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WHO PAYS FOR RISING HEALTH CARE PRICES?  
EVIDENCE FROM HOSPITAL MERGERS

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Who Pays for Rising Health Care Prices? Evidence from Hospital Mergers  
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**ABSTRACT**

We analyze the economic consequences of rising US health care prices. By increasing the cost of employer-sponsored health insurance, rising prices serve as a de facto payroll tax on labor. Using exposure to hospital mergers as an instrument, we estimate that a 1% increase in health care prices lowers payroll and employment at non-health-care employers by 0.4%. At the county level, a 1% increase in health care prices reduces labor income by 0.27%, increases flows into unemployment by 1%, and lowers federal income tax receipts by 0.4%. The disemployment effects of rising prices are concentrated among lower- and middle-income workers.

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# 1 Introduction

The prices for health care goods and services play a central role in driving the variation and growth in health spending in the United States (US) ([Cooper et al., 2019a](#); [Health Care Cost Institute, 2020](#)). Since 2000, the prices for health care services, medical devices, and pharmaceutical products have grown markedly faster than prices for goods and services outside the health sector ([Bureau of Labor Statistics, 2022](#)). During that period, for example, prices in the hospital sector — a \$1.3 trillion industry — increased faster than prices in any other sector of the economy ([Bureau of Labor Statistics, 2022](#)). While price growth need not be a problem if it reflects improvements in quality, a wide-ranging literature has illustrated that much of the growth in prices in the US health care sector over the last two decades has arisen from unproductive rent-seeking activities.<sup>1</sup>

In most markets, rising prices are paid directly by consumers. By contrast, for the majority of working-age adults in the US, health care is covered by employer-sponsored health insurance (ESI) ([Kaiser Family Foundation, 2019](#)). Employers pay insurers a fixed fee per worker to provide this benefit. As the cost of ESI rises, workers becomes increasingly expensive for employers to retain. As a result, rising health care prices serve as a de facto payroll tax on employers outside the health care sector. The incidence of this “hospital tax” could be borne by employers or workers.

In this paper, we measure the downstream consequences of rising health care prices. To do so, we trace out a causal chain that spans from increases in prices for health care services, to increases in premiums for ESI, and finally to reductions in pay and employment for workers. Since rising local incomes could increase local demand for health care services and, in turn, raise the price of health care services, simply correlating changes in health care prices with changes in insurance premiums and labor market outcomes could produce severely biased estimates. Therefore, we use local hospital mergers as a shock to health care prices and then measure the follow-on effects of the price increases that the mergers generate.

To estimate the effects of rising prices, we bring together a range of unique data. We measure health care prices and health spending using insurance claims data from the Health Care Cost Institute (HCCI), which contain data on 28% of the ESI-covered US population. We measure ESI premiums for fully insured employers using data from Department of Labor (DOL) Form 5500 filings. We also utilize a database of hospital mergers constructed by [Brot et al. \(forthcoming\)](#). Finally, we measure employment and earnings for health care and non-health care workers using individual tax filings from the Internal Revenue Service (IRS).

Our empirical approach relies on the fact that some hospital mergers generate meaningful changes in market concentration and, in turn, lead to price increases, while other mergers do not.

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<sup>1</sup>See, e.g., [Cooper et al. \(2019a\)](#), [Brand et al. \(2023\)](#), and [Brot et al. \(forthcoming\)](#) on hospital mergers; [Capps et al. \(2018\)](#) and [Lin et al. \(2021\)](#) on hospital-physician vertical integration; [Cooper et al. \(2020\)](#) on surprise billing; [Dafny \(2005\)](#) on hospital upcoding; and [Dafny et al. \(forthcoming\)](#) on drug copayment coupons.

We use a panel of 304 horizontal mergers between hospitals that were consummated between 2010 and 2015 and previously studied by [Brot et al. \(forthcoming\)](#). We take a difference-in-differences approach to measure the effect of mergers, comparing prices at merging hospitals with prices at non-merging hospitals before and after the mergers occurred. The post-merger price increases we estimate vary substantially across transactions, and much of the variation in post-merger price increases can be explained by the changes in market structure that specific mergers generate. Transactions which are predicted, *ex ante*, to substantially lessen competition raise prices by, on average, 5%, whereas mergers that do not appear to meaningfully reduce competition result in minimal price changes. We do not find that the variation in the price effects generated by mergers is correlated with trends in local economic activity prior to the merger.

It would be intuitively appealing to use a similar difference-in-differences approach to measure the downstream labor market consequences of rising health spending by comparing employers exposed to mergers with those unexposed. However, during our sample period, there were so many hospital mergers that virtually every US employer was proximate to at least one merging hospital, and often, employers were exposed to multiple mergers over short time horizons. Therefore, rather than using a difference-in-differences design, we instead exploit variation in the *intensity* of employers' exposure to merger-driven hospital price increases to estimate the downstream effects of health care price increases.

Employers face varying degrees of exposure to mergers based on their workers' *ex ante* demand for specific hospitals that eventually merged, and variation in the extent to which each merger raised prices. In practice, we construct an instrument that measures each employers' exposure to merger-driven changes in the average price of care over time, holding fixed other prices and quantities of health care consumption. We use this as an instrument for an employer's annual health care spending. This instrument is valid as long as an employer's exposure to mergers is independent of their counterfactual outcomes had no mergers occurred. We develop multiple approaches to test this exogeneity assumption.

We use this instrumental variables (IV) strategy to estimate the effect of rising health care prices on multiple employer-level outcomes. We find that merger-driven increases in hospital prices raise health care spending on a dollar-for-dollar basis. In turn, we observe that health spending is passed through on a dollar-for-dollar basis into employer ESI premiums. Finally, despite facing higher premiums, we show that employers do not appear to respond to health spending increases by shifting workers towards high-deductible health plans.

When exposed to a 1% increase in health care prices, employers outside the health care industry reduce their payroll by 0.37%. We find roughly equal-sized effects on employers' total count of workers — implying that rising health care prices were not passed through as simple wage reductions but instead resulted in job separations. Event study estimates suggest that these employer-level

effects were realized almost immediately after hospital prices increased, with little dynamic impact over time.

Together, our employment and payroll results imply that, for every \$1 in hospital revenue raised by a price increase, non-health labor income fell by between \$1.33 to \$1.69. This indicates that there is substantial deadweight loss from the “hospital tax.” We benchmark the disemployment responses we estimate from this “hospital tax” against prior estimates of employer responses to changes in payroll taxes. Our results imply a 1.8% disemployment response to a 1 percentage point increase in labor costs, which is within the 0.7% to 2.4% range of estimates from prior studies examining payroll taxes in the US (e.g., [Anderson and Meyer \(1997\)](#); [Johnston \(2021\)](#); [Guo \(2024\)](#)).

While we estimate large disemployment effects *at specific employers*, the job losses generated by rising health care prices may have simply resulted in reallocations of workers to other employers. Therefore, we estimate the broader regional effects of price increases — general equilibrium effects — by aggregating our employer-specific instrument to the county level. At the county level, we find that a 1% increase in *county-level* health care prices reduced *county-level* labor income per capita by 0.27% and increased flows into unemployment by 0.1 percentage points (1%). We do not find that health care price increases induced any economically or statistically significant changes in self-employment or migration across counties. As with our employer-level results, these effects occurred immediately after hospital prices increased. Ultimately, these labor market changes had substantial fiscal consequences for federal and state governments: a 1% increase in health care prices reduced income tax withholdings by 0.4%, while increasing unemployment insurance (UI) payments by approximately 2.5%.

Since premium increases are uniform across workers at the same employer, theory suggests they should serve as a regressive “head tax” that generates larger disemployment effects for lower-wage workers ([Finkelstein et al., 2023](#)). Consistent with this prediction, we find that unemployment effects are approximately zero for workers at the top of the income distribution (i.e., those previously earning more than \$100,000 annually). However, we also find no effects of rising prices on employment for workers at the bottom of the income distribution (i.e., those previously earning less than \$20,000 per year). This aligns with the fact that these low-wage workers rarely receive health insurance from their employers, and therefore do not become more expensive to employ when health care prices increase ([Lurie and Miller, 2023](#)). Ultimately, the job losses we observe are concentrated among low- and middle-income workers who were previously earning between \$20,000 and \$100,000 per year. As a result, our estimates highlight that the burden of rising prices falls primarily on these lower- and middle-income workers.

A growing literature suggests that job losses can precipitate increases in short-run mortality from drug overdose and suicide ([Venkataramani et al., 2020](#)). Echoing these findings, we estimate that a 1% increase in local health care prices leads to approximately one additional death from

suicide or overdose per 100,000 people. This result, combined with our estimates of unemployment effects, implies that one death occurred per 140 people who were shifted into unemployment, which is an effect size similar in scale to previously measured effects ([Eliason and Storrie, 2009](#); [Sullivan and von Wachter, 2009](#); [Pierce and Schott, 2020](#)).

Our work makes three distinct contributions to the literature. First, we contribute to a broad literature studying the incidence of employer-sponsored health insurance. Prior research has traditionally focused on the effects of adding new fringe benefits ([Summers, 1989](#); [Gruber and Krueger, 1991](#)). Since new benefits are valued by workers, the cost of these benefits can be passed through into wages without a reduction in employment. By contrast, when existing fringe benefits become more costly, workers likely will not value the cost to their employer. As a result, they will not easily accept a corresponding wage reduction. This implies, as we demonstrate empirically, that rising health care prices should likely result in job losses rather than full pass-through into wages.<sup>2</sup> This study highlights a new source of inefficiency caused by employer-sponsored health insurance: its ubiquity makes non-health care labor markets sensitive to institutional changes in the market for health care. We also provide new quasi-experimental evidence of the uneven distributional consequences of rising health costs. Our results support the idea that rising ESI premiums can serve as a regressive “head tax” on employment ([Saez and Zucman, 2019](#); [Case and Deaton, 2020](#); [Finkelstein et al., 2023](#)).

Second, we contribute to the industrial organization literature exploring the aggregate consequences of rising market power ([Berry et al., 2019](#); [De Loecker et al., 2020](#)). Since [Harberger \(1954\)](#), economists have traditionally focused on measuring the harms from market power as proportional to the quantity reductions in the focal market where the mergers occur (e.g., a partial equilibrium analysis). Because hospitals face very price-inelastic residual demand curves ([Gowrisankaran et al., 2015](#); [Brot-Goldberg et al., 2017](#)), the Harberger approach would suggest that the deadweight loss from hospital mergers is low. However, our results suggest that, in very inelastic markets, a first-order deadweight loss may instead arise in the market for close complements: in our case, the market for workers. We estimate that an individual merger that meaningfully lessens competition generates approximately \$42 million worth of harm *outside* the market for health care services, due to the resulting job losses and deaths.

Third, we provide new evidence on the macroeconomic consequences of rising domestic health care spending. Economists have previously focused on the federal tax burden of financing public health insurance ([Baicker and Skinner, 2011](#)). Our results suggest that the rising cost of

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<sup>2</sup>Indeed, empirical research on new benefits has found little disemployment response ([Gruber and Krueger, 1991](#); [Gruber, 1994](#); [Kolstad and Kowalski, 2016](#)), while work on the rising cost of existing benefits has typically found nontrivial disemployment responses ([Cutler and Madrian, 1998](#); [Baicker and Chandra, 2006](#); [Gao et al., 2023](#)). Other work on existing mandates has found wage effects, but has not studied employment effects ([Anand, 2016](#)), or has found no employment effects ([Arnold and Whaley, 2023](#)).

financing private health insurance also generates important macroeconomic consequences by raising employers' labor costs. A literature in macroeconomics has emphasized how sectoral shocks can be amplified to the broader economy when that sector's output is central to the production of many other sectors (Baumol, 1967; Baqaee and Farhi, 2019). Since virtually all major U.S. firms offer health insurance benefits, rising health care costs can slow down local economies. Our estimates suggest that, under very strong assumptions, growth in hospital prices from 2007 to 2014 suppressed labor income in 2014 by 2.7% after accounting for inflation.

Going forward, this paper is structured as follows: in Section 2, we provide background on the market for ESI in the US and an overview of the changes that have occurred in hospital markets in the last two decades. In Section 3, we describe how increases in health care prices can impact labor market outcomes. We describe the data used in this analysis in Section 4 and provide a discussion of our analytic strategy in Section 5. We present our employer-level results in Section 6 and county-level results in Section 7. We quantify the cumulative harm of hospital mergers in Section 8 and conclude in Section 9.

## 2 Background

### 2.1 Employer-Sponsored Health Insurance

Employer-sponsored health insurance in the US has its genesis in 1940s wage controls introduced by President Franklin Roosevelt. In response to wage caps, firms began offering non-monetary compensation to workers in the form of ESI. In 1943, the IRS allowed health insurance to be purchased with pre-tax income, lowering the cost of obtaining health insurance coverage (Starr, 1982). Today, insurers charge an annual premium, which is generally adjusted based on employers' own expected costs (Craig, 2022). Ultimately, insurance premiums are funded by a combination of employer contributions and employee contributions, with the employer contributions excluded from taxation.

Approximately 91% of workers are employed by firms that offer health benefits, and 59% of firms offer health benefits to at least some portion of workers (Claxon et al., 2021). As a result, 54.3% of the US population and 63% of the adult population under age 65 have ESI coverage (Keisler-Starkey and Bunch, 2022). Of those receiving ESI, 64% are covered by administrative services only (ASO) policies, under which an insurer administers the health benefits but the employer bears the risk. The remaining 36% are enrolled in fully insured health insurance plans offered by their employers, in which the insurer bears financial risk (Claxon et al., 2021). ASO plans are more prevalent among larger firms.

ESI coverage is notably higher among higher-income individuals. While 10% of individuals in the 0<sup>th</sup> through 25<sup>th</sup> percentiles of income have ESI coverage, 84% of individuals in the 95<sup>th</sup> through



99<sup>th</sup> percentiles have employer-based coverage (Lurie and Miller, 2023). Likewise, conditional on being insured through ESI, higher-income individuals select substantially more expensive policies (Lurie and Miller, 2023).

Private insurers form networks of providers accessible to their enrollees and design benefits packages, including the degree of cost-sharing patients face. Insurers negotiate over prices with hospitals and physicians. In exchange for favorable prices, insurers include those providers in their coverage networks, giving enrollees access to those providers at a discount (Handel and Ho, 2021). Hospital and physician prices vary substantially across geographic regions and within regions across providers (Cooper et al., 2019a).

## 2.2 Consolidation in the US Hospital Sector

From 2000 to 2020, there were over 1,000 hospital mergers. During that period, there were only 13 enforcement actions taken against hospital mergers by the Federal Trade Commission (FTC), the federal agency tasked with preserving competition among hospitals in the US (Brot et al., forthcoming). The joint 2010 Department of Justice (DOJ)/FTC Horizontal Merger Guidelines specify that mergers that result in increases in the Herfindahl-Hirschman Index (HHI) of at least 200 points and lead to a post-merger HHI of over 2,500 should be “presumed to be likely to enhance market power” (U.S. Department of Justice and the Federal Trade Commission, 2010). Brot et al. (forthcoming) show that, from 2010 to 2015, approximately 20% of hospital mergers could have been predicted *ex ante* to meaningfully lessen competition using the screening thresholds established in the Horizontal Merger Guidelines and find that these mergers resulted in price increases of over 5%.<sup>3</sup>

A growing literature demonstrates that mergers, particularly among hospitals that are close substitutes, can lead to increases in prices via a lessening of competition (Capps et al., 2003; Dafny, 2009; Tenn, 2011; Haas-Wilson and Garmon, 2011; Gaynor et al., 2015; Gowrisankaran et al., 2015; Cooper et al., 2019a; Brand et al., 2023). Conversely, there is little evidence that mergers raise hospitals’ quality (Beaulieu et al., 2020). Because of merger activity, market concentration in the hospital industry has been rising steadily since 2000 (Fulton, 2017). Likewise, given of the scale of the hospital industry and the scale of merger activity in the sector, hospital mergers have become a topic of interest and importance for policymakers and elected officials (Biden, 2021).

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<sup>3</sup>In 2023, the DOJ and FTC revised their merger guidelines and defined problematic transactions as those that increased HHI by greater than 100 points and led to a post-merger HHI of greater than 1,800 points (U.S. Department of Justice and Federal Trade Commission, 2023). Brot et al. (forthcoming) also show that mergers exceeding that threshold markedly increase hospital prices.



### 3 Theoretical Framework

We present a simple model to demonstrate the intuition for how, via increases in ESI premiums, increases in the price of health care can generate changes in local labor market outcomes.

**Hospital Markets:** Consider a hospital,  $h$ , providing a single service. It faces a residual (firm-specific) demand curve of  $D^h(p)$  for its services, given its own price  $p$ . Consider a rent-seeking activity, such as a merger, that raises the hospital's price by  $\Delta p$  without shifting demand (i.e., without shifting patients' preferences for getting care at that hospital).

In Panel A of Figure 1, we graph this event. As prices rise, quantities decline. The price increase generates a deadweight loss given by the red shaded triangle. As in Harberger (1954), the deadweight loss from the merger is proportional to the reduction in quantity. In Figure 1, as is typically the case, hospital-specific demand is extremely inelastic because patients do not tend to substitute to alternative hospitals when a hospital's relative price increases (Gowrisankaran et al., 2015; Brot-Goldberg et al., 2017; Lieber, 2017). Therefore, the deadweight loss generated in the hospital market is small. However, this deadweight loss is not the only effect. There is also a large transfer from health care payers to hospitals, which is represented by the blue rectangle.

**Insurance Markets:** In Kaldor-Hicks terms, the transfer to hospitals has no effect on the surplus of the hospital market. However, since patients often have health insurance, most of the transfer is paid by insurers rather than patients themselves. This raises the costs of providing insurance.

Consider a market for ESI where insurers sell contracts with actuarial value  $C$ .<sup>4</sup> We assume (without loss of generality) that insurance markets are perfectly competitive, and therefore the premium charged,  $\phi$ , is determined by the cost of providing insurance coverage of generosity  $C$ :  $\phi = P \times C$ , where  $P$  is the general price of care. Insurance is purchased by employers on behalf of their employees, with demand  $D^{\text{Ins}}(\phi)$ . In Panel B of Figure 1, we plot such an insurance market.

In Panel B of Figure 1, we model how the insurance market changes as insurers pay higher prices (transfers) to rent-seeking hospitals. As the price of care rises, it rotates the insurance supply curve counter-clockwise. A share of the price increase is passed through into higher premiums. Employers may respond by lowering the quality of the insurance they procure, offsetting some of the premium increase. Price increases rotate the supply curve because the cost increase is proportional to the baseline pre-shock equilibrium coverage level. That is, consider a \$100 price increase affecting two insurers, one of which covers 30% of expenses and the other of which covers 80%. The former will increase their premiums by \$30, while the latter will increase their premiums by \$80. As a result, employers that offer greater benefits at baseline will generally face a greater premium increase.

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<sup>4</sup> $C = 0$  represents the purchase of no insurance coverage.

**Labor Markets:** Increasing hospital prices raise costs for insurers, who pass through those costs to employers via increases in ESI premiums. To illustrate how employers respond, we set up a simple labor market model in the spirit of [Gruber and Krueger \(1991\)](#). We consider a single employer facing a labor market for homogeneously-skilled workers. The employer hires  $L$  workers according to its total cost, which includes both the wage  $w$  and insurance premiums,  $\phi$ , with its labor demand thus given by  $D^L(w + \phi)$ . Workers only value the wage they receive, not what their employer has paid for insurance on their behalf; thus, their labor supply is given by  $S^L(w)$ .

In Panel C of Figure 1, we plot this market. Increases in the price of health care raise ESI premiums by  $\Delta\phi$ . For now, we assume that employers are perfectly inelastic with respect to their insurance purchasing, so  $C$  is fixed. The premium increase shifts labor demand down by exactly  $\Delta\phi$ . However, since insurance value has not changed, labor supply remains the same. The result is that both wages  $w$  and employment  $L$  decline. The premium increase effectively serves as a tax on hiring — what we call a “hospital tax” — and therefore generates deadweight loss given by a Harberger triangle (highlighted in red in the figure) the same way other taxes on employers, such as payroll taxes, would. It is important to note that, while our diagram imagines a world starting at competitive hospital pricing, baseline pre-merger prices are likely to already be above competitive levels. Deadweight loss increases quadratically in the tax rate, and so the damages from price increases are likely to be even larger in an already-high-priced environment.

Why do both wages and employment go down, in contrast to the predictions of [Summers \(1989\)](#), who posits that the costs of mandated employer fringe benefits are fully passed through into wages? One key factor is that, while workers value the provision of ESI, they (likely) do *not* value increases in its price that are not associated with changes in the quality of benefits they receive. As a result, while premiums have increased, the value of the employment relationship to them has not changed, and so many workers will not accept a reduced wage. Therefore the employer’s cost of retaining workers will increase, and they will lay off workers. The changes in wages and employment and the percent change in total labor income ( $I = w \times L$ ) are given by:

$$\begin{aligned}\frac{\Delta w}{w} &= -\frac{\eta_D}{\eta_D + \eta_S} \frac{1}{w} \Delta\phi \\ \frac{\Delta L}{L} &= -\frac{\eta_D \eta_S}{\eta_D + \eta_S} \frac{1}{w} \Delta\phi \\ \frac{\Delta I}{I} &= -\frac{\eta^D(1 + \eta^S)}{\eta^S - \eta^D} \frac{1}{w} \Delta\phi\end{aligned}$$

where  $\eta_D$  and  $\eta_S$  are the absolute values of the elasticities of demand and supply, respectively.

The effect of rising health care prices on wages is similar to a simple tax: wage reductions will be greater when labor demand is more wage-elastic than labor supply. On the other hand, the changes in employment and total labor income are greater when either side is relatively more elastic.

For example, if workers are more substitutable with capital (and so labor demand is very elastic), employers will replace workers as they become costlier.

A key feature of the response is that it is proportionally larger when the pre-shock equilibrium wage level is lower. This occurs because, in contrast to income and payroll taxes, the change in premiums  $\Delta\phi$  is not (directly) a function of wages. It is instead, in the terminology of [Finkelstein et al. \(2023\)](#), a “head tax” that is paid per worker rather than per dollar. This means that, in lower-wage labor markets, the change will be a larger proportional change in the cost of retaining a worker. All else equal, health care price increases will have much larger effects in lower-wage markets.

## **4 Data and Measurement**

### **4.1 Employer Panel and Labor Outcomes**

We begin by building a panel of employers. We identify employers by their Employer Identification Numbers (EINs) as recorded on individuals’ W-2 forms. We select all employers with W-2 forms in 2009 and limit our sample to those that appeared on at least 50 W-2 forms (i.e., EINs that had at least 50 employees that year) and were located in the contiguous US. We also require 95% of employers’ workers to reside in a county where we have at least one HCCI beneficiary. From this group of employers, we enforce a balanced panel by keeping only those employers who appeared on at least one W-2 form in every year from 2008 to 2017. Our final sample includes 140,300 unique employers. We identify employers in the health care industry by whether they have a North American Industry Classification System (NAICS) code starting in “62,” as reported to the IRS. Approximately 7% of our employers have such a NAICS code starting with “62.”

For each employer, we measure their total payroll and count of workers using W-2 forms. In each year, we take all W-2 forms that listed the employer in question. We sum the total of wages subject to Medicare tax (Box 5) to measure total payroll and use the count of W-2s to reflect the total workers. We construct an adjustment to account for the fact that workers may not be with an employer the entire year. For individuals who file more than one W-2 in a year, when we allocate them to employers, we do so proportionally to their income for that employer. So, for example, if an individual works for employer A that year making \$10,000 and for employer B making \$30,000, we count them as  $\frac{1}{4}$  towards employer A’s count of workers that year and  $\frac{3}{4}$  towards employer B’s count of workers.

### **4.2 Health Care Prices and Spending**

We use insurance claims data from 2008 to 2017 provided by HCCI to measure health care prices and utilization for individuals enrolled in employer-sponsored coverage. The HCCI data are composed of health insurance claims from Aetna, Humana, and UnitedHealth. The data capture approximately

28% of individuals in the US with employer-sponsored coverage and cover more than \$125 billion in health care spending annually. Crucially, the data include the transaction prices that hospitals were actually paid by insurers, not merely their “chargemaster” (list) prices. We focus our analysis on individuals under age 65 for whom an HCCI payer is their primary insurer (e.g., the individual does not receive primary insurance coverage from their spouse’s employer). This population includes both the policyholders of employer-sponsored insurance plans and their dependents (spouses and children).

These data allow us to measure prices and quantities for health care services obtained at hospitals. We construct visit-level data containing prices for both inpatient and outpatient hospital care, where an observation is a hospital admission or an outpatient visit, respectively. We also measure average spending per beneficiary annually across all medical claims (e.g., physician claims, inpatient claims, and outpatient claims). We exclude pharmacy-dispensed pharmaceutical spending from our analysis.<sup>5</sup> For individuals who are not enrolled in coverage for a full calendar year, we take their average monthly spending and multiply it by 12 to construct an annualized measure.

We cannot directly link employers in the HCCI data with those in our IRS employer panel. We instead assume that an employer’s expected average health care spending is the weighted average of county-level HCCI spending per beneficiary, where the weights are  $\omega_{ic0}$ , the share of a employer’s employees that live in each county  $c$  in our base period of 2009, as measured by their listed ZIP codes on their filed W-2 forms.<sup>6</sup>

In practice, we measure an employer’s expected health care spending as the product of where an employer’s employees lived in 2009 and HCCI prices and utilization for beneficiaries in those locations across all health care providers, including doctors, hospitals, outpatient clinics, and physical therapists. Our constructed measure for employer  $i$  in year  $t$  is:

$$S_{it} = \sum_{c,k} \underbrace{\omega_{ic0}}_{\text{Employee share in county } c \text{ at baseline}} \times \underbrace{S_{ckt}}_{\text{Average HCCI spend in county } c, \text{ year } t \text{ at provider } k} \quad (1)$$

### 4.3 Insurance Market Outcomes

We also construct measures of employers’ insurance premiums. Data on insurance premiums are scarcely available and are often subject to reporting error or provided at high levels of geographic aggregation (Dafny et al., 2011). We measure employers’ insurance premiums directly using data from Form 5500, which is a regulatory filing collected by the DOL in cooperation with the IRS.

<sup>5</sup>Prescription drug claims are often offset by large rebates negotiated between payers and drug manufacturers. These rebates are not included in the HCCI data, rendering any claims-based spending measurement inaccurate.

<sup>6</sup>Based on our discussions with industry participants, this aggregation mimics the way health insurers would price premiums for an employer with beneficiaries spanning multiple geographies.

The data contain measures of total premiums, covered lives, and plan characteristics for employer-sponsored insurance groups covering at least 50 employees enrolled in fully-insured insurance plans. We use the data to construct a measure of average premiums per covered life at the employer level. We provide additional details on the construction of our premium series in Appendix C. Because Form 5500 identifies employers, we can link them directly to our panel in the IRS data.<sup>7</sup> As a result of the limited number of employers with 5500 data on premiums, our premiums analysis is carried out on a sub-sample of our main analytic sample.

We also use the IRS data to construct a proxy measure of the share of employers' employees enrolled in a high deductible health plan (HDHP), since this information is not available in the 5500 data. To do so, we proxy for employees who have a HDHP based on whether or not they report contributions to a Health Savings Account (HSA) in Box 12 of their W-2 filing. Employees can only legally be enrolled in an HSA if they are enrolled in an HDHP, so this serves as a lower bound on HDHP enrollment. For each employer, we measure the share of employees who report any individual or employer contributions to an HSA.

#### 4.4 County-Level Outcomes

To examine the general equilibrium effects of rising prices, we also undertake a county-level analysis. Our primary county-level outcomes of interest are per capita labor income, the share of the population who are unemployed, the share of the population who are self-employed, tax receipts and UI payments, and the share of the population who moved to another county. We focus on the population of individuals aged between 25 and 64 in the year of interest, who represent the individuals likely to receive ESI directly rather than receive coverage under Medicare or as a dependent on a commercial plan.

To measure labor income, we combine information from each individual's W-2 forms and Schedule SE forms to capture earned wages and self-employment income, when present. We measure unemployment based on whether an individual is receiving UI based on the presence of any income in Box 1 in filings of Form 1099-G (which denotes the amount of UI payments received) or earned no positive income from W-2 or Schedule SE forms. We allow these measures to only apply to those who were not unemployed (by this measure) in the year prior. As a result, our approach captures *flows into unemployment* rather than the stock of unemployed.<sup>8</sup> We count individuals as self-employed if they filed a Schedule SE form with any positive income during the year. We

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<sup>7</sup>While the 5500 data provide a granular measure of insurance premiums at the employer level, these data are limited to small- to medium-sized employers that purchase fully-insured plans. The premiums for self-funded plans are not well-documented in Form 5500, and they do not reliably reflect the total cost of insurance provision in the way that they do for the fully-insured.

<sup>8</sup>We do so since UI take-up explains much of the variation in our unemployment measure, and most states have time limits on UI receipt, meaning that previous recipients may be unable to claim it in the future. Therefore, our approach is likely to be a lower bound on the stock of unemployed individuals in a given year.

identify movers as individuals whose county of residence changed between  $t - 1$  and  $t$ . We measure tax payments as the total amount reported as withheld on W-2 forms.<sup>9</sup> Finally, we measure UI payments as the total amount paid as reported on Form 1099-G. We focus on labor market outcomes for individuals age 25 through 64 (i.e., working-age adults).

We measure per capita outcomes as the sum of the outcomes described above divided by a county-level population measure for individuals age 25 to 64. We measure the county population using population files from the IRS that contain information from all tax returns that provide individual-level information with location identifiers (e.g., W-2 forms, 1099s, etc.). We identify individuals' county of residence using multiple tax forms in a hierarchical order. In practice, we use the Form 1040 filed for the prior year (typically filed between January and April of the reference year). If an individual did not file a 1040 for the prior tax year, we use information from the W-2 form and the Form 1099-G in the reference year. A virtue of this approach is that it allows us to measure population by income group. However, a drawback of this approach is that individuals who lose their jobs and exit the labor market can potentially go missing from IRS data. As a result, we also show our results are robust to measuring the county population among individuals age 25 to 64 using the US Population Estimates from the Census Bureau.

We additionally measure county-level death by cause using microdata from the Restricted Vital Statistics database provided by the Centers for Disease Control and Prevention's (CDC) National Center for Health Statistics. These data capture information from all US death certificates, including the decedent's location, age, and cause of death. We match all recorded deaths between 2008 and 2017 to their counties. Using these data, we construct a measure of deaths per 100,000 people in a county and year.

#### **4.5 Summary Statistics**

We present descriptive statistics for our employer and county samples in Panels A and B of Table 1. Our primary employer sample contains 140,300 unique employers. Across 2008 to 2017, the average employer in our sample had 297 employees and average annual wages per worker of \$41,339. The average employer spent (according to our constructed measure) \$4,099 per employee on health care. Among our employers, 6.8% (9,471) were in the health care industry, with the other 93.2% (130,829) in other industries.

Our primary county sample includes 1,709 of 3,182 US continental counties. These 1,709 counties make up 160 million individuals aged 25 to 64 annually, which is roughly 96% of the total US population within that age group. There were approximately 31 million HCCI beneficiaries in those counties annually in our sample period. In these counties, the average income across 2008 to

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<sup>9</sup>Although withholding sometimes does not capture the full tax bill, especially given the over-withholding that is typical, we expect any error in this measure to be independent from exposure to hospital merger-related price increases.



2017 was \$41,908, the share of the population unemployed was 8.9% (3.6% with UI and 5.4% with zero income),<sup>10</sup> the self-employed share was 11%, average federal income tax payments per capita were \$7,009, and average UI payments were \$482. The mean county experienced 237 deaths per 100,000 population age 25 to 64 annually, with 23 deaths per 100,000 from overdoses and suicides.

In Appendix Table A.1, we show how the composition of our sample of employers changes as we introduce additional sample restrictions imposed by data availability. For example, our primary employer sample is composed of 140,300 employers, whereas our sample with data on insurance offerings from Box DD on W-2 returns is composed of 39,341 employers because only firms with over 250 employees are required to complete Box DD. Employers for which we have data on insurance provision are markedly larger than employers for which we do not. Employers outside the health sector are modestly larger than health care employers. In Appendix Table A.2, we show how our analytic sample of counties differs from the universe of counties in the US. The population in our analytic sample has modestly higher incomes and a lower share of the population that is self employed.

## 5 Empirical Strategy

Our goal is to trace out the causal impact of increases in health care prices on downstream outcomes. Our core empirical challenge is the potential for reverse causality: that increases in prices may be *caused* by changes in an employer's local economic conditions that affect the wages it pays and the workers it employs. For example, if demand for software spikes, the employers who produce software may increase their employees' salaries and hire additional employees. Since health care is a normal good, both of these changes in labor market outcomes will raise demand for local health care services. This increase in demand, in turn, could raise prices and health spending, inducing a spurious positive correlation between prices and labor market outcomes.

We therefore need to find a source of changes in health care prices that is not driven by local economic conditions. Our approach is to use exposure to price increases generated by new horizontal hospital mergers as an instrument for the price of hospital care consumed by employees. We do so in three steps. First, we estimate the effect of hospital mergers on hospital prices. Hospital mergers vary in the degree to which they reduce market competition, and therefore the degree to which they raise prices. Second, we map these hospital-specific price increases to changes in the employer-specific price of health care. Finally, we develop multiple approaches to illustrate that the hospital mergers that generate the largest price increases are not occurring in areas experiencing differential changes in the key labor market outcomes we study.

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<sup>10</sup>Note that our definition of the share of the population unemployed is different from standard unemployment rates as defined by the Bureau of Labor Statistics and other statistical agencies because we define an individual as being unemployed if they are ever without a job and receiving UI throughout the year.



## 5.1 Hospital Mergers, Competition, and Price Changes

We focus on mergers involving hospitals located less than 50 miles from their merging counterparts between 2010 and 2015.<sup>11</sup> Likewise, we only include the hospitals within a merger that are within 50 miles of one another. That is, if Hospital A merges with a system that includes B and C, and B is 10 miles from A but C is 75 miles from A, we include A and B but not C in our sample. Finally, we restrict our sample to hospitals for which we have sufficient data from HCCI to measure prices. After these restrictions, we are left with 304 mergers involving 654 hospitals. We discuss the selection of these mergers and a more detailed description of our estimation approach in Appendix B. Appendix Figure A.1 presents a map of the mergers we include in our analysis.

Across our analytic sample, the average merger generated a change in the HHI of 385 points and led to a post-merger HHI of 6,038 points.<sup>12</sup> As the DOJ and FTC noted in their 2010 Merger Guidelines, mergers that raise the relevant market’s HHI by 200 points or more and lead to a post-merger HHI of over 2,500 should be “presumed to be likely to enhance market power” (U.S. Department of Justice and the Federal Trade Commission, 2010).<sup>13</sup> Twenty-three percent of the mergers in our sample (69 transactions) generated a change in HHI of over 200 and led to a post-merger HHI of over 2,500.

An alternative method for characterizing the change in market power generated by a merger is the change the merger induces in willingness-to-pay (WTP). WTP is a standard screening tool used in hospital antitrust enforcement (Capps et al., 2019). The WTP approaches first measures the substitutability of merging hospitals, then uses this substitution estimate to predict the change in negotiated prices between when the hospitals are independent and when they jointly bargain as a merged unit. We describe the model and procedure we use to estimate WTP in Appendix B.4. Approximately 24% of our transactions generate a change in WTP of over 2.5%, and 11% involve a change in WTP of over 5%. As Garmon (2017) and Brot et al. (forthcoming) illustrate, the increase in WTP generated by a merger approximately predicts the increase in price that the merger will produce.

## 5.2 Measuring the Effect of Mergers on Hospital Prices

Our primary approach to estimating the effect of mergers on hospital prices is a difference-in-differences design that follows Brot et al. (forthcoming) and compares prices at merging hospitals before and after a merger is consummated to prices at control hospitals. We begin by constructing hospital-by-year price indices for inpatient and outpatient care. Because hospitals are multi-product

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<sup>11</sup>We do so since Cooper et al. (2019a) do not find meaningful price increases after transactions involving merging parties farther apart.

<sup>12</sup>We describe our approach to measuring hospital market HHI in Appendix B.3.

<sup>13</sup>In 2023, the DOJ and FTC introduced revised Merger Guidelines that define a problematic transaction as one that increases HHI by more than 100 and leads to a post-merger HHI of greater than 1,800.

firms, we construct hedonic price indices that adjust for the mix of services offered at each hospital and the age and sex of hospitals’ patients. We provide a detailed description of how we measure hospital prices in Appendix A.

In practice, we compare prices at merging hospitals pre- and post-merger to prices at comparable matched non-merging hospitals over the same time period. We use propensity scores to find the 25 most similar non-merging hospitals based on a range of observable hospital and market characteristics. We describe this matching procedure in more detail in Appendix B.2. We limit our analysis to the period covering two years before and after each merger, carving out the year the merger was consummated from our estimation. We then estimate a regression for each merging hospital:

$$\log(p_{eht}) = \lambda_{eh} \times \mathbb{1}\{\text{post-merger}\}_{ht} + \eta_{eh} + \eta_{et} + \varepsilon_{eht}, \quad (2)$$

where  $e$  is a merger event and  $t$  is a year. Our target parameter is  $\lambda_{eh}$ , the percent increase in price at hospital  $h$  due to merger event  $e$ . After estimating  $\lambda_{eh}$ , we shrink each estimate towards the mean to reduce measurement error, using the standard empirical Bayes approach.

### 5.3 Hospital Mergers and Hospital Prices

We estimate substantial variation in the scale of the post-merger price increases generated across our sample of transactions — the  $\lambda_{eh}$ s in our analysis. While the average merger in our sample raised prices by 1.2%, 23% of transactions raised the HHI of a merging party by 200 or more and resulted in a post-merger HHI of 2,500 or more (the threshold for a merger that “significantly increases concentration in a highly concentrated market” and thus is presumed likely to cause harm, as per the 2010 Horizontal Merger Guidelines), while the other 77% did not. In Figure 2, we plot event study figures showing the average difference in prices for merging and non-merging hospitals before and after mergers, separately for mergers that are presumed likely to cause harm and those that are not. Mergers that increased concentration in already-concentrated markets raised led to an average price increase of 5.9% in the two years after the merger occurred, while those that did not generated no meaningful price changes. Our event study figures suggest that there were no differential price trends pre-merger between merging and non-merging hospitals in either group.

In Appendix Figure A.2, we replicate this analysis but instead group mergers by their resulting post-merger increases in WTP. Mergers that increased WTP by 5% or more (11% of mergers) resulted in average inpatient price increases of 5.2%, while mergers that increased WTP by 2.5% or more (24% of mergers) resulted in average inpatient price increases of 4.1%.<sup>14</sup>

<sup>14</sup>We focus on inpatient hospital prices since WTP is constructed using demand for inpatient services. Changes in WTP measured this way are less predictive of post-merger increases in outpatient prices.

## 5.4 Instrument Construction

It might appear natural to take a similar difference-in-differences strategy to compare the outcomes of employers near a merging hospital to other, non-exposed employers. However, there are simply too many mergers in this period to make such an approach tractable. Virtually every employer in our sample had a hospital merger occur nearby (e.g., in the same commuting zone), and the small number of completely unexposed employers are located in unusually rural areas. Moreover, many employers were exposed to multiple mergers, making it untenable to neatly distinguish between pre- and post-merger treatment periods.

Instead, we use a continuous measure of employers' exposure to mergers across time to use as an instrument for health care prices. Specifically, we construct a single instrument that summarizes how the price of care consumed by an employer's employees would change over time if the only thing that occurred were the mergers we observe during our sample period. Our approach holds fixed the prices for other non-hospital health care services, as well as the mix and quantity of health care that is consumed. We then use this measure of employers' exposure to price changes as an instrument for employers' measured prices. This approach allows us to think about mergers as local shocks that generate price changes of varying size, which differentially raise health care prices across employers and over time.

We define an employer  $i$ 's exposure by time  $t$  to a merger event  $e$  occurring at a specific hospital  $h$  as:

$$Z_{ieht} = \underbrace{\lambda_{eh}}_{\substack{\% \text{ price change} \\ \text{at hospital } h}} \times \underbrace{\sigma_{ih0}}_{\substack{\text{share of spending at} \\ \text{hospital } h \\ \text{at baseline}}} \times \underbrace{1[t \geq \tau_e]}_{\text{Timing of merger}}, \quad (3)$$

Here  $Z_{ieht}$  captures the exposure each employer has to a specific merger. Exposure is a function of 1) the scale of the price increases generated by mergers at hospitals where their employees receive care, given by  $\lambda_{eh}$ ;<sup>15</sup> 2) the timing of when hospital mergers occur during our sample period, given by  $1[t \geq \tau_e]$ ; and 3) the extent to which an employer's employees receive care at merging hospitals, given by  $\sigma_{ih0}$ . We cannot match employees in the IRS data to utilization in the HCCI data. Instead, we identify the share of hospital spending that residents from each county spend at each hospital in the US, match those county-level shares to each employee in the IRS data based on the county where the employee lives, and then take an employer-level average across employees.

Rather than capturing the pure effect on price,  $Z_{ieht}$  simulates the expected change in *spending* as a result of the price increase. We use spending rather than prices alone for two reasons: first, by incorporating quantities, spending effectively weights different price changes by their relative importance to the employer. Second, spending, unlike price levels, can be measured per-person

<sup>15</sup>We assume that a merger only affects prices once it occurs, and has a constant effect on the price of care at that hospital for that year and every year following.

(which we do here), and therefore normalizes our instrument by the same unit (people) as is used by our outcome measures. To make sure that our instrument is driven *only* by changes in price, rather than changes in quantities, we measure spending shares in 2008 and 2009, in notation as “time 0,” and hold them fixed across time within our instrument.

To construct a single employer-year-level instrument, we simply sum these  $Z_{ieht}$  across the set of all hospital merger events  $\mathcal{E}$  nationwide for all hospitals  $\mathcal{H}$ :

$$z_{it} = \sum_{e \in \mathcal{E}, h \in \mathcal{H}} Z_{ieht}. \quad (4)$$

where  $z_{it}$  is “simulated spending,” i.e., how health care spending (per-covered-life) would evolve for employer  $i$  if the only thing that changed between 2009 and  $t$  were price changes resulting from hospital mergers. Across our sample, the mean 2009 to 2015 change in employer-level simulated spending was 0.1%, with a standard deviation of 0.92 percentage points. The top 5% of employers experienced an increase in simulated spending of 1.5%.

With our instrument  $z_{it}$  constructed, we estimate our model using two-stage least-squares (2SLS), regressing outcomes  $y_{it}$  on per-person spending  $S_{it}$ , instrumenting for  $x_{it}$  with simulated spending  $z_{it}$ , controlling for employer and year fixed effects:

$$S_{it} = \delta \times z_{it} + \Theta_i + K_t + u_{it} \quad (5)$$

$$y_{it} = \beta \times S_{it} + \theta_i + \kappa_t + \varepsilon_{it}. \quad (6)$$

where  $S_{it}$  is our measure of employer health care spending per covered life, as defined in Section 4.2. We cluster our standard errors in this analysis at the employer level.

