ESSAYS ON COMPANY TAXATION, FIRMS AND WORKERS

Mimosa Distefano

August 2022

Submitted to the Department of Economics in partial fulfilment of the requirements for the degree of

Doctor of Philosophy in Economics

UNIVERSITY COLLEGE LONDON

Declaration of Authorship

I, Mimosa Distefano, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Mimosa Distefano

Abstract

This thesis investigates how taxes levied on firms and firms' management composition affect firms and workers. The first essay focuses on Germany, where profit taxation is set at the municipality level. We show that an increase in the profit tax rate by 1 percentage point reduces municipality employment by 1.17%, municipality wages by 0.52%, and the number of establishments operating in the municipality by 0.5%. Whereas smaller, lower-paying establishments primarily drive establishment exit, within-establishment wage and employment declines are most pronounced in higher-paying establishments, causing a reduction in job-to-job mobility. Between-establishment wage growth contributes to the wage reductions experienced by workers hit by a profit tax increase. The second essay examines hiring subsidies—a temporary cut to payroll taxation for new hires—introduced in Italy in 2013. We combine a matched difference-in-differences design and zoom onto the firms that use the hiring subsidy. We find that these firms hire women with lengthy labor market interruptions and who are mothers. These women are better educated than the average firm's hire and remain employed long-term in the firm. Preliminary heterogeneity analysis suggests that the subsidy could operate as a mechanism which permits firms to learn about the potential productivity of these female workers. The third essay investigates the gender composition of management in the firm of first employment. It analyses its impact on the short- and long-term career and family-related decisions of female labor market entrants in Italy. We find that starting a labor market career in a firm with more female managers is associated with a higher probability of remaining in employment, higher job-to-job mobility, particularly towards better-paid jobs, and a higher probability of returning to work after maternity leave. Our analysis suggests that having women in executive positions could contribute to women's labor market success.

Impact Statement

This thesis examines how taxes levied on firms and firms' management characteristics affect workers' careers. The focus is on taxes, specifically profit (Essay 1) and payroll taxation (Essay 2), and on the gender of management (Essay 3). In the first essay, we find that firms respond to the increase in the cost of capital due to a profit tax hike by decreasing their employment and wages. These effects are more pronounced among larger and higher-paying firms, causing a reduction in workers' opportunities to move from less to more productive jobs. This, in turn, reduces worker wage growth over and above the direct (within-firm) wage decline. These findings contribute to the literature on profit taxation by providing a more comprehensive picture of the heterogeneous effect of a profit tax hike across firm types and workers. More generally, our findings suggest that, for the type of production technologies used by firms during our 1999 to 2014 period of analysis, capital and labor were not sufficiently substitutable for firms to replace capital with workers, when the cost of capital increases. However, the latest technologies, such as artificial intelligence, are ever more successful at conducting tasks that previously only humans could perform, pointing to increased substitutability between capital and labor. If, indeed, capital and labor are becoming more and more substitutable, an increase in profit tax rates could potentially increase employment and wages—contrary to what we find in our period of analysis.

The second essay examines hiring subsidies—a temporary cut to payroll taxation for new hires—introduced in Italy in 2013. To our knowledge, this study is the first in the literature to investigate gender-specific hiring subsidies aimed at reducing gender differences in employment. We find that firms that adopt the subsidy hire women with lengthy labor market interruptions and who are mothers—the group with the lowest employment rate in Italy. These women remain employed in the firm long-term, after the expiry of the subsidy. This is an important finding as lengthy labor market interruptions after childbearing can lead to a loss of skills, making re-entry

into the labor market difficult for mothers, with many dropping out of the labor force. These findings suggest that hiring subsidies could be an effective policy instrument for the re-entry of women into the labor market.

The third essay investigates the gender composition of management in the firm of first employment. We find that starting a labor market career in a firm with more female managers is associated with a higher probability of remaining in employment, higher job-to-job mobility, particularly towards better-paid jobs, and a higher probability of returning to work after maternity leave. We contribute to the literature by studying the effect of female managers in the firm of first employment and the impact of these female managers on different employment status, job-to-job mobility and maternity. Our analysis speaks to the current policy discussions regarding gender quotas and suggests that having women in executive positions could contribute to women's labor market success.

Acknowledgements

Funding Arrangements

The writing of this thesis was supported by funding from the Fondazione Luigi Einaudi (Torino) and grants from the Centre of Research and Analysis of Migration and the Economic and Social Research Council—for all of which I am deeply grateful.

Joint Work

Chapter 2 is based on joint work with Thomas Cornelissen (University of Essex), Christian Dustmann (University College London) and Uta Schönberg (University College London). Chapter 3 is based on joint work with Lorenzo Incoronato (University College London) and Anna Raute (Queen Mary University of London). Chapter 4 is based on joint work with Elena Ashtari Tafti (University College London) and Tatiana Sutovtseva (New York University Abu Dhabi).

Personal Acknowledgements

I would like to thank my supervisors and co-authors for the stimulating and exciting research discussions. I thank the friends–Anna, Carlo, Federica, Julen, Michele, Silvia–with whom I have shared the PhD journey, and that made it lighter and more exciting. I was lucky to share the office with the CReAM team–Barbara, Alice, Ines, Tanya, Sebastian–I couldn't have asked for a better group. A shout-out to my Milan friends–Carol, Eden, Giacomo, Marghe–who have been part of my life since middle school. I would like to thank my father for his continued support during my studies and the stimulating discussions about the current economic and political issues that the world is facing, that are always food for thought. Finally, I am thanking Riccardo, who has been with me along this PhD and life journey. He has been there during the best and the worst moments and taught me to always live life as an adventure.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

Firms are an important determinant of workers' lives and careers, as on average workers spend around 40 years of their life as employees of a firm[1]. The aim of this thesis is to examine the role of taxes levied on firms and firm's characteristics in affecting workers' labor market careers. The specific focus is on profit (Essay 1) and payroll taxation (Essay 2), and on the gender of management (Essay 3), and how they affect firm's employment, wages and hiring decisions and, subsequently, workers' career.

The first essay, "The Effects of Business Taxation on Local Labor Markets, Firms, and Workers", leverages variation in business tax rates, that is, profit taxes, in Germany over time and across local areas to investigate the heterogeneous effect of profit taxation on local labor markets, firms and workers. Opponents of business tax increases argue that they hurt the economy, especially if tax increases are local, with firms choosing to move to low-tax locations and workers' wages declining in response to a business tax increase (Kotlikoff and Summers, 1987; Suarez-Serrato and Zidar, 2016). In contrast, proponents of business tax increases argue that, besides raising tax revenues, they may create jobs, as a business tax hike raises the effective cost of capital (Stiglitz and Rosengard 2015, section 23), which may induce firms to replace capital with labor (Acemoglu, Manera, and Restrepo, 2020)[2].

In this study, we provide new evidence on the extent to which jobs are destroyed, or created, following a local tax hike, and investigate response heterogeneity across firm types and workers, by analyzing whether a

[1] Most workers in employment are employed by a firm. For example, In the European Union (27 countries), around 80 percent of people aged 18 or older are in employment, 66 percent of these as employees in a firm (Eurostat Database, 2018).

[2] Acemoglu, Manera, and Restrepo (2020) argue that the US tax code has favoured capital over labor, by taxing labor more than capital, and has led US firms to invest in automation at the expenses of labor.

business tax increase leads firms to exit or to reduce employment and its impacts on workers' wages and job mobility. We find that local employment and wage decline following a business tax hike. While about half of the local employment decline can be attributed to increased establishment exit, employment and wages also decline within surviving establishments. Our findings further highlight that establishment exit is primarily driven by smaller and lower-paying establishments, while within-firm employment and wages decline more in large and higher-paying establishments. Our results suggest that a local business tax hike affects worker wage growth over and above the direct (within-establishment) wage decline, through reductions in churning and in workers' opportunities to move from less productive to more productive jobs. This effect is especially pronounced in the case of workers who were employed in a low-paying firm before the tax hike and who would have had the most to gain from moving to a better-paying job. To guide the empirical analysis, we develop a model that includes heterogeneous firms, a monopolistic product market, and a monopsonistic labor market.

The second essay, "Hiring Subsidies and Female Employment", focuses on a hiring subsidy, namely a temporary cut to payroll taxation, introduced by the Italian government in 2013 for hiring women with employment interruptions. While such policies are often discussed as a policy lever to reduce labour costs for firms and have been applied in both the U.S. and Europe to increase employment, particularly among low-wage workers (Neumark and Grijalva, 2017; Cahuc, Carcillo and Le Barbanchon, 2019), this study is the first to investigate gender-specific hiring subsidies aimed at reducing gender differences in employment.

We investigate the effect of the hiring subsidy on firms' hiring decisions and the career of female workers with lengthy labor market interruptions. For the analysis, we exploit the Italian employer-employee administrative data (INPS) and combine a matched difference-in-differences design with an event-study approach. Our findings show that the firms that adopt the hiring subsidy increase their hiring of women with lengthy employment interruptions and who are mothers in the medium run. Albeit with lengthy employment interruptions, these new female hires seem more

positively selected than the average firm's hire on other observables, with higher education and a higher wage earned in their previous job. Our preliminary analysis shows that these female hires seem more likely to remain employed in the firm in the medium run and have an increased probability of obtaining secure employment compared to the average firm's hire. The hiring subsidy–by making the subsidised workers cheaper compared to the other workers in the labor market–could allow firms that use it to learn about the potential productivity of these workers. We provide preliminary evidence of this firm-learning mechanism by investigating whether the quality of the subsidised workers hired in the first year affects future hiring patterns. Our findings suggest that firms that hire a more productive female worker under the subsidy in the first year hire more subsidised workers in subsequent years.

The third essay, "Gender Composition of Management and Employment of Young Female Workers", focuses on the management characteristics of the firm of first employment, specifically on the gender of management, and investigates its effect on women's future labor market trajectories. The literature on the relationship between the gender of the firm's managers and gender-specific career outcomes is scant. This is primarily due to data availability, as managers' role is often identified through survey data or only in specific settings (Gagliarducci and Paserman, 2015; Lucifora and Vigani, 2016). Additionally, the studies that use high-quality administrative data sets, like ours, focus on contemporaneous effects and analyse the gender of management on female workers' wages (as in Cardoso and Winter-Ebmer, 2010; Flabbi, Macis, Moro, and Schivardi, 2019).

We contribute to the literature in two critical ways. First, we study the effect of female managers in the firm of first employment on future female labor market outcomes. We thus move from the contemporaneous firm effect and study the long-term effect of initial management. Second, we study the impact of female managers on different female labor market outcomes, such as employment status, job-to-job mobility and maternity. Our empirical design exploits the Italian employer-employee administrative data (INPS). It

leverages within-firm variation in the gender composition of its management across entry cohorts, as well as controlling for firms' time-varying characteristics, such as average wages, employment and wage growth. Our findings suggest that starting a labor market career in a firm with more female managers is associated with a higher probability of remaining in employment, higher job-to-job mobility, particularly towards better-paid jobs, and a higher likelihood of returning to work after maternity leave. Our analysis suggests that having women in managerial positions could contribute to women's labor market success.

Chapter 2

The Effects of Business Taxation on Local Labor Markets, Firms, and Workers

2.1 Introduction

While the average rate of business taxes—that is, taxes on firm profits—among OECD countries was close to 50 percent in the early 1980s, it fell to 30 percent around the turn of the century and below 25 percent in 2015 (OECD Tax Database). More recently, increases in business tax rates have been proposed as a means of raising tax revenues.[3] Opponents of business tax increases argue that they destroy jobs, especially if tax increases are local, as capital, goods, and workers are mobile across areas within a country, and firms may choose to move to low-tax locations. Moreover, even though the direct effect of a business tax increase is a decline in firm profits, workers may bear some of the incidence of a business tax hike as their wages may decline in response (Kotlikoff and Summers, 1987; Suarez-Serrato and Zidar, 2016). In contrast, proponents of business tax increases argue that, besides raising tax revenues, they may create jobs, as a business tax hike raises the effective

[3] While in 2018, under the Trump administration, the tax rate on profits of incorporated firms was reduced from 35 percent to an all-time low of 21 percent, President Joe Biden's latest budget proposal in 2022 seeks to raise the corporate tax rate from 21 percent to 28 percent (U.S. Government Publishing Office, 2022). In its March 2021 budget, the UK government announced an increase in its main rate of corporation tax from 19 percent to 25 percent from April 2023; however, there are now calls for a cut in the UK corporate tax rate to 15 percent instead (Financial Times, 2022).

cost of capital[4] (Stiglitz and Rosengard 2015, section 23), which may induce firms to replace capital with labor (Acemoglu, Manera, and Restrepo, 2020[5]).

In this paper, we provide new evidence not only on the extent to which jobs are destroyed or created following a local tax hike, but also on what type of jobs are particularly affected, and to what extent the quality of jobs changes in response to a local business tax hike. Moreover, we investigate response heterogeneity across firm types and workers, analyzing whether a business tax increase leads firms to exit or reduce employment, and what effect that has on the composition of worker-weighted firm quality. We further analyse whether a business tax increase impacts workers' wages, preventing them to move from less productive to more productive jobs. Based on administrative longitudinal data for Germany, our analysis provides a most comprehensive evaluation of how local labor markets, firms and workers adjust to a local business tax increase.

To guide the empirical analysis, we develop a model that includes heterogeneous firms, a monopolistic product market, and a monopsonistic labor market. While the model unambiguously predicts an increase in the number of exits of the least productive firms—which in equilibrium are small and pay lower wages—in response to a local business tax hike[6], the employment response of surviving firms is unclear. On the one hand, firms would like to replace capital with labor, as the effective cost of capital increases following the local business tax hike (the substitution effect). On the other hand, the increase in the effective cost of capital increases firms' cost of production and induces them to scale down their production and reduce employment (the scale effect). These heterogeneous employment and exit responses to a local business tax increase will alter the quality of jobs in the

[4] A business tax increase raises the effective cost of capital when firms can only deduct a share of their capital costs from profits. This is the case for the U.S. and EU legislation, where firms can deduct from the tax base a share (or all) of the cost of capital made via debt but cannot deduct the cost of capital made by direct investment.

[5] Acemoglu, Manera, and Restrepo (2020) argue that the US tax code has favored capital over labor, by taxing labor more than capital, and has led US firms to invest in automation at the expenses of labor (and employment)

[6] That small firms exit in greater number is a consequence of firm profits increasing in firm productivity. As a business tax hike reduces firm profits, the least productive firms are no longer able to recoup their fixed costs of production and, hence, exit the market.

economy. The direction of this effect is, however, unclear. Whereas the exit of less productive, smaller, and lower paying firms improves the quality of jobs, intensive margin responses may be larger for more productive, larger, and higher-paying firms, lowering the quality of jobs in the local economy. Relatedly, a local business tax hike may affect individual wage growth directly, in the case of workers who remain employed with their firm, and indirectly, as movements to a better-paying job may be disrupted.

To study the effects of business taxation on the local economy, firms, and workers empirically, we build on Fuest, Peichl and Siegloch (2018) and leverage variation in business tax rates in Germany over time and across local areas (municipalities), a highly disaggregated geographical level.[7] While municipalities in Germany can select the local business tax rate, they have no control over the tax base definition and which firms are liable. The local business tax applies to both incorporated firms and non-incorporated firms. In 2014, the mean local business tax rate in Germany was 13.3 percent. Business tax rates vary widely across municipalities and ranged between 7 and 31.5 percent in the municipalities with the lowest and highest tax rates in 2014. An important advantage of the German setting is that there are many persistent local business tax changes (4,815 in our sample), which allow us**,** in combination with high-quality data, to paint a more detailed picture of the economic consequences of business taxation than is obtained in the pre-existing literature.

Our empirical analysis exploits administrative linked employer-employee data for the years 1999 to 2014 provided by the Institute for Employment Research in Nuremberg (IAB), comprising all establishments and workers covered by the social security system. These data allow us to investigate both establishment entry and exit as well as the within-establishment wage and employment changes that follow a local business tax change, and how these vary across high and low productive establishments. The data further allow us to follow workers over time, as well as across municipalities and establishments, enabling us to analyze the effects of local

[7] In 2018, there were more than eleven thousand municipalities in Germany.

business taxation on individual mobility and individual within- and between-establishment wage growth.

We adopt an event study approach in our empirical design that enables us to trace out municipal and establishment employment and wages in treated municipalities (that is, municipalities that underwent a persistent local business tax change of at least 0.5 percentage points) and control municipalities (those municipalities that did not undergo a local business tax change over our sample period). The event study analysis shows that treated and control municipalities experienced similar trends in local employment, wages, and number of establishments prior to the local business tax change but diverged afterwards—lending support to a causal interpretation of our findings. Additional robustness checks performed further rule out that estimated effects are due to local business cycles or differences in the local industry structure and industry shocks.

We find that local employment (established as the number of workers employed in a municipality, where part-time employees receive a lower weight) declines following a business tax hike. Indeed, a one percentage point (percent) increase in the local business tax rate lowers local employment by 1.17 (0.17) percent. Similarly, and mirroring the findings of Fuest, Peichl and Siegloch (2018), a one percentage point (percent) increase in the local business tax rate lowers local wages by 0.52 (0.077) percent. While about half of the local employment decline can be attributed to increased establishment exit, employment and wages also decline within surviving establishments. Viewed through the lens of our model, these findings imply that the elasticity of substitution between goods (the key determinant of the scale effect) exceeds the elasticity of substitution between capital and labor (the key determinant of the substitution effect).

Our findings further highlight that establishment exit is primarily driven by smaller and lower-paying establishments, measured according to their establishment fixed effect in an AKM-style regression[8]. In contrast, among surviving establishments, higher-paying establishments sustain larger

[8] The computation of establishment fixed effect is explained in detail in Section 2.4

employment and wage declines, which is consistent with the predictions of the theoretical model.

These tax-induced heterogeneous employment and exit responses across establishments alter the composition of establishments in the municipality. We find that a local business tax hike leads to a small improvement in worker-weighted establishment quality, measured as the (worker-weighted) average wage premium that establishments pay to their workers **(**this is, their AKM establishment fixed effect estimated before the tax hike). Compositional effects stemming from the exit of small, low-paying establishments, therefore, dominate compositional effects from surviving establishments.

Workers do not benefit from the improvement in establishment quality. Rather, our findings suggest that a local business tax hike affects worker wage growth over and above the direct (within-establishment) wage decline, through reductions in churning**,** which reduces workers' opportunities to move from less productive to more productive jobs. On the one hand, the number of job seekers increases following the local tax hike as establishments exit the market. On the other hand, surviving establishments, particularly those that pay higher wage premiums to their workers, reduce hiring. Job change has been shown in the literature to be a major channel of wage growth (see Topel and Ward 1992; Lazear and Spletzler 2012), and it explains a substantial part of the reduced worker wage growth as a response to a business tax increase. This effect is especially pronounced in the case of workers who were employed in a low-paying firm before the tax hike and would have had the most to gain from moving to a better-paying job.

Our paper makes several contributions to the literatur**e** on corporate taxation. An earlier literature on business taxation leverages corporate tax changes across countries (or states) to study the impact of corporate taxation on workers' wages (as in Felix, 2007; Arulampalam, Devereux and Maffini, 2012[9]; Clausing, 2013; Azémar and Hubbard, 2015), firms' location choices or

[9] Arulampalam, Devereux and Maffini (2012) draw on firm-level data from nine European countries and leverage variation in tax liability across firms and over time to study the direct effect (holding labor productivity constant) of a corporate tax increase on workers' wages. In keeping with our findings, they

investment, often with a focus on multinationals (as in Coughlin, Terza, and Arromdee, 1991; Hines, 1996; Devereux and Griffith, 1998). While these studies have provided useful insights, they rely on only a handful of cross-country tax changes and require the application of strong assumptions for a causal interpretation. The German setting, with its large number of local tax changes, combined with our large-scale administrative linked employer-employee data, allows us to estimate the causal effects of business taxation, applying weaker assumptions. It also permits us to visually assess the plausibility of the assumptions through our event study design, which allows us to assess the causality, by looking at the outcome evolutions before the tax changes.

The German setting has been used to study the impact of business taxation on multinationals investment (Becker, Egger and Merlo, 2012), on workers' wages (Fuest, Peichl and Siegloch, 2018) and on research and development spending and innovation (Lichter, Löffler, Isphording, Nguyen, Pöge and Siegloch, 2021). Other papers have exploited changes in corporate tax rates across states in the United States to study the economic effects of business taxation. Suarez-Serrato and Zidar (2016) first estimated reduced form effects of state corporate tax cuts on wages, population, and the number of establishments, focusing on ten-year changes; they then calculated the incidence of the tax increase on workers, business owners, and landowners based on a structural spatial equilibrium model[10]. Giroud and Rauh (2019) focused their study on wage and employment adjustments to state corporate tax cuts within firms, examining multiple establishments across states.

We confirm the findings of these papers: that an increase in business tax rates decreases employment, and that firms therefore do not replace capital with workers when capital becomes more expensive. In terms of magnitude, our estimated within-establishment employment effects are larger than those found by Giroud and Rauh (2019), whereas our estimated local

find that firms lower wages in response to an increase in their tax liability, with a long-run elasticity of the wage bill with respect to taxation at 0.093.

[10] Additionally, a related literature examines the determinants of firms' location choices, as in Gabe and Bell (2004); Rathelot and Sillard (2008); Duranton, Gobillon, and Overman (2011).

employment effects—which capture establishment entry and exit in addition to within-establishment employment adjustments—are considerably smaller than those found by Suarez Serrato and Zidar (2016) (see Section 2.6.1.3 for details).

Our analysis goes beyond these papers in three important ways. First, we study, both theoretically and empirically, to what extent and how local employment adjusts to a local business tax hike, by investigating both firm entry and exit, as well as workers' hiring and firing. Second, we study, both theoretically and empirically, which types of firms are affected by the tax hike, high vs low productive, small vs large. Third, we highlight that the heterogeneous responses to a local tax hike across firms alter the composition of jobs in the local economy, thereby affecting workers' mobility decisions and gains from moving across firms.[11]

Our paper also relates to the growing literature on monopsonistic labor markets. We highlight that models of monopsonistic competition are useful not only to gain an understanding of wage differentials across firms (as in Card, Cardoso, Heining and Kline, 2018), or the employment effects of minimum wages (in Bhaskar and Tho, 1999; Bhaskar, Manning and Tho, 2002; Dustmann, Lindner, Schönberg, Umkehrer and vom Berge, 2022), but also to understand the heterogeneous responses across firms to local business tax hikes. Our empirical findings show that both employment and wages decline more in higher-paying than in lower-paying establishments, which is incompatible with a competitive labor market, where all firms pay the same wage rate. These empirical findings can further be used to isolate the labor supply elasticity to the firm from the labor supply elasticity to the local economy. We find a labor supply elasticity to the firm of between 2 and 4, implying that firms pay wages that are about one-fifth to one-third lower than workers' marginal revenue products.

[11] A related and complementary literature studies the effect of tax policies that directly affect firms' capital costs on employment, such as bonus depreciation (Garrett, Ohrn and Suarez Serrato, 2020; Curtis, Garrett, Ohrn, Roberts and Suárez Serrato, 2022; Tuzel and Zhang, 2021) or place-based policies that subsidise investment costs of typically manufacturing firms (as in, Siegloch, Wehrhöfer and Etzel, 2021; Lerche, 2019; Criscuolo, Martin, Overman and Van Reenen, 2019). While these papers find that reductions in firms' capital costs increase employment, in keeping with our findings, they do not investigate heterogeneous responses across firms and their consequences for the composition of jobs and individual wage growth.

The paper is organized as follows. In Section 2.2, we outline the institutional setting. To guide the empirical analysis, we set up a theoretical model in Section 2.3. We describe the data and sample selection in Section 2.4 and the empirical strategy in Section 2.5. We present results in Section 2.6 and conclude with a discussion of our findings in Section 2.7.

2.2 Business Taxation in Germany

Business profits in Germany are taxed in three ways. These consist of the federal corporate income tax, the federal personal income tax, and the local business tax.

Corporate and Personal Income Taxation. In Germany, the corporate income tax applies to incorporated firms only, constituting roughly 23% of firms and employing 58.4% of the workforce.[12] Operating profits of non-incorporated firms are subject to the progressive personal income tax. Corporate and personal income tax rates are set by the federal government and do not vary across municipalities. In 2014, the last year of our empirical analysis, the corporate tax rate was 15%, the same as the entry-level personal income tax. The top marginal personal income tax rate was 40%.[13]

Local Business Taxation. In contrast to the corporate and personal income tax, the local business tax applies to most firms with commercial activity, regardless of whether they are incorporated or non-incorporated. However, some firms are exempt from the local business tax, depending on their legal form and industry affiliation. These non-liable firms include most

[12] The business register of the German statistical office Destatis reports the existence of 764,904 incorporated firms out of a total of 3.37 million firm. See https://www.destatis.de/DE/Themen/Branchen-Unternehmen/Unternehmen/Unternehmensregister/Tabellen/unternehmen-rechtsformen-wz08.html [accessed on 11 July 2022]. For the employment shares of incorporated firms see p. 3 in https://www.ifm-bonn.org/fileadmin/data/redaktion/statistik/mittelstand_im_einzelnen/dokumente/SVB_2015-2019_D_RF.pdf [accessed on 11 July 2022].

[13] The corporate tax rate was reduced from 45 percent to 40 percent in 1999 and to 15 percent in 2008. The entry-level personal income tax rate was reduced from 25.9 percent to 22.9 percent in 2000 and to 15 percent in 2005. The top marginal tax rate was reduced from 53 percent to 51 percent in 2000 and to 42 percent in 2005.

firms in agriculture and the public sector and those of some practitioners in certain professions, such as accountancy, law, journalism, medicine, and art. Our empirical analysis excludes these non-liable firms and focuses on liable firms.

The business tax rate varies at the municipality level, Germany's smallest administrative unit—in 2018, there were 11,012 municipalities in Germany. The business tax rate in a municipality, τ^m (where the superscript *m* indexes municipalities), consists of the basic tax rate set by the federal government, t_t^{fed}, and a collection rate set by the municipality, c_{mt}. Prior to 2008, firms were allowed to deduct the local business tax rate from its own base. The effective business tax rate, which we exploit in our empirical analysis, equals

$$\tau_{mt}^{eff} = \begin{cases} \dfrac{t_t^{fed} * c_{mt}}{1 + t_t^{fed} * c_{mt}} & until\ 2007 \\ \\ t_t^{fed} * c_{mt} & from\ 2008 \end{cases} \qquad (1)$$

The federal government reduced the basic tax rate t_t^{fed} from 5% to 3.5% in 2008. In 2014, the collection rate c_{mt} varied between 2 and 9 so that the local business tax rate τ_{mt}^{eff} ranged between 7% and 31.5% across municipalities. Weighted by the number of employees in a municipality per year in the social security data, the mean business tax rate in 2014 was 13.31%, with a standard deviation of 1.62%. In the same year, the 10th, 50th, and 90th percentile of the weighted effective business tax rate distribution were 11%, 13%, and 17%, respectively.

Municipality councils vote on the collection rate c_{mt} on a yearly basis, in sessions held during the last three months of the year. While the collection rate is at the municipality's discretion, municipalities have no control over the tax base, the costs deducted from the tax base, or the firms liable to the local business tax.

The tax base of the local business tax is operating profits. While firms are allowed to deduct about 75 percent of their interest costs of debt financing from their tax base, costs of equity financing, such as retained profits, are not

deductible.[14] The typical firm in Germany finances 36 percent of its investments through debt, and 64 percent through equity and internal financing (European Investment Bank, 2019; Statistische Bundesamt, 2011), which suggests that they can deduct about 27 percent of their total capital costs from the tax base.

For firms with multiple establishments located across several municipalities, the firm's taxable profit is allocated across municipalities according to the municipality's wage bill share.[15] This rule limits firms' opportunities to shift profits to municipalities with lower collection rates, as such profit shifting is possible only if firms move workers across establishments from municipalities with high to municipalities with low collection rates.

Municipality Revenues and Expenditures. In terms of tax revenues generated, local business taxes are considerably more important than corporate taxes, making up approximately 44 percent of total tax revenues from profit taxation.[16]

Business taxes also form an important component of municipal revenues. In 2014, 37.2 percent of municipal revenues were from taxes, while the remainder were from upper-level general or specific transfers over which municipalities have little control, or other income sources such as fees for local public services. Of the municipality's revenues from taxes, revenues from the local business tax are the main source, accounting for 79 percent of municipality tax revenues (own calculation based on Destatis, 2022a), with revenues from property taxes a distant second.

[14] Sections §§ 7-22 of the German corporate tax legislation (KStg), sections §§ 15-17 of the income tax legislation (EStG) and sections §§ 8-9 of the business tax legislation (GewStG).

[15] To illustrate, suppose that a firm has three establishments in three different municipalities (indexed by subscripts 1, 2 and 3) with different collection rates. Denote the firm's total profit and total wage bill by $\pi = \pi_1 + \pi_2 + \pi_3$ and $w = w_1 + w_2 + w_3$. According to the apportionment formula, revenues for each municipality are equal to $m\ (w_m/w)\pi$.

[16] In 2021 tax revenue from the local business tax was €56bn, compared to €33bn from corporation tax—see Table ZR.1 in Destatis (2022a). The same source reports €60bn of personal income tax receipts from income sources other than wages, of which an estimated 65 percent is from business activity (own calculations on the income sources for income tax based on Table A5 in Destatis (2022b). This suggests a tax share of 56/(56+33+.65*60)=.44.

Municipalities are responsible for paying social benefits to eligible citizens, the municipality's main expenditure. They have little discretion over the amount of the social benefit or eligibility criteria. Municipalities are also responsible for the provision of local public services, such as rubbish disposal, water and sewage, the provision and maintenance of school and nursery buildings, public parks, sports facilities, and the like.

2.3 Theoretical Framework

In this section, we outline a model with firm heterogeneity, monopolistic competition in the product market and monopsonistic competition in the labor market to analyze how a local business tax affects firms' labor and capital choices, and which type of firms responds more strongly to an increase in the business tax rate. Our model shows that labor and capital adjustments depend on the share of capital costs firms are allowed to deduct from the tax base, along with the firm's capital intensity, the elasticity of substitution between capital and labor, and the elasticity of substitution between goods. Below, we discuss the main components and implications of the model–detailed derivations can be found in Appendix 2.A.

2.3.1 Set-Up

Consider a local economy m with N^m homogenous workers and a set of J^m heterogeneous firms that charges a local business tax of τ^m. We take the initial location choices of workers and firms as given and focus on the effects of an increase in the local business tax rate on workers' decisions to remain employed in the local economy, on firms' decisions to exit the local economy, and on labor and capital choices of surviving firms. We assume that the local economy is small compared to the rest of the economy and that firms sell their products not only in the local economy but in the entire national market. Consequently, the impact of a business tax increase in the local economy on the wages of other local labor markets and aggregate output and prices is negligible.

Firms' Profits. After-tax profits of firm *j* in local economy *m* are given by

$$\Pi_j = p_j y_j(1-\tau^m) - w_j l_j(1-\tau^m) - r k_j(1-\beta\tau^m) - C,$$

where p_j denotes the product price that firm j charges; y_j, l_j, and k_j denote the firm's output, labor and capital choices; τ^m is the local business tax rate; w_j is the wage rate that the firm pays to all its workers; *r* is the interest rate, *C* denotes the fixed cost of production, and β $(0 < \beta < 1)$ is a tax base parameter representing the share of the capital costs that can be deducted from the tax base. In the German context, a rough estimate for β is 27 percent (see Section 2.2).

It should be noted that if firms are allowed to deduct their entire capital costs from profits–that is, the tax base, (i.e., $\beta = 1$)–a business tax rate increase affects firms' exit decisions (as their profits decrease) but not their capital and labor choices. In contrast, if capital costs are only partially deductible from the tax base, an increase in the business tax rate increases the cost of capital and hence affects labor and capital choices also of surviving firms. Let $R = r\frac{1-\beta\tau^m}{1-\tau^m}$ denote the effective cost of capital. The effective cost of capital increases as the interest rate and the local business tax rate increases and decreases as the share of capital costs that firms can deduct from their tax base increases.

We assume that the interest rate r is determined in national or worldwide capital markets and is thus treated as exogenous. This assumption corresponds to an infinitely elastic capital supply to the local economy.

Production Function. Suppose that firms use capital *k* and labor *l* to produce output according to a CES production function with constant returns to scale:

$$y_j = F(k_j, l_j) = [l_j^{\nu} + (\theta_j k_j)^{\nu}]^{\frac{1}{\nu}}$$

Here, $\frac{1}{1-\nu} \coloneqq \sigma$ is the firm-level elasticity of substitution between labor and capital and θ_j is a firm-specific capital-augmenting productivity shifter. If $\sigma < 1$, capital and labor are gross complements. If, in contrast, $\sigma > 1$, capital and labor are gross substitutes (Acemoglu, 2002).

Monopolistic Product Market. Firms produce differentiated varieties, which are then demanded by consumers. The preferences of a representative consumer are given by a CES utility function over the goods (indexed here by *j* as we assume that each firm produces a different variety):

$$U = \left(\sum_{k \epsilon J} y_k^{\rho} \right)^{\frac{1}{\rho}},$$

where $\frac{1}{1-\rho} \coloneqq \eta$ is the elasticity of substitution between any two goods and J denotes the set of firms with which the firm competes in the product market (which is, as goods are traded in national or international markets, larger than the set of firms operating in the local market). We assume that goods are substitutes (i.e., $\eta > 1$). As was originally shown by Dixit and Stiglitz (1977) for the case of a continuum of goods, and assuming that there are many firms operating in the market, the optimal consumption for a good produced by firm *j*—that is, the consumer demand for firm j—can be approximated as:

$$y_j = Y \left(\frac{p_j}{P} \right)^{-\eta}$$

where $P = \left(\sum_{k \epsilon J} p_k^{1-\eta} \right)^{\frac{1}{1-\eta}}$ is the aggregate price index, Y is aggregate output and p_k is the price charged by firm *k*. The higher value is the elasticity of substitution between goods, the more competitive is the product market and the lower is the mark-up that a firm is able to charge.

Monopsonistic Local Labor Markets. There are N^m workers in the local economy. In a first step, workers decide whether to work in the local economy or work elsewhere (or to not work at all). In a second step, workers who have chosen to work in the local economy decide for which firm to work. Workers do not only derive utility from the wage a particular firm in the local economy pays, but also from the work environment that the firm provides, capturing, for example, commuting time, how well the worker gets along with her co-workers or boss, or his or her preferences for the work schedule the firm provides. Workers value these non-pecuniary job characteristics differently, and the indirect utility of worker i working at firm j is

$$u_{ij} = b\ log\ w_j\ \ + e_{ij}$$

where $\log w_j$ is the (log) wage that firm *j* pays to all its workers, e_{ij} denotes her idiosyncratic preferences for working at the firm, and b can be thought of the labor supply elasticity to the firm (conditional on labor supply to the local economy). Conditional on working in the local economy, and assuming that e_{ij} are independent draws from a type I Extreme Value distribution with scale parameter 1, the probability that a worker chooses to work for firm j equals:

$$P\left(\arg\max_{k\,\epsilon J^m}\{\mathrm{u}_{ij}\} = j\right) = \frac{\exp\{b\log w_j\}}{\sum_{k\epsilon J^m}\exp\{b\log w_k\}} = \frac{w_j^b}{W^m} \quad (2)$$

where J^m denotes the number of firms operating in the local market *m*, and $W^m := \sum_{k\epsilon J^m}\exp\{b\log w_k\}$ denotes the market wage in the local economy. Assume that workers derive utility

$$u_{iO} = logO + v_{iO}$$

from working in another local labor market or from non-employment, where we can think of *O* as workers' outside option and v_{iO} as capturing workers' preferences for that outside option. Assuming that v_{iO} is drawn from an extreme value distribution with scale parameter λ, workers' labor supply to the local economy equals

$$L^{Sm} = N^m \frac{exp\ (log\ W^m\ /\lambda)}{exp\ (log\ O\ /\lambda)\ + exp\ (log\ W^m\ /\lambda)}$$

where N^m denotes the pool of workers who could potentially work in the local economy and is exogenously given.

Equilibrium conditions. Firms choose capital and labor such that the marginal revenue of products equal the marginal costs of capital and labor, taking local wages W^m and local labor supply L^{Sm} as given. Similarly, firms set product prices such that the marginal revenue of producing one more unit of output is equal to its marginal cost, taking aggregate prices *P* as given. In equilibrium, local wages are set such that the local labor market clears. Aggregate product prices are set at the national or international level and assumed to be exogenous in our context.

Equilibrium Properties. As it is standard in models with firm heterogeneity, firms with a productivity parameter θ_j below θ^* will choose not to operate in the market, as profits are increasing in productivity and low-productivity firms are not able to recoup their fixed costs of production.

If goods are more substitutable than input factors (i.e., if $\eta > \sigma$), firms with a larger productivity parameter θ_j will employ more workers. Since firms must increase wages to attract more workers, larger firms will also pay higher wages.

If, in addition to $\eta > \sigma$, capital and labor are gross substitutes (i.e., $\sigma > 1$), an increase in the productivity parameter θ_j will raise the marginal product of capital by more than the marginal product of labor. Consequently, firms with a larger productivity parameter θ_j will be more capital intensive and will have a higher share of capital efficiency units s_j $\left(s_j = \frac{(\theta_j k_j)^\nu}{(\theta_j k_j)^\nu + l_j^\nu}\right)$. Notably, larger firms do not only pay higher wages, but are also more capital intensive, in keeping with the empirical evidence (Abowd et al., 1999; Arai, 2003; Oi and Idson, 1999).[17] We summarize these results in the following proposition (see Appendix 2.A.1 for details).

Proposition 1: Effects of θ on firm employment, wages, capital intensity, and profits.

a) $d\Pi_j/d\theta_j > 0$. Only firms with $\theta_j > \theta^*$ will operate in the market, where $\Pi(\theta^*) = 0$.

b) If $\eta > \sigma$, it follows that $\frac{dlog\, l_j}{d\theta_j} > 0$ and $\frac{dlog\ w_j}{d\theta_j} > 0$.

[17] While estimates for the elasticity of substitution between labor and capital are still in dispute, several studies uncover an *aggregate* elasticity point to estimates smaller than 1 (as in Gechert, Havranek, Irsova and Kolcunova, 2022). However, the elasticity of substitution *at the firm level* is likely to be higher than at the aggregate level and is rarely estimated in the literature (with the exception of Curtis et al., 2021). Empirically, larger and higher-paying firms are on average more capital intensive than smaller and lower-paying firms. Our model is able to replicate this empirical regularity provided that σ>1. If σ were smaller than 1, the model would predict a higher capital intensity in larger, higher-paying firms if the productivity shifter j were labor rather than capital-augmenting.

c) If $\eta > \sigma$ and $\sigma > 1$, it follows that firms with higher productivity parameter θ_j will be more capital intensive (i.e., $\frac{dlogk_j}{d\theta_j} - \frac{dlogl_j}{d\theta_j} > 0$) and have a higher share of capital efficiency units (i.e., $\frac{ds_j}{d\theta_j > 0,}$ with $s_j = \frac{(\theta_j k_j)^{\nu}}{(\theta_j k_j)^{\nu} + l_j^{\nu}}$).

2.3.2 The Effects of An Increase in the Business Tax Rate on Firms and the Local Labor Market

Now, consider a municipality that increases its business tax rate τ^m. How does this increase affect firms' entry and exit decisions, their labor demand and their wages? And how does the increase affect wages and employment in the local labor market? We assume that the local labor market is small and that goods and capital are traded nationally or internationally. Thus, an increase in the business tax rate will neither affect workers' outside option O, nor aggregate prices *P* or the interest rate.

Firm Exit. An increase in the business tax rate induces firms with the lowest values of the productivity parameter θ_j to exit the market, as the increase reduces firms' profits; and low productivity firms will no longer find it profitable to operate in the market. Since these firms are small and pay low wages (see Proposition 1), firm exit following a business tax hike will be driven by small and low-paying firms. More formally, the productivity threshold θ^* at which a firm is indifferent between operating and not operating in the market shifts up following a tax hike. We summarize this finding in the following proposition (see Appendix 2.A.2 for details):

Proposition 2: $\frac{d\theta^*}{d\tau^m} > 0$. Firms with lowest productivity parameter θ_j—that is, smaller and lower paying firms—exit the market.

Firms' Labor Demand and Wage Holding Local Wages and Labor Supply Constant. How does an increase in the business tax rate affect labor demand and wage offers of continuing firms—holding local wages and local labor supply (and, consequently, the number of firms) constant? Note that since firms can only deduct part of their capital costs from their profits, an increase in business tax rate increases the effective cost of capital, $R = r\frac{1-\beta\tau^m}{1-\tau^m}$. An increase in the total cost of capital has two opposing effects on firms' demand for labor. On the one hand, firms would like to replace capital with labor—and their ability to do so crucially depends on the elasticity of substitution between capital and labor, σ (a substitution effect). On the other hand, firms' costs increase, inducing firms to scale down their production (a scale effect). The scale effect depends on firms' ability to increase product prices in response to a business tax hike—which goes the higher the lower the elasticity of substitution between goods, η. In Appendix 2.A.3, we show that, holding local wages W^m and labor supply to the local economy L^{Sm} constant, firms will reduce their labor demand in response to an increase in the business tax rate if goods are more substitutable than the input factors, capital and labor (that is., if $\eta > \sigma$).

