEL) and maternal education. This section has two objectives. Firstly, it focuses on the effect of additional funds distributed for classroom construction due to the CEL on the take-up of the third dose of the Hepatitis B and DPT vaccines for children (i.e. RF estimates). Secondly, it examines the causal effects of maternal education on the same outcome interest (i.e. IV and OLS estimates).

Starting with the reduced form (RF) estimates, the results in Table 6 indicate that the exogenous rise in public spending on classroom construction because of the CEL had a positive and significant impact on the vaccination status of babies. To put it another way, based on the figures in Table 6, as a consequence of the CEL, there is 4 and 8 percentage points increase in the probability of the third (last) dose of DPT and Hepatitis B being administered, respectively. These results provide confirmatory

[23] A conclusive answer to this question would require using exactly the same data set and model used in Dinçer et al. (2014). However, because of the reasons explained earlier, it is impossible to use the same data set.

evidence that vaccination rates may differ because of the systematic difference between young and old individuals, or because of receiving a higher education due to the reform.

Turning to the OLS coefficients, both years of education and primary school completion also have a positive and statistically significant effect, but size of the effect is modest. An additional year of maternal education is associated with 1.3 and 1.4 percentage points rise in the likelihood of complete immunisation status of infants for DPT3 and Hepatitis B3, respectively. Completing 8 years of formal schooling results in an increase in the probability of vaccination of around 5% for DPT3 and 7% for Hepatitis B. Nevertheless, the regression coefficients of primary school completion are significant only at ten and five percent levels, respectively.

However, it is likely that the results of the OLS estimates may be misleading. As previously argued, educational attainment may be endogenously determined by unobservable omitted variables affecting both the educational attainments of women and their preference for the use of vaccines for their children. If this is the case, it violates the exogeneity assumption of OLS as it implies a correlation between the education variable and the error term of the regression. We address this issue by employing the Instrumental Variable (IV) technique.

The IV estimates also indicate a positive and significant causal association between maternal education and the vaccination. Specifically, an additional year of schooling increases the probability of receiving the third dose of DPT and Hepatitis B by around 13% and 22%, respectively, and completion of 8 years of schooling leads to an increase in the probability of receiving the third dose of these vaccines by 55% growth for DPT 3 and by 92 % for Hepatitis B. Hence, maternal education has a strongly positive significant effect on their children's vaccination rate.

Table 6 The Causal Impact of Education on the Complete Vaccination Status of children aged over 6 months

	DPT3	*Hepatitis B*
Reduced Form	0.001**	0.002***
	(0.001)	(0.001)
OLS		
Years of Education	0.013***	0.014***
	(0.004)	(0.003)
Completing 8 years of schooling	0.048*	0.070***
	(0.025)	(0.023)
IV		
Years of Education	0.127**	0.215***
	(0.054)	(0.058)
Completing 8 years of schooling	0.546**	0.920***
	(0.216)	(0.214)
Observations	3325	3326

Note: Women aged 18-29 in 2003 and 2008 form the sample of analysis. Women aged 18-21 form the treatment group. The intensity variable is measured as the difference between the 1997 and 1996 government funds distributed for primary school construction at the region of childhood. Robust standard errors in parentheses cluster at the region of childhood. F-statistics is the test of the joint significance of the triple interaction term (treatment*intensity*2008). For the analysis of all types of regressions, Model 8 is used. Therefore, all models include ethnicity, the urban/rural status of the place of childhood, region of childhood, year of birth and age of respondent fixed effects, the intensity variable, the interaction of year of birth with the gross enrolment rate in the region of childhood, the interaction of treatment and intensity variables. Significance: *** p<0.01, ** p<0.05, * p<0.1

5.5 Effects of education on channels affecting the childhood vaccination rates

After having obtained evidence for the causal effects of maternal education on the immunisation rates of children, the question of how education affects vaccination can be addressed. Although the research methodology of this study does not allow us to estimate the causal impacts of these channels on vaccination outcomes directly, it can shed some light on the potential role of these channels.

The results are given in Tables 7 and 8. The OLS estimates show that one extra year of education raises the age of first marriage by 0.27 years and the age at first birth by 0.05 (Table 7). Likewise, a ten percent increase in the proportion of women with 8 years of education increases the age of first marriage and age at first birth by around 0.15 and 0.03 years, respectively.