In the first column of Table 2, we display our estimates of Equation (5). Our standard employer-level first-stage regression has a coefficient of 0.65, with an F-statistic of 864.78. This coefficient illustrates the extent to which merger-driven price increases are passed through into total spending. Our measure of simulated spending reflects a level change, since it is measured in percentage points. Therefore, to estimate pass-through, we must exponentiate the estimated coefficient. After doing so, our results imply that 91% of the price increases from hospital mergers are reflected in spending changes, implying limited quantity responses. In Appendix Figure A.4, we present binned scatterplots of the relationship between the change in simulated and true spending from 2010 to 2015.<sup>16</sup>

<sup>16</sup>In various locations in the paper, we use alternative versions of our first-stage (for example, when we run our analysis of the impact of rising health care prices on insurance premiums and have to use a restricted sample of employers). In Appendix Table A.3, we include the alternative first-stage estimates we use throughout the analysis.

## 5.5 Identification

Our simulated spending instrument relies on two standard assumptions common to instrumental variables approaches: relevance and exclusion. The F-statistic from our first-stage regression presented above suggests that our instrument is relevant and strong. Satisfying exclusion requires assuming that any idiosyncratic changes in exposure to mergers are unrelated to idiosyncratic employer-specific changes in outcomes, except through the channel by which they make health insurance more expensive to purchase. This exclusion restriction is a continuous analog to the sort of parallel counterfactual trends assumptions required in standard difference-in-differences research designs.

Since our instrument has three components—merger timing, merger intensity, and employer-to-hospital exposure—the exclusion restriction must bind for all three separately. To illustrate the drivers of the variation in our instrument, in Appendix D, we construct alternative versions of our instrument in which we purge the identifying variation from one or more of these three components, using the method from [Borusyak and Hull \(2023\)](#). Measuring the strength of our instrument after sequentially removing each source of variation allows us to decompose the relative contribution of merger timing, merger intensity, and employer-to-hospital exposure to the variation in our instrument. As we illustrate in Appendix Table A.3, we find that our instrumental variation is primarily driven by variation in the scale of the post-merger price increases across mergers that employees are exposed to. This is illustrated by the fact that we obtain a weak and slightly negative first stage when the contribution of cross-merger variation in post-merger price increases is excluded from our instrument.<sup>17</sup>

Given that most of our variation comes from cross-merger variation in the price increases they generate, a primary threat to identification is the possibility that mergers increase prices by more in areas that are already on better or worse economic trajectories. We work to rule out this possibility in two ways. First, we run a regression estimating the relationship between lagged trends in economic activity and 1) the physical location of merging hospitals, 2) the physical location of mergers that generate substantial increases in market concentration and WTP, 3) the physical location of mergers that raise prices by 5% or more, and 4) the price effects of hospital mergers (our  $\lambda_{eh}$  from Equation (2)). As we illustrate in Appendix Figure A.3, there is no economically or statistically significant relationship between the trends in local economic performance (trends in income per capita and the share unemployed) in a region and the location of mergers, the changes in HHI and WTP the mergers generate, the scale of the price increase generated by mergers, and the location of mergers that raise prices by 5% or more.

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<sup>17</sup>Additionally, in the second column of Table 2, we display the coefficients from the first-stage regression where we only allow for identification from this cross-merger variation. The modified first-stage coefficient estimate is quite similar to the estimate from our primary approach, with an F-statistic of 528.

Second, as a more direct test, we construct an analogue to an event study analysis and test whether our instrument is correlated with past changes in economic trends. To do so, we adopt the distributed-lag approach of [Schmidheiny and Siegloch \(2023\)](#). Rather than separately estimating the effect of the instrument in  $t$  on outcomes in  $t - k$ , we simultaneously estimate the effect of the instrument in  $t - k$ , for all values of  $k$ , on outcomes in  $t$ . That is, we estimate the below specification:

$$y_{it} = \sum_{k \in \{-6, \dots, 7\}} \gamma_k z_{i(t-k)} + \theta_i + \kappa_t + \varepsilon_{it} \quad (7)$$

By estimating all of the leads and lags simultaneously, we can, for instance, estimate the effect of merger exposure today on outcomes in the past, *holding fixed merger exposure in the past*. [Schmidheiny and Siegloch \(2023\)](#) show that this is equivalent to an event study approach in simple settings. To recover event study estimates  $\beta_k$ , we cumulate the coefficients as follows:

$$\beta_k = \begin{cases} -\sum_{\ell=k+1}^{-1} \gamma_\ell & \text{if } k \leq -2 \\ 0 & \text{if } k = -1 \\ \sum_{\ell=0}^k \gamma_\ell & \text{if } k \geq 0. \end{cases} \quad (8)$$

As in typical event studies, we normalize  $\beta_{-1}$  to zero. The extent to which  $\beta_{-2}$  is different from  $\beta_{-1}$  serves as an analogue to a pre-trend test of parallel counterfactual trends: it measures the extent to which exposure to greater merger-driven price increases is correlated with changes in outcomes in earlier years.<sup>18</sup>

In [Figure 3](#), we present event study estimates of our first-stage as described above, regressing employer-level spending on simulated spending. We can interpret the difference between  $\beta_{-2}$  and  $\beta_{-1}$  as the trend in spending before mergers of differing intensity occur. The minimal declining pre-trend implies that employers facing greater exposure to merger-induced price increases experienced very slightly declining spending in the prior years. Once the merger is consummated (event time 0), spending increased precipitously and continued to rise in the following years.<sup>19</sup>

<sup>18</sup>Note that, when considering downstream outcomes, we estimate the reduced-form regression of outcomes on the instrument directly, rather than instrumenting for spending. We do so because, consistent with our identification assumptions, the effect of merger-driven price increases on *earlier* spending levels is close to zero. If we constructed the associated Wald estimator for these periods, it would produce an extremely large and biased estimate due to weak instruments bias, leading us to incorrectly reject parallel trends.

<sup>19</sup>The upward trend reflects institutional arrangements. Some insurer-hospital contracts are renegotiated infrequently, so even after a merger, it may take a year or two for the merging hospital's market power to be fully reflected in prices, as is also illustrated in [Figure 2](#).

## 5.6 Alternative Strategies for Measuring the Effect of Mergers

This analysis (and our later analysis of downstream outcomes) suggests that employer exposure to mergers is not correlated with pre-existing economic trends. However, our measure of post-merger price increases is obtained by estimating changes in prices for local hospitals. As a result, our estimated price effects may also pick up other shocks to the supply or demand of health care that happen to occur simultaneously with mergers. Even if these simultaneous shocks occur randomly, they may explain some of the variation in exposure and may themselves violate exclusion restrictions. For example, there may be simultaneous demand shocks that raise prices as a result of local income growth. Including variation from these shocks may bias our results.

To purge this bias, as an additional robustness test, we replicate our main analyses using a modified version of our instrument where, rather than using the true estimated post-merger price increase  $\lambda_{eh}$  for each merging hospital, we instead use the *predicted* change in price given the change in competition induced by the merger, as measured by the change in WTP described in Section 5.1.<sup>20</sup> Note that this approach is conservative: by using *only* changes in market structure, we are purging our instrument of any sources of error in estimated price effects. However, in turn, we are also purging it of any variation that is valid but not captured by the bargaining model used to estimate WTP (Capps et al., 2003) — e.g., differences in the negotiating skill of hospital executives. In the third column of Table 2, we present the coefficient from the first-stage regression using this alternative instrument. The coefficient is a similar magnitude to our primary estimate, and has a strong F-statistic.

## 6 Employer-Level Results

### 6.1 Health Insurance Outcomes

We begin by estimating the effect of rising health care prices two insurance market outcomes: ESI premiums and employee HDHP enrollment. Analyzing premiums is a vital link in our causal chain, since the market for health care and market for labor are intermediated by the price of employer-sponsored health insurance. Additionally, we proxy for the share of an employer’s employees enrolled in a HDHP by measuring the share of their employees who have a non-zero employee or employer contribution to an HSA, since contributions to an HSA cannot be made without being enrolled in an HDHP. Note that, as described in Section 4.3, whereas we can analyze the use of HSAs for all employers, our analysis of insurance premiums requires that we use a restricted panel of 3,970 employers that we can link to their Form 5500 filings.

We report estimates of the effect of rising health care prices on insurance premiums in Column

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<sup>20</sup>We describe the specific measurement of changes in WTP in Appendix B.4.



(1) of Table 3. Panel A presents results from (uninstrumented) OLS regressions estimating Equation (6). There is little cross-sectional relationship between health care prices and insurance premiums. By contrast, in our primary 2SLS estimates of Equation (6), presented in Panel B, we find that a 1% increase in health care prices leads to a 0.95% increase in ESI premiums. These estimates suggest that rising health care prices are virtually fully reflected in higher insurance premiums.<sup>21</sup> Our relatively low power for this outcome largely reflects the fact that the Form-5500-linked employer panel only contains 3,970 unique employers, compared to 140,300 in our primary sample.<sup>22</sup> As we demonstrate in Appendix Table A.5, when we use our alternative instrument that replaces estimated price effects with predicted price effects, based on changes in market structure (measured by WTP), we find effects of similar magnitude and statistical power.

Employers could respond to rising health care premiums due to rising prices by reducing the generosity of their coverage (e.g., shifting policyholders into higher deductible plans or plans with other forms of enhanced employee cost-sharing). In Column (2) of Table 3, we present OLS and IV estimates of the effect of rising health prices on employer provision of HDHPs (as measured by whether their employees contribute to an HSA). Whereas our OLS estimates suggest a positive relationship between health care prices and the use of HDHPs, when we instrument for health care prices, we do not find an economically or statistically significant relationship between health care prices and the use of HDHPs. This suggests that employer demand for coverage is very inelastic. While this may seem surprising at first, it is important to remember that employers must offer benefits uniformly to all workers, and so benefit adjustment is a coarse response to rising prices.

## 6.2 Labor Market Outcomes

We next turn to estimating the effect of rising prices on labor market outcomes. We focus on employers' payroll and their count of workers (in logs) as our primary outcomes. In Columns (1) and (2) of Panel A of Table 4, we present OLS estimates of Equation (6) and find no economically or statistically significant relationship between rising health care prices and employers' payroll or count of workers. By contrast, in Panel B, we present 2SLS estimates of Equation (6) and find that a 1% increase in health care prices reduces both payroll and employment by 0.36%.<sup>23</sup>

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<sup>21</sup>Note that the average actuarial value of the health plans in our sample, as measured in the HCCI data, is 0.81. As a result, our estimates imply a pass-through rate slightly above 1. However, our confidence interval on our premium response estimate includes 1.

<sup>22</sup>We have insurance premiums for 3,970 employers. We can merge 81% of employers from our Form 5500 insurance premium sample to the panel of employers we use to measure labor market outcomes. For employers from our insurance premium sample that do not merge into our analytic sample of EINs, we use a county-level measure of simulated spending rather than an employer-level measure.

<sup>23</sup>That we find equal payroll and employment effects suggests minimal wage pass-through, implying that the incidence primarily falls on employers. We caution, however, against such a strict interpretation of this result, for two reasons. First, our estimates average over many worker types. This result is consistent with higher-wage workers taking a wage cut while lower-wage workers see employment cuts, resulting in no effect on average income per retained worker

Our analytic sample includes employers from both the health sector and other sectors. Non-health care employers are likely to reduce payroll and employment following an increase in local health care prices because health care price increases raise their insurance premiums. By contrast, health care employers receive higher prices following a merger and thus higher revenue, which may *increase* their payroll and employment. In Table 4, we measure the employment effects separately at non-health care (Columns (3) and (4)) and health care employers (Columns (5) and (6)).<sup>24</sup> After segmenting employers by industry, we see that our overall results are entirely driven by changes in payroll and employment at non-health care employers. Among non-health care employers, we find that a 1% increase in health care prices lowers payroll by 0.37% and lowers the count of workers employed by 0.4%. Conversely, we do not find that an increase in health care prices generates any statistically significant changes in payroll or employment at health care employers.

We present quasi-event-study estimates of the effect of rising health care prices on payroll and the count of workers (in logs) at non-health employers in Panels A and B of Figure 4. The event studies show that there are flat trends in labor market outcomes in the two years before treatment, compared to significant effects after the instrument increases in value. This helps rule out differential trends in pre-merger labor outcomes for employers who are more versus less exposed to mergers. The coefficients for event time 0 are sizable, suggesting that employers respond immediately to increases in health care prices.<sup>25</sup>

In Appendix Figure A.6, we show our baseline employer-level estimates are similar in both magnitude and precision across a range of alternative specifications: 1) using only the identifying variation in our instrument coming from cross-hospital variation in post-merger price increases; 2) replacing our estimates of post-merger price increases with predicted price increases given post-merger changes in market structure (measured via WTP); 3) forcing our estimator to only compare outcomes for employers within comparable subsets of firms by interacting our year fixed effects with quartiles of employer size (measured by employee counts in 2009), industry (measured as NAICS code in 2009), quartiles of payroll growth (measured as the percent change between 2006 and 2009), or quartiles of size growth (measured as the percent change in employee count between 2006 and 2009); and 4) excluding employers in the bottom or top quartiles of payroll or size growth

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despite wage pass-through. Second, our payroll measure only includes wage compensation. If employers respond to premium increases by raising the employee ESI contribution, this will lower non-wage compensation but not appear in our payroll measure.

<sup>24</sup>We identify employers in the health care industry by whether they have a reported NAICS code starting in “62” as reported to the IRS.

<sup>25</sup>The estimated coefficients approximately double in magnitude between event time 0 and event time 2, which would initially suggest that effects are increasing over time. We would caution against such a conclusion, however, since these are reduced-form estimates, and, as Figure 3 shows, the first-stage coefficient also approximately doubles over this event time window. If we appropriately rescaled these estimates to account for the size of the first-stage, we would conclude that price increases primarily have immediate level effects on payroll and employment.

between 2006 and 2009.<sup>26</sup>

### 6.3 Scaling Employers' Response Rising Health Care Prices

Scaling our primary point estimates on a dollar-for-dollar basis, a 1% increase in health care spending translates into a \$40.09 increase in spending per insurance plan member. The average employee in our sample, according to our HCCI data, has an insurance plan that includes one dependent (two total members), generating a \$80.18 spending increase per employee. The median employer in our sample has 132 employees, so a 1% increase in in spending at an employer, summed across its employees and employees' dependents, is \$10,583.76. Our point estimate in Column (1) of Table 4 implies that a 1% increase in health spending leads to a 0.36% reduction in payroll, which is equivalent to an approximately \$17,900.00 decrease at the median employer.

The fact that the total payroll reduction is greater than the spending increase does not necessarily reflect greater than one-for-one pass-through of health insurance costs into wage levels. Instead, it reflects the extensive margin responses by employers to reduce the number of workers they employ, which can reduce total worker pay by far more than the direct cost shock. We interpret the gap between the revenue received by hospitals and the reduction in payroll as the deadweight loss of the “hospital tax” in which mergers transfer funds to the health sector from others to fund the increase in prices. By this calculation, there is roughly \$0.69 of deadweight loss per dollar of additional hospital revenue generated by price increases.

To put these figures in context, we benchmark our estimates against studies of employer-level responses to payroll tax changes. In our setting, a 1 percentage point increase in the payroll tax would cost the median employer \$365 per worker. The median employer spends \$4,039 per covered life (and thus \$8,078 per worker), meaning that a 1 percentage point increase in the payroll tax is roughly equivalent to a 4.5% increase in health care spending. Since our estimated coefficient (from Column (4) of Table 4) says that a 1% increase in health care spending reduces employment by 0.4%, a 4.5% change should reduce employment by 1.8%. In Appendix Table A.6, we compile a set of prior estimates of responses to payroll taxation. Estimates from US studies include 0.7 to 0.9% (Anderson and Meyer, 1997), 1.5% (Johnston, 2021), and 1.1 to 2.4% (Guo, 2024). We are reassured that our estimates of deadweight loss sit squarely in the range of what is found in the existing literature.

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<sup>26</sup>The lone estimates where we lose precision are when we exclude the 25% of employers with the greatest wage growth between 2006 and 2009 and the top 25% of employers with the biggest increase in employment between 2006 and 2009. When we exclude those cohorts of employers, our point estimates drop modestly and become marginally less precise.

## 7 County-Level Results

Our results from Section 6 show that employers respond to rising health care prices by reducing the number of workers they employ. However, the large effects we observe on employment may partially be driven by a re-sorting of workers across existing and new employers, as well as by a shift of workers away from wage employment to self-employment. As a result, in this section, we explore whether rising health care prices lead to aggregate effects on income per capita, self-employment, and unemployment. To do so, we aggregate our employer-level instrument up to the county level and focus on a range of new county-level outcomes.

Because the effects of rising health care prices are always intermediated by employers, we define county exposure to health care prices and mergers as the weighted average of employer-level exposure, with employer-by-county weights equal to the extent to which the employer typically hires workers from that county. Specifically, in 2009, we take every worker at an employer in our employer sample and assign them a county according to their address of residence, as reported on their W-2.<sup>27</sup> We then construct employer-by-county weights,  $\omega_{ic0}$ , as the share of workers in a given county  $c$  who worked for employer  $i$  in 2009. We can then construct our endogenous regressor  $S_{ct}$  (per-worker spending) and instrument  $z_{ct}$  as weighted averages:

$$S_{ct} = \sum_i \omega_{ic0} S_{it} \quad (9)$$

$$z_{ct} = \sum_i \omega_{ic0} z_{it} \quad (10)$$

Effectively, we are stipulating that county-level exposure to health care price increases is a function of how employers who hire in that county are exposed to price increases.<sup>28</sup> At the county level, our mean change in simulated spending is 0.1% and the standard deviation is 0.56 percentage points. The top 25% of counties have an increase in simulated spending of 0.2% and the top 5% have an increase in simulated spending of 0.9%. In Figure A.5, we plot a map of county-specific changes to simulated spending from 2009 to 2015.

We then estimate the county-level analog of our main 2SLS regressions:

$$S_{ct} = \delta \times z_{ct} + \Theta_c + K_t + u_{ct} \quad (11)$$

$$y_{ct} = \beta \times S_{ct} + \theta_c + \kappa_t + \varepsilon_{ct} \quad (12)$$

<sup>27</sup>In cases where a given worker reports multiple residences on multiple W-2s, we choose according to the W-2 with the highest wages.

<sup>28</sup>We are using the quantities of *employers* rather than *specific individuals* within the county. Therefore, a county's effective health care price exposure may depend on individuals from other counties, through co-working relationships and the ESI channel.

i.e., we regress outcomes at the county  $c$  and year  $t$  level on county-year health spending, instrumenting with county-year simulated spending and including county and year fixed effects. We cluster all regressions at the county level.

In Column (1) of Appendix Table A.4, we present the first-stage regression results, estimated using Equation (11). Since we have collapsed our data down to the county level, we have fewer observations and thus less power. The F-statistic on our first-stage from this approach is approximately 42, which is above standard thresholds for weak instrument tests.<sup>29</sup>

## 7.1 Labor Market Outcomes

In Table 5, we present estimates of Equation (12) and show the impact of rising health care prices on county-level income per capita, the share of the population unemployed, and federal income tax revenues per capita. As we illustrate in Column (1) of Panel B, a 1% increase in health care prices leads to a 0.27% decrease in county-level income per capita. Note that our measure of income per capita captures both self-employment income and W-2 income for workers inside and outside the health sector. However, as we illustrate in Appendix Table A.7, this overall income effect is driven by changes in the income of non-health care workers.<sup>30</sup> Notably, as we illustrate in Panel A, when we run our OLS regression, we observe a positive relationship between health spending and income per capita. Likewise, as our estimates in Appendix Table A.8 illustrate, we do not estimate that increases in health care prices lead to an economically or statistically significant change in whether an individual receives self-employment income.

In Column (2) of Panel B of Table 5, we show that a 1% increase in health care prices leads to a 0.09 percentage point (1%) increase in overall unemployment per capita, which we measure as the share of the population receiving unemployment insurance or receiving zero income.<sup>31</sup> Per our results in Appendix Table A.7, this overall increase in unemployment is being driven by increases in labor market separations among non-health care workers who gain UI when they lose their job.

In Panel A of Figure 5, we present event study estimates of the effects of rising health care prices on income per capita and unemployment. Our results are consistent with our employer-level results: we find no differences in pre-trends before the increase in spending and a change in labor

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<sup>29</sup>In this table, we also include alternative estimates of the first-stage regression, as we do in Appendix Table A.3. As in those estimates, the primary source of variation comes from post-merger pricing differences. Additionally, replacing price changes with predicted price changes given market structure changes results in an equally strong instrument.

<sup>30</sup>We classify a worker as being in the health care sector if, in the prior year, they filed a W-2 reporting work for an employer with a NAICS code indicating the health care industry. We categorize all other individuals, including those who filed no W-2s in the prior year, as workers outside the health care sector.

<sup>31</sup>Our measure of unemployment is conditional on an individual being employed in the prior year. As a result, our unemployment measure captures *flows* into unemployment rather than the *stock* of the unemployed. Since unemployment insurance is often time-limited, flows into it are more reliable than the stock of current recipients in terms of understanding unemployment patterns. Ideally, we would be able to measure time spent out of employment, but no relevant tax forms report hours worked in a given job or any other quantity measures.

market outcomes immediately after the spending increases occur. As we illustrate in Panel A, labor income falls immediately to a lower level in the year that the price shock occurs; then, it rises in later years as the local economy recovers. One concern with using IRS data to construct a population denominator is that individuals who do not have a job and do not receive UI or disability insurance might drop out of our population measure. As a result, in Appendix Figure A.7, we show our income event study using Census data to construct a denominator. When we use this denominator in lieu of our IRS denominator, we observe a slightly larger overall effect and somewhat less of a return to baseline income two years after the spending shock. In Panel B of Figure 5, we present event study estimates of the effect of rising health care prices on unemployment. The event study illustrates that unemployment spikes immediately in the year of the shock then falls back down to zero within the subsequent two years. This reflects the fact that our measure of unemployment is a flow measure rather than a stock measure.

