THREE ESSAYS IN LABOR ECONOMICS

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This thesis comprises three chapters of independent research on topics of labor economics. In Chapter 1, I show that empirical findings point to a robust negative relationship between local employment concentration and wages. Despite the scarcity of merger challenges on the grounds of decreased labor market competition, economic theory, case law, and amassed evidence justify the incorporation of labor markets in the scope of antitrust's scrutiny. This chapter provides a comprehensive review of the existing literature regarding the impact of mergers on employees and its relevance to antitrust policy. Additionally, it presents a summary of recommendations to antitrust agencies, drawing from the accumulated practice in the enforcement of product market competition.

In Chapter 2, I use matched employer-employee records merged with corporate tax information from 2003 to 2017 to estimate labor market-wide effects of mergers and acquisitions in Brazil. Pairs of commuting zone and industry sector define labor markets. In the following year of a merger, the market size falls by 10.8%. The employment adjustment is concentrated in merging firms. For the firms not involved in M&As, I estimate a 1.07% decline in workers' earnings and a positive, although not significant, increase in their size. Most mergers have a predicted impact of zero points in concentration, measured by the Herfindahl–Hirschman Index (HHI). In spillover firms, earnings decline similarly

for mergers with high and low predicted changes in HHI. Contrary to the recent literature on market concentration in developed economies, I find no evidence of oligopsonistic behavior in Brazilian labor markets.

Lastly, Chapter 3 estimates the public wage premium relative to the private sector in the Brazilian economy. I use a rich linked employer-employee administrative dataset to compute the premium over average wages and along the earnings distribution. Exploratory analysis suggests that the average worker in the two sectors varies with respect to observable attributes. The Oaxaca-Blinder decomposition shows that, beyond differences in characteristics, public sector workers from most government branches and levels enjoy a positive earnings premium related to the government's specific wage structure. Introducing worker fixed effects removes most of the premium variation along the earnings distribution; the total gap, as well as its characteristics and coefficients components, "flatten out" when endogenous selection is considered. For a policymaker to close the portion of the gap between public and private wages stemming from the public sector's specific wage structure, a horizontal public wage freeze can be used.

Biographical Sketch

Raised by politically engaged and intellectually curious parents, João Vítor Rego Costa was naturally drawn to the study of economics. He earned his B.A. and M.A. in Economics from the University of Brasília and subsequently served at the Ministry of Finance in 2015, where he assessed the competition impact of policy and bill proposals from other government departments. Seeking intellectual growth and the opportunity to engage with cutting-edge research, João Vítor applied to various Ph.D. programs in the United States, Canada, and the UK. He ultimately chose Cornell University for his doctoral studies, happily calling Ithaca his home during those years. He eagerly anticipates the future and the opportunities it will bring.

To my parents, Vânia and Abiatar, and my brothers, Artur and Ítalo,

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It takes a (global) village to raise a Ph.D. The web of events and people who made my doctoral studies possible is so wide and intricate that I can only hope not to be unjust in the next paragraphs.

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Table of Contents

List of Tables

List of Figures

Chapter 1

Merger Effects on Worker Outcomes:

A Review for Antitrust Enforcement

1.1 Introduction

The notion that labor markets are perfectly competitive has little traction in theoretical and empirical research (Card, 2022). Different aspects of the job market rationalize the existence of a wedge between the revenue accrued by workers' marginal product and their wages (Manning, 2011). One source of imperfect competition rising to prominence in empirical studies is employment concentration. Similar to models of oligopoly where higher market shares lead to decreased consumer surplus, monopsony, and oligopsony frameworks establish a negative relationship between the size of employers' shares in the labor market and the welfare of workers. By acquiring or merging with one another, firms may increase not only downstream concentration, i.e., the concentration in product markets, but also upstream concentration, i.e., the concentration faced by the merged firm's suppliers, including its employees and other workers in the same market. Despite the similarity between monopoly's and monopsony's consequences to welfare, antitrust enforcement has fallen behind in ensuring competition in labor markets (Marinescu and Posner, 2020).

Research on the impact of mergers on worker outcomes dates back to the mid-1980s, when Shleifer and Summers (1987) challenged the view that excess stock returns following hostile takeovers were evidence of net social surplus gains stemming from ownership changes made possible by capital markets. According to them, increased shareholder wealth did not necessarily originate in firms being more efficiently managed, but from the retraction and renegotiation of implicit

agreements with the workforce and other trading partners previously held by the departing owners and managers. A series of papers looking at hostile takeover consequences on workers ensued,[1] and later, authors focused on other types of ownership change, such as leveraged buyouts,[2] and foreign acquisitions,[3] and studied their effects on the workers in the merging or target firms. These studies depart from the collection of merger and acquisition events but do not directly account for employment concentration, indirectly affected workers, or labor markets, which challenges their ability to inform antitrust policy. Their estimates for wage and employment effects vary in size and magnitude, but it is unclear how much was due to changes in labor market competition induced by the mergers.

Around 30 years afterward, the observation of declining labor shares in Western economies sparked a burgeoning literature looking at the relationship between employment concentration, measured in local labor markets, and employee compensation.[4] Contrary to previous studies, the findings in the second wave of papers match the theoretical prediction of models of imperfect competition in labor markets. Workers in more concentrated labor markets face lower wages and, sometimes, lower employment levels, consistent with oligopsonistic markets (Boal and Ransom, 1997). This more robust evidence of reduced competition in concentrated labor markets invites antitrust authorities to consider the ramifications of mergers on worker outcomes.

[1] Rosett (1990); Gokhale et al. (1995); Conyon et al. (2001).
[2] Lichtenberg and Siegel (1990).
[3] Conyon et al. (2002b); Huttunen (2007).
[4] Martins (2018); Azar et al. (2020); Rinz (2020); Marinescu et al. (2021); Azar et al. (2022); Benmelech et al. (2022); Bassanini et al. (2023); Dodini et al. (2023).

This paper reviews the literature on merger activity and employment concentration effects on workers and provides a list of recommendations to antitrust authorities to guide their enforcement of competitive standards in labor markets. The rest of the paper is organized as follows. In Section 1.2, I review the first generation of papers comparing worker outcomes across merged and non-merged employers. In Sections 1.3 and 1.4, I describe the methods and findings from the second strand of papers, dedicated to the estimation of wage elasticities with respect to local employment concentration. I briefly summarize the legal basis and case law related to upstream antitrust enforcement in Section 1.5, and offer five recommendations to antitrust agencies in Section 1.6. Section 3.4 concludes the paper.

1.2 Literature on Direct Effects of M&A

1.2.1 Hostile Takeovers, Leverage Buyouts and Foreign Acquisitions

Changes in firm ownership through mergers and acquisitions have long been a subject of research. According to Nelson (1959), merger activity gains a national scale in the U.S. after the Civil War, with an unprecedented peak at the turn of the century, when 1,028 firms were acquired in 1899 alone, a number not sur-

passed at least until the late 1950s. The discussion around the impact of mergers on workers, however, is more recent and can be traced to the seminal work by Larry Summers and Andrei Shleifer in 1987. The 1980s had witnessed a surge in hostile takeovers, and it became a common view that increases in stock returns were evidence of the efficient and disciplinary character of these events and of capital markets in general. Shleifer and Summers (1987) challenge this view, explaining that shareholder gains were not necessarily reflective of higher net social surplus, but merely an appropriation of worker's welfare.

A firm takeover without the consent of the incumbent board of directors offers the new management an opportunity to renege on implicit agreements established between the firm and its employees – such as promises of future promotions and seniority pay raises not formally laid out in binding contracts. Previous managers are expected to uphold the informal agreements, while the new ownership can withdraw from them without any reputational damage or consideration of loyalty. The welfare of long-term trade partners, e.g., subcontractors and output distributors, can be adversely affected by the same "breach of trust" – the expression coined by the authors to represent the abrogation of long-standing informal arrangements guaranteed by the former management. Thus, researchers would have to look beyond shareholders' equity returns to measure the full impact of changes in firm ownership.

Shleifer and Summers (1987) kick-started an investigation relating hostile takeovers to employees' earnings and firm size, testing the hypothesis that stock

premiums following these transactions originated in the reduction of workers' above-market earnings. One well-known source of workers' wage premium is unionization, so it is natural that one of the earliest studies in this vein focused on collective bargaining. Indeed, Becker (1995) find that stock returns in unionized target firms were 5 percentage points higher than in non-unionized targets, but no direct evidence of wage impacts was provided, leaving the question about the source of shareholder gains open.[5]

Looking at wage contract records kept by the *Bureau of National Affairs* from 1973 to 1987, Rosett (1990) finds no significant effects on wages of unionized workers after both hostile and friendly takeovers relative to workers in non-acquired firms, contradicting the Shleifer and Summers (1987) prediction of harsher employee outcomes after hostile transactions. Given the fact that his sample only contained employees covered by collective agreements, one possible conclusion is that unionization shielded employees from the negative effects associated with takeovers.

At the national level, but nonetheless restricted to the manufacturing sector, McGuckin and Nguyen (2001) compares the wage and employment growth of plants within the same Census region and industry sector

In a study not restricted to workers covered by unions, Gokhale et al. (1995) used surveys conducted by the Federal Reserve Bank of Cleveland in Ohio's major

[5]Worth mentioning, Fallick and Hassett (1996) show that firms with unionized workers are more likely to be acquirers, rather than targets.

cities, from 1980 to 1991, in order to compute proxies of extra marginal payments and their dynamics around hostile takeover events. The proxies were three, occupational wage premiums, steepness of rewards to seniority, and concentration of employment in senior job titles, all at the employer level and within the same city-year cells. *Ex-ante*, they find that the extra marginal proxies are not significant in predicting a hostile takeover, suggesting that these transactions may not happen because of above-market worker benefits – a conclusion confirmed by Neumark and Sharpe (1996). However, *ex-post*, they find that the seniority premium wage bill is reduced by 33% after a hostile takeover, which happened not due to a fall in wages of senior employees, or by a flattening of the within-firm wage ladder, but by a reduction in the allocation of workers in upper positions. The authors calculated a 62% decrease in the concentration of employment in senior job titles following a hostile takeover; that is, the top of the job pyramid became relatively narrower in taken-over firms. At the same time, employer-specific excess occupational wages actually grew by 14.5%, irrespective of job steps. If takeovers transferred value from the workforce to shareholders, losses seemed to be reflected in the number of senior roles within companies.

A national evaluation of the impact of hostile takeovers is found in Conyon et al. (2001), where they gather data from the *London Share Price Database* and *Financial Times* between 1983 and 1996 to identify episodes of friendly and hostile changes in ownership of quoted firms in the UK, and to estimate a first difference dynamic panel at firm-level representing firms' labor demand. Compared to non-acquired firms in the period, they find that takeovers of both types gener-

ate a similar 7.5% decrease in the average acquired firm's demand for labor. The similarity of the two types of events can also be a consequence of the sample and time period considered. In a later study, including public and private firms, and a wider time window (1967-1996), Conyon et al. (2002a) find that labor demand declines by almost twice as much in hostile takeovers compared to friendly consolidations, with estimated effects of 17% and 9%, respectively. This shows that the empirical evidence of hostile events being particularly detrimental to workers is specific to the context, even within the same country. Conyon et al. (2001) and Conyon et al. (2002a) do not estimate earnings effects despite having the necessary information; wages are actually included as independent variables in the labor demand equation that the two papers estimate.

Leveraged buyouts were another form of substitution in corporate control that became more common in the 1980s. They are particularly different from other forms of acquisition, according to Lichtenberg and Siegel (1990), because oftentimes the wealth of new managers is offered as collateral to the debt hired to purchase the new company, which makes the interests of the firm and managers more aligned with each other. Using Census records at plant level, Lichtenberg and Siegel (1990) find that buyout plants become 2.2% more productive (measured by TFP) than plants not involved in buyouts in the period between 1972 and 1986. The Census *Longitudinal Research Database* also supplied information on the amount of production and non-production workers and their respective wage bills. Using that information, they estimate a decrease of 8.5% in non-production employment, while the size of production staff remains statistically unchanged.

Non-production workers also saw a decline of 5.2% in their annual compensation, while production employees' earnings grew by 3.6% after the buyout.

The intensifying economic integration in the European Union throughout the 1990s spurred a parallel line of research focused on the impact of foreign acquisitions on domestic workers. Foreign acquisitions are expected to instill new ideas and facilitate local firms' assimilation of superior production technology, with positive benefits to workers (Huttunen, 2007). On the other hand, multinational firms can alter the wage bargaining balance, as they can credibly threaten to halt local production or shift it temporarily to other countries if faced with strikes or tighter local labor laws (Conyon et al., 2002a). In the case of the UK, Conyon et al. (2002a) confirm that foreign acquisitions are followed by larger worker productivity (14.1%), but only a third of this is accrued by workers in the form of higher wages (3.4%), while labor demand fell by 6.7%. Using administrative records from Finland, Huttunen (2007) explore the heterogeneity of wage effects of foreign acquisitions across different skill levels. Three years after the acquisition, both low and high-skilled workers experienced wage gains (2.5% and 2.3%, respectively), but the employment share of high-skilled workers decreases by 3.4% compared to non-acquired firms.

1.2.2 Mergers and Acquisitions in General

The size and sign of the estimates in the aforementioned studies can be attributed, to some extent, to their attention to specific types of ownership change, namely hostile takeovers, leveraged buyouts, and foreign acquisitions. In this section, I describe the findings from the investigation of direct M&A effects on worker outcomes from consolidation events in general.

In the US, Brown and Medoff (1987) is perhaps the earliest study to employ administrative records to follow workers' earnings and firms' size after merger events. They use unemployment insurance records from the state of Michigan covering a variety of industry sectors between 1978 and 1984. By means of what can be considered an early version of current event studies, they find a modest, non-significant impact on wages, while employment exhibited up to a 9.4% increase, contingent on the year of the merger. While the merger events included were not limited to any specific type, any lesson taken from these findings has to be cognizant of the geographical scope and the absence of interstate merger activity in the data used in their investigation.

Using US Census data from all states, McGuckin and Nguyen (2001) build a panel of manufacturing firms at the plant level, and estimate a coefficient of ownership change with dependent variables being the plant's wage and size growth between the years of 1987 and 1987. Plants that changed ownership experienced a 3.3% annual increase in size growth compared to non-acquired plants within the

same Census region and 4-digit industry sector, independent of the original size of the acquired plant in 1977. Wages, on the other hand, grew 3.3% year faster, yearly, in acquired plants, but this effect is decreasing on plant size – it gets to zero for plants in the 90th percentile of the size distribution, and, in the top decile, where the majority of workers are employed, wage growth is comparatively slower in acquired plants.

Decreasing wage estimates on the size of the acquirer have also been found in the context of the UK economy. Conyon et al. (2004) combine data from the *London Share Price Database* and *Datastream* to build a panel of publicly listed firms going through M&A events between 1979 and 1991. Compared to firms that do not participate in M&A deals, but are of similar size, M&A participants have 0.14% higher wages. However, for acquirers with more than 12 employees, the vast majority of cases, the sign of the wage estimate is reversed. Independent of size, employment falls by 2%. Interestingly, these estimates only hold for mergers between firms in the same two-digit industry sector, i.e., the *horizontal* M&As - perhaps indicating that layoffs are a result of workforce overlap between the merging parties.[6]

It is common that mergers are followed by divestitures carried out by the merged firm, either due to planned restructuring or remedies imposed by antitrust authorities. Not taking this phenomenon into account can lead to overestimated employment effects. Gugler and Yurtoglu (2004) not only control their

[6]In a similar exercise, but this time including private firms in the sample, Conyon et al. (2002a) estimate decreases in employment of 19% in horizontally merged firms, larger than the 8% decrease from vertical consolidations.

labor demand estimation by divestitures but also gather data from multiple countries to study differential merger effects on employment. They implement a similar econometric approach to Conyon et al. (2002a, 2004) on a sample that includes the USA, UK, and 14 other continental Western European countries. Relative to non-acquiring firms, merged employers have an overall 2.9% lower demand after a merger. Broken down by region, the measured decline is of 12.4% in the UK, 7.9% in Western continental Europe, and null in the USA. The authors attribute this difference to more rigid labor laws in Europe, where labor adjustment is often slower and more costly than in the USA. Therefore, they conclude, insofar as mergers are a labor adjustment device, they are relatively more attractive in Europe than in the USA.

From Nordic countries, it is worth mentioning the findings of Lehto and Böckerman (2008), for the case of Finland, and Siegel and Simons (2010), for Sweden. In a sample that includes the manufacturing and construction sectors, Lehto and Böckerman (2008) employ a difference-in-differences procedure on matched establishments by propensity score. Merged establishments decrease in size by up 13% depending on the matching procedure. A close employment estimate, negative 12%, is found in Sweden, obtained by Siegel and Simons (2010) from a worker-level event study. For Swedish workers, the comparison of wages before and after the M&A event suggests a decrease of 0.5%, despite negative pre-trends in plant productivity being reversed *post*-M&A.

Beyond wages and employment, at least one example in the literature shows

that significant changes in workers' effort can follow a merger. From detailed financial reports of hospitals in California, Currie et al. (2005) measure changes in employment, wages, and effort of registered nurses after a hospital is acquired by one of the six biggest hospital chains in the state. Due to a strict segmentation of California's territory into health service areas, hospital acquisitions drastically reduce the number of employers available to registered nurses in specific markets. Surprisingly, however, the authors do not find significant changes in earnings and employment of registered nurses when their employer switches into a chain, but the number of patients allocated to them is 0.75 higher if compared to out-of-chain hospitals, that is, nurses tend to earn the same hourly rate, work similarly long, but more intensely after the acquisition.

1.2.3 Antitrust Lessons from M&A Literature

The presentation of the M&A literature in the previous sections shows that a single consistent lesson in terms of worker welfare following merger activity cannot be drawn. There are both positive and negative estimates for wage and employment trajectories following mergers and acquisitions. Moreover, from a competition policy point of view, it is not clear that the negative or positive wage and employment changes following mergers and acquisitions in the aforementioned papers have happened *because* of their impact on *labor market competition*.

Mergers are the result of merging entities' voluntary decisions (perhaps ex-

cept for target firms in hostile takeovers), and no particular empirical approach can address all endogeneity concerns – whether it is the Arellano-Bond type instrumental variables in (Conyon et al., 2001, 2002a, 2004; Gugler and Yurtoglu, 2004), Heckman correction (McGuckin and Nguyen, 2001), or pooled OLS with unit fixed effects (Brown and Medoff, 1987; Conyon et al., 2002b; Currie et al., 2005; Siegel and Simons, 2010). Unobserved relevant drivers of M&As and worker's outcomes, such as foreign competition, dwindling demand for the firm's product, proprietary production technology, and changes in product market concentration, may render non-acquired employers as inaccurate counterfactuals to firms taking part in the M&A event, which is the case for the majority of studies in the M&A literature, as can be seen in Table 1.1.

In terms of causality frameworks more commonly found in current applied economics research, Huttunen (2007) and Lehto and Böckerman (2008) set themselves apart by employing a matching procedure before the estimation of a difference-in-differences between merged and non-merged firms. From an antitrust perspective, however, it is not possible to affirm that their negative findings are a result of a merger-induced lack of labor market competition. This is because the contributing establishments for the control pool are not necessarily just those that could be alternative employers to the workers impacted by the mergers. Thus, while these studies might consistently estimate the differences between worker outcomes in merged *versus* non-merged firms, they are not necessarily doing so *within* any given labor market, which challenges the interpretation of their results from a competition perspective.

In the next section, I describe the findings from the literature that estimates the relationship between wages, employment, and a measure that is associated, on theoretical grounds, with the competition in clearly defined labor markets, the employment concentration.

1.3 Employment Concentration and Worker Outcomes

Throughout the early 2000s, both the US and other OECD countries experienced a decline in the labor share of their GDP Autor et al. (2020). This phenomenon has ignited a debate surrounding its causes. Although no consensus has been reached, many researchers suggest that the trend is largely due to a concentration of sales and profits within a smaller number of firms (Autor et al., 2020; Kehrig and Vincent, 2021; Grossman and Oberfield, 2021). While these firms in more monopolistic sectors reap greater profit margins, Grullon et al. (2019) find that their Total Factor Productivity (TFP) is not correspondingly increasing. This led to the conclusion that the increase in profitability is largely attributed to growth in market power – a hypothesis further confirmed by De Loecker et al. (2020) through their direct measurements of firms' price markups.

Another strand of papers investigates the relationship between industry concentration and workers' earnings (see Table 1.2), motivated by the conjecture that

the fall in the labor share could be explained, at least in part, by a shift in the balance of power between workers and employers. The idea is that if fewer firms concentrate economic activity in the product market, workers will have fewer, and larger, employers to negotiate within the labor market. This could potentially weaken the bargaining power of workers, and as a result, suppress their wages.

Table 1.1: Summary of the M&A Literature

Study	Type of M&A	Country	Data Source	Period	Data Remark	Control group	Method	Estimates	
								Wages	Employment
Brown and Medoff (1987)	Any	USA (MI)	MESC	1978-84	Unemployment Insurance	Non-acquired firms	Pooled OLS, Ind. FE	Null	Up to 9.4%
Rosett (1990)	Hostile	USA	BNA/CBNC WSJ	1973-87	Collecive wage contracts	Non-acquired firms	Cross-section OLS, Ind. and Year FEs	Null	-
Lichtenberg and Siegel (1990)	Leveraged Buyouts	USA	Census LRD Morgan Stanley	1972-86	Manufacturing only	Nonbuyout plants	Simultaneous Lagged Equations	3.6% (production), -5.2% (non-production)	Null (prod.), -8.5% (non-prod.)
Gokhale et al. (1995)	Hostile	USA (OH)	CSS (FRBC) WSJ	1980-91	Extramarginal payments	Non-acquired firms	First-difference OLS	14.5%[a]	-62%[b]
McGuckin and Nguyen (2001)	Any	USA	Census LRD	1977-87	Manufacturing only	Non-acquired plants	Cross section, Probit IV, Ind. and Region FEs	3%[c]	3.3%[d]
Conyon et al. (2001)	Hostile	UK	LSPD FT	1983-96	Public firms	Non-acquired firms	First-difference dynamic panel w/ lagged Ivs	-	-7.50%
Conyon et al. (2002a)	Any	UK	LSPD Cambridge DTI	1967-96	Public and Private firms	Non-M&A Firms	Dynamic Panel, Lagged IVs, Ind.-Year FEs	-	-19%(horizontal), -8%(vertical)
Conyon et al. (2002b)	Foreign Acquisition	UK	OneSource	1989-94	Private and Quoted firms	Non-acquired firms	TWFE Year and Firm FEs	3.4% (foreign), -2.1% (domestic)	-6.7% (for.), 0% (dom.)
Conyon et al. (2004)	Any	UK	LSPD Datastream	1979-91	Public firms	Non-M&A Firms	Dynamic Panel, Lagged IVs, Year FEs	0.14% (horizontal)[e]	-2% (horizontal)
Gugler and Yurtoglu (2004)	Any	USA, UK, West. Euro.	Thompson/Reuters Compustat	1987-98	Public and Private firms	Non-acquiring firms	Dynamic Panel, Lagged IVs, Firm and Year Fes	-	0% (USA), -12.4% (UK), -10% (Eur.)
Currie et al. (2005)	Any	USA (CA)	CADD/OSHPD	1989-99	Financial reports	Out-of-chain hospitals	Pooled OLS w/ Hosp. FEs	Increase in effort	Null
Huttunen (2007)	Foregin Acquisition	Finland	PESA/LDPM	1988-01	Manufacturing only	Non-acquired plants	DiD w/ propensity score matching at plant-level	2.5% (low ed.) 2.3% (high ed.)	-3.4% in high ed. emp. share
Lehto and Böckerman (2008)	Any	Finland	BRSF Talouselämä	1989-03	All sectors	Matched non-M&A establishments	DiD w/ Propensity score matching estab.-level	-	-13%
Siegel and Simons (2010)	Any	Sweden	Statistikmyndigheten	1985-98	Manufacturing only	Workers in non-M&A plants	Panel at worker-level w/ Year FEs	-0.5%	-12%

Note: This table summarizes the data, methodology, and estimates of some of the most cited papers looking into worker outcomes following mergers and acquisitions. [a] Increase in employer-specific excess occupational wages. [b] It represents the change in employer-specific concentration of workers in senior job titles, and not a decrease in the firm size as a whole. [c] Excess yearly wage growth for average-sized acquired firms. Above the 90th size percentile, wages in acquired plants start growing less than in their non-acquired counterparts. [d] Excess yearly size growth in acquired plants regardless of size. [e] Similar to McGukin, the wage effect decreases in the size acquirers. For acquirers larger than 12, in most cases, the effect becomes negative.

Notwithstanding its macroeconomic implications, this set of papers is relevant for antitrust policy for two reasons. First, the simple fact that when firms merge, they may not only pose a threat to the market's competitiveness for their products, but they effectively reduce the number of alternative employers to their workers, increasing the concentration in labor markets. Secondly, concentration in labor markets itself is akin to increased product market power in terms of reduced competition, as can be shown in models of oligopsony à la Cournot (Boal and Ransom, 1997; Azar et al., 2019).

Similar to models of oligopoly where firms independently and simultaneously choose the quantity of output to produce, in the oligopsony model, employers choose how many units of labor to hire. In the case of oligopoly, firms internalize a downward market demand curve; in the case of oligopsony, employers face an upward-sloping labor supply curve. Compared to the competitive benchmark, oligopsonistic labor markets have lower equilibrium quantities of labor and, consequently, lower equilibrium wage levels. Because the number of employers is inversely related to the wage markdown, it is possible to derive a negative relationship between the equilibrium level of wages, employment, and the concentration measured by employment Herfindahl-Hirschman Index (HHI) in an oligopsonistic labor market.[7]

Indeed, as can be seen in Table 1.2, there is widespread evidence of negative elasticities between local labor wages and local labor market concentration. An

[7] A derivation of the relationship between employment HHI and wages can be found in Section 2.1 of Arnold (2022).

increase of 10% in employment HHI correlates with decreasing wages from 0.14% up to 1.27%, depending on the context. The estimates for employment effects are more scarce, but in at least one instance, a decrease of 3.2% is found – the case of new hires in French labor markets (Marinescu et al., 2021). As mergers may significantly alter the employment concentration in local labor markets, these studies are informative to researchers and policymakers interested in the labor side effects of firm consolidation. Next, I discuss the different approaches found in the concentration literature in order to delineate labor markets and, most importantly, how authors attempt to correct the endogeneity between observed labor outcomes and local employment concentration.

The Definition of Labor Markets - The literature described in Section 1.2 sheds light on M&A's impact on merged firms, but the assessment of its competition effects requires first the delimitation of labor markets, a task carried out in the employment concentration literature. Column *Labor Market Definition* in Table 1.2 shows that most papers use commuting zones as one of the elements in constructing labor market cells.[8] The idea is that jobs predominantly require a physical presence in the workplace, which indicates that the job market is geographically bound. It has been shown that the distance to a prospective employer is a significant discouraging factor in job applications (Manning and Petrongolo, 2017; Marinescu and Rathelot, 2018). In the case of the US, commuting zones were defined to explicitly locate labor markets.[9]

[8]Except for Martins (2018), who uses Portuguese administrative divisions, the *Distritos*.

[9]`https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/` (accessed on June 22, 2023).

The other component used to define local labor markets are codes that designate economic activity, either occupation,[10] industry sector codes, [11] or skill cluster.[12] Authors then define the group of potential employers among which workers might transition using the intersection of commuting zone and economic activity, and it is within these cells that the concentration index, the HHI, is computed. The evaluation of which framing of economic activity – occupation, industry, or skill cluster – is more adequate to depict labor markets is beyond the scope of this paper. Nonetheless, negative elasticities between HHI and wages are pervasive across the different approaches used in the concentration literature.

In the case of the US economy, information on occupations is unavailable in the administrative matched employer-employee data (the LEHD), and other establishment-level datasets (such as the CMF, ASM, and LBD). For this reason, studies using the US Census records are limited to industry sector-based labor markets. The IV estimates in Rinz (2020) show a decrease of 0.5% in wage rates associated with an increase of 10% in the HHI of markets defined by commuting zone and 4-digit NAICS codes. Using the same local labor market definition, although restricted to manufacturing plants, Benmelech et al. (2022) estimate the same change in HHI to have a very similar effect, from -0.6% to -0.3%. Benmelech and colleagues also point to substantial heterogeneity in wage effects. The negative estimate is attenuated by the degree of unionization in the labor market.

[10] As is the case in Martins (2018); Azar et al. (2020); Marinescu et al. (2021); Azar et al. (2022); Bassanini et al. (2023).

[11] As in Rinz (2020); Benmelech et al. (2022)

[12] The only example of this approach so far, to the best of my knowledge, is Dodini et al. (2023).

In their preferred specification, they also show that a one standard deviation decrease in local HHI increases the plant productivity elasticity of wages by 8.5%; that is, wages are less responsive to plant productivity in more concentrated labor markets, consistent with the theory prediction about wage markdowns in oligopsony models.

For the estimation of concentration effects on US occupational labor markets, Azar et al. (2020) and Azar et al. (2022) use job vacancy posts from online platforms. In the earlier work, Azar et al. combine posts from Burning Glass Technologies into pairs of commuting zone and 6-digit occupation codes, and find a significant decrease of 0.43% in advertised wages following a 10% increase in HHI. In their second study, they use the same labor market definition, but this time the effect is estimated at -1.27%, obtained from wages posted on CareerBuilder.com.

Using administrative French records, Marinescu et al. (2021) investigate the impact of concentration on the wages and employment of new hires in labor markets defined by commuting zone and 4-digit occupation code. Similar to previous estimates in the industry sector US markets, they estimate a decrease of 0.5% in wages of new hires, at the same time that their number fall by 3.2% - both estimates associated with a 10% increase in HHI. The simultaneous negative effects on both wages and employment of new hires are consistent with oligopsony model of competition in labor markets. In line with Benmelech et al. (2022), Marinescu et al. also find that unionization rate is an attenuator of the negative impact of concentration. For French incumbent workers, Bassanini et al. (2023) estimate

a negative wage effect of 0.19%.

Both occupational and industry definitions of labor markets will form an inaccurate depiction of potential employers if worker mobility is not sufficiently contained within the reported classification codes. If workers often transfer across occupation and industry sector codes, then using such codes overestimates employment concentration. From Norwegian linked employer-employee data, Dodini et al. (2023) group workers into task-based skill clusters, which they show to attain, combined with commuting zones, lower HHI scores if compared to occupational and industry sector labor markets. Even with the new measure, they still find negative wage coefficients for concentration. They estimate that a one standard deviation increase in skill cluster HHI is associated with a 2.25% lower reemployment wage for previously dismissed workers. On the other hand, concentration has no significant impact on labor force exits.

Dealing with Endogeneity. Once labor markets are defined and the employment concentration is computed, the longitudinal nature of the datasets allows for the estimation of panel specifications where current market wages are regressed on the current level of market concentration. The models often contain time and unit-level fixed effects – e.g., market in Rinz (2020), worker in Marinescu et al. (2021), plant in Benmelech et al. (2022)) – plus a term for the market-level concentration. Depending on the level of detail in their data, studies may also include other controls such as unionization rate (Marinescu et al., 2021; Benmelech et al., 2022), plant productivity (Benmelech et al., 2022), and worker demographics (Marinescu

et al., 2021; Dodini et al., 2023).

Unobserved contemporaneous shocks, however, can alter the current level of wages and concentration simultaneously, even in perfectly competitive labor markets. If a new firm enters the labor market, for instance, demand for workers increases at the same time that employment concentration declines, as pointed out by Rose (2019). Conversely, lower demand for a product can force less efficient firms to close, which would result in lower wages and higher HHI levels in a competitive labor market.

Inspired by leave-one-out instruments used in the industrial organization literature (Nevo, 2001), authors instrument local concentration based on employment in other commuting zones under the same occupation or industry sector. In most cases, the first stage consists of regressing local labor market concentration on the inverse of the absolute number of employers in other markets (Martins, 2018; Marinescu et al., 2021; Bassanini et al., 2023) - Rinz (2020) uses the employment-weighted average of the HHIs in all other commuting zones. The logic behind this approach is that the first stage teases out changes in local concentration not related to national trends in the HHI of that occupation or industry sector. It is often the case that OLS estimates of the concentration coefficient are positive – higher concentration is correlated with higher wages – but the second stage estimates reveal the expected negative sign from the oligopsony model.

It is important to clearly state what the exclusion restriction of leave-one-out instruments means in the context of employment concentration. It requires that

local market wages are only affected by concentration in other geographical regions through the local HHI. That is, there can be no direct relationship between the inverse number of employers, or weighted average of external HHI, and local wages. This can be problematic if local wages are set to keep employees from moving to a more attractive labor market. For example, hotels in Saint-Tropez might have to pay their managers enough to keep them from working in Cannes's hotels.[13] More so, the assumption of no reverse causality requires that the concentration of hotels in Cannes cannot be caused by wages in Saint-Tropez. Because of the strenuous assumptions in leave-one-out instruments, these results must be carefully interpreted (Angrist, 2014).

Other sources of exogenous change explored in the literature are mergers and acquisition activity and mass layoffs. Due to the endogeneity of employers' entry and exit in labor markets defined by commuting zone and industry sector pairs, Benmelech et al. (2022) use an indicator for within-market M&A episodes in the first stage of their estimation. This way, the change in the predicted local HHI used for the second stage is merger-induced and does not contain the portion driven by the entry and exit of other establishments. Alternatively, in markets formed by commuting zones and skill clusters, Dodini et al. (2023) explore establishment closures and mass layoffs as shifters to the local labor demand curve. Because of the relationship between local concentration and wage-elasticity of the labor supply curve in oligopsony models (Azar et al., 2019), the estimated sharper

[13]Cannes and Saint-Tropez belong to two different commuting zones, as indicated by the *Bases de Zones d'Emploi* file - `https://www.insee.fr/fr/information/4652957` (accessed on June 23, 2023).

wage decline following downward exogenous shifts in more concentrated markets' demand curves is viewed by the authors as an indication of anticompetitive behavior in Norwegian labor markets. The ability to estimate labor market-wide effects of changes in local employment concentration comes with the cost of the assumptions needed to circumvent the endogeneity between wages, employment, and concentration. As an ensemble, these studies point to the robustness of the qualitative conclusion across various contexts and empirical approaches. The negative impact of HHI coefficients on worker outcomes found in this literature is consistent with the prediction of oligopsony models. To the extent that a merger might substantially alter labor market concentration, antitrust policymakers and researchers alike cannot ignore these results.

1.4 Employment Concentration as a Mediator of M&A Effects

The merger effects from the studies reviewed in Section 1.2 are, in their majority, identified by the difference between merged and non-merged firms or establishments. Without the explicit delimitation of labor markets, it is unclear how much of their estimates are driven by merger-induced changes in the competition for labor. The research mentioned in Section 1.3 departs from clearly defined labor markets to compute local employment concentration and estimate the effects

of concentration on wages and employment within the labor market. Although based on a theory of anticompetitive behavior in labor markets, the findings from the concentration literature do not offer evidence of the direct effects of mergers on worker outcomes within labor markets.

Two other papers by Prager and Schmitt (2021) and Arnold (2022) look at the impact of mergers on local HHI and wages. The main lesson from their investigation is that mergers with minor changes in local concentration do not significantly affect wages. As Card (2022) puts it, "... these designs provide the best available evidence that employer consolidations that raise HHI have significant negative effects on wages, at least for workers who are highly attached to the affected industry."

Prager and Schmitt group hospital mergers into quartiles according to how much change in local HHI they induce. Only events in the top quartile significantly impact wages, -4.0% for administrative staff and -6.8% for nursing and pharmacy professionals. Earnings of blue-collar workers remain unchanged across the whole distribution of mergers, possibly due to a broader range of employers in this category.

Table 1.2: Summary of the Concentration Literature

Study	Country	Data Source	Period	Data Remark	Type of Labor Market	Labor Market Definition	Endogeneity Correction	Estimates[a]	
								Wage	Employment
Martins (2018)	Portugal	Quadros de Pessoal (MTSS)	1991-2013	Matched employer-employee	Occupational	Districts and 6-digit occ. code	Leave-one-out IV	-0.14% (Employees) -0.13% (New Hires)	-
Rinz (2020)	USA	LDB/Census, ACS, W-2/IRS	1976-2015	Establishment-level, and Worke-level	Industry Sector	Commuting Zone and 4-digit NAICS	Leave-one-out IV	-0.5% (LBD) -1.1%(W-2s)	-
Azar et al. (2020)	USA	Burning Glass Technologies (BGT), OES/BLS	2016(I)-2016(IV)	Job vacancies	Occupational	Commuting Zone and 6-digit SOC	-	-0.43% (BGT)[b] -0.49%(OES)[c]	-
Marinescu et al. (2021)	France	DADS/INSEE, SUSE/DGI	2011(I)-2015(IV)	Matched employer-employee New hires	Occupational	Commuting Zone and 4-digit occ. code	Leave-one-out IV	-0.5%	-3.2%
Azar et al. (2022)	USA	CareerBuilder.com	2010(I)-2013(IV)	Job vacancies	Occupational	Commuting Zone and 6-digit SOC	Leave-one-out IV	-1.27%	-
Benmelech et al. (2022)	USA	CMF, ASM, LBD (Census)	1978-2016	Plant-level	Industry Sector	Commuting Zone and 4-digit NAICS	M&A Events	From -0.3% to -0.6%	-
Bassanini et al. (2023)	France	DADS (INSEE), SUSE (DGI)	2010-2017	Plant-level	Occupational	Commuting Zone and 4-digit occ.	Leave-one-out IV	-0.29% (new hires) -0.19% (incumbents)	-
Dodini et al. (2023)	Norway	Statistisk Sentralbyr	2003-2017	Matched employer-employee	Skill	Commuting Zone and Skill Cluster	Mass layoffs	-2.25%[d]	Null

Note: This table describes studies estimating the effects of local labor market concentration on workers' earnings and employment. Because of the endogeneity between local concentration and worker outcomes, I also report how authors aim to correct it. [a] The reported estimates represent the associated changes in wages and employment from a 10% increase in local labor market concentration measured by the HHI, except for Dodini et al. (2023), in the last row. [b] These are OLS estimates with no endogeneity correction. [c] These are OLS estimates with no endogeneity correction. [d] Measure of change in wages associated with an increase of 1,000 points in local labor market HHI, or one standard deviation in their context.

Similarly, Arnold (2022) finds that only mergers in the top ventile of the change in HHI distribution induce wage declines of 3.3% in affected labor markets. Arnold's negative estimate adds to the existing literature in three ways. First, he uses job-to-job flows to weigh all other employers within the same commuting zone, regardless of industry sector, to form a flow-probability-adjusted version of the HHI. Second, the estimation of market-level wage elasticities with respect to concentration excludes the merging firms, thus reducing confoundedness with changes in productivity or management unobserved in administrative records. Third, the estimate is robust to the inclusion of tradable industry sectors only, making it unlikely that the wage declines are a consequence of merger-induced competition changes in output markets.

On the employment margin, Prager and Schmitt (2021) find no evidence of labor quantity reduction after the mergers, and Arnold (2022) does not present estimates for market-wide size effects.[14] Prager and Schmitt use this fact to conclude that oligopsony may not be the underlying mechanism for the wage declines, since the model requires suppression of labor quantity to attain lower equilibrium wages. Both papers offer robust evidence that employment concentration is an informative predictor of M&A effects on worker outcomes.

[14] Arnold does find negative size effects for merged establishments, but, given that dismissed workers can be re-employed in the same industry and commuting zone, it is not possible to conclude that the size of the market as a whole decreases.

1.5 Basis for Antitrust Intervention in Labor Markets

Do mergers that lessen competition in labor markets configure actionable harm? Traditionally, antitrust regulation has been associated with the busting of monopolies, cartels, and collusion among sellers, with the primary objective of curbing anticompetitive practices that reduce consumer welfare (Naidu et al., 2018). In other words, antitrust norms are generally perceived as a method to protect *buyers* from non-competitive behavior by sellers. Contrarily, the service workers provide is *sold* to firms. At first glance, it might seem that workers, by their quality as sellers in a labor relationship, are out of the reach of antitrust protection. However, this notion is contradicted by case law, and the argument can be made that the current normative framework *requires* antitrust agencies to safeguard workers' interests in the face of mergers posing the risk of monopsony power.

Legal Basis and Case Law - There is consensus that labor markets are within reach of antitrust action under the U.S. legal framework. In the case of mergers and acquisitions, Section 7 of the Clayton Act does not differentiate the type of market it can be applied, and, if courts agree that a merger induces a considerable decline in competition, it can be applied to block the transaction (Marinescu and Hovenkamp, 2019; Shapiro, 2019).[15]

[15]From the 15 U.S. Code §18: "No person engaged in commerce or in any activity affecting commerce shall acquire, directly or indirectly, the whole or any part of the stock or other share capital and no person subject to the jurisdiction of the Federal Trade Commission shall acquire the whole or any part of the assets of another person engaged also in commerce or in any activity affecting commerce, where in any line of commerce or any activity affecting commerce in any section of the country, the effect of such acquisition may be substantially to lessen competition or to tend to create a monopoly."

Despite the relative scarcity of labor market considerations in merger analysis by courts (Marinescu and Posner, 2020), case law is abundant with DOJ challenges based on the risk of increased *buying power* among trading partners in various industries, such as agricultural and health sectors (Hemphill and Rose, 2018) - e.g., the merger of chicken processors pose competition harm in the market for purchase from chicken growers, or the merger between two health insurance providers may decrease the rates paid to physicians in a given area. As Hemphill and Rose (2018) argue, these cases' economic reasoning can be readily applied to merger challenges involving labor services.

Although this paper focuses on M&A activity, the literature also provides evidence of harmful conduct in labor markets unrelated to M&A activity. Marinescu and Hovenkamp (2019) and Shapiro (2019) mention the notable case of a no-poaching agreement among Adobe, Intel, Pixar, Google, Apple, and Intuit, where the firms decided not to cold call each other's tech engineers in an attempt to recruit them; the firms later settled in a class action brought by the DOJ.[16] No-poach agreement between employers are not restricted to highly specialized professionals. As shown by Krueger and Ashenfelter (2022), no-poaching clauses are pervasive in franchise contracts, covering jobs at the low end of the earnings distribution, such as food and tax preparers. The low-wage and high-turnover nature of these jobs weakens the appeal of arguments using job-specific training and investment costs to justify using such clauses economically. Collusion among

[16]DOJ Press Release 10-1076: `https://www.justice.gov/opa/pr/justice-department-requires-six-high-tech-companies-stop-entering-anticompetitive-employee`.

employers to restrict wages and worker mobility fall under practices forbidden by the Sherman Act (Naidu et al., 2018; Shapiro, 2019; Marinescu and Hovenkamp, 2019).

Consumer and Trade Partner Welfare Doctrines - In their widely cited "Mergers that Harm Sellers" paper, Professors Scott Hemphill and Nancy Rose argue, on the grounds of the existing legal framework and the numerous precedents in case law, that antitrust protection is not restricted to consumers only. In their view, courts' admittance of upstream harm caused by the concentration of buyers gave rise to a broader doctrine of antitrust reach, where consumers' and trading partners' welfare, in general, are included. Independent of the doctrine, however, monopsony, or oligopsony, does carry negative implications for the welfare of consumers under certain circumstances.

In a monopoly, an increase in output is accommodated by a decrease in the price per unit, given the negative slope of the demand curve, which causes the revenue raised on the preceding units to fall. This is represented by a marginal revenue curve that is decreasing on output and lies below the demand curve. Similarly, given the positive slope of the labor supply curve, when hiring a marginal unit of labor, the monopsonist faces an increase in the wage bill of the previously employed units. That is, the monopsonist's marginal cost of hiring is increasing on employment and lies above the labor supply curve. For this reason, argue Naidu et al. (2018), the monopsonist's overall marginal cost of production can be higher than that of an employer in a competitive labor market, where the marginal

cost of hiring is flat and equal to the prevailing wage rate. Coupled with pricing power in output markets, monopsony harms consumer welfare.

1.6 Recommendations to Antitrust Agents and Policy-makers

In this section, I offer suggestions for antitrust policy to address the potential harm in labor market competition stemming from mergers and acquisitions. My intention is not to provide a guideline list that covers all possible contingencies in merger analysis, but to develop principles based on the economic nature and empirical evidence of monopsony power in labor markets. The similarity between monopoly and monopsony is often reminded by scholars who study the subject, and not surprisingly, the recommendations are partly inspired by the existing apparatus of buyer protection.[17]

Recommendation I – Reject Lower Labor Costs From Monopsony Power as Merger Efficiencies

When challenged, the merging parties will often attempt to provide evidence

[17]There is a burgeoning literature on the topic of labor markets from the point of view of Law and Economics, and this section is based on some of the most prominent of these recent studies (Hemphill and Rose, 2018; Naidu et al., 2018; Shapiro, 2019; Marinescu and Hovenkamp, 2019; Marinescu and Posner, 2020).

that the merger creates efficiencies that offset the potential anticompetitive inclination to raise output prices. They have to prove that production efficiencies will be significant enough such that, on balance, the product price will fall regardless of the increase in market power. In addition, the proclaimed efficiencies must be merger-specific, i.e., unattainable in the absence of the merger (Naidu et al., 2018). Thus, if a firm consolidation yields wage-setting power in labor markets, challenged parties may use the lower labor costs as an efficiency gain from the merger, which can be passed along in the form of lower prices and larger quantities available to their consumers. There are at least two compelling reasons to doubt the validity of this claim. First, from case law, the antitrust doctrine does not allow the welfare gains in one market to offset the losses of another (Hemphill and Rose, 2018; Naidu et al., 2018). Second, while it is true that the total wage bill of a monopsonist is lower than that of an employer in a competitive labor market, the relevant variable for determining the profit-maximizing output is the *marginal* cost, which, all else equal, is higher for the monopsonist producer, from the logic presented before. Therefore, the justification for consumer welfare enhancement from lower labor costs is economically faulty. The most probable scenario, as pointed out by economic theory, and the empirical evidence of increased market concentration in product markets (Grullon et al., 2019), is one where merging parties with increased buying power in labor markets will restrict production and increase output prices even further, worsening the welfare of consumers and workers alike.

Recommendation II – Focus the Labor Market Analysis to Mergers with None to Narrow Product Overlap

Antitrust enforcement resources are scarce, and universally adding labor market scrutiny to all merger challenges may lead agencies to act upon fewer cases overall. In the words of Prof. Nancy Rose, "[this] tradeoff is not an obvious improvement for consumers, workers, or our overall society."[18] Mergers between competitors in sufficiently concentrated product markets already trigger analysis by the FTC, according to its Horizontal Merger Guidelines. Measured by the safeguard of workers' interests, the highest return of labor scrutiny might be in merger cases that would, under the *status quo*, "fly under the radar" given their lack of overlap in the product market, but where the merging entities source professionals from a common pool nonetheless. Empirically, Table 1.2 shows that negative elasticities between occupational markets concentration and wages were obtained both in the context of vacancy posts (Azar et al., 2020, 2022) and on-the-job records (Marinescu et al., 2021; Bassanini et al., 2023). Concomitantly, collusion among seemingly unrelated employers in anti-poaching agreements points to the irrelevance of product concentration in labor market competition, at least for some occupations. In Section 1.5, the case involving tech firms included Intuit and Pixar, a tax-software company and an animation studio. As highlighted in Marinescu and Hovenkamp (2019), the voluntary association of the participating firms indicates that they could profit from the agreement, that the workforce under the arrangement constituted a relevant labor market, and, third, that a merger

[18] See Rose (2019).

between them would be anticompetitive in that labor market.

Recommendation III – Use Current HHI Thresholds as Upper Bounds in Labor Markets

The empirical literature in Section 1.3 shows that employment concentration matters in labor markets. The negative wage elasticities are obtained from various worker subpopulations, be it applicants in the case of job vacancy posts (Azar et al., 2020, 2022), newly hired workers (Marinescu et al., 2021; Bassanini et al., 2023), or those already employed (Martins, 2018; Rinz, 2020; Benmelech et al., 2022). When mergers are directly accounted for, their impact on concentration is a predictor of the ultimate wage effect, whether labor markets are defined by occupation (Prager and Schmitt, 2021), or industry sectors (Arnold, 2022).[19]

The FTC's Horizontal Merger Guidelines offer a scale of concentration measured by HHI to classify markets as *Unconcentrated* (HHI below 1,500), *Moderately Concentrated* (HHI between 1,500 and 2,500), and *Highly Concentrated* (HHI above 2,500). The thresholds have been widely used for product markets scrutiny, but there are reasons to suspect their ampleness if promptly transferred to labor markets (Naidu et al., 2018; Marinescu and Hovenkamp, 2019). Monopsony is thought to be the mirrored version of monopoly, and the similarity may lead to

[19]Early indications of the importance of concentration can also be found in the literature covered in Section 1.2. Despite concentration not being an object of interest in their investigation, McGuckin and Nguyen (2001) and Conyon et al. (2004) estimate merger wage effects that are decreasing on the acquirer's size. All else equal, the larger an employer is, the more concentrated will be its labor market, and, according to their estimates, lower expected wages ensue.

the conclusion that competition in labor markets is similar to that of product markets. Naidu et al. (2018) raise the point that labor markets are more predisposed to monopsony than product markets are to monopoly, for the following reasons: (i) while the purchase of products only requires the consumer's willingness and ability to acquire that particular good, a labor contract requires both seller and buyer to agree – labor markets are *double-sided*, in the matching literature terminology; (ii) in the current globalized and connected economy, goods are quickly shipped, transferred, and commerce is less local than it used to be – contrarily, labor services require a physical presence in the workplace, restricting the available options to prospective workers and employers within geographical bounds; (iii) product characteristics are more easily comparable, and the purchase choice is often non-consequential, while non-wage benefits and work amenities make jobs challenging to compare, especially given long-term career consequences. Labor markets are thus likely *thinner* than product markets in general, which calls for lower thresholds to trigger antitrust scrutiny.

Recommendation IV – Drop Labor Quantity Requirements In Wage-Setting Evaluation

Lower competition among buyers does not always simultaneously result in lower prices and quantity. Hemphill and Rose (2018) distinguish buyer power between cases of *classical monopsony*, where changes in both the quantity and price margins occur, and cases of *increased bargaining leverage*, where there is no decline

in the amount purchased but the price per unit falls. In the increased bargaining leverage situation, buyers appropriate surplus from sellers by decreasing the outside value of the transaction, forcing sellers to negotiate in unfavorable terms. In the case of labor markets, a merger between two employers may not necessarily result in fewer jobs, but lower wage rates overall, since the merger precludes workers from each merging entity to seek the other as an outside option. This is consistent with empirical evidence related to hospital mergers and their negative wage impact on the nursing staff – Prager and Schmitt (2021) does not find evidence of employment decrease even among the mergers inducing wage decreases, similar to Currie et al. (2005) who finds negative effort-adjusted wages for registered nurses despite no increase in separations. According to Hemphill and Rose (2018), the absence of employment effects should not excuse the merger and prevent antitrust from protecting workers' welfare, because a reduction in competition is what entailed employers' increased leverage in the wage negotiation. Thus, if antitrust agencies and courts require proof of reduced quantity as a necessary condition to rule buyer power, they will risk approving anticompetitive mergers, especially if the workers involved are employed in sectors with inelastic demand, such as health-related activities.

Recommendation V – Apply The Hypothetical Monopsnist Test to Relevant Labor Markets

Authors have suggested that the FTC can include in its guidelines a monop-

sony version of the *Hypothetical Monopolist Test* to determine the relevant labor market (Naidu et al., 2018; Marinescu and Hovenkamp, 2019; Azar et al., 2020). The idea is to use supplemental measures of wage markdowns and labor supply elasticities to compute an equivalent expression for evaluating product markets. The test aims to find the smallest labor market on which a hypothetical monoposonist would find it profitable to impose a small, significant non-transitory wage decrease (SSNDW). More precisely, the test considers the critical labor supply elasticity with respect to wage w, $\varepsilon_{L,w}$, given by

$$\varepsilon_{L,w} = \frac{1}{\mu + \frac{\Delta w}{w}}$$

where μ is the wage markdown, and $\Delta w/w$ represents the SSNDW.[20] In monopolized product markets, the benchmark for price increase is 5%. Naidu et al. (2018) and Marinescu and Hovenkamp (2019) suggest that the same amount can be used for the SSNDW. Measures of the wage markdown μ will vary according to the case under analysis. Still, the average for the U.S. manufacturing sector is 0.538; that is, workers keep 65 cents of the marginal dollar revenue (Yeh et al., 2022).[21] With $\mu = 0.538$ and a hypothetical decrease in wages of 5%, the critical labor supply elasticity is equal to 1.7, approximately. That is, a hypothetical monopsonist would find it profitable to decrease wages by 5% in a market with labor supply elasticity less than or equal to 1.7. Suppose the elasticity is estimated to be higher than 1.7 for the suggested market under analysis. In that case, the market is too

[20] A derivation of the critical elasticity formula can be found in Section 3.4 of (Azar et al., 2020).
[21] Explicitly, $\mu = \frac{1-0.65}{0.65} \approx 0.538$.

narrow, and the next closest employer, occupation, or industry sector should be added before proceeding to another iteration of the elasticity estimation. Unless the merging parties are part of labor markets with a proven markdown above 53.8%, labor supply elasticities below 1.7 indicate the market's relevance.

1.7 Conclusion

The empirical study of merger activity's effects on workers has two strands. The first one stemmed from the surge in corporate control changes operated via capital markets in the early 1980s. The focus of economic research at the time was on the rupture of informal arrangements between management and other employees, and why the reneging of these implicit contracts could cause higher equity returns following hostile takeovers. Later, when globalization allowed the flow of capital across national borders, the attention switched to the effect on domestic workers in companies subject to foreign acquisition. The results of this literature vary in magnitude and sign, and their informative value to antitrust is limited due to the challenge of attributing their findings to changes in the competition for labor services.

A second literature originates in the late 2010s, when attention was brought to the rise of "superstar" firms and the possibility that their dominance could explain the downward trend in the labor share of national income observed on both sides of the North Atlantic. The debate around the causes of the fall in the labor share

is still open, but it instigated a proliferation of studies dedicated to employment concentration. Authors leveraged the availability of data that allows direct measurement of concentration in clearly defined labor markets. The findings point to a negative relationship between concentration and worker outcomes in different segments of the labor force (applicants, new hires, and incumbents), across choices of labor market design (whether by occupation or industry sector), and in several countries.

Unlike the earlier merger literature, the estimates in the second wave of papers has straight parallels in models of imperfectly competitive labor markets. As mergers mechanically increase the concentration faced by workers of the merging parties, and other workers in the same labor market, the associated lessening of competition suggested by the empirical literature calls for antitrust intervention.

Antitrust's legal basis and case law in the United States are compatible with intervention in mergers that threaten competition in the labor market, whether under a narrow interpretation of the consumer welfare doctrine or a broader one that includes merging firms' trading partners in general. Given the similarity between monopoly and monopsony, the accumulated experience and devices from antitrust enforcement in product markets require little adaptation in the context of labor markets, except for some consideration of intrinsic aspects of labor relationships.

Chapter 2

Local Labor Market Effects of Mergers and Acquisitions in Developing Countries: Evidence from Brazil

2.1 Introduction

A growing body of research in recent years has pointed in the direction of non-competitive behavior in labor markets (Card, 2022). The formalization of a model where profit-maximizing firms internalize an upward-sloping labor supply curve and, as a result, choose to hire fewer workers at a lower wage rate, dates back to the 1930s (Robinson, 1933), but a renewed interest in the subject was sparked by the debate around the reasons for the labor share decline observed in developed economies (Autor et al., 2020). The rise of firms that concentrate ever larger shares of industry sales in a globalized market, the so-called *superstar* firms, has launched many in the profession into the empirical investigation of labor market imperfection related to the increase in the size of employers. While the debate about the underlying causes of the fall in the labor share of the GDP is far from settled (Grossman and Oberfield, 2021), one cannot neglect the burgeoning literature pointing to a negative relationship between labor market outcomes and employment concentration, and proposals for policy intervention to protect workers abound, especially within reach of antitrust regulation (Naidu et al., 2018; Marinescu and Hovenkamp, 2019; Marinescu and Posner, 2020).

Naturally, mergers and acquisitions (M&As) raise an immediate concern about competitiveness in labor markets. By means of the consolidation of different employers under the same ownership and management, mergers mechanically alter the number of firms competing for labor services and, therefore, might, in principle, tilt the balance of bargaining power in an unfavorable manner to workers.

The relationship between a lower number of employers and worse workers' outcomes relies on more than just intuition, and it has both theoretical and empirical grounds. Building upon the monopsony model from Robinson (1933), Boal and Ransom (1997) show that, similar to the case of oligopoly, a model of *oligopsony* à la Cournot generates wage markdowns that decrease as the number of employers falls, i.e., wages represent a lower fraction of the marginal revenue product of labor as employment gets concentrated among fewer firms. Negative wage elasticities with respect to employment concentration have been confirmed in various contexts [1]. Typically, employment concentration is measured by the Herfindahl–Hirschman Index (HHI) over firms' workforce shares in local markets, sometimes defined by a combination of geographical region and industry sector, or region and occupational codes. This measurement is made possible by the use of linked employer-employee administrative datasets.

In this paper, I investigate the local labor market effects of mergers and acquisitions, and the role played by labor market concentration as a channel of these effects. I combine two different administrative datasets from Brazil that allow me to identify merged and acquired establishments, delineate local labor markets, and causally estimate changes in employment, workers' earnings, and local concentration measured by the HHI. The findings consist of three sequential steps. First, I show that null effects of the M&As on workers' earnings and market HHI cannot be rejected, while employment significantly declines in markets that witness

[1] See Martins (2018); Lipsius (2018); Azar et al. (2019); Marinescu et al. (2021); Azar et al. (2020); Devereux and Studnicka (2023); Dodini et al. (2023); Benmelech et al. (2022); Rinz (2020); Bassanini et al. (2023).

a firm consolidation event. Next, I split the estimation procedure between two separate groups of firms: the ones that participate in M&As, and the bystander employers in the same market, which I will call spillover firms. The separation of the two types of firms shows that they respond differently to merger activity. The negative employment effects found at the market level are primarily carried out by merging firms, while spillover firms show a small, albeit not significant, increase in their size. By looking at the trajectory of hires and separations, I find that the negative adjustment in merging firms' employment is given by an abrupt decline in new hiring, while separations are kept at pre-merger levels for at least one year after the event.

The third part of the analysis contains the main result of this paper. In order to evaluate if larger increments in local labor market concentration deepen the earnings effects of mergers, as predicted by the oligopsony theory, I compare the estimates in spillover firms between mergers with no change in concentration and mergers at the top of the distribution of concentration shocks. Contrary to the previous literature, I find that the earnings in spillover firms are similar in both cases, which indicates that concentration plays little to no role in explaining the market-wide effects of M&As in Brazil. In mergers that induce no change in concentration, workers' earnings in spillover firms fall by 1.1%; the same estimate is obtained from within-market mergers with significant increases in local HHI. Moreover, the events with no change in concentration seem to induce a growth in the size of spillover firms, consistent with the logic that the negative employment adjustment promoted within merging firms prompts an increase in the supply of

labor to other firms in the same labor market. This increase in the labor supply available to spillover firms is accommodated by an increase in their employment but at lower wage rate. This is confirmed when I explicitly compare the *pre* and *post*-merger earnings of new hires in spillover firms.

The empirical strategy of this paper relies on a event study design based on the comparison of different local labor markets regarding the year that they witness their first merger event. Due to the concern of treatment rollout and heterogeneity giving rise to problematic estimates (Sun and Abraham, 2021; Roth et al., 2022; de Chaisemartin and D'Haultfœuille, 2022), I depart from the more traditional two-way fixed effects model and implement the estimation proposed in Callaway and Sant'Anna (2021), where the control group to treated markets consists of other markets that will eventually be treated in subsequent years. Also, given that I'm interested in market-wide competition for labor services, I attempt to mitigate the effects of alternative mechanisms connecting merger activity to labor market outcomes. Employment level and wages may be altered by M&A activity through mechanisms other than the competition for labor services. When two firms in the same industry sector merge, higher price setting in their output market has ramifications for the market for inputs not necessarily related to higher bargaining power with upstream service providers – for instance, lower wages and employment could be driven by the monopolist's decision to produce below the competitive benchmark, absent of changes to their ability to set wages. Another possibility is that merged or acquired firms are better equipped to change the composition of their workforce – in case bigger firms can hire younger, less experienced work-

ers, or workers with lower educational attainment, without hindering productivity, lower observed wages could be merely a consequence of lower compensation corresponding to these attributes. In order to best control for the product market concentration and labor compositional mechanisms, I restrict the analysis to tradable sectors only and, leveraging on the demographic details available in the data, I estimate effects on an earnings measure that takes observable attributes of the workers into account.

This paper contributes to two strands of the literature on the effects of employer consolidation on labor outcomes. The first one is related to the direct effects of mergers and acquisitions on employment and earnings in acquired and merged establishments (Shleifer and Summers, 1987; Brown and Medoff, 1987; Conyon et al., 2001, 2002a; Lehto and Böckerman, 2008; Siegel and Simons, 2010; He and le Maire, 2019; Lagaras, 2023; Guanziroli, 2022). Expanding on this literature, I focus not only on target establishments or firms, but also on keeping track of earnings and employment at other employers operating in the same labor market. This observation increases the understanding of market-wide effects of merger activity. The second related literature is one that directly studies the relationship between labor market concentration and wages, and employment (Martins, 2018; Lipsius, 2018; Azar et al., 2019; Marinescu et al., 2021; Azar et al., 2020; Devereux and Studnicka, 2023; Dodini et al., 2023; Benmelech et al., 2022; Rinz, 2020; Bassanini et al., 2023). Here, my contribution is to expand the evidence beyond the context of developed economies, and look at the role of concentration in earnings and employment effects in a developing economy. To the best of my

knowledge, this is the first study exploring market-wide effects of merger activity from multiple M&A events, in a wide range of industry sectors, in a developing economy. The most closely related works to this paper are Arnold (2022) and Prager and Schmitt (2021); they both estimate M&A effects in U.S. labor markets, and study the role of employment concentration in mediating these effects, with the difference that Arnold uses administrative data for a wide range of industry sectors, while Prager and Schmitt focus on the hospital sector. They both confirm that, as predicted by oligopsony theory, mergers that induce little to no effect in concentration also have negligible earnings effects, and significant wage declines are only found among merger events in the top of the distribution of concentration changes. My work departs from theirs in the finding that M&As with larger concentration increases do not generate sharper wage declines in the Brazilian case, and mergers with predicted null change in concentration are followed by an increase in the size of spillover firms[2].

The remainder of the paper is organized as follows. In Section 3.1, I describe the data used in the paper. The main results are reported in Section 2.4, where I subdivide the analysis starting from merger activity effects in general, then across different firms in the labor market with respect to their participation in M&As, and, lastly, across events with different predicted impacts in local concentration. I present the robustness of the findings with respect to the possibility of treatment anticipation in Section 2.5. Section 2.6 offers a discussion of the results in view of

[2]While Arnold (2022) does not report market wide employment effects, employment pre-trends in Prager and Schmitt (2021)'s event studies preclude them from making assertive claims on the size of labor markets after hospital consolidation events.

the literature and auxiliary evidence of management practices in middle to low-income countries. Section 3.4 concludes the paper.

2.2 Data

The data used in this paper is composed of two different administrative releases by the Brazilian federal government. The first one is the *Relação Anual de Informações Sociais* - RAIS, the main source for worker and job characteristics information. The second is *Dados Públicos CNPJ* - DPC, a business registry from which I extract the list of establishments that went through a merger or acquisition. More detail about each dataset is provided below. For a complete step-by-step o the data handling and construction, see Appendix A.1.

2.2.1 Worker data - RAIS

RAIS is a matched employer-employee administrative record provided by the Brazilian Ministry of Labor on a yearly basis, and it covers the entirety of the country's formal labor market, which employs around 70% of the workers (Hallak Neto et al., 2012). In terms of the U.S. Census database, RAIS is similar to the Longitudinal Employer-Household Dynamics - LEHD.

Each observation of RAIS contains separate worker and establishment identi-

fiers. The establishment identifier is hierarchical, and from its first 8 digits, or its *root*, I can also retrieve the firm identifier. As for the workers, the available characteristics are color/race, sex, age, and educational achievement. In the case of establishments, it is possible to see the city where they are located, and the establishment's 5-digit industry sector. The data also contains variables related to the job itself, such as the average monthly earnings, contractual weekly hours, date of admission, and date of separation, in case the job was terminated within that year. Differently from the U.S. Census LEHD, RAIS also reports the occupation of the worker in that particular job.

I use the variables of location and industry sector to delineate local labor markets. Local labor market details are discussed in Section 2.3.1. For this paper, I use RAIS in years ranging from 2002 up to 2017, totaling more than 1.1 billion observations. A confidentiality agreement restricts access to the data at the individual level.

2.2.2 Business Data - DPC

The *Dados Públicos CNPJ* - DPC, is a business registry made available by *Receita Federal*, the Brazilian tax collection agency. A similar counterpart to the DPC in the U.S. Census system is the Longitudinal Business Database—LBD. The DPC contains information on the universe of establishments ever registered with the agency, and it is updated on a monthly basis. There is a total of more than 42

million observations. Access to the DPC is public, and the files can be downloaded from the revenue agency's website[3]

From the DPC, it is possible to see an establishment's identifier, postal code, industry sector, and, to some extent, its capital table. Primarily important for this paper, DPC also discloses the variable describing the reason for the termination of a business. By law, anytime a firm or establishment is acquired by or merged with another, its identifier is retired, and a new one is issued for the newly created enterprise. Therefore, merged and acquired establishments can be flagged by the reported reason its identifier was retired. Other grounds for business termination include bankruptcy and various forms of tax penalties.

Despite the richness of details in the business registry, one cannot use it to identify all the parties involved in an M&A event. Only the *acquired* or *pre-merger* entities are flagged as having been targeted by an M&A. However, the identifiers of the firms and establishments after the consolidation are not reported, i.e., the *acquirer* and newly-*merged* firm identifiers are not available. The identification of all parties is important to distinguish mergers that happen within any given labor market from those between employers previously operating in separate markets – such distinction will be used to derive an important result regarding the role of employment concentration and the outcome effects of M&As. In order to identify all firms involved in a concentration event, I use the dynamic feature of the worker data, RAIS. More specifically, I observe the flows of workers departing from ac-

[3]See `https://www.gov.br/receitafederal/pt-br/assuntos/orientacao-tributaria/cadastros/consultas/dados-publicos-cnpj`.

quired establishments to help me identify the acquirer firm. The most common destination of these workers in the last year I observe acquired establishments is considered to be the acquirer business[4].

2.2.3 Commuting Zones

The commuting zones are imported from the Brazilian Decennial Census of 2010. Each commuting zone represents contiguous cities between which people commute either for study or work. The worker data is merged with the commuting zone information using the city codes in both datasets.

2.3 Empirical Strategy

The parameter of interest in this paper is the average treatment effect of an M&A on local labor markets. A local labor market is defined as pairs of commuting zone and industry sector code. The main outcome variables are the workers' earnings, employment, and the local level of employment concentration. This section presents more detail about the estimation procedure and the identification assumptions.

[4]Figure A.1, in Appendix A.1.2, shows the distribution of coalition sizes leaving acquired establishments in years preceding the M&A.

2.3.1 Local Labor Markets

In order to measure the employment concentration of labor markets, one has to choose how to define the labor markets in the first place. Recent studies have used either a combination of industry sector codes and commuting zone (Rinz, 2020; Benmelech et al., 2022; Prager and Schmitt, 2021; Arnold, 2022), occupation codes and commuting zone (Azar et al., 2019, 2020; Marinescu et al., 2021), or a data-driven approach that leverages on worker-flow observations (Nimczik, 2018). In this paper, I will use the definition of local labor markets based on industry codes and commuting zones, i.e., each market will represent an industry activity geographically located in economically integrated cities. One drawback of this definition is that not all workers in a labor market are equally bound to the same industry sector or commuting zone. Accountants, for instance, have skills easily transferrable across different industries within the same region, while nurses are bound by job opportunities in the health sector only, and thus will likely be more willing to switch cities if necessary. On the other hand, I see two advantages to this approach – (i) this is a definition readily available to researchers and policy-makers alike, as it does not require further modeling assumptions based on other characteristics of jobs or workers, and (ii) it makes the present investigation comparable to most other studies in the related literature using a similar definition of labor markets.

As I am interested in the labor market effects of mergers, I will also restrict the analysis to tradable sectors only. The idea is that unobserved changes in *product*

markets that possibly follow M&As might explain some, if not all, of the post-merger wage and employment decline in treated labor markets. By virtue of increased pricing power on the product market after an M&A, bigger consolidated firms may decrease their output level and, therefore, reduce their demand for labor. Any declines in wages and employment could then be, to some extent, the result of lower overall demand for labor followed by increased monopoly power, and not necessarily a consequence of any changes in the competition for labor services. A strategy to deal with changes in downstream competition is to narrow the analysis to tradable sectors only (Arnold, 2022). The rationale behind this strategy is that the competition of foreign products and services precludes the rise in the market power of the merged firms, thus shutting down the monopoly channel connecting M&A events to labor market outcomes[5].

The employment concentration measure I will use in the remainder of the paper is the Herfindahl–Hirschman Index (HHI) computed at the local labor market level each year. More specifically, for a market m and its set of firms $\mathcal{F}_m$, the HHI in year t is given by

$$HHI_{m,t} = \sum_{f \in \mathcal{F}_m} s_{f,t}^2$$

where $s_{f,t}$ is the percentage share of employment of firm $f \in \mathcal{F}_m$ as of December 31st in year t. When the local labor market has one single employer, the HHI

[5]The selection of tradable industries follows the classification in Dix-Carneiro and Kovak (2017).

registers 10,000 points. For an HHI level of h points, the equivalent number n of equally sized employers is given by $n = 10,000/h$. Also, if two employers with shares s_1 and s_2 merge, the equivalent increase in HHI, absent of employment effects, is given by the product $2s_1s_2$. Lastly, an increase of δ points in HHI is equivalent to the merger of two equally sized firms with employment shares equal to $\sqrt{\delta/2}$ each.

2.3.2 Event Study Setup

It is possible to assume that consolidation of employers might have lingering effects on local labor markets, and therefore the measure of effects across different lengths of time after mergers take place should be part of the analysis. More so, for the years before the merger, it is also important to observe the difference between the outcomes of treated markets and their pool of control units. Thus, I will base the empirical analysis on the estimation of an event study around the time that local labor markets experience a merger.

The predominant approach to specifying the event study with multiple cross-sectional units is to use a two-way fixed effects equation with leads and lags of an indicator variable for the treated units (Callaway and Sant'Anna, 2021). However, some concerns raised in the recent literature investigating the two-way fixed effect approach seem applicable in the context of this paper (Roth et al., 2022). First, there is variation in the treatment timing of local labor markets, i.e., not all markets

witness a merger in the same year. Second, the treatment effects may not be the same for different cohorts of treated markets, e.g., the treatment effect on markets hosting a merger in 2008 may differ from that of markets treated in 2010, and so on. With multiple treatment periods and heterogeneous effects across cohorts, the two-way fixed effects specification generates questionable event study estimates that have been only recently brought to light[6] - namely the so called "negative weights" issue and the possibility of "cross-lag" contamination laid out in Sun and Abraham (2021).

In any particular year, the treatment rollout creates three different types of markets, namely the (i) treated markets, (ii) the not-yet-treated markets, and (iii) the never-treated markets, meaning the markets that never witness an M&A in the years under observation. If never hosting an M&A event is correlated to unobserved characteristics of the never-treated markets, then their ability to serve as counterfactuals to treated markets can be compromised. At the same time, if participation in the treated pool is also endogenous to the treated market's unobserved characteristics, then the endogeneity related to ever hosting M&A activity is likely ameliorated by using not-yet-treated markets as controls; after all, the parameter of interest is the average treatment effect on *treated* units, the ATT. For these reasons, I apply the estimation proposed by Callaway and Sant'Anna (2021) in my investigation. With their method, I can estimate the ATT of merger activity on local labor markets' outcomes using the not-yet-treated units as the counter-

[6]See de Chaisemartin and D'Haultfœuille (2022) for a survey of the literature on problems associated with TWFE models and proposed solutions.

factual group and avoid the TWFE specification's issues reported in the literature. In addition, in the context of mergers, treatment anticipation is a possibility that their ATT estimation can explicitly address. Next, I expose the identification assumptions from Callaway and Sant'Anna (2021) in the context of merger activity in local labor markets.

Let $Y_{m,t}$ denote the outcome variable of local labor market m in year t. If an M&A takes place at market m in a year g, the outcome variable is denoted by $Y_{m,t}(g)$, for all years t. Begin by considering the parameter

$$ATT(g,t) = \mathbb{E}\left[Y_{m,t}(g) - Y_{m,t}(0) \mid G_g(m) = 1\right] \tag{2.1}$$

where $Y_{m,t}(0)$ is the potential outcome of market m in year t had it not been subject to an M&A, and $G_g(m)$ is an indicator function that is equal to 1 if m is treated in year g and 0 otherwise. $ATT(g,t)$ is what Callaway and Sant'Anna (2021) call a *group-time average treatment effect*, and in the present case, it represents the expected change in outcome Y in year t for all markets that had an M&A event in year g, e.g., $ATT(2008, 2012)$ is the ATT value in 2012 for markets treated in 2008. The group-time average treatment effects can be combined into coefficients that recover event-study type estimates (Callaway and Sant'Anna, 2021). More precisely, the expected change in outcome Y at market m after l periods of exposure to treatment is given by

$$\beta_{ES}^{Y}(l) = \sum_{g \in \mathcal{G}} \mathbb{1}\{g + l \leq \mathcal{T}\} P(G = g \mid g + l \leq \mathcal{T}) ATT(g, g + l) \tag{2.2}$$

where $\mathcal{G}$ is the set of all treatment years and $\mathcal{T}$ is the last year of observation. The term $P(G = g \mid g+l \leq \mathcal{T})$ weighs all the group-time $ATT(g,t)$ in which t is observed, and $t = g + l$. The more units in a treatment cohort, the more weight that cohort gets in the event study coefficient $\beta_{ES}^{Y}(l)$.

Identification

The causal identification of $ATT(g,t)$ in Equation (2.1) relies on a modified version of the parallel trends' assumption used in canonical 2×2 difference-in-differences settings. This and all the other assumptions needed for identification are presented below.

Assumption 1 (Irreversible Treatment) For $t = 1, \ldots, \mathcal{T}$, $G_1(m) = 0$ almost surely (a.s.), and, if $G_t(m) = 1$, then $G_{t'}(m) = 1$ for all $t' > t$ almost surely.

Assumption 2 (Random Sampling) Treatment status and outcome variables of individual markets are independent and identically distributed, i.e. $\{Y_{m,t}, G_t(m)\}_{(m,t)=(1,1)}^{(M,\mathcal{T})}$ is *iid*.

Assumption 3 (Treatment Anticipation of ζ Years) Let $\mathcal{G}$ be the set of all years of

treatment. For all $t < g - \zeta$ and $g \in \mathcal{G}$,

$$\mathbb{E}\Big[Y_{m,t}(g) \mid G_g(m) = 1\Big] = \mathbb{E}\Big[Y_{m,t}(0) \mid G_g(m) = 1\Big] \text{ a.s.} \tag{2.3}$$

Assumption 4 (Parallel Trends Based on "Not-Yet-Treated" Units) For all $g \in \mathcal{G}$, $t \geq g - \zeta$, and $s > t + \zeta$, the following equality holds a.s.

$$\mathbb{E}\Big[Y_{m,t}(0) - Y_{m,t-1}(0) \mid G_g(m) = 1\Big] = \mathbb{E}\Big[Y_{m,t}(0) - Y_{m,t-1}(0) \mid G_g(m) = 0, G_s(m) = 0\Big] \tag{2.4}$$

Assumption 5 (Overlap) For each $t \in \{2, \ldots, \mathcal{T}\}$, and group g in $\mathcal{G}$, at least one market is treated in year g, and for years $g + t$, at least one market remains untreated.

Assumption 1 imposes that once a local labor market is treated with an M&A, it remains treated throughout the analysis and does not contribute again to the control group of the estimation. For markets with multiple M&A events in different years, I will use the first event as the treatment date. Most markets never had an M&A in the years of observation. Following that, out of any particular number of years with merger activity, the second most common case is that of markets with only one year of M&A events (Figure 2.1a) [7]. Assumption 2 is met by the use of a balanced panel. Assumption 3 is a relaxation of the canonical *no treatment anticipation* condition in 2×2 DiD setups. Here, one can allow the

[7] Markets with one or multiple M&As in the same year are equally considered to be treated in that year.

treatment to begin as far back as the context requires, effectively resulting in two changes: (i) moving the more commonly used normalization point in event studies from $t = -1$ to $t = -\zeta - 1$; and (ii), removing the units already under anticipated treatment dynamics from the control units pool. Mergers and Acquisitions can be lengthy processes, and the news of an M&A might induce changes in the behavior of firms and workers before the reported year of the event. I present results with no anticipation of treatment, i.e., $\zeta = 0$, but I also find robustness to $\zeta = 1$.

Assumption 4 states the main requirement for identification, namely that the year-over-year change in the outcome of treated markets, had they not been treated with an M&A, is the same as that of markets that will eventually get treated in the following years—the *not-yeat-treated* units. In conjunction with Assumption 3, the parallel trends assumption imposes that, in year t, the counterfactual of previously treated markets are the ones that get treated in years later than $t + 1 + \zeta$ – e.g., for a given treated market m, and anticipation $\zeta = 0$, its counterfactual in year, say, 2010, corresponds to all markets that will be treated in 2011 or later; notice that, in this example, markets treated in 2010 are already subject to treatment dynamics, and therefore cannot be used as controls to treated markets in 2010.

Figure 2.1b shows the percentage of markets by treatment status every year according to the occurrence of the first M&A event. The majority of labor markets have had no M&A activity throughout the years of observation. For pairs of commuting zone and 3-digit tradable industry codes, the share of never-treated

markets is 86.72% - or 7,526 out of 8,678 markets. The remaining markets are either treated or not-yet-treated, depending on the year. In addition, Figure 2.1b shows that in all years, there is a positive fraction of markets that remain untreated, on top of never there being a year with 100% of markets treated, which contemplates Assumption 5. At the same time, the trajectory of the fraction of Treated and Not-Yet-Treated markets is smooth across the years, not indicating any noticeable discontinuities that could demand inference by different time windows. As is common in event studies, fewer markets remain untreated towards the end of the timeline under observation. For this reason, I'll restrict the analysis to a window of 5 years around the merger event. Standard assumptions guarantee the estimation of cluster-robust standard errors by means of a multiplier bootstrap procedure [8].

The Estimand and Control Groups

Under the assumptions outlined above, one can rewrite the treatment effect in 2.1 as

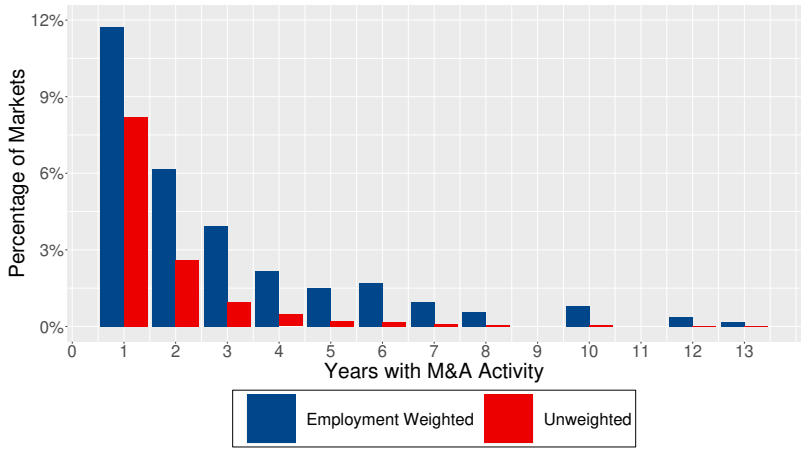
$$ATT(g,t) = \mathbb{E}\Big[Y_{m,t} - Y_{m,g-1} \mid G_g(m) = 1\Big] - \mathbb{E}\Big[Y_{m,t} - Y_{m,g-1} \mid G_{t+1}(m) = 0\Big] \tag{2.5}$$

effectively obtaining the main estimand used in this paper. Let me take a moment

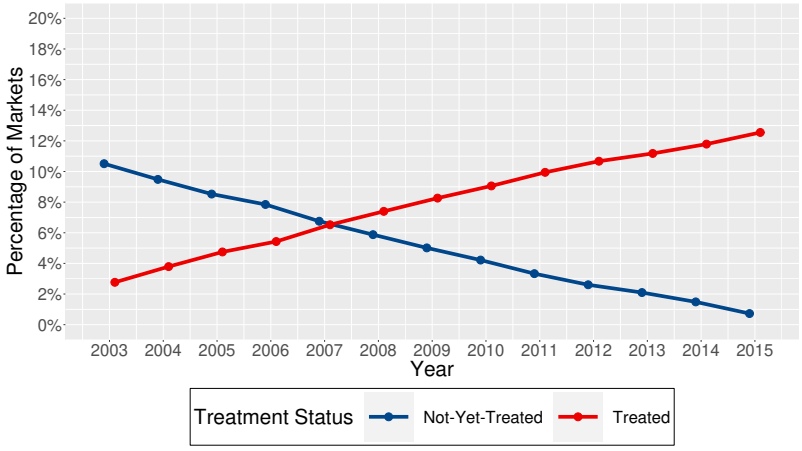
[8] An exposition of the assumptions necessary for inference is beyond the scope of this paper, and I refer the reader to Section 4, page 211, in Callaway and Sant'Anna (2021).

Figure 2.1: Years With Merger Activity per Market and Treatment Status

(a) Years With Merger Activity per Market



(b) Treatment Status



Note: Pairs of commuting zone and 3-digit industry code define local labor markets. The Not-Yet-Treated status refers to markets that eventually witness an M&A event in the following observed years. Most markets never experience an M&A event. The second most common case is that of markets with only one year of merger activity.

to describe the terms of the expression in Eq. 2.5. Suppose that a group of local labor markets had M&As in year g. The average treatment effect on this group, at any year t, the value $ATT(g,t)$, is equivalent to the difference in the average increment of their outcome variable since one year before treatment, i.e., year $g-1$, and the average increment of all markets not yet subject to an M&A by year $t+1$, relative to that same base year $g-1$. In other words, the 2012 wage effect on a labor market treated in 2008, noted by $ATT(2008, 2012)$, is identified by the difference between the wage growth since 2007 of all markets treated in 2008, and the wage growth among eventually treated markets that remain not treated by 2013. Notice that, in the case of allowing for a positive time length of treatment anticipation, $\zeta = 1$, for instance, the markets used in the control group would have to be those that remain untreated up to 2014, i.e., two years later than the year at which the effect is being measured, also moving the reference year from 2007 to 2006.

2.3.3 The Earnings Variable

One of the possible consequences of mergers is the change in workforce composition within merged firms, and, ultimately in the whole labor market. This is especially relevant if, for instance, bigger firms are able to hire younger, less skilled workers to replace more experienced, more educated, and thus more costly ones. Any estimated decline in wages would then be the result of turnover towards employees with less attractive observable attributes, and not necessarily due to market-wide changes in the competition for labor services. Given the de-

tails about workers' observed attributes in the data, I will estimate the effects of mergers on a measure of earnings that takes such attributes into account, thus obtaining an effect that is not driven by changes in the composition of attributes of workers. Similar to Arnold (2022), I estimate local labor market-level wages that control for workers' characteristics available in the data. The parameter I look for is $\theta_{m,t}$ in

$$w_{i,m,t} = \theta_{m,t} + \beta_t X_{i,t} + u_{i,m,t} \tag{2.6}$$

where $w_{i,m,t}$ is worker i's log annual earnings in market m and year t, and $X_{i,t}$ is their vector of observable attributes [9]. This model is estimated via OLS for every year in the data. This way, $\theta_{m,t}$ captures the annual market-level log wage net of trends in the workforce composition $X_{i,t}$[10].

2.3.4 Summary Statistics

Table 2.1 presents summary statistics from the pool of eventually treated local labor markets, where all means are computed based on the year prior to the first M&A event in the market. Despite the seemingly large number of firms per local

[9] $X_{i,t}$ contains dummies for race, college and high school diplomas, sex, and a quadratic binomial on age.

[10] Later on, in Section 2.4, when I split the analysis between spillover and merged firms, their respective earnings measures are obtained from a re-estimation of Equation 2.6 for each group separately.

Table 2.1: Summary Statistics on Local Labor Markets

	Mean	Std. Dev.
Annual Earnings (in 2010 BRL)	3,905.40	1,717.94
Workers	1,727.10	2,618.95
Workers in Merging Firms (%)	39.66	32.89
Firms	62.91	142.59
HHI	2,847.50	2,549.25
Market-Year Observations	10,348	

Notes: These are summary statistics of local labor market characteristics. Local labor markets are defined by pairs of commuting zone and 3-digit industry sector codes. Annual earnings were adjusted by inflation, with base year in 2010. All means are computed one year before the M&A event.

labor market (almost 63 on average), the HHI score shows that employment is unevenly distributed among employers; 2,847.50 is above the threshold of 2,500 used in DOF-FTC Horizontal Merger Guidelines to consider a product market highly concentrated. This score is equivalent to a market with 3.51 equally sized employers and is above the overall measured concentration in the U.S. (around 1,500 (Rinz, 2020)), but below the one found looking only at manufacturing (3,380 (Benmelech et al., 2022)).

2.4 Results

In this section, I present the event studies as well as overall estimates of the treatment effects of M&As on workers' earnings, log employment, and concentration measured by HHI. I also report estimates from among only the firms that constitute the merger deal, and all other firms in the same labor market. Finally, I ex-

plore the role of employment concentration on the treatment effects by comparing the estimates from events with predicted zero impact on HHI - the out-of-market mergers, and those on the top of the distribution of concentration increases – the high-impact mergers.

2.4.1 Market-level Effects

Figure 2.2 shows the event study estimates on earnings, employment, and concentration measured by HHI. In years before $t = 0$, the date of the M&A event, the outcome variables trend similarly among treated and not-yet-treated labor markets, which supports the plausibility of the parallel trends assumption. In the years post-M&A, I cannot rule out that market-level earnings of workers in treated local labor markets have changed on par with the earnings of workers in not-yet-treated units, despite the negative point estimates. In the next section, I estimate separate effects for the treated firms, i.e., those firms that took part in the M&A, and all other firms in the same market. This distinction will shed light on the source of negative earnings estimates and possible market interactions responsible for generating these results.