Proposition 3:

a) $\frac{d\,log\,l_j^*}{d\,log\,R}|_{L^{mS},W^m} = -\frac{(\eta-\sigma)bs_j^*}{\eta\left(1-s_j^*\right)+s_j^*\sigma+b}$, *where* $s_j^* = \frac{k_j^{*\nu}}{(\theta_j l_j^*)^\nu + k_j^{*\nu}}$. *Hence,* $\frac{d\,log\,l_j^*}{d\,log\,R}|_{L^{mS},W^m} < 0$ *if* $\eta > \sigma$.

b) $\frac{d\,log\,w_j^*}{d\,log\,R}|_{L^{Sm},W^m} = \frac{1}{b}\frac{d\,log\,l_j^*}{d\,log\,R}|_{L^{Sm},W^m} = -\frac{(\eta-\sigma)s_j^*}{\eta\left(1-s_j^*\right)+s_j^*\sigma+b}$. *Hence,* $\frac{d\,log\,w_j^*}{d\,log\,R}|_{L^{mS},W^m} < 0$ *if* $\eta > \sigma$.

c) $If\ \sigma > 1, d\left|\frac{d\,log\,w_j^*}{d\,log\,R}|_{L^{Sm},W^m}\right| / d\theta_j > 0$. *Hence, if* $\eta > \sigma > 1$, holding local wages and labor supply constant, larger, higher-paying and more capital-intensive firms reduce both employment and wages more than smaller, lower-paying, and less capital-intensive firms.

Since firms face an upward sloping labor supply curve and, hence, have to pay higher wages in order to attract more workers, a reduction in firms' labor demand will go hand in hand with lower firm wages. Hence, if $\eta > \sigma$, holding local wages and labor supply constant, firms will not only reduce employment but also lower wages in response to a business tax hike.

Capital-intensive firms—which are also larger and pay higher wages (as shown Proposition 1)—are generally more strongly affected by an increase in the cost of capital than less-capital intensive firms. In consequence, if $\eta > \sigma > 1$, and holding local wages and labor supply constant, larger, higher-paying and more capital-intensive firms will reduce both their employment and their wages relative to smaller, lower-paying, and less capital-intensive firms.

Employment and Wages in the Local Economy. How will local employment and local wages adjust? In Appendix 2.A.4, we show that if $\eta > \sigma$, local employment and local wages decline following an increase in the business tax rate. The decline in local employment will be larger, and the decline in local wages will be smaller, the larger the local labor supply elasticity. The exact size of the local employment and wage decline will additionally depend on the distribution of firm types (here, the distribution from which θ is drawn). The local labor supply elasticity, in turn, is larger the lower dispersed workers' preferences are over outside options (i.e., the lower λ); and the higher is workers' utility from the outside option.

Proposition 4:

a) If $\eta > \sigma$, $\frac{d \log L^{m^*}}{d \log R} \leq 0$.

b) $\frac{d \log W^{m^*}}{d \log R} = \frac{1}{\delta} \frac{d \log L^{m*}}{d \log R}$, where $\delta = \frac{1}{\lambda} \frac{\exp\left(\frac{\log O}{\lambda}\right)}{\exp\left(\frac{\log O}{\lambda}\right) + \exp\left(\frac{\log W^{m^*}}{\lambda}\right)}$ is the elasticity of labor supply to the local labor market.

Firms' Employment and Wage Choices Unconditional on Local Wages and Labor Supply. A decline in local labor supply L^{Sm} and local wages W^m

will have a feedback effect on firms' labor demand and hence their wage offers. A decrease of labor supply to the local economy lowers firm employment by shifting labor supply to the firm downward; a decrease in the local wage, in contrast, increases firm employment by shifting labor supply to the firm upward. Local employment adjustments will, therefore, generally reinforce the direct effect of an increase in the cost of capital on firms' labor demand, whereas local wage adjustments will generally dampen the direct effect—where the former effect will dominate the latter if the local labor supply elasticity is greater than 1 (see Appendix 2.A.5 for details).

Even when local employment and wage adjustments are taken into account, firms' adjustments to the business tax hike will be heterogeneous. While the smallest and lowest paying firms will exit in response to the business tax hike, larger, higher-paying, and more capital-intensive surviving firms will experience a decline in both their employment and wages relative to smaller, lower-paying, and more labor-intensive surviving firms (if $\eta > \sigma > 1$).

We would like to emphasize here that heterogeneous employment responses across firms coupled with heterogeneous wage responses that go in the same direction indicate a monopsonistic labor market and are incompatible with a perfectly competitive labor market. These heterogeneous responses across firms can also be used to back out the labor supply elasticity to the firm (see Section 2.6.3.1).

2.3.3 Remarks

Local vs national business tax changes. It is important to bear in mind that we consider here a local change in the business tax rate as opposed to a national (or international) change. The employment effects of a national tax change are likely to be smaller than those of a local tax change, for at least two reasons. First, a national business tax hike would likely increase aggregate product prices, which, in turn, would dampen firms' scale effect and, hence, their decline in employment. Second, workers are likely to be more mobile across local labor markets than across countries. The labor supply elasticity is, therefore, likely to be higher at the local than the national level.

In fact, in frictionless, perfectly competitive labor and product markets where wages equalize across local markets and do not decline in response to a business tax hike (where $\delta \to \infty$), and goods are perfect substitutes (where $\eta \to \infty$), business tax rates must equalize across local markets as all economic activity would disappear from those areas that charge higher tax rates, that is, where $\frac{dlogL^m}{dlogR}$ and $\frac{dlogK^m}{dlogR} \to -\infty \; if \; \eta, \delta, b \to \infty$ (see also Appendix 2.A.6). Thus, the widespread variation in business tax rates across municipalities in Germany points toward substantial frictions in local labor or product markets.

Compositional Firm Changes and Reduction in Workers' Mobility. A key prediction drawn from the model is that firms' employment and wage adjustments following a business tax hike are heterogenous, with the smallest and lowest-paying firms exiting the market and larger, higher-paying, and more capital-intensive surviving firms reducing employment and wages more than smaller, lower-paying, and less capital-intensive surviving firms (if $\eta > \sigma$). While the exit of the lowest-paying firms improves the composition of firms in the local economy, larger employment reductions among high-paying surviving firms would worsen the composition. We investigate such compositional changes in Section 2.6.3.3.

A disproportionate decline in labor demand in larger, higher-paying firms, in combination with an increased number of job seekers due to increased establishment exit, may also have consequences for the wage growth that workers experience after a business tax hike. As pointed out by Topel and Ward (1992), one third of wage growth stems from job mobility. Thus, a business tax hike may decrease the opportunity for transitions to better jobs, by making movements from smaller, lower-paying firms to larger, higher paying firms less likely and, in consequence, lead to lower individual wage growth. We provide evidence in support of such effects in Section 2.6.5.

2.4 Data

2.4.1 Data Overview

Our main data are Social Security Records from the *Beschäftigtenhistorik* (*BEH*), provided by the Institute for Employment Research in Nuremberg (IAB). This data source comprises all men and women covered by the social security system, roughly 77.2% of all workers in Germany. with self-employed workers, military personnel, and civil servants excluded. The data reports workers' main employment relationships as of June 30 of each year, for the years 1999 to 2014. Information on business tax rates, specifically the municipality's collection rate, is available from the statistical offices of each of the sixteen German states, from 1992 onward.

Our data have important advantages. First, they are much larger in size than the data used by Fuest el al. (2018) who based their empirical analysis on the Linked Employee-Employer IAB dataset (LIAB), an approximately 1% random sample of establishments linked to their employees' social security records. Second, our data contain information on the location of the establishment at a very disaggregated level—the municipality—at which the local business tax rate is set. The data allow us to not only precisely measure employment and wages at the municipality level, but also to follow workers across establishments and municipalities, making it possible to study how local employment and wages adjust. The large sample size combined with detailed information on workers and establishments—such as workers' education, establishment size and sector, and establishment entries and exits—enable us to paint a detailed picture of how particular types of workers and establishments differentially respond to business tax changes.

As is typical in administrative data sets, the wage variable is top-coded at the social security limit. Similar to Dustmann, Ludsteck and Schönberg (2009), we impute censored wages under the assumption that the error term is normally distributed while allowing the residual variance to differ by year, gender, education and the commuting zone—a geographic area characterized

by high commuting flows of workers within, but low commuting flows outside.[18] We further harmonize workers' education and full-time and part-time status following Fitzenberger, Osikominu and Völter (2006) and Fitzenberger and Seidlitz (2020). (Consider Appendix 2.B.1 for details.)

2.4.2 Sample Selection

Our empirical analysis focuses on establishments that are liable to the business tax rate (see Appendix 2.B.2 for a definition) and restrict the sample to employees not currently in an apprenticeship who are between 18 and 65 years old. To compute AKM-style worker and establishment fixed effects, we use the universe of full-time workers not in apprenticeship training aged between 18 and 65, including those in establishments that are not liable to the business tax.[19]

Our employment analysis is based on full-time and part-time workers as well as workers in marginal employment relationships, that is, employment contracts of less than ten work hours per week. We compute full-time equivalent employment by assigning weights of 1, 0.5 and 0.25 to full-time, part-time, and marginal work. In the absence of detailed information on hours worked, our wage analysis is based on full-time workers only.

We impose the following sample restrictions regarding municipalities (see Panel A of Table 2.1 for a summary). In our data, the territorial boundaries of municipalities are updated to 2015, when there were a total of 11,085 municipalities in Germany. Of these, 1,878 municipalities, most located in East Germany, have been subject to territorial changes at least once during our sample period. Since we are not able to correctly infer the business tax rate for these municipalities, we exclude them from our sample. We end up with a balanced sample of 9,207 municipalities.

[18] In our sample there are two hundred and forty-seven commuting zones in Germany. A commuting zone consists of fourteen municipalities on average.

[19] Specifically, we use social security records from the BEH referring to June 30 for the years 1992 to 2014 to estimate AKM-style wage regressions. We estimate regressions for all possible seven-year windows within the 1992 to 2014 period. When studying heterogeneous wage and employment responses across establishments (shown in Figure 2.3 and Table 2.4), we use establishment fixed effects estimated for the period t-τ-6 to t-τ to categorize establishments. When studying compositional changes following a business tax hike between periods t-τ and t in Table 2.6, we once again assign establishment fixed effects estimated over the period t-τ-6 to t-τ.

From this database, we construct a sample of “treated” and “control” municipalities. Control municipalities never experienced a tax change over the 1992 to 2014 period (1,118 control municipalities). “Treated” municipalities experienced no tax change for at least four years, after which their business tax rate changed at least once over the 1999 to 2014 period by at least 0.5 percentage points. This change was not followed by a subsequent change in the opposite direction in the four years following the tax change (4,815 treated municipalities). For treated municipalities that experienced more than one tax change in the same direction, we focus on the first tax change. In our baseline specification, treated municipalities are in our sample for a total of up to eight years (four years prior to and four years after the first tax change).

We drop municipalities that experienced so many tax changes that we are not able to isolate one tax change without observing previous tax changes, and municipalities that changed the business tax rate in opposite directions over a four-year period (2,025 municipalities). To make sure that our findings are not driven by outliers, we further trim the sample and exclude municipalities with an average wage in the top 3 percent of the local wage distribution **(**303 municipalities).

2.4.3 Descriptive Evidence on Changes in Business Tax Rates

Panel B of Table 2.1 provides an overview of the magnitude and direction of changes in effective business tax rates (computed as in equation (1)) in treated municipalities in our sample, where we focus on treated municipalities’ first tax change. Almost all business tax changes in our sample are tax increases (96 percent). This is a consequence of two trends. First, municipalities faced increased expenditures on compulsory items that they are required to provide but have little control over (such as social benefit payments). Second, municipalities experienced a decline in revenue because of two federal reforms: the abolition of the capital tax in 1998 and the decrease in the federal

business tax rate from 5 to 3.5 percentage points in 2008.[20] Municipalities were at best only partially compensated for the increased expenditures and reduced revenues through larger transfers from the state or federal government.

The mean (initial) tax change in our sample (weighted by municipality employment in the year before the tax change) amounts to 0.96 percentage points or 6.7 percent, with a standard deviation of 0.72 percentage points (5.3 percent). Tax changes smaller than 1 percentage point amount to 46%, while 54% surpass 2 percentage points. Only a handful of tax changes exceed 4 percentage points. Figure 2.1 further highlights that tax changes in our sample are, by construction, highly persistent, amounting to about 1 percentage point (7 percent) in the third year after the tax change.

Table 2.2 compares treated and control municipalities in the year before the tax change.[21] Treated and control municipalities are very similar in terms of industry structure, education structure, average wages. and average establishment size.

2.5 Empirical Strategy

Our baseline specification is an event study difference-in-difference specification that contrasts outcomes of treated and control municipalities (such as (log) employment or (log) full-time daily wages) in the years before and after the tax change. When we decompose the municipality employment effect induced by the business tax change into various components, such as the share that is attributable to establishment entry and exit, we revert to a first difference design. We describe both approaches in turn.

[20]Up until 1997, firms were liable to the local capital tax (Gewerbekapitalsteuer), whose revenues accrued the municipal budget. The same firms liable to the local business tax were liable to the local capital tax. The tax base of the local capital tax was firms' net assets.

[21] Since the year before the tax change is not defined for control municipalities, we weight the sample for control municipalities to mirror the year distribution in the sample for treated municipalities in the year before the tax change. Let f_t^T be the (worker-weighted) share of observations in the treated sample that fall in year *t*, and f_t^C the respective share for the control sample. We then weight the descriptive statistics for control municipalities by f_t^T / f_t^C.

2.5.1 Event Study Specification

We start by estimating the following regression at the municipality level:

$$y_{mt} = \sum_{\tau=-3}^{-1} \gamma_\tau \, Event_{m,t}^{\tau} + \sum_{\tau=1}^{5} \gamma_\tau \, Event_{m,t}^{\tau} + \delta_t + mun_m + e_{mt} \quad (3)$$

where the superscript τ denotes the time period relative to the (first) tax change in treated municipalities (the tax change occurs between $\tau = 0$ and $\tau = 1$); y_{mt} denotes outcomes of interest in municipality *m* in calendar year *t* such as (log) employment or average (log) wages; $Event_{m,t}^{\tau}$ are indicator variables equal to 1 (-1) if the municipality increased (decreased) the tax τ periods ago (or, for $\tau < 0$, will increase the tax in τ periods), and 0 otherwise; δ_t and mun_m denote calendar year and municipality fixed effects, respectively.[22]

The parameters of interest in equation (3) are coefficients γ_1 *to* γ_5, which measure the percentage change in the outcome of interest (that is, (log) employment or the average (log) wage in the municipality) in treated municipalities between τ years after and the year before ($\tau = 0$) the tax change, relative to control municipalities. Since the mean tax change in treated municipalities is close to 1 percentage point (or 7 percent) three years after the tax change (see Figure 2.1), these coefficients can be interpreted as the local employment or wage effects of a 1 percentage point (or 7 percent) increase in the business tax rate. We plot coefficients γ_{-3} to γ_5 in the event study figures, and focus on effects three years after the tax change in all tables.

A causal interpretation of estimated effects rests on the assumption that outcomes in treated municipalities would have evolved in the same way as outcomes in control municipalities if the change in business tax rates had not occurred, so that treated and control municipalities should experience similar

[22] Recall that treated municipalities are included in the sample for 8 years around the tax change (from -3 before the tax change to +4 after the tax change, with the tax change occurring in τ =0), whereas control municipalities are included in the sample over the entire 1999 to 2014 period. This sample restriction ensures that calendar year fixed effects δ_t are identified from control municipalities only.

trends in outcomes y_{mt} prior to the business tax change. Coefficients γ_{-3} *to* γ_{-1} should therefore be closely centered around zero and not exhibit a downward or upward trend—which is what we find (as shown in Figure 2.2). We provide additional support for the identification assumption in Table 2.3.

When estimating equation (3), we weight observations by employment in the municipality, to make estimates representative for *workers* in our sample.[23] We cluster standard errors at the municipality level.

2.5.2 First Difference Specification

To decompose the municipality employment effect induced by the business tax change into various components, such as the share that is attributable to establishment entry and exit, we estimate first difference regressions using *changes* in employment in a municipality in treated and control municipalities:

$$\frac{E_{m,t}-E_{m,t-\tau}}{E_{m,t-\tau}} = \beta_{\tau}\, Treat_m + \eta_t + u_{m,t}. \qquad (4)$$

Here, $\frac{E_{m,t}-E_{m,t-\tau}}{E_{m,t-\tau}}$ denotes the percentage change in employment in municipality *m* between t and $t-\tau$, $Treat_m$ is an indicator variable that is equal to 1 (-1) for treated municipalities that experience a tax increase (decrease) and 0 for control municipalities, η_t denotes calendar year fixed effects and $u_{m,t}$ is an error term.[24] While we estimate equation (4) separately for each year after the business tax change, our discussion focuses on effects three years after the tax change (i.e., $\tau = 3$). As before, estimates refer to a 1 percentage point (or 7 percent) change in the local business tax. We weight observations by municipality employment in $t-\tau$ and cluster standard errors at the municipality level.

To study within-establishment responses, we also estimate regression equation (4) at the establishment level, restricting the sample to surviving establishments with at least two employees. In these establishment level

[23] We weight by current municipality employment in the log-wage regressions and by municipality employment in 1999 (i.e., before the first tax change in our sample) in the employment regressions.

[24] Note that $\frac{E_{m,t}-E_{m,t-\tau}}{E_{m,t-\tau}} \approx logE_{m,t} - logE_{m,t-1}$ and $\eta_t \approx \Delta\delta_t$ of equation (3).

regressions, we weight observations by establishment employment in $t-\tau$ and once again cluster standard errors at the municipality level.

2.6 Results

2.6.1 The Impact of Local Business Taxation on Local Employment and Wages

The theoretical framework in Section 2.3 suggests that the impact of a business tax increase on firms' demand for labor is in principle ambiguous, depending on whether the scale or the substitution effect dominates. We now investigate how local employment and wages adjust in response to a local business tax hike.

2.6.1.1 Event Study Estimates

Panel A of Figure 2.2 displays estimates from an event study for employment, based on regression equation (3). The figure illustrates that local employment evolved similarly in treated and control municipalities in the four years before the tax change. After the tax increase, however, employment in treated municipalities gradually declines relative to control municipalities, with a one percentage point increase in the tax rate causing a decrease in employment of about 0.8 percent after one year and 1.17 percent after three years. Taking into account that the average tax increase is 7 percent (Part B of Figure 2.1), these estimates imply that a one percent increase in the business tax rate lowers employment in the municipality after three years by 0.17 percent.

According to the theoretical model, a decline in local employment should go hand in hand with a decline in local wages. Consistent with this prediction, Panel B of Figure 2.2 highlights that wages in treated municipalities, while progressing similarly before the tax increase, gradually decline relative to control municipalities after the tax increase. A one percentage point increase in the tax rate decreases wages by 0.3 percent after one year and 0.5 percent after three years (see also column (1) of Panel B, Table 2.3). Taking into account that the average tax increase in relative terms is 7 percent (see Part B of Figure 2.1), these estimates imply that a one

percent increase in the business tax rate lowers wages in the municipality after three years by 0.07 percent.

Thus, these estimates suggest that increases in local business taxes have effects on both local wages and employment, in keeping with our model. We next check the robustness of our findings vis-a-vis local and industry-wide shocks.

2.6.1.2 Robustness Checks and Sensitivity Analysis.

To probe the robustness of the results, we first augment regression equation (3) by including commuting zone-by-year fixed effects, thereby allowing for fully flexible differential time trends at the larger local labor market level (as seen in column (2) of Table 2.3). This specification leverages variation in business tax rates over time across neighboring municipalities within the same commuting zone. Estimates in column (2) of Table 2.3 show that the results remain qualitatively very similar to our baseline estimates for both employment and wages.

Next, we re-estimate regression equation (3) and include interactions between broad 1-digit industry employment shares (12 industries) in 1999 (the first year of our sample) and year fixed effects (column (3)), to eliminate any differential trends in employment between treated and control municipalities stemming from differences in their baseline industry structure. The results are very close to our baseline estimates in column (1).

In column (4), we report estimates from a first difference specification as described in Section 2.4.2 (equation (4), for $\tau = 3$, i.e. three years after the tax hike). Not surprisingly, estimates from the first difference design are very similar in magnitude as our baseline event study estimates.

In column (5), we re-estimate the first difference regression equation (4) at the establishment level, restricting the sample to surviving establishments. This specification captures employment adjustments within establishments in response to a tax hike. The estimated coefficients continue to be negative for both employment and wages, but are smaller in magnitude than the employment decline at the municipality level by a factor of nearly 40 percent. This finding suggests that surviving establishments become smaller in affected municipalities following the tax hike, but that local employment also

adjusts to a business take hike through reduced establishment entry and increased establishment exit—adjustment channels that we will investigate in more detail in Section 2.6.2.

Overall, these findings highlight that the scale effect dominates the substitution effect: even though the factor capital has become more expensive following a business tax hike, firms reduce their demand for labor as their overall costs increase. Viewed through the lens of the theoretical model outlined in Section 2.3, a negative employment effect (among surviving establishments) indicates that the elasticity of substitution between goods η, the key determinant of the scale effect, exceeds the elasticity of substitution between capital and labor σ, the key determinant of the substitution effect.

Local labor supply elasticities. Combining the estimates for the local employment and wage responses to a business tax hike reported in Table 2.3, we can back out the local labor supply elasticity δ ($\delta = \frac{\frac{dlogL^{m*}}{dlog\tau}}{\frac{dlogW^{m*}}{dlog\tau}}$). We obtain estimates of between 2.25 and 3.11.[25] Local labor supply therefore is fairly elastic, consistent with the evidence reported in Dustmann, Schönberg and Stuhler (2016).

2.6.1.3 Comparison to Existing Studies

How do these estimates compare to those found in the existing literature? Leveraging the same type of variation in local business tax rates as in this study and drawing on a similar though considerably smaller data source, Fuest et al. (2018) report a within-establishment wage response that is very similar to ours (which is -0.38 in Fuest et al. (2018), in response to a 1.15 percentage point increase in the business tax rate, as compared to our estimate of -0.33, shown in column (5) of Panel B, Table 2.3, in response to a one percentage point increase).[26] In contrast, leveraging variation in business tax rate changes

[25] Based on the baseline specification (column 1, Table 2.3) we get $b = -1.17/-0.52 = 2.25$, or based on the first difference specification (column 4, Table 2.3), $b = -1.34/-0.43 = 3.11$.

[26] Fuest et al. (2018) report results from a difference-in-difference specification that uses log(1-τ) as the key right-hand variable. Evaluated at the mean business tax rate of about 13.3 percent, a unit increase in log(1-τ) corresponds to a decrease in the business tax rate of about 1.15 percentage points ((0.867-0.857)/0.867).

across establishments *within multi-state firms* (as discussed in Giroud and Rauh, 2019) does not yield a finding that establishments lower wages in response to a state-wide business tax hike. One explanation for this finding is that multi-establishment firms follow a national wage policy such that establishment wages within the same firm vary with local conditions, in line with findings by Hazell, Patterson, Sarsons and Taska (2021).

Micro-based estimates for the employment effects of business taxation that use a credible identification strategy are rare. Using census data for the US, Suarez-Serrato and Zidar (2016) report that a cut in the state business tax rate by approximately one percentage point increases population in the state by between 3.78 and 4.28 percent over a ten-year period.[27] Our baseline estimate on local employment is 1.17 percent, a considerably smaller value—although we exploit variation in the local business tax rate at a much finer geographical level (at the municipality level rather than the state), where we would expect a stronger employment response.

Giroud and Rau (2019) leverage variation in business tax rates across U.S. states, and over time, in a manner similar to Suarez-Serrato and Zidar (2016), and report smaller employment effects than we have; they report an effect of about 0.4 percent in response to a one percentage point tax hike. Their empirical strategy, however, compares employment adjustments of establishments differentially affected by state-wide tax changes within the same multistate firm and, therefore, neglects the effects of business taxation on firm entry and exit. Firms' entry and exit may both play a key role in accounting for the overall decline in local employment following a tax hike, which we investigate next.

2.6.2 The Role of Establishment Entry and Exit

The theoretical model predicts increased establishment exit in response to an increase in the local business tax rate. In line with this prediction**,** Panel C of Figure 2.2 illustrates that the number of establishments, while evolving very

[27] Like Fuest et al. (2018), Suarez-Serrato and Zidar (2016) consider the effects of a one unit increase in log(1-τ) state population. The mean state corporate tax rate in the US is about 6 percent so that a one unit increase in log(1-τ) corresponds to a decrease in the corporate tax rate of approximately 1 percentage point ((0.94-0.93)/0.94).

similarly before the tax increase, gradually declines in treated municipalities compared to control municipalities after a tax increase. A one percentage point increase in the tax rate reduces the number of establishments by 0.26 percent after one year and 0.5 percent after three years. Taking into account that the average tax increase in relative terms is 7 percent (shown in Part B of Figure 2.1), these estimates imply that a one percent increase in the business tax rate lowers the number of establishments in the municipality after three years by 0.07 percent.

Decomposition of the Overall Local Employment Effect. This raises the question of how much of the overall local employment effect can be attributed to establishment entry and exit. Panel A of Figure 2.3 decomposes the magnitude of the total local employment effect (obtained from the first difference specification described by equation (4) and presented in column (4) of Panel A, Table 2.3) into four different effects: 1) an employment effect that arises within continuing establishments, 2) an employment effect that arises due to increased establishment exit, 3) an employment effect that arises due to reduced establishment entry, and 4) an employment effect that arises due to increased establishment reallocation–whereby the establishment continues to exist but relocates (see Appendix 2.C.1 for details). Panel A of Figure 2.3 highlights that close to half (48%) of the overall decline in local employment following a business tax hike is due to increased establishment exit, with within-establishment adjustments accounting for most of the remaining half (41%). In contrast, reduced establishment entry and establishment reallocation play only a minor role.

Our model predicts that smaller and lower-paying establishments exit the market in response to a business tax hike, as a business take hike reduces firms' profits and profits are higher for larger and higher-paying firms (Proposition 1). In Panels B and C of Figure 2.3, we report evidence that is in line with this prediction. In Panel B, we decompose the local employment decline that is attributable to establishment exit into three components. Each component corresponds respectively to the effect of small, medium, and large

establishments (such that each year, one-third of workers are employed in each category.) Panel C components correspond to establishments with low, medium, and high pay, based on their AKM establishment fixed effect estimated over a seven-year period prior to tax change. (Here, also, one-third of workers are employed in each category). See Appendix 2.C.1 for details).

Panels B and C further highlight that the entire tax-induced decline in local employment due to establishment exit can, in large part, be attributed to the exit of small, low-paying firms and, to a lesser extent, to medium-sized and medium-paying establishments. In contrast, both large and high-paying establishments are slightly less likely to exit following a tax hike, although this effect is small and imprecisely estimated.

2.6.3 Heterogenous Employment and Wage Responses

2.6.3.1 Heterogeneity Across Establishments

The model further predicts that larger and higher-paying surviving firms experience a stronger employment and wage decline in response to a business tax hike than smaller and lower paying surviving firms, as the former are more capital intensive. We provide evidence in support of this prediction in Table 2.4, which reports results from establishment-level regressions using the first difference design (Equation 4), estimated separately by establishment type, where we restrict the sample to surviving establishments.

The employment response is about three times larger in establishments with a high AKM fixed effect than in those with a low fixed effect. Similarly, wage declines are nearly four times larger in high-paying than in low-paying establishments. When distinguishing firms by establishment size, differences in tax-induced employment responses are less pronounced, while wage responses are again about four times larger compared to small establishments.

These heterogeneous employment responses, in conjunction with the heterogeneous wage responses that point in the same direction, are a strong indication of a monopsonistic labor market and are incompatible with a perfectly competitive labor market. These estimates can also be used to back out labor supply elasticity to the firm and isolate it from labor supply elasticity

to the local economy. We obtain a firm labor supply elasticity of 1.5 using heterogeneous responses across establishment AKM fixed-effect groups and one of 0.78 using heterogeneous responses across establishment size groups, which imply a wedge between the marginal revenue product of labor and wages of about 44-53 percent (see Appendix 2.C.2 for details).

2.6.3.2 Heterogeneity Across Workers

A business tax hike may also affect low- and high-skilled workers differently. In Table 2.5, we report municipality-level employment (in Panel A) and wage effects (in Panel B) of local business taxation separately for three types of workers: workers without post-secondary education (low-skilled workers), workers with an apprenticeship degree or high-school diploma (medium-skilled workers), and workers with a university degree (high-skilled workers). Estimates are based on the event study design (Equation (3), estimated separately by skill group. Whereas there is no clear pattern between workers' skill level and the local wage response to a business tax hike–the wage reduction following the tax hike is smallest among medium-skilled workers and similar for low- and high-skilled workers–employment declines become considerably more pronounced as workers' skill levels increase. These findings suggest that capital and high-skilled labor are more complementary than capital and medium- and low-skilled labor**.**

2.6.3.3 Compositional Changes

Since the local business tax hike differentially affects establishments' employment and exit decisions, the composition of establishments operating in the local economy may change in response, with the direction of the compositional shifts being ambiguous. On the one hand, small and low-paying establishments exit in response to the business tax hike, which will improve average establishment quality in the local economy. On the other hand, employment reductions among surviving establishments are most pronounced among larger and higher-paying establishments, which will lead to a deterioration of establishment quality.

To investigate which effect dominates, we estimate the effect of a business tax increase on the worker-weighted average establishment quality in the local

area, defined as the AKM establishment fixed effect estimated over a seven-year period prior to the tax change, weighted by the number of employees in each establishment, based on the first difference design (Equation 4). The results, reported in Table 2.6, suggest that the exit of lower-paying firms dominates employment reductions in higher-paying firms, so that the worker-weighted average establishment quality in the local area slightly increases. Thus, an increase in the local business tax hike by 1 percentage point increases worker-weighted establishment wage premiums by 0.16 percent (see column 1) or 0.6 standard deviations (see column 2).[28] In keeping with this, column 3 shows that the share of low fixed-effects establishments (that is, establishments with an AKM establishment fixed effect in the bottom third) slightly decreases following a business tax hike.

2.6.4 How Does Local Employment Adjust?

We next investigate how local employment adjusts, and whether workers face an increased risk of separation from their establishments following a local business tax hike. Moreover, we further explore whether workers transit into unemployment, or non-employment, *or* find employment elsewhere if separated due to a tax increase, and whether local employment adjusts primarily because establishments reduce hiring.

As a first step, we decompose the total local employment change (as reported in column 4 of Table 2.3) into reduced hiring and increased separations (see Appendix 2.C.3 for details). The estimates in row (i) of Table 2.7 indicate that nearly all of the tax-induced decline in local employment can be attributed to reduced hiring. Separations from establishments, in contrast, remain largely unchanged following a tax hike.

[28] Since the AKM fixed effects are estimated over a period that precedes the tax hike, changes in the average AKM fixed effect in the municipality are driven by establishment exit and the reallocation of workers across surviving establishments. A caveat to bear in mind is that AKM fixed effects do not exist for entering establishments (as they are estimated over a period preceding the tax hike). Estimates reported in Table 2.6 therefore do not capture potential compositional changes due to newly entering establishments. If a tax hike induces newly-entering establishments to pay lower wage premiums, then, Table 2.6 may somewhat overstate the positive composition effect.

We next distinguish between separations to employment and separations to non-employment. Estimates in row (ii) of Table 2.7 reveal that workers are more likely to transit into non-employment in response to a business tax hike. Here, an increase in the business tax by one percentage point leads to a 0.5 percent higher probability that a worker who was employed prior to the tax hike is not in employment three years later. Total separation rates do not increase in response to a tax hike because previously employed workers are less likely to move to another establishment–although this effect is imprecisely estimated. At the same time, hiring rates from employment strongly decline following the local business tax hike (see column 2).

Results shown in row (iii) further highlight that the reductions in hiring and separation rates from employment predominantly occur within the same commuting zone, either within the municipality hit by the tax increase or neighbouring municipalities within the commuting zone. This finding suggests that a business tax hike reduces job churning (hires plus separations from and to employment, divided by baseline employment) in the wider local labor market.

Transitions by Establishment Type. We next investigate which establishments contribute to the increased transitions into non-employment of incumbent workers. In Panel A of Table 2.8, we decompose the separation rate to non-employment into three components stemming from small, medium, and large establishments and establishments with low, medium, and high pay (see Appendix 2.C.4 for details). The findings highlight that the increased employment-to-non-employment transitions are entirely driven by workers who were employed in small and low-paying establishments before the tax hike—that is, exactly the type of establishments that exit the market following the business tax hike (as shown in Figure 2.3). The findings in Panel B of Table 2.8 further indicate that surviving establishments predominantly reduce their employment through reduced hiring rather than increased separations, regardless of their size and wage premium. In fact, separations from surviving

establishments decline in response to a business tax hike—although this effect is imprecisely estimated.[29]

2.6.5 Mobility and Individual Wage Growth

Mobility is a crucial determinant of individual wage growth, helping workers to overcome the lack of information about where their skills are most productive (Stigler, 1962) by experiencing different jobs (see Burdett, 1978 and Jovanovic, 1979 for early formalizations of this idea). Topel and Ward (1992) show that in the US more than a third of early career wage growth is associated with job changing. Adda and Dustmann (2022) confirm the importance of job mobility for wage growth in Germany. Our findings, elaborated in previous sections, suggest that workers in municipalities affected by a tax hike face not only increased competition for jobs from workers who lost their job due to establishment exit, but also from reduced churning, as surviving establishments, particularly those who pay higher wages, reduce their hiring in response to a local business tax hike, making it more difficult for workers to move to another job. The reduced mobility following a tax hike could then result in workers' wage reductions over and above the direct within-firm wage reduction.

.

Focusing on workers who were employed full-time prior to the tax hike and continue to be employed full-time, in any establishment three years later, Panel A of Table 2.9 shows that workers hit by a business tax hike are less likely to move to a new establishment, and have worked for fewer establishments than workers in control municipalities. In terms of magnitude, a one percentage point increase in the business tax hike reduces the probability of an employer switch by 0.98 percentage points. Moreover, the probability of upward movement, defined as a move to establishments that pay higher wage premiums, or movements that result in wage increases, is particularly affected.

[29] Because we measure employment in full-time equivalents, effects that can be obtained by subtracting separations from hirings would only add up to the heterogeneous employment effects in Table 2.4 if additional accounting for changes in full-time/ part-time status of continuing workers is conducted. These are on average very small.

In Panel B of Table 2.9, we report estimates for the effects of local business taxation on individual wage growth, focusing on the total wage change and the wage change that is conditional on staying with the previous establishment or moving to a different establishment. Business tax hikes appear to cause smaller wage reductions for workers who remain employed with their previous employer than for workers who switch to a new employer, a finding consistent with the idea that business takes hikes make it harder for workers to make an upward move.

In Panel C, we break down the wage analysis according to whether the worker was employed in a low-, medium- or high-paying establishment prior to the tax hike. In keeping with our findings in Table 2.4, within-establishment wage declines are smaller for workers who were employed in low-paying establishments before the tax hike, as these establishments are less capital-intensive and, hence, less affected by the increase in the effective cost of capital. Yet, workers in these establishments suffer at least as high an overall wage decline as workers who were previously employed in higher-paying establishments following the tax hike, due to the decrease in upward movements and the larger wage declines that occur when switching establishments. Workers from low-paying establishments have the most to gain from job mobility, however, the business tax hike makes it harder for them to do so. Not only they are more likely to be displaced from their pre-tax establishment, because of increased establishment exit, but establishments that pay higher wage premiums reduce their hiring in response to the business tax hike. A simple decomposition suggests that about 70 percent of the tax-induced overall wage decline experienced by workers who were employed in a low-paying establishment prior to the tax hike can be attributed to the decrease in churning, rather than the direct effect of the business tax hike (see Appendix 2.C.6 for details). The decrease in churning also implies that workers are not necessarily able to benefit from the improved composition in firm types documented above.

2.7 Discussion and Conclusion

In this paper, we leverage variation in business tax changes across municipalities in Germany to paint a detailed picture of how local labor markets, firms, and workers adjust to such tax changes. Our empirical design traces out municipality, firm and worker outcomes in “treated” municipalities (municipalities that experienced a persistent tax change of at least 0.5 percentage points between 1999 and 2014) and “control” municipalities (municipalities that did not experience any tax change over that period) before and after the tax change, in an event study design.

We find that a one percentage point increase in the business tax rate lowers employment and wages in the municipality by 1.17% and 0.52%, respectively. While increased establishment exit is an important channel through which local employment adjusts, accounting for close to half (48 percent) of the local decline, employment also declines at the intensive margin, within surviving establishments. The tax-induced increased establishment exit is primarily driven by smaller and lower-paying establishments. Within-establishment employment and wage reductions, by contrast, are more pronounced in larger, higher-paying establishments. Overall, the average worker-weighted quality of establishments in the municipality, measured in terms of the premium establishments pay to their workers and establishment size, slightly increases following a local business tax hike.

Yet, workers do not benefit from the tax-induced improvement in establishment quality, as continuing establishments adjust their employment, largely through reduced hiring rather than through increased separations after a business tax hike. This reduces job churning–job-to-job transitions–and affects workers’ wages over and above the direct effect of the tax increase. Workers are less likely to move to a better-paying job and experience smaller gains from moving to another establishment. A business tax hike, thus**,** disrupts movements up the job ladder, which contributes to the wage reductions experienced by affected workers. This holds particularly where workers were employed in a small, lower-paying establishment before the tax hike.

It is important to emphasize that our findings refer to local business tax changes and therefore cannot be extrapolated to employment and wages responses to national, or, even, international, business tax change. We would generally expect the employment effects of a local tax change to be larger than that of a national tax change, since workers are likely to be more mobile across local areas within a country than across countries, and since product prices are likely to increase following a national tax hike, dampening the employment response. These channels have opposite effects on wages, with workers' lower mobility increasing the wage response and with increasing product prices dampening the wage response, so that it is unclear whether wage effects would be smaller or larger for a national (compared to a local) tax change. Though, significantly, the heterogeneous wage and employment responses across firms highlighted by the theoretical model and uncovered in the empirical analysis are at work regardless of whether the business tax change is local or national.

Our finding that local employment declines in response to a business tax hike raises the possibility that an increase in the local business tax rate will lower rather than raise the tax revenue collected by municipalities. However, the magnitude of our estimated employment response— that a percentage point increase in the local business tax rate lowers employment by 1.17 percent—is sufficiently small to ensure a finding that tax revenues increase in response to the local business tax hike, albeit less than proportionally.[30]

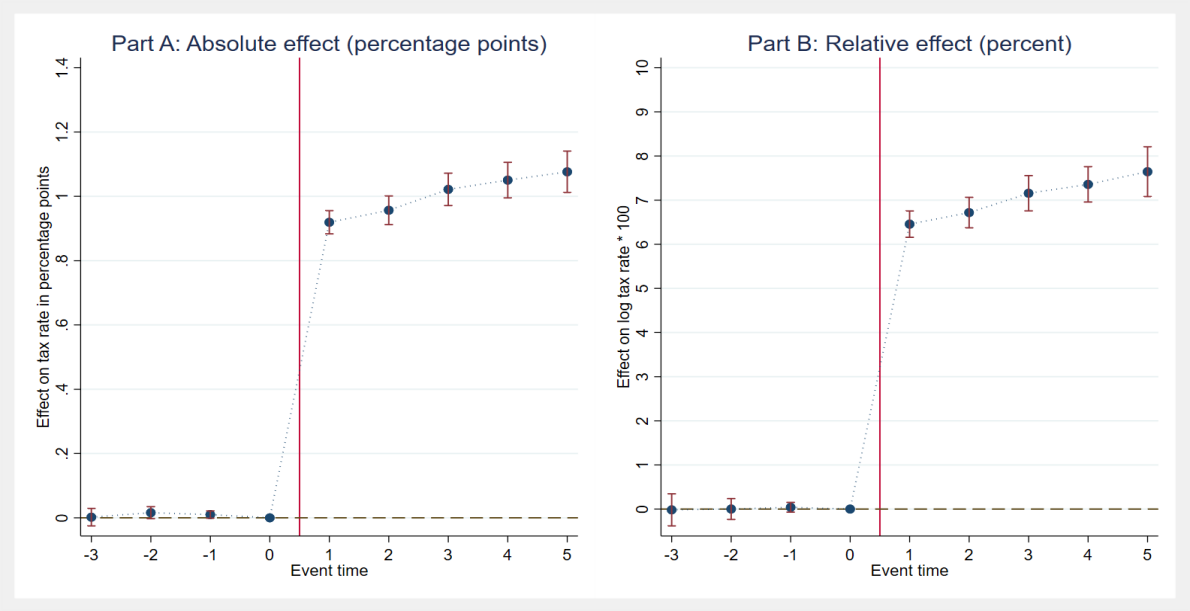
More generally, our findings suggest that, for the type of production technologies used by firms throughout our 1999 to 2014 sample period, capital and workers are not sufficiently substitutable for firms to replace capital with workers when the cost of capital increases. However, some economists have argued that the latest technologies such as artificial intelligence are ever more

[30] Let E_m and $\bar{\pi}_m$ denote the number of employees and average firm profits per employee in the municipality. The municipality therefore collects tax revenues equal to $TR_m = E_m \cdot \bar{\pi}_m \cdot \tau_m$. Assuming that average firm profits per employee are unaffected by a local business tax increase and taking into account that local employment declines by 1.17 percent in response to a one percentage point increase in the business tax (Table 3, column (1)), an increase in the local business tax rate by one percentage point will increase local tax revenues according to $\frac{dTR_m}{d\tau_m} = E_m \cdot \bar{\pi}_m + \frac{dlogE_m}{d\tau_m} \cdot E_m \cdot \bar{\pi}_m \cdot \tau_m = E_m \cdot \bar{\pi}_m \cdot (1 - 1.17\tau_m)$. This expression is negative only for implausibly high business tax rates exceeding 85%.

successful at conducting tasks that previously only humans were able to perform, pointing to increased substitutability between capital and labor (Acemoglu and Restrepo, 2020; Acemoglu and Restrepo, 2022). If, indeed, capital and labor are becoming more and more substitutable, then an increase in local business tax rates could potentially increase employment and wages within surviving firms, and to an even greater extent in larger and higher paying firms—contrary to what we find for our period of analysis.

