

EFFECTS OF EDUCATION ON LABOR MARKET OUTCOMES:
QUASI-EXPERIMENTAL EVIDENCE FROM THE 1997 TURKISH COMPULSORY
SCHOOLING REFORM

A THESIS SUBMITTED TO
THE GRADUATE SCHOOL OF SOCIAL SCIENCES
OF
MIDDLE EAST TECHNICAL UNIVERSITY

BY

CANKUTCEM ÜNAL

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR
THE DEGREE OF MASTER OF SCIENCE
IN
THE DEPARTMENT OF ECONOMICS

OCTOBER 2023

Approval of the thesis:

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SCHOOLING REFORM**

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ABSTRACT

EFFECTS OF EDUCATION ON LABOR MARKET OUTCOMES: QUASI-EXPERIMENTAL EVIDENCE FROM THE 1997 TURKISH COMPULSORY SCHOOLING REFORM

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October 2023, 82 pages

This thesis investigates the impact of the 1997 Turkish Compulsory Schooling Law (CSL) on educational attainment and subsequent labor market outcomes. Using data from the Household Labor Force Survey (HLFS) spanning 2007-2022, the analysis focuses on Turkish individuals aged 25-35. The causal inference method employed is the Instrumental Variable (IV) approach. Results indicate that the CSL policy significantly bolstered 8th-grade (middle school) degree attainment for both genders. Additionally, a spillover effect was observed, leading to increased high school degree attainment exclusively for women. The return on education manifested distinctly between genders. Women experienced a 10% and 15% rise in monthly and hourly wages, respectively, whereas men saw a statistically insignificant 7% rise in both monthly and hourly rates. Additionally, women's monthly work hours decreased with the additional year of schooling. Education also increased their likelihood of being wage workers, being employed in the public sector, having social security, and working in larger companies. Conversely, their

chances of being unpaid family workers diminished. All these employment transformations were not observed in men. This research underscores the multifaceted effects of compulsory education reforms on labor market dynamics, with pronounced gender-specific outcomes.

Keywords: Compulsory Education, Wages, Employment, Türkiye



Öz

EĞİTİMİN İŞ PIYASASASI ÜZERİNDEKİ ETKİLERİ:
1997 TÜRK ZORUNLU EĞİTİM REFORMUNDAN
YARI-DENEYSEL BULGULAR

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Yüksek Lisans, İktisat Bölümü

Tez Yöneticisi: Prof. Dr. Meltem DAYIOĞLU-TAYFUR

Ekim 2023, 82 sayfa

Bu tez, 1997 Türkiye Zorunlu Eğitim Kanunu'nun eğitim düzeyi ve sonraki işgücü piyasası sonuçları üzerindeki etkisini araştırmaktadır. 2007-2022 yıllarını kapsayan Hanehalkı İşgücü Anketi (HİA) verilerini kullanarak, analiz 25-35 yaş arası Türk bireylerine odaklanmaktadır. Nedensel çıkarım yöntemi olarak Araç Değişken (IV) yöntemi kullanılmıştır. Sonuçlar, zorunlu eğitim reformunun her iki cinsiyet için 8. sınıf (ortaokul) derecesi başarısını önemli ölçüde artırdığını göstermektedir. Ek olarak, yalnızca kadınlar için lise derecesi başarısının arttığını gösteren bir etki gözlemlenmiştir. Eğitimin getirisi cinsiyetler arasında belirgin bir şekilde farklılaşmıştır. Kadınlar, aylık ve saatlik ücretlerde sırasıyla %10 ve %15 artış yaşarken, erkekler her iki oranda da istatistiksel olarak anlamlı olmasa da %7'lik bir artış görmüştür. Ayrıca, kadınların aylık çalışma saatleri okullaşmayla azalmış, ücretli, sosyal güvenceli bir işte, kamu sektöründe veya büyük şirketlerde çalışma olasılıkları ise artmıştır. Buna karşılık, eğitim kadınların ücretsiz aile işçisi olarak gözlemlenme ihtimalini azaltmıştır. Tüm bu istihdam dönüşümleri erkeklerde

gözlemlenmemiştir. Bu araştırma, zorunlu eğitim reformunun işgücü piyasası dinamikleri üzerindeki çok yönlü etkilerini ve cinsiyete özgü sonuçlarını gün yüzüne çıkarmıştır.

Anahtar Kelimeler: Zorunlu Eğitim, Maaşlar, İstihdam, Türkiye



To all students, who dare to dream and strive for knowledge.



ACKNOWLEDGMENTS

First of all, I would like to express my sincere gratitude and appreciation to my thesis supervisor Prof. Dr. Meltem Dayıoğlu-Tayfur. She did not just help me with her wise comments and guidance, but also motivated me with her kind words when I felt down. This thesis would not have been possible without her mentorship.

I would also like to thank the thesis committee members. Firstly, I'd like to thank Assoc. Prof. Dr. İlhan Can Özen for his detailed examination of the text at such a short notice and his valuable comments. Secondly, I extend my thanks to Assist. Prof. Dr. Güneş A. Aşık for her kind and supportive comments. Their insightful feedback helped to improve the quality of this work.

I am grateful to my family, for their encouragement, and belief in my abilities. Your love and support has helped me in ways too numerous to list here.

I extend my appreciation to my friends and colleagues who offered their advice, encouragement, and camaraderie throughout this academic pursuit. I would particularly single out Can Deniz Turna, Çiğdem Ekiz, Duygu Çelik, Hazal Güzel, and Zeki Oğulcan Şengül. My office mates Yunus Bulut Topuz and Pınar Özpala were the best for being late-night study companions at the department. Finally, I owe a debt of gratitude to my friend Ismail Furkan Narlı for his generous technical support.

Lastly, but certainly not least, I would like to express my deep appreciation to my girlfriend, Ayşe Nur Kayahan. She was always ready to offer her help, whether it was a word of encouragement during stressful times or making sure I had a meal when time was too tight to eat. Her support sustained me throughout this challenging academic journey. Thank you for sharing both my burdens and excitement.

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LIST OF ABBREVIATIONS

ATE – Average Treatment Effect

CPI – Consumer Price Index

HLFS – Household Labor Force Survey

IV – Instrumental Variable

LATE – Local Average Treatment Effect

LFP – Labor Force Participation

MEB – Turkish Ministry of National Education

MOLSS – Ministry of Labour and Social Security

NUTS2 - Nomenclature of Units for Territorial Statistics 2

TDHS - Turkey Demographic and Health Survey

TSLS – Two-Stage Least Squares

CHAPTER 1

INTRODUCTION

In 1997, the Turkish government implemented a compulsory schooling law (CSL) aimed to improve the education system. The policy expanded the mandatory education duration from 5 to 8 years. This politically motivated policy created a significant exogenous shock in the educational attainments of the students who were exposed to it and enabled us to compare the ones exposed to policy (treatment) vs not exposed by it (control). Initially, this comparison between treatment and control groups allowed us to unearth the impact of the policy on the degree attainments. Secondly, derived from this impact, we could delve deeper to investigate the causal effect of education on various facets of labor market outcomes. Notably, we looked into wages as the primary metric but also extended our analysis to aspects like working hours, labor force participation, and the nature and conditions of employment. These conditions spanned a wide spectrum, from considerations of social security record and company size to the nuances of employment terms such as full-time status and varied employment roles, whether as unpaid family workers, salaried workers, self-employed individuals, or employers.

The impact of education on labor market outcomes has been a topic of interest among economists for decades. The interplay between education and its labor market consequences in Türkiye, as outlined above, provides insightful nuances that can be explained in the light of the two main theories; build on Becker's (1962) human capital model and Spence's (1973) signaling model. According to the human capital model, education endows individuals with skills and knowledge, making them more productive and thus potentially earning higher wages. In contrast, the

Signaling Model suggests that education primarily serves as an indicator to employers about an individual's inherent capabilities or potential, rather than the skills directly acquired through education.

Compulsory schooling laws have been the focal point of scholarly attention for many empirical studies. Specifically, the 1997 policy in Türkiye found its way to the center of two notable studies by Aydemir & Kirdar (2017) and Torun (2018). Leveraging the policy change as an instrument for schooling, they measured the causal effect of education on the labor market outcomes, especially wages. Their insights, however, were grounded in early labor market experiences of individuals, mostly those in their early 20s, constrained by the data available at the time. Building on this foundational work, the present study examines the longer-term effects of the reform focusing on individuals in their late 20s and early 30s. The central inquiry here is to discern whether the observed effects in earlier stages intensify, diminish, or remain stable as these individuals spend further time in the labor market. To this end, this study harnesses an expansive dataset, merging 16 waves of the Household Labor Force Survey spanning from 2007 to 2022. The rich scope of this dataset provides a unique lens to observe the evolving labor market trajectories of the reform-affected cohorts, offering insights into the lasting effects of the 1997 compulsory schooling reform.

The results of this study provide a compelling testimony to the reform's impact. With the policy, middle school completion rates witnessed a marked uptick - a rise of 11% for women and 5% for men. Intriguingly, the reform's reverberations didn't stop at middle school. A clear spillover effect manifested in high school attainments, with an augmentation of 4.3% observed only for women. These degree attainment effects translate to labor market which is analyzed utilizing an instrumental variable (IV) approach. The Two-Stage Least Squares (TSLS) estimates illuminate the tangible benefits reaped in the labor market due to increased schooling. For women, each additional year of schooling increased monthly wages by 10% and hourly wages by a remarkable 15%. The narrative is different for men, however. While they too experienced an uplift in wages - around 7% for both

monthly and hourly rates - these figures failed to achieve statistical significance. A noteworthy observation is that these effects are generally higher than previously reported.

The results also show women tended to work fewer hours with the additional years of schooling. This reduced work time manifest into higher hourly wage returns than monthly wage returns. Regarding the additional tangible effects, the impact of education on employment conditions further highlights its importance. Education decreased the probability of women being employed in a job without a social coverage and employed in an unpaid family job. Education also increased the probability of women being employed as a wage worker or working within the public sector or sizeable corporations with 50 employees or more. These shifts signal that education not only enhances women's potential income but does that through channels which pave the way for better job conditions and security.

The next chapter will explain the theoretical foundations that underpin the relationship between education and labor market outcomes. Chapter 2 will explain the basics of these fundamental theories that attempt to elucidate this nexus. Subsequently, chapter 3 will proceed to literature review. The discourse will first highlight empirical studies concerning developed countries, then studies on developing countries will be presented. After the international examples, the Turkish examples will be presented focusing on 1997 CSL. In chapter 4, the details of the 1997 CSL will be explained. Beyond the law itself, this segment will also traverse the evolution of compulsory education in Türkiye. Furthermore, it will shed light on the basics of the Turkish education system and provide a glimpse into the political environment of the time that motivated this reform. Chapter 5 will illuminate the dataset and the basic variables. The adopted methodology also will be introduced with the potential identification issues. Finally, chapter 6 will unveil the research findings with the relevant comparisons of the prior studies and explanatory discussions.

CHAPTER 2

THEORITICAL BACKGROUND

In the forthcoming chapter, the discourse commences with an examination of the theoretical literature. This exploration delves into an array of theories that strive to elucidate the potential mechanisms through which education can shape an individual's labor market outcomes. The literature is primarily constructed around two theoretical pillars: the Human Capital theory and the Signaling theory. The former explains education's beneficial impact on labor market outcomes via the enhancement of worker productivity and skill acquisition. Conversely, the latter theory highlights the 'signal' that potential employees transmit to the market, indicating their abilities. Although these explanations are not mutually exclusive, they are based on different ideas. The Human Capital theory centers on human productivity, while the Signaling theory, in part, concentrates on unobserved ability (Lang & Kropp, 1986). Collectively, they serve to elucidate the economic advantages derived from education.

Initially, an exploration of the Human Capital theory is warranted. The concept that individuals can acquire skills through education, and subsequently leverage these as capital for profit, traces back to the 18th-century scholar, Adam Smith. Smith offered an initial definition of human capital in his renowned work, "The Wealth of Nations". He stated, "The acquisition of such talents . . . during his education, study, or apprenticeship, always costs a real expense, which is a capital fixed and realized, as it were, in his person." (Smith, 1976, orig. publ. 1776). Despite Smith's early discussions of education and its economic returns, and notwithstanding early studies on schooling and income such as Gorseline (1932) and Walsh (1935), it is Gary S. Becker who is often recognized for his groundbreaking work in defining

human capital as it is understood today. Becker's seminal contributions to both the theoretical and empirical underpinnings of Human Capital (Becker, 1962; 1964) popularized the field within economics, culminating in his receipt of a Nobel Prize (Becker, 1992).

Education can be conceptualized as an investment in human capital, necessitating a cost-benefit analysis just like any investment decision. The Human Capital theory assumes that rational individuals analyze their lifetime earnings and decide to invest in their human capital by pursuing education. In this analysis, the present value of future returns net of costs is calculated. (Borjas, 2013). For example, an individual's decision to work after graduating from high school or alternatively, to continue to receive education in a college can be illustrated in the following figure:

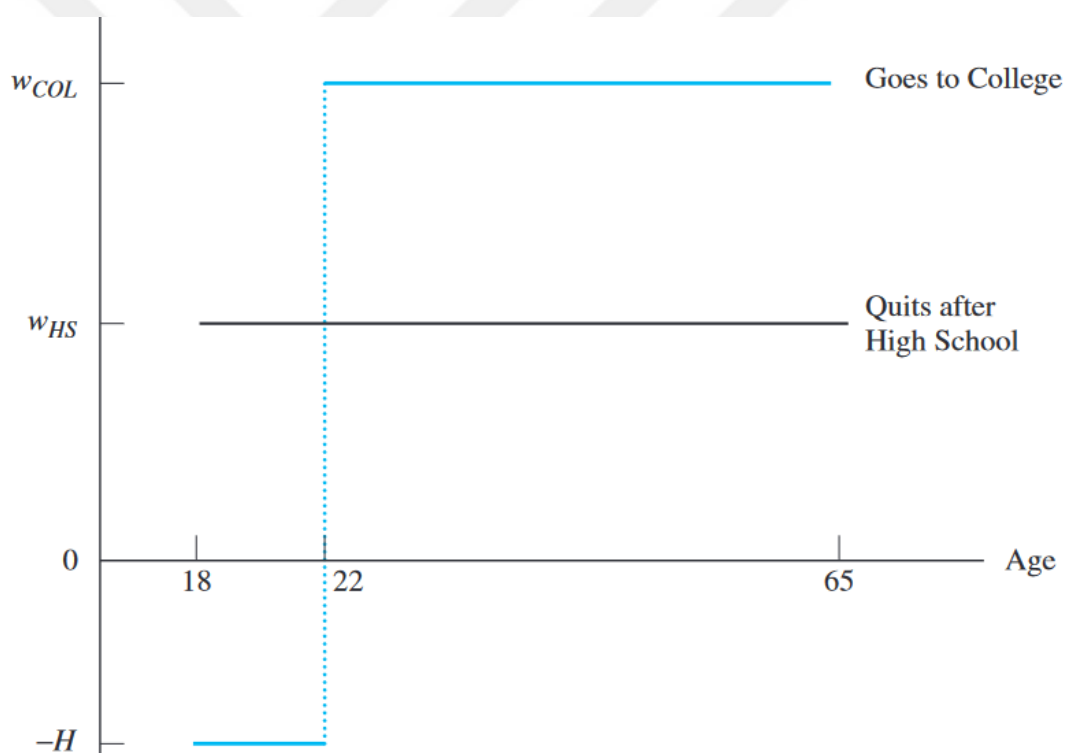


Figure 2.1 Potential Earnings Stream Faced by a High School Graduate (Borjas 2013)

Where $-H$ is the annual cost of receiving a college education, w_{HS} is the annual salary that a high school graduate would receive, and w_{COL} is the annual salary a college graduate would receive. In this simplified model, several assumptions are made for clarity: the salary doesn't change with age, the individual works until age

65, and there are no extra gains from education. The rational individual evaluates lifetime earnings, minus lifetime costs of college, under two scenarios: (1) working after high school or (2) after college. Every future gain or cost is discounted at a rate r . Obviously, this rate can differ from age to age, but again for simplicity this model does not account for age-to-age variation. Varying discount rates between individuals on the other hand, might depend on their preference for current versus future consumption. More formally, the present value of lifetime earnings is calculated as follows:

Present value of lifetime earnings under the scenario (1):

$$PV_1 = \sum_{t=1}^{47} \frac{w_{HS}}{(1+r)^{t-1}} \quad (1)$$

Present value of lifetime earnings under the scenario (2):

$$PV_2 = \sum_{t=1}^4 \frac{-H}{(1+r)^{t-1}} + \sum_{t=5}^{47} \frac{w_{COL}}{(1+r)^{t-1}} \quad (2)$$

Then two amounts are compared to choose the higher present value. If $PV_1 < PV_2$ chooses second scenario, and vice versa. As it can be derived from the equations above, probability of going to college is negatively correlated with three variables:

- 1- The cost of college, H .

First relationship is apparent, individuals may exhibit reduced propensity to pursue college education when it is a more expensive investment.

- 2- Wage gap between high school and college graduates, $w_{HS} - w_{COL}$

Secondly, individuals may be less inclined to pursue higher education if the relative earnings of college graduates diminish and/or if the earnings of high school graduates increase. Naturally, this wage gap is primarily contingent upon the specific labor market in which the individual seeks employment, and thus varies by

geographic location, country, and employment sector. However, it may also differ among individuals due to a host of factors, including unobservable intrinsic abilities and personal networks.

3- The discount rate, r

Third relationship, on the other hand, is a bit more mysterious. The more present oriented people are, the less they would want to wait to harvest the profit of an investment. Since receiving further education can be perceived as an investment, less patient people would be less likely to want to go to college. They would want to receive the benefit, w_{HS} , now. It is not clear why people have different time preferences. Lawrance (1991) suggests low-income households possess higher discount rates (Borjas, 2013). Becker & Mulligan (1997) also find that the discount rate changes with the income level of the household, but the relationship between son and parents is even stronger. They suggest either wealth decreases the discount rate, or time preference is transferred intergenerationally. There are also findings suggesting education also on its own makes people more patient which causes a positive feedback loop (Oreopoulos & Salvanes, 2011).

A person's decision to continue further study or to start to work is explained in a two-scenario case but it can be considered more broadly. Since people can decide to stop studying at any time in their study life, this dichotomous decision process can be considered as a more continuous concept. Let's say this decision is made at each education level, whether to continue high school, college, a post graduate program and so on. If each additional year spent in school teaches less essential knowledge for people's productivity and results in less increase in their income, i.e., the law of diminishing returns applies to schooling, at some point in time the marginal rate of return to schooling will be equal to r^* and people will stop their education. This process can be considered a "stopping rule" where people work until the marginal rate of return to schooling is not greater than their personal discount rate, r^* (Borjas, 2013).

Secondly, a brief overview of the Signaling theory is in order. Andrew Michael

Spence, like Becker, received a Nobel Prize for his theory of signaling (Spence, 2001). Spence developed the job-market signaling model (Spence 1973). His theory challenges the unrealistic assumptions of perfect information, instead proposes information asymmetry in the job market. This asymmetry emerges when workers are more informed about their skills and abilities than potential employers. This type of knowledge asymmetry is called information asymmetry¹. Workers with high human capital, let's call them productive workers, may attempt to bridge this information gap by sending signals, such as educational diplomas or certificates, to their prospective employers. On the other hand, less productive workers also would try to show their productivity higher than it really is. This is one of the key divergences between the Human Capital and Signaling theories - the latter allows for a display of abilities via education, irrespective of an actual increase in productivity (Borjas, 2013). There are studies trying to measure the return purely due to a degree effect also called sheepskin effect (Hungerford & Solon, 1987; Jaeger & Page, 1996). They use data on years of education to isolate this effect, but such an approach is beyond the scope of this study due to data limitations, as will be explained further in the data section.