Our results also suggest that the degradation of the labor market has negative consequences for federal and state budgets. As workers see their salaries reduced and jobs cut, they will have less taxable labor income. Indeed, as we illustrate in Column (3) of Panel B of Table 5, we estimate that a 1% increase in health care prices brings federal income tax receipts down by 0.4%. Likewise, since the employment effects we observe result in individuals receiving unemployment insurance, as we show in Column (5) of Appendix Table A.8, we find that a 1% increase in health care prices leads to a 2.5% increase in transfers to former workers from the UI system. Collectively, these estimates highlight that the incidence of rising health care prices also falls on state and federal governments and not just on employers and workers.

In Appendix Figure A.8, we show how our county-level income per capita and unemployment results shift when we shift our source of variation and alter our sample of counties. First, we show our results are robust to relying exclusively on the variation in post-merger price increases across transactions to drive our instrument. Second, we show that our income result is robust to replacing post-merger price effects with predicted post-merger markup increases (as measured by WTP).<sup>32</sup> Third, we show our results are robust to interacting our time fixed effects with income quartile fixed effects so that all comparisons are made within quartiles. Fourth, our results are robust to excluding the 25% of counties with the lowest income per capita in 2009. Excluding counties in the bottom 25% of change in income per capita from 2006 to 2009 lowers our point estimates and they become imprecise. Fifth, we show that our results are robust to excluding the top 25% of counties with the highest unemployment in 2009 and change in unemployment from 2006 to 2009 largely does not shift our results. Sixth, we show that our results are robust to excluding the 25% most rural counties does not change our results. Finally, we show that our results are robust to excluding the 25% of counties which, based on Autor et al. (2013), were the most exposed to import competition from

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<sup>32</sup>Note that our employment results are not robust to measuring the effect of mergers using WTP.



China in the 2000s does not shift our main point estimates (though it does modestly increase our standard errors).

In Section 6.2, we estimated that each \$1 increase in revenue generated \$0.69 in deadweight loss. We can undertake a similar exercise for our county-level estimates. Our primary county-level estimate finds that a 1% increase in health care spending reduces labor income by 0.268%. Average county-level spending per capita is \$4,210. As before, we know that each employee's plan covers one dependent, on average, so a 1% increase in average spending per *worker* is \$84.20.<sup>33</sup> Average county-level income per person in our sample is \$41,908, and a 0.268% reduction lowers average earnings by \$112.31. Again, we can interpret the gap as the deadweight loss of the hospital tax: for hospitals to increase their revenue by \$84.20, labor income must fall by \$112.31, a reduction of income by \$1.33 per \$1 revenue increase. Therefore, there is approximately \$0.33 of deadweight loss per \$1 revenue increase. This estimate is likely smaller than our employer-level estimate (\$0.69 per dollar) because it accounts for potential reallocation of workers across employers, whereas our employer-level estimate only accounts for the loss at the specific employer.<sup>34</sup> This estimate, however, is estimated with less statistical power than our employer-level estimate and, as a result, the two are not statistically distinguishable.

## 7.2 Distributional Effects

As we discussed in Section 3, rising premiums are costlier, proportionally, for lower-wage workers. Therefore, there should be heterogeneous effects on workers across the income distribution. To test this, for each year, we measure workers' W-2 income in the prior year and segment workers into \$10,000 bins up to \$100,000 in income, after which we use \$50,000 bins, up to a bin for workers who earned \$200,000 and over. We exclude any individuals who received UI in the prior year because their prior income may be an incomplete measure of their relative position in the labor force.<sup>35</sup> We then estimate the effect of price increases on unemployment for each of these groups separately.

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<sup>33</sup>For both estimates, we assume that there is one additional dependent per worker. However, our analysis is done per adult between the ages of 25 and 64. Dependents in this age range (e.g., spouses) will get double counted in our estimates of the spending change of a 1% increase in prices, resulting in overestimates. Therefore, one should think of our deadweight loss calculation here as a lower bound.

<sup>34</sup>A second difference is that this estimate accounts for all workers, whereas our employer-level estimate only accounts for non-health care workers. If we use the reduction in non-health care income estimates from Table A.7, we estimate a \$1.71 reduction in non-health-care labor income per \$1 of hospital revenue received, and a \$0.38 increase in health care labor income. These numbers should be treated with some caution since mergers might induce reallocation of labor between the two sectors. The latter number should also be treated with caution because the county where *patients* (or their coworkers) are located is not the same as the county where *health care workers* are located.

<sup>35</sup>E.g., consider a worker who should expect to make an annual salary of \$60,000. If they worked in the prior year until the end of April, then were laid off, collected UI, and did not work for the rest of the year, we will classify them as having made \$20,000. While they did indeed make this much that year, it does not reflect the labor market they participate in.



We present the results from this exercise in Figure 6. As the figure illustrates, we find close to zero unemployment effects at the top of the income distribution (i.e., those previously earning above \$100,000—approximately the 85<sup>th</sup> percentile of the individual income distribution). This is consistent with the notion that increases in premiums are small relative to overall compensation for this group of workers.

By contrast, however, we find close to zero effects for those at the bottom of the income distribution (i.e., those previously earning below \$20,000). These results are consistent with the fact that lower-wage workers tend not to receive employer-sponsored health insurance benefits (Lurie and Miller, 2023). Because they do not receive ESI, rising health insurance premiums will not make these workers more expensive to retain.

Finally, we observe relatively uniform effects in the middle of this range among workers who previously earned between \$20,000 and \$100,000. We attribute this finding to two forces. First, the “head tax” pushes unemployment effects to be relatively regressive. Second, however, there is a positive correlation between wages and health insurance generosity: employers that offer high average wages also, on average, offer more comprehensive health insurance coverage (Lurie and Miller, 2023). The sum of these two forces implies that the groups hardest-hit by the unemployment effects of rising health care prices are lower- and middle-income workers.

### 7.3 Mortality

Rising health care prices lead employers to lay off workers. A recent literature has documented that job loss can also impact other downstream outcomes, including divorce rates, political attitudes, and longevity. In particular, a growing literature has found that job losses can induce increases in individuals’ risk of premature mortality, particularly via deaths from self-harm, such as suicide, overdose, and liver disease (Eliason and Storrie, 2009; Sullivan and von Wachter, 2009; Pierce and Schott, 2020; Venkataramani et al., 2020). As a result, we analyze whether the job losses induced by rising health care prices also led to increases in mortality.

To do so, we estimate Equation (12) and test the effect of rising prices on county-level counts of suicides, accidental poisonings, or poisonings of undetermined intent per 100,000 people. We limit our analysis to working-age individuals aged between 25 and 64 at time of death (i.e., those who are most likely to receive ESI).<sup>36</sup> As a placebo check, we construct three alternative measures of

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<sup>36</sup>Accidental poisonings and poisonings of undetermined intent typically reflect drug overdoses. We exclude liver disease as it is likely to take significant time to accumulate, whereas poisonings and suicides are acute. We define our death measures, following Case and Deaton (2015) and Pierce and Schott (2020), using International Statistical Classification of Diseases and Related Health Problems (ICD) 10 codes. We define suicides as those with codes for intentional self-poisoning (X60 - X69), other intentional self-harm (X70 - X84), and sequelae of intentional self-harm (Y87.0). We also include accidental poisonings (X40 - X45) and poisonings with undetermined intent (Y10 - Y19). We also include prescription drug complications (Y45, Y47, and Y40) and other harms with other undetermined intent (Y20 - Y25).

mortality that should be largely unaffected by job losses induced by rising health care prices: 1) suicides, accidental poisonings, or poisonings of undetermined intent among individuals age 65 and older, who are likely to be outside the labor market; 2) all-cause mortality excluding suicides, accidental poisonings, or poisonings of undetermined intent among individuals age 25 to 64; and 3) cancer mortality among individuals age 25 to 64.

As we illustrate in Column (1) in Panel B of Table 6, a 1% increase in health care prices increases county-level suicides and overdoses by 0.62 deaths per 100,000 population among working-age adults (a 2.7% increase). OLS estimates are positive as well but much smaller in magnitude. By contrast, as we illustrate in Column (2), we do not estimate a statistically significant change in suicides and overdoses for individuals aged 65 and older, who we think are outside the labor market and therefore unlikely to be impacted by the increase in health care prices. Likewise, as we illustrate in Columns (3) and (4), we do not estimate that health care price increases lead to an increase in all deaths excluding suicides and overdoses or an increase in deaths from cancer.

In Figure 7, we present our event study estimates for deaths from suicides and overdoses among working-age adults. Here, we see flat trends in mortality in the years prior to the spending shock occurring. This suggests that merger shocks are uncorrelated with pre-existing mortality trends. Then, we show that the mortality effects from suicides and overdoses occur at  $k = 1$ , one year *after* the spending shock, with minimal effects in the year of the shock. The implied timing of deaths from suicides and overdoses — a flow measure — tells a clear story when combined with our labor market result: when health care prices rise, there is an immediate disemployment effect. Then, a portion of those who lost their jobs succumb to suicide or overdose in the year after.

As we illustrate in Appendix Table A.9, prior studies estimate rates of approximately 1 death per 300 to 600 job losses. The estimated death-per-job-loss rate has increased over time in these studies concurrently with the increasing intensity of the opioid epidemic in the US. We use our estimates of the effect of rising health spending on employment in Column (2) of Table 5 to scale our effect in terms of deaths per job loss induced by rising health spending. Combining our estimates from Table 5 and Table 6, we estimate that there is approximately 1 death per 140 job losses in our sample, which offers the most recent estimates of the effect of job losses and focuses on a period when opioid deaths in the US were higher than in any previous period.<sup>37</sup> In addition to measuring mortality effects at the peak of the opioid epidemic, our estimates are also likely higher than those of other studies in the literature because, whereas other studies look at the effect of *job loss* on mortality, our measure (receipt of unemployment insurance) captures only workers who do not quickly find another job after being let go.

In Appendix Figure A.9, we show how our county-level mortality results shift when we shift our source of variation and alter our sample of counties. First, we show that our results remain similarly

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<sup>37</sup>We calculate this as  $100,000 * (\text{IV coefficient on unemployment} / \text{IV coefficient on deaths})$ .

scaled but lose precision when we rely exclusively on the variation in post-merger price increases across transactions to drive our instrument. Second, we show that our results are robust to measuring the effect of mergers via WTP. Third, we show our results are robust to interacting our year fixed effects with county population quartile fixed effects so that all comparisons are done within county population quartiles. Fourth, we show our results are similarly scaled but lose some precision when we exclude the 25% of counties with the lowest income per capita in 2009. By contrast, our results remain robust when we exclude the 25% of counties with the lowest income growth from 2006 to 2009. Fifth, we show our results are robust to excluding the 25% of counties with the highest unemployment in 2009 and the counties with the biggest increase in unemployment from 2006 to 2009. Sixth, we show our results are robust to excluding the most rural 25% of counties. Finally, we show our results are robust to excluding the 25% of counties which, based on [Autor et al. \(2013\)](#), were the most exposed to import competition from China in the 2000s.

## 8 Scaling the Effect of Hospital Mergers

In this section, we use our estimates to quantify the average effect of individual mergers on aggregate income, employment, tax revenue, and mortality. To do so, for the mergers of interest, we compute the change in our instrument induced by those mergers for every county in the year the mergers occurred. This change is different across counties, since each county is differentially exposed to a given hospital by virtue of the frequency that its residents tended to go to that hospital before the merger. We multiply this quantity by our first-stage estimate and then by our IV estimate for the relevant outcome. To convert our estimates where the measured outcome is in logs or shares to levels, we multiply the estimate by the baseline county average (for logs) or by the baseline county population (for shares). This process produces estimated effects of mergers on levels of outcomes for each individual county. We then sum over counties to estimate the total effect. This effectively measures the consequences of mergers for one year after they occur.

Across all merging hospitals in our analytic sample, we find that the average post-merger price increase is 1.2%. Our estimates from Section 7 imply that, on average, one of these mergers would have led to a \$6 million reduction in income, 39 job losses, and a \$1.3 million reduction in income tax payments.<sup>38</sup> Because we obtain these figures by integrating over the set of observed price changes, population totals, and hospital spending, they reflect each hospital's observed post-merger price change and the degree to which those changes translate into dollars of additional health care spending.

Focusing on the 69 anticompetitive mergers in our analytic sample — those that generated a

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<sup>38</sup>Note that, unlike in our employer-level analysis, our county-level measure of aggregate job losses is derived from a flag for UI receipt. Since not all of those who become unemployed take up UI, this serves as a lower bound on the total effects of mergers on changes in employment.

change in HHI over 200 points and led to a post-merger increase in HHI of over 2,500 points — we find that the average anticompetitive merger led to a \$16 million reduction in income, a \$4 million reduction in federal tax revenue, 110 job losses, and one additional opioid or suicide death.<sup>39</sup> The US Department of Transportation (DOT) estimates the value of a statistical life in 2015 at \$9.6 million ([Department of Transportation, 2022](#)). This implies that, on average, a hospital merger that was not blocked but did run afoul of the Merger Guidelines led to approximately \$26 million in economic damages (\$16 million in forgone wages and \$9.6 million from deaths).

Collectively, based on our estimates, the 69 anticompetitive mergers in our sample from 2010 to 2015 generated income losses of more than \$1 billion, led to approximately 7,600 job losses, lowered federal income tax revenue by \$256 million, and precipitated 71 deaths — total losses of nearly \$2 billion in the year after the mergers occurred. Across all 304 mergers in our sample, the aggregate harm in the year following a merger added up to \$1.7 billion in forgone wages, 11,704 job losses, and 111 deaths — harms of approximately \$3 billion. For context, the average, annual FTC budget during that period was \$315 million; the antitrust enforcement budget during that period was \$136 million.<sup>40</sup>

## 9 Discussion and Concluding Thoughts

Over half of Americans are covered by an ESI plan. In this paper, we have shown that ESI creates a pathway through which rent-seeking and inefficiency in the health care industry can cause immense harm to local economies. The “hospital tax” that rising health care prices impose on employers lowers employment (both at individual employers and overall in local economies), reduces workers’ earnings, lowers tax revenue, squeezes government budgets, and increases suicides and overdoses. Moreover, the majority of these negative consequences are borne by lower- and middle-income individuals.

During our period of study, prices for inpatient and outpatient hospital care for the privately insured grew by 42.3% and 25.1%, respectively ([Cooper et al., 2019b](#)).<sup>41</sup> Given the share of total health services that hospital care represents, this price increase caused a 10% increase in overall health care spending. As a result, our estimates imply that the price growth between 2007 and 2014 reduced workers’ incomes by 2.7%, increased unemployment by approximately 0.86 percentage points (a 10% increase or 1.44 million jobs lost), lowered federal income tax revenues by 3.4%, and

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<sup>39</sup>The particular anticompetitive mergers in our sample are among smaller hospitals and labor markets than the average merger. If we instead assume a 5% price increase across all mergers in our sample, we estimate a \$32 million reduction in income, a \$6.8 million reduction in federal tax revenue, 203 job losses, and one to two deaths of despair.

<sup>40</sup>These budget figures are drawn from the annual reports of the FTC’s Congressional Budget Justifications (<https://www.ftc.gov/about-ftc/budget-strategy/budget-performance-financial-reporting>) and presented using 2017 dollars.

<sup>41</sup>Note that the [Cooper et al. \(2019b\)](#) estimates are net of inflation.

led to an increase in suicides and overdoses of 6.2 per 100,000 population (approximately 10,000 additional deaths across working-age adults). Based on the DOT's 2015 value of a statistical life (\$9.6 million), the economic value of this loss of life would be approximately \$96 billion. To be sure, some of this increase in hospital prices likely reflects quality increases that improved social welfare. However, those quality improvements would need to be substantial to offset harms on the scale we estimate.

As discussed in Section 8, the average hospital merger led to a \$6 million reduction in local income. From 2002 to 2020, there were over 1,000 hospital mergers in the US. During this period, the FTC took enforcement actions against 13 transactions. Our results are concerning because, as [Brot et al. \(forthcoming\)](#) note, approximately 20% of consummated mergers in our sample could be predicted *ex ante* to raise prices using standard screening tools available to the FTC (such as the WTP measure we use in this paper) and, in practice, did lead to *ex post* price increases that were often far higher than 5%. This suggests that a great deal of highly damaging hospital mergers were observed by, but not stopped by regulators, and that these mergers have had substantial effects on labor market outcomes and mortality outside the health sector. Our results suggest that the average transaction that could have been flagged by the FTC as likely to raise prices by lowering competition led to \$16 million in forgone wages, 110 job losses, and approximately one additional opioid or suicide death.

Ultimately, this work highlights that health care price growth is generating substantial macroeconomic and social consequences in the US. In the absence of concrete steps to address health care price growth, rising health spending will raise labor costs and reduce business dynamism outside the health sector, put pressure on the federal budget, and exacerbate income inequality. Rising health care spending will also precipitate suicides and overdoses. As a result, we hope this research motivates future analysis of strategies to address health care price growth in the US and ways to screen for and challenge hospital mergers that lessen competition and lead to higher prices. On the academic front, we hope this work motivates future analysis of the absolute and distributional effects of rising health care prices and health spending in the US. For instance, while we have focused on the harms to non-health care workers, it will be important for future research to assess who receives the rents accrued from health care price increases. Finally, we hope this work motivates further study of how rising health spending impacts regional growth and productivity across the US.

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**Table 1: Employer-Level and County-Level Summary Statistics****Panel A: Employer Characteristics**

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	N (6)
Employer Total Payroll*	12,721,000	25,129,000	2,391,000	4,977,000	11,418,000	140,300
Employer Count of Workers	297	509	75	132	282	140,300
Employer Average Wages per Worker	41,339	25,431	23,408	36,525	52,809	140,300
Share of Employees with Premiums	0.511	0.314	0.215	0.597	0.770	39,341
Share of Employees with a Health Savings Account	0.038	0.121	0.000	0.000	0.000	140,300
Health Spending per Beneficiary	4,099	704	3,649	4,039	4,478	140,300
Premiums from 5500 Data	5,036	1,574	3,943	4,930	6,001	3,970

**Panel B: County Characteristics**

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	N (6)
Income per Capita	41,908	9,205	35,872	39,911	45,369	1,709
Share with Unemployment Insurance	0.036	0.017	0.025	0.032	0.042	1,709
Share with Zero Income	0.054	0.014	0.043	0.053	0.062	1,709
Share Unemployed	0.089	0.024	0.073	0.085	0.101	1,709
Unemployment Insurance Payments per Capita	482	395	185	363	664	1,709
Share Self-Employed	0.110	0.023	0.095	0.107	0.123	1,709
Share Moving Annually	0.066	0.021	0.052	0.063	0.076	1,709
Income Tax Withholdings per Capita	7,009	2,088	5,634	6,583	7,836	1,709
Health Spending per Beneficiary	4,210	444	3,910	4,182	4,479	1,709
All Deaths per 100k People	237	72	184	229	282	1,709
Deaths from Suicides and Overdose per 100k People	23	12	15	21	29	1,709
Deaths from Cancer per 100k People	65	19	51	63	76	1,709

**Notes:** This table presents employer-level and county-level descriptive statistics for our main analytic samples, from 2008 to 2017. In Panel A, employer payroll, employer counts of workers, employer wages, the share of employees with premiums, and the share of employees with a health savings account come from the Internal Revenue Service (IRS). Data on health spending per beneficiary come from the Health Care Cost Institute (HCCI). Data on insurance premiums comes from the Department of Labor's 5500 forms. In Panel B, income per capita, the share with unemployment insurance, unemployment insurance payments per capita, the share of the population self-employed, the share of the population moving out of the county annually, and income tax withholdings per capita come from IRS returns. Income per capita is measured as the sum of wage (W-2) income and self-employment (Schedule SE) income. We define the share unemployed as the share of individuals with either positive unemployment insurance receipts and/or with zero income in the year. The deaths per 100,000 measures are from the Center for Disease Control and Prevention's Restricted Mortality Database.

\* Rounded to \$1,000.

