

Mergers and Acquisitions, Local Labor Market Concentration, and Worker Outcomes*

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Abstract

Thousands of establishments employing millions of workers change ownership each year, sometimes leading to large changes in local labor market concentration that potentially increase labor market power. Using matched employer-employee data from the U.S., this paper estimates the direct and indirect effects of mergers and acquisitions (M&As) and resulting local labor market concentration changes on worker outcomes. To measure local concentration, I derive an index of concentration that uses job-to-job mobility patterns to incorporate information on substitutability across industries. Causal effects are estimated using a matched difference-in-differences design and cross-sectional variation in the predicted impacts of M&As on local concentration. In mergers that have negligible impacts on local labor market concentration, annual earnings for workers in M&A firms remain stable after the ownership change. In sharp contrast, earnings fall by over 2 percent for M&A workers in mergers that cause significant increases in local labor market concentration, with the largest effects in already concentrated markets. These patterns are similar in tradable industries, suggesting the effects are not driven by changes in product market power. Mergers generating the largest concentration changes generate negative spillovers on other firms in the same labor market, with an implied elasticity of earnings with respect to local concentration equal to -0.22. Viewed through the lens of a standard Cournot model, the results imply local concentration depresses wages by about 4-5 percent relative to a fully competitive benchmark.

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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., 2020; Azar et al., 2020; Benmelech et al., 2020; Rinz, 2020) has spurred recent policy debates on whether regulatory agencies should pursue new policies to protect workers (Hemphill and Rose, 2017; Marinescu and Hovenkamp, 2019; Naidu et al., 2018). However, estimating the causal effect of concentration on labor market outcomes 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.¹ 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, 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

¹In 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).

(an establishment-level panel for the U.S.) to discern when establishments change ownership.² 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.

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. In particular, 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

²There 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 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.

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, 2019; Naidu, Posner and Weyl, 2018). 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-compete 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.

Lastly, I consider what these results imply for antitrust scrutiny of mergers. I find that 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.³ 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, 2019; Naidu, Posner and Weyl, 2018). Recent work in industrial organization mostly relies on estimating 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

³This comparison comes with a number of caveats that are discussed in detail in Section 6.7.2. 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.

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 (2021) 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., 2020; Benmelech et al., 2020; Hershbein et al., 2018; Rinz, 2020; 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 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 3 discusses the institutional details, data, and measurement of concentration in the data. Section 4 describes the research design. Section 5 estimates the direct impact of M&A on incumbent establishments and workers. Section 6 estimates the market-level impacts of merger activity due to increased concentration in the labor market. Section 7 concludes.

2 A Model of Mergers and Acquisitions

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. This gives the firm monopsony power. In contrast, in a perfectly 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 $w_j(l_j)$:

$$\max_{l_j} R_j(l_j) - w_j(l_j)l_j, \tag{1}$$

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:

$$w_j = \underbrace{\left(\frac{\eta_j}{\eta_j + 1} \right)}_{\text{fraction of MRPL to worker} = \gamma_j} \underbrace{\theta_j}_{\text{MRPL}}, \quad (2)$$

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.⁴

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 C.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 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.

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:

$$\mathbb{E}[\tilde{w}_j(1) - \tilde{w}_j(0)] = \underbrace{\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)]}_{\text{monopsony effect}} + \underbrace{\mathbb{E}[\tilde{\theta}_j(1) - \tilde{\theta}_j(0)]}_{\text{MRPL}}, \quad (3)$$

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

⁴See Appendix C for an illustrative wage-bargaining model that has this implication.

is separable in logs.⁵ In this case, we can write the total effect on wages as:

$$\mathbb{E}[\bar{w}_j(1) - \bar{w}_j(0)] = \underbrace{\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0)]}_{\text{monopsony effect}} + \underbrace{\mathbb{E}[\tilde{\mu}_j(1) - \tilde{\mu}_j(0)]}_{\text{market power effect}} + \underbrace{\mathbb{E}[\tilde{\psi}_j(1) - \tilde{\psi}_j(0)]}_{\text{productivity effect}} \quad (4)$$

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

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

$$\mathbb{E}[\mu_j(1) - \mu_j(0) | \text{Tradable}_j = 1] = 0 \quad (5)$$

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

$$\mathbb{E}[\tilde{\gamma}_j(1) - \tilde{\gamma}_j(0) | \Delta C = 0] = 0 \quad (6)$$

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

$$\mathbb{E}[\psi_j(1) - \psi_j(0) | \Delta C = \kappa] = \mathbb{E}[\psi_j(1) - \psi_j(0)] \quad (7)$$

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.⁶

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.

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

⁵For 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 $\tilde{\psi}_j = \frac{\partial F_j(l_j)}{\partial l_j}$.

⁶Some 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).

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.

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, including a dominant firm model (Landes and Posner, 1981), a general equilibrium oligopoly model as in Berger et al. (2019), or a search model Jarosch et al. (2019). 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.

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 C.1, I show that these assumptions yield the following market-level wage equation:

$$w_m = \underbrace{\left(\frac{\eta_m}{HHI + \eta_m} \right)}_{\text{worker fraction} = \gamma_m} \underbrace{\theta_m}_{\text{AMRPL}}, \quad (8)$$

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, 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.⁷ 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 Table A1 computes the probability a job transition is within a given occupation or industry cell using data from the CPS (1995-2014). In column 1, I find that conditional on switching jobs, the probability the job transition is within the same 4-digit industry is 36.6 percent. Turning to column 3 in Appendix Table A1, I find that conditional on switching jobs, the probability the job transition is within the same 3-digit occupation is about 34.9 percent, slightly lower than the probability of a within-industry transition. Therefore, regardless of whether industry or occupations are used to construct labor markets, it is important to consider the possibility of significant transitions outside of the proposed labor-market definition.

2.3.1 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 $\{L_1, \dots, L_M\}$ 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.⁸ To see this, let $U_i(k|m) = \ln(V(k|m)) + \xi_i$ 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

⁷Often 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.

⁸The 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, 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.

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:

$$P(k|m) = \lambda_m \cdot f_k \cdot Pr(k \succ m), \quad (9)$$

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:

$$\frac{P(k|m)}{P(m|m)} = \underbrace{\frac{f_k}{f_m}}_{\text{relative offers}} \cdot \underbrace{\frac{V(k|m)}{V(m|m)}}_{\text{relative values}} \quad (10)$$

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 (10):

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{f_m}{f_k} \quad (11)$$

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:

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{L_m}{L_k} \quad (12)$$

Note that everything on the right hand side of Equation (12) can be measured with data on job-to-job flows and industry employment. Going forward, I denote estimated value in Equation (12) as $\alpha_{m \rightarrow k}$ 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:

$$w_m(\bar{V}_m) = w_m\left(\sum_{k=1}^M \alpha_{m \rightarrow k} L_k\right) \quad (13)$$

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

$$\tilde{s}_j = \frac{l_j}{\sum_{k=1}^M \alpha_{m \rightarrow k} L_k} \quad (14)$$

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:

$$\tilde{w}_m = \tilde{\theta}_m + \ln \left(\frac{\eta_m^\alpha}{C + \eta_m^\alpha} \right), \quad (15)$$

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 $\alpha L = \sum_{k=1}^M \alpha_{m \rightarrow k} L_k$. 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 (15), which yields the following formula for the elasticity of wages with respect to concentration:

$$\frac{\partial \tilde{w}}{\partial \tilde{C}} = -\frac{C}{C + \eta_m^\alpha} < 0 \quad (16)$$

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.

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.⁹ However, interpreting this evidence as causal remains controversial due 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.¹⁰ 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 2.3. While I define the labor market at the narrow 4-digit NAICS by commuting zone level,

⁹See Azar et al. (2020); Benmelech et al. (2020); Hershbein et al. (2018); Rinz (2020); Lipsius (2018) among others.

¹⁰Benmelech et al. (2020) 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.

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 necessarily. A key component of this calculation is the market-level elasticity η_m (note Propositions 1 and 2 condition on η_m). In Section 6.6.1, 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.

3 Institutions, Data, and Measurement

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.¹¹ In general, however, most of the deals that the FTC and DOJ do get notified about are allowed to proceed without interference. Figure A2 reports the fraction of notifications that face some sort of antitrust enforcement for the years 1999-2009. 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.

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., 2018; Marinescu and Hovenkamp, 2019; 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 20th 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 20th Century Fox Animation have been purchased by Disney. Therefore, any

¹¹Wollmann (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.

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.¹²

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.

3.2.1 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.¹³ 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).¹⁴ 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 12th employment) and total annual payroll. Given employment reflects the employment level on March 12th, 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 12th 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. 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).

¹²I thank Orley Ashenfelter for pointing out this example.

¹³Unlike 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).

¹⁴Another 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.

3.2.2 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), which comprise about 53 percent of the total population in the United States as of the 2010 Census.¹⁵

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 LBD to correct for changing firm identifiers.¹⁶ 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 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.

¹⁵The 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.

¹⁶For 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.

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 transition 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 $\alpha_{m \rightarrow k}$ for every pair of industries m and k . To compute C in practice I make two modifications to the formula in Section 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:

$$\tilde{s}_{jmc} = \frac{\sum_{k \in c} \alpha_{m \rightarrow k} l_{jkc}}{\sum_{k \in c} \alpha_{m \rightarrow k} L_{kc}} \quad (17)$$

where

$$\alpha_{m \rightarrow k} = \frac{P(k|m)}{P(m|m)} \frac{1}{\mathbb{E}[\frac{L_k}{L_m}]} \quad (18)$$

These are modified versions of Equations (14) and (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 $\alpha_{m \rightarrow k}$ (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:

$$C_{mc} = \sum_{j \in c} (\tilde{s}_{jmc})^2 \quad (19)$$

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 C.2 shows this result algebraically. The proof can be seen by examining Equation (18). With no mobility across industries, $\alpha_{m \rightarrow k}$ will be zero for all industries $k \neq m$. Therefore Equation (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, $\alpha_{m \rightarrow k} = 1$ for all k and m , implying Equation (17) will be equal to simple employment shares in the commuting zone.

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 serially 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.¹⁷ 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 6. Choosing one counterfactual per control group ensures that the treated and control groups are balanced on the matched variables.¹⁸ 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 12th 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. 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.

3.5 Summary Statistics

Figure 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 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

¹⁷In other words, the stable unit treatment value assumption (SUTVA) may be violated in this setting.

¹⁸An 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.

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 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.¹⁹ 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.²⁰ 46 percent of the workers are female. About 32 percent of M&A workers have a college degree while 31 percent of control workers have a college degree.²¹

¹⁹11 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.

²⁰All earnings are adjusted to 2011 dollars.

²¹Education 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.