Notwithstanding the stability in earnings differences, the effect of M&As is negative on employment, measured in log, corresponding to -0.1143 ($SE = 0.0249$) one year after the event. The remaining lag estimates indicate that the level of employment does not recoup its counterfactual trajectory. Overall, in the

five years post-treatment, there is a 6.78 percent relative decline in employment on treated markets.[11] The fall in employment takes place even when the estimates show a close to null change in employment concentration, as can be seen in Panel 2.2c. M&As are expected to increase local concentration mechanically, but the dynamic estimates show that the workers in treated markets are not subject to a significantly more concentrated labor market after the M&A.

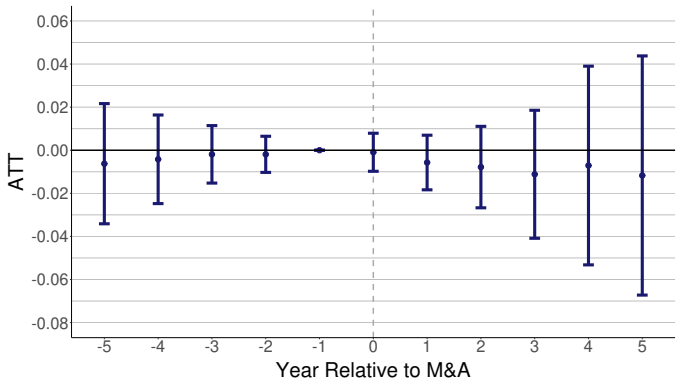
2.4.2 Effects from Within Merged Firms and Spillover Dynamics

A priori, there is no reason to expect that a merger event will equally impact all employers in a labor market, or that employment and wage adjustments will be similar across all workers. The merging firms might experience changes intrinsical to the merger that might not propagate to the rest of the labor market as a whole. Therefore, I will estimate separate earnings and employment effects for firms that participate in the merger and firms that do not participate in the merger. I will call firms that do not participate in the merger spillover firms. The observation of the two separate outcomes, one for within-merged and another for spillover firms, sheds light on what can be the market-wide effect of the M&As and what is related to unobserved changes mergers enact in their entities. It is possible to expect that the new ownership leads to changes in managerial practices, or even in worker productivity, both of which can affect earnings and employment within

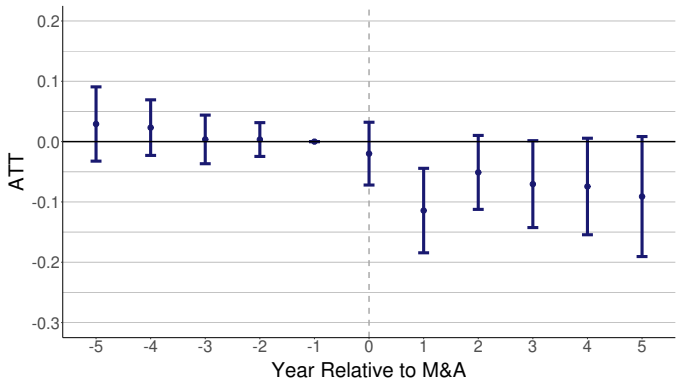
[11] This percentage effect is obtained from exponentiating the overall measured effect, $1 - \exp(-0.0702)$.

Figure 2.2: ATT of Earnings, Employment, and HHI

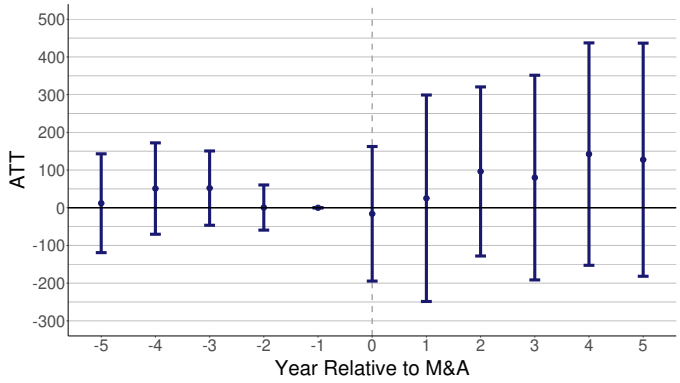
(a) Workers Earnings



(b) Log Employment



(c) Employment HHI



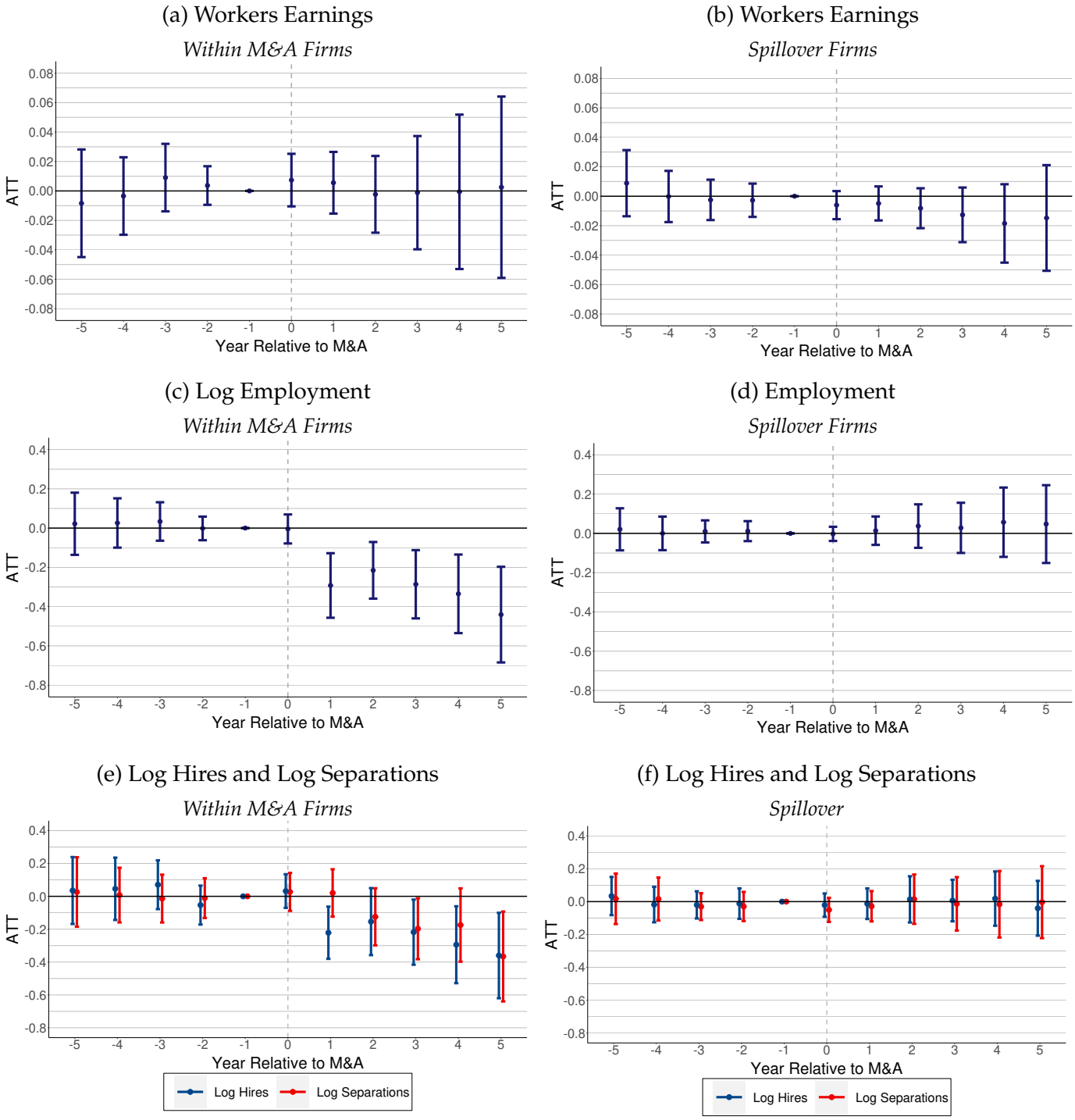
Note: 95% confidence intervals with standard errors clustered at the market level. The year $t = 0$ marks the first M&A in a local labor market, defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative employment size in each year.

the newly merged firm, but not necessarily those of competitors in the same labor market.

Figure 2.3 shows the event study estimates of earnings and employment effects of the merged and spillover firms separately. Panel 2.3a shows that earnings in merged firms do not diverge from the earnings in not-yet-merged firms in other

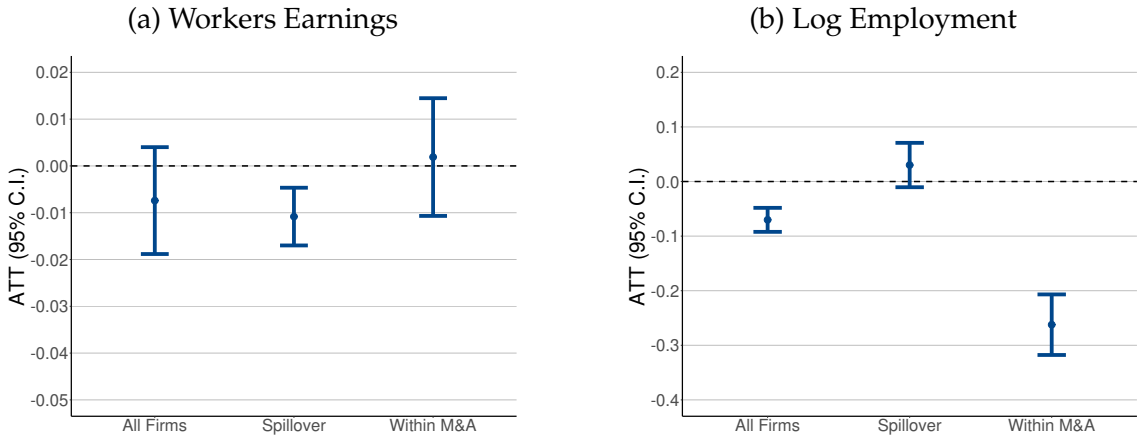
labor markets, with lag point estimates close to zero, especially after the second year of treatment exposure. Differently, earnings in spillover firms decline after the merger. The 95 percent confidence intervals contain zero for the separate lag estimates, but the overall effect five years after treatment represents a 1.07 percent decline in wages (Figure 2.4). The employment estimates show a different dynamic. Compared to their baseline difference from other merger participants in not-yet-treated markets, employment in merged firms is significantly lower. The estimate of one year of exposure to the M&A is -0.2923 (SE=0.0052), and, for all five years after the event, I estimate a 23.07 percent decrease in employment in merged firms (Figure 2.4). At the same time, I fail to reject zero employment effects in spillover firms – although the overall effect is positive but imprecisely estimated (Figure 2.4). Taken together, these findings show that the negative employment effects shown in Figure 2.2 were carried out primarily by the firms participating in the merger, while the negative wage point estimates originated from a decline in earnings in spillover firms, with a positive but, not significant, increase in employment of spillover firms. The bottom two panels in Figure 2.3 show the estimates for hiring and separations of each type of firm. Panel 2.3e shows that the negative employment in merged firms is adjusted via an abrupt decline in hires, while separations only start declining after the first year of exposure to treatment. Panel 2.3f reinforces the findings for employment in spillover firms, showing that both hires and separations remain similar to their *pre*-merger levels.

Figure 2.3: Effects from Within and Spillover Firms



Note: 95% confidence intervals with standard errors clustered at the market level. The year $t = 0$ marks the first tradable M&A in a local labor market, defined by pairs of commuting zone and 3-digit level industry code. Tradable M&As are mergers and acquisitions of tradable industry sectors. Spillover effects are obtained at the local labor market level by excluding the workers employed in the merged or acquired firms. Incumbents are employees with at least 30 months of tenure in their current job by the end of each year. New hires are workers with less than 12 months of tenure by Dec. 31 of each year. Markets are weighted by their relative size in each year.

Figure 2.4: Overall Effects



Note: The graph shows the ATT for all five years after treatment. 95% confidence intervals with standard errors clustered at the market level. Markets are defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative size each year.

2.4.3 Effects from M&As with Different Concentration Changes

There are many channels through which mergers can induce changes in wages and employment levels in local labor markets. On the side of merged firms themselves, one can think that new management or changes to worker productivity might be the cause of the observed outcomes. At a market level, on the other hand, changes in the competition for labor services might be the driver of the decline in wages and employment, especially if mergers foster an anticompetitive behavior from employers. Recent studies that look at wage and employment effects of mergers have found that labor market concentration is an important mediator of the relationship between employer consolidation and earnings declines. Using U.S. hospital mergers, Prager and Schmitt (2021) find that only consolidations in the top quartile of local concentration shocks induce negative wage effects

on health sector workers. In a more general setting, using a similar definition of the local labor market as the one in this paper, Arnold (2022) finds that mergers below the 80th percentile of predicted concentration change do not have a significant impact on workers' earnings. The fact that larger negative wage effects are found only when mergers enact larger shifts in concentration is consistent with the oligopsony theory à la Cournot of competition in labor markets (Azar et al., 2019; Arnold, 2022). In such models, the first-order condition of each firm's profit maximization problem can be combined to obtain a negative relationship between market wages and the employment concentration measured by the HHI. That is the main reason why wage declines associated with higher levels of concentration are viewed as supportive evidence of anticompetitive behavior in labor markets (Card, 2022).

My analysis so far has not distinguished the M&A events by their predicted impact on market HHI scores, and their effect on employment concentration was measured to be positive, although imprecise, point estimates (Panel 2.2c). In this section, I report the effects of two different types of mergers, namely *out-of-market* and *high-impact* M&As. Out-of-market mergers are consolidation events where the merging firms were not simultaneously active in the same labor market before the event date. In terms of employment concentration, these are the events with no predicted change in HHI, where the predicted change is the difference between a simulated measure of HHI where the two or more merging firms are considered as one single employer, and the employment HHI actually observed one year before the merger. In case the merging parties operate in the same labor

market, their merger has a positive predicted change in HHI, as the sum of their employment shares is greater than any of their individual shares in the year before their merger, making the local simulated HHI greater than the one observed in the data. I label as *high-mpact* the mergers at the 85th percentile, or above that, of the distribution of predicted change in HHI, [12]. The relevance of out-of-market M&As lies in the fact that they do not mechanically induce any changes in concentration,[13] and thus, their effects are expected to be unrelated to market-wide anticompetitive behavior resulting from any increases in concentration. Diversely, effects from high-impact M&As can be indicative of anticompetitive behavior related to the increase in concentration that they elicit. Table 2.2 shows that, out of all mergers and acquisitions in the sample of tradable industries, the median predicted change in HHI is 0, while the 85th percentile has a predicted change of 5.53 points – an event analogous to the merger of two equally sized employers with a 1.67% share of employment each. In Table 2.3, I report summary statistics of the markets used in the estimation of the effects of the two types of events. The markets in the out-of-market pool have similar earnings to those in the high-impact's, a little over 4,000 BRL, and they also depart from a close average number of firms, 81.16 and 79.83, respectively.

[12]One year post-merger, events at the 80th percentile and above cause a change in HHI of 253.48 points (SE=65.61) equivalent to the combination of two equally sized employers with an 11.25% employment share each. At the 85th percentile, this effect rises to 430.70 points (SE=112.35), analogous to the merger of two 14.67%-share partners, making it thus more suitable to test the effects of a change in HHI induced by the merger against out-of-market M&As. Moving the threshold higher up in the predicted change in HHI distribution, however, reduces the number of contributing markets from which to infer the effects, which compromises the statistical power and feasibility of the analysis.

[13]As it soon will be shown, out-of-market M&As do not induce changes in distribution post-treatment either.

Table 2.2: Distribution of the Predicted change in HHI

Percentile	**50th**	**75th**	**80th**	**85th**	**90th**	**95th**
Predicted Change in HHI	0.00	0.10	0.87	5.53	42.55	333.11

Notes: The predicted change in HHI is the difference between the simulated HHI and the observed HHI one year before the M&A takes place. The simulated HHI is obtained by considering the merging firms as one single employer in the year before the merger, effectively summing their employment shares in that year.

Table 2.3: Summary Statistics of Markets in Out-of-Market and High-Impact Events

	Out-of-Market		High-Impact	
	Mean	Std. Dev.	Mean	Std. Dev.
Market Earnings (2010 BRL)	4,066.80	1,917.33	4,024.70	1,477.92
Workers	2,060.02	3,055.59	2,342.88	3,058.78
Workers in Merging Firms (%)	38.11	40.50	46.13	32.33
Firms	81.16	213.94	79.83	169.01
HHI	2,836.08	2,604.09	2,242.50	2,084.95
Market-Year Observations	9,581		2,886	

Notes: Summary statistics of local labor market characteristics. Local labor markets are defined by pairs of commuting zone and 3-digit industry sector codes. Annual earnings were adjusted by inflation, with the base year in 2010. Out-of-Market M&As are the ones with no predicted increase in HHI before the consolidation date. High-Impact M&As are the ones in the top 15% of predicted change in HHI. All means are computed one year before the M&A event.

In Figure 2.5, the first two panels confirm that out-of-market M&As and high-impact M&As have distinctive post-treatment concentration dynamics. As expected, high-impact M&As increase local employment concentration – the two-year treatment exposure point estimate shows an increase in HHI of 522.48 points (SE=118.64), an increase analogous to one generated by a merger of two equally sized employers with a 16.16% share of local employment each. Post-treatment, I cannot reject the null effects of out-of-market M&As on local market concentration. In Panels 2.5c and 2.5d, I observe similar point estimates from both types of mergers, and in both contexts, that of merged firms and spillovers. Within merged

firms, earnings seem to decline, especially after the second year of exposure to treatment, while a downward trajectory is noticeable from the start in spillover firms. As for the employment outcome, the event studies in panels 2.5e and 2.5f indicate a different pattern of effects between the two types of mergers, with negative estimates in the case of high-impact M&As both within merged firms and spillovers.[14] For out-of-market M&As, while treatment effects are negative within the merged firms, they are positively estimated among spillover employers.

The earnings and employment effects of the two types of consolidation events are summarized in Figure 2.6. The overall estimates for the five-year post-treatment show a similarity in the magnitude of earnings decline within M&A firms for both out-of-market and high-impact mergers, -0.0083 (SE=0.0084) and -0.0095 (SE=0.0108), respectively. In the case of spillover firms, the estimates are -0.0108 (SE=0.0051) for out-of-market mergers and -0.0117 (SE=0.011) for high-impact. This finding is at odds with studies of mergers in the context of the U.S. labor market in two aspects. First, both in the specific case of hospital consolidation (Prager and Schmitt, 2021), and in the more general context of multiple industries (Arnold, 2022), mergers with little to no change in concentration do not have a significant impact on earnings. Here, I find a significant earnings decline after out-of-market mergers in spillover firms and, although less precisely estimated, within merged firms too. Even in the absence of changes to local concentration,

[14]The lead estimates of out-of-market M&A employment effects in Panel 2.5e show a potential positive linear trend in the employment of merged firms relative to their not-yet-treated counterparts in other markets. In such cases, the negative post-treatment estimates can be interpreted as a reverse in their employment growth trajectory. Either way, the qualitative conclusion remains the same and is consistent with the case of all mergers in Section 2.4.1, employment in merged firms would have been higher if not for the merger.

mergers impart a significant decrease in earnings. Second, looking at the case of spillover firms, the effects from the two types of events are similar in magnitude, indicating a decline of 1.1% in earnings. This similarity contradicts the view that larger increases in concentration are followed by larger wage declines, both theoretically and empirically. Assuming no changes to labor supply elasticity and productivity in the first five years after the merger, a likely scenario for the case of firms not involved in the merger, the theory of oligopsony à la Cournot would have predicted a more negative wage effect for the case of high-impact events. This finding also shows that, at least in the context of Brazilian labor markets, it should not be assumed that mergers only affect wages and employment, via the concentration channel only, an assumption that has been used before in instrumental variable estimations of the relationship between wages and employment concentration (Benmelech et al., 2022; Arnold, 2022). Without distinguishing between spillover and merged firms, the *All Firms* column in Panel 2.6a confirms that out-of-market mergers have a significant negative effect on earnings, estimated at -0.0143 (SE=0.0045).

