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

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Abstract

I use matched employer-employee data from the U.S. to estimate both the direct and market-level effects of mergers and acquisitions (M&As) and resulting 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 measure substitutability across industries. Earnings fall by 3.1 percent for M&A workers in mergers that cause significant increases in local labor market concentration in already concentrated markets, with negligible impacts in other types of mergers. Additionally, mergers generating the largest concentration changes generate negative spillovers on other firms in the same labor market.

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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., 2022; 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 as 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 illustrate a Cournot model of the labor market that links measures of concentration to labor-market outcomes. 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 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

[1] In many models of monopsony power (e.g. Card et al. (2018); Berger et al. (2022)), 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 and le Maire, 2025).

workers routinely move to jobs in different industries.

In the second part of the paper, I estimate the direct impact of M&A on workers using a difference-in-differences design that compares outcomes for M&A workers to a matched control group before and after an M&A event. To identify M&A events, I use enterprise-level identifiers in the Longitudinal Business Database (an establishment-level panel for the U.S.) to discern when establishments change ownership.[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 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 have positive predicted impacts 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. I find these patterns to be consistent whether concentration is measured using standard Herfindahl-Hirschmann Index (HHI) at the industry-by-commuting zone, or whether it is measured using the flows-adjusted concentration measure. Therefore, while theoretically substitutability across markets may be an important consideration, I find both measures are successful in predicting heterogeneity in impacts.[3]

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. In contrast to this prior literature, 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 that the largest (top-ventile) of merger-induced concentration changes cause decreases in market-level earnings. Average earnings fall by about 3.3 percent in these top-ventile markets

[2]There are some complications that arise by using this method to identify ownership changes which deal with how the Census classifies single-unit vs. multi-unit firms and is discussed in Section 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 and le Maire (2025) with Danish administrative data.

[3]In practice, industries that a standard HHI predicts to be highly concentrated also tend to be the most concentrated industries according to the flows-adjusted measure.

relative to other markets. Importantly, this analysis excludes merging firms, implying that the effect is entirely driven by impacts at *other* firms competing in the same labor market. Therefore, changes in productivity or management practices at merging firms cannot explain the presence of these impacts. Using a top-ventile change as an instrument for concentration yields an elasticity of earnings with respect to local concentration equal to -0.22, with a qualitatively similar estimate when replacing the flows-adjusted concentration measure with a standard HHI measure of concentration. This estimate is consistent in a sample of tradable industries as well as for national mergers. The point estimate is similar in magnitude to recent work that finds elasticities from -0.01 (Hershbein et al., 2018) to -0.28 (Qiu and Sojourner, 2023).[4]

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., 2021) or increases in non-competes (Starr et al., 2021; Balasubramanian et al., 2022; Lipsitz and Starr, 2022) and no-poaching agreements (Krueger and Ashenfelter, 2022; 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.[5] 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

[4] The estimates in both of these papers vary somewhat depending on the exact specification. Azar et al. (2022) estimates an elasticity between these two, equal to -0.127

[5] This comparison comes with a number of caveats that are discussed in detail in Section 6.5.2. 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.

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; Cooper et al., 2019; Dafny et al., 2019).

Second, this paper contributes to a smaller literature that studies the impact of M&A on workers. Brown and Medoff (1988) find that acquisitions in Michigan result in lower wages and increased employment. Siegel and Simons (2010) studies M&A in Sweden and finds increases in productivity but decreases in employment. He and le Maire (2025) 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. This is consistent with Shleifer and Summers (1988) who argue that M&A events will reduce wages if managers are replaced after a takeover. The key distinctions between this paper and He and le Maire (2025) is that I additionally focus on market-level effects of merger activity. These market-level effects are not predicted by models in which negative wage losses are driven by within-firm reorganizations after an M&A event, as in Shleifer and Summers (1988). 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.

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., 2020; Kline et al., 2019; Goolsbee and Syverson, 2023; 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., 2022; Benmelech et al., 2020; Hershbein et al., 2018; Rinz, 2020; Qiu and Sojourner, 2023). 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., 2024) or obtain a measure of outside options (Caldwell and Danieli, 2024) 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 Imperfect Competition in the Labor Market

In this section, I present a Cournot model of the labor market that clarifies the channels through which M&A events could impact the labor market. I then discuss assumptions that can be maintained to disentangle these channels. I conclude by extending the standard model to allow for a data-driven approach to measuring labor-market concentration.

2.1 Cournot Model of the Labor Market

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). The use of HHI is extremely common in antitrust evaluation of mergers (Naidu and Posner, 2022).[6] The main text focuses on a Cournot model of the labor market as this is a simple and influential model that has been used to link labor-market outcomes with concentration. Additionally, as discussed in Naidu and Posner (2022), it is relatively flexible, allowing for different types of extensions, including a flows-adjustment extension I discuss in the next section.

However, it does have some key limitations. Most notably, it is not an effective model of why firms merge in the first place, as those who benefit most are actually competitors. However, 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 search model (Jarosch et al., 2024), or a discrete choice model with firm differentiation (Berger et al., 2022). Azar and Marinescu (2024) provides a review of various models utilized in the literature on monopsony power, in particular linking many of these to measures of concentration.

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

[6] This model is used in Krueger and Ashenfelter (2022) to explore potential impacts of restrictions on hiring from other units within a franchise.

commuting zones. For now, I ignore substitutability across industries within a commuting zone although this will be incorporated in measuring labor market concentration in the next section.

An individual firm chooses l_j to maximize total profits:

$$\pi_j = R_j(l_j) - w_m(L_m)l_j, \tag{1}$$

where $R_j(l_j)$ is the revenue of firm j when employing l_j workers. This function in general may depend on product-market parameters, such as the degree of product-market competition, as well as productivity of firm j. Taking the first-order condition with respect to l_j yields:

$$\theta_j - \frac{\partial w_m(L_m)}{\partial l_j} l_j - w_m(L_m) = 0 \tag{2}$$

Given $L_m = \sum_j l_j$, $\frac{\partial w_m(L_m) l_j}{\partial l_j} = \frac{\partial w_m(L_m) l_j}{\partial L_M}$. Dividing by w_m and rearranging yields:

$$\frac{\theta_j}{w_m} = \frac{\partial w_m(L_m)}{\partial L_m} \frac{l_j}{w_m} \frac{L_m}{L_m} + 1 \tag{3}$$

The market-level elasticity of labor supply is denoted η_m and is equal to $\frac{\partial w_m(L_m)}{\partial L_m} \frac{L_m}{w_m}$. Therefore rearranging further yields:

$$\frac{\theta_j}{w_m} = \frac{s^l_j}{\eta_m} + 1, \tag{4}$$

where s^l_j is the share of labor in market m that is employed by j. Finally, summing up all N firms first-order conditions yields:

$$\sum_j s^l_j \frac{\theta_j}{w_m} = \sum_j s^l_j \left(\frac{s^l_j}{\eta_m} + 1 \right) = \frac{HHI}{\eta_m} + 1 \tag{5}$$

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

$$w_m = \left(\frac{\eta_m}{HHI + \eta_m} \right) \theta_m \tag{6}$$

In this model, wages are marked down relative to the average marginal product θ_m in the market. The markdown depends on two factors. First, as the elasticity of labor supply increases ($\eta_m \to \infty$), wages converge to marginal product. Intuitively, if workers are not tied to this particular market, then even small decreases in wages will generate large declines in the number of workers, incentivizing firms to pay a wage equal to the average marginal product of workers. The markdown also depends on overall concentration in the market (HHI). As concentration increases, wages decrease (conditional on η_m and θ_m). In the next section, I discuss how the various parameters of this wage equation may change in response to merger activity, and what variation in the data will prove useful for disentangling channels.

2.2 Potential Impacts of M&A Events

Product-Market Impacts: In this model, if product market power increases, then firms will restrict quantities to increase price. The reduction in quantity produced will lead to an overall lower level of employment (assuming fixed productivity). Given $w_m(L_m)$ is increasing in L_m, this will imply an overall lower level of wages in the labor market.

One important point to keep in mind, however, is that alternative models of wage setting may yield different predictions. For example, in Appendix C.1, I illustrate a simple wage bargaining model that has the opposite prediction. In that model, wages depend on the surplus at the firm. If profits increase, but employment levels fall, then the wages for the workers that remain employed will increase, as the total (higher) surplus is now split between fewer workers.

In either case, the important point to take away is that changes in product-market power will have impacts on wages, even absent monopsonistic impacts. Therefore, when turning to empirics, it will be important to disentangle these two channels.

Productivity Impacts: A common justification for mergers is the possibility of increased productivity. For example, Braguinsky et al. (2015) finds evidence of increased productivity in the Japanese cotton spinning industry after acquisitions, while Blonigen and Pierce (2016) finds little evidence of increased productivity in manufacturing acquisitions in the U.S. Additionally, prior work has found that misaligned empire-building incentives (Jensen, 1986) or CEO overconfidence (Malmendier and Tate, 2005) may drive M&As, suggesting that M&As could actually destroy value and lower productivity. Therefore, overall, the impact of M&As on productivity is ambiguous. Again, a key takeaway here is that many firm-specific factors could impact wages after a merger absent monopsonistic impacts.

Monopsonistic Impacts: Lastly, changes in labor-market competition due to the merger will impact wages. This can be seen clearly by the fact that the market wage depends on the level of concentration in the labor market, HHI. Additionally, while the model is implicitly assuming η_m is a fixed feature of the market, one could imagine that η_m is determined by other factors that determine the level of monopsony, such as search costs and market regulations. If these factors are also impacted by M&A, resulting in a decrease in η_m, then this will also lead to lower market wages.

A final channel that is related to monopsonistic impacts, but conceptually distinct, is changes in bargaining power for workers after a merger. For example, Shleifer and Summers (1988) propose that M&A may lower wages as high-wage managers are replaced, a finding that is supported in He and le Maire (2025).

2.2.1 Disentangling Impacts

Within-Firm vs. Market-Level Effects: The analysis of the impacts of M&A is composed of two separate empirical designs: effects on workers within M&A firms and market-level effects that exclude workers directly employed by the M&A firms. These two separate analyses are important not only to understand the total impacts of M&A on the labor-market, but also to understand the channels through which they arise. For example, many of the productivity channels would be unable to rationalize market-level declines in earnings. In particular, impacts due to ownership changes, such as those emphasized in the corporate finance literature, would be unable to explain why other firms in the same labor market alter wages. Therefore, existence of market-level effects will be used to eliminate some alternative stories for observed wage declines, such as breach of trust in hostile takeovers discussed in Shleifer and Summers (1988).

Tradable vs. Nontradable Industries: To disentangle wage effects due to product market competition and labor market competition, I compare differences between firms that sell tradable vs. nontradable goods. The logic is that firms that sell highly tradable goods are close to perfectly competitive, and therefore, a single merger is unlikely to have large impacts on product 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.

Changes in Labor-Market Concentration: Lastly, I consider how effects of mergers vary by the change in concentration in the local labor market. Before discussing this in detail, it should be noted that the recent literature on monopsony power in labor markets has generally used interactions between industry and region or occupation and region to define labor markets. 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.

Given the ambiguity regarding the appropriate market definition, the next section extends 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. After the new measure is constructed I discuss how variation in the effects of mergers by changes in predicted concentration will be an important component of the empirical analysis.

2.2.2 Incorporating Substitutes to Calculate Concentration

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

How should $V(k|m)$ be measured in practice? I argue that endogenous flows across markets are helpful in measuring $V(k|m)$ in the data, similar to Sorkin (2018) who uses flows between firms to estimate the value of a given firm.[7] 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 market-specific rate λ_m. When a job arrives, the worker must decide whether to remain in the current job or move to the new job. The probability we observe a worker from m move to a job in market k is given by:

$$P(k|m) = \lambda_m \cdot f_k \cdot Pr(k \succ m), \tag{7}$$

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

$$\frac{P(k|m)}{P(m|m)} = \underbrace{\frac{f_k}{f_m}}_{\text{relative offers}} \cdot \underbrace{\frac{V(k|m)}{V(m|m)}}_{\text{relative values}} \tag{8}$$

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 (8):

[7] The goal here is to understand what firms within a commuting zone are competing against each other. To do so, I use flows across industries to determine which industries compete against each other. Theoretically, one could use bilateral flows to measure competition between two firms, however the job-to-job network at the firm-level is quite sparse, making these competition measures likely poorly estimated. Sorkin (2018) uses a recursive algorithm similar to Google's page rank algorithm to identify an absolute ranking of firm's, however, this approach doesn't necessarily identify which firms compete with one another in the labor market.

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{f_m}{f_k} \tag{9}$$

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:

$$V(k|m) = \frac{P(k|m)}{P(m|m)} \cdot \frac{L_m}{L_k} \tag{10}$$

Note that everything on the right hand side of Equation (10) can be measured with data on job-to-job flows and industry employment. Going forward, I denote the estimated value in Equation (10) as $\alpha_{m\to k}$ to distinguish it from the theoretical object, $V(k|m)$. We can now substitute in for $V(k|m)$ in order to write the wage in market m as as a function of observables:

$$w_m(\bar{V}_m) = w_m(\sum_{k=1}^{M} \alpha_{m\to k} L_k) \tag{11}$$

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

$$\tilde{s}_j = \frac{l_j}{\sum_{k=1}^{M} \alpha_{m\to k} L_k} \tag{12}$$

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

$$\tilde{w}_m = \tilde{\theta}_m + ln\left(\frac{\eta_m^\alpha}{C + \eta_m^\alpha}\right), \tag{13}$$

where $C = \sum_j \tilde{s}_j^2$ is defined as the flows-adjusted concentration measure, η_m^α is equal to $\frac{\partial w_m}{\partial \alpha L}\frac{\alpha L}{w_m} \cdot s_m$, where $\alpha L = \sum_{k=1}^{M} \alpha_{m\to k} L_k$, and $s_m = \frac{L_m}{\alpha L}$.[8] η_m^α is similar to a standard market-level elasticity of labor supply, but adjusted for worker flows out of the market. If there are no flows, then $s_m = 1$ and $\alpha L = L_m$, so that we get back to the standard model above.

This wage equation implies that mergers that generate larger changes in concentration will yield larger changes in wages (conditional on both the initial level of concentration and η_m^α). Similarly, given an equal-sized concentration, the merger that occurs in the more concentrated market will again yield larger impacts on wages. These two observations will be key in the empirical results, which will emphasize heterogeneity with respect to both the change in concentration due to a merger, as well as the initial concentration level. They are also reflected in the Horizontal Merger guidelines, where mergers that raise concentration in already concentrated markets are the most likely to face antitrust scrutiny.

[8] Appendix C.2 details how to derive this equation.

2.3 Relationship to IO Literature and Wage-Concentration Regressions

A recent literature finds a robust negative relationship between local labor market concentration and wages.[9] As discussed in Berry et al. (2019) and Syverson (2019), 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.

