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Essays on the Determinants of Employment and Wages

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Essays on the Determinants of Employment and Wages

David Arnold

A Dissertation
Presented to the Faculty
of Princeton University
in Candidacy for the Degree
of Doctor of Philosophy

Recommended for Acceptance
by the Department of
Economics
Advisers: Henry Farber and Alexandre Mas

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Abstract

What determines the employment and wage levels paid by firms? While a long literature focuses on standard supply and demand factors as the key determinant, this dissertation consists of three chapters in which institutional factors and market or firm characteristics play a key role in determining labor-market outcomes. A central theme throughout is that in the presence of imperfections in the labor market, changes to the institutional framework or market environment can have large impacts on workers.

The first chapter studies whether labor market concentration impacts workers' outcomes. To study this question, I utilize a large sample of mergers and acquisitions in the United States between the years 1999-2009 to estimate the causal effect of local labor market concentration on labor-market outcomes. I find that mergers generating significant shifts in labor market concentration result in market-level declines in earnings. While antitrust authorities have historically focused on product-market competition, the results suggest that the largest mergers may warrant antitrust scrutiny based on labor market power alone.

The second chapter studies how privatization of state-owned enterprises impacts the labor market. While privatization remains a popular policy tool in many countries, workers and trade unions often fear it will reduce wages and employment. In this chapter, I study a privatization program that occurred in Brazil during the 1990s. I find that earnings for privatized workers firms fall substantially after privatization. Additionally, wages fall at private-sector firms connected to privatized firms by labor mobility.

The third chapter, co-authored with Joshua Bernstein, combines quasi-experimental evidence with a structural model of the labor market to identify the aggregate effect of employment protection legislation (EPL) on unemployment and output. In particular, we exploit the tenure-dependence built into EPL in Brazil (EPL only applies to jobs that last three months) to identify its effect on the hazard rate of job termination. We then map this effect to general equilibrium outcomes through a structural model of the labor market. We find

that EPL in Brazil leads to a significant drop in aggregate output through an increase in the unemployment rate.

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Chapter 1

Mergers and Acquisitions, Local Labor Market Concentration, and Worker Outcomes[1]

1.1 Introduction

About 2 percent of all workers each year are employed in an establishment that changes ownership. While antitrust authorities have historically focused on consumer welfare, new evidence linking poor labor market outcomes to both labor and product market concentration (Barkai, 2016; Autor et al., 2017; Azar et al., 2017; Benmelech et al., 2018; Rinz, 2018) has spurred recent policy debates on whether regulatory agencies should pursue new policies to protect workers (Hemphill and Rose, 2017; Marinescu and Hovenkamp, 2018; Naidu et al., Forthcoming). However, estimating the causal effect of concentration on labor market out-

[1]Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. A preliminary version of this chapter was presented at Western Economic Association International Graduate Student Workshop.

comes is complicated for two key reasons. First, concentration requires a market definition, which is often difficult to define and frequently contentious. Second, many factors change both concentration and earnings leading to endogeneity issues that can yield misleading correlations (Berry et al., 2019; Syverson, 2019).

This paper provides evidence on the impacts of M&A and local labor market concentration on workers using matched employer-employee data from the U.S. Census. The analysis is composed of four parts. First, I derive an index of concentration that incorporates data on job-to-job flows to measure substitutability between jobs in different industries. In this way, I provide a data-driven approach to address the difficult task of defining a labor market. Second, I estimate the direct impact of M&A on workers in M&A firms, which could be driven by changes in local labor market concentration, productivity, or product market power. Third, I estimate market-level effects of increased local labor market concentration due to merger activity. Fourth, I calibrate the Cournot model using the reduced-form estimates to determine whether changing labor market concentration has contributed to macroeconomic trends such as the falling labor share and stagnant wage growth. I now describe each of these parts in greater detail.

In the first part of the paper, I derive a simple Cournot model with three channels through which M&A impacts workers. First, increases in local labor market concentration will lower competition for workers and reduce wages and employment. Second, increases in product market power will incentivize firms to reduce quantities, resulting in falling employment with ambiguous impacts on wages.[2] Third, changes in the production process may increase productivity (e.g. better management practices (Bloom and Van Reenen, 2007; Lazear et al., 2015)) resulting in higher wages for workers, though some jobs may become redundant and eliminated. I use variation across mergers to disentangle these channels. In particular,

[2]In many models of monopsony power (e.g. Card et al. (2018); Berger et al. (2019)), wages depend on the labor demand, and not directly on the profits of firms. In these models, reductions in employment will result in reductions in wages regardless of whether firm profitability increases following a merger. In bargaining models, however, the surplus of the firm increases following M&A, resulting in higher wages for incumbent workers (He, 2018).

I explore heterogeneity by initial concentration, the size of the concentration change, and whether the firm produces a tradable or nontradable good.

Relative to a standard Cournot model, the key extension is that the wage in a given industry depends on both the total employment within that industry, as well as the weighted total employment in all other industries within the commuting zone. The weights depends on the substitutability between jobs in different industries, which I estimate using worker flows across industries. The model yields a simple relationship between a flows-adjusted concentration measure (that depends on the estimated weights between industries) and market wages. Intuitively, even if a given industry is comprised of only a few firms, the labor market for workers in this industry may not be concentrated if the workers routinely move to jobs in different industries.

In the second part of the paper, I estimate the direct impact of M&A on workers using a difference-in-differences design that compares outcomes for M&A workers to a matched control group before and after an M&A event. To identify M&A events, I use enterprise-level identifiers in the Longitudinal Business Database (an establishment-level panel for the U.S.) to discern when establishments change ownership.[3] To study the impact on worker-level earnings, I use the Longitudinal Employer Household Dynamics (LEHD) survey, a matched employer-employee dataset built from state unemployment insurance records. For this project I have access to 26 states. The worker-level data is crucial for two reasons. First, it allows me to compute job-to-job flows across industries which is an important component of the flows-adjusted measure of local labor market concentration. Second, it allows me to control flexibly for worker composition by tracking the same workers over time. In total, I identify roughly two million incumbent workers in M&A firms between the years 1999-2009.

[3]There are some complications that arise by using this method to identify ownership changes which deal with how the Census classifies single-unit vs. multi-unit firms and is discussed in Section 1.3. I follow the approaches utilized in (Maksimovic and Phillips, 2001; Tate and Yang, 2016; Atalay et al., 2019) who also use the LBD to identify changes in ownership. A similar approach is used in He (2018) with Danish administrative data.

I find that M&A workers' earnings remain stable in M&As that have negligible impacts on local labor market concentration. In stark contrast, mergers that result in significant increases in local labor market concentration result in a 2.1 percent decline in M&A workers' earnings relative to the matched control, with larger declines in already concentrated markets. While employment declines are larger in mergers that increase local concentration (16.5 percent), mergers with no impact on local concentration still result in economically meaningful declines in employment (13 percent), suggesting the bulk of employment losses are due to reductions in overhead labor and not increases in product or labor market power.

In tradable industries, I continue to find negative impacts on wages only in mergers that increase local labor market concentration, suggesting the effects are not driven by changes in product market power. I find similar patterns in a sample of mergers between firms operating in multiple commuting zones, for which local economic conditions likely did not trigger the M&A. This evidence is therefore consistent with M&A reducing wages through increased monopsony power in the labor market. However, these direct effects understate the impact of M&A on workers if increased local concentration reduces wages for all firms in the labor market.

In the third part of the paper, I estimate the market-level effects of increased local labor market concentration due to merger activity. As discussed previously, interpreting negative correlations between local labor market concentration and market wages as evidence of imperfect competition in labor markets remains controversial due to potential endogeneity issues. I find evidence that these concerns are likely warranted in practice. First, I find a significant *positive* relationship between market employment and local labor market concentration, a finding inconsistent with increased concentration causing increased monopsony power. Second, I find that changes in ownership account for only 1.4 percent of the variation in local concentration within a market over time, with much more being explained by shrinking and expansion of existing firms. Given these facts, it is not clear whether approaches that estimate the elasticity of earnings with respect to local concentration using any change

in concentration are useful for understanding how changing market structure impacts workers. In contrast, I estimate the elasticity of earnings with respect to *merger-induced* changes in local concentration, which is both theoretically justified as well as directly relevant to antitrust authorities.

I find only the largest (top-ventile) merger-induced concentration changes cause decreases in market-level earnings. Average earnings fall by about 3.3 percent in these top-ventile markets relative to other markets. Using a top-ventile change as an instrument for concentration yields an elasticity of earnings with respect to local concentration equal to -0.22. This estimate is consistent in a sample of tradable industries as well as for national mergers. The point estimate is larger in magnitude than recent work that finds elasticities in the range of $(-0.15, -0.01)$, though the 95 percent confidence interval for my estimate covers many of the estimates in prior work.

In the fourth part of the paper, I use the market-level estimates in combination with the Cournot model to assess whether changes in local concentration and M&A activity contribute to important labor market trends. Monopsony power has been posed as a potential source of stagnant wage growth for low-income workers (Krueger and Posner, 2018) and the falling labor share (Barkai, 2016), with lack of antitrust action as a potential contributing factor (Marinescu and Hovenkamp, 2018; Naidu, Posner and Weyl, Forthcoming). To inform these issues, I use the model to transform the distribution of concentration across markets into implied wage markdowns and document how this has changed over time. Then, to consider the role of antitrust scrutiny, I estimate what fraction of mergers could have been blocked on the basis of increased labor market power.

I find local concentration depresses wages by about 4-5 percent relative to a fully competitive benchmark, with a slight downward trend since the late 1980s. Therefore, changes in local concentration cannot rationalize stagnant wage growth or the declining labor share documented in the literature. These results do not necessarily imply that monopsony power in general has been decreasing over this time period. Local concentration is only one source

of monopsony power. Declining unionization rates (Farber et al., 2018) or increases in non-competes and no-poaching agreements (Krueger and Ashenfelter, 2018; Krueger and Posner, 2018) could lead to rising monopsony power even in the presence of falling local concentration.

A hypothetical antitrust authority that blocks any merger that decreases market-level wages by at least 5 percent would block about 1.2 percent of the mergers in the analysis sample. In product markets, a predicted 5 percent increase in prices is considered large enough to warrant antitrust enforcement. The hypothetical fraction of blocked mergers based on labor market power is only slightly smaller than the actual fraction challenged by antitrust authorities in the United States.[4] I interpret this as evidence that the labor market is an important market for which antitrust scrutiny is relevant, but likely only for very large mergers that generate considerable shifts in local concentration. The evidence, however, does not support the conclusion that lack of antitrust scrutiny for labor markets has been a major contributor to labor market trends such as the falling labor share or stagnant wage growth. Most mergers do not generate large shifts in concentration and I find no evidence that the number of anticompetitive mergers in labor markets has been increasing over time.

This paper contributes to three distinct literatures. First, it contributes to the literature on the anticompetitive effects of mergers and acquisitions. There is a long theoretical and empirical literature in industrial organization studying the impacts of M&A on consumer welfare (Dansby and Willig, 1979; Hart et al., 1990; Farrell and Shapiro, 1990; Nevo, 2000; Kaplow and Shapiro, 2007; Dafny et al., 2012; Gowrisankaran et al., 2015; Miller and Weinberg, 2017). Recently, a number of papers argue that antitrust should also consider monopsonistic impacts of M&A (Hemphill and Rose, 2017; Marinescu and Hovenkamp, 2018; Naidu, Posner and Weyl, Forthcoming). Recent work in industrial organization mostly relies on estimat-

[4]This comparison comes with a number of caveats that are discussed in detail in Section 1.6.8. There is recent evidence of antitrust scrutiny having a deterrence effect (Wollmann, 2019), suggesting the fraction of mergers that are blocked due to antitrust legislation is actually larger than the fraction challenged in practice by antitrust authorities.

ing structural demand models and simulating mergers to understand the impacts on prices and welfare. In contrast, I use a matched difference-in-differences design to identify labor market impacts on a sample of completed mergers. This study therefore contributes to the smaller but growing literature on "retrospective" merger analysis in industrial organization (Ashenfelter et al., 2013, 2015; Dafny et al., 2019).

Second, this paper contributes to a smaller literature that studies the impact of M&A on workers. Brown and Medoff (1988) find that acquisitions in Michigan result in lower wages and increased employment. Siegel and Simons (2010) studies M&A in Sweden and finds increases in productivity but decreases in employment. He (2018) studies M&A in Denmark and finds no impact on employment but negative effects on wages, and argues this is caused by high-wage managers being replaced in target establishments. Currie et al. (2005) and Prager and Schmitt (2018) both study mergers in hospitals and find evidence of increased monopsony power. Relative to these papers, I study a large sample of M&A in the United States and isolate the role of local labor market concentration in explaining heterogeneity in effects across mergers.

Lastly, this paper relates to the literature on imperfect competition in labor markets. A long literature in economics has argued that firms have some latitude to set wages (Robinson, 1933). A number of recent papers have found evidence of imperfect competition in labor markets (Hirsch et al., 2010; Ransom and Sims, 2010; Staiger et al., 2010; Manning, 2011; Depew and Sørensen, 2013; Hirsch et al., 2010; Webber, 2015; Naidu et al., 2016; Cho, 2018; Dube et al., Forthcoming; Kline et al., 2019; Goolsbee and Syverson, 2019; Lamadon et al., 2019). One strand of this broader literature argues local labor market concentration plays a role and documents a robust negative relationship between different measures of labor market concentration and wages (Azar et al., 2017; Benmelech et al., 2018; Hershbein et al., 2018; Rinz, 2018; Qiu and Sojourner, 2019). The methods used to measure concentration in this paper build on recent work that utilizes microdata to inform the definition of the labor

market (Schmutte, 2014; Nimczik, 2017; Jarosch et al., 2019) or obtain a measure of outside options (Caldwell and Danieli, 2019) and compensating differentials (Sorkin, 2018).

The structure of the paper is as follows. Section 1.2 develops a model that illustrates channels through which M&A activity may impact workers and then links these impacts to local labor market concentration. Section 1.3 discusses the institutional details, data, and measurement of concentration in the data. Section 1.4 describes the research design. Section 1.5 estimates the direct impact of M&A on incumbent establishments and workers. Section 1.6 estimates the market-level impacts of merger activity due to increased concentration in the labor market. Section 1.7 concludes.

1.2 A Model of Mergers and Acquisitions

1.2.1 Firm Problem

This section presents a simple model of the labor market through which to understand impacts of mergers and acquisitions on workers. To begin, I assume each firm j faces an upward sloping inverse labor supply curve given by $w_j(l_j)$. This implies that if a firm lowers its offered wage (w_j) it will not lose all its workers, and therefore endowing the firm with monopsony power. In contrast, in a perfectively competitive model, a firm would lose all its workers if it sets any wage below the equilibrium wage paid by other firms in the market.

I assume firms maximize profits by choosing employment, l_j, taking as given the inverse labor supply curve $w_j(l_j)$:

$$\max_{l_j} R_j(l_j) - w_j(l_j)l_j, \tag{1.1}$$

where $R_j(l_j)$ is the revenue function of firm j with l_j as the only input. In general, this revenue function will depend on the productivity of j as well as product market parameters. For example, if the product of firm j becomes more attractive to consumers, then the price will increase, implying the revenue at any given employment level l_j will also be higher. Profit maximization yields the following first-order condition:

$$w_j = \underbrace{\left(\frac{\eta_j}{\eta_j + 1}\right)}_{\text{fraction of MRPL to worker} = \gamma_j} \underbrace{\theta_j}_{\text{MRPL}}, \tag{1.2}$$

where η_j is the firm-specific elasticity of labor supply, θ_j is the marginal revenue product of labor and γ_j is the fraction of marginal revenue product of labor that accrues to the worker. This is the standard monopsony wage rule (Robinson, 1933). If firms hire in perfectly competitive labor markets, then firms will face a flat labor supply curve (which implies $\eta_j = \infty$). In this case, the wage will be equal to the marginal revenue product of labor. For any η_j less than infinity, workers will be paid a fraction of their marginal revenue product of labor.

In theory, mergers may impact both η_j (through monopsonistic impacts) as well as θ_j (through either productivity or product market changes). For example, increases in productivity due to M&A will increase wages for workers through an increase in θ_j. Increases in product market power will reduce the value of marginal product of labor, resulting in a decline in the wage. Intuitively, in this model, wages are a monotonic function of employment (given $w_j(l_j)$ is upward sloping). Therefore, if increased product market power results in lower output, then wages will fall as a result of declining employment. However, models based on wage bargaining will have the opposite prediction. Increased product market power will increase profits, leading to lower employment, but higher surplus per worker. This will lead to increased wages through rent-sharing.[5]

Lastly, a common justification for mergers is that they will reduce fixed costs. This may be realized by eliminating redundant jobs across employers. For example, a firm may only need one human resources or accounting department, and therefore may decide to lay off redundant workers following a merger or acquisition. In Appendix 1.9.3, I follow Bartelsman et al. (2013) and assume that firms use two types of labor: variable production labor and overhead labor. Mergers that reduce overhead labor will result in falling employment, but

[5]See Appendix 1.9 for an illustrative wage-bargaining model that has this implication.

will have no impact on the marginal revenue product of labor (θ_j). This is because the marginal revenue product of labor depends on the number of workers used in production, not the workers used for overhead, and therefore remains constant as the number of overhead workers changes.

1.2.2 Identifying the Direct Impacts of Mergers

In this section I decompose the effect of M&A into three components: monopsony power, product market power, and productivity. I then show how to use variation across mergers to disentangle these channels. To begin, consider the average treatment effect of a merger on the log wage:

$$\mathbb{E}[\tilde{w}_j(1) - \tilde{w}_j(0)] = \underbrace{\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)]}_{\text{monopsony effect}} + \underbrace{\mathbb{E}[\tilde{\theta}_j(1) - \tilde{\theta}_j(0)]}_{\text{MRPL}}, \tag{1.3}$$

where $\tilde{w}_j(1)$ is the log wage if the firm goes through the merger and $\tilde{w}_j(0)$ is the log wage if the firm does not go through the merger. In words, this impact can be caused by increased monopsony power of the firm (resulting in workers' receiving a lower fraction (γ_j) of marginal revenue product of labor) and changes in the marginal revenue product of labor (captured by θ_j). Further, I assume the impact on the marginal revenue product of labor can be decomposed into a product market effect (μ_j) and a productivity effect (ψ_j), which is separable in logs.[6] In this case, we can write the total effect on wages as:

$$\mathbb{E}[\tilde{w}_j(1) - \tilde{w}_j(0)] = \underbrace{\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)]}_{\text{monopsony effect}} + \underbrace{\mathbb{E}[\tilde{\mu}_j(1) - \tilde{\mu}_j(0)]}_{\text{market power effect}} + \underbrace{\mathbb{E}[\tilde{\psi}_j(1) - \tilde{\psi}_j(0)]}_{\text{productivity effect}} \tag{1.4}$$

To disentangle these channels I use variation across mergers. To do so, I must make a number of assumptions:

[6] For example, assuming constant elasticity of demand with an arbitrary one-input production function $F_j(l_j)$ would imply $\mu = p(1 + \frac{1}{\varepsilon_j})$ where ε_j is the elasticity of demand and $\tilde{\psi}_j = \frac{\partial F_j(l_j)}{\partial l_j}$.

Assumption 1. *No product market power effects for tradable goods*

$$\mathbb{E}[\mu_j(1) - \mu_j(0)|\text{Tradable}_j = 1] = 0 \tag{1.5}$$

Assumption 2. *No monopsony effects for zero changes in local labor market concentration (ΔC)*

$$\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)|\Delta C = 0] = 0 \tag{1.6}$$

Assumption 3. *Productivity effects are mean-independent with respect to changes in local labor market concentration (ΔC)*

$$\mathbb{E}[\psi_j(1) - \psi_j(0)|\Delta C = \kappa] = \mathbb{E}[\psi_j(1) - \psi_j(0)] \tag{1.7}$$

where $\kappa > 0$. The logic behind Assumption 1 is that product markets for which firms sell highly tradable goods are close to competitive, and therefore, a single merger is unlikely to have large impacts on market power. For example, a merger between two coal mines is unlikely to change the national price of coal. This assumption is often maintained in the literature on local labor markets (Moretti, 2011) while the international trade literature often models industries as being composed of a continuum of firms, again implying a single merger will not impact prices.[7]

Assumption 2 assumes that monopsony power does not change if there is no change in local labor market concentration. This implicitly assumes that labor market competition is local in nature. While this is a relatively common assumption, it could be violated for certain professions for which workers routinely move across commuting zones. In this case, I would underestimate the role of monopsony power.

[7]Some tradable industries are quite concentrated at a national level. Therefore, I also consider impacts in tradable industries for which national concentration is relatively low (HHI below 0.05).

Assumption 3 assumes that the effect on productivity is independent of the change in local labor market concentration. For example, if acquiring firms raise productivity by hiring better managers, then productivity gains would not depend on how much concentration changes due to the merger. This assumption would be violated, however, if productivity gains due to a merger are higher when the two businesses are located in the same region. For example, imagine sharing resources between two establishments in the same region increases the overall productivity of the establishments. In this case, mergers that increase local labor market concentration would also lead to greater gains in productivity that raise worker wages. Therefore, in this case, monopsony power would again be understated as mergers that increase monopsony power also tend to increase productivity.

Once these assumptions are maintained, it is straightforward to use variation across mergers to disentangle various channels. The intuition is simple. Take, for example, a merger in a tradeable goods industry between firms in different commuting zones. In this case, Assumption 1 implies there is no impact on product market power because the firms sell tradable goods. Assumption 2 implies there is no impact on monopsony power because the merger did not increase local concentration. Therefore any changes in wages at merging firms relative to control firms must be due to changes in productivity.

Now compare this to a merger between tradeable goods firms that are in the same industry and commuting zone. This merger will increase local labor market concentration by some positive amount, and will therefore impact wages through both the monopsony and productivity channels. Assumption 3 states that, in expectation, any wage changes due to productivity will be the same in this merger as the merger in which the firms were located in different commuting zones. Therefore, the differential wage effect is due solely to monopsony power. In contrast, in nontradable industries, a merger between firms in the same commuting zone may increase both market power and monopsony power.

In this section, I have assumed monopsony power depends on changes in local labor market concentration without providing an explicit mapping between concentration and earnings. The next section makes this mapping explicit.

1.2.3 Linking Impacts to Concentration

To map earnings to a measure of labor market concentration, I assume firms in a given market compete in the labor market à la Cournot. This assumption generates a simple relationship between market-level earnings and the Herfindahl-Hirschmann Index (HHI), which is commonly used in antitrust analysis to predict anticompetitive effects of mergers and acquisitions. While the main text focuses on the Cournot model, there are a number of potential models that can be used to link concentration and earnings. In Appendix 1.9.4, I go through a few alternative models including a dominant firm model (Landes and Posner, 1981) and a general equilibrium oligopoly model similar to Berger et al. (2019) and Atkeson and Burstein (2008).[8] Qualitatively, the insights from each model are similar. Mergers with larger shifts in concentration in already concentrated markets will have the largest impacts on wages. However, the relevant concentration measure will vary across models. For example, in the dominant firm model, the share of the labor market employed by the largest firm is the model-relevant concentration measure.

To begin, I assume there are N firms hiring in a given labor market m. Later, when turning to empirics, a market m will be an industry (4-digit NAICS) within a commuting zone. Each commuting zone should be thought of as an isolated island that does not interact with other commuting zones. For now, I ignore substitutability across industries within a commuting zone although this will be a crucial factor in measuring labor market concentration in the next section.

[8] Jarosch et al. (2019) takes a very different approach and uses a search model with finite firms to derive a relationship between market wages and concentration. Unlike the standard HHI, their measure depends on higher order terms of market shares.

In the model, each firm simultaneously chooses labor, l_j, taking as given the labor demand choices of the other $N-1$ firms in the market. Total employment in the market is given by $L_m \equiv \sum_{j=1}^{N} l_j$. There is a single market wage that depends on the total employment in the market ($w_m(L_m)$). In Appendix 1.9.1, I show that these assumptions yield the following market-level wage equation:

$$w_m = \underbrace{\left(\frac{\eta_m}{HHI + \eta_m}\right)}_{\text{worker fraction} = \gamma_m} \underbrace{\theta_m}_{\text{AMRPL}}, \tag{1.8}$$

where η_m is the elasticity of labor supply facing the entire market, $HHI = \sum_j (s_j)^2$ is the Herfindahl-Hirschman index based on employment shares, and θ_m is the average (employment-weighted) value of the marginal product of labor, and γ_m is the fraction of the average marginal revenue product that workers receives as wages.[9] In this model, increases in HHI caused by mergers will lower the fraction of marginal revenue product going to workers. Therefore, to predict anticompetitive effects, one should compute how much a given merger will change concentration. However, this computation requires choosing a market definition. In many cases, this definition will be debatable.

The recent literature on monopsony power in labor markets has generally used interactions between industry and region or occupation and region. However, some industries and occupations are very specific and there is considerable mobility across both industries and occupations in the data (Moscarini and Thomsson, 2007; Kambourov and Manovskii, 2008; Groes et al., 2014). Appendix Figure 1.11 computes the probability a job transition is within a given occupation or industry cell using Brazilian matched employer-employee data. As can be seen in the figure, at the 1-digit level, about 60 percent of transitions are within the same occupation, while about 50 percent are within the same industry. At the 4-digit level, about

[9] Often this equation is re-written as $\frac{w_m - \theta_m}{w_m} = \frac{HHI}{\eta_m}$ so that the equation is linear in concentration. Note that if one could get a measure of θ_m a linear regression of the left-hand side quantity on HHI would identify the market-level elasticity. This is not usually feasible though because θ_m is generally not observable.

22 percent of job transitions are within the same occupation, while about 19 percent are within the same industry.

Incorporating Substitutes to Calculate Concentration

Given the ambiguity regarding the appropriate market definition, I now extend the Cournot model discussed above to incorporate substitutes directly into the wage equation. Instead of wages in industry m being a function of employment only in m, the wage will now depend on the employment in both the industry m as well as all other industries within the commuting zone. However, industries in which a worker in m is unlikely to transition to will be down-weighted.

For a worker currently employed in industry m, I denote sum value of an allocation of employment across industries $\{L_1, ..., L_M\}$ as $\bar{V}_m = \sum_{k=1}^{M} V(k|m)L_k$, where $V(k|m)$ represents the value of a job in industry k for a worker currently employed in industry m. I assume the market wage in m is a direct function of this sum utility. Intuitively, this setup tries to capture how the availability of substitutes impacts wages. For example, imagine two commuting zones with the same level of employment in hospitals. In the simple version of the model, we would expect the wages to be exactly the same in the two commuting zones (assuming equal productivity and market elasticities of labor supply). However, imagine one of the commuting zones also has a very large nursing care facilities market. Jobs in this industry provide relatively high utility for workers in the hospital industry (i.e. $V(k|m)$ is large). Therefore, under the extended model, we would expect the wages for nurses at hospitals to be higher in the commuting zone with more skilled nursing facilities.

How should $V(k|m)$ be measured in practice? I argue that endogenous flows across markets are helpful in measuring $V(k|m)$ in the data, similar to Sorkin (2018) who uses flows between firms to estimate the value of a given firm.[10] To see this, let $U_i(k|m) =$

[10]The goal here is to understand what firms within a commuting zone are competing against each other. To do so, I use flows across industries to determine which industries compete against each other. Theoretically, one could use bilateral flows to measure competition between two firms,

$ln(V(k|m)) + \xi_i$ be the utility of a job in market k for worker i who is currently employed in market m. $ln(V(k|m)$ is a term that is common to all workers in market m, while ξ_i is an idiosyncratic shock that captures heterogeneity across workers. I assume job offers arrive at a market-specific rate λ_m. When a job arrives, the worker must decide whether to remain in the current job or move to the new job. The probability we observe a worker from m move to a job in market k is given by:

$$P(k|m) = \lambda_m \cdot f_k \cdot Pr(k \succ m), \tag{1.9}$$

where f_k denotes the probability that the offer comes from a firm in market k and $Pr(k \succ m)$ denotes the probability the offer from a firm in market k yields higher utility for the worker than the current job in market m. I assume the idiosyncratic shock, ξ_i, is distributed type I extreme value. This implies the probability we observe a job transition from m to k relative to a job transition within market m is given by:

$$\frac{P(k|m)}{P(m|m)} = \underbrace{\frac{f_k}{f_m}}_{\text{relative offers}} \cdot \underbrace{\frac{V(k|m)}{V(m|m)}}_{\text{relative values}} \tag{1.10}$$

The average utility of a market is only identified up to scale, therefore I normalize $V(m|m) = 1$. This implies $V(k|m)$ can be solved for in Equation (1.10):

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{f_m}{f_k} \tag{1.11}$$

In practice, I do not observe the distribution of offers from different markets. To proceed, I assume offers are a linear function of market size (i.e. $f_m = \kappa L_m$ for some $\kappa > 0$). This implies that I can replace the ratio of offers with the relative size of the markets, yielding:

however the job-to-job network at the firm-level is quite sparse, making these competition measures likely poorly estimated. Sorkin (2018) uses a recursive algorithm similar to Google's page rank algorithm to identify an absolute ranking of firm's, however, this approach doesn't necessarily identify which firms compete with one another in the labor market.

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{L_m}{L_k} \tag{1.12}$$

Note that everything on the right hand side of Equation (1.12) can be measured with data on job-to-job flows and industry employment. Going forward, I denote estimated value in Equation (1.12) as $\alpha_{m\to k}$ to distinguish it from the theoretical object, $V(k|m)$. We can now substitute in for $V(k|m)$ in order to write the wage in market m as as a function of observables:

$$w_m(\bar{V}_m) = w_m(\sum_{k=1}^{M} \alpha_{m\to k} L_k) \tag{1.13}$$

Let $\tilde{s}_j$ denote the market share of firm j:

$$\tilde{s}_j = \frac{l_j}{\sum_{k=1}^{M} \alpha_{m\to k} L_k} \tag{1.14}$$

This market share depends on the employment in all firms in the commuting zone. However, firms in industries that workers in market m rarely transition to will receive very low weight. The log market wage is now given by:

$$\tilde{w}_m = \tilde{\theta}_m + ln\left(\frac{\eta_m^{\alpha}}{C + \eta_m^{\alpha}}\right), \tag{1.15}$$

where $C = \sum_j \tilde{s}_j^2$ is defined as the flows-adjusted concentration measure, η_m^{α} is equal to $\frac{\partial w_m}{\partial \alpha L}\frac{\alpha L}{w_m}$, and $\alpha L = \sum_{k=1}^{M} \alpha_{m\to k} L_k$. This wage equation leads to the following two propositions regarding the impact of mergers on wages, both of which will be important in understanding heterogeneity in earnings effects across mergers. For both propositions, I assume the change in $\tilde{\theta}_m$ is the same across mergers, which is true in tradable industries (Assumption 1) if changes in productivity do not depend on changes in concentration (Assumption 3).

Proposition 1. *Conditional on the market-level elasticity and initial concentration (C_0), wages decline more in mergers that generate larger shifts in concentration (ΔC).*

This proposition rationalizes the use of predicting changes in concentration to predict anticompetitive impacts of merger. However, it also highlights the importance of controlling for initial concentration and market-level elasticities. In particular, regarding initial concentration, it is easy to prove the following proposition:

Proposition 2. *Conditional on the market-level elasticity and the change in concentration (ΔC), wages decline more in mergers in more concentrated markets (C_0).*

To see this, one can differentiate Equation (1.15), which yields the following formula for the elasticity of wages with respect to concentration:

$$\frac{\partial \tilde{w}}{\partial \tilde{C}} = -\frac{C}{C + \eta_m^{\alpha}} < 0 \tag{1.16}$$

Note that if the C is very low, the marginal effect of an increase in C will be small. That is, shifting concentration from very low levels to still low levels will not cause an appreciable decrease in wages. However, impacts will be larger at higher concentration levels. This nonlinearity is reflected in the Horizontal Merger guidelines. For example, while an increase in HHI of around 0.02 is not usually a concern in unconcentrated markets ($HHI < 0.15$), the same size increase in concentrated markets ($HHI > 0.25$) does raise antitrust concerns. When turning to empirics, both the size of the concentration change as well as the initial concentration will be important in predicting negative impacts of mergers and acquisitions on workers.

1.2.4 Relationship to IO Literature and Wage-Concentration Regressions

A recent literature finds a robust negative relationship between local labor market concentration and wages.[11] However, interpreting this evidence as causal remains controversial due

[11] See Azar et al. (2017); Benmelech et al. (2018); Hershbein et al. (2018); Rinz (2018); Lipsius (2018) among others.

to two main issues. The first issue, as discussed above, is due to measurement errors that arise due to potentially arbitrary market definitions.

The second issue, as discussed in Berry et al. (2019) and Syverson (2019), is that there are many factors that may impact both concentration and market outcomes. Therefore any given correlation can be rationalized in a number of ways. For example, increased import competition can rationalize the negative correlation between wages and concentration even if markets are perfectly competitive. If increased import competition causes low productivity firms to exit the market (Bernard et al., 2006), then the fall in labor demand will cause wages to fall (Autor et al., 2013; Dix-Carneiro and Kovak, 2017). Therefore, wages will be negatively correlated with increases in concentration, but in this case the correlation has nothing to do with monopsony power.[12] This issue is the primary reason why the industrial organization literature mostly abandoned using concentration indices to proxy for market power.

How are these issues addressed in this paper? The first issue involving market definition is discussed in detail in Section 1.2.3. While I define the labor market at the narrow 4-digit NAICS by commuting zone level, I directly incorporate flows across industries into the market concentration measure. To address the second issue I use variation in concentration driven solely by merger activity. Therefore, while there are multiple pathways from concentration to labor market outcomes, I isolate variation driven by merger activity and show that this variation predicts outcomes in a large sample of mergers.

There are important caveats, however, with using concentration to predict anticompetitive effects of mergers. In many cases, it will be difficult to predict the exact magnitude of the impact on a proposed merger which is often the goal of antitrust analysis. For example, imagine two mergers have the exact same predicted impact on concentration in equally concentrated markets. Will the two mergers have the same impact on wages? Not necessar-

[12] Benmelech et al. (2018) controls for the "China-shock" in Autor et al. (2013) and continues to find a negative relationship between market concentration and wages, indicating it is unlikely this correlation is driven entirely by trade-induced shocks to labor demand.

ily. A key component of this calculation is the market-level elasticity η_m (note Propositions 1 and 2 condition on η_m). In Section 1.6.7, I show that the difference-in-differences estimates can be used to compute an average market-level elasticity of labor supply across all markets. However, to predict the effect for a given merger, one would need to know the market-specific elasticity, which is in general not observed. Future work utilizing specifying a structural demand system would be helpful in performing prospective merger analysis (i.e. merger analysis for a proposed merger). See (Azar et al., 2019) for a recent example that adapts standard IO tools to the labor market.

1.3 Institutions, Data, and Measurement

1.3.1 Antitrust in the United States

In the United States, the Department of Justice and Federal Trade commission are tasked with blocking mergers that harm competition. The 1976 Hart-Scott Rodino Act requires merging entities to notify antitrust authorities before a transaction takes place. There are exemptions that depend on a number of factors, the most important being the value of the target firm's assets (Wollmann, 2019). Mergers in which the target firm's assets are below 50 million USD are generally exempt from scrutiny, presumably because mergers below this threshold are assumed to have no impacts on product market competition.[13] In general, however, most of the deals that the FTC and DOJ do get notified about are allowed to proceed without interference. Figure 1.12 reports the fraction of notifications that face some sort of antitrust enforcement. Most of these challenges by the DOJ and FTC do not lead to federal litigation, but instead the firms either modify the deal or abandon it altogether. On average, about 1.9 percent of all notifications face some enforcement from antitrust authorities.

[13] Wollmann (2019) finds that there was an increase in newly-exempt mergers after the threshold was moved from 10 million to 50 million in 2001, which suggests some firms will not go through a merger due to deterrence effects of antitrust scrutiny.

In practice, no merger has ever been challenged due to reducing competition in the labor market. However, challenging M&A due to anticompetitive impacts on labor markets does not require altering the current law (Naidu et al., Forthcoming; Marinescu and Hovenkamp, 2018; Hemphill and Rose, 2017). The Horizontal Merger guidelines state that the laws do not differentiate between "seller" power or "buyer" power. However, the guidelines analytical framework almost exclusively focuses on effects due to product market power.

While no merger has been ever been challenged, employers have been charged with anti-competitive practices in labor markets. For example, in 2017, a number of animation studios including Disney, Pixar, Dreamworks, Sony and 20^{th} Century Fox Animation were sued for agreeing not to poach workers from each other. The studios settled and agreed to pay $160 million USD to the impacted employees. Since the settlement, both Pixar and 20^{th} Century Fox Animation have been purchased by Disney. Therefore, any wage suppression that occurred due to the no-poach agreement between these firms would be completely legal, as these firms are all owned by the same parent company.[14]

1.3.2 Data

There are two datasets used for the analysis. First, I use the Longitudinal Business Database (LBD), an establishment-level dataset that covers the universe of non-farm employment in the United States. Second, I use the Longitudinal Employer Household Dynamics (LEHD) survey, a matched employee-employer dataset constructed from state unemployment insurance (UI) records. The version I have access to for this project covers 26 states in the United States. To prevent disclosure of potentially confidential information, the Census Bureau requires researchers to round estimates and observation counts.

[14] I thank Orley Ashenfelter for pointing out this example.

Longitudinal Business Database (LBD) Establishment-level Data

In the LBD, an establishment is defined as a specific physical location where business occurs. The LBD contains information on payroll, employment, industry, and location. In addition to establishment-level identifiers, the LBD contains enterprise-level identifiers, where an enterprise reflects all establishments under common ownership control.[15] Importantly for this project, when an establishment changes ownership, the enterprise identifier changes, while the establishment-level identifier remains stable. Therefore, M&A activity can be inferred by observing when enterprise-level identifiers change (Maksimovic and Phillips, 2001; Tate and Yang, 2016; Atalay et al., 2019).[16] Enterprise-level identifiers may also change when a single establishment becomes a multi-unit firm. Therefore, in order to identify M&A firms, I require either (1) the M&A event to involve multiple establishments or (2) the acquiring enterprise-level identifier to have existed in the years prior to the M&A event.

The key outcome variables are employment (which is equal to March 12^{th} employment) and total annual payroll. Given employment reflects the employment level on March 12^{th}, there is some ambiguity on the timing of the merger in relation to the outcome of interest. For example, imagine two firms merge in June 2001. In the data, I will observe that the ownership switches for the target firm between 2000 and 2001. However, measured employment in 2001 will reflect March 12^{th} employment, and therefore will not reflect any impacts of the merger. A merger that occurs in January of 2001, however, will reflect impacts of the merger.

[15] Unlike many administrative datasets, the enterprise identifier in the LBD is not based on tax identifiers (e.g. EIN numbers in the U.S.). Tax identifiers do not necessarily reflect the level of highest control, because some firms operate with multiple identifiers Song et al. (2018).

[16] Another way to identify M&A activity is to use the Thomson One database of Mergers and Acquisitions. However, in this case, the databases need to be matched based on firm name and location information. A fuzzy name matching algorithm yields a match rate of about 60 percent. Chains and franchises complicate the matching given the location from the SDC is often the headquarters, while in reality, all same-name establishments should be matched. The matching is also particularly problematic in conglomerates with complicated corporate structures. For example, if a subsidiary of a conglomerate is sold, one might unintentionally attribute the entire conglomerate being sold if the parent firm and subsidiary share a similar name. For example, in 2015 General Electric sold many divisions of its subsidiary company General Electric Financial to a number of different companies.

Therefore, in the analysis, the effect at year zero should be interpreted as a partial effect of the merger, given not all of the M&A establishments have actually been treated in this year. For further details on the LBD see Jarmin and Miranda (2002) and Haltiwanger et al. (2013).

Longitudinal Employer Household Dynamics (LEHD) Worker-Level Data

The worker-level data is drawn from the U.S. Census Bureau's Longitudinal-Employer Household Dynamics (LEHD) administrative files, which is used to construct quarterly workforce indicators (QWI) for local labor markets in the United States. The LEHD is constructed from state-level unemployment insurance files and includes worker-level information on quarterly earnings, employment, education, age, gender, and race, as well as information about the worker's firm, such as industry and location. While the LEHD partners with all 50 states, most projects are only approved for a subset of all states. This project utilizes data from 26 states (see Figure 1.1), which comprise about 53 percent of the total population in the United States as of the 2010 Census.[17]

The main outcome variable used in the worker-level results is log annual earnings which is aggregated across all employers. While earnings across all employers are included, I associate workers with the "dominant" employer (i.e. the employer for which the worker has the largest income). The firm-level variable in the LEHD is a State-Employer Identification Number (SEIN). A SEIN falls between an establishment and an enterprise identifier. Multi-unit enterprises may operate under multiple SEINs within a state, and a single SEIN may be associated with multiple establishments. In later results, I restrict the sample of workers to firm stayers, who are workers employed at the firm in the years following a merger. Given the firm-level variable in the LEHD is not necessarily invariant to ownership changes, I correct for false transitions in two ways. First, I use the entire sample of mergers identified in the

[17]The approved states are: AL, AZ, AR, CA, CO, DE, DC, HI, IL, IN, IA, KS, ME, MD, MO, MT, NV, NM, ND, OK, OR, PA, TN, TX, VA, WA.

LBD to correct for changing firm identifiers.[18] Next, I use worker flows between firms to capture reorganization events that are likely not true transitions, following Benedetto et al. (2007). For example, firms becoming incorporated may change tax identifiers. In practice, if more than 60 percent of the workers in a firm transition to the same firm in the next year, then I do not code any of these transitions as a job transition.

In later analyses, I construct average market-level earnings as the average earnings within in a commuting zone by industry cell after residualizing on worker observables (such as age, education, gender and race). Unlike the LBD, the location of the worker is sometimes ambiguous in the case of multi-unit enterprises. If an EIN owns only one establishment in a state, then the mapping from EIN to establishment is unique. For an EIN with multiple establishments in the same state, the assigned county of of the worker is the modal (employment-weighted) county. For example, if a given EIN employs 50 workers in Los Angeles, but 20 workers in San Francisco, then all of the workers in this EIN will be assigned to Los Angeles County in the LEHD.

I use the LBD to compute the true distribution of workers within an EIN across commuting zones. In the LEHD data, I then compute for every worker the probability the worker is employed in their assigned commuting zone (which is simply the number of workers in the EIN employed in that commuting zone divided by the total number of workers in the state employed in that EIN). For many workers, this is equal to 100 percent. In computing a market-level wage, I only include the workers that have at least 95 percent chance of actually being employed in that commuting zone. For example, in the example above, I would not include information from the workers in the EIN with 50 workers in Los Angeles and 20 workers in San Francisco, as for these workers it is uncertain which workers are employed in which location. In practice, market-level wages with and without this restriction lead to nearly identical results.

[18]For each establishment in the LBD, I use the Standard Statistical Establishment List (SSEL) to retrieve the associated EIN, which I can then link to SEINs in the LEHD.

1.3.3 Concentration Measurement

Relative to a standard HHI measure, the flows-adjusted concentrated measure (denoted C), requires computing transition rates across industries. A job in the LEHD is defined as any income earned at a given employer. For example, contractors that are hired by different firms will be coded as switching jobs (and in some cases, industries) very frequently. This will effectively increase the rate of cross-industry job mobility. Therefore, to compute transitions probabilities, I restrict to employment spells in which the worker is employed at the same firm for at least four quarters and require that annualized earnings exceed \$3,250. These restrictions are similar to Sorkin (2018) who uses transitions in the LEHD to measure compensating differentials across firms.

While, in theory, transition rates across industries may change, I instead choose to pool the entire sample (1995-2014) in order to retrieve a consistent and more precise measure of $\alpha_{m\to k}$ for every pair of industries m and k. To compute C in practice I make two modifications to the formula in Section 1.2.3. The model implicitly assumed there is one commuting zone and that firms only employed workers in a single industry. Allowing for multiple commuting zones and multi-industry firms changes the concentration measure slightly. In practice, I compute the share of firm j (denoted $\tilde{s}_{jmc}$) in industry m in commuting zone c as:

$$\tilde{s}_{jmc} = \frac{\sum_{k\in c} \alpha_{m\to k} l_{jkc}}{\sum_{k\in c} \alpha_{m\to k} L_{kc}} \tag{1.17}$$

where

$$\alpha_{m\to k} = \frac{P(k|m)}{P(m|m)} \frac{1}{\mathbb{E}[\frac{L_k}{L_m}]} \tag{1.18}$$

These are modified versions of Equations (1.14) and (1.12), respectively. First, the numerator of the market share is now a weighted total employment of firm j, indicating that firm j may hire workers in multiple industries. If jobs in industries m and k are relatively substitutable, then the market share of j in industry m will also depend on the number of

workers employed in industry k. If firm j employs a large number of workers in market k, then this will increase firm j's total share of market m.

Second, the relative size term in $\alpha_{m \to k}$ (i.e. $\mathbb{E}[\frac{L_k}{L_m}]$) is now the expected relative size of industries across commuting zones. To understand this factor, imagine there are two equally sized industries that use similar workers but are generally located in different areas. For example, imagine plastic manufacturing and rubber manufacturing plants hire similar workers, but plastic manufacturing primarily takes place in Texas while rubber manufacturing primarily takes place in Ohio. In this case, the aggregate relative size of the industries will be quite different than the expected relative size within a commuting zone given the two industries primarily operate in different commuting zones. Therefore, a low volume of flows between the two industries does not necessarily reflect low substitutability, but rather they are generally located in different areas.

The flows-adjusted local labor market concentration measure, C_{mc}, is defined as:

$$C_{mc} = \sum_{j \in c} (\tilde{s}_{jmc})^2 \tag{1.19}$$

One attractive feature of the C_{mc} measure is that it nests standard labor market definitions at the limits of worker mobility. If workers never transitions between industries, then C_{mc} is equal to an HHI index that uses an interaction between industry and commuting zone as the labor market definition. If workers transition randomly across industries, then C_{mc} is equal to an HHI index that uses commuting zone as the definition of a labor market.

Appendix 1.9.2 shows this result algebraically. The proof can be seen by examining Equation (1.18). With no mobility across industries, $\alpha_{m \to k}$ will be zero for all industries $k \neq m$. Therefore Equation (1.17) will be equal to simple employment shares in the industry-by-commuting zone cell. With random mobility, flows across industries are determined by the relative size of the industries, with larger industries mechanically attracting more workers.

In this case, $\alpha_{m \to k} = 1$ for all k and m, implying Equation (1.17) will be equal to simple employment shares in the commuting zone.

1.3.4 Matched Analysis Samples

I construct the M&A establishment-level analysis sample as follows. First, using enterprise-level identifiers I find every case in which the enterprise-level identifier changes for a given establishment to identify merger activity following past work (Maksimovic and Phillips, 2001; Tate and Yang, 2016; Atalay et al., 2019) between 1999 through 2009. In the LBD, firm identifiers also change when a single unit firm opens a new establishment and becomes a multi-unit firm. I immediately eliminate these cases as potential M&A events. Establishments belonging to the acquiring enterprise are defined as "acquiring" establishments, while those belonging to the sold firm are "target" establishments. In some cases, two firms switch to a new firm-level enterprise identifier, which can occur when two firms merge to join a completely new enterprise. In these relatively rare cases, I consider both firms target firms.

I begin with around 65,400 unique M&A events. In some cases, a firm will divest a portion or subset of all establishments to another enterprise. For example, in 2015, General Electric sold many divisions of GE Capital. I eliminate all "partial" mergers and acquisitions from the sample. This is done primarily because the worker-level data does not contain establishment-level identifiers. Therefore, in some cases it would not be possible to determine who in GE was employed in the target establishments that were sold. This eliminates about 1,500 mergers.

Next, I require the establishment to have an employment level greater than 50 workers and positive employment between years $[t-4, t-1]$. This done to focus on economically active firms with sufficient pre-period observations and eliminates a considerable number of small M&A events (50,000). While there could be potentially large effects on target workers in these acquisitions, the focus of this paper is on potential anticompetitive effects by considering how impacts vary by changes in local concentration. Small mergers will

mechanically have small impacts on concentration and may be very different than mergers between large firms of similar size.

Lastly, I restrict to mergers in which both firms are not too different in size. In particular, I require the target (or acquiring firm) to be at least 10 percent as large as the acquiring firm (or target firm). This is done so that the results are not dominated by extremely large acquiring firms that serial acquire smaller companies. This drops 6,800 events. In the end, these restrictions yield a final sample of 7,100 M&A events of relatively large and relatively stable firms.

I then match each establishment in the year prior to a M&A event to a "counterfactual" establishment in the same state and 4-digit NAICS industry as the M&A establishment. An establishment is a potential counterfactual establishment for firm j if: (1) the establishment is not part of a M&A event in year t, (2) the establishment has 50 or more employees in the year prior to the M&A event of the treated firm and positive employment in years $[t-4, t-1]$ and (3) the establishments are in the same size and average earnings decile in the year prior to the M&A event. Of all the possible counterfactual establishments for a given M&A establishment, I choose the establishment with the closest propensity score, where the propensity score is estimated by predicting treatment using a linear probability model with a quadratic in employment, a quadratic in payroll, a quadratic in establishment age, and an indicator for whether the firm is part of a multi-unit enterprise. This matching strategy is similar to a number of recent papers implementing a dynamic difference-in-differences research design (Jäger, 2016; Goldschmidt and Schmieder, 2017; Smith et al., 2019; Jaravel et al., 2018; He, 2018). The matching strategy finds a counterfactual establishment in about 64 percent of all cases.

Matching on size, earnings, state and industry finds establishments that would plausibly exhibit common trends in the absence of M&A activity. However, matching on industry and state is potentially problematic if mergers have impacts on local labor markets through

increased concentration.[19] If M&A has negative impacts on firms in the same industry and state, then the impact of M&A on establishments will be biased towards zero. As discussed previously, these spillover effects are potentially important in estimating the total impact of M&A on workers and will be directly estimated in Section 1.6. Choosing one counterfactual per control group ensures that the treated and control groups are balanced on the matched variables.[20] I construct a balanced panel of establishments which extends 4 years prior to the merger and 4 years after the merger. The main establishment level-outcomes are employment, which is equal to March 12^{th} employment.

To construct the worker-level sample, I extract all workers that were employed in the M&A firms in the two years prior to the M&A event. This tenure restriction is chosen to obtain a sample of workers with attachment to the M&A firm and is similar (though shorter) than tenure restrictions used in the mass layoff literature (Jacobson et al., 1993; Von Wachter et al., 2009; Lachowska et al., 2018). For each worker in the treated firms, I choose a worker in the same 4-digit NAICS industry, state, age bins (5 year bins), gender and firm size decile. I chose not to match workers based on earnings, given this is the endogenous outcome of interest, but results are of the same sign and significance for a matching procedure that matches explicitly on earnings. Again, if more than one match is found I choose the worker with the closest propensity score to the treated worker, where the propensity score is estimated by predicting treatment using a linear probability model with a quadratic in firm size, firm age, and worker age. In total, a counterfactual worker is found for about 72 percent of the treated M&A workers. To compute earnings in the worker-level data, I aggregate earnings across all employers if a worker is employed at more than one firm. As mentioned previously, the worker-level data only provides partial coverage of the U.S.

[19] In other words, the stable unit treatment value assumption (SUTVA) may be violated in this setting.

[20] An alternative to choosing one counterfactual is to choose all counterfactual establishments that meet the matching criterion, and then weight the data appropriately to balance the treated and control units. I chose to focus on one counterfactual as it simplifies weighting issues that occur when considering subsample splits, in which the weights would need to change across specifications.

Therefore, a number of M&A events occurring outside LEHD coverage are dropped from the worker-level analysis. To be included in the worker-level sample, I require both the target and acquiring firm to be present in the LEHD.

1.3.5 Summary Statistics

Figure 1.2 plots the number of workers employed in the M&A establishment sample over time on the left axis. The number of workers employed in the M&A sample establishments fluctuates widely over time, with a high of 1.5 million to a low of 0.5 million, with merger activity being somewhat procyclical. I also plot the number of M&A deals in the Thomson Reuters (SDC) database of Mergers & Acquisitions, a high-quality database that contains information on merger activity in the United States as well as characteristics of merger deals. As can be seen in Figure 1.2, the two time-series line up reasonably well. One important note, however, is that the M&A establishment sample from the LBD does make restrictions by eliminating small acquisitions and partial acquisitions and therefore is a subset of the total number of workers impacted by ownership changes. With no restrictions on sample size, I find about 2 percent of workers a year are employed in an establishment that changes ownership at some point over the year.

Panel A of Table 1 contains the summary statistics for M&A establishments and the matched control establishments. In total, there are about 46,000 treated M&A establishments belonging to 10,000 unique firms. The average annual payroll for M&A establishments is equal to 11 million USD, while it is equal to 10.3 million USD for control establishments. The M&A establishments are slightly larger on average (250 vs. 240) and have similar earnings per worker (43.9 thousand USD vs. 42.8 thousand USD). About 32 percent of establishments are target establishments, implying acquiring firms in general own more establishments than target firms.

Panel B of Table 1 reports the industries of the M&A and control establishments. About 17 percent of all establishments are in the manufacturing sector. Other prominent sectors

include health care (10 percent) accommodation and food (10 percent) and finance (9 percent). A key source of variation used to disentangle product market effects will be to compare effects in tradable vs. nontradable industries. I follow Berger et al. (2019) and Delgado et al. (2014) and define tradable goods as NAICS two-digit codes: 11, 21, 31, 32, 33, and 55. Codes 31-33 are manufacturing and make up the bulk of the tradable industries.[21] In total, about 24 percent of all M&A establishments are in a tradable industry.

