# ESSAYS ON THE EFFECTS OF COMPULSORY SCHOOLING: EDUCATION, LABOR MARKET, AND TIME USE OUTCOMES

A Ph.D. Dissertation

by
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Ankara
August 2021

To my family

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The Graduate School of Economics and Social Sciences
of
İhsan Doğramacı Bilkent University

by
BETÜL AKAR

In Partial Fulfillment of the Requirements for the Degree of
DOCTOR OF PHILOSOPHY IN ECONOMICS

THE DEPARTMENT OF
ECONOMICS
İHSAN DOĞRAMACI BİLKENT UNIVERSITY
ANKARA

August 2021

I certify that I have read this thesis and have found that it is fully adequate, in scope and in quality, as a thesis for the degree of Doctor of Philosophy in Economics.

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# ABSTRACT

## ESSAYS ON THE EFFECTS OF COMPULSORY SCHOOLING: EDUCATION, LABOR MARKET, AND TIME USE OUTCOMES

Akar, Betül

Ph.D., Department of Economics

Supervisor: Assoc. Prof. Dr. Çağla Ökten

August 2021

This thesis consists of four essays on the effects of the compulsory schooling reform, which extends the mandatory years of schooling from five to eight years in 1997 in Turkey. The first essay utilizes the exogenous education reform as an instrument for the educational attainment to investigate the causal effects of education on voluntary work. The reform increases the education levels of individuals significantly, and increased education of compliers has a negative but insignificant causal impact on the probability and hours of voluntary work for men.

The second essay investigates the impact of increased education, due to the exogenous education reform which extended mandatory secular education by three years, on time spent in religious activities. For women, increased education has a negative causal impact on the allocation of time to religious activities; however, for males, there are no significant

effects of education on time spent in religious activities.

The third essay examines the impact of the extension of mandatory education on mothers' time spent in early childhood care. The compulsory education reform increases mothers' completion of at least compulsory years of schooling. However, it has no significant effect on the primary components of early childhood education, namely time spent in reading to children, playing with children, and talking to children.

The forth essay investigates the impact of the reform on the ethnic gap in educational and labor market outcomes between two ethnic groups in Turkey. For females, the reform widens the ethnic gap in the educational outcomes but has no impact on the ethnic gap in the labor market outcomes. On the other hand, for males, it has no impact on the ethnic disparities in the educational attainment but widens the disparities in the labor market outcomes.

*Keywords:* Compulsory Schooling, Educational Outcomes, Employment, Labor Force Participation, Time Use

# ÖZET

## ZORUNLU EĞİTİMİN ETKİSİ ÜZERİNE MAKALELER: EĞİTİM, İŞ GÜCÜ PİYASASI VE ZAMAN KULLANIMI ÇIKTILARI

Akar, Betül

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Ağustos 2021

Bu tez, Türkiye'de 1997 yılında yasalaşarak zorunlu eğitimi 5 yıldan 8 yıla çıkaran eğitim reformunun etkilerini inceleyen dört makaleden oluşmaktadır. İlk makalede, kişilerin eğitim düzeyleri için eğitim reformu bir araç olarak kullanılarak eğitimin gönüllü davranışlara olan nedensel etkileri araştırılmaktadır. Zorunlu eğitim reformunun kişilerin en az sekiz yıllık zorunlu eğitime katılımlarını belirgin olarak artırdığı fakat artan eğitim düzeyinin erkeklerin gönüllü işler yayıp yapmamasına ve gönüllü işlere harcanan zamana negatif fakat belirgin etkisi olmadığı gösterilmektedir.

İkinci makale, zorunlu eğitim reform ile artan eğitimin dini faaliyetlere olan etkisini araştırmaktadır. Kadınlar için artan eğitim seviyesinin dini faaliyetlere ayrılan zamanı azalttığı erkekler içinse artan eğitimin dini faaliyetler üzerine nedensel etkisinin olmadığı gösterilmiştir.

Üçüncü makale, zorunlu eğitim süresinin uzatılmasının annelerin erken çocukluk eğitimine ve bakımına ayırdığı zamana olan nedensel etkilerini araştırmaktadır. Eğitim reformunun annelerin en az sekiz yıl zorunlu eğitim alma olasılığını artırdığı bulunmuştur. Fakat eğitim reformunun erken çocukluk dönemi eğitiminin en önemli bileşeni olan çocukla kitap okuma, oyun oynama ve konuşmaya ayırdığı zamana etkisi olmadığı gösterilmiştir.

Dördüncü makale, zorunlu eğitim reformunun Türkiye'deki iki etnik grup, Türkler ve Kürtler, arasındaki eğitim ve işgücü piyasası çıktılarındaki etnik farklılıklar üzerine etkisini araştırmaktadır. Kadınları için, reformun eğitim çıktılarındaki etnik farkı genişlettiğini ancak işgücü piyasası çıktılarındaki etnik fark üzerine etkisi olmadığı gösterilmektedir. Öte yandan, erkekler için, eğitim reformunun eğitim çıktılarındaki etnik eşitsizlikler üzerinde hiçbir etkisi olmadığı fakat işgücü piyasası çıktılarındaki etnik farkı genişlettiği bulunmuştur.

*Anahtar Kelimeler*: Zorunlu Eğitim, Eğitim Çıktıları, İstihdam, İşgücüne Katılım, Zaman Kullanımı

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# CHAPTER 1

## INTRODUCTION

The compulsory schooling law is a pervasive policy tool to achieve the higher participation in schooling. Therefore, the examination of the impact of the compulsory schooling policies has received the attention of both policy makers and academics. There is an extended literature on the effects of mandatory years of schooling that utilizes the compulsory schooling law as an instrument for education to generate an exogenous increase in the schooling level of individuals. In particular, recent literature examines the causal effects of the change in the compulsory schooling law on pecuniary as well as non-pecuniary outcomes such as drop-out decisions (Oreopoulos, 2006), returns to schooling (Angrist & Krueger, 1991; Acemoglu & Angrist, 2001), civic participation, trust, and political behavior outcomes (Dee, 2004; Milligan, Moretti, & Oreopoulos, 2004; Larreguy & Marshall, 2017; Yang, 2019); health outcomes (Lleras-Muney, 2005; Oreopoulos, 2007; Clark & Royer, 2013), marriage outcomes (Hener & Wilson, 2018), and fertility behavior (Black, Devereux, & Salvanes, 2008; McCrary & Royer, 2011).

This thesis contributes to the existing literature by investigating the impact of an additional three years of schooling induced by a change in the compulsory schooling reform in 1997 in Turkey on different outcome variables. In particular, the first essay utilizes Turkish compulsory schooling law as an instrument for the educational attainment, and find out how increased education affects the voluntary work of individuals. The second essay examines the effects of increased education which essentially extended compulsory secular education, from five to eight years, on religious outcomes of individuals. The third essay assesses how an extra three years of schooling induced by Turkish compulsory schooling reform affects early childhood care of mothers. The fourth essay investigates the impact of Turkish compulsory schooling reform on the ethnic gap in the educational attainment as well as the labor market outcomes of Turkish and Kurdish individuals, which are the largest ethnic groups in Turkey.

In Turkey, in 1997, the government implemented the compulsory schooling law, which increased the compulsory years of schooling from five to eight years. This law was unexpected and exogenous to parental decisions, and mostly motivated by political factors. More precisely, during Turkey's negotiation process for the European Union membership, the government immediately implemented the compulsory schooling reform that aimed to increase the level of education in Turkey to accelerate the process of the European Union membership (Dulger, 2004). The reform also limited the duration of religious education and postponed the starting age of religious education by three years (Cesur & Mocan, 2018). It affected a large part of population at different birth cohort levels in the 1997-1998 academic year. Thus, Turkish compulsory schooling reform

constitutes a natural experiment that generates an exogenous increase in individuals' schooling levels for certain birth cohorts.

Before 1997, the basic education in Turkey was composed of five years of primary school and three years of junior high school. The five years of primary school was compulsory but the subsequent three years of junior high school was voluntary. After the completion of five years of the primary school, for the voluntary part of the basic education, students could have three choices to choose: (i) going to traditional junior high schools, (ii) going to vocational schools, and (iii) going to the religious schools. Students could also drop out of the school after completing the five years of compulsory primary school and receiving the primary school diploma. In 1997, the government implemented the compulsory schooling law, which extended the mandatory education from five to eight years. Thus, the traditional or vocational junior high schools including the religious junior high school were closed, and did not accept students after the primary school. In other words, students could not obtain the primary school diploma without finishing three years of junior high school.

The year of birth and the school starting age determines the exposure to the education reform. In particular, individuals born before 1986 could drop out after graduating from five years of primary school. On the other hand, individuals born after 1986 had to go on to junior high school and complete eight years of the mandatory education. However, the exposure to the law is unclear for the 1986 birth cohort as the law does not impose strict age restriction, 6-years old starting age cut-off, for the enrollment of students in primary

school in Turkey (Kırdar, Dayioglu, & Koc, 2018; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018; Dursun, Cesur, & Mocan, 2019).

The compulsory schooling reform does not improve the quality of the schooling or the curriculum of the basic education, and it has an outstanding impact on enrollment rates of individuals. However, perfect compliance with the compulsory schooling reform does not occur as the non-attendance of students to the compulsory school results in monetary fines for parents, and the law does not strictly enforce students continue to the compulsory education. Moreover, although the compulsory education is free in Turkey in the state schools, i.e. there is no tuition fee in the compulsory part of education, the cost of school uniforms, transportation, meal or school supplies are incurred by parents. The instruction of language is Turkish for the compulsory schooling in Turkey, and ethnic minorities do not have any option to receive education in their mother tongue.

Turkish compulsory schooling reform is also widely used as an instrument for education to investigate the effects of education on individual level outcomes such as labor market outcomes (Aydemir & Kirdar, 2017; Torun, 2018), drop-out decisions (Kirdar, Dayioglu, & Koc, 2016; Caner, Guven, Okten, & Sakalli, 2016), political and religious behavior outcomes (Cesur & Mocan, 2018), health outcomes (Cesur, Dursun, & Mocan, 2018; Kırdar, Dayıoğlu, & Koç, 2018), and subjective well-being (Dursun & Cesur, 2016). In particular, the existing literature shows that Turkish compulsory schooling reform rises the compulsory years of schooling for both men and women. In line with previous literature, we estimate the local average treatment impact of Turkish compulsory schooling reform by utilizing the data from Turkish Time Use Survey Micro Data Set,

2014-2015, and find that the education reform increases at least middle school completion rates of individuals. In this thesis, we also investigate the impact of an additional three years of schooling induced by the education reform on maternal education. We show that Turkish compulsory schooling reform rises mothers' completion of at least eight years of compulsory schooling, consistent with previous studies investigating the impact of maternal education on different outcomes (see, for instance, Güneş, 2015; Dursun, Cesur, & Kelly, 2017; Özer, Fidrmuc, & Eryurt, 2018). Moreover, using a novel data set from the 2014-2017 Konda Barometer, which provides information about the ethnicity of individuals, we examine the differential impact of the compulsory schooling reform across ethnic Turks and Kurds who form the largest ethnic minority group. We show that the compulsory schooling reform increases junior high school completion rates of both Turkish and Kurdish men equally without having an impact on the ethnic gap while it widens the ethnic gap in junior high school attainment for females.

A number of papers also investigate whether Turkish compulsory schooling reform generates a spillover effect on post-compulsory schooling level, and documents that the reform rises the educational attainment beyond the compulsory level, especially high school attainment (Kirdar et al. 2016, Aydemir & Kirdar, 2017; Torun, 2018; Akyol & Mocan, 2020). We, therefore, examine the effects of the education reform on at least high school completion and at least university completion of individuals. We, first, investigate the spillover effects of the education reform on post-compulsory schooling of individuals by utilizing the data from Turkish Time Use Survey. Our findings indicate that Turkish compulsory schooling reform generates a positive impact on the high school completion as well as university completion of individuals; however, the effect is not robust across

different bandwidths. Moreover, we examine the spillover effects of the education reform in the sample of mothers, which is a rather select sample in terms of post-compulsory schooling choice, and find that the education reform has no significant impact on mothers' educational level beyond the compulsory years of schooling.

With a different data set, namely a nationally representative data from 2014-2017 Konda Barometer, we find heterogeneous effects of the reform on post-compulsory schooling, and observe that the education reform generates strong spillover effects on the high school completion of Turkish individuals. We also show that the education reform has a positive impact on at least university completion of Turkish individuals; however, the robustness of our findings depends on technical specifications, such as the bandwidth level, the sample specifications, and the order of polynomial of time trend. We should note that the implementation of other policies that overlap with the compulsory schooling reform may also contribute to an increase in university competition, and it is a major challenge to isolate the effect of the reform on university completion. Overall, we conclude that the education reform does not generate a strong and robust spillover effect on the university completion of individuals; however, it seems to increase the likelihood of obtaining at least high school diploma for Turkish individuals, which is consistent with the existing literature.

After we establish the effect of the reform on education levels which forms our first stage of analysis, we examine the effect of the increased education levels of compliers on our outcomes of interest. We, first, examine whether increased education, due to a change in the compulsory schooling reform, has any impact on voluntary work of individuals by

utilizing the change in the compulsory schooling law and using the nationally representative data from 2014-2015 Turkish Time Use Survey. Turkish Time Use Survey is designed to be part of Harmonized European Time Use Study (HETUS) and utilized EUROSTAT (2000a, 2000b) activity classifications and coding as its basis. The data compiling and publishing periods are decennial. Time Use Survey is one of the more comprehensive and reliable sources for understanding time use patterns of individuals as it gathers detailed information about the socioeconomic indicators for households, demographics, labor market, and general health status as well as indicators for understanding the time use patterns of individuals. The survey includes four questionnaires: Household Questionnaire, Individual Questionnaire, Diaries, and Weekly Work Schedule. In this thesis, we conduct our main analysis by using the data from the Individual Questionnaire and Diaries: The Individual Questionnaire collects data on individuals' age, gender, country of birth, technology device ownership, education status, general health status, and labor market outcomes. Apart from background characteristics, the Individual Questionnaire also collects detailed information about voluntary activities (organization-based volunteering and direct volunteering (*helping*)), leisure, and sports activities that respondents attend in the last four weeks. The diary part of Turkish Time Use Survey, on the other hand, collects the data daily activities the individuals do on weekdays and weekend. In particular, individuals are asked to fill two diaries one for a weekday and one for a weekend. The respondents record all of their daily activities for 24 hours at ten-minute intervals. Hence, this data provides rich information on how individuals spend their time in Turkey.

This thesis provides a first causal evidence of education on volunteering in the literature not only at the extensive margin but also at the intensive margin by using a compulsory schooling reform as an instrument for education. In particular, we assess whether an exogenous increase in the years of schooling affects voluntary work defined as organization-based volunteering, i.e., *formal volunteering* and direct volunteering (*helping*), i.e., *informal volunteering*. Consistent with the literature, we find that education is positively and significantly associated with men's voluntary work in OLS analysis. However, when we instrument education with compulsory schooling reform, a different picture emerges. We observe that education has a negative but insignificant impact on men's voluntary work, both in the probability of volunteering and time spent in voluntary activities. Education remains insignificant when we examine direct volunteering (helping) and organization-based volunteering outcomes separately. On the other hand, for women, both OLS estimations and IV results yield insignificant effects of education on volunteering. Considering our positive significant OLS results and negative insignificant IV results for men, we conclude that OLS analysis produces upward bias estimates of schooling on volunteering behavior outcomes due to omitted variable bias. In other words, variables such as family background and social values are likely to impact both education levels and volunteering behavior resulting in a positive association of education and volunteering in OLS studies.

We also explore the potential channels through which education may affect voluntary work. Specifically, we examine whether education has an effect on the voluntary work of individuals through the labor market channel. Examining the labor market channel, we find that education improves males' probability of earning higher than minimum wages

but has no impact on weekly work hours. Moreover, consistent with our main results, we document that increased education is positively correlated with voluntary work but has negative but insignificant causal effects for employed males. We do not find any significant impact of education on labor market outcomes of women. We also consider that the limited substitution effect of wage increase may account for our main findings for men.

In the third chapter of this thesis, we investigate the impact of increased education, due to an exogenous secular education reform, on time spent in religious activities by utilizing the Turkish Time Use Survey data. We employ the Instrumental Variable method by using the extension of compulsory education from five to eight years in Turkey as an instrument for education. In particular, we find out how the secular education reform affects time spent in religious activities on weekdays as well as weekend. We find that the increased education due to compulsory schooling has a negative causal impact on the allocation of time to religious activities; however, for males, there are no significant effects of education on time spent in religious activities. In contrast to previous studies examining the impact of education either on religious identity or religious activity participation and frequency, this essay contributes to the existing literature by providing the first causal evidence on the impact of education on the religiosity of individuals at the intensive margin, namely time spent in religious activities. Our results are in line with the previous studies that show that education reduces the religiosity of individuals. As a result, we suggest that increased secular education plays an important role in altering individuals' religious behavior through its effects on preferences.

Utilizing data from 2014-2015 Turkish Time Use Survey, we also assess whether an exogenous increase in the compulsory years of schooling affects time spent in early childhood care. In particular, we investigate how the exposure to the reform has any impact on mothers' time spent in early childhood care, particularly time spent in reading to children, playing with children, and talking to children. More precisely, we estimate the intent-to-treat impact of the compulsory schooling reform on time spent in early childhood care by implementing the reduced form regression models for mothers who have at least one child under the age of 3. We find that the reform had no significant effect on the primary components of early childhood education, namely time spent in reading to children, playing with children, and talking to children. We also explore the factors that may affect time spent in early childhood care. Specifically, we examine whether the education reform that increased mothers' educational attainment has any effect on labor market outcomes and time spent in household care and leisure activities as the reform may also affect preferences for work and leisure. However, although the reform has no impact on the quality of time spent with children as time spent in reading to children, playing with children, talking to children does not change, the reform increases time spent in other daily activities accompanied by children, especially time spent in leisure activities and household care with children. We do not find any impact of the education reform on the labor market outcomes of mothers. Our results differ from OLS studies that find a positive association between a mother's own schooling and time spent on children and as such can result from omitted variable bias due to ability and values affecting both maternal education and childcare outcomes.

We, finally, examine the impact of a compulsory schooling reform on the ethnic gap in education as well as labor market outcomes between ethnic Kurds and Turks in Turkey, where a gap in educational attainment and employment exists to the detriment of Kurds, especially for females. We use a novel data set retrieved from the Konda Barometer, which is collected by the respective research and consulting company, Konda Research and Consultancy, in Turkey. These surveys are periodical polling series and conducted on the first week of each month regularly; 11 monthly Barometer are produced each year. It aims to examine the political and social tendencies and preferences of society for different subgroups of Turkey. Most importantly, it provides unique information about the educational as well as labor market outcomes by the ethnicity of individuals in Turkey.

Utilizing data from Konda Barometer, we first examine the effects of the reform on the new compulsory schooling level as well as at the post-compulsory schooling level to examine possible spillover effects of the reform on the ethnic gap between Kurds and Turks. We provide the first piece of evidence on the effect of the Turkish compulsory reform on the ethnic gap at the post-compulsory level. The compulsory schooling reform increases at least junior high school completion rates of both Turkish and Kurdish males equally without having an impact on the ethnic gap in the educational outcomes. However, for females, the compulsory schooling reform widens the ethnic gap in the educational attainment between ethnic Kurds and Turks.

We then investigate how the education reform on the ethnic gap in the labor force participation, employment, and unemployment outcomes between ethnic Kurds and Turks. To best of our knowledge, this thesis is the first study that examines the effect of

the compulsory schooling reform on the ethnic gap in labor market outcomes. The education reform significantly increases the probability of participating in the labor force as well as being employed of both Turkish and Kurdish men although the ethnic gap in labor market outcomes increases between the two groups. For women, an increase in compulsory schooling increases the labor force participation rates of both Turkish and Kurdish females but ethnic disparities are not eliminated by the reform.

We also investigate the potential channels through which the education reform may affect the ethnic disparities in the educational attainment as well as the labor market outcomes of individuals. Differences in financial constraints, family structure, and social norms are likely to be the mediating factors for observed education outcomes. Kurds have larger household size and lower household income. Furthermore, they are less likely to consider themselves as conservative as opposed to modern and more likely to be pious compared to ethnic Turks. Albeit, the reform has an impact on social norms and decreases conservativeness and piousness for both groups, which might be an additional mediating factor for increased labor force participation of women in addition to an increase in their human capital.

# CHAPTER 2

# EDUCATION AND VOLUNTARY WORK

## 2.1. INTRODUCTION

This chapter is joint work with Pelin Akyol and Çağla Ökten. A previous version of this paper was circulated in 2019 with the following reference: Akar, Betul & Akyol, Pelin & Okten, Cagla, 2019. "Education and Prosocial Behavior: Evidence from Time Use Survey," IZA Discussion Papers 12558, Institute of Labor Economics (IZA).

“A growing empirical literature has shed light on the importance of voluntary work and shown that caring for others has crucial economic consequences for both developed and developing countries. OECD (2015) reports that the volunteer sector is a considerable economic force in the OECD area, where the economic value of volunteering is estimated as 1 trillion US dollars, a 1.9% substantial share of GDP. For instance, this report document that the value of volunteer time in the United States is 621.7 billion US dollars,

equivalent to 3.7% of GDP, whereas, in Mexico, the economic value of volunteered time is 19.7 billion US dollars, which constitutes 1% of GDP. On the other hand, in Turkey, the value of volunteered hours in 2015 is approximately 3,587 billion US dollars of paid work time, composing 0.62% of Turkey's GDP.[1]

Several studies document a strong and positive correlation between educational attainment and volunteering suggesting that more educated individuals are more likely to display voluntary behavior than low educated individuals (Smith, 1994; Freeman, 1997; Wilson, 2000; Bekkers, 2006; Gesthuizen, Van Der Meer, & Scheepers, 2008; Van Ingen & Dekker, 2011; Gesthuizen & Scheepers, 2012). However, existing studies fail to establish a causal effect of education on volunteering because they do not account for unobserved heterogeneity at the individual level. More precisely, there may be some unobservable individual factors such as ability and intelligence, and unobservable family factors such as values and social norms that may affect both educational attainment and volunteering that give rise to omitted variable bias. Furthermore, reverse causality may exist: not only schooling may promote engagement in voluntary activities but also displaying voluntary behavior may affect the level of education as hard and soft skills gained through voluntary activities may facilitate learning school subjects and satisfy college entrance requirements. In a departure from OLS studies that find a positive association between education and volunteering, Gibson (2001) analyses the effect of education on volunteering using a sample of twins, and hence can control for family fixed effects. Interestingly, this study

[1] This estimation is based on authors' calculations with the data from 2014-15 Turkish Time Use Survey. In 2015, the working age population in Turkey spent estimated 2,340 million volunteering hours.

finds a negative and significant association between education and volunteering[2]. However, as in all twin studies, the sample of twins is not a representative sample of the entire population, and twin studies address the *omitted variable bias* only to some extent as Griliches (1979), Ashenfelter and Krueger (1994) point out measurement error in schooling is seriously exacerbated in the within-twin estimates, and that the economic returns to schooling may have been underestimated in twin studies. Furthermore, twin studies do not address other endogeneity problems such as reverse causality that is relevant in our context.

In this paper, we address the crucial limitations in existing studies by investigating the causal effects of education on the voluntary work of individuals. Ours is the first paper that provides causal evidence of education on volunteering not only at the extensive margin but also at the intensive margin by using the 1997 Turkish compulsory schooling reform as an instrument for education. We also explore causal channels, in particular, the labor market channel, through which education may affect individuals' voluntary work. While our study uses Turkish data, our results on the causal effect of education on volunteering are relevant for many countries experiencing an increase in education levels.

Prior to 1997, five years of primary school was compulsory in Turkey. The subsequent three years of middle school was optional. After the compulsory schooling law was

[2] Dee (2004) also examines the causal effect of college entrance on adult civic behaviors by using several measures, including individuals' participation in voluntary activities in the last twelve months. This study shows that college entrance is positively associated with volunteer participation; however, it has a negative but statistically insignificant causal impact on the probability of volunteering in the last twelve months. Nevertheless, Dee (2004) only examines the impact of education on the volunteering at extensive margin, but a broader empirical investigation is needed to understand the effects of schooling on the voluntary behavior of individuals.

enacted in the 1997-1998 academic year, students had to complete eight years of mandatory schooling before having a right to drop out of school. Therefore, individuals born before 1986 could drop out after they complete five years of primary school, whereas those who were born after 1986 had to complete eight years of schooling, i.e., five years of primary school and three years of middle school. As a result, the law generated an exogenous increase in education of individuals born after 1986, but it did not affect those born before. This reform was unexpected and exogenous to parental decisions and mostly motivated by political factors. Hence, it is a natural experiment that creates a treatment group that consists of those born after 1986 and a control group that includes those born before.

We examine the causal effects of education on voluntary work by utilizing the change in the compulsory schooling law and using nationally representative data from the Turkish Time Use Survey. In particular, we assess whether an exogenous increase in the years of schooling affects voluntary work defined as organization-based volunteering, i.e., *formal volunteering* and direct volunteering (*helping*), i.e., *informal volunteering*. We first investigate the effects of the change in the compulsory schooling law on the schooling outcomes. Then, we examine whether the exogenous increase in schooling has any impact on the voluntary work of individuals. Finally, we assess how an increase in years of schooling affects the labor market outcomes of individuals.

We use the 2014-2015 Turkish Time Use Survey microdata set to estimate the causal effects of education on a number of voluntary work outcomes: *i*) voluntary activity participation, *ii*) the number of voluntary activities, *iii*) the frequency of voluntary

activities, and *iv*) time spent in the voluntary activities. Time Use Survey is one of the most comprehensive and reliable sources for understanding the voluntary work of individuals. It collects detailed data on socioeconomic indicators for households, demographics, and labor market outcomes as well as indicators for understanding the time use patterns of individuals. We use treatment status, defined according to the year of birth, as an instrument and employ the Instrumental Variable (IV) method to isolate the causal effects of education. Our identification strategy is similar to other studies in the literature that analyzes the effect of education on pecuniary and non-pecuniary outcomes by leveraging compulsory education reforms (Angrist & Krueger, 1991; Milligan, Moretti, & Oreopoulos, 2004; Oreopoulos, 2006; Güneş, 2015; Dursun & Cesur, 2016; Aydemir & Kirdar, 2017; Cesur & Mocan, 2018; Torun, 2018; Yang, 2019).

Our findings indicate that the change in the compulsory schooling law rises the educational attainment of individuals. In particular, the change in the compulsory schooling law increased the probability of being at least a middle school graduate, i.e., completing at least eight years of schooling, by 12.9 percentage points and 11.7 percentage points, for males and females, respectively. Consistent with the literature, we find that education is positively and significantly associated with men's voluntary work in OLS analysis. However, when we instrument education with compulsory schooling reform, a different picture emerges. We find that education has a negative but insignificant impact on men's voluntary work, both in the probability of volunteering and time spent in voluntary activities. Education remains insignificant when we examine direct volunteering (helping) and organization-based volunteering outcomes separately. When we consider our positive and significant OLS results and negative and insignificant IV

results for men, we conclude that OLS analysis produces upward bias estimates of schooling on volunteering behavior outcomes. For women, both our OLS estimations and IV results yield to insignificant effects of education on volunteering.

Examining the labor market channel, we find that education improves males' probability of earning higher than minimum wages but has no impact on weekly work hours. Moreover, consistent with our main results, we find that increased education is positively correlated with voluntary work but has negative but insignificant causal effects for employed males. We do not find any significant impact of education on labor market outcomes of women[3].

We check the robustness of our results by generating alternative outcome variables; estimating different models, namely IV Tobit and reduced form regressions; run our regressions in a range of alternative estimations windows; utilizing the data from *diary part of 2014-2015 Turkish Time Use Survey*; and running our regressions under the alternative sample specifications.

The organization of the paper is as follows: Next section provides the necessary background information on voluntary work in Turkey. Section 2.3 describes the 2014-15 Turkish Time Use Survey data. Section 2.4 discusses our conceptual framework that motivates our empirical analysis. Section 2.5 specifies our identification strategy. Section

[3] We should note that female labor force participation at around 32 percent has been low in Turkey and stagnant over time. Akyol and Okten (2019) show that religious and social norms are an impediment to female labor force participation in Turkey.

2.6 presents the descriptive statistics and shows the results. In Section 2.7, we document the impact of education on the labor market outcomes. In section 2.8, we present the robustness check results. Section 2.9 concludes.

## 2.2. BACKROUND: VOLUNTARY WORK

Many papers in sociology, psychology, and economics literature argue that volunteering has several important implications not only for individuals but also for society. Volunteering helps to create social capital and builds social trust and cooperation by developing the norms of altruism, solidarity, civic mindfulness, and respect for diversity (Putnam, 1995, 2000). There are also several positive effects of volunteering on individual outcomes: Engagement in voluntary activity improves the physical and mental health of individuals (Musick & Wilson, 2003; Piliavin & Siegl, 2007), raises self-esteem and self-confidence of individuals (Harlow & Cantor, 1996; Klein, 2017), increases the subjective well-being of individuals (Meier & Stutzer, 2008; Binder & Freytag, 2013; Magnani & Zhu, 2018; Okten, Osili, & Han, 2019), and provides individuals an opportunity to acquire hard and soft skills that enhance their career development and employment prospects (Sauer, 2015; Baert & Vujic, 2016).

Most of the studies in the economic literature use the term “volunteer work” to define actions that are intended to benefit others, whereas the psychology literature uses the more collective term, “prosocial behavior”, which covers all activities such as helping, sharing, volunteering, donating or comforting that are intended to benefit others or the society in general (Batson & Powell, 2003). Volunteering is a complex phenomenon that is hard to

define and measure with precision for cross-country comparison. The International Labor Organization (ILO) (2011) standardized definitions to conceptualize the type of voluntary activities to make a more accurate estimation of volunteer work. In particular, ILO (2011) defines volunteer work as "unpaid non-compulsory work; that is, time individuals give without pay to activities performed either *through an organization* or *directly* for others outside their own household." This definition differentiates volunteering from paid work and leisure activities by indicating that voluntary work should be unpaid and generate goods or services that benefit others. ILO (2011) also considers the degree of formality while categorizing the types of volunteering activities: direct volunteering and organization-based volunteering. Direct volunteering *(called as "informal volunteering"* by Wilson & Musick, 1997*)* is volunteer work performed directly for other households, excluding the household of the volunteer or family members living in other households. This type of volunteering consists of activities such as helping neighbors for preparing food, washing or ironing clothes, and caring for a child. Organization-based volunteering (called "*formal volunteering*" by Wilson & Musick, 1997), on the other hand, is volunteer work performed for or through an organization. It includes activities undertaken through organizations such as working for associations, schools, nurseries, neighborhood groups, or committees.

Salamon, Sokolowski, and Haddock (2018), more recently, consider the volunteer workforce as Full-Time Equivalent (FTE) workers and provides the data on volunteer work to understand the volunteering trends across the countries. They estimate that 70 percent of global volunteer activity occurs more informally through direct person-to-person engagement, while 30 percent takes place formally through organizations or

associations.[4] They find that there is also significant variation in the composition of voluntary activities across countries. For example, while in the US and Germany only about half of the voluntary activities are through direct person to person engagement, in Turkey and Spain, more than 80 percent of voluntary activities is through direct volunteering. Based on data published in Salamon et al. (2018), we calculate that in Turkey, the FTE volunteer workforce in total volunteering as a share of the working-age population is nearly 3 percent, where 1 percent is the male share of volunteer workforce in total volunteering, and 2 percent is the female share of volunteer workforce in total volunteering which is similar to Italy, Spain, and the world average. In the United States, as a share of the working-age population, the FTE volunteer workforce is 6 percent; where 2 percent of the FTE volunteer workforce is men, and 4 percent of the FTE volunteer workforce is women.[5]

## 2.3. DATA

We use data from the Turkish Time Use Survey (TUS) of 2014-15, a nationally representative survey that aims to measure the daily activity patterns, to identify differences in time-use patterns of different gender, age and socioeconomic group, to improve GNDP estimates, and to make international comparisons on time use possible. Turkish Time Use Survey was designed to be part of Harmonized European Time Use Study (HETUS) and utilized EUROSTAT (2000a, 2000b) activity classifications and

[4] Our data also confirms this result for Turkey. See, next section for the detailed information.
[5] For the US estimates, time use surveys are used. For Turkey, John Hopkins Comparative Nonprofit Sector Project and regional averages compiled from a variety of sources are used for organization based and direct volunteering, respectively.

coding as its basis. The data compiling and publishing periods are decennial. The survey, which was conducted among 11,440 households during the period of 1 August 2014-31 July 2015, covers data on socioeconomic indicators for households, demographics (age, gender, region of residence, country of birth), labor market outcomes as well as indicators to understand the time use patterns of individuals. The survey consists of four questionnaires: Household Questionnaire, Individual Questionnaire, Diaries, and Weekly Work Schedule. We conduct our main analysis by using the data from the Individual Questionnaire. In particular, the Individual Questionnaire collects data on individuals' age, gender, country of birth, education status, general health status, and labor market outcomes. Apart from background characteristics, the Individual Questionnaire collects detailed information about voluntary activities (organization-based volunteering and direct volunteering (*helping*)) that respondents attend in the last four weeks. In the Individual Questionnaire, the respondents are just asked about their time allocation to voluntary activities in the last four weeks. Therefore, we do not investigate how individuals allocate their time to other daily activities such as leisure activities, household care activities and personal care activities. Although the Diaries collect information about the other daily activities, we do not use the data from the Diaries as time reports in the Diaries are not comparable with time reports in the Individual Questionnaire. More precisely, the Diaries report respondents' time spent in activities one for a weekday and one for a weekend, while the questionnaire reports time spent in activities for four weeks. Although the diary part of the TUS contains the allocation of time to volunteering activities, we prefer to conduct our analysis by using the data from the individual questionnaire of the TUS as it provides us to assess the voluntary work of individuals both

at intensive and extensive margins. As a robustness check, we also show that the results derived from the diaries are in line with our main results.

In the organization-based volunteering part of the questionnaire, the respondents are asked the following three questions for each activity: *i*) Did you do this type of organization-based volunteering? *ii*) How often did you do this organization-based volunteering activity? and *iii*) How much time did you spend on this organization-based volunteering activity? In the direct volunteering (*helping*) part of the questionnaire, the respondents are asked to answer the following five questions for each activity: *i*) Did you help someone else outside the household?, *ii*) How often did you do this helping activity?, *iii*) How much time did you spend on this helping activity?, iv) For whom did you do this help?, and v) Did you receive any payment in return for this help? If the respondent's answer to the last question is "Yes", we consider that the respondent did not do any helping activities in the last four weeks.

In our main analysis sample, 5 percent of males and 3 percent of females participate in organizational-based volunteering activities, whereas 19 percent of males and 33 percent of females engage in direct volunteering *(helping)* activities.[6] By using the data derived from respondents' answers, we define outcome variables as a combination of thirty-two organization-based and direct volunteering (*helping*) activities to measure the voluntary

[6] Note that in the whole sample, 5 percent of males and 2 percent of females participate in organizational-based volunteering, while 18 percent of males and 26 percent of females engage in direct volunteering *(helping)* activities.

work of individuals.[7] More specifically, we define *four key outcome variables*: *i*) the voluntary activity participation, *ii*) the number of voluntary activities, *iii*) the frequency of voluntary activities, and *iv*) time spent in voluntary activities.[8]

Individual Questionnaire also allows us to study the labor market outcomes of individuals. In particular, the survey collects the data on respondents' employment status, earnings category, weekly hours worked, occupations, side jobs, and full-time/part-time employment. However, the survey data does not provide any information about the respondent's actual wage. In particular, the survey asks the respondent to indicate his/her wage group among five wage groups stated in the Individual Questionnaire.[9] We, therefore, construct an outcome variable, called *Higher than Minimum Wage*, to measure the effects of education on individuals' earnings. In particular, *Higher than Minimum Wage* is a binary variable that takes the value 1 if the respondent's wage group is in the higher than minimum wage groups, and takes the value 0 if the respondent's wage group is in the lower than minimum wage group. The cutoff value that separates the high-wage group and the low-wage group is close to the average minimum wage in Turkey in 2014 and 2015.

[7] See Table A.1. in Appendix A for detailed explanation of the composition of voluntary work.
[8] See Table A.2. in Appendix A for the explanation of the variables.
[9] In the Individual Questionnaire, there are five wage groups that represents the wage bracket of the respondent: Wage group 1: 0-1080 Turkish Liras, (0-397$), Wage group 2: 1081-1550 Turkish Liras (397$-570$), Wage Group 3: 1551-2170 Turkish Liras, (570$-798$), Wage Group 4: 2171-3180 Turkish Liras (798$-1169$), and Wage Group 5: 3181 Turkish Liras and higher (1169$, and higher). Minimum wage in 2014 and 2015 are 891.04 Turkish Liras, (327.5 $), and 1,000.55 Turkish Liras, (367.65$), respectively. We can consider that individuals in wage group 1 earn less than or equal to average minimum wage of Turkey in 2014 and 2015.