To address this 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. While I argue concentration is a valuable measure to use to predict anticompetitive impacts, it is not the only potential tool available. For example, Naidu et al. (2018) introduces a notion called downward wage pressure (DWP), which captures how workers from one firm will quit in response to a small change in wage at another firm. The DWP approach uses a concept known as diversion ratios, which conceptually could be estimated using information on how flows across firms respond to changes in wage differentials across firms.[11] A final tool available to researchers is a fully specific demand and supply system that would allow for a merger simulation.[12].

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

[9] See Azar et al. (2022); Benmelech et al. (2020); Hershbein et al. (2018); Rinz (2020); Lipsius (2018) among others. An older literature (Weiss, 1966) studies the impact of product-market concentration on labor market earnings.

[10] Benmelech 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.

[11] Appendix C.3 shows that flows across markets are informative of diversion ratios if the decision of which market to enter is given by a logit choice model.

[12] See Budzinski and Ruhmer (2010) for a review of merger simulation in competition policy

on product market competition.[13] In general, however, most of the deals that the FTC and DOJ do get notified about are allowed to proceed without interference. Figure A1 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 during this time period, about 1.9 percent of all notifications face some enforcement from antitrust authorities.[14]

While increases in labor-market power due to merger activity has traditionally not been a concern of antitrust authorities, 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. This was reflected in the most recent version of the Horizontal Merger guidelines released in 2023, which explicitly has sections on labor markets. However, to date, there have been limited cases in which the labor market is an important consideration in blocking a merger.

While no merger has been ever been challenged solely due to a predicted increase in labor market power, employers have been charged with anticompetitive practices in labor markets. For example, in 2017, a number of animation studios including Disney, Pixar, Dreamworks, Sony and 20^{th} Century Fox Animation were sued for agreeing not to poach workers from each other. The studios settled and agreed to pay $160 million USD to the impacted employees. Since the settlement, both Pixar and 20^{th} Century Fox Animation have been purchased by Disney. Therefore, any wage suppression that occurred due to the no-poach agreement between these firms would be completely legal, as these firms are all owned by the same parent company.[15]

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.

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

[14] Author's calculation derived from *Hart-Scott Rodino Annual Reports* which reports statistics on merger enforcement actions at the DOJ and FTC.

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

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.[16] 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).[17]

One complication that arises in the data is that when a single-unit establishment opens a second establishment, a brand new enterprise-level identifier is issued. In other words, there are cases in which enterprise-level identifiers change other than ownership changes. However, given single vs. multi-unit status is an included variable in the LBD, I drop these transitions from the sample and do not code them as M&A events.

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

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 A2), which

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

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

comprise about 53 percent of the total population in the United States as of the 2010 Census.[18]

The main outcome variable used in the worker-level results is log annual earnings which is aggregated across all employers. One important point is that I do not observe hours in the dataset, therefore changes in earnings may stem from changes in either hours or wages. While earnings across all employers are included, I associate workers with a "dominant" employer (i.e. the employer for which the worker earns the highest amount of 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.[19] Next, I use worker flows between firms to capture reorganization events that are likely not true transitions, following Benedetto et al. (2007). For example, firms becoming incorporated may change tax identifiers. In practice, if more than 60 percent of the workers in a firm transition to the same firm in the next year, then I do not code any of these transitions as a job transition.

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

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

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

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

3.3 Concentration Measurement

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

While, in theory, transition rates across industries may change, I instead choose to pool the entire sample (1995-2014) in order to retrieve a consistent and more precise measure of $\alpha_{m\to k}$ for every pair of industries m and k. To compute C in practice I make two modifications to the formula in Section 2.2.2. 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.[20] In practice, I compute the share of firm j (denoted $\tilde{s}_{jmc}$) in industry m in commuting zone c as:

$$\tilde{s}_{jmc} = \frac{\sum_{k\in c} \alpha_{m\to k} l_{jkc}}{\sum_{k\in c} \alpha_{m\to k} L_{kc}} \tag{14}$$

where

$$\alpha_{m\to k} = \frac{P(k|m)}{P(m|m)} \frac{1}{\mathbb{E}[\frac{L_k}{L_m}]} \tag{15}$$

These are modified versions of Equations (12) and (10), respectively. First, the numerator of the market share is now a weighted total employment of firm j, indicating that firm j may hire workers in multiple industries. If jobs in industries m and k are relatively substitutable, then the market share of j in industry m will also depend on the number of workers employed in industry k. If firm j employs a large number of workers in market k, then this will increase firm j's total share of market m.

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

[20] Industry is defined at the establishment-level. A firm with two or more establishments with different industry codes is defined as a multi-industry firm.

substitutability, but rather they are generally located in different areas.

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

$$C_{mc} = \sum_{j \in c} (\tilde{s}_{jmc})^2 \tag{16}$$

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. For some results, I split the sample by acquiring vs. target establishments. To understand how this is defined, consider an M&A event between two firms, one with enterprise identifier A and the other enterprise identifier B. If, after the event, both firms are listed as having enterprise identifier A, then establishments belonging to A before the event are acquiring establishments, while establishments belonging to B are target establishments. If both identifiers disappear from the sample (which is rare), and all establishments are listed under a new enterprise identifier, then I consider both firms as target firms for the purpose of the heterogeneity analysis.

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 establishments with sufficient pre-period observations and eliminates a considerable number of small M&A events (50,000).[21] While there could be potentially large effects on target workers in these acquisitions, the focus of this paper is on potential anticompetitive effects by considering how impacts vary by changes in local concentration. Small mergers will mechanically have small impacts on concentration and may be very different than mergers between large firms of similar size.

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

[21] Note if a firm has multiple establishments, this restrictions drops establishments with less than 50 workers, but keeps establishments with more than 50 workers.

checks, I also consider smaller size threshold (>10) workers and dropping the final restriction that the firms are similar in size.[22]

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 and Heining, 2022; Goldschmidt and Schmieder, 2017; Smith et al., 2019; Jaravel et al., 2018; He and le Maire, 2025). 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.[23] 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.[24] I construct a balanced panel of establishments which extends 4 years prior to the merger and 4 years after the merger. The main establishment level-outcomes are employment, which is equal to March 12^{th} employment.

To construct the worker-level sample, I extract all workers that were employed in the M&A firms in the two years prior to the M&A event. This tenure restriction is chosen to obtain a sample of workers with attachment to the M&A firm and is similar (though shorter) than tenure restrictions used in the mass layoff literature (Jacobson et al., 1993; Schmieder et al., 2023; Lachowska et al., 2020). 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.[25] Again, if more than

[22] Appendix Table A2 summarizes these sample restrictions, with Panel B showing the associated number of workers in the main analysis sample vs. an analysis sample that relaxes some of the restrictions.

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

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

[25] In particular, this analysis matches on the earnings of the individuals in the year prior to the M&A event.

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 1 plots the number of workers employed in the M&A establishment sample over time on the left axis. The number of workers employed in the M&A sample establishments fluctuates widely over time, with a high of 1.5 million to a low of 0.5 million, with merger activity being somewhat procyclical. I also plot the number of M&A deals in the Thomson Reuters (SDC) database of Mergers & Acquisitions, a high-quality database that contains information on merger activity in the United States as well as characteristics of merger deals. As can be seen in Figure 1, the two time-series line up reasonably well. One important note, however, is that the M&A establishment sample from the LBD does make restrictions by eliminating small acquisitions and partial acquisitions and therefore is a subset of the total number of workers impacted by ownership changes. Ignoring these sample size restrictions and instead including all potential mergers, I find that 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. (2022) 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

Specifically, workers are matched within deciles of earnings. Additionally, earnings are also included as an input into the propensity score in this alternative matching procedure.

make up the bulk of the tradable industries.[26] 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 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.[27] 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.[28]

4 Research Design

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

$$Y_{jt} = \sum_{k=-4}^{4} \delta_k^{MA} \mathbb{1}(t_j = t^* + k) \times MA_j + \psi_j + \tau_t + u_{jt} \tag{17}$$

where Y_{jt} is an outcome variable, 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

[26] 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.

[27] All earnings are adjusted to 2011 dollars.

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

M&A event and u_{jt} is an error term.[29]

To estimate worker-level impacts, I estimate a similar matched difference-in-difference design of the following form:

$$y_{it} = \sum_{k=-4}^{4} \delta_k^{MA} \mathbb{1}(t_i = t^* + k) \times MA_i + \omega_i + \tau_t + u_{it} \tag{18}$$

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 (17). 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, the specifications above do not suffer from the identification issues that arise in conventional difference-in-differences designs with staggered timing (Goodman-Bacon, 2021). The approach here is analogous to the stacked design in Cengiz et al. (2019) that avoids these identification issues. Identification here comes solely from differences in treated units and matched control units over time, not from units coming in and out of treatment. One important point, however, is that I do not require that control establishments are never acquired in the future. Indeed, future M&A activity may be an important impact of current M&A activity, so it is important to allow for this possibility. However, if this occurs, then this firm will appear twice in the data, once as a control unit, and another time as a treated unit. This avoids the identification issues that stem from staggered timing while also allowing one to empirically study whether current M&A impacts the probability of a future M&A event.

4.1 Identifying Assumptions

The key identifying assumption is that outcomes for M&A establishments and workers would follow similar trajectories to control establishments and workers in the absence of a merger. This may be a strong assumption in this setting, as mergers are the result of endogenous decisions by firms. For example, acquiring firms may selectively target firms that will be profitable in the future. In this case, wages in the firm may grow even absent of the merger. Therefore, the estimate of the impact of M&A on 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 earnings at target firms would have been even greater in the absence of the merger.

[29] As shown in Schmieder et al. (2023), 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. (2023).

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. For the worker-level results, I do not match on lagged earnings (the primary outcome), allowing for a transparent test of parallel trends, though strategies that do match explicitly on earnings yield results with similar sign and significance.[30]

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 2 plots $\hat{\delta}_k^{MA}$ from estimating Equation (17) 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. Appendix Table A3, Appendix Table A4 and Appendix Figure A3 show that the main effects are robust across a variety of deviations from the main specifications. Additionally, Appendix Figure A4 shows the impact of current M&A activity on future M&A activity, finding an initial spike at the timing of the initial event, and a small increased likelihood of M&A activity in years 2 and 3 after an M&A event which then fades away. Overall, the magnitudes imply that the effects are driven primarily by a single M&A event, rather than multiple events that happen in sequence.

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

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.

In Panel of A Figure 3, I plot $\hat{\delta}_k$ from estimating Equation (18). As can be seen in the figure, 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) (See Appendix Table A5). 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. Given hours are not observed in the data, this could also be driven by a decrease in hours, rather than a decrease in wages. 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. Panel B of Figure 3 plots the estimates of Equation (18) with an indicator for a job transition as the outcome. The year after the merger, job transitions spike, with M&A workers 10 percentage points more likely to switch jobs relative to control 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.

Given workers are more likely to transition from M&A firms, one potential concern is that differential attrition could bias the results. For example, it could be that some workers in M&A firms are able to find better jobs out of the LEHD sample, which can't be captured in the current analysis. To test this, Panel C of Figure 3 estimates the impact of M&A on an indicator for having any positive earnings. If M&A workers were more likely to exit the sample, then this figure would show a decline in the probability of having any positive earnings. However, as can be seen in the figure, while there is a slight marginally insignificant decline two years after the event, the overall impact is close to zero. Additionally, the magnitudes are very small, at less than a half a percentage point decrease in the probability of any positive earnings. Therefore, in practice, it does not appear that M&A causes workers to either be completely unemployed for a year or exit the sample due to movement across states.

To study the impacts solely due to within-firm changes in compensation, Panel D of Figure 3 restricts the analysis to firm stayers, who are workers that stay in the same firm in the years following the merger. This same restriction is applied to control workers, where the year of the merger for a control worker is equal to the year of the merger of the matched treated worker. 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. One can conceptualize the stayer analysis as estimating the change in firm-specific wage premia for firms that undergo M&A events. Intuitively, under the framework of Abowd et al. (1999), workers earnings are linear functions of individual fixed effects, firm effects, and year effects that are potentially interacted with worker characteristics. The matching strategy controls for individual fixed effects as well as worker characteristics-by-year effects through matching to control workers, leaving changes in firm effects after mergers as the remaining source of earnings variation over time. This approach is similar to Schmieder et al. (2023) and Dhyne et al. (Forthcoming), both of which study how firm-level shocks translate to worker earnings, using samples of stayers to adjust for changes in worker composition.

Log annual earnings for firm stayers in M&A firms decrease by -0.008 (SE=0.003), similar to the overall impact of -0.011. Appendix Table A6 presents additional results that study how the impacts of M&As depend on worker characteristics. Column 1 presents a difference-in-differences specification that allows for heterogeneity in effects by age. As can be seen in this column the impact is highest for older workers (age above 50). In column 1, all workers are included implying these impacts can be driven by either job displacement or within-firm earnings decline. However in Column 4, the difference-in-differences analysis restricts to firm stayers. Again, workers that are older than 50 suffer the largest impacts, with log earnings declines of -0.010 (SE=0.004). Turning to heterogeneity by within-firm earnings, I find in Column 2 of Appendix Table A6 that individuals with the highest earnings (quintile 5 of the within-firm earnings distribution) suffer the largest losses. While high-wage workers suffer the largest earnings losses, this stems from the fact that they are more likely to transition jobs after an M&A event, as shown in Column 2 of Table A7. However, conditional on remaining within the firm, there is limited heterogeneity in earnings impacts (Column 5 of Appendix Table A6). Lastly, Columns 3 and 6 present results by worker sex, finding similar results for male and female workers.

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 2.2.2, 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 in 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 4, 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 3 show that the differences between high-impact mergers and the other types of mergers are statistically significant.

These results so far utilize the flows-adjusted concentration measure. This adjustment impacts both the size of the change in concentration due to a merger as well as the initial concentration level. Therefore, it is possible that a standard HHI is unable to predict heterogeneity in impacts due to a merger. However, in practice, I find standard HHI measures based on 4-digit NAICS-by-CZ yield similar results. In particular, Figure A5 estimates the same heterogeneity as Figure 4, but instead uses the change in HHI and the initial HHI in order to split the sample by low, medium and high-impact mergers. As can be seen in Figure A5, the results are remarkably similar. In low-impact mergers, log annual earnings fall by -0.004 (SE=0.004). In medium-impact mergers log annual earnings fall by -0.015 (SE=0.007). In high-impact mergers log annual earnings fall by -0.040 (SE=0.013). The main difference between the two specifications is the initial level of concentration. For example, for high-impact mergers, the initial concentration using HHI is 0.222 (see Table A8), while for the flows-adjusted measure it is 0.072. However, in terms of predicting heterogeneity in impacts, there is little difference between the two.

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 spill over 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, Figure A6 and Panel B of Table 3 reports results for tradable goods industries, for which I assume there is no impact of a merger on product market power. In high-impact mergers, I continue to find an economically meaningful decline in log annual earnings of -0.067 (SE=0.023). In medium-impact mergers, I find a slight positive rise in log earnings of 0.001 (SE=0.012), while in low-impact mergers I find a marginally significant decline of -0.012 (SE=0.006). Columns 4-5 of Table 3 shows that the differences between high-impact mergers and other types of mergers are statistically significant.[31]

5.3.2 Robustness of Wage Losses Only in High-Impact Mergers

One concern is that I have eliminated many M&A events from the sample due to size restrictions. In the main sample, I restrict to firms that have 50 or more employees in the year prior to the M&A event. Additionally, I require target firms to be at least 10% as large as the acquirer. The goal of these restrictions is to focus on economically active firms for which an M&A event has the potential to increase labor-market concentration. However, there are many potentially reasonable sample restrictions.

To probe how this impacts the main results, Appendix Table A9 estimates the impact of M&As on stayers for low, medium and high-impact M&A events, but for an alternative sample of mergers. In this alternative sample, I drop the restriction that target firms need to be at least 10% as large as the acquirer and lower the size threshold to 10 workers instead of 50 workers. This increases the number of worker-year observations in the analysis sample from 25.7 million to 79.5 million. The main heterogeneity results, however, are qualitatively similar for this sample: no impact on incumbent workers earnings in low-impact or medium-impact M&A events, but a -0.019 decline in log annual earnings in high-impact mergers. The magnitude in the high-impact events is smaller than the main results (-0.019 vs -0.031) due to high-impact mergers being defined as a merger in the top quartile of predicted change in concentration in above-median concentration markets. In the expanded merger sample, the initial concentration level in the high-impact M&A events is lower (0.062 vs. 0.072) and the change in concentration is also lower (0.048 vs. 0.099). Therefore, if concentration changes are driving the results, we would expect to see smaller impacts here.

The primary remaining concern is that high-impact mergers may be correlated with some other aspect of the event that is decreasing earnings. First, 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. I find a very similar pattern of heterogeneity in these results in Figure A9 and Panel C of Table 3. Additionally, it is possible that mergers that increase concentration tend to be between firms that are much larger on average. Therefore, it is firm size driving the results, not necessarily monopsony power. Panel A of Appendix Table A10 estimates the effect on firms with above-median level of employment to restrict to relatively-large firms. I continue to find the same pattern of heterogeneity with respect to concentration. Lastly, while I argue the tradables industries limits the contribution of product-

[31] Note that the differences in wage impacts between low, medium and high-impact M&A is not due to differences in employment effects. In Appendix Figures A7 and A8 I estimate heterogeneity in employment effects by low, medium and high-impact M&A for nontradeables and tradeables respectively. In both cases I find similar employment effects in medium and high-impact M&A, but different wage impacts.

market power, it is possible that market power changes in very consolidated tradable industries. In Panel B of Appendix Table A10, 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 A10 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.[32]

However, the worker-level analysis has a few limitations. First, while I have provided evidence that is consistent with concentration being a contributing factor, there are other stories that could potentially rationalize the data. If productivity effects are negative and correlated with concentration, then this could also rationalize the findings. Additionally, hostile takeovers could result in breach of trust (Shleifer and Summers, 1988), as documented in He and le Maire (2025). If this breach of trust channel is correlated with concentration, then this would also predict the largest wage losses in high-impact mergers.

Ignoring potential endogeneity concerns, the worker-level results also likely understate the impact of mergers on workers for due to potential spillovers on non-merging firms hiring in the same labor market. Imperfect competition in the labor market implies wages will fall for all firms in the market, not just for the merging firms. In other words, the stable unit treatment value assumption may be violated and impacts will be attenuated toward zero. While I do find similar results utilizing a matching strategy that matches workers to different states, rather than within market (see Appendix Figure A10), a more direct way to probe this is to directly estimate spillovers.

Therefore, to address these limitations, I next proceed to the market-level analysis. The market-level results are complementary to the worker-level evidence by focusing on other firms competing in the same labor market. Therefore, changes in wages at merging firms due to the reorganization, such as productivity or breach of trust in contracts, will not be able to explain market-level declines in wages. Additionally, by focusing on other firms we can discern the total impacts of M&A events on the broader labor market. Lastly, these market-level results link the results directly to a recent literature that interprets negative correlations between local labor market concentration and market wages as evidence of imperfect competition in labor markets.

[32] I have additionally explored whether concentration is differentially predictive depending on the type of worker. For example, Prager and Schmitt (2021) finds impacts only for skilled nurses as hospitals after M&A events. I find high-impact mergers have larger impacts on older workers (above 50) and workers with above-median earnings, although differences between coefficients are not statistically significant.

6 Market-Level Impacts of Increased Concentration

6.1 Correlates of Concentration Changes

Before turning to market-level impacts of concentration changes, I first discuss prior approaches that seek to estimate the relationship between concentration and labor-market outcomes. In particular, I regress changes in labor market concentration on changes in market-level employment and earnings:

$$\Delta Y_{mt} = \beta \Delta \tilde{C}_{mt} + \tau_t + u_{mt} \tag{19}$$

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

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

$$\tilde{w}_{it} = \Phi_{mt} + \beta_t X_{it} + u_{it} \tag{20}$$

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.[33] 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 4 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., 2022; 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). Additionally, a standard approach in the literature is to use an instrument for concentration that is based on changes in the number of establishments in other locations within the same industry. In particular the average of $log(1/N)$, where N is the number of firms in the industry (Azar et al., 2022). When implementing this version, I find the elasticity of earnings with respect to flows-adjusted concentration measure equal to -0.120

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

(SE=0.072), again consistent with prior literature.

Column 3 of Table 4 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. However, instrumenting with $log(1/N)$ does flip the sign on the coefficient, implying increases in concentration are associated with decreases in employment, as standard monopsony theory would suggest.

There are two reasons why utilizing M&As as an instrument is potentially useful. First, in Appendix Table A11, I show that a large fraction of concentration changes are driven by factors such as entry and exit, which are often associated with changes in labor demand. Therefore, utilizing M&As as an instrument provides a novel source of concentration variation that may be less correlated with demand shocks. Second, using methods that use high-frequency variation in concentration make it harder to understand the dynamics of this process. For example, to understand outcomes in the present, it is unclear if contemporaneous concentration, lagged concentration, or multiple lagged concentration measures should be used. M&A events, instead, offer a way to observe a large discrete change in market-level concentration which makes it amenable to an event-student design. 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.

6.2 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 $\tilde{C}^{MA}_{mt}$. For the market-level results, I consider a broader class of M&A events which do not necessarily condition on firm size or stable positive employment in the years prior to the M&A event. This is due to the fact that this analysis does not compare M&A firms and workers to otherwise similar firms and workers, but instead compares markets that experience large concentration changes to markets that experience smaller concentration changes. Therefore, it is not necessary to have stable pre-event observations for M&A workers to test parallel trends for this sample.

I define a concentration event as a change in concentration of at least one percent.[34] For

[34]The reason positive changes below 0.01 are not considered "concentration events" is due to how the flows-

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.

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

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

$$\tilde{w}_{mt} = Post_{mt} \times \left[\sum_{b=1}^{4} s_b(\tilde{C}_m^{MA})\right] + \Phi_m + \tau_{t,k(m)} + u_{mt} \tag{21}$$

where $\tilde{w}_{mt}$ is the average log market wage obtained by first residualizing on worker observables, as described in Section 6.1.

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. Φ_m are labor-market fixed effects (i.e. 4-digit NAICS interacted with commuting zone), $\tau_{t,k(m)}$ are year fixed effects that potentially vary by some observable of the labor market m. The preferred specification interacts year fixed effects with consolidation year and 1-digit NAICS by state cells. Therefore, the impact of concentration on earnings is identified from two merger-induced concentration changes that occur in the same year, within the same state, same 1-digit industry, but have different magnitudes of 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 $\{s_b(.)\}_{b=1}^{4}$ is a set of basis functions defining a natural cubic spline with four knots. Following Harrell (2001), I place the knots at the 5^{th}, 35^{th}, 65^{th}, and 95^{th} percentiles of the distribution of concentration changes.[35] The "dose-response" function $d(x) = \sum_{b=1}^{4} s_b(x)$

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.

[35] As per Census restrictions, percentiles cannot be reported. Instead I report psuedo-percentiles, which correspond

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 5 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 95^{th} percentile of all predicted concentration changes. This implies only the top ventile of predicted concentration increases generate significant shifts in market-level wages.

6.4 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 A12 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. As discussed above, I allow for year effects that vary by broad industries (1-digit industry codes), implying estimates will be driven by comparing differences across different markets within a broad industry group.

First, I test whether mergers actually create persistent increases in local labor market concentration. If mergers incentivize more entry (for example, if the merged firm raises price, then more firms may enter), then increases in concentration due to merger activity may be transitory. To estimate the dynamic impacts of mergers on concentration, I estimate a dynamic difference-in-differences specification of the following form:

$$C_{mt} = \sum_{k=-4}^{4} \delta_k^C \mathbb{1}(t_m = t^* + k) \times Q20_m + \Phi_m + \tau_{t,k(m)} + u_{mt} \tag{22}$$

where $Q20_m$ indicates the market is involved in a predicted concentration change in the top ventile of all concentration changes. Panel A of Figure 6 plots the coefficients $\hat{\delta}_k^C$ from estimating Equation (22). 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.

to the averages of the percentiles around the knot. For example, the psuedo-95^th percentile is the average of the 94^{th}, 95^{th} and 96^{th} percentile. The pseudo-knots are equal to 0.011, 0.018, 0.037, 0.21. The spline is restricted to be linear below the 5^{th} and above the 95^{th} percentile.

Next I turn to the impact on market earnings by estimating Equation (22) with the average market-level earnings $\tilde{w}_{mt}$ as the outcome. Panel C of Figure 6 plots the results. On average, log average earnings in the top ventile markets fall by -0.034 (SE $= 0.013$) after the concentration event. Lastly, Panel D of Figure 6 plots the estimates of a top-ventile concentration change on employment. As can be seen in Panel D of Figure 6, there is no evidence of falling employment in the years prior to the M&A event. This is reassuring as one common critique is that increases in concentration may be confounded by declining demand for labor. However, top-ventile markets are experiencing similar trends in employment in the years prior to the M&A event. After the event, log market employment falls by -0.124 (SE $= 0.062$). This pattern of wage and employment changes is also consistent with the theoretical predictions of the model. Since firms are paying wages along a market-level labor supply curve, decreases in wages due to changes in concentration should also be accompanied by declines in employment.

6.4.1 Elasticity of Earnings with Respect to Local Labor Market Concentration

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

$$\tilde{C}_{mt} = \Phi_m + \tau_{t,k(m)} + Q20_m \times Post_{mt} + u_{mt} \tag{23}$$

$$\tilde{w}_{mt} = \Phi_m + \tau_{t,k(m)} + \beta\tilde{C}_{mt} + u_{mt} \tag{24}$$

where Equation (23) 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 5, I find top-ventile changes increase log concentration by 0.175 with a corresponding F-statistic equal to 16. In Column 1 of Table 6, 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 5 of Table 6, 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 a larger impact on earnings in high-concentration markets.

In Column 1 of Appendix Table A13 I use top-ventile M&As as an instrument for a standard HHI measure. As in the worker-level analysis, I continue to find qualitatively similar results. The elasticity of earnings with respect to concentration is equal to -0.115 (SE=0.040), slightly lower in magnitude than the estimates using the flows-adjusted concentration measure. Interestingly, this value is quite similar to prior work that uses standard HHI measure (Azar et al., 2022; Benmelech et al., 2020; Rinz, 2020). Therefore, while utilizing a top-ventile M&A as an instrument for concentration is distinct relative to the prior literature, the estimates by utilizing this variation are consistent with other sources of variation in concentration.

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. While the employment trends are reassuring, another approach is to focus on variation that is less likely to be driven by local economic conditions. Appendix Table A13 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).

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 A13, I find the elasticity of concentration is equal to -0.331 (SE=0.180) in tradable industries for which product market effects are likely ameliorated. Interestingly, elasticity is larger in tradable industries is consistent with the worker-level results, though the confidence intervals here are quite large, making the difference in elasticities between tradable and nontradable markets not statistically significant.

To summarize, I find the majority of mergers do not cause market-level spillovers, because, on average, mergers do not cause very large increases in market concentration. However, the largest mergers (top-ventile), do cause market-level declines in earnings that are not due 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 2.2.2.

6.5 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.

6.5.1 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:

$$\gamma_m = \frac{\eta_m^\alpha}{C_m + \eta_m^\alpha} \tag{25}$$

where η_m^α the flows-adjusted 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:

$$\Delta\tilde{w} = \Delta\tilde{\theta} + ln\left(\frac{\eta}{C_2 + \eta}\right) - ln\left(\frac{\eta}{C_1 + \eta}\right) \tag{26}$$

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 6.4.

That is, I set:

$$\hat{\eta} = \arg\min_{\eta}(\hat{\beta} - m(\eta))^2 \tag{27}$$

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.[36] 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 7 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 more concentrated markets by construction.

This analysis leads to two important points. Regardless of whether a standard HHI measure

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

or a flows-adjusted measure is used to compute wage markdowns, I find markdowns 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., 2020; 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, markdowns due to local concentration have 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., 2021) or increases in non-competes(Starr et al., 2021) and no-poaching agreements (Krueger and Ashenfelter, 2022; Krueger and Posner, 2018) could lead to rising monopsony power even in the presence of falling local concentration.

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

Figure 8 plots the fraction of mergers that would be blocked over time for a 1 percent decline in the wage (solid blue line) and a 5 percent decline in the wage (dashed orange line). As can be seen in the Figure, for a 1 percent decline in the wage, the percent blocked fluctuates between 2 to 8 percent a year, with an average equal to 4.6 percent 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 A1). 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. Despite recent calls-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. I argue that this evidence justifies antitrust authorities scrutinizing mergers on the basis of increased labor market power.

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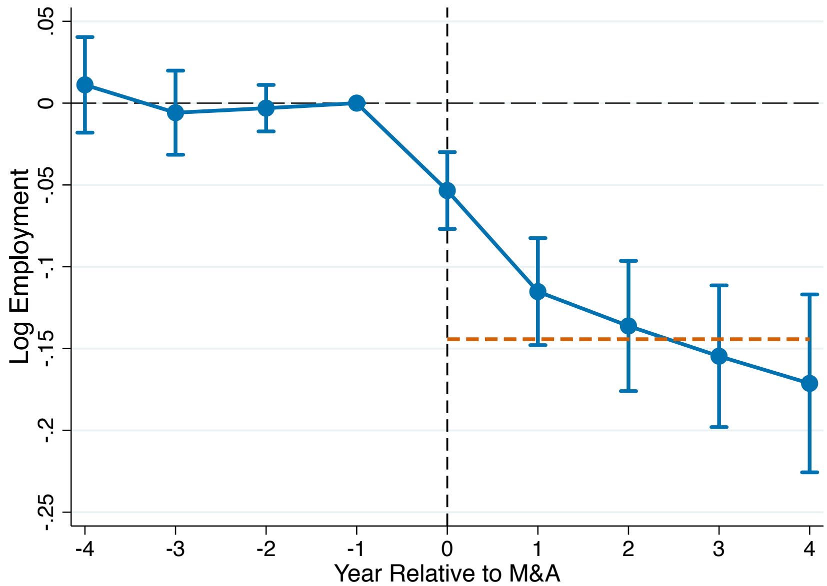
Figure 1: M&A Activity over Time

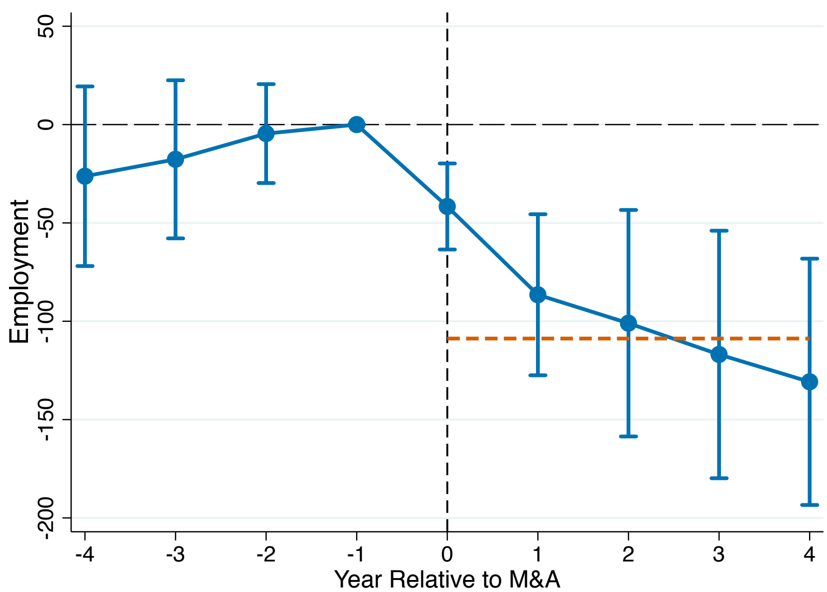
Note: This figure plots the total employment in M&A establishments (solid blue line) over time. This sample is a subset of all merger activity due to sample restrictions that drop small and partial M&As. For more details on sample construction see Section 3.4. The dashed orange line plots the number 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 2: Difference-in-Differences Estimates of the Effect of M&A on Employment

Panel A: Log Employment

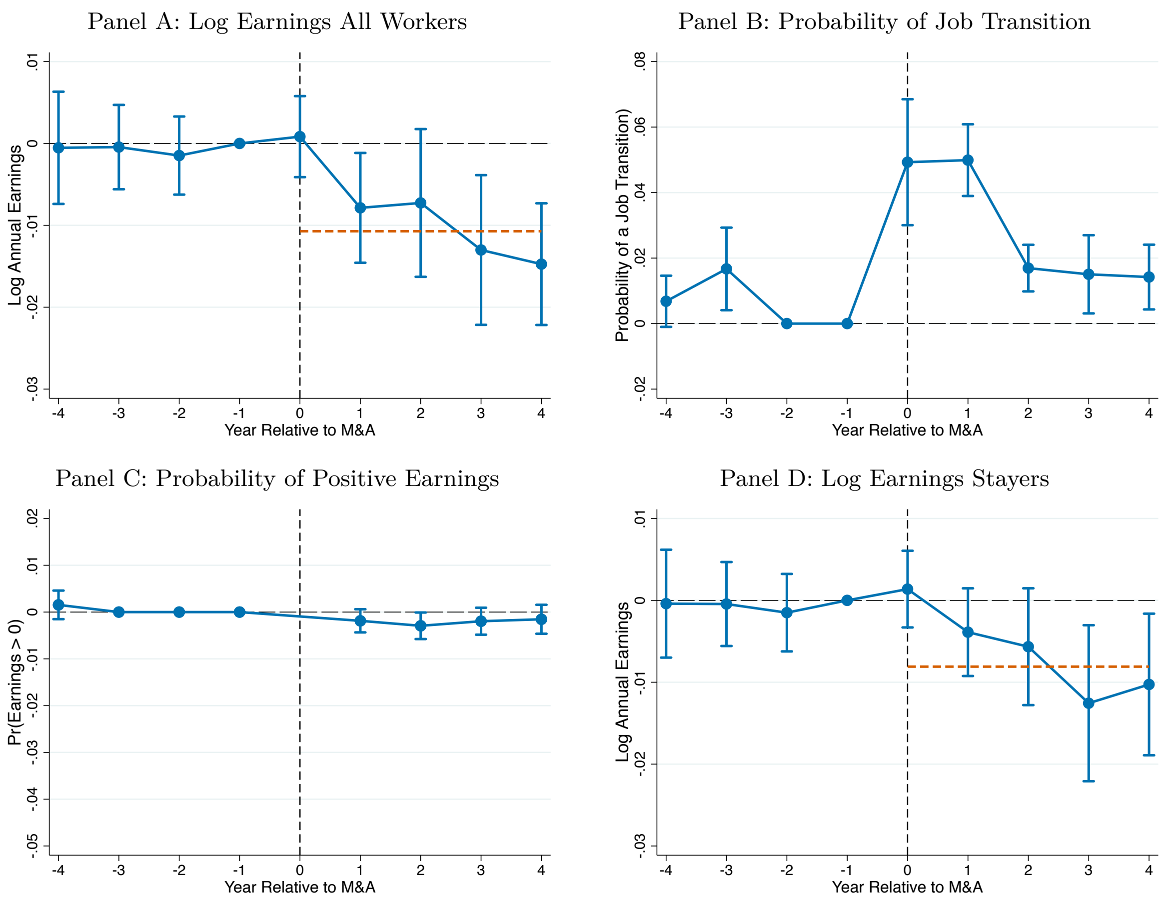


Panel B: Employment



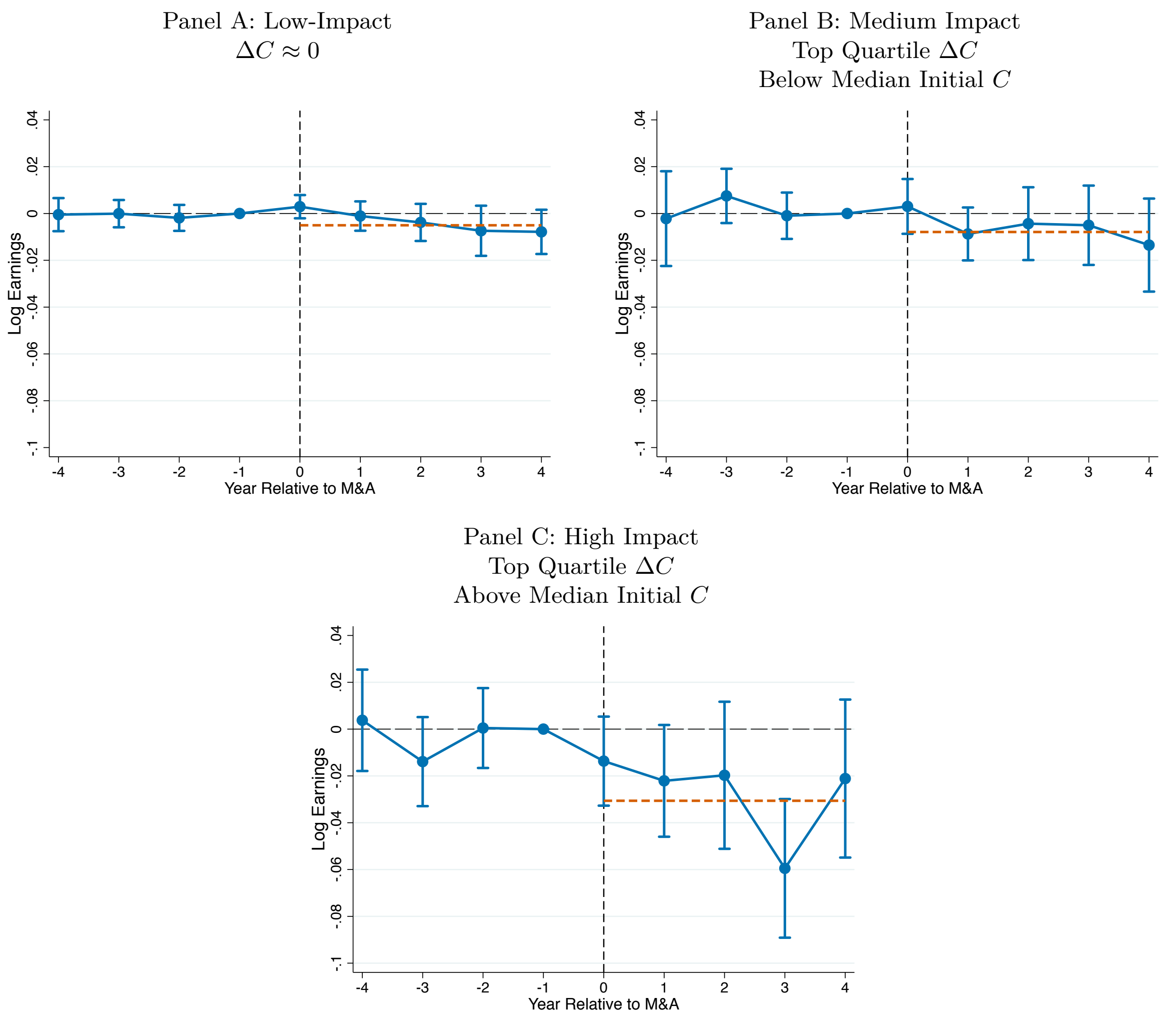
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on log establishment-level employment in Panel A and establishment-level employment in Panel B (including zeros). Due to the ambiguity in the timing of the merger, some M&A establishments have already gone through the merger at time $t = 0$, while others have yet to complete the merger. For each M&A establishment I find a counterfactual establishment by matching on 4-digit NAICS (industry codes), state, $t^* - 1$ employment decile, and $t^* - 1$ average earnings decile, where t^* indicates the year of the merger. 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 3: Difference-in-Differences Estimates of the Effect of M&A on Incumbent Worker Outcomes



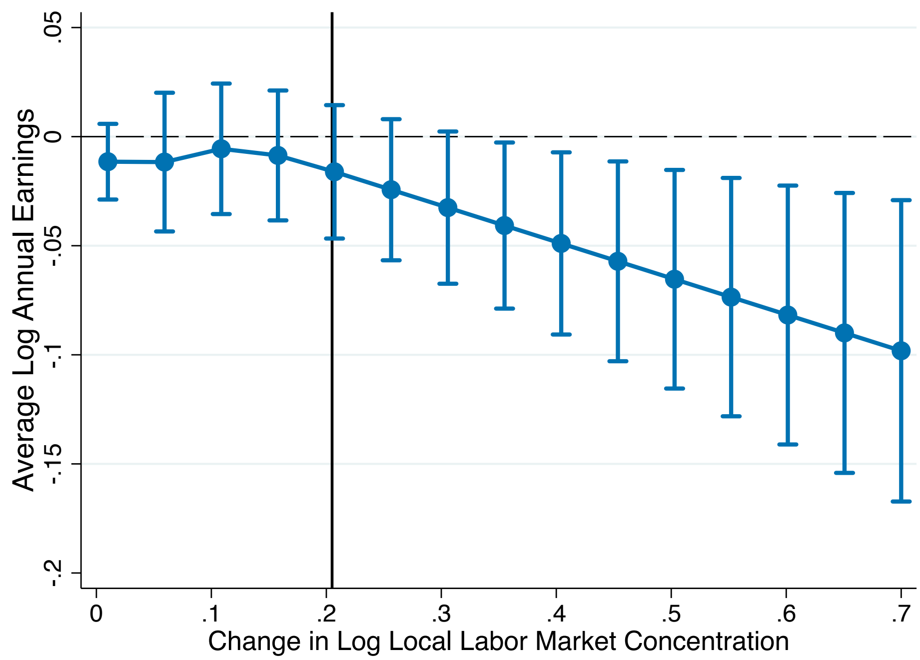
Note: This figure reports matched difference-in-differences estimates of the effect of M&A on worker outcomes. Panel A reports the impact on log annual earnings for all incumbent workers. Panel B reports the impact on job transitions. Panel C reports the impact on the probability of having zero earnings.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

Figure 4: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings



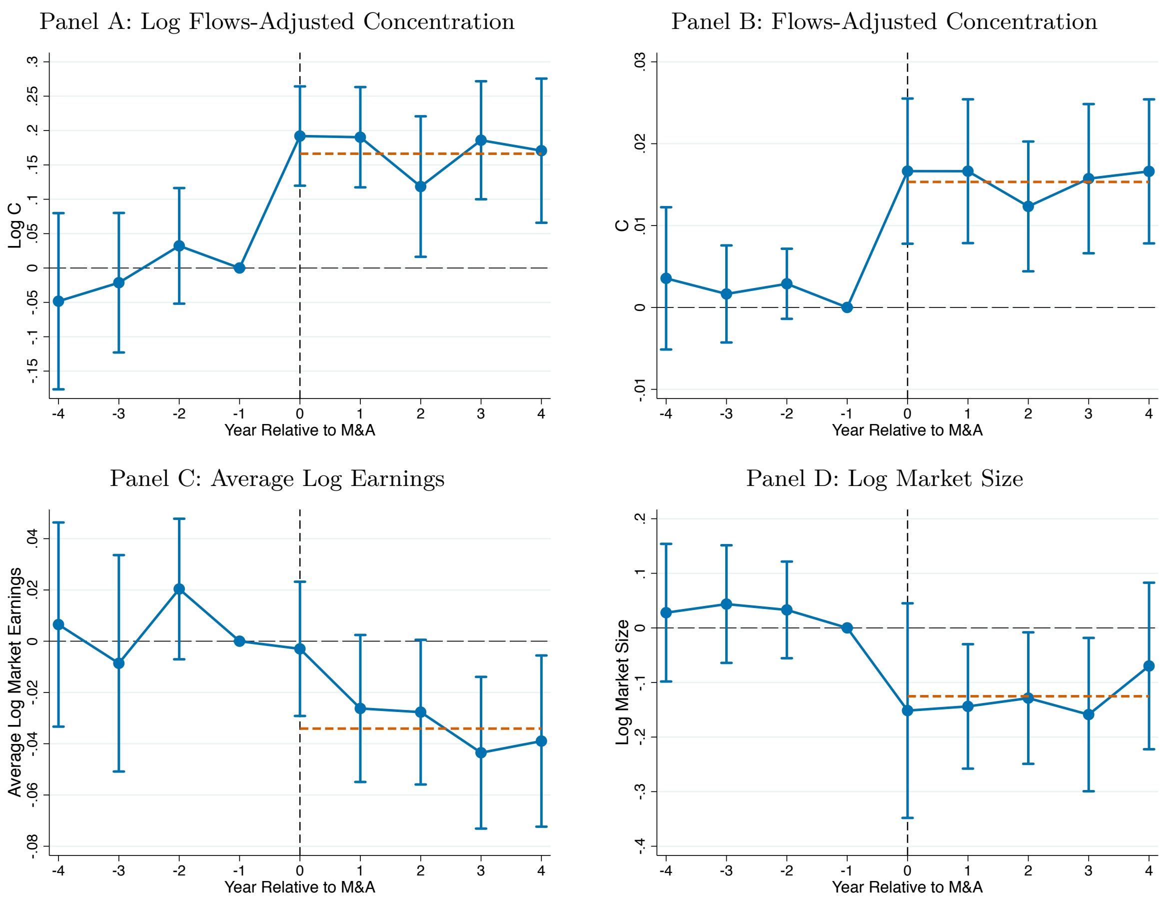
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. Concentration (C) is measured using the flows-adjusted measure of local labor market concentration that takes into account substitutability of jobs across industries. Panel A displays results 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 3 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.

