Health Insurer Mergers, Employer-Sponsored Premiums, and Labor Market Inequality *

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Abstract

This paper investigates whether health insurer mergers have contributed to the striking rise in employer-sponsored premiums in the U.S., and examines how these rising premiums impact labor market inequality. Using administrative data and an event study analysis of insurer mergers occurring over the last two decades, I find that insurer mergers lead to a 10 percent increase in premiums for vulnerable firms that directly purchase plans in affected markets. As a result, vulnerable firms facing higher premium costs suffer employment losses of 4.4 percent, concentrated among noncollege-educated, middle-income workers. Incorporating my findings into a competitive labor market model, I show that insurer mergers explain 22 percent of the overall premium increase and 10 percent of the U.S.-specific decline in employment among noncollege-educated workers from 1999 to 2019.

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1. Introduction

Health insurance premiums in the U.S. have increased fourfold from $2,761 per worker in 1977 to $11,864 per worker in 2019. One potential contributor is consolidation in the private health insurance industry. Concerned that less competitive markets have driven up premiums, antitrust authorities recently blocked mergers between industry giants Aetna and Humana, and Anthem and Cigna. However, recent scrutiny has overlooked the potential impact of insurer mergers on labor market inequality. By raising workplace-financed premiums, insurer mergers increase the cost of labor. As premiums are a fixed labor cost – for a given health plan firms incur the same cost for a low-paid janitor as for a highly-paid executive – lower-income workers are disproportionately affected by premium increases. Given the private health insurance industry's size (6% of GDP in annual expenditures) and scope (insuring 60% of non-elderly Americans)[1], understanding how insurer mergers affect premiums and subsequently impact firms and workers is highly relevant for policymakers and antitrust authorities.

Due to a lack of publicly-available data on private insurer markets (Dafny et al., 2011), our best empirical evidence on the effects of insurer mergers uses proprietary data to conduct case studies of the 1999 merger between Aetna and Prudential (Dafny et al., 2012) and the 2007 merger between United and Sierra Health (Guardado et al., 2013). They find premium increases post-merger, but it is unclear whether we should extrapolate the finding from two case studies as the effects on premiums are theoretically ambiguous and may vary across markets (Ho and Lee, 2017). Reduced competition may strengthen insurers' bargaining leverage to negotiate higher premiums with firms, but insurers may also negotiate lower medical payments with physicians and hospitals which would offset premium increases. Importantly, neither case study addresses potential labor market consequences.

This paper studies insurer mergers over the last two decades to provide new evidence on the effect of insurer mergers on premiums and labor market outcomes. Merger shocks are sizable as around 36 percent of workers are employed in vulnerable firms that negotiate with insurers over premiums. I use novel administrative Form 5500 (F55) data on employer health plans to obtain information on insurer markets. Using a difference-in-differences research design and F55 data, I compare the premiums of vulnerable firms in earlier-exposed commuting zones (CZs) with those in later-exposed commuting zones before and after insurer mergers. I

[1] Sources: Congressional Research Service (accessed at: https://sgp.fas.org/crs/misc/IF10830.pdf.) and KFF (2023).

then use matched employer-employee Census data to examine how the merger-induced premium increases affect wages and employment in vulnerable firms, and decompose effects by worker education and income. I draw on additional publicly-available and administrative data to offer a comprehensive assessment of merger effects. Finally, to study how antitrust intervention may remedy the labor market effects of mergers, I build and calibrate a structural model of a competitive labor market and counterfactually simulate premiums growth. My analysis proceeds in three steps.

First, I use administrative data on employer health plans and an event study analysis to estimate the impact of insurer mergers on the premiums of vulnerable firms. Vulnerable firms directly purchase plans from insurers and bargain with insurers over premiums. Exposure to insurer mergers depends on whether vulnerable firms are located in a CZ where both merging insurers hold market shares prior to the merger. Within these CZs, insurers can leverage the decrease in insurer competition to bargain higher premiums post-merger. To ensure exposure is plausibly exogenous, I focus on mergers between national or multi-state insurers. For example, in the 2004 merger between WellPoint and Anthem, each insurer had market shares in nearly 200 CZs (out of 741), but jointly had market shares in 94. My empirical strategy then leverages variation in the timing of merger exposure to compare the premiums of vulnerable firms in CZs exposed for the first-observed time to those in later-exposed CZs. I aggregate plan-level F55 data to the CZ level to study 21 mergers occurring between 2003 and 2015. Identification assumes that firms in earlier-exposed CZs and later-exposed CZs would have had a similar evolution in outcomes, had the merger not been executed. I show that this assumption is credible over the pre-merger period.

I find that insurer mergers increase annual premiums for vulnerable firms by 10 percent. Insurers both directly and not directly involved in mergers raise their premiums, resulting in large premium increases for vulnerable firms throughout exposed markets. Interestingly, I also find that vulnerable firms respond by offering less-costly plans with restrictive provider networks. The shift suggests that improvements in plan quality that benefit consumers are unlikely to play a role in generating aggregate premium increases. A back-of-the-envelope calculation suggests that insurer mergers may account for 22 percent of the overall growth in premiums over the past two decades.

What are the labor market effects of these merger-induced premium increases? I turn to matched employer-employee Census data to estimate the event study over the sample of workers in vulnerable firms. I aggregate the worker-level data to the CZ level to ease the computational burden of my analysis.

I show that vulnerable firms experience significant and sustained employment losses of 4.4 percent on average in earlier-exposed CZs relative to those in later-exposed CZs. Because low-income workers are not typically covered by employer-sponsored insurance, noncollege-educated middle-income workers are instead disproportionately affected by employment losses. The finding suggests that rising premiums contribute to the hollowing out of the middle class, as hypothesized in recent popular writing by Saez and Zucman (2019) and Deaton and Case (2021). Using American Community Survey data, I show that vulnerable firms also hire more part-time workers, for whom they are not required to provide health insurance benefits.

I then ask how the aggregate local labor market is affected by large employment losses in vulnerable firms. Individuals in the growing unemployment pool may respond by migrating to a different CZ, seeking work at a less-vulnerable firm within the same CZ, or remaining unemployment and taking up government transfers. I find a partial reallocation of workers from vulnerable to less-vulnerable firms, but aggregate employment falls in CZs. County Business Patterns data confirm an aggregate decline in employment. Government spending on unemployment insurance increases to support individuals out of work.

Finally, I extend and calibrate the structural model of a competitive labor market model in Finkelstein et al. (2023) to simulate employment under counterfactual growth paths of premiums. I conduct counterfactuals in partial equilibrium to focus on premiums growth as the key mechanism. To quantify the impact of the uniquely American explanation of rising ESHI costs from insurer mergers, I compare simulated employment changes to the U.S.-specific net-of-Canada change in employment. I find that rising costs from insurer mergers can account for 9.7 percent of the 3 percentage point decline in the U.S.-net-of-Canada non-college employment-to-population ratio and 8.6 percent of the 1.5 percentage point slowdown in the U.S.-net-of-Canada college employment-to-population ratio from 1999 to 2019. As another benchmark, I show that the size of employment losses from insurer mergers are most comparable to the size of employment losses caused by exposure to robots as estimated in Acemoglu and Restrepo (2020).

This paper makes three main contributions to the literature.

First, prior case studies analyze single insurer mergers using proprietary data due to a lack of publicly-available data on insurer markets (Dafny et al., 2012; Guardado et al., 2013). Ho and Lee (2017) build and calibrate a structural model of health care markets to predict how consolidation could affect premiums but do not validate these predictions using actual merger shocks. I use new administrative data to estimate the average effect of mergers occurring over the last two decades on premiums, and show that the average effect masks considerable differences across markets. I provide the first evidence that insurer mergers erode U.S. employment by increasing employer-sponsored premiums.

Second, my paper relates to the literature on the effects of rising ESHI costs on labor market outcomes. Early studies estimate models to relate higher ESHI costs to wages (Sheiner, 1999), and hours worked (Cutler and Madrian, 1998), but tend to focus a single margin of adjustment and do not leverage quasi-experimental variation. Recent papers using quasi-experimental variation share a common limitation: they rely on small variations in premiums to draw conclusions about how the dramatic increase in premiums has affected labor market inequality. Specifically, Gao et al. (2022) use insurers' past losses as an instrument to study 2 percent premium increases for a sub-sample of employers served by those insurers. Brot-Goldberg et al. (2024) use hospital mergers, which endogenously respond to local market conditions, as an instrument to study a 1 percent premium increase. This study complements previous papers by exploiting a 10 percent premium increase from mergers between large insurers to estimate short- and medium-term effects in a unified difference-in-differences framework. Using an array of publicly-available and administrative data, I analyze new outcomes like employer pension contributions, health insurance coverage, part-time workers, and hours worked, and estimate a structural model to consider counterfactuals. Estimating the effects of rising ESHI costs also contributes to the literature on mandated workplace benefits (Summers, 1989; Gruber and Krueger, 1991; Gruber and Madrian, 1994; Thurston, 1997; Buchmueller et al., 2011).

Third, this paper offers a uniquely American explanation – rising employer-sponsored premiums caused by insurer mergers – for the decline in U.S. noncollege-educated employment. Past work has studied global drivers of noncollege-educated employment loss, including skill-biased technical change (Katz and Murphy, 1992; Goldin and Katz, 2008; Autor et al., 2020), automation (Acemoglu and Restrepo, 2020), recessions (Yagan, 2019), import competition (Autor et al., 2013a), and occupational change (Autor, 2019). I incorporate my reduced-form estimates in a structural competitive labor market model to show that insurer mergers can explain a significant share of the U.S.-specific decline in noncollege-educated employment.

2. Background

2.1 Employer-Sponsored Health Insurance in the U.S.

The U.S. relies on health insurance provided through the workplace as the primary source of coverage for non-elderly Americans. Around 60 percent of Americans under 65 receive health insurance through the workplace (KFF, 2023). In contrast, most developed countries, including Canada and the U.K., primarily fund health insurance through the government. While firms are not legally required to offer ESHI, the Affordable Care Act introduced penalties for firms with 50 or more full-time workers if they failed to offer coverage to workers working 30 or more hours, effective in 2015. Nondiscrimination rules under IRS Code section 105(h) also prohibit firms from providing more generous coverage to highly-paid workers. Hence, ESHI costs are a fixed cost per worker regardless of income.

Health insurance benefits are offered by most firms with 50 or more workers, and 50 percent of firms with less than 50 workers (KFF, 2023). Among firms offering health insurance, 86 percent do not offer benefits to part-time workers (KFF, 2023). 80 percent of workers in firms offering ESHI are eligible and 80 percent of eligible workers take up ESHI (KFF, 2023). Firms and workers contribute to annual premiums for health insurance plans; firm contributions account for roughly 75 percent of total annual premiums (KFF, 2023). One reason for the prevalence of employment-based insurance is that firm contributions are tax exempt, which has its roots in World War II (Starr, 1982; Thomasson, 2003).

The method firms use to finance health insurance plans may affect their vulnerability to insurer mergers. There are two ways for an firm to finance health insurance. Firms can purchase an insurance plan from an insurer, referred to as a "fully-insured" plan as insurers bear the financial risk. Firms offering fully-insured plans are more vulnerable to mergers because premiums are bargained between firms and insurers. Alternatively, firms can pay for health care directly from their own funds, to as a "self-insured plan" as firms bear the financial risk. Self-insured firms are less vulnerable because premiums are self-determined, though self-insured firms often purchase stop-loss insurance and pay insurers to administer health plans. Larger firms are more likely to be self-insured because the health expenses of larger health plans are more predictable and so it is less risky to self-insure. Roughly 36 percent of workers are employed in a firm that offers fully-insured plans, while the remaining 64 percent are employed in a self-insured firm (KFF, 2023).

2.2 Insurer Mergers and Antitrust Enforcement

Health insurer mergers over the past few decades have led to less competitive and more concentrated markets. Since 1999, there have been about 130 mergers between large health insurers (see Figure 2). Meanwhile the average number of insurers in markets dropped from 16 in 2000 to 11 in 2019 (Figure 1). The median concentration, measured by the Herfindahl-Hirschman Index (HHI), increased by 555 points on a base of 2,778 HHI in 2000 (Appendix Figure A.3). The level of insurer concentration far exceeds the 1,800 HHI threshold used by the 2023 Department of Justice (DOJ) and Federal Trade Commission (FTC) Horizontal Merger Guidelines to classify markets as highly concentrated.

Despite the trend toward increasingly concentrated insurer markets, the DOJ and FTC have intervened to block only five insurer mergers since 1999. Merger deals in which the target's assets are above a certain threshold are required to notify the DOJ and FTC, which then decide whether to take enforcement actions. Typically these actions lead to modifications or abandonment of deals, resulting in only a small number being blocked. The blocked mergers include proposals between CareFirst and WellPoint in 2001, Independence Blue Cross and Highmark in 2009, Tufts and Harvard in 2011 (later approved in 2021), and both Cigna and Anthem, as well as Aetna and Humana, in 2017.

Policymakers have more recently expressed concerns about the potential harm to consumers and providers, but effects on firms and workers have been largely overlooked. For example, in 2015, the Senate Committee on the Judiciary held a hearing titled "Examining Consolidation in the Health Insurance Industry and its Impact on Consumers". Moreover, in a 2023 letter to the FTC Chair Lina Kahn, Senator Elizabeth Warren warns that "further consolidation could harm consumers".[2] Warren references a report from the American Medical Association (AMA) stating that "consolidation has resulted in the possession and exercise of health insurance monopoly power - the ability to raise and maintain premiums above competitive levels - instead of the passing of any benefits obtained through to consumers".[3]

[2] Accessible via: www.warren.senate.gov/imo/media/doc/2023.03.17%20Letter%20to%20FTC%20re%20CVS.Oak%20Street%20Merger.pdf

[3] Source: American Medical Association, "Competition in Health Insurance" (accessed at https://www.ama-assn.org/system/files/competition-health-insurance-us-markets.pdf).

3. Data and Empirical Strategy

This section begins by describing the data I use for the analysis; Appendix 9 provides more detail on the data and construction of variables. I then present my empirical strategy.

3.1 Data

Health Insurance Outcomes. Data on employer health insurance plans is from annual Form 5500 (F55) filings spanning 1999 to 2020 administered by the Department of Labor (DOL) and Internal Revenue Service (IRS).[4] For firms offering fully-insured plans, the F55 records plan-level information on the insurer, total annual premiums, plan participants (workers and dependents), plan type, and zip code. Because only firms with 100 or more participants are required to file, the data is a sub-sample of medium-sized firms offering fully insured plans.

I use the F55 data to calculate insurer HHI as a measure of concentration at the CZ level, which is the sum of squared market shares for parent insurers. To identify parent insurers, I link the insurer of F55 plans to the NAIC directory. Without public information on the boundary of insurer markets, I use CZs because they cover the area in which individuals typically live, work, and receive medical services.

I also construct average annual premiums per participant across fully-insured plans aggregated to the CZ level. Using plan-level F55 data for a balanced set of firms, I divide annual premiums by the number of participants, winsorize at the 1st and 99th percentiles, and take the average across plans in the CZ. I convert dollar values to 2019 dollars throughout the paper using the Consumer Price Index (CPI). As a proxy for health plan quality, I calculate the CZ plan share that are Health Maintenance Organizations (HMO), Preferred Provider Organizations (PPO) or Other. HMO plans only cover care received from in-network providers, while PPO and Other plans cover out-of-network care. HMO require referrals through primary care physicians for specialist care, while PPO do not require referrals. I use the Medical Expenditure Panel Survey Insurance/Employer Component (MEPS-IC) firm-level survey to construct quality and coverage outcomes not measured in the F55, collapsed at the CZ level. I complement with individual-level data from the Current Population Survey March Supplement to construct the CZ share of all workers or individuals covered by ESHI.

Labor Outcomes. I use the U.S. Census Bureau's Longitudinal Employer-Household Dy-

[4] Accessible via: https://www.dol.gov/agencies/ebsa/about-ebsa/our-activities/public-disclosure/foia/form-5500-datasets.

namics (LEHD) from 1999 to 2020 to measure labor outcomes. Constructed from state unemployment insurance (UI), the LEHD is a quarterly matched employer-employee dataset covering 96 percent of U.S. employment (Graham et al., 2022). I have access to 25 states.[5]

To assess merger effects on vulnerable firms and workers, I create a panel of workers employed in single-establishment firms which I collapse for my analysis to the CZ level. Because only a sub-sample of fully-insured firms can be identified by linking the F55 with the LEHD, I focus on single-establishment firms employing workers between 18 to 64 ("working age").[6] Owing to their smaller size, single-establishments are more likely to offer fully-insured plans and therefore more vulnerable to insurer mergers. I later verify my results using the smaller sample of linked F55-LEHD firms offering fully-insured plans. Following Card et al. (2013), I require that worker's annual wages across all jobs exceed $3,700 in 2019 dollars to drop workers weakly attached to their employer.

I use the LEHD panel to calculate the CZ share of working-age population employed in vulnerable single-establishment firms (my main outcome of interest). I sum across jobs with positive annual wages in single-establishment firms and divide by the working-age population. I aggregate annual wages to the CZ level by summing wages across quarters and jobs for each worker employed in single-establishment firms and averaging wages across workers in a CZ. Wages in the LEHD include "gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging" (BLS 1997). I also observe worker demographic (age, race/ethnicity, sex, and education). Missing data for workers is imputed by the Census; roughly 5 percent of data is missing for gender and age, 20 percent for race and ethnicity, and 85 percent for education. To assess heterogeneous effects, I calculate outcomes by worker demographics and collapse to the CZ level. I do not observe hours worked, hourly wage rates, or part-time status in the LEHD.

To consider mergers effects on the aggregate local labor market, I draw on several data sources. First, I use the LEHD to calculate the CZ-level share of working-age population employed in less-vulnerable multi-establishment firms, to assess between-firm effects. Second,

[5]The states are: Arizona, California, Colorado, Delaware, Idaho, Indiana, Kansas, Maine, Maryland, Massachusetts, Nebraska, Nevada, New Jersey, New Mexico, North Dakota, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, and Wisconsin.

[6]Another reason for focusing on single-establishment firms is that the LEHD does not record information on the headquarter or location of firms' establishments, making it difficult to identify whether multi-establishment firms are exposed to insurer mergers. Headquarter information is important because health insurance plans are typically set in firm headquarters, so treatment of multi-establishment firms may depend on whether the headquarter is in a treated CZ.

I aggregate county-level data from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) to examine unemployment and labor force exits. Third, I aggregate county-level data from the U.S. Bureau of Economic Analysis (BEA) to study self-employment and uptake of government transfers. Fourth, I examine employment outcomes not available in the LEHD, including share part-time and hours worked, by aggregating individual-level American Community Survey (ACS) data to the CZ level.

Additional Outcomes. I use publicly-available data from the County Business Patterns (CBP) and the U.S. Census Bureau's Longitudinal Business Database (LBD) to replicate my LEHD results for all states. Covering all private, non-farm U.S. employment, the establishment-level LBD data allows me to construct annual employment (measured during the week of March 12th) and payroll measures for vulnerable firms and less-vulnerable firms collapsed at the CZ level. I use the F55 to construct additional CZ-level measures on non-wage compensation for fully-insured firms, including pension plans and dental and vision plans. I use the National Cancer Institute SEER Program to calculate the annual working age population in CZs.

3.2 Empirical Strategy

3.2.1 Identifying Exposure to Insurer Mergers

Vulnerable firms offering fully-insured plans are exposed to an insurer merger if they are located in a CZ where both merging insurers have market shares prior to the merger. Reduced insurer competition in these CZs allows insurers to leverage their bargaining power to negotiate higher premiums with vulnerable firms. Out of the 683 CZs in the Form 5500, which cover 99.8 percent of U.S. population, I identify 246 CZs that are exposed to at least one merger, encompassing 85 percent of the U.S. population, and 437 CZs that are never exposed, accounting for only 14.8 percent of the U.S. population.

There are two challenges with isolating the effects of insurer mergers. First, never-exposed CZs are highly selected with significantly smaller population sizes and more concentrated insurer markets compared to exposed CZs (see Appendix Table A.1). Third, exposed CZs often experience successive mergers over time. Of the 246 exposed CZs, 135 CZs experience more than 1 merger, and the average exposed CZ experienced 4 mergers between 1999 to 2020.

To address the challenges, I focus on "earlier-exposed" CZs that experience a first observed insurer merger and are not exposed to any additional mergers in the six years following the merger (including the merger year). I restrict to earlier-exposed CZs that experience a first observed merger between 2003 and 2015 to ensure that these CZs are not exposed to any mergers in the years leading up to or following the merger. I identify 81 earlier-exposed CZs from 21 mergers occurring between 2003 and 2015. I expand my earlier-exposed sample of CZs to include successively exposed CZs that experience a first observed merger in Appendix Section 9. The distribution of my sample of mergers is very similar to the distribution of all mergers over the years, shown in Figure 2.

I compare vulnerable firms in earlier-exposed CZs to those in "later-exposed" CZs as the counterfactual. "Later-exposed" CZs experience a first merger seven or more years after the merger year of the earlier-exposed CZ. I identify 39 later-exposed CZs. In Table 1, I show that earlier- and later-exposed commuting zones are balanced across observable characteristics. On average, earlier-exposed and later-exposed CZs have similar levels of insurer concentration (12 vs. 10 insurers; 2,800 vs. 2,690 HHI points), per-enrollee premiums of \$4,700, median household incomes of \$54,000, unemployment rates (6.83 vs. 6.04 percent), and rural shares (0.51 vs. 0.53), but earlier-exposed CZs are larger in population than later-exposed CZs (481,000 vs. 367,000 residents).

3.2.2 Difference-in-differences

I use a difference-in-differences empirical strategy to estimate the effects of insurer mergers on ESHI premiums and labor market outcomes. I implement the Sun and Abraham (2021) estimator following recent advances in the difference-in-differences econometrics literature (De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). In brief, I obtain event study estimates for each cohort (defined by the year of the merger treatment), and then calculate the weighted average of cohort-specific effects, where weights are equal to each cohort's sample share. In this section, I present estimating equations as canonical two-way fixed effects models for ease of interpretation. I provide detail on the Sun and Abraham (2021) methodology in Appendix 9. My main estimating equation is:

$$y_{ct} = \sum_{l} \beta_l [\mathbb{1}(\text{Merger}_c) \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_t + \delta_c + \varepsilon_{ct} \tag{1}$$

where $\mathbb{1}(\text{Merger}_c)$ is a indicator for CZ c exposed to an insurer merger, $\mathbb{1}(\text{Year}_t = l)$ is an event-time indicator for calendar year t being l years away from the year of the merger, α_t and δ_c are calendar year and CZ fixed effects, and ε_{ct} is the error term. I cluster standard errors at the CZ level. The coefficients of interest are the β_ls; they measure the impact on outcome y_{ct} in event year l in earlier-exposed CZs after the merger, relative to later-exposed CZs. I also report the average of coefficients across the post-merger event years. I omit the event time indicator for the year before the merger throughout the paper, so that all β_l coefficients are relative to the year before the merger. I weight by the CZ population in the year before the merger, given differences in CZ population across the U.S.

For sub-group analyses, I estimate a fully-saturated model:

$$y_{ctg} = \sum_g \sum_l \beta_{lg}[\mathbb{1}(\text{Merger}_c) \cdot \mathbb{1}(\text{Year}_t = l) \cdot \mathbb{1}(\text{Group}_g)] + \alpha_{tg} + \delta_{cg} + \varepsilon_{ctg} \quad (2)$$

where y_{ctg} is a CZ-year-group outcome, $\mathbb{1}(\text{Group}_g)$ are indicators for sub-groups, α_{tg} and δ_{cg} are year-group and CZ-group fixed effects, and ε_{ctg} is the error term.

The difference-in-differences estimators are identified under two assumptions: parallel trends and no anticipation. The parallel trends assumption states that in the absence of the merger, outcomes for treated and control groups would have evolved in parallel. I test for pre-trends in event study results to examine the plausibility of the assumption. The second assumption requires that mergers are not occurring in anticipation of changes in treated CZs. One concern is that the decision to merge reflects insurers' expectations of future premiums growth in the set of CZs where they share a market presence. Although underlying motivations for mergers are not publicly revealed, I examine whether premiums increase in placebo markets that would have been treated if not for the merger being blocked by antitrust authorities to provide support for the no-anticipation assumption. Because the merging health insurers in my sample are national or multi-state, the decision to merge is unlikely to be driven by anticipated premium growth specifically in the overlapping markets. Prior to the merger, the geographic distribution of insurers' markets are largely fixed, and merging insurers cannot easily enter markets to alter the set of CZs in which they share overlapping market presence.[7]

Aggregate Trends Over Time. Figure 1 shows that the average annual ESHI premiums per participant (paid by firms and workers) have increased from \$3,250 in 2000 to \$5,000 in 2020,

[7] High barriers to entry exist because insurers must establish relationships with new downstream employers and upstream hospitals and medical providers when entering a new market.

as the average number of insurers in CZs have fallen from 16 in 2000 to 11 in 2020. The goal of this paper is to measure the extent to which insurer mergers have contributed to rising ESHI premiums and study firm and worker responses to merger-induced changes in ESHI costs.

Aggregate Trends Across Treatment Over Time. Figure 3 previews the effects of insurer mergers by plotting mean CZ trends in earlier-exposed and later-exposed CZs over the 4 years prior to and 6 years following an insurer merger. Figure 3 Panel A indicates that insurer concentration immediately and sharply increases post-merger. Insurer concentration trends in parallel before the merger, supporting the parallel trends assumption. Insurer concentration in earlier-exposed CZs jumps to a higher level immediately after the merger, and continues to increase from 2,500 to 3,000 HHI points (an 18 percent increase), while insurer concentration trends smoothly in later-exposed CZs post-merger. Meanwhile, Figure 3 Panel B shows that premiums exhibit parallel pre-trends before the merger and increase from \$4,500 to \$5,200 (a 15.5 percent increase) in earlier-exposed CZs after the merger. The increase occurs with a delay. Six years post-merger, premiums in earlier-exposed CZs have fallen slightly but remain higher than premiums in later-exposed CZs, whose premiums evolve smoothly around the merger.

In response to the ESHI premiums increase due to insurer mergers, Panels C and D indicate that annual wages remain unchanged, while employment falls in single-establishment firms. Wages are smoothly increasing before and after the merger and trend in parallel across treatment. While the employment-to-population ratio also increases in parallel across earlier-exposed and later-exposed CZs, the employment-to-population ratio gradually falls in the three years after the merger from 0.47 to 0.44 percentage points (a 6 percent decrease) before stabilizing in the medium-run.

4. Effect of Insurer Mergers on Employer-Sponsored Premiums

I study the effect of insurer mergers on the premiums of vulnerable fully-insured firms who purchase plans from insurers. I aggregate plan-level data to implement a difference-in-differences analysis at the CZ level, comparing the premiums of vulnerable firms in earlier-exposed CZs to later-exposed CZs, before and after mergers. I examine merger effects on the quality of insurance plans to consider whether consumers benefit. In the next section, I examine how premium changes from insurer mergers affect the labor market outcomes of workers in vulnerable firms.

4.1 CZ-Level Premium Effects in Vulnerable Firms

Baseline Premium Effects. Figure 4 presents results from estimating equation (1) for annual employer-sponsored health insurance premiums per participant in vulnerable firms. Premiums of vulnerable firms in earlier-exposed and later-exposed CZs evolve in parallel before the merger, suggesting that the parallel trends assumption necessary for identification is plausible.

Premiums increase for vulnerable fully-insured firms in earlier-exposed CZs after the merger relative to later-exposed CZs. On average, vulnerable firms in CZs exposed earlier to insurer mergers experience a \$434 (10 percent, standard error = \$202) increase in premiums compared to later-exposed CZs over the six post-merger years. Premiums rise sharply in the third year post-merger by \$643 (14 percent, standard error = 320), remain higher over the next two years, before falling to a \$448 (standard error = 305) increase in the sixth year. Premiums increase with a delay as insurers and employers typically negotiate over contracts at an annual frequency; increased insurer market power may not immediately translate into higher premiums. Moreover, premium changes are measured with a one-year lag in the F55 which records the year as the end of the plan year, whereas premiums are negotiated at the start of the plan year. The inverted u-shaped trajectory and magnitude of the increase are similar to those estimated in prior case studies ranging from 7 to 14 percent (Dafny et al., 2012; Guardado et al., 2013).

Appendix Figure A.7 indicates that rival non-merging insurers also increase premiums by \$568 (12.6 percent, standard error=212) for vulnerable firms in earlier-exposed CZs compared to those in later-exposed CZs. Hence, vulnerable firms throughout exposed markets face higher premiums, regardless of whether their insurer is directly involved in the merger. The finding is consistent with both merging and rival non-merging insurers in exposed markets exercising their increased market power to negotiate higher premiums (Dafny et al., 2012).

A simple extrapolation suggests that insurer mergers caused 22 percent of the overall premium growth over the last two decades. The estimate comes from four inputs. First, my baseline estimate finds that fully-insured premiums per participant increase by \$434 after a merger. Second, I assume the average worker has two participants in the health plan. Third, out of the 683 unique CZs in the Form 5500, I identify 246 CZs exposed by insurer mergers an average of 4 times. Thus, the average CZ experiences a \$1,237 increase in fully-insured premiums per worker due to insurer mergers.[8] The fourth input is that premiums across all plans increased by \$5,636 per worker from 1999 to 2020 (Figure 1). Extrapolation suggests that insurer mergers

[8] Average increase is: $434 \cdot 3 \cdot \frac{246}{683} \cdot 4 = 1237$

caused 22 percent (= 1237/5636) of overall premiums growth over the last two decades.[9]

4.2 Robustness

Placebo Test of Blocked Mergers. A key threat to identification is if merging insurers anticipate premiums growth in exposed markets where both insurers have pre-merger market shares. Exposed markets would experience premiums growth even in the absence of the merger, so my baseline premiums would be biased upwards. To test whether merging insurers anticipate premiums growth, I conduct a placebo test of two blocked mergers between Humana-Aetna and Cigna-Anthem in 2017.[10] I follow the same empirical strategy as used in my baseline analysis. Placebo-exposed markets are CZs where both merging insurers have market shares (N=33 CZs). Because the blocked mergers occur in 2017, I do not observe later-exposed markets, so I use never-exposed CZs (N=226 CZs) as controls. Figure A.8 indicates that insurer concentration and premiums for vulnerable firms remain stable in the years following the blocked merger. If anything, my results show a stabilization post-merger, as insurer HHI is rising at a faster rate and premiums are falling at a faster rate in placebo-exposed CZs compared to control CZs prior to the merger. The evidence provides support for the assumption that mergers are not motivated by anticipated premiums growth in treated markets.

Table 2 summarizes findings from additional tests, where the first row replicates baselines estimates from estimating equation (1) (Figure 4). Subsequent rows present one-off deviations from this baseline. The results are generally stable.

Robustness to Regional Shocks and Other Controls. Estimates are robust when accounting for regional shocks and other factors that may coincide with insurer mergers and affect premiums; see Table 2 Panel A. The estimate is robust to flexibly controlling for local exposure to Chinese import competition using a measure from Autor et al. (2013b) and the 2006 to 2009 change in net housing worth as a proxy for the local Great Recession shock from Mian and Sufi (2014). Including flexible census division trends results in an estimate of \$858, indicating that unobserved regional heterogeneity does not appear to be driving the effect. Estimates are robust to the controlling for lagged CZ hospital concentration, share of firms self-insured,

[9]The actual impact may be more or less than 22 percent due to statistical and specification uncertainty. Using the lowest to highest estimated effect on premiums of 253 to 858 from robustness checks (Table 2) yields extrapolation estimates of 13 to 44 percent.

[10]Two additional mergers during the sample period between Independence Blue Cross and Highmark in 2009 and between Tufts and Harvard Pilgrim in 2011 (later successful in 2021) were blocked. I do not study these mergers due to insufficient F55 data.

population size, or the rural share (see also Appendix Figure A.10).

Functional Forms. Table 2 Panel B indicates using premiums in logs as the outcome of interest in estimating model (1) yields an estimate of a premiums increase of 13 percent ($579, standard error = 5). Omitting CZ population weights leads to an estimated increase of $253 (standard error = 231) that is not statistically significant; Appendix Figure A.11 provides suggestive evidence that premium increases are driven by CZs with above-median populations. Appendix Figure A.14 compares baseline event study estimates using Sun and Abraham (2021) weights to the canonical two-way fixed effects specification. The evolution and magnitude of estimated premiums effects are nearly identical with a baseline estimate of $434 (standard error = 202) and TWFE estimate of $426 (standard error = 296).