As stated in previous sections, we use the compulsory schooling reform as an instrument in our analysis. We distinguish those who were exposed to the compulsory schooling reform and those who were not by leveraging the information on individuals' year of birth. Specifically, we generate a dummy variable, *Reform*, the instrumental variable of our IV regressions, which takes the value 1 if individuals were born after 1986, and 0 otherwise.[10] The TUS does not contain any information on individuals' years of schooling. Instead, the respondents are asked about what is the most recent degree they obtained. The respondents' answers to this question are grouped into 5 categories: *i*) No degree, *ii*) primary school degree, *iii*) middle school degree or vocational middle school degree, *iv*) high school or vocational high school degree, and *v*) university degree or master/Ph.D. By using the data on education status, we generate a binary variable, *Middle School*, which is the key independent variable in our analysis and takes the value 1 if individuals complete *at least* a middle school, i.e., *at least* eight years of schooling, and 0 otherwise.

Our main analysis sample includes individuals born between 1979 and 1993, which corresponds to individuals aged between 21 and 36. The treatment group consists of individuals born between 1987 and 1993 (*aged 21-28*), whereas the control group consists of individuals born between 1979 and 1985 (*aged 29-36*). As we mentioned before, the exposure to reform for the 1986 birth cohort is uncertain. We, therefore, exclude those born in 1986 from our main analysis similar to the studies that use the same education

[10] See Table A.3. in Appendix A for detailed explanation for the cohorts which were exposed to Turkish Compulsory Schooling Reform and the cohorts which were not.

reform in the literature (see Kırdar, Dayioglu, & Koc, 2018; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018).[11]

We also conduct outlier analysis for the data on time spent in direct volunteering activities as well as time spent in organization-based volunteering by using the STATA command *winsor2* with *trim* option. In particular, to exclude the outliers, we trim the top %1 of time spent in direct volunteering and organization-based volunteering distributions. Specifically, we exclude 108 outliers (1.8% of our analysis sample), in which time spent in organization-based volunteering is higher than 240 minutes over four weeks and time spent in direct volunteering (helping) is higher than 3440 minutes over four weeks from our main sample. As pointed out in the literature, stylized questions in a questionnaire need respondents to aggregate memories about several discrete events into a single estimate that may lead to over-reporting on regular events or under-reporting on infrequent events (Juster, Ono, & Stafford, 2003; Seymour, Malapit, & Quisumbing, 2017). It is, therefore, possible to observe respondents' over-reporting or under-reporting on voluntary work outcomes, which may result in outliers in our data. As a robustness check, we also include outliers and show the validity of our results[12]. In addition, we examine whether individuals' over-reporting or under-reporting alter our findings by using the diaries for a weekday and a weekend day, a part of the 2014-2015 Turkish Time Use Survey, where the respondents report their daily activities at ten minutes intervals in a day.

[11] We also estimate our model by including the birth cohort of 1986. Including those born in 1986 does not change our main results.
[12] See, Section 2.7: Robustness Checks, for detailed explanation.

## 2.4. CONCEPTUAL FRAMEWORK

In this section, we discuss the potential channels through which education may affect voluntary work by examining existing theories. First, education may improve the labor market outcomes of individuals. In particular, if schooling increases individuals' real wages, the opportunity cost of non-work hours increases, i.e., the opportunity cost of volunteering increases. Hence, individuals might substitute work for volunteering. However, an increase in wages also have a positive income effect, and assuming that volunteering is a normal good, individuals might increase volunteering (Menchik & Weisbroad, 1987; Freeman, 1997). Individuals might also consider volunteering as an investment good in order to increase their social capital, social reputation, gain experience and improve skills (Menchik & Weisbroad, 1987; Bénabou & Tirole, 2006). Rates of return from volunteering as an investment good might differ across education levels. Alternatively, individuals might view volunteering as an inferior good (Day & Devlin, 1998; Roy & Ziemek, 2000).

Education might also affect the voluntary behavior of individuals by shaping their preferences. For instance, an increase in schooling may affect individuals' altruistic behavior, which in turn influences the voluntary behavior of individuals (Bettinger & Slonim, 2007; Carpenter & Myers, 2010). Furthermore, as Andreoni (1990) points out, individuals may derive utility from helping others, i.e., receiving warm-glow, when they allocate their time to the provision of public good or service. Hence, education may foster altruism among individuals, also increase empathy, and thus rise individuals' engagement

in voluntary activities. On the other hand, education may promote individualistic and competitive attitudes, and hence decrease voluntary activities (Offe & Fuchs, 2002).

Therefore, the existing theories yield ambiguous results for the impact of education on voluntary work. It is ultimately an empirical investigation that will establish the causal link between education and voluntary work. While it is difficult to distinguish between the labor market and preferences channels, we explore the labor market channel and examine whether results in labor market outcomes are consistent with our results on volunteering behavior.

## 2.5. IDENTIFICATION

In the first part of our analysis, we study the effects of education on voluntary work and test our hypotheses by using the ordinary least squares (OLS) technique similar to existing studies in the literature:

$$V_i = \beta_0 + \beta_1 \, Middle\ School_i + \beta_2 \, X_i + \varepsilon_i \qquad (1)$$

$V_i$ represents a particular outcome for the *i*th individual, such as the voluntary activity participation, the number of voluntary activities, the frequency of voluntary activities, and time spent in voluntary activities. *Middle School* is a dummy variable equal to 1 if the individual completes *at least* a middle school, i.e., *at least* eight years of schooling. $X_i$ is a vector of control variables including survey-year fixed effects, survey-month fixed effects, and the region of residence fixed effects for the 12 regions in Turkey. We also

capture age trends in both voluntary work outcomes and education at each birth cohort level by using linear approximations on both sides of the cut off birth year, *1986*, with respect to the education reform status (Fort, Schneewis, & Winter-Ebmer, 2016; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018). Hence, we include control variables defined as re-centered birth year differentiated by exposure to the education reform status. In particular, we define the control variables, re-centered birth year, for the treatment group as $Birth\ Year_{tre} = Reform_i \times (Birth\ Year_i - 1986)$ and re-centered birth year for the control group as $Birth\ Year_{cont} = (1 - Reform_i) \times (Birth\ Year_i - 1986)$, where *Reform* takes the value 1 if the individual was born after 1986, and it takes the value 0 if the individual was born before 1986 and Birth Years is the year that the individual was born. We exclude those born in 1986 from our main analysis due to the uncertainty in the exposure to reform for the 1986 birth cohort. As we mentioned before, existing OLS studies find a positive association of education with volunteering. Estimating equation (1) by OLS may produce upward biased estimates of $\beta_1$ due to the endogeneity problems such as *omitted variable bias* and *reverse causality*. To address these endogeneity problems, we implement the Instrumental Variable (IV) approach, where the exposure to the 1997 Compulsory Schooling Law in Turkey is used as an instrument for education.

The IV approach allows us to distinguish the causal effect of education on individuals' voluntary work by exhibiting exogenous variation in the level of schooling. In particular, IV model estimates the local average treatment effect (LATE) for compliers who receive the treatment due to the exposure to the compulsory schooling reform, but would not

receive the treatment if they were not exposed to the reform. Whilst the set of compliers are not representative of the whole population, Oreopoulos (2006) indicates that an increase in the proportion of the population exposed to the policy change leads LATE estimates to converge the average treatment effect (ATE). Turkish compulsory schooling reform affected a large part of the population across the different birth cohorts' level. Therefore, the estimated local average treatment effect converges to the average treatment effect (Aydemir & Kirdar, 2017).

To assess the impact of education on voluntary work outcomes, we first regress educational attainment, *Middle School*, on the instrumental variable, *Reform. The first stage* regression is expressed in the following functional form

$$Middle\ School_i = \gamma_0 + \gamma_1 Reform_i + \gamma_2 X_i + \mu_i \quad (2)$$

We then identify the causal impact of education on voluntary work outcomes by using two-stage least squares (2SLS):

$$V_i = \varphi_0 + \varphi_1 \widehat{Middle\ School}_i + \varphi_2 X_i + \omega_i \quad (3)$$

Our main estimation sample includes individuals who were born seven years before and seven years after the cutoff year, 1986. We choose this bandwidth by using the STATA command *rdrobus*t for the variable, at least middle school completion. We also present our main results at different bandwidths in the following section.

## 2.6. RESULTS

In this section, we first present the descriptive statistics for our data. Next, we investigate the effects of the compulsory schooling reform on schooling outcomes. And, finally, we examine the impact of education on voluntary work outcomes.

### 2.6.1. DESCRIPTIVE STATISTICS

Table 2.1. displays the descriptive statistics by exposure to the compulsory schooling reform for males (columns 1 and 2) and females (columns 3 and 4). The treatment group consists of individuals born between 1987 and 1993 (*aged 21-28*), and the control group includes those born between 1979 and 1985 (*aged 29-36*). As shown in Panel A, compared with individuals in the control group, the proportion of individuals who hold at least a middle school diploma is higher among those who are in the treatment group (see also Figure 2.1). Specifically, at least middle school competition rate for men (women) is 91 (78) percent in the treatment group and 67 (49) percent in the control group. We should also emphasize that the compulsory schooling law does not have strict restrictions on the attendance of students to the middle school, and therefore on average 9 (22) percent of men (women) did not complete at least middle school although they were exposed to the education reform.

In Panel B of Table 2.1., we provide descriptive statistics for the voluntary work by using the data from the Individual Questionnaire. Our results first show that, on average,

individuals who were exposed to the education reform spent less time in voluntary activities than individuals who were not (see Figure 2.2)[13].

As also presented in Figure 2.3., the average frequency of voluntary activities is about 1 times for males and 2.6 times for females in the treatment group. The corresponding frequencies are 1.2 times and 3.1 times for males and females in the control group, respectively. We also show that, on average, the individuals in the control group participate in more voluntary activities than those in the treatment group that is also depicted in Figure 2.4. Finally, we report that in line with previous results, the proportion of individuals engaging in voluntary activities in the control group are larger than the proportion of individuals engaging in voluntary activities in the treatment group (see also Figure 2.5).

In Panel C of Table 1, we report descriptive statistics for labor market outcomes, which may be important mechanisms through which increased education may have an effect on voluntary work. The proportion of individuals who are employed in the treatment group is lower than those in the control group. The reason why we find these results are most probably due to the fact that the treatment group is younger. For employed individuals, we also report descriptive statistics for *Higher than Minimum Wage* and *Weekly Hours Worked.*[14] There is no significant difference between weekly working hours of employed

[13] For males, the average difference between time spent in voluntary activities in the treatment group and the control group is marginally insignificant, where p-value is 0.13.

[14] The number of observations for weekly hours worked and higher than minimum wage variables are smaller than the number of employed individuals as there is some missing information about individuals' weekly hours worked and earnings level in our data.

males in the treatment and the control groups. Moreover, individuals in the control group earn more than those in the treatment group.

We also display descriptive statistics for organization-based volunteering activities and direct volunteering activities (*helping*) in Table A.4. in Appendix A. We report that on average, individuals in the treatment and control groups engage in more direct volunteering activities (*helping*) than organization-based volunteering activities as the mean of time spent in direct volunteering activities, frequency of direct volunteering activities, number of direct volunteering activities, and direct volunteering activity participation are larger. Our descriptive statistics further shows that compared with males in the control group, males in the treatment group do less organization-based volunteering and direct volunteering activities (*helping*). There is no statistically significant difference between females in the treatment and control group in terms of organization-based volunteering activities. However, females exposed to education reform engage in less direct volunteering activities than those not exposed to the education reform.

Over all, our descriptive statistics show that individuals in the treatment group work less voluntarily than individuals in the control group. However, there are also differences in their marital status, employment status, and school enrolment status that is probably due to the difference in their ages. On average, individuals who were exposed to the education reform tend to be younger and more likely to be single.[15] Hence, these demographic

[15] Age information was given in 5-year intervals in the original data set of Turkish Time Use Survey. We were able to get exact age information from TurkStat via special request.

differences between treatment and control groups need to be addressed in the empirical analyses.

Table 2. 1. Descriptive Statistics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Males | | Females | |
| VARIABLES | Treatment | Control | Treatment | Control |
| Panel A: Personal Information | | | | |
| Middle School | 0.909 | 0.670 | 0.778 | 0.493 |
| | (0.287) | (0.470) | (0.416) | (0.500) |
| Age | 24.64 | 32.63 | 24.62 | 32.75 |
| | (2.073) | (2.015 | (2.028) | (2.046) |
| Marital Status: Single | 0.728 | 0.248 | 0.464 | 0.134 |
| | (0.445) | (0.432) | (0.499) | (0.341) |
| Enrolled in School | 0.277 | 0.083 | 0.216 | 0.085 |
| | (0.448) | (0.277) | (0.411) | (0.278) |
| Observations | 1,291 | 1,529 | 1,401 | 1,669 |
| Panel B: Voluntary Work (for a month) | | | | |
| Time Spent in Voluntary Activities (*in min.*) | 54.07 | 67.37 | 139.6 | 174.8 |
| | (218.7) | (249.6) | (364.7) | (426.9) |
| Frequency of Voluntary Activities | 1.022 | 1.199 | 2.614 | 3.056 |
| | (3.783) | (3.899) | (7.097) | (7.464) |
| Number of Voluntary Activities | 0.345 | 0.422 | 0.653 | 0.769 |
| | (0.920) | (1.032) | (1.284) | (1.311) |
| Voluntary Activity Participation | 0.185 | 0.217 | 0.294 | 0.355 |
| | (0.389) | (0.412) | (0.456) | (0.479) |
| Observations | 1,291 | 1,529 | 1,401 | 1,669 |
| Panel C: Labor Market Outcomes | | | | |
| Employment | 0.732 | 0.912 | 0.320 | 0.349 |
| | (0.443) | (0.284) | (0.467) | (0.477) |
| Observations | 1,291 | 1,529 | 1,401 | 1,669 |
| Weekly Hours Worked | 52.34 | 51.76 | 47.33 | 44.16 |
| | (13.15) | (12.58) | (11.93) | (13.38) |
| Observations | 793 | 1,110 | 379 | 463 |
| Higher than Minimum Wage | 0.519 | 0.674 | 0.464 | 0.543 |
| | (0.500) | (0.470) | (0.500) | (0.498) |
| Observations | 789 | 1,102 | 374 | 459 |

*Notes*: Table shows the mean, standard deviation, and the number of observations from the Individual Questionnaire of 2014-2015 Turkish Time Use Survey data. Column (1) and (2) report the results for males and column (4) and (5) displays the results for females. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. **Weekly Hours Worked* and *Higher than Minimum Wage* are calculated for employed individuals. Standard deviations are in the parenthesis.

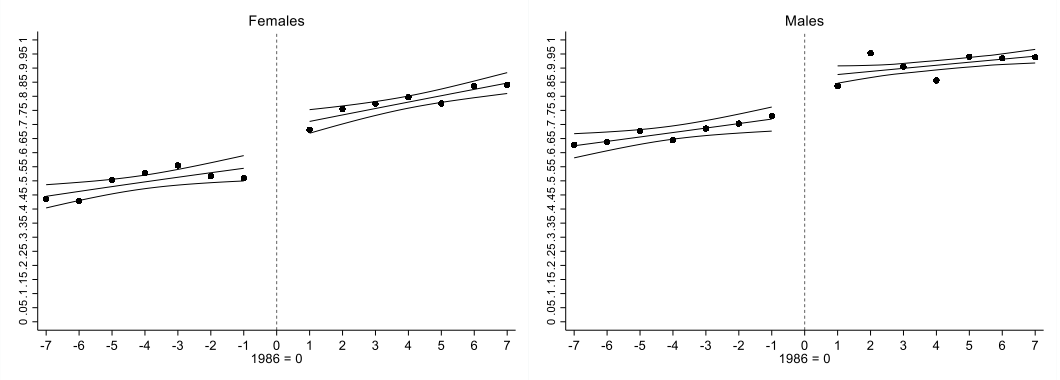


Figure 2. 1 Proportion of Individuals with At Least Middle School Diploma in 2014-2015 by Birth Cohorts 1979 to 1993

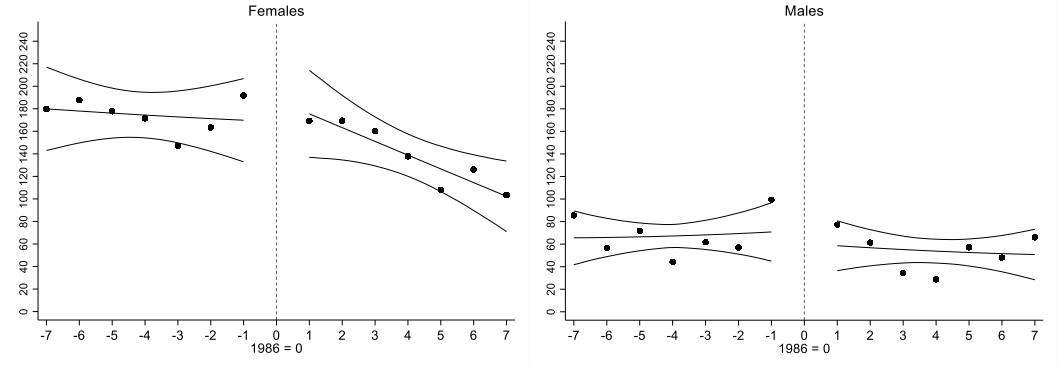


Figure 2. 2 Average Time Spent in Voluntary Activities by Birth Cohorts 1979-1993

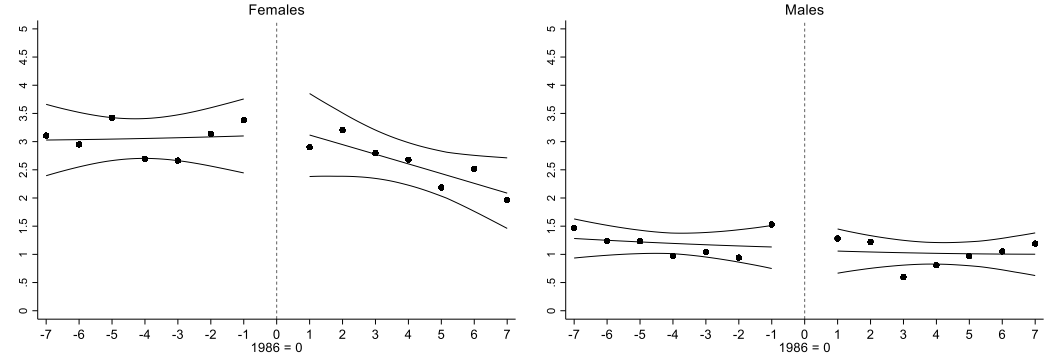


Figure 2. 3 Average Frequency of Voluntary Activities by Birth Cohorts 1979-1993

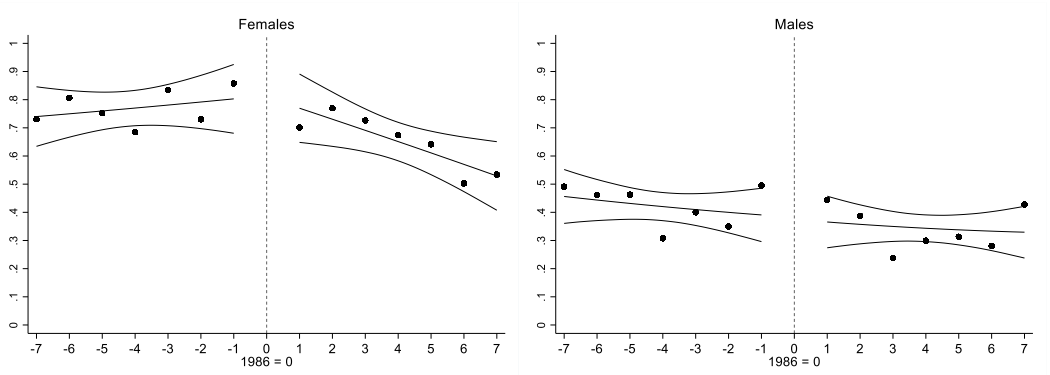


Figure 2. 4 Average Number of Voluntary Activities by Birth Cohorts 1979-1993

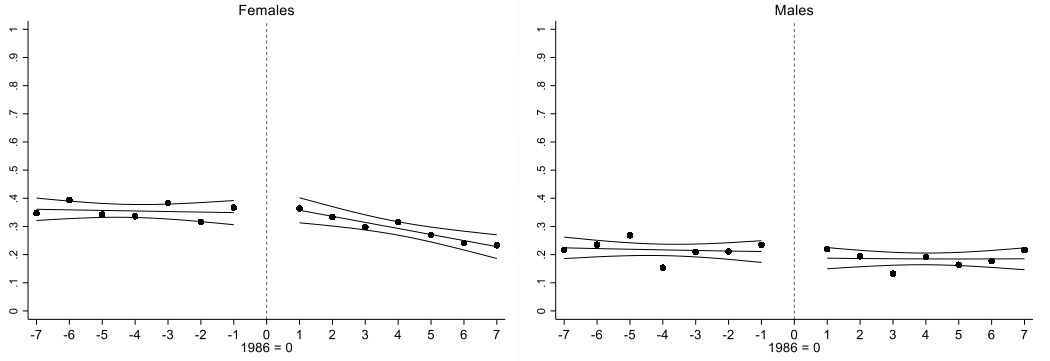


Figure 2. 5 Proportion of Individuals Participating in Voluntary Activities in 2014-2015 by Birth Cohorts 1979 to 1993

## 2.6.2. SCHOOLING OUTCOMES

Table 2.2. reports the effects of compulsory schooling reform on the educational attainment of individuals. The first stage results of IV regressions are derived from equation (2) and displayed in the Panel A and Panel B of Table 2.2., for the males and females, respectively. In addition to the results for our main analysis sample, the bandwidth 7, we also present the results for bandwidths 5, 6, 8 and 9. We find that the compulsory schooling reform significantly increases the educational attainment of individuals for all bandwidths. In particular, for our main analysis sample, the reform leads to an increase in the educational attainment by 12.9 ppt for males (column (3) of Panel A). Column (3) of Panel B shows that the education reform raises the propensity to complete at least eight years of schooling by 11.7 ppt for females. Depending on the estimation window, at least middle school completion rates increases by about 11.7 ppt to 14.4 ppt for females and 12.3 ppt to 15.2 ppt for males.

We also explore the impact of the compulsory schooling reform on being at least a high school graduate and a university graduate. However, as seen in the Table 2.3., we do not find any significant effects of the education reform on the likelihood of completing either high school or university for our main analysis sample.

Table 2. 2. The Effects of Compulsory Schooling Reform on the Probability of Completing at least Middle School

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Bandwidth | 9 | 8 | 7 | 6 | 5 |
| | | | Panel A: Males | | |
| Reform | 0.152*** | 0.148*** | 0.129*** | 0.123*** | 0.126*** |
| | (0.027) | (0.028) | (0.030) | (0.033) | (0.037) |
| Observations | 3,614 | 3,206 | 2,820 | 2,400 | 2,026 |
| R-squared | 0.128 | 0.125 | 0.12 | 0.119 | 0.11 |
| F-statistics | 31.946 | 27.860 | 18.843 | 14.005 | 11.372 |
| | | | Panel B: Females | | |
| Reform | 0.122*** | 0.121*** | 0.117*** | 0.117*** | 0.144*** |
| | (0.026) | (0.028) | (0.028) | (0.031) | (0.035) |
| Observations | 3,949 | 3,519 | 3,070 | 2,623 | 2,169 |
| R-squared | 0.190 | 0.178 | 0.167 | 0.158 | 0.15 |
| F-statistics | 21.227 | 18.410 | 17.243 | 14.759 | 16.997 |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 3. The Effects of Compulsory Schooling Reform on Post-Compulsory Schooling

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Bandwidth | 9 | 8 | 7 | 6 | 5 |
| | | | Panel A: Males | | |
| High School | 0.064* | 0.047 | 0.029 | 0.036 | 0.019 |
| | (0.037) | (0.040) | (0.043) | (0.046) | (0.049) |
| College | 0.081** | 0.054 | 0.029 | 0.023 | 0.014 |
| | (0.032) | (0.034) | (0.037) | (0.041) | (0.048) |
| Observations | 3,614 | 3,206 | 2,820 | 2,400 | 2,026 |
| | | | Panel B: Females | | |
| High School | 0.049 | 0.035 | 0.031 | 0.005 | -0.002 |
| | (0.033) | (0.035) | (0.037) | (0.042) | (0.046) |
| College | 0.100*** | 0.064* | 0.031 | 0.015 | 0.001 |
| | (0.032) | (0.033) | (0.035) | (0.038) | (0.042) |
| Observations | 3,949 | 3,519 | 3,070 | 2,623 | 2,169 |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

We also test the strength of the instrument and examine whether the F-statistic for the significance of the instrument in the first-stage exceeds 10 or not (Staiger & Stock,1997). Our findings indicate that the F-statistics of the first stage of IV regressions are 18.843 and 17.243, for males and females, respectively (see Table 2.2). These results conclude that our instrument, *Reform*, has enough power to explain the variation in the endogenous variable, *Middle School*.

### 2.6.3. VOLUNTARY WORK OUTCOMES

In this section, we investigate whether increased schooling, due to the change in the compulsory schooling reform, has a significant impact on voluntary work. We use the voluntary activity participation, number of voluntary activities, frequency of voluntary activities, and time spent in voluntary activities as our voluntary work outcomes. Table 2.4. reports the impact of education on voluntary work of individuals. We also report the effects of increased education on direct volunteering (*helping*) and organization-based volunteering in Table 2.5. and Table 2.6., respectively. Columns (5) of Panel A and B of Table 2.4-2.6 lists OLS estimates and column (6) of the Panel A and B of Table 2.4-2.6 presents IV estimates for our main sample of analysis with bandwidth 7. We first investigate the association between education and voluntary work of individuals. The OLS estimates in column (5) of the Panel A shows that for males, there is a significant positive correlation between education and voluntary work outcomes, which is consistent with existing OLS studies. Extending or narrowing the estimation window does not change our results (see column (1), (3), (5), and (9) of the Panel A). In order to provide causal evidence on the effects of education on time spent in voluntary activities, we report the

IV regression results in column (6) of Panel A. We find a negative but insignificant effect of education on voluntary work outcomes for males.[16]

For females, we do not find a positive association between education and voluntary work even in OLS estimations (see column 5 in Panel B). In IV estimations, we show that there is no significant causal effect of increased education level on time spent in voluntary activities for females (see column 6 in Panel B).

We next decompose total volunteering into two components and show results for direct volunteering (helping others) and organization-based volunteering in Tables 2.5. and 2.6., respectively. These results are similar to our results on total volunteering as we do not find a significant causal effect of education on either direct volunteering or organization-based volunteering.

As we mentioned before, we exclude those born in 1986 due to the fact that for this cohort, the exposure to the compulsory schooling reform is uncertain. To check the robustness of our results to the inclusion of 1986, we estimate our model by including the 1986 birth cohort (see columns (1) and (2) of Table 2.7.). We further assign the variable, *Reform*, to the value of 0.50 and 0.33 for the birth cohort 1986 (see column (3), (4) and column (5), (6), of Table 2.7.). As a result, we show that these modifications do not change our main results.

[16] Our results are in line with recent studies showing that the strong correlations between education and different outcome variables may exist due to endogeneity problems and documenting no causal impact of education (Yang, 2019; Avendano, de Coulon, & Nafilyan, 2020).

Table 2. 4. The Effects of Education on Voluntary Work (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Time Spent in Activities | 27.113*** | -66.445 | 29.766*** | -91.043 | 30.777*** | -100.431 | 25.647** | -149.606 | 23.133** | -133.616 |
| | (8.941) | (134.911) | (9.388) | (154.280) | (10.040) | (198.606) | (10.112) | (236.652) | (11.226) | (264.865) |
| Frequency of Activities | 0.512*** | -0.345 | 0.559*** | -0.546 | 0.618*** | -0.707 | 0.524*** | -1.042 | 0.499** | -0.835 |
| | (0.157) | (1.862) | (0.160) | (2.088) | (0.169) | (2.584) | (0.186) | (3.028) | (0.210) | (3.362) |
| Number of Activities | 0.161*** | -0.214 | 0.178*** | -0.129 | 0.184*** | -0.213 | 0.153*** | 0.001 | 0.140*** | -0.177 |
| | (0.039) | (0.474) | (0.039) | (0.510) | (0.041) | (0.655) | (0.043) | (0.766) | (0.046) | (0.850) |
| Participation | 0.054*** | -0.141 | 0.059*** | -0.150 | 0.061*** | -0.234 | 0.053*** | -0.108 | 0.046** | -0.102 |
| | (0.016) | (0.197) | (0.016) | (0.214) | (0.018) | (0.276) | (0.019) | (0.322) | (0.020) | (0.345) |
| Observations | 3,614 | | 3,206 | | 2,820 | | 2,400 | | 2,026 | |
| | | | | | Panel B: Females | | | | | |
| Time Spent in Activities | 6.810 | 76.208 | 3.227 | 19.067 | 2.658 | 89.633 | -4.141 | 69.718 | 2.099 | 95.914 |
| | (14.117) | (202.979) | (14.966) | (208.772) | (15.783) | (233.677) | (17.637) | (260.897) | (18.004) | (225.098) |
| Frequency of Activities | 0.143 | 0.026 | 0.060 | -0.312 | 0.055 | 0.230 | -0.113 | -1.111 | -0.006 | 0.976 |
| | (0.246) | (3.866) | (0.265) | (4.079) | (0.286) | (4.596) | (0.308) | (5.296) | (0.328) | (4.984) |
| Number of Activities | 0.081* | 0.061 | 0.076* | -0.212 | 0.065 | -0.283 | 0.038 | -0.391 | 0.079 | -0.698 |
| | (0.043) | (0.680) | (0.046) | (0.705) | (0.050) | (0.799) | (0.052) | (0.892) | (0.056) | (0.771) |
| Participation | 0.032** | 0.136 | 0.034* | -0.0001 | 0.031* | 0.160 | 0.025 | 0.183 | 0.038* | 0.057 |
| | (0.016) | (0.261) | (0.017) | (0.272) | (0.019) | (0.295) | (0.020) | (0.327) | (0.021) | (0.286) |
| Observations | 3,949 | | 3,519 | | 3,070 | | 2,623 | | 1,689 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 5. The Effects of Education on Direct Volunteering (Helping) (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Time Spent in Activities | 26.004*** | -59.996 | 28.270*** | -93.763 | 28.684*** | -104.449 | 24.059** | -153.637 | 22.013* | -135.458 |
| | (8.861) | (131.282) | (9.308) | (149.839) | (9.961) | (192.831) | -10.024 | (230.659) | (11.117) | (257.981) |
| Frequency of Activities | 0.497*** | -0.409 | 0.534*** | -0.780 | 0.583*** | -1.012 | 0.494*** | -1.550 | 0.494** | -1.054 |
| | (0.152) | (1.839) | (0.155) | (2.051) | (0.163) | (2.538) | (0.178) | (2.968) | (0.204) | (3.276) |
| Number of Activities | 0.151*** | -0.162 | 0.163*** | -0.147 | 0.165*** | -0.234 | 0.137*** | -0.085 | 0.136*** | -0.177 |
| | (0.036) | (0.441) | (0.036) | (0.467) | (0.037) | (0.596) | (0.038) | (0.692) | (0.041) | (0.775) |
| Participation | 0.056*** | -0.162 | 0.060*** | -0.211 | 0.061*** | -0.263 | 0.055*** | -0.163 | 0.055*** | -0.123 |
| | (0.015) | (0.182) | (0.015) | (0.200) | (0.016) | (0.256) | (0.017) | (0.300) | (0.018) | (0.324) |
| Observations | 3,614 | | 3,206 | | 2,820 | | 2,400 | | 2,026 | |
| | | | | | Panel B: Females | | | | | |
| Time Spent in Activities | 4.893 | 65.616 | 1.627 | 8.376 | 1.132 | 81.930 | -6.005 | 53.725 | -0.207 | 77.173 |
| | (14.173) | (201.872) | (15.016) | (207.317) | (15.798) | (232.823) | (17.634) | (259.768) | (18.082) | (224.494) |
| Frequency of Activities | 0.093 | -0.021 | 0.016 | -0.298 | 0.028 | 0.253 | -0.148 | -1.107 | -0.042 | 0.966 |
| | (0.247) | (3.831) | (0.265) | (4.046) | (0.287) | (4.566) | (0.308) | (5.272) | (0.329) | (4.968) |
| Number of Activities | 0.062 | 0.007 | 0.059 | -0.272 | 0.047 | -0.321 | 0.017 | -0.429 | 0.056 | -0.775 |
| | (0.043) | (0.671) | (0.045) | (0.693) | (0.049) | (0.784) | (0.051) | (0.878) | (0.055) | (0.765) |
| Participation | 0.021 | 0.200 | 0.023 | 0.040 | 0.019 | 0.210 | 0.010 | 0.253 | 0.023 | 0.040 |
| | (0.016) | (0.260) | (0.018) | (0.266) | (0.019) | (0.288) | (0.020) | (0.320) | (0.021) | (0.281) |
| Observations | 3,949 | | 3,519 | | 3,070 | | 2,623 | | 1,689 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 6. The Effects of Education on Organization-Based Volunteering (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Time Spent in Activities | 1.109 | -6.449 | 1.496** | 2.720 | 2.093*** | 4.018 | 1.588** | 4.031 | 1.120 | 1.843 |
| | (0.685) | -8.446 | (0.749) | -10.021 | (0.796) | -13.286 | (0.788) | -14.768 | (0.808) | -15.693 |
| Frequency of Activities | 0.015 | 0.064 | 0.025 | 0.234 | 0.035 | 0.305 | 0.030 | 0.508 | 0.005 | 0.219 |
| | (0.020) | (0.184) | (0.022) | (0.213) | (0.024) | (0.293) | (0.027) | (0.408) | (0.025) | (0.344) |
| Number of Activities | 0.010 | -0.053 | 0.015 | 0.018 | 0.019* | 0.021 | 0.017 | 0.086 | 0.004 | -0.000 |
| | (0.009) | (0.096) | (0.010) | (0.109) | (0.011) | (0.144) | (0.012) | (0.176) | (0.013) | (0.176) |
| Participation | 0.007 | -0.023 | 0.012 | 0.050 | 0.016 | 0.059 | 0.012 | 0.128 | 0.001 | 0.069 |
| | (0.008) | (0.085) | (0.009) | (0.096) | (0.010) | (0.129) | (0.011) | (0.157) | (0.012) | (0.149) |
| Observations | 3,614 | | 3,206 | | 2,820 | | 2,400 | | 2,026 | |
| | | | | | Panel B: Females | | | | | |
| Time Spent in Activities | 1.917*** | 10.593 | 1.599** | 10.691 | 1.526** | 7.704 | 1.864** | 15.994 | 2.306** | 18.741 |
| | (0.689) | (11.845) | (0.701) | (12.867) | (0.746) | (14.438) | (0.838) | (17.149) | (0.933) | (15.349) |
| Frequency of Activities | 0.050*** | 0.047 | 0.044** | -0.014 | 0.028** | -0.023 | 0.035*** | -0.004 | 0.035*** | 0.010 |
| | (0.019) | (0.249) | (0.020) | (0.344) | (0.011) | (0.179) | (0.012) | (0.210) | (0.013) | (0.191) |
| Number of Activities | 0.020*** | 0.054 | 0.018*** | 0.061 | 0.018*** | 0.038 | 0.021*** | 0.037 | 0.023*** | 0.077 |
| | (0.006) | (0.097) | (0.006) | (0.104) | (0.006) | (0.118) | (0.007) | (0.138) | (0.008) | (0.121) |
| Participation | 0.018*** | 0.046 | 0.016*** | 0.053 | 0.016*** | 0.033 | 0.018*** | 0.016 | 0.020*** | 0.052 |
| | (0.005) | (0.085) | (0.006) | (0.091) | (0.006) | (0.103) | (0.007) | (0.122) | (0.007) | (0.105) |
| Observations | 3,949 | | 3,519 | | 3,070 | | 2,623 | | 1,689 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Furthermore, we test whether our results are due to the differences in background characteristics between the treatment and control groups. Therefore, we investigate whether the marital status and the employment status affect the engagement in voluntary activities by adding the marital and employment status as control variables in our regressions. Note that considering the endogeneity problem, we do not include the control for marital and employment status in our main regressions. Column (10) of Table 2.7. shows that our results are not due to the differences in marital status or employment status between treatment and control groups.

We also use an alternative data set, the dairy part of the 2014-2015 Turkish Time Use Survey, to test the validity of our results. As shown in the Panel A of Table 2.8., similar to our main results, for males, the increased education has negative but insignificant impact on time spent in voluntary activities on weekdays as well as time spent in voluntary activities at weekend[17]. In particular, IV estimations change the sign of $\beta_1$ from positive to negative. With the help of the data on time spent in voluntary work, we also construct another outcome variable: *voluntary work participation*, a binary variable, that takes the value 1 if time spent in voluntary activities is higher than 0, and takes the value 0 if time spent in voluntary activities is equal to 0.

[17] We estimate time spent in voluntary work activities on weekdays (at weekend) by utilizing the data from diaries for a weekday and weekend, and summing up time spent in organizational work on weekdays (at weekend), time spent in informal help to other households on weekdays (at weekend), and participatory activities (except for religious activities) on weekdays (at weekend). We examine the impact of the education on time spent in religious activities on weekdays/at weekend in the next chapter as in Turkey, individuals may not consider religious activities as voluntary activities.

