THE EFFECTS OF LABOR MARKET POLICIES ON FIRMS AND WORKERS: EVIDENCE FROM ITALY

Laura Montenovo

Submitted to the faculty of the University Graduate School in partial fulfillment of the requirements for the degree Doctor of Philosophy in the Paul H. O’Neill School of Public and Environmental Affairs, Indiana University June 2023
Accepted by the Graduate Faculty, Indiana University, in partial fulfillment of the
requirements for the degree of Doctor of Philosophy.

Doctoral Committee

__________________________
Bradley Heim, PhD

__________________________
Seth Freedman, PhD

__________________________
Dan Sacks, PhD

__________________________
Kosali Simon, PhD

__________________________
Bruce Weinberg, PhD

__________________________
Coady Wing, PhD

May 16, 2023
For my amazing advisors, my loving parents, and Billie.
Acknowledgments

Many contributed to this 5-year long effort. I start by thanking my advisors. Professor Brad Heim always guided me through the steps of research and taught me how to proceed in a well-organized manner, advising me on what should be happening next, and showing me to never panic whenever the road was getting bumpy. I will never forget his sentence: “This is the best job in the world: to think about things we are interested in and write about them.” Professor Coady Wing, thank you for being the biggest fan of my research ideas, your support made me always walk out of your office with a smile whenever I walked in discouraged. You taught me to be a storyteller and you guided me through my massive dataset. You showed me what genuine enthusiasm for research looks like. Professor Kosali Simon is the closest person I know to a superwoman. I still cannot wrap my head around how she does it all. Thank you for trusting me enough to introduce me to amazing research teams and for providing me with the support and networking that made me grow so much as a researcher. Professors Seth Freedman, Dan Sacks, and Bruce Weinberg: for years you have given me great and prompt advice on my work, my presentations, my econometric techniques, and my job market. Outside of my committee, many professors contributed to this result. Professors Alex Hollingsworth, Justin Ross, Denvil Duncan, and Amanda Rutherford: you always believed in me, mentored me, and supported me. I am grateful. Next, I wish to thank my friends, starting from those across the pond. Thank you, Eugenia, for your wisdom, decisive advice, the sense of protection you have for those you love, your cute little gift packages, for sharing my taste in drinks, and for your movie recommendations. I cannot wait to finally go to the Caribbean together, but for now, let’s enjoy London! Thank you, Ale, for your understanding of all my bad adventures, your smiles, and energy, our amazing New Year’s Eve together, and for handling my hand squeezing during the soccer European Cup final. Thank you, Irene, for being my very best friend, and one of the main reasons I have always been excited to go back home. Our happy hour and sushi nights are among my
favorite things in life. You are one of the best people I know, and I hope you will keep being 
my reference point till my last day. Thank you, Giulia, for walking side by side with me 
in this crazy adventure, for deeply understanding the toughness of what we went through, 
for being the person I have the most fun with, for sending and listening to the longest voice 
notes (true highlight of our day), and for not letting academia ever change you. We see each 
other once a year, but every time it is like no time has passed. I am so blessed to have you 
in my life. To my friends in the U.S.: you have been my family away from home. Thank you 
for giving me all the love and support I needed to function well in a different country. Ruth, 
you have been my mentor, my friend, my cook (who did not allow me to starve on some job 
market days), the best auntie my dog can ask for, and the perfect person to share food and 
drinks with. I want us to stay this close, or closer. Wes, Pat, Maddy, Katie, Andrey, John, 
and Ashley: thank you for the fun, the walks, working together, and the mutual support we 
gave each other during these years. Veronica, you leaving Bloomington was one of the worst 
moments during my Ph.D. I will always cherish our dinners, you being always there, and 
the amazing person you are. Jen, thank you for being one of the most trustworthy people 
I know, for being wise, and for being my binge watching TV-shows buddy. Ash, thank you 
for being my family in Chicago and my escape shelter when I much needed it. I want to 
thank my puppy. While he is totally unaware of it, he helped me make it through this 
program. Our walks, our indissoluble bond, and his ability to be unconditionally loved by 
virtually everyone he meets have been key in overcoming difficult moments. Billie, thank 
you for being the most special dog (and many would agree). I wish to thank my family. I 
cannot have asked for more support and love. Dad, thank you for being so proud of me. 
Mom, thank you for the infinite motherly love you have shown me all my life. My parents 
and grandparents are the reason why I can be at this point today. I very much appreciate 
your unconditional love, and I treasure it. I truly cannot imagine what my life would look 
like without you in it. I love you. Finally, I want to thank me, for being my own rock when 
far from home and for pushing through all the challenges.
Preface

A significant share of the literature in labor economics has focused on causally estimating the impact of public programs and labor regulations on social welfare through their effects on the outcomes of workers and firms. When designing labor policies and programs, policymakers are likely to have quite clear in mind the desirable outcomes they hope to achieve. These include highly productive firms, employers flexible to adjust their production inputs (labor and capital), high-quality employment matches, low unemployment and underemployment, high job stability among workers, and adequate wages. Nevertheless, in most cases, policymakers face tradeoffs between these wanted effects.

For example, employment protection regulations, which essentially increase the monetary and non-monetary costs of terminations for employers, have the stated goal of improving job stability among workers. However, these regulations may also lock workers and firms in low-productive employment relationships as well as increase the divide between workers in protected and stable jobs and those in unprotected and temporary occupations, hence aggravating socio-economic inequalities. Another example, unemployment insurance, is a program providing income to workers who have recently lost a job, for some period of time and under eligibility rules that widely vary by country. The stated objective of the unemployment insurance program is to provide some consumption continuity to those who, by losing their job, experience a sudden drop in income. However, this program comes with unintended consequences as well. For example, previous work has found that unemployed individuals engage in moral hazard behavior, like adjusting their job retention and job search effort in a way that maximizes their reliance and dependence on the program.

The three essays included in this dissertation focus on the issues outlined above, by providing causal evidence that is helpful to better understand the nature of these tradeoffs. In them, I exploit quasi-experimental variation arising from recent reforms in Italy and I estimate the causal impact of changes in employment protections and unemployment insurance
design on firms and workers. By exploring the implications of tweaks to the design of these programs on several firms' and workers' outcomes, I show how policymakers can increase wanted effects while decreasing the undesirable ones.

The first and second essays relate to a change in employment protection regulations. In 2012 and 2015, two Italian reforms weakened employment protections for firms of size 16 and above, which have been historically subject to much stricter protections compared to their smaller counterparts. While differences in employment protection restrictions by firm size remain, the two reforms reduce the magnitude of such sharp discontinuity in termination costs. In the first two chapters, I estimate the implications of such policy changes on firms’ and workers’ outcomes.

In the first essay, I study the impact of the reforms weakening employment protections on firms’ size, dynamism, and productivity outcomes. First, I rely on a bunching design and find evidence of a statistically significant size distortion in the period preceding the reforms, in that firms strategically accumulated below the size threshold of 16 to avoid the sharp increase in termination costs. Moreover, I find that over the period of the reforms, the size distortion disappears. Second, using both descriptive and empirical evidence based on a difference-in-differences model, I find that, in the short term, treated firms shrink in size, and the delayed effect of the reforms is that they expand. Third, my results indicate that firms that were most constrained by the reforms experienced a positive and significant increase in dynamism, as defined by the absolute value of their yearly change in size. This result signals an increase in the flexibility of employers to expand and shrink their size as needed. Fourth, I show that firms experiencing a drop in protections also become more productive, likely through an increase in labor productivity, rather than other broader measures of a firm’s productivity.

In the second chapter, I turn to the effects of the two employment protection reforms on workers. I find that while weakening employment protection restrictions for firms of size 16 and above increases their termination rates, it boosts their hiring rates more dramatically.
Moreover, I estimate that the reforms increase job-to-job switches, but these results are less robust to different specifications. Next, I explore the impact of the reforms on job match quality, and I do not find strong evidence that the policy changes improve job match quality as measured by four different outcomes: tenure of at least two years, increase in job rank, earnings on the first week at the new job, and weekly earnings. Finally, I explore heterogeneous treatment effects for the hiring, termination, job transfers, and job match quality outcomes by the location of the firms, as well as the age and gender of the workers. My analysis broadly highlights an overall lack of heterogeneous treatment effects along these dimensions for all outcomes considered, with one exception. In fact, firms in the North of Italy appear to increase hiring by a lot more than their counterparts in the Center-South as a result of the reforms.

Finally, in the third essay, I consider a different Italian program. The Mobilità program was an unemployment insurance program started in 1991 to support workers who have been part of layoffs, often in mass, due to large restructurings of the firm or severe economic crises. The generosity of the Mobilità program as measured by the *duration* of the benefits (i.e., number of months) was heterogeneous based on the age of the worker and on the Italian macro-region where the firm was located. Older workers and those terminated in the South of Italy, the relatively underdeveloped macro-region of the country, were allowed more maximum months of Mobilità, with older workers in the South having access to four times more maximum months of benefits compared to younger workers in the Center-North. A 2012 reform reduced the Mobilità benefits and progressively alleviated the sharp age and geographical differences in generosity by eliminating the geographical heterogeneity and dramatically lowering the age-based one. I use a difference-in-differences design to study the effect of the 2012 reform on the take-up of the benefits as a first-stage effect analysis. I find that, as expected, the reform strongly decreased the days of Mobilità benefits recipiency. Then, I estimate the causal effect of the reform on migration patterns. Longer unemployment insurance benefits in the South could, on the one hand, incentivize workers to remain there
to keep benefiting from the higher generosity should they become unemployed again in the future. On the other hand, however, the unemployed, who are likely to lack the liquidity needed to relocate, may use the unemployment insurance income to finance the move towards the Center-North, historically characterized by stronger local labor markets. I show that shortening the duration of the benefits significantly decreased the probability of migration, providing evidence in support of the latter explanation: as the reform decreases the months of benefits, workers are less likely to have the financial resources to migrate and they become “stuck” in the South.

Overall, the three chapters in this dissertation focus on labor market regulations and programs whose reforms are highly debated across the world. By relying on causal identification strategies, rich datasets, and a vast set of firms’ and workers’ outcomes, I am able to investigate the broad implications of the labor market reforms I consider in the hope to provide helpful and comprehensive evidence to all policymakers who are considering dialing up, shrinking, or tweaking the employment protection regulations and unemployment insurance programs in their countries or local areas.
Laura Montenovo

THE EFFECTS OF LABOR MARKET POLICIES ON FIRMS AND WORKERS: EVIDENCE FROM ITALY

In this dissertation, I estimate how the features of major and ubiquitous labor market programs are causally associated with several firms’ and workers’ outcomes. For the causal identification of the effects, I exploit two recent Italian reforms that weakened employment protections for firms above a certain size and one that differentially changed the generosity of unemployment insurance by subgroups of workers in Italy, hence providing quasi-experimental settings. In Chapter One, I investigate how weakening employment protections impacts firms’ size distribution, their dynamism as measured by the tendency to change in size, and their productivity. In Chapter Two, I consider the same reform as the treatment, but I quantify its effects on workers by exploring how the national decrease in termination costs affects workers’ labor market outcomes. Hence, I explore hiring, termination, job transfers, and job match quality outcomes overall and by age, gender, and location of the workers. Finally, in Chapter Three I examine the effects of a reform that geographically equalizes the duration of unemployment insurance payments. If lowering unemployment insurance duration decreases internal migration rates, then the results provide evidence that the unemployed are liquidity-constrained and use the unemployment insurance payments to fund their relocation to different local labor markets. If, instead, the reform increases migration rates, then the findings would point to the dependency of workers on the generosity of public programs, and to their strategic behavior to remain where these are more generous. With the results across the three essays, my goal is to provide rigorous evidence about what features of the policies I consider lead to the desired effects, and to what extent and how any unwanted impacts emerge. By causally identifying such implications across both participants in the labor market, firms and workers, I am hoping to provide useful and comprehensive policy recommendations regarding the design of employment protections and of the unemployment insurance program.
Contents

Acceptance Page                             ii

Dedication                                  iii

Acknowledgments                             iv

Preface                                     vi

  Bibliography                               x

Abstract                                    x

1 Employment Protection, Dynamism, and Productivity  1

  1.1 Introduction                           2
  1.2 Literature Review                      5
    1.2.1 Employment Protection and Firm Size 5
    1.2.2 Employment Protection and Firm Productivity 6
  1.3 Employment Protections in Italy        8
    1.3.1 Protection Laws Before the Reforms 9
    1.3.2 The Implications of the Statute of Workers Rights 10
    1.3.3 Background on the Reforms          12
    1.3.4 The Fornero Reform                 12
    1.3.5 The Jobs Act                       14
  1.4 Theoretical Framework                  15
  1.5 Empirical Strategies                   24
    1.5.1 Bunching Estimation                 24
    1.5.2 Difference-in-Differences Estimation 26
  1.6 Orbis Data                             28
### Table of Contents

2.7.2  Job Match Quality ................................................. 104
2.7.3  Firm Location ...................................................... 108
2.8     Results ............................................................. 110
2.9     Conclusion .......................................................... 121
Bibliography ............................................................ 123
2.10    Appendix ............................................................. 127

3  Do Lower Benefits Prevent Relocations?  The Effect of Unemployment Insurance Generosity on Migration 134

3.1  Introduction ............................................................. 135
3.2  Literature Review ...................................................... 139
3.2.1  Unemployment Insurance, Unemployment Spells, and Labor Market Outcomes ...................................................... 139
3.2.2  Welfare-Induced Migration and Unemployment Insurance .............. 140
3.3  Unemployment Insurance system in Italy .................................. 143
3.4  Data ................................................................. 145
3.5  Outcomes and Empirical Framework ..................................... 150
3.6  Results ................................................................. 155
3.6.1  Descriptive Evidence ............................................... 155
3.6.2  Results from Regression Models .................................. 163
3.7  Conclusion .............................................................. 168
Bibliography ............................................................. 172
3.8  Appendix ................................................................. 174
3.8.1  Event Studies ......................................................... 174
3.8.2  Mobilità Spells ...................................................... 180

Curriculum Vitae
Chapter 1

Employment Protection, Dynamism, and Productivity

Laura Montenovo

Abstract

I study the effects of employment protection legislation on the size, dynamism, and productivity of firms. Using multiple identification strategies and a panel dataset of about 130,000 Italian firms, I investigate a pair of reforms that in 2012 and 2015 restricted labor courts’ authority to impose high termination costs on firms with 16 or more employees. I find that decreased employment protections alter the size distribution of firms and their performance. Prior to the reforms, firms were disproportionately likely to hire 13, 14, and 15 employees, avoiding the stricter employment protections. This excess mass of firms dissipated following the reforms, suggesting that they became less constrained. The reforms also increased firm dynamism, as measured by their rates of employment change, and firm productivity, determined by per-employee operating revenues, sales, and value added. These findings reveal the implicit costs of employment protections for firms and show evidence of a pent-up demand for labor in a historically highly regulated labor market.

Keywords: Employment Protections; Firm Size, Firm Dynamism; Productivity. JEL codes: L2; J21; J23; L11; L25; L5; J24; J83

I thankfully acknowledge funding from the Institute of Humane Studies.
1.1 Introduction

Employment protection policies, a set of measures regulating the termination of workers, are in place in over 100 countries across the globe (International Labour Organization, 2020). These laws benefit workers by enhancing their job stability and lessening their vulnerability to changing economic conditions and their own misconduct. However, especially when strict, employment protection legislation creates costs for firms and limits their productivity and scale. If firms behave strategically to avoid the higher termination costs, employment protection can result in fewer job openings, negatively affecting workers. Employment restrictions can also reduce aggregate productivity by locking firms into unproductive employment matches and hampering dynamism, the tendency of high-productivity firms to expand and of low-productivity firms to shrink.

In Italy, labor courts are an authoritative labor market intermediary. Beginning in the 1970s, judges could order firms with 16 or more employees to reinstate terminated workers and pay them compensation for lost wages and other damages. Following the financial crisis, Italy scaled back the power of the labor courts. The changes were implemented in two separate laws: the Fornero Reform in 2012 and the Jobs Act in 2015. These reforms are controversial. The concern is that they reduce job security for incumbent workers. However, the Italian government argues that they were necessary to foster the growth and dynamism in the economy. In Montenovo (2022), I examine the impact of these policies on workers.

This paper studies how the two reforms shape the size distribution, dynamism, and performance of firms. Using a quasi-experimental framework, I quantify the distortions firms experience in a strict employment protection regime and evaluate whether the reforms have reached some of their stated goals. I rely on Orbis data, which contain detailed firm-level information on the size and productivity of a large panel of Italian firms between 2010 and 2019. I use bunching designs to provide evidence on the size distortion of firms, and difference-in-differences models to show that weakening employment protection increases firm dynamism and labor productivity.
My first set of analyses uses a bunching design to demonstrate the presence of an excess mass of firms just below the 16-employee threshold in the pre-reform period, implying that they stayed small to avoid stricter employment protections. The bunching estimates indicate that the size distortion dissipates over the reforms’ implementation period. Descriptive evidence illustrates that after the Fornero Reform, firms above the threshold shrank, and those just below it expanded. In the aftermath of the Jobs Act, firms of all sizes increased, on average.

My second set of analyses investigates the impact of the reforms on the size, dynamism, and productivity of firms in a difference-in-differences framework. Treated firms are those above the threshold, and control firms are those below it. Because firms of size 13, 14, and 15 are indirectly constrained by the restrictions, I consider alternative control groups to check for the robustness of my results. Estimates from the difference-in-differences models suggest that the Fornero Reform reduced the size of firms between 16 and 25 workers by almost 6% and the Jobs Act rebounds that effect by about 1%. To operationalize the economic concept of dynamism, I use data on firms’ yearly changes in employment. I assign firms around the cutoff to the treatment group because they are those most constrained by the design of the regulation, and I find that the reforms enhanced their dynamism by 10%. I observe graphical evidence of higher per-employee productivity among firms just below the cutoff, suggesting that the sharp increase in termination costs at the threshold prevents them from making hires that would be profitable. Finally, I show that the reforms lead to higher labor productivity per employee for the affected firms. The Fornero Reform increased operating revenues by over 3% and the Jobs Act magnified that effect by 2.5%. Both policies raised sales by 5%. Profit margin boosted by 33% after the Fornero Reform, but the Jobs Act nullifies that improvement. The Fornero Reform lead to a 3% higher added value among the impacted firms.

This work contributes to existing knowledge on the relationship between employment protections and economic outcomes in several ways. First, I estimate whether two sequential
reforms that progressively relax employment protections in a historically strictly regulated labor market accomplish their stated goals. I measure the immediate and deferred impact of weakening employment protections on the size and performance of firms. These results shed light on the role that the timing of the reforms play in triggering a pent-up demand for labor. Second, to my knowledge, this is the first work that considers the effects of the reforms on a wide range of size and productivity outcomes among firms. Previous work has either focused on the impact of employment protections on size (Schivardi and Torrini, 2004; Bornhäll et al., 2017; Almeida and Carneiro, 2009) or on productivity (Garicano et al., 2016; Hijzen et al., 2017; Bratti et al., 2021). Third, I come up with a measure for dynamism. Quantifying this key but vague economic concept allows me to estimate how the reforms changed the flexibility of too-small firms to expand and that of too-large firms to shrink. Finally, I consider two recent reforms to estimate the effects of employment protections on a set of economic outcomes, while previous work either studies the relationships among these variables without exploiting policy changes (Schivardi and Torrini, 2004; Amendola, 2014; Garicano et al., 2016; Bratti et al., 2021) or refer to much earlier policy changes (Boeri and Jimeno, 2003; Garibaldi et al., 2004; Bornhäll et al., 2017).

With this paper, I help clarify the stakes involved in the design of employment protection regulation and contributes to the debate on its appropriate level and design by quantifying repercussions for firm dynamism and profitability. My evidence indicates that employment protections do indeed dampen the economic performance of firms, and that they are constrained when its design is based on a size cutoff. Hence, employment protection laws that are rigid and contingent on size may need to be reshaped to ensure their desired effects. Finally, the results add to the literature on the implications of policy design by investigating the distortions arising from a ubiquitous design type that imposes sharp cutoffs.

The paper proceeds as follows. Section 3.2 reviews the relevant literature on employment protection legislation and its impact on size and productivity. Section 3.3 provides details on employment protection legislation in Italy and the two recent reforms. In Section 1.4, I set
up a theoretical model, and in Section 1.5 I illustrate the empirical methodologies. Section 3.4 describes the firm-level data I use in the analyses. Section 3.6 reports the graphical and empirical results, and Section 3.7 concludes.

1.2 Literature Review

First, I review the existing literature on the relationship between employment protection and firm size. Then, I turn to prior work examining how employment regulations impact firm productivity.

1.2.1 Employment Protection and Firm Size

Several papers have studied the effect of discontinuities in termination costs by firm size on labor market outcomes and on firm behavior. Overall, the literature shows that firms respond to firm size cutoffs by altering their hiring and termination decisions, which in turn shape workers’ labor market outcomes. Firms behave strategically to control their size and avoid higher costs. Schivardi and Torrini (2004) exploit the design of employment protections that impose stricter rules on firms with 16 or more employees in Italy and find that right below that threshold firms are 2 percentage points less likely to grow. These results are confirmed by Garibaldi et al. (2004), who, in the same context, show that firms close to the threshold are more reluctant to grow and are 1.5% more persistent, that is, more likely to maintain their size. Further, Amendola (2014) uncovers that the distribution of Italian firms deflects after the 15-worker cutoff. Similar results arise in France, where Garicano et al. (2016) find a sudden drop in the number of firms precisely at 50 employees, corresponding to a sharp increase in termination costs.

To my knowledge, four papers have considered the impact of a change in the strictness of employment protections on the size distribution of firms. Garibaldi et al. (2004) find that a 1990 Italian reform tightening restrictions for firms with less than 16 employees increases their reluctance to change in size. Boeri and Jimeno (2003) focus on the same reform but
investigate layoff rates and growth in addition to firm persistence. The authors find that the policy changes mildly increased the persistence of small firms but left their average size unchanged. Bornhäll et al. (2017) consider a Swedish reform that weakened regulation for firms with less than 11 employees. They find that the reform increased the size of firms with 5 to 9 employees, while firms just below the threshold were 3.4 percentage points less likely to expand. Almeida and Carneiro (2009) show that the level of legislation enforcement constrains firm size in Brazil.

1.2.2 Employment Protection and Firm Productivity

Employment protection laws can impact productivity primarily through two channels: firm size and suboptimal workforce composition. If employment protection impacts size, and size has a causal effect on productivity, then employment protection alters firm productivity through its effect on size. Alternatively, employment protection laws could change productivity by limiting firms’ ability to adjust their workforce and forcing them to rely on a suboptimal composition of labor. Firms may be stuck with less productive workers or too many temporary workers who are not subject to restrictions, leading to less training and specialization. Finally, if effort is endogenous, employment protections may decrease the productivity of workers if they choose to exert less effort because their job is more secure.

Overall, existing evidence on the impact of firm size and productivity is mixed. Pagano and Schivardi (2003) find a significant positive relationship between firm size and productivity growth in Europe, which they explain through differences in R&D-related returns. Similarly, Tovar et al. (2011) show that the size of electrical companies in Brazil positively contributes to their total factor productivity. The same relationship is found in North America, where Leung et al. (2008) conclude that different firm size distributions explain half of the Canada-U.S. gap in labor productivity. Using U.S. cross-sectional industry data, Acs et al. (1999) find that industries characterized by higher market share present greater productivity and total factor productivity growth. The positive link between firm size and
productivity also holds in Sub-Saharan countries (Van Biesebroeck, 2005).

There is also evidence suggesting that firm size negatively impacts productivity, at least in some contexts. Kim et al. (2009) find that in both the pharmaceutical and semiconductor industries patents per R&D dollar decrease with firm size, while the number of patents per inventor increases with size only in the semiconductor industry. However, a related paper shows how the relationship between firm size and R&D productivity may depend on the technological regime (Revilla and Fernández, 2012). De and Nagaraj (2014) spot a strong negative correlation between size, measured by asset ownership, and productivity, defined as research investment and liquidity, among Indian manufacturing firms. These results are reinforced by Diaz and Sanchez (2008), who show that small and medium Spanish firms are more efficient than larger ones, possibly because it is easier for small firms to exit the market during economic hardships. Similarly, Dhawan (2001) concludes that small U.S. firms are significantly more productive, but that their higher flexibility and efficiency comes with economic risk.

As for the role of suboptimal workforce composition, there is overwhelming evidence that employment protection motivates firms to hire temporary workers and provide less training, hindering productivity. Martin and Scarpetta (2012) review the literature on the topic and highlight that stricter employment protections deter productivity growth through a lower reallocation of workers. Bratti et al. (2021) show that after an Italian reform dropped employment protections for firms with 16 or more employees, firms above the cutoff reduced worker turnover and temporary work, increasing the number of trained workers by about 1.5. Similar findings arise in Hijzen et al. (2017), who, in the same setting, find that fewer employment protections are associated with drops in labor productivity through the greater incidence of temporary work.

In the U.S., stricter regulations reduce employment flows and firm entry rates, possibly decreasing productivity and incentivizing firms to engage in capital deepening, which depresses total factor productivity (Autor et al., 2007). In the OECD countries, industries
with more binding layoff restrictions are characterized by lower productivity growth (Bassanini et al., 2009). Similarly, using cross-country data, Micco and Pagés (2007) conclude that more stringent employment protection decreases value added through the lower entry of firms. Garicano et al. (2016) show that in France, employment protection decreases productivity by a magnitude equivalent to a 2.3% variable tax of labor and GDP by about 3.4%. In Japan, variation in court decisions on employment protection explains differences in firm-level total factor productivity growth (Okudaira et al., 2013).

My work contributes to the literature in several ways. First, it uses two recent reforms to estimate how a change in employment protection restrictions impacts firm size and productivity, while previous work either studies relationships among these variables without exploiting policy changes, or refers to much earlier policy changes. Second, by considering two policies that progressively weakened the regulations after a long period of rigidity, I observe how firm behavior unfolds over time as both policies are implemented, and I evaluate whether their reaction to the first policy differs from their reaction to the second. Third, by building a measure of dynamism, I am able to quantify firms’ ability to change size as well as to investigate the impact of the reforms on this novel outcome. Last, to my knowledge, this is the first work investigating the impact of employment protection legislation on both firm size and productivity.

1.3 Employment Protections in Italy

The evolution of employment protection regulation in Italy is long and complex, but I focus on its three major regimes: the “Statute of Workers Rights” (L. May 20, 1970, n. 300), the Fornero Reform (Decree 92/2012), and the Jobs Act (Decree 23/2015). These laws implicitly determined firms’ hiring and termination costs and introduced sharp differences based on firm size, defined by number of employees.

As an illustration of the magnitude of the two recent reforms, the OECD Employment
Protection Strictness Indicator for Italy\textsuperscript{1} was 3.02 between 1991 and 2012; it dropped by 2.98\% after the Fornero Reform and 15.7\% following the Jobs Act. When I consider countries with non-missing data in both years, Italy went from the 83rd percentile in 2011 down to the 72nd in 2018.

\subsection*{1.3.1 Protection Laws Before the Reforms}

In 1970, the Italian government implemented the “Statute of Workers Rights” to regulate employment protections; it mandated that firms with 16 or more employees must have a “just cause” for termination.\textsuperscript{2} In the absence of a just cause, which amounted to either an economic reason (e.g. plant closure or the impossibility of relocating a worker within the firm) or a disciplinary reason (e.g. worker misbehavior), the termination would be considered unjust. Between 1970 and 1990, only firms with 16 or more employees had to obey the just cause rule, but a 1990 reform introduced the rule for all firms regardless of size. As a consequence, beginning in 1990, all terminated workers could sue their employer.\textsuperscript{3} However, the consequences of unfair termination were contingent on firm size. If the worker decided to appeal the termination, the employer had the burden of proof. If based on that evidence the court decided that the termination was unfair, higher costs were imposed on firms with at least 16 employees. For these firms, the statute entitled the unfairly dismissed worker to compensation corresponding to the wages lost during the trial, social security payments, legal expenses, and a choice between reinstatement at the firm or an additional 15 months

\textsuperscript{1}The Organisation for Economic Co-operation and Development (OECD) has built an indicator for the strictness of employment protections based on laws regulating the procedures and costs of terminating individuals or groups of workers in each country. Here, I consider the indicator referring to individual dismissals of workers on regular contracts. The indicator is based on the regulations in place as of January 1st of each year. This metric makes it easy to compare employment protection strictness across countries, and it displays the wide variation among OECD countries. In 2018, the ranking of employment protection strictness for individual dismissals and regular contracts had the Netherlands at the top (3.44) and the United States at the bottom of the list (0.09). Source: https://www.stats.oecd.org/

\textsuperscript{2}Specifically, the statute focused on the size of establishments and not of firms. While the dataset I use contains only data at the firm level and not at the establishment level, this is probably not a concern here. In fact, in the analysis I focus on firms between 5 and 25 employees in 2011, which are likely to be one-establishment firms. Leonardi and Pica (2013) make a similar assumption.

\textsuperscript{3}More institutional details about the 1990 reform can be found in Garibaldi et al. (2004).
worth of salary. For firms up to 15 employees, the employer had no obligation to reinstate
the worker, and the 1990 reform added a compensation between 2.5 and 6 months of salary,
regardless of the duration of the judicial case. In practice, for firms above the size cutoff,
the statute introduced termination costs that were higher, more uncertain, and dependant
on the discretion of the judge.

1.3.2 The Implications of the Statute of Workers Rights

The restrictions stipulated by the 1970’s Statute significantly constrained firms with 16 or
more employees, despite the changes that came with the 1990 reform. To understand the ex-
tent to which this was true in practice, I searched for data on the number, costs, and outcomes
of judicial termination cases. These would offer information on firm expectations regarding
the real or perceived repercussions of termination. Unfortunately, because the Ministry of
Justice was not required to record key statistics on these cases until the implementation of
the Fornero Reform, no such data was ever summarized and published before 2012. However,
several academic papers (Garibaldi and Violante, 2005; Gianfreda and Vallanti, 2017; Kugler
and Pica, 2008; Schivardi and Torrini, 2008; Leonardi and Pica, 2013; Bratti et al., 2021)
and news reports (Ichino and Pinotti, 2012) agree that firms above the cutoff experienced
higher and more uncertain termination costs, and that they were constrained as a result.
According to CGIA (Association of Artisans and Small Businesses Mestre C.G.I.A.), which
regularly publishes official data on Italian businesses and workers, though firms above 15
employees comprised only 2.4% of the total number of firms in Italy, these firms employed

Uncertainty around the outcomes of court trials was problematic for firms with 16 or
more employees. Overall, the verdict was almost a lottery outcome dependent on the firm’s
geographical area, the judge, and the worker’s preferences. Ichino and Pinotti (2012) use
data from three main cities in the Center-North of Italy: Milan, Rome, and Turin. Between
2003 and 2005, they find that judicial cases in their sample lasted between 97 and 693 days, depending on the judge and location. Gianfreda and Vallanti (2017) summarize the average trial duration across judicial districts in Italy between 2007 and 2010. They find a strong correlation between trial duration and geography, with the Southern courts taking much longer to reach a verdict than their Northern counterparts, which were characterized by stronger labor markets. For example in Turin, a large city in the North of Italy, the average number of days needed for a decision was 224 days. This is in stark contrast with Bari, a Southern city comparable in size, where courts took 1,433 days on average. One report\(^5\) suggests that the probability of the judge siding with the worker is high and positively correlated with the local labor market conditions, because judges consider the hardship involved in the worker finding another job. The author mentions that while in the Center-North of Italy about 30% of the labor court trials end in favor of the firm, the number drops to the single digits in the South. Because losing firms above the threshold have to compensate the worker for forgone wages in addition to paying a fine for late payment of social security contributions, the duration of court cases contributes to the magnitude of and uncertainty around termination costs for firms.