The IV estimates are again much larger than the OLS estimates. An additional year of formal education increases the age at first marriage and birth by 0.9 and 1.3 years, respectively, while a ten percent increase in the share of women with completed primary school education increases the age at first marriage by 0.33 and 0.50 years, respectively.[24]

Interestingly, the OLS estimates for the other outcome variables in Table 7 are mostly statistically significant at the one percent level (except mother`s working status) and have the expected signs for both years of education and completing 8 years of education. 2SLS estimates (both with the years of education and completing 8 years) indicate that the husbands of more educated women are more likely to be in the labour force, and that an increase in women completing 8 years of education has a negative impact on their labour force participation.

Lastly, the RF estimates are also statistically significant for the age of first marriage, first birth and labour market status of women and their husbands. However, the RF results for half of the dependent variables in Table 7 are statistically insignificant. This casts doubt on the usage of OLS estimates since this confirms that the IV results are substantially different and that endogeneity is an issue here.

Table 8, which represents the OLS, RF and IV estimates for attitudes on gender issues reveals similar conclusions. Specifically, the OLS coefficients for the years of education and completing 8 years of education show a correlation with the autonomy of the

[24] The results regarding the age of first marriage match those observed in earlier studies by Breirova and Duflo (2004) for years of education and Güneş (2015) for categorical education. However, Dinçer et al (2014) find completing 8 years of education plays no role as a source for an increase in the age of marriage and birth.

woman, attitude towards gender inequality and violence against women. However, in almost half of these cases, IV estimates are statistically insignificant except for the women`s attitudes towards violence against women and gender discrimination.[25]

The IV estimates of both the attitude of women towards spouse violence against women and gender discrimination show that education decreases the probability of accepting domestic violence and gender inequality. Importantly, the magnitude of the IV effect is much larger than the OLS estimate, regardless of the education variable used. The RF estimates are also very small and insignificant for all variables other than the attitude of women towards spousal violence and gender discrimination.[26]

Women`s education thus seems to have a causal impact on the age of first marriage and first birth, employment status of women and their husbands, and women's attitude towards domestic violence against women and gender inequality. All of these channels are consistent with empowering women. Greater autonomy for women, in turn, can contribute to the improvements in the completion rates of the DPT and Hepatitis B vaccinations.[27]

[25] To the best of our knowledge, in addition to being first study estimating causality between maternal education and their children's vaccination status, this study is also in the category of few studies estimating the causal effect of education on the autonomy of women (e.g. Samaracoon & Parinduri, 2015).

[26] These findings are in line Dinçer et al. (2014) and Mocan and Cannonier (2012) who found that education improves women`s attitude towards spouse violence against women. In contrast, Dinçer et al. (2014) found an insignificant impact of education on gender discrimination. Samarakoon and Parinduri (2015) found no significant association between female education and a range of autonomy variables considered.

[27] In an unreported regression, this study estimated several reduced form regressions explaining vaccination with age at first marriage and birth, husband`s working status and attitudes towards spousal violence as well as other covariates used in main estimates in Table 6. Even after controlling for these covariates jointly or separately, RF estimates reported in Table 6 for vaccination status are still robust and have an almost identical coefficient for the instrument for various specifications.

Table 7 Effects of maternal education and intensity of the reform on age of first marriage, birth, wealth, working status and knowledge

VARIABLES	Age at first marriage	Age at first birth	Mother`s working status	Spouse`s working status	Ownership of Flush toilet	Family wealth index	Household size	Knowledge of the ovulation cycle
RF	0.008***	0.012***	-0.001*	0.001**	0.000	-0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.004)	(0.000)
OLS								
Years of education	0.270***	0.050***	0.008*	0.034***	0.021***	0.110***	-0.184***	0.031***
	(0.027)	(0.014)	(0.004)	(0.003)	(0.004)	(0.006)	(0.025)	(0.004)
Primary school completion	1.529***	0.314***	0.009	0.204***	0.118***	0.630***	-0.982***	0.171***
	(0.195)	(0.112)	(0.023)	(0.021)	(0.024)	(0.042)	(0.145)	(0.032)
IV								
Years of education	0.879***	1.317***	-0.080	0.129**	0.046	-0.021	-0.047	-0.012
	(0.337)	(0.509)	(0.049)	(0.057)	(0.072)	(0.127)	(0.367)	(0.044)
Primary school completion	3.337***	5.001***	-0.305**	0.491***	0.173	-0.081	-0.177	-0.047
	(0.892)	(0.954)	(0.148)	(0.180)	(0.257)	(0.476)	(1.415)	(0.167)
Observations	3,327	3,327	3,327	3,257	3,313	3,327	3,327	3,324