Geographic Boundary of Markets. In panel C of Table 2, I find that premiums estimates at the narrower geographic delineation of core-based statistical areas, urban clusters of 10,000 or more residents, are insignificant and negative (-$76, standard error = 241), but CBSAs have the drawback that they do not cover all regions of the U.S. Estimates at the broader geographic delineation of hospital referral regions, medical care markets with a minimum population size of 120,000, are consistent with my baseline estimate ($444, standard error = 235).

4.3 Changes in Health Plan Quality

An alternative explanation is that premium increases reflect improvements in the quality of health plans after insurer mergers. In Figure 5, I examine the effect of insurer mergers on types of plans offered by vulnerable firms to shed light on whether plan quality changes post-merger. Using MEPS data, I show that that the share of fully-insured plans that require referrals from primary care physicians to see a specialist increases by 6.5 percentage points (24 percent, standard error = 3.2) in earlier-exposed CZs relative to later-exposed CZs. Types of plans requiring referrals include health maintenance organization (HMO) and point of service (POS) plans. I corroborate the finding using F55 data in Panel B which shows that the share of HMO plans in CZs increases by 3 percentage points (17 percent, standard error = 2.6), and Panel C indicates that employers offering non-HMO plans prior to the merger are 6 percent more likely to switch to a HMO plan afterwards (standard error = 2.5). Given that HMO plans offer access to a narrower network of medical providers and have stricter guidelines to keep costs low relative to other types of plans, the adoption of HMO plans suggests that employers switch to lower-quality, more restrictive plans to limit premium increases. The findings rule out that increases in premiums are driven by improvements in plan quality.

5. Effect of Insurer Mergers on Firms and Workers

In this section I study how vulnerable firms that typically negotiate with insurers over premiums respond to the merger-induced increase in ESHI premiums documented in the previous section. Using a parsimonious difference-in-differences analysis at the CZ level, I estimate the effect of insurer mergers on vulnerable firms in earlier-exposed CZs relative to those in later-exposed CZs. I document that vulnerable firms experience large employment losses. I then decompose the employment loss by worker characteristics to examine who suffers employment losses. I find that losses are primarily for noncollege-educated, middle-income individuals. In the next section I examine how employment losses in vulnerable firms affect the overall local labor market either by inducing out-migration, reallocating workers across firms within CZs, or increasing unemployment.

5.1 Standard Competitive Model

Following Summers (1989), I use a standard competitive model to consider how vulnerable firms might respond to an increase in ESHI costs due to insurer mergers. Starting from a pre-merger equilibrium where labor supply equals labor demand, Appendix Figure A.15 shows the expected post-merger response of wages and employment in vulnerable firms. We would expect the labor demand curve to shift down by approximately $900 per worker due to the increase in ESHI premiums (Figure 4).[11] Given that workers do not value the cost increase, the labor supply curve remains unchanged, resulting in an unambiguous decline in wages and employment post-merger.[12] The demand shift along the labor supply curve allows us to recover the elasticity of labor supply for vulnerable firms: highly elastic supply suggests larger employment and smaller wage declines, while inelastic supply implies smaller employment and larger wage declines.

Of course, the model over-simplifies how the labor market functions in at least four ways. First, by focusing only on employment as a discrete choice, the model overlooks labor force

[11] Assuming that the average worker enrolls one dependent in her ESHI plan, the merger-induced increase in ESHI costs is approximately $900 per worker ($\approx \$434 \cdot 2$).

[12] If workers value the increase in ESHI costs, the labor supply curve would shift down. Wages fall, while the effects on employment are ambiguous and depend on how much workers value the cost increase. In the case of full valuation, post-merger employment is the same level as in the pre-merger equilibrium.

participation and hours of work as inputs into labor supply. Second, wage rigidities may prevent firms from lowering wages. Third, rather than lower wages, firms may respond by dropping health insurance coverage or lowering pensions and other non-wage compensation. Fourth, the model does not consider heterogeneity across workers. Fifth, the model also assumes no pre-existing distortions in the labor market, and ignores job search frictions. I discuss these departures from the standard competitive model after I present the empirical evidence.

5.2 CZ-Level Wage Effects in Vulnerable Firms

Baseline Wage Effects. I test predictions of the benchmark model by comparing annual wage earnings in vulnerable firms in earlier-exposed CZs to those in later-exposed CZs. Using data from the LEHD, I focus on single-establishment firms who are vulnerable to insurer mergers because of their smaller size; smaller firms typically offer fully-insured plans which require negotiating with insurers over premiums (KFF, 2023).[13] To evaluate wage responses in vulnerable firms, I aggregate the annual wage earnings of working-age individuals (ages 18 to 64) in an unbalanced panel of single-establishment firms to the CZ level.

I separately analyze annual wage earnings for new hires and incumbent workers given substantial empirical evidence of downward nominal wage rigidities for incumbent workers (see, e.g., Grigsby et al. (2021) and Cajner et al. (2021) for recent analyses). Several plausible mechanisms predict wage differences between new hires and incumbent workers. For example, models incorporating implicit contracts predict that incumbents' wages are rigid, while new hires' wages are flexible (Harris and Holmstrom, 1982; Beaudry and DiNardo, 1991).

Figure 6 shows results from estimating equation (1) for the log annual wage earnings of new hires and incumbent workers in vulnerable firms. Wages in treated and control CZs trend in parallel leading up to the insurer merger, which supports the parallel trends assumption for identification. Following the merger, the wages of incumbent workers in vulnerable firms remain constant across earlier-exposed and later-exposed CZs, whereas the wages of new hires fall in the second and third year post-merger and remain at a lower level for at least eight years. The wages of new hires decreases by an average of 4.2 percent (standard error = 1.7)

[13] I exclude multi-establishment firms for three reasons. First, multi-establishment firms are less vulnerable to insurer mergers because larger firms tend to be self-insured. Second, even if multi-establishment firms offered fully-insured plans, their premiums may not increase because firm-wide plans tend to be set in the firm's headquarters. The location of firm headquarters, rather than establishment location, is more likely to determine establishment exposure to insurer mergers. The LEHD does not report key information on firm headquarters or establishment location to determine exposure.

in the post-merger period. One possible mechanism for the finding is the existence of implicit contracts that constrain the wages of incumbent workers but not the wages of new hires. Another mechanism could be that firms do not lower the wages of incumbent workers, who have a reference point based on their past wages, to preserve worker morale (Blinder and Choi, 1990; Bewley, 1999), whereas new hires lack a similar reference point.

The implied dollar decline in annual wages for new hires is approximately \$200 ($= 0.04 \cdot 4701$). Even for new hires whose wages are more downwardly flexible, only one-fifth of the \$900 increase in per-worker costs is passed through to workers. The finding implies that a small portion of the cost increase is at most borne by workers in the form of lower wages, and offers the first direct evidence that workers' wages are largely unresponsive to ESHI cost increases, contrary to the predictions of the benchmark model. The finding is consistent with Brot-Goldberg et al. (2024) who provide indirect evidence of minimal pass-through following ESHI cost increases by comparing the magnitude of employment and total payroll effects. Many studies of payroll taxes, which like ESHI costs are paid by the firm to fund benefits received by workers, find no or limited wage pass-through (e.g., see Saez et al. (2019), Benzarti and Harju (2021) and Guo (2024) for recent papers), which point to presence of frictions that hinder the free adjustment of wages. In contrast, papers on the incidence of mandated benefits find that the costs of the mandates are substantially shifted to workers' wages. For example, Gruber (1994) find that the costs associated with mandating insuring childbirth are fully shifted to the wages of workers who may benefit from the mandate, and Gruber and Krueger (1991) find that 56 to 86 percent of the cost increase from mandating workers' compensation insurance is shifted to workers' wages. Wages may adjust more freely to mandated benefits because the costs are more salient and workers place a higher value on the benefits.

One concern with interpreting the wage results is that changes in wages may reflect changes in worker composition over time across earlier-exposed and later-exposed CZs. I use a composition-adjusted measure of wages in Appendix Figure A.16 and find no effect on wages after accounting for changes in worker composition (t-statistic = 1.0).

Other Margins of Adjustment. Firms facing higher premiums may reduce health insurance compensation or other non-wage compensation, including dental and vision insurance or pensions, offered to workers. Table 3 presents coefficients estimated from equation (1) to examine whether vulnerable firms use alternative methods to pass costs onto workers. I separately analyze three datasets aggregated to the CZ level to examine non-wage margins of adjustment. First, I use the Form 5500 to examine non-wage compensation offered by firms that offer fully-insured health plans. Second, I use the MEPS-IC to study health insurance coverage, focusing on vulnerable single-establishment firms. Third, I use the ACS to examine the part-time worker share; the sample consists of workers in *all* firms from 2005 onwards.[14]

In response to higher ESHI costs from insurer mergers, vulnerable firms do not reduce the number of health insurance plans offered, decrease premium contributions, or discontinue employer-sponsored health insurance (ESHI), but instead reduce the share of workers eligible for ESHI by 3.6 percent (2.9 percentage points, standard error = 1.6 percentage points). At the same time, the part-time worker share increases by 4.8 percent (0.77 percentage points, standard error = 0.3). The finding suggests that firms hire more part-time workers who are not eligible for health insurance coverage to mitigate cost increases post-merger. I find similar (although noisier) evidence of a 7 percent increase in part-time work within vulnerable firms using MEPS-IC data in Appendix Figure A.17. I find no evidence to suggest that vulnerable firms enroll fewer workers in a health insurance plan offered through the workplace (Table 3 column 6). Appendix Figure A.17 shows that non-elderly individuals (under age 65) in exposed CZs are not less likely to be enrolled in an employment-based health insurance plan. Thus, neither workers nor dependents lose their health insurance coverage following merger-induced increases in ESHI costs.

Table 3 Panel B shows that vulnerable firms reduce workers' pensions in response to a merger-induced increase in ESHI costs. The number of pension plans offered by vulnerable firms in exposed CZs falls by 5 percent (= 3.9/70, 4 fewer plans), and vulnerable firm contributions to pension plans falls by $167 per worker (7 percent, standard error = 97).[15] For vulnerable firms offering pension plans, approximately 19 percent of the $900 per worker increase in ESHI costs is passed through to workers in the form of lower pensions. In contrast, firms do not lower the generosity of dental and vision plans, or life, sickness, and accident insurance plans (the number of dental and vision plans offered in exposed CZs actually increases post-merger).[16] However, I do not observe the share of premiums paid by the firm for these plans, which may be the more relevant margin of adjustment.

Summary. Contrary to the benchmark model, firms only reduce wages for new hires. Vul-

[14]CZ-level ACS data is only available beginning in 2005.

[15]I assume that pensions are typically extended only to the worker and not their dependents.

[16]Among firms offering health insurance benefits, over 90 percent provide separate dental plans, and over 80 percent offer separate vision plans from the health insurance plan (KFF, 2023).

nerable firms affected by insurer mergers pass part of the increase in ESHI costs onto workers by lowering the wages of new hires and reducing pensions for all employees. Without full pass-through, workers are more costly for vulnerable firms to employ. We would expect to see a decline in employment among vulnerable firms in the earlier-exposed CZs relative to later-exposed CZs, which I will examine next.

5.3 CZ-Level Employment Effects in Vulnerable Firms

Baseline Employment Effects. I examine the employment responses of vulnerable firms to insurer mergers in earlier-exposed CZs relative those in later-exposed CZs. I use the same sample of single-establishment firms in the LEHD used to evaluate wage effects. I aggregate employment in vulnerable firms to the CZ-level by calculating the share of the working-age population employed in vulnerable single-establishment firms (which I refer to as the employment-to-population ratio).

Figure 7 presents results from estimating equation (1) for the employment-to-population ratio. The absence of pre-trends supports the plausibility of the parallel trends assumption. Vulnerable firms in CZs impacted by merger-induced ESHI cost increases experience a gradual decline in the employment-to-population ratio over the first three years after the merger, which then stabilizes at a lower level for up to eight years after the merger. The employment-to-population ratio falls by 2.1 percentage points (≈4.4 percent, standard error = 0.9) on average over the eight years following the merger relative to the year before the merger. The average CZ has a pre-merger population of 267,800, so the employment decline corresponds to an estimated total loss of approximately 11,800 jobs ($= 0.044 \cdot 267,800$) in vulnerable firms. The employment loss in vulnerable firms aligns with the benchmark model's predictions that rising ESHI costs lead to decreased labor demand and consequently lower employment. Given that premiums are determined by contracts negotiated in the prior year, the gradual increase in premiums beginning in the year following the merger is consistent with a gradual decrease in employment beginning in the year of the merger.

I estimate employment losses that are seven to ten times larger than losses reported in studies examining other factors driving up ESHI costs. I find that employment in vulnerable firms fall by 4.4 percent following a 10 percent increase in ESHI costs from insurer mergers. In contrast, Gao et al. (2022) report a decline of 0.3 to 0.6 percent in employment following a 2 percent increase in ESHI costs, using recent insurer losses as an instrument. Similarly, Brot-

Goldberg et al. (2024) find a 0.4 percent drop in employment in vulnerable firms following a 1 percent increase in ESHI premiums due to hospital mergers. A key reason the estimated employment losses from insurer mergers are significantly larger than those from other factors is that insurer mergers cause a substantial market-wide increase in premiums of 10 percent. In contrast, shocks to individual insurers or hospitals analyzed in other studies lead to smaller premium hikes (less than 2 percent) and consequently smaller employment losses.

The Implied Labor Supply Elasticity of Vulnerable Firms. Together, the estimated employment decline of 4.4 percent (Figure 7) and an increase in net-of-tax-and-ESHI earnings of 3.5 percent[17] implies an elasticity of labor supply for vulnerable firms of 1.3.

The implied labor supply elasticity for vulnerable firms of 1.3 is comparable to the elasticity of 0.5 in Gao et al. (2022) and the elasticity of 1.0 in Brot-Goldberg et al. (2024) who leverage quasi-experimental variation in ESHI costs. The implied elasticity in my study is on the lower end of the range of elasticities reported in studies of payroll taxes. Saez et al. (2019) find an elasticity of 2.4 following a payroll tax cut for younger workers in Sweden, Lobel (2024) estimate an elasticity of 4.15 following a payroll tax cut for specific sectors largely in the manufacturing industry in Brazil, and Ku et al. (2020) calculate an elasticity of 4.28 following a place-based payroll tax increase in Norway.

Exposure to Insurer Mergers. To investigate dosage effects, I compare merger effects on vulnerable firms in CZs experiencing above- and below-median increases in post-merger insurer concentration in Figure 9. First, I show that earlier-exposed CZs with an above-median insurer count appear to experience larger post-merger increases in insurer concentration than those with a below-median insurer count (31 vs. 6 percent, t-statistic = 1.61). In turn premium increases appear to be driven by earlier-exposed CZs with an above-median insurer count that face larger increases in post-merger insurer concentration (14 vs. 5 percent, t-statistic = 0.49). We might expect that already-concentrated CZs with fewer insurers would experience larger post-merger premium increases. However, insurers may be less effective at extracting further increases in CZs where premiums are above competitive levels. Finally, I find that employment in vulnerable firms falls by 3.2 percentage points (≈6.5 percent, standard error = 1.0) in CZs with an above-median insurer count, but not in those with a below-median insurer count.

[17] Pre-merger ESHI costs are approximately \$4,500 per participant (Figure 4). Assuming the average worker has one dependent on their health plan, then the per-worker cost is \$9,000 ($= 2 \cdot 4,500$). Given that premiums increase by 10 percent (Figure 4), post-merger ESHI costs per worker are equal to \$9,900 ($= 9,000 \cdot 1.1$). Between the sample period of 1999 to 2020, the median worker's net-of-tax wages was \$35,000 (Luxembourg Income Study, 2024). Therefore, the change in net-of-tax-and-ESHI wages is equal to 3.5 percent in treated CZs. $\frac{-9{,}000+9{,}900}{35{,}000-9{,}000} \cdot 100 = 3.5$.

Overall, in CZs experiencing large post-merger increases in insurer concentration, insurers exercise market power to increase the premiums of vulnerable firms, resulting in large employment declines. The findings suggest that insurer market power is a key channel through which insurer mergers affect premiums and labor market outcomes.

My findings emphasize the importance of focusing on changes in concentration rather than levels when assessing mergers for potential harm. The evidence is consistent with Nocke and Whinston (2022), who provide a theoretical and empirical basis for prioritizing changes in concentration over levels, and past work that documents price effects only for mergers leading to largest increases in concentration post-merger (Arnold, 2019; Prager and Schmitt, 2021). Indeed, while the Department of Justice and Federal Trade Commission Horizontal Merger Guidelines have historically prioritized concentration levels, recent guidelines released in 2023 suggest that markets with a HHI of greater than 1,800 that would experience a change in HHI of greater than 100 warrant further scrutiny.

5.4 Decomposing the Employment Decline.

To investigate heterogeneous effects across workers, I decompose my baseline estimate of a 2.1 percentage point employment loss in vulnerable firms by pre-merger worker wages, education, and other characteristics.

By Wage Bins. Figure 10 decomposes the overall employment loss by bins of workers' pre-merger annual wage earnings. One challenge with the decomposition is that I cannot categorize workers based on their pre-merger annual wages using my unbalanced LEHD sample. For the wage decomposition exercise only, I therefore restrict the LEHD sample to include workers employed in vulnerable firms in the year preceding the merger and following them throughout the observation period. I report the share of the overall employment reduction explained by workers in each wage bin (depicted in orange), and compare to the share of pre-merger CZ employment in each wage bin (depicted in blue).

Figure 10 shows that employment losses are primarily driven by losses for workers earnings less than $80,000 in the bottom 85 percentiles of the national income distribution. Workers earning less than $80,000 make up 85 percent of employment in affected CZs, but can explain virtually all (99.8 percent) of the overall 2.1 percentage point decline in employment from vulnerable firms. The finding is consistent with a fixed dollar increase in costs per worker that makes lower-income workers relatively more expensive to hire. In particular, middle-income workers earning between $20,000 to $60,000 annually (roughly the 25th to 75th percentiles of the national income distribution) are disproportionately affected; these middle-income workers make up 60 percent of the pre-merger CZ employment, but account for 77 percent of the total employment losses. Workers earning less than $20,000 (the bottom 26 percentiles) do not experience disproportionately larger employment losses despite earning the lowest incomes. Many of these lowest-income workers are employed part time and not covered by their own firms' insurance plan (see Appendix Figure A.19), so are less vulnerable to an increase in ESHI costs relative to middle-income workers. The findings suggest that rising ESHI costs from insurer mergers contribute to the "hollowing out of the middle class".

By Education. Figure 11 decomposes the overall employment loss by worker education. Workers without college degrees make up over three-quarters of total employment (78 percent), but account for 88 percent of the total reduction in employment. The finding suggests that less-educated workers are disproportionately impacted by a fixed dollar increase in ESHI costs per worker as they become more expensive to hire compared to more-educated workers. Given that education is imputed for 85 percent of workers in the LEHD, I examine the employment effect by education using the ACS. Compared to the LEHD sample, the ACS sample includes workers employed in all firms and only begins in 2005 for which I can observe CZ-level outcomes. Appendix Figure A.20 shows that the decline in the noncollege-educated employment rate (0.97 p.p., 1.1 percent) is larger than the decline in the college-educated employment rate (0.63 p.p., 0.4 percent), but the impacts are not statistically distinguishable (p=0.3).

By Worker Sex, Age, Race/Ethnicity, and Tenure. Figure 11 shows estimates by worker sex, age, and race/ethnicity. Females and males both experience employment losses proportionate to their share of total CZ employment. We might expect firms to employ workers with lower health costs (e.g., younger workers) to mitigate the cost increases (Summers, 1989). However, I find that employment losses are largely driven by losses for younger under-50 workers compared to older over-50 workers, suggesting firms are not reducing employment for specific groups that are more expensive to insure. Black workers are disproportionately impacted by the increase in ESHI costs; black workers who account for only one-tenth of the population account for roughly one-third of the overall reduction, compared to white workers who make up 68 percent of the population but only 53 percent of the overall reduction.

Employment losses disproportionately affect new hires compared to incumbents. While new hires make up one-third of total employment, new hires account for roughly 50 percent of the overall 2.1 percentage point employment loss. The pronounced decline in employment for black workers and new hires may relate to worker turnover. Vulnerable firms that face merger-induced increases in ESHI costs may respond by hiring fewer new workers and allowing for greater attrition. The explanation would be consistent with the literature showing that black workers are more likely to lose their jobs and less likely to find new ones compared to white workers (Couch and Fairlie, 2010; Daly et al., 2020; Derenoncourt et al., 2024).

By Industry. Appendix Figure 11 decomposes the overall employment loss by industry. I focus on the 9 largest industries sorted by the share of pre-merger employment, and a residual category for all other industries. While most industries experience employment declines, manufacturing, retail trade, construction, and administrative services each account for close to 20 percent of the estimated overall reduction in employment, which is larger than each industry's share of total CZ employment. Increases in ESHI costs from insurer mergers lead to disproportionately larger losses in industries that largely employ noncollege-educated workers in low- or middle-skill occupations.

5.5 Robustness Checks

I explore the robustness of my baseline estimate in Figure 4. I begin by restricting the LEHD sample to firms that can be linked to the Form 5500, followed by examining alternative specifications and samples. My baseline estimates are generally consistent across different specifications and samples.

Linked LEHD-Form-5500 Panel. In my baseline analysis, I focus on single-establishment firms because they are more likely to be fully insured and therefore vulnerable to insurer mergers. To confirm that fully insured firms facing higher ESHI costs drive estimated employment losses, I restrict the sample to single-establishment firms in the LEHD linked to Form 5500. Approximately 22 percent of the working age population is employed by fully-insured firms[18]. After linking, I find that 6 percent of the working-age population works for a fully insured single-establishment firm, yielding a match rate of 27 percent ($= 6/22$). Conversely, when linking single-establishment firms in the LEHD to self-insured firms in the Form 5500, I find that about half of the CZs in my sample have no single-establishment firms that can be identified as self-insured. In CZs where such firms are identified, a negligible share of the working-age

[18] Approximately 36 percent of workers are employed in a fully-insured firm and 60 percent of the working-age population is employed. It follows that approximately 22 percent ($= 0.36 \cdot 60$) of the working-age population works in fully-insured firms.

population (0.2 p.p.) is employed in single-establishment self-insured firms, which suggests using single-establishment firms as a proxy for those offering fully-insured plans in my baseline sample is reasonable.

Table 4 Panel A presents the results from estimating equation (1) for linked LEHD-Form-5500 samples. Employment in fully-insured firms appears to decline by 0.3 percentage points (standard error = 0.2), but the coefficient is not statistically significant, while employment in self-insured firms remains relatively constant (0.01 percentage points, standard error = 0.02). The implied loss of employment in fully-insured single-establishment firms is 3.3 percent, similar to the baseline loss of 4.6 percent. The noisy estimates may result from the smaller sample size of linked firms. I link the Form 5500 with the LBD covering all U.S. states to examine the entire CZ sample. Table 4 Panel B shows that employment in single-establishment firms offering fully insured plans falls by 7.7 percent (standard error = 3.5). The finding suggests that employment losses are being driven by fully-insured firms.

Robustness to Sample Definition. Estimates are stable when varying the definition of the baseline sample; see Table 5 Panel A. Including workers earning less than $3,700 results in a similar-sized decline in the employment-to-population ratio of 2.73 percentage points (standard error = 1.09, 4.6 percent). Appendix Figure A.21 shows that payroll in vulnerable single-establishment firms in the LBD, available for all U.S. states, falls by 3.9 percent following a merger-induced increase in ESHI costs. The estimate is close to the implied 4.6 percent decline in vulnerable single-establishment firms in the 25 LEHD states.

Robustness to the Great Recession and Other Events. Estimates are stable when controlling for contemporaneous local labor demand shocks including the Great Recession or import competition with China, as shown in Table 5 Panel B. I account for the local severity of the Great Recession shock, which occurred during my sample period, by allowing changes in the net housing wealth from 2006 to 2009 to vary across time.[19] The estimate decreases slightly to 1.79 percentage points (standard error = 0.873). Given that construction and retail trade industries were heavily affected, I allow the pre-merger shares of employment in these sectors to have arbitrary effects across time. The estimate remains at 2.38 percentage points (standard error = 0.893) and 1.97 percentage points (standard error = 0.824) after including manufacturing and construction, and retail trade controls. I also flexibly control for local exposure to trade

[19] I obtain the measure from Mian and Sufi (2014) who show that the decline in net housing worth was a significant contributor to the decline in U.S. employment between 2007 and 2009.

competition with China using the Autor et al. (2013a) measure and find that the estimated decline of 2.297 percentage points (standard error = 0.882) is similar in magnitude to my baseline estimate. To broadly account for regional shocks, I include census division fixed effects which I allow to have arbitrary effects across time. The estimate is 1.737 percentage points (standard error = 0.670). The stability of these estimates suggests that contemporaneous shocks are unlikely driving the results.

Functional Form. Estimates are similar when using log employment or a job-adjusted employment-to-population ratio as the outcome, or when estimating equation (1) without population weights; see Table 5 Panel C. Using log employment, I find a 3.9 percent decline (standard error = 1.9), which corresponds to an implied change of 1.88 percentage points in the employment-to-population ratio. Using the job-adjusted ratio, the estimate is 1.3 percentage points (standard error = 0.6). The implied percent change is a 3.8 percent decline (outcome mean = 0.348), close to the baseline estimate. Estimating equation (1) without population weights yields a decline of 1.48 percentage points (standard error = 0.736).

Employment Effects for Vulnerable Firms at the Firm Level. The analysis of merger-induced employment effects of vulnerable firms presented so far are pooled at the CZ level using an unbalanced panel. The CZ-level results using an unbalanced panel are of interest in their own right, as they capture employment losses from both exiting and continuing vulnerable firms. But one concern is the CZ-level analysis may be biased by changes in firm composition. I implement a firm-level difference-in-differences analysis following a balanced sample of firms that are active in the year before the merger to address potential composition bias. I discuss results shown in Appendix Figure A.22 in Appendix Section 9. Reassuringly, there is a clear decline in vulnerable firms' employment in earlier-exposed CZs relative to later-exposed CZs. I find that the employment losses are driven by a combination of firm exits and employment losses in continuing firms.

Employment Effects for Workers in Vulnerable Firms at the Worker Level. A similar concern is that changes in the composition of workers post-merger may bias the baseline results estimated at the CZ-level using an unbalanced panel. I conduct a worker-level difference-in-differences analysis following a balanced worker sample of workers ages 25 to 55 in vulnerable single-establishment firms in the year before the merger. The first follow exposed workers in their pre-merger firm, and then follow exposed workers in any firm over the observation window. I discuss results shown in Appendix Figure A.4 in Appendix Section 9. Workers are 2.6 percentage points (standard error = 0.1) less-likely to be employed in their pre-merger firm after the merger, consistent with my baseline analysis, but are only 0.4 percentage points (standard error = 0.1) less likely to be employed in any firm after the merger. The worker-level results suggest that individuals who lose their jobs are partially reallocated to new firms, which I will next explore.

6. Effect of Insurer Mergers on Local Labor Markets

In this section I consider the local labor market effects of insurer mergers following large employment losses in vulnerable firms. Individuals without employment may respond by migrating to a different labor market, seeking new employment at a less-vulnerable firm within the same labor market, or remaining non-employed and taking up government transfers. I assess these margins of adjustment by comparing outcomes in earlier-exposed CZs to those in later-exposed CZs. I find a partial reallocation of workers from vulnerable firms to less-vulnerable ones. Overall, there is a decline in aggregate employment in the local labor market post-merger, which leads to increased spending on unemployment insurance programs.

6.1 Migration Effects in Local Labor Markets

I study whether individuals respond to local employment losses by moving out of the CZ. I focus first on migration effects because if individuals are leaving CZs that experience employment losses, I am unlikely to observe adjustments within the CZ, such as the reallocation of workers to less-vulnerable firms or increases in non-employment. Appendix Figure A.24 presents results from estimating equation (1) for the working-age population at the CZ-level to assess whether individuals change their place of residence. I find no evidence that individuals move to a different CZ. I cannot reject the null hypothesis that the working-age population in affected CZs is the same after the merger as in the year prior to the merger. The result aligns with recent studies that find no migration responses following other local labor demand shocks, such as Great Recession (Yagan, 2019) or local trade shocks (Autor et al., 2013a; Choi et al., 2024). In Appendix Table A.5, I implement an individual-level analysis using the LEHD and find corroborating evidence that individuals do not move their place of residence or work.

6.2 Employment Effects in Local Labor Markets

Baseline Effects Using LEHD. Given no evidence of out-migration in Appendix Figure A.24, individuals without employment either find employment in less-vulnerable firms, become self employed, or remain non-employed. To probe these margins of adjustment, Figure 12 examines the impact of merger-induced cost increases on the working-age population share in five categories: employed in vulnerable single-establishment firms (replicating Figure 7), employed in less-vulnerable multi-establishment firms, unemployed, self employed, and not in the labor force. The categories are mutually exclusive but not collectively exhaustive, as employment that is not covered by state unemployment insurance is not captured in the LEHD. The population shares do not sum up to one as data sources differ across panels.

While the population share employed in vulnerable firms falls by 2.1 percentage points (standard error = 0.9) (Panel A, which replicates Figure 7), the population share employed in less-vulnerable firms increases by 1.3 percentage points (standard error = 0.4, Panel B). The finding suggests that workers reallocate from vulnerable to less-vulnerable firms in CZs facing merger-induced increases in ESHI costs. The employment gains in less-vulnerable multi-establishment firms account for approximately 60% ($= 1.3/2.1$) of the employment losses in vulnerable single-establishment firms. The finding suggests that the larger pool of non-employed workers grows post-merger makes it easier for larger firms that are less-vulnerable to hire new workers. The aggregate employment decline as a share of population is 0.8 percentage points ($= -2.1 + 1.3$), equal to a 1.2 percent ($= 0.8/(47.2 + 18.4)$) decline in earlier-exposed CZs relative to later-exposed CZs.

Lower employment for vulnerable firms in CZs exposed to merger-induced cost increases also leads to higher unemployment post-merger, as shown in Panel C. The population share that is unemployed steadily increases reaching a high of 0.51 percentage points (standard error = 0.23) in the fifth year post-merger. The finding is consistent with some individuals failing to find new employment in less-vulnerable firms immediately after the merger, resulting in an aggregate employment decline. As unemployment falls from its peak in the fifth year to nearly zero in the eighth year (0.06 percentage points), the population share working in less-vulnerable firms further increases, suggesting that initially unemployed individuals later find employment in less-vulnerable firms. Panel D shows suggestive evidence that the population share that is self-employed steadily increases, reaching a higher of 0.23 percentage points (standard error = 0.39) in the eighth year post-merger, but the effect is not statistically significant. I find no evidence of labor force exit, as shown in Panel E.

Aggregate Effects Using Publicly-Available Data. One concern with interpreting the LEHD results is that I only have data for 25 states. To examine whether results are valid for all CZs in my sample, I draw on publicly-available data from the U.S. Census Bureau's County Business Patterns (CBP) and the American Community Survey (ACS).

Figure 13 presents results from estimating equation (1) for the population share working in any firm using the CBP. I find that aggregate employment falls post-merger in all earlier-exposed CZs relative to later-exposed CZs, consistent with my results that rely on only 25 LEHD states. The population share employed in any firm falls by 0.66 percentage points (standard error = 0.36), which is statistically significant at the 10 percent level and aligns with my LEHD result of a 0.88 percentage point decline. Moreover, the implied change in aggregate employment using the CBP is 1.2 percent ($= 0.66/53$), which is the same as the change implied by the LEHD results. The similar aggregate employment effects observed in the LEHD and CBP suggest that the LEHD results are credible and externally valid to the full CZ sample.

Figure 14 presents the aggregate labor market effects of insurer mergers using the ACS. The ACS CZ sample is smaller than the full CZ sample, as the ACS is only available at the CZ level from 2005 onwards. I use a pooled version of estimating equation (1) at the CZ level, by interacting a CZ merger exposure indicator with an indicator for whether the year falls on or after the merger year. I find that the employment rate (the labor force share that is employed in any firm) falls by 0.9 percentage points (standard error = 0.3). The implied percentage change of 1 percent ($= 0.9/93$) is similar to the implied 1.2 percent decline in aggregate employment using the LEHD, which serves as an additional validation of my baseline result.

