Causes and Consequences of Rising Employer-Sponsored Health Insurance Costs: Evidence from Insurer Mergers*

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Abstract

U.S. employer-sponsored health insurance costs have quadrupled over the past four decades, placing a significant burden on employers. This paper asks how these rising costs impact U.S. labor markets. I exploit local differences in exposure to national health insurer mergers between 1999 and 2019. Using new administrative data and a difference-in-differences research design, I estimate that insurer mergers account for 22 percent of the overall cost increase in the past two decades. Firms facing higher costs experience employment losses, concentrated among middle-income workers without a college education. I calculate an estimated loss of 5.2 percent for less-educated workers. While some workers reallocate between firms, aggregate employment declines within merger-exposed markets. The resulting increase in unemployment raises government spending on unemployment insurance by 15 percent. Compared to Canada, where health insurance is government-funded, U.S. workers without a college education have experienced 3 percentage points more job losses than their Canadian counterparts over the past two decades. Incorporating my findings into a competitive labor market model, I show that rising health insurance costs explain 44 percent of this excess job loss.

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1. Introduction

Employer-sponsored health insurance (ESHI) costs in the U.S. have increased fourfold since the 1980s, accounting for 10 percent of the average firm's labor costs in 2019.[1] Given that these costs are workplace-financed, a natural question arises: how has the striking increase in ESHI costs impacted U.S. firms and workers? By raising labor costs, we might expect an increase in ESHI costs to depress wages and employment. And because firms pay a fixed cost toward each worker's health plan regardless of income – incurring the same cost for a low-paid janitor as for a highly-paid executive – lower-income workers may be disproportionately affected.

One potential contributor to rising ESHI costs is consolidation in the private health insurance industry. As ESHI costs have steadily increased, insurer mergers have contributed to less competitive and more concentrated markets (Figure 1). These trends may be causally linked, as reduced competition could drive up health insurance premiums by strengthening the bargaining leverage of insurers during negotiations with firms (Dafny et al., 2012). However, premium increases from insurer mergers could be offset by insurers negotiating lower medical payments with physicians and hospitals, or realizing cost savings through economies of scale (Ho and Lee, 2017). Given the private health insurance industry's size (5% of GDP in annual expenditures) and scope (insuring 60% of non-elderly Americans), understanding whether and how rising ESHI costs from insurer mergers affect the labor market is crucial for policymakers and antitrust authorities.[2]

This paper estimates the effect of rising ESHI costs on U.S. local labor markets. I exploit local differences in exposure to national mergers over the last two decades. Merger shocks within affected markets are sizable as around 36 percent of workers are employed by fully-insured firms. These firms purchase plans from insurers and negotiate premiums with insurers, making them particularly vulnerable to reduced competition. Using a difference-in-differences research design and novel administrative data on employer health plans, I compare the premiums of fully-insured firms in earlier-exposed commuting zones (CZs) to those in later-exposed commuting zones before and after insurer mergers. Next, I use matched employer-employee Census data to examine how merger-induced increases in ESHI costs impact the wages and employment of affected firms and decompose effects by worker education and income. Finally,

[1] Figure calculated from the Bureau of Labor Statistics (BLS) Employer Costs for Employee Compensation (accessed at: https://www.bls.gov/ecec/tables.htm).

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

to study how antitrust intervention may remedy the labor market effects of merger-induced ESHI cost increases, I build and calibrate a structural model of a competitive labor market and counterfactually simulate premiums growth.

My data on insurer mergers combines the National Association of Insurance Commissioners' directory of health insurance companies with merger data from Thomson Reuters between 1999 and 2021. I identify a market as exposed to a merger if both merging insurers hold market shares in the market before the merger. After the merger, remaining insurers can exercise increased market power as the number of insurers has fallen by one. To ensure local exposure to mergers is plausibly exogenous, I focus on mergers between national insurers, of which there have been over 100 since 1999. The approach mitigates concerns that consolidation is driven by unobservable local factors in a given market. For example, in the 2004 merger between WellPoint and Anthem, each insurer served 200 CZs out of a total of 741 CZs. The broad coverage of both insurers makes it unlikely that the merger decision was driven by local changes in the 94 CZs where their market shares overlapped before the merger.

I begin by estimating the impact of insurer mergers on premiums. To isolate the causal impact of mergers, my empirical strategy compares outcomes in CZs exposed to a merger for the first time within the observation period to those in later-exposed CZs. I 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 fully-insured firms by 10 percent, without improving plan quality. Insurers both directly and not directly involved in mergers raise their premiums. The finding is consistent with all remaining insurers benefiting from reduced competition and exercising more market power. As a result, fully-insured firms in exposed markets face large premium increases, regardless of which insurer provides their plan. In response to higher premiums, firms offer lower-cost plans with restrictive provider networks. The shift to lower-quality plans suggests that insurers extract rents from mergers without benefiting consumers. My estimates suggest that insurer mergers account for 22 percent of the overall premium growth over the last two decades. Specifically, insurer mergers can explain $1,300 of the overall premium increase of $5,700 per insured worker in the average CZ from 1999 to 2019, amounting to approximately $100 billion after aggregating across all CZs

and insured workers.[3]

What are the effects of these merger-induced premium increases on firms and workers? I aggregate matched employer-employee Census data to the CZ level to estimate the difference-in-differences over a sample of workers in single-establishment firms within merger-exposed CZs. Because of their smaller size, single-establishment firms are typically fully insured, making them more vulnerable to insurer mergers.

I show that firms facing higher premiums experience employment losses following insurer mergers. I estimate that less-educated workers experience a 5.2 percent loss in employment, compared to a 2.5 percent loss for their more-educated counterparts. Losses disproportionately affect workers without a college degree, as they become relatively more expensive to hire. Furthermore, because low-income workers are less likely to be covered by employer-sponsored insurance, middle-income workers without a college degree bear the brunt of the employment losses. The findings indicate that rising premiums contribute to a 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 also show that firms hire more part-time workers to offset the burden of rising premiums, as part-time workers are not typically entitled to health insurance benefits.

I then ask how the aggregate local labor market is affected by the large employment losses. Individuals in the growing unemployment pool may respond by migrating to a different CZ, seeking work at a less vulnerable multi-establishment firm within the same CZ, or remaining unemployed and taking up government transfers. I find a partial reallocation of workers from single- to multi-establishment firms. Aggregate employment falls within affected CZs by 1.2 percent and government spending on unemployment insurance increases by 15 percent. Thus, the consequences of rising ESHI costs extend beyond firms and workers, as governments face increased fiscal burdens from higher unemployment insurance expenditures.

Finally, I extend and calibrate a structural model of a competitive labor market model in Finkelstein et al. (2023). I simulate employment under counterfactual growth paths of premiums if antitrust authorities had blocked the mergers. I compare the U.S. to Canada to quantify the impact of rising ESHI costs from insurer mergers on the U.S. labor market. Over the last

[3]Figures reported in 2019 dollars. The average CZ has approximately 350,000 individuals under the age of 65, 60 percent of whom are covered by employer-sponsored health insurance. Assuming half of these individuals directly receive health insurance from their employer and given 741 CZs across the U.S., the aggregate merger-induced increase is \$100 billion ($= 380,000 \cdot 0.6 \cdot 0.5 \cdot 1,300 \cdot 741$).

two decades, U.S. workers have experienced more job losses than their Canadian counterparts. I ask how much smaller the excess job losses among U.S. workers would have been under antitrust intervention. As used in Finkelstein et al. (2023), Canada serves as a natural counterfactual to the U.S. because of its geographic proximity and similar economic, legal, and political systems, but vastly different methods of financing healthcare. While most non-elderly Americans are covered by private health insurers, which creates opportunities for market players to engage in rent-seeking behavior, non-elderly Canadians are generally covered by public insurance.

Blocking insurer mergers through antitrust intervention would have reduced the 3 percentage point excess job loss among U.S. workers without a college degree by 10 percent, by slowing the growth of premiums from 1999 to 2019. Blocking insurer mergers would also have decreased the 1.5 percentage point excess job loss among U.S. workers with a college degree by 8.6 percent. A more realistic counterfactual, where antitrust policy blocks mergers that would have increased the Herfindahl-Hirschman Index by at least 200, would have yielded similar reductions in excess job losses. These results suggest that antitrust policy targeting the most concentration-increasing mergers would have been highly effective in mitigating the effects of insurer consolidation.

To broadly assess the consequences of rising ESHI costs, I examine a no-growth counterfactual in which U.S. premiums remained at their 1999 level. I show that rising employer-sponsored premiums may explain 44 percent of the excess job losses for less-educated workers and 40 percent for more-educated workers from 1999 to 2019. These results suggest that rising ESHI costs contribute to job losses on par with other leading factors, such as robot adoption and trade shocks.

This paper makes three main contributions to the literature. First, the best prior evidence on the effects of insurer mergers are case studies that rely on proprietary data obtained from benefits consulting firms. Due to the limited data availability, each case study focuses on a single merger: 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 show that premiums increased after these specific insurer mergers. However, it is unclear whether we should extrapolate the findings especially because the premium effects of mergers are theoretically ambiguous, as shown in Ho and Lee (2017). They draw on proprietary data from California's

state and public agency employee database to build and calibrate a structural model of health-care markets. They predict that consolidation may increase or even decrease premiums but do not validate these predictions using actual merger shocks.

I use new administrative Form 5500 (F55) data on employer health plans collected by the Department of Labor and Internal Revenue Service.[4] Using the F55, I exploit mergers over the last two decades to provide the first comprehensive evidence that insurer mergers increase employer-sponsored premiums. In turn, I show that rising premiums from insurer mergers erode U.S. employment, an important consequence not explored in the prior case studies.

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 on 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 increases in premiums affecting relatively few firms to draw conclusions about how the dramatic rise in premiums has impacted labor market inequality. Specifically, Gao et al. (2022) study a 2 percent increase in premiums for employers whose insurers experience significant losses in past profits, and Brot-Goldberg et al. (2024) investigate a 1 percent increase in premiums due to mergers between local hospitals, which are likely endogenously driven by local conditions and only affect employers whose workers use the merged hospital's services.

This study complements past work examining the labor market effects of rising ESHI costs by making three contributions. First, I exploit plausibly exogenous and large increases in premiums of 10 percent from national insurer mergers to provide new evidence in a unified difference-in-differences framework. Second, using an array of publicly available and administrative data, I provide a comprehensive assessment of firm and worker responses to rising ESHI costs. I analyze new outcomes including health plan quality, employer pension contributions, health insurance coverage, and hours worked. Third, I combine reduced-form estimates with 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).

[4] My data also has broader coverage than data used in past papers. The F55 data used in this paper includes 60,000 fully-insured plans covering 32 million lives annually from 1999 to 2020. In comparison, Dafny et al. (2012) use data covering 10 million participants from 1999 to 2006, Guardado et al. (2013) use data on 187 health plans from 2007 to 2008, and Ho and Lee (2017) use data covering 1.2 million participants who are California public employees and their dependents in 2004.

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 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, with around 60 percent of Americans under 65 receiving health insurance through their own employer or a family member's employer (KFF, 2023). From 1999 to 2019, annual inflation-adjusted premium costs doubled, rising from approximately $6,000 per worker in 1999 to $11,700 per worker in 2019 (Figure 1). In other developed countries, such as Canada and the U.K., where health insurance is primarily funded by the government, healthcare spending has grown more slowly.

Although firms are not legally required to offer ESHI, 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). Effective in 2015, the Affordable Care Act introduced penalties for firms with 50 or more full-time workers that failed to offer coverage to workers working 30 or more hours. However, the legislation had limited bite on firms' decisions to offer health insurance, as most companies with 50 or more workers already provided coverage (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). Among firms offering health insurance, 86 percent do not offer benefits to part-time workers (KFF, 2023). Within firms offering ESHI, 80 percent of workers are eligible and 80 percent of eligible workers take up ESHI (KFF, 2023).

Firms pay the same fixed cost for workers' health insurance plans regardless of income due to nondiscrimination rules under IRS Code section 105(h), which prohibits firms from providing more generous coverage to highly-paid workers. Firms and workers both contribute

to annual premiums for health insurance plans, with firm contributions accounting for roughly 75 percent of total annual premiums (KFF, 2023).

There are two main ways for an firm to finance health insurance, which affects their degree of vulnerability to insurer mergers. First, 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. Second, firms can pay for health care directly from their own funds, referred to as a "self-insured plan" as firms bear the financial risk. Self-insured firms are less vulnerable because they determine their own premiums. However, self-insured may still be affected, as they often purchase stop-loss insurance from insurers and pay insurers to administer health plans on their behalf. 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 fully-insured plan, 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 contributed to less competitive and more concentrated markets. Since 1999, there have been over 130 mergers between national or regional health insurers (see Figure 2). As premiums have steadily risen, the average number of insurers in markets has fallen from 16 in 2000 to 11 in 2019 (Figure 1). Over the same period, median insurer concentration increased by 555 points on a base of 2,778 Herfindahl-Hirschman Index (HHI) points in 2000 (Appendix Figure A.4), far exceeding 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. The goal of this paper is to measure the extent to which insurer mergers have contributed to rising premiums and study firm and worker responses to merger-induced changes in premiums.

Antitrust authorities have take relatively few actions to prevent insurer mergers. Merger deals in which the target's assets are above a certain threshold are required to notify the DOJ and FTC, who decide whether to take enforcement actions. Actions have led to three cases in which deals were modified, one case in which the deal was abandoned, and three cases in which mergers were blocked.[5] The blocked mergers include proposals between CareFirst and

[5]To merge with Prudential, Aetna was required to divest its NYLCare businesses in Houston and Dallas-Fort

WellPoint in 2001, Cigna and Anthem in 2017, and Aetna and Humana, in 2017.

Policymakers have more recently expressed concerns that reduced competition is increasing ESHI costs, but the effects of rising ESHI costs on firms and workers have been largely overlooked. For example, in a 2023 letter to the FTC Chair Lina Kahn, Senator Elizabeth 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".[6]

3. Data and Empirical Strategy

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

3.1 Data

Insurer Mergers. I assemble an ownership panel to identify mergers between health insurers from 1999 to 2020, based on the National Association of Insurance Commissioners' (NAIC) directory of health insurance companies. I infer mergers by identifying changes in the parent company. I verify the timing of mergers with Thomson Reuters's Securities Data Company Merger and Acquisitions database, newspaper articles, and insurers' own websites.

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).[7] Firms with 100 or more participants (workers and their dependents) are required to file a F55 annually. The F55 covers about 60 percent of all participants in group-health plans (Walsh, 2023). Enrollment in the 2.5 million group-health plans in 2019 amounted to roughly 139 million lives covered, of which about 79 million were covered by one of the 65,800 plans that filed a F55 (Walsh, 2023).

Worth in 1999; Pacificare divested a share of its business in Tuscon, Arizona and Boulder, Colorado and United divested all of PacifiCare's small-group business in the Tuscon area in 2006; to merge with Harvard Pilgrim, Tufts was required to divest its New Hampshire subsidiary in 2021. Independence Blue Cross and Highmark called off their merger deal in 2009 after the Pennsylvania state insurance commissioner required the insurers to give up either the Blue Cross or Blue Shield trademark.

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

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

I focus on firms offering fully-insured plans, which include 60,000 plans covering 32 million lives annually, as these firms are the most vulnerable to insurer consolidation. 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. Due to the size filing exemptions, the data is a sub-sample of medium-sized firms offering fully insured plans. I later use the F55 to examine plan outcomes for self-insured firms; however, my analysis is limited by the fact that F55 data understates self-insured plan outcomes.[8]

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 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). HMO plans only cover care received from in-network providers, while other types of plans, such as Preferred Provider Organizations (PPO) plans, cover out-of-network care. HMO plans also require referrals through primary care physicians for specialist care. I use the Medical Expenditure Panel Survey Insurance/Employer Component firm-level survey to construct quality and coverage outcomes not measured in the F55 collapsed to the CZ level, and use the Current Population Survey March Supplement to construct the CZ share covered by ESHI.

Labor Outcomes. I use the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (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.[9]

[8]Specifically, the F55 data only requires self-insured employers to record premiums or claims paid from financial trusts and not general assets. Half of the self-insured plans that filed a F55 in 2020 did not hold assets in a trust, so did not report financial information (Walsh, 2023).

[9]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.

To assess the effects on workers in firms that are more vulnerable to mergers, I create a panel of workers employed in single-establishment firms, which I then aggregate to the CZ level for my analysis. Because the F55 only collects data on a sub-sample of fully insured firms and only some of these can be identified by linking the F55 with the LEHD, I take a broader approach and focus on single-establishment firms employing workers aged 18 to 64 ("working age") in my baseline analysis.[10] Due to their smaller size, single-establishments tend to offer fully-insured plans, making them 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. To aggregate annual wages to the CZ level, I sum wage earnings across quarters and jobs for each worker employed in single-establishment firms and average wages across workers in a CZ.[11] I also observe worker demographics (age, race/ethnicity, sex, and education). Missing demographic 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. 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 working-age population share employed in multi-establishment firms to assess between-firm effects. Second, 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 by drawing on data from the American Community Survey (ACS).

Additional Outcomes. I use the County Business Patterns (CBP) and the U.S. Census Bureau's Longitudinal Business Database (LBD) to replicate my LEHD results for all states. Cov-

[10] The F55 and LEHD is linked via the Employer Identification Number. Issues with linking the F55 and LEHD arise as when companies change EINs or have multiple EINs, they may reallocate the reporting of health insurance plan participants between these EINs over time.

[11] 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).

ering all private, non-farm U.S. employment, the establishment-level LBD data allows me to construct annual firm-level employment (measured during the week of March 12th) and payroll measures 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's SEER Program to calculate the annual working age population in CZs.

3.2 Empirical Strategy

3.3 Identifying Exposure to Insurer Mergers

Firms are exposed to an insurer merger if they are located in a CZ where both merging insurers have market shares before the merger. After the merger, one fewer insurer in these exposed CZs leads to decreased competition and increased market concentration, enabling the newly merged insurer and its rivals to exercise greater bargaining power. 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 and 437 CZs that are never exposed, which account for 85 and 15 percent of the U.S. population respectively.

To ensure that merger shocks are plausibly exogenous, I focus on large mergers between national or multi-state insurers that serve extensive networks of CZs. The large and national scope of these mergers means that changes in local market conditions are unlikely to affect insurers' decisions to merge within a given local market where they have overlapping pre-merger market shares. In contrast, two small insurers serving the same market may decide to merge based on their expectations of future premium growth in the local market. Due to high barriers to market entry, merging insurers cannot easily enter markets to manipulate the set of CZs that would subsequently experience increased local market concentration due to the merger. One reason for high barriers to entry is that insurers must establish relationships with new downstream employers and upstream hospitals and medical providers when entering a new market. Another reason is because insurance is primarily regulated at the state level, insurers that are domiciled in one state must obtain a license to operate in another state where they wish to sell insurance products. Hence, insurers must navigate a complex framework of state regulations when expanding their operations across state lines.

There are two challenges with measuring the causal impact of insurer mergers. First, ex-

posed CZs often experience successive mergers over time. Of the 246 exposed CZs, 135 CZs experienced more than 1 merger, and the average exposed CZ experienced 4 mergers between 1999 to 2020. I overcome the challenge by focusing on "earlier-exposed" CZs that experience a first observed 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. The distribution of my sample of mergers is very similar to the distribution of all mergers over the years, shown in Figure 2.

Second, 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). I address the challenge by comparing firms in each earlier-exposed CZ 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 each earlier-exposed CZ. I identify 39 later-exposed CZs.[12]

In Table 1, I show that earlier- and later-exposed commuting zones have similar observable characteristics. For my main sample, Panel A shows earlier- and later-exposed CZs have comparable levels of average insurer concentration (12.9 vs. 10.4 insurers; 2,800 vs. 2,690 HHI points), annual premiums for fully-insured firms ($4,700), household incomes ($54,000), unemployment rates (6.8 vs. 6.0), and rural shares (0.5), but earlier-exposed CZs have larger population sizes than later-exposed CZs (481,000 vs. 367,000 residents). Panel B shows that earlier- and later-exposed CZs in the LEHD panel, available in only 25 states, have smaller population sizes (276,000 vs. 200,000). Single-establishment firms in earlier-exposed LEHD CZs employ a larger share of the population (45 percent) relative to those in later-exposed LEHD CZs (34 percent), but pay similar wages ($35,000).

3.4 Difference-in-Differences Event Study

I use a difference-in-differences event study design to estimate the effects of insurer mergers on outcomes in earlier-exposed CZs relative to outcomes in later-exposed CZs, before and after in-

[12] In Appendix Section B, I vary the group of counterfactual CZs, including up to 341 control CZs. Regardless of the choice of control CZs, I observe a similar (although noisier) pattern for premium estimates, while employment estimates remain robust. The positive pre-trend when comparing earlier-exposed CZs to never-exposed CZs reinforces my decision to exclude never-exposed CZs from the baseline counterfactual group. In Appendix Section C, I also the earlier-exposed sample of CZs to include CZs experiencing multiple mergers during the observation window. The expanded sample consists of 143 exposed CZs from 41 unique mergers and yields similar results to my baseline sample.

surer mergers. 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 merger year), and then calculate the weighted average of cohort-specific effects, where weights are equal to each cohort's sample share. I provide further detail on the Sun and Abraham (2021) methodology in Appendix D.

In this section, I present estimating equations as canonical two-way fixed effects models for ease of interpretation. 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 (denoted by $l = 0$), α_t and δ_c are calendar year and CZ fixed effects, and ε_{ct} is the error term. The inclusion of calendar year fixed effects control for time trends such as wage inflation and business cycles, while the CZ fixed effects control for underlying heterogeneity that is constant over time within each CZ such as geography and local and state regulatory environments. I cluster standard errors at the CZ level. I weight by the CZ population in the year before the merger, given differences in CZ population across the U.S.

The coefficients of interest are the β_ls. I omit the event-time indicator for the year before the merger throughout the paper, so that the β_ls measure the impact of insurer mergers in event year l relative to the year before the merger on outcome y_{ct} in earlier-exposed CZs relative to later-exposed CZs. I also report the average of coefficients across the post-merger event years.

The estimators are identified under three assumptions: parallel trends, no anticipation, and common shocks. The parallel trends assumption states that in the absence of the merger, outcomes for earlier- and later-exposed CZs would have evolved in parallel. I test for pre-trends in event study results to examine the plausibility of the assumption. The no-anticipation assumption requires that mergers are not driven by changes in the local conditions of exposed CZs. Focusing on national mergers ensures that concentration increases within local markets where merging insurers have overlapping pre-merger shares are not influenced by changes in local conditions. To provide further support for the assumption, I conduct a placebo test to examine whether premiums increase in markets that would have been exposed to mergers

that were blocked by antitrust authorities. The third assumption is that earlier- and later-exposed CZs experience common shocks. One concern is that earlier-exposed CZs may be disproportionately impacted by local shocks unrelated to insurer mergers that drive premium and labor market outcomes. I control for potentially confounding local shocks, such as local trade exposure and hospital merger shocks, in robustness specifications.

For sub-group analyses, I estimate a fully-saturated model identified under similar assumptions:

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

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

Aggregate Trends Over Time By Exposure to Insurer Mergers. Figure 3 previews the effects of insurer mergers by plotting mean trends in earlier- and later-exposed CZs at the CZ level. Panel A shows that insurer concentration in earlier-exposed CZs increases sharply post-merger, rising from 2,500 to 3,000 HHI points (an 18 percent increase), while concentration trends smoothly in later-exposed CZs. Panel B shows that fully-insured firms' premiums increase in earlier-exposed CZs from \$4,500 to \$5,200 (a 15.5 percent increase) post-merger, remaining at an elevated level six years after the merger. Following the merger-induced increase in premiums, Panel C shows that wages for single-establishment firms, which are typically fully-insured, in earlier- and later-exposed CZs smoothly increase and trend in parallel before and after the merger. The finding suggests no wage response to the merger. Panel D shows that employment in single-establishment firms falls from 0.47 to 0.44 percentage points (a 6 percent decrease) over the three years post-merger, before stabilizing in the medium-run. Outcomes in all four panels exhibit parallel trends for earlier- and later-exposed CZs before the merger, supporting the parallel trends assumption.

4. Effect of Insurer Mergers on Employer-Sponsored Premiums

I study the effect of insurer mergers on the premiums of fully-insured firms. I aggregate plan-level data to implement a difference-in-differences analysis at the CZ level, comparing the premiums of fully-insured firms in earlier-exposed CZs to later-exposed CZs, before and after

mergers. I examine effects on the quality of insurance plans to consider whether mergers deliver benefits to consumers. In the next section, I examine how merger-induced changes in employer-sponsored premiums affect firms and workers.

4.1 CZ-Level Premium Effects

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

Fully-insured firms in earlier-exposed CZs experience a $434 (10 percent, standard error = $202) increase in premiums compared to later-exposed CZs, over the six years after an insurer merger. Premiums across all fully-insured firms rise sharply in the third year after the 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. Because insurers and employers typically negotiate over contracts at an annual frequency, the increase in premiums occurs with a delay as greater insurer market power may not immediately translate into higher premiums. Premium changes are also 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 (Dafny et al., 2012; Guardado et al., 2013).

In Appendix Figure A.8, I examine the premiums of fully-insured firms served by rival insurers. I find that rival insurers not directly involved in the merger raise premiums by $568 (12.6 percent, standard error=212), contributing to the overall premium increase within exposed CZs. Therefore, fully-insured firms in exposed markets face higher premiums regardless of whether their insurer is directly involved in the merger. This finding is consistent with the newly-merged insurer and its rivals using similar premium pricing strategies in exposed markets due to their increase in market power following the merger.

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 one dependent, so enrolls two participants in the health plan. Third, out of the 683 unique CZs in the Form 5500, I identify 246 CZs ex-

posed by insurer mergers an average of 4 times. Thus, the average CZ experiences a $1,237 per worker increase in the premiums of fully-insured firms due to insurer mergers.[13] The fourth input is that premiums across all plans increased by $5,636 per worker from 1999 to 2019 (Figure 1). Extrapolation suggests that insurer mergers caused 22 percent (= 1237/5636) of overall premiums growth over the last two decades.[14]

4.2 Robustness

Placebo Test of Blocked Mergers. A key threat to identification is if merging insurers anticipate premiums growth in exposed local 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.[15]

I follow the same empirical strategy as used in my baseline analysis. I identify 33 placebo-exposed CZs where both merging insurers had pre-merger market shares. Because the blocked mergers occur in 2017, I do not observe later-exposed markets, so I use 226 never-exposed CZs as controls. Figure A.9 indicates that insurer concentration and premiums for fully-insured 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 exposed markets.

Premiums Effects for Self-Insured Firms. I find that the premiums of fully-insured firms increase post-merger. If market power is a key mechanism for the premium increases, we may not expect similar premium increases for self-insured employers, but fees paid to insurers for plan administration may increase. Appendix Figure A.16 examines the effect of insurer mergers on self-insured employers using F55 data. Unfortunately the F55 data understates the premiums of self-insured firms as the Form only collects information on costs paid from

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

[14] 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.