Rational firms, in response, scrutinize these signals to learn more about their prospective employees. Blaug (1985) argues that degrees serve as an artificial screening tool for firms. They offer a cost-effective method to evaluate candidates, while also fostering social consensus among employees, employers, and customers.

Until this point, post-school earnings were conceptualized as constant for an individual as they age, as depicted in Figure 2.1. However, empirical data indicates that earnings fluctuate with additional age or experience. In actuality, the age-earnings profile manifests as a concave curve with a positive slope, as evidenced by the data for Turkish salary workers ranging in age from 15 to 65 years old.

A plausible rationale for this observed trend is that productivity might persistently

¹ For more in depth discussion about *information asymmetry*, see George A. Akerlof's (1978) famous paper "The Market for "Lemons": Quality Uncertainty and the Market Mechanism".

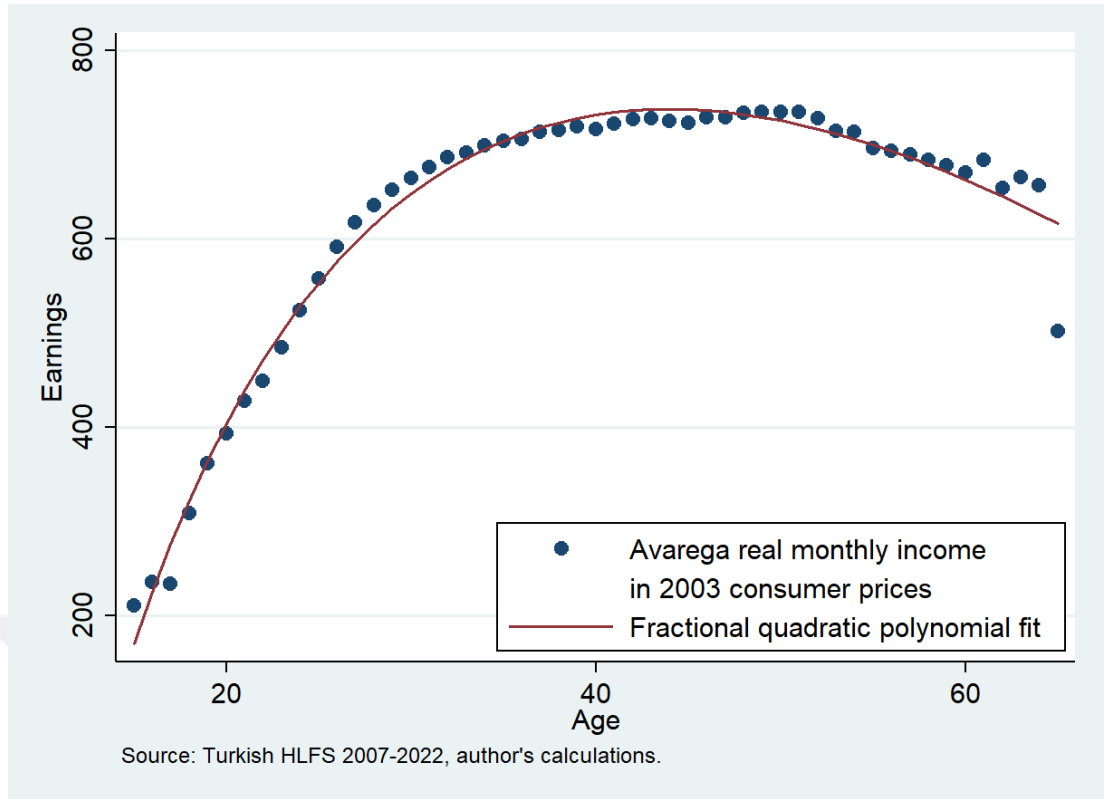


Figure 2.2 Turkish Age-Earnings Profile (2007-2022)

increase post-education. This could be attributed to formal or informal training individuals undergo after their education period. With experience, individuals often garner the so-called "know-how" knowledge, thereby specializing in their respective job roles. Moreover, there might exist a signaling effect wherein an additional year of experience information suggests that the worker is relatively more productive than their less-experienced counterparts. This information on its own may increase the wage without the productivity increase. Furthermore, the principle of diminishing marginal returns also applies to post-school investment. As individuals age, the amount invested on human capital falls. This is because the opportunity cost in terms of forgone wages rises, while additional benefit falls (due to diminishing marginal returns) rendering reduced work hours increasingly expensive. Another founding father of the labor economics, Jacob Mincer (1974) formulated this age-earnings relationship in the subsequent equation:

$$\ln E_{s,t} = r_s s + \rho_1 t - \rho_2 t^2 \quad (3)$$

Here $E_{s,t}$ represents the gross annual earnings of a worker with s years of schooling

and t years of work experience; r_s is the rate of return on schooling; the quadratic term t^2 captures the concavity of the earning-age profile.

There is also empirical evidence showing that education increases post-school human capital investment. This means that education not only elevates baseline productivity but also amplifies the rate of its growth, or in other words, accelerates productivity increases. The average wage gaps for the graduates of different levels of education are diverging. It is evident that the age-earnings profile becomes steeper when the education received increases as in the graph below for the Turkish workers for 3 different educational groups.

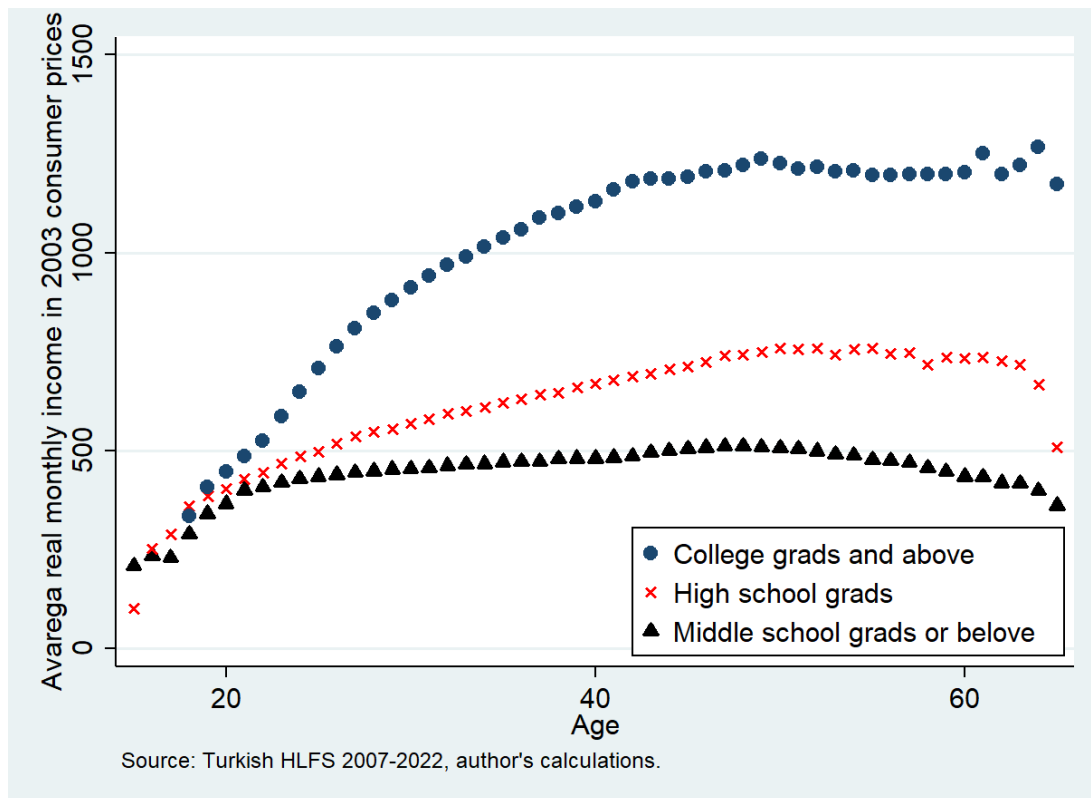


Figure 2.3: Turkish Age-Earnings Profile by Education Groups (2007-2022)

This divergence can imply that education fundamentally changes the way how people acquire more human capital. However, this trend is also likely influenced by latent variables such as innate abilities, familial backgrounds, motivation, and other intrinsic factors.

In summary, within the framework of this fundamental human capital model,

premised on a rational individual, the schooling decision conceptualized solely as an investment depends on three factors: the person's anticipated wage gain (which is rooted in productivity improvements, signaling effects, and other external factors), time preference (discount rate, r), and the cost of schooling (comprising forgone earnings during schooling and the direct price of schooling, H). However, for some individuals, education may also be viewed as a normal good, one that provides intrinsic value and enjoyment when consumed in greater quantities. Such individuals may derive pleasure from acquiring new knowledge and interacting with classmates in an educational setting. Consequently, this model may potentially underestimate the overall return on investment in education to some extent but mainly suggests that rational individual makes the most efficient decision.

Intervening in such a rational decision-making process could lead to inefficiency, but there are several circumstances where intervention is largely justified. The first case arises when the rational individual is not sufficiently informed. The lack of information may result in underestimating the returns, particularly in poorer countries, where lack of information is more acute. Second, as noted earlier, low-income households may exhibit higher discount rates (Lawrance, 1991; Becker & Mulligan, 1997), and children, in general, may be more susceptible to irrational myopia. Third, the cost of education can be disproportionately high for certain disadvantaged groups, such as people in rural areas (Kırdar, Dayıoğlu & Koç, 2016), or due to adverse social norms, such as opposition to girls' schooling (Tansel, 2002). Fourth, there may be a principal-agent problem between parents and their children, where the interests of parents may not necessarily align with those of their offspring. Lastly, positive externalities, like improvements in health and life/work satisfaction, may be overlooked by individual decision makers (Lleras-Muney, 2005; Oreopoulos, 2007; Lochner, 2011; Oreopoulos & Salvanes, 2011; Dursun, 2016; Dinçer, Kaushal & Grossman, 2017; Dursun, Cesur & Mocan, 2018; Ahsen, 2021; Dursun, Cesur & Kelly, 2022).

CHAPTER 3

EMPRICAL LITERATURE REVIEW

Following the theoretical dissection, the discussion will progress to incorporate the empirical literature. The abundant related studies available facilitate a comprehensive comparison of our findings within both Turkish and international contexts. The rich empirical literature on the labor market returns to education are developed in tandem with the theories mentioned in the preceding section. Nevertheless, the number of empirical studies has skyrocketed with the availability of new data sources and the advancement of more robust quasi-experimental causal inference methods starting in the 1990s which is also called “*the credibility revolution*” (Angrist & Pischke, 2010). In the following sub-sections of the literature review, first the related empirical literature on developed countries, then on developing countries with special focus on the Turkish case will be presented.

3.1 Related Literature in the Context of Developed Countries

Gunderson & Oreopoulos (2020) conclude that the causal effect of one more year of schooling on earnings in developed countries is mostly between the 10-15% range based on the most current empirical literature using natural experiments. Remarkably this range is pretty high and generally higher than the OLS estimates which are typically situated between 6-10%. That indicates downward bias in the OLS estimates. Additionally, returns are generally higher for females. In this sub-section, the results for the specific studies will be presented such as USA, UK, Germany, and Canada.

3.1.1 Studies on the USA

Angrist & Kruger (1991) published a landmark study, which uses the quarter of birth

as an instrument to compare the earnings of persons with 10 vs. 11 years of schooling. Compulsory schooling laws in the US force some students—who are a couple months younger than their classmates—to stay in high school more than other students. The study finds an additional year of schooling increased earnings 6-8% for men using 1980 US census data, and 10% for men using 1970 US census data. Later, Staiger & Stock (1997) found a larger effect, 9-10%, for men in the US using 1980 census data. Both studies find downward bias in the OLS estimates but the difference between the IV and OLS estimates was not statistically significant in the Angrist & Kruger (1991) study.

In a discerning literature review on instrumental variables based on institutional features of the school systems, Card (1999; 2001) finds that most studies report results with downward-biased OLS estimates—often by 30% or more. Another noteworthy study by Oreopoulos (2007) found one of the highest returns for the US—a 14% increase in earnings using the IV method.

One of the most recent studies for the US conducted by Clay, Lingwall, & Stephens (2021) corroborates the findings of previous literature, reporting a 7-8% increase in earnings for men in the US. The study also explores the disparate effects based on the income distribution using quantile IV regressions, concluding that individuals at the lowest quantile of the income distribution experienced the greatest returns.

3.1.2 Studies on the UK

The research by Harmon & Walker (1995) uses school-leaving age increase from 14 to 15 in the UK as an instrument and finds that an additional year of schooling increased earnings of British men by 15% without controlling for cohort effects. Using the same reform in the UK, Oreopoulos (2006a) finds 14% increase in earnings, but later Devereux & Hart (2010) re-analysed the data and find a much smaller effect: Between 4–7% return for British men, and no return at all for women. Oreopoulos (2006a) also studied Northern Ireland data and found an average return between 14-21%. A subsequent study by Grenet (2013) implemented a regression discontinuity design and used compulsory schooling

reform as a natural experiment. The study found a 6% return for England and Wales, coupled with a positive employment effect for women. When conducting a similar analysis with French data, however, the study found negligible and statistically insignificant returns for France, with no discernible impact on employment.

3.1.3 Studies on other developed countries: Germany & Canada

In the German context, Ichino & Winter-Ebmer (1999) found different earning returns to education using two separate instruments, revealing the potential for variation in the magnitude of returns depending on the chosen instrument. The study reported returns of 5% and 12 % for male German workers employed in 1986. This upper bound result approximately doubled the OLS value, suggesting the presence of a downward bias in the OLS estimates. Conversely, Pischke & Von Wachter (2008) found that the positive return estimated by OLS disappeared entirely when years of schooling was instrumented with changes in compulsory schooling laws in Germany. They attributed this divergence to Germany's distinctive education system, where students may acquire basic skills earlier than their peers in other countries. However, Cygan-Rehm (2018) re-examined their results, accounting for overlooked institutional details, and reported a 6-8% return, thus supporting the initial findings of the Ichino & Winter-Ebmer study.

In the Canadian context, Oreopoulos (2006b; 2007) used changes in compulsory schooling laws across different states as an instrument, reporting that annual income increased by 8-12% for each additional year of schooling.

Although this literature review primarily focuses on returns to education in terms of earnings, it is important to note that studies also investigate other pecuniary gains such as wealth (Oreopoulos, 2007) and pension wealth (Ersan, 2022). Moreover, a substantial body of literature exists on non-monetary returns, including health benefits (Oreopoulos, 2007; Lochner, 2011), increased lifespan (Lleras-Muney, 2005), satisfaction at work (Oreopoulos & Salvanes, 2011), life satisfaction (Oreopoulos, 2007), human capital gains in terms of personal traits (Oreopoulos &

Salvanes, 2011), better decision-making in health, family planning, marriage, parenting (Currie & Moretti, 2003; Black, Devereux & Salvanes, 2004; Oreopoulos, Page & Stevens, 2006), societal benefits such as reduction in crime rates (Lochner & Moretti, 2004; Lochner, 2011; Cano-Urbina & Lochner, 2019), changes in political behavior (Milligan, Moretti & Oreopoulos, 2003; Lochner, 2011), and positive externalities through spillovers (Moretti, 2004).

3.2 Related Literature in the Context of Developing Countries

In their comparative analysis of 17 low-income countries, Patrinos & Psacharopoulos (2020) argues that returns to education may be higher in developing nations relative to their developed counterparts. However, a similar study by Peet, Fink, & Fawzi (2015) examining 25 developing countries using simple OLS regressions, argues that the evidence is remarkably heterogeneous from one country to another. They do not find any evidence for systematic excess returns or a consistent trend over time. Although both analyses are comprehensive, they utilize non-causal methods, thereby limiting their conclusiveness. Only the studies making causal inference will be briefly presented in this sub-section.

On the other hand, the proposition that returns for women are higher is less contentious in both the developing and developed country contexts (Schultz, 2002; Tsai et al., 2009; Peet et al., 2015). Perhaps this consistently observed difference can be attributed as evidence for the disadvantageous position of woman both in developing and developed countries. The reasons behind these gender differences, such as the potential impact of cultural and economic factors or differences in the quality and quantity of education received, are subjects of ongoing research.

3.2.1 Studies from Philippines, Indonesia, Taiwan, Egypt & China

In one of the poorest region of Philippines, Maluccio (1997), uses distance to nearest school and existence of a local private school as instruments and finds 14.5% returns to education. Duflo (2001) employs the Indonesian school expansion program as an instrument to identify a 7-11% return on education for men only. Utilizing Taiwan's 1968 compulsory school reform as an instrument, Spohr (2003)

finds a 6% return for men and a 17% return for women. Tsai et al. (2009) employ the same reform to examine its effect on gender disparities in employment, concluding that an additional year of schooling increases a woman's probability of employment by 4%. Furthermore, they also find the likelihood of women working in the agricultural sector decreases by 5%. Utilizing the compulsory schooling reform in Egypt that decreased primary schooling from 6 to 5 years as an instrument, Assaad et al. (2023) found a statistically insignificant 2-4% return for men, which is one of the lowest returns for a developing country. Notably, their unique instrument keeps the primary school diploma factor constant between treatment and control groups, suggesting that the effect found is purely a human capital effect with the sheepskin effect extracted from their results.

Lastly, regarding examples for China, Guifu & Hamori (2009) finds 13% return for married men and 14-22% for married women in urban China using spouse's education as an instrument. A more related study to this thesis is Fang et al. (2012), which uses 1986 compulsory schooling reform as an instrument. They find 20% return on average with an exceptionally high return of 50% for men compared to 10% for woman. A current study on China by Chen, Jiang & Zhou (2020), uses middle school duration reform which decreases the duration by 1 year in 1960s and restores the change in 1970s. They find on average 13% return to an additional year of schooling.

3.2.2 Studies on Türkiye

There are early studies on return to education focusing on women in Türkiye by Tansel (1994, 1996). There are also studies comparing Türkiye and other developing countries such as Egypt, Iran, and Palestine (Tansel & Daoud, 1994; Salehi-Isfahani, Tunali & Assaad, 2009). However, these early studies ignore endogeneity of education and may not be treated as causal. In this sub-section, four closely related causal studies based on natural experiments will be briefly summarized.

- 1- Mocan (2014) examined the impact of education on labor market outcomes using the same compulsory schooling law (1997 CSL) as a basis for her study.

Utilizing data from the HLFS waves of 2011 and 2012, Mocan analyzed observations for both men and women between the ages of 20 and 30. She discovered that the reform led to a 22-percentage point (pp.) increase in middle school completion rates. Through instrumental variable analysis, Mocan found that earnings return in terms of hourly wages were approximately 14% per additional year of schooling for women, while no statistically significant return was observed for men. Additionally, Mocan investigated the probability of labor force participation and probability of full-time work but did not find any significant effect. A recent study corroborates this (non)finding by failing to identify any effect on probability of women being ever employed, attributing this surprising outcome to factors such as marriage and pregnancy (Gülberk, 2023).