Market-wide annual earnings in the ACS sample falls by 1.6 percent (standard error = 0.8), which implies a decrease of \$675 ($= 0.016 \cdot 42229$). The finding in the ACS sample suggests a substantial pass-through of the \$900 per-worker cost increase to workers in vulnerable firms, that may enable less-vulnerable firms to lower wages for their own workers. The fall in annual earnings is driven by a decline in hourly wages of 1.4 percent (standard error = 0.6), while the number of hours worked weekly remains stable. The share of workers that are part-time increases by 0.8 percentage points (5 percent, standard error = 0.3). The result is consistent with the finding in Table 3 that vulnerable firms hire more workers who are not extended health insurance coverage in response to higher ESHI costs.

Given that vulnerable firms hire more part-time workers, the true extent of employment losses in vulnerable firms is likely understated in Figure 7. Figure 7 relies on LEHD employment data which does not distinguish between full-time and part-time workers, so even larger employment losses of full-time workers may be masked by increased employment of part-time workers.

Implied Labor Supply Elasticity of Local Labor Markets. Together, the aggregate employment effect of 1.2 percent (Figure 12), and aggregate net-of-tax-and-health-insurance wages of 3.1 percent[20] implies a labor supply elasticity in exposed local labor markets of 0.39. My estimate of 0.39 falls in the range of elasticities from existing studies reviewed in Chetty (2012) ranging from 0.15 to 0.45.

6.3 Government Transfer Effects in Local Labor Markets

Unemployment Insurance. The employment losses uncovered in CZs facing merger-induced rising ESHI costs would likely leads to higher participation in unemployment insurance (UI) programs. As workers are typically eligible for up to 6 months of UI receipts, we would expect UI receipts to increase during periods of active job loss but to return to normal levels over the long term. Figure 15 confirms the predicted pattern of the effects on total UI receipts at the CZ level. UI receipts steadily increase, peak in the sixth year after the merger at a high of $82 per capita (standard error = 27), before falling to $34 per capita (standard error = 36) in the following year.

The magnitude of UI receipts are consistent with the magnitude of estimated job losses. Over the six years following the insurer merger, I estimate that cumulative UI receipts increased by $43,700 per 100 residents (standard error = 15420) in treated CZs relative to control CZs. Given the finding that roughly 4 jobs are lost per 100 workers post-merger in Figure 7, I estimate an total UI payment of $10,925 per job lost. The payment is equivalent to $420 $(= 10925/26)$ paid per week per job lost, which is approximately equal to the average weekly UI payment of around $400 nationwide.[21]

Other Transfer Programs. I examine whether individuals turned to other transfer programs by examining the growth of a variety of government transfer programs at the CZ level

[20] Recall that the change in the net-of-tax-and-health-insurance wages in vulnerable firms is 4.4 percent. Given that 70 percent $(47/(47+18))$ of workers in local labor markets are employed in vulnerable firms, the aggregate change in net-of-tax-and-health-insurance wages is 3.1 percent $(= 4.4 \cdot 0.7)$

[21] Of course, average weekly UI receipts differ across states, ranging from an average of $215 per week in Mississippi to $550 per week in Massachusetts, prior to the expansion of the UI system during COVID (Brookings, 2020).

in Appendix Figure A.25. The finding in Figure 12 that CZs facing insurer mergers do not experience an increase in individuals who exit the labor force suggests that we would not expect to see an increase in Social Security disability benefits. I find no increase in spending on transfer programs outside of unemployment insurance programs, such as medical benefits and disability insurance benefits. Because unemployment insurance benefits comprises of only a modest share of total government spending on transfer programs, total transfers per capita does not significantly increase post-merger.

7. Labor Market Outcomes under Antitrust Intervention

In this section I ask how U.S. employment would change under antitrust intervention from 1999 to 2019. I extend Finkelstein et al. (2023)'s model of a competitive labor market to consider counterfactual paths for premiums if antitrust authorities would have blocked insurer mergers. Given the global trend of declining non-college employment, driven by factors such as skill-biased technical change and trade competition with China, I ask how much of the U.S.-specific decline in non-college employment can be explained by rising ESHI costs from insurer mergers. Specifically, I consider how much more non-college and college employment would have increased under antitrust intervention as a share of the observed U.S.-net-of-Canada change.

7.1 Model of Competitive Labor Market

Labor Demand. I consider a market with two worker types $g \in \{N, C\}$, college- and noncollege-educated workers. Given per-worker costs ω_g, firms choose the number of college- and noncollege-educated workers L_g to employ to maximize:

$$\max_{L_N, L_C} (\lambda_N L_N^\rho + \lambda_c L_C^\rho)^{\frac{1}{\rho}} - \omega_N L_N - \omega_C L_C, \tag{3}$$

where firm output follows a CES production function, λ_g represents group-specific productivity shifters, and ρ is the relative substitutability of noncollege and college-educated workers. The group-specific cost of employing a worker is equal to the group-specific wage w_g and the cost of employer-sponsored health insurance τ. Taking first-order conditions of the firm's maximization problem (3), equilibrium wages are:

$$\omega_g = w_g + \tau = \lambda_g L_g^{\rho-1}(\lambda_N L_N^{\rho} + \lambda_C L_C^{\rho})^{\frac{1-\rho}{\rho}} \tag{4}$$

Labor Supply. Workers face a discrete choice of whether or not to work. The indirect utility from working for individual i in group g is equal to:

$$U_{gi} = w_g + \alpha\tau - \varepsilon_i. \tag{5}$$

Indirect utility of working U_{gi} depends on a systematic component, equal to the sum of wages w_g and the amenity value $\alpha \geq 0$ of health insurance costs τ relative to wages, and a idiosyncratic component $\varepsilon_i \geq 0$ which represents the outside option of not working.

I normalize the utility from not working equal to 0, so individuals work when the utility from working exceeds the utility from not working ($U_{gi} > 0$). Assuming ε_i is a random variable uniformly distributed between $[\underline{\kappa}, \overline{\kappa}]$, the share of type g individuals working is equal to $P_g = Pr(w_g + \alpha\tau > \varepsilon_i)$. The labor supply function is linear between $\underline{\kappa}$ and $\overline{\kappa}$:

$$P_g = \begin{cases} 0, & w_g + \alpha\tau < \underline{\kappa} \\ \frac{w_g + \alpha\tau - \underline{\kappa}}{\overline{\kappa} - \underline{\kappa}}, & w_g + \alpha\tau \in [\underline{\kappa}, \overline{\kappa}] \\ 1, & w_g + \alpha\tau > \overline{\kappa} \end{cases} \tag{6}$$

The number of workers of each type is equal to the share of individuals of type g that are working multiplied by the number of individuals of type g, $L_g = P_g \cdot N_g$.

7.2 Calibration

Calibration. I assume that observed group-specific employment and wages, and the cost of ESHI premiums are equilibrium values. I use the CPS to measure employment and wages, and the MEPS-IC to measure premiums. Annual premiums per worker across all plans increased by \$5,636 from \$6128 in 1999 and \$11,763 in 2019. From 1999 to 2019, the noncollege employment-to-population ratio fell by 9 percentage points in the U.S. and by only 6 percentage points in Canada; the observed net-of-Canada change in the noncollege employment-to-population ratio over this period is equal to 3 percentage points ($= 9 - 6$). Over the same period the college employment-to-population ratio increased by 8 percentage points in the

U.S. and by 10 percentage points in Canada; the observed net-of-Canada change in the college employment-to-population ratio over this period is equal to 2 percentage points ($= 10 - 8$). I ask how much more would employment have increased under the counterfactuals relative to the U.S.-specific change in employment.

I assume ρ is equal to 0.38, based on Autor et al. (2020), and the amenity value of changes in health insurance costs from insurer mergers is equal to zero ($\alpha = 0$). I assume a common extensive-margin labor supply elasticity across groups (e^S) equal to 0.32. I explore the sensitivity to other values of ρ, α, and e^S in Appendix Table XX.

Implied Parameter Values. With values of w_g, L_g, ρ, α, and τ, I solve for the group-specific productivity shifters λ_g in equation (4). I then solve for the value of labor supply slope ($\overline{\kappa}$-$\underline{\kappa}$) using the labor supply function in equation (6). See Appendix 9 for a detailed derivation.

Counterfactual Premiums Growth. I consider two counterfactuals paths for ESHI cost growth under antitrust intervention. First, a "no-merger" counterfactual in which all mergers are blocked. Under the no-merger counterfactual, I scale the premiums growth in each year relative to 1999 by 78 percent (subtracting the 22 percent contribution from insurer mergers). Second, a "threshold" counterfactual in which mergers that would have caused a HHI increase of 200 are blocked. Under the threshold counterfactual, I scale the premiums growth in each year relative to 1977 by 81 percent, subtracting the 19 percent contribution from insurer mergers that would have caused a post-merger HHI increase of at least 200.[22]

7.3 Results

Figure 16 presents results from the calibration exercise. Panel A shows that premiums would be \$1,239 ($= 0.22 \cdot 5,636$) lower under the no-merger counterfactual and \$1,070 ($= 0.19 \cdot 5,636$) lower under the threshold counterfactual in 2019.

Panel B indicates that both noncollege and college employment increases under antitrust intervention. Noncollege employment-to-population ratio would be 0.28 percentage points higher in 2019 under the no-merger counterfactual. Rising ESHI costs from insurer mergers account for 10 percent ($= 0.28/3$) of the 3 percentage point decline in the U.S.-specific noncol-

[22]I calculate the 19 percent contribution from above-threshold mergers from two inputs. First, 246 out of the 690 unique CZs in the Form 5500 are treated by insurer mergers, an average of four times. Second, 53 out of the 81 in-sample earlier-exposed CZs experience an insurer merger that causes post-merger HHI to increase by at least 200; premiums increase by \$572 in these CZs. Therefore, the average increase in premiums across all CZs due to only above-threshold mergers is equal to \$1080 ($=\frac{246}{690} \cdot 4 \cdot \frac{53}{81} \cdot 572$), equal to 19 percent of the \$5,636 increase from 1999 to 2020.

lege employment-to-population ratio from 1999 to 2021. Similarly, the college employment-to-population ratio would be 0.13 percentage points higher under the threshold counterfactual. Blocking insurer mergers would reduce the 2 percentage point decline in the U.S. net-of-Canada college employment-to-population ratio by 8.6 percent (0.13/2). Moreover, a more realistic antitrust counterfactual that blocks mergers that would cause a increase in HHI of at least 200 yields employment increases similar to the no-merger counterfactual. Noncollege employment would be 0.28 percentage points higher if all mergers were blocked and 0.24 percentage points higher if only mergers above the threshold were blocked. Compared to the no-merger counterfactual, the threshold counterfactual has an efficacy of roughly 86 percent (= 0.24/0.28). The results highlight the potential labor market gains that could be achieved by blocking the most concentration-increasing mergers.

I conclude by conducting two additional counterfactuals that more broadly consider the potential labor market effects of the dramatic increase in ESHI costs in the U.S. First, I consider a "Canada" counterfactual under which health care spending in the U.S. grew at the same rate as health care spending in Canada. In 2019, health expenditures accounted for 16.77 percent of GDP in the U.S. and for 10.84 percent of GDP in Canada; I scale the growth in U.S. premiums by the ratio of the Canadian GDP share to the U.S. GDP share of health care spending which is 65 percent (= 10.84/16.77). I consider a second "no-growth" counterfactual under which premiums in the U.S. remained at its 1999 level. Table 6 presents the results in columns 3 and 4, alongside the results from the earlier antitrust counterfactuals for comparison. Under the Canada counterfactual, the noncollege employment-to-population ratio would increase by 0.45 percentage points and the college employment-to-population ratio would increase by 0.21 percentage points. If premiums grew at a similar rate to Canada, the observed net-of-Canada change in the noncollege employment-to-population ratio would be 15 percent smaller and the change in the college employment-to-population ratio would be 14 percent smaller. Under the no-growth counterfactual, noncollege employment would be 1.3 percentage points and college employment would be 0.6 percentage points higher. Consequently, the U.S.-specific decline in noncollege employment would be 44 percent smaller and the U.S.-specific slowdown in college employment would be 40 percent smaller.

7.4 Benchmarking Employment Effects of Rising ESHI Costs

To assess the magnitude of employment effects, I compare simulated employment losses from rising ESHI costs to estimated losses from the literature studying other causes of employment losses in the U.S. My baseline estimate is that rising ESHI costs from insurer mergers led to employment losses of 0.41 percentage points (Table 6 column 1), while the overall increase in ESHI costs led to employment losses of 1.9 percentage points (column 4). I find that employment losses from rising ESHI costs are comparable in magnitude to employment losses caused by other leading factors, such as robot adoption, trade shocks and the Great Recession.

The employment losses from merger-induced rising ESHI costs is comparable to the size of the estimated fall in employment caused by exposure to robots that Acemoglu and Restrepo (2020) estimate. They find an increase of one robot per thousand workers in a CZ led to a decline in the employment-to-population ratio of 0.44 percentage points between 1993 and 2007. An additional exposure of one robot per thousand workers is roughly equal to the average increase in robot exposure in the U.S. over this period. The employment losses from robot exposure is approximately equal to the 0.41 percentage point decline that I estimate is caused by rising ESHI costs from insurer mergers, and one quarter of the 1.9 percentage point decline caused by overall cost increase.

I compare my estimate of employment losses from rising ESHI costs to the impact of Chinese import exposure. Autor et al. (2013a) find that a $1,000 per worker increase in local Chinese import exposure reduces the employment-to-population ratio by 0.77 percentage points. An additional $1,000 per worker increase in Chinese import exposure is equal to the difference in exposure of a CZ at the 25th percentile and another at the 75th percentile of exposure to Chinese import growth from 2000 to 2007. Rising ESHI costs from insurer mergers result in employment losses that are half of the losses caused by a $1,000 increase in Chinese import exposure, and the overall increase in ESHI costs is approximately double the size.

I compare my baseline estimate to the estimated decline in employment following the local unemployment shock due to the Great Recession that Yagan (2019) estimate. Yagan (2019) finds that exposure to a 1 percentage point larger Great Recession shock (measured by the drop in 2007-2009 employment) reduced the employment-to-population ratio by 0.39 percentage points in 2015. The average CZ exposure to the Great Recession shock was equal to 4.6 percentage points, so the average CZ experienced a 1.79 percentage point drop in the employment-to-population ratio. The employment losses from the average local Great Recession shock is five times the size of the estimated employment losses from rising ESHI costs from insurer mergers, but roughly equal to the estimated employment losses from the overall increase in ESHI costs.

8. Conclusion

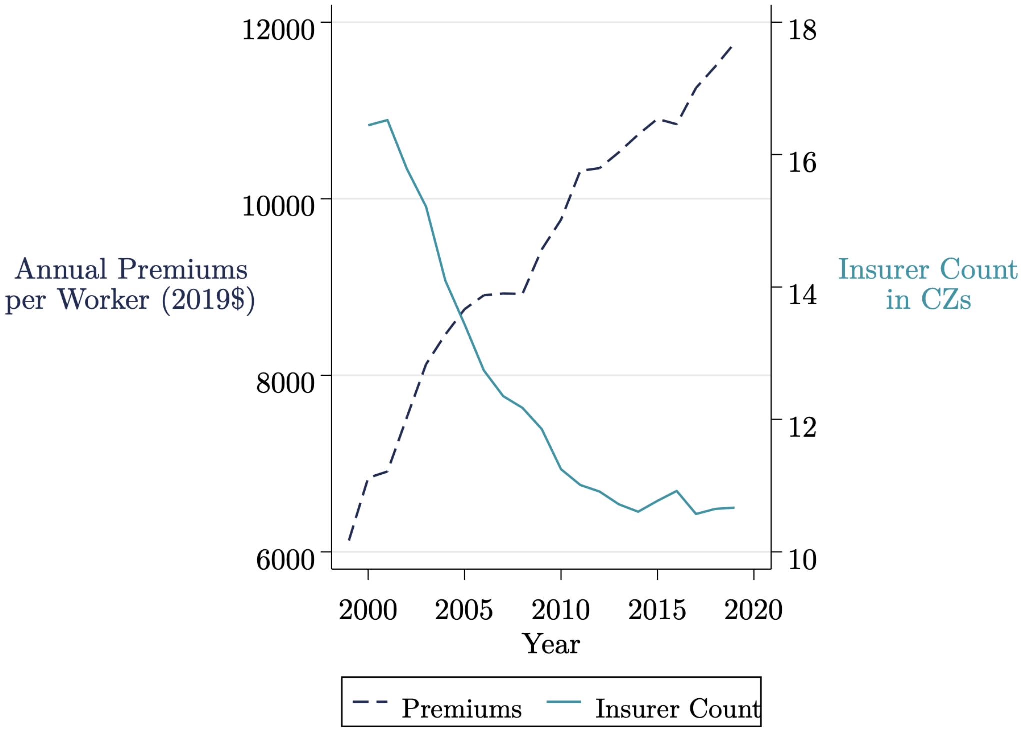
This paper highlights the role of insurer mergers as a key contributor to rising employer-sponsored premiums in the U.S. which, in turn, have resulted in employment losses for noncollege-educated middle-income workers. First, exploiting quasi-experimental variation in the timing of insurer mergers, I document large increases in premiums for vulnerable firms that purchase plans from insurers. Second, I show that vulnerable firms facing higher premiums experience employment losses of 4.4 percent, primarily concentrated among noncollege-educated middle-income workers. Third, combined with a model, I estimate that insurer mergers can explain up to 22 percent of the overall premium increase and 10 percent of the U.S.-specific decline in employment among noncollege-educated workers over the last two decades.

A surprising takeaway of my findings is that the market-wide impacts of mergers between large health insurers on labor outcomes are larger in magnitude and affect more local markets than mergers and acquisitions involving firms in other industries (Arnold, 2019). The finding suggests that it is crucial to consider potential harms to firms and workers when reviewing and deciding whether to challenge mergers within the insurance and healthcare sectors, a factor that has been overlooked by antitrust authorities and policymakers. Moreover, my results show that blocking insurer mergers that would have caused the largest increases in HHI can significantly mitigate the premium and labor market effects of insurer mergers. I argue that focusing on the predicted change in the post-merger HHI rather than the level is a more useful indicator to identify whether mergers substantially reduce competition (as discussed in Nocke and Whinston (2022)).

My findings also raise several new questions. First, the healthcare market has increasingly trended toward both vertical and horizontal integration along new dimensions. For example, the recent CVS-Aetna merger combined one of the largest health insurance companies with a major player in the retail pharmacy market. What are the consequences of these types of consolidation in healthcare markets? Second, while I emphasize the significance of health insurance mergers as a key driver of rising employer-sponsored premiums, other factors—such as new medical technologies and drugs—contributing to premium increases may have different implications for labor market outcomes. Finally, future research should investigate alternative potential adjustments in response to rising premiums, such as whether firms invest in more capital as a substitute for labor or outsource low-skill jobs to avoid paying health insurance costs.

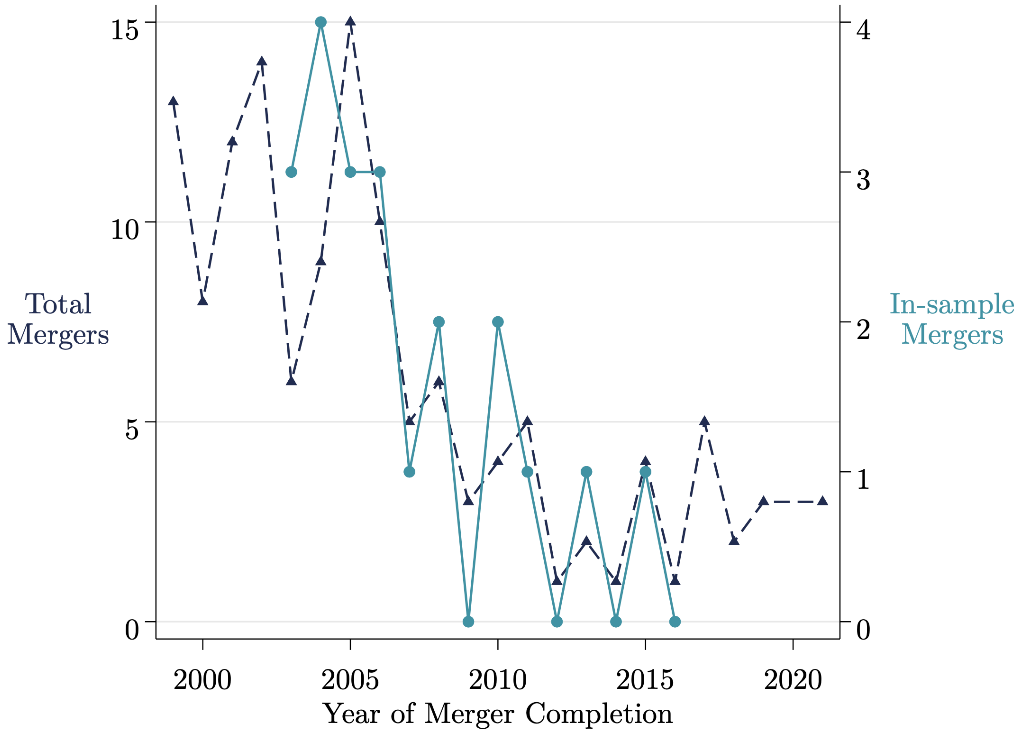
9. Figures & Tables

FIGURE 1: Average Premiums and Insurer Counts in Commuting Zones (CZs), 1999–2020



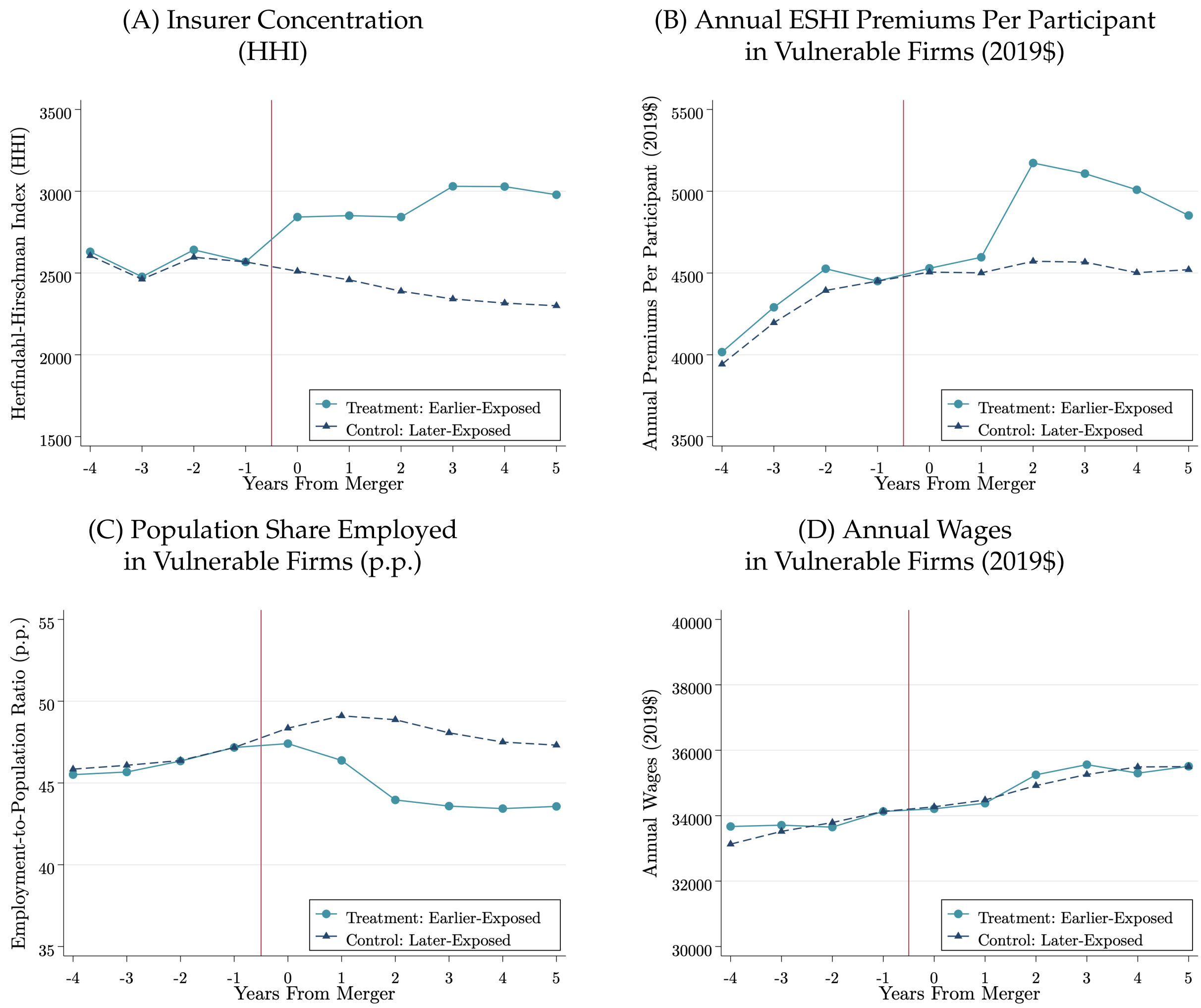
Notes: This figure shows trends in average annual premiums per worker at the national level and population-weighted insurer counts at the CZ level between 1999 and 2020. The dotted, dark blue line shows that premiums are increasing (left y-axis), and the solid, light blue line shows insurer counts are decreasing (right y-axis). Nominal dollars are reported in 2019 dollars throughout. Weights are the CZ working-age population aged 18 to 64. F55 N=683 CZs.
Source: Medical Expenditure Panel Survey (MEPS), Form 5500 (F55).

FIGURE 2: Insurer Merger Counts



Notes: This figure shows the number of insurer mergers by year of merger completion. The dotted, dark blue line shows the total mergers (left y-axis); the solid, light blue line shows the in-sample mergers of interest (right y-axis). Mergers are excluded from the sample if they involve insurers in non-overlapping markets, or if they occur in markets that already experienced a merger during the sample period between 1999 and 2020. Mergers before 2003 and after 2016 are not included in the sample.
Sources: Securities Data Company M&A Database, National Association of Insurance Commissioners Listing of Companies.

FIGURE 3: CZ-Level Raw Trends by Exposure to Insurer Mergers



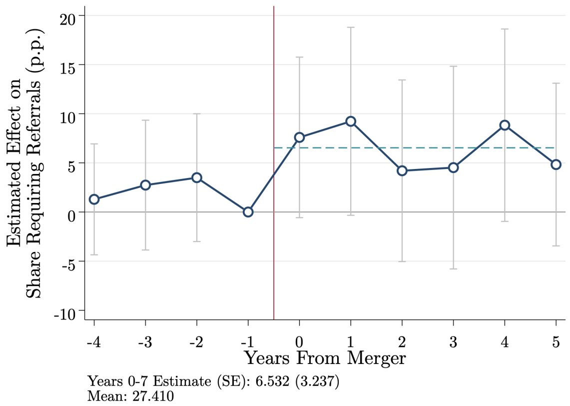
Notes: This figure plots CZ-level trends by exposure to insurer mergers. I show mean population-weighted trends for insurer concentration measured by the Herfindahl-Hirschman Index (HHI) in Panel A, annual ESHI premiums per participant in Panel B, employment in exposed single-establishment firms as a share of total population in Panel C, and annual wages in exposed single-establishment firms in Panel D. Outcomes for earlier-exposed treatment CZs are shown in solid light-blue lines; outcomes for later-exposed control CZs are shown in dashed dark-blue lines. Weights are the CZ working-age population aged 18 to 64 in the year before the merger. Outcomes for later-exposed CZs are normalized to equal the mean outcome for earlier-exposed CZs in the year before the merger. Nominal dollars are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treatment CZs and 39 control CZs in Panels A and B; LEHD N=40 treatment CZs and 20 control CZs in Panels C and D.
Sources: Form 5500, Longitudinal Employer-Household Dynamics (Panels C and D only).

FIGURE 4: CZ-Level Effect of Insurer Mergers on Premiums in Vulnerable Firms

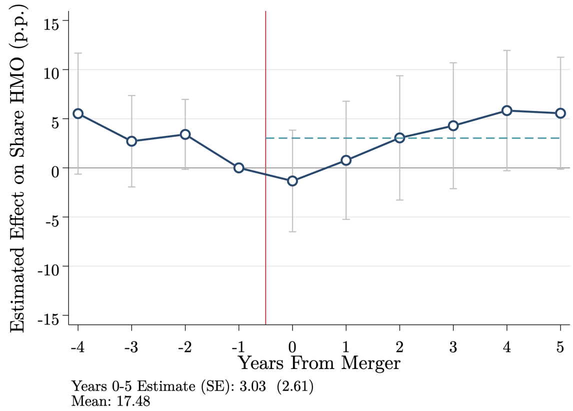
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is annual ESHI premiums per participant in vulnerable fully-insured firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs.
Source: Form 5500.

FIGURE 5: CZ-Level Effect of Insurer Mergers on Quality of Plans Offered by Vulnerable Firms

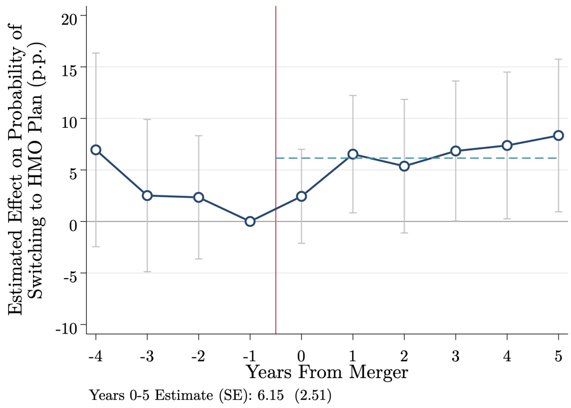
(A) Share Plans Requiring Referrals (p.p.)



(B) Share HMO Plans (p.p.)

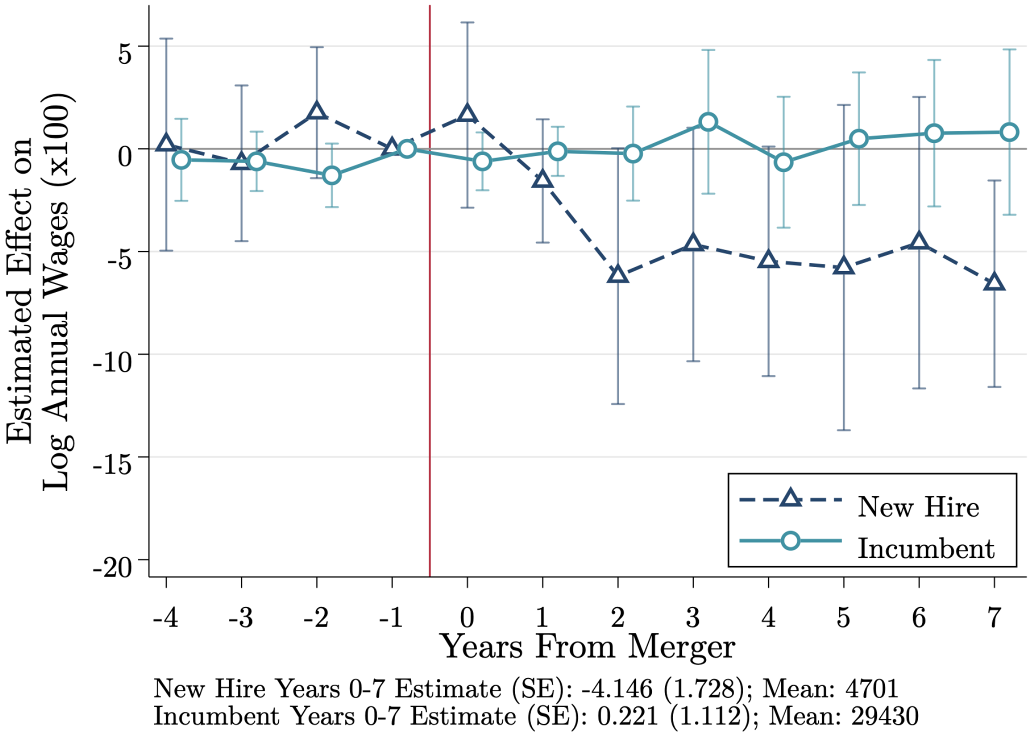


(B) Probability of Switching to HMO Plan (p.p.)



Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the share of plans requiring referrals to see specialist care (panel A), share of health maintenance organization (HMO) plans with more restrictive coverage (B), and the probability of non-HMO plans in $t-1$ switching to a HMO plan (c). The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs, 39 control CZs.
Source: Form 5500, Medical Expenditure Panel Survey Insurance/Employer Component (MEPS-IC, Panel A).