Panel C of Table 1 reports characteristics of the M&A deal. In total, only 29 percent of establishments are in commuting zones in which the other firm involved in the merger owns at least one establishment. Because the local labor market concentration measure is measured at the commuting zone level, this implies that roughly 71 percent of establishments involved in mergers experience no change in local labor market concentration due to the merger. This will be an important source of variation when disentangling alternative channels. The average change in flows-adjusted labor market concentration due to the merger (including zeros) is about 1 percent. Conditional on some positive increase, the average impact is around 5 percent.

Table 2 includes information on the worker-level data. In total, there are about 2,000,000 workers in the sample. This is about 18 percent of what would be expected from the establishment-level counts of employment. The reason the worker-level sample is lower than expected is due to three reasons: (1) the LEHD covers only 26 states, and therefore a large number of mergers are dropped from the sample if either the target or acquiring firm is in one of the states without coverage and (2) the worker-level sample restricts to workers with two years tenure therefore dropping workers with short tenure and (3) workers without a valid matched control are dropped from the analysis (this occurs in 28 percent of cases).

On average, incumbent workers in the M&A firms earn about 55,170 USD per year, while control workers earn roughly 52,400 USD per year.[22] 46 percent of the workers are female.

[21] 11 is agriculture, forestry and fishing, 21 is mining, quarrying and oil and gas extraction, 31-33 are manufacturing and 55 is management of companies and enterprises.

[22] All earnings are adjusted to 2011 dollars.

About 32 percent of M&A workers have a college degree while 31 percent of control workers have a college degree.[23]

1.4 Research Design

To estimate the impact of M&A on establishment-level outcomes, I estimate a matched difference-in-differences design of the following form:

$$Y_{jt} = \sum_{k=-4}^{4} \delta_k^{MA} \mathbb{1}(t_j = t^* + k) \times MA_j + \psi_j + \tau_t + u_{jt} \tag{1.20}$$

where Y_{jt} is an outcome variable (primarily log employment), MA_{jt} is an indicator for an M&A establishment, $\mathbb{1}(t_j = t^* + k)$ indicates an M&A event occurred k years in the past (or future) relative to the period of the M&A event t^*, ψ_j are establishment fixed effects, τ_t are year fixed effects that vary by the year of the M&A event and u_{jt} is an error term.[24] All standard errors are two-way clustered at the 4-digit NAICS and commuting zone level.[25] I omit the year prior to the merger so that each δ_k^{MA} measures the difference in the outcome variable Y_{jt} between the M&A establishments and counterfactual establishments relative to the difference that occurred in year $t = t^* - 1$. Regressions are weighted by the employment level of the establishment in the year prior to the merger to make them comparable to the worker-level results.[26] To summarize findings, I often report the average impact which is equal to $\frac{1}{4}\sum_{k=1}^{4} \delta_k^{MA}$.

[23] Education is imputed for a large portion of workers in the LEHD. This is done by linking the LEHD to the Decennial Census. State-specific logit models are then estimated to predict the education level. The variables included in these models are age categories, earnings categories and industry dummies.

[24] Other specifications, such as matched pair by event-time fixed effects, yield results with similar sign and significance.

[25] Clustering at the merger-level results in similar standard errors.

[26] As shown in Solon et al. (2015), when the parameter of interest is a treatment effect, it is not clear that weighting by population is justified. Therefore, in Column 2 of Table 3 I provide unweighted results. The magnitude of the effect is smaller in these results due to larger firms experiencing greater declines in employment after mergers. See Table 4 for results that split by median establishment size in the year prior to the merger.

A key outcome of interest is the effect of an M&A event on earnings of workers. Given the potential for changes in composition after a merger, I estimate this impact using worker-level data from the LEHD. In particular, I estimate the same matched difference-in-differences design at the worker-level:

$$y_{it} = \sum_{k=-4}^{4} \delta_k^{MA} \mathbb{1}(t_i = t^* + k) \times MA_i + \omega_i + \tau_t + u_{it} \tag{1.21}$$

where y_{it} is an outcome variable for incumbent worker i in time t, ω_i are worker fixed effects, with all other variables being defined as in Equation (1.20). All standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

A recent literature discusses a number of identification and interpretation issues that arise when using the timing of treatment to identify a treatment effect. By using a matched control group that is never treated, the specifications above do not suffer from the identification issues that arise in conventional event-study designs with never-treated units (Borusyak and Jaravel, 2017) or difference-in-differences designs with staggered timing (Goodman-Bacon, 2018). Identification here comes solely from differences in always-treated and never-treated units over time, not from units coming in and out of treatment.

1.4.1 Identifying Assumptions

The key identifying assumption is that outcomes for M&A establishments and workers would follow similar trajectories to control establishments and workers in the absence of a merger. This may be a strong assumption in this setting, as mergers are the result of endogenous decisions by firms. For example, acquiring firms may selectively target firms that will be profitable in the future. If a technology startup is acquired after a successful innovation, then we would expect the target firm to grow due to the increased productivity even absent any merger. Therefore, the estimate of the impact of M&A on employment and earnings would be biased upwards. On the other hand, acquiring firms could target mismanaged businesses

that are underperforming. If targets are chosen in such a way, we might expect employment and earnings to be falling in target firms before the merger. Therefore, the estimate could be downward biased if falling employment at target firms would have been even greater in the absence of the merger.

A simple way to gauge the direction of the potential bias is to compare outcomes for M&A establishments and workers to the control establishments and workers in the years prior to the M&A event. Importantly, the matching strategy does not match on trends of any outcome variables. For worker-level results, I do not match on lagged earnings (the primary outcome), though strategies that do match explicitly on earnings yield results with similar sign and significance.[27]

However, while common trends is reassuring for a causal interpretation, shocks that occur contemporaneously with M&A events could still bias the results. For example, imagine a negative demand shock hits a commuting zone and causes both a decline in employment as well as an increase in merger activity as establishments are purchased before they go out of business. In this case, merger activity is correlated with shocks that decrease demand. Of course, the opposite could be true. In fact, in the aggregate, merger activity tends to be procyclical (Rhodes-Kropf and Viswanathan, 2004).

One way to alleviate this concern is to focus analysis on mergers that are less likely to have been triggered by local economic conditions of the establishment. To do so, I also consider the impact in mergers between national firms that operate in at least 5 commuting zones, the logic being that these changes in ownership are less likely to be driven by the local conditions of the establishment or workers.

[27]This is the preferred wording describing qualitative results that have been approved by the Census disclosure review board but are based on quantitative results that have not gone through the full review process.

1.5 Effect of M&A on Establishments and Incumbent Workers

1.5.1 Effect of M&A on Establishment-Level Employment

Panel A of Figure 1.3 plots $\hat{\delta}_k^{MA}$ from estimating Equation (1.20) with log employment as the outcome. As can be seen in the figure, the trends in log employment between M&A establishments and matched control establishments are similar in the years prior to the merger. As discussed previously, establishments are partially treated at time $k = 0$. In this year, log employment falls by -0.051. The year after the merger, the effect grows to -0.115 with a slight downward trend over time. The average impact in the four years after the merger is equal to -0.144 (SE=0.021), which corresponds to a 13.4 percent decline in employment.

Table 3 shows this decline in establishment size is robust across different measures of size and weighting schemes. First, Column 2 presents unweighted results and finds a smaller, but still significant -0.081 (SE=0.015) fall in log employment. Column 3 uses raw employment (including zeros) rather than log employment and finds a roughly 13.4 percent decline in employment in M&A establishments.

Table 4 further explores the robustness of these results by isolating changes in ownership that are likely not driven by the idiosyncratic factors of a given establishment. In Column 1 of Table 4, I find that in national mergers between firms operating in multiple commuting zones, log employment falls by -0.185 (SE=0.027). The decline is similar in establishments that experience a "quasi-exogenous" change in ownership (Atalay et al., 2019). An establishment is defined as experiencing a quasi-exogenous changes in ownership if the establishment is in secondary or tertiary lines of business when two multi-industry firms merge. Columns 4-7 of Table 4 show that the negative effect of M&A on employment is much larger at target establishments and establishments that are initially larger.

Lastly, while this analysis shows employment at incumbent establishments fall, one possibility is that M&A firms are more likely to open new establishments. The employment

gains at these new establishments could outweigh the employment losses at incumbent establishments. For example, Davis et al. (2014) find that after private equity buyouts, (a specific type of acquisition in which a financial sponsor acquires a company) employment at incumbent target establishments falls significantly, but total firm employment falls only slightly. To test this mechanism, I compute firm growth rates for acquiring firms in the LBD that correct for growth through mergers and acquisitions using the procedure in Davis et al. (2014) (See Appendix 1.8 for details). As can be seen in Appendix Figure 1.15, I find that firm growth rates drop by about 10 percent the year after the merger relative to the controls firms, a gap that narrows in the years following the merger. These results imply that the large drop in employment at incumbent establishments is not made up by large increases in employment at new establishments opened by M&A firms.

1.5.2 Effect of M&A on Incumbent Worker Outcomes

Given the considerable turnover at M&A establishments, changes in average establishment earnings may reflect changes in worker composition. Therefore, I next turn to the worker-level data that allows me to control flexibly for composition by tracking the same workers over time.

As can be seen in Panel of A Figure 1.4, earnings for M&A workers trend similarly to the control workers in the years prior to the merger, but fall gradually after the merger. The average effect in the 4 years after the merger is equal to -0.011 (SE=0.004). This decline could be due to M&A workers being displaced and moving to lower-paying firms or M&A firms reducing wages for their incumbent workers. While the large drop in employment at M&A establishments suggest large displacement effects, the reduction in employment could come primarily through decreased hiring, implying incumbent workers may be relatively unaffected.

To test for displacement effects, I consider the impact of M&A on the probability a worker transitions from a job. This transition could be to another firm in the LEHD, to

a firm outside the LEHD coverage, or to non-employment. In practice, I cannot discern between a transition to a firm outside the LEHD coverage or non-employment. Panel B of Figure 1.4 plots estimates of Equation (1.21) with an indicator for a job transition as the outcome. Job transitions spike in the year of the merger. The year after the merger, an M&A worker is 10 percentage points more likely to have switched jobs than a control worker. Therefore, the roughly 13 percentage point drop in employment can be attributed mostly to increased job separations for incumbent workers. Given this large increase in job separations, part of the effect on earnings may be coming from job displacement rather than within-firm decreases in earnings.

To study the impacts solely due to within-firm changes in compensation, Panel C of Figure 1.4 restricts the analysis to firm stayers, who are workers that stay in the same firm in the years following the merger. I make this restriction for both M&A workers and control workers so that the treatment group does not mechanically contain workers that have more stable job histories. Log annual earnings for firm stayers in M&A firms decrease by -0.008 (SE=0.003) in the years following the merger. Interestingly, this effect is not significantly different than the -0.011 decrease for all workers, a seemingly contradictory finding to the large losses commonly documented in the mass layoff literature. One potential explanation is that the mass layoffs studied in the prior literature may be more common in years or industries with declining demand, for which it may be difficult for workers to find a suitable alternative job. Workers in M&A firms may be better situated to find equally well-paying jobs quickly.[28]

1.5.3 Potential Mechanisms and Heterogeneity

The Role of Local Labor Market Concentration

Declining employment and earnings at M&A firms can be rationalized through changes in production technology, changes in product market power, or changes in monopsony power.

[28] He (2018) finds similar results for firm stayers and firm leavers in Denmark.

I now use variation across mergers to disentangle these channels. The focus here is to understand how compensation policies within M&A firms change after the merger. Therefore, I report estimates for firm stayers that do not reflect any displacement effects.

I explore heterogeneity in three key dimensions: the size of the change in concentration, the initial concentration level, and whether the firm is in a tradable industry. First, mergers below the top-quartile (ordered by changes in concentration) have roughly zero impact on local labor market concentration (most of these are between firms operating in different commuting zones). I refer to these as "low-impact" mergers. Of course, there could still be large effects in these mergers in principle. New management practices could increase wages through productivity increases.

Following the model in Section 1.2.3, concentration changes should have larger impacts on wages in already concentrated markets. Therefore, I split top quartile mergers into two separate groups: mergers that occur in markets with below-median concentration markets are referred to as "medium-impact" mergers while mergers that occur in above-median level of concentration are referred to as "high-impact" mergers. Lastly, in many industries increases in local concentration are likely to increase both product and labor market power. To isolate labor market power, I also present results for tradable industries only. The logic for restricting to tradables is that prices for goods sold on a national or international market are less likely to be impacted by a single merger.

In Panel A of Figure 1.5, I find low-impact mergers result in an insignificant -0.005 (SE=0.004) decline in log annual earnings for incumbent M&A workers. In medium-impact mergers log annual earnings fall by -0.008 (SE=0.007). In contrast, in high-impact mergers, log annual earnings fall by -0.031 (SE=0.011). Columns 4-5 of Table 6 shows that the differences between these impacts are statistically significant.

These results support two main conclusions. First, mergers that only impact productivity (i.e. low-impact mergers) there is almost no change in earnings. Firms often argue that mergers with potentially anticompetitive impacts should be allowed based on intended pro-

ductivity gains. If these gains are realized, it does not appear that they spillover to workers. Second, the results are consistent with both market power or monopsony power resulting in lower wages for workers. There are negative impacts in mergers that increase concentration, with almost the entire impact being driven by mergers in already concentrated markets.

To isolate monopsony power, Panel B of Table 6 reports results for tradable goods industries, for which I assume there is no impact of a merger on product market power. In high-impact mergers, I continue to find an economically meaningful decline in log annual earnings of -0.067 (SE=0.023). In medium-impact mergers, I find a slight positive rise in log earnings of 0.001 (SE=0.012), while in low-impact mergers I find a marginally significant decline of -0.012 (SE=0.006). Columns 4-5 of Table 6 shows that the differences between these impacts are statistically significant.

Robustness of Wage Losses Only in High-Impact Mergers

One potential concern is that mergers that increase concentration in already concentrated markets are different in some unobservable way than mergers that do not increase concentration or mergers in unconcentrated markets. For example, imagine a negative demand shocks hits a commuting zone and many firms go out of business, resulting in an increase in local labor market concentration that pushes this labor market into the above-median concentration group. Additionally, imagine some low-productivity target firms are sold to high-productivity acquiring firms in the same area before they go out of business. In this case, high-impact mergers would be correlated with negative demand shocks.

To alleviate this concern, I also explore heterogeneity in a sample of national mergers between firms that operate in multiple commuting zones. In these mergers, the motive for the merger is unlikely to be driven by local economic conditions that are correlated with high-impact mergers. I find a very similar pattern of heterogeneity in these results. In Panel C of Table 6, I find low-impact mergers cause an insignificant -0.008 decline in log annual earnings, medium-impact mergers cause a marginally significant -0.013 decline in log annual

earnings, and high-impact mergers cause a -0.042 decline in log annual earnings, an effect significant at the 1 percent level.

A related concern is that mergers that increase concentration tend to be between firms that are much larger on average. Panel B of Appendix Table 13 estimates the effect on firms with above-median level of employment to ensure the effects are not driven by low-impact mergers also being driven by the smallest firms. Here I continue to find a similar pattern of heterogeneity across low-impact, medium-impact and high-impact mergers.

Turning to isolating labor market power, one concern is that there could be an effect on prices in tradable industries if the industry is very concentrated. Therefore, I would be misinterpreting the finding that the differential impact on earnings in high-impact mergers in tradables is driven by increased monopsony power. In Panel A of Appendix Table 13, I estimate the impact in mergers in tradable industries with a product market national HHI less than 0.05 (for comparison, the Horizontal Merger guidelines consider an HHI of 0.15 to be moderately concentrated) and continue to find very similar results.

Lastly, as discussed in Section 1.3, for multi-unit firms it is sometimes ambiguous which commuting zone a worker is actually employed in within a state, therefore, I could be misclassifying some workers leading to measurement error. In Panel C of Appendix Table 13 I restrict to workers in which the location of employment is known with certainty and continue to find similar results.

To summarize, concentration plays a key role in explaining heterogeneity impacts of M&A on workers' earnings. Impacts are consistently largest in mergers that increase concentration in already concentrated markets. This is true in industries with highly tradable goods as well as when restricting to national mergers for which the local economic conditions likely did not trigger the M&A event. However, these results likely understate the impact of mergers on workers for due to potential spillovers on non-merging firms hiring in the same labor market. Increased local concentration will lower the wages for all firms in the market, not just for the merging firms. These market-level effects are the focus of the next section.

1.6 Market-Level Impacts of Increased Concentration

1.6.1 Overview

In the first part of this section I explore trends and correlates of the flows-adjusted concentration measure C and discuss how these descriptive results relate to the recent labor and macroeconomics literature on the relation between concentration and labor market outcomes. I make two main points that suggest caution in using correlations between local concentration and earnings to make inference about how changes in market structure impact workers. First, while increases in local concentration within a market are correlated with declining earnings, they are also correlated with *increases* in employment. This positive correlation with concentration and employment is inconsistent with concentration changes increasing monopsony power. Second, most variation in concentration within a market is *not* driven by merger activity, but other factors such as entry or exit.

In the second part I use variation in concentration due to merger activity as an instrument for local market concentration. This estimates the impact of *merger-induced* changes in concentration on labor market outcomes. I argue this variation identifies a well-defined and theoretically relevant elasticity of earnings with respect to local labor market concentration.

1.6.2 Trends in Local Labor Market Concentration

To begin, I compute average (employment-weighted) concentration measures over time for three different measures of local labor market concentration. As can be seen in Figure 1.6, the average (employment-weighted) HHI when the labor market is defined as an industry (4-digit NAICS) within a commuting zone is equal to about 0.14 over the sample period with a slight downward trend. Using the Horizontal Merger Guidelines as a guide, this implies that the average labor market is close to moderately concentrated (0.15). The fact that local concentration has been decreasing over time is consistent with a number of recent papers (Benmelech et al., 2018; Rinz, 2018; Rossi-Hansberg et al., 2018; Berger et al., 2019).

Next, I compute the average (employment-weighted) concentration level using a commuting zone as the definition of the labor market. I find the average level of concentration is around 0.004, not even close to moderately concentrated. This is perhaps not surprising given that most commuting zones have relatively large populations, making it difficult for individual firms to dominate employment. The fact that most regions have many employers is one reason why historically antitrust may not have focused on labor market impacts (Naidu et al., Forthcoming).

Lastly, I compute the average (employment-weighted) value of the flows-adjusted concentration measure C. I find that the average C is around 0.05 over the sample period, significantly lower than the industry by commuting zone HHI. Therefore, concentration definition clearly matters for levels. In Appendix 1.10, I also show that while the measures are correlated at a market-level, there are important differences. Most importantly, small industries are not mechanically more concentrated according to C because C adjusts for flows out of the industry.

The low levels of concentration according to the flows-adjusted concentration measure C as well as the declining concentration over time might suggest a limited role for monopsony power in explaining wage dispersion or aggregate macroeconomic trends such as the falling labor share. However, this conclusion is premature. As discussed in Section 1.2, the relationship between concentration and wages (as well as the labor share) is nonlinear. Consistent with this prediction, I found small effects in mergers in markets with below-median levels of concentration. Therefore, declines in average concentration do not necessarily imply that wages overall would increase due to declined monopsony power. I will return to this point in Section 1.6.8 which uses the simple model of Cournot competition to translate changes in the distribution of concentration into implied wage changes.

1.6.3 Correlates of Concentration Changes

To understand how changes in concentration relate to real-world outcomes, I regress changes in labor market concentration on changes in market-level employment and earnings:

$$\Delta Y_{mt} = \beta \Delta \tilde{C}_{mt} + \tau_t + u_{mt} \tag{1.22}$$

where Y_{mt} is a market-level outcome, $\Delta \tilde{C}_{m,t}$ is the change in log of the flows-adjusted labor market concentration, and τ_t are year fixed effects. The regression is weighted by employment and standard errors are clustered at the market level.

To construct the average log market-level earnings in the LEHD ($\tilde{w}_{mt}$), I first estimate a Mincer-style regression of the following form at the worker level:

$$\tilde{w}_{it} = \Phi_{mt} + \beta_t X_{it} + u_{it} \tag{1.23}$$

where $\tilde{w}_{it}$ is the log annual earnings of worker i at time t, Φ_{mt} are labor-market fixed effects (i.e. 4-digit NAICS by commuting zone cells), and X_{it} contains worker-level observables including a polynomial in age, race, gender and education.[29] This regression is estimated every year (hence β_t) so that returns to characteristics can vary across years. The average market wage ($\tilde{w}_{mt}$) is equal to the fixed effect $\hat{\Phi}_{mt}$. As discussed in Section 1.3, for workers in EINs that employ workers in multiple commuting zones within a state, it is sometimes not possible to determine the commuting zone of employment for a given worker. In practice, I restrict to workers that have at least a 95 percent chance of actually working in the listed commuting zone (See Section 1.3 for more details), which is computed using the true

[29] Education is imputed for about 80 percent of workers in the LEHD. The imputation procedure is performed by the Census and is done by linking the LEHD to the Decennial Census. State-specific logit models are then estimated to predict the education levels for all workers with missing education using the following set of observables: age categories, earnings categories, and industry dummies.

distribution of workers across commuting zones in the LBD. However, the premiums with and without this restriction are similar and do not impact the market-level results.

Column 1 of Table 7 finds an elasticity of earnings with respect to the flows-adjusted concentration measure equal to -0.099 (SE=0.005), similar to results found in prior work (Azar et al., 2017; Benmelech et al., 2018; Rinz, 2018). Using a more standard HHI based on 4-digit NAICS by commuting zone yields very similar results (-0.085). These results are consistent with increased local labor market concentration resulting in higher monopsony power which leads to lower wages for workers.

However, column 3 of Table 7 displays the results with log market-level employment as the outcome. In stark contrast to the earnings results, I find that increases in concentration are correlated with *increases* in market size. The elasticity of employment with respect to C is equal to 0.31 (SE=0.010). The fact that market size increases with concentration is inconsistent with concentration increasing monopsony power. If increases in concentration are driven mostly by mergers and acquisitions, and mergers and acquisitions increase monopsony power, then these results appear inconsistent with the incumbent worker results in Section 1.5. However, in the next section I show that M&A drives a relatively small fraction of the total variation in local concentration over time.

1.6.4 What Drives Changes in Concentration

To understand how much variation in concentration is due to ownership changes, I compute ΔC_{mt}^{MA} as the change in concentration that would occur at time t in market m if employment at every establishment remained exactly the same as at time $t-1$, but the owners of establishment are set to the time t owners. I then estimate the following first-differences regression:

$$\Delta C_{mt} = \beta \Delta C_{mt}^{MA} + u_{mt}$$

where ΔC_{mt} is the actual change in concentration in market m at time t. Column 1 of Table 8 shows that changes in concentration due to ownership changes predict actual changes in concentration. The coefficient on ΔC_{mt}^{MA} is about 0.834 (SE=0.032). However, changes in ownership alone explain very little of the overall variation in concentration changes. The R^2 of this regression is equal to about 0.014 indicating that most of the changes in concentration in practice are not due to changes in ownership. Next, I construct ΔC_{mt}^{exit} and ΔC_{mt}^{entry} which are changes in concentration predicted by exit and entry, respectively. In Column 2, I find a coefficient of 0.778 (SE=0.016) on ΔC_{mt}^{exit} with a corresponding R^2 of 0.254. Therefore, relative to changes in ownership, exit explains much more of the variation in concentration within a market over time. In Column 3, I find a coefficient of 0.981 (SE=0.012) on ΔC_{mt}^{exit} with a corresponding R^2 of 0.107.

Lastly, I construct $\Delta C_{mt}^{reallocation}$, which is the change in concentration due to reallocation of employment across existing establishments, which can occur due to entry, exit, expansion, or shrinking. The coefficient on this factor is equal to 0.946 (SE=0.007) with a corresponding R^2 of 0.915. Note that because concentration is a nonlinear function of market employment, a regression with both $\Delta C_{mt}^{reallocation}$ and ΔC_{mt}^{MA} does not lead to an $R^2 = 1$ mechanically.[30]

Given that most changes in local concentration are not driven by ownership changes, it is unclear how the correlation between local concentration and earnings can be used make inference about anticompetitive impacts of changing market structure. In the next section, I address this issue by using merger activity directly as an instrument for local concentration.

1.6.5 Market-Level Merger Sample

In this section, I identify the impact of local concentration on market-level earnings by comparing the evolution of average market earnings and employment for markets that experience smaller merger-induced concentration changes to markets that experience larger merger-

[30] In other words, knowing ΔC_{mt}^{MA} and $\Delta C_{mt}^{reallocation}$ is not enough information to construct the actual change in concentration.

induced concentration increases. Therefore, while merger activity may itself be endogenous, the identification strategy conditions on a market experiencing some merger activity, with the identifying variation coming from differences in the size of the concentration changes across markets.

To construct the market-level sample, I follow a similar procedure as the establishment and worker-level sample. For each year t, I compute the predicted change in log market concentration in every market m due to merger activity, denoted $\tilde{C}_{mt}^{MA}$. I define a concentration event as a change in concentration of at least one percent.[31] For each concentration event I construct a 4-year window around the event, just as in the worker and establishment-level results. For 92.5 percent of markets, there is only one event during the sample period. For markets that experience multiple events, I follow Lafortune et al. (2018) and create duplicate observations, one duplicate associated with each event year. Approaches utilizing only the first event, the largest event, or dropping all multiple event markets yield results with the same sign and significance.

An alternative to this approach is to allow exposure to merger-induced concentration to accumulate over time within a market. I prefer to use the specification that breaks labor markets that experience multiple events into different observations with different corresponding event years because this provides a transparent way to validate the identification strategy by comparing outcomes before and after the concentration event. In total, I identify roughly 3500 merger-induced concentration events in the LEHD data.

[31] The reason positive changes below 0.01 are not considered "concentration events" is due to how the flows-adjusted concentration measure C is constructed. Because the concentration in market m depends on all industries m is connected to by labor mobility, a single merger affects many markets. Most of the changes though are very small. Therefore, using any positive change leads to an extremely large number of markets being impacted by merger activity, but the overwhelming majority of these increases are nearly zero.

1.6.6 Do Earnings Decrease in Markets with Larger Increases in Concentration?

To begin, I first test whether larger increases in concentration are associated with larger declines in market earnings. To allow for the effect to depend flexibly on the size of the concentration change, I fit an interacted difference-in-differences model of the following form:

$$\tilde{w}_{mt} = Post_{mt} \times \left[\sum_{b=1}^{4} s_b(\tilde{C}_m^{MA}) \right] + \Phi_m + \tau_{t,k(m)} + u_{mt} \tag{1.24}$$

where $\tilde{w}_{mt}$ is the average log market wage obtained by first residualizing on worker observables, as described in Section 1.6.3. To focus on spillovers and net out any direct impacts on the merging firms, I omit the merging firms when constructing the average market wage for the primary results. Φ_m are labor-market fixed effects (i.e. 4-digit NAICS interacted with commuting zone), $\tau_{t,k(m)}$ are year fixed effects that potentially vary by some observable of the labor market m. The preferred specification interacts year fixed effects with consolidation year and 1-digit NAICS by state cells. Therefore, the impact of concentration on earnings is identified from two merger-induced concentration changes that occur in the same year, within the same state, same 1-digit industry, but have different magnitudes of changes in the flows-adjusted concentration measure $\tilde{C}_m^{MA}$.[32] To make the results comparable to the worker and establishment-level results, most specifications weight by employment in the period prior to the concentration increase, though I also present unweighted results.

The function $\{s_b(.)\}_{b=1}^4$ is a set of basis functions defining a natural cubic spline with four knots. Following Harrell (2001), I place the knots at the 5^{th}, 35^{th}, 65^{th}, and 95^{th} percentiles of the distribution of concentration changes.[33] The "dose-response" function

[32] In Appendix 1.11, I shows this strategy estimates a weighted average of treatment effects across 1-digit NAICS by state cells. 1-digit NAICS by state cells with more variation in concentration changes will receive relatively larger weight.

[33] As per Census restrictions, percentiles cannot be reported. Instead I report psuedo-percentiles, which correspond to the averages of the percentiles around the knot. For example, the psuedo-95^th percentile is the average of the 94^{th}, 95^{th} and 96^{th} percentile. The pseudo-knots are equal to 0.011, 0.018, 0.037, 0.21. The spline is restricted to be linear below the 5^{th} and above the 95^{th} percentile.

$d(x) = \sum_{b=1}^{4} s_b(x)$ gives the effect of concentration change equal to x on the market-level wage. This specification can be interpreted as a nonlinear reduced form in which $\tilde{C}_m^{MA}$ is the instrument for actual concentration. The specification is similar to Kline et al. (2019) who use patents as an instrument for firm surplus.

Figure 1.7 plots the dose-response function over a grid of values of $\tilde{C}$. As can be seen in the figure, at low values of concentration changes, there is no impact on market-level wages. At concentration changes above 0.21, there are negative impacts that increase in absolute value as the concentration changes grow larger. The value of 0.21 corresponds to roughly the 95^{th} percentile of concentration changes. This implies only the top ventile of concentration increases generate significant shifts in market-level wages.

1.6.7 Market-Level Difference-in-Differences Estimates

The fact that larger changes in concentration generate larger shifts in outcomes could partially reflect different pretrends between markets that experience large vs. small changes in local labor market concentration. Motivated by the analysis in the last section, I compare outcomes for concentration changes in the top-ventile of all concentration changes vs. all other concentration changes. Appendix Table 14 presents summary statistics that compares these markets. On average, top-ventile concentration increases are more likely to occur in manufacturing industries and southern states, but the markets themselves are composed of workers with similar education, similar age, and similar gender composition.

First, I test whether mergers actually create persistent increases in local labor market concentration. As discussed earlier, concentration changes are not primarily driven by ownership changes. Therefore, changes in concentration due to mergers may be swamped by other changes in the market. If mergers incentivize more entry (for example, if the merged firm raises price, then more firms may enter), then the increase in concentration may be transitory. To estimate the dynamic impacts of mergers on concentration, I estimate a dynamic

difference-in-differences specification of the following form:

$$C_{mt} = \sum_{k=-4}^{4} \delta_k^C \mathbb{1}(t_m = t^* + k) \times Q20_m + \Phi_m + \tau_{t,k(m)} + u_{mt} \tag{1.25}$$

where $Q20_m$ indicates the market is involved in a concentration change in the top ventile of all concentration changes. Panel A of Figure 1.8 plots the coefficients $\hat{\delta}_k^C$ from estimating Equation (1.25). In the year after the merger, concentration jumps significantly in $Q20_m$ markets (18 percent), an effect that remains flat over time. This shows that mergers can generate significant increases in market concentration that persist over time. In other words, there is a strong first stage using top-ventile mergers as an instrument for local labor market concentration.

Next I turn to the impact on market earnings by estimating Equation (1.25) with the average market-level earnings $\tilde{w}_{mt}$ as the outcome. Panel C of Figure 1.8 plots the results. On average, log averge earnings in the top ventile markets fall by -0.034 (SE $= 0.013$) after the concentration event.

Elasticity of Earnings with Respect to Local Labor Market Concentration

Finally, I estimate the elasticity of earnings with respect to concentration in a two-stage least squares regression of the following form:

$$\tilde{C}_{mt} = \Phi_m + \tau_{t,k(m)} + Q20_m \times Post_{mt} + u_{mt} \tag{1.26}$$

$$\tilde{w}_{mt} = \Phi_m + \tau_{t,k(m)} + \beta \tilde{C}_{mt} + u_{mt} \tag{1.27}$$

where Equation (1.26) is the first-stage regression with an indicator for a top-ventile change interacted with post-merger indicator as the excluded instrument. In Column 1 of Table 9, I find top-ventile changes increase log concentration by 0.175 with a corresponding F-statistic equal to 16. In Column 1 of Table 10, I find the elasticity of earnings with respect to concentration is equal to -0.22 (SE=0.094).

As in the worker results, I find that this effect is driven entirely by markets with above the median level of concentration. In Column 4 of Table 10, I find the elasticity of earnings with respect to concentration is equal to -0.259 (SE=0.108) in above-median concentration markets. However, the elasticity is 0.059 (SE=0.121) in below-median concentration markets. Therefore, consistent with the theoretical model as well as the Horizontal Merger guidelines, increases in concentration have no impact on earnings in low-concentration markets, but relatively large effects in high-concentration markets.

While the common trends in the event-studies corroborate the causal interpretation of these results, merger activity is not random across markets. Variation in concentration changes across markets could be correlated with the economic conditions of the particular location or industry. Therefore, to isolate variation that is not driven by local economic conditions, Appendix Table 15 utilizes variation in concentration driven by mergers between national firms that operate in multiple commuting zones. This specification yields an estimate for the elasticity of earnings with respect to concentration of -0.262 (SE=0.128).[34]

These results show that increased concentration due to merger activity results in earnings declines. However, as discussed previously, increases in local labor market concentration may increase *both* labor market power and product market power. In Column 4 of Appendix Table 15, I find the elasticity of concentration is equal to -0.331 (SE=0.180) in tradable industries for which product market effects are likely ameliorated. Interestingly, elasticity is larger in tradable industries is consistent with the worker-level results, though the confidence intervals here are quite large, making the difference in elasticities between tradable and nontradable markets not statistically significant.

To summarize, I find the majority of mergers do not cause market-level spillovers, because, on average, mergers do not cause very large increases in market concentration. However, the largest mergers (top-ventile), do cause market-level declines in earnings that are not due

[34]In Columns 2-3, I vary the definition of multi-region firm by requiring the acquiring and target firms to operate in at least five (Column 2) or ten (Column 3) commuting zones and continue to find similar results.

solely to changes at merging firms or product market effects, making increases in labor market monopsony of potential interest to antitrust authorities. In the next section, I interpret the estimates in this section through the lens of the Cournot model discussed in Section 1.2.3.

1.6.8 Model-Based Interpretation

To interpret the magnitudes of these results, I perform two exercises. First, I use the Cournot model of competition to compute implied wage markdowns over time. This allows me to compute how much local labor market concentration depresses wages and to discuss how changes in the distribution of local concentration relate to important labor market trends such as the falling labor share and stagnant wage growth. Second, I estimate how many mergers would be blocked according to different threshold rules that antitrust authorities might adopt. This analysis informs the scope of antitrust scrutiny in the labor market and whether this has changed over time.

Wage Markdowns over Time

To begin, I first compute the fraction of the marginal revenue product of labor that accrues to the worker. Recall from the model that this fraction is given by:

$$\gamma_m = \frac{\eta_m^\alpha}{C_m + \eta_m^\alpha} \tag{1.28}$$

where η_m^α the market-level elasticity of labor supply. I denote implied wage markdown as $1 - \frac{\eta_m^\alpha}{C_m + \eta_m^\alpha}$. For now I will assume the market-specific parameter (η_m^α) is constant across markets and will denote it by η. This is certainly violated in practice, but serves as a natural benchmark. If η and C are positively correlated, then I will overstate monopsony power. This is because markets that are highly concentrated will also have elastic labor supply, implying the high concentration has a smaller impact on wages. If the two are negatively correlated then I will understate monopsony power. Assuming η is constant, then

the change in the log wage in a market due to a merger that shifts concentration from C_1 to C_2 is given by:

$$\Delta\tilde{w} = log\left(\frac{\eta}{C_1 + \eta}\right) - log\left(\frac{\eta}{C_2 + \eta}\right) \tag{1.29}$$

Therefore, for a given η, initial concentration C_1, and post-merger concentration C_2, it is straightforward to estimate the implied change in the log market wage. To estimate η in practice, I choose the value that minimizes the distance between the model-implied impact of a top-ventile merger $m(\eta)$ on market wages and the estimated impact $\hat{\beta} = -0.034$ found in Section 1.6.7. That is, I set:

$$\hat{\eta} = \arg\min_{\eta}(\hat{\beta} - m(\eta))^2 \tag{1.30}$$

This procedure yields an average labor market supply elasticity equal to $\hat{\eta} = 0.87$ (SE=0.44), which falls between aggregate and firm-specific (often referred to as residual) labor supply elasticities common in the literature.[35] With the estimated $\hat{\eta}$ it is straightforward to compute the implied wage markdown due to concentration by plugging in $\hat{\eta}$ for every market and then computing the employment-weighted average across all markets. Figure 1.9 plots these results over time. As can be seen in the figure, the implied markdown begins around 5 percent in 1988, implying local concentration reduces earnings by 5 percent relative to a setting in which concentration is approximately zero. This markdown has been trending downwards over time, falling slightly below 4 percent in 2014. If one instead used a standard HHI measure that assumed a labor market is given by a 4-digit NAICS by commuting zone cell, then the implied wage markdown would be about 11.4 percent. The difference here is completely mechanical due to the HHI measure resulting in strictly more concentrated markets.

[35]For example, a number of papers find aggregate elasticities between 0.15 and 0.5 (See Table 2 of Chetty (2012)). Firm-specific elasticities vary depending on the setting and industry, but a number or recent papers have found elasticities between 1 to 5 (See Manning (2011) for a review).

This analysis leads to two important points. First, I find markdown that are quite a bit lower than many papers estimating firm-specific labor supply elasticities (Hirsch et al., 2010; Ransom and Sims, 2010; Staiger et al., 2010; Manning, 2011; Depew and Sørensen, 2013; Hirsch et al., 2010; Webber, 2015; Cho, 2018; Dube et al., Forthcoming; Kline et al., 2019), with markdowns anywhere between 25 to 90 percent. However, monopsony power can stem from many sources. For example, search costs and workplace differentiation will lead to monopsony power even when firms are atomistic (Bhaskar, Manning and To, 2002; Manning, 2003; Card, Cardoso, Heining and Kline, 2018; Lamadon, Mogstad and Setzler, 2019). In contrast to prior papers, I identify this markdown from concentration changes only. Therefore, it should not be interpreted as reflecting all possible sources of monopsony power.

Second, while increased monopsony power has been suggested as playing a role in the declining labor share and stagnant wage growth, local labor market concentration does not appear to be the culprit. If anything, the model-implied markdown due to the distribution of concentration across markets has been trending downward since the late 1980s. However, these results do not necessarily imply that monopsony power in general has been decreasing over time. As discussed above, local concentration is only one source of monopsony power. Declining unionization rates (Farber et al., 2018) or increases in non-competes and no-poaching agreements (Krueger and Ashenfelter, 2018; Krueger and Posner, 2018) could lead to rising monopsony power even in the presence of falling local concentration.

The Scope of Antitrust Scrutiny

In this final section, I consider the fractions of mergers that would be blocked by a hypothetical antitrust authority that blocked any merger that was predicted to decrease wages by a given amount. To compute the predicted impact of a given merger on the market wage I simply compute Equation (1.29) for every merger in the data. Note that many mergers increase concentration in multiple markets, and therefore I consider a merger blocked if it

lowers wages by a given amount in at least one market. To be clear, in practice, this procedure could lead to misleading results for any given merger. The market-level elasticity of labor supply will certainly vary across markets, while this exercise assumes it is constant. However, the goal for this exercise is not to predict the change for a given merger, but rather get a sense of roughy how many mergers would be blocked based on different thresholds.

Figure 1.10 plots the fraction of mergers that would be blocked over time for a 1 percent decline in the wage (solid blue line) and a 5 percent decline in the wage (dashed orange line). As can be seen in the Figure, for a 1 percent decline in the wage, the percent blocked fluctuates between 2 to 8 percent a year, with an average equal to 4.6 percent. For a 5 percent decline, about 1.2 percent of all mergers would be blocked. In product markets, a 5 percent increase in product prices is considered large enough to warrant antitrust scrutiny. Over these years, the DOJ and FTC issued enforcement challenges in about 1.9 percent of all merger notifications (See Appendix Figure 1.12). While these numbers are close in magnitude, they are not directly comparable (both are subsets of all merger activity). Additionally, of the 1.9 percent that are challenged, many are modified while some are abandoned or blocked. Essentially, the 1.2 percent is the percent of *completed* mergers that would have been blocked by a hypothetical antitrust authority, not the percent of *proposed* mergers that would have been blocked.

This simple exercise leads to two conclusions. First, I interpret this as evidence that the labor market is an important market for which antitrust scrutiny is relevant, but likely only for very large mergers that generate considerable shifts in local concentration, similar to how antitrust is enforced for product markets. Second, it seems unlikely that lack of antitrust scrutiny in labor markets led to stagnant wage growth or falling labor share over time. There is no clear trend in the number of hypothetically blocked mergers over time and local concentration has actually been falling over this time period.

1.7 Conclusion

Labor market power poses a serious threat to workers. However, a merger has never been subjected to antitrust scrutiny due to potential harm in the labor market. Despite a recent call-to-action by both academics and policymakers, there is limited empirical evidence and little guidance on how to perform antitrust analysis in labor markets.

In this paper, I document the impacts of M&A on workers utilizing a matched employer-employee dataset for the United States. To link this evidence to monopsony power, I examine heterogeneity in impacts driven by differences in changes in local labor market concentration across mergers. Predicting anticompetitive effects from changes in concentration has a long history in antitrust, but is often criticized for relying on potentially arbitrary market definitions. I construct a measure of concentration that directly takes into account substitutability across industries by utilizing data on job-to-job flows.

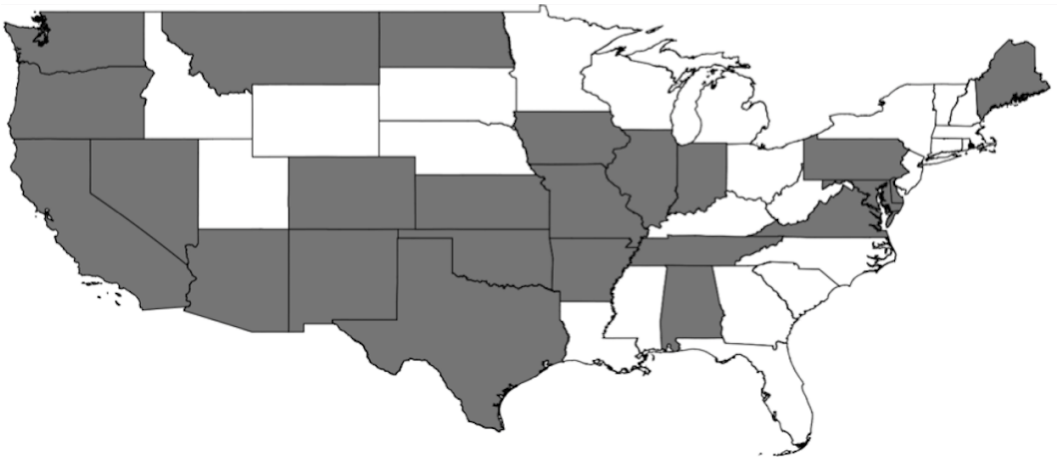
I find that mergers with small impacts in local labor market concentration do not have significant impacts on workers' earnings. However, mergers that generate large shifts in concentration have economically meaningful and statistically significant effects. These effects are larger in already concentrated markets, are consistent in tradable industries, and are consistent in a sample of national mergers that are likely not driven by local economic conditions. Additionally, I find evidence of spillovers in the labor market, with other firms in the labor market decreasing wages in response to merger activity. For top-ventile concentration changes market-level wages fall by about 3.4 log points. This implies an elasticity of earnings with respect to concentration equal to -0.22. I argue that this evidence justifies antitrust authorities scrutinizing mergers solely on the basis of increased labor market power.

In this final section, I highlight a few areas for future research. First, it is clear in many mergers that technology plays a role in reducing firm employment. However, how merged firms combine inputs remains unclear. Future work with more detailed data on production inputs and output would be helpful in understanding how M&A firms "combine" production functions after mergers and how these changes may impact different types of workers.

Second, future research would benefit from more careful case studies of specific industries, as in Currie et al. (2005) and Prager and Schmitt (2018). While this paper examined how to measure concentration in a rigorous way and provided evidence that this approach predicts heterogeneity in a sample of past mergers, another approach is to simulate a structural model of the labor market following the industrial organization literature. This would allow one to make more detailed predictions on the impacts for a given, proposed merger. However, performing such a simulation for labor markets will likely face a few hurdles.

For one, simulating such a model would require an understanding of how firms compete in the labor market. While product market simulations generally assume firms compete by Nash-Bertrand in prices, it is not clear whether this assumption is appropriate for the labor market. Again, careful case studies of past mergers may be instructive in learning more about wage competition in labor markets. Hospital mergers and airline mergers seem particularly attractive given the simplicity in defining the labor markets, the availability of both input and output prices, and the history of large consolidation events in both industries. This would allow one to directly compare consumer losses to worker losses in mergers that impact both product market power and labor market monopsony.

Figure 1.1: Sample of States with Worker-Level (LEHD) Data Available



Note: The states with worker-level (LEHD) data available are shaded in gray. The sample includes: AL, AZ, AR, CA, CO, DE, DC, HI, IL, IN, IA, KS, ME, MD, MO, MT, NV, NM, ND, OK, OR, PA, TN, TX, VA, WA. These states correspond to 53.8 percent of the U.S. population as of the 2010 U.S. Census.

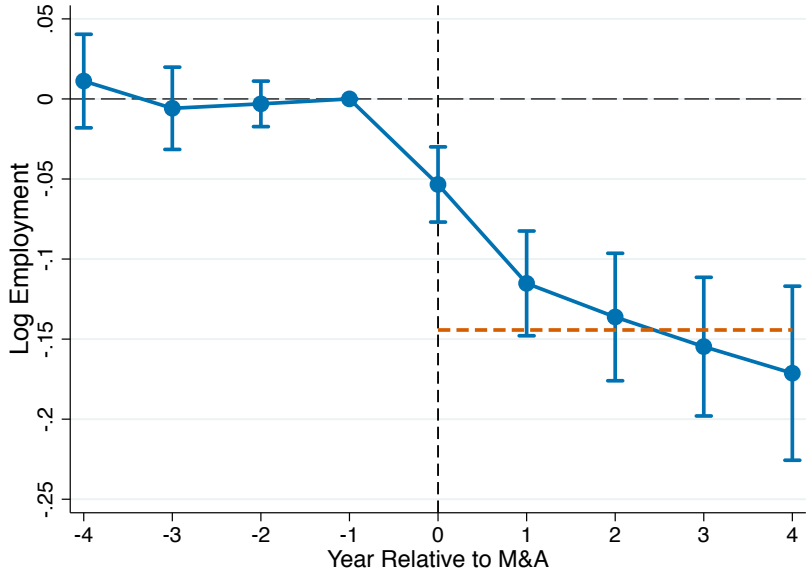
Figure 1.2: M&A Activity over Time



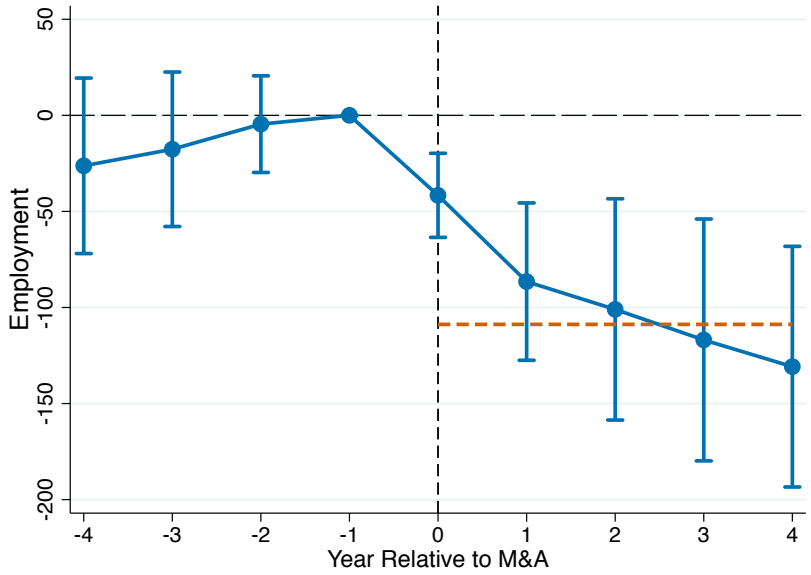
Note: This figure plots the total employment in M&A establishments (solid blue line) over time. This sample is a subset of all merger activity due to sample restrictions that drop small and partial M&As. For more details on sample construction see Section 1.3.4. The dashed orange line plots the number of deals completed in the Thomson Reuters Database of Mergers & Acquisitions (SDC). To compute the total number of deals, I drop leveraged buyouts, divestitures, deals that are never completed, and deals in which the acquiring firm acquired less than 100 percent of the target firm. However, I make no restrictions on firm size given employment is often missing in the SDC database.

Figure 1.3: Difference-in-Differences Estimates of the Effect of M&A on Employment

Panel A: Log Employment

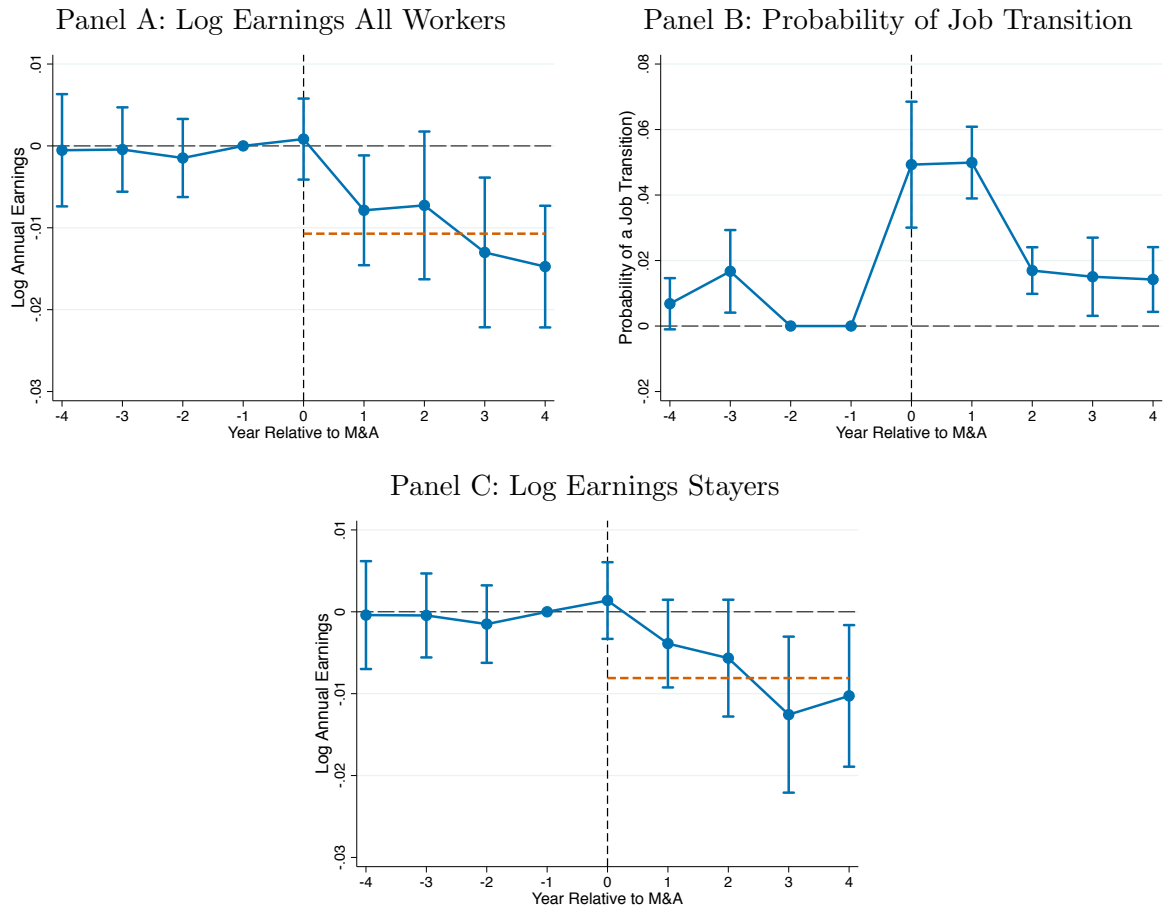


Panel B: Employment



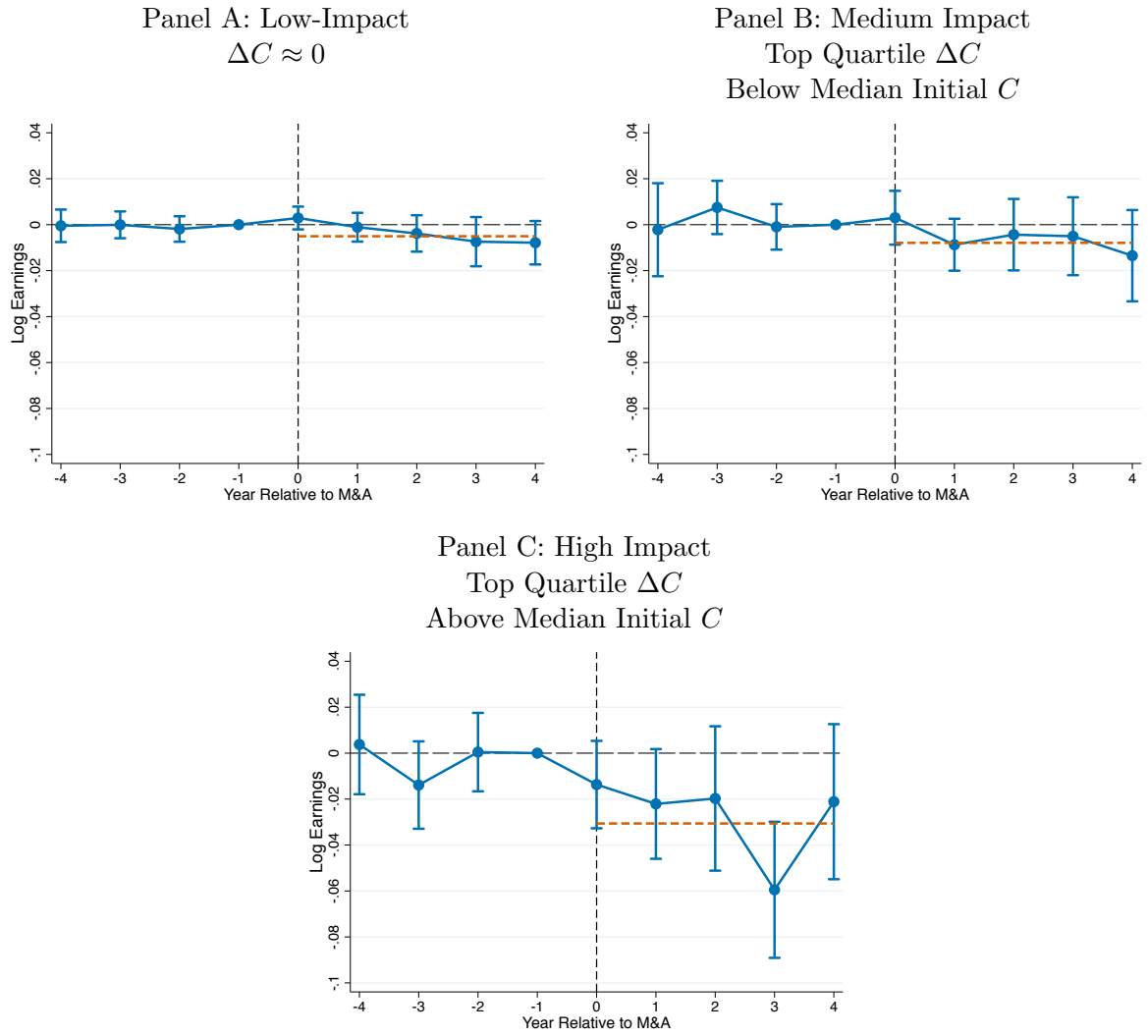
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on log establishment-level employment in Panel A and establishment-level employment in Panel B (including zeros). Due to the ambiguity in the timing of the merger, some M&A establishments have already gone through the merger at time $t = 0$, while others have yet to complete the merger. For each M&A establishment I find a counterfactual establishment by matching on 4-digit NAICS (industry codes), state, $t^* - 1$ employment decile, and $t^* - 1$ average earnings decile, where t^* indicates the year of the merger. 95 percent confidence intervals two-way clustered at the commuting zone and 4-digit NAICS level are displayed.

