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The Labor Market Impacts of Employer Consolidation: Evidence from Germany

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Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

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Abstract

We use detailed administrative data to study how acquisitions — specifically the acquisition of a plant by a firm with a similar plant in the same local labor market — affect workers. Using an event study framework with a control group of workers at unaffected plants, we find that acquisitions lead to employment losses for workers initially employed at the acquired (or “target”) firm, associated with labor force withdrawals by older female workers. At the same time we find evidence of a rise in wages for workers initially employed at targets and at the acquiring (or “buyer”) firm who remain with the combined enterprise, concentrated among lower-wage workers. We interpret these findings as suggesting that consolidations lead to a reduction in overall employment but a rise in rents per worker that lead to a pattern of losers and winners in the labor market.

Zusammenfassung

Wir verwenden detaillierte administrative Daten die Auswirkungen von Akquisitionen - hierunter sind Akquisitionen von Betrieben durch ähnliche Betriebe im gleichen lokalen Arbeitsmarkt zu verstehen - auf Arbeitnehmer zu untersuchen. Im Rahmen einer Event Study mit einer Kontrollgruppe von Arbeitnehmern aus Betrieben, die nicht durch eine Akquisition gekennzeichnet waren, zeigt sich, dass Akquisitionen hauptsächlich zu Beschäftigungsverlusten auf Seiten der Arbeitnehmer im akquirierten Betrieb (“Target”) führen. Diese Beschäftigungsverluste sind dabei mit dem Rückzug von älteren Arbeitnehmerinnen vom Arbeitsmarkt assoziiert. Gleichzeitig finden wir, sowohl für Arbeitnehmer aus dem akquirierten als auch dem akquirierenden Betrieb (“Buyer”), Evidenz für einen Anstieg der Löhne, sofern diese Arbeitnehmer in der konsolidierten Einheit weiter beschäftigt sind. Diese Anstiege konzentrieren sich dabei vor allem auf Arbeitnehmern mit niedrigen Löhnen. Wir interpretieren diese Ergebnisse derart, dass diese Art von Konsolidierung einerseits zwar zu einer Reduzierung der Gesamtbeschäftigung, aber auch andererseits zu steigenden Monopolrenten pro Arbeitnehmer führen. Insofern ergibt sich ein klares Muster an Gewinnern und Verlieren von Akquisitionen.

JEL

J3, J23, J63

Keywords

Employment and Wage Effects, Plant Acquisitions, Worker Level Outcomes

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1 Introduction

A growing body of research shows that workers' labor market outcomes are driven in part by employer-specific factors. Workers who lose a job typically experience persistent wage declines (e.g., Davis/von Wachter, 2011), while the earnings of those who remain at a given firm vary with the fortunes of their employer (see the recent review by Card/Cardoso/Heining/Kline, 2018). Moreover, there is increasing evidence that the decisions of just a few potential employers can substantially affect wages in a local labor market (Azar/Marinescu/Steinbaum/Taska, 2018). These patterns have led to renewed interest in the connections between product market concentration, labor market concentration, and aggregate wage and employment outcomes.

This paper contributes to the understanding of firm-specific pay premia and changes in concentration on workers by studying how specific expansions of the boundaries of firms — acquisitions — affect labor markets. We use matched employer-employee earnings records from West Germany to identify and study 243 horizontal acquisitions between 1989 and 2008. Expansions to the boundaries of firms provide a unique testing ground for understanding whether firms play a causal role in the determination of workers' outcomes. Because horizontal acquisitions serve to increase product market and labor market concentration, estimating their causal effect on workers' earnings sheds new light on how changes in industrial concentration impact specific workers instead of aggregates.

We identify acquisitions using a clustered worker outflow methodology based on methods used to identify firm births in administrative data.¹ Specifically, we focus on a set of acquisitions of a given plant by a buyer that operates a similar plant (in the same 5-digit industry) in the same local labor market. The acquisitions in this study are associated with gradual declines in employment at the consolidating establishments, which shrink by an average of 27 percent in the five years following an acquisition.

We hypothesize that such horizontal acquisitions exert three countervailing forces on workers. First, because employment declines, some workers are displaced. Second, for workers who remain, rents per worker have arguably increased, leading to upward pressure on wages. But third, because there are fewer employers competing for workers in the local market, workers' bargaining power may have fallen, leading to downward pressure on wages.

To estimate the effect of acquisitions on the earnings and employment of workers whose employers are acquired (“Targets”) and workers whose employers purchase another (“Buyers”) we use a simple generalized difference in differences framework. Our analysis focuses on workers with stable employment relationships lasting at least four consecutive years preced-

¹ See e.g. Benedetto/Haltiwanger/Lane/McKinney (2007); Hethcote/Maier/Schmieder (2013).

ing an acquisition. To identify earnings and employment changes caused by acquisitions, we use a sample of untreated workers and examine variation in outcomes associated with variation in treatment within cells derived from interactions of worker covariates. In our baseline results, we allow for both intensive margin and extensive margin responses to the acquisition to occur.² We find evidence that acquisitions reduce the employment earnings of Target workers by Euro 552 five years after the acquisition and do not lead to substantive changes in the earnings of Buyer workers.

Underlying these muted overall impacts of acquisitions on Target and Buyer workers, we find two groups of winners and losers. The main losers from acquisitions are women over 48 years of age, especially those employed at Targets in the years preceding an acquisition. For these individuals, acquisitions cause large (6 ppt.) decreases in private labor force participation. We see no decline in the labor force participation for men over 48. Our results are consistent with older women retiring early: the early old-age pension claiming age is lower for women in Germany (Börsch-Supan/Wilke, 2004). Winners include Buyer workers under the age of 40, whose earnings increase by Euro 379 five years after an acquisition. This is an increase of approximately 1 percent from Buyer workers' average pre-acquisition earnings of Euro 40,806.

To explore whether remaining within the boundary of the Buyer establishment matters for wage outcomes, we turn to the sample of workers who remain in the labor force in all years after the acquisition. We divide this sample into two groups: people who are employed at the Buyer in the fifth year after an acquisition (“retained”) and those who are not (“not retained”). We observe small differences in pre-determined characteristics between these two groups. This similarity suggests that selection on observable characteristics for retention is limited to nonexistent. Partial correlations between subsequent wages and treatment among workers in this selected sample yield strong evidence that retention within the boundary of an acquiring firm matters for wage growth. The wage growth of workers who are not retained by the Buyer is no different than those of control workers who are not retained by their pre-acquisition establishments (in which no identifiable acquisition occurs). In contrast, the wages of retained workers employed by the Buyer or Target in the years preceding acquisition rise by Euro 237 and Euro 509 per year, respectively. In short, the characteristics of workers retained within the boundary of consolidating establishments do not substantively differ from those who are not, but their wage growth is much higher. Our interpretation of this finding is that the boundary of the firm itself, not selection, drives wage outcomes.

Our results are consistent with a simple model of wage determination in which acquisitions cause rents to rise at the consolidating establishment by an amount that exceeds any erosion of workers' bargaining power. We posit that increased rents are one channel through which consolidation affects workers and inter-firm wage differentials emerge.³

² In our context, an “exit” from our labor force sample is an exit from private employment to East Germany, entrepreneurship, or government employment.

³ We cannot rule out that retained workers are selected based on an unobserved, time-varying component of productivity that abruptly changes when the firms consolidates.

To better understand how rents from the acquisition are shared with retained workers, we split Buyer and Target workers based on their position in the top half or bottom half of their establishment's pre-acquisition wage distribution. We find that ex ante *lower* wage workers experience the bulk of the wage growth. Lower paid Target workers' wages rise 2.9 percent more than controls with similar ex ante real wages in the five years following an acquisition while higher paid Target workers' wage growth does not differ from controls'. Among Buyer workers, the patterns are similar: wage growth of lower paid workers ex ante also exceeds that of controls by 2.0 percent and higher paid workers ex ante do not fare differently than controls with similar wages. This set of results is consistent with another prominent study of firm boundary changes, Goldschmidt/Schmieder (2017); firm-specific profits lead to wage premia paid to a firm's lower wage workers, which are extended only within the boundary of the firm.

To conclude, we present preliminary results that examine spillovers from acquisitions to other workers. This section builds on the observation that workers move up a "job ladder," taking jobs of successively higher pay or quality (Burdett/Mortensen, 1998). The average decline in the total number of workers at Buyers in the years following acquisitions reflects a slowdown in hiring. We document whether the decline in demand for labor at consolidating establishments affects the wage growth of workers below Buyer establishments on the job ladder. Simple correlations indicate that larger reductions in Buyer hiring result in slower average wage growth at establishments that typically send workers to Buyers. This is consistent with the idea that increases in labor market concentration may break a rung in some workers' job ladder, inhibiting their ability to renegotiate with current employers. In future work, we hope to explore the importance of this mechanism for explaining spillovers from changes in concentration and other firm-level shocks, such as firm death.

This paper is a contribution to three areas of research within labor economics. The first is to the longstanding question of how product and labor market concentration affect workers' employment and wages.⁴ Interest in this question has been revitalized in recent years (Autor/Dorn/Katz/Patterson/van Reenen, 2020; Barkai, 2017; Azar et al., 2018). Our contribution is to evaluate the impact of specific events that affect product and labor market concentration on a clearly defined group of treated workers relative to a set of counterfactual workers. Though data limitations prevent us from computing the first stage relationship between measures of product and labor market concentration and the acquisitions in our sample, close industrial and geographic proximity of the Buyer and Target establishments imply that our acquisitions place upward pressure on both. Our results show that costs and benefits of changes in concentration are not borne equally by all groups of workers. Older women work less and likely retire early, and only those workers who are retained by Buyers receive markedly higher wages. Among workers who remain in the labor force, retention by the target is not strongly

⁴ See Section 2 for a lengthy discussion of this literature.

related to observed worker characteristics. Taken together, these results suggest that rents shared with retained workers, not unobserved productivity, drives the returns to retention.

This paper adds to a nascent literature that documents the consequences of changes to the boundaries of firms for workers (Weil, 2014). In a seminal contribution, Goldschmidt/Schmieder (2017) study how specific contractions to the boundaries of German firms affect the wages of workers left outside of them. Our contribution is to study the consequences of *expanding* firm boundaries for workers. We find that remaining within a firm's borders is important for wage growth, especially for lower-paid workers, a result that is consistent with Goldschmidt/Schmieder (2017). The importance of remaining within the firm boundary after the acquisition supports the idea that some of inter-firm wage variation observed in the U.S. and Europe reflects a causal effect of firms themselves and not simply sorting.

Finally, we contribute to a smaller literature that has sought to understand the impact of acquisitions on workers. Prior research on this topic has been forced to rely on establishment-level data (Brown/Medoff, 1988) or has been concerned with firms' retention decisions (Margolis, 2006). Our worker-level data and our focus on outcomes that occur in the years after retention allow us to paint a more complete picture of the impact of acquisitions on workers than prior work.

2 Background

The idea that changes in product or labor market concentration may affect workers has arisen in several distinct areas of research within labor economics. In this section, we highlight important components of this prior research and contextualize our contribution.

Interest in the explanations for large and relatively persistent inter-industry wage differentials motivated early empirical studies of the relationship between product market concentration and labor market outcomes. This literature hypothesized that product market concentration created higher rents, which were in turn shared with workers in the form of higher wages. Typifying this era of research, Weiss (1966) examines the cross-sectional relationship between concentration and wages using a sample of workers from the 1960 Census. Weiss's cross-sectional regressions of wages on concentration and controls show that workers in more concentrated industries earn higher wages, but that this relationship is not robust to controlling for workers' observable characteristics. Weiss interprets the latter finding as evidence that observed concentration premia do not reflect the sharing of "monopoly" rents. Weiss's results also show that wages tend to be higher in concentrated industries in which there are unions, suggesting that organization of workers is an important determinant of or response to potential rent-sharing. There are many reasons — several capably outlined by Weiss himself — to doubt whether the partial correlations estimated in Weiss's models identified the structural relationship between rents and wages, but his findings motivated decades of subsequent research.

Economists revisited the relationship between product market concentration and labor earnings in the context of the deregulation and labor markets. This literature examined changes in wages paid to workers employed in industries experiencing deregulation in the United States in the late 1970s and early 1980s.⁵ One key contribution of these papers was to map *changes* in product market competition to the labor market. In cases such as the trucking, rail, and airline industries, those changes in competition resulted in changes in the number of firms operating in the market. On balance, the empirical evidence from this work supports the idea that average wages in industries such as trucking (Rose, 1987) or railroads (Davis/Wilson, 2003) fell relative to similarly-skilled workers after instances of deregulation that lead to increases in competition (and, implicitly, decreases in the extent of product market concentration) among firms in that industry. Taken broadly, these findings support the idea that a key mechanism by which product market concentration affects labor markets is the sharing of monopoly rents with workers.⁶ Because this research used aggregates or cross-sectional survey data to evaluate worker-level impacts, no definitive study was given to the disaggrega-

⁵ (Hendricks, 1994) provides a useful overview.