Figure 5: Market-Level (Excluding M&A Firms) Impacts by Predicted Change in Local Labor Market Concentration



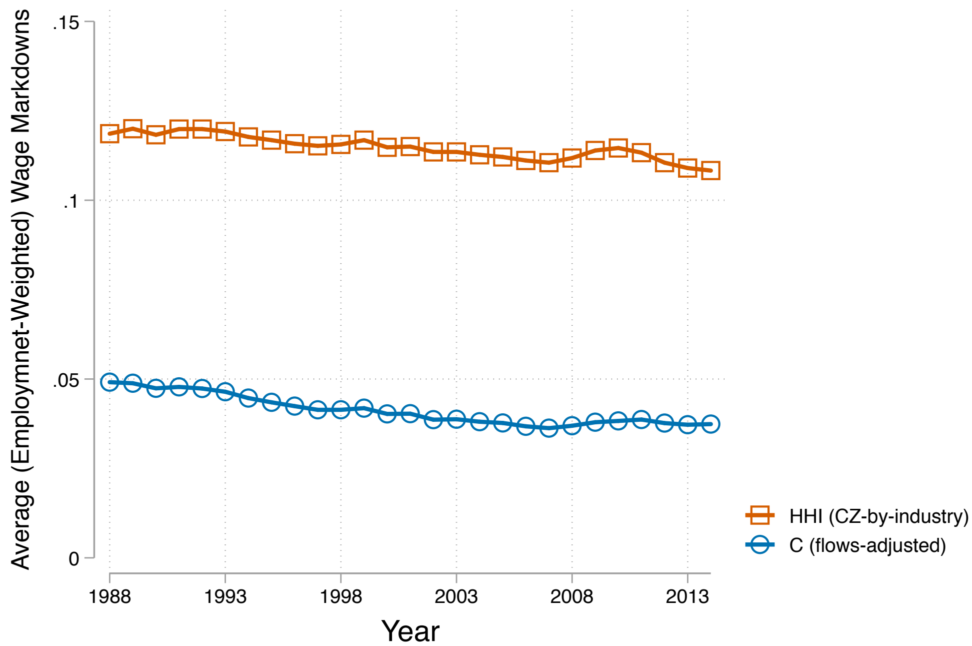
Note: This figure reports the impact of M&A on market-level earnings as a function of the predicted change in log local labor market concentration. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure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

Figure 6: Difference-in-Differences Estimates of Top-Ventile Concentration Increases on Market-Level Outcomes



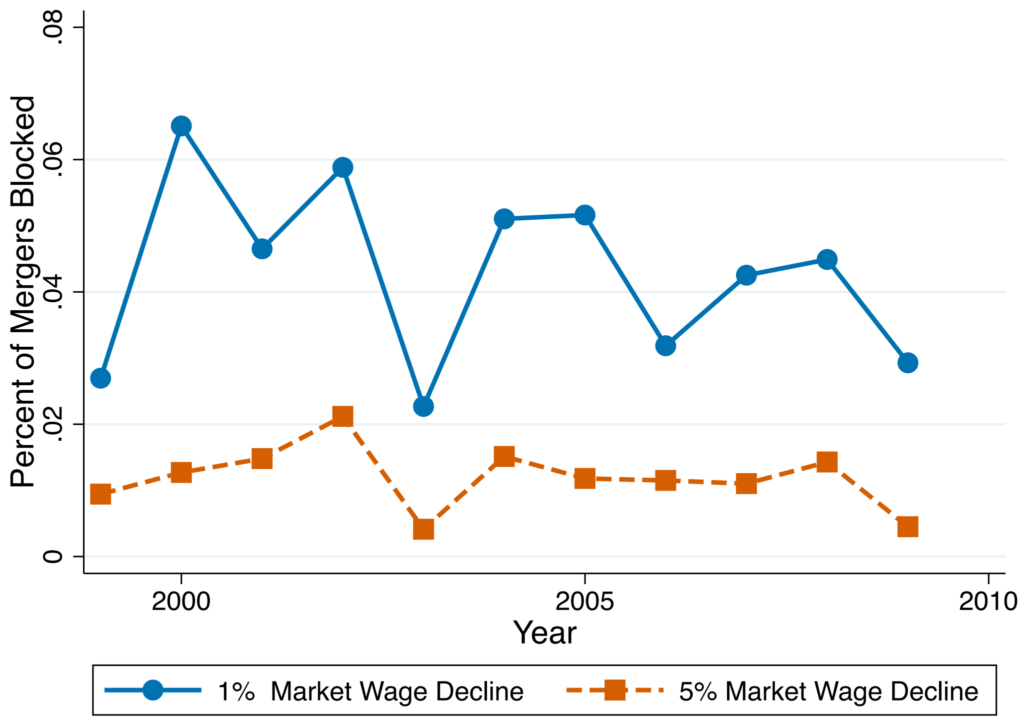
Note: This figure displays estimates of the effect of a top-ventile concentration increase on market-level outcomes. A market is defined as a 4-digit NAICS by commuting zone cell. Local labor market concentration is measured using the flows-adjusted concentration measure (C) that incorporates information 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, and Panel D reports the impact on average log market size. 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 7: 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 6. 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 8: 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 6. A merger is considered blocked if it lowers the market-wage in at least one market by more than the given threshold.

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

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

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

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

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

Note: This table displays summary statistics of M&A workers and matched control workers, which are drawn from the sample of M&A firms with coverage in the LEHD sample. 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 and an indicator equal to one if the worker is employed by a multi-unit firm.

Table 3: Hetereogeneity by Local Concentration: The Effect of M&A on Log Annual Earnings for Firm Stayers

	High Impact	Medium Impact	Low Impact		
	Pred $\Delta C^{MA} > 0$	Pred $\Delta C^{MA} > 0$			
	High Initial C	Low Initial C	Pred $\Delta C^{MA} \approx 0$	Diff (1)-(2)	Diff (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Panel A: All Mergers					
Post-M&A	−0.031	−0.008	−0.005	−0.023	−0.026
	(0.011)	(0.007)	(0.004)	(0.013)	(0.012)
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	–	–
Panel B: Tradables					
Post-M&A	−0.067	0.001	−0.012	−0.068	−0.056
	(0.023)	(0.012)	(0.006)	(0.026)	(0.024)
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	–	–
Panel C: National Mergers					
Post-M&A	−0.042	−0.013	−0.008	−0.029	−0.035
	(0.013)	(0.007)	(0.005)	(0.015)	(0.013)
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 4 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.

Table 4: Correlation between Concentration Changes and Market Outcomes

	Change in Market Earnings			Change in Market Size	
	OLS	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Change in Log C	−0.099		−0.120	0.309	−1.289
	(0.005)		(0.072)	(0.010)	(0.281)
Change in Log HHI		−0.085			
		(0.006)			
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Market-Years	1,083,000	1,083,000	1,083,000	1,083,000	1,083,000

Note: This table regresses changes in market-level outcomes on changes in market-level concentration. The HHI measure is defined at the 4-digit NAICS by CZ level. The generalized HHI measure (C) is also measured at the 4-digit NAICS by CZ level, but incorporates information on worker flows across industries to construct the concentration measure. Market size is the number of employees in the market in a given year. 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 with the market. Columns 3 and 5 instrument concentration with a leave-one-out measure of concentration based on changes in the number of firms in other locations within the same industry. Standard errors are clustered at the 4-digit NAICS by CZ level.

Table 5: First-Stage Impact of Top-Ventile Concentration Increase on Log Local Concentration

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

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

Table 6: Instrumental Variables Estimates of the Elasticity of Earnings with Respect to Local Labor Market Concentration

	Average Log Market Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Log C	−0.219	−0.182	−0.147			
	(0.094)	(0.067)	(0.083)			
Log C × Above Median C				−0.259	−0.230	−0.176
				(0.108)	(0.081)	(0.083)
Log C × Below Median C				0.059	0.065	0.058
				(0.121)	(0.120)	(0.141)
Market-Years	24,000	21,000	24,000	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 5 for the first-stage regression and Figure 5 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.

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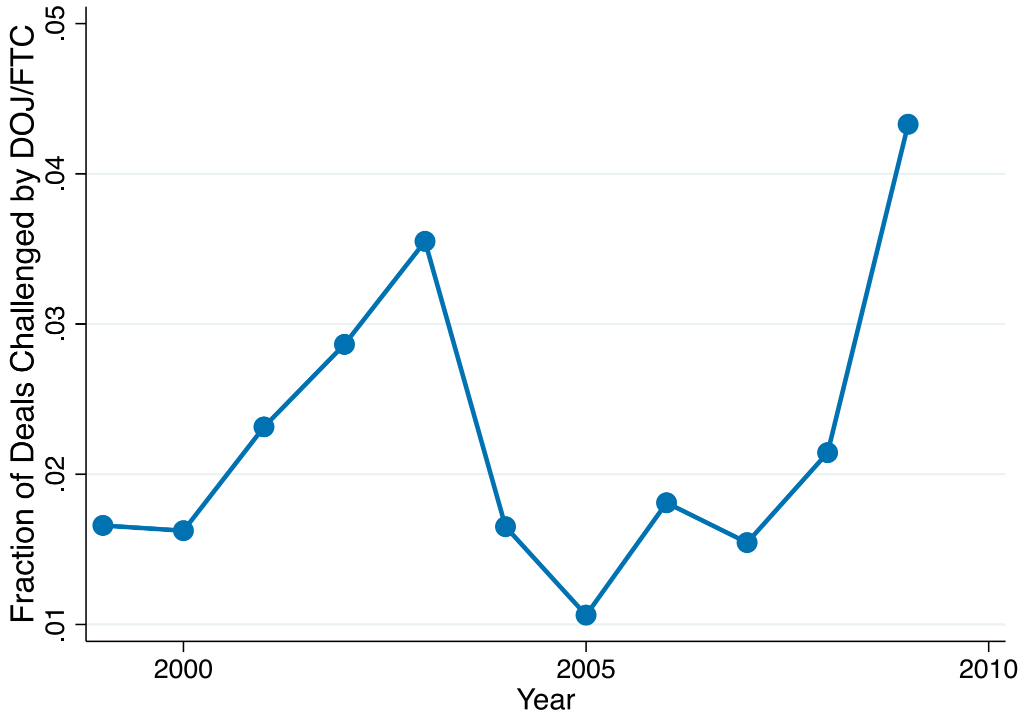
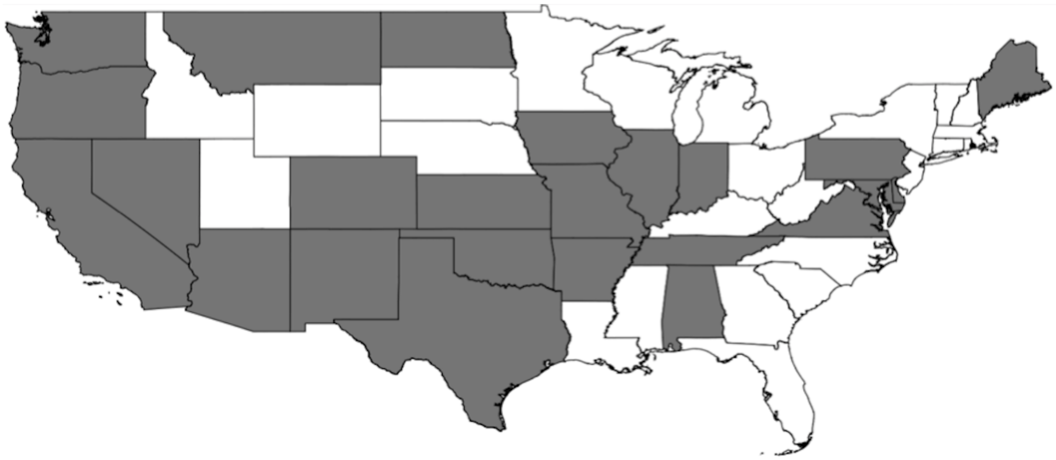
Appendix A: Additional Results

Appendix Figure A1: DOJ and FTC Antitrust Enforcement Actions over Time



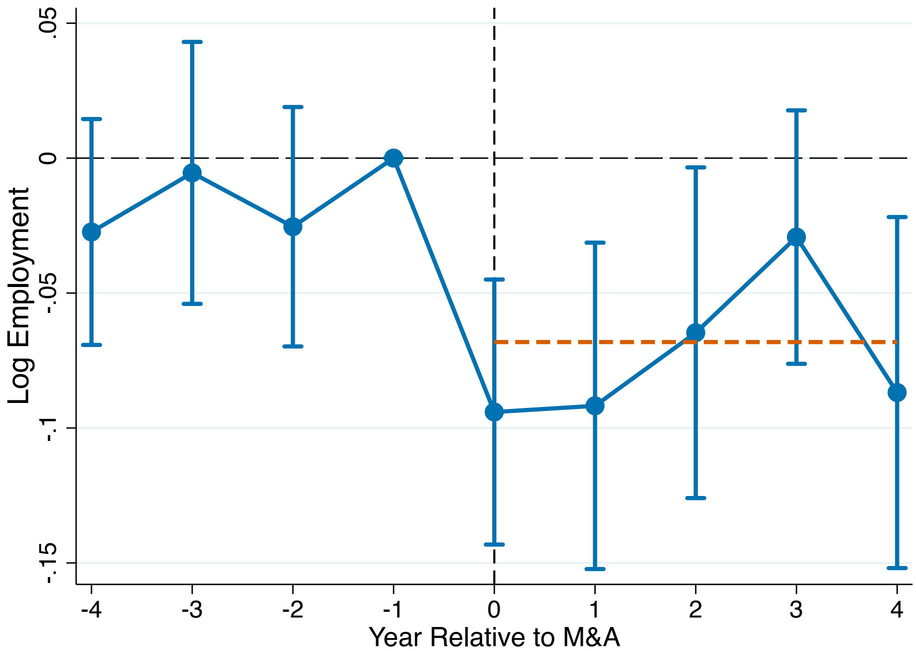
Note: This figure reports the fraction of merger notifications that are challenged each year between 1999-2009. Some deals in which the target asset's are relatively small are exempt from having to notify antitrust authorities (See Wollmann (2019) for more details). Data comes from *Hart-Scott Rodino 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 A2: Sample of States with Worker-Level (LEHD) Data Available