Table 2. 7. The Effects of Education on Voluntary Work: Alternative Specifications 1 (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Including 1986 birth cohort | | Alternative Reform Specification 1 | | Alternative Reform Specification 2 | | Including Employment Status and Marital Status as a Control Variable | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | Panel A: Males | | | | |
| Time Spent in Activities | 32.732*** | -231.458 | 32.842*** | -99.424 | 32.842*** | -120.900 | 34.524*** | -87.775 |
| | (9.527) | (222.433) | (9.525) | (196.959) | (9.525) | (188.849) | (10.501) | (202.684) |
| Frequency of Activities | 0.651*** | -3.530 | 0.652*** | -0.627 | 0.652*** | -1.310 | 0.652*** | -0.592 |
| | (0.161) | (3.384) | (0.161) | (2.565) | (0.161) | (2.600) | (0.172) | (2.618) |
| Number of Activities | 0.189*** | -1.072 | 0.189*** | -0.199 | 0.189*** | -0.406 | 0.210*** | -0.129 |
| | (0.039) | (0.868) | (0.039) | (0.645) | (0.039) | (0.641) | (0.042) | (0.668) |
| Participation | 0.060*** | -0.354 | 0.060*** | -0.241 | 0.060*** | -0.262 | 0.075*** | -0.203 |
| | (0.017) | (0.316) | (0.017) | (0.276) | (0.017) | (0.272) | (0.018) | (0.280) |
| Observations | | | | 3,019 | | | | 2,820 |
| | | | | Panel B: Females | | | | |
| Time Spent in Activities | -2.162 | -75.085 | -1.849 | 104.220 | -1.849 | 82.286 | 23.236 | 76.613 |
| | (15.137) | (272.613) | (15.135) | (236.646) | (15.135) | (240.198) | (16.479) | (226.176) |
| Frequency of Activities | 0.010 | -1.259 | 0.015 | 0.358 | 0.015 | 0.412 | 0.418 | -0.023 |
| | (0.272) | (4.954) | (0.272) | (4.621) | (0.272) | (4.786) | (0.298) | (4.403) |
| Number of Activities | 0.054 | -0.572 | 0.055 | -0.276 | 0.055 | -0.238 | 0.130** | -0.340 |
| | (0.049) | (0.923) | (0.049) | (0.808) | (0.049) | (0.840) | (0.052) | (0.770) |
| Participation | 0.022 | 0.011 | 0.022 | 0.171 | 0.022 | 0.179 | 0.055*** | 0.136 |
| | (0.018) | (0.342) | (0.018) | (0.299) | (0.018) | (0.316) | (0.019) | (0.282) |
| Observations | | | | 3,266 | | | | 3,070 |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source:* The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 8. The Effects of Education on Voluntary Work: Alternative Outcome Measures 1 (in min. per day)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Bandwidth | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Time Spent on Weekdays | 1.844 | -0.494 | 1.666 | -11.120 | 2.334* | -16.598 | 1.885 | -29.318* | 2.599* | -28.643 |
| | (1.194) | (12.011) | (1.184) | (12.222) | (1.226) | (15.047) | (1.293) | (17.547) | (1.460) | (21.956) |
| Time Spent at Weekend | 2.460* | -18.718 | 2.585* | -14.000 | 2.983** | -23.664 | 2.037 | -26.264 | 3.142* | -16.983 |
| | (1.339) | (14.158) | (1.460) | (15.847) | (1.435) | (20.890) | (1.578) | (22.001) | -1.673 | (23.616) |
| Participation on Weekdays | 0.033*** | -0.038 | 0.033*** | -0.093 | 0.032** | -0.132 | 0.039*** | -0.213 | 0.039*** | -0.229 |
| | (0.011) | (0.116) | (0.012) | (0.131) | (0.012) | (0.162) | (0.013) | (0.190) | (0.014) | (0.203) |
| Participation at Weekend | 0.026*** | -0.166 | 0.029*** | -0.142 | 0.028*** | -0.228 | 0.029** | -0.317* | 0.034*** | -0.298 |
| | (0.010) | (0.115) | (0.010) | (0.128) | (0.010) | (0.162) | (0.012) | (0.191) | (0.013) | (0.208) |
| Observations | 3,614 | | 3,206 | | 2,820 | | 2,400 | | 2,026 | |
| | | | | | Panel B: Females | | | | | |
| Time Spent on Weekdays | 0.810 | -3.360 | 0.718 | 5.514 | 0.661 | -13.927 | 0.759 | 7.209 | 0.975 | 3.323 |
| | (0.949) | (19.716 | (0.947) | (18.613) | (0.872) | (21.141) | (0.995) | (20.926) | (1.114) | (19.139) |
| Time Spent at Weekend | 1.341 | -5.442 | 1.075 | -2.577 | 0.839 | -14.951 | 0.698 | -16.677 | -0.217 | -34.974 |
| | (1.305) | (18.985) | (1.418) | (20.550) | (1.544) | (23.658) | (1.793) | (27.009) | (1.892) | (26.618) |
| Participation on Weekdays | (0.002) | -0.090 | -0.001 | -0.016 | -0.0001 | -0.111 | 0.001 | 0.014 | 0.003 | 0.011 |
| | (0.008) | (0.113) | (0.008) | (0.114) | (0.008) | (0.133) | (0.009) | (0.134) | (0.010) | (0.134) |
| Participation at Weekend | 0.003 | -0.128 | 0.001 | -0.084 | 0.001 | -0.179 | -0.003 | -0.230 | -0.004 | -0.291 |
| | (0.010) | (0.148) | (0.011) | (0.162) | (0.012) | (0.185) | (0.014) | (0.202) | (0.015) | (0.188) |
| Observations | 3,949 | | 3,519 | | 3,070 | | 2,623 | | 1,689 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: The Diary Part of 2014-2015 Turkish Time Use Survey, TURKSTAT

We present these results in the last two rows of Panel A and show that they are also consistent with our main findings. Similarly, for females, we do not observe any effect of education on voluntary work (See, the Panel B of Table 2.8.).

Overall, the causal effects of increased education, due to change in the compulsory schooling reform, on the voluntary activity participation, the number of voluntary activities, the frequency of voluntary activities, and time spent in voluntary activities is negative but statistically insignificant for all estimation intervals in the male sample. Our results are consistent with Gibson (2001), which documents that an additional year of schooling reduces individuals' probability of volunteering and time spent in volunteering by one hour per week. Moreover, we do not find any significant impact of increased schooling on female voluntary work. Our results suggest that OLS studies that find a positive and significant association between education and voluntary work have an upward bias due to omitted variable and/or reverse causality problems.

### 2.6.4. LABOR MARKET OUTCOMES

This section examines the effects of the change in the compulsory schooling law on labor market outcomes to examine whether results on labor market outcomes are consistent with our results on volunteering behavior. Table 2.9. presents the impact of education on labor market outcomes at different bandwidths. For our main analysis sample, column (5) of Panel A and B list OLS estimates for males and females, respectively. Column (6) of Panel A and B report IV estimates for males and females, respectively.

We first check whether increased education affects workers' earnings. We document that there is a positive and significant correlation between education and the probability of earning higher than minimum wages for individuals (column (5) of the Panel A and B, respectively). We also find that increased education of compliers increases the probability that males will earn higher than minimum wage (column (6) of Table 2.9.) in IV estimation.

According to theory of labor supply, when wage increases, the opportunity cost of leisure increases and the substitution effect implies that individuals will substitute work for leisure. Voluntary work is a part of leisure activities. Hence, according to the substitution effect, we expect individuals to decrease voluntary work and increase paid work hours when their wage increase. However, an increase in wage may also have an income effect. Individuals increase both consumption and leisure assuming that leisure is a normal good. If voluntary work is a normal good, individuals will increase hours spent on voluntary work. However, if voluntary work is an inferior good, then individuals might decrease voluntary work hours, and instead increase consumption on goods and services that can be substitute to goods and services that voluntary work help produce. For example, individuals may increase money donations to charities, family, and friends. Or they may hire service providers to help friends and family with their needs. Unfortunately, we do not observe donations and consumption of goods and services. However, we do observe time spent on work hours.

In our estimations on voluntary work, we found that education did not significantly affect hours spent on volunteering. Here, we examine whether education has any effect on

probability of employment and work hours for employed individuals. If we find that increased education levels do not increase probability of employment and work hours, we can conclude that the substitution effect of wage increase has been limited. This would complement our results that education does not have a significant causal impact on volunteer work.

We first investigate the impact of education on employment. OLS estimates in the column (5) of Table 2.9. indicate a positive relationship between education and employment. However, we find no causal evidence of a significant change caused by the education reform. This result is consistent with earlier studies that use the same education reform to examine the effect of education on labor market outcomes in Turkey (see Aydemir & Kirdar, 2017; Torun, 2018). We further examine whether the increased years of schooling due to the change in the education reform increases workers' hours using the sample of employed individuals.[18] In this estimation, we do not observe any significant impact of education on individuals' weekly work (Table 2.9., column 6). Hence, although education seems to have a causal impact on the probability of earning higher than minimum wage, it does not have any effect on work hours. These results suggest that the substitution effect of the wage increase has been limited.

It is possible that the income effect of the wage increase might have affected the composition of leisure hours. Volunteering hours may increase if volunteering is a normal good, stay constant if volunteering is an income neutral good and decrease if volunteering

[18] Note that if the respondent indicates that he/she is temporarily unemployed, we consider his/her weekly working hours as 0. Hours worked at side job is not included in hours worked at main job.

is an inferior good. Hence, we examine the effects of education on voluntary work in the subsample of employed individuals to determine whether there is an income effect of the wage increase on volunteer hours. Table 2.10. displays the results. As seen from column (5) of Table 2.10., there is a positive correlation between education and voluntary work outcomes for the employed male sample. However, IV estimates in column (6) of Table 2.10. indicate that the completion of at least middle school as a result of the change in the compulsory schooling reform does not have any effect on voluntary work outcomes consistent with our main results. Increased education due to reform had no causal effect on female labor market outcomes. We also find no evidence for a causal impact of education on female volunteer hours.

Table 2. 9. The Effects of Education on Labor Market Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Bandwidth | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Minimum Wage | 0.278*** | 0.638** | 0.285*** | 0.845** | 0.301*** | 0.888** | 0.310*** | 0.897* | 0.303*** | 0.523 |
| | (0.024) | (0.293) | (0.027) | (0.367) | (0.028) | (0.392) | (0.031) | (0.519) | (0.033) | (0.517) |
| Observations | 2,284 | 2,284 | 2,078 | 2,078 | 1,865 | 1,865 | 1,611 | 1,611 | 1,394 | 1,394 |
| Employment | 0.042*** | -0.026 | 0.040** | -0.120 | 0.044** | -0.287 | 0.055*** | -0.450* | 0.058*** | -0.626** |
| | (0.016) | (0.178) | (0.016) | (0.193) | (0.017) | (0.244) | (0.018) | (0.258) | (0.020) | (0.303) |
| Observations | 3,614 | 3,614 | 3,206 | 3,206 | 2,820 | 2,820 | 2,400 | 2,400 | 2,026 | 2,026 |
| Weekly Hours Worked | -3.474*** | 9.492 | -3.206*** | 10.857 | -3.612*** | 18.185 | -3.221*** | 19.054 | -3.615*** | 28.776 |
| | (0.678) | (9.259 | (0.741) | (10.789) | (0.799) | (12.190) | (0.907) | (17.012) | (0.971) | (20.575) |
| Observations | 2,286 | 2,286 | 2,080 | 2,080 | 1,867 | 1,867 | 1,613 | 1,613 | 1,395 | 1,395 |
| | | | | | Panel B: Females | | | | | |
| Minimum Wage | 0.533*** | 0.231 | 0.524*** | 0.139 | 0.528*** | 0.217 | 0.529*** | 0.023 | 0.557*** | 0.274 |
| | (0.028) | (0.564) | (0.030) | (0.509) | (0.034) | (0.500) | (0.037) | (0.608) | (0.042) | (0.461) |
| Observations | 1,024 | 1,024 | 921 | 921 | 804 | 804 | 698 | 698 | 589 | 589 |
| Employment | 0.177*** | 0.332 | 0.176*** | 0.120 | 0.185*** | 0.140 | 0.192*** | 0.281 | 0.196*** | 0.403 |
| | (0.016) | (0.277) | (0.018) | (0.291) | (0.018) | (0.320) | (0.020) | (0.353) | (0.022) | (0.321) |
| Observations | 3,949 | 3,949 | 3,519 | 3,519 | 3,070 | 3,070 | 2,623 | 2,623 | 2,169 | 2,169 |
| Weekly Hours Worked | 1.253 | 6.017 | 0.388 | 4.367 | 0.982 | -1.061 | 1.326 | 5.202 | 1.289 | -0.163 |
| | (1.299) | (16.395) | (1.366) | (13.684) | (1.546) | (13.039) | (1.688) | (14.814) | (1.884) | (11.886) |
| Observations | 1,024 | 1,024 | 921 | 921 | 804 | 804 | 698 | 698 | 589 | 589 |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 10. The Effects of Education on Voluntary Work (for employed individuals) (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Bandwidth | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Time Spent in Activities | 23.594** | 5.355 | 25.706** | -33.996 | 27.907** | -32.929 | 22.182* | -68.354 | 20.017 | -27.570 |
| | (10.082) | (148.444) | (10.688) | (179.311) | (11.489) | (220.228) | (11.381) | (281.703) | (12.537) | (296.450) |
| Frequency of Activities | 0.437** | 0.844 | 0.467*** | 0.614 | 0.548*** | 1.012 | 0.446** | 1.214 | 0.478** | 1.483 |
| | (0.169) | (2.066) | (0.176) | (2.512) | (0.186) | (2.940) | (0.206) | (3.655) | (0.239) | (3.803) |
| Number of Activities | 0.139*** | 0.090 | 0.152*** | 0.130 | 0.163*** | 0.219 | 0.130*** | 0.386 | 0.130** | 0.396 |
| | (0.041) | (0.508) | (0.041) | (0.601) | (0.043) | (0.729) | (0.045) | (0.937) | (0.050) | (0.957) |
| Participation | 0.044** | 0.002 | 0.047*** | -0.009 | 0.050*** | 0.005 | 0.043** | 0.091 | 0.038* | 0.139 |
| | (0.017) | (0.209) | (0.017) | (0.239) | (0.018) | (0.287) | (0.019) | (0.375) | (0.021) | (0.374) |
| Observations | 2,890 | | 2,620 | | 2,339 | | 2,016 | | 1,724 | |
| | | | | | Panel B: Females | | | | | |
| Time Spent in Activities | -25.730 | -288.574 | -27.534 | -374.033 | -40.719 | -165.969 | -40.686 | -205.825 | -22.931 | -350.310 |
| | (24.889) | (394.467) | (24.193) | (414.442) | (26.261) | (384.031) | (29.968) | (467.680) | (32.281) | (480.304) |
| Frequency of Activities | -0.259 | -9.497 | -0.305 | -10.666 | -0.524 | -9.201 | -0.709 | -9.651 | -0.481 | -11.864 |
| | (0.546) | (9.157) | (0.577) | (9.805) | (0.638) | (9.512) | (0.747) | (11.625) | (0.858) | (12.179) |
| Number of Activities | 0.010 | -0.797 | 0.008 | -0.651 | -0.052 | 0.095 | -0.048 | 0.467 | 0.029 | -0.555 |
| | (0.086) | (1.454) | (0.092) | (1.464) | (0.099) | (1.358) | (0.115) | -1.605 | (0.126) | -1.443 |
| Participation | 0.0001 | -0.145 | 0.011 | -0.056 | -0.004 | 0.301 | -0.007 | 0.343 | 0.014 | 0.130 |
| | (0.029) | (0.480) | (0.031) | (0.480) | (0.032) | (0.474) | (0.036) | (0.572) | (0.039) | (0.488) |
| Observations | 1,309 | | 1,177 | | 1,030 | | 881 | | 734 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

## 2.7. ROBUSTNESS CHECKS

In this section, we conduct several additional analyses to test the robustness of our results. First, we make alternative model specifications to test the validity of our model design. For instance, we implement an alternative method for correcting standard errors. Columns (1) and (2) of Table 2.11. present the results for which we cluster standard errors at the birth cohort level by the survey year. We estimate the p-values using wild cluster bootstrap as clustering the standard errors at the birth year by the survey year generates 28 clusters that is not large enough to get accurate inference (Cameron, Gelbach, & Miller, 2008). As a result, we show that this specification does not change our main results.

We next include a control variable, *school enrolment status*, a dummy variable that takes the value 1 if individuals are enrolled in the school when the survey was conducted, 0 otherwise in our regressions. On the one hand, the school enrollment of individuals may lead to an increase in the engagement in volunteering, as they may have more opportunity to the participate in the range of voluntary activities. On the other hand, the school curriculum may have a burden on students that results in decreasing the engagement of voluntary activities. Note that the age of individuals in our main sample ranges from 21 to 36, therefore those who are enrolled in a school are enrolled in a university or above. As a result, including the school enrolment status as a control variable does not change our results (see columns (3) and (4) of Table 2.11., for OLS and IV results, respectively).

As in Freeman (1997) and Gibson (2001), we add the number of children aged between 0-9 in household and the number of household members as control variables in our

regressions to assess whether these variables have any impact on voluntary work. We do not include the number of children and the number of household members in our main regressions due to the endogeneity problem, as higher educated individuals are more likely to have a smaller household size with fewer children. Columns (5) and (6) of Table 2.11. present the results derived from the regressions with control variables, the number of children in household and number of household members, and results are in line with our main OLS and IV estimates.

We then use alternative identification strategies, namely *IV-Tobit* and *Reduced Form*, to check the validity of our main identification method discussed in section 5. The results derived from the Reduced Form Regressions show that there is a negative but insignificant intent to treat effects of the compulsory schooling reform on voluntary activity outcomes. To account for too many zeros in the data, we also run Tobit and IV-Tobit regressions and report the results derived from these regressions in column (7) and column (8) of Table 2.11., respectively. The results derived from Tobit and IV-Tobit confirm the robustness of our findings.

We next generate three alternative voluntary activity outcome measures: *i*) voluntary activity number index, *ii*) voluntary activity frequency index, and *iii*) voluntary activity time index. We construct these variables by *averaging z-scores of thirty-two variables corresponding to each outcome measure.* The results presented in Table 2.12. confirm our main findings.

As we discussed in the data section, we perform outlier analysis on our data. To test the validity of our results, we also include outliers and run our OLS and IV models. We find a negative and statistically significant causal impact of education on time spent in voluntary activities when these outliers are included in the estimation. Adding them to our main analysis sample results in large and implausible negative causal effect of education on voluntary work. However, once again positive association of education with voluntary work found in OLS estimations disappears when instrumental variables method is used to addressed unobserved heterogeneity at the individual level.[19]

[19] We find that the completion of at least middle school significantly reduce time spent in voluntary activities by 779 minutes over four weeks in the male sample, while the average time spent in voluntary activities is nearly 100 minutes per four weeks. Detailed results are available in the previous version of this chapter circulated under the title of "Education and Prosocial Behavior: Evidence from Time Use Survey".

Table 2. 11. The Effects of Education on Voluntary Work: Alternative Specifications 2 (in min. per month)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Clustered at the birth year by the survey year | | Including the school enrolment status as a control variable | | Including the number of children and number of household members as control variables | | Alternative Model Specifications: IV TOBIT Regressions. Average Marginal Effects | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | Panel A: Males | | | | |
| Time Spent in Activities | 30.777*** | -100.431 | 28.050*** | -96.577 | 31.906*** | -81.871 | 30.656*** | 31.412 |
| | (9.898) | (189.403) | (10.234) | (196.160) | (9.865) | (231.485) | (9.083) | (137.017) |
| Frequency of Activities | 0.618*** | -0.707 | 0.583*** | -0.661 | 0.605*** | -0.409 | 0.561*** | 0.573 |
| | (0.143) | (3.015) | (0.174) | (2.549) | (0.173) | (2.958) | (0.153) | (2.103) |
| Number of Activities | 0.184*** | -0.213 | 0.172*** | -0.198 | 0.198*** | 0.001 | 0.166*** | 0.170 |
| | (0.036) | (0.701) | (0.042) | (0.644) | (0.041) | (0.766) | (0.043) | (0.609) |
| Participation | 0.061*** | -0.234 | 0.056*** | -0.227 | 0.066*** | -0.156 | 0.067*** | 0.068 |
| | (0.016) | (0.242) | (0.018) | (0.271) | (0.018) | (0.323) | (0.012) | (0.290) |
| Observations | | | | 2,820 | | | | |
| | | | | Panel B: Females | | | | |
| Time Spent in Activities | 2.658 | 89.633 | -4.882 | 102.002 | -9.531 | 87.481 | 14.670 | 14.249 |
| | (13.808) | (154.569) | (15.389) | (210.048) | (16.624) | (259.697) | (12.948) | (197.165) |
| Frequency of Activities | 0.055 | 0.230 | -0.006 | 0.383 | -0.173 | 0.032 | 0.267 | 0.080 |
| | (0.233) | (3.823) | (0.282) | (4.133) | (0.295) | (5.037) | (0.236) | (0.766) |
| Number of Activities | 0.065 | -0.283 | 0.048 | -0.202 | 0.027 | -0.357 | 0.080* | 0.080 |
| | (0.047) | (0.686) | (0.050) | (0.707) | (0.052) | (0.878) | (0.049) | (0.766) |
| Participation | 0.031 | 0.160 | 0.025 | 0.165 | 0.012 | 0.157 | 0.035* | 0.344 |
| | (0.018) | (0.245) | (0.019) | (0.266) | (0.019) | (0.326) | (0.020) | (0.319) |
| Observations | | | | 3,070 | | | | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 2. 12. The Effects of Education on Voluntary Work: Alternative Outcome Measures 2

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Bandwidth | 9 | | 8 | | 7 | | 6 | | 5 | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| | | | | | Panel A: Males | | | | | |
| Voluntary Activity Time Index | 0.018*** | -0.020 | 0.019*** | -0.025 | 0.019*** | -0.030 | 0.016*** | -0.016 | 0.016** | -0.013 |
| | (0.005) | (0.092) | (0.005) | (0.103) | (0.006) | (0.129) | (0.006) | (0.157) | (0.007) | (0.188) |
| Voluntary Activity Frequency Index | 0.026*** | -0.021 | 0.031*** | -0.011 | 0.034*** | 0.003 | 0.027*** | -0.005 | 0.027*** | 0.026 |
| | (0.008) | (0.084) | (0.008) | (0.094) | (0.009) | (0.121) | (0.009) | (0.135) | (0.010) | (0.152) |
| Voluntary Activity Number Index | 0.078*** | -0.194 | 0.089*** | -0.052 | 0.097*** | -0.076 | 0.080*** | 0.084 | 0.066** | -0.012 |
| | (0.020) | (0.266) | (0.021) | (0.295) | (0.022) | (0.381) | (0.024) | (0.463) | (0.026) | (0.520) |
| Observations | 3,614 | | 3,206 | | 2,820 | | 2,400 | | 2,026 | |
| | | | | | Panel B: Females | | | | | |
| Voluntary Activity Time Index | 0.003 | 0.016 | 0.002 | -0.005 | 0.002 | -0.003 | 0.001 | 0.009 | 0.004 | 0.009 |
| | (0.004) | (0.056) | (0.004) | (0.060) | (0.004) | (0.065) | (0.005) | (0.070) | (0.005) | (0.059) |
| Voluntary Activity Frequency Index | 0.009 | -0.003 | 0.006 | -0.013 | 0.006 | -0.017 | 0.003 | -0.037 | 0.007 | -0.022 |
| | (0.005) | (0.081) | (0.006) | (0.088) | (0.006) | (0.094) | (0.006) | (0.105) | (0.007) | (0.094) |
| Voluntary Activity Number Index | 0.047*** | -0.060 | 0.042** | -0.109 | 0.040** | -0.194 | 0.033* | -0.169 | 0.052** | -0.171 |
| | (0.016) | (0.252) | (0.016) | (0.274) | (0.018) | (0.319) | (0.019) | (0.357) | (0.020) | (0.298) |
| Observations | 3,949 | | 3,519 | | 3,070 | | 2,623 | | 1,689 | |

*Notes:* The main sample consists of individuals born between 1979 and 1993 (Bandwidth 7), where the corresponding age ranges from 21 to 36. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the region of residence fixed effects, birth $year_{tre}$, and birth $year_{cont}$. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.

*Source*: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

## 2.8. CONCLUSION

In this thesis, we investigated the causal effect of education on voluntary work by utilizing the extension in compulsory schooling from five to eight years in Turkey as an instrument for education and using the nationally representative Turkish Time Use Survey. Our results suggest that a strong correlation between educational attainment and voluntary work found in OLS studies in existing literature may result from unobserved heterogeneity at the individual level. Consistent with existing OLS studies, there is a significant positive correlation between schooling and males' voluntary work. However, we find that increased education induced by the compulsory schooling reform has a negative but insignificant causal impact on voluntary work of males. As a result, we argue that OLS may produce upward bias estimates as omitted variables such as family background and social values may impact both education and voluntary behavior. We do not find any significant effects of education on female voluntary work either in OLS or IV estimations.

The volunteer sector is a considerable economic force in the world (OECD, 2015; Salamon et al. 2018). In addition to its economic significance, volunteering performs important social functions by promoting social integration, civic participation and sentiments of altruism. Our findings have important consequences for the volunteering sector as we document that volunteering may not be an external benefit of education. Policies extending the compulsory schooling may not be an effective policy instrument to raise voluntary work of individuals; although it increases the educational attainment of individuals and has an impact on different outcome variables such as drop-out decisions (Oreopoulos, 2006; Oreopoulos, 2007; Caner, Guven, Okten and Sakalli, 2016), civic and

political behavior outcomes (Dee, 2004; Milligan, Moretti and Oreopoulos, 2004; Cesur and Mocan, 2018), and health outcomes (Cesur, Dursun, and Mocan, 2018; Kırdar, Dayıoğlu, and Koç, 2018)."

# CHAPTER 3

# EDUCATION AND RELIGIOSITY

## 3.1. INTRODUCTION

This chapter is a joint work with Cagla Okten and published in Applied Economics Letters on April 2021 with the following reference: Betul Akar & Cagla Okten (2021) The impact of education on time spent in religious activities: evidence from Turkey, Applied Economics Letters, DOI:10.1080/13504851.2021.1914316

"A growing number of papers in the economics literature establish a negative causal link between education and religiosity. For instance, Arias -Vazquez (2012) provides negative causal evidence for the US; Hungerman (2014) and Dilmaghani (2019) for Canada; Mocan and Pogorelova (2017) for European countries. On the other hand, there is also a number of studies investigating the impact of schooling on religiosity for the non-western part of the world, particularly for Islamic countries. Gulesci and Meyersson (2014) show that education reduces the likelihood of wearing headscarf and attending Quran study for

ever-married women by exploiting a change in compulsory schooling law in Turkey, where the great majority of the population are Muslims. A long the same line, Cesur and Mocan (2018) document that increased education decreases the probability of women's identification themselves as religious, wearing a headscarf, and voting for Islamic parties by utilizing the same education reform but using different data set. More recently, Masuada and Yudhistira (2020) implement the education reform in Indonesia to generate an exogenous variation in years of schooling, and show that an additional year of schooling leads to a decrease in self-reported religiousness, the number of times individuals pray, and the probability of eating halal food.

Previous studies examine the impact of education on either religious identity or religious activity participation and frequency. We contribute to the existing literature by providing the first causal evidence on the impact of education on the religiosity of individuals at the intensive margin, time spent in religious activities. Utilizing the Turkish Time Use Survey data, we employ the Instrumental Variable method by using the extension of compulsory education from five to eight years in Turkey as an instrument for education. Our results are in line with the previous studies which show that education reduce the religiosity of individuals.

## 3.2. DATA

We utilize the data from Turkish Time Use Survey (TUS), a nationally representative survey and a part of Harmonized European Time Use Study (HETUS). The TUS was conducted among 11,440 households between 1 August 2014-31 July 2015. In our

analysis, we combine two data set from the Diaries and the Individual Questionnaire of the TUS. The diaries gather the data about how much time individuals allocate to daily activities for a weekday and for a weekend. The individual questionnaire collects data on education and labor market outcomes of individuals as well as demographics (age, gender, the region of residence, country of birth).

We restrict the sample to those born between 1980 and 1992 (*aged 22-35*) and exclude the 1986 birth cohort. Here, we should also mention that including the 1986 birth cohort does not alter our main findings. We use the individuals' year of birth to find out those who are exposed to the education reform and those who are not. Thus, we construct our instrumental variable, *Reform*, by generating a binary variable that takes the value 1 if individuals born after 1986, and 0 otherwise. With the help of the data on the education status, we next generate a binary variable, *Educ*, which is equal to 1 if individuals obtain at least middle school degree, and 0 otherwise. Then, we measure the religious behavior of individuals by constructing two religious outcome variables: *time spent in religious activities on weekdays* and *time spent in religious activities at weekend*. Religious activities include activities such as the five daily prayers, praying, reading Quran, going to a mosque, and attending Islamic ceremony.

In our main analysis sample, the average completion rate of at least a middle school is 77 (91) percent for women (men) born after 1986; while the corresponding rate is 50 (68) percent for women (men) born before 1986 (see Figure 3.1). Moreover, as shown in Figure 3.2, the average time spent in religious activities for women (men) who are exposed to the reform is about 21 (13) minutes per a weekday. On the other hand, the average time spent

on weekdays is nearly 28 (18) minutes per a weekday for women (men) who are not exposed to the reform. Regarding to time spent in religious activities at weekend, on average, women (men) who are exposed to the reform spend approximately 20 (14) minutes per a weekend day, whereas among those who are not exposed to the reform, the average time allocated to religious activities is 25 (18) minutes per a weekend day (see Figure 3.3.).

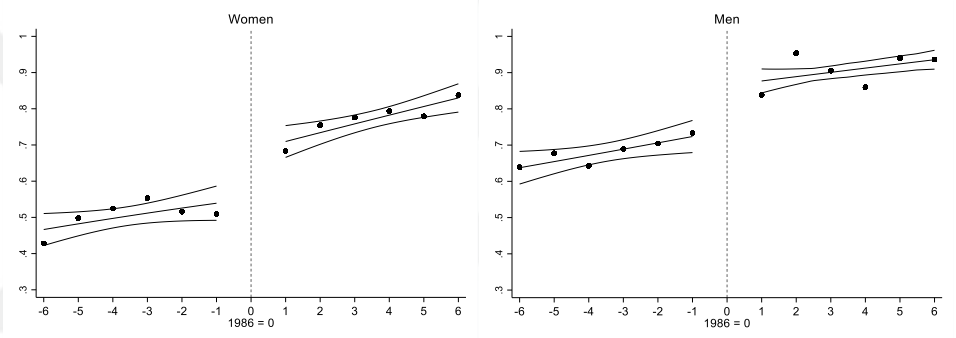


Figure 3. 1 Proportion of Individuals with at Least Middle School Degree by Birth Cohorts 1980-1992

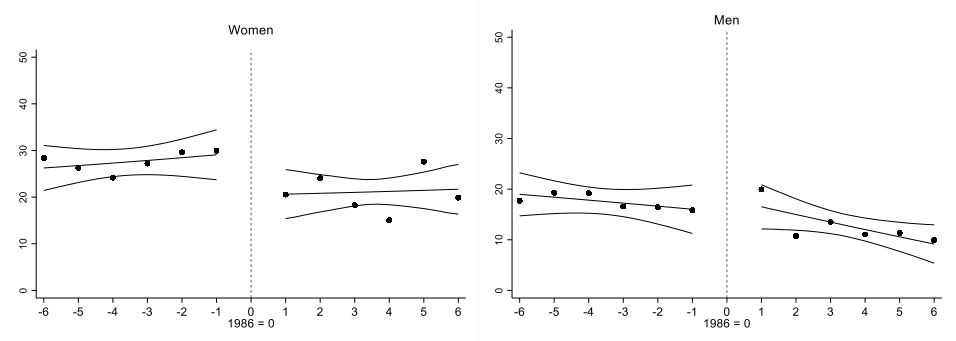


Figure 3. 2 Time Spent in Religious Activities on Weekdays (in min. per day) by Birth Cohorts 1980-1992

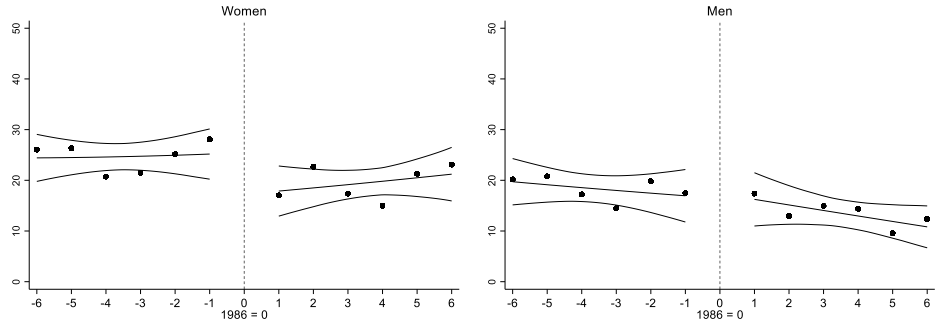


Figure 3. 3 Time Spent in Religious Activities at Weekend (in min. per day) by Birth Cohorts 1980-1992

## 3.3. EMPRICAL SPECIFICATION

We first examine the impact of education on religious behavior of individuals by running the following ordinary least squares (OLS) regressions

$$R_i = \beta_0 + \beta_1 Educ_i + \beta_2 X_i + \varepsilon_i, \quad (1)$$

where $R_i$ represents our two religious outcome variables: *time spent in religious activities on weekdays, time spent in religious activities at weekend.* $X_i$ is the vector of control variables containing re-centered birth year for the treatment group and the control group, the survey month fixed effects, the survey year fixed effects, and the region fixed effects. We also capture the impact of Islamic holy day, *Friday*, on time spent in religious activities. We, therefore, add the dummy variable, *Friday*, that takes value 1 if the day of survey week is Friday, and 0 otherwise as a control variable. We cluster standard errors at the region by the birth-year level.

We then employ the Instrumental Variable (IV) method by using the exposure to the exogenous education reform, as an instrument for schooling, because the unobservable factors may lead to a biased estimate of $\beta_1$. We estimate the causal impact of education on time spent in religious activities by running the following two equations:

$$Educ_i = \gamma_0 + \gamma_1 Reform_i + \gamma_2 X_i + \mu_i \quad (2)$$

$$R_i = \varphi_0 + \varphi_1 \widehat{Educ_i} + \varphi_2 X_i + \omega_i \quad (3)$$

Equation (2), the first stage of IV regressions, estimates the effects of the education reform on the likelihood of completion of at least a middle school (completing at least eight years of schooling). Equation (3) shows the causal impact of education on time spent in religious activities.

## 3.4. RESULTS

We first present the impact of the education reform on the probability of having at least a middle school diploma in Table 3.1. Our findings indicate that the reform significantly increases the likelihood of completing at least middle school by 12.1 ppt. for both women and men (column (1) and column (2), respectively). The first stage F-statistics also implies that our instrumental variable, Reform, is a strong instrument that is correlated with endogenous variable, $Educ_i$, but not with religious outcome variables.

We next document the effects of increased education on time spent in religious activities on weekdays and time spent in religious activities at weekend in Table 3.2. First of all, as

in Gulesci and Meyersson (2014) and Cesur and Mocan (2018), our OLS estimates for women report that there is a negative and significant correlation between education and time spent in religious activities (column (1) and (5)). Our results derived from IV regressions also report that for women, increased education significantly reduce time spent in religious activities by about 72 minutes for a weekday and 67 minutes for a weekend day (column (2) and (6), respectively). However, we document that there is no significant causal impact of education on time spent in religious activities for men (column (4) and (8)). In addition to that, we do not observe any significant association between education and men's allocation of time to religious activities (column (3) and (7)).

Table 3. 1. The Impact of Compulsory Schooling Reform on Education

| | (1) | (2) |
|---|---|---|
| | Women | Men |
| Variables | OLS | OLS |
| Reform | 0.121*** | 0.121*** |
| | (0.030) | (0.032) |
| Birth $Year_{tre}$ | 0.028*** | 0.013*** |
| | (0.005) | (0.005) |
| Birth $Year_{cont}$ | 0.018*** | 0.018*** |
| | (0.006) | (0.007) |
| Survey Year: 2015 | -0.203 | -0.344** |
| | (0.165) | (0.132) |
| Islamic Holy day: Friday | -0.0001 | 0.014 |
| | (0.018) | (0.020) |
| Survey Month Fixed Effects | YES | YES |
| Region Fixed Effects | YES | YES |
| F-stat | 15.691 | 13.856 |
| R-squared | 0.118 | 0.159 |
| Observations | 2,674 | 2,442 |

*Notes*: Sample is restricted to those born between 1980 and 1992, and the 1986 birth cohort is excluded. Robust standard errors are clustered at the region by the birth year level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively. The control variables include birth $year_{tre}$, birth $year_{cont}$, the survey-month fixed effects, the region of residence fixed effects, a dummy variable for Islamic Holy day: Friday, and a dummy variable for Survey Year: 2015.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 3. 2. The Impact of Education on Time Spent in Religious Activities (in min. per day)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Weekdays | | | | Weekend | | | |
| | Women | | Men | | Women | | Men | |
| Variables | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| Middle School | -12.930*** | -72.108** | 3.123 | 27.708 | -11.460*** | -67.112* | 0.281 | 11.805 |
| | (2.365) | (33.779) | (2.283) | (30.647) | (2.412) | (35.541) | (2.625) | (30.691) |
| Birth $Year_{tre}$ | -0.517 | 2.009 | -1.325*** | -2.018** | 0.003 | 2.379 | -0.996** | -1.320 |
| | (0.624) | (1.630) | (0.432) | (0.922) | (0.616) | (1.734) | (0.482) | (0.965) |
| Birth $Year_{cont}$ | -0.283 | 1.519 | -0.622 | -1.399 | -0.655 | 1.039 | -0.620 | -0.985 |
| | (0.593) | (1.211) | (0.503) | (1.094) | (0.653) | (1.266) | (0.535) | (1.113) |
| Survey Year: 2015 | 6.556 | -6.152 | -0.623 | 8.304 | -17.214 | -29.166 | 0.316 | 4.501 |
| | (6.393) | (9.155) | (4.743) | (12.616) | (19.269) | (21.456) | (4.148) | (11.910) |
| Islamic Holy day: Friday | 0.728 | 0.685 | 4.836** | 4.558* | 0.883 | 0.842 | 3.782 | 3.652 |
| | (2.643) | (2.693) | (2.425) | (2.495) | (2.531) | (2.681) | (2.579) | (2.624) |
| Survey Month Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Region Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 2,674 | | 2,442 | | 2,674 | | 2,442 | |

*Notes:* Sample is restricted to those born between 1980 and 1992, and the 1986 birth cohort is excluded. Robust standard errors are clustered at the region by the birth year level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively. The control variables include birth $year_{tre}$, birth $year_{cont}$, the survey-month fixed effects, the region of residence fixed effects, a dummy variable for Islamic Holy day: Friday, and a dummy variable for Survey Year: 2015.