⁶ A key consideration of this literature was to test the idea that monopoly rents may be shared with union and non-union workers in the same way (Dickens/Katz, 1987). The prevalence of firm- and sector-level bargaining in West Germany precludes analysis of within-firm variation in unionization status in this paper.

tion of these average impacts to workers at different firms that may have experienced varying changes in the amount of rents they received.

Nearly fifty years after Weiss's original paper, economists' attention has returned to the direct consideration of the relationship between product market concentration and labor earnings. Autor et al. (2020) and Barkai (2017) document that increases in industrial consolidation are related to declines in the labor share of output. Both papers interpret their results through the lens of neoclassical models of the labor market, but the within-industry changes that are the empirical basis for these papers are broadly consistent with the results from both the original and deregulation literatures which informally rely on rent-sharing.

The U.S. takeover boom of the 1980s and 1990s motivated research into whether an activity typically associated with changes in product market concentration — mergers and acquisitions — mattered for workers. As in this paper, the establishment-level shocks studied in this literature serve to increase the concentration of both the product and the labor market. Brown/Medoff (1988) examine the evolution of wages and employment of workers in Michigan after acquisitions and find limited evidence of earnings or employment losses among workers employed at consolidating establishments.⁷ As in this paper, this literature focuses on the outcomes of workers employed at consolidating establishments in the years following a merger or acquisition. For one subset of acquisitions, Brown and Medoff find that wages increase and employment declines at establishments in years following an acquisition. For another subset of acquisitions, they find that wages are flat and employment rises at consolidating establishments. Margolis (2006) examines the determinants of retention of workers after mergers and acquisitions in France using matched employer-employee data. Margolis finds that the probabilities of retention are relatively high for workers who are involved in a merger relative to a comparison group, and that workers from the “Buyer” establishment in his context have a higher probability of retention. A main finding from the acquisitions studied in this literature is that typically immediate employment losses due to acquisitions are not large.⁸ Our work echoes these findings, although we caution that the types of acquisitions considered in this literature may not be representative of all mergers and acquisitions. Findings pertaining to wages are mixed.

A likely consequence of a more concentrated product market is that some workers may face a more concentrated labor market. For this reason, economists interested in imperfect competition in labor markets have considered how changes in product market concentration have affected workers. Simple market-level models of Cournot oligopsony imply that the market-

⁷ Use of establishment-level data in Brown/Medoff (1988) constrains them from examining worker-level outcomes, as we do in this paper. The ability to examine workers directly lets us isolate a group of workers exposed to the acquisition ex ante and to avoid conflating changes in establishment-level pay with changes in the composition of establishments' workforce. It also lets us examine how outcomes vary by retention status.

⁸ In a paper that studies specific acquisitions by private equity firms in the U.S., Davis/Haltiwanger/Handley/Jarmin/Lerner/Miranda (2012) observe similarly small net employment losses.

level rate of exploitation is the product of the inverse labor supply elasticity and a Herfindahl index of employment concentration (Boal/Ransom, 1997). While an inability to observe variation in labor demand across markets precludes a convincing empirical assessment of such claims, the insight that a market with fewer employers may be one in which those employers have more wage-setting power is intuitive. Recently, Azar et al. (2018) examine how changes in one measure of labor market concentration may affect workers. They find that increases in the concentration of postings within local labor markets are negatively correlated with posted wages, and interpret this result as evidence of wage-setting power of employers.

This paper considers acquisitions among establishments that are extremely close in product market and geographic space. Data limitations prevent us from estimating the first stage relationships between product market concentration and our events and labor market concentration and our events, but the specific context the acquisitions we consider implies that both should be positive. Although we do not directly observe product market concentration, if the sales of the establishments in the same municipality and industry cell overlap, then the acquisitions we observe in our sample will increase product market concentration, holding demand and other producers' output fixed. Although we do not directly observe labor market concentration, the close proximity of Buyers and Targets implies that the events in our sample serve to reduce workers in their local labor market face a decrease in the number of employers they face.⁹

⁹ Although we observe all workers employed in West Germany, the rules for establishment identifier assignment prevent us from constructing indices of concentration that are comparable across markets. Work measuring changes in labor market concentration within local labor markets in this paper is ongoing.

3 Conceptual Framework

Most of the issues surrounding the effects of firm consolidations on workers can be captured in a very simple model of wage determination that includes a component attributable to “rents” at a worker’s current employer. A standard version of such a model has the form:

$$w_i = a_i + \lambda_i R_{j(i)} + e_i \tag{3.1}$$

In Equation 3.1 w_i is the wage earned by worker i in some period and a_i is a benchmark “alternative wage” that a worker could earn, for example, in a competitive sector, R_j is some measure of rents (or quasi-rents) at employer j , $j(i)$ is an index function giving the identity of i ’s employer in the period, and e_i is a term capturing all other factors, and λ_i is a factor representing “bargaining power” of worker i . Such a model is often specified in standard search and matching models (e.g. Pissarides, 2000), and it can also be derived in monopsonistic wage setting models (e.g. Card et al., 2018). In the latter case, the bargaining power term λ_i is related to the inverse elasticity of labor supply for workers in the same occupation or group as individual i . Equation 3.1 is also closely related to earlier bargaining models (de Mènil, 1971; Svenjar, 1982), and is widely used in the rent-sharing literature (e.g. Card/Devicienti/Maida, 2014). A special case of Equation 3.1 is a perfectly competitive labor market, which occurs if $\lambda_i = 0$.

Assume that a model like (3.1) describes the wages of two groups of workers:

1. Workers at an establishment that is currently owned by one company and will be acquired by another company that operates a nearby establishment in the same industry. Call this group “Target” workers.
2. Workers employed in the establishment owned by the acquiring firm. Call these workers “Buyer” workers.

Prior to the acquisition, Buyer and Target workers’ wages may vary due to differences in workers’ skills or qualifications (a_i), differences in the rents at the Buyer and Target establishments $R_{j(i)}$, and differences in the degree of rent-sharing λ_i across the two establishments.

The implications of an acquisition for each group of workers depend upon changes in a_i , λ_i , and $R_{j(i)}$. Several factors affect these responses, and we examine the changes that we expect based on the context of this paper, horizontal acquisitions in narrow geographic areas in Germany. We first consider the wage responses of workers who remain employed at the consolidating establishment. We consider separations second.

For Buyer and Target workers who remain employed by the consolidated establishment, rents per worker will be the same. The equalization results in an immediate increase in the wages of workers originating from the establishment with lower rents.¹⁰ Given our context, we find it reasonable to assume that the horizontal acquisitions enhance the product market power of the establishments involved. This product market power increases the amount of rents per worker at the consolidated establishment.¹¹ This places upward pressure on wages of all retained workers, regardless of their origin establishment. The timing of this change depends upon the duration of timing required to reap increased profits.

Models of bargaining in input markets show that the change in workers' bargaining power after consolidation of employers depends upon the degree of substitutability of those employers' products (Horn/Wolinsky, 1988). Because the context of our empirical exercise is consolidation within the narrowest industry codes in Germany, we assume that the Buyer and Target produce substitutes. In this case, consolidation decreases workers' bargaining power and λ_i falls, placing downward pressure on wages. Intuitively, this happens threats to work for competing firms if higher wage demands are unmet become less credible when there is one fewer competing firm.

For simplicity, we assume that workers' alternatives a_i are unchanged by acquisitions. This may be true for many reasons, including workers are mobile or if the establishments involved are relatively small relative to the labor market. Taken together, these effects imply that the net change in wages reflect whether the declines in bargaining power or increases in rents dominate.

A worker will separate when the wage offered by the consolidating establishment is lower than some outside option. We do not formally model it, but we consider the outside option to be a function of a worker's alternative wage a_i and other factors, such as luck and a predetermined arrival rate. Our model suggests that separations will occur among three groups of workers. The first is workers at establishments with ex ante lower rents, where a given uniform reduction in bargaining power will push more workers into separations. The second is workers who experience larger reductions in bargaining power as a result of the acquisition. The first two factors are separations driven by reductions in the wage offer received from the consolidating firm. The third is workers who could have shared in increased rents at the consolidating establishment but take an even better outside offer. Average wage trajectories of separating workers can help disentangle which of these impacts dominates. If separating and retained workers experience average wage growth, this is consistent with the consolidating establishment offering a share of rents to all workers. If wage growth of separating workers

¹⁰ This change will occur even if ex ante wage levels are similar.

¹¹ See e.g. FTC/DOJ Merger Guidelines 2010.

is modest while retained workers' wages rise, that implies some workers do not receive an opportunity to share in rents at the consolidating establishment.

The destination to which workers separate will reflect the source of their alternative offer. For younger workers, this alternative offer is more likely to be an employment offer from another private firm, public sector employment, or entrepreneurship. For older workers, the alternative offer is likely to be retirement.¹²

Our simple model implies that rents per worker likely put upward pressure on wages and a decline in workers' bargaining power after an acquisition exerts downward pressure on wages. Among workers who remain at the establishments involved in the acquisition, the net wage change indicates the dominant effect. Workers who separate are concentrated among firms with lower rents ex ante and groups of workers with larger reductions in bargaining power; the destination after separation reflects the source of the alternative wage offer. The similarity of retained and separating workers' wage growth reflects the extent to which all workers may share in product market rents.

¹² Collective bargaining, another factor that we do not directly observe, also plays a role in the determination of subsequent employment.

4 Data, Terminology, and Identifying Acquisitions

In this section, we describe our data, we construct a taxonomy of the entities that employ workers and discuss whether we observe them in our data, and we explain our worker-flow based methodology for identifying acquisitions.

4.1 Data

We use a dataset of matched worker-establishment earnings histories that covers all private employment spells qualifying for social security taxes in Germany.¹³ The data are maintained by the German Institute for Employment Research (IAB), they are commonly known as the Integrated Employment Biographies (IEB), and they have been used extensively in empirical economic research. The IEB data present two main advantages for our objective of identifying the impact of acquisitions throughout the labor market. First, they contain an identifier designating an employer associated with each employment spell. We combine the establishment identifiers with knowledge of the rules for establishment identifier assignment and mass outflows of workers across identifiers to pinpoint acquisitions of one workplace by another (see Section 4.3). Second, our ability to observe the earnings histories of specific workers affected by workplace consolidation allows us to observe the earnings and employment of workers over time regardless of their private employer. This lets us assess the impact of an acquisition on workers who separate.

To summarize workers' activities during a year, our analysis uses a person-by-year dataset that contains information about each worker's employer on June 30 of each year.¹⁴ We record each worker's total annual earnings and average daily wage across all employment spells and for those associated with the June 30th employer. We also include information on total annual unemployment benefit receipt, and we exclude workers with more than three years between employment or unemployment benefit receipt spells from our sample.

¹³ Approximately 80% of all spells in Germany qualify for social security (Card/Heining/Kline, 2013). The worker groups excluded from these data are civil servants and the self-employed.

¹⁴ Using June 30 or July 1 as a reference date is consistent with other research that converts the IEB spell data into a person-by-year panel (Jäger/Heining, 2019).

4.2 Terminology

Matched employer-employee earnings datasets such as ours are often derived from government records collected for the purposes of taxation or social benefit distribution. Researchers typically refer to the entities associated with various employer identifiers in such datasets as “firms,” even though the relationship between such identifiers and the entities that organize production may vary across datasets. To facilitate accurate comparisons of our findings to other contexts, we describe our taxonomy of the entities that employ workers in our data.

A **workplace** is a single physical location dedicated to the production of a good or services. An **establishment** is one or more workplaces allocated a single establishment identifier (*Betriebsummer*) by the German Employment Agency (BA). A **company** is a legal entity that owns one or more workplaces. In our dataset, we directly observe establishments *only*, but in certain contexts we can infer that establishments are owned by distinct companies.¹⁵ We describe this process, which is crucial for our ability to identify acquisitions, in the following section.

4.3 Identifying Acquisitions Using IEB Records

Our objective is to identify acquisitions, which we define as instances where a “Buyer” establishment owned by one company takes control of a large share of the factors of production of a “Target” establishment owned by a different company. An accurate methodology for identifying acquisitions using employer-employee earnings records must identify (1) distinct companies and (2) a change in the ownership of factors between distinct companies. To identify distinct companies, we use the German Employment Authority’s rules for assignment of establishment identifiers. It is these rules that govern the assignment and maintenance of the identifiers used for the purposes of selecting social security taxes and it is these identifiers which we observe for every employment spell in our data. To identify a change in control of the establishment’s productive resources, we examine instances in which a large share (over 50%) of an establishment’s workers appears at an establishment owned by a different company in the year after that establishment dies. Using the IEB data allows us to identify acquisitions only within narrow geographic and industry cells. We identify 243 acquisitions occurring within narrow industries and within close spatial proximity with our approach. To identify changes in the ownership of factors of production, we use clustered outflows of one factor, workers, combined with the death of the establishment originating the outflow. The remainder of this section describes the specific details of our methodology.

We distinguish distinct companies in the following way. Earnings records in the IEB are al-

¹⁵ Distinct establishments within the same municipality and detailed industry are distinct companies. See Section 4.3.

located an establishment identifier, also known as a Betriebsnummer. The Betriebsnummer, which provides the basis for the distinct employers identified in research papers such as Jäger/Heining (2019), is the administrative identifier used by the German social security system.