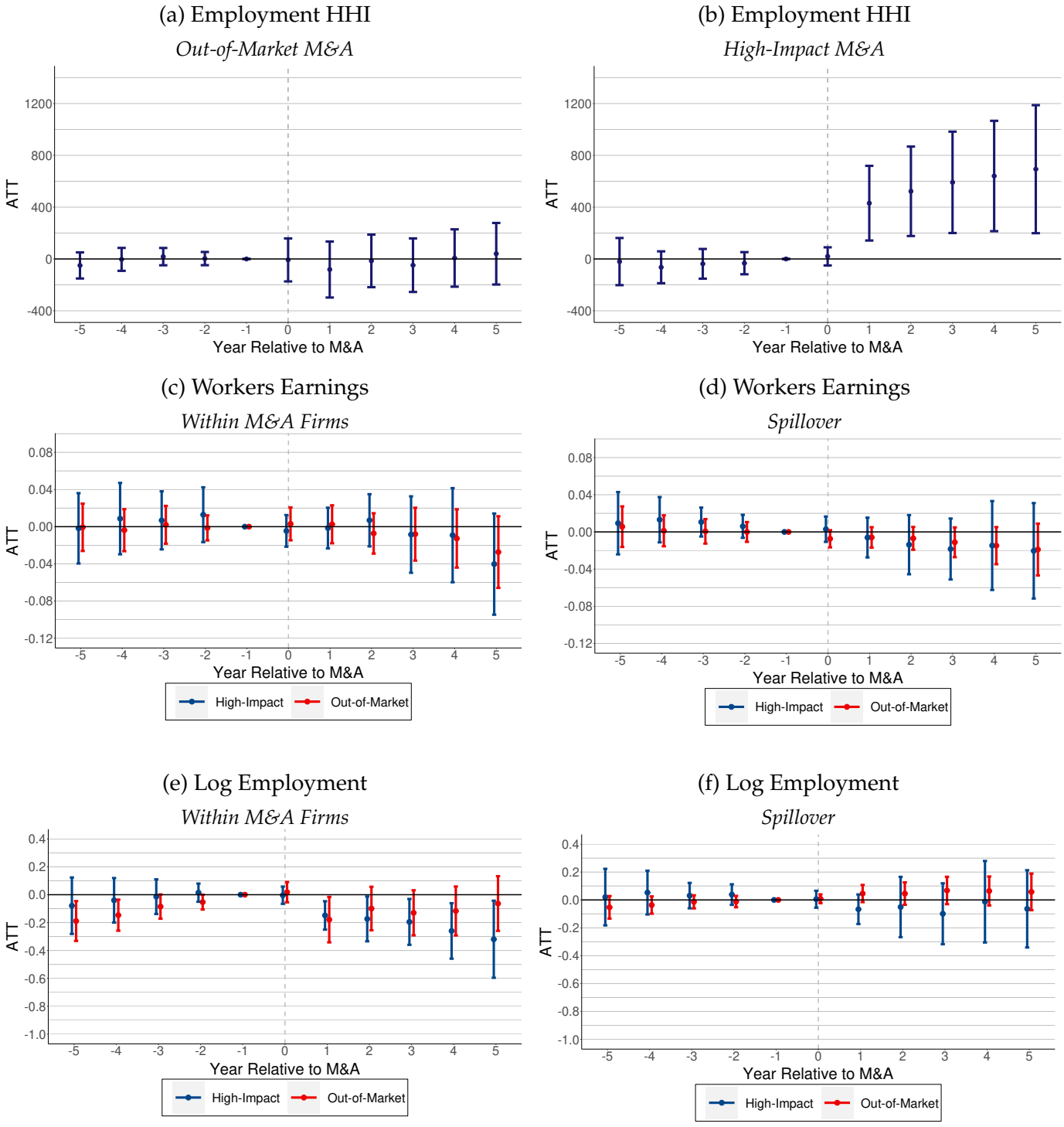
Panel 2.6b shows the overall employment effects. Here, the out-of-market mergers have a similar result to the one found in the case of all mergers presented in Section 2.4.1 - while a negative employment adjustment is observed in merged firms, spillover firms grow after the merger of their competitors, although at a rate that does not compensate for the separations in merged firms, as the *All Firms* column in Panel 2.6b indicates. For the case of high-impact mergers, the employment effect is negative across all firms, notwithstanding the estimates

are less precisely estimated in spillover firms. In principle, this is an expected result. Contrary to out-of-market events, high-impact mergers necessarily reduce the number of employers in the market, canceling to some extent the possibility of workers reallocating within the same labor market. Figure 2.7 shows that the higher concentration observed after high-impact mergers may not only stem from a change in the distribution of workers among big and small firms but also from the reduction in the number of employers altogether. The number of employers is close to 9.05% lower in markets that witness a high-impact merger two years after the event, while a null effect cannot be rejected in the case of out-of-market mergers. Another reason why employment may decline more sharply in high-impact mergers is that the merging entities might have more redundancy among their workers once under the same ownership and management, while the same level of overlap is likely not achievable in out-of-market consolidations.

2.5 Robustness to Anticipation

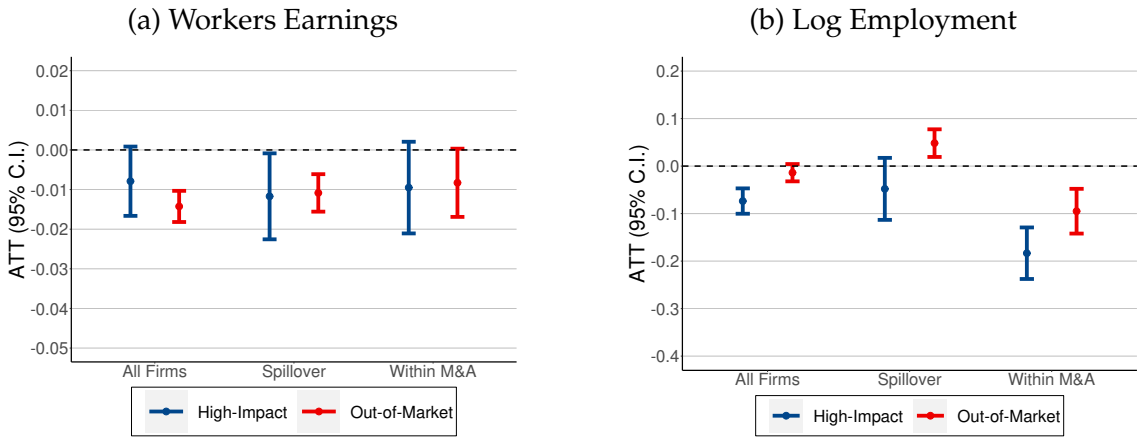
Mergers and acquisitions may take a long time to conclude, and both workers and firms can respond to the news of the event before its official date is reported in administrative records. Thus, it is important to check the robustness of the results to the possibility of treatment anticipation. Effectively, this means that some not-yet-treated markets that contribute to the control pool under the assumption of no treatment anticipation are not appropriate counterfactuals anymore, espe-

Figure 2.5: Out-of-Market and High-Impact M&As



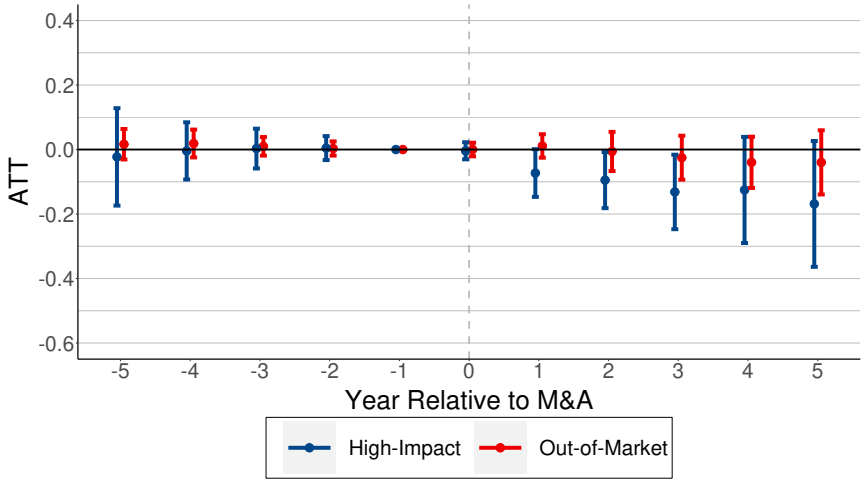
Note: 95% confidence intervals with standard errors clustered at the market level. The year $t = 0$ marks the first tradable M&A in a local labor market, defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative size in each year.

Figure 2.6: Overall Effects by Type of M&A



Note: The graph shows the ATT for all five years after treatment. 95% confidence intervals with standard errors clustered at the market level. Markets are defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative size each year. Out-of-market M&As are mergers with no predicted change in market HHI, while high-impact are the ones in the top 15% of the predicted change in HHI distribution.

Figure 2.7: Employers



Note: ATT of the M&As on the log number of employers in a local labor market. 95% confidence intervals with standard errors clustered at the market level. Markets are defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative size each year.

cially if firms engage in pre-merger adjustments ahead of the deal taking place. To take this possibility into account, I modify the Limited Treatment Anticipation Assumption in Section 2.3.2 by changing the value of the parameter ζ from 0 to 1. This implies that for markets treated in year g, their average treatment effect in any year t will be based upon the not-yet-treated markets by year $t + 2$, and not $t + 1$ as before.[15]

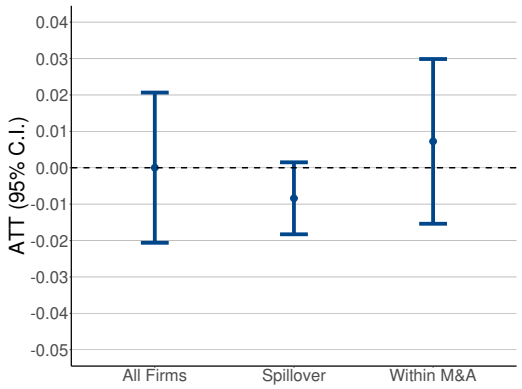
I present the overall 5-year ATT under the one-year treatment anticipation assumption in Figures 2.8 and 2.9. When pooling all mergers together, the lesson from the treatment anticipation case is similar to the findings from the no treatment anticipation (Figure 2.4) - (i) workers earnings are lower in spillover firms, although not as precisely estimated as before, at the same time that these firms grow in size, and (ii) merging firms primarily drive the negative employment adjustment observed in the whole market, while the null effect on their workers earnings cannot be rejected. The comparison between out-of-market and high-impact events with treatment anticipation is presented in Figure 2.9. Here, I do not find conclusive evidence that refutes the previous finding of similar earnings effects between the two types of events among spillover firms, which shows that even under the treatment anticipation assumption, the increase in concentration does not generate distinguishable earnings effects outside the merging firms. Within merged firms, the seemingly more negative wage and employment effects

[15]It is worth noticing that this adjustment is different from simply moving the normalization period in two-way fixed effects specifications from the more commonly reported lead $t - 1$ to $t - 2$, in view of the fact that it effectively changes the pool of units in the control group (Callaway and Sant'Anna, 2021).

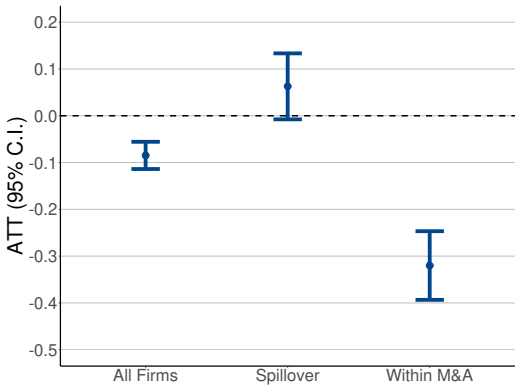
of high-impact events may originate from a higher degree of job title overlap between the merging firms when they already belong to the same labor market.

Figure 2.8: Overall ATT with Treatment Anticipation of One Year

(a) Workers Earnings



(b) Log Employment

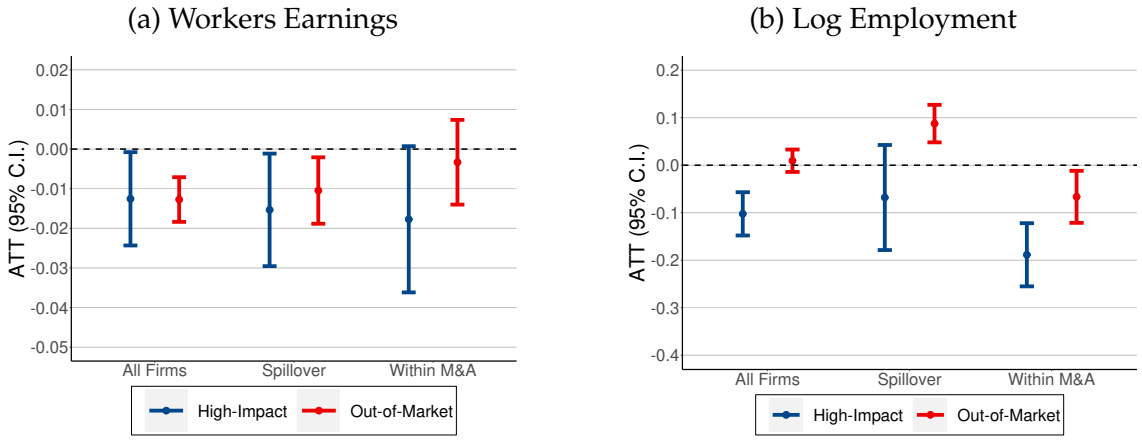


Note: The graph shows the ATT for all five years after treatment, under the assumption of treatment anticipation of one year. 95% confidence intervals with standard errors clustered at the market level. Pairs of commuting zone and 3-digit level industry code define markets. Markets are weighted by their relative size each year.

2.6 Discussion of Results

To summarize, mergers and acquisitions in the context of the Brazilian labor markets are shown to have significant negative employment effects at the local labor market level. The post-treatment dynamics also show increases in concentration and declines in worker earnings, although not significant. By splitting the sample between the firms that participate in the merger and all other firms in the same market, I show that the two types of firms have diverse responses on their employment and earnings margins. The negative employment effects are found primarily

Figure 2.9: ATT with Treatment Anticipation of One Year by Type of Merger



Note: The graph shows the ATT for all five years after treatment, under the assumption of treatment anticipation of one year. 95% confidence intervals with standard errors clustered at the market level. Markets are defined by pairs of commuting zone and 3-digit level industry code. Markets are weighted by their relative size each year. Out-of-market M&As are mergers with no predicited change in market HHI, while high-impact are the ones in the top 15% of the predicted change in HHI distribution.

within the merged firms, while spillover firms show a tendency to increase in the years after the event, but their increase is not large enough to offset the reduction in size of merging firms. When it comes to earnings, I cannot reject the null effect hypothesis from the merged firms' sample, but earnings are significantly lower in spillover firms. Most mergers and acquisitions have little to no impact on concentration. It is only in the top 15% of the distribution of *a priori* increases to HHI that I find noticeable employment concentration changes. The comparison of out-of-market and high-impact mergers reveals seemingly indistinguishable earnings effects in spillover and merging firms. The out-of-market employment effects follow the overall pattern found before, merging firms get smaller, and spillover firms grow in size. These findings are robust to the possibility of treatment anticipation, and are likely not related to changes in the composition of the labor force

(given the construction of the earnings variable) and changes in product market power (given the restriction to tradable industry sectors only).

The concentration channel connecting mergers and negative wage effects found in Prager and Schmitt (2021) and Arnold (2022) seems to be thus absent in the context of Brazilian labor markets. In addition, I find negative wage effects that affect other firms in the labor market, even in the case of merger activity not followed by increases in concentration. I do not empirically find a confirmation of the connection between increases in concentration and sharper wage declines from oligopsony models of the labor market (Boal and Ransom, 1997; Azar et al., 2019). But if not changes in concentration, what could be driving the observed decrease in earnings of firms unrelated to the M&As and the market-wide decline in employment?

2.6.1 M&A's Synergies, Managerial Practices, and Within-Market Dynamics

A way to rationalize the decreases in workers' earnings and employment, even in the case of mergers that do not affect local concentration, is to admit the possibility of efficiencies created by employer consolidation. Merger proponents argue that cost-saving measures can be taken once the merging parties operate under the same ownership. While the economics profession has been skeptical of efficiency

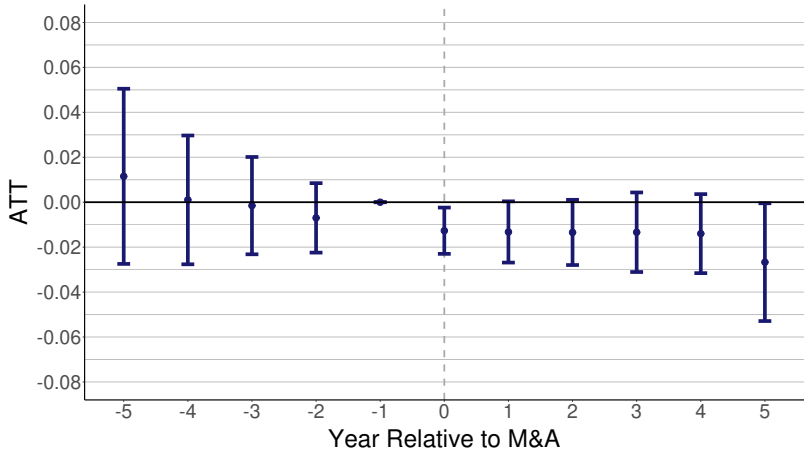
claims made by merger proponents;[16] the present case shows that merging firms do engage in a significant reduction of personnel, while other employers in the labor market show a tendency to grow after the merger. On the earnings margin, the results showed that workers in the merging firms do not face a significant decline in earnings, while the opposite happens with spillover firms' workers. This contrast suggests that the adjustment towards lower employment generated by potential efficiency gains in merging firms increases the labor supply to all the other firms in the same market. Assuming a stable demand curve for labor in spillover firms, the increased supply of workers is accommodated at a higher equilibrium level of employment and lower earnings in these firms. Indeed, I estimate negative earnings effects among new hires in spillover firms after an out-of-market merger (Figure 2.10), where the fiver year post-treatment average effect is -0.0156 (SE=0.0051).

The question of why cost-saving measures in the form of overhead reduction are more pervasive in Brazilian merger activity if compared to the context of the U.S. labor market remains.[17] One possibility is that changes in ownership and management can collect higher gains from efficiency in emerging economies due to inadequate management practices in target firms. Bloom et al. (2010) compares

[16]For a discussion on the credibility of such claims in the U.S. context, see Fisher (1987) and Farrell and Shapiro (2000). The possibility of merger-related efficiency gains is also explicitly acknowledged by regulators. See, for example, the DOJ-FTC Horizontal Merger Guidelines, Section 4.

[17]Due to a pre-trend in their employment event study, Prager and Schmitt (2021) refrain from making an assertive conclusion about the employment effects of hospital mergers. At the same time, Arnold (2022) finds negative employment effects in M&A establishments that range from 5% to 10% on average depending on the predicted change in concentration, I find a sharper decrease of 23.07% in merging firms' size in the case of all mergers.

Figure 2.10: New Hires' Earnings in Spillover Firms After an Out-of-Market Merger



Note: Pairs of commuting zone and 3-digit industry code define local labor markets. New hires' earnings refer to the log wage adjusted from equation 2.6 estimated among the pool of new hires in spillover firms. New hires are workers with less than 12 months of tenure in the current job by December 31st of each year. Spillover firms are the employers in the labor market that do not participate in any M&A either as acquirers, targets, or merging partners. An out-of-market merger is a merger with zero predicted change in local concentration measured by the HHI.

firms' productivity in developing economies vis-à-vis their counterparts in richer countries. In 2005, the sales per employee in American firms were more than 3.2 times as large as that of Brazilian firms. While previous studies have elicited structural, economy-wide reasons for the productivity gap, such as developing countries lack of infrastructure, lower human capital, and regulation, Bloom conjectures that managerial practices can also play an important role. Compared to higher-income economies, middle and low-income countries, including Brazil, have a lower prevalence of management practices related to clear target setting, production monitoring, and proper pay incentives (Scur et al., 2021). To the extent

that changes in ownership in Brazil can allow merging firms to reap managerial gains and dismiss excess workers, this could partly explain the difference in employment effects between merging firms and other employers in the same labor market. Simultaneously, M&As in developed countries such as the U.S. might not collect the same cost-related efficiency gains given their superior management practices beforehand.

2.6.2 Informality and Contractual Dualism

The fact that merger effects in Brazilian labor markets contrast the prediction of the oligopsony model should not be readily taken as evidence of them being perfectly competitive. That is, even as employment concentration does not have the same role in explaining merger effects the same way it does in developed economies such as the U.S., it could still be the case that wages are below the marginal revenue product of labor. Independent of the number of employers, wage markdowns can be rationalized in models of search friction (Burdett and Mortensen, 1998), or with heterogeneous worker preferences (Card et al., 2017), for a couple of examples.

Alternatively, characteristics specific to developing economies can give rise to wage markdowns, mainly their lack of state capacity to perfectly enforce minimum wage mandates and other labor regulations (Chau et al., 2022). As a consequence, a fixture of developing economies is the coexistence of formal and infor-

mal sectors; in Brazil, between 2002 and 2015, an average of 33.6% of all jobs were informal (Engbom et al., 2022). For reasons unrelated to employment concentration, Basu et al. (2015) show that wage markdowns are endogenously obtained when employers cannot credibly signal the upholding of contracts previously agreed upon with workers. For wages close or equal to the worker's marginal productivity, it is profitable for employers to renege on their promises under imperfect state surveillance. The resulting holdup can only be avoided by the posting of self-enforcing contracts, with limited viable wage options. The wedge between worker productivity and their take-home payment is thus a consequence of weak contract enforcement. Job creation in the formal sector, although more costly due to higher enforcement of contracts, signals workers that wage promises will be fulfilled, and therefore, under certain conditions, a wider set of viable wages prevails in the formal sector. The disparity of wages in the two sectors between workers with equivalent productivity is called "contractual dualism" by the authors.

Indeed, for similar workers, a job in the Brazilian formal sector is associated with a 12.3% wage premium over the informal sector (Engbom et al., 2022).[18] The model in Basu et al. (2015) and the findings in this paper point to an investigation of employer market power in developing countries that goes beyond the concentration paradigm of oligopsony models and includes the institutional aspects of these economies.

[18]Workers in the informal sector are also less likely to be white, to be employed in a large firm, to work in a white collar job, and to have completed high-school (Engbom et al., 2022).

2.7 Conclusion

What are the effects of merger and acquisition activity in the labor markets of a middle-income country? I attempt to answer this question by exploring linked employer-employee administrative records from Brazil to identify merger events, locate them in labor markets defined by pairs of industry and commuting zone, and, using an event study design, estimate their impact on workers' earnings, employment, and local concentration measured by the HHI. The worker-flow identification of merger events allows me to distinguish the size and worker compensation changes, both in merging firms and all the other employers doing business in the same labor market. Overall, mergers have clear negative impacts on labor market size, while null effects on earnings and local concentration cannot be rejected. I find that the market's negative employment adjustment is exclusively concentrated in merging firms, while other employers in the same market experience a positive, although not significant, size effect, while their worker's earnings show a subtle modest trend.

The apparent null effect of mergers in local concentration is explained by the fact that most M&As are of the out-of-market type, i.e., either the acquirer or the merging partner was not active in that same market before, and thus the predicted change in local HHI is zero. Only at the top 15% of the predicted change in HHI distribution do I find a noticeable local concentration impact of M&A events. Contrary to the previous literature findings, workers' earnings in spillover firms decline similarly, irrespective of the effect on concentration from the merger event.

The earnings decline in spillover firms can be rationalized by a positive shift of the labor supply curve in these firms, originating from a halt in hiring from the merging competitors. At the margin, I confirm that new hires in spillover firms earn relatively lower wages after an out-of-market event. By comparing the effects of M&As inducing different changes in local employment concentration, this paper also adds to the empirical investigation of oligopsony models that predict lower wages in concentrated labor markets.