*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

We also conduct several robustness checks to test the validity of our results. We run our OLS and IV regressions by narrowing and widening the estimation windows; implement alternative models, IV TOBIT and Reduced Form Regressions; cluster standard errors at the birth-year level by estimating p-values using the wild bootstrap as well as cluster standard errors at the birth-year level by the survey year; conduct a placebo test to identify whether our main results are due to the secular education reform; and construct an alternative outcome variable, *religious activity participation*, which is a binary variable that takes a value 1 if *time spent in religious activities* is not equal to 0, and 0 otherwise. As a result, all these analyses confirm our main results. Considering the endogeneity problem, we do not add the marital status, the employment status, the number of household members, the number of children under the age of 10, and the household income as control variables in our main analysis. However, we run our OLS and IV regressions by including these control variables and show that our findings are valid under these model specifications.

## 3.5. CONCLUSION

We document that increased education induced by the secular education reform has a negative causal impact on time allocated to religious activities. In particular, we document that the reform significantly boosts the educational attainment of individuals and reduce time allocated to religious activities for women; however, it has no impact on the religiosity of men. We complement the existing literature on the impact of education on religious behavior by providing the first causal evidence on the effects of schooling on time devoted to religious activities. Although we use Turkish data in our study, our

findings are relevant for many countries in which the educational levels have been rising. Hence, we conclude that policies such as extending compulsory secular education have the potential to impact religious behavior in a profound way. ”

# CHAPTER 4

# MATERNAL EDUCATION AND EARLY CHILDHOOD CARE

## 4.1. INTRODUCTION

A vast empirical literature in sociology, psychology, and economics have documented that early childhood care and education has a crucial impact on educational and labor market outcomes of children both in the short and long term (Heckman, 2006; Barnett & Masse, 2007; Heckman, Moon, Pinto, Savelyev, & Yavitz, 2010; Reynolds, Temple, Suh-Ruu Ou, Arteaga, & White, 2011; Heckman & Karapakula, 2019). Although many studies document the lifelong benefits of early childhood education and care, according to UNICEF report (2019), in 2017, about half of all pre-primary-age children in the world, which is more than 175 million children are not enrolled in the pre-primary education. Furthermore, in high-income countries, in 2018, 83 percent of children are enrolled while

the corresponding enrolment rate in pre-primary education in low-income countries is only 22 percent[20]. Due to the high cost of providing free pre-primary education, low and lower-middle-income countries struggle to rise enrollment rates in pre-primary education. Hence, children spend most of their time at home, making parental time spent on their care is an important factor in their early childhood education.

Recent literature has shown that not only early childhood education and care but also parental child care is a key input for the development of human capital. In particular, the literature has documented the importance of parental child care in the development of children's cognitive and non-cognitive skills for children especially for those at age 0-2 (Hart & Risley, 1995; Cunha & Heckman, 2008; Cunha, Heckman, & Schennach, 2010; Cartmill et al. 2013; Gunderson et al. 2013; Fiorini & Keane, 2014; Hernandez-Alava & Popli, 2017)[21]. It is well known that parents' education levels are associated with a higher parental time investment in children. Existing studies use ordinary least squares regressions and establish a positive correlation (Leibowitz, 1977; Hill & Stafford, 1980; Sayer, Gauthier, & Furstenberg, 2004; Sayer, Bianchi, & Robinson, 2004; Gauthier, Smeedeng, & Furstenberg, 2004; Bianchi, Robinson, & Milkie, 2006; Guryan, Hurst, & Kearney, 2008; Craig, Powell, & Smyth, 2014; Altintas, 2016; Salehi-Isfahani & Taghvatalab, 2018); however, unobservable individual factors (intelligence, ability and emotional intelligence) and unobservable family factors (social norms, values and social

[20] UNICEF (2019) calculates the enrolment ratio of pre-primary education for 2018 by using the data from the UIS global database in most recent year, 2010–2017.
[21] There are also studies showing that non-parental child care for children at age 0-2 has negative effects on children's development of cognitive and non-cognitive skills (Baker, Gruber, & Milligan, 2008; Herbst, 2013; Fort, Ichino, & Zanella, 2020).

preferences) might have an effect on both maternal education and child care behavior of mothers.

We, therefore, contribute to existing literature by investigating the impact of the extension of mandatory education from five to eight years in Turkey on activities that are considered to be primary components of early childhood education and care, namely time spent in reading to children, playing with children and talking to children. Research has shown that reading and talking to children and playing with children are important factors for brain development of children in early childhood period, especially for children under the age of 3 (Bergen, 2002; Hsin & Felfe, 2014; Hurtado, Marchman, & Fernald, 2008; Ginsberg, 2006; Kalb & Van Ours, 2014; Mol & Bus, 2011; Shneidman, Arroyo, Levine, & Goldin-Meadow, 2012; Reed, Hirsh-Pasek, & Golinkoff, 2017; Yogman, Garner, Hutchinson, Hirsh-Pasek, & Michnick, 2018; Weisleder & Fernald, 2013). Hence, our primary outcome variable appears to be well-chosen to measure early childhood care and education provided by mothers.

We examine the impact of the compulsory schooling reform on maternal education because mothers are the main child care providers due to social and cultural norms and financial constraints in many countries including Turkey (Caner, Guven, Okten, & Sakalli, 2016). In this study, we choose not to instrument mother's education with the education expansion reform because the reform as an instrument does not satisfy exclusion restrictions as more educated women tend to marry more educated men and the reform can affect potential outcomes through other channels. However, as a reduced form

approach, we analyze the effect of an expansion in education that increased mother's education levels on time spent in childcare.

In the 1997-1998 academic year, the compulsory schooling law, which extended mandatory education from five years of schooling to eight years of schooling, was enacted in Turkey. In particular, the reform allowed individuals born before 1986 to drop out of school after they completed five years of primary school, whereas those born after 1986 had to complete five years of primary school as well as three years of middle school. Turkish compulsory schooling reform was exogenous to parental decision making and mostly driven by political factors, which leads to an exogenous increase in educational attainments of individuals born after 1986, but not for those born before 1986.

Using a nationally representative survey, 2014-2015 Turkish Time Use Survey, and implementing the reduced form regression models, we assess whether an exogenous increase in the education level of mothers has any effect on time spent in early childhood care. In particular, we first find out how the mandatory schooling reform affects mothers' schooling outcomes. We next investigate whether the exposure to the reform has any impact on mothers' time spent in early childhood care, particularly time spent in reading to children, playing with children, and talking to children. More precisely, we estimate the intent-to-treat impact of the compulsory schooling reform on time spent in early childhood care by implementing the reduced form regression models for mothers who have at least one child under the age of 3. We also explore the factors that may affect time spent in early childhood care. Specifically, we examine whether the education reform that increased mothers' educational attainment has any effect on labor market outcomes and

time spent in household care and leisure activities as the reform may also affect preferences for work and leisure.

We document that the compulsory education reform increased mothers' completion of at least middle school while it did not affect the completion of at least high school or university. Our results show that the education reform does not significantly affect time spent in reading to children, playing with children, and talking to children. In terms of labor market outcomes, we do not observe any significant impact of the education reform on mothers' employment. We also show that the change in the compulsory years of schooling results in an increase in mothers' time spent in household care and leisure activities accompanied by children. As a result, our results show that increased education had no impact on time spent in early childhood care, and the positive association between time spent in early childhood care and education that is found in the literature is likely to result from omitted variables such as intelligence and values that affect both mother's education levels and time spent in early childhood care. We should also note that, it is possible that the extension of three years at the secondary school level has no impact on time spent in early childhood care and increased education at higher levels such as tertiary education attainment may affect time spent in early childhood education.

This paper is organized as follows: Section 4.2. describes the data. In section 4.3., we discuss the theoretical and conceptual framework. In Section 4.4., we explain the identification strategy. Section 4.5. presents the descriptive statistics and the results. Section 4.6. shows the robustness checks. Section 4.7. concludes.

## 4.2. DATA

To examine the impact of the maternal education on time spent in early childhood care, we utilize the data from the nationally representative Turkish Time Use Survey (TUS), conducted from 2014 to 2015 among 11,440 households. It was a part of Harmonized European Time Use Study (HETUS) and utilized EUROSTAT (2000a, 2000b) activity classifications and coding as its basis. This survey gathers the data on socioeconomic indicators for demographics, households, labor market outcomes, general health status as well as indicators to figure out the time use patterns of individuals. The survey contains four different questionnaires: Household Questionnaire, Individual Questionnaire, Diaries, and Weekly Work Schedule.

We make our main analysis by using the data from the Diaries. The diary part of the TUS asks respondents to fill a weekday diary and a weekend diary in which respondents record their daily activities in ten-minute intervals for 24-hours. In particular, the diary collects the data on how respondents allocate their time to daily activities on weekdays and at weekends. The Turkish Statistical Institute groups primary activities in the diary in ten categories: personal care, employment, education, household and family care, voluntary works and meetings, social life and entertainment, sports and outdoor activities, hobbies and computing, mass media, travel and unspecified time use. Specifically, regarding these activities, the diaries collects information about how much time individuals allocate to these daily activities, when and where individuals did these primary activities, and whether individuals were alone or not while doing these primary activities, if not, with

whom (wife/husband, mother/father, a child less than 10 years old, other household member or someone else outside the household) they did these primary activities.

We define one key outcome variables to measure mothers' time spent in early childhood care by utilizing the data from diaries. More precisely, our primary outcome variable is ***Time spent in playing games with children, reading to and talking to children.*** We also construct an alternative outcome variable to test the robustness of our results: ***Time spent in educational activities accompanied by children***. We generate this outcome variable, *educational activities accompanied by children*, by using "with whom" question in diaries, as in Guryan et al. (2009). In particular, to construct the outcome variable, *accompanied by children*, we use the data derived from respondents' answers to the following question: While you were doing this activity, was there any household member younger than 10 years old with you? ***Educational activities accompanied by children*** includes going to the cinema, exhibition, library, art-related activities, and reading books. We converted minutes per day reports for one weekday and one weekend day to minutes per week by multiplying respondents' time response for a weekday by five and time response for a weekend day by two and summing up multiplied time responses.[22] Following previous literature, we also generate six outcome variables to *i*) physical child care, *ii*) school-related child care activities, *iii*) supervisory child care, *iv*) travelling with child, *v*) other child care, and *vi*) total child care. More specifically, *physical child care* is time spent on the basic needs of children, including breast-feeding, feeding, changing diapers, rocking a child to sleep, bathing, looking at a child while playing, and so on.

[22] See, for instance, Sandburg and Hofferth (2001), Aguiar and Hurst (2007), Guryan et al. (2009) and Hsin and Felfe (2014) which convert daily time responses to weekly time response.

*School-Related child care* activities involves activities such as helping a child with homework, controlling a child's homework, and teaching a child. *Travelling with child* includes activities such as driving a child to school, to a cinema, to a music course, and to a doctor. *Supervisory child care* is time spent in accompanying a child while doing any activities. *The other child care* consists of the activities other than activities in physical, school-related, supervisory, travelling with child, and playing games with children, reading to, and talking to children. *Total child care* includes physical, school-related activities, playing games with children, reading to, and talking to children, travelling with child, supervisory, and the other child care activities.

The Individual Questionnaire collects data on individuals' age, gender, country of birth, technology device ownership, educational outcomes, and general health status. Moreover, it provides detailed information about the labor market outcomes of individuals. In particular, the Individual Questionnaire gathers information about respondents' employment status, earnings, hours worked, occupations, side jobs, and full-time/part-time employment. Nevertheless, it does not collect data on the respondent's actual wage. Specifically, the Individual Questionnaire categorizes the wage brackets into 5 groups and asks the respondent to indicate his/her wage group among these groups.[23] Therefore, we generate a binary variable, called *Higher than Minimum Wage*, which is equal to 1 if the respondent's wage group is in the higher wage groups, and is equal to 0 if the respondent's

[23] Wage group 1: 0-1080 Turkish Liras, (0-397$), Wage group 2: 1081-1550 Turkish Liras (397$-570$), Wage Group 3: 1551-2170 Turkish Liras, (570$-798$), Wage Group 4: 2171-3180 Turkish Liras (798$-1169$), and Wage Group 5: 3181 Turkish Liras and higher (1169$, and higher). Respondents, who report that their wage is in wage group 1, receive less than or equal to average minimum wage of Turkey in 2014 and 2015 as minimum wage in 2014 is 891.04 Turkish Liras, (327.5 $) and in 2015, it is 1,000.55 Turkish Liras, (367.65$).

wage group is in the low wage group to measure the effects of education on individuals' earnings.

Individuals' years of birth are used to disentangle those exposed to the mandatory schooling reform and those who were not. More precisely, *Reform,* is a dummy variable that is equal to 1 if individuals were born after 1986, and 0 otherwise. The Individual Questionnaire of TUS does not gather any information on individuals' years of schooling; however, it provides the data on the most recent degree that the individuals obtain. In particular, the survey categorizes the education status into 5 categories: *i*) No degree, *ii*) primary school degree, *iii*) middle school degree or vocational middle school degree, *iv*) high school or vocational high school degree, and *v*) university degree or above (master/Ph.D.). Therefore, we use the data on education status to construct a dummy variable, *Middle School*, which is equal to 1 if the individual obtains at least middle school diploma, i.e., completes at least eight years of schooling, and 0 otherwise.

According to OECD report (2019), on average across OECD countries in 2017, around one-third of children under the age of 3 were enrolled in early childhood education and care, either full time or part time, while this proportion is 5% in Turkey and Mexico. Hence, we focus on mothers with at least one child less than 3 years old in order to avoid sample selection to formal daycare. In addition to that, focusing on mothers with at least one child under 3 years old help us to form more homogeneous treatment and control groups. Therefore, our sample consists of married mothers who have at least one child in early childhood, i.e., aged between 0 and 2. Moreover, we limit our main analysis sample to those born between 1980 and 1992, which corresponds to married mothers aged

between 22 and 35. The treatment group includes married mothers born between 1987 and 1992 (*aged 22-28*), while the control group includes married mothers born between 1976 and 1985 (*aged 29-35*). As we discussed in the previous section, we exclude those born in 1986 due to the uncertainty in the exposure to the reform (Kırdar, Dayioglu, & Koc, 2018; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018; Dursun, Cesur, & Mocan, 2019; Akar & Okten, 2021).[24]

## 4.3. THEORETICAL & CONCEPTUAL FRAMEWORK

Our conceptual framework is based on the model described in Kimmel and Connelly (2007). This model assumes that a mother has a standard neoclassical individual-based utility maximization problem in which mother's utility is expressed as a function of leisure time, $t_l$, child care, *CC*, and aggregated adult consumption of final goods and services excluding child services, *G*. *G* and *CC* are home-produced goods and services. Adult consumption goods, *G*, is a function of household production time, $t_h$, and purchased intermediate goods, *X*; $G=G(t_h, X; \mu)$. $\mu$ is an efficiency parameter affected by differences in ability and personal investment including sleep time and educational time. Child care, *CC*, is a function of the mother's child care time, $t_{mcc}$, nonmaternal child care time (including father's child care time), $t_{ncc}$, and market-produced child goods, *CX*; $CS=CS(t_{mcc}, t_{ncc}, CX; \phi)$. $\phi$ is an efficiency parameter. In this model, mother maximizes her utility

[24] We also estimate our model by including the birth cohort of 1986. Including those born in 1986 does not change our main results.

function (1) subject to a time constraint (2), a budget constraint (3), and a child's time constraint (4). The following is the maximization problem of the mother

$$max\ U(t_l, CC, G) \tag{1}$$

subject to

$$T = t_m + t_h + t_{mcc} + t_l + t_s \tag{2}$$

$$P_x X + P_{ncc} t_{ncc} + P_{cx} CX = w t_m + V \tag{3}$$

$$CT = t_{mcc} + t_{ncc} + t_{scc} \tag{4}$$

where *T* is the mother's total time which includes time spent in market work, $t_m$; time spent in home production, $t_h$; time spent in child care, $t_{mcc}$; time spent in leisure, $t_l$; and time spent in personal investment, $t_s$. *CT* is the total amount of time available to children composed of maternal child care time, $t_{mcc}$; non-maternal child care time, $t_{ncc}$; and secondary child care time, $t_{scc}$. Secondary child care time is the total time spent in accompanying a child while doing other activities (Kimmel & Connelly, 2007).

In Kimmel and Connelly (2007) model, there are three different costs of a mother's time. First, if the mother does not work and take care of own children, there is the cost of non-work hours, w - $P_{ncc}$, where $P_{ncc}$ is the cost of nonmaternal child care time and w is the wage. Second, if the mother does not work, hire a caregiver for children, and allocate her time to leisure or household production activities, the cost of leisure and household production activities is the wage plus the nonmaternal child care, $w + P_{ncc}$. Third, if the mother just supervises her own children or the mother has school-aged children or the mother's children are old enough to take care of themselves, the opportunity cost of this

is the non-work hours, i.e., wage, *w*. Thus, this model has the following mothers' time use demand equations

$$t_j = f(w, P_{ncc}, V \mid Z, H, D), \qquad \text{for j=}m, h, mcc, L, s \qquad (5)$$

In Kimmel and Connelly (2007) model, time use depends on factors such as the wage, *w*, the prices of non-parental childcare for the mother with preschool-aged children and school-aged children, $P_{ncc}$, the amount of non-labor income available to the mother, $V$, preferences, and institutional structure, related to personal characteristics of the mother, Z, characteristics of the household in which mother resides, H, and characteristics of the diary day, D.

In this study, based on the Kimmel and Connelly (2007) model, we mainly conceptualize the following two frameworks. First of all, we consider that the compulsory schooling may improve the labor market outcomes of mothers. In particular, if the educational reform increases employed mothers' real wages, the opportunity cost of non-work hours rises. If the substitution effect dominates the income effect, individuals may substitute work hours for time spent in early childhood care. We expect that employed mothers decrease the time spent in early childhood care activities to work and earn more. If, on the other hand, the income effect dominates, mothers may increase their time spent in early childhood care by decreasing time spent in market work. The composition of unpaid home production may also change, more educated parents with higher earnings may outsource more housework, and hence allocate more time in early childhood care (Robinson & Godbey, 1999; Bianchi, Robinson, & Milkie, 2006; Aguiar & Hurst, 2007; Bianchi, 2011; Bianchi, Sayer, Milkie, & Robinson, 2012). However, it is also possible that better-

educated mothers with high family income not only outsource housework but also purchase high-quality substitutes for parental care and spare more time to their personal care and leisure activities.

The second framework we conceptualize is that the exposure to the reform may affect mothers' preferences. More precisely, the higher the level of a woman's educational attainment, the fewer children she is likely to bear (Leon, 2004; Breierova & Duflo, 2004; McCrary & Royer, 2011; Cygan-Rehm & Maeder, 2013; Amin & Behrman, 2014; Kirdar et al. 2018). Fewer children per woman and delayed marriage and childbearing could mean more time and monetary resources per child. Furthermore, education may affect social norms on parental attitudes. The mother might be better informed about the benefits of playing with and reading to their children. Parents with higher education may also put a higher value on their children's education and hence provide a better learning environment to their children to reach higher level of education. On the other hand, the mother might prefer to spend their time on household production, personal care, and leisure by reducing time spent in early childhood care even though the mother is not in the labor force.

The theory thus presents ambiguous findings for the effects of maternal education on early childhood care. Based on our conceptual framework, we will conduct our empirical investigation. For instance, notions of opportunity cost would imply that mothers may give up doing early childhood care to work more. We examine the preference hypothesis by investigating whether the education reform has any effects on how many children mothers have, the age of first-born, and last-born child. For example, the preference

hypothesis would suggest that the educational reform may increase time spent in early childhood care due to the change in fertility behavior of females. Nevertheless, it is also possible to find no impact of the education reform on time spent in early childhood care due to persistent social norms regarding the role of mothers in society. Moreover, the educational reform affects the allocation of time to leisure, personal care, and household production due to the change in mothers' preferences after having a child.

## 4.4. IDENTIFICATION

In our empirical analysis, we first examine the effects of the compulsory schooling reform on the educational attainment of mothers by regressing educational attainment, *Middle School*, on the independent variable, *Reform.* The empirical estimation is derived from the regression expressed in the following functional form

$$Middle\ School_i = \gamma_0 + \gamma_1 Reform_i + \gamma_2 X_i + \mu_i \qquad (6)$$

where *Middle School* is a dummy variable equal to 1 if the individual obtains at least middle school diploma, i.e., completes at least eight years of schooling, 0 otherwise. To examine whether educational reform has any impact on educational attainment at higher level, we also regress *High School* and *College* on a dummy variable *Reform*, which is equal to 1 if the individual was born after 1986, and 0 otherwise. $X_i$ is a vector of control variables and consists of survey-month fix effects, survey-year fixed effects, the region of residence fixed effects for the 12 regions in the country, the total number of children, the age of the first-born child, and the age of last-born child. To disentangle the impact of age

trend from outcome variables and education level of mothers at each birth year level, we also add two control variables that is defined as re-centered birth year differentiated by the exposure to the compulsory schooling reform status. In particular, we construct re-centered birth year for the treatment group as $Birth\,Year_{tre} = Reform_i \times (Birth\,Year - 1986)$ and re-centered birth year for the control group as $Birth\,Year_{cont} = (1 - Reform_i) \times (Birth\,Year - 1986)$, where Birth Year is individuals' year of birth (Fort, Schneewis, & Winter-Ebmer, 2016; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018; Akar, Akyol, & Okten, 2019). We exclude those born in 1986 due to the uncertainty in the exposure to the reform. In this regression, standard errors are clustered at the region of residence (NUTS 1) by the birth cohort level.

Estimating the impact of education on time spent in early childhood education and care activities by using the Ordinary Least Square (OLS) techniques may produce a biased estimate as an individual's educational attainment is endogenous. As in the previous literature, the exposure to the reform may be used as an instrument for educational attainment to exhibiting exogenous variation in the level of schooling but not in early childhood care outcomes. However, in our case, the Instrumental Variable (IV) approach, is not applicable because the exclusion restriction condition is more likely to be violated. In particular, an increase in education, due to the change in the compulsory schooling reform, may influence women's propensity to marry with higher educated men by changing their preferences for marriage. We confirm this finding by documenting that for mothers, the reform increases the probability of marrying a spouse who have at least middle school diploma. We, therefore, estimate the reduced form effects of the

compulsory schooling reform, which is equivalent to an "intent to treat" (ITT) analysis by estimating the following regression equation using the ordinary least squares (OLS) technique:

$$CC_i = \delta_0 + \delta_1 Reform_i + \delta_2 X_i + \alpha_i \quad (7)$$

where $CC_i$ is the early childhood care outcome for the $i^{th}$ individual. Our key outcome variable is: Time spent in reading to children, playing with children, and talking to children. We report these results as reduced form effects of the reform on time spent in early childhood care. Our control variables are the same as in equation 6. In the estimation of equation 7, standard errors are clustered at the region of residence by birth cohort level.

## 4.5. RESULTS

In this section, we first present the descriptive statistics of the main analysis sample. Next, we examine the impact of the mandatory schooling reform on educational attainment, and then on time spent in reading to children, playing with children, and talking to children. We show the effects of the educational reform on other child care outcomes and also present our results which shows that how the compulsory schooling reform affects mother's time spent in other daily activities.

### 4.5.1. DESCRIPTIVE STATISTICS

Table 4.1. presents the descriptive statistics for all mothers who have at least one child under the age of 3, and for those who were exposed to the reform and those who were not (columns 1 to 3). The treatment group includes married mothers born between 1987 and

1992, who have at least one child under the age of 3, and the control group includes those born between 1980 and 1985, who have at least one child under the age of 3. In other words, we provide descriptive statistics for married mothers between the ages of 22 and 35, who have at least one child under the age of 3. Our results show that, on average, mothers exposed to the education reform are around 25.5 years old, while mothers who were not exposed to the education reform are around 32 years old. As displayed in the Panel A of Table 4.1., compared with mothers who were born before 1986, the proportion of mothers obtaining at least a middle school diploma is higher among those who were born after 1986 (See also Figure 4.1.). At least middle school completion rate is 70 percent for mothers who were exposed to the reform, while around 49 percent of mothers who were not exposed to the reform completes a middle school or a higher degree.

The Panel B of Table 4.1. shows the descriptive statistics for labor market outcomes. In our sample, 20.8 percent of mothers are either employed or temporarily unemployed. In particular, on average, nearly 18 percent of mothers in the treatment group and 23.4 percent of mothers in the control group are either employed or temporarily unemployed. Moreover, on average, mothers born before 1986 work nearly 9 hours per week, while mothers born after 1986 work about 7 hours per week. In addition, we consider the sample of employed mothers to compare the share of mothers earning higher than minimum wage between treatment and control groups. For mothers who were not exposed to the reform, on average, the share of mothers earning more than the minimum wage (the average minimum wage of 2014 and 2015) is 77 percent, whereas the corresponding share is 69 percent for mothers in the treatment group.

The Panel C of Table 4.1. presents descriptive statistics for time spent in early childhood care. We first report that on average, mothers born after 1986 spent 332 minutes per week playing with children, reading to their children, and talking to children; on the other hand, mothers born before 1986 allocate about 279 minutes per week to these activities. (See also Figure 4.2.). Moreover, we report the descriptive statistics for time spent in educational activities accompanied by children, including going to the cinema, exhibition, library, art-related activities and reading books. Our findings indicate that, on average, time spent in educational activities accompanied by children for mothers in the treatment is nearly 6 minutes per week and in the control group is 5.5 minutes per week.

In the Panel D of Table 4.1., we present descriptive statistics for children's background characteristics. As we mentioned in the previous section, our analysis sample consists of mothers who have at least one child under the age of 3. First of all, we document that for mothers in the treatment group, the average age of the first-born child is around 3.5, while the corresponding age for mothers in the control group is 7. Moreover, on average, mothers in the control group have approximately 2.5 children, whereas mothers in the treatment group have approximately 1.8 children. When we look at the number of children under the age of 3, we report on average, mothers in the treatment group and the control group have around 1.1 children under the age of 3.

Overall, there are some differences in mothers' total number of children and the age of first-born child, and the age of last-born child that may result from the difference in mothers' ages. We will therefore point out these differences in our empirical analysis.

Table 4. 1. Descriptive Statistics

| | (1) | (2) | (3) |
|---|---|---|---|
| Variables | All | Treatment | Control |
| Panel A: Mothers' Background Information | | | |
| Age | 29.01 | 25.52 | 31.86 |
| | (1.754) | (3.575) | (1.586) |
| Middle School | 0.584 | 0.699 | 0.491 |
| | (0.501) | (0.493) | (0.460) |
| High School | 0.399 | 0.382 | 0.413 |
| | (0.493) | (0.490) | (0.487) |
| College | 0.201 | 0.143 | 0.249 |
| | (0.433) | (0.401) | (0.351) |
| Panel B: Labor Market Outcomes | | | |
| Employment | 0.208 | 0.176 | 0.234 |
| | (0.424) | (0.406) | (0.382) |
| Weekly Hours Worked | 7.949 | 6.592 | 9.054 |
| | (16.73) | (15.54) | (17.59) |
| Higher than Minimum Wage* | 0.742 | 0.686 | 0.774 |
| | (0.422) | (0.440) | (0.471) |
| Panel C: Time Spent in Child Care Activities *(in min. per week)* | | | |
| Early Childhood Care Outcomes | 301.5 | 331.9 | 276.8 |
| | (357.7) | (370.6) | (384.2) |
| Total Child Care | 1,430 | 1,483 | 1,386 |
| | (888.1) | (870.0) | (845.8) |
| Educational Activities | 5,743 | 6.066 | 5.479 |
| | (40.95) | (39.68) | (38.14) |
| Panel D: Children's Background Information | | | |
| Number of Total Children | 2.188 | 1.831 | 2.479 |
| | (1.272) | (1.013) | (1.383) |
| Number of Children under Age 3 | 1.068 | 1.070 | 1.066 |
| | (0.264) | (0.255) | (0.272) |
| Age of First-Born Child | 5.429 | 3.537 | 6.970 |
| | (4.635) | (4.309) | 2.921 |
| Age of Last-Born Child | 1.051 | 0.945 | 1.138 |
| | (0.770) | (0.762) | (0.739) |
| Observations | 606 | 272 | 334 |

*Notes*: Column (1), (2), and (3) report the results for all mothers who have at least one child under the age of 3, in the treatment group, and in the control group, respectively. The treatment group includes married mothers born between 1987 and 1992, and the control group contains married mothers born between 1980 and 1985. The 1986 cohort is excluded. **Higher than Minimum Wage* are calculated for employed mothers. Standard deviations are in the parenthesis.

*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

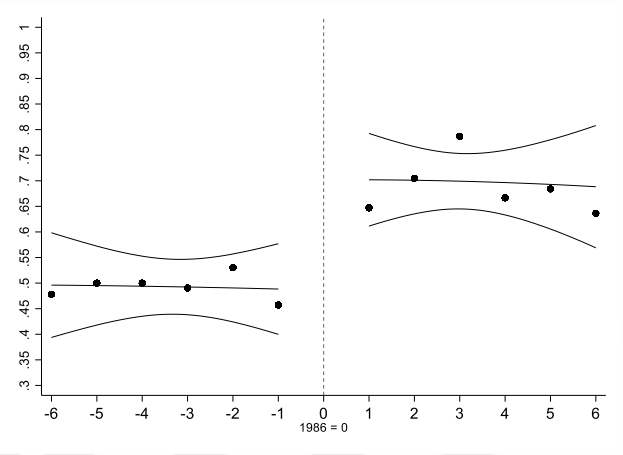


Figure 4. 1 Proportion of Individuals with At Least Middle School Diploma in 2014-2015 by Birth Cohorts 1980 to 1992

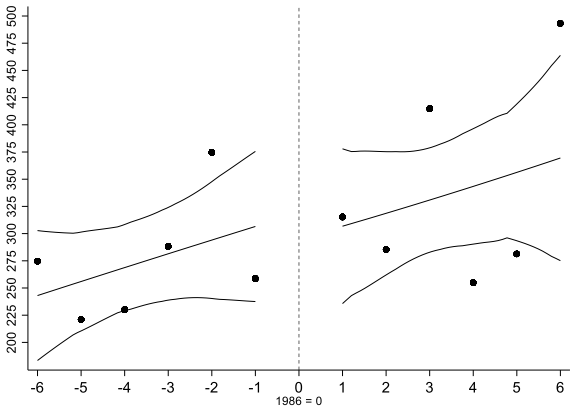


Figure 4. 2 Average Time Spent in Reading to Children, Playing with Children, and Talking to Children (in min. per week) by Birth Cohorts 1980 to 1992

### 4.5.2. SCHOOLING OUTCOMES

Table 4.2. reports the estimation results of Equation (6) by employment status and shows that the education reform significantly boosts the educational attainment of mothers who have at least one child under the age of 3. In particular, column (4) in Table 4.2. reports that the reform results in a rise in at least middle school completion rate by 20 percentage points (ppt) for mothers who have at least one child under the age of 3. We also investigate the effects of the education reform on the probability of being at least a high school graduate and at least a university graduate. However, there is no significant effects of the reform on the likelihood of completing either high school or university (See column (5) and (6) of Table 4.2.). Since female labor force participation rate is about 30 percent in Turkey (20 percent in our sample, possibly due to our requirement that women have at least one child under 3), working women is rather a select sample. Hence, in order to work with a more homogeneous sample, we also explore whether the change in the compulsory years of schooling affects the educational attainment of stay-at-home mothers, i.e., non-working mothers. The second panel of Table 4.2. presents these results. Column (4) in Table 4.2. documents that the compulsory schooling reform increases the propensity to obtain at least a middle school diploma by 21.2 percentage points (ppt) for stay-at-home mothers who have at least one child under the age of 3. Finally, we show that the impact of the education reform on at least being a middle school graduate for mothers as well as stay-at-home mothers is higher than for all women and stay-at-home women (See column (1), (2), and (3) of Table 4.2.).

Table 4. 2. The Impact of Exposure to the Education Reform on Educational Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variables | Middle School | High School | College | Middle School | High School | College |
| | All Women | | | All Mothers | | |
| Reform | 0.121*** | 0.015 | 0.026 | 0.200*** | 0.027 | -0.018 |
| | (0.030) | (0.041) | (0.038) | (0.073) | (0.075) | (0.068) |
| Observations | 2,674 | | | 606 | | |
| | All Stay-at-Home Women | | | Stay-at-Home Mothers | | |
| Variables | Middle School | High School | College | Middle School | High School | College |
| Reform | 0.100*** | -0.049 | -0.017 | 0.212** | -0.014 | -0.071 |
| | (0.038) | (0.044) | (0.035) | (0.093) | (0.084) | (0.063) |
| Observations | 1,732 | | | 480 | | |

*Notes: Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont.}$, total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

## 4.5.3. TIME SPENT ON EARLY CHILDHOOD CARE

In this section, we examine whether the education reform has any significant effects on time spent in early childhood care. We use time spent in reading to children, playing with children, and talking to children as our primary early childhood care outcome variable. Table 4.3. reports the reduced form effects (intent-to-treat effects) of the compulsory schooling reform on time spent in reading to children, playing with children, talking to children for all mothers (Panel A) and for stay-at-home mothers (Panel B). One reason to focus on stay-at-home mothers in some specifications is to work with a more homogeneous sample in terms of mothers' time constraints. In order to assure that this sample does not suffer from sample selection problem, we need to establish that treatment to reform is orthogonal to being a stay-at-home mother. Hence, we test whether the change

in the compulsory schooling reform has any impact on the labor market outcomes of mothers[25]. Table 4.3. displays the results for the labor market outcomes of mothers. We observe that the reform does not have any significant impact on labor market outcomes of mothers[26]. As a result, in some specifications, we restrict the sample to stay-at-home mothers, i.e., non-working mothers, generates more homogeneous sample, and conducting our analysis on this group of mothers may allow us to better understand the impact of mothers' education on time spent in early childhood care.

Table 4. 3. The Impact of Exposure to the Education Reform on Labor Market Outcomes

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Bandwidth | | |
| Variables | 9 | 8 | 7 | 6 | 5 |
| Employment | 0.021 | 0.015 | 0.042 | 0.082 | 0.099 |
| | (0.058) | (0.062) | (0.066) | (0.071) | (0.080) |
| Weekly Hours Worked (*0s included*) | 0.853 | 0.361 | 1.282 | 3.058 | 3.674 |
| | (2.368) | (2.493) | (2.652) | (2.820) | (3.151) |
| Observations | 765 | 726 | 678 | 606 | 539 |
| Higher than Minimum Wage | -0.010 | -0.035 | -0.003 | 0.033 | 0.209 |
| | (0.172) | (0.177) | (0.184) | (0.216) | (0.224) |
| Observations | 109 | 106 | 102 | 97 | 87 |

*Notes: Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont,}$ total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

[25] As a robustness check, we also exclude the control variables, the total number of children, the age of first-born child, and the age of last-born child, and find that excluding these variables from regression does not change our results.
[26] Our results are in line with previous literature. See, for instance, Torun (2018).

We limit our main analysis sample to those born between 1980 and 1992 (i.e. a 6-year window). Results derived from our main analysis sample are shown in the Table 4.4. To examine whether our results are due to the across the board age effect, we also estimate our model by narrowing estimation window to 5-year (the cohorts of 1991-1981) and widening the window of cohorts to 7-year (the cohorts of 1993-1979), 8-year (the cohorts of 1994-1978), and 9-year (the cohorts of 1995-1977). As a result, as seen in the Panel A of Table 3, for all estimation window, we do not find any significant impact of the compulsory schooling reform on time spent in reading to children, playing with children, and talking to children for mothers who have at least one child under the age of 3. Restricting to our sample to stay-at-home mothers does not change our main results as seen in the Panel B of Table 4.4.