We first describe how the German Employment Agency (BA) assigns establishment identifiers to new workplaces. Workers who are employed at a new workplace owned by a company that does not already own a different workplace within the same municipality and narrowly-defined (5-digit) industry are assigned to a new establishment identifier (Bundesagentur für Arbeit, 2007).¹⁶ If the company has already been assigned an establishment identifier that falls within the same municipality-by-industry cell, individuals at the new workplace are allocated that company's existing establishment identifier. In the latter case, no new establishment identifier is assigned despite the different physical locations of production.

When a workplace or establishment changes ownership, the BA assigns a new establishment identifier to the workers who have experienced the ownership change. The BA follows the rules governing the allocation of new establishment identifiers in these cases. Thus, if the company that acquired the target establishment owns other establishments in the same municipality and industry, the acquired workers are allocated the Target's establishment identifier in the following period. Otherwise, those workers receive a completely new establishment identifier.

These rules imply the following three-step process to identify the transfer of workers across distinct companies.

1. Using our worker-by-year dataset, identify a subset of mass clustered outflows from one establishment to another where the "sending" establishment dies in the year of the outflow. To be considered a mass outflow, over 50 percent of the workers employed at an establishment in year t must be employed together elsewhere in year $t+1$. We adapt these thresholds from work that uses mass clustered worker outflows to identify firm births (Benedetto et al., 2007; Hethey-Maier/Schmieder, 2013).
2. We identify the subset of mass outflows in which the establishment that receives the outflow existed in parallel with the originating establishment within the same detailed industry and municipality in the four years preceding the outflow. This condition ensures that the flow of workers occurs across distinct company lines.
3. We condition on the size of both the sending and receiving establishments exceeding 20 workers two years before the year of the outflow. We do this to ensure that the transac-

¹⁶ Industry codes have changed over time in Germany and were only available at the three-digit level before (Eberle/Jacobebbinghaus/Ludstek/Witter, 2011). Our understanding is that the most detailed contemporaneous industry code available has been used for betriebsnummer assignment. Thus we use 3-digit industry codes before 2000 and the appropriate contemporaneous 5-digit code afterward.

tion is real. We eliminate establishments with 10,000 workers or more to prevent a single transaction from dominating the results of our worker-level analysis.

Because both establishments had different identifiers but were within the same industry and municipality in the years before the acquisition, we infer from the rules for establishment identifier assignment that the workers at those two establishments were employed by different companies. If all criteria are satisfied, the establishment that dies and sends the outflow is considered to be the Target and the establishment that receives the outflow is the Buyer. The clustered outflow of workers from the target establishment and the target establishment's death imply that it was an acquisition.

We limit our analysis to acquisitions to establishments in the Finance and Insurance, Manufacturing, Wholesale and Retail Trade, and Service sectors in which both the target and the acquired establishments exceed 20 workers in size two years before the acquisition. We impose establishment size restrictions because mass clustered outflows from very small firms is unlikely to reflect changes in ownership. We select the four groups of industries as they comprise the vast majority of the acquisitions that we identify in our sample. For establishments that appear as Targets in different years, we select the acquisition that occurs first.

Our methodology detects several types of acquisition. It detects subsets of a national acquisition between multi-establishment Targets and Buyers where the Target and Buyer have workplaces in the same municipality and industry. It detects instances in a large, multi-establishment company combined with a single-establishment company that operates within the same municipality and industry cell. It detects consolidation of two single-establishment companies. And it detects partial acquisitions in circumstances when a Buyer acquires at least one establishment from a Target. A shortcoming to our analysis is that we cannot distinguish between the cases listed above.

The acquisitions in our sample may occur for a wide variety of reasons, including the ability to more productively utilize a Target's factors, a desire to exert pricing power in the product market, or to acquire the intellectual property. Some prior research has attempted to categorize acquisitions based on the reasons they occurred (Brown/Medoff, 1988). We do not have information that allows us to confidently categorize the reason for acquisitions, and so we leave analysis of motivations to researchers with data better equipped to explore this topic.¹⁷

¹⁷ We have examined variation in establishment-level outcomes among buyers and suppliers with differing wage and employment growth profiles in the pre-acquisition period and found no substantive differences in establishment-level employment in wages across permutations.

5 Characteristics of Targets and Buyers

The methodology outlined in Section 4.3 allows us to identify a specific set of acquisitions. In this section, we describe the evolution of employment and average wages at Buyers and Targets.

Table 1 reports the average characteristics of the 243 transactions that we identify between 1989 and 2015 in the year before the transaction occurs. Columns 1 and 2 of Table 1 compare the average characteristics of the target and buyer establishments in our sample. As one would expect of similar establishments located within a local labor market, buyers and targets are quite similar along many observable dimensions, including average wages, share of female workers, and skill groups. Buyers and Targets differ with respect to their size: the average Buyer size is 2.96 times the size of the average Target in the year preceding the acquisition. The number of Target establishments exceeding the number of Buyer establishments reflects the fact that there are several multi-Target transactions in our sample. In the remainder of this paper, we analyze multi- and single-Target transactions together. In cases where Buyers undertake multiple transactions, we analyze the first.

Panel A and Panel B of Figure 1 depict the average size of Targets and Buyers over event time in terms of total number of workers (Panel A) and log number of workers (Panel B). Logs and levels are presented as total establishment size exhibits some dispersion. Buyer establishments remain roughly flat in size in the four years preceding an acquisition, falling an average of .5 workers between the year four years before an acquisition and the year before an acquisition. Average Target establishment size declines in the years preceding an acquisition, which fall from an average size of 62 workers to an average of 58 workers (including both part time and full time employees) between four years and one year before an acquisition.

Event time 0 in Figure 1 is the year in which the acquisition occurs. Panels A and B show that in the year of the transaction, the average Buyer increases in size by 58.2 workers, approximately the average size of a Target establishment. This change in size reflects all workers employed at the establishment, not solely individuals who are acquired from Target establishments. The magnitude of the overall increase in event year Buyer size foreshadows a finding that we will explore in detail in Section 6: on average, there are not large layoffs of Target workers in the year of the acquisition.

The average Buyer establishment declines in size in the years after an acquisition. The evolution of the size of the Buyer establishments over time is shown between event time 0 and event time 5. The average size of Buyer establishments falls by 19.1 workers in the post-event period. The average decline is attenuated slightly by growth among larger Buyers; the average size of Buyers falls 27 log points (or approximately 27%) according to Panel B. The decline

in average size does not reflect the exit of Buyer establishments; in our sample we condition on the survival of Buyer establishments from event time 0 until event time 5.

Panel C of Figure 1 depicts the average real log daily wage paid by buyer and target establishments over event time.¹⁸ Compensation at average Buyer and Target establishments is nearly identical in the years preceding an acquisition. The trajectories of average wages at Buyers and Targets are quite similar in the years preceding the acquisition, with wage growth and levels at the average Buyer establishment slightly exceeding that of the average Target. Four years before the acquisition, the gap in average daily wages at Buyer and Target establishments is .4 percent, favoring Buyer establishments. In the year before the acquisition, this gap rises to 1.3 percent.

Increases in wages at Buyers over time are consistent with rents rising at the Buyer establishment and a degree of rent-sharing with workers. Yet looking at the average Buyer wage over time does not provide a satisfactory view of the importance of the boundary of the firm or the impacts of consolidation on workers. It only shows us what happens to retained workers and misses workers who leave to work elsewhere. Additionally, changes in the composition of workers at the Buyer over time obscure impacts on workers involved in the acquisition. For example, if the wages of incumbents rise drastically and offset decreases in the wages offered to new hires due to a Buyer's increased labor market power, we will mistakenly conclude that the consequences of acquisitions are limited.¹⁹ The purpose of the following section is to describe these effects in detail.

¹⁸ The phrase "event time" refers to the years preceding and succeeding an acquisition.

¹⁹ This reliance on establishment-level data is a shortcoming of prior analysis of this topic, e.g. (Brown/Medoff, 1988).

6 Direct Worker-Level Impacts

The objective of this section is to estimate the effect of an acquisition on the employment and earnings of workers employed at Target and Buyer establishments in the years preceding acquisition. We begin by describing the workers whom we define to be directly impacted by acquisitions and the process of selecting control workers.

6.1 Sample Construction

Our sample contains both treated (i.e. Target or Buyer) and control workers with persistent employment relationships at the establishments in question. We define a set of potential treated workers as individuals who are employed at a Buyer or a Target establishment on June 30 of the two years preceding an acquisition. The choice to condition on tenure follows research on job displacement, which conditions on tenure to isolate workers who have a substantive employment relationship at a treated establishment.²⁰

To construct counterfactual outcomes for workers involved in an acquisition, we select ten untreated “control” workers per treated worker at random and use variation in treatment status and outcomes within covariate combination cells to identify the effect of an acquisition. We select control workers from a risk set of untreated workers. To appear in the risk set of potential control workers matched to treatment workers in year t , we condition on the same criteria necessary for a worker’s inclusion in the treatment sample. Thus, potential controls for workers treated in an acquisition in year t must be employed at the same establishment in years $t = -1$ and $t = -2$, the size of that employer must exceed 20 workers in $t = -1$ and $t = -3$, the establishment must survive until $t = 5$, and they must be between age 25 and age 55 in year t . Our final control sample selects ten control workers at random (with replacement) from the set of potential year t controls for every worker treated in year t . Treatment and control workers in year t are assigned to a “cohort” c_i .

After selection of the control group, we further restrict the sample to include treated and control workers who are employed at the same establishment in each of the four years preceding the acquisition (or placebo acquisition) year. The number of controls is not exactly ten times the number of treated observations for this reason.²¹ We make this tenure restriction for the sake of comparability of other research on employer-level shocks. All of the comparisons our

²⁰ We also estimated the worker-level models considered in this section using the set of workers employed at the two years prior to an acquisition; the quantitative and qualitative conclusions of the direct worker-level analysis did not change.

²¹ Results with the 2-year and 4-year tenure restriction are qualitatively similar.

regressions undertaken are between treatment and control observations within cohorts because they satisfy the same employment and tenure restrictions in the same set of calendar years.²² We condition on employment in the four years prior to an acquisition to better compare the pre-acquisition wage growth between treatment and controls.

Table 2 presents means of worker and establishment characteristics for Buyer, Target, and control workers as of the year before treatment (or in the case of Control workers, the year of treatment of the cohort to which they are assigned). Consistent with the similarity of the average characteristics of Target and Buyer establishments described in Section 5, observable characteristics of Target and Buyer treated workers in the year before treatment are nearly identical. The average establishment size for Buyer and Target workers in Table 2 exceeds that reported in Table 1, a mechanical result of using a worker-level dataset. The average relative size of Buyer and Target workers' establishments is approximately 1/3, nearly the exact same ratio as that reported in the establishment-level sample.

The average educational attainment, age, and sex of the control group of workers are similar to both groups of treatment workers. Average earnings of control workers are roughly 5 percent lower among control workers compared to both Buyer and Target workers, whose earnings are very similar. Average establishment size among comparison workers also exceeds that of the average size of treated workers, a difference that is consistent with the random sampling of untreated individuals undertaken to construct the control group.

Our identification assumption is that within a cohort the occurrence of an acquisition is unrelated to those workers' potential outcomes, conditional on a wide array of interactions between worker and establishment characteristics. In its conditional independence assumption and the comparison of treatment and control workers within cells of demographic and establishment characteristic combinations, our approach is similar to propensity score matching (Imbens/Rubin, 2015), a research design utilized by several papers concerned with the worker-level impacts of establishment-level shocks (e.g. Jäger/Heining, 2019; Goldschmidt/Schmieder, 2017).

Estimating impacts of the acquisition on treated workers relative to controls within covariate groups presents two main advantages over a pairwise matching approach for constructing worker-level counterfactuals. First, it allows us to utilize a unique advantage of our data — that we observe complete earnings histories for all workers in Germany — to construct counterfactual outcomes. A nearest-neighbor propensity score match, for example, would force us to select a single control per treatment observation. Selection of a single nearest neighbor control unit may result in overfitting in the pre-event period, leading to differences between

²² Use of observations from distinct cohorts to identify a common year effect results in control observations from one cohort being used to identify year effects in which another cohort has been conditioned to be employed.

treatment and control to arise from the limited set of control workers' firm-level shocks in the post period. By law of large numbers logic, drawing ten controls per treated unit makes the average control worker's firm-level earnings innovation more representative and alleviates concerns of overfitting. Along with alleviating concerns of possible bias from overfitting in the pre-event period, drawing several controls per treatment improves the precision of our estimates. This implies that more controls per treatment are preferable. We choose ten to balance this advantage with computational constraints that arise from large samples.

The second advantage of variation within demographic by cohort cells is that it allows us to use standard methods of large sample variance estimation that do not require adjustment for estimation of the first step propensity score (Abadie/Imbens, 2016). We find our approach appealing because of its transparency and ease of implementation.

A potential drawback to using an OLS estimator relative to matching methods is that is not robust to substantial differences in covariates between treatment and control units (Imbens, 2014). We find that the means of the distributions of covariates are similar across Target, Buyer, and Control units. We interpret this similarity as evidence that our choice of OLS is likely to yield similar estimates to conventional matching estimators.

6.2 Unadjusted Differences Between Treatment and Control

Figure 2 depicts the evolution of employment earnings, employment, employment at the consolidated entity, and annual UI benefit receipt among workers in the Target, Buyer, and Comparison groups in the four years before the acquisition through five years after the acquisition. The figure plots means and standard errors of relevant variables without adjusting for any differences in other covariates. The purpose of these figures is to illustrate the nature of the underlying data.

Figure 2 Panel A depicts the average total earnings (in 2010 Euro) among Buyer, Target, and Comparison workers over event time. This figure includes both intensive and extensive margin responses of earnings; individuals who do not appear in the sample as employed or receiving unemployment insurance benefit income on June 30 of a particular year are assigned a value of Euro 0.²³ This figure depicts an average decline in total earnings for control workers from an average of Euro 37,716 in the year before the acquisition to Euro 32,588 in the fifth year after the acquisition, an average decline of 13.5 percent. In nominal and in percentage terms, average earnings losses were more severe for both types of treated workers. Target

²³ We choose June 30 so that our counts of workers correspond to an establishment roster dataset collected by the IAB (the BHP) whose data reflects establishment composition as of June 30 of each year. The decision to focus on individuals' activities as of June 30 of each year is common in research that uses the IAB worker histories (Jäger/Heining, 2019; Schmieder/von Wachter/Heining, 2018).

workers' average earnings fell from Euro 40,632 to Euro 34,503 (-15.1%) while Buyer workers' average earnings dropped from Euro 40,806 to Euro 35,419 (-13.2%) over the same time period.

Panel B shows that five years after the acquisition, employment losses are more severe for Target workers (-20.5%) than both Buyers (-18.5%) and Controls (-17.5%). Panel C depicts the average log daily wage among workers who remain employed and indicates that rates of average wage growth among workers who remain employed are *higher* for workers in Buyer (5.8%) and Target (5.3%) establishments than controls (4.0%). This suggests that it will be important to consider workers who remain in the labor force separately.

Panel D depicts that rates of employment at the “consolidated establishment” are lower for Target and Buyer workers than controls. For Target and Buyer workers, we define the consolidating entity a worker's $t = -1$ establishment in the pre-event period and the Buyer in the post-event period. For Controls, the consolidating entity is a worker's $t = -1$ establishment in all years. Average annual amounts of unemployment insurance benefit receipt (Panel D) rises in the years following the sample.

Taken together, the average trajectories of treated workers relative to controls suggest that acquisitions create two sets of winners and losers. Workers at Target and Buyer establishments are more likely than controls to leave private employment in West Germany and both groups receive more average annual unemployment insurance benefits. Those who remain employed, however, experience higher rates of wage growth than controls.

6.3 Worker-Level Regression Estimates

To quantify the effect of the acquisition on the outcomes of Buyer and Target workers, we estimate a standard event study regression model that identifies the effect of each treatment based on variation in treatment status within cohort by year by covariate group cells. Specifically, we estimate the following regression:

$$y_{it} = \alpha_i + \delta_{t,c(i),X(i)} + \xi_{j(i)t} + \sum_{k=-4}^5 T_{it}^k \beta^k + \sum_{k=-4}^5 B_{it}^k \gamma^k + \epsilon_{it}. \quad (6.1)$$

6.1, y_{it} denotes worker i 's year t outcome. α_i are person fixed effects, and $\delta_{t,c(i),X(i)}$ are cohort by year by covariate group fixed effects.²⁴ In our preferred specification, $X(i)$ is an indi-

²⁴ For control individuals characteristics are assigned on the basis of an individual's characteristics in the year

cator variable for the interaction of person i 's gender, skill group,²⁵ within cohort wage decile (measured in $t=-1$), and $t = -4$ to $t = -1$ establishment average wage growth. We discuss the selection of these covariates in the following section. $j(i)$ is the geographic district in which the worker is employed in year preceding the event $t = -1$. $\xi_{j(i)t}$ is a geographic district (*kreis*) by year effect that is common across cohorts.²⁶ T_{it}^k and B_{it}^k are interactions between indicator variables for event-time year k and pre-employment status at a Target or Buyer.

Estimates of the coefficients of interest in equation 6.1 summarize average differences between treatment and control workers of each type in each year from four years before to five years after the acquisition. Under our identification assumption that a worker's assignment to an acquisition is unrelated to their potential outcomes conditional on $X(i)$, β^k and γ^k identify the effect of the acquisition of treated workers k years from the treatment.

We estimate the regression on the balanced panel of 3,835,040 observations, comprised of 383,504 workers and 10 observations per worker. In all specifications, we compute standard errors clustering by pre-event year establishment.

6.4 Covariate Selection

In Equation 6.1 $X(i)$ is the indicator variable that denotes worker i 's combination of covariates. As is typically the case when conducting empirical research, we face a trade off between the internal consistency and external validity of our regression model (DiNardo/Lee, 2011). Adding more interactions to the set of characteristics that comprise the cells of $X(i)$ makes conditional independence more plausible while narrowing the generalizability of the parameter we estimate, which is the average treatment effect conditional on the set of covariates specified.

A typical benchmark for assessing the suitability of a control group to serve as a counterfactual for treated units in a generalized difference in differences framework is the evaluation of "pre-trends." In this context, we evaluate whether this assumption is satisfied for differing sets of potential covariates to be included in this paper's baseline regression model. To illustrate the covariate choices that we make and how they affect one's interpretation of the pre-trends in the event study specification, we conduct an analysis that summarizes the pre-trends conditional on our choice of covariates.

preceding the cohort to which they are assigned. See Section 6.4 for details.

²⁵ We use three skill groups based on an individual's education or vocational training: college educated, not college educated but with vocational training, and less than vocational training/other.

²⁶ Kreis, or "districts," are areas that are larger than municipalities but which tend to be smaller than local labor markets (Kropp/Schwengler, 2017).

Table 3 presents the set of potential covariates that we consider. The number recorded next to each covariate represents a regression model that includes that covariate and all lower numbered covariates in the set of discrete covariates used to construct X_i . Each number represents a regression model that includes interactions of the covariate next to the number along with all lower-numbered covariates with cohort by year effects.

The y-axis of Figure 3 plots the slope of a four-observation regression of the coefficients β^{-4} , β^{-3} , β^{-2} , and β^{-1} on event time for the corresponding event years. The x-axis denotes a particular regression model used to generate the estimates of $\{\beta^k, \gamma^k\}_{k=-4}^{k=-1}$. The covariates used in each model are listed in the bottom panel of Table 3.

Our preferred set of covariates is Model 5, as it minimizes the total pre-event trend slope. The addition of establishment size quartiles does not markedly change the magnitude of the pre-event estimated slope coefficient, while further limiting the set of comparisons to be conducted among controls.²⁷ Qualitatively and quantitatively, estimates from Models 4 and 6 are highly similar to those from Model 5.

²⁷ As the set of treated workers come from several establishments whereas comparison workers come from many different establishments, making comparisons within establishment size category creates many covariate cells with low proportions of treated workers.

7 Results

This section presents estimates of Equation 6.1, beginning with the full sample of workers in Section 7.1. We find that acquisitions cause modest earnings declines for Target workers and have no discernable effects on the outcomes of workers who were employed at Buyer establishments before the acquisition. In Section 7.2 we examine how acquisitions affect different demographic groups of workers. This analysis reveals substantial heterogeneity in acquisitions' impacts across demographic groups: acquisitions appear to cause women over 48 to retire early while there is evidence that younger men employed at Buyers earn more on average despite some employment losses.

7.1 Full Sample: All Workers

Coefficients and 95 percent confidence intervals from estimates of β^k and γ^k from Equation 6.1 using annual employment earnings, employment, employment at the “consolidating entity,” and annual UI benefit receipt as dependent variables. A clear pattern of reductions in the extensive margin of employment, especially among workers at Target establishments, dominate our full-sample results.²⁸

Figure 4 Panel A depicts the coefficient estimates from models in which the dependent variable is workers' total employment earnings. In this model, zeroes are assigned to workers who disappear from the sample, meaning that they either drop out of the labor force, leave West Germany, or work in Germany's public sector. Although many of the estimated Target worker impacts are not statistically significant at conventional levels, the temporal pattern of the coefficient estimates indicates that Target workers experience both an immediate loss in earnings which gradually increases in severity over time. Five years after the acquisition occurs, estimates show that workers employed at Target establishments in the years preceding acquisition experience reductions in employment earnings of Euro 552 ($t=-1.7$). This is a 1.6 percent decline from the target workers' average earnings in the year before the acquisition.

Figure 4 Panel B displays coefficients estimated by the regression in Equation 6.1 with an indicator for employment as the dependent variable and shows that both immediate and medium-run extensive margin impacts of the acquisition are more severe for Target workers than Buyer workers.²⁹ Acquisitions cause immediate reductions in employment of 1 percent-

²⁸ Recall that employment sample we use is the set of workers who are employed in West Germany at private establishments who are not entrepreneurs or in most parts of the public sector.

²⁹ Employment is having any employment spell ongoing as of June 30 of a calendar year. See Section 4.1 for details. Coefficients in pre-event years are normalized to zero because of sample restrictions.

age point for Target workers. The immediate reductions in employment increase over time, reaching 2.2 percentage points ($t=-3.4$) in the fifth year after the acquisition. The acquisitions do not cause immediate reductions in the employment of Buyer workers. Five years after the acquisition, however, Buyer workers experience 1.0 ppt ($t=-1.92$) declines in employment.

Results from regressions in which the dependent variable is an indicator for employment at the “consolidating entity” are reported in Panel C.³⁰ Consistent with the patterns of unadjusted differences in means in Figure 2, we observe large immediate declines in the probability of employment of Target workers in the year of the acquisition (5.2 ppt; $t=-6.8$) and five years later (7.8 ppt; $t=-5.4$). The acquisition also causes an increase in the probability of a separation from the consolidating entity for Buyer workers, albeit one that emerges less rapidly than that of Target workers. The probability of remaining at their pre-event employer falls by 3.3 ppt ($t=-2.5$) for Buyer workers.

Panel D displays results from models with total unemployment benefit receipt as the dependent variable. These results mirror the effect of acquisitions on employment losses and retention: UI receipt rises immediately for Target workers (Euro 36, $t=2.4$) and becomes higher five years after the acquisition. The acquisition causes medium-run (4- and 5-year) increases in UI receipt among Buyer workers, but no detectable increase in benefits in the short run.

7.2 Full Sample: by Age and Gender

Section 7.1 suggests that in the sample of all workers that assigns values of zero earnings to individuals who do not appear in the sample, the first order effect of acquisitions is to decrease the employment of Target and, to a lesser extent, Buyer workers. This section seeks to understand whether earnings and employment losses are shared equally across demographic groups. Though our conceptual framework does not directly address employment, our data present a unique opportunity to understand whether consolidation disproportionately affects certain types of workers, a fact that may be useful for understanding how market-level changes in concentration affect between-group inequality.

We separate the sample into four mutually exclusive and exhaustive groups, which include both treated and control workers satisfying four demographic criteria: men and women who are above 40 and men and women who are below 40.³¹ We summarize the average treatment effects in the post-acquisition period by pooling event time into pre-event years and post-event years and estimating a standard difference in differences adaptation of our event

³⁰ For Target and Buyer workers, we define the consolidating entity a worker’s $t = -1$ establishment in the pre-event period and the Buyer in the post-event period. For Controls, the consolidating entity is a worker’s $t = -1$ establishment in all years.

³¹ We measure age in the year before the acquisition.

study regression model from Equation 6.1 separately by demographic group. Specifically, separately for each group g , we estimate:

$$y_{it} = \alpha_i + \delta_{t,c(i),X(i)} + \xi_{j(i),t} + \beta^g T_i \times Post_{it} + \gamma^g B_i \times Post_{it} + e_{it}. \quad (7.1)$$

In Equation 7.1 α_i are person fixed effects, and $\delta_{t,c(i),X(i)}$ are cohort by year by covariate group fixed effects; $X(i)$ denotes the indicator for the age group by gender by skill group by wage group by firm wage growth group assigned on the basis of pre-treatment characteristics to person i .³² T_i and B_i are indicators for assignment to Target or Buyer treatment status, and $Post_{it}$ is an indicator variable that equals one if event time is greater than or equal to zero. Coefficients β^g, γ^g summarize the average within-worker change in outcomes in regression sample group g in the post-event period relative to the pre-event period caused by the acquisition for Target and Buyer treated workers, respectively.

Estimates and standard errors (clustered at the $t = -1$ establishment level) for these coefficients separately for each demographic group of treatment and control observations are presented separately for the full sample and separately for each age by gender group in Table 4.³³

The most striking pattern to emerge from Table 4 is the severity of the earnings losses incurred by both Target (Euro 1,152, $t=-3.8$) and Buyer (Euro 648, $t=-3.0$) women over 40. These earnings losses are driven by responses in the extensive margin of employment. Within this group, Target (4.0 ppt, $t=5.7$) and Buyer (2.1 ppt, $t=3.8$) employment losses in the post-acquisition period are the most severe across all four demographic groups for each treatment type. Sizable effects for women's over 40 employment benefit receipt income underscore the idea that this particular group was adversely affected by the acquisitions, relative to controls within the same demographic group.

Another important finding from Table 4 is that acquisitions cause younger Buyer males to earn Euro 379 per year ($t=2.2$) more after an acquisition, even counting workers who exit employment as having zero earnings. Acquisitions do not have a distinguishable impact on younger male Target earnings despite causing substantive employment declines (-1.4 ppt, $t=2.3$) for that group. These findings suggest that acquisitions cause wage growth among younger male workers who remain employed. We explore this finding and the role of retention in depth in Section 7.3.

³² For control individuals characteristics are assigned on the basis of an individual's characteristics in the year preceding the cohort to which they are assigned. See Section 6.4 for details.

³³ The weighted sum of the demographic coefficients does not exactly equal the full sample coefficient. This is because we are estimating Equation 7.1 separately for each group (not a fully interacted model) and so the set of observations that identifies the kreis by year effect changes in each specification.

Beyond modest declines in employment among Buyer women over 40 (-2.2 ppt), acquisitions do not lead to marked declines in the probability of employment among any other group of Buyer workers. This stands in contrast to Target workers, where each demographic group besides men over 40 experiences a statistically significant decline in employment after an acquisition.³⁴

Comparing the magnitudes of the average impacts on Buyer and Target employment and earnings reveals that the two impacts are positively correlated for most groups. This fact is consistent with the suggestive evidence from Section 7.1 that extensive margin employment impacts drive earnings changes, on the whole.

Examination of Column (3) of Table 4 Panel B reveals that within each demographic group, acquisitions cause probabilities of retention at the consolidating entity to be roughly 5 percentage points lower for workers who originated at Target establishments relative to their effect on workers from Buyer establishments. As in main results, the patterns of average changes in UI income after acquisitions mirror extensive margin employment responses.

Taken as a whole, the difference in differences regressions discussed in this section show that there are clear losers from acquisitions: women over 40 from Targets and Buyers are the main drivers of the average employment losses among treated workers. Employment losses for these women over 40 are associated with higher rates of sample exit caused by the acquisition. Men over 40 at Targets and Buyers do not suffer employment losses or earnings changes that are statistically distinguishable from zero.

We cannot directly observe the reason why workers are no longer employed in our sample. However, the pattern of results that emerges in this paper is consistent with women over 40 claiming retirement benefits. In major pension legislation passed in 1972, the German government set the minimum age for old-age pension claims to 60 for women and 65 for men (Börsch-Supan/Schnabel, 1999). Pension reforms passed in 1999 raised the retirement age for women gradually to 65 by 2010. Thus, for our entire sample, the female retirement age is earlier than the male retirement age. For half of the years our sample covers, the female retirement age is five years less than that of males, a substantive difference (Börsch-Supan/Wilke, 2004).

In practice, the age at which German workers retire is often less than the eligibility age. Börsch-Supan/Wilke (2004) calculates that 20 percent of men and 25 percent of women stop working before the minimum pension eligibility age and notes that the process of “onboarding” to public pensions typically begins with the uptake of unemployment benefits in years before

³⁴ However, our sample lacks the power to reject the null hypothesis that the employment reduction in the post period within each group is identical between Target and Buyer workers.

pension eligibility. In work for a separate project that uses a 2 percent sample of West German worker histories, we observe similar patterns of spikes in unemployment benefit income for older workers across genders in the years preceding a permanent exit from private employment.³⁵

To provide circumstantial evidence as to whether retirement drives the extensive margin impacts among older women at Targets and Buyers, we fit Equation 7.1 on the set of men and women between 48-55. We choose to focus on this age group because it splits the group of workers over 40 in half and reveals whether labor force exits occur uniformly among relatively young and relatively old workers within the group. We posit that if exit is concentrated among women older than 48, this is more consistent with retirement than if both halves of this age group respond in a similar fashion.

The results, shown in Table 5, are consistent with the early retirement among women driving the employment results in the main sample. Women above 48 experience severe declines that far exceed those of average results and imply that the entire employment effects for older women are driven by women over 48. Acquisitions cause employment benefit receipt to rise dramatically for the oldest women at both Target and Buyer establishments, rising by an average of Euro 127.70 ($t=3.8$) and Euro 77.4 ($t=2.9$) per year, respectively. Though we cannot rule out that this benefit receipt occurs among workers unsuccessfully searching for re-employment, this pattern is consistent with typical pre-retirement benefit receipt patterns in Germany (Börsch-Supan/Wilke, 2004).

In contrast to the women in our sample, acquisitions do not impact the earnings or employment of male workers who are 48 and over in the year before the acquisition. If anything there is tentative statistical evidence that the acquisition increases the probability of employment for 48 and older men at Target establishments slightly (1.1 ppt, $t=1.8$).

7.3 Understanding Benefits from Acquisitions: Labor Force Sample

The discussion in Section 3 leads us to hypothesize that workers who remain with consolidating establishments will experience wage increases if, on average, the increase in rents at consolidating establishments exceeds reductions in their bargaining power.³⁶ Results from the full sample of workers in Section 7 show that acquisitions cause male workers who worked

³⁵ These results are available upon request.

³⁶ In the limiting case of competitive labor markets, any changes in wages reflect changes in workers' outside options. We assume some degree of imperfect competition in this analysis.

at Buyer establishments before the acquisitions to make Euro 379 ($t=2.2$) more per year. This result, in conjunction with the Buyer workers' relatively higher rates of retention (Figure 2), suggests that increases in product market rents may drive wage increases among retained workers.

To better understand trends in changes in compensation we restrict our sample to individuals who continue to receive some employment earnings in all five years after an acquisition, a decision that follows sample restrictions typically made in research pertaining to the impact of layoffs on wages (e.g. Lachowska/Mas/Woodbury, 2018; Schmieder/von Wachter/Heining, 2018).³⁷ Specifically, our restriction is that to be included in the “Labor Force” sample, an individual must be either on an employment contract or receiving unemployment benefits as of June 30 of each of the first five calendar years after their acquisition (or, for controls, each of the five years following the acquisition year for which they satisfied sample eligibility criteria.). Inclusion also requires having employment earnings of at least Euro 500 in each year. It is important to note that because we have conditioned on an outcome — employment earnings during the post-acquisition years — the empirical results in this section are partial correlations that are not estimates of average treatment effects. Despite this limitation, our view is that the empirical exercise is informative for understanding which workers receive wage increases after acquisitions.

After conditioning on the set of workers who remain in the labor force in all years after an acquisition, we divide Buyer, Target, and Control workers into two groups. The first group is workers who are “Retained” by the Buyer after the acquisition. To select this group, we identify those workers in the labor force sample whose predominant employer in the fifth year after the acquisition is the Buyer. Controls are categorized in the retained sample if they are employed at their $t = -1$ establishment in the fifth year after their cohort reference year.³⁸ The second group is workers in the employment sample who are not employed at the Buyer (or, for controls, reference establishment) in the fifth year after the acquisition (reference year). The purpose of this distinction is to examine (1) whether the consolidation confers any wage benefits to Buyer or Target workers who remain in the labor market and (2) whether such benefits accrue to workers only if they remain at the consolidating entity.

Importantly, because of establishment identifier assignment rules we cannot identify workers who switch establishments but who are still retained by an acquiring company.³⁹ For example, we will categorize workers who are employed at the Buyer establishment's company but who are transferred to a workplace in a different municipality or industry as not retained, even though they remain at the same company. This shortcoming should lead us to under-

³⁷ An important outlier in this literature is von Wachter/Song/Manchester (2011), which examines the importance of the inclusion of workers exiting administrative employment data.

³⁸ This restriction allows for the possibility that “retained” workers are employed outside of the consolidating entity in at least one year between the acquisition year and $t = 5$.

³⁹ These rules are described in Section 4.3.

state differences between workers who are retained and not retained.

Table 2 displays observable characteristics of workers in each sample and compares them to workers in the full sample. There are two layers of selection that occur in this table: from the full sample to the labor force sample and within the labor force sample between retention and non-retention. Patterns into retention exhibit the same patterns of extensive margin employment responses reported in Table 4. Across Target, Buyer, and Control workers, retention into the labor force sample occurs for workers who are younger, more educated workers.

Patterns of retention among control workers in the labor force sample reflect a higher degree of inter-firm mobility among higher educated workers, men, and younger workers, all of whom are less likely to be retained. We do not observe differences in ex ante log wages across retention status for controls, nor are there substantive differences in average wage outcomes. Taken together, these patterns suggest that leaving a control establishment is associated with slightly lower earnings conditional on observable characteristics.

The patterns in selection on observables evident in the control group are also present among Target and Buyer workers. Despite these similarities, average wage growth from $t = -1$ to $t = 5$ among retained workers outpaces that of non-retained workers by approximately 3 ppt. Taken together, the means reported in Table 2 indicate that while overall patterns of retention do not vary much across treatment and Control workers, wage growth conditional on retention does. This average outcome is consistent with the acquisition causing a change in rents, with that change being passed on to retained workers. In the following section, we assess the magnitudes of this impact more formally.

7.4 Results for Workers Who Separate from Consolidating Entity

As the means in Table 2 suggest, there is little evidence to support the assertion that acquisitions lead to higher earnings of workers who separate from consolidating entities but remain employed. Panel A and Panel B of Figure 5 display coefficients from estimates of (6.1) using the sample of Target, Buyer, and Control workers who are in the labor force but not employed at their consolidating entity in the fifth year after an acquisition occurs. Neither wages nor annual earnings respond to the acquisition for treated group after controls.

Panels C and D of Figure 5 show that in contrast to workers who are not retained in $t = 5$, workers who are retained by the buyer experience wage growth. Annual earnings of retained Target workers increase immediately after the acquisition by Euro 161 ($t=1.4$) relative to controls. Retained Target earnings growth relative to retained controls continues in the years follow-

ing the acquisition, reaching Euro 509 (t=2.2) in the fifth year after the acquisition. Log daily wages follow a similar qualitative and quantitative pattern relative to the pre-acquisition mean, rising .006 percent (t=1.9) immediately and eventually increasing more than 1.2 percent (t=2.7). Though there are no detectable immediate earnings or wage responses among Retained Buyers, their earnings growth outpaces that of Controls in subsequent years. By the fifth year after the acquisition, retained workers' log daily wages are .006 percent (t=1.8) higher than counterfactual, in line with estimated earnings growth of Euro 237 (t=1.9).

Differential selective retention on time-varying productivity of Buyer and Target workers relative to retention among controls may drive the earnings and wage impacts we observe in Figure 5. However, there are no substantive differences between either group of treatment retained workers and retained controls in pre-event periods, a pattern that suggests that the earnings increases to retained treated workers reflects increased establishment rents shared with such workers.

7.5 Differences by Pre-Acquisition Within-Establishment Wage Group

Prior work assessing the wage impacts of acquisitions suggests that increased rents may be shared differentially with different groups of workers (Weiss, 1966). To this end, we examine how wage impacts vary through the pre-acquisition wage distribution for treated workers by augmenting our standard event study model with interactions between an indicator for being a high-wage ex ante treated worker and the appropriate event study indicator. Specifically, we estimate the following regression:

$$y_{it} = \alpha_i + \delta_{t,c(i),X(i)} + \xi_{j(i)t} + \sum_{k=-4}^5 T_{it}^k \beta_L^k + \sum_{k=-4}^5 B_{it}^k \gamma_L^k + \sum_{k=-4}^5 H_i T_{it}^k \beta_H^k + \sum_{k=-4}^5 H_i B_{it}^k \gamma_H^k + \epsilon_{it}. \quad (7.2)$$

This model differs from Equation 6.1 in one important respect, which is the inclusion of an interaction term of the event study indicator variables for treatment with H_i , which is an indicator for whether an individual's $t = -1$ wage exceeds the median wage of their establishment in that year. Estimates of β_L^k and γ_L^k summarize average deviations of workers in the bottom half their establishment's pre-acquisition wage distribution relative to controls within the real wage decile and demographic group k years from an acquisition. Estimates of β_H^k and γ_H^k quantify differential deviations for Target and Buyer workers whose pre-acquisition wages are or exceed the pre-acquisition establishment median.

Figure 6 presents estimates of β_L^k , γ_L^k , β_H^k , and γ_H^k , from Equation 7.2 in which the dependent variables are employment earnings and log daily wages. These estimates show that on average, wage and earnings increases occur among workers in the bottom of the pre-acquisition wage distribution at Targets and Buyer establishments. According to Panels A and B of Figure 6, lower-wage workers who originated from Target establishments experience an immediate, statistically significant increase in wages of 1.2 percent ($t=4.4$) which gradually widens to 2.9 percent five years following an acquisition (Figure 6 Panel B). The coefficients on the high wage interaction term β_H^k (hollow marker) show that above-median target workers do not reap any of the wage gains accrued by their lower-paid counterparts. There are no distinctive pre-trends among these workers, suggesting an absence of differential wage growth of above- and below-median wage Target workers relative to controls within similar covariate cells.

Post-acquisition trends in the earnings and wages for Buyer workers above- and below- median wages mirror those of Targets. These are presented in Figure 6 Panels C and D. Pre-trends among these groups suggest that below-median wage Buyer workers are on a downward wage trajectory in the years preceding the acquisition. This pattern reverse after the acquisition, as wage gains accrue to this group. Coefficients on the above-median interaction term show that above-median Buyer workers do not experience differential wage growth relative to controls in either the pre-acquisition or the post-acquisition period.

Taken as a whole, Figure 6 suggests that firm rents may be an important determinant of outcomes for lower paid workers. This finding is consistent with Goldschmidt/Schmieder (2017), who find that lower paid workers may lose rents when firm boundaries contract.

These results come with one important caveat. The IEB employment dataset censors individuals' earnings above a social security earnings threshold. In this version of the paper, we do not impute earnings above this threshold Card/Heining/Kline (2013); this sample construction decision is likely to bias or work against finding impacts on the top-half of the pre-acquisition wage distribution. However, only 12 percent of the workers in the years before an acquisition have wages that hit the social security earnings threshold and are censored and, in results that are available upon request, we do not observe changes in the share of workers whose wages are censored after an acquisition. Though our setting is not ideal for detecting wage impacts on top wage earners, the preponderance of evidence suggests that even accounting for this limitation, any impacts for above-median wage workers are small.

8 Other Impacts of Acquisitions

The previous sections showed that workers employed at establishments that are involved in acquisitions tend to benefit from the consolidation, if those workers remain employed. Although they are those for which there is a well-defined research design, these individuals are far from the only workers who may be impacted by the consolidation. In this section, we outline two important groups of workers who may be affected by an acquisition.

8.1 New Hires

An important component of understanding the extent to which employers may exert market power in the aftermath of consolidation is analyzing the extent to which wages for new hires may fall. Taken together, our results on retained workers' wage growth and muted average Buyer establishment wage growth suggest that lower wages paid to new hires may balance out the wage growth observed among retained workers. Such a result is consistent with e.g. employment protections for remaining workers and firms adjusting on the margin of the wages they pay to their new hires. Due to data access constraints, our analysis of new hires is not yet complete. It will be completed in a future version of this paper.

8.2 Spillovers to Wage Growth at other Establishments: Preliminary Findings

Many models of imperfect competition in labor markets posit that a key mechanism for workers' wage growth is their receipt of outside wage offers. If the reduction in average Buyer's post-acquisition size of 27 percent reflects a decrease in the number of offers sent to workers or a reduction in the wages offered, acquisitions may be associated with slower wage growth for workers at establishments beyond those which are directly involved in the consolidation.

Wage growth spillovers are most likely to affect workers at establishments that typically send workers to Buyers or Targets. Theory and evidence pertaining to job search and mobility through labor markets indicates that individuals tend to move across firms in a way that is intended to improve their compensation or reduce their unemployment risk (e.g. Jarosch 2018). In light of this observation, reductions in hiring by consolidating establishments are most likely to inhibit the wage growth of workers below the consolidating establishment on

the job ladder. By “breaking” the rungs of the job ladder, labor market consolidation may inhibit the mobility and slow wage growth of workers who would have otherwise received an offer from the consolidating establishment.

To examine the presence of such spillovers, we must identify establishments below Buyers and Targets on the job ladder, and we must examine the wage growth and mobility of such workers. In this analysis, we focus on spillovers among workers at establishments that typically send workers to Buyers.

We identify workers who are hired by Buyers from a separate establishment in the four years before the acquisition. Using all employment-to-employment inflows to Buyers in the four years preceding the acquisition, we identify a distinct “Feeder” establishment that comprises a large share of hires at each Buyer in the years before the acquisition.⁴⁰ To qualify as a Feeder, an establishment must exist from event year -4 to event year 5, it must send at least 30 workers to a Buyer in four years preceding the acquisition, and the number of hires must comprise over 15 percent of all of the Buyer’s hires from employment in the four years before the acquisition.⁴¹

We are able to identify 26 Buyer-Feeder relationships among the Buyers that associated with acquisitions in our sample. The average characteristics of Buyers for which we identify Feeders and those Buyers’ evolution before and after the acquisition are quite similar to the overall average characteristics displayed in Table 1. The average number of workers hired from the Feeder by the Buyer falls from 8.7 to 4.0 per year from the pre-acquisition period to the post-acquisition period. This decline indicates that, at least in the set of acquisitions for which we can identify Feeders, the average decline in establishment size comes in part from a reduction in hiring from Feeder establishments.

In the current version of this paper, we observe only the average wage paid to workers at Feeder establishments and the total reduction in hiring from employment at Buyer establishments. To examine the extent to which reductions in Target hiring are associated with changes in average wage growth, we estimate the following regression:

$$\Delta \ln \bar{y}_j = \pi_0 + \pi_1 \Delta H_j + e_j. \tag{8.1}$$

⁴⁰ In cases in which multiple establishments qualify as Feeders, we consider the establishment that sends the largest number of workers to the Buyer.

⁴¹ In light of the establishment identifier assignment rules described in Section 4.1, we cannot definitively conclude that workers at “Feeder” establishments are employed in distinct companies from the Buyer establishment. Nevertheless, we observe an average wage premium among pre-acquisition employment transitions from Feeder to Buyer of 8 percent, which we interpret as evidence that a transition across boundaries of a company has taken place in a typical Feeder-to-Buyer transition.

In Equation 8.1, j denotes a Feeder establishment, and $\Delta \bar{y}_j$ is the difference in the log average daily wage at the feeder between event time five and event time negative four. ΔH_j is the total number of workers moving from the Feeder to the Buyer in event years 0 through 5 less the total number of workers moving from the Feeder to the Buyer in event years -4 through -1. π_1 is the correlation between log average wage growth and the total reduction in hiring. If Buyers comprise a meaningful set of the offers given to Feeder workers and reductions in observed hires from Feeders reflect decreases in the volume or attractiveness of Buyer offers, then we expect π_1 to be greater than zero.

Figure 7 illustrates the bivariate relationship between average wage growth and the change in hires. In Panel A, we plot the average wage change against the change in the total number of hires (ΔH_j); in panel B we normalize the change in the total number of hires by the Feeder establishment size and find that the strength of the relationship decreases, indicating that much of the relationship between hiring and average wage changes may be confounded by differences in establishment size.

The analysis above provides suggestive evidence that reductions in hiring at Buyer establishments lead to slower wage growth for individuals who remain at Feeder establishments. In future work, we hope to use the full worker histories of all individuals employed at Feeders over time to conduct a complete analysis of changes in mobility, wage growth conditional on mobility, and wage growth conditional on retention among workers at Feeder establishments before and after the acquisition.

9 Conclusion

This paper provides direct, worker-level evidence on a question of longstanding interest in labor economics: how changes in product market and labor market concentration affect workers. We examine worker-level impacts of events associated with consolidation, acquisitions. The acquisitions that we study benefit some workers and hurt others. Workers who remain within the boundary of the consolidated establishment experience marked wage growth relative to control workers who remain within the boundaries of establishments that do not undergo an acquisition. Workers who remain in the labor force but are outside of the firm that consolidate do not. In terms of labor earnings, the primary losers from acquisitions are women over 48, who exit private employment early in a manner consistent with early retirement. Our findings are consistent with rents increasing at consolidating establishments or workers' bargaining power rising. As theory and context suggest that the consolidation should decrease workers' bargaining power, we interpret our findings as most consistent with the former.

We conclude by highlighting two important limitations to this study and their implications for further research. First, our comparisons workers who are retained by Buyers reflect partial correlations and cannot be interpreted causally. Though there is little evidence of selection on observable characteristics, development of a selection correction to address this limitation would provide a clearer picture of the importance of remaining within firm boundaries among consolidating firms.

Second, due to data limitations we cannot directly estimate the relationship between the acquisitions in our sample and conventional measures of labor and product market concentration. Instead, we must rely on the fact that acquisitions between companies in narrow horizontal industries and within confined geographic space likely, on their own, serve to increase concentration. Our estimates are best interpreted as a reduced form effect of product and labor market consolidation on workers. We view future work utilizing more comprehensive measures of product and labor market concentration as a promising avenue for future research.

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Tables

Table 1: Mean characteristics of Target and Buyer establishments in year preceding transaction, full sample

| | (1) | (2) |
|-------------------|--------|-------|
| | Target | Buyer |
| Number of workers | 58.7 | 173.7 |
| Log num. workers | 3.74 | 4.75 |
| Daily wage | 104.0 | 105.2 |
| College-educated | .10 | .10 |
| Male | .65 | .63 |
| Age | 42.13 | 41.49 |
| N | 264 | 243 |

Notes: Entries in table are means of Target (Column 1) and Buyer (Column 2) characteristics. Observations are not weighted by establishment size. Characteristics are measured in the year before the transaction occurs. Daily wages are measured in 2010 Euros. Targets and Buyer establishments are identified according to the methodology outlined in Section 4.3.

Source: Own calculations.

Table 2: Characteristics of Target and Buyer workers

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|------------------------------------|-----------|------------|-------------|-----------|-----------|------------|-----------|--------------|---------------|
| | Target | Target (R) | Target (NR) | Buyer | Buyer (R) | Buyer (NR) | Controls | Controls (R) | Controls (NR) |
| Employment earnings (t=-1) | 40,631.88 | 41,446.12 | 41,003.49 | 40,806.25 | 41,824.50 | 41,085.92 | 37,716.11 | 38,096.94 | 38,459.81 |
| Employment earnings (t=5) | 34,503.35 | 43,747.99 | 41,491.54 | 35,548.48 | 43,923.58 | 42,095.05 | 32,588.64 | 39,583.43 | 38,867.92 |
| P(Employed) (t=5) | .799 | 1 | .96 | .815 | 1 | .96 | .826 | 1 | .96 |
| UI earnings (t=-1) | 19.29 | 4.68 | 23.85 | 22.51 | 9.20 | 12.63 | 28.43 | 10.93 | 35.10 |
| Log daily wage (t=-1) | 4.66 | 4.68 | 4.67 | 4.65 | 4.68 | 4.67 | 4.58 | 4.59 | 4.60 |
| Log daily wage (t=5) | 4.71 | 4.74 | 4.70 | 4.71 | 4.73 | 4.70 | 4.62 | 4.63 | 4.63 |
| Male | .63 | .66 | .69 | .67 | .66 | .69 | .66 | .67 | .69 |
| College | .10 | .10 | .13 | .11 | .12 | .16 | .12 | .10 | .15 |
| Training | .77 | .78 | .78 | .73 | .74 | .76 | .75 | .75 | .73 |
| Age | 41.76 | 41.0 | 39.65 | 40.73 | 41.19 | 38.75 | 40.76 | 41.59 | 39.52 |
| Estab size | 196.2 | 201.9 | 172.9 | 195.5 | 613.3 | 522.5 | 595.5 | 776.8 | 812.0 |
| Employed $t = -4$ through $t = -1$ | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| In LF $t = 0$ through $t = 5$ | N | Y | Y | N | Y | Y | N | Y | Y |
| Includes Retained | Y | Y | N | Y | Y | N | Y | Y | N |
| Includes Not Retained | Y | N | Y | Y | N | Y | Y | N | Y |
| Includes non-LF | Y | N | N | Y | N | N | Y | N | N |
| N | 9,034 | 5147 | 1455 | 25138 | 15607 | 3280 | 349332 | 223981 | 39068 |

Notes: Table presents mean characteristics of treated workers in year preceding acquisition and mean characteristics of control workers in year preceding that in which they qualify to be treated and are selected as controls, unless otherwise noted. Both treatment and control workers are employed for four consecutive years at a qualifying establishment. To qualify for the Labor Force sample, individuals must have at least Euro 500 of employment earnings and be on UI or employed on June 30 in the five years after the acquisition. Means in the Labor Force (LF) Sample of workers are reported in columns 2, 3, 5, 6, 8, and 9. Columns 2, 5, and 8 labelled "(R)" report means for workers who are retained by the Buyer in t=5; columns 3, 6, and 9 labelled "(NR)" report means for workers who are not retained by Buyers in t=5. Earnings variables are measured in 2010 Euros. Daily wage is recorded in 2010 Euros before log transformation; individuals with zero earnings are excluded from group used to compute average log daily wage.

Source: Own calculations.

Table 3: Set of covariate groups

| Covariate number | Covariate | Discretization |
|-------------------------|--|------------------------|
| 1 | Gender | Male indicator |
| 2 | Age year before event | 5-year groupings |
| 3 | Skill group | College/Training/Other |
| 4 | Wage (year before event) | Deciles |
| 5 | Estab. pre-event wage growth | Quartiles |
| 6 | Estab. Size | Quartiles |
| Regression Model Number | Covariates | |
| 1 | Year×Event cohort × Gender | |
| 2 | Year×Event cohort × Gender × Age | |
| 3 | Year×Event cohort × Gender × Age × Skill | |
| 4 | Year×Event cohort × Gender × Age × Skill × Wage | |
| 5 | Year×Event cohort × Gender × Age × Skill × Wage × Estab. wage growth | |
| 6 | Year×Event cohort × Gender × Age × Skill × Wage × Estab. wage growth × Estab. Size | |

Notes: Table lists individual covariates to be included in regression estimates of parameters outline in Equation 6.1. The column labeled “discretization” describes the type of groupings we construct from the relevant variable. Each covariate number corresponds to a covariate. We add each covariate to models that include lower-numbered covariates in 3; e.g. regression Model 5 contains interactions of covariates 1 through 5 with indicators for event year cohort and year. The bottom panel of this table explicitly lists the interactions in term δ_{c_i, X_i} from Equation 6.1.

Source: Own calculations.

Table 4: Difference in differences results

| | (1) | (2) | (3) | (4) |
|---------------------------------------|-----------------------|-------------------|-------------------|--------------------|
| | Emp. Earnings | Employment | Retention | UI receipt |
| Panel A: Full Sample | | | | |
| Full sample | | | | |
| $T_i \times Post_{it}$ | -386.930 (225.283) | -0.017 (0.004) | -0.064 (0.008) | 33.677 (11.796) |
| $B_i \times Post_{it}$ | 18.674 (140.142) | -0.002 (0.002) | -0.017 (0.008) | 9.390 (10.690) |
| N | 3,835,040 | 3,835,040 | 3,835,040 | 3,835,040 |
| Panel B: Full Sample, By group | | | | |
| Men 40 and Over | | | | |
| $T_i \times Post_{it}$ | -224.629 (276.840) | -0.004 (0.005) | -0.064 (0.014) | 17.512 (22.364) |
| $B_i \times Post_{it}$ | 69.743 (225.401) | 0.005 (0.004) | -0.010 (0.009) | -7.064 (18.865) |
| N | 1,497,730 | 1,497,730 | 1,497,730 | 1,497,730 |
| Men 39 and Under | | | | |
| $T_i \times Post_{it}$ | -58.31 (338.62) | -0.014 (0.006) | -0.075 (0.016) | 19.89 (11.88) |
| $B_i \times Post_{it}$ | 379.22 (171.42) | -0.001 (0.003) | -0.033 (0.010) | 5.08 (7.54) |
| N | 1,019,030 | 1,019,030 | 1,019,030 | 1,019,030 |
| Women 40 and Over | | | | |
| $T_i \times Post_{it}$ | -1152.13 (306.95) | -0.040 (0.007) | -0.071 (0.012) | 84.69 (20.76) |
| $B_i \times Post_{it}$ | -647.84 (211.81) | -0.021 (0.006) | -0.023 (0.008) | 43.44 (16.85) |
| N | 883,030 | 883,030 | 883,030 | 883,030 |
| Women 39 and Under | | | | |
| $T_i \times Post_{it}$ | -302.04 (458.32) | -0.023 (0.010) | -0.051 (0.017) | 16.20 (15.08) |
| $B_i \times Post_{it}$ | 92.91 (245.43) | -0.000 (0.006) | -0.001 (0.011) | 5.06 (10.09) |
| tN | 435,250 | 435,250 | 435,250 | 435,250 |

Notes: Numeric table entries in labeled rows are difference-in-differences estimates of the effect of the acquisition on average yearly post-acquisition outcomes for pooled sample in Panel A and $\{\beta^g, \gamma^g\}$ for distinct demographic groups g separately in Panel B (see equation 7.1). Columns denote dependent variable of interest. T_i and B_i are indicators for Target and Buyer treatment status, respectively. $Post_{it}$ is an indicator denoting whether an observation occurs in a year after an acquisition (or, for Controls, after the treatment year of their cohort). Standard errors, clustered by pre-event year establishment, are in parentheses. Minimum and maximum ages for sample inclusion (measured in pre-event year) are 25 and 55, respectively. Columns denote estimates from models estimated separately for treated and control units in group designated in row header.

Source: Own calculations.

Table 5: Differences in differences results: Focus on older workers

| | (1) | (2) | (3) | (4) |
|--------------------------|----------------------|-------------------|-------------------|-------------------|
| | Emp. Earnings | Employment | Retention | UI receipt |
| Men 48 and Over | | | | |
| $T_i \times Post_{it}$ | -237.42 (416.82) | -0.003 (0.008) | -0.059 (0.015) | -31.70 (37.07) |
| $B_i \times Post_{it}$ | 303.08 (344.10) | 0.012 (0.007) | 0.005 (0.010) | -64.04 (30.90) |
| N | 738,030 | 738,030 | 738,030 | 738,030 |
| Women 48 and Over | | | | |
| $T_i \times Post_{it}$ | -2126.15 (476.32) | -0.067 (0.011) | -0.087 (0.013) | 129.70 (34.39) |
| $T_i \times Post_{it}$ | -1271.59 (337.64) | -0.041 (0.009) | -0.038 (0.010) | 77.40 (27.13) |
| N | 458,850 | 458,850 | 458,850 | 458,850 |

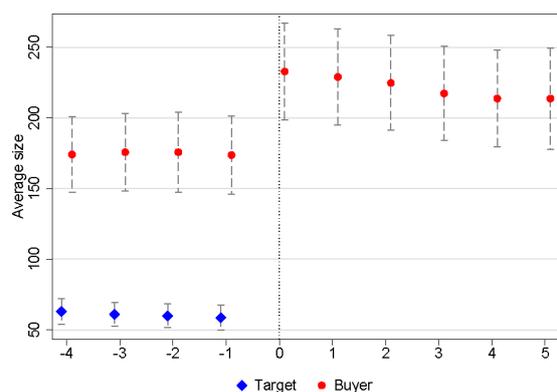
Notes: Numeric table entries in labeled rows are difference-in-differences estimates of the effect of the acquisition on the average yearly post-acquisition outcomes for all workers all workers over 48 separately by gender. Columns denote dependent variable of interest. T_i and B_i are indicators for Target and Buyer treatment status, respectively. $Post_{it}$ is an indicator denoting whether an observation occurs in a year after an acquisition (or, for Controls, after the treatment year of their cohort). Standard errors, clustered by pre-event year establishment, are in parentheses. The maximum age of sample inclusion is 55.

Source: Own calculations.

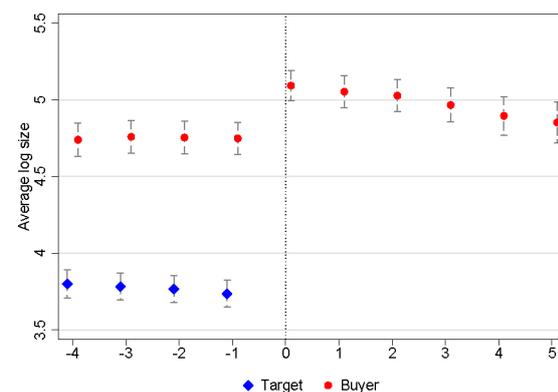
Figures

Figure 1: Mean Characteristics of Target and Buyer Establishments in Years Preceding and Following Transaction

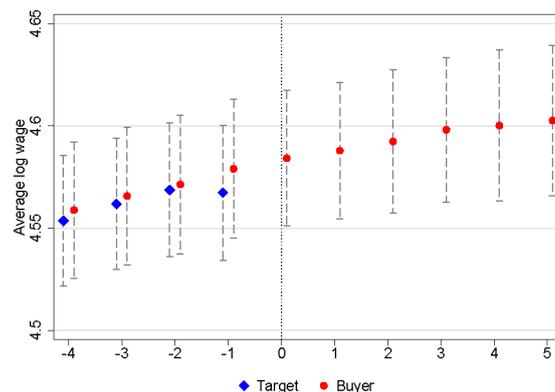
(a) Panel A: Number of Workers



(b) Panel B: Log Number of Workers



(c) Panel C: Average Log Daily Wage

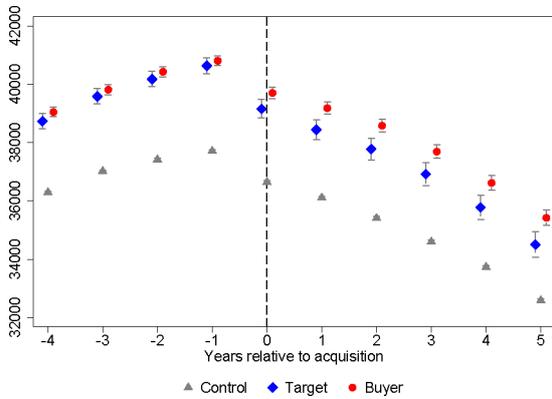


Notes: Panel A depicts the mean of the total number of part time and full time employees in across all Target (blue diamond) and Buyer (red circle) establishments as of June 30 of each year preceding and following the acquisition, which occurs at event time zero. Dotted lines depict 95% confidence intervals constructed using standard errors of the means. Target establishments do not exist in the year after the acquisition (See Section 4.3). Dashed line at $t = 0$ denotes the year of the acquisition. Panel B depicts the average log number of workers. Panel C depicts the average log daily wage (in 2010 Euros) among workers employed at Buyer and Target establishments. There are 243 Buyer establishments, 264 Target establishments, and 243 transactions underlying these figures.

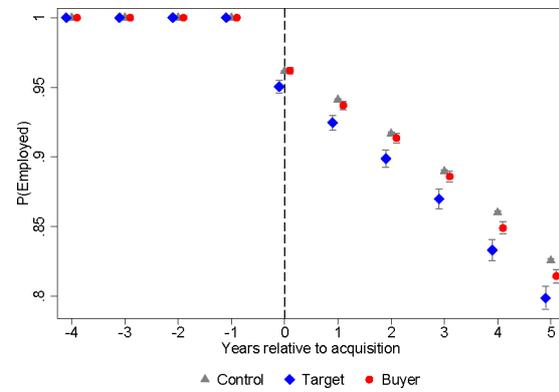
Source: Own calculations.

Figure 2: Average outcomes of Target, Buyer, and Control workers over event time

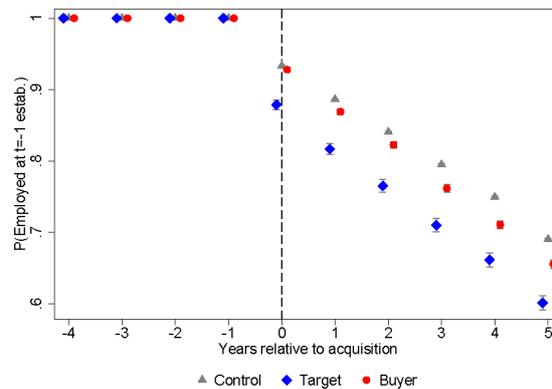
(a) Panel A: Employment Earnings



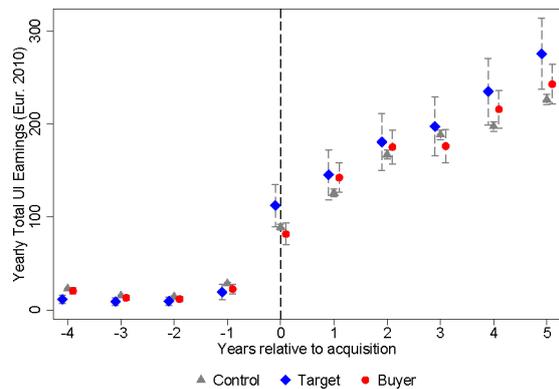
(b) Panel B: Probability of Employment



(c) Panel C: Probability of Employment at Consolidated Entity

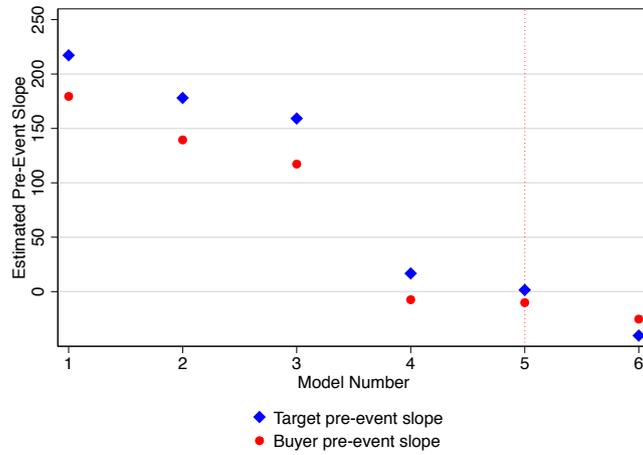


(d) Panel D: UI Benefit Receipt



Notes: Panel presents means over event time for the 9,123 Target, 25,285 Buyer, and 352,763 Control workers in the sample of directly-impacted workers. Characteristics of workers are reported in Table 2. Sample construction is described in Section 6. Dotted lines depict 95% confidence intervals constructed using standard errors of the means. Dashed line at $t = 0$ denotes the year of the acquisition. Panel A depicts the average employment earnings (in 2010 Euros) over event time by group. We assign individuals who do not appear in the sample Euro 0. Panel C depicts average retention rate at the consolidated entity over time. The consolidated entity is the Buyer for all treated in workers in the post-event period, otherwise it is a worker's $t = -1$ establishment. Panel D depicts the average UI annual amount of UI benefits received by workers across groups and event time.
Source: Own calculations.

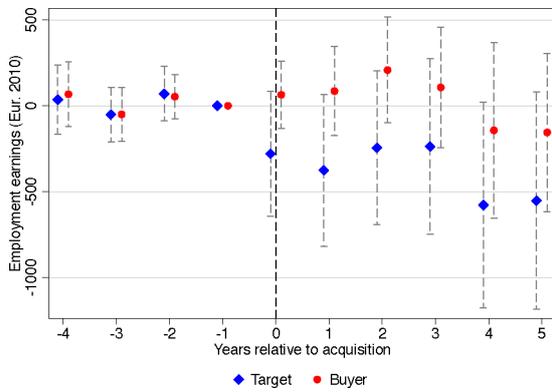
Figure 3: Baseline employment earnings pre-event trends conditional on different sets of covariates



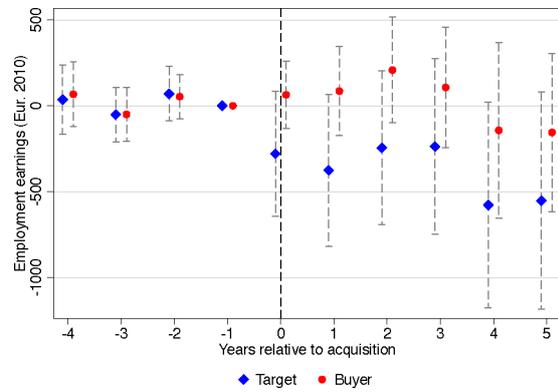
Notes: Figure depicts the slope of a bivariate regression line fitted through pre-event eventtime coefficients $\{\beta^k, \gamma^k\}_{k=-4}^{k=-1}$ separately for each covariate model. Covariates included in each model are described in 3. Red line shows that preferred model is Model 5.
Source: Own calculations.

Figure 4: Event study coefficients showing effect of acquisition on Target and Buyer outcomes

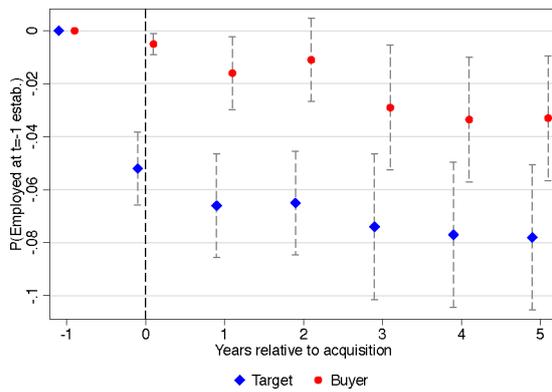
(a) Panel A: Employment Earnings



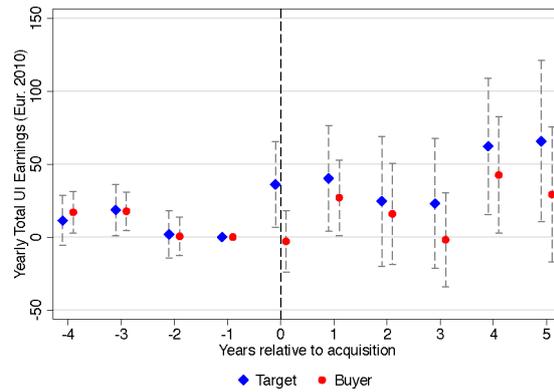
(b) Panel B: Probability of Employment



(c) Panel C: Probability of Employment at Consolidated Entity



(d) Panel D: UI Benefit Receipt

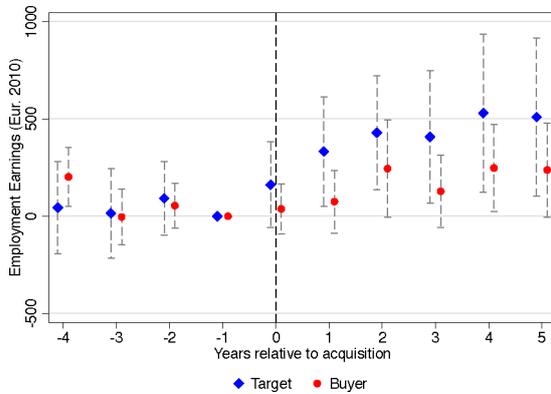


Notes: Panels present coefficients (points) and 95% confidence intervals (dotted bars) from estimates of Equation 6.1 using the dependent variable specified in the panel title. Standard errors are clustered by $t = -1$ establishment. Estimation sample contains 3,835,040 observations. Coefficients in the pre-acquisition period in Panels B and C are omitted as the sample of treated and control workers is restricted to be employed in these years.

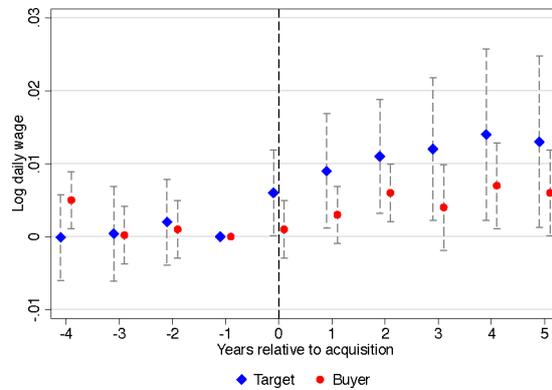
Source: Own calculations.

Figure 5: Differences in earnings and wages by retention status

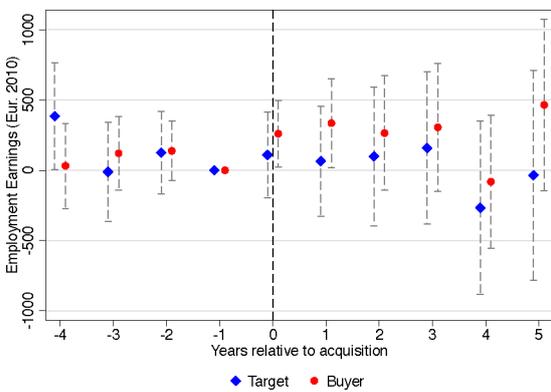
(a) Panel A: Employment earnings, retained group



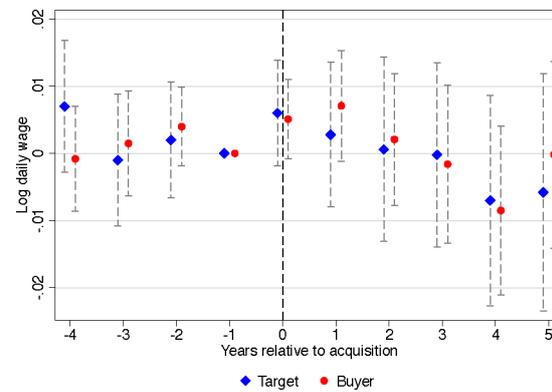
(b) Panel B: Log daily wage, retained group



(c) Panel C: Employment earnings, not retained group



(d) Panel D: Log daily wage, not retained group

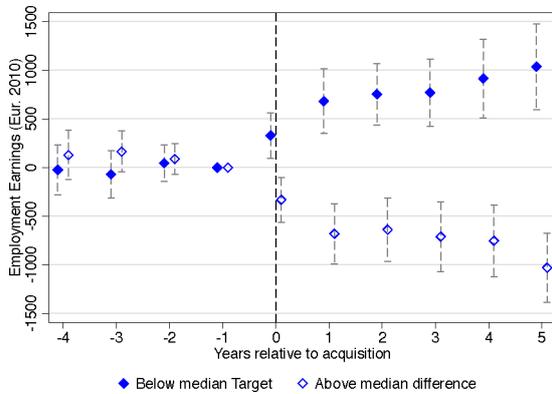


Notes: Figure presents coefficients and 95% confidence intervals from estimates of Equation 6.1 using the sample of treated workers who are retained by Buyers in $t = 5$ and control workers who are retained by their $t = -1$ establishment in $t = 5$. Panels A and B present coefficients and 95% confidence intervals from estimates of Equation 6.1 using the sample of treated workers who are *not* retained by Buyers in $t = 5$ and untreated workers who are *not* retained by their $t = -1$ establishment in $t = 5$. Standard errors are clustered by $t = -1$ establishment.

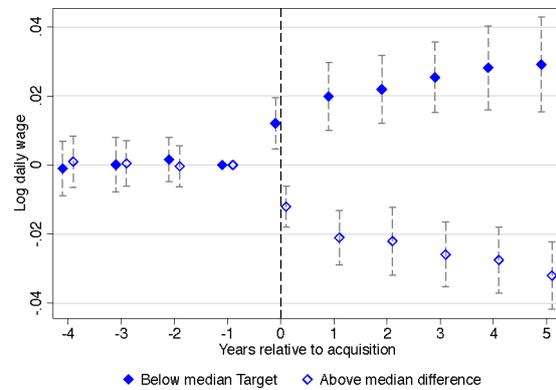
Source: Own calculations.

Figure 6: Trends in wages and earnings by within establishment wage group, Retained workers

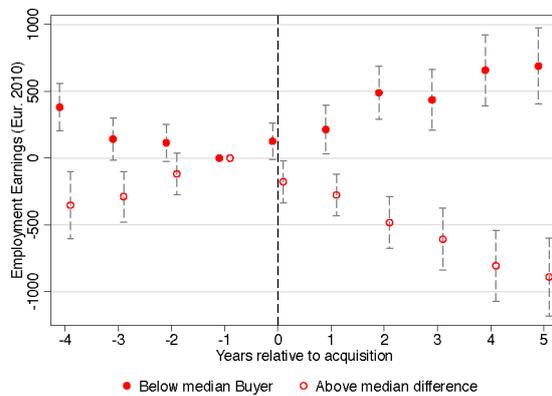
(a) Panel A: Employment earnings, Target workers



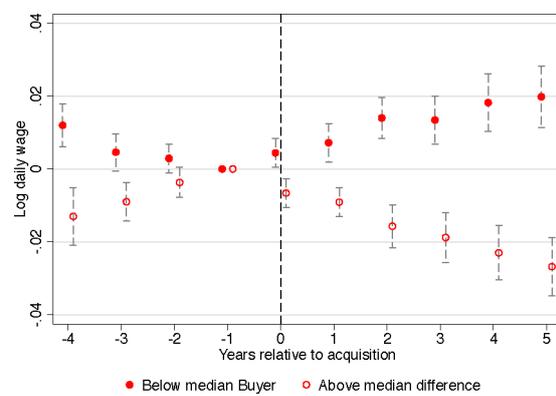
(b) Panel B: Log daily wage, Target workers



(c) Panel C: Employment earnings, Buyer workers



(d) Panel D: Log daily wage, Buyer workers



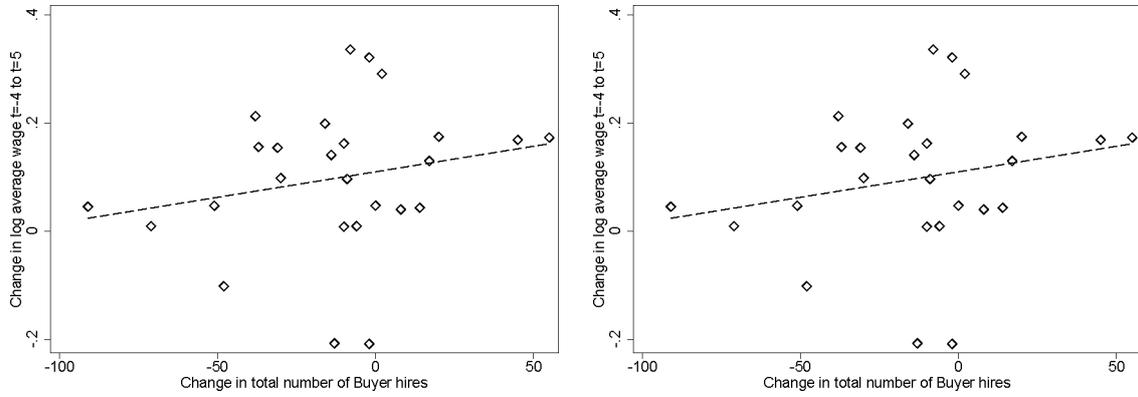
Notes: Figure present coefficients and 95% confidence intervals from estimates of Equation 7.2 using the sample of treated workers who are retained by Buyers in $t = 5$ and control workers who are retained by their $t = -1$ establishment in $t = 5$. Panels A and B show estimates where dependent variable is employment earnings and log daily wage among Target workers, respectively. Panels C and D display coefficients for Buyer workers. Filled markers denote coefficient estimates for below-median workers; hollow markers denote coefficient estimates on interaction between event time indicator and an indicator for high wage workers. Summing coefficients generates average outcome for above-median workers relative to controls. Standard errors are clustered by $t = -1$ establishment.

Source: Own calculations.

Figure 7: Changes in Feeder average wages and changes in Buyer total hires

(a) Panel A: All hires

(b) Panel B: Hires relative to size



Notes: Figures plot the changes in the average log wage at Feeder establishments between $t=-4$ and $t=5$ against the total change in hiring that occurs at the Buyer establishment associated with that Feeder between the pre-acquisition period and the post-acquisition period. Plots represent the regression in Equation 8.1.

Source: Own calculations.

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