[15] One additional merger during the sample period, between CareFirst and Wellpoint in 2001, was blocked, while the 2009 merger between Independence Blue Cross and Highmark was abandoned following a merger challenge. I do not study these mergers due to insufficient F55 data.

financial trusts and not general assets. Therefore, I do not observe premium information for 51 percent of self-insured firms that do not hold assets in a trust (Walsh, 2023). The sample consists of self-insured firms in 36 earlier-exposed and 17 later-exposed CZs.

Panel A shows that insurer mergers do not significantly raise the premiums for self-insured firms. While there is evidence that premiums for self-insured firms gradually and smoothly increase in the post-merger period, the absence of a sharp response to insurer mergers suggests that these increases cannot be attributed to the mergers. Panel B shows that claims paid by self-insured firms for workers' medical costs closely mirror the premium trajectory, suggesting that the premium increase is driven by increasing workers' medical expenses rather than insurer mergers. In contrast, Panel C reveals that insurer fees do not increase; in fact, there is suggestive evidence of a decline in insurer fees. Overall, I find no evidence that insurer mergers causally increase the fees or premiums of self-insured firms, which supports the claim that market power drives the increase in premiums for fully insured firms.

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, population size, or the rural share (see also Appendix Figure A.11).

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.12 provides suggestive evidence that premium increases are driven by CZs with above-median populations. Appendix Figure A.15 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 estimate results at the core-based statistical area (CBSA) level. CBSAs are urban regions with 10,000 or more residents, but CBSAs have the drawback that they do not cover all regions of the U.S. I find that premiums estimates at the narrower geographic delineation of CBSAs are insignificant and negative (-$76, standard error = 241). I also estimate results at the hospital referral region (HRR) level. HRRs are medical care markets with a minimum population size of 120,000. Estimates at the broader geographic delineation of HRRs are consistent with my baseline estimate ($444, standard error = 235).

4.3 Changes in Health Plan Quality

Premium increases may reflect improvements in the quality of health plans. In Figure 5, I examine the effect of insurer mergers on plan types offered by fully-insured 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 in Panel C which 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, the adoption of HMO plans suggests that employers switch to lower-quality, more restrictive plans to limit premium increases. The findings suggest that insurer mergers facilitate the exercise of market power to raise premiums without delivering any benefits to consumers.

5. Effect of Insurer Mergers on Firms and Workers

In this section I study how firms respond to the merger-induced premium increase documented in the previous section. Using a parsimonious difference-in-differences analysis at

the CZ level, I estimate the effect of insurer mergers on firms in earlier-exposed CZs relative to those in later-exposed CZs. I document that firms facing higher premiums 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 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

I use a standard competitive model to consider how compensation and employment in firms might respond to a merger-induced increase in employer-sponsored health insurance premiums, following Summers (1989). Starting in the pre-merger equilibrium where labor supply equals labor demand, Appendix Figure A.17 shows that equilibrium employment is given by E_0 and equilibrium compensation is the sum of wages w_0, employer-sponsored premiums h_0 and pension contributions p_0. After the merger, we expect the labor demand curve to shift down by \$900 per worker due to the increase in employer-sponsored premiums (Figure 4).[16] Given workers do not value the cost increase, the labor supply curve remains unchanged. Consequently, employment and workers' take-home compensation both fall, while firms' compensation costs increase.[17] If labor supply and demand are both relatively elastic, we would expect to see larger employment losses than if both are relatively inelastic. And if labor supply is more elastic than labor demand, we would observe a small decline in workers' take-home compensation, with the burden falling on firms in the form of higher compensation costs.

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 participation and hours of work as inputs into labor supply. Second, wage rigidities may prevent firms from lowering wages, while other frictions may prevent firms from lowering health insurance coverage, pension contributions, or other non-wage compensation costs. Third, the model does not consider heterogeneity across workers. Fourth, the model also assumes no

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

[17] What is the predicted response if workers actually value the premium increase? Worker valuation of the merger-induced premium increase would cause the labor supply curve to shift down. Take-home compensation would unambiguously fall, while the effects on employment are ambiguous and depend on the extent to which workers value the premium increase. In the case of full valuation, post-merger employment is the same level as in the pre-merger equilibrium, and post-merger compensation costs are equal to pre-merger compensation costs.

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 Firms

Baseline Wage Effects. I test the benchmark model's predictions by comparing annual wage earnings for firms in earlier-exposed CZs to those in later-exposed CZs. Using LEHD data, I focus on single-establishment firms, which tend to be fully insured due to their smaller size and are more vulnerable to insurer mergers. In contrast, multi-establishment firms are usually self-insured and set their own premiums, making them less vulnerable to mergers. Even if multi-establishment firms offer fully-insured plans, their premiums may not increase because firm-wide plans are determined at the corporate headquarters, which is more relevant than the establishment's location for assessing merger exposure. The LEHD lacks key information on firm headquarters, further justifying the decision to focus on single-establishment firms.

To evaluate wage responses, 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. Wages by merger exposure trend in parallel for both groups of workers leading up to the insurer merger, supporting the parallel trends assumption for identification. After the merger, incumbent workers' wages evolve similarly in earlier-exposed and later-exposed CZs, whereas new hires in earlier-exposed CZs experience a decline in wages during the second and third years post-merger compared to their counterparts in later-exposed CZs. The wages of new hires remain at a lower level for at least eight years, averaging a 4.2 percent decrease (standard error = 1.7) during the post-merger period. A possible explanation for the finding is the presence of implicit contracts that constrain the wages of incumbent workers but not the wages of new hires. Another explanation is that firms avoid lowering incumbent workers' wages—who have a reference point based on their past earn-

ings—to maintain morale (Blinder and Choi, 1990; Bewley, 1999), whereas new hires do not have a similar reference point.

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.18 and find no effect on wages after accounting for changes in worker composition (t-statistic = 1.0).

The implied dollar decline in annual wages for new hires is approximately \$200 ($= 0.04 \cdot 4700$). 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. Given only a small portion of the cost increase is borne by workers in the form of lower wages, the finding implies that workers' wages are largely unresponsive to increases in ESHI costs. The finding is consistent with Brot-Goldberg et al. (2024) who provide indirect evidence of minimal wage pass-through following ESHI cost increases by comparing the magnitude of employment and total payroll effects, and Gao et al. (2022) who find no wage response for continuing workers in firms experiencing higher premium costs.

Several recent studies of payroll taxes, which like ESHI costs are paid by the firm to fund benefits received by workers, also find no or limited wage pass-through (e.g., see Saez et al. (2019), Benzarti and Harju (2021) and Guo (2024) for recent papers). These studies point to existence of frictions that hinder the free adjustment of wages, including morale and fairness considerations. Yet papers studying mandated benefits find that costs are substantially shifted onto workers' wages. For example, Gruber (1994) shows that the costs associated with mandating insurance for 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 passed on to the wages of workers. Wages may adjust more readily to these mandated benefits because they are more salient and are valued by workers.

Non-Wage Margins of Adjustment. Rather than lowering wages, firms facing higher ESHI costs may lower the non-wage compensation offered to workers to reduce overall labor costs. Table 3 presents coefficients estimated from equation (1) to examine the effects of rising ESHI costs on firms' health insurance, pensions, and other non-wage compensation costs. I draw on three datasets aggregated to the CZ level. First, I use the Form 5500 to analyze the pension, dental and vision, and life, sickness, and accident contributions of firms that offer fully-insured

health plans. Second, I use the MEPS-IC to study health insurance coverage within single-establishment firms. Third, I use the ACS to examine the part-time worker share. Due to the lack of information on firm size or methods of financing health insurance, the ACS sample includes workers from all firms starting in 2005.[18]

Firms facing higher premiums do not reduce the number of health plans offered, lower premium contributions, or discontinue employer-sponsored health insurance (ESHI). Instead, firms reduce the share of workers eligible for ESHI by 3.6 percent (2.9 percentage points, standard error = 1.6 percentage points), while the part-time worker share within firms 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 premium increases post-merger. Corroborating these results, I find similar (although noisier) evidence of a 7 percent increase in part-time work within firms using MEPS-IC data in Appendix Figure A.19. I find no evidence to suggest that firms enroll fewer workers in health insurance plans (Table 3 column 6). Appendix Figure A.19 also shows that non-elderly individuals (under age 65) in earlier-exposed CZs are not less likely to be enrolled in an employment-based health insurance plan compared to those in later-exposed CZs. Overall, neither workers nor their dependents lose employer-sponsored health insurance coverage following merger-induced premium increases.

I show that firms reduce workers' pensions following the increase in ESHI costs from insurer mergers; see Table 3 Panel B. The number of pension plans offered by firms in earlier-exposed CZs falls by 5 percent (4 fewer plans) compared to those in later-exposed CZs, and firm contributions to pension plans falls by $167 per worker (7 percent, standard error = 97).[19] For 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.

Firms do not lower the generosity of their dental and vision plans, or life, sickness, and accident insurance plans.[20] However, I do not observe the share of premiums paid by the firm for these benefits, which may be the more relevant margin of adjustment.

Summary. Firms facing higher ESHI costs from mergers pass part of the cost increase onto workers by lowering new hires' wages and reducing their pension contributions, in line with

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

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

[20] 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).

the benchmark model. However, without full pass-through, workers are more costly for firms to employ. We would expect to see a decline in employment among firms in the earlier-exposed CZs relative to later-exposed CZs, which I will examine next.

5.3 CZ-Level Employment Effects in Firms

Baseline Employment Effects. I examine the employment responses of firms to insurer mergers in earlier-exposed CZs relative those in later-exposed CZs. I aggregate firm employment to the CZ level by calculating the share of the working-age population employed in 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. Firms facing higher merger-induced ESHI costs experience a gradual decline in employment over the first three years after the merger, which then stabilizes at a lower level for up to eight years after the merger. The gradual employment decline is consistent with the gradual increase in ESHI costs resulting from insurer-employer bargaining over premiums. On average, the employment-to-population ratio falls by 2.1 percentage points (≈4.4 percent, standard error = 0.9) over the eight years following the merger in earlier-exposed CZs compared to later-exposed CZs. Given that the average earlier-exposed CZ has a pre-merger population of 267,800, the estimated employment decline corresponds to a total loss of approximately 11,800 jobs ($= 0.044 \cdot 267,800$) from firms within a given exposed CZ. The employment losses support the benchmark model's predictions that increasing ESHI costs result in decreased labor demand and, consequently, lower employment.

Exposure to Insurer Mergers. To investigate dosage effects, I compare merger effects within CZs with above- and below-median pre-merger insurer concentration in Figure 9. First, I find that less-concentrated CZs with an above-median insurer count appear to experience larger post-merger increases in insurer concentration than more-concentrated CZs with a below-median insurer count (31 vs. 6 percent, t-statistic = 1.61). In turn, premium increases appear to be driven by CZs with an above-median insurer count, which experience larger increases in post-merger insurer concentration (14 vs. 5 percent, t-statistic = 0.49). Correspondingly, employment 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. Taken together, my findings suggest that in CZs experiencing substantial merger-induced increases

in insurer concentration, insurers exploit their market power to increase employer-sponsored premiums, leading to large employment declines due to higher labor costs for firms.

My findings emphasize the importance of focusing on changes in concentration rather than the level of concentration when evaluating mergers for potential harm. While we might expect more-concentrated CZs with fewer insurers to be more susceptible to reduced insurer competition, it seems that insurers are less effective at extracting further increases in areas where premiums are already above competitive levels. 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 that cause the largest increases in concentration (Arnold, 2019; Prager and Schmitt, 2021).

5.4 Implied Labor Elasticities of Exposed Firms

Labor Demand Elasticity. Together, the estimated employment decline of 4.4 percent (Figure 7) and an increase in firms' compensation costs of 1.6 percent implies an elasticity of labor supply for exposed firms of 2.75 (standard error = 1.1).[21] Benchmarking to the literature, Figure 8 Panel A shows that the implied labor demand elasticity of 2.75 falls within the range observed in studies of rising premium costs and payroll taxes. The implied elasticities for studies of rising ESHI costs (Gao et al., 2022; Brot-Goldberg et al., 2024) are close to 2. Both studies find no wage responses resulting in employment declines, however neither measure pension or other non-wage compensation responses which would lower firms' labor costs, so the elasticities represent a lower bound.

Recent papers of payroll taxes also find imperfect pass-through and calculate large elasticities in the range of 2 and 4. Notably, studies in the U.S. analyzing changes in unemployment insurance (UI) taxes yield similar elasticities: Anderson and Meyer (2000) calculate a labor demand elasticity of 2.3 following the introduction of an experience-rated UI tax system in Washington; Johnston (2021) estimate a labor demand elasticity of 4 following UI tax hikes in Florida; and Guo (2024) obtain a lower bound on the labor demand elasticity of 2.4 following state-level UI payroll tax increases.

[21] Given workers' wages do not fall on average, the percent change in firms' compensation costs is equal to the increase in ESHI costs net of pensions as a share of total compensation. Table 3 indicates that firm pension plans fall by 7 percent and the generosity falls by 5 percent, implying a 12 percent decline ($= 0.93 \cdot 0.95$) in firms' pension contribution on a base of \$2,500. Figure 4 indicates that premiums increase by 10 percent on a base of \$9,000 per worker. Annual wages net of taxes I assume to be approximately equal to \$25,000. Therefore, the change in firm compensation costs is equal to $1.6 = \frac{0.1 \cdot 9000 - 0.12 \cdot 2500}{25000+2500+9000}$.

Labor Supply Elasticity. Dividing the estimated employment decline of 4.4 percent by the estimated take-home compensation decline of 0.8 percent yields an implied firm-specific labor supply elasticity of 5.5 (standard error = 0.9).[22] Other studies of rising ESHI costs yield even larger (infinite) implied labor supply elasticities, given no evidence of wage effects and trivial declines in health insurance uptake and assuming no unobserved non-wage responses.

Compared to the elasticities calculated in studies of payroll taxes and meta-analyses (see Figure 8 Panel B), the implied labor supply elasticity in this paper of 5.5 is at the upper end of the elasticities found in the literature, while still falling within the overall range. For example, 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. The range of elasticities reported in the meta-analysis conducted by Manning (2003) spans from 2 to 5.

The labor demand elasticity of 2.75 and labor supply elasticity of 5.5 implies that firms bear the majority of the burden – about two-thirds – of the $900 per-worker increase in ESHI costs from insurer mergers, while workers bear the remaining third.[23]

5.5 Decomposing the Employment Decline.

To investigate heterogeneous effects across workers, I decompose my baseline estimate of a 2.1 percentage point employment loss 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. A challenge with this decomposition exercise using my unbalanced baseline sample is that some new workers cannot be categorized by their pre-merger annual wages. To create a balanced sample for the wage decomposition exercise, I restrict the sample to workers employed in single-establishment firms in the year before the merger and track them throughout the observation period. To assess whether workers in specific wage bins are disproportionately affected by mergers, I report the share of the overall employment reduction attributed to workers in each wage bin (depicted in orange) and compare it to the

[22] The percent change in workers' take-home compensation is equal to the decline in pensions as a share of total compensation. Table 3 indicates that firm pension plans fall by 7 percent and the generosity falls by 5 percent, implying a 12 percent decline ($= 0.93 \cdot 0.95$) in firms' pension contribution on a base of $2,500. Therefore, the change in workers' take-home compensation equals $0.8 = \frac{-0.12 \cdot 2500}{25000+2500+9000}$.

[23] The firm's burden is given by $\frac{\varepsilon_S}{\varepsilon_S+\varepsilon_D}$, and the worker's burden is given by $\frac{\varepsilon_D}{\varepsilon_S+\varepsilon_D}$.

share of pre-merger CZ employment in those bins (depicted in blue).

Figure 10 shows that employment losses are driven by losses for workers earnings less than $80,000 in the bottom 85 percentiles of the national income distribution. Workers in the bottom 85 percent can explain virtually all (99.8 percent) of the overall 2.1 percentage point decline in employment from single-establishment firms. The finding is consistent with an increased fixed dollar cost per worker that makes lower-income workers relatively more expensive to hire.

I find that middle-income workers earning between $20,000 to $60,000 annually (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. Despite earning lower incomes, workers earning less than $20,000 (the bottom 25 percent) do not experience disproportionately larger employment losses. Many of these lowest-income workers are employed part time and not covered by their own firms' insurance plan (see Appendix Figure A.21). Hence, the lowest-income workers are less likely to be exposed to an increase in ESHI costs relative to middle-income workers. The findings suggest that rising ESHI costs from insurer mergers contribute to a "hollowing out of the middle class".

By Education. Decomposing the losses by education, Figure 11 shows that workers without college degrees make up over three-quarters of total employment in CZs (78 percent), but account for 88 percent of the total reduction in employment. I calculate that the implied decline in employment for workers without a college degree is 5.2 percent, compared to a 2.5 percent decline for workers with a college degree.[24] Less-educated workers are disproportionately impacted by rising ESHI costs as they become more expensive to hire compared to their more-educated counterparts. Given that education is imputed for 85 percent of workers in the LEHD, I corroborate the employment effect by education using the ACS. Estimating over workers in all firms from 2005 onwards in the ACS, Appendix Figure A.22 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

[24]The implied decline for workers without a college degree is equal to the change in noncollege-educated employment share (=88% · 2.1p.p.) divided by the pre-merger noncollege-educated employment share (=78% · 47.2p.p.). The implied decline for workers with a college degree is similarly calculated as the change in college-educated employment share (=12% · 2.1p.p.) divided by the pre-merger college-educated employment share (=22% · 47.2p.p.).

sex, age, and race/ethnicity. Men and women 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, yet I find that employment losses are largely driven by losses for younger under-50 workers compared to older over-50 workers. Black workers are also disproportionately impacted by the increase in ESHI costs. Black workers, making up one-tenth of the population in CZs, account for roughly one-third of the overall reduction, compared to white workers who make up 68 percent of the population in CZs 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 new hires and black workers may relate to worker turnover. Firms that face merger-induced increases in ESHI costs may respond by hiring fewer new workers and allowing for greater attrition, which may disproportionately affect black workers. The explanation aligns with existing 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, employment losses disproportionately occur in the manufacturing, retail trade, construction, and administrative services industries that largely employ noncollege-educated workers in low- or middle-skill occupations.

5.6 Robustness Checks

I explore the robustness of my baseline estimate of employment losses 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-F55 Panel. My baseline analysis focuses on single-establishment firms as they are more likely to be fully insured. To examine if fully-insured firms are driving the employment losses, I link the LEHD to the Form 5500 using the EIN, creating a sub-sample of

single-establishments that filed a Form 5500 indicating their fully insured status. Fully-insured firms in the linked LEHD-F55 panel employ around 6 percent of the working-age population. Given that around 22 percent of the working age population is employed by fully-insured firms,[25] the linked panel identifies roughly one-third (27 percent= 6/22) of all fully-insured firms.[26] I also attempt to link single-establishment firms in the LEHD to self-insured firms in the Form 5500, but I find that about half of my sample of CZs have no single-establishment firms identified as self-insured. In CZs where such firms are identified, a negligible share of the working-age population is employed by these firms. The exercise confirms that single-establishment firms typically offer fully-insured plans.

Table 4 Panel A presents the results from estimating equation (1) for the linked LEHD-F55 samples. Employment in fully-insured firms appears to decline by 3.3 percent (t-statistic = 1.2), 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 noisy estimates may result from the small sample size of linked LEHD-F55 firms. To increase the sample size, I link the Form 5500 with the LBD covering all U.S. states. 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 verifies that fully insured firms facing merger-induced premium increases from reduced insurer competition subsequently experience employment losses.

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). Using the LBD which is available in all U.S. states, Appendix Figure A.23 shows that payroll in single-establishment firms falls by 3.9 percent. The estimate is close to the implied 4.6 percent decline for single-establishment firms in the 25 LEHD states, implying that the LEHD results are generalizable across all 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

[25] 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.

[26] The linking process yields only a sub-sample of all fully-insured firms because 1) the F55 only requires firms with 100 or more participants to file and some firms may fail to file, 2) imperfect matching between datasets due to the EIN changing over time, and 3) a small portion of fully-insured firms may be multi-establishments.

Great Recession shock by allowing changes in the net housing wealth from 2006 to 2009 to vary across time.[27] The estimate decreases slightly to 1.8 percentage points (standard error = 0.9). Given that construction and retail trade industries were heavily affected during the Great Recession, I also flexibly control for the pre-merger shares of employment in these sectors. The estimate remains at 2.4 percentage points (standard error = 0.9) and 2.0 percentage points (standard error = 0.8) after including industry controls respectively. Moreover, I flexibly control for local exposure to trade competition with China using the Autor et al. (2013a) measure and report an estimated decline of 2.3 percentage points (standard error = 0.8). To broadly account for regional shocks, I include flexible census division fixed effects. The estimate is 1.7 percentage points (standard error = 0.7). The stability of these estimates suggests that contemporaneous shocks are unlikely to be conflating 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.

Employment Effects at the Firm Level. The employment effects of rising ESHI costs presented so far are estimated over an unbalanced employer-employee panel pooled to the CZ level. CZ-level results using an unbalanced panel are of interest in their own right, as they capture employment losses from both exiting and continuing 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.24 in Appendix Section G. Reassuringly, there is a clear decline in 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 at the Worker Level. A related concern is that changes in the composition of workers may bias the baseline results estimated from an unbalanced panel aggregated to the CZ level. To address the concern, I conduct a worker-level difference-in-differences analysis following a balanced sample of workers ages 25 to 55 in single-establishment firms in the year before the merger. I begin by following these workers within their pre-merger firm over the observation window, and then analyze workers' outcomes allowing them to be employed

[27] 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.

at any firm. I discuss results shown in Appendix Figure A.5 in Appendix Section G. 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 a partial reallocation of individuals between firms, which I will explore in further detail in the next section.

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. Individuals without employment may respond by migrating to a different labor market, seeking new employment at a less-exposed multi-establishment 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 between firms; however, aggregate employment declines within local labor markets following mergers. The resulting increase in the pool of unemployed workers leads to higher government 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-exposed firms or increases in non-employment. Appendix Figure A.26 presents results from estimating equation (1) for the CZ working-age population 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.6, 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, individuals without employment either find employment in multi-establishment 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 exposed single-establishment firms, employed in less-exposed 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 employment in single-establishment firms falls by 2.1 percentage points following merger-induced premium increases (standard error = 0.9, replicating Figure 7), employment in multi-establishment firms increases by 1.3 percentage points (standard error = 0.4). The employment gains in multi-establishment firms account for approximately 60% ($= 1.3/2.1$) of the employment losses in single-establishment firms. The finding suggests that the larger pool of non-employed workers makes it easier for bigger firms that are less affected by mergers to hire new workers. Consequently, workers partially reallocate from single-establishment to multi-establishment firms within CZs. Aggregate employment declines by 0.8 percentage points (1.2 percent) in earlier-exposed CZs relative to later-exposed CZs.[28]

The local employment losses result in higher unemployment post-merger. The population share that is unemployed steadily increases following the merger, reaching a high of 0.51 percentage points (standard error = 0.23) in the fifth year post-merger. The finding implies that some individuals are unable to secure new employment in multi-establishment firms and remain unemployed in the short run. 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 multi-establishment firms further increases, suggesting that initially unemployed individuals later find employment in multi-establishment firms. Panel D provides suggestive evidence that self-employment increases, reaching a high 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 increased labor force exits.

[28]The aggregate employment decline of 0.8 percentage points is equal to the decline in single-establishment employment net of the increase in multi-establishment employment ($= -2.1 + 1.3$). The decline as a share of total employment across single- and multi-establishment firms is 1.2 percent ($= 0.8/(47.2 + 18.4)$).

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 by 0.66 percentage points (standard error = 0.36) post-merger. The implied change in aggregate employment using the CBP is 1.2 percent ($= 0.6/53$), which is the same magnitude as the change implied by the LEHD results which rely on only 25 states. The similar estimated effects using the LEHD and CBP suggest that the LEHD results are credible and generalize to the entire sample of CZs. In Appendix B and C, I further show that the result is robust to the choice of control CZs and generalizes to CZs that experience multiple mergers during the observation window.

Figure 14 presents the effect of insurer mergers on additional aggregate outcomes within exposed CZs 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), by interacting a CZ merger exposure indicator with an indicator for whether the calendar year falls on or after the merger year.

I find that the employment rate falls by 0.9 percentage points (standard error = 0.3). The implied change of 1 percent ($= 0.9/93$) is similar to the implied 1.2 percent decline in aggregate employment using the LEHD, serving as an additional validation of my baseline result. At the same time, market-wide annual earnings falls by 1.6 percent in the ACS sample (standard error = 0.8), which implies a decrease of \$675 ($= 0.016 \cdot 42229$). The aggregate decline in annual earnings may be driven by a combination of pass-through of ESHI costs to workers' wages in exposed firms, and less-exposed firms subsequently lowering wages for their own workers. I show that the decline in annual earnings is driven by a 1.4 percent (standard error = 0.6) decline in hourly wages, 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), as firms hire more workers who are not extended health insurance coverage in response to higher ESHI costs. Given that firms hire more part-time workers, the true extent of employment losses is likely understated. The results estimated using LEHD data do 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 effect on aggregate employment of 1.2 percent (Figure 12), and the effect on aggregate take-home wages of 0.6 to 1.6 percent implies a labor supply elasticity in exposed local labor markets of 0.75 to 2.[29] My estimates of 0.75 to 2 are larger than labor supply elasticities in existing quasi-experimental studies reviewed in Chetty (2012) ranging from 0.15 to 0.45, but within the range of macro labor supply elasticities in the literature reviewed in Keane and Rogerson (2012) ranging from 1 to 2. Overall, the implied elasticity is subject to statistical uncertainty, but remains within the plausible range of estimates in the literature.

6.3 Government Transfer Effects in Local Labor Markets

Unemployment Insurance. Employment losses uncovered in CZs facing merger-induced rising ESHI costs likely lead to higher participation in unemployment insurance (UI) programs. As workers are typically eligible for up to 6 months of UI, 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. UI spending steadily increases, peaking 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 observed increase in UI spending is consistent with the predicted increase based on state average benefit rates. I estimate that UI receipts increased cumulatively by \$43,700 per 100 residents (standard error = 15420) over the six years after the merger in earlier-exposed CZs relative to later-exposed CZs. Combined with the result of 4 jobs lost per 100 workers post-merger (Figure 7), I calculate an implied 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.[30]

Other Transfer Programs. I examine whether individuals experiencing job losses turn to other transfer programs in Appendix Figure A.27. The lack of labor force exits in CZs affected by insurer mergers suggests that we would not expect to see an increase in Social Security disability benefits. Indeed, I find no increase in spending on disability insurance benefits or

[29] For the lower bound wage estimate, recall that the change in workers' take-home compensation in single-establishment firms is 0.8 percent. Given that 70 percent ($47/(47+18)$) of workers in local labor markets are employed in single-establishment firms, the aggregate change in net-of-tax-and-health-insurance wages is 0.6 percent ($= 0.8 \cdot 0.7$). The upper bound wage estimate is obtained from the ACS sample in Figure 14.

[30] 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).

other transfer programs, such as retirement, medical or federal income assistance 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.

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 compare the U.S. to Canada and ask how much of the excess job losses among U.S. workers can be explained by rising ESHI costs due to insurer mergers.

