Who Bears the Burden of Rising Health Care Spending in the US? Evidence from Hospital Mergers*

PRELIMINARY DRAFT — DO NOT CITE OR CIRCULATE

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Abstract

We analyze how increases in health care spending on the privately insured in the US impact labor market outcomes. We use price increases caused by horizontal hospital mergers as an instrument for local health care spending, and trace the incidence of rising health spending through the labor market using the universe of individual and business tax filings from the IRS. We find that a 10% increase in health care spending (roughly $500 per person) lowers income by 2.7%, increases the unemployment rate by 0.7 percentage points, and lowers federal income tax receipts by 2.8%. These effects are driven by a worsening of labor market outcomes in non-health-care industries. Our results imply that workers shoulder the majority of additional health spending. The job losses we observe are concentrated among individuals in the bottom third of the income distribution. We find that the same increase in health care spending leads to 3.8 additional deaths from overdoses and suicides per 100,000 individuals each year, which implies that individuals who lose their jobs die at a rate of approximately 1 in 200.

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1 Introduction

The vast majority of working age adults in the United States receive their health insurance as a fringe benefit provided by their employer or via a spouse's or parent's employer (Kaiser Family Foundation, 2019). Employer-sponsored health insurance (ESI) plans are financed by a combination of employer contributions and worker contributions that are paid using pre-tax income. Health insurance premiums are a substantial share of worker compensation — 8.2% of compensation in 2020 — and are increasingly expensive: between 2000 to 2020, ESI premiums for a family of four more than doubled in real terms (Claxon et al., 2021).

The presence and ubiquity of ESI in the US creates a direct link between changes in the market for health care services and labor market outcomes. Employer-sponsored health insurance in the US has been likened to a head tax on workers and posited to disproportionately impact the labor market outcomes of lower income individuals, for whom insurance premiums account for a higher share of total compensation (Saez and Zucman, 2019; Finkelstein et al., in preparation). Prior theoretical and empirical work suggests that the costs of fringe benefits, like ESI, are financed on a nearly dollar-for-dollar basis out of workers' wages (Summers, 1989; Gruber, 1994; Gruber and Krueger, 1991). However, this past work assumes that the costs of fringe benefits are fully-valued by workers. Ultimately, the extent to which increases in the cost of ESI leads to reductions in wages versus decreases in employment will be a function of the extent to which employees value the benefits they are receiving, the elasticity of demand for health care services and insurance, labor demand and supply elasticities, and various labor market frictions that potentially impede adjustments to workers' wages (Gruber and Madrian, 2004). As a result, it is possible that rising health spending in the US will lower the wages of higher income workers and lead to job losses for lower income workers.

In this paper, we test how rising health spending on individuals with ESI impacts labor market outcomes. We focus on estimating the incidence of rising spending on patients, workers, employers, health insurers, and the federal government. In particular, we analyze how the labor market effects of rising health spending on the privately insured vary across individuals as a function of their position in the income distribution. To do so, we leverage quasi-experimental increases in health care spending in the US generated by horizontal mergers of hospitals. Hospital mergers are an appealing instrument for health care spending for two reasons. First, past work has shown that hospital mergers raise hospital prices while having little or no effect on the quantity of care provided or the quality of health care services that are delivered and thus likely generate health spending increases that are not valued by workers (Gowrisankaran et al., 2015; Cooper et al., 2019a; Beaulieu et al., 2020). Second, hospital mergers are an important policy issue in their own right. Between 2000 and 2020, there were over 1,000 mergers among the nation's approximately 5,000 hospitals (American Hospital Association, 2020).

We bring together a range of data to study the impact of rising health spending on premiums, labor market outcomes and mortality. First, we rely on insurance claims data from three of the five largest insurers in the US, provided by the Health Care Cost Institute (HCCI). These data include hospitals' negotiated prices and cover approximately 28% of individuals in the US with employer-sponsored insurance. Second, we build upon a database of hospital mergers constructed in Cooper et al. (2019a) that captures hospital mergers that occurred between 2010 and 2015. Third, we measure ESI premiums using data from Form 5500 filings, which are collected by the Department of Labor in collaboration with the Internal Revenue Service (IRS), and capture firm-level insurance premiums for fully-insured products at firms with over 100 employees. Fourth, we measure labor market outcomes using the universe of individual and business tax filings from the IRS, which we supplement with data from the Quarterly Census of Employment and Wages (QCEW). Finally, we measure county-level mortality using restricted micro data from the Centers for Disease Control and Prevention (CDC) that distinguish mortality by cause. Unfortunately, while each of these data sets are comprehensive, they cannot be linked at the individual level, so we rely on counties as the relevant units of treatment.

We begin by measuring the extent to which hospital mergers between 2010 and 2015 raised hospital prices. We take a difference-in-differences approach and compare prices at merging hospitals with non-merging hospitals before and after mergers occur. The average merger in our sample raised hospital prices by 2% within two years after the transaction, and 30% of mergers raised prices by over 8%. On average, consistent with prior literature, we do not find that mergers reduce output or impact clinical quality.

While it would be intuitively appealing to use a similar difference-in-differences approach to compare the labor market outcomes of regions that experienced a hospital merger with those that did not, it is not tractable in our setting. In practice, there were few regions in the US that were unaffected by a merger within their boundaries or in a neighboring region. Regions without exposure to mergers are structurally different from most other regions in the US, making them an inappropriate control group. Moreover, it is challenging to neatly define pre and post exposure periods because most regions in the US that experienced a hospital merger experienced multiple mergers within our period of study.

To address this challenge, we employ a shift-share design in which we leverage the variation in the timing and price effects of mergers across the country. This strategy allows us to compare regions more and less exposed to the merger-driven hospital price increases. In practice, we construct a measure of a regions' simulated health spending, which holds fixed the quantity of care consumed in each region and focuses on variation in spending that is driven only by the timing and price increases generated by mergers, and by regions' exposure to these mergers based on their pre-merger patient flows to merging hospitals. We use this measure of "simulated spending" as an instrument for actual

region-by-year-level spending.

We address the fact that regions may be non-randomly exposed to mergers — for example, urban areas might be more exposed to more price-increasing mergers than rural areas — in two ways. First, because our research strategy is akin to a shift-share instrumental design, we can use a new tool, developed by Borusyak and Hull (2021), that allows us to explicitly specify the components of our instrument that we believe are plausibly exogenous. The approach describe by Borusyak and Hull (2021) (which we refer to going forward as the "B&H correction") allows us to explicitly specify the components of our instrument that we believe are plausibly exogenous — in our case, the timing of the merger (within a two-year window on either side of its consummation) and the size of the price change (net of indicators for where the merger takes place) — and residualize out components of the instrument that might generate violations of the exclusion restriction. Second, we show that our results are robust to exercises that eliminate variation in our data where these issues may be more present. For example, we exclude flailing firms — hospitals experiencing low bed occupancy before transactions — from our sample of mergers, since those transactions may occur in regions experiencing economic decline that can no longer support multiple hospital systems. Likewise, we show our results are robust when we exclude counties experiencing outlier economic decline from our analysis.

Our analysis has four key findings. First, we show that merger-driven increases in health spending are passed through on a nearly dollar-for-dollar basis into insurance premiums. Likewise, we do not observe any statistically or economically significant effects on the benefit design of ESI plans, which we measure via the actuarial value of insurance plans in HCCI.

Second, in the aggregate, we show that increases health care spending result in decreases in wages, reductions in employment, and a drop in the tax revenue collected by the IRS. Our estimates suggest that a 10% increase in local health spending lowers local wage income by 3%, raises the share unemployed of the population unemployed 0.7 percentage points, and lowers federal tax revenue (as measured by withholding amounts on W-2 filings) by 2.8%. These changes in wages and employment are driven by labor market changes in industries outside the health sector. Our estimates imply that the incidence of increases in health spending on labor market compensation is at least 80%.

Third, we show that the labor market effects of rising health spending differ across individuals as a function of their position in the income distribution. Consistent with theory, the effects of increases in health care spending on unemployment are only realized for workers in the lower half of the income distribution. The effects are largest for workers with earnings between the 20th and 40th percentiles of the income distribution. These are workers likely to receive insurance via ESI and for whom insurance premiums are a large share of their income.

Fourth, we observe an increase in mortality in areas experiencing an increase in health spending

on the privately insured. A growing literature has highlighted how job losses can lead to substantial health and social harm (Autor et al., 2019, 2020; Case and Deaton, 2021; Pierce and Schott, 2020). We find that a 10% increase in health spending leads to approximately 4 additional "deaths of despair" — deaths from suicides and overdoses — per 100,000. When we combine our results on the effect of health spending on employment and on deaths, our estimates imply that approximately 1 in 200 to 1 in 400 individuals who lose their jobs due to rising health care costs die, which is consistent with prior estimates of the mortality effects of job loss (Eliason and Storrie, 2009; Sullivan and von Wachter, 2009; Venkataramani et al., 2020).

This paper makes four distinct contributions. First, we estimate the extent to which increases in the price of health care are passed through into the cost of ESI and the wages and employment status of workers. In doing so, we contribute to the growing literature analyzing how changes in the cost of fringe benefits, including ESI, impact labor market outcomes (Currie and Madrian, 1999; Gruber, 2000). Consistent with prior work, we find that the provision of fringe benefits generates compensating wage differentials (Gruber and Krueger, 1991; Gruber, 1994; Cutler and Madrian, 1998; Kolstad and Kowalski, 2016; Anand, 2016; Arnold and Whaley, 2020). However, unlike much of this prior work, our results suggest that, in addition to lowering wages, increasing the cost of ESI can lead to extensive margin employment reductions.

Second, we add to the literature documenting how ESI can lead to inequality. Past work has highlighted that ESI operates as a "head tax" on insured workers creates proportionally larger effects on lower wage workers (Finkelstein et al., in preparation). We document that job losses generated by higher health spending occur for workers in the bottom third of the income distribution, indicating that the distributional consequences of higher premiums for ESI extend to job losses as well as prevailing wages for affected workers.

Third, this paper contributes to the industrial organization literature on the impact of rising market power. Since Harberger (1954), economists have traditionally focused on measuring the harms from mergers and monopoly in partial equilibrium terms, with damage from mergers coming from reductions in quantity in the market where the merger is occurring. However, because the demand for care from a specific hospital is relatively inelastic with respect to that hospital's price (Gowrisankaran et al., 2015; Brot-Goldberg et al., 2017), price increases in the hospital sector do not lead to substantial reductions in the quantity of care provided, implying that the deadweight loss from hospital mergers is small. However, our results suggest that merger-driven price increases in inelastic markets, like the market for health care services, are not merely a transfer from consumers to producers, but can generate substantial harms and deadweight loss in downstream markets that are more elastic (in our case, the labor market). Indeed, we estimate that a hospital merger that raises prices by over 5% lowers local wages, on aggregate, by $25 million; generates 233 job losses; reduces federal tax withholdings by $3.4 million; and leads to 1.2 deaths of despair.

Finally, this paper contributes to a wider discussion about the macroeconomic consequences of rising hospital prices and health spending in the US. Since 2000, hospital prices in the US have grown faster than prices in any other sector of the economy, and health spending has grown faster than wages (Bureau of Labor Statistics, 2022). The level of health spending and growth in health spending in the US during this period has been an international outlier among most other OECD countries (for Economic Co-Operation and Development, 2022). Case and Deaton (2021) have described the US health care system as "a uniquely American calamity," and "a cancer at the heart of the American economy." Warren Buffett has said, "the ballooning costs of healthcare act as a hungry tapeworm on the American economy" (JPMorgan Chase & Co., 2018). Our results support these assertions. Our findings — that rising health care costs are lowering wages, leading to unemployment and mortality — highlights the benefits of raising the productivity of the US health sector and the consequences of failing to do so.

Going forward, this paper is structured as follows. In Section 2, we provide background on ESI in the US, the drivers of health spending growth, and hospital mergers in the US. In Section 3, we describe how increases in the cost of health care in the US can impact labor market outcomes. We describe the data used in this analysis in Section 4. Section 5 includes a discussion of our analytic strategy. We present our results in Section 6. We discuss our results and conclude in Section 7.

2 Background

2.1 Employer-Sponsored Private Health Insurance

Employer-sponsored health insurance in the US, which generates the link between health care markets and labor markets that we study in this paper, 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 employer-based health insurance. 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, private insurers cover 54.3% of the US population and 63% of the population under age 65 (Keisler-Starkey and Bunch, 2022). Approximately 91% of US 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).

