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MATERNAL EDUCATION AND CHILDHOOD IMMUNIZATION IN TURKEY

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Maternal education and childhood immunization in 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. The results are robust to variation in regression specification and including various individual and community variables.

Keywords: DPT (diphtheria, pertussis and tetanus); Hepatitis B; Maternal Education; Vaccination; Difference-in-Difference-in-Difference; Instrumental variable.

JEL Codes: H51; H52; I12

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1. Introduction

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, and 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 are prevented by vaccination each year. 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[2] increased from 46 percent in 1998 to 81 percent in 2008 (Table 1). The likelihood that a child will be vaccinated is not random, rather, it appears closely correlated with the mother's level of education.

The relationship between maternal education and children's immunization rates has been highlighted also in the previous literature (Abuya, Onsomu, Kimani, & Moore, 2011; Altınkaynak, Ertekin, Güraksın, & Kılıç, 2004; Fatiregun & Okoro, 2012; Schoeps et al., 2013; Singh, Haney, & Olorunsaiye, 2013; Vikram, Vanneman, & Desai, 2012). However, this relationship is not necessarily causal, 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). 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

[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].

[2] Children who are full vaccinated (i.e., those who have received BCG, measles, and three doses of DPT and polio).

choices (Angrist & Evans, 1998; Jensen &al Thornton, 2003). The instrumental variable (IV) method can be used to overcome this problem.

Turkey expanded its compulsory education from 5 to 8 years in August 1997: this reform affected children aged ten or less in the 1997-98 school year. The extension in compulsory education in turn generated an urgent need for the construction of new classrooms and employment of new teachers, which increased the government budget for education. The increase in government spending 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 IV framework to estimate the effect of maternal schooling on children's vaccination rates.

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 serve as an instrument for schooling of mothers. Breierova and Duflo (2004) in Indonesia, Chou et al. (2010) in Taiwan and Osili and Long (2008) in Nigeria find a negative correlation between maternal education and fertility and child mortality. 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 in Turkey in 1997 as the measure of the reform intensity. Their results indicate positive effects of maternal education on children's mortality, birth weight, and height and weight for age. To the best of our knowledge, our paper is the first study to explore the relationship between maternal education and children's vaccination rates in an analytical setting that takes due account of the likely endoneity bias. The findings suggest that maternal education, measured both 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 (diphtheria, pertussis and tetanus) immunizations. These findings are very robust to alterations in the regression specifications and accounting for individual and community level variables.

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

2. Data and Empirical Framework

Our study utilizes the last two rounds of the Turkey Demographic and Health Survey (TDHS) conducted in 2003 and 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. The TDHS surveys also include a wide range of information on women's socioeconomic characteristics, such as education (completed years of schooling and the highest level of education attained), parents' education, employment status, ethnicity, women's status in the family, and demographic information including age, gender, type of birth place (rural/urban), the region of birth, and the region of residence during childhood. As childbearing outside of marriage is uncommon in Turkey, we only consider married women. Our final sample consists of 3331 to 3382 young mothers between the ages 18 and 29 in both the 2003 and 2008 TDHS.

Two dichotomous variables are used to measure the completion of immunisation for children aged over six months, each taking the value of one if the child received the third and final dose of DPT (diphtheria, pertussis and tetanus) and Hepatitis B vaccines, respectively.[3] We construct two education variables: years of education as a continuous

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[3] DPT vaccination provides protection against diphtheria, pertussis, and tetanus and requires three vaccinations at six, ten and 14 weeks of age. Hepatitis B vaccination protects against Hepatitis B and

variable,[4] and a dummy variable capturing whether the woman completed 8 years of schooling (junior high school).

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 include dummy variables for 26 regions in which the women spent their childhood until the age of 12.[5] The women`s region of residence until the age of 12 allows us to identify the impact of compulsory schooling reform on education of women, as it allows us to link individual survey data with regional administrative data. Furthermore, we control for and control also for rural/urban type of birth place, ethnicity and include fixed effects for the mother`s year of birth (the latter serves to account for the potential impact of government programs, as well as changes in the utilisation of healthcare services and education preferences across cohorts). A dummy variable representing the child's gender is included in the regressions 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.