Garibaldi and Violante (2005) estimate the costs of unfairly firing a blue-collar worker with average tenure for a firm above the threshold and find that the ex-post costs, which assume that the judge sides with the worker, correspond to over 40 months of salary. When they compute the ex-ante expected costs, which instead account for the possibility that the judge sides with the firm, they estimate these costs to be around 18 months of wages.

Overall, Italian laws on dismissals have historically been vigorous in protecting workers, and firms with 16 or more employees have experienced tighter restrictions.

\(^5\)Source: https://st.ilsole24ore.com/art/notizie/2012-02-03.shtml
1.3.3 Background on the Reforms

On November 16th, 2011, amid a severe financial crisis, a technocratic government quickly and unexpectedly designed a large labor market reform (henceforth called the Fornero Reform\textsuperscript{6}) whose main objective was to make the Italian economy more flexible and productive. The reform aimed to boost the dynamism of the Italian economy and break down the dualism between workers who were more protected (i.e. with long-term contracts) and those who were less protected (i.e. with temporary contracts). To achieve its stated goals, the reform relaxed the rules on termination implemented in 1970. A new labor reform with similar goals, the Jobs Act, was enacted in 2015. The Jobs Act further weakened employment protection restrictions and reduced the uncertainty around termination costs for employers.

Both reforms altered the rules on unfair terminations for disciplinary (subjective) and economic (objective) reasons, as well as on judicial procedures. Importantly, the provisions of these reforms affected only the restrictions for firms with 16 or more employees. Further, while the Fornero Reform applies to the termination of \textit{all} workers in firms with 16 or more workers, the Jobs Act only covers the employees hired in such firms \textit{after} March 7th, 2015. In this sense, the Jobs Act introduced a less dramatic change than the Fornero Reform.

1.3.4 The Fornero Reform

The Fornero Reform was implemented on July 18th, 2012 and applies to all terminations of blue-collar workers, white-collar workers, and middle-managers in open-ended contracts with firms of 16 or more employees. The main change introduced by the reform regards the employer’s obligation to reinstate terminated employees:\textsuperscript{7} in some cases of unfair termination, the option for reinstatement was substituted with pecuniary compensation.

Since the Fornero Reform, reinstatement has been an option available to judges in cases

\textsuperscript{6}Dr. Elsa Fornero was the Minister of Labor who signed both reforms.

\textsuperscript{7}In all cases of forced reinstatement, the 1970’s provisions mandated firms to pay the worker all monthly stipends between termination and reinstatement, in addition to the social security payments. The Fornero Reform put a limit on these payments: they could not go over 5 months for discriminatory termination or 12 months for unfair disciplinary termination.
of unfair disciplinary termination only if one of the following holds:

- The disputed misconduct of the worker is non-existent (i.e. it is not true that the worker has misbehaved, though it was used as an excuse to terminate the contract);
- The reason for the disciplinary termination falls within wrongful behavior but can be punished with less severe sanctions (i.e. termination was disproportional to the fault).

Similarly, the Fornero Reform kept reinstatement as an option for judges in some cases of unfair objective termination (i.e. economic layoff). Objective reasons for termination are fully dependent on the firm and not on worker behavior. These include, for instance, plant closure, economic insolvency, and reorganization of tasks or production activities. According to the Fornero Reform, reinstatement remains possible following an unfair economic termination only if one of the following holds:

- The judge finds the “manifest non-existence of the fact underlying the termination for justified objective reason.” In other words, the judge finds no occurrence of economic problems, closure, or firm reorganization. The judge holds much discretion in this case;
- Termination was due to the physical or psychological unfitness of the worker;
- Termination was ordered during the period of “comporto.” Comporto is the maximum number of days the worker is allowed to be absent from work due to sickness without risking her employment.

All other cases of unlawful disciplinary or economic termination could only result in monetary compensation. The payment of the firm to the worker ranges between 12 and 24 months of salary depending on the seniority of the worker and the discretion of the judge.

A second novelty of the reform is a change in judicial procedure: workers wanting to sue their employers had to attempt a friendly agreement first, through a mandatory preliminary conciliation. However, the reform did not provide the parties with precise guidelines for facilitating such resolution.
1.3.5 The Jobs Act

The Jobs Act further dismantled the employment protection laws established in 1970. It applies to terminations of workers who are blue-collar, white-collar, or middle-managers hired on or after March 7th, 2015 with open-ended contracts. More specifically, the Jobs Act:

- Bases indemnities on workers’ seniority\(^8\) rather than leaving them to the discretion of the judge, thus lessening uncertainty around termination costs and reliance on the courts;
- Eliminates the possibility of reinstatement in cases where termination is deemed disproportionate to the misconduct and a fine or suspension is considered more appropriate;
- Eliminates the possibility of reinstatement in all cases of termination for economic reasons (arguably the most revolutionary change).

The Jobs Act maintains the mandatory preliminary conciliation introduced by the Fornero Reform. However, it also allows for a voluntary conciliation both parties can use to attempt to avoid a judicial trial. A feature of this novelty is that the conciliation is based on a fixed monetary indemnity fully determined by the seniority of the worker, sharply decreasing the uncertainty of the outcome and facilitating an agreement between parties. Neither reform changes the rules allowing the reinstatement of the worker following an unfair termination due to discriminatory reasons. Table 1 summarizes the changes introduced by the reforms regarding the authority of courts to reinstate workers.

In a recent article, Ichino (2022) reports the numbers on litigation in Italian labor courts between 2012 and 2021 and shows a significant drop in the number of disputes. For example, between 2012 and 2021, courts experienced declines of more than 65% in economic termination cases and more than 80% in disciplinary termination cases. Although there is

---

\(^8\)In particular, for firms with 16 or more workers, indemnities are equal to two months of the last reference salary for each year of work, but they cannot be less than 4 months or more than 24 months, and are not subject to social security contributions. For firms with less than 16 workers, indemnities are equal to one month of the last reference salary for each year of work, but they cannot be less than 2 months or more than 6 months, and will not be subject to social security contributions.
no established causal link between the reforms and the decrease in litigation,\textsuperscript{9} it is highly plausible that the reforms contributed to this decrease, especially through their conciliation procedures.

Table 1: Authority of Courts to Reinstate an Unfairly Terminated Worker over Time

<table>
<thead>
<tr>
<th>Type of Layoff:</th>
<th>Before 2012</th>
<th>Fornero Reform</th>
<th>Jobs Act</th>
</tr>
</thead>
<tbody>
<tr>
<td>Discriminatory</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Disciplinary</td>
<td>Yes</td>
<td>No evidence of the fault</td>
<td>No evidence of the fault</td>
</tr>
<tr>
<td></td>
<td>(unless judge confirms worker's fault and its gravity)</td>
<td>Weaker sanctions are more appropriate</td>
<td></td>
</tr>
<tr>
<td>Economic</td>
<td>Yes</td>
<td>No evidence of the economic distress or structural change</td>
<td>Never Possible</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Layoff occurred after max days of sickness allowed</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Layoff due to physical or psychological unfitness</td>
<td></td>
</tr>
</tbody>
</table>

1.4 Theoretical Framework

To illustrate the impact that the Fornero Reform and the Jobs Act may have had on firm size and workforce composition, I present a theoretical model of firm hiring and termination decisions in the presence of employment protection legislation. In this section, I derive predictions that I then test empirically.

In this model, firms make a first decision in period 1, and a second in period 2. In period 1, they decide whether they want to hire a worker and, if so, in period 2 they decide whether to terminate her based on the information gathered between periods 1 and 2.

\textsuperscript{9}This is partly due to the fact that the Fornero Reform introduced a mandate to record and publish such statistics only starting in 2012.
The price of the good is determined by its demand in the economy: in a high-demand economy the price is $P$, and in a low-demand economy the price drops to $p$. Because $P > p$, everything else equal, it is more profitable for the firm to sell the good when the demand is high.

In this labor market, there are two types of workers: high skilled and low skilled. Firms cannot observe a worker’s type during the interviewing process. When making a hiring decision, the probability that a worker is high skilled is $s$, and the probability that a worker is low skilled is $1 - s$. High-skilled workers produce $Y$, and low-skilled workers produce $y$, with $Y > y$. At the end of period 1, the firm observes the production level of the worker, which reveals her type.\(^{10}\)

The hiring decision at the beginning of period 1 depends on the firm’s expectations of the future stream of benefits and costs associated with the new hire. At this stage, these are only known with some probability, with the exception of the wage, $w$, that is agreed upon at the beginning of the contract and is independent of a worker’s type. For simplicity, I assume that the firm considers hiring only if the economic conditions are favorable. At the end of period 1, the firm discovers the output level of the worker, and at the beginning of period 2, it also knows the demand level for period 2. While favorable conditions in period 2 incentivize the firm to keep the worker, the incentive to do so decreases if economic conditions worsen, because the price of the good would drop to $p$. In this economy, firms face employment protection in the form of termination costs $t$. Termination costs, the observed output of the worker, and the economic conditions in period 2 influence the termination decision.

The expected probability of the new hire, $E(\pi)$, is the sum of the expected profit from hiring in period 1, $E(\pi_1)$, which equals $sPY + (1 - s)Py - w$, plus the discounted expected profit from the hire in period 2, $E(\pi_2)$:

$$E(\pi) = E(\pi_1) + \lambda E(\pi_2) = sPY + (1 - s)Py - w + \lambda E(\pi_2) \quad (1.1)$$

\(^{10}\)Throughout the model, I assume that skill level and worker effort are exogenous.
with $\lambda$ being the discount factor. $E(\pi_2)$ depends on the net income from the worker, which is defined as her output multiplied by the price of the good minus her wage (e.g. $py - w$). The net income of the new hire varies with two unknown parameters: her skill level and the economic conditions. The firm makes the termination decision comparing the worker’s net income to termination cost. If the net income is positive, the firm keeps the worker because she is profitable. If it is negative, the firm takes a loss from keeping the worker, and it terminates her as long as that loss is more severe than the termination cost.

Four different scenarios can arise based on the economic conditions and the worker’s skill level. They are shown in Table 2. For simplicity, I assume that when $PY - w > 0$ the firm will never want to terminate a high-skilled worker in a good economy because $-t < 0$.

I graphically represent the termination decision in the second period in Figure 1.\textsuperscript{11} The graph depicts the role that termination costs play in the termination decision. The higher these costs, the higher the net loss the firm tolerates before it terminates the worker. For the same net loss, which is the quantity on the y-axis, as termination costs decrease, the firm is more likely to keep the worker. On the negative side of the net loss axis, the worker is never terminated because she is profitable for the firm.

The Italian reforms decreased termination costs, shifting them from, say, $t'$ to $t''$ on the graph. In any situation where the drop in termination costs is such that $t'' < \text{net loss} < t'$, the reform flips the firm’s decision in period 2, moving it from the green area, where it keeps an unprofitable worker, to the red area, where it terminates her. With lower termination costs, the loss that firms are willing to take on a worker instead of terminating her decreases. After the reforms, more firms will terminate unprofitable workers rather than retain them and run losses.

\textsuperscript{11}In drawing Figure 1, I assume that $w - Py > w - pY$. While whether $pY > Py$ depends on how large $Y$ and $P$ are compared to $y$ and $p$, respectively, it is plausible that an unproductive worker is more damaging to the revenues of a firm than the lower price of a good due to low demand.
Following Table 2, I write $E(\pi_2)$ as:

$$E(\pi_2) = sq(PY - w) + (1 - s)(1 - q)\max(py - w, -t) + s(1 - q)\max(pY - w, -t) + q(1 - s)\max(Py - w, -t) \quad (1.2)$$

Now, using backward induction, I include the expected net income from the worker in period 2 in the hiring decision in period 1. The firm hires if $E(\pi) > 0$, that is, if $E(\pi_1) + \lambda E(\pi_2) > 0$:

$$E(\pi) = sPY + (1 - s)Py - w + \lambda E(\pi_2) > 0$$

$$\Rightarrow spY + (1 - s)PY - w + \lambda[sq(PY - w) + (1 - s)(1 - q)\max(py - w, -t) + s(1 - q)\max(pY - w, -t) + q(1 - s)\max(Py - w, -t)] > 0$$

$$\Rightarrow \lambda(1 - s)(1 - q)\max(py - w, -t) + \lambda s(1 - q)\max(pY - w, -t) + \lambda q(1 - s)\max(Py - w, -t)] > w - sPY - (1 - s)Py - \lambda sq(PY - w) \quad (1.3)$$

The final inequality in (1.3) yields a condition for hiring in period 1 based on termination costs in period 2: as long as the left-hand side is greater than the right-hand side, the firm finds it profitable to hire. The left-hand side of the inequality is the discounted expected net income from the worker if the firm has bad luck. This quantity is a weighted average of the net incomes resulting from the new hire in cases where low demand prevails in period 2, the worker is low skilled, or both. The weights are the probabilities of each scenario occurring. The right-hand side, by contrast, is the negative of the discounted expected profit from a high-skilled worker in a good economy, weighted by the probabilities that these two positive outcomes will arise. While the result of the inequality depends on the unknown parameters $\lambda, s, \text{ and } q$, I only consider the role that termination costs play in this comparison, because they are the focus of the policies I examine.

Since $t$ is strictly positive, if the net incomes $py - w, pY - w, \text{ or } Py - w$ are positive,
that is, if the worker is profitable, then \( py - w \), \( pY - w \), or \( Py - w \) are the values of the maximization functions. In this case, the firm keeps the worker and \( t \) plays no role in the hiring decision.

Instead, if in period 2 \( py - w \), \( pY - w \), or \( Py - w \) are negative, the firm compares that net loss with the termination costs and chooses the smaller loss. That is, termination costs impact the hiring decision through this comparison because they can flip the outcome of the maximization function. A reform reducing termination costs increases the expected value of hiring because it can raise the value of the maximization functions, and so of the expected profit of a firm with bad luck. That happens whenever, before the policy, the cost of maintaining a worker is lower than the termination cost and the policy lowers termination costs enough to make them the lower quantity (i.e. \( t'' < \text{net loss} < t' \)).

Figure 2 shows the firm hiring decision in period 1. In Figure 2, the x-axis represents termination costs, and the y-axis plots the two quantities in the inequality. The discounted expected net income from the worker in the negative scenarios varies according to termination costs. By contrast, because the discounted expected net income from a high-skilled worker in a good economy is independent of termination costs, it shows up as a horizontal line. The expected net income from a worker in the negative scenarios varies with termination costs as long as these costs are low enough to be the outcome of the maximization function. However, as termination costs increase, they will reach a point where they equal the net loss beyond which the net loss becomes the result of the maximization functions, making the expected net income from the worker in the negative scenarios independent of termination costs. On the left of that break-even point, the expected net income from the worker in the negative scenarios decreases with termination costs, because, as long as the maximization function yields \(-t\), it decreases with \( t \). For simplicity, I assume that the negative slope of this line is constant, implying that the parameters are equal, i.e. \( s = q = 0.5 \). When \( t = w - py \), the line for the expected net income from the worker in the negative scenarios turns flat because the net loss becomes the result of the maximization function, making the quantity
independent of \( t \). Based upon where the negative of the discounted expected net income from a high-skilled worker in a good economy lays compared to the line for the expected net income from the worker in the negative scenarios, there are 4 cases:

- If the discounted expected net income from a high-skilled worker in a good economy is the L1 line, it is always greater than the expected net income from the worker in the negative scenarios, and hiring does not occur in period 1, regardless of the termination costs;
- If the discounted expected net income from a high-skilled worker in a good economy is the L3 line, it is always lower than the expected net income from the worker in the negative scenarios, and hiring occurs in period 1, regardless of the termination costs;
- If the discounted expected net income from a high-skilled worker in a good economy is the L2 line, the hiring in period 1 occurs as long as the termination costs are lower than the intersection point between the two quantities.

The policy triggers hiring in the last scenario whenever it lowers termination costs enough to move them below \( w - py \). That would occur if, for example, the policy drops termination costs from \( t' \) to \( t'' \).

This model delineates how firms’ hiring and termination decisions are affected by the presence of termination costs, and how changes in these costs alter such decisions. In particular, the model makes the following predictions:

- By entering into the function for the expected profitability to firms of a new hire, termination costs impact hiring and termination. As these costs decrease, both termination and hiring increase. Termination increases because its cost can be lower than the net loss from workers, incentivizing the firm to terminate. Cutting termination costs also increases hiring because it leads to lower expected outlays arising from a new hire, and so reduces the risk of the firm entering an employment relationship. While the effect of higher termination and hiring on size is ambiguous because it depends on which of the two prevails, the model predicts increased dynamism among firms. I test this
prediction by operationalizing the concept of dynamism from information on the rate of employment change;

- If the revenues from a worker’s output are lower than her wages, the firm would like to terminate. However, termination costs imply that firms may choose to retain a worker if the loss they take from keeping her is lower than the cost of terminating her. As a consequence, high termination costs imply the presence of unprofitable workers within a firm. If termination costs decrease, the loss that firms are willing to take on an unprofitable worker before she is terminated decreases. Hence, lower termination costs raise the average profitability of workers within a firm. Thus, the model predicts that the reforms increase the average productivity of impacted firms because they are more able to retain the most profitable workers;

- Employment protection also affects productivity through a second potential channel: firm dynamism. Strict employment protection laws reduce firms’ ability to quickly adjust in size according to their profit maximizing choices in the short term. A reduction in termination costs can boost firm productivity through higher dynamism if they are increasingly able to satisfy their short-term preferences with regard to size.

I explore the impact of the reforms on several measures of labor productivity, though I cannot distinguish between the last two mechanisms.

Table 2: Termination Decision of the Firm in Period 2 - The Four Possible Comparisons

<table>
<thead>
<tr>
<th>Scenario</th>
<th>Probability of scenario</th>
<th>Net income from worker</th>
<th>Firm terminates if</th>
</tr>
</thead>
<tbody>
<tr>
<td>High skilled-High demand</td>
<td>sq</td>
<td>(PY - w)</td>
<td>(PY - w &lt; -t \rightarrow t &lt; w - PY)</td>
</tr>
<tr>
<td>Low skilled-Low demand</td>
<td>((1 - s)(1 - q))</td>
<td>(py - w)</td>
<td>(py - w &lt; -t \rightarrow t &lt; w - py)</td>
</tr>
<tr>
<td>High skilled-Low demand</td>
<td>(s(1 - q))</td>
<td>(pY - w)</td>
<td>(pY - w &lt; -t \rightarrow t &lt; w - pY)</td>
</tr>
<tr>
<td>Low skilled-High demand</td>
<td>((1 - s)q)</td>
<td>(Py - w)</td>
<td>(Py - w &lt; -t \rightarrow t &lt; w - Py)</td>
</tr>
</tbody>
</table>
Figure 1: Termination Decision of the firm in Period 2
Figure 2: Hiring Decision of the firm in Period 1

Discounted expected net income from the worker in the negative scenarios,
-(Discounted expected net income from a high skilled worker in a good economy)
1.5 Empirical Strategies

1.5.1 Bunching Estimation

Because in Italy employment protection legislation imposes higher costs on firms with 16 or more employees, it creates a nonlinear relationship between number of employees and profit of firms. Bunching estimators are widely used in the economic literature to recover behavioral responses to nonlinear incentives. Originally developed in the context of tax-and-transfer programs (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013; Gelber et al., 2020) where the magnitude of the discontinuity in incentives is known in monetary terms, bunching estimators are increasingly applied also in contexts where the exact size of the notch is unknown (Garicano et al., 2016). The idea is to exploit the strategic response of agents to sharp changes in costs or benefits. Because agents maximize the benefits of transfer programs or minimize taxes, their behavior generates excess mass or missing mass in their distribution. The magnitude of these bumps or holes in mass are proportional to the intensity of their response.

In this paper, I use the bunching method to estimate the distortion in firm size distribution generated by the employment protection notch at which restrictions jump, which is at 15 employees. The function for termination costs in this case is \( F(n) = f + \Delta f1(n > 15) \), where \( f \) represents the termination costs for firms with less than 16 workers, \( n \) represents the number of employees, and \( \Delta f \) represents the additional restrictions faced by firms with 16 or more employees. The basic assumption is that, without the discontinuity in termination costs, \( \Delta f1(n > 15) \), the density of firms would be smooth at the threshold and exhibit no bumps or holes. To my knowledge, no other Italian policy utilizes a 15-worker cutoff beyond employment protection, providing reassurance that the above assumption holds. The bunching design is helpful to show the size of the behavioral response of firms, its statistical significance, and its evolution over time as the reforms are implemented.

For the estimation, I follow the approach in Kleven (2016) and Chetty et al. (2011). The
goal is to come up with a counterfactual density distribution, which is the expected density of firms in the absence of the notch, and compare it to the density observed in the data. The difference between the two is explained by the behavioral response to the notch. I estimate counterfactual density by fitting a polynomial to the data on firms’ observed density, but omitting the size range around the notch. Then I use the values predicted by the polynomial to extrapolate the density in that excluded range. The polynomial I fit in each bin \( j \) of the number of employees is:

\[
C_j = \sum_{d=0}^{D} \beta_d (size_j)^d + \sum_{k=z_l}^{z_u} \gamma_k 1(size_j = k) + \epsilon_j
\]  

(1.4)

where \( C_j \) is the percentage of firms in each size bin \( j \) out of the total number of firms, \( size_j \) is the number of employees corresponding to bin \( j \), \( d \) is the degree of the flexible polynomial, and \( z_l \) and \( z_u \) are the lower and the upper bounds of the bandwidth I exclude around the notch at 15 workers.

In the notch case, the omitted range should be the part of the distribution that displays both bunching responses, the bump and the hole (Kleven, 2016). Following this principle, I determine the bandwidth by visual inspection, and I set \( z_l = 13 \) and \( z_u = 18 \), which is symmetrical around the cutoff.\(^{12}\) However, I show in Appendix 1.9.3 that the results are not sensitive to alternative bandwidth choices. Unsurprisingly, bunching is not only concentrated at the 15-workers bin, because firms face friction when making size decisions. For example, the rules determining number of employees for the purposes of employment protection are complex, and firms may prefer limiting their size to 13 or 14 workers to increase their confidence of being to the left of the discontinuity. The other choice I make is the order of the polynomial. While for the main results I show estimates based on a seventh-degree polynomial (i.e. \( d = 7 \)), in Appendix 1.9.3 I find that results are mostly robust to several alternative specifications (i.e. polynomials of degree 3 through 9), especially when

\(^{12}\)Kleven (2016) indicates this is preferred.
considering the more flexible specifications.

I estimate the bunching below the cutoff by summing up the excess mass in the observed density compared to the counterfactual predicted by the polynomial at each point between 13 and 15 workers, \( \hat{B}_l = \sum_{k=13}^{15} \hat{\gamma}_k \). Because theoretical considerations also predict a hole in firm density above the cutoff, I measure the missing mass simply with \( \hat{B}_u = \sum_{k=16}^{18} \hat{\gamma}_k \). Because the number of firms in Orbis increases after 2016 due to changes in the rules on financial reporting, I compute the excess and missing mass as percentages of all firms to improve the comparability of the bunching estimates over time. Specifically, in each year, I estimate the number of extra firms just beneath the cutoff and that of missing firms just above it as a percentage of the total number of firms in the dataset. I plot the evolution of both these percentages between 2011 and 2019.

Following the procedure in Chetty et al. (2011), I estimate bootstrapped standard errors at the 95% confidence level.\(^\text{13}\) I perform this estimation strategy for each year between 2011 and 2019, and I show how the estimates of the excess mass, the missing mass, and their confidence intervals evolve over time.

### 1.5.2 Difference-in-Differences Estimation

The right-hand side of the regressions includes the interaction between the indicator variables for the post-reform years and for the treated firms, which are those above the employment protection threshold in 2011, the baseline year. Because I add year and firm fixed effects, the inclusion of the main effects is redundant. Year fixed effects control for time-varying trends that impact all firms equally, and firm fixed effects account for firm characteristics that do not vary over time. This model identifies within-firm variation in the outcomes over time, excluding yearly trends affecting all firms equally. I run a specification that investigates the

\(^{13}\)I replicate the procedure by random sampling residuals 1,000 times with replacement and re-estimating the binned counts.
effect of both reforms on the data for years 2010 to 2019:

\[ \text{Outcome}_{ft} = \alpha_{ft} + \beta_{\text{ForneroReform}_t} \times \text{Large}_f + \gamma_{\text{JobsAct}_t} \times \text{Large}_f + \text{Firm}_f + \text{Year}_t + \epsilon_{ft} \] (1.5)

The model in Equation 1.5 identifies the impact of both reforms compared to 2011, the pre-reform year. Outcome is each of the following quantities measured per worker: number of employees, as well as average operating revenue, sales, profit margin, cash flow, or value added in year \( t \) for firm \( f \). Because both reforms were enacted during the calendar year, I turn their indicator variables in the following year for each: \( \text{ForneroReform}_t \) is a dummy variable equal to 1 for years 2013 through 2019, meaning \( \text{ForneroReform}_t = 1(2013 \leq t \leq 2019) \). Similarly, \( \text{JobsAct}_t \) equals 1 in years 2016 through 2019, that is \( \text{JobsAct}_t = 1(2016 \leq t \leq 2019) \). \( \text{Large}_f \) indicates the firm’s treatment status: it is equal to 1 if firm \( f \) had 16 to 25 employees in 2011, and it is 0 if the firm had between 6 to 15 employees in that year. \( \alpha_{ft} \) is the average outcome in the pre-reform year for firms with 6 to 15 workers during that period. \( \beta \) measures the impact of the Fornero Reform on the outcome for treated firms. I compute the magnitude of the \( \beta \) coefficients by dividing the estimated coefficient by the mean of the outcome variable in the estimation sample of control firms when \( \text{ForneroReform}_t = 1 \). \( \gamma \) is the differential effect of the Jobs Act in addition to that of the Fornero Reform for those firms. I obtain the magnitude of the \( \gamma \) coefficients by dividing the estimated coefficient by the mean of the outcome variable in the estimation sample of control firms when \( \text{ForneroReform}_t = 1 \) and \( \text{JobsAct}_t = 1 \). I report robust standard errors clustered at the firm level.

The regression analysis relies on two key identifying assumptions. First, the difference-in-differences method assumes that control firms would have followed trends in each outcome that are parallel to those of treated firms in the absence of the reforms. In other words, there are no time-varying differences in firm outcomes across the threshold that are not originated by the two reforms. Second, the analysis requires a strict exogeneity assumption.
that unobserved factors impacting each outcome are uncorrelated with the history of firms’
treatment status. In other words, the difference-in-differences model is identified as long as
there are no anticipation effects of the reforms, no differential pre-trends across treated and
control firms, and no time-varying treatment effects beyond those captured by the variables
in the regression.

It is common practice in this framework to fit event study regressions that help assess the
plausibility of both assumptions. Unfortunately, my data does not go back in time enough
to allow for a long period of pre-reform years in the event studies. I use 2010 and 2011 as
the pre-reform period, and compare that period to the years 2013 through 2019. In Section
3.8.1 of the Appendix, I describe the event study method and report the corresponding
results, which overall support the validity of the required assumptions. To further alleviate
any concerns on the causal identification of the effects, I rely on qualitative considerations
about the circumstances of the enactment of the Fornero reform. As Professor Fornero
herself explains, she designed the reform unexpectedly and swiftly, as a quick response to
the financial emergency the country was facing (Fornero, 2013). The unpredicted nature of
the Fornero Reform increases my confidence that its effects could not be anticipated.

In Section 1.7.6, I consider alternative specifications to ensure that the estimation results
are robust to different definitions of treated and control groups as well as post-reforms years.

1.6 Orbis Data

For this study, I use proprietary data from Orbis, a commercial firm-level database that
the Bureau van Dijk (BvD) publishes for economic research. Orbis contains commercial
compny information on more than 400 million non-farm, mostly private companies in over
200 countries as of May 2022, with the across- and within-country coverage steeply increasing
over time. The BvD gathers and harmonizes data from governments and national business
registers that collect financial and balance sheet data from firms according to the financial
reporting requirements of each country.
In Italy, all firms are supposed to file a company registration report at the Business Registry. Some companies also have to file annual accounts at the end of each financial year with the Central Business Registry. Small- and medium-sized companies are exempt from these standard filing requirements based on size and financial criteria, implying that information on financial performance may be limited for exempt firms.

From the Orbis database, I have selected all firms with a workforce between 2 and 80 employees at any point between the last year of available financial information for each firm and up to nine years of filing prior to that, when available. I exclude public authorities, states, and governments because these are not subject to employment protection rules. Further, I exclude companies lacking recent financial data, because for these firms I would not be able to run the analysis on productivity. Finally, I keep firms with size information in 2011, the pre-policies year, so that I can assign them to a treatment group in the baseline year. My analytic dataset is composed of 1,055,258 observations for a varying number of firms over time. For example, in 2011, the pre-reform year, there are 127,122 firms in my sample. Table 3 provides details about the time composition of my sample. The variables I use are firm IDs, industry, number of employees, and financial information that can be used to measure firm productivity. Sample statistics for the variables measured in year 2011 can be found in Table 4.

From the Orbis data, I pull five variables to measure per employee labor productivity: operating revenues (thousands of Euros), profit margin (percent), sales (thousands of Euros), cash flow (thousands of Euros), and added value (thousands of Euros). In evaluating which

---

14 The criteria on the size of firms and their accounting requirements are defined by Article 2435-bis of the Italian Civil Code and listed here: https://www.pwc.com/it/en/publications/assets/docs/new-financial-statements.pdf

15 These details on the source and quality of the data were provided to me via email correspondence with a representative of Moody’s Analytics, which owns BvD, in the Spring of 2022.

16 Operating revenues are generated from the core activities of a company, and they measure the efficiency of a company’s primary business. Sales represent money paid by customers for a good and service, and derive from a company’s core revenues in a given period. While operating revenues and sales move together and mostly match, revenues can be greater than sales because of the presence of business operations beyond selling goods or services. Profit margin is the percentage of selling price that is turned into profit. It is computed as profit before tax divided by operating revenues, times 100. Cash flow is the sum of net income and depreciation. Added value is the sum of net income, depreciation, taxation, interests paid, and cost of
variables to download, and choosing my preferred set of results on productivity, I considered three criteria: the number of missing observations for each measure, the ability to capture labor productivity (versus the productivity of other inputs, like external contractors), and which productivity measures are commonly used in the literature. Operating revenues is the measure with the highest number of observations and, together with sales, directly quantify employee output. Profit margin measures the net value of such output, because it subtracts costs but, in excluding taxation, frees the measure from fiscal considerations. Cash flow is the net profit, but it also considers the value of depreciation. Value added includes everything included in cash flow, plus taxation, interests paid, and cost of employees. Because cash flow and value added encompass sources of costs that are only indirectly related to labor, I consider them as less direct measures of labor productivity.

Orbis is the only dataset, to my knowledge, that includes both detailed information on Italian firm size and accounting over time from administrative sources. Still, Orbis comes with some limitations that the literature has thoroughly examined (Bajgar et al., 2020; Kalemli et al., 2022; Ribeiro et al., 2010), and which I discuss in Section 1.9.1 of the Appendix. These limitations revolve around the sample’s heterogeneous coverage, both within and across countries, raising concerns about its ability to entirely represent the distribution of Italian firms over time. I have followed suggestions available in the literature (Bajgar et al., 2020; Kalemli et al., 2022) that are applicable to my context to alleviate these concerns.

---

17 Operating revenues were used in Kalemli et al. (2022), sales per employee in Hijzen et al. (2017), and value added per worker in Leung et al. (2008). For more information, Gandhi et al. (2017) compare the validity of different measures of productivity.

18 Appendix Table A2 shows the number of Italian firms for which several productivity variables are available from an alternative source (a survey of Italian firms that relies on weights) and whose sample has information for about 80,000 companies below 250 workers, while all companies above that size are sampled.