Approximately 36% of workers in the US are enrolled in fully-insured health insurance plans offered by their employers, in which the insurer bears financial risk and sets premiums for firms' employees. Approximately 64% of workers in the US (21% of workers in small firms and 82% in large firms) are covered by administrative services only (ASO) policies, under which an insurer administers the health benefits, but the employer bears the risk (Claxon et al., 2021). ASO plans tend to be the preferred choice among larger firms. In 2021, 46% of workers were enrolled in a

preferred provider organization (PPO) plan, 28% were enrolled in a high-deductible plan with a health savings account, 16% were enrolled in a health maintenance organization (HMO), 9% were enrolled in a point of service (POS) plan, and 1% are enrolled in an indemnity plan (Kaiser Family Foundation, 2021). Ultimately, insurance premiums are funded by a combination of employer contributions and employee contributions that are deducted from wages using pre-tax dollars.

Private insurers form networks of providers accessible to their enrollees and design benefits packages, including the degree of cost-sharing patients face (Brot-Goldberg et al., 2017). Typically, in an exchange for a negotiated price, insurers include hospitals and physicians in their networks, which makes the providers more accessible to enrollees. Whereas the Medicare program largely pays hospitals, health care facilities, and physicians regulated reimbursements set prospectively by the Centers for Medicare and Medicaid Services (CMS), private insurers negotiate market-determined prices with providers (Cooper et al., 2019a). On a roughly annual basis, hospitals and health systems engage in bilateral negotiations with insurers over payments for the tens of thousands of services hospitals offer in exchange for inclusion in their networks (Handel and Ho, 2021; Cooper et al., 2019a; Gowrisankaran et al., 2015). Hospital prices vary substantially across regions, within regions across hospitals, and within hospitals across insurers (Cooper et al., 2019a; Craig et al., 2020).

Between 2000 and 2021, average annual premiums for covered workers with family coverage increased in nominal terms from $6,395 to $22,389 (Claxon et al., 2021). Particularly in the short run, growth in hospital prices is the primary driver the growth in private health spending (Health Care Cost Institute, 2020). From 2007 to 2014, for example, hospitals' negotiated transaction prices for inpatient care grew by 42% and outpatient prices grew by 25%. By contrast, physician prices grew by approximately 6% during the same time window (Cooper et al., 2019b). According to the Bureau of Labor Statistics (BLS), since 1998, the prices for health care services have grown faster than the prices for any other goods or services within the US economy, including pharmaceutical products (Bureau of Labor Statistics, 2022).

2.2 Consolidation in the US Hospital Sector

Over the last two decades, there has been significant consolidation in the US hospital industry. Between 2000 and 2020, there were over 1,600 hospital mergers in the US among the nation's approximately 5,000 hospitals (American Hospital Association, 2020). Many of those mergers were among hospitals that were close substitutes for one another (Cooper et al., 2019a). As a result, hospital market concentration has been rising steadily over this period, and has become a topic of interest and importance for policy-makers and elected officials (Fulton, 2017; Biden, 2021). At present, the majority of hospital markets have a Herfindahl-Hirshman Index (HHI) of greater than 2,500 and can be classified as "highly concentrated" according to the joint Department of Justice and Federal Trade Commission horizontal merger guidelines (Fulton, 2017; U.S. Department

of Justice and the Federal Trade Commission, 2010). There is a growing literature showing that hospitals in more concentrated markets have higher prices and that hospital mergers can lead to economically significant price increases (Gaynor et al., 2015; Capps et al., 2003; Dafny, 2009; Tenn, 2011; Haas-Wilson and Garmon, 2011; Gowrisankaran et al., 2015; Cooper et al., 2019a). Conversely, there is scant research showing that mergers impact quality. Most recently, Beaulieu et al. (2020) show that hospital quality, measured across a range of outcomes, did not change following hospital mergers.

3 Hospital Mergers, Insurance Premiums, and Labor Market Outcomes

We present a simple model to demonstrate the intuition for how price increases generated by hospital mergers can generate changes in local labor market outcomes. We begin by characterizing price increases in the market for hospital care generated by hospital mergers. We then then trace how those price increases flow through the market for health insurance and, via a model stylized after Gruber and Krueger (1991), can impact labor market outcomes. In Figure 1, we show graphically the intuition for how rising hospital prices from a merger filter through into changes in labor market outcomes.

Consider a hospital that provides a single uniform service facing a residual (firm-specific) demand curve of $D^{Hosp}(p)$ for its services, with p defined as the price difference between the focal hospital and the next-best alternative (e.g., Panel A of Figure 1). Assume that a merger increases a hospital's market power without changing the demand curve it faces, such that the hospital can now raise its price from p_0 to p_1. Under these conditions, a merger will lead to a reduction in the equilibrium quantity of care provided by the hospital from $q_0 = D^{Hosp}(p_0)$ to $q_1 = D^{Hosp}(p_1)$. The deadweight loss from this enhanced market power, as per Harberger (1954), is approximately equal to the area of the Harberger triangle, $\frac{1}{2}(p_1 - p_0)(q_0 - q_1)$, which is equal to $\frac{1}{2}(p_1 - p_0)^2 E^{Hosp}$, where E^{Hosp} is the elasticity of hospital-specific demand with respect to its (relative) price. This deadweight loss comes entirely from the quantity change in patients who are now being pushed away from care at that hospital because of higher prices.

In practice, patients are unlikely to be elastic to price increases generated by hospital mergers. Prior work has examined variation in insurance generosity and estimated substantial elasticity of health care demand with respect to "market-wide" prices (Manning et al., 1987; Finkelstein et al., 2012). However, the elasticity of demand for a *specific* hospital with respect to *that hospital's* prices tends to be close to zero (Gowrisankaran et al., 2015; Brot-Goldberg et al., 2017; Lieber, 2017). Patients, in general, do not move away from a specific hospital in response to higher relative prices at that hospital. In practice, this means that the Harberger triangle is also likely to be small, implying minimal deadweight loss in the market for hospital care. [1] This change in price also comes with a

[1] Assuming no effects on clinical quality, which tend to also be well-documented empirically (Beaulieu et al., 2020).

large transfer in surplus from buyers to the hospital of $(p_1 - p_0)q_1 - p_0(q_0 - q_1)$, with the latter term being approximately equal to zero if quantities are largely unchanged.

For there to be no deadweight loss from mergers, we would have to assume that hospital mergers do not affect quantities in any other market. However, this is not likely to play out in practice. Although patients' consumption decisions regarding hospital mergers may not be affected by post-merger hospital price increases, the payments for their hospital care are, in large part, made by patients' insurers (see Panel B of Figure 1). Therefore, changes in the price of hospital care will spill over into the market for health insurance. This connection has been used by other research to rationalize why hospital prices do not go to infinity even though patient demand elasticities are so low (Ho and Lee, 2017).

We therefore need a model of health insurance markets. Because we focus on employer-sponsored health insurance (ESHI), decisions about health insurance purchasing are delegated to employers. Consider a firm seeking to employ workers. The cost of hiring a worker is the wage the firm pays, w, along with the cost of buying ESHI, which is equal to the premium ϕ. The firm only needs to pay for ESHI if it chooses to offer it, as determined by the indicator I. The solution to its profit maximization problem generates a demand for labor $L^D(w+\phi I)$, and it faces face a labor supply curve $L^S(w+\alpha I)$, where α represents workers' dollarized value of insurance. Equilibrium wages w and employment e will be determined by the intersection of supply and demand. Additionally, the firm chooses whether to offer insurance as a function of its premium, i.e., $I = I(\phi)$. We assume, initially, that insurance covers 100% of costs, and that insurance is priced at actuarially fair rates with a stable multiplier, plus a fixed cost, i.e., $\phi = \mu S + F$, where S represents the total spending on health care per employee.

This model can be used to understand the comparative statics of how an increase in average health care spending for workers, S, affects equilibrium wages, employment, premiums, and insurance offerings (see Panel C of Figure 1). We first assume that mandates for ESI bind, so $I = 1$. The effects of spending increases on premiums are simple in this model: the change in premiums will be $\Delta\phi = \mu\Delta S$. This increase serves as an effective head tax on hiring: for each worker hired, the employer will now have to pay $\Delta\phi$ more to hire that worker, shifting the labor demand curve from $L^D(w+\phi)$ to $L^D(w+\phi+\Delta\phi)$. This increased payment is not valued by the worker (since insurance offerings do not change), and so the labor supply curve is unchanged. A simple application of the implicit function theorem finds that the effect on wages will be $\Delta w = -\frac{|E^D|}{|E^D|+|E^S|}\Delta\phi$, where E^S, E^D are the elasticities of labor supply and demand. Furthermore, the effect on employment will be given by $\Delta e = -\frac{dL^S}{dw}\frac{|E^D|}{|E^D|+|E^S|}\Delta\phi = -\frac{|E^D||E^S|}{|E^D|+|E^S|}\frac{e}{w}\Delta\phi$, with w and e in this equation as the initial wage and employment levels. The effect on log total earnings is then $\log M = \log w + \log e$ is $\frac{\Delta M}{M} = \frac{E^D(1+E^S)}{E^S-E^D}\frac{1}{w}$.

The intuition behind the algebra above is simple and applies to any head tax on labor. First,

the extent to which workers will pay for rising health care costs is increasing in the elasticity of labor demand. If workers are replaceable, then an institutional change that makes them more expensive relative to other factors of production will cause firms to substitute toward those other factors, reducing both wages and employment. Second, the relationship between the incidence on labor earnings and the elasticity of labor supply is ambiguous. However, the extent to which labor incidence is realized as employment reductions (as opposed to wage reductions) is increasing in the labor supply elasticity.[2]

Finally, the extent to which all labor market outcomes are changed, in proportional terms, will be larger when the initial equilibrium wages are lower. The intuition for this is simple: a head tax of a fixed size will have much larger effects when it is a larger share of compensation. A $1,000 per year increase in the cost of hiring a worker will be much more costly in relative terms for firms hiring workers at wages of $20,000 per year than for workers hired at wages of $500,000.

This simple model provides a handful of testable implications. First, we should expect to see effects only among employees at firms that offer health insurance; for firms that do not, the cost of total compensation will remain unchanged. Second, we should see larger proportional impacts on earnings for employees at firms with greater labor demand elasticities (e.g., where automation is easier) and for lower-wage employees. Finally, we should see larger employment effects and smaller wage effects for workers with greater labor supply elasticities.

4 Data

4.1 Measuring Health Care Spending and Hospital Prices

We use insurance claims data from 2008 to 2017 provided by the Health Care Cost Institute (HCCI) to measure health spending among individuals enrolled in employer-sponsored coverage and to capture hospital prices. The HCCI data is 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 covers more than $125 billion in health care spending annually. Crucially, the data includes the transaction prices that hospitals negotiate with insurers. While the HCCI dataset offers more comprehensive coverage of the US than virtually all other claims databases, the data density varies across the US with the HCCI data contributors' market shares. See Appendix Figure A.1 for a map showing the density of HCCI data coverage across states in the US and Appendix Figure A.7 showing a map of spending per beneficiary across US counties.

[2]Specifically, if the elasticity of labor demand is greater than 1, then the incidence is increasing in the elasticity of labor supply; if the elasticity of labor demand is less than 1, the incidence is decreasing in the elasticity of labor supply. We abstract from a microfounded model of labor supply, so the effective supply elasticity in response to a wage reduction may be governed by traditional determinants such as worker outside options (Caldwell and Harmon, 2019) or by rigidities that make wage cuts costly (e.g., workers' distaste for wage cuts) or impossible (e.g., a binding minimum wage) (Fallick et al., 2022).

We focus our analysis on health spending 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). We measure average spending per beneficiary per county annually across all medical claims (e.g., physician claims, inpatient claims, and outpatient claims). We exclude pharmacy-dispensed pharmaceutical spending from our analysis.[3] We sum member spending by month and divide by 12 to adjust for individuals who are not enrolled in coverage for a full calendar year. We limit our analysis to counties with at least 500 members annually. The HCCI data includes at least 500 enrollees in 1,710 of 3,006 counties. The populations in these counties account for over 95% of the US population.

We follow Gowrisankaran et al. (2015) and Cooper et al. (2019a) to construct hospital-year measures of inpatient and outpatient price levels based on the allowed amounts on hospital claims (the sum of the actual patient and insurer payments to hospitals). For each hospital-year, we construct hedonic price measures separately for inpatient and outpatient care that adjust for the mix of services hospitals offer (measured using diagnosis related groups (DRGs) and current procedures technology (CPT) codes for inpatient and outpatient care, respectively, and the age and sex of patients they treat. We also create a combined inpatient and outpatient price measure by averaging the two, weighting each component by its respective share of total spending at the hospital. We provide a detailed description of how we measure hospital prices in Appendix A.