The body of evidence showing the negative relationship between employment concentration and labor outcomes in developed economies has prompted the suggestion that antitrust authorities should use HHI benchmarks to flag mergers' potential anticompetitive impacts on labor markets (Naidu et al., 2018). In contrast, by explicitly comparing out-of-market and high-impact merger earnings' effects, I find that wage declines in spillover firms are similar in both cases. However, the finding that mergers with substantial increases to concentration are not followed by stronger wage reductions should not be taken as proof that labor markets in Brazil are hence perfectly competitive and that the local antitrust authority should not be cognizant of mergers' impacts on workers. The oligopsony theory is one way to rationalize wage markdowns. Still, models of job search friction, such as employer differentiation, and job ladder, can also generate firm-specific upward-sloping labor supply curves independent of employment concentration.[19] My result shows that *ad-hoc* thresholds of local concentration may not be

[19]See Card (2022) for comparison and historical perspective on models with employer's wage-setting power.

as informative about the competitiveness of developing economies' labor markets as they are in developed countries. Additionally, the existence of non-compete clauses standard in high-skilled occupations, and anti-poaching agreements documented among various U.S. franchisees are worth the attention of antitrust policy regardless of their connection to local market concentration (Balasubramanian et al., 2020; Krueger and Ashenfelter, 2022). The pervasiveness of such practices in the Brazilian context is still unexplored and undoubtedly deserves future research attention.

Chapter 3

The Public-Private Wage Gap in Brazil: New Evidence from Linked Employer-Employee Data

Brazil's expenditure on public sector personnel was close to 45% of its government's revenue in 2016, a share higher than in other 158 countries, including developed and emerging economies.[1] Such a significant public sector wage bill has its tradeoffs. Among Latin American economies, Brazil registers the lowest public investment as a share of GDP, 2%, compared with 3.5% in Argentina, 7% in Colombia, and 12% in Ecuador, all countries with lower ratios of public sector wages to GDP. Given its substantial fiscal size, how much of Brazil's public payroll can be credited to the public wage premium relative to the private sector? Or, in other words, when comparing two similar workers from each sector, how much more or less does the public sector employee earn compared to the one in the private sector?

The existing estimates of the public-private wage gap in Brazil vary widely, from -12% up to 65% depending on the data source and econometric method used, as summarized in Table 3.1. These studies recognize the importance of adjusting the observed differential with respect to workers' characteristics. Foguel et al. (2000) and Marconi (2004) do precisely that, by means of an OLS estimation that incorporates a worker's observable attributes and an indicator for participation in the public sector. In the two cases, the adjusted wage premium is smaller than the observed differential, confirming the attenuating role of workers' characteristics in explaining the gap. A similar approach is taken by Emilio et al. (2012), but with the advantage of repeated observations, although few, of a worker's earnings. This allowed them to address endogeneity selection by the inclusion of worker

[1] Adjusted by 2015 PPP, according to IMF staff calculations in Karpowicz and Soto (2018).

fixed effects, resulting in a much smaller adjusted wage premium (5% compared to 56% in the previous two studies).

The use of an indicator variable for public sector jobs in a Mincerian equation, with no interactions with other explanatory variables, however, imposes the assumption that compensation rules for private and public employees' observed attributes are the same in the two sectors. A way around this restriction is the well-known Oaxaca-Blinder decomposition (Oaxaca, 1973; Blinder, 1973). Estimates for the Brazilian case using this method indicate varying sizes to the wage structure component of the decomposition, from -12%, among the most educated workers of the public sector (Emilio et al., 2012), to 43.7% from voluntarily dismissed workers in a state-owned railroad company (Firpo and Gonzaga, 2001). Other methods used in the estimation of the Brazilian wage premium are a quantile regression approach - with estimates from 40% to 65% in Belluzzo et al. (2005), and the Juhn-Murphy-Pierce - based on Juhn et al. (1993), Souza and Medeiros (2013) estimate a premium ranging from 15% to 21%.

What is common to all studies cited so far is the use of household surveys as the data source, mainly Brazil's National Household Survey, the PNAD. PNAD covers an ample range of socioeconomic variables from its subjects on a yearly basis, including employment information, over the whole territory. More relevant to the public-private wage gap investigation, PNAD also covers informal labor relationships, which entails that its use to measure the wage premium of public sector workers includes informal jobs in the counterfactual group. In addition,

PNAD does not offer repeated observations of the same household or workers, limiting the researcher's ability to address endogenous selection with panel fixed effects. The other data source previously used is PME, a monthly household survey restricted to six metropolitan areas with, at most, two years of repeated observations for the same worker.

In this paper, I use 15 years of linked employer-employee administrative records covering the universe of formal jobs in the Brazilian territory. This dataset allows me to follow any worker's job history, and it contains the standard demographic variables used in Mincerain equations (sex, race, education, and age). While I do not observe informal labor arrangements, it is also true that all of the public sector jobs are formal. Also, in terms of characteristics and earnings, the formal employees in the private sector are closer to their public counterparts than informal workers, who tend to have lower earnings, educational attainment, and labor attachment overall (Corseuil et al., 2015).

I offer estimates of the *average* wage gap obtained from an Oaxaca-Blinder decomposition, and also of the wage gap *along the distribution* of earnings in the economy, with and without worker fixed effects. To the best of my knowledge, this is the first study looking at the Brazilian case that uses administrative records to estimate the public-private wage premium over the labor income distribution. In terms of method, the closest papers to mine are Hospido and Moral-Benito (2016) and Bargain et al. (2018), which studied the wage premium in the Spanish and French sectors, respectively.

Confirming previous analysis, I find that public sector workers have higher scores for observed attributes positively rewarded in the labor market. Compared to their private sector counterparts, public employees are older, have higher educational attainment, and have longer tenure in their job. Nonetheless, even in the absence of differential characteristics, the Oaxaca-Blinder decomposition points to a positive premium related to the public sector's specific wage structure, for all branches and levels of government, with the exception of municipal executive employees. The highest observed gap is among employees of judicial institutions of the federal government, who earn per hour, on average, 739.04% more than private sector workers - 193.22% due to average characteristics, and 545.82% associated with the federal judicial compensation schedule. Conversely, workers in executive municipal departments are penalized at 24.69% due to the wage structure of their employers. In terms of budget, the highest bill associated with the public sector's specific compensation rules is among workers of the federal executive branch, costing an average of 8.90 billion in 2010 BRL annually (or 8.64 billion in 2022 USD)[2].

For the estimation of the gap along the wage distribution, I find contrasting results depending on the adjustment for endogenous selection into the two sectors. Without worker fixed effects, the characteristics component of the gap is the highest among top earners, who also seem to be penalized by the public sector wage rules. For public workers in the bottom 10% of the earnings distribution,

[2]Using the cumulated CPI of 35% between 2010 and 2022, and PPP exchange rate of 1 USD = 1.39 BRL in 2010 (`https://www.bls.gov/data/inflation_calculator.htm` and `https://data.worldbank.org/indicator/PA.NUS.PPP`).

all the observed gap is associated with their own characteristics relative, with no difference in their compensation scheme relative to private sector workers in the same earnings quantile. The inclusion of worker fixed effects, contrarily, changes this conclusion. Net of workers' time-invariant unobserved factors, the intrinsic wage premium due to public sector compensation rules is constant at around 15% across all deciles of the earnings distribution, including among top earners. This means that in the case of a fiscal adjustment that requires wage freezes in the government budget, a horizontal wage stagnation can be applied, given that the public sector premium is not concentrated in any specific segment of the earnings distribution.

The remainder of the paper is organized as follows. In Section 3.1, I present the dataset used in the analysis, summary statistics for workers in each sector, and Mincer estimates using an indicator variable for public jobs. The estimates from the Oaxaca-Blinder decomposition for all public sector workers, and separate groups according to government branch and level are shown in 3.2. In Section 3.3, I outline the approach used for the decomposition by quantiles of the earnings distribution, the method for dealing with endogenous selection among the sectors, and the results. Section 3.4 discusses the findings and concludes the paper.

Table 3.1: Estimates of the Brazilian Public-Private Wage Gap

Study	Data Source	Years	Data Remark	Method	Estimate
Foguel et al. (2000)	PNAD[a]	1995	Includes Informal Sector Cross sectional	OLS, Mincer Equation w/ Dummy for Public Sector	70% (gross),[b] from 31% to 55% (adjusted)[c]
Firpo and Gonzaga (2001)	PNAD REDE[d]	1995 1997	Voluntary Dismissal Program at State-Owned Railroad	Oaxaca-Blinder	21% (gross), 43.7% (coefficients), -22% (characteristics)
Marconi (2004)	PNAD	1993 1996 1999	Includes Informal Sector Cross sectional	OLS, Mincer Equation w/ Dummy for Public Sector	203.22% (gross),[e] 56% (adjusted)[f]
Belluzzo et al. (2005)	PNAD	2001	Includes Informal Sector Cross sectional	Quantile Regression[g]	From 40% to 65%[h]
Braga et al. (2009)	PNAD	2005	Includes Informal Sector Cross sectional	Oaxaca-Blinder	24% (adjusted) From -12% to 42% (coefficients)[i]
Emilio et al. (2012)	PME[j]	2002 2004	Includes Informal Sector 6 Metropolitan Areas	Pooled OLS w/ Worker FEs and Dummy for Pub. Sec.	Up to 5%
Souza and Medeiros (2013)	PNAD	2009	Includes Informal Sector Cross sectional	Juhn-Murphy-Pierce (JMP)[k]	69.42% (gross), JMP from 15% to 21%[l]

Note: This table summarizes the estimates from the literature on the public-private wage gap in Brazil.
[a] PNAD is the main household survey in Brazil. It includes both formal and informal workers and has annual frequency. [b] Federal branch only.
[c] Depending on metropolitan region. [d] It covers workers who voluntarily terminated their jobs in privatized state-owned companies.
[e] Federal branch excluding the military. [f] In year 1999. [g] Using Koenker and Bassett (1978) for the quantile estimation, and Machado and Mata (2005) for the unconditional wage distribution. [h] Premium decreases from bottom to top earners in the distribution.
[i] Structural premium is higher among the least educated, and lowest among workers with 17 or more years of formal education.
[j] PME is a monthly survey on [k] From Juhn et al. (1993). [l] Varying with the correction of endogenous selection: (i) no correction, (ii) inverse Mills ratio, and (iii) a bivariate probit - with participation/non-participation in the labor market, and public/private sector.

3.1 Data

The data in this paper consists of linked employer-employee records from Brazil's Ministry of Labor between 2003 and 2017. The dataset, officially called *Relação Anual de Informações Socias*, or simply RAIS, covers the universe of formal employment relationships. By law, employers must submit a yearly report to the Ministry, and non-compliance is subject to fines.[3] In RAIS, each worker and establishment

[3] https://www.planalto.gov.br/ccivil_03/_ato2019-2022/2021/decreto/d10854.htm

have their unique identifier. Each observation contains data on monthly average earnings, occupation, industry sector, municipality of the establishment, date of worker's admission and termination, cause of termination - retirement, death, just cause, etc - and tenure of the worker in the current job. For workers' attributes, it is possible to observe race/color, sex, age, educational attainment, nationality, and disability status. The raw data files contain around 940 million observations.

3.1.1 Definition of Public Sector Workers and Sample Selection

To distinguish public and private sector workers, I use the information on the legal regime governing an employer's activity, a field available in RAIS.[4] The legal regime codes allow for the identification of public sector employers, their branches of government (executive, legislative, judicial), and their levels (federal, state, municipal). I classify workers linked with these employers as public sector workers; all other workers fall into the private sector definition. For workers with multiple jobs, I keep only the record with the highest mean monthly earnings by December 31st of each year. As customary in the literature, I narrowed the analysis to workers aged between 25 and 54. Further details on the sample construction are available in B.1.

Table 3.2 reports descriptive statistics for public and private sector workers. The majority of workers are in the private sector, around 50 million out of 60

[4]In RAIS, this field is called *Natureza Jurídica*.

Table 3.2: Summary Statistics of Private and Public Sector Workers

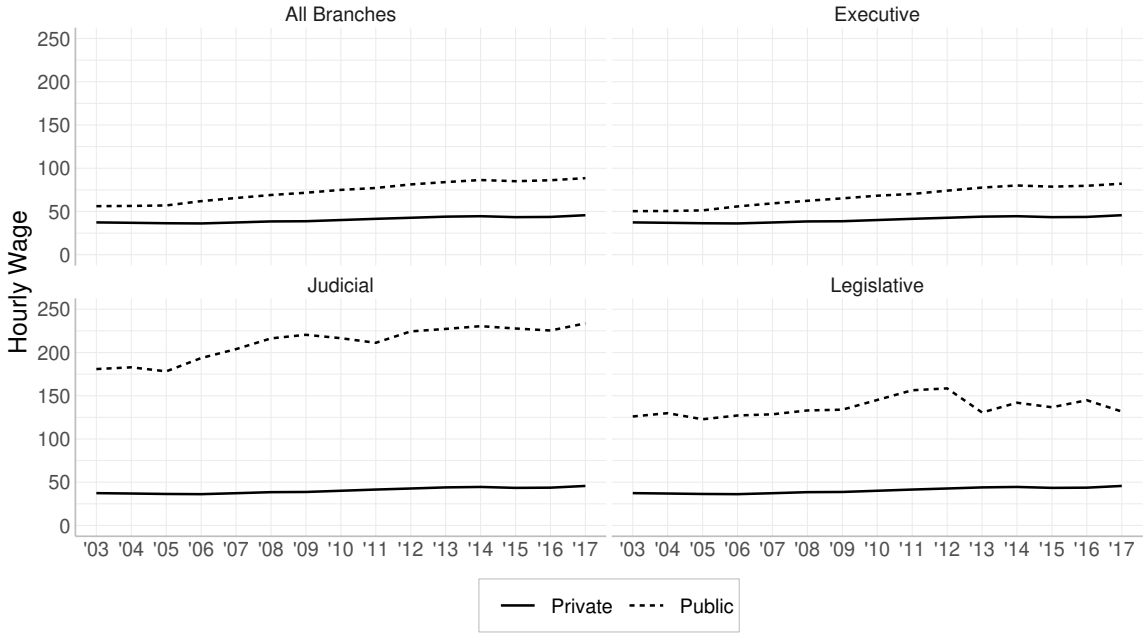
	Private		Public	
	Mean	Std. Dev.	Mean	Std. Dev.
Monthly Wage	1,794.74	2,499.03	2,684.33	3,389.36
Hourly Wage	41.00	95.08	72.81	103.04
Weekly Hours	42.48	4.75	36.19	7.47
Tenure	3.90	4.94	11.66	8.68
Female	38.05	48.55	59.23	49.14
Nonwhite	61.98	48.54	62.18	48.49
Age	36.33	7.96	41.33	7.58
High-skilled	13.53	34.20	38.72	48.71
Middle-skilled	55.49	49.70	41.94	49.35
Low-skilled	30.98	46.24	19.35	39.50
Observations	317,247,756		78,900,208	
Workers	49,735,510		10,162,666	

Note: Monthly and hourly wages measured in 2010 BRL. Female, Nonwhite, High-Skilled, Middle-Skilled, and Low-skilled represent percentages. High-skilled workers have a college degree or higher, middle-skilled are the ones with high-school or incomplete college degrees, and workers with less than a high-school diploma are classified as low-skilled.

million in total. As expected from previous studies, mean monthly wages are higher in the public sector, where the working week is also shorter on average. Given the discrepancy in weekly hours, I perform the decomposition estimates on *hourly* wages. Figure 3.1 shows the evolution of hourly wages over the years in the data. The observed gap increases towards the end of the period in analysis, and it varies with the government branch. Workers in the legislative and judicial branches enjoy larger observed premiums relative to the private sector than their counterparts in the executive offices.

From the information in Table 3.2, it is possible to see that characteristics

Figure 3.1: Hourly Wage per Government Branch



Note: Hourly wages at 2010 BRL. The gap between public and private hourly wages increased over the years, with workers from all government branches considered as a single group. Workers in the judicial branch register the largest gap relative to the private sector.

should play an important role in explaining the observed gap between public and private wages. On average, public sector workers have higher scores in observable attributes traditionally associated with better compensation in the labor market - they are older, more likely to have a college degree, and have higher tenures in their current jobs. A similar fraction of workers are nonwhite in both sectors, around 62%, while the sex ratio widely differs; women are 38% of the formal private sector workforce, compared to 59.23% in public sector jobs.

The estimation of a Mincer equation (Table 3.3) confirms that public sector workers' demographics have significant correlations with higher compensation.

The estimate also suggests a positive association with the public sector even after controlling for workers' characteristics, i.e., beyond its workers' observable attributes, employment in the public sector is associated with a 6.1% increase in hourly wages. What this specification does not capture, however, is the possibility that a worker's observable attributes may be rewarded differently across the two sectors. For instance, while career progression in the private sector may be based on performance and target achievement, many public sector jobs in Brazil have pre-determined rules based solely on tenure (Karpowicz and Soto, 2018). In the next section, the Oaxaca-Blinder decomposition attempts to measure the portion of the wage gap related to differences in characteristics separately from the potential heterogeneity in compensation schedules between the two sectors.

3.2 Results From the Oaxaca-Blinder Decomposition

In order to measure the size of the characteristics and wage structure components of the wage gap across the public and private sectors, I estimate an Oaxaca-Blinder decomposition using the natural logarithm of hourly wages as the dependent variable(Oaxaca, 1973; Blinder, 1973). Let $y^k_{i,t}$ denote the log hourly wage of individual i in year t working in sector k, where $k = 1$ ($k = 0$) represents the public (private) sector. Conditional on a set X^k_{it} of individual's i observable attributes, the expected log hourly wage is given by

Table 3.3: Mincer Estimates

Dep. Var.	Log Hourly Wage (1)	(2)
Female	-0.324***	
	(0.006)	
Nonwhite	-0.122***	
	(0.004)	
Age	0.044***	0.090***
	(0.001)	(0.000)
Age Sq.	-0.001***	-0.001***
	(0.000)	(0.000)
Tenure	0.042***	0.016***
	(0.001)	(0.000)
Tenure Sq.	0.000***	0.000***
	(0.000)	(0.000)
Low-skilled	-1.240***	-0.167***
	(0.023)	(0.001)
Middle-skilled	-0.948***	-0.162***
	(0.010)	(0.001)
Public	0.061***	0.121***
	(0.014)	(0.001)
Clustered Standard-Errors	Year	Worker
Observations	39,621,045	39,621,045
R^2	0.463	0.899
Within R^2	0.454	0.325
Year Fixed Effects (15)	✓	
Worker Fixed Effects (5,595,021)		✓

Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Clustered standard errors in parentheses. Based on a random sample of 10% of workers. High-skilled workers have a college degree or higher, middle-skilled are the ones with high-school or incomplete college degrees, and workers with less than a high-school diploma are classified as low-skilled.

$$y_{it}^k = X_{it}^k \beta^k + u_{it}^k, \quad \text{for } k=0,1 \tag{3.1}$$

with conditional mean independence, i.e., $\mathbb{E}\left[u_{it}^k \mid X_{it}^k\right] = 0$. Letting $\overline{Z}_t$ represent the sample mean of variable Z_t in year t, and $\widehat{\beta}$ the OLS estimate of coefficients β, the wage equations from the two sectors can be combined as

$$
\begin{aligned}
\overline{y}_t^1 - \overline{y}_t^0 &= \overline{X}_t^1\widehat{\beta}^1 - \overline{X}_t^0\widehat{\beta}^0 \\
&= \overline{X}_t^1\widehat{\beta}^1 - \overline{X}_t^0\widehat{\beta}^0 \pm \overline{X}_t^1\widehat{\beta}^0 \\
&= \underbrace{\left(\overline{X}_t^1 - \overline{X}_t^0\right)\widehat{\beta}^0}_{Characteristics} + \underbrace{\overline{X}_t^1\left(\widehat{\beta}^1 - \widehat{\beta}^0\right)}_{Coefficients}.
\end{aligned}
\tag{3.2}
$$

The term $\overline{X}_t^1\widehat{\beta}^0$ added and subtracted in the second line of equation 3.2 is the counterfactual mean wage of the average public sector worker had she been subject to the wage structure β^0 from the private sector. In other words, it measures the expected compensation of the average public sector worker if she was employed in the private sector. Equation 3.2 shows that the observed mean wage gap can be broken into two separate components. First, the *characteristics* term, sometimes called the *explained* part, measures the portion of the observed wage gap due to differences in observable attributes of workers in the two sectors. The second term, also called the *unexplained* or *discrimination* part, measures the role of heterogeneity in the wage structure across the two sectors. In a scenario where workers from both sectors have equal characteristics, if on average $\left(\mathbb{E}\left[X^1\right] = \mathbb{E}\left[X^0\right]\right)$, the wage gap would be fully attributed to the difference in compensation schedules between public and private sector jobs. On the other hand, if the wage structure was similar in both sectors ($\beta^1 = \beta^0$), the observed gap would be fully credited to differences in workers' characteristics.

How much of the wage differential in Brazil is explained by its public sector's

specific wage structure? As initially suggested by Figure 3.1, the wage gap remarkably varies by the branch of government. From a policy perspective, it is also important to determine which level of government is relatively more onerous in terms of public expenses. Thus, I estimate equation 3.2 for each segment of the public sector worker population in different branches and levels of government.[5] The estimation is based on a pooled OLS for each group separately, using the workers' demographics reported in column (1) of table 3.3. The results are summarized in Table 3.4. At the federal level, the total observed gap in the executive branch is equal to 253.26% of the mean hourly wage in the private sector, compared to 484.49% and 739.04% in the legislative and judicial branches, respectively.[6] Across all branches, workers in the federal government have the highest overall premium, followed by state, and municipal workers.