FIGURE 6: CZ-Level Effect of Insurer Mergers on Log Annual Wages in Vulnerable Firms

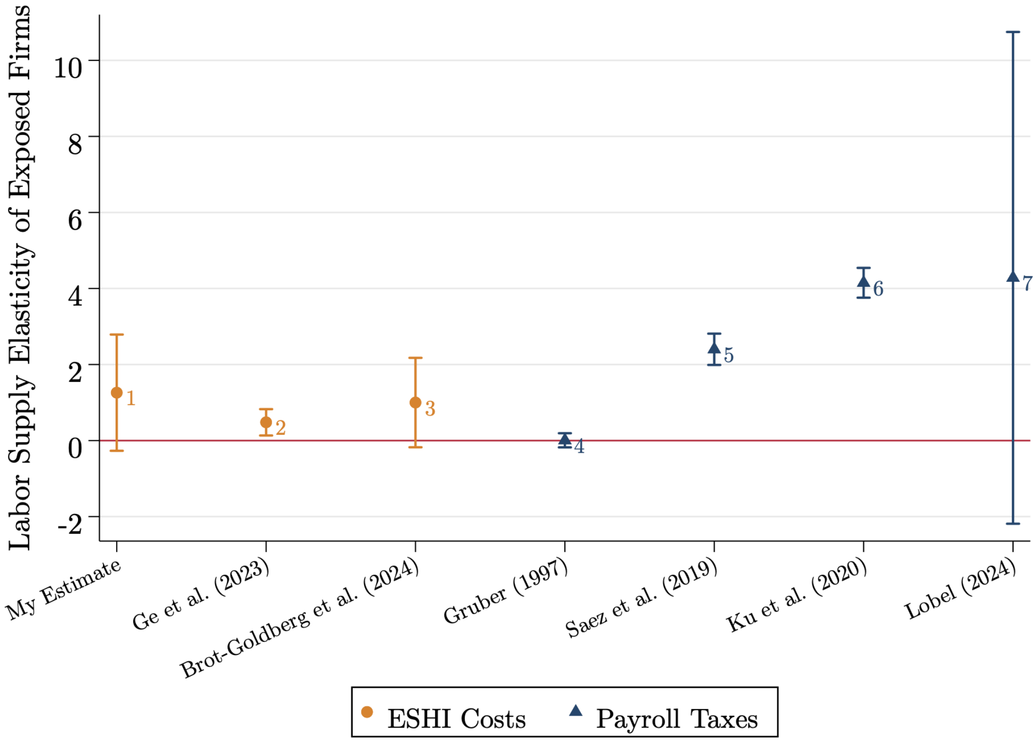


Notes: This figure plots the yearly group-specific coefficients β_{lg} from equation (2), where the outcome y_{ctg} is log annual earnings in vulnerable single-establishment firms, and groups g are defined as new hires and incumbent workers. Observations are weighted by CZ working-age population in the year before the merger. Coefficients, standard errors, and confidence intervals are multiplied by 100 for ease of interpretation. The group-specific point estimate for the average of coefficients from years 0 to 7 following the merger (with corresponding standard error) is reported in the lower left hand corner along with the group-specific weighted exponentiated mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. LEHD N=40 treated CZs and 20 control CZs.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics.

FIGURE 7: CZ-Level Effect of Insurer Mergers on Working-Age Population Share Employed in Vulnerable Firms

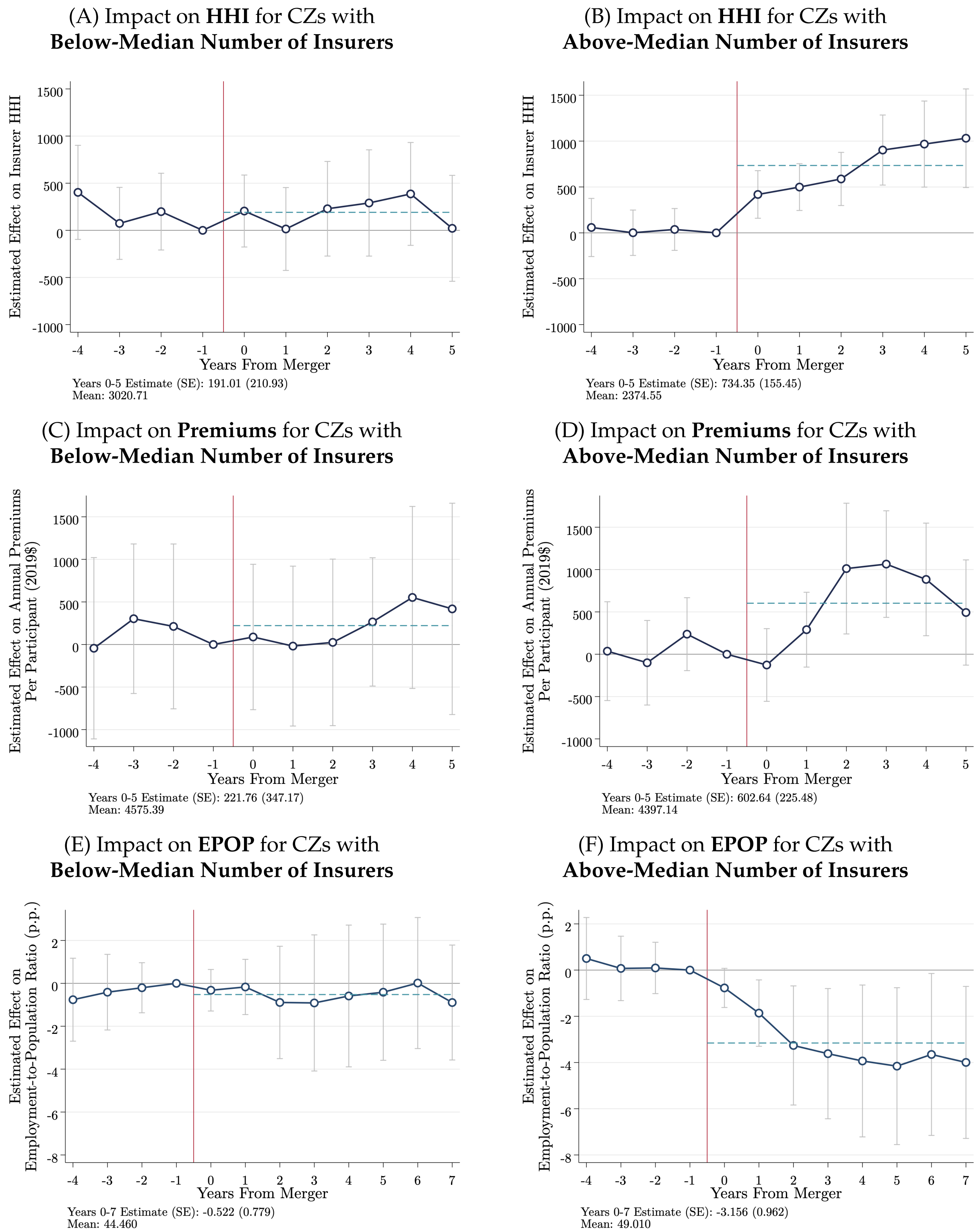
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in vulnerable single-establishment firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 7 following the merger ($\frac{1}{8}\sum_{l=0}^{7}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. LEHD N=40 treated CZs and 20 control CZs.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics.

FIGURE 8: Benchmarking the Implied Labor Supply Elasticity of **Vulnerable Firms**



Notes: This figure compares my estimated labor supply elasticity of vulnerable firms to elasticities from existing studies of rising ESHI costs and payroll taxes. Elasticities are defined as the log change in the employment rate divided by the log change in the net-of-average-tax-and-ESHI wages (corresponding numbers in Appendix Table A.6). Vertical lines indicate 95% confidence intervals on each estimate.
Sources: See Appendix Table A.6 for details of the elasticity estimates.

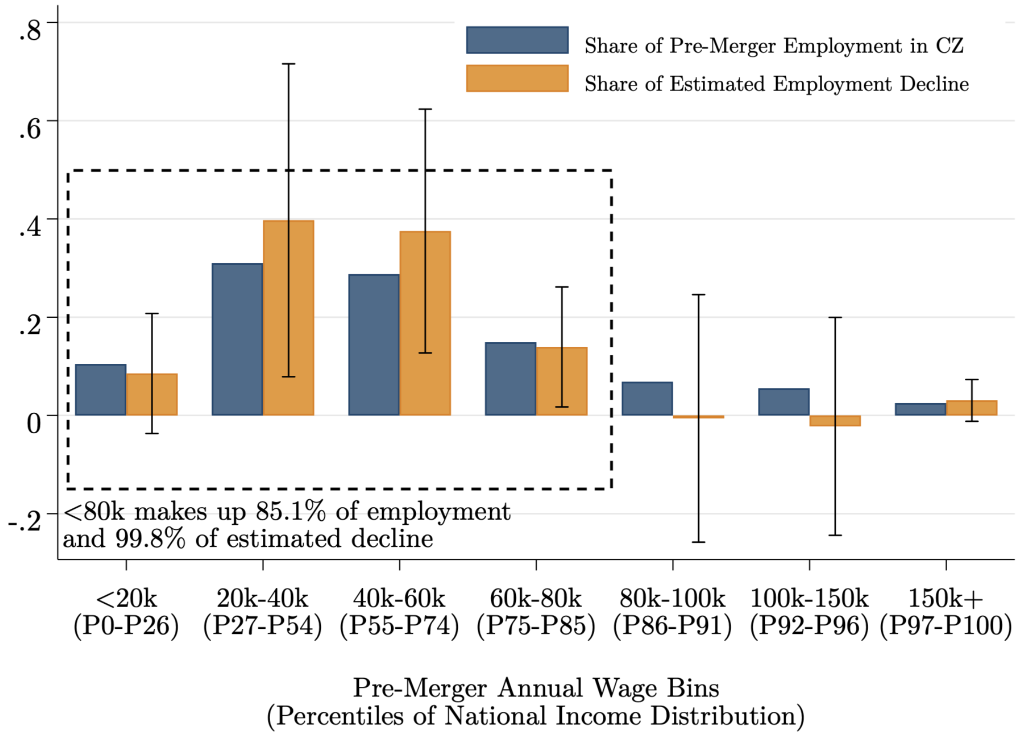
FIGURE 9: CZ-Level Effect of Insurer Mergers by Pre-Merger CZ Number of Insurers



Notes: This figure plots the yearly coefficients β_l from equation (1). Panels A, C, and E display the estimates for earlier-exposed CZs with a below-median number of insurers relative to all later-exposed CZs. Panels B, D, and F display the estimates for earlier-exposed CZs with an above-median number of insurers relative to all later-exposed CZs. The outcome y_{ct} is insurer HHI in Panels A and B, annual premiums per participant in Panels C and D, population share in vulnerable single-establishment firms in Panels E and F. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of post-merger coefficients following the merger. The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs in Panels A and B; LEHD N=40 treated CZs and 20 control CZs in Panels C and D.

Sources: Form 5500, and Longitudinal Employer-Household Dynamics (Panels C and D only).

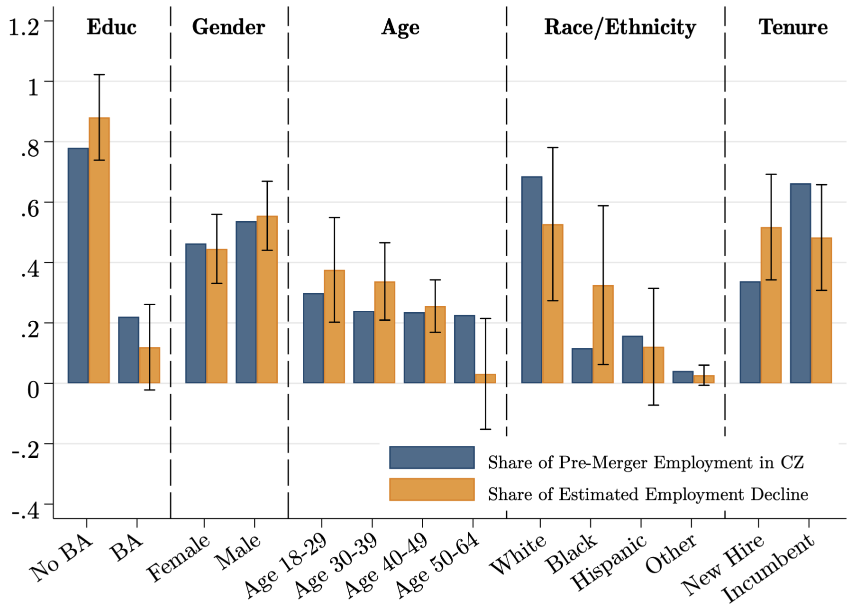
FIGURE 10: Decomposition of Employment Losses, by Pre-Merger Annual Wages



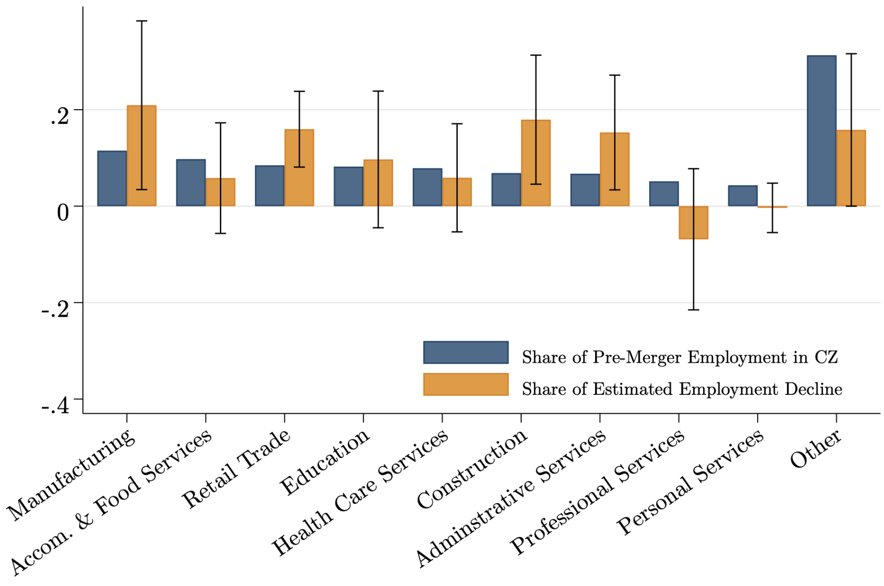
Notes: Figure 10 decomposes the overall estimated employment loss from vulnerable firms (in Figure 7) by pre-merger annual wage bins. For each wage bin, the corresponding percentile range of the national income distribution over the sample period is reported below each x-axis label in parentheses. The blue bars indicate each wage bin's share of CZ employment in the year before the insurer merger. The orange bars represent the implied share of the employment decline accounted for by the wage bin. To construct these, I divided the estimated decline in employment (in levels) for a wage bin over years 0 to 7 post-merger by the total employment decline (in levels). The sample is at the CZ-level from 1999 to 2020, and follows a balanced panel of individuals working in the year before the merger. 95% confidence intervals, clustered at the CZ level, are shown as vertical lines. LEHD N=40 treated CZs and 20 control CZs LEHD.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics.

FIGURE 11: Decomposition of Employment Decline in Vulnerable Firms, by Worker Characteristics

(A) By Worker Education, Gender, Age, Race/Ethnicity, and Tenure

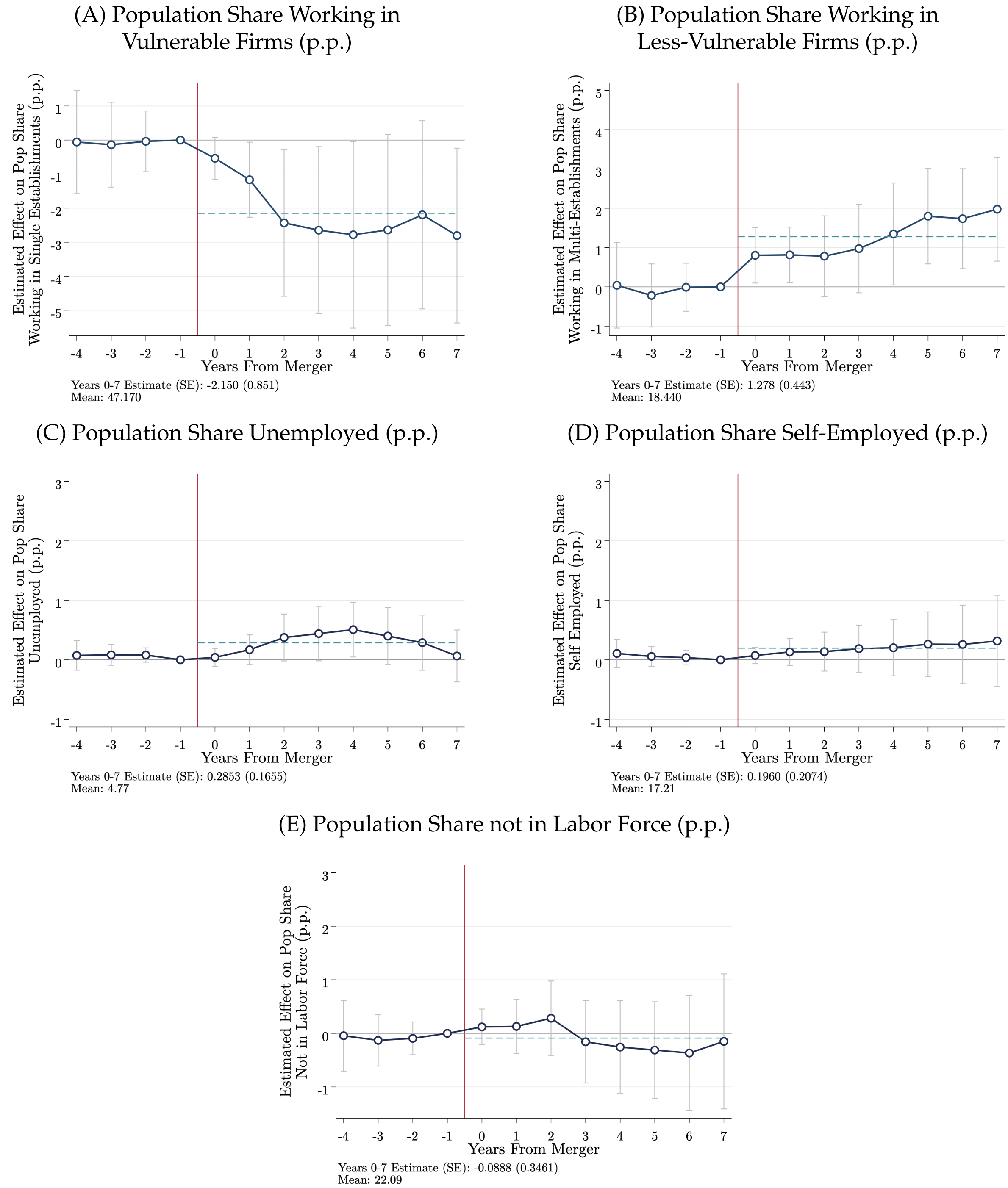


(B) By Worker Industry



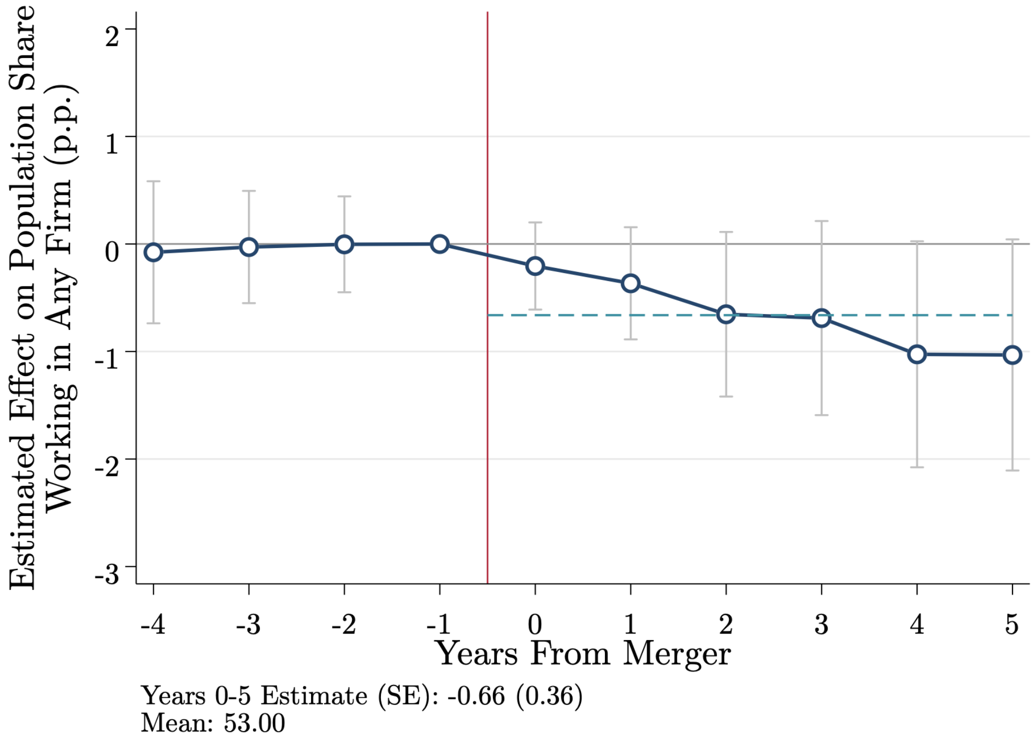
Notes: Figure 11 decomposes the contribution of each sub-group by category to the overall estimated employment loss from vulnerable firms (in Figure 7). Panel A divides workers by education (with or without college degrees), by gender, by age bins, by race/ethnicity, or by worker tenure (new hires or incumbents). For each category, sub-groups are mutually exclusive and collectively exhaustive. Panel B divides workers by 9 of the largest industries in CZs (presented in order of employment share), and the final category is a residual category which captures all other workers. The blue bars indicate each sub-group's share of employment in the year before the insurer merger. The orange bars represent the implied share of the employment decline accounted for by the sub-group. To construct these, I divided the estimated decline in employment (in levels) for a sub-group over years 0 to 7 post-merger by the total employment decline (in levels). 95% confidence intervals, clustered at the CZ level, are shown as vertical lines. LEHD N=40 treated CZs and 20 control CZs LEHD.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics.

FIGURE 12: CZ-Level Effect of Insurer Mergers on Employment Status of Working-Age Population



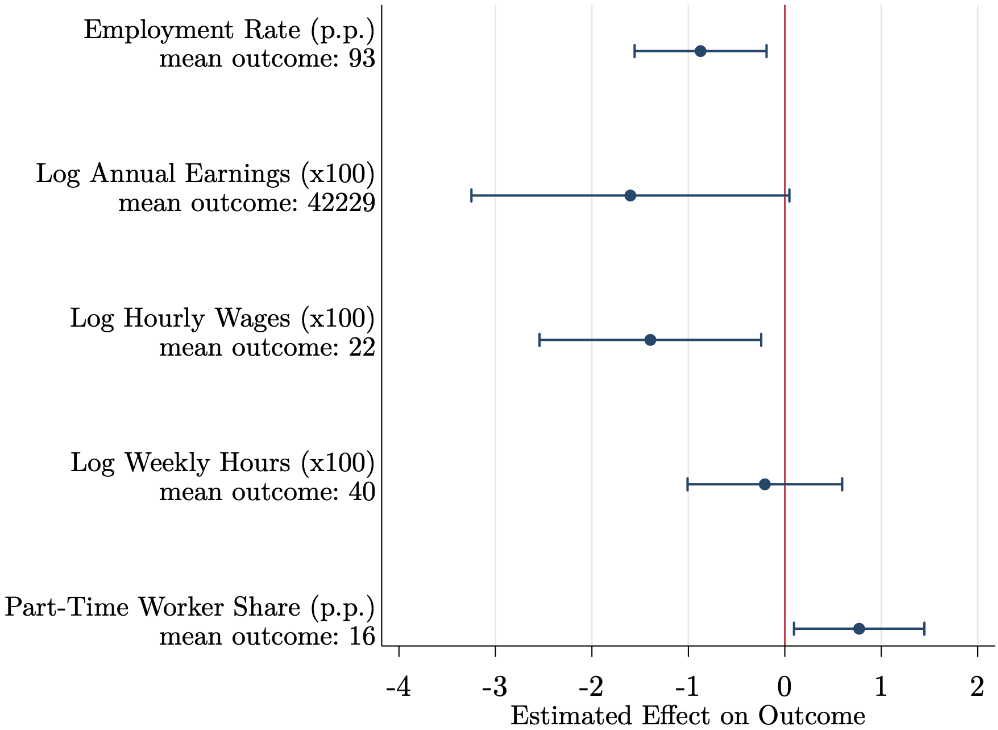
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in vulnerable single-establishment firms (A, replicates Figure 7), employed in less-vulnerable multi-establishment firms (B), unemployed (C), self-employed (D), and not in the labor force (E). The five panels are mutually exclusive but not collectively exhaustive – employment that is not covered by state unemployment insurance is not captured. Outcomes are from different datasets so shares do not add up to 1. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 7 following the merger ($\frac{1}{8}\sum_{l=0}^{7}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. LEHD N=40 treated CZs and 20 control CZs in Panels A and B; N=77 treated CZs, 39 control CZs in Panels C, D, and E.
Source: Form 5500, Longitudinal Employer-Household Dynamics (Panels A and B), Bureau of Labor Statistic Local Area Unemployment Statistics (Panels C and E), U.S. Bureau of Economic Analysis (Panel D).

FIGURE 13: CZ-Level Effect of Insurer Mergers on Population Share Working in Any Firm, Using County Business Patterns Data



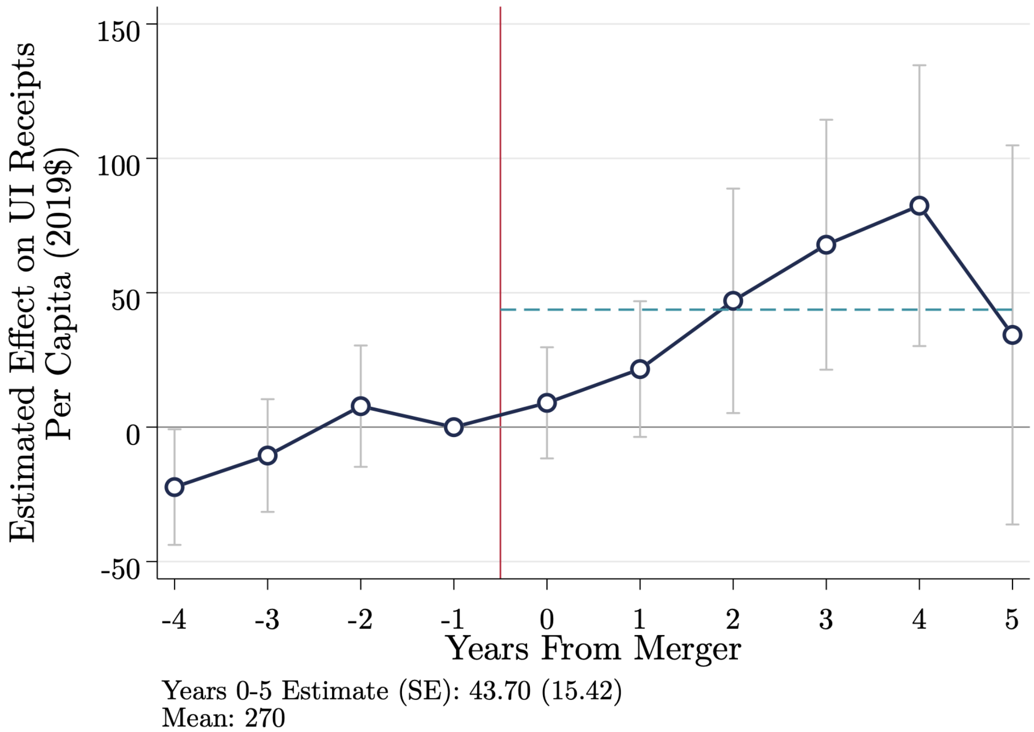
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in all firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs.
Source: Form 5500, and County Business Patterns (CBP).

FIGURE 14: CZ-Level Effect of Insurer Mergers on Aggregate Labor Market Outcomes, Using American Community Survey Data



Notes: This figure plots the coefficients β from a pooled version of equation (1). The outcome y_{ct} includes the employment rate (share of labor force working in any firm), log annual earnings, log hourly wages, log weekly hours, and the part-time worker share. Observations are weighted by CZ working-age population in the year before the merger. The coefficients, standard errors, and confidence intervals of log outcomes are multiplied by 100 for ease of interpretation. The weighted mean outcome for treated CZs in the year before the merger is reported underneath the label of each outcome. I report exponentiated weighted means for log outcomes. Standard errors are clustered at the CZ level, and horizontal blue lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 2005 to 2020. N=43 treated CZs and 32 control CZs.
Source: Form 5500, and American Community Survey (ACS).

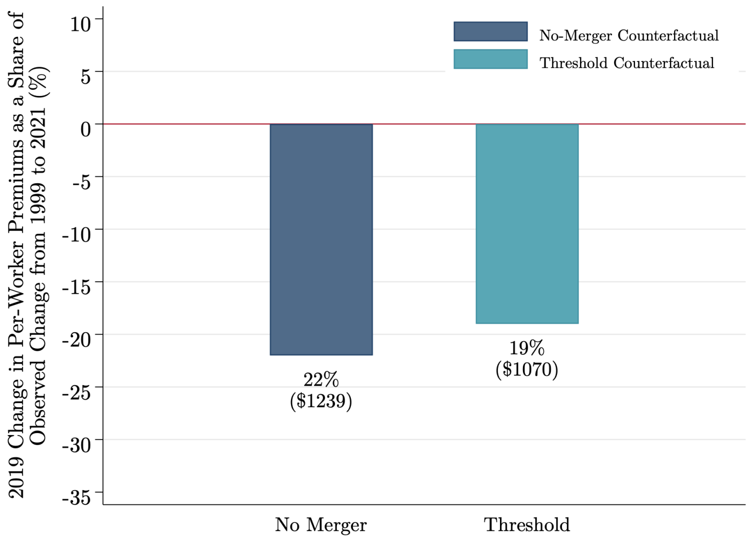
FIGURE 15: CZ-Level Effect of Insurer Mergers on Unemployment Insurance Receipts Per Capita



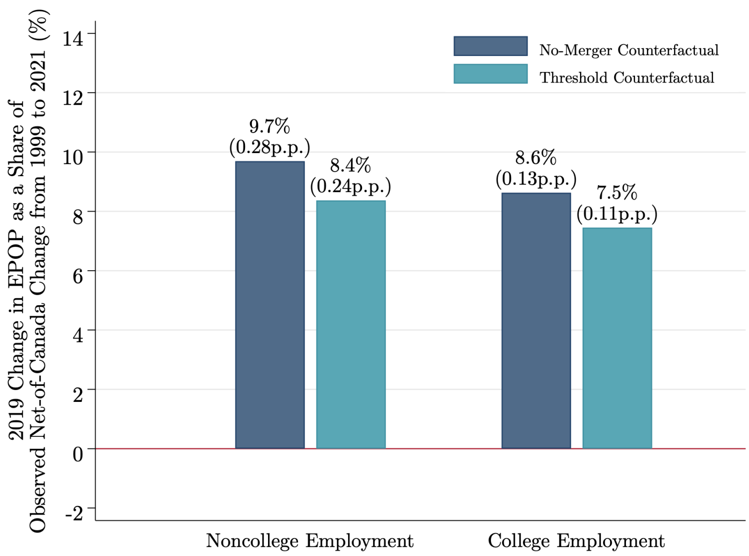
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is unemployment insurance per working-age capita. Unemployment insurance includes state unemployment insurance, and federal unemployment insurance for Federal civilian employees, railroad employees, and veterans. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{8}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Benefits are converted to 2019 dollars. N=81 treated CZs, 39 control CZs.
Sources: Form 5500, U.S. Bureau of Economic Analysis.

FIGURE 16: 2019 Labor Market Effects of ESHI Premiums Growth Counterfactuals

(A) 2019 Change in Annual Premiums Per Worker (%)



(B) 2019 Change in Labor Market Outcomes (%)



Notes: This figure plots the 2019 percent change in premiums and labor market outcomes under two counterfactuals. Percent changes are equal to the 2019 change as a share of the observed change from 1999 to 2021. When calculating the observed net-of-Canada change, I subtract the observed change in Canada from the observed change in the U.S. from 1999 to 2021. Corresponding level changes in 2019 are reported next to each bar. The dark blue bars are the counterfactual that antitrust authorities block all insurer mergers, and the light blue bars are the more realistic counterfactual that antitrust authorities block mergers that would have caused a post-merger Herfindahl-Hirschman Index (HHI) increase of at least 200. Nominal dollars are converted to 2019 dollars. I assume $\rho = 0.38$, $\alpha = 0$, and labor supply elasticities are equal to 0.32.
Source: Author's own calculations.