A problem may arise if the regional intensity of public spending on classroom construction is higher in regions with lower pre-reform enrolment rates in grades 6-8. To deal with this issue, we add an interaction of the year of birth fixed effect with the gross enrolment rate in 1996-1997[6] in the childhood region prior to education reform. This

requires three vaccinations at birth, second and six weeks of age. Intake of the third dose of these vaccines represents the completion of the course of immunisation necessary to be protected against these diseases.

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

[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).

[6] 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 was obtained from the Ministry of

controls for the potential link between the intensity of the implementation of the education reform, and the enrolment rates before the reform and other unobservable factors related to these enrolment rates.

3 Empirical Strategy

The December 1995 election in Turkey resulted in the victory of a religious Welfare Party. On 28 June 1996, the Welfare Party formed the first government in Turkey's modern history that was led by an Islamist party. On 28 February 1997, the National Security Council, dominated by the military, forced the government to resign as its religious orientation was seen as a threat to democracy and secularism. 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 prevent the expansion of religious education, as it effectively closed down the lower secondary (grades 6-8) parts of religious schools which were not in compliance with the state curriculum.

Figure 1 demonstrates that there was a decreasing trend in the gross enrolment rates prior to the reform. The reform led to a substantial increase in overall gross enrolment. To meet the additional demand for school places, 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). This necessitated increasing the budget for primary school construction by 30 percent from1996-97 to 1997-98 education years.[7] The first cohort affected by this change were the children who started the fifth grade in the 1997-98 academic year. Since school

National Education's National Education Statistics. The school-aged population in 1996 was based on the censuses conducted by the Turkish Statistical Institute in 1990 and 2000.

[7] Source: Statistical Yearbook on Public Expenditure from 1996/1997 to 1997-1998 education year, Turkish Ministry of Development.

enrolment in Turkey is determined according to calendar years, rather than schooling years,[8] children born in or after 1987 (aged 10 or less in 1997-98) were affected by the education reform whereas those older (aged 11 or more) were not.[9]

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 the innate ability and other individual characteristics, are not likely to be linked to the year of birth.

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 employ the intensity of the education reform as the difference in additional expenditure on classroom construction per 1000 children between 1997-98 and 1996-97 education years in the childhood region of the mother.[10] Since the identification assumption requires that individuals affected by the education reform experienced a higher intensity of construction expenditures, the intensity is

[8] 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.

[9] However, the implementation of the age cut off was not strict: some children 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 pupils born in 1986 could have been subject to the education reform, potentially contaminating the results. Therefore the 1986 cohort was excluded from the estimation as a robustness check. Excluding this cohort yielded results which were not materially different.

[10] The public expenditure figures are based on information from the Turkish Ministry of Development`s 1996 and 1997 statistics yearbooks and are adjusted for inflation. 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. 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.

required to be conditionally random. At first glance, the condition for the identification assumption seems to be satisfied. 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).[11]

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. The schooling decision of the individuals can be estimated with the standard difference-in-difference methodology, utilizing the 2008 survey, as follows:[12]

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

where S_{ijt} denotes the educational attainment of mother *i* who lived in the childhood region *j* and was interviewed for the TDHS survey in year *t*. As indicated previously, there are two education variables, years of education and a dummy for completing 8 years of formal education. The $young_i$ variable equals one for the treatment group and zero for the control group, while $intensity_j$ captures 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 fixed effect for the region in which the woman lived most of her life until age 12; and finally, the remaining control variables are represented by X_{ijt}. These are ethnicity, the interaction

[11] In order to eliminate potential bias, we account for the potential unobserved time-invariant impact of childhood environment on the distribution of additional spending on classroom construction across regions by controlling 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.

[12] This model was constructed similarly as in Duflo (2001).

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 captured ed 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 and the reform was exogenous.

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 treated and untreated women of the same age would be biased. The difference-in-difference methodology (DD) cannot account for the impact of age on the outcome of interest when treatment is determined by age. For that reason, we use the difference in difference in difference (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. It does so by controlling 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 implement the DDD method, we utilize data on mothers aged 18 to 29 both in the 2003 and 2008 TDHS surveys.[13] These are again divided into two sub-groups: young (aged 18-

[13] 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.