Figure 1.4: Difference-in-Differences Estimates of the Effect of M&A on Incumbent Worker Outcomes



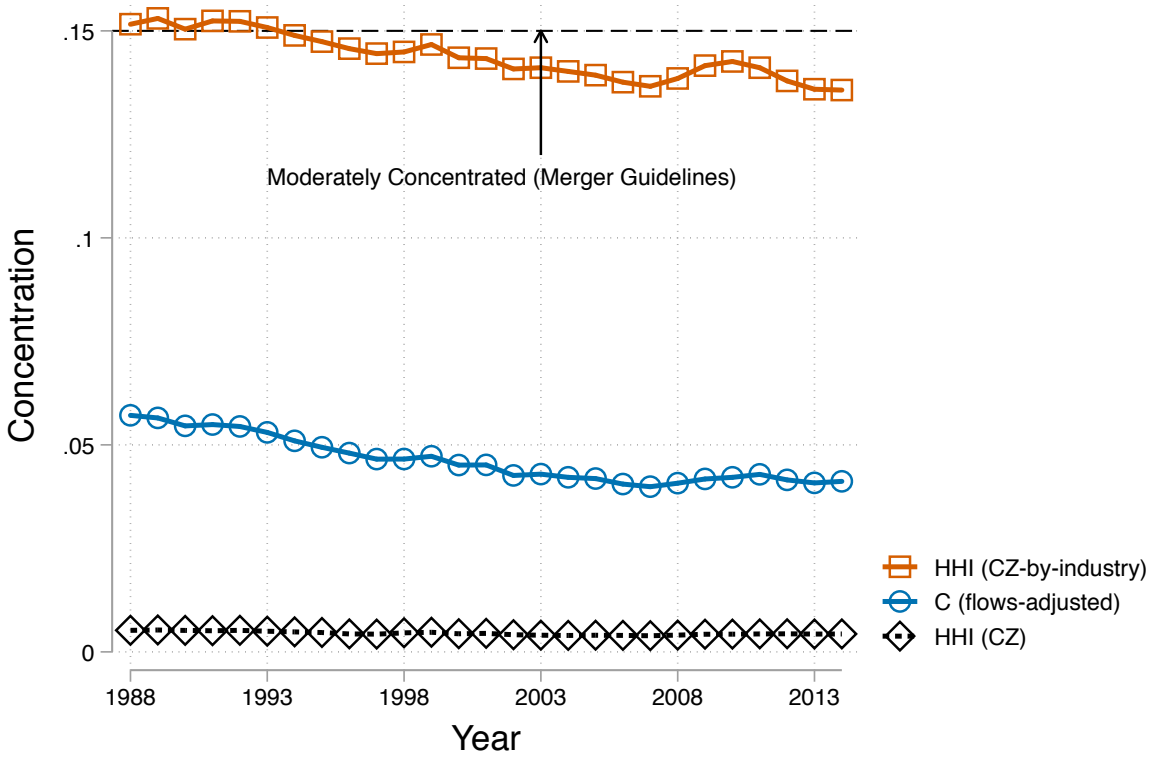
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on worker outcomes. Panel A reports the impact on log annual earnings for all incumbent workers. Panel B reports the impact on job transitions. Panel C reports the impact on log annual earnings for firm stayers. A stayer is defined as a worker who is employed in time t at the same firm as in $t^* - 1$. To prevent coding mechanical changes in firm identifiers as workers switching employers, I use the full set of M&A identified in the LBD as well as worker flows in the LEHD (Benedetto et al., 2007) to recode changes in EINs that are likely due to reorganizations rather than true job switching. Treated workers are drawn from the M&A sample for which there is coverage in the LEHD. For each M&A worker, I find a counterfactual worker by matching on 4-digit NAICS (industry codes), state, gender and age bins (5-year bins). If multiple counterfactuals are found for an M&A worker, I choose the counterfactual worker with the closest propensity score, where the propensity score is estimated by predicting treatment using a linear probability model with a quadratic in firm age, a quadratic in worker age, a quadratic in firm size, and and an indicator equal to one if the worker is employed by a multi-unit firm. 95 percent confidence intervals based on standard errors two-way clustered at the worker and 4-digit NAICS by commuting zone level are displayed.

Figure 1.5: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings



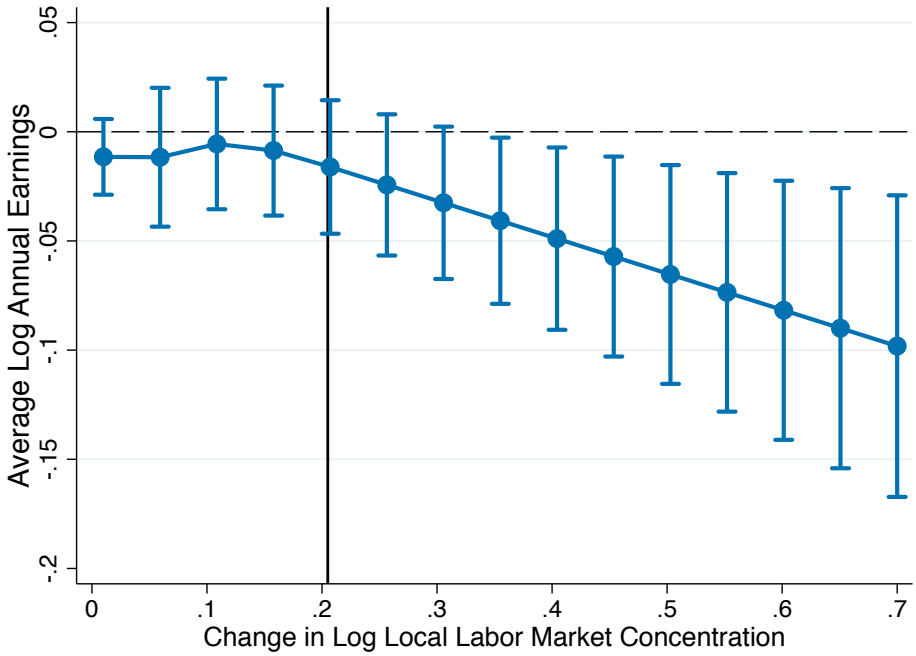
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. Concentration (C) is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. Panel A displays results for workers exposed to low-impact mergers, which occur when the change in concentration is below the top quartile ($\Delta C \approx 0$). Panel B displays results for workers exposed to medium-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in a below-median concentration market. Panel C displays results for workers exposed to high-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in an above-median concentration market. The figure restricts to firm stayers who are defined as workers employed in time t at the same firm as in $t^* - 1$. For details on the matching algorithm used to identify control workers, see the notes to Figure 1.4 and Section 1.3.4.

Figure 1.6: Average (Employment-Weighted) Local Concentration Over Time



Note: The figures plots the average (employment-weighted) level of local labor market concentration for three different measures. The orange squares correspond to a HHI in which the labor markets is defined by a 4-digit NAICS by commuting zone cell. The black diamonds correspond to a HHI in which the labor market is defined by a commuting zone. The blue circles correspond to the flows-adjusted local concentration index (C) described in Section 1.2.3. If workers are perfectly mobile across 4-digit industries, then C will be equal to the HHI based on commuting zone only. If there is no mobility across 4-digit industries, then C will be equal to the HHI based on a industry by commuting zone. Transitions across industries are estimated using the LEHD worker-level data.

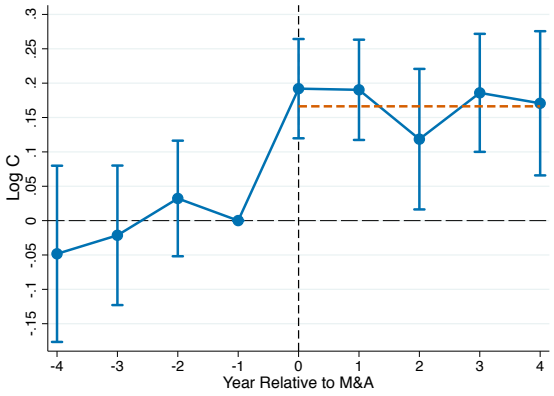
Figure 1.7: Market-Level (Excluding M&A Firms) Impacts by Predicted Change in Local Labor Market Concentration



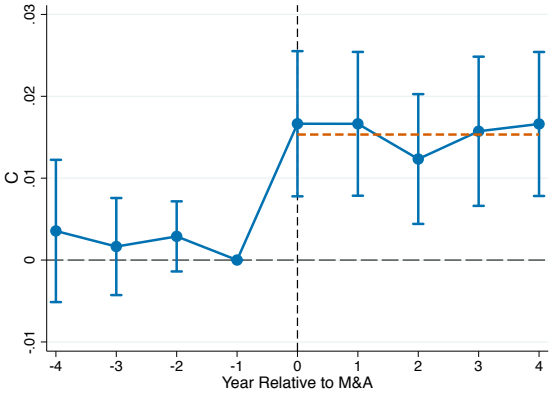
Note: This figure reports the impact of M&A on market-level earnings as a function of the predicted change in log local labor market concentration. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure (C) that incorporates information on worker flows across industries. Market-level earnings exclude the M&A firms and are constructed by residualizing on observables of the workforce, such as age, gender, imputed education, and race. The solid vertical line corresponds to the psuedo-95^{th} percentile, which is equal to the average of the 94^{th} through 96^{th} percentiles and is reported in place of the 95^{th} percentile to accommodate Census disclosure rules. 95 percent confidence intervals based on standard errors that cluster at the 4-digit NAICS by commuting zone level are displayed.

Figure 1.8: Difference-in-Differences Estimates of Top-Ventile Concentration Increases on Market-Level Outcomes

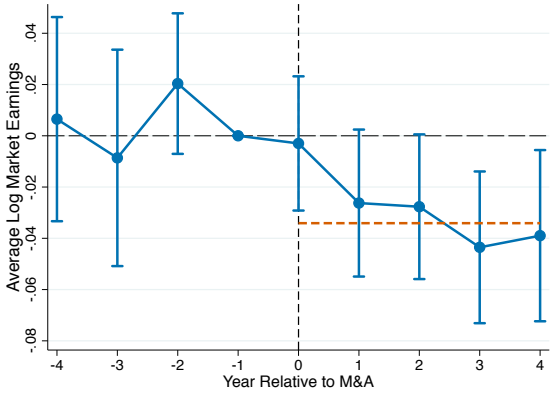
Panel A: Log Flows-Adjusted Concentration



Panel B: Flows-Adjusted Concentration

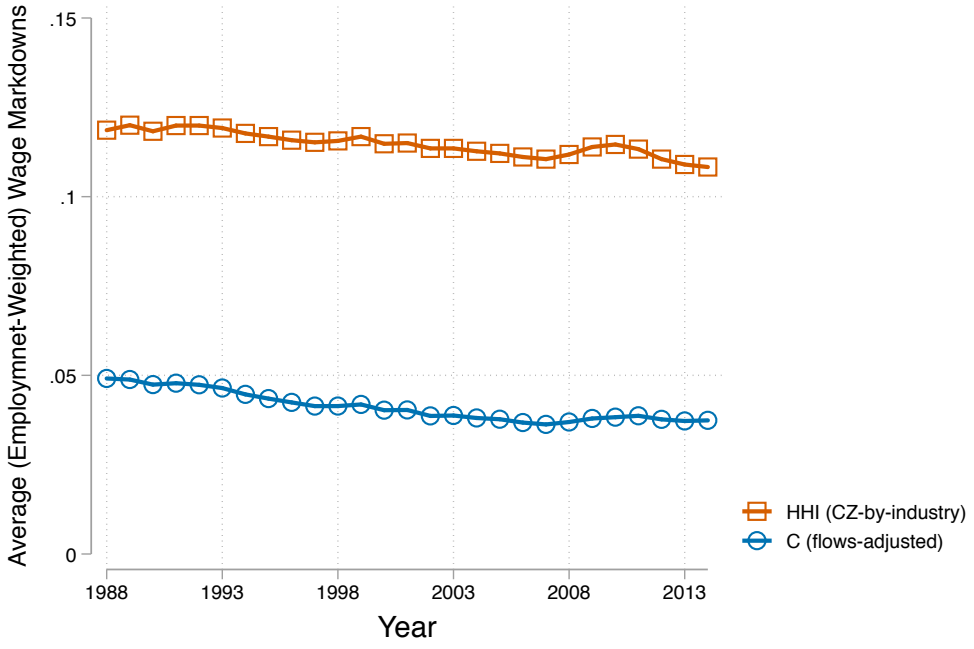


Panel C: Average Log Earnings



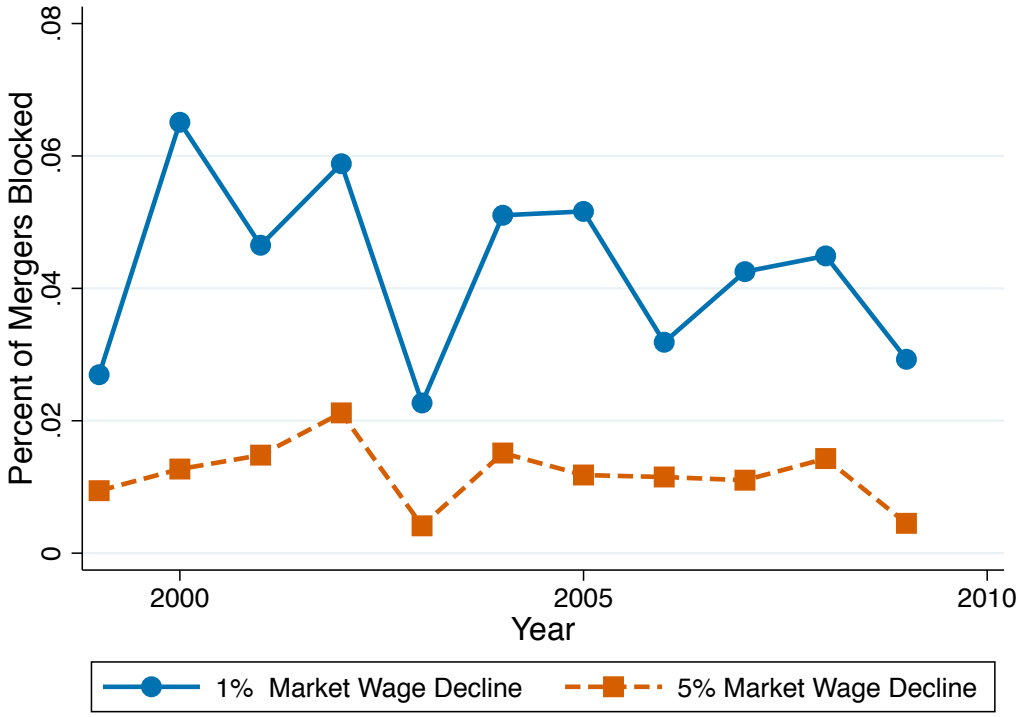
Note: This figure displays estimates of the effect of a top-ventile concentration increase on market-level outcomes. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure (C) that incorporates information on worker flows across industries. Panel A reports the impact on log flows-adjusted concentration, Panel B reports the impact on flows-adjusted concentration, and Panel C reports the impact on average log market-level earnings. Market-level earnings exclude the M&A firms and are constructed by residualizing on observables of the workforce, such as age, gender, imputed education, and race. 95 percent confidence intervals based on standard errors that cluster at the 4-digit NAICS by commuting zone level are displayed.

Figure 1.9: Model-Implied Wage Markdowns Due to Local Labor Market Concentration Over Time



Note: This figure plots the average (employment-weighted) wage markdown over time computed from the Cournot model of labor market competition in Section 1.2. In the model, the markdown is a function of local labor market concentration and the market-level elasticity of labor supply. I set the market-level elasticity of labor supply equal to 0.87 (SE=0.44) for all markets, which is the value that minimizes the distance between the model-implied impacts and the market-level reduced-form estimate in Panel C of Figure 1.8. The blue circles correspond to estimates that measure concentration using the flows-adjusted concentration measure (C) that incorporates information on worker flows across industries. The orange squares correspond to estimates that measure concentration using an HHI that defines the labor market as a 4-digit NAICS by commuting zone.

Figure 1.10: Estimated Fraction of Mergers Blocked According to Different Threshold Rules



Note: This figure reports the fraction of mergers that would be blocked according to different threshold rules. The predicted impact of a merger depends on (1) the initial concentration level (2) the change in concentration and (3) the market-level elasticity of labor supply. For this figure I set the market-level elasticity of labor supply equal to 0.87 (SE=0.44) for all markets, which is the value that minimizes the distance between the model-implied impacts and the market-level reduced-form estimates in Panel C of Figure 1.8. A merger is considered blocked if it lowers the market-wage in at least one market by more than the given threshold.

Table 1: Summary Statistics of M&A Establishments and Control Establishments

	M&A Establishments	Control
Panel A: Establishment Characteristics	(1)	(2)
Payroll ($1000s)	11,000.00	10,340.00
Employment	250.10	240.00
Pseudo-Median Employment	116.70	117.00
Earnings Per Worker ($1000s)	43.94	42.81
Target Establishment	0.32	–
Panel B: Sectors of Establishments		
Manufacturing	0.17	0.17
Wholesale Trade	0.06	0.06
Information	0.04	0.04
Finance	0.09	0.09
Professional, Scientific and Technical	0.07	0.07
Health Care	0.10	0.10
Accommodation and Food	0.10	0.10
Tradable	0.24	0.24
Panel C: Characteristics of M&A deal		
Merger within CZ	0.29	–
Merger within Industry (4-digit NAICS)	0.61	–
C (flows-adjusted concentration)	0.04	–
Log Change in C	0.01	–
National Merger	0.59	–
Incidental	0.08	–
Unique Establishments	46,000	46,000
Unique Firms	10,000	25,000

Note: This table displays summary statistics of M&A establishments and the matched control establishments. Payroll and Earnings Per Worker are in $1000s. Employment is the employment on March 12th the year prior to the M&A event. The Pseudo-Median Employment is the average of the 49th through 51st percentiles of employment and is reported in place of the median to accommodate Census disclosure rules. Tradable industries belong to the following NAICS two-digit codes: 11, 21, 31, 32, 33 and 55. An establishment is part of a "Merger within CZ" if the acquiring firm owns at least one establishment in the same CZ as the target establishment. An establishment is part of a "merger within industry" if the acquiring firm owns at least one establishment in the same industry (4-digit NAICS) as the target establishment. C is the flows-adjusted measure of local labor market concentration that incorporates worker flows across industries. Mergers between two firms that own establishments in at least 5 commuting zones are defined as national mergers. Establishments in second or tertiary lines of business are defined as incidental to the merger. For each M&A establishment I find a counterfactual establishment by matching on 4-digit-NAICS, state, $t^* - 1$ employment decile, and $t^* - 1$ average earnings decile, where t^* is the year of the merger. If multiple counterfactual establishments are found, I choose the counterfactual with the closest propensity score, where the propensity score is estimated by predicting treatment using a linear probability model with quadratics in employment, earnings, firm age, and an indicator equal to one if the establishment is part of a multi-unit firm.

Table 2: Summary Statistics of Incumbent M&A and Control Workers

	M&A Workers	Control Workers
Panel A: Worker Characteristics	(1)	(2)
Annual Earnings	55,170.00	52,400.00
Female	0.46	0.46
College Education (Imputed)	0.32	0.31
Age	43.65	43.65
Tradeable	0.27	0.27
Target	0.37	–
Panel B: Merger Characteristics		
Merger within CZ	0.49	–
Merger within Industry (4-digit)	0.64	–
C (flows-adjusted concentration)	0.07	–
Log Change in C	0.02	–
Unique Workers	1,941,000	1,941,000

Note: This table displays summary statistics of M&A workers and matched control workers, which are drawn from the sample of M&A firms with coverage in the LEHD sample (See Figure 1.1). Workers must be employed at the M&A firm for at least two years prior to the merger to be in the sample. Annual Earnings are in 2011 dollars and aggregated across all employers the worker is employed by in the year. Definitions for variables which appear in Panel B appear in Section 1.3 and the notes to Table 1. For each M&A worker, I find a counterfactual worker by matching on 4-digit NAICS, state, gender and age bins (5-year bins). If multiple counterfactuals are found for an M&A worker, I choose the counterfactual worker with the closest propensity score, where the propensity score is estimated by predicting treatment using a linear probability model with quadratics in firm age, a quadratic in worker age, a quadratic in firm size, and and an indicator equal to one if the worker is employed by a multi-unit firm.

Table 3: Effect of M&A on Establishment Outcomes

	Log Emp.	Log Emp.	Emp.	Log Payroll	Estab. Survival
	(1)	(2)	(3)	(4)	(5)
Post-MA	-0.144^{***}	-0.081^{***}	-108.800^{***}	-0.121^{***}	-0.031^{***}
	(0.021)	(0.015)	(28.010)	(0.019)	(0.006)
Mean at t=-1	5.955	4.965	767.900	9.574	–
R squared	0.803	0.777	0.824	0.845	0.425
Weighted	Yes	No	Yes	Yes	Yes
Establishment-Years	753,000	753,000	824,000	753,000	824,000

Notes: This table reports difference-in-differences estimates of the effect of M&A on establishment-level outcomes. I estimate a flexible specification that allows for dynamic treatment effects as depicted in Figure 1.3 and average the four post-event coefficients to estimate the aggregate effect reported in this table. The regressions are estimated on the sample described in the notes to Table 1, which includes details on the matching algorithm used to identify control establishments. Weighted results are weighted by the employment in the establishment in the year prior to the merger. Standard errors are two-way clustered at the 4-digit NAICS and commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 4: Heterogeneity and Robustness of the Effect of M&A on Log Establishment Employment

	National	Incidental	LEHD States	Acquirer	Target	Low Emp	High Emp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-MA	-0.185^{***}	-0.234^{***}	-0.151^{***}	-0.108^{***}	-0.225^{***}	-0.055^{***}	-0.162^{***}
	(0.027)	(0.050)	(0.024)	(0.019)	(0.033)	(0.018)	(0.024)
Obs.	440,000	60,000	400,000	510,000	240,000	380,000	380,000

Notes: This table presents difference-in-differences estimates of the effect of M&A on establishment-level log employment. I estimate a flexible specification that allows for dynamic treatment effects over time and average the four post-event coefficients as depicted in Figure 1.3 to estimate the aggregate effect reported in this table. National mergers are defined as mergers between two firms that operate in at least 5 commuting zones. Incidental establishments are establishments in secondary or tertiary industries of the merging entities. LEHD states are displayed in Figure 1.1. High employment establishments are above the median level of employment in the analysis sample, while low employment establishments are below the median level of employment. For details on the matching algorithm used to identify control establishments, see the notes to Table 1 and Section 1.3.4. Standard errors are two-way clustered at the 4-digit NAICS and commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 5: Impact of M&A on Worker Outcomes

	Log Annual Earnings		Job
	All Workers	Stayers	Transition
	(1)	(2)	(3)
Post-MA	-0.011^{***}	-0.008^{**}	0.032^{***}
	(0.004)	(0.003)	(0.004)
Mean at t=-1	10.550	10.550	–
R squared	0.715	0.800	0.228
Worker-Years	32,000,000	25,700,000	34,800,000

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings (Columns 1 and 2) and the probability a worker transitions jobs (Column 3). I estimate a flexible specification that allows for dynamic treatment effects as depicted in Figure 1.4 and average the four post-event coefficients to estimate the aggregate effect reported in this table. The regressions are estimated on the sample described in the notes to Table 2, which includes details on the matching algorithm used to identify control workers. A job transition occurs if a worker switches between two firms or a worker transitions from nonemployment to employment (or vice versa). A stayer is defined as a worker who is employed in time t at the same firm as in $t^* - 1$. To prevent coding mechanical changes in firm identifiers as workers switching employers, I use the full set of M&A identified in the LBD, as well as using worker-flows (Benedetto et al., 2007) to recode changes in EINs that are likely due to reorganizations rather than true job switching. Treated workers are drawn from the M&A sample for which there is coverage in the LEHD. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6: Hetereogeneity by Local Concentration: The Effect of M&A on Log Annual Earnings for Firm Stayers

	High Impact	Medium Impact	Low Impact		
	Pred $\Delta C^{MA} > 0$	Pred $\Delta C^{MA} > 0$			
	High Initial C	Low Initial C	Pred $\Delta C^{MA} \approx 0$	Diff (1)-(2)	Diff (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Panel A: All Mergers					
Post-M&A	-0.031^{***}	-0.008	-0.005	-0.023^{*}	-0.026^{**}
	(0.011)	(0.007)	(0.004)	(0.013)	(0.012)
Change in Log C	0.099	0.059	0.001	–	–
Approx Worker-Years	2,700,000	3,700,000	19,300,000	–	–
Panel B: Tradables					
Post-M&A	-0.067^{***}	0.001	-0.012^{*}	-0.068^{***}	-0.056^{**}
	(0.023)	(0.012)	(0.006)	(0.026)	(0.024)
Change in Log C	0.099	0.041	0.000	–	–
Approx Worker-Years	600,000	1,200,000	14,000,000	–	–
Panel C: National Mergers					
Post-M&A	-0.042^{***}	-0.013^{*}	-0.008	-0.029^{**}	-0.035^{***}
	(0.013)	(0.007)	(0.005)	(0.015)	(0.013)
Change in Log C	0.082	0.061	0.001	–	–
Approx Worker-Years	1,600,000	2,100,000	11,000,000	–	–

Note: This table presents difference-in-differences estimates of the effect of M&A on log annual earnings. I estimate a flexible specification that allows for dynamic treatment effects as depicted in Figure 1.5 and compute the aggregate effect as the average of the four post-event coefficients. C denotes the flow-adjusted measure of local labor market concentration. High-impact mergers are top quartile predicted changes in concentration in above-median concentration markets. Medium-impact mergers are top quartile predicted changes in concentration in below-median concentration markets. Low-impact mergers are below top quartile predicted changes in concentration. The sample restricts to firm stayers, defined as workers employed in time t at the same firm as in $t^* - 1$, where t^* is the year of the M&A. A national merger is defined as a merger between two firms that both operate in at least five commuting zones. Tradable industries are the following NAICS two-digit codes: 11, 21, 31, 32, 33 and 55. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 7: Correlation between Concentration Changes and Market Outcomes

	Change in Average Log Market Earnings		Change in Log Market Size
	(1)	(2)	(3)
Change in Log C	-0.099^{***}		0.309^{***}
	(0.005)		(0.010)
Change in Log HHI		-0.085^{***}	
		(0.006)	
Market-Years	1,083,000	1,083,000	1,083,000

Note: This table regresses changes in market-level outcomes on changes in local labor market concentration. The HHI measure is defined at the 4-digit NAICS by commuting zone level. C denotes the flow-adjusted measure of local labor market concentration. Market size is the number of employees in the market in a given year with annual earnings above \$3,250. Average market earnings are obtained by residualizing worker-level earnings using a polynomial in age, gender, race, and education and then taking the average of the residualized log wage within the market. Standard errors are clustered at the 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 8: Predicted vs. Actual Changes in Local Labor Market Concentration

	Actual Change in Local Labor Market Concentration				
	(1)	(2)	(3)	(4)	(5)
ΔC^{MA} (M&A Changes)	0.834***				0.659***
	(0.032)				(0.035)
ΔC^{Exit}		0.778***			
		(0.016)			
ΔC^{Entry}			0.981***		
			(0.012)		
$\Delta C^{Reallocation}$				0.946***	0.915***
				(0.007)	(0.010)
R^2	0.014	0.254	0.107	0.915	0.927
Market-Years	1,083,000	1,083,000	1,083,000	1,083,000	1,083,000

Note: This table regresses predicted changes in local labor market concentration on actual changes in local labor market concentration. Column 1 predicts changes in concentration due only to ownership changes. Column 2 predicts changes in concentration due only to firm exit. Column 3 predicts changes in concentration due only to firm entry. Column 4 predicts changes due to any reallocation in employment across firms, which includes entry, exit, contraction or expansion. Column 5 includes both changes due to any reallocation of employment as well as ownership changes. Column 5 does not perfectly predict changes in concentration because ownership changes and reallocation changes are computed separately. In other words, the predicted change due to ownership and the predicted change due to reallocation is not sufficient information to construct the actual change in concentration. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 9: First-Stage Impact of Top-Ventile Concentration Increase on Log Local Concentration

	Log Concentration		
	(1)	(2)	(3)
$Q20 \times Post$	0.175***	0.239***	0.188***
	(0.043)	(0.047)	(0.039)
Market-Years	24,000	21,000	24,000
F-statistic	16.278	25.630	22.997
4-digit NAICS-by-CZ FE	Yes	Yes	Yes
1-digit NAICS-by-CZ-year FE	Yes	No	Yes
2-digit NAICS-by-CZ-year FE	No	Yes	No
Weighted by Employment	Yes	Yes	No

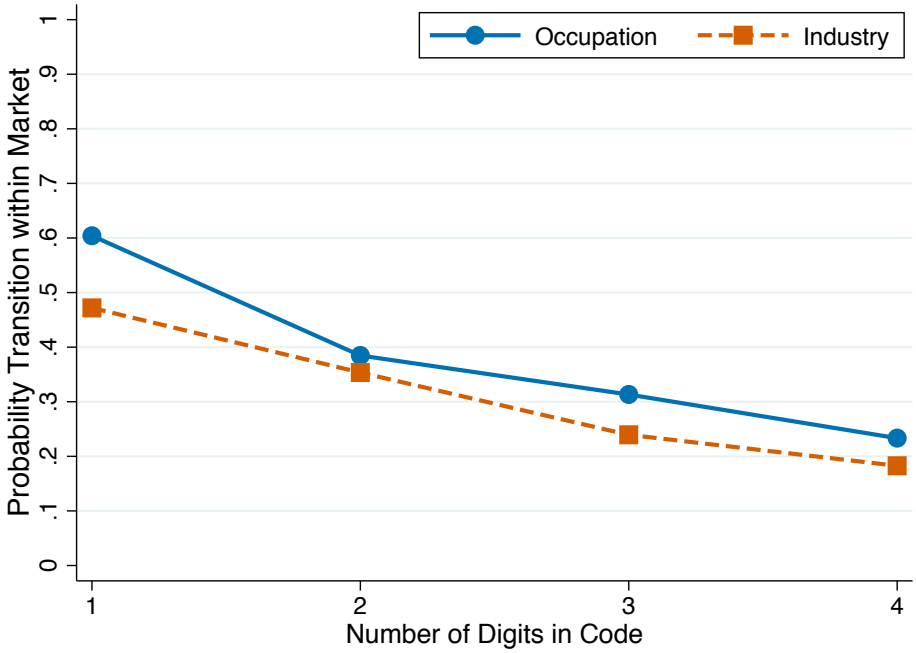
Note: This table presents first-stage estimates of the impact of a top-ventile concentration increase due to merger activity on the log of the flows-adjusted measure of local labor market concentration. To construct the sample, I restrict to markets that experience at least a 0.01 log increase in market concentration due to merger activity. I then split the markets at the 95^{th} percentile (ordered by changes in log market concentration). This table tests whether experiencing a top-ventile concentration increase leads to a persistent increase in log concentration in the years following the merger. Standard errors, clustered at the 4-digit NAICS by commuting zone level, appear in parentheses. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 10: Instrumental Variables Estimates of the Elasticity of Earnings with Respect to Local Labor Market Concentration (Flows-Adjusted)

	Average Log Market Earnings		
Panel A: All Markets	(1)	(2)	(3)
Log C	-0.219^{**}	-0.182^{***}	-0.147^{*}
	(0.094)	(0.067)	(0.083)
Panel B: By Initial Concentration			
Log C × Above Median C	-0.259^{**}	-0.230^{***}	-0.176^{**}
	(0.108)	(0.081)	(0.083)
Log C × Below Median C	0.059	0.065	0.058
	(0.121)	(0.120)	(0.141)
Market-Years	24,000	21,000	24,000
4-digit NAICS-by-CZ FE	Yes	Yes	Yes
1-digit NAICS-CZ-year FE	Yes	No	Yes
2-digit NAICS-CZ-year FE	No	Yes	No
Weighted by Employment	Yes	Yes	No

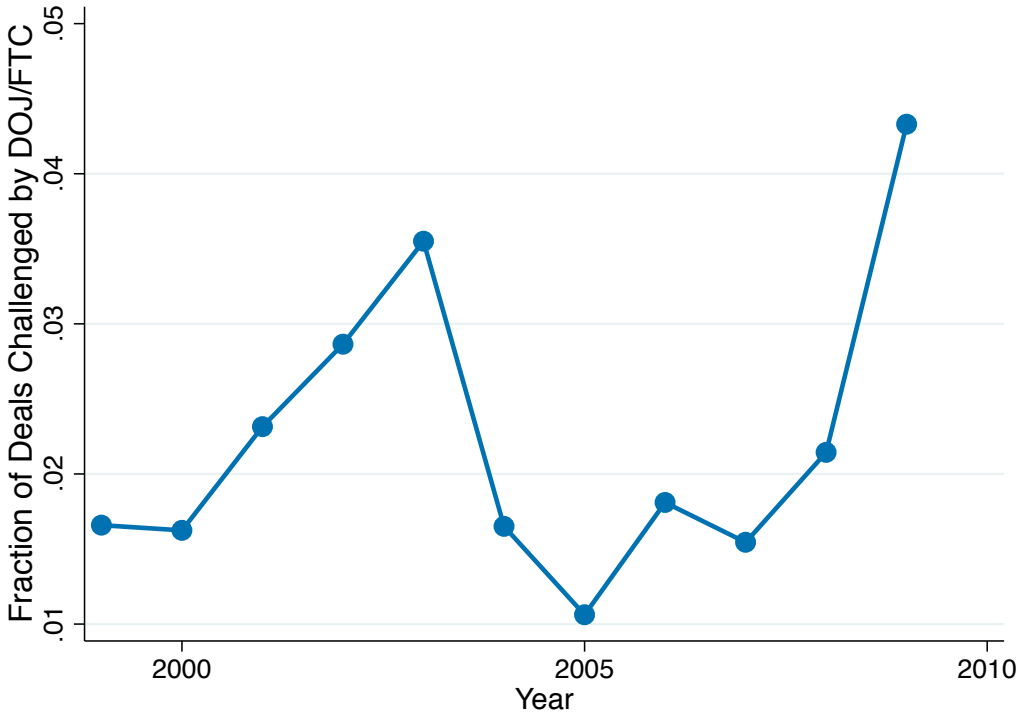
Note: This table reports instrumental variables estimates of the elasticity of earnings with respect to local labor market concentration (flows-adjusted). The instrument is an indicator for the market experiencing a top-ventile predicted concentration increase due to merger activity. See Table 9 for the first-stage regression and Figure 1.7 for the reduced form. A market is defined as a 4-digit NAICS by commuting zone cell. Standard errors appear in parentheses and are clustered at the 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Figure 1.11: Job Transitions Within Industries and Occupations in Brazil



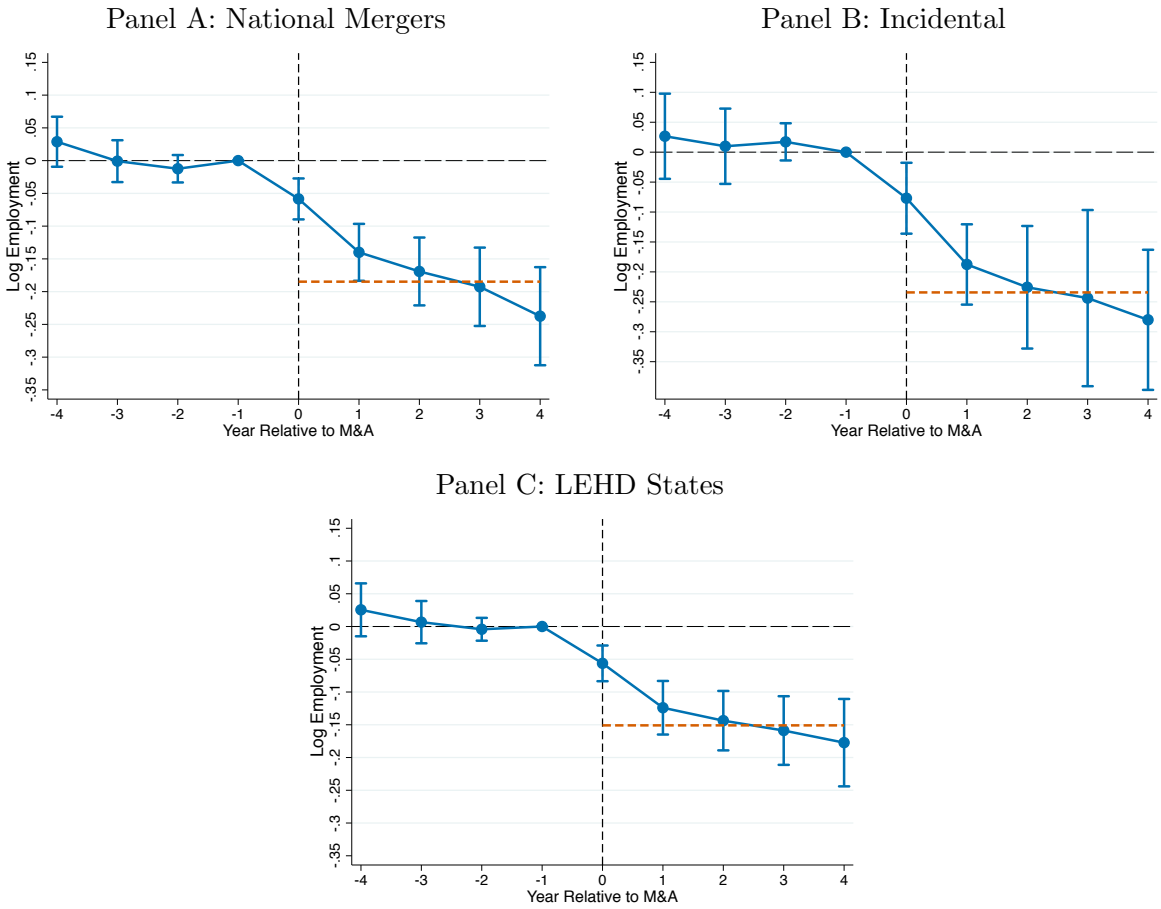
Note: This figure shows the probability a worker transitions within a given industry or occupation given the level of aggregation chosen. The data come from the *Relação Anual de Informações Sociais* (RAIS), a matched employer-employee dataset from Brazil.

Figure 1.12: DOJ and FTC Antitrust Enforcement Actions over Time



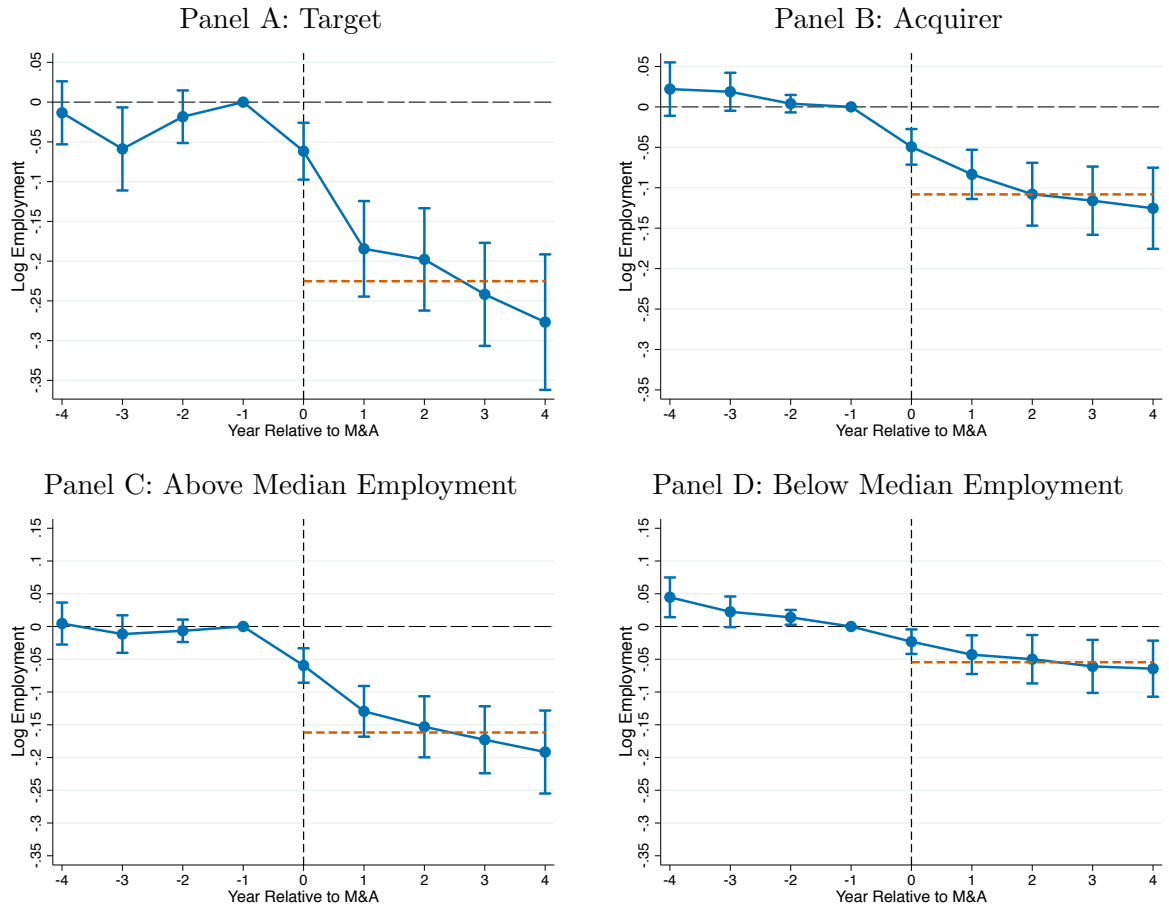
Note: This figure reports the fraction of merger notifications that are challenged each year between 1999-2009. Some deals in which the target asset's are relatively small are exempt from having to notify antitrust authorities (See Wollmann (2019) for more details). Data comes from *Hart-Scott Rodino Annual Reports* which reports the number of merger notifications as well as enforcement actions taken by the Department of Justice and Federal Trade Commission. Most of the time these enforcement actions result in the merging parties agreeing to modify their deal or abandoning the deal, with a small number eventually being blocked by federal litigation.

Figure 1.13: Difference-in-Differences Estimates of the Effect of M&A on Employment by Merger Characteristics



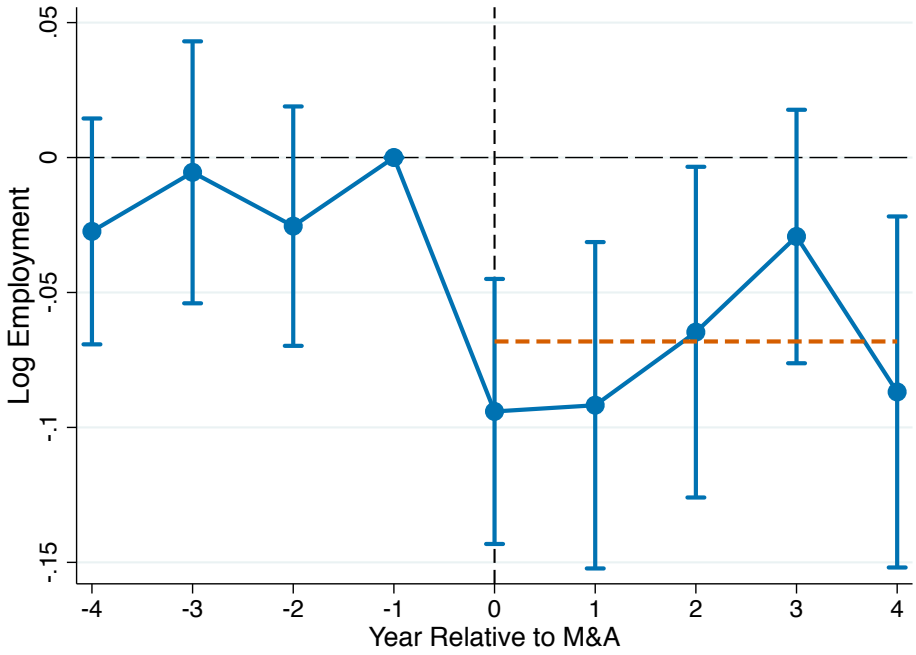
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment. The regressions are estimated on the sample described in the notes to Table 1, which contain details on the matching algorithm used to identify control establishments. Mergers between two firms that both own establishments in at least 5 commuting zones are defined as national mergers. Establishments in second or tertiary lines of business are defined as incidental to the merger. LEHD states are displayed in Figure 1.1. 95 percent confidence intervals based on standard errors two-way clustered at the 4-digit NAICS and commuting zone level are displayed.

Figure 1.14: Difference-in-Differences Estimates of the Effect of M&A on Employment by Establishment Characteristics



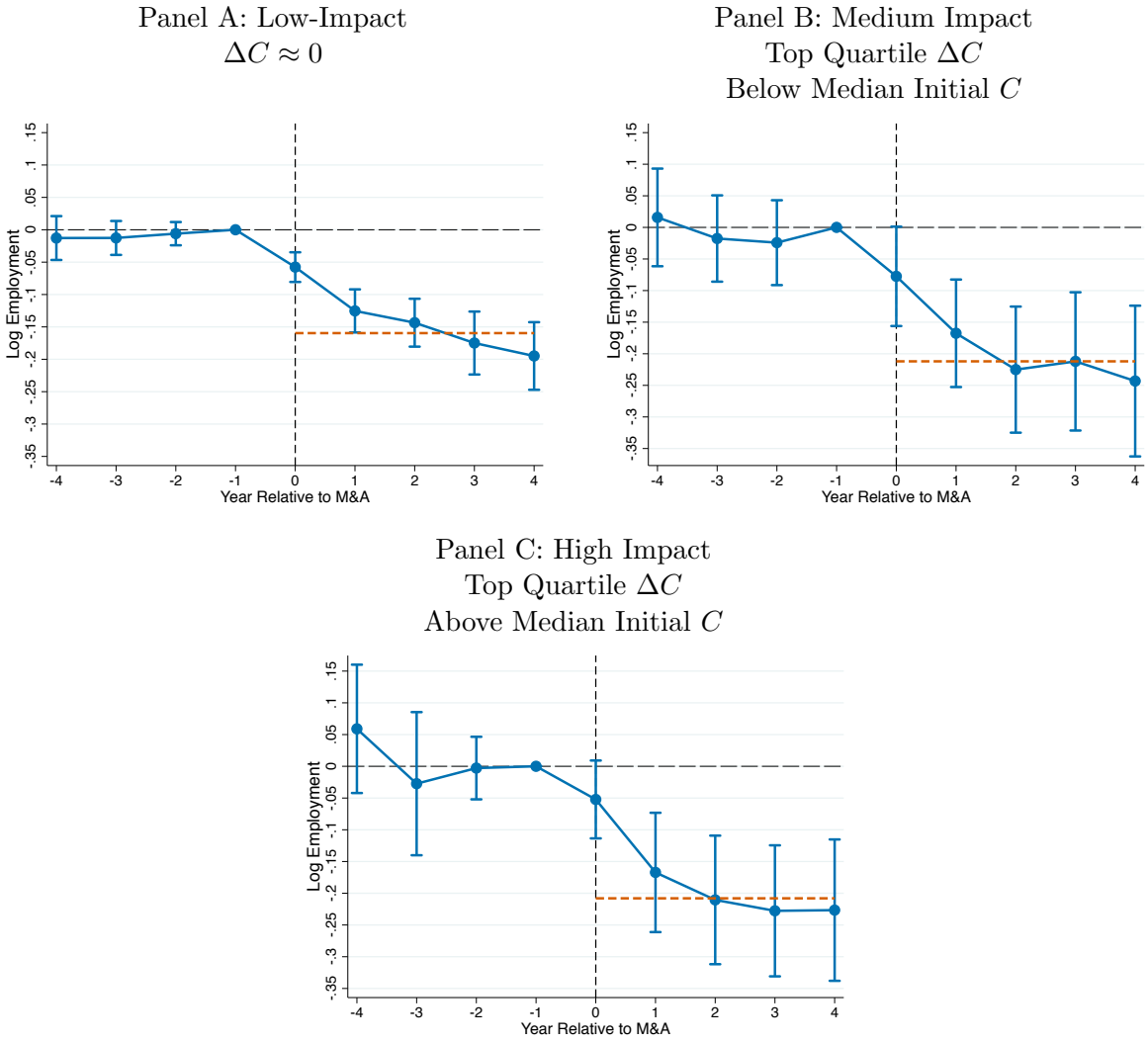
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment. The regressions are estimated on the sample described in the notes to Table 1, which contain details on the matching algorithm used to identify control establishments. High employment establishments are above the median level of employment, while low employment establishments are below the median level of employment. 95 percent confidence intervals based on standard errors two-way clustered at the 4-digit NAICS industry and commuting zone level are displayed.

Figure 1.15: Difference-in-Differences Estimates of the Effect of M&A on Acquiring Firms' Growth Rates



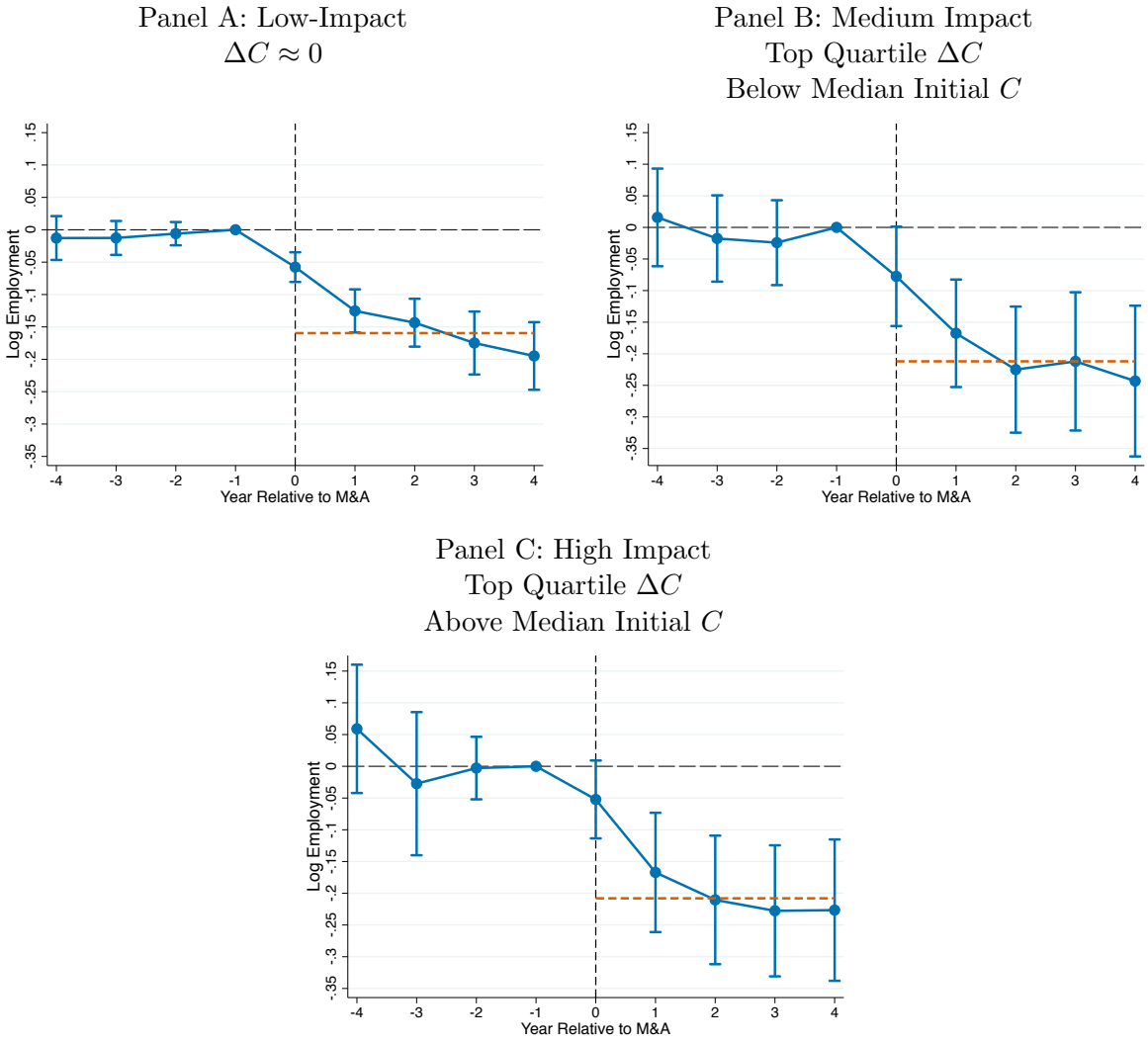
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on the growth rate of the acquiring firm. Growth rates for the acquiring firm are computed using the method described in Haltiwanger et al. (2013), which corrects for mechanical growth due to M&A. To find counterfactual firms, I implement the same matching procedure discussed in Section 1.3.4, at the firm-level rather than the establishment level. In the case of multi-industry and multi-state firms, I match on primary industry and primary state, where the primary industry and primary states are the 4-digit NAICS and states with the most employment of the firm. Regressions are weighted by pre-M&A employment. 95 percent confidence intervals two-way clustered at the primary NAICS-4-digit code and the primary commuting zone level.