Figures and Tables

Figure 2.1. Percentage (Part A) and Percent (Part B) Tax Change Over Time. Treated vs Control Municipalities

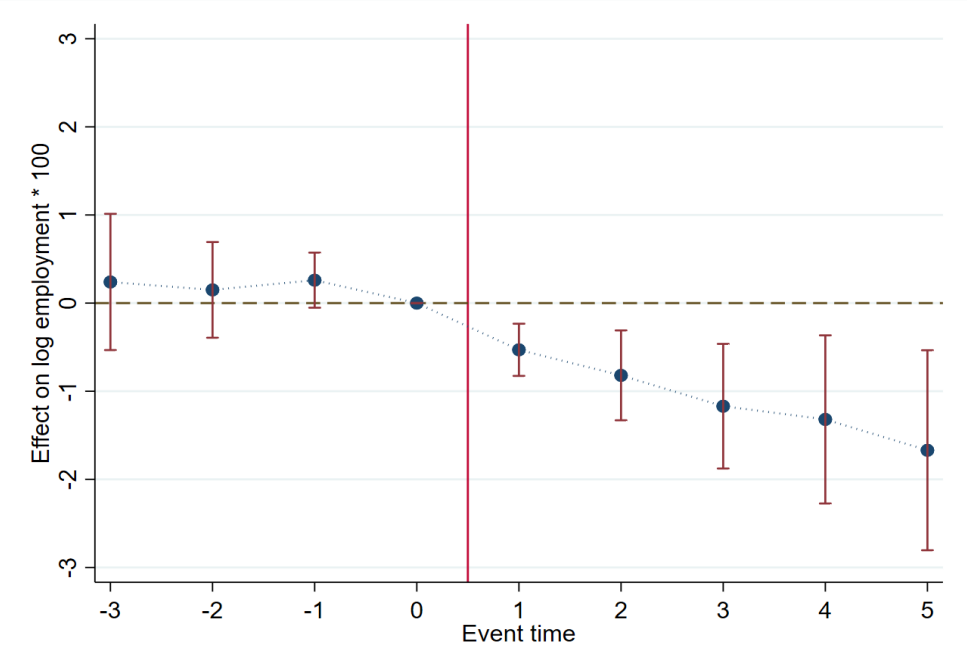


Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the percentage (Part A) and percent (Part B) tax change of treated vs control municipalities. Event study coefficients are estimated according to equation 3 (thus, controlling for year and municipality fixed effects). Control municipalities did not experience any tax change over the years 1992-2014. Differences between treated and control municipalities are normalized to 0 in the year before the tax change (even time = 0). The figure shows that treated municipalities did not experience tax changes between event time -3 and event time 0 and experienced a permanent tax increase between event time 0 and event time 1. Part A (Part B) shows that the average local business tax change experienced by treated municipalities between event time 0 and event time 1 amounts to 0.96 percentage points (6.7 percent). Part A (Part B) shows that three years after the first tax change (event time = 3), the average local business tax change experienced by treated municipalities amounts to 1 percentage point (7 percent). 95%-confidence intervals are based on standard errors clustered at the municipality level.

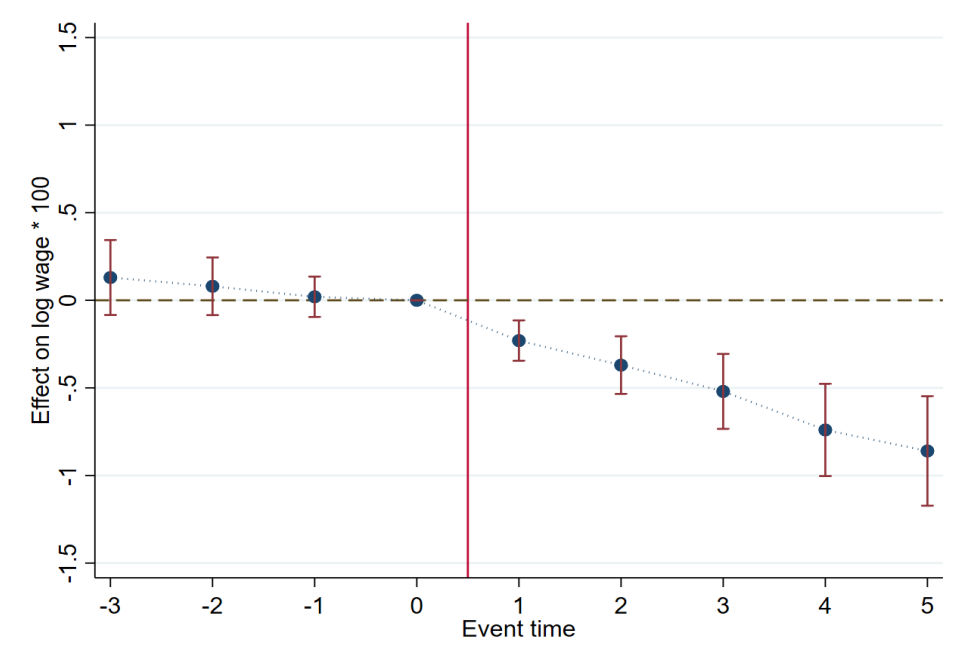
Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Figure 2.2. Effects of Business Taxation on Local Employment (Panel A), Wages (Panel B) and Number of Establishments (Panel C)

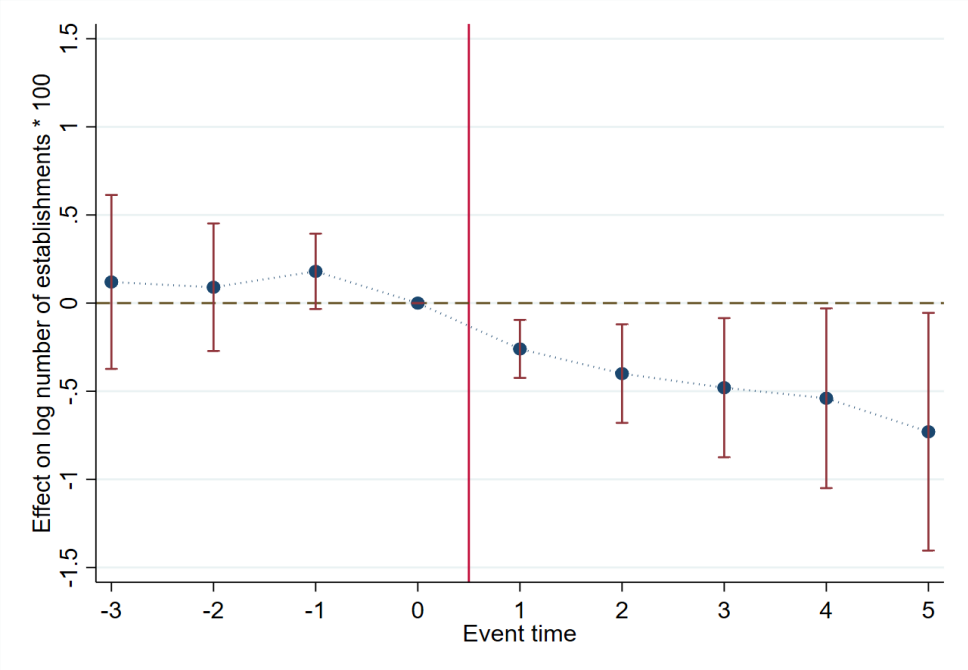
Panel A: Local (municipal) Employment



Panel B: Local (municipal) Wages



Part C: Local (municipal) Number of Establishments

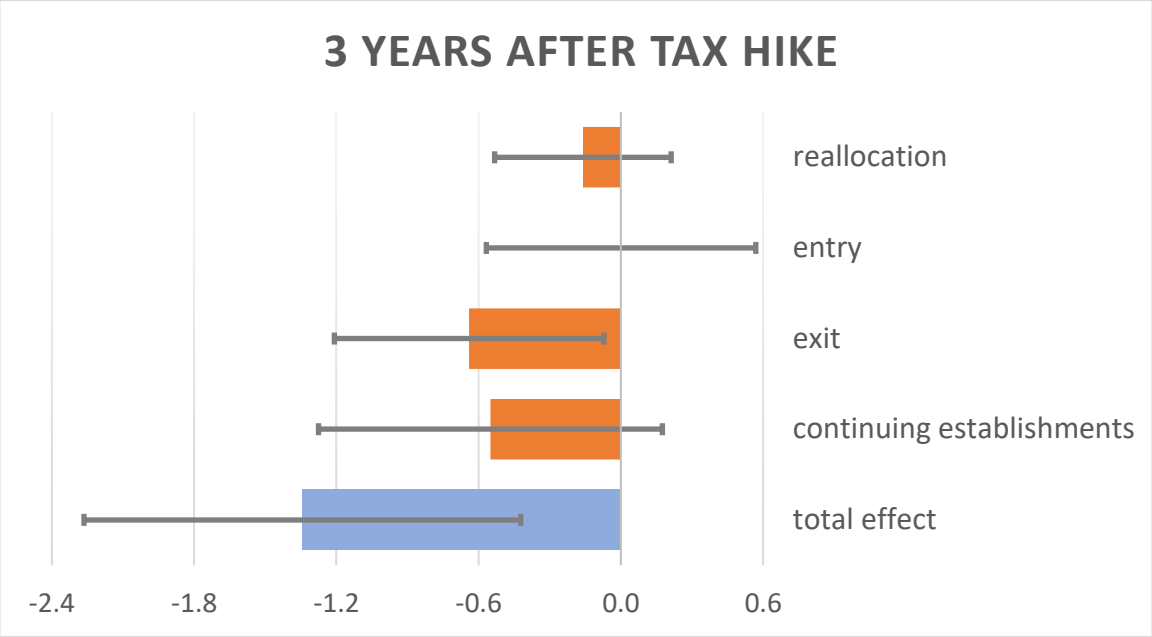


Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of a business tax increase on municipal employment (Panel A), municipal wages (Panel B) and total number of establishments in the municipality for treated vs control municipalities. Event study coefficients are estimated according to equation 3 (thus, controlling for year and municipality fixed effects). The figure shows that local employment (Panel A), local wages (Panel B) and local number of establishments (Panel C) evolved similarly in treated and control municipalities in the four years before the tax change. Panel A shows that after the tax increase, employment in treated municipalities gradually declines relative to control municipalities, with a one percentage point increase in the tax rate causing a decrease in employment of about 0.8 percent after one year and 1.17 percent after three years. Panel B shows that wages in treated municipalities gradually decline relative to control municipalities after the tax increase, with a one percentage point increase in the tax rate decreasing wages by 0.3 percent after one year and 0.5 percent after three years. Panel C shows that after the tax increase, the number of establishments in treated municipalities gradually declines relative to control municipalities, with a one percentage point increase in the tax rate reducing the number of establishments by 0.26 percent after one year and 0.5 percent after three years. 95%-confidence intervals are based on standard errors clustered at the municipality level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Figure 2.3: Decomposition of Employment Effects.

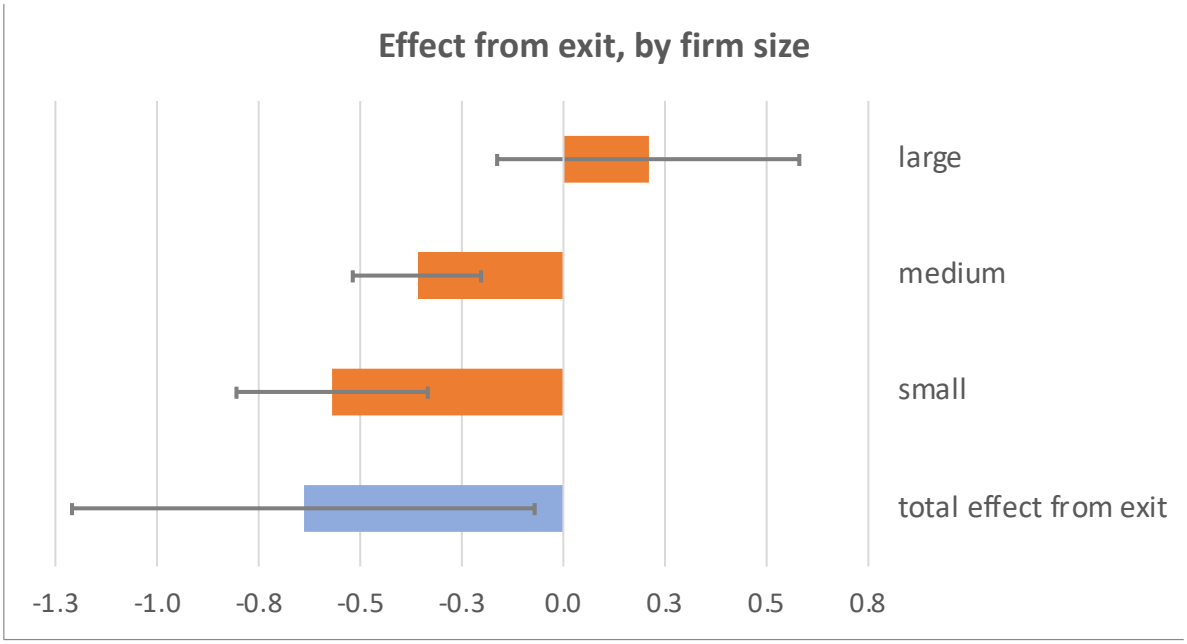
Panel A: Decomposition of the Total Local Employment Effect into Employment Effects among Continuing Establishments, Establishment Entry, Exit and Reallocation



Notes: Panel A of the figure decomposes the magnitude of the total local employment effect ("*total effect*") three years after the tax change in response to a one percentage point increase in the business tax (obtained from the first difference specification described by equation (4) and presented in column (4) of Panel A, Table 2.3) into four components: 1) "*reallocation*": an employment effect that arises due to increased establishment relocation, 2) "*entry*": an employment effect that arises due to reduced establishment entry, 3) "*exit*": an employment effect that arises due to increased establishment exit and. 4) "*continuing establishments*": an employment effect that arises within continuing establishments. Panel A shows that close to half (48%) of the overall decline in local employment following a business tax hike is due to increased establishment exit, with within-establishment adjustments accounting for most of the remaining half (41%). Reduce establishment entry and establishment relocations play only a minor role. 95%-confidence intervals are based on standard errors clustered at the municipality level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

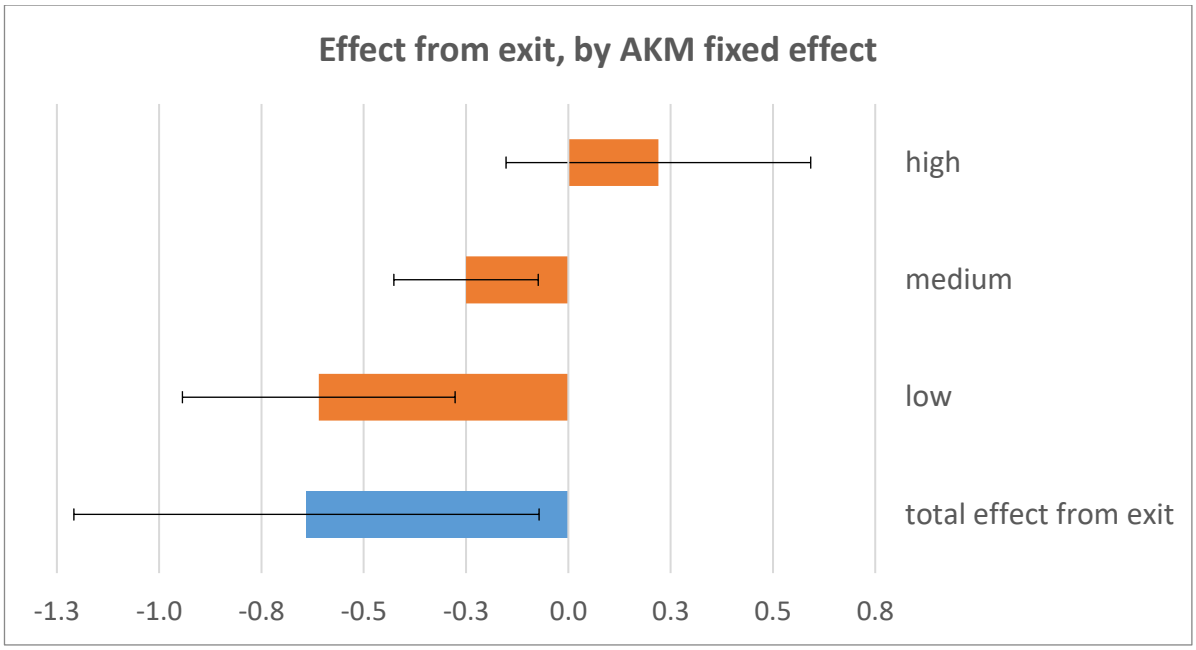
Panel B: Breakdown of Employment Effect due to Establishment Exit by Establishment Size



Notes: Panel B of the figure decomposes the magnitude of the employment effect due to exit ("*total effect from exit*") three years after the tax change in response to a one percentage point increase in the business tax (obtained from the first difference specification described by equation 4) into three components: 1) "large": an effect that arises due to increased exit of large establishments, 2) "medium": an effect that arises due to increased exit of medium establishments, 3) "small": an effect that arises due to increased exit of small establishments. Panel B suggests that the decline in employment due to establishment exit can be attributed to the exit of small and, to a lesser extent, medium establishments. The three categories--small, medium and large establishments—are defined over the year before the tax change such that one-third of workers are employed in each category. 95%-confidence intervals are based on standard errors clustered at the municipality level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Panel C: Breakdown of Employment Effect due to Establishment Exit by AKM Establishment Fixed Effect



Notes: Panel C of the figure decomposes the magnitude of the employment effect due to exit ("*total effect from exit*") three years after the tax change in response to a one percentage point increase in the business tax (obtained from the first difference specification described by equation 4) into three components: 1) "high": an effect that arises due to increased exit of high AKM fixed-effects establishments, 2) "medium": an effect that arises due to increased exit of medium AKM fixed-effects establishments, 3) "low": an effect that arises due to increased exit of low AKM fixed-effects establishments. Panel C suggests that the decline in employment due to establishment exit can be attributed to the exit of low and, to a lesser extent, medium AKM fixed effects establishments. AKM establishment fixed effects are estimated over seven years before the tax change. The three categories--low, medium and high AKM fixed effects establishments—are defined such that one-third of workers are employed in each category. 95%-confidence intervals are based on standard errors clustered at the municipality level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.1. Direction and Magnitude of Business Tax Changes (Treated Municipalities)

Panel A: Number of Treated, Control and Dropped Municipalities	
Treated municipalities	4,815
Control municipalities	1,118
Dropped municipalities:	
Boundary change	1,878
Too many changes or tax changes in opposite directions	2,025
Trimming	303
Panel B: Direction of Business Tax Changes	
Tax increases	96%
Tax decreases	4%
Panel C: Magnitude of Business Tax Changes	
Mean	0.96 percentage points (6.7 percent)
Standard Deviation	0.72 percentage points (5.3 percent)
Share of tax changes < ¦1pp¦	0.46
Share of tax changes > ¦2pp¦	0.54
Share of tax changes > ¦4pp¦	0.009

Notes: Panel A of the table reports the number of treated and control municipalities in our sample and the number of municipalities dropped from our sample. Treated municipalities (4,815) experienced a tax change of at least 0.5 percentage points over the years 1999-2014, without any tax changes in the 4 years prior and without tax changes of the opposite sign in the 4 years after the tax change. Control municipalities (1,118) did not experience any tax change over the years 1992-2014. 4,206 municipalities are dropped from our sample due to the impossibility of identifying the municipality of the tax change (1,878), the impossibility of isolating one tax change (2,025), and trimming (303). Panel B describes the share of treated municipalities with tax increases (96%) and the percentage of treated municipalities with tax decreases (4%). Panel C describes the magnitude of the tax change of treated municipalities. The mean tax change in our sample (weighted by municipality employment in the year before the tax change) amounts to 0.96 percentage points or 6.7 percent, with a standard deviation of 0.72 percentage points (5.3 percent). Tax changes smaller than one percentage point amount to 46%, while 54% surpass 2 percentage points. Tax changes exceeding 4 percentage points are 0.9% of the total.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.2 Treated vs Control Municipalities: Baseline Characteristics (employment weighted)

	Treated municipalities	Control municipalities
Panel A Industry structure (in percent)		
First sector (fishing, mining) and energy	2.1	1.8
Manufacturing	28.2	29.4
Construction and transport	12.9	13.4
Retail and hospitality	18.9	19.1
Real estate and other business activities	19.1	19.9
other	18.9	16.4
Panel B. Skill structure (in percent)		
Low educated	10.1	9.4
Middle educated	80.2	80.4
High educated	9.7	10.2
Panel C: Average Wages and Number of Employees		
(log) Daily full-time wage	4.36	4.38
Establishment size	300	308

Notes: The table compares (employment weighted) characteristics of treated and control municipalities in the year before the tax change. Panel A reports the (average) share of establishments in treated vs control municipalities by 6 industrial categories: 1) first sector, 2) manufacturing, 3) construction and transport, 4) retail and hospitality, 5) real estate and other business activities, and 6) other industries. Panel B reports the (average) share of workers in treated and control municipalities by educational category: 1) low educated, workers without apprenticeship degree or high-school diploma, 2) middle educated, workers with apprenticeship degree or high-school diploma, 3) high educated, workers with a university degree. Panel C reports the (average) log daily full-time wage and the average establishment size by treated and control municipalities. The table suggests that treated and control municipalities are similar in industry structure, education structure, average wages and average establishment size.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.3. Effects of Business Taxation on Local Employment and Wages

	(1)	(2)	(3)	(4)	(5)
	Baseline	Commuting zone by year FE	Industry shares by year FE	First difference specification (municipality level)	First difference specification (establishment level)
Panel A: Effect on employment *100					
Effects three years after tax change	-1.17**	-0.96*	-1.19**	-1.34***	-0.82**
	(0.43)	(0.40)	(0.39)	(0.21)	(0.40)
Panel B: Effect on log wage *100					
	-0.52***	-0.36**	-0.49***	-0.43***	-0.33***
	(0.13)	(0.14)	(0.13)	(0.06)	(0.12)
# Municipalities	5,933	5,933	5,933	5,933	5,933
Municipality FE	Yes	Yes	Yes	Yes (differenced out)	-
Establishment FE	-	-	-	-	Yes (differenced out)
Year FE	Yes	Yes	Yes	Yes	Yes
Commuting zone by year FE	No	Yes	No	No	No
Industry shares (1-digit) by year FE	No	No	Yes	No	No

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on employment (Panel A) and wages (Panel B) three years (event time = 3) after the tax change, estimated following different specifications. Column (1) reports the coefficients on municipal employment (municipal wages) following the event study specification of eq. (3) with the inclusion of year and municipality fixed effects; column (2) with the inclusion of commuting zone-year and municipality fixed effects; column (3) with the inclusion of 1-digit industry employment shares-year and municipality fixed effects; column (4) reports the coefficients on municipal employment (municipal wages) estimated following the first difference specification of eq. (4); column (5) on establishment employment (establishment wages) estimated following the first difference specification of eq. (4). The table shows that a one percentage point increase in the business tax decreases municipal employment (wages) by 1.17% (0.52%) and establishment employment (wages) by 0.82% (0.33%). The coefficients reported in column (1) are robust to the inclusion of commuting-zone-year (column 2) and 1-digit-industry-employment-shares-year fixed effects (column 3). Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.4: Heterogeneous Employment and Wage Responses among Continuing Establishments

Panel A: Effect on employment * 100	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		by establishment wage fixed effect			by establishment size		
	all	Low	Medium	High	Small	Medium	Large
Effects three years after tax change	-0.82**	-0.43	-0.87	-1.37**	-0.89**	-1.18**	-1.11
	(0.40)	(0.53)	(0.62)	(0.69)	(0.39)	(0.42)	(0.81)
Panel B: Effect on log wage * 100		by establishment wage fixed effect			by establishment size		
	all	Low	Medium	High	Small	Medium	Large
Effects three years after tax change	-0.32***	-0.18	-0.13	-0.80***	-0.15**	-0.2**	-0.42**
	(0.12)	(0.15)	(0.13)	(0.23)	(0.06)	(0.1)	(0.21)

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on establishment employment (establishment wages) three years after the tax change, estimated following the first difference specification of equation (4) and separately by establishment AKM fixed effects categories (columns 2-4) and by establishment size categories (columns 5-7). The table shows that the employment response is about three times larger in establishments with a high AKM fixed effect (Panel A, column 4) than in those with a low fixed effect (Panel A, column 2). Similarly, wage declines are nearly four times larger in high-paying (Panel B, column 4) than in low-paying establishments (Panel B, column 2). The employment response is similar between large (Panel A, column 7) and small establishments (Panel A, column 5). Wage declines are nearly three times larger in large (Panel B, column 5) than in small establishments (Panel B, column 7). The table also reports the estimated coefficient (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on (all) establishment employment (establishment wages) three years after the tax change (column 1), estimated following the first difference specification of equation (4). The three categories—low, medium, and high fixed effects (small, medium and large) establishments—are defined such that one-third of workers are employed in each category. Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.5. Heterogenous Employment and Wage Responses by Worker Skill

	(1)	(2)	(3)	(4)
			by worker skill	
	All	Low	Medium	High
Panel A: Effect on employment *100				
Effects three years after tax change	-1.17**	0.17	-1.15*	-2.51**
	(0.43)	(0.83)	(0.46)	(0.81)
Panel B: Effect on log wage * 100				
Effects three years after tax change	-0.52***	-0.64	-0.36***	-0.52
	(0.13)	(0.40)	(0.11)	(0.28)

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on total employment (mean wages) three years (event time = 3) after the tax change, estimated separately by workers skill and following the event study specification of equation (3). The table shows that magnitude of the employment decline is increasing in workers' skills, and is particularly pronounced for high-skilled workers (Panel A, columns 2-4). The pattern of the wage decline is less clear, with both low- and high-skilled workers experiencing large (but insignificant) decreases (Panel B, columns 2 and 4). The table also reports the estimated coefficient (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on municipal employment (municipal wages) three years (event time = 3) after the tax change (column 1), estimated following the event study specification of equation (3). Low-skilled workers are those without post-secondary education, medium-skilled workers are those with an apprenticeship or high-school diploma, and high-skilled workers are those with university degrees. Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.6. Average Establishment Weighted Fixed Effect

	(1) Raw fixed effect *100	(2) Standardised fixed effect * 100	(3) Lowest fixed effect tercile *100
Effects three years after tax change	0.16**	0.61**	-0.32**
	(0.70)	(0.2400)	(0.14)

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the business tax on the worker-weighted average establishment quality in the municipality (columns 1 and 2) based on the first difference design of equation 4, estimated three years after the tax change. The table shows that an increase in the local business tax hike by one percentage point increases worker-weighted establishment wage premiums by 0.16 percent (column 1) or 0.6 standard deviations (column 2). The worker-weighted establishment quality is defined as the AKM establishment fixed effect estimated over seven years before the tax change, weighted by the number of employees in each establishment. The table also reports the coefficient (and corresponding standard error, in parenthesis) of the effect of a one percentage point increase in the business tax on the share of low fixed-effects establishments, based on the first difference design of equation 4, estimated three years after the tax change. Column (3) shows that the share of low fixed-effects establishments decreases following a business tax hike. Low fixed effects establishments are those with an AKM establishment fixed effect in the bottom third of the establishment fixed effects distribution. Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.7. How Does Local Employment Adjust?

	(1) Overall employment effect	(2) Hiring		(3) Separations	
(i) Effects three years after tax change	-1.35***	-1.34***		0.01	
	(0.21)	(0.37)		(0.49)	
		↙ from jtj	↘ from ntj	↙ to jtj	↘ to jtn
(ii) Effects three years after tax change		-0.87**	-0.47*	-0.52	0.52**
		(0.29)	(0.23)	(0.34)	(0.19)
		↙ same CZ	↘ different CZ	↙ same CZ	↘ different CZ
(iii) Effects three years after tax change		-0.77**	-0.1	-0.70*	0.19
		(0.29)	(0.22)	(0.32)	(0.25)

Notes: The table decomposes the magnitude of the total local employment effect (column 1 "*overall employment effect*") in response to a one percentage point increase in the business tax three years after the tax change (obtained from the first difference specification described by equation 4) into different components. Row (i) decomposes the magnitude of the local employment effect into two effects: one that hires from a reduction in hiring (column 2, row i) and the other from an increase in separations (column 3, row i). Row (i) shows that nearly all of the tax-induced decline in local employment can be attributed to reduced hiring. Row (ii) decomposes the hiring (separations) effect into two additional effects: one that hires from a reduction (increase) in job-to-job transitions, "from jtj" ("to jtj"), and the other from a reduction (increase) in non-employment to job (job to non-employment) transitions, "from ntj" ("to jtn"). Row (ii) shows that job-to-job transitions decrease following a business tax hike, both for hiring and separations. Additionally, workers are more likely to transit into non-employment in response to a business tax hike. Row (iii) decomposes the decrease in job-to-job transitions, separately for hiring and separations, into two additional effects: one that arises from a reduction of job-to-job transitions within the same commuting zone, the other from a reduction in job-to-job transitions of workers coming from different commuting zones. Row (iii) shows that the reductions in hiring and separation rates from employment predominantly occur within the same commuting zone. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.8. Local Employment Adjustments by Establishment Type

Panel A: Which Establishments Contribute to the Increase in Job-to-Non-employment Transitions?

	(1)	(2)	(3)
by establishment size			
	Small	Medium	Large
Effects Three years after tax change	0.76**	0.34***	-0.58
	(0.38)	(0.11)	(0.4700)
by establishment fixed efect			
	Low	Medium	High
Effects Three years after tax change	0.64**	0.04	-0.30
	(0.27)	(0.13)	(0.39)

Panel B: Hiring and Separations in Surviving Establishments (Three Years after Tax Change)

by establishment size			
	Small	Medium	Large
hiring	-1.06	-2.13**	-2.07*
	(0.68)	(0.72)	(0.89)
separations	-0.06	-0.90	-1.00
	(0.36)	(0.79)	(0.87)
by establishment wage fixed effect			
	Low	Medium	High
hiring	-2.5	-1.12*	-1.74**
	(1.38)	(0.49)	(0.67)
separations	-1.79	-0.58	-0.25
	(1.33)	(0.57)	(0.51)

Notes: Panel A of the table decomposes the increased transition into non-employment in response to a business tax hike three years after the tax change (obtained from the first difference specification of eq. 4 and reported in Table 2.6 Separations, "jtn") into three components that arise from the increased transition into non-employment of 1) workers in small establishments (column 1, by establishment size), 2) workers in medium establishments (column 2, by establishment size), 3) workers in large establishments (column 3, by establishment size). The increased transition into non-employment is additionally decomposed into three components that arise from the increased transition into non-employment of 1) workers in low AKM fixed-effects establishments (column 1, by establishment fixed effect), 2) workers in medium AKM fixed-effects establishments (column 2, by establishment fixed effect), 3) workers in high AKM fixed-effects establishments (column 3, by establishment fixed effect). Panel A shows that small and low-paying establishments increase their employment-to-non-employment transitions. Panel B of the table decomposes the heterogeneous within establishment employment responses, estimated separately by establishment types (obtained from the first difference specification of eq. 4 and reported in Panel A of Table 2.4) into hiring and separations. Panel B shows that surviving establishments predominantly reduce their employment through reduced hiring rather than increased separations, regardless of their size and AKM fixed effect. Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Table 2.9: Effects of Business Taxation on Incumbent Workers

Panel A: Disruptions in the Job Ladder	(1)	(2)	(3)	(4)
			Upward Move (0/1), defined by …	
	Employer Change (0/1)	Number of Employers	… establishment fixed effect	… wage
Three years after tax change	-0.0098*	-0.0120*	-0.0081**	-0.0114**
	(0.0051)	(0.0064)	(0.0035)	(0.0048)

Panel B: Wage Changes Within and Between Establishments

	Total	Conditional on		Share attributable to
	Wage Change	Staying	Moving	moving
Three years after tax change	-0.57***	-0.48***	-0.74***	15.7%
	(0.16)	(0.13)	(0.26)	

Panel C: Wage Changes Within and Between Establishments, by Establishment Wage Fixed Effect (Three years after Tax Change)

	Total	Conditional on		Share attributable to
	Wage Change	Staying	Moving	moving
Low	-0.95**	-0.29***	-1.82**	69.5%
	(0.39)	(0.11)	(0.72)	
Medium	-0.50***	-0.39***	-0.84***	22.0%
	(0.11)	(0.10)	(0.27)	
High	-0.76***	-0.71***	-1.36***	6.6%
	(0.20)	(0.18)	(0.46)	

Notes: The table reports coefficients (and corresponding standard errors, in parenthesis) of first difference regressions of the effect of one percentage point increase in the business tax on worker level employment and wage outcomes, estimated on workers who were employed full-time before the tax hike and continue to be employed full-time, in any establishment three years later. Panel A report the coefficients of the probability of changing employer (column 1), the number of employers over the three years before (column 2), the probability of moving to an establishment with a higher fixed effect (column 3), the probability of moving to a better-paid job (column 4). Panel A shows that workers hit by a business tax hike are less likely to move to a new establishment, have worked for fewer establishments and are less likely to experience an upward move compared to workers in control municipalities. Panel B reports estimates for the effects of a one percentage point increase in the business tax on the total individual wage growth (column 1), on the wage change that is conditional on staying with the previous establishment (column 2) or moving to a different establishment (column 3). Panel B suggests that a business tax hike causes smaller wage reductions for workers who remain employed with their previous employer than for those who switch to a new employer. Panel C breaks down the wage analysis according to whether the worker was employed in a low-, medium- or high-paying establishment before the tax hike. Panel C shows that within-establishment wage declines are smaller for workers who were employed in low-paying establishments before the tax hike (column 1, Low) and that these workers suffer a larger wage decline (column 3, Low) when switching establishments than workers in high-paying establishments (column 3, High). Standard errors clustered at the municipality level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: German Social Security Records from the *Beschäftigtenhistorik* (BEH)

Appendix 2.A. Model

2.A.1 Effects of θ on Firm Profits, Employment, Wages, and Capital Intensity (Proposition 1)

Effects of θ on profits. Firm profits are given by $\Pi_j = p_j y_j - w_j l_j - R k_j - C$. Substituting in the inverse demand function for the firm's output $p_j = \left(\frac{P}{Y}\right)^{-\frac{1}{\eta}} y_j^{-\frac{1}{\eta}} := P' y_j^{-\frac{1}{\eta}}$, the inverse labor supply curve[31] to the firm $w_j = l_j^{\frac{1}{b}} L^{Sm^{-\frac{1}{b}}} W^{m^{\frac{1}{b}}}$, and the production function $y_j = F(k_j, l_j)$ yields

$$\Pi_j = P' F(k_j, l_j)^{\frac{\eta-1}{\eta}} - l_j^{\frac{b+1}{b}} L^{Sm^{-\frac{1}{b}}} W^{m^{\frac{1}{b}}} - R k_j - C. \qquad (A.1)$$

Let k_j^* and l_j^* be the firm's profit-maximising input choices of labor and capital. Optimised profits are thus equal to:

$$\Pi(k_j^*, l_j^*) = P' F(k_j^*, l_j^*)^{\frac{\eta-1}{\eta}} - l_j^{*\frac{b+1}{b}} L^{Sm^{-\frac{1}{b}}} W^{m^{\frac{1}{b}}} - R k_j^* - C \qquad (A.2)$$

Because $\frac{\partial \log F(k_j, l_j)}{\partial \log \theta_j} = \frac{(\theta_j k_j)^\nu}{(\theta_j k_j)^\nu + l_j^\nu} := s_j > 0$, and assuming $\eta > 1$, an increase in θ_j holding l_j^* and k_j^* constant, unambiguously increases profits, as it increases revenue but does not affect cost. The profit-maximising firm would only choose to further adjust l_j^* and k_j^* if this would lead to an additional profit increase. Therefore $\frac{d\Pi(k_j^*, l_j^*)}{d\theta_j} > 0$. Optimal profits are increasing in θ_j. This proves part a) or Proposition 1.

Effect of θ on employment, wages, and capital intensity. The profit function (A.1) yields first-order conditions for labor and capital equal to

[31] Equation (2) in the text implies labor supply to the firm as $l_j = L^{Sm} P\left(\arg\max_{k \in J, m} \{u_{ij}\} = j\right) = L^{Sm} \frac{w_j^b}{W^m}$..

$$P'\frac{\eta-1}{\eta}F(k_j,l_j)^{-\frac{1}{\eta}}F_L(k_j,l_j)-\frac{b+1}{b}l_j^{\frac{1}{b}}L^{Sm-\frac{1}{b}}W^{m\frac{1}{b}}=0.$$

$$P'\frac{\eta-1}{\eta}F(k_j,l_j)^{-\frac{1}{\eta}}F_K(k_j,l_j)-R=0.$$

Substituting in $F_K(k_j,l_j)=\frac{\partial F(k_j,l_j)}{\partial k_j}=\theta_j^{\frac{\sigma-1}{\sigma}}k_j^{-\frac{1}{\sigma}}F(k_j,l_j)^{\frac{1}{\sigma}}$ and $F_L(k_j,l_j)=\frac{\partial F(k_j,l_j)}{\partial l_j}=l_j^{-\frac{1}{\sigma}}F(k_j,l_j)^{\frac{1}{\sigma}}$, and taking logs yields:

$$FOC_L=\left(\frac{1}{\sigma}+\frac{1}{b}\right)\log l_j-\frac{1}{b}\log L^{Sm}+\frac{1}{b}\log W^m-\log\frac{b}{b+1}-\log\left(1-\frac{1}{\eta}\right)-\log P'$$
$$-\left(\frac{1}{\sigma}-\frac{1}{\eta}\right)\log F(k_j,l_j)=0.$$

$$FOC_K=\frac{1}{\sigma}\log k_j+\log R-\frac{\sigma-1}{\sigma}\log\theta_j-\log\left(1-\frac{1}{\eta}\right)-\log P'$$
$$-\left(\frac{1}{\sigma}-\frac{1}{\eta}\right)\log F(k_j,l_j)=0.$$

The total differential for this system of equations is:

$$\begin{bmatrix}\frac{\partial FOC_L}{\partial\log l_j} & \frac{\partial FOC_L}{\partial\log k_j}\\ \frac{\partial FOC_K}{\partial\log l_j} & \frac{\partial FOC_K}{\partial\log k_j}\end{bmatrix}\begin{bmatrix}d\log l_j\\ d\log k_j\end{bmatrix}$$
$$=\begin{bmatrix}-\frac{\partial FOC_L}{\partial\log P'}d\log P-\frac{\partial FOC_L}{\partial\log L^{Sm}}d\log L^{Sm}-\frac{\partial FOC_L}{\partial\log W^m}d\log W^m-\frac{\partial FOC_L}{\partial\log\theta_j}d\log\theta_j\\ -\frac{\partial FOC_K}{\partial\log P'}d\log P-\frac{\partial FOC_K}{\partial\log R}d\log R-\frac{\partial FOC_K}{\partial\log\theta_j}d\log\theta_j\end{bmatrix}\quad(A.3)$$

with

$$\frac{\partial FOC_L}{\partial\log l_j}=\left(\frac{1}{b}+\frac{1}{\eta}\right)+\left(\frac{1}{\sigma}-\frac{1}{\eta}\right)s_j,$$

$$\frac{\partial FOC_L}{\partial\log k_j}=-\left(\frac{1}{\sigma}-\frac{1}{\eta}\right)s_j,$$

$$\frac{\partial FOC_L}{\partial \log P'} = -1,$$

$$\frac{\partial FOC_L}{\partial \log L^{Sm}} = -\frac{1}{b},$$

$$\frac{\partial FOC_L}{\partial \log \mathrm{W}^m} = \frac{1}{b},$$

$$\frac{\partial FOC_L}{\partial \log \theta_j} = -\left(\frac{1}{\sigma} - \frac{1}{\eta}\right) s_j,$$

$$\frac{\partial FOC_K}{\partial \log l_j} = -\left(\frac{1}{\sigma} - \frac{1}{\eta}\right)(1 - s_j),$$

$$\frac{\partial FOC_K}{\partial \log k_j} = \frac{1}{\sigma} - \left(\frac{1}{\sigma} - \frac{1}{\eta}\right) s_j,$$

$$\frac{\partial FOC_K}{\partial \log P'} = -1,$$

$$\frac{\partial FOC_L}{\partial \log R} = 1,$$

and

$$\frac{\partial FOC_K}{\partial \log \theta_j} = -\frac{\sigma - 1}{\sigma} - \left(\frac{1}{\sigma} - \frac{1}{\eta}\right) s_j,$$

and where we used

$$\frac{\partial \log f(k_j, l_j)}{\partial \log k_j} = \frac{(\theta_j k_j)^{\nu}}{(\theta_j k_j)^{\nu} + l_j^{\nu}} := s_j$$

and

$$\frac{\partial \log f(k_j, l_j)}{\partial \log k_j} = \frac{l_j^{\nu}}{(\theta_j k_j)^{\nu} + l_j^{\nu}} = 1 - s_j.$$

s_j can be interpreted as the input share of capital efficiency units, and $1 - s_j$ as the input share of labor efficiency units.