21) and old (aged 22-29).[14] As a result, the DDD methodology can be applied in this setting with the combination of 2003 and 2008 TDHS as follows:

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

In the above regression, β_j stands for the age fixed effect; $\omega_l$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 birth cohorts. Controlling for the birth year is supposed to meet DDD assumption as 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 reform. However, age fixed effect 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. Finally, θ_t captures the impact of the reform intensity on the education of young mothers aged 18 to 21 who participated in TDHS 2008.

Moreover, the effects of maternal education on childhood immunisation in an OLS setting could be restated by modifying the above DDD equation as follows:

[14] Descriptive statistics regarding education variables allow us to make a few important observations (see Table 2). The differences between the young and old cohorts in 2003 and 2008 are 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 old cohort obtained significantly more education than the old cohort in the 2003 sample (difference of 1.5 years and 30 percentage points).

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

where Y_{ijt} is a dummy variable which equals 1 if children were vaccinated against DPT or Hepatitis B3 and 0 otherwise. OLS estimates of δ might be biased because it is possible that schooling is correlated with the error term. 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 DDD estimates in equation (2) capture the effects of the CEL on maternal education. The triple interaction term $young_i * intensity_j * 2008$ from equation 2 then can be used as the instrument for the schooling of mothers, so as to obtain unbiased estimates of the effect of education on the outcome considered.

Our final model therefore is as follows:

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

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, we use 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 the marginal treatment effect irrespective of whether the dependent variable is 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; Dinçer et al., 2014; Güneş, 2015; Osili & Long, 2008). OLS and 2SLS strategies are thus used to estimate structural equations in this study.

Finally, a modification of equation 2 (the first stage regression) yields reduced form (RF) estimates where the education outcome of interest in the first stage is replaced with vaccination status as follows:

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

In all regressions that we estimate, standard errors are clustered for the 26 regions of childhood. A problem might arise if the number of clusters is less than around 42-50, so that the null hypothesis may be rejected even when it is true (Angrist & Pischke, 2009; Bertrand, Duflo, & Mullainathan, 2004). However, Cameron, Gelbach, and Miller (2008) 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. Therefore, it is likely that the number of cluster is enough to obtain reliable estimates.

4 Results and Discussion

First Stage 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 (the instrument) is more than 10 for almost all specifications. This indicates that the instrument is strong (Staiger & Stock, 1997). Also, 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 of 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, an 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.

As explained before, our analysis compares young mothers between the ages of 18 and 21 in the 2003 and 2008 TDHS with those aged 22-29 in the same surveys. The young mothers in the 2008 TDHS were exposed to the reform. To test the validity of our methodology, we now replace the three-way interaction terms in equation 2 with 12 separate dummy variables, 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 4). In contrast, the coefficients are statistically significant and positive for women aged 18-21.[15]

Starting with the reduced form (RF) estimates, the results in Table 5 indicate that as a consequence of the CEL, there is between 4 and 8 percentage point increase in the probability of the third (last) dose of DPT and Hepatitis B being administered, respectively for the cohorts affected by the education reform. Turning to the OLS coefficients, an additional year of maternal education is associated with 1.3 and 1.4 percentage point rise in the likelihood of complete immunisation status of infants for DPT3 and Hepatitis B3, respectively. Completing 8 years of formal schooling results in

[15] We also run a logistic regression to estimate results presented in Table 4, and the marginal effects from the logistic regression shows similar effect of the CEL for each age.

an increase in the probability of vaccination of around 5% for DPT3 and 7% for Hepatitis B.

The IV estimates similarly indicate a positive and significant causal effect of maternal education on vaccination status.[16] 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. Completion of 8 years of schooling increases the probability of receiving the third dose of these vaccines by 55% growth for DPT and by 92 % for Hepatitis B. Hence, maternal education has a strongly positive significant effect on their children's vaccination rate.[17]

It can be seen from Table 5 that the magnitude of IV coefficients are several times larger than OLS. This can be attributed to the following reasons. Firstly, the IV technique addresses the endogeneity of education whereas OLS does not. Secondly, according to Imbens and Angrist (1994), the IV coefficients capture the Local Average Treatment Effect (LATE), which is the marginal effect of education on the dependent variable for those who altered their schooling choice due to the reform. It is assumed that because of higher marginal cost of schooling, those people would choose lower years of schooling in the absence of the reform. In additıon, assuming higher return to schooling for these individuals, the IV estimates will result in larger coefficients. Having said that, we can still obtain reliable coefficients for the causal correlation from maternal education to immunization outcomes for that sub-population (Imbens & Angrist, 1994).