7.1 Model of Competitive Labor Market

Labor Demand. I consider a market with two worker types $g \in \{C, N\}$, 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 (L_g) is equal to the share of individuals of type g that are working (P_g) multiplied by the number of individuals of type g (N_g), i.e., $L_g = P_g \cdot N_g$.

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 excess decline in the U.S. noncollege employment-to-population ratio relative to Canada over this period is equal to 3 percentage points. 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. Hence, the excess decline in the U.S. college employment-to-population ratio relative to Canada over this period is equal to 2 percentage points. I ask how much more would employment have increased under the counterfactuals as a share of the U.S.-specific net-of-Canada changes 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.

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 F 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.[31]

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 increase under antitrust intervention that slows the growth of premiums. Under the no-merger counterfactual, the U.S. noncollege employment-to-population ratio would be 0.28 percentage points higher in 2019. Therefore, rising ESHI costs from insurer mergers account for 10 percent ($= 0.28/3$) of the 3 percentage point excess job loss among noncollege-educated workers from 1999 to 2021. Meanwhile, the U.S. college employment-to-population ratio would be 0.13 percentage points higher in 2019, implying that insurer mergers explain 8.6 percent ($0.13/2$) of the contemporaneous 2 percentage point excess job loss among college-educated workers.

A more realistic antitrust counterfactual, which blocks mergers that would have caused an increase in the Herfindahl-Hirschman Index (HHI) of at least 200, yields employment increases similar to those in the no-merger counterfactual. While noncollege employment would be 0.28 percentage points higher if all mergers were blocked, it was still be 0.24 percentage points higher if only mergers above the threshold were blocked. Hence, the threshold counterfactual

[31] 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.

has an efficacy of roughly 86 percent (= 0.24/0.28) compared to the no-merger counterfactual. 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, while the college employment-to-population ratio would increase by 0.21 percentage points. If premiums in the U.S. grew at a similar rate as in Canada, excess job losses for U.S. workers without a college degree would be 15 percent smaller, and 14 percent smaller for U.S. workers with a college degree. Additionally, under the no-growth counterfactual, noncollege employment would be 1.3 percentage points higher 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 if premiums had remained at their 1999 level.

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 merger-induced rising ESHI costs decreases aggregate employment by 0.41 percentage points (Table 6 column 1), while the overall increase in ESHI costs from 1999 to 2019 decreases aggregate employment by 1.9 percentage points (column 4). Employment losses from rising ESHI costs are comparable in magnitude to those caused by other leading factors, such as robot adoption and trade shocks.

Employment losses from merger-induced rising ESHI costs are 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. 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 the overall cost increase.

I compare my estimate of employment losses from rising ESHI costs to those caused by exposure to trade competition with China. 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. Thus, rising ESHI costs from insurer mergers result in employment losses that are half the size of those caused by a $1,000 increase in Chinese import exposure. And employment losses from the overall increase in ESHI costs are approximately double the size of those from the China trade shock.

I benchmark my baseline estimate of employment losses 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. Overall, the decline in employment from the average local Great Recession shock are five times larger than estimated declines from merger-induced rising ESHI costs, but roughly equal to the estimated declines from the overall increase in ESHI costs over the last two decades.

8. Conclusion

This paper highlights how rising employer-sponsored premiums cause employment losses that contributes to a hollowing out of the middle class. First, exploiting differences in local

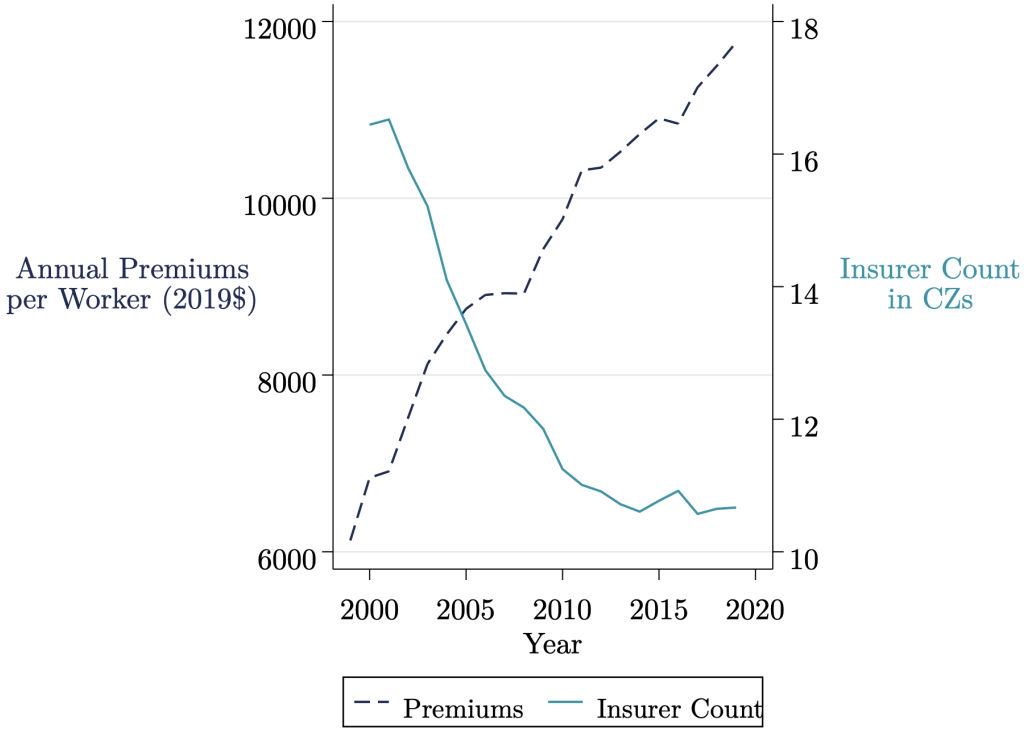
exposure to national insurer mergers, I document that these mergers explain 22 percent of the overall premium increase from 1999 to 2019 by reducing insurer competition. Second, I show that firms facing higher premiums experience employment losses, disproportionately affecting middle-income workers without a college degree. Third, I show that by raising firms' labor costs, insurer mergers can explain 10 percent of the excess U.S. job losses among workers without a college degree relative to Canada over the last two decades.

A surprising takeaway of my findings is that the market-wide employment losses caused by consolidation in the private health insurance industry is larger and affects more local markets than mergers and acquisitions in other industries (Arnold, 2019). Therefore, antitrust efforts that scrutinize consolidation in healthcare markets are crucial for slowing the rise in employer-sponsored premiums and protecting the jobs of middle-class Americans. Despite resource constraints faced by antitrust regulators, my results suggest that focusing on blocking mergers that would lead to the largest increases in the Herfindahl-Hirschman Index (HHI) can significantly mitigate the premium and labor market effects associated with those mergers.

My findings also raise several new questions. First, healthcare markets have become more vertically and horizontally integrated 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 labor market consequences of these new types of consolidation in healthcare markets? Second, while I emphasize the role 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 explore other potentially important responses of firms and workers to rising premiums, such as whether firms invest in more capital as a substitute for labor or outsource low-skill jobs to avoid health insurance costs.

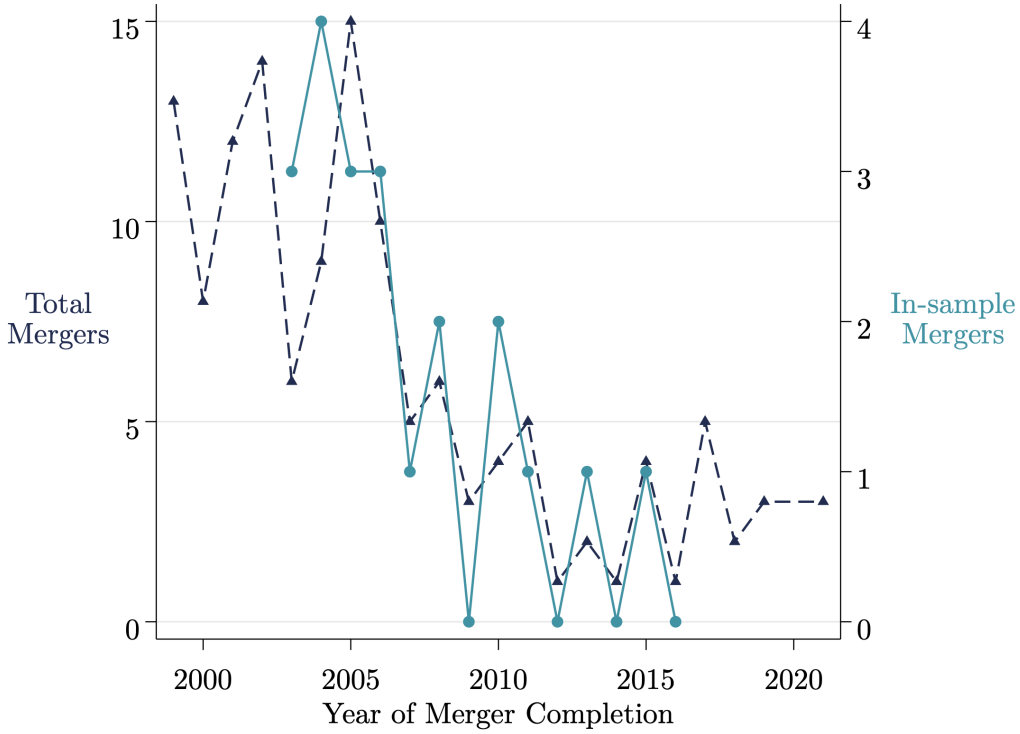
9. Figures & Tables

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



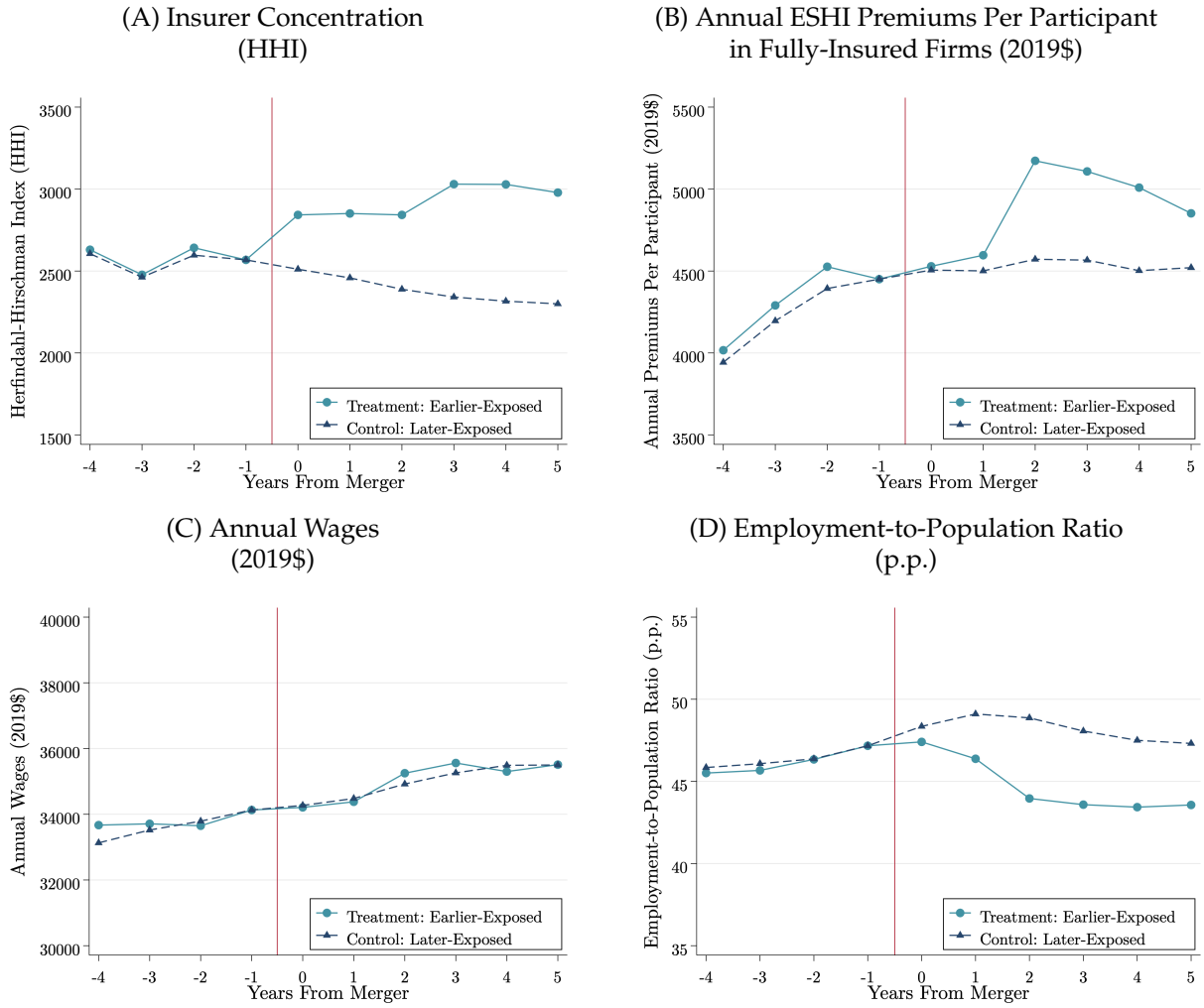
Notes: This figure shows trends in average annual premiums per worker across all plan types at the national level and population-weighted insurer counts at the CZ level between 1999 and 2020. The dotted, dark blue line shows that premiums are increasing (left y-axis), and the solid, light blue line shows 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



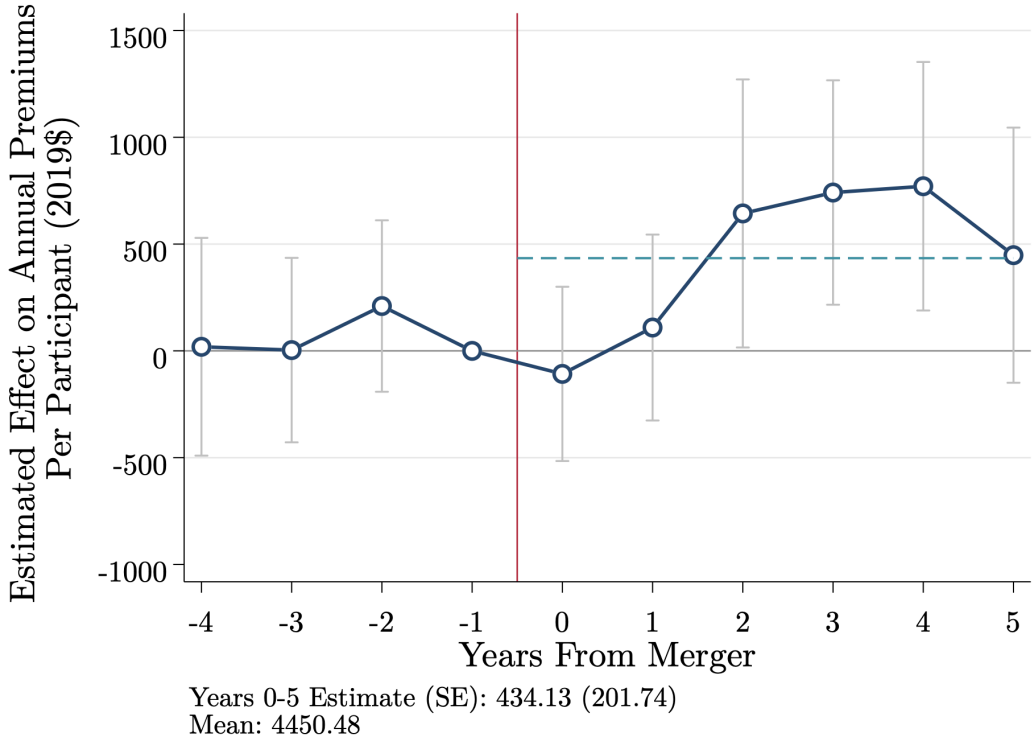
Notes: This figure shows the number of insurer mergers by year of merger completion. The dotted, dark blue line shows the total mergers (left y-axis); the solid, light blue line shows the in-sample mergers of interest (right y-axis). Mergers are excluded from the sample if they involve insurers in non-overlapping 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 premiums per participant for fully-insured firms in Panel B, annual wages in exposed single-establishment firms in Panel C, and employment in exposed single-establishment firms as a share of total population 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).

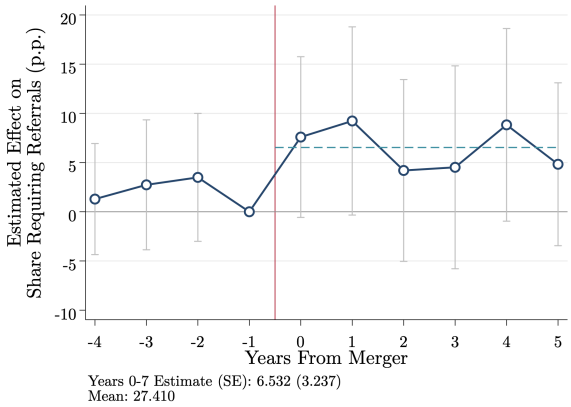
FIGURE 4: CZ-Level Effect of Insurer Mergers on Premiums in Fully-Insured Firms



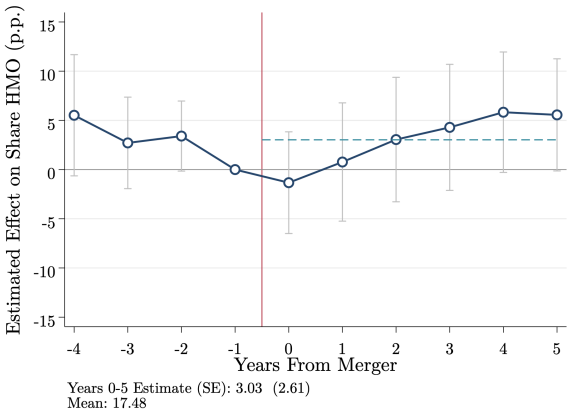
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is annual ESHI premiums per participant in fully-insured firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate 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 Plan Quality

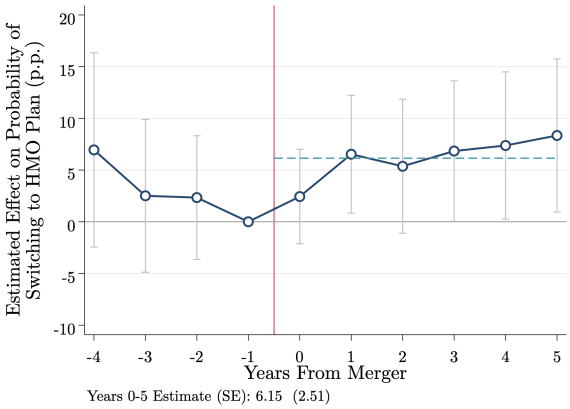
(A) Share Plans Requiring Referrals (p.p.)



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

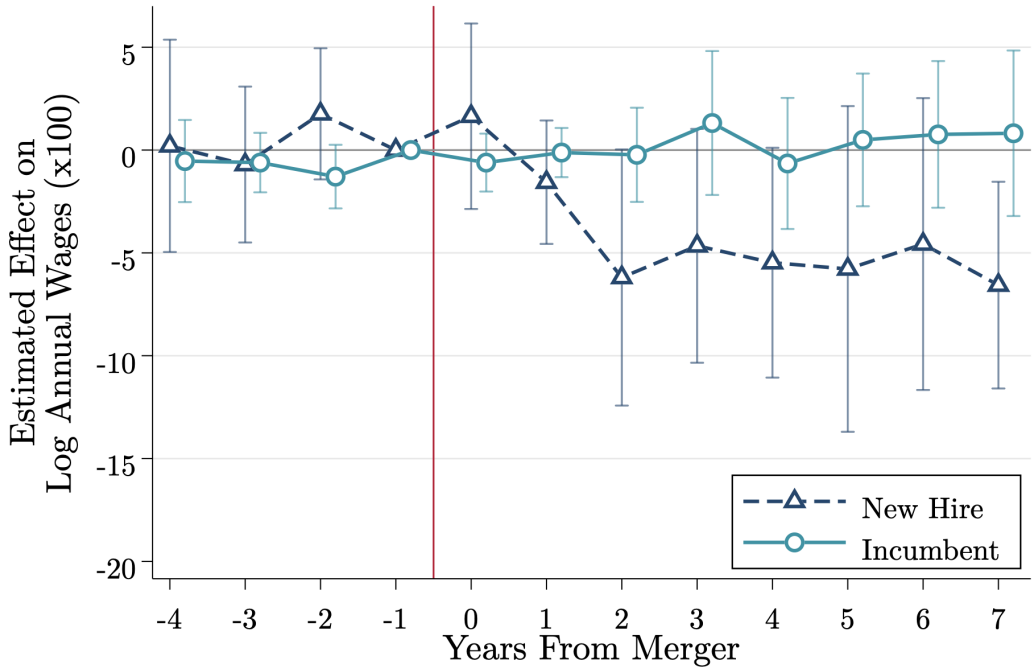


(C) Probability Switch to a HMO Plan (p.p.)



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 plans that are health maintenance organization (HMO) with more restrictive coverage (B), and the probability that an employer not offering any HMO plans in $t-1$ offers at least one HMO plan in event year t (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 Exposed Firms



New Hire Years 0-7 Estimate (SE): -4.146 (1.728); Mean: 4701
Incumbent Years 0-7 Estimate (SE): 0.221 (1.112); Mean: 29430

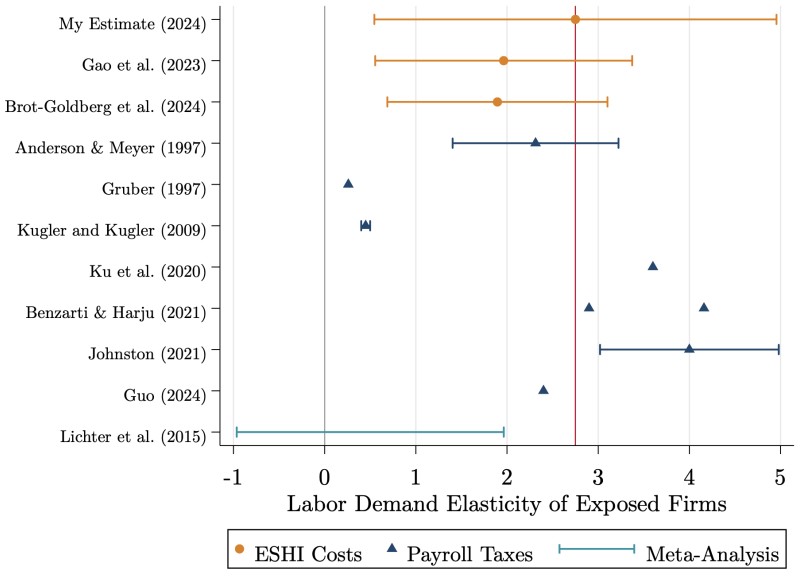
Notes: This figure plots the yearly group-specific coefficients β_{lg} from equation (2), where the outcome y_{ctg} is log annual earnings in exposed single-establishment firms, and groups g are defined as new hires and incumbent workers. Observations are weighted by CZ working-age population in the 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 Working in Exposed Firms

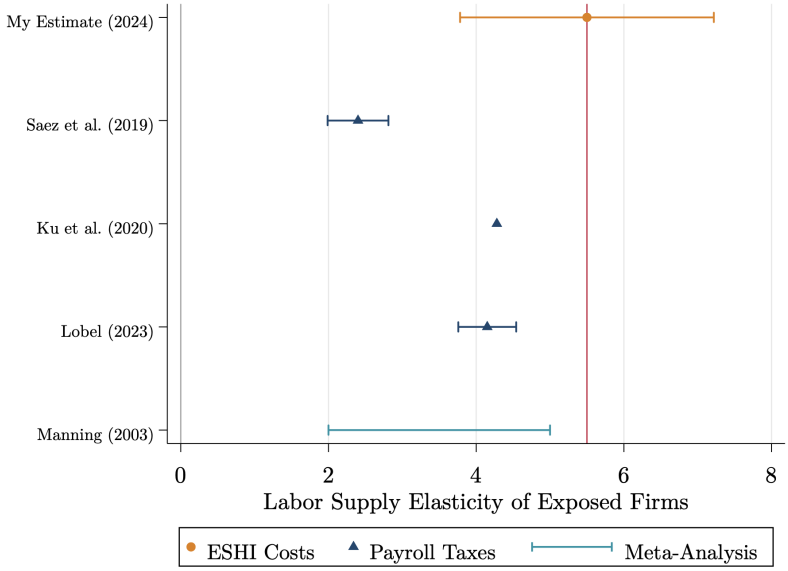
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in exposed single-establishment firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate 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 Elasticities of Exposed Firms

(A) Labor Demand Elasticity

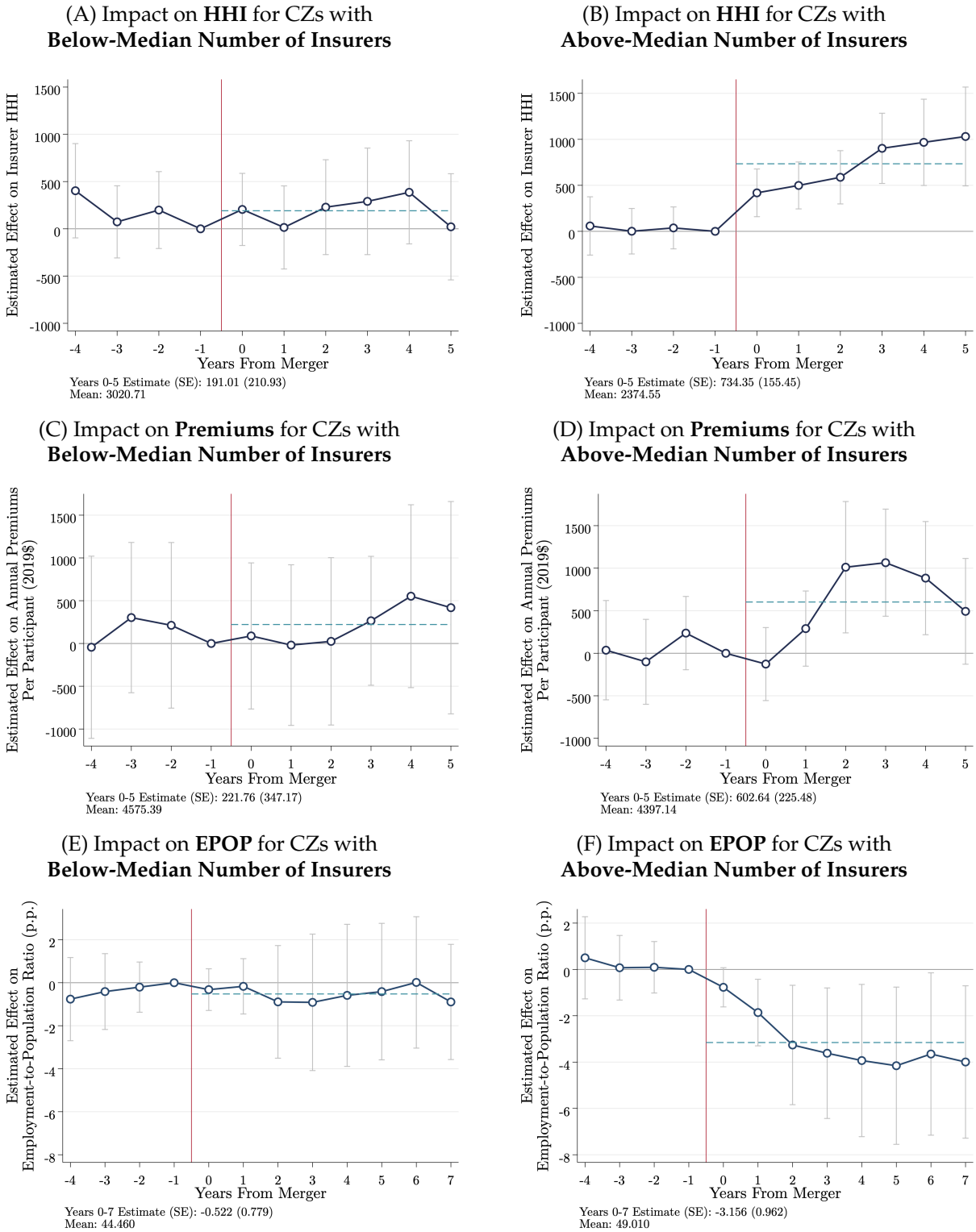


(B) Labor Supply Elasticity



Notes: This figure compares my estimated labor demand and supply elasticities of affected firms to elasticities from existing studies of rising ESHI costs, payroll taxes, and meta-analyses. Labor demand elasticities are defined as the percent change in employment divided by the percent change in labor costs. Labor supply elasticities are defined as the percent change in employment divided by the percent change in take-home compensation. Horizontal lines indicate 95% confidence intervals on each estimate when available.
Sources: See Appendix Table A.7 for details on the elasticity estimates.