- 2- Aydemir & Kırdar (2017) also used the same reform (1997 CSL), integrating 10 more waves of Turkish HLFS to previous HLFS rounds, resulting the dataset with the waves from 2002 to 2013. Their main analysis excluded college graduates, focusing instead on the early labor market outcomes, within the age bracket of 18-35. They further expanded their investigation to include other age ranges (18-45, 18-55) by adjusting the upper age limit (or lower limit for birth year), thereby increasing the observational data within the control group and facilitating the application of higher-order polynomials. However, their examination is essentially limited to the oldest treated individuals, who were 26 years old in the 2013 survey. They employed a fuzzy regression discontinuity design for their methodology, which is essentially same with the Instrumental Variables (IV) method. They find policy increased the years of schooling 0.5-0.7 years for both men and woman. They observed an increase in women's hourly income by 7-8% per schooling year and a statistically insignificant 2-2.5% for men. Their OLS estimates were downward biased, consistent with earlier studies. They also explored the possibility of a compositional shift within their treatment group and identified an employment effect for wage-earner positions ranging from 0.4 to 1.1 percentage points (pp), exclusively for women. They also further

expanded their analysis by including college graduates for the robustness checks. They found no spillover effect on college degree, and similar return for women but return for men disappeared.

- 3- Torun, (2018) is one of the most related studies to this thesis. He uses the same reform (1997 CSL) with Turkish HLFS waves from 2004 to 2011. He, like Aydemir & Kirdar (2017) study, focuses early labor market outcomes of the non-college bound individuals aged between 20-29 with the oldest treateds being 24 years old using IV method. He found a positive policy effect on the years of schooling for women, amounting to 0.6 years, and 0.4 years for men. The OLS estimates for monthly wage return for women doubles from 5% to 10% compared to IV estimates. Yet, for men, a mere 2% return was observed, which vanishes entirely when the age range was restricted to 22-27, due to compulsory military service concerns. He additionally investigates the potential mechanism for woman's returns, and claims that women moved to formal sector and changed jobs which required higher skills. He controls whether there is a selection into employment and the composition of the employed group changes finding only small effect of 2 pp on men's labor force participation.
- 4- In their innovative 2018 study, Ozturk & Tumen utilized a rather unusual instrument to analyze the exogenous variation in education for Turkish males aged between 40-51, using the 2005 data from the Turkish Household Labor Force Survey (HLFS). They employed Türkiye's politically turbulent era in the late 1970s, characterized by widespread and violent street protests, as an analytical framework. The impact of these chaotic years on men's education was anticipated to be negative. They identified a reduction in the average years of schooling by 0.2-0.3 years for the affected age group (40-45 in 2005) relative to the control group (46-51 in 2005). Although the single-year wave from 2005 prohibits a direct comparison between individuals of the same age in the treatment and control groups, they supplemented their analysis with other HLFS waves for robustness checks. Their key findings demonstrated a 12-14% return on hourly wages and 8-11% on monthly

wages for men. Upon examining the influence of post-secondary education, they discovered a 6% increase in hourly wage attributable to college degrees for men. Their OLS estimates were downward biased similar to earlier literature findings. Consequently, all IV estimates were 10-20% higher than the OLS results; for instance, a 10% OLS estimate would result in an IV estimate of 11 or 12%.

Lastly, numerous other studies making inferences on non-monetary outcomes for Türkiye using this commonly used reform (1997 CSL) find predominantly positive social impact from education. For instance, there are studies noting improvements in health and health behaviors (Dinçer, Kaushal & Grossman, 2017; Dursun, Cesur & Mocan, 2018; Ahsen, 2021; Dursun, Cesur & Kelly, 2022) and self-reported happiness (Dursun, 2016). Evidence also suggests the reform has reduced urban-rural educational disparity, albeit without persisting changes in gender disparities in cities (Kırdar, Dayıoğlu & Koç, 2016). The policy has been linked to decreases in early teenage marriage and early teenage pregnancy (Kırdar, Koç & Dayıoğlu, 2018); consanguineous marriage (Akyol & Mocan, 2023); and domestic violence (Akyol & Kırdar, 2022). Additionally, it has reinforced attitudes against domestic violence among daughters (Gulesci, Meyersson & Trommlerová, 2020) and affected political behavior, religious expression, and immigration decisions (Cesur & Mocan, 2018; Gulesci & Meyersson, 2013; Aydemir, Kırdar & Torun, 2022).

CHAPTER 4

THE 1997 TURKISH COMPULSORY SCHOOLING REFORM: AN OVERVIEW

In the empirical literature chapter, it was outlined how the 1997 Turkish compulsory reform has been a popular instrument in numerous studies over the past decade. To fully grasp the intricacies of the reform, it is important to have a basics knowledge about Turkish education system at that time, as well as a brief historical context of history of Türkiye's compulsory education journey. This chapter will also touch upon the political motivations behind implementing such a reform. Concluding the section, we will delve into the other components of the reform package, elucidating the mechanics and implications of the 1997 reform.

In Türkiye, primary and secondary education is regulated under single national body, the Ministry of National Education (MEB). Before 1961, the compulsory schooling duration only encompassed a 5-year primary education span. While this duration was initially extended to 8 years in 1961, various constraints, notably the insufficient number of schools and teachers, meant that the extended duration could not be fully enforced until the late 1990s (Dülger, 2004). As of 1996, while the enrolment rate for the initial 5-year compulsory education stood at an impressive 96.68%, the enrolment for the following 3-year span was significantly lower, registering at only 64.73% (Dülger, 2004). In an effort to bridge this discrepancy, the 1997 Compulsory Schooling Law (Law No.: 4306) was enacted, expanding the compulsory schooling duration from 5 to 8 years (Official Gazette, 1997).

In 2001, Türkiye established the goal within its 8th five-year development plan to extend the compulsory basic education duration to 12 years (State Planning

Organization, 2001). By 2005, while the high school (upper secondary education) duration was lengthened from 3 to 4 years, it hadn't yet become mandatory. It was clear from the statements of the ministry that aligning with European standards was a primary catalyst for these reforms (Karabigin, 2020). Becoming a member of the European Union was an important ambition for the country at the time. Over a decade after the articulation of the 2001 development plan, high school education was declared compulsory in 2012, thus extending the overall mandatory education tenure to 12 years (Official Gazette, 2012). This law (No. 6287) restructured the national education system, breaking it down into three distinct, equally distributed phases with the so called 4+4+4 system. In the new system, primary education was truncated to a 4-year term, middle school (or lower secondary education, sometimes referred to as junior high) was re-separated and extended to 4 years, and finally high school (upper secondary education) duration did not change and became compulsory (OECD, 2013)². To sum up, compulsory schooling duration was 5 years before 1997, 8 years between 1997-2012 and 12 years after 2012.

Recentring our discussion on the 1997 reform, which serves as the instrumental variable in this study, it's essential to note that a primary motivation behind its implementation was political in nature. The reform led to the discontinuation of religious middle schools, commonly referred to as “imam hatip” schools. These schools integrated religious instruction alongside the national curriculum. However, the high school counterparts of these religious institutions remained operational. To grasp the underlying political motives more effectively, it is beneficial to delve into a brief overview of the prevailing political climate of the era.

In 1997, the government was a two-party coalition, perceived by the military establishment as a potential threat to secular character of the country. During the National Security Council meeting on February 28th, the military proposed a series

² The 2012 Compulsory Schooling Reform does not affect our sample. The eldest cohort influenced by this reform is from 1998 which does not intersect with our treatment group even when considering the unrestricted sample. A comprehensive examination of the effects of this reform on enrolment rates and categories has been articulated by Erten & Keskin (2019).

of measures from the government aimed at curbing supposed Islamization of the country (Cizre & Cinar, 2003). The measures reportedly included provisions such as instituting an uninterrupted eight-year basic education, terminating the junior high-school branches of the religious 'imam hatip' high schools, and bringing the 'Quranic' schools under the supervision of the Directorate of Religious Affairs (Birand Yapım, 2017). This military memorandum led to the collapse of the coalition in June. Replacing government, another coalition, quickly implemented these measures, notably enacting the 1997 compulsory schooling law by August of that same year.

This reform served to curtail religious education by closing junior high-school branches of the 'imam hatip' high schools and also by delaying the admission of younger pupils into the Quranic schools, which they typically attended after completing the standard five-year primary education before the enactment of the reform. Given that the impetus behind these reforms was primarily political and independent of the economic conditions of the time, the reform consequently exerts an exogenous impact on the educational outcomes. Additionally, the reform eliminated the lower secondary segments of the technical high schools, which accounted for only 1.3% of the overall enrolments (Aydemir & Kırdar, 2017).

With the introduction of the reform, the ministry eliminated the diploma awarded at the conclusion of the 5-year education. Instead, an 8-year primary school diploma was established, encompassing both primary and middle school education. This change provided a further incentive for students to continue their education beyond the initial 5 years. If students chose to drop out after this period, they would not receive any diploma at all. This consideration significantly influenced completion rates, as families recognized the potential value and signaling power of possessing a diploma in the labor market (Dülger, 2004; Kırdar, Dayıoğlu & Koç, 2016).

The implementation of the reform led to a pronounced increase in student enrolments. As highlighted by Aydemir & Kırdar (2017), the four years following the

reform saw the registration of approximately 1.5 million new students in the newly designated 8-year primary schools, marking an increase of 16%. This sudden spike in demand for education was met with a commensurate rise in supply. The government took multiple measures to finance this expansion: It initially introduced temporary new taxes, raising US\$2 billion, and increased the public investment share of the ministry (MEB) by around 2.5-fold, later government also sought an additional \$3 billion loan from the World Bank to back the reform and requested from IMF to not to set any restriction on education expenditures when negotiating stabilization policies. To accommodate the growing student population, the Ministry of Education undertook several initiatives. These included infrastructure investments, the construction of new educational institutions, the recruitment of new teachers, and the implementing of a double shift education regime. Furthermore, to boost enrolment rates, particularly among the less privileged, the Ministry provided free educational materials and meals, and school busing (Dülger, 2004). Kırdar, Dayioğlu & Koç (2016) characterizes the impact of these education policies as “...one of the ...highest impact on enrolment ever implemented”. All these policies seem to work considering student to class ration remained consistent at around 28 students per class after four years of the reform. Similarly, the student-to-teacher ratio, which stood at 30 students for every teacher prior to the reform, dropped to just below 28 within after three years. Notably, students also demonstrated improved performance in international math and science test scores (TIMSS) when comparing scores from 1999³ to 2007 (Aydemir & Kırdar, 2017).

Lastly, the application of the law took into account the current grade of the student. Students who had completed the 4th grade and continued to 5th grade in 1997-1998 schooling year was the first cohort affected by the new legislation. They were now required to remain in school for an additional three years. These 5th graders were youngest treated cohort by the reform, meaning they were the ones nearest to the treatment cut-off. This cohort typically started to 1st grade in September of 1993

³ The students in 8th grade who participated in the 1999 TIMSS were not affected by the 1997 reform.

assuming no class repetition. Given that the typical school starting age in Türkiye is 6, the cohort predominantly comprises children born in 1987. However, a small group of children born in 1986 may also be affected – those born later in 1986 might have been enrolled a year later. This segment is considered not to be substantial.



CHAPTER 5

DATA & IDENTIFICATION

In this chapter, the data sources and main variables that form the backbone of our analysis will be introduced. Subsequently, the identification strategy and the analytical model underpinning this study will be described. Finally, we will address any potential issues related to identification that might arise during our investigation.

5.1 Data

The dataset employed for this study is a merged compilation of the Turkish Household Labor Survey (HLFS) rounds from 2007 to 2022. The scope of the analysis is limited to native-born individuals falling in the age bracket of 25 to 35 years. This decision stems from that foreign-born individuals might navigate distinct legal landscapes and may have diverged in their educational trajectories⁴. During the data cleaning phase, a small portion, specifically 0.1% of the total dataset, had to be dropped. This amounted to 1342 observations which presented issues like missing or inconsistent data on crucial variables such as demographics, employment status, or educational background. There was also a set of 1628 observations that were missing the birth year information but had data on age and survey year. For these cases, the missing birth year was computed by subtracting the age from the respective survey year. This imputation was particularly done for records that did

⁴While the data from the 2007 and 2008 rounds does not specify whether individuals are native-born, this missing information represents only a minor segment of the entire dataset. Drawing insights from the subsequent rounds, foreign-born individuals account for under 2% of the entire sample. This translates to an estimated 0.25% of foreign-born individuals not being dropped from the unrestricted sample, and 0.12% from the restricted sample. Given this minor proportion, their inclusion is unlikely to have a substantial effect on the overall results.

not hover around the cutoff point, namely years 1986 or 1987.

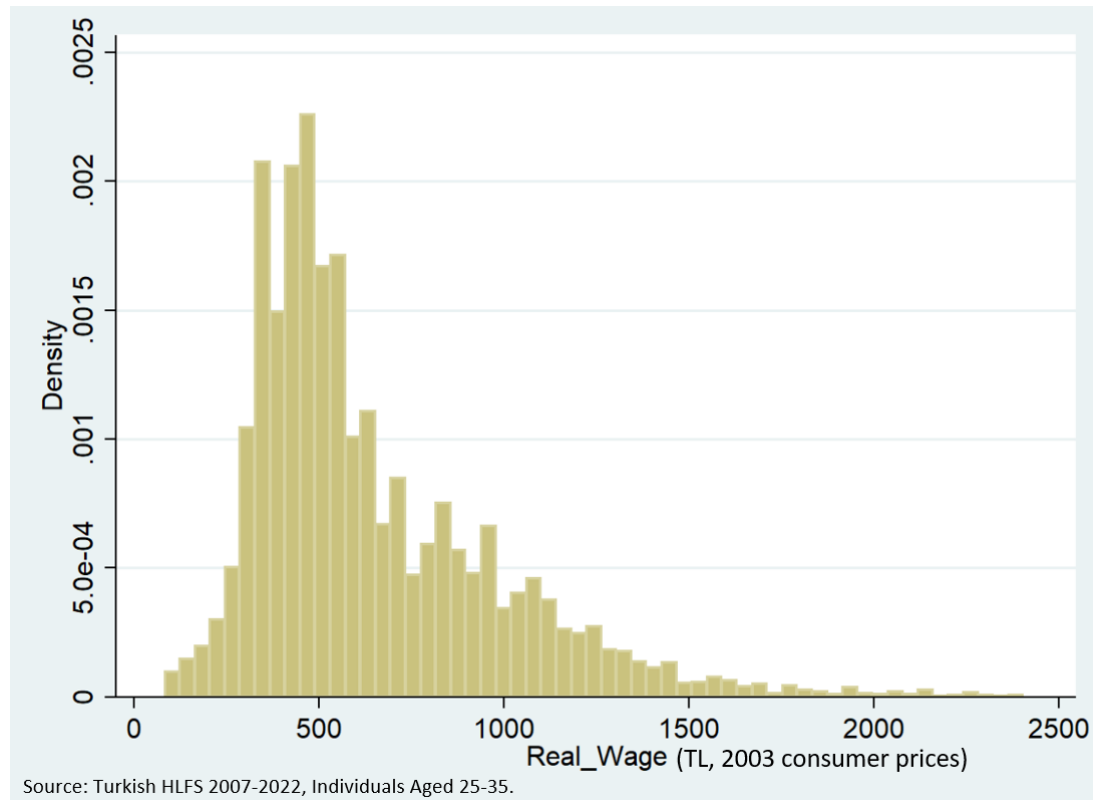


Figure 5.1: Real Income Distribution

The HLFS only records the net earnings from the previous month's main job. The earnings data covers regular wage earners, as well as casual and seasonal workers, and public employees. The real wage is calculated using the 2003 consumer price index (CPI). Excluding the zero income earners, outliers are deleted, which are determined as data points lying half of a percent from each side of the real wage distribution. This represents 1% of all wage workers but 0.4% of the total sample because over half of the sample is not wage earners, even with that young age bracket of 25-35. Trimming the data balances the income distribution, although the distribution remains right-skewed, evident in Figure 5.1. The outliers could be attributed to reporting errors or peculiar extreme cases. To provide context, the minimum wage in 2003 was 226 TL (MOLSS, 2023). In the untrimmed sample, the lowest monthly real income, based on 2003 prices, was a mere 0.10 TL, while the highest was 58.9 thousand TL. After trimming the data, these figures adjusted to a range between 83 TL and 2404 TL per month. Nevertheless, the study's results rem-

ain largely consistent, whether the data is trimmed or not.

The HLFS doesn't provide direct data on the number of years of schooling. Instead, this has been deduced based on the last level of education completed and the literacy status. Drawing from the 2008 Turkish Health and Demographic Survey (TDHS), Aydemir & Kirdar (2017) calculated, on average, literates without any formal degree had about 2 years of schooling. They also show that very few individuals dropped out before completing a specific educational level, such as primary, middle, or high school. Hence, the number of years of schooling is inferred from the completed educational tiers:

- Illiterates without any formal school degree are assigned 0 years.
- Literates without a degree are assigned 2 years.
- Primary school graduates are assigned 5 years.
- Middle school graduates assigned 8 years.
- High school graduates are assigned 11 years.
- Those with a college degree or higher assigned 15 years.

For students born in 1991 or later, an additional year is added to high school or college+ graduates.

5.2 Identification Strategy

The strategy to identify the causal relationship between education and wages is complicated due to the endogenous nature of education. Simply comparing the wages of people with different educational backgrounds would yield correlative, rather than causal, results. This is because education decisions are influenced by various factors that can also influence labor market outcomes, such as innate abilities, motivation, personal tastes, family background, network and so on. Best way to account for all these factors in an analysis from a randomized control trial. It would be impractical and unethical to establish such a setup. However, the Instrumental Variable (IV) method provides a way around this challenge by isolating the pure effect of education on wages. The instrument isolates variation in

education that is unrelated to the other determinants of wages, thus providing a more accurate estimate of the causal impact of education on earnings. This approach has been widely employed in academic literature to understand the returns to education, especially since the seminal work by Angrist & Kruger (1991).