4.2 Measuring the Effect of Mergers on Hospital Prices

We extend the database of hospital mergers used in Cooper et al. (2019a) and analyze the post-merger inpatient and outpatient price changes generated by 316 hospital mergers that occurred in the US between 2010 and 2015. As we illustrate in Panel A of Figure 2, there were, on average, 53 mergers per year involving 702 hospitals in our treatment window between 2010 and 2015. As we illustrate in Panel B, there was significant geographic heterogeneity in these mergers.

We measure the post-merger changes in hospital prices using a difference-in-difference regression that compares the change in prices at merging hospitals, before and after a merger, to concurrent changes in prices at non-merging hospitals over the same period. For each merging hospital, we construct a comparison group of matched controls. Each set of one merging hospital and its matched controls forms an "experiment" around each merger event, e. For each merger, we limit the event to the period covering two years before and after the merger. We then estimate a regression of the form:

[3]Prescription drug claims are often offset by large rebates negotiated between payers and drug manufacturers. These rebates are unobserved in the HCCI data, rendering any claims-based spending measurement inaccurate.

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

Our outcome of interest is the hedonic price index generated by the process described in the previous section. This approach allows us to estimate a merger retrospective for each merger event separately, and generate unique merger-by-hospital-specific price effects λ_{eh}. The estimated price effects for any given merger are independent from those estimated for other mergers by construction.

As we illustrate in Panel (a) of Figure 3, on average, mergers between hospitals located less than 50 miles apart raise hospital prices by 1.6%. Past work by Cooper et al. (2019a) demonstrated significant variation in the effect of hospital mergers on prices. In general, mergers involving hospitals located within 5 miles of each other generated markedly larger price increases than mergers involving hospitals located farther apart. Our results show similar heterogeneity in post-merger price increases. As we illustrate in Panel (b) of Figure 3, 55% of hospitals involved in mergers have combined inpatient and outpatient price increases below 2%, 5% of hospitals involved in mergers have price increases between 2% and 4%, 7% of hospitals involved in mergers have price increases of between 4% and 6%, 6% of hospitals involved in a merger have price increases between 6% and 8%, and 26% of hospitals involved in mergers have price increases of greater than 8%. In our companion piece, Brot-Goldberg et al. (2023), we provide further discussion of which mergers generate the largest increases in hospital prices.

In addition to measuring the effect of mergers on hospitals' prices, we also measure the effect of mergers on hospitals' patient mortality and their volume of care delivered. We measure in-hospital mortality using a hospital-level, risk-adjusted measure based on HCCI claims data.[4] We measure hospital volumes by using total inpatient days in the HCRIS data. As we illustrate in Panels (a) and (b) of Appendix Figure A.2, we do not observe that hospital mergers either harm quality or reduce the volume of care hospitals deliver. This is consistent with prior literature (e.g, Beaulieu et al. (2020)).

4.3 Measuring Insurance Premiums and Actuarial Values

We measure firms' insurance premiums using data from the Form 5500, which is a regulatory filing collected by the Department of Labor in cooperation with the IRS. The data contain measures of total premiums, covered lives, and plan characteristics for employer-sponsored insurance groups covering at least 100 employees enrolled in fully-insured insurance plans.[5] We use the data to construct a

[4]There is an expansive literature on using in-hospital mortality as a quality measure (see Doyle et al. (2015); Department of Health and Human Services (2007) for reviews). Past work has illustrated a high correlation between in-hospital mortality and 30-day mortality across a range of medical conditions (Rosenthal et al., 2000; Borzecki et al., 2010).

[5]The premiums for self-funded plans are not well-documented in the Form 5500, and do not generally reflect the total cost of insurance provision in the way that they do for the fully-insured.

measure of average premiums per covered life at the employer-level. We then calculate county-level average premiums based on the location of the employer, following Craig (2022). We measure premiums for all insurers offering plans in a region and for the HCCI data contributors Aetna, Humana, and UnitedHealth. We provide additional details on the construction of our premium series in Appendix B.

We also use the HCCI claims data to measure the average actuarial value of insurance by county by year. For each county and year, we measure the amount of total health spending paid for by insurers, as a share of the total amount paid by both insurers and by members in the form of out-of-pocket payments from members.

4.4 Measuring Income, Employment, and Tax Withholdings

We measure labor market outcomes using data from tax filings held by the IRS. We construct a denominator based on population files that contain information from all tax returns filed per year and from informational returns (e.g., W-2 forms, 1099s, etc.). Our primary labor market outcomes of interest are, for each county and year, per capita labor income, the share of workers who are self-employed, and the share of workers who are unemployed. We focus on individuals aged between 25 and 64 in the year of interest, who represent the individuals likely to receive employer-sponsored health insurance.

For each individual in our sample, we link information from two forms (when applicable): W-2 forms, which capture wage income from standard employment, and Schedule SE forms, which measures income from self-employment. We then add up individuals' total income reported from both sources. We measure unemployment in two ways: first, we analyze whether an individual is receiving unemployment insurance based on information that is sent by the state in form 1099-G; second, we rule out unemployment if there was a W-2 or filed Schedule SE form with positive income at all during the year. Finally, we count individuals as self-employed if they filed a Schedule SE form with positive income at all during the year. To construct a county-by-year per capita measure for each of these outcomes, we generate counts of individual in each county and year using Census population estimates.[6]

We also measure per capita tax payments in the county and year. Measuring all tax payments is challenging. We focus primarily on federal income tax payments. We estimate an individual's total labor income tax payment by summing the amount reported as withheld on their W-2 forms.[7] We then measure this at a county-by-year per capita level using the same denominator as described

[6]We attribute individuals in the tax data to a specific county in a year by identifying the zip code where each person resides using different tax forms in a hierarchical order. In practice, we default to the tax return filed for the prior year (filed at the beginning of the reference year), then the W-2, then the Form 1099-G.

[7]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.

above.

In later analyses, we separate out employed labor income by whether it originated in the health care sector or not. We do so by matching the employer ID on W-2 forms to business tax filings on all business forms. These forms report the North American Industry Classification System (NAICS) industry code of the employer. We classify an employer as a "health care employer" if it reports a NAICS code beginning in "62" for any year starting in 1999. We classify a worker as a "health care worker" if, in a given year, the majority of their W-2 income was paid by a health care employer.[8]

Finally, we augment our labor market analysis with data from the Quarterly Census of Employment and Wages (QCEW), which measures total workers and total wage payments at an employer level. We measure per capita wages and employment from this data by summing the relevant outcomes within county and year over employers. In this data set, we can identify the industry of an employer by its reported NAICS code; again, we determine if an employer is a health care employer if it reports a NAICS code beginning in "62."

4.5 Measuring Mortality

We measure mortality as the number of deaths per 100,000 population per county and using National Center for Health Statistics (NCHS) Restricted Vital Statistics from the CDC. The NCHS Restricted Vital Statistics includes hundreds of causes of death broken down into "internal" causes (e.g., liver disease) and "external" causes (e.g., drug overdose). The data also classify deaths by the decedent's age. Following Pierce and Schott (2020), we combine the counts of death for individuals age 25 through age 64 by county, by year, and by cause with population data from the American Community Survey that captures the annual county population of individuals age 25 to 64. We focus what Case and Deaton (2020) have termed "deaths of despair," cancer deaths, and overall deaths exclusive of deaths of despair.In our measure, deaths of despair include deaths from accidental poisonings, deaths from poisonings of undetermined intent, deaths from alcohol-related liver disease, and suicides. We take the same approach to construct per-100,000 mortality rates for other causes of death, including all causes (excluding deaths of despair) and cancer deaths.

5 Empirical Strategy

Our goal is to trace out the causal impact of increases in health spending on employer-sponsored health insurance premiums, labor market outcomes, and mortality. The structural equation we are interested in takes the form:

$$y_{ct} = \beta x_{ct} + \theta_c + \eta_t + \varepsilon_{ct} \tag{2}$$

[8]Unemployed or self-employed individuals are always classified as not being health care workers because they are not matched to a business filing.

where y_{ct} represents one of our outcomes of interest – premiums, wages, employment, and mortality – and x_{ct} represents health spending for county c in year t. Our coefficient of interest is β, which represents the causal effect of a change in local health care spending on a given outcome. θ_c and η_t are county and year fixed effects, respectively, so that we focus on the effects of health care spending on outcomes net of county-specific level differences, as well as secular trends for all counties over time. β is identified from a regression of outcomes on local health care spending if the residual variation in spending (net of county and year fixed effects) is independent from ε_{ct}, representing idiosyncratic county-specific trends in outcomes.

Our core empirical challenge is the potential for reverse causality: within-county changes in total health care spending may be *caused* by changes in local economic conditions, rather than local economic conditions changing as a result of changes in health care spending. For example, in regions experiencing economic growth, resulting increases in salaries could help drive increases in the demand for medical services in that region.[9]

We therefore need to find a source of changes in health care spending that is not driven by local economic conditions. Our general approach is to rely on price increases generated by new hospital mergers as sources of variation across counties, over time, in health spending. As we document in Section 4.2 (and further in Brot-Goldberg et al., 2023), hospital mergers raise prices by nearly 2% on average.

Unfortunately, while it is tempting to use the same difference-in-differences approach we introduced in Section 4.2 and compare our relevant outcomes between regions exposed to a hospital merger with regions not exposed, before and after the merger, such an approach is not possible. Of, for example, commuting zones (CZs) in the US that have a hospital within their borders, fewer than 1% were not exposed to a hospital merger occurring in either their CZ or a neighboring CZ between 2007 and 2017. The CZs that did not experience a merger have lower average incomes and are more rural than "treated" CZs, making them a poor control group. Furthermore, among the CZs that experienced a hospital merger in their CZ or a neighboring CZ between 2007 and 2017, all but one experienced multiple mergers during this time period. This makes distinguishing pre- and post-merger treatment periods using a difference-in-difference strategy untenable.

As a result, rather than comparing regions that were or were not exposed to a merger, we instead use the variation in the intensity and timing of regions' exposure to merger-driven price increases. The extent to which a county is exposed to price increases from mergers depends on 1) the extent to which mergers raise prices at merging hospitals; and 2) the extent to which patients from the county tend to receive care at merging hospitals. Total exposure also varies across time for a county based on 3) the number of mergers a county is exposed to in each year. This approach allows us to think

[9] As we illustrate in Appendix Figure A.3, there is a negative relationship between spending per beneficiary and wages per capita in the cross section.

about mergers as local "shocks" of differential size that raise health spending differentially across counties and time. We construct a measure of "simulated health care spending" that summarizes merger exposure by estimating how county-level health care spending would evolve over time if the *only* changes that occurred were price increases due to mergers, with no changes in quantities of care received.

5.1 Simulated Health Spending

Our primary instrument is a measure of how local health care spending would have changed if all other factors, other than merger-driven hospital price increases, had remained fixed. This requires us to map the price effects of mergers, which are defined at the hospital level, into an instrument focused at the county and year level.

We define a county's exposure to a merger event e occurring at a specific hospital h as:

$$Z_{ceht} = \underbrace{\lambda_{eh}}_{\% \text{ price change at hospital } h} \times \overbrace{s_{ch0}}^{\text{Share to hospital } h} \times \underbrace{1\left[t \geq \tau_e\right]}_{\text{Timing of merger}} \tag{3}$$

where we define the exposure of a county in a given year to a merger, Z_{ceht}, by how much that merger would have raised spending at the county. This instrument has three components.

First, the term $1\left[t \geq \tau_e\right]$ reflects the timing of the merger. We assume that a merger only affects a county's health care spending once it occurs, and has a constant effect on the price of care at that hospital for that year and every year following. Second, the term λ_{eh} represents the extent to which the merger event e raises service prices at the merging hospital h. Mergers that raise prices by more will be expected to raise local spending by more. The λ_{eh} values here are estimated by our post-merger price effect regressions in Section 4.2. If a merger event e did not include a hospital h, λ_{eh} is set to zero.

Finally, the term s_{ch0} represents how much a merging hospital h matters in driving health care spending, measured by the share of hospital spending by HCCI beneficiaries at h. This term reflects the fact that a merger that raises prices by 5% at a hospital will have a larger effect on spending at a county c if the hospital is one that most local patients go to. For example, a merger of a hospital in Brooklyn is likely to have a larger effect on health care spending in New York County (Manhattan, in New York City) than a hospital merger in San Francisco. We measure county-by-hospital spending shares from 2008 and 2009, in notation as "0", which is before the mergers in our sample were consummated. As a result, the quantities of care provided in each county are held fixed across time, and only prices are allowed to change. We additionally set s_{ch0} to zero if the hospital is located at least 100 miles from the centroid of the county.