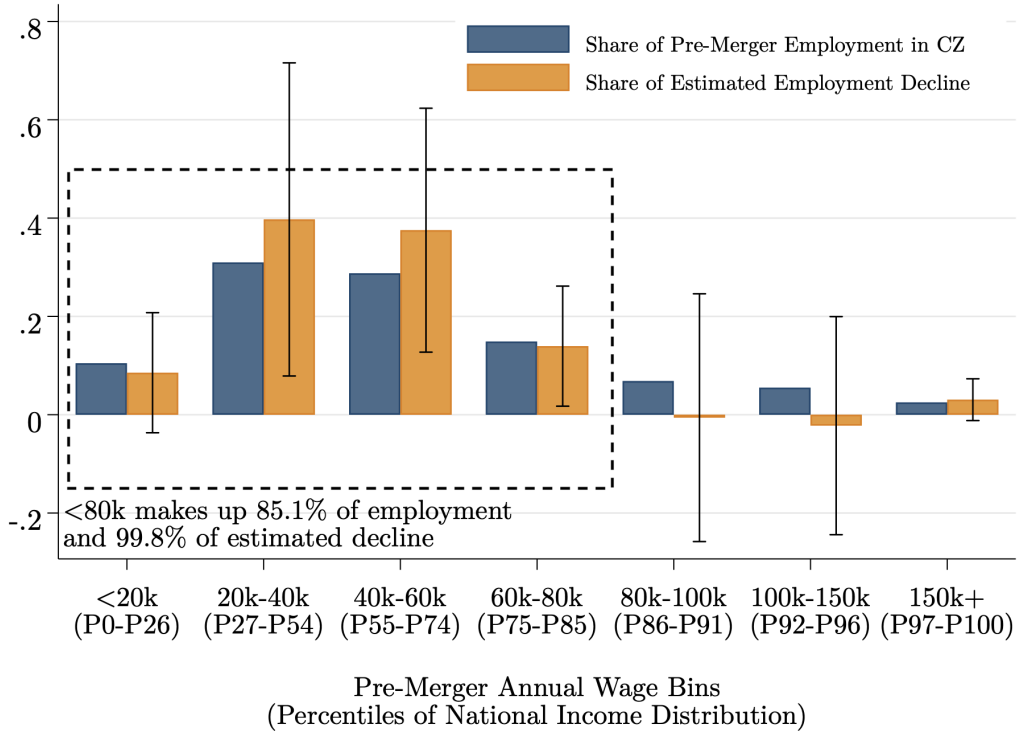
FIGURE 9: CZ-Level Effect of Insurer Mergers by Pre-Merger CZ Number of Insurers



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 in fully-insured firms in Panels C and D, population share in exposed 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).

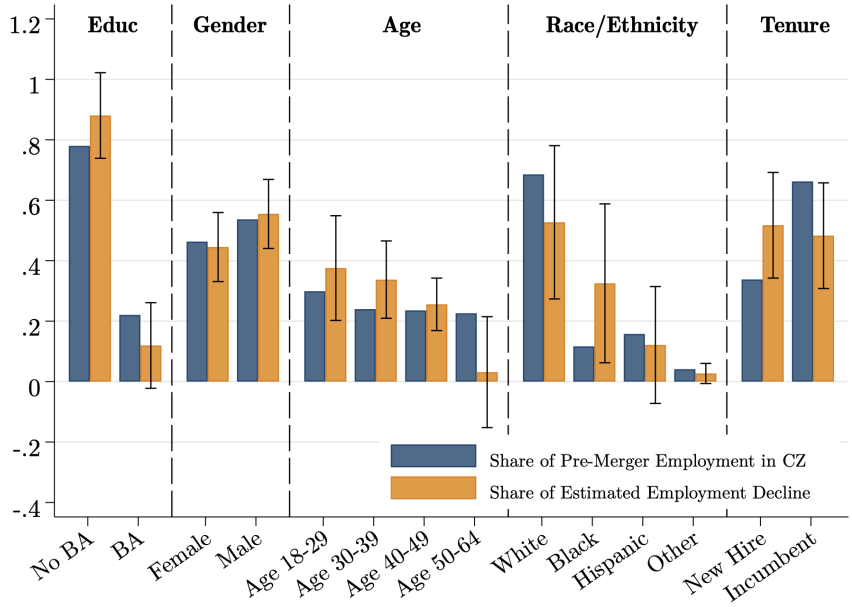
FIGURE 10: Decomposition of Employment Losses, by Pre-Merger Annual Wages



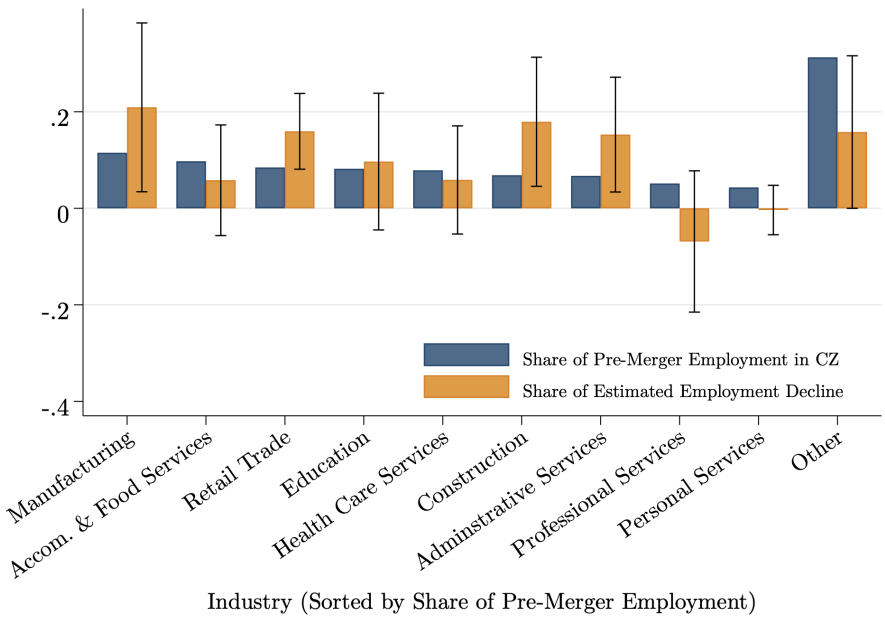
Notes: Figure 10 decomposes the overall estimated employment loss from exposed single-establishment firms (in Figure 7) by pre-merger annual wage bins. For each wage bin, the corresponding percentile range of the national income distribution over the sample period is reported below each x-axis label 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 Losses, by Worker Characteristics

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

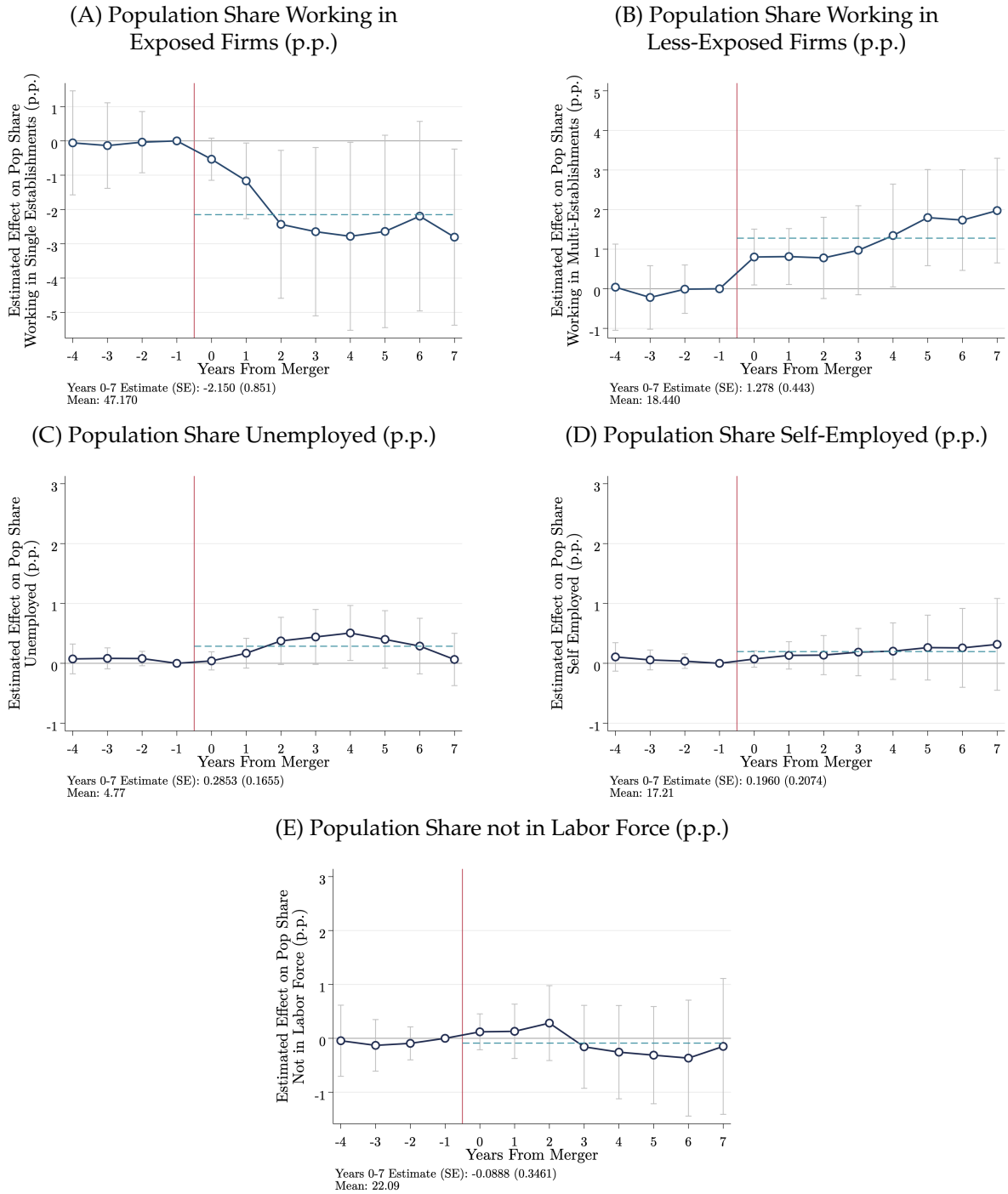


(B) By Worker Industry



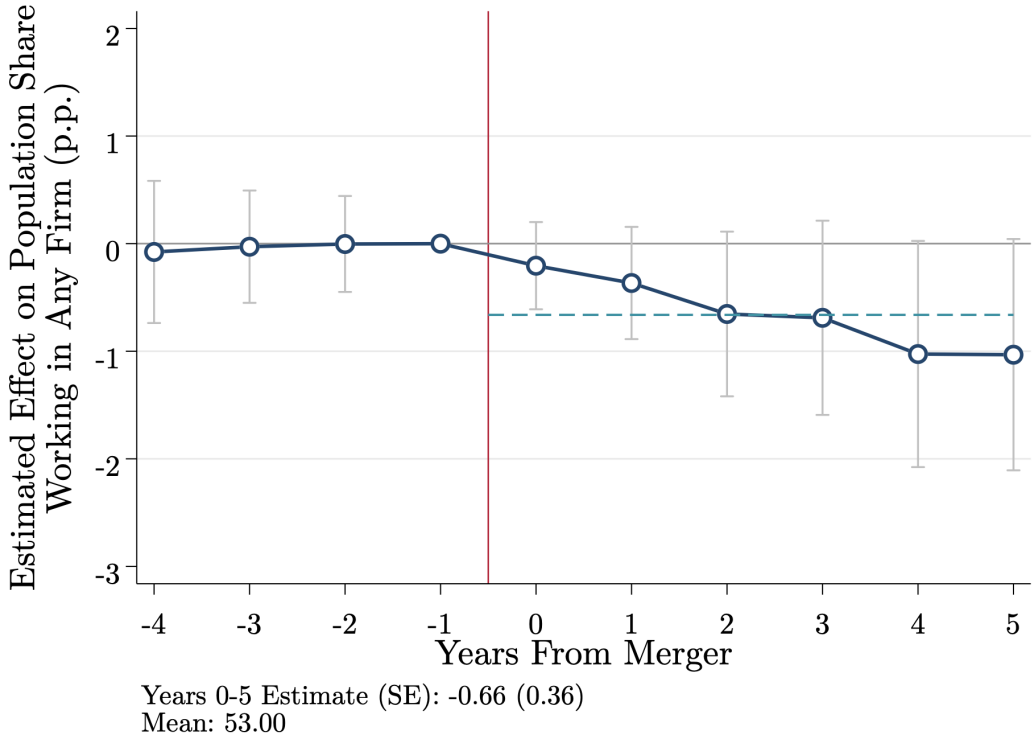
Notes: Figure 11 decomposes the contribution of each sub-group by category to the overall estimated employment loss from exposed single-establishment firms (in Figure 7). Panel A divides workers by education (with or without college degrees), by gender, by age bins, by race/ethnicity, or by worker tenure (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



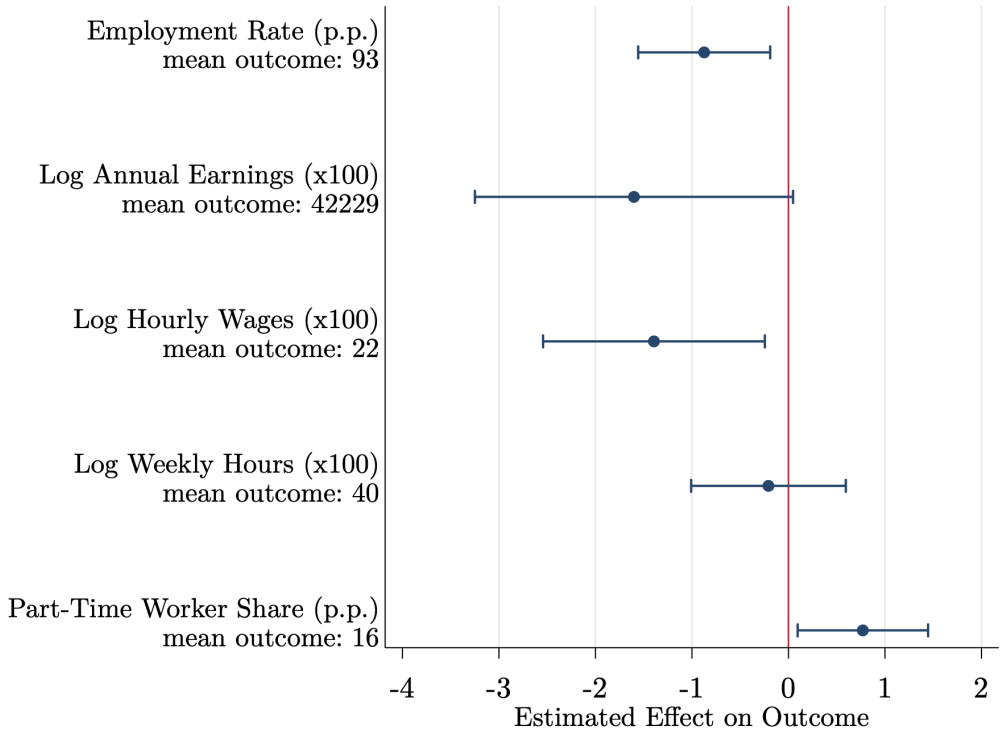
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in exposed single-establishment firms (A, replicates Figure 7), employed in less-exposed multi-establishment firms (B), unemployed (C), self-employed (D), and not in the labor force (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



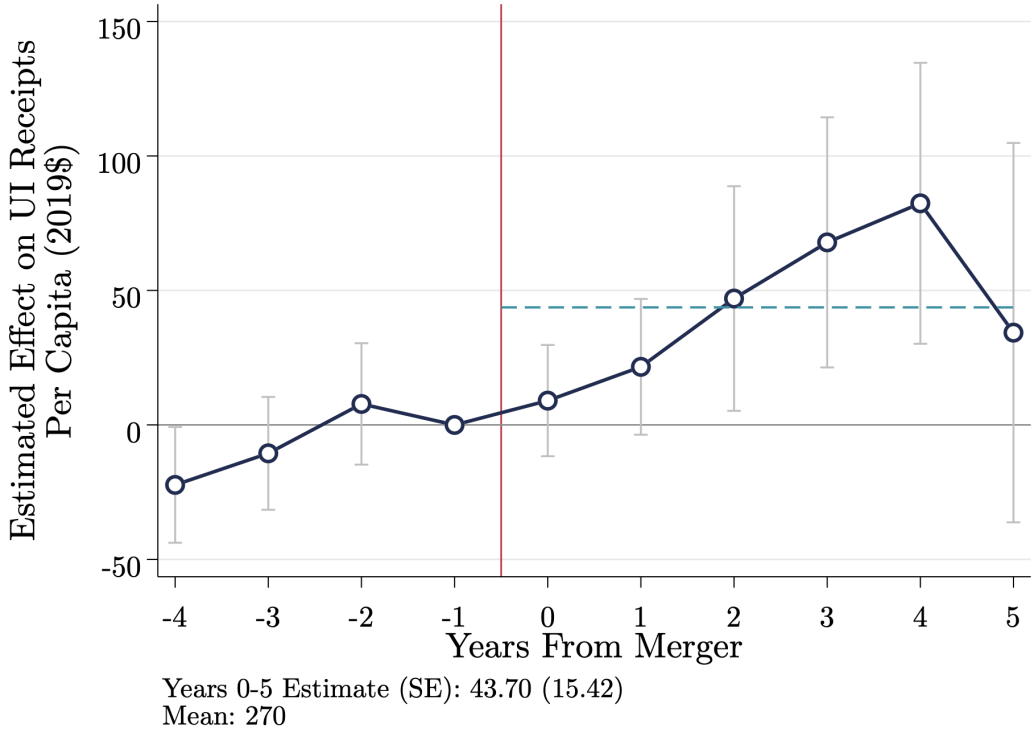
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is the population share employed in all firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of 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).

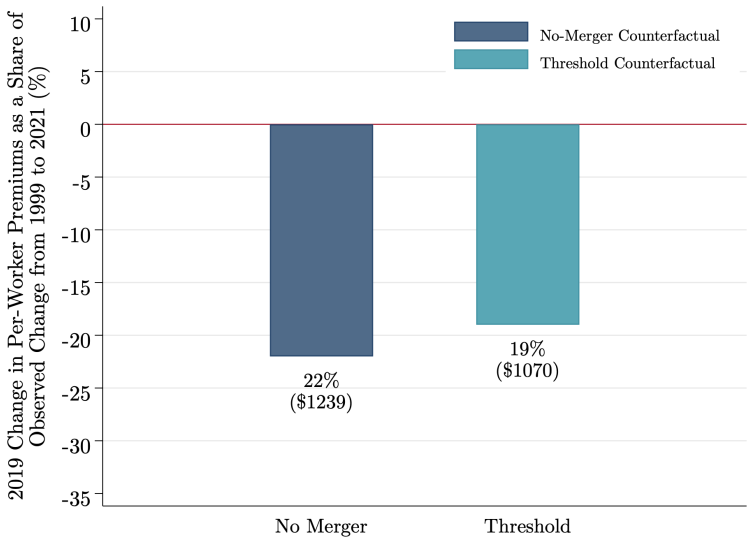
FIGURE 15: CZ-Level Effect of Insurer Mergers on Unemployment Insurance Payments



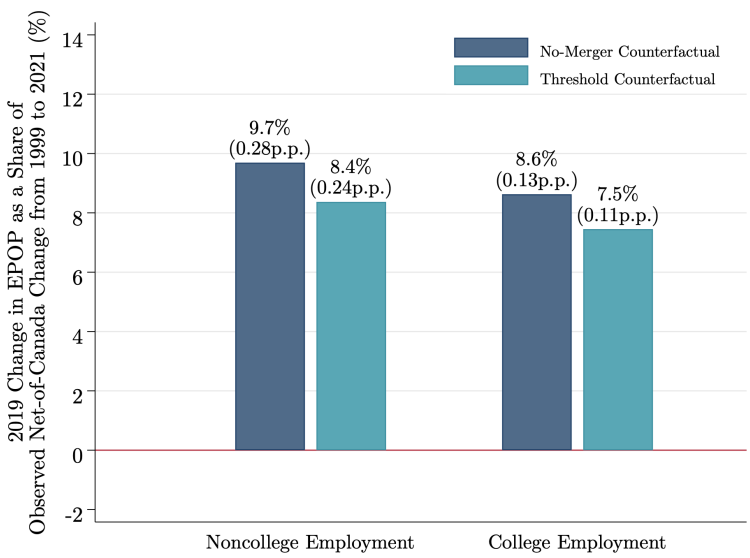
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is unemployment insurance payments per working-age capita. Unemployment insurance includes state unemployment insurance, and federal unemployment insurance for Federal civilian employees, railroad employees, and 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

	Earlier-Exposed CZs (1)	Later-Exposed CZs (2)
Panel A: Main Sample (All U.S. States)		
Number of Insurers	12.89	10.46
	(6.78)	(4.57)
Insurer HHI	2,788.90	2,707.80
	(1,537.53)	(1,263.27)
Per-Participant Premiums ($)	4,654.99	4,766.53
	(2,111.59)	(2,163.24)
Population	480,963.05	369,304.94
	(244,575.72)	(164,596.69)
Median Household Income ($)	53,358.33	54,068.06
	(7,925.50)	(7,429.74)
Unemployment Rate (%)	6.84	6.02
	(2.69)	(2.18)
Share Rural	0.51	0.53
	(0.19)	(0.15)
Share West	0.20	0.16
	(0.40)	(0.37)
Share Northeast	0.03	0.06
	(0.16)	(0.25)
Share Midwest	0.20	0.26
	(0.40)	(0.44)
Share South	0.57	0.52
	(0.50)	(0.50)
Observations	810	1,840
Unique CZs	81	39
Panel B: LEHD Sample (25 States)		
Annual Wages in Exposed Firms	34,790	34,870
	(3,243)	(3,688)
Population	276,000	200,000
	(158,000)	(75,000)
Employment in Exposed Firms	125,700	67,930
	(79,400)	(32,300)
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 which is 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. I report the premiums of firms offering fully-insured plans in the F55 data, and employment and wages of exposed single-establishment firms in the LEHD data. Statistics are weighted by CZ population in each year. 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 × Year Effects	424.08**
	(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 premiums in fully-insured firms 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 Exposed 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	2.06	-0.75	-2.90*	0.77**	-2.61
	(1.94)	(2.46)	(2.55)	(1.56)	(0.34)	(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***	-166.52*	5.51**	-5.01	-1.07	-6.77
	(1.38)	(97.21)	(2.45)	(173.28)	(0.90)	(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 exposed single-establishment firms in Panel A, and log employment in exposed 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 columns 1 and 2 of Panel A; N=20 treated and 10 control CZs in column 3 of 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 & Construction × Year Effects	-2.38***
	(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: Fully-Insured Plan Outcomes. 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 of fully-insured plans. I restrict to fully-insured 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 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 by the employer employment identification number (EIN) and plan ID to obtain in the physical zip code of employers. I 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 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 identify 683 CZs with F55-filing employers offering fully-insured plans 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), focus on CZs treated from 2003 to 2015 and require a balanced CZ panel over the 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.

To calculate CZ-level measures of fully-insured plan premiums and plan quality, I use a balanced employer panel over the observation window which avoids potential bias due to changes in the composition of Form-5500-filing employers. I take the average of premiums across all fully-insured 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. To assess the response of rival insurers, I take the average of per-participant premiums across plans of insurance carriers not involved in insurer mergers. As proxies for plan quality, I calculate the share of plans that are HMO, and the probability that an employer not offering any HMO plans in $t-1$ may offer at least one HMO plan in event year t. I also calculate the number of fully-insured health plans offered at the CZ-year level. I use an unbalanced employer panel to calculate CZ-level measures of health insurer market shares and concentration.[32]

Form 5500: Self-Insured Plan Outcomes. I rely on the Form 5500 Schedule Hs to create my analysis panel of self-insured plans, following the same steps used to construct fully-insured plan outcomes. I drop employers who file more than 6 times in the same year, or provide benefits under multiemployer or multiple-employer plans. I link to the main form using the employer EIN and plan ID. Using information from the main form, I restrict to employers filing a Form 5500 for health insurance benefits, and obtain the zip code of employers and the number of plan participants. I then calculate CZ-level annual measures of per-participant insurer fees, claims (the total cost of medical services paid to doctors and hospitals on behalf of participants), and premiums (total contributions paid by employers and workers). I obtain the share of premiums paid by employers by dividing employer contributions by total premiums.

A key limitation in analyzing self-insured plan outcomes using the F55 is that the form only requires health benefits paid from financial trusts and not from general assets. Given that 51 percent of self-insured plans that filed an F55 did not hold assets in a trust, the F55 data on self-insured fees, premiums, and claims are likely to be significantly mismeasured.

Form 5500: Other Compensation Outcomes. I also leverage the Form 5500 Schedule As to calculate CZ-level measures of compensation, such as pensions, and vision and dental insur-

[32] I omit pre-2001 years as employer filing is significantly lower during this period. 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). I also do not observe information breaking down premiums across by individual plans versus family plans.

ance, paid by employers offering fully-insured health insurance plans. I separately restrict to plans that are listed as providing one of the following benefits: pensions (either defined benefit or defined contribution), life insurance and temporary disability (accident and sickness), and dental and vision insurance. I drop the plan if it also covers health insurance 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 by the employer employment identification number (EIN) and plan ID to obtain in the physical zip code of employers. I keep employers that have ever filed a F55 for a fully-insured plan over the sample period. Using a balanced employer panel over the observation window, I calculate the annual CZ-level per-participant employer contributions to pensions, dental and vision insurance premiums (paid by employers and workers), and life, sickness, and accident insurance benefits (paid by employers and workers). Using an unbalanced employer panel, I calculate the total number of plans by compensation type offered by fully-insured employers at the CZ level.