Figure 1.16: Difference-in-Differences Estimates of the Effect of M&A on Establishment Employment in Nontradable Industries



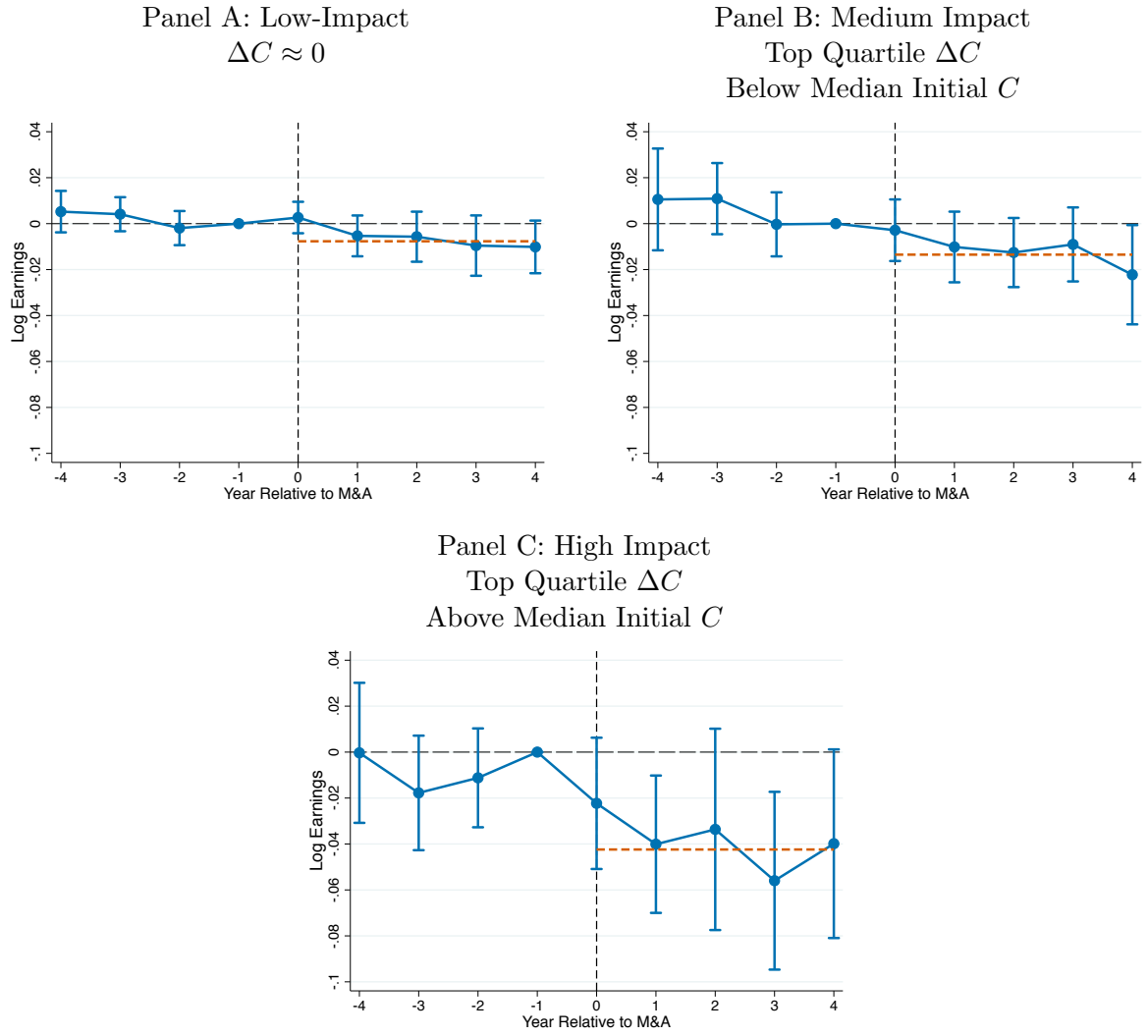
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment in nontradable industries, defined as industries that do not belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. Details on the matching algorithm used to identify control establishments appear in the notes to Table 1 and Section 1.3.4. Panel A displays results for establishments exposed to low-impact mergers, which occur when the change in concentration is below the top quartile ($\Delta C \approx 0$). Panel B displays results for establishments exposed to medium-impact mergers, which occur when the change in concentration is in the upper quartile and the establishment is in a below-median concentration market. Panel C displays results for establishments in high-impact mergers, which occur when the change in concentration is in the upper quartile and the establishment is in an above-median concentration market. Concentration is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. 95 percent confidence intervals are two-way clustered at the 4-digit NAICS and commuting zone level.

Figure 1.17: Difference-in-Differences Estimates of the Effect of M&A on Establishment Employment in Tradable Industries



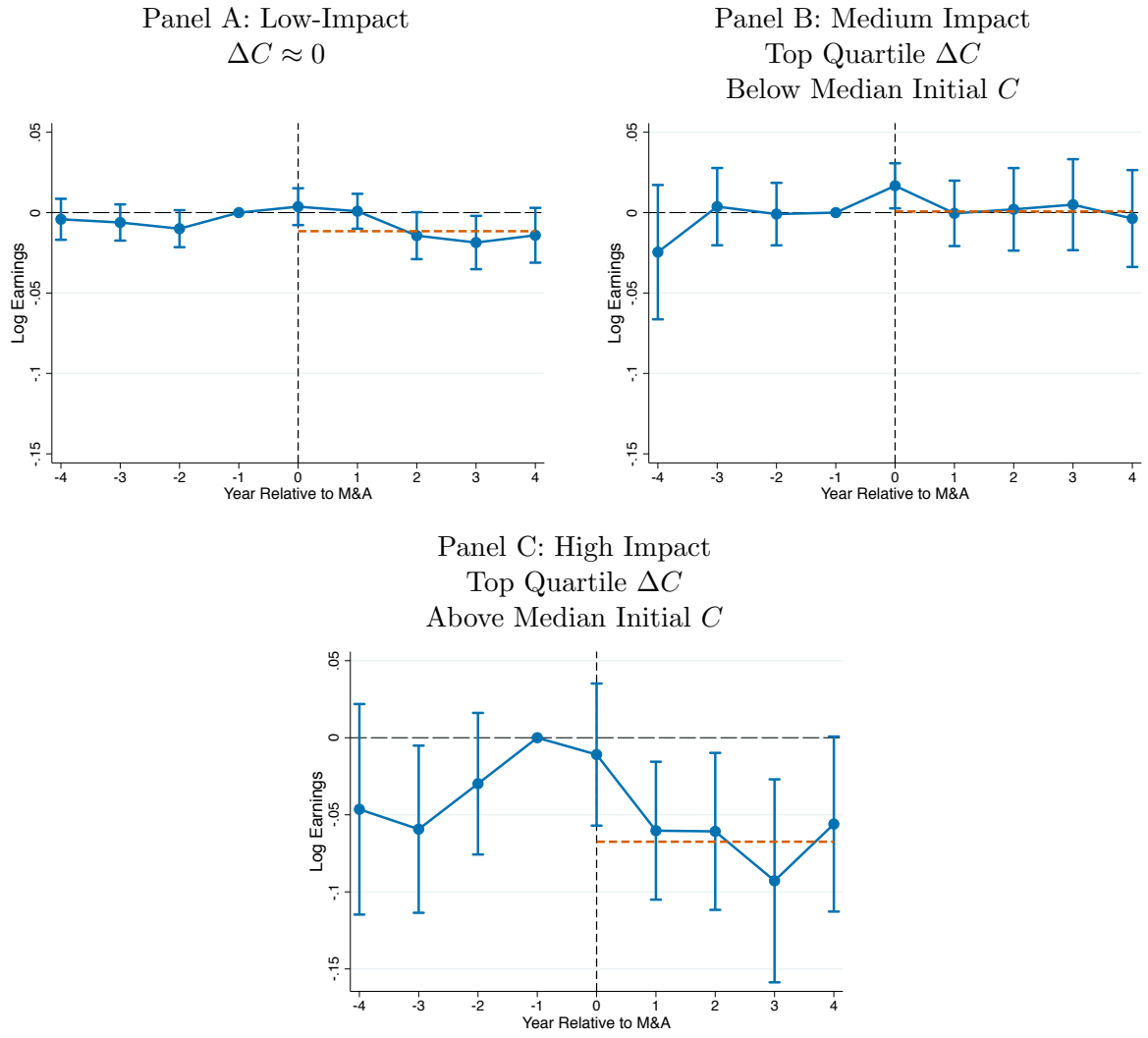
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment in tradable industries, defined as industries that belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. For details on the matching algorithm used to identify control establishments appear in the notes to Table 1 and Section 1.3.4. Panel A displays results for establishments exposed to low-impact mergers, which occur when the change in concentration is below the top quartile ($\Delta C \approx 0$). Panel B displays results for establishments exposed to medium-impact mergers, which occur when the change in concentration is in the upper quartile and the establishment is in a below-median concentration market. Panel C displays results for establishments in high-impact mergers, which occur when the change in concentration is in the upper quartile and the establishment is in an above-median concentration market. Concentration is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. 95 percent confidence intervals are two-way clustered at the 4-digit NAICS and commuting zone level.

Figure 1.18: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings in National Mergers



Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings for mergers between firms that operate in at least 5 commuting zones (i.e. national mergers). Panel A displays results for workers exposed to low-impact mergers, which occur when the change in concentration is below the top quartile ($\Delta C \approx 0$). Panel B displays results for workers exposed to medium-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in a below-median concentration market. Panel C displays results for workers exposed to high-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in an above-median concentration market. Concentration is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. The figure restricts to firm stayers, defined as workers employed in time t at the same firm as in $t^* - 1$. For details on the matching algorithm used to identify control workers, see Section 1.3.4. 95 percent confidence intervals are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

Figure 1.19: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings in Tradable Industries



Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings for firm stayers in tradable industries, which are defined as industries that belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. Panel A displays results for workers exposed to low-impact mergers, which occur when the change in concentration is below the top quartile ($\Delta C \approx 0$). Panel B displays results for workers exposed to medium-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in a below-median concentration market. Panel C displays results for workers exposed to high-impact mergers, which occur when the change in concentration is in the upper quartile and the worker is employed in an above-median concentration market. Concentration is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. The figure restricts to firm stayers, defined as workers employed in time t at the same firm as in $t^* - 1$. For details on the matching algorithm used to identify control workers, see Section 1.3.4. 95 percent confidence intervals are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

Table 11: Impact of M&A on Workers' Log Annual Earnings by Worker Characteristics

	All Workers				Firm Stayers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-M&A x Age < 40	-0.002				-0.003			
	(0.003)				(0.003)			
Post-M&A x Age 40-50	-0.006				-0.000			
	(0.004)				(0.003)			
Post-M&A x Age >50	-0.018^{***}				-0.010^{***}			
	(0.005)				(0.004)			
Post-M&A x Q1		-0.003				0.001		
		(0.012)				(0.012)		
Post-M&A x Q2		-0.006				-0.006		
		(0.005)				(0.004)		
Post-M&A x Q3		-0.003				-0.004		
		(0.003)				(0.003)		
Post-M&A x Q4		-0.006^{*}				-0.004		
		(0.003)				(0.003)		
Post-M&A x Q5		-0.012^{***}				-0.004		
		(0.003)				(0.003)		
Post-M&A x Acquirer			-0.003				-0.005	
			(0.004)				(0.003)	
Post-M&A x Target			-0.015^{***}				-0.003	
			(0.004)				(0.005)	
Post-M&A x Male				-0.008^{*}				-0.004
				(0.003)				(0.003)
Post-M&A x Female				-0.008^{*}				-0.004
				(0.004)				(0.004)
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Worker-Years	32,000,000	32,000,000	32,000,000	32,000,000	25,700,000	25,700,000	25,700,000	25,700,000

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings for different observable characteristics of the workers. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 12: Impact of M&A on Job Transitions by Worker Characteristics

	Job Transition			
	(1)	(2)	(3)	(4)
Post-M&A x ¡ 40	0.022***			
	(0.004)			
Post-M&A x 40-50	0.023***			
	(0.004)			
Post-M&A x ¿50	0.024***			
	(0.005)			
Post-M&A x Q1		0.008***		
		(0.003)		
Post-M&A x Q2		0.016***		
		(0.003)		
Post-M&A x Q3		0.021***		
		(0.004)		
Post-M&A x Q4		0.026***		
		(0.005)		
Post-M&A x Q5		0.031***		
		(0.006)		
Post-M&A x Acquirer			0.004	
			(0.004)	
Post-M&A x Target			0.056***	
			(0.006)	
Post-M&A x Male				0.024***
				(0.004)
Post-M&A x Female				0.031***
				(0.007)
Worker FE	Yes	Yes	Yes	Yes
R squared	0.232	0.233	0.229	0.251
Worker-Years	34,800,000	34,800,000	34,800,000	34,800,000

Note: This table reports difference-in-differences estimates of the effect of M&A on a job transition. A job transition occurs if a worker switches between two firms or a worker transitions from nonemployment to employment (or vice versa). For details on the matching algorithm used to identify control workers, see the notes to Table 2 and Section 1.3.4. Age¡40 indicates the worker is less than 40 years old in the year prior to the merger. Age 40-50 indicates the worker is between 40 and 50 years old. Age¿50 indicates the worker is greater than 50 years old in the year prior to the merger. Q1-Q5 refer to the worker's earnings quintile within the firm, where Q1 indicates bottom quintile and Q5 indicates top quintile. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 13: Robustness: Impact of M&A on Log Annual Earnings for Firm Stayers by Concentration Changes

	High Impact	Medium Impact	Low Impact		
	Pred $\Delta C^{MA} > 0$	Pred $\Delta C^{MA} > 0$			
	High Initial C	Low Initial C	Pred $\Delta C^{MA} \approx 0$	Diff (1)-(2)	Diff (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Panel A: Tradables Industrial HHI<500					
Post-M&A	-0.072^{***}	0.007	-0.013	-0.079^{***}	-0.059^{**}
	(0.026)	(0.010)	(0.008)	(0.028)	(0.027)
Panel B: Above Median Employment Firms					
Post-M&A	-0.049^{***}	-0.011	-0.017^{**}	-0.038^{**}	-0.031^{**}
	(0.012)	(0.009)	(0.008)	(0.015)	(0.014)
Panel C: Known Location in LEHD					
Post-M&A	-0.032^{**}	0.002	-0.005	-0.034^{**}	-0.027^{*}
	(0.014)	(0.009)	(0.004)	(0.016)	(0.015)
Approx Worker-Years	2,700,000	3,700,000	19,300,000	–	–

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings. I estimate a flexible specification that allows for dynamic treatment effects and compute the aggregate effect as the average of the four post-event coefficients. The sample restricts to firm stayers who are defined as workers employed in time t at the same firm as in $t^* - 1$. For details on the matching algorithm used to identify control workers, see the notes to Table 2 and Section 1.3.4. Tradable industries belong to the following NAICS two-digit codes: 11, 21, 31, 32, 33 and 55. HHI¡500 implies the firm produces in a 4-digit industry in which the national HHI for the 4-digit industry is less than 500. Known location in the LEHD implies that the commuting zone of the worker in the LEHD data is known with certainty. Standard errors are two-way clustered at the worker and 4-digit-NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 14: Summary Statistics of Top-Ventile Markets vs. Other Markets

	Top Ventile Markets	Below Top Ventile Markets
	(1)	(2)
Manufacturing	0.23	0.15
Wholesale Trade	0.12	0.10
Retail Trade	0.10	0.15
Finance	0.07	0.06
Health	0.15	0.09
College Graduate	0.24	0.25
West	0.29	0.36
South	0.36	0.25
Age	39.51	39.31
Female	0.45	0.45
Total Markets	200	3,300

Note: This table displays summary statistics for the sample of markets that experience at least one percent change in the flows-adjusted concentration measure due to merger activity. I further split the summary statistics by whether the market experiences a concentration increase in the top-ventile of all concentration increases. An indicator for top-ventile is used as an instrument to identify the impact of local labor market concentration on labor market outcomes in Table 10.

Table 15: Heterogeneity and Robustness: IV Estimates of the Elasticity of Earnings with Respect to Local Labor Market Concentration (Flows-Adjusted)

	Average Log Market Earnings				
	(1)	(2)	(3)	(4)	(5)
LogC×National	-0.262^{*}	-0.258^{**}	-0.309^{*}		
	(0.146)	(0.128)	(0.176)		
Log C×Tradable				-0.331^{*}	
				(0.180)	
Log C×Nontrad.				-0.202^{**}	
				(0.102)	
Log C×Tradable×High C					-0.392^{*}
					(0.216)
Log C×Tradable×Low C					0.048
					(0.096)
Log C×Nontrad.×High C					-0.235^{**}
					(0.117)
Log C× Nontrad.×Low C					0.051
					(0.121)
Market-Years	24,000	24,000	24,000	24,000	24,000
4-dig. NAICS-by-CZ FE	Yes	Yes	Yes	Yes	Yes
1-dig. NAICS-by-CZ-year FE	Yes	Yes	Yes	Yes	Yes

Note: This table reports instrumental variables estimates of the elasticity of earnings with respect to local labor market concentration (flows-adjusted) by using a top-ventile merger as the excluded instrument for concentration. A market is defined as a 4-digit NAICS by commuting zone cell. In Column 1, a national merger is defined as a merger between two firms both operating in at least two commuting zones. In Column 2, a national merger is defined as a merger between two firms both operating in at least 5 commuting zones. In Column 3, a national merger is defined as merger between two firms both operating in at least 10 commuting zones. Tradable industries belong to the following NAICS two-digit codes: 11, 21, 31, 32, 33 and 55. Nontradable industries belong to any other NAICS two-digit code. Standard errors clustered at the 4-digit NAICS by commuting zone level appear in parentheses. All regressions are weighted by employment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 16: Examples of 4-digit NAICS codes

NAICS code	Description
1124	Sheep and Goat Farming
2121	Coal Mining
2212	Natural Gas Distribution
2382	Building Equipment Contractors
3115	Dairy Product Manufacturing
3131	Dairy Product Manufacturing
3241	Basic Chemical Manufacturing
3342	Communications Equipment Manufacturing
4251	Wholesale Electronic Markets
4411	Automobile Dealers
4841	General Freight Trucking
4911	Postal Services
5174	Satellite Communications
5232	Securities and Commodities Exchange
5621	Waste Collection
5622	Waste Treatment and Disposal
6212	Office of Dentists
6216	Home Health Care Services
7132	Gambling Industries
8111	Auto Repair
9231	Administration of Human Resources

Note: This table lists descriptions for a number of different 4-digit NAICS (2007) industry codes.

Table 17: Industries Ranked by Flows-Adjusted Measure of Local Labor Market Concentration

Rank	Industry	Concentration
1	Construction	0.014
2	Other	0.015
3	Real Estate	0.016
4	Professional, Scientific and Technical	0.017
5	Management of Businesses	0.018
6	Administrative Support	0.023
7	Wholesale Trade	0.026
8	Agriculture	0.041
9	Food and Accommodation	0.042
10	Arts and Entertainment	0.054
11	Retail Trade	0.056
12	Health Care	0.056
13	Education	0.063
14	Finance	0.068
15	Public Administration	0.080
16	Transportation	0.097
17	Information	0.108
18	Mining	0.144
19	Manufacturing	0.172
20	Utilities	0.347

Note: This table orders industries by average (employment-weighted) concentration, where labor market concentration is measured using the flows-adjusted measure of local labor market concentration that takes into account flows across industries.

Table 18: Industries Ranked by HHI (4-digit NAICS by commuting zone) of Local Labor Market Concentration

Rank	Industry	Concentration
1	Construction	0.056
2	Professional, Scientific and Technical	0.059
3	Other	0.083
4	Agriculture	0.092
5	Real Estate	0.099
6	Food and Accommodation	0.110
7	Wholesale Trade	0.112
8	Administrative Support	0.115
9	Management of Businesses	0.156
10	Finance	0.169
11	Health Care	0.184
12	Arts and Entertainment	0.202
13	Education	0.220
14	Mining	0.234
15	Retail Trade	0.247
16	Information	0.307
17	Transportation	0.312
18	Manufacturing	0.346
19	Public Administration	0.365
20	Utilities	0.617

Note: This table orders industries by average (employment-weighted) concentration, where labor market concentration is measured using 4-digit NAICS industries by commuting zone cells as the definition of the labor market.

Table 19: Probability of Within 4-digit NAICS transitions

Rank	Industry	Within Industry Transition Rate
1	Management of Businesses	0.092
2	Arts and Entertainment	0.139
3	Real Estate	0.149
4	Wholesale Trade	0.149
5	Retail Trade	0.157
6	Other	0.193
7	Administrative Support	0.209
8	Manufacturing	0.211
9	Transportation	0.217
10	Public Administration	0.229
11	Information	0.248
12	Food and Accommodation	0.258
13	Professional, Scientific and Technical	0.268
14	Construction	0.283
15	Health Care	0.309
16	Education	0.310
17	Agriculture	0.313
18	Utilities	0.325
19	Finance	0.337
20	Mining	0.347

Note: This table orders industries by average (employment-weighted) within 4-digit industry transitions rates. The interpretation of the 0.21 on manufacturing is as follows: of all the job transitions from workers in 4-digit NAICS codes that belong to manufacturing (i.e. 2-digit codes 31-33), 21 percent of those transitions are to a job in the same 4-digit NAICS code.

Table 20: Across-Market Correlation between Employment and Local Concentration

	Log HHI CZ-by-industry	Log C
	(1)	(2)
Log Employment	-0.284^{***}	-0.038^{***}
	(0.002)	(0.002)
R^2	0.317	0.004
Market-Years	1,166,000	1,166,000

Note: This table regresses log flows-adjusted concentration (Column 1) and log HHI (Column 2) on total market employment. An observation in this regression is a market (4-digit NAICS by commuting zone) by year. Standard errors appear in parentheses and are clustered at the 4-digit NAICS by commuting zone level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

1.8 Data Appendix

1.8.1 Longitudinal Business Database

Overview

The establishment-level data is drawn from the U.S. Census Bureau's Longitudinal Business Database (LBD), a near-universe of establishments operating with positive employment in the United States, from 1975-2015 (for this project I have access to data starting from 1985). In the LBD, an establishment is defined as a specific physical location where business occurs. The LBD contains information on payroll, employment, industry, and location. In addition to establishment-level identifiers, the LBD contains enterprise-level identifiers (labeled *firmid*), where an enterprise reflects all establishments under common ownership control.

LBD Variable Definitions

firmid: The enterprise-level identifier that identifies the ultimate ownership of the establishment. While the variable name is *firmid*, this is distinct to the firm-level identifier that is available in the LEHD, which is the EIN. Therefore, throughout the paper, I refer to the *firmid* available in the LBD as the enterprise ID.

lbdnum: The establishment-level identifier that indicates a single physical location. The identifier is time-invariant and does not change due to changes in ownership of the establishment.

Employment: Establishment-level employment as of March 12^{th}.

Payroll: Annual establishment-level payroll.

Industry: Unless otherwise stated, industry is defined by 4-digit North American Industry Classification Systems (NAICS) codes. In 1997, the U.S. Census switched from using Standard Industrial Classification (SIC) to NAICS. While most of the analysis in the paper does

not require industrial classification pre-1997 (I study mergers 1999-2009), the analysis that does require pre-1997 industrial classification uses time-consistent NAICS codes provided by Fort et al. (2016).

Tradable: Tradable establishments are listed as belonging to the following NAICS two-digit codes: 11, 21, 31, 32, 33 and 55. 11 is agriculture, forestry and fishing, 21 is mining, quarrying and oil and gas extraction, 31-33 are manufacturing and 55 is management of companies and enterprises.

Nontradable: Nontradable establishments are any establishments that are not in the tradable group.

HHI CZ-by-industry: The sum of squared market shares where the market is defined by a commuting zone by 4-digit NAICS interaction.

HHI CZ: The sum of squared market shares where the market is defined by a commuting zone.

National merger: A national merger is defined as a merger between two firms that own establishments in multiple commuting zones. For most results, I require both firms involved in the merger to own establishments across at least 5 commuting zones. In some specifications I alter this definition to at least 2 or at least 10 commuting zones.

Incidental: An establishment is incidental to a merger if the establishment produces in a secondary or tertiary lines of business.

1.8.2 Constructing Firm Growth Rates in the LBD

To construct the growth rate of firm j in year t, I compute:

$$g_{jt} = \frac{E_{j,t} - E_{j,t-1}}{\frac{1}{2}(E_{j,t} + E_{j,t-1})} \tag{1.31}$$

Where $E_{j,t}$ is employment in firm j at time t and $E_{j,t-1}$ is employment in firm j at time $t-1$. In constructing $E_{j,t}$ and $E_{j,t-1}$ I use the longitudinal establishment identifiers to correct for changes in employment due only to changes in ownership. For example, imagine a single unit firm with 100 employees buys another single unit firm with 100 employees. If no employees are laid off, then $E_{j,t} = 200$ and $E_{j,t-1} = 200$. Therefore, the increase in 100 workers in firm j is not counted as employment growth, given all of those workers were previously employed by the target firm. Therefore, in the absence of layoffs, the merger will result in zero employment growth.

If instead, the firm lays off half the workforce in the target firm, then $g_{jt} = \frac{-50}{200} = -0.25$. Therefore, in this case, the firm shrank by -0.25 even though firm j technically employs more workers at time t than at time $t-1$. If M&A incentivizes organic growth through new establishments, then this will be captured in the firm-level analysis. For example, if the acquiring firm lays off half the workforce at the target firm (-50), but subsequently opens a brand new establishment with 100 workers, the net growth will be equal to $\frac{50}{200} = 0.25$.

1.8.3 Longitudinal Employer Household Dynamics

Earnings: The cumulative annual earnings paid to a given worker aggregated across all employers. Earnings in the LEHD include "gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging" (BLS, 1997). Therefore, earnings do not include health care benefits.

Dominant Employer: If an individual has earnings from multiple employers in a given year, then the employer associated with the most earnings is the dominant employer.

Education: I primarily distinguish between college and no college in this paper. One important caveat for the education variable is that a large portion of the education variables are imputed (around 80 percent). The imputation procedure is performed by Census researchers and is done by linking the LEHD to the Decennial Census. State-specific logit models are

then estimated to predict the education levels for all workers with missing education using the following set of observables: age categories, earnings categories, and industry dummies.

Age: The age of the worker.

EIN: A federal employer identification number used for tax purposes. A given firm (e.g. General Electric) may own multiple EINs. Additionally, a given EIN may own multiple establishments. Therefore, the EIN is a concept between an enterprise and an establishment.

SEIN: state employer identification number. A given firm (e.g. General Electric) may own multiple EINs. Within each state, a firm has a unique SEIN. A given SEIN, however, may own multiple establishments within a state. Therefore, the SEIN is a unit of aggregation between a firm (i.e. firmid in the LBD) and an establishment (i.e. lbdnum in the LBD).

1.8.4 Linking the LBD and LEHD

In the LBD, I identify M&A by switches in the variable "firmid." Therefore, when turning to worker-level analysis, I sample all the workers that are employed in the firms engaged in the merger activity. However, the LEHD contains EIN numbers, and not a "firmid." To link the two datasets, I use the Standard Statistical Establishment List (SSEL) as a bridge. The SSEL is an establishment-level dataset that is used to construct the LBD. The SSEL contains EIN and therefore can be used to link the LEHD and LBD.

1.9 Model Appendix

1.9.1 Derivation of Market-Level Wage in Cournot Model

The firm-specific labor supply elasticity in the Cournot model is given by:

$$\frac{1}{\eta_j} = \frac{\partial w_m(L_m)}{\partial l_j}\frac{l_j}{w_m(L_m)} = \frac{\partial w_m(L_m)}{\partial L_M}\frac{\partial L_m}{\partial l_j}\frac{l_j}{w_m(L_m)}\frac{L_m}{L_m} = \frac{s_j^l}{\eta_m} \tag{1.32}$$

Plugging η_j into Equation (1.2) and rearranging yields:

$$\frac{\theta_j}{w_m} = \frac{s_j^l}{\eta_m} + 1 \tag{1.33}$$

where η_m is the elasticity of labor supply facing the entire market and s_j^l is firm j's employment share. Multiplying both sides of the equation by s_j^l and summing over all j first-order conditions yields:

$$\sum_j s_j^l \frac{\theta_j}{w_m} = \sum_j s_j^l \left(\frac{s_j^l}{\eta_m} + 1\right) = \frac{HHI}{\eta_m} + 1 \tag{1.34}$$

where $HHI = \sum_j (s_j^l)^2$ is the Herfindahl-Hirschman index based on employment shares. Therefore, letting θ_m be the average value of marginal product in the market, the market wage is equal to:

$$w_m = \left(\frac{\eta_m}{HHI + \eta_m}\right)\theta_m \tag{1.35}$$

1.9.2 Relationship between C and standard HHI measures

Denote HHI_{mc}^{CZ-IND} as the HHI index if the definition of the labor market is an industry by commuting zone cell (in practice 4-digit NAICS by commuting zone). Denote HHI_{mc}^{CZ} as the HHI index if the definition of labor market is a commuting zone. Then it is straightforward to show the following proposition:

Proposition 3. *With no job mobility between industries, then* $C_{mc} = HHI_{mc}^{CZ-IND}$. *With random mobility across industries, then* $C_{mc} = HHI_{mc}^{CZ}$.

First, take the case in which there is zero mobility between industries. In this case, $P(m|m) = 1$ and $P(k|m) = 0$ for all $m \neq k$. Therefore, $\alpha_{m\to k} = 0$ for all $k \neq m$. This implies that the weighted market share of firm j in market m in commuting zone c is equal to the standard labor market share ($\tilde{s}_{jmc} = s_{jmc}$). Therefore:

$$C_{mc} = \sum_{j\in c} (\tilde{s}_{jmc})^2 = \sum_{j\in m} (s_{jmc})^2 = HHI_{mc}^{CZ-IND}$$

Where the second equality substitutes $\tilde{s}_{jmc} = s_{jmc}$ and follows from the fact that $s_{jmc} = 0$ for all firms that are not employing workers in industry m (indicating the second summation is not over all firms in the commuting zone, but rather all firms in the given industry m).

If workers move randomly across industries, then within a commuting zone $\frac{P(k|m)}{P(m|m)} = \mathbb{E}[\frac{L_k}{L_m}]$. That is, the relative transition probabilities are simply equal to the relative sizes, where again, the relative size is the expectation across commuting zones. Therefore, $\alpha_{m\to k} = 1$ for all k. Denoting $\sum_{k\in c} l_{jkc} = l_{jc}$ as the total employment of firm j in commuting zone c and s_{jc} as firm j's share of total employment, C_{mc} becomes:

$$C_{mc} = \sum_{j\in c} \left(\frac{\sum_{m'\in c} l_{jm'c}}{\sum_{m'\in c} L_{m'c}} \right)^2 = \sum_{j\in c} (s_{jc})^2 = HHI_{mc}^{CZ}$$

□

1.9.3 Example of production function with overhead labor

Following Bartelsman et al. (2013), I assume each firm has a production technology of the following form:

$$Y_j = \Omega_j \left(l_j - f_j\right)^{\alpha} k^{\beta} \tag{1.36}$$

Where f_j is a fixed level of overhead labor needed for production. While f_j is firm-specific, it is not a parameter chosen by the firm. Each firm has a potentially different amount of overhead labor it needs to employ to produce and this is taken as exogenous. Given this functional form, the marginal product of labor is given by:

$$\Omega_j \frac{\partial F}{\partial l_j} = \Omega_j \alpha (l_j - f_j)^{\alpha-1} k^{\beta} \tag{1.37}$$

Conceptually, I allow mergers to impact technology in two ways. First, mergers could reduce the level of f_j for a firm through pooling resources. For example, imagine a fixed cost of production is setting up a human resources department. The merged firm may not need two human resources departments and therefore can layoff the entire human resources department at one of the firms. Note that in this case, the layoffs have no impact on marginal product of the remaining workers. To see this, note that total labor is equal to the labor employed for fixed costs of production, and labor employed for variable costs (i.e. $l_j = v_j - f_j$). Therefore:

$$\frac{\partial \Omega_j \frac{\partial F}{\partial l_j}}{\partial f_j} = \Omega_j \alpha (\alpha - 1)(l_j - f_j)^{\alpha-2} k^{\alpha} (\frac{\partial l_j}{\partial f_j} - \frac{\partial f_j}{\partial f_j}) = 0 \tag{1.38}$$

where the last equality follows due to the fact $\frac{\partial l_j}{\partial f_j} = -\frac{\partial f_j}{\partial f_j}$. Therefore, laying off workers related to fixed costs of production has no impact on the marginal product of labor. Therefore, assuming no changes in labor market power or product market power, reductions in labor due to reductions in fixed cost should result in decreases in employment with no change in wages. In this case, mergers lower the labor share of the combined firm. This is the same channel discussed in Autor et al. (2017), who argue the fall in the labor share is due to production shifting to large firms that have lower share of fixed costs in labor over total value-added.

1.9.4 Alternative Models linking Market Shares or Concentration to Wages

This appendix discusses alternative modeling decisions all of which yield equilibrium relationships between concentration and wages. A common theme throughout all models is that (1): concentration lowers wages and (2) the impact of concentration on wages depends on the initial concentration.

To setup the exposition, note that wages will be set according to the "standard" markdown equation:

$$w_j = \left(\frac{\eta_j}{\eta_j + 1}\right) \theta_j \tag{1.39}$$

Where θ_j is the value of marginal product of workers which is assumed to be fixed for simplicity. The main differences between the models will be exactly how to create a mapping from the labor-supply elasticity η_j to concentration. This will imply different models yield different model-relevant measures of concentration.

Dominant Firm Model

The dominant firm model is a simple model of imperfect competition sometimes utilized in antitrust cases. While simple, many of the insights generated by the dominant firm model are similar to those generated in more complicated models of competition. The illustration here closely follows Kaplow and Shapiro (2007), though it applies the model to the labor market rather than the product market.

Assume there is a dominant firm j. The labor supplied to the dominant firm j is equal to the market supply at wage w minus the labor supplied to fringe firms:

$$L_j(w) = M(w) - R(w) \tag{1.40}$$

We can rewrite this equation as:

$$\frac{w_j}{L_j}\frac{dL_j}{dw_j} = \frac{w_j}{L_j}\frac{dM_j}{dw_j} - \frac{w_j}{L_j}\frac{dR_j}{dw_j} \tag{1.41}$$

Rewriting these terms in the form of elasticities yields:

$$\eta_j = \frac{\eta_M}{s_j} - \frac{\eta_R(1-s_j)}{s_j} \tag{1.42}$$

Where s_j is the market share of firm j. Note that if the firm makes up the entire market, then the elasticity of the firm will be equal to the market elasticity and the firm will choose w_j as a monopsonist. Given $\eta_R < 0$, a fringe set of firms increases the elasticity facing the dominant firm. In an extreme version, if $\eta_R = -\infty$ then if the dominant firm lowers the wage, all workers move to the fringe firms, and therefore we are in a perfectly competitive scenario where no firms have market power. Therefore, in the dominant firm model, η_R is capturing the availability of substitutes available to workers.

A merger can be seen as the dominant firm increasing s_j. Differentiating Equation (1.42) with respect to market share s_j yields:

$$\frac{\partial \eta_j}{\partial s_j} = \frac{1}{s_j}\left[\eta + \eta_R\right] \tag{1.43}$$

Therefore the elasticity of wages with respect to market shares is given by:

$$\frac{\partial ln(w_j)}{\partial ln(s_j)} = \frac{\partial ln(w_j)}{\partial \eta}\frac{\partial \eta}{\partial s_j}s_j = \frac{1}{\eta+1}\left[1 + \frac{\eta_R}{\eta}\right] \tag{1.44}$$

Given that s_j is a sufficient statistic for η and η is strictly declining in s, this implies that the elasticity of wages will be larger the higher the initial market share. Therefore, it yields a similar insight as the main model. Increases in concentration at low initial values of

concentration generate smaller changes in market outcomes. However, in the dominant firm model the market share of the largest firm is the relevant concentration measure.

A Discrete Choice Model of the Labor Market

In this section I present a discrete model of the labor market following Card et al. (2018) and Berger et al. (2019). In the model, workers have preferences over firms which gives firms monopsony power over their workers. As in Berger et al. (2019), I assume preferences are drawn from a generalized extreme value distribution (GEV) with a nested logit structure. This implies that strategic considerations within a nest matter for wage decisions. Therefore, individual firms are not atomistic and changes in concentration of markets will have impacts on wages and employment.

To begin, assume that utility worker i derives from working in firm j in nest B_l is given by:

$$U_{ij} = lnw_j + a_j + v_{ij}, \tag{1.45}$$

where w_j is a firm-specific wage, a_j is an amenity of the firm that is valued the same by all workers, and v_{ij} is an idiosyncratic preference for firm j, which could take into account, for example, commuting costs. I assume:

$$v \sim exp\left[-\sum_{i=1}^{I}\left(\sum_{j\in B_i} e^{-\frac{\varepsilon_{ij}}{\sigma_w}}\right)^{\frac{\sigma_w}{\sigma_{b_l}}}\right] \tag{1.46}$$

Given this error structure, the probability a worker choose to work in firm j is given by:

$$P(U_{ij} = \arg\max_{j'} U_{ij'}) = \underbrace{\frac{exp(\frac{ln(w_j)}{\sigma_w})}{\sum_{j'\in S_l} exp(\frac{ln(w_{j'})}{\sigma_w})}}_{\text{Prob choose } j \text{ in } l} \times \underbrace{\frac{\left[\sum_{j\in S_l} exp(\frac{ln(w_j)}{\sigma_w})\right]^{\frac{\sigma_w}{\sigma_{b_l}}}}{\sum_{l'}\left[\sum_{k\in S_{l'}} exp(\frac{ln(w_k)}{\sigma_w})\right]^{\frac{\sigma_w}{\sigma_{b_l}}}}}_{\text{Prob choose market } l} \tag{1.47}$$

Therefore, the labor supply of firm j is simply Equation (1.47) multiplied by the total number of workers. Taking the log of the labor supply equation yields:

$$lnl_j = \frac{ln(w_j)}{\sigma_w} - ln\left(\sum_{j' \in S_l} exp(\frac{ln(w_{j'})}{\sigma_w})\right) + ln\left(\frac{\left[\sum_{j \in S_l} exp(\frac{ln(w_j)}{\sigma_w})\right]^{\frac{\sigma_w}{\sigma_{b_l}}}}{\sum_{l'}\left[\sum_{k \in S_{l'}} exp(\frac{ln(w_k)}{\sigma_w})\right]^{\frac{\sigma_w}{\sigma_{b_l}}}}\right) + log(N), \quad (1.48)$$

where N is the number of workers. Therefore:

$$\frac{\partial lnl_j}{\partial lnw_j} = \frac{1}{\sigma_w} - \frac{1}{\sigma_w}s_j + \frac{1}{\sigma_{b_l}}s_j - \frac{1}{\sigma_{b_l}}\frac{exp(\frac{ln(w_j)}{\sigma_w})}{\sum_{l'}\left[\sum_{k \in S_{l'}} exp(\frac{ln(w_k)}{\sigma_w})\right]^{\frac{\sigma_w}{\sigma_{b_l}}}}, \quad (1.49)$$

where I have assumed that the total number of workers N does not change in response to the change in the wage at a single firm. Additionally, I assume the final term is approximately zero. This is equivalent to assuming that one firm is negligible compared to *all* firms. That is, with enough markets, the share of any given firm is approximately zero. In this case, the labor supply elasticity is therefore given by:

$$\eta_j(s_j) = \frac{\partial lnl_j}{\partial lnw_j} = \frac{1}{\sigma_w}(1 - s_j) + \frac{1}{\sigma_{b_l}}s_j, \quad (1.50)$$

where s_j is based on the labor market (i.e. the share of employment in firm j within a nest). Therefore, in this model, monopsony power (as measured by the labor supply elasticity) depends on σ_w, σ_{b_l} and s_j. σ_w captures substitutability across firms within a market. If σ_w is very high, that implies idiosyncratic utility dominates workers' decisions. Intuitively, if workers have very strong preferences for specific firms, this gives firms more monopsony power given that only large wage differentials across firms will drive workers to transition firms. If σ_w is low, however, then the mean utility of the firm (i.e. $lnw_j + a_j$) will matter a great deal in determining where workers decide to work. In this case, wage competition across firms will be greater, as workers are relatively mobile across firms within a nest.

On the other hand, σ_{b_l} governs substitutability across markets. If σ_{b_l} is large, then this implies that workers are not mobile across labor markets. Therefore, a large σ_{b_l} will tend to reduce wages by reducing wage competition between labor markets. Throughout, I assume $\sigma_w < \sigma_{b_l}$. This implies that jobs within a labor market are better substitutes than jobs in other labor markets.[36] Given this assumption, it is clear that increases in size will reduce the labor supply elasticity, and therefore increase the wage markdown.

$$\frac{\partial \eta_j}{\partial s_j} = \frac{1}{\sigma_{b_l}} - \frac{1}{\sigma_w} < 0 \tag{1.51}$$

This model has a similar insights as the model in the main text. First, market shares are endogenous and therefore regressing changes in wages on changes in market shares may be confounded by omitted variables and simultaneity bias. Second, changes in wages due to changes in market shares depend on the initial market share.

1.9.5 Simple Cournot Model with Entry

This section presents a model of a labor market where firms compete under Cournot assumptions and there is free entry. The main point of this section is to provide a simple formulation of a model in which concentration and earnings will be correlated, but the source of the correlation is not necessarily monopsony power. To begin, I assume each firm has to pay a fixed cost F. Firms are homogenous and produce a perfectly competitive good at with constant marginal revenue product of labor θ. To solve the model analytically, I assume a linear form for the the market wage:

$$w = \alpha + \beta L \tag{1.52}$$

[36] If $\sigma_w = \sigma_{b_l} \forall l$ then the nest structure is irrelevant and the model collapses to a standard logit model.

Where $L = \sum_{j \in m} l_j$ is the total labor demand of the market. Firm j chooses labor input l_j to maximize profits, taking as given the labor demands of all other firms. This results in the following first-order condition:

$$\theta - \beta l_j - (\alpha + \beta L) = 0 \tag{1.53}$$

Summing up the FOCs for all firms in the market yields the aggregate employment L equal to:

$$L = \frac{N}{N+1}(\theta - \alpha) \tag{1.54}$$

Noting that all firms are identical and plugging this expression into firm-specific profits yields:

$$\pi_j = \frac{(\theta - \alpha)^2}{(N+1)^2}\frac{1}{\beta} \tag{1.55}$$

With free entry, profits must equal the fixed cost of entry F. This implies the total number of firms in equilibrium N^* is equal to:

$$N^* = \frac{\theta - \alpha}{\sqrt{\beta F}} - 1 \tag{1.56}$$

Given all firms are identical with equal market shares, the HHI in this case is simply the inverse of the total number of firms N^*. The wage markdown, on the other hand, is given by:

$$\frac{\theta - w}{w} = \frac{\sqrt{\beta F}}{\sqrt{\beta F} + \alpha} \tag{1.57}$$

In this model, if variation in concentration is driven by differences in fixed costs F, then variation in concentration across markets will be reflected in different wage markdowns across markets. However, if variation is driven solely by differences in θ, then markets will have different levels of concentration, different wage levels, but identical monopsony power.

For example, assume a trade shock reduces the value of marginal product, implying a lower θ. Then N^* will decreases implying concentration increases. The wage markdown will remain exactly the same, as it is a function of F, β and α, and none of these parameters have changed. Therefore, to maintain equality, wages must also fall. Therefore, reductions in α will simultaneously increase concentration and lower wages.

1.9.6 Wage Bargaining Model

This section illustrate a model of wage bargaining following Abowd and Lemieux (1993) and He (2018). The key difference in this model is that increases in product market power will tend to increase wages.

To begin, consider a group of $\bar{l}_j$ workers bargaining over both wages and employment level with firm j. The workers seek to maximize $l_j w_j + (\bar{l}_j - l_j)v$, where w_j is the bargained wage, l_j is the employment level, and v is the value of the outside option to the workers. In this case, I assume workers who do not obtain employment reenter the labor force and search for a new job. Therefore, the value of the outside option is equal to the expected wage of the new job minus any search costs c associated with finding a new job.

The workers bargain with a firm that has a profit function $p_j(F(l_j))F(l_j) - w_j l_j$. The threat point for workers is the value of the workers' outside option, while the threat point for the firm is zero profits. The bargaining solution chooses l_j and w_j to maximize:

$$\max_{l_j, w_j} [l_j w_j + (\bar{l} - l_j)v - \bar{l}v]^{\gamma_j} [p_j(F(l_j))F(l_j) - w_j l_j]^{1-\gamma_j} \tag{1.58}$$

Taking the first order conditions for the bargaining problem yields the following two optimality conditions:

$$w_j = \gamma_j \left(\frac{p_j(F(l_j))F(l_j)}{l_j} - v \right) + v \tag{1.59}$$

$$F'(l_j) p_j(F(l_j)) \left(\frac{1}{\varepsilon_j} + 1 \right) = v \tag{1.60}$$

The key difference in this model is that wages depend on three parameters: the bargaining parameter γ_j, the value of workers outside option v, and the revenue per worker, $\frac{p_j(F(l_j))F(l_j)}{l_j}$. Firms with higher revenue per worker, all else equal, will have higher earnings. Therefore, while increases in product market power may decrease the size of the firm, it may raise the average revenue per worker, which leads to higher earnings for incumbent workers.

1.10 Comparisons Between Flows-Adjusted Concentration and HHI

In this section I discuss how the flows-adjusted concentration measure C and a standard HHI compare. Both measures are computed at the 4-digit NAICS by commuting zone level. However, the flows-adjusted concentration measure takes into account flows out of the industry. The two measures will tend to diverge when cross-industry mobility is high.

In Appendix Tables 17 and 18, I report the average (employment-weighted) concentration level for different aggregated sectors and then rank them from least to most concentrated. Appendix Table 17 measures concentration using the flows-adjusted concentration measure while Appendix Table 17 uses the standard HHI measured at the 4-digit NAICS by commuting zone level.

As can be seen in these tablres, the first clear difference is that the levels are much lower for the flows-adjusted concentration measure. This is because, in general, many transitions between jobs are not within the same 4-digit NAICS code, with 76 percent of all job transitions occurring between 4-digit NAICS codes. Therefore, incorporating this information drastically reduces the level of local labor market concentration.

However, the rankings across industries are roughly similar across the two measures of local labor market concentration. For example, the construction industry is the least concentrated according to both measures while utilities is the most concentrated according to both measures. Many of the other industries receive the same ranking according to both measures. A regression of the rank according to C on the rank according to HHI yields a coefficient of 0.9 with an R squared of 0.82.

However, there are a few industries in which the metric of concentration seems to matter a great deal. Finance, for example, is ranked the 14^{th} least concentrated according to flows-adjusted C, but 10^{th} according to HHI. Similarly, mining is ranked 18^{th} least concentrated

according to flows-adjusted C, but 14^{th} according to HHI. Management of businesses is ranked 5^{th} least concentrated according to C, but 9^{th} least according to HHI.

The reason the concentration measures differ for these industries is because they tend to have the most extreme mobility patterns (either higher than average within-NAICS transition rates or lower than average within-NAICS transitions rates). To see this, Appendix Table 19 reports the probability a job transition is within the same 4-digit NAICS code for the different industry groupings.

As can be seen from the table, the industries that are more concentrated under the flows-adjusted concentration measure C (e.g. mining and finance) also have the highest within-industry transition rates. Industries that are less concentrated according to the flows-adjusted concentration measure C (e.g. management of business) have the lowest within-industry transition rates. The transition rates do vary quite a bit across industries, with a minimum of 9.2 percent and a maximum of 34.7 percent.

Another important factor that impacts differences between the flows-adjusted concentration measure C and the HHI is the size of the market. Intuitively, some definitions of industries are very specific while others are quite broad. Offices of physicians (NAICS code 6211), for example, is relatively broad and likely encapsulates many different establishments. Sheep and goat farming (NAICS code 1124) is clearly quite specific and a relatively small industry. This will of course impact concentration if standard industry by commuting zone definitions are used. The sheep and goat farming industry will be mechanically quite concentrated due to the industry being relatively small.

The flows-adjusted concentration measure, however, takes this into account by adjusting for the fact that many flows may be to other industries. To see how this effects concentration measurement in practice, Appendix Table 20 regresses the log of different concentration measures on log employment to see how size relates to measured concentration.

For a standard HHI measured at the commuting zone-by-industry level, a 1 log point increase in employment is associated with a -0.28 log point decline in concentration. Larger

markets tend to be less concentrated. The R^2 of this regression is 0.317, indicating that employment alone explains a substantial portion of the variation in concentration across markets. In column 2, I find that for the flows-adjusted concentration measure, a 1 log point increase in employment is associated with a -0.04 log point decline in concentration. Additionally, employment explains very little of the variation in concentration across markets, with an R^2 of 0.004. Intuitively, there is no mechanical relationship between market size and concentration according to C because C adjusts for flows out of the industry.

1.11 Econometric Appendix

1.11.1 Estimating Elasticities in the Presence of Treatment Effect Heterogeneity

In this section I discuss the identification of the elasticity of wages with respect to concentration in the presence of treatment effect heterogeneity. The exposition follows from a number of papers that discuss estimating treatment effects in setting where treatment effect heterogeneity plays an important role (Angrist and Krueger, 1999; Dobbie and Song, 2017). This section assumes that concentration changes are exogenous and market definition is well-defined.

I assume the true causal relationship between log market concentration $\tilde{C}_m$ and log wages $\tilde{w}_m$ is given by:

$$\tilde{w}_m = \beta_m \tilde{C}_m + \tilde{\theta}_m + u_m \tag{1.61}$$

Where $\tilde{\theta}_m$ is a function of technology and demand-side parameters. For the purposes of this appendix, I assume $\tilde{\theta}_m$ is constant across time periods, and therefore does not bias estimation once first-differences are taken. Taking the first difference yields:

$$\Delta\tilde{w}_m = \beta_m \Delta\tilde{C}_m + \Delta u_m \tag{1.62}$$

There are a number of reasons why β_m would vary by market. For one, workers in some industries may be much more tied to their jobs than workers in others. For example, nurses are likely very tied to the hospital industry. To control for this heterogeneity across markets, I assume there exists a variable W_m such that:

$$\mathbb{E}[\beta_m | \Delta\tilde{C}_m, W_m] = \mathbb{E}[\beta_m | W_m] \tag{1.63}$$

In other words, once we condition on W_m, then treatment intensity is not correlated with treatment effect heterogeneity. In the empirical application, W_m is a categorical variable for 1-digit NAICS by state. Therefore, this assumption would apply that treatment effect heterogeneity within 1-digit NAICS by state cells is uncorrelated with treatment intensity. I will now show under this assumption including W_m into Equation (1.62) will estimate a causal effect of concentration on earnings. To be clear, the effect is a weighted average of covariate-specific effects, and therefore is not identical to the weighted average identified by averaging the subgroup-specific effects β_m over the distribution of workers across markets.