Table 4. 4. The Impact of Exposure to the Education Reform on Time Spent in Reading to Children, Playing with Children, and Talking to Children (in min. per week)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Bandwidth | | |
| Variables | 9 | 8 | 7 | 6 | 5 |
| | | | Panel A: All Mothers | | |
| Reform | -18.089 | -33.550 | -26.978 | -27.271 | 5.854 |
| | (48.658) | (53.342) | (56.799) | (66.571) | (68.661) |
| Observations | 765 | 726 | 678 | 606 | 539 |
| | | | Panel B: Stay-at-Home Mothers | | |
| Reform | -14.794 | -31.753 | -20.338 | -15.973 | 14.866 |
| | (54.041) | (58.929) | (61.173) | (71.385) | (73.565) |
| Observations | 613 | 580 | 542 | 480 | 425 |

*Notes: Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont,}$ total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

We also show that the mandatory education reform has no significant effects on the total number of children that mothers have as well as the age of children. Table 4.5. shows the impact of the exposure to the reform on the number of children as well as the age of children. Therefore, in all regressions reported in Table 4.4., we include the total number of children, the age of first-born, and the age of last-born child as control variables. As a robustness check, we also exclude the control variables, the total number of children, the age of first-born child, and the age of last-born child, and find that excluding these variables from regression does not change our results.

Table 4. 5. The Impact of Exposure to the Education Reform on the Number of Children and the Age of Child

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variables | Total Number of Children | Number of Children under age of 3 | Age of First-Born Child | Age of Last-Born Child |
| Panel A: All Mothers | | | | |
| Reform | 0.016 | -0.057 | 0.205 | -0.069 |
| | (0.197) | (0.042) | (0.620) | (0.159) |
| Observations | 606 | | | |
| Panel B: Stay-at-Home Mothers | | | | |
| Reform | 0.034 | -0.097* | 0.432 | -0.067 |
| | (0.238) | (0.053) | (0.695) | (0.169) |
| Observations | 480 | | | |

*Notes*: *Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. Total Number of Children, Number of Children under age of 3, age of the first-born child, age of last-born child are dependent variables. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont.}$ Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

Overall, our results show that increased education had no impact on time spent in early childhood care. The positive association between time spent in early childhood care and

mother's education that has been found in the earlier OLS studies is likely to result from omitted variable bias, as individual characteristics such as ability and family characteristics such as values and social norms can affect both early childhood care and education levels of mothers.[27] It is also possible that the extension of three years at the secondary school level has no impact on time spent in early childhood care and increased education at higher levels such as tertiary education attainment may affect time spent in early childhood education.

### 4.5.4. OTHER CHILD CARE MEASURES

As we mentioned in Section 4. 2., we generate an alternative outcome variable, *Time spent in educational activities accompanied by children* including going to the cinema, exhibition, library, art-related activities, and reading books to test the validity of our main outcome variable. Column (1) of Table 4.6. confirms our main findings in Section 4.5.3. Our main analysis focuses on the effects of maternal education on time spent in reading to children, playing with children, and talking to children. Other childcare measures such as time spent in physical, school-related, supervisory child care activities, and traveling with child may depend on a child's specific needs. A mother may have to spend more time in helping their children with homework or teaching children depending on their children's needs. Apart from the school-related child care, travelling with child can be endogenous, as it depends on the distance where a child lives. More educated mothers may have a house that is so close to the city center, where the most of activities are more

[27] In line with previous OLS studies, we also show that there is a positive correlation between education and time spent in early childhood care activities.

likely to be held. Moreover, we suggest that mothers' allocation of time in physical child care do not necessarily help children to develop their cognitive and non-cognitive skills. Hence, in some sense time spent in these activities is determined by both demand (from the child) and supply (from the mother) side factors. Therefore, we do not focus on these since we want to estimate the effect of mother's education on her supply of early childhood care. However, to produce comparable results to existing literature, here we shortly discuss the impact of mother's education on these types of child care activities. Column (2) of Table 4.6. shows that there is no significant reduced form impact of the education reform on time spent in school-related child care activities for both all mothers and stay-at-home mothers who have at least one child under the age of 3. For stay-at-home mothers, we find that the compulsory schooling reform has a positive and significant effect on time spent in physical child care activities, but no significant impact on time spent in physical child care activities for all mothers who have at least one child under the age of 3, as seen in Column (3) of Table 4.6. For all mothers who have at least one child under the age of 3, Column (4) of Table 4.6. documents that the education reform significantly reduces time spent in supervisory activities. For all sample specifications, we find significant and negative effects of education reform on time spent in traveling with children (See column (5) of Table 4.6.). We also investigate how the compulsory schooling reform affects time spent in total child care activities. However, we do not find any intent-to-treat effect of the reform on time spent in total child care, as seen in the column (6) of Table 4.6.

Table 4. 6. The Impact of Exposure to the Education Reform on Time Spent in Other Child Care Outcomes (in min. per week)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variables | Educational Activities Accompanied by Children | School-Related Child Care Activities | Physical Child Care | Supervisory Child Care | Traveling with Child | Total Child Care |
| | | | Panel A: All Mothers | | | |
| Reform | -4.622 | -32.417 | 134.595 | -11.306* | -27.649** | -53.385 |
| | (7.749) | (21.853) | (104.800) | (5.786) | (12.068) | (125.645) |
| Observations | | | 606 | | | |
| | | | Panel B: Stay-at-Home Mothers | | | |
| Reform | 8.437 | -29.823 | 259.704** | -9.505 | -38.553*** | 89.323 |
| | (6.190) | (27.378) | (105.351) | (6.967) | (14.563) | (119.746) |
| Observations | | | 480 | | | |

*Notes*: *Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont,}$ total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by the birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

Table 4. 7. The Impact of Exposure to the Education Reform on Other Daily Activities (in min. per week)

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Time Spent Accompanied by Children | | | Time Spent Alone | |
| Variables | Leisure | Household Care | Household Care | Sleeping | Leisure |
| | | | Panel A: All Mothers | | |
| Reform | 136.404* | 68.096 | 92.462 | -129.939* | -221.345* |
| | (76.069) | (49.686) | (101.386) | (78.411) | (122.224) |
| Observations | | | 606 | | |
| | | | Panel B: Stay-at-Home Mothers | | |
| Reform | 234.658*** | 120.449** | 223.424* | -165.062 | -171.122 |
| | (83.79) | (60.477) | (124.987) | (102.494) | (136.067) |
| Observations | | | 480 | | |

*Notes*: *Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont,}$ total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by the birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

### 4.5.5. TIME SPENT IN OTHER DAILY ACTIVITIES

In this section, we examine whether the compulsory schooling reform affects time spent in other daily activities. Part of our motivation in doing so is that since we find insignificant results for time spent in early childhood care, we wonder whether the change in compulsory schooling reform, the extension of three years at the secondary school level, was too little to have any effect on any type of time use. As we discussed in the conceptual framework, the reform may affect mothers' preferences over time spent in other daily activities. We examine the exposure to the education reform on time spent in other daily activities in Table 4.7. We report that for all sample specifications, there is a positive and significant impact of the reform on time spent in leisure accompanied by children (column 1). Moreover, for stay-at-home mothers, the reform significantly increases time spent in household care accompanied by children and time spent in shopping, as seen in column 2 and column 3, respectively. For all mothers, we also find that the compulsory schooling reform significantly reduces time spent in sleeping and leisure (see column (4) and column (5), respectively). Overall, we conclude that the change in the compulsory schooling reform lead to a rise in time spent in shopping, leisure, and household care accompanied by children, but reduce time spent in sleeping and leisure activities done alone.

## 4.6. ROBUSTNESS CHECK

We conduct several additional analyses to check the validity of our results. First of all, as we discussed in previous sections, we do not include those born in 1986 from our main analysis due to the uncertainty in the exposure of 1986 birth cohort to the reform. As a

robustness check, we estimate our model by including the 1986 birth cohort (See column (1) of Table 4.8.) Furthermore, we assign the variable, *Reform*, to the value of 0.50 (See column (2) of Table 4.8.) and 0.33 for the birth cohort 1986 (See column (3) of Table 4.8.) These specifications also confirm our main results.

Then, we explore whether our results due to the fact that mothers in our sample receive child care from others. We restrict our sample to those who do not receive child care from others and show that the compulsory schooling reform has no significant impact on time spent in early childhood care activities, similar to our main results (See column (4) of Table 4.8.)

We further investigate whether our results are due to the fact that mothers in our sample have older children that can help them to do child care activities. We, therefore, restrict our sample to those who have at least one child under the age of 3 but not older than the age of 10. Our main results are also valid under this sample specification (See column (5) of Table 4.8.).

Finally, we implement an alternative method for correcting standard errors. We estimate the p-values using wild cluster bootstrap as clustering standard errors at birth cohort by survey year results in 24 clusters that may not be large enough to get accurate inference. These results confirm our main findings. Furthermore, to account for too many zeros in time use survey data, we also run Tobit and IV-Tobit regressions and show that these model modifications do not change our main results.

Table 4. 8. The Impact of Exposure to the Education Reform on Time Spent in Reading to Children, Playing with Children, and Talking to Children (in min. per week)

| | (1) | (2) | (3) | (5) | (6) |
|---|---|---|---|---|---|
| Variables | Including 1986 birth cohort | Alternative Reform Specification 1 | Alternative Reform Specification 2 | Restrict to sample those who do not receive child care from others | Restrict sample to mother who have at least one child under the age of 3 but not have a child older than 9 |
| | | | Panel A: All Mothers | | |
| Reform | -14.830 | -21.611 | -25.350 | -7.512 | -82.087 |
| | (52.507) | (65.204) | (66.083) | (70.570) | (74.992) |
| Observations | 672 | 672 | 672 | 524 | 505 |
| | | | Panel B: Stay-at-Home Mothers | | |
| Reform | -11.293 | -11.699 | -16.288 | -0.376 | -83.867 |
| | (58.994) | (69.703) | (69.152) | (72.544) | (83.915) |
| Observations | 534 | 534 | 534 | 449 | 396 |

*Notes: Reform* is a binary variable equal to 1 if the individual was born between 1987 and 1992, and equal to 0 if the individual was born between 1980 and 1985. The 1986 cohort is excluded. Stay-at-Home mothers represent all mothers who are not working for one hour with/without receiving payment during the last week. The control variables include the survey-month fixed effects, the survey-year fixed effects, the region of residence fixed effects, re-centered birth $year_{tre}$, and re-centered birth $year_{cont,}$ total number of children, the age of the first-born child, and the age of last-born child. Standard errors are clustered at the region by birth cohort level. ***, **, * indicate significance at 1 %, 5 %, and 10 %, respectively.
*Source*: 2014-2015 Turkish Time Use Survey, TURKSTAT

## 4.7. CONCLUSION

Although many studies document the lifelong benefits of early childhood education and care, according to UNICEF report (2019), in 2017, more than 175 million children, nearly half of all pre-primary-age children in the world, are not enrolled in the pre-primary education. Since mothers are the primary caregivers of children in most parts of the world, it is reasonable to incur that these children spend most of their time at home with their mothers.

In this paper, we investigate the impact of the extension in compulsory schooling in Turkey on time spent in reading to children, playing with children, talking to children. Existing studies use ordinary least squares regressions and establish a positive correlation that may not consider unobservable individual factors (intelligence, ability, emotional intelligence) and unobservable family factors (values, social norms, and social preferences) that may have an effect on maternal education as well as child care. Therefore, we point out the crucial limitations in existing studies by examining the effects of the education reform on activities that are considered to be primary components of early childhood education and care, namely time spent in reading to children, playing with children and talking to children.

Using the 2014-2015 Turkish Time Use Survey, we show that the change in the compulsory years of schooling increases the mothers' education levels significantly. However, there are no significant reduced form effects of the reform on time spent in reading to children, playing with children and talking to children for mothers who have at

least one child under the age of 3. Our results also show that the reform change mothers' allocation of time to other daily activities. We document that for stay-at-home mothers, the reform increases time spent in leisure and household care accompanied by children as well as time spent in shopping. These findings indicate that although the reform increases time spent in other daily activities accompanied by children, it has no impact on the quality of time spent with children as time spent in reading to children, playing with children, talking to children does not change. It is also possible that the extension of three years at the secondary school level has no impact on time spent in early childhood care and increased education at higher levels such as tertiary education attainment may affect time spent in early childhood care.

# CHAPTER 5

# ETHNIC GAP IN EDUCATION AND LABOR MARKET OUTCOMES

## 5.1. INTRODUCTION

Persistent gaps in economic outcomes among ethnic and racial groups is still a primary concern for many countries. The literature argues that the ethnic and racial inequalities in the labor market outcomes as well as other individual level outcomes can be narrowed by increasing human capital through schooling (Belman & Heywood, 1991; Leslie & Drinkwater, 1999; Lindley, 2009; Bandiera, Mohnen, Rasul, & Viarengo, 2018; Adamecz-Volgyi & Scharle, 2020). Therefore, policies targeting to eliminate educational disparities among ethnic and racial groups have received the attention of both academics and policy makers.

In this paper, we examine the impact of a compulsory schooling reform, which extended mandatory education from five to eight years, on the ethnic gap in education and labor market outcomes between ethnic Kurds and Turks in Turkey, where a gap in education attainment and employment exists to the detriment of Kurds, especially more pronounced for females. In Turkey, Kurds are the largest ethnic and linguistic minority; and the Kurdish population is estimated to be around 13.2 million, 18.3 percent of total population in 2010, while 73.5 % of total population is Turkish and other ethnic groups constitutes a small percentage of the total population (Konda, 2011). However, data on ethnic identity is not available in Household Labor Force Survey or census data. Utilizing a novel data set from the nationally representative Konda Barometer conducted from 2014 to 2017, which provides unique information about education as well as labor market outcomes by ethnicity, we investigate the effects of the compulsory schooling reform on the ethnic gap in education and labor outcomes between ethnic Kurds and Turks.

In Turkey, the instruction of language is Turkish, and for ethnic minority groups, there is no option to receive education in their mother tongue. In 1997, the compulsory schooling law was enacted and prolonged the mandatory education from five years of primary education to eight years incorporating junior high school into compulsory education. Prior the reform, the primary education composed of five years of compulsory primary school and three years of non-compulsory junior high school. The exposure to the reform depends on the school starting age and the age of individuals. Generally, individuals who are 11 eleven years old or younger in 1997 (born after 1986) are exposed to the reform, those who are older than 11 eleven years old in 1997 (born before 1986) are not exposed to the

reform. Thus, the reform generates an exogenous change in the educational level of individuals, and provides a natural experimental setting.

There is very little literature investigating the effect of Turkish compulsory schooling reform on ethnic gap in educational attainment. A working paper by Kirdar, Dayioglu, and Koc (2012) examines the effect of the compulsory schooling reform on the probability of completing junior high school, which is the new compulsory level for ethnic Kurds and Turks. However, the published version of this article focuses on gender gap and urban-rural gap in compulsory schooling and does not report any results on the ethnic gap in education outcomes. In this article, we examine the effects of the reform at the new compulsory schooling level as well as at the post compulsory level in order to analyze possible spillover effects of the reform on the ethnic gap between Kurds and Turks. Ours is the first paper that examines the effect of the Turkish compulsory reform on the ethnic gap at the post-compulsory level.

In addition to an ethnic gap in education outcomes, there exists an ethnic gap in labor market outcomes between Kurdish and Turkish women such that Kurdish women are less likely to be in employment than their Turkish counterparts. Utilizing a new and rich data set, we examine the effects of the education reform on the ethnic gap in labor force participation and employment outcomes between ethnic Kurds and Turks. To our knowledge, ours, is the first paper that examines the effect of the Turkish compulsory schooling reform on the ethnic gap in labor market outcomes. Finally, we investigate the potential channels through which the education reform may affect the ethnic disparities in the educational attainment as well as the labor market outcomes of individuals.

Our findings show that the compulsory schooling reform widens the ethnic gap in the educational outcomes of ethnic Kurdish and Turkish females at both the compulsory and post-compulsory schooling levels. In particular, the reform widens the ethnic gap in at least junior high school completion by 20.1 ppt and at least high school completion by 10.8 ppt at the optimal bandwidth. Perhaps even more striking is that, the reform reduces the probability of completing at least junior high school for Kurdish women. Specifically, we show that the reform reduces the likelihood of completing at least eight years of schooling by 12 ppt at the optimal bandwidth. For men, the reform increases at least junior high school completion rates of both Kurdish and Turkish males but the ethnic gap is not eliminated between the two groups. Furthermore, the reform generates spillover effects thereby increasing the educational attainment of Turkish and Kurdish males beyond the compulsory schooling level. However, for females, such spillover effects only exist for Turkish women and not for Kurdish women, as a result of which the ethnic gap in beyond the compulsory schooling levels widen between the two groups.

As the education reform increases the educational attainment of men, it also increases the likelihood of their employment although the ethnic gap in the employment rate is not eliminated by the reform. For women, the reform increases labor force participation rates of both Kurdish and Turkish women though the ethnic gap between the two groups is not eliminated.

There are several channels that may lead to differential effects of the reform on education and labor outcomes across ethnic groups. We develop these channels in our conceptual framework and examine some of them empirically in our results section. First, financial

constraints and family structure differ between ethnic Kurds and Turks. In particular, Kurdish families have larger household size and lower household income compared to their Turkish counterparts. Kurds are also more likely to live in the East, where supply of schools may not be as favorable and students have to travel far to go to school and face higher transportation costs. Second, there are important differences in social norms between the two groups. In particular, Kurds are less likely to consider themselves as modern and more likely to be pious. In addition, Kurdish women are more likely to wear a headscarf compared to Turkish women, a sign of religiosity and conservative social norms. Compulsory schooling reform, in addition to extending schooling, postponed religious education until high school as religious junior high schools were closed. Kurdish families with more religious conservative norms may be less willing to send their children to secular schools and this reluctance might be more fervent for daughters as girls at the time of the reform were not allowed to wear a headscarf in secular schools. Third, differences in labor market opportunities between the two groups may contribute to differential results. Finally, public education may not be as compelling for Kurdish children as it is for Turkish children since the language of instruction is Turkish and Kurds' native language is Kurdish.

Our paper contributes to several strands in the literature. First, we contribute to a small literature that analyzes the heterogeneous impact of the compulsory schooling on the ethnic gap in educational outcomes (Kirdar et al., 2012; Rauscher, 2014; Dustmann, Machin, & Schonber, 2019; Gayoso de Ervin, 2021). Kirdar et al. (2012) finds that the compulsory schooling reform decreases the ethnic gap for males at the compulsory level while there is no decrease in the ethnic gap for women. Rauscher (2014) shows that the

compulsory schooling laws in the U.S increases the equality of educational attainment by class and race; however, equalizing effects are more pronounced for young men. Moreover, Dustmann, Machin, and Schonber (2019) document that although ethnic minority groups in Britain have large disadvantages at the beginning of the primary school, throughout the compulsory schooling, the achievement gaps decline between White-British and other ethnic groups. More recently, Gayoso de Ervin (2021) examine the impact of the compulsory schooling reform on the language based ethnic gap in education in Paraguay, and document that the compulsory schooling reform helps to reduce the language-based educational gap. We contribute to this literature by analyzing the impact of the Turkish compulsory schooling reform both at the compulsory level and beyond compulsory level, and examine potential spillover effects of the reform. We also examine potential channels such as financial constraints, family structure, and social norms through which compulsory schooling can have differential impact on education outcomes across ethnic groups.

Second, in addition to education outcomes, we examine the effect of the compulsory schooling reform on labor market outcomes of ethnic Kurds and Turks. To the best of our knowledge, ours is the first paper in the literature that examines the effect of a compulsory education reform on ethnic gap in labor market outcomes. In doing so, we contribute to the literature documenting that the education can account for the disparities in labor market outcomes across ethnic and racial groups (see, for instance, Leslie & Drinkwater, 1999; Lindley, Dale, & Dex, 2006; Lindley, 2009; Li & Heath, 2008, 2020).

Finally, we contribute to the literature on the effects of social norms on labor market outcomes (Lehrer, 1995; Antecol, 2000; Vendrik, 2003; Fernández, Fogli, Olivetti, 2004; Guiso, Spaienza, & Zingales, 2006; Burda, Hamermesh, Weil, 2013; Scoppa & Stranges, 2019). We examine the effect of the reform on self-expressed social norms such as modernity and piousness since these might be mediating factors through which compulsory schooling can affect labor market outcomes in addition to an increase in human capital. Interestingly, we find that the reform increases the likelihood of being modern as opposed to being religious conservative or traditional conservative and decreases the likelihood of being pious for both Turkish and Kurdish females. Furthermore, among conservative men, the reform decreases the likelihood of being religious conservative as opposed to being traditional conservative. Results on piousness are consistent with the literature that find that the Turkish compulsory schooling reform decreases piousness of females in the overall population (Cesur & Mocan, 2018; Akar & Okten, 2021). These results suggest that social norms can be an important mediating factor through which compulsory schooling has increased labor force participation of both Turkish and Kurdish women.

This paper is organized as follows. Section 5.2 gives the background information on Kurds in Turkey. Section 5.3 presents the conceptual framework that motivates our empirical investigation. Section 5.4 describes the data and variables used in our empirical analysis. In Section 5.5, we specify our identification method. Section 5.6 shows the descriptive statistics and regression results. Section 5.7 empirically examines the potential mechanisms through which the reform may have had differential effects on the

educational and labor market outcomes of ethnic Kurds and Turks. Section 5.8 shows the robustness of our results. Section 5.9 concludes.

## 5.2. BACKGROUND: KURDS IN TURKEY

Kurds are the largest ethnic minority in Turkey. However, the ethnic distribution of the population is controversial as no official data has been collected on ethnicity since the 1965 census. According to 1965 census data, 8 % of the population indicate that Kurdish is their mother tongue (Sirkeci, 2000). Based on 1965 census data and THDS data, a number of studies estimates that the percentage of Kurdish population in total population ranges from 6 percent to 23 percent (Sirkeci, 2000). More recently, Koc, Hancioglu, and Cavlin (2008) use THDS conducted in 2003, and document that 14.5 percent of the population in Turkey are Kurdish-speaking. Using different data set, Konda (2011) reports that Kurdish population constitutes 18.3% of total population in Turkey[28].

Majority of Kurds, 66 % of Kurds, live in the less developed eastern and south-eastern part of Turkey; and 17.5 percent of Kurdish population live in Istanbul the largest city in Turkey[29] (Koc et al. 2008; Konda, 2011). There also exist significant income differences between Turks and Kurds. According to the Konda report (2011), based on the data on the household income per person, 53 % of Kurdish individuals lives below the poverty line, whereas 19 % of Turkish individuals lives below the poverty line.

[28] Using the data from 2017 Konda Barometer, we also estimate Kurdish population constitutes 14.3 % of total population in Turkey.
[29] In line with Konda (2011), we report that in 2017, majority of Kurds live in the eastern and south eastern part of Turkey.

Kurds also have larger household size, around 54 % of Kurds live in a household with at least 6 members (Sirkeci, 2000; Konda, 2011). The large proportion of Kurdish population is composed of younger individuals aged 0-15 (Sirkeci, 2000). The fertility rate of Kurdish women is also higher than Turkish counterparts. Specifically, Kurdish families gives birth to average 4.07 children, while the corresponding number for Turkish women is 1.88 during their reproductive years. In addition to that, intermarriage between Turks and Kurds is also quite uncommon in Turkey; only 1.6 % of Turkish women are married to Kurdish males, 6.6 % of Kurdish women are married to Turkish men (Koc et al. 2008). Kurdish individuals are also less educated compared to Turkish individuals. On average, the years of schooling for Kurdish individuals is 6 years, whereas for Turkish individuals, the corresponding number of years is 8.1. Moreover, the proportion of Kurdish individuals who have no degree is 26 %, while 6.5 % of Turkish individuals have no degree (Konda, 2011).

## 5.3. CONCEPTUAL FRAMEWORK

This section presents a discussion of the potential channels through which the compulsory schooling reform may have differential effects across ethnic Kurds and Turks in Turkey. We would argue that these channels may also be relevant for other countries where important cultural and economic differences exist across ethnic groups.

Compulsory schooling reform has been shown to increase educational attainment and improve labor market outcomes in both developed and developing countries (Angrist & Krueger, 1991; Acemoglu & Angrist, 2000; Card, 1999; Oreopoulos, 2006; Brunello,

Fort, & Weber, 2009; Grenet, 2013). Therefore, we consider that if Turkish compulsory schooling reform increases the educational attainment of individuals, an increase in the human capital through schooling may lead to an improvement in labor market outcomes of individuals.

There is also a number of recent papers documenting that the compulsory schooling diminishes the educational disadvantages among minorities (Rauscher, 2014; Dustmann, Machin, & Schonber, 2019; Gayoso de Ervin, 2021). However, in Turkey, the compulsory schooling reform may also widen the gap in individual level outcomes among minorities due to the socioeconomic and cultural differences among ethnic groups. The compulsory schooling law was implemented for all primary school aged children in Turkey; however, the law does not enforce perfect compliance. For this reason, the non-attendance of the compulsory schooling could be high among the ethnic minorities in Turkey. As indicated in the literature, the effectiveness of the education reform is also determined by the degree of the enforcement (Margo & Finegan 1996; Murtin & Viarengo, 2011; Gayoso de Ervin, 2021). We, therefore, conceptualize the following potential channels through which the effect of compulsory schooling reform may have a different impact on ethnic Kurds compared to the Turkish majority:

- ***Supply of schools:***

There was substantial investment in school infrastructure to ensure universal accessibility to the newly mandated compulsory grade levels as well as the recruitment of additional teachers (Kirdar et al. 2015). However, compared to Turkish families, Kurdish families live in more rural areas of Turkey, especially in less developed eastern region, where the

availability of schools and the quality of education tends to be lower. Kurdish students may have an access to the primary school nearest to where they live in but the junior high school may be far away from their home. Under the extreme weather conditions of the east region in winter, it may be impossible for Kurdish students to walk to school. Bussing school for students may not be available in the region or have a high cost for families. Due to the economic burden of school transportation on Kurdish parents, Kurdish students may not continue to go to a junior high school. On the other hand, there is also an option for students to choose to go regional boarding school which provides students a stay after school and students' expenses are covered by the government. Nevertheless, due to the persistent social norms and traditional views among Kurds, families are reluctant to allow their daughters to stay at regional boarding schools alone far away from home without family members (Tunc, 2009; UNICEF, 2011; Adiguzel, 2013).

- ***Family Structure:***

Kurdish families tend to be large and Kurdish children tend to have many siblings compared to their Turkish counterparts. This diminishes resources per child within a family. Due to the large number of children in the households, daughters are expected to help with the care of their younger siblings and also with housework and farm work (Tunc, 2009; UNICEF, 2011; Adiguzel, 2013). Kurdish parents have lower levels of education and live in more rural areas than their Turkish counterparts. Hence, they may be less aware of the benefits of education for their offspring and more conscious of their value as potential farm workers.

It is also widely common that Kurdish families not to register their born children in the population record officially or not to register on time, and so the biological age of the children and the age of the population records do not match. For this reason, they may not be able to send their children to school or start their children to school at later ages (Tunc, 2009; UNICEF, 2011; Adiguzel, 2013).

The engagement and marriage of girls at a younger age is also widely common among Kurds (Koc et al., 2008). For about 50 percent of all women in Eastern Turkey, the age of first marriage is under the 18 years old (Ertem and Kocturk, 2008). Due to the norms of tradition and religion, Kurdish families consider their daughters at aged 11-12 physically developed, and when their daughters reach the age of puberty, they can marry[30]. Families may even believe that sending their daughters to school lowers their prospects in marriage. Hence, for Kurdish families, the benefit of allowing their daughter to continue junior high school can easily be lower than the opportunity cost of her education.

- ***Financial Constraints:***

Kurdish families especially living in rural areas may have poor economic conditions, i.e. have lower household income than their Turkish counterparts (Toros, Cilasun, & Toros, 2018). Hence, they may face financial constraints in meeting school expenses. Although public schools are free of charge, families still have to buy uniforms and school supplies, and pay for transportation. In addition, students had to buy all their textbooks until 2003-

[30] Though the legal marriage age is 18, marriages decreed by religious authorities are common in more traditional parts of Turkey.

2004 school year. Hence, with their lower household income and more numerous families, Kurds may be more financially constrained in meeting all these school expenses. These constraints may especially be more binding for daughters if returns to their education are deemed to be lower. Therefore, children often have to drop out of school to work as an unpaid family worker or worker in agriculture (Tunc, 2009; UNICEF, 2011; Adiguzel, 2013; Dayioglu, 2005; Erturk & Dayioglu, 2004). Those who drop out of the compulsory school and have to work in low-skill occupations at earlier ages may continue on this path at later ages. Turkish families with better economic conditions may provide more educational opportunities for their children.

- ***Social Norms:***

Muslim girls from conservative families typically start to wear a headscarf when they reach the age of puberty and often continue to wear it their whole life. The age of puberty typically coincides with the starting age of junior high school. Wearing a headscarf among Muslim women is considered as a string indication of religiosity (Cesur &Mocan, 2018). Kurdish individuals are more likely to have strong conservative social norms, and therefore Kurdish females are more likely to wear a headscarf compared to their Turkish counterparts in our data.

Until 2014, there was a ban on wearing a headscarf in all schools. However, girls could wear headscarf in the religious public schools (imam hatip okullari) and get junior high school and high school degrees in these schools despite this ban. The compulsory schooling reform essentially abolishes the religious junior high schools by extending secular primary education and incorporates junior high school into primary education.

In addition, due to the conservative social norms, among Kurds, sending girls to a mixed-sex school as all secular public schools are mixed, is considered as inappropriate (Tunc, 2009), as contacting with male strangers sexually, physically, verbally or even imaginary damages honor of family (Him & Hosgur, 2011).

Hence, Kurdish families may consider their daughters not abiding by the rules of Islam if they continue with their junior high school education in the post-reform period. As a result, Kurdish girls may be less likely to comply with the compulsory schooling reform compared to Turkish girls.

In addition, there is a ban on wearing a headscarf until 2013 for university students and public sector employees. Kurdish families may also consider that although they allow their girls to continue to the junior high school, their daughters could not go to university or be employed in the public sector with their headscarf. Even in the private sector, employers may discriminate against women with headscarf (Cindoglu, 2010). Thus, lower labor market returns to women wearing headscarf might also discourage Kurdish families to send their daughters to school beyond primary level. Hence, differences in social norms may result in differential effects of the reform on the education outcomes of ethnic Kurds and Turks, especially for girls.

However, for compliers of the reform, the reform means three additional years of secular education. If the reform reduces the probability of wearing a headscarf for Kurdish females who comply with the reform or reduce the effect of social norms that discourage

female labor supply, the reform may increase the labor force participation of Kurdish females, and reduce the ethnic gap between Turkish and Kurdish females in labor market outcomes.

- ***Labor Market Preferences & Opportunities:***

Kurdish women are less likely to be in the labor force than Turkish women before the compulsory schooling reform in our data, whereas there are no significant differences between Kurdish and Turkish men. The reform may narrow or widen the ethnic gap in the labor market outcomes due to the differences in labor market preferences and opportunities. First, if the reform has differential impact on the educational attainment of the ethnic groups, it may lead the two ethnic groups to have different preferences for the labor/leisure supply choice. In particular, Kurdish individuals, especially Kurdish women, may have lower reservation wage and job quality expectations due to their lower education levels compared to Turks, and thus at given market wage, the likelihood of participating in the labor force may be higher for Kurdish individuals. Thus, the ethnic gap in the labor market outcomes may be narrowed between two ethnic groups by the reform. Second, the reform may improve the human capital of Turkish individuals more than Kurdish counterparts as Turkish individuals may have an access to better education environment than Kurdish individuals. Therefore, Turkish individuals may have better employment opportunities, and receive higher market wages than Kurdish individuals. In addition, finding employment could be easier for Turkish individuals as they may have strong social networks. As a result, ethnic gap in the labor market outcomes may be widened between the two ethnic groups. Hence, the effect of the reform on labor market outcomes is ambiguous and requires empirical investigation.

- ***Instruction in Mother Tongue***

Many studies in the literature document that the one of the crucial factors affecting the minorities' educational outcomes is the language of instructions (Angrist & Lavy 1997; Chiswick, Patrinos, & Hurst 2000; Glewwe & Kremer 2006; Dustmann, Machin, & Schönberg, 2010). The language of the compulsory education in Turkey is Turkish; however, as we argued before, the ethnic differences in Turkey arises from difference in the mother tongue, and ethnic Kurds should be able to speak Turkish before they start school. For the minority groups, there is no option to receive education in their mother tongues[31]. Therefore, Kurdish families may not be willing to start their children to school as Kurdish children may not be proficient enough to speak Turkish and understand the school subjects (Smits & Gunduz-Hosgor, 2003).

## 5.4. DATA

### 5.4.1. DATA DESCRIPTION

In this paper, to investigate whether the compulsory schooling close the ethnic gap in the educational attainment as well as the labor market outcomes of ethnic minorities, we use a novel data set retrieved from the Konda Barometer, which is collected by the respective research and consulting company, Konda Research and Consultancy, in Turkey. The Konda Barometer surveys are nationally representative survey at the NUTS-1 regional level, where 12 regions at NUTS-1 level in Turkey. These surveys are periodical polling series and conducted on the first week of each month regularly; 11 monthly Barometer are

[31] Here, we should also note that in Turkey, elective courses in Kurdish have been introduced to the curriculum in the junior high school in 2012.

produced each year. It aims to examine the political and social tendencies and preferences of society for different subgroups of Turkey.

For our analysis, we pool the data sets from 2014 to 2017 Konda Barometer[32]. The Konda Barometer gather the data on individuals' background characteristics, demographics, and labor market outcomes. In particular, each monthly survey includes the common set of information on age, education level, the region of birth, the region of residence, the type of survey, the type of current residence, the employment status, the household income, and religious denomination. More importantly, the Konda Barometer survey is the unique survey which collects the data on individuals' ethnicity as the Turkish Household Labor Force Survey (THLFS) does not include any information on ethnic identity of individuals. Although Turkish Demographics Household Survey (THDS) gathers information regarding the ethnicity of individuals, it targets females aged between 15 and 49, and not provide any data on males' labor market outcomes except for husbands of married women in the survey. Therefore, we consider the data from Konda barometer as a primary data source as it provides the ethnicity information for both men and women of working age.

The school starting age and the year of birth identifies those who are exposed to the reform and those who are not. In particular, those born after 1986 are affected by the education reform, those born before 1986 are not. In Turkey, most of students start to school at age of 6; however, the school starting age is not strictly enforced, and therefore some of students, especially Kurdish students, starts school at later ages as we discussed in the

[32] The data from each month's survey is retrieved from the following websites: https://konda.com.tr/tr/konda-barometresi/

previous section. Due to the fuzziness of the treatment status, we drop those who born in 1986 from our main sample (Kırdar, Dayioglu, & Koc, 2018; Dursun & Cesur, 2016; Cesur & Mocan, 2018; Dursun, Cesur, & Mocan, 2018; Dursun, Cesur, & Mocan, 2019; Akar & Okten, 2021). As a robustness check, similar to Kırdar et al. 2015, we also exclude those born in 1985 and 1986. We also consider that those who were born in 1987 were not exposed to the education reform and exclude them from our analysis as the compulsory schooling law does not enforce perfect-compliance, and especially Kurdish girls, may not be affected by the reform as soon as the law was enacted. We, thus, exploit annual variation in the birth cohorts for this reason. However, to confirm our validity of our findings, we include those cohorts and show the robustness of our results. We should also note that our data does not include information about individuals' month of birth.

## 5.4.2. MEASURE OF VARIABLES

- ***Measures of Identification & Educational Attainment***

Identification of those who are exposed to the education reform and those who are not is determined by the school starting age as well as the year of birth. Thus, we measure the exposure to the compulsory schooling reform by generating a binary variable, *Reform*, which is equal to 1 if individuals born after 1986, and 0 otherwise.

Konda Barometer survey categorizes the educational level of individuals as: i) illiterate, ii) literate but having no diploma, iii) primary school graduate, iv) junior high school graduate, v) high school graduate, vi) university graduate, vii) Master or PhD graduate. In our analysis, we examine the impact of the reform on the compulsory schooling level as

well as beyond the compulsory schooling level by generating three educational outcome variables. First of all, our key outcome variable is a binary variable, *junior high school*, measures whether individuals have obtained at least junior high school degree or above. In other words, the outcome variable, *junior high school*, takes the value 1 if individuals complete at least junior high school, at least eight years of schooling, and 0 otherwise. We also generate a binary variable, *high school*, which is equal to 1 if individuals obtain at least high school diploma, and 0 otherwise. Finally, we construct the binary variable, *university,* which takes the value 1 if individuals complete university/Master/PhD.

- ***Measure of Ethnicity***

Konda survey asks the respondents to identify their ethnic origin as either Turkish, Kurdish, Zaza, Arab or others. In Turkey, Kurds are the largest ethnic minority group; Zazas, Arabs, and others are smaller percentage of total population[33]. Therefore, we focus on the ethnic gap in the individual outcomes between Turkish and Kurdish individuals. We estimate the ethnic distribution of the population by using 2014-2017 Konda data, and report that about 13 percent of the population constitutes ethnic Kurds, which is consistent with previous literature. In addition, in our main analysis sample including those born in between 1979-1993 is 17 percent of individuals report their identity as Kurdish. To investigate the impact of the education reform on Kurdish individuals, we construct the *Kurdish* dummy variable, which takes the value 1 if the respondents report their identity as Kurdish, and 0 otherwise.

[33] There is also a dispute on whether Zazas are Kurds or not in the literature. We, therefore, do not consider Zazas as Kurds. However, we should also note that 1 % of total population is Zaza and including them as Kurds in our analysis does not change our main results.

- ***Measures of Labor Market Outcomes:***

In this paper, we also examine the impact of the compulsory schooling reform on labor market outcomes. Konda Barometer surveys gathers information about individuals' employment status. The survey divides the employment status into thirteen categories: civil servant, private sector, worker, small retailer, merchant/businessman, self-employed, farmer/agriculturist/stock breeder, employed-other, retired, housewife, student, unemployed, and cannot work. Based on the Konda survey classification, we consider that individuals are not in the labor force if their employment status is retired/housewife/student and cannot work. Thus, we define *the labor force participation rate* variable as a binary variable which takes the value 1 if individuals in the labor force, and 0 otherwise. However, Konda survey does not contain *unpaid family worker* category in the classification of employment status. Therefore, in our data, we document that the average female labor force participation is around 22 percent, which is lower than THLFS estimation, 31 percent. THLFS includes unpaid family worker as the employment status, and excluding this employment category, then estimating the female labor force participation shows that 23 percent of females is in the labor force, which is close to our estimation with Konda data. In Turkey, the considerable proportion of women works as unpaid family worker, while working as unpaid family worker is not widely common among men. Our data also confirms this argument as we estimate the male labor force participation rate as 69 percent, which is close to the estimation by THLFS, 69 percent;

and excluding the unpaid family work does not change the estimation for the male labor force participation by THLFS data[34].

In our analysis, we also generate two other labor market outcomes: employment and unemployment. We first construct a dummy variable, *employment*, which is equal to 1 if individuals are in the labor force and employed (civil servant, private sector, worker, small retailer, merchant/businessman, self-employed, farmer/agriculturist/stock breeder, employed-other), and 0 otherwise. To investigate the effects of education on unemployment of individuals, we generate a dummy variable, *unemployment*, which takes the value 1 if individuals' employment status is unemployed, and 0 if individuals is employed (i.e. civil servant, private sector, worker, small retailer, merchant/businessman, self-employed, farmer/agriculturist/stock breeder, employed-other).

- ***Measures of Social Norms:***

Konda survey gathers the information about whether individuals regard themselves as modern, traditional conservative, or religious conservative, and also asks a question about whether individuals consider themselves as pious. It also collects the data on whether females wear a headscarf/turban/burka. Using the data on life style and religiosity of individuals, we examine the differences in the modernity, conservativeness, piousness, and wearing a headscarf/turban/burka among Turkish and Kurdish individuals. To measure the modernity of individuals, we generate a dummy variable, *modern*, that takes the value 1 if the individuals consider themselves as modern, and 0 if individuals are

[34] The estimations of the labor force participation rates are based on authors calculations. We use 2015 Turkish HLFS data to estimate the labor force participation rate of females and males by including and excluding the unpaid family workers' category as employment status.

religious conservative or traditional conservative. Regarding piousness, Konda survey ask respondents whether they consider themselves as: i) believer and abides by the rules of religion, ii) believer and tries to abide by the rules of religion, iii) believer but does not much abide by the rules of Islam, and iv) not a believer. In particular, we construct a dummy variable, *pious*, which takes the value 1 if individual is a believer and abides or tries to abide by the rules of religion, and 0 if the individual is a believer but does not much abide by the rules of Islam. We exclude those who report that they are not believer, atheist, as the number of observations is small[35]. Apart from this, we measure the religious behavior of individuals by generating a dummy variable, *headscarf*, is equal to 1 if the female respondent wears a headscarf/turban/burka, and 0 otherwise.

## 5.5. IDENTIFICATION STRATEGY

In our analysis, due to the fuzziness in the treatment status, we investigate the intention to treat (ITT) impact of the compulsory education reform on the ethnic gap in individual outcomes. In particular, we estimate the following linear probability model for females and males, separately:

$$Outcomes_i = \gamma_0 + \gamma_1 Reform_i + \gamma_2 Kurdish_i + \gamma_3 Reform_i * Kurdish_i + \gamma_4 BY_{ti} + \gamma_5 BY_{ci} + \gamma_6 X_i + \mu_i \quad (1)$$

[35] Including those who regard them as atheist does not change our results.

where $Outcomes_i$ denotes a particular education or labor outcome for individual $i$. $Reform_i$ takes the value 1 if the individual was born after 1986, and it takes the value 0 if the individual was born before 1986. $Kurdish_i$ is a dummy variable equal to 1 if the ethnic origin of individual is Kurdish, and the coefficient of estimates for the binary variable $Kurdish_i$ indicates the relationship between the individual level outcomes and having Kurdish identity during the pre-treatment period. $Reform_i * Kurdish_i$ is the interaction term; and the estimate of $\gamma_3$ measures the effects of the compulsory schooling reform on the individual level outcomes for Kurds compared to Turks. Our parameter of interest $\gamma_1 + \gamma_3$ reports the impact of education on the individual outcomes for Kurds.

To account for the time trends in the outcome variable, we use the linear time trend in which the birth year is normalized by subtracting the cut-off birth year, 1986, for Kurdish and Turkish individuals, separately. In particular, Kurdish and Turkish individuals may have different time trends that may have different impact on the outcome variables; we, therefore, add the control variables, re-centered birth year differentiated by ethnicity, for treatment group as $BY_{ti} = Reform_i \times (Birth\ Year - 1986) \times Kurdish$ and re-centered birth year differentiated by ethnicity, for the control group as $BY_{ci} = (1 - Reform_i) \times (Birth\ Year - 1986) \times Kurdish$, where Birth Year is the year that the individual is born. $X_i$ is a vector of control variables including the survey year fixed effects, the survey month fixed effects, the region of residence fixed effects, the type of residence fixed effects, the birth region fixed effects, and religious denominations. As a robustness check, we also include region and birth region controls differentiated by

ethnicity to capture the region trends in outcome variables that may affect Turkish and Kurdish differently.

We cluster the robust standard errors at the birth region, NUTS1 level, by the birth cohort level. As a robustness check, we also cluster the standard errors at the current region, NUTS1 level, by the birth cohort level. In addition, not for all observation, but for the most of monthly Konda surveys, we also have data on the birth city of the respondents. We test the validity of our result by adding birth city fixed effects in regressions, and clustering the standard errors at the birth region, NUTS2 level, by birth year. Note that our results are robust for all these specifications[36].

Disentangling the impact of the education reform from the time trends in our outcome variables is a crucial issue for our identification strategy. Therefore, we check the robustness of our findings by widening and narrowing the estimation windows around the cut-off birth year, 1986. First of all, we run our regression model by 9-year estimation window around the cut-off, and then narrow the estimation window to 7-year and 5-year estimation windows. We use symmetric bandwidths for the control and treatment group, and determine these bandwidths by employing the optimal bandwidth selection methods.[37] In particular, we find that the optimal bandwidth for pooled female (male) sample is 9 (7), and the optimal bandwidth for both Turkish and Kurdish sample is 7. Furthermore, narrowing the symmetric estimation window around the cut-off helps us to eliminate the

[36] Results for Robustness checks are available upon request.
[37] We employ STATA command *rdrobust* to find out the optimal bandwidth.

impact of the other policies implemented in Turkey after 1997[38]. In particular, after 1997, the government increases the construction of schools as well as dormitories and the employment of teachers. Moreover, after 2004, the government starts providing school books for free in order to ease financial constraints by families. In addition to that, the social benefits are rapidly increased by the government in the 2000s. The social aids are provided to mothers who send their children to school regularly, especially for those who send their daughters to a junior high school receives higher amount of benefits compared to the others. There are also two Conditional Cash Transfer (CCT) program implemented after 1997 which target girls living in the region with the lowest rates of enrollment and highest rates of dropouts and encourage families to send daughters to school. In particular, the CCT program "Haydi Kizlar Okula!' was started in 2003; and another CCT program, Baba Beni Okula Gonder, was implemented in 2005. However, the effects of these programs are negligible compared to the compulsory schooling reform, which is implemented nationwide and affects the large portion of student at the different birth cohort (Kirdar et al. 2015). In addition, adding the linear time trend for Kurdish and Turkish individuals will capture the impact of these policies during the estimation of our model at wider bandwidth, 9.

## 5.6. RESULTS

In this section, we first present the descriptive statistics for educational and labor market outcomes. Then, we examine the differential effects of the compulsory schooling reform on the educational outcomes for both Turkish and Kurdish individuals. Finally, we

[38] We also run our model by using the bandwidth level 3 and 4; and our main results are robust under these specifications.

investigate how the compulsory schooling reform mitigates the gap in the labor market outcomes among ethnic minorities.

### 5.6.1. DESCRIPTIVE STATISTICS

Table 5.1. and Table 5.2. show the descriptive statistics of our main analysis sample by the exposure to the education reform for females and males, respectively. As shown in the first of row of the Panel A, the household income is lower but the household size is higher for Kurds both in the treatment and control group compared to Turks. The fraction of Alevis, the second larger Muslim denomination after Sunnis appear slightly higher among ethnic Turks than Kurd.

Panel B of Table 5.1 and Table 5.2. reports the descriptive statistics for the education outcomes, females and males, respectively. On average, the proportion of individuals who have at least junior high school or high school or university degree is higher among those who are exposed to the education reform than those who are not (see the Panel B of Table 5.1. and Table 5.2.) for both ethnic Kurds and Turks.

Table 5.1. and Table 5.2. provide averages of key variables for treatment and control cohorts for females and males, respectively. We also provide cohort specific education outcomes in Figure 5.1, 5.2, and 5.3, which show at least junior high school competition rate, at least high school competition rate, and at least university competition rate, respectively for each cohort of ethnic Kurds and Turks, separately. As shown in these figures, both before and after the reform is implemented, at least junior high school/high

school/university completion rate of Turkish individuals are higher than Kurdish individuals for each cohort. However, the gap in the educational outcomes between Turkish and Kurdish males narrows for those who are exposed to the education reform. On the other hand, for females, the ethnic gap in the educational outcomes appears to widen for cohorts in the treatment group (for cohorts born after 1986).

Although our descriptive statistics indicates that at least junior high school completion rate of Kurdish females in the treatment group is higher than those in the control group, as seen in the Figure 5.1, around the neighborhood of the cut-off birth year, 1986, which disentangles those who are exposed to the reform and those who are not, there is a sharp decrease in the junior high school rate for Kurdish females. The increasing trend in junior high school competition rate for Kurdish females appears to be disrupted with the implementation of the reform. Specifically, the proportion of Kurdish females with at least junior high school diploma is 43 percent in the cohort of 1984; and the rate goes down to 40 percent in the cohort of 1985, and then drops to 35 percent in the cohort of 1986. The rate increases to 40 percent in the cohort of 1987, and rise up to 45 percent in the cohort of 1988, which is only slightly higher than the rate in the birth cohort, 1984. On the other hand, for both Turkish males and females as well as Kurdish males, we observe that there is a sharp increase at the year of policy implementation, 1986. As we discussed in the previous section, there are also other policies implemented after 1997 to increase the educational attainment of individuals, especially for the educational attainment of females, detangling the impact of these policies from the compulsory schooling reform rises the importance of our empirical investigation at different bandwidths.

The Panel C of Table 5.1 and Table 5.2. presents the descriptive statistics for the labor market outcomes of females and males, respectively. First of all, for females, we observe that on average, the labor force participation rate and employment rate (unemployment rate) is larger (lower) among those who are exposed to the education reform than those who are not, whereas for males, the labor force participation rate and the employment rate (the unemployment rate) is higher (lower) among those who are not exposed to the education reform than those who are exposed to. As seen in Figure 5.4, 5.5, and 5.6, for females both who are born before 1986 and after 1986, there is a persistent ethnic gap in the labor market outcomes, and Turkish females in the treatment and control groups are more likely to participate in the labor force and being employed but less likely to be unemployed than their Kurdish counterparts. However, for males, prior to the reform, there is no ethnic gap in the labor market outcomes, the ethnic gap in the labor market outcomes between two ethnic groups who are exposed to the reform widens.

We also present the descriptive statistics for characteristics regarding social norms in Panel D of Table 5.1. and Table 5.2., for females and males, respectively. On average, for both Kurdish and Turkish individuals, the proportion of individuals in the treatment group who consider themselves as religious conservative and pious is lower and those who consider themselves modern is higher compared to the control group. In addition, on average, the proportion of females who wears a headscarf in the treatment group is lower than the control group. In cohort specific graphs, we also show that Kurdish in both treatment and control groups are less likely to be modern and more likely to be religious conservative and pious compared to their Turkish counterparts, as seen in Figure 5.7., 5.8., and 5.9., respectively. In addition, Figure 5.10. shows that Kurdish women are more likely

to wear a headscarf than Turkish women, and the ethnic gap in wearing a headscarf widens for those who are exposed to the reform.

Table 5. 1. Descriptive Statistics (for females)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Turkish Females | | | | Kurdish Females | | | |
| | Treatment | | Control | | Treatment | | Control | |
| VARIABLES | N | mean | N | Mean | N | mean | N | mean |
| Panel A: Background Information | | | | | | | | |
| Denomination: Alevi | 3,656 | 0.050 | 4,228 | 0.050 | 867 | 0.033 | 716 | 0.045 |
| | | (0.219) | | (0.218) | | (0.180) | | (0.207) |
| Ever-Married | 1,513 | 0.601 | 1,802 | 0.928 | 358 | 0.712 | 316 | 0.915 |
| | | (0.490) | | (0.259) | | (0.453) | | (0.280) |
| Num. of Household Members | 3,189 | 3.858 | 3,73 | 4.101 | 734 | 5.125 | 613 | 5.225 |
| | | (1.373) | | (1.249) | | (2.038) | | (1.832) |
| Household Income* | 3,472 | 829.3 | 3,993 | 799.6 | 830 | 596.7 | 679 | 629.4 |
| | | (685.7) | | (675.6) | | (407.3) | | (1.233) |
| Panel B: Education Outcomes | | | | | | | | |
| Junior high school | 3,656 | 0.872 | 4,228 | 0.632 | 867 | 0.584 | 716 | 0.330 |
| | | (0.334) | | (0.482) | | (0.493) | | (0.470) |
| High school | 3,656 | 0.703 | 4,228 | 0.454 | 867 | 0.384 | 716 | 0.197 |
| | | (0.457) | | (0.498) | | (0.487) | | (0.398) |
| University/Master/PhD | 3,656 | 0.316 | 4,228 | 0.166 | 867 | 0.137 | 716 | 0.0782 |
| | | (0.465) | | (0.372) | | (0.344) | | (0.269) |
| Panel C: Labor Market Outcomes | | | | | | | | |
| LFPR | 3,656 | 0.398 | 4,228 | 0.301 | 867 | 0.238 | 716 | 0.166 |
| | | (0.490) | | (0.459) | | (0.426) | | (0.373) |
| Employment | 3,656 | 0.298 | 4,228 | 0.282 | 867 | 0.171 | 716 | 0.142 |
| | | (0.458) | | (0.450) | | (0.376) | | (0.350) |
| Unemployment | 1,455 | 0.251 | 1,272 | 0.0637 | 206 | 0.282 | 119 | 0.143 |
| | | (0.434) | | (0.244) | | (0.451) | | (0.351) |
| Panel D: Social Norms | | | | | | | | |
| Modern | 3,453 | 0.399 | 3,991 | 0.257 | 820 | 0.220 | 668 | 0.144 |
| | | (0.490) | | (0.437) | | (0.414) | | (0.351) |
| Religious Conservative | 2,074 | 0.284 | 2,964 | 0.325 | 640 | 0.391 | 572 | 0.442 |
| | | (0.451) | | (0.468) | | (0.488) | | (0.497) |
| Pious | 3,514 | 0.645 | 4,13 | 0.760 | 841 | 0.771 | 700 | 0.853 |
| | | (0.479) | | (0.427) | | (0.421) | | (0.355) |
| Wearing a headscarf | 3,489 | 0.436 | 4,031 | 0.589 | 819 | 0.714 | 681 | 0.819 |
| | | (0.496) | | (0.492) | | (0.452) | | (0.385) |

*Notes*: Table displays the mean, the standard deviation (given in the parenthesis), and the number of observations (N). *Household income is deflated by CPI. Treatment group includes individuals born between 1987 and 1993, and the control group includes those born between 1979 and 1985. The 1986 cohort is excluded.
*Source*: KONDA Barometer, 2014-2017

Table 5. 2. Descriptive Statistics (for males)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Turkish Males | | | | Kurdish Males | | | |
| | Treatment | | Control | | Treatment | | Control | |
| VARIABLES | N | mean | N | mean | N | mean | N | mean |
| Panel A: Background Information | | | | | | | | |
| Denomination: Alevi | 3,980 | 0.051 | 3,789 | 0.046 | 969 | 0.039 | 749 | 0.041 |
| | | (0.219) | | (0.209) | | (0.194) | | (0.199) |
| Ever-Married | 1,533 | 0.326 | 1,52 | 0.832 | 389 | 0.452 | 306 | 0.850 |
| | | (0.469) | | (0.374) | | (0.498) | | (0.358) |
| Num. of Household Members | 3,429 | 3.747 | 3,278 | 3.790 | 829 | 5.168 | 650 | 4.955 |
| | | (1.462) | | (1.400) | | (2.280) | | (1.931) |
| Household Income* | 3,796 | 958.7 | 3,639 | 908.4 | 936 | 748.1 | 724 | 721.1 |
| | | (859.1) | | (653.2) | | (800.2) | | (648.3) |
| Panel B: Education Outcomes | | | | | | | | |
| Junior high school | 3,980 | 0.937 | 3,789 | 0.826 | 969 | 0.851 | 749 | 0.636 |
| | | (0.243) | | (0.379) | | (0.356) | | (0.482) |
| High school | 3,980 | 0.803 | 3,789 | 0.665 | 969 | 0.662 | 749 | 0.445 |
| | | (0.398) | | (0.472) | | (0.473) | | (0.497) |
| University/Master/PhD | 3,980 | 0.371 | 3,789 | 0.270 | 969 | 0.264 | 749 | 0.171 |
| | | (0.483) | | (0.444) | | (0.441) | | (0.377) |
| Panel C: Labor Market Outcomes | | | | | | | | |
| LFPR | 3,980 | 0.796 | 3,789 | 0.981 | 969 | 0.799 | 749 | 0.983 |
| | | (0.403) | | (0.137) | | (0.401) | | (0.131) |
| Employment | 3,980 | 0.673 | 3,789 | 0.931 | 969 | 0.647 | 749 | 0.884 |
| | | (0.469) | | (0.253) | | (0.478) | | (0.321) |
| Unemployment | 3,170 | 0.155 | 3,716 | 0.0506 | 774 | 0.190 | 736 | 0.101 |
| | | (0.362) | | (0.219) | | (0.392) | | (0.301) |
| Panel D: Social Norms | | | | | | | | |
| Modern | 3,778 | 0.396 | 3,578 | 0.276 | 923 | 0.291 | 719 | 0.202 |
| | | (0.489) | | (0.447) | | (0.455) | | (0.402) |
| Religious Conservative | 2,281 | 0.260 | 2,592 | 0.289 | 654 | 0.289 | 574 | 0.383 |
| | | (0.439) | | (0.454) | | (0.454) | | (0.487) |
| Pious | 3,789 | 0.573 | 3,661 | 0.645 | 901 | 0.628 | 713 | 0.719 |
| | | (0.495) | | (0.479) | | (0.484) | | (0.450) |

Notes: Table displays the mean, the standard deviation (given in the parenthesis), and the number of observations (N). *Household income is deflated by CPI. Treatment group includes individuals born between 1987 and 1993, and the control group includes those born between 1979 and 1985. The 1986 cohort is excluded.
*Source*: KONDA Barometer, 2014-2017

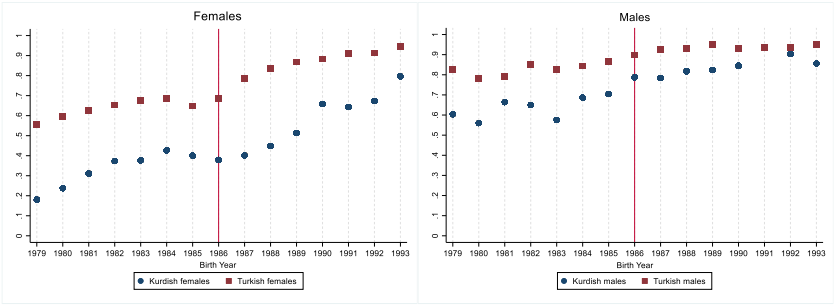


Figure 5. 1 Ethnic Gap in at least Junior High School Completion Rate by Birth Cohorts 1979-1993

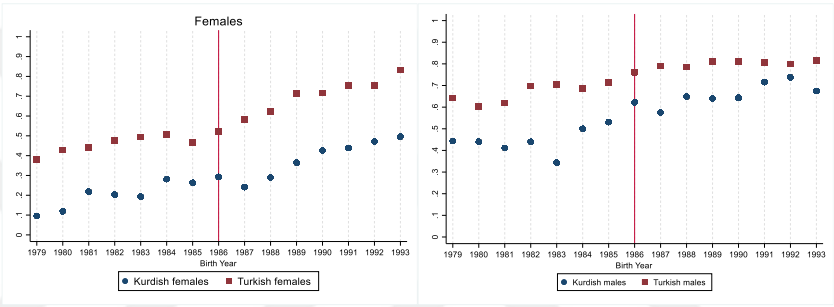


Figure 5. 2 Ethnic Gap in at least High School Completion Rate by Birth Cohorts 1979-1993

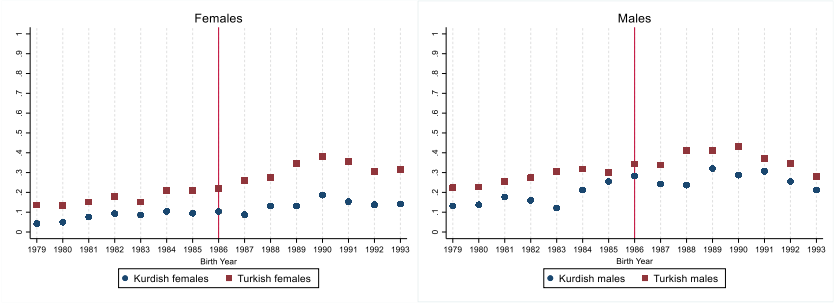


Figure 5. 3 Ethnic Gap in at least University Completion Rate by Birth Cohorts 1979-1993

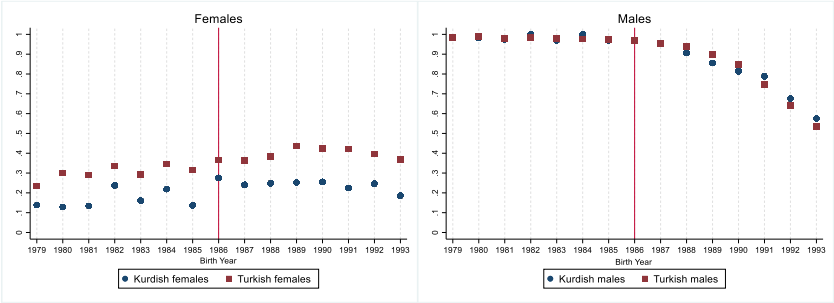


Figure 5. 4 Ethnic Gap in the Labor Force Participation Rate by Birth Cohorts 1979-1993

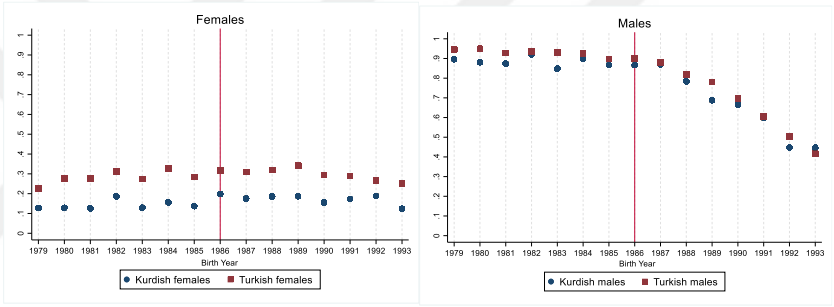


Figure 5. 5 Ethnic Gap in the Employment Rate by Birth Cohorts 1979-1993

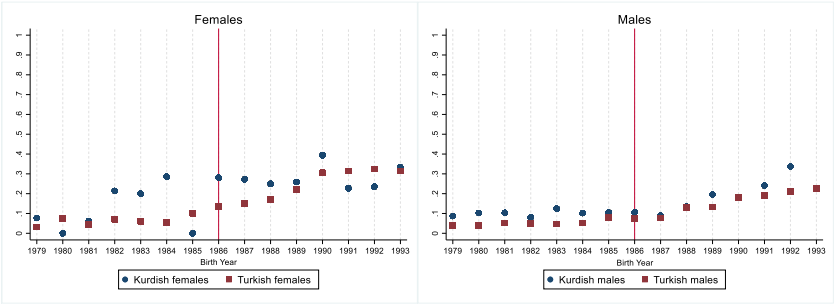


Figure 5. 6 Ethnic Gap in the Unemployment Rate by Birth Cohorts 1979-1993

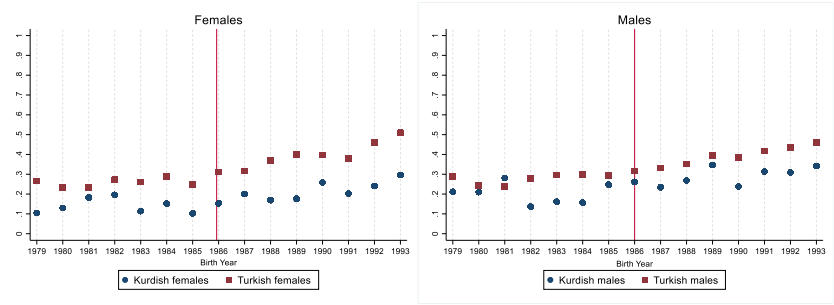


Figure 5. 7 Ethnic Gap in the Modernity by Birth Cohorts 1979-1993

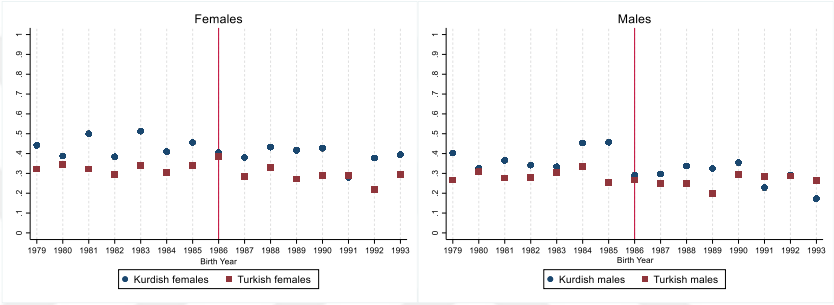


Figure 5. 8 Ethnic Gap in the Religious Conservativeness by Birth Cohorts 1979-1993

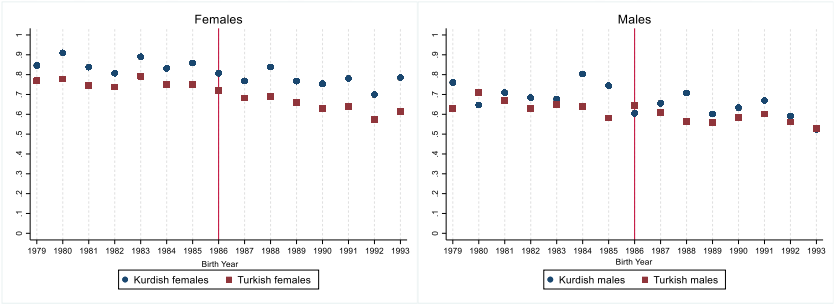


Figure 5. 9 Ethnic Gap in the Piousness by Birth Cohorts 1979-1993

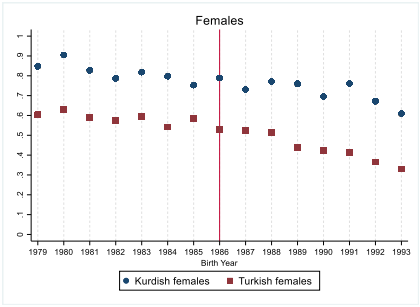


Figure 5. 10 Ethnic Gap in Wearing a Headscarf by Birth Cohorts 1979-1993

## 5.6.2. REGRESSION RESULTS

In this section we report results from estimating the Equation (1), which is a Fuzzy Regression Discontinuity Design, where cohorts born after 1986 are exposed to the reform, and hence are in the treatment group; and those born before 1986 are in the control group. Table 5.3. and Table 5.4. present results where dependent variables are education and labor market outcomes, respectively. All regressions control for the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, the re-centered birth year differentiated by the ethnicity in the treatment and the control groups.

The column (1)-(3) of Table 5.3. and 5.4. displays results for females at the bandwidth 9, 7, and 5, respectively; and the column (4)-(6) of Table 5.3. and 5.4. presents the results for males at the bandwidth 9, 7, and 5, respectively. The row of "Reform" shows the impact of the reform on the educational attainment of Turkish individuals, while the row of "Composite Kurdish" displays the effects of the education reform on the educational attainment of Kurdish individuals, i.e., sum of the coefficient of "Reform" and "Reform*Kurdish". The row of "Ethnicity: Kurdish" presents the association between being Kurdish and having at least junior high school diploma prior to the reform. The row of "Reform*Kurdish" reports the impact of the education reform on the ethnic gap in the educational attainment. We first discuss are our results on education outcomes, and next our results on labor market outcomes.

### 5.6.2.1. EDUCATIONAL OUTCOMES

Table 5.3. presents the estimates of the Equation (1) at a range of different bandwidths for females and males, respectively. First, we examine whether the reform increases individuals' schooling at the new compulsory schooling level, i.e. at least eight years of schooling. Then, we examine whether the reform generates the spillover effects on the educational attainment, above and beyond the compulsory schooling level. More specifically, Panel A/B/C of Table 5.3. shows the impact of an exogenous education reform on at least junior high school completion/high school/university levels, respectively.

As seen in the Panel A of Table 5.3., for Turkish females, the compulsory schooling reform significantly increases the probability of completion at least junior high school for all estimation windows (column (1)-(3)). In particular, the coefficient estimates of "Reform" ranges from 9.1 ppt to 8.1 ppt for bandwidth 9 to 5. Being Kurdish is negatively and significantly associated with completing at least junior high school in the pre-reform period for females (see the row of "Ethnicity: Kurdish" of the Panel A). Furthermore, as seen in the "Reform*Kurdish" row, we document that Kurdish females are less likely to complete at least junior high school compared to their Turkish counterparts as a result of the reform. In other words, the reform widens the ethnic gap in education outcomes for females. Perhaps even more striking is the fact that the reform significantly reduces the likelihood of receiving eight years of schooling for Kurdish females (see the row of "Composite Kurdish" of the Panel A).

When we examine the impact of the education reform on the ethnic gap in the educational outcomes for males, we find that the reform does not generate any significant difference in completing at least eight years of schooling between Turkish and Kurdish males as the coefficient of the interaction terms, "Reform*Kurdish", are statistically insignificant at different bandwidth levels. In particular, in the column (4)-(6), we report that the reform significantly increases the likelihood of obtaining at least a junior high school diploma for both Turkish and Kurdish males (see the row of "Reform" and "Composite Kurdish", respectively). We next investigate whether the education reform has any impact on the post-compulsory schooling levels. Following the previous literature, we consider that although the compulsory schooling reform does not cover the grades of high school and university, it may have an indirect impact, i.e. spillover impact (see Kirdar et al., 2012, 2015). Therefore, we examine the education reform on the likelihood of completing at least high school and university degree, and show the results in the Panel B and C of Table 5.3., respectively. We first document that for Turkish females, the compulsory schooling reform significantly rises the probability of completing at least high school at all estimation bandwidth, whereas for Kurdish females, it reduces the likelihood of obtaining at least high school diploma at the optimal bandwidth level, 7. As a result, in the row of "Reform*Kurdish" of Table 5.3., we show that the education reform widens the gap in at least high school completion rate between Turkish and Kurdish females. On the other hand, for Turkish males, the education reform significantly boosts the probability of obtaining at least high school, i.e. generates the spillover effects, and does not eliminate the disparities in the likelihood of completing at least high school diploma between Kurdish and Turkish males.

Table 5. 3. The Effects of Compulsory Schooling Reform on Educational Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| Panel A: At Least Junior High School Completion | | | | | | |
| Reform | 0.088*** | 0.081*** | 0.091*** | 0.055*** | 0.062*** | 0.053*** |
| | (0.019) | (0.022) | (0.024) | (0.013) | (0.015) | (0.019) |
| Ethnicity: Kurdish | -0.166*** | -0.110** | -0.131** | -0.139*** | -0.106*** | -0.128*** |
| | (0.037) | (0.046) | (0.052) | (0.028) | (0.028) | (0.034) |
| Reform*Kurdish | -0.148*** | -0.201*** | -0.173** | 0.047 | 0.007 | -0.010 |
| | (0.047) | (0.059) | (0.069) | (0.036) | (0.038) | (0.043) |
| Composite Kurdish | -0.060 | -0.120** | -0.082 | 0.102*** | 0.069** | 0.043 |
| | (0.042) | (0.054) | (0.061) | (0.033) | (0.035) | (0.039) |
| Panel B: At Least High School Completion | | | | | | |
| Reform | 0.056** | 0.041* | 0.047* | 0.064*** | 0.058*** | 0.052** |
| | (0.022) | (0.024) | (0.028) | (0.016) | (0.018) | (0.021) |
| Ethnicity: Kurdish | -0.157*** | -0.131*** | -0.143*** | -0.177*** | -0.173*** | -0.125*** |
| | (0.031) | (0.038) | (0.048) | (0.033) | (0.039) | (0.039) |
| Reform*Kurdish | -0.090** | -0.108** | -0.105* | 0.037 | 0.023 | -0.039 |
| | (0.039) | (0.046) | (0.059) | (0.045) | (0.053) | (0.054) |
| Composite Kurdish | -0.033 | -0.067* | -0.058 | 0.100** | 0.081* | 0.014 |
| | (0.031) | (0.037) | (0.047) | (0.041) | (0.048) | (0.047) |
| Panel C: At Least University Completion | | | | | | |
| Reform | 0.116*** | 0.068*** | 0.015 | 0.133*** | 0.083*** | 0.044 |
| | (0.023) | (0.024) | (0.026) | (0.024) | (0.026) | (0.028) |
| Ethnicity: Kurdish | -0.064** | -0.057* | -0.077* | -0.081*** | -0.080** | -0.056 |
| | (0.028) | (0.034) | (0.043) | (0.029) | (0.033) | (0.040) |
| Reform*Kurdish | -0.099** | -0.065 | -0.026 | -0.059 | -0.057 | -0.075 |
| | (0.042) | (0.048) | (0.059) | (0.049) | (0.049) | (0.053) |
| Composite Kurdish | 0.016 | 0.003 | -0.010 | 0.073* | 0.026 | -0.032 |
| | (0.035) | (0.038) | (0.048) | (0.043) | (0.041) | (0.042) |
| Observations | 11,862 | 9,465 | 6,744 | 11,936 | 9,485 | 6,755 |

*Notes*: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Source:* KONDA Barometer, 2014-2017

In the Panel C of Table 5.3., we present the impact of the education reform on at least university completion rate. The row of "Composite Kurdish" document that for both Kurdish males and females, the reform has no impact on the likelihood of obtaining at least university diploma. On the other hand, we find that an additional three years of schooling lead to a rise in the completion rate of at least university among Turkish individuals (column (1)-(2) of the row of "Reform" for females and column (4)-(5) of the row of "Reform" for males). However, narrowing the bandwidth shows that the reform has no significant impact on the post-compulsory schooling level. When the estimation windows are 9 and 7, the treatment group consists of individuals aged between 19 to 22, those who have not completed the university yet. Therefore, our findings for the bandwidth 9 and 7 may not reflect the accurate impact of the education reform on university completion rate. Finally, the row of "Reform*Kurdish" of Panel C report that the reform has no impact on the ethnic gap in at least university completion rate between Turks and Kurds. Over all, we conclude that the gap in the post-compulsory schooling outcomes, at least high school/at least university is not eliminated by the reform.

To summarize, we find that the compulsory schooling reform enacted in 1997 in Turkey widens the ethnic gap in the educational outcomes between Kurdish and Turkish females by reducing the likelihood of completing at least junior high school of Kurdish females. An additional three years of schooling also generate spillover effect for Turkish females, and widens the ethnic gap in the post-compulsory schooling level. On the other hand, the education reform has no impact on the ethnic disparities in the educational outcomes between Kurdish and Turkish males. However, it increases the educational attainment of both Turkish and Kurdish males. In addition, the compulsory schooling reform generates

spillover effects; rises the educational attainment of Turkish and Kurdish males beyond the compulsory schooling level.

### 5.6.6.2. LABOR MARKET OUTCOMES

In this section, we investigate the impact of the compulsory schooling reform on the disparities in labor market outcomes across ethnic groups. Table 5.4. shows the effects of the compulsory schooling policy on labor market outcomes. Panel A of Table 5.4. presents the estimates for the labor force participation rate (LFPR), Panel B displays the results for the employment, and Panel C shows the estimates for the unemployment.

We find that the reform significantly increases the labor force participation rate of Turkish females by 5 ppt at the optimal bandwidth, 7, as seen in the row of "Reform" in the column (2) of Table 5.4. Our findings also show that prior to 1997 compulsory schooling reform, Kurdish females are less likely to participate in the labor force (see the row of "Ethnicity: Kurdish" in the column (2) of Panel A). However, similar to Turkish females, the reform rises the labor force participation rate of Kurdish females by around 8 ppt (see the row of "Composite Kurdish" of Table 5.4.). As a result, as seen in the last row of Panel A, we show that the reform does not lead to any significant difference in the LFPR between Kurdish and Turkish women, i.e. the interaction term, "Reform*Kurdish" is statistically insignificant. Thus, the gap in the LFPR is not eliminated by the compulsory schooling reform. We do not find any significant impact of the reform on the employment and unemployment outcomes of women belonging to either ethnic group (Panels B and C) at the optimal bandwidth, 7. However, we should note that for the reform seems to increase

the employment of Kurdish women by 5 ppt though the significance level is only at 13.4 percent.

The results regarding the LFPR and employment outcomes of Kurdish women are intriguing as we find a negative impact of the reform on the educational outcomes of Kurdish females. In order to focus on women with low levels of education, we exclude those who have university degree or above from our main analysis sample, and examine the effects of the reform on the labor market outcomes using this sample specification.

For non-college graduate Kurdish females, we find that the reform not only increases the probability of participating in the labor force but also rises the likelihood of the employment at 5 percent significance (the row of "Composite Kurdish" of Table 5.5.).

We should also note that the reform rises the likelihood of being a worker or farmer/agriculturist/stock breeder for Kurdish women, as seen in Table 5.6. These results complement earlier studies that find that children have to drop out of school to work as an unpaid family worker or a paid worker in agriculture (Tunc, 2009; UNICEF, 2011; Adiguzel, 2013; Dayioglu, 2005; Erturk and Dayioglu, 2004).

Table 5. 4. The Effects of the Compulsory Schooling Reform on Labor Market Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| | Panel A: LFPR | | | | | |
| Reform | 0.112*** | 0.050** | 0.027 | 0.121*** | 0.110*** | 0.054*** |
| | (0.024) | (0.025) | (0.028) | (0.016) | (0.017) | (0.014) |
| Ethnicity: Kurdish | -0.065* | -0.089** | -0.102* | 0.004 | 0.002 | 0.007 |
| | (0.038) | (0.044) | (0.056) | (0.014) | (0.016) | (0.015) |
| Reform*Kurdish | -0.062 | 0.030 | 0.059 | -0.082*** | -0.059** | -0.046 |
| | (0.046) | (0.051) | (0.064) | (0.025) | (0.028) | (0.030) |
| Composite Kurdish | 0.051 | 0.080* | 0.086 | 0.040* | 0.052** | 0.008 |
| | (0.038) | (0.043) | (0.055) | (0.021) | (0.025) | (0.027) |
| Observations | 11,862 | 9,465 | 6,744 | 11,936 | 9,485 | 6,755 |
| | Panel B: Employment | | | | | |
| Reform | 0.067*** | 0.028 | 0.029 | 0.073*** | 0.086*** | 0.054*** |
| | (0.021) | (0.024) | (0.027) | (0.014) | (0.016) | (0.020) |
| Ethnicity: Kurdish | -0.081*** | -0.101*** | -0.096** | -0.018 | -0.028 | -0.022 |
| | (0.030) | (0.035) | (0.046) | (0.028) | (0.027) | (0.029) |
| Reform*Kurdish | -0.029 | 0.023 | 0.022 | -0.066* | -0.021 | -0.008 |
| | (0.039) | (0.043) | (0.054) | (0.037) | (0.036) | (0.045) |
| Composite Kurdish | 0.037 | 0.051 | 0.051 | 0.007 | 0.065** | 0.046 |
| | (0.031) | (0.034) | (0.044) | (0.034) | (0.032) | (0.041) |
| Observations | 11,862 | 9,465 | 6,744 | 11,936 | 9,485 | 6,755 |
| | Panel C: Unemployment | | | | | |
| Reform | 0.050** | 0.036 | 0.009 | 0.005 | -0.009 | -0.017 |
| | (0.023) | (0.026) | (0.029) | (0.015) | (0.016) | (0.020) |
| Ethnicity: Kurdish | 0.102 | 0.113 | 0.075 | 0.022 | 0.030 | 0.028 |
| | (0.069) | (0.085) | (0.106) | (0.024) | (0.023) | (0.026) |
| Reform*Kurdish | -0.067 | -0.028 | 0.023 | 0.004 | -0.036 | -0.033 |
| | (0.087) | (0.103) | (0.127) | (0.037) | (0.037) | (0.042) |
| Composite Kurdish | -0.017 | 0.008 | 0.032 | 0.009 | -0.045 | -0.050 |
| | (0.082) | (0.097) | (0.122) | (0.033) | (0.033) | (0.036) |
| Observations | 3,664 | 3,052 | 2,254 | 10,131 | 8,395 | 6,237 |

*Notes*: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Source:* KONDA Barometer, 2014-2017

Table 5. 5. The Effects of Compulsory Schooling Reform on Labor Market Outcomes (Excluding University/Master/PhD Graduates)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| | Panel A: LFPR | | | | | |
| Reform | 0.034 | 0.002 | 0.018 | 0.137*** | 0.125*** | 0.061*** |
| | (0.021) | (0.023) | (0.028) | (0.017) | (0.019) | (0.016) |
| Ethnicity: Kurdish | -0.040 | -0.071** | -0.077** | 0.005 | 0.007 | 0.022 |
| | (0.028) | (0.031) | (0.038) | (0.017) | (0.019) | (0.023) |
| Reform*Kurdish | 0.006 | 0.088** | 0.118** | -0.094*** | -0.066** | -0.043 |
| | (0.042) | (0.042) | (0.051) | (0.029) | (0.033) | (0.034) |
| Composite Kurdish | 0.040 | 0.090** | 0.136*** | 0.043* | 0.059** | 0.018 |
| | (0.036) | (0.037) | (0.045) | (0.024) | (0.029) | (0.030) |
| Observations | 9,473 | 7,433 | 5,229 | 8,587 | 6,604 | 4,567 |
| | Panel B: Employment | | | | | |
| Reform | 0.012 | -0.012 | 0.007 | 0.128*** | 0.133*** | 0.086*** |
| | (0.021) | (0.023) | (0.027) | (0.020) | (0.023) | (0.028) |
| Ethnicity: Kurdish | -0.047* | -0.075*** | -0.069* | -0.017 | -0.023 | -0.001 |
| | (0.025) | (0.029) | (0.035) | (0.034) | (0.036) | (0.041) |
| Reform*Kurdish | 0.031 | 0.088** | 0.114** | -0.093** | -0.041 | -0.010 |
| | (0.038) | (0.041) | (0.049) | (0.041) | (0.045) | (0.055) |
| Composite Kurdish | 0.042 | 0.076** | 0.122*** | 0.035 | 0.091** | 0.075 |
| | (0.032) | (0.034) | (0.040) | (0.034) | (0.038) | (0.046) |
| Observations | 9,473 | 7,433 | 5,229 | 8,587 | 6,604 | 4,567 |
| | Panel C: Unemployment | | | | | |
| Reform | 0.075** | 0.074* | 0.055 | -0.035** | -0.037* | -0.037 |
| | (0.036) | (0.041) | (0.052) | (0.017) | (0.019) | (0.024) |
| Ethnicity: Kurdish | 0.074 | 0.111 | 0.091 | 0.022 | 0.030 | 0.022 |
| | (0.077) | (0.098) | (0.140) | (0.028) | (0.028) | (0.032) |
| Reform*Kurdish | -0.116 | -0.135 | -0.196 | 0.021 | -0.022 | -0.033 |
| | (0.102) | (0.120) | (0.166) | (0.038) | (0.041) | (0.049) |
| Composite Kurdish | -0.041 | -0.062 | -0.141 | -0.014 | -0.059 | -0.070 |
| | (0.094) | (0.109) | (0.155) | (0.033) | (0.036) | (0.042) |
| Observations | 1,952 | 1,548 | 1,099 | 7,298 | 5,901 | 4,266 |

*Notes*: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The 1986 cohort and university/ Master/PhD graduates are excluded from analysis sample. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1

*Source:* KONDA Barometer, 2014-2017

Table 5. 6. The Effects of Compulsory Schooling Reform on the probability of being a worker or farmer/agriculturist/stock breeder

| | Females | | | Males | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| Reform | -0.003 | -0.015 | -0.010 | 0.058*** | 0.053*** | 0.064** |
| | (0.010) | (0.010) | (0.012) | (0.018) | (0.019) | (0.024) |
| Ethnicity: Kurdish | -0.035** | -0.037** | -0.030* | 0.096*** | 0.102*** | 0.130*** |
| | (0.015) | (0.015) | (0.017) | (0.030) | (0.031) | (0.040) |
| Reform*Kurdish | 0.037* | 0.046** | 0.066*** | -0.046 | -0.032 | -0.059 |
| | (0.020) | (0.022) | (0.024) | (0.040) | (0.044) | (0.058) |
| Composite Kurdish | 0.034* | 0.032* | 0.055** | 0.013 | 0.0214 | 0.0044 |
| | (0.017) | (0.019) | (0.020) | (0.036) | (0.039) | (0.051) |
| Observations | 11,862 | 9,465 | 6,744 | 11,936 | 9,485 | 6,755 |

*Notes*: Optimal bandwidth for all females and males are 9 and 7, respectively. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors clustered *at the birth year by the region of birth* are given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1* The sample is restricted to those who receive at least five years of primary school education and at most junior high school education. **The sample is restricted to those who receive at most five years of primary school education.
*Source:* KONDA Barometer, 2014-2017

As we find that the reform significantly increases the educational attainment of Kurdish and Turkish males, we expect that an increase in education level improves the labor market outcomes of both Kurdish and Turkish males. Our results are in line with this argument. The Panel A and B of Table 5.4. shows that an additional three years of schooling rises the likelihood of participating in the labor force and in employment for Turkish and Kurdish males although the ethnic gap in labor force participation increases slightly and the ethnic gap in employment is not eliminated (see the row of "Reform" and "Composite Kurdish", respectively). The reform does not seem to have any significant effect on unemployment of the two ethnic groups. Restricting the sample those who do not hold at least a university degree, we find that the reform rises the likelihood of participating in the

labor force and being employed but reduces the probability of being unemployed for both Turkish and Kurdish males. Although the reform has increased the labor force participation rates of Turkish men more than Kurdish men, it has no impact on the ethnic gap in the employment rate or unemployment rate, (see the row of "Reform*Kurdish" of Table 5.5.), similar to our results with the main sample.

In summary, we find that the education reform significantly rises the probability of participating in the labor force as well as being employed for both Turkish and Kurdish males although the ethnic gap in these outcomes seems to increase. We find that an increase in the years of compulsory schooling rises the labor force participation rate of Turkish and Kurdish females but the ethnic disparities is not eliminated by the reform.

## 5.7. POTENTIAL CHANNELS

We have established that the ethnic gap in the educational outcomes is not eliminated by the reform for Turkish and Kurdish males; while the reform widens the ethnic disparities in the educational outcomes between Turkish and Kurdish females. The reform also seems to increase the labor force participation and employment of both ethnic Kurds and Turks. In this section, we discuss and when data permits empirically examine the potential channels outlined in the conceptual framework section through which the education reform has impacted education and labor market outcomes.

### 5.7.1. SUPPLY OF SCHOOLS

We consider that the inadequate supply of schools in areas where Kurds live may be one of the possible channels that explains adverse effects of the reform on education outcomes of Kurdish females. Table 5.7. shows the result for the differential impact of the reform on ethnic gap in compulsory years of schooling according to region of birth. Specifically, we define a dummy variable East, which takes the value 1 if individual is born in the less developed east or southeast regions of Turkey, and 0 otherwise. We control for this dummy variable and interactions of this dummy variable with Reform and Kurdish. We also include a triple interaction term "Reform*Kurdish*East". In this specification, the row of "Reform" shows the effect of the reform on Turks born in the west and is positive and significant for both females and males. The row "Kurdish" shows the effect of being Kurdish in the West for pre-reform cohorts as opposed to being Turkish and though negative it is statistically not significant. The row "Kurdish*East shows the effect of being Kurdish in the East regions for pre-reform cohorts as opposed to being Turkish in the same region, and it is negative and significant for both females and males, implying that there is an ethnic gap in the East, in completing junior high school for pre-reform cohorts. The row of "Reform*Kurdish" shows the effect of the reform on Kurds born in the West compared to Turks born in the same region and it is negative and significant for females. Furthermore, it is much larger in absolute value terms than the coefficient on "Reform". This means that in the West, while the reform increases the probability of completing junior high school for Turkish females, it decreases completing junior high school for Kurdish females consistent with our results for the overall sample.

Table 5. 7. The Effects of Compulsory Schooling Reform on at Least Junior High School Completion

| | Females | | | Males | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| Reform | 0.091*** | 0.081*** | 0.099*** | 0.054*** | 0.062*** | 0.054*** |
| | (0.024) | (0.028) | (0.034) | (0.014) | (0.016) | (0.020) |
| Kurdish | -0.102* | -0.041 | -0.066 | -0.060 | -0.005 | -0.017 |
| | (0.053) | (0.058) | (0.064) | (0.039) | (0.039) | (0.045) |
| East | -0.179*** | -0.199*** | -0.171*** | -0.072*** | -0.077*** | -0.078*** |
| | (0.021) | (0.022) | (0.026) | (0.020) | (0.022) | (0.028) |
| Reform*Kurdish | -0.243*** | -0.296*** | -0.249*** | -0.039 | -0.106* | -0.149** |
| | (0.065) | (0.074) | (0.083) | (0.049) | (0.054) | (0.059) |
| Reform*East | -0.007 | 0.006 | -0.052 | 0.013 | -0.003 | -0.007 |
| | (0.030) | (0.032) | (0.035) | (0.024) | (0.027) | (0.035) |
| Kurdish*East | -0.085* | -0.093* | -0.116** | -0.089** | -0.125*** | -0.139*** |
| | (0.044) | (0.053) | (0.056) | (0.042) | (0.042) | (0.050) |
| Reform*Kurdish*East | 0.120** | 0.114* | 0.131* | 0.087* | 0.135*** | 0.169*** |
| | (0.056) | (0.067) | (0.073) | (0.050) | (0.051) | (0.059) |
| Composite Kurdish born in East | -0.039 | -0.096* | -0.070 | 0.116*** | 0.088** | 0.066* |
| | (0.043) | (0.053) | (0.061) | (0.034) | (0.036) | (0.039) |
| Composite Kurdish born in West | -0.152** | -0.215*** | -0.150* | 0.016 | -0.044 | -0.096* |
| | (0.062) | (0.071) | (0.080) | (0.047) | (0.052) | (0.056) |
| Observations | 11,862 | 9,465 | 6,744 | 11,936 | 9,485 | 6,755 |

*Notes*: Optimal bandwidth for all females and males are 9 and 7, respectively. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors clustered at the birth year by the region of birth are given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1* The sample is restricted to those who receive at least five years of primary school education and at most junior high school education. **The sample is restricted to those who receive at most five years of primary school education.

*Source:* KONDA Barometer, 2014-2017

The triple interaction term of "Reform*Kurdish*East" shows the additional effect of the reform on Kurds for being born in the East compared to being born in the West. This triple interaction term is positive and significant for both females and males. Hence, if anything the reform has a higher positive impact on the junior high school completion rates of Kurds born in the East compared to Kurds born in the West. This suggests that supply of schools may not be the primary channel that lead to our results that the reform increases ethnic gap in school competition rates for females. We compute the composite effect of the reform for Kurds separately in the East and the West, reported in the rows "Composite Kurdish born in the East" and "Composite Kurdish born in the West", respectively. The reform decreases the likelihood of completing junior high school for Kurdish females born in both regions; however, coefficients in the West are more significant and larger in absolute value terms than coefficient in the East. Hence, rather than regional infrastructure, family structure, financial constraints and social norms that differ between the two ethnic groups may be more relevant explanations for observed outcomes. It is also possible that Kurdish girls born in the West are from families who have migrated from the East, and hence might have experienced additional financial and social difficulties in continuing their education.

### 5.7.2. FAMILY STRUCTURE AND FINANCIAL CONSTRAINTS

In the conceptual framework section, we consider how differences in family structure and financial constraints can cause differential impact of the education reform across ethnic groups. Here, we empirically examine how the family structure and household income varies between Kurds and Turks before the reform and whether the reform has any effects

on the ethnic gap in the family structure. Table 5.8. presents the effects of the compulsory schooling on the ethnic gap in the marital status, household size, and household income. Specifically, we estimate the Equation (1) with marital status, household size and household income as dependent variables in Panels A, B and C, respectively. The control variables are same as in the Equation (1), namely the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, the re-centered birth year differentiated by the ethnicity in the treatment and the control groups. The coefficient of "Kurdish" dummy shows the pre-reform difference of Kurds compared to Turks, the coefficient of "Reform" dummy shows the effect of reform on Turks, and the coefficient on the interaction variable "Kurdish*Reform" shows the differential effect of the reform on Kurds compared to Turks. We also report "Composite Kurdish" which shows the effect of the reform on Kurds; that is, the sum of the coefficient of "Reform" and "Reform*Kurdish".

In the Panel A of Table 5.8., we examine the impact of the education reform on the ethnic gap in the likelihood of being ever-married. Perhaps surprisingly, we find that for pre-reform cohorts, Kurdish women are less likely to be ever-married compared to Turkish women as the coefficient of "Kurdish" dummy is negative and statistically significant (see the row of "Ethnicity: Kurdish" of Panel A). The education reform has no significant impact on the likelihood of being ever-married as shown in the row of "Reform". However, the reform increases the probability of being ever-married for Kurdish women compared to Turkish women as shown by the coefficient of the interaction term "Kurdish*Reform". Furthermore, the composite effect is positive and significant which indicates that the reform increases the probability of being ever-married for Kurdish

women. As a result, our results support the argument discussed in the conceptual framework section that an additional three years of schooling may rise the likelihood of being married as Kurdish women may be more likely to marry when they drop out of the compulsory schooling.

In the Panel B of Table 5.8., we examine the impact of education on the ethnic gap in the household size, i.e. the number of household members. As discussed in our conceptual framework, Kurdish females are more likely to live in larger household as the coefficient of "Kurdish" dummy is positive and statistically significant for pre-reform cohorts. The education reform significantly reduces the number of people per household for Turkish females (see the row of "Reform" in the Panel B). However, the ethnic gap in the household size between Turkish and Kurdish females is not eliminated by the education reform. We also show that the compulsory schooling reform has no impact on the number of individuals living in the same household as seen in the row of "Composite Kurdish". Hence, our findings suggest that larger household size of Kurdish families is likely to be a contributing factor for the non-compliance of Kurdish families in sending their daughters to junior high school. As we discussed in the conceptual framework section, large household size means lower resource per child and more responsibilities at home for daughters.

We examine the impact of the education reform on household income in the Panel C of Table 5.8. For pre-reform cohorts, Kurdish females are less likely to have larger household income compared to Turkish counterparts as the coefficient of "Kurdish" dummy is negative. The coefficient of Kurdish dummy is negative but insignificant for females,

while we have significant and negative impact of the education reform on household income for males. The reason could be measurement error in the outcome variables, household income, as in Turkey, the head of households are commonly males who are the financial provider of family, and have the perfect knowledge about the household income. In addition, Kurdish females may consider telling accurate household income to pollster is not appropriate without their husbands'/fathers' consent. The coefficient of "Reform" dummy implies that the education reform has no impact on the household income for Turkish females, whereas the row of "Composite Kurdish" shows that the reform has a marginally significant impact at the level, %18.6 and reduces household income for Kurdish females. Thus, the disparities in the household income between Turkish and Kurdish females are not eliminated by the education reform (see the row of "Reform*Kurdish"). These results may account for our main findings in which we document that the education reform widens the ethnic gap in education among women. More precisely, as we discussed in the conceptual framework section, Kurdish families may be more financially constrained, and hence not afford children's cost of schooling such as the cost of school uniforms, transportation, meal or school supplies. If expected benefits of sending daughters to school are less than opportunity cost of school, Kurdish females may drop out of the junior high school to work and contribute to their household income.

Table 5. 8. The Effects of the Compulsory Schooling Reform on Family Structure

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| | Panel A: Ever-Married | | | | | |
| Reform | -0.006 | 0.017 | -0.014 | -0.109*** | -0.065* | -0.004 |
| | (0.023) | (0.028) | (0.032) | (0.030) | (0.035) | (0.045) |
| Ethnicity: Kurdish | -0.079* | -0.107* | -0.101 | -0.005 | 0.014 | 0.043 |
| | (0.045) | (0.058) | (0.079) | (0.041) | (0.049) | (0.066) |
| Reform*Kurdish | 0.133** | 0.087 | 0.128 | 0.213*** | 0.193*** | 0.136 |
| | (0.057) | (0.069) | (0.087) | (0.059) | (0.067) | (0.088) |
| Composite Kurdish | 0.126*** | 0.104** | 0.114* | 0.103** | 0.128*** | 0.132** |
| | (0.046) | (0.053) | (0.069) | (0.045) | (0.050) | (0.066) |
| Observations | 5,036 | 3,989 | 2,821 | 4,789 | 3,748 | 2,644 |
| | Panel B: Number of Household members | | | | | |
| Reform | -0.220*** | -0.127** | -0.048 | -0.106 | -0.077 | -0.054 |
| | (0.062) | (0.062) | (0.077) | (0.079) | (0.089) | (0.116) |
| Ethnicity: Kurdish | 0.431*** | 0.495*** | 0.466*** | 0.580*** | 0.630*** | 0.497** |
| | (0.138) | (0.144) | (0.175) | (0.141) | (0.174) | (0.221) |
| Reform*Kurdish | 0.159 | 0.157 | -0.047 | 0.078 | 0.013 | 0.079 |
| | (0.177) | (0.173) | (0.209) | (0.258) | (0.293) | (0.318) |
| Composite Kurdish | -0.061 | 0.030 | -0.095 | -0.029 | -0.064 | 0.026 |
| | (0.163) | (0.159) | (0.177) | (0.240) | (0.268) | (0.274) |
| Observations | 10,345 | 8,264 | 5,883 | 10,317 | 8,184 | 5,831 |
| | Panel C: Household Income* | | | | | |
| Reform | 5.472 | -1.073 | 16.148 | 72.988** | 45.164 | 12.540 |
| | (28.248) | (35.742) | (38.335) | (30.443) | (36.904) | (40.663) |
| Ethnicity: Kurdish | -66.185 | -78.824 | -106.146 | -143.438*** | -214.118*** | -173.178*** |
| | (111.760) | (123.185) | (115.318) | (47.782) | (58.458) | (61.045) |
| Reform*Kurdish | -161.745 | -90.064 | -71.117 | 10.192 | 108.022 | 54.233 |
| | (118.565) | (119.706) | (111.186) | (69.094) | (79.151) | (75.344) |
| Composite Kurdish | -156.3 | -91.14 | -54.97 | 83.18 | 153.2** | 66.77 |
| | (117.8) | (114.3) | (104) | (61.23) | (69.56) | (64.25) |
| Observations | 11,229 | 8,972 | 6,402 | 11,425 | 9,093 | 6,477 |

Notes: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. *Household income is deflated by CPI. The robust standard errors are clustered at the birth year by the region of birth and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* KONDA Barometer, 2014-2017

To summarize, our results show that there exist important pre-reform differences in the family structure of two ethnic groups. Kurdish families are larger and have lower income than their Turkish counterparts. However, the reform does not eliminate the ethnic disparities in the family structure. As a result, we conclude that pre-reform differences in household size and household income may account for our main findings in which we find that the reform widens the ethnic gap in the educational outcomes among ethnic Kurdish and Turkish women. In addition, an additional three years of schooling may rise the likelihood of being ever-married as Kurdish individuals are more likely to marry at earlier ages, and therefore drop out of the compulsory schooling.

### 5.7.3. SOCIAL NORMS

As discussed in the conceptual framework section, the education reform may improve labor market outcomes through its effect on increased human capital due to extended education. Our findings for males and also for Turkish females are consistent with this argument. The reform increases the educational attainment of both Turkish and Kurdish males, and hence rises the likelihood of participating in the labor force as well as being employed.

However, we observe that for Kurdish females, the reform increased the likelihood of participating in the labor force despite the fact that the reform reduced their educational attainment. One explanation for this intriguing result might be that Kurdish girls who drop out of compulsory schooling and start to work in low-skill occupations as unpaid family worker or paid farm or industry worker at earlier ages may continue to keep working on

at later ages. However, it is also possible that a change in the compulsory schooling reform may affect the social norms of individuals. As we discussed in the conceptual framework section, the reform extends three years of secular education and postpones religious education at least by three years. Hence, social norms of both compliers of the reform and non-compliers of the reform may be affected by lack of religious education at the junior high school level. Furthermore, the reform increases the education level of Kurdish males. Since husbands can affect wives labor supply, a change in men's social norms due to increased schooling can also have an impact on female labor supply.

Hence, we investigate how social norms vary between Kurds and Turks for pre-reform cohorts and whether the reform has any impact on social norms of the two ethnic groups by estimating Equation (1) with measures of social norms as dependent variables. Table 5.9. presents these results. The coefficient of "Kurdish" dummy shows the pre-reform difference of Kurds compared to Turks, the coefficient of "Reform" dummy shows the effect of reform on Turks, and the coefficient on the interaction variable "Kurdish*Reform" shows the differential effect of the reform on Kurds compared to Turks. We also report the estimates of "Composite Kurdish" which shows the effect of the reform on Kurds; i.e., the sum of the coefficient of "Reform" and "Reform*Kurdish".

In the Panel A of Table 5.9., our dependent variable, *modern*, is equal to 1 if individual considers herself as modern, and 0 if she considers herself as religious conservative or traditional conservative. We find that prior to the reform, Kurdish women are less likely to consider themselves as modern (see the row of "Ethnicity: Kurdish") compared to Turkish women. However, we find that for both Turkish and Kurdish females, the

education reform rises the probability of considering themselves as modern (see the column (3) of Panel A) though it has no impact on ethnic gap in modernity. Hence, we suggest that the reform rises the probability that women consider themselves modern, and this may increase female labor participation. We do not find any significant differences between Kurdish and Turkish men in terms of modernity for pre-reform cohorts and also, we do not find a significant impact of the reform on modernity of men.

The religious social norms are also crucial determinant of labor/supply decisions of females in Turkey (Akyol & Okten, 2019). Those who do not consider themselves as modern either classify themselves as religious conservative and traditional conservative. We, therefore, restrict our sample those who do not consider themselves as modern, and examine the whether the reform has any impact on the probability of being religious conservative as opposed to being traditional conservative. Our results are displayed in the Panel B of Table 5.9. We find that for pre-reform cohorts, Kurdish females are more likely to consider themselves as religious conservative rather than traditional conservative compared to ethnic Turks (see the row of "Ethnicity: Kurdish" of Panel B). However, the row of "Reform" reports that an additional three years of mandatory schooling has negative but insignificant effect on the probability of being religious conservative for Turkish females. For Kurdish females, on the other hand, we document that the reform has a marginally significant impact at the level, 11.5 % and reduces the probability of considering themselves as religious conservative by 7 ppt at the bandwidth 9 (the row of "Composite Kurdish").

Table 5. 9. The Effects of the Compulsory Schooling Reform on Social Norms

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| | Panel A: Modern | | | | | |
| Reform | 0.029 | 0.033 | 0.049* | 0.011 | 0.011 | -0.007 |
| | (0.018) | (0.022) | (0.027) | (0.017) | (0.020) | (0.024) |
| Ethnicity: Kurdish | -0.072** | -0.081** | -0.138*** | -0.034 | -0.048 | -0.065 |
| | (0.032) | (0.038) | (0.042) | (0.036) | (0.041) | (0.054) |
| Reform*Kurdish | -0.004 | 0.007 | 0.046 | 0.033 | 0.029 | 0.058 |
| | (0.042) | (0.047) | (0.056) | (0.050) | (0.055) | (0.069) |
| Composite Kurdish | 0.026 | 0.041 | 0.095** | 0.044 | 0.040 | 0.052 |
| | (0.037) | (0.040) | (0.046) | (0.046) | (0.050) | (0.063) |
| Observations | 11,188 | 8,930 | 6,355 | 11,312 | 8,996 | 6,404 |
| | Panel B: Religious Conservative | | | | | |
| Reform | -0.025 | -0.015 | -0.030 | -0.049** | -0.068** | -0.081** |
| | (0.027) | (0.030) | (0.037) | (0.022) | (0.026) | (0.031) |
| Ethnicity: Kurdish | 0.115*** | 0.109** | 0.090 | 0.122*** | 0.120** | 0.156*** |
| | (0.043) | (0.049) | (0.063) | (0.041) | (0.046) | (0.052) |
| Reform*Kurdish | -0.045 | -0.043 | 0.009 | -0.064 | -0.000 | -0.043 |
| | (0.051) | (0.057) | (0.071) | (0.052) | (0.063) | (0.071) |
| Composite Kurdish | -0.070 | -0.058 | -0.021 | -0.114** | -0.068 | -0.124** |
| | (0.044) | (0.049) | (0.061) | (0.047) | (0.055) | (0.062) |
| Observations | 7,734 | 6,249 | 4,506 | 7,603 | 6,100 | 4,389 |
| | Panel C: Pious | | | | | |
| Reform | -0.028 | -0.038* | -0.050** | 0.010 | -0.001 | -0.008 |
| | (0.018) | (0.020) | (0.023) | (0.022) | (0.025) | (0.032) |
| Ethnicity: Kurdish | 0.043 | 0.054 | 0.074 | 0.069* | 0.090** | 0.121*** |
| | (0.036) | (0.042) | (0.047) | (0.037) | (0.040) | (0.046) |
| Reform*Kurdish | -0.033 | -0.015 | -0.011 | -0.043 | -0.044 | -0.103* |
| | (0.044) | (0.049) | (0.054) | (0.047) | (0.056) | (0.062) |
| Composite Kurdish | -0.061 | -0.053 | -0.061 | -0.032 | -0.045 | -0.111** |
| | (0.040) | (0.050) | (0.053) | (0.039) | (0.044) | (0.048) |
| Observations | 11,491 | 9,183 | 6,569 | 11,403 | 9,062 | 6,477 |

Notes: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered at the birth year by the region of birth and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* KONDA Barometer, 2014-2017

We get stronger results for males. Prior to the reform, Kurdish men are more likely to consider themselves as religious conservative rather than traditional conservative compared to Turkish men. Furthermore, the reform significantly decreases the probability that Kurdish men consider themselves as religious conservative as shown with the significant coefficient in row of "Composite Kurdish". The results for men are important as husbands and fathers affect female labor supply in conservative societies.

We also show the effects of the education reform on the probability of being pious in the Panel C of Table 5.9. Here, again we use the whole sample and our dependent variable, *pious*, is equal to 1 if individual is a believer and abides or tries to abide by the rules of religion, and 0 if the individual is a believer but does not much abide by the rules of Islam. Similar to our findings in the Panel A and B, prior to the reform, there is a positive but insignificant association between being Kurdish and regarding themselves as pious. However, as seen in the row of "Reform" in the Panel C, the reform significantly reduces the likelihood of being pious for both Turkish and Kurdish females, it has a marginally significant impact at the level, 13 % and diminishes the probability of considering themselves as pious by 6.1 ppt (see the column (1) in the row of "Composite Kurdish"). Similar to Kurdish females, prior to the reform, Kurdish males are more likely to be pious compared to their Turkish counterparts. We also document that the education reform significantly diminishes the likelihood of being pious for Kurdish males (see the row of "Composite Kurdish").

We should also note that our findings in the Panel A, B and C are also consistent with the previous literature which shows that Turkish education reform reduces the religiosity of females (Cesur & Mocan, 2018; Akar & Okten, 2021).

Thus, we conclude that pre-reform differences in the modernity and religious social norms among two ethnic groups may account for our main results in which we show that the reform widens the ethnic gap in the education outcomes among females. In addition to that, the reform mitigates the impact of the religious social norms, and therefore the education reform may increase labor force participation of Kurdish women.

Wearing a headscarf among Muslim women is also considered as a strong indication of religiosity (Cesur & Mocan, 2018). Therefore, we examine the impact of the secular education reform on the likelihood of wearing a headscarf for females. The detailed results are displayed in Table 5.10. As we discussed in the background section, Kurdish women are more likely to wear a headscarf compared to Turkish women for pre-reform cohorts as shown by the positive and significant coefficient in the row of "Kurdish". However, we show that the reform has no impact on the probability of wearing a headscarf for Turkish females (see the row of "Reform") and Kurdish females (see the row of "Composite Kurdish").

Table 5. 10 The Effects of the Compulsory Schooling Reform on the probability of Wearing a Headscarf

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Females | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 |
| Reform | 0.001 | -0.001 | -0.014 |
| | (0.019) | (0.021) | (0.025) |
| Ethnicity: Kurdish | 0.124*** | 0.100** | 0.101* |
| | (0.035) | (0.042) | (0.052) |
| Reform*Kurdish | 0.015 | 0.029 | 0.001 |
| | (0.046) | (0.054) | (0.061) |
| Composite Kurdish | 0.016 | 0.028 | -0.013 |
| | (0.043) | (0.051) | (0.056) |
| Observations | 11,266 | 9,018 | 6,421 |

Notes: Optimal bandwidth for pooled female and male samples are 9 and 7, respectively; and for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered at the birth year by the region of birth and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source:* KONDA Barometer, 2014-2017

Hence, our findings support the idea that the secular education reform which postpones going to the religious schools by at least three years and required that girls attend secular schools where wearing a headscarf is not allowed, may contribute to Kurdish females dropping out of the compulsory junior high school. In addition to wearing a headscarf in schools, there was also a ban on wearing a headscarf until 2013 for university students and public sector employees. Cindoglu (2010) finds that this ban had also effects in the private sector in that women wearing headscarf faced discrimination. Such policies may further discourage women wearing headscarves from getting an education and finding formal employment, and hence dropping out of school and working as unpaid family worker or informal farm and industry workers may remain only viable alternatives for

work for women wearing headscarves and Kurdish women are more likely to be in this group.

### 5.7.4. LANGUAGE OF INSTRUCTION

As we discussed in the conceptual framework section, the instruction of language is the one of the crucial factors that may affect the educational attainment of Kurds, because the instruction of the compulsory education is Turkish in Turkey, and there is no option for ethnic minorities to receive education in their mother tongue. Although the lack of instruction in mother tongue affect the educational outcomes of ethnic Kurds, we find that the compulsory schooling reform, which rises the mandatory education from five to eight years, increases the educational attainment of Kurdish males. In particular, if the lack of instruction in the mother tongue was the primary factor that affected the effectiveness of the compulsory schooling reform and reduced junior high school completion rate of Kurdish females, we would observe the similar negative impact of the reform on Kurdish males. We, therefore, conclude that the lack of instruction in mother tongue does not entirely explain our results for Kurdish females. However, we should mention a potential caveat to this conclusion. Boys spend more time outside of the house and socialize with their peers while girls spend more time at home due to their responsibilities at home and conservative social norms in Kurdish families. Hence, it is possible that boys have more opportunities learning Turkish than girls as result of which they have an easier time at school.

## 5.8. ROBUSTNESS CHECKS

In this section, we also conduct several additional analyses to test the robustness of our results. We first analyze the robustness of our main findings on the educational outcomes. Then, we test the validity of our results on the labor market outcomes.

First of all, as we mentioned before, we exclude those born in 1986 due to the uncertainty in the exposure to the reform. Therefore, to check the validity of our design, we include those born in 1986 and investigate the impact of the compulsory schooling reform on the educational attainment of individuals. This sample specification confirms our main findings in the Table 5.3. (see the Panel A of Table 5.11.). In the conceptual framework, we also argue that Kurdish girls may start to school later than their counterparts. That makes it possible that those born in 1985 were exposed to the compulsory schooling reform. We therefore exclude those born in 1985 and 1986 to check validity of results. Excluding those cohorts also does not change our main findings as seen in the Panel B of Table 5.11. We also consider that those who were born in 1987 were not exposed to the education reform as the compulsory schooling law does not enforce perfect-compliance, and therefore especially Kurdish girl may not be affected by the reform as soon as the law was enacted. We therefore exclude those born in 1985, 1986, and 1987, and shows that these results are consistent with our main findings (see the Panel C of Table 5.11.). Under these sample specifications, we also confirm that our main results regarding the ethnic gap in the labor market outcomes between Turks and Kurds are robust.

To investigate whether our results are due to the change in the compulsory years of education, we conduct placebo test by restricting to the sample to our control group, i.e. those who are not exposed to the compulsory schooling reform as well as limiting to our treatment group, i.e. those who are exposed to the compulsory schooling reform. First of all, we examine the impact of the placebo reform 1, which takes the value 1 if individuals were born between 1982 and 1985, and takes the value 0 if individuals were born between 1977 and 1980. The placebo cut-off 1 is 1981. Our placebo sample consists of those who are not exposed to Turkish compulsory schooling reform in 1997. As seen in the Panel A of Table 5.12., we do not find any impact of the placebo reform 1 on the probability of at least junior high school completion.

Second, we examine the impact of the placebo reform 2, which is equal 1 if individuals were born between 1987 and 1990, while it is equal to 0 if individuals were born between 1992 and 1995. The placebo cut-off 2 is 1991. Our placebo sample consists of those who are exposed to Turkish compulsory schooling reform in 1997. Similar to the placebo reform 1, our findings indicate that the placebo reform 2 has no effects (Panel B of Table 5.12.). As a result, we show that our results are due to the change in the compulsory years of schooling in 1997 in Turkey.