LEHD. The LEHD is a quarterly matched employer-employee dataset covering over 96 percent of workers in the U.S. The LEHD only covers workers in the state UI system, so independent contractors, the self-employed, federal government workers, railroad workers, and workers in non-profits with fewer than four workers are excluded.

I use the LEHD Job History File (JHF) to construct a unbalanced panel of workers in single-establishment firms to calculate my main outcomes of interest. Employers are defined in the LEHD at the state tax identifier level (SEIN). Although employer SEIN can change in the LEHD data due to firm restructuring, the JHF accounts for job spells which can be split across multiple SEINs for the same employer by assigning PIK-SEIN pairs to a unique identifier (FID). I restrict to working-age individuals employed in single-establishment firms earning at least $3,700 in 2019 dollars in annual wages across all jobs. To construct CZ-level annual wages, I sum wage earnings across quarters and jobs at the individual (PIK) level and take the average across workers with positive earnings at the CZ-year level. Earnings are top-coded at a threshold defined by each state which varies over time (Abowd et al., 2009). To account for changes in worker composition as a robustness test, I calculate a composition-adjusted wage measure for a random 20 percent sample of my main worker sample. I obtain the residuals from regressing log annual wage earnings on age, age-squared, sex, gender, and race/ethnicity at the CZ-year level, which is the composition-adjusted measure. To construct the employment-to-population ratio, I sum the number of jobs with positive wages in single-establishment firms at

the CZ-year level and divide by the working-age population obtained from the National Cancer Institute's SEER Program. To account for workers with multiple jobs as a robustness test, 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 \$90,000 from firm B would be assigned a value of $\frac{1}{10}$ to firm A and $\frac{9}{10}$ to firm B.

I also create balanced firm and worker panels to conduct firm-level and worker-level regressions that hold firm and worker composition fixed. To create a balanced firm panel, I identify single-establishment firms located in an exposed CZs that are active and hire at least fifty workers in the year before the merger ($t-1$). I then follow the firms over the observation window regardless of firm activity status. I calculate two outcomes at the firm-level: employment (number of workers, includes zeros for inactive firms), and a binary indicator for firm activity. To examine outcomes in firms that are always active, I further restrict to firms with at least one worker for each year in the observation window and calculate employment, and employment by education (college or noncollege-educated) at the firm level. To create a balanced worker panel, I identify workers between the ages of 25 and 55 working in exposed single-establishment firms and earning more than \$3,700 across all jobs. I further restrict to workers with only one employer and receiving wages in all four quarters in the year before the merger. I then follow exposed workers within their exposed $t-1$ firm, and also follow exposed workers in any firm over the observation window. I calculate annual wages earned (includes zeros), and the share employed in the $t-1$ or any firm. For heterogeneity analysis, I restrict to sub-samples of exposed workers in the top and bottom quartile of the annual wage distribution in the year before the merger.

Despite missing information on establishment location in the LEHD, I extend my main analysis by examining employment in multi-establishment LEHD firms. I use information on imputed establishment location provided by the Census. I sum jobs with positive earnings across multi-establishment firms to the CZ-year level and divide by the working-age population. As a robustness test, I focus on multi-unit firms with establishments located in a single CZ, ensuring that the CZ is clearly identified.

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

exposed 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 exposed 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 Longitudinal Business Database is an annual establishment-level dataset covering all private, non-farm U.S. employment, constructed from the Census Bureau's Business Register. An establishment in the LBD is defined as the physical location where business occurs (Chow et al., 2021). For each establishment, I observe employment, annual payroll, an indicator for multi-establishment firm, industry, and zip code. Employment is a point-in-time measure equal to the number of workers with positive earnings during the week of March 12th. Annual payroll includes all taxable earnings (i.e., wages and 401(k) contributions), but not employer contributions to health insurance which are tax-exempt. I sum annual payroll across establishments to calculate CZ-level payroll. With data on all states in the country, the LBD is useful for checking the external validity of the LEHD results for which I have access to only 25 states.

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 drawn, 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. I restrict my sample to private-sector single-establishment establishments that are vulnerable to insurer mergers and for which the location of the firm's headquarters are known. For each variable of interest, I set missing observations equal to 0 and generate the CZ-year mean according to respondent weights. Given MEPS-IC data was not collected in 2007, I impute the 2007 mean for each outcome in a given CZ. I use the MEPS-IC to supplement variables not available in

the F55 or LEHD.

Establishment-level outcomes that I aggregate by averaging to the CZ level 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.
- Share of employees working part time. The definition of part time is not explicitly stated in the survey question.

Fully-insured plan-level outcomes that I aggregate to the CZ level include:

- Share of plan premiums paid by employer for fully-insured plans, averaged across single and family plans.
- Share of plans requiring referrals.

ACS Data. I use the American Community Survey (ACS) to supplement labor force variables missing from the LEHD. 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 in households between the ages of 18 to 64 who work for wages. I calculate the employment rate at the CZ level equal to the share of the labor force employed, and the employment rate by education (the share of the college/noncollege-educated labor force employed). I obtain the part-time worker share equal to the CZ share of employed workers who usually worked less than 30 hours weekly during the previous 12 months. I obtain CZ-level annual wage earnings, hourly wages, and hours worked by averaging across employed individuals in a given year. Hourly wage rates are calculated by dividing annual income earned over the previous 12 months by usual 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.

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 homogeneous 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. I identify households' residences by merging the ASEC with 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 Graham et al. (2017) for detailed methodology on the data construction). My analyses focuses on an annual, repeated cross-section of ASEC respondents from 2001 to 2018.

I calculate two outcomes at the CZ level. First, I restrict the sample to employed workers between the ages of 18 to 64 in the private-sector. I then calculate the share of workers in the CZ enrolled in an ESHI plan offered by their own employer, using respondent weights. Second, I restrict the sample to all individuals under the age of 65. I calculate the share of individuals in the CZ covered by ESHI, either directly or as a dependent, again using respondent weights.

Calibration Data. I use the same dataset on employment and employer-sponsored premiums from 1999 to 2019 as Finkelstein et al. (2023). Employment outcomes, including wages and employment rates by education, are obtained from the Current Population Survey, while premium outcomes are obtained from the Medical Expenditure Panel Survey. Data accessed August 2024 via:
`https://www.brookings.edu/articles/the-health-wedge-and-labor-market-inequality/`.

To calculate employment changes in Canada as the counterfactual, I obtain publicly-available data from Canada Statistics. I sum employment counts for individuals between 15 to 64 and divide by the working-age population (15 to 64). Data accessed September 2024 via:
`https://www150.statcan.gc.ca/n1/en/type/dataMMĪ#tables`.

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

- **Labor force participation and unemployment, Bureau of Labor Statistic Local Area Unemployment Statistics 1999–2020.** I sum counts of unemployed and not in labor force

individuals across counties within a CZ for a given year and divide by the working-age population. Accessed July 2024 via: `https://www.bls.gov/lau/tables.htm#cntyaa`.

- **Employment, U.S. Census Bureau County Business Patterns 1999–2020.** I sum employment counts across counties within a CZ for a given year and divide by the working-age population. Accessed August 2024 via: `https://www.census.gov/programs-surveys/cbp/data/datasets.html`.
- **Self employment, U.S. Bureau of Economic Analysis 1999–2020.** I sum counts of self-employed individuals across counties in a CZ for a given year and divide by the working-age population.
- **Government transfers per capita, U.S. Bureau of Economic Analysis 1999–2020.** I sum transfers by category across counties in a CZ for a given year and divide by the working-age population. Categories include medical care (mostly Medicare and Medicaid), federal income assistance (mostly EITC, SSI, SNAP, and AFDC/TANF programs), education assistance, unemployment benefits, and other benefits (net of SSA benefits).
- **Retirement and disability benefits per capita, Social Security Administration (SSA) 1999–2020.** I sum retirement benefits received by retired workers, and disability benefits received by workers with disabilities across counties in a CZ for a given year, and divide by the working-age population. Accessed July 2024 via: `https://www.ssa.gov/policy/docs/statcomps/oasdi_sc/1999/index.html`.
- **Hospital HHI, AHA Annual Survey of Hospitals 1999–2020.** 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 1999-2020.** I sum population between 18 to 64 across all counties in a CZ annually. Accessed via: `https://seer.cancer.gov/popdata/download.html`.
- **Rural share, Census 2000.** I calculate the average rural share across counties in a CZ.
- **Median household income, Census 2000.** I calculate the average median house value across counties in a CZ.
- **Crosswalks Files.** Crosswalks linking counties to commuting zones, public use micro areas to commuting zones, and commuting zones to census divisions made available by Autor and Dorn (2013) (`https://www.ddorn.net/data.htm`). Additional crosswalk linking zip codes to counties downloaded from HUD USPS website.

B. Alternative Control Groups

My baseline sample compares firms in 81 earlier-exposed CZs to firms in 39 later-exposed CZs. The choice of control CZs is justified by the similar characteristics between earlier-exposed and later-exposed CZs and the parallel trends in affected firms' premiums and employment leading up to the insurer mergers by merger exposure, as shown in Figure 4 and 7. This section examines two alternative definitions of the control CZ group: never-exposed CZs and out-of-market CZs. Never-exposed CZs are defined as CZs where the two merging insurers are not both active in the CZ prior to the merger for all observed mergers from 1999 to 2021. Never-exposed and ever-exposed CZs are mutually exclusive and collectively exhaustive; out of the 683 CZs in the Form 5500, I identify 246 CZs that are exposed to at least one merger, and 437 CZs that are never exposed. Among the 437 never-exposed CZs in the Form 5500, I obtain a balanced sample of 302 never-exposed CZs, for which I observe premiums for each year of the observation window. Out-of-market CZs are a subset of later-exposed and never-exposed CZs, defined as those CZs with one but not both merging insurers present in the CZ before a given merger. My sample includes 222 out-of-market CZs.

Appendix Table A.1 presents summary statistics for earlier-exposed CZs (column 1), later-exposed CZs (the baseline control group, column 2), and alternative control groups: never-exposed CZs (column 3) and out-of-market CZs (column 4). Never-exposed CZs differ in market characteristics from earlier-exposed CZs; never-exposed CZs tend to have more concentrated insurer markets, smaller populations, lower household incomes, higher unemployment rates, and are generally more rural. In contrast, the out-of-market CZs share similar characteristics with earlier-exposed CZs. Note that out-of-market CZs may be unsuitable as controls because they could still experience merger effects even if they do not experience reduced insurer competition, such as efficiency cost savings, or changes in health plan quality.

I present regressions results from equation (1) with different control CZ groups in Appendix Figure A.1 (see Appendix Tables A.2 and A.3 for corresponding estimates). The outcome of interest is annual premiums per participant in fully-insured firms using the Form 5500 data in Panel A, and aggregate employment (i.e., the share of working-age population employed in any firm) using the County Business Patterns (CBP) data in Panel B. Given that the U.S. Census restricts repeated disclosure of results for similar sets of sample CZs, I use publicly available CBP data to test the robustness of my employment findings to varying the

control group.

Panel A indicates that the estimated effects on annual employer-sponsored premiums, using alternative control CZs, exhibit a similar inverted U-shaped increase as the baseline estimates with later-treated CZs (indicated by the blue circular markers). While the estimates for the alternative control CZs are not statistically distinguishable from zero, their confidence intervals significantly overlap with those of the baseline estimates.[33]

Panel B indicates that the estimated effects on the population share employed in any firm are statistically significant and similar in magnitude across different control CZs, with the estimated decline in the aggregate employment-to-population ratio ranging from 0.66 to 0.88 percentage points (1.2 to 1.7 percent). However, there appears to be a positive pre-trend for employment in earlier-exposed CZs when compared to never-exposed CZs. Earlier-exposed CZs appear to lose employment relative to never-exposed CZs even before the merger event, which suggests that never-exposed CZs may not be an appropriate counterfactual to the earlier-exposed CZs.

[33]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 yield similar and also insignificant results.

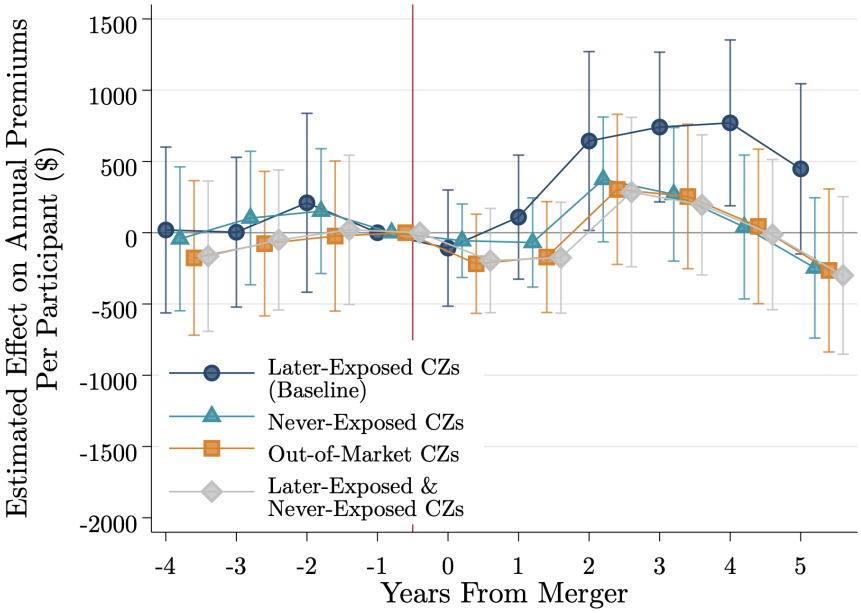
TABLE A.1: CZ-Level Summary Statistics: Alternative Control CZs

	(1) Earlier-Exposed CZs	(2) Later-Exposed CZs	(3) Never-Exposed CZs	(4) Out-of-Market CZs
Number of Insurers	12.89	10.46	8.57	11.03
	(6.78)	(4.57)	(6.21)	(6.58)
Insurer HHI	2,788.90	2,707.80	3,492.92	2,774.49
	(1,537.53)	(1,263.27)	(2,112.46)	(1,585.86)
Per-Participant Premiums	4,654.99	4,766.53	4,502.17	4,372.70
	(2,111.59)	(2,163.24)	(2,345.30)	(2,212.23)
Population	474,868.22	362,758.77	344,319.83	444,161.02
	(240,030.48)	(161,038.95)	(367,126.99)	(394,827.86)
Median Household Income	53,358.33	54,068.06	48,292.90	49,990.90
	(7,925.50)	(7,429.74)	(6,861.48)	(7,466.95)
Unemployment Rate	6.84	6.02	7.32	6.99
	(2.69)	(2.18)	(3.27)	(3.06)
Share Rural	0.51	0.53	0.57	0.54
	(0.19)	(0.15)	(0.18)	(0.18)
Observations	810	1,840	19,000	7,190
Unique CZs	81	39	302	222

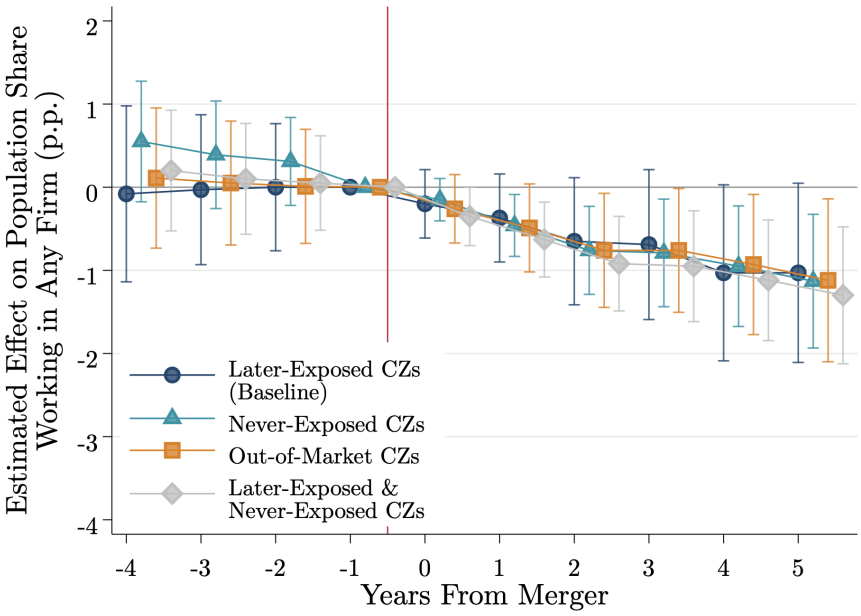
Notes: This table displays summary statistics for earlier-exposed CZs (column 1), later-exposed CZs (column 2), out-of-market CZs (column 3), and never-exposed CZs (column 4). Premiums are the sum of premiums paid by employers and employees divided by the number of participants. Nominal dollars are converted 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.

FIGURE A.1: CZ-Level Effects of Insurer Mergers: Alternative Control CZs

(A) Premiums



(B) Aggregate Employment



Notes: This figure plots the yearly coefficients β_l from equation (1), varying the control CZ group. The outcome y_{ct} is the annual employer-sponsored premiums per participant in fully-insured firms in Panel A, and the share of the working-age population employed in any firm in Panel B. 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; 39 later-exposed CZs; 302 never-exposed CZs; 222 out-of-market CZs; and 341 later-exposed or never-exposed CZs.
Sources: Form 5500, and County Business Patterns (Panel B).

TABLE A.2: CZ-Level Effect of Insurer Mergers on Premiums: Alternative Control CZs

	Outcome: Annual Premiums Per Participant (2019)			
	(1)	(2)	(3)	(4)
		Control Group:		
	Baseline: Later-Exposed CZs	Never-Exposed CZs	Out-of-Market CZs	Later-Exposed & Never-Exposed CZs
Merger $\times$(Year $= -4$)	19.15	-43.05	-176.84	-164.49
	(296.83)	(257.59)	(276.69)	(268.92)
Merger $\times$(Year $= -3$)	3.54	102.97	-76.57	-50.81
	(268.01)	(239.00)	(258.64)	(250.72)
Merger $\times$(Year $= -2$)	209.91	151.30	-23.37	20.27
	(320.14)	(223.41)	(268.68)	(267.32)
Merger $\times$(Year $= -1$)	-	-	-	-
Merger $\times$(Year $= 0$)	-107.87	-56.30	-218.06	-195.43
	(208.10)	(131.55)	(177.53)	(186.55)
Merger $\times$(Year $= 1$)	109.27	-68.69	-171.05	-175.33
	(222.08)	(159.68)	(198.45)	(198.49)
Merger $\times$(Year $= 2$)	643.38**	373.89*	304.15	285.25
	(320.14)	(223.41)	(268.68)	(267.32)
Merger $\times$(Year $= 3$)	741.30***	269.13	254.03	195.09
	(268.01)	(239.00)	(258.64)	(250.72)
Merger $\times$(Year $= 4$)	770.69**	39.94	44.28	-13.20
	(296.83)	(257.59)	(276.69)	(268.92)
Merger $\times$(Year $= 5$)	448.01	-246.97	-264.83	-299.99
	(304.74)	(251.19)	(291.90)	(282.12)
Years 0-5 Estimate	434.13**	51.83	-8.58	-33.94
	(201.74)	(157.33)	(196.89)	(190.56)
Mean Premiums	4450.48	4450.48	4450.48	4450.48
Unique Earlier-Exposed CZs	81	81	81	81
Unique Control CZs	39	302	222	341
Observations	2,650	19,810	8,000	21,650
R-squared	0.56	0.51	0.56	0.51

Notes: This table reports difference-in-differences estimates with different control groups. The outcome is annual premiums per participant. Column 1 replicates results in Figure 4 using later-exposed CZs as controls. Never-exposed CZs do not experience insurer merger activity between 1999 and 2021. Out-of-market CZs are a sub-sample of later-exposed and never-exposed CZs that only have one merging insurer active in the market before the merger, so are not exposed to a merger-induced increase in insurer concentration. Nominal dollars are converted to 2019 dollars. Standard errors clustered at the CZ level are in parentheses.
Sources: Form 5500.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.3: CZ-Level Effect of Insurer Mergers on Employment: Alternative Control CZs

	Outcome: Employment-to-Population Ratio (EPOP)			
	(1)	(2)	(3)	(4)
		Control Group:		
	Baseline: Later-Exposed CZs	Never-Exposed CZs	Out-of-Market CZs	Later-Exposed & Never-Exposed CZs
Merger $\times$(Year $= -4$)	-0.08	0.55*	0.11	0.20
	(0.54)	(0.37)	(0.43)	(0.37)
Merger $\times$(Year $= -3$)	-0.03	0.39	0.05	0.10
	(0.46)	(0.33)	(0.38)	(0.34)
Merger $\times$(Year $= -2$)	-0.00	0.31	0.01	0.05
	(0.39)	(0.27)	(0.35)	(0.29)
Merger $\times$(Year $= -1$)	-	-	-	-
Merger $\times$(Year $= 0$)	-0.20	-0.15	-0.26	-0.35*
	(0.21)	(0.13)	(0.21)	(0.18)
Merger $\times$(Year $= 1$)	-0.37	-0.46**	-0.49*	-0.63***
	(0.27)	(0.19)	(0.27)	(0.23)
Merger $\times$(Year $= 2$)	-0.65*	-0.76***	-0.76**	-0.92***
	(0.39)	(0.27)	(0.35)	(0.29)
Merger $\times$(Year $= 3$)	-0.69	-0.79**	-0.76**	-0.95***
	(0.46)	(0.33)	(0.38)	(0.34)
Merger $\times$(Year $= 4$)	-1.03*	-0.95**	-0.93**	-1.12***
	(0.54)	(0.37)	(0.43)	(0.37)
Merger $\times$(Year $= 5$)	-1.03*	-1.13***	-1.12**	-1.30***
	(0.55)	(0.41)	(0.50)	(0.42)
Years 0-5 Estimate	-0.66*	-0.71***	-0.72**	-0.88***
	(0.36)	(0.23)	(0.31)	(0.25)
Mean EPOP	53.00	53.00	53.00	53.00
Unique Earlier-Exposed CZs	81	81	81	81
Unique Control CZs	39	302	222	341
Observations	2,650	19,810	8,000	21,650
R-squared	0.98	0.96	0.97	0.96

Notes: This table reports difference-in-differences estimates with different control groups. The outcome is employment-to-population ratio. Column 1 replicates results in Figure 7 using later-exposed CZs as controls. Never-exposed CZs do not experience insurer merger activity between 1999 and 2021. Out-of-market CZs are a sub-sample of later-exposed and never-exposed CZs that only have one merging insurer active in the market before the merger, so are not exposed to a merger-induced increase in insurer concentration. Nominal dollars are converted to 2019 dollars. Standard errors clustered at the CZ level are in parentheses.

Sources: Form 5500, County Business Patterns (CBP).

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

C. Successive Mergers

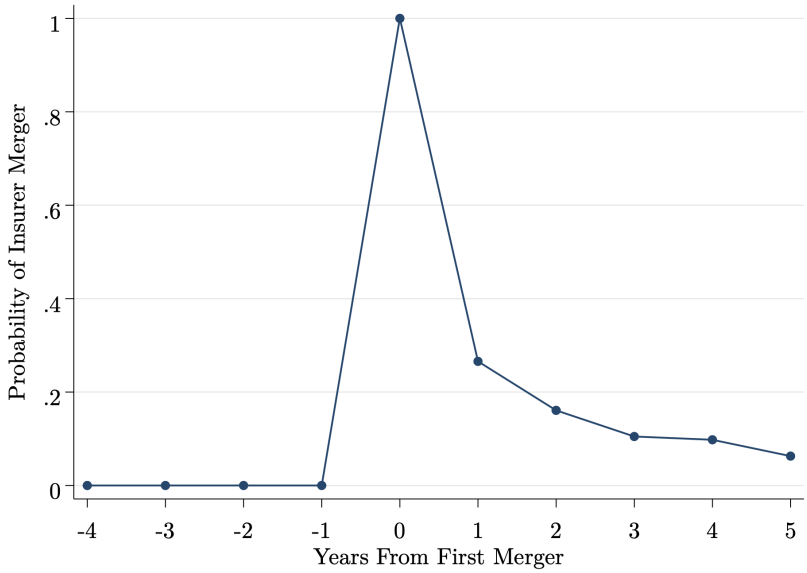
In the main analysis, I focus on 81 earlier-exposed treatment CZs that experience a first-observed insurer merger and are not exposed again over the 6 years after the merger. To examine the impact on CZs that experience mergers in successive years, I expand the sample of earlier-exposed treatment CZs to include CZs that experience a first-observed merger and additional mergers in the following 6 years after the merger. 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 $t = 0$. In the expanded sample, there are 41 unique mergers occurring either in the year of the first merger or in subsequent years within the observation window. Appendix Figure A.2 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 affected 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.

Appendix Figure A.3 present the difference-in-differences results from estimation equation (1) using the expanded sample and varying the control group of CZs (see Appendix Section B for construction of control CZ groups). Focusing on my baseline specification that compares first-exposed CZs to later-exposed CZs, I find a negative pre-trend in the number of insurers in the CZ in Panel A, with earlier-exposed CZs having more entrants than later-exposed ones. However, this trend reverses sharply in the year of the first merger, dropping by 7 percent (from 27 to 25 insurers), and continues to decline steadily in the post-merger period due to subsequent mergers within the observation window. The finding is consistent with a reduction in insurer concentration due to the insurer mergers. Correspondingly, Panel B shows that insurer concentration increases 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.