We then sum these hospital-specific effects of mergers Z_{ceht} across the set of all hospital mergers

nationwide:

$$z_{ct} = \sum_{e \in \mathscr{E}, h \in \mathscr{H}} Z_{ceht} \tag{4}$$

to yield the effect of all hospital mergers, $\mathscr{E}$, for all merging hospitals, $\mathscr{H}$, on spending in each county c in year t. z_{ct} evolves over time as mergers switch on, and increases to a greater extent if the county had greater dependence on hospitals which merged, especially those that generated larger increases in prices. In Appendix Figure A.4, we present a map showing change in simulated spending between 2010 and 2015. In Panel A of Appendix Figure A.5, we show a scatter plot illustrating the relationship between true spending and simulated spending, and in Panel B, we show the relationship between change in simulated spending and change in true spending.

With our instrument z_{ct} constructed, we estimate instrument for actual health spending xct via two-stage least-squares (2SLS) estimation of the system:

$$x_{ct} = \beta_1 \times z_{ct} + \kappa_c + \kappa_t + u_{ct} \tag{5}$$

$$y_{ct} = \beta_2 \times x_{ct} + \gamma_c + \gamma_t + \varepsilon_{ct} \tag{6}$$

In Column (1) of Table 1, we show our first stage estimates of Equation (5). We observe that a 10% increase in simulated spending leads to a 4.5% increase in health spending, with an F-statistic of 28.5. In Appendix Figure A.5, we present scatter plots of the relationship between the change in simulated spending and true spending from 2010 to 2015.

5.2 Addressing Other Endogeneity Concerns

Our approach of instrumenting for actual local health care spending with simulated health care spending relies on an assumption: even though, in general, changes in local health care spending are potentially caused by idiosyncratic county-specific trends in outcomes of interest, this is not true for merger-driven price increases, which are the component of these spending changes that our simulated instrument captures. Put formally, we must assume that z_{ct} is orthogonal to ε_{ct}.

Variation in our simulated instrument comes from three sources: differences in when mergers occur, the extent to which mergers raise specific hospitals' prices, and the extent to which counties are connected to specific hospitals. A natural concern is that some parts of this variation are not plausibly orthogonal to ε_{ct}. For example, it may be the case that more urban counties have more hospitals nearby, and thus are likely to be exposed to more mergers than rural counties. However, urban counties could also be on different labor market trajectories than rural counties, thus biasing our estimates. Similarly, as shown by Brot-Goldberg et al. (2023), mergers that involve smaller

hospitals (those where the deal size is below the threshold for filing a report of their merger to federal agencies as per the Hart-Scott-Rodino (HSR) Act) are less likely to face antitrust scrutiny and are therefore more likely to produce larger effects on price (Wollmann, 2019). Those mergers are also more likely to take place in counties with lower population density, which may be experiencing different trends in economic outcomes than counties with high population density.

To the extent that some components of our IV may be contaminated by potential endogeneity concerns in this way, we would ideally like to purge the unusable variation from our instrument and focus only on the plausibly exogenous variation. In this way, our design is analogous to recent "shift-share" research designs used in other empirical settings. We can thus use new tools from this literature to correct for potential biases from reverse causality.

To address these challenges, we use the method developed by Borusyak and Hull (2021). In their framework, bias comes from the fact that the expected value of the instrument (what Borusyak and Hull (2021) call the "expected instrument")—averaging over potential realizations of the *random* exogenous components of the instrument—may be correlated with ε_{ct} due to the presence of the endogenous components. Their solution, which we employ, is to explicitly define the distribution of the random components of the instrument, and use it compute the expected instrument. We can then add this expected value as a control in the first and second stages of our estimation (Equations (5) and (6)). This method will purge our estimates of the bias coming from the relationship between potentially endogenous components of the instrument and ε_{ct}, while still allowing those potentially endogenous components to scale up (or down) the impact of the assumed-exogenous components.

In practice, this method requires that we explicitly decide what components of our instrument are plausibly exogenous, *and* make assumptions about their joint distribution. The largest concern is that the hospital market shares are a function of where a county is located, which is most likely to determine its labor market trends, as well as those of premiums and mortality. We therefore do not assume that these shares are random.

While the decision to merge may be related to local economic trends, we assume that the *specific timing* of a merger with a short time window is random. There is an immense amount of logistical work necessary to complete mergers. If mergers require additional screening by state or federal regulators, that will also impact the timing of deals. Specifically, we assume that any merger we observed in year t could have otherwise counterfactually occurred in any year between $t-2$ and $t+2$. We assume that the merger could have occurred with probability $\frac{1}{5}$ in each of these years, and that this short-run timing is independent of any local trends. While this assumption is not testable, we do show in Appendix Figure A.6 that the timing of mergers does not appear to be spatially determined in any clear way.

Finally, we assume that the merger price effects λ_{eh} are conditionally random. As mentioned above, mergers that are below the HSR threshold for filing tend both to have larger price effects

and be in less dense areas. We assume that λ_{eh} is conditional random on whether e was an above- or below-HSR merger. We assume that $\lambda_{eh} = \zeta_0 + \zeta_1 \times \text{AboveHSR}_e + \nu_{eh}$, where AboveHSR_e is a binary indicator for whether the merger deal size was above the HSR threshold. We estimate ζ by regressing λ_{eh} on such an indicator, and assume that ν_{eh} is distributed according to the empirical distribution of $\hat{\nu}_{eh}$, the estimated residuals from such a regression. With this approach, we are assuming that component of the merger price effect net of the effect of HSR reporting is independent from local economic trends. While this may appear strong, we view this residual component as largely coming from how close hospitals are to each other. As Cooper et al. (2019a) show, hospital mergers involving merging firms that are more closely co-located produce larger effects than mergers involving hospitals separated by larger distances. [10] In Appendix Figure A.8, we scatter the estimated price effect λ_{eh} and its estimated residual component $\hat{\nu}_{eh}$ against county density in 2010 and find that, while the price effect is correlated with density, the residual is not.

Once we have the expected instrument, $\tilde{z}_{ct}$, constructed, we can estimate β_2, the instrumental variables estimate of the effect of health care spending on outcomes, with two-stage least-squares (2SLS) estimation of the system:

$$x_{ct} = \beta_1 \times z_{ct} + \kappa_c + \kappa_t + \delta_1 \tilde{z}_{ct} + u_{ct} \quad (7)$$

$$y_{ct} = \beta_2 \times x_{ct} + \gamma_c + \gamma_t + \delta_2 \tilde{z}_{ct} + \varepsilon_{ct} \quad (8)$$

This is equivalent to the system in Equations (5) and (6), adding $\tilde{z}_{ct}$ as a control variable.

In Columns (2), (3), and (4) of Table 1, we show our first-stage estimates of Equation (7) while controlling for the Borusyak and Hull (2021) "expected instrument" control, under different assumptions about the exogenous components of z_{ct}. This approach allows us, in each column, to rely on different underlying sources of variation in our measure of simulated spending. In Column (2), we construct $\tilde{z}_{ct}$, assuming that post-merger price effects and the timing of mergers within a window on two years of either side of the true date the transaction was consummated are conditionally random.[11] In Column (3), we only assume that post-merger price effects are conditionally random. Finally, in Column (4), we only assume that merger timing is random.

Across all four specifications presented in Table 1, our first-stage estimates are between 0.429 and 0.477, and the F-statistics range from 15.783 to 26.287. Results in Column (2) of Table 1, our

[10] We view the distance of hospitals from one another as largely a function of the geographic distribution of hospitals across the US, which is generally a historical accident that dates back to to the Hill-Burton hospital construction subsidies that were in place between 1947 and 1971 (Chung et al., 2017).

[11] In this approach, we deviate slightly from the above description. We assume that post-merger price effects are random conditional on deal size being above the HSR threshold **and** the year of the merger, $\lambda_{eh} = \zeta_0 + \zeta_1 \times \text{AboveHSR}_e + \sum_t \zeta_{2,t} \times 1[t = \tau_e] + \nu_{eh}$. We then construct the distribution of residuals ν_{eh} in the same way as described above. This allows the merger timing and price effects to be jointly dependent, in a way that is estimated directly.

preferred specification, suggest that a 10% increase in simulated spending leads to a 4.5% increase in health spending.

To further address potential endogeneity concerns, across our analyses, we exclude flailing firms — hospitals with low bed occupancy before mergers occur — from our sample of mergers, since those transactions may be in occurring regions experiencing economic decline that can no longer support multiple independent hospital systems. We also perform a series of robustness checks in which we exclude from our analysis counties that have low income or high unemployment in 2009, the year before our analysis period starts. We also run our analysis excluding regions experiencing economic decline in the period of 2006-2009, before our analysis period starts. We also repeat this exercise and exclude regions with high employment, high wages, high employment growth, and high wage growth from our analysis. Finally, we run a robustness check excluding counties more exposed to Chinese import competition, as described by Autor et al. (2013). These strategies allow us to gauge whether our estimated results are coming primarily from variation in counties that may be experiencing unusually extreme economic trends or differentially high or low economic performance.

6 Results

6.1 Summary Statistics

Our analysis focuses on 1,710 of 3,006 US counties from 2008 to 2017 that include 160 million individuals age 25 to 64 in the population annually (that is 96% of the total population of the contiguous US within that age group). There were approximately 31 million HCCI beneficiaries in those counties annually in our sample period. From 2010 to 2015, these counties experienced 316 hospital mergers involving 702 hospitals for which we have data on provider prices. Appendix Figure A.9 shows the counties in the continental US included in our analytic sample.

In Table 2, we show descriptive statistics for key local county characteristics for the 1,710 counties included in our analysis. Within our sample, the average health spending per beneficiary per county was $4,934 per person in 2010, which increased by 4% in real terms between 2010 and 2015. We have cleaned data on ESI premiums from the DOL 5500 forms for 1,255 of the 1,710 counties in our main analysis.[12] According to the DOL 5500 forms, average county-level insurance premiums per individual with coverage from an HCCI insurer were $4,792, and the average county-level actuarial value among HCCI policyholders was 81.2%. The weighted mean hospital HHI per county was 3,249 in 2010 and increased, on average, by 230 points (7%) between 2010 and 2015. The average income during our sample period was $30,230, the share self-employed

[12] Our results are qualitatively unchanged when we run our main analysis on the sample of counties for which we have insurance premium data.

per capita was 9%, the share unemployed per capita was 10%, and mean federal tax withholdings were $3,308. The mean county experienced 238 deaths per 100,000 annually. These included 24 deaths per 100,000 from deaths of despair and 67 deaths per 100,000 from cancer.

6.2 The Impact of Rising Health Spending on Insurance Premiums and Plans' Actuarial Value

In Columns (2) and (3) in Panel (A) of Table 3, we show the impact that rising health spending on ESI beneficiaries has on county-level insurance premiums for plans offered by HCCI insurers and the county-level mean actuarial values of plans offered to HCCI beneficiaries via estimates of 2SLS estimates of Equations (7) and (8). Our 2SLS reports health spending in log points, so a one-unit increase in log spending is a 100% increase in spending. As a result, for convenience, we focus on reporting the effect of a 10% increase in health spending (approximately $499 per person).

As we report in Column (2) in Panel (A) of Table 3, we observe that a 10% increase in health spending on ESI beneficiaries raises insurance premiums by 7.1% ($346 per person and approximately 70% of the health spending increase) ($p < 0.05$). We do not find that increases in health spending lead to statistically or economically significant changes in the actuarial value of insurance plans. Given that out-of-pocket costs account for approximately 20% of health spending, this estimate implies that rising health care spending is passed through on a nearly dollar-for-dollar basis into rising insurance premiums. Our point estimates are similarly scaled — a 7.1% increase — when we measure insurance spending across all insurers, rather than just HCCI beneficiaries. This implies that the price and spending effects we observe using HCCI data are likely generalizable to the broader health insurance market.

6.3 The Impact of Rising Health Spending on Labor Market Outcomes and Tax Withholdings

As we present in Column (1) in Panel (B) of Table 3, our 2SLS estimates show that a 10% increase in health spending on the privately insured with ESI coverage leads to a 2.5% decrease in log income ($p < 0.01$) off of a mean income of $30,230 (a $755.75 reduction per worker). To calculate the pass-through rate from rising health spending into labor income, we need to account for the fact that our spending estimates are per-beneficiary (with a denominator including all HCCI beneficiaries aged 64 or younger), whereas our labor income estimates are per-worker (with a denominator including all *workers* between the ages of 25 and 64). To adjust these comparisons, we need to estimate the share of HCCI beneficiaries who are actively participating in the labor market. The average HCCI policy includes 2.01 beneficiaries, which can include a spouse and/or children under the age of 25. If we assume none of these dependents are active participants in the labor market, our results imply a pass-through rate of health spending to income of 78%. Conversely, if we assume

that all of the HCCI dependents are active participants in the labor market, the results imply a pass-through of 99%.[13]

These estimates average over individuals who continue to receive wage income, individuals who continue to receive income but switch to self-employment, and individuals who lose their jobs entirely. We estimate effects on whether the employment status of workers changes in Columns (2) and (3) in Panel B of Table 3. In Column (2), we estimate that a 10% increase in health spending on ESI beneficiaries leads to a 0.7 percentage point increase in the share of individuals claiming unemployment benefits (with a mean share of 10%). This represents 1,183 additional individuals collecting unemployment benefits in the average sized county. In Column (3), we estimate that a 10% increase in health spending leads to a 0.9 percentage point increase in the share of individuals in the population who receive self-employment income (with a mean share of 9%). As we illustrate in Appendix Table A.2 (Columns (1) and (4)), our estimates of the effects on income and unemployment are robust to measuring labor market outcomes using employment data from the QCEW rather than from tax returns. By contrast, OLS estimates of the relationship between health spending and labor market outcomes, presented in Appendix Table A.1, do not reveal a relationship between health spending and labor market outcomes.