Note: Women aged 18-29 in 2003 and 2008 form the sample of analysis. Young mothers between the age of 18 and 21 form the treatment group. The intensity variable is measured as the difference between the 1997 and 1996 government funds distributed for primary school construction at the region of childhood. Robust standard errors in parentheses cluster at the region of the childhood. Family wealth index was calculated using principal component analysis with household's ownership of assets such as a television and car as well as housing characteristics, including facilities for sanitation, drinking water sources and flooring material types. F-statistics is the test of the joint significance of the triple interaction term (treated*intensity*2008). For the analysis of all types of regressions, Model 8 is used. Therefore, all models include ethnicity, the urban/rural status of the place of childhood, region of childhood, year of birth and age of respondent fixed effects, the intensity variable, the interaction of year of birth with the gross enrolment rate in the region, in which individuals lived during their childhood at age 11 in 1996 and the interaction between the treated and intensity variables. *** p<0.01, ** p<0.05, * p<0.1

Table 8 Estimates of the impact of education on the autonomy of women and their attitude towards gender inequality and violence against women

	Responsibility for the Budget	Responsibility for official work	How marriage was arranged	Domestic violence index	Gender discrimination index
Reduced form	-0.000	0.000	-0.001	-0.001***	-0.001***
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
OLS					
Years of education	0.015***	0.006**	0.011***	-0.021***	-0.031***
	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)
Primary school completion	0.077***	0.021	0.070***	-0.113***	-0.174***
	(0.022)	(0.016)	(0.024)	(0.015)	(0.017)
IV					
Years of education	-0.049	0.014	-0.084	-0.118**	-0.078**
	(0.058)	(0.033)	(0.076)	(0.049)	(0.032)
Primary school completion	-0.187	0.053	-0.319	-0.448***	-0.296***
	(0.210)	(0.129)	(0.234)	(0.154)	(0.094)
Observations	3,324	3,326	3,327	3,327	3,327

Note: Women aged 18-29 in 2003 and 2008 form the sample of analysis. Young mothers between the ages of 18-21 form the treatment group. The intensity variable is measured as the difference between the 1997 and 1996 government funds distributed for primary school construction at the region of childhood. Robust standard errors in parentheses cluster at the region of the childhood. F-statistics is the test of the joint significance of the triple interaction term (treated*intensity*2008). For the analysis of all types of regressions, Model 8 is used. Therefore, all models include ethnicity, the urban/rural status of the place of childhood, region of childhood, year of birth and age of respondent fixed effects, the intensity variable, the interaction of year of birth with the gross enrolment rate in the region, in which individuals lived during their childhood at age 11 in 1996 and the interaction between the treated and intensity variables. *** p<0.01, ** p<0.05, * p<0.1

6 Conclusions

The literature exploring the effects of maternal education on the vaccination status of children and channels affecting these outcomes generally finds a positive correlation. As a response to these findings, policymakers tend to argue in favour of increasing female education to improve the overall vaccination coverage of babies and children.

However, the previous studies have failed to address the endogeneity of education. To the best of our knowledge, this paper is the first study providing evidence as to whether the observed correlation between maternal education and childhood immunisation rates implies causation. To do this, we use a natural experiment from Turkey: adoption of the Compulsory Education Law (CEL) which lead to an exogenous increase in the compulsory schooling from 5 to 8 years for those born after 1986. This, in turn, has led to an increase in spending on the construction of new classrooms and employment of new teachers. Importantly, the additional spending on teaching infrastructure varied substantially across the regions of Turkey.

This paper uses the regional variation in the intensity of the CEL`s application as an instrument for the schooling of young women aged 18-21, in order to find the causal relationship between maternal education and the intake of the third (last) dose of the Hepatitis B and DPT as well as other potential channels affecting these outcomes. Importantly, the difference-in- difference-in- differences (DDD) methodology employed in this study ensures that rather than comparing relatively old and young mothers, we are comparing groups within the same age ranges in 2003 and 2008.

We find that an exogenous rise in maternal education improves the coverage of the last dose of Hepatitis B and DPT immunisation, even after controlling for the gender and birth order of the child. We also find that education increases the age of first marriage and birth and alters women`s and their spouse`s labour market status, and affects women`s attitude towards spousal violence against women and gender discrimination in a manner that empowers women. Therefore, the improvements in the vaccination rates of children may be attributable to either the direct effects of education or to the changes in women`s behaviour and attitudes that empower them (or to the combination of the two types of effects). Hence, improving women's education has important positive effects, not only for the wellbeing of the women themselves, but also for their position in the society and for the health outcomes of the future generation.

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