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:

$$Y_{jt} = \sum_{k=-4}^4 \delta_k^{MA} \mathbb{1}(t_j = t^* + k) \times MA_j + \psi_j + \tau_t + u_{jt} \quad (20)$$

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.²² All standard errors are two-way clustered at the 4-digit NAICS and commuting zone level.²³ 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.²⁴ To summarize findings, I often report the average impact which is equal to $\frac{1}{4} \sum_{k=1}^4 \delta_k^{MA}$.

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:

$$y_{it} = \sum_{k=-4}^4 \delta_k^{MA} \mathbb{1}(t_i = t^* + k) \times MA_i + \omega_i + \tau_t + u_{it} \quad (21)$$

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 (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.

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 assump-

²²As shown in Schmieder et al. (2020), when workers must satisfy a tenure restriction to be included in the sample, it is common to observe a hump-shaped pattern in earnings, given individuals with a stable job are likely on a positive earnings trajectory. Controlling for years alone when pooling across multiple cohorts of treated and matched controls does not capture this hump-shaped pattern. This is why M&A event by year fixed effects are included in the estimation, which can control for this pattern of selection into the sample. This is equivalent to specification 4 in Appendix 2 of Schmieder et al. (2020).)

²³Clustering at the merger-level results in similar standard errors.

²⁴As 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.

tion 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.²⁵

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.

5 Effect of M&A on Establishments and Incumbent Workers

5.1 Effect of M&A on Establishment-Level Employment

Panel A of Figure 3 plots $\hat{\delta}_k^{MA}$ from estimating Equation (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

²⁵This 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.

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 B for details). As can be seen in Appendix Figure A5, 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.

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 A of Figure 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 4 plots estimates of Equation (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 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.²⁶

5.3 Potential Mechanisms and Heterogeneity

5.3.1 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. 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 predicted 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 predicted changes in concentration) have roughly zero impact on local labor market concentration (most of these are between firms operating in different commuting zones). Predicted impact on concentration is computed by taking employment levels in the year prior to merger and computing the impact on concentration by merging the two entities. Therefore, predicted impacts do not reflect any endogenous changes of the merger itself, as they are computed before the merger has taken place.

I refer to these mergers that have negligible predicted impacts on local labor market concentration 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. “Low-impact” here refers to the fact that the merger has low predicted impact on local labor market concentration.

Following the model in Section 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 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 productivity 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

²⁶He (2018) finds similar results for firm stayers and firm leavers in Denmark.

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.

5.3.2 Robustness of Wage Losses Only in High-Impact Mergers

One potential concern is that mergers that are predicted to 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 A2 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 monopoly power. In Panel A of Appendix Table A2, 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 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 A2 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 are predicted to 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.

6 Market-Level Impacts of Increased Concentration

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.

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 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., 2020; Rinz, 2020; Rossi-Hansberg et al., 2020; 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., 2018).

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 D, 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 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 6.7.1 which uses the simple model of Cournot competition to translate changes in the distribution of concentration into an implied wage changes.

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:

$$\Delta Y_{mt} = \beta \Delta \tilde{C}_{mt} + \tau_t + u_{mt} \quad (22)$$

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:

$$\tilde{w}_{it} = \Phi_{mt} + \beta_t X_{it} + u_{it} \quad (23)$$

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.²⁷ 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 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 probability of actually working in the listed commuting zone (See Section 3 for more details), which is computed using the true 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., 2020; Benmelech et al., 2020; Rinz, 2020). 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. Additionally, in Table 8, I decompose changes in concentration into various sources, including mergers, exit, entry, and reallocation of employment across firms. I find only about 1.4 percent of the variation in

²⁷Education 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.

concentration over time is due to merger activity.

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 the next section, 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.

6.4 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 predicted merger-induced concentration changes to markets that experience larger predicted merger-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.²⁸ 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.

6.5 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:

$$\tilde{w}_{mt} = Post_{mt} \times \left[\sum_{b=1}^4 s_b(\tilde{C}_m^{MA}) \right] + \Phi_m + \tau_{t,k(m)} + u_{mt} \quad (24)$$

where \tilde{w}_{mt} is the average log market wage obtained by first residualizing on worker observables, as described in Section 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

²⁸The 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.

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 predicted changes in the flows-adjusted concentration measure \tilde{C}_m^{MA} . 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 $\{s_b(\cdot)\}_{b=1}^4$ is a set of basis functions defining a natural cubic spline with four knots. Following Harrell (2001), I place the knots at the 5th, 35th, 65th, and 95th percentiles of the distribution of concentration changes.²⁹ The “dose-response” function $d(x) = \sum_{b=1}^4 s_b(x)$ gives the effect of a predicted 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 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 predicted concentration changes, there is no impact on market-level wages. At predicted 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 95th percentile of all predicted concentration changes. This implies only the top ventile of predicted concentration increases generate significant shifts in market-level wages.

6.6 Market-Level Difference-in-Differences Estimates

The fact that larger predicted changes in concentration generate larger shifts in outcomes could partially reflect different pretrends between markets that experience large vs. small predicted changes in local labor market concentration. Motivated by the analysis in the last section, I compare outcomes for predicted concentration changes in the top-ventile vs. all other predicted concentration changes. Appendix Table A3 presents summary statistics that compares these markets. On average, top-ventile predicted 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, even if a merger has a relatively large predicted impact on concentration, changes over time may be swamped by other changes in the market. Additionally, if mergers incentivize more entry (for example, if the merged firm raises price, then more firms may enter), then increases 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:

$$C_{mt} = \sum_{k=-4}^4 \delta_k^C \mathbb{1}(t_m = t^* + k) \times Q20_m + \Phi_m + \tau_{t,k(m)} + u_{mt} \quad (25)$$

where $Q20_m$ indicates the market is involved in a predicted concentration change in the top ventile of all

²⁹As 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-95th percentile is the average of the 94th, 95th and 96th percentile. The psuedo-knots are equal to 0.011, 0.018, 0.037, 0.21. The spline is restricted to be linear below the 5th and above the 95th percentile.

concentration changes. Panel A of Figure 8 plots the coefficients $\hat{\delta}_k^C$ from estimating Equation (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 (25) with the average market-level earnings \tilde{w}_{mt} as the outcome. Panel C of Figure 8 plots the results. On average, log average earnings in the top ventile markets fall by -0.034 ($SE = 0.013$) after the concentration event.

6.6.1 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:

$$\tilde{C}_{mt} = \Phi_m + \tau_{t,k(m)} + Q20_m \times Post_{mt} + u_{mt} \quad (26)$$

$$\tilde{w}_{mt} = \Phi_m + \tau_{t,k(m)} + \beta \tilde{C}_{mt} + u_{mt} \quad (27)$$

where Equation (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 A4 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 ($SE=0.128$).³⁰

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 A4, 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),

³⁰In 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.

do cause market-level declines in earnings that are not due 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 2.3.

6.7 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.

6.7.1 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:

$$\gamma_m = \frac{\eta_m^\alpha}{C_m + \eta_m^\alpha} \quad (28)$$

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:

$$\Delta \tilde{w} = \Delta \tilde{\theta} + \ln \left(\frac{\eta}{C_1 + \eta} \right) - \ln \left(\frac{\eta}{C_2 + \eta} \right) \quad (29)$$

Where $\Delta \tilde{\theta}$ is the change in average marginal revenue product in the market. 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 (assuming the change in average marginal revenue product is known). 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 6.6.

That is, I set:

$$\hat{\eta} = \arg \min_{\eta} (\hat{\beta} - m(\eta))^2 \quad (30)$$

Because $m(\eta)$ in general depends on the differential effect of the merger on market-level $\tilde{\theta}$, I assume for tractability that the change in $\tilde{\theta}$ is the same for top-ventile mergers and all other mergers. This implies that $m(\eta)$ now only depends on changes in market concentration between mergers and the elasticity of labor supply.

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.³¹ 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 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.

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-compete 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.

6.7.2 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 (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 roughly how many mergers would be blocked based on different thresholds.

Figure 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,

³¹For 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 of recent papers have found elasticities between 1 to 5 (See Manning (2011) for a review).

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 of all mergers blocked. 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 A2). 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.

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 on the basis of increased labor market power.

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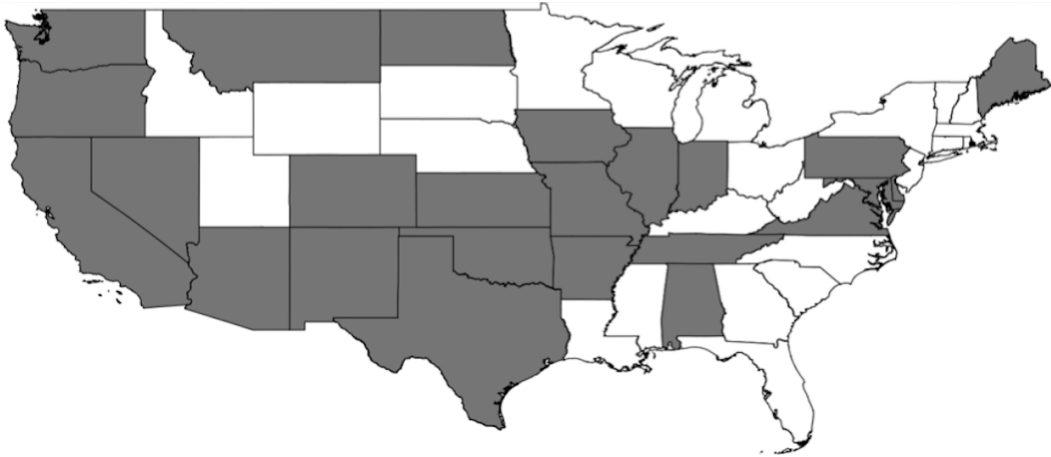
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Figure 1: Sample of States with Worker-Level (LEHD) Data Available