19 An alternative dataset for this project is the Statistical Register of Active Firms (ASIA is the acronym in Italian) because it gets closest to covering the universe of Italian companies whose output has contributed to the Italian GDP. Firms in the ASIA dataset have been active for at least 6 months until 2019 and for at least 1 day starting in 2019 https://www.istat.it/it/archivio/263692. However, while ASIA data is reliable for the distribution of firm sizes in Italy, it does not provide information on productivity beyond annual turnover.

20 First, I provide robustness results from analyses that only include firms with 10 or more employees. I
Table 3: Tabulation of the Orbis Sample Downloaded and of the Analytic Sample

<table>
<thead>
<tr>
<th>Year</th>
<th>Frequency</th>
<th>Percent</th>
<th>Frequency Analytic Sample</th>
<th>Percent Analytic Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>2004</td>
<td>1</td>
<td>0.00</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2005</td>
<td>156</td>
<td>0.00</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2006</td>
<td>847</td>
<td>0.01</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2007</td>
<td>2,070</td>
<td>0.03</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2008</td>
<td>3,828</td>
<td>0.06</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2009</td>
<td>13,338</td>
<td>0.21</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2010</td>
<td>25,912</td>
<td>0.42</td>
<td>8,137</td>
<td>0.77</td>
</tr>
<tr>
<td>2011</td>
<td>306,160</td>
<td>4.91</td>
<td>127,122</td>
<td>12.05</td>
</tr>
<tr>
<td>2012</td>
<td>346,609</td>
<td>5.56</td>
<td>125,240</td>
<td>11.87</td>
</tr>
<tr>
<td>2013</td>
<td>376,239</td>
<td>6.03</td>
<td>121,795</td>
<td>11.54</td>
</tr>
<tr>
<td>2014</td>
<td>404,961</td>
<td>6.49</td>
<td>119,020</td>
<td>11.28</td>
</tr>
<tr>
<td>2015</td>
<td>435,985</td>
<td>6.99</td>
<td>116,150</td>
<td>11.01</td>
</tr>
<tr>
<td>2016</td>
<td>465,646</td>
<td>7.47</td>
<td>113,687</td>
<td>10.77</td>
</tr>
<tr>
<td>2017</td>
<td>881,083</td>
<td>14.13</td>
<td>111,445</td>
<td>10.56</td>
</tr>
<tr>
<td>2018</td>
<td>961,720</td>
<td>15.42</td>
<td>109,015</td>
<td>10.33</td>
</tr>
<tr>
<td>2019</td>
<td>1,012,259</td>
<td>16.23</td>
<td>103,647</td>
<td>9.82</td>
</tr>
<tr>
<td>2020</td>
<td>993,029</td>
<td>15.93</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>2021</td>
<td>5,778</td>
<td>0.09</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Total</td>
<td>6,235,621</td>
<td>100.00</td>
<td>1,055,258</td>
<td>100.00</td>
</tr>
</tbody>
</table>

1.7 Results

1.7.1 Employment Protection Legislation and Size Distortion

I provide graphical evidence of size distortion by showing the shape of the size distribution of firms before the policies and then considering the evolution of that distortion following policy implementation. The patterns of changes in size uncover the type and magnitude of the constraints that firms experienced due to employment regulation.

Figure 3 shows the average change in size for firms between 2011 and 2014 by number of employees in 2011. Because the Fornero Reform was implemented in 2012, comparing 2011 to 2014 is helpful for assessing the size adjustment of firms in response to the first weakening in the legislation. Firms with 12, 13, and 14 employees expand and, in particular, firms with show the estimates, which are broadly similar to the main results but present larger standard errors, in Table A5 of the Appendix. Second, I analyze within-firm response to the reforms through the use of firm-level fixed effects. Third, I carry out a separate analysis for the effects of the second policy, implemented in 2015, when data coverage is larger.
14 workers in 2011 increase their workforce by over 1 employee on average, implying that they pass the threshold. This trend suggests that they were constrained by the employment protection threshold, and grew past it after the policy weakened employment protections. Unsurprisingly, firms with 19 and 20 workers also expand, suggesting that the hiring effect described in Section 1.4 dominates for them. By contrast, firms just above the threshold shrink on average. Figure 3 shows that after a first weakening in the restrictions, increased hiring seems to prevail among firms below the cutoff and well above it, while termination dominates among firms with 16 and 17 workers.

Figure 4 plots the same quantities but focuses on the average change in size between 2014 and 2019, isolating the period of the Jobs Act. The trends are in contrast with those arising after the first policy change. Figure 4 shows that all firms in this range increase in size. Firms with 13 through 15 employees in 2014 increase, on average, by almost 1.5 employees. Additionally, unlike the previous case, the same also holds for firms with 16 workers in 2014. It appears that their average decrease in size was only temporary, because they expand after the Jobs Act. This descriptive pattern supports the conjecture that firms just above the cutoff first terminated low-quality matches and then initiated potentially higher-quality ones. Finally, between 2014 and 2017, firms with 17 through 20 employees experience a large average increase in their number of employees, indicating that the hiring effect prevails in
this range of the size distribution. The Jobs Act represented a trigger for the expansion of firms across the board.

So far, these exhibits provide consistent evidence that employment protection legislation introduces a size distortion, and that its design based on a size threshold constrains firms around the cutoff. The next two graphs confirm this conclusion.

Figures 5 and 6 show how firms that were concentrated in a narrow range around the cutoff in 2011 changed their size over time. I choose the two ranges based on their location around the threshold. The size evolution of firms right below the cutoff in 2011 and specifically of firms with 12 to 15 workers is displayed in Figure 5, and the size evolution distribution of firms originally just above the cutoff, between 16 and 19 employees, is displayed in Figure 6. Figure 5 shows that, while the distribution of firm sizes tends to spread out over time, the mode of that distribution remains in the 12- to 15-employee range. In fact, between 2011 and 2019, the peak of the frequency stays within the two dashed lines. Secondly, it appears that the right part of the distributions gets thicker, especially between 2017 and 2019, providing descriptive evidence that the delayed response of firms below the cutoff to the policies is expansion. A stronger size distortion emerges from Figure 6, which shows how firms with between 16 and 19 workers in 2011 move in the distribution over time. In this case, there is an obvious shift of the mode toward the left, where the lower termination costs apply. Firms that were right above the cutoff before the reforms appear to shift below it as the reforms are implemented. Figure 6 suggests the presence of a strong size distortion that these firms experience due to the employment protection legislation and its design. When the reforms alleviate such constraints, firms are more free to decide their optimal size and the distortions become less severe. In Section 1.9.2 of the Appendix, I show these distributions for alternative choices of placebo bandwidths further away from the threshold to exclude that these movements correspond to reversions to the mean. The absence of atypical patterns in the size evolution of placebo firms strengthens the validity of these conclusions.
Figure 3: Changes in Firm Size after the Fornero Reform

Note: In 2011, I keep firms between 10 and 20 workers to focus around the size threshold where the legislation strictness changes sharply. I compute the average change in the number of employees in 2014 in firms by their number of employees in 2011.
Figure 4: Changes in Firm Size after the Jobs Act

Note: In 2014, I keep firms between 10 and 20 workers to focus around the size threshold where the legislation strictness changes sharply. I compute the average change in the number of employees in 2017 in firms by their number of employees in 2014.
Figure 5: Evolution of Size for firms with 12 to 15 employees in 2011

Size Distribution over Time of Firms between 12 and 15 Workers in 2011

Note: I consider firms between size 12 and 15 in 2011. I plot their size distribution, in years 2014, 2016, 2017, and 2019. To improve readability, I curtailed the right hand tails at 40, excluding firms that have grown above that size. Of course, as some of these firms drop out of the sample over time, or grow beyond 40, the size of the sample in each frequency graph decreases. In particular: in 2011, the sample is composed of 254,922 firms, in 2014 there are 249,883 firms, decreasing to 243,514 in 2016, then to 240,616 in 2017, and finally the sample has 226,344 firms in 2019. The two vertical dashed lines correspond to size 12 to 15, which is the range that in 2011 contained all the firms displayed in the distribution.
Figure 6: Evolution of Size for firms with 16 to 19 employees in 2011

Note: I consider firms between size 16 and 19 in 2011. I plot their size distribution, in years 2014, 2016, 2017, and 2019. To improve readability, I curtailed the right hand tails at 40, excluding firms that have grown above that size. Of course, as some of these firms drop out of the sample, or grow beyond 40, the size of the sample in each frequency graph decreases over time. In particular: in 2011, the sample is composed of 127,252 firms, in 2014 there are 123,949 firms, decreasing to 120,049 in 2016, then to 118,067 in 2017, and finally the sample has 110,184 firms in 2019. The two vertical dashed lines correspond to size 16 to 19, which is the range that in 2011 contained all the firms displayed in the distribution.
1.7.2 Bunching

The bunching estimation is helpful for characterizing the direction and evolution of the size distortion that employment laws introduce. The descriptive evidence reported so far shows the overall trends of the size distortion, but the bunching methodology outlined in Section 1.5.1 gives those trends empirical rigor and provides information on their statistical significance. For the main results, I consider a seventh order polynomial and a bandwidth of range 6 (i.e. between 13 and 18 employees), but in Section 1.9.3 of the Appendix I show that the estimates are overall not sensitive to alternative degrees for polynomial or bandwidth size.

The bunching methodology is based on the comparison of two quantities: the observed density of firm sizes and the one predicted by a polynomial that approximates the shape of the distribution without the notch in employment protection. Figure 7 plots these two quantities in 2011, the pre-reform year. The dots are the scatters of the observed logarithmic counts of firms with between 6 and 25 employees in 2011, and the dashed line reports the predicted values of a seventh-order polynomial fitted on the observed data, excluding the 13-18 employee range. The dashed line also reports the fitted values for the omitted range, which have been extrapolated from the polynomial prediction outside that bandwidth. The discrepancy between these two quantities validates the presence of a size distortion, and the vertical lines help to visualize it. I used this discrepancy around the cutoff as guidance in choosing the bandwidth, which coincides with the range between 13 and 18 employees where I drew the two solid lines. The vertical dashed line, meanwhile, corresponds to the discontinuity in the employment restriction legislation.

The area between the dots and the dashes on the left-hand side of the bandwidth shows the excess mass just beneath the cutoff, and the same area on the right-hand side indicates weaker evidence of a density hole above the cutoff. Outside of the excluded range, the polynomial predicts the observed data well, as these mostly overlap. Because the first reform was implemented in 2012, 2011 is the year in my data where the distortions are likely largest:
as yet, no weakening in the legislation has occurred. With the implementation of the reforms in 2012 and 2015, the size of the legislation notch is lowered, possibly lessening the magnitude of the distortion. The bunching analysis provides evidence of this shift.

Figures 8 and 9 summarize the main estimates of the bunching methodology. They plot the progression of the bunching estimates \( \hat{b}_l \) and \( \hat{b}_u \), which are the excess and missing percent of firms over the years. In each graph, the solid line plots the point estimates in percentage of firms, and the dashed lines indicate the 95 percent confidence intervals obtained from the bootstrapping procedure. The two vertical lines mark the years of the Fornero Reform (2012) and the Jobs Act (2015). First, Figure 8 shows that, below the cutoff, the excess mass was positive and statistically significant between 2011 and 2016, providing evidence of a size distortion in the pre-reform period, as firms accumulated below the notch to avoid the stricter legislation. By contrast, Figure 9 indicates that there was no statistically significant missing mass above the cutoff because the confidence interval of the point estimate \( \hat{b}_u \) in that period always included zero. Second, there is evidence that the reforms reduced the magnitude of the size distortion, because the point estimate \( \hat{b}_l \) approaches zero in the post-reform period. While in 2011 more than 1% of excess firms accumulated just beneath the cutoff, with that estimate being statistically significant, by 2019 the excess mass cannot be distinguished from zero. Moreover, the estimates suggest that the excess percentage of firms with 13, 14, and 15 employees drops by over 70% in the aftermath of the reforms. Figure 8 shows that the reforms lead to a dissipation of the size distortion below the cutoff. Figure 9, instead, shows that the missing mass above the threshold was small and not statistically different from zero between 2011 and 2016. Even if the confidence interval for \( \hat{b}_u \) slightly deviates from zero following the reforms, the point estimate remains small and similar in magnitude to the one in the pre-reform period. Moreover, the sensitivity checks show that when considering polynomials of alternative orders, the point estimate never becomes statistically different from zero. Third, despite the changes in the point estimates and in their confidence intervals, the evolution of the confidence intervals in both figures shows that the bunching magnitudes over the years
are not statistically different from each other. In fact, as long as the confidence intervals of the estimates overlap over time, I cannot reject the hypothesis that the policies have not significantly impacted the magnitude of the response.

Overall, these results illustrate that the policies have eliminated the size distortion below the threshold. Over the reform period, firms stopped accumulating just below the part of the size distribution where termination costs jump, implying that they are less reluctant to grow past that point. Moreover, these findings imply that the size distortion above the threshold is minimal, and appears to have slightly increased during the reform period, but that the point estimates are small and the confidence interval remains close to zero (indeed it includes zero when using alternative polynomials). The conclusions from the bunching design imply that although the reforms did not eliminate the discontinuity in restrictions, their interventions significantly limited the response of firms to the size incentives introduced in 1970.
Figure 7: Observed and Predicted Firm Size Distribution in 2011

Note: I ran one seventh degree polynomial regression of the log count of firms on the number of employees. I have run that regression for firms between 2 and 40 workers excluding those between 13 and 18 workers in 2011, but in the figure I focus on firms of size 6 through 25 in 2011. The scatterplot shows the actual log count of firms by the number of employees in 2011. The dashed line shows the predicted values obtained from the regression and the resulting fitted log number of firms with number of employees in the range of 13 to 18. The vertical dashed line is at 15.5 and shows the location of the notch, where the jump in the restrictions occurs. The two solid lines indicate the width of the omitted range.
Figure 8: Evolution of the Excess Mass between 13 and 15 Employees and Confidence Interval

Note: I estimate the bunching estimate for the range between 13 to 15 workers, that is below the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using a seventh degree polynomial regression of the percentage of firms in each size bin on the number of employees, omitting the range 13 to 18 employees. The estimate is derived summing up the excess of that percent mass of firms at 13, 14, and 15 workers. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and bootstrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
Figure 9: Evolution of the “Hole” between 16 and 18 Employees and Confidence Interval

Note: I estimate the bunching estimate for the range between 16 to 18 workers, that is above the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using a seventh degree polynomial regression of the percentage of firms in each size bin on the number of employees, omitting the range 13 to 18 employees. The estimate is derived summing up the missing of that percent mass of firms at 16, 17, and 18 workers. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and bootstrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
1.7.3 Regression Results for Number of Employees

Table 5 shows the regression estimates for Model 1.5 described in Section 1.5.2. The outcome is number of employees. Errors have been clustered at the firm level. In the model, it appears that weakening labor protection legislation for firms of 16 or more employees leads to a decrease in their size that is statistically significant and non-negligible in magnitude. When considering the average number of employees in the control group in the period after the Fornero Reform, I estimate that the Fornero Reform lowers the number of workers by 5.8%. The Jobs Act rebounds this negative trend. Compared to control firms in the period after the Jobs Act, treated firms experience an average decrease in size of 4.8%, representing a decrease 1 percentage point smaller than the decrease that followed the first reform. The estimates imply that due to the Fornero Reform, a treated firm will shrink by over half a worker on average, and will gain a tenth of a worker following the Jobs Act. These results support the conjecture from the graphical evidence. Firms respond to the first weakening in regulations after several decades by curtailing their workforce. Following a second reform, which further alleviates the restrictions, they slow down their drop in size.

1.7.4 Regression Results for Dynamism

An alternative way of assessing the extent to which firms are constrained by employment protection regulations is to use a generalized measure of size change that does not distinguish between expansion and restriction. In fact, as laid out by the theoretical model in Section 1.4, weaker employment protections predict higher termination as well as hiring. In an environment that has been historically characterized by strict employment protections, increases in both these rates likely reflect the greater ability of firms to adjust their size as their needs change. In the context of employment protections, firm dynamism is mostly associated with the ability and flexibility to change size as desired.

This concept could be quantified in several ways. The novel metric I build here is easy to implement across different settings. I measure dynamism by computing the absolute value
Table 5: The Effects of Weakening Employment Protection on the Size of Firms

<table>
<thead>
<tr>
<th>(1) Number Employees</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Size 16+ x Fornero Reform</strong></td>
</tr>
<tr>
<td>(0.03449)</td>
</tr>
<tr>
<td><strong>Size 16+ x Jobs Act</strong></td>
</tr>
<tr>
<td>(0.03804)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
</tr>
<tr>
<td>(0.006288)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
</tr>
</tbody>
</table>

Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01

Note: Fornero Reform is equal to 1 for years 2013 through 2019. Jobs Act is equal to 1 for years 2016 to 2019. Treated firms are firms of size 16 to 25 in 2011 and control firms are between 6 and 15 in 2011. The sample is composed of firms between 3 and 50 workers in any year in the Orbis dataset. All models include year and firm fixed effects. Standard errors in parentheses. Errors clustered at the firm level. Number of clusters, i.e. firms, is 127,122. * p < 0.10, ** p < 0.05, *** p < 0.01.

of each firm’s year-to-year change in number of employees. By using the absolute value, I do not distinguish between increases or decreases in number of employees, but I consider the magnitude of any change. For example, if firm \( f \) has 12 employees in 2011, and 9 in 2012, then the dynamism variable equals 3 in 2012. Similarly, if firm \( f \) has 17 employees in 2014, and increases to 19 in 2015, then dynamism would equal 2 in 2015. Importantly, because with this metric I want to trace how firms around the cutoff are constrained by the reforms, I do not compare those just below the threshold to those just above it. In fact, my theoretical predictions suggest that the legislation hinders the flexibility of both these groups to change in size. Consequently, I assign to the treated group firms with 13 to 18 employees, while control firms are those further away from the cutoff: they have 10 through 12 and 19 to 20 employees. For this outcome, I chose a narrower size range for the control units to improve their homogeneity, because, when looking at dynamism, I pooled in control group firms on opposite sides of the cutoff. Additionally, the structure of the outcome variable requires different time subscripts compared to those in the specifications in Section 1.5. For
dynamism, the regression model is:

\[ \text{Dynamism}_{ft} = \alpha_{ft} + \beta \text{ForneroReform}_{t-1} \times \text{Large}_{f,t-1} + \gamma \text{JobsAct}_{t-1} \times \text{Large}_{f,t-1} + \text{Firm}_f + \text{Year}_t + \epsilon_{ft} \]  

(1.6)

That is, the indicator variable for the treated group, large_{f,t-1}, turns on if the firm had 16 or more employees. Similarly, the dummies for the post-reforms years, ForneroReform_{t-1} and JobsAct_{t-1}, refer to t – 1 and turn on if t – 1 is a post-reform year. The event study for this outcome, which shows that in the pre-reform period there were common trends and no anticipation effects across the treated and control groups, can be found in Figure A8 of Section 3.8.1.

Table 6 reports the estimates for the regressions with dynamism as the outcome variable. The coefficients indicate that the Fornero Reform increased firm dynamism around the cutoff by about 3.4%, and the Jobs Act boosted that effect by an additional 5.6% compared to the pre-reform period. Overall, both reforms increased dynamism by 10%.

The results on dynamism confirm the graphical and empirical evidence on size. They are also consistent with the prediction of the theoretical model: firms that are, by policy’s design, more constrained by employment protection legislation become more able to adjust their size once that legislation is weakened. These findings imply that the reforms were successful at decreasing firms’ hiring and termination rigidity, and at strengthening the flexibility of firms to choose their desired size.
Table 6: The Effects of Weakening Employment Protection on the Dynamism of Firms

<table>
<thead>
<tr>
<th></th>
<th>Dynamism</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fornero Reform</td>
<td>0.418***</td>
</tr>
<tr>
<td></td>
<td>(0.01627)</td>
</tr>
<tr>
<td>Jobs Act</td>
<td>0.213***</td>
</tr>
<tr>
<td></td>
<td>(0.01414)</td>
</tr>
<tr>
<td>Size 16+</td>
<td>-0.0167</td>
</tr>
<tr>
<td></td>
<td>(0.02107)</td>
</tr>
<tr>
<td>Size 16+ x Fornero Reform</td>
<td>0.0620***</td>
</tr>
<tr>
<td></td>
<td>(0.02209)</td>
</tr>
<tr>
<td>Size 16+ x Jobs Act</td>
<td>0.0540***</td>
</tr>
<tr>
<td></td>
<td>(0.01534)</td>
</tr>
<tr>
<td>Constant</td>
<td>1.424***</td>
</tr>
<tr>
<td></td>
<td>(0.01654)</td>
</tr>
<tr>
<td>Observations</td>
<td>1230294</td>
</tr>
</tbody>
</table>

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dynamism is the absolute value of the year-to-year change in firm size. I drop all firms that are lower than 3 or larger than 50 in any year, and I only keep those between 10 and 20 in 2011. Treated firms are those between 13 and 18 workers included. Control firms are those of size 10 to 12 and 19 to 20. Col. 1 focuses on the years around both policy changes (2011 to 2018). The post indicator variable for the Fornero Reform turns on if the year of reference for the size change, $year - 1$, is greater or equal to 2012. The Jobs Act indicator turns on if the year of reference for the size change is greater or equal to 2015. The model includes year and firm fixed effects. Standard errors in parentheses. Errors clustered at the firm level.
1.7.5 Regression Results for the Productivity Outcomes

The repercussions of employment protections on size are linked to those on productivity. Figure 10, which exhibits a discontinuity in the average per employee productivity of firms across the two sides of the cutoff, provides evidence of a productivity distortion in the pre-reform period. Firms with 13, 14, and 15 employees have higher average productivity compared to those on their left, suggesting that just below the threshold firms are generally more productive. Firms right above the cutoff display a lower average productivity per worker than their smaller and larger counterparts. If high termination costs incentivize firms to retain low productivity workers, those just above the threshold may be disproportionately affected by unprofitable labor because their size is relatively small.

In this section, I evaluate the impact of the reforms on the productivity of treated firms, those that employ 16 to 25 workers. In particular, I empirically test the last prediction of the model stating that the reforms had a positive effect on the performance of firms. To do so, I estimate the effects on per worker productivity outcomes, specifically operating revenue, sales, profit margin, cash flow, and value added, using the difference-in-differences identification model. The first two are quite direct measures of labor productivity. Profit margin is a metric for a firm’s overall profitability, so it accounts for both its total revenues and expenses. Cash flow and value added are productivity measures that encompass several aspects of a firm’s performance, including its capital structure, its financial situation, its investments, and its external contractors. Consequently, they act as less direct measures of employee productivity.

Operating revenues and sales are very similar measures because they both track the core activity of firms in thousands of Euros. Unsurprisingly, the regression results for these two outcomes match closely. Column (1) of Table 7 shows the estimates for operating revenues per employee in thousands of Euros, while Column (2) shows the estimates for sales in the same unit. The Fornero Reform increased average operating revenues per worker by almost 8,000 Euros, which represents a 3.2% increase, and the Jobs Act almost doubled that
effect, increasing operating revenues by over 6,709 Euros. The joint effect of the policies on operating revenue is an increase of 5.8%. Sales, meanwhile, grow by 2% after the Fornero Reform, and both policies cause an increase in sales of almost 5%, which corresponds to over 11,000 Euros more per worker in the average treated firm. Column (3) of Table 7 displays the estimates for profit margin. The Fornero Reform boosts profitability, which increases by almost 33% after 2012. However, that positive effect is eliminated by the Jobs Act, which decreases profit margins in treated firms by a similar amount. Unlike operating revenues and sales, profit margins account for the total cost of the inputs, in addition to revenue. This difference may explain why the Jobs Act nullifies the positive effect of the Fornero Reform on profit margins. An alternative explanation arises if I consider the implications of the reforms on the extensive margin of firms. In the longer run, higher profits caused by the Fornero Reform might generate firm creation which, in turn, increases competition. Hence, it is possible that the negative effect of the Jobs Act on profitability is driven simply by the new entrants putting downward pressure on profits. Columns (4) and (5) of Table 7 present the estimates for cash flow and added value, respectively. The estimated coefficients for cash flow suggest that the policies left it mostly unaffected. However, the Fornero Reform increased added value by over 3%, showing that it led to mild improvement in a productivity measure that captures the performance of firms more broadly.

Taken together, these results suggest that labor productivity has increased as a result of the reforms.
Figure 10: The Productivity Distribution of Firms around the Regulatory Threshold

Note: The dots represent the average operating revenues per employee by firms’ size in 2011 for firms between 6 and 25 employees in 2011. The shaded area highlights the firms that are more directly impacted by the employment protection legislation design because they are closer to the threshold.
Table 7: The Effects of Weakening Employment Protection on Productivity

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Operating Revenue</td>
<td>Sales Revenue</td>
<td>Profit Margin</td>
<td>Cash Flow</td>
<td>Added Value</td>
</tr>
<tr>
<td>Size 16+ x Fornero Reform</td>
<td>7.812*** (2.9241)</td>
<td>5.086* (2.9339)</td>
<td>0.0496*** (0.006780)</td>
<td>0.146 (0.7743)</td>
<td>1.541* (0.8417)</td>
</tr>
<tr>
<td>Size 16+ x Jobs Act</td>
<td>6.709** (2.8156)</td>
<td>6.263** (2.9776)</td>
<td>-0.0537*** (0.007524)</td>
<td>0.969 (0.7705)</td>
<td>0.880 (0.8992)</td>
</tr>
<tr>
<td>Constant</td>
<td>250.5*** (0.5638)</td>
<td>243.4*** (0.5190)</td>
<td>0.215*** (0.001041)</td>
<td>10.42*** (0.1350)</td>
<td>50.42*** (0.1565)</td>
</tr>
<tr>
<td>Observations</td>
<td>1054645</td>
<td>1049829</td>
<td>1044077</td>
<td>1021779</td>
<td>995824</td>
</tr>
</tbody>
</table>

Standard errors in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

Note: Fornero Reform is equal to 1 for years 2013 through 2019. Jobs Act is equal to 1 for years 2016 to 2019. Treated firms are firms of size 16 to 25 in 2011 and control firms are between 6 and 15 in 2011. The sample is composed of firms between 3 and 50 workers in any year in the Orbis dataset. All models include year and firm fixed effects. Errors clustered at the firm level. Number of clusters, i.e. firms, is 126,931 in Col. 1, 126,696 in Col. 2, 126,179 in Col. 3, 125,832 in Col. 4, and 124,986 in Col. 5. * p < 0.10, ** p < 0.05, *** p < 0.01.
1.7.6 Sensitivity Checks

To make sure the estimates from the difference-in-differences results are robust to alternative specifications, I run several sensitivity checks. First, I exclude firms with 13, 14, and 15 employees from the control group. Because firms just below the threshold were indirectly constrained by the legislation, the reforms might motivate them to make profitable hires and move above the cutoff. I report the estimates in Table A4 of the Appendix. When I exclude from the control groups firms just beneath the threshold, which may indirectly benefit from the reforms, the magnitude of the coefficients is stronger.

Second, because the reforms were implemented in March and July, I run alternative specifications where I consider 2012 as the first year post Fornero Reform, rather than 2013, and 2015 as the first year post Jobs Act, rather than 2016. These specifications, whose estimates can be found in Table A3 of the Appendix, yield similar results although the standards errors are larger.

Third, considering that Orbis does not comprehensively represent small firms, I also run a version of the difference-in-differences model where I use a smaller range of firm sizes. In particular, I assign to the control group firms between 10 and 15 employees, and to the treatment group those between 16 and 21. Results can be found in Table A5 of the Appendix and suggest that, overall, the main results are robust to this alternative choice for the treatment and control groups, though the estimation is less precise, likely due to the lower sample size.

1.8 Conclusion

Understanding the repercussions of regulations aimed at protecting workers is important for determining their social and economic value, especially in relation to their design. While job security is desirable, economists also measure the health of a labor market through other indicators, like the employment rate, productivity, and economic growth.

In this paper, I consider two reforms that aimed to boost the economy’s dynamism and
productivity by weakening employment protections. I study how employment protection legislation shapes the size distribution of firms as well as their performance through impacting their ability to change the number or composition of their employees. I evaluate how weakening employment protection legislation alters these outcomes and provide evidence on firms' response to the progressive loosening of such regulations, after they had been in place for decades. I evaluate the distortions introduced by a policy design that is common well beyond the realm of employment protection, which utilizes cutoffs to determine benefits and costs.

I rely on a theoretical model to show that employment protections affect the hiring and termination decisions of firms, and that the policies weakening such restrictions lead to a greater incidence of both these decisions, resulting in higher dynamism. Further, the model implies that termination costs motivate firms to retain unprofitable workers. Hence, the policy changes incentivize firms to terminate unprofitable workers they would have otherwise retained. Because of this mechanism, the model predicts that the reforms lead to an increase in the average productivity of firms. I test these predictions in the Italian context, where the strictness of employment protection legislation changes sharply depending on firm size, and where such a discontinuity was reduced by two recent reforms.

Using graphical evidence, I show that firms above the cutoff in 2011 distinctly move below it following the Fornero Reform. Moreover, all firms expand in the aftermath of the second reform. Additional preliminary evidence hints at strategic behavior on the part of firms: they tend to locate under the cutoff where regulation is weaker, and they avoid being right above it, suggesting that a few extra workers is generally not worth the harsher restrictions. Relying on a bunching design, I document the presence of significant excess mass below the cutoff, and weaker evidence of missing mass above it. Further, I find that the bunching and the "hole" along the size distribution of firms decrease in magnitude after the policies are implemented, though they are not statistically different from the pre-reform quantities. In particular, the excess mass below the cutoff decreases by 70% during the reform period.
To explore how the new policies impacted firms’ size, dynamism, and productivity, I rely on a difference-in-differences framework. I show that after over 40 years of restrictions, firms mostly respond to the weakening of the legislation first by removing employees, perhaps the most unproductive ones, and that this effect lessens over time. I operationalize the dynamism of firms with a novel measure capturing year-to-year changes in size to assess the ability of firms to grow or shrink. I find that both policies increase the dynamism of firms by about 10%, denoting that weaker regulations make firms less reluctant to alter their dimensions. Finally, I focus on productivity outcomes. I provide descriptive evidence on the productivity distortion: firms right below the cutoff are more productive, on average, than those right above. This indicates that productive firms are hesitant to expand, and unproductive firms are hesitant to shrink as desired. The difference-in-differences estimates indicate that the reforms positively impact outcomes that more directly measure labor productivity. Operating revenues grow by almost 6%, and sales by over 4.5%. Profit margin boosts at first, but go back to their initial levels following the Jobs Act. Cash flow appears unaffected by the reforms, while added value increases by 3.2% following the Fornero Reform. The productivity results suggest that the increase in firm performance following the reforms is likely explained by more efficient employment matches.

These results are not free of limitations. The lack of data on earlier years and, especially, on a long period before the reforms were implemented, calls for some caution in interpreting the results. In fact, despite the robustness checks and sensitivity analyses I provide, my ability to ascertain the lack of nonparallel trends or anticipation effects long before the reform period is limited. However, the sudden nature of the Fornero Reform, and the presence of several consistent graphical and empirical results, alleviate this concern.

Debates on the appropriate design and strictness level of employment protection regulations are commonly ongoing across the world, including in the U.S. (Kings, 2022; Nguyen, 2022), Canada (Human Resources Director, 2022), United Kingdom (Ford, 2022), Germany (Ritter, 2022), France (Artus and Ragu, 2022), and the Netherlands (Jan Kamps, 2022).
This work contributes to these discussions on the wanted and undesired effects of employment protection laws and of their ubiquitous design based on a sharp cutoff in the number of employees in a firm. My findings show potential inefficiencies deriving from the sharp increase in termination costs by the size of firms. Moreover, I estimate that, in the short- and medium-term, recent reforms that progressively relaxed employment protections in a country where these have been historically strict had positive effects on firms’ performance.

Several channels could be behind the positive impact of the reforms on labor productivity: firms terminating their least productive workers, increased dynamism, or employees working harder in order to keep their less secure jobs. Disentangling these mechanisms would be an interesting venue for future research. Further, more evidence is needed on the impact of alternative employment protection designs to help identify those that work best at ensuring job security in the short and long term, and comprehensively across all members of society.
Bibliography


Human Resources Director, C. (2022). How can employers prevent constructive dismissal claims?


1.9 Appendix

1.9.1 Orbis Data

To understand the implications of the limitations related to the representativeness of the Orbis sample, it is important to keep in mind that larger and highly functioning firms are more likely to produce and disclose financial statements, leading to an underrepresentation of small and underperforming companies. Using alternative data sources, Bajgar et al. (2020) has shown that there are significantly more small firms in the economy than represented in Orbis. Firms in Orbis are also more productive, on average, compared to the true productivity distribution, even within the same size class (Bajgar et al., 2020). Further, due to the fast increase in coverage, the Orbis data quickly change in composition over time, imposing the need for caution in over time comparisons. In fact, because the representativeness increases with coverage, Orbis becomes less disproportionate in the share of large and productive firms represented in the dataset over time. Because this study focuses on two Italian policies, I discuss the data representativeness and its implications on the analysis and results for Italy. Starting in 2016, Italy introduced regulations on filing rules imposing firms of all sizes to report to the local business registries, which increases the availability of information on size and financial performance. However, coverage is not full in Orbis because the specifics of these regulations vary depending on size and financial criteria.21 This also means that, before the 2016 financial reform, coverage is even more incomplete.

I used aggregated ASIA data available in the online portal of ISTAT, the Italian Statistics Institute, to measure the total number of active enterprises by size class and year. I compare these counts to the corresponding Orbis counts.

---

21Details on the changes introduced at: https://www.pwc.com/
Table A1: Orbis Coverage by Firm Size Class and Year

<table>
<thead>
<tr>
<th>Year</th>
<th>ASIA 0 to 9</th>
<th>ORBIS 0 to 9</th>
<th>Cvg.</th>
<th>ASIA 10 to 49</th>
<th>ORBIS 10 to 49</th>
<th>Cvg.</th>
<th>ASIA 50 to 249</th>
<th>ORBIS 50 to 249</th>
<th>Cvg.</th>
</tr>
</thead>
<tbody>
<tr>
<td>2013</td>
<td>4,185,081</td>
<td>286,600</td>
<td>0.068</td>
<td>190,464</td>
<td>91,751</td>
<td>0.482</td>
<td>21,385</td>
<td>15,050</td>
<td>0.704</td>
</tr>
<tr>
<td>2014</td>
<td>4,158,660</td>
<td>310,220</td>
<td>0.075</td>
<td>175,742</td>
<td>96,839</td>
<td>0.551</td>
<td>21,106</td>
<td>15,945</td>
<td>0.755</td>
</tr>
<tr>
<td>2015</td>
<td>4,136,831</td>
<td>329,854</td>
<td>0.08</td>
<td>176,332</td>
<td>105,677</td>
<td>0.599</td>
<td>21,256</td>
<td>17,255</td>
<td>0.812</td>
</tr>
<tr>
<td>2016</td>
<td>4,180,870</td>
<td>347,979</td>
<td>0.08</td>
<td>184,098</td>
<td>113,313</td>
<td>0.616</td>
<td>22,156</td>
<td>18,512</td>
<td>0.836</td>
</tr>
<tr>
<td>2017</td>
<td>4,179,818</td>
<td>1,827,415</td>
<td>0.437</td>
<td>191,004</td>
<td>135,259</td>
<td>0.708</td>
<td>22,906</td>
<td>20,380</td>
<td>0.890</td>
</tr>
<tr>
<td>2018</td>
<td>4,180,761</td>
<td>1,997,697</td>
<td>0.478</td>
<td>196,076</td>
<td>145,466</td>
<td>0.742</td>
<td>23,647</td>
<td>21,844</td>
<td>0.924</td>
</tr>
<tr>
<td>2019</td>
<td>4,149,572</td>
<td>2,184,393</td>
<td>0.526</td>
<td>199,340</td>
<td>153,251</td>
<td>0.769</td>
<td>24,288</td>
<td>23,193</td>
<td>0.955</td>
</tr>
<tr>
<td>2020</td>
<td>4,211,615</td>
<td>2,307,819</td>
<td>0.548</td>
<td>187,674</td>
<td>152,509</td>
<td>0.813</td>
<td>23,831</td>
<td>23,155</td>
<td>0.972</td>
</tr>
</tbody>
</table>

Note: Orbis Coverage (i.e. “Cvg.” on the table) is the Orbis count divided by the ASIA count. ASIA count is obtained from [https://www.dati.istat.it/](https://www.dati.istat.it/) by clicking on: Enterprise -> Structure -> Enterprises and persons employed -> Data Summary.

When information on the number of employees is missing, firms size distribution might be misrepresented. The higher the coverage, the more Orbis includes the universe of Italian firms. The more homogeneous is the coverage across firm sizes and years, the more representative the Orbis sample is over time and across firm size classes. Table A1 shows, however, that coverage is highly heterogeneous: it increases over time (i.e. between 2013 and 2020) and it increases with the size of firms. The lowest coverage is for firms between 0 and 9 employees in 2013 (less than 7%) and the highest being for firms between 50 to 249 employees in 2020 (about 97%). Between 2013 and 2020, coverage increases by 800% for firms of class 0-9 workers, it increases by 67% for firms between 10 and 49 workers, and by 38% for those with 50 and 249 workers. The disproportionate increase in coverage for small firms suggests that Orbis becomes more representative of Italian firms over time, especially between 2016 and 2017 (coverage jumps from 0.08 to 0.43) as a result of the change in reporting rules. Because the data composition changes so dramatically, I cannot treat the entire distribution of firms in 2020 and that in 2013 as comparable. As mentioned in Section 3.4, I deal with these limitations following the suggestions in Bajgar et al. (2020); Kalemli et al. (2022) that are applicable to my context. In particular, I provide robustness results from analyses that only include firms with 10 or more employees. Second, I analyze within-firm response to the
reforms through the use of firm level fixed effects. Third, I carry out a separate analysis for the effects of the second policy, implemented in 2015, when the coverage was larger.

The representativeness problem also concerns the productivity variables.\footnote{Bajgar et al. (2020) and Gal (2013) discuss several ways to increase coverage of productivity measures (i.e. value added and total factor productivity), either by internal or external imputation. However, in their ranking of the coverage of TFP variables as a share of the observations with financial variables in Orbis, they find Italy to be at the top of the list among all countries they consider.} Hence, it is likely that firms with no financial record have a different productivity distribution compared to those that are included. However, in the data, I found that most firms with information on the number of employees have also information about their operating revenues and/or sales. These two productivity measures are very well represented: in 2011, only 50 out of 306,160 observations don’t have information on operating revenues, and 12,109 miss information on sales. By using many productivity measures, I can investigate the impact of the reforms on different aspects of firms’ performance while at the same time exploring the trends of variables with different degrees of missing values. As a useful comparison from an alternative data source, Table A2 shows the availability of accounting information from survey data, which, however, largely imputes information for Italian firms under 50.

Note: These numbers come from an Italian Survey of firms. https://www.dati.istat.it/ by clicking on: Enterprises – >Competitiveness – >Enterprises economic indicators – >Data Summary

<table>
<thead>
<tr>
<th>Year</th>
<th>ASIA 0 to 9</th>
<th>ASIA 10 to 19</th>
<th>ASIA 20 to 49</th>
<th>ASIA 50 to 249</th>
</tr>
</thead>
<tbody>
<tr>
<td>2013</td>
<td>4,094,444</td>
<td>127,998</td>
<td>50,760</td>
<td>20,897</td>
</tr>
<tr>
<td>2014</td>
<td>4,065,829</td>
<td>124,461</td>
<td>49,571</td>
<td>20,639</td>
</tr>
<tr>
<td>2015</td>
<td>4,043,032</td>
<td>125,029</td>
<td>49,584</td>
<td>20,795</td>
</tr>
<tr>
<td>2016</td>
<td>4,085,324</td>
<td>130,714</td>
<td>51,610</td>
<td>21,716</td>
</tr>
<tr>
<td>2017</td>
<td>4,095,213</td>
<td>131,560</td>
<td>52,341</td>
<td>22,058</td>
</tr>
<tr>
<td>2018</td>
<td>4,088,057</td>
<td>134,193</td>
<td>53,914</td>
<td>22,603</td>
</tr>
<tr>
<td>2019</td>
<td>3,990,961</td>
<td>135,638</td>
<td>55,137</td>
<td>23,186</td>
</tr>
</tbody>
</table>
1.9.2 Placebo Checks of the Graphs

In this section, I reproduce Figures 5 and 6 using placebo sizes. I consider firms between 7 and 10 employees in 2011, and between 21 and 24 employees in 2011. Firms in both of these ranges are far enough from the cutoff that they should be mostly unconstrained by it. Hence, they should experience no size distortion. Figure A1 plots firms between size 7 and 10 in 2011, and Figure A2 plots those between 21 and 24 in 2011. Both figures are reassuring because, as expected, their mode does not shift from the initial size interval, showing no evidence of these firms response to the reforms.

1.9.3 Sensitivity of the Bunching Estimates

In this section, I show the robustness of the results in Section 1.7.2 by reporting the replication of those graphs using alternative degrees for the polynomial and a different bandwidth. Overall, the main takeaways listed the Section 1.7.2 hold because they are not sensitive to these alternative specifications, especially for the more flexible specifications using polynomials of fifth order or higher. Figure A4 shows bunching estimates corresponding to the excess percentage of firms below the cutoff when varying the degree of the polynomial and keeping the bandwidth fixed at 13 to 18 workers. Figure A3 shows the estimates of the missing percent of firms above the cutoff when varying the degree of the polynomial and again keeping the bandwidth fixed between 13 and 18 workers. Figures A5 and A6 show the sensitivity to the bandwidth by plotting estimates of the excess and missing percentage of firms below and above the notch, respectively, when varying the order of the polynomial with a bandwidth at 14 to 17 employees.
Figure A1: Size Evolution of Firms in the 7 to 10 employees range in 2011

Size Distribution over Time of Firms between 7 and 10 Workers in 2011

Note: I consider only firms that were between size 7 and 10 in 2011. I plot their size distribution over time, in years 2014, 2016, 2017, and 2019. To improve readability, I curtailed the right hand tails at 40, excluding firms that have grown above that size. Of course, as some of these firms drop out of the sample, or perhaps grow beyond 40, the size of the sample in each frequency graph decreases over time. The two vertical red dashed lines are drawn corresponding to size 7 to 10, which is the range that, in 2011, contained all the firms displayed in the distributions.
Figure A2: Size Evolution of Firms in the 21 to 24 employees range in 2011

Note: I consider only firms that were between size 21 and 24 in 2011. I plot their size distribution over time, in years 2014, 2016, 2017, and 2019. To improve readability, I curtailed the right hand tails at 40, excluding firms that have grown above that size. Of course, as some of these firms drop out of the sample, or perhaps grow beyond 40, the size of the sample in each frequency graph decreases over time. The two vertical red dashed lines are drawn corresponding to size 21 to 24, which is the range that, in 2011, contained all the firms displayed in the distributions.
Figure A3: Bunching between 13 and 15 Employees Varying Polynomial Order from Third to Ninth

Note: I estimate the bunching estimate for the range between 13 to 15 workers, that is below the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using regressions with polynomials of varying order - between 3 and 9 - of the percentage of firms in each size bin on the number of employees, omitting the range 13 to 18 employees. The estimate is derived summing up the excess of that percent mass of firms at 13, 14, and 15 workers. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and boostrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
Figure A4: Missing Mass between 16 and 18 Employees Varying Polynomial Order from Third to Ninth

Note: I estimate the bunching estimate for the range between 16 to 18 workers, that is above the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using regressions with polynomials of varying order - between 3 and 9 - of the percentage of firms in each size bin on the number of employees, omitting the range 13 to 18 employees. The estimate is derived summing up the missing of that percent mass of firms at 16, 17, and 18 workers. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and bootstrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
Figure A5: Bunching in between 14 and 15 Employees Varying Polynomial Order from Third to Ninth

Excess Mass of Firms between 14 and 15 employees over Time by Polynomial Degrees

Note: I estimate the bunching estimate for the range between 14 to 15 workers, that is below the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using regressions with polynomials of varying order - between 3 and 9 - of the percentage of firms in each size bin on the number of employees, omitting the range 14 to 17 employees. The estimate is derived summing up the excess of that percent mass of firms at 14 and 15 workers. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and boostrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
Figure A6: Missing Mass between 16 and 17 Employees Varying Polynomial Order from Third to Ninth

Note: I estimate the bunching estimate for the range between 16 to 17 workers, that is below the notch, following the methodology described in Section 1.5.1. I estimate the counterfactual density using regressions with polynomials of varying order - between 3 and 9 - of the percentage of firms in each size bin on the number of employees, omitting the range 14 to 17 employees. The estimate is derived summing up the excess of that percent mass of firms at 16 and 17 employees. The 95 level confidence intervals are computed using standard errors from a procedure of bootstrapping with replacement as described in Chetty et al. (2011). I repeat the bunching estimation and bootstrapping procedure for each year between 2011 and 2019, and I plot the evolution of the estimates. The two vertical lines mark the timing of the reforms: the Fornero Reform was passed in 2012 and the Jobs Act in 2015.
1.9.4 Event Studies

The event study regressions include indicator functions that trace out changes in the outcome variables in the years leading to and following the implementation of each reform. These regressions estimate the presence of differential effects in the outcomes in the years around the implementation of the reform compared to a baseline reference period. In my data, the earliest year for which I have enough available observations is 2010. Hence, for the purposes of the event study exercise, I only keep firms present in 2010. As a consequence, the dataset gets smaller for the upcoming years, as some of these firms drop out of the sample. The pre-reforms period I consider is 2010 to 2011. The baseline reference year is 2011. In the event studies, I consider 2012 to be the adoption year, which makes the impacts of the Jobs Act refer to 2011. I generate a variable, $YSA_t = t - 2012$, which measures the number of years between year $t$ and 2012, the implementation year of the Fornero Reform. For example $YSA_t = 2$ in year $t = 2014$, and $YSA_t = -2$ in year $t = 2010$. $P(k)_t$ is an indicator variable equal to 1 when $YSA_t = k$, that is $P(k)_t = 1(YSA_t = k)$. Treated firms are those of size between 16 and 25 in 2011, that is $treated_{2011} = 1(16 \leq employees_{2011} \leq 25)$. The event study model regresses each outcome for firm $f$ in year $t$ on dummy variables for each level of $P(k)_t$ interacted with $treated_{2011}$, and includes year and firm fixed effects:

$$Outcome_{ft} = \alpha_{-2}P(-2)_{treat2011} + \sum_{k=0}^{K=7} [\alpha_k P(k)_{t}treat\}_{t2011} + \gamma_t + \delta_f + \epsilon_{ft} \quad (1.7)$$

Because 2011 is the reference year, its interaction with $treated_{2011}$, $P(-1)_{t}Treat_{2011}$, is omitted from the model. This implies that all the $\alpha$ coefficients should be interpreted as the differential effects of the policies in year $k$ compared to 2011. Because I use data up to 2019, $K = 7$ refers to 2019, the last one available in the data. I clustered standard errors at the firm level. In the model, $\alpha_{-2}$ estimates the response of each outcome variable to the future implementation of the Fornero Reform. It suggests whether there are anticipation effects of the upcoming reform that vary across treatment and control firms. I expect $\alpha_{-2}$ not to
be statistically different from zero because, under the strict exogeneity assumption, future events should not impact present outcomes. In contrast, $\sum_{k=0}^{K=7} \alpha_k$ are the post-reform(s) coefficients. In particular, $\sum_{k=0}^{k=3} \alpha_k$ measures the effect of the Fornero Reform during each post-reform year before the Jobs Act. Instead, $\sum_{k=4}^{k=7} \alpha_k$ measures the effect of the Jobs Act and the Fornero Reform in the aftermath of the Jobs Act compared to the baseline year. The variation in $\alpha_k$ provides information on the time varying effect of these reforms. If the impact of the policy is drastic and remains constant over the years, then $\alpha_k$ will be positive and fixed as $k$ changes. However, in the more likely case that the effects arise progressively and change over time, $\alpha_k$ will estimate these varying patterns. In the graphs below I plot the estimates of the event studies obtained by running Equation 1.7 for all the outcomes: log number of employees, operating revenues, sales, profit margin, cash flow, and value added. No significant pre-trends during 2010-2011 emerge from any of the graphs below, alleviating the concerns on anticipation effects. Profit Margin represents an exception, because it appears to be slightly increasing between 2010 and 2011, but by a weak magnitude. Unsurprisingly, the confidence intervals tend to become wider over the years as firms in 2010 drop out of the sample. Overall, the event studies contribute to the plausibility of the parallel trends and strict exogeneity assumptions. The large published literature, part of which is cited in Section 3.2, that exploits this threshold in the Italian legislation further reinforces the identification of the models I use in this paper.
Figure A7: Event Study for Number of Employees

Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Figure A11: Event Study for Profit Margin Before Tax

Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Figure A12: Event Study for Cash Flow

Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
Figure A13: Event Study for Added Value

Event Study
Lead and Lags of Added Value per Employee
Control firms 5-15 in 2011
2011 is Baseline Year

Note: The reference year is 2012, where the x-axis indicates 0. The Fornero Reform occurs in 2012, at 0, and the Jobs Act in 2015, at 3. The pre-policy period used as reference is 2010 to 2012, corresponding to the points -2 to 0 in the x-axis. The graph also plots the 95% confidence interval obtained from clustering standard errors at the firm level.
1.9.5 Sensitivity Checks for the Difference-in-Differences Models
Table A3: Model with Alternative Post-reform years

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number</td>
<td>Operating</td>
<td>Sales</td>
<td>Profit</td>
<td>Cash</td>
<td>Added</td>
</tr>
<tr>
<td></td>
<td>Workers</td>
<td>Revenues</td>
<td>Margin</td>
<td>Flow</td>
<td>Value</td>
<td></td>
</tr>
<tr>
<td>Size 16+ x Fornero Reform</td>
<td>-0.490***</td>
<td>4.044</td>
<td>4.079</td>
<td>0.0952***</td>
<td>-0.456</td>
<td>0.730</td>
</tr>
<tr>
<td></td>
<td>(0.03163)</td>
<td>(2.7490)</td>
<td>(2.7956)</td>
<td>(0.007032)</td>
<td>(0.7239)</td>
<td>(0.8065)</td>
</tr>
<tr>
<td>Size 16+ x Jobs Act</td>
<td>-0.0926**</td>
<td>10.14***</td>
<td>9.005***</td>
<td>-0.0533***</td>
<td>1.382</td>
<td>1.931*</td>
</tr>
<tr>
<td></td>
<td>(0.03951)</td>
<td>(3.4751)</td>
<td>(3.3822)</td>
<td>(0.006952)</td>
<td>(0.9210)</td>
<td>(1.1069)</td>
</tr>
<tr>
<td>Constant</td>
<td>12.21***</td>
<td>250.5***</td>
<td>243.0***</td>
<td>0.207***</td>
<td>10.45***</td>
<td>50.40***</td>
</tr>
<tr>
<td></td>
<td>(0.007259)</td>
<td>(0.5996)</td>
<td>(0.5491)</td>
<td>(0.001315)</td>
<td>(0.1646)</td>
<td>(0.1858)</td>
</tr>
<tr>
<td>Observations</td>
<td>1055258</td>
<td>1054645</td>
<td>1049829</td>
<td>1044077</td>
<td>1021779</td>
<td>995824</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01

Note: Fornero Reform is equal to 1 for years 2012 through 2019. Jobs Act is equal to 1 for years 2015 to 2019. Treated firms are firms of size 16 to 25 in 2011 and control firms are between 6 and 15 in 2011. The sample is composed of firms between 3 and 50 workers in any year in the Orbis dataset. All models include year and firm fixed effects. Standard errors in parentheses. Errors clustered at the firm level. All productivity outcomes (i.e. operating revenues, sales, profit margin, cash flow, and added value) are measured per worker.

Table A4: Model with Alternative Control Group

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number</td>
<td>Operating</td>
<td>Sales</td>
<td>Profit</td>
<td>Cash</td>
<td>Added</td>
</tr>
<tr>
<td></td>
<td>Workers</td>
<td>Revenues</td>
<td>Margin</td>
<td>Flow</td>
<td>Value</td>
<td></td>
</tr>
<tr>
<td>Size 16+ x Fornero Reform</td>
<td>-0.594***</td>
<td>8.485***</td>
<td>5.005</td>
<td>0.0560***</td>
<td>0.198</td>
<td>1.737**</td>
</tr>
<tr>
<td></td>
<td>(0.03460)</td>
<td>(3.1145)</td>
<td>(3.1312)</td>
<td>(0.007387)</td>
<td>(0.7954)</td>
<td>(0.8626)</td>
</tr>
<tr>
<td>Size 16+ x Jobs Act</td>
<td>0.110***</td>
<td>7.346**</td>
<td>7.141**</td>
<td>-0.0615***</td>
<td>1.368*</td>
<td>1.208</td>
</tr>
<tr>
<td></td>
<td>(0.03826)</td>
<td>(2.9999)</td>
<td>(3.2202)</td>
<td>(0.008072)</td>
<td>(0.7647)</td>
<td>(0.9382)</td>
</tr>
<tr>
<td>Constant</td>
<td>11.79***</td>
<td>245.6***</td>
<td>238.6***</td>
<td>0.226***</td>
<td>10.24***</td>
<td>49.68***</td>
</tr>
<tr>
<td></td>
<td>(0.007468)</td>
<td>(0.7069)</td>
<td>(0.6388)</td>
<td>(0.001333)</td>
<td>(0.1596)</td>
<td>(0.1880)</td>
</tr>
<tr>
<td>Observations</td>
<td>892482</td>
<td>891951</td>
<td>887815</td>
<td>882937</td>
<td>863511</td>
<td>841099</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01

Note: Fornero Reform is equal to 1 for years 2013 through 2019. Jobs Act is equal to 1 for years 2016 to 2019. Treated firms are firms of size 16 to 25 in 2011 and control firms are between 6 and 12 in 2011, hence it excludes those right below the threshold. These firms, which were indirectly constrained by the employment protection legislation, may indirectly benefit from them. The sample is composed of firms between 3 and 50 workers in any year in the Orbis dataset. All models include year and firm fixed effects. Standard errors in parentheses. Errors clustered at the firm level. All productivity outcomes (i.e. operating revenues, sales, profit margin, cash flow, and added value) are measured per worker.
Table A5: Model including firms from a narrower range of the size distribution

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number Workers</td>
<td>Operating Revenues</td>
<td>Sales</td>
<td>Profit Margin</td>
<td>Cash Flow</td>
<td>Added Value</td>
</tr>
<tr>
<td>Size 16+ x Fornero Reform</td>
<td>-0.345***</td>
<td>2.391</td>
<td>2.419</td>
<td>0.0179**</td>
<td>-0.0697</td>
<td>0.672</td>
</tr>
<tr>
<td></td>
<td>(0.04096)</td>
<td>(3.3681)</td>
<td>(3.3751)</td>
<td>(0.008040)</td>
<td>(1.0964)</td>
<td>(1.1565)</td>
</tr>
<tr>
<td>Size 16+ x Jobs Act</td>
<td>0.0872*</td>
<td>2.987</td>
<td>1.402</td>
<td>-0.0237***</td>
<td>-0.0279</td>
<td>0.516</td>
</tr>
<tr>
<td></td>
<td>(0.04575)</td>
<td>(2.9674)</td>
<td>(2.9466)</td>
<td>(0.009039)</td>
<td>(1.0630)</td>
<td>(1.1256)</td>
</tr>
<tr>
<td>Constant</td>
<td>14.41***</td>
<td>256.7***</td>
<td>248.9***</td>
<td>0.156***</td>
<td>11.01***</td>
<td>52.47***</td>
</tr>
<tr>
<td></td>
<td>(0.01068)</td>
<td>(0.8960)</td>
<td>(0.8924)</td>
<td>(0.001756)</td>
<td>(0.2827)</td>
<td>(0.3054)</td>
</tr>
<tr>
<td>Observations</td>
<td>533401</td>
<td>533164</td>
<td>530911</td>
<td>528133</td>
<td>518131</td>
<td>506305</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
* p < 0.10, ** p < 0.05, *** p < 0.01

Note: Fornero Reform is equal to 1 for years 2013 through 2019. Jobs Act is equal to 1 for years 2016 to 2019. Treated firms are firms of size 10 to 15 in 2011 and control firms are between 16 and 21 in 2011. These firms, which were indirectly constrained by the employment protection legislation, may indirectly benefit from them. The sample is composed of firms between 3 and 50 workers in any year in the Orbis dataset. All models include year and firm fixed effects. Standard errors in parentheses. Errors clustered at the firm level. All productivity outcomes (i.e. operating revenues, sales, profit margin, cash flow, and added value) are measured per worker.
Chapter 2

The Effects of Weakening Employment Protection Legislation on Job Flows

Laura Montenovo

Abstract

Using administrative data on the employment histories of about 1.8 million workers between 2007 and 2018, I explore the impact of weakening employment protection legislation on job flows and job match quality. I consider two Italian reforms that in 2012 and 2015 progressively reduced the termination costs for firms with 16 or more employees by rescinding the power of labor courts. Relying on a difference-in-differences strategy, I find that both reforms jointly increased termination by 36% and hiring by 45% among the impacted firms. The first reform increased the rate at which workers switch jobs by 23%. I consider several measures for job match quality: tenure, starting earnings, increase in job rank, and overall earnings. The reforms did not significantly impact any of these outcomes. I explore the heterogeneity of these impacts by gender, age, and geography. Overall, women experience a lower increase in hiring compared to men, but 4% greater earnings on their first year of their next job. Workers in Northern firms benefit from 45% greater hiring rates compared to their counterparts in the Center-South. I conclude that policies relaxing historically strict employment protection result in greater termination, but they also lead to significantly higher hiring and job turnover, which overall increase the flexibility of the labor market.

Keywords: Employment Protections; Job Flows; Workers’ Rights; Job Match Quality.
JEL codes: J24; J28; J30; J41; L5; J62; J63; J81; J83
2.1 Introduction

Employment protection legislation broadly regulates the costs firms have to face when terminating employees. If these costs are high, workers with jobs protected by such legislation benefit from more stable job trajectories. However, in an economy characterized by strict employment protection, firms might be less willing to hire to avoid the risk of incurring those costs in the future. Similarly, firms might be more hesitant to replace poor-quality employment matches with better ones due to the costs arising from changing the composition of the workforce.

Employment protection laws apply to long-term employment positions, leaving workers in temporary jobs unprotected. However, workers sort into different occupations based on their socio-demographic characteristics. For example, young workers are more likely to be temporary workers or unemployed. Unemployment rates of workers between 15 and 24 years old in Italy reached 43 percent in the first quarter of 2014, and it was almost 33 percent for those between 18 and 29 years old in the same time period (ISTAT, National Institute of Statistics in Italy). As a consequence, in tightly regulated labor markets, employment protection legislation might perpetuate undesirable economic trends, like high youth unemployment, that disproportionately affect some population subgroups. Hence, understanding the extent to which employment protection represents a barrier to hiring, especially for the socio-demographic groups that are most vulnerable in the labor market, is paramount.

In this paper, I investigate the effect of a progressive relaxation of employment protection on job flows, in particular on termination, hiring, and job switching, and on job match quality. In 2012 and 2015, the Italian government took concrete steps to increase the flexibility of the labor market by easing the expected costs of dismissal for firms with 16 or more employees. Among the approved measures, one reduced the number of cases where judges were allowed to order the reinstatement of workers in case of unfair termination. The measures, in addition to lowering termination costs, de facto decreased the economic risk of employers with more than 15 workers to hire workers. I use Italian administrative data on the entire working
histories of a large random sample of individuals, and I rely on a difference-in-differences identification strategy where firms of size 16 or more in the pre-reforms period are in the treatment group. I consider post-reforms changes in the rate of termination and hiring among these firms. Next, I use job switching as an outcome to study whether the reforms facilitated the reallocation of workers across jobs. Further, because workers' reallocation might result, in the medium- or long-term, in higher quality job matches, I build outcomes aimed at measuring job match quality and I explore whether they change post-reforms. I measure job match quality through four different outcomes: tenure, starting earnings, changes in job ranks, and weekly earnings. Finally, because employment protection does not affect all workers equally and because labor courts display great variation across macro-regions in Italy, I explore heterogeneous treatment effects by age, gender, and geographical area.

I find that the Fornero Reform, which weakened employment restriction legislation in 2012, increased termination rates by 8% and hiring rates by 24%. The Jobs Act, which was enacted in 2015, led to a further boost in hiring by 29%, and increased termination by 9% but this estimate is not statistically significant. The Fornero Reform increases job switching by 23%, and the Jobs Act does not change that trend. Neither reform seems to significantly impact any of the job match quality measures. After analyzing the effects on the average levels of the above outcomes across the entire population of outcomes, I focus on the heterogeneous effects of the reforms. In contrast with theoretical considerations and with the stated goals of the policies, young workers do not seem to particularly benefit from the changes. Following the Jobs Act, female workers experience 10% less hiring compared to male workers, but their starting earnings increase by 4% more compared to males after the Fornero Reform. The regions in the North of Italy have historically been more developed than the South. Moreover, the labor court system, which has been a major intermediary for the enforcement of employment protection laws, presents a great variety in its functioning depending on geographical location. As a result, I examine how the reforms apply to different local labor markets in Italy. By using the information on the firm’s location, I found that the
increase in hiring is 45% greater for firms in the North compared to the ones in the rest of Italy, pointing at a better response to the reforms among firms in stronger local economies. I found no heterogeneous treatment effect by region in regard to job match quality outcomes.

The findings contribute to the literature on the effects of large labor market reforms that relax employment protection legislation on labor market dynamics. I quantify the changes in termination and hiring rates following such reforms. I show that concerns regarding job security under weaker employment protection are alleviated by the presence of large gains in hiring that happen simultaneously. Moreover, job turnover, as measured by workers’ probability to switch jobs in a three-months time window, increases. This finding signals improvements in the labor market flexibility, which was originally a stated goal of both reforms. By contrast, I found no evidence that such added flexibility resulted in improved quality of job matches in the short term. However, updating the data to several post-reform years might lead to different findings. Further, I showed that the reforms were unsuccessful at improving the labor market outcomes of the most vulnerable workers: women, youth, and those in the South, where labor markets are more fragile.

This paper is organized as follows. Section 3.2 summarizes the relevant literature. Sections 2.3 and 3.3 summarize the conditions of the labor markets in Italy and the details on both 2012 and 2015 EPL reforms. Section 2.5 highlight the expected theoretical mechanisms that labor economics theory would suggest I should find in the data. Section 3.4 describes the dataset used in the analysis, Section 3.5 explains the empirical strategy employed, while Section 3.6 reports the results. Finally, Section 3.7 concludes.

2.2 Literature Review

Most of the literature that is relevant to this work focuses on the role that employment protection legislation plays in shaping labor market dynamics and outcomes like employment, unemployment, and job flows. For a review of the existing literature on employment protection refer to Bertola et al. (2000) and Addison and Teixeira (2003). Much of the
literature on employment protection uses cross-country data. Cazes and Nesporova (2003) employ descriptive cross-European country analyses to show that strict employment protection legislation is associated with higher temporary contracts, higher unemployment, lower job turnover, lower self-employment, higher tenure for permanent contract workers, and lower tenure for the most vulnerable groups. Similar findings arise in Millán et al. (2013), which looks at the effects of employment protection on hiring and termination among small firms in Europe using survey data. Although the authors cannot draw conclusions on the net effects, they find that employment protection laws are negatively associated with the probability that own-account workers take on employees and hence become employers. On the other hand, they also find that strict employment protection lowers the number of job dismissals. Feldmann (2009) uses survey data from 19 countries to capture the strictness of hiring and termination regulations perceived among senior business executives and finds that more flexible regulations decrease unemployment and increase employment rates. Martin and Scarpetta (2012) review recent empirical evidence on the relationship between employment protection legislation and job flows, labor reallocation, and productivity across OECD countries. They conclude that strict employment protection legislation negatively impacts labor flows and slows down productivity growth. Other literature uses within-country variation to study the role that employment protection legislation plays in labor market dynamics. Gimpelson et al. (2010) exploit wide variation in enforcement of employment protection legislation across Russian regions, cities, or segments of firms to show that higher enforcement is associated with lower employment and higher unemployment. Using U.S. data on state courts and exploiting their progressive adoption of wrongful-discharge protection between 1970 and 1999, Autor et al. (2007) similarly finds that employment protection legislation reduces employment flows and flow entry rates.

In Italy, there is a size cutoff that determines the strictness of employment protection. In particular, termination costs are higher for firms with 16 employees or more. Such threshold shapes the distribution of firm sizes and labor market outcomes for workers employed in firms.
across the sides of the threshold. Several papers using data from Italy exploit the 15-worker
cutoff to investigate how employment protection shapes the labor market. Leonardi and
Pica (2013) focuses on an Italian 1990 reform that introduced dismissal costs for small firms
to explore its effects on wages. They find a small average wage reduction which, however,
hides strong heterogeneous effects. Workers who switch jobs after the policy experience a
drop in entry wage, while incumbent workers bear no change. Their results point to the role
that workers’ bargaining power plays in employment protection systems of varying strictness.
Hijzen et al. (2017) consider the same cutoff and, using a regression discontinuity design on
2008-2009 data, compare outcomes for firms above and below the 15-workers threshold. The
authors find that employment protection legislation increases worker turnover as measured
by hires and separations, but that this is fully accounted for by the higher reliance on
temporary work for firms above the cutoff. Consistent conclusions emerge from Boeri and
Jimeno (2005), who use a double difference based on the 15-workers cutoff in Italy and a
1990 reform that increased employment protection for small firms to find that the threshold
does not impact employment growth nor the size distribution of firms, but it does affect
dismissal and hiring probabilities. It also mildly increases firms’ persistence, that is, the
probability that a firm does not change the number of employees from one year to another.
Stronger evidence of the negative effect of employment protection on firms’ growth comes
from 1999 and 2014 to show that the stricter protections for firms of size 16 or more limit
their expansion. Further, he computes that removing such restriction could have increased
the workforce by about 4 to 6% in 2014. Montenovo (2022) finds that in 2011 there were
1.3% excess firms just beneath the cutoff. However, she shows that between 2011 and 2019,
the period of the two reforms examined here, the excess mass drops drastically, and in 2019
it cannot be distinguished from zero. Similarly, Garibaldi et al. (2004) use the Italian 15-
workers threshold to study firms’ reluctance to grow. The paper uses a 1990 reform that
increased employment protection legislation for those above the threshold and shows that
the persistence of small firms increased relative to that of large firms. Moreover, stronger employment protection seems to reduce employment and output through a lower entry of firms rather than by impacting the average per-plant reduction in employment (Micco and Pages, 2006). Kugler and Pica (2008) find that stricter employment protection in Italy for firms below the cutoff decreased both access and separation of workers in the affected firms. Further, however, they show that the reform impacted firms’ employment trends both at the external margin, by changing their entry and exit rates, as well as at the internal margin.

2.2.1 Literature on the Italian 2012 and 2015 Policies

There is some literature exploring the effects of either one of the two policies I consider here on various employment outcomes. Much of this existing work relies on descriptive analysis. Both reforms, the Fornero Reform in 2012 and the Jobs Act in 2015, decreased the uncertainty and the level of layoff and firing costs for firms of size 16 and higher. In addition, for firms of all sizes, the Jobs Act pushed for an increase in permanent contracts through social security payment discounts for employers hiring permanent workers. Cirillo et al. (2017) exploit this dual aspect of the Jobs Act and provide descriptive evidence of the key importance of social security discounts in explaining labor market dynamics compared to the role played by changes in the termination costs. Similarly, Guarascio et al. (2017) provide a qualitative analysis of the Jobs Act, again focusing on the evolution of job contracts of different types, this time by gender and Italian macro-region. Their chapter shows descriptive insights on the increase of permanent workers and highlights the importance of accounting for the wide regional variation. Berton et al. (2017) use survey data to analyze how the 2012 reform on employment protection impacted the quality of job matches. The authors find that job turnover increases as a result of the policy. Further, they show that the workers’ reallocation leads to improved job matches as measured by how workers’ education achievement compares to the median level in their department. Sestito and Eliana (2016) use administrative data from one Italian region, Veneto, to study how the Jobs Act, specifically its hiring subsidy
for permanent workers and the change in termination costs, contributed to the increase in 
permanent jobs. They show that about 40% of new permanent jobs can be explained by the 
hiring subsidies, while 5% is due to the changes in layoff and firing rules. The authors were 
able to distinguish between these two effects because the two policies apply to different types 
of workers and firms, allowing them to identify appropriate treatment and control groups. 
Similar to Sestito and Eliana (2016), Ardito et al. (2019) take advantage of the Jobs Act 
to look at how firms responded to hiring subsidies and changes in employment protection. 
They use data from another Italian region, Piedmont, and found that large firms are less 
sensitive than small ones to hiring subsidies unless that is coupled with the lower layoff and 
firing costs. While small firms substitute temporary with permanent reemployment as a 
result of hiring subsidies, larger firms still use fixed-term contracts, likely as a probationary 
period. Differently from Sestito and Eliana (2016), Ardito et al. (2019) use a non-linear 
difference-in-differences applied to a longer post-reform period, and show some heterogeneous 
effects by country of origin, human capital, and gender of the workers. Ardito et al. (2021) 
measure the impact of the drop in employment protection enacted by the Jobs Act on firms’ 
hires and performance. First, they find that firms increase job stabilization by converting 
contracts. Second, firms apply cost-saving strategies and decrease per-worker value-added, 
casting doubts on the ability of these reforms to improve long-term productivity.

Because Sestito and Eliana (2016) and Ardito et al. (2019) focus on two of the most 
economically developed regions in Italy, the external validity of their study may be limited. 
The authors mention that the effects of changes in layoff and firing costs strongly depend 
on the characteristics of the local court system. The vast geographical heterogeneity in 
the characteristics of labor courts across Italian regions has been documented in previous 
research (Ichino and Pinotti, 2012; Gianfreda and Vallanti, 2017), further strengthening these 
concerns. The high uncertainty around termination costs, which are greatly determined 
by the role of labor courts, is not peculiar to Italy but characterizes most systems with 
employment protection laws (Güell, 2010).
Some limitations of the literature reviewed so far have been overcome in Boeri and Garibaldi (2019) and Pigini and Staffolani (2021), which use administrative data to study the impact of the Jobs Act on job flows. Boeri and Garibaldi (2019) uses data on the work histories of the entire workforce for the universe of private firms with 10 to 20 employees between 2013 and 2016. They find a sharp increase in permanent hiring after 2015, an increase in the conversion of short-term contracts into long-term work relationships, a substantial increase in net job creation, a growth in the sum of job creation and destruction, as well as job destruction alone. Pigini and Staffolani (2021) use the same administrative data I employ here and focus on the Jobs Act over the period between 2014 and 2018. Their main outcome is the probability of being still employed about three and a half years later. They show that the job survival probability is not lower for larger firms. Instead, in some cases, it is higher. They investigate the composition of workers who are permanently hired after the reform and found that they are potentially more productive, more likely to be younger, and less known by the employer.

My work contributes to the existing literature in several ways. First, the Italian territory presents a marked geographical heterogeneity, especially between the Northern and Southern regions. As the judiciary system in Italy is also characterized by a wide variation, I investigate differences across macro-regions in Italy. Second, I use a larger time period in order to account for the role played by the Fornero Reform, which paved the way for the Jobs Act that followed three years later. Accounting for the Fornero Reform is paramount, as isolating the effects of the Jobs Act would be impossible given that a reform making similar changes was implemented three years prior. Because any impact of the Jobs Act may be contaminated by some delayed effects of the Fornero Reform, it is important to include the Fornero Reform in the analysis both for precisely identifying the effects and excluding the presence of differential pre-trends across treated and control firms prior to the Jobs Act, and to estimate the joint impacts of the reforms. Third, I explore heterogeneity by gender and age, which was not directly addressed in previous work. While both reforms introduced different changes aimed
at increasing flexibility in the labor market, I identify and focus only on the effects of the changes in employment protection, which applied to firms of size 16 or larger. Last, I expand the set of outcomes considered. In addition to job flows, I build new measures of workers’ reallocation and job match quality, which were not explicitly considered in this literature.

2.3 Background - The Italian Labor Market

The unemployment rate is an important indicator of labor market health. Italy has been characterized by extremely high unemployment rates, especially among the youth. Figure 1 displays the unemployment rate between 2000 and 2020 for Italy and the United States, and 2 shows the same information for individuals between 15 and 24 years old between 1995 and 2020. With the exception of the Great Recession years and of the COVID-19 pandemic, Italy has had a much higher - in some years more than three times as much - unemployment rate throughout the period. While employment protection legislation helps prevent sharp increases in unemployment rates during strong recessions because it discourages terminations, it can also function as a hiring disincentive during periods of economic growth. As a consequence, rigid labor markets are likely to display high long-term unemployment, and less steep increases in unemployment during harsh economic recessions compared to labor markets with weaker employment protection. Figure 1 is a representation of these trends. The differences in youth unemployment trends between the two countries are even starker: youth unemployment in Italy has been much higher than in the United States between 1995 and 2020, with the widest gap between 2013 and 2019. Although there is no unique and obvious cause for these differences, the contractual rigidity that characterizes the Italian labor market plays a prominent role. In fact, strict employment protection regulation generates an employment contract dualism in the labor market, which is a divide between highly protected and hard-to-get jobs and unstable temporary positions. Young workers tend to be disproportionately in the latter category.
Figure 1: Unemployment Trends in Italy and the US
In this paper, I quantify the labor market effects of two policies aimed at decreasing labor market rigidity and contractual dualism in Italy. In Montenovo (2022), I discuss some descriptive facts about dismissal and labor court cases in Italy which point to the large and highly uncertain termination costs firms face. Moreover, I report information documenting the presence of large geographical heterogeneity in such dimensions (Montenovo, 2022).

2.4 Policies

2.4.1 Employment Protection Before the Reforms

In 1970, the “Statute of Workers’ Rights” implemented employment protection rules and mandated that firms with 16 or more employees had a “just cause” for termination. In the absence of a “just cause”, the labor court would deem the termination unfair. A 1990 reform changed this disposition and introduced the rule for all firms regardless of size, allowing all
workers to sue their employer following a termination. However, the consequences of unfair termination were contingent on size. If, based on the evidence provided by the firm, the court decided that the termination was unfair, higher costs were imposed on firms with at least 16 employees. In practice, for firms above the size cutoff, the statute introduced termination costs that are not only higher, but also more uncertain because heavily dependant on the discretion of the judge. In Montenovo (2022), I review literature showing that, in reality, employers faced most of the negative consequences of such employment protections and the uncertainty around layoff legal trials that was associated with them.

2.4.2 The Reforms

On November 16th, 2011, amid a severe financial crisis, a technocratic government quickly and unexpectedly wrote a labor market reform (henceforth called the Fornero Reform\(^1\)) aimed at making the Italian economy more flexible and productive. The stated goal was to boost the dynamism of the Italian economy and to break down the labor market contractual dualism. To achieve these goals, the reform relaxed the employment protection rules implemented in 1970. A few years later, in 2015, a second labor market reform was enacted with similar goals: the Jobs Act further weakened the employment protection restrictions and reduced the uncertainty of termination costs for employers.

The reforms reduced the expected costs that firms with at least 16 workers incur for some types of illegitimate termination. Both reforms altered the rules on unfair terminations due to disciplinary (subjective) or economic (objective) reasons and the directives on judicial procedures. They do so through mainly two channels. First, the policy changes progressively almost eliminated the cases of unfair dismissal where the judge could order the reinstatement of the worker in the firm and substituted them with a pecuniary compensation proportional to the seniority of the worker. Moreover, the reforms introduced a system of friendly conciliation procedures that the worker and the firm had to attempt first before going to court. These

\(^1\)Dr. Elsa Fornero was the Minister of Labor who signed the reform.
conciliation venues further reduced the size and the uncertainty of termination costs by allowing the parties to avoid any legal fees and significantly speeding up the resolution.

A detailed description of the employment protection legislation in Italy and of the reforms considered here can be found in Montenovo (2022).

2.5 Theoretical Predictions

Because the reforms decreased the cases of unfair dismissals where labor courts can reinstate workers in the firm and award them with a compensation, and substituted these cases with an indemnity based on workers’ seniority, expected termination costs decreased.

The impact that the policies have on termination is straightforward. By lowering employment protection, termination should increase as firms are less reluctant to dismiss workers. Following the policies, then, bloated firms can cut back on their workforce and reach their desired number of employees without facing prohibitive costs. Therefore, the policies are expected to increase firing and layoffs. In this case, I use data about the employment relationships ending in a dismissal, and specifically about the reason for terminating the contract, to draw conclusions on the effects of the policies on termination.

However, weaker employment protections likely incentivize hiring as well. For every new hire, the firm faces some uncertainty around the employment match quality. The risk is that the match quality between the firm and the employer is poor and results in low productivity. While the selection process is helpful to alleviate this risk, only enough time spent working at the firm reveals the true quality level of a job match. Strict employment protection exacerbates this risk because it increases firms’ expected costs to break poor quality matches, perpetuating the losses arising from these unproductive relationships. The higher the termination costs, the riskier the commitment to a new hire, and the stronger the incentive to refrain from hiring. Moreover, strict employment protection hampers hiring regardless of the risk of match quality. In fact, the employer might need to downsize simply due to economic downturns. The policies, by decreasing termination costs, reduce firms’
commitment to new hires, implying that employers are less concerned about the potential need to terminate in case economic conditions worsen in the future or if the employment match turns out to be of low quality. Using the information on the dates corresponding to the start of a new employment relationship, I analyze the impact of the reforms on hiring.

The increase in hiring and dismissals should in turn increase job switching rates. If the policies induce both termination and hiring, more workers are going to leave some firms and join new ones shortly after. As a consequence, workers’ lower job security may be associated with shorter unemployment spells. Hence, overall, the policy may lead to growing turnover and job switching rates.

If a decrease in employment protection legislation triggers job switching, the allocation of workers may result in higher productivity matches. In fact, if employers are less hesitant to break low-productivity job matches, they are either left with the most productive workers or they replace low-productivity workers with better quality matches. Overall, in the medium-to long-term, the reforms may generate a new equilibrium characterized by higher productivity employment matches. In my dataset, I approximate job match quality with tenure, starting weekly earnings, increase in job ranks, and weekly earnings. More details on how I built these variables can be found in Section 3.5.

For some of these variables, it is theoretically hard to predict in which direction they will be impacted by the policies. While the expectation of increased productivity would predict a higher tenure, professional advancement, and earnings, there might be other mechanisms in place. For example, tenure decreases if the policies accelerate the rate at which workers switch jobs.

Similarly, the policies have an ambiguous effect on earnings. Higher-quality job matches are more productive, which puts upward pressure on wages. However, the reforms could result in another mechanism having an opposite effect on wages. As mentioned above, lowering termination costs decrease workers’ bargaining power in the litigation process following termination. If such lower bargaining power is extended to the hiring phase, it can translate
into lower wages. Similarly, one more alternative channel could put downward pressure on earnings. Weaker employment protection marginally decreases the value of finding a job, because a less secure job is not as valuable to workers. As a consequence, some unemployed workers might choose to drop out of the labor force because they find a preferred outside option, like housekeeping. Alternatively, for the unemployed individuals who still decide to remain in the labor force, weaker employment protection might lead to an increase in reservation wages. In fact, workers might be willing to accept a less secure job only if they are compensated with higher wages. Higher reservation wages extend the duration of unemployment spells while increasing the starting wage at a job. Ultimately, which of the effects prevails is an empirical question.

Demographic characteristics contribute to the riskiness employers face from the uncertainty of the job match quality. When hiring a worker, the employer incurs the risk that the quality of the match between the firm and the employer is poor and will result in low productivity. Although any hiring carries with it some level of risk, the consequences of a bad match vary with demographic characteristics.

In the presence of employment protection legislation, young individuals are characterized by high levels of hiring risk. In fact, in a strict employment protection regime, hiring low productivity young workers implies that the employer is stuck with them for a long time, that is, until retirement, unless they are willing to pay for high termination costs. In other words, the stricter the employment protections, the riskier the firm’s commitment to a newly hired worker, and the stronger the incentive to employ older workers to reduce such commitment. The reforms, by weakening employment protection, reduce firms’ commitment to newly employed workers and, with it, their risk if they end up being low-quality matches. Hence, I expect that the policies disproportionately boost the employment of younger workers, because they were originally the most penalized from this uncertainty.
A similar reasoning applies to the gender of workers. Women of childbearing age represent another demographic group whose uncertainty around the job match quality is higher for employers. In fact, employers may take into account the potential costs of maternity leave associated with these new hires. In addition, the potential costs to the employer extend well beyond maternity leave if women have to manage their time resources between their job and childcare duties. In these cases, if the employment match with a female employee turns out to be low, the losses to the employer might be higher compared to male workers of the same age. As a result, decreasing the expected risk involved in a new hire might disproportionately benefit women in childbearing years. As mentioned above, however, lower employment protection simultaneously decreases the value of having a job, which affects the labor supply. Then, if women are more likely to experience this trade-off between being in the labor force and housekeeping, they may be more likely to drop out of the labor force as a result of lower job security than their male counterparts. Even in this case, which effect dominates is an empirical question.

Finally, the policies explicitly aimed at benefiting the South by making its local economy more dynamic and flexible. In fact, the much slower and more burdensome judicial system in the Southern regions made termination for their firms even more expensive and uncertain, suggesting the perceived effects of employment protection regulations would be greater in those areas. As a consequence, firms in the South might disproportionately benefit from the policies.

2.6 Data

Starting in April 2013, the Italian Department of Work and Social Policies published several sources of administrative data for research purposes. The dataset I use in this paper, which is provided by the Social Security Administration office in Italy, contains information on the entire working history of a large random sample of Italian individuals. In particular, the data

2In Italy, maternity leave is fully financed by the government. However, firms face the difficulty of finding a replacement for women on leave.
captures individuals born on the first and ninth day of any month, which amounts to about 6.6% of the Italian population. For these employees, the dataset contains basic demographic information and, for those who entered the labor force, complete longitudinal data on their working history and social security contributions. The information is either collected by the social security office from employers via disclosure requirements or it is generated by the social security office for social security contribution purposes.

In this paper, I use information on the gender, age, and region of birth of workers. Moreover, I use yearly information about their working relationships, including the firm identification code, dates of beginning and end (if any) of the contract with corresponding reasons, the number of weeks worked, the income earned, and the job title corresponding to each employment relationship. In addition, for all employees, I have information about the firms they work at, including their size class and industry. In particular, firm size classes in my data are 1-5 employees, 6-10, 11-15, 16-20, 21-25, 26-30, and so on. Such information is paramount for my identification strategy that relies on differences across outcomes for firms affected by the reforms, that is, those of size 16 or above, compared to those unaffected, which are below the 16-threshold. Because I explore the implications of two reforms implemented in 2012 and 2015, I consider the years 2007 to 2018, and individuals in their prime working age, specifically those between 25 to 54 years old. Finally, I drop workers in short-term contracts and firms in the agricultural industry because these were not affected by employment protection regulations. For the earnings variables, I only keep the job relationships whose work time is measured in weeks and months, rather than days or “other”. These represent 99.3% of all observations.

The summary statistics of the outcomes measured in 2011, the pre-reforms year, are on Table 1.

---

3In 2010, I have a total of 1,072,366 employees in my sample. From official data, I found that in the same year, the total number of employees (excluding self-employed) was 16,833,000, and 6.6% of that amounts to 1,110,978, which is close to my sample size for that year.
Table 1: Summary Statistics in 2011

<table>
<thead>
<tr>
<th></th>
<th>Control Group</th>
<th>Treatment Group</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
</tr>
<tr>
<td>Hire</td>
<td>.2408597</td>
<td>.5305699</td>
</tr>
<tr>
<td>Observations</td>
<td>59927</td>
<td></td>
</tr>
<tr>
<td>Termination</td>
<td>.0832143</td>
<td>.3524603</td>
</tr>
<tr>
<td>Observations</td>
<td>35715</td>
<td></td>
</tr>
<tr>
<td>Job Switching</td>
<td>.0126657</td>
<td>.1118279</td>
</tr>
<tr>
<td>Observations</td>
<td>52741</td>
<td></td>
</tr>
<tr>
<td>Tenure</td>
<td>0.7452756</td>
<td>0.4357491</td>
</tr>
<tr>
<td>Observations</td>
<td>8659</td>
<td></td>
</tr>
<tr>
<td>IHS Starting Wk Earnings</td>
<td>6.526571</td>
<td>.669599</td>
</tr>
<tr>
<td>Observations</td>
<td>8345</td>
<td></td>
</tr>
<tr>
<td>Increase Rank</td>
<td>.0104212</td>
<td>.1015518</td>
</tr>
<tr>
<td>Observations</td>
<td>55368</td>
<td></td>
</tr>
<tr>
<td>IHS Wk Earnings</td>
<td>6.714903</td>
<td>.5193123</td>
</tr>
<tr>
<td>Observations</td>
<td>53270</td>
<td></td>
</tr>
</tbody>
</table>

Note: The table shows the means and the standard deviations of all the outcome variables in 2011, the pre-reforms variable. I compute them by treatment status. Control firms are those with 11 to 15 workers for all outcomes except Hire, for which I consider firms of size class 6 to 10 as control firms. Treated firms have between 16 and 20 employees.
2.7 Outcomes and Empirical Framework

The empirical analysis focuses on job flows and various measures of job match quality, and how these differentially change across firms above and below the 16-workers threshold as a result of the reforms. The job flows outcomes are termination, hiring, and job switches, and the job match quality outcomes are tenure, starting weekly earnings, changes in job rank, and weekly earnings.

Firms that in 2011 were in the 16 to 20 dimension class are treated. For all outcomes except hiring, control firms are those right beneath the threshold, that have between 11 to 15 employees. However, for hiring I consider an alternative control group, that of firms between 6 and 10 employees. In fact, firms between 11 and 15 workers are not a good control group for hiring, because their hiring rate is indirectly affected by the employment protection rules. In Montenovo (2022) I show that firms strategically remain below the threshold to avoid the higher termination costs that affect establishments above it. The evidence in Montenovo (2022) suggests that firms just beneath the threshold expand as the reforms are implemented, because the reforms, by lowering termination costs for firms above the threshold, reduce their incentives to remain undersized. Furthermore, the event study graph confirms the adequacy of the firms between 11 to 15 employees to be the control group for all outcomes except hiring. Meanwhile, firms of size 6 to 10 have parallel trends with treated firms when considering the hiring outcome.

Starting in 2005, for each new hire or dismissal, the data contain information on the reason why the employment relationship started or ended. Termination and hiring outcomes in the main regressions are computed at the firm level. For each firm I count, in any given year, the number of hires on long-term contracts as well as both disciplinary and economic dismissals. To build the hiring variable, I compute how many employment relationships have started in any year and firm. The hiring variable is the total number of employment relationships that started in each firm and year. For termination, I exploit the variable on the reason for separation, and I count the number of layoffs for subjective (disciplinary)
and objective (economic) reasons yearly at each firm. Termination is the total number of economic and disciplinary dismissals that occurred in each firm and year.

For the firm-level hiring and termination outcomes, I run Poisson models, which are appropriate for count data, with year and firm fixed effects. I also check for the robustness of these models by running linear specifications.

To account for the two policies and study their effects in the same model I build two time dummies indicating post-reform years. The first dummy turns on only for the years 2013 through 2018, and the second indicator variable is one starting in 2015 until 2018.

\[
\text{Outcome}_{ft} = \alpha_{st} + \beta \text{ForneroReform}_t \times \text{Large}_f +
\gamma \text{JobsAct}_t \times \text{Large}_f + \text{Firm}_f + \text{Year}_t + \epsilon_{ft}
\] (2.1)

\text{Outcome} is either hiring or termination. In the model above \(\beta\) is the estimated effect of the Fornero Reform on the outcome, \(\gamma\) is the estimated additional impact of the Jobs Act on top of the one triggered by the Fornero Reform, and \(\beta + \gamma\) is the estimated effect of both reforms jointly on the outcome.

2.7.1 Job to Job Flows

To build the job switching variable I use data on the social security contributions. I keep all observations corresponding to full- and part-time work, identifying an ongoing working relationship for the corresponding year. There are workers with multiple jobs in any year, either because they have multiple occupations or because they are in the process of switching jobs. I built the job switching variable by first counting the number of jobs each worker has at different firms in any given year. Second, I chronologically rank them by the starting date of each job. Then, I count the number of days between the end of the first relationship and the beginning of the second, if any. When there is a third working relationship at a different firm in that same year, I look at the number of days between the end of the second and the
beginning of the third, and so on. I define a job switch a situation where the absolute value of the number of days in between different job relationships for the same worker is equal to or less than 90. Since I take the absolute value, a job switch occurs in any situation of some overlap between two jobs, as long as the overlap is not greater than 90 days. I apply the same criterion to job switches that happen over two calendar years. In short, the dummy for job switching is equal to one if the worker starts working at one firm in any 90 days window and ends up at a different firm at most 90 days after. All other employment relationships are coded as zero for this outcome.

I run the model for job switching including worker, firm, and year-fixed effects. The regression is as follows:

\[
JobSwitch_{i,t} = \alpha_{i,t} + \alpha_1 ForneroReform_t \times Large_f + \alpha_2 JobsAct_t \times Large_f + Worker_i + Firm_f + Year_t + \epsilon_{i,t}
\] (2.2)

The outcomes on job match quality, which I describe below, are only considered at the job or worker level.

### 2.7.2 Job Match Quality

To measure job match quality I consider four variables: tenure, starting weekly earnings, increases in job rank, and weekly earnings.

These measures were inspired by some of the literature on job match quality, especially those relying on administrative data. Wages, especially starting wages, and job duration, as measured by separation rates, are the most common measures of job match quality in past work (Card et al., 2007; van Ours and Vodopivec, 2008; Gaure et al., 2012; Caliendo et al., 2013; Cappellari and Tatsiramos, 2015; Rebollo-Sanz and Rodríguez-Planas, 2020). For example, a job switch occurs if a worker leaves a firm on December 31st, 2009, and starts a new job on February 7th. Similarly, a job switch occurs if the beginning of a second job was on December 31st with the first job ending on February 7th.
example, Card et al. (2007) use wage at the next job (for workers following unemployment) and their durations, as, they claim, “better matches should last longer”. Moreover, they consider wage growth and increase in rank from blue collar to white collar. Similarly, van Ours and Vodopivec (2008) use wage increase, with the distinction that they compute this as the difference between the post-unemployment wage with the pre-unemployment wage. As further measures of job match quality, they consider workers finding permanent, versus temporary, jobs, and the probability of job loss within a year.

Tenure measures job match quality because the higher the productivity of a job match, the larger the incentives for the employer and the employee to continue the job relationship over time. The starting weekly earnings measure job match quality because the larger the expected productivity of an employment relationship, the higher the beginning salary the employer is willing to pay to incentivize a worker to accept the offer. For these outcomes, I collapse the dataset to new employment relationships, such that each year indicates the year when the relationship began. I drop every subsequent year of the employment relationship because I focus on labor dynamics that occur at the beginning of a contract.

A high job match quality results in a faster advancement in the career ladder, as higher-productivity employment relationships lead to faster professional development and progress within the firm. Hence, I consider two more measures of job match quality. First, I use increases in the level of job title within the same firm over time. Second, I measure earnings growth within each employee-employer match over time.

**Tenure**

The dummy variable tenure exploits the information from the dataset on the entire working histories of each worker. At the beginning of each employment relationship, I count the number of years the new hire will remain in that firm. For each new employment relationship, the indicator variable for tenure turns on if the worker remains at the new job for at least two years.
Starting Weekly Earnings

I compute weekly income by dividing the yearly gross income corresponding to each new employment relationship by number of weeks worked as part of that relationship in each year. Starting income is the weekly earnings received in the first year of each employment relationship. By considering this outcome, I explore whether the expected productivity of the employment matches, measured by the income new hires receive in their first year at a firm, increases after the policies. In the regression models, I apply an inverse hyperbolic sine (IHS) transformation to the earnings variable; a regression of $IHS(\text{Earnings}_{f,ti})$ on the covariates is comparable to a conventional log-linear regression specification, but the IHS transformation is defined also for workers with zero earnings.

Job Title

The data include a variable on the job title for the employment relationship within a firm each year. In ascending order, the professional ladder is as follows: “Apprentice,” “Blue Collar,” “White collar,” “Middle Manager,” and “Manager.” I build a dummy variable for job title increase that turns on each time a worker improves her job rank within the same firm compared to the previous year. For example, if worker $i$ at firm $f$ was a blue-collar in year $t$ and then becomes a white-collar in year $t+1$, the dummy is equal to 1 in year $t+1$. Similarly, the dummy turns on if the increase in job title is greater than just one step, for example switching from blue-collar worker in year $t$ to manager in year $t+1$.

Earnings Growth

For the analysis of earnings growth, I consider weekly earnings. By adding firm and worker fixed effects, I expect to capture information on a worker’s earnings progress within the firm.

For all job match quality outcomes, the main model I run is:
\[ \text{Outcome}_{ift} = \alpha_{st} + \alpha_1 \text{ForneroReform}_t \times \text{Large}_f + \alpha_2 \text{JobsAct}_t \times \text{Large}_f + \alpha_3 \text{ForneroReform}_t \times \text{Female}_i + \alpha_4 \text{JobsAct}_t \times \text{Female}_i + \alpha_5 \text{Large}_f \times \text{Female}_i + \text{Worker}_i + \text{Firm}_f + \text{Year}_t + \epsilon_{ft} \] (2.3)

Finally, I consider heterogeneous treatment effects by gender, age, and geographical variables.

For the heterogeneous treatment effects analysis by age and gender, I keep the regressions at the individual level. When examining hiring and termination, rather than summing up these outcomes at the firm level, I consider them as dummy variables at the worker level. For example, hiring is equal to 1 if the working relationship between the employee and the firm started in that year, and 0 otherwise. Termination is equal to 1 if the working relationship resulted in a termination for objective and subjective reasons in that year, and it equals 0 if the relationship continued or ended for other reasons. I run heterogeneous treatment effects analyses using a triple interaction term for the category of interest. For instance, for heterogeneous treatment effects by gender, the model is the following:

\[ \text{Outcome}_{ift} = \alpha_{st} + \alpha_1 \text{ForneroReform}_t \times \text{Large}_f + \alpha_2 \text{JobsAct}_t \times \text{Large}_f + \alpha_3 \text{ForneroReform}_t \times \text{Female}_i + \alpha_4 \text{JobsAct}_t \times \text{Female}_i + \alpha_5 \text{Large}_f \times \text{Female}_i + \text{Worker}_i + \text{Firm}_f + \text{Year}_t + \epsilon_{ft} \] (2.4)

In the model above, \( \alpha_4 \) is the differential effect of the Fornero Reform on the outcome for female workers compared to male workers, \( \alpha_4 + \alpha_5 \) is the differential effect of both the Fornero Reform and the Jobs Act experienced by female workers compared to their male counterparts.

In addition to gender, I explore the heterogeneous treatment effects of the reforms for
workers between 25 and 34 years old and by the location of the firms. In each of these models, I include worker, firm, and year fixed effects.

### 2.7.3 Firm Location

I use information on the region of birth of the employees in each firm to draw conclusions about the firm’s location. There are 20 regions in Italy, and in my analysis, I want to identify whether the firm is located in the North (8 regions) versus the Center or South (12 regions). For each firm, I compute the mode of the region of birth of its workers overall and in each year.\(^5\) The location mode over the years matches the yearly one in 95% of the years. Hence, the inference of the location of a firm from its workers’ modal region of birth is precise, even more so after I aggregate the inferred location to two macro-areas (North and Center-South). In fact, aggregating to two macro-areas would solve the problem of a few wrong inferences across regions as long as they are part of the same macro-region.

The regression analyses in this paper rely on two key identifying assumptions. First, the difference-in-differences method assumes that control firms would have followed trends in each outcome that are *parallel* to those of treated firms in the absence of the reforms. In other words, there are no time-varying differences in firm outcomes across the threshold that are not originated by the two reforms. Second, the analysis requires a strict exogeneity assumption, requiring that unobserved factors impacting each outcome are uncorrelated with the history of firms’ treatment status. In other words, the difference-in-differences model is identified as long as there are no anticipation effects of the reforms, no differential pre-trends across treated and control firms, and no time-varying treatment effects beyond those captured by the variables in the regression.

Fitting event study regressions helps assess the plausibility of both assumptions. I show the event studies for each outcome in Appendix 3.8. The event studies in Section 3.8 show

---

5I drop firms for which I have only information about one worker in one year because in that case, the mode of the workforce’s region of birth would not be reliable to draw conclusions about firms’ location.
the lack of differential pre-trends between these two groups in the years preceding the first reform across all main outcomes considered: hiring (Graph A1), termination (Graph A2), job switches (Graph A3), tenure (Graph A4), starting weekly earnings (Graph A5), increase in rank (Graph A6), and weekly earnings (Graph A7). Overall, the event studies graphs show the lack of differential pre-trends and anticipation effects in the outcomes between the control and treatment firms in the pre-reforms period. As expected, the treatment effects of the reforms on all outcomes center around zero for the years before 2012, when the Fornero Reform was enacted, supporting the validity of the identifying assumptions required in difference-in-differences models.
2.8 Results

There are two sets of results. First, I show regression estimates for job flows: termination, hiring, and job switching. By focusing on job flows, I examine whether the policies increased the movement of workers out of employment relationships, the transition in new employment relationships, and in and out of jobs. The second set of results shows the impact of the reform on job match quality. The reforms might affect job match quality because, by making termination cheaper, employers may find it easier to hire and terminate workers in ways that lead to more productive job matches.

Finally, I investigate the heterogeneous treatment effects of such outcomes. I introduce dummies for young workers, specifically 25 to 34 years old, female workers, and geographical macro-regions to identify differential effects by Northern and Central/Southern regions in Italy.

Table 2 shows the impact of the reforms on hiring in Column (1), termination in Column (2), and job switch in Columns (3) and (4). The estimates for columns (1) and (2) refer to firm-level outcomes and are computed from running a firm-level Poisson regression with year and firm fixed effects included. However, because job switch is an outcome at the worker level, Column (3) shows estimates from a worker-level linear regression with year and firm fixed effects, while Column (4) further includes worker fixed effects.

Because the coefficients on Columns (1) and (2) come from a Poisson estimation, they indicate the log scale percentage change in the outcome per one unit change in the independent variable. Hence, to derive the percentage change corresponding to each coefficient, I computed \( e^\beta \) where \( \beta \) is the estimated coefficient, and the result is the number of times the outcome has changed in the treated group compared to the control group due to the reforms. For example, considering the estimated coefficient for hiring corresponding to the Fornero Reform: \( e^{0.147} = 1.16 \) implies that the Fornero Reform increased hiring among treated firms by 16% on average. If I am interested in the joint effects of the reforms, then I have to elevate \( e \) to the sum of the coefficients, i.e. \( 0.147 + 0.225 = 0.372 \), implying a percentage increase
in hiring caused by both reforms of about 45%, computed from $e^{0.372} = 1.451$. Hence, the Fornero Reform has increased hiring by 16% and the Jobs Act has accelerated that effect, implying that the policies had the joint effect of a 45% increase in hiring rates among treated firms.

Using this procedure for termination, I conclude that the Fornero Reform increased termination rates by 26%. By contrast, the effect of the Jobs Act on termination is positive but not statistically significant, suggesting that it did not introduce any significant change to that outcome compared to the trends the Fornero Reform triggered. Estimating the joint effects of the policies, however, is still possible regardless of the size of the standard errors. When doing that, I conclude that Fornero Reform and the Jobs Act caused an increase in termination rates among the treated firms of 36%.

Table 3 shows the estimates for hiring and termination when considering a linear regression model. Overall, the results confirm the conclusion I drew above based on the Poisson models.

Columns (3) and (4) of Table 2 show the results for job switching. When only considering firm and year fixed effects, results suggest that the Fornero Reform increased job switching by 23%, which I computed by dividing the estimated coefficient by the mean of the outcome in the treated group in 2011. The Jobs Act, instead, did not significantly affect that outcome. Moreover, the Fornero Reform effect on job switching disappears once I add the worker fixed effects, as shown in Column (4).

Table 4 shows the results on job quality. Column (1) shows the coefficients for tenure, Column (2) those for the weekly earnings at the beginning of a job, Column (3) reports the estimates for increase in rank, and Column (4) displays the coefficients for weekly earnings. Because the coefficients on the interaction terms on Table 4 are statistically insignificant for all outcomes considered, I conclude that neither reform has significantly changed any of the measures I use to capture the quality of job matches. One potential explanation for such evidence is that improvements in the quality job matches are not captured in my dataset.
because they emerge in a longer time frame than the one analyzed here.

Table 2: The Impact of the Reforms on Job Flows

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hiring</td>
<td>Termination</td>
<td>Job Switch</td>
<td>Job Switch</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20</td>
<td>0.147***</td>
<td>0.234***</td>
<td>0.003**</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.090)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20</td>
<td>0.225***</td>
<td>0.079</td>
<td>-0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.084)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.542***</td>
<td>-0.763***</td>
<td>0.013***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Observations</td>
<td>399867</td>
<td>102049</td>
<td>940057</td>
<td>834877</td>
</tr>
</tbody>
</table>

Note: In Column (1), I plot the estimates corresponding to the firm-level Poisson regression for Hiring. Control firms are of size 6 to 10, and treated firms are of size 16 to 20. Year and firm fixed effects are included. In Column (2), I plot the estimates corresponding to the firm-level Poisson regression for Termination. Control firms are of size 11 to 15, and treated firms are of size 16 to 20. Year and firm fixed effects are included. In Columns (3) and (4), I plot the estimates corresponding to the individual-level linear regression for Job Switches. Control firms are of size 11 to 15, and treated firms are of size 16 to 20. I create a dummy variable for job switching. I consider a job switch to occur (hence the variable is equal to 1) if a worker already employed at firm A starts a new job at firm B and ends her employment at firm A within 90 days. In all other cases, job switching is equal to zero. In Column (3) I include firm and year fixed effects. In Column (4) I included firm, year, and worker fixed effects. In all regressions, errors are clustered at the firm level.

Table 3: The Impact of the Reforms on Hiring and Termination - Linear Regression

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hiring</td>
<td>Termination</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20</td>
<td>0.013***</td>
<td>0.007**</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20</td>
<td>0.036***</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.208***</td>
<td>0.059***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Observations</td>
<td>773693</td>
<td>555253</td>
</tr>
</tbody>
</table>

Note: In Column (1), I plot the estimates corresponding to the firm-level linear regression for Hiring. Control firms are of size 6 to 10, and treated firms are of size 16 to 20. Year and firm fixed effects are included. In Column (2), I plot the estimates corresponding to the firm-level linear regression for Termination. Control firms are of size 11 to 15, and treated firms are of size 16 to 20. Year and firm fixed effects are included. In all regressions, Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Errors clustered at the firm level.
Table 4: The Impact of the Reforms on Job Quality

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Tenure</td>
<td>Starting Earnings</td>
<td>Increase Rank</td>
<td>Earnings</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20</td>
<td>-0.024</td>
<td>0.091</td>
<td>0.002</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.086)</td>
<td>(0.140)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20</td>
<td>0.112</td>
<td>-0.308</td>
<td>0.001</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.124)</td>
<td>(0.205)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.658***</td>
<td>6.547***</td>
<td>0.011***</td>
<td>6.750***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.012)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Observations</td>
<td>14133</td>
<td>13966</td>
<td>880939</td>
<td>862026</td>
</tr>
</tbody>
</table>

Note: In Column (1), I plot the estimates corresponding to the individual-level linear regression for Tenure. Tenure is equal to 1 if the worker starts a new job and will stay for at least two years. In Column (2), I plot the estimates corresponding to the individual-level linear regression for Starting Earnings. Starting earnings are the weekly earnings on the first year of an employment relationship. Column (3), I plot the estimates corresponding to the individual-level linear regression for Increase Rank. Increase rank is a dummy variable equal to 1 for each time a worker increases in professional rank within her employment relationship. Starting earnings are the earnings on the first year of an employment relationship. Column (4), I plot the estimates corresponding to the individual-level linear regression for Earnings. Earnings are the weekly earnings received by each worker. In all regressions, control firms are of size 11 to 15, and treated firms are of size 16 to 20. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Year, firm, and individual fixed effects are included. Errors clustered at the firm level.
Next, I discuss the results of the heterogeneous treatment effects of the reforms by gender, age, and geography.

Table 5 shows the heterogeneity of the job flows variables by gender. While there seems to be no differential effect in termination or job switch between genders, the impact of the Jobs Act on hiring appears to be lower for female workers than for male workers. Specifically, women experience about 10% less hiring than male workers in the aftermath of the Jobs Act. As a consequence, because of the theoretical considerations in Section 2.5, the results suggest that women appear to be less risky employees than men.

Table 7 reports the results on job flows by the age of the worker. The coefficients on the triple interactions represent the differential effect that younger workers experience compared to their older counterparts. Since no heterogeneity emerges from the table, I conclude that younger workers experienced similar changes in job flows trends to their more senior counterparts following the reforms.

Table 9 shows the differential effects by geography using a Poisson model. The estimates show that the Fornero Reform increased hiring in the North by 13% more than it did in the Center-South. Moreover, the Jobs Act amplified this difference, and further increased hiring among the Northern firms by 28% more than those in the Center-South. Hence, while the reforms have increased hiring on average, the stronger economies in the North of Italy have experienced 45% more hiring than their counterparts in the Center-South as a consequence of the reforms. These results on hiring suggest that the reforms were not successful at decreasing the differences in performance between weaker and stronger local labor markets in Italy, at least not in the short-term. In fact, much of the gains in hiring are experienced by workers in the North, where firms are already economically stronger and more developed.

Table 6 shows results on the heterogeneous treatment effects on the quality of job matches by gender. While for most job quality outcomes the reforms do not appear to have affected the two genders differently, Column (2) displays that the Jobs Act lead to higher start weekly earnings for women than for men. In particular, when dividing the 0.253 coefficient by the
2011 average of starting earnings among female workers in treated firms, the coefficient suggests that the Jobs Act increased starting earnings among female workers by 3.4% more than for male workers. Regressions on job match quality outcomes for younger workers are shown in Table 8, which reports no differences by age in the effects of either reform. Finally, Table 10 shows that workers in Northern firms did not experience any differential effects in the quality of their employment relationships compared to those in the Center-North as a result of the reforms.

Table 5: The Heterogeneous Treatment Effects of the Reforms on Job Flows for Women

<table>
<thead>
<tr>
<th></th>
<th>(1) Hiring</th>
<th>(2) Termination</th>
<th>(3) Job Switch</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fornero Reform x Firm 16-20</td>
<td>0.017***</td>
<td>0.002</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Jobs Act x Firm 16-20</td>
<td>0.045***</td>
<td>-0.004</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Fornero Reform x Female</td>
<td>-0.006**</td>
<td>-0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Jobs Act x Female</td>
<td>0.012***</td>
<td>-0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Firm 16-20 x Female</td>
<td>0.006</td>
<td>-0.007</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.008)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Fornero Reform x Firm 16-20 x Female</td>
<td>0.002</td>
<td>-0.004</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.004)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Jobs Act x Firm 16-20 x Female</td>
<td>-0.016**</td>
<td>0.007</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.005)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.142***</td>
<td>0.037***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
</tbody>
</table>

Observations: 1028319 815969 834877

Note: All regressions are linear and at the individual level. I examine heterogeneous treatment effect for female workers using a triple interaction term. Female is equal to 1 for female workers. In Column (1) I plot the estimates for hiring, which is a dummy equal to 1 if the worker started a new employment relationship that year. In Column (2) I plot the estimates for termination, which is a dummy equal to 1 if the worker ends an employment relationship that year, for subjective or objective reasons. In Column (3) I plot the estimates for job switching, which is a dummy equal to 1 if the worker changes jobs within a 90-days window. In all regressions, treated firms are of size 16 to 20. Control firms are of size 6 to 10 for Hiring, and 11 to 15 for Termination and Job Switch. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Year, firm, and individual fixed effects are included. Errors clustered at the firm level.
Table 6: The Heterogeneous Treatment Effects of the Reforms on Job Quality for Women

<table>
<thead>
<tr>
<th></th>
<th>(1) Tenure</th>
<th>(2) Start Weekly Earnings</th>
<th>(3) Increase Rank</th>
<th>(4) Weekly Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fornero Reform x Size 16-20</td>
<td>-0.041</td>
<td>0.125</td>
<td>0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.092)</td>
<td>(0.149)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20</td>
<td>0.117</td>
<td>-0.383*</td>
<td>0.001</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.131)</td>
<td>(0.218)</td>
<td>(0.002)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Fornero Reform x Female</td>
<td>-0.007</td>
<td>0.113*</td>
<td>-0.001</td>
<td>0.017***</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.060)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Jobs Act x Female</td>
<td>0.018</td>
<td>-0.113</td>
<td>0.001</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.087)</td>
<td>(0.001)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Size 16-20 x Female</td>
<td>0.059</td>
<td>0.000</td>
<td>-0.002</td>
<td>0.020</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.043)</td>
<td>(0.003)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20 x Female</td>
<td>0.038</td>
<td>-0.131</td>
<td>0.002</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.078)</td>
<td>(0.108)</td>
<td>(0.002)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20 x Female</td>
<td>-0.026</td>
<td>0.253*</td>
<td>-0.002</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.097)</td>
<td>(0.138)</td>
<td>(0.003)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.653***</td>
<td>6.540***</td>
<td>0.011***</td>
<td>6.746***</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.014)</td>
<td>(0.000)</td>
<td>(0.001)</td>
</tr>
</tbody>
</table>

Observations: 14133 13966 880939 862026

Note: All regressions are linear and at the individual level. I examine heterogeneous treatment effect for female workers using a triple interaction term. Female is equal to 1 for female workers. In Column (1) I plot the estimates for Tenure. Tenure is equal to 1 if the worker starts a new job and will stay for at least two years. In Column (2), I plot the estimates corresponding to the individual-level linear regression for Starting Earnings. Starting earnings are the weekly earnings on the first year of an employment relationship. Column (3), I plot the estimates corresponding to the individual-level linear regression for Increase Rank. Increase rank is a dummy variable equal to 1 for each time a worker increases in professional rank within her employment relationship. Starting earnings are the earnings on the first year of an employment relationship. Column (4), I plot the estimates corresponding to the individual-level linear regression for Earnings. Earnings are the weekly earnings received by each worker. In all regressions, control firms are of size 11 to 15, and treated firms are of size 16 to 20. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Year, firm, and individual fixed effects are included. Errors clustered at the firm level.
Table 7: The Heterogeneous Treatment Effects of the Reforms on Job Flows for Young Workers

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Hiring</td>
<td>Termination</td>
<td>Job Switch</td>
</tr>
<tr>
<td>Age 25-29</td>
<td>0.111***</td>
<td>-0.012***</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Fornero Reform x Firm 16-20</td>
<td>0.016***</td>
<td>0.001</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Jobs Act x Firm 16-20</td>
<td>0.037***</td>
<td>-0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Fornero Reform x Age 25-29</td>
<td>-0.037***</td>
<td>0.004</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Jobs Act x Age 25-29</td>
<td>-0.138***</td>
<td>0.016***</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.002)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>1Firm 16-20 x Age 25-29</td>
<td>-0.013*</td>
<td>0.008*</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Fornero Reform x Firm 16-20 x Age 25-29</td>
<td>0.002</td>
<td>-0.001</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.007)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Jobs Act x Firm 16-20 x Age 25-29</td>
<td>-0.009</td>
<td>-0.002</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.008)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.135***</td>
<td>0.037***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Observations</td>
<td>1028319</td>
<td>815969</td>
<td>834877</td>
</tr>
</tbody>
</table>

Note: All regressions are linear and at the individual level. I examine heterogeneous treatment effect for young workers using a triple interaction term. I consider young workers to be between 25 to 29. In Column (1) I plot the estimates for hiring, which is a dummy equal to 1 if the worker started a new employment relationship that year. In Column (2) I plot the estimates for termination, which is a dummy equal to 1 if the worker ends an employment relationship that year, for subjective or objective reasons. In Column (3) I plot the estimates for job switching, which is a dummy equal to 1 if the worker changes jobs within a 90-days window. In all regressions, treated firms are of size 16 to 20. Control firms are of size 6 to 10 for Hiring, and 11 to 15 for Termination and Job Switch. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Year, firm, and individual fixed effects are included. Errors clustered at the firm level.
Table 8: The Heterogeneous Treatment Effects of the Reforms on Job Quality for Young Workers

<table>
<thead>
<tr>
<th></th>
<th>(1) Tenure</th>
<th>(2) Start Weekly Earnings</th>
<th>(3) Increase Rank</th>
<th>(4) Weekly Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Young</td>
<td>-0.019</td>
<td>-0.014</td>
<td>0.010***</td>
<td>-0.026***</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.030)</td>
<td>(0.001)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20</td>
<td>-0.014</td>
<td>0.106</td>
<td>0.001</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.086)</td>
<td>(0.142)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20</td>
<td>0.112</td>
<td>-0.317</td>
<td>0.001</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.124)</td>
<td>(0.204)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Fornero Reform x Age 25-29</td>
<td>0.152**</td>
<td>0.066</td>
<td>-0.011***</td>
<td>0.009**</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
<td>(0.074)</td>
<td>(0.002)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Jobs Act x Age 25-29</td>
<td>-0.089</td>
<td>-0.185</td>
<td>-0.001</td>
<td>0.013**</td>
</tr>
<tr>
<td></td>
<td>(0.087)</td>
<td>(0.192)</td>
<td>(0.003)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Size 16-20 x Age 25-29</td>
<td>0.055</td>
<td>0.009</td>
<td>-0.003</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.034)</td>
<td>(0.003)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Fornero Reform x Size 16-20 x Age 25-29</td>
<td>-0.063</td>
<td>-0.112</td>
<td>0.005</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.083)</td>
<td>(0.085)</td>
<td>(0.006)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Jobs Act x Size 16-20 x Age 25-29</td>
<td>0.134</td>
<td>0.062</td>
<td>-0.010</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.130)</td>
<td>(0.219)</td>
<td>(0.008)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.652***</td>
<td>6.548***</td>
<td>0.010***</td>
<td>6.752***</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.013)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
</tbody>
</table>

Observations: 14133 13966 880939 862026

Note: All regressions are linear and at the individual level. I examine heterogeneous treatment effect for young workers using a triple interaction term. I consider young workers to be between 25 to 29. In Column (1) I plot the estimates for Tenure. Tenure is equal to 1 if the worker starts a new job and will stay for at least two years. In Column (2), I plot the estimates corresponding to the individual-level linear regression for Starting Earnings. Starting earnings are the weekly earnings on the first year of an employment relationship. Column (3), I plot the estimates corresponding to the individual-level linear regression for Increase Rank. Increase rank is a dummy variable equal to 1 for each time a worker increases in professional rank within her employment relationship. Starting earnings are the earnings on the first year of an employment relationship. Column (4), I plot the estimates corresponding to the individual-level linear regression for Earnings. Earnings are the weekly earnings received by each worker. In all regressions, control firms are of size 11 to 15, and treated firms are of size 16 to 20. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. Year, firm, and individual fixed effects are included. Errors clustered at the firm level.
Table 9: The Heterogeneous Treatment Effects of the Reforms on Job Flows for the Northern Macro-Region

<table>
<thead>
<tr>
<th></th>
<th>(1) Hiring</th>
<th>(2) Termination</th>
<th>(3) Job Switch</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fornero Reform x 16-20 Class Size</td>
<td>0.096**</td>
<td>0.249**</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.118)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Fornero Reform x North</td>
<td>-0.182***</td>
<td>0.421***</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.051)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Fornero Reform x 16-20 Class Size x North</td>
<td>0.124**</td>
<td>-0.063</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.180)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Jobs Act x 16-20 Class Size</td>
<td>0.088*</td>
<td>0.077</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.109)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Jobs Act x North</td>
<td>-0.034</td>
<td>0.011</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.052)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Jobs Act x 16-20 Class Size x North</td>
<td>0.249***</td>
<td>0.005</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.168)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.502***</td>
<td>-0.848***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.012)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Observations</td>
<td>406088</td>
<td>102271</td>
<td>828750</td>
</tr>
</tbody>
</table>

Note: I examine heterogeneous treatment effect for firms located in the North using a triple interaction term. North is equal to 1 if the firm is located in the North. In Column (1), I plot the estimates corresponding to the firm-level Poisson regression for Hiring. Control firms are of size 6 to 10, and treated firms are of size 16 to 20. In Column (2), I plot the estimates corresponding to the firm-level Poisson regression for Termination. Control firms are of size 11 to 15, and treated firms are of size 16 to 20. In Columns (3) I plot the estimates corresponding to the individual-level linear regression for Job Switches. Control firms are of size 11 to 15, and treated firms are of size 16 to 20. I create a dummy variable for job switching. I consider a job switch to occur (hence the variable is equal to 1) if a worker already employed at firm A starts a new job at firm B and ends her employment at firm A within 90 days. In all other cases, job switching is equal to zero. In Columns (1) and (2) I include firm and year fixed effects. In Column (3) I include firm, year, and individual fixed effects. Errors are clustered at the firm level.
Table 10: The Heterogeneous Treatment Effects of the Reforms on Job Quality for the Northern Macro-Region

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Tenure Start</td>
<td>Weekly Earnings</td>
<td>Rank</td>
<td>Weekly Earnings</td>
</tr>
<tr>
<td>Fornero Reform x 16-20 Class Size</td>
<td>-0.197</td>
<td>0.045</td>
<td>0.004*</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.125)</td>
<td>(0.127)</td>
<td>(0.002)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Fornero Reform x North</td>
<td>0.136*</td>
<td>-0.133*</td>
<td>0.001</td>
<td>0.005**</td>
</tr>
<tr>
<td></td>
<td>(0.081)</td>
<td>(0.069)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Fornero Reform x 16-20 Class Size x North</td>
<td>0.248</td>
<td>0.025</td>
<td>-0.004</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.167)</td>
<td>(0.175)</td>
<td>(0.002)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Jobs Act x 16-20 Class Size</td>
<td>0.236</td>
<td>-0.105</td>
<td>-0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.198)</td>
<td>(0.170)</td>
<td>(0.002)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Jobs Act x North</td>
<td>-0.039</td>
<td>-0.153</td>
<td>0.000</td>
<td>-0.006**</td>
</tr>
<tr>
<td></td>
<td>(0.097)</td>
<td>(0.116)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Jobs Act x 16-20 Class Size x North</td>
<td>-0.147</td>
<td>-0.055</td>
<td>0.002</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.250)</td>
<td>(0.237)</td>
<td>(0.003)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.640***</td>
<td>6.564***</td>
<td>0.010***</td>
<td>6.739***</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.018)</td>
<td>(0.000)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Observations</td>
<td>13541</td>
<td>19740</td>
<td>879197</td>
<td>1027838</td>
</tr>
</tbody>
</table>

Note: I examine heterogeneous treatment effect for firms located in the North using a triple interaction term. North is equal to 1 if the firm is located in the North. In Column (1) I plot the estimates corresponding to the individual-level linear regression for Tenure. Tenure is equal to 1 if the worker starts a new job and will stay for at least two years. In Column (2), I plot the estimates corresponding to the individual-level linear regression for Starting Earnings. Starting earnings are the weekly earnings on the first year of an employment relationship. Column (3), I plot the estimates corresponding to the individual-level linear regression for Increase Rank. Increase rank is a dummy variable equal to 1 for each time a worker increases in professional rank within her employment relationship. Starting earnings are the earnings on the first year of an employment relationship. Column (4), I plot the estimates corresponding to the individual-level linear regression for Earnings. Earnings are the weekly earnings received by each worker. In all regressions, control firms are of size 11 to 15, and treated firms are of size 16 to 20. Fornero Reform is a dummy equal to 1 in years 2013 through 2018, and the Jobs Act is a dummy equal to 1 in years 2016 through 2018. I include year, firm, and individual fixed effects. Errors clustered at the firm level.
2.9 Conclusion

In this paper, I consider the impact on job flows and job match quality of two reforms that in 2012 and 2015 reduced the strictness of employment protections in Italy among firms above 15 workers. Both reforms aimed at increasing labor market flexibility by reducing the costs of unfair economic and disciplinary dismissals for firms above this size threshold. I use a difference-in-differences framework and administrative data on a random sample of Italian workers to quantify the ability of the two reforms to increase job flows and job match quality in a historically rigid labor market. Further, I explore heterogeneous effects by gender, age, and geography, because there are theoretical reasons to believe that the reforms have differential impacts for subgroups across these variables. The results show that both policies led to an increase in termination, hiring, and job switching, indicating an increase in workers’ reallocation across firms. The Fornero Reform increased termination by 26%, and while the effect of the Jobs Act is not statistically significant, considering its coefficient implies that both reforms jointly increased termination by 36%. The effects on hiring are significantly more striking. The Fornero Reform increased hiring by 16%, and the Jobs Act accelerate that impact by over 25%, leading to a joint effect of the reforms on hiring corresponding to a 45% increase, which is statistically significant. While the Fornero Reform boosts job switching by 23%, the Jobs Act does not have a extra differential effect on that outcome.

This is suggestive evidence that, while a reduction in employment protection in a historically rigid labor market does lead to higher termination rates, it also improves the ability of workers to switch jobs and, importantly, it dramatically increases hiring rates among the affected firms. Moreover, while the increase in termination occurs only in the immediate period following the policies, the positive effects of hiring are longer lasting. The findings imply that the reforms, in the short term, incentivized bloated firms to dismiss workers. Moreover, the policy changes reduce the risks of employing new workers enough to trigger significant hiring in the short and medium term.

By contrast, the regressions considering different measures of job match quality point at
no significant changes in any of these outcomes as a result of the reforms.

From the heterogeneous treatment effect analyses, it appears that the increase in hiring was about 10% lower for women than for men, possibly suggesting that in Italy males are considered, on average, riskier employees. Moreover, women’s starting earnings at a new job grew by 4% more compared to their male counterparts, pointing at an improved job match quality for female workers as a result of the reforms.

When considering geographical differences, I show that the increase in hiring for firms located in the North was 45% higher than those in the Center-South. These results show that the reforms were unsuccessful at alleviating geographical differences in the quality of local labor markets. It appears that, while the negative implications of weaker job protection in the form of termination rates were homogeneous across the Italian territory, the stronger local economies were more able to reap the benefits through higher hiring rates. However, these results might imply that the Center-South needs a longer time frame than the one considered here before it can experience such benefits of the reforms. Finally, I find no heterogeneous treatment effect for young workers across the range of outcomes I considered.

My next steps for this project are twofold. First, I am planning to proxy reinstatement in the dataset to see whether the policies caused a drop in reinstatement instances. This would allow me to examine if the policies only led to a change in “expected” costs of termination, or if they impacted the real and ex-post costs of terminating a worker. The second step consists of creating an industry-level proxy of exposure to employment protection legislation in Italy. This industry measure will quantify the level of constraint experienced by each industry as a result of termination costs, generating the possibility to focus on the industries that are more vulnerable to these laws and to study how they, in particular, are affected by the reforms.
Bibliography


2.10 Appendix

In this section, I report the figures corresponding to the event studies regressions for each outcome I consider in the paper. These graphs overall show a lack of anticipation effects or differential pre-trends for the years prior to the first reform in 2012. Hence, as mentioned in Section 3.6, the evidence arising from the figures shows that the assumptions needed for the causal interpretation of the results are highly plausible.

Figure A1: Event Study for Gross Permanent Hiring
Figure A2: Event Study for Termination

Event Study
Lead and Lags of Firing
Control firms of size class 11-15

Firing

Years surrounding the Fornero Reform

-0.02
-0.01
0
0.01
0.02

-5 -4 -3 -2 -1 0 1 2 3 4 5 6
Figure A3: Event Study for Job Switching

Event Study
Lead and Lags of Job Switching
Control firms of size class 11-15

Years surrounding the Fornero Reform

Job Switching
Figure A4: Event Study for Tenure

Event Study
Lead and Lags of Tenure
Control firms of size class 11-15

Tenure

Years surrounding the Fornero Reform
Figure A5: Event Study for Starting Weekly Earnings

Event Study
Lead and Lags of Starting Earnings
CControl firms of size class 11-15

IHS Starting Weekly Earnings

Years surrounding the Fornero Reform
Figure A6: Event Study for Increasing Rank

Event Study
Lead and Lags of Increase Rank
Control firms of size class 11-15

Increase Rank

Years surrounding the Fornero Reform
Figure A7: Event Study for Weekly Earnings

Event Study
Lead and Lags of Weekly Earnings
Control firms of size class 11-15

Weekly Earnings

Years surrounding the Fornero Reform

-5 -4 -3 -2 -1 0 1 2 3 4 5 6
Chapter 3

Do Lower Benefits Prevent Relocations? The Effect of Unemployment Insurance Generosity on Migration

Laura Montenovo

Abstract

The Mobilità program was an Italian unemployment insurance program implemented in 1991 and phased out in 2017 for workers who were laid off, often in mass, due to economic reasons or sizeable restructuring of the firm. In its initial form, the program was more generous, in terms of maximum months of benefits, among older workers and in the Southern regions, which are relatively underdeveloped. Using administrative data on over 80,000 workers between 2000 and 2017, I explore the effects on recipiency and migration of an Italian policy that progressively decreased the generosity of the program and made it homogeneous across geography and age groups in 2015 and 2016. I rely on a difference-in-differences method where the treatment dose is the reduction in the maximum duration of Mobilità benefits. I find that dropping the maximum number of Mobilità months by 12, 18, and 24 decreases the effective days of recipiency for the affected groups by 181, 292, and 257 days, on average, and lowers the probability of migration by 33.5%, 37.8%, and 31.5%, respectively. The levels of the outcome variables at baseline and the composition of the treatment groups may determine the lack of linearity of these estimates. Because I show that the drop in the duration of benefits decreases migration, my results imply that the unemployed workers use the benefits to finance the move. The liquidity constraint explanation dominates over the alternative possibility that the unemployed prefer to stay wherever the benefits are more generous. This natural experiment framework uncovers some unexplored consequences of heterogeneous unemployment insurance generosity.

Keywords: Unemployment Insurance; Public Programs Generosity; Unemployment Duration; Income Support; Internal Migration.