Collectively, the reduction in income and the increase in unemployment generated by increases in health spending on ESI enrollees leads to a reduction in federal tax withholdings. In Column (4) of Panel B in Table 3, we estimate that a 10% increase in health spending leads to a 2.8% decrease in income tax payments, $106 off a baseline average of $17,100 per worker. Scaled up to the national population, our results imply that a 1% increase in health spending on ESI enrollees would lower federal tax withholdings by $1.44 billion annually (0.284%), a 5% increase in health spending would lower federal tax withholdings by $7.22 billion annually (1.42%), and a 10% increase in health spending would lower federal tax withholdings by $14.44 billion annually (2.84%) among our sample of 1,710 of 3,006 counties.

In Table 4, we show that the decrease in income and the increase in the overall share unemployed is wholly driven by labor market changes among workers outside of the health sector. Columns (1) and (2) illustrate how increases in health spending on ESI enrollees reduce income and increase unemployment for workers *outside* of the health care sector; Columns (3) and (4) present estimates for individuals working *inside* the health care sector. These results show that a 10% increase in health spending leads to a 2.95% decrease in income for non-health sector workers and a 1.53 percentage point increase in the share unemployed. By contrast, we estimate that rising health care spending increases income for health care workers and lowers unemployment, although our estimates are statistically insignificant. As we illustrate in Appendix Table A.2, these results are

[13]OLS estimates of Equation 6 (without an instrument) produce opposite-signed and smaller effects. We present these estimates in Appendix Table A.1.

also robust to measuring sector-specific wages and employment in the QCEW.

6.4 Further Tests of Robustness

Rising health spending on the privately insured should only impact labor market outcomes for individuals employed in jobs that provide ESI. While we cannot directly measure whether individuals receive employment-based health benefits during our period of study, we can identify whether employees are enrolled in a retirement plan through the employer (either defined contribution or defined benefit) using the check box on line 13 of the W-2 form.[14] As reported in Heim et al. (2021), more than 90% of employees with employer-sponsored retirement plans are covered by ESI. We therefore use retirement plan indicators on the W-2 as a proxy for whether individuals receive ESI. In Appendix Table A.3, we estimate the effects of health care spending on labor market and tax outcomes, restricting the sample to only include individuals with retirement plan indicators.[15]

We also present a placebo test to illustrate that the labor market effects we observe are indeed driven by ex post hospital price increases generated by hospital mergers. We first run the "reduced form" regression that regresses log average county-year income against our instrument, county-year simulated spending change, with county and year fixed effects and the expected instrument control. This produces a coefficient of -0.19, consistent with our general result that rising health care spending lowers wages. We plot a binned scatter plot of these values, along with this estimated line of best fit, in blue in Panel (a) of Figure 4. We also include a placebo version of this regression in which we construct simulated spending as normal, except that we replace every price effect λ_{eh} with $\tilde{\lambda}_{eh} = \eta_0 + \eta_1 \times \text{AboveHSR}_e + \sum_t \eta_{2,t} \times 1[t = \tau_e] + \tilde{\nu}_{eh}$, where $\tilde{\nu}_{eh}$ is randomly drawn from the distribution of residuals estimated in Section 5.2. We draw $\tilde{\lambda}_{eh}$ 1,000 times, and compute the average across draws. Since this average is unrelated to the true simulated spending increase, if we regress our outcomes against it, we should estimate a coefficient of zero unless we have misspecified our regression or there is a confounding factor. As we illustrate, we estimate a precise zero. In Panel (b), we replicate this same exercise, but for a dependent variable of the county-year share of the population claiming unemployment benefits. Once again, we find a strong relationship between actual simulated spending changes and unemployment, but no relationship between placebo simulated spending changes and unemployment.

In Appendix Figures **??** and **??**, we regress county-year labor income and unemployment against the placebo simulated spending draw, and do this separately for each of our 500 draws. We plot

[14] Under the Affordable Care Act, employers with more than 250 W-2s in the prior year were required to report ESI premiums on their employees' W-2 (Box 12 code DD) starting in 2012. The ACA also required health insurance companies, employers, and all other agencies providing health insurance coverage (including ESI) to report health insurance to the IRS on forms 1095A/B/C starting in 2014 (for more information see Lurie and Pearce (2021)). Neither of these measures provide complete support over our sample period.

[15] This table to be completed in a later draft.

a histogram of the distribution of the reduced form estimates generated by these counterfactual placebo instruments. We find that our estimates for labor income and share unemployed are, respectively, in the 85th and 90th percentile of the distribution of placebo estimates.

In Figure 5, we display 2SLS estimates of Equations (7) and (8) in which we use the first stages presented in Table 1 with various versions of the Borusyak and Hull (2021) corrections, including no correction. The first row of Figure 5 shows estimates of the impact of changes in health spending on log income and the share unemployed when we do not introduce the correction and allow all the underlying variation to drive variation in health spending. Row 2 includes our baseline specification, presented in Table 3. In Row 3, we only rely on variation driven by the difference in the price effects generated across mergers. In Row 4, we only rely on the variation in the timing of hospital mergers within two years on either side of the deal. As Figure 5 illustrates, our results do not differ meaningfully as a function of the variation we rely on to drive changes in simulated spending. This is reassuring, because if we thought that one of the components we use for variation (either price effects or timing) might not be plausibly exogenous, it would be nonetheless true that using only the other provides estimates of identical magnitude. Moreover, if we were worried that both were contaminated by an endogeneity concern, it is unlikely that they would nonetheless produce identical IV estimates.

In Figure 6, we show that our results are not a function of changes that are occurring exclusively in counties with outlier labor markets. We focus on outlier status in terms of high exposure to trade from China, low baseline wages, low baseline wage growth, low baseline employment, or high baseline increases in job loss during our pre-period (2006 to 2009). We also exclude the most rural counties. Each row shows our main estimates of Equation (8) on log income and the share unemployed when we exclude specific cohorts of outlier counties, such as the 25% of counties that were most exposed to trade from China, as measured by Autor et al. (2013).[16] Likewise, we also show our results are not being driven by labor market changes occurring in the 25% most rural counties in the US. Across each of our specifications where we exclude a quarter of counties nationwide, our core result — that rising health spending decreases income and raises unemployment — remains similarly scaled.

In Appendix Figure A.10, we repeat this exercise, but instead focus outlier counties that have high wages, high employment, or extremely large gains in wages or employment. In this figure, we present results excluding the counties with the *least* exposure to trade from China, counties with the largest gains in employment and highest employment rates, counties with the largest increases in levels and highest income levels, and the least rural counties. As we illustrate, our results also remain qualitatively unchanged when we introduce these sample restrictions.

[16]We use QCEW to measure baseline wage and employment levels and trends. We use American Community Survey data on population density in 2009 to define county rural status.

6.5 The Effect of Rising Health Spending on Labor Market Outcomes Across the Income Distribution

As discussed in Section 3, rising ESI should produce larger employment effects for lower-income workers because ESI, fundamentally, should be regarded as a head tax. Likewise, the labor market effects we observe should be larger for groups of workers facing more elastic labor demand, which will likely be the case for lower-income workers. To test this proposition, in Figure 7, we estimate our primary specification on log average income and share unemployed, while restricting our sample to subsamples partitioned by deciles of the local income distribution.[17] For each three-digit ZIP code, we measure deciles of the income distribution based on our wage data from the IRS, measured in the prior year.

As we illustrate in Panel (a) of Figure 7, we see larger income increases among deciles in the upper end of the income distribution. Whereas the relationship between changes in health spending on ESI beneficiaries in log income is not statistically distinguishable from zero for the bottom five deciles, in the top half of the income distribution, we observe that a 10% increase in health spending leads to a 9% decrease in log income ($p<0.03$). By contrast, as we illustrate in Panel (b) of Figure 7, we see the converse relationship between rising health spending on ESI beneficiaries and the share of counties uninsured. Here, rising health spending leads to an increase in unemployment among individuals in the bottom half of the income distribution. Indeed, when we analyze the relationship between rising health spending and job losses, we see the largest effects concentrated among individuals in the bottom third of the income distribution.[18]

6.6 Rising Health Spending and Deaths From Job Loss

A growing literature has documented that job losses lead to premature death, particularly from overdoses and self-harm (Sullivan and von Wachter, 2009; Eliason and Storrie, 2009; Pierce and Schott, 2020; Venkataramani et al., 2020). Because of the job losses we observed that occurred after health spending increased, we also analyze whether increases in health spending led to increases in local mortality. In particular, we analyze whether increases in health spending led to increases in deaths of despair.

As we illustrate in Table 5, we find that a 10% increase in health spending per county on ESI beneficiaries leads to 3.8 additional deaths of despair off a mean county incidence of 26.5 deaths of despair annually. By contrast, we do not observe that rising health spending is associated with a statistically significant change in total mortality per county (excluding deaths of despair) or mortality from cancer. In Appendix Table A.5, we show the effects of rising health care spending on individual components of deaths of despair. Here, our results show that the increase in deaths of

[17] We focus on *local* deciles rather than national cutoffs because supply and demand elasticities are likely to reflect relative positions in the labor market.

[18] We present the actual point estimates in Appendix Table A.4.

despair we observe are primarily being driven by overdoses.

The past literature has found that there is approximately 1 death of despair per 300 to 600 job losses (Sullivan and von Wachter, 2009; Eliason and Storrie, 2009; Pierce and Schott, 2020; Venkataramani et al., 2020). The estimated death-per-job-loss rate has increased over time 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 (5) of Table 3 to scale our effect in terms of deaths per job loss induced by rising health spending. Combining our estimates from Tables 3 and 5, we observe approximately 1 death per 192 job losses in our sample. Likewise, if we rely on the employment effects estimated using the QCEW data in Appendix Table A.2, we observe approximately 1 death per 400 job losses.

In Figure 9, we show our estimates of the relationship between rising health spending and deaths of despair are similarly scaled when we rely on alternative sources of variation. As we illustrate, when we rely on all our variation, the variation from the post merger price increases only, and the variation in the timing of mergers, we get similarly-scaled elasticities. Likewise, in Figure 10, we again illustrate that are results are qualitatively unchanged when we drop particular groups of counties based on their pre-treatment period characteristics (e.g., dropping the 25% of counties most exposed to trade from China based on Autor et al. (2013) or the 25% of counties with the lowest wage growth from 2006 to 2009). In Appendix Figure A.11, we repeat this exercise, but exclude the regions with the best economic performance from 2006 to 2009. This check illustrates that our finds are not simply being driven by concurrent changes in mortality occurring in counties that are outliers in terms of economic decline or growth.

6.7 Scaling the Effect of Hospital Mergers

There were 316 hospital mergers that occurred between 2010 and 2015. In this section, we quantify the economic and social harm from these mergers by estimating their cumulative effects on health spending, insurance premiums, income, employment, tax revenue, and mortality. Our estimates reveal that the 316 mergers that occurred between 2010 and 2015 raised health spending by $931 million, lowered total income by $745 million, led to 6,882 job losses, lowered tax revenue by $86 million, and led to 36 deaths of despair. The US Department of Transportation (DOT) estimates the value of a statistical life in 2015 at $9.6 million (Department of Transportation, 2022). As a result, the 36 deaths we observe imply a loss of life with an economic value of $349 million. This implies the collective harm from these transactions is over $1 billion ($745 million in forgone wages and $349 million from deaths). For context, the annual budget of the FTC, the federal agency tasked with regulating hospital mergers, was $293 million in 2015 overall, with $127 million purely dedicated to antitrust enforcement.