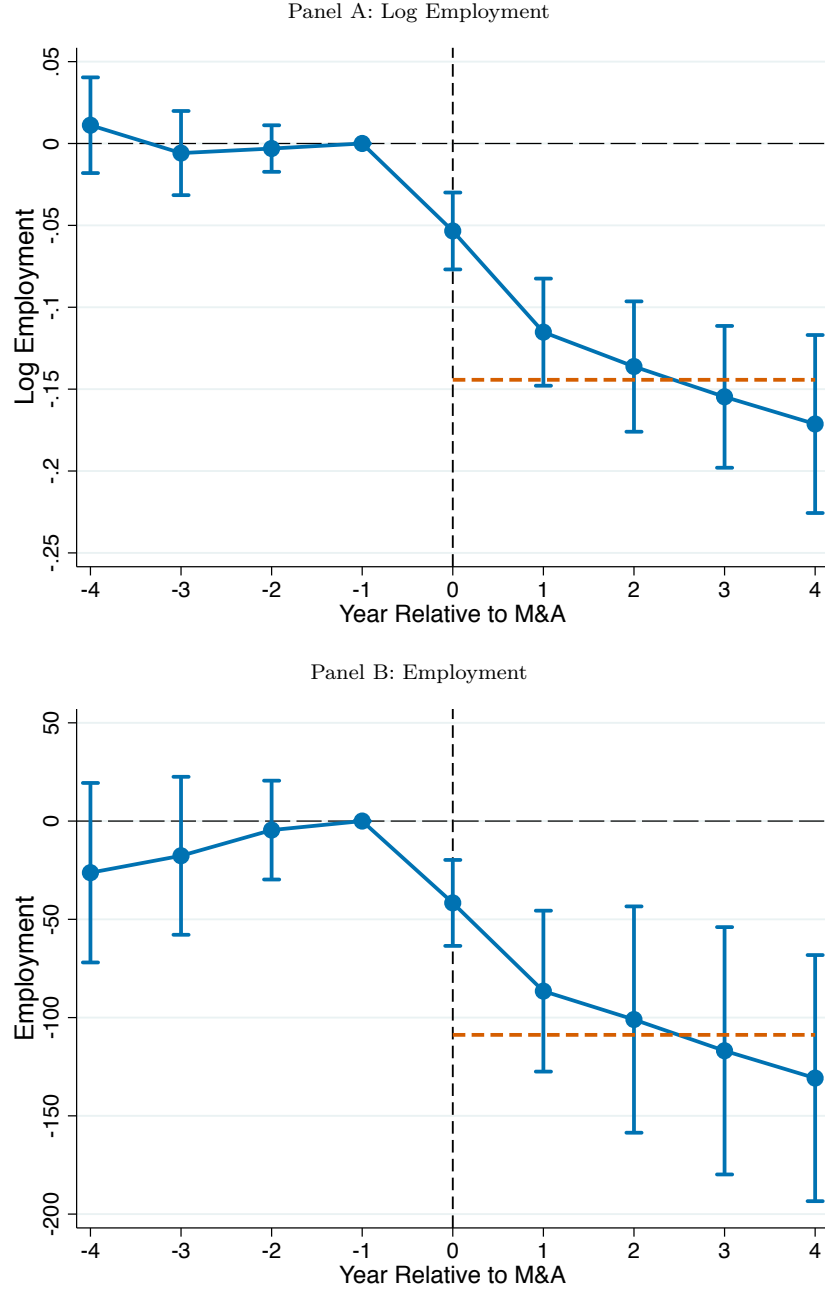
Note: The states with worker-level (LEHD) data available are shaded in gray. The sample includes: AL, AZ, AR, CA, CO, DE, DC, HI, IL, IN, IA, KS, ME, MD, MO, MT, NV, NM, ND, OK, OR, PA, TN, TX, VA, WA. These states correspond to 53.8 percent of the U.S. population as of the 2010 U.S. Census.

Figure 2: M&A Activity over Time



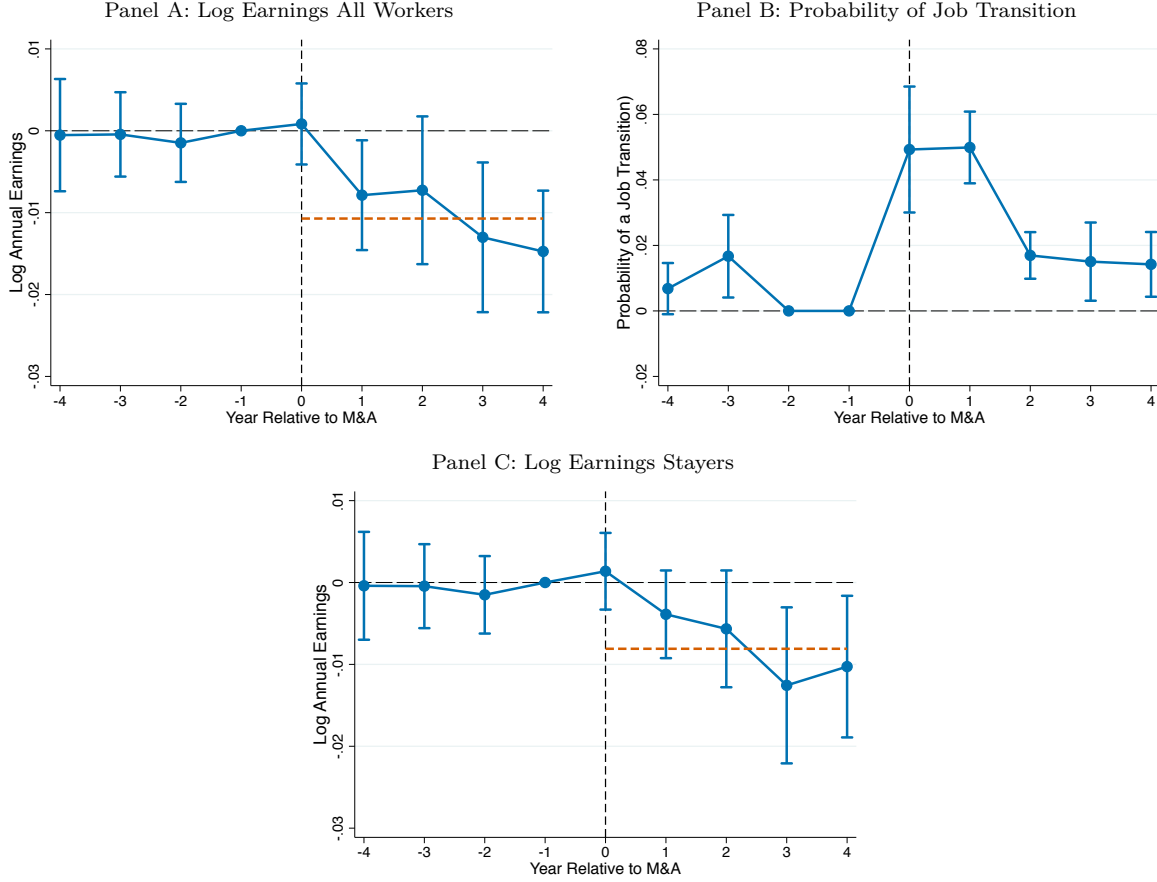
Note: This figure plots the total employment in M&A establishments (solid blue line) over time. This sample is a subset of all merger activity due to sample restrictions that drop small and partial M&As. For more details on sample construction see Section 3.4. The dashed orange line plots the number 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 3: Difference-in-Differences Estimates of the Effect of M&A on Employment



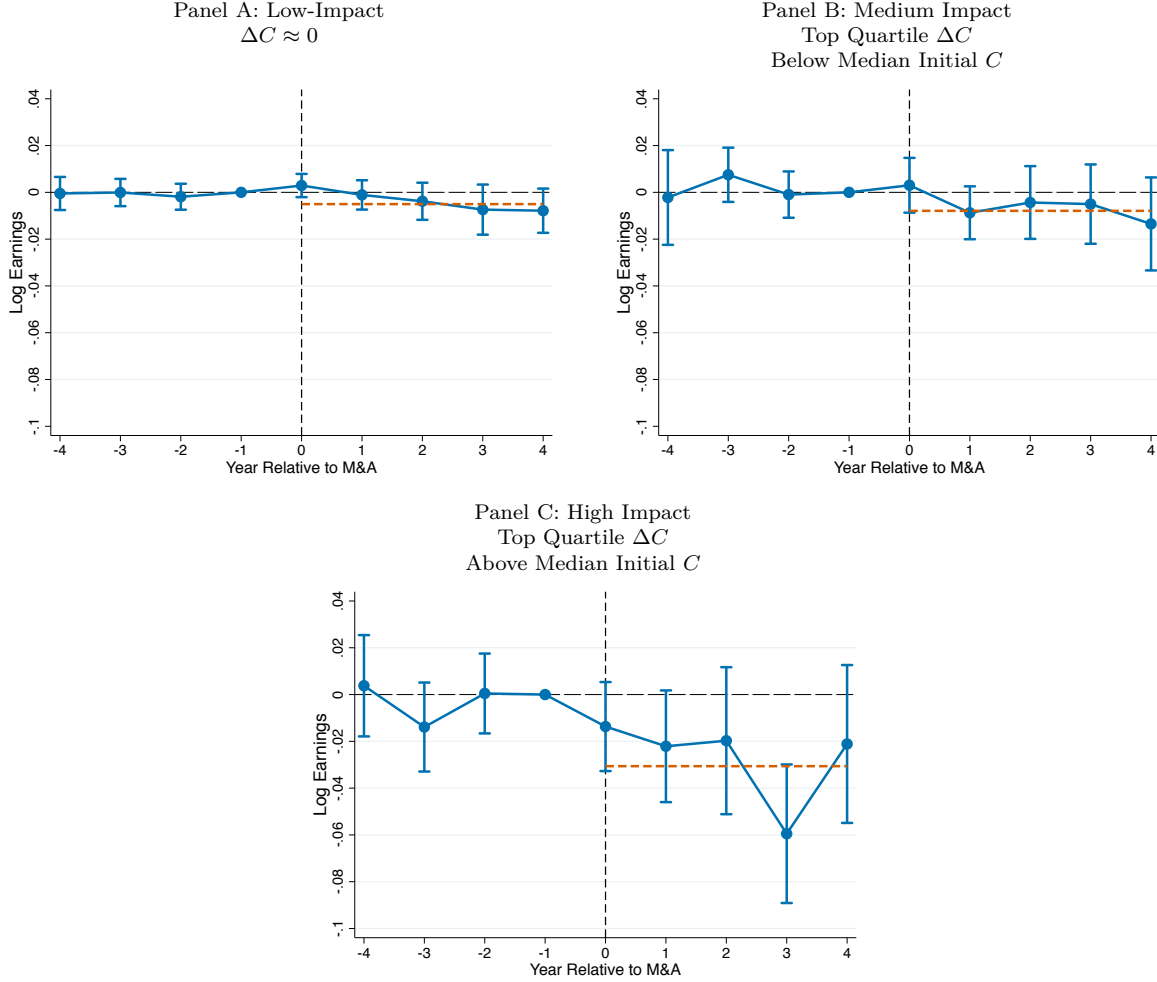
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on log establishment-level employment in Panel A and establishment-level employment in Panel B (including zeros). Due to the ambiguity in the timing of the merger, some M&A establishments have already gone through 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. 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. Regressions are weighted by the employment of the establishment in the year prior the merger. 95 percent confidence intervals two-way clustered at the commuting zone and 4-digit NAICS level are displayed.