Solving system (A.3) for $\begin{matrix} d\log l_j \\ d\log k_j \end{matrix}$ allows deriving comparative statics with respect to θ_j. This yields:

$$\frac{d\log l_j}{d\log \theta\theta_j} = \frac{(\eta-\sigma)bs_j}{\eta(1-s_j)+s_j\sigma+b}$$

$$\frac{d\log w_j}{d\log \theta_j} = \frac{\partial \log w_j}{\partial \log l_j}\frac{\partial \log l_j}{\partial \log \theta_j} = \frac{1}{b}\frac{\partial \log l_j}{\partial \log \theta_j} = \frac{(\eta-\sigma)s_j}{\eta(1-s_j)+s_j\sigma+b}$$

Because $\eta > 0$, $\sigma > 0$, $b > 0$, and $s_j \in [0,1]$, it follows that if $\eta > \sigma$, $d\log l_j/d\theta_j > 0$ and $d\log\ w_j/d\theta_j > 0$, proving part b of proposition 1.

We also get:

$$\frac{d\log k_j}{d\log \theta_j} = \frac{(b+1)s_j(\eta-\sigma)+(\sigma-1)(b+\eta)}{\eta(1-s_j)+s_j\sigma+b},$$

$$\frac{d\log k_j}{d\log \theta} - \frac{d\log l_j}{d\log \theta_j} = \frac{s_j(\eta-\sigma)+(\sigma-1)(b+\eta)}{\eta\left(1-s_j\right)+s_j\sigma+b},$$

$$\frac{ds_j}{d\theta_j} = \frac{\sigma-1}{\sigma}s_j^2\theta_j^{-\nu}\left(\frac{k_j}{l_j}\right)^{-\nu}\left[\left(\frac{k_j}{l_j}\right)^{-1}\frac{\partial\, k_j/l_j}{\partial\theta_j}+\theta_j^{-1}\right].$$

Thus, if $\eta > \sigma$ and $\sigma > 1$, firms with higher productivity parameter θ_j will employ more capital, will be more capital intensive, and will have a higher share of capital efficiency units $\left(s_j = \frac{(\theta_j k_j)^{\nu}}{(\theta_j k_j)^{\nu}+l_j^{\nu}}\right)$, proving part c of Proposition1.

2.A.2 Effects of Local Taxes τ^m on Firm Profits and Exit (Proposition 2)

The tax rate τ^m affects firm outcomes via the effective cost of capital $R = r\frac{1-\beta\tau^m}{1-\tau^m}$ with $\frac{dR}{d\tau^m} = r\frac{1-\beta}{(1-\tau^m)^2} > 0$. That is, a rise in the tax rate τ^m increases the effective cost of capital *R*. We therefore derive the effect of tax changes by doing comparative statics with respect to *R*.

Consider the profit function at optimal input choices given by (A.2). If the firm holds capital and labor inputs constant, then a rise in *R* merely

increases cost but does not affect revenue, and therefore unambiguously reduces profits. Any adjustments in capital and labor that the firm can do in response to this can at most partly offset the reduction in profits, but can never lead to higher profits than before, because otherwise the firm would not have maximized its profits prior to the increase in *R*. Therefore $\frac{d\Pi(k_j^*,l_j^*)}{dR} < 0$, profits at optimal labor and capital inputs are decreasing in R (and therefore also in the tax rate τ^m).

The threshold θ^* that makes a firm just indifferent to participation is implicitly defined as the value of θ_j that solves $\Pi(k_j^*, l_j^*) = 0$, where $\Pi(k_j^*, l_j^*)$ is given in (A.2).[32] The effect of R on θ^* is given by implicit differentiation as:

$$\frac{d\theta^*}{dR} = -\frac{\frac{\partial \Pi(k_j^*, l_j^*)}{\partial R}}{\frac{\partial \Pi(k_j^*, l_j^*)}{\partial \theta}} > 0.$$

Given $\frac{d\Pi(k_j^*,l_j^*)}{dR} < 0$ and $\frac{d\Pi(k_j^*,l_j^*)}{d\theta_j} > 0$ (as shown in Appendix 2.A.1 under the assumption $\eta > 1$), we get that $\frac{d\theta^*}{dR} > 0$. A rise in the cost of capital (and therefore a rise in the tax rate τ^m) increases the participation threshold θ^*, driving low-productivity firms out of business. This proves proposition 2. The threshold θ^* determines the set of firms operating in the local labor market.

2.A.3 Effects of Local Taxes τ^m on Firm Employment and Wages Conditional on Local Employment and Wages (Proposition 3)

Solving system (A.3) and deriving comparative statics with respect to R, holding L^{mS} and W^m constant, yields

$$\frac{d \log l_j^*}{d \log R}\Big|_{L^{mS}, W^m} = -\frac{(\eta - \sigma) b s_j}{\eta(1 - s_j) + s_j \sigma + b},$$

[32] $\Pi(k_j^*, l_j^*)$ in equation A.2 depends implicitly on θ_j through optimal capital and labor choices l_j^* and k_j^* and through the occurrence of θ_j as technology factor in the production function.

proving part a) of proposition 3.

Since $\frac{\partial \log w_j}{\partial \log l_j} = \frac{1}{b}$ from the firm's labor supply curve, we get

$$\frac{d \log w_j^*}{d \log R}|_{\mathrm{L}^{mS},\mathrm{W}^m} = \frac{\partial \log w_j^*}{\partial \log l_j^*} \frac{d \log l_j^*}{d \log R}|_{\mathrm{L}^{mS},\mathrm{W}^m} = -\frac{(\eta - \sigma)s_j}{\eta(1 - s_j) + s_j\sigma + b}$$

proving part b) of proposition 3.

The magnitude of this wage effect depends positively on the share of capital efficiency units:

$$d\left|\frac{d \log w_j^*}{d \log R}|_{\mathrm{L}^{Sm},\mathrm{W}^m}\right| / ds_j = \frac{|\eta - \sigma|(\eta + b)}{\left[\eta(1 - s_j) + s_j\sigma + b\right]^2} > 0.$$

Moreover, if $\eta > \sigma$ and $\sigma > 1$, which ensures that $\frac{ds_j}{d\theta_j} > 0$ (see Appendix 2.A.1), we get

$$d\left|\frac{d \log w_j^*}{d \log R}|_{\mathrm{L}^{Sm},\mathrm{W}^m}\right| / d\theta_j = d\left|\frac{d \log w_j^*}{d \log R}|_{\mathrm{L}^{Sm},\mathrm{W}^m}\right| / ds_j \ \frac{ds_j}{d\theta_j} > 0.$$

That is, that the magnitude of the wage effect also increases in the productivity parameter θ_j, proving part c) of Proposition 3.

2.A.4 Effects of Local Taxes τ^m on Local Employment and Wages (Proposition 4)

The local labor market equilibrium requires labor supply to the local labor market to equal local labor demand:

$$log\, L^{Sm} = log \sum_{j \epsilon J^{m*}} l_j^*.$$

Local labor demand is the aggregation of the individual firm's labor demand. Local labor supply is given by:

$$logL^{Sm} = logN^{m} + \frac{\log \mathrm{W}^{m}}{\lambda} - \log\left(\exp\left(\frac{\log \mathrm{O}}{\lambda}\right) + \exp\left(\frac{\log \mathrm{W}^{m}}{\lambda}\right)\right).$$

The local labor supply elasticity therefore equals:

$$\frac{dlogL^{mS}}{dlogW^{m}} = \frac{1}{\lambda}\frac{exp\left(\frac{logO}{\lambda}\right)}{exp\left(\frac{logO}{\lambda}\right) + exp\left(\frac{logW^{m}}{\lambda}\right)} \equiv \delta.$$

Some firms will exit, and surviving firms will reduce their labor demand (holding local wages and labor supply constant) in response to an increase in the effective cost of capital (i.e., $\frac{d\theta^{*}}{dR} > 0$ and $\frac{d\log l_j^{*}}{d\log R}|_{\mathrm{L}^{mS},\mathrm{W}^{m}} < 0$ if $\eta > \sigma$, derived in Appendices 2.A.2 and 2.A.3 under the assumption $\eta > \sigma$). Both effects translate to an aggregate effect $\frac{dlog\sum_{j\epsilon Jm} l_j^{*}}{d\,log\,R}|_{L^{mS},W^{m}} < 0$, which is a downward shift of the local labor demand curve. Because the size of each individual firm's effect depends on the level of θ_j, the size of the aggregate labor demand shift will depend on the distribution of θ_j (firm types). In response to the downward labor demand shift local wages and/or local labor supply must decline to preserve the local labor market equilibrium.

The size of the local wage and employment adjustment for a given downward shift in labor demand will depend on the local labor supply elasticity δ. If local labor supply is inelastic ($\delta = 0$), the local labor market will adjust entirely via a drop in the aggregate wage W^{m}, leaving equilibrium employment unchanged. If local labor supply is perfectly elastic ($\delta \to \infty$), the local labor market will instead adjust entirely via a decline in employment, leaving the aggregate wage level W^{m} unchanged. For intermediate values of the elasticity, the adjustment will be partly via a drop in employment, and partly via a drop in wages. Hence, if $\eta > \sigma$:

$$\frac{d\log W^{m}}{d\log R} \leq 0$$

and

$$\frac{d\log L^{m}}{d\log R} \leq 0.$$

Since the adjustment corresponds to a shift along the labor supply curve, we also have

$$\frac{d\log W^m}{d\log R} = \frac{1}{\delta}\frac{d\log L^m}{d\log R}.$$

2.A.5 Firms' Employment and Wage Choices Unconditional on Local Wages and Labor Supply

Solving system (A.3) and deriving comparative statics with respect to L^{mS} and W^m yields

$$\frac{d\log l_j}{dL^{Sm}} = \frac{\eta\left(1-s_j\right)+s_j\sigma}{\eta\left(1-s_j\right)+s_j\sigma+b} > 0$$

and

$$\frac{d\log l_j}{dW^m} = -\frac{\eta\left(1-s_j\right)+s_j\sigma}{\eta\left(1-s_j\right)+s_j\sigma+b} < 0.$$

A reduction in labor supply to the local labor market therefore reinforces the effect of a tax rise on individual firms—reduced local labor supply means that firms need to pay higher wages to attract the same number of workers (corresponding to a downward shift of labor supply to the individual firm). In contrast, a reduction in local wages dampens the effect of a tax rise on individual firms—reduced local wages allow firms to attract the same number of workers at a lower wage (corresponding to an upward shift of labor supply to the individual firm). Putting both elements of the feedback effect together yields:

$$\begin{aligned}
&\frac{d\log l_j}{dL^{Sm}}\frac{d\log L^{Sm}}{d\log R} + \frac{d\log l_j}{dW^m}\frac{d\log \mathrm{W}^m}{d\log R} \\
&\quad = \frac{\eta(1-\mathrm{s_k})+s_j\sigma}{\eta(1-\mathrm{s_k})+s_j\sigma+b}\frac{d\log\sum_{j\epsilon J^m} l_j^*}{d\log R}\Big|_{\mathrm{L}^{mS},\mathrm{W}^m} \\
&\quad - \frac{\eta\left(1-s_j\right)+s_j\sigma}{\eta\left(1-s_j\right)+s_j\sigma+b}\frac{1}{\delta}\frac{d\log\sum_{j\epsilon J^m} l_j^*}{d\log R}\Big|_{\mathrm{L}^{mS},\mathrm{W}^m} \\
&\quad = \left(\frac{\delta-1}{\delta}\right)\frac{\eta(1-\mathrm{s_k})+s_j\sigma}{\eta(1-\mathrm{s_k})+s_j\sigma+b}\frac{d\log\sum_{j\epsilon J^m} l_j^*}{d\log R}\Big|_{\mathrm{L}^{mS},\mathrm{W}^m}
\end{aligned}$$

That is, if $\delta > 1$, local labor market adjustments will have a net reinforcing feedback effect, while for $\delta < 1$, they will have a net dampening effect.

2.A.6 Employment Effects of Business Taxation When Product and Labor Markets are Frictionless and Perfectly Competitive

Solving system (A.3) and deriving comparative statics with respect to R, holding L^{mS} and W^m constant, yields

$$\frac{d\log l_j^*}{d\log R}|_{\mathrm{L}^{mS},\mathrm{W}^m} = -\frac{(\eta-\sigma)bs_j}{\eta(1-s_j)+s_j\sigma+b}$$

and

$$\frac{d\log k_j^*}{d\log R}|_{\mathrm{L}^{mS},\mathrm{W}^m} = -\frac{(\eta-\sigma)bs_j+\sigma(b+\eta)}{\eta(1-s_j)+s_j\sigma+b}.$$

Suppose that $\delta \to \infty$ (perfectly elastic labor supply to the local economy). In this case, wages equalize across local labor markets (in our simplified set-up, local wages must be equal to workers' outside option). In consequence, wages do not adjust in response to a business tax hike and equilibrium local labor market adjustment becomes irrelevant for firms' labor demand. Thus, $\frac{d\log l_j^*}{d\log R}|_{L^{mS},W^m} = \frac{d\log l_j^*}{d\log R}$ *and* $\frac{d\log k_j^*}{d\log R}|_{L^{mS},W^m} = \frac{d\log k_j^*}{d\log R}$.

If in addition labor supply to the individual firm is perfectly elastic (perfectly competitive labor market), firms' employment and capital adjustments simplify to:

$$\lim_{\delta,b\to\infty,} \frac{d\log l_j^*}{d\log R} = -(\eta-\sigma)s_j$$

and

$$\lim_{\delta,b\to\infty} \frac{d\log k_j^*}{d\log R} = -(\eta-\sigma)s_j+\sigma.$$

For a competitive product market, on the other hand, the effects reduce to:

$$\lim_{\delta,\eta\to\infty} \frac{d\log l_j^*}{d\log R} = -\frac{bs_j}{1-s_j}$$

and

$$\lim_{\delta,\eta\to\infty} \frac{d\log k_j^*}{d\log R} = -\frac{bs_j + \sigma}{1 - s_j}.$$

For fully competitive labor and product markets, in turn, all economic activity disappears from the local economy:

$$\lim_{\delta,b,\eta\to\infty} \frac{d\log l_j^*}{d\log R} = -\infty$$

and

$$\lim_{\delta,b,\eta\to\infty} \frac{d\log k_j^*}{d\log R} = -\infty$$

Hence, some forms of market imperfections are necessary for business tax rates to vary across local economies.

Appendix 2.B: Data

Appendix 2.B.1 Harmonization of Education and Full-Time Status

Education.

The categorization of workers in education groups is performed following Fitzenberger et al. (2006):

a) we group the original education variable into three categories: 1) secondary education (no completion of high school or of an apprenticeship) 2) post-secondary education (completion of high school (Abitur) or of an apprenticeship) 3) tertiary education (completion of a university degree)

b) for each worker-year observation, missing values in the education variable are imputed using non-missing values of adjacent years.

c) we assign one, time-constant, education category to each worker by using the mode of the worker's education observations.

Finally, we categorize workers into three skill groups: low-skilled workers enter the labor market without post-secondary education, medium-skilled workers completed an apprenticeship or graduated from high school, high-skilled workers graduated from university.

Full-Time Status.

The Social Security Records data provide an indicator of the worker's part-time and full-time status. In 2011 there was a change in the employers' reporting procedure that increased the share of missing observations from less than 1% to 30% in the raw data (Fitzenberg et al., 2020). The new reporting procedure also increased the share of workers reported in part-time work from 2012 onwards (Fitzenberg et al., 2020). As the new reporting procedure made reporting part-time status more salient, some workers reported working part-time after 2011 were incorrectly reported as working full-time before 2011. To correct for this, we follow Fitzenberg et al. (2020) and we estimate the probability of working part-time based on observable characteristics, such as the wage and sector of work, to reweight the full-time spells potentially misreported. This correction is performed separately by men and women, as a much larger share of women works in a part-time job.

Appendix 2.B.2 Definition of Liable Establishments

Firms with a commercial activity are liable to the local business tax, regardless of whether they are incorporated or unincorporated. Our analysis is focused on liable firms only, with non-liable firms excluded from our sample. Firm liability is defined in the German code[33] and depends on the industry affiliation and, in some cases, the legal form of the firm. Non-liable firms include firms in agriculture and the public sector and those of some practitioners in certain professions, such as accountancy, law, journalism, medicine**,** and art. Given the lack of information about the legal form in our data, that would be useful in assessing the status of commercial activity of some practitioners, we identify liable firms based on the 5 digits industry code. The 5 digits industry code, with 840 industrial categories, allows for a very detailed industry definition. We drop from our sample all establishments in the agriculture, farming and forestry sector, fisheries employing less than 7 employees, and liberal professionals such as lawyers, journalists, accountants, architects, researchers, artists,

[33] Gewerbesteuergesetz (GewStG) http://www.gesetze-im-internet.de/gewstg/

physicians, the few firms in the public and education sectors, as well as public and video libraries and museums.

Appendix 2.C: Empirical Analysis

2.C.1 Decomposing the Local Employment Decline into Within-Establishment Declines and Establishment Entry and Exit (Figure 3)

Figure 2.3, Panel A. Changes in local employment between period t and $t-\tau$ can be decomposed as follows:

$$\frac{E_{m,t}-E_{m,t-\tau}}{E_{m,t-\tau}} = \underbrace{\frac{E^{con}_{m,t-\tau}}{E_{m,t-\tau}}\frac{E^{con}_{m,t}-E^{con}_{m,t-\tau}}{E^{con}_{m,t-\tau}}}_{\substack{(1)\text{within-establishment}\\ \text{adjustments}}} + \underbrace{\frac{E^{\text{entry}}_{m,t}}{E_{m,t-\tau}}}_{\substack{\text{establishment}\\ \text{entry}}} + \underbrace{\frac{E^{\text{exit}}_{m,t}}{E_{m,t-\tau}}}_{\substack{\text{establishment}\\ \text{exit}}} + \underbrace{\frac{E^{\text{reall,entry}}_{m,t}-E^{\text{reall,exit}}_{m,t}}{E_{m,t-\tau}}}_{\text{reallocation}}$$

The first component captures within-establishment employment adjustments. This term is equal to the product between the employment share of continuing firms in the base period ($\frac{E^{con}_{m,t-\tau}}{E_{m,t-\tau}}$) and employment declines within establishments ($\frac{E^{con}_{m,t}-E^{con}_{m,t-\tau}}{E^{con}_{m,t-\tau}}$). The second and third terms capture local employment changes due to establishment entry and exit (i.e., the establishment identification number appears or disappears from the data base), while the fourth component captures local employment changes due to establishment relocation to another municipality (i.e., the establishment is located in a different municipality in t than in period t than in period $t-\tau$).

In Panel A of Figure 2.3, we use each of the four components as dependent variables in regression equation (4) (first difference design). The coefficients add up to the total decline in local employment following the business take hike from the first difference specification (i.e., column (4) in Table 2.3).

Figure 2.3, Panels B and C. The change in local employment due to establishment exit, $\frac{E_{m,t}^{exit}}{E_{m,t-\tau}}$, can be decomposed into components representing different establishment types, such as low-, medium- and high-paying establishments (or small, medium or large establishments):

$$\frac{E_{m,t}^{\text{exit}}}{E_{m,t-\tau}} = \frac{E_{m,t-\tau}^{\text{Low}}}{E_{m,t-\tau}} \frac{E_{m,t}^{\text{Low,exit}}}{E_{m,t-\tau}^{\text{Low}}} + \frac{E_{m,t-\tau}^{\text{Medium}}}{E_{m,t-\tau}} \frac{E_{m,t}^{\text{Medium,exit}}}{E_{m,t-\tau}^{\text{Medium}}} + \frac{E_{m,t-\tau}^{\text{High}}}{E_{m,t-\tau}} \frac{E_{m,t}^{\text{High,exit}}}{E_{m,t-\tau}^{\text{High}}}.$$

.

In Panels B and C of Figure 2.3, we use each of the three components as dependent variables in regression equation (4) (first difference design). The coefficients add up to the tax-induced decline in local employment attributable to establishment exit. It should be noted that the three groups of establishments roughly employ the same number of workers (i.e., $\frac{E_{m,t-\tau}^{Low}}{E_{m,t-\tau}} \approx \frac{E_{m,t-\tau}^{Medium}}{E_{m,t-\tau}} \approx \frac{E_{m,t-\tau}^{High}}{E_{m,t-\tau}}$).

Appendix 2.C.2 Backing Out the Labor Supply Elasticity to Firms

Firms' labor supply unconditional on working in the local economy equals:

$$logl_j^* = \log N^m + \frac{1}{\lambda}\log \mathrm{W}^m - \log\left(\exp\left(\frac{\log 0}{\lambda}\right) + \exp\left(\frac{\log \mathrm{W}^m}{\lambda}\right)\right) + b log w_j^* - log W^m.$$

Totally differentiating this expression with respect to the business tax yields:

$$\frac{dlogl_j^*}{dlog\tau} = \underbrace{\delta \frac{dlogW^m}{dlog\tau}}_{\substack{\text{as in a competitive} \\ \text{local labor market:how many} \\ \text{workers work in local economy}}} + \underbrace{b\frac{dlogw_j^*}{dlog\tau} - \frac{dlogW^m}{dlog\tau}}_{\substack{\text{consequence of monopsonistic} \\ \text{competition:how workers are allocated} \\ \text{to firms in local economy}}}$$

where δ is the labor supply elasticity to the local economy and b is the labor supply elasticity to the firm. Next, consider the difference in the employment response between two firm groups j and j' (i.e., small and large firms):

$$\left(\frac{dlogl_j^*}{dlog\tau} - \frac{dlogl_{j'}^*}{dlog\tau}\right) = b\left(\frac{dlogw_j^*}{dlog\tau} - \frac{dlogw_{j'}^*}{dlog\tau}\right).$$

We can therefore obtain an estimate of the labor supply elasticity of the firm by dividing the difference in the employment response of two firm types by the difference in the wage response of the two firm types. We obtain an estimate for the labor supply elasticity to the firm of 2.1 ((-1.37+0.0043)/(-0.8+0.18)) when using heterogenous responses across establishment wage types, and of 4.4 (-1.1+0.89)/(-0.42+0.15)) when using heterogeneous responses across establishment wage types.

In a monopsonistic labor market, wages are below the marginal revenue product of labor (i.e., Manning 2003):

$$w_j = \underbrace{p_j F_L(k_j, l_j)}_{\substack{\text{marginal revenue} \\ \text{product of labor}}} \underbrace{\frac{b}{b+1}}_{\text{wedge}}$$

A labor supply elasticity to the firm of between 2 and 4 therefore implies a wedge of between one third and one fifth (20-33%).

Appendix 2.C.3 Decomposing the Local Employment Decline into Hiring and Separations (Table 2.7)

Changes in local employment between period t and $t-\tau$ can be decomposed as follows:

$$\frac{E_{m,t} - E_{m,t-\tau}}{E_{m,t-\tau}} = \underbrace{\frac{\text{Hires}_{m,(t-\tau,t)}}{E_{m,t-\tau}}}_{\text{hiring rate}} - \underbrace{\frac{\text{Sep}_{m,(t-\tau,t)}}{E_{m,t-\tau}}}_{\text{separation rate}}$$

where $Hires_{m,(t-\tau,t)}$ denotes the number of employees who were hired between $t-\tau$ and t by one of the establishments in the municipality and $Sep_{m,(t-\tau,t)}$ denotes the number of employees who separated from one of the establishments in the municipality between $t-\tau$ and t.[34]

In Panel A of Table 2.7, we use the hiring and separation rate as dependent variables in the first difference specification given by regression

[34] Workers who switch between establishments in the municipality count as both hires and separations and leave total employment in the municipality unchanged.

equation (4), focusing on effects three years after the tax change. The coefficients add up to the total tax-induced employment decline in the municipality obtained from the first difference design.

In Panel B, we break down hires and separations into hires from employment and non-employment, depending on workers' labor market status in $t-\tau$ and *t*, respectively. In Panel C, we additionally break down hires and separations from employment into employment in an establishment in the same municipality, a different municipality but same commuting zone, and a different commuting zone, depending on where workers were or are employed in periods $t-\tau$ and *t*, respectively.

Appendix 2.C.4 Decomposing Separation Rates into Non-Employment by Establishment Type (Panel A of Table 2.8)

The separation rate into non-employment, $\frac{Sep^{Non}_{m,(t-\tau,t)}}{E_{m,t-\tau}}$, can be decomposed into components representing different establishment types such as low-, medium- and high-paying establishments (or small, medium-sized and large establishments):

$$\frac{\mathrm{Sep}^{\mathrm{Non}}_{m,(t-\tau,t)}}{E_{m,t-\tau}} = \frac{E^{\mathrm{Low}}_{m,t-\tau}}{E_{m,t-\tau}} \frac{\mathrm{Sep}^{\mathrm{Non,Low}}_{m,(t-\tau,t)}}{E^{\mathrm{Low}}_{m,t-\tau}} + \frac{E^{\mathrm{Medium}}_{m,t-\tau}}{E_{m,t-\tau}} \frac{\mathrm{Sep}^{\mathrm{Non,Medium}}_{m,(t-\tau,t)}}{E^{\mathrm{Medium}}_{m,t-\tau}} + \frac{E^{\mathrm{High}}_{m,t-\tau}}{E_{m,t-\tau}} \frac{\mathrm{Sep}^{\mathrm{Non,High}}_{m,(t-\tau,t)}}{E^{\mathrm{High}}_{m,t-\tau}},$$

where $Sep^{Non,Low}_{m,(t-\tau,t)}$, $Sep^{Non,Medium}_{m,(t-\tau,t)}$ and $Sep^{Non,High}_{m,(t-\tau,t)}$ denotes, respectively, the number of workers who were employed in a low-, medium- or high-paying establishment and separated from the establishment between $t-\tau$ and t. In Panel A of Table 2.8, we use each of the three components as dependent variables in regression equation (4) (first difference design). The coefficients add up to the effects of business taxation on separation rates into non-employment. It should be noted that the three groups of establishments roughly employ the same number of workers (i.e., $\frac{E^{Low}_{m,t-\tau}}{E_{m,t-\tau}} \approx \frac{E^{Medium}_{m,t-\tau}}{E_{m,t-\tau}} \approx \frac{E^{High}_{m,t-\tau}}{E_{m,t-\tau}}$). Moreover, for our decompositions by AKM fixed effect, there typically also is a fourth group of firms where the fixed effects group is unknown, because they

have not been in the market long enough in order to have a valid AKM fixed effect estimate in the year before the tax change. This explains why the effects for low, medium, and high-paying firms in Panel A, Table 2.8, do not add up exactly to the total employment effect.

Appendix 2.C.5 Decomposing Individual Wage Growth into Within- and Between-Establishment Components (Panels B and C of Table 2.9)

Individual wage growth can be decomposed as follows:

$$logw_{it} - logw_{it-\tau} = \underbrace{\Delta logw_{it-\tau}^{\text{stay}}}_{\substack{\text{wage growth of} \\ \text{establishment stayers}}} + \underbrace{\frac{\text{Sep}_{m,(t-\tau,t)}^{\text{Emp}}}{E_{m,t-\tau}}}_{\substack{\text{share workers moving} \\ \text{to another establishment}}} \underbrace{\left(\Delta logw_{it-\tau}^{\text{move}} - \Delta logw_{it-\tau}^{\text{stay}}\right)}_{\substack{\text{gains from moving to a} \\ \text{new establishment}}}$$

A local business tax increase may lower wage growth of workers who remain employed with their establishment. We interpret this effect as the direct effect of the business tax on individual wage growth, as investigated in Section 2.3.2. In addition, a local business tax increase lowers the probability of a job-to-job movement (see Panel A of Table 2.9) and reduces the gains from moving to a new establishment (see Panels B and C of Table 2.9). In the last column of Panels B and C in Table 2.9, we compute the share of overall individual wage growth that can be attributed due to disruptions in the job ladder as 1 minus the effects of the business tax increase on the wage growth of establishment stayers divided by the effects on overall individual wage growth (i.e., 1-0.29/0.95=0.695 for workers who were employed in a low-paying establishment before the tax increase). This share captures the effects of a business tax increase on both the probability of moving and the gains from moving to a new establishment.

Chapter 3

Hiring Subsidies and Female Employment

3.1 Introduction

Women remain underrepresented in the labour market, which hinders progress towards economic growth and social well-being. A more gender-equal European Union could provide an increase in GDP and a higher level of employment and productivity (EIGE, 2019). The economic loss in the European Union due to the underrepresentation of women in the labor market amounts to €370 billion per year[35]. In the European Union, the gender employment gap, namely the difference between the employment rate of men and that of women, stands at 10.4 percentage points (Eurostat Database, 2021a). Decreasing the gender employment gap would lead to an additional 10.5 million jobs in Europe by 2050 and to a decrease in the number of people in poverty, since women are affected by poverty more often than men because of lower employment, a higher share of part-time employment and, in turns, lower salary (EIGE, 2016).

Italy, the focus of this paper, is of particular interest as it has the largest gender employment gap in the European Union, at 20.1 percentage points (Eurostat Database, 2021a). The employment rate of mothers is particularly striking in Italy. On average, only 53.6% of mothers with a young child are employed, compared to 62.6% of women without children and 87.7% of fathers (Eurostat Database, 2021b). Grasping the employment opportunities of women with career gaps, including those of women with children, is instrumental to an understanding of gender inequalities in the labor market. Indeed, a large part of the gender inequality in the labor market of developed countries can be accounted for by the arrival of children

[35] https://ec.europa.eu/info/policies/justice-and-fundamental-rights/gender-equality/women-labour-market-work-life-balance/womens-situation-labour-market_en

(Kleven, Landais, and Søgaard, 2019; Kleven, Landais, Posch, Steinhauer, and Zweimüller. 2019; Cortés and Pan, 2020).

In this paper, we investigate a policy instrument aimed at increasing female employment in Italy. This policy consists of a hiring subsidy, namely a temporary cut to the employer's social security contributions on new hires, paid to firms to hire women after significant employment interruption. While such policies are often discussed as a policy lever to reduce labor costs for firms and have been applied in both the U.S. and Europe to increase employment, particularly of low-wage workers (Neumark and Grijalva, 2017; Cahuc, Carcillo and Le Barbanchon, 2019), this study is the first to investigate gender-specific hiring subsidies aimed at reducing gender differences in employment. We exploit a 2013 Italian hiring subsidy which offered a one-year, large--fifty percent--cut to employer's social security contributions to hire women who were out of employment. The policy aimed to stimulate female employment in Italy by effectively making hiring women cheaper than hiring men.

We draw on unusually rich data that encompass the population of workers and firms covered by the social security system in Italy, providing detailed information on firms that use the subsidy (*hiring-subsidized firms*) and the workers that are hired under the subsidy (*subsidized workers*). The information the data provides on the adoption of the subsidy in tandem with the data panel structure allows us to go beyond the intention to treatment effects and focus on the hiring-subsidized firms and on the subsidized workers. The data provides information on the (past) characteristics of subsidized workers, such as length of the non-employment spell, last wage before the non-employment spell (which can be used to proxy for workers' productivity), and maternity leave take-up, which allows researchers to identify mothers among female workers. Thanks to the panel structure of the data, we are able to follow the evolution of hiring decisions by the hiring-subsidized firms and the career of subsidized workers after firms' use of the subsidized hiring .

Our data suggest that the hiring-subsidized firms differ from the average Italian firm along multiple dimensions. The hiring-subsidized firms are, for

example, bigger and have a larger share of women among their employees. A comparison between the hiring-subsidized firms and the average Italian firm will unlikely uncover the effect of the hiring subsidy on a firm's hiring decisions. Thus, for the analysis, we combine a difference-in-differences estimator with an event-study approach. We first construct, for each hiring-subsidized firm, a control firm by matching on an extensive set of predetermined firm characteristics, such as size and share of women among employees, which ensures that we compare firms that evolved similarly in the years before the adoption of the hiring subsidy. The estimation then flexibly compares changes in the hiring pattern of hiring-subsidized firms and control firms. Our identifying assumption is that control firms form a valid counterfactual for the hiring-subsidized firms, conditional on our control variables. We can partially assess the plausibility of this assumption by comparing trends in outcome variables between the two groups in the years before the adoption of the subsidy.

Our findings show that the composition of new hires at hiring-subsidized firms is different from that of control firms at the time of adoption of the subsidy; whereas it does not differ in the years before the adoption of the hiring subsidy. Indeed, hiring-subsidized firms increase their hiring of women with lengthy labor market interruptions (*long-term non-employed*), and women who are mothers. This is an important finding as lengthy labor market interruptions after childbearing can lead to a loss of skills, making re-entry into the labor market difficult for mothers (Adda, Dustmann and Stevens, 2017). Although they are long-term non-employed, these new female hires, who possess higher education and a higher wage earned in their previous job, seem more positively selected than the average firm's hire on other observables. Finally, preliminary analysis shows that these new female hires seem more likely to remain employed in the firm in the medium-run and have an increased probability of obtaining secure employment compared to the average firm's hire, with their contract changed from a fixed-term to an open-ended contract[36]. The change of contract from fixed-term to

[36] Under the Italian legislation, an open-ended contract is a contract with no time limit. Usually, the contract ends by mutual agreement between the employer and the employee, as firing without reason

open-ended is incentivized by the subsidy's legislation, as the length of the hiring subsidy is extended from 12 to 18 months when the contract under which the worker is hired is transformed into an open-ended contract.

Although these female hires seem more positively selected than the average hire on some observables, such as education, they also have longer non-employment spells, which could lead to human capital depreciation and, in turn, to a decrease in productivity (Adda et al., 2017; Blundell, Costa Dias, Meghir and Shaw, 2016). Our findings suggest that firms needed a policy incentive to hire these workers. One reason why firms might not have hired these workers in absence of the subsidy could be that firms perceive their productivity to be low. The hiring subsidy–by making the subsidized workers cheaper compared to the other workers in the labor market–may allow firms that use it to learn about the potential productivity of these workers. We provide some preliminary evidence of this firm-learning mechanism by investigating whether the quality of the subsidized workers hired in the first year has an effect on future hiring patterns. Our findings suggest that firms that hire a more productive female worker under the subsidy in the first year, hire more subsidized workers in subsequent years. In addition, the new female hires hired in subsequent years also have similar characteristics: they are long-term non-employed, middle-skilled female workers.

Our research contributes to several bodies of literature**.** A principal contribution is that our study relates to the empirical debate on the effectiveness of hiring credits. These policies have been applied to both the U.S. and Europe and are often discussed as a policy lever to reduce the unemployment of low-wage workers (Katz, 1998); more recently, they have been used to increase employment during economic recessions (Neumark, 2013). Empirical studies on hiring subsidies date back to the 1980s and focus on the effect of hiring subsidies at improving the employment rate of long-term unemployed workers (Perloff and Wachter, 1979; Bishop, 1981). The recent evidence concerning hiring subsidies focuses on job growth during economic recessions (consider Neumark et al., 2017 for the U.S. and

is impossible under Italian Law. Firing for economic reasons is now possible but needs to be extensively justified.

Cahuc et al., 2019 for France[37]). These studies find that hiring credits are effective at increasing employment, especially during recessions. We provide two main contributions to this literature. First, we focus on a hiring subsidy targeted at women not in employment. This is a novel group, as the hiring subsidy usually analysed in the literature is targeted at disadvantage workers (Perloff et al., 1979) or low-wage workers (Neumark et al., 2017; Cahuc et al., 2019). Second, ours is the first study that relies on administrative data on the universe of private firms and workers, with information on the adoption of the subsidy at both the firm and worker level[38]. This allows us to conduct novel analysis. We zoom into the firms that use the hiring subsidy and investigate their medium-run behaviour using an event study approach, which allows us to investigate how firm hiring decision change after the adoption of the subsidy. Finally, the ability to follow workers over time permits us to zoom into subsidized workers characteristics and their career evolution.

This study also contributes to the literature that examines governmental policies that influence female labor supply. The literature is mostly focused on the effect of maternity leave and subsidized childcare, as these are the most popular governmental policies aimed at increasing female employment in developed countries (Olivetti and Petrongolo, 2017) [39]. However, these governmental policies have only had limited success in improving mothers' labor force attachment. Within country studies which

[37] Other studies that analyse cuts to the employer's social security contributions in European countries mainly focus on permanent cuts (in essence**,** cuts to payroll taxation). Most notably, these include Saez, Matsaganis, and Tsakloglou (2012) on Greece; Saez, Schoefer and Seim (2019) and Saez, Schoefer, and Seim (2021) on Sweden; Benzarti and Harju (2021a) and Benzarti and Harju (2021b) on Finland; and Ku, Schönberg, and Schreiner (2020) on Norway. A cut to payroll taxation applies to both new hires and to workers already employed in the firm and is automatically applied. As hiring subsidies only apply to new hires and are not automatically applied, findings from the payroll tax cut literature are only partially informative about hiring subsidies.

[38] Cahuc et al. (2019) and Rubolino (2022) also rely on administrative data. However, Cahuc et al. (2019) have access only to firm level administrative data and Rubolino (2022), who also analyses the 2013 Italian subsidy, does not fully exploit the information on the adoption of the hiring subsidy at the firm level, relying instead on the intention to treatment effects for the firm level analysis.

[39] There also exists a vast literature investigating factors that could affect female labor force participation. Research has shown that female labor force participation can be influenced, among other factors, by cultural and social norms (Fernández 2007; Alesina, Giuliano and Nunn, 2013; Boelmann, Raute, and Schönberg, 2021), biological differences (Ichino and Moretti 2009); legal rights (Doepke and Tertilt 2009); and industrial structure (Olivetti and Petrongolo 2016).

have exploited the extension of maternity leave rights find that that parental leave extensions delay mothers' return to work in the short-term, but have no long-run effects on women's employment (Baker and Milligan, 2008 on Canada; Lalive and Zweimüller, 2009 and Lalive, Schlosser, Steihauer, and Zweimüller, 2013 on Austria; Schönberg and Ludsteck, 2014 on Germany; Dahl, Løken, Mogstad, and Salvanes, 2016 for evidence on Norway). Within country studies that investigate the effect of subsidised child-care find positive, but small, effects on the employment of mothers (see Cascio and Schanzenbach, 2013 on the U.S.; Havnes and Mogstad, 2011 on Norway; Givord and Marbot, 2015 on France; Bettendorf, Jongen, and Muller, 2015 on the Netherlands; Nollenberger and Rodríguez-Planas, 2015 on Spain; Kleven, H., Landais, C., Posch, J., Steinhauer, A., and Zweimüller, J., 2020 on Austria; Lefebvre, Merrigan, and Verstraete (2009) and Haeck, Lefebvre, and Merrigan (2015) on Canada--notably, they find positive and long-lasting effects of childcare subsidy on the employment of mothers in the long-run). We contribute to this literature by analysing a novel governmental policy, a hiring subsidy, targeted at women not in employment–that is, not only targeted at mothers–and aimed at increasing female employment. Our findings suggest that firms applying the hiring subsidy increase their hiring of long-term non-employed women and of mothers. In addition, we find that hiring-subsidized women remain employed long-term in the firm, with a higher likelihood of obtaining an open-ended contract (compared to the average firm's hire).

Finally, this study contributes to the literature on firm learning and discrimination. The recent research holds that employers may be uncertain about both the individual productivity of potential workers and the underlying productivity of their group (Lepage, 2021). A source of learning, then, is the employer experience with workers through hiring. The initial signal received on the quality of such workers might affect the subsequent behaviour of firms (Bardhi, Guo and Strulovici, 2020, Lepage, 2021; Leung, 2018). We offer to the literature an investigational study of a governmental policy, the hiring subsidy, which reduces the cost of hiring long-term non-employed women compared to other groups and which may help firms learn about the

productivity of this group. Our findings suggest that the introduction of the hiring subsidy increased the hiring of long-term non-employed women by the hiring-subsidized firms and that firms that hire a more productive female worker under the subsidy in the first year hire more subsidized workers—that is, workers from the same group—in subsequent years.

This study is organised as delineated below. Section 3.2 describes the institutional setting in Italy, that is, the gender employment gap, the wage gap and the legislation of the hiring subsidy implemented in 2013**.** Section 3.3 presents our data, the descriptive statistics of the female workers hired under the hiring subsidy, the firms that adopt it and the attention rate of the policy[40]. Section 3.4 presents the empirical strategy. Section 3.5 presents the difference-in-differences estimates and robustness. Section 3.6 concludes, discusses findings, and explores areas of future research.

3.2 Institutional Background and Bonus Donne

3.2.1 Female Employment and Gender Gap in Italy

Women's employment and labor force participation in Italy are low compared to other developed countries: indeed, the gender employment gap, the difference between the employment rate of men and that of women, lies at 20.1 percentage points (Eurostat Database, 2021a). The Italian gender employment gap is nearly ten percentage points larger than the European average, which measured 10.4 percentage points in 2021, and is the largest among European countries (Eurostat Database, 2021a). The employment rate of women with children is particularly low, as on average 53.6 percent of mothers with children are employed, compared to 62.6 percent of women without children and 87.7 percent of fathers (Eurostat Database, 2021b).

On the other hand—and perhaps not surprisingly—in Italy, the gender wage gap, the difference in the average wage earned by men and that earned by women, is low; at 4.2 percent (Eurostat Database, 2020), it is one of the lowest among European countries. This small difference in average

[40] The attention rate is the share of workers hired under the subsidy as a share of eligible workers

wages between men and women could be explained by the positive selection into employment of high-skilled Italian women. Indeed, accounting for women out of the labor force and for selection into employment would cause the wage gap in Italy to rise dramatically: the Italian gender wage gap would increase five-fold (as discussed in Olivetti and Petrolongo, 2008).

3.2.2 The Hiring Subsidy, The "Bonus Donne"

3.2.2.1 The Legislation

The hiring subsidy legislation, the so-called "Bonus Donne", was voted into law on June 28, 2012 (as Law 92/2012), and came into force on January 1st 2013. The subsidy provides a 50 percent temporary cut to employer's social security contributions for the hiring of women not in employment. In Italy, the employer's share of the social security contribution is 21.6% of gross salary[41]; the hiring subsidy thus provided a 10.3% decrease to firms' cost of hiring non-employed women. The subsidy effectively made hiring non-employed women cheaper than hiring both women already in employment and men. The policy aimed to increase female employment, which is particularly low in Italy.

The magnitude of the Italian subsidy accords with that of the French subsidy introduced in 2008**,** which was 12 percent of the gross salary of low-wage workers (Cahuc et al., 2019). It is also consistent with that of the U.S. New Jobs Tax Credit subsidy of the late 1970s**,** which cut employers' social security contribution to 50 percent for low-wage workers (Neumark, 2016**).** In contrast to the French and the U.S. subsidy, the Italian subsidy is not capped: the employer's social security contributions are halved independently of the wage level and are thus applied to both low-wage and high-wage workers.

The length of the tax cut is one year if the worker is employed under a fixed-term contract and eighteen months if the worker is employed under an open-ended contract, or if the fixed-term contract is transformed into an

[41] In Italy the tax on social security contributions (the payroll tax) is paid by both the employer and the employee. Both tax rates have been relatively constant since 2005, the employer's tax rate at 21.6% and the employee's tax rate at around 10%.

open-ended contract within twelve months. Inclusion of an extension to eighteen months sought to provide firms an incentive to create jobs that could last in the long-term, as an open-ended contract is a contract with no time limit. The subsidy applies to workers hired both under a full-time and a part-time contract.

Workers targeted by the reform are women out of employment for at least twenty-four months: the potential female hire had to not have been employed in a firm or through self-employment in the twenty-four months before hire, without the need to be registered as an unemployed worker**.** Since the legislation requires eligible workers not to be in employment during the two years prior to hiring, the hiring subsidy also applied to new labor market entrants. More lenient requirements apply to the required length of the non-employment spell for some categories of female workers. Women older than fifty years of age are eligible for the subsidy, if they are out of employment for at least twelve months[42]; female workers hired in male-dominated occupations or sectors[43] (see Tables 3.A-1 and 3.A-2 for details) and female workers living in municipalities eligible for EU Structural funds[44], mainly comprising residents of the South of Italy and a few areas of the Centre and North of Italy (see Figure 3.A-1 for details) , are eligible if they have been out of employment for at least six months.

The legislation set out qualifying requirements for firm**s.** First, only private sector firms and associations are eligible for the hiring subsidy and must request it for each hire separately, by sending an online claim to the Italian National Institute for Social Security (INPS). Reimbursements of firms' social contributions payments are made at the beginning of the subsequent

[42] Men older than fifty years old are also eligible for the subsidy if they are out of employment for twelve months or more. Few men were hired under the subsidy--a total of 50,195 between 2013 and 2019, and in very specific industries, such as construction and waste management, where the subsidized women were not hired. Investigating subsidised men is out of the scope of our analysis.
[43] Male-dominated occupations and sectors are defined every year by ministerial decree. These are occupations and sectors with a gender employment gap twenty-five percent larger than the average gender employment gap. There is little time variation of the sectors and occupations that are identified as male dominated**.**
[44] Municipalities eligible for EU structural funds are identified by ministerial decree. For the years of analysis, these municipalities were identified by two ministerial decrees, in 2008 and in 2014. Areas eligible to receive EU-structural funds are reported in the Appendix A in Figure 3.A-1. The South of Italy, including Sicily, most of Sardinia and a few locations in the Centre and North of Italy are eligible.

year of hire**.** This is consistent with the French hiring subsidy, where employers were also required to request the subsidy for each individual hire, by filling out a one-page form. In the French case, reimbursements were paid at the end of each quarter (Cahuc et al., 2019).

Second, the hiring subsidy is restricted to firms with no decrease in net employment over the twelve months prior to the hiring of a subsidised worker[45]. No such requirement is relevant in cases where employment decreases are due to voluntary worker resignation or worker retirement. Finally, the subsidy might be applied only if no other worker with the right to preferential hiring could be hired[46]**.**

These strict requirements were put in place to prevent workers' loss of employment as a result of the subsidy. Net job growth requirement is not uncommon--for example, the U.S. New Jobs Tax Credit was an "incremental employment" subsidy (Katz, 1998)--but could impose large administrative costs to firms and restricts the set of firms that could use the hiring credit (Neumark, 2013). These provisions partially explain the low take-up of the subsidy and motivates our identification strategy. We will return to their consideration in sections 3.3 and 3.4**.**

Our a**n**alysis of subsidised workers and firms is especially relevant to policy given that the current government extended the subsidy and has, starting in 2022, increased its magnitude to 100 percent of the employer's share of social security contribution[47]**.**

3.2.2.2 The Political Setting

The hiring subsidy was part of a package of laws, the Labor Market Reform, implemented by a technocratic government in 2013**.** In 2010, Italy was hit by the so-called Sovereign Debt Crisis: Eurozone countries[48] with high budget deficits, high debt levels, and low economic growth struggled to

[45] The law specifically states that the requirement of net employment growth refers to the firm as entity, not to the establishment.

[46] In Italy, a worker hired by a firm under a fixed-term contract longer than six months is subject to preferential hiring by the firm within six months of her contract's end. The subsidised worker cannot be hired as a replacement for a fixed-term worker with preferential access.

[47] Since 2022, the hiring subsidy will be capped at euro 6,000

[48] The nations of Italy, Portugal, Ireland, Greece, and Spain.

finance their budgets when financial markets started demanding higher interest rates, due to a fear of national defaults. As a consequence, on November 16, 2011, a technocratic government led by Mario Monti came into power with the goal of implementing structural reforms to address these major issues of high sovereign debt and stagnating economic growth. The timing of the reforms implemented by the Monti government was rapid due to the pressure of financial markets and of external institutions[49].

The Monti government's Labor Minister implemented two reforms, approved six months apart: The Pension Reform, approved on December 22, 2011, and The Labor Market Reform, approved on June 28, 2012. The Pension Reform created new pension rules to ease pressure on the public debt[50]. The Labor Market Reform had four major policy components: 1) eased employment protection legislation for workers under an open-ended contract, by allowing, under strict rules, the firing of workers due to economic reasons[51]; 2) made temporary and atypical contracts marginally more stringent[52]; 3) expanded the coverage of the unemployment insurance scheme; and 4) introduced the hiring subsidy, the "Bonus Donne", which is the focus of our paper. Importantly for our identification**,** the hiring subsidy was the only subsidy targeted to firms and, in particular, to the hiring of women.

There was strikingly little news coverage of the hiring subsidy, as pension Reform and easing of the employment protection legislation took over the news. These new laws were, in fact, highly controversial and very

[49] The European Central Bank and the International Monetary Fund.

[50] The major changes put in place by the Pension Reform, also known as the Fornero Reform, were a transition from a defined benefit to a defined contribution scheme for the older cohorts that were not on the defined benefit scheme**,** and an increase in retirement age. The reform was deemed necessary to decrease pressure on Italian public debt, as the large, older cohorts were close to retirement and the younger cohorts were facing high unemployment rates, with many young workers not in employment and unable to contribute through taxes.

[51] The firing of workers for economic reason had never been allowed, under any circumstance (apart from bankruptcy), as this is part of the Italian constitution. Changing articles of the constitution requires a large parliamentary majority and is a very unpopular move (as it was for the Fornero's Labor Market Reform and indeed for subsequent reforms).

[52] Regarding temporary and atypical contracts, The Labor Reform made two major changes. First, it decreased the maximal number of years during which the same worker could be hired under a temporary contract in the same firm. Second, it increased the minimum time span than needs to incur between two fixed-term contracts for a worker to be re-hired by the same employee under a new fixed-term contract.

much disliked by the public--as the Pension Reform meant a worsening of pension conditions for all cohorts and the easing of the employment protection legislation amended one of the main articles of the Italian constitution, Article 18, by allowing the firing of workers for economic reasons. Protests were organized across Italy**,** the labor minister received threats to her life, was placed under protection, and became the person "most hated by Italians" (Panorama, 2018).

We compare the small amount of coverage received by the hiring subsidy—the Bonus Donne—to both the Pension Reform and the amendment to Article 18 and illustrate the comparison in Figure 3.1**.** Panel A shows the evolution of Google searches for the item "Bonus Donne" (the hiring subsidy) over time. It shows that google searches of the hiring subsidy were low when the Labor Market Reform was announced in 2012 and came into force in 2013. Google searches remained low through the first months of 2014, to slowly increase and reach a peak in 2015. Google searches remained relatively lower and stable from 2016 onwards. Interest in the hiring subsidy was significantly lower than for the other, more controversial and more discussed, policies implemented by the same Labor Minister—as illustrated by the last two panels of Figure 3.1. Panel B shows Google searches for the item "Bonus Donne", the hiring subsidy (shown as a pink line), against Google searches for "Pensioni Fornero", the Pension Reform (shown as a blue line). During the announcement and Parliamentary discussion of the Pension Reform in 2012, Google searches for the Pension Reform reached their maximum, whereas Google searches for the term "Bonus Donne" were close to zero. Even at its peak in 2015, "Bonus Donne" Google searches constituted 66 percent of Google searches for "Pensioni Fornero". This gap is even more striking when Google searches of "Bonus Donne" (the pink line) are juxtaposed with those for "Article 18" (the blue line), as plotted in Panel C of Figure 3.1. At the time of the Labor Market Reform's announcement, Google searches for "Article 18" reached a peak and the item "Article 18" continued to be searched relatively more frequently than "Bonus Donne" throughout the period.

3.3 Data and Descriptives

3.3.1 Data

In this study, we use administrative data from the Italian Social Security Institute (INPS) over the years 2009 to 2019. We obtained access to the data by winning a highly competitive bid through the VisitINPS programme. The data encompasses the population of workers and firms covered by the social security system in Italy since 1975; the self-employed, civil servants, and military personnel are not included. Three characteristics make this data set uniquely suited to the analysis of the research questions under examination. First, the dataset is a linked employer-employee dataset, which allows workers and firms to be tracked over time by using the panel structure of the data. This feature also allows the workforce at the firms exploiting the hiring subsidy to be characterized. Second, the INPS data uniquely exposes the type of contract the worker is hired under, and so, permits us to observe whether the worker is hired under the hiring subsidy. To our knowledge, no other dataset allows observation of both whether a worker is hired under any hiring subsidy, as well as the firm and its workforce[53]. This capacity is crucial for our study, as it enables correct and accurate characterization of take-up of the policy and permits both subsidized workers and firms to be followed over time, without resorting to inferences regarding the adoption of the subsidy. In addition, it allows analysis on the level of the (subsidized) firm. Third, maternity leave since 2005 of all the workers included in the data can be tracked[54]. This feature of the data is important for our research because the hiring subsidy is targeted at women out of employment. As described in Section 3.2.1, women with children have the lowest employment rate in Italy, at 53.6% (as opposed to

[53] The study on hiring subsidies that exploits administrative data is that of Cahuc et al. (2019), and has access to firm level administrative data. In addition to firm level administrative data, we also have access to worker level administrative data, with information on each contract stipulated under the hiring subsidy.

[54] In Italy, paid maternity leave lasts 5 months, where payment is 80% of the salary. Paid paternal leave for fathers can be of up to two weeks. Parents can take additional leave, up to 6 months, until their child is 12 years old, with compensation that amounts to 30 percent of the salary. There were no policy changes regarding paid maternity leave over the period of analysis.

62.6% for women without children and 87.7% for men with children). It is crucial to be able to investigate whether mothers, one of the groups implicitly targeted by the policy, are re-entering the labor market thanks to the subsidy.

In more detailed consideration, the data include information on wages (adjusted hourly) and the number of weeks worked each year (in full-time equivalent); which allow us to compute the worker's weekly wage (in full-time equivalent)[55], a wage variable that is defined for both full-time and part-time workers. This is an important feature of the data, as the hiring subsidy is targeted to women, who are more likely to be employed in part-time work (Casarico and Lattanzio, 2019). Further, our data include information on type of employment (whether it is full-time or part-time); type of contract (whether it is a fixed-term or an open-ended contract); broad occupational category; demographic characteristics such as age, gender, nationality, year of birth, year of entry into the labor market; and hiring conditions (in essence, whether the worker was hired under the hiring subsidy). The broad occupational category classifies workers into the following categories: apprentices, blue-collar workers, white-collar workers, and managers. Since 2008, data on education level and a detailed occupation variable have been made available for new hires. Since 2005, maternity leave spells can be identified in the data. With respect to the firm, the data encompasses information on geographic location of the firm and the five-digit industry code.

An advantage of our data is the observation of the maternity leave linked not only to first birth but also to births of subsequent children, as opposed to comparable administrative data sets, such as the German administrative dataset, where only the first birth is observed (Boelmann et al., 2021). A drawback of the data is that maternity leave information is available only for employed workers, as workers not in employment do not receive maternity leave contributions paid by the employer, and, correspondingly, are not

[55]The "full-time equivalent weeks of work" variable exploits the information on the number of hours worked in a month (available to INPS, but not to researchers). Full-time equivalent weeks are computed by multiplying the number of actual weeks worked by the ratio between the number of hours worked in a month and the number of contractual hours for the full-time equivalent position.

recorded in the data. As a result—because we do not have the full actual maternal leave information--our estimates of the share of female workers who become mothers since 2005 may be lower than the true value.

3.3.2 Sample and Descriptives

We build a panel dataset that comprises one observation per worker per year[56]. We restrict our analysis to workers between the ages of 18 and 65. We harmonize workers' education by imputing missing observations in the original education variable[57], we categorize workers into the following three education groups 1) low-skilled workers, without post-secondary education; 2) medium-skilled workers, who have an apprenticeship degree or graduated from high school; and 3) high-skilled workers, who graduated from university.

Our data enables us to compute the take-up rate, that is, the percentage of firms that adopt the hiring subsidy among all firms (all firms can use the subsidy by hiring a woman who has had a non-employment spell of two years) between 2013 and 2019. The take-up rate amounts to 3.7%. The attention rate (the share of subsidized female hires among eligible female hires) amounts to 5%.

The take-up rate and attention rate in the case of the Italian subsidy are lower than those found in comparable studies that examine the effect of hiring subsidy on employment. For example, Cahuc et al. (2019) find a take-up rate of 24% and an attention rate of 47% in their study of a French hiring subsidy targeting low-wage workers[58]. The Italian subsidy's low take-up rate may be explained by the rules introduced by the legislation and by the public's knowledge of the policy. First, only firms with non-negative employment growth in the year before could apply for the hiring subsidy. In contrast, the French hiring subsidy was not restricted to firms with net

[56] If a worker holds multiple job in the year, we select the main job, namely the job that is associated with the highest number of weeks worked. Where two or more observations are characterized by the same number of weeks, we retain the observation with the highest weekly earnings.

[57] As the education information is available from 2008 and only for new hires, we obtain a non-missing education variable for the majority of new hires since 2008. However, the education variable remains missing for most incumbents and for new hires before 2008.

[58] Earlier studies on US hiring subsidies find attention-rates that range between 17% and 33% (0' Neill, 1982; Hamersma, 2003)

employment growth (Cahuc et al., 2019). Second, the Italian subsidy, in contrast to the French case, was limited to the hiring of workers not already in employment. Some firms might be less willing to hire workers that experienced employment interruptions and, indeed, previous studies have shown that long-term non-employed workers could be stigmatized by firms (Katz, 1998; Hamersma, 2003)[59]. Third, it seems that there was lack of awareness of the Italian hiring subsidy. Previous studies have shown lack of awareness of hiring subsidies, for example, only 34 percent of U.S. firms knew about the U.S. New Jobs Tax Credit (Perloff and Wachter, 1979). As has been noted, this lack of awareness was likely more severe in the Italian case as the subsidy was introduced at the same time as more controversial policies that dominated the public discussion (see Figure 3.1).

We now turn to the focus of our analysis: the firms that adopt the hiring subsidy**.** Table 3.1 shows the characteristics of firms which use the hiring subsidy (*hiring-subsidized firms*) compared to other firms in the private sector (*the average Italian firm*) in Italy in 2012, the year before the introduction of the policy. A total of 26,497 firms hire at least one female worker under the subsidy during the years 2013 to 2019. The table shows that hiring-subsidized firms differ from the average Italian firm in multiple respects. Hiring-subsidized firms are larger firms, with an average size of forty-eight employees, against an average size of eighteen employees at the average firm[60] and employ a larger share of women among their employees, at 57 percent, compared to an average share of 44 percent at the average firm. The larger share of female workers among employees suggests that hiring-subsidized firms could be more women-friendly. This is further reinforced by the fact that these firms employ a larger share of workers in part-time jobs, with a share of full-time workers at 63 percent**,** compared to an average of 70 percent for the average firm; employ a larger share of mothers, at 30

[59] Finally, we note that the French hiring credit's high take-up rate and effectiveness relied on exceptional circumstances–a one-off, non-anticipated temporary hiring credit, targeted at a small subset of firms and implemented in a context with high binding wage floors and high unemployment (Cahuc et al., 2019). These were not present in the Italian context.

[60] Appendix Table 3.B-1 shows the distribution of firms by size cut-offs, further highlighting that a greater share of hiring-subsidized firms has more than fifteen employees.

percent, compared to an average of 28 percent for the average firm; and hired a large share of women among new hires in 2012, at 54 percent, compared to an average of 42 percent for the average firm. In addition, hiring-subsidised firms hired more workers between 2010 and 2012, with an average of twelve workers hired, compared to an average of three new hires at the average firm. Finally, the average weekly wage is slightly lower in hiring-subsidized firms, at 5.82 log points compared to an average of 5.87 log points for the average firm.

The less-restrictive eligibility characteristics created by the policy involve firms located in areas subject to EU-structural funds or operating in male-dominated sectors or hiring subsidized workers in male-dominated occupations (whose rules are described in the Legislation section). Table 3.1 also shows that hiring-subsidized firms are more likely to be located in areas subject to EU-structural funds. That share lies at 48 percent, compared to an average of 34 percent for the average firm. Indeed, firms that use the subsidy are more likely to be located in the South of Italy and its islands–in locations such as Sicily and Sardinia)–that receive EU structural funds (see also Appendix Table 3.B-2). On the contrary, on average, hiring-subsidized firms do not seem more likely to operate in male-dominated sectors and to have a larger share of male-dominated occupations.

Appendix Table 3.B-3 captures the characteristics of the women hired under the subsidy compared to all other newly hired women (*the average female hire*) between 2013 and 2019. A total of 183,615 female workers are hired under the subsidy from 2013 to 2019. On one hand, hiring-subsidized women are older, more likely to be middle-skilled**,** less likely to be an apprentice, and used to earn a higher wage in their previous job. These characteristics suggest that the hiring-subsidized women are positively selected compared to the average female hire. By contrast, hiring-subsidized women have longer employment interruptions, are more likely to be mothers, and are more likely to be hired under a fixed-term contract. These characteristics suggest negative selection compared to the average female hire. We investigate the differences between the subsidized hire and the

average female hire in the event study analysis. Finally, Figure 3.B-1 in Appendix B, shows that, when focussing on the evolution of the number of subsidised hires over time, few women are found to have been hired under the subsidy in 2013. Over time, we observe a slow and gradual increase in the number of female subsidised hires (with the proviso of a bump in 2015 and 2016).

3.4 Empirical Strategy

In this section we zoom into the firms that adopt the hiring subsidy to investigate the effect of the subsidy on firm hiring decisions and on the evolution of worker outcomes at these firms. The descriptive statistics previously described suggest that hiring-subsidized firms differ from the average Italian firm along multiple dimensions: they are bigger, have a larger share of women among employees and hired more workers in the 2010 to 2012 period. A comparison between the hiring-subsidized firms and the average Italian firm will unlikely uncover the effect of the hiring subsidy on a firm's hiring decisions. To capture a causal effect, we combine a difference-in-differences estimator with an event-study approach.

3.4.1 Matching

We identify as treated firms, firms that hire at least one female worker under the hiring subsidy from 2013 to 2019[61]. In order to observe the evolution of firms' outcomes over time, such as the hiring pattern, we restrict our sample to firms that existed throughout the observation period of 2008 to 2019 and had at least two employees each year. We match on an extensive set of predetermined firm variables to ensure that we compare firms that evolved similarly in terms of wages, size, and hiring decisions before the adoption of the subsidy. While we take the adoption of the subsidy by the firm as given, we do not match on either firm location or firm industry (nor on

[61] We exclude year 2020 from the analysis as 2020 was the first year of the covid pandemic. Italy was the first European country badly hit by the pandemic, with a nationwide lockdown implemented in March 2020.

the occupation of the new hire) to partially exploit the margins created by the hiring subsidy legislation in affecting firm adoption of the subsidy (as also shown in the Balancing Table 3.2)[62].

More precisely, we perform a logit propensity score matching and we match on the following variables over the three years before the adoption of the subsidy: firm average weekly wage, firm natural logarithm of total employment, firm share of female workers among employees and on (quartiles of) the number of workers hired over the three years before the adoption of the subsidy. In addition, we require control firms to expand in the initial period, by selecting control firms that hire at least one female worker in the same year the treated firm hires a subsidized female worker for the first time[63] [64].

Our final sample consists of 38,270 firms, of which half (19,135) are in the treated group and half are in the control group. Table 3.2 shows descriptive statistics for treated and control firms and the p-value of the balancing test to determine whether the difference in the variables between treated and control firms is statistically significant. Treated and control firms are well matched on average weekly wages, total workforce, and share of female workers among employees**.** Treated and control firms differ on firm location, as treated firms are more likely to be in municipalities eligible for EU structural funds, at 44 percent against a share of 27.6 percent for control firms. Treated firms are also more likely to be in a male-dominated sector, at 36.2 percent compared to 31.8 percent for control firms, and contain a larger share of male-dominated occupations, at 40.2 percent compared to a share of 37.2 percent for control firms. These differences between treated and control firms suggest that the additional margins of variation created by the policy were partially exploited by firms**,** with a higher likelihood that firms located in areas eligible for EU-structural funds or operating in a male-

[62] As described in the Legislation section, the required non-employment spell length for female hires eligible to the subsidy was reduced from 2 years to 6 months for firms located in areas eligible for EU-structural funds or operating in a male-dominate sector or hiring a worker in a male dominated occupation.

[63] Female workers hired by the control firm in the initial period identify the average female hire, the female hire that female workers in treated firms are compared to in our event study analysis.

[64] Our results are also robust to requiring control firms to hire a worker, independently of gender, in the same year the treated firm hires a female worker under the subsidy for the first time.

dominated sector or hiring a worker in a male-dominated occupation would adopt the subsidy.

3.4.2 Difference in Differences Estimation

Based on the sample of the pairwise matched treated and control firms, we compare changes in outcome for treated and control firms in the period before and after the adoption of the hiring subsidy. We estimate the following model at the level of the firm:

(5) $y_{j\tau t} = \sum_{\tau=-4}^{-2} \beta_\tau \, Event_{j,t}^{\tau} + \sum_{\tau=0}^{5} \gamma_\tau \, Event_{j,t}^{\tau} + \ \delta_t + \ \theta_\tau + \ \lambda_j + e_{j\tau t}$

Where the subscript τ denotes the time period relative to the year that the treated firm adopts the hiring subsidy for the first time (the treated firm adopts the subsidy for the first time at τ = 0)[65]; $y_{j\tau t}$ is the outcome variable, for example the (log) number of long-term non-employed female new hires of firm j in calendar year t and τ periods before (or after) the first adoption of the subsidy. We focus on four years before and five years after first subsidy adoption ($-4 \leq \tau \leq 5$); $Event_{j,t}^{\tau}$ are indicator variables equal to 1 for the treated firm in period τ, and 0 otherwise. It is to be noted that τ and t differ because the first adoption of the subsidy can occur in any year between 2013 and 2019. Equation (5) controls for time-invariant differences across firms through firm fixed effects λ_j and for aggregate shocks through year fixed effects δ_t. In addition, the equation controls for event period fixed effect θ_τ, measured relative to the first adoption of the hiring subsidy. We omit the dummy for the period before the treated firm hires a subsidised worker for the first time, τ = −1. Our post-treatment coefficients γ_τ identify the dynamic changes in outcome between treated and control firms over the five post-treatment periods relative to the same difference at τ = −1. Standard errors are clustered at the firm level.

[65] τ = 0 the first time the treated firm hires a worker under the hiring subsidy. In cases where firms hire female workers under the subsidy in different periods, we identify τ = 0 as the period the firm hired a subsidised female worker for the first time.

Under the model's identifying assumption, the outcomes (such as the hiring decisions) of treated and control firms would have followed parallel trends in the absence of subsidy adoption. Controlling for both calendar year fixed effects δ_t, and event period fixed effect θ_τ, ensures that we compare outcomes of treated and control firms in the same calendar year and in the same period relative to the first adoption of the subsidy, and circumvents challenges scrutinised in recent research of event-study models that rely on variation in the timing of treatment (Sun and Abraham, 2021). Indeed, in absence of the event period fixed effect θ_τ, equation (5) would implicitly assume homogeneity of the effect of the hiring subsidy between early and late adopters and might distort the weights that the OLS estimator of γ_τ gives to the effects, with the risk of some negative weights and an invalidation of the tests based on pre-trends (Sun and Abraham, 2021). By allowing the assignment of the event period fixed effect θ_τ of each treated firm to its matched control, matching allows the inclusion of the event period fixed effect θ_τ in equation (5) and avoids these distortions (as also implemented in Cengiz, Dube, Lindner and Zipperer, 2019; and discussed in Baker, Larcker, and Wang, 2022).

To probe the robustness of the results, we augment regression equation (5) by including province-by-year and 2-digits industry-by-year fixed effects, thereby allowing for fully flexible differential time trends at the province and 2 digits industry level. This specification leverages variation in the adoption of the subsidy over time between treated and control firms within the province and the same 2 digits sector. In the results section, we show that our estimates are robust to the inclusion of these fixed effects.

3.5 Results

We start by providing, in Figure 3.2, a graphical description of the share of female workers hired under the subsidy as a percentage of new

female hires in treated firms[66]. The figure shows that in the first year of adoption of the subsidy, denoted as time from event 0, 70 percent of new female hires in treated firms were subsidized hires. While adoption of the subsidy is concentrated in the initial event study year, there is repeated adoption in subsequent years, with an average of 6 percent of subsidized hires among female hires in subsequent years.

Figure 3.3 plots event study coefficients, and corresponding confidence intervals, for the effect of the adoption of the hiring subsidy on the share of female workers hired as a percentage of new hires, estimated according to Equation 5. The figure shows that the share of female workers hired evolved similarly in treated and control firms before the use of the hiring subsidy at time 0. At time 0, at the time of first hiring subsidy utilization, treated firms increase the share of female workers hired among new hires by 3.3 percentage points. Given an average share of women among new hires of 54 percentage points, there is an increase of 6 percent at the mean. This result suggests that the subsidy increased the share of women hired under the subsidy, but only marginally. We are particularly interested in whether the characteristics of these new female hires differ from those of the average female hire. To answer this question, we investigate whether subsidized female hires differ from the average female hire[67]. After completing this investigation, we then turn to the evolution of the subsidized workers' career outcomes and compare them to those of the average female hire.

3.5.1 Characteristics of the new female hires

Figure 3.4 plots event study coefficients, and corresponding confidence intervals, of equation 5 for the effect of the adoption of the hiring subsidy on the share of female workers hired from non-employment as a

[66] It is to be noted that Figure 3.2 shows, for treated firms only, the percentage of subsidized hires as a share of female hires every period. As control firms never hire under the subsidy, Figure 3.2 only depicts the evolution of subsidized female hires in treated firms, without using control firms.

[67] For the analysis, we compare the subsidized female hire to the average female hire. The average female hire at time 0 is identified by the female hire in control firms. As explained in the Matching section, we require control firms to hire at least one female worker at time 0 such that the subsidised hire at time 0

percentage of new female hires. Prior to their use of the hiring subsidy at time 0, treated and control firms hired non-employed female workers at the same rate, as shown by the pre-trends coefficients. At time 0, treated firms increase the share of non-employed female workers hired by 15 percentage points. Given an average share of non-employed women among new female hires of 28 percent, there is an increase of 50 percent at the mean. Given that subsidized female workers are 70 percent of the new female hires at time 0, 38 percent of the 50 percent increase at the mean results from the hiring of subsidized female workers.

Investigating more closely the non-employment spell length of female new hires[68], Figure 3.5 plots event study coefficients, and corresponding confidence intervals of equation 5**,** for the effect of the adoption of the hiring subsidy on the average length of non-employment spell of the firms' new female hires. While treated and control firms had a similar hiring patter before the adoption of the subsidy, as is shown by the flat pre-trends, at time 0 the average non-employment spell length of new female hires increases of 150 percentage points in treated compared to control firms[69]. With an average non-employment spell length of two years and 6 months, this is an increase of 3 years and 9 months, for a total non-employment spell length of 6 years and 3 months. Treated firms, then, hire women with lengthy labor market interruptions in the first year of adoption of the subsidy. Interestingly, treated firms continue to hire women with longer labor market interruptions in subsequent periods: the average non-employment spell length of new female hires in treated firms is 7 months longer than that of control firms in subsequent years.

Figure 3.6 plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the share of new female hires who are mothers as a percentage of new female

[68] The worker's length of the non-employment spell is the number of years spent out of employment before being hired.

[69] Similar results are obtained if we look at the average non-employment spell length among all hires, suggesting that this increase is explained by the new female hires.

hires[70]. The figure shows that the share of mothers hired evolved similarly in treated and control firms, before the use of the hiring subsidy. However, treated firms increase the hiring of women with children at time 0, with an increase of new female hires who are mothers of 1.3 percentage points. Given an average share of mothers among new female hires of 6.3 percent, this is an increase of 21 percent at the mean. It is to be noted that firms that use the hiring subsidy keep hiring mothers at a larger share compared to control firms, albeit at a lower, statistically insignificant, rate than at time 0 and that there is an average increase of the share of mothers among new hires of 0.5 percentage points (8 percent at the mean) in subsequent periods.

Our results show that treated firms increase their hiring of women with labor market interruptions. These long labor market interruptions could be partially explained by the fact that treated firms increase their hiring of women with children, as some women drop out of the labor force after the birth of children (Kleven et al., 2019; see Casarico and Lattanzio, 2021 for evidence on Italy). As these female workers are long-term non-employed, we might conclude that these female workers are negatively selected compared to the average hire. This does not seem to be the case when we investigate the average observable characteristics of the new female hires in treated firms in comparison to that of control firms, as shown by Figure 3.7 and Figure 3.8.

Figure 3.7 plots the event study coefficients, and the corresponding confidence intervals, of the average wage (the net of the employer's social security contributions) earned in the last job by firms' new female hires[71]. The figure shows that the average wage earned in the last job by new female hires in treated firms at time 0 is 6.9 percent higher than that earned by new

[70] We define a new female hire to be a mother if we have a record of her maternity leave before hiring. To account for the left censoring of the data, we drop workers older than fifty years from the computation of the denominator (as we are not able to observe maternity leave for older women, as the maternity leave data are available from 2005).

[71] The average wage earned in the last job is the wage earned by the new female hires in the job of previous employment and it is the net of the employer's social security contributions.

female hires in control firms. The wage earned in the previous job could be interpreted as a proxy of ability. This result suggests that, on average, treated firms' new female hires at time 0 might be of higher quality**.** Relatedly, Figure 3.8 plots the event study coefficients, along with the corresponding confidence intervals, of the share of middle-skilled new female hires as a percentage of new female hires; it shows that, while treated and control firms hired middle-skilled women at the same rate before the adoption of the hiring subsidy, treated firms increase the hiring of these women at time 0 by 16.6 percentage points. This finding suggests that on average female new hires by treated firms at time 0 are more likely to be middle-skilled (also, in line with the workers' descriptive statistics in Table 3.B-3). It is to be noted that treated firms also increase the share of middle-skilled female workers hired in subsequent years, albeit this result is not statistically significant.

Robustness. Figure 3.9 plots event study coefficients (and corresponding standard errors) estimated according to equation 5 with the additional inclusion of province-by-year and 2-digits industry-by-year fixed effects for the effect of the adoption of the hiring subsidy on the percentage of workers hired from non-employment (Panel A) and the average non-employment spell length of new female hires (Panel B). This specification leverages variation in the adoption of the subsidy over time between treated and control firms within the province and the same 2 digits sector. The figure shows that our baseline estimates (blue dashed line), namely that of equation (5) with the inclusion of year-fixed effect solely, are very similar to the estimates derived by the inclusion of province-by-year and 2-digits industry-by-year fixed effects (pink dot-dashed line), thereby suggesting that our baseline results are not driven by differential trends at the province or at the industry level.

3.5.2 Career evolution of female hires

We exploit the panel dimension of our dataset to compute two outcomes in each -4 to 5 period. These outcomes are 1) the total number of female workers hired in that period that will still be employed in the firm within three years after the hiring and 2) the total number of female workers hired under a fixed-term contract in that period that will have their contract changed to an open-ended contract[72]. Both outcomes allow us to provide a first investigation of career evolution of the female workers hired in each period, by analysing whether treated firms increase the number of female new hires who remain employed in the firm in the medium-run (Outcome 1) and who remain employed in the firm with a better contract (Outcome 2)--as an open-ended contract is a contract with no time limit.

Panel A of Figure 3.10 plots the event study coefficients, and the corresponding confidence intervals, for Outcome 1. While treated and control firms do not retain **(**employ for at least 3 years as above**)** new female hires at a differential rate in the periods before adoption of the subsidy, treated firms retain 9.7 percent more female new hires at time 0. Relatedly, Panel B of Figure 3.10 plots the event study coefficients, and the corresponding confidence intervals, for Outcome 2. Whereas the trends for treated and control firms do not differ before adoption of subsidy, treated firms transform the contract into an open-ended one to 7 percent more new female hires at time 0. This transition is itself incentivized by the subsidy legislation, as the length of the hiring subsidy is extended from twelve to eighteen months when the contract under which the worker is hired becomes an open-ended contract.

[72] The outcomes are computed such that they are recorded at the time of worker hiring (not at the time of the transition). In order to compute the outcomes, we follow each worker from the time hired to learn whether she is still employed three years after hiring (Outcome 1) and whether her contract has changed to an open-ended contract (in future) within the same job spell. We then record the change at the time of hiring.

3.5.3 Wages of Female Hires

In the next section, we provide a preliminary analysis of the costs of female hires to firms. Panel A of Figure 3.11 plots the event study coefficients, and the corresponding confidence intervals, of the average gross wage earned by new female hires. While the trends in average wages of new female hires between treated and control firms do not differ before the adoption of the subsidy, at time 0 treated firms pay an average gross wage that is 2.6 percent lower than that paid by control firms. This decrease in the average gross wage at time 0 in treated firms may be explained by the hiring subsidy, as the majority of the female hires at time 0 are subsidized female hires and the subsidy halves the employer's social security contributions.

Panel B of Figure 3.11 plots the event study coefficients, and the corresponding confidence intervals, of the average wage—net of the employer's social security contributions--earned by new female hires. While the trends in new female hires' average net wages do not differ between treated and control firms before the adoption of the subsidy, at time 0 treated firms pay an average net wage that is 3.6 percent higher than paid by control firms. This higher net average wage earned by female hires at time 0 may be explained either by pass-through to workers—namely, the fraction of the tax cut that benefits the worker—or by worker composition, as our findings show that female hires at time 0 seem positively selected on observables. Let us recall that the average net wage earned in the *previous* job held by the female new hires in treated firms at time 0 is 6.9 percent higher (Figure 3.7) than that of the average hire**,** while the average net wage earned at time 0 is 3.6 percent higher**.** In sum**,** the current net wage is 50 percent of the past net wage and is the same as the amount of tax cut provided by the reform. This preliminary back-of-the-envelope calculation seems to suggest that there was either no pass-through or only a small pass-through of the tax cut to workers[73]. This calculation identifies that firms benefited from the full tax cut, without shifting some of the tax cut to workers by increasing their wages[74].

[73] As 70 percent of the new female hires at time 0 are subsidised hired, but not all.

[74] It is to be noted though that the female hires at time 0 have longer employment interruptions and might have suffered an even larger wage cut absent the subsidy. This observation would work against evidence of lack of pass-through to workers and must be further investigated. Our next step is to

3.6 Firm Learning

The findings presented in the results section suggest that the composition of new hires of hiring-subsidized firms is different from that of control firms at the time of subsidy adoption. On one hand, treated firms increase the hiring of middle-skilled female workers and of female workers who earned a higher wage in their previous job (as compared to the average female hire in control firms). In addition, our preliminary analysis shows that treated firms are more likely to still employ these female workers in the medium run, as compared to the average female hire. An important question emerges: If these female workers are of higher quality and remain employed in the firm, why are they not hired by the firm over the years before the adoption of the subsidy?

One reason could be that employers are initially uncertain about the productivity of this group, since these female workers are more likely to be long-term non-employed and mothers. By making these female workers cheaper compared to other workers in the labor market (as is suggested by the preliminary back-of-the-envelope calculation presented in section 3.5.3), the hiring subsidy could allow firms that use it to learn about the potential productivity of these workers through their hiring.

We provide a first test of this mechanism. We focus on treated firms that adopt the subsidy in 2013 and 2014, so as to follow their hiring patterns after the first adoption of the subsidy at time 0. We, then, exploit the individual level data to construct workers' productivity proxy (described below) and categorize treated firms accordingly.

First, we construct a measure of the productivity of each subsidized female worker hired at time 0 by a treated firm, by running a Mincerian wage regression of the wage earned in the previous job on the sample of all subsidized female workers hired at time 0 by a treated firm[75]. We then use

exploit the individual-level data to investigate the extent of the pass-through to workers more closely and to provide a more detailed individual-level analysis of the career progression of the new female hires. We also highlight this consideration in the conclusion of our study.

[75] More precisely, we regress the wage earned in the previous job on age, education, a dummy for open-ended contract, fulltime status, municipality of work, and sector. We then calculate the residuals.

the estimated residual from the regression as the worker's productivity measure. Using estimated residuals, as opposed to the wage level, allows us to account for observable characteristics such as age and education, that affect the wage level (on top of the effect of workers' productivity)[76]. Second, we assign a quartile of the proxied productivity distribution to each subsidized female worker hired at time 0 by a treated firm. Workers with residuals that are lower than the 25th percentile of the wage residuals distribution are assigned to the bottom quartile (the fourth category) and workers with residuals larger than the 75th percentile of the wage residuals distribution are assigned to the top quartile (the first category). In a similar fashion, workers with residuals between the 25th and the 50th and between the 50th and the 75th percentile are assigned to the second and third category.

Third, we classify treated firms into two categories based on their hiring at time 0: 1) treated firms that hire a high-quality female worker are firms that hire a subsidized worker in the top quartile of the wage residuals distribution (top quartile firms) and 2) treated firms that hire a low-quality female worker are firms that hire a subsidized worker in the bottom quartile of the wage residuals distribution (bottom quartile firms). If firms hire more than one subsidized female worker, we assign the treated firm to its category using the mode of the quartiles of subsidized workers at time 0. The aim of this exercise is to investigate whether the future hiring pattern, in period 1 to 5, of treated firms that hire high-quality female workers differs from that of treated firms that hire a low-quality female worker at time 0. We, thus, separately estimate equation (5) by these two categories of treated firms, where each treated firm is matched to its control firm, as described in section 3.4, and we average the post-treatment coefficients of periods 1 to 5.

Table 3.3 reports the average post-treatment coefficients, averaged over post-treatment periods 1 to 5, for different outcomes of treated firms that

[76] In a next step, we aim to improve our productivity proxy using the worker fixed-effect estimated by an AKM model (Abowd, Kramarz and Margolis, 1999), which allows us to account also for the role of the previous firm in affecting the wage level (in addition to the workers observable characteristics).

hire a low-quality subsidized worker at time 0 (bottom quartile firms) and treated firms that hire a high-quality subsidized worker at time 0 (top quartile firms). Top quartile treated firms are more likely to hire subsidized workers in subsequent periods, with an increase in the hiring of subsidized workers by 17 percent, compared to an increase of 12 percent for bottom quartile treated firms. Top quartile treated firms also increase the percentage of female workers hired from non-employment (as a share of all new female hires): there is a 5 percentage points increase for top quartile treated firms compared to a 1.6 percentage point increase for bottom quartile treated firms. Further, top quartile treated firms are twice as likely to hire long-term non-employed female workers[77]: Finally, top quartile treated firms are four times as more likely to hire middle-skilled women.

Our preliminary analysis suggests that firms that hire a high-quality subsidized worker at time 0 hire more subsidized workers in subsequent years. Firms with negative experiences with subsidized workers, that is, those that hire a low-quality worker at time 0, are less likely to hire more subsidized workers in the future. Our findings suggest that the previous positive hiring experiences of the employer may affect their subsequent decisions to hire from the group again (Bardhi et al., 2020; Lepage, 2021; Leung, 2018). Hiring through the subsidy may, thus, serve as a source of learning for the firm.

3.7 Discussion and Conclusion

In this paper, we focus on a hiring subsidy which targets hiring of female workers who are out of employment introduced in Italy in 2013. This is the first gender-related hiring subsidy. We combine a matched difference-in-differences estimator with an event-study approach to investigate the effect of the hiring subsidy on the hiring decisions of firms that adopt the hiring subsidy. Our findings show that the composition of new hires at hiring-subsidized firms is different in several ways from that at control firms at the

[77] Long-term non-employed female workers are defined as workers with a labor market interruption of 4 years or more.

time of adoption of the subsidy. Hiring-subsidized firms increase their hiring of women with labor market interruptions of 50 percent (at the mean) and of women who are mothers of 21 percent (at the mean). Although they experienced lengthy labor market interruptions, these new female hires seem more positively selected than the average firm hire on other observables: they have higher education levels and earned a higher wage in their previous jobs. Finally, our preliminary analysis shows that these female hires seem more likely to remain employed in the firm in the medium-run and have an increased probability of obtaining secure employment compared to the average firm's hire, with their contract changed from a fixed-term to an open-ended contract. Our analysis of hiring-subsidized firms that hire a high-quality subsidized worker and of hiring-subsidized firms that hire a low-quality subsidized worker suggests that hiring through the subsidy may have operated as a source of learning for firms, as firms that hire a high-quality subsidized worker at time 0 (the period of first adoption of the subsidy) hire more subsidized workers in subsequent years. All in all, our findings suggest that gender-related hiring subsidies could increase the hiring of women with lengthy labor market interruptions and their presence in the labor market.

In future research, we aim to exploit the richness of our data further by investigating whether the hiring subsidy has an effect on incumbents: the workers already employed in the firm before the adoption of the subsidy. While the terms of the subsidy legislation prevent firms from firing already employed workers at the time of subsidy adoption[78], incumbents may be affected in subsequent years. We undertake to exploit the data available at the level of the individual worker and investigate the probability of incumbents remaining employed within her firm from onset of the subsidy, along with the wage evolution of incumbent workers. In similar fashion, we aim to exploit the individual level data for a more detailed analysis of the subsidized workers career progression, investigating the probability of remaining employed in the same firm and of moving to another firm, as well as to a higher quality or lower quality firm. Finally, we are planning to develop

[78] The hiring-subsidy legislation requires firms to have non-negative employment growth to the able to qualify for the subsidy.

a model to explain the firm's learning mechanism and further investigate the predictions provided by the model in the data.

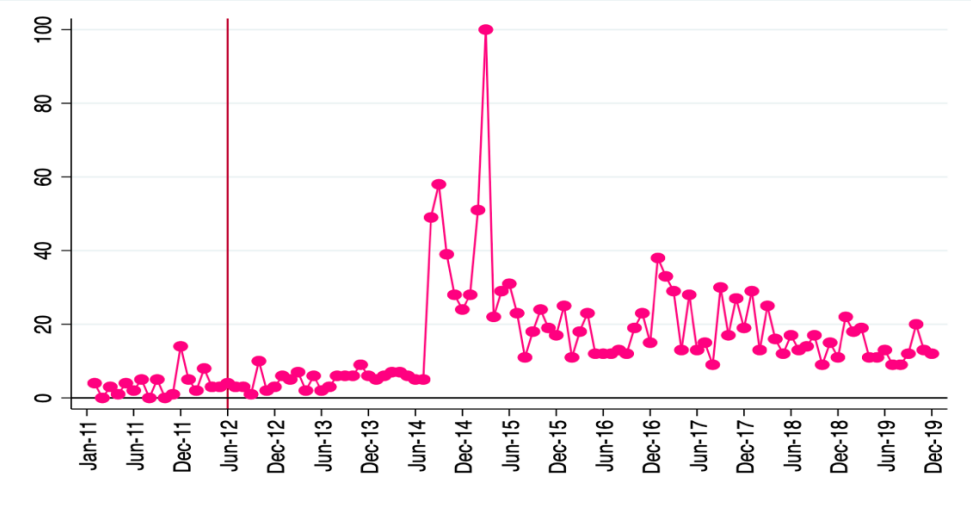
Further, we seek to conduct a survey[79] to consider and further understand the reasons behind firm adoption, or rejection**,** of the "Bonus Donne" subsidy by Italian firms. A principal area of investigation will be to investigate through the survey's questionnaire both whether firms perceive workers targeted by the subsidy as low-productivity and whether the adoption of the subsidy is affected by the magnitude of the tax cut, by creating hypothetical scenarios to assess whether a (potential) larger tax cut would increase the share of firms that use the subsidy.

[79] We are in contact with researchers at the Bank of Italy to include our questions in the Bank of Italy yearly firms survey.

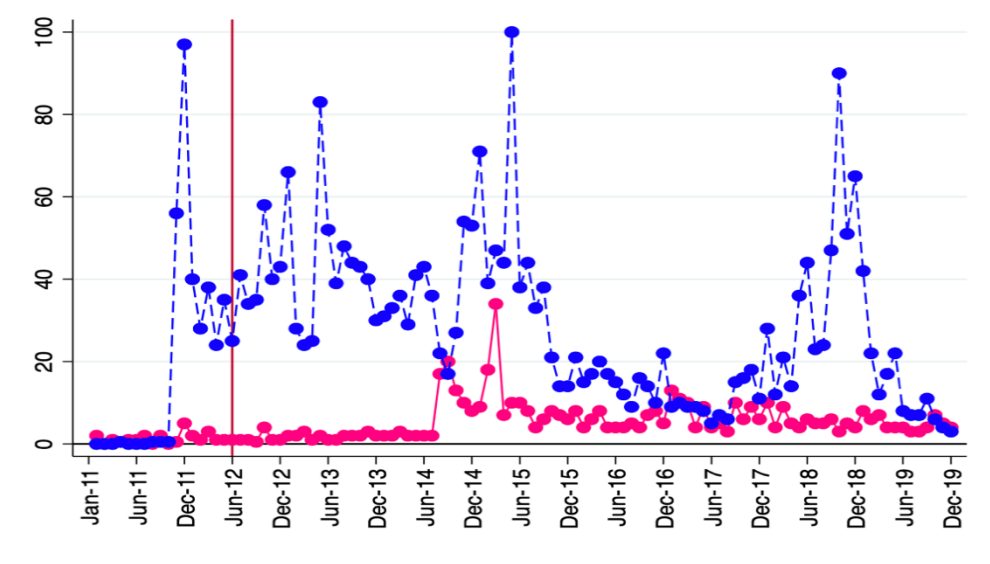
Figures and Tables

Figure 3.1. Google Searches

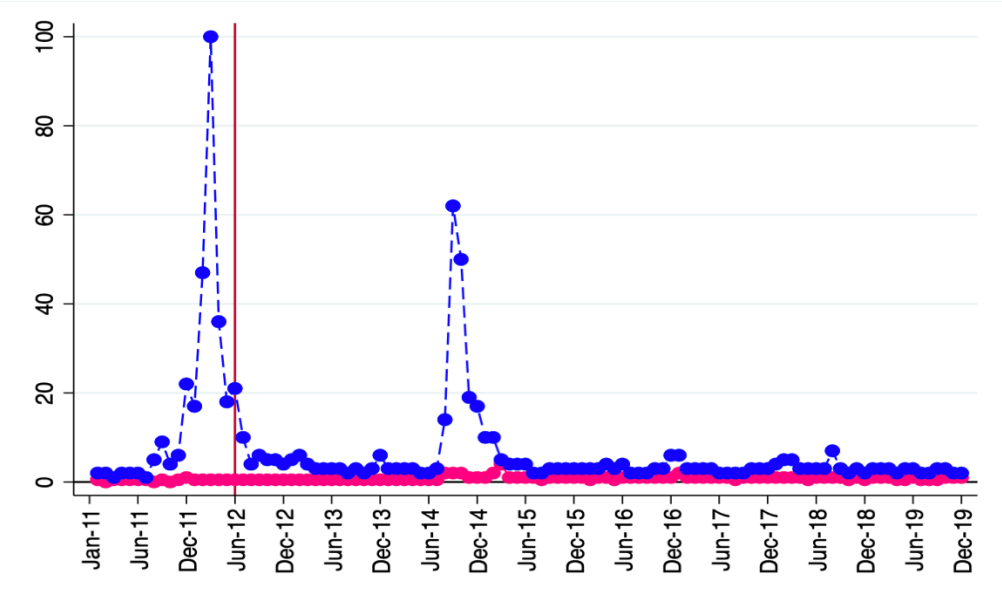
Panel A Google Searches for item “Bonus Donne” (hiring subsidy)



Panel B. Google Searches for item “Bonus Donne” (hiring subsidy, in pink) and item “Pensioni Fornero” (Pension Reform, in blue)



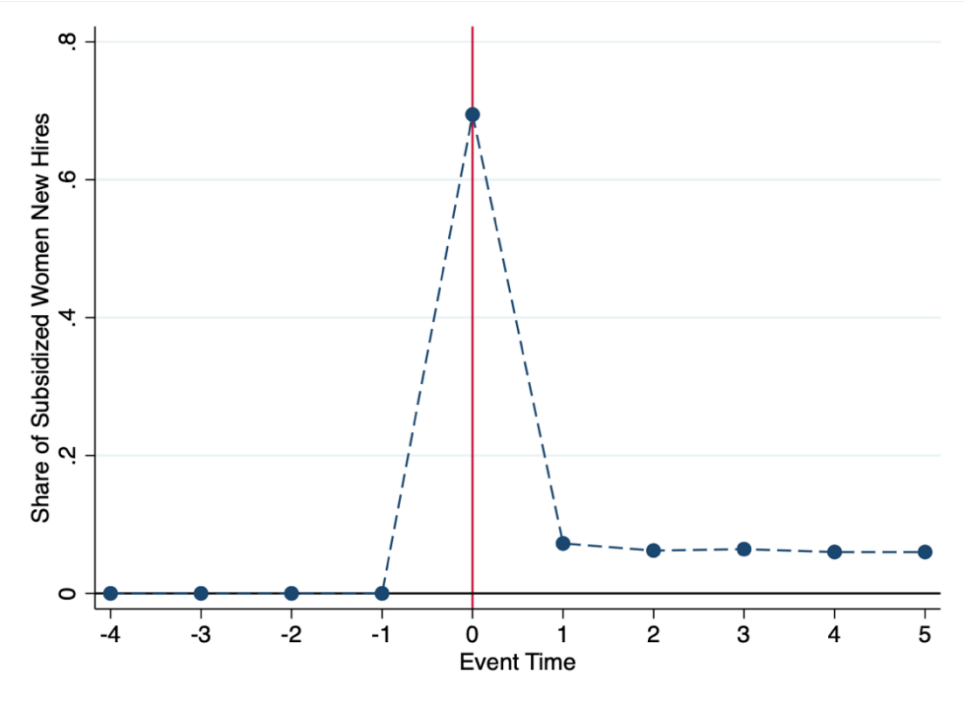
Panel C. Google Searches for the item “Bonus Donne” (hiring subsidy, in pink) and item “Articolo 18” (article 18, in blue)



Notes: The figure plots google searches for the item “Bonus Donne” (hiring subsidy) in Panel A; google searches for the item “Bonus Donne” (hiring subsidy, in pink) against the item “Pensioni Fornero” (Pension Reform, in blue) in Panel B and google searches for the item “Bonus Donne” (hiring subsidy, in pink) against the item “Articolo 18” (Article 18, in blue) in Panel C between January 2011 and December 2019. The vertical axis reports the monthly number of searches for one term relative to the highest point on the figure. The vertical red line indicates the date at which the Italian Parliament approved the hiring credit.

Source: Google Trends website.

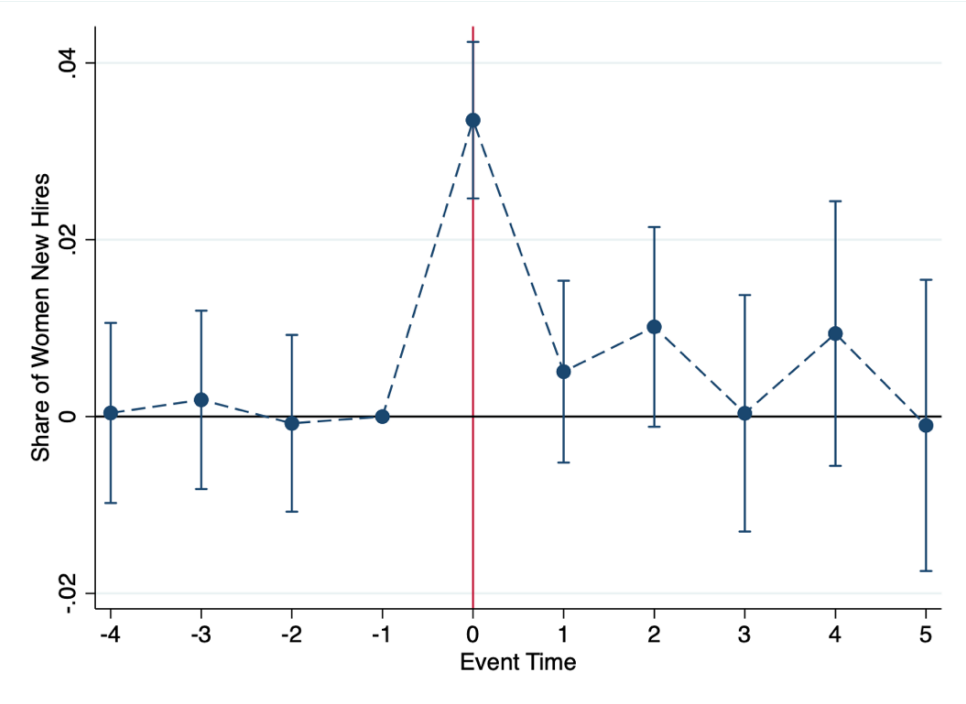
Figure 3.2. Share of subsidized women



Notes: The figure plots the evolution of the share of female workers hired under the subsidy as a percentage of new female hires in treated firms between event time -4 and event time 5. Treated firms are firms that hire at least one worker under the hiring subsidy. The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

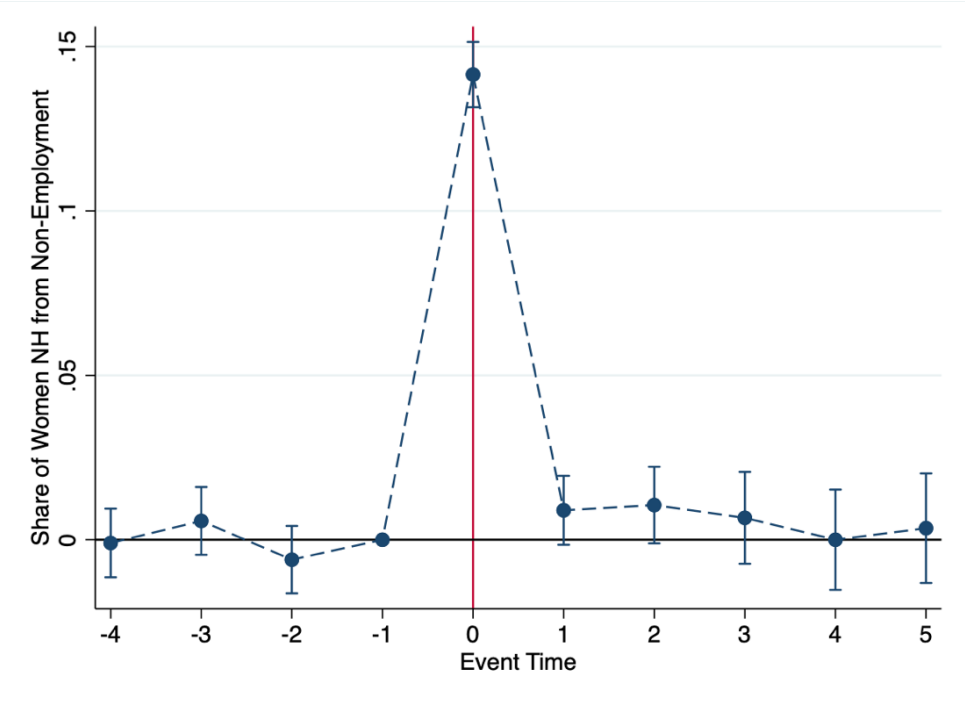
Figure 3.3. Share of female hires (event study)



Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the share of female workers hired as a percentage of new hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The figure shows that the share of female workers hired evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, treated firms increased the share of female workers hired among new hires by 3.3 percentage points compared to control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

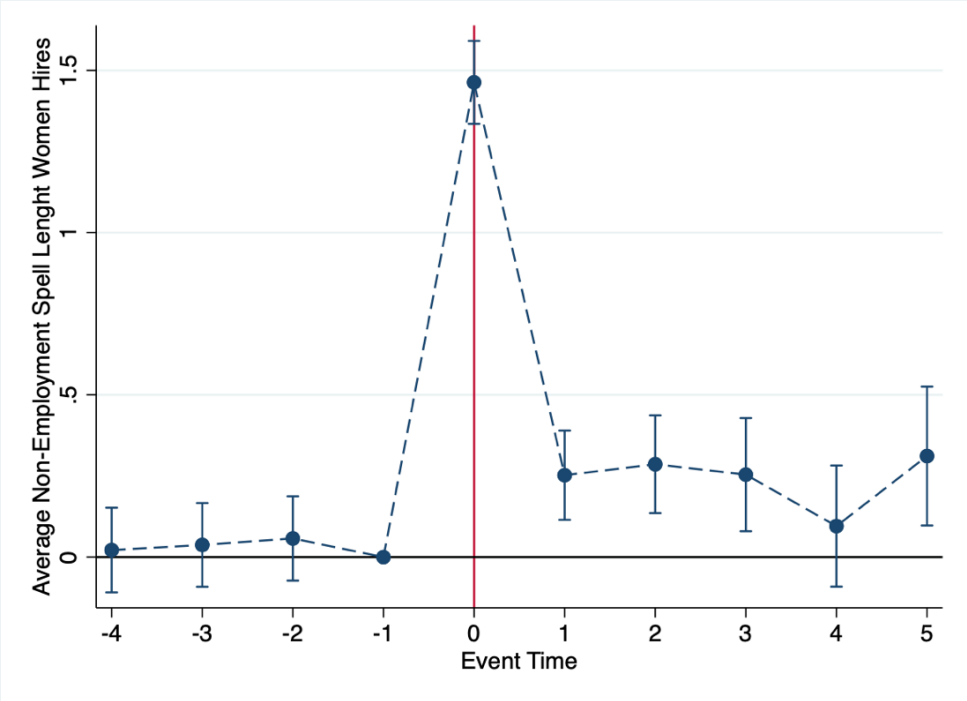
Figure 3.4. Share of female hires with employment interruptions (event study)



Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the share of female workers hired from non-employment as a percentage of new female hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. Workers hired from non-employment are those who were not employed before being hired. The figure shows that the share of female workers hired from non-employment evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, treated firms increased the share of female workers hired from non-employment by 15 percentage points compared to control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

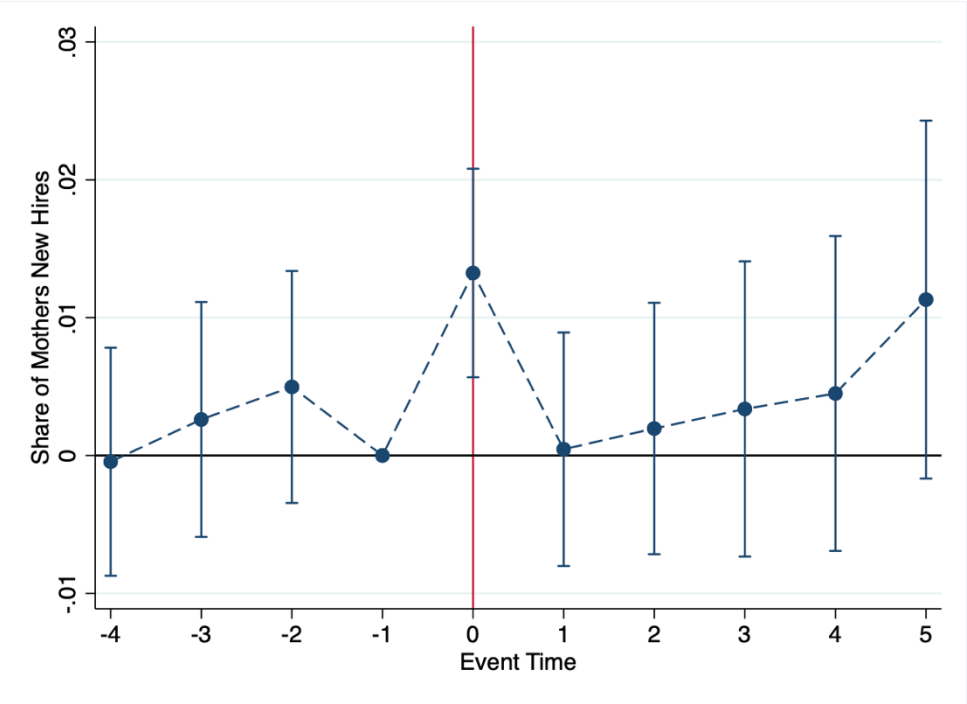
Figure 3.5. Average non-employment spell of new female hires (event study)



Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the average length of the non-employment spell of the firms' new female hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The worker's length of the non-employment spell is the number of years spent out of employment before being hired. The figure shows that the average length of the non-employment spell of new female hires evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, the average length of the non-employment spell of new female hires increased by 150 percentage points in treated firms compared to control firms, and remained larger in subsequent periods. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

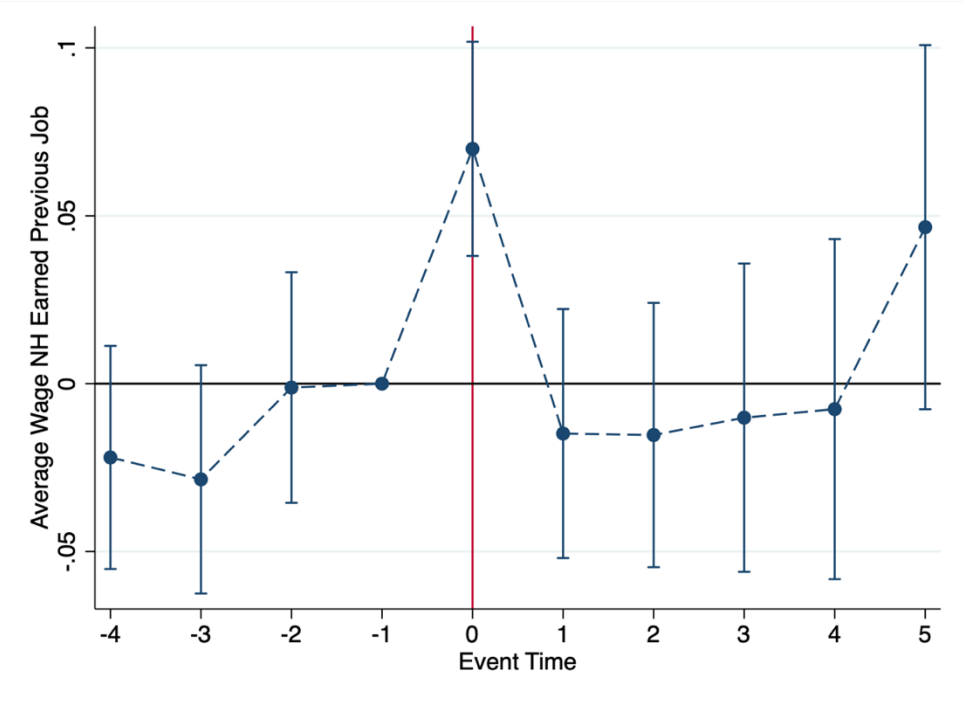
Figure 3.6. Share of female hires who have a child (event study)



Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the share of new female hires who are mothers as a percentage of new female hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. Workers are defined as mothers if they took maternity leave over the years before being hired. The figure shows that the share of mothers hired evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, treated firms increased the share of mothers hired by 1.3 percentage points compared to control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

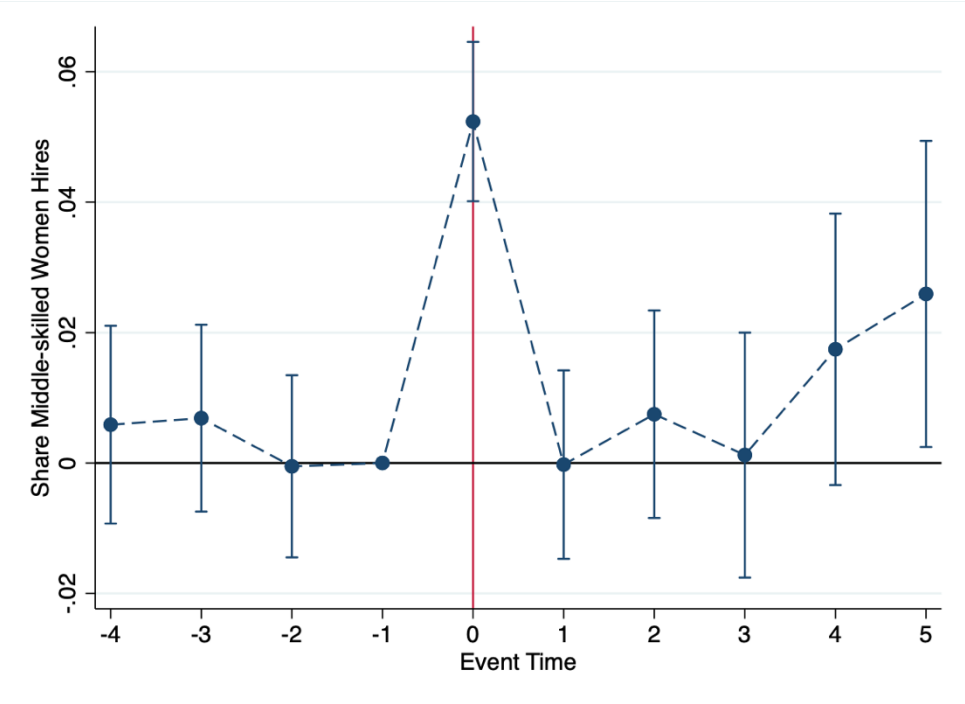
Figure 3.7. Average net wage earned in the last job (event study)



Notes: The figure plots event study coefficients (and corresponding confidence intervals) of the average net (of the employer's social security contributions) wage earned in the last job by firms' new female hires each period, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The average wage earned in the last job is the wage earned by the new female hires in the job of previous employment and it is the net of the employer's social security contributions. The figure shows that the average wage earned in the last job by the new female hires evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, the average wage earned in the last job by new female hires in treated firms is 6.9 percent higher than that earned by new female hires in control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Figure 3.8. Share of middle-skilled female hires (event study)

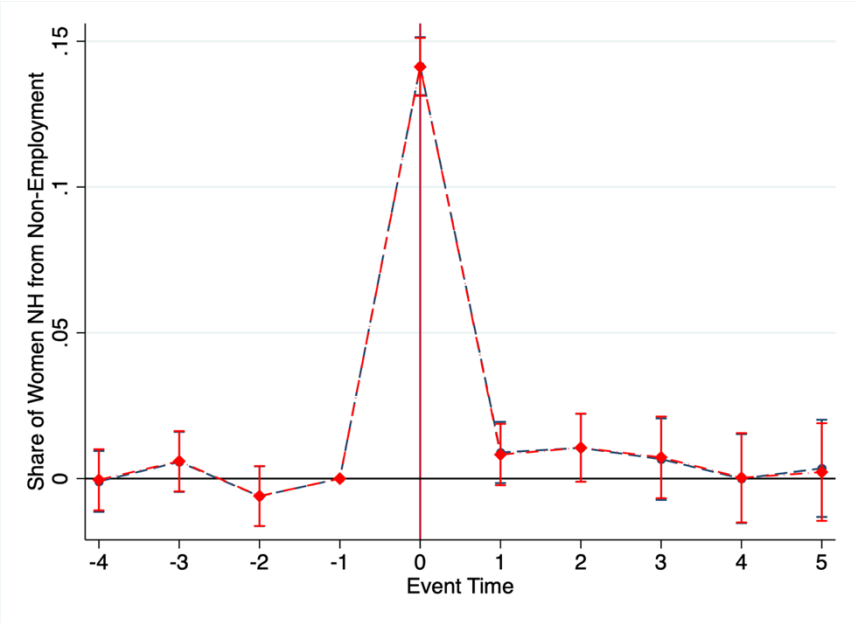


Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the share of middle-skilled female workers as a percentage of new female hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. Middle-skilled workers are those with an apprentice degree or a high school diploma. The figure shows that the share of middle-skilled female workers hired evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, treated firms increased the share of middle-skilled female workers hired by 16.6 percentage points compared to control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

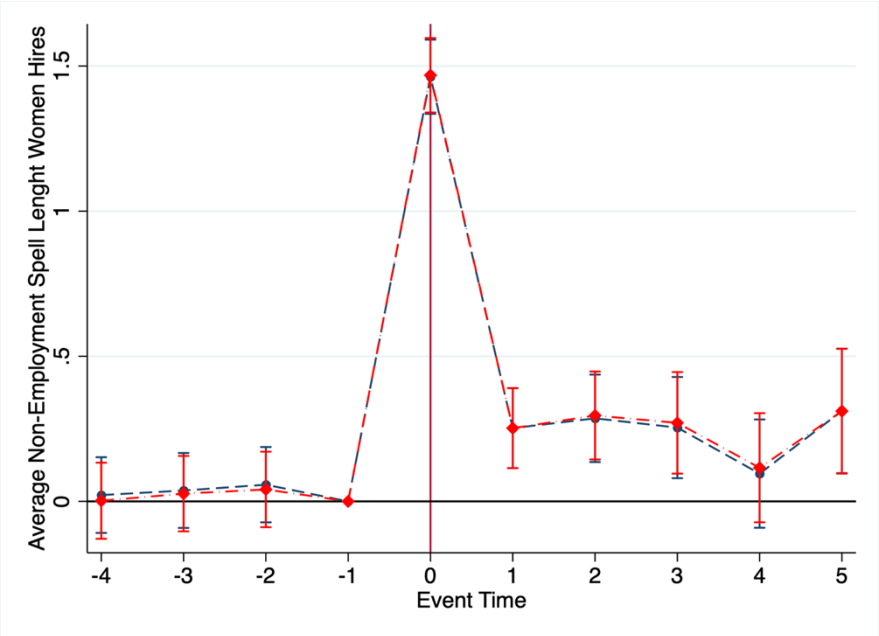
Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Figure 3.9. Robustness checks

Panel A. Percentage of female hires with employment interruptions as a share of female hires (event study).



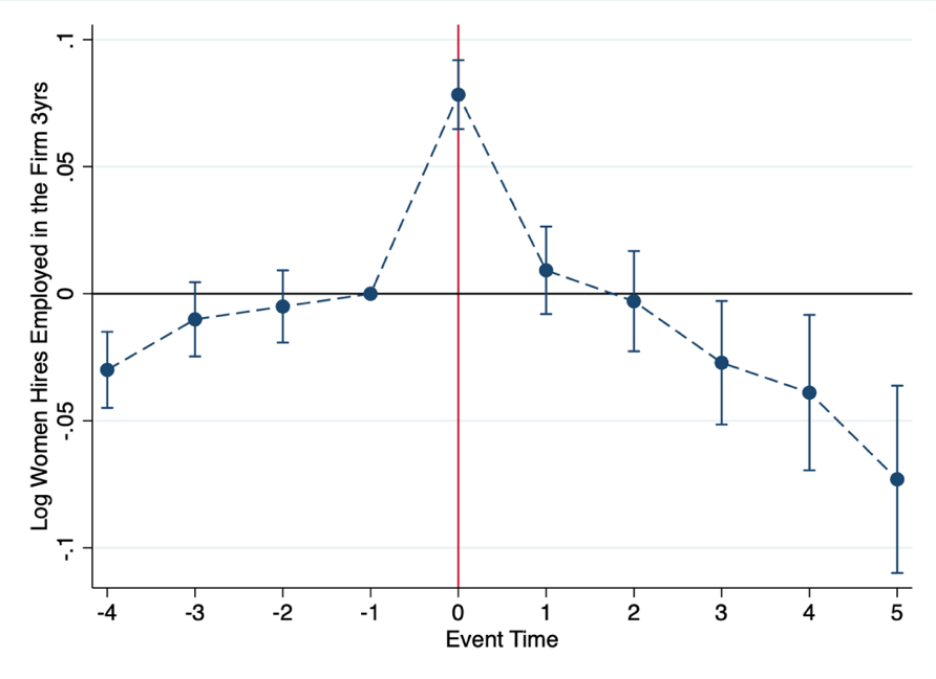
Panel B. Average non-employment spell of new female hires (event study).



Notes: The figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the percentage of female workers hired from non-employment as a share of new female hires (Panel A) and on the average length of the non-employment spell of the firms' new female hires (Panel B), estimated according to Equation 5 and with different controls: 1) with year, time from event and firm fixed effects (baseline estimates, in blue) and province-year, industry-year, time from event and firm fixed effects (robustness estimates, in blue). The figure shows that the inclusion of province-year and industry-year fixed effects does not alter baseline estimates. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

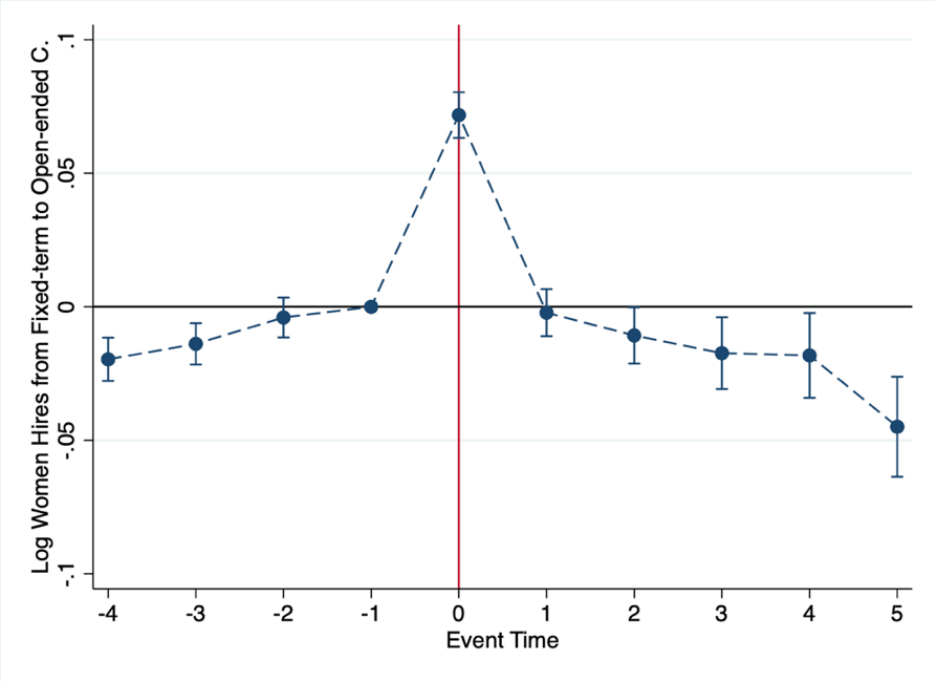
Figure 3.10. Dynamic outcomes
Panel A. Number of female new hires who will stay employed in the firm (event study)



Notes: Panel A of the figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the natural logarithm of the total number of female workers hired in that period that will still be employed in the firm within three years after the hiring, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The outcome is computed such that it is recorded at the time of the worker's hiring. The figure shows that treated and control firms did not retain (i.e. employ for at least 3 years) new female hires at a differential rate before the use of the hiring subsidy. At time 0, treated firms retain 9.7 percent more female new hires than control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Panel B. Number of female workers hired with their contract changed to an open-ended contract (event study)

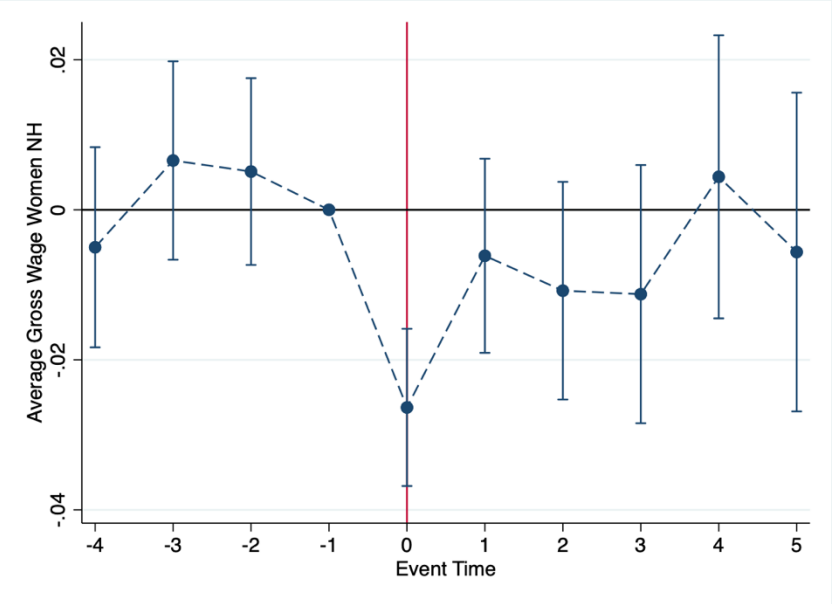


Notes: Panel B of the figure plots event study coefficients (and corresponding confidence intervals) for the effect of the adoption of the hiring subsidy on the natural logarithm of the total number of female workers hired under a fixed-term contract in that period that will have their contract changed to an open-ended one within the job spell, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The outcome is computed such that it is recorded at the time of the worker's hiring. The figure shows that treated and control firms did not change the contract of new female hires at a differential rate before using the hiring subsidy. At time 0, treated firms transform the fixed-term contract into an open-ended one to 7 percent more female new hires than control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

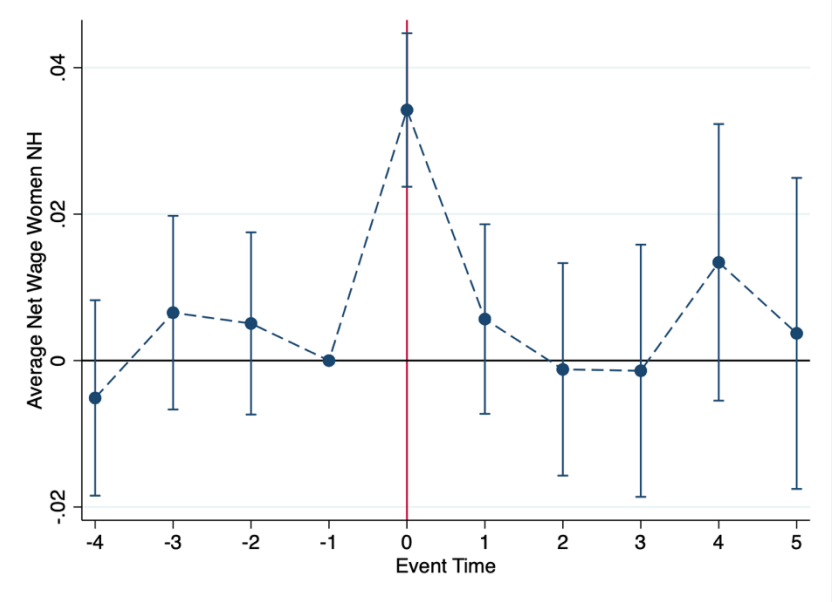
Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Figure 3.11. Average wage of new female hires (event study)

Panel A. Average gross wage



Panel B. Average net wage



Notes: The figure plots event study coefficients (and corresponding confidence intervals) of the average gross (Panel A) and net (of the employer's social security contributions) wage (Panel B) paid to new female hires, estimated according to Equation 5 (controlling for year, time from event and firm fixed effects). Differences between treated and control firms are normalized to 0 in the year before the first adoption of the hiring subsidy by treated firms (even time = 0). The vertical red line at event time 0 indicates the event time at which the treated firm hired a subsidized female worker for the first time. The figure shows that the average gross (net) wage paid to new female hires evolved similarly in treated and control firms, before the use of the hiring subsidy. At time 0, the average gross (net) wage paid to new female hires in treated firms is 2.6 (3.6) percent higher (lower) than that earned by new female hires in control firms. 95%-confidence intervals are based on standard errors clustered at the firm level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.1. Firms Descriptive Statistics.

	Hiring-Subsidized Firms	Average Italian Firm
Number of Employees (Average)	48.17	17.96
Log Weekly Wage (Average)	5.82	5.87
Share of Women among Employees (Average)	57.04%	44.08%
Share of Workers with Full-time Contracts (Average)	63.17%	69.68%
Share of Mothers among Employees (Average)	30.44%	28.61%
Share of Women among New Hires (Average)	54.42%	41.92%
Number of New Hires 2010-2012 (Average)	12.32	3.47
Share located in EU-Structural Funds Areas	47.58%	33.61%
Share of Male Dominated Occupations	38.93%	47.42%
Share of Male Dominated Industries	34.48%	43.18%
Total Firms	26,497	660,764

Notes: The table compares the characteristics of the firms that adopted the hiring subsidy ("Hiring-Subsidized Firms") and those of the other firms operating in the private sector in Italy ("Average Italian Firm") in 2012 (the year before the introduction of the hiring subsidy by the Italian government). The table suggests that hiring-subsidized firms differ from the average Italian firm.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.2 Balancing Table

	Treated Firms	Matched control firms	P-value difference
Log Weekly Wage (Average)	5.94	5.94	0.85
Number of Employees (Average)	44.8	43.8	0.85
Share of Women among Employees (Average)	55.5%	55.5%	0.81
Share located in EU-Structural Funds Areas	44.0%	27.6%	0
Share of Male Dominated Occupations	40.2%	37.2%	0
Share of Male Dominated Industries	36.2%	31.8%	0
Total number of firms	19,135	19,135	

Notes: The table compares the characteristics of treated firms and those of control firms in the year before the adoption of the hiring subsidy by the treated firm. The table also reports the p-value of the balancing test to determine whether the difference in the variables between treated and control firms is statistically significant. Treated firms are firms that use the hiring subsidy at least once between the years 2013-2019. Control firms are firms that never use the hiring subsidy. Each treated firm is matched to a control firm (following the matching procedure described in section 3.4.1). The table shows that treated and control firms are well matched on average weekly wages, total workforce, and share of female workers among employees. Instead, treated firms are more likely to be located in areas eligible for EU-structural funds (defined in Figure 3.A-1), to operate in a male-dominated sector (defined in Table 3.A-1), and to have a larger share of male-dominated occupations (defined in Table 3.A-2). According to the legislation (highlighted in section 3.2.2.1), firms located in these locations, or operating in male-dominated industries, or hiring a female worker in a male-dominated occupation, could use the subsidy to hire a female worker with an employment interruption of 6 months (as opposed to an employment interruption of 2 years, which is the standard length of non-employment spell otherwise required by the legislation for the use of the hiring subsidy).

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.3 Regression Results for Bottom Quartile and Top Quartile

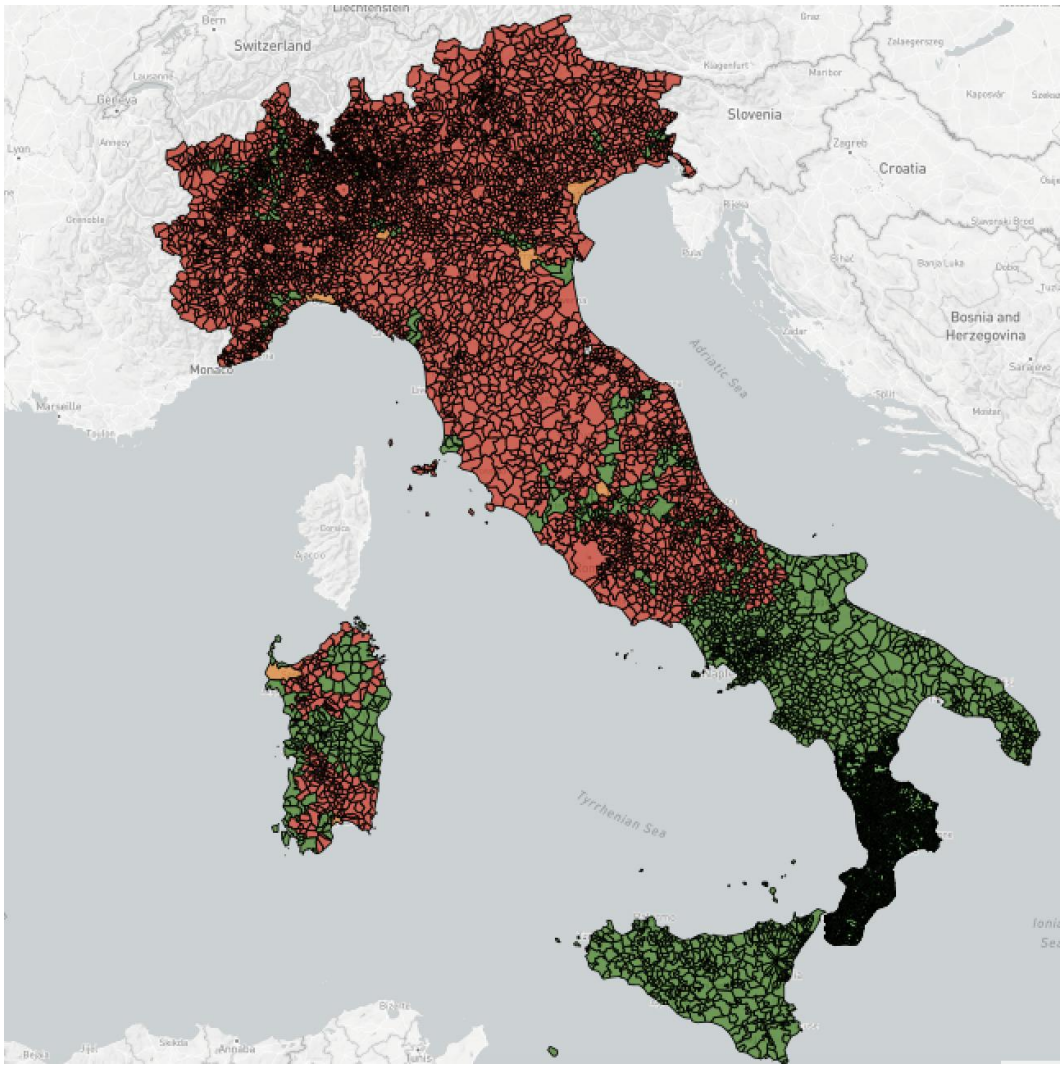
	(1)	(2)	(3)	(4)
	Log Subsized Hires	Share of Women new Hires from Non-Empl	Log Long Term Non-Employed workers	Log Middle-Skilled New female Hires
Bottom Quartile	0.12***	0.016***	0.031***	0.024***
	(0.01)	(0.021)	(0.016)	(0.025)
Top Quartile	0.17***	0.050***	0.061***	0.083***
	(0.02)	(0.020)	(0.016)	(0.025)

Notes: The table compares the average post-treatment coefficients (and corresponding standard errors, in parenthesis) of different outcomes between treated firms in the bottom quartile and those in the top quartile. Treated firms in the bottom quartile hire a low-quality subsidized worker at time 0, while treated firms in the top quartile hire a high-quality subsidized worker at time 0 (top quartile firms). The worker's quality is defined by the worker's wage residual (estimated on the wage earned in the previous job, before being hired under the hiring subsidy as described in section 3.6) and its position in the workers' wage residuals distribution. The regressions are estimated following equation (5), separately by treated firms in the bottom quartile (and their matched control firms) and treated firms in the top quartile (and their matched control firms). The table reports the average magnitude (and standard errors) of the coefficients estimated over event time 1-5 (i.e. over the periods after the first adoption of the hiring subsidy at time 0). The table shows that Top quartile treated firms are more likely to hire 1) subsidized workers in subsequent periods (column 1), 2) female workers with employment interruptions (column 2), and 3) with employment interruptions longer than 4 years (column 3) and 4) middle-skilled workers. Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Appendix 3.A. EU-Structural Funds Area and Male-Dominated Sectors and Occupations

Figure 3.A-1. Municipalities eligible for EU structural funds.



Notes: The figure shows Italy and its municipalities (whose boundaries are delimited by a continuous black line). Municipalities eligible for EU structural funds are coloured in green, municipalities with only some areas eligible for EU structural funds are coloured in orange and municipalities not eligible for EU structural funds are coloured in red. Municipalities eligible for EU structural funds are identified by two Ministerial Decrees, in 2008 and 2014. The figure shows that the whole of the South of Italy, including Sicily, and part of Sardinia, are eligible, while few locations in the Centre and North of Italy are.

Source: 2008 Ministerial Decree and 2013 Ministerial Decree

Table 3.A-1 Male Dominated Sectors

Sector Code	Sector	Male Dominated Sector
A	Agriculture, Forestry and Fishing	Y
B	Mining and Quarrying	Y
C	Manufacturing	Y
D	Electricity, Gas and Steam Supply	-
E	Water Supply; Sewerage and Waste Management Activities	Y
F	Construction	Y
G	Wholesale and Retail Trade; Repair of Motor Vehicles and Motorcycles	-
H	Transportation and Storage	Y
I	Accommodation and Food Service Activities	-
J	Information and Communication	Y
K	Financial and Insurance Activities	-
L	Real Estate Activities	-
M	Professional, scientific and technical activities	-
N	Administrative and Support Service Activities	-
O	Public Administration and Defence	Y
P	Education	-
Q	Human Health and Social Work Activities	-
R	Arts, Entertainment and Recreation	-
S	Other Service Activities	-
T-U	Other Activities (Households as Employers and Extraterritorial Organisations)	-

Notes: The table reports the sectors identified as male-dominated ("Y") and those not ("-") over the years 2013-2019. The sector code follows the 1-digit NACE Rev.2 industry classification. Male-dominated sectors have a gender employment gap 25% larger than Italy's average gender employment gap. They are identified by yearly Ministerial Decrees. There was no variation in the male-dominated sectors over the years 2013-2019.

Source: Yearly Ministerial Decrees between 2013 and 2019.

Table 3.A-2 Male Dominated Occupations

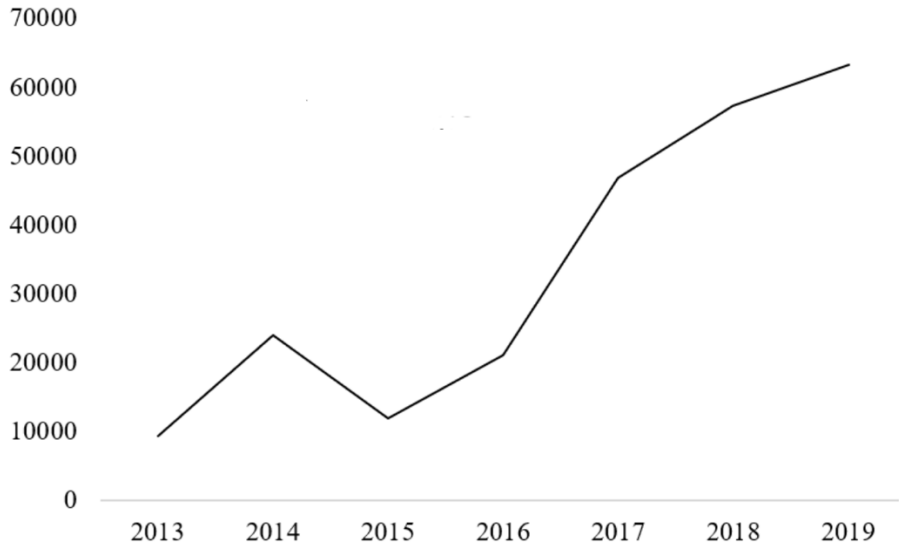
ISCO-88	Occupation	Male Dominated - Years
11	Legislators and Senior Officials	2013, 2015-2019
12	Corporate Managers	2013-2019
13	Managers of Small Enterprises	2013-2019
21	Physical, Mathematical and Engineering Science Professionals	2013-2019
22	Life Science and Health Professionals	2013-2017
24	Other Professionals	2016
31	Physical and Engineering Science Associate Professionals	2013-2019
61	Skilled Agricultural and Fishery Workers	2013-2019
71	Extraction and Building Trades Workers	2013-2019
72	Metal, Machinery and related Trades Workers	2013-2019
73	Precision, Handicraft, Craft Printing and related Trades Workers	2013-2019
74	Other Craft and related Trades Workers	2013-2019
81	Stationary Plant and related Operators	2013-2019
82	Machine Operators and Assemblers	2013-2019
83	Drivers and Mobile Plant Operators	2013-2019
91	Sales and Services Elementary Occupations	2017-2019
92	Agricultural, Fishery and related Labourers	2013-2019
93	Labourers in Mining, Construction, Manufacturing and Transport	2013-2019
01	Armed Forces	2013-2019

Notes: The table reports the occupations identified as male-dominated and the years when they were identified as male-dominated over the period 2013-2019. The occupation code follows the ISCO-88 classification. Male-dominated occupations have a gender employment gap 25% larger than Italy's average gender employment gap. They are identified by yearly Ministerial Decrees.

Source: Yearly Ministerial Decrees between 2013 and 2019.

Appendix 3.B. Additional Figures and Tables

Figure 3.B-1 Number of women hired under the hiring subsidy over time.



Notes: The figure plots the number of women hired under the hiring subsidy between 2013 and 2019. The vertical axis reports the total number of women yearly hired each year. The figure suggests that few women are hired under the subsidy in 2013 (the year of the introduction of the subsidy by the Italian government). Over time, we observe a slow and gradual increase in the number of female subsidised hires.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.B-1. Firms Size Distribution

Employees	Hiring-Subsidized Firms	%	Average Italian Firm	%
1-4	8,346	31%	285,188	43%
5-15	11,595	44%	274,251	42%
15-50	4,691	18%	82,316	12%
51-250	1,552	6%	20,592	3%
more than 250	313	1%	3,237	0%
	26,497		660,764	

Notes: The table compares the size distribution of the firms that adopted the hiring subsidy ("Hiring-Subsidized Firms") and that of the other firms operating in the private sector in Italy ("Average Italian Firm") in 2012 (the year before the introduction of the hiring subsidy by the Italian government). The table shows that a greater share of hiring-subsidized firms has more than fifteen employees.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.B-2. Firms Geographical Distribution

	Hiring-Subsidized Firms (%)	Average Italian Firm (%)
Centre	21.13	21
Islands	11.67	8.47
North East	17.79	23.21
North West	20.06	29.38
South	29.35	17.94
	26,497	660,764

Notes: The table compares the geographical distribution of the firms that adopted the hiring subsidy ("Hiring-Subsidized Firms") and that of the other firms operating in the private sector in Italy ("Average Italian Firm") in 2012 (the year before the introduction of the hiring subsidy by the Italian government). The table shows that hiring-subsidized firms are more likely to be located in the South of Italy and in the Islands (Sardinia and Sicily), that is, areas eligible for EU-structural funds (as shown in Figure 3.A-1).

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 3.B-3 Workers Descriptive Characteristics.

	Subsidized Women	Average Female Hire
Mean Age	38.54	35.19
Share of low-skilled workers	12.3%	14.6%
Share of medium-skilled workers	78.3%	56.9%
Share of high-skilled workers	9.5%	9.9%
Mean log weekly wage last job	5.76	5.73
Mean log weekly wage current job	5.76	5.69
Average non-employment spell length (years)	6.08	5.42
Share of mothers	7.2%	6.4%
Share hired under a fixed term contract	73.3%	57.4%
Share hired under a part time contract	68.5%	72.8%
Share of apprentice	0.0%	7.1%
Share of blue collar	60.8%	56.6%
Share of white collar	39.1%	35.5%
Share of managers	0.1%	1.3%
Observations	183,615	10,043,297

Notes: The table compares the characteristics of the female workers hired under the subsidy ("Subsidized Women") and those of all other women newly hired by a firm ("Average Female Hire") between 2013 and 2019.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Chapter 4

Gender Composition of Management and Employment of Young Female Workers

4.1 Introduction

Women's labor force participation in developed countries has grown at an unprecedented pace over the twentieth century; in the US it was at a rate of around 20 percentage points in the 1920s and increased to around 60 percentage points in the 1990s (Olivetti and Petrongolo, 2016), narrowing the employment gap between men and women. However, over the last few decades, the convergence in labor force participation between men and women has slowed considerably, with women's labor force participation still below 80 percentage points in the mid-2000s (as documented for the US by Goldin, 2006; and by Olivetti and Petrongolo, 2016, for 19 high-income countries). Despite this increase in women's labor force participation, women on average earn less than men (as documented, among others, by Olivetti and Petrongolo, 2016).

There are important reasons behind the sustained gender disparities in employment and wages. First, women tend to be employed in lower-paying occupations and firms (Blau and Kahn, 2017). Second, women are both over-represented in part-time employment and are more likely than men to have employment interruptions, particularly when they have children (Bertrand, Goldin, and Katz, 2010; Kleven, Landais, Posch, Steinhauer, and Zweimuller, 2019). Third, women tend to be overrepresented at the lower and underrepresented at the upper end of the career ladder (Andrew, Bandiera, Costa-Dias, and Landais, 2021).

These three dimensions of gender inequality mutually feed and re-enforce one other. On the one hand, employment in a part-time job and more flexibility in hours worked hinders workers' likelihood of career progression and promotion (as shown in Bertrand et al., 2010, for business type occupations and in Goldin, 2014). Thus, a higher concentration of women in part-time jobs may be an important contributor to the female underrepresentation at the higher levels of the firm hierarchy. On the other hand, the strong underrepresentation of women in positions of power in the corporate world might be bound to perpetuate differences in labor market outcomes between men and women, either by creating differences in expectations across genders, limiting the number of role models and mentors available to women, or directly affecting promotion decisions (Bertrand, 2018).

Perhaps surprisingly, at the entry-level, men and women with the same education and comparable occupations earn similar wages (Bertrand et al., 2010; Goldin, Kerr, Olivetti and Barth, 2017). As their careers progress, differences between men's and women's careers start to emerge; and the divergence continues throughout their labor market trajectories (Bertrand et al., 2010; Goldin et al., 2017).

In this study, we aim to tackle these dynamics by shedding light on the impact of the gender composition of management on the employment outcomes of women. We specifically focus on the worker's initial firm and analyse the impact of its gender composition of management on women's future labor market trajectories[80]. There are various channels through which the gender composition of management could affect women's labor market outcomes. One the one hand, female managers might act as mentors or role models to other women by helping new female labor market entrants with on-the-job learning, setting goals, and advocating for them (Athey, Avery and Zemsky, 2000). This arguably should positively impact women's labor market trajectories, such as improved employment and full-time employment probabilities. On the other hand, the impact of female management can be

[80] By focusing on the firm's characteristics at the time of entry, we abstract from dynamic considerations, such as path dependence.

negative if the ‘‘queen bee’’ phenomenon is common or if some implicit limits exist on the number of women who are expected to advance within a firm (Kunze and Miller, 2017). Finally, since new labor market entrants are more mobile, often being employed in a firm other than the firm of first employment within the first 10 years of labor market entrance (Topel and Ward, 1992), the characteristics of the gender of management at the initial firm might not be relevant in the long run. If this is the case, we would expect no effect of the initial management on future labor market outcomes.

Our empirical analysis draws on Italian administrative linked employer-employee data, comprising all firms and workers of the private sector. It is provided by the Italian National Institute for Pension and Welfare (INPS**).** These data are ideally suited for our purposes. They allow us to reliably identify labor market entrants, managers, as well as executives and mid-range managers. This latter feature is unique to our dataset, as managers' role is often identified through survey data (Gagliarducci and Paserman, 2015) and is rarely available in administrative data (Flabbi, Macis, Moro, and Schivardi, 2019). The data further allow us to follow workers over time, throughout their whole labor market trajectories.

As our focal workers, we extract the entire population of male and female labour market entrants who started their first full-time job in Italy over the period 2000-2009. We compute the share of female managers during these workers’ first year in the labour market. We subsequently track the female workers for the first ten years in the labour market to investigate whether their future labour market success, such as employment status and job-to-job transition**,** is affected by the share of female managers**.** While our research question is focused on women, we also investigate the effect of the share of female managers on male workers to elicit whether the impact of the gender of management on future labor market outcomes is the same between women and men.

Our empirical design exploits within-firm variation in the gender composition of its management across entry cohorts. In our baseline analysis we also control for the firm time-varying characteristics, such as employment, average wages and wage growth. Focusing on the within-firm variation and

wage levels, we account for the differential sorting of women in lower-paying firms (Blau and Kahn, 2017). By controlling for the firm's employment and wage growth, we account for the firm's time-varying shocks that could have an impact on future labor market outcomes of new entrants, over and above the effect of the gender composition of management, which is our effect of interest.

Our results show that entering the labor market at a firm with more women among managers positively affects the future employment probabilities of female labor market entrants. Our preferred estimates show that an increase in the share of female management of 10 percentage points leads to a 0.2 percentage points increase in the probability of being full-time employed ten years after labor market entry. Similarly, for the same increase in the share of female managers, our results show an increase of 0.3 percentage points in the probability of changing jobs ten years after labor market entry, with a higher likelihood of moving to a better-paid job. Significantly, management gender composition appears to have an impact on the decision to return to work after maternity leave. This is an important finding, given that the divergence in earnings between men and women widens after the birth of a child, with women earning 45 percent less than men after ten years since childbirth (as shown, among others, by Kleven, Landais, and Søgaard, 2019). The impact of female management on the post-maternity return decisions is also particularly relevant in the poignant context of Italy, where 40 percent of mothers are out of the labor force.

Our paper speaks to several strands of economic literature. First and foremost, we contribute to the literature on the role of women in leadership positions in the firm. Prompt by the introduction of quotas on firm's corporate board, this literature has primarily focused on the increase of women on corporate boards and its effect on firm's performance (Matsa and Miller for Norway, 2013 and Comi, Grasseni, Origo, and Pagani, 2020, for France, Italy and Spain and Smith, 2018, for a review). Notable exceptions are Bertrand, Black, Jensen and Lleras-Muney (2019), who investigate the effect of board quotas on female workers in Norway and Maida and Weber (2022), who study the effect of board quotas on women in high-rank in Italian firms.

The results of both studies, however, suggests that such quotas have little effect, if any, on female workers.[81] This lack of spillovers from corporate boards to the rest of the firms' workers may not be as surprising as may seem at the first glance. This is especially so if we consider the role of corporate boards in firm's governance, which is limited to the representation of shareholders' interests, supervision of corporate activities and performance assessment, without a direct involvement in the management of the firm.

If the goal is to understand the role of women in positions of power on workers, it is of primary importance to assess the impact of gender among managers, as these relate directly to the day-to-day running of the firm. This literature, however, is slight and its findings are often mixed. This is partly due to data availability, as managers' role is often identified through survey data or only in specific settings. For example, Gagliarducci and Paserman (2015) identify managers using survey data and find that controlling for firm fixed-effects, no significant association exists between women's labor market outcomes and female leadership. Lucifora and Vigani (2021) exploits cross-sectional data for a large set of European countries and find that female supervisors reduce the gender gap. Bagues, Sylos-Labini, and Zinovyeva (2017) find evidence of detrimental effect of female evaluators on academic promotions of female candidates in Spain and Italy, driven by the shifts in behaviour of male evaluators. The Italian setting, which allows us to examine large-scale administrative linked employer-employee data and identify managerial roles, permits us to estimate the effect of female managers on many labor market outcomes.

Few studies investigate the effect of female managers using administrative linked employer-employee data. Nearly all focus on contemporaneous worker-firm relationships and only examine the effect of female managers on female and male workers' wages. Cardoso and Winter-Ebmer (2010) focus on Portugal and find that female managers have a

[81] Bertrand et al (2019) find that although women who made it to the board are considerably better off, there was little spillover over the rest of women in the firm. Maida and Weber (2022) show that although quotas indeed changed the gender composition of corporate boards, there was no impact on the gender composition among firms' high ranks in general.

negative effect on wages but a positive effect on the gender wage gap. The Italian data have been used to study the impact of the gender of executives on the wage distribution of male and female workers (Flabbi et al., 2019). They show that under a female CEO, females in high-wage positions gain relative to their male colleagues, whereas females in low-wage positions tend to lose relative to males. A notable exception in the investigation of female managers on workers' wages is Kunze and Miller (2017). They use a similar identification strategy as ours and investigate the effect of female managers on promotions, finding evidence of positive effects for female promotions in Norwegian firms.

This paper contributes to the literature on several important dimensions. First, we use state-of-the-art large-scale administrative linked employer-employee data. This data allows us to focus on workers in a crucial moment of their careers, upon their entry in the labor market, as opposed to the studies mentioned that focus on contemporaneous effects. Furthermore, the panel structure of the data also allows us to study the dynamics of these effects. Second, career interruptions seem to be an important contributor to the labor market gender disparities, hence we focus on the employment effect of female managers. Third, in addition to employment, we analyse the impact of female managers on a number of labor market outcomes, including job-to-job mobility and maternity. Finally, exploiting the special features of this data, we can analyse the impact of gender among mid-range managers versus executives thus directly talking to the particular channels through which managers' gender composition may affect workers.

Finally, our paper also contributes to the recent literature on the characteristics of the first job on long-term labor market outcomes. Early studies in this area have focused on specialised workers such as PhD-level economists (Oyer, 2006), MBAs (Oyer, 2008) or CEOs (Schoar and Zuo, 2017). Few studies have focused on a broader category of workers. Von Wachter and Bender (2006), who document long-lasting losses for German apprentice workers who involuntarily separate from their training firm and Müller and Neubaeumer (2018) argue that training at a larger firm leads to lower unemployment later on in Germany. Finally, Arellano-Bover (2022)

shows that starting employment in a larger firm improves long-term outcomes such as lifetime income. To our knowledge, this study is the first to establish a direct link between young female workers' first-management characteristics and long-term outcomes, tracing how early-career management heterogeneity can have implications for long-term labor market outcomes.

The chapter is organised as follows. Section 4.2 describes the gender employment and wage gap in Italy. Section 4.3 presents the data and descriptive statistics of the female and male labor market entrants we study, who enter employed between 2000 and 2009. Section 4.4 presents the identification strategy. Section 4.5 presents the estimates and robustness. Section 4.6 concludes.

4.2 Institutional Setting

Women's labour force participation in Italy is low compared to other developed countries; indeed, in Italy, the gender employment gap, the difference between the employment rate of men and that of women, lies at 20.1 percentage points (Eurostat Database, 2021a). Thus, the Italian gender employment gap is nearly ten percentage points larger than the European average, which measured at 10.4 percentage points in 2021, and is the largest among European countries (Eurostat Database, 2021a). The employment rate of women with children is particularly low, as on average 53.6% of mothers with children are employed, compared to 62.6% of women without children and 87.7% of fathers (Eurostat Database, 2021b).
On the other hand **--** and perhaps not surprisingly -- in Italy, the gender wage gap, the difference in the average wage earned by men and that earned by women, is low, at 4.2% (Eurostat Database, 2020), and one of the lowest among European countries. This small difference can be explained by the positive selection into employment of high-skilled Italian women. Indeed, accounting for the non-employed and the selection into employment would cause the wage gap in Italy to rise dramatically: the Italian gender wage gap would increase fivefold (as shown by Olivetti and Petrolongo, 2008).

4.3 Data and Descriptives

4.3.1 Data

The principal data source for our analysis is linked employer-employee data provided by the Italian Social Security Institute (INPS). This high-quality administrative data covers the universe of workers and firms in the Italian private sector from 1980 to 2020. The data allow us to follow workers over time, starting from the time of their labor market entry. It includes information on individual wages, the number of weeks worked each year (in full-time equivalent), year of labor market entry, type of employment (full-time and part-time), type of contract (fixed-term and open-ended contract), broad occupational category, different types of employment regimes, and presence and type of employment related benefits (such as maternity leave take-up, sick-leave, hiring subsidies). The data also include demographic characteristics such as age, gender, nationality, year of birth, and year of death.

To proxy for individual skill level, we rely on the broad-occupational variable that classifies workers' jobs into apprentice, blue-collar, white-collar and managerial occupations[82]. Since 1996. the same broad occupational variable also separately identifies mid-range managers and executives within a firm**.** We use this unique feature of the data in our analysis to shed lights on the channels through which gender among managers may affect the outcomes of new entrants. Finally, the information that the data provides on benefits is particularly useful as it allows us to identify maternity leave spells among female workers since 2005. On the firm side, the data covers information on the firm's geographic location and the five-digit industry code. Given the linked employer-employee nature of the data, further firm

[82] Educational attainment and detailed occupation information are unavailable for most workers and cannot be used consistently in the analysis (these variables are only available for new hires starting in 2008).

characteristics, such as employment composition and average wages, can be computed from the data.

4.3.2 Sample

As our focal workers, we extract the entire population of Italian workers aged 16 to 30 who entered the labor market for the first time as full-time employees between 2000 and 2009[83]. We restrict our analysis to the 2000-2009 cohort based on two considerations. First, we seek to follow workers' careers for ten years from labor market entry. Given the strongly unequal effect that COVID19 pandemic had on men and women, we do not use information for years 2020 and 2021, which means that the last entry cohort we can observe for ten years is that entering in 2009. Second, maternity leave can only be observed after year 2005. While we might miss some maternity leaves of female new entrants between 2000 and 2004, these are expected to be relatively few, given the fact that in Italy the median age at first birth is 32. Going further back in time, however, would make the problem more prominent. To allow workers to work in a part-time job during the years of education, we allow our focal workers to have worked a maximum of 4 years part-time before the full-time labor market entry[84].

We additionally impose some sample restrictions on the firm where the focal worker is hired when entering the labor market. We drop from the sample the primary sector and international organisations, as these sectors tend to have peculiar working patterns. We drop single-gender firms: these are firms that, between 2000 and 2009, only employed workers of one gender. Finally, we drop firms with less than five employees in the year before the focal worker's labor market entry. We exclude these firms because the role of management in these firms tend to be performed by the firms' owner and business owners are not covered in our data (as the INPS data cover employees only). Furthermore, in order to be able to control for

[83] More in detail, we first build a panel dataset that comprises one observation per worker per year. If a worker holds multiple jobs in the year, we select the primary job, the job associated with the highest number of weeks worked. We then select our sample of focal workers

[84] 92% of our focal workers enter the labor market for the first time as full-time workers

employment and wage growth in a firm, we require the firm to be operating one year before the focal worker's labor market entry.
We then compute measures of the number of female managers (in relative terms) during the focal workers' first year in the labour market. We subsequently follow the focal workers for ten years and track their labor market trajectories, such as full-time employment and job-to-job transition, in year 2, 5 and 10 since labor market entry. It is to be noted that although our main focus is on the focal workers, all the firm-level outcomes and controls are based on the whole population of focal worker's colleagues.

Table 4.1 shows the summary statistics for the data. Although, our primary sample of interest is women, throughout the paper we look at both genders, analysing how a firm's gender composition may affect both women and men. The descriptive characteristics for female and male entrants are reported in columns 1 and 2 of Table 4.1, respectively.
Women appear to enter full-time employment one year later than men on average. Although this is likely to be because women stay longer in full-time education, in the first year of employment, women's wages are about 4 per cent lower than those of male entrants. Additionally, women start their labor market careers in vastly different conditions than men. Women's firms of initial employment tend to be considerably smaller and with a higher share of women among employees and managers. These firms also seem to grow less in terms of employment and wages. Both female and male labor market entrants are employed in firms with negative wage growth, but this is even more prominent in firms where women start their employment careers.
In conclusion, women and men sort into different firms from the time of career onset. In our analysis, we account for this sorting by including the firm's fixed effect (see section 4.4).

4.4 Empirical Strategy

Measuring firms' gender composition. This study aims to understand how firm gender composition of management impacts the career

development of men and women**.** To capture firm management gender composition, we use the share of women among managers:

$$FMNG_{j\tau} = \frac{\sum_{i \in j\tau}^{N_{j\tau}^{mng}} I[Female_i = 1] \times I[Manager_i = 1]}{N_{j\tau}^{mng}}$$

where $N_{j\tau}^{mng} = \sum_{i \in j\tau}^{N} I[Manager = 1]$ comprises the number of managers in a firm j in labor market entry τ. Hence, $FMNG_{j\tau}$ specifies the share of women among all managers in a firm in a given year of labor market entry τ.

Empirical specification.

An important challenge to identification is the differential sorting of women and men across firms and occupations. This generates non-random variation in firms' gender composition. A comparison between workers entering different firms will not recover the causal effect of firm's management gender composition on the career path of labor market entrants. To deal with this endogeneity, we exploit within-firm variation in management gender composition, controlling for (fixed) unobserved firm components**,** by including the firm fixed effect of the initial firm μ_j. Similarly, to account for the differential sorting in occupations of men and women, we control for (broad) occupation fixed effect, included in the set of controls of $X_{i\tau}$. Even when controlling for unobserved firm and occupation heterogeneity, we may still encounter changes in firm performance, policies as well as management gender composition. If faster-growing firms or firms with faster-growing wages are more likely to hire women, regressing labor market outcomes on firm composition would still not avail us to identify the causal impact of interest. For this reason, we additionally control for time-varying firm characteristics that proxy for firm's growth: employment growth, wage growth and lagged average wages and employment.

The following equation describes our main empirical specification:

$$y_{i,\tau+t} = \beta_1 FMNG_{ij\tau} + X_{i\tau} + \xi_{ij\tau} + \mu_j + \delta_\tau + u_{i\tau+t} \quad (6)$$

The main regressor is $FMNG_{ij\tau}$**.** This is the share of women among managers in firm j, in year τ of labor market entry of individual i. The coefficient of interest is β_1, which estimates the impact of the share of women among managers on $y_{i,\tau+t}$, future labor outcomes of individual i in year $\tau + t$ since labor market entry; $X_{i\tau}$ is the set of individual characteristics measured at year of labor market entry τ: workers' age and skill (occupational) level; $\xi_{ij\tau}$ is the set of time-varying firm characteristics presented before (lagged average wage and employment, wage and employment growth of the initial firm j over the year before labor market entry τ of individual i); μ_j and δ_τ are firm and year (or, equivalently, entry cohort) fixed effects. We are interested in the impact of the initial employment conditions; hence our regressors of interest are measured in the initial employment year for each worker. The outcome variables $y_{i,\tau+t}$ are defined below and reflect the individual's employment outcomes measured years two, five and ten, representing short-, mid- and long-term effects.

Finally, to prove the robustness of our results we include firm-occupation fixed effects to control for the potential sorting of men and women into different occupations in the same firm[85] and show that the magnitude of our coefficients is in line with the estimates obtained from the inclusion of occupation-year fixed effects.

Outcomes. We compute from the data several outcomes designed to measure individuals' progress in the labor market at different stages of their careers. These can be classified into three blocks: employment, cross-firm mobility and maternity. Our main outcomes of interest are employment and full-time employment indicators. We look at whether a worker is observed in (full-time) employment in years two (short-term), five (medium-term) and ten years (long-term) post labor market entry. We then decompose employment effects into employment in the same firm and employment in a different firm

[85] Additionally, in future research, we plan to exploit managers' death as an instrument for the change in the share of female managers in the firm. The year of deaths is indeed available in the INPS data. Two recent working papers have adopted the instrument (Acemoglu, He and Le Maire, 2022, for Denmark; and Schivardi and Sauvagnant, 2020, in the Italian context)

(that is, job-to-job transition). In a seminal study, Topel and Ward (1992) show that a large part of wage growth among young workers stems from firm changes early in their career. With this goal in mind, namely to understand movement to a better job, job-to-job mobility is further decomposed into upwards and downwards movement, namely movement to a job where the worker earns a higher (lower) wage. Finally, we analyse maternity-related outcomes and decisions, given that a large part of the gender-related labor market differences seem to emerge after the birth of the first child. This "child penalty" can be mainly attributed to women either exiting the labor force, working part-time or working in lower paying firms and occupations after having a child (Kleven, Landais, Posch, Steinhauer, and Zweimuller, 2019; Kleven, Landais and Søgaard, 2019). For this reason, we study the impact of firm gender composition on the maternity leave take-up at each stage of the career, as well as the probability of returning to work and returning to work as a full-time worker.

4.5 Results

4.5.1 Employment

Impact on employment and full-time employment. We begin by analyzing the impact of gender of management, on employment probabilities. In panel A of Table 4.2, we look at the impact of the share of female managers on woman's employment and on full-time employment probabilities in years two, five, and ten after labor market entry.
Having more women among managers has a small and statistically insignificant effect on the probability of being employed in year 2 and 5 after labor market entry. In year 10, increasing the share of female managers in the initial firm by 10 percentage points increases employment of female labor market entrants by 0.22 percentage points. This effect seems to be mostly explained by the increase in full-time employment, as the probability of working full-time increases by nearly the same magnitude, 0.2 percentage points, in year 10 since labor market entry**.**

In Panel B of Table 4.2 we show the same analysis for male labor market entrants. The pattern of employment effect is similar among male workers, but the magnitude of the effects is considerably larger: having more women among managers has a positive, insignificant, effect on the probability of being employed in year 2 and 5 for male labor market entrants. In year 10, increasing the share of female managers by 10 percentage points increases employment of male labor market entrants by 0.3 percentage points. The magnitude of the effect on full-time employment is even larger than on employment overall (0.4 percentage points for a 10 percentage points increase in the share of female managers), which also implies a reduction in part-time employment among men.

These employment results lead us to a two-fold conclusion. First, gender composition of management in a first firm seems to have positive and increasing impact on employment. While previous research by Müller and Neubaeumer (2018) and Arellano-Bover (2022) show that the size of the first firm could matter for workers' career, our findings suggest that firm's gender composition could have a long-term impact on worker's labor market trajectory. Second, the results show that having more women among managers in a firm has a positive impact on (full-time) employment of both men and women, although the impact on men is larger than that for women. This means that although the effect on female employment is positive, the employment gap is actually increasing. This is in contrast with Gagliarducci et al. (2015) who find no impact of the gender of management on future employment probabilities of female workers in Germany. It is to be noted that there are important differences between our study and Gagliarducci et al. (2015). First, we focus on Italy, while they investigate the German context. Second, we focus on new labor market entrants, while they focus on all workers employed.

Impact of mid-range managers and executives

In our previous table, we have pulled together mid-range managers and executives in our definition of managers. A unique feature of the data is that it allows us to distinguish between these two groups of managers. The channels

through which gender composition of mid-range management and executives may impact women's careers may be very different. On one side, mid-range managers are more likely to have a more direct interaction with the new hires, they may be their line managers and they may act as mentors. Any wage negotiations are also more likely to occur between workers and mid-range managers as opposed to executives. Since women are more likely to engage in salary negotiation with female managers (Biasi and Sarson, 2022) having a larger share of female mid-range managers could also positively impact the career progression of women through better negotiation opportunities.

On the other side, executives may serve as role models and aspirational models for new hires, which would positively impact women's career progression through an impact on women's aspirations. Furthermore, female executives may shape firms' female-friendly policies (Gagliarducci et al., 2015) that could have an additional impact on female workers[86].

In Table 4.3, we separately consider the impact of gender composition at different levels of management on employment. To ease exposition, the table follows the same structure as Table 4.2. Panel A shows the coefficients of the analysis for female workers. The results suggest that the positive effect of the gender of management on female employment (presented in Panel A of Table 4.2) seems to be that of female executives. Executives have a positive and significant effect on the probability of remaining employed in the medium and long run. In year 10, increasing the share of female executives by 10 percentage points increases employment of female labor market entrants by 0.38 percentage points, with the effect stemming from an increase in full-time employment. On the contrary, mid-range managers have no effect on the probability of remaining employed in the short (year 2), medium (year 5) and long run (year 10).

In Panel B of Table 4.3 we show the same analysis for male labor market entrants. The pattern is different compared to that of female labor market entrants. Having more women among mid-range managers has a positive, albeit insignificant, effect on the probability of remaining employed

[86] Our dataset does not provide information on the introduction and the existence of female friendly firm policies. For this reason, we cannot directly investigate this channel in our analysis.

in the short, medium and long run. In year 10, increasing the share of female mid-managers by 10 percentage points increases employment of male labor market entrants by 0.2 percentage points, specifically as full-time employed, as the probability of working as a full-time worker increases more, by 0.3 percentage points**.** On the contrary, having more women among executives has no effect on employment overall, but a positive and persistent, albeit insignificant, effect on full-time employment probabilities. In terms of magnitude of the effects, in year 10, increasing the share of female executives by 10 percentage points increases employment of male labor market entrants by 0.07 percentage points and the probability of working full-time increases by 0.18 percentage points**.** None of these effects are, however, statistically significant.

Results thus differ when we look at the impact of gender composition of mid-range managers and executives on the probability of being employed in the future among female and male labor market entrants. For women, the positive effect shown in Table 4.2 seems to stem from the role of female executives, which have a statistically significant effect on the probability of remaining employed in the future. On the contrary, for men, the positive effect shown in Table 4.2 seems to stem from the role of female mid-range managers, which have a large, albeit not always statistically significant effect, on the probability of remaining employed in the future.

Concerning the channels discussed at the beginning of the section, the role model channel seems to be more prominent among women, while men benefit more from having mid-range managers of the other gender. This finding suggests that, in the Italian context, the role model impact of female executives (investigated for the choice of major by, among others, Porter and Serra, 2020) seems more important than the role of (potential) mentoring played by mid-range managers and immediate supervisors (theorized by Athey et al., 2000 and which is found in the army context as in Kofoed, 2019).

Robustness. To account for the potential sorting of men and women into different occupations in the same firm, we perform robustness check

including firm-occupation fixed effect. The results for the effect of the share of women mid-range managers and executives on employment and full-time employment probabilities are reported in Table 4.4. The table shows that our coefficients of interest barely change when including firm-occupation fixed effects: increasing the share of female executives has a positive and significant effect on the employment (and full-time employment) probability of female labor market entrants, while female mid-range managers have no effect. The estimates for men (Panel B) are mostly insignificant and point towards a positive effect on male employment of women mid-range managers.

4.5.2 Job to Job Mobility

Female management and inter-firm mobility. Young workers are more mobile and improve their match by changing jobs early in their career (Topel and Ward, 1992). To understand how gender composition of managers in the initial firm affects job-to-job mobility, we analyse its impact on firm changes in years 2, 5 and 10 post-entry. To understand movement to a better match, job-to-job mobility is further decomposed into upwards and downwards movement, namely movement to a job where the workers earn a higher (lower) wage. In Table 4.5, we investigate the employment effects of firm's gender composition of management on job-to-job mobility (columns 1 to 3), that is on transitions to other firms, movement to a better paid job (job-to-job good, in columns 4 to 6) and movement to a worse paid job (job-to-job bad, in column 7 to 9).

Panel A of Table 4.5 presents the analysis for female new labor market entrants. The presence of more female managers in a firm is accompanied by a very consistent pattern of job-to-job mobility among new female hires. In year 10, increasing the share of female managers by 10 percentage points increases job-to-job transitions of female labor market entrants by 0.3 percentage points. We further decompose the job-to-job mobility effect into movement to a better-paid job and movement to a worse-paid job. Almost all of the magnitude of the job-to-job mobility coefficient–98 percent–is explained by mobility into to a better-paid job. Additionally,

decomposing the effect into that of female mid-range managers and executives (Panel A Table 4.6) suggests that executives have a more positive and statistically significant effect than mid-managers. In year 10, increasing the share of female executives by 10 percentage points increases job-to-job transitions of female labor market entrants by 0.4 percentage points, while increasing the share of female mid-managers by 10 percentage points increases job-to-job transitions of female labor market entrants by 0.14 percentage points (without being statistically significant).

Panel B of Table 4.5 presents the analysis for male labor market entrants. For men, the presence of more female managers in a firm is accompanied by a positive increase in job-to-job mobility among new male hires, albeit not always statistically significant. The magnitude of the effects is very similar for men and women, although standard errors tend to be larger in the male sample, which makes these results less significant. Just as with women, the magnitude of the job-to-job mobility coefficient is explained by a movement to a better-paid job, with movement towards a worse paid job is even negatively affected (albeit not statistically significant). As for the employment probabilities the positive effect of female managers on better paying job-to-job transitions seems to stem from mid-range managers not executives (Panel B Table 4.6). In year 10, increasing the share of female mid-range managers by 10 percentage points increases job-to-job transitions of male labor market entrants by 0.2 percentage points, while increasing the share of female executives by 10 percentage points increases job-to-job transitions of male labor market entrants by 0.09 percentage points (it is to be noted that both coefficients are not statistically significant).

Our findings thus suggest that having more female managers in a firm, increases job-to-job transitions throughout the career for both men and women, specifically to better-paid jobs. However, while the effect mostly comes from the impact of female executives for women, for men it stems from mid-range managers

4.5.3 Maternity

We study the impact of firm gender composition on the maternity leave take-up at each stage of the career, as well as the probability of returning to work and returning to work as a full-time worker. Table 4.7 presents the results[87]. Having more female managers increases the probability of taking maternity leave in the medium run (column 2), but it does not seem to affect maternity leave take-up in the short (column 1) and long-run (column 3). However, it increases the probability of returning to work and returning to work as a fulltime worker. In year 10, increasing the share of female managers by 10 percentage points increases return to work from maternity leave by 0.21 percentage points (column 6) and return from maternity leave fulltime by 0.16 (column 9).

This is an important finding, given that the divergence in earnings between men and women widens after the birth of a child, with women earning, on average, 45 percent less than men after ten years since childbirth (Kleven, Landais, and Søgaard, 2019). The fact that having more female managers increases the probability of returning to work and returning to work as a full-time worker is an important result, especially for Italy, where 40 percent of mothers are out of the labor force.

4.6 Discussion and Conclusion

This study contributes to a large and growing literature on the underlying causes of gender inequality. In examining the impact of gender composition of management on employment, inter-firm mobility and fertility, we find evidence that a firm's employment of a higher share of female managers improves labor market outcomes of more junior female workers in several ways. It increases junior female employees' likelihood of being employed in the medium and long run; it increases job-to-job transition to a higher paying job; and it increases return to work after maternity leave. Our

[87] Italy only allows for two weeks of paternity leave and few fathers take it. Additionally, the data does not provide information on fathers and children. We thus cannot perform the same analysis of maternity leave and child penalty for male labor market entrants.

findings thus show that initial conditions, namely, the characteristics of the firm of the first hire, matter for future labor market outcomes of female labor market entrants. These findings align with recent studies that find that the size of the firm of the first hire impacts workers' future labor market outcomes (Müller et al., 2018; Arellano-Bover, 2022).

Additionally, we exploit the richness of the Italian administrative linked employer-employee data (INPS) and separately investigate the effect of female mid-range managers and of female executives. In our research, we go beyond recent studies that investigate the relationship between the gender of the firm's managers and gender-specific wages, that focus on the role of female executives only (as in Cardoso and Winter-Ebmer, 2010; and Flabbi et al., 2019). Our results suggest that female executives are the contributors to the increase in the probability of a) remaing in employment, b) job-to-job transition to a higher paying job, and c) returning to work after maternity leave. We find that mid-range managers seem to have no effect on the labor market outcomes of female labor market entrance. This finding suggests that, in the Italian context, the role model impact of female executives (investigated for the choice of major by**,** among others, Porter and Serra, 2020) seems more important than the role of (potential) mentoring played by mid-range managers and immediate supervisors (theorized by Athey et al., 2000 and which is found in the army context as in Kofoed, 2019). Finally, female managers have a positive effect on coming back from maternity leave. The fact that the presence of more female managers in the firm increases the probability of a return to work and of a return to work as a full-time worker are important results, especially in the case of Italy, where 40 percent of mothers are not in employment**.** All in all, our analysis suggests that having women in managerial positions could be an important contributor to women's labor market success.

In future research, we aim to investigate the heterogeneity effects of female managers, by studying whether the impact of female managers differ between high and low female share industries: having more women among

managers might have a stronger effect in industries where there are few women[88].

[88] Additionally, we intend to adopt an instrumental variable strategy to probe the robustness of our results. More exactly, we aim to exploit managers' death as an instrument for the change in the share of female managers in the firm, which has been shown to be viable in similar contexts (Acemoglu, He and Le Maire, 2022, for Denmark; and Schivardi and Sauvagnant, 2020, for Italy).

Figures and Tables

Table 4.1: Summary Statistics

	Women (1)	Men (2)
Age	23.34	22.20
	(4.53)	(4.54)
log weekly wage	5.58	5.62
	(0.54)	(0.49)
Share female managers	0.057	0.048
	(0.13)	(0.11)
Share female executives	0.028	0.025
	(0.09)	(0.08)
Firm employment at the baseline	1350.27	1665.05
	(6264)	(7183.17)
Firm female employment at the baseline	503.22	549.41
	(2209.55)	(2378.56)
Employment growth	358.23	387.23
	(3660.95)	(3856.30)
Wage growth	-0.04	-0.03
	(0.23)	(0.20)
Average firm employment	2868.74	3319.84
	(10035.05)	(10906.99)
Observations	838,252	1,146,704

Notes: The table compares the characteristics of female (column 1) and male (column 2) labor market entrants (our baseline sample). Labor market entrants are defined as workers that obtained their first full-time employment between 2001 and 2009. Share of female managers measures the percentage of women employed in managerial jobs over the total number of workers employed in such positions. Share of female executives is defined similarly. “Baseline” is the year 2000, if a firm was born after the year 2000, then the year of that firm's birth will be considered as baseline. Employment and wage growth measure changes in employment and wages over the previous year.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.2. Employment Status

Panel A -- Women

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Managers	-0.00983	0.00130	0.0216**	0.00443	0.0110	0.0197**
	(0.0133)	(0.0105)	(0.0108)	(0.0146)	(0.0110)	(0.00850)
Observations	838,252	838,252	838,252	838,252	838,252	838,252

Panel B - Men

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Managers	0.0250	0.0145	0.0304*	0.0305	0.0188	0.0419**
	(0.0216)	(0.0164)	(0.0177)	(0.0212)	(0.0172)	(0.0186)
Observations	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female managers on employment (columns 1-3) and full-time employment status (columns 4-6) in years two (columns 1 and 4), five (columns 2 and 5), and ten (columns 3 and 6) after labor market entry of female (Panel A) and male workers (Panel B). The coefficients are estimated following equation (6), including a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), firm and year fixed-effects. The table suggests that increasing the share of female managers has a positive (albeit insignificant, apart from year 10) effect on employment and full-time employment status of both women and men. Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.3. Employment Status. Mid-range and Executives

Panel A -- Women

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Mid-range Managers	-0.0121	-0.00438	0.00785	-0.00508	0.00133	0.00343
	(0.0124)	(0.0105)	(0.0102)	(0.0141)	(0.0113)	(0.00854)
Share of Female Executives	0.0198	0.0211**	0.0385***	0.0317**	0.0260**	0.0334***
	(0.0141)	(0.0103)	(0.0103)	(0.0135)	(0.0106)	(0.00993)
Observations	838,252	838,252	838,252	838,252	838,252	838,252

Panel B -- Men

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Mid-range Managers	0.0222	0.0129	0.0243	0.0255	0.0134	0.0319*
	(0.0164)	(0.0156)	(0.0170)	(0.0180)	(0.0164)	(0.0173)
Share of Female Executives	0.0144	0.00729	0.00702	0.0161	0.0196	0.0182
	(0.0201)	(0.0128)	(0.0111)	(0.0190)	(0.0135)	(0.0119)
Observations	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female mid-range managers and the share of female executives on employment (columns 1-3) and full-time employment status (columns 4-6) in years two (columns 1 and 4), five (columns 2 and 5), and ten (columns 3 and 6) after labor market entry of female (Panel A) and male workers (Panel B). The coefficients are estimated following equation (6), including a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), firm and year fixed-effects. The table suggests that the positive (but noisy) effect of female managers on employment probabilities (found in table 4.2) seems to stem from the positive effect of female executives for women (Panel A) and the positive (but insignificant) effect of female mid-range managers for men (Panel B). Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.4 Robustness

Panel A -- Women

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Mid-range Managers	-0.0122	-0.00291	0.00878	-0.00500	0.00230	0.00541
	(0.0126)	(0.0108)	(0.0106)	(0.0140)	(0.0116)	(0.00862)
Share of Female Executives	0.0165	0.0198*	0.0373***	0.0309**	0.0260**	0.0301***
	(0.0143)	(0.0105)	(0.0106)	(0.0140)	(0.0108)	(0.00990)
Observations	793,702	793,702	793,702	793,702	793,702	793,702

Panel B -- Men

	(1) Empl in year 2	(2) Empl in year 5	(3) Empl in year 10	(4) FT in year 2	(5) FT in year 5	(6) FT in year 10
Share of Female Mid-range Managers	0.0202	0.0110	0.0235	0.0233	0.0119	0.0310*
	(0.0175)	(0.0166)	(0.0180)	(0.0193)	(0.0174)	(0.0184)
Share of Female Executives	0.0166	0.00878	0.00772	0.0194	0.0221	0.0197
	(0.0197)	(0.0131)	(0.0116)	(0.0185)	(0.0141)	(0.0124)
Observations	1,072,147	1,072,147	1,072,147	1,072,147	1,072,147	1,072,147

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female mid-range managers and the share of female executives on employment (columns 1-3) and full-time employment status (columns 4-6) in years two (columns 1 and 4), five (columns 2 and 5), and ten (columns 3 and 6) after labor market entry of female (Panel A) and male workers (Panel B). The coefficients are estimated following equation (6), including baseline controls (a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), year fixed-effects) and firm-occupation fixed-effects (as opposed to firm fixed-effects only). The table shows that our baseline results (reported in Table 4.3) are robust to the inclusion of firm-occupation fixed effects. The number of observations is smaller than that reported in table 4.3, as some (small) firms only hire workers in one (broad) occupation. Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.5. Job-to-job mobility

Panel A -- Women

	(1) job-to-job year 2	(2) job-to-job year 5	(3) job-to-job year 10	(4) job-to-job good year 2	(5) job-to-job good year 5	(6) job-to-job good year 10	(7) job-to-job bad year 2	(8) job-to-job bad year 5	(9) job-to-job bad year 10
Share of Female Managers	0.00365	0.0232**	0.0294***	0.0194	0.0161**	0.0118*	-0.0103	0.00563	0.0109
	(0.0149)	(0.0112)	(0.0103)	(0.0151)	(0.00801)	(0.00691)	(0.0183)	(0.00739)	(0.00734)
Observations	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252

Panel B - Men

	(1) job-to-job year 2	(2) job-to-job year 5	(3) job-to-job year 10	(4) job-to-job good year 2	(5) job-to-job good year 5	(6) job-to-job good year 10	(7) job-to-job bad year 2	(8) job-to-job bad year 5	(9) job-to-job bad year 10
Share of Female Managers	0.0125	0.0250	0.0294*	0.0318**	0.0185	0.0423***	-0.0165	0.00854	-0.0151
	(0.0266)	(0.0168)	(0.0172)	(0.0155)	(0.0120)	(0.0112)	(0.0244)	(0.0111)	(0.00929)
Observations	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female managers on the probability of 1) moving to another firm ("job-to job", columns 1-3), 2) moving to a better-paid job ("job-to-job good", columns 4-6), 3) moving to a worse-paid job ("job-to-job bad", columns 7-9) in years two (columns 1, 4 and 7), five (columns 2, 5 and 8), and ten (columns 3 and 6) after labor market entry of female (Panel A) and male workers (Panel B). The coefficients are estimated following equation (6), including a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), firm and year fixed-effects. The table suggests that increasing the share of female managers has a positive effect on job-to-job transitions, particularly towards better-paid jobs, for both women and men. Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.6. Job-to-job Mobility. Mid-range and Executives

Panel A -- Women

	(1) job-to-job year 2	(2) job-to-job year 5	(3) job-to-job year 10	(4) job-to-job good year 2	(5) job-to-job good year 5	(6) job-to-job good year 10	(7) job-to-job bad year 2	(8) job-to-job bad year 5	(9) job-to-job bad year 10
Share of Female Mid-range Managers	0.0312**	0.0141	0.0145	0.0366	0.0133	0.000971	5.54e-05	0.000354	0.00698
	(0.0152)	(0.0103)	(0.00920)	(0.0224)	(0.00809)	(0.00662)	(0.0130)	(0.00686)	(0.00629)
Share of Female Executives	-0.0435	0.0344**	0.0412***	-0.0158	0.0213**	0.0253***	-0.0256	0.0143**	0.00644
	(0.0276)	(0.0135)	(0.0107)	(0.0127)	(0.00935)	(0.00921)	(0.0193)	(0.00687)	(0.00988)
Observations	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252

Panel B -- Men

	(1) job-to-job year 2	(2) job-to-job year 5	(3) job-to-job year 10	(4) job-to-job good year 2	(5) job-to-job good year 5	(6) job-to-job good year 10	(7) job-to-job bad year 2	(8) job-to-job bad year 5	(9) job-to-job bad year 10
Share of Female Mid-range Managers	0.0454**	0.0221	0.0238	0.0459**	0.0193*	0.0333***	-0.000623	0.00207	-0.0146*
	(0.0197)	(0.0162)	(0.0168)	(0.0220)	(0.0115)	(0.0115)	(0.0165)	(0.00987)	(0.00802)
Share of Female Executives	-0.0789*	0.0133	0.00385	-0.0183	0.00895	0.0134	-0.0514*	0.0136	-0.00158
	(0.0415)	(0.0202)	(0.0135)	(0.0156)	(0.0157)	(0.0133)	(0.0273)	(0.00932)	(0.00942)
Observations	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704	1,146,704

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female mid-range managers and female executives on the probability of 1) moving to another firm ("job-to job", columns 1-3), 2) moving to a better-paid job ("job-to-job good", columns 4-6), 3) moving to a worse-paid job ("job-to-job bad", columns 7-9) in years two (columns 1, 4 and 7), five (columns 2, 5 and 8), and ten (columns 3 and 6) after labor market entry of female (Panel A) and male workers (Panel B). The coefficients are estimated following equation (6), including a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), firm and year fixed-effects. The table suggests that the positive effect of female managers on job-to-job transitions (found in table 4.5) seems to stem from the positive effect of female executives for women (Panel A) and the positive effect of female mid-range managers for men (Panel B). Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Table 4.7. Maternity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	maternity leave year 2	maternity leave year 5	maternity leave year 10	empl after maternity year 2	empl after maternity year 5	empl after maternity year 10	FT after maternity year 2	FT after maternity year 5	FT after maternity year 10
Share of Female Managers	-0.00257	0.0118*	0.00774	0.000652	0.00870**	0.0210***	0.000894	0.00841**	0.0157***
	(0.00357)	(0.00610)	(0.00868)	(0.00170)	(0.00410)	(0.00720)	(0.00148)	(0.00331)	(0.00534)
Observations	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252	838,252

Notes: The table reports the estimated coefficients (and corresponding standard errors, in parenthesis) of the effect of a one percentage point increase in the share of female managers on the probability of 1) taking maternity leave (columns 1-3), 2) returning to work after maternity leave (columns 4-6), 3) returning to full-time work after maternity leave (columns 7-9) in years two (columns 1, 4 and 7), five (columns 2, 5 and 8), and ten (columns 3 and 6) after labor market entry of female workers. The coefficients are estimated following equation (6), including a set of controls defined in the year of labor market entry: the worker's age and occupational level, firm's varying characteristics (lagged average wage and employment, wage and employment growth over the year before labor market entry), firm and year fixed-effects. The table suggests that increasing the share of female managers has a positive effect on the probability of returning to work after maternity leave. Standard errors clustered at the firm level. * statistically significant at the 0.05 level, ** at the 0.01 level, *** at the 0.001 level.

Source: Italian Social Security Records from the Italian Social Security Institute (INPS)

Bibliography

Abowd, J. M., Kramarz, F., & Margolis, D. N. (1999). High wage workers and high wage firms. *Econometrica*, *67*(2), 251-333.

Acemoglu, D. (2002). Directed technical change. *The Review of Economic Studies*, 69(4), 781-809.

Acemoglu, D., He, A., & le Maire, D. (2022). Eclipse of Rent-Sharing: The Effects of Managers' Business Education on Wages and the Labor Share in the US and Denmark (No. w29874). *National Bureau of Economic Research*.

Acemoglu, D., Manera, A., and Restrepo, P. (2020). Does the US tax code favor automation? (No. w27052). *National Bureau of Economic Research.*

Acemoglu, D., and Restrepo, P. (2020). Robots and jobs: Evidence from US labor markets*. Journal of Political Economy*, 128(6), 2188-2244.

Acemoglu, D., and Restrepo, P. (2022). Tasks, automation, and the rise in US wage inequality. *Econometrica*, forthcoming

Adda, J., Dustmann, C., and Stevens, K. (2017). The career costs of children. *Journal of Political Economy*, 125(2), 293-337.

Adda, J., and Dustmann C. (2022). Sources of wage growth. *Journal of Political Economy*, accepted

Alesina, A., Giuliano, P., and Nunn, N. (2013). On the origins of gender roles: Women and the plough. *The Quarterly Journal of Economics*, *128*(2), 469-530.

Andrew, A., Bandiera, O., Costa-Dias, M., and Landais, C. (2021). Women and men at work. *IFS Deaton Review of Inequalities*.

Arai, M. (2003). Wages, profits, and capital intensity: Evidence from matched worker-firm data. *Journal of Labor Economics*, 21(3), 593-618.

Arellano-Bover, J. (2022). Career consequences of firm heterogeneity for young workers: First job and firm size. *Journal of Labor Economics*, forthcoming

Arulampalam, W., Devereux, M.P. and Maffini, G. (2012). The Direct Incidence of Corporate Income Tax on Wages. *European Economic Review*. 56 (6), 1038–54.

Athey, S., Avery, C. and Zemsky, P. (2000). “Mentoring and diversity.” *American Economic Review* 90(4), 765-786.

Azémar, C. and Hubbard, R.G. (2015). Country Characteristics and the Incidence of Capital Income Taxation on Wages: An Empirical Assessment. *Canadian Journal of Economics*, 48 (5), 1762–1802.

Bagues, M., Sylos-Labini, M., and Zinovyeva, N. (2017). Does the gender composition of scientific committees matter?. *American Economic Review*, 107(4), 1207-38.

Baker, M., and Milligan, K. (2008). How does job-protected maternity leave affect mothers’ employment?. *Journal of Labor Economics*, 26(4), 655-691.

Baker, A. C., Larcker, D. F., and Wang, C. C. (2022). How much should we trust staggered difference-in-differences estimates?. *Journal of Financial Economics*, *144*(2), 370-395.

Bardhi, A., Guo, Y., & Strulovici, B. (2020). Early-Career Discrimination: Spiraling or Self-Correcting?. *Working Paper, Duke and Northwestern*.

Becker, S.O., Egger, P.H., and Merlo, V. (2012). *How low business tax rates attract MNE activity: Municipality-level evidence from Germany.* Journal of Public Economics, 96(9-10), 698-711.

Benzarti, Y., and Harju, J. (2021a). Using payroll tax variation to unpack the black box of firm-level production. *Journal of the European Economic Association*, *19*(5), 2737-2764.

Benzarti, Y., and Harju, J. (2021b). Can payroll tax cuts help firms during recessions?. *Journal of Public Economics*, *200*, 104472.

Bettendorf, L. J. H., Jongen E. L. W. and Muller P. (2015). “Childcare Subsidies and Labour Supply—Evidence from a Dutch Reform.” *Labour Economics* 36: 112–23.

Bertrand, M. (2018). Coase lecture–the glass ceiling. *Economica*, *85*(338), 205-231.

Bertrand, M., Black, S. E., Jensen, S., and Lleras-Muney, A. (2019). Breaking the glass ceiling? The effect of board quotas on female labour market outcomes in Norway. *The Review of Economic Studies*, 86(1), 191-239.

Bertrand, M., Goldin, C., & Katz, L. F. (2010). Dynamics of the gender gap for young professionals in the financial and corporate sectors. *American economic journal: applied economics*, *2*(3), 228-55.

Bhaskar, V. and To, T. (1999). Minimum wages for Ronald McDonald monopsonies: A theory of monopsonistic competition. *The Economic Journal*, 109(455), 190–203.

Bhaskar, V., Manning, A., and To, T. (2002). Oligopsony and monopsonistic competition in labor markets. *Journal of Economic Perspectives*, 16(2), 155–174.

Biasi, B., and Sarsons, H. (2022). Flexible wages, bargaining, and the gender gap. *The Quarterly Journal of Economics*, 137(1), 215-266.

Bishop, J. H. (1981), Employment in Construction and Distribution Industries: The impact of the New Jobs Tax Credit", in S. Rosen, (ed.), *Studies in Labour Markets* (Chicago: University of Chicago Press) 209–246.

Blau, F. D., and Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. *Journal of Economic Literature*, 55(3), 789-865.

Blundell, R., Costa Dias, M., Meghir, C., and Shaw, J. (2016). Female labor supply, human capital, and welfare reform. *Econometrica*, *84*(5), 1705-1753.

Boelmann, B, Raute, A. and Schönberg, U. (2021), "DP16149 Wind of Change? Cultural Determinants of Maternal Labor Supply", *CEPR Press Discussion Paper* No. 16149.

Burdett, K. (1978). A theory of employee job search and quit rates. *The American Economic Review*, 68(1), 212-220.

Cahuc, P., Carcillo, S., and Le Barbanchon, T. (2019). The effectiveness of hiring credits. *The Review of Economic Studies*, 86(2), 593-626.

Card, D., Cardoso, A. R., Heining, J., and Kline, P. (2018). Firms and labor market inequality: Evidence and some theory. *Journal of Labor Economics*, 36(S1), S13-S70.

Cardoso, A. R., & Winter-Ebmer, R. (2010). Female-led firms and gender wage policies. *ILR Review*, 64(1), 143-163.

Casarico, A., and Lattanzio, S. (2019). What firms do: Gender inequality in linked employer-employee data. *Cambridge-INET Working Paper Series* No: 2019/15

Casarico, A., and Lattanzio, S. (2021). Behind the child penalty: Understanding what contributes to the labour market costs of motherhood. *CESifo Working Paper No. 9155*

Cascio, E. and Schanzebach W. M. (2013). "The Impacts of Expanding Access to High-Quality Preschool Education*." Brookings Papers on Economic Activity*, no. 2 (Fall): 127–92.

Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, *134*(3), 1405-1454.

Clausing, K.A. (2013). Who Pays the Corporate Tax in a Global Economy? *National Tax Journal*, 66(1), 151–84.

Comi, S., Grasseni, M., Origo, F., & Pagani, L. (2020). Where women make a difference: gender quotas and firms' performance in three European countries. *ILR Review*, *73*(3), 768-793.

Cortés, P., & Pan, J. (2020). Children and the remaining gender gaps in the labor market *Journal of Economic Literature* forthcoming

Coughlin, C. C., Terza, J. V., and Arromdee, V. (1991). State characteristics and the location of foreign direct investment within the United States. *The Review of Economics and Statistics*, 675-683.

Criscuolo, C., Martin, R., Overman, H.G., and Van Reenen, J. (2019). Some causal effects of an industrial policy. *American Economic Review*, 109(1), 48–85.

Curtis, M.E., Garrett, D.G., Ohrn, G., Roberts, K.A. and Suárez Serrato, J.C. (2022). Capital Investment and Labor Demand. *NBER Working Paper* No. 29485.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer effects in program participation. *American Economic Review*, 104(7), 2049-74.

Destatis (2022a). “Finanzen und Steuern. Steuerhaushalt.” Fachserie 14 Reihe 4, Statistisches Bundesamt (Destatis).

Destatis (2022b). “Finanzen und Steuern. Lohn- und Einkommensteuer.” Fachserie 14 Reihe 7.1, Statistisches Bundesamt (Destatis).

Devereux, M. P., and R. Griffith (1998). Taxes and the Location of Production: Evidence from a Panel of US Multinationals. *Journal of Public Economics*, 68 (3), 335–67.

Dixit, A. K., and Stiglitz, J. E. (1977). Monopolistic competition and optimum product diversity. *American Economic Review*, 67(3), 297-308.

Doepke, M., & Tertilt, M. (2009). Women's Liberation: What's in it for Men?. *The Quarterly Journal of Economics*, *124*(4), 1541-1591.

Duranton, G., Gobillon, L., and Overman, H. G. (2011). Assessing the effects of local taxation using microgeographic data. *The economic journal*, *121*(555), 1017-1046.

Dustmann, C., Lindner, A., Schönberg, U., Umkehrer, M. and P. Vom Berge (2022). Reallocation Effects of the Minimum Wage. *Quarterly Journal of Economics*, 137(1), 267-328.

Dustmann, C., Ludsteck, J., and Schönberg, U. (2009). Revisiting the German wage structure. *The Quarterly journal of economics*, *124*(2), 843-881.

Dustmann, C., Schönberg, U., and Stuhler, J. (2016). The impact of immigration: Why do studies reach such different results?. *Journal of Economic Perspectives*, *30*(4), 31-56.

The European Institute for Gender Equality (EIGE). (2019). Economic Benefits of Gender Equality in the European Union: Overall economic impacts of gender equality https://eige.europa.eu/gender-mainstreaming/policy-areas/economic-and-financial-affairs/economic-benefits-gender-equality

European Investment Bank (2019). EIB Group survey on investment and investment finance 2019 - Country overview Germany. https://www.eib.org/attachments/efs/eibis_2019_germany_en.pdf.

Eurostat Database (2018), Employment by sex, age and professional status

Eurostat Database. (2020). Gender pay gap in unadjusted form by NACE Rev. 2 activity

Eurostat Database. (2021a). Employment rate by sex, age groups, educational attainment level and household composition (%), female and male employment Italy ages 25-54

Eurostat Database. (2021b). Employment rate of adults by sex, age groups, educational attainment level, number of children, and age of youngest child (%).

Felix, R. A. (2007). Passing the burden: Corporate tax incidence in open economies. *LIS Working Paper Series* No. 468.

Fernandez, R. (2007). Women, work, and culture. *Journal of the European Economic Association*, *5*(2-3), 305-332.

Financial Times (2022). “Tory leadership hopefuls set out stall on tax cuts.” Financial Times, 10th July 2022. https://www.ft.com/content/11e6a49b-ec21-405d-8bf9-8644c4042d4e

Fitzenberger, B., Osikominu, A., and Völter, R. (2006). Imputation Rules to Improve the Education Variable in the IAB Employment Subsample. in *Schmollers Jahrbuch: J. Appl. Soc. Sci. Stud. / Zeitschrift für Wirtschafts- und Sozialwissenschaften*, 126(3), pp. 405-436

Fitzenberger, B., and Seidlitz, A. (2020). The 2011 break in the part-time indicator and the evolution of wage inequality in Germany. *Journal for Labour Market Research*, 54(1), 1-14.

Flabbi, L., Macis, M., Moro, A., and Schivardi, F. (2019). Do female executives make a difference? The impact of female leadership on gender gaps and firm performance. *The Economic Journal*, *129*(622), 2390-2423.

Fuest, C., Peichl, A., and Siegloch, S. (2018). Do higher corporate taxes reduce wages? Micro evidence from Germany. *American Economic Review*, 108(2), 393-418.

Gabe, T. M. and Bell, K. P. (2004). Tradeoffs between local taxes and government spending as determinants of business location. *Journal of Regional Science*, 44(1), 21-41.

Gagliarducci, S. and Paserman, M.D. (2015). ‘Gender interactions in firm hierarchies: evidence from linked employer- employee data’, *Research in Labor Economics*, vol. 41, pp. 343–75.

Garrett, D. G., Ohrn, E., and Suárez Serrato, J. C. (2020). Tax policy and local labor market behavior. *American Economic Review: Insights*, 2(1), 83-100.

Gechert, S., Havranek, T., Irsova, D. And Kolcunova, D. (2022). Measuring capital-labor substitution: The importance of method choices and publication bias. *Review of Economic Dynamics*, 45, 55-82.

Giroud, X. and Rauh, J. D. (2019). State Taxation and the Reallocation of Business Activity: Evidence from Establishment Level Data. *Journal Political Economy* 127(3), 1262-1316.

Givord, P., and Marbot, C. (2015). Does the cost of child care affect female labor market participation? An evaluation of a French reform of childcare subsidies. *Labour Economics*, *36*, 99-111.

Goldin, C. (2006). The quiet revolution that transformed women's employment, education, and family. *American economic review*, *96*(2), 1-21.

Goldin, C. (2014). A grand gender convergence: Its last chapter. *American Economic Review*, 104(4), 1091-1119.

Goldin, C., Kerr, S. P., Olivetti, C., and Barth, E. (2017). The expanding gender earnings gap: Evidence from the LEHD-2000 Census. *American Economic Review*, *107*(5), 110-14.

Haeck, C., Lefebvre, P., and Merrigan, P. (2015). Canadian evidence on ten years of universal preschool policies: The good and the bad. *Labour Economics*, *36*, 137-157.

Hamersma S (2003) The work opportunity and welfare-to-work tax credits: Participation rates among eligible workers. *National Tax Journal* 56:725–738

Havnes, T., & Mogstad, M. (2011). Money for nothing? Universal child care and maternal employment. *Journal of Public Economics*, *95*(11-12), 1455-1465.

Hazell, J., Patterson, C., Sarsons, H., and Taska, B. (2021). National wage setting. *Working Paper.*

Hines, J. R., Jr. (1996). Altered States: Taxes and the Location of Foreign Direct Investment in America. *American Economic Review*, 86(5), 1076–94.

Ichino, A., and Moretti, E. (2009). Biological gender differences, absenteeism, and the earnings gap*. American economic journal: applied economics,* 1(1), 183-218.

Jovanovic, B. (1979). Job matching and the theory of turnover. *Journal of political economy*, 87(5, Part 1), 972-990.

Katz, L. F. (1998). Wage subsidies for the disadvantaged. In Richard B. Freeman and Peter Gottschalk (Eds.), *Generating Jobs: How to Increase Demand for Less-Skilled Workers*, pp. 21–53. New York: Russell Sage Foundation

Kleven, H., Landais, C., Posch, J., Steinhauer, A., and Zweimuller, J. (2019). Child penalties across countries: Evidence and explanations. *In AEA Papers and Proceedings*, 109, pp. 122-26.

Kleven, H., Landais, C., Posch, J., Steinhauer, A., and Zweimüller, J. (2020). Do family policies reduce gender inequality? Evidence from 60 years of policy experimentation (No. w28082). *National Bureau of Economic Research.*

Kleven, H., Landais, C., and Søgaard, J. E. (2019). Children and gender inequality: Evidence from Denmark. *American Economic Journal: Applied Economics*, 11(4), 181-209.

Kofoed, M. S. (2019). The effect of same-gender or same-race role models on occupation choice evidence from randomly assigned mentors at west point. *Journal of Human Resources*, 54(2), 430-467.

Kotlikoff, L. J., and Summers, L. H. (1987). Tax incidence. In *Handbook of public economics* (Vol. 2, pp. 1043-1092). Elsevier.

Ku, H., Schönberg, U., and Schreiner, R. C. (2020). Do place-based tax incentives create jobs?. *Journal of Public Economics*, *191*, 104105.

Kunze, A., and Miller, A. R. (2017). Women helping women? Evidence from private sector data on workplace hierarchies. *Review of Economics and Statistics*, *99*(5), 769-775.

Lalive, R., Schlosser, A., Steinhauer, A., and Zweimüller, J. (2014). Parental leave and mothers' careers: The relative importance of job protection and cash benefits. *Review of Economic Studies*, 81(1), 219-265.

Lalive, R., and Zweimüller, J. (2009). How does parental leave affect fertility and return to work? Evidence from two natural experiments. *The Quarterly Journal of Economics*, 124(3), 1363-1402.

Lazear, E. P., and Spletzer, J. R. (2012). Hiring, churn, and the business cycle. *American Economic Review*, 102(3), 575-79.

Lefebvre, P., Merrigan, P., and Verstraete, M. (2009). Dynamic labour supply effects of childcare subsidies: Evidence from a Canadian natural experiment on low-fee universal child care. *Labour Economics*, *16*(5), 490-502.

Lepage, L. P. (2021). Endogenous learning, persistent employer biases, and discrimination. Unpublished manuscript

Lerche, A. (2019). Investment tax credits and the response of firms. *Unpublished Manuscript*

Leung, M. D. (2018). Learning to hire? Hiring as a dynamic experiential learning process in an online market for contract labor. *Management Science*, *64*(12), 5651-5668.

Lichter, A., Löffler, M., Isphording, I. E., Nguyen, T. V., Pöge, F., and Siegloch, S. (2021). Profit taxation, R&D spending, and innovation. *ZEW-Centre for European Economic Research Discussion Paper*, (21-080).

Lucifora, C., and Vigani, D. (2021). What if your boss is a woman? Work organization, work-life balance and gender discrimination at the workplace. *Review of the Economics of the Households*

Maida, A., and Weber, A. (2022). Female leadership and gender gap within firms: Evidence from an Italian board reform. *ILR Review*, 75(2), 488-515.

Manning, A. (2003). The real thin theory: monopsony in modern labour markets. *Labour economics*, 10(2), 105-131.

Matsa, D. A. and Miller, A. R. (2013). A female style in corporate leadership? Evidence from quotas. *American Economic Journal: Applied Economics*, *5*(3), 136-69.

Müller, S. and R. Neubaeumer (2018). Size of training firms: The role of firms, luck, and ability in young workers' careers. *International Journal of Manpower* 39(5), 658–673

Neumark, D. (2013). Spurring job creation in response to severe recessions: Reconsidering hiring credits. *Journal of Policy Analysis & Management* 32(1): 142–71.

Neumark, D. (2016). Policy levers to increase jobs and increase income from work after the Great Recession. *IZA Journal of Labor Policy,* 5(1), 1-38.

Neumark, D., and Grijalva, D. (2017). The employment effects of state hiring credits. *ILR Review*, 70(5), 1111-1145.

New York Times (2021). "Biden Finds Raising Corporate Taxes to Be Easier Abroad." https://www.nytimes.com/2021/10/30/world/europe/g20-biden-corporate-tax-agreement.html

Nollenberger, N., & Rodríguez-Planas, N. (2015). Full-time universal childcare in a context of low maternal employment: Quasi-experimental evidence from Spain. *Labour Economics*, *36*, 124-136.

O'Neill, D. M. (1982). Employment tax credit programs: The effects of socioeconomic targeting provisions. *The Journal of Human Resources*, *17*(3), 449-459.

OECD Tax Database, Table II.1 – Statutory corporate income tax rate. https://stats.oecd.org/index.aspx?DataSetCode=TABLE_II1

Oi, W. Y., and Idson, T. L. (1999). Firm size and wages. *Handbook of Labor Economics*, 3, 2165-2214.

Olivetti, C., and Petrongolo, B. (2008). Unequal pay or unequal employment? A cross-country analysis of gender gaps. *Journal of Labor Economics*, *26*(4), 621-654.

Olivetti, C., and Petrongolo, B. (2016). The evolution of gender gaps in industrialized countries. *Annual review of Economics*, *8*, 405-434.

Olivetti, C., and Petrongolo, B. (2017). The economic consequences of family policies: lessons from a century of legislation in high-income countries. *Journal of Economic Perspectives*, *31*(1), 205-30.

Oyer, P. (2006). Initial labor market conditions and long-term outcomes for economists. *The Journal of Economic Perspectives 20*(3), 143–160.

Oyer, P. (2008). The making of an investment banker: Stock market shocks, career choice, and lifetime income. *The Journal of Finance 63*(6), 2601–2628.

Panorama (2018) https://www.panorama.it/news/le-pensioni-le-minacce-e-le-mie-ragioni-intervista-elsa-fornero

Perloff, J. M., and Wachter, M. L. (1979). The New Jobs Tax Credit: An evaluation of the 1977-78 wage subsidy program. *The American Economic Review*, *69*(2), 173-179.

Porter, C., & Serra, D. (2020). Gender differences in the choice of major: The importance of female role models. *American Economic Journal: Applied Economics*, 12(3), 226-54.

Rathelot, R., and Sillard, P. (2008). The importance of local corporate taxes in business location decisions: Evidence from French micro data. *The Economic Journal*, *118*(527), 499-514.

Rubolino, E. (2022). Taxing the Gender Gap: Labor Market Effects of a Payroll Tax Cut for Women in Italy*. CESifo Working Paper* No. 9671

Ruhm, C. J. (1998). The economic consequences of parental leave mandates: Lessons from Europe. *The Quarterly Journal of Economics*, *113*(1), 285-317.

Saez, E., Matsaganis, M., and Tsakloglou, P. (2012). Earnings determination and taxes: Evidence from a cohort-based payroll tax reform in Greece. *The Quarterly Journal of Economics*, *127*(1), 493-533.

Saez, E., Schoefer, B., and Seim, D. (2019). Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden. *American Economic Review*, *109*(5), 1717-63.

Saez, E., Schoefer, B., and Seim, D. (2021). Hysteresis from employer subsidies. *Journal of Public Economics*, *200*, 104459.

Schivardi, F., & Sauvagnat, J. (2020). Are Executives in Short Supply? Evidence from Deaths' Events. Unpublished manuscript

Schoar, A. and L. Zuo (2017). Shaped by booms and busts: How the economy impacts CEO careers and management styles. *The Review of Financial Studies* 30(5), 1425–1456.

Schönberg, U., and Ludsteck, J. (2014). Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth. *Journal of Labor Economics*, 32(3), 469-505.

Siegloch, S., Wehrhöfer, N. and Etzel, T. (2021). Direct, Spillover and Welfare Effects of Regional Firm Subsidies. *CEPR Press Discussion Paper* No. 16129.

Smith, N. (2018). Gender quotas on boards of directors. *IZA World of Labor*.

Statistische Bundesamt (2011). Zum Zugang kleiner und mittlerer Unternehmen zu Finanzmitteln. https://www.destatis.de/DE/Themen/Branchen-Unternehmen/Unternehmen/Kleine-Unternehmen-Mittlere-Unternehmen/Publikationen/Downloads-Kleine-und-mittlere-Unternehmen/kmu-finanzmittel-5482101109004.pdf?__blob=publicationFile

Stigler, G. J. (1962). Information in the labor market. *Journal of Political Economy*, 70(5, Part 2), 94-105.

Stiglitz, J. E., and Rosengard, J. K. (2015). Economics of the public sector: Fourth edition. *WW Norton & Company*.

Suárez Serrato, J. C., and Zidar, O. (2016). Who benefits from state corporate tax cuts? A local labor markets approach with heterogeneous firms. *American Economic Review,* 106(9), 2582-2624.

Sun, L., and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2), 175-199.

Topel, R. H., & Ward, M. P. (1992). Job mobility and the careers of young men. The *Quarterly Journal of Economics*, 107(2), 439-479

Tuzel, S. and Zhang, M.B. (2021). Economic Stimulus at the Expense of Routine-Task Jobs. *The Journal of Finance*, 76(6), 3347–3399.

U.S. Government (2022). “Budget of the U.S. Government—FISCAL YEAR 2023.” *U.S. Government Publishing Office*, Washington, D.C. https://www.whitehouse.gov/wp-content/uploads/2022/03/budget_fy2023.pdf

Von Wachter, T. and S. Bender (2006). In the right place at the wrong time: The role of firms and luck in young workers’ careers. *American Economic Review 96*(5), 1679–1705