We can also quantity the effect of classes of hospital mergers based on the increases in hospital

prices they generate. For example, our estimates suggest that, on average, a merger that raises hospital prices by 5% or more will lead to approximately a $25 million reduction in wages, 233 job losses, a $3.4 million reduction in federal tax withholdings, and 1.2 deaths of despair. As a result, using the DOT's value of a statistical life in 2015, we estimate that mergers that raise hospital prices by 5% or more lead to economic losses from deaths of despair alone that are valued at $11.5. Such mergers account for 35% of the merging hospitals in our sample.

7 Discussion and Concluding Thoughts

This paper analyzes the effect of rising health spending on insurance premiums, labor market outcomes, tax revenue, and mortality. We use the price increases driven by 316 hospital mergers that occurred between 2010 and 2015 to construct an instrument for changes in local health spending. We observe that hospital mergers lead to increases in hospital prices without concurrent changes in the quality or quantity of care provided. As a result, our findings speak to specific harms created by hospital mergers, as well as the more general consequences of rising health spending in the US. We believe our results speak particularly to the drivers of health spending growth in the US, like surprise medical billing (Cooper et al., 2020), which raise providers' prices without improving clinical quality.

We find nearly complete pass-through of rising health spending into increases in insurance premiums and income. Ultimately, we find that a 10% increase in health spending lowers wages by 2.5%, leads to a 8.2 percentage point increase in individuals with self-employment income, a 0.7 percentage point increase in the share receiving unemployment insurance, and a 2.8% reduction in federal income tax revenue. These results imply that between 80 and 92% of the incidence of rising health spending falls on workers directly. We also find that a 10% increase in health spending leads to 3.8 "deaths of despair" per 100,000. Past literature has suggested that between 1 in 300 and 1 in 500 individuals who lose their job die from suicides or overdoses shortly after their job separation (Venkataramani et al., 2020; Sullivan and von Wachter, 2009). When we combine our mortality estimates with our estimates of employment, our results are similarly scaled and imply that, in our setting, between 1 in 200 and 1 in 400 individuals who lose their jobs die from a suicide or overdose.

Our findings are distinct from past work, which has found that increases in health spending generally lead to statistically significant decreases in wages but no effects on employment (e.g., Gruber and Krueger (1991) and Baicker and Chandra (2005). We posit that the employment effects we observe are a function of both the health spending shocks we rely on in our identification strategy and rich data we use in our analysis. In the past, it was assumed that if increases in the cost of benefits were fully valued by workers, then the pass-through of rising costs of fringe benefits to wages would be 100%, with no employment effects (Summers, 1989). However, in general, we

do not believe that individuals likely fully value the increase in health spending caused by hospital mergers. Our results show that the increases in spending generated by hospital mergers result from price increases without concurrent changes in quality or quantity. Likewise, while past research primarily relied on survey measures of labor market outcomes, with limited sample sizes, we are instead able to utilize the universe of individual and firm tax returns filed with the IRS, which gives us a more nuanced picture of labor market outcomes in the US and the statistical power to measure labor market effects for individuals at different positions in the income distribution.

A crucial finding from our work is that when health spending goes up, individuals in the bottom third of the income distribution bear the greatest burden of the increases and see their employment opportunities decrease. In the past, employer-sponsored insurance has been likened to a regressive head tax because, for the most part, health insurance premiums do not vary proportionally across individuals of different salaries within the same firm (Case and Deaton, 2021; Saez and Zucman, 2019). Our result is consistent with these assertions.

Over the last two decades, the US has experienced approximately 1,000 mergers among the nation's 5,000 hospitals. This has led to calls for greater antitrust enforcement, increases in funding for antitrust enforcement agencies, and regulation of hospitals' prices (Chernew et al., 2020; Dafny, 2021; Baer et al., 2020). Our estimates allow us to quantify the specific harm from individual hospital mergers and the cumulative impact mergers have had on labor markets and mortality in the US. Our analysis highlights that the harm from hospital mergers in the US has been substantial. The hospital industry accounts for less than 6% of GDP. We estimate that the monetary harms from a single year of mergers, measured via the increases in spending they produce, are larger than the entirety of the FTC's annual budget.

We also find that individual mergers can generate significant harm in non-health sector labor markets for nearby workers. For example, we estimate that a merger that raises hospital prices by 5% will lead to approximately a $25 million reduction in wages, 233 job losses, a $3.4 million reduction in federal tax withholdings, and 1.2 deaths of despair. As a result, using the DOT's value of a statistical life in 2015, we estimate that mergers of this scale would generate economic losses from deaths of despair alone that would be valued at $11.5 million.

There are widespread concerns that the federal antitrust agencies are substantially underfunded. Our suggest that the harms from hospital mergers — mergers in a sector that accounts for 6% of GDP — are markedly larger than the entirety of the FTC's budget. In 2015, the total budget of the FTC was $292 million and their antitrust enforcement budget was $127 million. Cumulatively, we analyze that the 316 hospital mergers that occurred during our sample period from 2010 to 2015 led to a $931 million increase in health spending, $745 million in forgone income, 6,882 job losses, $86 decrease in federal income tax revenue, and 36 "deaths of despair" (which have a monetary value of $349 million). As a result, our results imply that the monetary value of the harm from hospital

mergers — mergers in a sector that counts for 6% of US GDP — was larger than the entirety of the FTC's antitrust enforcement budget.

Going forward, we hope this research motivates future work analyzing the absolute and distributional consequences of rising health spending in the US. Likewise, more research is needed to assess who benefits when health spending goes up and who receives the rents from hospital mergers. Finally, we hope this work highlights that in the absence of concrete steps to restrain the growth in health spending, rising health care costs in the US will lower wages and and lead to job losses for the lowest earners in the nation.

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Table 1: First Stage — Simulated Spending as an Instrument for Health Spending

	Log Health Spending per Beneficiary **(1)**	Log Health Spending per Beneficiary **(2)**	Log Health Spending per Beneficiary **(3)**	Log Health Spending per Beneficiary **(4)**
Simulated Spending	0.461*** (0.091)	0.448*** (0.092)	0.429*** (0.092)	0.477*** (0.127)
Variation	All (Timing, Price Effect, Spending Share, HSR Threshold)	Timing, Price Effect	Price Effect	Timing
Observations	17,100	17,100	17,100	17,100
F Stat	28.494	26.287	24.055	15.783

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table presents first-stage estimates of Equation (5) for Column (1) and Equation (7) for Columns (2), (3), and (4), where the dependent variable is logged, annual, county-level health spending per beneficiary and the independent variable is the simulated predicted percent change in annual spending per beneficiary. Both measures of spending are generated using data from HCCI. Standard errors are included in parentheses and are clustered at the county level. Each set of first-stage estimates includes county and year fixed effects. Columns (2), (3), and (4) each include a control, following Borusyak and Hull (2021), which adjusts for the potentially-endogenous components of the instrument, as discussed in Section 5.2.

Table 2: Summary Statistics

Panel A: 2010 Levels

	Mean (1)	SD (2)	p10 (3)	p25 (4)	p50 (5)	p75 (6)	p90 (7)	N (8)
HCCI Beneficiaries	13,831	38,444	836	1,314	2,810	9,067	30,267	1,710
Spending per HCCI Beneficiary	4,934	1,046	3,761	4,239	4,807	5,556	6,314	1,710
Actuarial Value	0.73	0.04	0.69	0.71	0.73	0.75	0.78	1,710
Insurance Premiums All Payors	4,792	1,281	3,348	3,996	4,702	5,515	6,413	1,255
Insurance Premiums HCCI Payors	4,931	1,196	3,243	4,209	4,872	5,871	6,989	1,255
Hospitals per County	2.04	3.30	1.00	1.00	1.00	2.00	4.00	1,710
Hospital HHI	3,249	2,231	446	1,996	2,916	4,176	5,823	1,710
Income	30,230	8,642	20,800	24,200	28,700	34,200	41,400	1,710
Share Unemployed	0.10	0.03	0.06	0.08	0.10	0.12	0.15	1,710
Share Self-Employed	0.09	0.02	0.06	0.07	0.08	0.10	0.11	1,710
Federal Income Tax Withholding	3,308	1,309	1,900	2,400	3,000	3,800	4,900	1,710
Population	169,019	410,280	21,641	33,148	59,580	143,679	381,098	1,710
Population Density Per Square Mile	443.56	2,317.58	30.30	52.64	99.31	259.00	716.04	1,710
Deaths of Despair per 100k	23.90	12.15	11.49	15.99	22.05	29.25	38.28	1,710
All Deaths per 100k	238.44	70.09	153.53	186.54	231.95	283.41	331.82	1,710
Cancer Deaths per 100k	67.00	19.22	44.41	53.82	65.00	78.40	91.96	1,710

Panel B: Trends 2010-2015

	Mean (1)	SD (2)	p10 (3)	p25 (4)	p50 (5)	p75 (6)	p90 (7)	N (8)
Spending per HCCI Beneficiary	0.04	0.18	-0.15	-0.06	0.03	0.13	0.24	1,710
Insurance Premiums	0.11	0.29	-0.18	-0.04	0.08	0.21	0.44	1,255
Hospital HHI	230	782	-185	0	80	367	877	1,710
Income	0.09	0.05	0.03	0.06	0.08	0.11	0.14	1,710
Share Unemployed	-0.06	0.02	-0.09	-0.07	-0.05	-0.04	-0.03	1,710
Share Self-Employed	0.00	0.00	-0.00	-0.00	0.00	0.00	0.01	1,710
Federal Income Tax Withholding	0.20	0.07	0.12	0.16	0.20	0.25	0.29	1,710
Deaths of Despair per 100k	4.84	12.58	-9.25	-1.78	4.38	11.69	19.49	1,710

Note: This table reports summary statistics for the 1,710 counties used in our analysis. These counties capture 95% of the US population age 25 through age 64 in the continental US. These include data from HCCI, the DOL 5500 forms, the American Hospital Association, IRS tax returns, the Census, the CDC, and the American Community Survey. We have rounded the county-level variables derived from the IRS data to the nearest thousand.

Table 3: The Impact of Changes in Health Spending on Insurance and Labor Market Outcomes

Panel A: Insurance Market Outcomes

	Log Premiums All Payors (1)	Log HCCI Payor Premiums (2)	Actuarial Value (3)
Log Health Spending per Beneficiary	0.705**	0.705***	-0.050
	(0.284)	(0.280)	(0.051)
Mean	$4,792	$4,931	0.730
10% Spending Increase ($499)	$346	$352	-0.005
Observations	12,550	12,550	17,100

Panel B: Labor Market Outcomes

	Log Income (1)	Share Unemployed (2)	Self-Employed (3)	Log Withholding (4)
Log Health Spending per Beneficiary	-0.249***	0.073**	0.081**	-0.284***
	(0.087)	(0.029)	(0.030)	(0.103)
Mean	$30,230	0.10	0.09	$3,308
10% Spending Increase ($499)	-$787	0.007	0.009	-$106
Observations	17,100	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are 2SLS estimates of Equations (7) and (8) with logged simulated spending as the instrument. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. The data are measured at the county-year level. Insurance premiums are measured using data from the DOL 5500 forms. Premium results are limited to the 1,255 of 1,710 counties in our analytic sample with premium data available. Actuarial values are measured using data from HCCI. We measure labor market outcomes using tax returns from the IRS. Health spending is measured using data from HCCI. The point estimates report the impact of a log point (100%) increase in health spending. In Appendix Table A.2, we repeat the analysis from Panel B using labor market data from the QCEW.

Table 4: Health Spending and Labor Market Outcomes for Health Care and Non-Health Care Workers

	Log Income		Share Unemployed	
	Non-Health Care Workers **(1)**	Health Care Workers **(2)**	Non-Health Care Workers **(3)**	Health Care Workers **(4)**
Log Health Spending per Beneficiary	-0.295***	0.168	0.153**	-0.019
	(0.095)	(0.133)	(0.071)	(0.036)
10% Spending Increase ($499)	-$837	$53	0.015	-0.002
Observations	17,100	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are 2SLS estimates of Equations (7) and (8) with simulated spending as the instrument. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. We measure wages outcomes using tax returns from the IRS, unemployment using data from the QCEW, and health spending via HCCI. All data are measured at the county-year level. The point estimates report the impact of a log point (100%) increase in county-level health spending. In Appendix Table A.2, we repeat this analysis using labor market outcomes measured using data from the QCEW.

Table 5: The Impact of Changes in Health Spending on Deaths of Despair

	Deaths per 100k		
		Placebos	
	"Deaths of Despair" (Age 25-64) **(1)**	All Deaths (Excl. "Deaths of Despair") **(2)**	Cancer Deaths **(3)**
Log Health Spending per Beneficiary	38.423**	12.933	2.824
	(17.706)	(44.543)	(22.631)
Mean	23.09	238.44	67.00
10% Spending Increase ($499)	3.842	1.293	0.282
Observations	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are 2SLS estimates of Equations (7) and (8) with simulated spending as the instrument. All data are measured at the county-year level. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. Data is at the county-year level. Our mortality measures are constructed using data from the CDC and health spending data comes from HCCI. The point estimates report the impact of a log point (100%) increase in county-level health spending. In Appendix Table A.5, we present estimates of "deaths of despair" per 100,000 population, broken down by cause.