Consequently, I find that premiums increase by 7 percent ($315, standard error = 161) on average in the years following the merger following an inverted u-shape pattern. The estimated increase of 7 percent using the expanded sample is similar in size to the estimated increase of 10 percent using the baseline sample. The expanded sample exhibits significantly different characteristics, including more insurers (27 compared to 14) and less concentrated markets (2077 vs. 2500 HHI points) than the baseline sample. These differences, along with the possi-

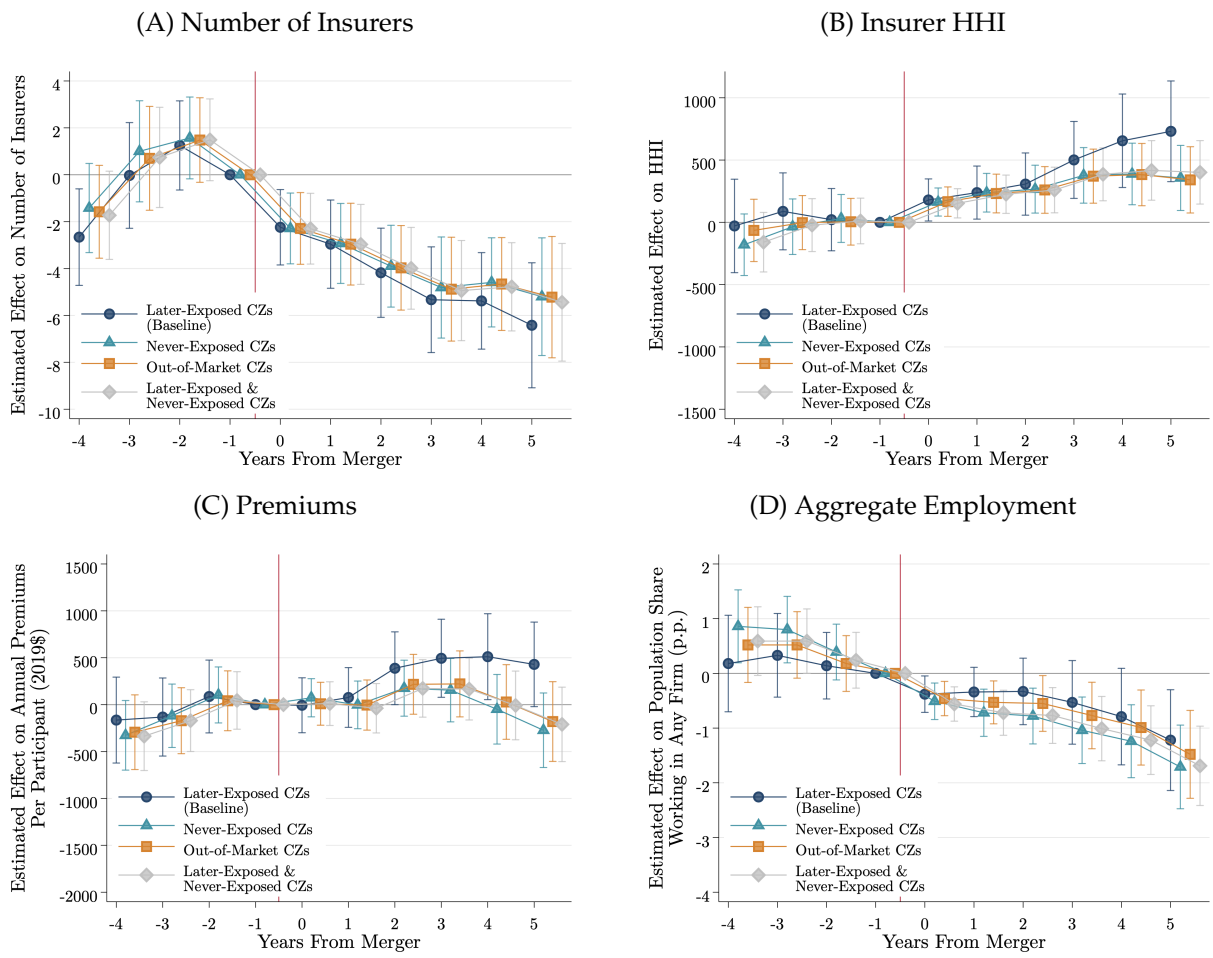
bility that additional mergers may lead to smaller post-merger increases in HHI, could explain why we do not observe even larger, sustained premium increases after adding successively exposed CZs. Examining the employment effects of insurer mergers, I find that the share of the working-age population employed in any firm declines by 0.6 percentage points (1 percent) following the first merger and continues to decrease in subsequent years as more mergers are consummated. Overall, the findings from expanded samples of treatment and control CZs yield relatively robust results that confirm those obtained from the baseline sample.

FIGURE A.2: Successive Insurer Mergers and Probability of Merger



Notes: This figure plots the probability that an earlier-exposed CZ experiences one or more insurer mergers 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.3: CZ-Level Effect of Successive Insurer Mergers on Concentration, Premiums, and Employment, with Varying Groups of Control CZs



Notes: This figure plots the yearly coefficients β_l from equation (1) examining the impact of successive insurer mergers on insurer concentration (A and B), fully-insured firms' annual premiums (C), and aggregate employment (D) at the CZ level. My baseline specification (blue circle markers) compares outcomes in earlier-exposed CZs that may experience additional mergers in the 6 years following the first merger to later-exposed CZs. Alternative specifications include never-treated and/or out-of-market CZs. 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 first-exposed CZs experiencing at least 1 merger during the observation window; 40 later-exposed CZs; 303 never-exposed CZs; 247 out-of-market CZs; and 343 later-exposed or never-exposed CZs.
Source: Form 5500.

D. Sun and Abraham (2021) Estimation

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). I address these concerns by implementing the Sun and Abraham (2021) interaction-weighted

(IW) estimator.

Conceptual Framework. I lay out a simple framework to spell out what is being estimated in the event studies. I consider a setting with N CZs observed over $T+1$ time periods. I compare the treatment group of earlier-exposed CZs (who experience a first observed merger between 2003 and 2015) to a matched control group of later-exposed CZs (for a given merger year, all CZs that experience an insurer merger 7 or more years are assigned a placebo merger equal to the merger year). A CZ c is categorized into a cohort based on the year of first merger treatment E_c. I define the cohort-specific average treatment effect on the treated in commuting zone c and event year l relative to the first-observed merger year e (where $e+l=t$) as follows:

$$CATT_{el} = E[y^e_{c,e+l} - y^f_{c,e+l} | E_c = e]. \tag{7}$$

where $y^e_{c,e+l} - y^f_{c,e+l}$ is the CZ-level treatment effect (the difference between the observed outcome relative to the later-treated counterfactual outcome) and later-treated CZs are exposed to a first merger in year $f > e$. With this framework, I outline the Sun and Abraham (2021) three-step procedure.

Step 1. I estimate cohort-specific average treatment effects on the treated ($CATT_{el}$), where cohorts are uniquely defined by their merger year e. I restrict the observation window to 4 years before and 6 years after the merger (including the merger year) such that $l \in [-4, 5]$. The estimating equation is:

$$y^e_{ct} = \sum_e \sum_{l \neq -1} \beta_{el} [\mathbb{1}(\text{Merger}_c) \cdot \mathbb{1}(\text{Year}_t = l) \cdot \mathbb{1}(E_c = e)] + \alpha_t + \delta_c + \varepsilon_{ct} \tag{8}$$

where $\mathbb{1}(\text{Merger}_c)$ is a binary indicator equal to 1 for all t if CZ c is first treated by an insurer merger, and equal to 0 if CZ c is later treated in year $f > e$, $\mathbb{1}(\text{Year}_t = l)$ is an event-time indicator, $\mathbb{1}(E_c = e)$ is a cohort indicator if CZ c is first exposed to a merger in calendar year e, 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. I omit the event-time indicator in the year before the merger ($l = -1$). I weight each CZ-year by the CZ population in the year before the merger. 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 $Pr\{E_c = e | E_c \in [-l, T-l]\}$ by calculating sample shares for

each cohort in the relevant period $l \in g$.

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

$$\hat{v}_l = \frac{1}{|g|} \sum_{l \in g} \sum_{e} \hat{\beta}_{el} \hat{Pr}\{E_c = e | E_c \in [-l, T-l]\} \tag{9}$$

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 normalized by the size of g.

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

E. An Instrumental Variables Approach

In my baseline difference-in-differences specification (equation (1)), I regress premiums on an interaction between a merger exposure indicator and an event-time indicator. The approach has the strength that it 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. However, the model assumes merger exposure is randomly assigned which may be a strong assumption. In this Section, I explore alternative models with weaker assumptions to estimate the impact of mergers on premiums.

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

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

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.4. Given that insurer HHI increases by roughly 20 percent post-merger (Appendix Figure A.7), the estimate implies a 36 percent increase ($= 20 \cdot 1.88$) in premiums. The estimate is significantly larger than the baseline estimate of 10 percent (Figure 4).

Of course, one obvious problem with the OLS model (10) 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 bias from omitted variables, 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. Following Dafny et al. (2012), the predicted merger exposure is equal to the sum of merging insurers' market shares before the merger. The first-stage estimation equation is:

$$\text{Log HHI}_{ct} = \sum_l \beta_l [\text{Merger Exposure}_{ct} \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_t + \delta_c + \varepsilon_{ct}, \tag{11}$$

which assumes the level of exposure to merger exposure is randomly assigned ($E[\varepsilon_{ct} | \mathbb{1}(\text{Merger Exposure}_c) \cdot \mathbb{1}(\text{Year}_t = l), \alpha_t, \delta_c] = 0$). The assumption is plausible because merging insurers' local market

shares are pre-determined before the merger, and therefore weaker than the baseline model. I report first-stage results in column (1) of Appendix Table A.4. Unfortunately, the proposed instrument is not valid due to its weak correlation with the observed change in insurer HHI; insurer HHI increases by only 0.25 percent (standard error = 0.20) for each 1 percentage point increase in the combined market share of merging insurers. The result is insignificant, violating the relevance assumption, and the instrument is considered weak, as the F-statistic is below 10 (F = 6.21).[34] For completeness, I include the reduced form and instrumental variables (IV) results in columns 2 and 3, but I do not place much emphasis on these results due to the weak first stage.

[34] I also tried specifications using a simulated change in insurer HHI as the exposure measure, but found similarly results.

TABLE A.4: CZ-Level Effect of Mergers: 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 the yearly coefficients β_l examining the impact of insurer mergers using alternative regression specifications. All specifications include CZ and year fixed effects. The outcome is log insurer HHI in columns 1, and log premiums in columns 2 to 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 exponentiated mean outcome for earlier-exposed CZs in the year before the merger and F-statistics from the first stage in column 1. Premiums are converted to 2019 dollars. The sample is at the CZ-year level from 1999 to 2021. N=81 treated CZs and 39 control CZs.
Source: Form 5500.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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:

$$\omega_g = w_g + \tau = \lambda_g L_g^{\rho-1}(\lambda_N L_N^{\rho} + \lambda_C L_C^{\rho})^{\frac{1-\rho}{\rho}} \tag{12}$$

I take the ratio of (12) for college to non-college workers and solve for λ_N as a function of λ_C.

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

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

Subbing (14) into (12) I can solve for λ_C:

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

$$\lambda_C = [(w_C + \tau) L_C^{1-\rho}((\frac{w_N + \tau}{w_C + \tau}) \cdot (\frac{L_N}{L_C})^{1-\rho} \cdot L_N^{\rho} + L_C^{\rho})^{\frac{\rho-1}{\rho}}]^{\rho} \tag{16}$$

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 $\underline{\kappa}$:

$$\underline{\kappa} = (w_g + \alpha\tau) - P_g \cdot (\overline{\kappa} - \underline{\kappa}) \tag{17}$$

$$\Rightarrow \underline{\kappa} = (w_g + \alpha\tau) - \frac{w_g}{e_g^S} \tag{18}$$

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

$$\overline{\kappa} = \frac{w_g + \alpha\tau - \underline{\kappa}}{P_g} + \underline{\kappa} \tag{19}$$

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:

$$y_{j(c)t} = \sum_{l} \beta_l [\mathbb{1}(\text{Merger}_{j(c)}) \cdot \mathbb{1}(\text{Year}_t = l)] + \alpha_{lt} + \delta_{j(c)} + \varepsilon_{j(c)t} \tag{20}$$

where $\mathbb{1}(\text{Merger}_{j(c)})$ is a indicator for firm j in CZ c that is earlier-exposed to a first insurer merger, $\mathbb{1}(\text{Year}_t = l)$ is an event-time indicator, 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 (20) in Appendix Figure A.24. 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 to 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 an unbalanced firm panel at the CZ-level are 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.24 Panel D presents results from estimating equation (20). 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 noncollege-educated workers, which is consistent with the CZ-level evidence shown in Panel B of Figure 11. The finding is consistent with a fixed per-worker cost increase regardless of workers' wage earnings which makes noncollege-educated workers relatively more expensive to hire than their college-educated counterparts.

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 before 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.25 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 ($t-1$). 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 causal evidence of the impact of merger-induced ESHI cost increases on workers. Given the 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 quantify merger effects. I use the following worker-level difference-in-difference model:

$$y_{i(c)t} = \beta[\mathbb{1}(\text{Merger}_{i(c)}) \cdot \mathbb{1}(\text{Year}_t \geq 0)] + \alpha_{lt} + \delta_{i(c)} + \boldsymbol{X}'_{i(c),t}\boldsymbol{\Gamma} + \varepsilon_{i(c)t} \tag{21}$$

where $\mathbb{1}(\text{Merger}_{i(c)})$ is a indicator for worker i who is employed in CZ c that is earlier-exposed to a first insurer merger, $\mathbb{1}(\text{Year}_t \geq 0)$ is an indicator that calendar year t falls on or after the year of the merger, 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.

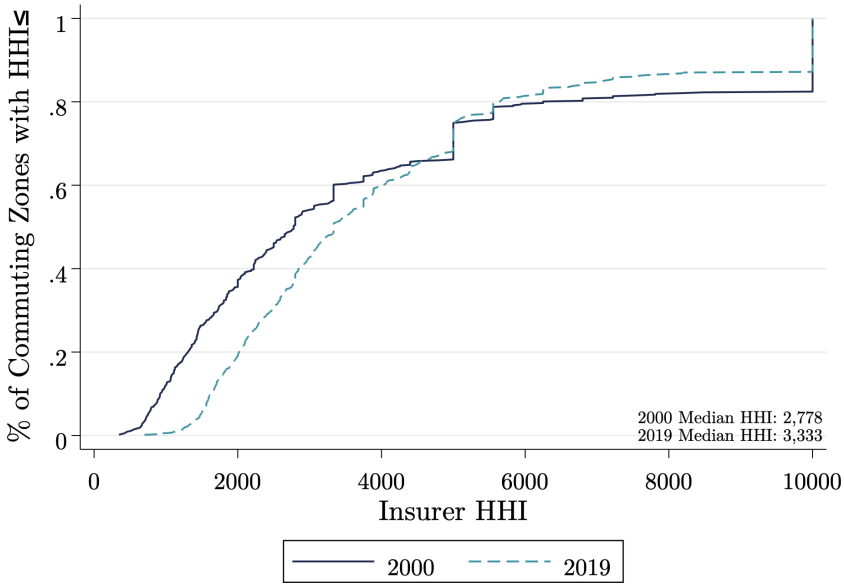
Table A.5 Panel A presents the corresponding estimates to Figure A.25 from estimating equation (21). Workers are 2.6 percentage points (standard error = 0.1) less likely to be employed in their pre-merger firm but are only 0.4 percentage points (standard error = 0.1) less likely to be employed in any 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. These findings suggest that most workers without work following mergers reallocate to new firms leading to smaller overall declines in employment and wages in the labor market.

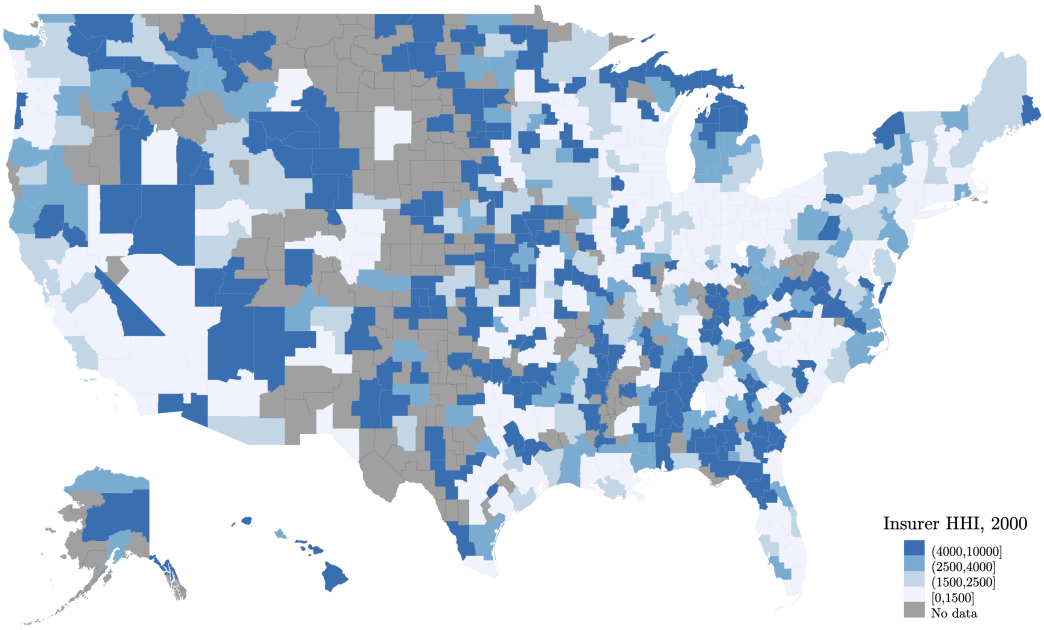
H. Additional Figures

FIGURE A.4: Insurer Concentration

(A) CDF of Insurer HHI, 2000 and 2020



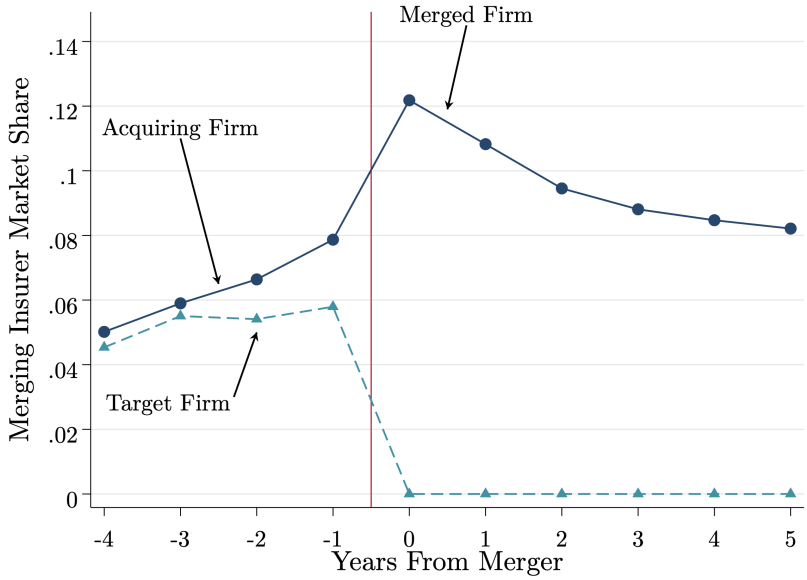
(B) Insurer HHI by Commuting Zone, 2000



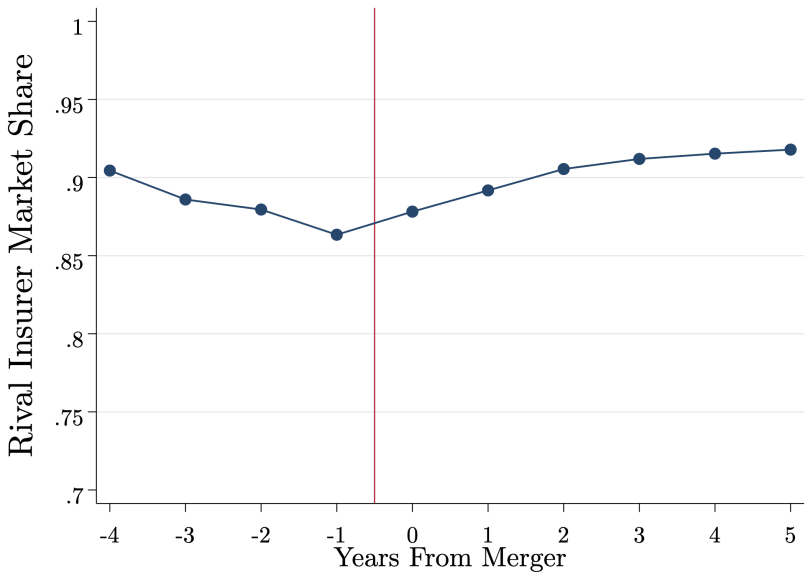
Source: Form 5500.

FIGURE A.5: Market Share Trends in Treated Commuting Zones

(A) Merging Insurer Market Shares

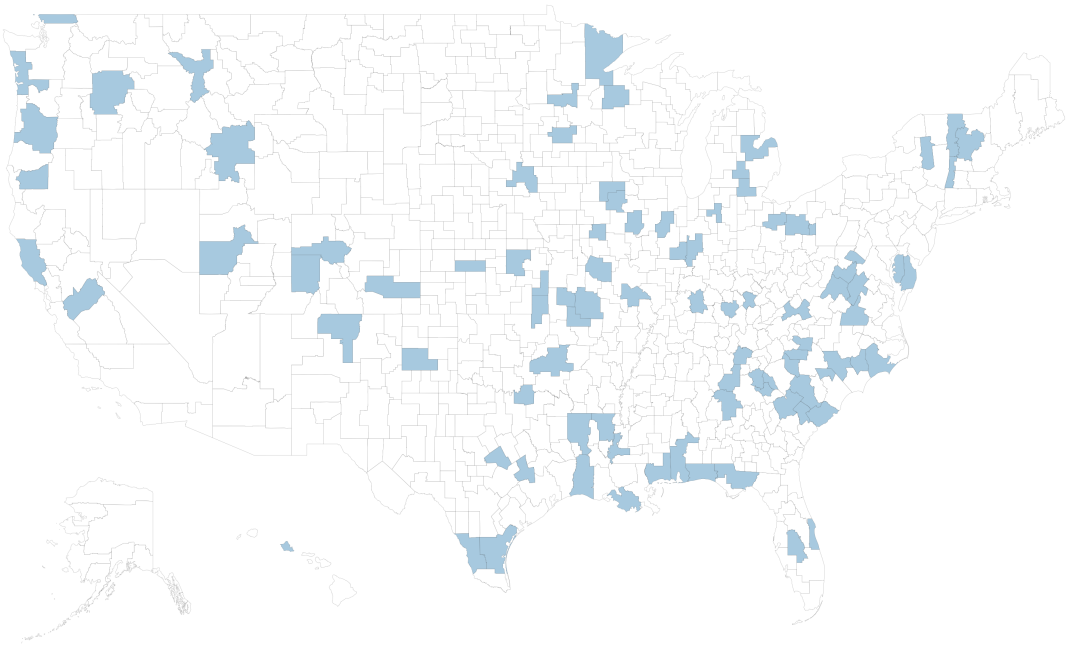


(B) Rival Insurer Market Share



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 before 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.

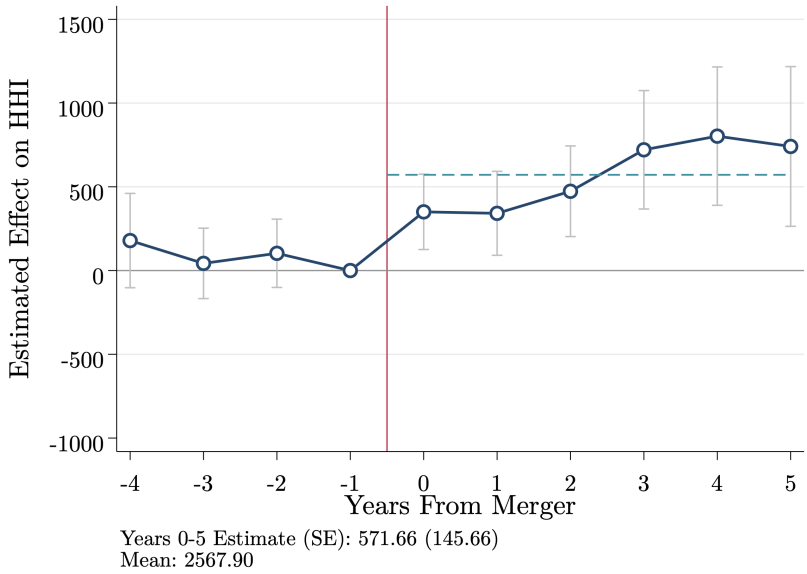
FIGURE A.6: Commuting Zone Sample



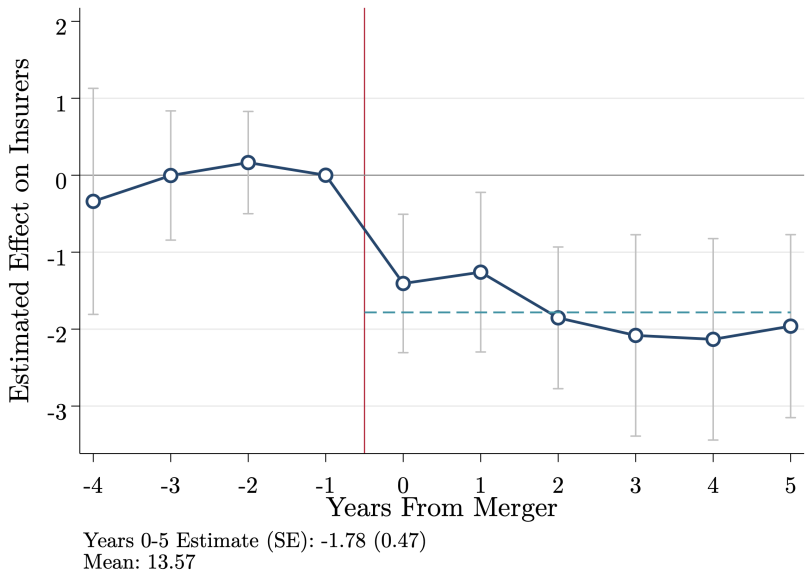
Notes: This figure shows the analysis sample of commuting zones shaded in blue. N=93 CZs.
Source: Form 5500.

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

(A) Insurer HHI

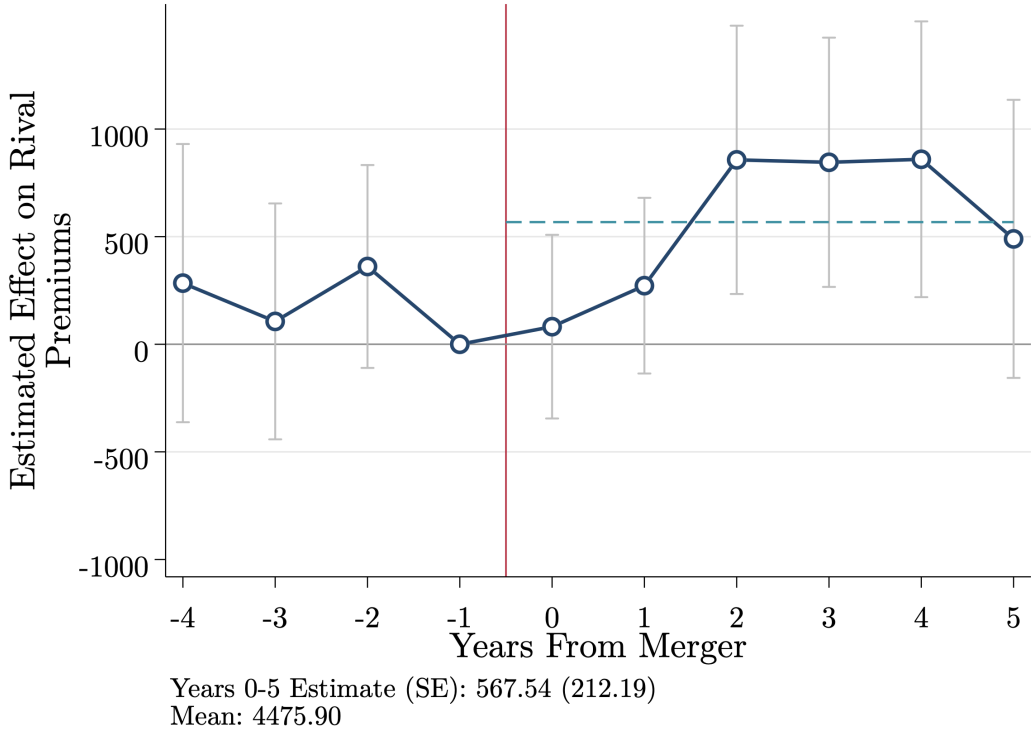


(B) Number of Insurers



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.