The regression of interest is:

$$\Delta\tilde{w}_m = \alpha + \beta\Delta\tilde{C}_m + W_m'\gamma + \Delta u_i \tag{1.64}$$

where W_m are market fixed effects. By the Frisch-Waugh-Lovell theorem:

$$\beta = \frac{\mathbb{E}[\Delta\tilde{C}_m^R \Delta\tilde{w}_m]}{\mathbb{E}[(\Delta\tilde{C}_m^R)^2]} \tag{1.65}$$

where $\Delta\tilde{C}_m^R$ are residuals from regression of $\tilde{C}_m$ on all other covariates W_m. Next, note that:

$$\Delta\tilde{C}_m^R = \Delta\tilde{C}_m - W_m'\pi = \Delta\tilde{C}_m - \mathbb{E}[\Delta\tilde{C}_m|W_m] \tag{1.66}$$

where the second equality holds if the expectation of $\Delta\tilde{C}_m$ given the covariates is linear, which is always true if W_m is a set of group indicators. Equation (1.66) implies that $\mathbb{E}[(\Delta\tilde{C}_m^R)^2] = \mathbb{E}[var(\Delta\tilde{C}_m|W_m)]$. We can now rewrite the numerator of Equation (1.65) as:

$$\mathbb{E}[\Delta\tilde{C}_m^R \Delta\tilde{w}_m] = \mathbb{E}[(\Delta\tilde{C}_m - \mathbb{E}[\Delta\tilde{C}_m|W_m])(\Delta\tilde{w}_m - \mathbb{E}[\Delta\tilde{w}_m|W_m)] = Cov(\Delta\tilde{w}_m, \Delta\tilde{C}_m|W_m) \tag{1.67}$$

where the first equality holds because $\mathbb{E}[(\Delta\tilde{C}_m - \mathbb{E}[\Delta\tilde{C}_m|W_m])\mathbb{E}[\Delta\tilde{w}_m|W_m]] = 0$ by iterating expectations. We can then rewrite β as:

$$\beta = \frac{\mathbb{E}[Cov(\Delta\tilde{w}_m, \Delta\tilde{C}_m|W_m)]}{\mathbb{E}[var(\Delta\tilde{C}_m|W_m)]} = \frac{\mathbb{E}[\beta(W_m)var(\Delta\tilde{C}_m|W_m)]}{\mathbb{E}[var(\Delta\tilde{C}_m|W_m)]} = \mathbb{E}[\omega(W_m)\beta(W_m)] \tag{1.68}$$

where the second equality follows from Equation (1.65). Equation (1.64) therefore identifies a weighted average of covariate-specific average treatment effects $\beta(W_m)$. The weights are given by:

$$\omega(W_m) = \frac{var(\Delta\tilde{C}_m|W_m)}{\mathbb{E}[var(\Delta\tilde{C}_m|W_m]} \tag{1.69}$$

The weights therefore depend on the variation in treatment conditional on W_m, where values of $W_m = w$ where there is more variation in treatment receive larger weights in computing β. That is, markets with more variation in concentration receive larger weights in estimating β.

Chapter 2

The Impact of Privatization of State-Owned Enterprises on Workers

2.1 Introduction

State-owned enterprises (SOEs) employ about 2 percent of all workers in OECD countries, with a substantially higher proportion in many developing countries (Subramanian and Megginson, 2018). Given the value of SOEs in many countries, privatization remains a popular policy tool for debt-burdened governments to quickly raise large amounts of revenue.[1] For example, in 2018, former Brazilian president Michal Temer introduced a privatization program to sell 57 state-owned assets, including Eletrobras, the largest power utility company in Latin America. Supporters of the privatization program argue that privatization will both raise revenue for the government as well as improve the efficiency of firms. At the same time, workers and trade unions have organized multiple protests, fearing privatization will lower wages and employment.[2] Despite worker and union fears, there is little direct evidence on the impact of privatization on worker outcomes.

[1] In 2015, privatization revenues peaked at over 300 billion USD (Megginson, 2017).

[2] See markets.businessinsider.com/news/interestrates/eletrobras-workers-hold-72-hour-strike-1026878928

This paper studies both the direct impact of privatization on incumbent workers in privatized SOEs as well as the broader labor market impacts of privatization activity. Given the scope of privatization in many countries, large reductions in employment at privatized SOEs will increase the supply of workers to private-sector firms, resulting in lower equilibrium wages. To estimate both the direct impacts and spillovers, I utilize a matched employer-employee dataset which contains information on all formal-sector workers in Brazil. The identification strategy makes use of Brazil's 1990s privatization program which privatized about 3 percent of all formal-sector employment in Brazil.[3] To identify spillovers, I construct a measure of exposure to privatization at the occupation-by-region level which traces how the impacts of privatization propagate through firms in labor markets connected to privatized SOEs by labor mobility.

In the first part of the paper, I estimate the impact of privatization on incumbent workers in privatized SOEs. To do so, I construct a control group using a coarsened-exact-matching algorithm (CEM) to match privatized workers to similar workers in private-sector establishments or never-privatized SOEs. I find that while wages experienced similar trends in the years prior to privatization, wages fall by 30 log points in the long run among privatized workers following privatization.[4] The large decline in earnings is due to two separate effects. First, workers displaced by privatization move to lower-paying establishments. Second, workers who remain employed at the privatized SOEs experience wage reductions relative to control workers. In order to decompose the effect on wages, I estimate the impact of privatization for workers who stay within the same establishment in the years following privatization. I find that wages for establishment stayers fall by about 17 log points in the

[3]Author's calculation. As will be discussed in Section 2.2, this paper restricts analysis to privatization events which occur between 1996-2000. A number of large SOEs were privatized in the early 1990s in Brazil. Therefore, the number cited understates the total number of workers directly impacted by privatization in Brazil in the 1990s.

[4]This analysis excludes observations with zero earnings as Brazil has a large informal sector (40 percent) and therefore years of zero earnings in the administrative data are not indicative of unemployment for many workers. However, I do find workers in SOEs are 10 percentage points less likely to be working in the formal sector following privatization, suggesting the results using only positive earnings is likely a lower bound.

long run, implying that roughly half of the wage decline for privatized workers is due to a displacement effect, while half is due to within-establishment changes in compensation policies.

In the second part of the paper, I consider the impact of privatization on establishment-level employment. While the worker-level analysis finds privatized workers are more likely to be displaced than control workers, privatized SOEs may increase hiring so that total employment is unchanged.[5] Understanding how privatization affects establishment-level employment is important for predicting how privatization will impact workers in private-sector firms. For example, if privatized SOEs replace entrenched workers with new hires from the private sector, then privatization may expand job opportunities to private-sector workers. Alternatively, privatization could result in permanent declines in employment which would increase the supply of workers to the private sector and lower equilibrium wages. To identify the impact on establishment-level employment, I use the fact that privatization occurred gradually over a number of years (1996-2000). Comparing privatized SOEs to SOEs that will be privatized at a later date, I find employment in privatized establishments falls by about 34 percent following privatization. Therefore, together with the worker-level analysis, this implies privatized SOEs shed incumbent workers and do not replace these workers through increased hiring.

In the last part of the paper, I explore whether these large negative impacts on privatized SOEs spillover to other firms in the same labor market. Theoretically, these spillovers may arise for three reasons. First, given the local nature of the industries impacted (e.g. banking, electricity, telecommunications), changes in product-market competition due to privatization may impact labor demand. Second, large declines in employment at SOEs will increase the supply of workers to private-sector firms, again reducing the equilibrium wage in the private sector. Third, in many models of wage determination (Abowd and Lemieux, 1993; Pissarides,

[5] Olsson and Tåg (2018) finds that privatized workers are more likely to become unemployed, however, employment does not fall at privatized firms, implying that privatized SOEs increase hiring to replace workers displaced during privatization in Sweden.

2000; Postel-Vinay and Robin, 2002; Hornstein et al., 2011; Card et al., 2018; Kline et al., 2019), the wage paid to a worker depends on the outside option of the worker. Decreases in employment and wages at privatized SOEs lowers the expected value of the outside option for private-sector workers, resulting in lower equilibrium wages.

To identify general equilibrium spillovers, I construct a measure of exposure to privatization at the labor market level. I follow Azar, Marinescu and Steinbaum (2017) and define a labor market as an occupation-by-region cell. While defined at the occupation-by-region level, the exposure measure depends on both the fraction of jobs privatized within the occupation-by-region cell, as well as the fraction of jobs privatized in closely related occupation-by-region cells, where two occupation-by-region cells are closely related if workers commonly transition between them. More formally, I define the distance between labor market i and labor market j as the probability a worker from labor market i transitions to labor market j conditional on a job transition. The exposure to privatization of labor market i is equal to the fraction of workers privatized in labor market i plus the fraction of workers privatized in all other labor markets, weighted by the distance between labor markets.[6]

I find wages fall in labor markets that are more exposed to privatization relative to labor markets that are less exposed to privatization. For example, moving from three percent of the labor market being privatized (the average level across all labor markets) to eight percent of the labor market being privatized (roughly one standard deviation) is associated with an average market wage decline of roughly 1.6 log points. This analysis excludes all observations from privatized SOEs and therefore does not incorporate any direct impacts of privatization. To quantify the total impact of privatization on the labor market, I perform a simple back-of-the-envelope calculation. Conceptually, the calculation computes counterfactual wages for all workers in 1995 (the year prior to the first privatization events studied in this paper) using

[6]To ensure the maximum exposure measure sums to 1, the fraction of workers privatized in labor market i is weighted by the fraction of workers in labor market i who transition jobs, but stay within labor market i. For example, if 100 percent of electrical technician jobs in a region are privatized, the exposure measure will not necessarily be equal to 1, because electrical technicians may transition into other types of jobs that were not directly impacted by privatization.

the event-study estimates, assuming the long-run impact of privatization occurs immediately in 1995. I find the direct impacts (i.e. the effect on workers in privatized SOEs) predicts an aggregate wage decline of about 1.1 log points. Incorporating spillovers magnifies the direct impact and results in an aggregate wage decline of about 4.5 log points. Therefore, in this case, incorporating spillovers is an essential component in estimating the total impact of privatization on workers. While the direct impacts are large, they impact relatively few workers. Spillovers, on the other hand, impact a much larger fraction of workers in the economy.

This paper contributes to a number of distinct literatures. First, it contributes to the long literature on the impacts of privatization on firm profits and efficiency (Boycko, Shleifer and Vishny, 1997; La Porta and Lopez-de Silanes, 1999; Megginson and Netter, 2001; Chong and Lopez de Silanes, 2005; Megginson, 2017). These studies generally find that many measures of profitability increase following privatization. In the particular case of Brazil, Anuatti-Neto, Barossi-Filho, De Carvalho and Macedo (2003) find that return on assets, return on equity, and sales all rise more in privatized firms relative to private-sector firms following privatization. While most papers studying privatization utilize firm-level financial data, two notable exceptions are Firpo and Gonzaga (2010) and Olsson and Tåg (2018), both of which use worker-level data and study the impacts of privatization on workers. To my knowledge, this is the first paper to consider not only the direct impact on incumbent workers, but also the broader labor market impacts of privatization.

The method used to identify spillovers in this paper is similar in spirit to prior work that empirically estimates spillovers in response to firm-level shocks. For example, Bloom, Schankerman and Van Reenen (2013) estimate R&D knowledge spillovers by creating measures of a firm's position in both product space and technology space. Lane (2017) studies the spillovers related to "big-push" industrial policies in South Korea by constructing measures of exposure to the policy using the sectoral input-output network. A number of studies estimate productivity spillovers due to agglomeration (Rosenthal and Strange, 2004; Moretti,

2011). Similarly, this paper uses a measure of distance in the labor market based on empirical worker flows in order to construct a measure of exposure to privatization.

Lastly, a recent literature documents significant wage dispersion across firms (Card et al., 2013; Barth et al., 2016; Song et al., 2018), even after controlling flexibly for worker quality. This significant dispersion is inconsistent with models of perfect competition and suggests labor market frictions may be important components in understanding the aggregate wage distribution. I find that SOEs pay significant wage premiums over private-sector firms. However, this wage premium declines significantly following privatization, suggesting that differential rent-sharing between SOEs and private-sector firms drives the dramatic decline in earnings in privatized SOEs following privatization.

The structure of the paper is as follows. Section 2.2 discusses the institutional details and data. Section 2.3 presents a model of privatization that illustrates the channels through which privatization impacts workers. Section 2.4 presents event studies that estimate the impact of privatization on incumbent workers in privatized SOEs. Section 2.5 presents event studies that estimate the impact of privatization on establishment-level wages and employment. Section 2.6 estimates general equilibrium spillovers associated with privatization and Section 2.7 concludes.

2.2 Institutions and Data

2.2.1 Privatization in Brazil

In Brazil, SOEs played a fundamental role in the government's economic development goals. The government of Brazil invested heavily in a number of strategic industries, including mining, steel, chemicals, banking, telecommunications, and electricity. However, in the late 1980s, large fiscal debts led Brazil to sell a number of SOEs as a means to raise revenue. The privatization program began in 1991 with the federal government selling large federal SOEs. Similar programs were enacted at the state-level beginning in 1996. Due to data limitations

which will be discussed further below, this paper restricts the analysis to privatization events occurring after 1995 and therefore will exclude many privatization events of the federal program. Given these data restrictions, the primary industries studied in this paper are telecommunications, electricity, and banking.[7]

All of the primary industries in the sample were only partially privatized. For example, while many banks were sold as part of the privatization process, the Bank of Brazil was considered an important Brazilian institution and deemed politically infeasible to privatize (Firpo and Gonzaga, 2010). In order to determine which firms would be sold, a privatization committee was formed. After choosing the list of proposed companies, the Brazilian Development Bank (BNDES) hired two private consulting companies to value the SOEs that would be sold and to determine an acceptable minimum price. A public auction would then be held, and the enterprise would be sold as long as the minimum acceptable price was reached. In the early 1990s, restrictions were placed on foreign investment, but these restrictions were later relaxed before the period of privatization (post 1994) studied in this paper.

The privatization program was part of broader market reforms. Perhaps the most important reform involved a large trade liberalization program that started in 1988 and ended in 1994. Many studies (Menezes-Filho and Muendler, 2011a; Kovak, 2013) have found that trade liberalization in Brazil had important impacts on workers' wages. However, all privatization events studied in this paper occur after the end of the trade liberalization. Additionally, to bias the results, trade liberalization would have to differentially impact privatized SOEs and private-sector firms within the same industry. It is unclear why firms in the same industry would be differentially impacted, but in any case, these differences would be evident in differential pre-trends between privatized SOEs and control establishments. As will

[7]The Brazilian government sold Vale mining in 1997 which is currently the largest producer of iron ore and nickel in the world. This privatization event is also included, though the number of workers impacted is less than three percent of the total sample.

be shown in Section 2.4, I find no differential pre-trends between privatized workers and a matched control group.

2.2.2 Data

The analysis utilizes administrative data from the *Relação Anual de Informações Sociais* (RAIS) for the years 1992 to 2010. The RAIS data contain linked employer-employee records from a mandatory survey administered by the Brazilian Ministry of Labor and Employment (MTE). Fines are levied on firms that provide inaccurate or incomplete information on the survey. Brazil, however, does have a large informal sector that accounts for about 40 percent of all employment. Therefore, periods of zero earnings do not necessarily indicate unemployment. In Section 2.4, I discuss how the informal sector impacts the interpretation of the results.

Each entry in the RAIS dataset is an employee-employer match. Each individual and establishment are assigned unique administrative identifiers that do not change over time. Demographic data includes worker-level variables such as education, gender, ethnicity, and age. The data also include information about the job, such as occupation, tenure, wage, hours, type of labor contract, whether the job has ended, and why the job has ended. Lastly, the data includes establishment-level information, including region and industry.

Most importantly for this study, the RAIS contains data on the legal nature of the establishment. SOEs and private-sector establishments are assigned different codes. When an establishment is privatized, the legal nature of the establishment changes from state-owned to private, but the tax identifier of the establishment does not change (Firpo and Gonzaga, 2010; Muendler, Rauch and Tocoian, 2012). Therefore, privatization events can be identified at the establishment level by tracking when legal codes change from year to

year.[8] However, the legal nature variable is only available from 1995 onwards. Therefore, only privatization events after 1995 can be identified in the RAIS data.

Occupational classifications in the RAIS follow the *CBO* (*Classificação Brasilieira de Ocupações*). The classification system contains 2,355 categories. As part of the identification strategy, I construct a control group by matching privatized SOE workers to workers in the same two-digit occupation, therefore reducing the dimensionality of the occupational classification system and allowing for more matches. Industries are reported under the *CNAE* four-digit classification (*Classificação Nacional de Atividade Econômica*) for 654 industries. Again, when matching, I match workers in privatized SOEs to workers in the same two-digit industry. However, as will be discussed further later, matching strategies using more narrow definitions of occupation and industries yield similar results.

2.2.3 Sample Selection

I restrict attention to workers age 18-65 working in full-time jobs (i.e. at least 35 hours per week). I exclude individuals with invalid identifiers (less than one percent of the data). In the RAIS, the wage is reported in multiples of the monthly minimum wage. The wage refers to total payments, including regular salary payments, holiday bonuses, performance-based and commission bonuses, tips, and profit sharing agreements, divided by total months worked during the year for that employer. To convert this number to real earnings, I use data on the real value of the minimum wage from Brazil's Institute of Applied Economic Research (IPEA).

The RAIS dataset includes a variable that indicates the type of employment contract the worker is hired under (e.g. temporary vs. permanent). The most common contract in the data is *Consolidação das Leis do Trabalho* (CLT) which is the standard full-time employment

[8]For a subset of large privatization events, the privatization event and timing is verified in the RAIS. For example, the privatization of the mining company, Vale, was a historic event and documented in the newspapers throughout 1997. I find that Vale's legal identifier changes the year of the privatization event in the RAIS data as well.

contract in Brazil. CLT contracts come with government mandated benefits such as severance pay if dismissed without cause. Importantly for this study, workers in SOEs and workers in the private sector are employed under the same legal contract. Therefore, changes in wages and employment due to privatization do not reflect changes in the contract held by workers. In contrast, public administration workers are employed under different contracts and entitled to further benefits, such as employment stabilization policies which limit the state's ability to fire civil servants (Firpo and Gonzaga, 2010). However, this project only compares workers in SOEs and workers in private-sector establishments and excludes public administration workers.

2.2.4 Identifying Privatization Events

As discussed previously, I follow Firpo and Gonzaga (2010) and identify privatization events by identifying when the legal nature of an establishment changes.[9] However, before 1995, legal nature is not included in the RAIS data, making it impossible to infer privatization events from changes in the legal nature variable. Therefore, this paper restricts analysis only to privatization events occurring from 1996-2000.[10] In Figure 2.1, I plot the number of workers currently employed in privatized establishments during each year of privatization in the sample. As can be seen in Figure 2.1, from 1996 to 2000, around 900,000 workers are employed in privatized establishments. Overall, this corresponds to about 3 percent of all formal sector employment.[11]

[9] In some cases, the legal nature of an establishment changes and then immediately reverts back in the following year. As in Firpo and Gonzaga (2010), I only include privatization events in which the legal nature switches from SOE to private-sector and the establishment remains in the private-sector in all following years of operation.

[10] While there are a few privatization events after 2000, Brazil's privatization stalled significantly around 2000.

[11] Note that this number understates the total number of workers impacted by privatization in Brazil because it does not include any of the privatization events which occurred prior to 1996.

2.2.5 Do SOEs Pay Higher Wages?

Before considering the effect of privatization on wages, I first establish that there is a significant wage premium associated with working for an SOE before privatization occurs. To do so, I estimate a two-way fixed effects model with worker and establishment fixed effects following Abowd, Kramarz and Margolis (1999) (AKM) and Card, Devicienti and Maida (2013) utilizing data from the years prior to the privatization (1992-1995).[12] Formally, I estimate the following regression model:

$$ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \gamma_t + x'_{it}\beta \tag{2.1}$$

where α_i is an individual fixed effect, ψ_j is an establishment fixed effect, γ_t is a year indicator, x'_{it} are time-varying covariates which include education and an age cubic interacted with year indicators, and $J(i,t)$ is a function which indicates the establishment individual i is employed at in time period t. Given the individual fixed effects, the establishment-specific wage premium ψ_j is interpreted as the premium paid by establishment j controlling for the quality of workers employed at establishment j. As discussed in Card et al. (2013) and Card et al. (2018), ψ_j is only identified relative to an omitted establishment. In practice, I omit the largest private-sector establishment when estimating the AKM model.

For the estimation to yield an unbiased estimate of ψ_j, the "exogenous mobility" assumption must be satisfied. This assumption allows high-wage workers to sort to high-wage establishments but does not allow workers to sort based on idiosyncratic match effects. This implies firms offer a proportional wage premium to all workers regardless of their skill level and job. While a restrictive assumption in theory, it appears to hold in many contexts (Card

[12] Instead of estimating an AKM model which relies on transitions between establishments, one could instead estimate a wage premium by regressing wages on a list of controls. Regressing wages on education, age, tenure, occupation fixed effects, industry fixed effects, and an indicator for working in an SOE yields a wage premium associated with working in an SOE of 22 log points.

et al., 2013; Song et al., 2018), and in particular, in Brazil (Alvarez, Benguria, Engbom and Moser, 2018).[13]

I estimate Equation (2.1) on the entire sample of establishments and collect the estimated $\hat{\psi}_j$ for all firms.[14] I then make two sample restrictions. First, I restrict the sample to the primary privatized industries which includes banking, telecommunications, and electricity. Second, to focus analysis on privatized SOEs, I exclude SOEs that are never privatized.[15] To estimate the wage premium associated with a privatized establishment prior to privatization, I estimate the following regression:

$$\hat{\psi}_j = \alpha + \omega Privatized_j + \xi_j \tag{2.2}$$

where ξ_j are industry fixed effects. Therefore, this regression compares the wage premium of SOEs that will become privatized at a later date to private-sector establishments that employ workers in the same industry. Following the AKM literature, the regression is performed at the worker level and therefore gives more weight to larger establishments. In this specification, $\hat{\omega}$ is equal to the wage premium associated with privatized establishments.

As can be seen in Table 1, on average, the establishment effect for SOEs is 23.1 log points higher in SOEs relative to private-sector establishments. There is a consistent wage premium across the main industries associated with privatization. The lowest wage premium (14.1 log points) is associated with banking while the largest wage premium (49.2 log points) is associated with telecommunications. One important caveat to this estimation procedure is that I identify establishments as private-sector based on their legal nature in 1995. Therefore, it is possible that I identify some privatized SOEs as private, given I can only identify

[13](Card et al., 2013) offers a number of tests aimed at validating the AKM model assumptions. (Alvarez et al., 2018) replicates these tests in Brazil.

[14]As discussed in length in Card et al. (2018), the AKM model is only identified on a set of establishments connected by labor mobility. In practice, I restrict the sample to the largest connected set.

[15]The wage premium associated with the never-privatized SOEs is similar to the wage premium associated with privatized SOEs.

privatization events after 1995. Therefore, in theory, ω may be underestimated given some establishments identified as private may not have been private over the entire estimation period.

This analysis provides the first evidence that even conditioning on worker quality, SOEs pay higher wages. If the large wage premium is due to differential rent-sharing between workers and firms at SOEs relative to private-sector firms, then this could explain why unions and workers fear privatization will lead to lower wages for workers.

2.3 Model of Privatization

2.3.1 Overview

Before proceeding to the empirical analysis, I present a simple model of state-owned enterprises to illustrate channels through which privatization may impact workers. This model builds from the union-wage determination models illustrated in Brown and Ashenfelter (1986) and Abowd and Lemieux (1993). In the model, increases in productivity due to privatization will increase wages, while decreases in bargaining power will reduce wages. The model also clarifies how privatization spills over to private-sector establishments.

2.3.2 Setup

To begin, I consider a group of $\bar{L}_j$ workers bargaining over both wages and employment level with a firm of type $j \in \{soe, p\}$, where *soe* denotes an SOE and p indicates a private-sector firm. The workers seek to maximize $L_j w_j + (\bar{L}_j - L_j)v$, where w_j is the bargained wage, L_j is the employment level, and v is the value of the outside option to the workers. In this case, I assume workers who do not obtain employment reenter the labor force and search for a new job. Therefore, the value of the outside option is equal to the expected wage of the new job minus any search costs c associated with finding a new job.

The workers bargain with a firm that has a concave production function $F(L_j)$, where the production function is constant across SOEs and private-sector establishments.[16] The profits of a firm are given by $\theta_j F(L_j) - w_j L_j$, where θ_j is a revenue shifter. For example, if the firm becomes more efficient, or the demand for the firm's good increases, then these changes will be captured by an increase in θ_j. I assume workers and firms Nash-bargain over wages, where the bargaining weight associated with labor is given by γ_j, which is allowed to differ between SOEs and private-sector firms. If $\gamma_{soe} > \gamma_p$ then SOEs allocate more rents to labor than private-sector firms.

The threat point for workers is the value of the workers' outside option, while the threat point for the firm is zero profits. SOEs, in addition to profits, may also receive additional revenue per worker, τ_j. For simplicity, I assume $\tau_{soe} > 0$ and $\tau_p = 0$, capturing the idea that politically connected firms may receive preferential treatment (Khwaja and Mian, 2005). Alternatively, one may conceptualize τ_{soe} as arising from political pressure and not directly from cash transfers. The parameter is included to capture any additional benefit of employment for an SOE above and beyond a worker's output. The bargaining solution chooses L_j and w_j to maximize:

$$\max_{L_j, w_j} [L_j w_j + (\bar{L} - L_j)v - \bar{L}v]^{\gamma_j} [\theta_j F(L_j) + \tau_j L_j - w_j L_j]^{1-\gamma_j} \tag{2.3}$$

Taking the first order conditions for the bargaining problem yields the following two optimality conditions:

$$w_j = \gamma_j \left(\frac{\theta_j F(L_j)}{L_j} + \tau_j - v \right) + v \tag{2.4}$$

$$\theta_j F'(L_j) = v - \tau_j \tag{2.5}$$

[16] While the homogeneity of the production function is not strictly necessary, it simplifies the illustration.

As can be seen in Equation (2.4), wages will be higher in firms with higher productivity θ_j, in firms with more incentives per worker from the state τ_j, and when the outside option v is higher. Equation (2.5) dictates the optimal size of the firm. If $F(L)$ is concave, then conditional on productivity and the outside option available to workers, SOEs will be larger as long as $\tau_{soe} > 0$. By allowing workers and firms to bargain over both wages and employment, the resulting bargaining outcome is strongly efficient (Farber, 1986). This implies that firm size does not depend on the negotiated wage. Restricting firms and workers to bargain over the wage only, however, yields similar predictions regarding the impact of privatization on workers. Therefore, I present only the strongly efficient bargaining solution as it simplifies the illustration.

2.3.3 Effect of Privatization on Workers in Privatized SOEs

In this formulation, there are three reasons why SOEs would offer higher wages than private-sector firms. First, they could be more productive due to differences in θ_j. This would cause SOEs to have both higher wages and higher employment levels, consistent with the empirical facts. Second, as informally argued by unions opposing privatization, the bargaining parameter, γ_j, may differ between SOEs and private-sector firms. Lastly, I allow SOEs to retrieve transfers τ_j, which increases the surplus available, some of which gets transferred to workers in the form of higher wages.

Many policy-makers claim that privatization increases efficiency, which can be captured in this model as an increase in θ_j. In this model, as in search (Burdett and Mortensen, 1998) and wage-posting models (Kline et al., 2019), firm size and wages are positively correlated with productivity. Therefore, increases in productivity generate increases in both firm size and wages. In contrast, falling incentives per worker, τ_j, generate declines in wages and employment, while falling worker bargaining power, γ_j, generates declines in wages.[17]

[17] If the strong efficiency assumption is violated, then falling worker bargaining power will generate declines in both wages and employment.

2.3.4 Spillovers on Private-Sector Firms Due to Privatization

In the model, there are three possible ways in which privatization will spill over to other firms. The first is through a product-market spillover. For example, if privatization changes competition in the product market, this could shift θ_j for private-sector firms, which would then impact wages. In Section 2.6, I discuss specifications which eliminate product-market spillovers as a channel which allows me to gauge their importance empirically.

A second spillover channel arises in the model due to changes in the outside option v, which is defined as the expected wage of a new job minus any search costs associated with finding the job:

$$v = \mathbb{E}[w] - c = \lambda_u b + \lambda_p \mathbb{E}[w|j = p] + \lambda_{soe}\mathbb{E}[w|j = soe] - c \tag{2.6}$$

where λ_u is the probability a worker becomes unemployed, b is the value of unemployment, λ_p is the probability a worker gets a job at a private-sector firm, $\mathbb{E}[w|j = p]$ is the expected wage at a private-sector firm, λ_{soe} is the probability a worker gets a job at an SOE and $\mathbb{E}[w|j = soe]$ is the expected wage at an SOE.[18]

To understand how changes in wages and employment at privatized SOEs spill over to other firms, imagine that privatization decreased only employment at privatized SOEs, but kept wages exactly the same. This would cause λ_{soe} to decrease (i.e. it is now more difficult to get a job at an SOE given employment has fallen at SOEs). This decreases the value of the outside option because it is now less likely a private-sector worker can become employed at a high-wage establishment. A second effect arises if privatized firms decrease wages, holding employment fixed. In this case, the quality of the outside options has decreased, again leading to a decrease in the negotiated wage.

[18] As discussed previously, many workers are employed in the informal sector. While the informal sector is not directly accounted for in the model, unemployment can be reinterpreted as a combination of unemployment and informal sector employment.

In the model, falling and employment and wages at SOEs impact private-sector workers through the outside option. However, declines in employment at SOEs may impact private-sector workers even in perfectly competitive markets. Workers displaced by SOEs will increase the supply of workers to the private sector, therefore decreasing the equilibrium wage through a labor-supply effect. In this case, as the wage falls, private-sector firms will increase employment to equate marginal revenue product and marginal cost (i.e. the wage). Through the lens of the model, privatization may directly impact $\mathbb{E}[w|j = p]$ if privatization increases the supply of workers to private-sector firms. While decomposing the impact of spillovers due to changing outside options and changing labor supply is beyond the scope of this paper, I do provide suggestive evidence that the outside option is at least important in empirically explaining spillovers.

2.4 Effect of Privatization on Incumbent Workers

2.4.1 Overview

The goal of this section is to estimate the effect of privatization on wages and employment for incumbent workers in privatized SOEs. A priori, it is not obvious how privatization will impact wages. As shown in Section 2.2, workers in SOEs earn higher wages prior to privatization, however, it is not clear whether this stems from differences in unobserved productivity or due to differences in rent-sharing between workers and establishments. To estimate the causal impact of privatization, I utilize a matching algorithm to find a control group for privatized workers and then estimate event studies around the time of privatization.

2.4.2 Finding a Control Group

Workers in SOEs are likely different than the average worker. For one, SOEs are generally larger and produce in industries deemed strategic by the government. Therefore, there might be important differences between workers in SOEs compared to the general population of

workers. In order to create a valid control group, I use a matching algorithm to find workers which are in the same occupation, industry, and of similar age. In the main specifications, I allow the control worker to be drawn from either never-privatized SOEs or private-sector establishments. A priori, it seems plausible that either group could be a potentially valid counterfactual and I therefore allow matches to be drawn from either group to increase the likelihood a match is found for a given treated worker. However, it is true that workers in never-privatized SOEs and private-sector establishments earn very different wages even conditional on occupation and sector. While the wage levels differ greatly between SOE workers and private-sector workers, the trends are similar in practice, and therefore the qualitative results hold regardless of the control group. Appendix 2.8 presents additional specifications which restrict the control group to either private-sector workers only or never-privatized SOE workers only.

Before finding the control group, I restrict the treated sample to individuals who are employed a privatized SOE in years $t-1$ and year $t-2$ prior to the privatization event in year t. This tenure restriction is chosen in order to limit the analysis to workers with attachment to the establishment. In order to find a control group for workers in a privatization event in year t, I first impose the same tenure restriction on private-sector workers and non-privatized SOE workers. Next, I implement a coarsened-exact-matching (CEM) procedure following Iacus et al. (2012). Specifically, the algorithm matches workers exactly on two-digit occupation codes, two-digit industry codes and five-year age bins. In additional results in Appendix 2.8, I add more variables to the matching algorithm, such as establishment size and microregion, as well as match on more specific occupation and industry codes. In all cases I find results to be similar across matching algorithms. The matching algorithm utilized is similar to a number of recent papers (Goldschmidt and Schmieder, 2017; Smith et al., 2019; Jaravel et al., 2018) which estimate event studies with a matched control group.

Table 2 displays the summary statistics for the privatized workers and the control group in the year prior to privatization. The privatized and control workers have similar educa-

tional attainment levels (48 percent of privatized workers are high-school graduates while 44 percent of control group workers are high-school graduates). The log monthly wage for privatized workers is slightly higher (8.64) than the control group (8.58). Privatized workers are employed in larger establishments than non-privatized workers (median establishment size in privatized establishments is 765 compared to 416 in the control group). Therefore, matching on occupation, industry, and age does not fully eliminate differences between privatized workers and non-privatized workers. However, the identification strategy relies on parallel trends between privatized workers and non-privatized workers, not complete balance on covariates.

One benefit of the matching approach is that comparing raw averages across groups is a meaningful comparison. Figure 2.2 plots the average monthly wage for the group of privatized workers and the matched control group. Given workers can transition into unemployment, the informal sector, or out of the labor force, Figure 2.2 is not restricted to a balanced panel. However, as can be seen in Figure 2.2, there is a stark divergence in wages following privatization, with the average log monthly wage in the privatized group falling roughly 30.0 log relative to the control group.[19]

2.4.3 Worker-Level Event Studies

To estimate the effect of privatization on workers' outcomes, I use an event-study framework by estimating regression models of the following form:

$$y_{it} = \sum_{k=-4}^{10} \delta_k (t = t^* + k) Privatized_i + \alpha_i + \gamma_t + x'_{it}\beta + \varepsilon_{it} \tag{2.7}$$

[19]In Appendix Figure 2.7 and Appendix Table 6, results for matching algorithms which restrict to only private workers or to only never-privatized SOE workers are displayed. For the private-sector control, wages in the control group are much lower than in the privatized group prior to privatization, consistent with the fact that SOEs pay higher wages even conditional on detailed characteristics. Results which add microregion and establishment size to the list of matching variables are also displayed in Appendix Table 6.

where y_{it} is an outcome variable, $Privatized_i = 1$ if individual i was employed in an establishment that is privatized, α_i are worker fixed effects, γ_t are time fixed effects, x_{it} are time-varying worker controls that include a cubic in age, and ε_{it} is an error term. Each δ_k measures the change in the outcome variable (y_{it}) for the privatized workers relative to the control group in year k relative to the privatization year t^*. Standard errors are two-way clustered at the individual and establishment level.

There are two key assumptions in estimating Equation (2.7). First, results in Table 2 show that the matched control group is similar but not identical to the privatized workers group. If workers in the privatized group have differential wage trends, then I will incorrectly attribute wage changes to privatization. These differential trends may arise for a variety of reasons. For example, it is likely more difficult to obtain a job in an SOE than in a private-sector establishment, and so when we compare a worker in an SOE to a worker in a private establishment, it may not be surprising to observe both different wage levels and different trends. Similarly, comparing a privatized worker to a worker in a never-privatized SOE may be confounded if there is selection into which establishments get privatized. For example, if the most productive SOEs also have the most productive workers, then privatization may be correlated with unobserved productivity which could cause privatized workers to be on a different wage trend than workers in never-privatized SOEs.[20]

The second assumption is that there are no shocks that coincide with privatization. One consequence of this assumption is that privatization has no treatment effect on non-privatized workers. However, as discussed in Section 2.3, the impacts of privatization could spillover to other establishments. This general equilibrium response will attenuate the direct impact of privatization toward zero and understate the impact of privatization on workers. In Section 2.6, I directly estimate these types of spillovers and find that general equilibrium effects are important to take into consideration. Therefore, the estimates in this section may be interpreted as lower bounds.

[20]Managers for public-sector establishments in Brazil must pass a difficult test in order to be hired.

Panel A of Figure 2.3 plots $\hat{\delta}_k$ from estimating Equation (2.7). As can be seen in the figure, wages between the privatized workers and the control group experience similar trends in the years before privatization, lending credibility to the parallel trends assumption. In the year after privatization there is a small but insignificant decline in wages. However, wages continue to fall in the following years, reaching a decline of roughly 30.0 log points that levels off and does not recover. The gradual decline in wages may be due to a combination of wage rigidity and declining union bargaining power. In particular, Guimarães (2007) documents anecdotal evidence that following privatization in Brazil, the telecommunications unions were unable to secure mandatory raises or raises pegged to inflation, which may explain the gradual decline in wages post-privatization.

Panel A of Table 3 summarizes the event-study results. Given the dynamic nature of the treatment effects, I aggregate the impacts into a short-run effect (average δ^k within first four years of privatization) and a long-run effect (average δ^k years five through ten following privatization).[21] Column 1 shows that in the short run wages fall by 5.8 log points in privatized establishments relative to the matched control group, while in the long run, this loss grows to 22.3 log points, with both effects being significant at the one percent level.

There are two factors contributing to the decline in wages post-privatization. First, individuals in privatized establishments may lose their jobs during privatization and find jobs at lower-paying establishments. Second, even conditional on staying in the establishment, wages may fall relative to the control workers if privatization causes a decline in wages within an establishment. To isolate the latter channel, I estimate the effect for establishment stayers. A worker is defined as a stayer in time period t if the individual is still employed at the same firm she was employed at in time period $t = t^* - 1$, where $t = t^*$ is the period the establishment is privatized.[22] For control workers, the year of privatization is defined as the

[21]Following the advice of Borusyak and Jaravel (2017), the short-run and long-run effects are estimated by first estimating Equation (2.7) allowing for flexible dynamics in treatment effects and then manually averaging the coefficients, weighting by the sample size at each point.

[22]In administrative datasets firm identifiers can change for multiple reasons. For example, tax identifiers may change after mergers and acquisitions. I recode these transitions as remaining within

year the corresponding treated worker's establishment is privatized. Panel B of Figure 2.3 plots δ^k from estimating Equation (2.7) restricting the estimation to individuals who remain employed at the establishment they were working for in the year prior to privatization.

As can be seen in Panel B of Figure 2.3, the impact of privatization conditional on remaining in the privatized establishment is equal to about -17.0 log points ten years after privatization, about half the size of the unconditional effect. Therefore, about half the decrease in wages in privatization is due to workers being displaced and finding jobs at lower-paying establishments, while half is due to within-establishment changes in wages. This suggests that the large wage premium associated with SOEs is not due to unobserved productivity, but is true conditional on worker productivity. Panel B of Table 3 presents wage regression results for establishment stayers. In the short run, there is an insignificant drop in wages of 1.0 log points, while in the long run there is a significant drop of 11.0 log points.

One important caveat to this analysis is that it is conditional on positive earnings. In Brazil, nearly 40 percent of the workforce is employed in the informal sector, therefore it would be incorrect to interpret an individual dropping out of the sample as unemployment or out of the labor force. However, if privatization increases the probability a worker is displaced, then the estimates so far would understate the total negative impact of privatization on worker welfare, given that displacement is associated with large persistent earnings losses (Jacobson, LaLonde and Sullivan, 1993).

To explore this possibility, Panel A of Figure 2.4 plots the probability a worker is employed in the formal sector relative to the year of privatization. The probability of remaining in the sample decreases over time for both the privatized sample and the matched control group due to workers switching to informal jobs, unemployment, and retirement. In ten years, approximately half of all workers are no longer employed in the formal sector. However, the

the firm by identifying such administrative changes using worker flows following Benedetto et al. (2007). If at least 70 percent of workers in firm A move to firm B in period t, then all workers in firm A that move to firm B are classified as stayers.

probability of remaining employed in the formal sector is higher for the control group. Panel B of Figure 2.4 plots δ^k from estimating Equation (2.7) with formal sector employment as the outcome. As can be seen in the figure, the probability of formal sector employment drops by 10 percentage points in the privatized SOEs relative to the control group two years after privatization. This effect remains relatively constant throughout the next ten years.

This gap has important consequences for the interpretation of the wage effects. If informal sector jobs pay lower on average, then the long-run wage effect of -30.0 log points in Figure 2.3 represents a lower bound, as it does not capture the fact that privatized workers are more likely to transition into the informal sector where they earn lower wages on average. While it is not possible to observe informal workers in the RAIS, the Brazilian National Household Survey, Pesquisa Nacional por Amostra de Domicìlios (PNAD) contains information on informal work as well as information on wages and sector. Therefore, it allows one to estimate formal-informal wage gaps while controlling for a variety of observables.[23]

To estimate the informal-formal wage gap, I estimate a regression of the following form:

$$ln(w_{it}) = \beta Informal_{it} + \gamma_t + \xi_s + \chi_o + \zeta_r + \pi X_{it} + \varepsilon_{it} \tag{2.8}$$

Where $Informal_{it}$ indicates that worker i is employed in the informal sector at time t, γ_t are year indicators, ξ_S are industry fixed effects, χ_o are occupation fixed effects, ζ_r are state fixed effects, and X_{it} is a vector of covariates which includes a cubic in age, tenure within the firm in months, union status, education in years, and number of jobs held. As can be seen in Column 1 of Appendix Table 7, when pooling all industries, I find an informal-sector wage penalty equal to -0.324.[24] Restricting to just the privatized industries dramatically reduces the sample, but I still find a statistically significant wage penalty equal to -0.264. Therefore,

[23]The PNAD microdata and documentation is available at the Brazilian Statistical Agencies website:https://ww2.ibge.gov.br. I am thankful to Data Zoom, developed by the Department of Economics at PUC-Rio, for providing the codes for accessing IBGE microdata.

[24]Estimating an earnings function without state, occupation and industry fixed effects yields an informal sector wage penalty equal to -0.422.

if privatization increases the probability a worker transitions to the informal sector, as Figure 2.4 finds, then this transition is associated with a large wage penalty which is not captured in the previous event-study specifications, given the estimates are conditional on positive earnings in the formal sector. Therefore, the total impact of privatization on earnings for incumbents workers is likely larger than previously estimated. However, one caveat to this analysis is that I do not observe severance payments in the data, and therefore, the impact of job displacement may be overstated. Severance packages can be quite large in Brazil (Gonzaga et al., 2003), and therefore likely reduce the impact of job displacement on workers.

2.4.4 Heterogeneity in Effects

Guimarães (2007) documents anecdotal evidence that following privatization in Brazil, the telecommunications unions were unable to secure mandatory raises or raises pegged to inflation, which may explain the gradual decline in wages post-privatization. Therefore, declines in union bargaining power may play an important role in explaining the decline in wages in privatized SOEs. A number of studies find that unions compress wages within firms (Card, 1992; Farber et al., 2018), and therefore, we might expect low-skill workers to suffer the greatest losses from privatization. In this section, I split the subsample by education. Education is divided into three groups: no high-school degree, high-school degree, and college degree.[25] For each education level, Equation (2.7) is estimated. Panel A of Appendix Figure 2.8 displays estimates of δ^k for the entire sample, while Panel B of Appendix Figure 2.8 restricts to establishment stayers.

As can be seen in Panel A of Appendix Figure 2.8, wage losses are largest for individuals without a high-school degree, reaching -50.1 log points ten years after privatization. For comparison, wages for individuals with a high-school degree fall -22.1 log points relative to

[25]In the RAIS data, education is coded by the employers and is prone to miscoding. Education changes year to year and across employers for the same worker. To obtain a consistent education variable, I take the modal level of education for a worker across all years in the data as a worker's education level. A specification which splits by the skill-level of the occupation instead of the education-level yields similar results.

the matched control group while workers with a college degree fall -17.0 log points relative to the control group. Columns 2-4 of Table 3 presents these results, aggregating to a short-run effect (average effect for first four years after privatization) and a long-run effect (average δ^k years five through ten following privatization). The short-term effect is negative in all cases, but substantially smaller than the long-run effect, consistent with decreases in bargaining power manifesting as an inability to secure wage increases, rather than immediate cuts in nominal wages.

Turning to Panel B of Appendix Figure 2.8, while the wage losses for stayers are still largest for workers without a high-school degree, the differences are much smaller in magnitude. Panel B, columns 2-4 of Table 3 present the results for stayers. In the short run, there is a significant wage loss of 4.0 log points only for workers without a high school degree. In the long run, wages of workers without a high-school degree in privatized establishments fall by 15.6 log points relative to the matched control group. For comparison, wages for workers with a high-school degree (Column 3) and with a college degree (Column 4) fall by 10.5 log points and 10.6 log points, respectively. Therefore, while the effect of privatization on wages still depends on education, the impact is ameliorated when estimating effects only for establishment stayers. Therefore, the large differences in Panel A of Figure 2.8 are driven by low-educated workers being displaced and finding lower-paying jobs at other establishments. This suggests that low-educated workers especially benefit from being employed in an SOE.[26] Interestingly, post-privatization, wages fall by similar amounts for all workers conditional on staying within the establishment, suggesting the privatized establishments are unable to reverse the wage compression, possibly due to nominal wage rigidities.

[26]In unreported results, I estimate AKM models separately for each education group and find the wage-premium associated with SOEs is larger for lower-educated workers, consistent with the low-educated workers gaining more from working at an SOE. Note that this violates the exogenous mobility assumption of the AKM framework, which assumes all workers earn the same premium at a given establishment. Therefore, when turning to establishment-level estimates of changes in AKM premiums, I allow for different premiums by education group.

Appendix Figure 2.9 presents results for the impact of privatization on the probability a worker is employed in the formal section by education. As can be seen in the figure, there is a 10 percentage point decrease in the probability of formal sector employment for privatized workers, and this effect is consistent across the education subsamples. This implies that while low-educated and high-educated workers are laid off at similar rates following privatization, the low-educated workers suffer particularly large losses from displacement.

2.4.5 Robustness

Appendix Table 6 presents a number of robustness checks. Column 1 matches workers in privatized establishments to workers in the private sector only. Column 2 matches workers in privatized establishments to workers in never-privatized SOEs only. Column 3 presents results which winsorizes the bottom five percentiles of wages and the top five percentiles of wages. Column 4 matches workers two years prior to privatization rather than one year prior to privatization. Column 5 adds microregion and bins for establishment size to the list of variables which workers are matched on.[27] Column 6 matches on more narrow definitions of industry and occupation.

Results are consistent across specifications. The most noticeable difference is that the decrease in wages due to privatization is larger when comparing to SOE workers only. This can be rationalized by the theoretical model if worker bargaining power is larger at SOEs. To see how, consider an extreme case in which workers extract all the surplus from the employment relationship in SOEs (i.e. $\gamma_{soe} = 1$), but none of the surplus at private-sector establishments (i.e. $\gamma_p = 0$). In this case, wages at SOEs are set so that profits are zero. Therefore, wages do not depend on the outside option available to SOE workers. If privatized establishments lower wages by 10 percent, non-privatized SOEs will not respond, therefore

[27] A microregion is an administrative boundary in Brazil similar to a commuting zone in the United States.

an event study will estimate that privatization lowers wages by 10 percent for privatized workers.

Now consider how private establishments respond to changes in wages at privatized SOEs. If $\gamma_p = 0$, then the wage is set to the outside option for private-sector workers. Imagine a case in which privatized SOEs lower wages by 10 percent. If workers are equally likely to be hired at SOEs and private-sector establishments, this will lower the value of the outside option (in expectation) by 5 percent. Therefore, privatization impacts both private-sector workers and privatized workers in this case. An event study comparing privatized workers to private-sector workers will estimate that privatization lowers wages by 5 percent. In this way, equilibrium responses by private-sector establishments are attenuating the direct impact of privatization on wages towards zero.

2.4.6 External Validity

An important caveat to the results presented so far is that they may not necessarily extrapolate to other countries undergoing privatization, and may be particular to the Brazilian setting. For example, in Section 2.2 I found a substantial wage premium associated with working for an SOE in the years prior to privatization. Therefore, when establishments were privatized, there may have been substantial scope for reducing wages given wages were above the market level. If SOEs in other countries do not pay such substantial wage premiums, then it may be unlikely to observe such large declines in earnings following privatization. Additionally, in other countries, even if workers in SOEs are paid higher wages, privatization may not result in significant declines in wages and employment in the presence of high worker bargaining power. While Brazil does have high unionization rates, anecdotal evidence in Guimarães (2007) suggests union bargaining power fell after privatization in the

telecommunications industry. Both of these explanations could explain why Olsson and Tåg (2018) find that privatization does not significantly impact earnings in Sweden.[28]

2.5 The Effect of Privatization on Privatized Establishment Wages and Employment

2.5.1 Setup

While the worker-level analysis is informative about incumbents, it is important to understand how privatization impacts establishment-level outcomes. The first part of this section considers the impact of privatization on establishment-level wages. Unlike the worker-level analysis, impacts on establishment wages will incorporate impacts on both incumbents and new entrants. The second part of this section considers the impact of privatization on establishment-level employment. Understanding how establishment-level employment responds to privatization is crucial for predicting the impact of privatization on private-sector workers. For example, if privatized SOEs replace entrenched workers with new hires from the private-sector, then privatization may expand job opportunities to private-sector workers and improve outcomes. If privatization results in permanent drops in employment at privatized SOEs, then increased labor supply to the private-sector may drive down wages for all workers.

To identify the impact on establishment-level outcomes, I use the fact that privatization occurred gradually over a number of years (1996-2000). Due to the gradual rollout, the impact of privatization can be identified by comparing privatized establishments to SOEs that will be privatized at a later date. To estimate the effect of privatization on establishment outcomes, I use an event-study framework by estimating regression models of the following

[28]Theoretically, SOEs could pay even less than private sector establishments. For example, in Malaysia, state workers have fewer collective bargaining rights than private-sector workers (Waghorne, 1999). In these cases, we might expect privatization to increase wages.

form:

$$y_{jt} = \sum_{k=-4}^{4} \delta_k (t = t^* + k) Privatized_{jt} + \psi_j + \gamma_t + \varepsilon_{jt} \tag{2.9}$$

where y_{jt} is an outcome variable, $Privatized_{jt} = 1$ if establishment j has been privatized before time t, ψ_j are establishment fixed effects, γ_t are time fixed effects and ε_{jt} is an error term. All standard errors are clustered at the establishment level. Each δ_k measures the change in the outcome variable y_{jt} for the privatized establishment relative to establishments that will be privatized at a later (or earlier) date. In the worker-level regressions, the key source of identifying variation is the difference in outcomes between workers in SOEs that are privatized and workers in SOEs that are never privatized or private-sector establishments. In Equation (2.9), the identifying variation comes from the timing of privatization, given that all establishments in this sample are privatized at some point.

2.5.2 Impact of Privatization on Establishment Wages

Panel A of Figure 2.5 plots δ_k from Equation (2.9) with average log wage as the outcome. As can be seen in Panel A of Figure 2.5, wages trend similarly in the years prior to privatization and then fall by roughly 14.0 log points in the years following privatization, consistent with the worker-level results which restrict to establishment stayers. Again there is a lag between privatization and decreases in wages. As discussed in Section 2.4, one potential explanation is that downward wage rigidities prevent establishments from adjusting wages immediately. To explore this possibility, Panel B of Figure 2.5 restricts the sample to entrants. Unlike incumbent workers, there is no clear reason why entrants' wages could not adjust immediately. As can be seen in Panel B of Figure 2.5, wages of new hires fall dramatically post-privatization, but begin to fall the year *prior* to privatization. Taken together, these results are consistent with unions losing bargaining power post-privatization. For incumbent workers, this implies they do not receive wage raises comparable to workers in private or never-privatized employers. For entrants, this implies wages fall relative to wages for entrants immediately.

One weakness of the establishment-level event study is that it may not adequately control for changes in the composition of the workforce around privatization. This may be of particular concern in this context, given that workers in privatized SOEs are more likely to be displaced than control workers. To control flexibly for worker quality, I estimate the following regression model at the worker level:

$$ln(w_{it}) = \alpha_i + \psi_{J(i,t)} + \delta^{sr} Privatized_{J(i,t)} \cdot SR_t + \delta^{lr} Privatized_{J(i,t)} \cdot LR_t + x'_{it}\beta + \varepsilon_{it} \quad (2.10)$$

where α_i are worker fixed effects, $\psi_{J(i,t)}$ are establishment fixed effects, $Privatized_{J(i,t)}$ is an indicator which is equal to one if establishment $J(i,t)$ has been privatized at some point before year t, SR_t is an indicator that is equal to one in the four years after privatization and zero otherwise, LR_t is an indicator equal to one if it has been more than four years since privatization and zero otherwise, x_{it} includes an age cubic interacted with year effects, and ε_{it} is an idiosyncratic error term.

The coefficients of interest are δ^{sr} and δ^{lr}, which captures on average how wages of workers within privatized establishments change in response to privatization, controlling for the worker and the establishment. I include both a short-run indicator of privatization as well as a long-run indicator to capture the empirical fact that wages respond slowly in response to privatization. Unlike the establishment-level event studies, this specification controls flexibly for worker quality. For example, if a privatized establishment upgrades the skill-level of the workforce, but does not alter compensation policies, then both δs will be equal to zero, indicating that conditional on worker quality, privatization does not impact wages paid by the establishment. If workers' wages fall within privatized establishments, then we would expect both δs to be negative. Given the evidence from the worker-level results, we expect both δs to be negative, and that $\delta^{lr} < \delta^{sr}$, capturing the fact the effects gradually grow in magnitude.

To implement the estimation, I pool all years from 1993 to 2004 to estimate Equation (2.10). I also estimate the equation separately by education group, which allows establishment-specific wage premiums to vary by education group. Therefore, this allows high-educated workers to gain more from working at high-wage establishments and therefore relaxes the exogenous mobility assumption in the AKM literature along one dimension. Splitting by education also alleviates the computational burden of estimating the model. As can be seen in Appendix Table 9, privatization decreases wages in the short run by 4.5 log points for the no-high-school degree sample, by 6.5 log points for the high-school sample, and 7.6 log points for the college degree sample. Moving to the long-run effects, the effect of privatization on wage premiums is consistently larger in magnitude than the short-run effects, which is consistent with the worker-level event studies. In the long run, wages fall by 12.1 log points in the no high-school sample, 10.1 log points in the high-school sample, and 9.8 log points in the college sample. All effects are significant at the 1 percent level. Therefore, even conditional on worker quality, wages fall substantially in privatized establishments.