Figure 4: Difference-in-Differences Estimates of the Effect of M&A on Incumbent Worker Outcomes



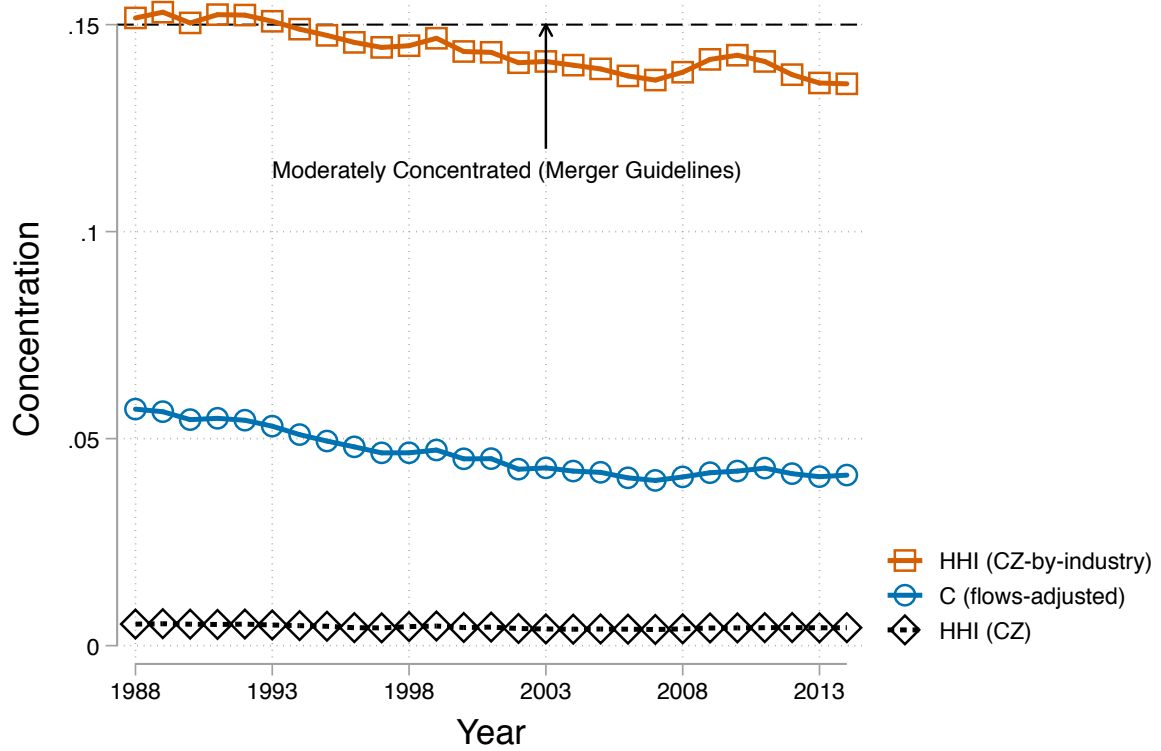
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on worker outcomes. Panel A reports the impact on log annual earnings for all incumbent workers. Panel B reports the impact on job transitions. Panel C reports the impact on log annual earnings for firm stayers. 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 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 5: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings



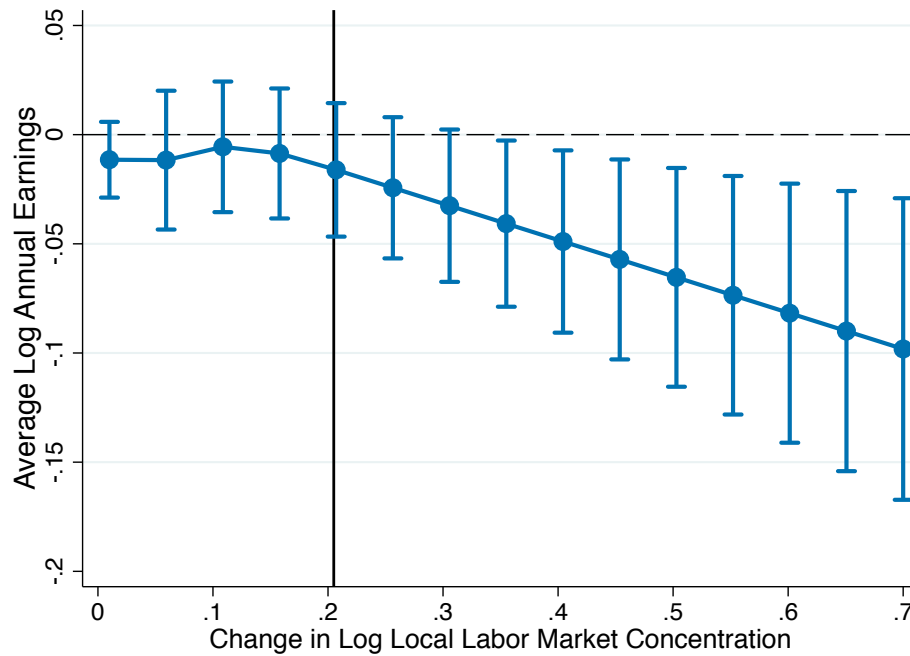
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. Concentration (C) is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. Panel A displays results for workers exposed to low-impact mergers, which occur when the predicted 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 predicted 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 predicted 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 4 and Section 3.4.

Figure 6: Average (Employment-Weighted) Local Concentration Over Time



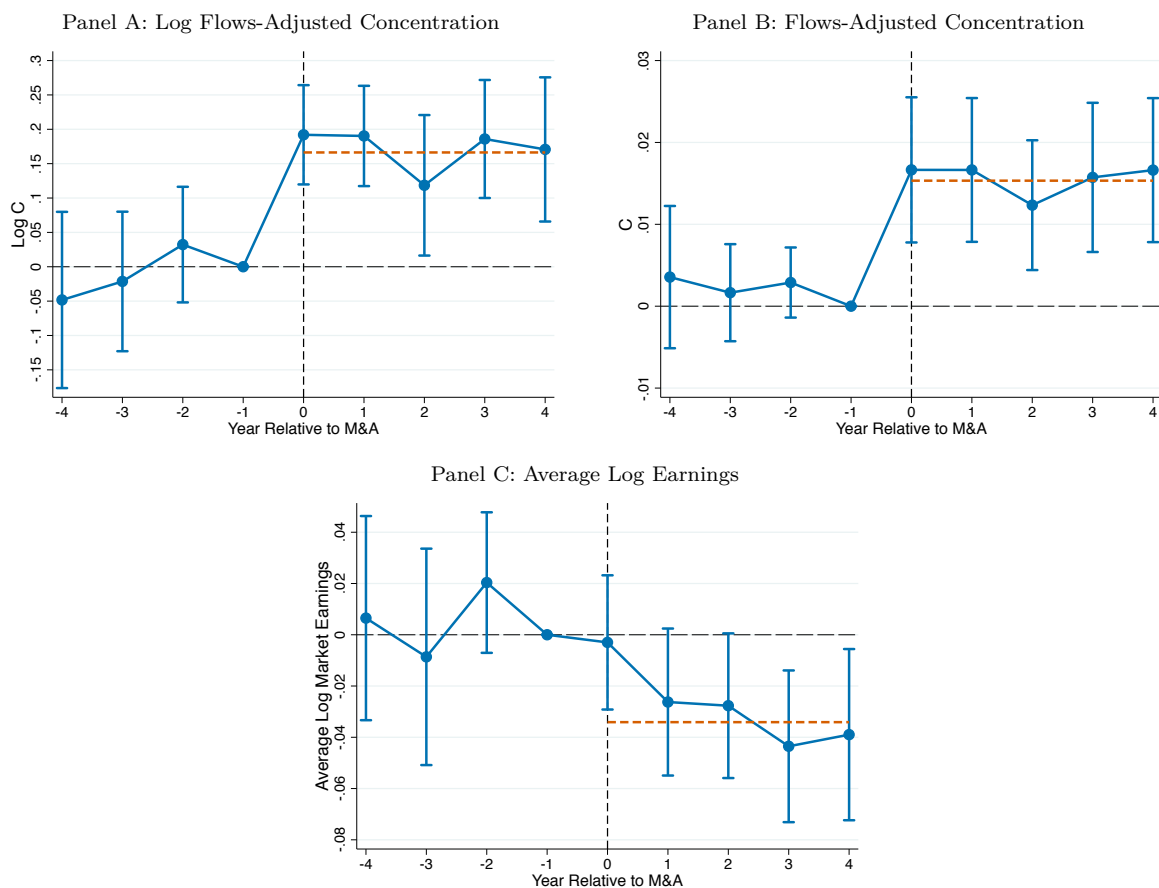
Note: The figure plots the average (employment-weighted) level of local labor market concentration for three different measures. The orange squares correspond to a HHI in which the labor market is defined by a 4-digit NAICS by commuting zone cell. The black diamonds correspond to a HHI in which the labor market is defined by a commuting zone. The blue circles correspond to the flows-adjusted local concentration index (C) described in Section 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 an industry by commuting zone. Transitions across industries are estimated using the LEHD worker-level data.

Figure 7: Market-Level (Excluding M&A Firms) Impacts by Predicted Change in Local Labor Market Concentration



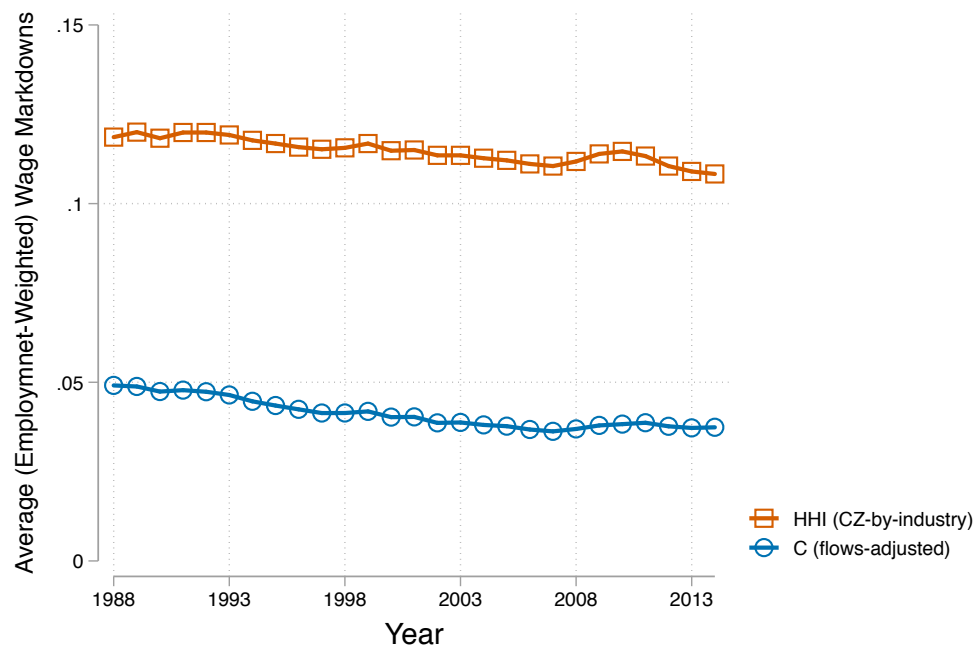
Note: This figure reports the impact of M&A on market-level earnings as a function of the predicted change in log local labor market concentration. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure (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 pseudo-95th percentile, which is equal to the average of the 94th through 96th percentiles and is reported in place of the 95th 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 8: Difference-in-Differences Estimates of Top-Ventile Concentration Increases on Market-Level Outcomes