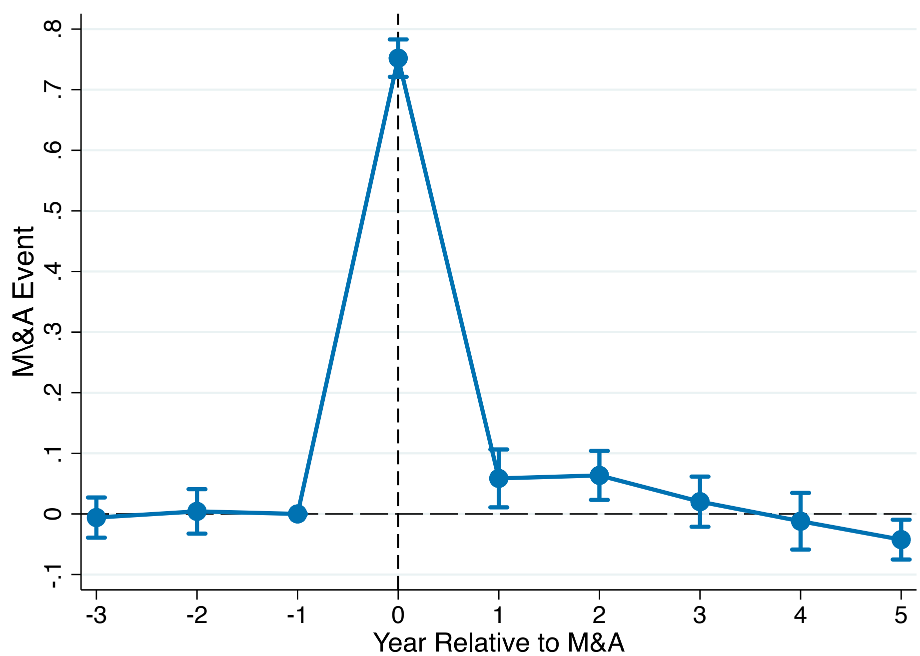
Note: The states with worker-level (LEHD) data available are shaded in gray. The sample includes: AL, AZ, AR, CA, CO, DE, DC, HI, IL, IN, IA, KS, ME, MD, MO, MT, NV, NM, ND, OK, OR, PA, TN, TX, VA, WA. These states correspond to 53.8 percent of the U.S. population as of the 2010 U.S. Census.

Appendix Figure A3: Difference-in-Differences Estimates of the Effect of M&A on Acquiring Firms' Growth Rates



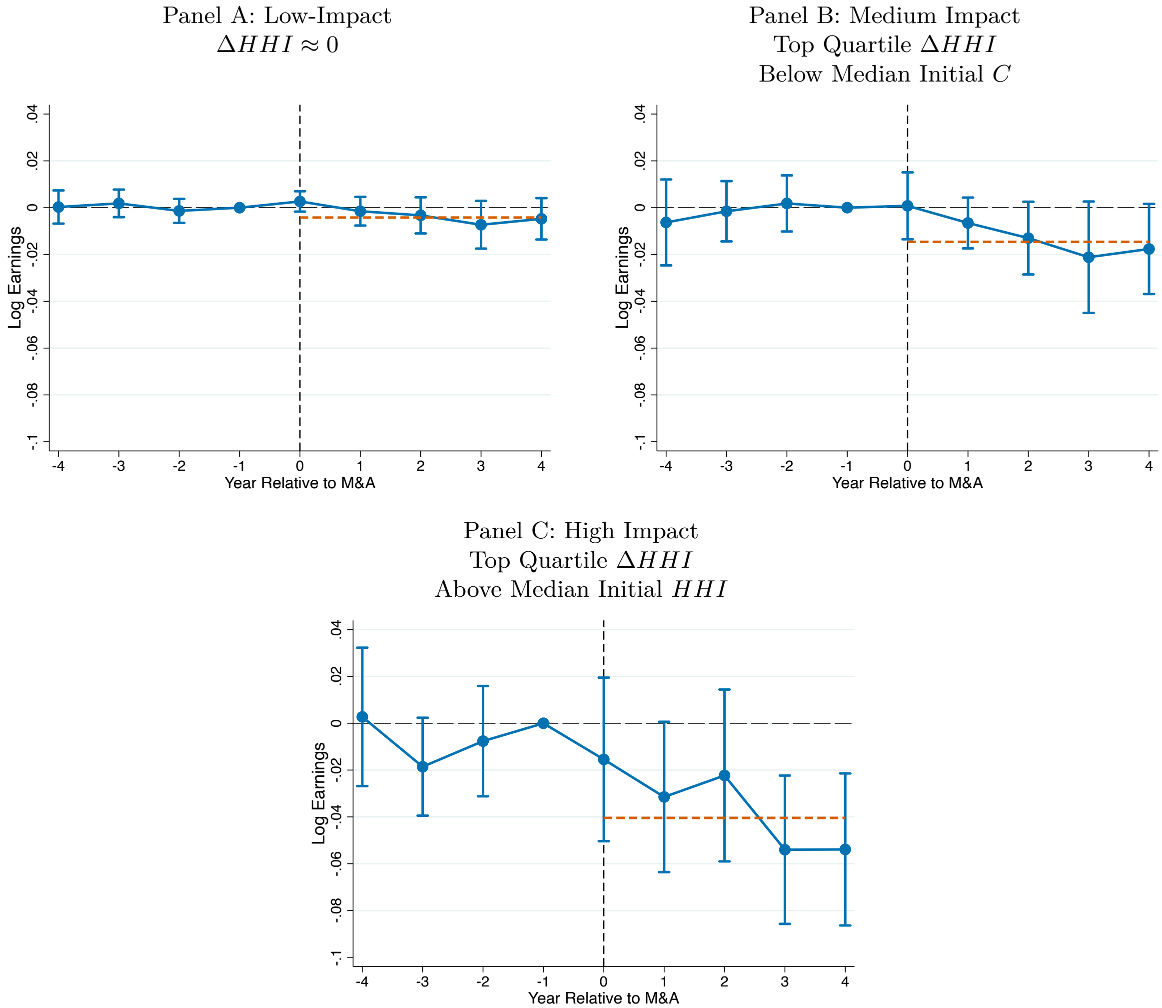
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on the growth rate of the acquiring firm. Growth rates for the acquiring firm are computed using the method described in Haltiwanger et al. (2013), which corrects for mechanical growth due to M&A. To find counterfactual 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 are two-way clustered at the primary NAICS-4-digit code and the primary commuting zone level.

Appendix Figure A4: Difference-in-Differences Estimates of the Effect of M&A on Future M&A Events



Note: This figure shows matched difference-in-differences estimates of the effect of M&A on whether the establishment is involved in a future M&A events. 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. 95 percent confidence intervals are two-way clustered at the NAICS-4-digit code and commuting zone level.

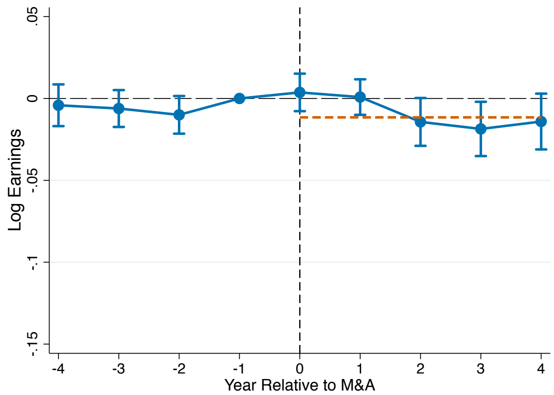
Appendix Figure A5: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings: Using HHI instead of Flows-Adjusted Concentration



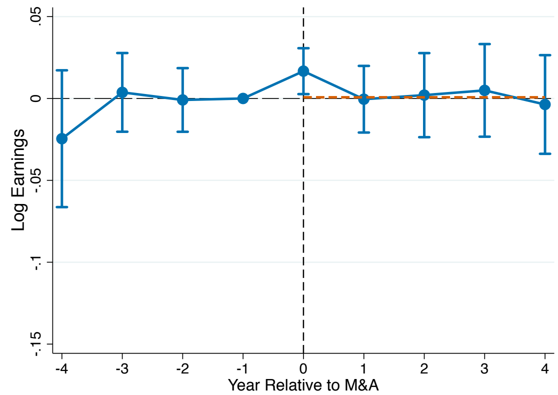
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. Concentration is measured using a standard HHI at the 4-digit NAICS-by-industry level. Panel A displays results for workers exposed to low-impact mergers, which occur when the predicted change in concentration is below the top quartile ($\Delta HHI \approx 0$). 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 3 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 A6: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings in Tradable Industries

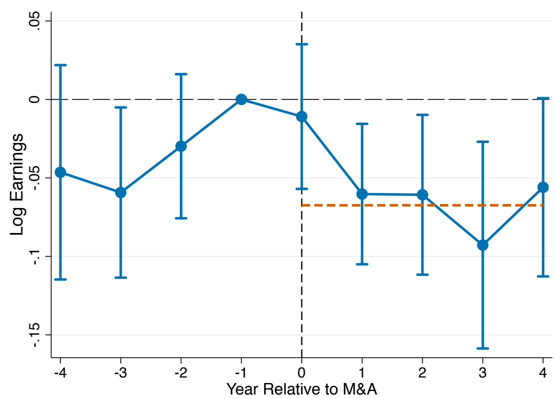
Panel A: Low-Impact
$\Delta C \approx 0$