[16] Similar to Currie and Moretti (2003), we re-run Model 4 for the sample of first time mothers. The findings are still robust to removing higher parity mothers. The results are available upon request.
[17] In an unreported regression, we add additional controls to test the robustness of the IV estimates. To do this, we add the age at first marriage and birth, husband`s and respondent's labour force status, household size, women's attitudes towards gender equality and domestic violence, and the wealth index of the family. Even after controlling for these covariates jointly or separately, the IV estimates are robust and similar to those in Table 6. Both IV and DDD results are robust to estimating the results with a more balanced sample of women in the treatment and control groups (i.e., 4 young cohorts and 4 older cohorts).

5. Conclusions

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 led 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 investigate the causal relationship between maternal education and the intake of the third (last) dose of the Hepatitis B and DPT. 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 between two periods, 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. These results seem to be very robust to changes in the regression definition and controlling for individual and community level variables.

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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

Table 2. Descriptive Statistics

	TDHS 2003				TDHS 2008			
	Young aged 22-29		**Old** aged 18-21		**Young** aged 22-29		**Old** aged 18-21	
Description of Selected Variables	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean
Variables								
Children immunized against DPT3	1918	0.624	316	0.520	952	0.821	194	0.810
Children immunized against Hepatitis3	1918	0.491	316	0.362	953	0.788	194	0.772
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
Other		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

Table 3 The impact of the Compulsory Education Law on Formal Schooling-DDD analysis (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

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

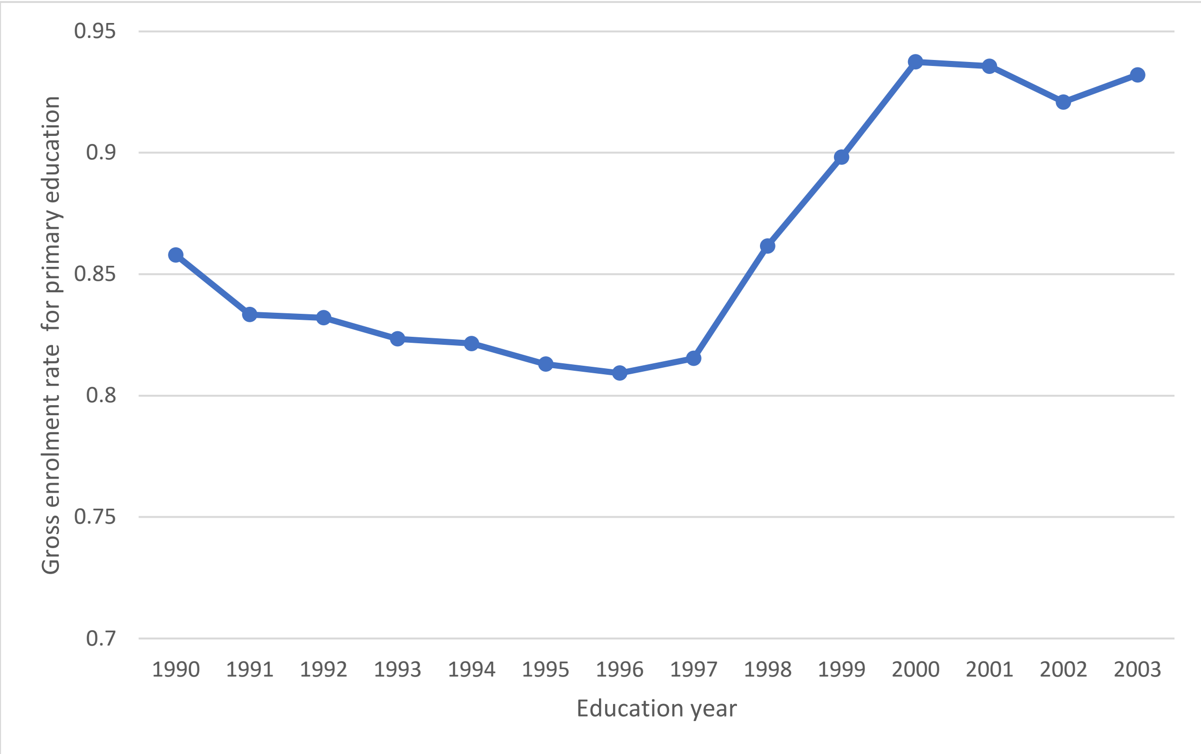
Table 5 The Causal Impact of Education on the Complete Vaccination Status of children aged over 6 months

	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.

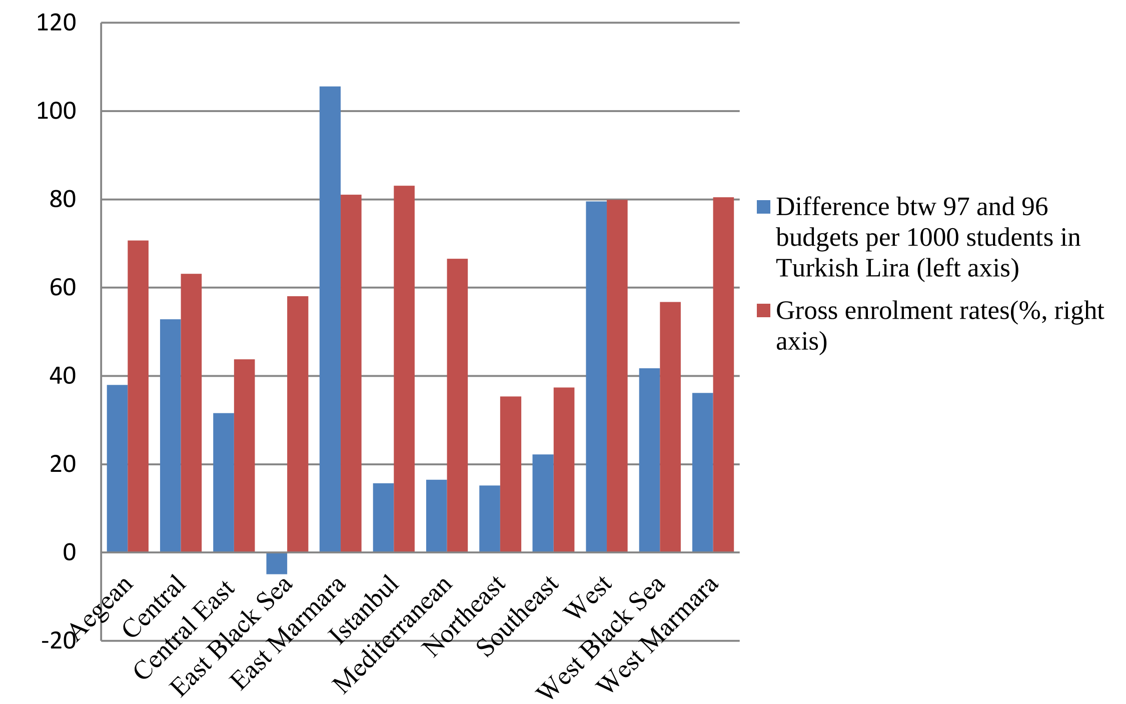
*** p<0.01, ** p<0.05, * p<0.1

Figure 1. The trend of gross enrolment rate in 8-year primary schools in Turkey



Notes: Gross enrolment rate is calculated by dividing the number of students in grade 1 to 8 with the relevant population in that age group (i.e. aged 6-13); 1990/1991-1996/1997 school years are before to the introduction of the CER. For these years, the number of students is calculated as the total of the students in the 5-year primary school and 3 years junior high school. The graph is calculated by MONE statistical data, between 1990/91 and 2003/04. Each point on the line represents the enrolment rate for that school year and the years are academic, so that the 1990 figure corresponds to the 1990/91 academic year.

Figure 2. The variation in gross enrolment rates and investments in infrastructures



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.