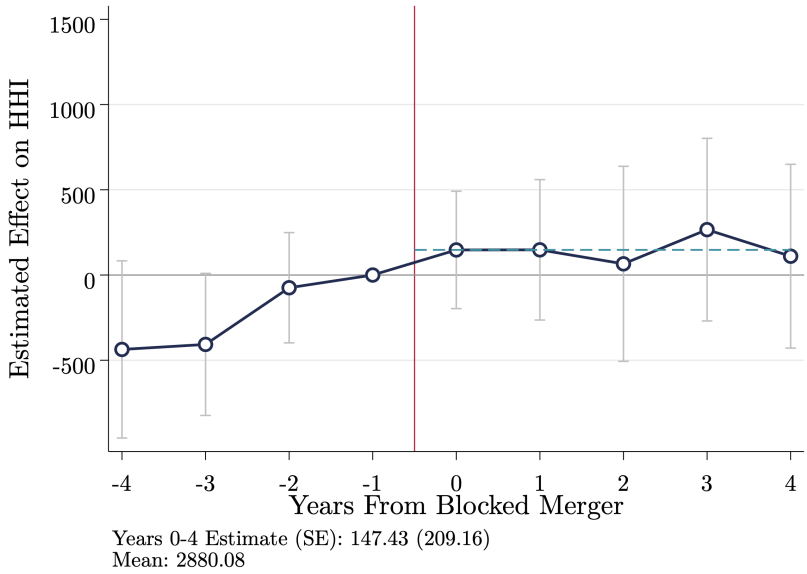
FIGURE A.8: CZ-Level Effect of Insurer Mergers on Rival Premiums



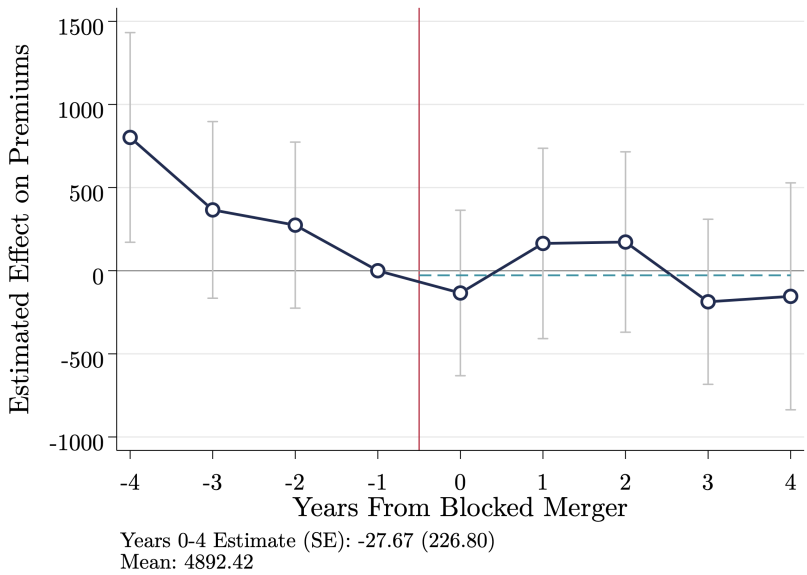
Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is rival premiums. Later-exposed CZs are included once per earlier-exposed CZ, and I include merger fixed effects. Observations are weighted by CZ working-age population in the year before the merger. The horizontal 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.9: CZ-Level Effect of **Blocked** Insurer Mergers on Concentration & Premiums

(A) Insurer HHI



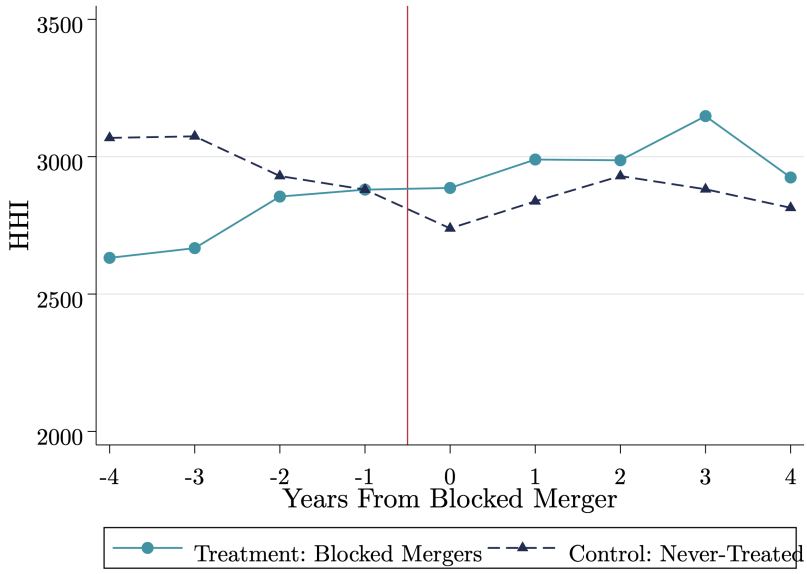
(B) Premiums



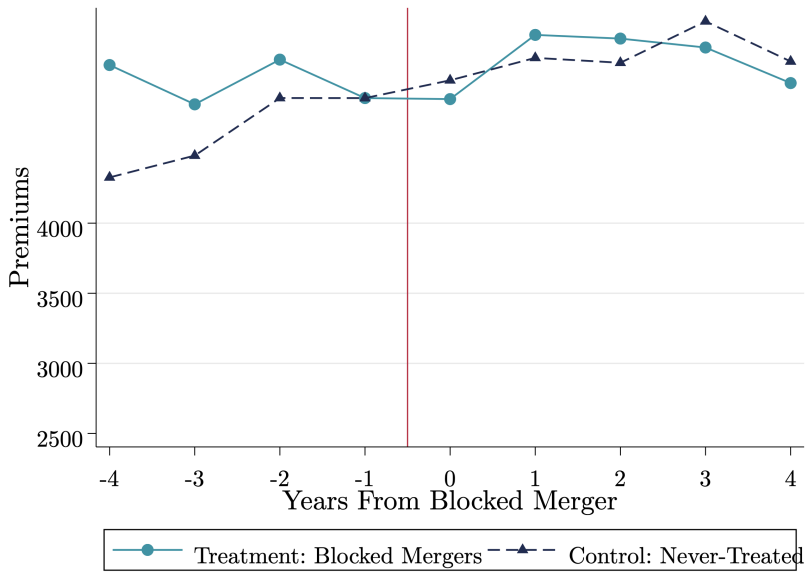
Notes: This figure plots the yearly coefficients β_l from equation (1) of a placebo test studying blocked insurer mergers. Placebo exposed CZs are CZs that would have been earlier-exposed by a merger blocked by antitrust action and control CZs are never exposed by a merger. The outcome y_{ct} is insurer 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 exposed 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 exposed CZ and 226 control CZs. Figures of raw trends are shown in Appendix Figure A.10.
Source: Form 5500.

FIGURE A.10: Blocked Mergers: Insurer Concentration & Premiums by Treatment

(A) Insurer HHI

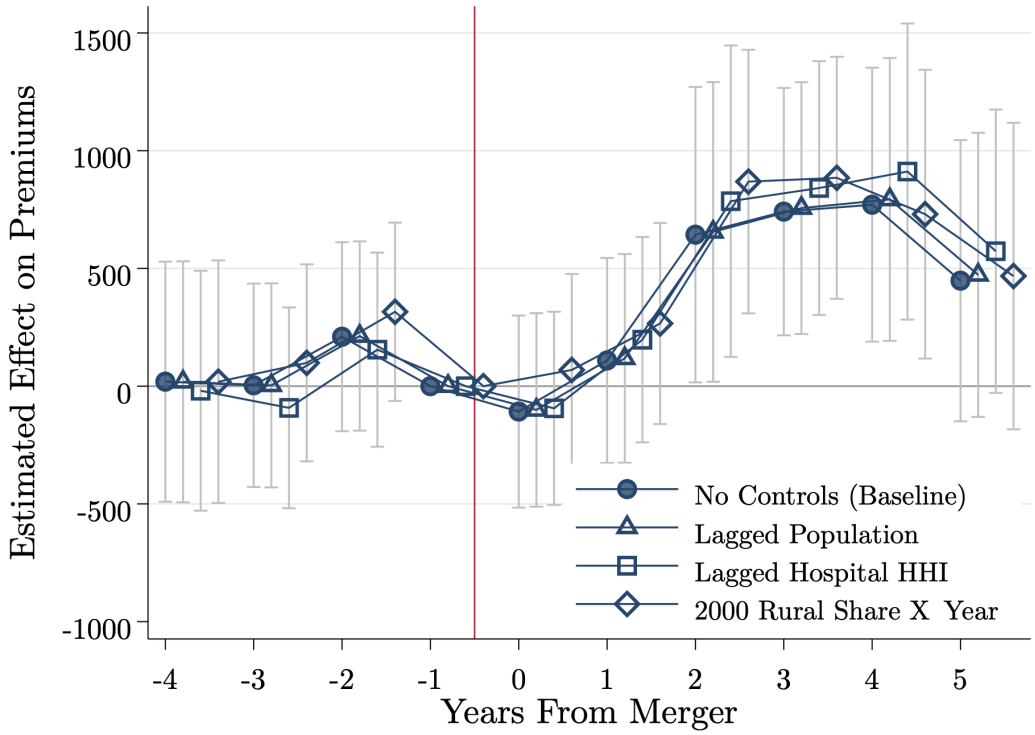


(B) Premiums



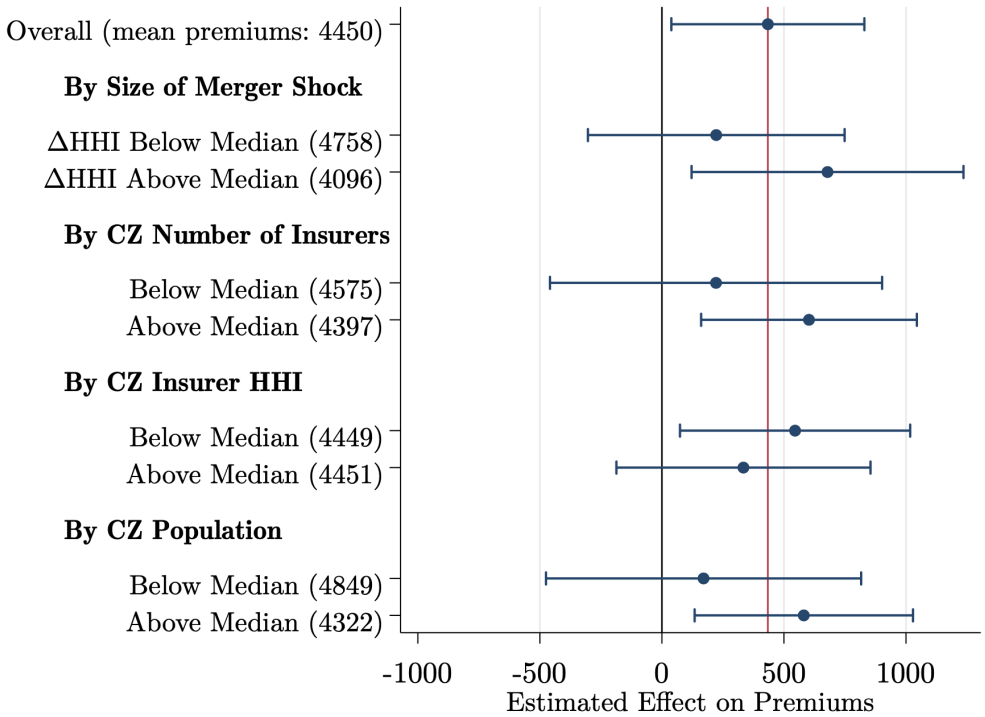
Notes: This figure plots trends in the population-weighted mean CZ insurer HHI (panel A), and premiums (panel B). Placebo treated CZs are CZs that would have been earlier-exposed by a merger blocked by antitrust action and control CZs are never treated by a merger. Observations are weighted by CZ working-age 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.11: CZ-Level Effect of Insurer Mergers on Premiums in Fully-Insured Firms: Varying Controls



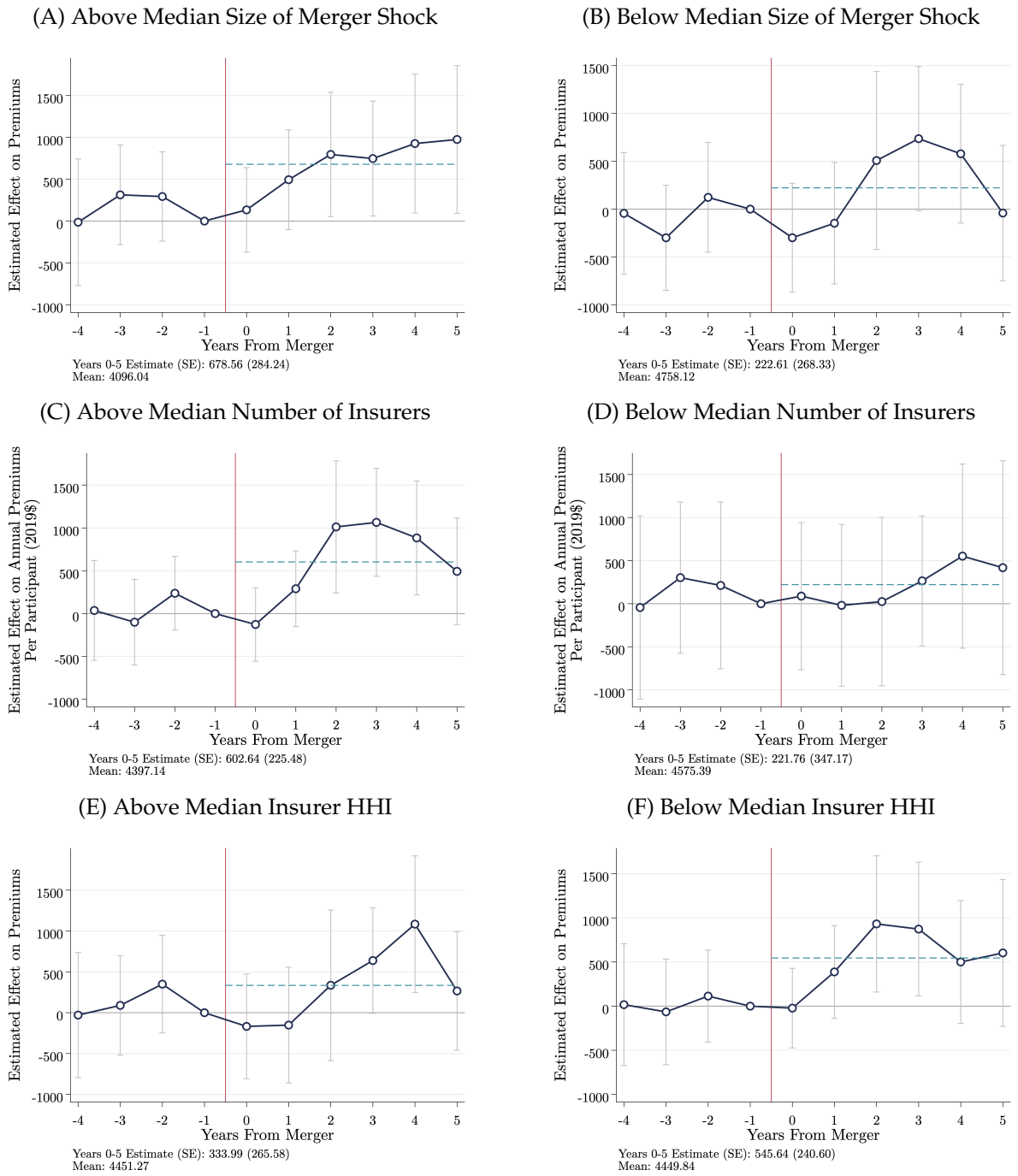
Notes: This figure plots the yearly coefficients β_l from equation (1), varying the set of regression controls. The outcome y_{ct} is premiums. Shaded circle markers report baseline results with no controls, triangle markers report results with lagged population controls, square markers report results with lagged hospital HHI as controls, and diamond markers report results controlling flexibly for the 2000 rural share. 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.12: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Analysis



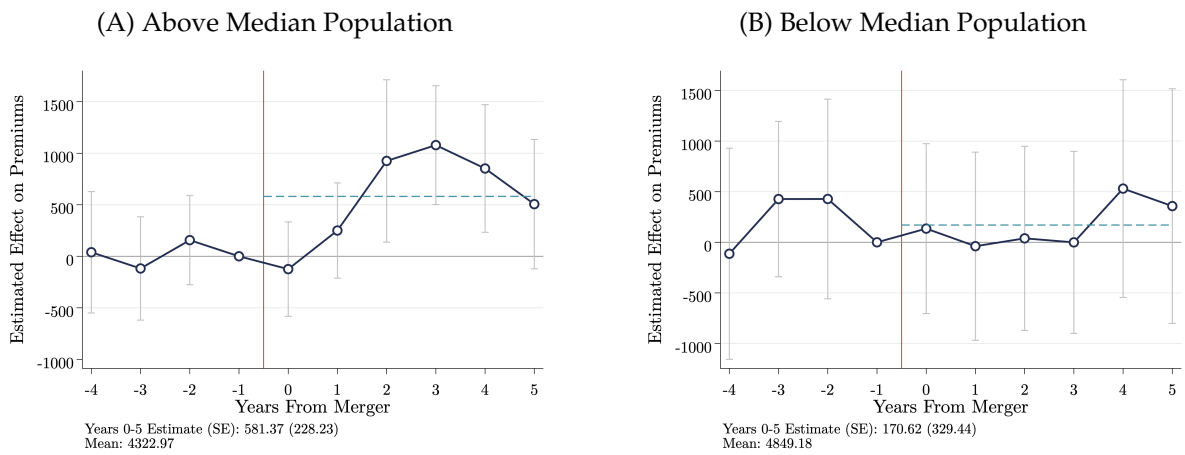
Notes: This figure plots the subsample-specific average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$) estimated from equation (1). The outcome y_{ct} is premiums. Observations are weighted by CZ working-age population in the year before the merger. The first 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.13: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Event Studies



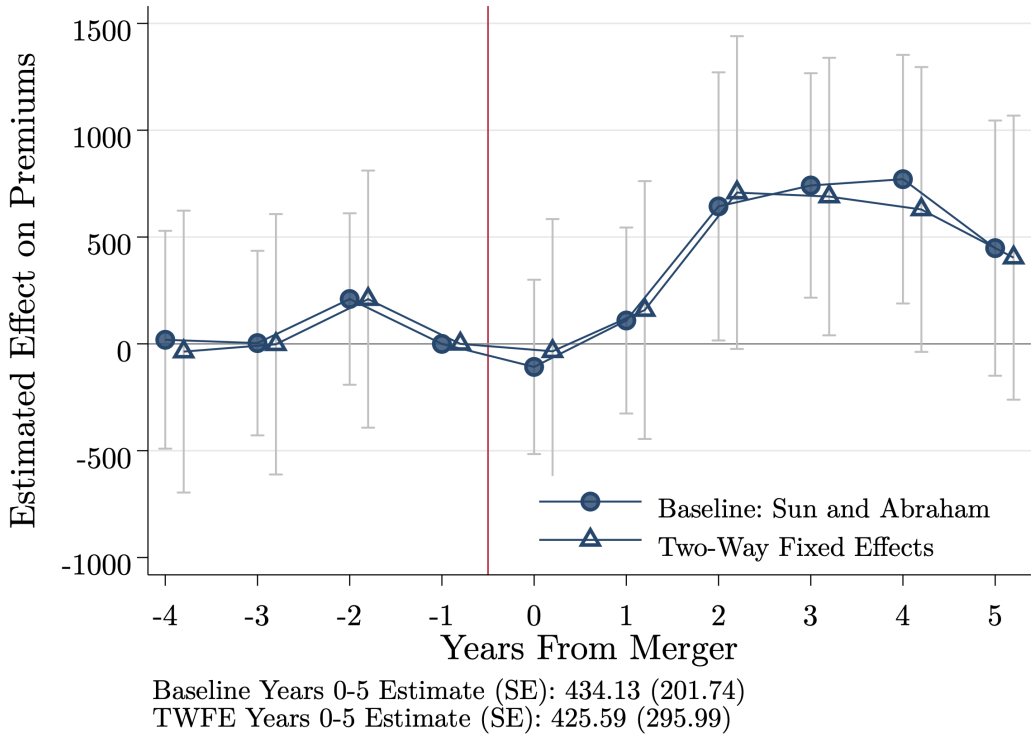
Notes: This figure displays the subsample-specific yearly coefficients β_l from equation (1). The outcome y_{ct} is premiums. I report coefficients for CZ samples with above and below median size of merger shock as measured by the post-merger change in insurer HHI (median=580 ΔHHI), number of insurers 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.14: CZ-Level Effect of Insurer Mergers on Premiums: Heterogeneity Analysis Event Studies



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.15: CZ-Level Effect of Insurer Mergers on Premiums in Fully-Insured Firms: Two-Way Fixed Effects Specification

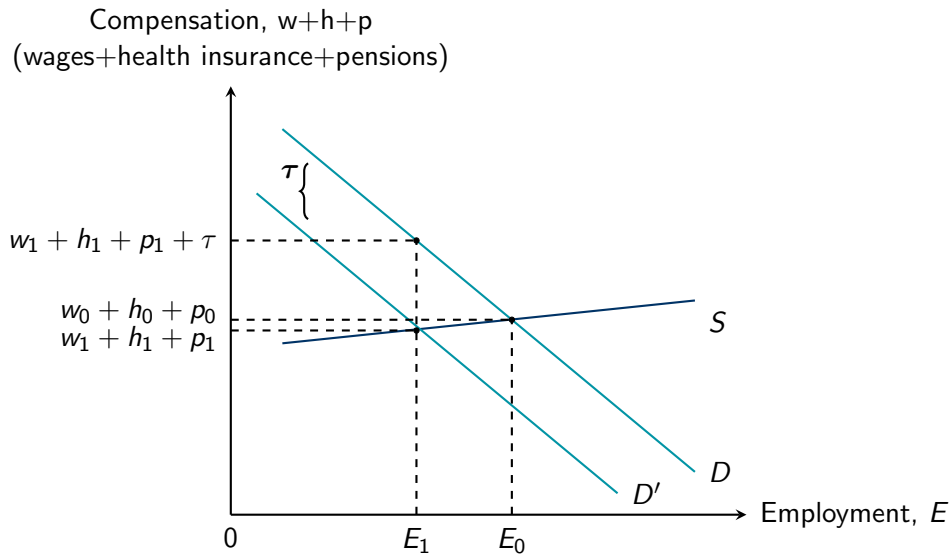


Notes: This figure plots the effect of insurer mergers on premiums. Shaded circle markers report baseline estimates using Sun and Abraham (2021) weights, and triangle markers report two-way fixed effect estimates. Observations are weighted by CZ working-age population in the year before the merger. Standard 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.16: CZ-Level Effect of Insurer Mergers on Self-Insured Firms

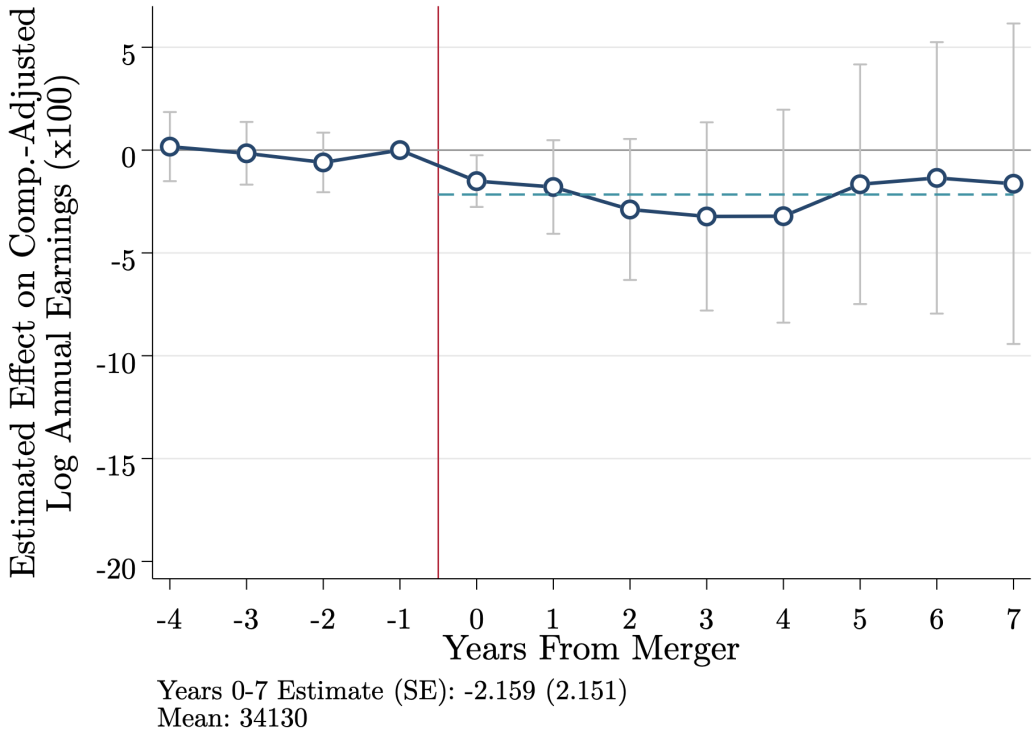
Notes: This figure plots the yearly coefficients β_l from equation (1) for self-insred firm outcomes. The outcome y_{ct} is annual fees paid to insurers per participant (Panel A), annual claims per participant (B), annual premiums (total paid by firms and workers) per participant (C), and premiums share paid by firms (D). 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. N= 36 treated CZ, and 17 control CZs.
Source: Form 5500.