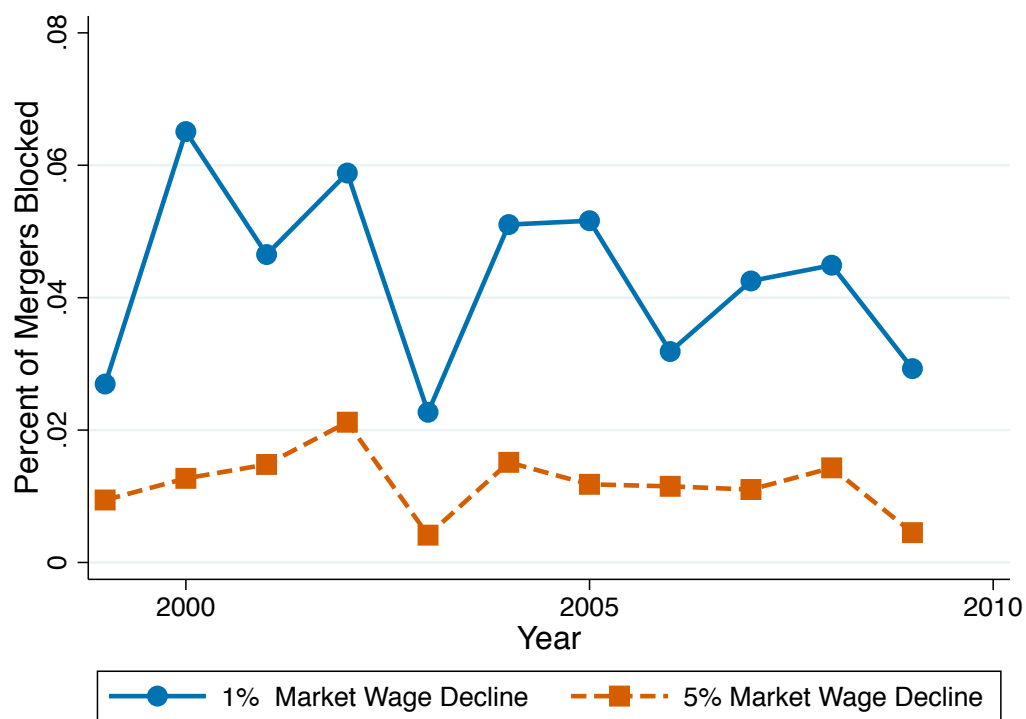
Note: This figure displays estimates of the effect of a top-ventile concentration increase on market-level outcomes. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure (C) that incorporates information on worker flows across industries. Panel A reports the impact on log flows-adjusted concentration, Panel B reports the impact on flows-adjusted concentration, 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 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 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 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 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 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
<i>Panel A: Establishment Characteristics</i>	(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	—
<i>Panel B: Sectors of Establishments</i>		
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
<i>Panel C: Characteristics of M&A deal</i>		
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
<i>Panel A: Worker Characteristics</i>	(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	–
<i>Panel B: Merger Characteristics</i>		
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). 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 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 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. (1)	Log Emp. (2)	Emp. (3)	Log Payroll (4)	Estab. Survival (5)
Post-MA	-0.144*** (0.021)	-0.081*** (0.015)	-108.800*** (28.010)	-0.121*** (0.019)	-0.031*** (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 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*** (0.027)	-0.234*** (0.050)	-0.151*** (0.024)	-0.108*** (0.019)	-0.225*** (0.033)	-0.055*** (0.018)	-0.162*** (0.024)
Estab-Years	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 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. 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 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*** (0.004)	-0.008** (0.003)	0.032*** (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 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: Heterogeneity 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 $\Delta C^{MA} > 0$		Pred $\Delta C^{MA} > 0$	
	High Initial C	Low Initial C	High Initial C	Low Initial C	High Initial C	Low Initial C
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All Mergers</i>						
Post-M&A	-0.031*** (0.011)	-0.008 (0.007)	-0.005 (0.004)	-0.023* (0.013)	-0.026*** (0.012)	
Initial C	0.072	0.011	0.079	-	-	
Change in Log C	0.099	0.059	0.001	-	-	
Approx Worker-Years	2,700,000	3,700,000	19,300,000	-	-	
<i>Panel B: Tradables</i>						
Post-M&A	-0.067*** (0.023)	0.001 (0.012)	-0.012* (0.006)	-0.068*** (0.026)	-0.056*** (0.024)	
Initial C	0.136	0.018	0.152	-	-	
Change in Log C	0.099	0.041	0.000	-	-	
Approx Worker-Years	600,000	1,200,000	14,000,000	-	-	
<i>Panel C: National Mergers</i>						
Post-M&A	-0.042*** (0.013)	-0.013* (0.007)	-0.008 (0.005)	-0.029** (0.015)	-0.035*** (0.013)	
Initial C	0.070	0.011	0.076	-	-	
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 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 who are defined as workers employed in time t at the same firm as in $t^* - 1$, where t^* is the year of the M&A. For details on the matching algorithm used to identify control workers, see the notes to Table 2 and Section 3.4. A national merger is defined as a merger between two firms that both operate in at least five commuting zones. Tradable industries belong to 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*** (0.005)		0.309*** (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} (Ownership Changes)	0.834*** (0.032)				0.659*** (0.035)
ΔC^{Exit}		0.778*** (0.016)			
ΔC^{Entry}			0.981*** (0.012)		
$\Delta C^{Reallocation}$				0.946*** (0.007)	0.915*** (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*** (0.043)	0.239*** (0.047)	0.188*** (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 95th 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 appear in parentheses. 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.

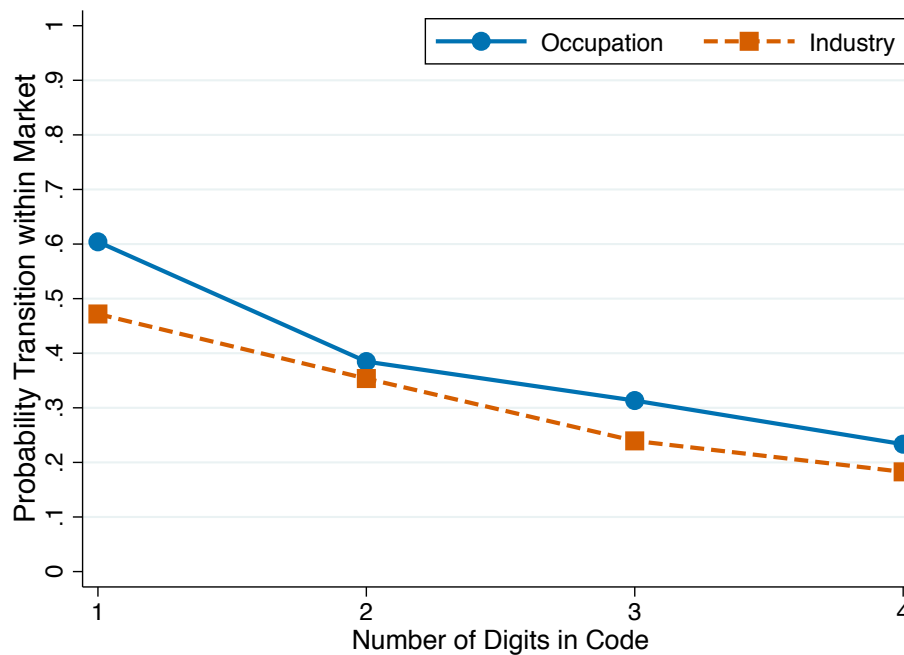
Table 10: Instrumental Variables Estimates of the Elasticity of Earnings with Respect to Local Labor Market Concentration (Flows-Adjusted)

	Average Log Market Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Log C	-0.219** (0.094)	-0.182*** (0.067)	-0.147* (0.083)			
Log C \times Above Median C				-0.259** (0.108)	-0.230*** (0.081)	-0.176** (0.083)
Log C \times Below Median C				0.059 (0.121)	0.065 (0.120)	0.058 (0.141)
Market-Years	24,000	21,000	24,000	24,000	21,000	24,000
4-digit NAICS-by-CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
1-digit NAICS-CZ-year FE	Yes	No	Yes	Yes	No	Yes
2-digit NAICS-CZ-year FE	No	Yes	No	No	Yes	No
Weighted by Employment	Yes	Yes	No	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 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.

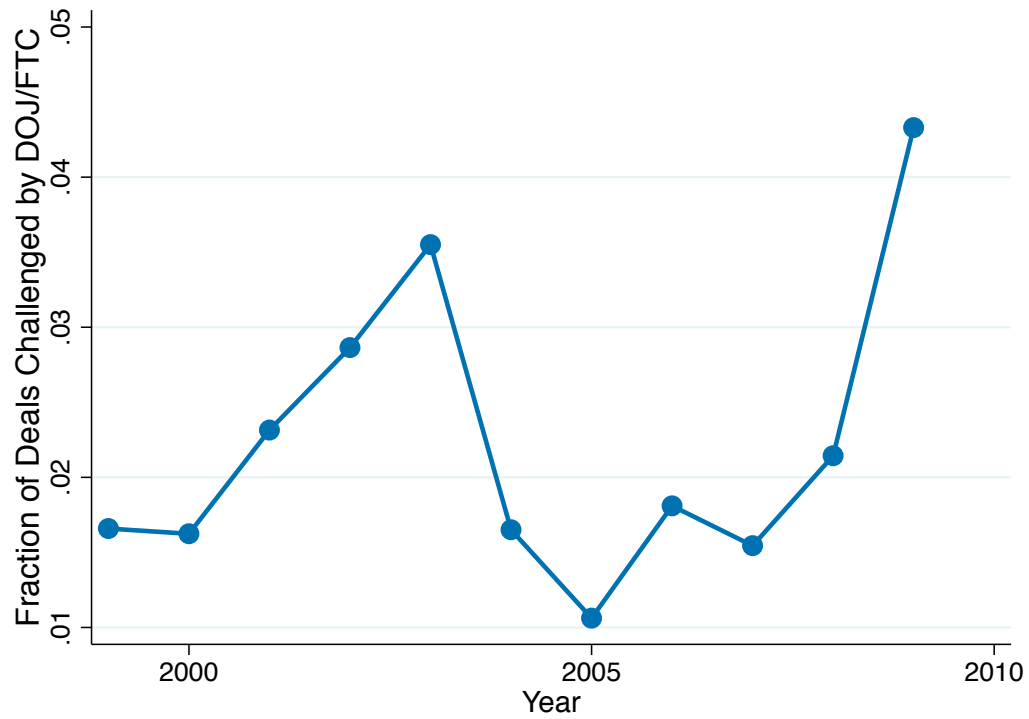
Appendix A: Additional Results

Appendix Figure A1: Job Transitions Within Industries and Occupations in Brazil



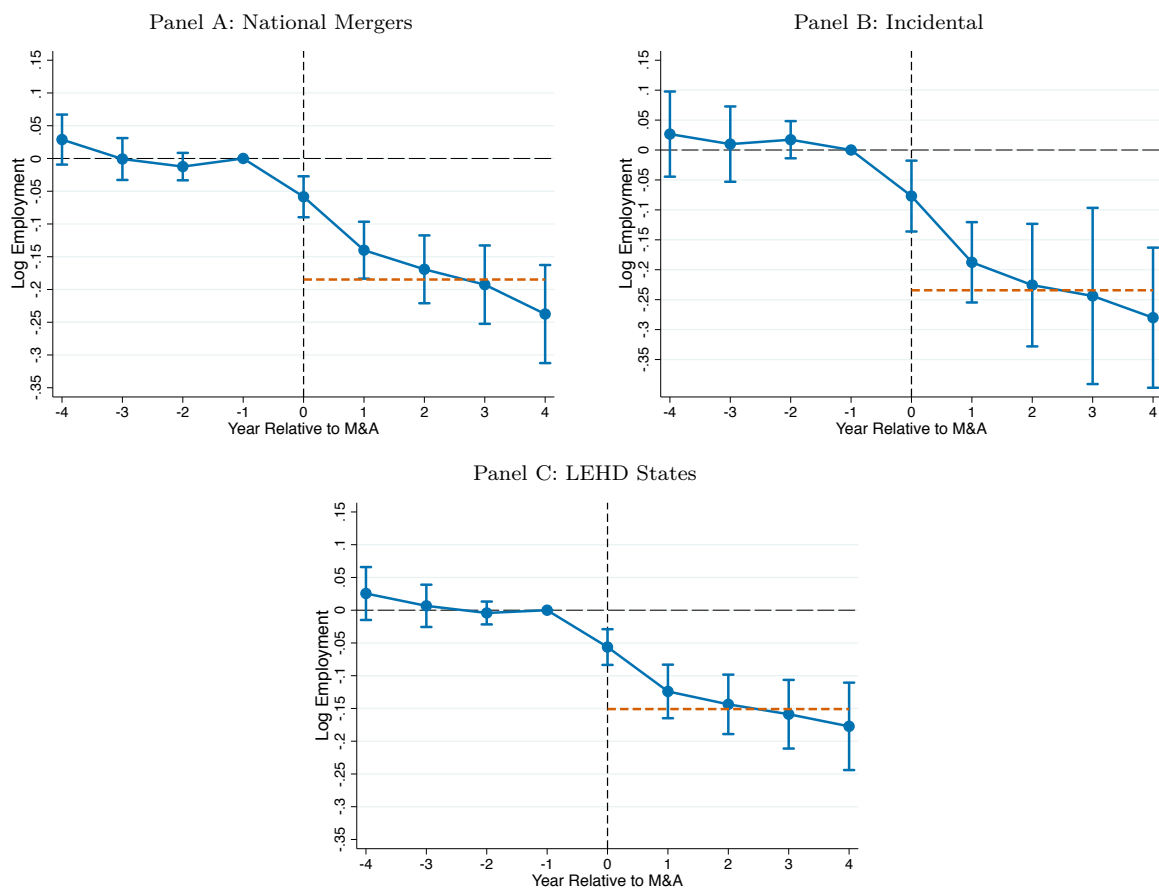
Note: This figure shows the probability a worker transitions within a given industry or occupation given the level of aggregation chosen. The data come from the *Relação Anual de Informações Sociais* (RAIS), a matched employer-employee dataset from Brazil.

Appendix Figure A2: DOJ and FTC Antitrust Enforcement Actions over Time



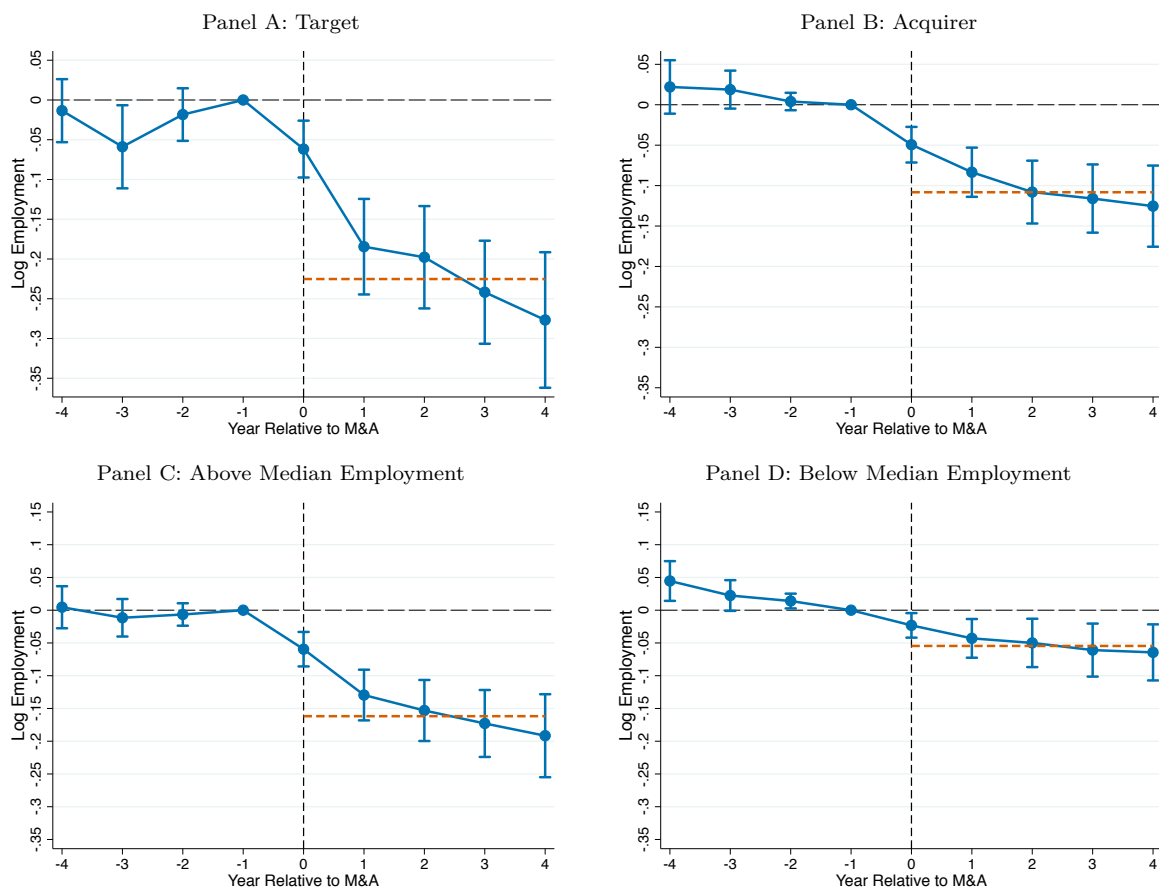
Note: This figure reports the fraction of merger notifications that are challenged each year between 1999-2009. Some deals in which the target asset's are relatively small are exempt from having to notify antitrust authorities (See Wollmann (2019) for more details). Data comes from *Hart-Scott Rodino 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.

Appendix Figure A3: Difference-in-Differences Estimates of the Effect of M&A on Employment by Merger Characteristics



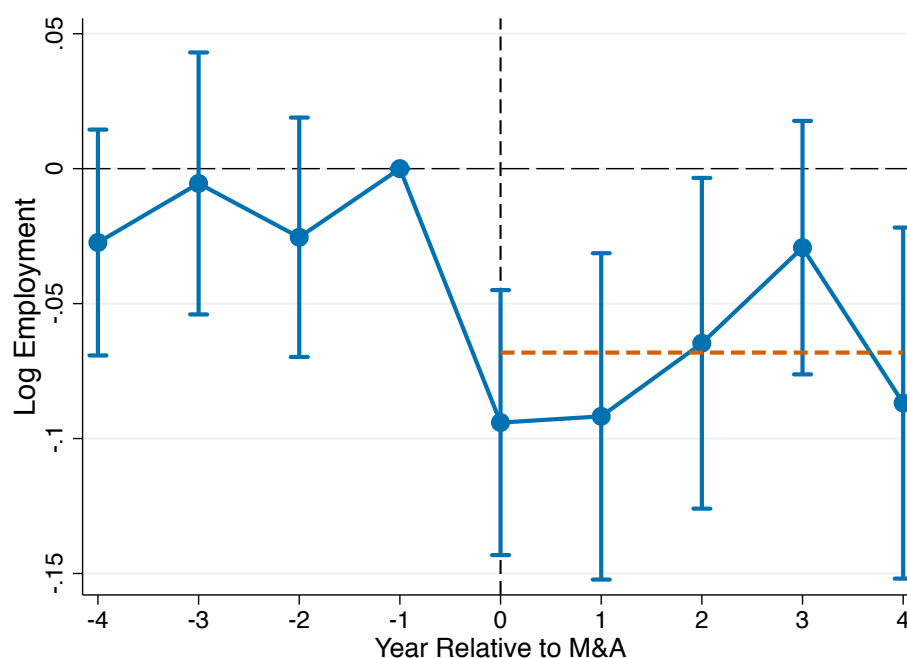
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment. The regressions are estimated on the sample described in the notes to Table 1, which contain details on the matching algorithm used to identify control establishments. Mergers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

Appendix Figure A4: Difference-in-Differences Estimates of the Effect of M&A on Employment by Establishment Characteristics



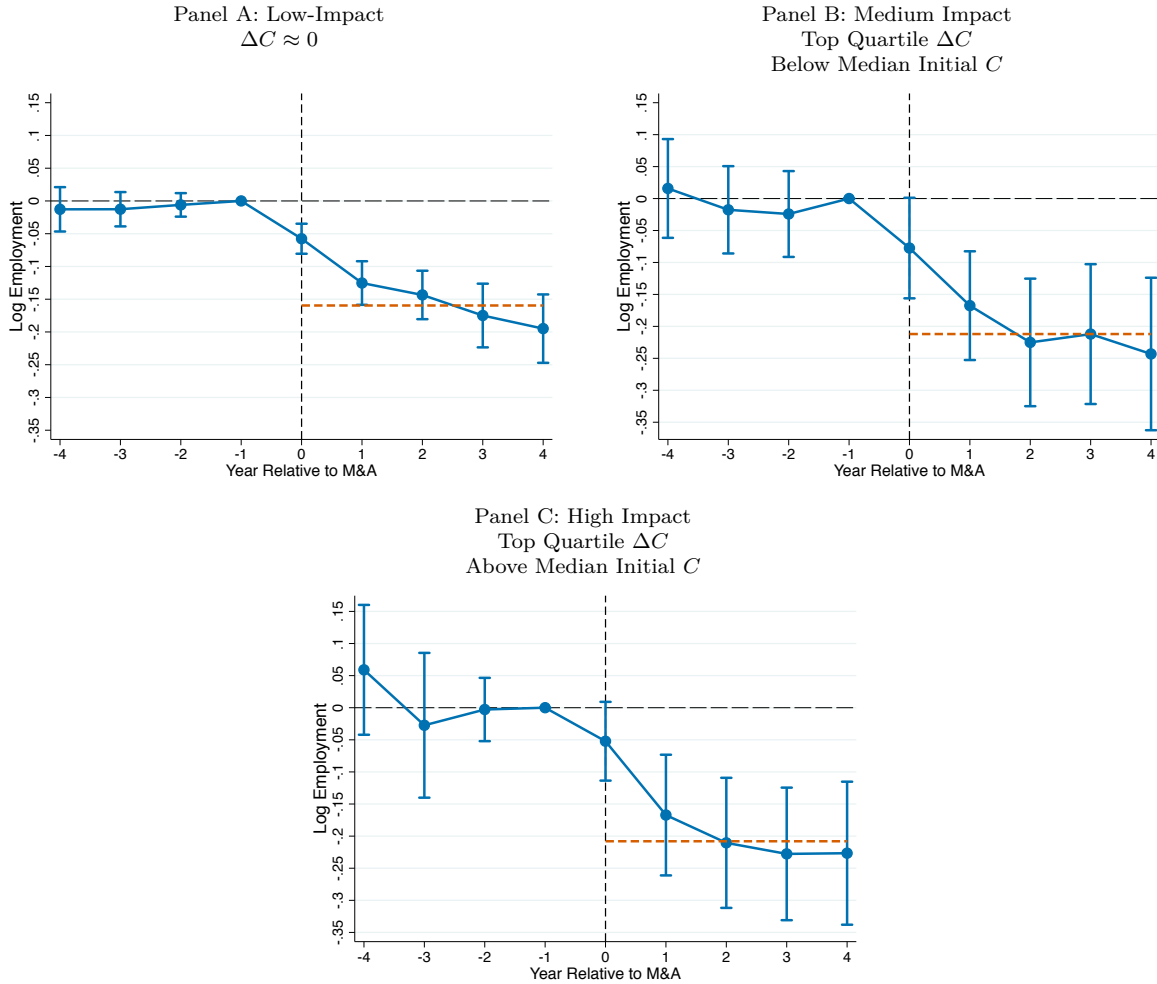
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment. The regressions are estimated on the sample described in the notes to Table 1, which contain details on the matching algorithm used to identify control establishments. High 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.

Appendix Figure A5: Difference-in-Differences Estimates of the Effect of M&A on Acquiring Firms' Growth Rates



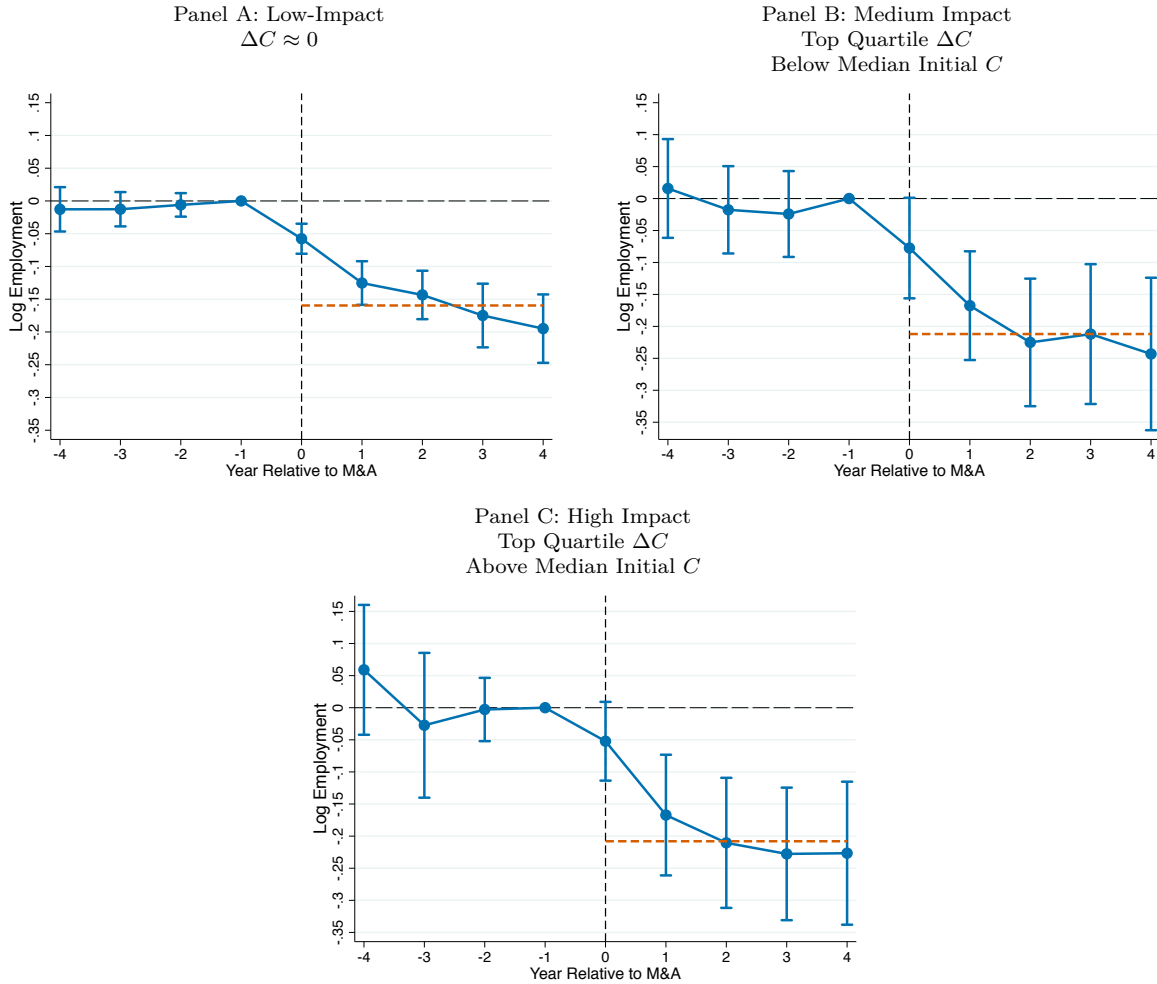
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on the growth rate of the acquiring firm. Growth rates for the acquiring firm are computed using the method described in Haltiwanger et al. (2013), which corrects for mechanical growth due to M&A. To find counterfactual firms, I implement the same matching procedure discussed in Section 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.

Appendix Figure A6: Difference-in-Differences Estimates of the Effect of M&A on Establishment Employment in Nontradable Industries



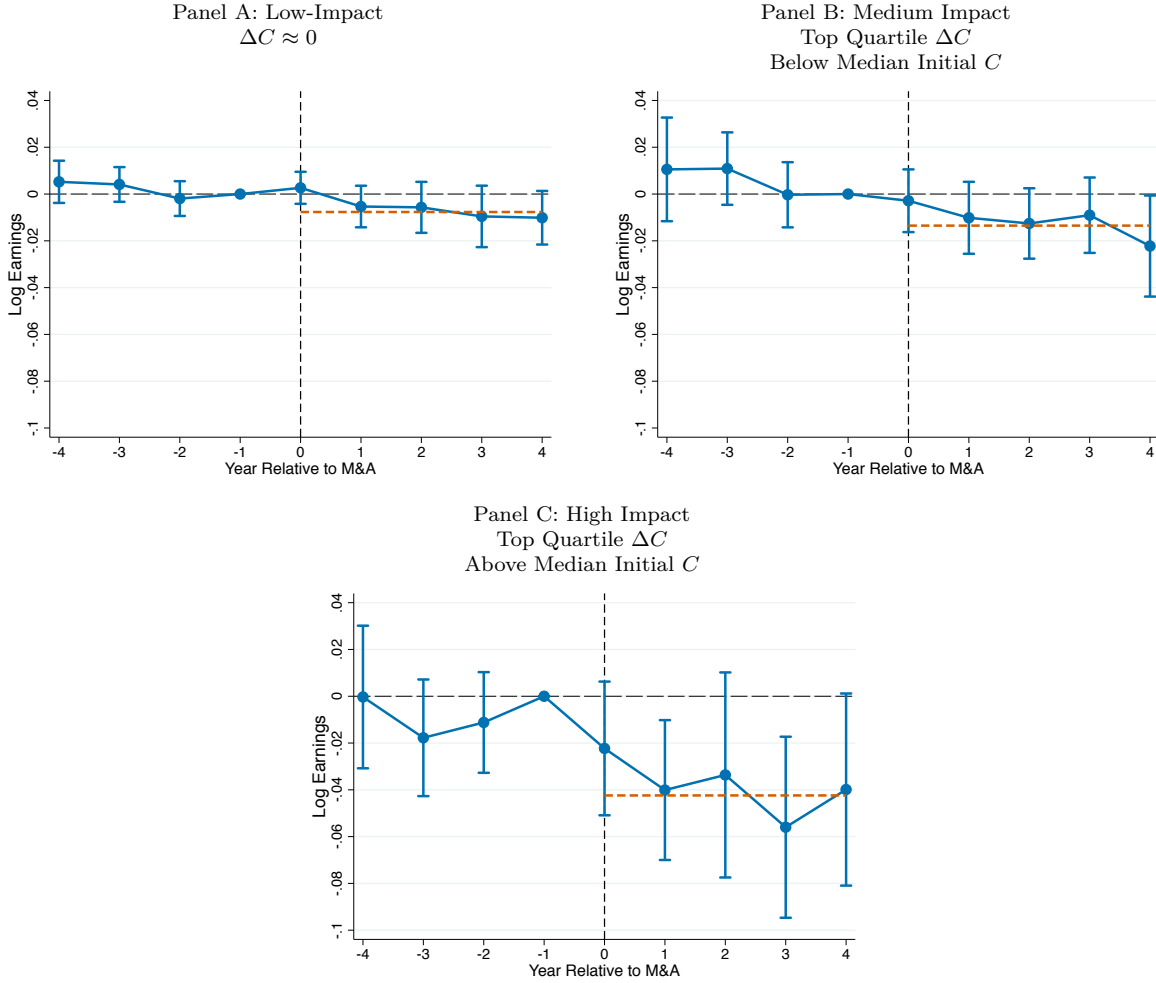
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment in nontradable industries, which are defined as industries that do not belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. Details on the matching algorithm used to identify control establishments appear in the notes to Table 1 and Section 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 based on standard errors two-way clustered at the 4-digit NAICS and commuting zone level are displayed.