**Table 2:** First Stage: Regressing Annual Health Care Spending on Simulated Health Care Spending

	Log(Health Spending per Beneficiary)		
	(1)	(2)	(3)
Simulated Spending	0.649*** (0.022)	0.521*** (0.023)	2.458*** (0.051)
Instrument	Baseline	Using Only Price Change Variation	Using Predicted Price Changes
Mean Dependent Variable	4,042	4,042	4,042
Observations	1,403,000	1,403,000	1,403,000
Number of Unique Employers	140,300	140,300	140,300
F-Statistic on First Stage	864.776	528.533	2,350.093

**Notes:** This table presents coefficient estimates from a regression of employer-level annual health spending per beneficiary on employer-level simulated spending per beneficiary, as given in Equation (5). Each estimate includes employer and year fixed effects. Each column presents estimates from a regression using a different instrument. Column (1) presents estimates using our baseline instrument. Column (2) presents estimates using a modified version of our baseline instrument that purges any variation other than that coming from differences in post-merger price changes across mergers. Column (3) presents estimates using a modified version of our baseline instrument that replaces the estimated post-merger price increases with the estimated post-merger change in WTP. Data on health spending and simulated spending come from the Health Care Cost Institute. Means are reported in levels rather than in logs. Standard errors are reported in parentheses and are clustered at the employer level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 3:** The Impact of Rising Health Care Prices on Health Insurance Market Outcomes

<b>Panel A: OLS Estimates</b>		
	Log(Insurance Premiums) (1)	Share of Employees with a Health Savings Account (2)
Log(Spending per Beneficiary)	0.025 (0.032)	0.012*** (0.001)
<b>Panel B: IV Estimates</b>		
	Log(Insurance Premiums) (1)	Share of Employees with a Health Savings Account (2)
Log(Spending per Beneficiary) <i>(Instrumented Using Merger-Driven Price Increases)</i>	0.947* (0.535)	0.0004 (0.042)
Mean Dependent Variable	5,036	0.038
Observations	39,700	1,403,000
Number of Unique Employers	3,970	140,300
F-Statistic on First Stage	43.391	864.776

**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of annual employer-level log health insurance premiums per enrollee (Column (1)) and the share of employees with contributions to a health savings account (Column (2)) on employer-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with employer-level simulated spending per beneficiary. Each estimate includes employer and year fixed effects. Data on insurance premiums come from the Department of Labor Form 5500 filings. Data on an employer's share of enrollees with a health savings account come from the Internal Revenue Service. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the employer level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



**Table 4:** The Impact of Rising Health Care Prices on Employer Payroll and Employment**Panel A: OLS Estimates**

	All Employers		Non-Health Care Employers		Health Care Employers	
	Log(Payroll) (1)	Log(Workers) (2)	Log(Payroll) (3)	Log(Workers) (4)	Log(Payroll) (5)	Log(Workers) (6)
Log(Spending per Beneficiary)	0.005 (0.004)	0.003 (0.004)	0.005 (0.004)	0.004 (0.004)	0.003 (0.019)	0.001 (0.018)

**Panel B: IV Estimates**

	All Employers		Non-Health Care Employers		Health Care Employers	
	Log(Payroll) (1)	Log(Workers) (2)	Log(Payroll) (3)	Log(Workers) (4)	Log(Payroll) (5)	Log(Workers) (6)
Log(Spending per Beneficiary) <i>(Instrumented using Merger-Driven Price Increases)</i>	-0.362*** (0.130)	-0.356*** (0.129)	-0.373*** (0.134)	-0.402*** (0.133)	-0.256 (0.558)	0.283 (0.551)
Mean Dependent Variable*	12,721,000	297	13,045,000	304	8,242,000	203
Observations	1,403,000	1,403,000	1,308,290	1,308,290	94,710	94,710
Number of Unique Employers	140,300	140,300	130,829	130,829	9,471	9,471
F-Statistic on First Stage	864.776	864.776	813.863	813.863	51.582	51.582

**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of annual employer-level log payroll (Columns (1), (3), (5)) and log worker counts (Columns (2), (4), (6)) on employer-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with employer-level simulated spending per beneficiary. In Columns (1) and (2), we include all employers. In Columns (3) through (6), we include only those employers categorized as not being in the health care industry (Columns (3) and (4)) or being in the health care industry (Columns (5) and (6)), as determined by their reported NAICS code. Each estimate includes employer and year fixed effects. Our labor market data come from the Internal Revenue Service. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the employer level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

\* Rounded to \$1,000 for payroll measures.

**Table 5:** The Impact of Rising Health Care Prices on County-Level Labor Income and Employment

<b>Panel A: OLS Estimates</b>			
	Log(Income per Capita) (1)	Share Unemployed (2)	Log(Income Tax per Capita) (3)
Log(Spending per Beneficiary)	0.018* (0.010)	0.011*** (0.003)	0.019 (0.012)
<b>Panel B: IV Estimates</b>			
	Log(Income per Capita) (1)	Share Unemployed (2)	Log(Income Tax per Capita) (3)
Log(Spending per Beneficiary) <i>(Instrumented Using Merger-Driven Price Increases)</i>	-0.268* (0.149)	0.086** (0.041)	-0.358* (0.188)
Mean Dependent Variable	41,908	0.089	7,009
Observations	17,090	17,090	17,090
Number of Unique Counties	1,709	1,709	1,709
F-Statistic on First Stage	41.960	41.960	41.960

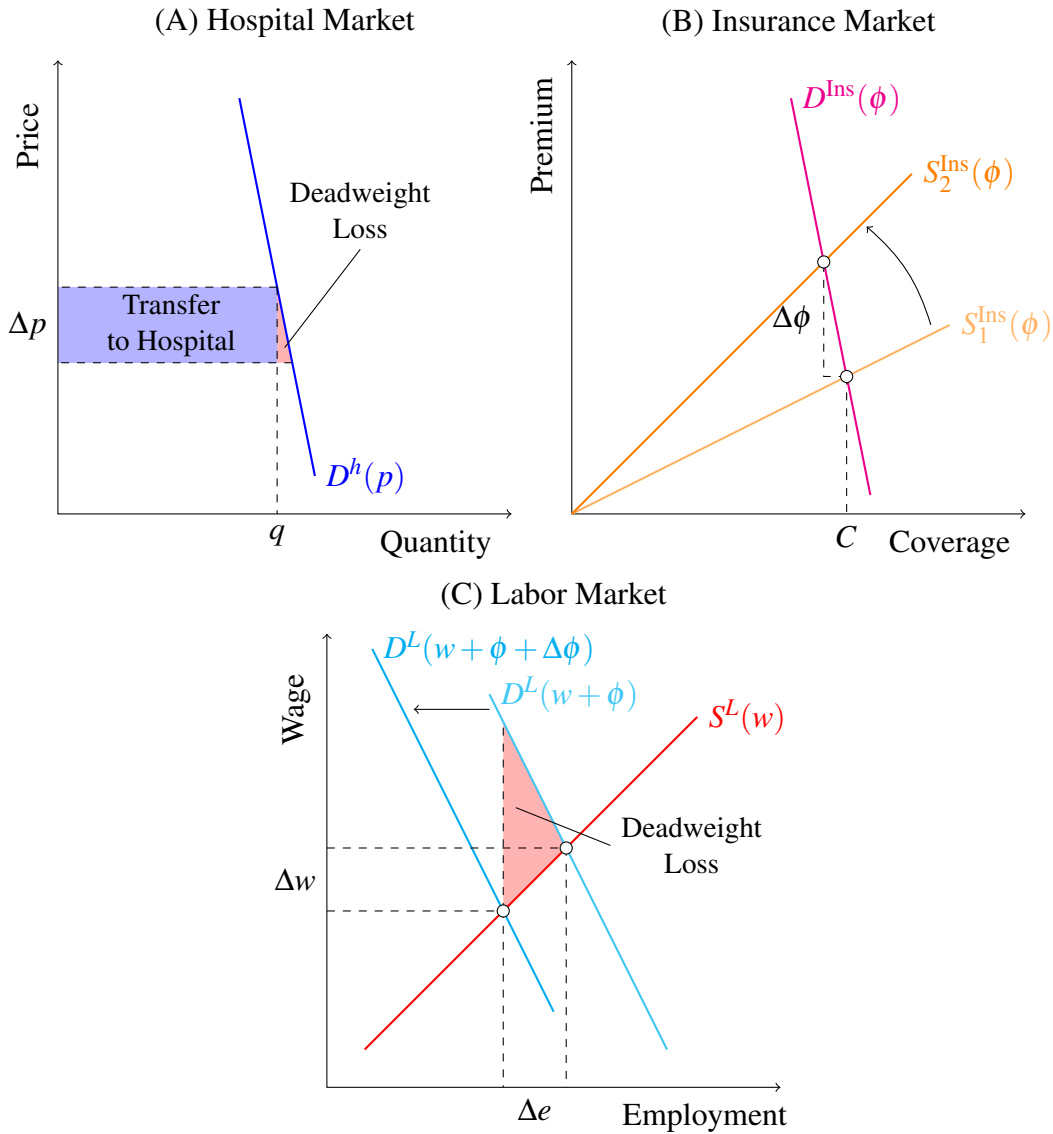
**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of annual county-level log income per capita (Column (1)), share of the population collecting unemployment insurance or earning zero labor income (Column (2)), and log federal income tax receipts per capita (Column (3)) on county-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with county-level simulated spending per beneficiary. Each estimate includes county and year fixed effects. Our labor market and tax revenue data come from the Internal Revenue Service. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 6:** The Impact of Rising Health Care Prices on County-Level Mortality

<b>Panel A: OLS Estimates</b>				
	Deaths from Suicide and Overdoses per 100k Pop. Age 25-64 (1)	Deaths from Suicide and Overdoses per 100k Pop. Age 65 and Up (2)	All Other Deaths per 100k Pop. Age 25-64 (3)	Deaths from Cancer per 100k Pop. Age 25-64 (4)
Log(Spending per Beneficiary)	15.131*** (2.362)	0.185 (0.737)	-4.417 (6.324)	9.059*** (3.162)
<b>Panel B: IV Estimates</b>				
	Deaths from Suicide and Overdoses Age 25-64 (1)	Deaths from Suicide and Overdoses Age 65 and Up (2)	All Deaths Excluding Suicides and Overdoses Age 25-64 (3)	Deaths from Cancer Age 25-64 (4)
Log(Spending per Beneficiary) <i>(Instrumented Using Merger-Driven Price Increases)</i>	61.873** (29.744)	-10.428 (9.589)	-39.985 (73.574)	6.362 (34.464)
Mean Dependent Variable	23.467	3.402	213.858	65.207
Observations	17,090	17,090	17,090	17,090
F-Statistic on First Stage	41.960	41.960	41.960	41.960

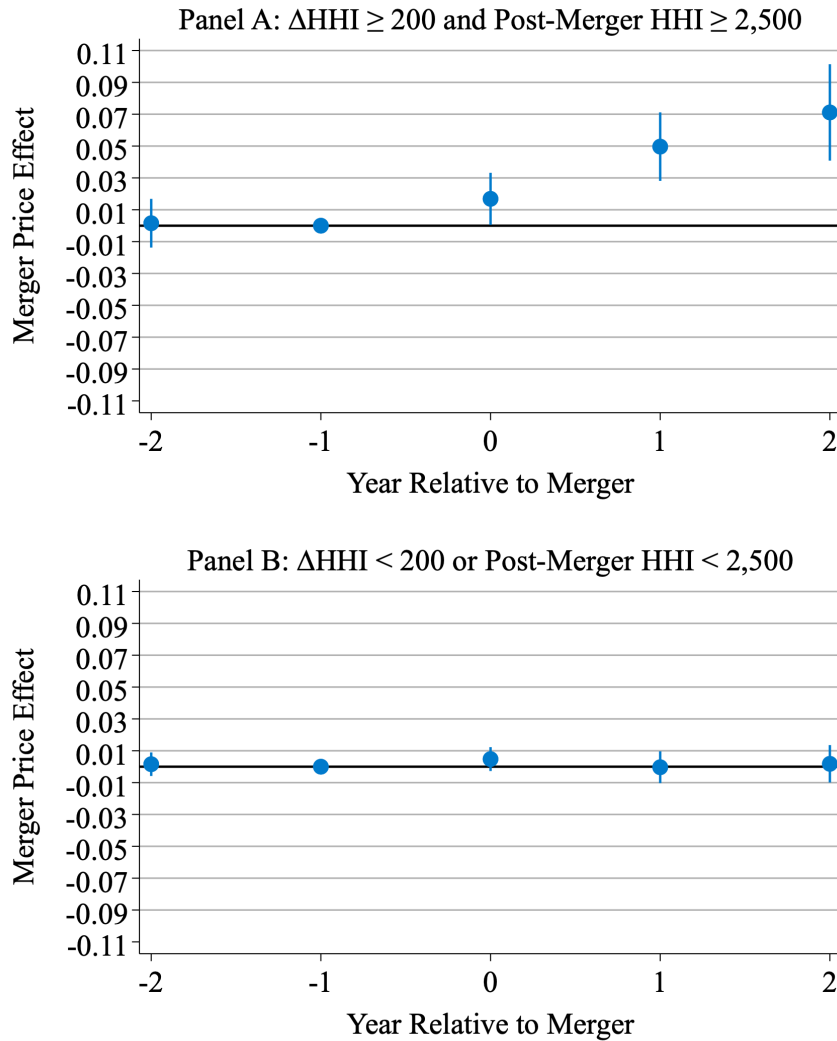
**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of county-level deaths per 100,000 population on county-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with county-level simulated spending per beneficiary. Column (1) presents our primary estimates where the outcome is deaths from suicide and overdose for those aged 25-64. Columns (2) through (4) present three placebo tests: deaths from suicide and overdose for those aged 65 and up (2), deaths from any cause *other than* suicide and overdose for those aged 25-64 (3), and deaths from cancer for those aged 25-64 (4). Each estimate includes county and year fixed effects. Mortality data come from the Centers for Disease Control and Prevention's Restricted Mortality Database. Standard errors are in parentheses and are clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Figure 1:** The Impact of Hospital Mergers in Hospital Markets, Insurance Markets, and Labor Markets



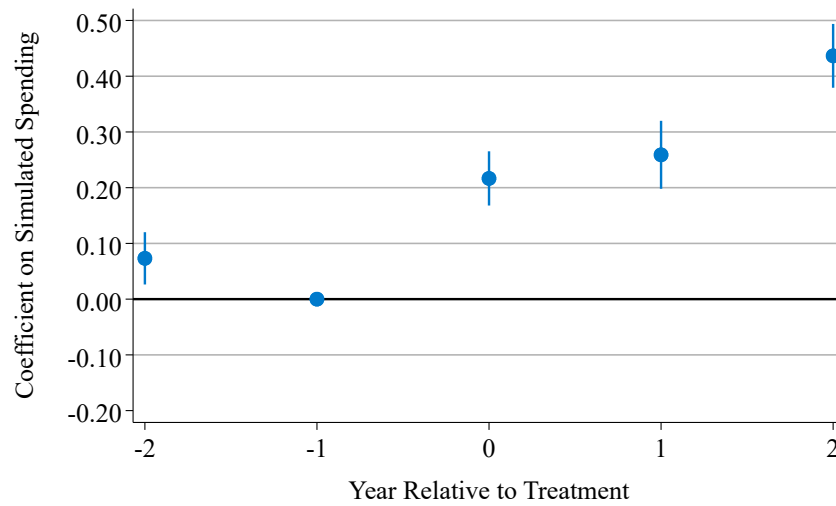
**Notes:** This illustrates our theory of how rising health care prices results rising insurance premiums and downstream changes in labor market outcomes. In Panel A, we highlight the fact that, after a merger (or other source of rent-seeking), prices rise. This generates deadweight loss (the red triangle), and a transfer from payers to the merging hospital (the blue rectangle). In Panel B, we highlight the effect on the market for health insurance coverage. The insurance supply curve rotates around the origin, from  $S_1^{Ins}(\phi)$  to  $S_2^{Ins}(\phi)$ . In Panel C, we highlight the effects on the market for labor. The insurance premium increase shifts labor demand down by  $\Delta\phi$ , the change in premiums. This results in a fall in equilibrium wages and employment,  $\Delta w$  and  $\Delta L$ . It also results in deadweight loss, given by the red triangle.

**Figure 2:** The Impact of Mergers on Hospital Prices by FTC Reporting Guidelines



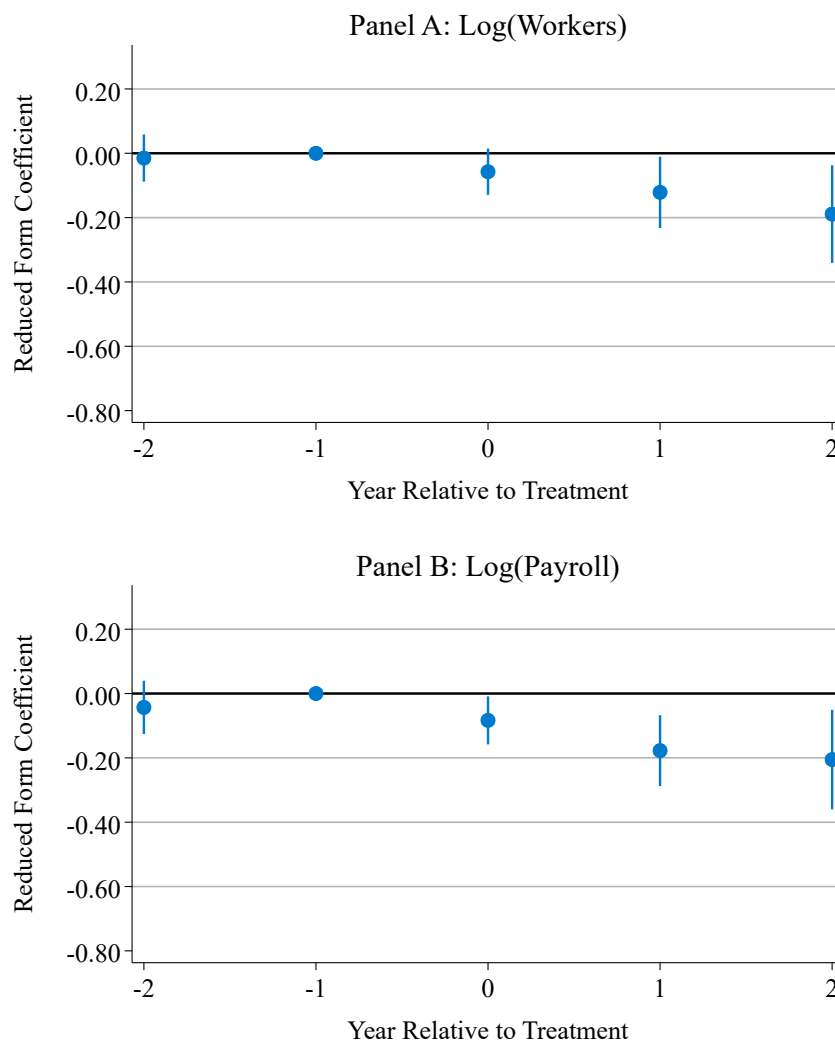
**Notes:** This figure presents difference-in-difference estimates of the effect of 2010 to 2015 hospital mergers on inpatient hospital prices. We estimate a pooled version of Equation (2) that estimates the average price increase within a group of mergers. We do so for the following groups: mergers that generated a  $\Delta HHI \geq 200$  and post-merger  $HHI \geq 2,500$  (Panel A) and mergers that generated a  $\Delta HHI < 200$  or post-merger  $HHI < 2,500$  (Panel B). Hospital prices are a weighted average of inpatient and outpatient prices. The weights are constructed as the average share of inpatient and outpatient revenue for the treated hospital in 2008 and 2009, the two years prior to the first merger in our sample. Each dot represents a point estimate, and the vertical line displays the corresponding 95% confidence interval. Standard errors are clustered at the hospital level. Hospital pricing data come from the Health Care Cost Institute. The average difference-in-differences estimates comparing the two years prior to the merger with two years post for mergers in Panel A is 0.059 (0.012) and in Panel B is 0.000 (0.005).

**Figure 3:** Event Study-Style Estimates of First Stage: Regressing Employer Spending on Simulated Employer Spending



**Notes:** This figure presents event study-style coefficient estimates from our first-stage regression, given by Equation (7), in which we regress employer-level annual spending per beneficiary on leads and lags of employer-level simulated spending per beneficiary. The dots are point estimates and the vertical lines are 95% confidence intervals.

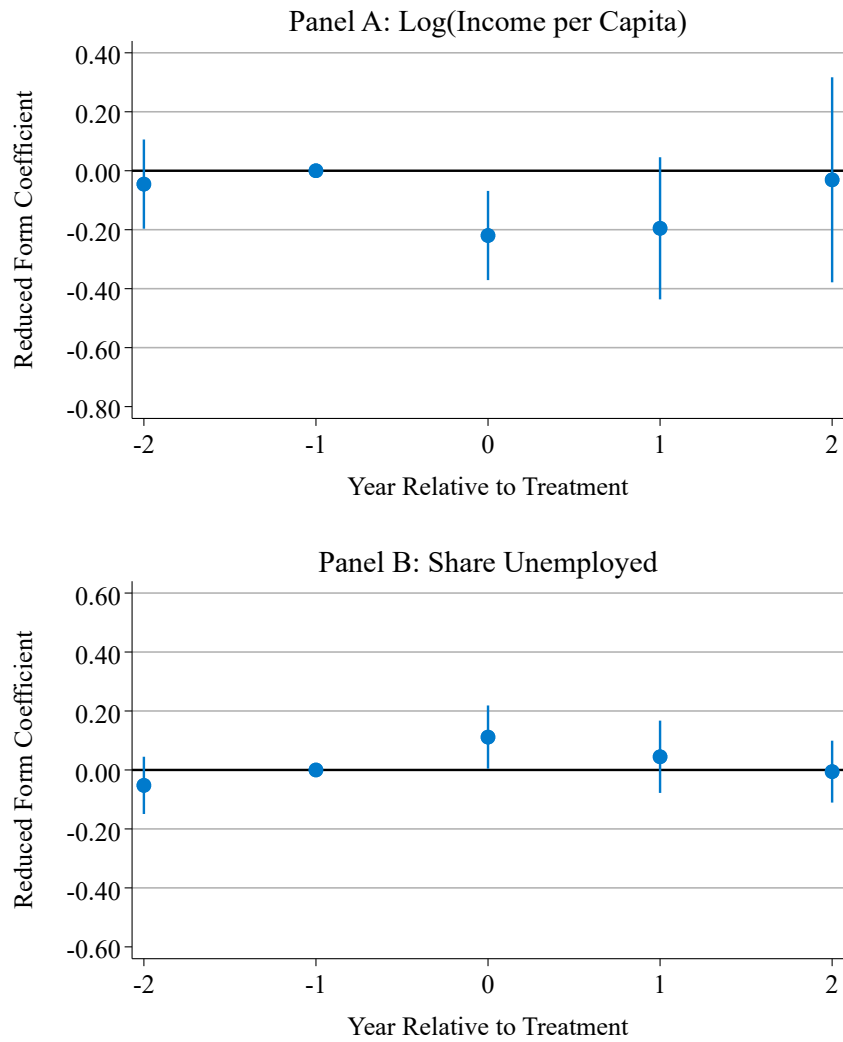
**Figure 4:** Event Study Estimates of the Impact of Rising Health Care Prices on Employer Payroll and Worker Count at Non-Health Care Employers



**Notes:** This figure presents event study-style estimates of the effect on rising health care prices on employer-level labor market outcomes, given by Equation (7), in which we regress employer-level labor market outcomes on leads and lags of employer-level simulated spending per beneficiary. The dots are point estimates and the vertical lines are 95% confidence intervals.