JEL codes: J61; J62; J63; J64; J65; I38
3.1 Introduction

Unemployment insurance provides income for workers who unwillingly become unemployed. The income these workers receive as part of the unemployment insurance program depends on their seniority and earnings at their previous job. The primary goal of unemployment insurance systems is to provide consumption continuity to the unemployed when they experience a sudden loss in income in case of termination. Hence, the intended effect of unemployment insurance is to smooth the consumption of individuals during their unemployment spells. Gruber (1997) is one of the founding papers showing that, in the U.S., this goal was broadly achieved, and Ganong and Noel (2019) is a more recent example.

However, a wide literature agrees that unemployment insurance may lead to unwanted effects by increasing the dependency of workers on social programs. Prior work found evidence of unintended effects of the unemployment insurance program arising from behavioral distortions and moral hazards on the part of workers and program recipients. In fact, individuals seem to adjust their labor supply to maximize the duration and level of benefit receptions. Previous work has found that unemployment insurance generosity lowers job search efforts and job-finding rates, and it raises reservation wages (Meyer, 1990; Card et al., 2007; DellaVigna et al., 2017; Krueger and Mueller, 2010; Marinescu and Skandalis, 2021).

Chetty (2008) empirically disentangles the impact of unemployment insurance benefits on unemployment duration in a welfare-reducing “moral hazard” component and a welfare-enhancing “liquidity” component. In the United States, he finds evidence that the latter determines about 60% of that relationship, implying that the unemployed, who are unable to perfectly smooth consumption, are highly sensitive to cash on hand in order to consume.

In this paper, I expand the existing literature on this trade-off between the intended and unintended effects of unemployment insurance generosity by focusing on internal migration, a relatively unexplored aspect so far. In particular, I investigate whether more generous unemployment insurance facilitates or dampens the long-term economic opportunities and outcomes of the recipients by shaping their incentives to move across regions.
characterized by different levels of economic development. To do that, I consider an Italian unemployment insurance design that historically provided more generous benefits to workers considered vulnerable based on their age and geographical location, with older workers and workers in the South benefitting from more months of unemployment insurance. Because the maximum number of monthly unemployment insurance payments was greatest for older workers located in the South, this design provided more protection to workers who were traditionally and statistically more likely to experience weaker labor markets and to remain unemployed for longer. This program was called Mobilità, and, starting in 1991, it assisted workers experiencing layoffs caused by economic reasons often tied to macroeconomic trends or by structural transformations within firms. Due to the type of terminations the Mobilità program was associated with, most workers in the program were part of mass layoffs.

The longer duration of unemployment insurance benefits allowed in the Southern regions may have incentivized workers dismissed in the South to stay unemployed for longer compared to their counterparts in the Center-North. In addition, because of this difference in generosity, the Mobilità may have encouraged workers to be employed by firms in the South, hence incentivizing them to remain in the relatively underdeveloped area of the country.

Italy represents an ideal setting to understand these patterns because of a reform that, in 2012, changed unemployment insurance generosity in a way that differed by workers’ geographic location and age. The reform, legislative decree 92/2012, was implemented with the ultimate goal of eliminating the Mobilità program. To achieve this goal, the reform introduced progressive and differential reductions to the generosity of the Mobilità program across several age-geography groups of workers over a period of two years, until it merged with the traditional unemployment insurance system already in place. By 2017, Italy had a single unemployment insurance program regardless of the reasons behind the termination, although some differences remained based on the age, characteristics, or job history of workers. However, heterogeneous generosity based on geography was completely eliminated. This pattern of policy changes provides a clear identification strategy for computing the causal
effect of changes in unemployment insurance generosity based on duration. I explore the effects of these changes on the duration of the program’s recipiency and on the probability of migration for unemployed workers.

If the original design prompted unemployed workers to stay in, or potentially move to, underdeveloped areas so that they could be eligible for more maximum months of unemployment insurance, then the reform might incentivize workers to intensify their job search as well as to move to areas characterized by stronger labor markets, which in turn may improve their economic outcomes. Alternatively, it is possible that individuals use the more generous unemployment insurance to help subsidize their relocation to more industrialized and developed areas. In fact, moving requires financial liquidity and it involves risks in that the migrants leave a known environment and network to settle somewhere new. Unemployed workers are even more vulnerable to these costs because they have no earned income. If the liquidity mechanism dominates, then the reform that differentially decreases unemployment insurance generosity by age and geography will result in lower internal migration rates among the unemployed workers who experienced the largest drops in benefits.

My research contributes to estimating the wide effects of such a large government insurance program. In my analysis, I rely on an administrative panel dataset from the Italian Social Security Institute that includes demographics and complete employment information on a large random sample of Italian individuals.

First, I descriptively observe how the initial design of the Mobilità program affected the duration of unemployment insurance recipiency across age and geographical groups. Second, I examine how the reform that made these benefits progressively homogeneous impacted the duration of such payments both graphically and using a difference-in-differences model. Third, because the design of the Mobilità program could have influenced the migration incentives of the unemployed individuals, I rely on a similar empirical model to estimate how the reform impacted the relocation of workers across different local labor markets.

Descriptive evidence suggests that the geographical gap in benefits recipiency decreases
with the age of the unemployed and that this gap sharply drops towards the end of the Mobilità program across age groups. Empirical estimates from the difference-in-differences models show that the reforms have decreased both the number of actual days the affected workers receive Mobilità benefits, by about 15 days for each one-month-decrease in maximum duration, and their probability to migrate by over 30%.

My research aims at quantifying whether and to what extent public programs design, specifically that of unemployment insurance, perpetuates undesirable outcomes. To my knowledge, this is the first work empirically estimating the causal impact of the progressive homogenization in unemployment insurance benefits duration on internal migration decisions in a quasi-experimental setting. My results offer insights into how policy can strengthen or weaken attachment to low-development areas, impact the dependency on public programs, and affect the relocation to relatively more advanced regions. If giving more generous unemployment insurance to the more vulnerable workers functions as a magnet for the unemployed to stay in lower-productivity regions, this may prolong their time in unemployment, as well as lower the chances to find high-quality job matches and climb the professional ladder. By contrast, it is possible that more generous unemployment insurance benefits play a positive role in the migration decision by providing economic security to unemployed workers who are hesitant to face the risk of moving due to financial constraints and instability. If that is the case, by lowering the benefits, the reform might decrease the economic buffer that the unemployed rely on to migrate and could lower relocation rates as a result.

My findings allow for a more informed understanding of the effects of heterogeneous unemployment insurance generosity within a country, providing directions for improving it.
3.2 Literature Review

3.2.1 Unemployment Insurance, Unemployment Spells, and Labor Market Outcomes

The benefits of unemployment insurance programs, mostly their ability to smooth the consumption of individuals during their unemployment spells, have been estimated and shown in Gruber (1997). More recently, Ganong and Noel (2019) find that spending dramatically drops following the decrease in income due to the exhaustion of unemployment insurance benefits. In addition, the authors conclude that extending unemployment insurance benefits provides four times as much consumption smoothing than when raising them.

However, the unemployment insurance program has been found to have unintended consequences arising from behavioral distortions on the part of recipients. Meyer (1990) shows that more generous unemployment insurance benefits negatively impact the unemployment hazard rate, which sharply rises close to the expiration of the benefits. Similarly, Krueger and Mueller (2010) has found that job search intensity increases prior to the exhaustion of unemployment insurance benefits, while no change arises among the workers ineligible for the benefits. Using administrative data from Austria, Card et al. (2007) estimate that an extension of the potential duration of unemployment insurance benefits from 20 to 30 weeks lowers job finding rates by 5 to 9 percent in the first 20 weeks of job search. Marinescu and Skandalis (2021) rely on French data to show similar mechanisms: job applications increase by more than 50% in the year prior to benefits exhaustion. Further, reservation wages decrease by at least 2.4% during that same year and remain low. However, DellaVigna et al. (2017) show that individuals’ job search effort follows patterns tied to their reference points of consumption, with individuals relaxing their job search efforts as they get used to their new consumption reference points. Not only does unemployment insurance generosity determine the behavior of the individual during unemployment, but there is evidence that its eligibility requirements shape the behavior of workers when employed. For example, Baker
and Rea (1998) found that an increase in seniority required for eligibility leads to an increase in employment hazard rates corresponding to that new temporal threshold.

Previous works have also focused on how unemployment insurance generosity impacts the quality of employment matches following the unemployment spell. The idea is that the more generous the benefits, the more time the unemployed have to find their preferred job or simply a better-paid job, hence raising their reservation wage. The evidence for this mechanism is mixed. In fact, it is not obvious that more time spent receiving the benefits enhances the future job opportunities of the individual exiting unemployment. For example, Card et al. (2007) found no evidence that increases in the duration of job search arising from extended unemployment insurance benefits result in improved job match quality. By contrast, Nekoei and Weber (2017) shows that extending the benefits by 9 weeks leads to an increase in average wages at the next job by 0.5%, with the effect being persistent and not interfering with other aspects of the employment position.

3.2.2 Welfare-Induced Migration and Unemployment Insurance

Because I am interested in understanding whether the internal migration choices of unemployed individuals in Italy are affected by changes in the maximum duration of unemployment insurance benefits, literature on welfare-induced migration is relevant to this work. Welfare-induced migration is a phenomenon of migrants or potential migrants choosing where to move based on the varying levels of welfare system generosity in their original or destination areas. Fields (1979) reviews some existing literature on welfare migration from decades ago, and highlights that unemployment insurance is the only welfare program impacting in-migration in the expected direction, though with large variations in magnitude and sometimes insignificantly. Goss and Paul (1990) uses cross-sectional PSID data and shows that, on average, receiving unemployment benefits has a statistically insignificant impact on the probability of migration, but recipients who have been involuntarily terminated are more
likely to migrate.\textsuperscript{1} Enchautegui (1997) finds evidence that welfare payments in the hosting location are a significant determinant of the probability for women to move interstate in the U.S., with effects being stronger for women with a higher propensity to use welfare. Heitmueller (2005) considers a framework where unemployment insurance acts as a device that alleviates the risk of migration among risk-averse unemployed individuals. Their calibration exercise shows that unemployment benefits increase returns to migration, especially among the risk-averse. Kennan and Walker (2010) adopt a dynamic choice model where individuals make migration decisions to locations with varying degrees of welfare benefits and their simulations imply that welfare generosity does not explain migration decisions among young welfare-eligible women. Using data from the European Community Household Panel, De Giorgi and Pellizzari (2006) show that welfare generosity at the destination is one of the determinants of the probability of migrating.

Day and Winer (2011) reviews the empirical literature about the effect of the Canadian unemployment insurance program design, which, like the one considered here, allocated more generous benefits in relatively disadvantaged regions, on internal mobility. The results discussed mostly rely on explanatory regressions using cross-sectional, high-level, survey data, or, for just a few years, microdata, between 1950s and 1990s. The authors conclude that, due to a lack of quasi-experimental evidence, none of these studies could disentangle the effects on migration of the regional heterogeneity in the unemployment insurance benefits from those of the local labor markets conditions and unemployment rates.

Using data from the European Community Household Panel, Tatsiramos (2009) investigates the effect of unemployment insurance benefits on the geographic labor mobility of males in 5 countries. They consider that unemployment insurance generosity could have effects on workers’ mobility that go in opposite directions: it could increase reservation wages and decrease the willingness to move for a job or it could provide the liquidity to move. They run country-level and pooled country binary choice models to study how the probability to

\textsuperscript{1}One potential explanation the author offers for this could be that workers who have been involuntarily terminated are more likely to wait in the same location hoping to be recalled by the former employer.
move within the same country changes across countries characterized by different generosity levels. Overall, they find that receiving benefits is not associated with a statistically lower probability of moving. Because recipients with the lowest probability to move are in the UK, which is the least generous country among those considered, the authors suggest that the liquidity constraints explanation may dominate, as receiving benefits appears to increase mobility on average.

To my knowledge, only a few papers attempted to causally estimate the effect of welfare generosity in the receiving location on migration. In McKinnish (2005), the author exploits the fact that individuals closer to state borders experience lower migration costs compared to those in more internal counties. The estimates suggest that border counties with $100 more generous AFDC benefits compared to their neighboring counties have AFDC expenditures that are up to 7 percent higher relative to their interior counterparts. McKinnish (2007) finds some further, though statistically insignificant, evidence of welfare-induced migration.

Finally, in Nunn et al. (2018), the authors consider the generosity of the unemployment insurance benefits across U.S. states, and, by aggregating tax data to the state-pair year level, examine whether the unemployment insurance generosity in the origin state affects migration. The authors estimate that a one-week increase in unemployment benefits in the origin state increases the probability of moving by 0.24 percent, highlighting the importance of the portability of such benefits. Their results signal that more generous unemployment insurance allows for a more ambitious job search, which more likely results in across-state migration.

Due to the lack of quasi-experimental variation in employment insurance generosity within-country and over time, while these prior works provide invaluable contributions, their estimates may be hardly interpreted as causal.

With this paper, I contribute to the literature in several ways. First, using historical discontinuities in unemployment insurance duration generosity, I strengthen the evidence on how unemployment insurance generosity impacts the actual recipiency of the benefits.
Moreover, I utilize the age and geographical discontinuities, and their change over time, in generosity to look at migration probability. In particular, I focus on how unemployment insurance duration affects the choice to migrate internally. However, my main contribution is the ability to rely, in my analysis, on an administrative longitudinal dataset and on a policy change that provides a quasi-experimental setting where unemployment generosity is progressively reduced across age and geographic groups. To my knowledge, this is the first time such empirical framework and policy change are used to answer causal questions on the impact of unemployment insurance generosity and their different levels within a country on the migration of the beneficiaries.

3.3 Unemployment Insurance system in Italy

In Italy, between 1991 and 2017, two different unemployment insurance systems existed to protect workers depending on the type of involuntary layoff they were involved in. Workers who were part of a mass layoff due to economic reasons or a wide structural transformation of a firm’s activities were enrolled in the Mobilità program. The Mobilità program provided unemployed workers affected by mass layoffs or layoffs related to large macroeconomic downturns with unemployment insurance, as well as some re-employment services and benefits. For example, firms hiring unemployed workers in the Mobilità program could benefit from hiring subsidies. Similarly, if the firms that initially laid off the workers in Mobilità were in the process of re-hiring, they had to consider those workers in the Mobilità list first. The second unemployment insurance system is the more traditional one, similar to other unemployment insurance systems across the world, and it applies to workers losing their job individually and against their will.

In this paper, I focus on the Mobilità program and on the evolution of its generosity levels between 2014 and 2017. The generosity of the Mobilità program was historically, and specifically starting in 1991, tied to geographical and demographic factors so that the more vulnerable workers, as defined by the location of their dismissal or age, were eligible for more
months of payments. In particular, older workers located in the South benefited from the longest possible duration of Mobilità payments, up to 48 months.

In order to identify the variation in the generosity of unemployment insurance, I use an Italian policy (Law 92/2012) implemented in 2012, which aimed at the eventual elimination of the Mobilità program in 2017. The reform triggered the progressive reduction of the maximum unemployment insurance duration across most age-geography groups between 2013 and 2017. In particular, it progressively decreased the maximum number of recipiency months by the age group of the worker and geographical location of the firm dismissing the worker, introducing a variation in generosity changes that offers a clear identification strategy for my research questions. The plan of the reform was to completely eliminate the Mobilità program by 2017 and to substitute it with the traditional unemployment insurance program for all workers, regardless of the type of dismissal they were part of.

Based on the Mobilità program, there were three age-based generosity groups, up to 39, 40-49, and 50+ years old, and two geography-based groups, the Center-North and South macro-regions. Out of the 20 Italian regions, 8 are in the North, 5 in the Center (hence, 13 are in the Centre-North macro-region), and 7 in the South. The initial generosity level as well as its progressive reduction depends on which of the age-geography groups the unemployed workers belonged to. For example, while workers below 40 years old in the North-Central macro-region experienced no change in maximum duration, which remained at 12 months, their counterparts in the South saw the maximum duration of benefits halve from 24 to 12 months between 2015 and 2016, and match the young workers in the Center-North.

For the other age groups, the reform decreases the relative advantage of staying in the South. The 40-49 and 50+ age groups keep experiencing a geographical difference until 2016, but there is a decrease in their relative advantage of staying in the South. Before the reform, the Southern unemployed workers of age 40-50 and 50+ could benefit from 12 extra months of Mobilità payments compared to their Center-Northern counterparts, but between 2015 and 2016 this advantage was halved for both of these age groups. No age or geographical
differences in the level of Mobilità benefits were ever implemented as part of this program’s design, as only the duration was affected.

Starting in 2017, the Mobilità program is eliminated, and with it all geographical differences in unemployment insurance generosity. Italian workers become part of a unique unemployment insurance program called ASPI.

Table 1 summarizes the progressive reduction in maximum months of benefit recipiency by workers’ age group and macro-region brought about by the 2012 reform.

Table 1: Maximum duration (months) of eligibility for the Mobility unemployment insurance program by age and geography

<table>
<thead>
<tr>
<th>Age Group</th>
<th>Till Dec 31 2014</th>
<th>Jan 1-Dec 31 2015</th>
<th>Jan 1-Dec 31 2016</th>
<th>From Jan 1 2017 (Aspi)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Up to 39 yo</td>
<td>12</td>
<td>12</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>40 yo 49 yo</td>
<td>24</td>
<td>12</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>50 to 55 yo</td>
<td>36</td>
<td>24</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>55+ yo</td>
<td>36</td>
<td>24</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>Center-North</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to 39 yo</td>
<td>24</td>
<td>12</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>40 to 49 yo</td>
<td>36</td>
<td>24</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>50 to 55 yo</td>
<td>48</td>
<td>36</td>
<td>24</td>
<td>12</td>
</tr>
<tr>
<td>55+ yo</td>
<td>48</td>
<td>36</td>
<td>24</td>
<td>16</td>
</tr>
<tr>
<td>South</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to 39 yo</td>
<td>24</td>
<td>12</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>40 to 49 yo</td>
<td>36</td>
<td>24</td>
<td>12</td>
<td>10</td>
</tr>
<tr>
<td>50 to 55 yo</td>
<td>48</td>
<td>36</td>
<td>24</td>
<td>12</td>
</tr>
<tr>
<td>55+ yo</td>
<td>48</td>
<td>36</td>
<td>24</td>
<td>16</td>
</tr>
</tbody>
</table>

3.4 Data

Starting in April 2013, the Italian Ministry of Work and Social Policies started providing administrative data for research purposes. The information is either collected from employers by the social security office via disclosure requirements or it is generated by the office itself for purposes of social security contribution. For this paper, the Ministry provided me with a dataset containing information on the entire working history of a large random sample of Italian individuals. In particular, the data captures individuals born on the first and ninth day of any month, which amounts to about 6.6% of the Italian population.\footnote{In 2010, I have a total of 1,072,366 employees in my sample. From official data, I found that in the same year, the total number of employees (excluding self-employed) was 16,833,000, and 6.6% of that amounts to 1,110,978, which is close to my sample size for that year.} For
these employees, the dataset contains basic demographic information and, for those who ever entered the labor force, complete longitudinal data on their working history, including their participation in the unemployment insurance system and reception of corresponding benefits.

In this paper, I focus on all workers who were part of the Mobility program and received corresponding benefits, and I use their information on the date and region of birth, their dismissal date, and dates on the beginning and end of the Mobility benefits recipiency. I rely on the workers’ IDs and the date of their dismissal which started the Mobility period to merge in firm-level information from a larger separate dataset that includes all employment relationships of the workers in the sample. Between 2000 and 2016, 95,604 dismissals led to the workers involved becoming recipients of Mobility benefits. Of these, I was able to retrieve firm-level data and match the dismissal with the corresponding firm in 85,548 cases.\(^3\)

Because there is no variable on the firms’ location, I have to impute that information using data on the regions of birth of workers in each firm. In order to uncover the geographical distribution of the workers, I compute a variable for the modal macro-region of birth of the workers in each firm.

In other words, I impute the location of the firm from the geographical macro-region where the majority of its workers, over all years, were born. First, I drop all observations corresponding to firms for which I have only one worker in the dataset, as I do not have enough information to impute the modal location for these firms. Second, I drop all observations corresponding to firms for which there are two or more modes. Overall, this imputation is quite precise because the level of geography that is relevant for the Mobility program generosity is much coarser compared to the one I have in the variable for the region of birth. In fact, there are 20 regions in Italy, and while I have information at the regional level for workers’ birth location, I only need to know whether a firm is in the Center-North macro-

\(^3\)If a worker was dismissed on the same day by multiple firms, I had to drop these observations as I cannot pinpoint which of these terminations resulted in the participation in the Mobility program, and so I would not be able to be sure about the macro-region of dismissal.
region or Southern macro-region for the purpose of the reform. This improves the precision of my estimate because I rely on detailed geographical information to build a higher-level geographical variable. To build the variable for the macro-region of each firm over time, I compute the modal macro-region of birth of its workers across all years the firm appears on the data. Overall, I conclude that the variable for firms’ location imputed using the information on the region of birth of their workers is reliable. Because I could not merge some of the firms in the sample to their modal location, I end up dropping 2,878 observations out of the 85,548. Finally, I exclude from my analytic sample 170 workers for whom I have missing demographic information.

As a result of this cleaning process, my analytic sample refers to the years 2000 to 2017 and is composed of 82,500 spells in the Mobility program which involve 28,242 unique firms and 80,079 unique workers.

For the analysis of migration trends, I clean the dataset further. Because the only way for me to know the location of workers is through the firm they work at, I can deduce the migration patterns only for the individuals who get re-employed after their Mobility period. As a consequence, from the analysis of migration patterns, I have to exclude workers who do not re-gain employment following their Mobility recipiency, because I cannot know their location. Moreover, because the dataset ends in 2017, and the Mobility program ends in 2016, I consider migration that occurs only up until the end of the year following the dismissal year. Hence, if the worker gets dismissed in year $t$ and enters the Mobility program, they are included in the sample of the migration analysis only if they become re-employed at most by the end of year $t + 1$. This allows me to make migration patterns pre- and post-policy comparable. From the location of the firm re-employing the worker, I deduce whether, and where, the worker migrated. After this cleaning procedure, I am left with 36,663 workers in the migration sample.

Table 2 shows the means, standard deviations, and numbers of observations for each of the treatment groups for both outcomes, days of recipiency, and migration rate, between
2000 and 2014, the pre-reform year. Table 3 shows the same statistics over the same time period, but this time for the sample split by age and macro-region rather than by treatment status.

Table 2: Summary Statistics of the Outcomes by Treatment Group - Averages on the pre-reform period

<table>
<thead>
<tr>
<th>Composition</th>
<th>Days Recipiency</th>
<th>Migration</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (SD)</td>
<td>Mean (SD)</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>N</td>
</tr>
<tr>
<td>12 Months Benefits</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to 39 Ys Old - North-Center</td>
<td>292 (307)</td>
<td>0.02 (0.138)</td>
</tr>
<tr>
<td></td>
<td>16,488</td>
<td>10,275</td>
</tr>
<tr>
<td>24 Months Benefits</td>
<td></td>
<td></td>
</tr>
<tr>
<td>40-49 Ys Old - North-Center</td>
<td>567 (487)</td>
<td>0.05 (0.21)</td>
</tr>
<tr>
<td></td>
<td>21,889</td>
<td>10,185</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>36 Months Benefits</td>
<td></td>
<td></td>
</tr>
<tr>
<td>50+ Ys Old - North-Center</td>
<td>830 (501)</td>
<td>0.027 (0.17)</td>
</tr>
<tr>
<td></td>
<td>32,670</td>
<td>11,114</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>48 Months Benefits</td>
<td></td>
<td></td>
</tr>
<tr>
<td>50+ Ys Old - South</td>
<td>960 (611)</td>
<td>0.07 (0.26)</td>
</tr>
<tr>
<td></td>
<td>5,319</td>
<td>1,558</td>
</tr>
</tbody>
</table>
Table 3: Summary Statistics of the Outcomes by Age and Geography Group - Averages on the pre-reform period

<table>
<thead>
<tr>
<th>Age Group</th>
<th>Days Recipiency</th>
<th>Migration</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (SD)</td>
<td>Mean (SD)</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>N</td>
</tr>
<tr>
<td><strong>South</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to 39 Ys Old</td>
<td>666 (608)</td>
<td>0.16 (0.37)</td>
</tr>
<tr>
<td></td>
<td>6,295</td>
<td>2,274</td>
</tr>
<tr>
<td>40 - 49 Ys Old</td>
<td>835 (677)</td>
<td>0.13 (0.34)</td>
</tr>
<tr>
<td></td>
<td>4,593</td>
<td>1,605</td>
</tr>
<tr>
<td>50+ Ys Old</td>
<td>960 (611)</td>
<td>0.07 (0.26)</td>
</tr>
<tr>
<td></td>
<td>5,319</td>
<td>1,558</td>
</tr>
<tr>
<td><strong>North-Center</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to 39 Ys Old</td>
<td>292 (306)</td>
<td>0.02 (0.14)</td>
</tr>
<tr>
<td></td>
<td>16,488</td>
<td>10,275</td>
</tr>
<tr>
<td>40 - 49 Ys Old</td>
<td>527 (422)</td>
<td>0.02 (0.14)</td>
</tr>
<tr>
<td></td>
<td>15,594</td>
<td>7,911</td>
</tr>
<tr>
<td>50+ Ys Old</td>
<td>960 (611)</td>
<td>0.01 (0.10)</td>
</tr>
<tr>
<td></td>
<td>28,077</td>
<td>9,509</td>
</tr>
</tbody>
</table>
3.5 Outcomes and Empirical Framework

The generosity of an unemployment insurance program can be determined by two institutional features. First, the maximum number of months an unemployed worker is eligible to receive unemployment insurance payments following an involuntary layoff. Depending on the unemployment insurance system design, different durations in benefits impact the distress unemployed workers experience from the lack of labor income. The second measure of generosity is the unemployment insurance replacement ratio, a measure of the share of previous labor earnings that is disbursed to the dismissed workers as unemployment benefits. The higher the proportion of previous labor earnings that is replaced through the unemployment insurance system, the more generous is the system. In this work, I exploit variation in the first dimension, the unemployment insurance maximum duration, to study the impact of unemployment insurance generosity on duration spells and migration across local labor markets.

First, I provide descriptive evidence of the differences in the duration of the effective recipiency days of Mobilità benefits by year of dismissal, age, and geographic group.

The purpose of the exhibits is to graphically show what differences in the effective take-up days of Mobilità receipt were present by age group and location in the period before the reforms and in the years corresponding to the policy changes.

Next, I will turn to difference-in-differences models to empirically estimate the impact of the reform that changed the Mobilità program’s maximum duration first on the effective recipiency of the benefits and then on the probability of migrating within Italy. In the model, I exploit the differential drop in Mobilità benefits duration by age and geographical group to identify the treatment groups. In such empirical estimation, I aim to study the causal effect of maximum unemployment insurance generosity on recipiency days and migration across the Center-Northern and Southern macro-regions.

To empirically estimate the effect that the reform has on the actual number of days of Mobilità recipiency, I run a difference-in-differences model where the outcome is the number
of days each worker has received Mobilità benefits following a dismissal. This regression estimates whether the reform, by decreasing the maximum number of months any unemployed worker could receive the Mobilità benefit, lowered the actual number of days the recipients received such benefits. In some way, this estimation is a first-stage check that, as expected, the actual average number of days of Mobilità benefits decreased as a result of the drop in the maximum number of days allowed. The independent variables are the interactions between the post-reform dummy, equal to 1 for the years 2015 and 2016, and each of the four treatment groups, which can be identified from Table 1, as well as the main effects. Then I add a control variable for being a Female, and I progressively include four sets of fixed effects: region of birth, year of birth, the macro-region of dismissal, and the year of dismissal.

The fully specified model estimating the effect of dropping the maximum generosity on Mobilità benefit take-up is a linear regression that looks as follows:

\[
\text{DaysRecipiency}_{igt} = \alpha_1 + \alpha_2 \text{12MonthsLess}_{gt} + \alpha_3 \text{18MonthsLess}_{gt} + \\
\alpha_4 \text{24MonthsLess}_{gt} + \alpha_5 \text{12MonthsLess}_{gt} \times \text{Post}_i + \alpha_6 \text{18MonthsLess}_{gt} \times \text{Post}_i + \\
\alpha_7 \text{24MonthsLess}_{gt} \times \text{Post}_i + \alpha_8 \text{Female}_i + \text{YearBirth}_i + \\
\text{RegionBirth}_i + \text{YearDismissal}_i + \text{MacroRegionDismissal}_i + \epsilon_{igt}
\]  

(3.1)

In the model above, \(\alpha_5\), \(\alpha_6\), and \(\alpha_7\) estimate the impact of the reform decreasing the maximum number of months of Mobilità recipiency by 12, 18, and 24 respectively, on the effective number of days the average worker in the corresponding treatment subgroups receives benefits in the post-reform period. Given that the reform decreases the maximum duration of recipiency, we would expect these coefficients to be negative, pointing at a decrease in the actual benefit recipiency as well.

Next, I focus on estimating the effects of the reform on migration patterns. For this analysis, because the only way I can know information about the location of workers is
through the firms they work at, I have to focus on a subgroup of individuals in the sample, and in particular on those that have found employment after the Mobilità period. In fact, because I cannot impute information on location for individuals who do not become re-employed following Mobilità, I would not be able to build a migration outcome for them. In building the variable for migration, I consider workers who were participating in the Mobilità program and become re-employed before the end of the year following their dismissal at the latest. This allows me to compare migration patterns before and after the implementation of the reform, considering that the dataset ends in 2017.

By exploiting the location of the firm that dismissed the worker, and that of the firm that re-employed her next, I deduce the migration patterns for this worker. I only consider workers who were in the Mobilità program due to a dismissal that occurred at some point between 2000 and 2016, and who were then re-employed at any firm by the end of the year following the dismissal. The outcome variable for the first set of regressions is a dummy variable equal to 1 if the macro-region where the firm that terminated this subset of workers differs from that of the firm that re-employed it. For example, migration equals 1 if the firm that terminated worker $i$ in year $t$ (with $t$ being any year between 2000 and 2016) was in the South and the firm that re-employed $i$ by date $t + j$, with $j$ being a number of days in $1 < j < 728$, is in the Center-North. Similarly, migration equals 1 if worker $i$ lost their job in the Center-North and regained employment in the South. Migration, instead, equals zero if the worker becomes re-employed in the same macro-region where they were terminated. For this question, the right-hand side of the regression is identical to Model 3.1, but this time I run a logistic regression because the outcome variable is a dummy indicator, as I am trying to estimate how the reforms impact the probability of migrating.
\[
\text{Migration}_{igt} = \alpha_{1igt} + \alpha_{2}12\text{MonthsLess}_{igt} + \alpha_{3}18\text{MonthsLess}_{igt} + \\
\alpha_{4}24\text{MonthsLess}_{igt} + \alpha_{5}12\text{MonthsLess}_{igt} \times \text{Post}_{t} + \alpha_{6}18\text{MonthsLess}_{igt} \times \text{Post}_{t} + \\
\alpha_{7}24\text{MonthsLess}_{igt} \times \text{Post}_{t} + \alpha_{8}\text{Female}_{i} + \text{YearBirth}_{i} + \text{RegionBirth}_{i} + \\
\text{YearDismissal}_{it} + \text{MacroRegionDismissal}_{it} + \epsilon_{igt}
\] (3.2)

In a logistic regression, the coefficients are interpreted as the impact on the log odd ratio of migration. For example, the log odd ratio of migration for having 12 fewer months in maximum Mobilità benefit months is \(\alpha_{5}\). To simplify the interpretation, the percentage change in the probability of migrating can be more easily obtained by computing \(e^{\alpha_{5}}\). In Section 3.6, I compute and interpret the magnitude of these coefficients.

Throughout the empirical analysis, I cluster errors by a variable that identifies the treatment groups. In particular, I build a variable that identifies the following 8 groups: workers up to 39 years old dismissed in the Center-North and, separately, in the South, workers between 40 and 49 years old dismissed in the Center-North and in the South, workers between 50 and 55 years old dismissed in the Center-North and South, and workers older than 55 dismissed in the Center-North and in the South. Because this variable follows the treatment assignment, and because it is likely that the observations within each of these 8 subgroups are correlated, I use these age-geography subgroups as the level of clusters. I compute robust clustered standard errors in each of the regression models I run.