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

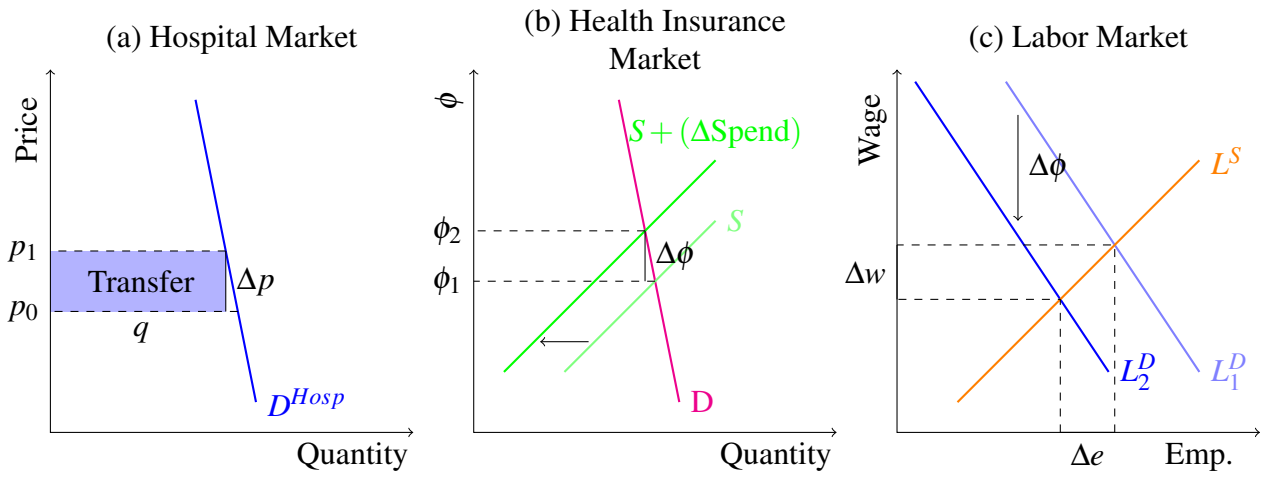
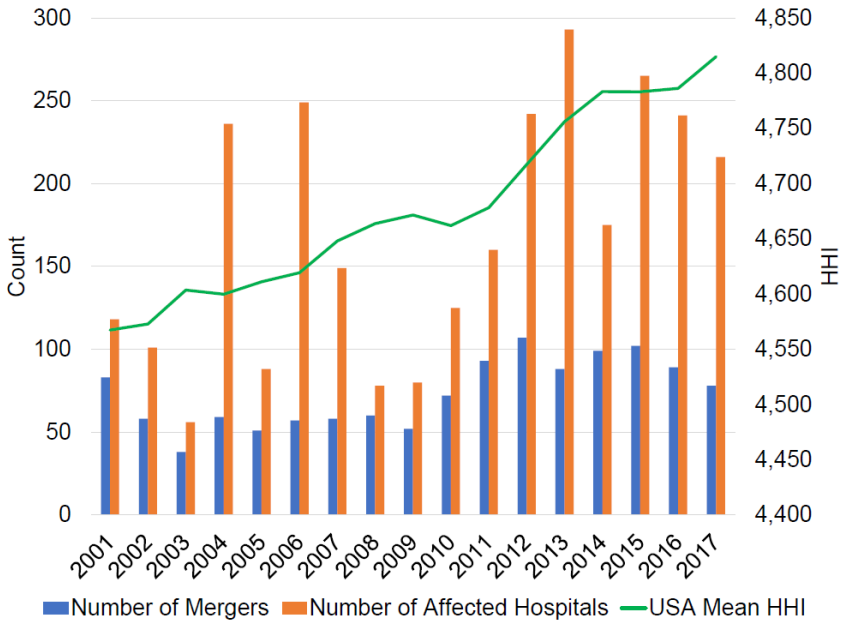
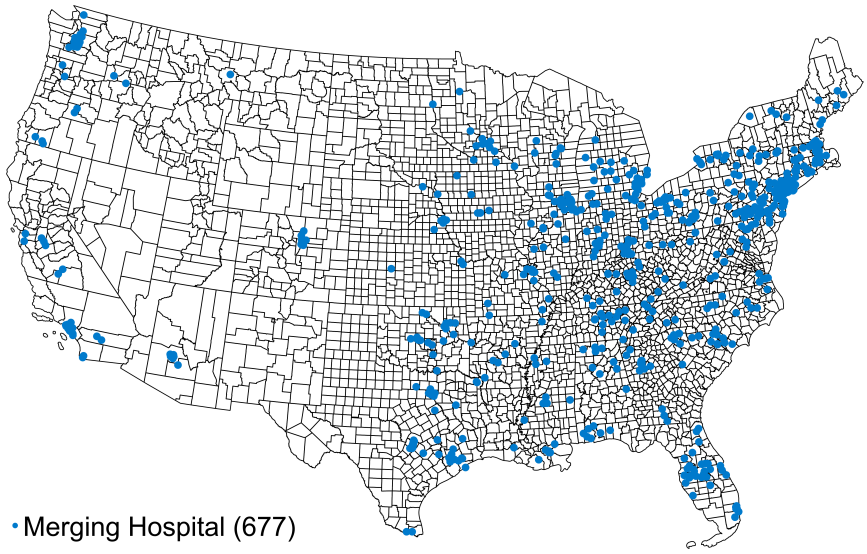


Figure 2: The Timing and Location of Hospital Mergers

(a) Hospital Mergers, 2001 — 2017



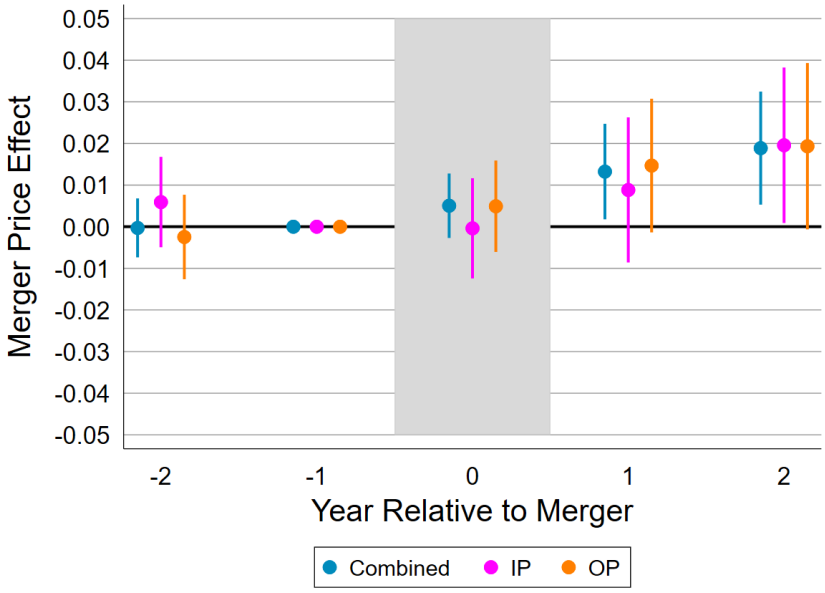
(b) Location of Hospital Mergers in the 2010 — 2015 Analytic Period



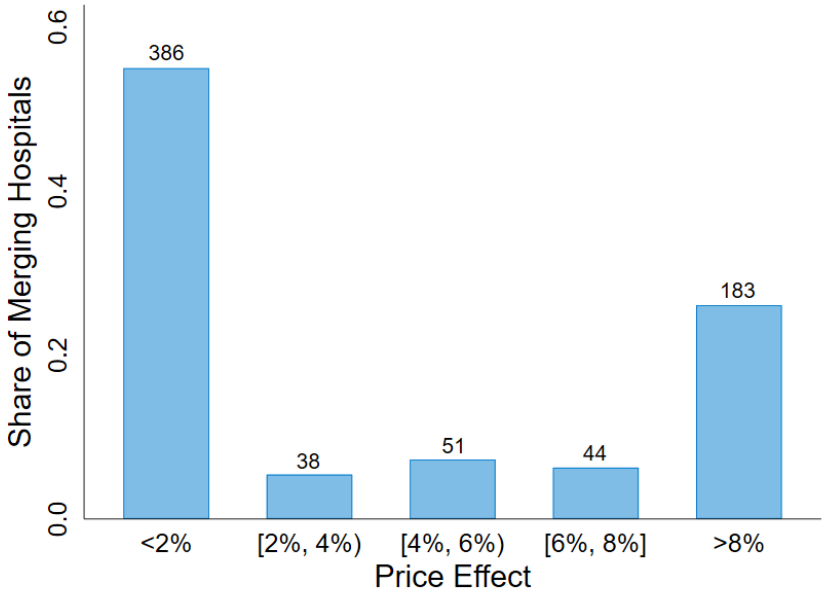
Note: Panel A presents the number of hospital mergers and count of hospitals involved in a merger from 2001 to 2017. We limit the results to mergers involving parties within 50 miles of each other. The mean HHI is the mean of hospital-defined HHIs. For each hospital, we construct a market for care based on a 30-minute drive time from the hospital. We measure the size of each hospital's market as the total number of inpatient beds within that 30-minute area. Each hospital's market share is its count of inpatient beds divided by the total number of inpatient beds in its market. In Panel B, each dot denotes a hospital involved in a merger.

Figure 3: The Impact of Hospital Mergers on Hospital Prices

(a) The Impact of Hospital Mergers on Inpatient, Outpatient, and Combined Inpatient and Outpatient Prices

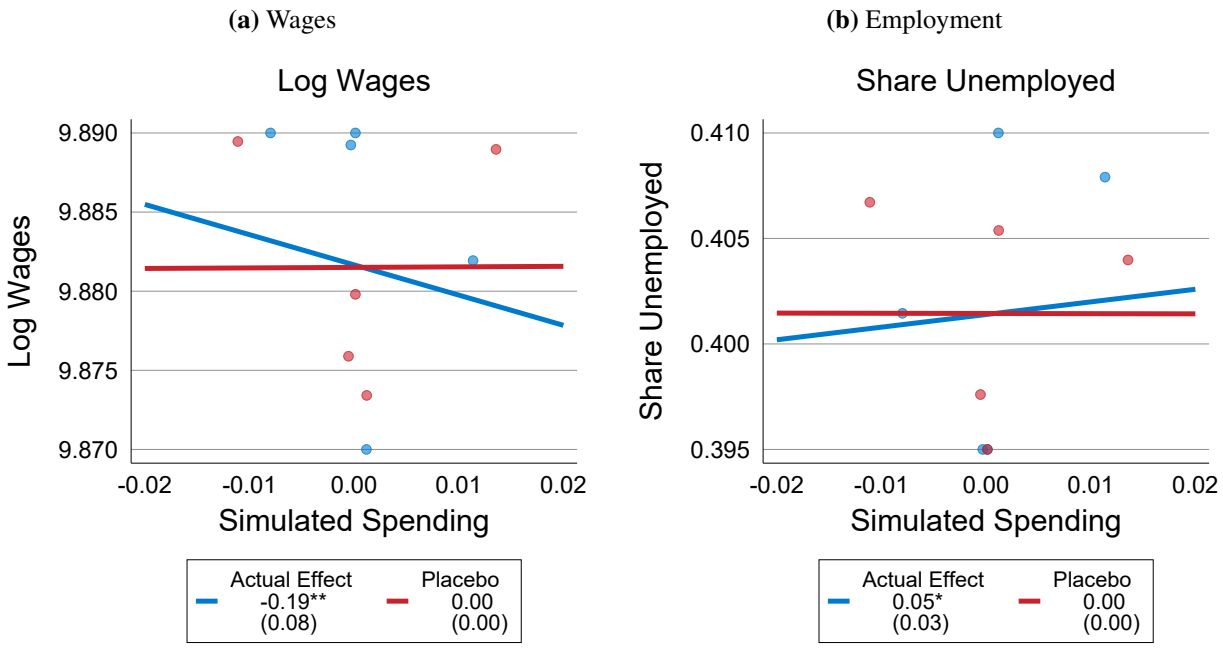


(b) The Distribution of Hospital Price Increases Generated by Hospital Mergers



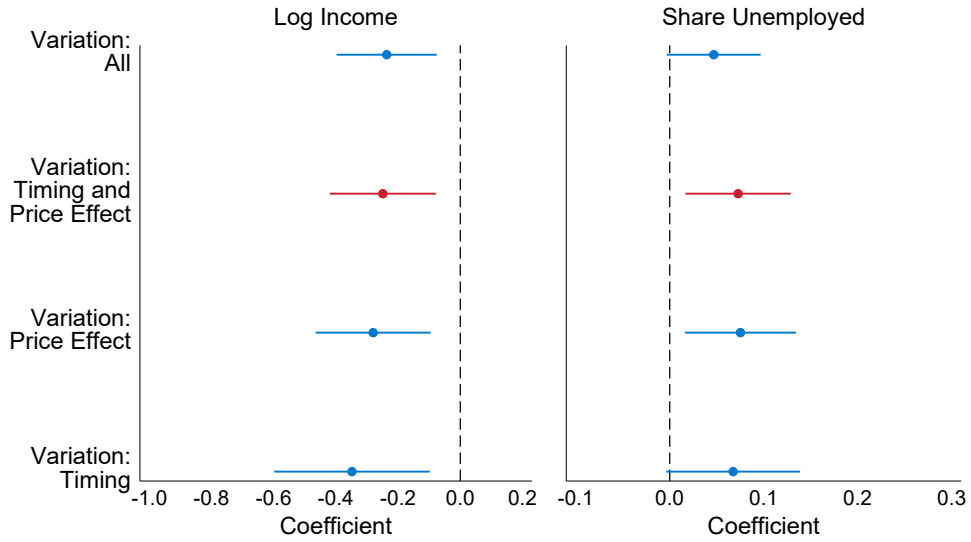
Note: Panel (a) presents estimates of Equation (1) on our sample of mergers involving targets and acquirers located less than 50 miles from each other. Each dot represents a point estimate and the corresponding 95% confidence interval. In Panel (b), we report the distribution of hospital-specific price increases generated by the hospital mergers. Hospital prices are measured using data from HCCI.