With the exception of municipal executive employees, all other branches and levels have a positive estimated coefficients term, with the highest value among workers of the judicial branch at the federal level - notwithstanding their characteristics, these workers would earn on average 545.82% more relative to private sector workers. Table 3.4 also shows the wage bill of each branch and level, and the corresponding monetary value of the coefficients term. From the average an-

[5] Another motivation for separate estimation of the decomposition for each government branch and level separately is the diversity of rules governing compensation and career progression within the public sector. Public sector workers are not perfectly mobile across different government institutions and job titles. They are most commonly hired for narrowly defined functions with specific rules of promotion and earnings schedule - according to Karpowicz and Soto (2018), there are over 130 career tracks, or *carreiras* in Brazil's public sector.

[6] The percentage figures are estimated as follows. Let x denote the estimated component of the log hourly wage decomposition. Then, in percentage terms, this is equivalent to $(100 \times (e^x - 1))\%$ of the private sector hourly wage.

Table 3.4: Oaxaca-Blinder Decomposition

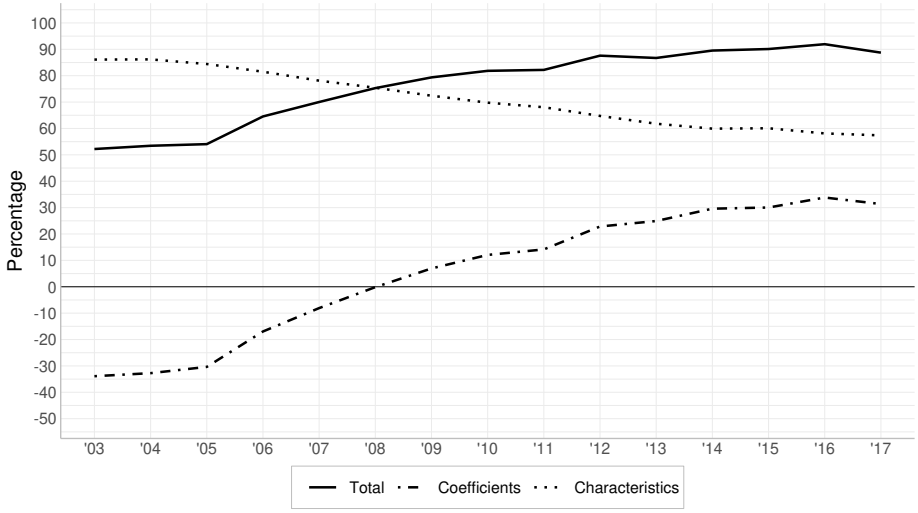
Branch	Level	Total	Characteristics	Coefficients	Yearly Wage Bill (billions)	Coefficients Bill (billions)	Coef. Perc. of Wage Bill
Executive	Federal	253.26	137.75	115.52	27.23	8.90	32.70
	State	109.69	93.48	16.21	56.46	4.36	7.73
	Municipal	19.59	44.29	-24.69	50.02	-10.33	-20.65
Legislative	Federal	484.49	95.62	388.88	1.77	1.18	66.53
	State	268.86	83.55	185.31	2.16	1.09	50.24
	Municipal	75.52	39.85	35.68	1.41	0.29	20.33
Judicial	Federal	739.04	193.22	545.82	8.10	5.27	65.05
	State	395.39	133.15	262.24	9.57	5.07	52.94
All	All	70.72	70.18	0.54	156.75	0.50	0.32

Note: The decomposition terms Total, Characteristics, and Coefficients are reported as percentages, e.g., the total wage gap in the federal executive branch was 253.26% of the prevailing mean wage in the private sector. The mean yearly wage bill of the federal executive government was 27.23 billion BRL, out of which 8.9 billion, or 32.70% of it, corresponded to the coefficients term in the wage gap. Yearly Wage Bill and Coeficients Bill are measured in 2010 BRL, and computed across the years between 2003 and 2017.

nual 56.46 billion BRL spent on wages of workers from the state executive branch in the data, 4.36 billion (7.73%) corresponds to the coefficients term of the wage premium. Proportionately, the two highest wage bills associated with the public sector wage structure are in the federal legislative (66.53%) and federal judicial (65.05%) branches. In absolute terms, the federal executive workers represent the largest coefficients bill.

When all public sector workers are considered as a single group, the coefficients effect size falls to 0.54%. However, this does not seem to be the case over time. In Figure 3.2, I plot the estimates of the decomposition by year. In the initial years, the public sector seems to have underpaid its workers compared to private wage structures, as shown by the negative coefficients component from 2003 to 2008. From 2009 onwards, the relative share of the coefficients term grows, as the effects of characteristics simultaneously decrease.

Figure 3.2: Oaxaca-Blinder Decomposition Across the Years



Note: The decomposition used in the graph considers workers from all government branches and levels as a single group. Before 2008, the coefficients term was negative, meaning that, given their observable attributes, workers in the public sector were underpaid relative to their private counterparts. After 2008, both characteristics and coefficients components are positive, and the total gap is close to 40 percentage points higher in 2017 relative to 2003.

3.3 The Gap Along the Wage Distribution

Beyond the decomposition of the average wage gap, it is valuable to ask how the gap varies over the distribution of earnings. Are low and high earners subject to the same characteristics' effects? Is the premium associated with the public sector's wage structure constant across different wage levels? To answer these questions, one has to estimate the wage premium per earnings quantiles, for instance, and compute a counterfactual distribution of earnings for the reference population, the public sector workers, in my case. In this paper, I use the proce-

dure proposed in Chernozhukov et al. (2013), which allows for the estimation of the characteristics and coefficients effects over the wage distribution.

I will also consider the possibility that workers endogenously select into the public and private sectors. Public sector jobs in Brazil offer stability, and workers can only be dismissed under extenuating circumstances after the first three years of tenure Emilio et al. (2012). Suppose more risk-averse individuals are disproportionately attracted to public jobs, and risk-aversion is correlated with observed attributes or even worker's earnings directly. In that case, OLS estimates such as the ones used in column (1) of 3.3 or Table 3.4 are not consistent.

If, however, workers' unobserved characteristics, such as risk aversion or ability, are constant across the earnings quantiles and periods of observation, meaning that all quantiles are affected the same way by a worker's constant unobserved attributes, then Canay (2011) proves the consistency for a two-step estimation procedure. In the first step, workers' unobserved factors are estimated from a panel specification that includes worker fixed effects, as in column (2) of table 3.3. Then, the quantile estimation, such as Chernozhukov et al. (2013), is performed on a transformed version of the dependent variable, where the estimates for worker fixed effects are subtracted from the observed log hourly wages.[7] Hospido and Moral-Benito (2016) and Bargain et al. (2018) use a similar procedure to study the public sector wage premium distribution for Spanish and French civil servants, respectively.

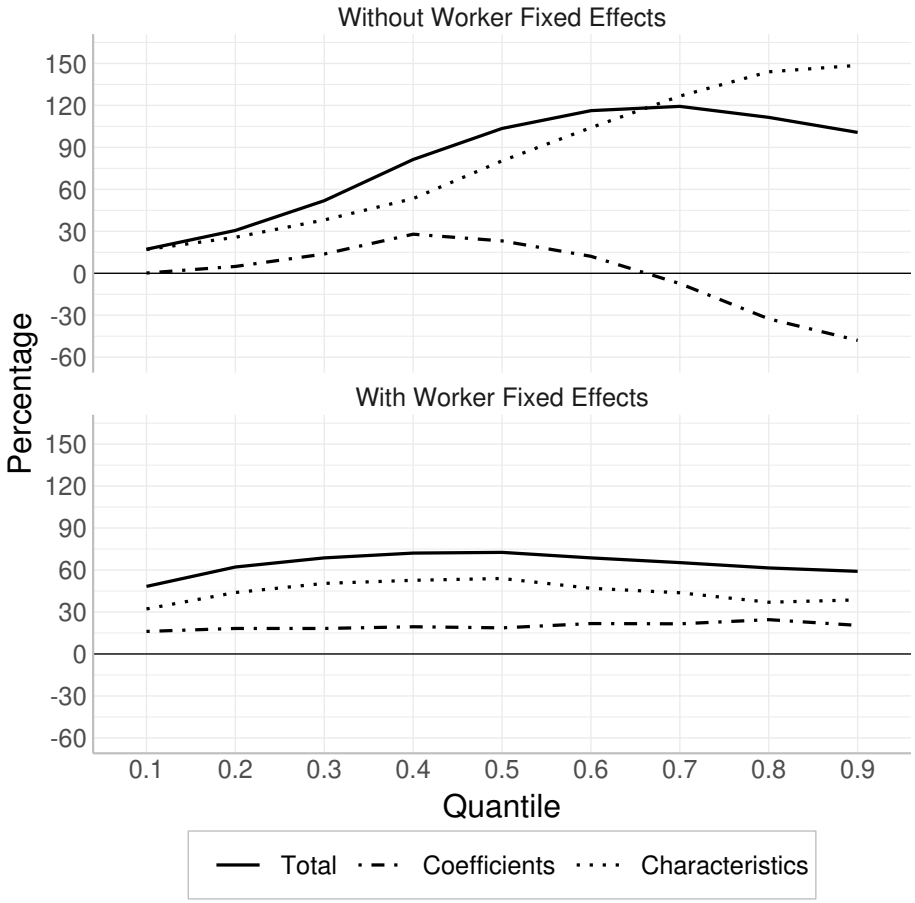
[7]The quantile decomposition step relies on the R package `Counterfactual` by Chen et al. (2017).

3.3.1 Quantile Decomposition Results

Figure 3.3 shows the estimates of the total hourly wage gap and the characteristics and coefficients components for deciles of the earnings distribution. In the first panel, the premium is estimated on the observed wage variable, without accounting for endogenous selection. Assuming random assignment into the two sectors, the conclusion is that public employees in the top three deciles of the distribution are underpaid by the government relative to the private sector, as the coefficients effect is negative, consisting with a wage *penalty* instead of a *premium*. At the same time, the impact of the characteristics increases as one moves up the earnings distribution, indicating that top earners are positively selected into the public sector. At the bottom 10% of the distribution, close to the totality of the wage gap is explained by differences in workers' characteristics across the two sectors.

Incorporating workers' fixed effects changes the overall shape of the public sector wage premium, the three curves - total, characteristics, and coefficients - get flattened. Once endogenous selection is considered, the public sector wage premium increases at the bottom 30% of the distribution, with positive coefficients effect. For the top 30% of earners, the conclusion is reversed; now, the decomposition points to a lower characteristics effect and higher coefficients effect, indicating that the wage structure specific to the public sector also benefits the top earners in public jobs. Overall, assuming that the fixed effects capture the individual's earnings potential regardless of the sector in which they are employed, the de-

Figure 3.3: Quantile Decomposition



Note: The decomposition used in the graph considers workers from all government branches and levels as a single group. In the top panel, the estimation assumes that workers are randomly assigned to public and private sector jobs. In the bottom panel, the endogenous selection is addressed by decomposing a transformed wage variable, where workers' fixed effects are subtracted from the observed wages.

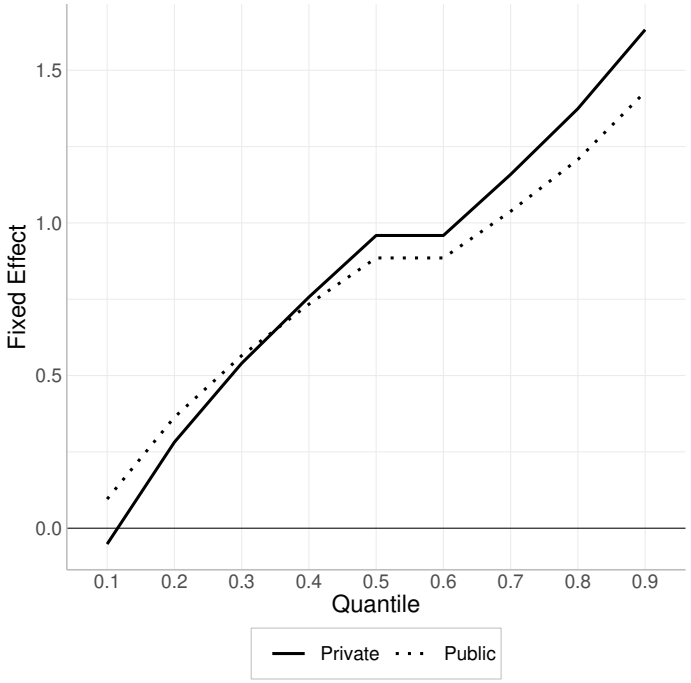
composition suggests that the public sector's wage structure benefits its workers uniformly across the wage distribution.

The distribution worker fixed effects helps understand the decrease in the total gap and its components for the top earners. In Figure 3.4, it is possible to see that worker fixed effects are higher for private sector workers in the top 60% of the fixed effects distribution. This is consistent with the idea that the public sector fails to retain top earners, and those that remain in the public sector are negatively selected. A similar finding is present in the Spanish and French contexts (Hospido and Moral-Benito, 2016; Bargain et al., 2018).

3.4 Conclusion

This paper estimates the public wage premium relative to the private sector in the Brazilian economy. I use a rich linked employer-employee administrative dataset to compute the premium over average wages and along the earnings distribution. Exploratory analysis suggests that the average worker in the two sectors varies with respect to observable attributes. The Oaxaca-Blinder decomposition shows that, beyond differences in characteristics, public sector workers from most government branches and levels enjoy a positive earnings premium related to the government's specific wage structure. Introducing worker fixed effects removes most of the premium variation along the earnings distribution; the total gap, as well as its characteristics and coefficients components, "flatten out" when endoge-

Figure 3.4: Distribution of Worker Fixed Effects



Note: The plot shows the value of worker fixed effects along the earnings distribution. The fixed effects are obtained from a panel specification that includes worker observable attributes that can change over time (age, tenure, educational attainment), as well as an indicator variable for public jobs (see column (2) of Table 3.3).

nous selection is considered. For a policymaker to close the portion of the gap between public and private wages stemming from the public sector's specific wage structure, a horizontal public wage freeze can be used.

Appendix A

Chapter 2

A.1 Data Handling

A.1.1 Preparing RAIS

The *Relação Anual de Informações Sociais* - RAIS version used in this paper starts in 2002 and ends in 2017. The files are made available by the Ministry of Labor in Brazil, and their transfer is conditional on a confidentiality agreement celebrated between Cornell's Labor Dynamic Institute and the Ministry. The files are hosted in a secured cluster within Cornell's BioHPC server ecosystem. RAIS' raw files are year-by-region (in some years, year-by-state) `.txt` tables. I first stack all files

within the same year, then merge the yearly files with the commuting zone list using the city codes as the key. Job records outside the reach of commuting zones are dropped.

For each job record, RAIS reports the job status on December 31st – if this variable has entry 0, it means that the work contract was terminated at some point in that year. I keep only the job records with active employment contracts on December 31st. As standard, if the same worker has more than one employer, I keep the highest-paying job. For the demographics used in the estimation of 2.6, I use the worker's *age*, *age squared*, and dummy variables for *female*, *white*, *college or higher* (codes greater than or equal to '9'), and *high school* (codes '7' or '8'). RAIS is an employer-reported database, and sometimes, a worker's history will show different colors/races, depending upon either the perception of the current employer or the worker's informed race when the job started (Cornwell et al., 2016). If a worker is ever reported as *non-white* (race/color codes different than '2'), I set the *white* dummy to 0.

In Section 2.4.2, I present event studies looking at the dynamic of new hires and separations within merged and spillover firms. In order to exclude spurious work contract terminations, such as transfers across establishments of the same firm or to the newly merged firm, I exclude the separations with reported reason coded with labels '30' and '31'. New hires are flagged using the tenure variable, measured in months as of December 31st of each year. Active jobs on December 31st with tenure less than or equal to 12 months are flagged as new hires. To

avoid counting spurious admissions, similar to the case of separations, I exclude the admissions coded with types '3' and '4'.

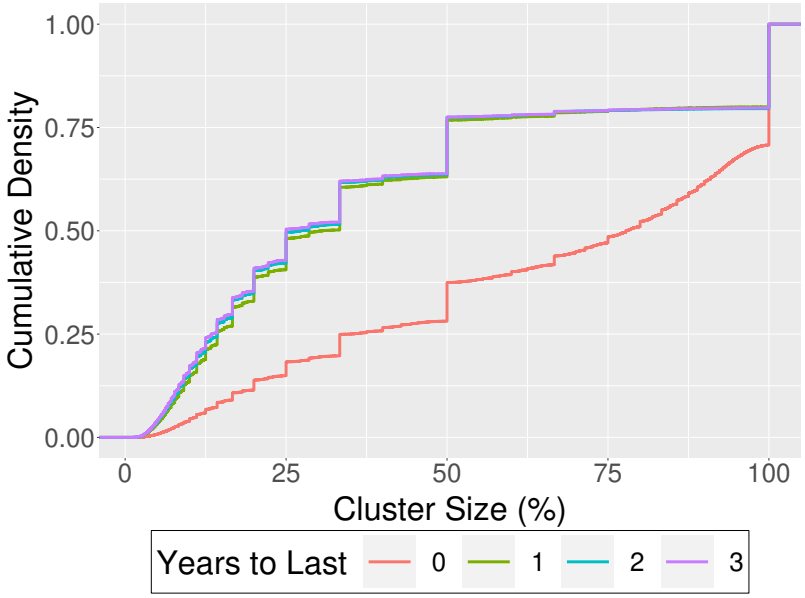
A.1.2 Identification of Establishments in M&A events

The identification of merger activity starts with the *Dados Públicos de CNPJ* - DPC, released monthly by the revenue agency in Brazil. The release I used in the paper is from September 5th, 2020. The file contains approximately 42.5 million observations at the establishment level. Every time an establishment is acquired or merged with others, its identifier is retired, and the revenue agency issues a new one. The establishment identifier, also called the establishment's CNPJ, is a hierarchical 14-digit code, where the first eight digits identify the firm, the next four digits identify the establishment, and the last two digits are used for checksums. I rely on the column describing the reason for the retirement of a CNPJ to flag acquired (code '2') or merged (code '3') establishments.

After flagging the acquired or merged establishments, I resort to the matched character of RAIS to keep a record of the next destination of workers re-employed in other firms. For each destination firm, I compute the relative size of the leaving coalition and choose the largest coalition's next firm as the most common destination. Figure A.1 shows the distribution of worker coalition sizes departing from acquired or merged establishments towards the most common destination firm in the last few years of such establishments. In the last year of an acquired estab-

lishment, in at least 70% of cases, more than 50% of workers are reported to be working at the same top destination firm in the following year. In the second to last year, and before that, a coalition of less than 10% of the acquired establishment workers can be found in the following year's most common employer. Therefore, I declare the firm that admits the most number of workers from an acquired or soon-to-be merged establishment, after the last year of observation of this establishment, as the buyer, or newly-merged firm, side of the M&A. The identification of both acquirer and acquired allows me to compute the predicted impact on HHI of each local labor market event, or events, in a given year. The predicted HHI is then used to separate out-of-market events from those that induce higher concentration changes.

Figure A.1: **Distribution of the Percentage of Workers Departing From Acquired or Merged Establishments to Most Common Destination Firm**



Note: Consider that year T is the last year of an acquired or merged establishment in RAIS, the worker data. There is a positive fraction of workers leaving acquired or merged establishments to the most common destination in years $T-1$, $T-2$, and $T-3$. In year T, however, the fraction of workers decamping towards the same firm is significantly larger than in previous years. The most common destination firm of workers leaving in year T is flagged as the acquirer or newly merged firm.

Appendix B

Chapter 3

B.1 Details in Data Construction

The raw records RAIS are grouped in state-by-year *.txt* files, with 941,297,560 observations in total. Then, I drop all records with faulty worker identifiers (*PIS* with less than 11 digits), those with entry 0 in the fields of mean monthly earnings and contract hours, negative values for tenure, and unknown contract type. As is customary in the literature, rural workers are not considered (codes 20, 25, 70, 75 in *Tipo de Contrato*). Jobs terminated as of December 31st of each year are also dropped.

Employers with legal type codes beginning with *1* are flagged as public sec-

tor employers, all others fall into the private sector category, except international institutions, such as embassies and multilateral organizations (*Natureza Jurídica* codes starting with *5* are dropped). Legal type codes were also used to assign public sector employers to government branches and levels, as follows: (i) executive federal (1015, 1104, 1139, 1198, 1201, 1210, 1228), (ii) executive state (1023, 1112, 1147, 1260), (iii) executive municipal (1031, 1120, 1155, 1244, 1279), (iv) judicial federal (1074, 1163), (v) judicial state (1082, 1171), (vi) legislative federal (1040), (vii) legislative state (1058), (viii) legislative municipal (1066).

I keep only the records with the highest monthly mean earnings for workers with more than one active job as of December 31st of each year. In constructing the *nonwhite* variable, I use the entire available job history of the worker. In RAIS, different employers may disagree on the reported race/color of a given worker (Cornwell et al., 2016). If at any point in the worker's job history, their race/color entry is different than *2* (code for white), I classify them as *nonwhite*. Given the relevance of race/color in the Brazilian labor market(Arcand and D'hombres, 2004), I drop all worker observations for whom there is no available information to assign the *nonwhite* value.

For educational attainment, I divide workers into three categories, based on the codes in the field *Escolaridade*; (i) completion of up to middle school is considered *Low-skilled* (codes *1, 2, 3, 4, 5*), (ii) incomplete high school or incomplete college degrees are labeled *Middle-skilled* (codes *6, 7, 8*), and (iii) workers with a college degree or higher are considered *High-skilled* (codes *9* or superior). The

observations of workers younger than 25 or older than 54 are dropped.

All earnings variables are deflated by the most widely used consumer price index, the IPCA, with the base year 2010. Because endogenous selection is addressed by estimating worker fixed effects, I exclude workers with only one observation in the data. Finally, I winsorize earnings below the 1st and above the 99th percentiles to avoid the influence of earning outliers or misreporting. After these steps, the data used in the analysis has 396,147,964 worker-year observations.

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