Table 5. 11. The Effects of the Compulsory Schooling Reform on at Least Junior High School

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Females | | | Males | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| Panel A: Including the 1986 birth cohort | | | | | | |
| Reform | 0.091*** | 0.084*** | 0.081*** | 0.043*** | 0.047*** | 0.041*** |
| | (0.017) | (0.018) | (0.020) | (0.011) | (0.013) | (0.015) |
| Ethnicity: Kurdish | -0.185*** | -0.154*** | -0.171*** | -0.108*** | -0.084*** | -0.089*** |
| | (0.029) | (0.034) | (0.032) | (0.026) | (0.024) | (0.025) |
| Reform*Kurdish | -0.126*** | -0.155*** | -0.130** | 0.015 | -0.017 | -0.052 |
| | (0.042) | (0.052) | (0.056) | (0.036) | (0.037) | (0.040) |
| Composite Kurdish | -0.061 | -0.112** | -0.067 | 0.104*** | 0.076** | 0.070** |
| | (0.038) | (0.046) | (0.048) | (0.029) | (0.030) | (0.034) |
| Observations | 12,537 | 10,140 | 7,419 | 12,655 | 10,204 | 7,474 |
| Panel B: Excluding the 1986 and 1985 birth cohort | | | | | | |
| Reform | 0.060*** | 0.045* | 0.047 | 0.059*** | 0.072*** | 0.058** |
| | (0.021) | (0.025) | (0.032) | (0.017) | (0.019) | (0.026) |
| Ethnicity: Kurdish | -0.175*** | -0.091** | -0.120** | -0.157*** | -0.114*** | -0.168*** |
| | (0.038) | (0.042) | (0.048) | (0.034) | (0.038) | (0.052) |
| Reform*Kurdish | -0.138*** | -0.219*** | -0.182*** | 0.062 | 0.012 | 0.025 |
| | (0.047) | (0.056) | (0.067) | (0.042) | (0.046) | (0.058) |
| Composite Kurdish | -0.078* | -0.173*** | -0.134* | 0.122*** | 0.084** | 0.083 |
| | (0.044) | (0.051) | (0.059) | (0.038) | (0.042) | (0.055) |
| Observations | 11,238 | 8,841 | 6,120 | 11,322 | 8,871 | 6,141 |
| Panel C: Excluding the 1985, 1986, and 1987 birth cohort | | | | | | |
| Reform | 0.084*** | 0.070** | 0.072* | 0.062*** | 0.077*** | 0.069** |
| | (0.024) | (0.029) | (0.040) | (0.017) | (0.021) | (0.028) |
| Ethnicity: Kurdish | -0.175*** | -0.089** | -0.118** | -0.157*** | -0.114*** | -0.170*** |
| | (0.038) | (0.042) | (0.047) | (0.034) | (0.037) | (0.052) |
| Reform*Kurdish | -0.151** | -0.248*** | -0.218** | 0.080* | 0.028 | -0.001 |
| | (0.059) | (0.070) | (0.087) | (0.043) | (0.049) | (0.076) |
| Composite Kurdish | -0.067 | -0.178*** | -0.146* | 0.142*** | 0.105** | 0.0674 |
| | (0.056) | (0.065) | (0.078) | (0.040) | (0.046) | (0.073) |
| Observations | 10,448 | 8,051 | 5,330 | 10,583 | 8,132 | 5,402 |

*Notes*: Optimal bandwidth for all females and males are 9 and 7, respectively. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, denomination, the region of residence fixed effects, the birth region fixed effects, decentralized birth year for Turkish and Kurdish individuals in the treatment group, and decentralized birth year for Turkish and Kurdish individuals in the control group. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1*
*Source: KONDA Barometer, 2014-2017*

Table 5. 12. The Effects of the Placebo Compulsory Schooling Reform on at Least Junior High School Completion

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Kurdish Sample | | Turkish Sample | |
| VARIABLES | Females | Males | Females | Males |
| Panel A: Control Group of our main analysis sample (those born in 1985-1977) | | | | |
| Placebo Reform 1 | 0.089 | 0.039 | 0.043 | 0.022 |
| | (0.067) | (0.057) | (0.031) | (0.031) |
| Observations | 805 | 785 | 4,745 | 4,255 |
| Panel B: Treatment Group of our main analysis sample (those born in 1987-1995) | | | | |
| Placebo Reform 2 | 0.042 | -0.011 | 0.017 | 0.014 |
| | (0.046) | (0.037) | (0.024) | (0.016) |
| Observations | 967 | 1,066 | 4,051 | 4,397 |

*Notes*: Panel A shows the impact of the placebo reform 1, which takes the value 1 if individuals born between 1977 and 1980, and takes the value 0 if individuals born between 1982 and 1985. Placebo cut-off 1 is the birth cohort, 1981. None of individuals born between 1977 and 1985 were exposed to Turkish compulsory schooling reform in 1997. Panel B shows the impact of the placebo reform 2, which takes the value 1 if individuals born between 1987 and 1990, and takes the value 0 if individuals born between 1992 and 1995. Placebo cut-off 2 is the birth cohort, 1991. All individuals born between 1987 and 1995 were exposed to Turkish compulsory schooling reform in 1997. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1*

*Source*: KONDA Barometer, 2014-2017

We also examine whether the labor market opportunities of the city that individuals live in generates a differential impact of the reform on the labor market outcomes of Turks and Kurds. For the surveys conducted in 2016 and 2017, Konda Barometer asks questions to the respondents about the current city they live in, and we add the current city fixed effects as a control variable in our regression. We should also note that adding the current city fixed effects reduces our sample size by nearly half; and the small sample size may give us inaccurate estimates of the reform, especially for Kurdish sample. As a result, we show that including current city fixed effects in the regressions lose the significance of the impact of the reform on the labor market outcomes for females; however, our results are robust for males.

To check the comparability of our results with existing literature examining the same education reform on the labor market outcomes, we include all individuals whose ethnic origin other than Kurdish in our main analysis sample, and pool Turks, Kurds, and all other ethnic groups. We then examine the impact of the reform on the labor market outcomes for pooled sample. Table 5.13. reports our results.

We document that the compulsory schooling reform leads to a rise in the LFPR and employment rate, and has no impact on the unemployment rate of females (Panel A), whereas we do not find any effects of the education reform on LFPR and employment rate of non-college graduate women. For males, we show that the reform significantly rises the likelihood of participating in the labor force and being employed but reduces the probability of being unemployed (Panel B). Our results are consistent with the previous literature in some respect. Aydemir and Kirdar (2017) utilize THLFS data set and find that for individuals without university degree, there is no significant impact of the reform on the employment of males, whereas the education reform rises the likelihood of being employed. Torun (2018) also documents that increased education induced by Turkish education reform has no impact on the employment of males and also females; however, it rises the likelihood of being employed in the formal sector for both males and females.

Table 5. 13. The Effects of the Compulsory Schooling Reform on Labor Market Outcomes (For pooled sample (including Turks, Kurds, Zazas, Arabs, and Other Ethnic Groups))

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | LFPR | | Employment | | Unemployment | |
| Variables | All | No-college† graduates | All | No-college† graduates | All | No-college† graduates |
| | | | Panel A: Females | | | |
| Reform | 0.060*** | 0.024 | 0.037* | 0.009 | 0.035 | 0.064* |
| | (0.021) | (0.020) | (0.020) | (0.020) | (0.022) | (0.036) |
| Observations | 10,061 | 7,882 | 10,061 | 7,882 | 3,221 | 1,630 |
| | | | Panel B: Males | | | |
| Reform | 0.097*** | 0.107*** | 0.081*** | 0.120*** | -0.016 | -0.041** |
| | (0.015) | (0.017) | (0.015) | (0.020) | (0.015) | (0.017) |
| Observations | 10,189 | 7,088 | 10,189 | 7,088 | 9,017 | 6,336 |

*Notes*: The sample consists of the individuals born between 1993 and 1979. The 1986 cohort is excluded. †The sample does not contain those who have university/master/ PhD graduates. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, ethnicity, and decentralized birth year for treatment and the control groups. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source: KONDA Barometer, 2014-2017*

Finally, we separate the pooled males and females sample into two subsamples according to individuals' ethnic origin, namely Kurdish and Turkish samples; and examine the effects of the compulsory schooling reform on the educational attainment and labor market outcomes, separately for males and females. Dividing the pooled sample into subsamples also confirms our main results.

Although our aim is not to investigate the impact of the reform on the gender gap for Turks and Kurds, we examine and document that for ethnic Turks, the education reform has no impact on the gender gap in the educational outcomes, while for ethnic Kurds, we observe that the reform widens the gap in the educational outcomes. Our findings for Turkish individuals are consistent with Kirdar et al. (2015). In addition, we find that the reform

widens the gender gap in the labor force participation rates of Turkish females and males but no impact on the employment as well as the unemployment rate. On the other hand, for Kurdish individuals, we document that the gender gap in the labor market outcomes is not eliminated by the reform. Detailed results are shown in the Appendix B.

## 5.9. CONCLUSION

In this paper, we examine the differential impact of the compulsory schooling reform enacted in 1997 in Turkey across ethnic Turkish majority and ethnic Kurds who form the largest ethnic minority group. In particular, using a novel data set from the 2014-2017 Konda Barometer, we investigate the effects of the exogenous compulsory education reform, which rises the mandatory years of schooling from five to eight years in Turkey, on the ethnic gap in the educational attainment as well as labor market outcomes.

One of the interesting findings of our paper is that the education reform widens the ethnic gap in the educational outcomes between Turkish and Kurdish females. The reform rises the probability of completing at compulsory years of schooling (at least eight years of schooling) for Turkish females; nevertheless, it significantly reduces the likelihood of receiving at least eight years of schooling for Kurdish females. On the other hand, we show that the exogenous education reform boosts the likelihood of receiving at least compulsory years of schooling for both Turkish and Kurdish males. However, for males, the reform has no impact on the ethnic gap in the educational outcomes. Another important finding is that the reform generates a spillover effects and increase the educational attainment of individuals beyond the compulsory schooling for both Turkish and Kurdish

males. However, for females, such spillover effects only exist for Turkish females and the ethnic gap widens beyond the compulsory schooling level between ethnic Turkish and Kurdish females.

We consider that if the education reform rises the educational attainment of individuals, an increase in the human capital through extended schooling may lead to an improvement in the labor market outcomes. As expected, for men, the education reform significantly rises the probability of participating in the labor force as well as being employed. Nevertheless, the reform leads Turkish males to be more likely to participate in labor force compared to Kurdish males, and thus it widens the ethnic gap in the labor force participation between two ethnic minority groups. For Turkish females, we also find that an increase in the compulsory years of schooling increases the LFPR consistent with increased human capital due to the reform. However, for Kurdish females, we document that the education reform rises the probability of participating in the labor force even though the reform reduces the educational attainment of Kurdish females.

We also discuss and empirically examine the possible channels that give rise to our findings. Kurds have lower income, larger household size and live in less developed areas that are more remote to junior high schools compared to ethnic Turks, and hence face higher opportunity cost of sending their offspring to junior high school. Especially for girls, these opportunity costs may be far greater than expected benefits from extra schooling, since daughters tend to take care of younger siblings in large Kurdish families. Furthermore, Kurds are less likely to be modern and more likely to be religious and conservative compared to ethnic Turks, and hence sending daughters to far away schools

or boarding schools can have high psychic costs for Kurdish families. In addition, compulsory schooling abolishes religious junior high schools where girls are allowed to wear headscarves and make secular junior high schools compulsory, where girls are not allowed to wear headscarves. Kurdish girls are more likely to wear headscarf than their Turkish counterparts, and this might be yet another social factor decreasing probability of complying with the reform for Kurdish females. We investigate the effect of the reform on modernity and religious conservativeness, and find that reform increases likelihood to be modern and reduces religious conservativeness for both ethnic Kurds and Turks. This might be a relevant channel for increased labor force participation of Kurdish females despite the fact that reform reduces their education levels both at the compulsory and post-compulsory level. We also consider that the differences in labor market opportunities and preferences may account for the results that the education reform widens the ethnic gap in the labor force participation between Turkish and Kurdish males.

Overall, our findings have important consequences for the countries in which persistent differences in the family structure and social norms exist across ethnic groups. From a policy perspective, an increase in years of mandatory schooling has not been effective policy for improving the educational attainment of ethnic minority women in Turkey. Turkish government takes additional steps. In 2003-2004 school year, government starts providing school books for free in order to ease financial constraints by families. In 2004, an initiative to encourage families to send daughters to school has been implemented. In hindsight, this might be a compensating policy intervention, since policymakers may have recognized that the compulsory school reform fails to achieve the intended objective in poor areas, where families have severe financial constraints and conservative social norms

that result in non-compliance with compulsory schooling reform. Hence, we also contribute to the literature that points out that the effectiveness of the compulsory education reform depends on the degree of the enforcement (Margo & Finegan 1996; Murtin & Viarengo, 2011; Gayoso de Ervin, 2020) by suggesting that the financial and social constraints faced by families also need to be addressed in any extensive compulsory education reform.

# CHAPTER 6

# CONCLUSION

In this thesis, we have examined the impact of a compulsory schooling reform, which increases the mandatory years of schooling from five to eight years in 1997 in Turkey, on education, labor market, and time use outcomes. In line with the previous literature that estimates the local average treatment effects of compulsory schooling, we show that Turkish compulsory schooling reform rises the likelihood of completing at least compulsory years of schooling. Examining the heterogeneous impact of the education reform, we also find that the reform widens the ethnic gap in the compulsory years of schooling between Kurdish and Turkish females, while the compulsory schooling reform increases junior high school completion rates of both Turkish and Kurdish men equally without having an impact on the ethnic gap. In addition, for Turks, the compulsory schooling reform generates strong spillover effects on the post-compulsory schooling. On the other hand, focusing on a sample of mothers, which is a rather select group in terms of their selection into educational attainment, we also show that the compulsory education

reform increases mothers' completion of at least compulsory years of schooling; however, it has no significant impact on the post-compulsory education.

This thesis is the first study that provides a causal evidence of education on voluntary work at the extensive margin as well as intensive margin by utilizing the data from a nationally representative Turkish Time Use Survey and using Turkish compulsory schooling reform as instrument for education. Consistent with the existing literature, we establish a positive and significant association between schooling and men's voluntary work. However, increased education of compliers has a negative but insignificant causal impact on the probability and hours of voluntary work for men when we utilize the education reform as an instrument for education. Our results suggest that omitted variable bias and reverse causality are likely to have played a role in the positive association of education with voluntary work found in OLS studies.

Examining the intent-to-treat impact of Turkish compulsory schooling reform on early childhood care of mothers by utilizing Turkish Time Use Survey data, we find that an additional three years of schooling has no significant effect on time spent in reading to children, playing with children, and talking to children. However, our reduced form regression results differ from OLS studies, which documents that there is a positive association between maternal education and time spent in early childhood care activities. Consistent with our argument to account for insignificant and negative impact of education on voluntary work of individuals, we suggest that such correlation may result from omitted variable bias due to ability and social values and norms affecting both maternal education and the quality of time allocated to early childhood care activities.

In this thesis, we also provide a first causal evidence on the impact of education on the religiosity of individuals at intensive margin, namely time spent in religious activities. Although we do not find any impact of the change in mandatory years of schooling in Turkey on voluntary work of individuals and early childhood care of mothers by utilizing the data from Turkish Time Use Survey, we document that for females, increased education has a negative and significant causal impact on time spent in religious activities on weekdays as well as weekend. Hence, we suggest that increased secular education may shape females' religious behavior through its effects on preferences as the compulsory schooling reform extended mandatory secular education from five to eight years, and thereby postponed possibility of religious education by three years.

Utilizing the data from Turkish Time Use Survey, we also document that increased education induced by Turkish compulsory schooling reform has no significant causal impact on the probability of being employed and weekly hours worked of individuals. However, for males, education rises the likelihood of earning higher than minimum wage. The TUS and Turkish HLFS does not include any information on the ethnic identity of individuals. Therefore, in this thesis, we use a novel data set from the nationally representative Konda Barometer conducted from 2014 to 2017, which provides unique information about labor market outcomes by ethnicity to investigate the effects of the compulsory schooling reform on the ethnic gap in labor market outcomes between ethnic Kurds and Turks. To best of our knowledge, this essay is the first that provides an evidence on the impact of a compulsory schooling reform on the ethnic gap in post-compulsory education and labor market outcomes. We document that the compulsory schooling

reform significantly increases the probability of participating in the labor force as well as being employed of both Turkish and Kurdish men despite the fact that the ethnic gap in labor market outcomes widens between the two groups. The labor market discrimination against ethnic groups could account for these results which will be addressed in the future work. For women, an increase in compulsory schooling rises the likelihood of participating in the labor force of both Turkish and Kurdish females but ethnic disparities are not closed by the reform. The reform has an impact on social norms and decreases conservativeness and piousness for both Kurds and Turks, which may also explain increased labor force participation of women as well as an increase in their human capital. The potential limitations of our results for the ethnic groups is that utilizing the data from the Konda survey may generate a larger impact of the education reform on the labor market outcomes of ethnic groups as the classification of the status in employment in the Konda survey does not match with the classification in Turkish HLFS or TUS.

Overall, policies extending the compulsory schooling may not be an effective policy instrument to raise voluntary work of individuals or the quality of early childhood care provided by mothers; although it increases the educational attainment of individuals and has an impact on different outcome variables such as drop-out decisions (Oreopoulos, 2006; Oreopoulos, 2007; Caner, Guven, Okten & Sakalli, 2016), civic and religious behavior outcomes (Dee, 2004; Milligan, Moretti & Oreopoulos, 2004; Cesur & Mocan, 2018), and health outcomes (Cesur, Dursun, & Mocan, 2018; Kırdar, Dayıoğlu, & Koç, 2018). In addition, the extension of three years at the secondary school level may not have an impact on the voluntary work of individuals and early child care of mothers while increased education at higher levels such as high school and tertiary education attainment

may affect these individual level outcomes. Therefore, it would be interesting future research topic to examine the impact of a compulsory schooling reform enacted in Turkey in 2012, which integrates high school education into compulsory education, on voluntary work of individuals as well as early child care of mothers. In addition, an increase in years of mandatory schooling has also not been effective policy for improving the educational attainment of ethnic minority women in Turkey. We provide suggestive evidence that the financial and social constraints faced by Kurdish families are likely to be responsible for ineffectiveness of the reform on education outcomes of Kurdish females. Hence, public policies need to take into account financial and social constraints that might limit their effectiveness.

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# APPENDICES

## APPENDIX A: ADDITIONAL RESULTS FOR CHAPTER 2

Table A. 1. Voluntary Work

| **Organization-Based Volunteering Activities** (*working for volunteer groups*) | **Direct Volunteering Activities (*Helping*)** (*helping to someone else outside the household*) |
|---|---|
| • social welfare groups<br>• sports clubs<br>• political groups and clubs<br>• youth clubs<br>• security/first aid groups<br>• environmental groups<br>• justice/human right groups<br>• local community associations and regional solidarity groups<br>• art and hobby groups<br>• professional solidarity associations<br>• school council<br>• adult education groups<br>• helping a place of worship (mosque building, cleaning, repair etc.)<br>• other unspecified voluntary activities | • preparing food<br>• cleaning home<br>• washing or ironing clothes<br>• gardening<br>• caring for pet<br>• caring for child<br>• caring for disabled patient<br>• paying bills<br>• transporting a child to school, kinder garden, etc.<br>• transporting an adult to shopping center, hospital, etc.<br>• doing one's shopping<br>• repairing house<br>• repairing home appliance<br>• repairing or fixing furniture<br>• washing car<br>• providing educational training<br>• doing health related activities (blood pressure, measuring sugar, needless, etc.)<br>• other unspecified helping activities |

Table A. 2. The Definition of Variables

| VARIABLE | DEFINITION |
|---|---|
| Reform | A dummy variable that takes the value 1 if the respondent was born after 1986, and 0 otherwise. |
| Birth $Year_{tre}$ | A control variable that is jointly determined by the re-centered birth year (birth year-1986) and the dummy variable, Reform, that takes the value 1 for the respondent in the treatment group. |
| Birth $Year_{cont}$ | A control variable that is jointly determined by the re-centered birth year (birth year-1986) and the dummy variable, Reform, that takes the value 0 for the respondent in the control group. |
| Marital Status | A dummy variable that takes the value 1 if the respondent is single, i.e., the respondent has never been married or the respondent is divorced or widowed, and 0otherwise. |
| Middle School | A dummy variable that takes the value 1 if the respondent completes at least middle school; i.e. at least eight years of schooling, and 0otherwise. |
| High School | A dummy variable that takes the value 1 if the respondent completes at least high school, i.e. at least twelve years of schooling, and 0 otherwise. |
| University Completion | A dummy variable that takes the value 1 if the respondent completes at least university, i.e. at least sixteen years of schooling, and 0 otherwise. |
| Voluntary Activity Participation | A dummy variable that takes the value 1 if the respondent attends at least one voluntary activity among thirty-two activities in the last four weeks, and 0otherwise. |
| Number of Voluntary Activities | An outcome variable that represents how many voluntary activities the respondent attends in the last four weeks. It is calculated by summing up the value of thirty-two dummy variables for each voluntary activity attended by the respondent in the last four weeks. |
| Frequency of Voluntary Activities | An outcome variable that shows how often respondent attends voluntary activities in the last four weeks. It is defined as total frequency of each voluntary activity attended by the respondent in the last four weeks. |
| Time Spent in Voluntary Activities | An outcome variable that represents how much time the respondent spends while participating in the voluntary activities in the last four weeks. It is calculated by summing up the time spent in each voluntary activity attended by the respondent in the last four weeks. |
| Voluntary Activity Number Index | A z-score constructed by averaging the z-scores of thirty-two dummy variables for each voluntary activity attended by the respondent in the last four weeks. |
| Voluntary Activity Frequency Index | A z-score constructed by averaging the z-scores of the frequency of each voluntary activity attended by the respondent in the last four weeks. |

| | |
|---|---|
| Voluntary Activity Time Index | A z-score constructed by averaging the z-scores of time spent in each voluntary activity attended by the respondent in the last four weeks. |
| Organization-Based Volunteering Participation | A dummy variable that takes the value 1 if the respondent attends at least one organization-based volunteering activity among fourteen activities in the last four weeks, and 0 otherwise. |
| Number of Organization-Based Volunteering Activities | An outcome variable that represents how many organization-based volunteering activities the respondent attends in the last four weeks. It is calculated by summing up the value of fourteen dummy variables for each organization-based volunteering activity attended by the respondent in the last four weeks. |
| Frequency of Organization-Based Volunteering Activities | An outcome variable that shows how often respondent attends organization-based volunteering activities in the last four weeks. It is defined as total frequency of each organization-based volunteering activity attended by the respondent in the last four weeks. |
| Time Spent in Organization-Based Volunteering Activity | An outcome variable that represents how much time the respondent spends while participating in organization-based volunteering activities in the last four weeks. It is calculated by summing up time spent in each organization-based volunteering activity attended by the respondent in the last four weeks. |
| Direct Volunteering Activity Participation | A dummy variable that takes the value 1 if the respondent attends at least one helping activity among eighteen helping activities in the last four weeks, and 0 otherwise. |
| Number of Direct Volunteering Activities | An outcome variable that represents how many helping activities the respondent attends in the last four weeks. It is calculated by summing up the value of eighteen dummy variables for each voluntary activity attended by the respondent in the last four weeks. |
| Frequency of Direct Volunteering Activities | An outcome variable that shows how often respondent attends helping activities in the last four weeks. It is defined as the total frequency of each helping activity attended by the respondent in the last four weeks. |
| Time Spent in Direct Volunteering Activities | An outcome variable that represents how much time the respondent spends while participating in helping activities in the last four weeks. It is calculated by summing up time spent in each helping activity attended by the respondent in the last four weeks. |
| Employment | A dummy variable that takes the value 1 if the respondent works for one hour with/without receiving payment during the last week. |
| Weekly Hours Worked | An outcome variable that represents monthly/daily paid respondent's total hours worked in a week at the main job. |

| | |
|---|---|
| Higher than Minimum Wage | A dummy variable that takes the value 1 if monthly/daily paid respondent's wage group is in the higher wage groups, and it takes the value 0 if monthly/daily paid respondents' wage group is in the lowest wage group. High-Wage group consists of the wage group 2, 3, 4, and 5. Low-Wage group is the lowest wage group, i.e., the wage group 1. : Wage group 1: 0-1080 Turkish Liras, (0-397$), Wage group 2: 1081-1550 Turkish Liras (397$-570$), Wage Group 3: 1551-2170 Turkish Liras, (570$-798$), Wage Group 4: 2171-3180 Turkish Liras (798$-1169$), and Wage Group 5: 3181 Turkish Liras and higher (1169$, and higher). Minimum wage in 2014 and 2015 are 891.04 Turkish Liras, (327.5 $), and 1,000.55 Turkish Liras, (367.65$), respectively. |

Table A. 3. Exposure to 1997 Compulsory Schooling Law in the 2014-2015 Survey Years

| Exposure to the Law | Year of Birth | Age in 2014 | Age in 2015 |
|---|---|---|---|
| Yes | 1993 | 21 | 22 |
| Yes | 1992 | 22 | 23 |
| Yes | 1991 | 23 | 24 |
| Yes | 1990 | 24 | 25 |
| Yes | 1989 | 25 | 26 |
| Yes | 1988 | 26 | 27 |
| Yes | 1987 | 27 | 28 |
| Uncertain | 1986 | 28 | 29 |
| No | 1985 | 29 | 30 |
| No | 1984 | 30 | 31 |
| No | 1983 | 31 | 32 |
| No | 1982 | 32 | 33 |
| No | 1981 | 33 | 34 |
| No | 1980 | 34 | 35 |
| No | 1979 | 35 | 36 |

Table A. 4. Descriptive Statistics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Males | | Females | |
| Variables | Treatment | Control | Treatment | Control |
| PANEL A: Organization-Based Volunteering Activity | | | | |
| Time Spent in Activities | 1.754 | 3.191 | 2.348 | 2.118 |
| | (13.41) | (20.80) | (18.65) | (17.31) |
| Frequency of Activities | 0.0581 | 0.0798 | 0.0321 | 0.0407 |
| | (0.398) | (0.521) | (0.241) | (0.305) |
| Number of Activities | 0.0302 | 0.0412 | 0.0228 | 0.0228 |
| | (0.176) | (0.212) | (0.163) | (0.157) |
| Participation | 0.0294 | 0.0386 | 0.0207 | 0.0216 |
| | (0.169) | (0.193) | (0.142) | (0.145) |
| PANEL B: Direct Volunteering (*Helping*) Activity | | | | |
| Time Spent in Activities | 52.32 | 64.18 | 137.2 | 172.7 |
| | (217.7) | (247.2) | (362.2) | (426.5) |
| Frequency of Activities | 0.964 | 1.119 | 2.582 | 3.015 |
| | (3.729) | (3.820) | (7.070) | (7.461) |
| Number of Activities | 0.314 | 0.381 | 0.630 | 0.746 |
| | (0.882) | (0.972) | (1.242) | (1.298) |
| Participation | 0.167 | 0.201 | 0.287 | 0.343 |
| | (0.373) | (0.401) | (0.452) | (0.475) |
| Observations | 1,291 | 1,529 | 1,401 | 1,669 |

*Notes*: Table shows the mean, standard deviation, and the number of observations from the Individual Questionnaire of 2014-2015 Turkish Time Use Survey data. Column (1) and (2) report the results for males and column (4) and (5) displays the results for females. The treatment group consists of individuals born between 1987 and 1993, and the control group consists of those born between 1979 and 1985. The 1986 cohort is excluded. Standard deviations are in the parenthesis.
Source: The Individual Questionnaire of 2014-2015 Turkish Time Use Survey, TURKSTAT

# APPENDIX B: ADDITIONAL RESULTS FOR CHAPTER 5

In this section, we examine whether the mandatory education reform, which increases the compulsory years of schooling from five to eight years, closes the gender gap in the educational as well as labor market outcomes in Turkish and Kurdish sample, separately. We first present the impact of the reform on the gender gap in the educational attainment. Then, we show the effects of the compulsory education reform on the gender gap in the labor market outcomes such as the labor force participation rate, employment, and unemployment rates.

## B.1. Gender Gap in Educational Outcomes

We first investigate the impact of the education reform on the probability of completing at least junior high school, i.e. at least eight years of schooling and present the results in Table B.1. The Panel A/B/C shows the impact of the reform on the gender gap in at least junior high school completion/high school completion/ university completion. The column (1), (2), and (3) displays the results for Kurdish individuals, the column (4), (5), and (6) presents the findings for Turkish individuals.

As seen in the "Reform" row of Panel A, for Kurdish males, the reform significantly increases the probability of completing at least junior high school by around 7 ppt, whereas for Kurdish females, it reduces the likelihood of obtaining at least junior high school diploma by around 12 ppt as seen in the row of "Composite Females". At pre-

reform period, there is also negative relation between being females and at least middle school completion rate as the estimates for "Female" dummy is statistically significant and negative across all estimation window. In other words, the change in the compulsory schooling reform has benefited Kurdish males more than Kurdish females. Therefore, the gender gap in the probability of completing at least junior high is not eliminated by the reform, in fact the compulsory schooling reform widens the gender gap in the education outcomes. On the other hand, for Turks, we do not find any evidence for supporting the idea that the reform has positive effects on females rather than on males. In particular, the estimates for the coefficient of the interaction term implies that the impact of the reform is statistically same for females and males as the coefficient of the interaction term is statistically insignificant (see the column (4) -(6) of Panel A). Our findings are consistent with Kırdar et al. 2015.

As we find that there is a spillover effect of the reform on the post-compulsory schooling level, we examine the impact of the reform on the gender gap in at least high school completion rate as well as at least college completion rate (see the Panel B and C of Table B.1.). For Turkish individuals, the reform does not generate any differences in at least high school completion rate or at least college completion rate. On the other hand, for Kurdish individuals, the reform widens the gender gap in the at least high school completion rate but the gender gap is not reduced with the reform.

Table B. 1. The Effects of Compulsory Schooling Reform on the Gender Gap in the Educational Attainment

| | Kurdish Sample | | | Turkish Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| PANEL A: At Least Junior High School Completion | | | | | | |
| Reform | 0.097*** | 0.066* | 0.041 | 0.055*** | 0.063*** | 0.054** |
| | (0.032) | (0.035) | (0.045) | (0.015) | (0.018) | (0.021) |
| Female | -0.246*** | -0.232*** | -0.265*** | -0.158*** | -0.158*** | -0.190*** |
| | (0.040) | (0.048) | (0.048) | (0.023) | (0.026) | (0.031) |
| Reform*Female | -0.164*** | -0.193*** | -0.126 | 0.030 | 0.015 | 0.034 |
| | (0.059) | (0.071) | (0.080) | (0.030) | (0.034) | (0.040) |
| Composite Females | -0.068 | -0.127** | -0.085 | 0.085*** | 0.077*** | 0.089*** |
| | (0.042) | (0.052) | (0.057) | (0.021) | (0.024) | (0.027) |
| Observations | 4,096 | 3,301 | 2,363 | 19,702 | 15,649 | 11,136 |
| PANEL B: At Least High School Completion | | | | | | |
| Reform | 0.092** | 0.077* | 0.011 | 0.063*** | 0.058*** | 0.053** |
| | (0.039) | (0.045) | (0.047) | (0.018) | (0.020) | (0.025) |
| Female | -0.211*** | -0.194*** | -0.278*** | -0.198*** | -0.206*** | -0.220*** |
| | (0.033) | (0.047) | (0.039) | (0.024) | (0.025) | (0.033) |
| Reform*Female | -0.131*** | -0.152** | -0.073 | -0.007 | -0.018 | -0.009 |
| | (0.047) | (0.059) | (0.063) | (0.032) | (0.036) | (0.046) |
| Composite Females | -0.034 | -0.075* | -0.062 | 0.057** | 0.041 | 0.046 |
| | (0.033) | (0.039) | (0.046) | (0.023) | (0.025) | (0.030) |
| Observations | 4,096 | 3,301 | 2,363 | 19,702 | 15,649 | 11,136 |
| PANEL C: At Least College Graduate | | | | | | |
| Reform | 0.068 | 0.024 | -0.033 | 0.133*** | 0.083*** | 0.042 |
| | (0.041) | (0.038) | (0.042) | (0.024) | (0.026) | (0.028) |
| Female | -0.128*** | -0.121*** | -0.145*** | -0.115*** | -0.122*** | -0.101*** |
| | (0.033) | (0.039) | (0.047) | (0.017) | (0.021) | (0.026) |
| Reform*Female | -0.050 | -0.021 | 0.024 | -0.017 | -0.014 | -0.026 |
| | (0.039) | (0.045) | (0.059) | (0.026) | (0.031) | (0.038) |
| Composite Females | 0.018 | 0.004 | -0.009 | 0.116*** | 0.069*** | 0.016 |
| | (0.038) | (0.043) | (0.052) | (0.023) | (0.024) | (0.026) |
| Observations | 4,096 | 3,301 | 2,363 | 19,702 | 15,649 | 11,136 |

*Notes*: Optimal bandwidth for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, decentralized birth year differentiated by the gender in the treatment group, and decentralized birth year differentiated by the gender in the control group. The robust standard errors clustered at the birth year by the region of birth are given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1*

*Source: KONDA Barometer, 2014-2017*

## B.2. Gender Gap in Labor Market Outcomes

We then examine the effects of the reform on the gender gap in the labor market outcomes of individuals and report our findings in Table B.2. Panel A/B/C shows the impact of the reform on the labor force participation rate/employment/unemployment, respectively. The column (1), (2), and (3) displays the results for Kurdish individuals, the column (4), (5), and (6) presents the findings for Turkish individuals.

We first document that the education reform does not generate any significant differences in labor market outcomes of Kurdish males and females as the estimates for the interaction term, "Reform*Female" is statistically insignificant at all estimation window (Panel A, B, and C of Table B.2.). However, for Turkish individuals, we show that the reform leads Turkish males to be more likely to participate in labor force (i.e. the coefficient of the interaction term is statistically significant and negative at the optimal bandwidth). Moreover, we estimate the effects of the reform on the probability of the employment and unemployment, and our findings suggest that the effects of the reform are statistically same across genders.

Table B. 2. The Effects of the Compulsory Schooling Reform on the Gender Gap in Labor Market Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Kurdish Sample | | | Turkish Sample | | |
| Cohorts | 1995-1977 | 1993-1979 | 1991-1981 | 1995-1977 | 1993-1979 | 1991-1981 |
| Bandwidths | 9 | 7 | 5 | 9 | 7 | 5 |
| PANEL A: LFPR | | | | | | |
| Reform | 0.040* | 0.047* | 0.004 | 0.121*** | 0.111*** | 0.057*** |
| | (0.020) | (0.025) | (0.029) | (0.017) | (0.021) | (0.020) |
| Female | -0.781*** | -0.791*** | -0.816*** | -0.639*** | -0.622*** | -0.635*** |
| | (0.034) | (0.039) | (0.048) | (0.019) | (0.023) | (0.029) |
| Reform*Female | 0.005 | 0.022 | 0.088 | -0.012 | -0.064* | -0.032 |
| | (0.045) | (0.051) | (0.074) | (0.029) | (0.035) | (0.041) |
| Composite Females | 0.045 | 0.069 | 0.092 | 0.109*** | 0.047* | 0.025 |
| | (0.041) | (0.048) | (0.060) | (0.025) | (0.027) | (0.030) |
| Observations | 4,096 | 3,301 | 2,363 | 19,702 | 15,649 | 11,136 |
| PANEL B: Employment | | | | | | |
| Reform | 0.009 | 0.063* | 0.046 | 0.072*** | 0.086*** | 0.058** |
| | (0.037) | (0.035) | (0.045) | (0.017) | (0.021) | (0.026) |
| Female | -0.728*** | -0.723*** | -0.736*** | -0.601*** | -0.584*** | -0.599*** |
| | (0.035) | (0.041) | (0.050) | (0.021) | (0.024) | (0.029) |
| Reform*Female | 0.025 | -0.020 | 0.010 | -0.008 | -0.060 | -0.030 |
| | (0.052) | (0.054) | (0.073) | (0.032) | (0.037) | (0.045) |
| Composite Females | 0.034 | 0.043 | 0.056 | 0.064*** | 0.026 | 0.029 |
| | (0.034) | (0.038) | (0.049) | (0.023) | (0.026) | (0.030) |
| Observations | 4,096 | 3,301 | 2,363 | 19,702 | 15,649 | 11,136 |
| PANEL C: Unemployment | | | | | | |
| Reform | -0.002 | -0.052 | -0.056 | 0.004 | -0.010 | -0.018 |
| | (0.036) | (0.035) | (0.040) | (0.015) | (0.017) | (0.021) |
| Female | 0.131* | 0.138 | 0.103 | 0.016 | 0.020 | 0.023 |
| | (0.079) | (0.093) | (0.115) | (0.016) | (0.018) | (0.022) |
| Reform*Female | -0.006 | 0.061 | 0.095 | 0.047 | 0.049 | 0.025 |
| | (0.102) | (0.115) | (0.145) | (0.031) | (0.033) | (0.039) |
| Composite Females | -0.008 | 0.008 | 0.039 | 0.051** | 0.039 | 0.006 |
| | (0.089) | (0.107) | (0.136) | (0.024) | (0.026) | (0.029) |
| Observations | 2,194 | 1,835 | 1,369 | 11,601 | 9,612 | 7,122 |

*Notes*: Optimal bandwidth for both Kurdish and Turkish samples is 7. The 1986 cohort is excluded. The control variables include the survey month fixed effects, the survey year fixed effects, the type of residence, the region of residence fixed effects, the birth region fixed effects, denomination, decentralized birth year differentiated by the gender in the treatment group, and decentralized birth year differentiated by the gender in the control group. The robust standard errors are clustered *at the birth year by the region of birth* and given in the parentheses. *** p<0.01, ** p<0.05, * p<0.1.
*Source: KONDA Barometer, 2014-2017*