TABLE 1: CZ-Level Summary Statistics by Merger Exposure

	Treatment: Earlier-Exposed CZs (1)	Control: Later-Exposed CZs (2)
Panel A: Main Panel		
Number of Insurers	12.89	10.41
	(6.72)	(4.54)
Insurer HHI	2,804.89	2,691.01
	(1,529.81)	(1,255.21)
Per-Participant Annual Premiums	4,651.25	4,767.59
	(2,092.72)	(2,147.67)
Population	480,710.69	367,395.51
	(242,309.97)	(163,403.27)
Median Household Income	53,673.78	54,063.60
	(8,181.72)	(7,485.07)
Unemployment Rate	6.83	6.04
	(2.68)	(2.17)
Share Rural	0.51	0.53
	(0.19)	(0.15)
Share West	0.20	0.16
	(0.40)	(0.36)
Share Midwest	0.21	0.26
	(0.41)	(0.44)
Share Northeast	0.03	0.07
	(0.16)	(0.25)
Share South	0.56	0.52
	(0.50)	(0.50)
Observations	820	1,900
Unique CZs	81	39
Panel B: LEHD Panel		
Population	276,000	200,000
	(158,000)	(75,000)
Employment in Exposed Firms	125,700	67,930
	(79,400)	(32,300)
Annual Wages in Exposed Firms	34,790	34,870
	(3,243)	(3,688)
Observations	400	850
Unique CZs	40	20

Notes: This table displays summary statistics for earlier-exposed treatment CZs (column 1) and later-exposed control CZs (column 2). Panel A reports statistics for the main analysis panel that includes 81 unique first-exposed CZs and 39 unique last-exposed CZs. Panel B reports statistics for the LEHD panel, only available for 25 states. Statistics in Panel B are rounded following the U.S. Census Bureau's rounding rules. Nominal dollars are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2021.
Sources: Form 5500, 2000 Census, SEER Population Data, BLS LAUS, and LEHD.

TABLE 2: Sensitivity Analysis of Premiums Effect

	Years 0-5 Estimate (2019$) (1)
Baseline	434.13**
	(201.74)
Panel A: Controls	
Add China Shock × Year Effects	439.78*
	(234.76)
Add 2006–2009 Change in Net Housing Wealth	424.08**
× Year Effects	(203.57)
Add Census-Division × Year Effects	857.98***
	(203.14)
Add Lagged Hospital HHI	535.86***
	(206.73)
Add Lagged Share of Firms Self-Insured	433.52**
	(200.70)
Add Lagged Population	449.01**
	(188.90)
Panel B: Functional Form	
Premiums in Logs	0.13***
	(0.05)
Implied Dollar Change in Premiums	578.50
No Population Weights	253.35
	(230.61)
Panel C: Geography	
Core-Based Statistical Area (CBSA)	-75.83
	(241.13)
Hospital Referral Region (HRR)	444.13*
	(235.38)

Notes: This table reports the average of coefficients from years 0 to 5 following the merger estimated from one-off deviations of equation (1). The outcome y_{ct} is annual ESHI premiums per participant in 2019 dollars. The first row replicates the baseline estimate from Figure 4. In the first row of panel A, I control for Chinese import competition by adding an interaction between the CZ increase in Chinese imports between 1990 to 2000 and year indicators. In the second row of panel A, I control for local Great Recession shocks by adding an interaction between the 2006-2009 CZ change in net housing wealth and year indicators. In panel B, the implied dollar change in premiums is calculated by multiplying the estimated coefficient with the mean population-weighted premiums in treated CZs the year before the merger. In panel C, I estimate equation (1) at the CBSA and HRR level, weighting by population and clustering standard errors at the same level. All estimates unless otherwise noted are weighted by CZ working-age population in the year before the merger, with standard errors clustered at the CZ level. N=81 treated and 39 control CZs for CZ-level analysis; N= 76 treated and 31 control CBSAs for CBSA-level analysis; N= 40 treated and 17 control HRRs for HRR-level analysis.
Sources: Form 5500, Autor et al. (2013a), Mian and Sufi (2014), American Hospital Association (AHA) Annual Surveys of Hospitals, National Cancer Institute SEER Program.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3: CZ-Level Effect of Insurer Mergers on Non-Wage Compensation in Vulnerable Firms

	Panel A. Employer-Sponsored Health Insurance (ESHI) Benefits					
	No. of Health Plans (1)	Share of Premiums Paid by Firm (%) (2)	Share of Firms Offer ESHI (%) (3)	Share of Workers Eligible (%) (4)	Share of Workers Part-Time (%) (5)	Share of Workers Enrolled (%) (6)
Years 0-5 Estimate	0.74 (1.94)	2.06 (2.46)	-0.75 (2.55)	-2.90* (1.56)	0.77** (0.34)	-2.61 (1.87)
Outcome Mean	49.83	59.74	44.13	81.31	15.89	64.44
Observations	2,650	2,400	2,600	2,400	784	2,400
R^2	0.95	0.19	0.29	0.20	0.88	0.20
Data Source	F55	MEPS-IC	MEPS-IC	MEPS-IC	ACS	MEPS-IC
	Panel B. Other Non-Wage Compensation					
	No. of Pension Plans (7)	Pension Contrib. Per Worker Paid by Firm ($) (8)	No. of Dental & Vision Plans (9)	Dental & Vision Premiums Per Particpt. ($) (10)	No. of Life, Sickness or Acc. Plans (11)	Life, Sickness or Acc. Premiums Per Particpt. ($) (12)
Years 0-5 Estimate	-3.91*** (1.38)	-166.52* (97.21)	5.51** (2.45)	-5.01 (173.28)	-1.07 (0.90)	-6.77 (22.88)
Outcome Mean	70.04	2,262.72	43.12	1,296.33	29.40	251.08
Observations	2,650	2,650	2,650	2,650	2,650	2,650
R^2	0.96	0.60	0.97	0.54	0.95	0.34
Data Source	F55	F55	F55	F55	F55	F55

Notes: This table reports the average of β_l coefficients following the merger estimated from equation (1) for employer-sponsored health insurance benefit outcomes (panel A) and other non-wage compensation outcomes (panel B). The coefficient reported in column (5) using the ACS is from a pooled version of equation (1). Estimates are weighted by CZ working-age population in the year before the merger, with standard errors in parentheses are clustered at the CZ level. Nominal dollars are converted to 2019 dollars. The F55 sample consisting of firms offering fully-insured plans is at the CZ-level from 1999 to 2021. The MEPS-IC sample consisting of single-establishment firms is at the CZ-level from 1999 to 2021. The ACS sample consisting of all firms is at the CZ-level from 2005 to 2021. MEPS-IC and F55 N=81 treated and 39 control CZs (fewer CZs in columns 2 to 5 due to variable non-response in MEPS-IC); ACS N=43 treated and 32 control CZs.

Sources: Form 5500, Medical Expenditure Panel Survey Insurer/Employer Component (MEPS-IC), and American Community Survey (ACS).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 4: CZ-Level Effect of Insurer Mergers on Firms Linked to the Form 5500

	Panel A. Employment as a Share of Working-Age Population (p.p.)			Panel B. Log Employment (x100)
	(1)	(2)	(3)	(4)
Years 0-7 Estimate	-2.15**	-0.28	0.013	7.66**
	(0.85)	(0.23)	(0.022)	(3.47)
Implied Percent Change	-4.56	-3.33	5.36	
Sample	Baseline (LEHD)	LEHD-F5500	LEHD-F5500	LBD-F5500
Sample Restriction		Fully-Insured	Self-Insured	Fully-Insured
Outcome Mean	47.2	6.19	0.24	5617
Observations	1300	1300	650	3100
R^2	0.98	0.92	0.89	0.93

Notes: This table reports the average of β_l coefficients from years 0 to 7 following the merger estimated from equation (1). The outcome y_{ct} is working-age population share working in vulnerable single-establishment firms in Panel A, and log employment in vulnerable single-establishment firms in Panel B. Column (1) replicates the estimate from Figure 7, columns (2) and (4) restrict to Form 5500 fully-insured firms in the LEHD and LBD, and column (3) restricts to Form 5500 self-insured firms in the LEHD. Coefficients and standard errors for log outcomes are multiplied by 100 for ease of interpretation, and reported means are exponentiated means for log outcomes. The sample is at the CZ-level from 1999 to 2021. LEHD N=40 treated and 20 control CZs in Panel A. N=81 treated and 39 control CZs in Panel B.

Sources: Form 5500, Longitudinal Employer-Household Dynamics (LEHD), and Longitudinal Business Database (LBD).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 5: Sensitivity Analysis of Employment-to-Population Effect

	Years 0-7 Estimate (p.p.)
	(1)
Baseline	-2.15**
	(0.85)
Implied Percent Change	4.4
Panel A: Sample	
Keep Workers Earning < $3700	-2.73**
	(1.09)
Implied Percent Change	-4.56
Panel B: Controls	
Add 2006–2009 Change in Net Housing Wealth × Year Effects	-1.79**
	(0.87)
Add Pre-Merger Share Manufacturing	-2.38***
& Construction × Year Effects	(0.89)
Add Pre-Merger Share Retail Trade × Year Effects	-1.97***
	(0.82)
Add China Shock × Year Effects	-2.30***
	(0.88)
Add Census Division × Year Effects	-1.74***
	(0.67)
Panel C: Functional Form	
Employment in Logs (x100)	-3.85**
	(1.93)
Implied Percentage Point Change	1.88
No Population Weights	-1.48**
	(0.736)
Job-Adjusted EPOP	-1.31**
	(0.60)
Implied Percent Change	-3.75

Notes: This table reports the average of coefficients from years 0 to 7 following the merger estimated from one-off deviations of equation (1). The outcome y_{ct} is population share employed in exposed single-establishment firms (employment-to-population ratio). The first row replicates the baseline estimate from Figure 7. In the first row of panel A, I control for local Great Recession shocks by adding an interaction between the 2006-2009 CZ change in net housing wealth and year indicators.In the fourth row of panel A, I control for Chinese import competition by adding an interaction between the CZ increase in Chinese imports between 1990 to 2000 and year indicators. In panel C, the implied percentage point change is calculated by multiplying the estimated coefficient with the mean population in treated CZs the year before the merger, while the implied percent change is calculated by dividing the estimated coefficient by the mean population in treated CZs the year before the merger. All estimates unless otherwise noted are weighted by CZ working-age population in the year before the merger, with standard errors clustered at the CZ level. N= 40 treated and 20 control CZs (LEHD data is only available for 25 states, number of unique CZs is rounded).
Sources: Form 5500, Longitudinal Employer-Household Dynamics, American Community Survey, Autor et al. (2013a), and Mian and Sufi (2014).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 6: 2019 Labor Market Effects of Premiums Counterfactuals

	(1) No-merger counterfactual	(2) Threshold counterfactual	(3) Canada counterfactual	(4) No-growth counterfactual
Change in per-worker premiums (2019$)	-$1240	-$1071	-$1972	-$5636
Percent of observed change, 1999-2021	22%	19%	35%	100%
Change in noncollege employment to population	0.28p.p.	0.24p.p.	0.45p.p.	1.29p.p.
Percent of observed net-of-Canada change, 1999-2021	9.69%	8.37%	15.42%	44.10%
Change in college employment to population	0.13p.p.	0.11p.p.	0.21p.p.	0.59p.p.
Percent of observed net-of-Canada change, 1999-2021	8.63%	7.45%	13.72%	39.18%

Notes: Changes are calculated as the 2019 change in the value when moving from the counterfactual to observed premiums growth path. I calculate the change in premiums as a percent of the observed change from 1999 to 2021, and the change in employment as a percent of the observed net-of-Canada change from 1999 to 2021. The observed net-of-Canada change is specific to the U.S., and calculated as the U.S. change minus the Canadian change from 1999 to 2021.
Sources: Author's own calculations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Appendices

A. Data

This Section provides additional information on the underlying data sources and construction of variables used in my analysis.

Form 5500. The Form 5500 collects information on ESHI plans and is jointly administered by the Department of Labor and Internal Revenue Service. Employers are required to file a Form annually, however employers in the public sector or employers offering plans with less than 100 participants are exempt from filing.

I primarily rely on the Form 5500 Schedule As to create my analysis panel. I restrict to plans that are listed as providing one of the following benefits: health (other than dental or vision), HMO contract, PPO contract, or indemnity contract. Plan sponsors that provide multiple types of benefits may report health benefits together with certain other benefits, such as disability or dental insurance, on a single Form 5500. I drop plans that lists providing other non-health related benefits. I drop employers who file more than 15 times in the same year, or provide benefits under multiemployer (or multiple-employer) plans. I link Schedule As to the main form to obtain in the physical zip code of employers and use a HUD-USPS crosswalk to match employers' zip codes to counties and a crosswalk to match counties to CZs (Autor and Dorn, 2013). I merge the Form 5500 with the NAIC Listing of companies by using the insurer's employment identification number (EIN). For observations with incorrect or missing insurer EINs in the Form 5500, I use a combination of fuzzy string matching from reported insurer names and manual matching to identify the parent company. I use an unbalanced employer panel from 2002 onwards to calculate CZ-level measures of health insurer market shares and concentration. (I omit pre-2001 years as employer filing is significantly lower during this period.) I use a balanced employer panel from 1999 to 2020 to obtain CZ-level measures of premiums, which avoids potential bias due to changes in the composition of Form-5500-filing employers.

I identify 683 CZs after cleaning the Form 5500. Of the 683 CZs, I identify 437 CZs that are never exposed to insurer mergers, and 246 CZs that are exposed at least once by an insurer merger, i.e., both merging insurers have market shares in the CZ before the merger. Exposed CZs experience an average of 3.8 mergers over the sample period. My final sample consists of 81 earlier-exposed commuting zones. I reach the final sample of earlier-exposed CZs through the following steps: begin with 246 CZs that are treated at least once, restrict to those treated once by a merger and not again in the six years following the merger (111 CZs), including the year of the merger, focus on CZs treated from 2003 onwards and require a balanced CZ panel over observation window (81 CZs). I compare the 81 earlier-exposed CZs to 39 later-exposed CZs that experience a first-observed merger seven or more years after the merger year of earlier-exposed CZs. I later expand the earlier-exposed sample to include successively exposed CZs in Section 9 to examine the generalizability of my baseline results.

To calculate CZ-level annual premiums, I take the average of premiums across all plans irrespective of the plan's insurance carrier because an increase in market power may impact the premiums of all insurers in an impacted market. I do not observe information breaking down premiums across by individual plans versus family plans, or by employer versus employee contributions. To assess the response of rival insurers, I take the average of per-participant premiums and claims across plans of insurance carriers not involved in insurer mergers. I investigate whether self-insured employers are exposed to insurer mergers in section **??**. I calculate per-participant premiums and claims (total cost of medical services paid on behalf of participants) for a plan and average across all plans in a CZ. I obtain the share of premiums paid by employers by dividing employer contributions by total premiums.[23]

LEHD. The LEHD is a quarterly matched employer-employee dataset. The LEHD only covers workers in the state UI system; excluded workers include independent contractors, the self-employed, federal government workers, railroad workers, workers in non-profits with fewer than four workers. Detailed information on the coverage of state UI laws is available at: `https://oui.doleta.gov/unemploy/statelaws.asp`. Employers are defined in the LEHD at the state tax identifier level (SEIN). I use the LEHD Job History File (JHF) to construct my worker and employer LEHD panels. The employer SEIN can change often in the LEHD data, due to firm restructuring, and the JHF accounts for job spells which can be split across multiple SEINs by assigning PIK-SEIN pairs for the same employer to a unique identifier (FID).

I construct additional CZ-level firm employment and wage outcomes for robustness checks. To account for workers with multiple jobs, I construct a jobs-adjusted employment measure, where each worker is counted towards a firm according to the worker's share of total annual earnings accrued from the firm. For example, a worker earning $10,000 from firm A and

[23] For fully-insured plans, I do not observe employer and employee premiums separately and only observe claims for a small subset of fully-insured plans that are experience-rated (i.e., premiums are determined based on the employer's claims history).

$90,000 from firm B would be assigned a value of $\frac{1}{10}$ to firm A and $\frac{9}{10}$ to firm B. To account for changes in worker composition, I calculate a composition-adjusted earnings measure for a random 20 percent sample of my main worker sample. I obtain the residuals from regressing log annual earnings on age, age-squared, sex, gender, and race/ethnicity at the CZ-year level, which is the composition-adjusted measure. Note that earnings are top-coded at a threshold defined by each state which varies over time (Abowd et al., 2009). Despite missing information on establishment location, I extend my main analysis by examining employment in multi-establishment LEHD firms. I use information on imputed establishment location provided by the Census and restrict to multi-unit firms with establishments located in only one CZ.

I also construct balanced samples to account for potential biases arising from changes in firm or worker composition. The balanced firm panel consists of all single-establishment firms with 50 or more workers in the year before the insurer merger. The balanced worker panel includes all workers in firms with 50 or more workers and with four-quarters of positive earnings in the year before the insurer merger.

To examine whether individuals relocate following merger shocks, I calculate two different measures of migration using the LEHD. First, I examine whether individuals move to a different CZ post-merger. Using the LEHD residence data, I identify all individuals living in an affected CZ and follow individuals six years later. The LEHD residence data records the place of residence of all workers in the LEHD, even for years when workers do not have jobs, and is available for all U.S. states. Second, I examine individuals work in a different CZ post-merger. I identify all individuals working in only one affected CZ and follow individuals six years later. I classify an individual as have moved residence or work if they are not observed in the data six years later.

LBD Data. The LBD is constructed from the Census Bureau's Business Register, a listing of private businesses in the U.S. collected from surveying businesses and administrative tax records.[24] An establishment in the LBD is defined as the physical location where business occurs (Chow et al., 2021).

My primary firm sample consists of an unbalanced sample of firms. I later account for possible composition bias by constructing a balanced firm sample. Specifically, I follow single-establishment firms with positive employment in the year before the insurer merger to hold firm composition constant over time. I focus on single-establishment firms because they are

[24]See Chow et al. (2021) for further details on the LBD.

more likely to be exposed to insurer merger shocks than multi-establishment firms that are typically self-insured. Moreover, human resources divisions located in the headquarters of multi-establishment firms typically determine firm-wide health plans offerings. The lack of information on firms' headquarters poses a challenge to identifying whether multi-establishment firms are treated, which depends on whether the headquarters are in a CZ experiencing an insurer merger.

ACS Data. I use the American Community Survey (ACS) to supplement labor force variables missing from the LEHD and LBD. The ACS is an annual U.S. household survey collected by the U.S. Census Bureau. I use the ACS single-year files from 2005 to 2020 to study employment changes over time. The ACS surveys approximately 3 million randomly-drawn addresses annually. Information on household addresses is available from 2005 onwards at the Public Use Microdata Area (PUMA) level for PUMAs with a population of at least 100,000 people, which I match to CZs using a crosswalk (Autor and Dorn, 2013).

I restrict the sample to individuals aged 18 to 64 with paid employment in the past 12 months. I obtain annual income and hours worked weekly, and calculate hourly wage rates, dividing annual income by weekly hours worked multiplied by 48 weeks. Annual income is measured as the total pre-tax money received as an employee, and includes wages, salaries, commissions, bonuses, tips and other money income.

Medical Expenditure Panel Survey Insurance/Employer Component Data. The Medical Expenditure Panel Survey Insurance/Employer Component (MEPS-IC) is an annual establishment-level questionnaire administered to private and public sector employers to collect data on private health insurance plans and employer characteristics. Health insurance plan questions are asked about each plan offered by the employer, up to a maximum of four plans. The data is collected by U.S. Census Bureau from a sample of independently draw, nationwide sample of establishments and local governments. The MEPS-IC is designed to produce representative estimates of these responses at the state level, and a limited set of metropolitan-level estimates for select variables. Each data release includes post-stratification weights.

I obtain access to the restricted-use MEPS-IC microdata between 1999 to 2020 (MEPS-IC data was not collected in 2007). My sample consists of private-sector single-establishment establishments. For each variable of interest, I set missing observations equal to 0 and generate the CZ-year mean according to respondent weights. I impute the 2007 mean for each outcome in a given CZ. My analysis then proceeds at the CZ-year level.

MEPS-IC Variable Definitions. Variables used in my analysis include:

- Share of employers offering health insurance.
- Share of employees eligible for health insurance, conditional on health insurance offered by employer.
- Share of employees enrolled in health insurance, conditional on health insurance offered by employer.
- Share of employees working are part-time. Part-time workers are defined as less than 30 hours a week.
- Share of premiums paid by employer, averaged across single and family plans.
- Share of plans requiring referrals.

Current Population Survey Annual Social and Economic Supplement Data. The Current Population Survey (CPS) is a monthly U.S. household survey collected by the U.S. Bureau and the Bureau of Labor Statistics. The March CPS Annual Social and Economic Supplement (ASEC) Supplement provides monthly labor force and supplemental data. I use the ASEC to examine health insurance coverage outcomes. Households are sampled according to a multi-stage sampling procedure which first divides states into Primary Sampling Units (a metropolitan area, large county or group of smaller adjacent counties). Second, within each state, Primary Sample Units are grouped into homogenous strata with respect to social and economic characteristics, and one Primary Sample Unit is sampled per stratum. Third, housing units are sampled from each chosen Primary Sample Unit. Roughly 50,000 housing units with 100,000 individuals 15 years old and over are interviewed each month.

I obtain access to a restricted-use version of the ASEC that allows me to observe location of residence for households who appear in the LEHD, by merging the ASEC to the LEHD Residence Data. The LEHD Residence Data is available for all U.S. states and sourced from a set of federal administrative datasets (see source for detailed methodology). My analyses focuses on an annual, repeated cross-section of ASEC respondents from 2001 to 2018. I restrict the main sample to currently-employed, private-sector workers between the ages of 18 to 64. Workers are classified into three bins depending on their health insurance coverage. They are either insured by their employer, insured but not by their employer (i.e., covered by a spouse's plan, purchased directly, or public coverage), or uninsured. To assess worker coverage rates, I calculate the CZ-year mean share of workers covered by their employer, covered but not by their employer, and not covered, using respondent weights. I create a second sample that includes all respondents under the age of 65 to examine CZ-level rates of employer-sponsored health insurance coverage. I generate a binary indicator for whether they are covered by employer-sponsored health insurance, either directly or as a dependent. I calculate the CZ-year mean share of the population covered by employer-sponsored health insurance, using respondent weights. My analysis then proceeds at the CZ-year level.

Data sources and construction of additional variables in my analysis include:

- **Per-Capita Income, U.S. BEA**. I sum personal income across counties in a CZ and divide by population. Personal income includes wages and salaries, supplements to wages and salaries, inventory valuation adjustments, capital consumption adjustments, rental income, personal dividend and interest income and personal current transfer receipts less contributions for public social insurance.
- **Government transfers, U.S. BEA.** I sum transfers by category across counties in a CZ and divide by working-age population.
- **Labor force participation and unemployment rates, BLS LAUS**. I sum counts across counties within a CZ to calculate annual CZ-level rates.
- **Rural share, Census 2000.** I calculate the average rural share across counties in a CZ.
- **Median house value, Census 2000.** I calculate the average median house value across counties in a CZ.
- **Median house value, Census 2000.**
- **Hospital HHI, AHA Annual Survey of Hospitals.** I calculate the market share of hospital beds using the number of beds for all general hospitals in the CZ, assigning hospitals under the same system to a common owner.
- **Working-age population, SEER.** I sum population between 18 to 64 across all counties in a CZ annually.

B. Sun and Abraham (2021) Estimation

I implement Sun and Abraham (2021) interaction-weighted (IW) estimator. The recent econometrics literature on difference-in-differences has shown that in settings with variation in treatment timing and more than two time periods, coefficient estimates from two-way fixed effects (TWFE) models may not represent the causal parameter of interest (De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). The Sun and

Abraham (2021) procedure which addresses these concerns proceeds in three steps.

Step 1. I estimate cohort-specific average treatment effects on the treated ($CATT_{el}$), where cohorts are defined by the year of treatment. The $CATT_{el}$s are the average treatment effect for earlier-exposed CZ cohorts l years away from year e of the merger, relative to later-exposed CZ cohorts that remain untreated in year e. I restrict the observation window to 4 years before and 5 years after the merger such that $l \in [-4, 5]$. The estimating equation is:

$$y_{ct} = \sum_e \sum_l \beta_{el} [\mathbb{1}(\text{Merger}_c) \cdot \mathbb{1}(\text{Year}_t = l) \cdot \mathbb{1}(\text{Year of Merger}_c = e)] + \alpha_t + \delta_c + \varepsilon_{ct} \tag{7}$$

where $\mathbb{1}(\text{Merger}_c)$ is a indicator for CZ c treated by an insurer merger, $\mathbb{1}(\text{Year}_t = l)$ is an event-time indicator for year t being l years away from the year of the merger, $\mathbb{1}(\text{Year of Merger}_c = e)$ is a cohort indicator if CZ c is exposed to a merger in year e, α_t and δ_c are year and CZ fixed effects, and ε_{ct} is the error term. I cluster standard errors at the CZ level. I omit the event time indicator for the year before the merger. I weight each CZ-year by the CZ population in the year before the merger, given differences in CZ population across the U.S. The coefficients $\hat{\beta}_{el}$s are the difference-in-differences estimators for cohort-specific average treatment effects on the treated.

Step 2. I estimate the weights $Pr\{\text{Year of Merger}_c = e | l \in [-4, 5]\}$ by calculating sample shares for each cohort. Weights for a given cohort are constant across event times, as my sample is a balanced panel of CZs over the observation period.

Step 3. I calculate interaction-weighted (IW) estimators for event time l:

$$\hat{v}_l = \sum_e \hat{\beta}_{el} \hat{Pr}\{\text{Year of Merger}_c = e | l \in [-4, 5]\} \tag{8}$$

where $\hat{\beta}_{el}$ is from step 1 and $\hat{Pr}\{\text{Year of Merger}_c = e | E_c \in [-4, 5]\}$ is from step 2. The coefficients of interest $\hat{v}_l$s are a weighted average of cohort-specific average treatment effects on the treated, where weights are equal to each cohort's sample share.

As with the two-way fixed effects estimators, the IW estimators are identified under the assumptions of parallel trends and no anticipation.

C. Alternative Control Groups

My baseline sample compares earlier-exposed CZs with later-exposed CZs. The choice of controls is justified by the similar characteristics between earlier-exposed and later-exposed CZs, as shown in columns 1 and 2 of Appendix Table A.1, and the parallel trends in premiums prior to insurer mergers by premiums, as shown in Figure 4.

This section considers alternative definitions of control CZs. Appendix Table A.1 reports summary statistics for earlier-exposed CZs, later-exposed CZs (the baseline control group), and alternative groupings of control CZs. I divide all possible control CZs into three distinct categories: later-exposed CZs, out-of-market CZs, and never-exposed CZs. I identify 39 later-exposed CZs that experience a first-observed insurer merger six years or after the earlier-exposed CZs, 189 out-of-market CZs that only one of the two merging insurers present prior to the merger, and 300 never-exposed CZs that do not have either of the two merging insurers present prior to insurer mergers. While out-of-market CZs (column 3) have similar characteristics to treated CZs (column 1) in Appendix Table A.1, never-exposed CZs (column 4) are less-populated, more rural CZs with more concentrated insurer markets compared to treated CZs and may not be suitable as controls. Out-of-market CZs may also be unsuitable as controls because they could experience merger effects unrelated to market power. Out-of-market mergers involve at least one insurer already present prior to the merger, potentially leading to post-merger premium effects due to efficiency cost savings, changes in pricing strategies, or adjustments in health plan quality, which I investigate in Section XX.

I present regressions results from equation (1) with different control CZ groups in Appendix Table A.2. Column 1 replicates results in Figure 4 using later-exposed CZs as the baseline control group. Using out-of-market CZs or never-exposed CZs as the control group, I find a similar evolution and increase in premiums, but the estimates are smaller in magnitude and insignificant; see columns (2) and (3) of Appendix Table A.2. never-exposed CZs are notably different from earlier-exposed CZs so are not a suitable counterfactual, but in an effort to increase the number of CZs in the control group, I use all later-exposed and out-of-market CZs as controls and present results in column (4). I find similar qualitative results to my baseline specification, but results are not statistically significant. Specifications using "matched" control groups in the spirit of Kline and Moretti (2014), where I predict the probability of treatment for control CZs and keep the top quartile of "likely-treated" control CZs yields similar but also insignificant results.

TABLE A.1: Varying Control Group: CZ-Level Summary Statistics

	(1) Treatment	(2) Baseline Control	(3) Alternative Controls	(4)
	Earlier-Exposed CZs	Later-Exposed CZs	Out-of-Market CZs	Never-Exposed CZs
Number of Insurers	12.89	10.41	11.01	7.83
	(6.72)	(4.54)	(6.98)	(5.75)
Insurer HHI	2,804.89	2,691.01	2,773.64	3,685.55
	(1,529.81)	(1,255.21)	(1,643.36)	(2,179.31)
Per-Participant Premiums	4,651.25	4,767.59	4,278.55	4,520.60
	(2,092.72)	(2,147.67)	(2,209.20)	(2,405.73)
Population	474,700.63	360,953.91	456,976.34	306,965.00
	(237,802.69)	(159,819.64)	(435,350.80)	(329,544.06)
Median Household Income	53,673.78	54,063.60	48,834.08	48,117.02
	(8,181.72)	(7,485.07)	(7,144.21)	(6,787.18)
Unemployment Rate	6.83	6.04	7.30	7.31
	(2.68)	(2.17)	(3.19)	(3.23)
Share Rural	0.51	0.53	0.54	0.59
	(0.19)	(0.15)	(0.19)	(0.17)
Observations	820	1,900	6,040	18,860
Unique CZs	81	39	189	300

Notes: This table displays summary statistics for earlier-exposed CZs (column 1), later-exposed CZs (column 2), out-of-market CZs (column 3), and never-exposed CZs (column 4). Premiums are the sum of premiums paid by employers and employees divided by the number of participants. Nominal dollars are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2021.
Sources: Form 5500, 2000 Census, SEER Population Data, and BLS LAUS.

TABLE A.2: Alternative Control CZs

	(1) Last-Treated CZs	(2) Out-of-Market CZs	(3) Never-Treated CZs	(4) Last-Treated & Out-of-Market CZs
Merger $\times$(Year $= -4$)	19.15	-16.76	-55.74	-176.84
	(296.83)	(273.46)	(258.22)	(276.69)
Merger $\times$(Year $= -3$)	3.54	113.43	94.70	-76.57
	(268.01)	(253.11)	(239.03)	(258.64)
Merger $\times$(Year $= -2$)	209.91	131.23	154.57	-23.37
	(320.14)	(231.64)	(224.08)	(268.68)
Merger $\times$(Year $= -1$)	-	-	-	-
Merger $\times$(Year $= 0$)	-107.87	-39.90	-55.90	-218.06
	(208.10)	(135.81)	(131.30)	(177.53)
Merger $\times$(Year $= 1$)	109.27	-19.47	-85.57	-171.05
	(222.08)	(166.59)	(159.50)	(198.45)
Merger $\times$(Year $= 2$)	643.38**	448.29*	347.62	304.15
	(320.14)	(231.64)	(224.08)	(268.68)
Merger $\times$(Year $= 3$)	741.30***	375.98	233.44	254.03
	(268.01)	(253.11)	(239.03)	(258.64)
Merger $\times$(Year $= 4$)	770.69**	135.77	-4.79	44.28
	(296.83)	(273.46)	(258.22)	(276.69)
Merger $\times$(Year $= 5$)	448.01	-175.48	-262.29	-264.83
	(304.74)	(263.97)	(253.13)	(291.90)
Years 0-5 Estimate	434.13**	120.86	28.75	-8.58
	(201.74)	(167.96)	(157.97)	(196.89)
Mean Premiums	4450.48	4450.48	4450.48	4450.48
Unique Control CZs	39	189	300	222
Observations	2,650	6,940	17,440	8,000
R-squared	0.56	0.56	0.52	0.56

Notes: This table reports difference-in-differences estimates with different control groups. The outcome is premiums. Column 1 replicates results in Figure 4 using last-treated as controls. Out-of-market CZs are impacted by insurer mergers but do not experience an increase in insurer concentration. Never-treated CZs do not experience insurer merger activity between 1999 and 2021. Nominal dollars are converted to 2019 dollars. Standard errors clustered at the CZ level are in parentheses.

Sources: Form 5500.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D. An Instrumental Variables Approach

In my baseline regression model equation (1), I regress premiums on an interaction between a merger exposure indicator and an event-time indicator. The model allows for a clear and straightforward interpretation of estimated coefficients; the β_ls represent the change in outcome following an insurer merger in earlier-exposed CZs relative to later-exposed CZs. The model assumes that merger exposure is orthogonal to the error term conditional on year and CZ fixed effects, i.e. $E[\varepsilon|\mathbb{1}(\text{Merger}_c) \cdot \mathbb{1}(\text{Year}_t = l), \alpha_t, \delta_c] = 0$. In other words, the model assumes merger exposure is randomly assigned which may be a strong assumption. In this Section, I explore alternative regressions models with weaker assumptions.

A possible alternative model regresses premiums on an interaction between the change in insurer HHI and an event-time indicator. The Ordinary Least Squares (OLS) estimation equation is:

$$\text{Log Premiums}_{ct} = \sum_{l} \beta_l [\text{Log HHI}_{ct} \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_t + \delta_c + \varepsilon_{ct} \tag{9}$$

which assumes merger-induced changes in insurer HHI are uncorrelated with other unobserved determinants of premiums. Using OLS, I find premiums increase by 1.88 percent for every 1 percent increase in insurer HHI due to insurer mergers; see column (4) of Appendix Table A.3. In Appendix Figure A.6, I find that insurer concentration as measured by HHI increases by roughly 20 percent in the average earlier-exposed CZs, so the OLS estimates imply a 36 percent increase (= 20 · 1.88) in premiums which is significantly larger than the estimated premiums effect of 10 percent from the baseline model using estimating equation (1).

Of course, one obvious problem with the OLS model (9) is the possibility of omitted variable bias. For example, one potential omitted variable is improvements in plan quality, which may be correlated with increases in premiums and insurer concentration. To address potential biases, I take an instrumental variables approach. I regress the actual change in insurer HHI on a measure of predicted exposure to insurer mergers interacted with an event-time indicator. The first-stage estimation equation is:

$$\text{Log HHI}_{ct} = \sum_{l} \beta_l [\text{Merger Exposure}_{ct} \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_t + \delta_c + \varepsilon_{ct}, \tag{10}$$

where predicted merger exposure is equal to the sum of merging insurers' market shares prior to the merger. I set the exposure measure equal to 0 if both merging insurers are not present. The instrument is plausibly exogenous because merging insurers' market shares are determined before the merger.

I report first-stage results in column (1) of Appendix Table A.3. Unfortunately, the proposed instrument is not a valid instrument due to its weak correlation with the realized change in insurer HHI. Insurer HHI increases by 0.25 percent (standard error = 0.20) for every 1 percentage point increase in the combined market share of merging insurers. The result is insignificant violating the relevance assumption and the instrument is weak as the F-statistic falls below 10 (F=6.21).[25] For completeness, I include the reduced form and instrumental variables (IV) results in columns 2 and 3, but I do not read into these results given the weak first stage.

[25] I try specifications with an exposure measure equal to the simulated change in insurer HHI, but I similarly find a weak first stage.

E. Successive Mergers

In the main analysis, I focus on 81 earlier-exposed treatment CZs that experience a first-observed insurer merger and are not exposed again over the 6 years after the merger. The sample restriction ensures that estimated premiums effects are not contaminated by earlier- or later-occurring mergers in the same CZ during the observation window. To examine the generalizability of my findings, I expand the sample of earlier-exposed treatment CZs to include CZs successively exposed by mergers after the first merger event and present results in Appendix Figure A.2. The expanded sample of 143 CZs includes the 81 CZs in my baseline sample exposed to only one merger and an additional 62 CZs that experience multiple mergers after the first exposure in year $t = 0$. Appendix Figure A.1 shows the probability that an earlier-exposed CZ impacted by a first-observed merger in year t experiences an additional insurer merger. One-quarter of earlier-exposed CZs are impacted by another merger in the year after the first merger, one-sixth of CZs in the second year, and no more than one-tenth of CZs in the later years.

I estimate positive and significant premiums effects using the expanded sample, consistent with the baseline result. While the number of insurers appears to be increasing at a faster rate in earlier-exposed CZs prior to the merger, the number of insurers decreases sharply in response to the first merger by 7 percent from 27 to 25 insurers. The number of insurers steadily declines in the post-merger period with 6 fewer insurers overall 6 years later, which reflects successive mergers occuring in the earlier-exposed CZs in the post-merger period. The estimate effect on insurer concentration reflects the effects on the number of insurers in earlier-exposed CZs relative to later-exposed CZs. Insurer concentration rises sharply in response to the first merger by 10 percent from 2,000 to 2,200 HHI points and continues to increase to 2,800 HHI points. As insurer concentration steadily increases post-merger, I find that premiums increase by 7 percent ($315, standard error = 161) on average in the years following the merger. In both samples, I estimate increases in insurer concentration of roughly 20 percent, resulting in similar-sized premiums increases of 10 percent using the baseline sample and 7 percent using the expanded sample. Compared to the baseline sample, the expanded sample of CZs have more insurers (27 vs. 14) and is less concentrated (2077 vs. 2500 HHI points) on average. Each additional merger in the newly-added successively-exposed CZs yields small postmerger increases in HHI, which may explain the slightly smaller estimated effect for the expanded sample.

TABLE A.3: CZ-Level Effect of Mergers on Premiums: Simulated Exposure Measure

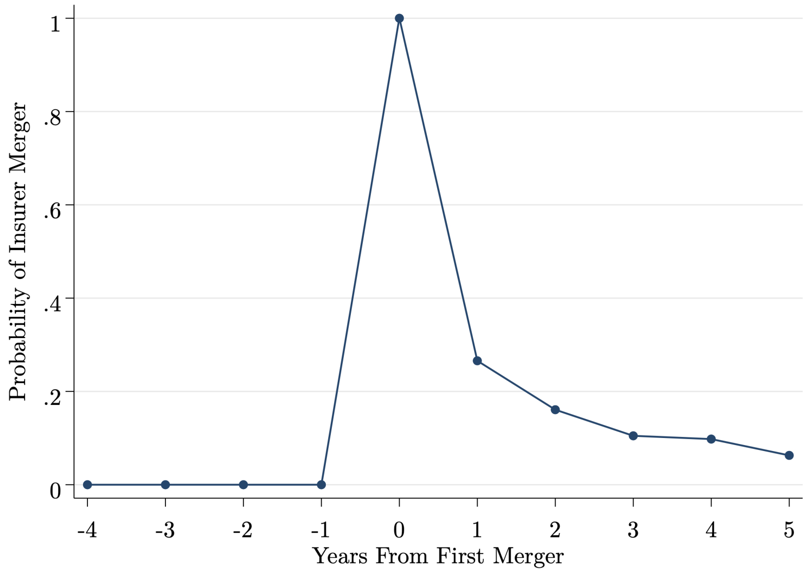
	(1) Outcome: Log HHI	(2) Outcome: Log Premiums	(3)	(4)
	First Stage	Reduced Form	IV	OLS
Merger Exposure $\times$(Year $= -4$)	0.0016	0.0034		
	(0.0027)	(0.0042)		
Merger Exposure $\times$(Year $= -3$)	-0.0031	0.0020		
	(0.0024)	(0.0042)		
Merger Exposure $\times$(Year $= -2$)	-0.0024	0.0030		
	(0.0021)	(0.0045)		
Merger Exposure $\times$(Year $= -1$)	-	-		
Merger Exposure $\times$(Year $= 0$)	0.0006	-0.0002		
	(0.0015)	(0.0042)		
Merger Exposure $\times$(Year $= 1$)	0.0012	0.0011		
	(0.0017)	(0.0042)		
Merger Exposure $\times$(Year $= 2$)	0.0020	0.0041		
	(0.0021)	(0.0045)		
Merger Exposure $\times$(Year $= 3$)	0.0036	0.0049		
	(0.0024)	(0.0042)		
Merger Exposure $\times$(Year $= 4$)	0.0052*	0.0067		
	(0.0027)	(0.0042)		
Merger Exposure $\times$(Year $= 5$)	0.0024	0.0042		
	(0.0029)	(0.0042)		
Log HHI $\times$(Year $= -4$)			0.0171	0.0067
			(0.0005)	(0.0116)
Log HHI $\times$(Year $= -3$)			0.0107	0.0085
			(0.0004)	(0.0100)
Log HHI $\times$(Year $= -2$)			0.0156	0.0138
			(0.0004)	(0.0112)
Log HHI $\times$(Year $= -1$)			-	-
Log HHI $\times$(Year $= 0$)			0.0011	0.0048
			(0.0004)	(0.0100)
Log HHI $\times$(Year $= 1$)			0.0080	0.0102
			(0.0005)	(0.0103)
Log HHI $\times$(Year $= 2$)			0.0232	0.0260**
			(0.0005)	(0.0112)
Log HHI $\times$(Year $= 3$)			0.0284	0.0267***
			(0.0005)	(0.0100)
Log HHI $\times$(Year $= 4$)			0.0385	0.0271**
			(0.0006)	(0.0116)
Log HHI $\times$(Year $= 5$)			0.0276	0.0179
			(0.0006)	(0.0116)
Years 0-5 Estimate	0.0025	0.0035	0.0211	0.0188*
	(0.0020)	(0.0041)	(0.0220)	(0.0099)
F-statistic	2.29	-	6.21	-
Mean Outcome	2567.90	4450.48	4450.48	4450.48
Observations	2,650	2,650	2,650	2,650
R-squared	0.67	0.61	0.01	0.62

Notes: This tables reports . All specifications include CZ and year fixed effects. The outcome is log insurer HHI in columns 1 and 2, and log premiums in column 3 and 4. The exposure measure is the total market share of merging insurers in the year prior to the merger. 'Years 0-5 Estimate' reports the point estimate for the average of coefficients from years 0 to 5 following the merger. Standard errors clustered at the CZ level are in parentheses. I report the exponential of the mean outcome for treated CZs in the year before the merger and F-statistics from the first stage. Premiums are converted to 2000 dollars. The sample is at the CZ-year level from 1999 to 2021. N=81 treated CZs and 39 control CZs.

Source: Form 5500.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

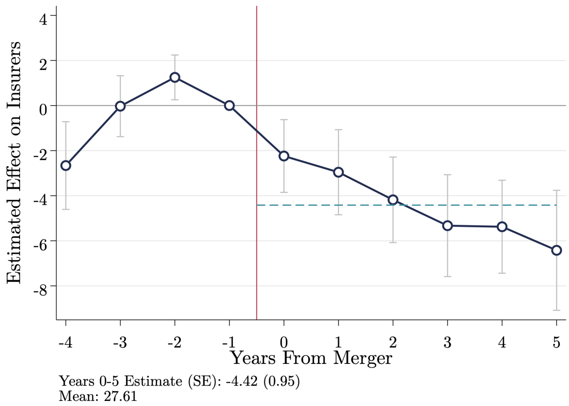
FIGURE A.1: Successive Insurer Mergers and Probability of Merger



Notes: This figure plots the probability that an earlier-exposed CZ experiences at least one insurer merger in a given year after the first exposure in year $t = 0$. The sample is at the CZ-year level from 1999 to 2020. N=143 CZs.
Source: Form 5500.

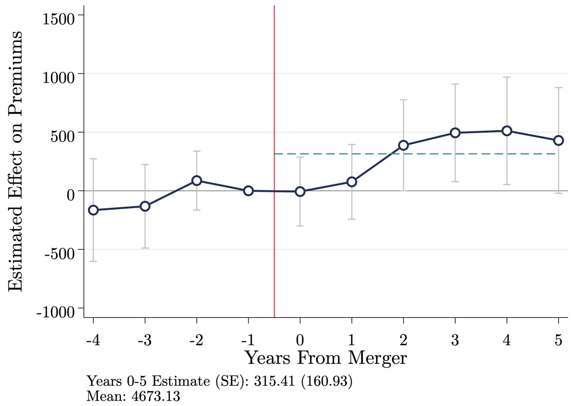
FIGURE A.2: CZ-Level Effect of Successive Insurer Mergers on Concentration & Premiums

(A) Number of Insurers



(B) Insurer HHI

(C) Premiums



Notes: This figure plots the yearly coefficients β_l from equation (1) examining the impact of successive insurer mergers on concentration and premiums. Treated CZs may experience additional mergers in the 6 years following the first merger, control CZs are later-exposed by mergers. The outcome y_{ct} is the CZ number of insurers in panel A, insurer HHI in panel B, and premiums in panel C. Observations are weighted by CZ working-age population in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. N= 143 treated CZ and 41 control CZs.
Source: Form 5500.

F. Calibration

Assuming the observed employment and wages for both groups and cost of employer-sponsored health insurance τ is the equilibrium, I can solve the firm's maximization problem for the productivity shifters λ_N and λ_C.

The first-order conditions for the firm's maximization problem are:

$$\omega_g = w_g + \tau = \lambda_g L_g^{\rho-1}(\lambda_N L_N^{\rho} + \lambda_C L_C^{\rho})^{\frac{1-\rho}{\rho}} \tag{11}$$

I take the ratio of (11) for college to non-college workers and solve for λ_N as a function of λ_C.

$$\frac{w_N + \tau}{w_C + \tau} = (\frac{\lambda_N}{\lambda_C}) \cdot (\frac{L_N}{L_C})^{\rho-1} \tag{12}$$

$$\lambda_N = (\frac{w_N + \tau}{w_C + \tau}) \cdot (\frac{L_N}{L_C})^{1-\rho} \cdot \lambda_C \tag{13}$$

Subbing (13) into (11) I can solve for λ_C:

$$w_C + \tau = \lambda_C L_C^{\rho-1}((\frac{w_N + \tau}{w_C + \tau}) \cdot (\frac{L_N}{L_C})^{1-\rho} \cdot \lambda_C L_N^{\rho} + \lambda_C L_C^{\rho})^{\frac{1-\rho}{\rho}} \tag{14}$$

$$\lambda_C = [(w_C + \tau) L_C^{1-\rho}((\frac{w_N + \tau}{w_C + \tau}) \cdot (\frac{L_N}{L_C})^{1-\rho} \cdot L_N^{\rho} + L_C^{\rho})^{\frac{\rho-1}{\rho}}]^{\rho} \tag{15}$$

Next, with the values of w_g, L_g, ρ, τ, and λ_g and assuming the extensive-margin labor supply elasticities are equal to 0.32 for both groups, I solve for the $\overline{\kappa}$ and $\underline{\kappa}$ in the labor supply function in equation (3). I rewrite the labor supply function in (3) as a function of the labor supply elasticity to calculate $\underline{\kappa}$:

$$\underline{\kappa} = (w_g + \alpha\tau) - P_g \cdot (\overline{\kappa} - \underline{\kappa}) \tag{16}$$

$$\Rightarrow \underline{\kappa} = (w_g + \alpha\tau) - \frac{w_g}{e_g^S} \tag{17}$$

Rewriting equation (6), I solve for $\overline{\kappa}$:

$$\overline{\kappa} = \frac{w_g + \alpha\tau - \underline{\kappa}}{P_g} + \underline{\kappa} \tag{18}$$

G. Firm-Level and Worker-Level Employment Effects

For my baseline results in Section 5, I conduct a CZ-level analysis of the labor market effects of insurer mergers using an unbalanced sample of firms and workers. In this Section, I conduct firm-level and worker-level difference-in-difference analyses using a balanced sample of firms and workers which account for potential biases from compositional changes.

Firm-Level Employment Effects. To decompose employment losses into those losses caused by inactive firms and continuing firms, I follow a balanced sample of firms over the observation window period regardless of firm inactivity. The sample consists of exposed single-establishment firms in the LEHD that are active (i.e., at least one employee) and hire at least fifty workers (so are likely to provide ESHI benefits) in the year before the merger. I implement the following firm-level difference-in-difference model:

$$y_{j(c)t} = \sum_{l} \beta_l [\mathbb{1}(\text{Merger}_{j(c)}) \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_{lt} + \delta_{j(c)} + \varepsilon_{j(c)t} \tag{19}$$

where $\mathbb{1}(\text{Merger}_{j(c)})$ is a indicator for firm j in CZ c that is exposed to an insurer merger, $\mathbb{1}(\text{Year}_t = l)$ is an event-time indicator for calendar year t being l years away from the year of the merger, α_{lt} are calendar-and-event-year fixed effects, $\delta_{j(c)}$ are firm fixed effects, and $\varepsilon_{j(c)t}$ is the error term. I cluster standard errors at the firm level. The coefficients of interest are the β_ls; they measure the impact on outcome $y_{j(c)t}$ in event year l for firms in earlier-exposed CZs, relative to firms in later-exposed CZs. I also report the average of coefficients across the post-merger event years.

I show results from estimating equation (19) in Appendix Figure A.22. Panel A indicates that employment in all exposed firms (where employment is zero in years of firm inactivity) trends in parallel across earlier-exposed and later-exposed CZs, which suggests the parallel trends assumption necessary for identification is plausible. After the merger, employment in all exposed firms falls by 3.6 workers (5.3 percent, se = 1.0) on average. Exposed firms lose 2 workers in the year of the worker and continue experience employment losses over the next two years, remaining at a lower level of 4 fewer workers for at least eight years. The finding is evidence that the baseline employment effects estimated using a unbalanced firm panel at the CZ-level is not simply driven by firm composition.

To decompose the overall employment losses, I examine the share of firms that are active (i.e., employ at least one worker) in Panel B and employment in continuing firms that are active over the entire observation period ("always active") in Panel C. Panel B indicates that the share of firms that are active falls by 2.6 percentage points (standard error = 0.5), while Panel C shows that employment in always-active firms falls by roughly 2.5 workers (2.2 percent, standard error = 1.3) on average after the merger. The presence of merger effects on firm activity supports the use of an unbalanced panel for my CZ-level baseline results, which captures losses from firm inactivity and workforce reductions in continuing firms.

How much of the employment losses in exposed firms can be attributed to firm inactivity versus employment losses in continuing firms? The immediate fall in employment of 2.2 fewer workers (standard error = 0.7) in the year of the insurer merger can be attributed to firm inactivity, as the share of active firms falls by 2.9 percentage points (standard error = 0.4) in the same year. In contrast, employment in always-active firms does not begin to fall until the following year. Firm activity can explain at most 55 percent ($= 2.2/4$) of the overall employment losses, and employment losses in continuing firms explain at least 45 percent of the overall employment losses in Panel A.

To examine whether noncollege-educated workers are losing their jobs, I study employment in always-active exposed firms by education level. Appendix Figure A.22 Panel D presents results from estimating equation (19). Firm employment of noncollege-educated workers falls sharply by 2.5 workers (standard error = 1.0) in the year following the merger and gradually falls to a low of 4.6 fewer workers (standard error = 1.7) by the eighth year after the merger. In contrast, firm employment of college-educated workers remains stable in exposed firms in earlier-exposed CZs relative to those in later-exposed CZs. Panel D provides striking evidence that employment losses are concentrated for less-educated workers, which is consistent with the CZ-level evidence shown in Panel B of Figure 11. The finding is consistent with an increase in the fixed costs per worker (regardless of worker wage earnings) that disproportionately hurts lower-income noncollege-educated workers.

Worker-Level Employment Effects. I analyze a balanced sample of workers, regardless of their employment status, to mitigate potential bias from changes in worker composition. Using the LEHD data, I define exposed workers as those employed in single-establishment firms with at least fifty employees in the year prior to the merger. To refine the group of exposed workers, I apply three additional criteria. First, I limit the analysis to workers aged 25 to 55 in the year before the merger to avoid bias from workers "aging in or out" of the sample. Second, I focus on workers who have only one employer and receive wages for all four quarters at an exposed firm in the year before the merger, which helps identify them based on their pre-merger employer. Third, I focus on workers employed in only one CZ over the entire sample period to assign workers to a single CZ. After identifying the set of exposed workers, I first track their outcomes within their pre-merger firms and then assess their outcomes at any firm during the observation period.

Figure A.23 plots the raw trends of average outcomes for exposed workers in earlier-exposed and later-exposed CZs. The top two panels focus on employment and annual wages in the pre-merger firm where the worker was employed in the year before the merger (i.e., in $t-1$). Panels A and B show that workers are less likely to be employed (i.e., receive positive earnings) and receive lower annual wages (including zero earnings) at their pre-merger firm. The bottom two panels examine employment and annual wages in any firm. Panels C and D show that workers are also less likely to be employed and earn lower annual wages at any firm. The gap in worker outcomes at any firm by CZ merger exposure is smaller than the gap in worker outcomes at the pre-merger firm alone. The finding suggests that some workers who lose their jobs find employment and earn wages in new firms after the merger. The raw trends offer evidence of how merger-induced ESHI cost increases impact workers. Given evidence of some pre-trends in employment and earnings in Panels A and B, I use a basic difference-in-difference model at the worker-level to compare pooled worker outcomes before and after the merger.

I use the following worker-level difference-in-difference model:

$$y_{i(c)t} = \beta[\mathbb{1}(\text{Merger}_{i(c)}) \cdot \mathbb{1}(\text{Year}_t \geq 0)] + \alpha_{lt} + \delta_{i(c)} + \boldsymbol{X}'_{i(c),t}\boldsymbol{\Gamma} + \varepsilon_{i(c)t} \tag{20}$$

where $\mathbb{1}(\text{Merger}_{i(c)})$ is a indicator for worker i who is employed in CZ c that is exposed to an insurer merger, $\mathbb{1}(\text{Year}_t \geq 0)$ is an event-time indicator for calendar year t equal to the year of the merger or a subsequent year, α_{lt} are calendar-and-event-year fixed effects, $\delta_{i(c)}$ are worker fixed effects, and $\varepsilon_{i(c)t}$ is the error term. $\boldsymbol{X}'_{i(c),t}$ is a vector of worker-level controls, including age, age-squared, gender, race/ethnicity, and education level. I cluster standard errors at the worker level. The coefficients of interest are the β_ls; they measure the impact on outcome $y_{i(c)t}$ in event year l for workers in earlier-exposed CZs, relative to workers in later-exposed CZs.

Table A.4 Panel A presents the corresponding estimates to Figure A.23 from estimating equation (20). Workers are 2.6 percentage points (standard error = 0.1) less likely to be employed in their pre-merger firm, but are only 0.4 percentage points (standard error = 0.1) less likely to be employed in any firm, which implies that 85% (=$\frac{2.6-0.4}{2.6}$) of workers who lose their jobs find employment at a new firm. Workers also earn $1,579 (2.9 percent, standard error = 121) less in their pre-merger firm, but earn only $382 (0.7 percent, standard error = 145) less in any firm. Consistent with Figure A.23, these findings suggest that workers experience employment and earnings losses in pre-merger firms, but most workers who lose their jobs reallocate to new firms leading to smaller overall declines in employment and wages at any firm after the merger.

H. Implied Labor Supply Elasticity of Vulnerable Firms: Data Sources and Derivations

1. **My estimate (2024).** The estimated employment decline in vulnerable single-establishment firms is 4.4 percent (Figure 7). Given that average premiums are $4,500 per participant in the sample and increase by 10 percent post-merger (Figure 4), and assuming two plan participants per worker and earnings net of taxes is equal to $35,000, the change in net-of-tax-and-ESHI earnings is equal to 3.5 percent.

2. **Gao et al. (2023).** They find that a one standard deviation in insurers' losses predicts a 2 percent increase in premiums (page 17). The percent change in employment from a 2 percent increase in premiums is equal to 0.6 ($= 2 \cdot 0.3$) percent from table 2 column 1. Average premiums in the sample is equal to $6,700 (table 1). Assuming two plan participants per worker and earnings net of taxes is equal to $35,000, the percent change in net-of-tax-and-ESHI wages is equal to 1.2 percent ($(-6700 * 2 + 6700 * 1.02 * 2)/(35000 - 6700 * 2)$).

3. **Cooper et al. (2024).** They estimate a percent change in employment of 0.4 percent from table 4 panel B column 4. They also find a 1 percent change in premiums in table 3 Panel B column 1. They report average premiums of $5,000 per participant which an average of 2 participants per worker. Assuming net-of-tax annual earnings is $35,000, the percent change in the net-of-tax-and-ESHI wages is equal to 0.4 percent ($(-5000 * 2 + 5000 * 1.01 * 2)/(35000 - 5000 * 2)$).

4. **Gruber (1997).** Basic pooled difference-in-difference estimates in row 1 of table 3.

5. **Saez et al. (2019).** Implied elasticity of 1.8 to 2.4 on page 1759. Corresponding standard errors calculated using delta method from estimates in Table 4, column 1.

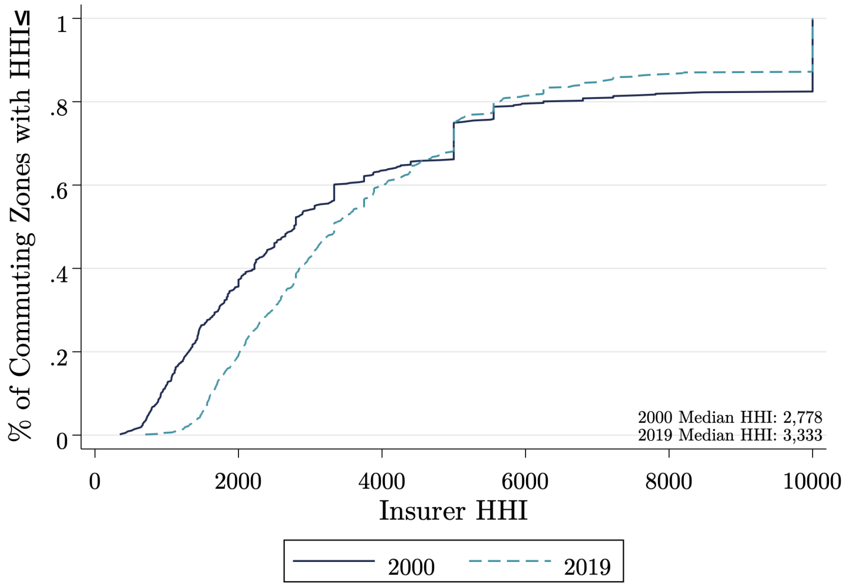
6. **Ku et al. (2020).** Implied labor supply elasticity on page 17. Corresponding standard errors calculated using delta method from estimates in Table 2, Panel A.

7. **Lobel (2023).** Implied labor supply elasticity estimate and standard error in Table 3 column 1 row 1.

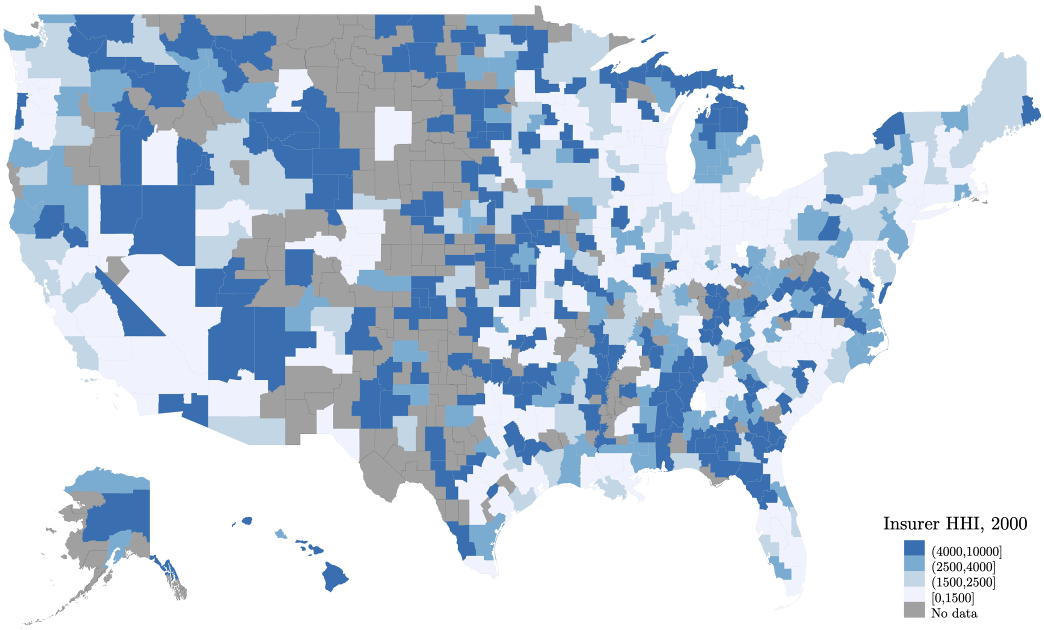
I. Additional Figures

FIGURE A.3: Insurer Concentration

(A) CDF of Insurer HHI, 2000 and 2020



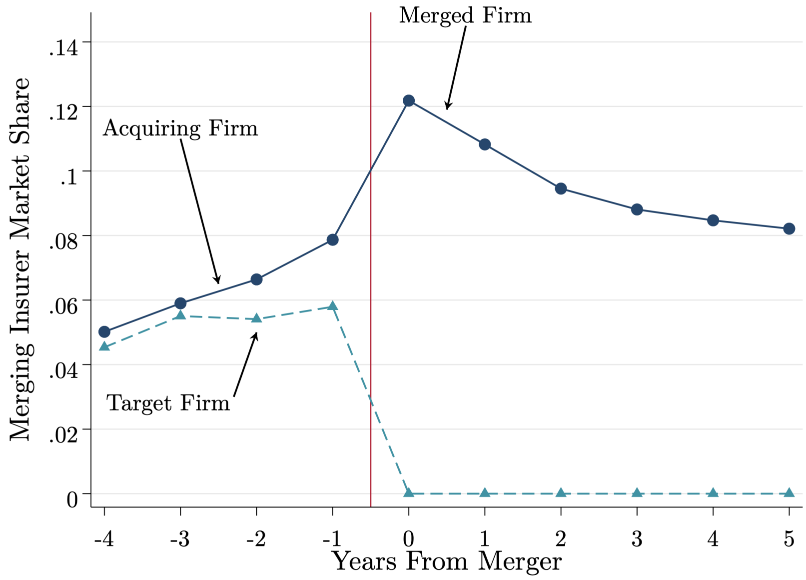
(B) Insurer HHI by Commuting Zone, 2000



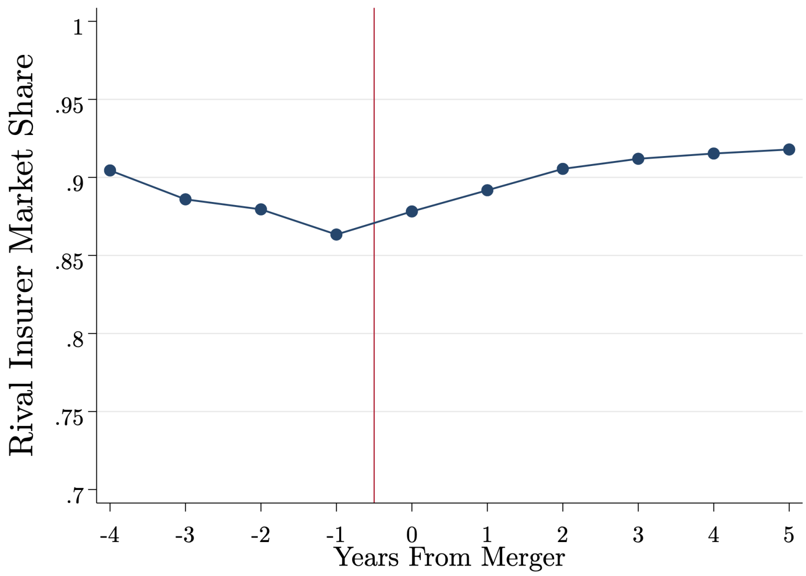
Source: Form 5500.