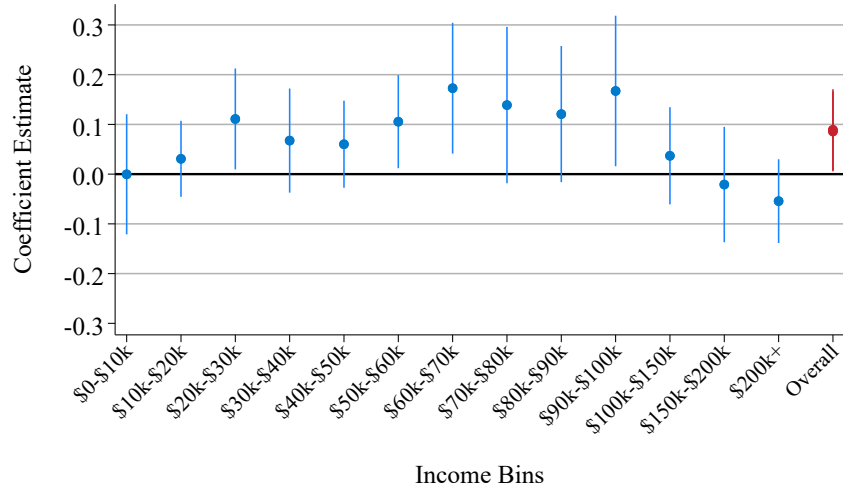


**Figure 5:** Event Study Estimates of the Impact of Rising Health Care Prices on County-Level Income per Capita and Employment



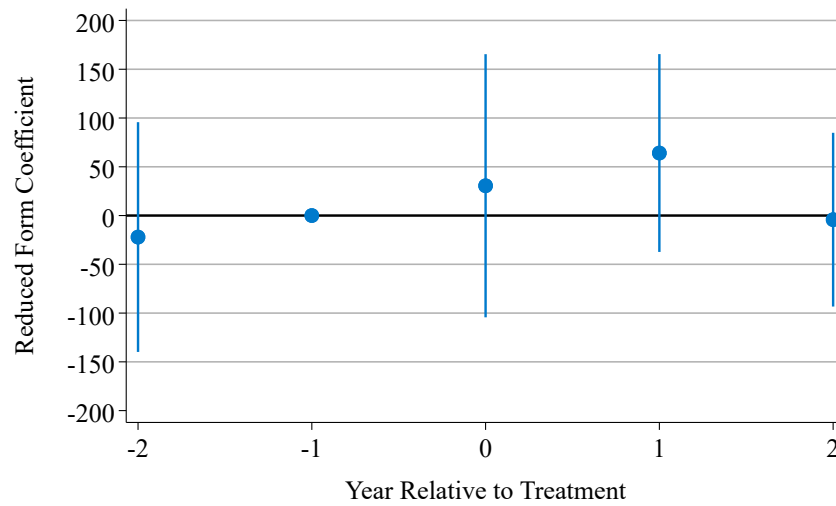
**Notes:** This figure presents event study-style estimates of the effect on rising health care prices on county-level labor market outcomes, given by Equation (7), in which we regress county-level labor market outcomes on leads and lags of county-level simulated spending per beneficiary. The dots are point estimates and the vertical lines are 95% confidence intervals.

**Figure 6:** The Impact of Rising Health Care Prices on Changes in Unemployment across the Income Distribution



**Notes:** This figure shows estimates of Equation (12) on the share of the population collecting unemployment insurance. Effects are estimated separately for individuals based on bins of their income measured in the prior year. The dots represent point estimates and vertical lines are 95% confidence intervals.

**Figure 7:** The Impact of Rising Health Care Prices on County-Level Deaths from Suicide and Overdose per 100,000 People



**Notes:** This figure presents event study-style estimates of the effect on rising health care prices on county-level mortality outcomes, given by Equation (7), in which we regress county-level mortality outcomes on leads and lags of county-level simulated spending per beneficiary. The dots are point estimates and the vertical lines are 95% confidence intervals.

## ONLINE APPENDICES

## A Measuring Prices in the HCCI Data

Following [Cooper et al. \(2019a\)](#), we take the approach of constructing a sample of patient visits, or “cases.” For each case, we observe the negotiated transaction price. We then use clinical codes indicating the procedure performed during a case and the severity of a patient’s illness, along with demographic characteristics of the patient to adjust for the mix of services provided by each hospital. Specifically, we estimate a regression of the form

$$\ln(p_{idht}) = \alpha_{ht} + \beta X_i + \pi_{dt} + \varepsilon_{idht}. \quad (13)$$

$X_i$  includes controls for each patient’s age and gender.  $\pi_{dt}$  is a year-specific fixed-effect, which flexibly adjusts for the complexity of a patients’ condition and treatment by year.

We then use the estimates from Equation (13) to generate predicted values for each hospital-year, holding fixed the coefficients accounting for patient characteristics and clinical severity at the average levels in the data:

$$p_{ht}^{INDEX} = \hat{\alpha}_{ht} + \hat{\beta} \bar{X} + \hat{\pi}_{dt} \bar{dt}. \quad (14)$$

We generate price indices for both inpatient and outpatient samples separately. For the inpatient sample, we adhere closely to the methodology described in [Cooper et al. \(2019a\)](#), limiting the sample to individuals who are between 18 and 65 years old. We also limit our sample to individuals with a valid Diagnostic-Related Group (DRG) code, which we use to define the  $\pi_{dt}$  fixed effect.

For the outpatient sample, we apply similar restrictions, limiting our sample to patients between 18 and 65 years old. To ensure the prices we measure are complete — not payments negotiated as a bundle of services — we limit our analysis to patient days in which there is only one outpatient visit. We also limit our analysis to patient days in which there is only one CPT procedure code that maps to a valid Medicare APC payment rate. Although this restriction limits the data to approximately 30% of patient days, we view this sample as one that provides a clean distinction between price and quantity. We use APC codes to define the  $\pi_{dt}$  fixed effects for the outpatient price index.

For both samples, we restrict our analysis to the subset of hospitals that match to an acute general surgical hospital in the roster of hospitals we derive from the American Hospital Association’s Annual Survey Data.

## B Measuring Merger Activity and the Price Effects of Mergers

This appendix describes how we construct the panel of mergers we use, as well as how we measure potential competitive effects for mergers. Much of this material is reproduced from [Brot et al. \(forthcoming\)](#).

### B.1 Merger Panel

The primary data we use to measure merger activity come from the American Hospital Association’s (AHA’s) Annual Survey of Hospitals. These data contain biographical information on the near universe of general acute care hospitals in the US, including a measure of system ownership. Our final roster contains 4,846 hospitals in the continental US. We track mergers in our hospital panel using changes to the system identifier provided by the AHA for 2002 to 2020. We leverage several additional data sources — the FactSet Research Systems database, the Irving Levin Associates’ Health Care Services Acquisition Reports, and Securities Data Company Platinum — to verify the existence and timing of mergers.<sup>42</sup> We observe 1,164 mergers of general acute care hospitals from 2002-2020. Of these, we use 304 in our analysis, that (1) are consummated between 2010 and 2015; (2) have at least two parties within 50 miles of each other and (3) have at least one case in the HCCI data during both the two years before the merger and the two years after. Appendix Figure [A.1](#) includes a map of the mergers we focus on in this analysis.

### B.2 Matching Merging and Non-Merging Hospitals

In order to identify the treatment effect of mergers on hospital prices, we need a control group of non-merging hospitals to estimate counterfactual trends in prices for merging hospitals. Our control group is composed of hospitals that did not experience a merger between 2008 and  $t + 2$ , where  $t$  is the year that the focal merging hospital merged.

To ensure that our control hospitals represent plausible counterfactual trends, we use propensity scores to match comparison hospitals to merging hospitals on pre-merger observable characteristics. We estimate a probit regression of the form:

$$\mathbb{P}\{h \text{ merged}\} = X'\beta + \varepsilon_h \quad (15)$$

where  $X$  contains a vector of hospital characteristics — drawn from the AHA data and measured in the year before our first merger (2009) — that may meaningfully determine price trends at hospitals: total number of hospital beds; total inpatient admissions; full time equivalents; number of unique technologies; share of Medicare patients; share of Medicaid patients; whether the hospital is a teaching hospital; a non-profit hospital; or a government hospital; the distance to the hospital’s nearest competitor; the distance to the hospital’s nearest hospital in its system or not; and whether the hospital is independent or part of a system.  $X$  also includes measures of local area characteristics around the hospital. First, we include the local HHI, measured as described in Appendix [B.3](#). Second, we include the share of the hospital’s county covered by private insurance, which we construct using data from the Census’s Small Area Health Insurance Estimates (SAHIE).

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<sup>42</sup>For more information on how we use these datasets to track hospital ownership, see Appendix D of [Cooper et al. \(2019a\)](#).

Finally, we include the share of the county insured by HCCI payors specifically, using data from HCCI to form the numerator and data from SAHIE to form the denominator.

We then use this model to construct propensity scores for each hospital. For each merging hospital, we find its 25 nearest neighbors (in terms of propensity scores) from the set of potential control hospitals. We also impose a caliper restriction so that the propensity scores of matched controls must be within 20% of a standard deviation from the treated merging hospital, even if this requires that the control group contain fewer than 25 hospitals.

### B.3 Measuring the Herfindahl-Hirschman Index

To measure the Herfindahl-Hirschman Index (HHI), assume that a market  $M$  includes many hospital systems  $S \in \mathcal{S}(M)$ , where  $\mathcal{S}(M)$  is the set of systems in  $M$ . Each system is defined as a set of one or more hospitals  $h$ , which have a collective owner. Formally,

$$HHI_M = 10,000 \times \sum_{S \in \mathcal{S}(M)} \left( \sum_{h \in S} s_{hM} \right)^2$$

where  $s_{hM}$  is  $h$ 's market share within  $M$ . A monopoly market has an HHI of 10,000; if instead there are many small independent hospitals, the HHI will be closer to 0.

Measuring HHIs requires us to define relevant geographic markets and measure hospitals' market shares. We assume that a hospital's relevant market includes every hospital within a 30-minute drive time from their facility. We measure a hospital's market share as its share of inpatient hospital beds. We use hospital beds rather than activity to define concentration because, unlike hospital activity, changes in bed volume in the short run are unlikely to be highly correlated with changes in hospital quality or prices. We measure the change in HHI for a hospital  $h$  due to merger  $e$  as the difference between the HHI in its market in the year before the merger and a computed counterfactual where we change system membership to reflect the merger, holding bed counts and the system membership of non-participating hospitals fixed.

### B.4 Measuring Willingness-to-Pay

Theory predicts that the extent to which mergers raise prices depends on the extent to which merging hospitals are good substitutes for one another, and whether their patients are unwilling to go to another hospital. As pointed out by [Capps et al. \(2003\)](#) and [Gowrisankaran et al. \(2015\)](#), since demand for hospitals is very inelastic, a standard model of Nash-Bertrand pricing would predict extremely high prices following mergers and suggest mergers could raise prices by implausibly large amounts. Instead, these prior studies have developed a theory of price-setting in which prices are bilaterally negotiated between hospital systems and insurers, which bargain on behalf of their enrollees. In these models, prices are not determined by patients' price elasticities but are instead driven by what is effectively the insurer's elasticity — in terms of how much insurers can subsequently raise premiums if the hospital system is included in the insurer's preferred network of hospitals. In this way, hospital prices are determined by patients' *ex ante* willingness to pay for the option to go to the hospital when buying an insurance plan ([Ho and Lee, 2017](#)).



### B.4.1 Hospital-Insurer Bargaining and $\Delta WTP$

Capps et al. (2003) model the *ex post* utility of patient  $i$  at hospital  $h$  as  $U_{ih} = U(X_{ih}) + \varepsilon_{ih}$ , where  $U(\cdot)$  denotes expected utility at the hospital and  $\varepsilon_{ih}$  represents idiosyncratic patient preferences at specific hospitals with  $\varepsilon$  distributed i.i.d. standard Gumbel.  $X_{ih}$  contains patient and hospital characteristics that determine preferences for a given hospital, including the patient's specific health care needs as well as the distance between them and the hospital.

If a patient faces a hospital network  $\mathcal{N}$  that limits what hospitals she has access to, then the patient's *ex ante* expected utility of access to a network  $\mathcal{N}$  is

$$\begin{aligned} EU_i(\mathcal{N}) &= E[\max_{j \in \mathcal{N}} U_{ij}] \\ &= \ln \left( \sum_{j \in \mathcal{N}} \exp(U_{ij}) \right) \end{aligned}$$

Moreover, say that a hospital  $h$  is dropped from the network. Capps et al. (2003) show that the change in expected utility as a result of this network change is:

$$\begin{aligned} \Delta EU_{ih} &= EU_i(\mathcal{N}) - EU_i(\mathcal{N} \setminus h) \\ &= \ln \left( \sum_{j \in \mathcal{N}} \exp(U_{ij}) \right) - \ln \left( \sum_{j \in \mathcal{N} \setminus h} \exp(U_{ij}) \right) \\ &= \ln \left( \frac{1}{1 - s_{ih}} \right) \end{aligned}$$

where  $s_{ih}$  is that hospital's expected market share from patient  $i$  under network  $\mathcal{N}$ . If consumers are always indifferent between receiving a 1-point increase in  $EU$  and a  $\$ \gamma_i$  payment, then we can describe patients' *ex ante* "willingness-to-pay" for hospital  $h$  as  $W_{ih} = \gamma_i \Delta EU_{ih}$ . We integrate over the distribution of consumers  $F_i$  to calculate market-level willingness to pay as

$W_h = \int_i \gamma_i \ln \left( \frac{1}{1 - s_{ih}} \right) dF_i$ . Where  $W_h$  represents the amount that the average consumer is willing to pay for access to hospital  $h$ . Both Capps et al. (2003) and Gowrisankaran et al. (2015) show that, in standard models of bargaining (either pure Nash or Nash-in-Nash), the price for  $h$ 's services that will be negotiated jointly by the hospital and insurer is proportional to  $W_h$ .

The above notation assumes that all hospitals are independent. If, instead, hospitals are part of some system  $S$ , the hospitals will bargain jointly. That is, prices will be determined by the willingness to pay for the *entire system*,  $W_S = \int_i \gamma_i \ln \left( \frac{1}{1 - s_{iS}} \right) dF_i$ , with  $s_{iS} = \sum_{j \in S} s_{ij}$ . Systems are able to exert greater leverage than individual hospitals because they can threaten to hold out the entire system from the insurer's network if a deal on prices fails to be realized.<sup>43</sup>

We model the case of a merger ( $m$ ) between two hospitals  $h$  and  $h'$ .<sup>44</sup> The impact of the merger

<sup>43</sup>In practice, we consider the relevant bargaining entity to be the only the hospitals in a system within a given HRR, to avoid diffusing local changes in bargaining leverage over large acquiring systems.

<sup>44</sup>This is without a loss of generality and can be replaced with systems.

on the bargaining leverage of the two hospitals is the difference between the willingness to pay of the merged system and the sum of the willingness to pay for  $h$  and  $h'$  individually. Due to a lack of data on individual insurance take-up, we follow [Capps et al. \(2003\)](#) and assume that  $\gamma_i = \gamma$  for all patients. The percent change in willingness to pay due to the merger is:

$$\Delta WTP_m = \frac{\int_i \left[ \ln \left( \frac{1}{1-(s_{ih}+s_{ih'})} \right) - \left( \ln \left( \frac{1}{1-s_{ih}} \right) + \ln \left( \frac{1}{1-s_{ih'}} \right) \right) \right] dF_i}{\int_i \left[ \ln \left( \frac{1}{1-s_{ih}} \right) + \ln \left( \frac{1}{1-s_{ih'}} \right) \right] dF_i}$$

where  $\gamma_i$  drops out of the equation under the assumption of homogeneity. Importantly, we focus on the joint complementarities created by a merger, excluding the effect of pure scale increases for each participating hospital. In unpublished results, we found that, particularly for mergers in which a single independent hospital was acquired by a large chain, allowing the scale effects to enter into the change in WTP predicted implausibly (and incorrectly) large post-merger price increases.

Under these assumptions, the potential price changes due to a merger should be proportional to  $\Delta WTP_m$ .

#### B.4.2 Estimating Demand for Hospitals

Measuring  $\Delta WTP_m$  requires us to estimate substitution patterns in the relevant market. [Capps et al. \(2003\)](#) underscore the importance of patient heterogeneity in this calculation — heart attack patients may care much more about hospital closeness than patients undergoing elective surgeries.

We therefore take the semiparametric approach to demand estimation developed by [Raval et al. \(2017\)](#). That is, we estimate  $U(X_{ih})$  by assuming we can partition patients into groups  $g \in G$  based on their characteristics, such that

$$U_{ih} = U_{g(i)h} = \delta_{g(i)h} + \varepsilon_{ih}$$

Patients within the same groups are assumed to have the same *ex ante* expected utility for any particular hospital, but patients across groups may have different preferences in an unrestricted way. It is then true that, for patients within the same group, expected market shares at each hospital are equal within groups, such that:

$$s_{ih} = s_{g(i)h} = \frac{\exp(\delta_{g(i)h})}{\sum_{j \in \mathcal{N}} \exp(\delta_{g(i)j})}$$

Using this procedure, a valid partition of patients allows us to use the observed group-level market shares as an equivalent measure to individual-specific choice probabilities, and therefore patient utility for each hospital-by-group pair.

We calculate group-specific market shares for each hospital using every inpatient hospitalization for Medicare patients (in the group) during our relevant time period.<sup>45</sup> We exclude any hospitalization in which a patient attended a hospital more than 100 miles from their home. The [Raval et al. \(2017\)](#) approach provides an algorithm that partitions patients into increasingly small groups until the resulting groups are no smaller than  $S_{min}$ . This minimum group size parameter is

<sup>45</sup>That is, we assume that there is no relevant extensive margin substitution to no hospitalization as a result of changes in market structure.

set to balance a bias-variance trade-off: allowing for smaller groups reduces bias by allowing us to capture consumers' heterogeneous preferences for hospitals. However, smaller bins also increase variance by estimating preferences over smaller samples, where market shares may be estimated with error.

The algorithm proceeds as follows:

**Step 1:** The econometrician first establishes a set of discrete patient characteristics, ordered by "importance." Specifically, we group according to the following characteristics:

1. Patient home county
2. Patient home 5-digit zip code
3. Major Diagnostic Category of the patient's illness
4. Binary indicator for whether the patient's illness was such that the hospitalization was likely to be discretionary (rather than an emergency)
5. Binary indicator for whether the patient's illness was likely to require a surgical treatment (rather than a purely medical treatment)
6. Quartiles of the weight placed on the Diagnosis-Related Group for the patient's illness<sup>46</sup>
7. The Diagnosis-Related Group for a patient's illness (as measured by their primary diagnosis code)
8. Patient age, in 10-year buckets
9. Patient sex

**Step 2:** We partition patients into groups based on their unique values for every characteristic (e.g., if the characteristics are gender, race, and county, there will be one group for black female patients in New York County, another group for white male patients in Cook County, etc.).

**Step 3:** We assign groups based on any partitions from Step 2, as long as the partition has a size above  $S_{min}$ . Any patients in partitions with size below  $S_{min}$  are left ungrouped.

**Step 4:** We then disregard the lowest-priority characteristic.

**Step 5:** We repeat Steps 2-4 until we reach a single characteristic (the patient's home county).

The partitions this algorithm produces vary in granularity to allow for more heterogeneity among patients characteristics when larger sample sizes are available. For example, denser counties will have more groups, subdivided by illness and patient demographics. By contrast all patients will be grouped together in smaller counties where data are sparser.

We run the algorithm separately for each year of mergers in our data. To ensure that we capture finer partition of groups — and therefore flexible substitution patterns — we pool data from the two years prior for each year of mergers. We then compute patient choice probabilities for each hospital ( $\hat{s}_{gh}$ ) for each group. To compute expected proportional changes in price, we compute the percent change in willingness-to-pay,

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<sup>46</sup>This DRG weight is used to determine hospital payments under Medicare's reimbursement system.

$$\Delta WTP_m = \frac{\sum_g P_g \left[ \ln \left( \frac{1}{1 - (\hat{s}_{gh} + \hat{s}_{gh'})} \right) - \left( \ln \left( \frac{1}{1 - \hat{s}_{gh}} \right) + \ln \left( \frac{1}{1 - \hat{s}_{gh'}} \right) \right) \right]}{\sum_g P_g \left[ \ln \left( \frac{1}{1 - \hat{s}_{gh}} \right) + \ln \left( \frac{1}{1 - \hat{s}_{gh'}} \right) \right]} \quad (16)$$

where  $P_g$  is the share of patients within group  $g$ . Our primary specification uses a minimum group size of 50, resulting in 27,525 groups sized between 50 and 1,449.

## C Measuring Premiums in the Form 5500

### C.1 Sample Construction

Form 5500 is a regulatory filing required of all employers that offer a benefit plan to at least 100 employees. Although the data provide a rich source of employer-level data on premiums, there are many idiosyncrasies in the filing process that obfuscate true levels of premiums. Following [Craig \(2022\)](#), we implement a series of data restrictions and cleaning steps described below.

For fully insured benefits, employers are required to file a Schedule A form, which reports enrollment, premiums, plan type, and insurer for the plan. We use these data to construct an employer-year measure of average premiums per covered life. Self-insured employers are required to submit separate forms related to the administration of their plans. These forms pertain primarily to the financial details of the trust that is established to maintain plan funding. Self-insured employers are inconsistent in the degree to which their plans are funded through such trusts or the employers' general assets, making premium measurement for these employers intractable. However, levels and trends for fully and self-insured premiums are broadly similar ([Craig, 2022](#)).

We construct a panel of employers based on a combination of 5500 base forms and Schedule A forms. We exclude groups reporting on behalf of multi-employer plans, employers that operate plans in multiple states, and voluntary filers – i.e., employers that file despite the fact that their plans fall below filing thresholds (100 employees) in all years of the data.