Given an IV setup with a generalized potential outcomes concept where Y_i denotes the potential outcome of the dependent variable of interest for the individual i . In our case, it is a potential labor market outcome such as wage. $Treat_i$ is the treatment dummy for the individual i :

$$Treat_i = \begin{cases} 1 & \text{if } birth\ year \geq 1987 \\ 0 & \text{if } birth\ year \leq 1986 \end{cases} \quad (5.1)$$

Let S_{0i} denotes the years of schooling with the treatment status zero for the individual i . Similarly, S_{1i} is the years of schooling for the untreated case of the individual i . Therefore, the heterogenous causal effect of the compulsory schooling reform is $(S_{1i} - S_{0i})$. However, we only observe the years of schooling S_i where :

$$S_i = S_{0i} + (S_{1i} - S_{0i})Treat_i \quad (5.2)$$

IV estimates can be interpreted as a Local Average Treatment Effect (LATE) if the following four assumptions hold (Theorem 4.4.1 from Angrist & Pischke, 2009):

- 1) Independence assumption: Instrument is independent of the potential outcomes. It is almost randomly assigned so that it does not directly affect the labor market outcomes. It is formally stated as:

$$[\{Y_i(S_i = s, Treat_i = z); \forall s, z\}, S_{1i}, S_{0i}] \perp\!\!\!\perp Treat_i \quad (A1)$$

- 2) Exclusion restriction: Instrument affects the outcome only through the single known causal channel. Specifically, treatment which is based on the birth year only affects the labor market outcomes through schooling choices. Formally:

$$Y_i(S_i = s, Treat_i = 0) = Y_i(S_i = s, Treat_i = 1), \forall s \quad (A2)$$

- 3) First stage: The instrument affects the treatment variable S_i . Specifically, the policy affects the years of schooling. Formally:

$$E(S_{1i} - S_{0i}) \neq 0 \quad (\text{A3})$$

- 4) Monotonicity assumption: The instrument should have a monotone effect on the treatment variable S_i . It translates, even if the policy does not specifically increase the schooling years of everyone, it should not decrease it for anyone. Here is a formal statement:

$$S_{1i} - S_{0i} \geq 0, \forall i \quad (\text{A4})$$

The internal validity of our results depends on these four conditions. The results cannot be interpreted the causal return to education if the source of exogenous variation in schooling choices does not satisfy these conditions. Even when the conditions are satisfied, IV estimates give the effect for only the set of compliers – those who change their schooling choice due to the policy. However, the set of compliers may not be representative of the whole population. The estimates derived from the changes in compulsory schooling duration can be interpreted as a local treatment effect (LATE) for compliers (Imbens & Angrist, 1994; Angrist, Imbens, & Rubin, 1996).

The LATE only for the complier group may imply a heterogeneity bias. Card (1999) argues that a compulsory school policy instrument would cause a positive bias in the IV estimates if the CSL affects the people with low schooling who would have higher marginal benefits from schooling. Obviously, this requires assuming that the return to schooling is negatively correlated with the schooling level across population. There is a similar explanation for why the return for women is expected to be higher. Given women's education levels and wages are relatively lower, therefore, the return to education (marginal benefit) for women is expected to be higher.

5.3 The Model

Using two-stage least squares estimation the causal effect of education on wages

and various other employment variables are analysed. The generic first stage equation for these IV models is given as follows:

$$S_i = \alpha_0 + \alpha_1 Treat_i + \sum_{j=26}^{35} \alpha_j Age_{j,i} + \sum_{t=2008}^{2022} \alpha_t Year_{t,i} + X'_i \gamma + \varepsilon_i \quad (5.3)$$

Where S_i denotes the received years of schooling for the observation i . Observation i is an individual in a year which the survey is conducted. Since the survey is conducted via repeated sampling with replacement, it is possible to observe same individuals more than one time in different survey years. However, we cannot know if the individual is the same person because the dataset is not longitudinal, i.e., does not have individual identifiers.

$$Treat_i = \begin{cases} 1 & \text{if } birth\ year \geq 1987 \\ 0 & \text{if } birth\ year \leq 1986 \end{cases} \quad (5.4)$$

$Treat_i$ is treatment dummy for observation i , where it takes the value 1 if the year of birth of the individual in observation i is greater than or equal to 1987, it takes the value 0 if it is less than 1987. Similarly, $Age_{j,i}$ is the age dummy for the age j for the individual of observation i , and $Year_{t,i}$ is the survey year dummy for the year t for observation i . Lastly, X'_i is the covariate vector. Covariates consist of NUTS2 level region dummies, a dummy for current marital status, and a linear trend variable for the birth cohort. An additional quadratic birth cohort trend is also added for some cases.⁵ Actually, having both age and survey year dummies automatically controls for the liner birth cohort trends (O'Brien, 2000). Since HLFS collects data at the different months of the same year, there exist different birth cohort observations with the same age in the same survey year, i.e., there exist birth cohort variation within both age and survey year. This variation prevents multicollinearity and allows adding a birth cohort trend. Errors are clustered at the

⁵ Results given in the tables 6.1, A1 and A3.

year and month of birth level. Clustering at this level gives enough clusters to calculate healthy first stage F statistics. Instead, using robust (heteroskedastic-consistent) standard errors produce significantly smaller standard errors but not preferable.

The second stage equation for log wage and other employment variables are given in the equation 5.5 as follows:

$$Y_i = \beta_0 + \beta_1 S_i + \sum_{j=26}^{35} \beta_j Age_{j,i} + \sum_{t=2008}^{2022} \beta_t Year_{t,i} + X'_i \delta + \epsilon_i \quad (5.5)$$

Y_i is the variable of interest for the observation i . It denotes the outcome of interest such as $\ln(wage)$ measured either in monthly or hourly frequency, or monthly working hours. β_1 can be interpreted as the Local Average Treatment Effect (LATE), i.e., the causal relationship between education and wages. This is the rate of return from education in the case of $\ln(wage)$. There are arguments both in favor of hourly wage and monthly wage which includes hours of work. Based on US data, Card (1999), argues that more educated people tend to work more due to the rise in the opportunity cost of not working. It means substitution effect dominates the income effect for leisure. So, the additional hours worked with increasing education is an endogenous part of the return to education. Therefore, hourly wage may reflect the productivity effects of education of worker better (Gunderson & Oreopoulos, 2020). However, monthly wage is also a common measure to calculate return to education. It gives a better idea about the living condition of an individual. Happily, HLFS contains data on usual weekly hours worked and the hourly wage can be calculated separately.

Moreover, regarding certain variables of interest, we introduce Y_i as a binomial variable which take the value 1 if the observed individual is in the labor force, or in a specific kind of employment such as full-time worker, wage worker, unpaid family worker, self-employed, employer, registered, public worker, in a second job, in a small or a big company. It takes the value zero if the person is not employed.

5.4 Other Identification Issues

Classical measurement error is random and OLS estimates may overstate merging with omitted variable bias and it can also understate returns by overcoming omitting variable bias. IV estimates produce consistent results with the classical measurement error assumptions. On the other hand, non-classical measurement error where the error is systematically correlated with other unobserved variables is more problematic. In that case, IV estimates may not produce consistent results. For example, if the measurement error is correlated with the education level, there are different non-classical scenarios possible.

Consider the first scenario where education level is negatively correlated with the measurement error. That non-classical measurement error causes OLS to overestimate returns to education. People with the lowest education levels cannot underreport their education levels. Additionally, people with low education levels may be more prone to over report education levels due to some inferiority complex or simple carelessness, while people with more education may be more careful and accurate. This would cause an artificial inflation of the education levels only for the people with low education levels. Remember that the simple IV estimate of the return to education can be composed as the ratio of wage differences by 1997 CSL treatment status to the differences by treatment status in average years of schooling. While measurement error in educational levels does not affect the numerator of this ratio but decreases the denominator in this case. As a result, the estimate would overestimate the returns to education (Kane, Rouse & Staiger, 1999; Gunderson & Oreopoulos, 2020).

Consider another scenario where education level is positively correlated with the measurement error. That non-classical measurement error causes OLS to underestimate returns to education. People with high education level have more opportunity to exaggerate their education levels given that various types of degree granting institutions are available at their disposal to show a more ostentatious background. This would cause an artificial inflation of the education levels only for

the people with high education levels causing an underestimation of returns to education (Card, 1999; Kane, Rouse & Staiger, 1999; Gunderson & Oreopoulos, 2020).

Additionally, people tend to report degree attainments more accurately than educational attainments. OLS estimates tend to overstate returns to degree attainment because people forget to report the extra education received without a received degree (Kane, Rouse & Staiger, 1999). In our case, using degree attainments to calculate schooling years avoid classical measurement error but causes a non-classical one. Aydemir & Kirdar (2017), argues this non-classical error is small based on comparison from TDHS, and argues IV estimation solves this small problem if the instrument is only correlated with real years of schooling but not with the measurement error.

CHAPTER 6

RESULTS

In this section, the analysis begins by exploring the impact of the 1997 compulsory schooling reform on educational outcomes. Further, adequacy of the instrument will be justified via first stage results. Subsequently, the causal effect of receiving additional year of schooling on both hourly and monthly wages will be examined for women and men. In light of the human capital and signaling theories, the underlying mechanisms that explain the returns to education will be explored. The discussion seeks to discern if the observed positive returns to education indicate a causal relationship or if other channels are at play. This discourse will be further enriched with more in detail examination of the employment types to inspect whether is there a compositional change in the treatment group threatening the causal inferences. Finally, attributes related to employment, such as informality, public sector, company size, and engagement in secondary employment, will be evaluated.

6.1 Policy Effect on Degree Attainments

The fundamental premise of this study is that people born just before and after a specific date should be similar in observed and unobserved attributes, and therefore labor market outcomes. The argument is that the compulsory schooling reform does not introduce any systemic variations between these two groups, apart from the exogenously induced increase in schooling years. This exogenous rise in schooling, instigated by the policy, serves as an instrument. Figure 6.1 displays this jump in middle school completion brought about by the increase in compulsory schooling. The average proportion of individuals having completed at least eight

years of basic education or middle school (the 8th grade) is denoted by red crosses for women and blue circles for men. A similar jump is discernible in Figure 6.2 for high school completion, indicating a spillover effect onto high school. Both figures incorporate a fitted linear line. This graphical evidence, illustrating the policy's influence on degree attainment, will be examined further in Table 6.1 using ordinary least squares (OLS) regression estimations. These estimations include basic controls such as age fixed effects, NUTS2-level regional fixed effects, year fixed effects, marital status, and a linear trend for birth year.

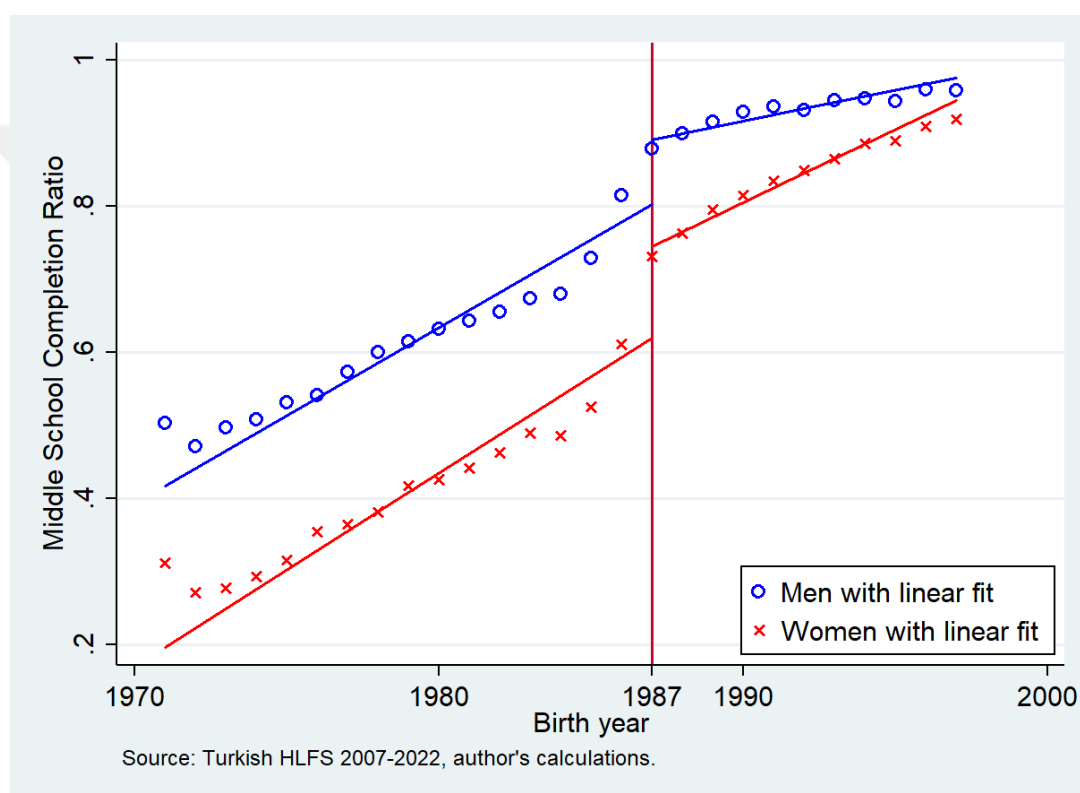


Figure 6.1: Average Middle School Degree Attainment by Birth Cohort

In Table 6.1, the first two panels present results for the whole sample, aged between 25-35. The last panel displays analogous outcomes for a restricted birth cohort interval, spanning 1983-1990. Several rationales underpin this cohort restriction. Primarily, narrowing the birth cohort might yield more precise outcomes, as trend issues are less of a concern. The inherent structure of the instrument precludes controlling for fixed birth cohort effects, as explained in the identification section. Given that the 1997 compulsory schooling law come into play

for those born in 1987 onward, no treatment variation exists within birth years, permitting only controls for trend effects (linear or higher degree).

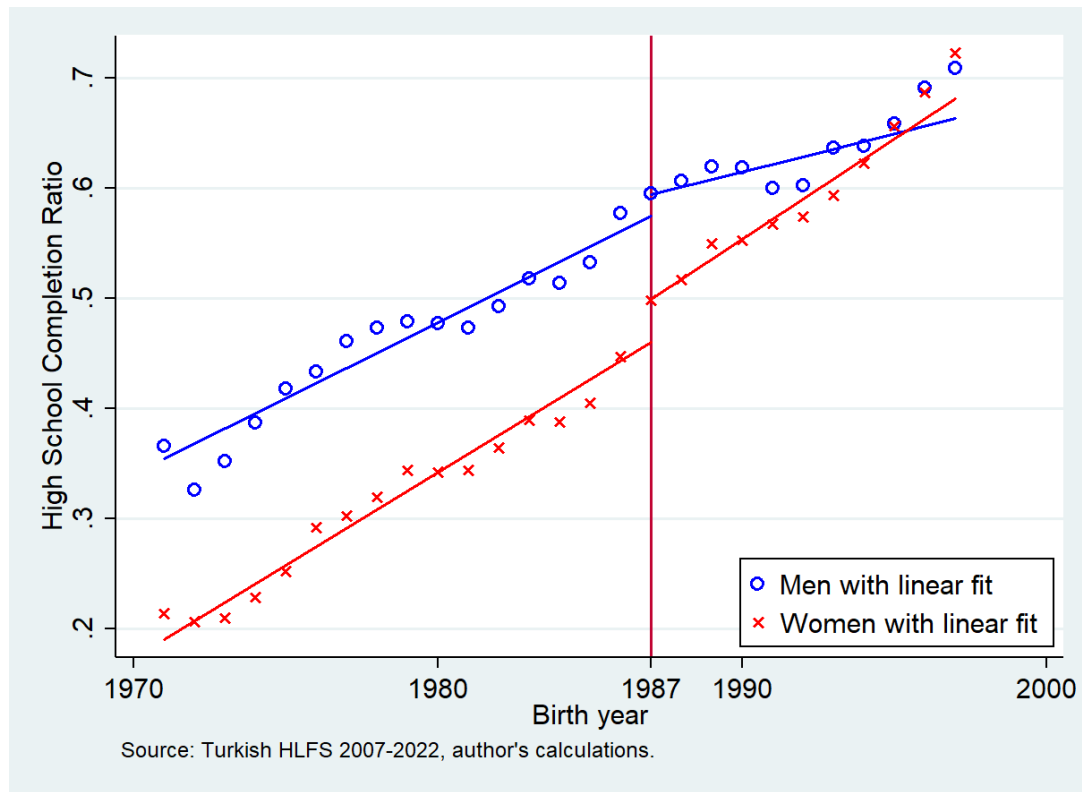


Figure 6.2: Average High School Degree Attainment by Birth Cohort

Secondly, restricting the dataset fosters more congruent treatment and control groups concerning both sample size and generational differences. Prior to this birth year limitation, the control group's sample size was twice that of the treatment group; this ratio was subsequently reduced to 1.26 post-restriction. While augmenting the control group's sample size doesn't introduce bias, it would shrink the standard errors.

Third, we cannot control for birth region fixed effects due to the unavailability of data. By truncating birth cohorts, one can potentially mitigate that potential regional heterogeneity such as disparities in school quality.⁶

⁶ This does not cause any bias since our methodology does not depend on the regional differences to infer causality. It's worth noting pertinent findings of Stephens & Yang (2014), demonstrated that

Lastly, there may be other policies affecting the variables of interest. For instance, there is another policy extending the duration of high school by one year for people born in and after 1991.⁷ Restricting the birth cohorts narrows the focus exclusively on the 1997 compulsory schooling reform. In sum, adopting a narrower birth cohort window is more prudent.

In Table 6.1, Panel A presents the OLS results incorporating basic covariates, while Panel B integrates the same outcomes with an added quadratic time trend for the same birth cohorts. Evidently, across both panels, the reform appears to be highly effective for the attainment of middle school for both genders. In panel B, reform seems to have spillover effects to high school degrees for both genders. Furthermore, the spillover effects seemingly extend to university degrees.⁸ This finding, at first sight, seems to contradict with the findings of Aydemir & Kırdar (2017). There are two plausible explanations for this observed college spillover effect. Firstly, there may be an unintentionally captured influence of another policy. The number of universities in Türkiye began to rise with a marked surge starting in 2006, an aggressive expansion with the goal of establishing a university in every city. Fifty new universities were inaugurated between 2006 and 2008. The affected generations from that university expansion policy coincides with the generations in our treatment group and becomes more pronounced for subsequent generations. That is, policy exposure intensifies with each subsequent birth year.⁹

Secondly, the observed effect could stem from not properly accounting for birth cohort effects, given that the influence wanes in magnitude and loses its statistical significance upon cohort restriction. Both the middle school and high school degree effects also diminish with cohort restriction causing the high school effect for men

not controlling for the region and year of birth fixed effects and their interactions causes totally biased results for studies where the causality is inferred from the state level differences in USA.

⁷ Karabilgin (2020) examines this policy, uncovering effects promoting school enrolment and against child labor.

⁸ 4th columns are for graduate level studies including both master and doctoral level degree holders and also students enrolled to these programs. Number of observations fall sharply because the information for post graduate education is provided only in the surveys from years 2014-2022.

⁹ Caner et al (2022) and Polat (2017) investigates the impact of this higher education expansion.

to turn statistically insignificant, further supports the latter explanation. Finally, the introduction of a quadratic time trend makes almost no difference for the restricted cohorts (results not presented in the table) meaning a narrowed birth cohort range seems to flatten the curvature.

Table 6.1: Policy Effect on School Degree Attainments

	Dependent Variables:	Women			
		(1)	(2)	(3)	(4)
		Middle School Degree	High School Degree	College Degree	Graduate Degree or Student
(A) Birth Cohorts: 1971-1997	Treatment	.164*** (.010)	.051*** (.007)	.037*** (.006)	-0.001 (.002)
	Number of Observations	656911	656911	656911	357649
	F-stat	703.858	693.114	281.478	66.594
	Quadratic Trend for Birth Cohort	No	No	No	No
(B) Birth Cohorts: 1971-1997	Treatment	.166*** (.010)	.053*** (.008)	.018*** (.005)	-.001 (.002)
	Number of Observations	656911	656911	656911	357649
	F-stat	717.139	682.194	366.594	65.148
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes
(C) Birth Cohorts: 1983-1990	Treatment	.111*** (.014)	.043*** (.012)	.008 (.008)	-.003 (.002)
	Number of Observations	289653	289653	289653	220718
	F-stat	559.964	591.652	207.456	54.311
	Quadratic Trend for Birth Cohort	No	No	No	No

		Men			
		(1)	(2)	(3)	(4)
	Dependent Variables:	Middle School Degree	High School Degree	College Degree	Graduate Degree or Student
(A) Birth Cohorts: 1971-1997	Treatment	.104*** (.011)	.013 (.008)	.022*** (.006)	.005** (.002)
	Number of Observations	610663	610663	610663	335448
	F-stat	264.731	241.793	196.806	24.929
	Quadratic Trend for Birth Cohort	No	No	No	No
(B) Birth Cohorts: 1971-1997	Treatment	.121*** (.010)	.028*** (.008)	.018*** (.006)	.0036 (.0025)
	Number of Observations	610663	610663	610663	335448
	F-stat	342.76	218.648	202.432	24.22
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes
(C) Birth Cohorts: 1983-1990	Treatment	.05*** (.015)	.013 (.013)	.003 (.009)	0 (.003)
	Number of Observations	272870	272870	272870	207207
	F-stat	208.858	221.363	153.218	18.142
	Quadratic Trend for Birth Cohort	No	No	No	No

Coefficients are given for the treatment dummy. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variable.

Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

To sum up, the policy increases the average rate of middle school degree completion by 11 percentage points (pp) for women and 5 pp for men. The spillover

effects persist till high school degree accounting for an increase of 4 pp only for women. However, spillover effects do not extend beyond high school for either gender. The first stage regression results on the effect of policy on years of schooling are given in Table 6.2. The 'average years of schooling' metric derived from degree attainment data, suggests that the policy augmented the average schooling years for women by 0.5 years and, it's half that at 0.2 years for men with 11% significance. Compared to prior studies, these policy effects on avg. years of schooling are smaller. Torun (2018) reports 0.2 years less than these effects for both genders. First stage F-statistics of the two-stage least squares (TSLS) estimates in table 6.3 are 68.386 for women, and 147.85 for men. First stage F-statistics for the TSLS estimates in tables 6.5 and 6.6 are even higher with 647.39 for women and 216.055 for men. These high F statistics in the first stage results shows that the instrument in use is effective.

Table 6.2: Policy Effect on Average Years of Schooling

		Women	Men
		Average Years of Schooling	Average Years of Schooling
(A) Birth Cohorts: 1971-1997	Treatment	0.667*** (0.097)	0.424*** (0.081)
	Number of Observations	656911	610663
	F-stat	948.732	282.212
	P - Value	0.000	0.000
(B) Birth Cohorts: 1983-1990	Treatment	.456*** (0.130)	0.204 (0.125)
	Number of Observations	289653	272870
	F-stat	647.39	216.055
	P - Value	0.001	0.106

Coefficients are given for the treatment dummy. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variable.

Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

6.2 Returns to Education

Table 6.3: Monetary Returns to Education

	Dependent Variables:	(1) OLS Monthly Wage	(2) TSLS Monthly Wage	(3) OLS Hourly Wage	(4) TSLS Hourly Wage	(5) TSLS Working Hours
Women	Years of Schooling	0.075*** (0.001)	0.097* (0.057)	0.095*** (0.001)	0.140** (0.064)	-8.633* (4.636)
	Number of Observations	67758	67758	67758	67758	67758
	P - Value	0.000	0.091	0.000	0.028	0.063
Men	Years of Schooling	0.059*** (0.000)	0.07 (0.06)	0.075*** (0.001)	0.067 (0.067)	0.722 (5.371)
	Number of Observations	168488	168488	168488	168488	168488
	P - Value	0.000	0.239	0.000	0.313	0.893

Given coefficients are for the variable years of schooling. Dependent variable for the: model (1) & model (2) is natural logarithm of real monthly wage; for model (3) & model (4) is natural logarithm of real hourly wage, model (5) is total monthly working hours.

All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variables.

First stage F-statistics are 68.386 for women, 147.85 for men.

Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

Table 6.3 shows the educational returns for both women and men in terms of monthly and hourly wages for full-time wage earners. The results are only for the restricted birth cohorts but results for the unrestricted birth cohorts can be found in the appendix Table A1. Considering the second column of the table, an additional year of schooling causes a 10% increase in monthly wages for women with a 10% significance. A 7% increase for men is observed but is not significant at conventional levels, p-values for the estimates are provided below. Using monthly wages as the unit of measure, Torun (2018) finds the return to schooling for women to be on the same order, 10%, and a 1.7% return to schooling for men (significant at the 10%

significance level). Given that our dataset incorporates the data set used by Torun (2004-2011 rounds of the HLFS), we replicated the same results as a robustness check.

Turning to hourly wages, we find that the hourly wage returns surpass those of monthly wages for women, with 15%¹⁰ return per additional year of schooling. For men, we find a 6.7% return, but it is again not statistically significant at conventional levels. Mocan (2014) reports a near-identical 14% return for women's hourly wages, and a statistically insignificant 2% return for men. On the other hand, Aydemir & Kirdar (2017), finds 7-8 % return for women and again an insignificant 2-2.5% for men.

In conclusion, the returns for women's hourly wages were founded to be twice as high as those reported by Aydemir & Kirdar (2017). While the returns for men were not statistically significant, the findings were notably higher than those in prior studies, including that by Mocan (2014) and Torun (2018). This variation may be attributable to the difference in the age composition of the samples across studies. Earlier research has been largely confined to early labor market outcomes for individuals mostly in their early 20s due to data limitations. However, leveraging the most recent rounds of the Household Labor Force Survey (HLFS) enables us to incorporate data from individuals in their 30s who were affected by the 1997 Turkish Compulsory Schooling Reform. Therefore, this difference may emerge from the lasting effects of the reform on post-school human capital accumulation. As mentioned in the theoretical background section, education amplifies post-school human capital investment, leading to an increase of the wage gap between educational levels as individuals age—particularly among individuals in their 30s as compared to those in their early 20s. Experience may benefit the educated better through several channels. Education can help people to find jobs that are better matches for them. Education increases the available options and sends the signals

¹⁰In log-linear regressions, a coefficient of 0.14 corresponds to a $100(e^{0.14} - 1)\% = 15\%$ increase in the dependent variable. The percentage change and the coefficient approximate to each other more accurately for coefficients with magnitudes less than 0.1.

to the market about their ability and skills. High-skilled jobs may be more open for improvement in terms of productivity compared to low skilled jobs where simpler and more repetitive tasks are present. Although, this observation is visually supported in Figure 2.3: “Age-Earnings Profile by Education Groups”, the research design of this study does not allow us to test these explanations.

When comparing Instrumental Variable (IV) to OLS estimates, IV estimates are 30% higher for women’s monthly wage, and 50% higher for hourly wages Consistent with Card (1999)’s findings. This suggests a pronounced measurement error bias in OLS estimates. The disparity between monthly and hourly wages for women indicates a reduction in monthly working hours. Column 5 of Table 6.3 reveals that an additional year of education leads to a significant decrease (by 8.6 hours) in monthly working hours for women, conditional on full-time wage employment. Such a marked reduction in work hours is not observed for men, as evidenced by their statistically insignificant monthly and hourly wage returns, both hovering around 7%. This is unexpected given Card (1999) suggests more educated people would work more, at least in U.S context. In Turkish context, It seems the income effect of the return from education is dominant for women. Models without a quadratic birth cohort trend yield same results. Furthermore, results remain robust with the data where wage outliers are not trimmed. In summation, these findings endorse the excess returns argument both for women compared to men and for developing countries compared to developed countries.

6.3 Policy Effect on Employment & Labor Force Participation

A potential reason for the wage increase among women identified in the previous part can be employment selection; if education motivates individuals to actively seek employment or shift towards wage employment, this could engender a compositional change. Table 6.4 probes this possibility. The first three columns present the policy's impact on the likelihood of being in the labor force, employment, and full-time employment, respectively. All these effects are statistically insignificant. Yet, an examination of the fourth and fifth columns reveals

that the probability of being in the wage employment rises by 1.2 pp, and the probability of being in the full-time wage employment increases by 0.9 pp. Given that 24% of women in the sample are full-time wage workers, this represents less than 4% increase. This suggests a minor compositional shift, particularly since the wage analysis includes full-time wage earners only.

Table 6.4: Policy Effects on Labor Force Participation and Employment

	Dependent Variables:	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
		LFP	Emp.	Full-Time Emp.	Wage Emp.	Full-Time Wage Emp.
Women	Treatment	0.006 (0.005)	0.007 (0.005)	0.006 (0.005)	0.012** (0.005)	0.009* (0.005)
	Number of Observations	289653	289653	289653	289653	289653
Men	Treatment	0.002 (0.004)	-0.001 (0.005)	-0.003 (0.005)	0.004 (0.007)	0.003 (0.007)
	Number of Observations	272870	272870	272870	272870	272870

Coefficients are given for the treatment dummy. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variables.

Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

In conclusion, there appears to be a small compositional shift wherein education propels women into wage employment. This transition would not be problematic if the new entrants to full time wage employment resemble the control group in terms of unobserved traits. However, if these new entrants differ based on unobserved factors, such as motivation, ability, or family structure, it introduces potential bias to the results. Aydemir & Kirdar (2017) observed a similar trend and cautioned about this very issue. Nonetheless, they argue that this concern should not be overstated. Their rationale is that the newcomers to full-time wage employment are not very different to the untreated employed women, given that these individuals were teetering on the margin of employment prior to the policy. In simpler terms, the people who are at the merge of dropping or joining the wage employment pool are likely to be similar.

6.4 Education's Effect on Other Labor Market Outcomes

The observed monetary returns to education, in terms of wages, are substantial and statistically significant for women. In contrast, for men the returns are not statistically significant though the estimated effect size is around 7%. As delineated in the theoretical background chapter, education increases wages through human capital investments boosting productivity or possibly serve as a signal to prospective employers. However, this wage increase can be orchestrated through various channels in practice. A deeper examination into the influence of education on employment trajectories can shed light on the pronounced returns observed for women. Table 6.5 presents the two-stage least squares (TSLS) estimates pertaining to diverse employment conditions, resultant from an additional year of schooling. The first three columns show no causal education impact on labor force participation, employment, and full-time employment as in line with the findings of the compositional policy effects in Table 6.4. However, the fourth and fifth columns show additional year of education increases the probability of being a wage earner by 2.7 pp and decreases the probability of being an unpaid family worker by 1.5 pp. Compared to sample averages for women, these effects correspond to a 10% increase and a 22% decline respectively.

Given that the probabilities for being self-employed or an employer remain statistically insignificant as demonstrated in columns (6) and (7), it can be inferred that education has played a pivotal role in shifting the role of women from unpaid familial roles to salaried positions. Put differently, those women, who might have otherwise been unpaid family workers, became wage earners thanks to their extended educational attainment forced by the law. This narrative seems to diverge from the conclusions drawn by Torun (2018) and Gülberk (2023). Whereas Torun (2018) found no wage employment effects for women, and Gülberk (2023) argues the missing employment effects can be due to marriage related reasons. It's worth noting that although marital status has been factored in as a basic covariate, reiterations of the analysis without marital control variables do not alter the

employment effects (results reported in Appendix Table A2). Moreover, the results men given in the second panel of the table show that the compulsory policy change none of the employment outcomes.

Table 6.5: Education's Effects on Labor Force Participation and Work Status

	Dependent Variables:	(1) TSLS	(2) TSLS	(3) TSLS	(4) TSLS	(5) TSLS	(6) TSLS	(7) TSLS
		LFP	Employed	Full-Time Worker	Wage Worker	Unpaid Family Worker	Self-Employed	Employer
Women	Years of Schooling	0.014 (0.011)	0.016 (0.013)	0.013 (0.012)	0.027*** (0.010)	- 0.015** (0.007)	0.003 (0.003)	0.001 (0.002)
	Number of Obs.	289653	289653	289653	289653	289653	289653	289653
Men	Years of Schooling	0.012 (0.017)	-0.007 (0.025)	-0.017 (0.031)	0.02 (0.025)	-0.002 (0.012)	-0.022 (0.019)	-0.004 (0.012)
	Number of Obs.	272870	272870	272870	272870	272870	272870	272870

Coefficients are given for the instrumented variable years of schooling. Dependent variable for the: model (1) is Dummy for Labor Force Participation, model (2) is dummy for employment, model (3) is dummy for wage employment, model (4) is dummy for full-time employment, model (5) is dummy for self being unpaid family worker, model (6) is dummy for self-employment, model (7) is dummy for being an employer. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variables. First stage F-statistics are 647.39 for women, 216.055 for men. Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

Table 6.6 further indicates that education augments the likelihood of women being employed in positions covered by social security by 3.2 pp, an increase of 13%. Likewise, the probability of women being employed in the public sector rises by 2.4 pp, marking a 28% increase. It is no surprise given most of the public positions requires a degree, generally high school or higher. Furthermore, the likelihood of women being employed in establishments with over 50 employees escalates by 1.8 pp, an increase of 17%, while no significant impact is observed in smaller companies. Consistent with previous employment outcomes, these effects remain statistically insignificant for men. Analogous outcomes for employment, without the birth cohort restrictions, are documented in the Appendix Tables A3 and A4. These

employment effects are also similar in direction for the models with weighted regressions, and with data where wage outliers are not trimmed.

Table 6.6: Education's Effects on Work Status (Continued)

	Dependent Variables:	(1) TSLS	(2) TSLS	(3) TSLS	(4) TSLS	(5) TSLS
		Social Security	Public Employee	Second Job or More	Small Company	Big Company
Women	Years of Schooling	0.032*** (0.009)	0.024*** (0.006)	-0.002 (0.002)	-0.009 (0.011)	0.018** (0.009)
	Number of Observations	289653	289653	289653	289653	289653
Men	Years of Schooling	0.011 (0.036)	0.006 (0.025)	0.012 (0.008)	-0.034 (0.025)	0.007 (0.029)
	Number of Observations	272870	272870	272870	272870	272870

Coefficients are for the instrumented variable years of schooling. Dependent variable for the: model (1) is dummy for formal employment, model (2) is dummy for public employment, model (3) is dummy for having more than one job, model (4) is dummy for being employed in a small company with 10 or less people, model (5) is dummy for being employed in a big company with 50 or more people. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variables. First stage F-statistics are 647.39 for women, 216.055 for men. Standard errors are in parentheses, clustered at year and month of birth level.

*** $p < .01$, ** $p < .05$, * $p < .1$

6.5 Conclusion

Considering the provided data in Table 6.7 where the descriptive statistics are given for the degree attainments for all the individuals between 25-35. It is clear that women in Türkiye exhibit lower educational levels. Additionally, in Table 6.8, descriptive statistics for the labor market outcomes such as labor force participation, employment, monthly work hours, and real wage variables (in 2003 consumer prices) are given. Real wage variables and monthly work hours are only for full time wage workers. Similar to the educational levels, women also lag behind in labor force participation and employment rates. Therefore, it is expected the education's marginal benefit for women might be higher than their male

counterparts. This expectation requires assuming that education has diminishing returns. Put it differently, the return to education is higher for low education levels. As a result, the identified return for women is non-surprisingly higher.

Table 6.7 Descriptive Statistics for Degree Attainments

	Variables:	Primary School	Middle School	High School	College	MS/PhD
Women	Years of Schooling	0.868469	0.566452	0.420131	0.228116	0.024157
	Standard Deviation	0.338	0.496	0.494	0.420	0.154
	Number of Observations	671469	671469	671469	671469	366852
Men	Average	0.955985	0.738759	0.531027	0.251716	0.025103
	Standard Deviation	0.205	0.439	0.499	0.434	0.156
	Number of Observations	622225	622225	622225	622225	342425
Total	Years of Schooling	0.910562	0.649326	0.473468	0.239467	0.024614
	Standard Deviation	0.285	0.477	0.499	0.427	0.155
	Number of Observations	1293694	1293694	1293694	1293694	709277

Source: Turkish HILFS 2007-2022 (Age: 25-35)

On the other hand, the TLSL estimates in table 6.4 revealed a somewhat surprising trend: women tended to work fewer hours with the additional years of schooling. This reduced work time leads to higher hourly wage returns than monthly wage returns. It may be women value the leisure time more as they earn more at the hourly rate, leading to fewer working hours — an instance where the income effect outweighs the substitution effect. However, the change in the work hours may not happen that smoothly. The main reason for this result seems to be because the improvement in the employment status. Women are transitioned into safer jobs where the work hours are regulated. Education decreased the probability of women being employed with a social security and employed as an unpaid family worker; and increased the probability of being a wage worker or working within the public

sector or sizeable corporations with 50 employees or more. These changes indicate that education not only boosts the earning potential of women but also does so by creating pathways to improved job conditions and security.

Table 6.8 Descriptive Statistics for Labor Market Outcomes

Variables:		LFP	Employment	Real Monthly Wage	Real Hourly Wage	Monthly Work Hours
Women	Years of Schooling	0.403664	0.3412503	664.7218	3.625862	199.427
	Standard Deviation	0.491	0.474	346.199	2.303	43.182
	Number of Observations	671469	671469	143370	143370	149213
Men	Average	0.92913	0.8329607	641.5809	3.11267	222.757
	Standard Deviation	0.257	0.373	335.181	2.008	48.963
	Number of Observations	622225	622225	374250	374248	392580
Total	Years of Schooling	0.656396	0.5777471	647.9905	3.254814	216.332
	Standard Deviation	0.475	0.494	338.427	2.107	48.573
	Number of Observations	1293694	1293694	517620	517618	541793

Source: Turkish HLFS 2007-2022 (Age: 25-35)

To sum up, education increased women's hourly wages by potentially enhancing their productivity and sending its signal to the labor market through diplomas. It has also reduced their working hours given they are wage employees, shifted their employment status from unpaid family roles to wage-based jobs, and transitioned them into positions with social security coverage, in the public sector, and in larger companies where the wages are higher. However, the evidence isn't as definitive for men as it is for women.

CHAPTER 7

CONCLUSION

This research was embarked upon with the prime intention to delve into the ramifications of the 1997 Compulsory Schooling Law (CSL) in Türkiye. A significant shift in the education levels, the CSL promised a substantial instrument in how education would interface with labor market outcomes. Leveraging prior foundational works, specifically those of Aydemir & Kırdar (2017) and Torun (2018), this investigation probed into the long-term effects of the law focusing on individuals who were in their late 20s and early 30s. It uses one of the largest data currently available from 16 rounds of Turkish HLFS containing the total of 1,293,694 observations in the given age bracket.

The findings revealed the pronounced impact of the CSL on education. Middle school completion rates witnessed a considerable rise, with an unexpected yet heartening spillover seen in high school attainments for women. These educational surges bore direct consequences for the labor market. The TSLS estimates show that women, with every additional year of schooling, reaped considerable rewards, not just in terms of wages but also in employment quality. The shifts towards more stable jobs with better conditions underscored the overarching positive influence of education on employment trajectories. To clarify, women shifted employment roles from unpaid family worker to registered wage worker in a large company or in public sector. However, the narrative for men was more nuanced. While there was a statistically insignificant rise in their wages, the suggestive evidence was higher than previous studies. Men were missing all these improvements on their employment conditions.

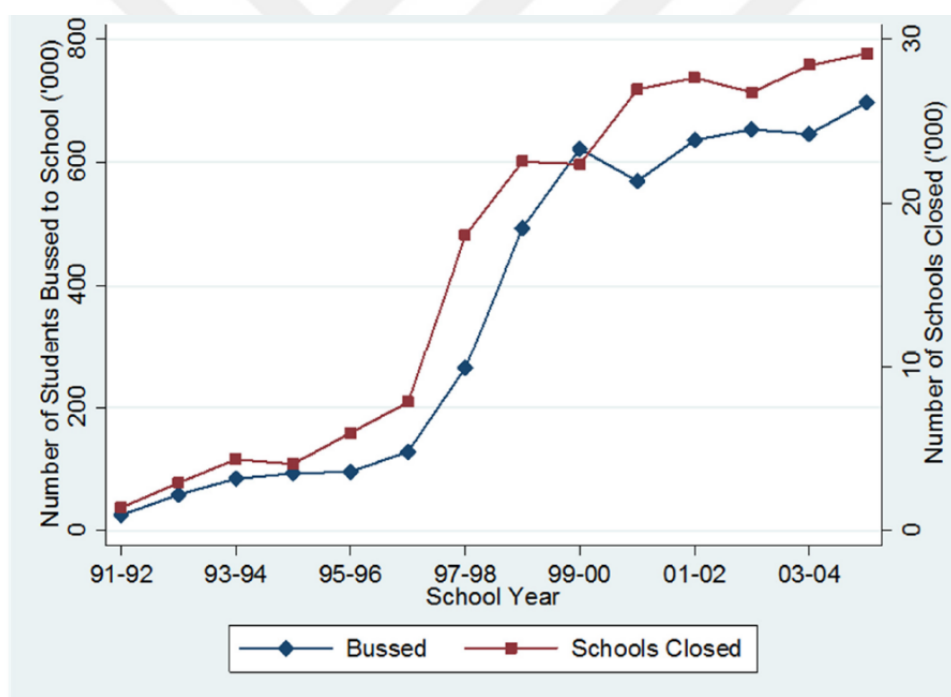
While the insights drawn from this study are substantial, it's critical to acknowledge its limitations. Firstly, the time trends for the birth cohorts are not easy to identify which forced us to focus on a restricted birth cohort interval. It weakened the statistical power of analysis by significantly decreasing the sample size. The methodology employed, though rigorous, also has its constraints. The instrumental variable approach delivers findings predominantly centered on a specific set of compliers. Local average treatment effect (LATE) for these compliers may measure the marginal effect for people mostly in rural areas. The disadvantaged students in rural areas may have higher marginal benefit from education comparing the average student in the country. To conclude, the extrapolation of these results to the entire population needs a cautious approach.

On the other hand, there are not many examples in the world where a nation-wide compulsory schooling reform impacted a significant fraction of targeted students. Roughly 1.5 million pupils were added to the education system, a sudden increase by more than 15% within just 3 years after the reform first implemented. Observing this type of strong policy impact with the available data is not possible for the developed countries where the enrolment rates were already high. Moreover, there are not many studies for the developing countries¹¹. This may allow us to observe the causal effect derived from a larger group of compliers, not only from the children at the margin. In other words, since the 1997 CSL impacted a big part of the school age students at the time, this can result LATE to be close to ATE (Average treatment effect). This is one of the reasons why this education reform in particular became popular in the empirical literature as elaborated in chapter 3.

Another important, perhaps unintended, result of the reform is its effect promoting gender equality in education. Since the Turkish education system is unified, does not segregate genders, the reform was implemented to all. Torun (2018) and this study have consistently found larger policy impact for girls' degree attainments

¹¹ To our knowledge, there are only a handful of studies investigating sizeable compulsory schooling policies on developing countries. Spohr (2003) & Tsai et al. (2009) investigates Taiwan's 1968 compulsory school reform, and Fang et al. (2012) studies China's 1986 compulsory schooling reform.

both for middle school and high school. It suggests education policies targeted to whole students may help to escalate already narrowing schooling gender gap¹². From a vast empirical literature, Orazem & King (2007) concludes the price elasticity of demand for women is higher than boys in developing countries where girls' enrolment rates are lower than boys. Which means since the 1997 CSL decreased the cost of schooling in some form, so that the girls' demand for schooling increased higher than boys. The policy seems to lift a number of impediments blocking girls' entrance to the education system. As mentioned in the chapter 4, the policy is implemented with the comprehensive supporting programmes. These include massive infrastructure investments to build new schools and hiring new teachers. The ministry (MEB) also expanded the school bussing program to transport the students located in rural areas.



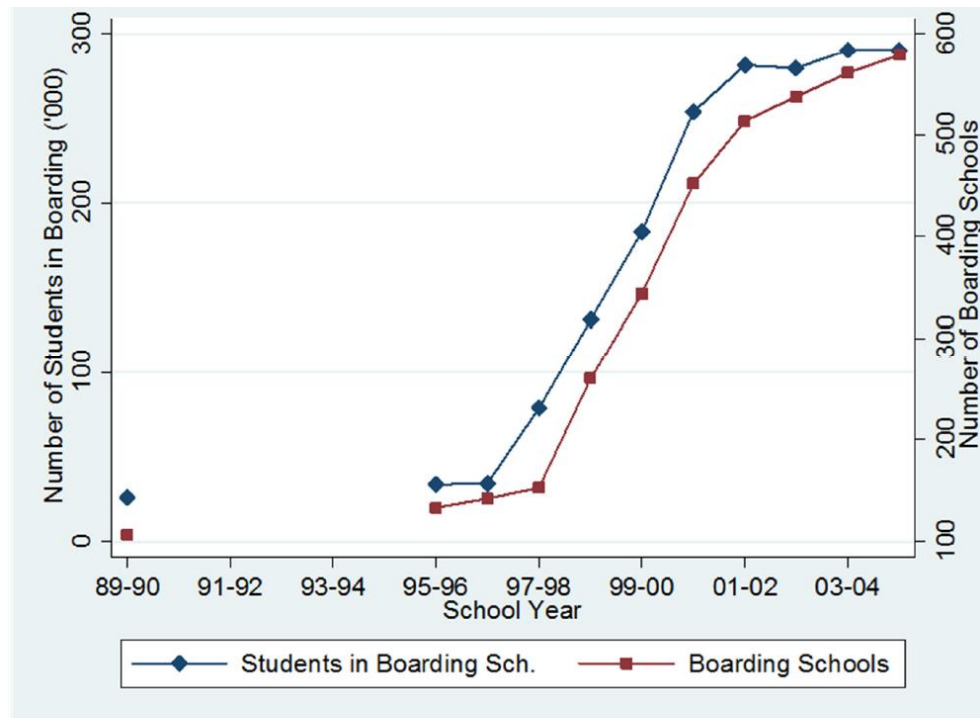
Source: Ministry of National Education (1992-2005).

Figure 7.1: Students Bussed to School - School Closures (Kirdar et al., 2016, Figure S3)

The policy also closed some of the small village schools which are unable to provide the comprehensive compulsory education for all the grades from 1 to 8. There was

¹² Although there is some counter evidence by Kirdar, Dayioğlu & Koç (2016)

also significant amount of boarding school built. These supportive policies are visible in the figures 7.1 and 7.2.



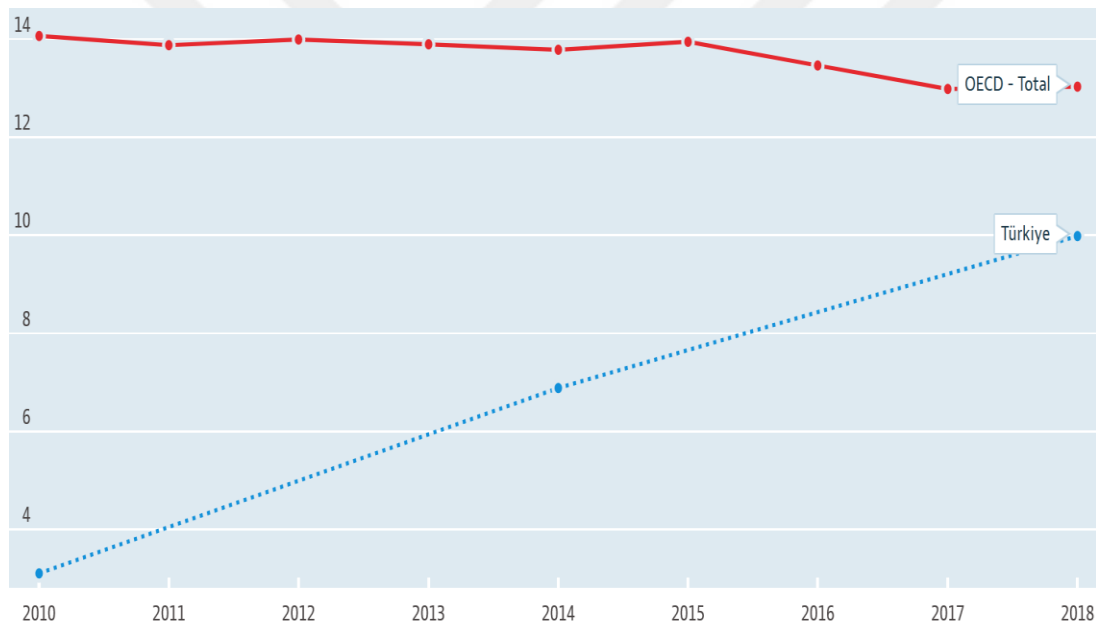
Source: Ministry of National Education (1990, 1996-2005).

Figure 7.2: Boarding Schools (Kirdar et al., 2016, Figure S4)

The low-income households are also the ones expected to have higher discount rates which can cause them irrationally myopic schooling decisions. The 1997 CSL may work against this decision problem. The reform poses some legal penalties who fails to meet its requirements, from monetary fines up to some jail time. It would increase the psychological/mental cost of not sending kids to school. Some of the low-income households also may suffer from lack of information. If so, they undervalue the returns to education. Although the time spend in the school may inform both the parent and the child, the reform may be a remedy only to some extent. There may be additional supportive campaigns promoting basic education in action¹³. This may also decrease the cost of girls' education in terms of social norms.

¹³ "The girls Education Campaign in Turkey" ("Haydi Kızlar Okula" in Turkish) launched in 2003. It was a very good and successful example of such a campaign.

For developing nations like Türkiye, we believe a particularly effective approach to narrowing this gap involves the formulation of robust educational policies that ensure women and men have equal opportunities in the job market. The dichotomy in education between genders have a similar picture on the labor market outcomes. The results suggests that the policy measure may have played a role in mitigating the gender disparity within Turkish labor market as well. It is worth noting that historically, both nationally and globally, women have consistently earned significantly less than their male counterparts. Even though the gender wage gap¹⁴ in Türkiye is lower than the OECD total, they come closer with time as evident in Figure 7.3. Most recent available data in 2018 shows the gender wage gap for Türkiye being 10% and for all the OECD countries being 13%.



Source: OECD (2023), Gender wage gap (indicator).

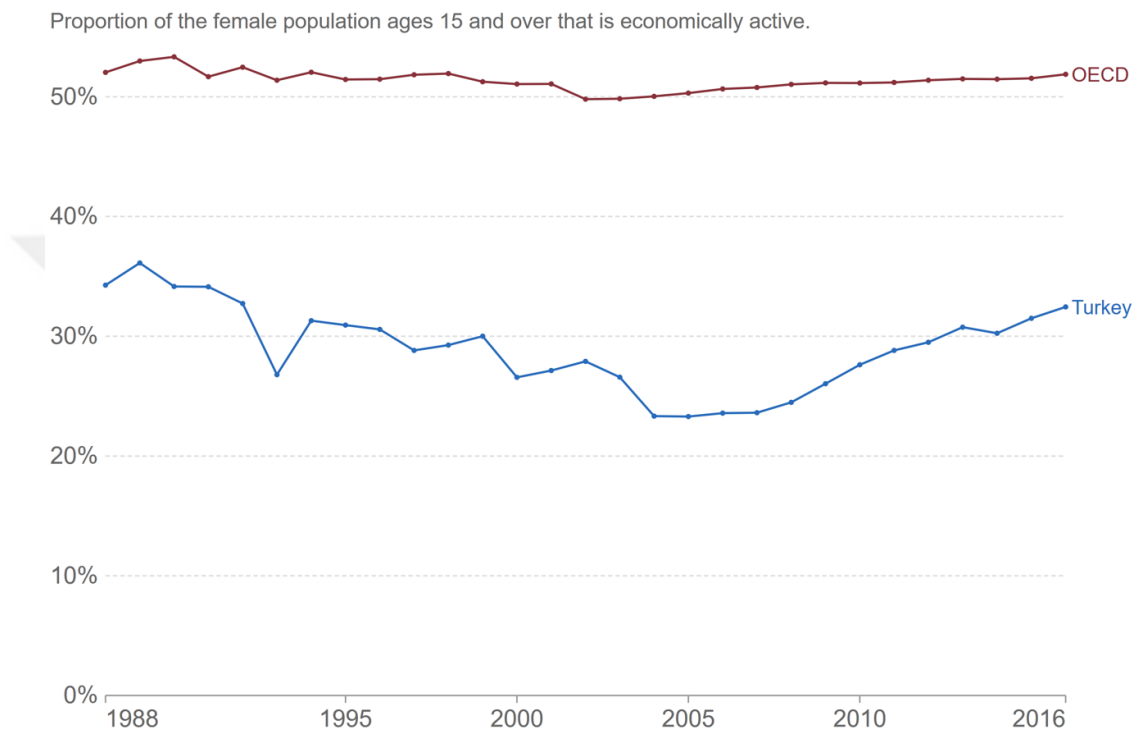
<https://data.oecd.org/earnwage/gender-wage-gap.htm> (Accessed on 04 October 2023)

Figure 7.3: Gender Wage Gap (Percentage)

It seems even though the compulsory schooling policy helped to narrow this persistent gender wage gap, it is clearly not enough. The Figure 7.4 illustrates another dire problem of the country, the female labor force participation (LFP). LFP

¹⁴OECD defines the gender wage gap metric as the difference between median earnings of men and women relative to median earnings of men.

for Türkiye was always lower than other OECD countries for a long time. The 1997 CSL did not have any causal effect on the LFP for both genders. The worsening situation on these two chronic problems of the country imply that there is room for other policies. Policies that are directly aiming to solve these problems such as equal pay laws, pay equity audits, policies supporting paid family leave, and childcare provisions.



Source: Our World in Data based on OECD (2017)

<https://ourworldindata.org/grapher/female-labor-force-participation-oecd>

Figure 7.4: Female Labor Force Participation

Although reform did not significantly affect the labor force participation and employment it did improve the employment conditions of only women. It was no surprise that women's public sector employment increased where the degrees are generally a priori condition to apply for these positions. However, women also become more likely to work in bigger companies with better conditions. Their work status also shifts from unpaid family worker which is very common in agricultural communities to wage worker. These changes can happen through migrating to the places with better jobs such as big cities. Tansel (2002) finds higher levels of

industry employment is correlated with higher levels of primary school education compared to agricultural communities. Aydemir, Kırdar & Torun (2022) finds education increases women to migrate at earlier ages. They also suggest women's reasons to migrate to another city change because of the 1997 reform. They become more prone to migrate because of labor market related reasons including human capital investments and less likely to be "tied movers". Evaluating together with our findings, women seem to invest more on their human capital with the additional schooling received. A more detailed spatial and sectoral investigation is needed to further highlight such effects. It is also needed further investigation to see why these effects were missing for men.

In sum, this research elucidates the transformative capability of education policies on labor market dynamics. The findings from the 1997 CSL reaffirm the intertwined nature of education and employment, serving as a directive for policymakers and economists. The lessons from this study are pivotal, not just for Türkiye, but potentially for similar economies striving to optimize their educational frameworks for better labor market outcomes for its citizens.

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APPENDICES

A. ADDITIONAL TABLES

Table A1: Monetary Returns to Education (Unrestricted Birth Cohorts)

	Dependent Variables:	(1) OLS Monthly Wage	(2) TSLS Monthly Wage	(3) OLS Hourly Wage	(4) TSLS Hourly Wage	(5) TSLS Working Hours
Women	Years of Schooling	0.078*** (0.000)	0.06*** (0.017)	0.097*** (0.001)	0.079*** (0.019)	-5.254*** (1.227)
	Number of Observations	140400	140400	140400	140400	140400
	Quadratic Trend for Birth Cohort	No	No	No	No	No
Women	Years of Schooling	0.078*** (0.000)	0.072** (0.035)	0.097*** (0.001)	0.098*** (0.04)	-7.022** (2.835)
	Number of Observations	140400	140400	140400	140400	140400
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes
Men	Years of Schooling	0.061*** (0.000)	0.068*** (0.014)	0.077*** (0.001)	0.067*** (0.016)	-0.617* (1.399)
	Number of Observations	368726	368726	368726	368726	368726
	Quadratic Trend for Birth Cohort	No	No	No	No	No
Men	Years of Schooling	0.061*** (0.000)	0.021 (0.024)	0.077*** (0.001)	0.025 (0.028)	-0.92 (2.007)
	Number of Observations	368726	368726	368726	368726	368726
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes

Given coefficients are for the variable years of schooling. Dependent variable for the: model (1) & model (2) is natural logarithm of real monthly wage; for model (3) & model (4) is natural logarithm of real hourly wage, model (5) is total monthly working hours.

All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear birth year trend variables. Standard errors are in parentheses, clustered at year and month of birth level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A2: Education's Effects on Work Status (Without Marital Control)

	Dependent Variables:	(1) TSLS	(2) TSLS	(3) TSLS	(4) TSLS	(5) TSLS	(6) TSLS	(7) TSLS
		LFP	Employed	Full-Time Worker	Wage Worker	Unpaid Family Worker	Self-Employed	Employer
Women	Years of Schooling	0.02	0.02	0.018	0.031***	-0.015**	0.003	0.001
		(0.013)	(0.014)	(0.013)	(0.011)	(0.007)	(0.003)	(0.002)
	Number of Observations	289653	289653	289653	289653	289653	289653	289653
Men	Years of Schooling	0.015	-0.003	-0.013	0.024	-0.002	-0.021	-0.003
		(0.018)	(0.029)	(0.034)	(0.027)	(0.013)	(0.019)	(0.012)
	Number of Observations	272870	272870	272870	272870	272870	272870	272870

Coefficients are given for the instrumented variable years of schooling. Dependent variable for the: model (1) is Dummy for Labor Force Participation, model (2) is dummy for employment, model (3) is dummy for wage employment, model (4) is dummy for full-time employment, model (5) is dummy for self being unpaid family worker, model (6) is dummy for self-employment, model (7) is dummy for being an employer.

All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, and linear birth year trend variables. Standard errors are in parentheses, clustered at year and month of birth level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A3: Education's Effects on Work Status (Unrestricted Birth Cohorts)

	Dependent Variables:	(1) TSLS	(2) TSLS	(3) TSLS	(4) TSLS	(5) TSLS	(6) TSLS	(7) TSLS
		LFP	Employed	Full-Time Worker	Wage Worker	Unpaid Family Worker	Self-Employed	Employer
Women	Years of Schooling	0.011 (0.009)	0.016 (0.01)	0.015* (0.009)	0.026*** (0.007)	-0.008 (0.007)	-0.002 (0.003)	-0.001 (0.001)
	Number of Observations	658589	658589	658589	658589	658589	658589	658589
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Men	Years of Schooling	0.003 (0.007)	0.004 (0.009)	0.004 (0.009)	0.025** (0.011)	-0.005 (0.005)	-0.014* (0.008)	-0.001 (0.005)
	Number of Observations	612588	612588	612588	612588	612588	612588	612588
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Coefficients are given for the instrumented variable years of schooling. Dependent variable for the: model (1) is Dummy for Labor Force Participation, model (2) is dummy for employment, model (3) is dummy for wage employment, model (4) is dummy for full-time employment, model (5) is dummy for self being unpaid family worker, model (6) is dummy for self-employment, model (7) is dummy for being an employer.

All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear and quadratic birth year trend variables.

Standard errors are in parentheses, clustered at year and month of birth level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A4: Education's Effects on Work Status
(Unrestricted Birth Cohorts - Continued)

	Dependent Variables:	(1) TSLS	(2) TSLS	(3) TSLS	(4) TSLS	(5) TSLS
		Social Security	Public Employee	Second Job or More	Small Company	Big Company
Women	Years of Schooling	0.029***	0.023***	-.0003***	-0.013	0.02***
		(0.007)	(0.005)	(0.001)	(0.008)	(0.006)
	Number of Observations	658589	658589	658589	658589	658589
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes
Men	Years of Schooling	0.017	0.039***	-0.001	-0.029**	0.019*
		(0.013)	(0.009)	(0.003)	(0.011)	(0.01)
	Number of Observations	612588	612588	612588	612588	612588
	Quadratic Trend for Birth Cohort	Yes	Yes	Yes	Yes	Yes

Coefficients are for the instrumented variable years of schooling. Dependent variable for the: model (1) is dummy for formal employment, model (2) is dummy for public employment, model (3) is dummy for having more than one job, model (4) is dummy for being employed in a small company with 10 or less people, model (5) is dummy for being employed in a big company with 50 or more people. All the models control for age fixed effects, nuts2 level regional fixed effects, survey year fixed effects, marital status, and linear and quadratic birth year trend variables. Standard errors are in parentheses, clustered at birth year and month level. *** $p < .01$, ** $p < .05$, * $p < .1$

B. TURKISH SUMMARY / TRKE ZET

Bu alıřma, 1997 yılında Trk hkmeti tarafından uygulanan zorunlu eēitim yasaının eēitim sisteminin iyileřtirilmesini amalayan bir politika olarak hayata geirilmesiyle bařlayan bir dizi nemli deēiřikliēin sonularını incelemektedir. Zorunlu eēitim sresini 5 yıldan 8 yıla geniřleten bu politika, bařlangıta siyasi bir motivasyonla hayata geirilmiş olup, bu politikaya maruz kalanlar (deney grubu) ile maruz kalmayanlar (kontrol grubu) arasında eēitim seviyeleri zerinde meydana gelen anlamlı bir dıřsal řok yaratmış ve bu sayede politika etkisinin edinilen diplomalar zerindeki etkisini karřılařtırmamıza olanak saēlamıştır. Bařlangıta bu deney ve kontrol grupları arasındaki karřılařtırma, politikanın diploma sahibi olma ihtimali zerindeki etkisini ortaya ıkarmamıza imkn tanımıştır. İkinci olarak, bu etkiden tretilen sonular zerine giderek, eēitimin iřēc piyasası ıktıları zerindeki neden-sonu iliřkisini daha derinlemesine inceleyebilmemize imkn vermiştir. ncelikli olarak cretler, daha sonra aylık alıřma sreleri, iřēcne katılım ihtimali ve istihdam durumu ve kořulları gibi diēer ynlerde de alıřma geniřletilmiştir. Bu istihdam kořulları, sosyal gvenlik kaydı ve řirket byklē gibi konuların yanı sıra tam zamanlı alıřanlar, cretli aile alıřanları, maařlı alıřanlar, kendi hesabına alıřanlar veya iřverenler gibi alıřma durumlarını ieren geniř bir yelpazeyi kapsıyor.

Eēitimin iřēc piyasası sonuları zerindeki etkisi, ekonomistler arasında yıllardır ilgi eken bir konu olmuřtur. Trkiye'deki eēitim ile iřēc sonuları arasındaki etkileřim, Gary S. Becker'in (1962) beřer sermaye modeli ve Andrew Michael Spence'in (1973) sinyal modeli temelinde aydınlatıcı nanslarla aıklanabilir. Beřer sermaye modeline gre, eēitim bireylere beceri ve bilgi kazandırarak onları daha verimli hale getirir ve potansiyel olarak daha yksek cretler kazanmalarını saēlar. Buna karřılık, sinyal modeli, eēitimin temel olarak iřverenlere gnderilen ve bir

bireyin doğal yetenekleri veya potansiyeli hakkında olan bir sinyal olarak hizmet ettiği üzerine odaklanır. Bu sinyal doğrudan eğitimle kazanılan beceriler ile bağlantılı olmak zorunda değilken bile gönderilerek eğitim alan kişinin çıkarına olabilir. Özetle, bu temel beşeri sermaye modeli çerçevesinde, rasyonel bir birey üzerine kurulu olan eğitim kararı, yalnızca bir yatırım kararı olarak kavramsallaştırılabilir. Bu kararı temelde üç ana faktöre bağlı olarak değerlendirebiliriz. Bunların ilki kişinin eğitim sonucu kazanmayı beklediği gelecekteki gelir artışı. Bu artış verimlilik artışı, kazandığı diplomalarının yarattığı sinyal etkisi ve diğer dışsal faktörler kökenli olabilir. İkincisi, kişinin gelecek ve şimdi arasında yaptığı kişisel zaman tercihinin gösteren indirim oranı katsayısı, r . Son olarak eğitimin maliyeti değerlendirilebilir. Bu maliyet eğitim sırasında kaybedilen kazançlar ve doğrudan eğitim maliyetlerinin toplamı olarak basitçe özetlenebilir. Buna ek olarak bazı bireyler için eğitim, tüketildiğinde içsel değer ve zevk sunan normal bir mal olarak da görülebilir. Bireyler, yeni bilgi edinmekten ve eğitim kontekstinde sınıf arkadaşlarıyla etkileşimde bulunmaktan keyif alabilirler. Sonuç olarak, eğitimdeki yatırım kararını modelleyen böyle bir teorik altyapı rasyonel bir bireyin en verimli kararı verdiği savını önerir ancak eğitimin tüketim faydasını hesaba katmayan bir model eğitimin getirisini gerçek değerinden daha az bir seviyede hesaplayabilir.

Rasyonel bireylerin aldığı neredeyse mükemmel kararlara müdahale etmenin verimsiz bir sonuç yaratacağı düşünülebilir. Ancak piyasa hatası yaratabilecek potansiyel durumlar devletlerin zorunlu eğitim yasalarını savunmak için bazı argümanlar doğurur. Bu tip müdahalelerin büyük ölçüde haklı olduğu birkaç durum özetlenebilir. İlk durum, rasyonel bireyin yeterince bilgilendirilmediği durumlarda ortaya çıkar. Hanehalklarının bilgi eksikliğinin özellikle daha fakir ülkelerde daha yaygın olduğu düşünüldüğünde, eğitim kararın bireylere bırakılmasının sonuçlarının ağır olacağı sonucuna varılabilir. İkinci olarak, düşük gelirli haneler daha yüksek indirim oranları sergileyebilirler (Lawrance, 1991; Becker & Mulligan, 1997), ve genel olarak genç yaştaki çocuklar irrasyonel kararlar almaya daha yatkın olabilirler. Üçüncü olarak, eğitimin maliyeti, belirli dezavantajlı gruplar için orantısız derecede yüksek olabilir, örneğin, kırsal bölgelerde yaşayan insanlar (Kırdar, Dayıoğlu & Koç,

2016) veya kızların okul eğitimine karşı olan olumsuz sosyal ve kültürel normlar nedeniyle (Tansel, 2002). Dördüncü olarak, ebeveynler ile çocukları arasında bir asil vekil problemi olabilir, burada ebeveynlerin çıkarları mutlaka çocuklarının çıkarlarıyla örtüşmeyebilir. Son olarak, bireysel karar vericiler tarafından göz ardı edilebilecek, sağlıkta ve yaşam/iş tatmininde iyileşmeler gibi pozitif dışsallıklar göz ardı edilebilirler.

Zorunlu eğitim yasaları, birçok deneysel çalışmanın odak noktası olmuştur. Özellikle, Türkiye'nin 1997 zorunlu eğitim reformu, Aydemir & Kırdar (2017) ve Torun (2018) tarafından yapılan iki dikkat çekici çalışmanın merkezine yerleşmiştir. Bu iki çalışma, bu reformun eğitimde dışsal bir sıçrama yapmasını araç olarak kullanarak, eğitimin işgücü sonuçları üzerindeki neden-sonuç ilişkisini, özellikle ücretler üzerinden ölçtüler. Ancak, bu çalışmaların bulguları genellikle zamanın verisiyle sınırlı olduğu için, erken 20'li yaşlarındaki bireylerin erken işgücü deneyimlerine dayanıyordu. Bu temel çalışmaların üzerine inşa ederek, bu tez daha ileriki yaşlardaki bireyler üzerinden reformun uzun vadeli etkilerini incelemektedir. Buradaki temel soru, bu bireylerin işgücü piyasasında daha fazla zaman geçirdikçe gözlenen etkilerin istikrarlı bir şekilde sürüp sürmediğini belirlemektir. Bu amaçla, bu çalışma 2007'den 2022'ye kadar uzanan 16 yıllık Hanehalkı İşgücü Anketi (HIA) verilerini birleştirerek 25-35 yaş aralığındaki 1.3 milyon kişilik geniş bir örneklemden yararlanmaktadır. Bu veri setinin zengin kapsamı, reformdan etkilenen grupların emek piyasası çıktılarını gözlemlemek için benzersiz bir imkân ve 1997 zorunlu eğitim reformunun kalıcı etkilerine dair önemli bir içgörü sunar.

HIA, doğrudan okul yıllarının sayısını sağlamadığı için bunun yerine tamamlanan eğitim seviyesi ve okur yazarlık durumuna dayalı olarak eğitim yılları belirlenmiştir. Aydemir & Kırdar (2017), 2008 Türk Nüfus ve Sağlık Araştırması verilerinden yola çıkarak, okuma yazma bilen ancak formal bir diploması olmayanların ortalama olarak yaklaşık 2 yıl okula gittiklerini hesapladılar. Ayrıca, belirli bir eğitim seviyesini tamamlamadan önce çok az kişinin okulu bıraktığını aynı araştırmadan göstermek mümkün. Bu nedenle, okula gidilen sürenin tamamlanan eğitim seviyelerinden çıkarımları aşağıdaki gibidir:

• Okuma yazma bilmeyen	0 yıl.
• Okuma yazma bilen diplomasız	2 yıl.
• İlkokul mezunu	5 yıl.
• Ortaokul mezunu	8 yıl.
• Lise mezunu	11 yıl.
• Yüksekokul, üniversite ve üstü	15 yıl.

Her ne kadar 1991 doğumlular sınırlandırılmış doğum yılları aralığında (1983-1990) bulunmasa da, ek yapılan analizlerdeki tüm doğum yıllarını içeren örnekleme 1991 yılı veya sonrasında doğan bireyler liseyi 4 yıl okuduğu için bu yıl içinde ve daha sonra doğan lise veya üniversite mezunlarına ek bir yıl eklenmiştir.

Eğitim ile ücretler gibi işgücü piyasası değişkenleri arasındaki nedensel ilişkiyi belirlemek için kullanılan strateji, eğitimin endojen doğasının karmaşıklığı nedeniyle incelikli bir yöntem gerekliliği kılar. Farklı eğitim arka planlarına sahip insanların ücretlerini sadece karşılaştırmak, nedensel değil, korelatif sonuçlar verir. Bu korelatif sonuçlar, eğitim kararlarının, doğuştan gelen yetenekler, motivasyon, kişisel tercihler, aile geçmişi, sosyal ağlar gibi işgücü piyasası sonuçlarını da etkileyebilecek çeşitli faktörlerden etkilenmesi riskini barındırır. Bu faktörleri bir analizde hesaba katmanın en iyi yolu, randomize kontrollü çalışmaları ile olur. Bu tür bir düzeneği kurmak pratik ve etik açılarından sorunludur. Bununla birlikte, araç değişken (IV) yöntemi, eğitimin ücretler üzerindeki saf etkisini izole edebilmek için bu zorluğun üstesinden gelmenin bir yolunu sunar. Bu araç ya da diğer adıyla enstrüman, işgücü piyasası çıktıları üzerinde belirleyici olan diğer değişkenlerinden bağımsız bir şekilde eğitimdeki değişimi izole eden bir özelliğe sahip olmalıdır. Böylece eğitimin ücretler gibi işgücü piyasa çıktıları üzerindeki nedensel etkisini daha doğru bir şekilde tahmin eder. Bu yaklaşım, özellikle Angrist & Kruger (1991) tarafından yapılan çığır açan çalışmadan bu yana eğitimin parasal getirisini anlamak için akademik literatürde yaygın bir şekilde kullanılmıştır. Bu modelde Y_i terimi, i bireyi için bağımlı değişkenin potansiyel gerçekleşen sonucunu ifade eder. Bizim durumumuzda bu, ücret gibi potansiyel bir işgücü piyasası çıktısıdır. $Treat_i$, değişkeni bireyin reforma maruz kalıp kalmamasını gösteren bir kukla değişkendir:

$$Treat_i = \begin{cases} 1 & \text{eğer } doğum\ yılı \geq 1987 \\ 0 & \text{eğer } doğum\ yılı \leq 1986 \end{cases} \quad (5.1)$$

S_{0i} , zorunlu eğitim reformuna maruz kalmayan i bireyinin kaç yıl okula gittiğinin gösterirke, S_{1i} ise reforma maruz kaldığı durumda i bireyinin okula gittiği süreyi gösterir. Bu nedenle, zorunlu eğitim reformunun heterojen nedensel etkisi ($S_{1i} - S_{0i}$) ile ifade edilebilir. Ancak realitede sadece okula gitme süresini aşağıdaki şekilde gözlemlenebilir:

$$S_i = S_{0i} + (S_{1i} - S_{0i})Treat_i \quad (5.2)$$

Eğer aşağıdaki 4 varsayım doğru kabul edilirse, IV ya da İki Aşamalı En Küçük Kareler (TSLS) tahminleri Lokal Ortalama Etkileri (LATE) veriyor olarak yorumlanabilir (Teorem 4.1, Angrist & Pischke, 2009):

- 1) Bağımsızlık varsayımı: Araç değişken/ Enstrüman, potansiyel çıktılarından bağımsızdır. Reforma maruz kalan deney grubundaki bireyler neredeyse rastgele seçilmiş gibi olmalıdır. Matematiksel olarak şöyle ifade edebiliriz:

$$[Y_i(S_i = s, Treat_i = z); \forall s, z], S_{1i}, S_{0i} \perp\!\!\!\perp Treat_i \quad (A1)$$

- 2) Hariç tutma koşulu: Enstrüman çıktıları sadece tek bilinen kanal üzerinden etkiler. Bu varsayımın formal matematiksel gösterimi aşağıdaki gibidir:

$$Y_i(S_i = s, Treat_i = 0) = Y_i(S_i = s, Treat_i = 1), \forall s \quad (A2)$$

- 3) İlk aşama varsayımı: Enstrüman S_i değişkeninin etkiler. Yani reform kişilerin okullaşma süresini etkiler. Formal olarak:

$$E(S_{1i} - S_{0i}) \neq 0 \quad (A3)$$

- 4) Monotonluk varsayımı: Enstrümanın S_i üzerinde monoton bir etkisi olmalıdır. Bu reformun etki etmediği bireyler olsa da kimsenin okullaşmasına negatif bir etki yapmadığını varsaymak olarak anlaşılabilir. Formal olarak:

$$S_{1i} - S_{0i} \geq 0, \forall i \quad (A4)$$

Sonuçlarımızın içsel geçerliliği, bu dört koşula bağlıdır. Eğer Eğitim kararındaki dışsal

değişim bu koşulları karşılamıyorsa, sonuçlar eğitim için nedensel bir getiri olarak yorumlanamaz. Koşullar karşılandığında bile, IV tahminleri yalnızca politika nedeniyle eğitim seçimlerini değiştirenler kümesi için geçerli olur. Ancak, bu küme tüm popülasyonu temsil etmeyebilir. Zorunlu okul süresindeki değişikliklerden elde edilen tahminler, bu küme için yerel bir etki (LATE) olarak yorumlanabilir (Imbens & Angrist, 1994; Angrist, Imbens, & Rubin, 1996).

Bu IV modeli için genel ilk aşama denklemi aşağıda verilmiştir:

$$S_i = \alpha_0 + \alpha_1 Treat_i + \sum_{j=26}^{35} \alpha_j Age_{j,i} + \sum_{t=2008}^{2022} \alpha_t Year_{t,i} + X'_i \gamma + \varepsilon_i \quad (5.3)$$

S_i , i gözlemi için okulda geçirilen süreyi gösterir. Anket, tekrarlı örnekleme ile gerçekleştirildiğinden, aynı bireylerin farklı anket yıllarında birden fazla kez gözlemlenmesi mümkün ama muhtemel değildir. Bununla birlikte, örneklem panel veriden oluşmadığı için, yani birey tanımlayıcıları içermediği için, bu bireyin aynı kişi olup olmadığını bilemeyiz.

Yukarıda 5.1 denklemiyle açıklandığı üzere, $Treat_i$, gözlem i için reform kukla değişkenidir. Gözlem i bireyinin doğum yılı 1987 veya daha büyükse 1 değeri alır, 1987'den küçükse 0 değeri alır. Benzer şekilde, $Age_{j,i}$, j yaşındaki bireyin i gözlemi için yaş kukla değişkenidir. 25 yaş, çoklu eşdoğrusallık problem oluşmaması için baz olarak kullanılmıştır. $Year_{t,i}$, yıl kukla değişkenidir ve t yılındaki i gözlemi içindir. Benzer şekilde 2007 yıl kukla değişkeni için baz yıl olarak düşünülebilir. Son olarak, X'_i gözlem i için diğer kontrolleri içeren kovaryans vektörünün transpozudur. NUTS2 seviyesinde bölgesel, mevcut medeni durum ve bir de lineer doğum yılı trendi kontrol edilmektedir. Ek olarak gerekli görüldüğünde ikinci derece doğum yılı trendi kontrol değişkeni de eklenmiştir. Aslında hem yaş hem de anket yılı kukla değişkenlerine sahip olmak, lineer doğum yılı trendini otomatik olarak kontrol eder (O'Brien, 2000). Ancak HIA, aynı yılın farklı aylarında veri topladığı için, aynı yaşta olan farklı doğum yıllarına sahip gözlemleri ankette bulabiliriz, böylelikle hem aynı

yaş içinde hem de aynı anket yılı içinde doğum yılında varyasyon gözlemlemek mümkündür. Bu varyasyon, çoklu eşdoğrusallığı önler ve bir doğum yılı trendi kontrolü eklemeye izin verir. Standart hatalar doğum yılı ve ayı seviyesinde kümelenendirilerek hesaplanmıştır. Bu seviyede kümeleme, sağlıklı bir ilk aşama F istatistiği hesaplamak için yeterli sayıda küme verir. Bunun yerine, sağlam (heterosedastisiti konstant -hc) standart hatalar hesaplamak, daha küçük standart hatalar verir ancak bu tercih edilmemiştir.