FIGURE A.4: Market Share Trends in Treated Commuting Zones

(A) Merging Insurer Market Shares

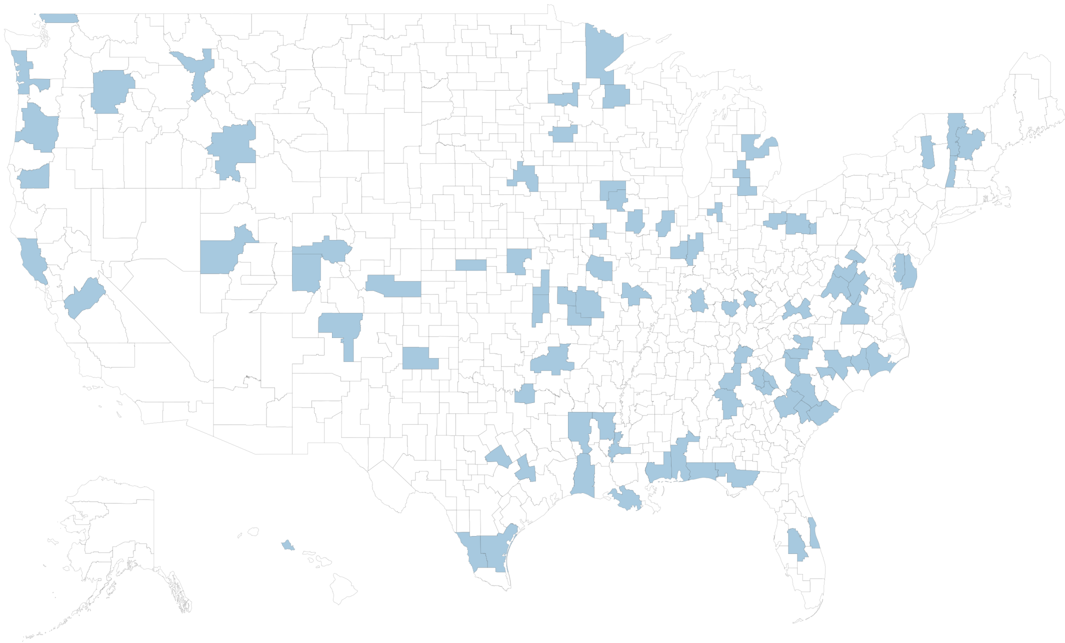


(B) Rival Insurer Market Share



Notes: This figure plots market shares of merging insurers (panel A) and rival insurers (panel B) in 81 earlier-exposed CZs. Treated markets have overlapping market shares of both merging insurers in the four years prior to the merger, and increased market share of the merged insurer in the six years following the merger. Rival insurers increase market share post-merger, as insurers switch away from the merged insurer.
Source: Form 5500.

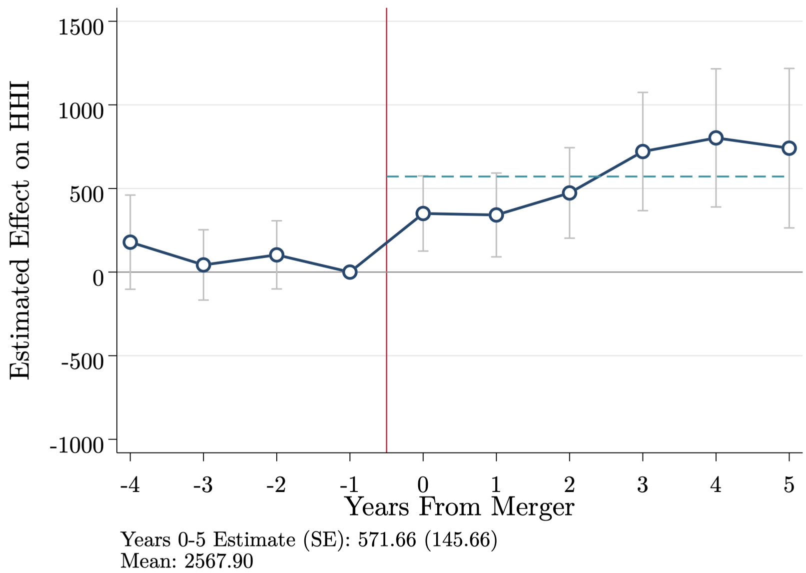
FIGURE A.5: Commuting Zone Sample



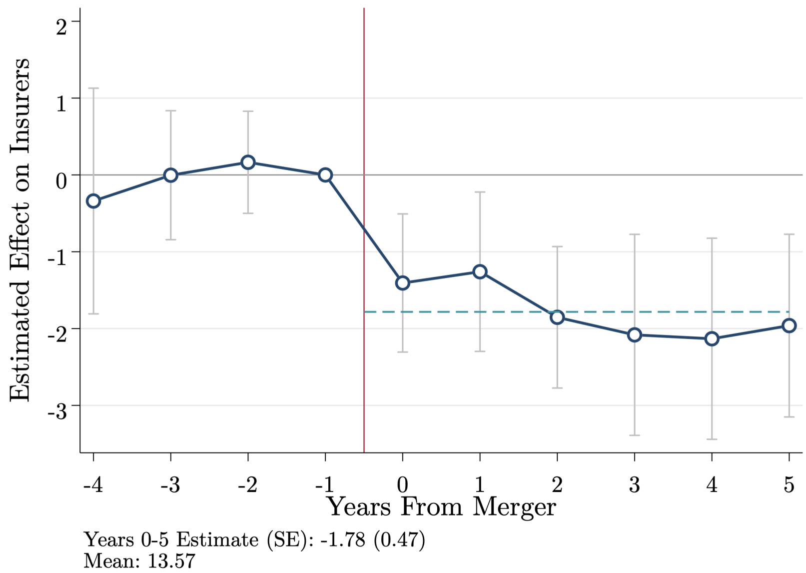
Notes: This figure shows the analysis sample of commuting zones shaded in blue. N=93 CZs.
Source: Form 5500.

FIGURE A.6: CZ-Level Effect of Insurer Mergers on Concentration

(A) Insurer HHI

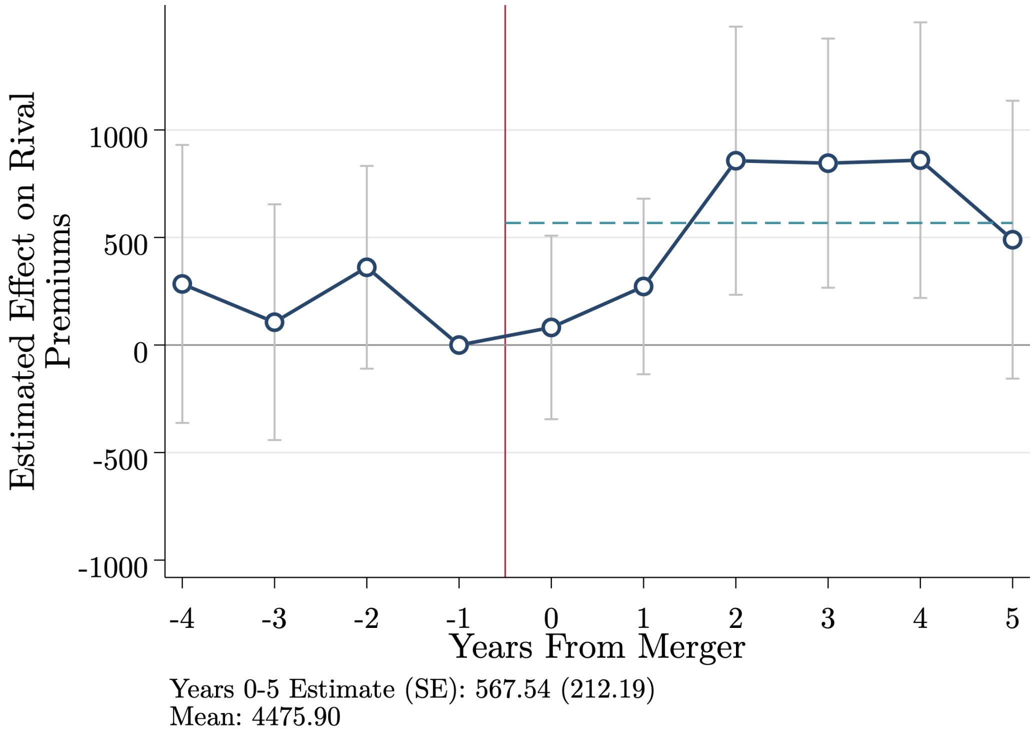


(B) Number of Insurers



Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is insurer HHI (panel A) and number of insurers (B). Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs.
Source: Form 5500.

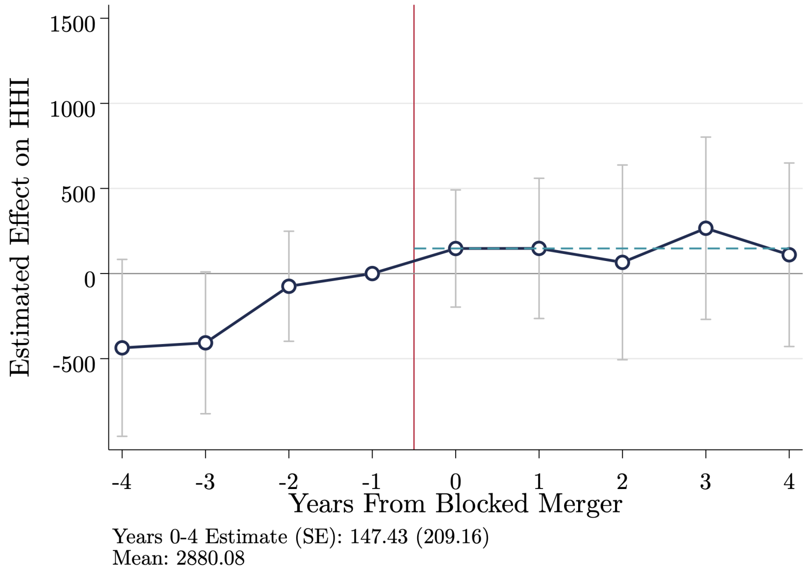
FIGURE A.7: CZ-Level Effect of Insurer Mergers on Rival Premiums



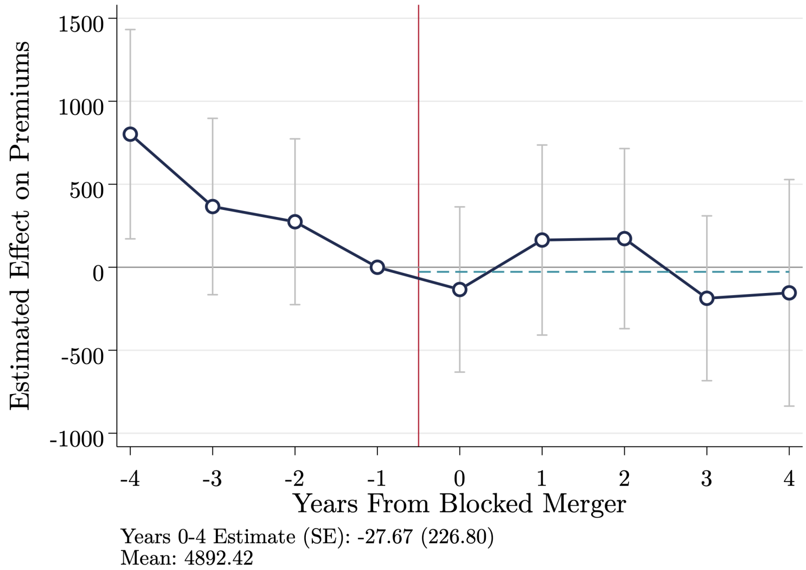
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is rival premiums. Later-exposed CZs are included once per earlier-exposed CZ, and I include merger fixed effects. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. N= 81 treated CZ and 39 control CZs.
Source: Form 5500.

FIGURE A.8: CZ-Level Effect of **Blocked** Insurer Mergers on Concentration & Premiums

(A) Insurer HHI



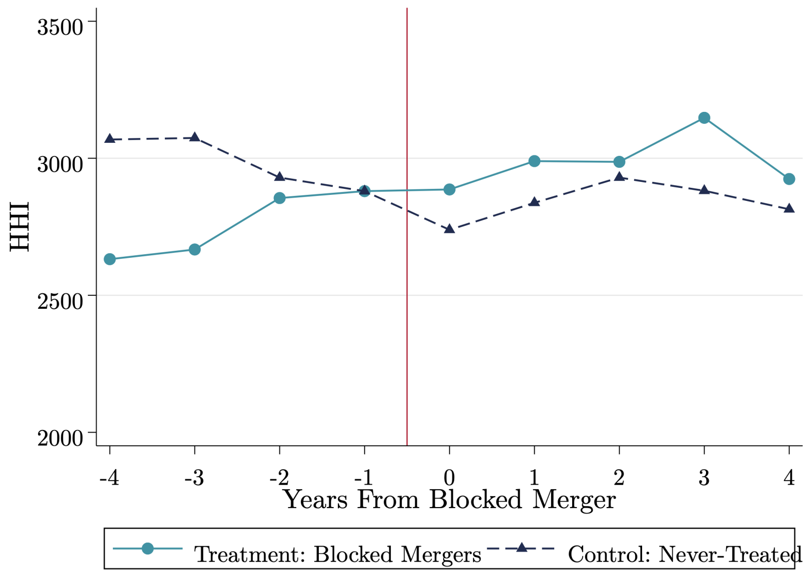
(B) Premiums



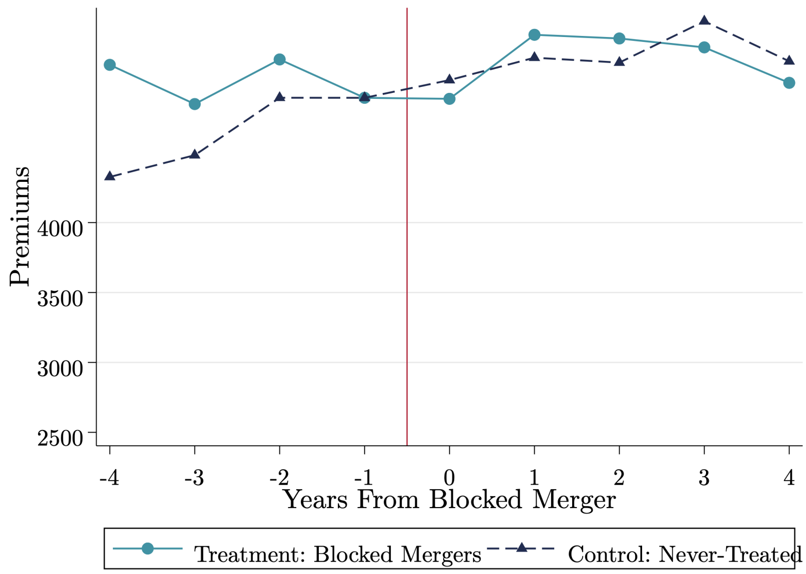
Notes: This figure plots the yearly coefficients β_l from equation (1) of a placebo test studying blocked insurer mergers. Placebo treated CZs are CZs that would have been earlier-exposed by a merger blocked by antitrust action and control CZs are never treated by a merger. The outcome y_{ct} is insurer HHI in panel A, and premiums in panel B. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 4 following the merger ($\frac{1}{5}\sum_{l=0}^{4}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. N= 33 placebo treated CZ and 226 control CZs. Figures of raw trends are shown in Appendix Figure A.9 and estimates are reported in Appendix Table **??**.
Source: Form 5500.

FIGURE A.9: Blocked Mergers: Insurer Concentration & Premiums by Treatment

(A) Insurer HHI

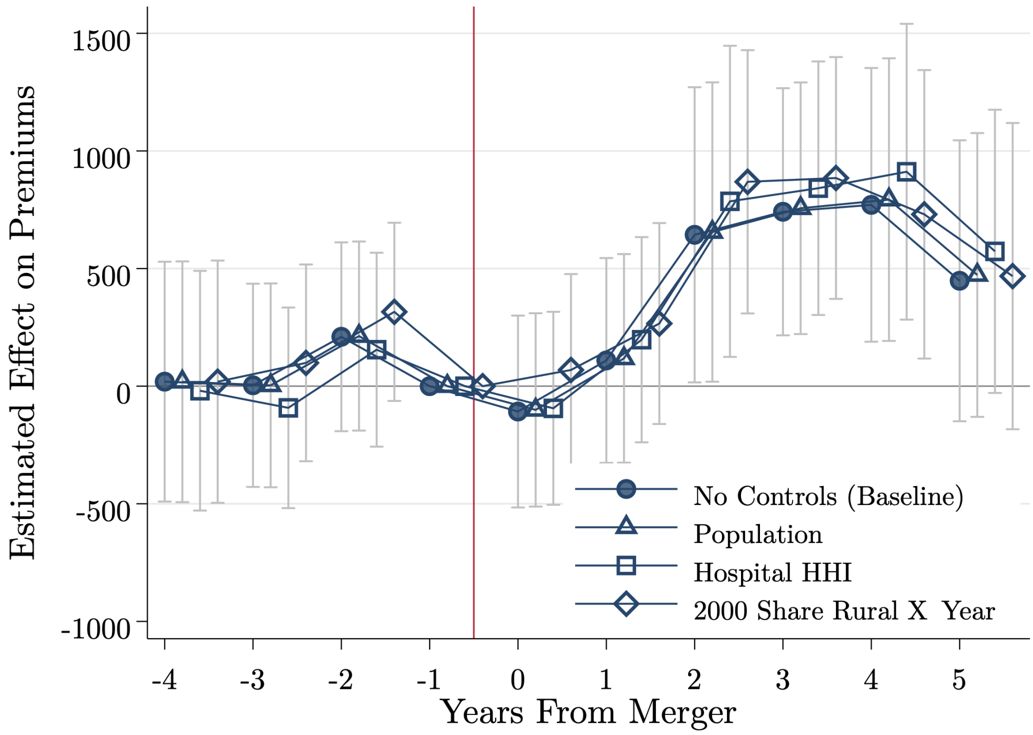


(B) Premiums



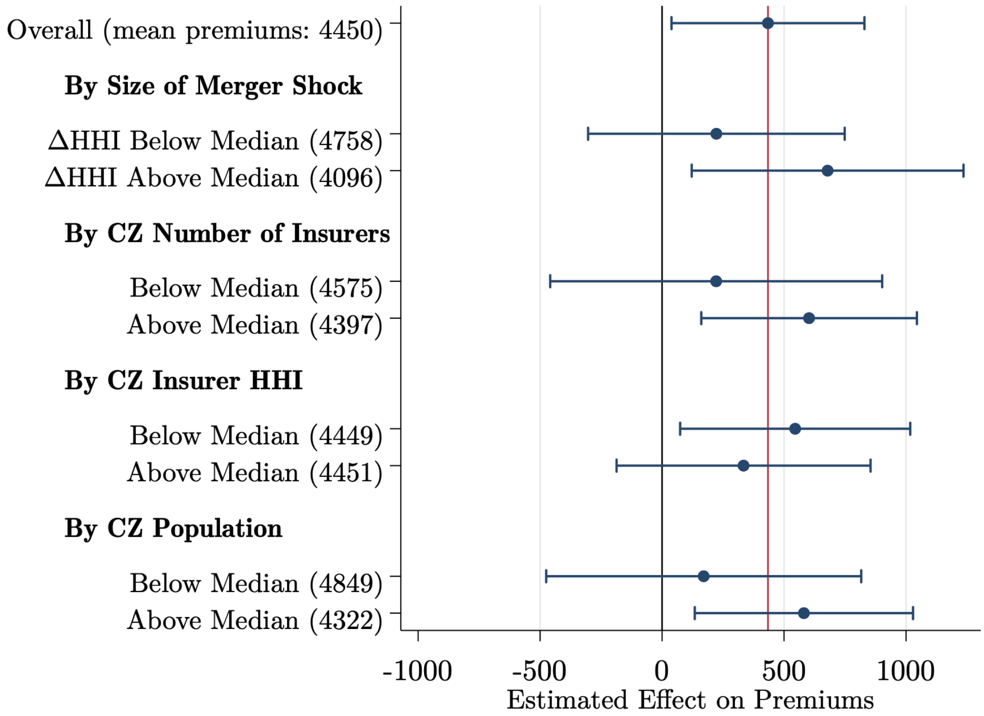
Notes: This figure plots trends in the population-weighted mean CZ insurer HHI (panel A), and premiums (panel B). Placebo treated CZs are CZs that would have been earlier-exposed by a merger blocked by antitrust action and control CZs are never treated by a merger. Observations are weighted by CZ working-age population in the year before the merger. Outcomes for control CZs are normalized to equal the mean outcome for treated CZs in the year before the merger. N= 33 placebo treated CZ and 226 control CZs.
Source: Form 5500.

FIGURE A.10: CZ-Level Effect of Insurer Mergers on ESHI Premiums in Vulnerable Firms: Varying Controls



Notes: This figure plots the yearly coefficients β_l from equation (1), varying the set of regression controls. The outcome y_{ct} is premiums. Shaded circle markers report baseline results with no controls, triangle markers report results with lagged population and share of population over 65 as controls, square markers report results with lagged hospital HHI as a control, and diamond markers report results with 2000 rural share times year indicators as controls. Observations are weighted by CZ working-age population in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs.
Source: Form 5500.

FIGURE A.11: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Analysis



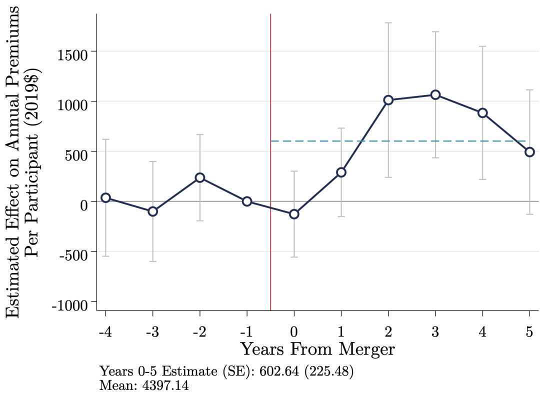
Notes: This figure plots the subsample-specific average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$) estimated from equation (1). The outcome y_{ct} is premiums. Observations are weighted by CZ working-age population in the year before the merger. The first row replicates the baseline estimate for the full sample from Figure 4, which is also represented by the red vertical line. I report coefficients for CZ samples with above and below median CZ size of merger shock as measured by the post-merger change in insurer HHI (580 HHI), number of insurers (median=10 insurers), insurer HHI (2300 HHI), and population (265,000). Standard errors are clustered at the CZ level, and horizontal lines indicate 95% confidence intervals on each coefficient. The sub-sample population-weighted mean premiums is noted in parenthesis next to each sub-sample label.
Source: Form 5500.

FIGURE A.12: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Event Studies

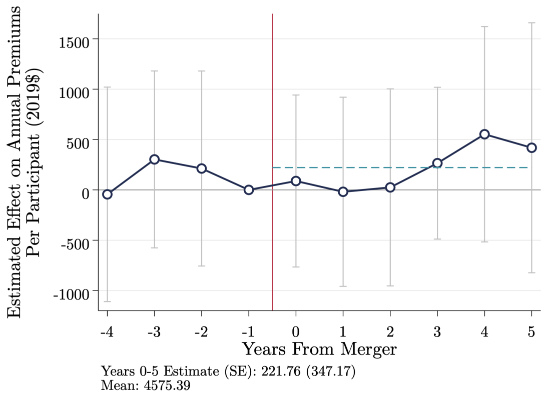
(A) Above Median Size of Merger Shock

(B) Below Median Size of Merger Shock

(C) Above Median Number of Insurers

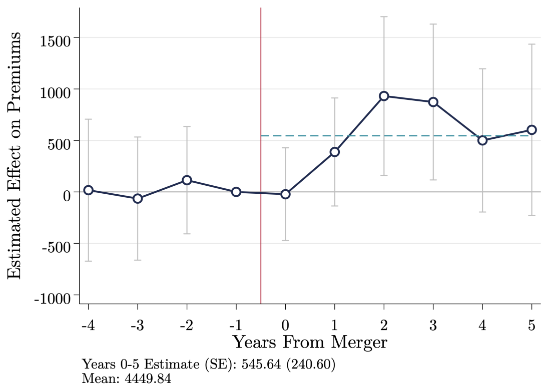


(D) Below Median Number of Insurers



(E) Above Median Insurer HHI

(F) Below Median Insurer HHI

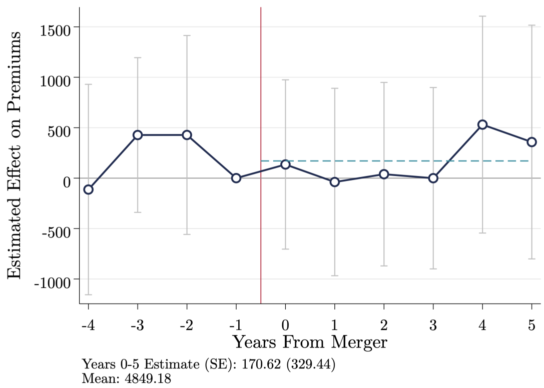


Notes: This figure displays the subsample-specific yearly coefficients β_l from equation (1). The outcome y_{ct} is premiums. I report coefficients for CZ samples with above and below median size of merger shock as measured by the post-merger change in insurer HHI (median=580 ΔHHI), number of insurers in panels C and D (median=10 insurers), and above and below median insurer HHI in panels E and F (2300 HHI). Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2000 dollars. The sample is at the CZ-year level from 1999 to 2020.
Source: Form 5500.

FIGURE A.13: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Analysis Event Studies

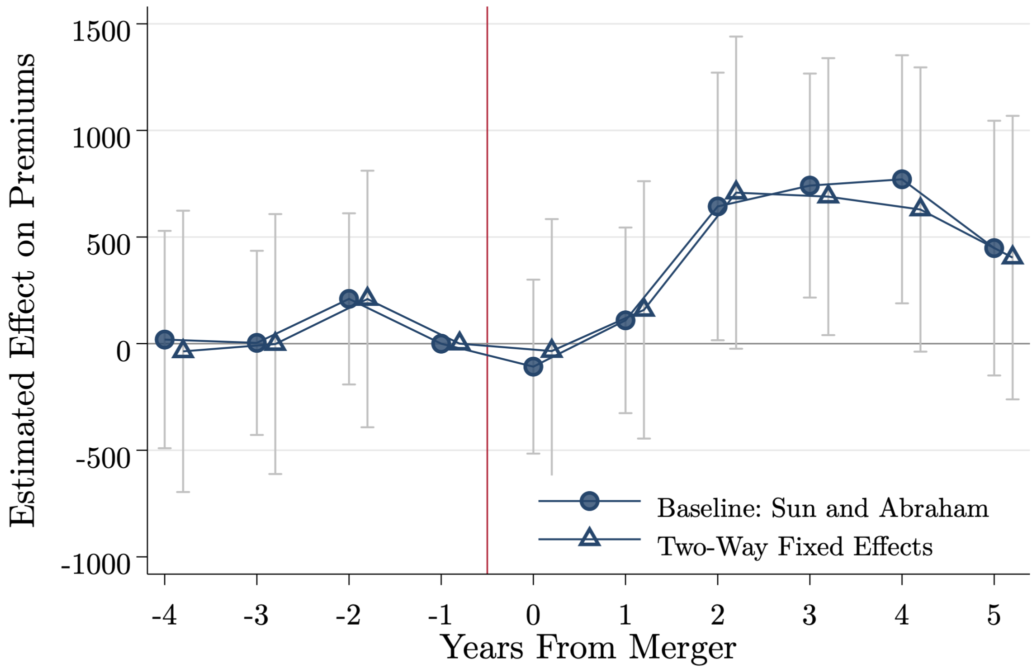
(A) Above Median Population

(B) Below Median Population



Notes: This figure displays the subsample-specific yearly coefficients β_l from equation (1). The outcome y_{ct} is premiums. I report coefficients for CZ samples with above and below median population in panels A and B (265,000). Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2000 dollars. The sample is at the CZ-year level from 1999 to 2020.
Source: Form 5500.

FIGURE A.14: CZ-Level Effect of Insurer Mergers on ESHI Premiums in Vulnerable Firms: Two-Way Fixed Effects Specification



Baseline Years 0-5 Estimate (SE): 434.13 (201.74)
TWFE Years 0-5 Estimate (SE): 425.59 (295.99)

Notes: This figure plots the effect of insurer mergers on premiums. Shaded circle markers report baseline estimates using Sun and Abraham (2021) weights, and triangle markers report two-way fixed effect estimates. Observations are weighted by CZ working-age population in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. Premiums are converted to 2000 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs and 39 control CZs.
Source: Form 5500.

FIGURE A.15: Benchmark Model of Competitive Labor Market

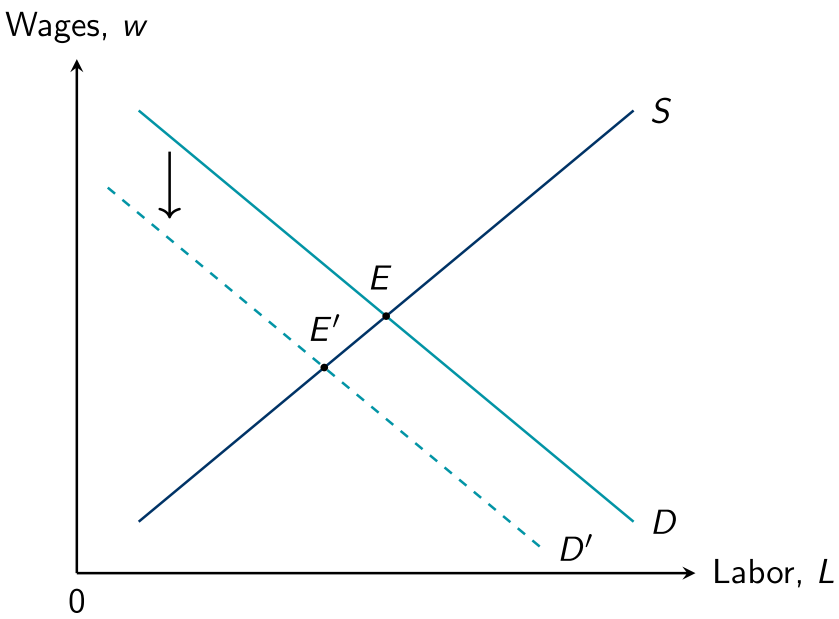
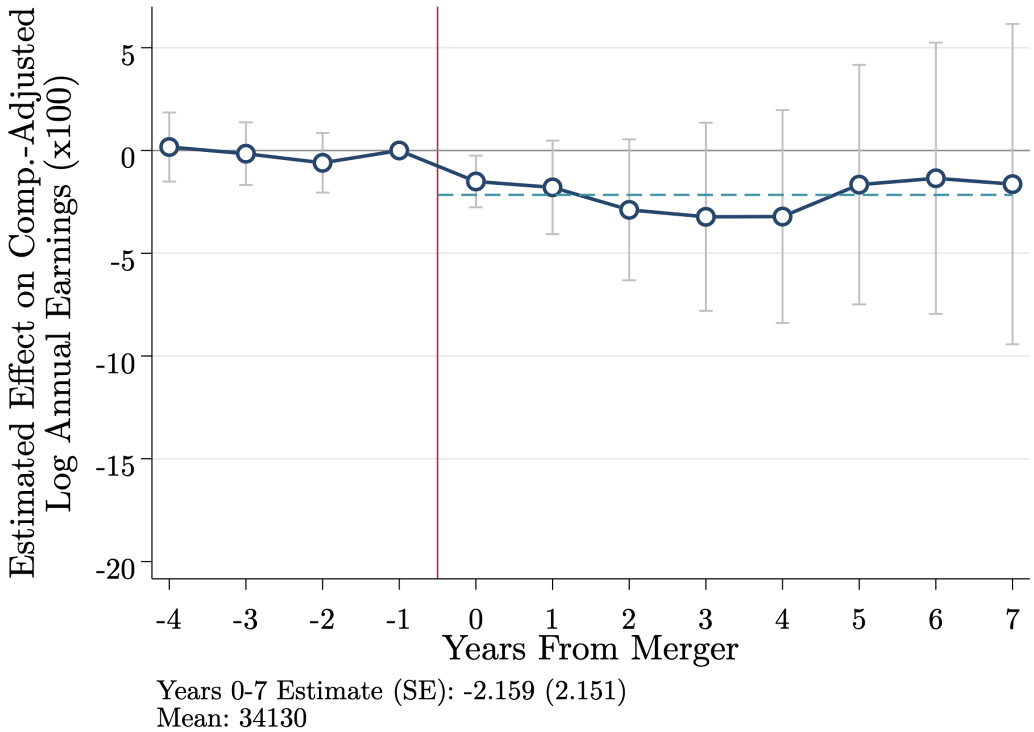
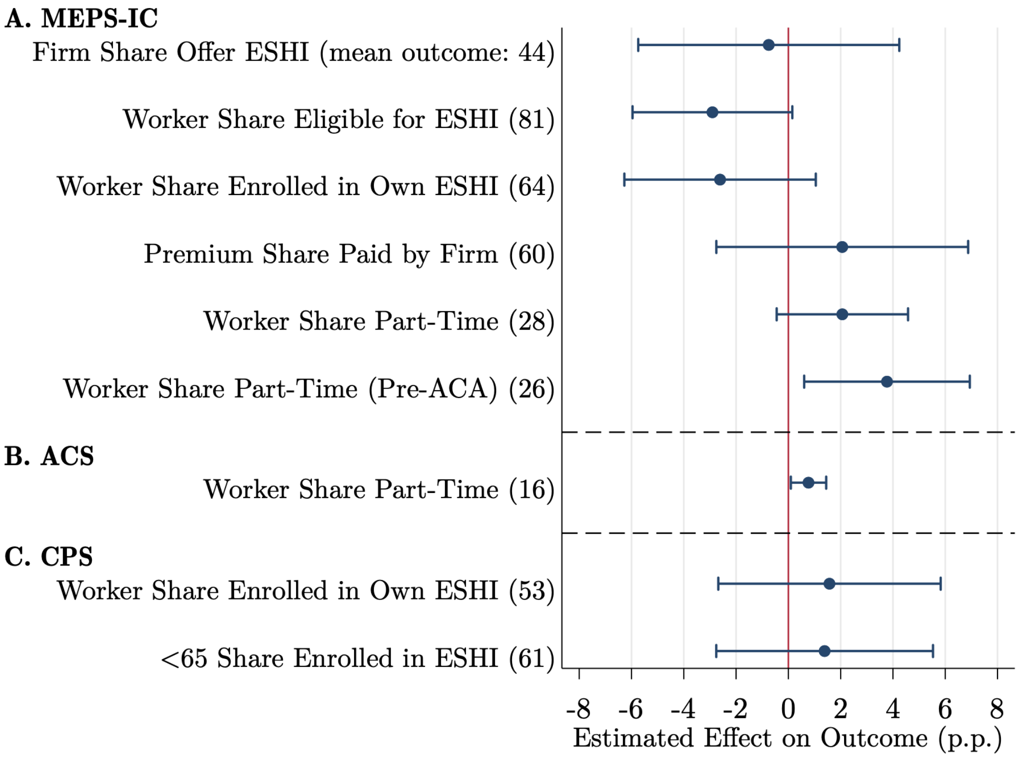


FIGURE A.16: CZ-Level Effect of Insurer Mergers on Log **Composition-Adjusted** Annual Wages in Vulnerable Firms



Notes: This figure plots the yearly coefficients β_l from equation (1), where the outcome y_{ct} is log **composition-adjusted** annual earnings in vulnerable single-establishment firms. Observations are weighted by CZ working-age population in the year before the merger. Coefficients, standard errors, and confidence intervals are multiplied by 100 for ease of interpretation. The point estimate for the average of coefficients from years 0 to 7 following the merger (with corresponding standard error) is reported in the lower left hand corner along with the weighted exponentiated mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. LEHD N=40 treated CZs and 20 control CZs.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics (LEHD).