### C.2 Premium Measurement

We measure each plan's total premiums directly from the Schedule A form. However, employers can change in both absolute size and health plan enrollment from year to year, and a "per-person" measure of premiums lends itself more closely to comparisons with the scale of our health spending measure from the first-stage. We, therefore, standardize premiums using the total number of plan participants to compute average premiums per covered life per year. This ensures that the premium fluctuations we observe are related to changes in price, rather than fluctuations in employer size or plan participation.

The best measure of plan coverage comes from Line 1e of the Schedule A, which requires employers to report the "approximate number of persons covered at end of policy or contract year." However, it is clear from the data and documentation that employers are inconsistent in whether they include dependents in this field. From a preparer's manual for the 5500, [Fisher and Andersen \(2019\)](#) note that

*"The DOL says dependents should be included in the count reported on Line 1e, although whether dependents are include or excluded in the data provided by an insurance company varies depending on the carrier's own internal procedures. Generally, preparers simply use the information provided by the insurance company without further analysis. Dependents are not counted for any other purposes on the Form 5500 or its schedules."*

Line 6a of the 5500 base form asks employers to report the number of active plan participants (employees) at the beginning and end of the year. Although Line 6a consistently excludes dependents, it does not pertain to a single insurance policy, whereas the Schedule A Line 1e

measure does. Instead, Line 6a typically represents the super set of enrolled employees across a number of benefit plans (e.g., life insurance, dental insurance, accidental death and dismemberment). Following [Craig \(2022\)](#), we standardize reporting across employer-years by identifying observations in which Line 1e reflects employee participation, rather than total plan coverage. We then adjust the coverage measure from Line 1e to match the average ratio of health plan coverage to overall benefit participation in adjacent years.

The approach from [Craig \(2022\)](#) requires manually reviewing rosters using a range of information in the other 5500 filings an employer submits including Schedule A filings for non-health plan contracts to adjudicate whether Line 1e reflects dependents or not. In order to scale this process, we manually classify Line 1e observations for four states: Massachusetts, Montana, North Carolina, and Texas. We then use these states to train a random forest algorithm to perform the remaining assignments.

The random forest algorithm classifies observations as to whether or not the coverage figure from Line 1e of the Schedule A includes dependents. In order to replicate the information set used to perform the manual assignments, we include the following measures as potential predictors:

- $r$ , which is defined as the ratio of Schedule A coverage (Line 1e) to base form plan participation (Line 6a)
- Whether  $r$  is large enough to suggest clear dependent reporting ( $r > 1.1$ )
- The change in  $r$  from the previous year of an employers' reporting
- The standard deviation of  $r$  within employer
- The value and first difference in Line 6a reporting
- The largest Line 1e value reported among non-health plans in the year
- The measure and first difference of total premiums per person implied by naive use of Line 1e

The random forest prediction summarizes the average prediction over a large number of decision trees. At each node within a given decision tree, the sample is split to optimally categorize each side as high or low probability of including dependent coverage, resulting two "leaves." This decision is made by evaluating possible splits among a randomly selected subset of the potential predictors. At the next node, a new variable split is defined and this process repeats until the groups of observations within each "leaf" reach a minimum threshold. We use 10-fold cross validation to choose the hyper-parameters that minimize our classification error: we choose among eight variables at each node, allow trees to deepen until a minimum leaf size of 40, and average over 200 individual decision trees.

We then use these predictions to adjust observations classified as reporting covered employees to reflect dependents as described above. Finally, we deal with remaining classification error by implementing trims at the 5th and 95th percentiles of the premium distribution.

## D Decomposing Instrumental Variation

In Section 5.5, we are interested in measuring the extent to which the three components of our primary instrument—merger timing, merger price effects, and employer-hospital exposure—drive

the identifying variation that we use to estimate the effects of rising health care prices, so that we can focus our tests of identification assumptions on the right components. Specifically, we are interested in the following question: If we prevented one of these components from providing identifying variation, what would we be left with? This is especially important since some of the components of our instrument might face individual exogeneity issues. For instance, an employer may be more or less exposed to merging hospitals based on where they are; in turn, locations with differing exposure may be on different economic trends.

To diagnose this, we borrow a method from [Borusyak and Hull \(2023\)](#). They show that one can remove the identifying variation from one or more potentially endogenous components (while preserving the ability of those components to appropriately scale the instrument) by explicitly specifying the data-generating process of the plausibly exogenous components. An econometrician can then take the expected value of the instrument given the potentially endogenous components (averaged over values of the exogenous components). By subtracting this “expected instrument” from the primary instrument, the remainder is purged of any identifying variation from the potentially endogenous components while retaining that of the plausibly exogenous components. This method also allows us to check our estimates for robustness to using these alternative instruments if one is worried about exogeneity violations.

We model two components: The post-merger price increases, and the merger timing. It is much more difficult to think about an explicit model of the process that generates variation in employer exposure, which involves employer location choice. Moreover, employer exposure variation likely has the greatest threat to exogeneity, as described above. We therefore always allow this variation to be purged in our recentered instruments. When we allow merger timing to be exogenous, we assume that any merger we observe could have potentially occurred in any year between 2010 and 2015 with uniform probability  $\frac{1}{6}$ . When we allow post-merger price effects to be exogenous, we assume that they are drawn randomly from the empirical distribution.

In Columns 2 through 4 of Appendix Table [A.3](#) and Appendix Table [A.4](#), we plot versions of our first stage with these recentered instruments. In Column 2, we allow price effects and timing to be modeled as exogenous. In Column 3, we allow *only* price effects to be modeled as exogenous. In Column 4, we allow *only* timing to be modeled as exogenous (i.e., the specifications in 4 and 3 are non-nested). We see that excluding employer exposure (going from column 1 to 2 in Appendix Table [A.3](#)) reduces the instrumental strength somewhat (moving the F-statistic from 864 to 567) and reduces the magnitude of the first-stage coefficient slightly. Removing the variation from timing as well (moving from column 2 to 3) does very little to either, implying that timing provides little instrumental variation. Indeed, when we *only* use variation from timing (column 4), our instrument is significantly weaker and has a wrong-signed first-stage coefficient. These descriptions are true for both the employer-level and county-level specifications.

We conclude from this exercise that the cross-merger variation is the primary source of variation driving our instrument. The timing provides very little variation; employer exposure provides a large amount of variation, but is not essential given the price effect variation, and is more troublesome if we are worried about violations of the exclusion restriction.



## **E Additional Tables and Figures**

**Table A.1:** Comparison of Analytic Sample of Employers to Universe of Employers - 2008-2017

	Overall Analytic Sample	Non-HC Analytic Sample	HC Analytic Sample	Box DD Analytic Sample	5500 Analytic Sample
	Mean (1)	Mean (2)	Mean (3)	Mean (4)	Mean (5)
Health Spending per Beneficiary	4,099	4,100	4,079	4,261	
Share of Employees with a Health Savings Account	0.038	0.039	0.032	0.054	
Employer Total Payroll*	12,721,000	13,045,000	8,242,000	35,631,000	
Employer Count of Workers	297	304	203	784	
Employer Average Wages per Worker	41,339	41,463	39,624	45,184	
Share of Employees with Premiums Premiums from 5500 Data				0.511	5,036
Observations	140,300	130,829	9,471	39,341	3,970

**Notes:** This table presents descriptive statistics for our sample of employers with the various additional sample restrictions we add to cohorts of firms for analysis, based on data from 2008 to 2017. Data on employer payroll, employer counts of workers, employer wages, the share of employees with premiums, and the share of employees with a health savings account come from the Internal Revenue Service (IRS). Data on health spending per beneficiary come from the Health Care Cost Institute (HCCI). Data on insurance premiums comes from the Department of Labor’s 5500 forms.

\* Rounded to \$1,000 for privacy protection.

**Table A.2:** Comparison of Analytic Sample of Counties to Universe of Counties - 2008-2017

	All Counties	Analytic Sample
	Mean (1)	Mean (2)
Income Per Capita	38,378	41,908
Share with Unemployment Insurance	0.033	0.036
Share with Zero Income	0.059	0.054
Share Unemployed	0.092	0.089
Unemployment Insurance Payments per Capita	396	482
Share Self-Employed	0.126	0.110
Share Moving Annually	0.067	0.066
Income Tax Withholdings per Capita	6,054	7,009
All Deaths per 100k	241	237
Deaths from Suicides and Overdose per 100k	21	23
Deaths from Cancer per 100k	66	65
Observations	3,182	1,709

**Notes:** This table compares our analytic sample of counties with the universe of counties in the US using data from 2008 to 2017. Our analytic sample captures approximately 96% of the US population. Data on income per capita, the share with unemployment insurance, unemployment insurance payments per capita, the share of the population self-employed, the share of the population moving out of the county annually, and income tax withholdings per capita come from Internal Revenue Service returns. Income per capita is measured as the sum of wage (W-2) income and self-employment (Schedule SE) income. We define share unemployed as share of individuals with either positive unemployment insurance receipts and/or with zero income in the year. The deaths per 100,000 people measures are from the Center for Disease Control and Prevention's Restricted Mortality Database.

**Table A.3:** Alternative Employer-Level First-Stage Estimates

	Baseline (1)	Only Price and Timing Variation (2)	Only Price Variation (3)	Only Timing Variation (4)	Using Predicted Price Changes (5)	Health Care Employers (6)	Non-Health Care Employers (7)
Simulated Spending	0.649*** (0.022)	0.536*** (0.022)	0.521*** (0.023)	-0.184*** (0.037)	2.458*** (0.053)	0.604*** (0.084)	0.652*** (0.023)
Observations	1,403,000	1,403,000	1,403,000	1,403,000	1,403,000	94,710	1,308,290
Number of Unique Employers	140,300	140,300	140,300	140,300	140,300	9,471	130,829
F-Statistic on First Stage	864.776	567.689	528.533	24.330	2,350.093	51.582	813.863

**Notes:** This table presents coefficient estimates from a regression of employer-level annual health spending per beneficiary on employer-level simulated spending per beneficiary, as given in Equation (5). Each estimate includes employer and year fixed effects. Each column presents estimates from a different regression. Column (1) presents estimates using our baseline instrument. Columns (2)-(4) present estimates using a modified version of our baseline instrument that purges any variation other than that coming from differences in post-merger price changes across mergers and timing across mergers (2), only differences in post-merger price changes (3), and only differences in merger timing (4). Column (5) presents estimates using a modified version of our baseline instrument that replaces the estimated post-merger price increases with the estimated post-merger change in WTP. Column (6) presents estimates using our baseline instrument but restricting to only employers identified as being in the health care sector. Column (7) presents estimates using our baseline instrument but restricting to only employers identified as not being in the health care sector. Data on health spending and simulated spending come from the Health Care Cost Institute. Means are reported in levels rather than in logs. Standard errors are reported in parentheses and are clustered at the employer level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.4:** County-Level First-Stage Estimates

	Baseline (1)	Only Price and Timing Variation (2)	Only Price Variation (3)	Only Timing Variation (4)	Using Predicted Price Changes (5)
Simulated Spending	1.034*** (0.160)	0.899*** (0.157)	0.887*** (0.158)	-0.032 (0.288)	5.799*** (0.947)
Observations	17,090	17,090	17,090	17,090	17,090
Number of Unique Counties	1,709	1,709	1,709	1,709	1,709
F-Statistic on First Stage	41.960	32.810	31.580	0.010	37.480

**Notes:** This table presents coefficient estimates from a regression of county-level annual health spending per beneficiary on county-level simulated spending per beneficiary, as given in Equation (11). Each estimate includes county and year fixed effects. Each column presents estimates from a different regression. Column (1) presents estimates using our baseline instrument. Columns (2)-(4) present estimates using a modified version of our baseline instrument that purges any variation other than that coming from differences in post-merger price changes across mergers and timing across mergers (2), only differences in post-merger price changes (3), and only differences in merger timing (4). Column (5) presents estimates using a modified version of our baseline instrument that replaces the estimated post-merger price increases with the estimated post-merger change in WTP. Data on health spending and simulated spending come from the Health Care Cost Institute. Means are reported in levels rather than in logs. Standard errors are reported in parentheses and are clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.5:** Alternative Insurance Premiums and Health Savings Account Results using Predicted Price Changes (i.e., Changes in Willingness-to-Pay Estimation)

<b>Panel A: OLS Estimates</b>		
	Log(Insurance Premiums) (1)	Share of Employees with a Health Savings Account (2)
Log(Spending per Beneficiary)	0.025 (0.032)	0.012*** (0.001)
<b>Panel B: IV Estimates</b>		
	Log(Insurance Premiums) (1)	Share of Employees with a Health Savings Account (2)
Log(Spending per Beneficiary) <i>(Instrumented using Merger-Driven Predicted Price Changes)</i>	0.868* (0.471)	-0.037 (0.025)
Mean Dependent Variable	5,036	0.038
Observations	39,700	1,403,000
Number of Unique Employers	3,970	140,300
F-Statistic on First Stage	89.600	2,923.997

**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of annual employer-level log health insurance premiums per enrollee (Column (1)) and the share of employees with contributions to a health savings account (Column (2)) on employer-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with employer-level simulated spending per beneficiary. However, when constructing our measure of simulated health spending, in lieu of estimating our merger-driven price increases using difference-in-differences regression, we use predicted price changes measured by changes in willingness-to-pay estimation to estimate the gains in market power for each merger. Each estimate includes employer and year fixed effects. Data on insurance premiums come from the Department of Labor Form 5500 filings. Data on an employer's share of enrollees with a health savings account comes from the Internal Revenue Service. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the employer level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.6:** Comparison of Our Employment Estimate to Prior Payroll Tax Elasticity Estimates

	Setting	Disemployment Response to 1pp Payroll Tax Increase
Our estimate <sup>†</sup>	US, 2008-2017	1.8%
<u>US Studies</u>		
<a href="#">Anderson and Meyer (1997)</a>	US, 1978-1984	0.7-0.9%
<a href="#">Johnston (2021)</a>	US, 2003-2012	1.5%
<a href="#">Guo (2024)</a>	US, 2008-2013	1.1-2.4%
<u>Other Studies</u>		
<a href="#">Gruber (1997)</a>	Chile, 1979-1985	0.0-0.3%
<a href="#">Saez et al. (2019)</a>	Sweden, 2003-2013	1.0%
<a href="#">Benzarti and Harju (2021)</a>	Finland, 1996-2015	3.4%
<a href="#">Bíró et al. (2022)</a>	Hungary, 2010-2015	0.3%
<a href="#">Lobel (2024)</a>	Brazil, 2008-2017	0.5%

**Notes:** We present estimates of the implied average employer-level reduction in the number of workers employed as a result of a 1 percentage point increase in payroll taxation from prior studies, as well as the equivalent as implied from our estimate of the effect of rising health costs on wages.

<sup>†</sup> We compute this estimate in two steps. First, we note that HCCI spending per covered life at the median employer is \$4,039 and the average worker has 1 dependent, meaning employers spend \$8,078 per worker. Second, we note that payroll per worker at the median employer is \$36,525. For this employer, a 1pp payroll tax increase is \$365.25 per worker, equivalent in dollar terms to a 4.5% increase in health care spending. Our estimate of the employment elasticity of health care prices for employers outside the health care industry is -0.4 (from Column (4) of Table 4). Multiplying these quantities together, our results imply that a 1pp payroll tax would reduce employment by 1.8%.

**Table A.7:** The Impact of Rising Health Care Prices on County-Level Income and Employment - Non-Health Care

<b>Panel A: OLS Estimates</b>			
	Log(Income per Capita) (1)	Share Unemployed (2)	Log(Income Tax per Capita) (3)
Log(Spending per Beneficiary)	0.023** (0.011)	0.010*** (0.003)	0.025* (0.013)
<b>Panel B: IV Estimates</b>			
	Log(Income per Capita) (1)	Share Unemployed (2)	Log(Income Tax per Capita) (3)
Log(Spending per Beneficiary) <i>(Instrumented using Merger-Driven Price Increases)</i>	-0.336** (0.162)	0.092** (0.043)	-0.434** (0.208)
Mean Dependent Variable	42,767	0.092	7,128
Observations	17,090	17,090	17,090
Number of Unique Counties	1,709	1,709	1,709
F-Statistic on First Stage	41.960	41.960	41.960

**Notes:** This table presents ordinary least squares (Panel A) and instrumental variables (Panel B) coefficient estimates from regressions of annual county-level log income per capita (Column (1)), share of the population collecting unemployment insurance or earning zero labor income (Column (2)), and log federal income tax receipts per capita (Column (3)) on county-level annual spending per beneficiary, instrumenting for annual spending per beneficiary with county-level simulated spending per beneficiary. We restrict our analysis in this sample to employers outside the health sector. We identify employers in the health care industry by whether they have a North American Industry Classification System code starting in '62', as reported to the Internal Revenue Service (IRS). Each estimate includes county and year fixed effects. Our labor market and tax revenue data comes from the IRS. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .



**Table A.8:** Additional County-Level Labor Market Outcomes

	Share Moving to Another County	Share Self-employed	Share with Zero Income	Share Receiving Unemployment Insurance	Log (Unemployment Insurance Payments per Capita)
	(1)	(2)	(3)	(4)	(5)
Log(Spending per Beneficiary) <i>(Instrumented using Merger-Driven Price Increases)</i>	-0.003 (0.014)	0.001 (0.026)	0.001 (0.014)	0.085** (0.037)	2.511* (1.479)
Mean Dependent Variable	0.066	0.110	0.054	0.036	482.128
Observations	17,090	17,090	17,090	17,090	17,090
Number of Unique Counties	1,709	1,709	1,709	1,709	1,709
F-Statistic on First Stage	41.960	41.960	41.960	41.960	41.960

**Notes:** This table presents instrumental variables coefficient estimates from regressions of annual county-level share of the population who moved to another county (Column (1)), the share of the population receiving self-employment income (Column (2)), the share of the population who received zero income (Column (3)), the share of the population receiving unemployment insurance (Column (4)), and the log unemployment insurance payments per capita (Column (5)). Each estimate includes county and year fixed effects. Our labor market and tax revenue data comes from the Internal Revenue Service. Means are reported in levels rather than logs. Standard errors are in parentheses and are clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.9:** Comparison of Our Death Per Labor Market Separation Estimate to the Prior Deaths per Job Loss Estimates

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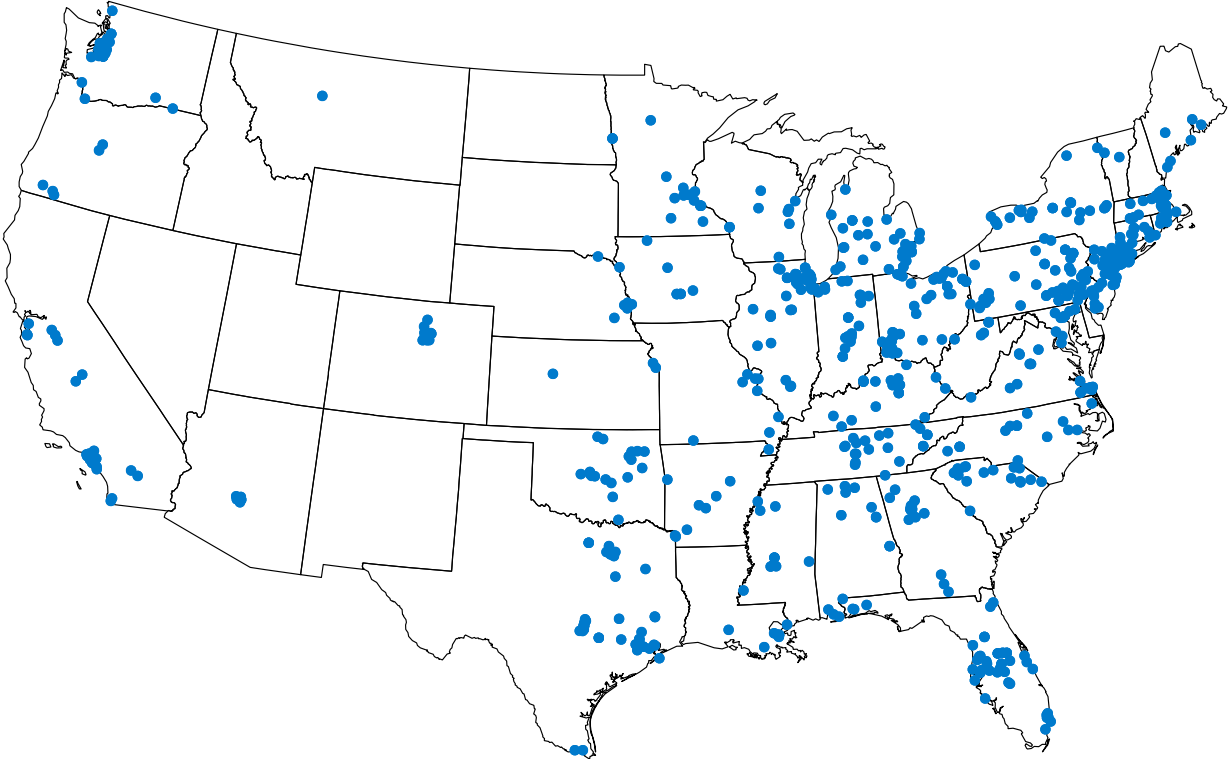
	Setting	Time Period	Death per job losses
<a href="#">Sullivan and von Wachter (2009)</a>	USA	1970s, 1980s	1 in 546
<a href="#">Eliason and Storrie (2009)</a>	Sweden	1980s	1 in 587
<a href="#">Pierce and Schott (2020)</a>	USA	2000s	1 in 400
<a href="#">Venkataramani et al. (2020)</a>	USA	2000s	1 in 300
Our estimate:			<b>1 in 140</b>

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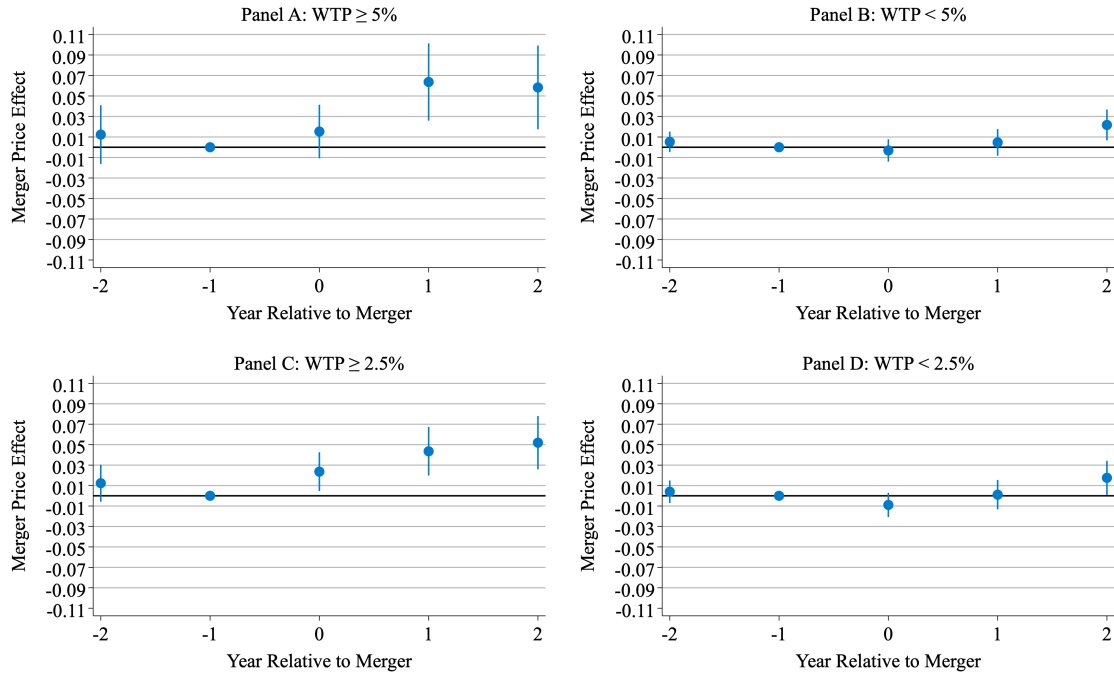
**Notes:** We present estimates of the implied number of deaths as a result of job loss from prior studies, as well as the equivalent as implied from our own estimates. We compute the ratio from prior estimates as the increase in deaths relative to the increase in job losses. We compute the ratio from our own estimates as the increase in deaths relative to the increase in unemployment insurance take-up.