Appendix Figure A7: Difference-in-Differences Estimates of the Effect of M&A on Establishment Employment in Tradable Industries



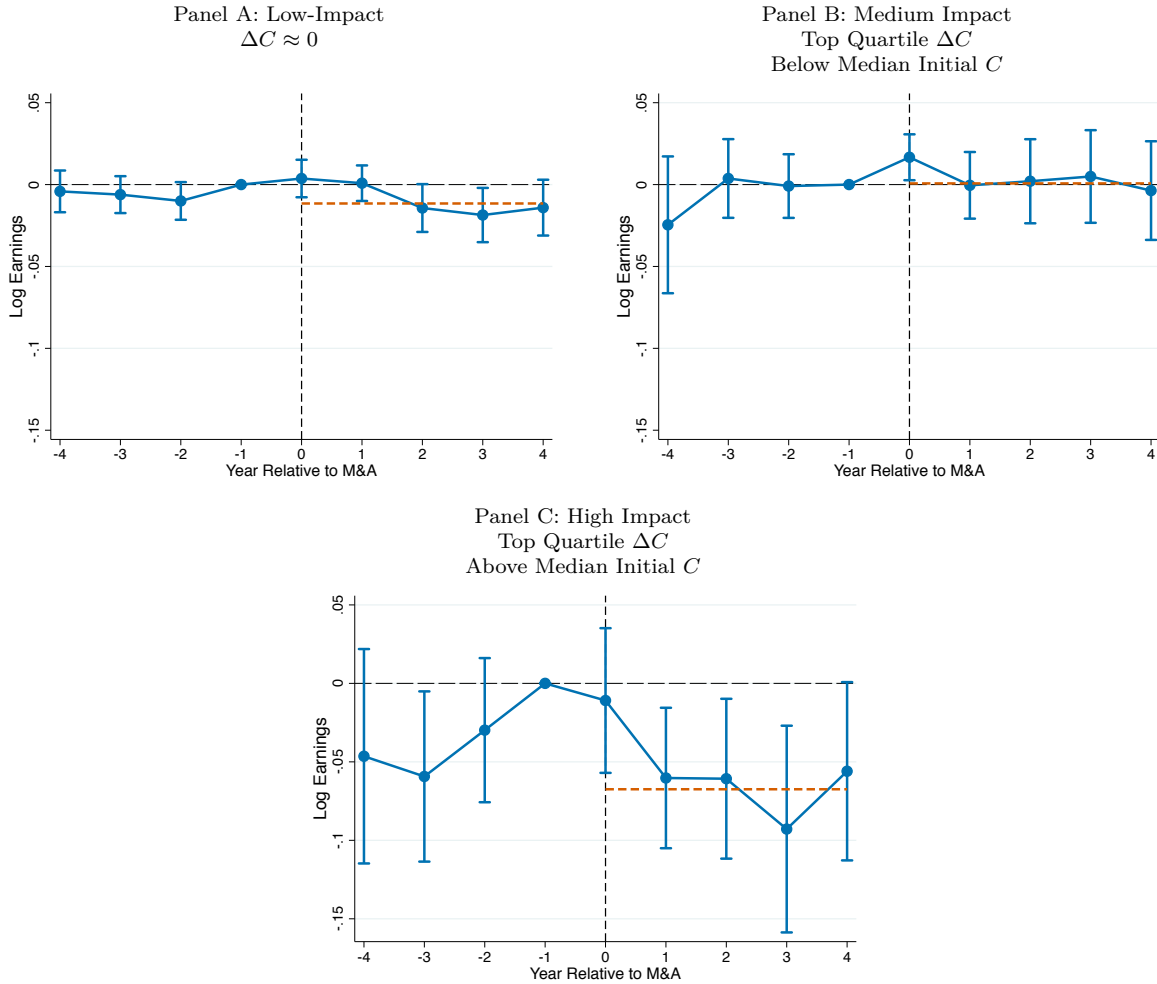
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment in tradable industries, which are defined as industries that belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. For details on the matching algorithm used to identify control establishments appear in the notes to Table 1 and Section 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 based on standard errors two-way clustered at the 4-digit NAICS and commuting zone level are displayed.

Appendix Figure A8: 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 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 4 and Section 3.4. 95 percent confidence intervals based on standard errors two-way clustered at the worker and 4-digit NAICS by commuting zone level are displayed.

Appendix Figure A9: 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 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 3.4. 95 percent confidence intervals based on standard errors two-way clustered at the worker and 4-digit NAICS by commuting zone level are displayed.

Appendix Table A1: Job Transitions Within Industries and Occupations in the CPS

	Industry 4-digit	Occupation 4-digit	Occupation 3-digit
	(1)	(2)	(3)
Within	0.366	0.323	0.349
Between	0.634	0.677	0.651
Observations	22,639	22,639	22,639

Note: This table uses data from the CPS (1995-2014) to compute the probability that a job transition is within occupations and industries vs. between occupations and industries. Among this sample of job switchers that have non-missing industry and occupation values, the total number of unique 4-digit industries is 474, the total number of unique 4-digit occupations is 904, and the total number of unique 4-digit occupations is 493.

Appendix Table A2: Robustness: Impact of M&A on Log Annual Earnings for Firm Stayers by Concentration Changes

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

Appendix Table A3: 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.

Appendix Table A4: 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)
Log C \times National	-0.262*	-0.258**	-0.309*		
	(0.146)	(0.128)	(0.176)		
Log C \times Tradable				-0.331*	
				(0.180)	
Log C \times Nontradable				-0.202**	
				(0.102)	
Log C \times Tradable \times High C					-0.392*
					(0.216)
Log C \times Tradable \times Low C					0.048
					(0.096)
Log C \times Non-tradable \times High C					-0.235**
					(0.117)
Log C \times Non-tradable \times Low C					0.051
					(0.121)
Market-Years	24,000	24,000	24,000	24,000	24,000
4-digit NAICS-by-CZ FE	Yes	Yes	Yes	Yes	Yes
1-digit 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.

Appendix Table A5: Industries Ranked by Labor Market Concentration Measures

<i>Panel A: Ordered by Flows-Adjusted Local Concentration</i>		
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
<i>Panel B: Ordered by HHI (4-digit NAICS by commuting zone)</i>		
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. In Panel A, local labor market concentration is measured using the flows-adjusted concentration measure that adjusts for cross-industry labor mobility. In Panel B, local labor market concentration is measured using a standard Herfindahl-Hirschman Index measured at the 4-digit NAICS by commuting zone level.

Appendix Table A6: 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.

Appendix Table A7: Across-Market Correlation between Employment and Local Concentration

	Log HHI CZ-by-industry	Log C
	(1)	(2)
Log Employment	-0.284*** (0.002)	-0.038*** (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.

Appendix B: Data Appendix

B.1 Longitudinal Business Database

B.1.1 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.

B.1.2 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 12th.

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.

B.2 Constructing Firm Growth Rates in the LBD

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

$$g_{jt} = \frac{E_{j,t} - E_{j,t-1}}{\frac{1}{2}(E_{j,t} + E_{j,t-1})} \quad (31)$$

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 $\frac{50}{200} = 0.25$.

B.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).

B.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.

Appendix C: Model Appendix

C.1 Derivation of Market-Level Wage in Cournot Model

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

$$\frac{1}{\eta_j} = \frac{\partial w_m(L_m)}{\partial l_j} \frac{l_j}{w_m(L_m)} = \frac{\partial w_m(L_m)}{\partial L_m} \frac{\partial L_m}{\partial l_j} \frac{l_j}{w_m(L_m)} \frac{L_m}{L_m} = \frac{s_j^l}{\eta_m} \quad (32)$$

Plugging η_j into Equation (2) and rearranging yields:

$$\frac{\theta_j}{w_m} = \frac{s_j^l}{\eta_m} + 1 \quad (33)$$

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:

$$\sum_j s_j^l \frac{\theta_j}{w_m} = \sum_j s_j^l \left(\frac{s_j^l}{\eta_m} + 1 \right) = \frac{HHI}{\eta_m} + 1 \quad (34)$$

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:

$$w_m = \left(\frac{\eta_m}{HHI + \eta_m} \right) \theta_m \quad (35)$$

C.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, $\alpha_{m \rightarrow k} = 0$ 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, $\alpha_{m \rightarrow k} = 1$ 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:

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

□

C.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:

$$Y_j = \Omega_j (l_j - f_j)^\alpha k^\beta \quad (36)$$

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:

$$\Omega_j \frac{\partial F}{\partial l_j} = \Omega_j \alpha (l_j - f_j)^{\alpha-1} k^\beta \quad (37)$$

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:

$$\frac{\partial \Omega_j \frac{\partial F}{\partial l_j}}{\partial f_j} = \Omega_j \alpha (\alpha - 1) (l_j - f_j)^{\alpha-2} k^\alpha \left(\frac{\partial l_j}{\partial f_j} - \frac{\partial f_j}{\partial f_j} \right) = 0 \quad (38)$$

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. (2020), 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.

C.4 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:

$$w = \alpha + \beta L \quad (39)$$

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:

$$\theta - \beta l_j - (\alpha + \beta L) = 0 \quad (40)$$

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

$$L = \frac{N}{N+1} (\theta - \alpha) \quad (41)$$

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

$$\pi_j = \frac{(\theta - \alpha)^2}{(N+1)^2} \frac{1}{\beta} \quad (42)$$

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:

$$N^* = \frac{\theta - \alpha}{\sqrt{\beta F}} - 1 \quad (43)$$

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:

$$\frac{\theta - w}{w} = \frac{\sqrt{\beta F}}{\sqrt{\beta F} + \alpha} \quad (44)$$

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.

C.5 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:

$$\max_{l_j, w_j} [l_j w_j + (\bar{l}_j - l_j)v - \bar{l}_j v]^{\gamma_j} [p_j(F(l_j))F(l_j) - w_j l_j]^{1-\gamma_j} \quad (45)$$

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

$$w_j = \gamma_j \left(\frac{p_j(F(l_j))F(l_j)}{l_j} - v \right) + v \quad (46)$$

$$F'(l_j)p_j(F(l_j)) \left(\frac{1}{\varepsilon_j} + 1 \right) = v \quad (47)$$

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.

Appendix D: 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 Table A5, I report the average (employment-weighted) concentration level for different aggregated sectors and then rank them from least to most concentrated. Panel A measures concentration using the flows-adjusted concentration measure while Panel B uses the standard HHI measured at the 4-digit NAICS by commuting zone level.

As can be seen in Appendix Table A5, 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 14th least concentrated according to flows-adjusted C , but 10th according to HHI . Similarly, mining is ranked 18th least concentrated according to flows-adjusted C , but 14th according to HHI . Management of businesses is ranked 5th least concentrated according to C , but 9th 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 A6 reports the probability a job transition is within the same 4-digit NAICS code for the same broad industry groupings as in Appendix Table A5.

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 A7 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.