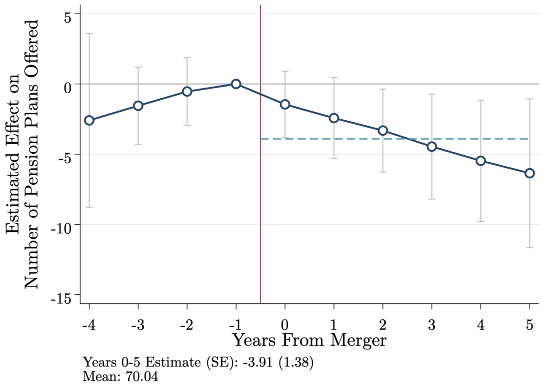
FIGURE A.17: CZ-Level Effect of Insurer Mergers on ESHI Coverage



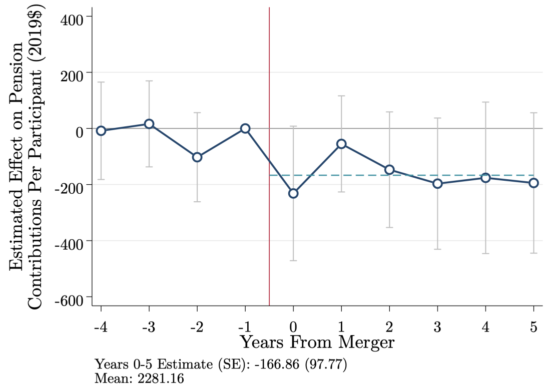
Notes: This figure plots the average of coefficients from years 0 to 5 following the merger ($\frac{1}{5}\sum_{l=0}^{5}\beta_l$) estimated from equation (1). For the ACS sample, I estimate a pooled version of equation (1). The population-weighted mean outcome for treated CZs in the year before the merger is reported in parenthesis. Standard errors are clustered at the CZ level, and horizontal lines indicate 95% confidence intervals on each coefficient. Each sample of firms is at the CZ-year level from 1999 to 2020. The MEPS-IC sample includes single-establishment firms, while the ACS and CPS samples includes workers in all firms. MEPS-IC N=81 treated CZs, 39 control CZs; ACS N=81 treated CZs, 39 control CZs; CPS N=50 treated CZs, 30 control CZs (rounded).
Sources: Form 5500, Medical Expenditure Panel Survey Insurer/Employer Component (MEPS-IC), American Community Survey (ACS), and Current Population Survey (CPS).

FIGURE A.18: CZ-Level Effect of Insurer Mergers on Other Compensation in Vulnerable Firms

(A) Number of **Pension** Plans Offered

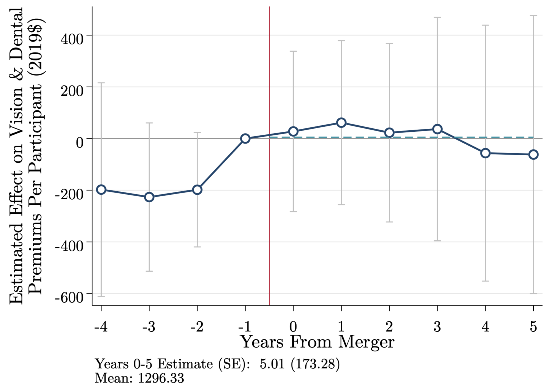


(B) **Pension** Contributions Per Participant (2019$)



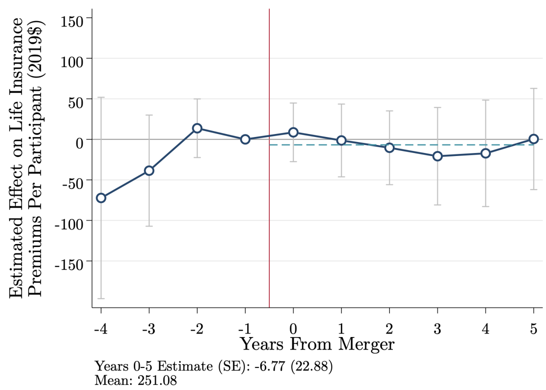
(C) Number of **Vision** & **Dental** Plans Offered

(D) **Vision** & **Dental** Premiums Per Participant (2019$)



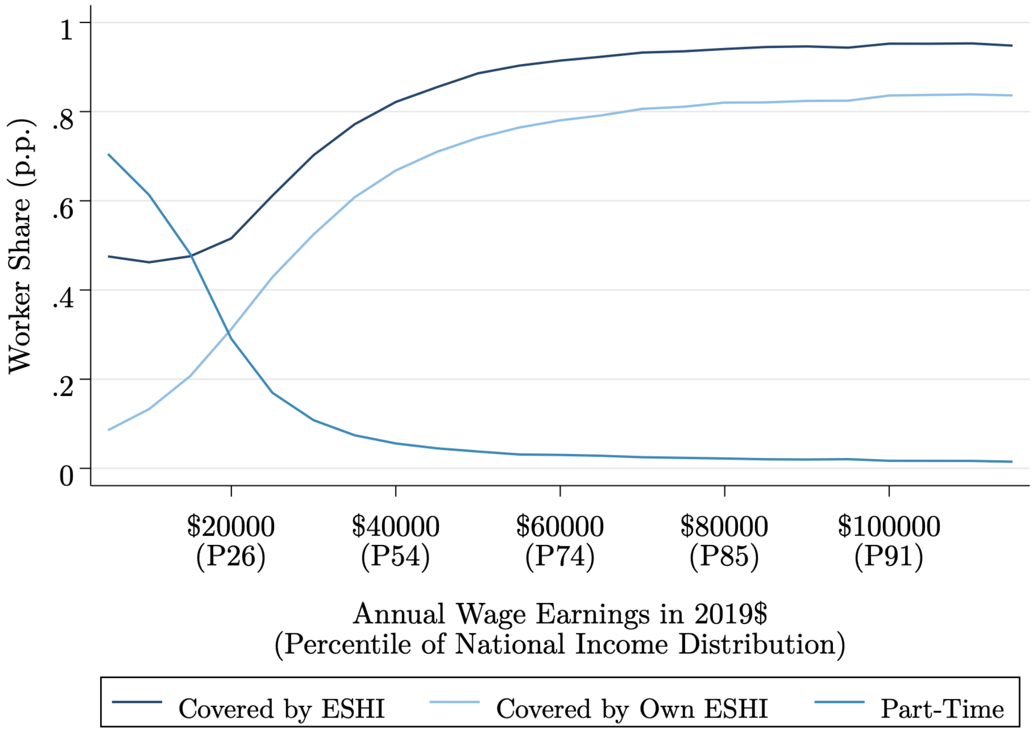
(E) Number of **Life, Sickness, & Accident** Insurance Plans

(F) **Life, Sickness, & Accident** Insurance Premiums Per Participant (2019$)



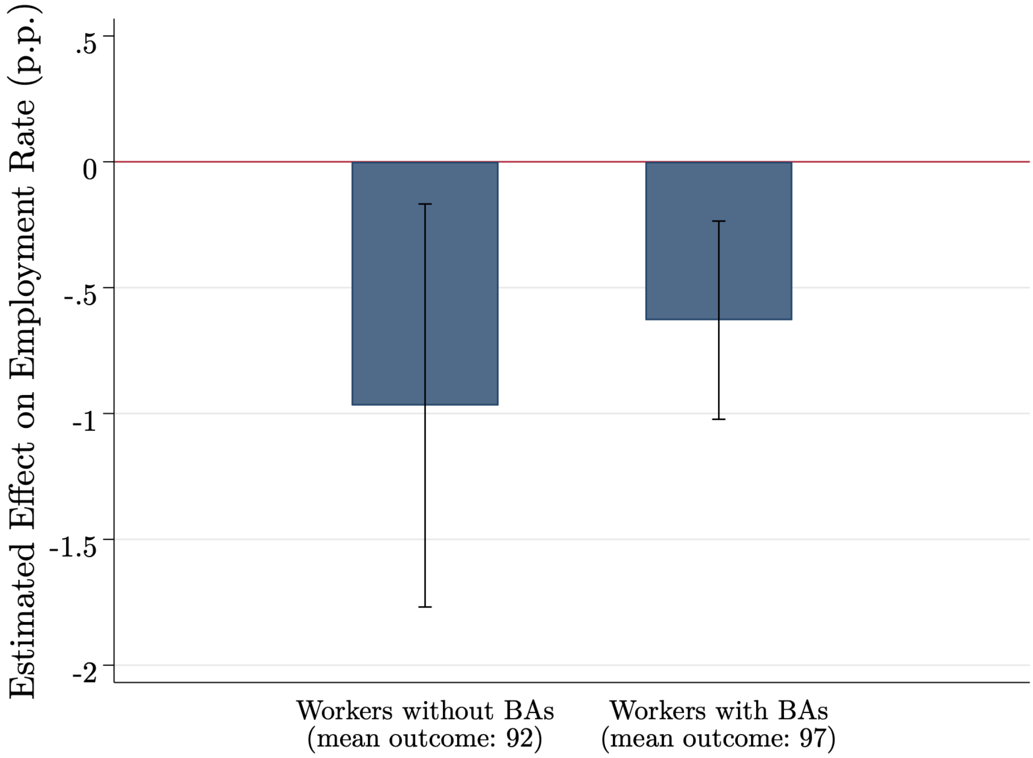
Notes: This figure plots the yearly coefficients β_l from equation (1). Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs, 39 control CZs.
Source: Form 5500.

FIGURE A.19: Worker Share Covered by ESHI and Part-Time, by Annual Wages Earnings



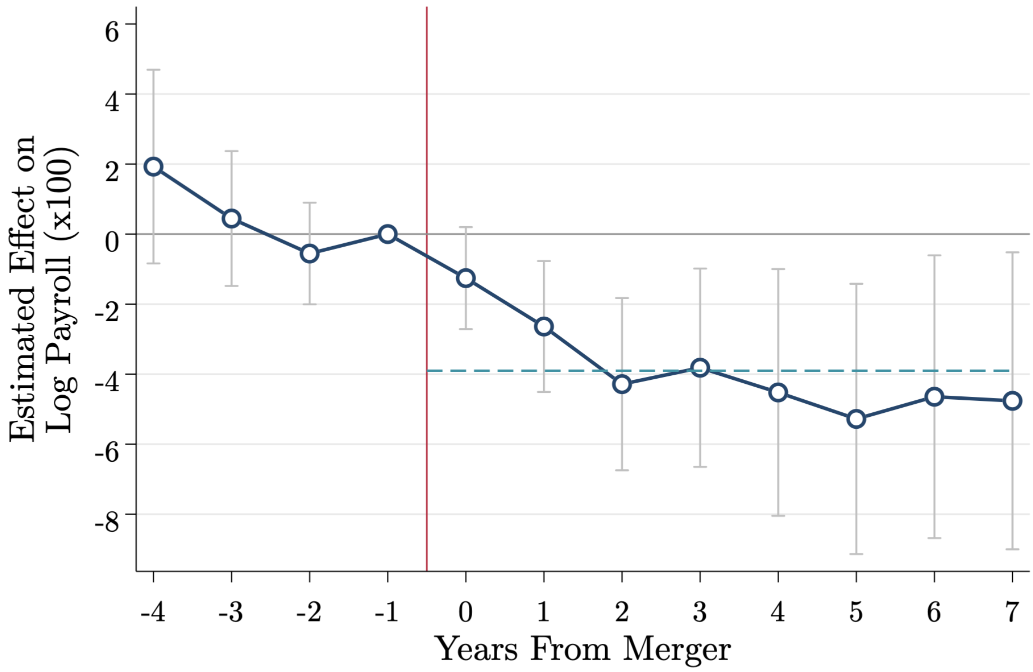
Notes: This figure plots the average share of workers that are covered by employer-sponsored health insurance (any or own plan) and employed part time by workers' annual wage earnings from 1999 to 2021. The corresponding percentile of the national income distribution is reported beneath each wage label in parentheses.
Sources: Current Population Survey (CPS).

FIGURE A.20: CZ-Level Effect of Insurer Mergers on Employment by Worker Education, Using the American Community Survey



Notes: This figure plots the coefficients β from a pooled version of equation (1). The outcome y_{ct} includes the employment rate (share of labor force working in any firm) by workers with and without college degrees. Observations are weighted by CZ working-age population in the year before the merger. The weighted mean outcome for treated CZs in the year before the merger is reported underneath the label of each outcome. Standard errors are clustered at the CZ level, and horizontal blue lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 2005 to 2020. N=43 treated CZs and 32 control CZs.
Source: Form 5500, and American Community Survey (ACS).

FIGURE A.21: CZ-Level Effect of Insurer Mergers on Log Payroll in Vulnerable Firms, Using the Longitudinal Business Database (LBD)



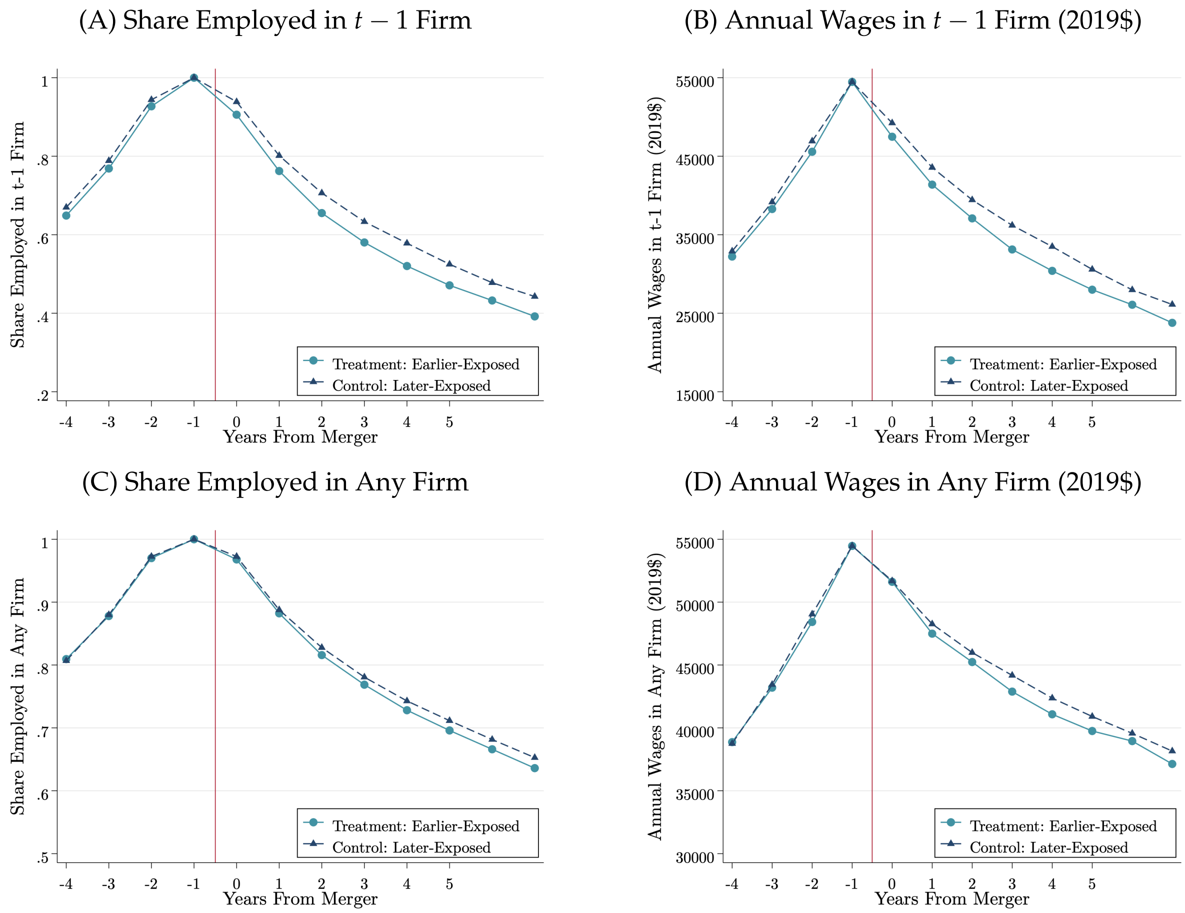
Years 0-7 Estimate (SE): -3.902 (1.370)
Mean: 3,323M

Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is log annual payroll in vulnerable single-establishment firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 7 following the merger ($\frac{1}{8}\sum_{l=0}^{7}\beta_l$). Coefficients, standard errors, and confidence intervals are multiplied by 100 for ease of interpretation. The point estimate for the average of coefficients from years 0 to 7 following the merger (with corresponding standard error) is reported in the lower left hand corner along with the weighted exponentiated mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. LBD N=81 treated CZs and 39 control CZs.
Source: Form 5500, and LBD.

FIGURE A.22: Firm-Level Effect of Insurer Mergers on Vulnerable Firms

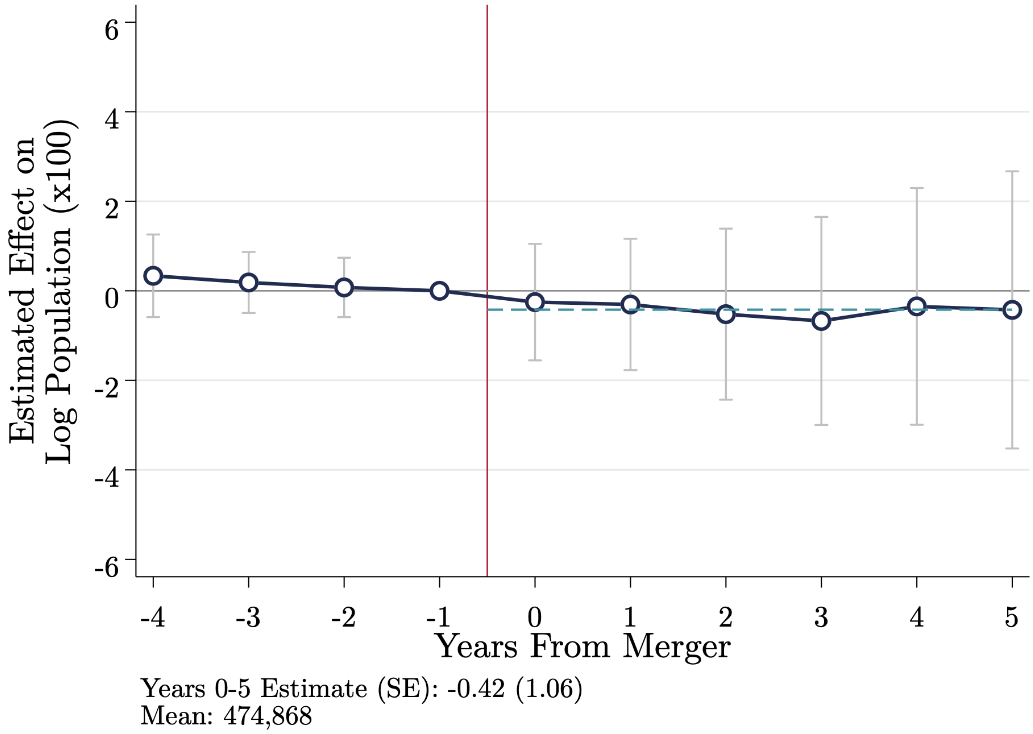
Notes: This figure plots the yearly coefficients β_l from equation (19). The firm-level outcome $y_{j(c)t}$ is employment in all firms (Panel A), the share of active firms with non-zero employment (B), employment in always-active firms (C), and noncollege- and college-educated employment in always-active firms (D). The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 7 following the merger ($\frac{1}{8}\sum_{l=0}^{7}\beta_l$). The point estimate for the average of coefficients from years 0 to 7 following the merger (with corresponding standard error) is reported in the lower left hand corner along with the mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the firm level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the firm-year level from 1999 to 2020, and follows single-establishment firms with 50 or more workers in the year before the merger. LEHD N=40 treated CZs and 20 control CZs.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics (LEHD)..

FIGURE A.23: Worker-Level Raw Trends by Exposure to Insurer Mergers



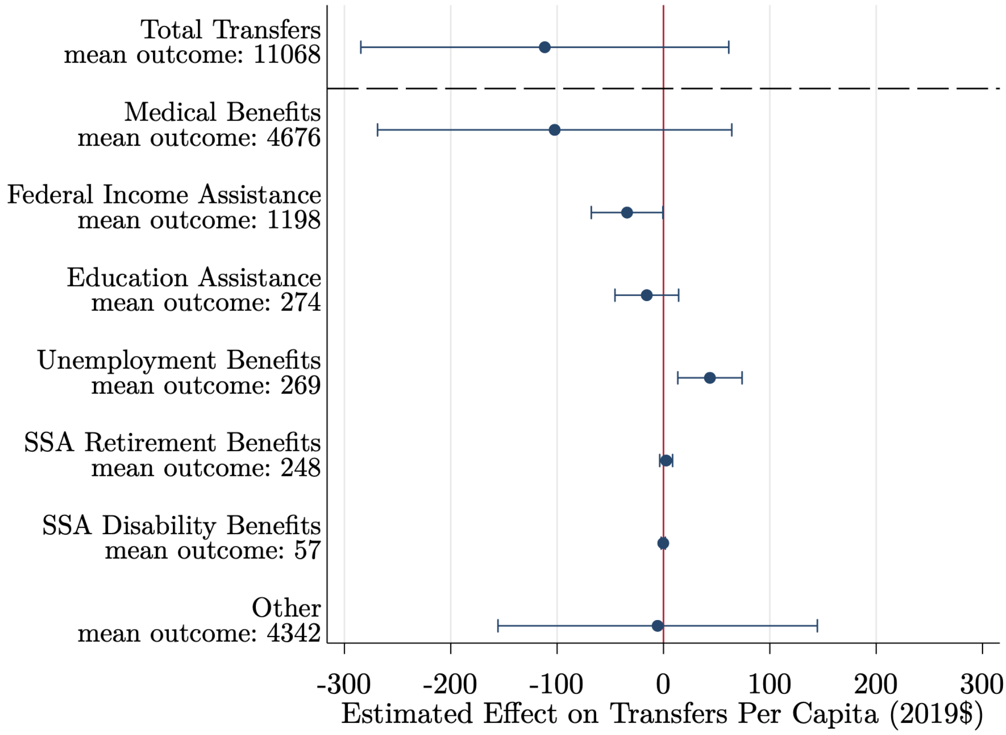
Notes: This figure plots worker-level trends by exposure to insurer mergers. I show mean trends for the share of workers employed in their pre-merger firm in Panel A, annual wages earned in their pre-merger firm in Panel B, share of workers employed in any firm in Panel C, and annual wages earned in any firm in Panel D. Outcomes for workers in earlier-exposed treatment CZs are shown in solid light-blue lines; outcomes for workers in later-exposed control CZs are shown in dashed dark-blue lines. Outcomes for workers in later-exposed CZs are normalized to equal the mean outcome for workers in earlier-exposed CZs in the year before the merger. Nominal dollars are converted to 2019 dollars. The sample is at the worker-year level from 1999 to 2020, and follows working-age individuals employed in single-establishment firms with 50 or more workers in the year before the merger. LEHD N=40 treatment CZs and 20 control CZs in Panels C and D.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics (LEHD).

FIGURE A.24: CZ-Level Effect of Insurer Mergers on Log Population



Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is log working-age population. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 7 following the merger ($\frac{1}{8}\sum_{l=0}^{7}\beta_l$). The estimate (with corresponding standard error) is reported in the lower left hand corner along with the weighted mean outcome for treated CZs in the year before the merger. Standard errors are clustered at the CZ level, and vertical gray lines indicate 95% confidence intervals on each coefficient. The sample is at the CZ-year level from 1999 to 2020. N=77 treated CZs, 39 control CZs.
Sources: Form 5500, and National Cancer Institute SEER Program.

FIGURE A.25: CZ-Level Effect of Insurer Mergers on Transfers Per Capita



Notes: This figure plots the average of coefficients from years 0 to 5 following the merger ($\frac{1}{5}\sum_{l=0}^{5}\beta_l$) estimated from equation (1). The outcome y_{ct} is transfers per working-age capita. The population-weighted mean outcome for treated CZs in the year before the merger is reported in parenthesis. The seven categories of government transfers (ordered in order of decreasing program size) are medical benefits consisting mostly of Medicare and Medicaid, federal income assistance consisting mostly of the EITC, SSI, SNAP, and AFDC/TANF programs, education assistance, state and federal unemployment insurance benefits, SSA retirement benefits, SSA disability insurance benefits, and all other unclassified benefits. Observations are weighted by CZ working-age population in the year before the merger. Standard errors are clustered at the CZ level, and horizontal lines indicate 95% confidence intervals on each coefficient. Nominal dollars are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2020. N=81 treated CZs, 39 control CZs.
Source: Form 5500, U.S. Bureau of Economic Analysis, Social Security Administration (SSA).

J. Additional Tables

TABLE A.4: Worker-Level Effect of Insurer Mergers on Employment and Wages

	Share Working in $t-1$ Firm (p.p.) (1)	Annual Wages in $t-1$ Firm (2019$) (2)	Share Working in Any Firm (p.p.) (3)	Annual Wages in Any Firm (2019$) (4)
	Panel A. All Workers			
$1(\text{Merger}_{i(c)}) \cdot \mathbb{1}(\text{Year}_t \geq 0)$	-2.628***	-1,579***	-0.381***	-382.3**
	(0.0949)	(120.8)	(0.0808)	(145.1)
Implied Percent Change		2.898		0.702
Outcome Mean		54,470		54,470
Observations	14,790,000	14,790,000	14,790,000	14,790,000
R^2	0.535	0.503	0.482	0.375
	Panel B. Workers with Bottom-Quartile Pre-Merger Wages			
$1(\text{Merger}_{i(c)}) \cdot \mathbb{1}(\text{Year}_t \geq 0)$	-4.09***	-1571***	-1.487***	-881.9***
	(0.194)	(59.08)	(0.177)	(61.75)
Implied Percent Change		7.557		4.242
Outcome Mean		20,790		20,790
Observations	3,697,000	3,697,000	3,697,000	3,697,000
R^2	0.552	0.563	0.486	0.544
	Panel C. Workers with Top-Quartile Pre-Merger Wages			
$1(\text{Merger}_{i(c)}) \cdot \mathbb{1}(\text{Year}_t \geq 0)$	-1.626***	-1355***	0.411***	750.0
	(0.191)	(457.3)	(0.155)	(554.2)
Implied Percent Change		1.289		0.714
Outcome Mean		105,100		105,100
Observations	3,696,000	3,696,000	3,696,000	3,696,000
R^2	0.523	0.445	0.486	0.32

Notes: This table reports the β coefficients estimated from equation (20) at the worker level. The worker-level outcomes $y_{i(c)t}$ are share of workers employed at the pre-merger firm (column 1), annual wages earned at the pre-merger firm (column 2), the share of workers in any firm (column 3), and annual wages earned at any firm (column 4). The sample in Panel A includes all working-age individuals employed in single-establishment firms with 50 or more workers in the year before the merger. Panel B and C show results for workers in the bottom and top pre-merger wage quartile. Standard errors are clustered at the worker level and reported in parentheses. Nominal dollars are converted to 2019 dollars. The sample is at the worker-year level from 1999 to 2020. LEHD N=40 treatment CZs and 20 control CZs in Panels C and D.
Sources: Form 5500, and Longitudinal Employer-Houshold Dynamics (LEHD).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.5: Worker-Level Effect of Insurer Mergers on Out-Migration from Exposed CZs

	Probability Migrated $t-1$ CZ of Residence by $t+5$ (1)	Probability Migrated $t-1$ of CZ Work by $t+5$ (2)
$\mathbb{1}(\text{Merger}_{i(c)})$	-0.0148***	0.00239***
	(0.000137)	(0.0008908)
Outcome Mean	0.195	0.505
Observations	59,830,000	2,164,000
R^2	0.001	0.001

Notes: This table reports the β coefficient of the effect of insurer mergers on out-migration from the following individual-level model:

$$\mathbb{1}(Migrated_i) = \beta[\mathbb{1}(\text{Merger}_{i(c)})] + \varepsilon_i$$

where $\mathbb{1}(Migrated_{i(c)})$ is an indicator for individual i having migrated between event year $t-1$ to $t+5$, $\mathbb{1}(\text{Merger}_{i(c)})$ is an indicator for individual i living in an exposed CZ c in $t-1$. I cluster standard errors at the individual level. In column (1), I focus on a panel of individuals living in an exposed CZ in the year before the merger ($t-1$) and ask whether they are living in the same CZ 6 years later ($t+5$). In column (2), I focus on a panel of individuals working in an exposed CZ the year before the merger ($t-1$) and ask whether they are working in the same CZ 6 years later ($t+5$). The sample is at the individual level from 1999 to 2021. N= 81 treated and 39 control CZs in column 1; N=40 treated and 20 control CZs in column 2.
Sources: Form 5500, and Longitudinal Employer-Household Dynamics (LEHD).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.6: Benchmarking the Implied Labor Supply Elasticity of Vulnerable Firms

Study	$\frac{\%\Delta \text{Emp}}{\%\Delta \text{Net Wages}}$	s.e.	Identification and Population
A. ESHI Costs			
1. My Estimate (2024)	1.3	0.78	U.S. insurer mergers 1999-2020
2. Gao et al. (2023)	0.48	0.18	U.S. insurers' past losses 2012-2019
3. Cooper et al. (2024)	1.0	0.60	U.S. hospital mergers 2008-2017
B. Payroll Taxes			
4. Gruber (1997)	0.01	0.10	Chile payroll tax cut 1979-1985, manufacturing firms
5. Saez et al. (2019)	2.4	0.21	Sweden younger worker payroll tax cut 2002-2013
6. Ku et al. (2020)	4.3	3.3	Norway place-based payroll tax cut 2000-2006
7. Lobel (2023)	4.2	0.2	Brazil sector-specific payroll tax cut 2008-2017

Notes: This table compared my implied labor supply elasticity of vulnerable firms to elasticities from existing studies of rising ESHI costs (panel A) and payroll taxes (panel B). Elasticities are defined as the log change in the employment rate divided by the log change in net-of-average-tax(-and-ESHI) wages. Column 2 shows the point estimate of the elasticity, column 3 shows the associated standard error of the point estimate.

Sources: See Appendix Section 9 for derivations and sources of estimates.