2.5.3 Impact of Privatization on Establishment Employment

Next, I estimate the impact of privatization on log establishment employment. Panel C of Figure 2.5 plots δ_k from Equation (2.9) with log employment as the outcome, where employment is measured as the number of workers employed in the establishment that work at least 35 hours a week. As can be seen in Panel C of Figure 2.5, log employment gradually falls in privatized establishments, dropping roughly 60 log points in the four years after privatization. Converting this into a proportional effects yields a 45 percent decline in employment following privatization. However, as can be seen in the figure, while there is a stark trend break after privatization, log employment is falling in the year prior to privatization. Therefore, the 45 percent decline may overstate the direct impact of privatization. To correct for this, I linearly extrapolate the pre-trend using the estimates in the years prior to privatization (i.e. years -4 through -1 relative to privatization). If log employment had remained

on this pre-trend, it would have fallen by 17 log points. Therefore, my preferred estimate of the impact of privatization on log employment is equal to $-60 - (-17) = -42$. Converting this to a proportional effect implies that privatization decreased employment by 34 percent. Together with the worker-level analysis, this shows that privatized establishments shed incumbent workers and do not replace these workers through increased hiring.

This result has important implications for other workers employed in the same labor market as workers in privatized SOEs. Through the lens of the bargaining model illustrated in Section 2.3, this fall in employment has two implications. First, this will cause an influx of workers into the private-sector labor market. This labor-supply effect will decrease the equilibrium wage in the private sector, as there are now more workers competing over private-sector jobs. Second, falling employment at privatized SOEs may decrease the probability a private-sector worker can transition into employment at the privatized SOE. This reduces the value (in expectation) of the private-sector worker's outside option and therefore further reduces the equilibrium wage.

2.6 Estimating Spillovers Due to Privatization

2.6.1 Overview

The results in Section 2.4 and Section 2.5 establish that privatization decreases both wages and employment in privatized SOEs. In this section, I estimate labor market spillovers due to privatization by exploiting variation across labor markets in the level of exposure to privatization. To proceed, I first provide a definition of a labor market and then define the exposure measure which will be used to identify the spillovers associated with privatization. Intuitively, the exposure measure will depend on two factors: (1) the fraction of jobs privatized within a labor market and (2) the fraction of jobs privatized in closely related labor markets, where labor markets are closely related if workers commonly transition between them.

2.6.2 Labor Market Definition

I follow Azar et al. (2017), and define the labor market by a microregion-by-occupation cell. A microregion in Brazil is a concept similar to commuting zone in the United States and has been used in Kovak (2013) and Menezes-Filho and Muendler (2011a) to define labor markets in Brazil. In the RAIS data, approximately 75 percent of job transitions are within microregions, indicating that most transitions do occur within microregions rather than across microregions.

Occupational classifications in the RAIS follow the *CBO* (*Classificação Brasilieira de Ocupaçõ̃es*), a five-digit classification system with a total of 2,355 individual occupations. This system is more narrowly defined than the Standard Occupational Classification (SOC) system commonly used in the United States, and in particular, in Azar et al. (2017), which contains 840 different occupations. Using the narrowest definition of the labor market may overstate the level of exposure if workers easily transition into closely related occupations. I deal with this problem in two ways. First, as will be discussed below, the exposure measure will directly take into account the possibility that workers can transition across labor markets. Second, while the main specification defines occupations at the five-digit level, I present a series of robustness checks which vary the definition of an occupation (from one-digit to four-digit).

2.6.3 Exposure Measure

To begin, let P_{it} be the proportion of jobs in labor market l_i in 1995 that are privatized at time t. One way to define the exposure measure would be to use P_{it} directly as the measure. However, this would ignore the fact that many transitions in the data are between labor markets, rather than within, and could understate (or overstate) a given labor market's exposure to privatization. For example, suppose all electrical technician jobs in a region are privatized, but electrical technicians can easily transition into technician jobs available in

other engineering fields, such as civil or computer engineering. Then using $P_{it} = 100$ for electrical technicians is likely to overstate the exposure to privatization.[29]

Therefore, the exposure measure for a worker in labor market l_i is based on the level of privatization (P_{it}) in labor market l_i, as well as the level of privatization (P_{mt}) in other labor markets l_m, weighted by the distance between two labor markets. The distance metric $d_{i \to m}$, is defined by the fraction of individuals in labor market l_i who transition to a job in labor market l_m in the years prior to privatization. Formally, for labor market l_i, the exposure to privatization at time t is given by:

$$Exposure_{it} = \sum_{t' \leq t} \sum_{m=1}^{L} d_{i \to m} P_{mt'} \tag{2.11}$$

where $P_{mt'}$ is the proportion of jobs in labor market m that are privatized in time period t'. Note that if all the jobs a worker may transition to are privatized, the exposure measure for this worker's labor market will be equal to 100. If none of the jobs a worker may transition to are privatized, the exposure measure will be equal to zero. Also, note that the exposure measure is cumulative. Since I restrict analysis to events which occur between 1996-2000, the exposure measure for labor market l_i reaches its maximum value in 2000, at which point it stays constant across the sample period.

At the five-digit occupation level, there are 94,777 unique labor markets. Overall, about 3 percent of the workforce in 1995 is employed in an establishment that is eventually privatized.[30] The exposure level, however, is highly skewed. About 14 percent of labor markets have an exposure level equal to zero, whereas the maximum exposure level is equal to 100. Overall, the mean exposure level across labor markets is equal to 3.0 with a standard devia-

[29]For example, 43 percent of electrical technicians who switch jobs transition into a different occupation. Of those who transition into other occupations, the most common transition is to mechanical engineering technician, which occurs in 10 percent of cases.

[30]This statistic is restricted to privatization events occurring between 1996-2000 and therefore understates the total number of workers impacted by privatization in Brazil. As discussed in Section 2.2, a number of large federal companies were privatized before 1995, but due to data limitations, these privatization events cannot be identified in the RAIS data.

tion equal to 5.4. Appendix Figure 2.10 presents the distribution of $Exposure_{it}$ across labor markets in Panel A, as well as the distribution of $ln(Exposure_{it}+1)$ in Panel B. In the next section, $ln(Exposure_{it}+1)$ will be used to identify the impact of exposure on market wages.

2.6.4 The Impact of Exposure to Privatization on Market Wages

To estimate the general equilibrium spillovers, I estimate the following regression specification:

$$ln(w_{it}) = \delta ln(Exposure_{it}+1) + \alpha_i + \gamma_t + \epsilon_{it} \tag{2.12}$$

Where $ln(w_{it})$ is the average log wage in labor market i at time t, which is computed excluding all observations from privatized SOEs, $Exposure_{it}$ is the exposure measure for labor market i in year t, α_i are labor market fixed effects, and γ_t are year fixed effects. The estimation includes years prior to privatization (1992-1995), and therefore in these years $Exposure_{it}$ is mechanically zero for all labor markets. In order to include labor markets with zero exposure into the estimation, I add one to $Exposure_{it}$ before taking the logarithm so that the minimum of $ln(Exposure_{it}+1)$ is equal to zero. An alternative way to incorporate zeros into the analysis is to take hyperbolic sine (asinh) of the exposure measure. The asinh function closely parallels the natural logarithm function, but is well defined at zero. To show robustness to the exact transformation of the exposure measure, I include results using both $ln(Exposure_{it}+1)$ and $asinh(Exposure_{it})$.[31] All standard errors are clustered at the labor market level.

Table 5 presents the results from estimating Equation (2.12). A one standard deviation in log exposure (0.53) decreases wages by 1.6 log points (0.031×0.53), indicating exposure is associated with a significant decline in the market wage. However, there are a number of concerns when estimating Equation (2.12). First, while the exposure measure does allow

[31] As can be seen in Column 3 of 5, this yields similar results. For an application of the asinh function in economics see Card and DellaVigna (Forthcoming). Additionally, eliminating all labor markets with zero exposure from the analysis again yields similar results.

for links across labor markets, a priori, it is unclear what the "correct" definition of a labor market should be in practice. This issue can be interpreted as inducing measurement error into the exposure measure. If the measurement error is classical, then this will attenuate the impact of exposure to privatization toward zero. However, given the large mass of labor markets with zero exposure level, the measurement error is likely non-classical, making it impossible to determine the sign of the bias. Therefore, Appendix Table 10 estimates the impact of exposure to privatization using a number of alternative market definitions. In all specifications, I continue to find a negative impact of exposure on average market wages.[32]

A natural concern with estimating Equation (2.12) is that the level of exposure is assumed to be random after year and labor market fixed effects are included in the specification. Suppose, however, that high-skill occupations, such as electrical technicians, are more likely to be exposed to privatization, as well as more exposed to technological change which makes them more productive. In this case, the coefficient on $ln(Exposure_{it}+1)$ would be biased upward, given that exposure to privatization is correlated with exposure to technological change which enhances worker productivity.

To test whether exposure is correlated with trends in wages, I estimate the following event-study specification:

$$ln(w_{it}) = \sum_{t=1992}^{2008} \delta_t \cdot (ln(Exposure_{i,2000}+1) \cdot \gamma_t) + \alpha_i + \gamma_t + \pi X_{it} + \varepsilon_{it} \tag{2.13}$$

where $ln(Exposure_{i,2000}+1)$ is the total cumulative exposure of a labor market given the few privatization events after 2000 are excluded from the analysis. The coefficients $\hat{\delta}_t$ capture the effect of exposure to privatization in year t. If exposure to privatization is random conditional on control variables, then δ_t should be zero in the years prior to privatization. If $ln(Exposure_{i,2000}+1)$ is still correlated with other factors that impact wages even condi-

[32]While the qualitative results hold across alternative labor market definitions, the quantitative implications differ by labor-market definition. For example, when defining the labor market at the one-digit occupation level, the marginal impact of exposure on log monthly wages is equal to -0.070 compared to -0.031 at the five-digit level.

tioning on labor market fixed effects and year effects, then δ_t will not be equal to zero in the years prior to privatization. For example, if electrical technicians benefited from skill-biased technological change in the years before privatization, then δ_t would be trending upward in the years prior to privatization.

As can be seen in Figure 2.6, there is no clear pre-trend in the years prior to privatization. In 1996, privatization begins, and wages gradually fall in the more exposed labor markets relative to less exposed labor markets. This supports the assumption that conditional on labor market and year fixed effects, exposure to privatization is not correlated with pre-existing trends in wages.

Appendix Table 10 presents a few additional robustness checks. First, outcomes within a microregion or within an occupation may be correlated. In Column 6 I two-way cluster at the microregion and occupation level and continue to find a statistically significant impact of exposure to privatization on wages. Similarly, one may be concerned that certain microregions are on different trends and that the exposure measure is correlated with these trends. While the event-study suggests this is not the case, another way to show robustness is to include microregion-by-year fixed effects into the estimation. In this case, the effect of exposure will be identified by differences in exposure levels between different occupations in the same microregion. Column 7 includes microregion-by-year fixed effects and continues to find a negative correlation between exposure to privatization and market wages. Column 8 excludes all workers that ever work for privatized SOEs when computing the average market wage. Privatized workers who transition into the private-sector may need to "start at the bottom". In this case, declines in wages in adjacent markets may not be due to spillovers, but direct effects on the careers of privatized workers. However, excluding workers that ever work for privatized SOEs yields nearly identical results.

2.6.5 The Aggregate Impact of Privatization on the Average Wage

The goal of this section is to describe a simple exercise to quantify the total impact of privatization on the aggregate wage. The exercise also allows one to decompose this impact into a component due to direct impacts on privatized workers, and a component due to indirect spillovers. Conceptually, the exercise takes all workers in 1995 (the year prior to the privatization period discussed in this paper) and uses the event-study estimates to create the counterfactual wages which would occur if the impacts of privatization occurred immediately in 1995. I first compute the impact of privatization using only the direct impacts on privatized workers. I then compare to an exercise which incorporates the general equilibrium spillovers. First, to construct the counterfactual wage with no spillovers, w_i^{ns}, for worker i, I form:

$$w_i^{ns} = w_i + \hat{\beta} Privatized_i \tag{2.14}$$

where $\hat{\beta}$ is the direct impact on privatized workers and w_i is the actual wage. Note that for any worker not in a privatized establishment, the counterfactual wage will be equal to the actual wage. In Figure 2.3, I found the long-run impact of privatization on wages is equal to about -0.30. However, this estimate is downward biased if control workers are exposed to privatization, and therefore also experience wage declines following privatization. Therefore, I form $\hat{\beta}$ by taking into account the exposure of the control group to privatization:

$$\hat{\beta} = -0.30 + \hat{\delta} \cdot \mathbb{E}[Exposure|Control] \tag{2.15}$$

where $\hat{\delta}$ is the estimated impact of exposure to privatization on wages. I find $\hat{\delta} = -.031$ and $\mathbb{E}[Exposure|Control] = 1.34$, implying $\hat{\beta} = -0.34$. Therefore, the aggregate impact of privatization using only direct impacts is equal to:

$$\Delta w^{direct} = \mathbb{E}[w_i^{ns}] - \mathbb{E}[w_i] = -0.011 \tag{2.16}$$

Next, I form the counterfactual wages which incorporate spillovers::

$$w_i^s = w_i + \hat{\beta} Privatized_i + (1 - Privatized_i) \cdot \hat{\delta} Exposure_i \tag{2.17}$$

The aggregate impact of privatization on wages with both direct and spillovers is then formed by:

$$\Delta w^{total} = \mathbb{E}[w_i^s] - \mathbb{E}[w_i] = -0.045 \tag{2.18}$$

Therefore, the aggregate impact incorporating spillovers is about four times as large as the direct impact alone.[33] In this setting, incorporating spillovers is crucial for estimating the total impact of privatization on the labor market.[34]

2.6.6 Distinguishing Between Alternative Spillover Channels

In terms of the model presented in Section 2.3, these negative spillovers may arise through three different channels. First, changes in product-market competition due to privatization may impact labor demand. Second, large declines in employment at SOEs will increase the supply of workers to private-sector firms, reducing the equilibrium wage. Third, the outside option for workers may decline in response to employment and wage cuts at privatized SOEs. Unfortunately, without data on product market prices, it is difficult to predict how product market spillovers contribute to the negative spillovers. However, to understand whether

[33] This number changes depending on the exact specification used for estimating spillovers. For example, the specification which restricts to manufacturing establishments finds $\hat{\delta} = -0.02$. In this case, the direct impact of privatization is still equal to -0.011, but the aggregate impact is equal to -0.032. Therefore, while the magnitudes change slightly, the qualitative conclusions remain the same. Additionally, these decompositions may have different interpretations. The decomposition in the main text may incorporate impacts of product market competition which spillover to workers, while the decomposition using the manufacturing estimates abstracts from product market effects by focusing on establishments in which prices are determined on a global market.

[34] The magnitude of spillovers is similar to Beaudry et al. (2012) who study a similar setting in which declines in employment in a high-wage industry (e.g. manufacturing) spill over to other industries through a reduction in the outside option available to workers in other industries. They find that spillovers through reductions in the outside option magnify the direct impact by about 3-4 times.

product-market effects drive the negative correlation, I estimate Equation (2.12) restricting to the manufacturing sector in Columns 3-6 of Table 5. In manufacturing industries, the price of the output is substantially determined on a global market, and therefore, may be less prone to changes driven by privatization. In manufacturing industries, I find a one standard deviation in log exposure (0.53) decreases wages by 1.2 log points (0.022×0.53). In comparison, a specification which includes all industries finds that a one standard deviation decreases wages by 1.6 log points. Therefore, product-market competition may potentially explain about one-third of the total effect. The remaining portion can be explained by either (1) deteriorating outside options (2) increases in labor supply.

One way to potentially disentangle the remaining mechanisms is to control directly for the size of the labor market. If increases in labor supply explain the results, then exposure should impact wages through the size of the labor market. It is important to note that when estimating the impact of exposure on market wages, I exclude workers from privatized SOEs. Therefore, a labor market will increase in size if privatized workers move to private-sector firms, as these workers were previously not included in estimating the average market wage. It is important to note that this is certainly not a perfect test. Falling employment at SOEs impacts both the outside option and labor-supply of the market and therefore does not cleanly isolate the impact of outside options only. However, even controlling for the labor market size, privatization will impact the outside option for workers through reduced wages at privatized SOEs. In Column 3 of Table 5, I add the logarithm of labor-market size to the linear regression and find the effect of exposure on wages to be unchanged. Alternative specifications, such as controlling more flexibly for size yields nearly identical results. I interpret these results as suggestive evidence that the outside option may be driving the negative spillovers, however, as discussed above, controlling for labor market size does not necessarily cleanly isolate the effects due to changing outside options.

2.7 Conclusion

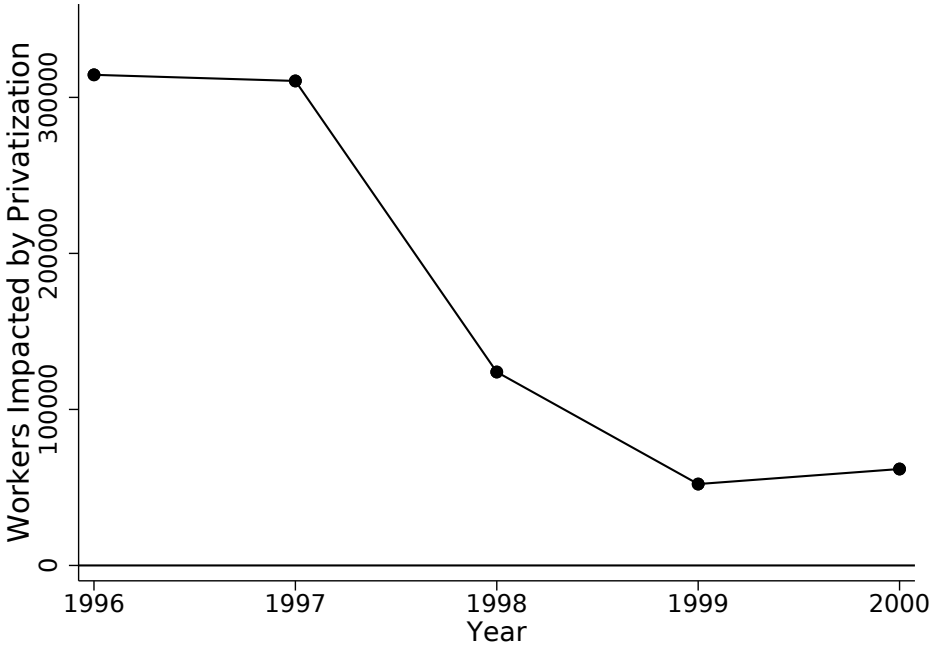
Despite the importance of SOEs and the scope of privatization, few studies explore the impact on the labor market. While many studies find privatization increases the profits of firms Megginson and Netter (2001), theoretically, privatization could decrease worker welfare (Birdsall and Nellis, 2003). This paper studies the impact of privatization on workers in Brazil, which implemented one of the world's largest privatization programs the 1990s. I find that wages fall by roughly 30 log points ten years after privatization for workers in privatized SOEs relative to control workers. Decomposing this wage loss, I find that approximately half is due to workers in privatized establishments being displaced and transitioning to lower-paying establishments, while half is due to within-establishment changes in compensation policies.

Turning to establishment-level outcomes, I find that establishments significantly reduce employment. The large wage and employment reductions at privatized SOEs may have important impacts on other workers in the same labor markets. First, large reductions in employment at SOEs will increase the supply of labor to the private sector, resulting in lower equilibrium wages. Second, wage and employment cuts at privatized SOEs will reduce the expected value of the outside option for workers, again resulting in wage declines. To identify spillovers, I construct a measure of exposure to privatization at the occupation-by-region level which traces how the impacts of privatization propagate through other firms in labor markets connected to privatized SOEs by labor mobility. I find that wages decline in labor markets that are more exposed to privatization relative to labor markets that are less exposed to privatization. A simple back-of-the-envelope calculation suggests that privatization decreased the aggregate wage in Brazil by about 4.5 log points, with about one-quarter of this effect attributable to the direct impact on privatized workers and three-quarters attributable to spillovers.

These results suggest that privatization can have large, negative impacts on workers in privatized firms, as well as private-sector workers in similar occupations. However, the total

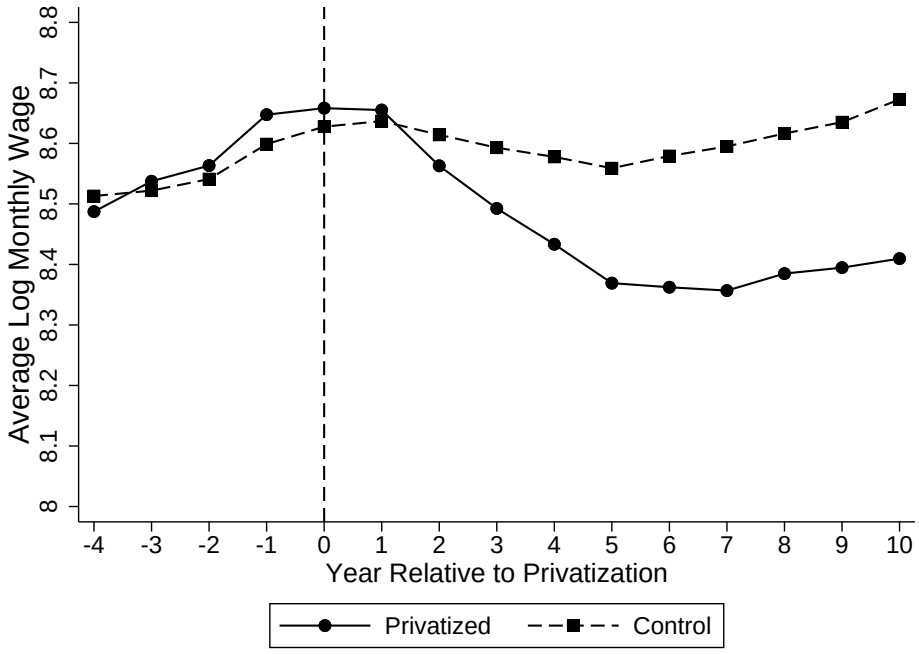
welfare consequences of privatization are still unclear. First, it could be wages and employment in SOEs were inefficiently high and subsidized by tax payers. Therefore, privatization could redistribute income from high-paid SOE employees to the general population through reducing the tax burden. Second, without firm-level data on output and prices, it is unclear how consumers are impacted by privatization. While a long literature finds firm profitability increases following privatization, the source of the gain remains unclear in many contexts. Increases in efficiency will contribute to economic growth and therefore privatization may be welfare enhancing even if workers exposed to privatization are negatively impacted. On the other hand, if privatization increases profits only through reduced wages and employment, then the perceived benefits of privatization may work through decreased worker welfare and not increased output and lower prices.

Figure 2.1: Number of Workers Employed in Privatized Establishments by Year



Note: This figure reports the number of workers employed in establishments that undergo privatization in a given year. Establishments are identified as privatized if the legal nature of the establishment changes from SOE to private and does not change back in a later year.

Figure 2.2: Average Log Monthly Wages of Privatized Workers and Matched Control Group

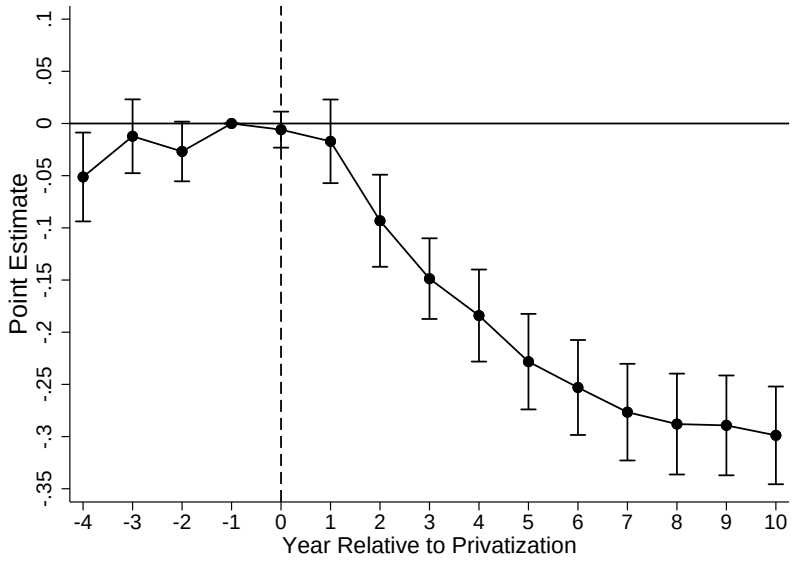


Note: This figure displays the average log monthly wage for workers in privatized establishments compared to a control group. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). The panel is not balanced as individuals may transition into unemployment, retirement or the informal sector. The informal sector makes up roughly 40 percent of all employment in Brazil.

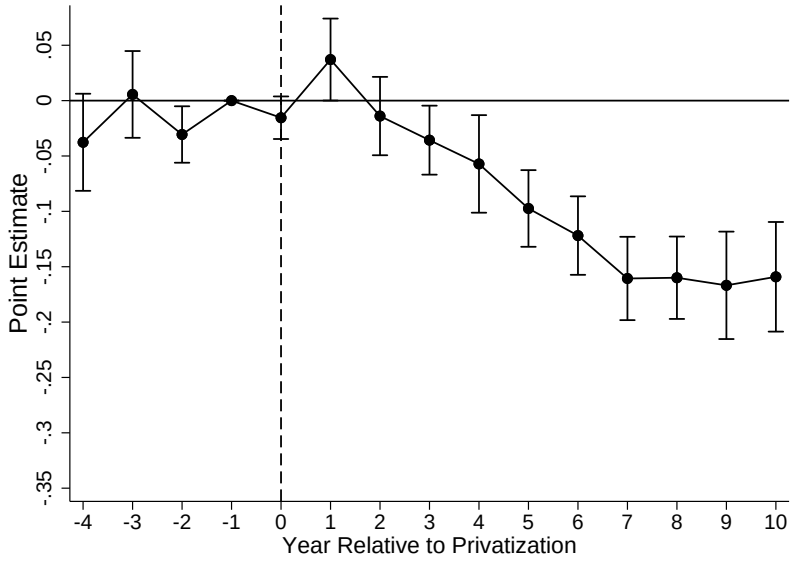
Figure 2.3: Event-Study Estimates of the Effect of Privatization on Log Monthly Wages

$$y_{it} = \sum_{k=-4}^{10} \delta_k (t = t^* + k) Privatized_i + \alpha_i + \gamma_t + x'_{it}\beta + \varepsilon_{it}$$

Panel A: All Workers

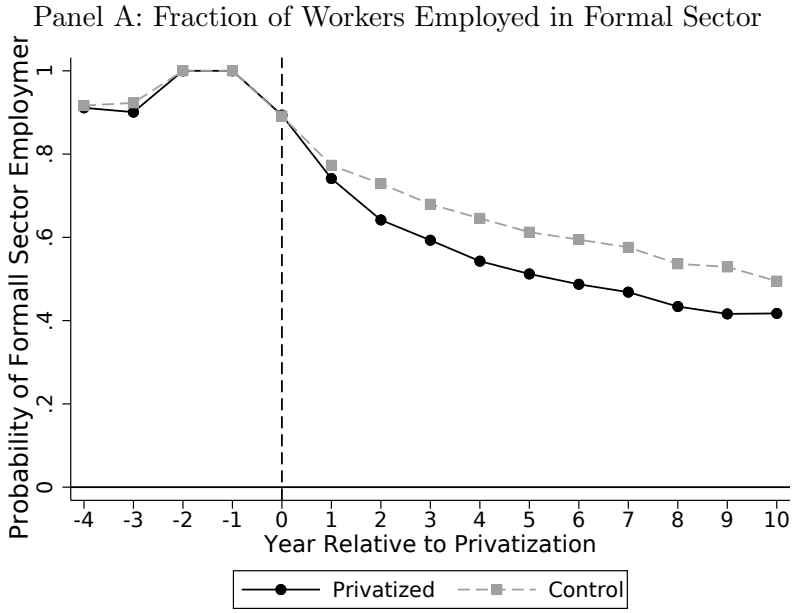


Panel B: Establishment Stayers

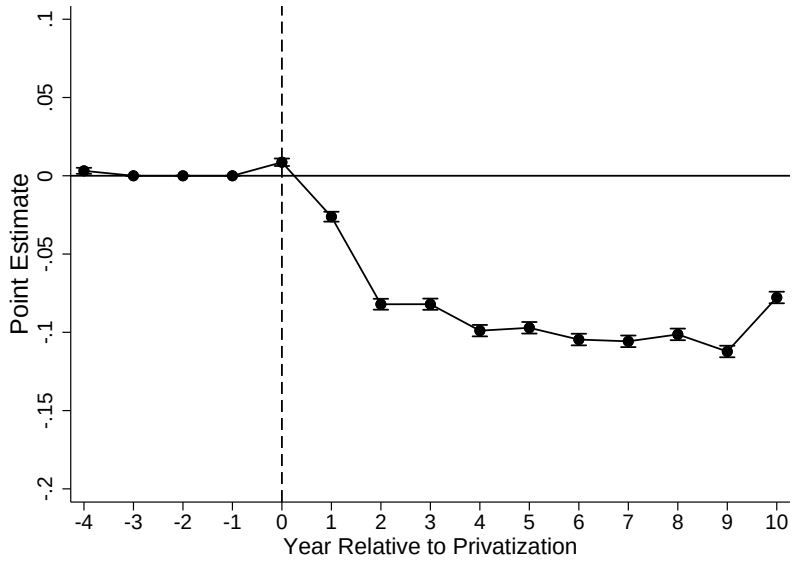


Note: This figure shows event-study estimates of the effect of privatization on log monthly wages.. The regressions control for individual fixed effects, year fixed effects, and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered at the individual and establishment level. Panel A includes all workers while Panel B restricts to establishment stayers. Stayers are defined as workers in time t who are employed in the same establishment as they were prior to privatization.

Figure 2.4: The Effect of Privatization on Formal Sector Employment



Panel B: Event Study Plot of the Impact of Privatization on Probability of Formal Sector Employment

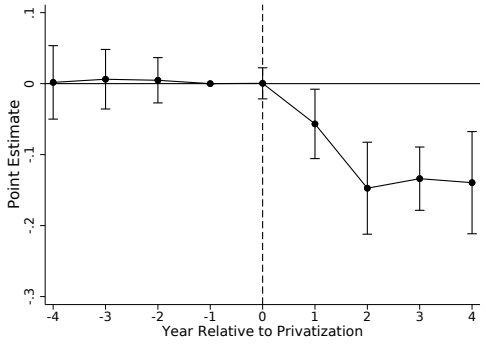


Note: Panel A displays the fraction of the sample that is employed in the formal sector in each year relative to the privatization event and Panel B presents estimates of the effect of privatization on the probability of formal sector employment. Workers must be in the formal sector from years $t^* - 2$ to $t^* - 1$ to be in the analysis sample. The regressions control for worker fixed effects, year fixed effects and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs (see text for additional details). 95% confidence intervals are clustered at the individual level.

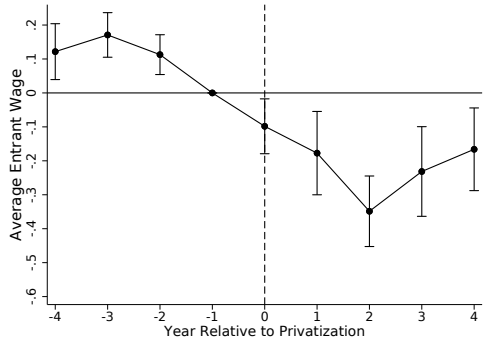
Figure 2.5: Event-Study Estimates of the Effect of Privatization on Establishment-Level Outcomes

$$y_{jt} = \sum_{k=-4}^{4} \delta_k (t = t^* + k) Privatized_{jt} + \psi_j + \gamma_t + \varepsilon_{jt}$$

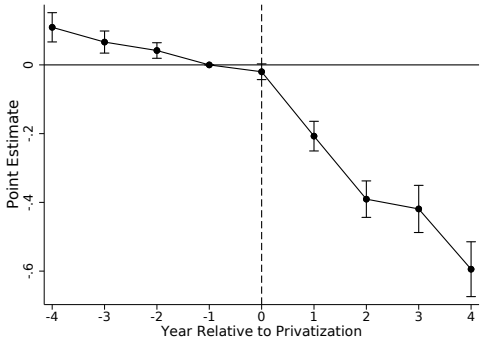
Panel A: Effect on Log Wage (All Workers)



Panel B: Effect on Log Wage (Entrants)



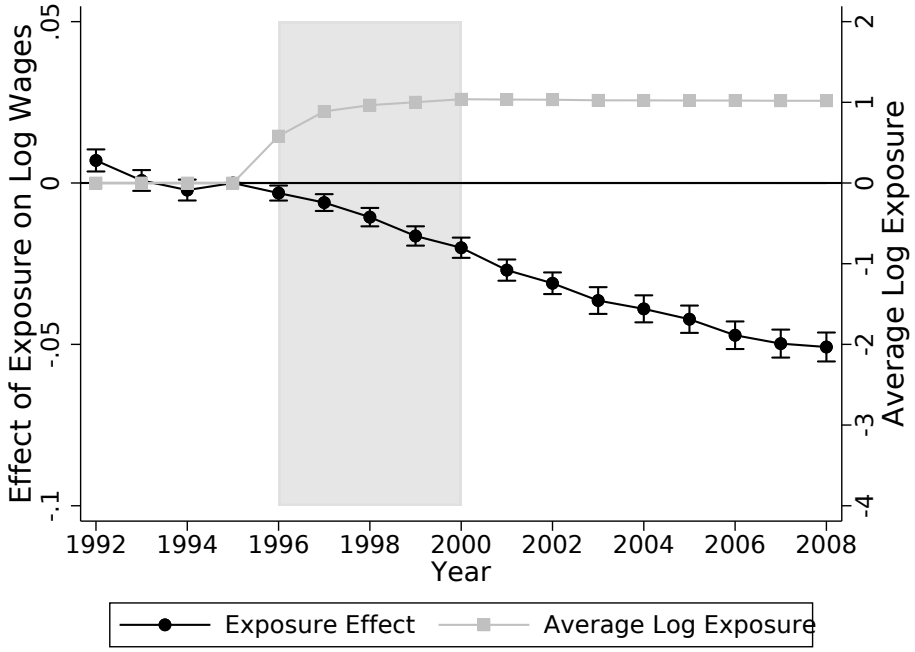
Panel C: Effect on Log Employment



Note: This figure shows event-study estimates of the effect of privatization of establishment-level log monthly wages in Panel A, entrants' log monthly wages in Panel B and log total employment in Panel C. The omitted category is the year prior to the privatization event. The regressions control for establishment fixed effects and year fixed effects. The sample is restricted to all establishments that are privatized at some point during the sample period (1996-2000). Standard errors are clustered at the establishment level. See Appendix Table 8 for the corresponding table.

Figure 2.6: Effect of Exposure to Privatization on Average Log Monthly Market Wages

$$ln(w_{it}) = \sum_{t=1992}^{2008} \delta_t \cdot (ln(Exposure_{i,2000} + 1) \cdot \gamma_t) + \alpha_i + \gamma_t + \pi X_{it} + \varepsilon_{it}$$



Note: This figure shows event-study estimates of the effect of exposure to privatization on log monthly market wages. The labor market is defined as an occupation-by-microregion cell, where occupations are defined at the five-digit level. The regressions control for labor market fixed effects and year fixed effects. I exclude all privatized establishments when constructing the average market wage. The gray shaded area indicates the period of privatization. Standard errors are clustered at the labor market level.

Table 1: Wage Premium in Privatized Establishments Before Privatization

	All Workers	Banking	Telecomm.	Electricity
	(1)	(2)	(3)	(4)
AKM Wage Premium	0.231	0.141	0.492	0.174
	(0.001)	(0.003)	(0.002)	(0.002)
	[8.339]	[8.605]	[8.019]	[8.617]

Note: The wage premium is estimated in two steps using data from 1992-1995, the years prior to any privatization event studied in this paper. In the first step, I estimate an AKM model with worker and establishment fixed effects and collect the estimated establishment fixed effects. In the second step, I restrict to SOEs that will be privatized eventually and private-sector establishments in the banking, telecommunications, and electricity sectors. I then regress the establishment fixed effects on an indicator for privatized SOE and sector fixed effects. The coefficient on the indicator for privatized SOE captures the premium associated with working for a privatized SOE in the years prior to privatization. This regression is weighted by the number of workers in a given establishment. Heteroskedastic-robust standard errors are presented.

Table 2: Characteristics of Privatized and Non-Privatized Workers

	Privatized	Control
Panel A: Demographics	(1)	(2)
Age	39.17	39.11
High School Graduate	0.48	0.44
College Graduate	0.19	0.25
Male	0.71	0.73
Panel B: Job Characteristics		
Log Monthly Wage	8.64	8.58
Panel C: Firm Characteristics		
Median Establishment Size	765	416
Financial Sector	0.27	0.27
Telecommunications	0.25	0.25
Electricity	0.44	0.44
Mining Metals	0.03	0.03
Unique Establishments	3,196	14,661
Unique Workers	142,854	142,854

Note: This table displays the average characteristics for privatized workers and the matched control group. Privatized workers are in the sample if they worked at the privatized firm for at least two years prior to privatization and a matched worker is found among all potential control workers. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments).

Table 3: The Effect of Privatization on Log Monthly Wages

	All Workers	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)	(4)
Panel A: Effect of privatization on all workers				
Post Short-run	−0.058	−0.111	−0.037	−0.039
	(0.015)	(0.019)	(0.013)	(0.015)
Post Long-run	−0.223	−0.366	−0.191	−0.152
	(0.021)	(0.028)	(0.019)	(0.022)
Avg. Outcome at $t = -1$	8.612	8.386	8.542	8.999
Observations	3,867,427	933,239	1,864,219	1,069,969
Panel B: Effect of privatization on stayers				
Post Short-run	−0.010	−0.040	−0.007	−0.015
	(0.012)	(0.020)	(0.012)	(0.012)
Post Long-run	−0.110	−0.156	−0.105	−0.106
	(0.017)	(0.029)	(0.015)	(0.019)
Avg. Outcome at $t = -1$	8.612	8.386	8.542	8.999
Observations	2,524,325	661,353	1,201,209	661,763
Worker FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes

Note: This table presents difference-in-differences estimates of the effect of privatization on log monthly wages. Column 1 pools all workers while columns 2-4 split the sample by whether the worker has no high school degree, a high-school degree or a college degree. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. Panel A includes all workers while Panel B restricts to establishment stayers which are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are two-way clustered by worker and establishment.

Table 4: The Effect of Privatization on Formal Sector Employment

	All Workers	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)	(4)
Post Short-run	−0.045	−0.049	−0.034	−0.045
	(0.001)	(0.003)	(0.002)	(0.002)
Post Long-run	−0.098	−0.098	−0.089	−0.086
	(0.002)	(0.003)	(0.002)	(0.003)
Avg. Outcome	0.677	0.564	0.710	0.739
Observations	4,148,940	1,150,110	1,924,875	1,073,955
Worker FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes

Note: This table presents difference-in-differences estimates of the effect of privatization on the probability a worker is employed in the formal sector. Column 1 pools all workers while columns 2-4 split the sample by whether the worker has no high-school degree, a high school degree or a college degree. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are clustered at the worker level.

Table 5: The Effect of Exposure to Privatization on Average Log Monthly Market Wages

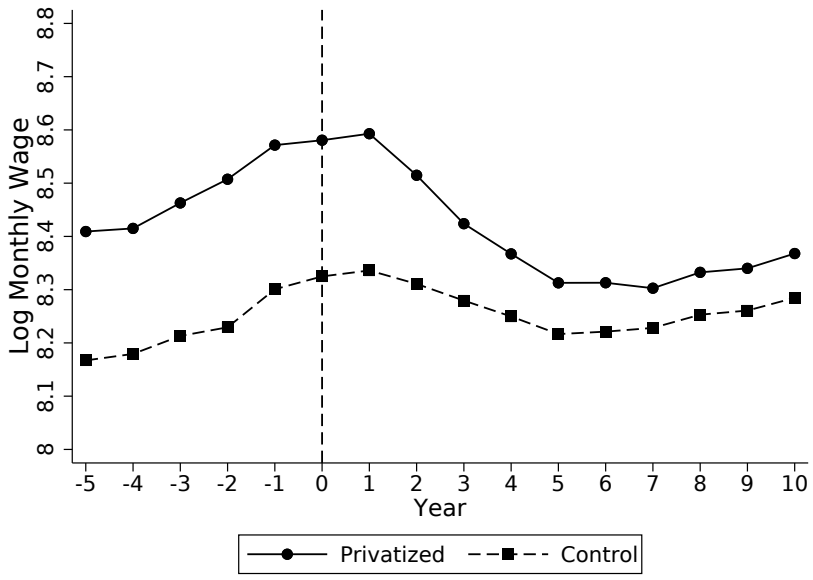
	All Industries			Tradables Only (Manufacturing)		
	(1)	(2)	(3)	(4)	(5)	(6)
ln(Exposure+1)	−0.031		−0.031	−0.022		−0.022
	(0.001)		(0.001)	(0.002)		(0.002)
asinh(Exposure)		−0.025			−0.018	
		(0.001)			(0.001)	
ln(Market Size)			0.001			0.006
			(0.001)			(0.001)
Observations	1,410,906	1,410,906	1,410,906	724,837	724,837	724,837
R-squared	0.891	0.891	0.891	0.914	0.915	0.915
Labor Market FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents OLS estimates of the effect of exposure to privatization on log average market wage in all sectors in Columns 1-3 and in manufacturing industries only in Columns 4-6. The labor market is defined as an occupation-by-microregion cell. To construct the average market wage, all observations from privatized establishments are excluded, and therefore, changes in the market wage do not reflect the direct impacts of privatization. Exposure to privatization depends on (1) fraction of jobs privatized within the labor market and (2) fraction of jobs privatized in closely related labor market. See Section 2.6 for more details on the construction of the exposure measure. Asinh is the inverse hyperbolic sine function which closely approximates the natural logarithm, but is well-defined at zero. Standard errors are are clustered at the labor market level.

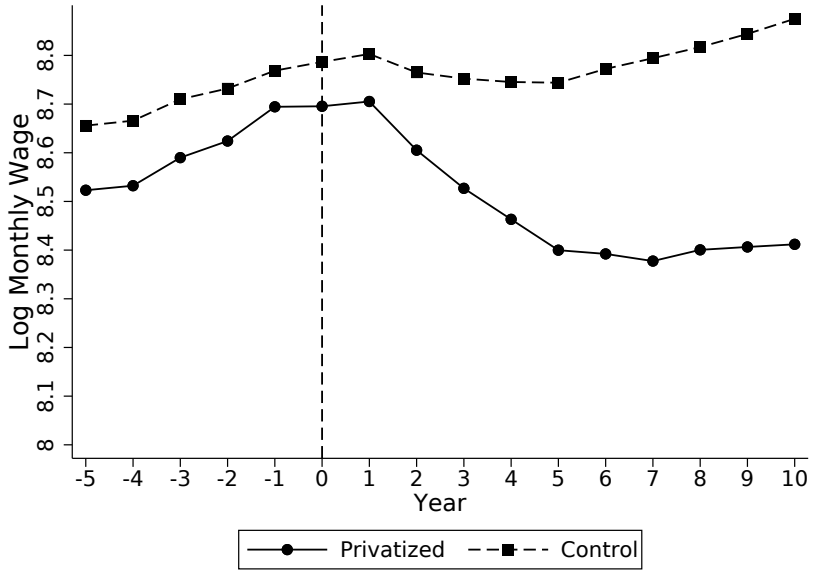
2.8 Additional Results

Figure 2.7: Log Monthly Wages of Privatized Workers and Matched Control Group

Panel A: Control Group: Private-Sector Workers



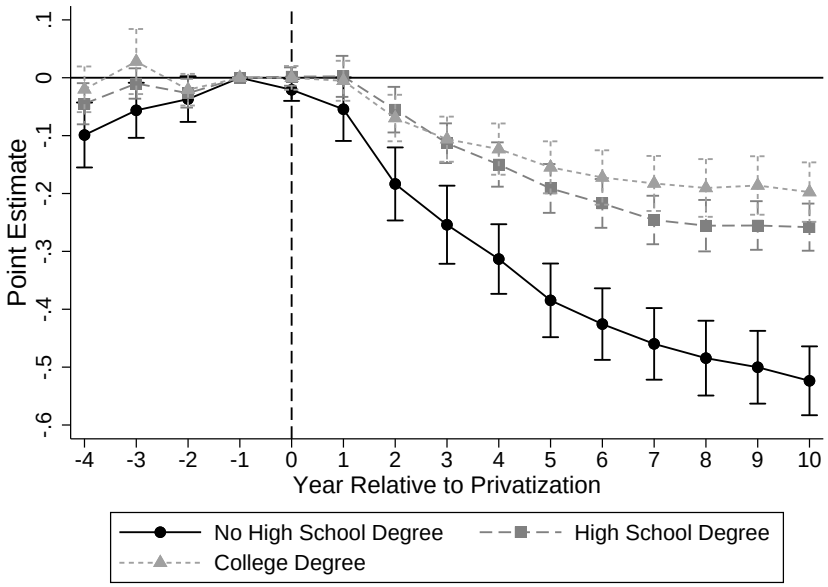
Panel B: Control Group: Never-privatized SOE Workers



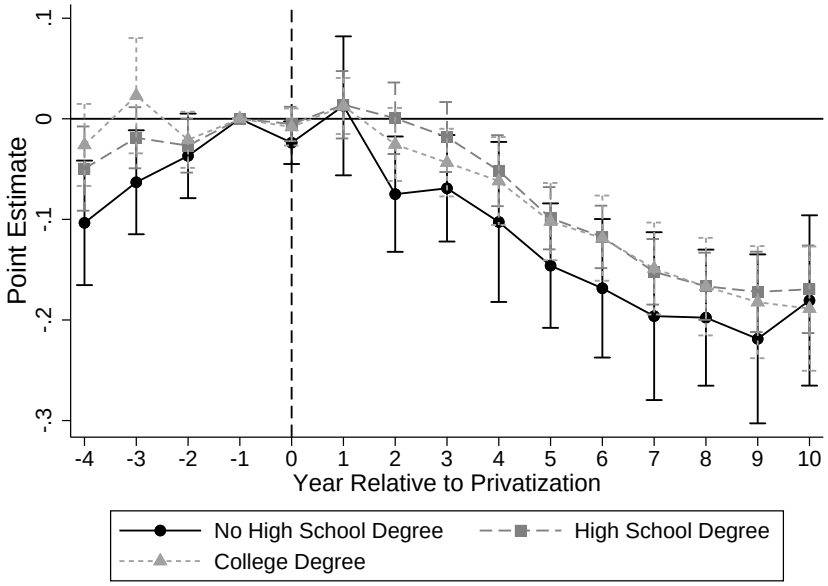
Note: This figure displays the average log monthly wage for workers in privatized establishments compared to a matched control group. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments (Panel A) or never-privatized SOEs (Panel B). The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments).

Figure 2.8: Event-Study Estimates of the Effect of Privatization on Log Monthly Wages by Education

Panel A: All Workers



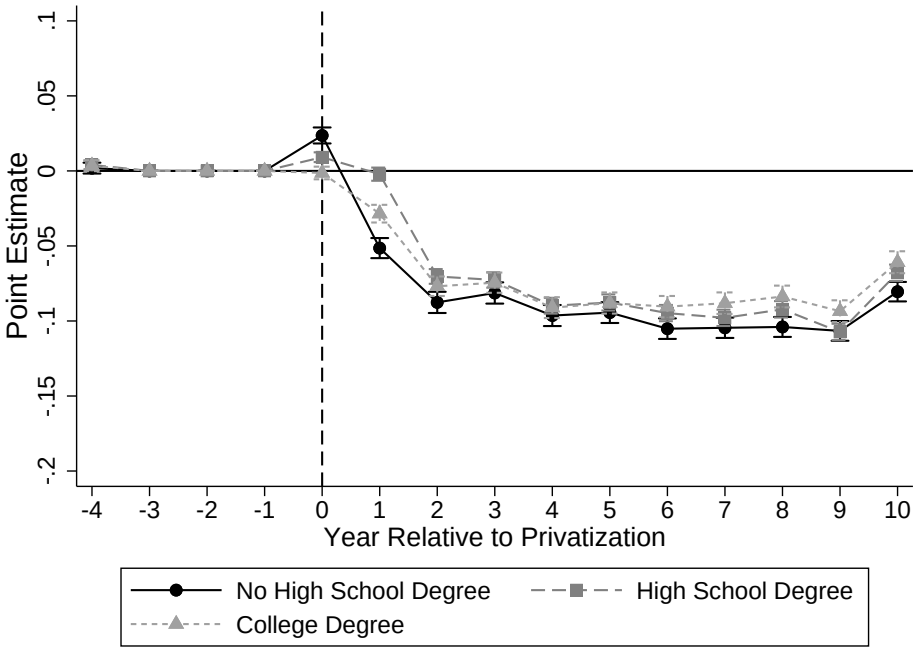
Panel B: Establishment Stayers



Note: This figure shows event-study estimates of the effect of privatization of log monthly wages separately by education. The regressions control for individual fixed effects, year dummies, and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and 5-year age bins. 95 percent confidence intervals, two-way clustered at the individual and establishment level, are displayed.

Figure 2.9: Event-Study Estimates of the Effect of Privatization on Formal Sector Employment by Education

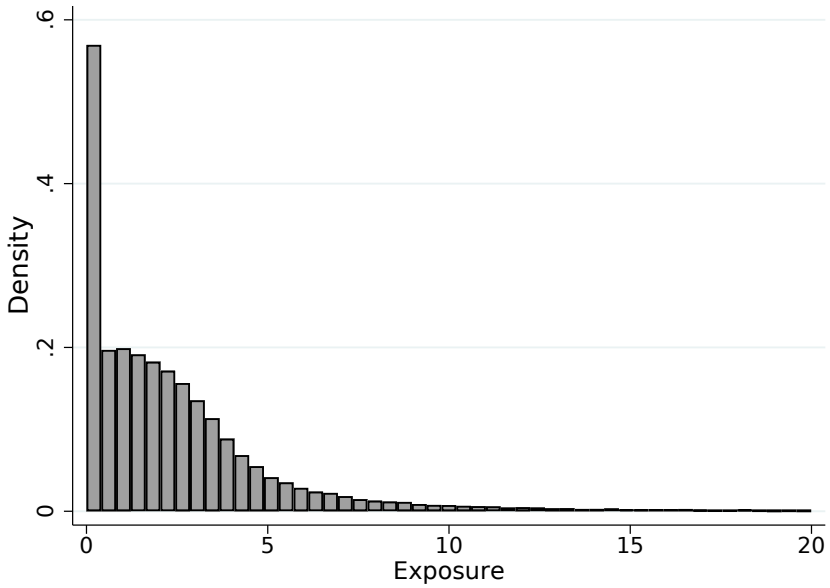
$$y_{it} = \sum_{k=-4}^{10} \delta_k (t = t^* + k) Privatized_i + \alpha_i + \gamma_t + x'_{it}\beta + \varepsilon_{it}$$



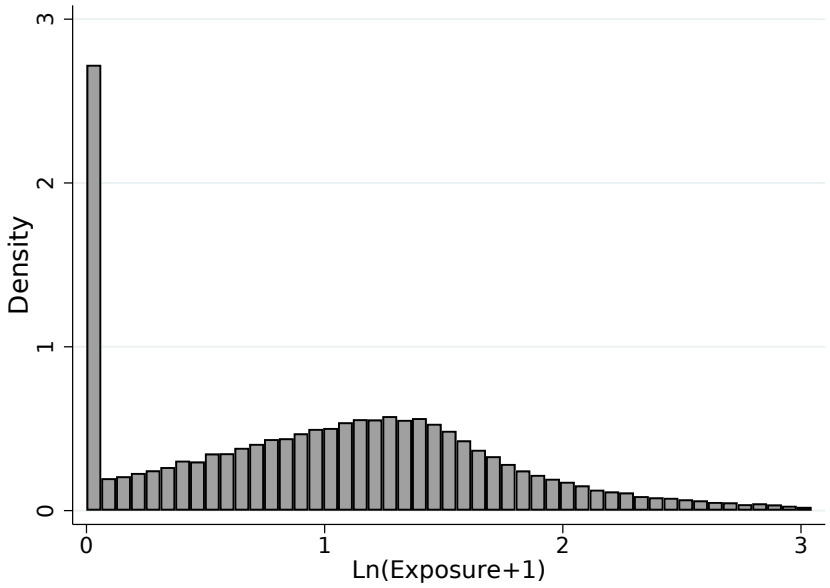
Note: This figure shows event-study estimates of the effect of privatization on the probability of formal sector employment. The omitted category is three years prior to the privatization event, given workers must be in the formal sector from years t^*-2 to t^*-1 to be in the analysis sample. The regressions control for worker fixed effects, year fixed effects and a cubic in age. The control group is constructed by matching workers in privatized SOEs to workers employed in either private-sector establishments or never-privatized SOEs. The matching variables include two-digit occupation, two-digit industry and bins for age (where age bins are five-year increments). Standard errors are clustered at the individual level.

Figure 2.10: Histograms of Exposure Measures

Panel A: Histogram of Exposure



Panel B: Histogram of Ln(Exposure+1)



Note: This figure displays a histogram of exposure to privatization in Panel A and $ln(Exposure+1)$ in Panel B. Each observation is a labor market, where a labor market is defined as an occupation-by-microregion cell. This plot defines occupations at the five-digit level.

Table 6: Robustness Results

	Private Control	SOE Control	Winsorize	Match Earlier	Add Vars.	5 digit Occ./Sec.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of privatization on all workers						
Post Short-run	−0.032	−0.056	−0.048	−0.047	−0.023	−0.050
	(0.015)	(0.015)	(0.014)	(0.017)	(0.008)	(0.014)
Post Long-run	−0.154	−0.258	−0.203	−0.226	−0.214	−0.221
	(0.019)	(0.024)	(0.019)	(0.023)	(0.012)	(0.021)
Avg. at $t = -1$	8.436	8.732	8.620	8.649	8.671	8.652
Observations	1,965,433	2,686,613	3,746,461	3,101,673	970,427	3,127,210
Panel B: Effect of privatization on stayers						
Post Short-run	−0.000	0.002	−0.001	0.005	0.011	−0.013
	(0.012)	(0.014)	(0.010)	(0.017)	(0.007)	(0.012)
Post Long-run	−0.057	−0.120	−0.090	−0.109	−0.119	−0.127
	(0.019)	(0.019)	(0.014)	(0.021)	(0.012)	(0.016)
Avg. at $t = -1$	8.436	8.732	8.620	8.649	8.671	8.652
Observations	1,288,697	1,799,471	2,437,597	2,095,286	663,911	2,104,587
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents difference-in-differences estimates of the effect of privatization on workers' wages. Column 1 matches workers in privatized firms to workers in private-sector firms. Column 2 matches workers in privatized firms to workers in never-privatized SOEs. Column 3 uses the main control samples, which allows workers to be matched to either a private-sector establishment or an SOE, and winzorizes log wages at the bottom 5 percentiles and the top 5 percentiles. Column 4 matches workers two years prior to privatization rather than one year prior to privatization. Column 5 adds microregion and bins for firm size to the coarsened-exact-matching algorithm. Column 6 matches on five-digit occupation and sector rather than two-digit occupation and sector. The short-run effect is equal to the effect of privatization in the four years following privatization. The long-run effect is equal to the effect of privatization in years five through ten following privatization. Panel A includes all workers while Panel B restricts to establishment stayers which are defined as workers in time t who are in the same firm as they were prior to privatization (i.e. year $t^* - 1$). Standard errors are two-way clustered by worker and establishment.

Table 7: Informal Sector Wage Penalty

	All Sectors	Privatized Sectors
	(1)	(2)
Informal	−0.324	−0.264
	(0.001)	(0.011)
Union	0.099	0.136
	(0.002)	(0.008)
Education (Years)	0.044	0.078
	(0.000)	(0.001)
Average outcome	6.101	6.904
Informality rate	0.420	0.139
Observations	830,226	21,762
R Squared	0.646	0.691
Year FE	Yes	Yes
Sector FE	Yes	Yes
Occupation FE	Yes	Yes
State FE	Yes	Yes
Demographics	Yes	Yes

Note: This table presents OLS estimates of the informal sector wage penalty. Data is from the Brazilian National Household Survey, years 2002-2009. Additional demographic covariates include gender, within-job tenure, number of jobs held, and a cubic in age. Heteroskedastic-robust standard errors are presented in parentheses.

Table 8: The Effect of Privatization on Average Log Wages and Log Employment

	Avg. Log Wage	Avg. Entrant Log Wage	Log Emp
	(1)	(2)	(2)
Event time $= -4$	0.002	0.122	0.109
	(0.026)	(0.042)	(0.022)
Event time $= -3$	0.006	0.171	0.066
	(0.021)	(0.034)	(0.016)
Event time $= -2$	0.005	0.113	0.042
	(0.016)	(0.030)	(0.012)
Event time $= 0$	0.000	-0.098	-0.020
	(0.011)	(0.041)	(0.012)
Event time $= 1$	-0.057	-0.177	-0.207
	(0.025)	(0.063)	(0.022)
Event time $= 2$	-0.147	-0.349	-0.391
	(0.033)	(0.053)	(0.027)
Event time $= 3$	-0.134	-0.231	-0.419
	(0.023)	(0.067)	(0.035)
Event time $= 4$	-0.140	-0.166	-0.594
	(0.037)	(0.062)	(0.041)
Avg. Outcome at $t = -1$	8.473	8.606	2.284
Observations	22,040	15,047	22,040
Establishment FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: This table presents event-study estimates of the effect of privatization on average log wages (Column 1) average log entrant wages (Column 2) and log employment (Column 3) . The sample is restricted to establishments which are privatized at some point between 1996 and 2000. Standard errors are clustered at the establishment level.

Table 9: Effect of Privatization on AKM Wage Premiums

	No HS Degree	HS Degree	College Degree
	(1)	(2)	(3)
Short-run	−0.045	−0.065	−0.076
	(0.002)	(0.002)	(0.002)
Long-run	−0.121	−0.101	−0.098
	(0.006)	(0.004)	(0.006)
Observations	80,786,003	23,844,432	9,242,929

This table estimates an AKM model with worker fixed effects, establishment fixed effects, a cubic in age interacted with year dummies, as well as in indicator for the short-run effect of privatization which is equal to one in the four years after privatization and a long-run effect of privatization which is equal to one if it has been more than four years since privatization. The sample includes all years between 1993-2004. Homoscedastic-standard errors are reported in parentheses.

Table 10: Robustness Results: Effect of Exposure to Privatization on the Log Market Wage

	1 Digit Occ.	2 Digit Occ.	3 Digit Occ.	4 Digit Occ.	5 Digit Occ.	Broader Cluster	Add Micro by Year	Never in Priv. SOE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	−0.070	−0.063	−0.049	−0.035	−0.031	−0.031	−0.029	−0.031
	(0.010)	(0.003)	(0.002)	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)
Observations	83,492	365,683	804,337	1,363,475	1,410,906	1,410,906	1,410,825	1,393,912
R-squared	0.857	0.870	0.876	0.883	0.891	0.891	0.892	
Labor Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Microregion X Year FE	No	No	No	No	No	No	Yes	No

Note: This table presents OLS estimates of the effect of exposure to privatization on log average market wage. The labor market is defined as an occupation-by-microregion cell. Columns 1-5 vary the level of aggregation for the occupation from one digit (Column 1) to five digits (Column 5). Column 7 adds microregion-by-year fixed effects to the specification. Column 8 eliminates any workers that have ever been employed in a privatized SOE when computing the average market wage. In Columns 1-5 and Columns 7-8, standard errors are clustered at the labor market level. Column 6 two-way clusters standard errors at the more general microregion and occupation level.

2.9 Data Appendix

2.9.1 Overview of RAIS data

The *Relação Anual de Informações Sociais* (RAIS) is an employer-employee matched dataset which includes information on all workers and establishments in the formal sector of Brazil. The main use of the RAIS is to compute federal wage-supplements (*Abono Salarial*). While not reporting can in theory result in fines, these fines are rarely issued in practice. However, workers and establishments are incentivized to provide accurate wage information given the federal public wage-supplement is based on the wage reported in the RAIS.

2.9.2 Sample Selection

In the RAIS, workers are identified by an individual-specific PIS (Programa de Integração Social), a unique time-invariant worker identifier similar to a social security number. I follow Menezes-Filho and Muendler (2011a) and drop workers with PIS identifiers less than 11 digits, as these are not valid identifiers. Errors in worker identifiers may be caused by (1) bad compliance and bookkeeping errors or (2) to allow workers to withdraw from their severance account through fake layoffs and rehires.

Most of the analysis (with the exception of the AKM models), is restricted to either state-owned enterprises or private-sector establishments. This eliminates public administration workers or workers for non-profits and charities. The distinction between SOEs and public administration is important given public administration workers are employed under different contracts than workers in SOEs or private-sector establishments. The public administration contract includes restrictions on firing employees while contracts for workers in SOEs do not.

2.9.3 Variable Definitions

PIS: A PIS is a worker identifier that is unique to a given worker over time.

CNPJ: The CNJP is an establishment-level identifier issued by the Brazilian tax authority which is unique to a given establishment over time. The first eight digits of the CNPJ corresponds to the firm of the establishment, while the last six correspond to the establishment within the firm.

Education: The RAIS records education at eight different categories. I re-code these variables to four categories: (1) Less than High-School (2) High-School Graduate (3) College Graduate. Education for an individual worker is set to the modal value of education for the worker over the sample period.

Occupation: Occupations are defined by the Classificação Brasileira de Ocupações (CBO) into 2,355 distinct groups. To increase the number of matches, I use subgroups consisting of the first two digits of the five-digit occupation code, which reduces the number of distinct occupations down to 84.

Sector: Sectors are reported under the *CNAE* four-digit classification (*Classificação Nacional de Atividade Econômica*) for 654 industries. I aggregate sectors to the two-digit level in the paper, unless specified otherwise.

Wage: Wage refers to total payments, including regular salary payments, holiday bonuses, performance-based and commission bonuses, tips, and profit sharing agreements, divided by total months worked during the year for that employer. Payments that are not considered part of the wage include severance payments for layoffs and indemnity pay for maternal leave. Wages in the dataset are reported in terms of multiples the monthly minimum wage, which are then converted to real earnings using inflation adjustments available from Brazil's Institute of Applied Economic Research (IPEA).[35]

Legal Nature: An establishment is issued a tax identifier (CNPJ) along with a legal form. There are many legal forms possible. For the purposes of this paper, any legal form which

[35]The early 1990s was a period of rapid inflation in Brazil. By 1995, price stabilization had succeeded due to the Plano Real reform which was implemented in August 1994.

indicates at least partial state ownership is designated as an SOE. This differs from public administration establishments which have different legal forms. Legal form is only included in the RAIS from 1995 onwards. When a given establishment is privatized, the legal form changes and is updated in the Brazilian business register, but the tax identifier does not change.[36]

Microregion: Microregions in Brazil are defined by the Brazilian Statistical Agency (IBGE). Microregions group together clusters of contiguous municipalities with similar geographic and economic characteristics, similar to commuting zones in the United States.

2.9.4 Overview of PNAD

The Brazilian National Household Survey, Pesquisa Nacional por Amostra de Domicìlios (PNAD) is a survey conducted by the Brazilian statistical agency IBGE, since 1981. From 2001 onward, the Brazilian microdata from the PNAD is available online at the IBGE website.[37] The PNAD uses the same sector classification (CNAE) and occupation classification (CBO) as the RAIS data. Most importantly for this project, the PNAD also contains a question about whether the worker is employed in the formal or informal sector, allowing to estimate the informal-sector wage penalty controlling for both the sector of employment as well as the occupation of the worker.

[36]For a searchable online database of the business register see http://www.receita.fazenda.gov.br

[37]See website:https://ww2.ibge.gov.br. A package in STATA, called Data Zoom, is made available by the Department of Economics at PUC-Rio which standardizes questions across years.

Chapter 3

Employment Protection Legislation and Efficiency: Evidence from Brazil

3.1 Introduction

Employment Protection Legislation (EPL) is a pervasive feature of modern labor markets. While EPL mandates certain benefits to workers, increased hiring costs could lead to higher unemployment and lower aggregate output. Throughout the late 1990s, a number of European countries relaxed EPL laws, believing increased flexibility would improve labor-market outcomes (OECD, 2000). However, EPL also raises the cost to fire workers, and could therefore lead to increases in aggregate employment.

Despite a large body of research, the general equilibrium effects of EPL are still uncertain. Early studies utilizing cross-country policy variation may suffer from omitted variable bias. More recent work uses within-country, between-firm policy variation to identify the effects of EPL on employment. However, this approach can only identify *partial* effects of EPL, for example, differences in employment levels between firms that face high vs. low firing costs. This analysis therefore cannot in general capture the aggregate effect of interest.

Additionally, the broad and often non-monetary nature of EPL makes it difficult to measure holistically, implying policy variation can generally only capture the effect of changes in certain components of EPL. In our setting, Brazil, EPL consists of both monetary costs to firms in the form of firing penalties, and non-monetary costs in the form of legal recourse and termination notice periods.

In this paper, we combine quasi-experimental evidence with a structural model of the labor market to identify the aggregate effect of EPL on unemployment and output. In the first part of the paper, we exploit the tenure-dependence built into the EPL system in Brazil to cleanly identify its effect on the hazard rate of job termination. Since EPL in Brazil only applies once a job reaches a tenure of 3 months, the cost of terminating a job as a function of tenure features a discrete jump. Using administrative data from the *Relação Anual de Informações Sociais* (RAIS), we find this creates a stark spike in the job termination hazard rate (See Figure 3.1) at exactly 3 months of tenure.[1]

We argue that this spike in job termination arises because firms are uncertain about worker productivity and prefer to end matches at 3 months if the expected productivity is too low. To bolster this argument, we rule out a number of alternative explanations for the observed spike in the hazard rate at 3 months. First, we find that the spike in job termination occurs consistently across industries and is uncorrelated with month-to-month employment changes, implying seasonal demand volatility cannot explain the results. Second, we find bunching occurs consistently across occupations, including high-wage occupations, implying the bulk of this spike is likely not driven by firms rotating through low-skill workers to avoid paying firing costs. Given these results, we interpret the spike in the job termination hazard as firms firing permanent workers with low expected productivity.

[1]As can be seen in Figure 1, the job termination hazard also spikes at both 6 months and 1 year. Van Doornik et al. (2018) show that the increase in job termination at 6 months is due to "fake separations" in which workers who become eligible for UI at 6 months are transitioned into informal work so that they can collect unemployment insurance. The spike at 1 year is likely due to increased firing costs due to investigations that take place when workers with tenure longer than 1 year are fired (**?**). Both of these incentives are outside the model and therefore, we do not actually use the hazard from 6 months to one year to estimate our structural model.

In the next part of the paper, we develop a structural model of the labor market to map the effect of EPL on the job termination hazard to the general equilibrium outcomes of interest. Guided by the empirical results, we add tenure-dependent EPL to the model of Moscarini (2005), which combines endogenous job destruction through learning about match productivity with a frictional labor market à la Diamond-Mortensen-Pissarides. We model EPL as a fixed cost that firms must pay if they terminate a worker with at least 3 months' tenure. This fixed cost captures the equivalent real cost to firms of the wide variety of EPL regulation that firms face in reality.

We estimate the model by simulated methods of moments. Intuitively, the spike in the empirical job termination hazard is used to infer the size of the fixed cost parameter that firms must pay due to EPL. This allows us to infer the cost of EPL without having to make assumption about hard-to-measure components of EPL such as litigation costs and advanced notices of dismissal. This strategy of inferring hard-to-measure policy parameters from their identified effects in the data builds on Garicano, Lelarge and Van Reenen (2016), and ensures that the estimated model parameter captures the equivalent real cost that EPL imposes on firms. In addition, variation in the empirical hazard rate at early tenures allows us to identify and estimate model parameters governing the initial productivity of matches and the variance of the idiosyncratic shocks to a worker's production.[2]

Using our estimated model, we perform a counterfactual exercise and compare macroeconomic outcomes with and without EPL. We find that the presence of tenure-dependent EPL lowers aggregate output by 7.6%, and causes a 8.3 percentage point increase in the formal sector unemployment rate, which we interpret as a reallocation of workers into the informal sector. The increase in formal sector unemployment in turn causes firms to post more vacancies in equilibrium. The combination of higher unemployment and larger aggregate vacancy posting costs results in a 13.6% drop in formal sector production. Importantly,

[2]To fully estimate the model, we use additional aggregate moments that measure the share of the informal sector in Brazil, and the formal sector wage premium. We also calibrate a small subset of parameters in line with the rich literature that studies frictional labor markets.

these joint effects quantitatively swamp the gain in formal sector labor productivity that tenure-dependent EPL creates due to the earlier termination of low quality matches.

Our structural model also sheds new light on the role of tenure-dependence in generating these efficiency losses. Comparing economies, one with tenure-dependent EPL and one with EPL that applies to jobs of all tenures, we find that simply removing tenure-dependence increases aggregate output by 5.1%. Tenure-dependence therefore accounts for around 70% of the efficiency losses of EPL. This finding further suggests that the gains in labor productivity created by tenure-dependence, and often used as justification for such policy design, are overstated, and are dominated by the extra vacancy creation costs that tenure-dependence induces.

We confirm that our results do not depend on how we calibrate unidentified model parameters. First, we show that the quantitative magnitude of the efficiency loss is robust to changing the elasticity of matches to unemployment (holding the bargaining weight at its estimated value), and thus changing the quantitative violation of the Hosios (1990) condition. Hence our quantitative statement about efficiency losses is not simply a function of "correctly" calibrated parameters.

Second, we show that the efficiency losses remain large even when the cost of EPL is rebated back to newly terminated workers as a transfer. Intuitively, our results hinge on how the size and timing of EPL affect firm termination decisions. These decisions do not take into account where the costs of EPL go once the firm has paid them. Therefore, our results are robust to the fraction of EPL costs that are a transfer to newly unemployed workers.

Finally, we show that introducing a minimum wage does not affect the equilibrium impact of EPL. In our estimated model, 3% of employed workers earn the minimum wage during the sample period. This fraction is line with our data as well as the evidence presented by Engbom and Moser (2018). In our setting, we find that the interaction between EPL and minimum wage policy is quantitatively insignificant.

Related Literature Our paper relates to two distinct literatures. First, our clean identification of the impact of EPL on firms' job termination decisions contributes to an extensive literature that explores the impact of EPL on macroeconomic outcomes (Lazear, 1990; Botero et al., 2004; Di Tella and MacCulloch, 2005; Garibaldi and Violante, 2005; Autor et al., 2007; Kugler and Pica, 2008; Daruich et al., 2017). These papers generally rely on two identification strategies. The first uses cross-country variation in EPL to estimate its impact, which may be confounded by omitted variables. The second uses within-country variation between-firm variation, for example, by exploiting EPL reforms or size-contingent laws to estimate similar causal effects.

Relative to these methods, we exploit tenure-dependence in the design of EPL in Brazil to identify its effect on job termination decisions. The identification strategy is similar to the public finance literature which utilizes discrete kinks or notches in budget constraints to estimate structural parameters (Saez, 2010; Chetty, 2012; Kleven, 2016). Furthermore, by estimating the behavioral response of firms at the microeconomic level, our estimation strategy relies on weaker assumptions than papers estimating the aggregate effect of EPL on macroeconomic outcomes. Instead, we build and estimate a structural model that makes this mapping explicit.

Our structural estimation exercise relates to the literature that studies the effects of EPL on macroeconomic outcomes through the lens of a search model (Ljungqvist, 2002; Garibaldi and Violante, 2005; Pries and Rogerson, 2005; Pinto, 2015). In these papers, EPL is modeled as a fixed cost that applies to jobs of all tenures, thus missing the tenure-dependence that is a key feature of many labor markets, including Brazil's (Cahuc et al., 2016). Our contribution, therefore, is to capture the tenure-dependence of EPL within a structural model, and to use our rich data to estimate key model parameters rather than relying solely on calibration.

In contemporaneous work, Cahuc et al. (2019) use similar structural methods to study the effects of EPL on French labor market outcomes.[3] In addition to focusing on a markedly

[3]The authors exploit the fact that severance payments feature a discrete jump in magnitude for jobs with tenure greater than 2 years.

different labor market, our work and results differ in a number of meaningful, but complementary ways. First, we offer a micro-foundation for stochastic productivity by embedding EPL into the learning model of Moscarini (2005). In contrast, Cahuc et al. (2019) assume that productivity follows an exogenous geometric brownian motion, and calibrate the drift of the process exogenously. We use the richness of the Brazilian matched employer-employee to provide evidence that learning about productivity is a key driver of job separations in our setting. Second, our model naturally features a stationary equilibrium, linking hazard rates to macroeconomic outcomes such as unemployment and GDP. We exploit this fact to study how the presence of EPL and its tenure-dependence affect the macroeconomy as a whole. This focus complements Cahuc et al. (2019), who focus instead on how EPL affects workers differently depending on their status as protected or non-protected.

The paper proceeds as follows: Section 3.2 introduces our data and institutional setting within the Brazilian labor market. In Section 3.3, we document the hazard rate spike created by the EPL, and show that it is robust feature of a wide variety of labor submarkets. We develop our theoretical framework in Section 3.4, and describe the estimation and calibration procedure in Section 3.5. We present our main quantitative exercise together with robustness checks in Section 3.6. Finally, Section 3.7 concludes.

3.2 Institutional Setting and Data

3.2.1 EPL in Brazil

In Brazil, EPL is composed of many components. For example, formal sector workers in Brazil are guaranteed severance pay if dismissed without cause, yearly bonuses equivalent to one month's salary, and 30 days' notice for any separation.[4] Furthermore, in the event of a separation, the employer firm must pay a firing penalty, which is equal to roughly one month of the worker's salary for every year the worker has been employed at the firm.

[4]In the event that a worker is given advance notice of a termination, the worker must be allotted time to search for a new job.

The key feature of EPL in Brazil that facilitates our analysis is its tenure-dependence: all dimensions of EPL only apply to firms and workers that have been in an employer-employee match for at least 3 months. This sharp discontinuity in the cost of EPL as a function of tenure is the basis of our identification strategy in Section 3.3. Intuitively, the jump in EPL costs at 3 months incentivizes firms to terminate matches just before this tenure is reached to avoid incurring the higher costs of termination should the match deteriorate soon after 3 months. A trial period for workers is common in many countries, though restrictions and durations vary across countries (OECD, 2008).

3.2.2 Data

Our analysis utilizes administrative data from the *Relação Anual de Informações Sociais* (RAIS), years 2002-2007. The RAIS data contains linked employer-employee records from a mandatory survey administered by the Brazilian Ministry of Labor and Employment (MTE). Fines are levied on firms which provide inaccurate or incomplete information on the survey.

Each entry in the RAIS dataset is a employee-employer match. Each individual, firm, and establishment are assigned unique administrative identifiers which do not change over time. Importantly, the data track each the tenure of each employer-employee match (job). For our analysis, we bin tenure into 15 day intervals due to "heaping" in the distribution of tenure (i.e. it is much more likely to observe a 30 days job spell than a 29 day job spell). The data include additional information about the job, such as occupation, wage, hours, type of labor contract, whether the job has ended, and why the job has ended, and also contain demographic data on individuals, such as education, gender, ethnicity, and occupation. For more information about the dataset and the definition of variables, see Appendix Section 3.11.

3.2.3 Sample Selection

Our identification strategy hinges on the spike in job terminations at 3 months' tenure being solely driven by the timing of EPL. A natural confounder is therefore the presence of workers on temporary 3 month contracts. In Brazil, temporary contracts are subject to approval by the Ministry of Labor (MTE) and about 5 percent of workers at a given time are employed under such contracts. These contracts are approved to meet temporary increases in demand and many of these contracts last for three months. Therefore, a spike in the job termination hazard may naturally arise at 3 months due to the existence of such contracts.[5] Given the focus of our paper is on the effects of EPL on permanent employment contracts, we therefore eliminate temporary contracts from the majority of our empirical and theoretical analysis.[6]

In addition to eliminating temporary contracts, we restrict attention to workers aged 18-65, and working in full-time jobs (at least 35 hours per week). We exclude individuals with invalid identifiers (less than one percent of the data). Column 1 of Table 1 presents summary statistics for the population of 18-65 year olds. Column 2 presents summary statistics for jobs which last less than or equal to 3 months. Short-duration workers are slightly younger (30.4 vs. 31.5), less likely to be a college graduate (3.8 percent vs. 7.2 percent), are paid lower monthly wages (670.90 Real vs 819.21 Real), and are less likely to be in public administration jobs (7.4 percent vs. 2.2 percent) and more likely to be in agricultural jobs (14.0 percent vs. 9.3 percent). In total, there are 92,023,307 jobs corresponding to 29,438,306 unique workers. 24,427,409 jobs last 3 months or less (i.e. 26.5 percent of all jobs).

[5]While the presence of EPL may also theoretically cause increased substitution towards temporary contracts (Daruich et al., 2017), the regulated usage of temporary contracts likely limits this substitution in Brazil.

[6]Importantly for our study, the RAIS dataset includes information on the type of contract.

3.3 The Impact of EPL on the Job Termination Hazard

In this section we document a visible spike in the job termination hazard rate at a tenure of 3 months, and argue that it predominantly reflects the early termination of permanent employment contracts caused by the tenure dependence built into EPL in Brazil.

3.3.1 Estimating Bunching in Job Terminations

To summarize the quantitative magnitude of the spike in the job termination hazard rate at 3 months tenure, we estimate a "bunching" statistic. This summary statistic is useful as it is comparable across different labor submarkets and can be used to explore potential driving mechanisms of the hazard rate spike.

We follow the public finance literature (See Kleven (2016) for a review) to estimate bunching. Specifically, we fit a flexible polynomial to the empirical job termination hazard, excluding data from around the notch point T, which is the tenure length in which EPL takes affect. Formally, let $B_j = \{15, 30, ...\}$ define the bins and H_j indicate the hazard rate in bin j (i.e. H_{90} denotes the probability a job ends between 75 and 90 days, given the job has lasted for 75 days). To estimate the counterfactual hazard, we estimate the following regression:

$$H_j = \sum_{i=0}^{q} \beta_i \cdot (B_j)^i + \sum_{i=-R}^{R} \gamma_i \cdot \mathbb{1}[B_j = i] + \varepsilon_j^0 \tag{3.1}$$

where q is the order of the polynomial and R denotes the width of the excluded region around the notch in firing costs. In practice, we set $q = 10$ and $R = 15$, and therefore exclude any tenure durations that end between 75 and 105 days in the estimation of the counterfactual hazard rate. We use the results from Equation (3.1) to estimate the counterfactual hazard as:

$$\hat{H}_i = \sum_{i=0}^{q} \hat{\beta}_i (B_j)^i \tag{3.2}$$

The excess mass is defined as the difference in the true hazard rate and the counterfactual hazard rate at $T = 90$:

$$B = H_T - \hat{H}_T \tag{3.3}$$

The normalized excess mass, which we will refer to as bunching, b, is defined as the excess mass divided by the counterfactual hazard rate at tenure duration T.

$$b = \frac{B}{\hat{H}_T} \tag{3.4}$$

To compute standard errors for the excess mass and bunching, we generate hazards and excess mass by resampling the residuals in Equation (3.1). The standard error is then equal to standard deviation of the distribution of excess mass estimates over 500 bootstrap samples.

Figure 3.1 displays the job termination hazard rate. There is a visible spike in the hazard rate at a tenure of 3 months, which results in significant bunching.[7] We find that the excess mass is equal to 1.4, with a standard error of 0.234, indicating that the true hazard rate is more than double the predicted counterfactual hazard rate at 3 months' tenure.

The sizable excess mass indicates a non-trivial response by firms, who alter their job termination decisions in the presence of tenure-dependent EPL. An intuitive explanation, and indeed the mechanism we are most interested in, is that the presence of EPL for jobs with tenures of at least 3 months incentivizes firms to terminate a significant number of jobs with low expected productivity at exactly three months. This learning mechanism is formalized by our structural model in Section 3.4, in which the size of EPL exactly pins down the size of the hazard rate spike at 3 months' tenure.

[7] There is another much smaller spike in the hazard rate at around six months' tenure. Van Doornik et al. (2018) shows that this spike is due to "fake" separations. If a worker is fired after six months of tenure, the worker can receive unemployment insurance from the government. This incentivizes firms to fire workers and then split the unemployment insurance. In order to focus on the effects driven by the timing of EPL, and to keep the structural model as parsimonious as possible, we abstract from the effects of EPL beyond the bunching that occurs at 3 months' tenure.

In order to justify this intuition empirically, we now argue that the observed bunching is indeed predominantly driven by the early termination of permanent employment contracts, as opposed to firms optimally choosing to hire workers on short-term or temporary contracts.

3.3.2 Temporary Contracts

An advantage of our data is that we can drop all matches labeled as temporary contracts to ensure that our bunching analysis is not confounded by mechanical job terminations at 3 months' tenure.[8] However, hiring workers on temporary contracts is itself a regulated process in Brazil.[9] Therefore, in order to sidestep these regulatory frictions, firms desiring short-term employment arrangements may simply hire workers on permanent contracts with the intention of simply firing them after 3 months, thus mimicking the temporary contract. This behavior of creating artificial temporary contracts is not directly observable in our data and would have important implications for the appropriate way to model firms' responses in our structural model. We now examine two key reasons that could cause firms to create artificial temporary contracts.

Demand Volatility Firms that face volatile demand for their product will naturally have volatile labor demand that is best served via short term employment contracts. Such firms will therefore find it optimal to create artificial temporary contracts when official temporary contracts are unavailable.

One simple way to discern how much of the bunching is driven by this behavior is to compare bunching across different industries, where some industries are naturally more prone to short-term labor hiring than others. Specifically, we estimate bunching separately across

[8]To understand the role of temporary contracts, Appendix Figure 3.7 plots the job termination hazard rate which includes temporary contracts. As can be seen in the figure, the amount if bunching is larger with temporary contracts include (1.9 vs. 1.4).

[9]To hire a worker under a temporary contracts, firms must get permission from the Ministry of Labor. They must also establish that the worker needs to be hired on a temporary contract in order to meet seasonal fluctuations in demand.

industries at the 3-digit level, and then correlate bunching with the month-to-month variation in employment in the given 3-digit industry.[10] If labor demand volatility is a significant driver of bunching, we would expect industries with higher employment volatility to also display greater magnitudes of bunching.

To estimate demand volatility, we compute a normalized measure of monthly employment changes in industry j at time t as:

$$\Delta E_{j,t} = \frac{hires_{jt} - fires_{jt}}{hires_{jt}} \tag{3.5}$$

We then compute volatility of sector j as $V_j = Var(\Delta E_{j,t} - \Delta E_{j,t-1})$. In words, we create a time series of month-to-month net employment changes scaled by the total number of hires. We then take the variance of the first difference as our measure of employment volatility. We then correlate this measure with bunching by running the following regression:

$$ln(\hat{V}_j) = \beta_0 + \beta_1 \hat{b}_j + \varepsilon_j \tag{3.6}$$

where β_1 capture the correlation between bunching and employment volatility. Appendix Figure 3.2 shows the results of this regression as well as a scatterplot of bunching across industries. The first thing to notice in the figure is that bunching is positive in every single 3-digit industry, indicating that positive bunching is an important feature across many industries. Additionally, it does not appear as if bunching is strongly correlated with demand volatility. Demand volatility is actually *negatively* correlated with bunching, although the correlation is not significant. This suggests that demand volatility does not play a quantitatively important role in determining the amount of bunching that we estimate once official temporary contracts have been removed from the analysis sample.

Rotating through Low-Skill Workers In addition to volatile labor demand, firms may want to create artificial temporary contracts if high worker turnover does not impact the

[10]Industries are reported under the *CNAE* (*Classificação Nacional de Atividade Econômica*).

production process. For example, if firms can simply replace production workers every 3 months, then it may be profit maximizing to continually rotate through workers. In this case, the bunching would not be driven by firms learning about worker quality, and would confound our structural estimates.

Intuitively, this channel seems most prevalent for low-skill occupations (constant replacement of engineers, for example, seems very unrealistic). Therefore, in order to examine how much it contributes towards the bunching we observe, we divide occupations into different skill levels, where skill level is defined by the International Standard Classification of Occupations (ISCO). Low-skill occupations are characterized by the performance of simple and routine physical tasks, and includes occupations such as cleaners and construction laborers. Medium-skill jobs involve performing more complex tasks, such as operating machinery, and includes occupations such as office clerks and skilled craftsman. High-skill jobs require workers to perform complex tasks and in many cases, some form of advanced education. High-skill occupations include technicians, managers and professionals.[11].

Given these definitions, we expect the channel to be stronger for low-skill workers, as the tasks they perform require little training. As can be seen in Figure 3.9, however, bunching occurs across all skill levels. For example, bunching in both the high-skill and low-skill categories is equal to 1.5. While it is true that job termination is in general higher in low-skill occupations, the excess firing at three months is similar across skill levels.

To show this result is consistent, we estimate bunching across all 3-digit occupations. To capture a crude measure of skill level, we use the average wage in the occupation. In Figure 3.9, we plot the estimated bunching against the average log monthly wage. As can be seen in the figure, there is a slight but insignificant negative correlation between average log monthly wage and bunching. This suggests that bunching is not driven by firms rotating through low-skill workers that are relatively easy to replace quickly, but is a feature even

[11]ISCO also breaks down high-skill occupations into medium-high skill and high-skill. For this paper, we have aggregated these two groups and defined them as high-skilled

in high-skill, high-wage occupations, and hence must reflect the early termination of truly permanent employment contracts.

3.3.3 Summary

In this section we have documented a visible spike in the job termination hazard rate at a tenure of 3 months, and have argued that it predominantly reflects the early termination of permanent employment contracts caused by the tenure dependence built into EPL in Brazil.

While our empirical analysis exploits this tenure dependence allows us to cleanly identify the effect of EPL on the hazard rate of job termination via firms' decisions at the micro level, it cannot say how these decisions aggregate up and affect macroeconomic outcomes such as unemployment and aggregate output. In order to address these questions, we now develop a structural framework that formalizes the mapping between EPL, hazard rates, and general equilibrium macroeconomic outcomes.

3.4 Model

In order to study the consequences of EPL for macroeconomic outcomes such as unemployment and aggregate output, we introduce EPL into the general equilibrium model of Moscarini (2005), which merges the theories of job turnover and unemployment in a computationally tractable manner.[12]

We study the steady state equilibrium of this economy. As such, aggregate variables do not have a time subscript, and we use $t \geq 0$ to unambiguously denote tenure for individual level variables.

[12]Given the non-negligible role that the informal sector plays in the Brazilian economy, we will interpret unemployment as both true unemployment as employment in the informal sector. This interpretation will inform our estimation and calibration below.

3.4.1 Production and Belief Dynamics

A final good is produced in continuous time by pairwise firm-worker matches. The match-specific productivity of a match, μ, is ex-ante unknown by both the firm and worker. Upon forming the match, both firm and worker share a common prior on μ that is independent of their histories. The prior puts mass on two productivity levels, μ^L and μ^H, where $\mu^L < \mu^H$. We refer to μ^L as a bad match, and μ^H as a good match. Let $p_0 = \Pr\left(\mu = \mu^H\right)$ be the initial prior that the match is good.

Final good production is linear in match productivity, but is subject to idiosyncratic shocks. In a small interval dt, production of the good X is given by

$$dX_t = \mu dt + \sigma dZ_t$$

where dZ_t is a standard Brownian motion. In other words, output is a noisy indicator of true match productivity, with the noise being scaled by $\sigma > 0$.

The presence of noise creates an inference problem that firms and workers solve by using the information provided by the history of output, denoted by the filtration $\mathcal{F}_t^X$, to update their prior belief in a Bayesian manner,

$$p_t = \Pr\left(\mu = \mu^H | \mathcal{F}_t^X\right)$$

The solution to the continuous time inference problem for p_t has a well known solution in the form of the stochastic differential equation

$$dp_t = p_t\left(1 - p_t\right) s d\bar{Z}_t$$

where $s = \frac{\mu^H - \mu^L}{\sigma}$ is the signal-to-noise ratio and

$$d\bar{Z}_t = \frac{1}{\sigma}\left(dX_t - \left(p_t \mu^H + \left(1 - p_t\right)\mu^L\right) dt\right)$$

is a standard Brownian motion with respect to the filtration generated by X_t, $\mathcal{F}_t^X$.[13] Intuitively, beliefs move faster the more uncertain is the current belief ($p(1-p)$ has a maximum at $p = \frac{1}{2}$). In addition, it is useful to define

$$\Sigma(p) = \frac{1}{2} s^2 p^2 (1-p)^2$$

which is interpreted as half the variance of the change in beliefs over a small change in tenure.

3.4.2 EPL and Job Termination

When the belief, and hence expected match productivity, is low enough, firms and workers will optimally choose to terminate the match endogenously. However, upon termination, firms must pay a cost $\kappa(t)$ that depends on the tenure t of the match in the following way:

$$\kappa(t) = \begin{cases} 0 & \text{if} \quad t < T_1 \\ \kappa & \text{if} \quad t \geq T_1 \end{cases}$$

where $\kappa > 0$ is a constant, and T_1 is a tenure after which the termination cost becomes active. This cost represents the effect of EPL on firms in the model economy. Ultimately, we will estimate κ to match the spike in the hazard rate of job termination so that we can interpret it as the effective real cost of the EPL regulation imposed on firms.

In addition to endogenous terminations, matches are also subject to exogenous termination at rate $\delta > 0$. We assume that exogenous terminations also result in firms incurring the costs of EPL.

[13] For a discussion of this result, see Moscarini (2005) and the references therein.

3.4.3 Value Functions

Given the presence of EPL costs, it is useful to divide the analysis into two stages of tenure: $t \in [0, T_1)$, and $t \geq T_1$. Throughout, let superscripts on functions and variables $i \in \{1, 2\}$ refer to each respective tenure stage, and let subscripts denote derivatives. E.g. J^i_{xx} is the second derivative of the function J^i with respect to the argument x.

Also, define $\bar{\mu}(p) = p\mu^H + (1-p)\mu^L$ as the expected productivity of a match when the belief is p, and let V be the value to a vacant firm of opening a vacancy. In equilibrium, free entry into the vacancy posting market ensures that $V = 0$. Finally, we assume that all agents discount the future at rate $r > 0$.

$t \geq T_1$

We proceed by backward induction, and start in stage two of a job's tenure. Since the cost of EPL is fixed, firm and worker value functions only depend the current belief, which is therefore the natural state variable.

Firms The value to a firm of a match with current belief p, $J^2(p)$ must satisfy the Hamilton-Jacobi-Bellman (HJB) equation

$$rJ^2(p) = \bar{\mu}(p) - w^2(p) + \Sigma(p) J^2_{pp}(p) - \delta\left(J^2(p) + \kappa\right)$$

where $w^2(p)$ is the wage paid to the worker in a match with belief p.

The flow value of the match to the firm consists of 3 components. First, the firm receives the flow profits of production minus the wage she pays the worker. Second, the firm value has a capital gain component due to the change in belief that occurs when the firm updates her belief. Finally, the possibility of exogenous separation creates another source of capital gain in which the firm loses the current value and the cost of EPL (and gains the value of vacancy posting which is equal to zero).

When the belief of the match reaches some threshold $\underline{p}^{2,J}$, the firm will optimally choose to terminate the match. Optimality requires that the firm's choice of threshold and HJB equation jointly satisfy the two boundary conditions

$$J^2\left(\underline{p}^{2,J}\right) = -\kappa$$

$$J_p^2\left(\underline{p}^{2,J}\right) = 0$$

The first condition states that, at the termination threshold $\underline{p}^{2,J}$, the value of the match to the firm equals the value of termination, which is equal to the value of entering the vacancy posting stage (zero in equilibrium) minus the EPL cost that the firm must pay to terminate the match in the second stage of tenure.[14]

The second condition states that, at the termination threshold, the slope of the value function must be zero. Intuitively, this condition ensures that the firm is indifferent between terminating the match as soon as the belief hits the threshold, and waiting for a small amount of time to see what happens.[15]

Workers The value to an employed worker of a match with current belief p satisfies the HJB equation

$$rW^2(p) = w^2(p) + \Sigma(p) W_{pp}^2(p) - \delta\left(W^2(p) - U - \alpha\kappa\right)$$

The value of the match to the worker equals the flow benefit (the wage) plus capital gains stemming from the change in beliefs and the possibility of exogenous separation, where U is the value to the worker of entering unemployment, and is defined shortly. The parameter $\alpha \in [0,1]$ governs the share of EPL that flows to the worker as a transfer payment upon separation.

[14]This condition is often referred to as the "value matching" condition in the optimal stopping literature.

[15]This condition is referred to as the "smooth pasting" condition.

Similarly to the firm, when the belief of the match reaches some threshold $\underline{p}^{2,W}$, the worker will optimally choose to terminate the match (a quit). Optimality requires that the worker's threshold choice and HJB equation jointly satisfy the two boundary conditions

$$W^2\left(\underline{p}^{2,W}\right) = U$$

$$W_p^2\left(\underline{p}^{2,W}\right) = 0$$

The first condition states that value of the match at the threshold is equal to the value to the worker from quitting, which is equal to the value of unemployment (EPL does not apply when the worker chooses to quit the match). The second condition states that, at the termination threshold, the slope of the value function must be zero so that the worker is indifferent between terminating the match as soon as the belief hits the threshold, and waiting for a small amount of time to see what happens.

$t \in [0, T_1)$

For matches of tenure $t \in [0, T_1)$, the value functions depends on both the belief p and tenure t since the time until EPL costs become non-zero changes with tenure.

Firms The value to a firm of a match with current belief p and tenure t must satisfy the HJB equation

$$rJ^1(p,t) = \bar{\mu}(p) - w^1(p,t) + J_t^1(p,t) + \Sigma(p) J_{pp}^1(p,t) - \delta J^1(p,t)$$

where w^1 is the wage of a match with belief p and tenure t. This has the same interpretation as the stage two HJB equation, except that the capital gains now includes the change in value due to the increase in tenure and hence the reduction in time until the firing cost becomes active.

As in stage two, when the belief reaches a tenure threshold, the firm will choose to terminate the match. The key difference is that the thresholds now depend on tenure, and so satisfy the boundary conditions

$$J^1\left(\underline{p}^{1,J}(t),t\right)=0$$

$$J_p^1\left(\underline{p}^{1,J}(t),t\right)=0$$

The first condition ensures that at the threshold $\underline{p}^{1,J}$ at tenure t, the value to the firm of the match equals the value of entering the vacancy posting stage, which is now zero since there is no firing cost to pay. The second condition ensures that the threshold is optimal by a similar logic to the stage two case.

In addition to these conditions, there is another boundary condition in the tenure dimension that pins down the function $J^1(p,T_1)$,

$$J^1(p,T_1)=J^2(p)+\kappa$$

Intuitively, as tenure crosses T_1, the firm understands that she must now subtract κ from her match profit in order to pay the costs of EPL in the future. Therefore, the value of a given match drops by κ as tenure crosses T_1.

Workers The value to an employed worker of a match with current belief p and tenure t must satisfy the HJB equation

$$rW^1(p,t)=w^1(p,t)+W_t^1(p,t)+\Sigma(p)W_{pp}^1(p,t)-\delta\left(W^1(p,t)-U\right)$$

As in the firm case, in stage one, worker belief thresholds now depend on tenure, and satisfy the boundary conditions

$$W^1\left(\underline{p}^{1,W}(t), t\right) = U$$

$$W_p^1\left(\underline{p}^{1,W}(t), t\right) = 0$$

As in the firm case, $W^1(p, T_1)$ is pinned down by the additional boundary condition

$$W^1(p, T_1) = W^2(p)$$

Since workers do not have to pay EPL, the value of a match does not change as tenure crosses T_1.

Finally, the value of unemployment, U, satisfies the HJB equation

$$rU = b + \lambda\left(W^1(p_0, 0) - U\right)$$

The value of being unemployed equals the flow value from unemployment, b, plus the gain from becoming employed times the probability of entering a match, given by the job finding rate λ.[16]

3.4.4 Wage Determination via Nash Bargaining

Once a firm and worker have formed a match, they must decide how to split the surplus generated by the production process. We assume that the surplus is split according to a

[16] In line with our interpretation of unemployment including employment in the informal sector, we interpret b as a home production parameter rather than the flow value of unemployment insurance benefits.

Nash Bargaining protocol that is subject to a minimum wage constraint.[17] To this end, let $\beta \in (0,1)$ denote the bargaining weight of workers.

It is again useful to proceed by backward induction.

$t \geq T_1$

Given the firm and worker value functions, we can define the surplus of a match with current belief p and tenure $t \geq T_1$ as the sum of the values of the match to the firm and worker minus their respective outside options,

$$S^2(p) = J^2(p) + \kappa + W^2(p) - U$$

The Nash Bargaining protocol selects a wage according to

$$w^2_{NB}(p) = \arg\max_w \left(W^2(p) - U\right)^{\beta} \left(J^2(p) + \kappa\right)^{1-\beta}$$

which has the FOC

$$\beta\left(J^2(p) + \kappa\right) = (1-\beta)\left(W^2(p) - U\right)$$

Note that this condition together with the first boundary conditions of the firm and worker HJB equations imply that

$$\underline{p}^{2,J} = \underline{p}^{2,W} = \underline{p}^2$$

so that optimal firm and worker belief thresholds coincide at all tenures in stage two.

Using the expressions for the value functions to solve for the Nash Bargaining wage function yields

$$w^2_{NB}(p) = \beta\left(\bar{\mu}(p) + r\kappa\right) + (1-\beta)\left(rU - \delta\alpha\kappa\right)$$

[17] In Brazil, 2-3% of employed workers earn wages at the minimum wage. We target a similar level when estimating the model with this constraint.

which shows how the Nash Bargaining wage is a bargaining share weighted average of the flow value of the match (production plus the interest earned on the resources set aside for EPL by the firm) and the worker's outside option (the flow value of unemployment minus the share of EPL the worker receives should her match be terminated exogenously).[18] Combining this wage with the minimum wage level w_{min} yields the equilibrium wage function

$$w^2(p) = \max\left\{w^2_{NB}(p), w_{min}\right\}$$

$t \in [0, T_1)$

Similarly to stage two, we can define the surplus of a match with current belief p and tenure t as the value of the match to the firm and worker minus their respective outside options,

$$S^1(p,t) = J^1(p,t) + W^1(p,t) - U$$

where the cost of EPL is now zero. The Nash Bargaining solution is hence given by

$$w^1_{NB}(p,t) = \arg\max_w \left(W^1(p,t) - U\right)^{\beta} \left(J^1(p,t)\right)^{1-\beta}$$

which has the FOC

$$\beta\left(J^1(p,t)\right) = (1-\beta)\left(W^1(p,t) - U\right)$$

Note that this condition together with the first boundary conditions of the firm and worker HJB equations imply that

$$\underline{p}^{1,J}(t) = \underline{p}^{1,W}(t) = \underline{p}^1(t)$$

[18] This expression also shows how EPL affects the splitting of the match surplus between the firm and worker. In particular, when $\beta > \frac{\delta\alpha}{r+\delta\alpha}$, the worker receives a larger share of the match surplus in equilibrium than she would in the absence of EPL.

so that optimal firm and worker belief thresholds coincide at all tenures. Using the expressions for the value functions to solve for the Nash Bargaining wage function yields

$$w_{NB}^{1}(p,t) = \beta\bar{\mu}(p) + (1-\beta)\,rU$$

which is the standard expression for wages determined by Nash Bargaining.

Combining this wage with the minimum wage level w_{min} yields the equilibrium wage function

$$w^{1}(p,t) = \max\left\{w_{NB}^{1}(p,t), w_{min}\right\}$$

3.4.5 Hazard Rate Spike at Tenure $t = T_1$

The key feature of our model is that firms must pay EPL costs in order to terminate a match that has tenure of at least T_1. The following logic formalizes how this firing cost creates a spike in the hazard rate of match termination at T_1, thus providing an explicit micro-foundation for the link between EPL and the hazard rate spike that we documented in the data.

Define the firm-specific thresholds $\underline{p}^{1,J}(T_1)$ and $\bar{p}^{1,J}(T_1)$, that satisfy the conditions

$$J^{1}\left(\underline{p}^{1,J}(T_1), T_1\right) = 0$$

$$J^{2}\left(\bar{p}^{1,J}(T_1)\right) = J^{1}\left(\bar{p}^{1,J}(T_1), T_1\right) - \kappa = 0$$

where $\bar{p}^{1,J}(T_1) > \underline{p}^{1,J}(T_1)$ since firm value functions are increasing in the current belief. Given these threshold definitions, any match with belief $p_{T_1} \in \left[\underline{p}^{1,J}(T_1), \bar{p}^{1,J}(T_1)\right]$ at tenure $t = T_1$ will be immediately terminated since the value of such a match will instantaneously drop to $J^{2}(p, T_1) < 0$. This extra termination creates a spike in the hazard rate at T_1.

Intuitively, when tenure reaches T_1, there is an interval of beliefs such that a match with a belief in that interval would not be terminated were it not for the presence of a firing cost.

While such matches are reasonably productive, they are not productive enough to warrant the firm continuing the match and paying the firing cost if productivity deteriorates later. All matches with beliefs in this interval are therefore terminated at tenure T_1, thus creating a spike in the hazard rate at tenure T_1.

3.4.6 Ergodic Distribution of Beliefs

We again divide to derivation into 2 stages, according to match tenure, and define 2 belief distribution functions f^1, and f^2, where the function $f^i(p,t)$ is the distribution over the unit interval of beliefs for a cohort of matches that began production at the same moment in calendar time, and have reached tenure t.[19]

$t \leq T_1$

We can characterize the evolution of the distribution of beliefs using the Kolmogorov Forward Equation (KFE), which states that f^1 evolves according to the equation

$$\frac{\partial}{\partial t} f^1(p,t) = \frac{\partial^2}{\partial p^2}\left[\Sigma(p) f^1(p,t)\right] - \delta f^1(p,t)$$

The KFE states that the change in density at a belief p for a small change in tenure is the sum of two components. First, beliefs move around in the distribution according the evolution equation. The change in density caused by these movements is captured by the first term on the right-hand side. Second, at any belief, a fraction δ of matches end exogenously, causing a negative change to the density captured by the second term.

The initial distribution satisfies the conditions

$$\int_{\underline{p}^1(0)}^{1} f^1(p,0)\, dp = \lambda u$$

[19]The f functions are not strictly distributions since we do not require them to sum to 1. Instead, their total mass will be total employment, and hence 1 minus total unemployment since we have normalized the mass of workers to unity.

$$f^1(p,0) = \Delta(p - p_0)$$

where we recall that λ is the finding rate and u is the mass of unemployed workers, and we define Δ as the Dirac delta function that places all mass at the initial prior p_0.

In addition, at each tenure $t < T_1$, f^1 satisfies the boundary condition

$$f^1\left(\underline{p}^1(t), t\right) = 0$$

which ensures that there is always zero mass at the termination threshold at tenure t, since when a match belief reaches the threshold, it is immediately terminated.

$t \geq T_1$

The KFE again states that f^2 evolves according to

$$\frac{\partial}{\partial t} f^2(p,t) = \frac{\partial^2}{\partial p^2}\left[\Sigma(p) f^2(p,t)\right] - \delta f^2(p,t)$$

At tenure $t = T_1$, we initialize f^2 with the condition

$$f^2(p, T_1) = f^1(p, T_1)\, \mathbf{1}\left\{p \in \left[\bar{p}^{1,J}(T_1), 1\right]\right\}$$

where $\mathbf{1}$ is the indicator function. This condition ensures that only matches with belief above the threshold $\bar{p}^{1,J}$ survive the termination process at tenure T_1.

Similarly to stage one, at each tenure $t \geq T_1$, f^2 satisfies the boundary condition

$$f^2\left(\underline{p}^2, t\right) = 0$$

which has the same interpretation as in stage one.

Ergodic Distribution of Beliefs

Combining f^1 and f^2 yields the ergodic distribution of beliefs,

$$g(p) = \int_0^{T_1} f^1(p,t)\,\mathbf{1}\left\{p \geq \underline{p}^1(t)\right\} dt + \int_{T_1}^{\infty} f^2(p,t)\,\mathbf{1}\left\{p \geq \underline{p}^2\right\} dt$$

Normalizing the mass of workers to 1, the mass of unemployed workers is given by

$$u = 1 - \int_0^1 g(p)\,dp$$

3.4.7 Vacancy Posting

A vacant firm has a value function V that satisfies

$$(r+q)\,V = qJ^1(p_0, 0) - c$$

where q is the job filling rate, and c is the per-period cost of maintaining a vacancy expressed in units of the final good. We assume that there is a large mass of firms in the economy, so that free entry into the market for vacancies guarantees that $V = 0$ in equilibrium.

3.4.8 The Matching Function

Let v be the mass of open vacancies. Together with the mass of unemployed workers u, v vacancies generate m new job matches according to the matching function

$$m = zu^{\eta}v^{1-\eta}$$

where z is matching efficiency and $\eta \in (0,1)$ is the elasticity of matches to unemployment. This implies the job finding rate $\lambda = z\theta^{1-\eta}$, and job filling rate $q = z\theta^{-\eta}$, where $\theta = \frac{v}{u}$ is labor market tightness.

3.4.9 Equilibrium

A steady state equilibrium is a set of scalars

$$\left\{\lambda, q, \theta, u, v, \underline{p}^2\right\}$$

and functions

$$\left\{J^1, J^2, W^1, W^2, U, \underline{p}^1, w^1, w^2, f^1, f^2\right\}$$

such that:

1. $\left\{J^1, J^2, W^1, W^2, U, \underline{p}^1, \underline{p}^2\right\}$ satisfy their HJB equations and boundary conditions.
2. $\{w^1, w^2\}$ satisfy the Nash Bargaining condition.
3. $\{f^1, f^2\}$ satisfy their KFEs, and boundary and initial conditions.
4. $\{\lambda, q, \theta\}$ satisfy their definitions.
5. v ensures free entry in the vacancy posting market.
6. u is consistent with total unemployment implied by $\{f^1, f^2\}$.

3.4.10 Aggregate Output and Welfare

Since production is linear in productivity beliefs, we can define formal sector output as

$$Y = \mu^H \int_0^1 pg\left(p\right) dp + \mu^L \int_0^1 \left(1-p\right) g\left(p\right) dp - cv$$

which has the simple interpretation of the number of good matches times the productivity of a good match plus the number of bad matches times the productivity of a bad match minus the flow costs of vacancy creation.

Exploiting the linearity of preferences, and recalling that $u = 1 - \int_0^1 g\left(p\right) dp$, we can define steady state welfare (equal to aggregate output across both the formal and informal

sectors) as

$$\mathcal{W} = b\left(1 - \int_0^1 g\left(p\right) dp\right) + \mu^H \int_0^1 pg\left(p\right) dp + \mu^L \int_0^1 \left(1-p\right) g\left(p\right) dp - cv$$

which is the flow value of unemployment times the mass of unemployed workers plus formal sector production.

Importantly, these expressions also show that EPL affects production and welfare through 3 channels: the level of unemployment as captured by total mass of the function g, the level of labor productivity as captured by the shape of the function g, and the total costs of posting vacancies.