Panel B: Medium Impact
Top Quartile ΔC
Below Median Initial C

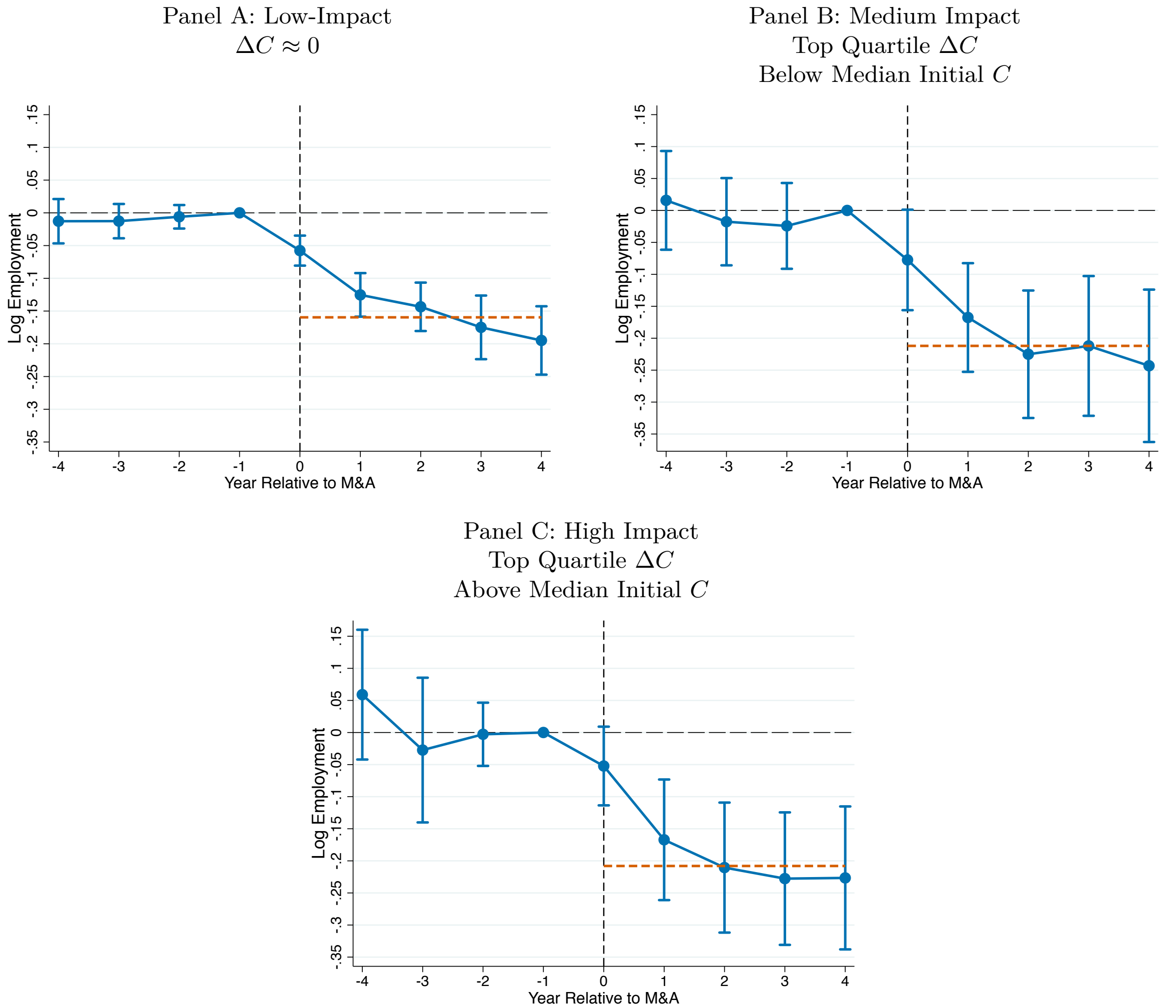


Panel C: High Impact
Top Quartile ΔC
Above Median Initial C



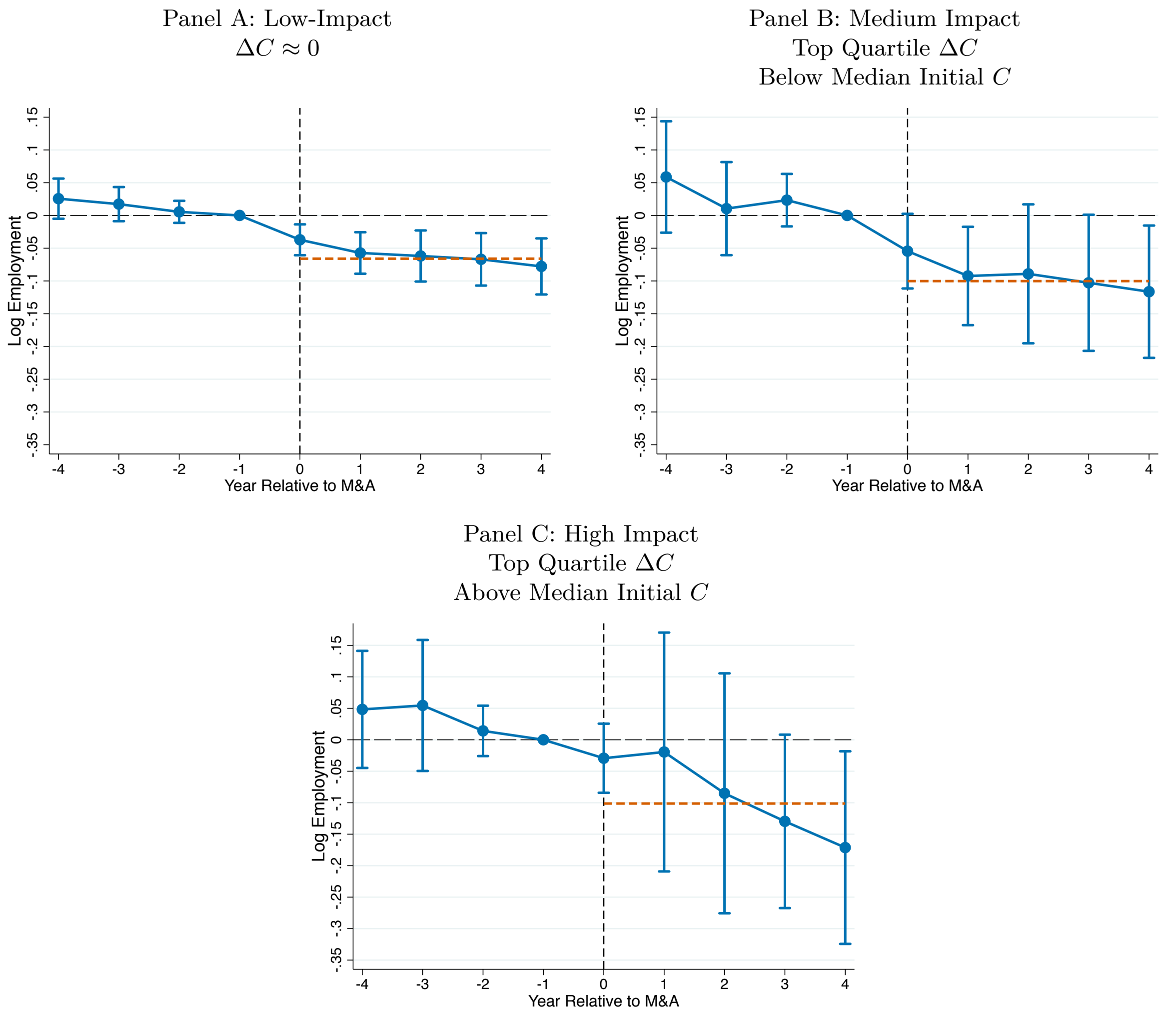
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings for firm stayers in tradable industries, which are defined as industries that belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. Panel A displays results for 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 Figure A7: Difference-in-Differences Estimates of the Effect of M&A on Establishment Employment in Nontradable Industries



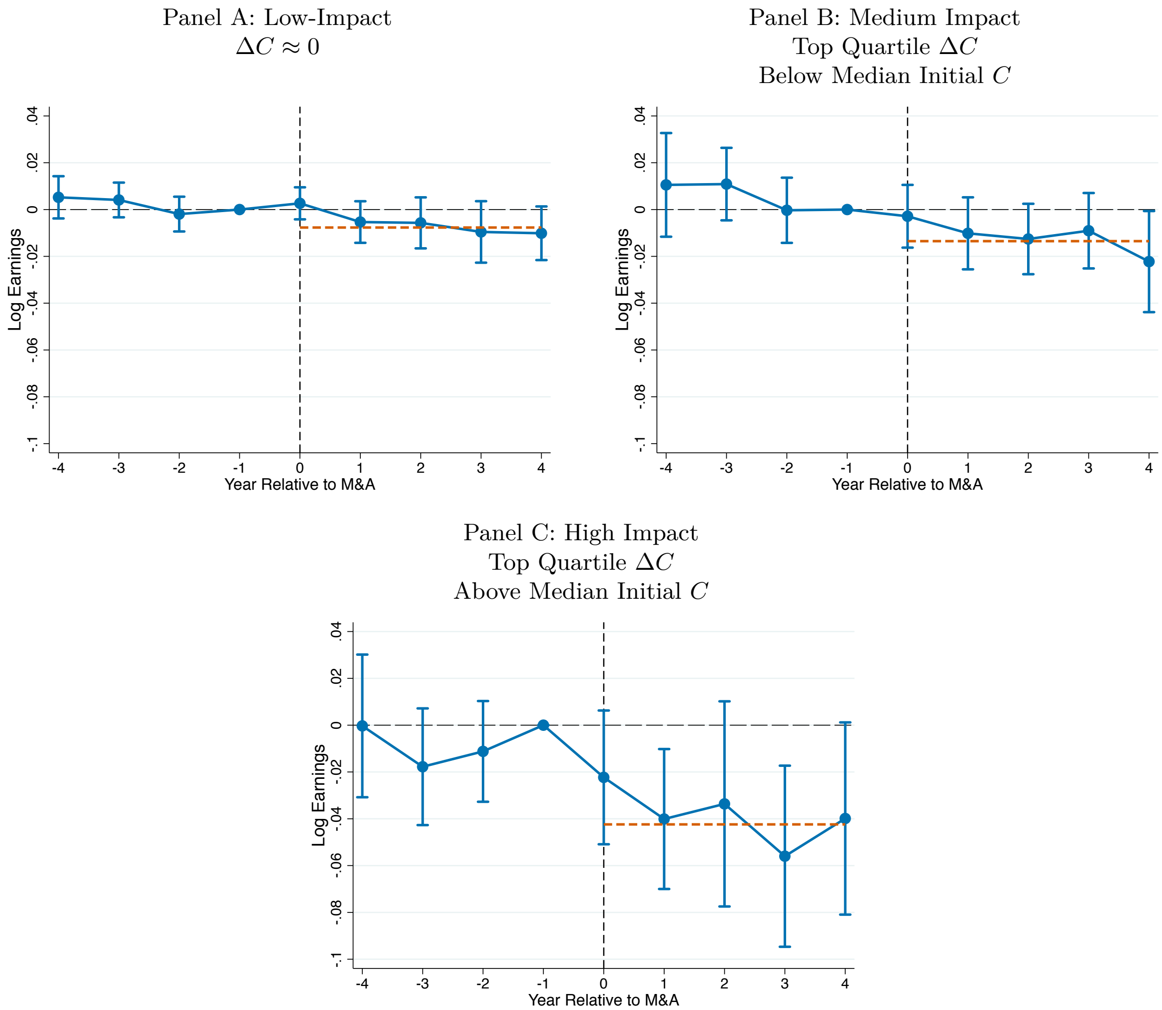
Note: This figure shows matched difference-in-differences estimates of the effect of M&A on establishment-level log employment in nontradable industries, which are defined as industries that do not belong to the following two-digit NAICS industries: 11, 21, 31, 32, 33, and 55. Details on the matching 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 A8: 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.

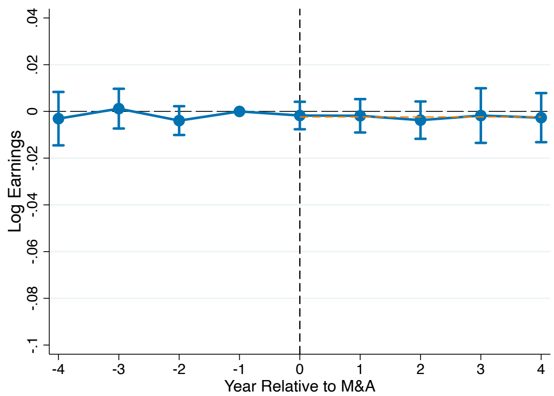
Appendix Figure A9: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings in National Mergers



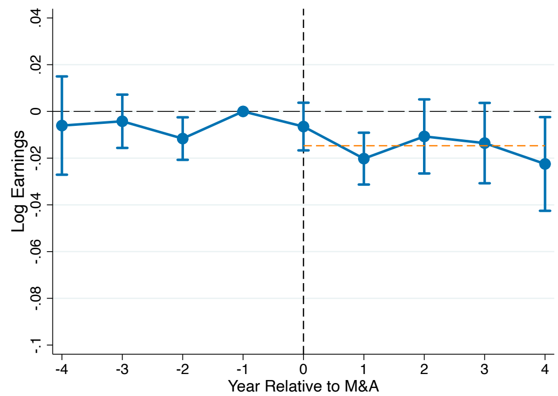
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings for mergers between firms that operate in at least 5 commuting zones (i.e. national mergers). Panel A displays results for workers exposed to low-impact mergers, which occur when the change 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 3 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 A10: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings: Workers From Alternative States

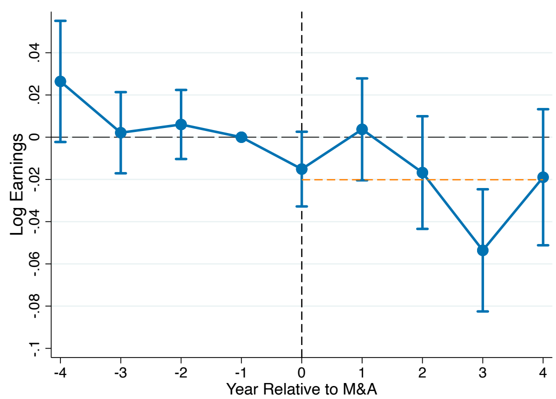
Panel A: Low-Impact
$\Delta C \approx 0$



Panel B: Medium Impact
Top Quartile ΔC
Below Median Initial C



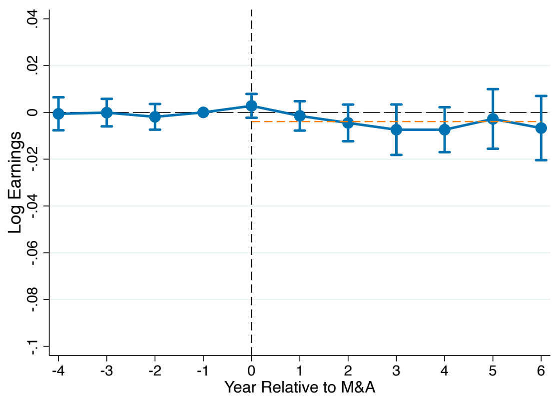
Panel C: High Impact
Top Quartile ΔC
Above Median Initial C



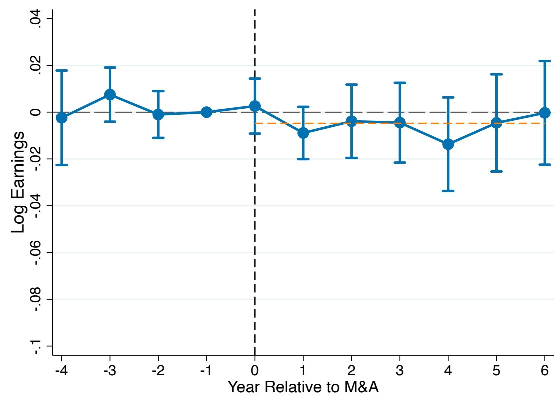
Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. Relative to the matching algorithm used to identify control workers described in the notes Table 2 and Section 3.4, this version matches workers to similar workers in different states to avoid potential spillovers of the M&A event. 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 Figure A11: Difference-in-Differences Estimates of the Effect of M&A on Firm Stayers' Earnings: Longer Time Horizon

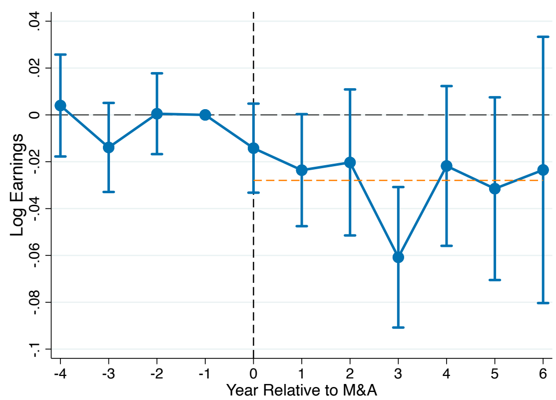
Panel A: Low-Impact
$\Delta C \approx 0$



Panel B: Medium Impact
Top Quartile ΔC
Below Median Initial C



Panel C: High Impact
Top Quartile ΔC
Above Median Initial C



Note: This figure displays matched difference-in-differences estimates of the effect of M&A on log annual earnings. 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 $t^* - 1$. 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: Sample Selection Waterfall M&A Events and Worker–Year Observations

	Observations
Initial sample of unique M&A events	$65,400$
Exclude partial divestitures	$63,900$
Restrict to establishments with > 50 workers	$13,900$
and positive employment in $[t-4, t-1]$	
Restrict to target and acquirer within 10% of each other's size	$7,100$
Final sample of M&A events	**7,100**
Worker–Year Observations (baseline sample)	$25,700,000$
Worker–Year Observations (expanded sample: > 10 workers, no 10% rule)	$79,500,000$

Note: This table reports the number of mergers and acquisitions (M&A) remaining after various sample restriction. The baseline sample excludes partial divestitures, requires establishments to have more than 50 workers with positive employment in the four pre-period years, and requires the target and acquirer to be within 10 percent of each other's size. The final sample yields 7,100 M&A events. When turning to the matching procedure at the worker-level, this procedure yields 25.7 million workers in the stayer estimation sample, which is displayed in Panel (b). The expanded sample relaxes restrictions to allow establishments with more than 10 workers and does not impose the 10 percent size rule, which increases the number of worker–year observations in the stayer analysis to 79.5 million.

Appendix Table A3: Effect of M&A on Establishment Outcomes

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

Notes: This table reports difference-in-differences estimates of the effect of M&A on establishment-level outcomes. I estimate a flexible specification that allows for dynamic treatment effects as depicted in Figure 2 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.

Appendix Table A4: Heterogeneity and Robustness of the Effect of M&A on Log Establishment Employment

	National	Incidental	LEHD States	Acquirer	Target	Low Emp	High Emp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-MA	−0.185	−0.234	−0.151	−0.108	−0.225	−0.055	−0.162
	(0.027)	(0.050)	(0.024)	(0.019)	(0.033)	(0.018)	(0.024)
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 2 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 A2. 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.

Appendix Table A5: Impact of M&A on Worker Outcomes

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

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings (Columns 1 and 2) and the probability a worker transitions jobs (Column 3). I estimate a flexible specification that allows for dynamic treatment effects as depicted in Figure 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 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.