FIGURE A.17: Benchmark Model of Competitive Labor Market



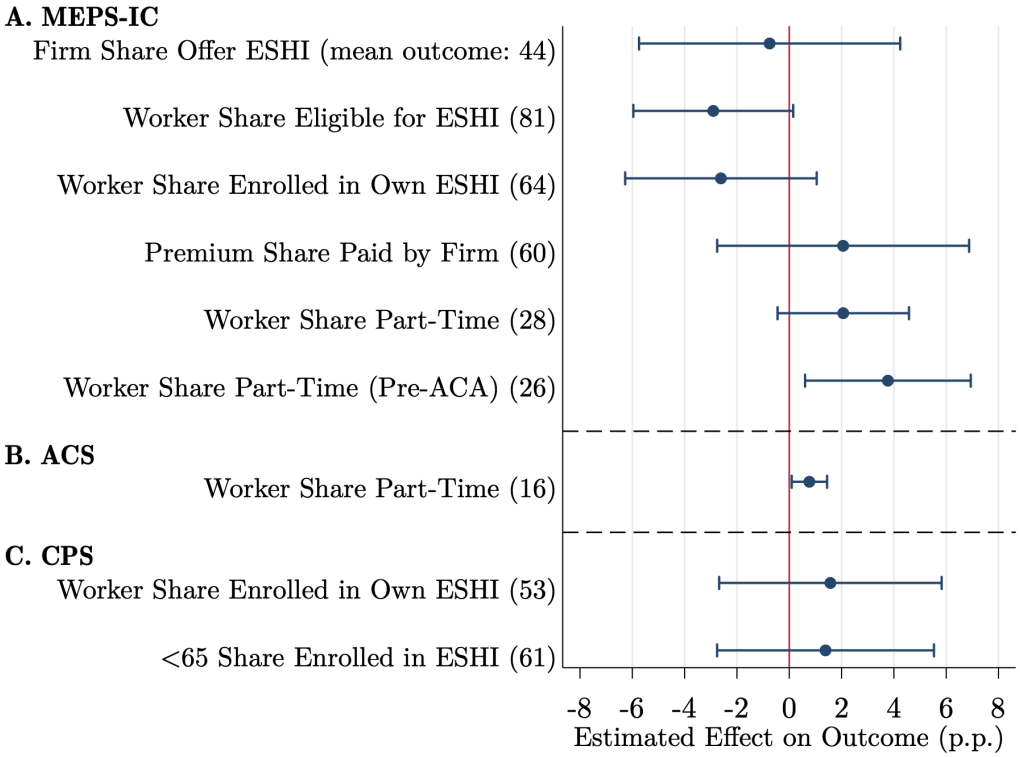
Notes: This figure shows the predicted response of compensation and employment in affected firms. In the pre-merger equilibrium, equilibrium employment is equal to E_0 and equilibrium compensation equal to the sum of wages w_0, employer-sponsored premiums h_0 and pension contributions p_0. Following a merger-induced increase in employer-sponsored premiums τ, equal to approximately \$900 per worker, the labor demand curve shifts downwards. Assuming workers do not value the premium increase, the labor supply curve remains unchanged. The model predicts unambiguous declines in workers' take-home compensation ($w_0 + h_0 + p_0$ to $w_1 + h_1 + p_1$) and employment (E_0 to E_1), and an increase in firms' labor costs ($w_0 + h_0 + p_0$ to $w_1 + h_1 + p_1 + \tau$) post-merger.

FIGURE A.18: CZ-Level Effect of Insurer Mergers on Log **Composition-Adjusted** Annual Wages in 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 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).

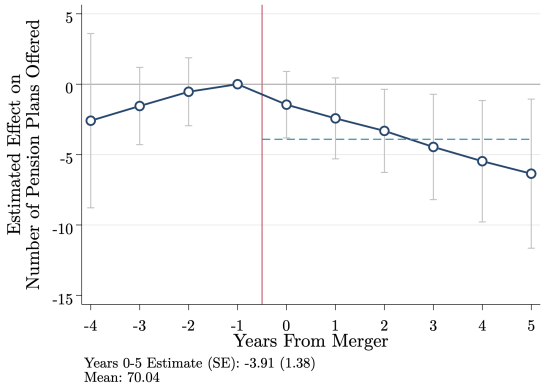
FIGURE A.19: CZ-Level Effect of Insurer Mergers on ESHI Coverage



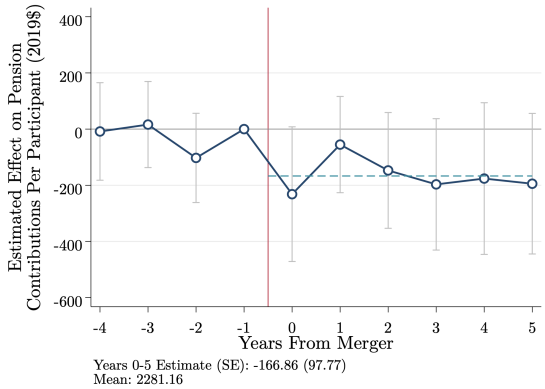
Notes: This figure plots the average of coefficients from years 0 to 5 following the merger ($\frac{1}{5}\sum_{l=0}^{5}\beta_l$) estimated from equation (1). For the ACS sample, I estimate a pooled version of equation (1). The population-weighted mean outcome for treated CZs in the year before the merger 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.20: CZ-Level Effect of Insurer Mergers on Other Compensation in Firms

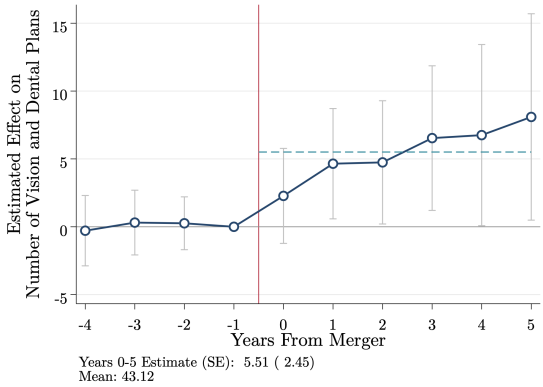
(A) Number of **Pension** Plans Offered



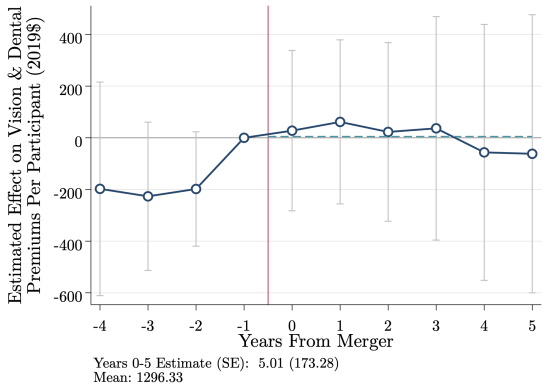
(B) **Pension** Contributions Per Participant (2019$)



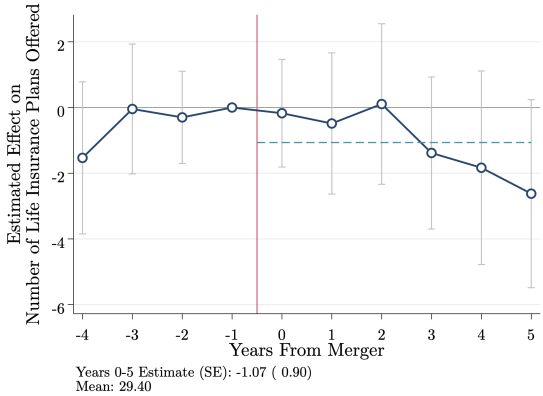
(C) Number of **Vision** & **Dental** Plans Offered



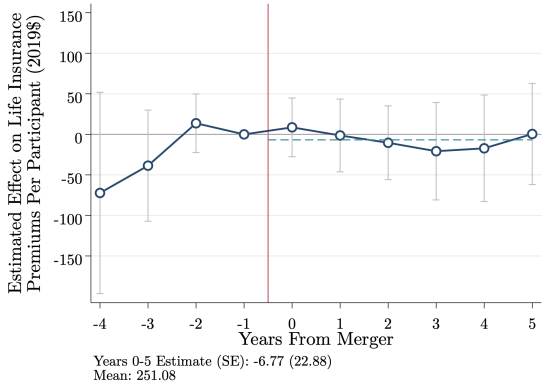
(D) **Vision** & **Dental** Premiums Per Participant (2019$)



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

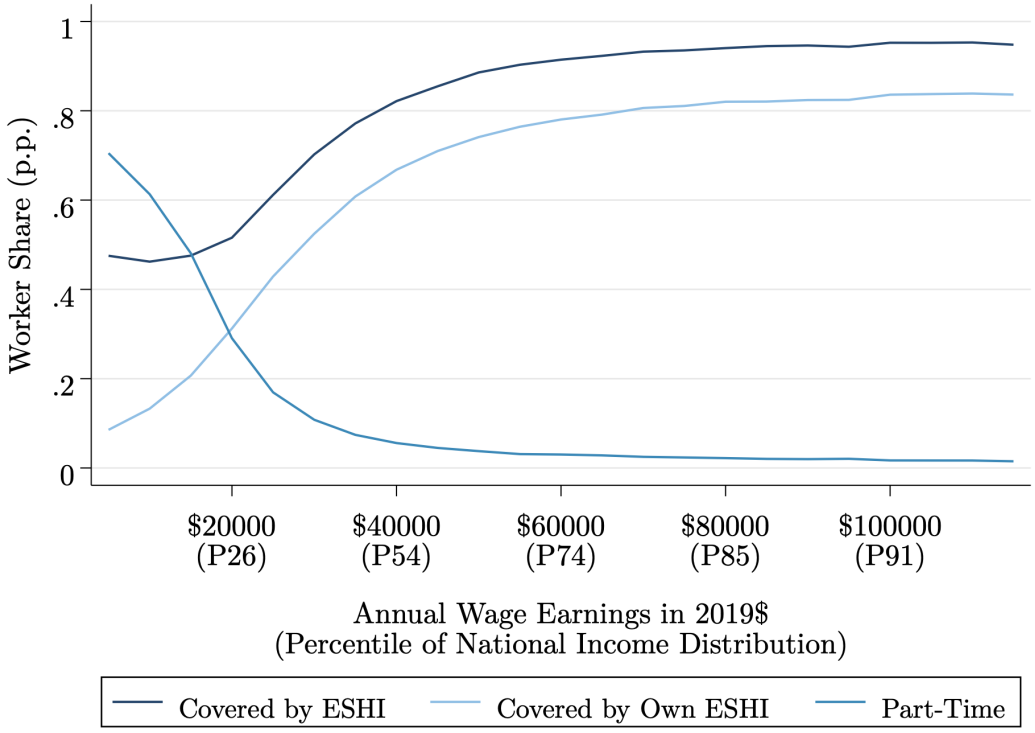


(F) **Life, Sickness,** & **Accident** Insurance Premiums Per Participant (2019$)



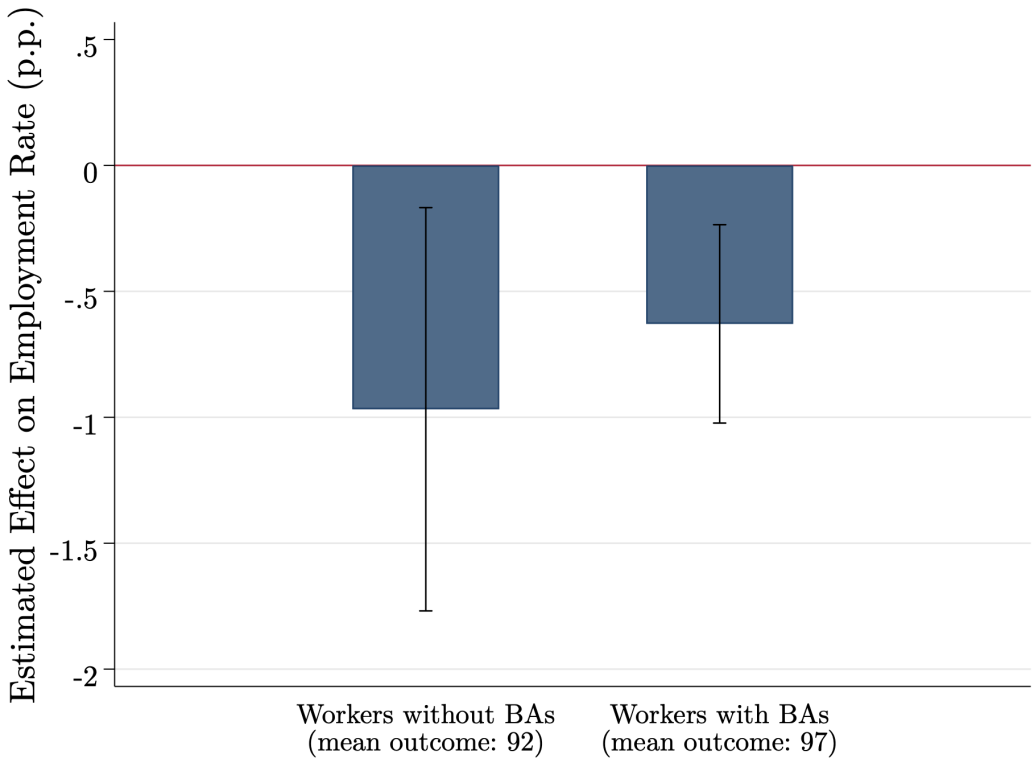
Notes: This figure plots the yearly coefficients β_l from equation (1). Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for the average of coefficients from years 0 to 5 following the merger ($\frac{1}{6}\sum_{l=0}^{5}\beta_l$). 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.21: Worker Share Covered by ESHI and Part-Time, by Annual Wages Earnings



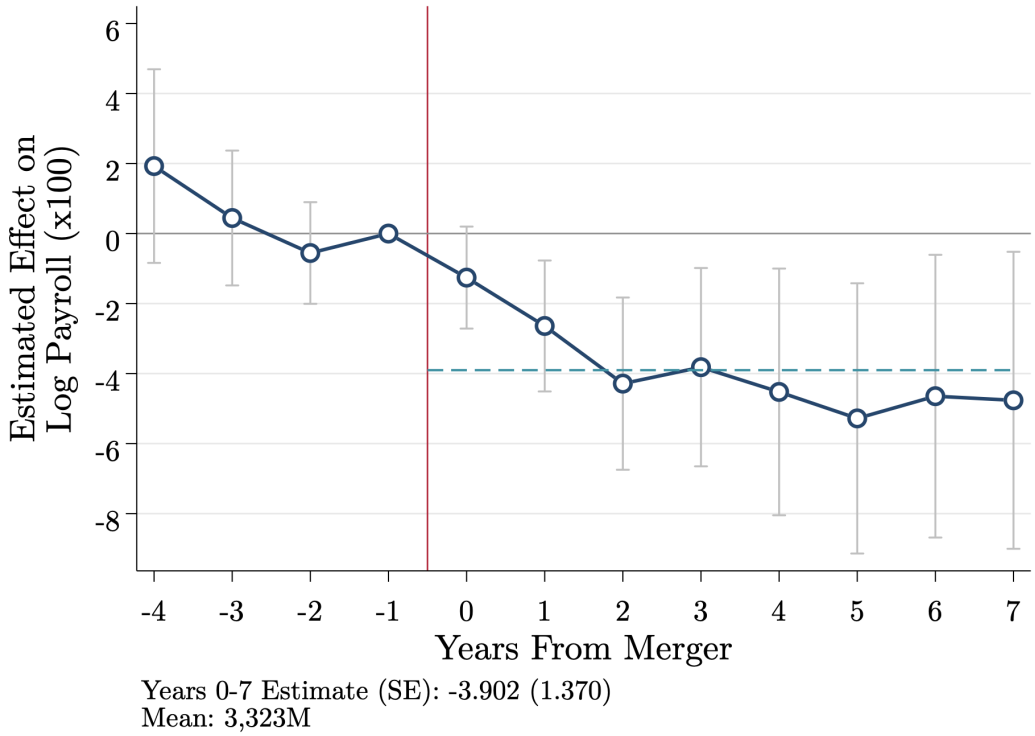
Notes: This figure plots the average share of workers covered by employer-sponsored health insurance (any or own plan) and employed part time in annual income bins of $5,000 from 1999 to 2021. The x-axis is annual income in 2019 dollars and the corresponding percentile of the national income distribution is reported below in parentheses. The sample includes working-age individuals who work for wages and earned positive earnings in the previous calendar year, excluding those working in the armed forces, federal government, agriculture, or are unpaid family workers.
Sources: Current Population Survey (CPS).

FIGURE A.22: 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.23: CZ-Level Effect of Insurer Mergers on Log Payroll in Firms, Using the Longitudinal Business Database (LBD)

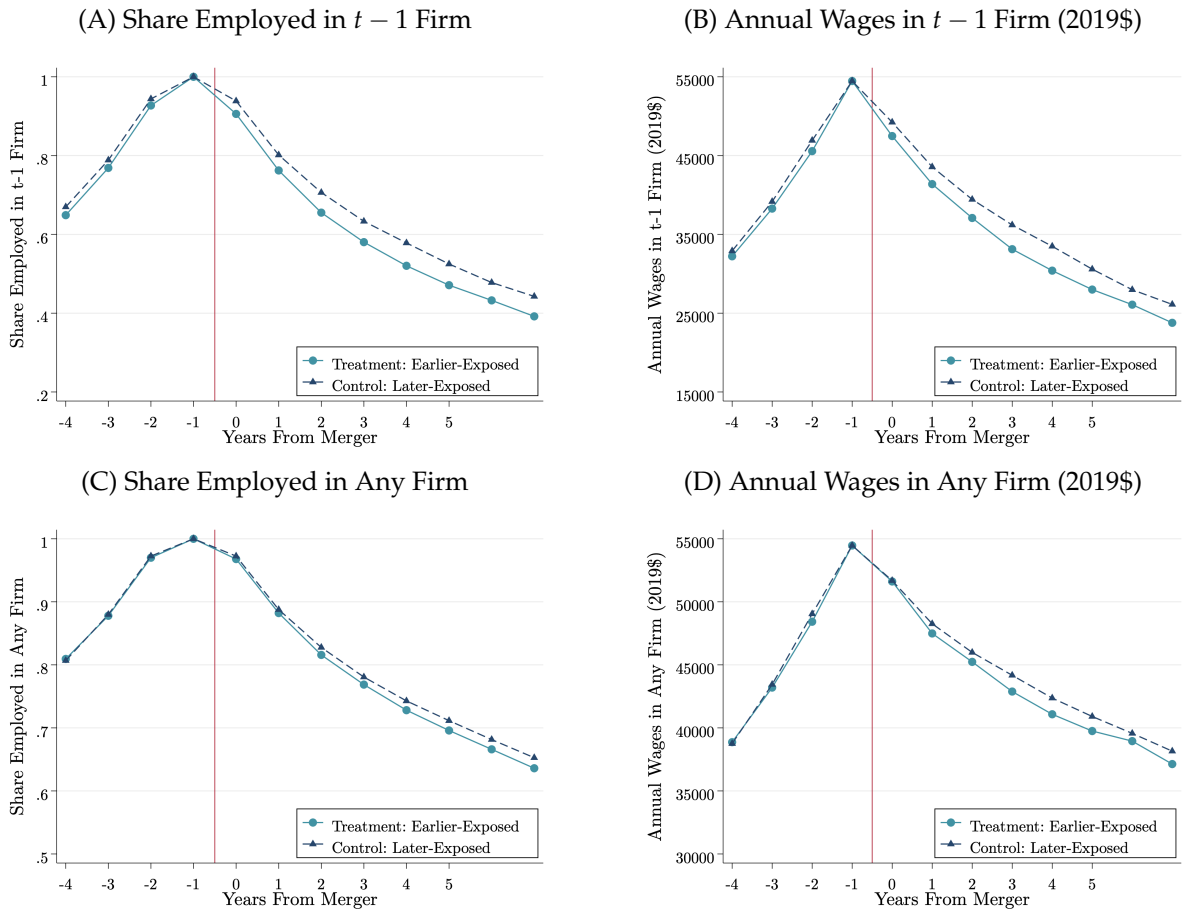


Notes: This figure plots the yearly coefficients β_l from equation (1). The outcome y_{ct} is log annual payroll in exposed single-establishment firms. Observations are weighted by CZ working-age population in the year before the merger. The horizontal dashed blue line indicate the point estimate for 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.24: Firm-Level Effect of Insurer Mergers

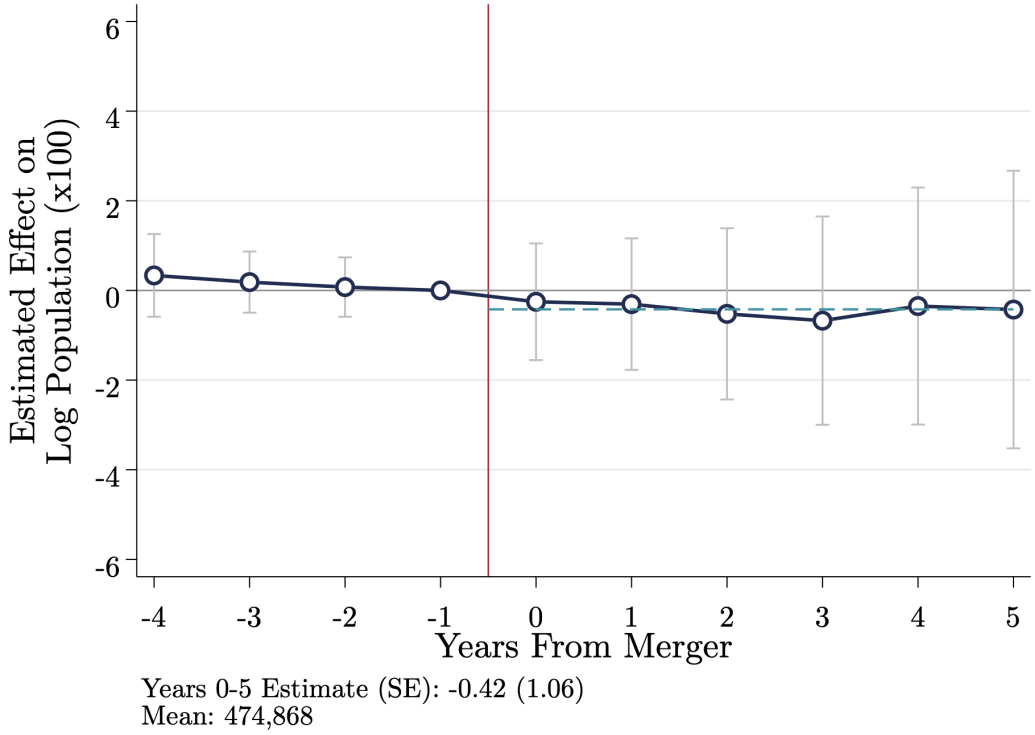
Notes: This figure plots the yearly coefficients β_l from equation (20). The firm-level outcome $y_{j(c)t}$ is employment in all firms (Panel A), the share of active firms with non-zero employment (B), employment in always-active firms (C), and noncollege- and college-educated employment in always-active 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.25: Worker-Level Raw Trends by Exposure to Insurer Mergers



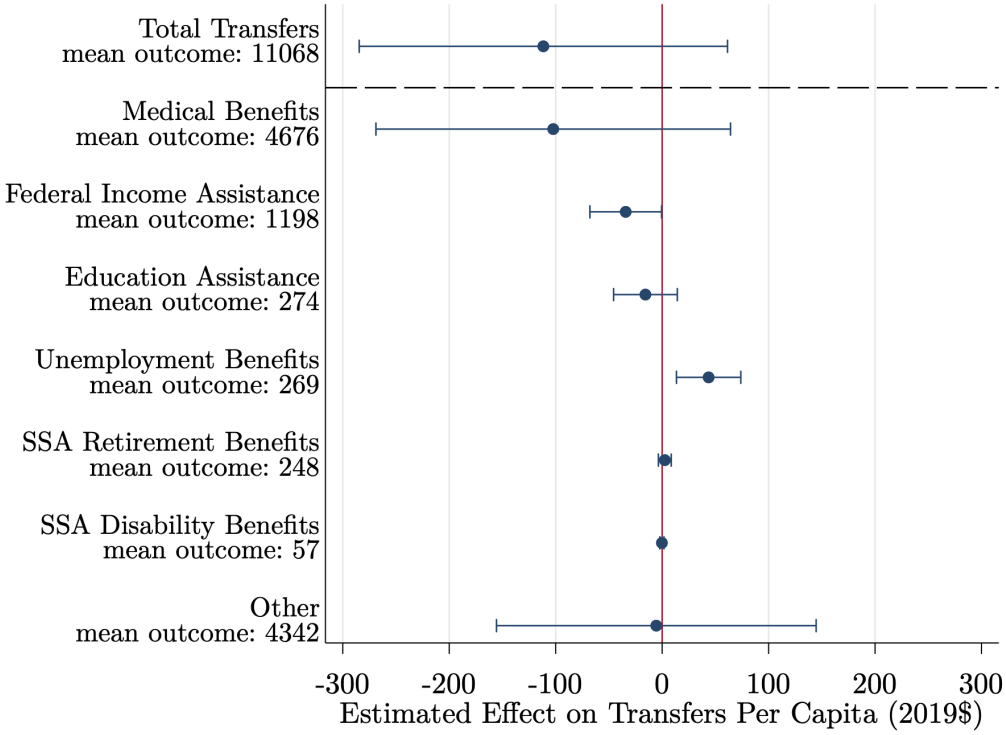
Notes: This figure plots worker-level trends by exposure to insurer mergers. I show the mean trends for the share of workers employed in their pre-merger firm in Panel A, annual wages earned in their pre-merger firm in Panel B, the share of workers employed in any firm in Panel C, and annual wages earned 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.26: 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.27: 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).

I. Additional Tables

TABLE A.5: 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 (21) 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 exposed workers in the bottom and top quartiles of the annual wage distribution in the year before the merger. 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.6: 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.7: Benchmarking the Implied Labor Elasticities of Exposed Firms

Study	η	s.e.	Identification and Population
A. Labor Demand Elasticities ($\frac{\%\Delta \text{emp}}{\%\Delta \text{comp costs}}$)			
A.I ESHI Costs			
My Estimate (2024)	2.75	1.13	U.S. insurer mergers, 1999-2020
Gao et al. (2022)	1.96	0.72	U.S. insurers' past losses, 2012-2019
Brot-Goldberg et al. (2024)	1.89	0.62	U.S. hospital mergers, 2008-2017
A.II Payroll Taxes			
Anderson and Meyer (1997)	2.31	0.46	Experience-rating payroll tax change in Washington U.S., 1980s
Gruber (1997)	0.26		Chile payroll tax cut 1979-1985, manufacturing firms
Kugler and Kugler (2009)	0.45	0.025	Columbia payroll tax increase, 1980s to 1990s
Ku et al. (2020)	3.6		Norway place-based payroll tax cut, 2000-2006
Benzarti and Harju (2021)	2.9		Finland payroll tax increase, 1996 to 2015
Johnston (2021)	4.0	0.5	Unemployment insurance payroll tax change Florida U.S., 2003-2012
Guo (2024)	2.4		Unemployment insurance payroll tax changes U.S. states, 2006-2013
B. Labor Demand Elasticities ($\frac{\%\Delta \text{emp}}{\%\Delta \text{take-home comp}}$)			
B.I ESHI Costs			
My Estimate (2024)	5.5	0.88	U.S. insurer mergers, 1999-2020
Gao et al. (2022)	∞		U.S. insurers' past losses, 2012-2019
Brot-Goldberg et al. (2024)	∞		U.S. hospital mergers, 2008-2017
B.II Payroll Taxes			
Saez et al. (2019)	2.4	0.21	Sweden younger worker payroll tax cut 2002-2013
Ku et al. (2020)	4.3		Norway place-based payroll tax cut 2000-2006
Lobel (2024)	4.2	0.2	Brazil sector-specific payroll tax cut 2008-2017

Notes: This table compared my implied labor demand and supply elasticity of exposed firms to elasticities from existing studies of rising ESHI costs and payroll taxes. Column 2 shows the point estimate of the elasticity, column 3 shows the associated standard error of the point estimate.