Figure 4: Placebo and Actual Estimates of the Relationship Between Health Spending and Labor Market Outcomes



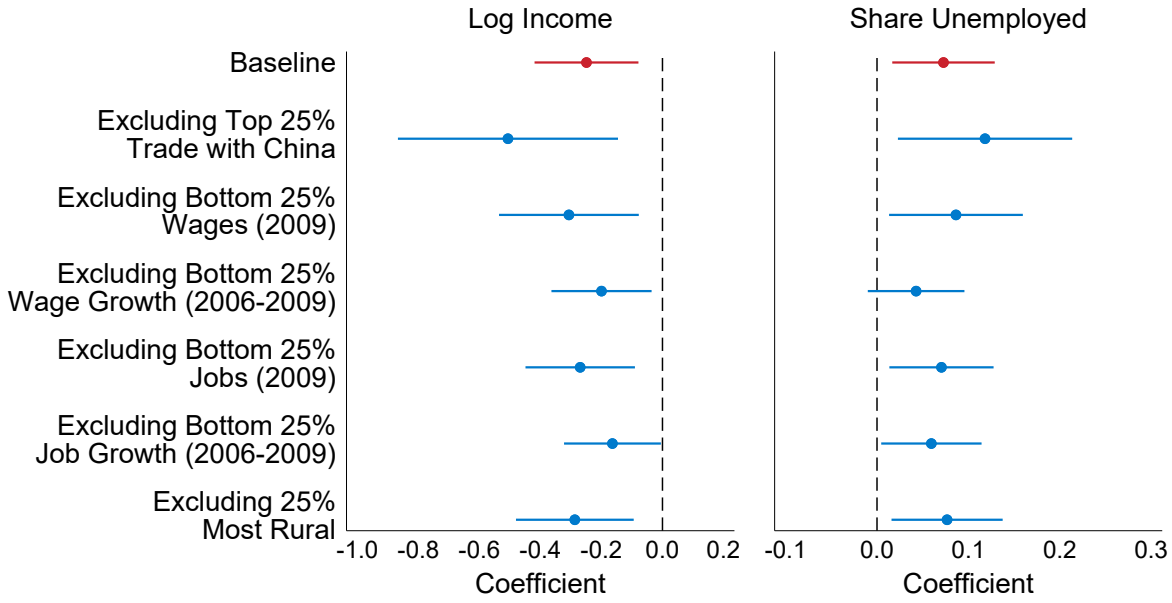
Note: The scatter plots present OLS regressions of the dependent variable against true and placebo simulated spending. The OLS regressions in both panels include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers as described in Section 5.2. The standard errors in our estimates are clustered at the county level. The data used in the analysis is at the county-year level. The figure includes health spending data from HCCI and labor market outcomes measured using data from the QCEW.

Figure 5: The Robustness of Labor Market Estimates to Alternative Sources of Variation



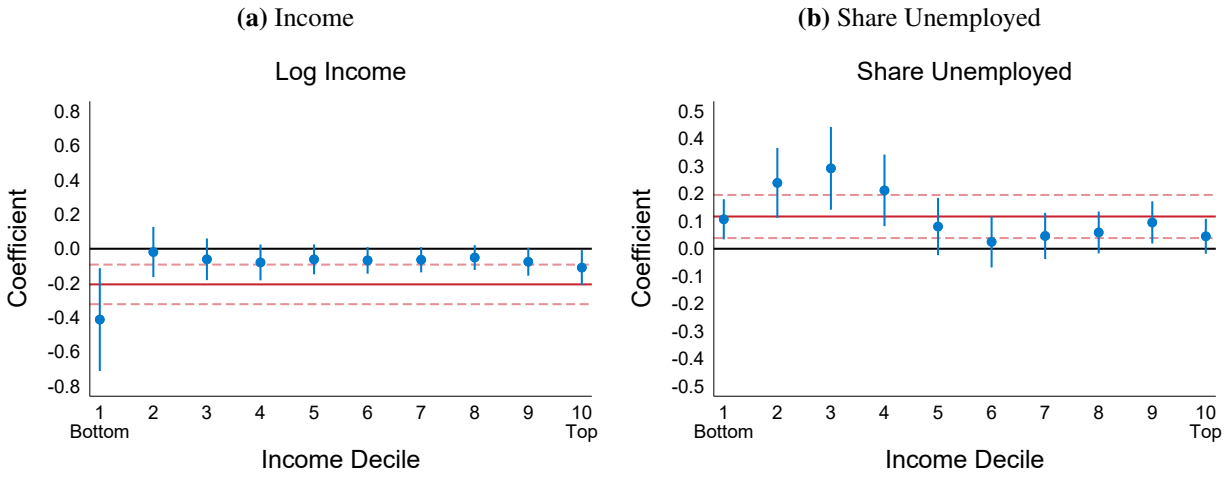
Note: Each row presents our 2SLS estimate of health spending on log income and the share unemployed, respectively. Dots are point estimates from our 2SLS; the lines represent our 95% confidence intervals. The first row presents estimates of Equation (6), in which we rely on all variation in simulated spending to estimate the relationship between health spending, income, and unemployment. The second row presents estimates from Table 3 via estimates of Equation (8). In rows 3 and 4, we present estimates of Equation (8) and rely on variation in simulated spending driven exclusively by the price variation generated by mergers (row 3) and the variation in simulated spending driven exclusively by the variation in the timing of mergers (row 4).

Figure 6: The Robustness of Labor Market Estimates to Excluding Rural Counties, Counties Exposed to More Trade from China, and Counties with Lower Economic Performance



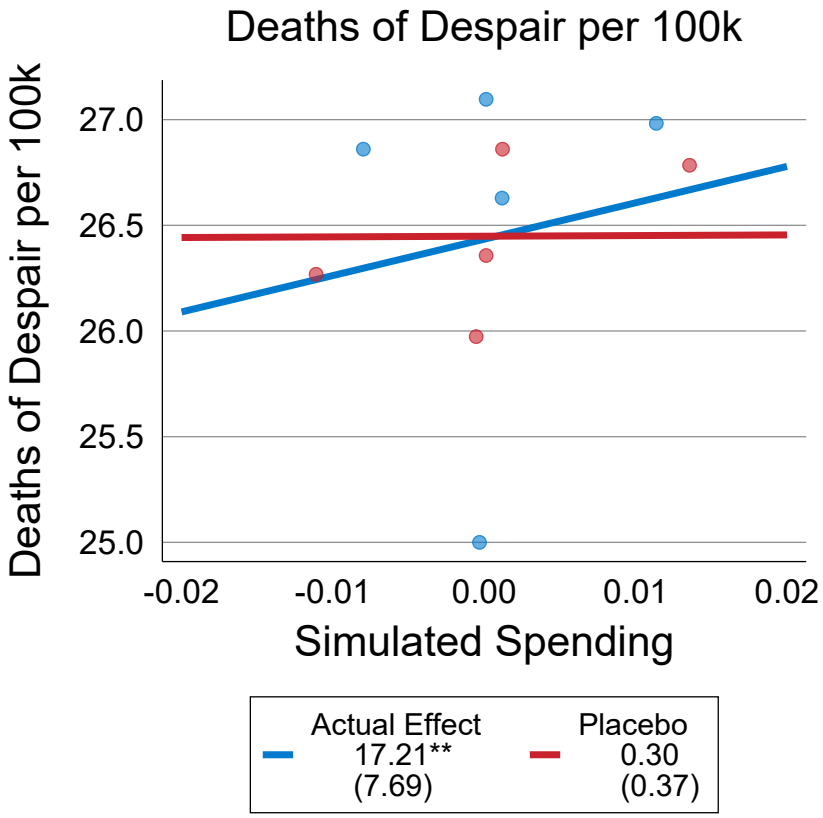
Note: Each row presents our 2SLS estimates of Equation (8) on labor market outcomes. Dots are point estimates from our 2SLS; the lines represent the 95% confidence intervals for each regression. Row 1 presents our baseline estimates of Equation (8) from Table 3. Subsequent rows present 2SLS estimates of Equation (8) on log income and the share unemployed after excluding cohorts of counties from our analysis. In Row 2, we exclude the 25% of all counties that, according to data from Autor et al. (2013), were most exposed to trade from China from 2000 to 2007 based on data from Autor et al. (2013). In row 3, we exclude counties in the bottom 25% of wages nation-wide in 2009 according to the QCEW. In row 4, we exclude counties with the lowest wage growth from 2006 to 2009 according to the QCEW. In row 5, we exclude counties with the lowest employment rate in 2009 according to the QCEW. In row 6, we exclude counties with the biggest declines in employment from 2006 to 2009 according to the QCEW. In row 7, we exclude the 25% of the most rural counties in 2009 based on data from the Census. We measure spending using data from HCCI and labor market outcomes using tax returns from the IRS.

Figure 7: The Effect of Rising Health Spending on Labor Market Outcomes Across Individuals at Different Positions in the Income Distribution



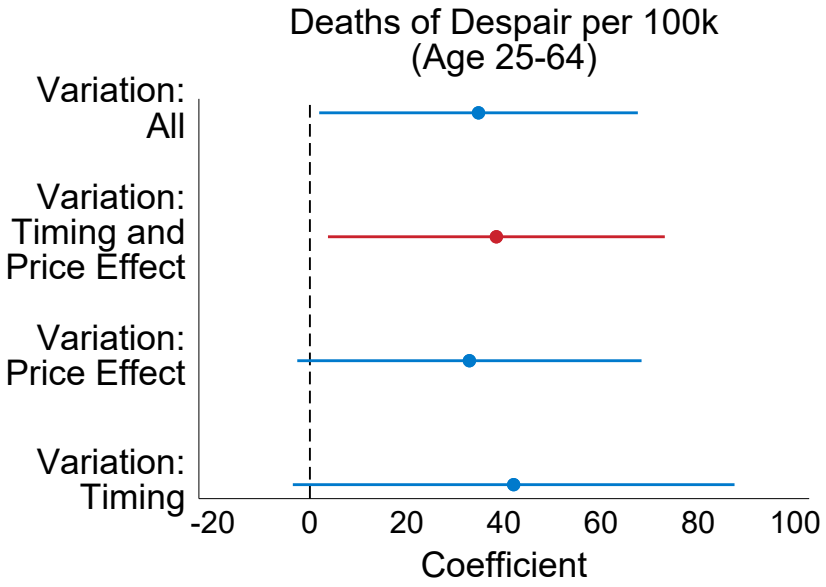
Note: Each dot and line represent the point estimates and 95% confidence intervals from 2SLS estimates of Equation (8) on log income and the share unemployed. Each column represents a separate estimate of Equation (8) on a sub-sample of our population, including each decile of the income distribution. The deciles are defined based on wage and self-employment income in each 3-digit zip code in 2008. Individuals are categorized into deciles in each year based on their income in the prior year, after adjusting for inflation using the CPI. