THE ROLE OF MINIMUM WAGE AND EMPLOYMENT SUBSIDIES ON EMPLOYMENT, INVESTMENT AND EXPORT DECISIONS OF FIRMS: EVIDENCE FROM TURKEY

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ÇAĞLAR YÜNCÜLER

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DOCTOR OF PHILOSOPHY

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I certify that this thesis satisfies all the requirements as a thesis for the degree of Doctor of Philosophy.

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This is to certify that I have read this thesis and that it in my opinion is fully adequate, in scope and quality, as a thesis for the Degree of Doctor of Philosophy in the field of Economics at the Graduate School of Social Sciences.

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Çağlar YÜNCÜLER

ABSTRACT

THE ROLE OF MINIMUM WAGE AND EMPLOYMENT SUBSIDIES ON EMPLOYMENT, INVESTMENT AND EXPORT DECISIONS OF FIRMS: EVIDENCE FROM TURKEY

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This thesis investigates how wage policies—both regulatory and incentive-based—shape firm behavior and labor market outcomes in a developing economy context. Using rich administrative microdata from Turkey and leveraging quasi-experimental variation, the study explores the effects of minimum wage hikes and employment subsidies on firm-level employment, capital accumulation, workforce composition, and export behavior.

The first chapter analyzes the 2016 minimum wage hike using a bunching estimation framework. The results show that the employment effects are concentrated near the wage threshold, with negative impacts particularly evident among formal, low-skilled, and young workers. These findings underscore the importance of heterogeneity in the wage distribution when evaluating the impact of statutory wage floors.

The second chapter explores the impact of a 2016 subsidy reform that extended a 6-percentage-point reduction in employer-side social security contributions to previously ineligible micro firms. Exploiting regional eligibility variation in a difference-in-differences design, the study finds that micro firms responded with significant employment gains, while small firms increased capital investment and capital–labor intensity in the form of tangible assets. However, the

policy had no measurable effect on skill upgrading, highlighting its limits in fostering structural labor market transformation.

The third chapter evaluates whether the same wage subsidy influenced export behavior. The analysis reveals that the policy significantly improved export performance along the intensive margin, particularly for micro firms. In contrast, the policy had no significant impact on the extensive margin, suggesting it did not induce non-exporting firms to enter foreign markets.

Overall, the thesis shows that wage policies can effectively stimulate employment and investment in financially constrained firms, but their impact on structural upgrading and export expansion remains limited without complementary interventions. These findings inform labor market policy design in middle-income economies where informality, inequality, and productivity differentials continue to pose major challenges.

Keywords: Minimum Wage, Subsidies, Employment, Exports, Difference-in-Differences,

ÖZ

TÜRKİYE'DE ASGARİ ÜCRET VE İSTİHDAM TEŞVİKLERİNİN FİRMALARIN İSTİHDAM, YATIRIM VE İHRACAT KARARLARINA ETKİSİ

YÜNCÜLER, Çağlar
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Bu tez, hem düzenleyici hem de teşvik temelli ücret politikalarının, gelişmekte olan bir ekonomi bağlamında firma davranışı ve işgücü piyasası sonuçlarını nasıl şekillendirdiğini incelemektedir. Türkiye'ye ait kapsamlı idari mikro verilerden ve yarı-deneysel politika değişimlerinden faydalanarak, asgari ücret artışlarının ve istihdam teşviklerinin firma düzeyinde istihdam, sermaye birikimi, işgücü bileşimi ve ihracat davranışı üzerindeki etkileri araştırılmaktadır.

İlk çalışmada, 2016 yılında gerçekleşen asgari ücret artışı yığılma ("bunching") tahmin yöntemi kullanılarak analiz edilmiştir. Bulgular, istihdam üzerindeki etkinin özellikle asgari ücrete yakın dilimlerde yoğunlaştığını; bu etkinin en çok kayıtlı çalışanlar, düşük vasıflı bireyler ve gençler üzerinde olumsuz olduğunu göstermektedir. Bu sonuçlar, yasal ücret tabanlarının etkisini değerlendirirken, ücret dağılımındaki heterojenliğin önemine işaret etmektedir.

İkinci çalışmada, 2016 yılında yürürlüğe giren ve daha önce bu kapsama dahil olmayan mikro ölçekli firmalara işveren taraflı sosyal güvenlik primlerinde 6 puanlık bir indirim sunan teşvik uygulaması ele alınmaktadır. Bölgesel uygunluk farklarını kullanan bir farkların farkı yaklaşımıyla yapılan analiz, mikro firmaların anlamlı düzeyde istihdam artışıyla yanıt verdiğini, küçük ölçekli firmaların ise maddi duran varlık şeklindeki sermaye yatırımlarını ve sermaye–emek oranlarını artırdığını ortaya koymaktadır. Bununla birlikte, politika işgücü niteliğinde bir

artışa yol açmamış, bu da yapısal dönüşüm sağlama açısından teşvik sınırlı etkisini gözler önüne sermiştir.

Üçüncü çalışmada aynı teşvik uygulamasının ihracat davranışı üzerindeki etkisi incelenmektedir. Yapılan analiz, politikanın yoğun marjda—özellikle mikro firmalar için—ihracat performansını belirgin şekilde artırdığını göstermektedir. Buna karşın, politika yaygın marj üzerinde anlamlı bir etki yaratmamış; yani ihracat yapmayan firmaların uluslararası pazarlara girmesini teşvik etmemiştir.

Genel olarak tez, ücret politikalarının finansal kısıt altındaki firmalarda istihdamı ve yatırımı etkili biçimde teşvik edebileceğini, ancak yapısal dönüşüm ve ihracat artışı gibi uzun vadeli hedefler için tamamlayıcı müdahaleler olmadan bu etkilerin sınırlı kalabileceğini ortaya koymaktadır.

Anahtar Kelimeler: Asgari Ücret, Teşvikler, İstihdam, İhracat, Farkların Farkı,

To my daughter, Defne

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Throughout this journey, I have constantly felt the genuine support of my dear family, close friends, and colleagues. Their unwavering belief in me has been a crucial source of motivation. Most special thanks go to my beloved daughter, Defne. I will always cherish the strength she gave me until the very end. As a father, it brings me immense peace and happiness to know that I did not disappoint her. Finally, I thank myself—for the determination and resilience it took to complete this PhD program.

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LIST OF ABBREVIATIONS

ALMP : Active Labor Market Policy
CPI : Consumer Price Index
CBRT : The Central Bank of the Republic of Turkey
DiD : Difference-in-Differences
EIS : Entrepreneurship Information System
EU : European Union
GDDA : General Directorate of Development Agencies
GDP : Gross Domestic Product
GIB : Revenue Administration
GTIP : Harmonized System (Turkish Custom Tariff Statistics Position)
HLFS : Household Labor Force Survey
ILO : International Labor Organization
ISCO : International Standard Classification of Occupations
ITT : Intention-To-Treat
KOSGEB : Small and Medium Enterprises Development Organization
LFP : Labor Force Participation
MIGs : Main Industrial Groupings
MLSS : Ministry of Labor and Social Security
NACE : Nomenclature of Economic Activities
NUTS : Nomenclature of Territorial Units for Statistics
OECD : Organization for Economic Co-operation and Development
PRODTR : Turkish Industrial Product Classification
RDD : Regression Discontinuity Design
R&D : Research and Development
SEDI : Socio-Economic Development Index
SGK : Social Security Institution
SME : Small and Medium-Sized Enterprise
TURKSTAT: Turkish Statistical Institute

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CHAPTER I

INTRODUCTION

The design of effective labor market policy remains a pressing concern for policymakers, particularly in developing and middle-income economies where informality, low labor force participation, and productivity gaps persist. Among the most widely used interventions are wage policies—including minimum wages and employment subsidies—that aim to influence firm behavior, formalize labor, and enhance economic inclusion. While extensively studied in developed countries, the full impact of these interventions on employment dynamics, firm investment, and broader economic outcomes remains less well understood in the context of emerging economies. This thesis addresses this gap by offering a comprehensive and empirically grounded evaluation of how wage policies—both regulatory and incentive-based—affect firm-level labor decisions, capital accumulation, and export performance in Turkey.

The dissertation is structured around three interrelated chapters, each of which contributes a distinct empirical perspective on the mechanisms through which wage policies shape firm behavior. The chapters draw on rich administrative data and quasi-experimental variation in Turkish labor policy, employing advanced econometric strategies such as bunching estimation and difference-in-differences to identify causal effects.

Chapter II focuses on the labor market effects of the 2016 minimum wage hike in Turkey, one of the most substantial nominal increases in the country's

history. Using a bunching estimation framework, the chapter explores how employment levels adjusted across the wage distribution in response to this sharp policy shock. The analysis shows that the employment response is concentrated in wage bins most directly affected by the new minimum wage, with disemployment effects observed especially among formal workers and vulnerable subgroups such as youth and low-educated individuals. These findings underscore the localized nature of minimum wage effects and the importance of accounting for wage structure heterogeneity in policy evaluation.

Chapter III turns to employment subsidies as a complementary tool for labor market regulation. Specifically, it evaluates the 2016 reform that expanded eligibility for a 6-percentage-point reduction in employer-side social security contributions to include micro firms—those with fewer than 10 employees—for the first time. Using matched firm-level administrative data and a difference-in-differences (DiD) approach, the chapter analyzes the policy's impact on capital formation and the skill composition of employment. The results indicate that micro firms responded primarily by increasing employment, while small firms boosted investment in tangible assets and machinery. However, no significant change in the share or level of skilled employment was observed, suggesting that the policy stimulated firm activity along quantitative margins without shifting the quality of labor inputs. These outcomes reveal the limits of wage subsidies as tools for structural upgrading in the absence of complementary human capital investments.

Chapter IV further extends the analysis of wage subsidies by examining their impact on firm exporting behavior—a key indicator of competitiveness and productivity growth. The chapter investigates whether the same 2016 subsidy

reform facilitated firm entry into export markets or expanded existing export activity. Using matched administrative panel data and a DiD framework, the chapter analyzes changes in export outcomes among firms located in newly eligible regions, leveraging a quasi-experimental control group based on counties not treated until 2020. The findings show that the subsidy significantly improved export performance along the intensive margin—particularly for micro firms in moderately underdeveloped regions. In contrast, the reform had no measurable effect on the extensive margin: it did not induce non-exporting firms to enter international markets. These results imply that while employment subsidies can enhance export capacity for already active firms, they are insufficient to overcome the fixed costs of export market entry without complementary support for capability development and market access.

Taken together, the three chapters of this thesis provide a multi-dimensional perspective on how wage-related labor market policies affect firm behavior in a developing country setting. While wage subsidies and minimum wage regulation can both influence firm employment and investment behavior, their impacts vary significantly depending on firm size, and the specific margin of adjustment—whether labor, capital, or trade. The findings offer practical insights for policymakers designing targeted labor market interventions and contribute to broader theoretical debates on the efficacy and limitations of wage policies in dual labor market settings.

CHAPTER II

MINIMUM WAGE, MAXIMUM PRECISION: A BUNCHING APPROACH TO EMPLOYMENT EFFECTS IN TURKEY

2.1. Introduction

Minimum wage legislation plays a pivotal role in shaping labor market outcomes, particularly in economies where a substantial share of workers earn close to the statutory wage floor. In Turkey, this share is significantly high, at around 40 percent (Gürcihan-Yüncüler and Yüncüler 2016), which amplifies the importance of understanding the minimum wage's effects on employment, informality, and wage distribution.

The global literature on minimum wage policies is vast but yields mixed findings. While there is broad consensus on the wage compression effects of minimum wages (Brown 1999; Lemos 2009), the employment effects remain hotly debated. Classical labor market theory suggests that binding minimum wages reduce employment by pricing out workers whose productivity falls below the mandated wage (Stigler 1946). This framework is particularly applicable to high-income economies with limited minimum wage coverage and relatively flexible labor markets. In contrast, dual-sector models (Welch 1974; Mincer 1976; Gramlich et al. 1976) emphasize the possibility of labor reallocation from formal to informal sectors in developing countries, where compliance may be incomplete. This shift may depress wages and increase employment in the informal economy. Alternatively, the “lighthouse effect” (Boeri et al. 2011) posits that minimum wages

can serve as a behavioral benchmark, indirectly raising wages even in uncovered sectors.

Empirical evidence from developing countries reflects both perspectives. In Brazil, studies document substantial wage compression and minimal job loss following minimum wage hikes. Engbom and Moser (2022) show that Brazil's minimum wage policy reduced inequality and elevated earnings throughout the wage distribution. Fajnzylber (2001) and Lemos (2009) similarly report wage gains without significant disemployment, including some evidence of spillovers into the informal sector. In Honduras, Gindling and Terrell (2009) find that while formal wages rose, employment declined in large firms subject to compliance, while informal effects were mixed.

In Indonesia, Hohberg and Lay (2015) find rising wages across sectors but formal sector job losses, underlining the importance of regional enforcement strength. In Peru, Del Carpio and Pabon (2017) find modest reductions in informality and wage gains among low-paid workers. Jales (2018), using a density discontinuity approach, documents bunching around the statutory minimum in Brazil, suggesting behavioral adjustment near the threshold. In Colombia, Pérez (2020) shows that informal wages responded to statutory minimum changes, indicating a reference-wage dynamic even in the absence of direct enforcement.

Within the Turkish context, findings remain inconclusive. Some studies report minimal disemployment effects (Gürcihan-Yüncüler and Yüncüler 2016; Işık et al. 2020; Pelek 2015), while others find negative employment impacts among youth or low-skilled workers (Bakış et al. 2015; Dayıoğlu et al. 2022; Bossavie et al. 2019). There is also evidence of increased informality in response to minimum wage hikes

(Öztek 2021). Most studies find consistent wage compression (Pelek 2018; Bakış and Polat 2023), although often limited to the formal sector. Despite Turkey's centralized minimum wage policy and high bite, evidence on spillovers and reallocation effects remains mixed.

In developed economies, the research focus has shifted toward distributional outcomes and firm-level heterogeneity. In the United States (US), Cengiz et al. (2019) document no significant employment effect for low-paid jobs, while minimum wage has spillover effects at the bottom of the wage distribution. Derenoncourt et al. (2021) also show that voluntary employer wage floors produce spillovers across firms and sectors, raising low-wage earnings. Cribb et al. (2021) analyze the UK's National Living Wage and find strong wage compression without significant employment loss. In Belgium, Desiere and Vandekerckhove (2020) report within-industry wage compression, again with no large-scale job loss.

Evidence from Germany and Portugal supports these patterns. Frings et al. (2013) find modest spillovers and stable employment following Germany's minimum wage introduction. Oliveira (2022) shows significant reductions in wage inequality after minimum wage hikes in Portugal, especially among small firms and in low-wage sectors. Fang et al. (2021) find consistent spillovers in Canada's service sector without adverse job impacts. Zhang (2018), using a spatial job search model, documents short-term and localized employment adjustments but broader wage transmission following US municipal minimum wage hikes.

Taken together, the literature indicates that wage compression is a robust consequence of minimum wage increases in both developing and developed settings. Employment effects are more varied in developing economies, often

depending on enforcement, labor market segmentation, and demographic composition. In developed countries, minimum wage hikes rarely result in job losses, likely due to stronger enforcement, centralized bargaining, and well-functioning institutions.

Spillover effects of minimum wage policies differ markedly between developed and developing country contexts. In developed economies with relatively strong enforcement institutions, centralized wage-setting mechanisms, and high formal sector coverage, spillovers tend to be more predictable and broad-based—spreading across firms, sectors, and regions (Cribb et al. 2021; Neumark and Wascher 2007). For example, evidence from the UK and Canada shows that wage compression and wage growth are not limited to directly affected workers but extend to adjacent wage groups in the formal sector (Fang et al. 2021; Cribb et al. 2021). In contrast, the spillover effects observed in developing economies are often more irregular, reflecting the fragmented nature of labor markets, weak enforcement, and high rates of informality. Studies from countries such as Honduras and Brazil highlight that minimum wage hikes may not propagate uniformly and often fail to influence wages in uncovered or informal sectors (Gindling and Terrell 2009; Lemos 2009). Cross-country analyses reinforce this distinction: Rani et al. (2013) show that in countries with low compliance and poor administrative capacity, minimum wage increases frequently have little to no impact beyond the immediate wage floor. Similarly, ILO Global Wage Reports and comparative studies find that the institutional design of minimum wage systems—including frequency of adjustment, stakeholder involvement, and enforcement mechanisms—plays a critical role in shaping the scale and nature of spillovers (ILO

2016; Betcherman 2015). These contrasts underscore that minimum wage policy effectiveness depends not only on the statutory level but also on the structure and governance of the labor market in which it operates.

This literature provides a strong basis for applying modern identification strategies—such as bunching estimation—to detect and isolate the effects of minimum wage changes in labor markets like Turkey's, where formal-informal duality and centralized wage-setting create both challenges and opportunities for empirical analysis.

Accordingly, this study seeks to examine the employment effects of the 2016 minimum wage hike in Turkey using a bunching estimation framework. The analysis covers heterogeneity by formality, gender, age, education, sector, and occupation. While the research question itself is not new, the empirical contribution lies in the application of a novel identification strategy that addresses the shortcomings of conventional approaches in a middle-income context.

Traditional evaluations of minimum wage effects often assess average effects across broad wage distributions. This can dilute the true impact of minimum wages, which likely manifests around the "bite" point rather than the upper tail. This is especially relevant in Turkey, where the minimum wage is nationally set and highly binding. Gürcihan-Yüncüler and Yüncüler (2016) show that its influence fades above the sixth decile of the wage distribution, suggesting conventional methods may underestimate local effects.

To address these limitations, this study applies "bunching estimation", a method originally developed to study tax kinks (Saez 2010; Kleven 2016) and recently extended to labor markets (Cengiz et al. 2019). Bunching methods focus on

behavioral responses around policy thresholds, allowing for a more accurate estimation of localized effects. This approach has shown promise in other settings: Cengiz et al. (2019) report wage gains without job losses in the US; Harasztosi and Lindner (2019) find job reductions in Hungarian firms exposed to large hikes; and Cribb et al. (2021) observe mild employment responses in the UK. Jales (2018) uses density discontinuity in Brazil to estimate informal–formal transitions, providing methodological relevance to Turkey's context.

Due to the absence of regional variation in wage policy, counterfactual distribution for Turkey has to be constructed using historical data (as in Harasztosi and Lindner 2019). Turkey's high share of minimum wage workers, at around 40 percent, and the centralized wage-setting structure provide a strong setting for testing bunching-based identification.

Using microdata from the Household Labor Force Survey (HLFS) provided by TURKSTAT from 2014 to 2019, this study focuses on the 2016 minimum wage hike, which raised the net minimum wage by 30 percent. This unexpected and sizable nominal increase, introduced after the elections and exceeding year-end inflation expectations (high single-digit levels) back then, constitutes a plausibly exogenous policy shock. By concentrating on employment responses near the wage floor, this study aims to improve the precision of estimates. In sum, the analysis aligns theoretical expectations with methodological innovation and provides a robust evaluation of minimum wage effects in a labor market characterized by informality, heterogeneity, and centralized wage setting.

This study finds that the 2016 minimum wage hike in Turkey produced localized negative employment effects, particularly in wage bins directly

surrounding the new minimum wage threshold. Using a bunching estimation strategy, we show that the policy led to short-term employment losses, especially among formally employed, low-educated, and young workers—groups with the highest exposure to the wage floor. These effects were concentrated immediately after the reform and diminished in subsequent years, suggesting a transitory labor market disruption followed by partial adjustment.

The results are robust to alternative specifications, including wider treatment windows and continuous measures of policy exposure, and hold up under falsification tests using unaffected wage segments. Notably, we find no significant increase in informal employment, nor any measurable effect on female employment. Overall, the findings support the use of distribution-sensitive estimation techniques in minimum wage research and suggest that while wage compression goals may be achieved, short-run costs for vulnerable groups must be carefully managed.

This study makes several contributions to literature. First, it is the first to apply bunching estimation to the Turkish labor market, offering a novel methodological lens for detecting localized employment effects of minimum wage policy. Second, it provides rich empirical evidence on short-term disemployment and subsequent adjustment dynamics in a high-informality, middle-income setting. Finally, it contributes to the broader debate on minimum wage design by highlighting the importance of enforcement, segmentation, and demographic vulnerabilities in shaping labor market responses.

The remainder of this study is organized as follows: Section 2.2 presents a summary of minimum wage legislation in Turkey. Sections 2.3 and 2.4 introduce

the data set and the methodology, respectively. In Section 2.5, empirical analyses are given. In Section 2.6 the policy implications are discussed and Section 2.7 concludes.

2.2. A Background: Minimum Wage Legislation in Turkey, Its Historical and Institutional Overview

The nation-wide minimum wage legislation in Turkey dates back to 1974, following the enactment of the Labor Law No. 1475, which laid the foundation for regulating basic employment rights, including wage floors. The law mandated that workers should be paid at least a minimum standard of income sufficient to sustain a decent standard of living, considering national economic conditions.

Currently the task of determining the minimum wage belongs to the Minimum Wage Fixing Commission, a tripartite body that includes representatives from the government, employers, and workers' unions. The commission meets either twice a year, which was more of a historical practice in the past, or once a year, which is the most recent practice, to review macroeconomic indicators such as inflation, GDP growth, labor productivity, and cost-of-living benchmarks. The final decision is typically announced in late December and implemented in January. While the process appears consensus-based, in practice, the government holds *de facto* decision-making power. Employer and union representatives frequently dissent, especially when inflationary conditions and exchange rate fluctuations accelerate real wage erosion.

The minimum wage in Turkey is a nationally uniform nominal value — i.e., it does not vary by region, sector, or age group, with rare exceptions in the past. Until

2014 there was a distinct minimum wage level for youth, who were below 16. This nationwide scope contrasts with systems in federal countries like the US or Canada and introduces specific identification challenges for empirical studies. The legal minimum wage applies to all wage and salary workers, regardless of gender or age (no youth exemptions), sector (formal or informal), occupation, or employment contract type (fixed or indefinite). Compliance, however, is largely limited to the formal sector. Studies such as Gürcihan-Yüncüler and Yüncüler (2016) and Öztek (2021) show that informal workers often earn less than the legal minimum, indicating partial or non-compliance due to weak enforcement.

The minimum wage is declared in gross and net terms, with the net wage reflecting deductions for income tax, social security premiums, and unemployment insurance contributions. In recent years, some of these deductions have been subsidized by the state to ease the burden on employers.

Despite the broad legal coverage, enforcement remains asymmetric. Formal firms generally comply, especially due to payroll audits and tax filings. In the informal sector, compliance is low due to limited labor inspections and the cost pressures on small firms.[1] Seasonal workers, domestic workers, and unregistered migrants are particularly vulnerable to underpayment. Moreover, there are indirect consequences of aggressive minimum wage hikes: Firm exit and downsizing among low-productivity SMEs (Bossavie et al. 2019), shifts to informal employment contracts, especially in sectors with tight margins, non-wage adjustments, such as reduced hours or fringe benefits.

[1] According to TURKSTAT's yearly data bulletin, informality rate during our sample period is on average 34 percent for the whole labor market, higher at 82.6 percent for agricultural sector and lower at 22 percent for non-agricultural sectors, which we concentrate on in our empirical analysis.

To understand the historical evolution of the minimum wage in Turkey, it is crucial to examine its real value adjusted by the Consumer Price Index (CPI), as nominal trends alone are obscured by inflation. The sharp devaluation of the Turkish Lira in 1979, followed by a prolonged period of high inflation, significantly eroded the real purchasing power of the minimum wage. Throughout the 1980s, the real minimum wage remained largely stagnant.

Although the 1990s brought some improvement through real increases, these gains were short-lived. The economic crises of 1994 and 2001, accompanied by further currency depreciations, reversed earlier progress. A sustained recovery in the real minimum wage began after the 2001 crisis. Between 2003 and 2025, the real minimum wage more than tripled (Figure 2.1). However, with a few exceptions, annual growth in the real minimum wage remained modest throughout this period.

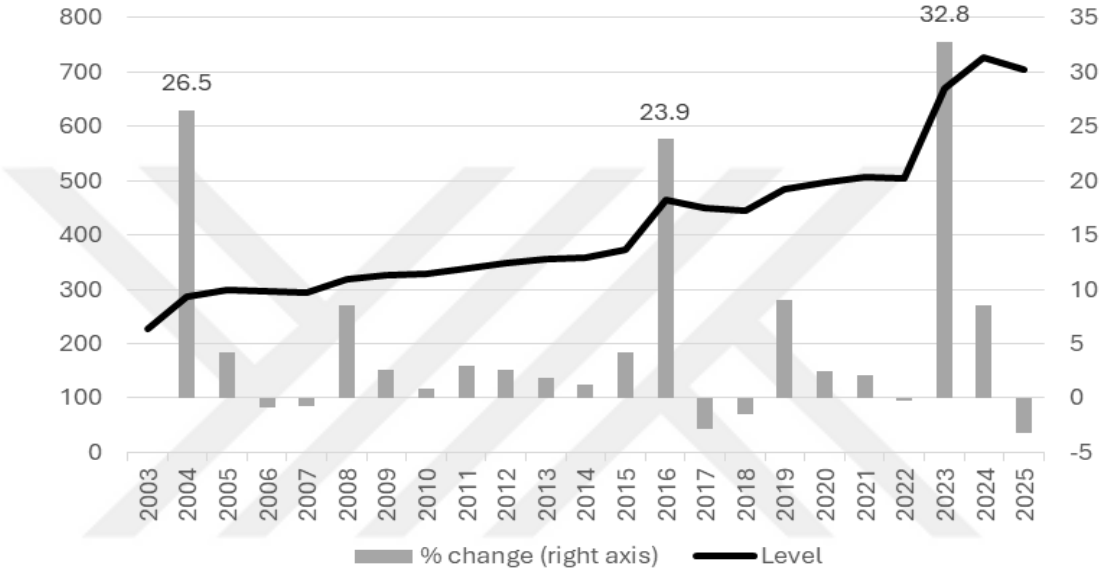
Notable increases occurred in 2004, 2016, and 2023. The 2016 hike, which was an election promise by the ruling government, marked a significant policy rupture, raising the net minimum wage by 30 percent in nominal terms. This study investigates the labor market impacts of that specific increase.

After 2020, minimum wage adjustments became more frequent, with mid-year revisions introduced in both 2022 and 2023.[2] These were aimed at mitigating the impact of high inflation and persistent currency volatility. The large adjustment

[2] This study does not examine the effects of the 2023 minimum wage increases for several reasons. First, the substantial wage hikes appear to reflect a delayed adjustment aimed at offsetting prior losses in real purchasing power—losses that were more severe than those suggested by official CPI figures. As such, the exogeneity of the 2023 adjustment is questionable. Second, the pre-hike period was marked by macroeconomic instability, making it difficult to construct a credible counterfactual scenario. Third, there is currently insufficient post-hike data available to evaluate the short- or long-term effects of the policy. Lastly, a major shift in macroeconomic policy-making occurred in mid-2023, with a reversion to orthodox economic measures, which has triggered widespread rebalancing across the economy and further complicates causal attribution.

in 2023 was also a response to distortions in relative prices within the CPI. Specifically, the cost of living—particularly for food, energy, and rent, which constitute a larger share of low-income households' consumption—rose faster than the official CPI, creating additional pressure for real wage adjustments at the lower end of the distribution.

Figure 2.1. Net Minimum Wage in Turkey (real, in 2003 prices)

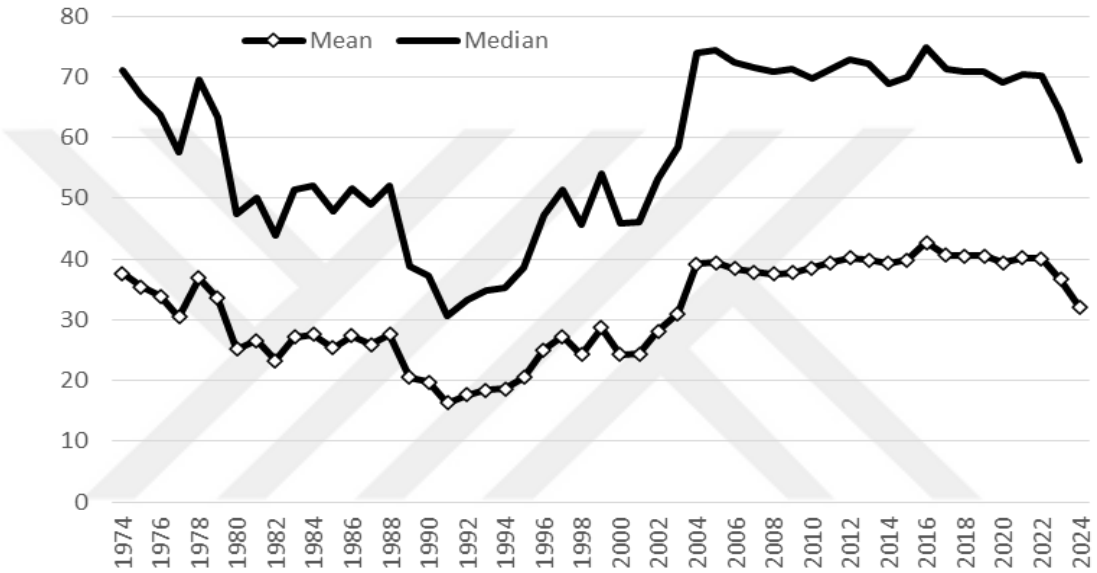


Source: Ministry of Labor and Social Security (MLSS), TURKSTAT
Note: The deflated net minimum wage is author's own calculation.

The ratio of the minimum wage to the median wage (commonly referred to as the Kaitz Index) has historically been relatively high in Turkey. Throughout the 2000s and 2010s, it hovered around 70 percent, before declining to just under 60 percent in recent years. This level remains among the highest in the OECD (Figures 2.2 and 2.3), indicating that the minimum wage in Turkey is highly binding and plays a central role in wage-setting across the labor market. It is estimated that over 40 percent of wage earners receive wages at or near the minimum, a concentration considerably higher than in most advanced economies. This share is especially

pronounced in low-skilled, female-dominated, and youth-intensive occupations, where the minimum wage effectively sets the prevailing wage floor. In this context, the minimum wage not only defines the lower bound of the wage distribution but also serves as a wage anchor, exerting influence over mid-range earnings and contributing to overall wage compression (Pelek 2018).

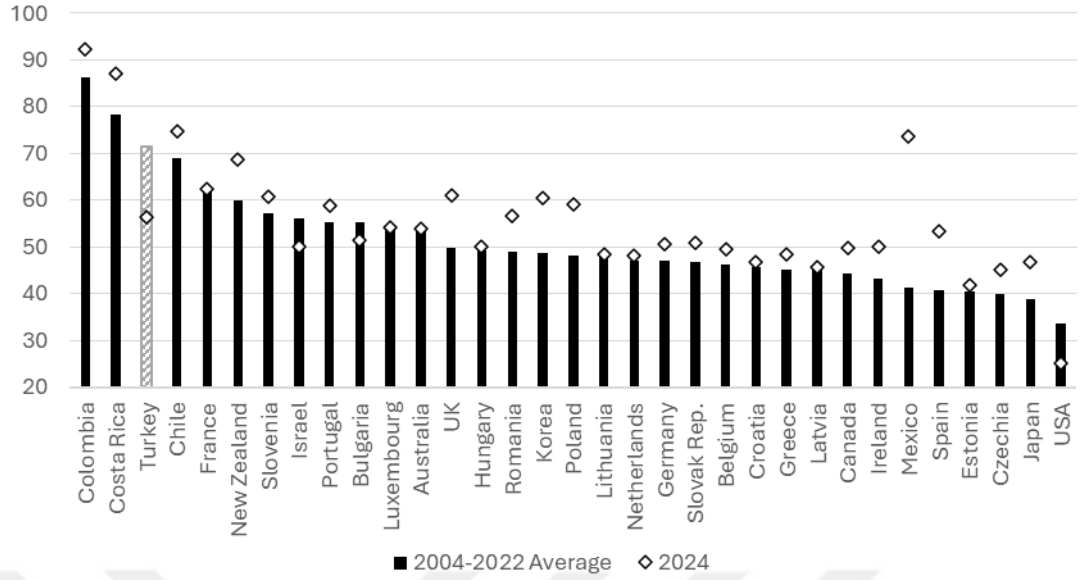
Figure 2.2. Minimum Wage in Turkey for Full-Time Workers (as a % of Mean and Median Wage)



Source: OECD

In sum, Turkey's minimum wage system is legally universal but institutionally complex, combining centralized decision-making with decentralized enforcement realities. The wage floor is relatively high by international standards, with profound implications for wage structure, employment dynamics, and informality. These features make Turkey an ideal case to study localized labor market effects of minimum wage policy using modern estimation techniques such as bunching.

Figure 2.3. Minimum Wage in OECD Countries for Full-Time Workers (% of Median Wage)



Source: OECD

2.3. Data

This study uses individual-level data from the Household Labor Force Survey (HLFS) micro datasets. HLFS is the principal source of nationally representative labor market data in Turkey. Conducted by the Turkish Statistical Institute (TURKSTAT), HLFS provides rich micro-level information on individuals' employment status, demographic characteristics, education, occupation, earnings, and labor market behavior. The survey is harmonized with International Labor Organization (ILO) definitions and European Union (EU) labor force survey standards, enabling cross-country comparability.

The dataset includes broad coverage of both formal and informal employment—particularly important in the Turkish context, where labor market duality is a defining feature. In 2014, TURKSTAT implemented a major methodological revision to HLFS. While not elaborated in detail here, this revision introduced a structural break in time series comparability, especially for panel-

based or pre/post analyses. To maintain internal consistency, this study uses only post-2014 data, as bunching estimation is highly sensitive to distributional structure and bin-level employment counts.

Accordingly, the analysis draws on HLFS microdata for the years 2014 to 2019, capturing the period around the 2016 minimum wage hike. The pre-treatment period includes 2014 and 2015, while post-treatment data extends up to 2019. Earlier datasets are excluded due to the methodological break in 2014, while 2020 and beyond are omitted because of labor market distortions caused by the COVID-19 pandemic.[3]

To ensure consistency across years, several sample restrictions are applied. First, we retain only employed individuals, specifically those identified as paid employees or casual workers. We exclude self-employed individuals and unpaid family workers, whose earnings are either missing or not comparable. We further restrict the sample to individuals working between 30 and 85 hours per week. The lower bound excludes part-time workers, as Turkey's minimum wage applies to full-time employment. The upper bound eliminates potential outliers and reporting errors. Workers reporting zero income are also excluded to avoid distortions in the wage distribution.

Minimum wage values used in the analysis are net monthly wages, obtained from the Ministry of Labor and Social Security (MLSS). In 2014 and 2015, the minimum wage was adjusted twice per year; since HLFS does not contain interview

[3] In response to the pandemic, the Turkish government introduced emergency labor market policies—such as job retention subsidies, layoff bans, and short-term work allowances—which would confound any analysis of minimum wage effects. The selected time window (2014–2019) allows for a clean identification strategy uninfluenced by these exogenous shocks.

dates, we use annual averages for these years. From 2016 onward, the minimum wage has been set once annually in January, avoiding mid-year ambiguity.

Given Turkey's high-inflation environment and the frequency of wage floor adjustments, all wages are converted to 2016 constant prices. Pre-2016 wages are inflated, and post-2016 wages are deflated using average annual Consumer Price Index (CPI) changes.[4] Furthermore, we rescale the real wage distributions based on changes in the real minimum wage to ensure temporal consistency—specifically, aligning the minimum wage spikes in 2017 and 2019 with the 2016 benchmark, and the 2014 spike with that of 2015. This adjustment allows for a consistent comparison of wage bin positions across years despite changes in the wage floor.

To ensure comparability of employment levels across years, we normalize employment figures by the cumulative population change relative to 2016, addressing Turkey's dynamic demographic trends during this period.

At the individual level, we construct binary indicators for demographic and job characteristics: female, married, young (under age 25), low-educated (illiterate or below high school), informal (not registered with the social security system), public sector employment, and residence in a high-income region (NUTS-2 regions with above-average per capita GDP in 2015).

The data are then collapsed into wage-bin-level aggregates, grouped by year, industry, and occupation, using sampling weights provided by TURKSTAT. Individual characteristics are averaged at the industry–occupation group level.

[4] In 2016 prices, the previous minimum wage was approximately 1050 TL, and the new wage was set at 1300 TL.

HLFS classifies employment across nine major industries[5] and nine occupational groups[6]. We exclude agriculture (Industry 1) and skilled agricultural occupations (Occupation 6), as employment in these categories tends to respond less to macroeconomic conditions and policy shifts. The final analytical sample comprises 64 industry–occupation cells (8 industries × 8 occupations), providing the backbone for the estimation framework.

Finally, the economic activity indicator for each industry is compiled from the annual national accounts statistics of TURKSTAT, i.e. from Gross Domestic Product (GDP) in chain linked volume index (2009=100).

2.4. Methodology

This section outlines the empirical strategy used to estimate the employment effects of the 2016 minimum wage hike in Turkey. Given the national scope and binding nature of the policy, alongside the high density of workers near the statutory wage floor, this study adopts a bunching estimation framework to detect localized labor market responses.

Conventional methods such as difference-in-differences (DiD) or regression discontinuity designs often rely on regional variation or administrative thresholds for identification. However, in Turkey, the uniform national minimum wage leaves no untreated regions or groups. Consequently, these approaches are limited in their

[5] Industrial groups are with respect to NACE classification are: 1- agriculture, forestry and fishing, 2-mining and quarrying, 3-manufacturing, 4-electricity and water supply, 5-construction, 6-wholesale and retail trade, 7-transportation and storage, 8-accommodation and food services, 9-other services

[6] Occupational groups with respect to ISCO 88 classification are: 1-managers, 2-professionals, 3-technicians and associate professionals, 4-clerical support workers, 5-service and sales workers, 6-skilled agricultural workers, 7-craft and related trades workers, 8-plant and machine operators, 9-elementary occupations

applicability. In contrast, bunching estimation exploits kinks or discontinuities in policy thresholds, such as a legally binding minimum wage, to identify behavioral responses even in a fully treated setting.

Bunching methods were originally developed in the context of tax policy (e.g., Saez 2010; Kleven 2016) and have since been extended to minimum wage evaluations (Cengiz et al. 2019; Harasztosi and Lindner 2019; Jales 2018). The approach involves comparing the observed wage distribution after the policy change with a counterfactual distribution that reflects what the wage distribution would have looked like in the absence of the reform. Deviations—such as an excess mass above the new minimum wage or a missing mass below it—provide insights into the effects of the policy on employment allocation and wage-setting behavior.

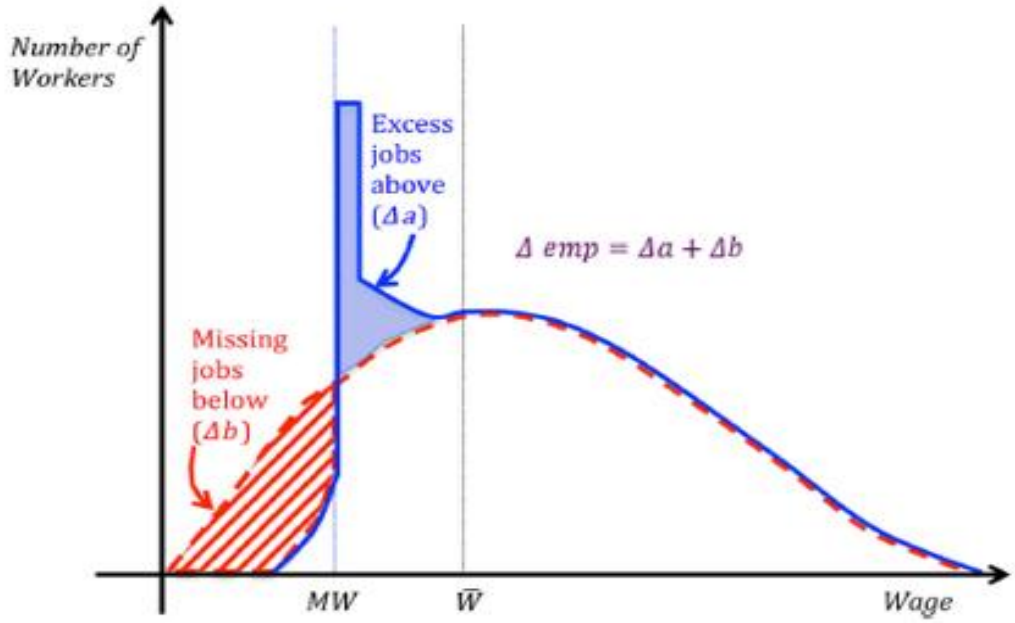
2.4.a. Conceptual Framework

Figure 2.4 illustrates the core logic of the bunching estimation approach as applied to minimum wage analysis, adapted from Cengiz et al. (2019). Under the stylized assumption that a minimum wage is introduced for the first time, the red line represents the pre-policy wage distribution and the blue line represents the post-policy distribution. W-bar in x-axis represents the extent the minimum wage change is influential on the wage distribution.

The red shaded area beneath the minimum wage represents the "missing mass"—jobs that previously paid below the wage floor but are no longer observed under full compliance. Conversely, the blue shaded area just above the minimum wage captures the "excess mass"—jobs that either shifted upward in compliance or

new jobs created near the wage floor. The net employment effect of the policy is interpreted as the sum of these two shaded areas.

Figure 2.4. Employment Density Around the Minimum Wage



Source: Cengiz et al. (2019)

2.4.b. Counterfactual Distribution Estimation

A crucial component of bunching analysis is the construction of a counterfactual wage distribution—that is, an estimate of what the employment distribution would have looked like had the minimum wage not been increased in 2016.

In this study, the counterfactual is constructed using pre-hike data from 2014 and 2015, with all wages expressed in 2016 constant prices. The difference between actual post-policy employment and the counterfactual represents either bunching (excess mass) or job loss (missing mass). Summing up these differences across a

relevant window provides an estimate of the policy's net impact on employment in the affected wage region.

2.4.c. Wage Bin Construction

To implement the bunching estimation framework and to identify the relevant window in the wage distribution to estimate the impact, wage distribution has to be divided into multiple wage bins. However, there is no clear-cut rule to determine the number of wage bins. Ensuring a sufficient number of observations within each wage bin and positioning both pre-hike and post-hike minimum wage levels near the center of the relevant bin are important criteria for assessing the robustness of the wage bin structure

In this study, individual-level wages from HLFS are grouped into 20 wage bins, defined in 2016 constant prices. These bins are designed to capture localized distortions in the wage distribution around the minimum wage threshold. Unlike Cengiz et al. (2019)[7], bin widths are not uniform: they are narrower near the statutory minimum wage to increase precision in the region where bunching is most likely to occur and wider in the tails of the distribution to improve statistical efficiency, reduce noise and ensure sufficient number of observations.

The binning structure is as follows: **Bins 1 to 3** capture the lower end of the wage distribution (wages below 1000 TL), using broader intervals to absorb sparse observations. **Bins 4 to 7** narrow the range as the distribution approaches the minimum wage, covering wages between 1000 TL and 1475 TL. Old and new

[7] In Cengiz et al. (2019), the analysis of minimum wage effects in US is based on hourly wages, with the wage distribution divided into narrow intervals of $0.25. However, their study leverages a rich dataset comprising 138 state-level minimum wage changes between 1979 and 2016, which allows them to maintain a sufficient number of observations within each bin despite the small interval size.

minimum wages fall into this interval. In real terms (i.e. in 2016 prices), the minimum wage in 2015 was approximately 1050 TL, and the minimum wage rose to 1300 TL in 2016. **Bins 8 to 14** use 200 TL intervals, spanning wages from 1475 TL to 3075 TL, encompassing the near-above-minimum-wage segment where displacement and compression effects are likely to occur. **Bins 15 to 19** widen to 500 TL intervals, capturing upper wage segments where minimum wage policy is unlikely to exert influence. **Bin 20** aggregates all wages above 5075 TL, acting as a right-censored tail bin. Wage bins are fixed across years, allowing for comparison of pre- and post-policy wage distributions in real terms.

This dynamic binning structure ensures granularity to identify behavioral responses near the bite while preserving computational tractability in less relevant regions of the wage distribution. Each worker's wage is placed into one of the 20 bins.

2.4.d. Estimation Strategy

2.4.d.i. Construction of the Impact Variable

Since Turkey's minimum wage is set at the national level and applies uniformly across all sectors and regions, we cannot exploit cross-regional or cross-sectoral policy variation. However, different sectors and occupations vary in how intensely they are affected by the minimum wage hike. We capture this exposure through an "impact variable" defined at the industry–occupation group level, following Dickens et al. (1999).

In this regard, there are two main categories of indicators in the literature:

1. **Relative measures** such as the Kaitz Index (minimum wage over average or median wage),
2. **Bite-based measures** such as the share of workers earning around the minimum wage (e.g., fraction at, below, or affected).

While relative indicators are easier to interpret, they are sensitive to movements in the denominator (e.g., median wages), which may be influenced by other economic forces (Lemos 2004). In contrast, “fraction at” indicators are directly tied to the minimum wage threshold, though their interpretation may require additional adjustment.

We adopt the "fraction at" approach. For each of the 64 industry–occupation groups, we compute the share of total employment in 2015 that falls within ±2 percent of the minimum wage[8]. This gives a continuous measure of minimum wage exposure.

To classify treated vs. control groups, we construct a binary version: Groups with a "fraction at" above the sample-wide mean are defined as high-impact (treated). Those below are defined as low-impact (control). This pre-treatment classification preserves causal identification by ensuring the impact variable is not influenced by post-policy outcomes. We also estimate the model using the continuous version of fraction at to capture varying degrees of intensity.

Table 2.1 presents the computed values of the “fraction at” indicator for each group. Red-colored cells indicate those above the sample mean, forming the treated

[8] There is no universally accepted threshold for defining the wage range that constitutes the close neighborhood of the minimum wage. For example, Gürcihan-Yüncüler and Yüncüler (2016) define the “fraction at” metric using a 5 percent bandwidth around the minimum wage. In this study, we adopt a narrower 2 percent bandwidth to enhance the precision of our estimates.

group in the binary specification. These are predominantly found in service-oriented sectors.

		Occupation							
		1	**2**	**3**	**4**	**5**	**7**	**8**	**9**
Industry	**2**	0.0	0.0	7.1	5.1	36.3	12.4	14.5	18.6
	3	2.1	5.8	9.8	22.2	32.3	26.4	29.0	39.1
	4	0.0	1.2	2.2	6.5	21.7	12.1	13.2	29.7
	5	10.8	3.1	12.1	22.5	35.7	21.1	20.5	28.2
	6	3.1	6.5	18.3	25.1	32.8	24.3	23.9	38.4
	7	1.3	3.0	8.2	12.7	24.0	5.4	22.1	28.9
	8	7.5	18.3	6.3	25.4	28.9	24.3	23.9	35.0
	9	4.3	2.8	10.9	15.5	18.2	19.5	14.4	36.5

Table 2.1. Fraction at by Industry-Occupation Groups

2.4.d.ii. Defining the Treatment Window (Bite of the Minimum Wage)

To complete the treatment definition, we identify the wage bins most directly affected by the 2016 minimum wage hike. In real 2016 prices, the previous minimum wage was approximately 1050 TL, and the new wage rose to 1300 TL. Therefore, wage bins 3 to 6 (800–1350 TL) are designated as the treatment window in the baseline specification[9].

2.4.d.iii. Model Specification

To estimate the employment effects of the 2016 minimum wage hike, we use a regression model that captures variation across wage bins, industry–occupation groups, and time. The core specification is:

$$Emp_{b,i,t} = \alpha + \delta_1 \cdot Post_t + \delta_2 \cdot Impact_i + \theta \cdot Treatment_{b,i,t} + \gamma\, Z_{b,i,t} + \lambda_t + \phi_{b,i} + \psi_{b,t} + \varepsilon_{b,i,t}$$

[9] Although a wage level of 800 TL is low for formal employment—given that no worker should legally earn below the statutory minimum—this threshold is included in our analysis to capture potential instances of informal employment earning sub-minimum wages, as well as possible non-compliance within the formal sector and also to consider spillover effects of minimum wage for lower wages.

- where, b, i and t denote wage bin, industry-occupation group and year, respectively.
- $Emp_{b,i,t}$ is the number of employed individuals in wage bin b, industry–occupation group i, and year t, aggregated from HLFS microdata using person-level sampling weights.
- $Post_t$ is the dummy variable equal to 1 for post-hike years (2016 and after), 0 otherwise.
- $Impact_i$ indicates whether industry–occupation group i is classified as high-exposure to the minimum wage. It has binary and continuous versions. The details will be discussed later.
- $Treatment_{b,i,t}$ is the triple interaction term identifying whether bin b in group i during year t lies within the affected range of the wage distribution in a high-exposure group after 2016.
- $Z_{b,i,t}$ is a vector of control variables: log GDP by industry, and the group-level averages of indicators for female, married, youth (age < 25), low education, informal employment, public sector status, region, and working hours.
- λ_t is the year fixed effects, controlling macroeconomic shocks and time trends common to all groups.
- $\phi_{b,i}$ is wage bin × industry–occupation fixed effects, capturing time-invariant structural differences across wage segments within sectors.
- $\psi_{b,t}$ is the (wage bin × year) interaction, allowing each bin to evolve differently across time.
- $\varepsilon_{b,i,t}$ is the error term, clustered at the industry–occupation level.

The coefficient of interest is θ, which captures the differential employment change in wage bins near the minimum wage, within sectors highly exposed to the reform, relative to lower-exposure groups or unaffected wage bins. A negative and significant estimate of θ would suggest job loss or reallocation, while a zero or positive coefficient would imply wage adjustment without disemployment.

We estimate this model for the entire sample as well as key subgroups (female, youth, low-educated, formal and informal workers) to explore heterogeneous effects across vulnerable populations in the labor market.

2.4.d.iv. Robustness Check

We assess the robustness of our results in two dimensions:

1. **Bin window sensitivity**: We re-estimate the model using an alternative treatment window, covering bins 2 to 7 (650–1475 TL), to test whether results depend on bin choice and to capture potential spillovers documented in prior studies (e.g., Gürcihan-Yüncüler and Yüncüler 2016).
2. **Impact variable specification:** We compare results from the binary impact variable with those using the continuous version (fraction at), to examine sensitivity to exposure intensity classification. Here, we keep the bite of the minimum wage as the wage bin interval 3 to 6, as in the baseline estimates.

As a falsification test, we estimate the model in the upper tail of the wage distribution—specifically, the last 5 wage bins—where the minimum wage should

have little to no impact. Finding no significant effects in this range would support the validity of our identification strategy.

2.4.d.v. Identifying Short-term and Long-term Effects

We explore the temporal dynamics of the policy impact by estimating short-term (2016 only) and longer-term (2019 only) effects separately. This distinction is important, as labor market responses may differ in the immediate aftermath of a large wage shock versus after adjustment periods. Immediate job losses may partially recover as firms adapt to new cost structures over time. Here, we keep the bite of the minimum wage as the wage bin interval 3 to 6, as in the baseline estimates.

2.5. Empirical Results

This section presents the empirical results of the bunching estimation framework applied to the 2016 minimum wage hike in Turkey. The goal is to evaluate the effect of the policy on employment outcomes, with a particular focus on the lower end of the wage distribution where the minimum wage is binding.

2.5.a. Results Using the Full Wage Distribution

We begin empirical analysis by examining the effect of the 2016 minimum wage hike on employment across the entire wage distribution, using a conventional DiD framework. In this approach, we compare average employment changes in treated industry–occupation groups to those in less exposed groups, without restricting the analysis to wage bins directly affected by the policy.

Table 2.2 reports these results using a binary treatment indicator and the full sample, along with subgroup estimates.

	Total	Formal	Informal	Low-educated	Female	Young
post	-3,201***	-2,434**	-1,327*	-2,193**	-681.0	-680.6
	(1,202)	(1,038)	(781.2)	(1,058)	(937.2)	(694.3)
impact	-594.6	-247.8	198.7	-412.5	-995.5	-987.8**
	(566.9)	(454.8)	(309.7)	(385.7)	(686.5)	(445.4)
treatment	596.9	682.4	-87.51	202.2	-43.55	22.21
	(671.0)	(663.5)	(407.0)	(614.2)	(808.1)	(335.3)
lgdp	17,368***	13,002***	6,820**	11,362**	9,469*	3,371
	(5,600)	(4,617)	(3,276)	(4,675)	(5,286)	(2,181)
female	-757.1	-1,985*	-64.24	999.9	-	229.8
	(1,164)	(1,011)	(273.3)	(1,285)	-	(291.1)
married	-55.51	21.42	-217.3	342.9	-218.9	217.0
	(747.4)	(679.1)	(188.5)	(680.3)	(286.6)	(203.9)
youth	-1,064	-951.8	-137.1	-1,624	-222.4	-
	(1,511)	(1,231)	(214.0)	(1,431)	(633.7)	-
loweduc	429.5	144.6	-41.94	-	-35.23	-377.0
	(650.9)	(596.0)	(154.4)	-	(568.1)	(262.0)
informal	-8,450**	-	-	-4,435**	-2,919**	-1,422**
	(3,333)	-	-	(1,982)	(1,183)	(649.7)
public	-1,198	-763.2	2,665	71.23	-442.2	-517.3**
	(1,268)	(1,058)	(2,770)	(726.6)	(656.6)	(237.9)
region	782.3	1,923***	172.1	-580.9	444.6	334.9
	(656.0)	(647.2)	(221.4)	(473.9)	(350.2)	(206.6)
working_hour	41.34	44.02	-2.574	1.217	12.95	6.950
	(33.05)	(30.89)	(5.907)	(21.68)	(21.54)	(12.86)
Constant	-87,918***	-66,754***	-33,588**	-56,930**	-48,145*	-16,906
	(28,414)	(23,534)	(16,278)	(23,522)	(26,928)	(10,845)
Observations	6,533	6,370	3,254	4,963	3,952	3,587
R-squared	0.818	0.816	0.916	0.787	0.839	0.752

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 2.2. Standard Estimation Results by Groups (Treatment: Binary)

Across all groups, the estimated coefficients on the treatment variable are not statistically significant, and their magnitudes are modest. These results highlight a key limitation of using the entire wage distribution in policy evaluation: by averaging across wage bins—many of which are not directly affected by the policy—the model may dilute the actual treatment effect. This lack of precision

underscores the need for a more focused identification strategy that targets the segment of the wage distribution most sensitive to the policy.

In the next section, we apply a bunching estimation framework centered around wage bins near the minimum wage, where the density of affected workers is highest. This approach is more sensitive to localized employment changes and provides a more accurate assessment of the policy's true impact.

2.5.b. Bunching Estimation Results

We begin by presenting the baseline estimates of the empirical strategy described in Section 2.4. These results compare employment changes in treated wage bins within industry–occupation groups with high exposure to the minimum wage, against less affected groups. As a reminder, we implement this using two alternative specifications of the treatment variable: a binary classification based on "fraction at" thresholds, and a continuous version that captures the degree of exposure more flexibly.

In addition to the full sample, we report estimates for several subgroups of interest, including female, youth, low-educated, formal, and informal workers, to assess heterogeneity in the employment effects across vulnerable segments of the labor market.

To assess the robustness of our findings, we explore sensitivity to the definition of the treatment window—i.e., the wage bin range used to capture the bite of the minimum wage. We also conduct falsification tests by applying the estimation strategy to wage bins well above the minimum wage threshold, where the policy should have no effect.

Finally, we distinguish between short-term and long-term effects of the policy by separately estimating immediate impacts (in 2016) and cumulative changes over the post-treatment period (2016–2019). This allows us to assess whether initial shocks were transitory or whether the labor market adjusted over time.

2.5.b.i. Baseline Estimates

Tables 2.3 and 2.4 present the estimation result of the baseline model with binary specification and continuous specification of the treatment variable, respectively.

According to Table 2.3, in the binary version, there is a significant decline in employment for the full sample, formal workers, and low-educated workers, suggesting that the 2016 minimum wage hike led to measurable job losses in the most affected groups. The impact on formal workers is stronger than the overall impact. The effect for young workers is also statistically significant at the 10 percent level, indicating some displacement among young workers. The treatment effect for informal and female workers is negative but not statistically significant.

In the continuous treatment setup (Table 2.4), the results show strong and statistically significant negative effects for the total sample and formal workers, indicating that greater exposure to the minimum wage leads to sharper employment declines. Effects remain significant and negative for low-educate and young workers, reinforcing findings from the binary specification. Again, female and informal workers show negative but statistically insignificant effects, suggesting that either these groups are less responsive or the policy's effects are masked by other forces.

	Total	Formal	Informal	Low-educated	Female	Young
post	-2,859**	-2,130**	-1,345*	-2,212**	-677.6	-722.6
	(1,164)	(997.3)	(792.6)	(1,048)	(955.0)	(718.6)
impact	-188.3	184.1	149.3	-275.0	-996.8*	-833.7*
	(336.5)	(299.4)	(236.1)	(195.1)	(595.3)	(432.8)
treatment	-5,505*	-7,284**	-218.5	-5,768*	-671.6	-1,511*
	(2,994)	(3,116)	(464.0)	(3,205)	(1,216)	(904.5)
lgdp	16,037***	11,903**	6,817**	11,023**	9,396*	3,298
	(5,634)	(4,700)	(3,287)	(4,772)	(5,366)	(2,190)
female	-712.2	-1,755*	-61.65	1,040	-	242.1
	(1,177)	(954.6)	(274.0)	(1,349)	-	(286.1)
married	-89.46	-55.13	-219.8	414.3	-224.6	221.7
	(763.4)	(691.8)	(189.7)	(677.8)	(290.3)	(207.6)
youth	-815.4	-749.0	-137.9	-1,480	-216.2	-
	(1,444)	(1,188)	(211.9)	(1,372)	(635.2)	-
loweduc	463.8	274.7	-37.48	-	-47.62	-386.9
	(648.6)	(591.6)	(154.5)	-	(575.4)	(260.8)
informal	-7,930**	-	-	-4,229**	-2,863**	-1,329**
	(3,095)	-	-	(1,868)	(1,149)	(617.2)
public	-1,124	-705.5	2,676	160.5	-426.7	-475.0*
	(1,275)	(1,098)	(2,774)	(735.6)	(655.5)	(238.2)
region	755.9	1,836***	169.0	-594.4	437.2	320.7
	(656.8)	(618.7)	(216.8)	(479.5)	(352.0)	(200.0)
working_hour	43.72	46.57	-2.547	7.282	13.48	8.580
	(33.32)	(31.19)	(5.870)	(22.24)	(21.42)	(13.28)
Constant	-81,597***	-61,624**	-33,572**	-55,726**	-47,829*	-16,715
	(28,608)	(24,060)	(16,321)	(24,071)	(27,341)	(10,896)
Observations	6,533	6,370	3,254	4,963	3,952	3,587
R-squared	0.818	0.816	0.916	0.788	0.839	0.752

Robust standard errors in parentheses

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

Table 2.3. Baseline Estimation Results by Subgroups (Treatment: Binary)

	Total	Formal	Informal	Low-educated	Female	Young
post	-3,023**	-2,246**	-1,335*	-2,345**	-868.6	-727.6
	(1,178)	(986.3)	(794.5)	(1,042)	(971.5)	(726.7)
impact	-9.123	6.230	37.37	-15.13	475.2***	-62.78
	(14.99)	(12.06)	(47.43)	(9.549)	(147.5)	(38.68)
treatment	-312.9***	-334.8***	11.72	-154.1**	-18.69	-57.92**
	(109.9)	(111.2)	(14.70)	(68.41)	(31.38)	(23.13)
lgdp	16,308***	12,055**	6,875**	11,212**	10,253*	3,131
	(5,563)	(4,549)	(3,304)	(4,715)	(5,434)	(2,150)
female	-612.1	-1,718*	-68.13	1,046	-	275.6
	(1,179)	(974.2)	(273.9)	(1,317)	-	(289.7)
married	-47.80	-37.17	-214.7	354.4	-213.9	238.2
	(766.1)	(694.4)	(188.8)	(671.5)	(295.3)	(212.2)
youth	-691.6	-686.8	-139.8	-1,545	-252.6	-
	(1,439)	(1,177)	(211.8)	(1,400)	(649.5)	-
loweduc	473.2	256.2	-48.82	-	-161.5	-380.1
	(637.8)	(569.6)	(155.7)	-	(593.2)	(259.5)
informal	-7,500**	-	-	-4,114**	-2,847**	-1,286**
	(3,024)	-	-	(1,877)	(1,157)	(614.4)
public	-1,007	-640.4	2,641	164.4	-417.7	-447.9*
	(1,276)	(1,102)	(2,728)	(732.5)	(644.0)	(235.8)
region	747.1	1,788***	183.3	-637.7	441.7	291.7
	(651.5)	(606.4)	(211.7)	(479.8)	(350.7)	(195.5)
working_hour	45.75	49.38	-2.603	3.330	19.67	8.292
	(33.55)	(31.40)	(5.897)	(22.10)	(22.26)	(13.45)
Constant	-83,113***	-62,700***	-34,231**	-56,215**	-60,087**	-14,605
	(28,360)	(23,426)	(16,438)	(23,801)	(28,814)	(10,689)
Observations	6,533	6,370	3,237	4,948	3,913	3,574
R-squared	0.819	0.816	0.915	0.788	0.839	0.752

Robust standard errors in parentheses

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

Table 2.4. Baseline Estimation Results by Subgroups (Treatment: Continuous)

Across both models, the treatment coefficient clearly identifies a significant drop in employment localized near the minimum wage, particularly for formal, youth, and low-educated workers. The consistency across binary and continuous definitions strengthens the interpretation that the 2016 minimum wage hike

produced localized disemployment effects, rather than broad-based labor market changes.

2.5.b.ii. Robustness Chcek for the Baseline Estimates

To test the sensitivity of our baseline results to the definition of the treatment window, we re-estimate the main specification using a wider wage interval—specifically, bins 2 to 7 (650–1475 TL). We repeat the estimation using both the binary treatment and continuous exposure definitions.

For the binary definition under the wider bin window, the estimated treatment effects remain negative across most groups, though fewer coefficients are statistically significant (Table 2.5). Only the formal sector shows a weakly significant decline in employment, while all other estimates become statistically insignificant despite maintaining similar magnitudes. This is consistent with the bunching theory, which predicts that the strongest distortions should be concentrated in a narrow window around the minimum wage.

With the continuous exposure measure, the results show more robust patterns of significance (Table 2.6). The total, formal, and youth groups display statistically significant negative treatment effects. The formal sector, particularly, continues to show the strongest decline in employment, suggesting the policy's disemployment effects persist under broader definitions of exposure.

While coefficients for informal, female, and low-educated workers are small and insignificant, the directional consistency with baseline estimates provides additional credibility.

	Total	Formal	Informal	Low-educated	Female	Young
post	-2,901**	-2,175**	-1,344*	-2,176**	-684.8	-705.9
	(1,160)	(985.3)	(791.7)	(1,041)	(954.4)	(714.7)
impact	-180.5	182.2	144.6	-277.8	-1,025*	-873.9*
	(334.6)	(299.2)	(235.8)	(194.0)	(593.7)	(437.5)
treatment	-2,352	-3,353*	-137.9	-2,844	-37.56	-812.7
	(1,801)	(1,892)	(590.0)	(1,985)	(1,133)	(586.9)
lgdp	16,345***	12,192**	6,819**	11,055**	9,475*	3,310
	(5,628)	(4,691)	(3,251)	(4,752)	(5,337)	(2,186)
female	-750.5	-1,841*	-63.59	1,021	-	233.9
	(1,169)	(975.6)	(272.1)	(1,319)	-	(288.0)
married	-117.6	-44.65	-220.8	321.1	-219.5	226.1
	(753.6)	(685.1)	(187.8)	(669.6)	(291.0)	(206.9)
youth	-935.5	-872.8	-141.4	-1,565	-224.7	-
	(1,473)	(1,221)	(209.4)	(1,396)	(634.2)	-
loweduc	425.7	217.0	-38.50	-	-36.60	-374.6
	(648.4)	(592.1)	(152.0)	-	(571.8)	(259.0)
informal	-8,130**	-	-	-4,300**	-2,919**	-1,375**
	(3,179)	-	-	(1,907)	(1,170)	(632.1)
public	-1,149	-746.3	2,670	135.8	-441.8	-478.2**
	(1,272)	(1,083)	(2,787)	(729.8)	(648.6)	(236.2)
region	767.0	1,852***	171.9	-596.1	444.2	334.3
	(658.5)	(627.5)	(224.3)	(478.7)	(352.7)	(202.9)
working_hour	42.51	43.31	-2.582	4.544	13.07	8.103
	(33.41)	(30.94)	(5.911)	(22.19)	(21.39)	(13.11)
Constant	-82,989***	-62,829**	-33,576**	-55,644**	-48,173*	-16,741
	(28,569)	(23,972)	(16,147)	(23,963)	(27,195)	(10,880)
Observations	6,533	6,370	3,254	4,963	3,952	3,587
R-squared	0.818	0.816	0.916	0.788	0.839	0.752

Robust standard errors in parentheses

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

Table 2.5. Robustness Check for Baseline Estimation Results by Subgroups (Treatment: Binary)

Overall, the robustness checks confirm that the employment effects of the minimum wage hike remain negative under broader bin definitions, although statistical significance weakens somewhat in the binary treatment case. The continuous treatment specification is more stable and continues to yield statistically significant effects for core groups. These findings validate the localized bunching

assumption, affirming that policy impacts are most concentrated near the minimum wage, but extend somewhat beyond its immediate threshold.

	Total	Formal	Informal	Low-educated	Female	Young
post	-3,003**	-2,250**	-1,335	-2,250**	-877.8	-715.7
	(1,171)	(977.8)	(806.2)	(1,034)	(971.1)	(720.5)
impact	-8.701	5.843	38.39	-14.53	462.7***	-67.90*
	(14.94)	(12.15)	(46.79)	(9.524)	(149.4)	(38.98)
treatment	-144.4**	-169.1**	10.42	-67.57	3.480	-31.49**
	(68.68)	(71.01)	(17.35)	(44.16)	(26.02)	(15.50)
lgdp	16,483***	12,227***	6,881**	11,212**	10,379*	3,178
	(5,586)	(4,577)	(3,281)	(4,709)	(5,405)	(2,159)
female	-696.9	-1,813*	-62.65	999.6	-	258.3
	(1,169)	(982.4)	(275.0)	(1,297)	-	(292.5)
married	-109.1	-36.87	-211.8	323.0	-203.8	228.2
	(755.3)	(687.9)	(186.7)	(666.0)	(297.5)	(210.1)
youth	-876.4	-807.1	-132.2	-1,590	-259.5	-
	(1,467)	(1,210)	(210.9)	(1,409)	(648.9)	-
loweduc	460.3	234.1	-47.89	-	-139.0	-374.9
	(643.3)	(578.2)	(151.7)	-	(584.5)	(259.8)
informal	-7,871**	-	-	-4,283**	-2,931**	-1,354**
	(3,152)	-	-	(1,927)	(1,182)	(630.4)
public	-1,063	-690.6	2,643	131.9	-443.3	-451.0*
	(1,264)	(1,083)	(2,759)	(724.5)	(634.8)	(231.6)
region	766.0	1,826***	182.5	-612.4	451.1	320.2
	(654.5)	(619.4)	(219.6)	(477.2)	(353.5)	(199.8)
working_hour	43.49	43.18	-2.478	2.188	19.04	8.024
	(33.73)	(31.33)	(5.859)	(22.12)	(21.96)	(13.27)
Constant	-83,745***	-63,182***	-34,283**	-56,099**	-60,438**	-14,693
	(28,434)	(23,505)	(16,351)	(23,762)	(28,709)	(10,726)
Observations	6,533	6,370	3,237	4,948	3,913	3,574
R-squared	0.818	0.816	0.915	0.787	0.839	0.752

Robust standard errors in parentheses

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

Table 2.6. Robustness Check for Baseline Estimation Results by Subgroups (Treatment: Continuous)

2.5.b.iii. Falsification tests

To validate the identification strategy and confirm that the estimated effects of the 2016 minimum wage hike are not driven by spurious correlations, we conduct falsification tests. Specifically, we apply the same empirical specification used in the baseline analysis to the upper part of the wage distribution—namely, the last five wage bins, where the minimum wage should have no binding influence. If the bunching framework correctly identifies the treatment effect, we expect no significant employment changes in these unaffected wage bins.

All treatment coefficients in the falsification regression using the binary treatment variable are statistically insignificant (Table 2.7). The signs vary in direction across groups and are relatively small in magnitude, indicating that there are no systematic employment changes in the upper wage bins. This supports the view that the employment effects detected in the baseline are indeed localized around the minimum wage threshold, rather than artifacts of time trends or unobserved shocks.

The falsification test using the continuous version of the treatment variable confirms the same pattern: none of the treatment effects in the upper tail of the wage distribution are statistically significant (Table 2.8). This further reinforces the validity of the baseline estimates and suggests that the disemployment effects found earlier are not due to random variation in employment across the wage spectrum.

	Total	Formal	Informal	Low-educated	Female	Young
post	-2,989**	-2,238**	-1,368*	-2,120**	-594.2	-740.5
	(1,196)	(1,001)	(807.5)	(1,043)	(976.1)	(722.0)
impact	-152.7	226.1	65.51	-212.4	-360.1	-988.2**
	(481.8)	(466.4)	(282.3)	(316.6)	(719.6)	(469.6)
treatment	-23.49	12.43	112.1	-99.74	-967.2	423.3
	(422.5)	(456.7)	(221.8)	(315.7)	(606.9)	(385.6)
lgdp	16,955***	12,725***	6,854**	11,326**	9,472*	3,380
	(5,645)	(4,665)	(3,296)	(4,668)	(5,452)	(2,184)
female	-764.4	-1,966*	-62.42	1,005	-	227.2
	(1,164)	(1,008)	(274.2)	(1,290)	-	(288.9)
married	-78.46	2.275	-216.6	332.0	-211.9	221.0
	(749.7)	(680.3)	(188.8)	(678.6)	(286.6)	(204.4)
youth	-1,022	-926.6	-140.3	-1,625	-211.6	-
	(1,506)	(1,232)	(212.3)	(1,431)	(632.0)	-
loweduc	412.2	142.8	-40.85	-	-49.63	-377.6
	(648.7)	(594.0)	(154.2)	-	(573.5)	(263.0)
informal	-8,382**	-	-	-4,432**	-2,932**	-1,423**
	(3,319)	-	-	(1,983)	(1,178)	(656.6)
public	-1,191	-758.9	2,661	79.72	-449.2	-509.8**
	(1,273)	(1,066)	(2,754)	(738.4)	(657.1)	(237.9)
region	773.2	1,903***	176.4	-579.7	449.9	334.3
	(659.8)	(649.9)	(214.3)	(473.2)	(352.1)	(205.4)
working_hour	40.83	43.63	-2.540	1.545	12.71	7.164
	(32.98)	(30.71)	(5.892)	(21.86)	(21.29)	(13.03)
Constant	-85,912***	-65,449***	-33,738**	-56,803**	-48,214*	-16,950
	(28,606)	(23,780)	(16,354)	(23,505)	(27,747)	(10,843)
Observations	6,533	6,370	3,254	4,963	3,952	3,587
R-squared	0.818	0.816	0.916	0.787	0.839	0.752

Robust standard errors in parentheses

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

Table 2.7. Falsification Tests by Subgroups (Treatment: Binary)

	Total	Formal	Informal	Low-educated	Female	Young
post	1,000***	733.5**	63.83	757.0**	792.4***	314.9*
	(314.0)	(281.0)	(148.6)	(291.3)	(290.4)	(158.2)
impact	8.059	18.27	58.79	3.882	431.0***	-50.10
	(21.86)	(19.99)	(47.49)	(15.48)	(137.4)	(41.98)
treatment	-15.27	-7.534	-5.808	-16.35	-30.33	9.592
	(21.05)	(19.74)	(8.909)	(12.71)	(19.65)	(14.56)
lgdp	14,217***	10,015***	5,818**	9,363**	8,171*	2,766
	(4,650)	(3,671)	(2,896)	(4,211)	(4,505)	(1,872)
female	-781.6	-1,984*	-62.19	1,015	-	249.0
	(1,161)	(1,005)	(271.2)	(1,298)	-	(290.5)
married	-92.12	-19.53	-237.2	296.2	-225.1	237.0
	(751.4)	(681.7)	(186.0)	(674.7)	(297.9)	(205.5)
youth	-1,082	-972.2	-143.3	-1,670	-298.0	-
	(1,503)	(1,220)	(211.8)	(1,425)	(632.4)	-
loweduc	403.6	134.5	-31.08	-	-193.6	-337.3
	(650.5)	(594.9)	(155.1)	-	(585.5)	(256.7)
informal	-8,471**	-	-	-4,463**	-3,001**	-1,442**
	(3,334)	-	-	(1,989)	(1,190)	(654.6)
public	-1,162	-749.3	2,710	45.79	-435.4	-484.6**
	(1,269)	(1,060)	(2,755)	(736.5)	(638.6)	(225.2)
region	780.0	1,901***	179.9	-582.9	435.5	328.0
	(664.3)	(653.1)	(211.5)	(474.8)	(359.8)	(206.5)
working_hour	39.91	41.93	-1.979	2.067	19.16	6.864
	(33.41)	(30.80)	(5.917)	(21.93)	(21.96)	(12.89)
Constant	1.385.000***	1.061.000**	457,085*	885,336**	451,941	482,757
	(486,816)	(420,471)	(256,621)	(434,820)	(366,364)	(354,031)
Observations	6,533	6,370	3,237	4,948	3,913	3,574
R-squared	0.818	0.816	0.915	0.787	0.839	0.752

Robust standard errors in parentheses

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

Table 2.8. Falsification Tests by Subgroups (Treatment: Continuous)

In sum, the falsification tests provide strong support for the internal validity of the bunching estimation strategy. The absence of significant effects in wage bins far above the minimum wage threshold confirms that the estimated job losses in the baseline analysis are truly concentrated where the policy is binding.

2.5.b.iv. Short-term results

To assess how quickly the labor market responded to the 2016 minimum wage reform, we estimate short-term effects using data from the immediate aftermath of the policy. These results help distinguish initial disemployment or adjustment effects from longer-run labor market responses.

For the binary version, in the short-term, employment losses are concentrated among formal workers, with a statistically significant treatment effect. All other coefficients are negative but not statistically significant, suggesting that immediate disemployment was relatively contained. These results indicate that formal sector firms may have responded quickly to the cost shock, while other groups adjusted more gradually or were initially shielded from the effects.

Using the continuous "fraction at" exposure measure reveals statistically significant short-term disemployment effects for the total sample, formal workers, low-educated, and youth, all at the 5 percent significance level. These results confirm that employment dropped immediately after the policy, particularly among groups with high exposure to the minimum wage. Effects for informal and female workers remain negative but statistically insignificant, indicating slower or muted adjustment in these segments.

In sum, the initial disemployment effects were concentrated among formal, low-educated, and young workers—the groups most likely to be directly affected by a sharp increase in labor costs. These findings complement the baseline and robustness results and highlight that the bulk of the policy's effects were front-loaded and most visible immediately after implementation.

	Total	Formal	Informal	Low-educated	Female	Young
post	-1,445**	-1,206*	133.8	-294.9	-141.8	869.8
	(703.7)	(690.8)	(385.2)	(1,076)	(893.0)	(1,071)
impact	336.8	287.4	-1,047	-119.1	-852.2	-601.5
	(741.2)	(713.5)	(649.7)	(297.9)	(1,527)	(1,013)
treatment	-4,669	-6,357*	-196.8	-4,625	-1,048	-1,465
	(2,945)	(3,350)	(630.7)	(2,978)	(1,492)	(975.6)
lgdp	4,182	4,945	-626.2	-1,728	8,073	215.4
	(6,508)	(7,120)	(2,559)	(5,059)	(8,661)	(4,683)
female	-423.3	-496.7	-280.6	3,171	-	419.0
	(1,873)	(1,602)	(512.9)	(2,455)	-	(700.1)
married	2,015	1,063	172.6	239.8	-1,011	684.7
	(1,769)	(1,583)	(445.8)	(1,505)	(633.1)	(579.1)
youth	-2,023	-1,348	-88.53	-3,097	-64.50	-
	(2,356)	(2,127)	(533.6)	(2,502)	(1,111)	-
loweduc	519.7	349.3	-242.0	-	459.4	-407.1
	(1,482)	(1,403)	(360.7)	-	(1,149)	(631.2)
informal	-7,721*	-	-	-4,690*	-2,949	-1,618*
	(4,018)	-	-	(2,476)	(2,145)	(928.6)
public	-2,822*	-2,609*	4,060	503.0	-1,528	-195.4
	(1,597)	(1,422)	(4,283)	(1,001)	(1,193)	(456.7)
region	1,019	940.4	500.7	-93.93	-1.842	338.7
	(966.8)	(1,080)	(361.7)	(826.0)	(905.6)	(553.4)
working_hour	36.89	33.25	-1.076	40.01	20.98	22.76
	(77.45)	(73.02)	(11.01)	(47.18)	(35.45)	(23.69)
Constant	-21,194	-24,118	3,698	6,658	-40,244	-3,415
	(31,158)	(34,255)	(12,607)	(24,337)	(42,944)	(23,042)
Observations	3,317	3,247	1,599	2,535	1,984	1,855
R-squared	0.822	0.813	0.949	0.792	0.848	0.779

Robust standard errors in parentheses

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

Table 2.9. Short-term Estimation Results by Subgroups (Treatment: Binary)

	Total	Formal	Informal	Low-educated	Female	Young
post	-1,503**	-1,300*	152.8	-227.9	-131.1	742.4
	(706.3)	(677.9)	(382.2)	(1,069)	(917.6)	(994.5)
impact	18.22	13.91	-211.0	-3.330	564.1***	-13.61
	(32.30)	(28.21)	(145.5)	(13.69)	(176.9)	(90.57)
treatment	-283.7**	-299.8**	-21.08	-171.5**	-41.25	-92.77**
	(114.9)	(123.5)	(21.09)	(79.29)	(43.47)	(36.91)
lgdp	4,786	5,883	-645.7	-2,087	8,121	272.9
	(6,508)	(7,082)	(2,423)	(4,998)	(8,832)	(4,615)
female	-313.7	-363.2	-267.1	3,036	-	453.3
	(1,842)	(1,593)	(525.2)	(2,399)	-	(709.9)
married	2,061	1,155	177.0	177.1	-1,014	-
	(1,750)	(1,573)	(446.3)	(1,489)	(631.1)	-
youth	-1,767	-1,051	-96.21	-3,072	-79.86	731.7
	(2,350)	(2,130)	(541.6)	(2,488)	(1,126)	(587.5)
loweduc	528.6	351.6	-237.2	-	431.6	-381.1
	(1,440)	(1,380)	(357.5)	-	(1,154)	(631.7)
informal	-7,202*	-	-	-4,493*	-2,851	-1,503*
	(3,945)	-	-	(2,510)	(2,142)	(889.5)
public	-2,693*	-2,549*	4,084	502.0	-1,521	-117.2
	(1,587)	(1,430)	(4,242)	(994.8)	(1,197)	(457.2)
region	1,020	899.1	503.0	-113.2	13.35	333.2
	(973.5)	(1,081)	(359.6)	(832.5)	(902.2)	(553.6)
working_hour	43.30	37.49	-0.871	41.98	23.53	25.92
	(76.96)	(72.84)	(10.98)	(46.63)	(36.06)	(24.67)
Constant	-24,767	-29,144	5,995	8,550	-49,765	-3,682
	(31,437)	(34,384)	(12,369)	(24,033)	(44,876)	(22,782)
Observations	3,317	3,247	1,594	2,530	1,968	1,848
R-squared	0.822	0.813	0.949	0.792	0.848	0.780

Robust standard errors in parentheses

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

Table 2.10. Short-term Estimation Results by Subgroups (Treatment: Continuous)

2.5.b.v. Long-term results

To understand the durability of the minimum wage policy's impact, we estimate long-term treatment effects by comparing the counterfactual data from 2014-2015 with the 2019 data. These models assess whether initial disemployment effects observed in 2016 persisted, intensified, or faded over time.

Results are again presented for both the binary treatment variable and the continuous exposure measure, focusing again on wage bins surrounding the minimum wage threshold, i.e. wage bins 3 to 6.

	Total	Formal	Informal	Low-educated	Female	Young
post	-4,590**	-3,691**	-2,084	-2,636	-2,599	-611.4
	(1,752)	(1,456)	(1,342)	(1,956)	(1,610)	(1,292)
impact	1,730	1,234	79.19	-1,340*	1,050	-522.6
	(1,665)	(1,608)	(685.5)	(733.1)	(1,195)	(1,054)
treatment	1,144	-776.2	391.1	-2,609	1,564	-1,395
	(3,179)	(2,801)	(1,063)	(3,490)	(2,377)	(1,524)
lgdp	23,346**	17,896**	10,652*	15,753**	17,476*	3,578
	(8,854)	(7,449)	(5,777)	(7,541)	(9,008)	(3,756)
female	-1,234	-2,144	-139.3	493.5	-	230.9
	(2,106)	(1,801)	(792.3)	(2,371)	-	(507.8)
married	654.6	731.6	43.65	-1,083	-1,081*	71.92
	(1,895)	(1,688)	(613.7)	(1,679)	(624.3)	(533.8)
youth	-1,409	107.8	-173.9	-2,772	90.04	-
	(2,649)	(2,473)	(662.3)	(2,879)	(1,205)	-
loweduc	-148.2	19.40	-347.1	-	-336.7	-776.8
	(1,490)	(1,486)	(467.4)	-	(1,318)	(757.0)
informal	1,436	-	-	-1,416	63.09	203.6
	(3,585)	-	-	(2,379)	(1,642)	(884.0)
public	-34.77	-428.9	3,656	1,707	-269.5	-10.84
	(2,395)	(1,956)	(4,488)	(1,717)	(1,172)	(746.7)
region	538.6	946.2	105.6	-1,726	-268.5	523.7
	(1,490)	(1,275)	(508.8)	(1,197)	(776.3)	(561.1)
working_hour	142.6*	94.45	-7.916	45.91	13.24	25.77
	(85.04)	(73.49)	(18.81)	(61.85)	(45.64)	(30.71)
Constant	-123,394***	-93,891**	-52,305*	-79,032**	-88,360*	-19,357
	(45,671)	(37,906)	(28,796)	(37,572)	(46,050)	(18,335)
Observations	3,280	3,215	1,593	2,495	1,974	1,807
R-squared	0.809	0.810	0.922	0.783	0.832	0.770

Robust standard errors in parentheses

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

Table 2.11. Long-term Estimation Results by Subgroups (Treatment: Binary)

Under the binary specification, all treatment effects are statistically insignificant, suggesting that employment levels in treated bins and high-impact groups stabilized in the long run (Table 2.11). Some coefficients reverse direction

relative to the short-term estimates (e.g., total, informal, female), hinting at possible labor market adjustments or partial recovery after the initial shock. However, the lack of statistical significance limits definitive conclusions.

	Total	Formal	Informal	Low-educated	Female	Young
post	-4,531**	-3,682**	-2,071	-2,707	-2,750*	-497.9
	(1,744)	(1,454)	(1,334)	(1,904)	(1,620)	(1,281)
impact	77.49	50.34	20.67	-66.68*	1,166***	-36.77
	(74.14)	(64.27)	(173.8)	(35.73)	(316.0)	(91.53)
treatment	-37.89	-63.33	24.77	8.570	53.29	-77.59
	(159.0)	(140.6)	(58.78)	(152.9)	(73.38)	(75.35)
lgdp	22,982**	17,797**	10,622*	16,024**	18,227**	3,138
	(8,800)	(7,400)	(5,753)	(7,344)	(9,024)	(3,714)
female	-1,184	-2,109	-139.8	531.8	-	243.3
	(2,113)	(1,812)	(792.2)	(2,335)	-	(506.5)
married	649.3	719.6	47.46	-1,093	-1,089*	90.65
	(1,894)	(1,688)	(610.5)	(1,679)	(625.0)	(541.3)
youth	-1,242	130.7	-178.0	-2,851	21.95	-
	(2,657)	(2,476)	(656.4)	(2,953)	(1,217)	-
loweduc	-148.6	6.063	-357.9	-	-340.8	-764.9
	(1,494)	(1,486)	(471.9)	-	(1,331)	(759.7)
informal	1,470	-	-	-1,413	22.43	226.8
	(3,569)	-	-	(2,410)	(1,660)	(880.0)
public	-48.95	-421.9	3,641	1,744	-193.1	-1.444
	(2,389)	(1,960)	(4,483)	(1,721)	(1,162)	(742.8)
region	527.4	936.9	105.9	-1,722	-208.6	519.0
	(1,481)	(1,273)	(520.3)	(1,195)	(777.2)	(557.5)
working_hour	143.4*	95.05	-8.148	42.62	19.41	27.14
	(84.88)	(73.11)	(18.79)	(61.26)	(45.20)	(31.48)
Constant	-122,642***	-94,074**	-52,358*	-79,172**	-111,308**	-16,493
	(45,803)	(38,075)	(28,261)	(36,409)	(50,066)	(17,948)
Observations	3,280	3,215	1,588	2,488	1,960	1,800
R-squared	0.809	0.810	0.922	0.783	0.833	0.769

Robust standard errors in parentheses

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

Table 2.12. Long-term Estimation Results by Subgroups (Treatment: Continuous)

Using the continuous exposure measure, treatment effects in all groups are small in magnitude and statistically insignificant (Table 2.12). The signs vary across subgroups, indicating that long-term effects are not systematically negative and that

the labor market may have absorbed or adjusted to the initial wage shock over time. These findings suggest a dissipation of short-term disemployment effects by 2019.

Both specifications show no statistically significant employment effects in the long-term, implying that the initial job losses observed in 2016 did not persist. This result aligns with theoretical expectations that short-term rigidity gives way to labor market adjustment, especially in formal sectors where adaptation to wage floors may occur through reorganization, productivity improvements, or wage smoothing over time.

The absence of persistent negative effects supports the interpretation that the 2016 minimum wage hike had a transitory impact on employment, rather than causing lasting damage to labor demand.

2.5.c. Summary of Empirical Findings

Taken together, the empirical results provide strong evidence that the 2016 minimum wage hike in Turkey led to localized employment losses concentrated near the minimum wage threshold, particularly among formally employed, low-educated, and young workers. These effects are consistently observed across both binary and continuous specifications of minimum wage exposure and are robust to alternative definitions of the treatment window.

The robustness checks, which apply the same methodology to a broader wage interval, confirm the direction and magnitude of the effects, albeit with slightly weaker statistical precision—reinforcing the interpretation that the strongest impact is concentrated close to the wage floor. The falsification tests, applied to higher-

wage bins unaffected by the policy, yield no statistically significant effects, lending credibility to the identification strategy.

When distinguishing between short-term and long-term effects, the analysis shows that the disemployment impacts were most pronounced immediately after the policy was implemented, especially in the formal sector. However, these effects dissipated in the subsequent years, with no statistically significant long-term impacts detected across any subgroup. This suggests that while the policy initially disrupted employment in affected sectors, the labor market partially adjusted over time, potentially through firm-level adaptations or worker reallocation.

Overall, the results underscore the importance of using distribution-sensitive methods like bunching estimation to detect localized labor market impacts, which may be obscured in conventional full-sample DiD approaches. They also highlight the heterogeneous vulnerability of certain groups—particularly the formal, low-skilled, and young—to sharp changes in statutory wage floors.

2.6. Discussion

The empirical results presented above indicate that the 2016 minimum wage hike in Turkey produced localized negative employment effects, particularly for formal, low-educated, and youth workers. These effects were concentrated in wage bins near the minimum wage, were strongest in the short-term, and dissipated over time, suggesting a transitory shock followed by partial labor market adjustment.

The negative employment effects are consistent with standard competitive labor market theory, which predicts that binding wage floors reduce demand for low-productivity workers (Stigler 1946). The stronger and statistically significant

effects among formal sector workers likely reflect greater compliance pressure, as these firms face stricter legal and administrative enforcement. In contrast, the informal sector showed weaker and statistically insignificant responses, in line with expectations from dual labor market theory, where employers may evade binding wage floors (Fields 1975; Gindling and Terrell 2009).

The concentration of employment losses among low-educated and young workers aligns with existing empirical work in both developing and developed countries, which finds that these groups are more vulnerable to minimum wage shocks (Neumark and Wascher 2007; Harasztosi and Lindner 2019; Dayıoğlu et al. 2022). These groups often occupy marginal positions in the labor market, with fewer skills and lower bargaining power, making them more susceptible to displacement.

The transitory nature of the disemployment effect—present in 2016 but largely absent in subsequent years—suggests that Turkish firms may have engaged in initial workforce reductions, followed by reallocation, productivity adjustments, or wage smoothing to accommodate the new cost structure. This is consistent with the findings of Engbom and Moser (2022) in Brazil and Harasztosi and Lindner (2019) in Hungary, who show that firms adjust over time via capital deepening or margin compression, rather than sustaining long-term employment losses.

While disemployment effects were clear among formal, youth, and low-educated groups, the results for informal and female employment were statistically insignificant, though directionally negative.

The lack of a significant increase in informal employment might appear to contradict the predictions of dual labor market theory, which posits that displaced

formal workers may shift into the informal economy. However, this finding is not necessarily inconsistent with the theory. Informal employment in Turkey is already structurally high, and the sector may lack the absorptive capacity to accommodate displaced workers—especially in urban and regulated settings. Moreover, increased enforcement efforts and administrative tightening around 2015–2016 may have suppressed informal hiring even in the face of formal sector job loss. This is consistent with findings from Öztek (2021) and Gürcihan-Yüncüler and Yüncüler (2016), who show that informal employment does not always rise following wage floor hikes.

Similarly, the absence of significant effects on female employment likely reflects Turkey's persistently low female labor force participation, which remains among the lowest in the OECD. A robust body of research (Tansel 2002; İlkkaracan 2012; Bossavie et al. 2019) points to multiple structural barriers: sociocultural norms, limited childcare availability, and occupational segregation, all of which restrict women's access to wage employment. Women's informality rate (25 percent in our sample) is higher than that of men (21 percent in our sample) too. Therefore, even a sizable minimum wage shock may have limited short-run impact on female employment at the wage bin level.

This study contributes to a growing literature that applies bunching estimation techniques to minimum wage analysis, following the methodology developed by Cengiz et al. (2019). Unlike conventional DiD approaches, which may dilute treatment effects by averaging over the entire distribution, bunching focuses on localized employment changes near the policy threshold, where behavioral responses are expected to be strongest.

Our findings are broadly consistent with existing literature in both emerging and advanced economies. In Brazil, Lemos (2009) and Engbom and Moser (2022) find that minimum wages compress the wage distribution without strong disemployment effects. In contrast, our results show that in a high-informality setting like Turkey, the effects are more heterogeneous and sensitive to sector and skill characteristics. The observed short-term disemployment among formal, low-skilled, and young workers highlights the need to account for labor market segmentation and enforcement capacity when designing and evaluating wage floor policies.

By focusing on wage bin–level data, this study provides more granular evidence than previous work in Turkey (e.g., Gürcihan-Yüncüler and Yüncüler 2016; Işık et al. 2020), which typically used aggregate DiD or sector-level variation. Furthermore, our use of both binary and continuous exposure metrics enhances robustness and helps quantify the intensity of treatment effects, which is often overlooked.

The results highlight the importance of targeted support mechanisms for vulnerable worker groups during minimum wage hikes. While raising the wage floor can reduce wage inequality, it may also produce short-run disruptions in employment for workers with limited skills or formal sector experience. Policymakers should consider transition support, wage subsidies, or training programs to mitigate these impacts.

Additionally, the lack of long-term disemployment effects suggests that well-anticipated and coordinated policy implementation can help firms and workers adjust. Ensuring predictability, transparency, and institutional enforcement can help

reduce uncertainty and promote compliance, especially in formal sectors. At the same time, complementary policies are needed to address female labor force participation and to ensure that minimum wage policy interacts meaningfully with broader gender and informality reforms.

2.7. Conclusion

This study evaluates the employment effects of the 2016 minimum wage hike in Turkey using a bunching estimation framework. Unlike traditional approaches that rely on regional or sectoral variation, our method leverages the localized discontinuity created by the statutory wage floor, focusing on wage bins directly affected by the policy. Drawing on HLFS microdata from 2014 to 2019, we document the short-term and longer-term impacts of the reform across worker groups, sectors, and wage segments.

The findings reveal that the policy led to statistically significant short-term disemployment effects, particularly among formally employed, low-educated, and young workers. These effects were concentrated near the minimum wage threshold and diminished in the following years, suggesting a pattern of initial adjustment followed by partial recovery. Importantly, the results were robust to alternative specifications, including different treatment window definitions and both binary and continuous measures of exposure. Falsification tests conducted in unaffected wage bins further support the validity of our identification strategy.

Contrary to the predictions of dual labor market theory, we find no evidence of compensatory increases in informal employment, suggesting limited substitutability between formal and informal labor at the margin. Similarly, the

policy had no statistically significant effect on female employment—likely due to persistent structural barriers to women's participation in the Turkish labor market.

Methodologically, this study contributes to literature by applying bunching estimation in a middle-income, high-informality setting, offering a more precise picture of how wage floors affect labor allocation across wage levels. By introducing a wage-bin-level analysis with group-specific exposure metrics, the study advances empirical tools used in minimum wage evaluation. Substantively, the results underscore that while minimum wages can serve redistributive goals, they also carry short-run costs for vulnerable workers, especially in rigid or segmented labor markets.

For policymakers, the findings suggest that large, sudden increases in minimum wages—especially in the absence of supporting measures—may lead to temporary job displacement, particularly among already marginalized groups.

Future research could extend this analysis to explore effects on job quality, hours worked, or firm-level responses, and to assess how minimum wage changes interact with macroeconomic shocks, such as inflation or migration flows.

CHAPTER III

DO EMPLOYMENT SUBSIDIES AFFECT SKILL INTENSITY AND CAPITAL FORMATION? EVIDENCE FROM TURKISH MANUFACTURING

3.1. Introduction

Active labor market policies (ALMPs) are designed to address structural labor market challenges such as rising inequality, technological disruption, and the changing nature of work. These policies include job guarantees, universal basic income, work-sharing arrangements, retraining and reskilling programs, employment subsidies, and cooperative business models. The overarching goal is to either complement or serve as alternatives to traditional mechanisms such as minimum wage laws, collective bargaining, and unemployment insurance. By promoting both labor market flexibility and security, ALMPs seek to enhance economic resilience, reduce inequality, and foster more inclusive labor markets that better serve the needs of both workers and employers.

Among the most widely adopted ALMPs are employment (or wage) subsidies, which provide financial incentives to employers with the goal of increasing employment, particularly among disadvantaged groups such as long-term unemployed individuals, youth, low-skilled workers, and marginalized populations. These subsidies can take various forms, including direct wage payments, tax credits, or reductions in employer-side social security contributions. They are often

implemented during periods of economic downturn or recession, when labor demand weakens and unemployment risks intensify.

The core rationale behind employment subsidies is to offset perceived or actual hiring risks—such as lower productivity or high training costs—by lowering the cost of employment. In doing so, these programs aim to reduce unemployment by encouraging firms to expand their workforce, promote labor market inclusion by supporting the employment of vulnerable populations, stimulate economic growth through increased household income and consumption, alleviate poverty and income inequality particularly for low-wage and low-productivity workers and encourage formalization, especially in countries with high levels of informal employment.

However, employment subsidies also face several limitations. A central concern is deadweight loss: many subsidized jobs may have been created even without the subsidy, thereby reducing the cost-effectiveness of the intervention. Additionally, substitution effects may arise if firms replace unsubsidized workers with subsidized ones, particularly when the subsidy is temporary. There is also the risk of dependency, whereby firms refrain from hiring unsubsidized workers once the incentive is withdrawn, potentially undermining the sustainability of job creation. Finally, employment subsidies often entail substantial fiscal costs, which can be especially burdensome if the policy is broad-based or extended over long durations.

Employment subsidies have long been a component of Turkey's ALMP toolkit, implemented in various forms. These programs have targeted a wide range of objectives, including first-time employment, specific economic sectors,

population subgroups such as women or youth, particular firm characteristics (e.g., small businesses or exporters), or specific geographic regions such as provinces or counties. Regardless of the specific target group, the primary design of these policies has consistently emphasized reducing the labor cost burden for employers, rather than providing direct income support to employees (Aşık et al. 2022).

This emphasis on cost reduction is particularly relevant in Turkey, where the combined burden of taxes and social security contributions on labor is 39 percent of gross labor cost, above the OECD average of 35 percent (OECD 2025). Consequently, from the perspective of firms, any meaningful reduction in labor costs through subsidies can affect a wide array of operational and strategic decisions, including hiring behavior, by increasing the demand for labor through lower marginal cost; production capacity, as increased hiring may support output expansion; pricing strategies, since labor cost reductions may translate into lower marginal costs; market access, particularly where enhanced competitiveness enables firms to expand into new domestic or export markets; human capital choices, affecting the skill composition of the workforce; Capital accumulation and technological upgrading, through shifts in factor substitution or investment incentives.

The expected outcomes for the last two dimensions—human capital formation and capital investment—are less clear-cut and may even be interrelated, both with important implications for firm-level productivity. On the human capital side, evidence suggests that employment subsidies, particularly those targeting low-skilled labor, may increase the share of low-skilled employment and potentially dampen incentives for skill acquisition (Oskamp and Snower 2006). However, from

a theoretical standpoint, it is equally plausible that firms could leverage the cost advantage created by the subsidy to hire higher-skilled workers at a reduced effective wage, thereby upgrading the skill composition of their workforce. As such, the impact of employment subsidies on workforce skills is ambiguous and may vary across sectors, firm types, and policy designs.

On the capital side, the effects of lower labor costs are similarly dual. Firms may respond by expanding their workforce as a substitute for capital, especially if labor becomes relatively cheaper in production. Alternatively, some firms might increase capital investment, either to complement a growing workforce and maintain the marginal productivity of labor, or by reinvesting the cost savings derived from the subsidy into technological upgrading or capacity expansion. Classical and neoclassical frameworks generally support the complementarity of production factors, predicting that labor subsidies could stimulate capital accumulation (Judd 1987; Shi and Wen 1999; Fuest and Huber 2000; Daveri and Tabellini 2000). In contrast, substitution effects are also possible. Petrucci and Phelps (2005), for example, argue that subsidies tend to disproportionately benefit the targeted factor, potentially crowding out investment in the untargeted one—suggesting a trade-off rather than complementarity in some settings.

In existing literature, studies evaluating the effectiveness of employment subsidies have primarily focused on their impact on the level of employment, with the bulk of this research concentrated in developed countries (Blundell et al. 2004; Deidda et al. 2015; Goos and Konings 2007; Huttunen et al. 2013). The findings are mixed. Some studies report no statistically significant effects, while others find positive employment impacts for specific groups, such as older women and youth,

or observe gains that are transitory in nature. Evidence from developing countries is relatively more limited but similarly heterogeneous: some studies report negligible or null effects overall, while others identify positive impacts on formal employment, particularly in high-informality contexts (Gruber 1997; Kugler and Kugler 2009; Cruces et al. 2010).[10]

Turkey has also been the subject of several empirical evaluations of employment subsidy programs. For example, Betcherman et al. (2010) document a positive effect on formal employment from two regional subsidy programs launched in 2004 and 2005, which targeted new hires in firms with more than 10 employees. The 2011 employment subsidy—targeted at young people and women—has been evaluated using the HLFS in studies by Ayhan (2013), Uysal (2013), and Balkan et al. (2016). While Balkan et al. (2016) find no measurable effect, the other two report positive outcomes, especially for women, though concerns remain about the duration and sustainability of these effects. More recently, Basbuga et al. (2022) assess the impacts of multiple subsidy schemes and find positive employment outcomes. The most comprehensive analysis to date is by Aşık et al. (2022), who evaluate a geographically targeted 2016 subsidy that reduced employer social security contributions in underdeveloped regions. Using firm-level administrative data from the Social Security Institution, they find a significant and persistent increase in registered employment, particularly in small firms. However, they argue that the effect primarily reflects formalization of pre-existing informal jobs, rather than expansion at the extensive margin.

[10] See Almeida et al. (2014) for a review for developing economies.

While these studies provide valuable evidence on employment outcomes, limiting policy evaluation solely to employment levels risks overlooking broader firm responses. In particular, the impact of employment subsidies on workforce skill composition and firm investment behavior has critical implications for productivity growth and long-term competitiveness. These dimensions are especially important in middle-income economies like Turkey, where productivity stagnation and low investment rates constrain inclusive development.

Against this background, the present study extends prior analyses by examining the broader firm-level effects of employment subsidies. Specifically, we investigate the causal impact of the 2016 regional subsidy expansion on capital accumulation and skill composition in the manufacturing sector, using high-quality matched employer–employee administrative data. In doing so, we provide a deeper understanding of how wage subsidies affect both labor and capital dynamics in developing economies. We restrict our analysis to manufacturing industry as technological level is only defined for manufacturing industry.

To this end, this study examines the effects of a geographically targeted employment subsidy introduced under Law No. 6846, which provided an additional 6-percentage-point reduction in employer-side social security contributions.[11] This subsidy was applied on top of the existing, nationwide 5-percentage-point reduction under Law No. 5510 and was specifically designed to encourage registered employment in underdeveloped regions of Turkey.

Turkey's regional development framework classifies provinces into six development regions based on socioeconomic indicators. Region 1 represents the

[11] This is the same subsidy program Aşık et al. (2022) investigates.

most developed areas, while Region 6 includes the least developed. Regions 1 through 3 are above the national average in terms of development, whereas Regions 4, 5, and 6 are officially designated as underdeveloped and are thus eligible for targeted policy interventions. The employment subsidy analyzed in this study specifically targets firms operating in these lower-tier regions, with the aim of promoting formal employment and stimulating regional economic activity.

The additional 6-percentage-point reduction in employer social security contributions was first introduced in 2013 under Law No. 6486, as an added incentive to support firms operating in underdeveloped areas—specifically, 51 provinces and 2 districts classified under Regions 4, 5, and 6. This policy effectively brought the total reduction in labor taxes to 11 percentage points when combined with the universal 5-point discount under Law No. 5510, representing a substantial cost saving for eligible employers.

A key institutional feature of the 2013 subsidy, which is central to this study's identification strategy, was its eligibility restriction: initially, the subsidy applied only to firms with 10 or more employees, and even then, only to new hires beyond a firm's existing workforce. This changed markedly in 2016, when the subsidy's scope was significantly broadened. From that year onward, all firms operating in Regions 4, 5, and 6 became eligible, regardless of their employment size, and the subsidy was extended to all employees, not just additional hires. This reform marked a turning point for micro firms—those with fewer than 10 employees—which had previously been excluded from the program and now became eligible for the first time.

A second major reform occurred in August 2020, when the spatial unit used to define subsidy eligibility shifted from the province level to the county level. Under this new classification, several counties—previously assigned to Regions 1, 2, or 3 due to their provincial affiliation—were reclassified into Region 4 based on their own local development indicators. As a result, 99 counties that had been ineligible during earlier periods became subsidy-eligible after 2020 (see Table A1). This change not only expanded the program's reach but also created a natural comparison group for empirical analysis, as these counties remained untreated during the earlier subsidy expansion analyzed in this study.

We leverage this second legislative change—the 2020 reclassification of subsidy eligibility based on counties rather than provinces—to construct a refined control group for identifying the causal effects of employment subsidies on firms in treated areas. Specifically, we focus primarily on micro firms (with 1 to 9 employees), as they were explicitly excluded from the subsidy program prior to the 2016 expansion. These firms thus constitute a more refined treatment group once they became eligible under the new policy.

While the primary analysis centers on micro firms, we also account for a subset of small firms (10 to 49 employees), some of which may have been marginally exposed to the pre-2016 subsidy depending on their employment size trajectory. Firms operating near the 10-employee eligibility threshold prior to 2016 may have intermittently been qualified under the earlier program design.

We implement a difference-in-differences (DiD) framework to estimate the causal impact of the 2016 subsidy expansion, exploiting variation in eligibility criteria over time. Similar to Aşık et al. (2022), we treat the 2016 reform as the

policy intervention and identify the treatment group as firms located in Region 4 counties, which became newly eligible for the subsidy regardless of firm size or employment growth.

To construct a valid control group, we draw on the 99 counties that were reclassified as subsidy-eligible only after 2020—despite having socioeconomic development levels comparable to Region 4—but were not treated during the sample period of 2006 to 2019 due to their provincial affiliation with more developed regions (i.e. Regions 1–3). This change in policy design—shifting the eligibility unit from provinces to counties in 2020—created a quasi-natural experiment, enabling us to compare treated firms with otherwise similar untreated firms that remained outside the program until after the study period.

By restricting our analysis to 2006–2019, we ensure that control units are not contaminated by later eligibility. The validity of this identification strategy rests on the parallel trends assumption: that firms in treated Region 4 counties and in later-treated counties would have followed similar employment and investment trajectories in the absence of the 2016 reform. As we demonstrate in our event study results, pre-trends are largely parallel, lending credibility to this approach. We therefore estimate the causal effect of the subsidy as the difference in the change of key outcomes—such as skill-based employment and capital investment—before and after 2016, between treated and control firms in the manufacturing sector. The parallel pre-trends assumption for treated and control groups in Regions 5 and 6 are not validated.

This study contributes to the literature in several important ways. First, it adds to the relatively limited body of research on the effects of employment subsidies in

Turkey and other developing economies, where program evaluations remain scarce despite widespread policy use. Second, it is among the few studies to examine the relationship between employment subsidies, capital formation and the skill composition of employment. While similar questions have been explored in the context of labor supply shocks—such as migration-induced wage declines (e.g. Clemens et al. 2018)—evidence directly linking subsidies to workforce upgrading or downgrading remains limited. Third, whereas most existing research on capital accumulation focuses on the effects of capital subsidies, this study is, to the best of our knowledge, the first to assess the impact of employment subsidies on new capital formation. In doing so, it offers novel insights into how wage cost reductions influence firm-level investment behavior and factor substitution decisions in a middle-income, high-informality context.

Findings indicate that the 2016 expansion of Turkey's regional employment subsidy had its strongest effects on micro and small manufacturing firms, which had previously been ineligible. Micro firms experienced statistically significant employment growth, while small firms responded with increases in capital investment and capital intensity, particularly in tangible assets. These effects were most pronounced among firms near the 10-employee threshold targeted by the policy. In contrast, medium and large firms, despite benefiting most in monetary terms, exhibited no measurable response in employment or capital outcomes. Additionally, the subsidy had no significant effect on the skill composition of the workforce, suggesting limited structural upgrading. Overall, the reform functioned effectively as a labor-cost intervention that stimulated employment and capital

accumulation among smaller firms but did not shift broader production or workforce dynamics.

The remainder of this chapter is structured as follows: Section 3.2 introduces the Turkish labor market and employment subsidy programs. Section 3.3 describes the dataset. Section 3.4 presents the empirical methodology. Section 3.5 discusses the main results, followed by a detailed interpretation in Section 3.6. Section 3.7 concludes.

3.2. Labor Market and Subsidy Schemes in Turkey

The Turkish labor market has undergone substantial transformation in recent decades, shaped by structural challenges, demographic shifts, regional disparities, and evolving economic policies. Despite these changes, several persistent characteristics remain: a relatively low labor force participation rate, particularly for women; a dual labor market structure with high levels of informality; elevated unemployment rates, especially among youth; a compressed wage distribution near the minimum wage; and pronounced regional inequalities in labor market outcomes.

Structural shifts in the economy have moved employment from agriculture toward industry and services. According to the latest figures from TURKSTAT, the service sector accounts for approximately 58 percent of total employment, followed by industry (20%), agriculture (15%), and construction (5%).

Youth unemployment remains persistently high, frequently exceeding 15 percent, while women face disproportionate barriers to formal employment, including limited access to childcare and rigid workplace structures (Yeldan 2017). Regional disparities further compound labor market challenges: unemployment is

significantly higher in eastern and southeastern provinces compared to the more industrialized western regions (Betcherman et al. 2010). Informal employment remains widespread, affecting approximately 25 percent of the labor force, particularly in agriculture, construction, and small-scale enterprises. Informality undermines job security and access to social protection systems. Additionally, Turkey maintains a relatively high minimum wage in relation to GDP per capita, which, while supporting low-income households, creates challenges for small and medium-sized enterprises (SMEs).

The Turkish government has introduced a series of ALMPs aimed at promoting job creation, reducing informality, and improving labor market flexibility. These include:

Vocational Training and Skill Development: Programs such as Specialized Vocational Skills Training Centers and On-the-Job Training aim to enhance employability by aligning worker skills with labor market demand. These initiatives frequently target women, youth, and other disadvantaged groups.

Employment Subsidies and Incentives: Legal instruments like Laws 5510 and 6111 provide employers with social security premium reductions and tax incentives to stimulate hiring and formalization. These subsidies are often demographic or region specific.

Entrepreneurship Support: Government-funded training and grant programs encourage self-employment and microenterprise creation, particularly among women.

Public Works and Social Benefit Programs: Initiatives like the “Working for the Benefit of Society” program create short-term employment opportunities,

primarily for low-skilled or unemployed individuals, while delivering community services.

Targeted Youth and Women Programs: Internships, mentoring schemes, and employment readiness programs assist young people in transitioning from education to work. Women-specific training and employment support are designed to reduce gender disparities.

Turkey's high labor tax burden makes it a particularly relevant case for evaluating employment subsidies. Approximately 39 percent of the gross wage is allocated to taxes and social security contributions, significantly above the OECD average of 35 percent (OECD, 2025). Employers bear a substantial portion of this burden, contributing between 21.5 percent and 27 percent of gross wages to cover disability, old age, death, unemployment, and health insurance premiums. The combination of high labor costs and widespread informality provides both motivation and the opportunity for policy intervention.

In response, the Turkish government has launched more than a dozen employment subsidy programs, many of which aim to offset the cost of formal job creation by reducing employer-side social security obligations. The cornerstone of these efforts is the 5-Point Reduction introduced under Law No. 5510, which reduces employers' disability, old age, and death insurance premiums from 11 percent to 6 pereent. Firms meeting basic compliance criteria qualify automatically for this subsidy.

Employers may also combine compatible subsidy programs under certain conditions. One of the most significant additions to this policy architecture was the Additional 6-Point Subsidy, introduced in 2013 under Law No. 6486. This initiative

specifically targeted underdeveloped regions—51 provinces and 2 districts classified as Regions 4, 5, and 6—by offering a further 6-percentage point reduction in employer-side social security premiums. When combined with the existing 5-point subsidy, this effectively halved employers' social security contributions, reducing total labor costs by approximately 6 percent for minimum-wage earners.

Eligibility criteria for the 6-point subsidy were initially more stringent than for the 5-point program. Firms were required to operate in eligible regions, maintain timely social security payments, and ensure full compliance across all business branches. Initially, only firms with at least 10 employees could qualify, and the subsidy applied only to additional hires, not to existing staff.

A major reform in 2016 expanded the 6-point subsidy by eliminating both the firm size and new-hire restrictions, making the program available to all firms and all employees in eligible regions. This policy expansion significantly increased subsidy coverage, particularly among micro firms, which had previously been excluded.

In 2020, the targeting framework was further refined when the unit of eligibility shifted from the province to the county level. This change allowed previously ineligible counties—those located within relatively developed provinces but suffering from local underdevelopment—to become subsidy-eligible. A total of 99 counties were reclassified under Region 4, expanding the reach of the program and improving its geographic precision (Table A1). This 2020 change would later serve as a quasi-experimental setting for the empirical strategy in this study.

Overall, the Additional 6-Point Subsidy represents a substantial intervention aimed at both stimulating employment and encouraging formalization in Turkey's labor market. By lowering labor costs, the policy incentivizes especially smaller

firms to transition toward formal employment relationships, thereby improving worker protection and expanding the social insurance base. Moreover, the transition to county-level targeting reflects a more nuanced approach to regional development policy—ensuring that subsidies reach the localities most in need.

In sum, Turkey's labor market is shaped by structural challenges—including high informality, low female labor force participation, skill mismatches, and significant regional disparities—alongside a heavy labor tax burden that discourages formal employment. In response, the government has implemented a range of ALMPs, most notably employment subsidies aimed at reducing employer-side labor costs. The core programs—such as the 5-point universal subsidy and the regionally targeted 6-point reduction—have evolved over time to broaden eligibility and enhance their effectiveness. Particularly, the 2016 expansion and the 2020 county-level reclassification represent key moments of policy change that allow for causal identification of their impact. Given these features, Turkey offers a valuable setting to study not only the employment effects of wage subsidies but also their broader implications for skill composition, capital formation, and formalization at the firm level.

3.3. Data

This study utilizes rich administrative microdata obtained from the Entrepreneurship Information System (EIS) of the Ministry of Industry and Technology. EIS is a large-scale data integration platform that consolidates firm-level information from multiple public institutions in Turkey. Developed to support data-driven policy design and economic research, the EIS provides a unified and

standardized view of firm behavior, enabling comprehensive analyses of employment, production, investment, and trade dynamics at both regional and national levels.

EIS integrates information from several key administrative sources:

- **Revenue Administration (GİB):** Financial declarations, including balance sheets and income statements,
- **Social Security Institution (SGK):** Employment-related data, such as firm registrations, insured employees, and demographic characteristics,
- **Ministry of Trade:** Foreign trade records,
- **Small and Medium Enterprises Development Organization (KOSGEB):** Data on SME loans and support programs,
- **Turkish Patent and Trademark Office:** R&D support records, intellectual property applications, and patent data,
- **Ministry of Industry and Technology:** Annual operating statement data on actual production quantities.

The database covers all enterprises subject to corporate and income taxes that generate income from commercial or industrial activity and employ workers registered with SGK. It excludes non-commercial entities (e.g. residential associations), freelance professionals (e.g. lawyers, architects), banks, and joint ventures governed under separate reporting rules.

EIS currently includes over 3 million registered firms outside the finance and defense sectors. The data is available at province and county levels, enabling fine-grained geographic analysis. Most variables are consistently available from 2006

onward, allowing for longitudinal analysis of firm outcomes. The firms can be classified across regions, counties and subsidy regions, sectors, firm-size, and technology level.

Firms are categorized by size, sector, region, and technology level. Size classes are based on official Turkish definitions, combining employee count and annual turnover. As for the employee counts, the classification is the following:

- Micro: 0–9 employees
- Small: 10–49 employees
- Medium: 50–249 employees
- Large: 250+ employees

The analysis is restricted to firms in the manufacturing sector, which is a major focus of industrial policy and subsidy programs. Manufacturing firms are further classified into four technology levels, based on international standards: low technology, medium-low, medium-high, and high technology.

EIS provides firm-level employment data disaggregated by age, gender, occupation, and wage level. While individual education levels are not recorded, the dataset includes International Standard Classification of Occupations (ISCO) codes for each job title. To proxy skill level, we map ISCO codes to the ESCO framework, which links occupations to required competencies. This allows us to differentiate between high-skill and low-skill employment segments in our analysis.

To ensure cross-agency consistency, EIS adopts standardized classification systems:

- NACE Rev. 2 for economic activities,
- PRODTR 2010 for industrial products,

- GTIP, BEC, and SITC for trade classifications.

Enterprise records are linked across data sources using tax identification numbers, ensuring high-quality matching and longitudinal tracking. Financial indicators at the enterprise level are distributed across firm locations based on the number of workers insured per branch, which allows for sectoral and regional disaggregation. Purchase and sales data are aggregated from mandatory VAT forms, enabling detailed analysis of firm revenue dynamics.

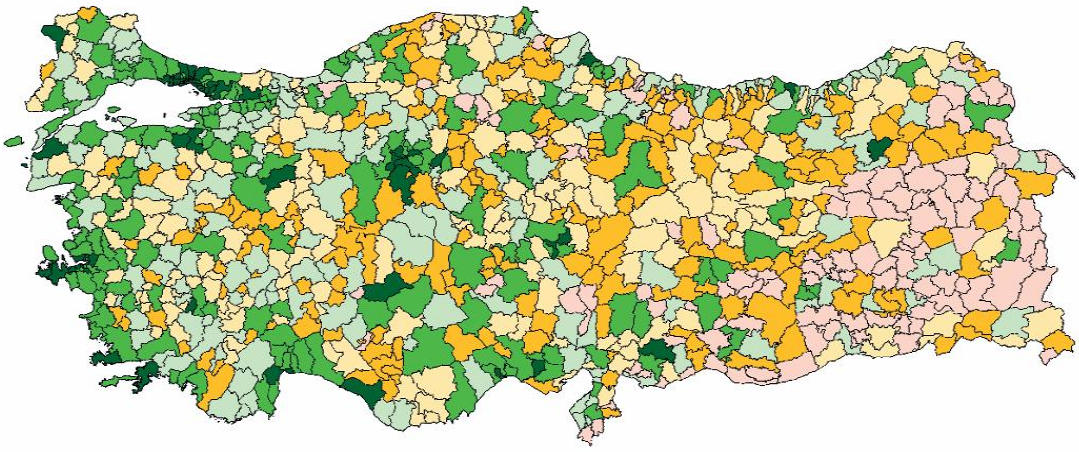
In sum, EIS administrative dataset provides a uniquely rich source of firm-level information, enabling a detailed examination of employment structures, investment behavior, and production technology within Turkey's manufacturing sector. By integrating financial, employment, trade, and innovation data from multiple public institutions, and offering consistent coverage over time and space, EIS facilitates robust causal analysis of policy impacts. Its granularity—especially in firm size, location, and occupation structure—makes it particularly well-suited to evaluating the effects of employment subsidies on both labor market and firm-level outcomes.

3.4. Methodology

This study estimates the causal effect of a 2016 policy reform in Turkey that expanded eligibility for an additional 6-percentage-point reduction in employer-side social security contributions to include micro firms. The reform created a quasi-natural experiment, which we exploit using a DiD framework, comparing changes in outcomes for treated and untreated firms before and after the reform. We utilize the same identification strategy as in Aşık et al. (2022)

The Turkish government classifies all provinces and counties into six development regions based on a Socio-economic Development Index (SEDI), prepared by General Directorate of Development Agencies (GDDA) of Ministry of Industry and Technology. Region 1 is the most developed and Region 6 is the least developed region. SEDI score computation is based on a principal component analysis and takes into account high number of variables (56 as of 2022) in the dimensions of demography, employment and social security, education, health, finance, competitiveness, innovation and quality of life (GDDA 2023). The districts are classified in six development levels, taking into account rankings and the natural breaks of the index scores of the districts. According to the results of 2022, there are 67 districts in Region 1, 173 in Region 2, 175 in Region 3, 215 in Region 4, 222 in Region 5 and 121 in Region 6 (Map 3.1).

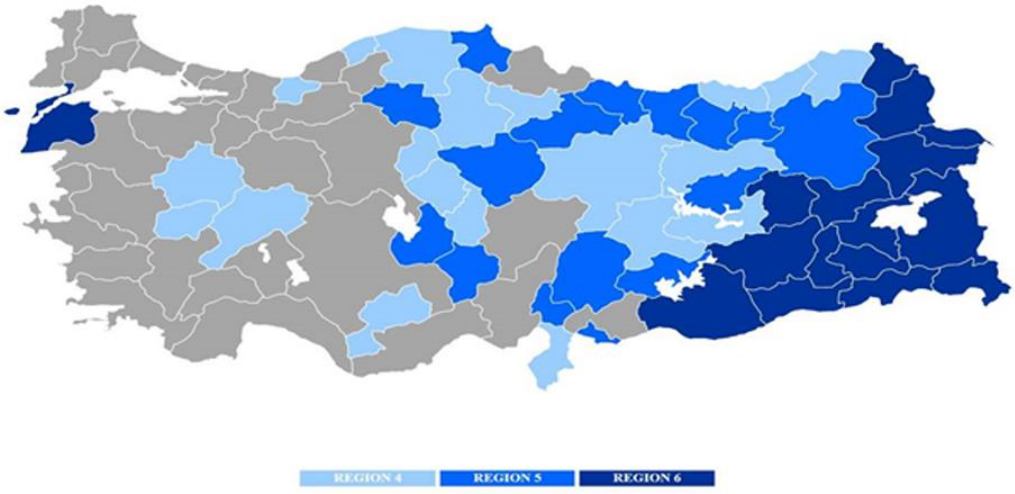
Map 3.1. Socio-economic Development Levels by Counties in Turkey



Source: General Directorate of Development Agencies (GDDA) (2023)

The additional 6-point subsidy was targeted to Regions 4, 5, and 6, with eligibility initially defined at the provincial level (Map 3.2). In 2016, a policy change expanded the subsidy to include all firms in eligible provinces, regardless of size or hiring behavior. This expansion marked the first time micro firms—previously excluded—became eligible for the subsidy.

Map 3.2. Provinces Eligible for the 6 Additional Points Reduction in 2016



Note: For the west, most province in Region 6, Canakkale, the only eligible districts are two islands, Gokceada and Bozcaada.

Source: Aşık et al. (2022)

However, in August 2020, a subsequent reform redefined regional subsidy eligibility at the county level, recognizing that counties within the same province may differ significantly in their economic development. As a result of this reclassification, 99 counties, previously included in Regions 1–3 due to their provincial affiliation, were reclassified as Region 4 based on local economic indicators (Table A1). While these counties became eligible for the 6-point subsidy after 2020, they were ineligible during our sample period of 2006–2019.

This temporal and spatial variation in policy rollout provides a credible source of exogenous variation in subsidy exposure. Specifically, the newly eligible counties—though similar in socioeconomic development to the earlier treated counties—were not eligible for the 6-point subsidy during our study window (2006–2019), making them well-suited as a control group. In contrast, the treatment group consists of firms located in provinces classified as Regions 4, 5, or 6, which began receiving the subsidy following the 2016 reform.

To improve the comparability of the treatment and control groups, we implement additional sample refinements. First, we exclude seven counties[12] from the control group that were reclassified from Region 4 to Region 3 based on the most recent Socio-Economic Development Index (SEDI 2022). These counties no longer meet the underdevelopment criteria applied during the study period. Second, we exclude from the treatment group 136 counties classified as Regions 1–3 in terms of development score, but which were included in the subsidy program solely because they belonged to a Region 4–6 province. This adjustment mitigates the risk of misclassifying economically stronger counties as treated and enhances the internal validity of our comparison.

By refining both the treatment and control samples in this way, we reduce heterogeneity in baseline development levels, ensuring that our estimates capture the effect of subsidy eligibility rather than underlying differences in regional economic conditions.[13]

[12] These counties (affiliated province) are Kulu (Konya), Havza (Samsun), Yeniçağa (Bolu), Ferizli (Sakarya), İncirliova (Aydın), Nizip (Gaziantep), Gökçebey (Zonguldak).
[13] After the adjustments, for Region 4, the total number of counties in the treatment group is 112 and the total number of counties in the control group is 86.

In order to show that control group is similar to the treated regions in terms of development level, we also run a simple regression to compare the mean of each group. We find the difference between the mean of SEDI of control and treatment groups are statistically insignificant, while that of between the control and non-treatment group is statistically significant (Table 3.1).

	Treated vs. Control				**Non-treated vs. Control**			
	Mean (Treated)	Mean (Control)	Mean difference	p-value	Mean (Not-treated)	Mean (Control)	Mean difference	p-value
2004	-0.26	-0.24	-0.02	0.74	1.37	-0.24	1.61***	0.000
2017	-0.34	-0.31	-0.03	0.50	0.76	-0.31	1.07***	0.000
2022	-0.40	-0.45	0.05	0.31	0.71	-0.45	1.16***	0.000

Notes: *** p<0.01, ** p<0.05, * p<0.1

Table 3.1. Statistical Significance of Mean Differences between Groups

As an additional, though conceptually weaker, approach to defining treatment and control groups, we consider an alternative comparison limited to Region 5 counties only. In this specification, the treated group consists of Region 5 counties that were part of provinces classified as Region 5, while the control group includes Region 5 counties that remained ineligible for the 6-point subsidy during the study period because they were administratively located within Region 1–3 provinces.

While this approach helps further isolate the role of county-level characteristics by holding the development classification constant (Region 5), it is less ideal from an identification standpoint. Specifically, the comparison may not satisfy the parallel trends assumption, as the administrative and economic environments of these counties may still differ systematically based on their provincial affiliations. Nevertheless, we include this specification as a supplementary robustness check, presenting estimation results that focus

exclusively on Region 5 counties, to assess whether the effects observed in the main analysis persist within a more narrowly defined subset of regions.

It is important to note that our DiD estimates should be interpreted as Intention-To-Treat (ITT) effects. EIS dataset does not contain information on whether individual firms actually received the subsidy, but only indicates their location, which determines eligibility for the program. As such, our estimates capture the average effect of being exposed to treatment, rather than the effect of actual take-up.

This distinction is common in program evaluation settings where compliance is unobservable or imperfect, but treatment eligibility is exogenous and well-defined. As Bloom (2008) notes, ITT analysis is not only statistically valid under such conditions, but also policy-relevant, since policymakers often control eligibility and access, but not firm-level participation decisions. In this sense, our estimates reflect the real-world effect of the policy's expansion on eligible firms, regardless of take-up, and are therefore more generalizable for evaluating subsidy design and coverage.

We acknowledge potential challenges to the validity of our identification strategy. The core assumption underlying the DiD framework is that, in the absence of the policy reform, the treatment and control groups would have followed parallel trends in the outcomes of interest. If this assumption does not hold, estimated treatment effects may be biased due to differential pre-trends or unobserved shocks.

To assess the plausibility of the parallel trends assumption, we implement an event study specification, which allows us to visually and statistically examine pre-treatment dynamics. These results, presented in the next section, provide supporting

evidence that treatment and control groups were evolving similarly prior to the 2016 subsidy expansion—thereby reinforcing the credibility of our DiD estimates.

Against this background, our core identification strategy compares changes in key firm-level outcomes—including employment (L), capital stock (K), the capital–labor ratio (K/L), and the skill composition of labor—between treated regions and untreated regions before and after the implementation of the 2016 subsidy policy. This DiD approach allows us to isolate the causal effect of subsidy exposure by netting out common time trends and region-specific shocks that may influence firm behavior.

To estimate the causal effect of the 2016 subsidy reform, we employ a difference-in-differences regression model of the following form:

$$Y_{i,t} = \beta_0 + \beta_1(\text{Treated}_{i,t} \times D2016_t) + \phi_i + \phi_s + \tau_t + \epsilon_{i,t}$$

where

- i, t, r stand for firm, year, region, respectively.
- $Y_{i,t}$ denotes the outcome variable for firm i in year t,
- $(\text{Treated}_{i,t} \times D2016t)$ is the treatment interaction term, equal to 1 for firms in treated counties and years after 2016
- ϕ_i captures firm fixed effects,
- ϕ_s denotes technology-level fixed effects,
- τ_t represents year fixed effects,
- $\epsilon_{i,t}$ is the idiosyncratic error term.

The coefficient of interest is β_1, which captures the average treatment effect of the 2016 policy change—i.e., the introduction of eligibility for the additional 6-

point subsidy—on the outcome variable. We refer to this as the DiD coefficient in the result tables. The dependent variables include:

- The logarithm of firm-level employment (L)
- The logarithm of capital stock (K),
- The capital–labor ratio (K/L)
- And the logarithm of employment and employment share disaggregated by skill levels (proxied using ISCO-based occupation categories).

To account for potential confounding variation, we further control for province-specific linear time trends and 4-digit industry-by-year fixed effects, which help relax the strict parallel trends assumption by allowing for differential temporal patterns across regions and sectors.

To sum up, this study employs a DiD approach to estimate the causal impact of a 2016 subsidy expansion that extended eligibility for a 6-point reduction in employer-side social security contributions to micro firms in underdeveloped regions of Turkey. Exploiting the 2020 reclassification of subsidy eligibility from the provincial to the county level, we construct a credible control group of counties that were economically similar to treated areas but remained ineligible during the study period. The empirical strategy is implemented using firm-level panel data from 2006 to 2019 and includes a range of fixed effects and time trends to account for unobserved heterogeneity. Our specifications evaluate the policy's effect on multiple outcomes—including employment, capital stock, capital–labor ratios, and the skill composition of labor—yielding ITT estimates that are policy-relevant and robust to common identification threats.

3.5. Empirical Results

3.5.a. Employment (L)

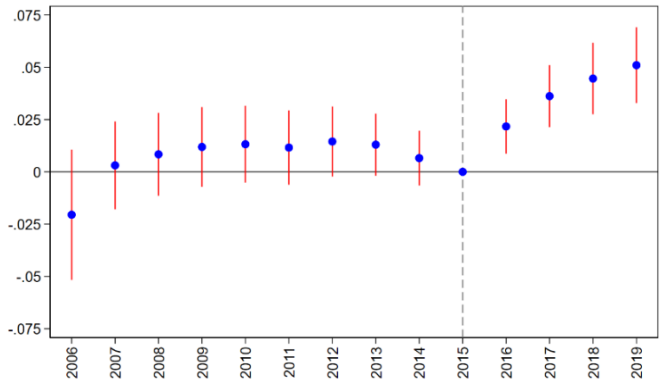
We begin the empirical analysis by examining the impact of the 2016 subsidy expansion on firm-level employment in the manufacturing sector. Given that the primary policy goal of the additional 6-point employer-side subsidy was to stimulate job creation in underdeveloped regions, we focus first on how the policy affected the employment trajectories of firms, disaggregated by size. To isolate the treatment effect on existing businesses, we restrict the sample to firms that were already established before 2016, thereby shutting down potential firm entry effects. The analysis covers micro, small, and medium-to-large firms separately.

Figure 3.1 provides an event study specification that plots the dynamic treatment effects over time. In Panel A, micro firms show a clear upward shift in employment after 2016, with statistically significant and sustained gains relative to the control group. The confidence intervals tighten post-reform, further reinforcing the stability of the estimates.

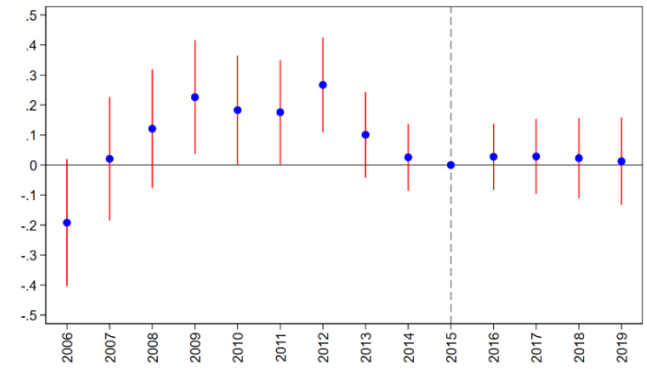
In contrast, Panel B (small firms) and Panel C (medium and large firms) show more muted responses, with coefficient estimates fluctuating around zero and confidence intervals that cross the null line. These patterns are consistent with the DiD estimates and confirm that the employment response to the policy was largely confined to micro firms, which were newly exposed to the 6-point subsidy following the 2016 expansion.

Figure 3.5. Event Study Estimates of Firm Employment Growth in Treated Region 4 Counties

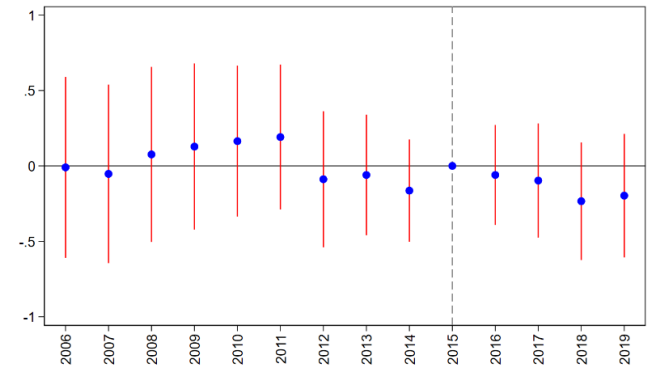
Panel A. Employment Growth in Micro Firms



Panel B.: Employment Growth in Small Firms



Panel C. Employment Growth in Medium and Large Firms



Source: Own estimations using EIS Database. The dependent variable is quarterly firm level employment (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects, year fixed effects, NUTS2 specific linear trends and 4-digit industry-year fixed effects are included. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at province level.

Table 3.2 presents the main DiD estimates of the policy's impact on log employment. The findings reveal a statistically significant and positive effect for micro firms, with a DiD coefficient of 0.025. This implies that micro firms in treated regions experienced, on average, a 2.5 percent increase in employment relative to comparable firms in untreated regions following the policy reform. This finding is compatible with the findings of Aşık et al. (2022).

For small firms, the coefficient is negative but statistically insignificant, suggesting no robust effect of the subsidy on this group. Similarly, for medium and large firms, the estimated effect is positive but also statistically insignificant. These results suggest that the employment effect of the policy was concentrated among the smallest firms, consistent with the design of the reform, which newly extended eligibility to firms with fewer than 10 employees.

	Micro	Small	Medium-Large
DID Coefficient	0.0250***	-0.0621	0.0828
	(0.00633)	(0.0668)	(0.230)
Observations	182,797	16,366	3,278
R-squared	0.737	0.590	0.652
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm employment. *** p<0.01, ** p<0.05, * p<0.1

Table 3.2. Firm Employment in Treated Region 4 Counties

Together, these results indicate that the policy was effective in stimulating employment growth where it was newly binding—i.e., among firms previously ineligible for the program. This aligns with theoretical expectations that micro firms, which are typically more cost-sensitive, would respond more strongly to reductions in labor costs.

3.5.b. Capital Accumulation (K)

We next investigate whether the 2016 subsidy expansion affected firms' capital accumulation, which serves as a key mechanism through which employment subsidies might generate longer-term productivity gains. In line with the previous theoretical discussions, employment subsidies can alter the marginal rate of substitution between labor and capital, potentially encouraging firms to either substitute labor for capital or invest more to complement expanded labor capacity.

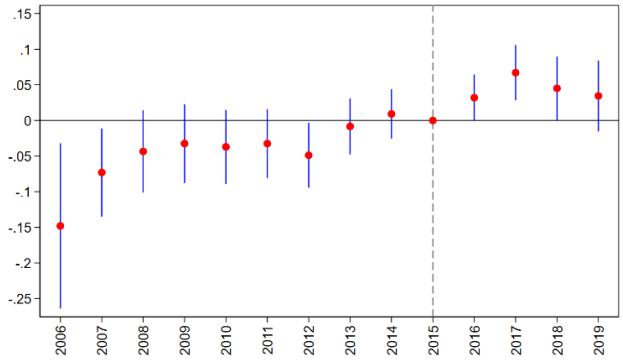
In this analysis, we use two distinct measures of capital:

- **Total Tangible Assets**, which captures the firm's entire stock of physical capital (e.g., land, buildings, machinery), and
- **Machinery and Equipment**, a narrower but more directly productivity-linked capital input.

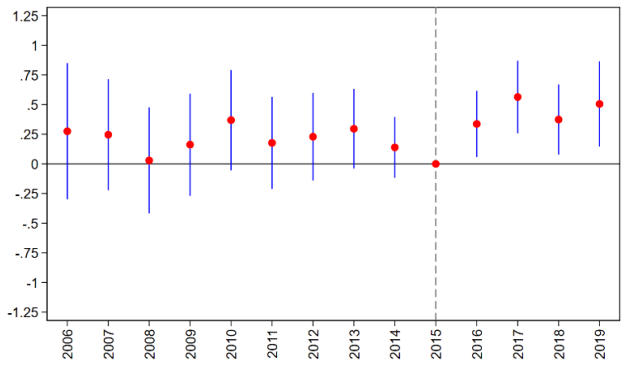
We begin by presenting the event study results. Figure 3.2 (Panels A–C) displays the dynamic treatment effects on total tangible assets by firm size. In Panel A, micro firms exhibit a visible change in the trend of capital accumulation following the policy; however, the wide confidence intervals surrounding the estimates make it difficult to interpret this as a statistically significant effect. In Panel B, small firms show a gradual and statistically significant increase in capital stock after 2016, suggesting a positive response to the policy reform. In contrast, Panel C indicates that medium and large firms followed a relatively flat trajectory with broad confidence bands, signaling no robust treatment effect. Figure 3.3 presents the corresponding event study analysis for machinery and equipment investments. None of the firm types exhibit any systematic post-treatment deviation from baseline trends for the machinery and equipment investment..

Figure 3.2. Event Study Estimates of Firm Capital Stock Growth in Treated Region 4 Counties, (K: Total Tangible Assets, In Logs)

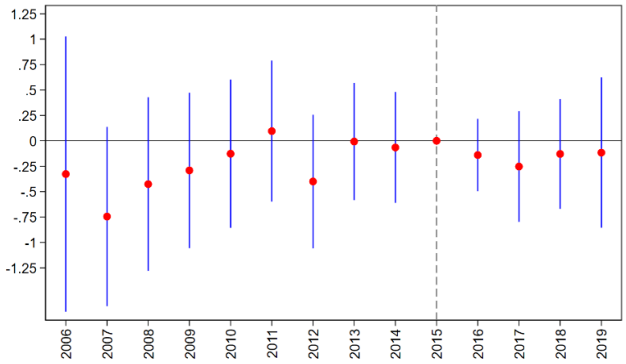
Panel A. Capital Stock Growth in Micro Firms



Panel B. Capital Stock Growth in Small Firms



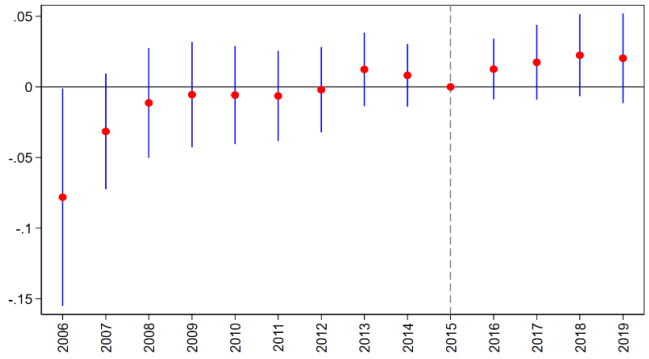
Panel C. Capital Stock Growth in Medium and Large Firms



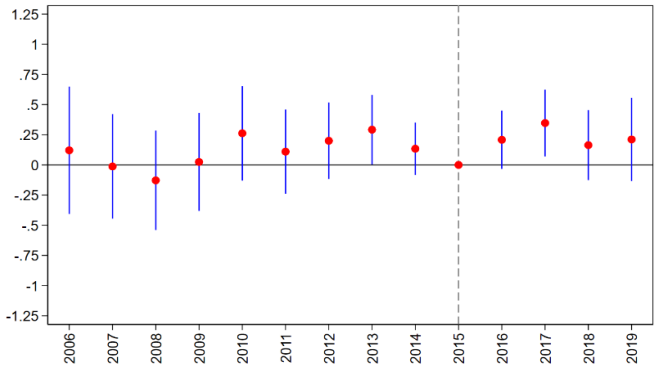
Source: Own estimations using EIS Database. The dependent variable is quarterly firm level tangible assets (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects, year fixed effects, NUTS2 specific linear trends and 4-digit industry-year fixed effects are included. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at province level.

Figure 3.3. Event Study Estimates of Firm Machinery Growth in Treated Region 4 Counties, (K: Machinery and Equipment, In Logs)

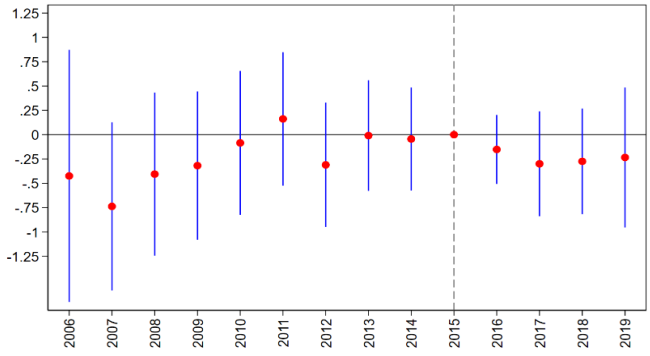
Panel A. Machinery Growth in Micro Firms



Panel B. Machinery Growth in Small Firms



Panel C. Machinery Growth in Medium and Large Firms



Source: Own estimations using EIS Database. The dependent variable is quarterly firm level machinery and equipment (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects, year fixed effects, NUTS2 specific linear trends and 4-digit industry-year fixed effects are included. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at province level

Table 3.3 reports the DiD estimates of the policy's impact on the logarithm of capital stock, broken down by capital type and firm size. Panel A shows that the policy led to statistically significant increases in tangible assets for both micro and small firms, with the effect being more pronounced among small firms. In contrast, no statistically significant impact is observed for medium and large firms. Panel B, which focuses on machinery and equipment, reveals no statistically significant effects across any firm size category. These results confirm the findings of the event study.

	Micro	Small	Medium-Large
Panel A: K=Tangible Assets (in logs)			
DID Coefficient	0.0310*	0.317**	-0.0966
	(0.0179)	(0.145)	(0.262)
Observations	182,797	16,366	3,278
R-squared	0.805	0.752	0.728
Panel B: K=Machinery and Equipment (in logs)			
DID Coefficient	0.00787	0.0908	-0.363
	(0.0115)	(0.138)	(0.356)
Observations	182,797	16,366	3,278
R-squared	0.806	0.808	0.770
Year FE	YES	YES	YES
4 Digit Industry Year FE	YES	YES	YES
Nuts2 Linear Trends	YES	YES	YES
Firm FE	YES	YES	YES

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm tangible assets (Panel A) or firm machinery and equipment (Panel B).
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3. Firm Capital Stock in Treated Region 4 Counties

These results suggest that small firms—though not directly targeted by the 2016 expansion—have redirected resources toward non-productive capital accumulation. Meanwhile, micro firms, despite employment growth, could not increase productive capital investment, possibly due to liquidity constraints or the nature of their production processes.

3.5.c. Capital Intensity (K/L)

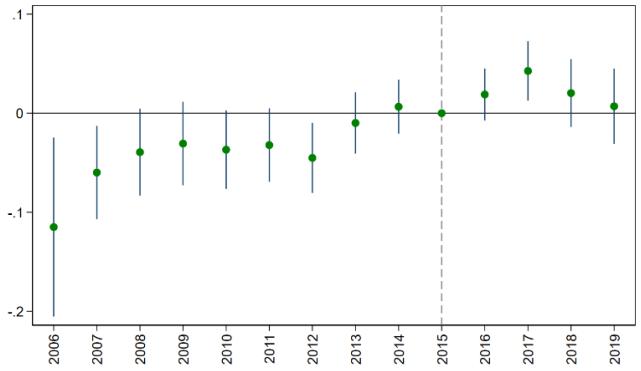
In this section, we examine the impact of the 2016 subsidy expansion on capital intensity, measured as the ratio of capital stock to labor input (K/L). Capital intensity provides insight into how firms adjust their input mix in response to changes in labor costs. A decline in labor cost—induced by the subsidy—may reduce capital intensity if firms substitute labor for capital or increase it if firms reinvest cost savings in productivity-enhancing assets alongside labor expansion. As in the previous section, we use the two definitions of capital, i.e. tangible assets and machinery-equipment.

Figure 3.4 shows the dynamic treatment effects for K/L based on total tangible assets. In Panel B (small firms), there is a clear post-2016 upward shift in capital intensity. In Panels A and C (micro and medium-large firms), the estimates remain close to zero with wide confidence intervals. Figure 3.5 focuses on K/L measured using machinery and equipment. Neither of the panels show consistent treatment effect. Table 3.4 presents DiD estimates of the effect of the policy on log(K/L) across firm size categories and capital definitions. The results are consistent with the event study results. The only statistically significant result is found for small firms under the capital definition of tangible assets.

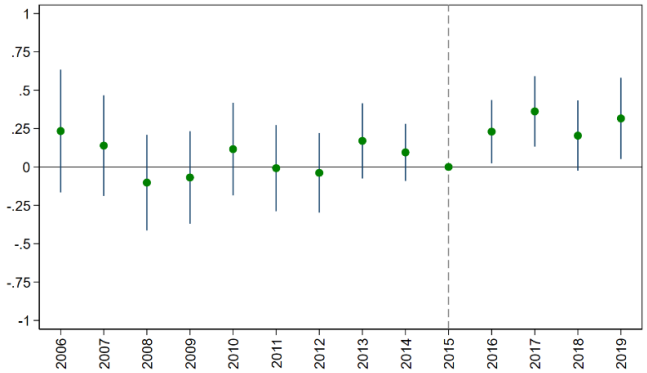
Taken together, the evidence suggests that capital deepening occurred selectively, only at small firms, but for tangible assets not the machinery and equipment. For micro firms, the absence of a capital response alongside employment growth likely reflects a labor-intensive production model or limited access to investment finance. Meanwhile, larger firms do not appear to have systematically adjusted their K/L in response to the policy.

Figure 3.4. Event Study Estimates of Firm Capital Intensity (K/L) in Treated Region 4 Counties, (K: Total Tangible Assets)

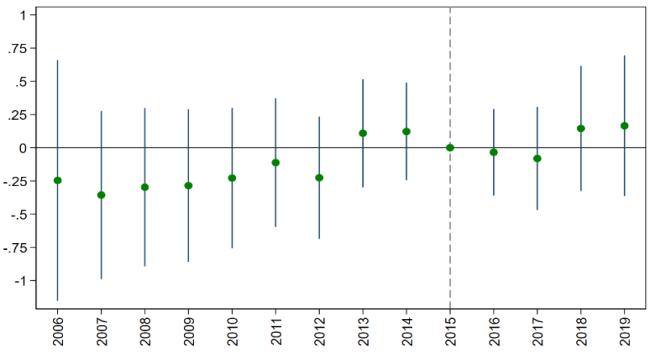
Panel A. Capital Intensity in Micro Firms



Panel B. Capital Intensity in Small Firms



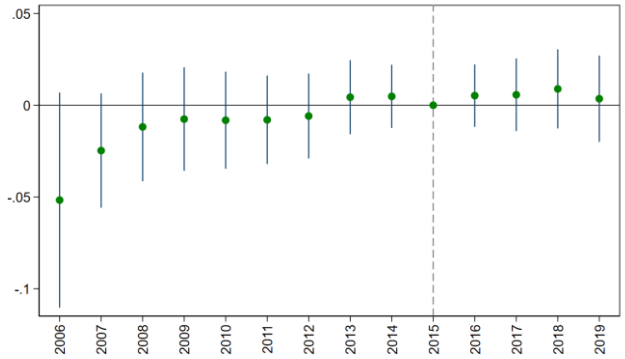
Panel C. Capital Intensity in Medium and Large Firms



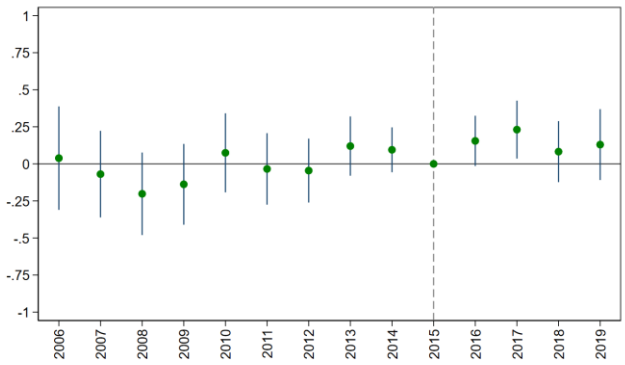
Source: Own estimations using EIS Database. The dependent variable is quarterly firm level capital intensity (tangible assets divided by employment, in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects, year fixed effects, NUTS2 specific linear trends and 4-digit industry-year fixed effects are included. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at province level.

Figure 3.5. Event Study Estimates of Firm Capital Intensity (K/L) in Treated Region 4 Counties, (K: Machinery and Equipment)

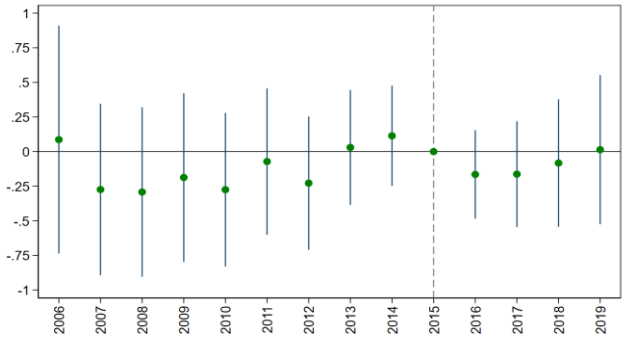
Panel A. Capital Intensity in Micro Firms



Panel B. Capital Intensity in Small Firms



Panel C. Capital Intensity in Medium and Large Firms



Source: Own estimations using EIS Database. The dependent variable is quarterly firm level capital intensity (machinery and equipment divided by employment, in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects, year fixed effects, NUTS2 specific linear trends and 4 digit industry-year fixed effects are included. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at province level.

	Micro	Small	Medium-Large
Panel A: K=Tangible Assets (in logs)			
DID Coefficient	0.0179	0.240**	-0.0770
	(0.0132)	(0.106)	(0.189)
Observations	182,797	16,366	3,278
R-squared	0.793	0.742	0.711
Panel B: K=Machinery and Equipment (in logs)			
DID Coefficient	0.00253	0.102	-0.176
	(0.00828)	(0.0943)	(0.208)
Observations	182,797	16,366	3,278
R-squared	0.792	0.794	0.752
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is capital intensity (K/L) measured by firm tangible assets (Panel A) or firm machinery and equipment (Panel B). *** p<0.01, ** p<0.05, * p<0.1

Table 3.4. Firm Capital Intensity (K/L) in Treated Region 4 Counties

3.5.d. Heterogeneity Check by Pre-Treatment Average Firm Size

To further test the robustness of our findings, we reclassify firms by their average employment during the pre-treatment period (2006–2015). This alternative classification allows us to examine which firms—relative to the original eligibility threshold of 10 employees—were most affected by the 2016 reform. This design helps address potential misclassification caused by temporary employment fluctuations or firm growth and isolates groups more or less likely to be newly treated. Firms are grouped into four size bands based on their pre-reform average employment:

- **1–4 employees:** clearly ineligible before 2016 (micro firms),
- **5–7 employees**: likely close to the eligibility threshold, mostly ineligible,

- **8–12 employees:** near or just above threshold; potentially marginally eligible,
- **13+ employees:** most likely to be eligible pre-reform; expected to serve as placebo.

The results are presented in Table 3.5, with each row corresponding to a size band and columns reporting DiD estimates for: log employment (Column 1), log capital – total tangible assets (Column 2), log capital – machinery & equipment (Column 3), Capital–labor ratio (K/L) with total tangible assets (Column 4) and K/L with machinery and equipment (Column 5).

1–4 Employees: This group shows the strongest and most consistent policy response across all outcomes. Employment rises by 4.93 percent, and capital stock increases significantly—9.96 percent for tangible assets and 5.76 percent for machinery. Capital intensity also improves, with statistically significant gains in the K/L ratio using tangible assets. These findings challenge the assumption that the smallest micro firms are too resource-constrained to invest and instead suggest that subsidy access can meaningfully enable both labor and capital expansion.

5–7 Employees: This group exhibits positive but statistically insignificant effects across all outcomes. These firms may have been near or partially eligible before the reform, leading to more muted responses.

8–12 Employees: Notably, this group shows statistically significant gains in capital stock and capital intensity, using tangible assets. The coefficient for tangible assets is positive and significant (at the 5% level), and the K/L ratio (based on the same definition) also increases significantly. This indicates that marginally eligible firms responded by investing in broad physical capital, even if employment effects

were not statistically significant. The response suggests capital deepening in firms that were already relatively close to full eligibility.

13+ Employees: As expected, no statistically significant effects are observed. These firms were most likely to be eligible before the reform, and their stable post-2016 outcomes serve as a useful placebo check, supporting the validity of the identification strategy.

	Employment	Tangible Assets	Machinery & Equipment	K/L (Tang. Assets)	K/L (Mach.&Eqp.)	Obs.
Firm size: 1-4 employees						
DID Coefficient	0.0493***	0.0996**	0.0576*	0.0577*	0.0348	63,630
	(0.0167)	(0.0478)	(0.0312)	(0.0350)	(0.0231)	
Firm size: 5-7 employees						
DID Coefficient	0.00167	-0.0103	-0.121	-0.0112	-0.0461	9,254
	(0.0629)	(0.176)	(0.128)	(0.120)	(0.0868)	
Firm size: 8-12 employees						
DID Coefficient	0.0409	0.567**	0.247	0.389**	0.195	5,869
	(0.105)	(0.266)	(0.244)	(0.190)	(0.162)	
Firm size: 13+ employees						
DID Coefficient	0.0285	0.139	-0.0458	0.0852	-0.0245	23,116
	(0.0921)	(0.147)	(0.166)	(0.0996)	(0.103)	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Industry-year fixed effects are at 4-digit industry level. Standard errors are clustered at the firm level. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.5. Employment, Capital and Capital Intensity (K/L) in Treated Region 4 Counties

This heterogeneity analysis confirms that the employment subsidy reform had differentiated impacts depending on firms' pre-policy proximity to eligibility thresholds. While the strongest effects are observed in the smallest micro firms (1–4 employees), firms with 8–12 employees also showed significant capital responses—particularly in tangible asset accumulation and capital deepening. These patterns

reinforce the finding that relaxing eligibility constraints for small firms can generate multi-dimensional firm growth, both along labor and capital margins.

3.5.e. Supplementary Analysis: Region 5 Counties

As a supplementary robustness check, we estimate treatment effects using an alternative sample restricted to Region 5 counties. In this specification, the treatment group includes Region 5 counties located in provinces officially designated as Region 5, while the control group comprises Region 5 counties located in provinces classified as Regions 1–3, which were ineligible for the 6-point subsidy during the study period. While this approach narrows the developmental heterogeneity between groups, it does not satisfy the parallel trends assumption, as pre-treatment dynamics diverge. Therefore, the results, which are presented in the Appendix, should be interpreted with caution and are not used for causal inference.

Table A2 presents the DiD estimates for log employment. For micro firms, the estimated effect is positive and statistically significant, indicating that these firms in treated Region 5 counties experienced employment growth relative to their ineligible peers. The estimates for small and medium-large firms are statistically insignificant, with the latter even showing a large negative point estimate, albeit with a very wide confidence interval.

Table A3 presents the DiD estimates for capital investment. For micro firms, the coefficient on machinery and equipment is positive and statistically significant, indicating that even among this narrowly defined sample, treated micro firms increased their machinery investment following the policy expansion. The estimate for total tangible assets, however, is smaller and not statistically significant. For

small firms, the results are positive in magnitude for both capital measures, but neither estimate is statistically significant. For medium and large firms, both coefficients are negative and imprecise, again consistent with the overall finding that the policy had limited or no effect on this group.

As reported in Table A4, there is no evidence of statistically significant effects on capital–labor ratios for any firm size group. While the signs for micro and small firms are positive, the coefficients are small and not distinguishable from zero.

Although the results for micro firms in treated Region 5 counties show modest employment and capital stock gains, the lack of common pre-treatment trends means these estimates cannot be interpreted causally. Instead, they serve as descriptive evidence, providing support for the direction and magnitude of effects seen in the main results, particularly among micro firms. Importantly, the null results for medium-large firms in this sample align with the primary findings and reinforce the notion that the policy's impact was concentrated among smaller enterprises.

3.5.f. Skill Composition of Labor

In the final component of our analysis, we examine whether the 2016 subsidy expansion affected the skill composition of the workforce. Given the policy's design as a labor cost-reduction tool primarily targeted at formalization and job creation, its potential to induce shifts in the skill profile of employment is theoretically ambiguous. On one hand, lower labor costs could allow firms to hire more skilled workers at reduced effective wages. On the other hand, subsidies may

encourage employment of lower-skilled or informal workers, particularly in labor-intensive firms. We evaluate both:

- The share of skilled labor in total employment (Table 3.6), and
- The level of skilled labor employment, in log terms (Table 3.7).

Skill categories include total skilled labor, as well as subgroups: managers, professionals, technicians, and craftsmen, based on occupation codes.

Across all firm size categories—micro, small, and medium-large—none of the estimated treatment effects are statistically significant at conventional levels. While some coefficients are positive (e.g. for technicians in micro firms, or total skilled labor in small firms), standard errors are large, and no consistent pattern emerges. A few negative estimates appear (e.g. professionals in medium-large firms), but again, with wide confidence intervals.

These null findings suggest that the subsidy did not materially affect the skill composition of employment. This is consistent with our earlier conclusion that employment gains were concentrated in micro firms, which tend to operate with low-skilled labor and may have limited capacity or incentive to shift toward higher-skilled personnel. It also supports concerns in the literature (Oskamp and Snower 2006) that employment subsidies can reduce incentives for skill upgrading, especially when tied to low-wage or informal-sector transitions.

The absence of significant effects on skill upgrading implies that the 2016 subsidy expansion functioned primarily as a job quantity intervention, rather than one that improved job quality or workforce structure.

	Total Skilled	Managers	Professionals	Technicians	Craftsmen
Micro firms					
DID Coefficient	0.00108	-0.000313	0.00308	0.00139	-0.0124
	(0.00608)	(0.00224)	(0.00361)	(0.00578)	(0.0132)
Small firms					
DID Coefficient	-0.00264	0.00527	-0.00676	-0.00791	-0.0215
	(0.0180)	(0.00620)	(0.0124)	(0.0171)	(0.0279)
Medium-large firms					
DID Coefficient	-0.00741	0.00598	-0.0402	-0.0134	-0.0105
	(0.0422)	(0.00898)	(0.0234)	(0.0422)	(0.0689)
Year FE	Yes	Yes	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

Notes: Industry-year fixed effects are at 4-digit industry level. Standard errors are clustered at the firm level. Outcome variable is the share of employment in total employment. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.6. The Share of Skilled Labor Employment in Treated Region 4 Counties

	Total Skilled	Managers	Professionals	Technicians	Craftsmen
Micro firms					
DID Coefficient	0.00517	-0.0107	-0.124	0.140	0.479
	(0.260)	(0.0238)	(0.184)	(0.181)	(0.378)
Small firms					
DID Coefficient	0.308	0.135	-0.0519	0.225	-1.280
	(1.367)	(0.501)	(0.756)	(0.941)	(2.871)
Medium-large firms					
DID Coefficient	-3.550	-7.704	-7.229	11.38	-56.37
	(18.36)	(6.942)	(8.031)	(12.85)	(46.47)
Year FE	Yes	Yes	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

Notes: Industry-year fixed effects are at 4-digit industry level. Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm employment. *** p<0.01, ** p<0.05, * p<0.1

Table 3.7. Skilled Labor Employment in Treated Region 4 Counties

3.5.g. Summary of Empirical Findings

The empirical analysis reveals a nuanced but consistent pattern in the impact of the 2016 employment subsidy reform on Turkish manufacturing firms. The

strongest and most robust effects were observed among micro firms, which experienced significant increases in employment, while small firms responded primarily through increased capital investment—particularly in tangible assets rather than machinery and equipment—and enhanced capital intensity given this definition of the capital. These effects were absent for medium and large firms, suggesting the policy's impact was concentrated among previously ineligible or marginally exposed firms.

Heterogeneity analysis based on pre-treatment firm size further confirms that very small firms (1–4 employees) experienced significant growth across labor and capital margins, while firms near the eligibility threshold (8–12 employees) showed signs of capital deepening. Supplementary analysis on Region 5 counties offers supportive but non-causal evidence, aligning in direction and magnitude with the main results. However, the policy appears to have had no discernible effect on skill composition, as estimates for both skilled labor share and skilled employment levels were statistically insignificant. Overall, the results indicate that the 2016 subsidy expansion functioned effectively as a quantity-focused intervention, stimulating employment and capital accumulation among small firms, without triggering structural upgrading in the workforce.

3.6. Discussion

One of the most striking features among the results is the divergence between employment and capital deepening across micro and small firms. Micro firms increased employment without altering productive capital stocks (but weak evidence on building tangible assets), while small firms responded mainly through

tangible asset investment without any rise in employment. This divergence may be explained by several interacting mechanisms:

- **Liquidity constraints:** Micro firms are more likely to face credit access and liquidity constraints (Beck et al. 2008), which may prevent them from reinvesting cost savings into long-term assets. For these firms, the subsidy may have enabled hiring decisions that would otherwise have been unaffordable, but not large upfront capital purchases.
- **Production structure:** Micro firms are typically more labor-intensive, operating in low-tech sectors with limited capital requirements (OECD 2017). As such, they may not have a need—or the capacity—to increase capital alongside labor.
- **Substitution vs. complementarity:** Theoretical models suggest that firms respond differently to labor subsidies depending on the elasticity of substitution between capital and labor (Petrucci and Phelps 2005). For micro firms, the subsidy may have shifted the optimal input mix toward labor substitution, whereas for small firms, labor and capital may have been more complementary.

This divergence is consistent with prior research showing that smaller firms respond on the extensive margin of hiring, while slightly larger firms (closer to formalization thresholds) are more likely to use subsidies to upgrade their input mix (Crépon and Desplatz 2003; Kugler and Kugler 2009).

Another notable finding is the lack of responsiveness among medium and large firms, despite the fact that these firms likely benefited most in absolute

monetary terms from the 2016 policy expansion, which extended subsidies to all existing employees rather than only new hires. This raises the question: where did the subsidy money go? Several interpretations are possible:

- **Pure windfall gains:** Larger firms may have treated the subsidy as a financial transfer rather than a behavioral incentive. As argued by Neumark (2013), wage subsidies without conditionality often produce deadweight loss, especially when applied retroactively to existing employment.
- **Higher reservation thresholds:** Larger firms may operate with more rigid investment plans and higher hurdle rates for expanding capital or labor. Thus, while the subsidy lowered costs, it may not have shifted marginal incentives enough to trigger new behavior.
- **Capital misallocation or profit absorption:** It is possible that cost savings were retained as increased profits, used to smooth cash flow, or diverted toward non-productive expenditures—such as debt service or dividend payouts—rather than reinvested into labor or technology.

This interpretation echoes concerns in the literature about subsidy leakage and the difficulty of inducing behavioral change through unconditional subsidies (Almeida et al. 2014; Betcherman et al. 2010).

Despite observable gains in employment and capital (especially among micro and small firms), we find no statistically significant effects on the skill composition of employment. This null result can be reconciled with the other findings in several ways:

- **Low-skill employment response:** The employment gains among micro firms likely came from lower-skill labor segments, where the marginal cost reduction was most binding. In contexts with high informality and underemployment, subsidies may first absorb excess low-skill labor before affecting skill upgrading.
- **Subsidy design:** The policy was not targeted at skill upgrading. Without training components, hiring quotas, or differentiated rates based on occupation, firms lacked incentives to reconfigure their workforce composition.
- **Complementarity with machinery investment:** The limited response in machinery and equipment investment, especially among micro firms, is consistent with the absence of skill upgrading. Firms that do not invest in higher technology are unlikely to increase their demand for skilled labor (Autor et al. 2003).

Together, these results suggest that the 2016 subsidy was effective as a quantity-focused intervention but did not contribute to structural changes in workforce quality or productivity-enhancing inputs.

The results also show a stronger response in capital accumulation for total tangible assets than for machinery and equipment. This asymmetry has two important implications:

- **Asset composition bias:** Firms may have invested in buildings, vehicles, or non-productive fixed assets rather than in productivity-enhancing capital. This is especially important for a country like Turkey, which suffers from structural imbalances. Such responses can

boost accounting capital but have limited effects on efficiency or innovation.

- **Policy design shortcoming:** The subsidy may not have been sufficient to induce investment in technology-intensive equipment, which often requires complementary skills, financing, and long-term planning.
- **The lack of machinery-focused investment** is compatible with the flat skill composition results, reinforcing the interpretation that firms did not shift toward higher-value production or higher-skilled labor.

This aligns with prior studies in developing countries (e.g. Bruhn 2020; Clemens et al. 2018), which emphasize that labor cost reductions alone are not sufficient to induce transformation in input composition or production structure. For long-run effectiveness, subsidies should be strategically integrated with policies aimed at skill upgrading, capital deepening, and formal sector development.

3.7. Conclusion

This study examines the firm-level effects of Turkey's 2016 employment subsidy reform, which significantly expanded the scope of existing labor cost support by granting a 6-percentage-point reduction in employer-side social security contributions to all employees of firms in underdeveloped regions—most notably including micro firms for the first time. Using detailed, longitudinal administrative data from EIS and a quasi-experimental DiD design, the study evaluates how firms adjusted not only their employment levels but also their capital investment, capital–labor ratios, and workforce composition in response to this policy change.

The motivation behind this research lies in the growing but inconclusive literature on whether employment subsidies act merely as short-term job creation tools or whether they also catalyze broader transformations in firm behavior. While prior studies have assessed the effects of such policies on formal employment, relatively little is known about their impact on capital formation, capital–labor substitution, or skill upgrading—particularly in developing and emerging economies. This study contributes to filling this gap by analyzing the heterogeneous effects of the reform across firms of different sizes and stages of formalization.

Our analysis reveals a clear pattern of size-based heterogeneity in firm responses to the policy. The most robust and significant effects were observed among micro firms (1–9 employees), which experienced statistically significant employment gains following the reform. These firms had previously been excluded from the subsidy scheme and responded on the extensive margin once eligibility was extended. However, this employment expansion was not accompanied by parallel increases in machinery and equipment investment or capital–labor ratios, suggesting that micro firms used the subsidy primarily to scale labor rather than to transform production technology or input composition.

In contrast, small firms (10–49 employees) exhibited capital deepening in the form of tangible assets, with no significant effects on employment. These results suggest that small firms were more likely to reinvest cost savings into tangible asset accumulation other than machinery and equipment. In other words, firms may have invested in buildings, vehicles, or non-productive fixed assets rather than in productivity-enhancing capital. The impact was especially pronounced in firms near the eligibility threshold prior to the reform (e.g., firms with 8–12 pre-reform

employees), pointing to the importance of marginal eligibility status in determining firm responsiveness.

Interestingly, medium and large firms—which benefited the most in absolute monetary terms after the policy shifted from new hires to all employees—exhibited no statistically significant behavioral response in any of the outcomes analyzed. This suggests that for these firms, the policy functioned largely as a fiscal transfer or windfall, with no discernible effect on hiring, investment, or input mix. The absence of any employment or capital response from this group raises questions about the cost-effectiveness of untargeted subsidies and supports arguments in the literature regarding deadweight loss and inefficiency in universal policy designs (e.g. Neumark 2013; Bloom 2008).

Turning to the skill dimension, the subsidy appears to have had no significant impact on the skill composition of the workforce across all firm sizes. Both the share and level of skilled employment (based on ISCO occupational categories) remained unchanged. This null result complements the non-responsiveness in machinery and equipment investment, reinforcing the interpretation that the policy did not induce structural transformation or workforce upgrading. The labor and capital adjustments that occurred were primarily extensive rather than qualitative.

These findings carry important implications for both labor economics theory and public policy design. First, the asymmetric responses across firm sizes support theoretical models that highlight the role of credit constraints, production technologies, and marginal incentives in mediating subsidy effectiveness (Petrucci and Phelps 2005; Crépon and Desplatz 2003). For micro firms, labor subsidies act as a cost relief mechanism, enabling short-run hiring but without catalyzing more

durable investment behavior. In contrast, small firms with more formalized structures may interpret the subsidy as a signal or opportunity for enhancing investment, particularly in tangible capital.

Second, the lack of any observable skill upgrading or capital deepening in technology-intensive assets suggests that subsidies alone are insufficient to push firms toward higher-value production. Without complementary policies—such as vocational training, digitalization incentives, or innovation support—wage subsidies may reinforce low-skill, low-productivity employment structures. This is especially critical in middle-income countries like Turkey, where wage compression, informality, and limited technological absorption remain key challenges.

While the study leverages high-quality data and a credible identification strategy, it is subject to several limitations. First, the analysis uses Intention-To-Treat (ITT) estimates based on firm location and eligibility, not actual subsidy take-up. If compliance varied systematically across firms, the effects may be understated or masked by take-up heterogeneity. Second, the study focuses on short to medium-run outcomes; longer-term impacts on productivity, survival, or export behavior remain unexplored. Third, the data do not contain education-level information, which would allow for a more precise measurement of human capital changes.

Future research could address these gaps by exploiting administrative data on actual subsidy claims, extending the analysis into the post-pandemic period, and linking firm performance to broader economic outcomes such as innovation, exports, or job quality. Experimental or quasi-experimental evaluations of multi-instrument policy bundles (e.g., wage subsidies plus training or technology adoption grants) would also be highly valuable.

This study provides new and nuanced evidence on how employment subsidies affect firm behavior in a middle-income, high-informality context. The findings suggest that while such policies can stimulate employment and capital accumulation in small enterprises, they are unlikely to drive structural upgrading on their own. Effective labor market policy requires a strategic combination of cost relief, capability building, and investment in workforce quality. Turkey's experience underscores both the potential and the limits of wage subsidies as a tool for inclusive and productive employment growth.

CHAPTER IV

CAN EMPLOYMENT SUBSIDIES CREATE COMPETITIVENESS GAINS FOR MANUFACTURING FIRMS IN EXPORT MARKETS? EVIDENCE FROM TURKEY

4.1. Introduction

Export-led growth remains a cornerstone of structural transformation and long-run economic development, particularly in emerging economies. By leveraging global demand, countries can transcend the limitations of domestic markets, specialize in internationally competitive sectors, attract foreign direct investment, and benefit from knowledge spillovers and scale economies (Bernard and Jensen 1999; Moreno-Brid 2013; Taglioni and Winkler 2016; Koswatta 2023). The sustained industrial upgrading achieved by several East Asian economies reinforces the strategic role of exports in reshaping productive structures.

At the macroeconomic level, the determinants of export performance have often been modeled through demand-side frameworks, particularly those rooted in Keynesian open economy theory. These models emphasize foreign income as the primary driver of export demand, rendering export performance closely aligned with global economic cycles (Blecker 2002; Kenen 1985). Alongside this, relative prices, particularly the real exchange rate, play a key role in shaping international competitiveness. A depreciation in the domestic currency reduces the foreign-currency price of exports, potentially increasing demand, as formalized in the

Marshall–Lerner condition (Nguyen 1993; Pariboni and Meloni 2024). Yet, empirical findings complicate this narrative. Firms' exposure to exchange rate fluctuations varies with their cost structure, import dependency, and financial liabilities, which can offset or amplify the expected competitiveness effects (Toraganlı and Yalçın 2016; Akdoğan et al. 2023).

Beyond macroeconomic conditions, supply-side and structural factors have increasingly come into focus. These include the productive capacity of firms, access to finance, labor costs, innovation systems, infrastructure, and institutional quality (Rodrik 2006; Hoedoafia 2019; Herrero et al. 2024). To address market imperfections and information asymmetries, governments have introduced various export promotion instruments—ranging from tax rebates and duty drawbacks to export insurance and market access services (Koswatta 2023; Seguino 1994). The success of these interventions, however, appears to hinge on firm-level characteristics such as productivity, technological capability, and financial health (Bernard and Jensen 2004; Manova 2013; Onaran and Stockhammer 2001).

This has led to a growing consensus around the importance of firm-level heterogeneity. Building on Melitz's (2003) seminal model, trade literature now recognizes that only a subset of firms, typically more productive and better resourced, can overcome the fixed and sunk costs of exporting. Empirical studies across regions confirm that export participation is highly selective and persistent, shaped by differences in size, ownership, R&D intensity, and integration into global value chains (Uminski and Fornalska 2021; Ganguly 2023).

Public policy has attempted to address these asymmetries, primarily through fiscal incentives, credit programs, and participation support in export activities.

Notably, subsidies now account for a majority of such interventions: by 2009, they constituted around 60 percent of all trade-related public support globally (Rotunno and Ruta 2024). Yet outcomes remain mixed and highly context-dependent. In Latin America, export promotion succeeded mainly when targeting firms with pre-existing capabilities (Martincus and Carballo 2008), while inefficiencies in subsidy allocation undermined impacts in countries like Argentina (Reis and Kume 2000). Similar heterogeneity is observed in Europe, where effectiveness varied by sector, firm size, and absorptive capacity (Brancati et al. 2018; Girma et al. 2009).

A less explored, yet increasingly relevant policy instrument is the wage or labor cost subsidy. Labor cost subsidies have traditionally been studied through the lens of employment outcomes, especially in the context of formalization and hiring incentives.[14] Much of this literature focuses on developed countries, with mixed findings: while some studies report no significant effects, others identify positive impacts, particularly for youth and older women or in short-term durations (Blundell et al. 2004; Deidda et al. 2015; Goos and Konings 2007; Huttunen et al. 2013). Evidence from developing economies is less abundant but similarly mixed, often showing modest improvements in formal employment (Gruber 1997; Kugler and Kugler 2009; Cruces et al. 2010).

Often introduced with the aim of boosting domestic employment or reducing regional disparities, such subsidies may indirectly influence export dynamics by lowering marginal costs and relaxing liquidity constraints. From a theoretical perspective, labor subsidies can ease the financial frictions that inhibit export participation, particularly for SMEs and firms operating in labor-intensive sectors

[14] See more detailed discussion in Chapter III.

(Oreiro 2005; Baccaro and Höpner 2022; Myrodias 2024). They may also influence both whether a firm enters export markets (the extensive margin) and how much it exports (the intensive margin). Liquidity-constrained firms may be unable to export even if marginally productive enough to do so (Chaney 2005; Manova 2013), and labor subsidies, by lowering wage burdens and improving internal cash flow, may act as indirect liquidity injections, enabling firms to reallocate resources toward export-related investments. These effects are unlikely to be uniform. SMEs, which typically face tighter financial constraints, may derive greater benefits than large firms. Likewise, labor-intensive sectors may see stronger responses due to the higher share of wages in their cost structures. As such, firm size and sectoral composition are likely to mediate the effect of wage subsidies on export behavior—an empirical question this study aims to investigate. However, despite the plausibility of these mechanisms, empirical evaluation remains scarce. Some exceptions include Silva and Carreira's (2011) study of Portuguese firms and Enami and Ghosh's (2024) analysis of US wage subsidies during the pandemic, both of which find tentative evidence linking labor cost support to export outcomes.

Against this backdrop, microeconomic interventions such as wage subsidies may offer a more precise and less distortionary policy lever. By directly lowering unit labor costs without fueling domestic inflation, these subsidies can help restore or preserve external competitiveness without relying on currency depreciation. Nonetheless, the effectiveness of such subsidies hinges on their design, targeting, and fiscal sustainability, which are critical considerations in a country with chronic budgetary pressures and limited policy space (WTO 2006).

In this study, we aim to assess how labor cost subsidies influence firm-level export performance, by utilizing a policy change in Turkey. In this regard, this study contributes to the literature by shifting the analytical lens from employment to trade, evaluating the extent to which labor cost subsidies influence export performance. Focusing on Turkey, an upper-middle-income country with a sizable manufacturing sector, a long tradition of industrial policy, and geographically targeted wage subsidy programs, this research investigates whether such subsidies affect firms' export market entry (extensive margin) and export intensity (intensive margin). By analyzing firm-level administered panel data and accounting for sectoral, regional, and firm-level variation, the study seeks to identify the conditions under which labor subsidies may operate not only as employment support mechanisms but also as strategic tools for export competitiveness. In doing so, the study contributes to a more integrated understanding of how labor market policies interact with trade outcomes—an issue of growing relevance for middle-income economies navigating currency instability, fiscal constraints, and structural competitiveness gaps.

The structural features of the Turkish economy make it an especially pertinent case for examining the limits of macroeconomic tools, particularly exchange rate policy, in sustaining export competitiveness. Turkey has historically run persistent current account deficits, driven largely by its heavy reliance on imported intermediate goods, notably energy and capital equipment. This dependence heightens the economy’s exposure to external price shocks and exchange rate fluctuations. Empirical studies confirm that exchange rate pass-through (ERPT) into domestic prices in Turkey is both rapid and significant, with a higher impact on

producer prices due to the elevated use of imported inputs (Ertuğ et al. 2020). In periods of currency depreciation such as 2018 and post-2021, imported input costs surged, amplifying inflationary pressures and undermining the competitiveness gains typically expected from real devaluation (Ozdogan 2022). In this context, the traditional strategy of using a weak currency to stimulate exports has had diminishing returns. Moreover, Turkish firms often operate under conditions of financial fragility and foreign currency liabilities, which compound the inflationary feedback loop: exchange rate shocks quickly feed into higher domestic prices, triggering wage demands and further cost pressures. This exchange rate–inflation–wage spiral weakens the real effective exchange rate adjustment mechanism and creates a volatile trade environment (Torun and Yassa, 2023). The historical episodes of 2014–15 and the 2018 currency crisis underscore this vulnerability: while the lira sharply depreciated, the anticipated export boom failed to materialize at scale, as firms simultaneously faced cost surges and credit constraints (Özmen et al. 2012).

Export subsidies have long been a core component of Turkey’s export promotion toolkit, particularly in manufacturing sectors. The Turkish government has implemented a range of financial incentives, including tax exemptions, credit schemes, and direct subsidies, aimed at easing firms' entry into international markets and supporting export expansion. Empirical evidence suggests that these instruments have had measurable but uneven impacts across firm types and sectors. For example, Özler et al. (2009) find that export subsidies contributed positively to the export decision at the plant level, especially for firms already engaged in foreign markets. Similarly, Demirhan (2013) shows that subsidies were more effective for

medium-to-large enterprises, while their impact on small exporters was relatively limited due to administrative barriers and capacity constraints. More recent work by Seven and Tok (2024) using firm-level panel data confirms that subsidized export credits improved firms' export intensity, particularly in capital and technology-intensive industries. However, the efficiency of these policies has often been questioned, with concerns around their cost-effectiveness, targeting, and long-term sustainability. These findings highlight the importance of designing export subsidies that are not only generous but also well-targeted, complementing other structural interventions such as labor cost support and productivity enhancement programs.

Although export subsidies have received significant attention, relatively fewer studies have focused on how labor cost subsidies influence export outcomes in Turkey. Rather, several studies have evaluated the impact of wage subsidies on employment. For instance, Betcherman et al. (2010) found a positive formal employment effect from regional hiring subsidies in 2004–2005, while Ayhan (2013) and Uysal (2013) reported increased employment, especially among women, after the 2011 youth-focused subsidy. More recently, Aşık et al. (2022) used firm-level social security data to show that a 2016 geographically targeted subsidy significantly increased registered employment among small firms, though much of this gain appeared to result from formalization rather than net job creation.

However, while the employment effects of labor subsidies are well-studied, their potential impact on export performance remains underexplored. Wage subsidies, by lowering unit labor costs and improving firms' liquidity positions, may affect price competitiveness and encourage export activity—particularly in labor-intensive and financially constrained firms. Recent micro-level research

provides initial support for this hypothesis. Seven and Tok (2024), using Turkish firm-level panel data, show that cost-based support schemes—particularly those reducing production costs—are associated with higher export intensity and greater resilience to exchange rate shocks. Likewise, Akdoğan et al. (2023) find that labor-intensive exporters are especially responsive to labor cost dynamics, suggesting that wage subsidies can function as indirect export promotion tools.

In this study, we aim to identify the causal effect of labor subsidies on the export decision of the manufacturing firms in Turkey, by utilizing firm data at county level. To this end, we examine the effects of a geographically targeted subsidy introduced under Law No. 6846, which provided an additional 6 percentage point reduction in employers' social security contributions for registered employees—supplementing an existing 5 percent contribution discount already in place. Although the subsidy has been active since 2013, its eligibility criteria have evolved over time. Initially, until 2016, only firms with more than 10 employees located in 51 designated provinces and 2 additional districts were eligible to benefit. These provinces were selected based on regional development classifications, with the subsidy limited to regions 4, 5, and 6—identified as the three least developed among Turkey's six official development regions. In March 2016, the program was expanded to include all firms in eligible provinces, irrespective of their employment size. A further policy adjustment occurred in August 2020, when the unit of eligibility was shifted from the provincial to the county level. As a result, 99 counties previously excluded due to their association with more developed provinces became newly eligible, based on their own underdevelopment status, comparable to that of region 4. This redefinition of eligibility creates a natural

control group that enables a credible identification strategy for estimating the causal impact of the 2016 policy expansion. Further, the Additional 6-Point Subsidy constitutes a substantial cost-reducing intervention in Turkey's labor market policy, particularly for firms operating in underdeveloped regions. By offering a 6-percentage-point reduction in the employer's social security contributions—on top of an existing 5-point subsidy—the program enabled firms to cut their social security costs substantially, translating to an overall 6 percent reduction in total labor costs for minimum-wage employees (Aşık et al. 2022).

For the empirical analysis, we adopt a difference-in-differences (DiD) framework, exploiting the variation in subsidy eligibility generated by the 2016 policy reform. Similar to the approach used by Aşık et al. (2022), we consider the 2016 legislative change as the treatment. Firms located in counties that were newly classified as eligible in 2020 serve as the control group, as they were not exposed to the subsidy during our sample period. In contrast, the treatment group consists of firms in counties with comparable development levels that became eligible for the subsidy as of 2016. The sample period spans from 2010 to 2019, ensuring that the control units remained unaffected by the policy throughout the duration of the analysis. The key identifying assumption underlying this strategy is that, in the absence of the policy change, firms in both the treatment and control groups exhibit parallel trends in the outcomes of interest. We estimate the causal impact of the subsidy by comparing the pre- and post-2016 changes in the level of the real exports, the share of real exports in total sales, and probability to export between the treatment and control groups. We distinguish the results with respect to the firm size, labor intensity and export intensity of firms and sectors. This study draws on a

rich, firm-level administrative data obtained from the Entrepreneurship Information System (EIS) maintained by the Ministry of Industry and Technology of the Republic of Türkiye.

Therefore, this research makes several contributions to the literature. First, it extends the analysis of export determinants by incorporating labor market policy tools into the framework of trade. Second, it provides new evidence on the trade-related impacts of non-export-contingent subsidies, which are increasingly used in both developed and developing countries as part of broader industrial policy agendas. Third, it contributes to the policy debate on subsidy design by highlighting the channels through which labor cost reductions can influence external competitiveness without violating international trade norms. To the best of our knowledge, our study will be the first, or one of the very few, if there is any, to link wage subsidies to the export decision of firms in Turkey.

The empirical findings of this thesis reveal that the 6-point wage subsidy significantly increased export performance along the intensive margin, particularly for micro firms in moderately underdeveloped regions. However, the policy had no measurable impact on the extensive margin: it did not induce non-exporting firms to enter international markets. These results highlight the role of wage subsidies as a supportive—though partial—tool for strengthening export intensity in structurally constrained settings.

The remainder of this study is organized as follows: Section 4.2 presents a multi-dimensional overview of the composition of Turkish exports. Section 4.3 introduces the data set, while Section 4.4 elaborates on the methodology and

identification strategy. In Section 4.5, we share the result of empirical analyses. Later, Section 4.6 discusses the policy implications and Section 4.7 concludes.

4.2. The Structure of Turkish Exports: A Multi-Dimensional Overview

The structure of Turkish exports reflects a complex interplay of industrial capabilities, firm heterogeneity, regional economic disparities, and external market orientation. Turkey's export base is overwhelmingly industrial: manufactured goods consistently represent over 93 percent of total exports, underscoring the country's integration into global manufacturing networks and its strategic positioning as a bridge between Europe, Asia, and the Middle East.

From a sectoral standpoint, based on the NACE Rev.2 classification, Turkish exports are led by a handful of core industries. The automotive sector (C29), including motor vehicles, trailers, and part, stands out as the top contributor, accounting for approximately 13 percent of total exports. This is followed by machinery and mechanical equipment (C28) at 9 percent, apparel and textiles (C14) at 9 percent, and electrical equipment (C27) at 7 percent. Other significant sectors include basic metals (C24) and chemical products (C20), each contributing around 6 percent, and food manufacturing (C10) at 5 percent, particularly processed agri-foods and packaged goods. This composition reveals both a diversified industrial base and Turkey's role as a key supplier of intermediate and finished goods to the EU and MENA regions.

Complementing the NACE-based industrial lens, the Main Industrial Groupings (MIGs) classification provides an economic use-based perspective. Here, intermediate goods dominate Turkey's export structure, representing roughly 50

percent of total exports. This includes components, subassemblies, and semi-finished products—indicative of Turkey's deep integration into transnational supply chains, particularly in automotive, electronics, and machinery. Capital goods, such as machinery and transport equipment, follow with a share of 22 percent, reflecting the country's growing capacity in engineering-intensive industries. Consumer goods—especially non-durables like clothing and home appliances—make up approximately 25 percent, while energy exports, including refined petroleum and electricity, account for around 5 percent. The share of non-durable goods, such as food and beverages, is modest, comprising about 3 percent. These figures suggest that Turkey's export model is still largely production-oriented rather than resource-driven, with a strategic emphasis on value-added industrial goods.

Export performance is also highly asymmetric by firm size. While Turkey is home to a large population of SMEs, large firms (250+ employees) are responsible for approximately 65 percent of export value. These firms dominate in sectors requiring economies of scale, capital intensity, and strong compliance with international standards, such as automotive, white goods, and defense-related manufacturing. Medium-sized enterprises (50–249 employees) contribute around 25 percent, playing a vital role in sectors like machinery, textiles, and processed food. In contrast, small (10–49 employees) and micro enterprises (1–9 employees) together account for less than 10 percent of exports, reflecting the steep barriers to entry into global markets for smaller, less capitalized firms. This distribution reinforces insights from heterogeneous firm trade models (e.g., Melitz 2003), which predict that only the most productive firms are able to absorb the sunk costs of exporting.

In terms of geographical orientation, Turkey's exports are primarily directed toward European and regional partners. The European Union remains Turkey's largest trading bloc, absorbing approximately 41–43 percent of exports. Key individual markets include Germany, Italy, Spain, and France, primarily in machinery, automotive parts, textiles, and chemicals. Middle Eastern and North African (MENA) countries form the second-largest regional cluster, particularly for textiles, food products, iron and steel, and construction materials. The United States and United Kingdom are increasingly significant non-regional partners, especially for niche high-tech and defense exports. Russia and Central Asia, while less central overall, remain important for energy-intensive and agricultural trade. In recent years, diversification has accelerated toward Sub-Saharan Africa, East Asia, and Latin America, though these markets still represent a small share of total exports.

Domestically, export capacity is highly concentrated in western Turkey, with notable regional disparities in industrial output and logistics infrastructure. Istanbul alone accounts for approximately 40 percent of Turkey's total exports, due to its dense industrial base, concentration of headquarters, and control over port and air logistics. The Marmara region—especially Kocaeli, Bursa, Sakarya, and Tekirdağ—is the country's industrial backbone, specializing in automotive, chemicals, white goods, and electrical machinery. The Aegean region, centered around İzmir, plays a vital role in chemicals, agriculture, and apparel. The Central Anatolian cities such as Ankara and Konya are gaining momentum in defense manufacturing and agricultural machinery. In contrast, Eastern and Southeastern Anatolia—including provinces in Regions 4, 5, and 6 per the Ministry of Industry and Technology's classification—remain underrepresented in export statistics.

These regions suffer from lower industrial density, weaker logistics access, and higher labor informality. The government has attempted to address these disparities through targeted support programs, including regional development incentives, wage subsidies, and infrastructural investments.

In sum, Turkey's export structure is characterized by a dual concentration—sectorally in capital- and intermediate goods, and organizationally among large firms in specific industrial regions. While the country has made progress in diversifying its export destinations and enhancing value-added production, challenges remain in spreading export capacity more equitably across regions and firm sizes. Understanding this landscape is crucial for evaluating the design and distributional impacts of export- and employment-related subsidy programs, particularly those aimed at SMEs or lagging regions.

4.3. Data

This study draws on firm-level administrative data obtained from the Entrepreneurship Information System (EIS) maintained by the Ministry of Industry and Technology of the Republic of Türkiye. The EIS consolidates information from multiple public institutions, including the Social Security Institution (SGK), Revenue Administration (GIB), Turkish Statistical Institute (TURKSTAT), the Ministry of Trade, and other relevant agencies. The resulting dataset offers rich, longitudinal micro-data on firms' financial statements, employment records, geographic location, sectoral classifications, and foreign trade activity.

The dataset covers more than three million registered firms and their registered employees, excluding those operating in the finance and defense sectors.

It offers fine-grained information at both the provincial and district levels, providing detailed insights into firm-level characteristics across time. Although temporal coverage may vary by source, the majority of core variables—including employment, revenues, sector, and location—are available from 2006 onward. Firms in the dataset can be categorized by region, county, subsidy eligibility, industry classification, size, and technological intensity.

The empirical analysis is restricted to the manufacturing sector, defined by Section C of NACE Rev. 2. The focus on manufacturing is motivated by its dominant role in Turkey's export activity, as described in Section 4.2. The sample includes only firms that are liable for corporate income tax and employ at least one registered worker, thereby excluding informal businesses, non-commercial entities, and firms in finance, insurance, and defense. Firms are geographically matched to their respective subsidy region based on administrative location data.

The primary outcomes of interest in this study relate to firm-level export behavior, measured along both the extensive and intensive margins. The extensive margin is operationalized as a binary indicator denoting whether a firm reported export turnover in a given year. The intensive margin is captured through the real export value, measured in constant Turkish lira. For complementary analysis, we also consider real domestic sales and calculate the export-to-total revenue ratio. Export and domestic sales figures are extracted from firms' financial statements[15]. To obtain real values, domestic sales are deflated using the Domestic Producer Price Index (PPI), and export sales using the Non-Domestic PPI, as published by

[15] We only used financial statement data in compiling export and domestic sales revenues. However, micro trade data sets can be used as an alternative to determine the size of the exports for each of the firm.

TURKSTAT. Since non-domestic PPI starts from January 2010, our data span for the empirical analysis is 2010-2019.

To account for firm heterogeneity, enterprises are classified into three size categories—micro, small, and medium-large—according to official definitions set by Turkish authorities. Classification is based on both employment headcount and annual turnover. As for the employment size, firms with 0–9 employees are considered micro-scale. Those with 10–49 employees fall into the small-scale category. Firms employing 50 or more workers are grouped together as medium-large enterprises. This grouping allows for more meaningful analysis of how firm size moderates the effects of labor cost subsidies on export behavior.

We further segment firms by sectoral characteristics, particularly by labor intensity and export orientation. Drawing on the latest Input–Output Tables published by TURKSTAT, we identify food, textiles, leather, apparel, and furniture manufacturing as labor-intensive sectors, due to their high share of labor inputs in total production costs. In identifying export-oriented sectors, we follow the classification outlined in Section 4.2, which highlights apparel, textiles, electrical equipment, motor vehicles, and chemical products as the industries with the largest contributions to national export value. Finally, at the firm level, we differentiate between exporters and non-exporters based on their pre-treatment turnover composition. Firms are classified as exporters if, on average, their real export sales has a share of at least 35 percent in real total sales during the pre-treatment period. This definition ensures that firms' export status reflects persistent market orientation rather than transitory export events.

4.4. Methodology

Although firm-level data from the EIS do not indicate actual subsidy uptake, they do contain detailed geographic identifiers. Accordingly, we define treatment based on policy exposure: firms are considered treated if they are located in counties that became eligible for the 6-point subsidy after the 2016 reform.[16] Eligibility was originally determined at the provincial level, targeting underdeveloped provinces categorized as Regions 4, 5, and 6. In 2020, however, the classification system was updated to reflect county-level development rankings, resulting in the reclassification of 99 previously ineligible counties (administratively part of Regions 1–3) as Region 4 areas (Table A1).

This reclassification in 2020 enables the construction of a credible control group: firms located in these newly reclassified counties share similar socioeconomic profiles with treated counties but remained ineligible for the 6-point subsidy throughout the study period (2010–2019). The treatment group comprises firms in Regions 4–6 that gained eligibility for the 6-point subsidy following the 2016 reform. All firms, treated and control alike, had access to the baseline 5-point subsidy, which remained constant throughout.

To further refine the comparability between groups and reduce development-level heterogeneity, we implement the following sample restrictions:

- We exclude seven counties from the control group that were upgraded from Region 4 to Region 3 in the 2022 Socio-Economic Development Index (SEDI).

[16] This is the same identification used in Chapter III.

- We exclude from the treatment group any counties that has a development level better than Region 4, but included in the subsidy program in 2016 solely due to their affiliated province's classification under Regions 4 -6.

These adjustments help ensure that treatment effects are not driven by systematic differences in underlying economic conditions. We only focus on Region 4 countries in our analyses.[17]

Because the 2016 policy specifically expanded eligibility to micro firms, we do not impose any firm-size restrictions in the main analysis but expect the strongest effects among firms with fewer than 10 employees. We later explore heterogeneity by firm size, sector, and pre-treatment exporter status.

We estimate the following baseline difference-in-differences model:

$$Y_{i,t} = \beta_0 + \beta_1 \text{Treatment}_i * \text{Post}_t + \phi_i + \tau_t + \gamma_{rt} + \delta st + \epsilon_{i,t} \ ,$$

Where,

$Y_{i,t}$ is the outcome variable of interest for firm i in year t

- **For extensive margin**: binary indicator for whether the firm exported
- **For intensive margin:** real export value (in logarithms) and export share in total sales

$\text{Treatment}_i * \text{Post}_t$ is an interaction term equal to 1 for firms located in eligible regions after the 2016 reform. ϕ_i is firm fixed effects, capturing time-invariant firm-specific characteristics. τ_t is year fixed effects, controlling common macroeconomic shocks. γ_{rt} is region (NUTS2) × year fixed effects, δ_{st} is sector × year fixed effects

[17] We exclude Regions 5 and 6 due to insufficient observation.

(based on NACE Rev. 2 2-digit codes), $\epsilon_{i,t}$ is the idiosyncratic error term. Standard errors are clustered at the firm level.

This empirical strategy estimates the Intention-To-Treat (ITT) effect of subsidy exposure, rather than actual take-up. While this may attenuate treatment estimates, it ensures policy relevance. As Bloom (2008) argues, ITT effects are particularly meaningful for policy design when participation is voluntary or unobserved. By incorporating fixed effects at the firm, sector-year, and region-year levels, we control for both time-invariant unobserved firm characteristics and macro-regional trends, thereby enhancing internal validity.

This empirical approach allows us to isolate the policy-induced change in export performance, particularly focusing on how cost-side labor subsidies affect firms' ability to enter foreign markets and expand their presence therein. We estimate heterogeneous effects by firm size, sector, and exporter status in the pre-treatment period. These specifications allow us to evaluate whether the policy had differential impacts on micro vs. larger firms, or on export-intensive vs. domestically oriented firms—a key concern given the policy's stated intent to encourage formalization and competitiveness in underdeveloped areas.

4.5. Empirical Results

This section presents the empirical findings from the DiD estimations that assess the causal impact of the 2016 expansion of the 6-point employer social security subsidy on firms' export behavior in Turkey. The results are analyzed across multiple dimensions: firm size, sectoral characteristics, and regional variation. We examine both the intensive margin (export sales and export share) and

the extensive margin (export participation), and we evaluate the robustness of our findings by testing the treatment effects on domestic and total sales.

All estimates represent ITT effects, as treatment is assigned based on geographic eligibility rather than verified take-up. Standard errors are clustered at the firm level, and all models include firm, year, industry-year, and region-year fixed effects, as described in Section 4.4.

4.5.a. Intensive Margin Results

This subsection presents the empirical results for the intensive margin of export performance, focusing on two key outcomes: real export sales and the share of exports in total firm sales. We begin by examining the impact of the 6-point wage subsidy on firms' log-transformed export sales, followed by an analysis of its effect on the export share, calculated as the proportion of total sales attributed to exports. The analyses are disaggregated by firm size and sectoral characteristics, to uncover potential heterogeneity in policy response.

The findings on real export sales are summarized in Tables 4.1 through 4.5, where the coefficient of the treatment indicator (diff) captures the percentage change in export sales associated with exposure to the subsidy. In contrast, Tables 4.6 through 4.10 report the corresponding results for the export share, where the treatment coefficient reflects the percentage point change in the share of exports in total firm sales, attributable to the policy intervention.

4.5.a.i. Real Export Sales

In Region 4, micro firms experienced a positive and statistically significant 2.1 percent increase in export sales suggesting that the subsidy increased exports among the smallest firms in moderately underdeveloped areas. Small and medium-large firms exhibited positive but statistically insignificant effects, indicating that the policy was not as effective among these groups. This suggests the policy had greater marginal effects in more financially constrained firms, consistent with theoretical predictions that cost-based subsidies should yield higher marginal returns, where firms face tighter liquidity constraints and shallower access to finance.

	Micro firms	Small firms	Medium-large firms
Diff	0.0214**	0.152	0.473
	(0.0106)	(0.214)	(0.578)
Constant	0.0564***	1.257***	4.759***
	(0.00388)	(0.0994)	(0.367)
Observations	131,603	11,945	2,418
R^2	0.568	0.706	0.851

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.1. Real Export Sales by Firm Scales in Treated Region 4 Counties

Table 4.2 reports treatment effects for all firm sizes grouped by sectoral characteristics (labor-intensive sectors, exporter sectors, and firms already classified as exporters in the pre-treatment period). In Region 4, none of the treatment effects for any firm type are statistically significant, although the signs are all positive. This suggests that sectoral classification alone may not determine responsiveness, especially when firm size is not accounted for. Against this background, we run estimations by interacting firms size and sectoral characteristics and present them in Tables 4.3, 4.4 and 4.5.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.0137	0.0621	0.0114
	(0.0295)	(0.0403)	(0.00983)
Constant	0.294***	0.262***	0.185***
	(0.0144)	(0.0200)	(0.00459)
Observations	67,951	26,561	76,374
R^2	0.776	0.767	0.821

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.2. Real Export Sales by Firm Types in Treated Region 4 Counties

Table 4.3 focuses on micro firms, breaking them down by type: In Region 4, only exporter firms display a statistically significant increase in real export sales (1.27 percent). This effect is slightly larger than the average effect for all exporter firms (1.14 percent in Table 4.1), suggesting that micro exporter firms are particularly more responsive to labor cost reductions to a certain extent. On the other hand, surprisingly, labor-intensive micro firms do not show any significant effect, despite the fact that these firms should be most sensitive to reductions in labor costs. For micro firms in exporter sectors, the effect is positive but not statistically significant.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.0188	0.0164	0.0127*
	(0.0168)	(0.0281)	(0.00755)
Constant	0.0508***	0.0614***	0.0454***
	(0.00665)	(0.00821)	(0.00311)
Observations	53,073	21,383	69,237
R^2	0.593	0.674	0.659

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.3. Real Export Sales for Micro Firms by Firm Type in Treated Region 4 Counties

Table 4.4 estimates effects for small firms, divided by sector: In Region 4, treatment effects are statistically significant only for small firms in exporter sectors. Labor-intensive small firms and small exporter firms display positive but statistically insignificant estimates.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.268	0.727*	0.944
	(0.239)	(0.401)	(0.708)
Constant	1.067***	1.334***	1.527***
	(0.0957)	(0.159)	(0.535)
Observations	6,466	1,681	4,890
R^2	0.719	0.742	0.834

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.4. Real Export Sales for Small Firms by Firm Type in Treated Region 4 Counties

Table 4.5 presents results for medium and large firms by sectoral classification. In Region 4, exporter firms exhibit a negative and statistically significant effect (-2.634), however the coefficient is economically not meaningful. Labor-intensive and exporter-sector firms in this region show positive but insignificant results.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	-0.976	-0.933	-2.634**
	(0.733)	(0.862)	(1.147)
Constant	5.668***	3.662***	6.381***
	(0.310)	(0.574)	(1.308)
Observations	1,392	723	1,009
R^2	0.866	0.819	0.924

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.5. Real Export Sales for Medium-Large Firms by Firm Type in Treated Region 4 Counties

The empirical evidence presented in Tables 4.1 to 4.5 reveals that the 6-point wage subsidy had differential impacts on real export sales. The findings show that micro firms in Region 4 responded strongly to wage subsidies, however these have no specific sectoral characteristics. On the other hand, small firms in exporter sectors respond significantly than those in non-exporter sectors. These results collectively suggest that labor cost subsidies function best when targeted to smaller firms, irrespective of their sectoral type. For small firms which have relatively less financially constraints compared to the micro firms, being within an exporting sector may create advantages following the marginal labor cost reductions.

4.5.a.ii. Real Export Share

This subsection evaluates whether the 6-point wage subsidy influenced firms' export orientation, as measured by the share of exports in total firm sales. Tables 4.6 through 4.10 present the estimated treatment effects across different firm sizes and firm types.

Table 4.6 examines the impact of the 6-point wage subsidy on the export share of total sales, disaggregated by firm size for Region 4. Accordingly, micro firms experienced a statistically significant increase in export share (1.38 percentage points), indicating a shift in their sales orientation toward foreign markets. For small firms, the coefficient is positive but small (0.128) and not statistically significant. Medium and large firms show a negative, insignificant estimate (−1.453), suggesting no substantial effect on export orientation. These results suggest that the export-enhancing effect of the subsidy on sales orientation is strongest among micro firms in Region 4.

	Micro Firms	Small Firms	Medium-Large Firms
Diff	1.384**	0.128	-1.453
	(0.671)	(0.611)	(1.622)
Constant	1.695***	3.408***	11.76***
	(0.352)	(0.373)	(1.086)
Observations	131,603	11,945	2,418
R^2	0.603	0.730	0.814

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.6. Real Export Share by Firm Scales in Treated Region 4 Counties

Table 4.7 presents the effect of the subsidy on the export share of total sales for firms categorized by sectoral characteristics, regardless of their size. These categories include labor-intensive sectors, exporter sectors, and firms identified as exporters in the pre-treatment period. In Region 4, all firm types show statistically insignificant coefficients: Labor-intensive: 0.402, Exporter sectors: 1.234, Exporter firms: −1.430. These results suggest that sectoral identity alone does not predict strong or consistent responses in export share, and that size-specific dynamics may be more informative.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.402	1.234	-1.430
	(0.548)	(1.630)	(4.985)
Constant	3.430***	5.735***	4.512
	(0.283)	(1.070)	(3.992)
Observations	67,951	26,561	76,374
R^2	0.708	0.755	0.830

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 4.7. Real Export Share by Firm Types in Treated Region 4 Counties

Table 4.8 disaggregates the export share response among micro firms, further divided by labor-intensive sectors, exporter sectors, and exporter status. In Region

4, all firm types display positive effects, though none are statistically significant: Labor-intensive: 0.996, Exporter sectors: 8.155, Exporter firms: 3.281.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.996	8.155	3.281
	(0.844)	(14.94)	(4.271)
Constant	1.722***	-19.76**	5.080***
	(0.399)	(9.570)	(1.798)
Observations	53,073	21,383	69,237
R^2	0.576	0.743	0.785

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.8. Real Export Share for Micro Firms by Firm Type in Treated Region 4 Counties

Table 4.9 reveals the results for small firms by different firm types. In Region 4, the effects are positive but again statistically insignificant for small firms too: Labor-intensive: 0.548, Exporter sectors: 2.593, Exporter firms: 6.995 (with a large standard error).

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	0.548	2.593	6.995
	(0.591)	(1.725)	(13.68)
Constant	3.014***	3.540***	8.246
	(0.311)	(0.900)	(8.654)
Observations	6,466	1,681	4,890
R^2	0.674	0.730	0.823

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.9. Real Export Share for Small Firms by Firm Type in Treated Region 4 Counties

Table 4.10 focuses on the impact of the subsidy on export share among medium and large firms: In Region 4, all firm types experienced negative and statistically significant effects: Labor-intensive: −5.631, exporter sectors: −8.689, exporter firms: −23.52. Yet, the coefficients are economically not meaningful.

Although imprecise, these estimates indicate potential for negative export share responses among larger firms in disadvantaged regions.

	Labor-Intensive sectors	Exporter sectors	Exporter firms
Diff	-5.631**	-8.689*	-23.52***
	(2.676)	(4.832)	(7.040)
Constant	15.50***	13.43***	24.29***
	(1.390)	(3.596)	(5.543)
Observations	1,392	723	1,009
R^2	0.866	0.819	0.924

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.10. Real Export Share for Medium/Large Firms by Firm Type in Treated Region 4 Counties

The export share analysis reveals a nuanced and size-dependent pattern of firm responses to the wage subsidy along the intensive margin. The most robust and positive response appears among micro firms in Region 4, who experienced a statistically significant increase in export share, suggesting that labor cost relief enabled the smallest firms in moderately underdeveloped areas to reorient toward international markets. Among small firms, in Region 4, the treatment effects are positive across sector types but not statistically significant, implying modest but inconclusive shifts in export orientation. The effects for medium and large firms are even more asymmetric. In Region 4, these firms consistently exhibit large and statistically significant declines in export share, particularly among exporter and exporter-sector firms. This could reflect substitution toward domestic markets, saturation in export capacity, or declining marginal gains from subsidies among firms already operating at scale.

4.5.b. Results for the Extensive Margin

This section evaluates whether the 6-point wage subsidy affected firms' probability of entering export markets—capturing effects on the extensive margin of trade. While the intensive margin results demonstrated significant export gains among certain firm groups already engaged in trade, the extensive margin analysis focuses on export market entry, assessing whether the subsidy enabled previously non-exporting firms to start exporting.

The outcome variable is a binary indicator equal to 1 if a firm engages in exporting in a given year. Tables 4.11 and 4.12 present the DiD estimates across firm size categories and firm type–size interactions (labor-intensive × size), respectively, for Region 4.

Table 4.11 shows that the estimated effects of the wage subsidy on the probability of becoming an exporter are statistically insignificant and economically negligible across all firm sizes in both regions. This suggests that the subsidy did not improve firms' likelihood of initiating exports, regardless of firm scale. Even among small firms—where we observed strong effects in the intensive margin (Table 4.1)—no meaningful export entry effect was found. This is consistent with the notion that export entry involves fixed costs (e.g., learning about foreign markets, adapting products, securing compliance) that go beyond labor cost reductions and require more comprehensive support mechanisms.

	Micro Firms	Small Firms	Medium & Large Firms
Diff	0.00297	-0.00184	-0.0564
	(0.00529)	(0.0350)	(0.0719)
Constant	0.845***	0.268***	0.540***
	(0.00143)	(0.0124)	(0.0236)
Observations	131,603	11,945	2,418
R^2	0.894	0.832	0.900

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.11. Probability to Start Exporting by Firm Scales in Treated Region 4 Counties

The only statistically significant result in Table 4.12 is for labor-intensive medium-large firms in Region 4, where the treatment coefficient is −0.132 ($p < 0.1$), indicating a significant decline in the likelihood of export entry. This could reflect structural rigidities or a misalignment between the subsidy and the needs of larger, cost-sensitive firms in moderately underdeveloped regions. It may also reflect reallocation toward domestic markets in this group, consistent with earlier findings of declining export share among similar firms (Table 4.10). Yet the coefficient is economically not very much meaningful.

	Micro Firms	Small Firms	Medium -Large Firms
Diff	0.00653	0.0312	-0.132*
	(0.00900)	(0.0459)	(0.0704)
Constant	0.826***	0.250***	0.564***
	(0.00267)	(0.0154)	(0.0217)
Observations	53,073	6,466	1,392
R^2	0.907	0.824	0.933

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.12. Probability to Start Exporting for Labor Intensive Sectors by Firm Scales in Treated Region 4 Counties

In all other cases, including labor-intensive micro and small firms in both regions, the estimates are statistically insignificant. This reinforces the view that the

subsidy was not effective in lowering the fixed barriers to export market entry, even among firms most exposed to labor costs.

The evidence across both tables shows a clear absence of extensive margin effects. The wage subsidy, while effective in boosting export intensity (real export sales and export share), did not induce non-exporting firms to enter foreign markets. This finding is consistent with trade models that differentiate between fixed costs of entry and variable costs of operation (Melitz 2003; Chaney 2005; Manova 2013).

Export market entry involves a range of non-wage fixed costs—such as regulatory compliance, product adaptation, foreign certification, marketing, and distribution investments—that are not directly alleviated by labor cost subsidies. Even among labor-intensive and small firms, who are theoretically more responsive to cost-side incentives, the data provide no evidence of extensive margin responsiveness.

In sum, while the subsidy supported export scaling (intensive margin), it had no consistent impact on expanding the exporter base (extensive margin).

4.6. Discussion and Policy Implications

The findings support the theoretical distinction between marginal cost reductions, which affect export scaling, and fixed/sunk costs, which shape export market entry (Melitz 2003; Chaney 2005; Manova 2013). Further, these empirical findings provide critical insights into how firm-level trade behavior responds to labor cost interventions. The results confirm that the smallest firms were the most responsive to wage subsidies. This aligns with existing models of heterogeneous

firms in trade, which predict that only those near the productivity threshold will respond to cost shocks (Melitz 2003).

However, the study also highlights clear limits to the subsidy's reach: Larger firms showed muted or even negative responses in export share, suggesting that they may be less sensitive to marginal cost relief. Firms not already engaged in trade did not respond to the policy in terms of export participation, reinforcing the importance of non-cost barriers to internationalization—such as compliance, logistics, product-market fit, and marketing channels.

Overall, the evidence illustrates that cost-based interventions are necessary but not sufficient to stimulate broad-based trade participation. Subsidies may reinforce existing patterns rather than transform them, particularly in environments with deep structural constraints.

The results offer three core policy implications:

- **Region, firm size, and export capacity should guide subsidy design:** The effects of the wage subsidy were most pronounced in micro firms. This suggests that firm-level targeting is essential to maximize the return on fiscal instruments. A one-size-fits-all subsidy program is unlikely to generate uniform trade gains.

- **Wage subsidies support scaling, not entry:** While the policy helped some firm types expand exports, it did not facilitate new export activity. Export entry probably requires other support mechanisms that go beyond cost reduction—such as trade promotion agencies, export readiness training, and information on foreign demand.

- **Complementary policies are needed to enhance total productivity and resilience:** To ensure long-term competitiveness, wage subsidies should be combined with investments in technology, innovation, and infrastructure, particularly in underserved regions.

This study shows that wage subsidies can be a tool for deepening export engagement. However, their effectiveness is conditional on firms' pre-existing capabilities and external constraints. The 6-point subsidy provides an important case for understanding these dynamics and refining future export promotion strategies in Turkey and beyond.

4.7. Conclusion

This study examined the causal effects of a regionally differentiated wage subsidy policy—the additional 6-point reduction in employer social security contributions—on export performance among manufacturing firms in Turkey. By combining detailed administrative firm-level panel data with variation in policy exposure across time and space, the study sought to evaluate whether and how labor cost subsidies affect firm behavior at the intensive and extensive margins of trade.

Using a DiD framework, the analysis provided robust evidence that the subsidy significantly influenced firms' export activity along the intensive margin, particularly for micro firms. In contrast, the subsidy had no significant impact on the extensive margin: it did not induce non-exporting firms to enter foreign markets.

This research makes several contributions. Empirically, it provides one of the first causal estimates of wage subsidies on export behavior in a middle-income

country context, using administrative microdata. Methodologically, it applies a clean identification strategy based on regional policy variation. Substantively, it highlights the limits of wage subsidies as an instrument for export promotion: while effective at intensifying export activity, they fall short of enabling new market entry.

The findings are not without limitations. The analysis does not measure the cost-effectiveness or fiscal sustainability of the subsidy scheme. Nor does it evaluate long-term productivity spillovers, employment dynamics, or firm survival—important aspects for a full understanding of industrial policy impacts. Additionally, the study focuses on formal manufacturing firms, excluding potentially affected informal or service-sector enterprises.

Future research could address these gaps by exploring dynamic effects of wage subsidies over longer horizons, incorporating employment and investment outcomes, or analyzing the interaction between cost-side and capability-enhancing policies. Comparative studies across countries or policy types could also enrich our understanding of how labor market instruments contribute to trade competitiveness.

In sum, this thesis shows that labor cost subsidies can play a targeted and supportive role in export development, particularly when directed toward financially constrained firms in structurally disadvantaged regions.

CHAPTER V

CONCLUSION

This thesis has examined how wage policies—both in the form of statutory minimum wage increases and targeted labor cost subsidies—affect firm-level outcomes in a developing economy context. Drawing on rich administrative microdata from Turkey and exploiting natural policy variations, the thesis has addressed the labor market, capital investment, and trade-related consequences of these interventions through three empirical chapters. Collectively, the findings provide a multi-dimensional perspective on how firms respond to labor cost shocks and incentives, with implications for labor market regulation, industrial upgrading, and export competitiveness.

Chapter II explored the labor market consequences of Turkey's 2016 minimum wage hike, applying a bunching estimation approach to identify the localized effects of a sharp and unanticipated increase in the wage floor. The analysis revealed significant disemployment effects concentrated among formal, low-skilled, and young workers in wage bins directly affected by the new minimum threshold. These findings are consistent with theoretical predictions that binding wage floors can reduce employment when firms are unable or unwilling to adjust on other margins. The study underscores the importance of targeting and calibrating minimum wage policies in settings where large segments of the workforce earn near the statutory floor.

Chapter III shifted attention to the 2016 reform that expanded eligibility for a 6-point reduction in employer-side social security contributions to include previously excluded micro firms. Using a difference-in-differences framework, the chapter analyzed firm-level responses in terms of employment, capital accumulation, and skill composition. The findings revealed that micro firms responded primarily through job creation, while small firms increased capital investment and capital–labor ratios—particularly in tangible assets. However, the policy had no statistically significant effect on the share or level of skilled employment, suggesting that while the subsidy was effective in stimulating expansion along the extensive margin, it failed to promote workforce upgrading or productivity-enhancing input shifts. These results highlight the limits of untargeted employment subsidies in achieving structural transformation, particularly in the absence of complementary measures that incentivize training, innovation, or technology adoption.

Chapter IV extended the analysis by investigating whether the same wage subsidy reform affected firm behavior in global markets. Focusing on export outcomes, the study found that the policy significantly improved export performance along the intensive margin—with micro firms in moderately underdeveloped areas, increasing their export values and export shares in total sales. However, the reform had no measurable impact on the extensive margin: non-exporting firms did not enter international markets in response to the subsidy. This suggests that while wage subsidies can reduce the marginal cost of expanding export operations, they are unlikely to overcome the fixed entry barriers associated with internationalization on their own.

Taken together, the thesis reveals that wage policies in middle-income, dual labor market economies like Turkey yield highly heterogeneous effects across firms and outcomes. Both statutory wage regulation and fiscal labor incentives have the capacity to shift firm behavior, but the direction and magnitude of that shift depend critically on firm size, informality status, input mix, and existing market participation. The results point to the effectiveness of labor cost reductions in stimulating employment and modest investment among micro and small firms, but also underscore their limitations in driving broader structural upgrading, whether in skill composition or global competitiveness.

These insights carry several policy implications. First, minimum wage hikes must be carefully designed to avoid disproportionate employment losses among vulnerable worker groups. Second, wage subsidies are most impactful when they are targeted at firms that are financially constrained but have growth potential. Third, to achieve long-term productivity gains, employment subsidies may be accompanied by complementary programs—such as vocational training, R&D incentives, or digital adoption support—that help shift firms from survivalist expansion to strategic upgrading. Lastly, effective monitoring and fine-tuning mechanisms are essential to ensure that such subsidies generate additionality rather than merely transferring public resources to firms already operating at capacity.

This thesis also opens avenues for future research. Further studies could explore the dynamic, long-term effects of wage subsidies on firm survival, innovation, or workforce transitions. Other promising directions include evaluating policy interactions between labor subsidies and tax incentives, and examining the spatial spillovers of these interventions on local labor markets.

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APPENDIX

Province	Region	County	Subsidy region after 2020
Adana	3	Yumurtalık, İmamoğlu, Karataş, Karaisali	4
Bilecik	3	Gölpazarı, İnhisar, Yenipazar	4
Ankara	1	Haymana, Bala, Çamlıdere	4
Aydın	2	Kuyucak, Germencil, Karacasu, Sultanhisar, Köşk, Buharkent, Yenipazar, İncirliova, Bozdoğan	4
Balıkesir	2	Savaştepe, Dursunbey, Sındırgı Havran, Kepsut	4
Bolu	2	Yeniçağa, Mudurnu, Göynük	4
Burdur	3	Kemer, Ağlasun, Cavdır, Çeltikçi, Yeşilova, Altınyayla	4
Bursa	1	Haymancık, Keleş	4
Çanakkale	2	Bayramiç, Yenice	4
Gaziantep	3	Nizip, Islahiye, Oğuzeli, Nurdağı, Karkamış, Araban, Yavuzeli	4
Isparta	2	Şarkıkaraağaç, Aksu	4
Karabük	2	Eskipazar, Yenice	4
Kayseri	2	İncesu, Felahiye, Yahyalı, Bünyan, Yeşilhisar, Pınarbaşı	4
Konya	3	Kulu, Sarayönü, Hadim, Taşkent, Güneysınır, Kadınhanı, Doğanhisar, Tuzlukçu, Yalıhüyük, Bozkır, Derebucak	4
Denizli	2	Babadağ, Kale, Beyağaç, Baklan, Güney, Çameli	4
Edirne	2	Enez, İpsala, Meriç	4
Eskişehir	1	Alpu, Günyüzü	4
Kırklareli	2	Demirköy, Pehlivanköy	4
Manisa	2	Saruhanlı, Köprübaşı, Ahmetli, Gölmarmara, Selendi	4
Sakarya	2	Kaynarca, Ferizli	4
Mersin	3	Aydıncık, Mut, Gülnar	4
Samsun	3	Kavak, Havza, Alacam, Yakakent, Salıpazarı, Vezirköprü, Asarcık, Ayvacık	4
Zonguldak	3	Kilimli, Gökçebey	4

Source: Aşık et al. (2022)

Table A.1. Counties which were included under Region 4 after 2020.

	Micro firms	Small firms	Medium-large firms
DID Coefficient	0.0397***	0.0470	-0.530
	(0.00823)	(0.106)	(0.413)
Observations	102,865	7,605	1,142
R-squared	0.738	0.645	0.749
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm employment. *** p<0.01, ** p<0.05, * p<0.1

Table A.2. Firm Employment in Treated Region 5 Counties

	MICRO	SMALL	MEDIUM-LARGE
Panel A: K=Tangible Assets (in logs)			
DID Coefficient	0.0185	0.331	-1.198
	(0.0249)	(0.230)	(0.796)
Observations	102,865	7,605	1,142
R-squared	0.804	0.828	0.859
Panel B: K=Machinery and Equipment (in logs)			
DID Coefficient	0.0327**	0.0841	-0.975
	(0.0136)	(0.215)	(0.628)
Observations	102,865	7,605	1,142
R-squared	0.819	0.867	0.897
Year FE	YES	YES	YES
4 Digit Industry Year FE	YES	YES	YES
Nuts2 Linear Trends	YES	YES	YES
Firm FE	YES	YES	YES

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm tangible assets (Panel A) or firm machinery and equipment (Panel B) . *** p<0.01, ** p<0.05, * p<0.1

Table A.3. Firm Capital Stock in Treated Region 5 Counties

	MICRO	SMALL	MEDIUM-LARGE
	(1)	(2)	(3)
Panel A: K=Tangible Assets (in logs)			
DID Coefficient	0.00435	0.190	-0.325
	(0.0181)	(0.171)	(0.506)
Observations	102,865	7,605	1,142
R-squared	0.793	0.812	0.825
Panel B: K=Machinery and Equipment (in logs)			
DID Coefficient	0.00893	0.0232	-0.231
	(0.00981)	(0.139)	(0.343)
Observations	102,865	7,605	1,142
R-squared	0.803	0.848	0.872
Year FE	YES	YES	YES
4 Digit Industry Year FE	YES	YES	YES
Nuts2 Linear Trends	YES	YES	YES
Firm FE	YES	YES	YES

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is capital intensity (K/L) measured by firm tangible assets (Panel A) or firm machinery and equipment (Panel B). *** p<0.01, ** p<0.05, * p<0.1

Table A.4. Firm Capital Intensity (K/L) in Treated Region 5 Counties

TÜRKÇE ÖZET

Bu tez çalışması, gelişmekte olan ülkelerde uygulanan ücret politikalarının — hem düzenleyici (örneğin asgari ücret) hem de teşvik temelli (örneğin istihdam sübvansiyonları) araçlar olarak — firma davranışları üzerindeki etkilerini incelemektedir. Tezin temel amacı, bu tür politikaların istihdam, sermaye birikimi, işgücü bileşimi ve dış ticaret performansı üzerindeki etkilerini Türkiye örneği üzerinden mikro düzeyde, yani firma bazında analiz etmektir. Türkiye gibi kayıt dışılığın yaygın, işgücü piyasasının çift yapılı (dual) olduğu ve bölgesel eşitsizliklerin ciddi boyutta görüldüğü bir ülkede bu politikaların nasıl çalıştığına dair daha önce yapılmış çalışmalar sınırlıdır. Bu bağlamda, tez kapsamında üç farklı ampirik analiz gerçekleştirilmiş ve her biri tezin birer bölümü olarak sunulmuştur.

Tezde kullanılan veriler, Türkiye Cumhuriyeti Merkez Bankası ve Sanayi ve Teknoloji Bakanlığı başta olmak üzere kamu kurumlarının sağladığı mikro düzeydeki idari kayıtlardan oluşmaktadır. Ayrıca, her bir analizde doğal deney niteliği taşıyan politika değişikliklerinden faydalanılarak yarı-deneysel (quasi-experimental) yöntemler uygulanmıştır. Bu çerçevede, yığılma tahmini (bunching estimation) ve "farkların farkı (difference-in-differences)" gibi çağdaş iktisadi analiz yöntemleri kullanılarak nedensel ilişkiler tahmin edilmiştir.

Tezdeki ilk ampirik çalışma, 2016 yılında Türkiye'de uygulanan yüksek oranlı asgari ücret artışının istihdam üzerindeki etkilerini analiz etmektedir. Bu dönemde net asgari ücret, nominal olarak yaklaşık yüzde 30 oranında artırılmıştır. Bu analizde, istihdam değişimlerinin asgari ücret seviyesine yakın ücret gruplarında yoğunlaştığı varsayımıyla, yığılma tahmini (bunching estimation) yöntemi kullanılmıştır. Bu yöntem, asgari ücretin uygulandığı seviyede ücretlerin "yığılması" olgusunu inceleyerek, istihdam üzerindeki etkileri doğrudan etkilenen ücret aralıklarında gözlemlemeye olanak tanımaktadır. Analiz bulgularına göre, istihdam üzerindeki olumsuz etkiler, özellikle kayıtlı sektörlerde, düşük vasıflı çalışanlar ve genç işgücü gibi kırılgan gruplar üzerinde belirginleşmiştir. Kayıt dışı istihdamda ve kadın istihdamında ise anlamlı etkiler bulunmamıştır. Asgari ücretin yüksek oranda artırılması, bu gruplar için iş kaybı riskini artırmış ve toplam istihdamda sınırlı bir azalmaya yol açmıştır. Bulgular, bu tür politikaların tüm ücret

dağılımı üzerindeki etkilerinden ziyade, doğrudan etkilenen düşük ücret dilimlerinde yoğunlaştığını ve istihdam üzerindeki negatif etkinin daha çok kısa vadede belirgin olduğunu göstermektedir. Uzun vadede olumsuz etkiler ortadan kalkmaktadır.

İkinci ampirik çalışmada ise, yine 2016 yılında uygulamaya alınan ve işverenin sosyal güvenlik primi yükünü azaltmayı amaçlayan 6 puanlık ücret sübvansiyonu analiz edilmiştir. Bu reformla birlikte, daha önce bu destekten yararlanamayan 10 kişiden az çalışanı olan mikro işletmeler, 6 puanlık ek prim indiriminden faydalanmaya başlamıştır. Bu politika, Türkiye'nin daha az gelişmiş bölgelerinde kayıtlı istihdamı artırmak amacıyla uygulamaya konulmuştur. Bu analizde, firmaların sübvansiyona maruz kalıp kalmadığı, bulundukları ilçelerin politika öncesi ve sonrası sübvansiyon uygunluk durumlarına göre belirlenmiştir. Sübvansiyonun uygulanmaya başlandığı 2016 yılı öncesinde uygun olmayan ama 2020 yılında uygun hale gelen ilçelerdeki firmalar, kontrol grubu olarak kullanılmıştır. Böylece, sosyoekonomik gelişmişlik açısından benzer özelliklere sahip ama politika etkisine maruz kalmamış firmalarla karşılaştırma yapılabilmiştir. Farkların farkı yöntemi ile yapılan analizde, mikro ölçekli firmaların istihdam düzeylerini anlamlı şekilde artırdığı, buna karşılık vasıf kompozisyonunun değişmediği tespit edilmiştir. Küçük ölçekli firmalar ise sermaye birikimi göstergelerinde artış göstermiştir. Ancak dikkat çekici biçimde, orta ve büyük ölçekli firmalarda herhangi bir istihdam ya da yatırım etkisi gözlenmemiştir. Bu bulgu, firmaların sübvansiyonlara verdikleri tepkinin sadece sübvansiyonun mali boyutuna değil, aynı zamanda firmanın büyüklüğüne, yapısal kapasitesine ve sermaye-yoğunluğuna bağlı olduğunu göstermektedir. Ayrıca, herhangi bir beceri kompozisyonu değişimi gözlemlenmemesi, bu tür sübvansiyonların istihdam artışını teşvik ettiğini ancak niteliksel dönüşüm sağlama konusunda sınırlı etkili olduğunu ortaya koymaktadır.

Tezin dördüncü ve son ampirik bölümü, aynı 2016 yılı ücret sübvansiyonu reformunun firma düzeyinde ihracat performansı üzerindeki etkilerini incelemektedir. Bu analizde, firmaların sübvansiyona erişimlerinin ihracata giriş (extensive margin) ve ihracat hacmini artırma (intensive margin) üzerindeki etkileri araştırılmıştır. Analiz bulguları, ihracata giriş (yani ilk kez ihracat yapma) açısından

anlamlı bir politika etkisi gözlemlenmediğini göstermektedir. Ancak, mikro firmalar açısından ihracat hacminde anlamlı artışlar gözlenmiştir. Bununla birlikte, sübvansiyonun ihracatçı olmayan firmaları dış pazara girmeye teşvik etmede yetersiz kaldığı tespit edilmiştir. Bu durum, ihracata girişin yalnızca maliyet avantajlarıyla değil, aynı zamanda bilgi, kapasite ve ağ (network) gibi daha derin yapısal gerekliliklerle ilişkili olduğunu ortaya koymaktadır.

Bu tez, Türkiye örneğinde iki farklı politika aracını inceleyerek, ücret temelli işgücü piyasası müdahalelerinin çok boyutlu etkilerini ortaya koymaktadır. Bulgular, bu politikaların firma davranışı üzerindeki etkilerinin ölçek, sektör, coğrafi konum ve finansal kısıtlar gibi faktörlere bağlı olarak değiştiğini göstermektedir. Politika yapıcılar için önemli çıkarımlar şunlardır:

- Asgari ücret artışları, düşük ücretli ve kayıtlı çalışanlar üzerinde olumsuz etkiler yaratabileceğinden dikkatle planlanmalıdır.
- Ücret sübvansiyonları, özellikle mikro ve küçük ölçekli firmalarda istihdam ve yatırım artışı sağlayabilir; ancak verimlilik ve beceri gelişimi gibi uzun vadeli hedefler için tamamlayıcı politika araçlarına ihtiyaç vardır.
- İhracat kapasitesini artırmak için sadece maliyet düşürücü politikalar değil, yenilikçilik, dijitalleşme ve insan sermayesi yatırımları gibi yapısal reformlarla desteklenmiş bütüncül bir yaklaşım gereklidir.

Bu yönleriyle tez, hem Türkiye özelinde hem de benzer sosyoekonomik özellikler taşıyan ülkeler için işgücü piyasası politikalarının etki analizine katkı sunmaktadır.

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- CBRT Working Paper Series, No: 21/03. Download

"The Role of Imported Inputs in Pass-through Dynamics" with Dilara Ertuğ, Pınar Özlü, Mustafa Utku Özmen, 2020, CBRT Working Paper Series, No: 20/03. Download

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