**Figure A.1:** Map of Hospital Mergers



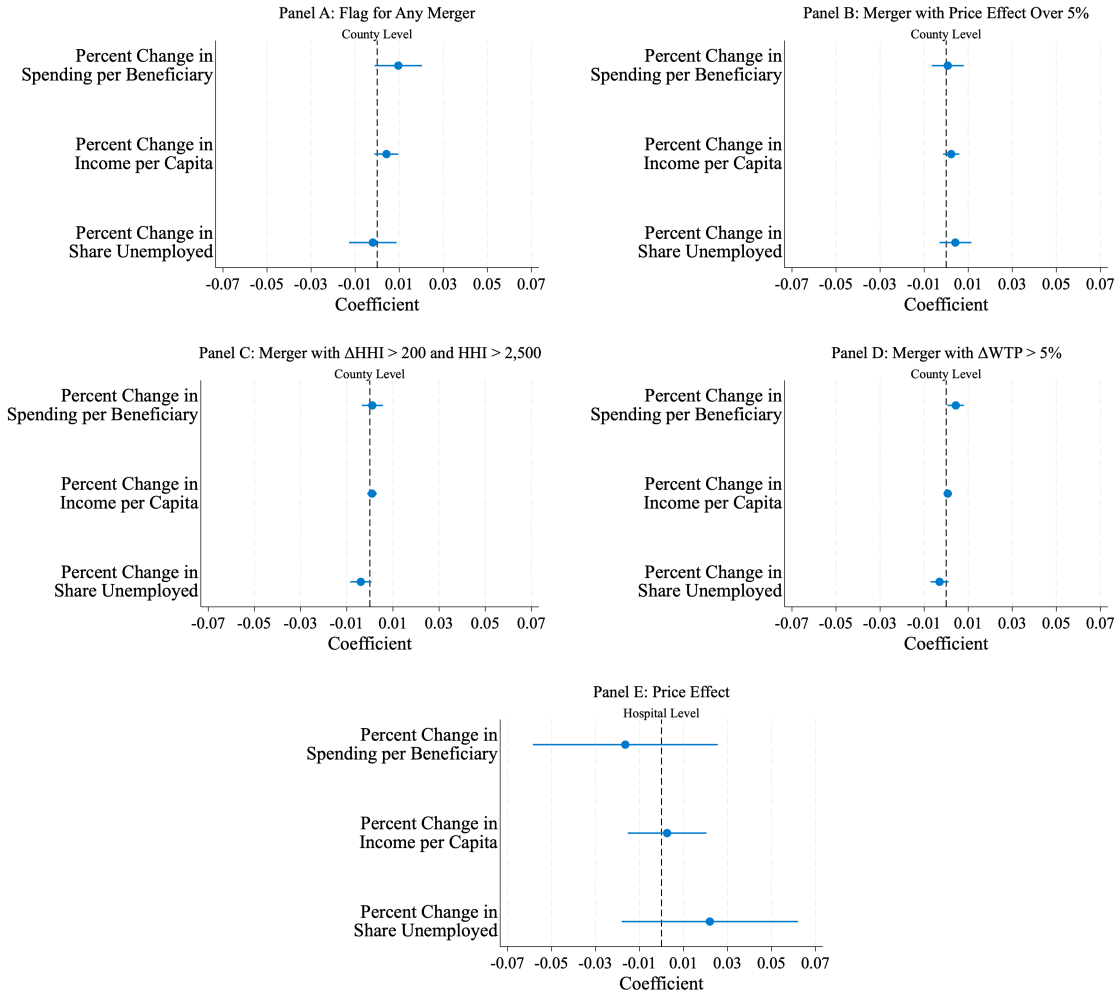
**Notes:** This presents the hospitals involved in the 304 hospital mergers from 2010 to 2015 we used in our analysis.

**Figure A.2:** The Impact of Mergers on Inpatient Hospital Prices by Willingness-to-Pay



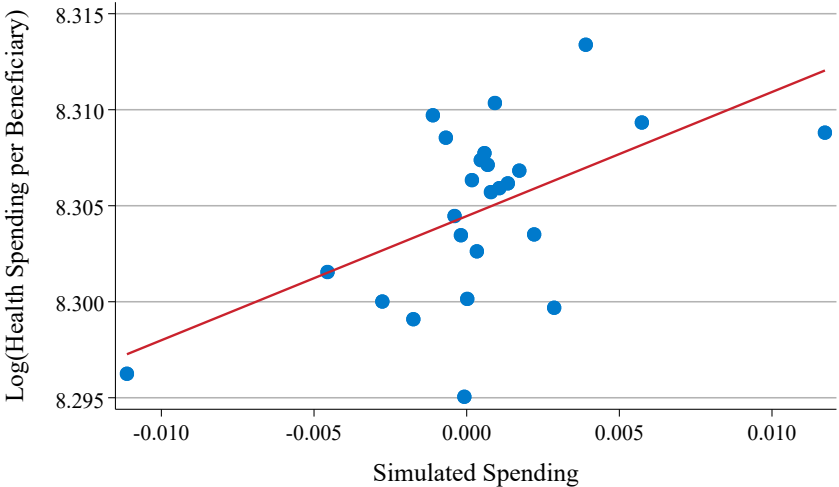
**Notes:** This figure presents difference-in-difference estimates of the effect of 2010 to 2015 hospital mergers on inpatient hospital prices. We estimate a pooled version of Equation (2) that estimates the average price increase within a group of mergers. We do so for the following groups: mergers that generated a  $\Delta WTP \geq 5\%$  (Panel A), mergers that generated a  $\Delta WTP < 5\%$  (Panel B), mergers that generated a  $\Delta WTP \geq 2.5\%$  (Panel C), and mergers that generated a  $\Delta WTP < 2.5\%$  (Panel D). Each dot represents a point estimate, and the vertical line displays the corresponding 95% confidence interval. Standard errors are clustered at the hospital level. Hospital pricing data come from the Health Care Cost Institute. The average difference-in-differences estimates comparing the two years prior to the merger with two years after the mergers in Panel A is 0.053 (0.026), in Panel B is 0.011 (0.009), in Panel C is 0.041 (0.016), and in Panel D is 0.008 (0.010).

**Figure A.3:** Relationship Between Trends in Economic Activity Prior to Mergers and Merger Location and Price Effects



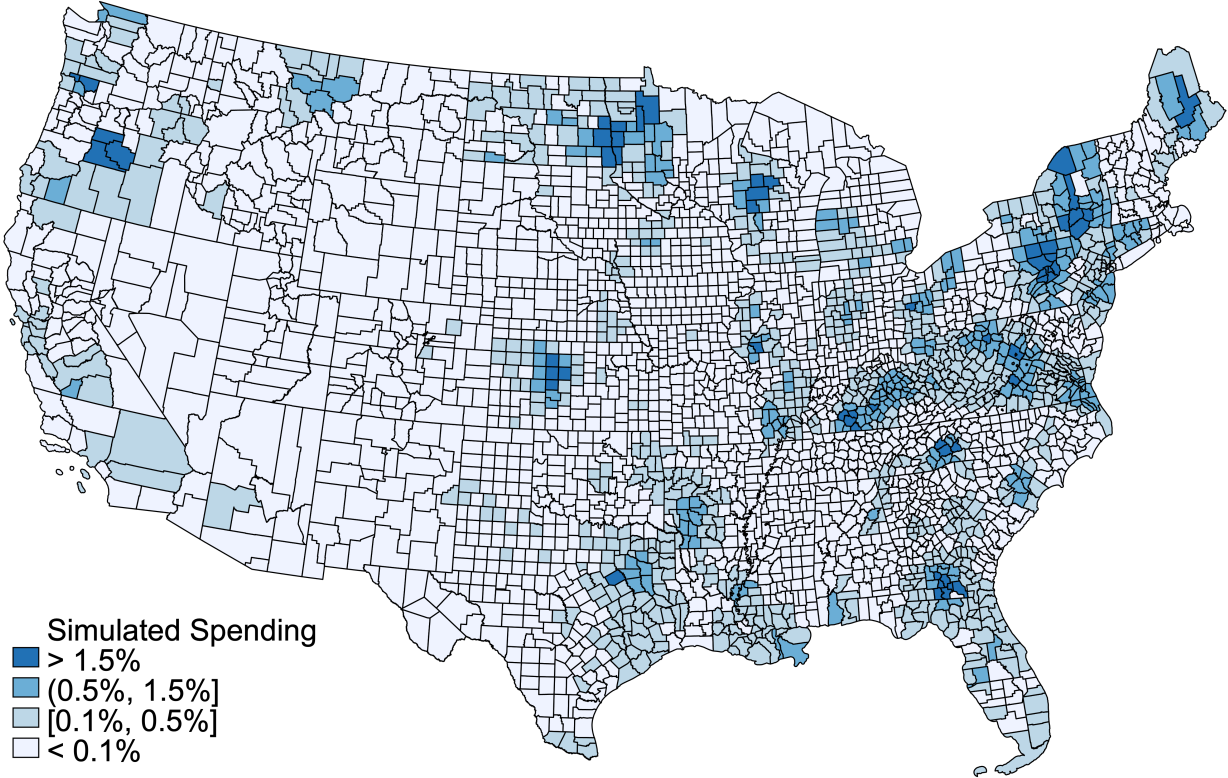
**Notes:** This figure presents the relationship between changes health spending per beneficiary and economic trends in the year prior to the merger and merger location and price effects. The independent variables are measured as the lagged percent change and Z scored. The regressions also control for the lagged levels of spending per beneficiary, income per capita, and share unemployed. Panel A shows a probit regression at the county-year level, where the dependent variable is an indicator that equals one if the county experienced a merger that year within its borders, while Panel B presents a similar regression with the dependent variable indicating whether the merger led to a price increase of 5% or more. Panels C and D present similar regressions where the dependent variable indicates whether a merger could be flagged using FTC guidelines and led to an increase in WTP of 5% or more, respectively. Panel E displays a hospital-level regression for the 654 merging hospitals in our sample, with the dependent variable being the merger’s price effect,  $\lambda_{eh}$ , as estimated in Equation 2. The data for Panels A, B, C, and D cover all counties in our primary analysis sample from 2010 to 2015.

**Figure A.4:** Binned Scatterplot of Employer-Level Health Spending Per Beneficiary and Simulated Health Spending



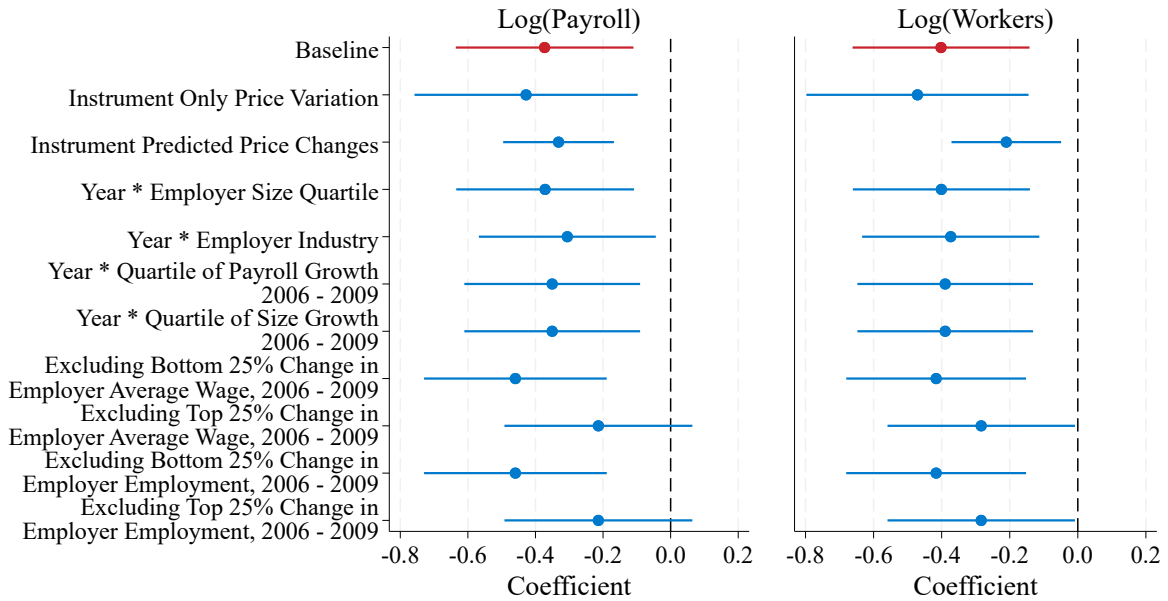
**Notes:** This figure presents a binned scatterplot of employer-level annual health spending per beneficiary against employer-level simulated spending per beneficiary, taking into account employer and year fixed effects.

**Figure A.5:** Change in Simulated Health Spending by County, 2009 to 2015



**Notes:** This figure presents the county-level change in simulated spending between 2009 and 2015. Darker areas are counties more exposed to the price increases generated by hospital mergers.

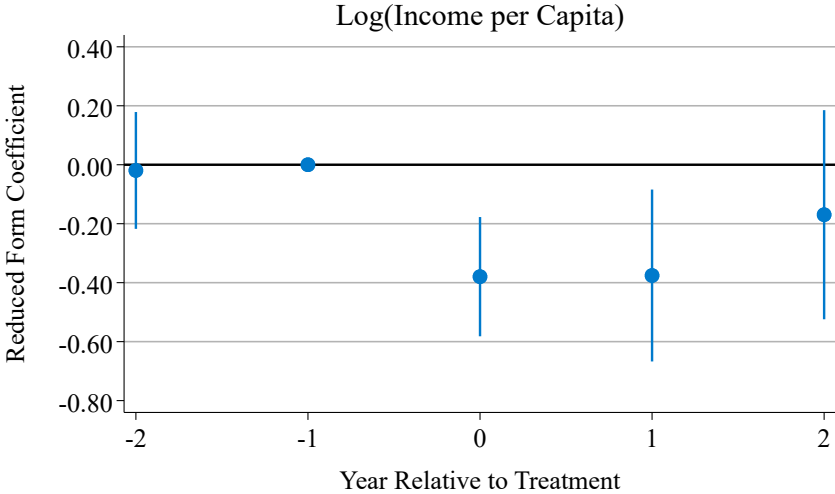
**Figure A.6:** Robustness Tests of Employer-Level Employment Effects



**Notes:** This figure presents two-stage least-squares estimates of Equation (6) of the effect of price increases on logged employer payroll and count of workers. Each estimate includes employer and year fixed effects. Observations are unique at the employer-year level. Our labor market data comes from the Internal Revenue Service. The dots represent point estimates and bars represent 95% confidence intervals.

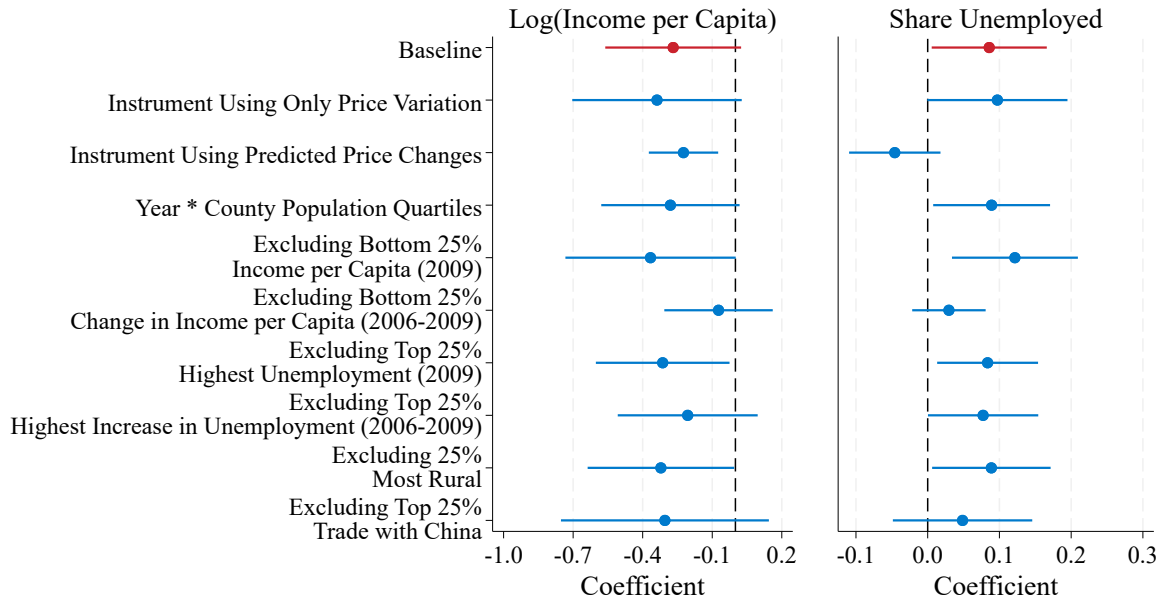


**Figure A.7:** Event Studies of the Impact of Rising Health Care Prices on Counties' Income Per Capita - Overall - Census Denominator



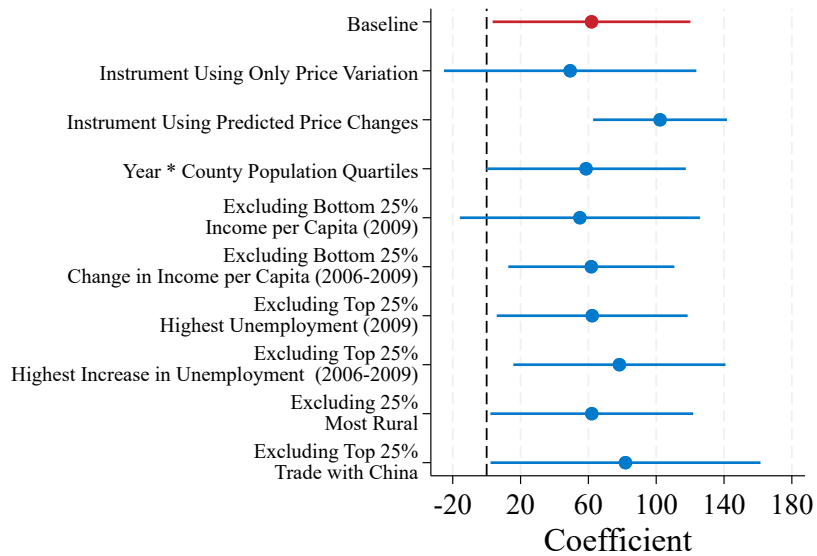
**Notes:** This figure presents alternative estimates of Figure 5 where we use Census-based population estimates to construct a county denominator, rather than IRS-based population estimates.

**Figure A.8:** Robustness Tests of County-Level Employment Effects - Overall - IRS Denominator



**Notes:** This figure presents two-stage least-squares estimates of Equation (12) of the effect of price increases on logged employer payroll and count of workers per employer. Each estimate includes employer and year fixed effects. Observations are unique at the employer-year level. Our labor market data comes from the Internal Revenue Service. The dots represent point estimates and bars represent 95% confidence intervals.

**Figure A.9:** Robustness Tests of Mortality Effects - Deaths from Suicides and Overdoses per 100,000 Population



**Notes:** This figure presents two-stage least-squares estimates of Equation (12) of the effect of price increases on logged employer payroll and count of workers. Each estimate includes employer and year fixed effects. Observations are unique at the employer-year level. Our mortality data comes from the Centers for Disease Control and Prevention’s Restricted Mortality Database. The dots represent point estimates and bars represent 95% confidence intervals.