Appendix Table A6: Impact of M&A on Workers' Earnings by Worker Characteristics

	Log Annual Earnings					
	All Workers			Firm Stayers		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-M&A x Age < 40	−0.002			−0.003		
	(0.003)			(0.003)		
Post-M&A x Age 40-50	−0.006			−0.000		
	(0.004)			(0.003)		
Post-M&A x Age >50	−0.018			−0.010		
	(0.005)			(0.004)		
Post-M&A x Q1		−0.003			0.001	
		(0.012)			(0.012)	
Post-M&A x Q2		−0.006			−0.006	
		(0.005)			(0.004)	
Post-M&A x Q3		−0.003			−0.004	
		(0.003)			(0.003)	
Post-M&A x Q4		−0.006			−0.004	
		(0.003)			(0.003)	
Post-M&A x Q5		−0.012			−0.004	
		(0.003)			(0.003)	
Post-M&A x Male			−0.008			−0.004
			(0.003)			(0.003)
Post-M&A x Female			−0.008			−0.004
			(0.004)			(0.004)
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
R squared	0.720	0.722	0.715	0.803	0.807	0.800
Worker-Years	32,000,000	32,000,000	32,000,000	25,700,000	25,700,000	25,700,000

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings. Columns 1-4 do not condition on whether the worker remains at the same firm after the M&A event, and therefore captures any fall in earnings due to being displaced and moving to a lower-paying firm. Columns 5-8 restrict 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. Age<40 indicates the worker is less than 40 years old in the year prior to the merger. Age 40-50 indicates the worker is between 40 and 50 years old. Age>50 indicates the worker is greater than 50 years old in the year prior to the merger. Q1-Q5 refer to the worker's earnings quintile within the firm, where Q1 indicates bottom quintile and Q5 indicates top quintile. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

Appendix Table A7: Impact of M&A on Job Transitions by Worker Characteristics

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

Note: This figure shows difference-in-differences estimates of the effect of M&A on the probability of a job transition. A job transition occurs if a worker moves between employers or if the worker moves from nonemployment to employment (or vice-versa). 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 matching on worker characteristics as described in the text and the notes to Table A5. Q1-Q5 refer to the worker's earnings quintile within the firm, where Q1 indicates bottom quintile and Q5 indicates top quintile. <40 indicates the worker is less than 40 years old in the year prior to the merger. 40-50 indicates the worker is between 40 and 50 years old. >50 indicates the worker is greater than 50 years old in the year prior to the merger. Standard errors are two-way clustered at the worker and 4-digit-NAICS by commuting zone level.

Appendix Table A8: Impact of M&A on Log Annual Earnings for Firm Stayers: HHI

	High Impact	Medium Impact	Low Impact		
	Pred $\Delta HHI > 0$ High Initial HHI	Pred $\Delta HHI > 0$ Low Initial HHI	Pred $\Delta HHI \approx 0$	Diff (1)-(2)	Diff (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Panel A: All Mergers					
Post-M&A	−0.040	−0.015	−0.004	−0.026	−0.036
	(0.013)	(0.007)	(0.004)	(0.015)	(0.014)
Initial HHI	0.223	0.049	0.207	–	–
Change in Log HHI	0.092	0.077	0.000	–	–
Approx Worker-Years	2,700,000	3,700,000	19,300,000	–	–

Note: This figure shows difference-in-differences estimates of the effect of M&A on log annual earnings 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. These results use a standard HHI at the 4-digit NAICS by commuting zone level to break M&A events into low, medium and high impact, rather than the flows-adjusted concentration measure. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

Appendix Table A9: Impact of M&A on Log Annual Earnings for Firm Stayers: Alternative Merger Sample

	High Impact	Medium Impact	Low Impact		
	Pred $\Delta C^{MA} > 0$	Pred $\Delta C^{MA} > 0$			
	High Initial C	Low Initial C	Pred $\Delta C^{MA} \approx 0$	Diff (1)-(2)	Diff (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Panel A: All Mergers					
Post-M&A	−0.019	0.004	0.005	−0.023	−0.024
	(0.010)	(0.003)	(0.003)	(0.010)	(0.010)
Initial C	0.062	0.008	0.086	–	–
Change in Log C	0.048	0.012	0.000	–	–
Approx Worker-Years	8,400,000	11,500,000	59,600,000	–	–

Note: This figure shows difference-in-differences estimates of the effect of M&A on log annual earnings 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. These results drop restrictions on the firm size when defining mergers. In particular, instead of requiring the firm to be larger than 50 workers in the year prior to the merger, M&A events in this sample need to be between firms that have 10 or more employees in years prior to the M&A event. Additionally, there is no restriction on the relative size of target vs. acquiring firms, where the baseline sample requires the target to be 10% as large as the acquirer. Standard errors are two-way clustered at the worker and 4-digit NAICS by commuting zone level.

Appendix Table A10: Robustness: Impact of M&A on Log Annual Earnings for Firm Stayers by Concentration Changes

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

Note: This table reports difference-in-differences estimates of the effect of M&A on log annual earnings. I estimate a flexible specification that allows for dynamic treatment effects and compute the aggregate effect as the average of the four post-event coefficients. The sample restricts to firm stayers who are defined as workers employed in time t at the same firm as in $t^* - 1$. For details on the matching algorithm used to identify control workers, see the notes to Table 2 and Section 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.

Appendix Table A11: 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				0.659
	(0.032)				(0.035)
ΔC^{Exit}		0.778			
		(0.016)			
ΔC^{Entry}			0.981		
			(0.012)		
$\Delta C^{Reallocation}$				0.946	0.915
				(0.007)	(0.010)
R^2	0.014	0.254	0.107	0.915	0.927
Market-Years	1,083,000	1,083,000	1,083,000	1,083,000	1,083,000

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

Appendix Table A12: 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 6.

Appendix Table A13: Heterogeneity and Robustness: IV Estimates of the Elasticity of Earnings with Respect to Local Labor Market Concentration

	Average Log Market Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Log HHI	-0.115					
	(0.040)					
Log C × National		-0.262	-0.258	-0.309		
		(0.146)	(0.128)	(0.176)		
Log C × Tradable					-0.331	
					(0.180)	
Log C × Nontradable					-0.202	
					(0.102)	
Log C × Tradable × High C						-0.392
						(0.216)
Log C × Tradable × Low C						0.048
						(0.096)
Log C × Non-tradable × High C						-0.235
						(0.117)
Log C × Non-tradable × Low C						0.051
						(0.121)
Market-Years	24,000	24,000	24,000	24,000	24,000	24,000
4-digit NAICS-by-CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
1-digit NAICS-by-CZ-year FE	Yes	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, concentration is measured using a standard HHI index at the 4-digit NAICS-by-Commuting Zone level. In Column 2, a national merger is defined as a merger between two firms both operating in at least two commuting zones. In Column 3, a national merger is defined as a merger between two firms both operating in at least 5 commuting zones. In Column 4, 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.

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 12^{th}.

Payroll: Annual establishment-level payroll.

Industry: Unless otherwise stated, industry is defined by 4-digit North American Industry Classification Systems (NAICS) codes. In 1997, the U.S. Census switched from using Standard Industrial Classification (SIC) to NAICS. While most of the analysis in the paper does not require industrial classification pre-1997 (I study mergers 1999-2009), the analysis that does require pre-1997 industrial classification uses time-consistent NAICS codes provided by Fort et al. (2016).

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

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

B.2 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: 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.3 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 Wage Bargaining Model

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

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

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

$$\max_{l_j, w_j}[l_j w_j + (\bar{l} - l_j)v - \bar{l}v]^{\gamma_j}[p_j(F(l_j))F(l_j) - w_j l_j]^{1-\gamma_j} \tag{28}$$

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

$$w_j = \gamma_j \left(\frac{p_j(F(l_j))F(l_j)}{l_j} - v \right) + v \tag{29}$$

$$F'(l_j)p_j(F(l_j)) \left(\frac{1}{\varepsilon_j} + 1 \right) = v \tag{30}$$

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.

C.2 Derivation of Market-Level Wage in Cournot Model with Flows-based Concentration measure

In this subsection, assume that the wage equation of a market is given by:

$$w_m(\sum_k \alpha_{m \to k} L_k) \tag{31}$$

where $\alpha_{m \to k}$ is the fraction of workers that move from market m to market k. Relative to the standard Cournot model, this version assumes that the wage in market m depends on the employment in m ($\alpha_{m \to m} = 1$), as well as the employment in other markets in the same commuting zone. The problem of firm j in market m is to maximize profits:

$$\pi_j = R_j(l_j) - w_m(\sum_k \alpha_{m \to k} L_k) l_j \tag{32}$$

Taking the first-order condition with respect to l_j yields:

$$\theta_j - \frac{\partial w_m(\sum_k \alpha_{m\to k} L_k)}{\partial(\sum_k \alpha_{m\to k} L_k)} \cdot \frac{\partial(\sum_k \alpha_{m\to k} L_k)}{\partial l_j} l_j - w_m(\sum_k \alpha_{m\to k} L_k) = 0 \tag{33}$$

Rearranging and dividing by $w_m(\sum_k \alpha_{m\to k} L_k)$ yields:

$$\frac{\theta_j}{w_m(\sum_k \alpha_{m\to k} L_k)} = \frac{\partial w_m(\sum_k \alpha_{m\to k} L_k)}{\partial(\sum_k \alpha_{m\to k} L_k)} \cdot \frac{\partial(\sum_k \alpha_{m\to k} L_k)}{\partial l_j} \cdot \frac{l_j}{w_m(\sum_k \alpha_{m\to k} L_k)} + 1 \tag{34}$$

Note that $\frac{\partial(\sum_k \alpha_{m\to k} L_k)}{\partial l_j} = 1$ because $\alpha_{m\to m} = 1$. Multiplying and dividing the first expression on the right-hand side by $\sum_k \alpha_{m\to k} L_k$ yields:

$$\frac{\theta_j}{w_m(\sum_k \alpha_{m\to k} L_k)} = \frac{\partial w_m(\sum_k \alpha_{m\to k} L_k)}{\partial(\sum_k \alpha_{m\to k} L_k)} \cdot \frac{\sum_k \alpha_{m\to k} L_k}{w_m(\sum_k \alpha_{m\to k} L_k)} \cdot \frac{l_j}{\sum_k \alpha_{m\to k} L_k} + 1 \tag{35}$$

Let:

$$\frac{1}{\eta_m} = \frac{\partial w_m(\sum_k \alpha_{m\to k} L_k)}{\partial(\sum_k \alpha_{m\to k} L_k)} \cdot \frac{\sum_k \alpha_{m\to k} L_k}{w_m(\sum_k \alpha_{m\to k} L_k)} \tag{36}$$

Then:

$$\frac{\theta_j}{w_m(\sum_k \alpha_{m\to k} L_k)} = \frac{1}{\eta_m} \tilde{s}_j + 1 \tag{37}$$

Where $\tilde{s}_j = \frac{l_j}{\sum_k \alpha_{m\to k} L_k}$. Multiplying each side by $\tilde{s}_j$ and summing across all j firms in market m yields:

$$\frac{\sum_j \tilde{s}_j \theta_j}{w_m} = \sum_j \frac{1}{\eta_m} \tilde{s}_j^2 + \sum_j \tilde{s}_j \tag{38}$$

Where I have dropped the conditioning of w_m on the flows-adjusted total employment. Note that $C = \sum_j \tilde{s}_j^2$ and $\sum_j \tilde{s}_j = \frac{L_m}{\sum_k \alpha_{m\to k} L_k}$. Denote, $\sum_j \tilde{s}_j = s_m$. Then we can rewrite the equation above as:

$$\frac{s_m \sum_j s_j \theta_j}{w_m} = \sum_j \frac{1}{\eta_m} \tilde{s}_j^2 + s_m \tag{39}$$

Where the left-hand side has been multiplied and divided by L_m and then rearranged. Denote, $\sum_j s_j \theta_j = \theta_m$, which is the average MRPL in the market. Denote $s_m \eta_m = \eta_m^\alpha$ which is referred to as the flows-adjusted elasticity of labor supply at the market level. This is equal to the standard elasticity of labor supply at the market level, but adjusted for flows out of the market. If there are no flows out of the market, then $s_m = 1$. Therefore, we can rewrite the equation above as:

$$\frac{\theta_m}{w_m} = \frac{C}{\eta_m^\alpha} + 1 \tag{40}$$

Rearranging yields:

$$w_m = \left(\frac{\eta_m^\alpha}{C + \eta_m^\alpha} \right) \cdot \theta_m \tag{41}$$

Taking logs yields

$$ln(w_m) = \tilde{\theta}_m + ln\left(\frac{\eta_m^\alpha}{C + \eta_m^\alpha} \right) \tag{42}$$

C.3 Relationship between flows-based measure and diversion ratios

In the model, the decision to move jobs in the model is a logit choice model, with utility that takes the form of $U_i(k|m) = ln(V(k|m)) + \xi_i$. The first difference relative to a standard consumer purchasing problem is I assume offers only arrive at certain rates, so workers cannot choose any market to work in. They must receive an offer in order to take a job in another market, which is why relative market sizes appear in the measures for concentration.

Ignoring these market size terms, however, allows one to make a clear connection with the model of job transitions to a measure of the diversion ratio, which is one measure that has been suggested as a means to analyze potential merger effects. The diversion ratio in this model is slightly different as it depends on the origin of the worker. In other words, if we are considering workers in the electricity industry, then there is a diversion ratio for these workers which captures how movement across industries for electricity workers changes in response to a change in the wage in the electricity industry. To be concrete, the general form of this diversion ratio between m and k is equal to:

$$D_{m\to k} = \frac{\partial P(k|m)/\partial V(m|m)}{-\partial P(m|m)/\partial V(m|m)} \tag{43}$$

This captures how changes in job transition rates to k change after a change in the value of working in market m. One could also write out the diversion ratio as a function of wage changes, for example, if $V(m|m) = \beta_m w_m + \xi_m$ where β_m dictates how sensitive worker utility is to the wage and ξ_m is some amenity common to all workers in market m. Instead, I have written the diversion ratio as a direct function of the value of the job, rather than the wage associated with the job. Under the assumption that utility is given by $U_i(k|m) = ln(V(k|m)) + \xi_i$ and the error term is type I extreme value, this reduces to:

$$D_{m\to k} = \frac{P(k|m)}{1 - P(m|m)} \tag{44}$$

Therefore, in this model, flows across markets are directly informative of diversion ratios. One potential criticism of using worker flows across industries is that they could reflect aspects other than substitutability across these industries. An approach taken in the industrial organization literature would be to estimate cross-price elasticities (or cross-wage elasticities here). This would be another effective way to define markets assuming the availability of an instrument for wage changes. Instead, in this paper, I use the empirical job flows, which can be interpreted as a measure of relative substitutability across industries by assuming workers flows are generated by a discrete choice model in which idiosyncratic errors are distributed type I extreme value.