3.4.11 Efficiency

In our economy, there are two decision margins: match termination by firms and workers, and vacancy creation by firms. In the absence of EPL (when $\kappa = 0$), match termination decisions are efficient due to the Nash Bargaining protocol, which ensures that a match is terminated when its surplus is zero.[20] Formally, Nash Bargaining ensures that firm and worker values are linear functions of the match surplus, so that the boundary conditions for match termination occur when the match surplus is zero.

Vacancy creation by firms is efficient when the Hosios (1990) condition, $\beta = \eta$, is satisfied. As is well known, vacancy creation by firms imposes 2 externalities on the economy. First, an additional vacancy lowers the job filling rate of all other vacant firms, thus creating a negative externality. Second, an additional vacancy increases the job finding rate for all unemployed workers, thus creating a positive externality. When $\beta = \eta$, the externalities exactly cancel each other out, resulting in an efficient equilibrium.

The imposition of EPL affects both decision margins in the economy. First, EPL distorts the match termination decision of firms, and creates a spike in the hazard rate of match ter-

[20] In order to focus on the efficiency implications of EPL, we assume that the minimum wage is not binding. We show that our results are robust to including the minimum wage in section 3.8.

mination at 3 months' tenure, as we have shown both empirically and theoretically. Second, EPL alters the incentive for firms to create vacancies, since it affects their valuation of new matches, $J^1(p_0, 0)$.

If the Hosios condition holds, then the imposition of EPL must be inefficient. However, if the Hosios condition does not hold, then the overall effect of EPL on efficiency and welfare is ambiguous. For example, EPL may increase overall labor productivity, but could also increase unemployment while decreasing vacancy creation. In order to quantify these competing channels, and to measure whether the Hosios condition holds in the data we now turn to our quantitative exercise.

3.5 Model Estimation Strategy

The model contains 15 parameters: $T_1, \mu^L, \mu^H, r, \eta, \alpha, \delta, c, z, b, \beta, w_{min}, \sigma, p_0, \kappa$. Since our key data variation is the hazard rate schedule, we use these data in conjunction with other aggregate moments to identify and estimate z, b, σ, p_0, β, and κ. We normalize or calibrate the remaining parameters.

3.5.1 Calibrated Parameters

Table 2 summarizes the parameters that we calibrate. We set $T_1 = 3$ months to reflect the tenure at which EPL becomes active in Brazil. Since μ^L and μ^H simply set the location and scale of production in the economy, we normalize them to $\mu^L = 0$ and $\mu^H = 1$ respectively. We set the annual discount rate to $r = 7.5\%$, which is in line with Brazilian interest rates.

The weight on unemployment in the matching function, η, is set to 0.25, in line with the estimates in Hoek (2007) who estimates a Cobb-Douglas matching function for Brazil. As a baseline, we set $\alpha = 0$, and assume that the minimum wage does not bind by setting

$w_{min} = 0$. We confirm that our results are robust to other values for η, α, and w_{min} in section 3.8.[21]

The exogenous match termination rate δ determines the level to which the hazard rate converges as tenure increases and the learning process becomes less prevalent. Therefore, we set $\delta = 0.013$ to match the long run level of the empirical hazard rate (beyond 24 months' tenure).

Finally, the flow cost of posting a vacancy, c, is pinned down endogenously by the free entry condition for vacancy posting, $c = z\theta^{-\eta} J^1(p_0, 0)$.

3.5.2 Estimation Procedure

We estimate the remaining vector of parameters ($\Xi = (z, b, \beta, \sigma, p_0, \kappa)'$) by the method of simulated moments. Formally, let M be a vector of six empirical moments which we will define below and let $M(\Xi)$ be the vector of corresponding moments generated by the model. We choose Ξ^* to solve

$$\Xi^* = \arg\min_{\Xi} \left(M(\Xi) - M\right) W^{-1} \left(M(\Xi) - M\right)'$$

where W is a weighting matrix. We set W to a diagonal matrix with diagonal equal to the square of each empirical moment.

3.5.3 Identification

Following Shimer (2005), we normalize market tightness $\theta = 1$. Given this, the per-period job finding rate is equal to the matching efficiency parameter, $\lambda = z$, which we estimate to target a steady state unemployment rate of approximately 30%. This figure is consistent

[21] While the minimum wage is certainly binding in Brazil, as shown in Engbom and Moser (2018), we show in the appendix that the bindingness of the minimum wage does not change our qualitative results concerning the impact of EPL on macroeconomic outcomes.

with recent Brazilian data (Engbom and Moser, 2018), where we interpret unemployment as including both true unemployment, and employment in the informal sector.

In order to estimate the flow value of unemployment b, we target an income share of the informal sector equal to 20% of total income (**?**). Intuitively, b pins down the productivity of the informal sector, and hence its share of aggregate income.

We choose β so that the expected cost of vacancy creation, c/q is equal to 4 months' average wages, similar to the value of vacancy costs in Hornstein et al. (2005). Intuitively, a higher value for β increases the wages of employed workers, and hence lowers the expected profits from a new job, $J^1(p_0, 0)$. Since $J^1(p_0, 0) = c/q$ in equilibrium, β directly affects the expected cost of posting a new vacancy.

In order to estimate σ, p_0 and κ, we aim to match the empirical job termination hazard rate. p_0 and σ determine the initial level and slope of the hazard rate pattern. The larger is the initial prior p_0, the less likely it is that the belief falls low enough to warrant endogenous match termination, and hence the lower are the initial hazard rates of match termination. Similarly, the larger is σ, the slower firms and workers learn about match productivity, and the lower is the initial slope in the hazard rate of match termination. Therefore, we use the hazard rates at tenures of 0.5 and 1 month to estimate p_0 and σ.[22]

Finally, the identification of κ follows similar arguments to the empirical identification as described in Section 3.4.5. The fact that EPL only becomes active at 3 months of tenure creates a strong incentive for firms to terminate matches just before this tenure is reached. This behavior creates a spike in the hazard rate schedule at 3 months' tenure. Hence, we use the spike in the empirical hazard, captured by the hazard rates at tenures of 2.5, 3, and 3.5 months to infer the value of κ.

[22]The particular moments of the hazard rate to choose for estimation is a bit ambiguous in general. We have chosen two parameters so that the model is just identified, but utilizing more moments of the job termination hazard rate leads to very similar estimates. In particular, we have estimated alternative over-identified specifications in which we use the entire job termination hazard rate to estimate the parameters of the model.

To illustrate the identification of the model's parameters, we first show that the estimated parameters, denoted Ξ^*, are indeed a global minimum. To do so we proceed parameter by parameter, following Bilal et al. (2019). To proceed, we move each parameter, Ξ_i around the estimated value Ξ_i^*, keeping fixed the other estimated values at Ξ_{-i}^*. If the estimated parameter vector is indeed a global minimum of the estimation objection, then the objective function should increase as we move away from the estimated parameter value. Figure 3.4 plots the the minimized value of the estimation objective as we vary each parameter. As can be seen in the figure, the U-shaped curves offer evidence that the objective is globally minimized at our estimated parameter vector.

3.5.4 Model Fit

Hazard Rates Table 3 presents the parameter estimates along with the targeted moments. While each parameter is associated with a given moment, in practice, all moments are used simultaneously to estimate the vector of parameters. To understand how these parameters map to the data, it is useful to plot the model-implied job termination hazard rate. Recall from the model that the hazard rate is driven by three separate parameters: the initial productivity belief, the speed of learning, and the cost of EPL. As can be seen in Figure 3.5, this sparse parameterization does a good job of capturing key features of the empirical job termination hazard rate. Intuitively, the firing cost κ successfully replicates the spike in the hazard at three months. The relatively high initial belief of productivity indicates that the hazard rate of job termination starts at a relatively low level. Lastly, the speed of learning will dictate the overall shape of the curve. The higher the speed of learning, the quicker the hazard rate will flatten out, indicating that at long tenures job destruction is driven by exogenous reasons (δ in the model) and not by endogenous learning about match quality.[23]

[23]Table 3 also presents estimates for the bargaining power of workers ($\beta = 0.56$) and the flow value of unemployment ($b = 0.39$). Although often applied in a different settings, these numbers are quite similar to numbers often used in the search literature (See Shimer (2005) for example).

3.6 Results

3.6.1 The Equilibrium Effects of EPL

We now turn to our main counterfactual exercise, and study how EPL impacts macroeconomic outcomes.

Counterfactual Hazard Rates Figure 3.6 plots the hazard rate in the data, in the estimated model, and in the counterfactual model with the EPL removed ($\kappa = 0$). In the absence of firing costs, the hazard rate function resembles the standard hump shape (Farber, 1994). In particular, the hazard rate without firing costs does not spike at three months, and is uniformly higher than the hazard with firing costs at tenures greater than three months, but lower than the hazard with firing costs at tenures less than three months. In other words, the presence of firing costs causes firms to terminate matches earlier than they otherwise would, just as the theory predicted and as we found in the data.

Counterfactual Macroeconomic Outcomes Column 1 of Table 4 summarizes the key macroeconomic implications of EPL in our estimated model. Intuitively, imposing EPL causes firms to terminate more workers, especially as tenure approaches 3 months. As a result, the unemployment rate increases by 8.3 percentage points, and workers are reallocated from the formal sector to the relatively less productive informal sector, which includes true unemployment. This reallocation in turn causes a decrease in formal sector output of 13.6%, and a decline in welfare (aggregate output) of 7.6%. Therefore, the imposition of EPL has a significant negative effect on the macroeconomy, and causes a relatively large shift from formal sector employment to informal sector employment.

Decomposing the Fall in Output We can shed light on the driving force behind the decline in formal sector output by examining a simple decomposition that breaks the total output decline into components that reflect each of the decision margins in the economy.

Letting asterisks * denote outcomes in the counterfactual economy without EPL, we can write the change in aggregate output as

$$Y - Y^* = \int_0^1 p\left(g\left(p\right) - g^*\left(p\right)\right) dp - c\left(v - v^*\right)$$

which can be decomposed into 3 parts,

$$Y - Y^* = \underbrace{\int_0^1 p\left(g\left(p\right) - \frac{1-u^*}{1-u} g\left(p\right)\right) dp}_{\text{unemployment}} + \underbrace{\int_0^1 p\left(\frac{1-u^*}{1-u} g\left(p\right) - g^*\left(p\right)\right) dp}_{\text{productivity}} - \underbrace{c\left(v - v^*\right)}_{\text{vacancies}}$$

The unemployment channel measures how much of the change in output is driven by the change in unemployment, holding the distribution over productivities fixed at g. Naturally, if EPL causes higher unemployment due to increased match termination at 3 months' tenure, then $u > u^*$ and aggregate output is lower, ceteris paribus.

The productivity channel measures how much of the change in output is driven by the change in the distribution over beliefs, and hence productivities. Intuitively, we expect that EPL causes firms to terminate matches with lower expected productivity at 3 months' tenure, leading to an increase in average productivity for matches with tenure greater than 3 months. This increase in productivity is a force for EPL to increase output.

Finally, the vacancies channel measures how much of the change in output is due to a change in the total costs of posting vacancies in the economy. Firms' incentives to create vacancies hinges on the job-filling rate, q, which is decreasing in the overall level of labor market tightness, $\theta = \frac{v}{u}$. Therefore, if EPL causes higher unemployment, the larger mass of unemployed workers increases the probability that a vacant firm finds a match. This increase in the job-filling rate incentivizes firms to post more vacancies, so that EPL causes an increase in total vacancy posting costs, which is a force for lower aggregate output.

Applying this decomposition to our estimated model, we find that the overall fall in formal sector output caused by EPL is driven mainly by the increase in unemployment,

which accounts for a 12.1% drop. The increase in vacancy posting costs account for an additional 2.6% drop. While the imposition of EPL does lead to an increase in overall labor productivity, its offsetting effect is very small, and only causes a 1.1% increase in formal sector output.[24]

The fact that EPL causes only a small increase in labor productivity can be seen by inspecting the estimated and counterfactual hazard rate plots in figure 3.6. In the absence of EPL, matches that would have been terminated at 3 months' tenure are instead terminated shortly after. In other words, the presence of EPL does not distort the optimal termination decision of firms in a quantitatively significant manner. The fact that firms and workers are able to quickly infer the productivity of a given match is driven by our estimates $\hat{p}_0 = 0.87$ and $\hat{\sigma} = 1.95$ which imply that matches start with a relatively strong prior, and that idiosyncratic shocks play a relatively minor role in the production process.

3.6.2 The Role of Tenure Dependence

In this section, we ask how much of the total effect of EPL is driven purely by its tenure-dependent nature. An obvious rationalization for designing the system with a probationary period is that it allows firms to learn about worker quality before committing to a long-term relationship. Therefore, the hope is that the tenure-dependence of EPL allows for more vacancies that end up translating into more jobs and more output.

To study this question, we consider the effects of a EPL that applies equally to all tenures, and therefore does not have a probationary trial period. In Column 2 of Table 4 we report the changes in macroeconomics indicators that results from such an EPL design relative to an economy with no EPL. Imposing tenure-*in*dependent EPL causes formal sector output to decline by 5.7%, and welfare to decline by 2.7%, both of which are much smaller than the effects of tenure-dependent EPL, which lowered output. Similarly, the unemployment rate

[24]This result is robust to deriving the unemployment and productivity channels using the counterfactual distribution g^* instead of g.

increases by 4.6 percentage points, which is around half the effect of tenure-dependent EPL. As a result, the decline in formal sector output is driven by the increase in unemployment: there is no change in labor productivity, and total vacancy posting costs actually decline since firms realize they must pay the EPL cost immediately if a match turns out to be of low quality.

Summarizing, in our setting eliminating probationary periods would actually improve macroeconomic outcomes. This implies that the potential increases in productivity stemming from the probationary period is overstated in practice. In practice, the loss in output due to increased vacancy costs swamps the very small increases in productivity due to a probationary period.

3.7 Conclusion

We make two contributions to the literature that studies the effects of EPL on macroeconomic outcomes. First, we exploit tenure-dependence in the design of Brazilian EPL to obtain clean identification of its effect on firms' decisions to terminate jobs. Second, we estimate a structural model of a frictional labor market augmented to include tenure-dependent EPL in order to compute the counterfactual implications of removing EPL on unemployment and aggregate output.

Our results imply that EPL has substantial negative effects on macroeconomic outcomes. These negative effects stem from the fact that EPL causes a significant shift of workers from the formal sector to the informal sector or unemployment, leading to aggregated declines in output and welfare. Furthermore, we find that a substantial portion of the lost output due to EPL is due to the tenure-dependence, which is a common feature of EPL across many countries. It would be interesting to apply our methods we have developed here to other

settings, in order to further improve our understanding of the macroeconomic impacts of EPL.[25]

[25] In contemporaneous work, Cahuc et al. (2019) use similar methods to explore the consequences of French labor market regulations on job finding and separation rates.

Table 1: Descriptive Statistics for Estimation Sample, 2002-2007

	All Jobs	Short Duration Jobs
Panel A: Demographics	(1)	(2)
Age	31.535	30.405
High School Graduate	0.336	0.305
College Graduate	0.072	0.038
Male	0.658	0.693
Panel B: Job Characteristics		
Monthly Wage	819.212	670.909
Tenure	13.332	1.681
Hours	43.108	43.444
Panel C: Firm Characteristics		
Manufacturing	0.199	0.185
Agriculture	0.093	0.140
Public Administration	0.074	0.022
Health and Education	0.039	0.023
All Other Sectors	0.595	0.631
Unique Workers	29,438,306	13,312,346
Number of Jobs	92,023,307	24,427,409

Note: Column 1 reports descriptive statistics for jobs held by workers between age 18-65 from the years 2002-2007, excluding workers on temporary contracts. Column 2 reports descriptive statistics for jobs which last less than three months. Tenure is measured in months. Wages are denominated in Brazilian Real. The data is drawn from the *Relação Anual de Informações Sociais* (RAIS)

Table 2: Parameter Calibration

Parameter		Value	Target
T_1	Timing of EPL	3 months	Timing of EPL
μ^L	Low Match Productivity	0	Normalization
μ^H	High Match Productivity	1	Normalization
r	Discount rate	7.5%	Annual real interest rate
δ	Exogenous Separation Rate	0.013	Long run hazard rate
η	Elasticity of Matches to unemployment	0.25	Hoek (2007)
c	Vacancy Posting Cost	0.19	Free-entry condition
α	EPL Transfer Component	0	Baseline Calibration

Note: This table reports the calibrated parameters model parameters along with the moments used to calibrate the parameters. $T_1 = 3$ reflects that in Brazil there is a three-month probationary period at which EPL costs are set to zero. r is set to match interest rates in Brazil, δ is equal to the long-term hazard rate in the data, η is the elasticity of matches to unemployment and c is the flow cost of a vacancy which is determined endogenously by the free entry condition for vacancy posting. For the baseline calibration, we set the amount of EPL costs that get transferred to workers as zero, but show robustness to this decision in Appendix Table 5.

Table 3: Estimated Parameters and Targeted Moments

Parameter		Value	Moment	Model	Data
z	Matching Efficiency	0.03	Unemployment Rate	0.34	0.30
b	Flow Value of U	0.39	Income share of Informal Sector	0.19	0.20
β	Bargaining Share	0.56	Cost of a Vacancy / Mean wage	3.8	4
p_0	Initial Belief	0.87	0.5 month Hazard Rate	0.01	0.009
σ	SD of Output Shocks	1.95	1 month Hazard Rate	0.02	0.02
κ	EPL Cost	1.23	3 month Hazard Rate Spike	0.05	0.04

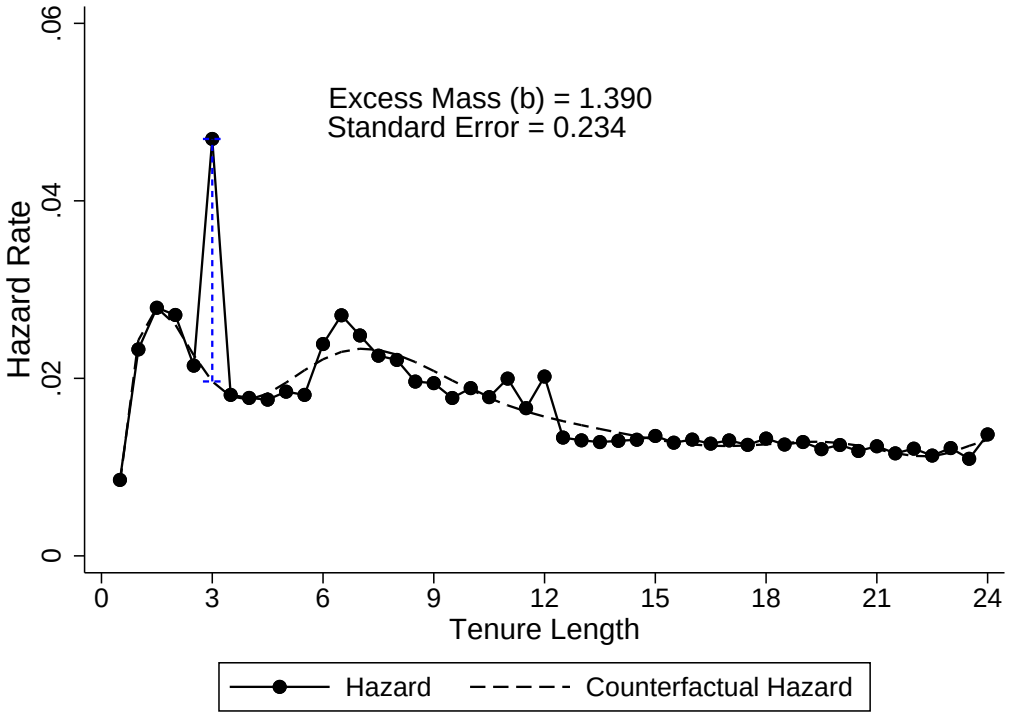
Note: This table reports the estimated model parameters. Parameters are estimated jointly using simulated methods of moments. The parameter, κ, is estimated using the hazard rates at 2.5, 3, and 3.5 months, which together are informative of the hazard rate spike at 3 months' tenure.

Table 4: The Effect of Different EPL Designs on Macroeconomic Outcomes Relative to No EPL

	Tenure-Dep. EPL	Constant EPL
	(1)	(2)
Panel A: Macroeconomic Outcomes		
$\Delta\mathcal{W}$ (%)	-7.6	-2.7
Δu (p.p.)	8.3	4.6
ΔY (%)	-13.6	-5.7
Panel B: Formal Sector Output Decomposition		
Productivity (%)	1.1	-0.2
Vacancies (%)	-2.6	1.1
Unemployment (%)	-12.1	-6.6

Note: This table reports the change in macroeconomic outcomes for different changes to employment protection legislation (EPL). The baseline is an economy without any EPL (i.e. $\kappa = 0$). The first column reports changes when we impose tenure-dependent EPL (i.e. setting κ to its estimated value for all tenures greater than 3 months). This is the design of EPL currently imposed in Brazil. In the second column we show the changes when we impose tenure-*in*dependent EPL (i.e. setting κ to its estimated value for all tenures, implying $T_1 = 0$).

Figure 3.1: Hazard Rates around Firing Cost Notch



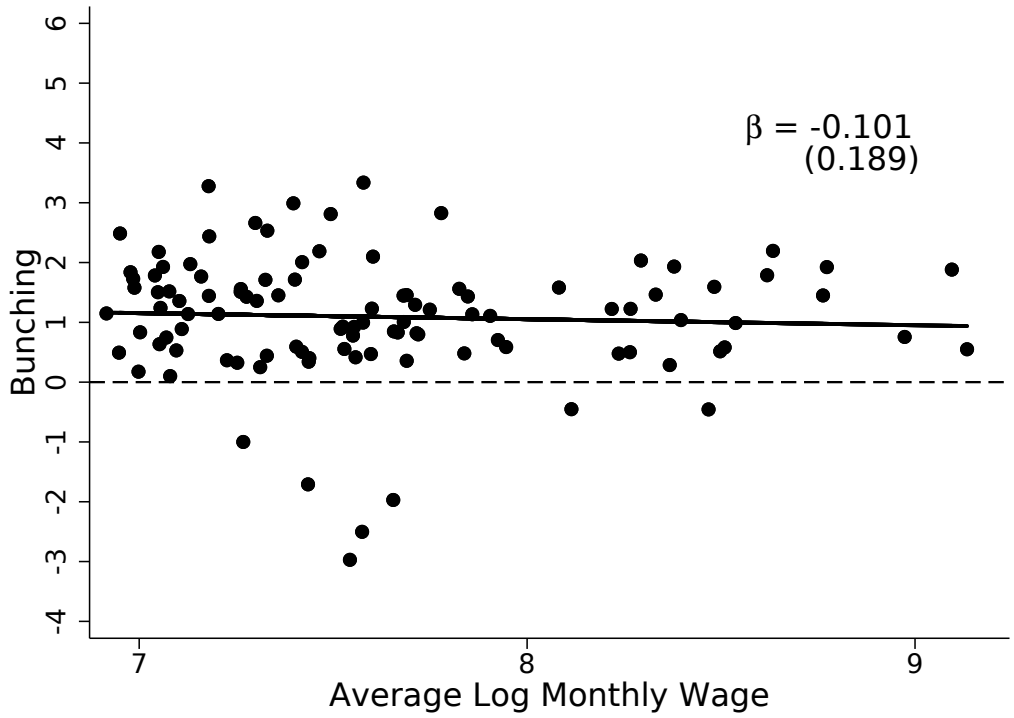
Note: This figure plots the layoff hazard rate. Tenure duration is binned into 15 day intervals. The dashed line is a tenth-degree polynomial fitted to the empirical hazard rate, excluding points 15 days away from the notch, as in Equation (3.1). The vertical dotted line displays the excess mass B, while the normalized excess mass b and standard error is reported in the figure. The standard error is computed using a residual bootstrap procedure.

Figure 3.2: Bunching and Demand Volatility



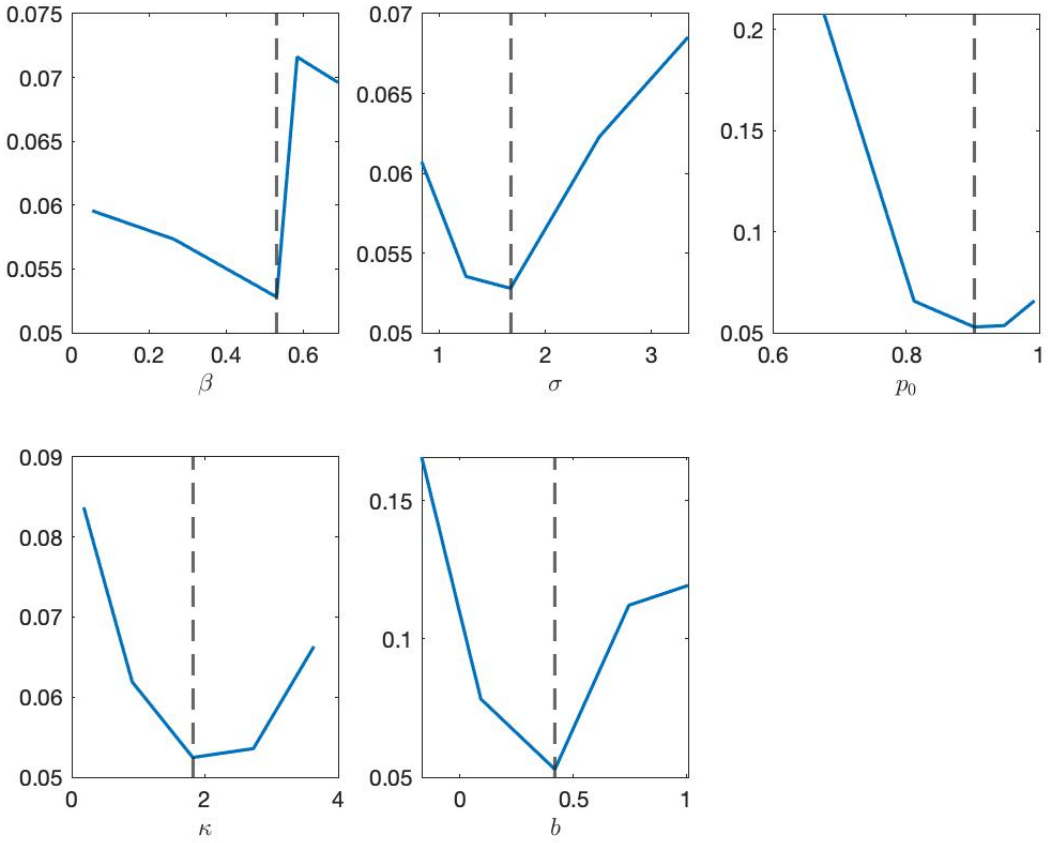
Note: This figure plots the bunching estimate by sector (defined by three-digit CNAE classification) and correlates the bunching to volatility in employment. The volatility is calculated as the standard deviation of month-to-month employment changes over the course of a year.

Figure 3.3: Bunching is Consistent Across Occupations and Uncorrelated with Wages



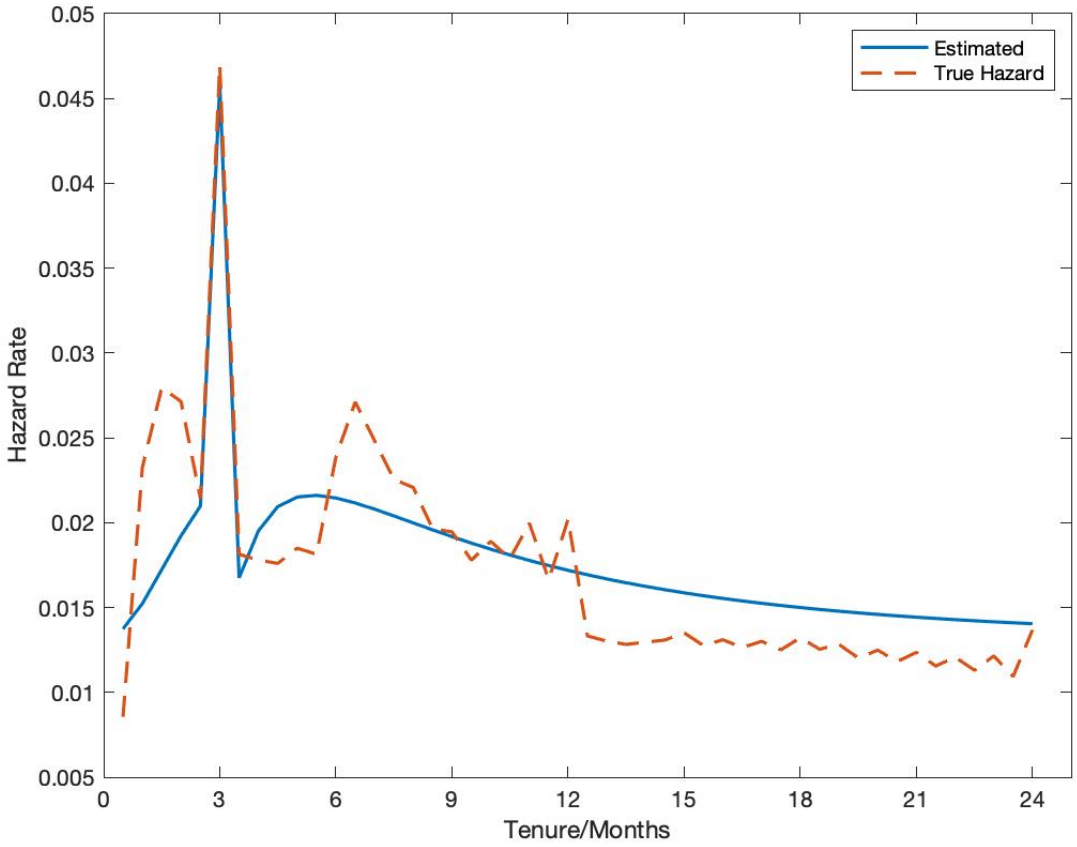
Note: This figure plots the bunching estimate by occupation (defined by three-digit ISCO identifier) and correlates the bunching to average wages. The coefficient displayed is the estimate of the coefficient of a regression of the bunching estimate on the variable on the x-axis.

Figure 3.4: Parameter Estimates Represent a Global Minimum



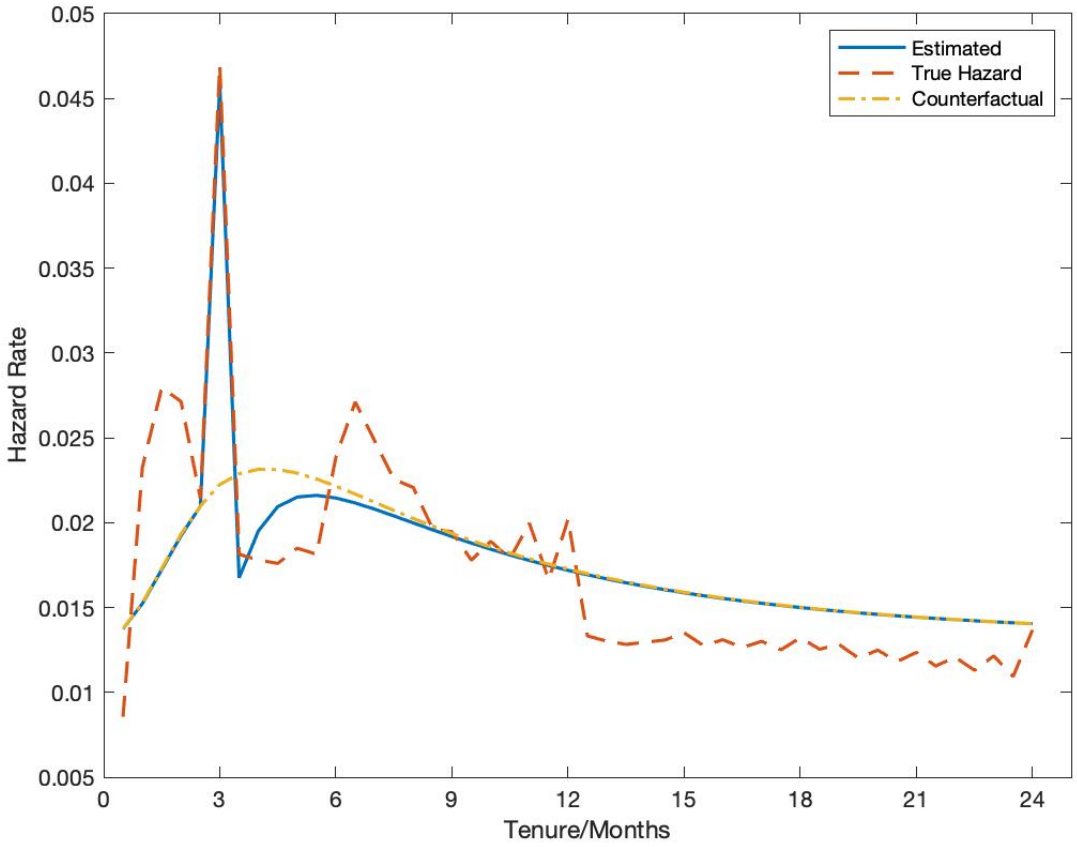
Note: This figure plots the minimized value of the estimation objective as we vary each parameter, holding all other parameters fixed at the estimated values. The black dashed line indicates the estimated parameter value.

Figure 3.5: Estimated and Empirical Hazard Rates



Note: This figure plots the empirical layoff hazard rate (dashed) as well as the hazard rate from the estimated model (solid). Tenure duration is binned into 15 day intervals to estimate the empirical layoff hazard.

Figure 3.6: Estimated, Empirical, and Counterfactual Hazard Rates



Note: This figure plots the empirical layoff hazard rate (dashed), the estimated hazard rate generated by the model (solid), and a counterfactual hazard rate (dotted) when $\kappa = 0$. Tenure duration is binned into 15 day intervals to estimate the empirical layoff hazard.

3.8 Robustness

In this section we show that our results are robust to changes in the value of calibrated parameters: the elasticity of matches to unemployment η, the share of EPL that workers receive upon involuntary termination α, and the minimum wage w_{min}.

Relative to the baseline economy, we consider $\eta = 0.75$, $\alpha = 0.5$, and $w_{min} = 0.7$. This minimum wage ensures that 3% of employed workers earn the minimum wage in equilibrium. This fraction is consistent with our data, and with the results in Engbom and Moser (2018). For each parameter variation, we compute the results of our counterfactual exercise in which we set the EPL parameter to zero.

The results are contained in Table 5. For ease of comparison, column 1 reports the effects of EPL in the baseline economy. Columns 2 - 4 then report the effects when we vary η, α, and w_{min} in turn.

In all cases, the effects of EPL are negligibly different from the baseline scenario. Therefore, we can safely conclude that our results are not driven by features of our calibration.

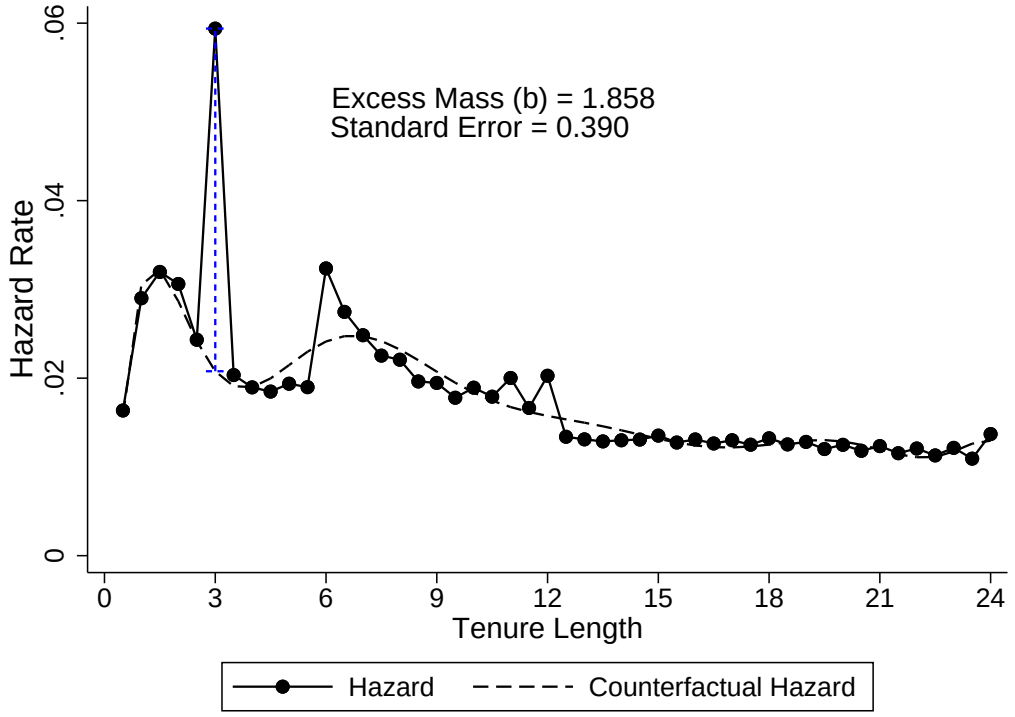
3.9 Figures and Tables

Table 5: Robustness Exercises

	Baseline	$\eta = 0.75$	$\alpha = 1$	Min. Wage
	(1)	(2)	(3)	(4)
$\Delta\mathcal{W}$ (%)	−7.6	−7.7	−6.9	−7.6
Δu (p.p.)	8.3	8.5	7.4	7.8
ΔY (%)	−13.6	−13.8	−12.5	−13.4

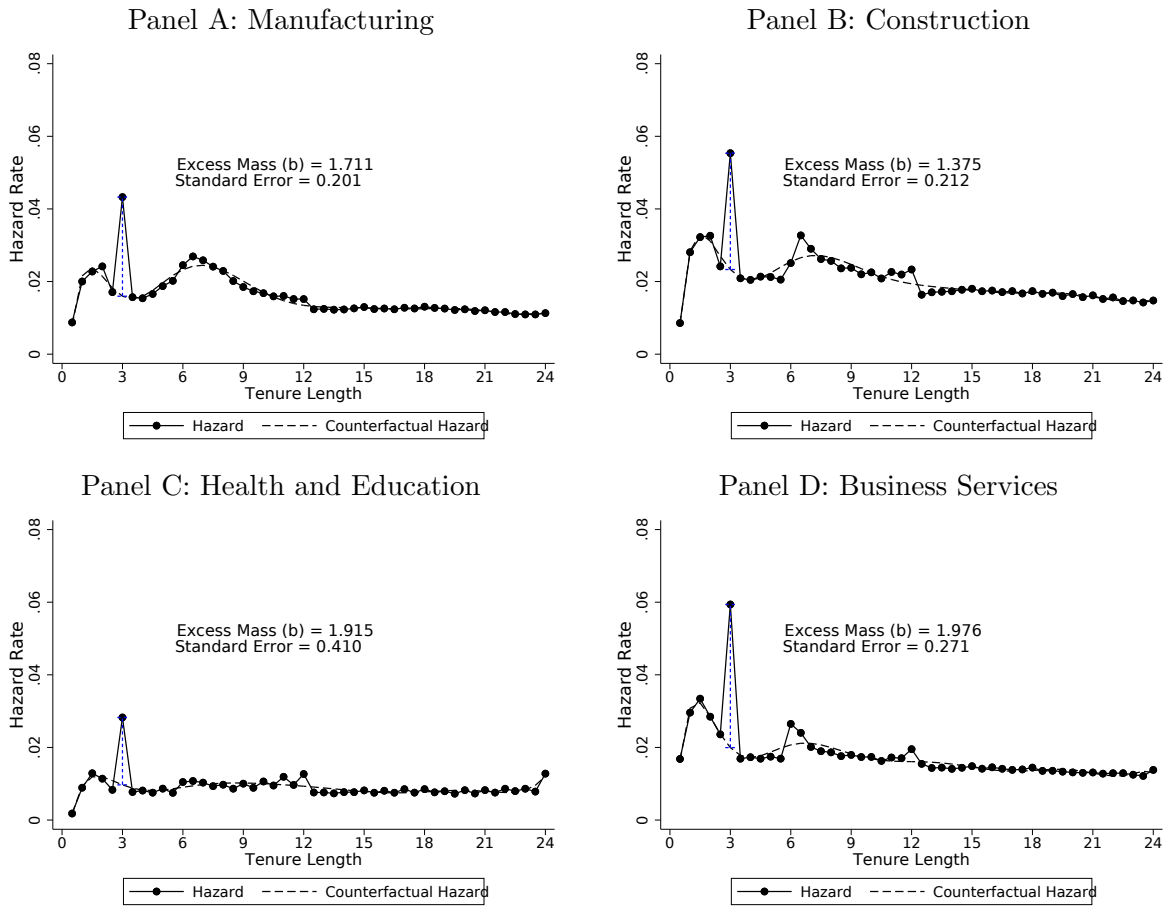
Note: This table reports the effect of EPL on macroeconomic outcomes under different parameter specifications. Column (1) contains the baseline results. In column (2), we set the elasticity of matches with respect to unemployment, η, equal to 0.75, much higher than the baseline estimate that uses $\eta = 0.75$. In column (3), we set $\alpha = 1$, so that upon separation, the cost of EPL is directly transferred to the newly unemployed worker. In column (4), we allow for a minimum wage, which we estimate so that 3% of the employed workforce earn the minimum wage in equilibrium, in line with our data, and Engbom and Moser (2018).

Figure 3.7: Bunching Including Temporary Contracts



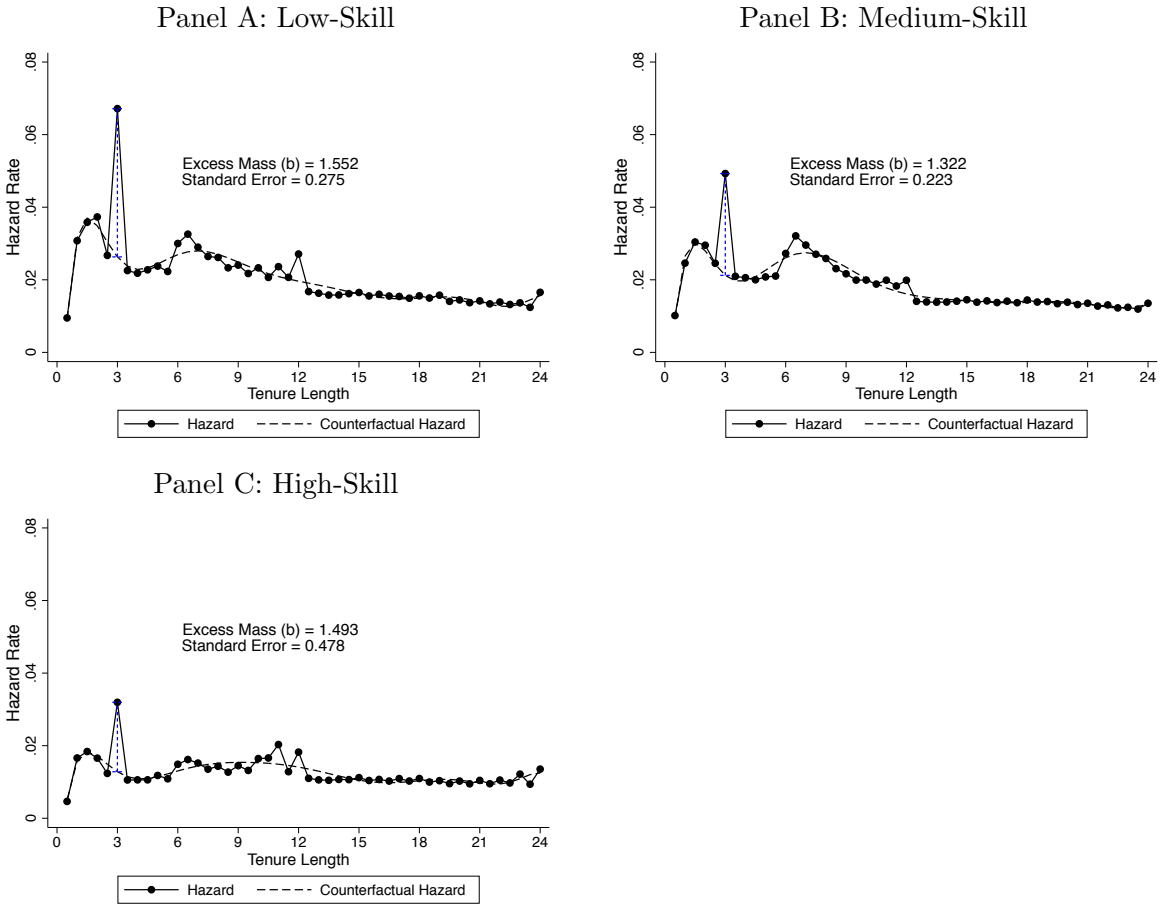
Note: This figure plots the job termination hazard rate which includes temporary contracts.

Figure 3.8: Heterogeneity in Bunching by Industry



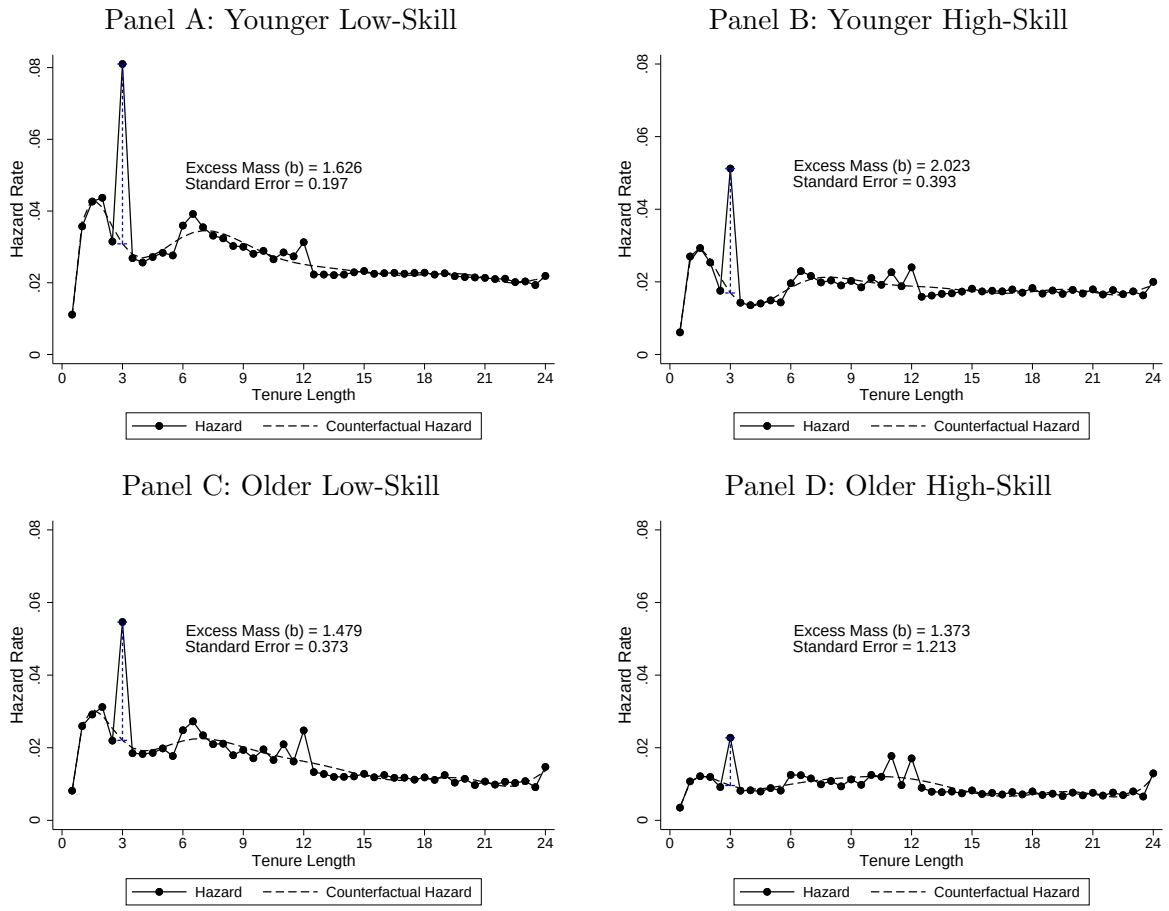
Note: This figure plots the layoff hazard rate by different industries. Tenure duration is binned into 15 day intervals. The dashed line is a tenth-degree polynomial fitted to the empirical hazard rate, excluding points 15 days away from the notch, as in Equation (3.1). The vertical dotted line displays the excess mass B, while the normalized excess mass b and standard error is reported in the figure. The standard error is computed using a residual bootstrap procedure.

Figure 3.9: Heterogeneity in Bunching by Occupation and Skill



Note: This figure plots the layoff hazard rate by different occupation skill levels. Skill level is defined by the International Standard Classification of Occupations (ISCO). Low skill occupations are characterized by the performance of simple and routine physical tasks, and includes occupations such as cleaners and construction laborers. Medium-skill jobs involve performing more complex tasks, such as operating machinery, and includes occupations such as office clerks and skilled craftsman. High-skill jobs require workers to perform complex tasks and requires significant practical knowledge, and is composed of technicians, managers and scientific professionals. Tenure duration is binned into 15 day intervals. The dashed line is a tenth-degree polynomial fitted to the empirical hazard rate, excluding points 15 days away from the notch, as in Equation (3.1). The vertical dotted line displays the excess mass B, while the normalized excess mass b and standard error is reported in the figure. The standard error is computed using a residual bootstrap procedure.

Figure 3.10: Relationship between Bunching and Age



Note: This figure plots the layoff hazard rate by different occupation skill levels and age. Skill level is defined by the International Standard Classification of Occupations (ISCO). Younger workers are individuals who are between 20 and 25 years of age at the beginning of the employment spell. Older workers are individuals who are between 40 and 45 years of age at the beginning of the employment spell. Tenure duration is binned into 15 day intervals. The dashed line is a tenth-degree polynomial fitted to the empirical hazard rate, excluding points 15 days away from the notch, as in Equation (3.1). The vertical dotted line displays the excess mass B, while the normalized excess mass b and standard error is reported in the figure. The standard error is computed using a residual bootstrap procedure.

3.10 Numerical Implementation

3.10.1 Algorithm to solve the model

Solving the model can be broken into two steps: first solve for the equilibrium firm value functions and belief thresholds, and then solve for the equilibrium unemployment rate.

Solving for firm value functions and termination thresholds

After substituting the wage into the firm value functions, we essentially have to solve for 2 HJB equations together with the optimal belief threshold functions. To do this, we use a finite-difference approximation to the HJB equation, and exploit the fact that the optimal stopping problem characterizing the thresholds can be solved as a linear complementarity problem (Huang and Pang (2003)). Specifically, we start by solving for J^2 and $\underline{p}^2$ for $t \geq T_1$ since these objects are stationary. Given these objects, we can then work backwards to solve for J^1 and $\underline{p}^1$.

Solving for f^i

Given the thresholds and a guess of the unemployment rate, we can use a similar finite-difference approximation to solve the KFE forward in time, using the appropriate initial and boundary conditions, to get f^1, and f^2. Using these distributions to compute an implied unemployment rate then yields a simple iterative scheme to find the equilibrium unemployment rate.

Given these objects, all other equilibrium objects can be computed using the relationships stated in the theoretical exposition.

3.10.2 Estimation Algorithm

In order to estimate the model, we use a pattern search algorithm in MATLAB to minimize the objective function, using the algorithm stated above to find the model equilibrium and

compute model moments for each candidate parameter vector Ξ. In order to check out estimated parameter vector attains the global minimum, we fix a given parameter in Ξ, and use pattern search to minimize the estimation objective over the remaining parameters. Repeating this over a grid of values for the parameter in question, we then plot the objective as a function of the parameter to confirm that it is minimized globally at the parameter estimate.

3.11 Data Appendix

3.11.1 Overview

The *Relação Anual de Informações Sociais* (RAIS) is an employer-employee matched dataset which includes information on all workers and firms in the formal sector of Brazil. The main use of the RAIS is to compute federal wage-supplements (*Abono Salarial*). While not reporting can in theory result in fines, these fines are rarely issued in practice. However, workers and firms are incentivized to provide accurate wage information given the federal public wage-supplement is based on the wage reported in the RAIS.

3.11.2 Sample Selection

In the RAIS, workers are identified by an individual-specific PIS (Programa de Integração Social), a unique time-invariant worker identifier similar to a social security number. We follow Menezes-Filho and Muendler (2011b) and drop workers with PIS identifiers less than 11 digits, as these are not valid identifiers. Errors in worker identifiers may be caused by (1) bad compliance and bookkeeping errors or (2) to allow workers to withdraw from their severance account through fake layoffs and rehires. We eliminate jobs for workers which begin on the same day for the same employer. A single employer may report multiple accounts for one worker so that the workers may access their employer-funded severance payment account, which by law should only be accessed in the case of a firing or for health-related reasons. However, individuals must work at an employer for more than six months in order to access the FGTS account. Therefore, the spike in the job termination hazard cannot be due to employers reporting multiple jobs for the same worker.

3.11.3 Variable Definitions

PIS: A PIS is a worker identifier that is unique to a given worker over time.

Occupation: Occupations are defined by the Classificação Brasileira de Ocupações (CBO) into 2355 distinct groups. We map these occupations to International Standard Classification of Occupations (ISCO) for comparability. Additionally, ISCO classifies occupations by skill level, where occupations that require more training or credentials, and require more specialized work have higher skill levels.

Industry: Industries are reported under the *CNAE* four-digit classification (*Classificação Nacional de Atividade Econômica*) for 654 industries.

Wage: Wage refers to total payments, including regular salary payments, holiday bonuses, performance-based and commission bonuses, tips, and profit sharing agreements, divided by total months worked during the year for that employer. Payments that are not considered part of the wage include severance payments for layoffs and indemnity pay for maternal leave.

Tenure: The duration the worker has been employed at the establishment. We recode the tenure duration so that it increases in increments of two weeks.

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