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THREE ESSAYS ON RETURNS TO EDUCATION, HEALTH AND THE SPATIAL WAGE CURVE IN TURKEY

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Abstract

This dissertation comprises three papers on wage and health returns to education, and spatial wage curve in Turkey. The second and third chapters make use of the exogenous variations generated by the 1997 Eight-Year Compulsory Schooling Law and the accompanying middle-school class openings to estimate economic and health returns to education. The findings suggest that the reform and the intensity of the reform substantially increased the educational attainment. The second chapter investigates the wage returns to education. Results show that one additional year of schooling increases individual wages by around 9 percent. The third chapter examines the causal impact of education on health outcomes and health behavior. The estimates indicate that there is no statistically significant effect of education on health outcomes. The final chapter focuses on the spatial wage curve in Turkey. Results show that individual real wages are more responsive to the adjacent regions' unemployment rates than the local unemployment rates. Further investigation reveals that the wage curve estimates are sensitive to the use of group-specific regional unemployment rates.

THREE ESSAYS ON RETURNS TO EDUCATION, HEALTH AND THE SPATIAL WAGE CURVE IN TURKEY

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Contents

List of Figures

List of Tables

Chapter 1

Three Essays on Returns to Education, Health and the Spatial Wage Curve in Turkey

This dissertation is a collection of three essays on health and earning effects of education and the spatial wage curve in Turkey. Chapter 2 and Chapter 3 analyzes the causal impact of education on individual hourly wages and health outcomes using compulsory schooling change in 1997. Chapter 4 investigates the spatial wage curve in Turkey.

The compulsory schooling increased from five to eight years in Turkey in 1997. The law change is accompanied by a massive investment in the construction of schools/classrooms and recruitment of teachers to accommodate the expected increase in the middle-school enrollment. These investments varied substantially across regions based on the pre-reform enrollment profiles. The timing and implementation of the 1997 reform provide a plausibly exogenous variation in educational attainment that can be used to identify the causal effect of education on the labor market and non-market outcomes. Chapter 2 and Chapter 3 make use of these changes to identify the effect of education on individual hourly wages, and health outcomes such as weight problems, smoking behavior, self-rated health, infant's birth weight, etc.

Chapter 2 first examines the effect of the 1997 eight-year compulsory schooling law and the accompanying middle-school class openings per 1000 children on educational attainment in Turkey. This chapter uses the Turkish Household Labor Force Survey (THLFS) data. The results indicate that the 1997 reform increased the educational attainment by 0.77 years and the probability of completing eight years or more of schooling by 19 percent. In addition, one additional middle-school class opening per 1000 children raises the treated cohort's schooling by 0.19 years and the probability of completing middle school or high education levels by 4 percent. The results based on the gender disaggregation suggest that the reform and the accompanying middle-school class openings were more effective in increasing educational attainment of females than males. Further analyses show that one additional year of schooling is associated with a 5-percent increase in individual wages. Using the variation generated by the 1997 reform as an instrument for schooling, results indicate that one-year education increases wages by 10 percent for individuals who were born in 1988-1990. Besides, using the regional and cohort variation generated by the middle-school class openings associated with the reform as an instrument, the results suggest economic returns to education ranging from 5.8 percent to 9 percent for the first three treated cohorts.

Chapter 3 reconsiders the impact of the 1997 reform and the accompanying middle-school classes on educational attainment using two different data sets, the Turkish Health Survey and the Turkey Demography and Health Survey. The results corroborate the effects of the reform and the intensity of the reform on schooling. The results suggest that completing middle school or higher education levels is associated with a 2-percent decrease in the body mass index (BMI) or in the probability of being obese/overweight, a 5-percent increase in the probability of quitting smoking, a 10-percent increase in the probability of reporting good health. Examining the effect of maternal education on infants' birth outcomes, the findings show that one additional year of maternal education increases birth weight of infants by 0.4 percent, and decreases the probability of giving birth to a low-birth-weighted baby by 1 percent. Using the variations created by the 1997 reform and the accompanying middle-school classes as an instrument for schooling, the results suggest that there is no significant impact of education on health outcomes of individuals who were born in 1988-1991, except for a weak positive effect on BMI.

Chapter 4 studies the spatial wage curve in Turkey. This chapter uses the THLFS data for the years between 2004-2013. First, the Turkish wage curve is revisited with a longer period.

Using a fixed effects two stage least squares (FE-2SLS) estimation method and instrumenting regional unemployment rates with its one-year lagged values, the results indicate that the unemployment elasticity of real wages is -0.07. Second, the spatially weighted regional unemployment rate is included in the model besides the regional unemployment rate. The spatial FE-2SLS results show that the own-region unemployment elasticity of real wage is -0.056, and the spatial spillover unemployment elasticity of pay is -0.087. Lastly, using group-specific unemployment rates for the disaggregated subsamples based on gender, skill level and age groups, results indicate that there is neither wage curve nor spatial spillover for females. In addition, there is own-region of unemployment rate elasticity of -0.05 and the spatial spillovers unemployment elasticity of -0.06 for males.

Chapter 2

Returns to Education in Turkey: Evidence from the 1997 Basic Education Law and New Class Openings

2.1 Introduction

The relationship between education and earnings is one of the most studied topics in labor economics since the seminal works of Gary Becker, and Jacob Mincer's specification. A vast literature is devoted to the estimation of economic returns to education for different samples and different time periods. It is well documented that education is one of the most important components determining individual earnings. Psacharopoulos (1985, 1994), and Psacharopoulos and Patrinos (2004) review estimated returns to education for more than 90 countries. They conclude that the average rate of return is around 10%, women have higher returns to education than men, and low/middle-income countries experience higher returns than high-income countries.

Despite the popularity of the basic Mincerian wage equation, potential endogeneity of schooling cast doubt on the causal link between educational attainment and individual wages. The instrumental variables (IV) method, in which schooling is instrumented by plausibly exogenous variable(s), has been commonly used to draw inference on the causal relationship between education and income. Since Angrist and Krueger (1991), numerous studies have employed interventions on the supply side of the education market such as compulsory schooling laws, availability of the schools in a given area as an instrument for schooling. In general, the IV estimates are found to be at least as high as the OLS estimates of returns to education.

The compulsory schooling has been increased twice in Turkey, which is an OECD-member country with low level of average schooling in the last two decades. In 1997, compulsory schooling raised from five to eight years, followed by a further increase from eight to twelve years in 2012. Despite the significance of the research on the causal relationship between education and earnings in developing countries which can serve as a guideline to government policies (Psacharopoulos and Patrinos, 2004), the literature on returns to education in Turkey is somewhat limited. The existing literature on the returns to education in Turkey is mostly based on the OLS estimations. To our best knowledge, there is no published paper on the causal relationship between education and wages, although several attempts have been made.

The compulsory schooling changes in 1997 and 2012 provide exogenous variations in schooling that can be used in estimating the causal impact of education on individual wages. However, it is too soon to observe the effect of the 2012 law. Thus, we focus on the 1997 Basic Education Law which came into effect during a highly unstable political and economic era. The 1997 law increased the compulsory education from 5 to 8 years and abolished the primary school diploma. It was followed by a massive investment in the construction of new classrooms and recruitment of teachers to meet the expected increase in enrollment in the 6th, 7th, and 8th grades in a differential rate across regions. This change, which was mostly driven by political concerns rather than economic incentives, had a large effect on the years of education attained in Turkey. This plausibly exogenous shock on schooling, along with the regional variation in new middle school class openings creates an opportunity to lift the veil on the causal relationship between education and individual wages.

In this study, we analyzed the effect of the 1997 Basic Education Law and accompanying middle-school class openings on educational attainment. Then, we used these exogenous variations to estimate the causal relationship between education and individual wages. We estimate the returns to education in Turkey using Turkish Household Labor Force Survey (THLFS) for the years 2009-2014. We found that the reform increased the educational attainment of the individuals who were born between 1988-1990 by 0.77 years. Moreover, one additional middle-school class per 1000 children increased the schooling attainment of the first three treated cohorts by 0.19 years. We find evidence that the reform and the accompanying class openings were more effective in increasing women's schooling than men's. Furthermore, instrumenting the schooling variable by new middle-school classes per 1000 children, we found that the return to one year of education in Turkey is around 9 percent.

The rest of the paper is organized as follows. In Section 2, we present previous literature. In Section 3, we introduce the Turkish education system and the 1997 Basic Education Law. The data set and samples that are used in our analyses are presented in Section 4. In Section 5, we discuss the model and the identification strategy. In Section 6, we estimate the effect of the 1997 Law and the accompanying class openings on educational attainment. In Section 7, we present the returns to education estimates. We provide closing remarks in section 8.

2.2 Previous Literature

It has been long realized that education is endogenous in Mincerian wage equation, such that the estimated coefficient of education in the OLS estimation does not show the causal relationship between education and earnings. The sources of bias in the estimated rate of return include ability bias, discount rate bias, measurement error bias, and heterogeneity in the rate of returns to education. These biases were addressed by different methods, such as the instrumental variables and fixed effects estimation with twins'/siblings' data or longitudinal data over the same individuals. Most of the literature find evidence of a downward bias in the OLS estimates, which indicates that the actual rate of return would be higher than those estimates obtained by using the OLS. Griliches (1977) surveys the early returns to education studies and the concerns related to the reported estimates. He introduces the ability variable explicitly in the earnings equation, and points out that the bias attributable to ability is indeed small. In fact, it may even be dominated by other sources of bias which results in a downward bias in the OLS estimation.

Since the pioneering study of Angrist and Krueger (1991), using supply side variables to instrument the endogenous variable is one amongst the most popular approaches to draw a causal effect of education on individual earnings. Angrist and Krueger (1991) use the quarter of birth as an instrument for education, observing that people born in the early period of a year, leave school with less education because of minimum school-leaving-age laws. They find that the OLS and IV are close to each other, except in some cases in which the OLS estimates are smaller than IV estimates. Their findings corroborate the conclusion of Griliches (1977). However, a recent study by Stephens and Yang (2014) tests the possibility of the different trends across states, which could affect different birth cohorts, including cohort-state fixed effects. They show that the IV estimates become insignificant or wrong-signed. Moreover, the instruments that are used in Angrist and Krueger (1991) have been criticized for being weak (e.g. Bound, Jaeger, and Baker, 1995; Staiger and Stock, 1997).

However, the idea of using compulsory schooling law has attracted many researchers to implement similar strategies. The studies using the compulsory schooling laws in other developed countries as an instrument corroborate the findings that the IV estimates exceed OLS estimates. For a detailed review see Card (2001). It is also noted that the estimates of returns to education based on schooling reforms, reflect the marginal rate of return for the group that is affected by the reform. The group typically consists of individuals who have a higher marginal cost of education. Thus, the OLS estimates of the returns to education, which provide an estimate of the average returns, and the IV estimates are not necessarily comparable. The IV estimates based on institutional changes that affect a group of individuals are labeled as local average treatment effect (LATE) (e.g. Imbens and Angrist, 1994; Angrist and Imbens, 1995; Angrist, Imbens, and Rubin, 1996).

Harmon and Walker (1995) use the changes on the minimum school-leaving-age law in Britain, taking place in 1947 and 1973, as an instrument for the schooling variable. They find that the IV estimates of the returns to education, 15%, considerably higher than the corresponding the OLS estimates, 6%. Later, Harmon and Walker (1999) reconsider the returns to education using only the 1973 law change as an instrument along with some other instruments considering the criticism of Card (1999) and find similar results with their earlier study. Moreover, Oreopoulos (2006) uses the minimum school-leaving age laws of the United Kingdom, Canada, and the United States in a regression discontinuity framework. He assumes that the local average treatment effect (LATE) approaches to the average treatment effect (ATE) since the influenced population approaches the whole population. He concludes that the causal effect of education on earnings is high, around 14-15%, for the UK. On the other hand, Devereux and Hart (2010) use the same approach with more accurate data on earnings for the UK and find no evidence of a downward bias of the OLS estimates. Pischke and von Wachter (2008) utilize the compulsory schooling changes in West German states and find zero returns to the compulsory schooling changes. More recently, Fang et al. (2012) use the compulsory schooling law enacted in 1986 in China. They find that the IV estimates of the rate of return are around 20%, a rate considerably higher than the OLS estimate of 9%.

Another strand of literature exploits the exogenous changes in the cost of education that, in turn, affect marginal returns to education of some groups. These include the number of universities available in the municipality of the individual (Card, 1993), distance to a nearby college (Kane and Rouse, 1995), or living in a university city (Conneely and Uusitalo, 1998). The instruments based on the availability of schools mostly consider higher education institutions in developed countries. The school expansions targeting lower levels of education in developing countries started to be used in the literature, recently. Duflo (2001) uses a boost in primary-school construction between 1973 and 1978 in Indonesia, to evaluate the effect of the increase in the number of primary schools on years of education attained. Consequently, she estimates the effect of the education on individual wages. She uses the number of the primary schools that are constructed between 1973 and 1978 per 1000 children to measure the intensity of the reform. Duflo (2001) points out that exposure of an individual to the program is jointly determined by the birth year and birth region. Exploiting variation in the number of schools by region and birth cohort, she finds that, one additional school constructed per 1000 children increased completed years of education by 0.12 to 0.19 years. Furthermore, using the number of constructed primary schools interacted by the birth cohorts as an instrument, she finds that returns to education in Indonesia are between 6.8% and 10.6%. She concludes that the IV estimates of returns to education in Indonesia are either close to the OLS estimates or not significantly different than each other. Clark and Hsieh (2000) use an increase in the compulsory schooling from 6 to 9 years in Taiwan in 1968, along with the regional variation in new junior high schools as an instrument for schooling to examine the impact of education on individual wages. They find that one new school per 1000 children aged 12-14, increased years of education of children aged 6-11, in 1968, by 0.6 years. Moreover, they use these exogenous variations in schooling to estimate the causal relationship between education and wages. They find that either the IV estimates are smaller than or not statistically different from OLS estimates. They use region of residence in their analysis because of unavailability of the individuals' region of education which may introduce some degree of bias.

There are two other papers that are closely related to our identification strategy. They use compulsory schooling and school construction as instruments for analyzing the causal relationship between education and non-pecuniary outcomes in developing countries. Breierova and Duflo (2004) use the same instrument as Duflo (2001) for education in analyzing the effect of education on fertility and child mortality. Chou, Grossman, and Joyce (2010) use the compulsory schooling law and regional variation in junior high school construction in Taiwan in 1968 to study the effect of parental education on child health. They construct their instrument, the program intensity, as the number of junior high school per 1000 children aged 12-14 in the year that a cohort enters junior high school. They find that an increase in the number of junior high schools increased female and male education around the same amount. Using program intensity as an instrument for parents' education, they find that parental education effects infant's well-being, but the IV estimates are not statistically different than OLS estimates.

Previous studies on Turkey

The estimates of schooling returns in Turkey, mostly obtained by employing OLS and using cross-sectional surveys provided by the Turkish Statistical Institute (TURKSTAT), are somewhat mixed. Some studies deal with the selectivity problem while most of them provide the OLS and the MLE estimates for different levels of education for several disaggregated samples. Some papers compare the returns to education at different points of time. Other papers provide some evidence on the effects of education on wage inequality. Most of the papers employ the basic human capital model using education and a quadratic in the potential experience in their estimation and find high returns to education. However, the endogeneity of the schooling and the bias in the OLS estimations are mentioned in the literature, to our best knowledge, there is no published paper dealing with this problem in the case of Turkey.

The very first study on returns to education in Turkey is done by Anne Krueger (1972). She uses two surveys which are conducted in 1968 by the Turkish Association of Metal Manufacturers and the US Military, and 1965 Census, on the number of students at each level of education and the average income by occupation and schooling level in her study. She provides some descriptive evidence that the private returns increase by the school-level and emphasizes that the private returns to the college education are quite high, around 25%. She concludes that these high private returns might be result of the very low cost of higher education.

On the other hand, Tansel (1994) is the first study that uses a Mincerian wage equation to estimate the returns to education. She also use Heckman's selection model to estimate the effect of education on the probability of participation in wage employment in Turkey using the 1987 Household Income and Consumption Expenditure Survey (HICES). She finds that the probability of participation in employment increases by education level for both urban men and women. This effect is higher for women than men. She also finds that returns to one additional year of schooling increases with the level of education for men with the magnitudes, 1.8%, 8.8%, 8.2%, and 16% for primary school, middle school, high school and university levels, respectively. Female workers with middle school or university levels have higher returns to one-year education, 19%, and 13.4%, respectively. Moreover, she finds that the return to one year of education in vocational high school is higher than general-curricula high schools. This contrast to the findings in developed countries. Tansel (2010) re-examine returns to schooling and compares for different years by using the Household Budget Survey (HBS) for the years 1994, 2002, 2003, 2004, and 2005. She estimates OLS and Heckman two-step corrected rate of returns for each year separately. She finds that selectivity bias is not a problem for men, but the Heckman two-step estimates are higher than the OLS estimates for women. However, it is unclear what are the selection variables that are used in the Heckman two-step model. Furthermore, she concludes that returns are declining between 1994 and 2004 for men and women. Further evidence on the relationship between education and wages shows that the rate of return is in the range of 4%-15%, returns are increased from 1980's to 2000's, and returns to education are higher for female workers than male workers (Vural and Gülcan, 2008; Güriş and Çağlayan, 2012; Filiztekin, 2011).

There are a couple working papers addressing the endogeneity of education in the wage equation in the case of Turkey. Using the THLFS for the years 2011 and 2012, Mocan (2014) employs the 1997 Basic Education Law as an instrument in estimating returns to education in Turkey. She finds that the 1997 law has a significant effect on increasing educational attainment in Turkey and IV estimates of the returns to middle school completion exceed OLS estimates. Mocan (2014) finds that return to one-year education for women is 14 percent. There is no significant effect of education on wages for men. She considers only full-time workers. Her findings based on a sample in which the highest completed degree is high school. However, she excludes the vocational high school graduates which is equivalent to the general curriculum high schools. She argues that the compulsory education reform has a spillover effect on general curricula high schools excluding vocational high school and college graduates. However, this conclusion should be taken with caution because the Higher Education Council (YÖK) put some restrictions on vocational high school graduates in the university entrance exam since 1999 (OSYM, 1999), making vocational high schools undesirable to attend. We discuss these restrictions in further detail in section 3. These restrictions more likely shifted high school enrollments toward general-curricula high schools from vocational high schools. Therefore, the variation in the treated and control groups in their high school enrollments might have been resulted from the new policy of YÖK rather than the 1997 compulsory schooling law. Consequently, an analysis excluding vocational high school graduates would find a spurious increase in general-curricula high school graduates, a finding mistakenly attributed to the 1997 compulsory schooling law.

Aydemir and Kirdar (2015) estimate the causal effect of years of schooling on wages using THLFS for the years 2002-2013. They instrument years of schooling by a treatment dummy variable indicating exposure to the 1997 compulsory schooling reform. They restrict their sample to full-time workers aged 18-26 in the survey year and find that the reform significantly increased the completed years of schooling of Turkish men and women. Their 2SLS estimates show that there is no statistically significant effect of education on wages of men, while there is a significant effect of education on female wages by 3.8%. This might be driven by the fact that their identification is based on the comparison of 10 birth cohorts on each side of the treatment and their sample is restricted based on age rather than birth cohorts.

Furthermore, there are two papers related to our identification in which non-monetary returns to education are examined. Gunes (2015) estimates the effect of female education on teenage fertility and infant outcomes using 1997 compulsory schooling law exposure and regional variation in the additional classrooms constructed. She uses additional classrooms built between the 1996/1997 and 1997/1998 school years to construct an intensity measure for the exposure to the reform. She claims that 58,726 additional classrooms were built between the 1996/1997 and 1997/1998 school years. However, the source of the data is not given clearly in the paper. She finds that the compulsory schooling reform increased mothers' probability of completing 8-year primary school by around 15%. Furthermore, she finds that mothers' completion of 8 years of schooling improves infants' health, reduces teenage fertility and increases the age at first birth. However, it should be noted that the identification of the model is based on the additional classrooms constructed between 1996/1997 and 1997/1998 school years, and the 1997 Basic Education Law (No:4306), enacted in August 1997. The reform became effective on those who started 6th grade in September 1998. It does not seem reasonable that many classrooms were constructed in less than one month. Also, the relevant intensity measure should account for the 1998/1999 school year, the year in which the first affected group enrolled in the 6th grade.

Lastly, Dincer, Kaushal and Grossman (2014) use the 1997 Basic Education Law and the regional variation in the additional primary-school (1st-8th grade) teacher recruitments as an intensity measure of the exposure to the reform to examine the causal effect of education on women's fertility and their children's health. They find evidence that the reform increased the ever-married women's probability of completing middle school or higher degrees by 11%. Using the intensity measure as an instrument, they find some evidence that schooling reduces teenage fertility, but there is no significant effect of female education on child mortality.

2.3 The Turkish Education System and the 1997 Basic Education Law

In August 1997, the Turkish Parliament enacted the Eight-Year Compulsory Education Enforcement Law (No:4306) [1] which increased compulsory schooling from five to eight

[1] Resmi Gazete, No: 23084; August 18,1997

years.This law combined 5-year compulsory primary schools and 3-year non-compulsory middle schools into a single level of uninterrupted eight-year basic education, complemented by a secular national core curriculum.

Prior to 1997, the Turkish education system was comprised of a 5-year compulsory primary school, a 3-year non-mandatory middle school, a 3 or 4-year high school, 2- to 4-year college and graduate studies. Five years of primary school were compulsory since the establishment of the Turkish Republic in 1923. Upon completion of five years of education, students used to receive a primary school diploma. After primary school, students were free to choose whether or not to continue with their education. Although all middle schools were offering general secular education, some of them were specialized in vocational, technical, or religious studies. Students in vocational and technical schools also had the opportunity to complete industry internships during their studies. After completing middle school, students could continue to 3-year high schools in which general, vocational, technical, and religious electives were available. All three levels of education were governed by the Ministry of National Education (MONE) and were free of charge public schools. Those students who attained a high school diploma had the right to take a highly competitive university entrance exam in order to pursue a college degree. For the most part, tuition-free public universities provided a university education, with private universities established only after 1984. The government continues to be the main "educator", with large public universities still dominating higher education.

Increasing compulsory basic education from five to eight years was not a new thought, but, interestingly, its implementation was achieved at a time of unstable political and economic conditions in 1997. The National Education Council had been suggesting compulsory middle school since 1946. There were several attempts to implement this change, but the lack of buildings and teachers left these attempts unrealized. In 1961, the state mandated three-year middle school education, but deferred realizing these ambitions until physical requirements had been met. In 1973, an edict to increase compulsory basic education requirements to 8 years was issued, but the mandate was not enforced because of an insufficient number of buildings and teachers (Dulger, 2004). The 1990s was an era of Turkey's struggle with political instability and economic crises, denoted by the fact that 11 governments were formed during just that period of the young Republic's history. Primary school enrollment was high at around 96%, but middle school enrollment was only around 64% in 1996 (Dulger, 2004). However, the main motivation of the 1997 reform law was political, using education as a tool to achieve certain political aims. Wielding school reform as a tool, the secular government and military officials sought to restrict religious studies in middle schools. The original electives scheme at the middle school level permitted students to obtain religious, vocational, or technical education in addition to their secular education in vocational middle schools. Students attending these schools were able to get into any college program depending on their performance in the nation-wide university entrance exam. The increasing number of university students with a religious high school (imam-hatip) background raised secular concerns among military officials and secular bureaucrats. On February 28, 1997, the National Security Council (NSC) released a set of so-called "eighteen recommendations" to the government after a nine-hour meeting on the matter. While the memo was rolled out under the guise of a "recommendation", the military wing of the NSC made it clear that government was required to implement them (Günay, 2001). This was regarded as "post-modern military coup" (Candar, 1999). One of the "eighteen recommendations" was a mandate for continuous, secular compulsory schooling of eight years. The main motivation was to restrict religious education. The NSC meeting and subsequent actions taken by the state agencies based on the decisions of the meeting led to the resignation of the government. The subsequent three-party coalition government, along with executive and judiciary, took a series of strong actions to implement the NSC's "eighteen recommendations" and to ensure that religion in schools was restricted (For a good review of the "eighteen recommendations" and actions that had been taken by state agencies to implement those measures, see (Günay, 2001)).

The secular three-party coalition government, formed right after the military intervention to the religious-oriented government, took a revolutionary approach to replace the then current system of a five-year primary school and three-year middle school by eight-year uninterrupted basic education, with almost no public discussion. Beside the dictated "recommendations" of NSC, it was declared that the motivation for the law was to pull the hardest-to-reach students into middle school education (Dulger, 2004). Passed on August 16, 1997 in the Turkish Parliament, the Eight-Year Compulsory Education Law required all students who were enrolled in 1st-5th grades in the 1997-1998 school year, and those who would enroll in the future, to complete eight years of basic education. Although there were no penalties on families who did not enroll their children to 6th grade, the government abolished the primary school (5-year) diploma and replaced it with a primary basic education diploma, to be earned only by completing eight years of schooling. This was done to create a strong incentive for students to complete eight years of primary education.

The 1997 Eight-Year Compulsory Education Law also introduced temporary earmarked taxes in order to finance the physical needs to meet the expected increase in enrollments for grades six through eight. The Turkish Ministry of National Education (MONE) was responsible for the implementation of the law. MONE took a series of measures to meet the expected enrollment boom such as construction of new schools/classrooms, hiring new teachers, expanding the bussing system and boarding schools. The share of MONE in the public investment budget increased from 15 percent prior to 1997 to 37.3 percent in 1998, leveling out at 30 percent until 2000 (Kırdar, Dayıoğlu, and Koc, 2016). The Ministry of National Education (MONE) aimed to increase the capacity of eight-year primary education by around 3 million individuals by constructing around 100,000 new classrooms for basic education between 1997 and 2001 (Dulger, 2004). Moreover, the number of students bussed to nearby schools increased from 127,683 to 621,986 between 1996 and 2000, and the number of students in boarding schools rose dramatically from 34,465 to 281,069 between the 1996 and 2001 school years (Kırdar et al., 2016). The legislation was supported financially by several international organizations as well as by donations from the private sector. By 2001, the government spent about 3 billion dollars annually for the implementation of eight-year basic education, bolstered by financing from the World Bank, the European Union, the private sector in Turkey and the Turkish Government (Dulger, 2004).

The Higher Education Council (YÖK) took further action relating to the "recommendations" of the NSC that affected the composition of high-school enrollments. YÖK redefined the conditions of the nationwide university entrance and placement exam by discriminating against vocational high school graduates. YÖK initiated a differential multiplier for the high school GPA of students from different kinds of high schools for university placement. Vocational and technical high school graduates were restricted to continue only to programs related to the type of their vocational education. If they wanted to continue their university education in another major, they were evaluated with a negative multiplier, making it difficult to get into university from those schools (OSYM,1999). These restrictions shifted many students from vocational high schools to the general curricula high schools. National Education Statistics show that the share of students in vocational high schools decreased from 45% in 1997 to 34.8% in 2003.

2.4 Data

The data used in this paper is the Turkish Household Labor Force Survey (THLFS) for the years 2009-2014 and National Education Statistics for the years 1992-2003, obtained from the Turkish Statistical Institute (TURKSTAT). Our primary dataset, the THLFS, is similar to the US's CPS data in terms of design and coverage. TURKSTAT started to conduct these surveys in 1988 and has had two significant methodological changes in 2000 and 2004. The survey is a cross-section individual level survey hence providing individual characteristics with a large sample coverage (around 500,000 observations in each year). The THLFS covers the whole population except for non-residents, those who are living in institutions and conscripts in the territory of Turkey. Moreover, TURKSTAT, in collaboration with MONE, publishes National Education Statistics which shows the statistics on various variables including the number of students, teachers, classrooms, and classes (branches) in each grade for each level of education in detail.

The THLFS provides demographic characteristics, such as age, gender, marital status, education, and region of residence (NUTS-2 level[2]) for each individual, as well as labor force related information such as employment status, occupation, industry, hours of work, monthly income, etc., for individuals who are 15 years old or older.

Schooling is given as the highest completed degree in the THLFS. Moreover, individuals are also asked if they were currently continue education at the time of the survey. Those who continue their education were asked their level of education and current grade level. We use this information to construct two schooling variables; a continuous, years of schooling variable, and a dummy variable which indicates whether a person completed at least middle school (8 years) or not. Aydemir and Kirdar (2015) use the information on the highest degree and years of schooling completed in Turkey Demographic and Health Survey (TDHS-2008) and conclude that the years required to complete a degree is a good approximation to the years of schooling completed [3]. Following Aydemir and Kirdar (2015), we imputed years of schooling by assigning each completed level of education with the minimum required years of education to complete the given level of education. We further, refined this variable for those who continued their education by adding the completed years in the degree they currently pursue to the years to complete the degree they completed. Note that those individuals who are "literate but do not hold a degree" are considered to complete 2 years of education. This might be valid imputation for the those who are not affected by the law. However, it is problematic for those individuals in the treatment group because those who are in the treatment group and have finished 5 years of schooling but did not complete the 8th grade

[2]Turkey is divided into three levels of NUTS regions. NUTS-1 consists of 12 regions, NUTS-2 consists of 26 regions, and NUTS-3 consists of 81 provinces of Turkey.

[3]They impute years of schooling as follows: "illiterate", 0; "literate but don't hold a degree", 2; "primary school", 5; "middle school", 8; "high school (general or vocational)", 11; "any college degree or above",15.

are also coded as "literate but do not hold a diploma". Thus, assigning them by 2 year of schooling would underrepresent the actual schooling of these people in the treated group. There is no way to distinguish between those who completed 5th grade in the treatment group. Thus, to avoid this problem we assigned 5 years of schooling for all individuals in the "literate but don't hold a diploma" group.

We restrict our analyses to individuals who were born between 1983 and 1990 [4] to obtain relatively similar individuals except for their exposure status to the compulsory schooling law[5]. Furthermore, we excluded those individuals born in 1986 or 1987 because of the uncertainty of their treatment status. This will be discussed in the next section in detail. The resulting sample contains 258,131 individuals of which 135,097 are women, and 123,034 are men. This sample is used to analyze the effect of the reform and new class openings on the educational attainment. Summary statistics of selected variables for treatment and control groups corresponding to this sample is shown in Panel A of Table 2.1.

The THLFS provides labor related information for individuals who are 15 years old and above as well as demographic information of all individuals observed. Moreover, earning is reported as monthly income which is recorded only for regular and casual workers; therefore, employer, self-employed, and unpaid family workers are excluded from the sample. Furthermore, individuals who started to work in the survey month or did not report their monthly income are also excluded from the sample. The reported income is monthly take-home pay, that is, income after deduction of taxes, compulsory social security, and other life insurance premiums [6]. Working hours are reported as usual hours of work per week and the actual hours worked during the reference week. If an individual had a job but was not at work during the reference week, the actual hours worked is coded as 0. Thus, there are missing values

[4]All students who started high school in 2005/2006 school year or later are required to attain 4 years of high school education (MONE Board of Education, Decree No: 184, June 7, 2005). Thus, high-school graduates who were born after 1990 attained 12 years of education. This is another reason to restrict our sample to 1983-1990 birth cohorts

[5]We used 1981-1992 birth cohorts to show the trends in the middle school completion rate in Figure 2.1.

[6]Monthly income includes bonuses except in 2011,2012, and 2013 surveys. Bonuses are recorded separately for those years, so we added bonuses on monthly income.

for the actual hours worked. Also, there is a slight difference between these two measures of hours worked. Hence, we used usual hours of work per week in our analyses.

We used monthly income and usual hours of work per week to calculate hourly wages. All wages are deflated to 2003 liras using the consumer price index (CPI) for each NUTS-2 level region provided by TURKSTAT. The upper and lower 1% of the distribution of the wages for each year are trimmed in order to exclude the influential observations. The resulting sample includes 89,413 workers of which 26,488 are female, and 62,925 are male workers. This sample is used to estimate the returns to education. Summary statistics of selected variables for treatment and control groups corresponding to this sample are presented in Panel B of Table 2.1.

The THLFS provides the region of residence at the NUTS-2 level. The region of residence is used to match the individual level data with region-level data. The regional data on the number of middle school classes (6th-8th grades) is extracted from the National Education Statistics books for years 1991-2003. TURKSTAT, in collaboration with MONE, releases data on the number of students, the number of teachers, the number of classes in each grade, and the number of schools at each level of education at the province level for each school year in the National Education Statistics books. We extracted the province-level number of classes and number of students in 6th, 7th, and 8th grades for years 1991-2003. Furthermore, we aggregated the province-level data into 26 NUTS-2 level. Moreover, we used 1990 and 2000 Population Censuses and the 2007 Address Based Population Registration System in order to obtain the population estimates of children aged 11-13 by province and year. The population for the inter-census years is interpolated assuming a linear trend [7].

[7] The boundaries of provinces have changed several times since 1990. Eight districts have broken off from the provinces that they were in and became separate provinces (Bartın, Ardahan, Iğdır, Yalova, Karabük, Kilis, Osmaniye, Düzce) from 1990 to 2001. The regional level data on the population, the number of classes and the number of students are adjusted to the boundaries in 2001 in each year.

2.5 The Model and Identification Strategy

2.5.1 The Model

Following the previous literature (Card, 1995; Harmon and Walker, 1995), we consider a two-equation system of wage and schooling that we estimate using ordinary least squares (OLS) and two-stage least squares (2SLS).

$$log(w_i) = \beta_1 S_i + \beta_2 X_i + u_i \tag{2.1}$$

$$S_i = \gamma_1 T_i + \gamma_2 X_i + \epsilon_i \tag{2.2}$$

where log of hourly wage rate, $log(w_i)$, is determined by schooling, S_i, a vector of observed individual characteristics, X_i, and an unobserved error component u_i. The coefficient of interest in this paper is β_1 which represents the returns to education. Estimation of equation (2.1) by OLS results in bias estimates of β_1 because of the endogeneity of schooling arising from unobserved ability, discount rate, heterogeneous marginal returns or measurement errors (e.g. Card, 1995; Harmon and Walker, 1995; Duflo, 2001). One can account for this endogeneity by employing an instrumental variables (IV) method if there is a variable, T_i, that can be legitimately excluded from equation (2.1). That is, schooling can be predicted by equation (2.2) then the predicted schooling can be used in equation (2.1) to estimate the causal effect of education on wage rate.

The use of plausibly exogenous variations in schooling such as compulsory schooling laws, availability of schools in a region, regional variations in school construction, etc., as an instrument for education has gained popularity in the literature due to Angrist and Krueger (1991). Returns to education estimates using an instrumental variables method in which schooling is instrumented by an exogenous shock reflect the average returns of a group of people who attained more years of schooling as a result of the shock rather than average returns to education of the whole population (Card, 1995). In other words, the IV estimate of β_1 represents local treatment effect (LATE) rather than average treatment effect (ATE).

2.5.2 The Identification Strategy

The identification of our model is based on the 1997 Basic Education Law and the variation in new middle school classes (6th-8th grade) openings. The birth year of children randomly assigns individuals into treatment and control groups. The compulsory schooling law mandates individuals to complete at least eight years of education. Thus, individuals in the treatment group are induced to attain more years of education than individuals in the control group. Therefore, the reform creates an exogenous variation in schooling across different cohorts. We use this exogenous variation as an instrument for the schooling in our analyses.

Furthermore, note that the new mandate changes schooling behavior of those who would have been dropped right after the 5th grade in the absence of the reform. The dropout rate after the compulsory years of schooling varies across regions of Turkey, possibly because of the insufficient schools or teachers. This group was the target of the reform. The Turkish government took an immediate action to construct new schools/classrooms and recruit new teachers in order to meet the expected increase in the enrollment, which varied across regions (see Figure 2.2). These investments are meant to serve as a means to reduce the marginal cost of schooling of individuals and to induce them to complete eight years of schooling. Therefore, the exposure to the reform is determined by the number of classes available in the region to attain a middle school education and the year in which a student decides whether or not to enroll in the 6th grade. If there are more new middle-school classes opened thus increasing the stock of classes in a region, it is expected that the number of students enrolled in the 6th will be increased. In short, there is a variation in schooling across birth cohorts that is created by the schooling mandate. Also, there is a regional variation in the intensity of the exposure to the reform that is resulted from the new schools/classrooms and teachers. We use this exogenous variation to construct another instrument for the schooling.

2.5.3 The Treatment Dummy Variable

The Eight-Year Compulsory Basic Education Law (No: 4306) was enacted on August 16, 1997 and affected the individuals in the 5th or lower grades in the 1997/1998 school year. Thus, the first group who are mandated to complete eight years of formal schooling was 5th graders in that school year. In other words, students enrolled in first grade in the 1993/1994 school year or later were affected by the 1997 eight-year compulsory education law. Here, we need to refer to the primary school-entry age regulations in Turkey to be able to determine the treatment status of individuals in the data. The school entry age is determined based on the calendar year rather than the school year in Turkey. A regulation of Ministry of National Education in 1992 states, "...children who complete 72 months of age at the end of the calendar year will enroll in first grade in that school year. Upon written request of the family, the children who meet the age criteria but are physically underdeveloped can continue in the kindergarten for one more year"[8] (Resmi Gazete, August 7,1992). Thus, individuals born in 1987 or later could have enrolled in the first grade in 1993/1994 and thus would be mandated to complete eight years of education. Note that, individuals born in 1986 that deferred enrollment for one year due to the exception in the regulation would also have enrolled in first grade in 1993. Moreover, there might be "repeaters" among individuals born in 1986 and enrolled in primary school in 1992. These individuals were also mandated to complete eight years of schooling. In the light of these arguments, there is no way that we can distinguish affected individuals from those who are not affected among children born in 1986.

Given, the ambiguity of the treatment status of the 1986 birth cohort explained in the previous paragraph, one can construct a dummy variable indicating the treatment status of an individual excluding the 1986 birth cohort. It can be used as an instrument only if the

[8] Authors' translation from Turkish.

exact birth year is known. However, the THLFS provides the completed age of an individual rather than birth year. Thus, we calculated the birth year by subtracting the completed age of individuals from the survey year. Note that the THLFS has been conducted throughout the year, and the completed age of individuals at the time of the survey is recorded. Thus, the derived birth year will consist of people who were born in the year that is derived or one year before. The derived birth year and actual birth year is the same for individuals who are surveyed after their birthday. However, the actual birth year will be one-year before the derived birth year for individuals who are surveyed before their birthday. For example, the group of people who report 23-years of completed age in the 2010 survey consists of some individuals who were born in 1986 but surveyed before their birthday and some individuals who were born in 1987 but surveyed after their birthday. Therefore, the derived 1987 birth cohort includes individuals who were born in 1986 and thus were not affected by the reform, hence introducing an ambiguity in the treatment status of this cohort, as well.

Alternatively, we provide graphical evidence on our identification strategy in Figure 2.1. The figure depicts the rate of completion of eight years of schooling by birth cohorts (hereafter, birth cohort or birth year refer to the derived birth year). Figure 2.1 demonstrates several aspects of the reform on birth cohorts. First, the middle school completion rate increased sharply from around 60 percent in the 1985 cohort to around 83 percent in the 1988 birth cohort. Secondly, the ambiguity in the treatment status of 1986 and 1987 birth cohorts is somewhat revealed in Figure 2.1. Lastly, the rate of middle school completion reached the 90-percent level for the 1992 birth cohort. That is, the reform substantially increased the percentage of children who completed eight years or more of schooling, however, some still dropped out before finishing the mandated eight years.

Building upon our discussion on the treatment status from the previous paragraphs, we can now define a dummy variable to indicate the treatment status of an individual. We exclude both 1986 and 1987 birth cohorts in our analyses because of the ambiguity in their treatment status. Furthermore, we restrict our sample to 1983-1990 birth cohorts in order to have relatively comparable individuals since the identification relies on the assumption that control and treatment groups are otherwise identical. Consequently, we create a dummy variable, T_i , which is equal to 1 if individual i was born in 1988-1990 and 0 if individual i was born in 1983-1985. The summary statistics in Panel A of Table 2.1 show that the control group has an average of 8.77 years of schooling, whereas treatment group has 9.92 years. Moreover, the rate of completion of at least middle school is 58 percent for 1983-1985 cohorts, while it jumps to 85 percent for the 1988-1990 cohorts.

2.5.4 The Intensity of the Reform

The 1997 compulsory schooling law was universal for all regions across the country and mandated all students to complete eight years of schooling. Thus, we can deduce that the treated cohorts should have attained more schooling than the control cohorts. Moreover, the enrollment rate in the middle-school level (6th-8th grade) was around 65 percent before the reform. However, the enrollment rate was low among girls, children who live in urban slums or rural areas, especially in the Eastern and Southeastern provinces, due to the negative public perception on the benefits of education and the lack of teachers and/or schools/classrooms (Dulger, 2004). We show the middle-school enrollment rate in 1996 by regions in Figure 2.2. Figure 2.2 illustrates that enrollment rates varied dramatically among regions in the range of 30-percent (TRB2-Van Region) and 96-percent (TR51- Ankara Region) just before the reform. Also, it reveals the fact that the enrollment rate was quite low in the Eastern and Southern regions. One of the objectives of the reform was pulling the 35-percent of non-enrolled children into 6th-8th grades. Based on the aforementioned characteristics of the target group, government efforts varied across regions (Dulger, 2004).

Subsequently, the Turkish government, in collaboration with international organizations and the private sector, immediately began to construct new classrooms/schools, to repair existing ones and to recruit new teachers at a differential rate across regions. This was in response to the expected increase in 6th-8th grades based on pre-reform endowments of regions. The regional variation in the intensity of investments should have created a variation in the number of students who are induced to attain more schooling by lowering the marginal cost of education at a differential rate. In other words, one might expect that the higher the intensity of the reform, the larger the expected effect of the reform. These regional differences in classroom construction and teacher recruitment provide another source of variation along with the variation between cohorts, a facet that we exploit in analyzing the causal effect of education on individual wages. This is a similar identification strategy to Duflo (2001), Clark and Hsieh (2000), Chou et al.(2010) and Dincer et al.(2014).

Having discussed regional and temporal variations in the number of classrooms constructed and the number of teachers recruited associated with the reform, we use the number of new class openings to construct an intensity measure for the 1997 reform. In the literature, the intensity of the reform is proxied by the number of new schools/classrooms (e.g. Duflo, 2001; Clark and Hsieh, 2000; Chou et al., 2010), or number of teachers (1st-8th grades) (Dinçer et al., 2014). Unfortunately, the regional level disaggregated data on the number of teachers and the number of classrooms exclusively for the 6th-8th grades is not available. However, the number of classes in each grade, which is disaggregated by provinces and school year, is given in the National Education Statistics. We argue that use of the number of classes would be a better measure for investments because one additional class in any grade would require an additional classroom and at least one additional teacher. Note that the expected increase is accommodated by adopting double-shift schooling in some regions (Dulger, 2004). Thus, the additional resources might have been used to recruit new teachers without constructing new classrooms in those schools. Therefore, the number of new classes in the 6th through 8th grades in each region and the school year is used to measure the variation in the intensity of the reform.

Before discussing the construction of the intensity measure in detail, it is useful to show the pre- and post-reform trends in the number of middle school classes per 1000 children aged 11-13, a rate that we call the class-to-child ratio. We present the disaggregated class-to-child ratio by region (NUTS-2 level) and school year between 1992 and 2003 in Table 2.2. It shows that the number of middle-school classes per 1000 children varies substantially across regions over each school year. Moreover, Figure 2.3 depicts the temporal and regional trends in the class-to-child ratio. Figure 2.3 (a) shows the average class-to-child ratio in Turkey for the school years of 1992-2003. It is clear that there is a dramatic increase in the number of middle school classes per 1000 children after 1997. The average increase in the class-to-child ratio is around 20% from 1992 to 1997 in the whole country. However, the rate of increase in 1997-2003 is three times higher than the pre-reform rate. Alternatively, we divided 26 regions into two groups - TR-Low and TR-High - based on their class-to-child ratio in 1997. The TR-Low (TR-High) consists of those regions of which class-to-child ratio is lower (higher) than the average ratio of the whole country. The average class-to-child ratio for TR-Low and TR-High are also shown in Figure 2.3 (a). The average rate of increase in the class-to-child ratio is slightly higher in the "TR-High" regions in the pre-reform period. However, the average increase in the "TR-Low" regions is steeper in the post-reform period. Additionally, Figure 2.3 (b) shows the regional variation of the percentage increase in the class-to-child ratio for pre-reform (from 1992 to 1997) and post-reform (from 1997 to 2003) periods. It is clear that the percentage increase in the class-to-child ratio from 1992 to 1997 was similar across regions, around 20%, while the percentage increase from 1997 to 2003 varies dramatically in the range of 30% (TR51- Ankara) and 169% (TRC3- Mardin) across regions. Note that the regional variation in the post-reform period in Figure 2.3 (b) shows an opposite pattern to the variation in the middle school enrollment rate in 1996 in Figure 2.2.

From the regional and yearly variations in the number of middle-school classes per 1000 children aged 11-13, clearly visible after 1997, we create an intensity measure. As we discussed above, we expect that the more the new classrooms/teachers at the middle-school level, the higher the effect will be of the reform in attainment of eight years or more of schooling. Because of the data restrictions, we assume that the increase in the number of the middle-school classes over the 1997-level are characterized by the number of new middle-school classes opened as a result of the reform. Specifically, we take the difference in the number of classes in the 6th-8th grades at a given year from 1997 for each region and assume that these differences are the cumulative number of the new middle school classes. Also, note that the number of new middle-school classes in and prior to 1997 is assumed to be zero since the reform was not effective in those years. From there, this cumulative number of the new classes is expressed as per 1000 children aged 11-13 for each region (NUTS-2 level). This measure is termed as the intensity of the reform. Note that intensity of the reform differs from the class-to-child ratio such that the former considers only the number of new middle-school class openings associated with the 1997 reform, while the latter deals with the total number of classes in 6th-8th grades. Following Chou et al. (2010), we matched the resulting intensity of the reform for each region in a given year with the cohort who began 6th grade in that year. For example, the 1988 birth cohort started 6th grade in 1999, so they are assigned with the intensity measure of the reform in 1999; and the 1989 birth cohort is assigned with intensity in the year 2000 and so on.

2.6 The Effect of the 1997 Reform and New Middle-School Class Openings on Education

As discussed in the previous section, the exposure to the reform varied substantially across regions based on the level of pre-reform enrollment rates. Thus, it is crucial to know where exactly, that is, in what region, an individual attained his/her middle-school education. This will allow us to be able to determine the intensity of exposure. Unfortunately, the THLFS does not provide any information on the region in which an individual received middle-school education (hereafter termed as the region of education). In fact, the THLFS provides only the region of residence of individuals. If there were migration among regions, then the use of region of residence as a proxy for the region of education would introduce some bias in our estimation. Migration statistics in Turkey show that inter-regional migration has taken place from historically less-developed Eastern and Southeastern regions to the more-developed Western regions, and that the probability of migration increases by the level of education (Gokhan and Filiztekin, 2008). Thus, if individuals induced to attain more schooling due to the high intensity of the reform in their region of education move to a low-intensity region, the estimated effect of intensity on schooling would be biased downward.

Although the THLFS provides information only on the region of residence, there are two other questions that can be used to partially determine the migration history of individuals. The survey asks individuals whether or not they have been living in their region of residence since birth. Subsequently, those who migrated to the residence region are asked the year in which they moved. Based on this information, we deduce that if an individual has been living in their city of birth, the region of residence is then the region of education. Subsequently, individuals who moved to the region of residence before the age of eleven would have received their middle school education in the region that they currently reside. However, we cannot determine the region of education for those individuals who moved to the residence region after the age of eleven. Thus, in our analyses, we restrict our attention to the sample individuals who attained middle school in the region of residence, representing 73 percent of the whole sample. Given that people with more education are more likely to migrate between regions, thus disallowing us from being able to identify their region of education, the estimated effects of the intensity of reform with our restricted sample will also be biased downwards.

2.6.1 The Impact of the 1997 Reform on Educational Attainment

The descriptive evidence in Table 2.1 and Figure 2.1, discussed above, shows that the 1997 compulsory education law created a substantial increase in the educational attainment of individuals who are affected by the reform. Moreover, we can assess the effectiveness of the reform and middle-school openings on the educational attainment in Turkey controlling for the effect of age, region, gender, etc. Re-writing the equation (2.2), we estimate the following equation by OLS.

$$S_{ijt} = \gamma_1 T_i + \gamma_2 X_{ijt} + \mu_j + \lambda_t + v_{ijt} \tag{2.3}$$

where S_{ijt} is schooling of individual i in region j at time t. T_i is the treatment dummy variable which takes a value of 1 if an individual belongs to the 1988-1990 birth cohort and thus is affected by the law, and 0, otherwise. X_{ijt} vector includes a quadratic in age, female dummy, and a dummy for ongoing schooling. The region dummies, μ_j, are included to control for the time-invariant regional characteristics. Moreover, λ_t represents dummies for the survey year, which captures the time fixed effects in educational attainment. Equation (2.3) is estimated using either years of schooling or a dummy variable which indicates whether an individual completed at least 8 years of schooling (hereafter, middle school completion refers to the dummy variable that indicates the completion of middle school or higher levels) or not as dependent variables. Table 2.3 presents the OLS estimates of γ_1 in equation (2.3) for the completed years of schooling in the first three columns. Moreover, we estimate a linear probability model (LPM) for the probability of completion of at least middle school in the last three columns of Table 2.3. Panel A shows results for the restricted sample in which region of residence and region of education is the same while Panel B of Table 2.3 presents results for all-resident sample using region of residence as a proxy for the region of education. All standard errors are clustered by region and year of birth.

Columns 1 and 4 of Table 2.3 (Panel A) indicate that the 1997 reform increased the completed years of education by 0.77 years and the probability of completing middle school or a higher level of education by 19 percent, respectively. Women attain significantly less schooling in terms of both years of schooling and middle school completion [9]. Thus, we present estimated coefficients for men and women separately. We find that the effect of the reform is more pronounced for women than men with an increase of 1.07 years of education

[9]The estimated coefficients of the controls including female dummy are not presented here for space reasons. However, they are available upon request

among women and around 0.48 years among men. A similar pattern is also found in the effect of the reform on the probability of completing eight years or more of schooling, with a probability of 22 percent for women, and 16 percent for men. Moreover, the results of estimation in the Panel B, obtained by using the all-resident sample, suggest very similar effects in terms of both years of schooling and the middle school completion. This is in line with our expectations because estimation of equation (2.3) is based on the comparison of the educational attainment of cohorts in the whole country, rather than on a comparison of cohorts in different regions.

2.6.2 The Impact of the Intensity of the 1997 Reform on Educational Attainment

Having shown that the Eight-Year Compulsory Schooling Law increased the educational attainment substantially in Turkey, we now turn our attention to the impact of middle-school class openings on educational attainment. We use the following equation, which is a slightly modified version of equation (2.3), to estimate the effect of new classes on schooling.

$$S_{ijkt} = \gamma_1(P_{jk} * T_i) + \gamma_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + v_{ijkt} \tag{2.4}$$

where T_i is the treatment dummy variable that is used in equation (2.3). Here, P_{jk} is the intensity of the reform for cohort k in region j, which represents the number of new middle school class openings in region j at the time cohort k starts 6th grade. X_{ijkt} vector includes a female dummy variable, a dummy for ongoing schooling. We also controlled for unrestricted birth cohort fixed effects, θ_k, instead of a quadratic in age in equation (2.3). μ_j and λ_t are the region and year of survey dummies. Furthermore, we include an interaction term between the treatment dummy and the middle school enrollment rate in 1996 in order to control for pre-reform trends. The first three columns of Table 2.4 show the OLS estimates of γ_1 in equation (2.4) using the years of schooling as a dependent variable. Also, we use

LPM to estimate the effect of the intensity of the reform on the probability of completing middle school or a higher education level. In Panel A, we use the restricted sample which includes individuals who are educated in the region in which they reside. In Panel B, on the other hand, we use all-residents sample using region of residence as a proxy for the region of education.

Column 1 of Panel A suggests that one additional new middle-school class per 1000 children aged 11-13 increased the completed years of education of children born in 1988-1990 by 0.19 years. Moreover, the effect of the intensity of the reform is higher for females (column 2) than males (column 3). One additional class opening for 1000 children leads to a 0.23-year increase in the education of females, and to a 0.15-year increase in the education of males in the treatment cohorts. Furthermore, using a dummy variable for middle school or higher-level completion as a dependent variable (column 4), we find that one additional class increases the probability of completing at least eight years of schooling by 4 percent. Again, we find that new middle-school classes are more effective in increasing the treated women's probability of completing middle school or higher levels (column 5) more than the probability of completion for treated men (column 6), with magnitudes of 5.3 percent and 3 percent, respectively.

Additionally, we present the estimation results for all-residents sample in which the region of residence is used as a proxy for the region of education. Overall, the estimations are in line with our expectations, such that ignoring migration and assuming all individuals are educated in their region of residence results in smaller estimated coefficients for the effect of intensity of the reform on schooling. However, the effects are found to be higher for females than males. Therefore, the bias is more pronounced in the estimation of the impact of new middle-school classes on the treated women's education than the treated men's education. This might be explained by the slightly lower percentage of women who received education in the region of residence in our sample.

In short, we have shown that the eight-year compulsory schooling law and the openings of new middle-school classes were effective in increasing the overall educational level in Turkey. We found that the reform and the accompanying class openings increased the schooling of females more than that of males. However, resulting from the limitations of our dataset, it appears that migration does introduce some bias to our estimates. It is important to note that in this section we evaluated the effect of the reform and the intensity on the education of people without considering whether they are wage earners or not. Thus, these results are not the first stage estimates of the 2SLS estimations for returns to education because it is based on a sample of individuals for which a positive wage is observed.

2.6.3 Spillover Effects of the 1997 Reform and the Intensity on Levels of Education

We further examine the levels of education at which the Eight-Year Compulsory Schooling Law and the accompanying class openings were effective. Following Duflo (2001), and Clark and Hsieh (2000), we estimate a set of linear probability models (LPM) for the probability of completing at least m years of schooling using the following equation:

$$S_{ijtm} = \gamma_{1m} T_i + \gamma_2 X_{ijt} + \mu_j + \lambda_t + v_{ijt} \tag{2.5}$$

where S_{ijtm} is a dummy variable indicating whether individual i in region j at time t completed at least m years of schooling or not, for $m = 1$ to 17. The covariates are the same as in equation (2.3). γ_{1m} shows the impact of the reform on completing m or more years of schooling. The LPM for each level of education is estimated for male and female samples separately using the restricted sample (individuals who are educated in the region of residence). The estimated coefficients, γ_{1m}, and the associated 95% confidence intervals are plotted in Figure 2.4 (a) and (b) for females and males, respectively. The shape of the figures indicates the level at which the reform was effective. Figure 2.4 (a) shows that the reform was mainly effective in increasing women's completion of 6th-8th grades with a probability of around 20 percent. Even though the effect becomes smaller beyond the 8th grade, the figure shows that there are spillover effects of the program on the probability of completing education levels beyond middle school for females. Moreover, Figure 2.4 (b) shows that the program was effective on men's probability of completing middle school by around 15 percent. The effect becomes insignificant beyond 8th grade for males. Overall, Figure 2.4 (a) and (b) show that the compulsory schooling law was more effective in increasing female education in and beyond middle school than male education.

Alternatively, we perform the same procedure in examining the impact of the intensity of the reform on the levels of education using the following linear probability models.

$$S_{ijktm} = \gamma_{1m}(P_{jk} * T_i) + \gamma_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + \upsilon_{ijkt} \tag{2.6}$$

where S_{ijktm} is a dummy variable which indicates whether individual i in region j in birth cohort k and observed in year t completed m years or more of schooling. The regressors are the same as in equation (2.4). The point estimates along with 95% confidence intervals are plotted for females in Figure 2.4 (c) and for males in Figure 2.4 (d). Figure 2.4 (c) suggests that one additional middle-school class per 1000 children was effective in increasing the treated women's probability of completing middle school grades (6th-8th grade) by around 5 percent. The effect beyond 8th grade is significant and decreasing with the level of education. Moreover, Figure 2.4 (d) shows that the middle-school class openings were also effective in increasing the treated men's probability of completing 6th grade or more. The effect was the highest in the middle-school level with a probability of around 3 percent. There is also some significant spillover on the probability of completing high school and college education.

2.7 Returns to Education

Having shown that the eight-year compulsory schooling change and the intensity of the reform considerably increased the educational attainment of the treated individuals, we turn our attention to estimating the causal effect of education on wages. We estimate the following equation using the OLS and 2SLS.

$$log(w_{ijkt}) = \beta_1 S_{ijkt} + \beta_2 X_{ijkt} + \mu_j + \gamma_k + \lambda_t + u_{ijkt} \tag{2.7}$$

where $log(w_{ijkt})$ is the natural logarithm of hourly wage of an individual i, in region j, in cohort k, and at time t. The OLS estimates of the equation (2.7) will lead bias estimates if the schooling is endogenous in this model. Using plausibly exogenous variation in the schooling created by the 1997 reform and the new middle-school classes opened to accommodate the expected increase in the enrollment, equation (2.7) can be estimated employing 2SLS. Here, we assume that the 1997 reform and the accompanying middle-school class openings have an effect on hourly wages only through an increase in education. We also assume that the systematic change in the wages across cohorts is attributable to the reform and the intensity of the reform.

Equation (2.7) is estimated for a sample of individuals who report a positive wage and are educated in the region of residence per our discussion in the previous section. Note that this sample is different than the sample that we used in the estimation of the effect of the reform on education in the section prior. The sample size is small for females due to the low labor force participation rate of women in Turkey. Therefore, we present the corresponding first stage estimates for both instruments in Table 2.5.

The first three columns of Table 2.5 show that the effect of the reform and the intensity of the reform on the years of education are higher for the sample of wage earners than for the whole sample. On the other hand, the last three columns show that the reform and the middle-school class openings were more effective in increasing the probability of completion of 8 years or more of schooling for all workers as well as the male workers. However, it reveals an interesting result for the female wage-earners. In the fifth column, we find that the reform leads an increase in the treated female workers' probability of completing middle school or higher levels of education by 14 percent. This is smaller than the effect on treated male workers' and the effect on all treated women's (in the Panel A of Table 2.3). Also, we find that one additional middle-school class per 1000 children increased the probability of the treated women's completion of middle school or higher levels of education by 3.5 percent which is smaller than that found in Table 2.3.

Comparison of Table 2.5 with Table 2.3 and Table 2.4 shows that the reform and the intensity of the reform were more effective in increasing the schooling of women than that of men. However, this improvement in female education could not be converted into labor market gains as much. Furthermore, comparison of columns 2 and 5 of Table 2.5 suggest that female workers who completed middle school or higher as a result of the reform or the intensity, also continued to attain more years of schooling beyond 8th grade. We test this conjecture by providing graphical evidence in Figure 2.5 replicating Figure 2.4 for the wage earner sample. Figure 2.5 (b) and (d) depict very similar patterns to their counterparts in Figure 2.4. This suggests that the impact of the reform and the intensity of the reform are similar for both wage-earner men and all men. On the other hand, Figure 2.5 (a) and (c) has different shapes from their counterparts in Figure 2.4. Figure 2.5 (a) and (b) indicate that the reform and the accompanying middle school class openings were effective in increasing the probability of completing high school and college as well as middle school for working women. In fact, Figure 2.5 (c) indicates that one additional middle-school class per 1000 children increased the working-women's probability of completing university more than high school and middle school.

These findings are in line with the literature that the female labor participation rate is low and increases with education level in Turkey (Dayıoğlu and Kırdar, 2009). However, this raises some concerns about the validity of our instrument in the estimation of returns to education for women. Note that our identification is based on the assumption that the reform and the intensity of the reform increase individual wages through an impact on education. The findings in Table 2.5 and Figure 2.5 show that the probability of wage employment of females might be affected by the reform and the accompanying middle-school class openings. Hence, the estimated returns to education for women using 2SLS should be taken with caution.

Next, we present the OLS and 2SLS estimation results of equation (2.7) for the completed years of education in Table 2.6 and for middle school completion in Table 2.7. We use the treatment dummy (IV-1) and the intensity of the reform (IV-2) as instruments for the schooling variable in the 2SLS estimations. All standard errors are clustered by the region and birth cohort. F-statistics of the excluded instruments in the first stage are shown in brackets. We use the rule of thumb critical value of 10, which is proposed by Staiger and Stock (1997), to test weakness of instruments.

Table 2.6 presents the estimates of the coefficient, β_1 in equation (2.7) in which schooling variable is the completed years of education. The results are presented for a sample of all wage earners in Panel A, for females in Panel B, and for males in Panel C.

The first column of Table 2.6 shows the OLS estimates of equation (2.7). We find that one additional year of education increases hourly wages by 5 percent. Moreover, the female workers experience higher returns to one additional year of education than the male workers with magnitudes of 6.4 percent and 4.4 percent, respectively. The second column of Table 2.6 shows the 2SLS estimates of β_1 instrumenting schooling by the treatment dummy. The results suggest that one additional year of schooling leads to an 11-percent increase in the hourly wages of the individuals in the cohort 1988-1990. The corresponding F-statistics of the excluded instruments in the first stage do not exceed the critical value while remaining close to 10. This indicates that the treatment dummy is relatively weak. The estimated coefficients for females is higher than for males, 14 percent, and 7 percent, respectively.

The corresponding F-statistics of the first stage show that the treatment dummy is a weak instrument. Furthermore, the third column of Table 2.6 presents the returns to education using the 2SLS in which schooling is instrumented by the intensity of the reform. We find that one additional year of education increases wages of individuals who were born in 1988-1990 by around 9 percent. The returns to one additional year of schooling for the treated women and men are found to be very similar with a magnitude of 8.5 percent. The F-statistics of the first stages, which are around 30, indicate that the intensity of the reform is not a weak instrument.

Note that our identification strategy is based on the assumption that the reform and the intensity of the reform affects wages only through increasing the educational attainment, controlling for cohort and region fixed effects. If the regional trends in the enrollments are correlated to the regional trends in the future earnings, our identification could be jeopardized. We address this possibility by including the interactions of the region and survey year fixed effects in columns 4 and 5 of Table 2.6. Comparing columns 2 and 3 with columns 4 and 5, respectively, we find that controlling for differential regional trends hardly changes the estimated returns to education.

Furthermore, Table 2.7 presents the estimated coefficient, β_1, of equation (2.7) in which schooling is measured by a dummy variable which indicates whether or not an individual completed at a minimum middle school education. The first column of Table 2.7 shows the OLS estimation results. It suggests that having at least middle school education increases the hourly wages by 24 percent (2.4 percent per year) for all workers. Again, the returns to middle school completion are higher for the female workers than for male workers, 38 percent (3.8 percent per year) and 20 percent (2 percent per year), respectively. The second column presents the estimated returns to middle school completion using the treatment dummy as an instrument. The results show that middle school completion leads to a 44-percent increase in the hourly wages of the treated cohort. It is also found that middle school completion raises the treated women's wages by 157 percent. This looks implausibly high. However, it should be noted that those who completed at least middle school could have attained schooling between 8 and 17 years. Thus, assuming linear returns for each year of schooling, this might indicate a 15-percent returns to one additional year of education. As we discussed above, these high returns to education might be a result of the selection-into-employment of women. In other words, the probability of wage employment of women might also be affected by the reform and thus inflates the returns to education. On the other hand, having at least middle school education increases the treated men's wages by 19 percent. However, this is estimated less precisely as it is significant at the 10 percent level. The estimated coefficients in the fourth column of Table 2.7 controls for the differential region trends. The results are quite similar to their counterparts in the second column.

The third and fifth columns show the returns to the middle school completion using the intensity of the reform as an instrument for schooling. Unlike the treatment dummy, F-statistics indicate that the intensity of the reform is a strong instrument for middle school completion for all-worker sample as well as for female and male sub-samples. It is found that completion of middle school or higher education levels increases the hourly wages of the individuals who were born in 1988-1990 by 56 percent. Assuming linear returns for each year of education, this implies a 5.6-percent increase for an additional year of education. Furthermore, it is found that having completed at least middle school education leads an increase in the treated women's wage by 82 percent (8.2 percent per additional year). Also, the estimated coefficient for males indicates that completion of middle school or higher levels of education raises the treated men's wages by 43 percent (4.3 percent per additional year). Again, including region-survey year fixed effects hardly changes the estimated coefficients.

Tables 2.6 and 2.7 show that the 2SLS estimates exceed their OLS counterparts. However, these two estimates are not necessarily comparable as the first one represents local average treatment effect (LATE) for those who are affected by the reform and the accompanying middle-school class openings. Moreover, we find that the treatment dummy is a weak instrument for years of education and for women's completion of middle school or higher levels of education. However, the intensity of the reform is a strong instrument for both specification and all subsamples considered in the tables. The 2SLS coefficients are hardly differ when we control for the region-survey year fixed effects.

Furthermore, a special attention is required to compare the estimated coefficients in the third columns of both Table 2.6 and Table 2.7. We found that returns to one additional year of schooling for the treated group is around 9 percent and it is very similar for females and males in Table 2.6. On the other hand, using a dummy variable for the completion of middle school or higher levels of education as the schooling variable, we found that one additional year of education leads a 5.6-percent increase in the treated individuals' hourly wages. The difference between the two estimates might be resulting from the differential returns in the different educational levels. For example, one-year college education might have higher returns than one-year high-school education. Moreover, the returns to education are found to be similar for females in both tables. However, return to one-year education for males in the Table 2.7 is considerably smaller than that in Table 2.6. Recall that, Figure 2.5 shows that the intensity of the reform was mainly effective in increasing treated male workers' middle school education while it was effective in increasing treated female workers' middle school education as well as their college education. Thus, the differences between the returns to one-year education in both tables might be indicative that the return to one-year education is higher for the college education than high school or middle school.

Additionally, we replicate Table 2.6 and Table 2.7 restricting the sample to the individuals whose highest attained education level is high-school (either general or vocational high school). Table 2.8 replicates Table 2.6 where the schooling variable is the years of education. The first column of Table 2.8 shows that the OLS estimates are smaller than that found in Table 2.6. The second column, in which the treatment dummy instruments education, shows similar results to the corresponding results in Table 2.6. Furthermore, the third column of Table 2.8, where we use the intensity of the reform as an instrument, shows some interesting findings. First, we find that one additional year of education in middle school or high school levels increases the hourly wages of individuals born between 1988-1990 by 5.6 percent. This is significant at the 10 percent significance level. The F-statistics of the excluded instruments in the first stage indicates that the instrument is not weak. Second, we find that there is no statistically significant effect of middle or high school education on the treated female workers' wages. The F-statistics of the first stage, which is lower than 10, indicates that the intensity of the reform is a weak instrument for female education when we exclude college goers. This is not surprising, as we have shown that the intensity of the reform was more effective in the college education of the female wage earners. Lastly, we find that the return to one year of education is 5.4 percent for the treated male workers who do not have a college education. Controlling for region-survey year fixed effects hardly alters the estimated coefficients, except for the female sample in the fifth column. The F-statistics of the first stage got closer to the critical value of 10. The estimated coefficient becomes higher, but it still remains insignificant.

Table 2.9 replicates Table 2.7 considering high school as the highest attained education level. Note that the schooling variable is the dummy variable which indicates that whether an individual completed middle school or higher levels of education. The first column of Table 2.9 suggests that the OLS estimates of the per-year rate of return are close to the corresponding estimates in Table 2.7. Moreover, instrumenting schooling by the treatment dummy, the second column shows that the completion of middle school or high school increases wages of the treated workers by 31 percent. Similar to Table 2.7, we find that the treatment dummy is a weak instrument for the female sample. Moreover, the returns to middle school completion are found to be considerably high with a magnitude of 80 percent. As has been discussed, this might be driven by the selection-into-employment which is more problematic for women. Furthermore, we found that the middle school or high school completion increases the treated male workers' hourly wages by 26 percent. The results are very similar in column 4 in which the region-year fixed effects are included. Finally, the third column, in which middle school completion is instrumented by the intensity of the reform, shows that the per-year returns to middle school or high school graduation are quite similar in Table 2.7 and Table 2.9 for all workers sample (5.2 percent per year) as well as for the male sample (4.5 percent per year). However, there is no statistically significant effect for female workers, even though the instrument is strong. Controlling for the region-survey year fixed effects results in slightly higher estimates.

The findings in Table 2.8 and Table 2.9 confirm our explanations on the discrepancies between the estimated returns to one-year schooling in Table 2.6 and 2.7. The returns to one-year education in college level seems to be higher than at the middle school or high school level, is more pronounced for female workers.

2.8 Conclusion

In this paper, we examined the effect of the Eight-Year Compulsory Education Law and the accompanying middle-school class openings on educational attainment in Turkey. Subsequently, we used these exogenous variations to estimate the causal effect of education on individual wages.

The estimations suggest that the 1997 law increased the completed years of schooling of the people born between 1988-1990 by 0.77 years and the probability of completing middle school or higher education levels by 19 percent. The effect of the program is higher on women's education than on men's. Moreover, we find that one additional middle-school class per 1000 children aged 11-13 increases the years of completed schooling by 0.19 years. It also increases the probability of completion of at least middle school by 4 percent for the first three treated cohorts. Again, the effect of middle school class openings was higher on educational attainment of women than that of men.

Further evidence suggests that the intensity of the reform was mainly effective in increasing men's probability of completion of middle school (6th-8th grades) education. However, it was more effective in increasing the women's probability of completing middle school as well as education levels beyond the 8th grade. Furthermore, we find that despite the fact that women attained more schooling as a result of the reform and the accompanying middle-school class openings; they were unable to convert these gains into the labor market outcomes to the same degree as men. There is some descriptive evidence that the reform and the intensity of the reform might increase the probability of wage employment for women.

Having shown that the reform and the intensity of the reform increased educational attainment significantly, we used the treatment dummy and the intensity of the reform as instruments for schooling. The treatment dummy is found to be a weak instrument in some of our specifications, especially for female education. On the other hand, the intensity of the reform, which accounts for the regional and yearly variations, is a strong instrument in most of the specifications. Our 2SLS estimations suggest that one additional year of schooling increases the treated group's hourly wages by 5.6 to 9 percent. However, there is some evidence that the returns to one-year education may be different at different education levels.

The rate of return is found to be higher for females than for males. We find that returns to education for the female workers are in the range of 8 to 15 percent. Further investigation shows some evidence that the higher returns to education for females are mainly driven by considerably high returns to college education. When we exclude the college goers, the instruments become weak for females. We were unable to find any statistically significant returns to education for females using the intensity of the reform. However, there are implausibly high returns to education when we instrument schooling by the reform. We note that the returns to education estimates for females should be taken with caution because of a severe sample selection bias for females.

On the other hand, we showed that the intensity of the reform is a quite strong instrument for men's schooling in all cases. We find that returns to one-year education are in the range of 4.3 to 8.5 percent. When we exclude college goers, the returns to education are found to be around 5 percent for male workers.

Furthermore, the estimated coefficients hardly changed when we include region-year fixed effects in order to control for the differential effect of the regional trends in the factors that potentially affect the wages.

Note that our 2SLS estimations reflect the local average treatment effects of people who are induced to attain more schooling due to the compulsory schooling and the accompanying middle-school class openings. However, these findings are important for the policy purposes in Turkey. In 2012, the Turkish government further increased years of compulsory schooling and has been implementing several policies to decrease the gaps in educational attainment and income across regions. Thus, our results indicate that policies targeting to increase the educational level of individuals might be a good tool for improving individuals' welfare. In a broader context, the effectiveness of the reform and the middle-school class openings on educational attainment and wages in Turkey also have important policy implications for the developing countries. Our estimates show that increasing compulsory schooling and investing those regions which are lagging behind in the enrollment rates would be a good policy for the developing countries to increase overall educational attainment and individual wages.

Another limitation in this study is that we were forced to estimate returns to education with a sample of non-movers because of the design of the dataset. If those individuals who attained more years of schooling as a result of the reform and the accompanying middle-class openings migrated from their region of education, our estimates can be seen as a lower bound. This limitation can be addressed in a future study using another data set that provides information on the region of education.

Overall, we provide evidence on the causal effect of education on hourly wages exploiting the exogenous variation in educational attainment in Turkey. The estimated returns to education range from 5.6 to 9 percent. We provide somewhat robust estimates for males. However, there are potential problems in the estimation of the women's returns to education resulting from the selection-into-employment bias. Further research is warranted to address the selection bias in the returns to education for females.

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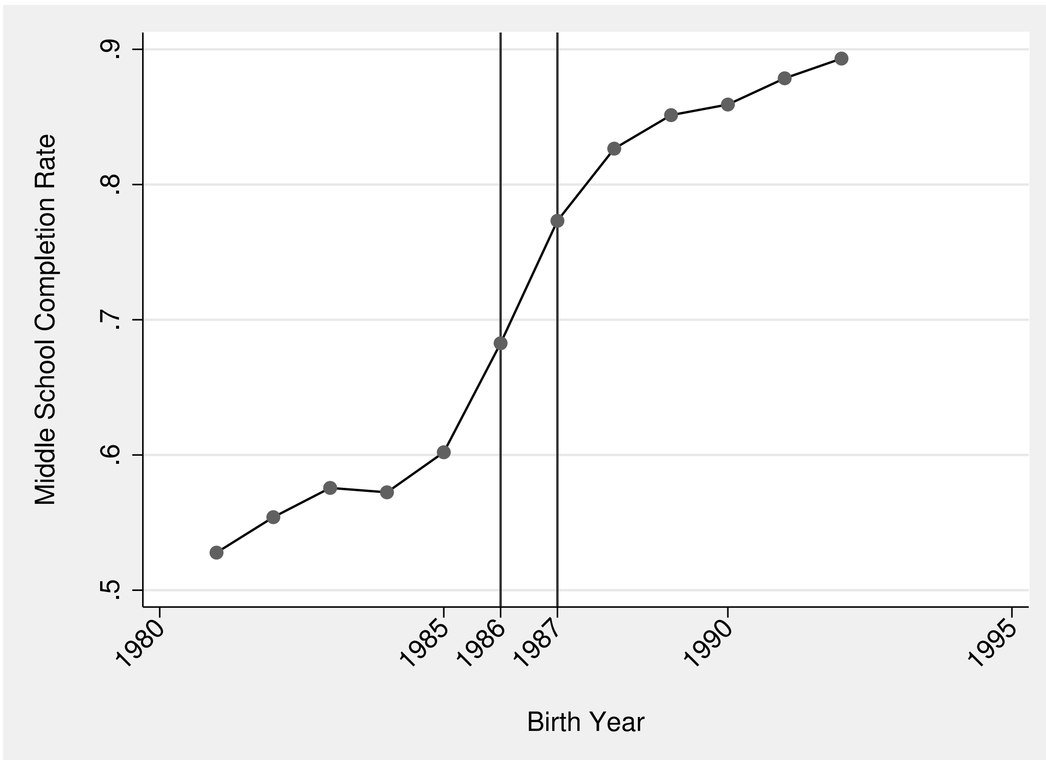
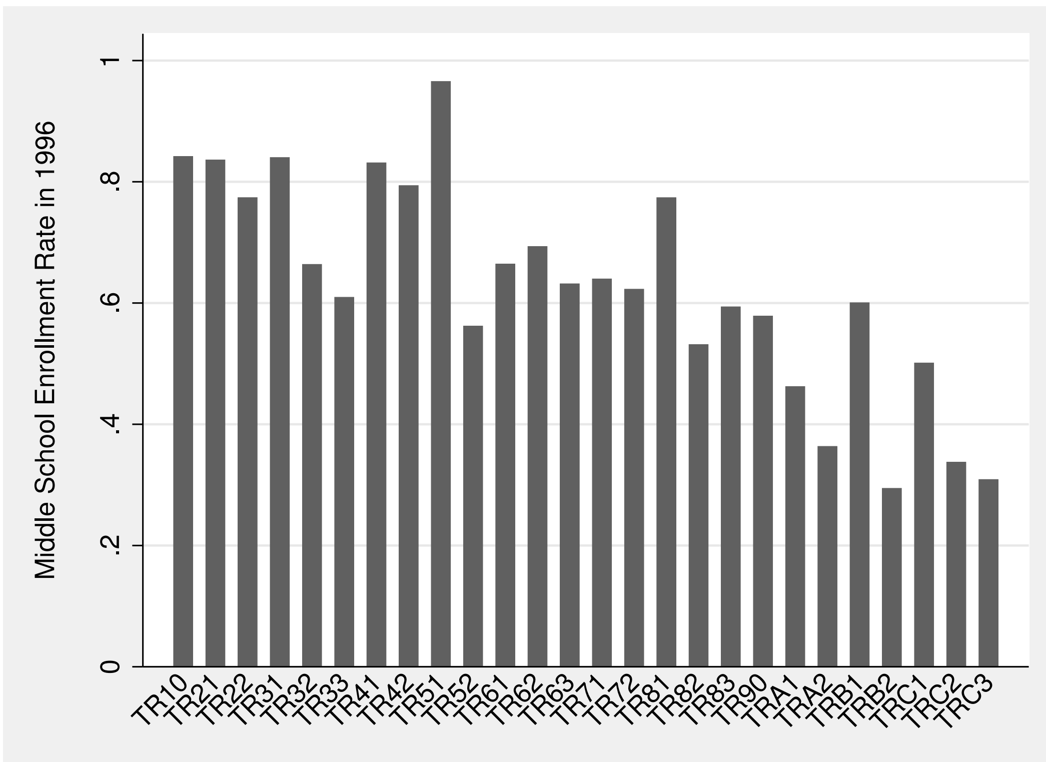
Figures and Tables

Figure 2.1: At Least Middle School Completion Rate by Birth Year



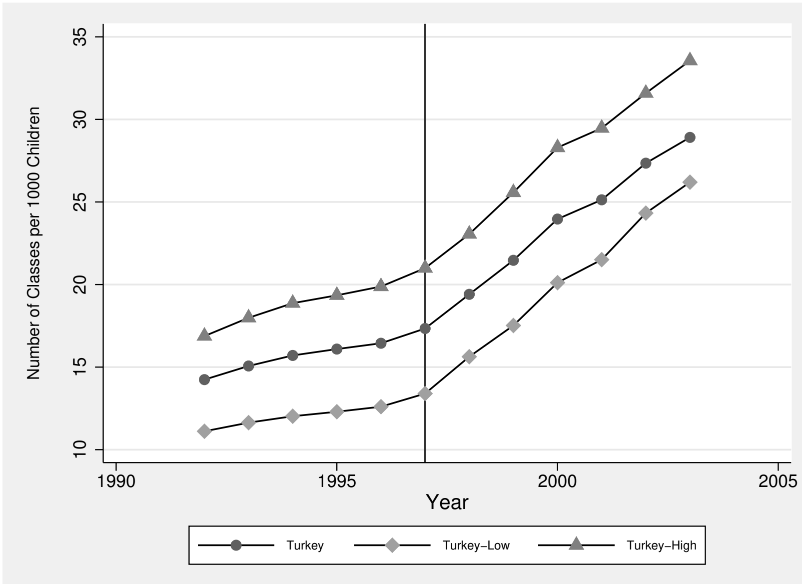
Source: Authors' Calculation based on THLFS 2009-2014

Figure 2.2: Middle School Enrollment Rate by NUTS-2 Regions

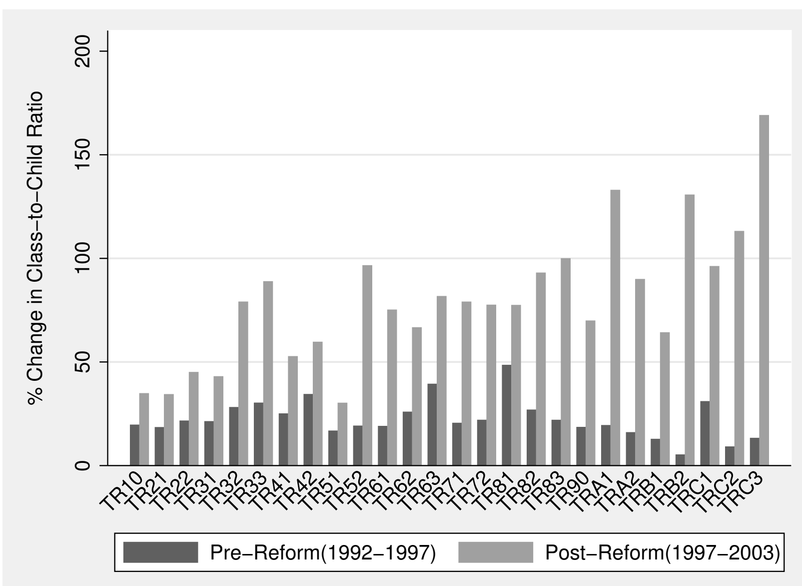


Source: Authors' Calculation based on 1990, 2000, 2007 Population Censuses of Turkey and MONE's National Education Statistics 1992-2003.

Figure 2.3: Trends in the Number of Middle School Classes per 1000 Children



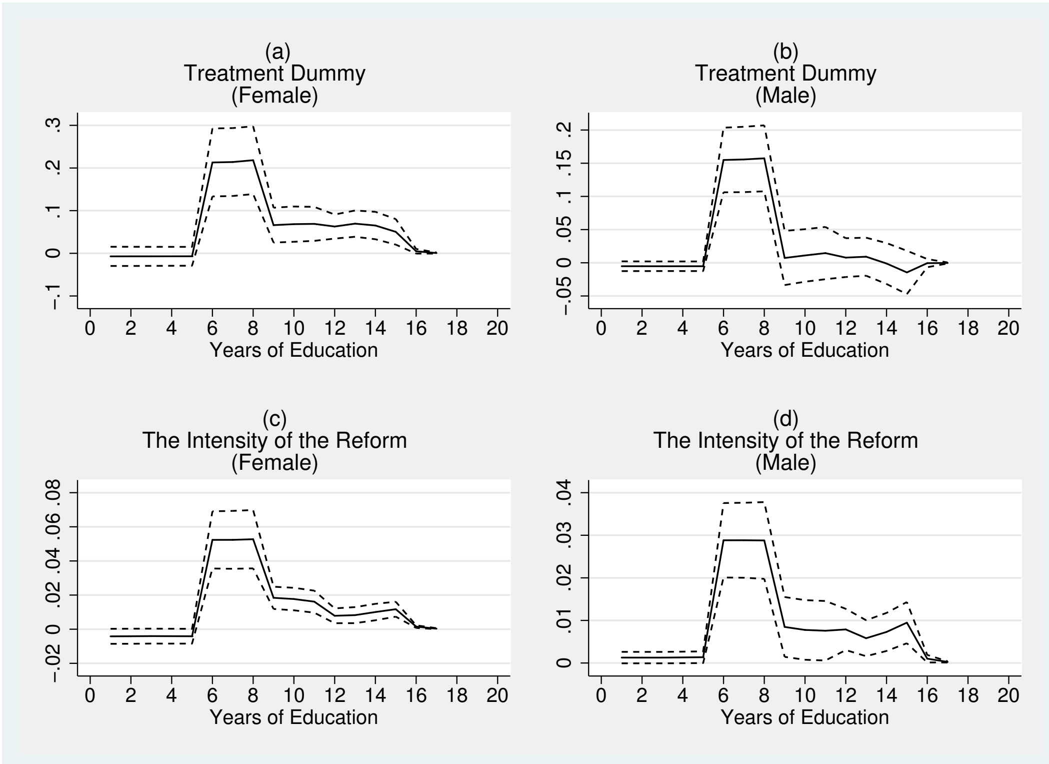
(a) The Class-to-Child Ratio by School Year



(b) Change in Class-to-Child Ratio by Regions

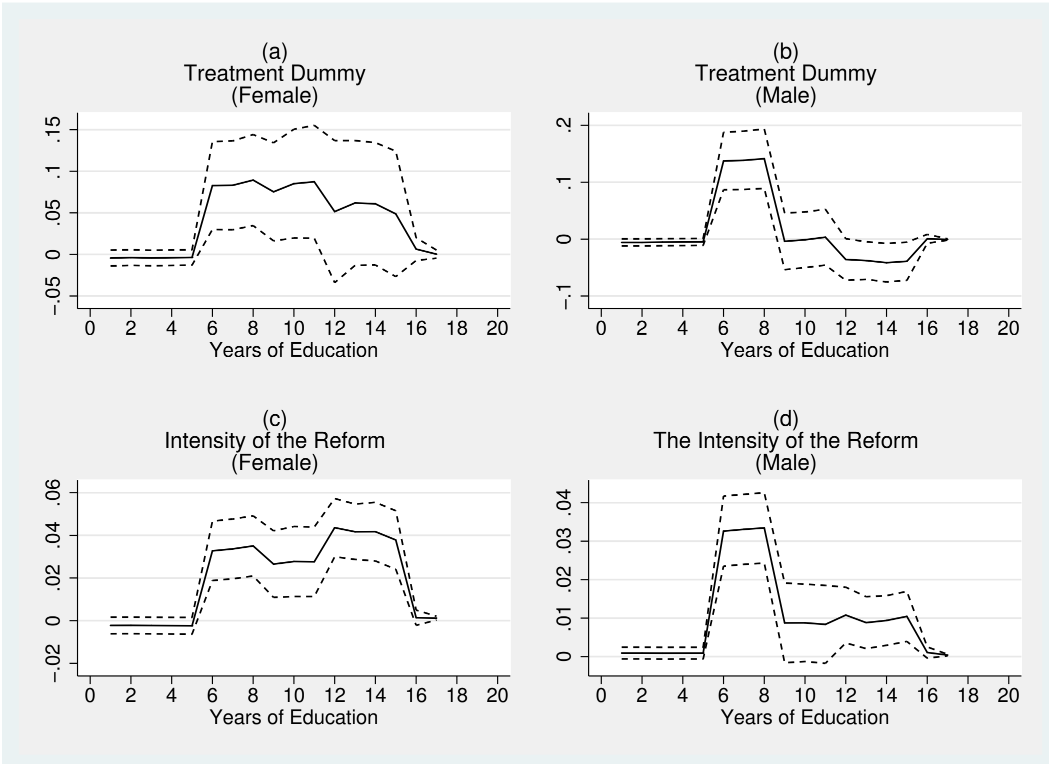
Source: Authors' Calculation based on the 1990, 2000, 2007 Population Censuses of Turkey and the MONE's National Education Statistics 1992-2003.

Figure 2.4: The Effect of the Instrument on The Probability of Completing at Least 'm' Years of Education-Whole Sample



Note: All point estimates and 95% CI's are estimated using linear probability models with a sample of **individuals** who are educated in their region of residence.

Figure 2.5: The Effect of the Instrument on The Probability of Completing at Least 'm' Years of Education-Wage Earners Sample



Note: All point estimates and 95% CI's are estimated using linear probability models with a sample of **workers** who are educated in their region of residence.

Table 2.1: Descriptive Statistics

	Control			Treated		
VARIABLES	# of Obs.	Mean	Std. Dev.	# of Obs.	Mean	Std. Dev.
Panel A: Whole Sample						
Female*	133,142	0.519	0.500	124,989	0.528	0.499
Married*	133,142	0.647	0.478	124,989	0.309	0.462
Age	133,142	27.43	1.899	124,989	22.52	1.888
Living in the Same City*	133,142	0.699	0.459	124,989	0.765	0.424
Currently in School*	133,142	0.089	0.285	124,989	0.239	0.426
Years of Education	133,142	8.771	4.241	124,989	9.916	3.678
At least Middle School Completion*	133,142	0.580	0.494	124,989	0.847	0.360
Illiterate*	133,142	0.040	0.195	124,989	0.041	0.199
No Diploma*	133,142	0.083	0.276	124,989	0.142	0.349
Primary School*	133,142	0.337	0.473	124,989	0.011	0.105
Middle School*	133,142	0.135	0.342	124,989	0.300	0.458
High School*	133,142	0.124	0.330	124,989	0.251	0.434
Vocational High School*	133,142	0.119	0.324	124,989	0.128	0.335
College*	133,142	0.202	0.401	124,989	0.168	0.373
Panel B: Sample of Wage Earners						
Female*	52,458	0.274	0.446	36,955	0.328	0.470
Married*	52,458	0.571	0.495	36,955	0.220	0.414
Age	52,458	27.58	1.852	36,955	23.07	1.756
Living in the Same City*	52,458	0.649	0.477	36,955	0.759	0.428
Currently in School*	52,458	0.109	0.312	36,955	0.203	0.402
Years of Education	52,458	10.55	3.920	36,955	10.87	3.151
At least Middle School Completion	52,458	0.762	0.426	36,955	0.934	0.248
Illiterate*	52,458	0.005	0.0704	36,955	0.008	0.0870
No Schooling*	52,458	0.022	0.148	36,955	0.058	0.234
Primary School*	52,458	0.215	0.411	36,955	0.007	0.0850
Middle School*	52,458	0.138	0.345	36,955	0.297	0.457
High School*	52,458	0.130	0.336	36,955	0.197	0.397
Vocational High School*	52,458	0.163	0.369	36,955	0.182	0.386
College*	52,458	0.331	0.471	36,955	0.259	0.438
Tenure in the Current Job	52,458	3.090	2.909	36,955	1.619	1.842
Part-time*	52,458	0.023	0.150	36,955	0.027	0.162
Work Hours per Week	52,458	51.07	12.44	36,955	52.46	12.44
Hourly Wage	52,458	2.683	1.807	36,955	2.095	1.367
Log of Hourly Wage	52,458	0.809	0.572	36,955	0.595	0.502

Notes: Treated: 1988-1990 birth cohorts, Control : 1983-1985 birth cohorts. * Dichotomous variables.

Table 2.2: Number of Middle School Classes(6th-8th) per 1000 Children Aged 11-13 by NUTS-2 Regions and School Year

NUTS2	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
TR10	15.53	16.59	17.29	17.63	17.77	18.58	19.85	21.80	23.82	24.29	24.73	25.06
TR21	21.11	22.34	23.00	23.49	23.85	25.01	26.87	28.88	31.38	32.05	32.73	33.61
TR22	21.27	22.34	23.31	23.97	24.41	25.88	27.13	30.70	33.85	34.19	36.38	37.54
TR31	18.10	19.06	19.87	20.36	20.81	21.96	23.70	26.29	28.16	29.09	30.87	31.41
TR32	15.84	16.78	17.90	18.43	18.76	20.31	23.72	26.24	29.19	30.31	32.77	36.36
TR33	14.57	15.67	16.74	17.28	17.78	18.98	22.21	25.50	29.19	30.25	35.72	35.84
TR41	17.83	19.45	20.18	21.07	21.85	22.30	24.20	25.94	28.32	29.07	31.42	34.06
TR42	15.98	17.32	18.70	19.57	19.21	21.49	23.37	25.03	28.07	29.97	31.26	34.31
TR51	20.92	21.77	22.55	23.37	23.61	24.44	25.53	26.47	27.94	28.20	29.87	31.82
TR52	12.87	13.36	14.15	14.24	14.69	15.34	18.33	20.83	24.14	24.00	28.30	30.16
TR61	16.63	17.85	18.45	18.99	19.29	19.80	21.54	24.34	27.43	28.17	30.97	34.69
TR62	13.41	14.11	14.86	15.42	16.17	16.89	19.28	21.67	23.73	25.15	26.43	28.14
TR63	11.68	12.65	13.60	14.22	14.78	16.28	19.46	22.19	24.93	26.03	28.25	29.59
TR71	15.11	15.96	16.70	17.08	17.75	18.21	20.49	23.62	26.61	28.52	30.77	32.61
TR72	14.19	14.84	15.37	15.82	16.18	17.32	19.60	22.11	25.65	27.61	30.41	30.75
TR81	14.45	15.79	17.23	16.98	19.50	21.46	24.44	28.71	31.52	33.71	34.70	38.08
TR82	14.01	15.09	16.01	16.05	16.66	17.78	19.71	22.79	26.28	28.36	30.94	34.33
TR83	13.42	14.06	14.75	15.14	15.81	16.37	20.04	22.57	26.17	27.82	32.13	32.74
TR90	14.93	15.76	16.27	16.57	17.05	17.70	19.99	21.75	24.36	26.42	29.11	30.07
TRA1	11.56	11.89	12.06	12.76	12.94	13.81	15.66	18.22	21.10	20.16	26.00	32.17
TRA2	9.21	9.60	9.78	9.82	9.80	10.69	12.82	13.53	15.46	17.15	18.89	20.31
TRB1	14.96	15.60	15.83	15.43	16.32	16.87	18.78	20.00	22.49	24.04	26.07	27.71
TRB2	7.65	7.64	7.58	7.53	7.40	8.06	9.09	9.83	12.14	13.95	17.29	18.59
TRC1	10.18	11.00	11.77	12.34	12.48	13.33	15.21	17.59	19.83	20.75	23.45	26.15
TRC2	7.27	7.59	7.38	7.40	7.32	7.93	9.88	10.18	11.63	12.81	14.87	16.91
TRC3	6.94	7.28	7.17	7.42	7.33	7.87	9.41	11.50	14.08	18.60	19.82	21.17
Turkey	14.24	15.07	15.71	16.09	16.44	17.34	19.41	21.47	23.97	25.13	27.35	28.91
TR-Low	11.11	11.64	12.02	12.30	12.60	13.40	15.63	17.52	20.11	21.51	24.32	26.20
TR-High	16.88	17.98	18.87	19.35	19.88	20.99	23.05	25.57	28.30	29.47	31.59	33.56

Source: MONE's National Education Statistics (Formal Education) for years 1992-2003.
Notes: 1- "Turkey" row shows the number of middle classes per 1000 children for the whole country.
2- "TR-Low" row shows the average number of middle school classes for those regions whose average was lower than that for the whole country in 1997(base year in construction of intensity measure).
3- "TR-High" row shows the average number of middle school classes for those regions whose average was higher than that for the whole country in 1997(base year in construction of intensity measure).

Table 2.3: Effect of the 1997 Reform on Education-OLS

	Years of Education			Middle School Completion		
VARIABLES	All	Female	Male	All	Female	Male
	Panel A: Restricted Sample (Educated in the Region of Residence)					
Treated	0.772***	1.067***	0.476***	0.189***	0.219***	0.158***
	(0.173)	(0.232)	(0.171)	(0.0299)	(0.0402)	(0.0252)
Observations	188,634	96,147	92,487	188,634	96,147	92,487
R-squared	0.221	0.274	0.156	0.230	0.270	0.151
	Panel B: Whole Sample (All Residents)					
Treated	0.815***	1.136***	0.489***	0.184***	0.227***	0.140***
	(0.113)	(0.165)	(0.114)	(0.0186)	(0.0263)	(0.0166)
Observations	258,131	135,097	123,034	258,131	135,097	123,034
R-squared	0.185	0.207	0.145	0.198	0.216	0.135

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.

2- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.

3- All regressions control for age, age square, a dummy for ongoing education as well as 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Table 2.4: Effect of the Intensity of the 1997 Reform on Education-OLS

	Years of Education			Middle School Completion		
VARIABLES	All	Female	Male	All	Female	Male
	Panel A: Restricted Sample (Educated in the Region of Residence)					
Intensity	0.191***	0.229***	0.148***	0.0411***	0.0527***	0.0288***
	(0.0296)	(0.0378)	(0.0268)	(0.00629)	(0.00866)	(0.00455)
Observations	188,634	96,147	92,487	188,634	96,147	92,487
R-squared	0.222	0.274	0.157	0.236	0.278	0.156
	Panel B: Whole Sample (All Residents)					
Intensity	0.159***	0.172***	0.138***	0.0313***	0.0387***	0.0228***
	(0.0231)	(0.0307)	(0.0226)	(0.00470)	(0.00658)	(0.00356)
Observations	258,131	135,097	123,034	258,131	135,097	123,034
R-squared	0.185	0.206	0.145	0.201	0.220	0.138

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1
2- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
3- All regressions control for birth year, the 26 NUTS-2 region, and year fixed effects as well as a dummy variable for ongoing education, an interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 2.5: First-Stage Estimates

	Years of Education			Middle School Completion		
INSTRUMENT	All	Female	Male	All	Female	Male
Treatment Dummy	1.007***	1.458***	0.645*	0.254***	0.141**	0.249***
	(0.334)	(0.532)	(0.328)	(0.0416)	(0.0579)	(0.0434)
Observations	62,116	17,173	44,943	62,116	17,173	44,943
R-squared	0.132	0.080	0.105	0.111	0.066	0.122
Intensity	0.200***	0.339***	0.170***	0.0315***	0.0352***	0.0334***
	(0.0363)	(0.0625)	(0.0319)	(0.00484)	(0.00716)	(0.00463)
Observations	62,116	17,173	44,943	62,116	17,173	44,943
R-squared	0.131	0.070	0.107	0.114	0.070	0.126

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1
2- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
3- All regressions control for age, age square (birth year fixed effects for the intensity of the reform), tenure in the firm and its square, a dummy for ongoing education, 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Table 2.6: Returns to Years of Education-OLS and 2SLS

VARIABLES	(1) OLS	(2) IV-1	(3) IV-2	(4) IV-1	(5) IV-2
			Panel A: All		
Years of Education	0.0507***	0.109***	0.0883***	0.102***	0.0890***
	(0.00192)	(0.0250)	(0.0175)	(0.0218)	(0.0166)
1st Stage F-stat		[9.545]	[30.76]	[10.29]	[28.95]
#Obs.	62,116	62,116	62,116	62,116	62,116
			Panel B: Female		
Years of Education	0.0640***	0.143***	0.0840***	0.139***	0.0824***
	(0.00258)	(0.0380)	(0.0246)	(0.0355)	(0.0239)
1st Stage F-stat		[9.002]	[29.94]	[9.108]	[29.66]
#Obs.	17,173	17,173	17,173	17,173	17,173
			Panel C: Male		
Years of Education	0.0444***	0.0725*	0.0855***	0.0634*	0.0886***
	(0.00193)	(0.0386)	(0.0211)	(0.0325)	(0.0197)
1st Stage F-stat		[4.061]	[28.17]	[4.794]	[26.72]
#Obs.	44,943	44,943	44,943	44,943	44,943
Region Effects	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES
Year*Region Effects	NO	NO	NO	YES	YES

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics of the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity is the instrument.
4- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
5- All regressions control for age, age square (birth year fixed effects in columns (3) and (5)), tenure in the firm and its square, a dummy for ongoing education, 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Table 2.7: Returns to at Least Middle School Completion-OLS and 2SLS

VARIABLES	(1) OLS	(2) IV-1	(3) IV-2	(4) IV-1	(5) IV-2
			Panel A: All		
Middle School	0.242***	0.437***	0.560***	0.434***	0.573***
Completion	(0.0145)	(0.105)	(0.120)	(0.101)	(0.114)
1st Stage F-stat		[38.92]	[42.66]	[39.84]	[42.63]
#Obs.	62,116	62,116	62,116	62,116	62,116
			Panel B: Female		
Middle School	0.381***	1.575***	0.817***	1.566***	0.797***
Completion	(0.0219)	(0.599)	(0.278)	(0.594)	(0.271)
1st Stage F-stat		[6.389]	[24.17]	[6.233]	[23.55]
#Obs.	17,173	17,173	17,173	17,173	17,173
			Panel C: Male		
Middle School	0.197***	0.191*	0.432***	0.181*	0.454***
Completion	(0.0141)	(0.112)	(0.110)	(0.107)	(0.102)
1st Stage F-stat		[33.81]	[52.93]	[36.47]	[56.20]
#Obs.	44,943	44,943	44,943	44,943	44,943
Region Effects	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES
Year*Region Effects	NO	NO	NO	YES	YES

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics of the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity is the instrument.
4- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
5- All regressions control for age, age square (birth year fixed effects in columns (3) and (5)), tenure in the firm and its square, a dummy for ongoing education, 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Table 2.8: Returns to Years of Education (Highest Degree attained is High School) -OLS and 2SLS

VARIABLES	(1) OLS	(2) IV-1	(3) IV-2	(4) IV-1	(5) IV-2
			Panel A: All		
Years of Education	0.0255***	0.109***	0.0561*	0.0977***	0.0600**
	(0.00150)	(0.0378)	(0.0313)	(0.0333)	(0.0304)
1st Stage F-stat		[11.04]	[15.39]	[11.96]	[15.58]
#Obs.	48,285	48,285	48,285	48,285	48,285
			Panel B: Female		
Years of Education	0.0338***	0.175*	0.0272	0.159*	0.0412
	(0.00188)	(0.0896)	(0.0607)	(0.0825)	(0.0575)
1st Stage F-stat		[4.774]	[8.341]	[4.877]	[9.765]
#Obs.	10,806	10,806	10,806	10,806	10,806
			Panel C: Male		
Years of Education	0.0228***	0.0982**	0.0544*	0.0844**	0.0606**
	(0.00168)	(0.0425)	(0.0292)	(0.0365)	(0.0273)
1st Stage F-stat		[10.30]	[17.34]	[11.69]	[18.46]
#Obs.	37,479	37,479	37,479	37,479	37,479
Region Effects	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES
Year*Region Effects	NO	NO	NO	YES	YES

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics of the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity is the instrument.
4- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
5- All regressions control for age, age square (birth year fixed effects in columns (3) and (5)), tenure in the firm and its square, a dummy for ongoing education, 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Table 2.9: Returns to at Least Middle School Completion(High-School is the Highest Degree)-OLS and 2SLS

VARIABLES	(1) OLS	(2) IV-1	(3) IV-2	(4) IV-1	(5) IV-2
			Panel A: All		
Middle School	0.114***	0.310***	0.207*	0.286***	0.224**
Completion	(0.00959)	(0.0930)	(0.111)	(0.0854)	(0.107)
1st Stage F-stat		[47.11]	[39.09]	[49.08]	[40.02]
#Obs.	48,285	48,285	48,285	48,285	48,285
			Panel B: Female		
Middle School	0.168***	0.806**	0.113	0.759**	0.178
Completion	(0.0137)	(0.375)	(0.255)	(0.360)	(0.252)
1st Stage F-stat		[7.755]	[18.53]	[7.367]	[20.18]
#Obs.	10,806	10,806	10,806	10,806	10,806
			Panel C: Male		
Middle School	0.0992***	0.260***	0.190**	0.228**	0.215**
Completion	(0.0102)	(0.101)	(0.0956)	(0.0909)	(0.0881)
1st Stage F-stat		[45.15]	[45.10]	[49.69]	[49.41]
#Obs.	37,479	37,479	37,479	37,479	37,479
Region Effects	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES
Year*Region Effects	NO	NO	NO	YES	YES

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1.
2- F-statistics of the excluded instruments in the first stage are in brackets.
3- IV-1: Treatment dummy is the instrument; IV-2: Intensity is the instrument.
4- Treatment group 1988-1990 birth cohorts vs Control group 1983-1985.
5- All regressions control for age, age square (birth year fixed effects in columns (3) and (5)), tenure in the firm and its square, a dummy for ongoing education, 26 NUTS-2 region, and year fixed effects. All regressions are weighted by the sample weights provided in the data set.

Chapter 3

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

3.1 Introduction

The prevalence of the two main public health problems, obesity and cigarette smoking, show interesting patterns among Turkish men and women. According to OECD health statistics in 2014 based on self-reported height and weight measures 24.5% of Turkish female population is obese which puts Turkey in second place after the USA among OECD countries while the prevalence of obesity among Turkish males is less than the OECD average. On the other hand, smoking is more problematic among Turkish men. OECD statistics show that in 2012, 37% of Turkish men are smokers which is the highest rate among OECD countries while the smoking rate for females is 10.7% which is one of the lowest rates among OECD countries. The percentage of obese people is increasing over time. Overall the obesity rate increased from 12% in 2003 to 19.9% in 2014. However; the smoking rate shows a declining trend. 43.6% of Turkish population who are 15 years old or older consume tobacco daily in 1995, while this percentage declines to 23.8% in 2012.

It is well documented that there is a strong relation between individuals' education level and health. The mechanism behind the association between health and education and the causality is highly debated in the literature with mixed evidence. Turkey provides an opportunity to investigate the causality from schooling to health besides the interesting statistics shown above. The Turkish government took serious measures to increase the formal schooling by introducing compulsory schooling changes in the last two decades. Compulsory schooling increased from 5 to 8 years in 1997 and from 8 to 12 years in 2012. The timing and implementation of the 1997 Basic Education Law provide a natural experiment to investigate the effect of education on labor market and non-market outcomes. The change in 2012 is still too young to measure its effect.

We use the 1997 Basic Education Law and the accompanying investments to increase the number of classrooms/teachers which vary across regions and birth cohorts to study the relationship between education and health outcomes/behavior. Our health outcomes are obesity/BMI, smoking behavior, and self-rated health. We also investigate the effect of education of women on infants' well-being. We show that the 1997 reform substantially increased the education of Turkish people. Using the new class openings in the 6th-8th grades as an intensity measure of the 1997 reform we find that, on average, one additional middle-school class increases the probability of completion of 8 years or more of schooling by 2%-5%. The reform itself and new class openings were more effective in increasing the schooling of women than that of men.

We find a significant relationship between health outcomes/health behavior and education in Turkey, using ordinary least squares. However, instrumenting education by plausibly exogenous policy changes, we could not find a significant evidence on the causal relationship between education and the health outcomes we considered.

Section 2 presents the related literature, while we discuss the Turkish education system and the compulsory schooling change in section 3. The data sets used in this paper are explained in section 4. The Model and the identification strategy is explained in section 5. The effect of the 1997 reform and new class openings on educational attainment is analyzed in section 6. Then, the effect of education on several health outcomes is examined in section 7. Finally, section 8 concludes the paper.

3.2 Literature Review

The relationship between health and education has been empirically well studied for different countries and different time periods. These studies include various health outcomes such as mortality rates, smoking, self-rated health variables, fertility, obesity etc. Higher returns to education on health have been found for poorer countries (Cutler and Lleras-Muney, 2006).

The mechanisms behind the relationship between schooling and health based on three models/approaches. Grossman (1972) modeled the production of health based on the seminal paper of Becker (1965) to study the productive efficiency resulting from formal schooling. According to this model more schooling allows individuals to produce health outcomes more efficiently, as individuals with more education accumulate more knowledge. On the other hand, some critiques of this approach argued that schooling would make individuals better in the allocation of resources in the production of health (see Grossman, 2006, 2015 for a comprehensive review). Schooling has a direct effect on health in a productive efficiency model while it has an indirect effect in an allocative efficiency model (Grossman, 2006). The question of the possibility of the omitted confounding variable was raised by Fuchs (1982). It is possible that there might be a third variable that affects both education and health in the same way, in turn producing a large correlation between these two variables. Fuchs (1982) points out that those who discount the future at a low or moderate rate would invest more in education as well as health. Furthermore, there might be a reverse causality such that individuals with poor health conditions might have lower educational attainment (Grossman, 2006).

The first generation studies focus on the estimation of this relationship by including covariates such as family background, socioeconomic and individual characteristics, to test the productive and allocative efficiency models. There were also attempts to include measures for omitted third variables such as time preference and ability. In the last two decades, the availability of more data and popularity of the use of the instrumental variables approach have attracted more attention on the investigation of the relationship between education and health (Grossman, 2015). Moreover, recent attempts to improve education in developing countries and the trends in health outcomes enabled more scholars to study the causality from education to health in developing countries. For a good review see Grossman (2006, 2015), Cutler and LlerasMuney (2006), and Eide and Showalter (2011).

The compulsory schooling changes are widely used as instruments in an attempt to circumvent the endogeneity of schooling or omitted third variable problems in the estimation of a wide range of health outcomes (Grossman, 2006). Health outcomes considered include mortality, self-rated health, the prevalence of illnesses, fertility, and health risk factors; the prevalence of obesity and smoking. Also, infant birth outcomes, such as birth weight, infant mortality, etc. are used to study the relationship between maternal education and infant health.

Lleras-Muney (2005) uses the compulsory schooling laws and child labor laws between 1915 and 1939 in the US as an instrument for the schooling of individuals in examining the effect of education on mortality rates. She points out that if more education results in better health, those individuals who live in states that increased the compulsory schooling would be healthier and live longer. She constructs synthetic cohorts from the censuses and follows these cohorts over time. Lleras-Muney (2005) assigns these cohorts into treatment and control groups depending upon their birth year which is used to instrument schooling. She finds a significant effect of education on the mortality rate and concludes that this effect might even be larger than those found in the literature without controlling for endogeneity. Mazumder (2008) reexamines the effect of education on mortality and other health-related outcomes and checks the results of Lleras-Muney (2005) using the same instruments. He tests the sensitivity of the large impacts found in Lleras-Muney (2005) to the inclusion of state-specific trends. The results show that the effect of education on mortality rates in the US are indeed sensitive to the inclusion of state-specific trends resulting in smaller, insignificant effects. He points out that the results in Lleras-Muney (2005) might be driven by some other policies related to increasing the nutrition of students in public schools which may overlap with the compulsory schooling law changes. Adams (2002) examines the self-rated health and functioning ability of older people. He uses family background variables as proxies for the unobserved characteristics in the OLS estimations. Furthermore, he uses the quarter of birth of individuals as an instrument for schooling following Angrist and Kruger (1992). He tries to deal with the weak instrument problem by using family background information as additional instruments. He concludes that education causes better health even controlling for omitted variables and the endogeneity problem.

The effect of schooling on smoking and obesity is examined by Kenkel, Lillard and Mathios (2006). They investigate whether the differences in smoking rate and the probability of being overweight could be causally explained by the education level differences using the NLYS-79. They use the state policies related to the high-school graduation and GED attainment as instruments for the high school completion as well as controlling for parental education, state cigarette taxes, and anti-smoking initiatives. Their instruments were quite strong in terms of the F-test of the first stage. The IV results show that high school completion reduces the probability of smoking by 23 percent for men and 10 percent for women, but they are estimated with low precision. However, they find no strong evidence on the effect of high school completion or GED attainment on the probability of being obese or overweight. de Walque (2007) considers the impact of education on the smoking behavior of individuals. He uses the probability of being inducted in the Vietnam War draft as an instrument for schooling pointing out that individuals avoid being drafted by going to college. The outcomes for his model is current smoking and the cessation of smoking. He finds that there is a causal negative effect of education on the probability of being a current smoker. The more educated people are less likely to smoke, by 6 percent, and in case they smoke, they are more likely to quit, by 7 percent, in their IV estimations.

Recent papers provide mixed evidence on the relationship between education and health using non-US data and natural experiments in developed and developing countries. Arendt

(2005) uses the Danish compulsory schooling reforms enacted in 1958 and 1975 as an instrument for schooling. He uses a Danish panel data set of employed people in examining the effect of education on self-rated health and BMI. The first stage estimates show that the reforms increased schooling; however, the instruments are weak. He finds that the effects are larger using IV estimation, but these estimates are insignificant.

Clark and Royer (2013) examine the effect of education on a wide range of health outcomes such as mortality, self-rated health, smoking behavior, and BMI employing a regression discontinuity design making use of education reform in the UK in 1947 and 1972 which increased the school leaving age from 14 to 15 and from 15 to 16, respectively. They show that the reforms had a substantial effect on increasing educational attainment. However, they find no significant effect of education on health outcomes or health behavior. Silles (2009) provides some evidence on the relationship between higher schooling and better self-rated health status using the same instruments used in Clark and Royer (2013) but with a different data set.

Kemptner, Jurges and Reinhold (2011) use the compulsory schooling changes in West Germany between 1949 and 1969 which increased the compulsory schooling from 8 to 9 years. The states implemented these changes in different years which creates an exogenous variation in education and can be used to study the relationship between education and health. They show that there is a significant effect of education on long-term illness of German men but not women. They find that one year education reduces men's BMI by 0.3 kg/m^2, probability of being obese or overweight by 3 percent. However, they could not detect any evidence on the effect of education on smoking behavior. On the other hand, Jurges, Reinhold, and Salm (2011) use the sharp increase in the number of grammar schools in 1960's and 1970's which substantially vary among West-German states as an exogenous shock in the estimation of the effect of education on smoking and obesity. They find that the increase in the number of grammar schools increased the educational attainment of Germans. Using this exogenous increase in estimating the effect of education on smoking and obesity, they find a large negative effect on current smoking rates for both men and women, -0.06 and -0.15, respectively. However, the effect for men became inconclusive when they considered only the cohorts which are most affected by the grammar school increase. They also find that one additional year of schooling increases men's probability of being obese (overweight) by 13 percent (12 percent). Again, considering a narrower range of birth cohorts, they find insignificant positive effects of education on the probability of being overweight or obese.

Park and Kang (2008) exploit a rapid increase in the number of high school classrooms in Korea in the mid-1970's and the birth order of Korean men as an exogenous source of variation in educational attainment. This exogenous source of variation is used in estimating the effect of education on health and health-related behavior. They find that education has a positive effect on health check-up and regular exercising; however, there are no statistically significant effects of education on smoking or drinking behavior of Korean men.

The effect of maternal education on the well-being of children is also widely examined. It is well documented that the infant's health is related to the education level of the mothers which in turn determines the well-being of children as an adult (Grossman, 2006). Currie and Moretti (2003) first provided evidence that this relationship is indeed causal from the mothers' education to infant health outcomes. They use the college availability in the county of the mothers when they were 17 years of age. They argue that the availability of a college in the county when a woman is 17 years old is exogenous once the county and year of birth are controlled for. They find that the college openings increased the educational level of white women. Exploiting this variation in an IV setting and considering various outcomes related to the infant's health, they find that increasing maternal schooling by one year would reduce the probability of low-birthweight birth by 2 percent and preterm delivery by 1 percent.

Breierova and Duflo (2004) investigate the effect of mother's and father's education on fertility and child mortality in Indonesia exploiting the primary school construction program in the 1970's. The Indonesian government went into a massive primary school expansion between 1973 and 1978 which lead to a substantial increase in the educational attainment of

Indonesian people. They specifically tried to test if the mother's education affects fertility decisions and child mortality more than the father's education. They instrument average family education or the difference in the years of education between the wife and husband with the primary school construction of which the intensity varies across districts over cohorts. Their IV results indicate that education has a significant effect on child mortality and there is no difference between the mother's or father's education. However, they find that female education is more effective on the fertility decision.

Chou, Liu, Grossman, and Joyce (2010) estimate the effect of parental education on several infant health outcomes such as low birthweight, neonatal deaths, etc. in Taiwan. They use the compulsory schooling change that took place in 1968 and the variation in the implementation of the law across regions over cohorts. The compulsory schooling increased from 6 to 9 years in Taiwan in 1968 and the government built new junior high schools to meet the expected increase in enrollment caused by the increase in compulsory schooling. The intensity of school construction varied across regions. This enabled the authors to instrument the schooling of parents in the estimation of infant health outcomes. They show that the increased number of junior high schools as a result of compulsory schooling change increases the years of schooling. Furthermore, they conclude that the parental education causes better health of infants, especially in terms of birthweight. They find that the increase in the mother's (father's) schooling which is caused by the 1968 reform reduces the number of low-birthweight births by 4.7 percent (4.2 percent). These effects are smaller than those found in their OLS estimates.

The association between individual education and either own health outcomes or well-being of their children in Turkey has recently attracted some attention. These studies mainly make use of the compulsory schooling change which increased the compulsory education from 5 to 8 years in August 1997 in Turkey. Dinçer, Kaushal and Grossman (2014) use the 1997 compulsory schooling change and the accompanying variation in teacher recruitments across regions over birth cohorts in estimating the effect of the female education on female own health and her children's well-being using the Turkey Demographic and Health Surveys (TDHS) for the years 2003 and 2008. They measure the intensity of the reform by the number of teachers (1st-8th grades) as a proportion of the number of primary education school aged (6-13 years old) children. This intensity measure varies across regions and years for which the student is 11 years of age. Intensity is interacted with a dummy variable which indicates whether an individual is affected by the reform. Furthermore, they control for the pre-reform trends by including interaction terms of birth cohorts with the middle school enrollment rate in 1996. They find that the increase in the number of primary school teachers associated with the compulsory schooling change increased the women's educational attainment in Turkey. Their IV results show that there is a significant negative relationship between education and number of pregnancies and the average number of kids of ever-married women. In fact, they find that a 10-percent increase in the proportion of women that completed at least 8 years of schooling is associated with 0.13 less children per woman. They also find weak evidence on the impact of mother's education on the child mortality, in fact, it loses its significance in their preferred estimation. The identification in the paper is based on cohorts that are affected by the reform and the intensity of the treatment which is defined as the teacher-child ratio. They define the first cohort affected by the law as the 1986 birth cohort. However, the school starting age is 6 in Turkey (Resmi Gazete, August 7, 1992). Thus, the 1986 birth cohort is expected to complete 5 years of primary school in June 1997 which is right before the law change. Hence, this cohort is not affected by the law. However, a small portion of the 1986 birth cohort, that deferred entrance to the primary school or repeated any grade in primary school and consequently could have been in the 5th grade in 1997/1998 and hence, affected by the law. There is no way to distinguish between treated individuals in the 1986 cohort from those that are not treated. Thus, inclusion of 1986 cohort in the treated group might be responsible for the weakness of the results.

Cesur, Dursun and Mocan (2014) estimate the effect of education on self-rated health, BMI and smoking behavior of Turkish men and women using the 2008, 2010, and 2012 waves of the Turkish Health Survey (THS). The relationship between education and health is examined by instrumenting education using the compulsory schooling law in 1997. They use a dummy variable which takes a value of 1 if an individual is born after 1986 and 0 otherwise. Furthermore, they exclude the 1986 birth cohort because of the fact that some people born in 1986 were affected by the reform while some were not. They restrict their attention to the birth cohorts 1982-1990, which means 4 birth cohorts in each treatment arm. Their OLS estimates confirm, in most of the cases, the positive effects of the education on health outcomes and health behavior. However, the IV estimates show that there is no significant effect of education on self-rated health, fruit consumption or smoking for both men and women. Furthermore, they find that having completed at least middle school increases men's BMI by $7.1 kg/m^2$ and the probability of being obese by 35 percent. However, there is no significant effect of education on women's BMI or propensity of being obese. These implausibly high estimates for men are explained by a possible detrimental effect of an increase in personal income and the change in the lifestyle of Turkish men over the last two decades.

Tansel and Karaoglan (2016) use the same data set as Cesur et al. (2014) in estimating the effect of education on health behavior in Turkey. They use some changes in the education system which mainly effects primary school (5-year) education in the early 1960's by comparing cohorts who are affected by the new system with those that are not. They use individuals older than 25 years old. They construct their treatment group as those born in or after 1952 and affected by the educational reforms in the early 1960s. They argue that a dummy variable indicating whether an individual is born in 1952 or later is a valid instrument for individuals' schooling. Moreover, they claim that the health reforms introduced in the early 1960s have no effect on individuals' health behavior. They find that there is no effect of education on the probability of smoking and exercising. Their 2SLS results show that one additional year of schooling increases BMI by 0.26 kg/m^2. This is significant only at the 10 percent significance level. However, their identification strategy is based on a comparison of very different cohorts. The treatment group contains individuals born between 1952 and 1987, while the control group includes all individuals born before 1952[1].

3.3 The Turkish Education System and the 1997 Basic Education Reforms

Until the summer of 1997, the education system in Turkey had a 5-year compulsory primary school, 3-year middle school, 3-year high school, 2- or 4-year college and graduate studies. The three-party-coalition government took an immediate and unexpected action in changing the compulsory schooling from 5-year to 8-year primary education. This was done by combining the 5-year compulsory primary school and the 3-year middle school. The primary school diploma which used to be awarded upon completion of the 5 years of schooling was abolished. The middle school diploma which used to be awarded upon completion of the 3 additional years of schooling after primary school was replaced with a primary education diploma which is awarded upon completion of the 8 years of schooling.

The idea of increasing the compulsory schooling was on the government's agenda for a long time but it could not be implemented because of the lack of physical requirements. The Basic Education Law, which increased the compulsory schooling, was enacted in August 1997 (Law No: 4306) [2] due mainly to political reasons rather than economic incentives with almost no public discussion on it. Turkey experienced several economic crises in the 1990's in which 11 different government cabinets were formed. This political and economic instability in the 1990's is paired with the military intervention in February 1997. The National Security Council held a lengthy meeting on February 28, 1997, in which the military wing of the council expressed their concerns about the rise of the religiosity in the country. The military

[1] As a check of this identification strategy, we used a different cut-off birth year randomly before 1952 which gave a similar or better strength and power in the first stage. This shows that an identification strategy based on comparison of two very different groups may not be valid in the estimation of the effect of education on health in Turkey.

[2] Resmi Gazete, No: 23084; August 18,1997

wing of the council sent a memorandum to the government requesting the resignation of the Prime Minister Necmettin Erbakan, who was the leader of the religious Welfare Party (Refah Partisi). The memorandum also included the armed forces "recommendations" for an increase in compulsory schooling (Günay, 2001). Consequently, Necmettin Erbakan resigned in June 1997 and a new three-party-coalition government was formed on June 30, 1997. This newly established government took the "recommendations" of the National Security Council and passed The Basic Education Law on August 16, 1997[3].

The Basic Education Law required all students in the 5th or lower grades in the 1997/1998 school year to complete 8 years of education in schools which had a universal secular curriculum. There were elective-curricula middle schools (6th-8th grades) including vocational, technical and religious middle schools besides the secular-curricula middle schools before 1997. The government cut off religious studies in the 6th-8th grades by abolishing all elective-curricula middle schools and implementing the Basic Education Law as "8-year continuous compulsory basic education".

This unprecedented enactment of the law created a huge need for investments in new classrooms and teachers to meet the expected increase in the number of students in the 6th through 8th grades. The Ministry of National Education(MONE) increased the capacity of primary education by 3 million through the construction of new classrooms and recruitment of teachers to implement of the reform (Dulger, 2004). A detailed discussion of the Turkish education System and the Basic education law can be found in Dulger (2004) and Karatas (2017).

The school entry policy in primary school also deserves a special attention as it is crucial in the identification of the individuals who are affected or just missed the reform. The school starting age is determined by the calendar year rather than the school year. The enrollment rule is determined by the byLaw published on August 7, 1992 in the Official Gazette of the Republic of Turkey, which states that: "All children who completed 72 months at the end of

[3]See Gunay (2001) for a detailed discussion of the "eighteen recommendations" of the National Security Council.

the calendar year shall be enrolled in the 1st grade. Those who are required to be enrolled but physically underdeveloped can continue pre-school education one more year with the written consent of the parents."[4] (Resmi Gazete, August 7,1992). The school starting age was not strictly enforced. The byLaw allows some students to defer their enrollment one year. There are also early entrants of which birthday is close to the 72-month cut-off (Cesur et al., 2014). The national education statistics show that early enrollment consists of a small portion (around 5%) of each birth cohort (National Education Statistics, 1991-2003).

3.4 Data

The individual-level data sets used in this study are the Turkish Health Survey (THS) for the years 2008, 2010, 2012, and 2014. Also, the Turkey Demographic and Health Survey (TDHS) for the years 2008 and 2013 are used. Both surveys are nationally representative; however, each has strengths and weakness. We restrict our attention to those born between 1982 and 1991.

The Turkey Demographic and Health Survey (TDHS) has been carried out by Hacettepe University Institute of Population Studies (HUIPS) since 1968. This is done in every five years and collects data on socio-demographic characteristics of households such as age, education, marital status, region of birth of each member of the household. The main interest of the survey is on ever-married women aged 15-49 for which very detailed information on individual characteristics as well as marriage, fertility, migration, health of mothers and their children are obtained. The TDHS conducted individual interviews with ever-married women aged 15-49 until 2008. In the TDHS-2013 all women aged 15-49 regardless of their marital status were individually interviewed. The TDHS has some unique advantages over other nationally representative surveys. The TDHS provides very detailed information on individuals' education, age, birth year, region of birth, region of residence, region of childhood, and migration history. Moreover, the TDHS reports women's measured height and weight

[4] Authors' translation

as well as very detailed information on marriage, fertility, use of birth control methods and their children's health.

The Turkish Health Survey (THS) is conducted by the Turkish Statistical Institute (TURKSTAT) biennially, since 2008. The THS is a nationally representative survey which aims at providing health indicators such as infant and child health conditions, adults' self-reported health, height and weight, smoking behavior, and utilization of health services covering all settlements in the territory of Turkey except for the institutionalized population. The THS is a self-reported survey, unlike the TDHS. The biggest advantage of the THS over the TDHS is that it covers all men and women, unlike the TDHS which covers only women aged 15-49.

We use the 1982-1991 birth cohorts from both surveys. The THS provides the completed age of the respondents, so we obtained the birth year by subtracting the completed age from the survey year. The birth year is crucial in our identification as it allows us to assign individuals into control and treatment groups. Since it is not known whether the respondent was surveyed before or after his/her birthday, any birth year obtained by the survey year and completed age includes individuals from two birth cohorts. For example in the 2010 THS survey, an individual that was born in 1987 and surveyed after his/her birthday would report the completed age as 23, while another individual who was born in 1986 and surveyed before his/her birthday will also report the completed age as 23. Thus, a birth cohort obtained by subtracting completed age from the survey year, which in this example is the 1987 birth cohort, consists of some of those whose actual birth year is 1986 and others whose actual birth year is 1987. In contrast, the TDHS provides the actual birth year. But in our analyses, we use the calculated birth year for both data sets for comparability reasons. We excluded the 1986 and 1987 birth cohorts in our analysis because we were unable to distinguish between those affected and those who are not affected by the law. We will discuss this in more detail in the next section. Moreover, observations with missing values in our outcome variables and education variables are excluded in our analyses. Summary statistics of selected variables is presented in Table 3.1 for both the THS data (Panel A) and the TDHS data (Panel B).

The implementation of the reform varies across regions and this exogenous variation is used for instrumenting schooling in our model. Therefore, the information on the region where an individual had his/her schooling is important. The TDHS provides the region of birth, region of residence, the region where an individual spent most of his/her time until the age of 12 (hereafter termed as "the childhood region"). On the other hand, the THS provides only the region of residence[5].

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

[5]Region names are not provided in the data set but randomized numbers are assigned to the regions in 12 NUTS-1 and 26 NUTS-2 levels. We used the population of each region in each survey year and the sample proportions of each region in the surveys as well as the number of subregions to determine which number is assigned for each 12 regions. There were only two regions that didn't match in each year. After assigning all other regions, the average years of schooling for these two regions is used to assign the numbers provided in the survey with the names of the regions. Also, this way of assignment of the regions matched with the assignment of the regions by the ascending order of the names of the region. However, we couldn't find a good way to assign regions in NUTS-2 level.

Our intensity measure also requires the information on the population of children aged 11-13 by regions for each year. We used the 1990, 2000 Censuses and the 2007 Address Based Population Registration System[6], obtained from TURKSTAT, and inter-census years are interpolated assuming a constant growth rate of the population.

3.5 The Model and Identification Strategy

3.5.1 The Model

In this section, we analyze the effect of education on the individuals' health assuming a Mincerian type equation:

$$H_i = \beta_1 S_i + \beta_2 X_i + u_i \tag{3.1}$$

where the health outcome or health behavior, H_i, is determined by schooling, S_i, observed individual characteristics and related factors, X_i, and an unobserved error component u_i. In this model, schooling might have a direct effect which is defined as productive efficiency by Grossman(1972) or allocative efficiency. Schooling can be endogenous since an individual with poor health will have low schooling. Moreover, there might be some omitted variables, such as ability or time preference that would affect the individuals' decisions on both health and education. Thus, any estimation based on equation (3.1) would suffer from endogeneity. To instrument for schooling, we consider the following equation;

$$S_i = \gamma_1 T_i + \gamma_2 X_i + v_i \tag{3.2}$$

where schooling is determined by the excluded variable, T_i, the same variables included in X_i in equation (3.1), and an unobserved v_i. We use the compulsory schooling change in 1997 in Turkey and the new class openings which varies across regions as T_i in the examination

[6]Address Based Population Registration System began in 2007 and TURKSTAT releases population statistics on it website.

of the effect of education on health outcomes and health behavior.

The health variables we consider in our analysis are self-rated health, having health problems in the last 6 months, body mass index (BMI), smoking behavior, and infants' birth weight. BMI is defined as the weight of an individual in kilograms divided by the square of his/her height in meters. We use BMI as a continuous variable, and we define a dummy variable for being obese ($BMI \geq 30kg/m^2$), a dummy variable for being overweight ($30kg/m^2 > BMI \geq 25kg/m^2$). Smoking behavior is also defined with several variables; a dummy variable for whether an individual ever regularly smoked, a dummy variable for quitting smoking, the number of cigarettes smoked per day.

Schooling is given as years of schooling and completed level of education in the TDHS. However, only completed level of education is available in the THS. We imputed years of education in the THS data by the years of schooling required to complete each level of education. Thus, for those individuals who completed some years in any level of education but did not finish that level, are assigned the years of education required for the last completed education level. For example, a person who left school after the 10th grade, completed middle school (basic primary education) but not high school. This individual will be assigned 8 years of schooling. Hence, the imputed years of schooling variable will introduce bias in some degree. In order to circumvent this bias we define another schooling variable, completion of at least middle school, which is a dummy variable indicating whether an individual completed at least middle school (8th grade) or not.

3.5.2 The Identification Strategy

The identification strategy is based on the Basic Education Law enacted in 1997 which is defined in Section 3. The law increased the compulsory schooling from 5 to 8 years of schooling. Students used to be awarded a primary school diploma upon completion of 5 years in the pre-1997 period; however, the earliest diploma can only be obtained upon completion of 8 years of schooling under the new regime. Since this diploma is considered crucial in the labor market, the abolishment of the primary school diploma created some incentives to continue schooling until the end of the 8th year. Moreover, as it is mentioned in Section 3, the law change was driven by political incentives. The construction of new classrooms and recruitment of new teachers to accommodate the expected increase which shows variation across regions based on their pre-reform enrollment rate also provide an exogenous variation in the implementation of the new compulsory schooling law. We took advantage of these exogenous variations in education to explain the impact of education on health.

The Basic Education Law (Law No: 4306) was enacted on August 16, 1997 and the first group affected by the law were those in the 5th grade in the 1997/1998 school year. Therefore, the birth year and school starting age play a crucial role in the determination of those who are affected and who just missed the reform. Those who started the 1st grade in 1993 were in the 5th grade in the 1997/1998 school year. Since the byLaw of MONE's primary schools issued in 1992 defines the school starting rule as those who finish 72 months at the end of the calendar year will be enrolled in the 1st grade in the month of September in that year. In this case, the 1987 birth cohort should have started the 1st grade in 1993 and would be the first cohort affected by the reform. However, the exception in the byLaw allows children who are physically underdeveloped to be able to defer enrollment by one year. Therefore, some children born in 1986 were also affected by the reform. We define a dummy variable, T_i, which is 1 for those affected by the reform and 0 for those who missed the reform. There is no way to distinguish between the affected and unaffected groups in the1986 birth cohort because of the reasons mentioned earlier. We, therefore, exclude the 1986 birth cohort in our analyses. Furthermore, since the THS has no birth year variable and we are forced to obtain it using the survey year and the completed age; the computed 1987 birth cohort represents some people whose actual birth year is 1986 as well as others whose actual birth year is 1987. Thus, we further exclude the 1987 birth cohort from our analyses.

Instrumenting education by a dummy variable for the law change has been employed by Lleras-Muney (2005), Angrist and Kruger (1992), Cesur et al. (2014), and Karatas (2017) to mention a few. However, the identification is based on a comparison of two groups. The control and treatment groups are assumed to be similar except for their treatment status. If there are other policy changes or shocks that affect the treatment and control groups differently in their outcome variable, the identification strategy would be jeopardized and become invalid.

The 1997 reform was universal across the country. However, the intensity of the reform varied across regions based on the pre-reform enrollment rates, and classroom and teacher endowments. The Eastern Turkey regions were lagging behind in the enrollment rates in the 6th-8th grades. One of the main objectives of the reform is referred to as the inclusion of those "hard-to-reach" group of students who live in rural areas and have low socio-economic background (Dulger, 2004). Thus, measuring the intensity of the reform would provide us with another source of variation across regions and cohorts.

The Turkish government, in collaboration with international organizations and the private sector, took an immediate action in constructing new classrooms, repairing existing ones and recruiting new teachers to meet the expected increase in the 6th-8th grades. These investments varied across regions based on the pre-reform endowments of the regions. The investments in the classroom and teachers would induce more students to attain more schooling. Thus, the larger the intensity of the reform, the larger is the effect of the reform on educational attainment (Chou et al., 2010). The regional and temporal variation in the investments is used in the literature as exogenous variation in educational attainment. Currie and Moretti (2003) use the variation in the availability of colleges in the residence county in the US, Duflo (2001), and Breierova and Duflo (2004) use primary school construction boom in Indonesia, Chou et al. (2010) use the middle school construction variation along with compulsory schooling change in Taiwan, Dincer et al. (2014) use the regional variation in the number of primary-school (1st-8th) teachers to instrument educational attainment in search of the effect of education on various outcomes, such as wages, infant mortality, smoking, and marriage decisions. Following this literature, Karatas (2017) shows that using variations in the number of classes in middle school (6th-8th grades) is a strong tool for measuring the intensity of the reform. It turns out to be a good instrument for education in Turkey using the Turkish Household Labor Force Surveys (THLFS) over the period 2009-2013.

We use regional and temporal variations in the new class openings to construct an intensity measure for the 1997 reform. First, we show the trends in the number of middle school classes per 1000 children aged 11-13, which we call class-to-child ratio, for the school years 1992-2003. Figure 3.1 shows that after 1997 there is a sharp increase in the number of middle school classes per 1000 children. The average increase in the class-to-child ratio is around 20% from 1992 to 1997. The rates of increase is three-folds between 1997 and 2003. This increase varies across regions and the year at which a birth cohort starts the 6th grade. Table 3.2 shows the disaggregated class-to-child ratio by region and school year. The rate of increase in the class-to-child ratio were similar across regions in the pre-reform period; however, it varies between 34% (TR1-Istanbul) and 117% (TRC-Southeast Anatolia) after the reform. The 12 NUTS-1 regions are grouped as TR-Low and TR-High based on their class-to-child ratio in 1997. Regions with a class-to-child ratio that is lower than the average ratio of the whole country (TRA, TRB, TRC) are represented by "TR-Low" and the rest of the regions are represented by "TR-High". The average class-to-child ratio for TR-Low and TR-High are also shown in Figure 3.1. It is shown that the average rate of increase in the class-to-student ratio is a little bit higher in TR-High regions in the pre-reform period; however, the average increase in the TR-Low regions is steeper in the post-reform period.

Showing the variations in the number of middle-school classes per 1000 children across regions and cohorts that is more apparent after 1997, we are confident that the sharp increase is related to the 1997 reform. Hence, we create an intensity measure, which is the same as that in Karatas (2017), except, here, we use 12 NUTS1 regions instead of 26 NUTS2 regions, following Duflo (2001) and Chou et al. (2010). Figure 3.1 and Table 3.2 show that there is a huge increase after the 1997 reform. Thus, we assume that the new class openings associated with the 1997 Basic Education Law measures the intensity of the reform across regions and cohorts and use 1997 as the base year in constructing the intensity measure. Then, the cumulative new class opening for each year after 1997 is computed as the difference in the number of classes in a specific year and the base year 1997. This cumulative number of new middle school classes is then expressed as per 1000 children aged 11-13 in each 12 NUTS1 regions. This measure is termed the intensity of the reform. Note that intensity of the reform differs from class-to-child ratio as the first one considers only the number of new middle-school class openings associated with the 1997 reform. This intensity of the reform in each year is matched with the cohort that starts the 6th grade in that year. For example, the 1988 birth cohort starts the 6th grade in 1999, so they are assigned the intensity measure of the reform in 1999; and the 1989 birth cohort is assigned the intensity in 2000 and so on.

3.6 The Schooling Effect of the 1997 Reform and New Class Openings

It is shown that the 1997 Basic Education Law and the middle-school class openings increased the educational attainment of Turkish men and women significantly in Karatas (2017) using the THLFS data. Since the THLFS provides information on the region of residence and whether an individual has been living in the same region throughout his/her life, the effect of new class openings on schooling is estimated with a sample of individuals who have not migrated and educated in the region they live. The estimated effect might be mitigated if individuals who are induced to attain more education by new class openings migrates to another region. Therefore, it is important to know the region that an individual is educated to gauge the effect of the new class openings associated with the 1997 reform. However, we were able to identify only the region of residence in the THS data. On the other hand, the TDHS reports information on region of birth, region of residence and the region in which the individual lived until the age of 12. In this section, we start assessing the effect of the reform by defining a dummy variable representing the treatment status of the individual. Then, we estimate the effect of the intensity of the reform on educational attainment using the THS data. Lastly, the TDHS data is used to check the robustness of the intensity measure making use of the information on the region in which the individual lived until the age of 12. Furthermore, we test whether there is a spillover effect of the reform and its intensity on education levels beyond middle school using the actual years of schooling of ever-married women in the TDHS.

The 1997 Basic Education Law increased the compulsory schooling from 5 to 8 years with an intention to increase the educational attainment of Turkish people. Thus, we define a dummy variable, T_i, indicating the status of the treatment by the 1997 reform. The effect of the reform then is estimated using the following equation;

$$S_{ijt} = \gamma_1 T_i + \gamma_2 X_{ijt} + \mu_j + \lambda_t + v_{ijt} \tag{3.3}$$

where S_{ijt} is schooling of individual i in region j at time t. Schooling is represented by the imputed years of education (for the THS data) or a dummy variable indicating whether an individual attained 8 years or more of schooling. T_i is a dummy variable which takes a value of 1 if an individual was born after 1987 (1986 and 1987 birth cohorts are excluded because of the reasons mentioned above). X_{ijt} includes control variables such as age, female dummy, etc.. μ_j represent the 12 NUTS-1 region dummies which are used to control for the time-invariant region characteristics that might affect the education of individuals. λ_t represents dummy variables for each survey year which captures the time fixed effects in the educational attainment. All standard errors are clustered by region and birth year.

Education is recorded as the last completed degree in the THS. We imputed the years of education assigning the years required to attain a given degree. This may not show the actual years of education as the treated group might be in school at the time of the survey.

For example some people in the treatment group might be in college; however, they will be assigned to complete only 11 years of schooling which is required to attain a high school diploma. Thus, the years of education may not be a good indicator for educational attainment. Therefore, we prefer the dummy variable which indicates middle school completion in investigating the effect of the reform on educational attainment. Furthermore, the effect of the reform is expected to be higher for female as the middle school completion rate was lower for women than that for men in the pre-reform years. Therefore, we estimated equation (3.3) for all individuals as well as for females and males separately. The OLS results are shown in Table 3.3. The first three columns use the years of education while the last three columns use a dummy variable indicating whether an individual completed at least middle school (8th grade) in the estimation of equation (3.3).

Table 3.3 shows that the 1997 reform is effective in increasing the educational attainment in Turkey. The compulsory education law increased the years of education by 0.80 years. Also, it is found that females significantly attain less schooling than males[7]. The columns 2 and 3 show that the 1997 reform increased the years of education for women more than that for men, 0.90 and 0.75, respectively. The last three columns indicate that the 1997 reform increased at least middle school completion by 17 percent. The effect is found to be higher for women than for men, 23 percent, and 11 percent, respectively. Overall, Table 3.3 shows that the 1997 reform was effective in increasing the educational attainment in Turkey. These estimates are very similar to those found in Karatas (2017), Cesur et al. (2014), and Tansel and Karaoglan (2016). However, note that the estimation based on comparisons of the two groups would be biased if there are other policy changes that would affect control and treatment groups differently in their educational attainment. For that reason, we make use of the variation in the implementation of the reform that is measured by the variation in the new middle-school class openings across regions and birth cohorts. Following Duflo

[7] The coefficient of female dummy as well as other covariates is not reported here for space reasons.

(2001) and Chou et al. (2010) we estimate the following equation:

$$S_{ijkt} = \gamma_1(P_{jk} * T_i) + \gamma_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + v_{ijkt} \tag{3.4}$$

where T_i is the same dummy variable that is used in equation (3.3) which is 1 for individuals born after 1987. Note that equations (3.3) and (3.4) differ only in the variable representing the change in compulsory schooling. Here, P_{jk} is the intensity measure for cohort k in region j, which represents the number of new middle school class openings in region j at the time cohort k starts 6th grade. We controlled for the birth cohort fixed effects θ_k as well as the controls in equation (3.3).

The OLS estimates for γ_1 in equation (3.4) are shown in Table 3.4 for the whole sample as well as for males and females, separately. As in Table 3.3, we present the OLS estimates for the dependent variables; years of schooling and at least middle school completion. The OLS estimates in Table 3.4 show that on average, one additional new middle-school class opening associated with the 1997 reform increases the years of schooling of the treatment group by 0.11 years. The impact is higher for women than men in terms of years of education attained, 0.16 and 0.04, respectively. However, the estimate for men is insignificant. The same pattern is found when the educational attainment is represented by the middle school completion dummy. The fourth column of Table 3.4 indicates that one additional new middle-school class opening per 1000 children, on average, increases the treated group's probability of completing at least middle school by 2.5 percent. The OLS results for females and males in the fifth and sixth columns indicates that the new class openings associated with the reform was more effective in increasing the women's probability of completion of middle school or higher education by 3.4 percent. The impact of the new class openings on the men's probability of completing middle school or higher is 1.3 percent but it is insignificant. Note that these findings are in line with the findings of Karatas (2017) but here the effect is found to be weaker especially for males. This might be a result of the relatively small sample size of men, which may be under-represented. Also, the main motivation behind the use of middle-school class openings as a measure of the intensity of the 1997 reform is that the higher is the number of available classes the higher the probability of continuing the education which varies across regions and school year. Recall that the THS provides information on the region of residence. Thus, the possibility of migration would bias our estimates. Karatas (2017) shows that using the region of residence as a proxy for the region in which an individual received his/her education leads to smaller estimates of the effect of the intensity on educational attainment. Here, we take advantage of the TDHS to address these concerns.

The TDHS collects data on basic demographic characteristics of household members with the main interest being women aged 15-49. The TDHS provides very detailed information on education, birth year, the region of birth, region of childhood and region of residence, measured height and weight as well as fertility, use of birth control methods, children health for a nationally representative sample of ever-married women aged 15-49 in TDHS-2008 and a sample of all women aged 15-49 in TDHS-2013. The married women were also asked about their husband's age, education, region of childhood. The education is recorded as the completed years of education and the last completed level of education. The women are asked about their province of birth and the province that they spent most of their time until the age of 12, which is called the region of childhood. Therefore, we can use the information on women's own education as well as their husband's education, age, and childhood region to test our results obtained by using the THS to address our concerns in the use of the intensity of the reform. The data shows that around 22 percent of the sample of individuals who were born between 1982 and 1991 (the 1986 and 1987 birth cohorts are excluded) reside in a different region from their region of childhood[8].

Table 3.5 shows the OLS estimates of γ_1 in equation (3.4) using the TDHS-2008 and TDHS-2013 for different samples in which we matched the intensity measure based on the

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

childhood region of the individuals. Table 3.5 shows that, on average, one additional middle-school class opening per 1000 children increases the years of schooling of the treatment group by 0.28 years. The second column of Table 3.5 shows that the new class openings increased women's educational attainment by 0.24 years. The third and fourth columns show the OLS estimates for the ever-married sample. It is found that the impact is higher for husbands than for wives, 0.26 and 0.34 years, respectively. However, the coefficient for the husbands is significant at the 10 percent level.

The last four columns of Table 3.5 show the coefficients of the intensity on the probability of completing at least middle school. As we mentioned before, the middle-school completion is our preferred schooling measure because the treatment group might still be in school in the survey year. The coefficient of interest γ_1 is estimated significantly for all samples. The fifth column of Table 3.5 shows that, on average, one additional middle-school class opening associated with the 1997 reform increases the probability of completing at least 8 years of schooling by 7 percent which is almost triple the size found in Table 3.4. The sixth column shows that the impact on all women is 7.1 percent. For the ever-married sample, it is found that the middle-school class openings are more effective in increasing the treated women's probability of completing at least 8 years of schooling with a magnitude of 8.4 percent. The middle-school class openings also increased the ever-married men's probability of completing middle school or higher by 7.2 percent. However, we cannot make inference on the actual size of the impact on males' education because of the lack of information on unmarried men. Table 3.5 reveals that new middle-school class openings per 1000 children associated with the 1997 reform is effective in increasing the educational attainment of Turkish people and the size of the effect is higher than that found in Table 3.4.

Furthermore, we examine the effect of the reform and the intensity of the reform on different levels of education following Duflo (2001) and Karatas (2017) using the equations (3.3) and (3.4) with the TDHS data. We define "m", for $m = 1 to 17$, dummy variables indicating whether an individual completed at least "m" years of education or not. Then, the effects of the reform (using equation (3.3)) and the intensity of the reform (using equation (3.4)) are estimated for each dummy variable using linear probability models. The estimated coefficients, γ_1, and the corresponding 95% confidence intervals are plotted in Figure 3.2. The shape of Figure 3.2 shows the education level in which the reform and the intensity of the reform were effective. Figure 3.2 (a) shows the estimated effects of the reform on each level of education. It indicates that the 1997 reform was effective in increasing the ever-married women's probability of completing middle school (6th-8th grades) with a magnitude of around 30 percent. There are some spillovers beyond middle school; however, the effects become insignificant beyond the 9th grade. Moreover, Figure 3.2 (b) shows the estimated effects of the intensity of the reform on each level of education using equation (3.4). Note that the regional intensity measure is matched to the individual-level data based on the region of childhood. Figure 3.2 (b) shows that one additional middle-school class opening per 1000 children in the childhood region increases the ever-married women's propensity of completing middle school (6th-8th grades) by around 8 percent. There are no significant spillover effects beyond the 8th grade. These findings are similar to that found in Karatas (2017) except for the insignificant spillovers beyond the middle school. Note that Karatas (2017) use all women sample, while we use the ever-married women sample here.

Overall, we find that the 1997 reform and the new middle-school class openings at a differential rate across regions increased the educational attainment significantly. The impact is more pronounced on women's educational attainment than men's. The reform and the intensity of the reform were mainly effective in increasing middle school (6th-8th grades) education. Also, we find that the use of the region of residence as the region that education is attained mitigate the size of the effect. Furthermore, a dummy variable indicating competing middle school or higher education better represents educational attainment of individuals.

3.7 The Effect of Education on Health Behavior and Health Outcomes

Having shown that the 1997 Basic Education Law and the variation in the implementation of the reform across regions increased the educational attainment, we start analyzing the effect of education on self-rated health, smoking, BMI, obesity and being overweight. Note that the estimates in the previous section serve as our first stage estimates. We also examine the effect of maternal education on the well-being of infants which is measured by the birthweight of babies. The estimated model is the following:

$$H_{ijkt} = \beta_1 S_{ijkt} + \beta_2 X_{ijkt} + \mu_j + \theta_k + \lambda_t + u_{ijkt} \tag{3.5}$$

where H_{ijkt} is a health outcome or a health behavior for individual i in region j who is in birth cohort k and observed in survey year t. This is a function of schooling S_{ijkt}, individual characteristics X_{ijkt}. Region fixed effects, cohort fixed effects and survey year fixed effects are represented by α_j, θ_k , and λ_t, respectively. The coefficient of interest β_1 is estimated by OLS and instrumental variables (IV) and the estimates of β_1 are presented for each health outcome or health behavior considered separately in Tables 3.6-3.12. The schooling variable used in our estimates is a dummy variable indicating whether an individual completed middle school or higher. We also present estimates using the years of schooling, even though they are weak in terms of the first stage F-statistics. The IV estimates, for each dependent variable considered, presented for instruments, T_i (treatment dummy) and $P_{jk}T_i$ (intensity of the program) for middle-school completion.

3.7.1 BMI, Obesity and Being Overweight

The first health outcome is related to weight problems. The weight problems for men and women show some interesting patterns in Turkey. According to the OECD Health Statistics

2014, the obesity rate for women ranked 2nd place among OECD countries, while, the obesity rate for men is below the OECD average. Figure 3.3 (a) shows the proportion of obese and overweight men and female by birth cohort using THS data. The figure indicates that the prevalence of being obese and overweight is lower among younger cohorts for men and women. The rate of obesity is higher for females than for males. However, the rate of being overweight is more pronounced among male cohorts than among female cohorts. Furthermore, Turkish women have lower educational attainment than Turkish men do. Would the educational attainment discrepancy be responsible for the discrepancy in the obesity rates among Turkish women and men?

Table 3.6 shows OLS and 2SLS results for equation (3.5) with the natural logarithm of BMI, being obese, and being overweight as the dependent variable using the THS data for the whole sample (Panel A) as well as for women (Panel B) and men (Panel C). The schooling variable is having completed at least middle school. All standard errors are clustered by birth cohort and region. The first column of Table 3.6 shows the OLS results for log of BMI. It is found that having at least middle school education decreases the body mass index by 2 percent for the whole sample. Disaggregation of the sample shows that middle school completion decreases the BMI of women by 4 percent but increases the BMI of men by 0.3 percent. However, the estimated coefficient for males is not statistically significant. The effect of education on BMI might not be linear, therefore, we defined dummy variables for being obese or not ($BMI \geq 30kg/m^2$) and being overweight ($30kg/m^2 > BMI \geq 25kg/m^2$) and the. Columns (4) and (7) show the OLS results for the dependent variables being obese and being overweight, respectively. We find that having at least middle school education decreases the probability of being obese and being overweight for the whole sample by 2 percent for each of the dependent variables. It is also found that education is more effective in decreasing the propensity of being obese or overweight for women than for men. Women with at least 8 years of schooling are 5 percent less likely to be obese and 4 percent less likely to be overweight relative to those women who have less than 8 years of schooling. The effect of middle school completion increases Turkish men's probability of being obese or overweight by 1 percent. However, the estimated effects are insignificant for men.

Having shown the negative effect of educational attainment on weight problems for Turkey, which is also found in other studies (see Cesur et al., 2014; and Tansel and Karaoglan, 2016) and the differential effects on female and male samples, we present our 2SLS results. We use two different instruments, which we defined earlier as a dummy variable for being treated (IV-1), and an intensity measure of the 1997 reform (IV-2). Columns (2), (5), and (8) show the 2SLS results of equation (3.5) instrumenting the middle school completion by the treatment dummy, while columns (3), (6), and (9) show the 2SLS results instrumenting schooling variable by intensity of the 1997 reform. The F-statistics of the excluded instruments in the first stage (shown in brackets) are greater than 10 indicating that for the whole sample both instruments are not weak. This follows the rule of thumb suggested by Staiger and Stock (1997). On the other hand, the intensity measure of the 1997 reform is found to be weak for female sample. For the men sample, both instruments become weak. Cesur et al. (2014) find that the treatment dummy is not a weak instrument for male sample. This discrepancy might be driven the fact that we exclude the 1987 birth cohort, unlike Cesur et al. (2014). As has been discussed, the 1987 birth cohort might include some people that are not affected by the law. Moreover, Karatas (2017) shows that the intensity of the reform is a strong instrument for schooling using a large sample of men and women who are educated in their region of residence. Recall that we are forced to use the region of residence as a proxy for the region of education. We showed that this procedure introduces some bias in our first stage estimates. Therefore, the relatively smaller sample size and the use of region of residence might be the driving factors behind the weakness of our instrument.

The 2SLS results in the second column of Table 3.6 show that instrumenting middle school completion by the treatment dummy there is a positive effect of education on BMI with a magnitude of 9 percent for the whole sample, but it is only significant at the 10%level. The effects are found to be positive for male and female samples, but none of them are significant.

Moreover, the third column shows that instrumenting the schooling by the intensity of reform, there is a 5 percent reduction in the BMI of the whole sample. However, it is not significant. Moreover, there are no significant effect of education on BMI for males and females when we instrument the education variable with the intensity of the 1997 reform. Furthermore, we find that there is a positive effect of middle school completion on the probability of being obese for the whole sample as well as for the female sample and a negative effect for the male sample using both instruments. However, none of the 2SLS estimates for being obese are significant. Lastly, we could not find any significant effect of having at least 8 years of schooling on the probability of being overweight.

Alternatively, we replicate Table 3.6 using imputed years of education as schooling variable instead of the dummy variable indicating the middle school completion. The OLS and 2SLS results shown in Table 3.7. The first column of Table 3.7 shows that one additional year of education decreases the BMI by 0.2 percent for the whole sample. Also, there is a significant negative effect of education on BMI of women with a magnitude of 1 percent. However, one additional year of education increases men's BMI by 0.2 percent. Moreover, columns 4 and 7 indicate that one additional year of schooling is associated with 1 percent decrease in the women's probability of being obese or overweight. However, the more schooling increases the men's probability of being overweight by 1 percent. Furthermore, 2SLS results using either the treatment dummy or the intensity of the reforms as an instrument for the years of schooling shows similar patterns as those found in Table 3.6. The instruments are found to be weak except for the treatment dummy in the whole sample.

Overall, Table 3.6 and 3.7 show that there is a significantly negative relationship between education and the weight problems of individuals. However, addressing the endogeneity problem by instrumenting the schooling variable we found no significant causal effect exception of a weak positive effect on BMI for the whole sample. The instruments we used might have some problems, such that using a treatment dummy variable as an instrument might not distinguish the other policies that could have affected treatment and control groups differently in their outcome variable. On the other hand, using the variation in the implementation of the reform across regions and cohorts as an instrument can better detect the causal effect of education on weight problems. However, the limitations of the data forced us to use the region of residence as the region of education which mitigates the effect of intensity of the reform on education and might be responsible for the lack of the evidence for the causal effect of education on weight problems.

We take advantage of the TDHS data to address the concerns about the use of the intensity of the reform as an instrument, since it has information on women's childhood region which can be a better representative for the region of education. Moreover, the TDHS provides measured height and weight of women, unlike self-reported height and weight information in the THS. Table 3.8 shows the OLS and 2SLS results for only ever-married women using the dummy variable for middle school completion in Panel A and using years of education in Panel B. We find that the OLS results are still significant for BMI and obesity but insignificant for being overweight. The size of the effects are larger than those found in Table 3.6 and same as those found in Table 3.7. Using the childhood region as the region of education shows that the intensity of the 1997 reform has higher F-statistics for the excluded instruments in the first stage than the treatment dummy. Using treatment dummy as an instrument for the middle school completion, we find that there are small negative and insignificant effects. However, when we use intensity of the reform as an instrument, the effects are positive but insignificant.

Overall, we find no causal effect of education on BMI, obesity, and being overweight based on the estimated effects in Table 3.6, Table 3.7 and Table 3.8 except for one case where the treatment dummy is used as an instrument for the whole sample. These findings are in line with most of the literature (e.g. Kenkel et al., 2006; Arendt, 2005; Clark and Royer, 2013). Our 2SLS results for men somewhat contrast with Cesur et al. (2014). They find a large positive effect of education on the men's probability of being obese. This might be a result of the inclusion of the 1987 birth cohort in their study. Moreover, Cutler and

Lleras-Muney (2010) show that the effect of education on obesity is greater at the higher levels of education, particularly beyond 12 years of schooling. As has been shown in Figure 3.2, the reform and the intensity of the reform increased the average years of education in Turkey by mainly increasing completing middle school education (6th-8th grades). Also, our results show the local average treatment effects of the treated group rather than the average treatment effect. Thus, our findings indicate that there is no significant causal effect of education on weight problems on the middle school margin in Turkey. This does not rule out the possibility of an effect of high school or college education on BMI/obesity.

3.7.2 Smoking

As we mentioned earlier, the high smoking rate among men in Turkey is also another public health problem. The smoking rate differs for men and women in Turkey. Turkey is the first ranked among OECD countries in terms of men's smoking rate, while one of the lowest in terms of women's smoking rate. Figure 3.3 (b) shows the percentage of people who have been ever smoked regularly as well as the percentage of those who quitted smoking for males and females by birth cohorts using the THS data. Figure 3.3 (b) shows the prevalence of smoking is higher among males than females. The smoking rate is lower among younger cohorts of men and women. The probability of cessation of smoking is somewhat similar for men and women. If education has a causal effect on smoking, we expect to find a negative effect of education on starting smoking and the number of cigarettes smoked, and a positive effect on quitting.

Table 3.9 shows OLS and 2SLS results on the probability of having been a regular smoker, the probability of cessation of smoking conditional on ever smoked regularly and the number of cigarettes smoked daily. All standard errors are clustered by region and birth cohort. The THS does not ask questions related to smoking in the 2008 survey. Thus, the results in Table 3.9 are obtained by using the 2010, 2012, and 2014 THS surveys. Column (1) of Table 3.9 shows that at least middle school completion increases the probability of having been a regular smoker by 0.3 percent, but the coefficient is insignificant. Disaggregation by gender shows that women with at least 8 years of schooling are 3 percent more likely to smoke regularly relative to those women with less than 8 years of schooling.This is significant at the 5% level. However, there is no significant effect for men. The probability of quitting smoking conditional on having smoked regularly increases for those who attain at least 8 years of education relative to those who had less than 8 years of schooling for the whole sample as well as for men by 5 percent. There is no significant effect of education on quitting behavior of women. We also investigated the effect of middle school completion on the number of cigarettes smoked daily conditional on smoking every day. Column (7) shows that the completion of middle school or higher leads to a decrease in the number of cigarettes consumed per day by 1.26 for the whole sample and by 1.84 for males compared to those who completed 5 years of primary school or no schooling. There is an insignificant small negative effect for women.

The OLS estimation coefficients are in line with expectations except for the positive effect on the probability of having been a regular smoker for women. However, these estimates do not show the causal relationship between education and smoking behavior. Thus, we employed the instrumental variables approach using the treatment dummy in columns (2), (5), and (8) and the intensity of the 1997 reform, in columns (3), (6), and (9). In most cases, the first stage F-statistics show that the intensity of the reform is a weak instrument while the treatment dummy is strong. This might be due to the concerns about the region of residence that we used as a proxy for the region of education for the intensity of the reform and the small sample sizes. However, we find no significant causal effect of education on smoking behavior based on the 2SLS results in Table 3.9. These findings are in line with that found in Cesur et al. (2014).

Furthermore, we replicated Table 3.9 using the imputed years of education as schooling variable. The corresponding OLS and 2SLS results are presented in Table 3.10. Both instruments are found to be weak in all cases. The 2SLS results show that there is no causal effect of years of schooling on smoking behavior using either instruments.

While we find some relationship between smoking behavior and schooling, we could not find any causal effect of education on smoking behavior in Turkey. These results might be driven by the fact that the sample is quite young. However, Global Adult Tobacco Survey (2014) shows that around 80 percent of those who smoke regularly start smoking before the age of 20. Therefore, if there is any effect of education on the decision to smoke we could have been able to detect it at least in the first three columns of Table 3.9 and Table 3.10. Note that these findings are in line with those studies in which the instruments are based on the interventions in the lower levels of education (e.g. Clark and Royer, 2013; Park and Kang, 2008; Kemptner et al., 2011). On the other hand, de Walque (2007) finds a significant causal of university education on smoking behavior using the Vietnam War draft as an instrument. Cutler and Lleras-Muney (2010) show that the favorable effect of education on smoking behavior is more pronounced at the higher levels of education than the lower levels of education. The instruments we used are based on an intervention in middle school level and the compliers are mainly those who finish 8 years of schooling. This might be the driving factor behind the lack of evidence on the causal effect of education on smoking behavior.

3.7.3 Self-rated Health

The THS includes a self-rated health question in which individuals are asked to assess their general health status. The respondents grade their overall health as very poor, poor, fair, good, and excellent. Self-rated health is considered as a good indicator for mortality and morbidity (Idler and Benyamini, 1997) or a measure for general health status (Arendt, 2005). We define a dummy variable, "Good Health" for self-rated health which takes a value of 1 if an individual assesses his/her health as good or excellent, and 0 otherwise. The THS, also asks whether the individual has had any health problems in the last 6 months or not. We use these two variables to examine if there is any causal relationship between self-rated health and education.

Table 3.11 reports the OLS and 2SLS results of equation (3.5) with the dependent variables “good health” and “have any health problems in the last 6 months”. The OLS results show that there is a significant positive relationship between self-rated health and the completion of at least 8 years of schooling with an estimated coefficient of 10 percent. The effect is slightly higher for men than women. However, instrumenting middle school completion by the treatment dummy or intensity of the reform leads to no significant causal relationship between education and self-rated health. We also find that those with middle-school and higher education are 5 percent less likely to have any health problems in the last 6 months relative to those with less than middle-school education based on the OLS estimation results. It does not differ much among gender groups. Again, IV estimation results show that there is no causal effect of education on having any health problems in the last 6 months.

3.7.4 Well-being of Infants

It is also well documented that the maternal education and infants’ birth outcomes are closely related (e.g. Currie and Moretti, 2003; Chou et al., 2010). We take advantage of the detailed information on mothers’ characteristics and birth outcomes in the TDHS data for the years 2008 and 2013, and analyze the effect of maternal education on infant birth-weight. Table 3.12 shows the OLS and 2SLS results of the effect of mothers’ education, at least middle school completion in Panel A, and years of education in Panel B, on the log of infants’ weight at birth. We also test whether the effect differs for the birth-weight of the first child of a mother. Moreover, we define a dummy variable for low birthweight which takes a value of 1 if the birth-weight is lower than $2.5kg$ and 0 otherwise. The OLS estimation result in column (1) shows that mothers’ completion of middle school or higher schools increases the birth weight of infants by 2 percent. However, it is insignificant. Moreover, we find that one additional year of mothers’ schooling increases the birthweight by 0.4 percent in the first column of Panel B. We also checked if the effect is different for the mother’s first birth considering a sample in which only first births are considered. Column (4) shows that the estimated coefficient are almost same for all births and the first birth. Lastly, column (7) shows that the mother's completion of at least middle school lowers the probability of giving birth to a low-weight infant by 2 percent related to the mothers with less than middle school education, but it is insignificant. On the other hand, using years of education as schooling variable, we find that one additional year of mothers' schooling is associated with a 1-percent reduction in the probability of giving birth to a low-birthweight infant.

We examined if these infant birth outcomes are causal from education by employing instrumental variables. In Panel A, the first-stage F-statistics for the treatment dummy is over 10 which shows it is not a weak instrument, while the intensity of the reform is a weak instrument. This might be driven the fact that the relatively smaller sample size. However, none of the estimated coefficients are found to be significant regardless of the instrument or the sample used. Also, using years of education as schooling variable in Panel B, we find that both instruments are weak and none of the 2SLS estimates are significant.

Overall, even though the OLS estimation results suggest that infants' birth weight is improved by the increase in the years of maternal education, the instrumental variable results show that there is no significant evidence on the causality between maternal education and infant birth-weight outcome.

3.8 Conclusion

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

We first examined the effect of the reform on educational attainment in Turkey by defining a treatment dummy for those who are affected by the reform. Our findings corroborate previous findings in the literature that find that the reform increased the educational attainment significantly. We further define another instrument, intensity of the 1997 reform, which takes account for the regional and cohort variations in the new middle-school class openings per 1000 middle-school-aged children. We found that one additional middle-school class opening associated with the 1997 reform increases the years of education by 0.18 years and the rate of completion at least middle school by around 5 percent, on average. The effects are more pronounced for Turkish women than for Turkish men. Taking advantage of the detailed information on the region of childhood, birth, and residence in the TDHS data, we showed that use of the region of residence as a proxy for region in which an individual attained his/her middle-school education mitigates the effect of middle-school class openings related to the 1997 reform.

Further, we analyzed the effect of education on various health measures, including BMI, obesity, smoking behavior, self-rated health and the impact of maternal education on the birth weight outcome of infants. The OLS estimations for weight problems corroborate the findings in the literature for the whole sample as well as women showing there is a negative relationship between education and BMI, obesity, or being overweight. There is no statistically significant effect of education on weight problems of Turkish men. Using an instrumental variables approach, we find some weak evidence that middle school completion increases the BMI, calculated from self-reported height and weight, for the whole sample instrumenting education by the treatment dummy. However, we find no evidence using the intensity of the reform as an instrument. We checked this findings with measured height and weight for women in the TDHS data and found no evidence for casual relationship between education and weight problems.

The causal relationship between education and smoking behavior is also examined. The OLS results show that the middle school completion increases the probability of having been a regular smoker for females. This is contrary to expectations. On the other hand, we find that at least middle school completion is associated with a higher probability of quitting smoking and lowers the number of cigarettes smoked per day conditional on being a current smoker. Again, addressing the endogeneity of education by instrumenting at least middle school completion, we find no evidence on the causal effect of education on smoking behavior. Furthermore, considering self-rated health and having any health problem in the last 6 months, we found that at least 8 years of schooling is associated with a higher probability of reporting good health and lower probability of having any health problem in last 6 months compared to those who have less than 8 years of schooling. However, there is no causal effect detected using the instrumental variables method.

Lastly, we considered the infant birth weight and search for its possible causal relationship with maternal education. We showed that at least middle school completion is associated with higher birth weight and lower probability of giving birth to a low-birth-weight baby compared to those women who had less than middle-school education. However, we found no evidence on the causal effect of maternal education on birth weight outcomes of infants once we instrument middle school completion by either the treatment dummy or the intensity measure of the 1997 reform.

Overall, we found no evidence on the causal effect of education on health outcomes. It should be noted that our estimates represent the local average treatment effect on the treated birth cohorts. Our identification strategy is based on an intervention to the schooling in middle school level. We showed that the reform and the intensity of the reform increased the average education in Turkey by increasing years of education in 6th-8th grades. Therefore, our estimates indicate that there is no causal effect of education in middle school level. However, there might be effect of schooling in high school and college levels. Future research can shed light on these conjectures using the 2012 reform which increased the compulsory schooling from 8 to 12 years.

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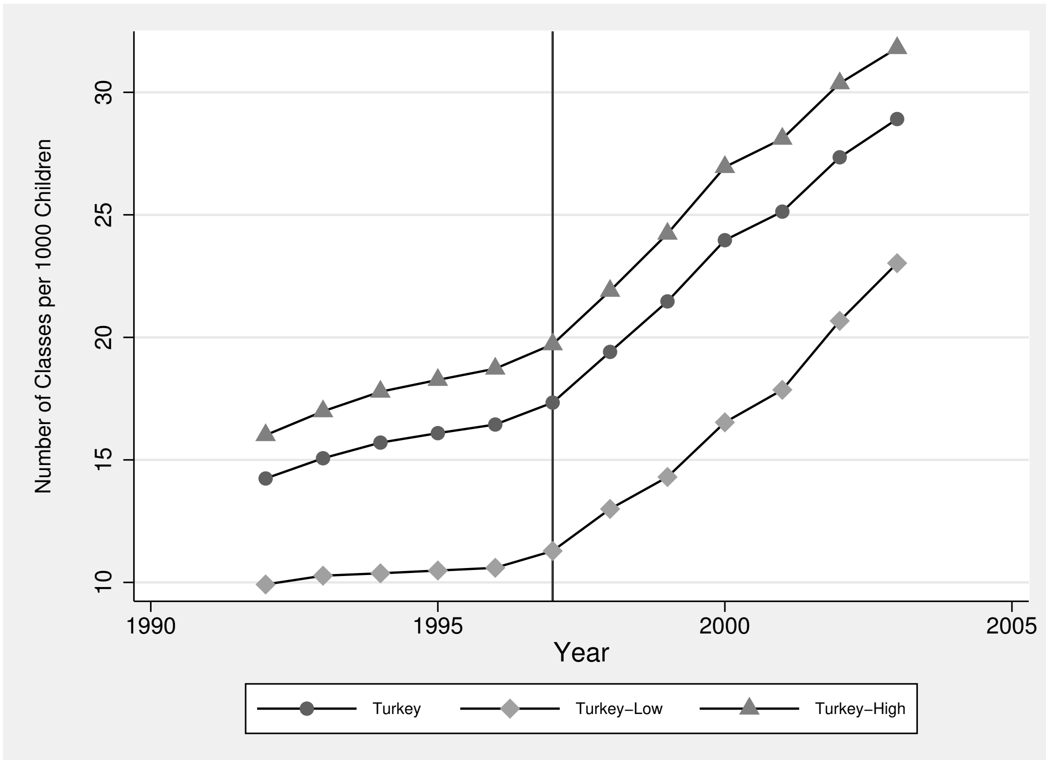
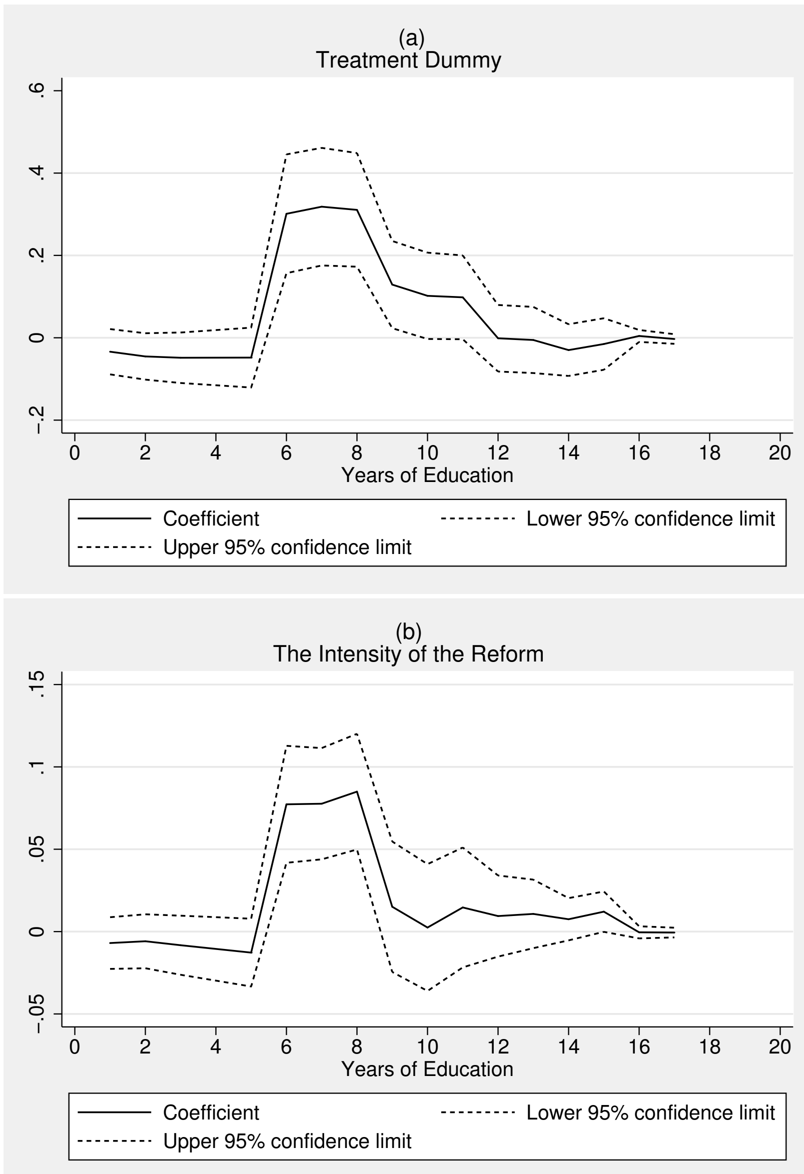
Figures and Tables

Figure 3.1: Number of Middle School Classes per 1000 Children by School Year



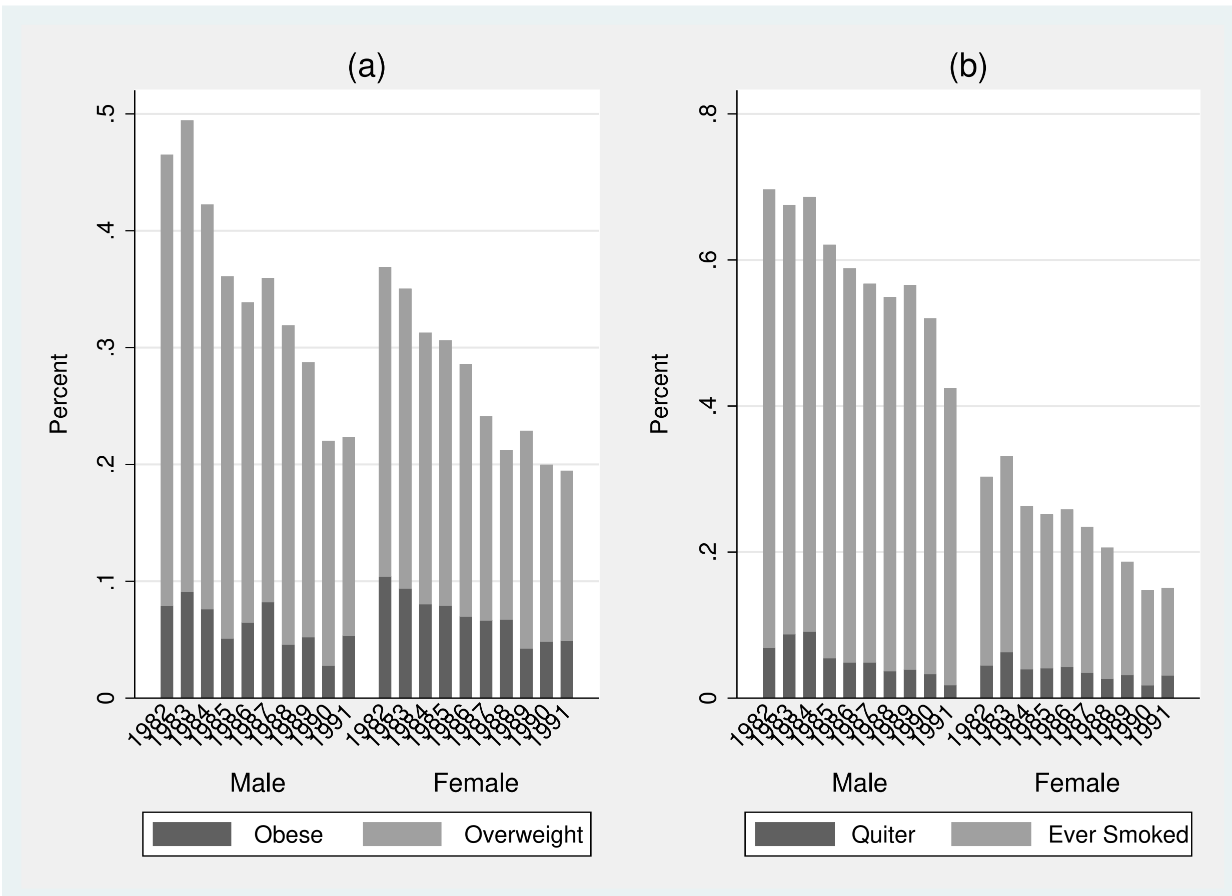
Source: Authors' Calculation based on 1990, 2000, 2007 Population Censuses of Turkey and MONE's National Education Statistics 1992-2003.

Figure 3.2: The Effect of the Instruments on the Probability of Completing at Least ‘m’ Years of Schooling-(The TDHS Data)



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

Figure 3.3: Obesity/Overweight and Smoking Behavior by Gender and Cohort



Notes: (a) Authors' calculation based on the THS for the years 2008-2014. (b) Authors' calculation based on the THS for the years 2010-2014. "Ever Smoked" includes those who had been smoked regularly. "Quitter" includes those who had smoked regularly and quitted smoking.

Table 3.1: Descriptive Statistics

	Control			Treated		
VARIABLES	# of Obs.	Mean	Std. Dev.	# of Obs.	Mean	Std. Dev.
Panel A: The Turkish Health Survey (THS)						
Age	5,814	27.90	2.404	5,100	21.73	2.521
Female*	5,814	0.561	0.496	5,100	0.552	0.497
Years of Education	5,814	8.941	4.222	5,100	9.602	3.270
At Least Middle School*	5,814	0.605	0.489	5,100	0.877	0.328
BMI	5,509	24.30	4.052	4,760	22.84	3.803
Obese*	5,509	0.083	0.276	4,760	0.049	0.215
Overweight*	5,509	0.299	0.458	4,760	0.185	0.388
Ever Smoked Regularly*	4,634	0.395	0.489	3,897	0.298	0.457
Quit Smoking*	1,832	0.151	0.358	1,160	0.098	0.298
Number of Daily Cigarettes	1,434	13.08	9.629	973	12.93	9.576
Good Health*	5,812	0.815	0.389	5,100	0.859	0.348
Health Problem in the last 6 Months*	5,810	0.226	0.418	5,093	0.182	0.386
Panel B: The Turkey Demographic and Health Survey (TDHS)-Female						
Age	2,151	27.38	2.711	1,454	22.55	2.177
Years of Education	2,151	6.784	4.309	1,454	8.402	4.452
At Least Middle School*	2,151	0.375	0.484	1,454	0.697	0.460
BMI	1,916	26.04	4.909	1,299	24.41	4.684
Obese*	1,916	0.195	0.396	1,299	0.120	0.325
Overweight*	1,916	0.336	0.473	1,299	0.256	0.436

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

Table 3.2: Number of Middle School Classes (6th-8th) per 1000 Children Aged 11-13 by NUTS-1 Region and School Years

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

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

Table 3.3: The Effect of the 1997 Reform on Education-The THS Data-OLS

	Years of Education			Middle School Completion		
VARIABLES	All	Female	Male	All	Female	Male
Treatment Dummy	0.799***	0.902***	0.747**	0.166***	0.230***	0.106***
	(0.210)	(0.313)	(0.312)	(0.0237)	(0.0362)	(0.0328)
Observations	10,914	6,076	4,838	10,914	6,076	4,838
R-squared	0.101	0.114	0.054	0.170	0.184	0.112

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1

2- Treatment group 1988-1991 birth cohorts vs Control group 1982-1985.

3- All regressions control for age, age square, 12 NUTS-1 region, year fixed effects as well as an interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 3.4: The Effect of the Intensity of the 1997 Reform on Education-The THS Data-OLS

	Years of Education			Middle School Completion		
VARIABLES	All	Female	Male	All	Female	Male
Intensity	0.108**	0.157**	0.0383	0.0251***	0.0335***	0.0134
	(0.0497)	(0.0713)	(0.0782)	(0.00579)	(0.00916)	(0.00891)
# of Obs.	10,914	6,076	4,838	10,914	6,076	4,838
R-squared	0.101	0.115	0.056	0.168	0.182	0.114

Notes: 1- Standard errors clustered by region and birth cohort are in parentheses *** p<0.01, ** p<0.05, * p<0.1

2- Treatment group 1988-1991 birth cohorts vs Control group 1982-1985.

3- All regressions control for birth year, 12 NUTS-1 region, year fixed effects as well as an interaction of treatment dummy and middle school enrollment rate in 1996. All regressions are weighted by the sample weights.

Table 3.5: The Effect of the Intensity of the 1997 Reform on Education-The TDHS data-OLS

	Years of Education				Middle School Completion			
		All	Ever-married			All	Ever-married	
VARIABLES	All	Female	Female	Male	All	Female	Female	Male
Intensity	0.281**	0.241**	0.265**	0.343*	0.0709***	0.0711***	0.0837***	0.0721***
	(0.112)	(0.120)	(0.130)	(0.174)	(0.0113)	(0.0131)	(0.0182)	(0.0210)
# of Obs.	5,309	3,556	2,938	1,753	5,309	3,556	2,938	1,753
R-squared	0.177	0.234	0.191	0.057	0.194	0.252	0.207	0.072

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

Table 3.6: The Effect of at Least Middle School Completion on BMI/Obesity (OLS and 2SLS)

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

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

Table 3.7: The Effect of Years of Education on BMI/Obesity (OLS and 2SLS)

	Dep.var: log(BMI)			Dep.Var: Obese			Dep.Var: Overweight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
					Panel A: Whole Sample				
Years of Education	-0.002***	0.02*	-0.02	-0.003***	0.01	0.04	-0.00	0.01	-0.04
	(0.00)	(0.01)	(0.04)	(0.00)	(0.01)	(0.07)	(0.00)	(0.03)	(0.12)
1st Stage F-stat		[12.97]	[1.167]		[12.97]	[1.167]		[12.97]	[1.167]
# of Obs.	10,269	10,269	10,269	10,269	10,269	10,269	10,269	10,269	10,269
					Panel B: Female				
Years of Education	-0.01***	0.01	0.02	-0.01***	0.02	0.13	-0.01***	-0.07	-0.06
	(0.00)	(0.02)	(0.04)	(0.00)	(0.03)	(0.13)	(0.00)	(0.05)	(0.10)
1st Stage F-stat		[5.992]	[1.470]		[5.992]	[1.470]		[5.992]	[1.470]
# of Obs.	5,590	5,590	5,590	5,590	5,590	5,590	5,590	5,590	5,590
					Panel C: Male				
Years of Education	0.002***	0.02	0.31	-0.00	-0.003	0.65	0.01***	0.10	0.07
	(0.00)	(0.02)	(2.24)	(0.00)	(0.03)	(4.65)	(0.00)	(0.06)	(1.15)
1st Stage F-stat		[5.357]	[0.019]		[5.357]	[0.019]		[5.357]	[0.019]
# of Obs.	4,679	4,679	4,679	4,679	4,679	4,679	4,679	4,679	4,679

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

Table 3.8: The Effect of Schooling on Measured BMI/Obesity (OLS and 2SLS) - The Ever-Married Female TDHS data

	Dep.var: log(BMI)			Dep.Var: Obese			Dep.Var: Overweight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Middle School Completion					
Middle School Completion	-0.06***	-0.00	0.09	-0.09***	0.00	0.10	-0.00	-0.09	0.26
	(0.01)	(0.08)	(0.09)	(0.02)	(0.16)	(0.18)	(0.02)	(0.17)	(0.18)
1st Stage F-stat		[17.88]	[19.76]		[17.88]	[19.76]		[17.88]	[19.76]
# of Obs.	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695
				Panel B: Years of Education					
Years of Education	-0.01***	-0.00	0.02	-0.01***	0.00	0.03	0.00	-0.03	0.07
	(0.00)	(0.03)	(0.03)	(0.00)	(0.05)	(0.05)	(0.00)	(0.06)	(0.06)
1st Stage F-stat		[3.833]	[5.069]		[3.833]	[5.069]		[3.833]	[5.069]
# of Obs.	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695	2,695

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

Table 3.9: The Effect of at Least Middle School Completion on Smoking Behavior (OLS and 2SLS)

	Dep.var: Ever Smoke Regular			Dep.Var: Quit Smoking			Dep.Var:Num.of Cigarette		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Whole Sample					
Middle School	0.003	0.19	0.59	0.05***	-0.05	0.70	-1.26**	-15.99	20.91
Completion	(0.01)	(0.18)	(0.57)	(0.01)	(0.35)	(0.71)	(0.55)	(10.60)	(29.73)
1st Stage F-stat		[39.95]	[5.904]		[9.816]	[2.046]		[13.70]	[1.457]
# of Obs.	8,531	8,531	8,531	2,992	2,992	2,992	2,407	2,407	2,407
				Panel B: Female					
Middle School	0.03**	0.14	0.52	0.04	-0.29	0.27	-0.08	1.41	-2.95
Completion	(0.01)	(0.14)	(0.38)	(0.03)	(0.85)	(0.47)	(0.66)	(14.82)	(11.99)
1st Stage F-stat		[33.51]	[5.209]		[1.680]	[4.205]		[1.297]	[2.273]
# of Obs.	4,769	4,769	4,769	944	944	944	675	675	675
				Panel C: Male					
Middle School	-0.04	0.27	0.87	0.05***	0.13	1.79	-1.84***	-21.17	56.32
Completion	(0.02)	(0.54)	(2.31)	(0.01)	(0.35)	(4.56)	(0.70)	(14.86)	(137.09)
1st Stage F-stat		[7.089]	[0.538]		[4.631]	[0.146]		[6.653]	[0.200]
# of Obs.	3,762	3,762	3,762	2,048	2,048	2,048	1,732	1,732	1,732

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

Table 3.10: The Effect of Years of Education on Smoking Behavior (OLS and 2SLS)

	Dep.var: Ever Smoke Regular			Dep.Var: Quit Smoking			Dep.Var:Num.of Cigarette		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Whole Sample					
Years of Education	-0.01***	0.05	0.27	0.01***	-0.03	-19.56	-0.19***	-5.39	-28.68
	(0.00)	(0.05)	(0.49)	(0.00)	(0.23)	(3,495.05)	(0.06)	(6.04)	(291.40)
1st Stage F-stat		[5.732]	[0.401]		[0.218]	[0.001]		[1.047]	[0.01]
# of Obs.	8,531	8,531	8,531	2,992	2,992	2,992	2,407	2,407	2,407
				Panel B: Female					
Years of Education	0.00	0.04	0.15	0.01*	-0.86	0.11	-0.07	2.11	-3.18
	(0.00)	(0.04)	(0.20)	(0.00)	(21.66)	(0.29)	(0.08)	(33.54)	(26.71)
1st Stage F-stat		[4.372]	[0.77]		[0.002]	[0.24]		[0.06]	[0.02]
# of Obs.	4,769	4,769	4,769	944	944	944	675	675	675
				Panel C: Male					
Years of Education	-0.02***	0.07	-0.21	0.01***	0.07	-0.32	-0.25***	-5.82	-10.25
	(0.00)	(0.16)	(0.67)	(0.00)	(0.21)	(1.29)	(0.08)	(7.17)	(37.72)
1st Stage F-stat		[1.040]	[0.162]		[0.226]	[0.07]		[1.013]	[0.09]
# of Obs.	3,762	3,762	3,762	2,048	2,048	2,048	1,732	1,732	1,732

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

Table 3.11: The Effect of at least Middle School Completion on Self-Reported Health (OLS and 2SLS)

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

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

Table 3.12: The Effect of Maternal Education on Infant Birth Weight (OLS and 2SLS)

	BirthWeight(Log)			1st Child BirthWeight(Log)			Low BirthWeight		
VARIABLES	OLS	IV-1	IV-2	OLS	IV-1	IV-2	OLS	IV-1	IV-2
				Panel A: Middle School Completion					
Middle School Completion	0.02	0.04	-0.06	0.02	0.10	-0.19	-0.02	0.02	-0.10
	(0.01)	(0.12)	(0.16)	(0.01)	(0.14)	(0.20)	(0.02)	(0.16)	(0.22)
1st Stage F-stat		[13.57]	[8.570]		[5.751]	[5.273]		[13.57]	[8.570]
# of Obs.	2,360	2,360	2,360	1,115	1,115	1,115	2,360	2,360	2,360
				Panel B: Years of Education					
Years of Education	0.004***	0.01	-0.02	0.003*	0.05	-0.04	-0.01***	0.01	-0.03
	(0.00)	(0.04)	(0.06)	(0.00)	(0.08)	(0.06)	(0.00)	(0.05)	(0.07)
1st Stage F-stat		[3.491]	[1.625]		[0.594]	[1.411]		[3.491]	[1.625]
# of Obs.	2,360	2,360	2,360	1,115	1,115	1,115	2,360	2,360	2,360

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

Chapter 4

The Turkish Spatial Wage Curve

4.1 Introduction

The wage curve is an empirical phenomenon featuring the negative relationship between regional unemployment rate and real wage level. This relationship has been first discovered by Blanchflower and Oswald (1990; 1994) using microeconomic data for over a dozen countries including the UK and the US. The strength of this relationship was found to be interestingly similar around -0.10 across countries and time periods, therefore, it is considered as an "empirical law of economics" (Card, 1995). This uniformity of the wage elasticity with respect to regional unemployment attracted many other researchers in search of the wage curve for other countries. Nijkamp and Poot (2005) confirm the existence of wage curve by using meta-analysis on 208 wage curve estimates, but with a lower elasticity after bias corrected around -0.07. Moreover, Blanchflower and Oswald (2005) summarize the wage curve estimates from over 40 countries and re-estimate the wage curve of the US with the contemporary data and conclude that long-term elasticity of wage with respect to regional unemployment is -0.10.

In the wage curve estimates, regional permanent effects are controlled for including region fixed effects. However, ignoring interdependence of regions, may result in bias in the wage curve estimates. Studies addressing spatial effects and spillover effects of dependent and/or independent variables have emerged in the wage curve literature in the last decade (e.g. Longhi, Nijkamp, and Poot, 2006; Baltagi and Rokicki, 2014; Baltagi, Baskaya, and Gul, 2015). Moreover, Card (1995) points out the possible bias arisen from the use of overall unemployment rate instead of group-specific unemployment rates in the wage curve estimations of various worker groups. However, a few studies provide the wage curve estimates using group-specific unemployment rates for gender, age, or race groups (e.g. Boushey, 2002; Konyali, 2012; Baltagi and Rokicki, 2014), a more relevant disaggregation in the labor force, skill groups, has not been used.

The existing wage curve literature on Turkey find evidence on the existence of the Turkish wage curve for various groups of workers. The overall unemployment elasticity of real wages is found to be in line with international findings, around -0.10 (e.g. Ilkkaracan and Selim, 2003; Baltagi, Baskaya, and Hulagu 2012, 2013). Ilkkarcan and Selim (2003) provide the first evidence for the Turkish wage curve, but fail to control for regional effects because of the data limitation. Baltagi et al. (2012) is the first economy-wide study, on the Turkish wage curve which controls for regional differences and endogeneity of unemployment rate, using data for 26 NUTS-2 regions over the period 2005-2008. The wage curve for males shows similarity across studies in Turkey. However, a significant wage curve for the female workers have been found only in those studies that exclude agriculture sector (e.g. Baltagi et al., 2012, 2013; and Baltagi et al., 2015). Interestingly, whenever female wage curve is significant, it is more elastic than the male wage curve, except Baltagi et al. (2015). Moreover, Konyali (2012) considers group-specific unemployment rates in her study and finds a weak evidence on a wage curve for males with the male-unemployment rate. The above studies use data on a boom period of the Turkish economy, such that only one part of the business cycle has been considered in these analyses, except for Ilkkaracan, Levent, and Polat (2013) which use only one year of the boom period. Furthermore, Baltagi et al. (2015) use a longer period of data that can capture the whole business cycle, investigating the differences in spatial wage curves of the formal and informal workers. They use regional non-agricultural unemployment rates instead of the overall unemployment rate. Therefore, these estimates may not show the whole picture of the Turkish wage curve.

This study explores the spatial effects in the wage curve estimations in Turkey providing a broader picture than the existing studies. Here, the Turkish wage curve is estimated for a longer period, 2004-2013. The spatial spillover effect is investigated following Baltagi and Rokicki (2014) for 26 NUTS-2 regions with the overall regional unemployment rate and group-specific unemployment rates. Baltagi et al. (2012, 2013) and Baltagi et al. (2015) exclude the agriculture sector and use non-agricultural unemployment rate, while Ilkkaracan et al. (2013) exclude the public workers from their analysis. Unlike previous studies on the Turkish wage curve, we have not excluded those groups. Therefore we examine the economy-wide existence and strength of the Turkish wage curve. We also estimated disaggregated wage curves for several sub-samples.

This study corroborates the Turkish wage curve evidence with unemployment elasticity of real wages of -0.071 when endogeneity problem is addressed. We find that unemployment elasticity of real wages is slightly higher for Turkish men than women. This contrast with the previous studies on Turkey, except for Baltagi et al. (2015), but in line with most of the studies on the other countries. Moreover, the workers aged 15-24, least- skilled workers, less-experienced workers, and informal workers have higher elasticities than the workers aged 25 years and above, skilled workers, more experienced workers, and formal workers, respectively. There is a strong evidence on spatial effects of unemployment rate of contiguous regions on wage level, which is larger, in absolute value, than the effect of own-regional unemployment rate. The FE-2SLS results show that own region elasticity is -0.056 while spatial spillover unemployment elasticity is -0.087. We show that these results hold for various groups of workers defined in terms of gender, age, skill, experience, and formality status. Robustness check for the spatial weight matrix shows that the use of full Euclidean distance matrix averages out the effect of positive correlation between neighboring regions.

Furthermore, using group-specific unemployment rates in the estimation of wage curves for various groups, we find that unemployment elasticity of pay for the female workers become smaller and lose its significance. However, the unemployment elasticity for the male workers changes slightly. Moreover, the estimations of the wage curve for workers aged 15-24, and workers aged 25 years and above show that the use of the group-specific regional unemployment rates, rather than overall regional unemployment rate, increases the magnitude of unemployment elasticity for workers aged 15-24, while decreases the magnitude of unemployment elasticity for workers aged 25 years and above, in absolute value. More interestingly, disaggregating based on skill level of workers, we show that the most responsive group to the overall unemployment is the low-skilled workers with an elasticity of -0.099, whereas the most responsive group to the group-specific unemployment rates is medium-skilled workers, with elasticity of -0.094. In addition, we estimate a spatial model with group-specific unemployment rates. We find that the spatial spillovers unemployment elasticities of pay are higher, in absolute value, than unemployment elasticities for all groups except for the female workers and high-skilled workers. Wages of medium-skilled workers are more responsive to the group-specific unemployment rate of both own and contiguous regions, than that of the low-skilled workers. Similarly, both the unemployment elasticity of pay and spatial spillovers are higher for the workers aged 15-24 than workers aged 25 years and above.

The paper is organized as follows. Section 2 summarizes the literature on the international wage curve and Turkish wage curve. The model and data is presented in section 3. The empirical results for Turkish wage curve are presented in section 4. Finally, section 5 concludes the paper.

4.2 Literature on The Wage Curve

4.2.1 International Wage Curve

Blanchflower and Oswald (1990) has established the so-called wage curve featuring the negative relationship between wage level and local unemployment rate based on micro-level data for the US and the UK controlling for the permanent features of the local markets by including regional fixed effects. Their use of cross section data and focus on the relationship between wage level and regional or industry unemployment challenged the Phillips curve in which a relationship between wage inflation and the unemployment rate has been established using macroeconomic data. Furthermore, Blanchflower and Oswald (1990) extended the estimations of wage curve for 16 countries. Interestingly, the estimation results came out around -0.10, which is considered an "empirical law of economics" (Card, 1995). In other words, real wages would be depressed by 10 percent if the unemployment rate is doubled in a region. Nijkamp and Poot (2005) corroborate the existence of a wage curve as an international phenomenon by conducting a meta-analysis with a sample of 208 wage curve estimations available in the literature by then. However, they discuss that the bias-corrected unemployment elasticity of real wage is around -0.07. Moreover, Blanchflower and Oswald (2005) summarize the wage curve studies across the world and re-evaluate the US wage curve with modern data. They conclude that the long-run wage elasticity with respect to regional unemployment rate is -0.10.

Blanchflower and Oswald (1990) tried to find some theoretical background for their empirical findings and came up with three alternative explanations: contract model, union bargaining model, and efficiency wage model. Among these, contract model has been shown irrelevant by Card (1995). The union bargaining model, featuring the loss in the bargaining power of unions in a slack labor market causes a decrease in individual wages, would not be valid for those countries with low rate of unionization. The last and widely accepted explanation is efficiency wage model, in which regional unemployment rate acts as a threat to the workers not to shirk. In other words, in a tight labor market, employers pay more than the equilibrium wages to avoid the monitoring cost. On the other hand, when the unemployment rate is high in a region, workers find it risky to shirk and being dismissed since it is harder to find a job in a slack market. Therefore, employers have an incentive and opportunity to lower the wage at the times of high regional unemployment (Longhi et al.,2006). In addition, Campbell and Orszag (1998) discuss the labor turnover cost model in which firms pay a premium to avoid expenses associated with the recruitment and training of new workers in a tight labor market. This may not be needed in slack labor market, as high regional unemployment rate makes it costly to quit the job for workers. Morrison, Papps, and Poot (2006) further, extend this model incorporating by the possibility of migration between regions. Moreover, Sato (2000) provides a search model as an explanation for the wage curve in which labor force is mobile between regions, cities are assumed to be in a mono-centered structure, and regions differ in their productivity levels, such that high productive regions have higher wages and lower unemployment.

A number of issues related to the estimation and explanations of the wage curve have been pointed out by Card (1995) in his review paper. Most of the wage curve estimations use individual wages regressed on the regional unemployment rate and individual characteristics. Therefore, the relevant degrees of freedom for the estimation of the unemployment elasticity of real wage is the number of regions times the number of years used in the sample, not the number of individuals in the sample. Card (1995) warns that it might not be a serious problem for those countries that have 200-500 region-year observation, but it may lead serious downward bias in standard errors and misleading inference for those countries with a low number of regions and time periods. Moreover, the error term in the model specification will be correlated for people from the same local labor market. It was attempted to be taken care of by averaging dependent variable as well as individual characteristics over individuals in each region-year cell in Blanchflower and Oswald (1990). Furthermore, the regression- adjusted averages that control for individual observable characteristics can be used as an alternative way as suggested by Card (1995). Nijkamp and Poot (2005) report that 27.4% of the wage curve estimations, until then, uses averages of individual wages and other characteristics.

The responsiveness of the wages to the regional unemployment rate shows differences across groups defined according to the worker characteristics or sector of the work. As Longhi et al. (2006) point out; if the wage curve results from efficiency wage or labor turnover models, then it is expected that those workers who have less bargaining power such as young, less experienced, less educated, informal, temporary workers would have a higher unemployment elasticity of real wages. These expected results are found in most of the wage curve studies (e.g. Blanchflower and Oswald, 1994; Card 1995; Baltagi and Blien, 1998; Baltagi, Baskaya and Hulagu, 2012). However, male wage curve has been found to be more elastic for the US, the UK, and Australia than female wage curve (Blanchflower and Oswald, 1994; Card, 1995), the opposite of this relationship has been shown for Germany (Baltagi and Blien,1998; Baltagi, Blien, and Wolf, 2000) for Turkey (Baltagi, Baskaya, and Hulagu, 2012,2013). Although the above-mentioned studies disaggregate the samples, they fail to disaggregate the regional unemployment rate accordingly. It should be noted that workers are more likely compete each other within the groups they are in. As mentioned before, the regional unemployment rate may be considered as a threat to the workers not to shirk or quit. Therefore, it may be the case that various groups of workers respond differently to the overall regional unemployment variations than their group-specific regional unemployment variations (Card, 1995).

These problems have been addressed in the literature using disaggregated unemployment rates for various groups defined by gender, age, experience. Boushey (2002) estimated wage curves for women versus men and black versus white using the group-specific unemployment rates. She further investigates if the differences in elasticities for different groups are due to the different relationship between group-specific unemployment rate and overall regional unemployment rate. She finds that group-specific wage curves do not mask the relationship between group-specific unemployment rate and overall regional unemployment rate. She shows that wage curve differs across groups such that wage curve is more elastic for black workers than for white workers. Moreover, she shows that wage curve differs across business cycle and space. Konyali (2012) uses group-specific unemployment rates for male versus female and young versus old workers in Turkey. She finds a weak evidence only for the male wage curve when male-regional unemployment is used. Furthermore, Baltagi and Rokicki

(2014) explore the variation in the slope of wage curve based on gender groups for Poland. They show that Polish wage curve is sensitive to the group-specific unemployment rate and men have a statistically significant wage curve, unlike women. A more relevant disaggregation in the unemployment rate would be according to the skill level of workers, as it is more likely that workers compete for jobs among the workers with similar skills, but it has not been as yet explored.

Another problem about the wage curve estimates is endogeneity problem due to the simultaneity of the regional unemployment rate. Although Blanchflower and Oswald (1990) were aware of the endogeneity bias as they pointed out that failing to address this problem may lead upward bias in unemployment elasticity of wage and make it hard to get a negative elasticity, they failed to find a good instrument for the local unemployment rate. Baltagi and Blien (1998) use lagged values of explanatory variables as additional instruments and estimate the model with fixed effects 2SLS (FE-2SLS). Using German data for 1981-1990 they show that unemployment elasticity is sensitive to the endogeneity. They were not able to find a negative relationship with conventional fixed effects estimates, but once they use lagged values of explanatory variables as additional instruments, their findings are in line with the wage curve estimates from other countries. Baltagi, Blien and Wolf (2000) find evidence for wage curve for East Germany for the period 1993-1998 when unemployment is treated as endogenous and using first difference 2SLS. Furthermore, Baltagi, Baskaya and Hulagu (2012, 2013) and Baltagi et al. (2015) treated unemployment as endogenous and use FE-2SLS in their wage curve studies for Turkey and they find an evidence for Turkish wage curve that is around -0.10.

4.2.2 Spatial Wage Curve

The above literature assumes independence of regions in the wage curve estimations, such that wage in one region is only determined by the employment conditions within that region. However, the regions may be interrelated due to the labor mobility or contiguous effects of employment conditions between regions. Moreover, regions used in wage curve studies are generally defined as administrative regions which do not necessarily represent the true functional labor markets. Therefore, a spatial correlation among the primary units of the wage curve is expected. Hence, neglecting the spatial correlation might be resulted in misspecification of wage equation and introduce a bias in estimation. There is a small strand of literature considers these issues by testing the spatial dependence and/or accounting for spatial effects (e.g. Buettner, 1999; Longhi et al. 2006; Baltagi and Rokicki, 2014) .

The efficiency wage and labor turnover costs explanations of the wage curve assume that if unemployment is high in a local labor market, employers have an incentive and opportunity to lower the wages (Longhi et al.,2006). However, if the unemployment rate is low in the adjacent labor markets, and cost of mobility between regions is low, the employers may not be able to lower the wages (Fingleton and Palombi, 2013). On the other hand, Longhi et al. (2006) and Morrison et al. (2006) provide another explanation, monopsonistic competition, for wage curve due to Bhaskar and To (1999). In this model, there is a free entry into the market but firms are constrained by a lump-sum start-up cost and heterogeneous workers face commuting and migration costs which vary depending upon the location of the job, such that individual firm has a monopsony power over wage setting. Longhi et al. (2006) point out that if the wage curve results from worker heterogeneity and monopsony power of individual employer, the elasticity of labor supply, and in turn, wage curve will depend on the employment conditions of adjacent labor markets and the cost of mobility between labor markets. Therefore, ignoring the interaction between adjacent economies and spatial spillover effects may lead misspecification of the wage equation and overestimation of the own-region unemployment elasticity of real wages.

There are mainly three different ways of controlling for the spatial effects depending on the source of correlation. The spatial correlation can be due to the common permanent characteristics of regions which are usually taken into account including region fixed effects in conventional wage curve studies. On the other hand, if some regions exposed to some common shocks, the spatial correlation occurs in the error terms which is known as spatial autocorrelation residuals (SAR). Buettner (1999) [20] tests for the spatial autocorrelation in German data but could not find evidence in favor of this hypothesis. There might be a spatial correlation between wages, such that the wage level in proximate regions may have a direct effect on the level of wage in one region. This can be controlled for by including spatially lagged dependent variable in explanatory variables. Buettner (1999) finds a significant effect of spatially lagged wages in the German context. Fingleton and Palombi (2013) point out a commonly recognized phenomenon in labor economics such that a region with high unemployment rate is usually found to be surrounded by regions with a high unemployment rate or vice versa. This may imply the spatial spillovers of the regional unemployment rate. Therefore, the unemployment rate of adjacent regions may have a direct effect on the wage level, so the omission of this may bias the estimates of unemployment elasticity of pay. The inclusion of a spatially weighted unemployment rate is the easiest and most common way to take account of the spatial effects in the wage curve estimations (e.g. Buettner, 1999; Longhi et al. 2006; Baltagi and Rokicki, 2014).

Most of the empirical spatial wage curve studies focus on the German data. However, there are some evidence in favor of the spatial spillovers in a couple of other countries. Buettner (1999) tests for spatial correlation in error terms, wages and unemployment rate using German data for 327 regions over the period 1987-1994. He could not find evidence for SAR but showed that the contiguity in wages and unemployment rate matters in the wage curve estimation such that, failing to take into account the spatial correlation leads a bias in the estimation of unemployment elasticity of pay. Furthermore, Longhi et al. (2006) confirm the wage curve and spatial effects in using data for 327 regions of West Germany for the period 1990-1997. They assume that the wage curve resulted from the worker heterogeneity and monopsonistic competition in local labor markets. They employ region-year averages of data and include spatially lagged log unemployment rate, unlike the use of inverse of the unemployment rate of adjacent regions as in Buettner (1999), as well as controlling for accessibility of the region and agglomeration effect. The wage curve for Western Germany has been confirmed along with the evidence on the spatial spillovers of regional unemployment rates. Furthermore, they conclude that wages in highly agglomerated regions are higher and the unemployment elasticity of pay is higher in more isolated regions. Moreover, these findings have been echoed by Morrison et al. (2006) using New Zealand data and measuring the regional accessibility by travel time between regions. They conclude that the interaction between regions has a significant effect on the strength of wage curve and various groups have different unemployment elasticity.

Elhorst, Blien, and Wolf (2007) investigate the potential endogeneity of regional unemployment rates in combination with time-specific effects. They compare different panel date methods for the 114 regions of East Germany over the period 1993-1999. They found that the most appropriate method is spatial first difference 2SLS and corroborates the wage curve for East Germany is -0.039 and conclude that the regional unemployment rate is not strictly exogenous in space. Baltagi, Blien, and Wolf (2012) explore the spatial aspects of the wage curve in a dynamic model, using 326 West Germany districts over the period 1980-2004. The paper follows two-step estimation technique proposed by Bell, Nickell, and Quintini (2002) in which composition effects controlled for in the first step. Subsequently, the compositionally corrected regional wages is used as dependent variable and its one-year lagged values used as independent variable along with regional unemployment rate, spatially lagged unemployment rate, and region and time fixed effects in the second step. They account for spillover effects between regions, the effect of agglomeration, and the effects of regional monopsony in wage curve estimations. Their findings confirm the existence of wage curve in West Germany even though it is relatively small. However, the strength of the wage curve is stronger for those who have lower bargaining power in labor market, such as female, younger, foreigners. The lagged wages are found significant while spatial spillovers of the local unemployment rates are insignificant. The absence of the spatial spillover effects is explained by the lack of foresight of the local agents in Germany.

Fingleton and Palombi (2013) provide some evidence for the spatial effects in the UK wage curve using 408 British local administrative units for the period 1998-2010. They test wage curve against two competing theories; Urban Economics and New Economic Geography. However, they could not find evidence in favor of one theory over the other. They were able to conclude that wage curve holds in the neighborhood of -0.10. More importantly, their findings show that local wages are affected most by the unemployment rate of regions within commuting distance. Deller (2011) examines the spatial heterogeneity in the US wage curve exploiting the county level data for the year 2006. The OLS estimations confirm the existence of unemployment elasticity of pay which is around -0.05, while using geographically weighted regression (GWR) enable him to conclude that there is spatial heterogeneity in wage curve estimations in the US.

Ramos, Nicodemo, and Sanroma (2015) estimate a dynamic spatial wage curve for Spain using individual data for 50 NUTS-3 regions over the period 2000-2010 following the estimation methods used in Baltagi, Blien, and Wolf (2012). They show that spatial lags of unemployment and wages matter and should be controlled for in wage curve estimations. Their findings confirm the dynamic wage curve with relatively small but significant short-run elasticity of -0.02 which becomes -0.10 in the long-run. Falk and Leoni (2011) provide some evidence on spatial wage curve for Austria based on 99 administrative districts for the year 2001. They show that ignoring spatially lagged wages in wage curve estimations leads to bias in unemployment elasticity of pay. The direct effect of unemployment is found as -0.03 while the total effect is about -0.05. Also, the unemployment elasticity of pay is found to be -0.04 when the spatial correlation is ignored.

Baltagi and Rokicki (2014) reexamine the Polish wage curve considering the spatial spillovers between 16 NUTS-2 level Polish regions using individual data for the period 1999-2010. The spatial spillovers are taken into account by including spatially weighted regional unemployment rates along with another independent variable. Furthermore, the study employs group-specific regional unemployment rates, to test the responsiveness of the wages of various groups to the own-group unemployment rate. Also, endogeneity of unemployment rates is accounted for with instrumenting the regional unemployment rates by their one-year lagged values in an FE-2SLS model. They confirm the existence of Polish wage curve with an unemployment elasticity around -0.06 and it is more elastic for men. The inclusion of spatial spillover effects leads to a decrease, in the estimation of own-region elasticity from -0.06 to -0.05, while the effect of the unemployment rate of adjacent regions is found around -0.11. Moreover, using gender-specific unemployment rates result in a decrease of both own-region elasticity and spatial spillover for men, and lose of significance in estimates for women.

Finally, Baltagi, Baskaya and Gul (2015) revisited the differences between wage curves of formal and informal worker considering the spatial-spillover effect of the regional unemployment rates using 26 NUTS-2 level Turkish regions over the period 2005-2013. They use non-agricultural unemployment rates as a measure of regional unemployment rates. They construct spatial weight matrices using inverse distance and expansion in the four-lane highways in Turkey as a proxy to the quality of roads between regions. Their results show that wages of workers are responsive to both local and neighboring regions' unemployment rates with elasticities -0.11 and -0.09 for all workers, respectively. They show the wages of informal workers changes in response to the local and neighboring regions' unemployment rates more than the wages of formal workers do. They find that local wage elasticity of informal workers is around -0.17, while the effect of the unemployment rate of adjacent regions for informal workers is around -0.085. Furthermore, they show that the difference between the spatial-spillover effect of unemployment rates of formal and informal workers is higher for younger, less experienced and less educated workers.

4.2.3 The Turkish Wage Curve

Ilkkaracan and Selim (2003) is the first study on wage curve in Turkey. They find a wage curve for men but not for women using Labor Force Participation and Wage Structure Survey for 1994. This study based on a cross-section data over 7 regions, so that the regional differences are not controlled for. Moreover, the actual degrees of freedom for the parameter of interest, the logarithm of the unemployment rate, becomes even negative with other control variables, which may lead unreliable results, as indicated by Card (1995).

Baltagi et al. (2012) show that there is a wage curve for Turkey with unemployment elasticity of pay -0.099 using micro-level data for the period 2005-2008 over 26 NUTS-2 level regions, controlling for the endogeneity of the regional unemployment with FE-2SLS. They also interestingly show that women have a more elastic wage curve than men do. Their further disaggregation indicates that wages of more vulnerable groups such as, young, less-educated and less-experienced are more responsive to the regional unemployment variations. Furthermore, Baltagi et al. (2013) investigate the differences in the wage curves between formal and informal workers using THLFS for years 2005-2009. They confirm the wage curve of Turkey as -0.10 and women are more sensitive to the unemployment rate once endogeneity problem is addressed by using the FE-2SLS method and instrumenting regional unemployment rate by the lagged unemployment rate. They show that informal workers are more responsive to the regional variations of the unemployment rate. This finding also holds for disaggregated groups based on age and gender.

Konyali (2012) finds weak evidence on the existence of wage curve in Turkey for the period 2007-2009 using Living Condition Survey. She finds evidence for the wage curve for men when the group-specific unemployment rate is used. However, she finds no evidence for a wage curve for women in Turkey, regardless of the use of aggregate unemployment rate or group-specific unemployment rate. The drawback of the study is the use of 12 NUTS-1 regions over three-year observation. As it has been pointed out in Card (1995) the actual degrees of freedom for the unemployment variable is number of regions times number of years, in this case only 36, which may cause a significant problem as the estimates only significant at the 10% level. On the other hand, Ilkkaracan et al. (2013) compare the wage curve estimates based on various definitions of unemployment using THLFS for the period 2005-2010. They find an unemployment elasticity of pay of -0.05 for private workers. They show that wages of male workers are more sensitive to the regional unemployment than that of women, using unemployment rates of 26 NUTS-2 level regions, disaggregated with three level of education. None of the above wage curve studies for the Turkish case addresses the spatial effects with overall regional unemployment rates and regional group-specific unemployment rates.

4.3 Data and Model

4.3.1 Data

In this study, we exploit the Turkish Household Labor Force Survey (THLFS) data collected and released by Turkish Statistical Institute (TURKSTAT). The THLFS is a micro level annual survey on the whole nation in which around 500.000 individuals are surveyed on their demographic and work-related characteristics. This rich data set has been conducted since 1988 and has had two major changes in 2000 and 2004. Therefore we used the data for the period 2004-2013.

Demographic characteristics of all individuals surveyed are available in the THLFS; the employment status and related information are observed for those who are 15 years old or above. Furthermore, income information is available only for regular and casual workers. Thus, we exclude individuals younger than 15 years old and employers, self-employed, and unpaid family workers. Also, individuals who started to work in the month of the survey or did not provide their wage information are excluded. Moreover, those individuals with missing information on employment, wage, and demographic characteristics are dropped from the sample. After exclusion of those observations, we are left with 821,166 observations of which 192,721 are female. Wages are reported as monthly income after deduction of taxes, compulsory social security and other life insurance premiums.

In our estimations, we used natural logarithm of hourly real wages, calculated by using monthly wages and usual working hours. All wages are expressed in 2003 liras using the consumer price index (CPI) which is available in NUTS-2 level regions in TURKSTAT.

Unemployment rates data is obtained also from TURKSTAT. Regional aggregate unemployment rates, as well as disaggregated unemployment rates by gender, age group, and education level, are available publicly in TURKSTAT's webpage. Moreover, shape files for the NUTS-2 regions that are used to create spatial weights are extracted from the shape files available in EUROSTAT for all European countries in all NUTS levels. Summary statistics of our sample for the variables used in our estimations are presented in Table 4.1.

As we are interested in investigating the spatial spillover effects of the unemployment rate on individual wages over 26 NUTS-2 regions of Turkey and the effect of disaggregated unemployment rate for various groups, Figure 4.1 and Figure 4.2 show the spatial distribution of unemployment rates for the year 2008. Although we use the overall unemployment rate in our estimation, we also provide estimations using the non-agricultural unemployment rate in order to compare our results with earlier wage curve estimates for Turkey (e.g. Baltagi et.al. 2012, 2013). Figure 4.1 shows, the spatial variation of the unemployment rate across 26 NUTS-2 level regions of Turkey.

The spatial spillovers seem to be more pronounced in the male unemployment rate than female unemployment rate, while the female workers experience a wider range of unemployment rate than male workers. Also, the unemployment rate for younger workers, who are between 15 and 24 years old, is higher and span in a wider range, while the spatial spillover of the unemployment rate is more apparent for older, who are 25 years old or above. Furthermore, as Card (1995) points out, the responsiveness of wages to the group-specific unemployment rate may be more relevant to the skill levels. TURKSTAT provide unemployment rates for four different education levels, which enable us to map the group-specific unemployment rates in Turkey. Figure 4.2 shows the spatial variation of group-specific unemployment rates for levels of schooling in the year 2008. It seems that the patterns of the spatial spillovers of the unemployment rate vary across skill levels

4.3.2 The Model

We start our analysis with the conventional wage curve equation in which logarithm of individual real wages are regressed on the logarithm of the regional unemployment rate and other individual characteristics as well as regional and time fixed effects. Following Blanchflower and Oswald (1990) and Card (1995) the model we use in our analysis can be written as;

$$lnw_{ijt} = \beta lnU_{jt} + X'_{ijt}\boldsymbol{\delta} + \mu_j + \lambda_t + u_{ijt} \tag{4.1}$$

where lnw_{ijt} is the natural logarithm of individual real hourly wages and lnU_{jt} is the regional unemployment rate. Individual characteristics are included in the vector X_{ijt}. Blanchflower and Oswald (1990, 1994) use equation (4.1) to estimate unemployment elasticity of pay, β, using the OLS assuming the unemployment rate is predetermined. They find that the elasticity is robust across countries and time periods as well as various group disaggregation. However, later studies point out that the unemployment elasticity of pay differs for different groups of workers using aggregate unemployment rates (e.g. Card, 1995, Baltagi et al., 2012). The unemployment elasticity of pay may show differences depending on whether the unemployment rate is measured as overall regional unemployment or the regional group-specific unemployment. It is more likely that a worker competes with other workers within groups, such that unemployment rate of skilled workers may not be a threat for unskilled workers (Card, 1995). Therefore, we compare the wage curve for different type of workers using overall unemployment rates and the group-specific unemployment rates.

The data used in the wage curve literature is either individual data or regional averages. It is also realized that the relevant degrees of freedom is not the number of individuals used in the analyses, but the number of regions times the number of periods. Thus, the use of individual data may cause serious bias in the standard errors and misleading inference if the number of regions and time periods are small (see Card, 1995). However, it may not be a serious problem for countries with 200-500 region-year cell observations. We follow Baltagi et al. (2012), and use individual level data, as the THLFS is available in 26 NUTS-2 level regions over ten years period. Another problem with the estimation of responsiveness of individual wages to the regional unemployment rate is the simultaneity bias due to the endogenous regional unemployment rate. Although this problem has been realized by Blanchflower and Oswald (1990), they fail to find a good instrument. Baltagi and Blien (1998) show that the wage curve estimates are sensitive to the endogeneity. They use lagged unemployment rate as an instrument for the unemployment rate in a fixed effect 2SLS (FE-2SLS) setting. This procedure has been followed by many other wage curve studies including Turkish case (e.g. Baltagi et al. 2012, 2013; Baltagi and Rokicki, 2014). We also address the endogeneity of local unemployment rate instrumenting with its one-year lagged values.

If the wage curve results in the workers' heterogeneity and monopsonistic competition, the responsiveness of the wage curve depends on the spatial spillover of local employment conditions (Longhi et al., 2006). Also, the efficiency wage and labor turnover cost models suggest that the regional unemployment rate is being used as a threat to workers in order to convince them not to shirk or quit (Fingleton and Palombi, 2013). The power of this threat is likely to be influenced by the unemployment rate in proximate regions. Thus, ignoring the effect of the unemployment rate of surrounding regions may introduce a bias. The spatial spillovers of the unemployment rate can be taken account by including spatially weighted unemployment rate. Following Longhi et al. (2006), and Baltagi and Rokicki (2014), we use the following equation to estimate spatial spillovers of unemployment rate in Turkey;

$$lnw_{ijt} = \beta lnU_{jt} + \gamma(\Sigma_k W_{jk} lnU_{jt}) + X'_{ijt}\boldsymbol{\delta} + \mu_j + \lambda_t + u_{ijt} \tag{4.2}$$

where all variables are the same as described above for equation (4.1), except, here we include spatially weighted local unemployment rates. The weight matrix, W, describes the connections among regions which is based on the contiguity of regions. That is, each row of W assigns the neighbor regions with 1 and 0 otherwise. Then each row is normalized.

In other words, the average of the unemployment rate of contiguous regions is included as another explanatory variable in our wage equation. We defined two other spatial weight matrices to check the robustness of our specification. First, we checked our results against the spatial weight matrix of which each element is the inverse Eucledian distance between the corresponding regions. This matrix is also row normalized. Longhi et al. (2006) discuss that the use of full distance matrix may not be ideal because the regions further away may average out the positive dependence between the neighboring regions. Therefore, we check our results with a hybrid weight matrix suggested by Longhi et al. (2006) in which distance between non-contiguous regions are assumed to be infinite. In other words, the inverse Eucledian distance between contiguous regions are kept in the matrix while entries for the non-contiguous regions are assumed to be zero, then the matrix is row normalized. Furthermore, we address the endogeneity of own-region unemployment and regional spatial spillovers by instrumenting them with their one year lagged values in a FE-2SLS setting following Baltagi and Rokicki (2014).

4.4 Results

4.4.1 The Turkish Wage Curve Revisited

We present the standard wage curve results in Table 4.2 in which only the estimates of unemployment elasticity of pay, β, has been shown for the sake of saving space. The dependent variable is the real individual wages and the variable of interest is the regional unemployment rate which is available for 26 NUTS-2 level regions of Turkey. As it has been mentioned in the previous sections, the existing wage curve estimates for Turkey based on some subsamples of the Turkish labor force, and time periods are relatively short. On the other hand, our data covers a 10-year period so that we can have a clear picture of the Turkish wage curve. Table 4.2 shows estimation for all workers as well as some subgroups using official unemployment rates for the whole population, except for column 2 in which we use non-agricultural unemployment rate. Using regional and time fixed effects, the results show that overall unemployment elasticity is -0.048 for all workers. In the second column, we exclude workers in the agriculture sector and use non-agricultural unemployment rate in the wage curve estimation. It results that using non-agricultural unemployment rate leads a higher elasticity, -0.067. This estimation also enabled us to compare our results with the earlier wage curve estimates for Turkey, Baltagi et al. (2012, 2013). It is found that using a longer period results in a higher elasticity. The official unemployment rate for the whole population is used for the estimates of disaggregated samples. Disaggregation by gender shows that the wage curve of male workers, -0.054, is more elastic than that of female workers, -0.031. This result is in line with that found in other countries, in general. However, the previous studies on the Turkish wage curve either find a higher unemployment elasticity for women (e.g. Baltagi et al., 2012, 2013) or no statistically significant unemployment elasticity (e.g. Ilkkaracan and Selim, 2003; Konyali, 2012).The first two studies use regional non-agricultural unemployment rates while the last two use regional unemployment rates. We tested our results for different time periods, i.e. period 2005-2008 and 2005-2009 to be able to investigate the discrepancy between our results and the previous studies. Unemployment elasticity of pay for the comparison periods with unemployment rates as well as with non-agricultural unemployment rates are presented in the Appendix, Table 4.A1. It results in Panel A of Table 4.A1 that there is no evidence for a wage curve in the periods 2005-2008 and 2005-2009, except for one case, the male workers in 2005-2009 sample, when the overall unemployment rate is used which is similar to Ilkkaracan et al. (2013) and Konyali (2012). Furthermore, Panel B of Table 4.A1 shows the same comparison but with the non-agricultural unemployment rate. We were able to replicate the results from Baltagi et al. (2012, 2013), such that the wage curve for women is more elastic than for men in the periods, 2005-2008 and 2005-2009. However, a reverse relationship pops-out for the period 2004-2013, that is, women have an elasticity of -0.044 and men of -0.074. This comparison may indicate that women labor force participation is getting stronger in the Turkish labor market and Turkish women get more stick to their job.

Estimates for the formal and informal workers are in line with Baltagi et al. (2012) which show that informal workers' wages are more affected from variations in the regional unemployment rate. Also, we disaggregate our sample as workers in the public and private sector. The results confirm the expectations that workers in public sector would not be affected from the variation of the regional unemployment rate due to the nationwide bargaining process. However, the wage curve phenomenon is more about the private sector. It is also found that less educated/low-skilled workers have more elastic wage curve than more educated/high-skilled do. Appendix Table 4.A2 shows unemployment elasticities for various disaggregated samples. It is found that younger, temporary, and less experienced workers have more elastic wage curves than, older, permanent, and more experienced workers do, respectively. Furthermore, the unemployment elasticity of pay is higher for those who work in small firms than those work in medium-sized or large firms.

Table 4.2, shows the FE-2SLS estimation results, in which contemporary regional unemployment rates are instrumented by one-year lagged regional unemployment rate to address the endogeneity problem. We find that unemployment elasticity of real wages for all workers is -0.071 when the regional unemployment rate is used, and endogeneity of the regional unemployment rate is taken account. The FE-2SLS results for the non-agriculture sectors with non-agricultural regional unemployment rate show that the unemployment elasticity is -0.107, which is very similar to Baltagi et al. (2012, 2013). This indicates that the wage curve for the non-agricultural sector is robust to the different time periods. FE-2SLS results also show that men are more responsive to the regional unemployment rate than women are; -0.73, -0.063, respectively. Formality status has an effect on the responsiveness of the wages to the regional unemployment rate variations, such that wage curve of informal workers is more elastic than formal workers; -0.09 and -0.046, respectively. Although the fixed effect results for the formal and informal workers are quite similar to the fixed effect estimates of Baltagi et al. (2013), our FE-2SLS estimates are smaller in absolute value, especially for the informal workers. Results show that there is no wage curve for the workers in the public sector, whereas workers in the private sector have an unemployment elasticity of pay of -0.098. Controlling for the endogeneity of the regional unemployment rate, we find that workers with less than high school education have the most elastic wage curve among education groups and the elasticity is decreasing with education level. Appendix Table 4.A2 shows that, although the magnitudes of the elasticity change when we take account for the endogeneity, the general pattern remains same such that more fragile groups; less experienced, temporary, younger and workers in small firms, have a higher unemployment elasticity of pay than that of the corresponding groups.

We present the unemployment elasticities of real wages for the various groups of male and female workers separately in Table 4.3 and Table 4.4, and appendix tables Table 4.A3 and Table 4.A4. Table 4.3 shows the unemployment elasticities of pay for the male worker groups. We find statistically significant wage curves for all groups of male workers with fixed effects estimation, while the FE-2SLS leads to insignificant elasticities for the male workers in the public sector. The FE-2SLS results show that formal and informal men have almost same elasticities, -0.053, -0.054, respectively. These findings contrast with Baltagi et al. (2013). The highest elasticity among the education/skill level groups of men is found to be -0.094 for those who have less than high school education. Moreover, the high school graduates and college graduates have about the same elasticity. Furthermore, Appendix Table 4.A3 shows that younger, less experienced, temporary male workers and those who work in the small firms have higher elasticities than their counterparts.

Table 4.4 shows that there are no significant elasticities for female workers who are formal workers and college graduate with fixed effects estimation. FE-2SLS estimation shows that informal female workers have the highest unemployment elasticity of real wages, -0.22, among all groups. Women working in the public sector have a significant positive unemployment elasticity, 0.09, while women working in the private sector have an elasticity of -0.131. We were able to find statistically significant wage curve for only those women who have less than high school education and high school graduates among education/skill groups, such that the first group has a higher elasticity of -0.17 while the second group has an elasticity of -0.085. Appendix Table 4.A4 shows that women who are middle-aged (25-44), have high tenure in the firm, and work in the large firms have no significant wage curve in the fixed effects estimation. However, the unemployment elasticity for the middle-aged (25-44) women becomes statistically significant in the FE-2SLS estimation while estimates for women working in medium-sized firms lose its significance. According to the age groups of the women workers, the middle-aged (25-44) women is the least responsive group with an elasticity of -0.049 with respect to the regional unemployment rates, whereas young women (aged 15-24) is the most responsive age group with an elasticity of -0.085. Moreover, women in the temporary jobs have a higher elasticity, -0.136, than their counterparts in the permanent jobs, -0.048. Women with low tenure have an unemployment elasticity of pay of -0.089 and only women in the small firms have a wage curve with an elasticity of -0.172.

4.4.2 The Turkish Spatial Wage Curve

The wage curve estimations including spatially weighted unemployment rate are presented in Table 4.5-Table 4.9. We report only the unemployment elasticity of pay, β, and the spatial spillover the unemployment elasticity of real wages, γ, in equation (4.2).

Table 4.5 shows the estimation results of equation (4.2) with the spatial weight matrix defined as row-normalized contiguity of regions matrix. Results show that while the fixed effect estimates of the unemployment elasticities become slightly small in absolute value, and the spatially weighted unemployment rate has a significant direct effect on the individual wages. We find that the unemployment elasticity of wage is -0.046 for all workers, while spillover unemployment elasticity of proximate regions is -0.027. On the other hand, we take account for the endogeneity of the regional unemployment rate and spatially weighted unemployment rate by employing the FE-2SLS instrumenting one-year lagged values of unemployment variables. The FE-2SLS results show that the unemployment elasticity of pay of own region is -0.056, and the spatial spillover unemployment elasticity of contiguous regions is -0.087. These results indicate that the unemployment rate of contiguous regions has a significant direct effect on individual wages, even higher than the own-region unemployment rate effect. Comparison of the unemployment elasticity of real wages in Table 4.2 and Table 4.5, suggests that ignoring spatially weighted unemployment rate introduces a downward bias, such that inclusion of the spatially weighted unemployment rate lowers the unemployment elasticity in absolute term. There is a statistically significant spatial spillover effect in the fixed effect estimates for all groups except for the formal workers, who work in the private sector, and who have more than high school education. However, with the FE-2SLS model, we find statistically significant spatial spillovers for all groups. Elasticities for the further disaggregated worker groups are presented in Appendix Table 4.A5. We find statistically significant unemployment elasticities for all groups and spatial spillovers for all groups except for workers who work in the large firms ignoring the endogeneity of the regional unemployment rate.

The results above hold for the non-agriculture sample with greater magnitudes; the FE-2SLS results show that the non-agricultural unemployment elasticity is -0.084 while the spatial spillover of non-agricultural unemployment rate of contiguous regions is -0.162. Thus, for the non-agricultural sector, individual wages are almost twice as responsive to the surrounding regions' unemployment rate as own region unemployment rate. The unemployment elasticity of pay for own-region unemployment rate for men and women are very close, -0.051 and -0.058, respectively. On the other hand, wages of women are more responsive to the spatial spillovers of unemployment rate than that of men, -0.118, -0.081, respectively. These results are similar to Baltagi and Rokicki (2014) for the Polish case.

Own-region unemployment elasticity of pay and spatial spillover unemployment elasticity of pay for informal workers is higher than that for formal workers. Moreover, informal workers are more responsive to the unemployment rate of neighboring regions than that of own-region, -0.099, -0.065, respectively. We could not find evidence for the public sector wage curve. However, the spatial spillover of unemployment rate is found to be significant. On the other hand, for the fixed effects estimation, we could not find a significant spatial spillover effect for private sector workers. Controlling for the endogeneity in the FE-2SLS, we find workers in the public sector are affected positively by own-region unemployment rate with elasticity of 0.040 while the responsiveness to the surrounding regions' unemployment rate is -0.086. Furthermore, unemployment elasticity of pay for workers in the private sector is -0.087, and the spatial spillover is -0.079. Public vs private sector disaggregation reveals some interesting results. First, workers in the public sector are responsive to the unemployment rate in the neighboring regions, which may indicate that the wages in the public sector are affected by aggregate unemployment rate rather than the regional unemployment rate. This result is in line with the Blanchflower and Oswald (1990), but contrast with Baltagi and Rokicki (2014). Second, we find that the wages in the private sector are more sensitive to the unemployment rate of own-region than that of surrounding regions. Whenever we find significant spatial spillovers for various groups, unemployment elasticity of pay for own-region was smaller, in absolute value, than the spatial spillovers, except for private sector. This may indicate that the unemployment rates in the proximate regions have a direct effect on individual wages in the private sector. However it is less than the unemployment rate effect of own-region. The FE-2SLS estimates for the skill groups show that the highest unemployment elasticity of real wages and the spatial spillover effects, in absolute value, are experienced by workers who have less than high school education that are considered as the low-skilled workers. The low-skilled workers are the most sensitive skill group to the local unemployment rate and the surrounding regions' unemployment rates with elasticities of -0.075, -0.132, respectively. Both elasticities getting lower by skill level. Unemployment elasticity of real wages is insignificant for the high-skilled workers while the spatial spillover effect is significant, -0.067. In other words, high-skilled workers are more sensitive to the employment conditions of proximate regions than that of their own region.

Furthermore, in Appendix Table 4.A5, among the age groups, young workers (aged 15-24) are the most sensitive group to the unemployment rate variation in their own-local market and in surrounding labor markets. Workers with low tenure have higher unemployment elasticity of earnings and spatial spillover effects. Workers in the small firms have higher unemployment elasticity of pay and spatial spillover elasticity, in absolute value, than the workers in medium- or large-sized firms with the fixed effects estimation. On the other hand, the FE-2SLS estimations show that workers in small firms have unemployment elasticity of -0.096 and spatial spillover elasticity of -0.106. Moreover, the workers in the medium-size firms have the smallest, in absolute terms, unemployment elasticity, -0.028, but the highest spatial spillover elasticity, - 0.163. There is no significant spillover effect for those who work in the large firms but we find an unemployment elasticity of -0.041. These results show that the workers in the large firms are only affected by own region employment conditions. However, the workers in the medium-size firms are the most affected group by the unemployment rate in the neighboring regions. We find that temporary workers have the highest spatial spillover elasticity of -0.221, among all groups, while there is no significant unemployment elasticity for this group of workers.

We present unemployment elasticities and spatial spillover elasticities for the disaggregated groups of male workers in Table 4.6 and Appendix Table 4.A6. The spatial spillovers for the male workers are found to be insignificant for most of the cases in the fixed effects estimation, but the FE-2SLS results show that spatial spillovers of the unemployment rate has a significant direct effect on individual wages of males. Men work in temporary jobs have the highest spatial spillover elasticity, -0.218. However, the unemployment elasticity of pay is insignificant. Also, the FE-2SLS results show that the informal male workers have no significant unemployment elasticity, but have a high spatial spillover of -0.155. However, the formal male workers are more responsive to own-region unemployment rate than contiguous regions' unemployment rate. Men with more than high school education have no significant spatial spillover effects, when the endogeneity problem has been addressed. Moreover, we could not find significant spatial spillover unemployment elasticity of pay for men who work in the large firms.

Table 4.7 shows unemployment elasticities of pay and spatial spillovers for the female worker groups. We find that the women wages are more responsive to the unemployment rate variations in proximate regions than in their own region. The FE-2SLS results show that the formal women workers have the only significant spillover effect, -0.081. However, the informal women have a significant unemployment elasticity of pay, -0.212. That is, formal women are more responsive to the employment conditions in surrounding regions, whereas informal workers are more responsive to own-region employment conditions. Also, we find that women with more than high school education have a significant positive elasticity for own region unemployment, but significant negative spatial spillover elasticities of -0.158.

Furthermore, Appendix Table 4.A7 presents the unemployment elasticity and spatial spillovers for the disaggregated female workers. The table shows that younger female workers get more affected by unemployment in the contiguous regions, whereas the unemployment elasticity is higher for older workers. We find statistically significant spatial spillovers for women who work in the medium-size firms, -0.247. However, there is no significant spillovers for women work in the small or large firms. The highest spatial spillover among all female groups is found for women who work in temporary jobs, -0.384.

4.4.3 Robustness Check for the Spatial Weight

We check our result against two other spatial weight matrices. First, we use a spatial weight matrix based on the inverse distance between regions, described in section 4.3, and present results in Table 4.8. Longhi et al. (2006) points out that using full inverse distances between regions may average out the positive spatial dependence between the contiguous regions. We compare results in Table 4.8 with those in Table 4.5. We find very similar results for the unemployment elasticities of pay with these two different spatial matrices in the fixed effects estimation. However, the spatial spillover effects become positive or insignificant, except for women, public workers, high school graduates. The spatial spillovers are found to be higher for all these groups than their counterpart in Table 4.5. Furthermore, the FE-2SLS estimates of the spatial spillovers are negative and smaller than own-region unemployment elasticities. However, they are statistically insignificant in most of the cases.

Second, having found spatial spillovers does not matter if we use full inverse distance spatial weight matrix, we check our results against a hybrid spatial weight matrix following Longhi et al. (2006) that is described in subsection 4.3.1. The estimates are presented in Table 4.9 and compared with Table 4.5. We find that the unemployment elasticity is -0.053 and the spatial spillovers is -0.096 for all workers with the hybrid spatial weight matrix which are slightly higher than those found using the contiguity matrix. The results are hardly changed in Tables 4.9 comparing Table 4.5.

Based on our different weight specifications, we can conclude that spatial spillovers of unemployment rate in the contiguous regions matter, even in a greater magnitude than own-region unemployment rate. However, using distance of one region to the rest of the regions, wipes out this effect as it has been mentioned in Longhi et al. (2006).

4.4.4 Unemployment Elasticities with Group-Specific Regional Unemployment Rates

While most of the wage curve studies present disaggregated wage curves for various groups, only a handful of them use disaggregated unemployment rates following Card's (1995) suggestion. The group-specific regional unemployment rates are available for disaggregated groups based on gender, education, and age in the TURKSTAT's website. We make use of these disaggregated unemployment rates and estimate the wage curves for various group of workers.

Table 4.10 shows the wage curve estimates along with estimates based on group-specific regional unemployment rates of various groups; gender, skill, and age groups. The results show that the unemployment elasticity for women become smaller in absolute value, in the fixed effects estimation when using regional women unemployment rates, -0.021 comparing wage curve estimates in Table 4.2. Moreover, while unemployment elasticity for women with the overall unemployment rate is found to be -0.063 with the FE-2SLS estimation, using the regional female unemployment rate leads to a smaller and insignificant unemployment elasticity. On the other hand, using regional male unemployment rate instead of the overall unemployment, lowers the elasticities for men, in absolute value, -0.068, -0.073, respectively.

Next, we present for the first time, wage curve estimates for workers disaggregated by three skill levels with overall regional unemployment rates and group-specific regional unemployment rates. We can compare Table 4.10 with Table 4.2 to see how unemployment elasticities change with the use of overall unemployment rate and group-specific unemployment rates for disaggregated samples. In Table 4.2, where the overall unemployment rate is used, the fixed effects and FE-2SLS estimation results show that workers with less than high school education/low-skilled is the most responsive group to the overall regional unemployment rate variations.However, the workers with more than high school education is the least responsive group. Controlling for the endogeneity of unemployment, the elasticities become higher for all of the three skill levels.

The results obtained using group-specific unemployment rate reveals some different conclusions than those obtained using overall regional unemployment rate in Table 4.10. Using the skill-specific unemployment rates in the fixed effect estimation, we find that the low-skilled workers have a higher elasticity than high-skill workers but a lower elasticity than medium-skilled workers, -0.043, -0.027, -0.054, respectively. Furthermore, the FE-2SLS results show that elasticities become larger for all groups. However, the low-skilled workers experience the lowest unemployment elasticity of -0.054. On the other hand, the high-skilled workers are less responsive to the group-specific unemployment rate variations than medium-skilled workers are, -0.076, -0.094, respectively.

TURKSTAT also releases the unemployment rates based on age groups, considering differences in the labor market behavior of the young workers (aged 15-24) and older workers (aged 25+). The last two columns of Table 4.10 show unemployment elasticities for these two groups. We find, in Appendix Table 4.A2, that unemployment elasticity for young workers is higher than that for old workers when we use the overall unemployment rate, -0.096, -0.065, respectively. However, in Table 4.10, using age-specific unemployment rates shows that the young workers are even more sensitive to the group-specific unemployment rate, whereas older workers are less sensitive. The FE-2SLS results show that young workers have an elasticity of -0.131 while unemployment elasticity for older workers is -0.052.

Furthermore, Table 4.11 shows the estimates of the unemployment elasticity of pay and spatial spillover unemployment elasticity with the group-specific unemployment rates. Using group-specific unemployment rate in the fixed effect estimation results in small changes in the magnitude of the unemployment elasticity of the female and male workers. However, the spatial spillovers for female workers is almost as half as that found using the overall unemployment rate, -0.046. There is no significant spatial spillovers effect for the male workers. On the other hand, there are no wage curve or spatial spillovers for women in the FE-2SLS estimation, whereas the male workers have significant unemployment rate and spillovers unemployment elasticity, -0.056, -0.061, respectively.

There are no spatial spillovers for skill groups except for the medium-skilled group, -0.033, in the fixed effects estimation. On the other hand, wages of the medium-skilled workers are the most responsive wages to own-region group-specific unemployment rates.The FE-2SLS results show that there is no evidence for the unemployment elasticity or spatial spillovers for the high-skilled workers once group-specific unemployment rate is used. On the contrary, we find a strong evidence for the unemployment elasticities and spatial effects for the low-skilled and medium-skilled workers. We find that medium-skilled workers are more sensitive to the regional unemployment of medium-skilled workers in their own local market and the surrounding labor markets with elasticities of -0.070 and -0.112, respectively. Similarly, the low-skilled workers are less sensitive to the unemployment rate of own-region than that of contiguous regions with elasticities, -0.039, -0.093, respectively.

Lastly, there was no evidence for the spatial spillovers effects for young workers (15-24) with the fixed effects estimation using the overall unemployment rate in Table 4.A5. However, we find a significant spatial spillovers, -0.056, for this group using group-specific unemployment rate. The fixed effects and FE-2SLS results show that the wages of the young workers are responsive to the own-region unemployment rate and contiguous regions' unemployment rate with elasticities of -0.091, -0.138, respectively. Those are higher than that found for the old workers, -0.041, -0.071, respectively.

4.5 Conclusion

In this paper, we reexamined the Turkish wage curve considering the endogeneity problem, spatial dependence and effect of group-specific regional unemployment rates. The data used in this study comes from the THLFS of TURKSTAT over 26 NUTS-2 regions for the period 2004-2013. Using lagged value of unemployment rate as an instrument for the current unemployment rate in the FE-2SLS estimation, we find that unemployment elasticity of pay is -0.071 for all workers in Turkey.

Furthermore, disaggregated wage curves show that the male workers have a more elastic wage curve than female workers with elasticities of -0.073, -0.063, respectively. We further compare the male and female wage curves for different periods and confirm that women wages were more responsive to the regional unemployment rate. However, this relationship has been reversed in the recent years. This finding is reasonable, as the female employment has been increasing and women get more stick to their job in the last decade in Turkey. Moreover, the highest unemployment elasticity of real wages is found for the workers who work in small firms as, -0.116.

The effect of the spatial dependence of local labor markets on the wage curve estimates is addressed by introducing a spatially weighted regional unemployment rate as an additional explanatory variable. Treating regional unemployment as predetermined and estimating wage curve with the fixed effects, shows that there is a spatial spillover unemployment elasticity, -0.027, but own-region unemployment elasticity changes slightly higher, -0.046, for all workers. On the other hand, instrumenting the unemployment rate and spatially weighted unemployment rates with their one-year-lagged values, in the FE-2SLS estimation, we find that own-region unemployment elasticity of real wage is -0.056, and spatial spillover unemployment elasticity of pay is -0.087 for all workers. This shows that individual wages are more sensitive to the employment conditions in the contiguous regions than their own region. This indicates that ignoring spatial spillovers of unemployment rates would bias the wage curve estimates. We find that female wages are more sensitive to the unemployment rate of contiguous regions than male wages, -0.118, -0.081, respectively. However, women are less affected by own-region unemployment rate than men, -0.051, - 0.058, respectively. Another important finding is that we find that workers in the public sector have a positive relationship with their own-region unemployment, while there is a significantly negative spatial spillover unemployment elasticity, -0.086. The estimations for various groups of workers show that the highest unemployment elasticity of pay is found for workers who work in the small firms, -0.096, and the highest spatial spillover unemployment elasticity of pay is found for those who work in temporary jobs, -0.221. Checking for different spatial weight matrix, we confirm the Longhi et al. (2006) such that using full Euclidean distances among regions averages out the positive spatial dependence, such that no significant spatial spillover effects have been found.

Lastly, we reconsider responsiveness of wages of various groups to their group-specific regional unemployment rates rather than the overall regional unemployment rates. We find that the use of group-specific unemployment rate has very subtle changes in the men's wage curve. However, the unemployment elasticity of pay for women become very small and insignificant. Moreover, for the first time we tested wage curves for skill-specific unemployment rates and find that the medium-skilled workers have the highest unemployment elasticity, while low-skilled workers have the lowest elasticity which is a different conclusion than that found using overall regional unemployment rate. Furthermore, we estimate the wage curve with group-specific unemployment and spatially weighted group-specific unemployment rates. Moreover, we find negative elasticities for own region and contiguous regions for both female and male workers. However, instrumenting the unemployment rates with their one-year lagged values, the unemployment elasticity and spatial spillovers for females lose their significance. We find that the medium-skilled workers are more sensitive to both own-region and contiguous regions' unemployment rate variations than their low and high-skilled counterparts. Also, young workers are more responsive to the unemployment in their own region and proximate regions than old workers.

Overall, we estimated the spatial wage equation in Turkey. We find strong evidence on the direct effect of unemployment rates of contiguous regions as well as own-region. These findings may be an evidence for the efficiency wage curve, labor turnover cost, or monopsonistic competition hypotheses of the wage curve.

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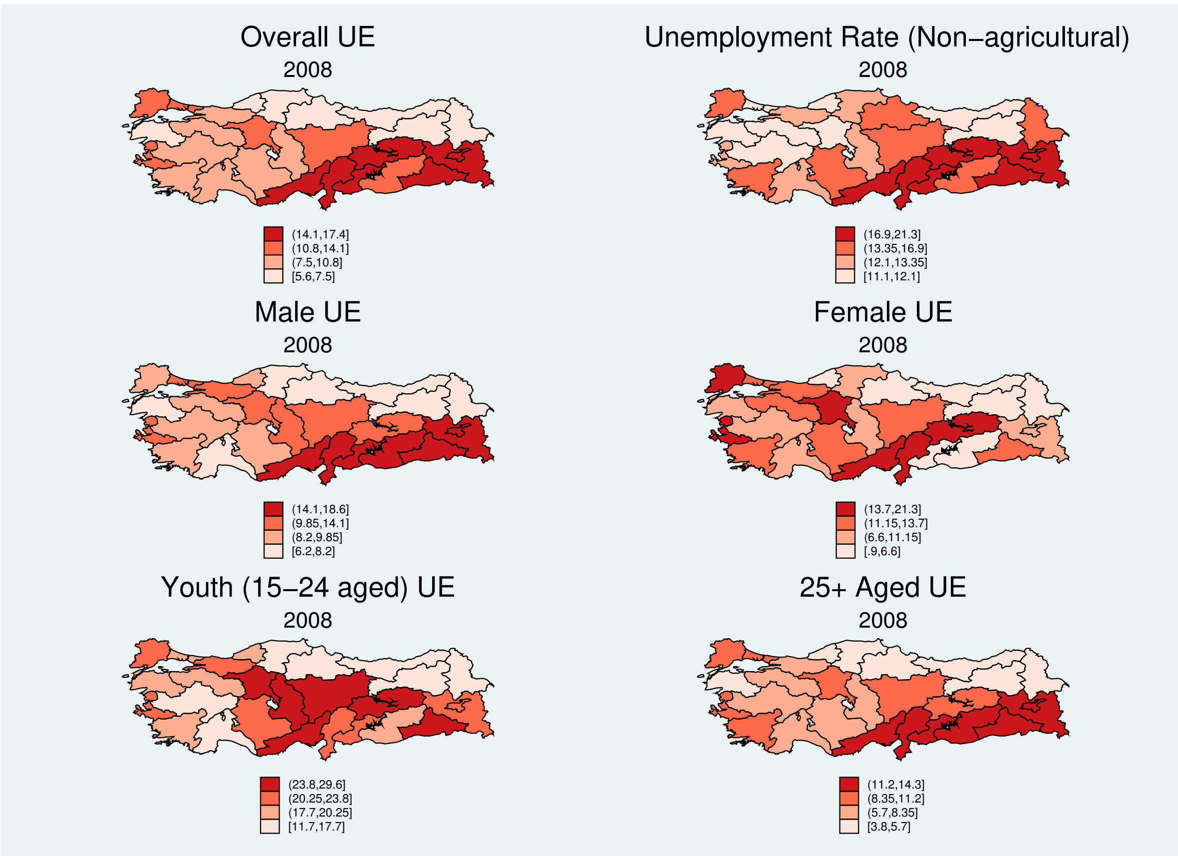
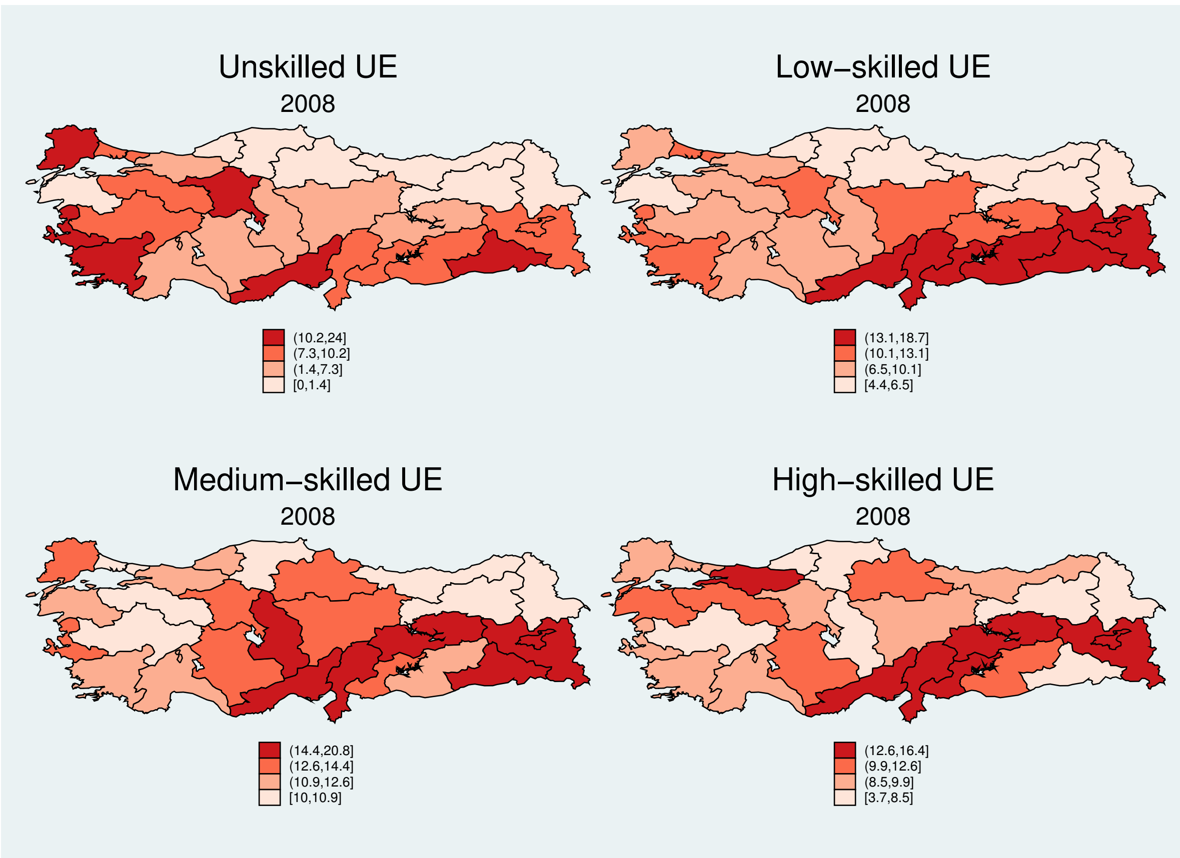
Figures and Tables

Figure 4.1: Spatial Distribution of Overall and Group-Specific Unemployment Rates (UE) in the year 2008.



Source: TURKSTAT. (a) Spatial distribution of overall UE. (b) Spatial distribution of UE in which agriculture sector is excluded. (c) and (d) Spatial distributions of male and female-specific UE, respectively. (e) and (f) Spatial distributions of for youth (15-24 years old) UE, and UE of workers aged 25 and older, respectively

Figure 4.2: Spatial Distribution of Skill-Specific Unemployment Rates (UE) in the year 2008.



Source: TURKSTAT. (a)Spatial distribution of unskilled (no schooling) UE. (b) Spatial distribution of low-skilled (less than high-school education) UE. (c) Spatial distribution of medium-skilled (high school graduates) UE. (d) Spatial distribution of high-skilled (college graduates) UE.

Table 4.1: Descriptive Statistics

VARIABLES	# of Obs.	Mean	Std. Dev.
Age	821,166	34.63	10.39
Having Social Security	821,166	0.755	0.430
Permanency of the job	821,166	0.902	0.297
Hours worked in a week	821,166	51.08	13.53
Urban	821,166	0.820	0.384
Overall Unemployment(%)	821,166	10.54	3.479
Female	821,166	0.235	0.424
Married	821,166	0.704	0.457
Years of Education	821,166	9.127	4.197
Education Levels			
Illiterate	821,166	0.0148	0.121
No Schooling	821,166	0.0409	0.198
Primary School	821,166	0.306	0.461
Middle School	821,166	0.166	0.372
High School	821,166	0.128	0.334
Vocational High School	821,166	0.130	0.337
College	821,166	0.228	0.420
Part-time Job	821,166	0.0309	0.173
Hourly Wage	821,166	2.732	2.872
Tenure in the Current Job	821,166	6.698	7.629
Firm Size			
Firm (less than 10 Workers)	821,166	0.340	0.474
Firm (1-24 Workers)	821,166	0.120	0.325
Firm (25-49 Workers)	821,166	0.178	0.383
Firm (more than 49 Workers)	821,166	0.362	0.481
Private Sector	821,166	0.737	0.440
Disaggregated Unemployment Rates (%)			
Non-agricultural UE	821,166	13.17	3.522
High-Skilled UE	821,166	10.42	2.691
Medium-Skilled UE (%)	821,166	12.96	3.667
Low-Skilled UE	821,166	10.24	4.057
Unskilled UE	821,166	7.991	6.382
Male UE	821,166	9.960	3.329
Female UE	821,166	11.91	5.129
15-24 Aged UE	821,166	19.92	5.389
25+ Aged UE	821,166	8.533	3.103

Source: Authors calculation from TURKSTAT Household Labor Force Surveys (2004-2013).Unskilled refers to those who have no schooling; low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.2: Unemployment Elasticity of Pay (β)

					Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Female	Male	Formal	Informal	Public	Private	Low	Medium	High
Panel A: Fixed Effects											
β	-0.048***	-0.067***	-0.031***	-0.054***	-0.033***	-0.057***	0.000	-0.065***	-0.065***	-0.042***	-0.013*
	(0.003)	(0.004)	(0.008)	(0.004)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)	(0.006)	(0.007)
# of Obs.	821,166	795,563	192,721	628,445	619,730	201,436	215,627	605,539	409,278	212,287	187,437
R-squared	0.63	0.63	0.68	0.62	0.62	0.32	0.54	0.51	0.42	0.54	0.49
Panel B: FE-2SLS											
β	-0.071***	-0.107**	-0.063***	-0.073***	-0.046***	-0.090***	0.021**	-0.098***	-0.099***	-0.052***	-0.022*
	(0.007)	(0.008)	(0.014)	(0.008)	(0.007)	(0.015)	(0.010)	(0.008)	(0.010)	(0.012)	(0.013)
# of Obs.	752,634	729,761	178,531	574,103	572,798	179,836	195,253	557,381	371,572	194,271	176,084
R-squared	0.64	0.64	0.69	0.63	0.63	0.33	0.57	0.51	0.42	0.55	0.51

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.3: Male Unemployment Elasticity of Pay (β)

			Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Formal	Informal	Public	Private	Low	Medium	High
				Panel A: Fixed Effects					
β	-0.054***	-0.074***	-0.039***	-0.063***	-0.014**	-0.068***	-0.068***	-0.043***	-0.028***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.007)	(0.009)
# of Obs.	628,445	611,422	475,492	152,953	157,865	470,580	340,666	163,734	117,408
R-squared	0.62	0.62	0.60	0.34	0.53	0.50	0.44	0.53	0.48
				Panel B: FE-2SLS					
β	-0.073***	-0.111***	-0.053***	-0.054***	-0.005	-0.092***	-0.094***	-0.044***	-0.047***
	(0.008)	(0.009)	(0.008)	(0.017)	(0.011)	(0.009)	(0.010)	(0.013)	(0.017)
# of Obs.	574,103	558,883	438,114	135,989	141,700	432,403	308,668	149,621	110,063
R-squared	0.63	0.63	0.62	0.34	0.55	0.50	0.44	0.55	0.50

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.4: Female Unemployment Elasticity of Pay (β)

			Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Formal	Informal	Public	Private	Low	Medium	High
				Panel A: Fixed Effects					
β	-0.031***	-0.044***	-0.010	-0.079***	0.046***	-0.063***	-0.083***	-0.040***	0.016
	(0.008)	(0.009)	(0.008)	(0.018)	(0.010)	(0.010)	(0.015)	(0.013)	(0.012)
# of Obs.	192,721	184,141	144,238	48,483	57,762	134,959	68,612	48,553	70,029
R-squared	0.68	0.68	0.67	0.35	0.60	0.56	0.36	0.55	0.51
				Panel B: FE-2SLS					
β	-0.063***	-0.086***	-0.019	-0.226***	0.092***	-0.131***	-0.170***	-0.085***	0.025
	(0.014)	(0.015)	(0.014)	(0.036)	(0.018)	(0.019)	(0.027)	(0.025)	(0.020)
# of Obs.	178,531	170,878	134,684	43,847	53,553	124,978	62,904	44,650	66,021
R-squared	0.69	0.69	0.68	0.35	0.63	0.56	0.35	0.55	0.53

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.5: Unemployment Elasticity of Pay with Spatial Spillover Effects- Contiguity Weighted (β and γ)

					Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Female	Male	Formal	Informal	Public	Private	Low	Medium	High
					Panel A: Fixed Effects						
β	-0.046***	-0.065***	-0.027***	-0.053***	-0.033***	-0.055***	0.004	-0.064***	-0.061***	-0.040***	-0.014**
	(0.003)	(0.004)	(0.008)	(0.004)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)	(0.006)	(0.007)
γ	-0.027***	-0.063***	-0.083***	-0.014**	-0.002	-0.030**	-0.040***	-0.013	- -0.052***	-0.029**	0.013
	(0.006)	(0.008)	(0.014)	(0.007)	(0.007)	(0.013)	(0.010)	(0.008)	(0.009)	(0.012)	(0.014)
# of Obs.	821,166	795,563	192,721	628,445	619,730	201,436	215,627	605,539	409,278	212,287	187,437
R-squared	0.63	0.63	0.68	0.62	0.62	0.32	0.54	0.51	0.42	0.54	0.49
					Panel B: FE-2SLS						
β	-0.056***	-0.084***	-0.051***	-0.058***	-0.040***	-0.065***	0.040***	-0.087***	-0.075***	-0.039***	-0.012
	(0.008)	(0.009)	(0.015)	(0.009)	(0.008)	(0.020)	(0.012)	(0.009)	(0.011)	(0.014)	(0.015)
γ	-0.087***	-0.162***	-0.118***	-0.081***	-0.041***	-0.099***	-0.086***	-0.079***	-0.132***	-0.079***	-0.067**
	(0.015)	(0.022)	(0.030)	(0.017)	(0.015)	(0.038)	(0.023)	(0.018)	(0.022)	(0.026)	(0.030)
# of Obs.	752,634	729,761	178,531	574,103	572,798	179,836	195,253	557,381	371,572	194,271	176,084
R-squared	0.64	0.64	0.69	0.63	0.63	0.33	0.57	0.51	0.42	0.55	0.51

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.6: Male Unemployment Elasticity of Pay with Spatial Spillovers- Contiguity Weighted (β and γ)

			Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Formal	Informal	Public	Private	Low	Medium	High
				Panel A: Fixed Effects					
β	-0.053***	-0.073***	-0.039***	-0.063***	-0.009	-0.069***	-0.065***	-0.042***	-0.029***
	(0.004)	(0.004)	(0.004)	(0.008)	(0.006)	(0.005)	(0.005)	(0.007)	(0.009)
γ	-0.014**	-0.050***	0.002	-0.003	-0.049***	0.007	-0.033***	-0.017	0.009
	(0.007)	(0.009)	(0.008)	(0.014)	(0.011)	(0.008)	(0.009)	(0.014)	(0.017)
# of Obs.	628,445	611,422	475,492	152,953	157,865	470,580	340,666	163,734	117,408
R-squared	0.62	0.62	0.60	0.34	0.53	0.50	0.44	0.53	0.48
				Panel B: FE-2SLS					
β	-0.058***	-0.086***	-0.048***	-0.011	0.016	-0.079***	-0.066***	-0.029*	-0.044**
	(0.009)	(0.010)	(0.009)	(0.024)	(0.015)	(0.011)	(0.013)	(0.016)	(0.019)
γ	-0.081***	-0.163***	-0.030*	-0.155***	-0.084***	-0.077***	-0.138***	-0.078***	-0.022
	(0.017)	(0.025)	(0.018)	(0.043)	(0.028)	(0.021)	(0.024)	(0.030)	(0.039)
# of Obs.	574,103	558,883	438,114	135,989	141,700	432,403	308,668	149,621	110,063
R-squared	0.63	0.63	0.62	0.34	0.55	0.50	0.44	0.55	0.50

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.7: Female Unemployment Elasticity of Pay with Spatial Spillovers- Contiguity Weighted (β and γ)

			Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Formal	Informal	Public	Private	Low	Medium	High
					Panel A: Fixed Effects				
β	-0.027***	-0.039***	-0.009	-0.074***	0.048***	-0.060***	-0.080***	-0.035***	0.015
	(0.008)	(0.009)	(0.008)	(0.018)	(0.010)	(0.010)	(0.015)	(0.013)	(0.012)
γ	-0.083***	-0.115***	-0.021	-0.159***	-0.020	-0.093***	-0.152***	-0.078***	0.013
	(0.014)	(0.017)	(0.014)	(0.033)	(0.019)	(0.018)	(0.025)	(0.023)	(0.023)
# of Obs.	192,721	184,141	144,238	48,483	57,762	134,959	68,612	48,553	70,029
R-squared	0.68	0.68	0.67	0.35	0.60	0.56	0.36	0.55	0.51
					Panel B: FE-2SLS				
β	-0.051***	-0.071***	-0.012	-0.212***	0.106***	-0.124***	-0.160***	-0.077***	0.044**
	(0.015)	(0.017)	(0.014)	(0.040)	(0.021)	(0.020)	(0.028)	(0.026)	(0.022)
γ	-0.118***	-0.137***	-0.081***	-0.095	-0.085**	-0.093**	-0.109**	-0.080	-0.158***
	(0.030)	(0.042)	(0.029)	(0.078)	(0.041)	(0.038)	(0.053)	(0.050)	(0.047)
# of Obs.	178,531	170,878	134,684	43,847	53,553	124,978	62,904	44,650	66,021
R-squared	0.69	0.69	0.68	0.35	0.63	0.56	0.35	0.55	0.53

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.8: Unemployment Elasticity of Pay with Spatial Spillovers- Full Inverse Distance Weighted (β and γ)

					Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Female	Male	Formal	Informal	Public	Private	Low	Medium	High
					Panel A: Fixed Effects						
β	-0.048***	-0.067***	-0.032***	-0.054***	-0.033***	-0.057***	0.001	-0.063***	-0.065***	-0.042***	-0.013*
	(0.003)	(0.004)	(0.008)	(0.004)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)	(0.006)	(0.007)
γ	0.041**	0.015	-0.155***	0.058***	0.041*	0.095***	-0.091***	0.181***	0.028	-0.079**	0.129***
	(0.019)	(0.025)	(0.048)	(0.021)	(0.022)	(0.036)	(0.026)	(0.025)	(0.027)	(0.034)	(0.041)
# of Obs.	821,166	795,563	192,721	628,445	619,730	201,436	215,627	605,539	409,278	212,287	187,437
R-squared	0.63	0.63	0.68	0.62	0.62	0.32	0.54	0.51	0.42	0.54	0.49
					Panel B: FE-2SLS						
β	-0.071***	-0.109***	-0.064***	-0.073***	-0.046***	-0.094***	0.030***	-0.099***	-0.100***	-0.049***	-0.021
	(0.007)	(0.007)	(0.014)	(0.008)	(0.007)	(0.017)	(0.010)	(0.008)	(0.010)	(0.012)	(0.013)
γ	-0.015	-0.063	-0.049	-0.044	-0.095*	0.162	-0.300***	0.210***	0.102	-0.237***	-0.103
	(0.047)	(0.064)	(0.113)	(0.052)	(0.051)	(0.099)	(0.063)	(0.063)	(0.069)	(0.082)	(0.097)
# of Obs.	752,634	729,761	178,531	574,103	572,798	179,836	195,253	557,381	371,572	194,271	176,084
R-squared	0.64	0.64	0.69	0.63	0.63	0.33	0.57	0.51	0.42	0.55	0.51

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.9: Unemployment Elasticity of Pay with Spatial Spillovers- Contiguous Inverse Distance (Hybrid) Weighted (β and γ)

					Formality		Sector		Skill Groups		
VARIABLES	All	All-NAUE	Female	Male	Formal	Informal	Public	Private	Low	Medium	High
					Panel A: Fixed Effects						
β	-0.046***	-0.065***	-0.026***	-0.053***	-0.033***	-0.054***	0.004	-0.063***	-0.061***	-0.039***	-0.014**
	(0.003)	(0.004)	(0.008)	(0.004)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)	(0.006)	(0.007)
γ	-0.030***	-0.058***	-0.076***	-0.020***	-0.003	-0.035***	-0.036***	-0.018**	-0.058***	-0.034***	0.015
	(0.006)	(0.008)	(0.014)	(0.007)	(0.007)	(0.012)	(0.009)	(0.008)	(0.008)	(0.012)	(0.013)
# of Obs.	821,166	795,563	192,721	628,445	619,730	201,436	215,627	605,539	409,278	212,287	187,437
R-squared	0.63	0.63	0.68	0.62	0.62	0.32	0.54	0.51	0.42	0.54	0.49
					Panel B: FE-2SLS						
β	-0.053***	-0.086***	-0.050***	-0.054***	-0.039***	-0.058***	0.042***	-0.084***	-0.071***	-0.036**	-0.012
	(0.008)	(0.009)	(0.015)	(0.009)	(0.008)	(0.021)	(0.012)	(0.010)	(0.012)	(0.014)	(0.015)
γ	-0.096***	-0.153***	-0.114***	-0.094***	-0.048***	-0.119***	-0.090***	-0.092***	-0.146***	-0.090***	-0.063**
	(0.015)	(0.022)	(0.030)	(0.017)	(0.015)	(0.036)	(0.022)	(0.018)	(0.022)	(0.025)	(0.030)
# of Obs.	752,634	729,761	178,531	574,103	572,798	179,836	195,253	557,381	371,572	194,271	176,084
R-squared	0.64	0.64	0.69	0.63	0.63	0.33	0.57	0.51	0.42	0.55	0.51

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.10: Group-specific Unemployment Elasticity of Pay (β and γ)

				Skill Group			Age Group	
VARIABLES	All	Female	Male	LHS	HS	MHS	15-24	25+
				Panel A: Fixed Effects				
β	-0.048***	-0.020***	-0.051***	-0.043***	-0.054***	-0.027***	-0.083***	-0.039***
	(0.003)	(0.006)	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)	(0.003)
# of Obs.	821,166	192,721	628,445	409,278	212,287	187,437	147,016	674,150
R-squared	0.63	0.68	0.62	0.42	0.54	0.49	0.50	0.62
				Panel B: FE-2SLS				
β	-0.071***	-0.024	-0.068***	-0.054***	-0.094***	-0.076**	-0.131***	-0.052***
	(0.007)	(0.016)	(0.008)	(0.009)	(0.014)	(0.034)	(0.018)	(0.007)
# of Obs.	752,634	178,531	574,103	371,572	194,271	176,084	133,543	619,091
R-squared	0.64	0.69	0.63	0.42	0.55	0.51	0.50	0.63

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Table 4.11: Group-specific Unemployment Elasticity of Pay with Spatial Contiguity Weight (β and γ)

				Skill Group			Age Group	
VARIABLES	All	Female	Male	LHS	HS	MHS	15-24	25+
				Panel A: Fixed Effects				
β	-0.046***	-0.023***	-0.051***	-0.042***	-0.051***	-0.027***	-0.077***	-0.037***
	(0.003)	0.006)	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)	(0.003)
γ	-0.027***	-0.046***	0.006	-0.011	-0.033**	-0.002	-0.056***	-0.028***
	(0.006)	(0.009)	(0.007)	(0.008)	(0.014)	(0.010)	(0.016)	(0.006)
# of Obs.	821,166	192,721	628,445	409,278	212,287	187,437	147,016	674,150
R-squared	0.63	0.68	0.62	0.42	0.54	0.49	0.50	0.62
				Panel B: FE-2SLS				
β	-0.056***	-0.023	-0.056***	-0.039***	-0.070***	-0.188	-0.091***	-0.041***
	(0.008)	(0.016)	(0.010)	(0.010)	(0.018)	(0.135)	(0.024)	(0.008)
γ	-0.087***	-0.012	-0.061***	-0.093***	-0.112***	0.352	-0.138***	-0.071***
	(0.015)	(0.036)	(0.019)	(0.019)	(0.039)	(0.346)	(0.045)	(0.015)
# of Obs.	752,634	178,531	574,103	371,572	194,271	176,084	133,543	619,091
R-squared	0.64	0.69	0.63	0.42	0.55	0.50	0.50	0.63

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects All regressions are weighted by the sampling weight provided in THLFS.Low-skilled refers to those have less than high school education; medium-skilled refers to those have high school education; and, high-skilled refers to those have more than high school education.

Appendix Tables

Table 4.A1: Comparison of Different Time Periods for Overall Unemployment Elasticity of Pay (β)

	Years 2004-2013			Years 2005-2008			Years 2005-2009		
VARIABLES	All	Female	Male	All	Female	Male	All	Female	Male
				Panel A: Overall Unemployment Rate					
β (FE)	-0.048***	-0.031***	-0.054***	0.012	0.006	0.011	-0.010	-0.007	-0.014*
	(0.003)	(0.008)	(0.004)	(0.008)	(0.019)	(0.009)	(0.007)	(0.016)	(0.008)
# of Obs.	821,166	192,721	628,445	302,225	67,189	235,036	380,047	85,322	294,725
R-squared	0.63	0.68	0.62	0.62	0.67	0.61	0.63	0.67	0.61
β (FE-2SLS)	-0.071***	-0.063***	-0.073***	-0.009	-0.011	-0.008	-0.014	-0.029	-0.012
	(0.007)	(0.014)	(0.008)	(0.018)	(0.044)	(0.020)	(0.015)	(0.034)	(0.017)
# of Obs	752,634	178,531	574,103	302,225	67,189	235,036	380,047	85,322	294,725
R-squared	0.64	0.69	0.63	0.62	0.67	0.61	0.63	0.67	0.61
				Panel B: Non-agricultural Unemployment Rate					
β (FE)	-0.067***	-0.044***	-0.074***	-0.022**	-0.048**	-0.019*	-0.042***	-0.051***	-0.043***
	(0.004)	(0.009)	(0.004)	(0.010)	(0.022)	(0.011)	(0.008)	(0.017)	(0.009)
# of Obs.	795,563	184,141	611,422	292,086	63,663	228,423	367,726	81,055	286,671
R-squared	0.63	0.68	0.62	0.61	0.66	0.61	0.62	0.67	0.61
β (FE-2SLS)	-0.107***	-0.086***	-0.111***	-0.113***	-0.221***	-0.089***	-0.125***	-0.186***	-0.110***
	(0.008)	(0.015)	(0.009)	(0.022)	(0.054)	(0.024)	(0.019)	(0.040)	(0.021)
# of Obs.	729,761	170,878	558,883	292,086	63,663	228,423	367,726	81,055	286,671
R-squared	0.64	0.69	0.63	0.61	0.66	0.61	0.62	0.67	0.61

Notes:Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS.

Table 4.A2: Unemployment Elasticity of Pay β

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
					Panel A: Fixed Effects						
β	-0.076***	-0.043***	-0.036***	-0.042***	-0.062***	-0.026***	-0.064***	-0.049***	-0.025***	-0.070***	-0.044***
	(0.008)	(0.004)	(0.009)	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.011)	(0.003)
# of Obs.	147,016	522,448	151,702	674,150	527,443	293,723	279,483	244,507	297,176	80,409	740,757
R-squared	0.50	0.61	0.63	0.62	0.55	0.65	0.43	0.65	0.63	0.33	0.65
					Panel B: FE-2SLS						
β	-0.096***	-0.069***	-0.043***	-0.065***	-0.091***	-0.045***	-0.116***	-0.057***	-0.039***	-0.086***	-0.070***
	(0.016)	(0.008)	(0.016)	(0.007)	(0.009)	(0.010)	(0.012)	(0.011)	(0.010)	(0.024)	(0.007)
# of Obs.	133,543	478,588	140,503	619,091	487,886	264,748	254,260	225,994	272,380	74,607	678,027
R-squared	0.50	0.63	0.64	0.63	0.56	0.66	0.44	0.65	0.64	0.33	0.66

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

Table 4.A3: Male Unemployment Elasticity of Pay (β)

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
	Panel A: Fixed Effects										
β	-0.077***	-0.055***	-0.034***	-0.051***	-0.068***	-0.035***	-0.073***	-0.054***	-0.029***	-0.072***	-0.052***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.012)	(0.004)
# of Obs.	101,148	399,914	127,383	527,297	390,530	237,915	222,047	180,000	226,398	62,300	566,145
R-squared	0.49	0.59	0.62	0.60	0.53	0.64	0.44	0.62	0.61	0.31	0.64
	Panel B: FE-2SLS										
β	-0.102***	-0.074***	-0.038**	-0.068***	-0.091***	-0.056***	-0.110***	-0.064***	-0.043***	-0.082***	-0.076***
	(0.020)	(0.009)	(0.018)	(0.008)	(0.010)	(0.011)	(0.013)	(0.012)	(0.012)	(0.026)	(0.008)
# of Obs.	91,844	364,614	117,645	482,259	360,501	213,602	201,348	165,966	206,789	57,834	516,269
R-squared	0.49	0.61	0.63	0.61	0.54	0.65	0.44	0.63	0.63	0.31	0.65

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

Table 4.A4: Female Unemployment Elasticity of Pay (β)

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
					Panel A: Fixed Effects						
β	-0.080***	-0.008	-0.060**	-0.015	-0.049***	0.015	-0.063***	-0.030***	-0.011	-0.083***	-0.017**
	(0.015)	(0.009)	(0.028)	(0.009)	(0.009)	(0.014)	(0.016)	(0.012)	(0.012)	(0.030)	(0.008)
# of Obs.	45,868	122,534	24,319	146,853	136,913	55,808	57,436	64,507	70,778	18,109	174,612
R-squared	0.54	0.68	0.71	0.68	0.61	0.69	0.50	0.72	0.68	0.39	0.69
					Panel B: FE-2SLS						
β	-0.085***	-0.049***	-0.077*	-0.053***	-0.089***	0.008	-0.172***	-0.028	-0.022	-0.136**	-0.048***
	(0.030)	(0.017)	(0.041)	(0.016)	(0.018)	(0.022)	(0.030)	(0.022)	(0.021)	(0.063)	(0.014)
# of Obs.	41,699	113,974	22,858	136,832	127,385	51,146	52,912	60,028	65,591	16,773	161,758
R-squared	0.54	0.68	0.73	0.69	0.61	0.69	0.50	0.72	0.70	0.38	0.70

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

Table 4.A5: Unemployment Elasticity of Pay with Spatial Spillovers-Contiguity Weighted for Disaggregated Samples (β and γ)

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
					Panel A: Fixed Effects						
β	-0.076***	-0.042***	-0.031***	-0.040***	-0.061***	-0.022***	-0.061***	-0.047***	-0.024***	-0.067***	-0.041***
	(0.008)	(0.004)	(0.009)	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.005)	(0.011)	(0.003)
γ	-0.004	-0.025***	-0.076***	-0.036***	-0.024***	-0.043***	-0.048***	-0.021*	-0.015	-0.049**	-0.034***
	(0.014)	(0.008)	(0.016)	(0.007)	(0.008)	(0.010)	(0.011)	(0.011)	(0.010)	(0.022)	(0.006)
# of Obs.	147,016	522,448	151,702	674,150	527,443	293,723	279,483	244,507	297,176	80,409	740,757
R-squared	0.50	0.61	0.63	0.62	0.55	0.65	0.43	0.65	0.63	0.33	0.65
					Panel B: FE-2SLS						
β	-0.067***	-0.055***	-0.031*	-0.051***	-0.072***	-0.037***	-0.096***	-0.028**	-0.041***	-0.024	-0.059***
	(0.020)	(0.009)	(0.018)	(0.008)	(0.010)	(0.011)	(0.015)	(0.013)	(0.012)	(0.032)	(0.008)
γ	-0.143***	-0.080***	-0.074**	-0.081***	-0.119***	-0.043*	-0.106***	-0.163***	0.008	-0.221***	-0.070***
	(0.038)	(0.018)	(0.037)	(0.016)	(0.020)	(0.023)	(0.028)	(0.026)	(0.022)	(0.056)	(0.015)
# of Obs.	133,543	478,588	140,503	619,091	487,886	264,748	254,260	225,994	272,380	74,607	678,027
R-squared	0.50	0.63	0.64	0.63	0.56	0.66	0.44	0.65	0.64	0.32	0.66

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

Table 4.A6: Male Unemployment Elasticity of Pay with Spatial Spillover Effects-Contiguity Weighted (β and γ)

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
					Panel A: Fixed Effects						
β	-0.078***	-0.054***	-0.029***	-0.049***	-0.068***	-0.031***	-0.070***	-0.053***	-0.029***	-0.070***	-0.050***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.004)
γ	0.024	-0.011	-0.084***	-0.027***	-0.005	-0.039***	-0.033***	-0.011	-0.008	-0.025	-0.028***
	(0.017)	(0.008)	(0.017)	(0.008)	(0.009)	(0.010)	(0.012)	(0.012)	(0.011)	(0.023)	(0.007)
# of Obs.	101,148	399,914	127,383	527,297	390,530	237,915	222,047	180,000	226,398	62,300	566,145
R-squared	0.49	0.59	0.62	0.60	0.53	0.64	0.44	0.62	0.61	0.31	0.64
					Panel B: FE-2SLS						
β	-0.081***	-0.061***	-0.017	-0.053***	-0.071***	-0.047***	-0.081***	-0.040***	-0.047***	-0.015	-0.064***
	(0.024)	(0.011)	(0.020)	(0.009)	(0.012)	(0.013)	(0.016)	(0.015)	(0.014)	(0.036)	(0.009)
γ	-0.101**	-0.070***	-0.117***	-0.082***	-0.109***	-0.044*	-0.141***	-0.133***	0.024	-0.218***	-0.070***
	(0.046)	(0.021)	(0.041)	(0.019)	(0.023)	(0.025)	(0.031)	(0.031)	(0.026)	(0.062)	(0.018)
# of Obs.	91,844	364,614	117,645	482,259	360,501	213,602	201,348	165,966	206,789	57,834	516,269
R-squared	0.49	0.61	0.63	0.61	0.54	0.65	0.44	0.63	0.63	0.31	0.65

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

Table 4.A7: Female Unemployment Elasticity of Pay with Spatial Spillover Effects-Contiguity Weighted (β and γ)

	Age Groups				Tenure		Firm Size			Permanency	
VARIABLES	15-24	25-44	45+	25+	Low	High	Small	Medium	Large	Temporary	Permanent
					Panel A: Fixed Effects						
β	-0.075***	-0.005	-0.060**	-0.011	-0.045***	0.018	-0.059***	-0.027**	-0.009	-0.083***	-0.013*
	(0.015)	(0.009)	(0.028)	(0.009)	(0.009)	(0.013)	(0.016)	(0.012)	(0.012)	(0.030)	(0.008)
γ	-0.082***	-0.082***	-0.004	-0.077***	-0.088***	-0.055**	-0.135***	-0.051**	-0.049**	-0.141**	-0.060***
	(0.026)	(0.017)	(0.054)	(0.016)	(0.017)	(0.025)	(0.029)	(0.021)	(0.022)	(0.059)	(0.014)
# of Obs.	45,868	122,534	24,319	146,853	136,913	55,808	57,436	64,507	70,778	18,109	174,612
R-squared	0.54	0.68	0.71	0.68	0.61	0.69	0.50	0.72	0.68	0.39	0.69
					Panel B: FE-2SLS						
β	-0.051	-0.037**	-0.088**	-0.045***	-0.073***	0.013	-0.166***	0.009	-0.018	-0.083	-0.041***
	(0.034)	(0.018)	(0.042)	(0.016)	(0.019)	(0.023)	(0.031)	(0.024)	(0.022)	(0.068)	(0.014)
γ	-0.220***	-0.124***	0.147*	-0.092***	-0.142***	-0.052	-0.074	-0.247***	-0.048	-0.384***	-0.071**
	(0.065)	(0.036)	(0.085)	(0.033)	(0.037)	(0.046)	(0.062)	(0.050)	(0.042)	(0.128)	(0.029)
# of Obs.	41,699	113,974	22,858	136,832	127,385	51,146	52,912	60,028	65,591	16,773	161,758
R-squared	0.54	0.68	0.73	0.69	0.61	0.69	0.50	0.72	0.70	0.38	0.70

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include age, age square, tenure at the firm and its square, and dummy variables for gender, marital status, 6 education level, ongoing education and if attendance to it required, formality, permanency, urban, extra job, part-time, private sector, 4 firm size, 9 occupation and 10 industry categories as well as time and region (26 NUTS2 level) fixed effects. All regressions are weighted by the sampling weight provided in THLFS. Low(high) tenure refers those workers who have less(more) than the average year of tenure in the current job. Small, Medium, and Large Firm refers those workers who work in a firm which has less than 10 workers, between 10-49 workers, and more than 49 workers, respectively.

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