Log ücret ve diğer istihdam değişkenleri için ikinci aşama (TSLS) denklemi, aşağıda verilmiştir:

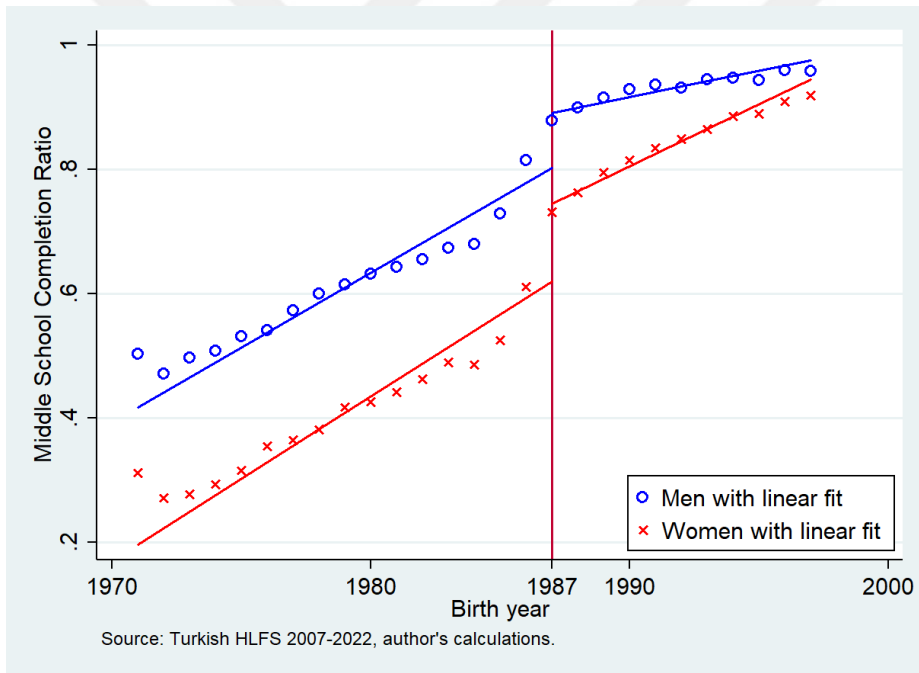
$$Y_i = \beta_0 + \beta_1 S_i + \sum_{j=26}^{35} \beta_j Age_{j,i} + \sum_{t=2008}^{2022} \beta_t Year_{t,i} + X'_i \delta + \epsilon_i \quad (5.5)$$

Y_i , i gözlemi için ilgili değişkendir. Bu ilgili değişken, aylık veya saatlik sıklıkla ölçülen ücretin doğal logaritması $-\ln(wage)$ veya aylık çalışma süresi olabilir. β_1 , LATE olarak yorumlanabilir, yani eğitimin ilgili değişkeni nedensel olarak ne kadar etkilediği. Bu ilgili değişken ücretlerken, eğitimin parasal getirisi olarak yorumlanabilir. Saatlik ücret ve aylık ücret değişkenlerinin her ikisini kullanmak için de argümanlar bulunmaktadır. ABD verilerine dayanarak, Card (1999), daha eğitilmiş insanların çalışma fırsat maliyetinin yükselmesi nedeniyle daha fazla çalışma eğiliminde olduğunu iddia ediyor. Bu nedenle, eğitimle artan çalışma saatlerine eklenen ek saatler, eğitim getirisinin endojen bir parçasıdır. Bu nedenle saatlik ücret, işçinin eğitiminin üretkenlik etkilerini daha iyi yansıtabilir (Gunderson & Oreopoulos, 2020). Bununla birlikte, aylık ücret, eğitim getirisini hesaplamak için yaygın bir ölçüdür. Bu, bir bireyin yaşam koşulları hakkında daha iyi bir fikir verebilir. Neyse ki, HIA, olağan haftalık çalışma saatleri ve saatlik ücreti ayrı ayrı hesaplamak için veriler içermektedir.

Ayrıca, istihdam değişkenleriyle alakalı olarak Y_i bir binom değişkeni olarak da tanımlanabilir. Bu durumda değişken, gözlemlenen kişinin işgücünde olup olmadığı veya tam zamanlı çalışan, ücretli çalışan, ücretsiz aile işçisi, kendi hesabına çalışan, işveren, sigortalı çalışan, kamu çalışanı, ikinci bir işte çalışan, küçük veya büyük bir

şirkette çalışan olup olmadığını gösteren bir kukla değişkenidir. Kişi çalışmıyorsa değeri sıfırdır.

Sonuç olarak bu çalışmanın temel varsayımı, belirli bir tarihten hemen önce veya hemen sonra doğan insanların gözlemlenen ve gözlemlenmeyen özellikler ve dolayısıyla işgücü piyasasındaki durumları açısından benzer olmaları gerektiğidir. Argüman şudur ki, zorunlu eğitim reformu, bu iki grup arasında, eğitim sürelerindeki dışsal artış dışında herhangi bir sistemik farklılığa yol açmaz. Bu politika tarafından başlatılan dışsal eğitim artışı, bir enstrüman olarak hizmet eder. Şekil 6.1, zorunlu eğitimin artışıyla ortaokul mezuniyetindeki bu artışı göstermektedir. Temel eğitim veya ortaokulu (8. sınıf) tamamlamış olan bireylerin ortalama oranı, kadınlar için kırmızı x simgeleri ve erkekler için mavi daire simgelerle gösterilir.



Şekil 6.1: Doğum Tarihine Göre Ortalama Ortaokul Diploması Sahiplik Oranı

Benzer bir artış, lise mezuniyeti için de gözlemlenebilir ve bu da liseye yayılan/devam eden bir etkiyi işaret eder. Grafiklerdeki bu görsel deliller, diploma derecesi kazanımı üzerindeki politika etkisini göstermektedir. Bu etkiler bir dizi değişkenler kontrol edilerek oluşturulan basit en küçük kareler (OLS) regresyonları kullanılarak daha ayrıntılı bir şekilde incelenecektir. Bu kontrol değişkenleri yaş kukla değişkenleri, NUTS2 düzeyinde bölgesel kukla değişkenleri, anket yılı kukla

değişkenleri, güncel medeni durum için bir kukla değişken ve doğum yılı için doğrusal bir trend değişkeni içeriyor.

Analiz için kullanılan örneklem, 1983-1990 yıllarını kapsayan kısıtlı bir doğum yılı aralığını içerir. Doğum yılında yapılan bu sınırlamanın altında yatan birkaç neden bulunmaktadır. Başta, doğum yılını sınırlamak, doğum trend etkilerinin daha az etkili olduğu daha kesin sonuçlar üretebilir. Enstrüman olarak kullanılan 1997 reformunun doğası, sabit doğum yılı etkilerini kontrol etmeyi engelliyor. Zorunlu eğitim yasasının 1987 doğumlu olan herkes için geçerli olması, doğum yılları içinde yasaya maruz kalan ve kalmayan insanlardan oluşacak bir varyasyonun söz konusu olmadığı bir durum yaratıyor. Bu durum bizi sadece lineer veya daha yüksek dereceli trend etkilerini kontrol edebilir şekilde kısıtlıyor. İkinci olarak, veri setini sınırlamak, hem kontrol ve deney grupları örneklem büyüklüklerini eşitlemek hem de nesiller arasındaki farkları kısıtlamak açısından daha dengeli kontrol ve deney grupları veriyor. Bu doğum yılı sınırlamasından önce, kontrol grubunun örneklem büyüklüğü deney grubununkinin iki katıydı; bu oran sınırlamadan sonra 1.26'ya düşürüldü. Üçüncü olarak, veri bulunmaması nedeniyle doğulan bölge sabit etkilerini kontrol edemiyoruz. Doğum yılını sınırlayarak, okul kalitesindeki farklılıklar gibi potansiyel bölgesel heterojenliği hafifletebilirsiniz. Son olarak, ilgili değişkenleri etkileyebilecek diğer politikalar olabilir. Örneğin, 1991 ve sonrasında doğanlar için lise süresini bir yıl uzatan başka bir politika bulunmaktadır. Doğum yıllarını sınırlamak, odak noktasını 1997 zorunlu eğitim reformunun etkileri ile sınırlamaya yardımcı oluyor. Sonuç olarak, olabildiğince dar bir doğum yılı penceresi benimsemek en mantıklısı gibi gözüküyor.

Politika ile ortaokul tamamlama oranları önemli ölçüde artmıştır —kadınlar için %11 ve erkekler için %5 artış gözlemlenmiştir. İlginç bir şekilde, reformun etkileri sadece ortaokulda sona ermeyip, kadınlar için lise eğitimine de %4,3 bir sıçrama etkisi yaratmıştır.

Bu eğitim kazanımlarının işgücü piyasası üstüne etkisi de analiz edilmektedir. Bu analizde araç değişken (IV) yöntemi kullanılmaktadır. İki Aşamalı En Küçük Kareler

(TSLS) tahminleri, artan eğitimin işgücü piyasasında elde edilen somut faydalarını göstermektedir. Kadınlar için her bir ek yıl eğitim, aylık ücretleri %10, saatlik ücretleri ise dikkate değer bir şekilde %15 artırmıştır. Bu artışlar IV yöntemi sayesinde nedensellik çıkarımı olarak yorumlanabilir. Ancak, erkekler için bu rakamlar farklıdır. Onların da ücretlerinde hem aylık hem de saatlik oranlar için yaklaşık %7 artış görülmüş ancak bu rakamlar istatistiksel olarak anlamlı değildir. Dikkate değer bir gözlem olarak bu etkilerin genellikle daha önce rapor edilenlerden daha yüksek olduğu söylenebilir.

Araç Değişken (IV) tahminlerini OLS tahminleriyle karşılaştırdığımızda, IV tahminlerinin kadınların aylık ücreti için %30, saatlik ücret için %50 daha yüksek olduğunu görüyoruz. Bu, OLS tahminlerinde belirgin bir ölçüm hatası yanlılığına işaret ediyor. Kadınlar için aylık ve saatlik ücretler arasındaki fark, aylık çalışma saatlerinde bir azalma olduğunu göstermektedir. Tam zamanlı ücretli çalışanlar kadınlar için her bir yıllık eğitim artışı aylık 8.6 saatlik daha az çalışma süresi gözlemlememize sebep oluyor. Card (1999) daha eğitilmiş insanların en azından ABD bağlamında daha fazla çalışacağını öngörüyor. Türkiye bağlamında, kadınlar için eğitim sayesinde elde edilen gelir artışı etkisinin farklı olduğu görülüyor. Çalışmamanın fırsat maliyeti artsa da gelir artışıyla çalışmamaya doğru bir eylem gözlemleniyor denilebilir. Daha gerçekçi bir açıklama kadınların işlerindeki değişimlerle beraber değerlendirilerek yapılabilir. Özetle, bu bulgular, literatürde yaygınca gözlenen kadınlar için erkeklere oranla; gelişmekte olan ülkeler için gelişmiş ülkelere oranla daha fazla eğitim getirisi görüldüğü savını desteklemektedir.

Kadınlar arasındaki ücret artışının potansiyel bir nedeni, daha yüksek ücret almaya yatkın kadınların istihdama girmesi yani yanlı bir seçimle açıklanabilir. Eğer eğitim bireyleri işgücüne katılmaya veya aktif olarak iş aramaya yönlendiriyorsa, bu deney grubunda bir kompozisyon değişikliğine neden olabilir. İşgücünde, istihdamda ve tam zamanlı istihdamda olma olasılıkları üzerindeki politikanın etkileri istatistiksel olarak anlamlı değildir. Ancak incelediğimizde, politikaya maruz kalanların ücretli işgücünde olma olasılığının 1,2 puan arttığı ve tam zamanlı ücretli işgücünde olma olasılığının 0,9 puan arttığı görülmektedir. Örneklemdaki kadınların %24'ü tam

zamanlı ücretli çalışanlar olduğu için, bu yüzde 4'ten az bir artışa tekabül eder. Sonuç olarak, eğitim kadınları ücretli işgücüne yönlendiren küçük bir kompozisyonel değişime yol açıyor gibi görünmektedir. Eğer bu istihdama katılan yeni tam zamanlı ücretli kadın çalışanların, gözlemlenmeyen özellikler açısından kontrol grubuna benziyorsa bu geçiş sorun bir teşkil etmeyecektir. Ancak, bu yeni girenler veride gözlemlenmeyen özellikleriyle, motivasyon, yetenek veya aile yapısı gibi gözlemlenmeyen faktörlere dayalı olarak farklılık gösteriyorsa, bu sonuçlara potansiyel bir yanlılık getirebilir. Aydemir & Kırdar (2017) benzer bir eğilimi gözlemlemiş ve bu konuda uyarıda bulunmuş ancak bununla birlikte, bu endişenin abartılmaması gerektiğini savunmuşlardır. Argümanları, tam zamanlı ücretli çalışmaya yeni gelenlerin, politika öncesinde istihdamın sınırında olan bireyler oldukları göz önüne alındığında, bu bireylerin reforma maruz kalmamış istihdam edilen kadınlardan çok farklı olmadığıdır. Daha basit bir ifadeyle, ücretli işgücü havuzuna katılmak veya katılmamak arasında tereddüt eden marjindeki insanların benzer olma olasılığının yüksek olduğu düşünülmektedir.

Gözlemlenen bu ücret artışı uygulamada çeşitli kanallar aracılığıyla gerçekleştirilebilir. Kadınlar için gözlemlenen belirgin getirilere ışık tutabilecek eğitimin istihdam etkileri üzerine araç değişken (IV) metot çerçevesinde iki aşamalı en küçük kareler (TSLS) yöntemiyle daha derinlemesine bir bakış sağlanabilir. Kompozisyonel değişimi inceleyen reform etkilerinin bulgularıyla uyumlu olarak işgücüne katılma, istihdam ve tam zamanlı istihdam üzerinde nedensel bir eğitim etkisi olmadığını gözlemlenmektedir. Bununla birlikte bir eğitim yılı artışının ücretli bir işçi olma olasılığını 2.7 yüzdelik puan artırdığını ve aile içi ücretsiz işçi olma olasılığını 1.5 yüzdelik puan azalttığını göstermektedir. Kadınlar için örneklem ortalamaları ile karşılaştırıldığında, bu etkiler sırasıyla %10 artışı ve %22 azalmayı temsil etmektedir. Kendi hesabına çalışan veya işveren olma olasılıklarının istatistiksel olarak anlamlı olmadığı göz önüne alındığında, eğitimin kadınları ücretsiz aile işçisi pozisyonlarından ücretli işçi pozisyonlarına kaydırma konusunda kilit bir rol oynadığı söylenebilir. Başka bir deyişle, aksi takdirde ücretsiz aile içi işçi olabilecek bu kadınlar, yasaya zorla uzatılan eğitim seviyeleri sayesinde ücretli işçiler haline

geldiler. Bu sonuçların, Torun (2018) ve Gülberk (2023) tarafından bulunan bulgulardan farklı olduğu görünmektedir. Torun (2018), kadınlar için ücretli istihdam etkileri bulmamış ve Gülberk (2023), eksik olan bu istihdam etkilerinin evlilikle ilgili nedenlere bağlı olabileceğini savunmuştur. Evlilik durumunun bir eşdeğişken faktör (kovaryat) olarak dikkate alınmasına rağmen, evlilik kontrol değişkenleri olmadan yapılan analizlerin istihdam etkilerini değiştirmedikçe dikkat çekmek önemlidir. Ayrıca, erkekler için sonuçlar zorunlu Eğitim politikasının hiçbir istihdam sonucunu etkilemediğini göstermektedir.

Son olarak IV analizi eğitimin kadınların sosyal güvence kapsamındaki işlerde istihdam edilme olasılığını %13 artırarak 3.2 yüzdeler puan arttırdığını, aynı zamanda kadınların kamu sektöründe istihdam edilme olasılığını %28 artırarak 2.4 yüzdeler puan arttırdığını göstermektedir. Genellikle başvuru için lise veya daha yüksek bir derece gerektiren çoğu kamu pozisyonu göz önüne alındığında, bu bir sürpriz olmamalıdır. Ayrıca, kadınların 50'den fazla çalışanı olan işletmelerde istihdam edilme olasılığı, %17 artışla 1.8 yüzdeler puan artarken, küçük şirketlerde çalışma olasılıkları üzerinde istatistiksel olarak anlamlı bir etki gözlemlenmemiştir. Önceki istihdam sonuçlarıyla da uyumlu olarak, istatistiksel olarak anlamlı etkiler erkekler için bulunamamıştır.

Özetlemek gerekirse, eğitim, kadınların saatlik ücretlerini artırarak üretkenliklerini potansiyel olarak artırıp diploması aracılığıyla işgücü piyasasına sinyal göndererek artırdı. Ayrıca, ücretli çalışan oldukları için çalışma saatlerini azaltmış, ücretsiz aile içi rollerden ücretli işlere geçişlerini sağlamış ve onları sosyal güvence kapsamında, kamu sektöründe ve ücretlerin daha yüksek olduğu büyük şirketlerde pozisyonlara geçirmiştir. Bununla birlikte, bulgular kadınlar için olduğu kadar erkekler için açık ve net değil gibi görünüyor.

Bu çalışmadan çıkarılan görüşler önemli olsa da çalışmanın sınırlamalarını kabul etmek kritik bir husustur. İlk olarak, doğum yılı trendlerini belirlemek kolay değildir, bu da bizi sınırlı bir doğum yılı aralığına odaklanmaya zorlamıştır. Bu, örneklem büyüklüğünü önemli ölçüde azaltarak analizlerin istatistiksel gücünü zayıflatmıştır.

Kullanılan metodoloji, sıkı olmasına rağmen kendi kısıtlamalarına sahiptir. Araç değişken (IV) yaklaşımı, genellikle belirli bir gruba odaklanan bulgular sunar. Bu yasaya uyan kişiler için yerel ortalama etki (LATE), genellikle kırsal bölgelerde yaşayan kişilerin marjinal etkisini kapsıyor olabilir. Kırsal bölgelerdeki dezavantajlı öğrenciler, ülke genelindeki ortalama öğrenci ile karşılaştırıldığında eğitimden daha yüksek marjinal fayda elde edebilirler. Öte yandan, dünya genelinde, zorunlu bir eğitim reformunun hedeflenen öğrencilerin büyük bir bölümünü etkilediği pek örnek yoktur. 1997 zorunlu Eğitim reformu, o dönemdeki okul çağındaki öğrencilerin büyük bir bölümünü etkilediği için bu, LATE'nin ortalama etkiye (ATE) yakın olmasına neden olabilir. Sonuç olarak, bu sonuçların tüm nüfusa genellenmesi dikkatli bir yaklaşım gerektirir.

Özetle, bu araştırma eğitim politikalarının işgücü piyasası dinamikleri üzerindeki dönüşümsel yeteneğini açıklar. 1997 reformundan elde edilen bulgular, eğitim ile istihdamın birbirine dolaylı olarak bağlı olduğunu ve bu nedenle politika yapıcılar ve ekonomistler için bir yol gösterici olduğunu yeniden teyit eder. Bu çalışmadan çıkarılan dersleri, sadece Türkiye için değil, vatandaşları için daha iyi işgücü piyasası sonuçları elde etmek isteyen ve bu amaçla eğitim politikalarını geliştirmeye çalışan benzer ülkelerin politika yapıcıları için de önemlidir.

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