The regression analysis relies on two key identifying assumptions. First, the difference-in-differences method assumes that workers in the control group would have followed trends in each outcome that are parallel to those in the treatment in the absence of the reform. In other words, there are no time-varying differences in the outcomes I consider, namely days of Mobilità recipiency and migration, across the treatment and control groups other than those originated by the reform. This context is a bit different in that there is only one control
group that does not experience any change in the maximum number of Mobilità months, workers under 40 years old dismissed by firms in the Center-North, and several treatment groups. First, workers between 40 and 49 years old dismissed in the Center-North and those up to 39 years old dismissed in the South experience an overall drop in maximum Mobilità months of 12. Second, the maximum Mobilità months for workers older than 49 years old dismissed in the Center-North and for workers between 40 and 49 years old dismissed in the South drops by 18. Finally, workers older than 49 in the South experience a drop in maximum Mobilità months by 24. The assumption is that there are common trends between each of these treated groups and the control group. Second, the analysis requires a strict exogeneity assumption that unobserved factors impacting each outcome are uncorrelated with the history of workers’ treatment statuses. In other words, the difference-in-differences model is identified as long as there are no anticipation effects of the reform, no differential pre-trends across treated and control workers, and no time-varying treatment effects beyond those captured by the variables in the regression.

It is common practice in this framework to fit event study regressions that help assess the plausibility of both assumptions. My data goes back in time for several years prior to the reform: the data starts in 2000 and the reform in 2015. I run two versions of the event studies, both considering the same control group, but differing in the definition of the treatment groups. Here, I show the event studies that group together all the treatment subgroups in one, because the sample size of each subgroup is a lot smaller, making the event studies for such comparisons much noisier. Years 2000 to 2014 are the pre-reform period, 2014 is the baseline year, and years 2015 and 2016 are the post-reform period. In Section 3.8.1 of the Appendix, I describe the event study method in detail and I report the corresponding results, which overall support the validity of the required assumptions. In fact, there are no differential pre-trends or anticipation effects for migration, the main outcome of interest. Moreover, while some positive pre-trends appear for a few years of the pre-period referring to days of Mobilità recipiency as the outcome, such effects disappear or
strongly weaken when I run the event studies analysis separately for the treatment groups based on the number of months the maximum recipiency dropped by as a result of the policy (i.e. 12 and 18). Importantly, for both of these treatment groups, there are no anticipation effects for the three years preceding the reform for the recipiency outcome, implying that any observed effect post-reform is likely to arise from the policy change.

3.6 Results

3.6.1 Descriptive Evidence

Figure 1 shows the effective average duration of Mobilità recipiency, measured as the number of days over time, and specifically by year of dismissal, for the Center-North and South separately and up to 2014, the last year before the reform altered the generosity in Mobilità benefits. In addition to showing spikes in duration corresponding to macro-economic crises, the graph suggests that throughout the period considered, the average number of days on Mobilità is greater in the South, though this gap appears to marginally shrink as we get closer to the first year of policy changes, 2015. This is not surprising, given that the maximum generosity of the program was historically set to be higher in the South, and that the South has historically had higher unemployment rates compared to the Center-North.

In Figures 2, 3, and 4 I plot the effective average number of days on the Mobilità program by each of the three age groups that are relevant for the Mobilità design, 18 to 39, 40 to 49, and over 50 years old, respectively, and separately for the Center-North and South macro-regions. The data plotted shows information for the entire period in the dataset: it shows the averages by year of dismissal between 2000 and 2016, the last year of the Mobilità program. In each graph, that is, for each age group, I show the trends by year of dismissal of the average days the workers in that age interval received Mobilità benefits. Some patterns arise from these three figures. First, the older the workers, the smaller the gap in recipiency between Center-Northern regions and Southern regions over the period. Second, the geographical gaps decrease over time, that is, by the year of dismissal, regardless of the age group considered.
Third, the geographical gaps across age groups seem to become similar towards the end of the period, although they were, overall, largely different between 2000 and 2010.

Figure 5 plots the average number of days of Mobilità recipiency by age of the worker and by Italian macro-region using data between 2000 and 2014. From the figure, it appears that the largest geographical differences in the duration of benefits occur for workers younger than 40, while this gap steadily decreases for older workers. These results point to the fact that younger workers are those most benefitting from the relatively more generous Mobilità duration present in the South compared to the Center-North. However, the same information, but this time plotted for the post-reform period is shown in Figure 6 and shows trends toward the homogenization of differences in Mobilità duration over the workers’ ages and across Italian macro-regions. This is preliminary evidence that the reform was successful in alleviating take-up gaps across age and macro-regions. In addition, these two figures jointly show that, regardless of the relative geographical differences in Mobilità duration across age groups, as the age of the unemployed workers increases, the duration of Mobilità recipiency increases, which is to be expected given that the maximum number of Mobilità months steadily increases with age. Finally, these graphs confirm what I outlined above based on Figures 2, 3, and 4: differences in Mobilità recipiency between Center-North and South are starkest among younger workers.
Figure 1: Average Duration of Mobilità benefits over time and by Italian macro-region
Figure 2: Average Duration of Mobilità benefits over time for workers of age 18 to 39 by Italian macro-region
Figure 3: Average Duration of Mobilità benefits over time for workers of age 40 to 49 by Italian macro-region.
Figure 4: Average Duration of Mobilità benefits over time for workers of age 50 and above by Italian macro-region
Figure 5: Average Duration of Mobilità benefits over time and by workers’ age group between 2000 and 2014, the pre-reform period
Figure 6: Average Duration of Mobilità benefits over time and by workers’ age group in 2015 and afterward, the post-reform period.
3.6.2 Results from Regression Models

Table 4 shows the results for linear Model 3.1, which estimates the impact of the reform on the effective average days of Mobilità benefit recipiency. The regression considers three treatment groups based on the drop in the maximum number of months of benefits the policy change introduced. Column (2) adds the control variable for Female and the fixed effects for the year of birth and region of birth of the workers. Column (3) further includes fixed effects for the year of dismissal and macro-region of dismissal. These models aim at testing a sort of first-stage effect of the reform. The coefficients estimate whether decreasing the maximum number of months each treated group can receive Mobilità benefits for leads to a decrease in the effective average number of days they actually receive them. Overall, as expected, we see that the reform decreases the effective number of days the treated groups are recipients of Mobilità payments in the post-reform period. The coefficients on the interaction terms should be interpreted compared to the control group, which was omitted, and is composed of workers under 40 years old dismissed in the Center-North. The estimates from the fully specified model in Column (3) imply that decreasing the maximum number of Mobilità months by 12 months leads to a decrease in the number of days of Mobilità recipiency by 181 compared to the group that has experienced no change in the maximum Mobilità duration. For the second treatment group, for which the maximum benefits are decreased by 18 months, the policy leads to a drop in the effective number of days on Mobilità by 292 days. Finally, the group of workers (smallest treatment group in size) that experiences a drop in the maximum Mobilità duration by 24 months has a 257-day decrease in their effective number of Mobilità recipiency as a result of the reform.

The lower magnitude for the estimate corresponding to the group with the highest treatment dose compared to the group with the intermediate treatment dose (i.e. 18-month drop in the maximum duration) could be surprising. However, this could easily be explained by some baseline characteristics regarding the maximum Mobilità period and the effective recipiency days for these groups. The group with the highest treatment dosage is also the group
of workers who, prior to the reform, could count on the highest maximum duration of Mobilità benefits, i.e. 48 months. It is likely that they were not actually using proportionately more days compared to the groups whose benefits were less generous. In fact, the summary statistics in Table 2 confirm this idea: while between 2000 and 2014 the average days of reception increased with the maximum number of months available to each treatment group, the increase is not linear. Hence, it is possible that the group with the highest treatment usage was more likely, compared to the other groups, to stay in the Mobilità program for less than their pre-policy maximum, i.e. 48 months, thus making their decrease in effective recipiency days due to the policy lower than the counterparts who experienced an 18-months drop and who had 36 months of maximum Mobilità duration available up to 2014. Being a female worker is associated with receiving about 70 extra days of Mobilità benefits. The table shows that the addition of control variables and of the fixed effects decreases the magnitude of the effects, but it does not change their significance or relative differences in size across the three treatment groups.

Table 5 shows the impact of the reform on migration by presenting the estimates of Model 3.2. Similarly to Table 4, Column (1) only includes the main and interaction terms for the key difference-in-differences variables, $treat$ and $post$. In Column (2), I add the control variable for Female, and the fixed effects for the region and year of birth of the worker. In Column (3), I further include the fixed effects for the macro-region and year of dismissal. Again, the coefficients on the interaction terms measure the impact of the reform on the corresponding treated group relative to the control group, which was omitted, and is composed of workers under 40 years old dismissed in the Center-North. The table overall provides clear evidence that, for workers who become re-employed by the end of the year following their participation in the Mobilità program, the reform decreasing the maximum duration of Mobilità benefits leads to a lower probability of migrating. Because this is a logit regression, an easy transformation of the coefficients is needed for a useful interpretation. In particular, because the coefficients on the interaction terms of interest on the table are
negative, each coefficient \( \alpha \) on the interaction term needs to be transformed as follows to obtain a percentage effect of the reform on migration probability: \((1 - e^{\alpha}) \times 100\). In the most specified model, for example, the coefficient on the first interaction term, \(-0.408\), suggests that a 12-month drop of the maximum number of Mobilità months leads to a \((1 - e^{-0.408}) \times 100 = 33.5\%\) lower probability to migrate in the short-term following the dismissal year. The group of workers experiencing a decrease by 18 months in the maximum duration of Mobilità benefits as a result of the reform is \((1 - e^{-0.474}) \times 100 = 37.8\%\) less likely to migrate in the post-reform period. Finally, lowering the maximum Mobilità benefits by 24 months leads to a drop in the migration probability within the following year by \((1 - e^{-0.474}) \times 100 = 31.5\%\).

These results suggest that the access to unemployment insurance benefits provided by the Mobilità program help alleviate the liquidity constraints that are involved in the migration decision and that are so much tighter among the unemployed. In fact, because a decrease in the duration of such benefits results in lower, rather than higher, migration, the estimates imply that the unemployed rely on these financial resources in order to move to other locations, and when these resources are cut, they are less financially equipped to move. If the alternative channels dominated, and workers tended to stay wherever the benefits are more generous, then a movement towards the equalization of such benefits would have triggered a migration away from the locations previously characterized by more generous benefits and towards locations currently characterized by stronger labor markets. The fact that the effects of the policy on migration are so stark even when considering a short time frame following dismissal (at most, slightly less than 2 years) is further confirmation that those benefits are used to offer the liquidity needed to support the move. Given that the migration trends within Italy are overwhelmingly characterized by a net movement from the South to the North, the cut of the Mobilità benefits that occurred as a result of the elimination of the program ended up blocking some of the potential unemployed migrants in the South, due to their inability to pay for the move.
Table 4: Effects of the Reform Changing the Maximum Days of Mobility Program Duration on the Effective Days of Benefits Recipiency

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) Days Benefits</th>
<th>(2) Days Benefits</th>
<th>(3) Days Benefits</th>
</tr>
</thead>
<tbody>
<tr>
<td>12 less benefit months</td>
<td>275.4***</td>
<td>228.0***</td>
<td>280.2***</td>
</tr>
<tr>
<td></td>
<td>(0.000348)</td>
<td>(8.53e-07)</td>
<td>(0.000505)</td>
</tr>
<tr>
<td>18 less benefit months</td>
<td>538.7***</td>
<td>472.0***</td>
<td>573.7***</td>
</tr>
<tr>
<td></td>
<td>(9.52e-09)</td>
<td>(0.000160)</td>
<td>(0.000249)</td>
</tr>
<tr>
<td>24 less benefit months</td>
<td>668.1***</td>
<td>524.2***</td>
<td>685.4***</td>
</tr>
<tr>
<td></td>
<td>(2.10e-09)</td>
<td>(0.000203)</td>
<td>(0.000697)</td>
</tr>
<tr>
<td>Post (2015-2016)</td>
<td>-22.23*</td>
<td>-10.29</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0782)</td>
<td>(0.639)</td>
<td></td>
</tr>
<tr>
<td>12 less benefit months x Post (2015-2016)</td>
<td>-189.9***</td>
<td>-171.8***</td>
<td>-181.0***</td>
</tr>
<tr>
<td></td>
<td>(0.00565)</td>
<td>(0.00525)</td>
<td>(0.00650)</td>
</tr>
<tr>
<td>18 less benefit months x Post (2015-2016)</td>
<td>-286.0***</td>
<td>-287.1***</td>
<td>-292.4***</td>
</tr>
<tr>
<td></td>
<td>(3.23e-08)</td>
<td>(3.17e-06)</td>
<td>(7.44e-06)</td>
</tr>
<tr>
<td>24 less benefit months x Post (2015-2016)</td>
<td>-228.1***</td>
<td>-249.5***</td>
<td>-256.9***</td>
</tr>
<tr>
<td></td>
<td>(1.33e-07)</td>
<td>(1.84e-05)</td>
<td>(3.91e-05)</td>
</tr>
<tr>
<td>Female</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>72.58***</td>
<td>69.85***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00278)</td>
<td>(0.00502)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>291.9***</td>
<td>318.5***</td>
<td>249.4***</td>
</tr>
<tr>
<td></td>
<td>(6.51e-07)</td>
<td>(5.09e-05)</td>
<td>(0.00148)</td>
</tr>
<tr>
<td>Observations</td>
<td>82,500</td>
<td>82,365</td>
<td>82,365</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.195</td>
<td>0.234</td>
<td>0.242</td>
</tr>
</tbody>
</table>

Robust pval in parentheses

*** p<0.01, ** p<0.05, * p<0.10

Note: All standard errors have been clustered at the level the reforms introduced changed. That is, the clusters are the geography-age groups present in Table 1.
Table 5: Effects of Shortening the Unemployment Insurance Duration on Migration

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No Controls</td>
<td>Controls</td>
<td>Fully Spec.</td>
</tr>
<tr>
<td>Post (2015-2016)</td>
<td>0.913***</td>
<td>0.793***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(4.05e-09)</td>
<td></td>
</tr>
<tr>
<td>12 less benefit months</td>
<td>0.969</td>
<td>0.396</td>
<td>0.0649</td>
</tr>
<tr>
<td></td>
<td>(0.167)</td>
<td>(0.580)</td>
<td>(0.635)</td>
</tr>
<tr>
<td>18 less benefit months</td>
<td>0.363</td>
<td>0.340</td>
<td>-0.115</td>
</tr>
<tr>
<td></td>
<td>(0.665)</td>
<td>(0.775)</td>
<td>(0.758)</td>
</tr>
<tr>
<td>24 less benefit months</td>
<td>1.385***</td>
<td>0.583</td>
<td>-0.189</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(0.708)</td>
<td>(0.639)</td>
</tr>
<tr>
<td>12 less benefit months x Post (2015-2016)</td>
<td>-0.759***</td>
<td>-0.493***</td>
<td>-0.408***</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(0.000583)</td>
<td>(1.43e-09)</td>
</tr>
<tr>
<td>18 less benefit months x Post (2015-2016)</td>
<td>-0.404***</td>
<td>-0.501***</td>
<td>-0.474***</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(0.00958)</td>
<td>(0.000857)</td>
</tr>
<tr>
<td>24 less benefit months x Post (2015-2016)</td>
<td>-0.0193</td>
<td>-0.413***</td>
<td>-0.379*</td>
</tr>
<tr>
<td></td>
<td>(0.481)</td>
<td>(0.00667)</td>
<td>(0.0903)</td>
</tr>
<tr>
<td>Female</td>
<td>-0.606***</td>
<td>-0.614***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.37e-08)</td>
<td>(4.48e-08)</td>
<td></td>
</tr>
<tr>
<td>Region of Birth FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year of Birth FE</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Macro-Region of Dismissal FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Year of Dismissal FE</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Constant</td>
<td>-3.914***</td>
<td>-3.777***</td>
<td>-3.594***</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(0)</td>
<td>(6.39e-09)</td>
</tr>
<tr>
<td>Observations</td>
<td>36,663</td>
<td>35,871</td>
<td>35,871</td>
</tr>
</tbody>
</table>

Robust pval in parentheses

*** p<0.01, ** p<0.05, * p<0.10

Note: All standard errors have been clustered at the level the reforms introduced changed. That is, the clusters are the geography-age groups present in Table 1. Col. 1: No Controls; Col. 2: Female, Region of Birth FE, Region of Birth FE; Col. 3: Female, Macro-Region of Dismissal FE, Region of Birth FE; Year of Birth FE.
3.7 Conclusion

One of the main goals of public policy is to support individuals in underdeveloped areas by giving them the tools to establish stable employment trajectories and sustained income flows throughout their life. In this paper, I consider the role that the generosity of unemployment insurance programs, and in particular their duration, can play in this context.

In Italy, the Mobilità program was a special type of unemployment insurance dedicated to workers who were part of large layoffs arising from deep economic reasons or firms’ restructuring. The program was implemented in 1991, but a reform in 2012 aimed at phasing it out by 2017 and merging it with the traditional unemployment insurance system that was simultaneously in place. The Mobilità program was peculiar in that it allowed for more maximum months on the program depending on the age and macro-region of dismissal of the worker, which defined groups of workers characterized by different levels of vulnerability in the labor market. Hence, historically, the Mobilità program was more generous for dismissals occurring in the South and involving older workers.

As part of the phase-out of the Mobilità program, the reform has decreased the total number of maximum months of Mobilità benefits for all age-geography groups but one, the workers dismissed in the Center-North and younger than 40 years old, it eliminated all geographic differences in such generosity, and it lowered the variability of differences by age.

First, I use this quasi-experimental policy variation to study the causal effects of reducing the maximum generosity of an unemployment insurance program on its actual recipiency. Second, once I establish the existence of such first-stage effects, I explore the causal impact of dropping the generosity of unemployment insurance on the incentives to migrate of the unemployed workers. There are two contrasting mechanisms potentially in place. On the one hand, it is possible that a higher maximum duration of unemployment insurance benefits for some age-geography subgroup of workers incentivizes them to stay in the underdeveloped areas where local labor markets are weaker in order to take advantage of the benefits for longer, should they be dismissed again from their next job. If so, the heterogeneous gen-
erosity of the Mobilità program perpetuated the negative labor market outcomes of these workers, who did not relocate to local areas characterized by more and better quality jobs. By contrast, more generous benefits may help alleviate the liquidity constraints experienced by the unemployed workers when they are considering a potential relocation to the stronger local labor markets. In fact, moving is costly for everyone, and unemployed individuals who are more likely to struggle financially, may rely on the longer period of unemployment insurance payments to face the economic risk of moving. If the first channel dominates, the reform should increase migration rates. If the second channel dominates, the reform should result in lower migration rates.

I use an administrative panel dataset on a large random sample of workers containing information their employment histories and participation in the Mobilità program. I rely on a dose-based difference-in-differences analysis. The treatment groups are defined by the decrease in the number of maximum months of Mobilità benefits occurred due to the policy, and the control group is composed of workers up to 39 years old dismissed in the North, because they experience no change.

I find that the reform, as expected, decreases the number of days the treatment group receives the Mobilità benefits. On average, the most specified model implies that decreasing the maximum duration of benefits by 12 months lowers the number of recipiency days by 181, decreasing the maximum duration of benefits by 18 months lowers the number of days on Mobilità by 292, and shortening the maximum duration by 24 months leads to 257 fewer days on Mobilità. Because the last group, which was composed of unemployed workers of at least 50 years old dismissed in the South, was the only one allowed to receive up to 48 months of benefits before the reform, it is possible they were more likely to use well below that amount, leading to a lack of linearity in the estimates. These estimates mean that lowering the maximum duration of Mobilità by one month decreases the number of days on Mobilità by over 10 days on average.

Lowering the maximum duration of Mobilità benefits not only lowers the recipiency days
as expected, but has also strong negative effects on the probability of migrating. The fully specified logit regression model I consider suggests that, decreasing the number of maximum Mobilità months by 12, 18, and 24, leads to drops in the probability to migrate by 33.5%, 37.8%, and 31.5%, respectively. These estimates are the most conservative out of all the models I consider. Overall, in this work I offer unambiguous evidence that reducing the maximum duration of unemployment benefits for workers terminated for economic reasons leads to lower days of recipiency and sharply drops their probability to migrate across regions of the same country.

The results on migration provide strong evidence that the liquidity constraint explanation dominates. Because cutting the duration of the unemployment insurance benefits overwhelmingly leads to lower migration probabilities, it is highly plausible workers where not staying where the generosity of the benefits is higher so that they can benefit from it in case they get dismissed again from the next job. Quite the contrary, they were using those resources to relocate internally to areas where they could count on stronger local economies and have better chances in the labor market.

My results show strong evidence that unemployment insurance programs have the potential to support unemployed individuals in their decisions to migrate. In this sense, designing unemployment insurance generosity by expanding their duration might be an important tool to support the unemployed in their job search attempts, and to reallocate the labor force across regions in a way that is efficient and puts them in better positions to achieve long-term economic stability.

In future work, I will examine the type and quality of employment positions migrants have found post-relocation. In particular, I will use the difference-in-differences model to estimate the employment outcomes at the next job. I will consider wages at the new job following Mobilità and the rank information. Moreover, given that each age-location subgroup experiences a drop in generosity that varies by magnitude and time period, I will consider the recent developments in the literature about difference-in-differences estimation that are
relevant in cases of staggered adoption of policies.
Bibliography


3.8 Appendix

3.8.1 Event Studies

The event study regressions include indicator functions that trace out changes in the outcome variables in the years leading to and following the implementation of the reform. The regressions estimate the presence of differential effects between the treated group, which experiences at least 12 months of change in maximum recipiency (i.e. 12, 18, or 24), and the control group, which experiences no change in maximum recipiency, before the 2015 reform and after.

In my data, the earliest year is 2000. From 2000 to 2014, because no policy change occurred yet, we expect no differential or anticipation effects between the two treatment groups. I consider 2015 to be the adoption year and, because I omit the variable for 2014 from the regression, I consider it to be the reference year to interpret as a baseline for all the coefficients. I generate a variable, $YSA_t = t - 2015$, which measures the number of years between year $t$ and 2015, the implementation year of the Reform. For example $YSA_t = 1$ in year $t = 2016$, and $YSA_t = -2$ in year $t = 2013$. $P(k)_t$ is an indicator variable equal to 1 when $YSA_t = k$, that is $P(k)_t = 1(YSA_t = k)$. Treated workers are those that have experienced at least a 12 months decrease in the maximum number of Mobilità months as a result of the reform.

The event study model regresses each outcome, effective Mobilità recipiency and migration, for firm $i$ in year $t$ on dummy variables for each level of $P(k)_t$ interacted with $treated_{g,2015}$, and includes workers’ region of birth fixed effects, workers’ year of birth fixed effects, macro-region of dismissal fixed effects, and year of dismissal fixed effects:

$$\text{Outcome}_{igt} = \beta + \sum_{k=-14}^{K=-2} [\alpha_k P(k)_t \text{treat}_{g,2015}] + \alpha_0 P(0)_t \text{treat}_{g,2015} + \alpha_1 P(1)_t \text{treat}_{g,2015} + \alpha_2 \text{Female}_i + \text{YearBirth}_i + \text{RegionBirth}_i + \text{YearDismissal}_it + \text{MacroRegionDismissal}_it + \epsilon_{igt}$$

(3.3)
Because 2014 is the reference year, its interaction with $treated_{g,2015}, P(-1), Treat_{g,2015}$, is omitted from the model. This implies that all the $\alpha$ coefficients in front of the interaction terms should be interpreted as the differential effects of the policies in year $k$ compared to 2014. In the model, $K = 0$ refers to 2015, and $K = 1$ refers to 2016, the last year of the Mobilità program. I clustered standard errors at the age-geographical level. In the model, $\alpha_K$, with $-15 < k < -1$, estimate the response of the outcome variables to the future implementation of the Reform. They suggest whether there are anticipation effects of the upcoming reform that vary across treatment and control workers. I expect all these coefficients to be statistically equal to zero because, under the strict exogeneity assumption, future events should not impact present outcomes.

In contrast, $\alpha_0$ and $\alpha_1$ are the post-reform coefficients. In particular, $\alpha_0$ measures the effect of changes one year post-reform, and $\alpha_1$ estimates the effect of the reform after one year from its implementation, compared to 2014, the baseline year. The variation between $\alpha_0$ and $\alpha_1$ provides information on the potential time-varying effect of the reform. If the impact of the policy is drastic and remains constant over the years, then $\alpha_0$ will be positive and similar to $\alpha_1$. However, in the more likely case that the effects arise progressively and change over time, then $\alpha_1$ may be stronger than $\alpha_0$.

In the graphs below, I plot the estimates of the event studies obtained by running Equation 3.3 for the two outcomes: days of Mobilità recipiency and migration. When estimating the leads and lags for Mobilità recipiency I run a linear regression, while for migration I use a logit model. No significant pre-trends during 2010-2011 emerge from any of the graphs below, alleviating the concerns on anticipation effects. The event studies for the days of Mobilità recipiency are in Figures A1 and A2. In these two figures, I broke down the treated group into two subgroups: those experiencing a drop in maximum Mobilità duration by 12 and 18 months, respectively. That is, in Figure A1, I consider as the treatment group all workers whose maximum benefits decreased by 12 months as a result of the reform. Instead, in Figure A2, treated workers are those whose maximum benefits decreased by 18 months.

175
I excluded the event study for the group of workers whose maximum benefits decreased by 24 months because this subgroup is small, leading to a lack of power in the estimation of the event study regression. In fact, there would be too few observations in each event study year. Overall, the event studies for these two treated groups show weak or no pre-trends compared to the control group. Both Figures A1 and A2 show that, while the estimates for the pre-trends tend to be positive, their confidence interval are likely to overlap zero, and they are statistically insignificant for the 2 or 3 years preceding the reform. The absence of pre-trends and anticipation effects in the few years preceding the reform strengthen my confidence in the ability of the post-reform estimates to identify the causal effect of the reform.

Figure A3, instead, presents the event study graph for the main outcome of interest in this paper, migration. The figure shows the absence of any differential pre-trend between the treated and control groups in all years preceding the reform and exhibits a strong negative effect of the reform arising in 2016. Because all of the coefficients plotted between 2000 and 2013 revolve around zero, and because all their confidence intervals overlap zero, I can conclude that the drop in migration rate among the treated group that we observed in 2016 represents the causal of the reform on the outcome of interest. Moreover, I have run the versions of event studies similar to the above where I separate the treated group based on the exact decrease in the maximum number of unemployment insurance months workers experience, that is 12 and 18 respectively. Even these “broken-down” versions of the treatment groups generate event studies that lead broadly to the same conclusion of lack of anticipation effects and differential pre-trends. This holds with only a few exceptions on some scattered years where the treatment group that experienced an 18 months drop in unemployment insurance duration had some differential pre-trends, though these are small in magnitude and their confidence interval is relatively large.
Figure A1: Event Study for Days of Mobilità Recipiency - Treatment group: Workers Experiencing 12 Months Drop in Maximum Benefit Duration

Event Study
Lead and Lags of Days Mobility Recipiency
Control Group: 12 Fewer Months
Figure A2: Event Study for Days of Mobilità Recipiency - Treatment group: Workers Experiencing 18 Months Drop in Maximum Benefit Duration
Figure A3: Event Study for Migration - Treatment group: Workers Experiencing Any Drop in Maximum Benefit Duration
3.8.2 Mobilità Spells

As mentioned in the paper, the Mobilità program is a special type of program that exists for workers who have been dismissed from their employers due to large restructuring of the firm, or economic reasons. This program provides a form of unemployment insurance and benefits to unemployed workers that include the need of their former employers to prioritize workers in the Mobilità lists in case they are in need of labor. Because of this structure, workers are likely to experience multiple spells of the Mobilità benefits for the same dismissal event, as they are temporarily called back to the firm, and then re-sent home to receive the benefits again. Given this system, it is possible that the elimination of the program in 2016 led to a lower effective number of recipiency days because it decreases the number of Mobilità spells each worker dismissed towards the end of the program experiences over time. In fact, it is possible that workers dismissed closer to 2016 experience fewer days of Mobilità not because of the 2015 policy change, but because they stop being able to go back and forth between their previous employer and the Mobilità program, given that such a program is removed.

To check for this possibility, I plot the average number of spells for each worker and date of dismissal over the years in Figure A4. If it is true that the drop of effective days of Mobilità recipiency arises simply from the expiration of the Mobilità program which causes each worker to have access to fewer spells for their dismissal when closer to the program’s end, then we should observe that closer to 2016 the average number of spells drops because the program expires. The Figure does not show a lower average number of spells in the most recent years. In fact, the evidence is the contrary: the number of Mobilità spells for each worker’s dismissal is, on average, higher in the last few years. Figure A4 supports that the drop in effective days of recipiency observed in the post-reform and shown in Table 4 is indeed an effect of the policy change and not an artificial feature of the data arising from the program being eliminated.
Figure A4: Number of Mobility Spells for each Dismissal and Worker over time
Laura Montenovo, Ph.D.

Indiana University Bloomington
Paul H. O’Neill School of Public and Environmental Affairs
1315 E 10th St.,
Bloomington, IN 47405

Education

**Indiana University Bloomington**
Ph.D. in Public Affairs (June 2023).

**Bocconi University**
Master of Science in Economic and Social Sciences (April 2017).

**Ca’ Foscari University**

**Georgia State University**
Bachelor of Science (April 2014). Major: Economics.

Peer-Reviewed Publications


Under Review


Working Papers


Media Coverage

Montenovo, Laura, Xuan Jiang, Felipe Lozano Rojas, Ian M. Schmutte, Kosali Simon, Bruce A. Weinberg and Coady Wing. “Unequal Employment Impacts of COVID-19.” Econofact, June 1, 2020

Grants, Awards, and Fellowships

1. Publication Accelerator Grant, January 2023. Institute for Humane Studies at George Mason University, $5,000.

2. Publication Accelerator Grant, September 2022. Institute for Humane Studies at George Mason University, $5,000.

3. Frédéric Bastiat Fellowship Research Sequence, Fall 2022. Mercatus Center at George Mason University, $5,000.


5. Humane Studies Fellowship, Summer 2022. Institute for Humane Studies at George Mason University, $3,000.

6. Summer Merit Fellowship, Summer 2022. O’Neill School of Public and Environmental Affairs, Indiana University, $1,000.

7. Frédéric Bastiat Fellowship, Summer 2022. Mercatus Center at George Mason University, $2,500.

8. Don Lavoie Fellowship, Spring 2021. Mercatus Center at George Mason University, $1,250.

**Teaching**

*Instructor of Record*

V-202 Contemporary Economic Issues in Public Affairs at Indiana University. Spring 2020, Fall 2021, Spring 2022, Fall 2022, Spring 2023.

**Teaching Interests**

Labor Economics, Public Programs, Microeconometrics, Microeconomics, Economic Inequality, Public Finance.

---

**Conference Presentations**


Professional Service

*Workshop Coordinator*


*Journal Reviewer*


*Administrative Roles*


---

Professional Experience

Research Assistant, Dr. Kosali Simon, Indiana University (August 2019 - August 2020).
Research Assistant, Dr. Coady Wing, Indiana University (August 2018 - August 2019).
Research Assistant, Dr. Bradley Heim, Indiana University (August 2018 - August 2020).
Research Assistant, Dr. Fernando Vega Redondo, Bocconi University (April 2017 - May 2018).

---

Language & Technical Skills

Statistical Software: STATA, RStudio, \LaTeX.

Languages: English (fluent), Italian (native), French (elementary), Portuguese (elementary).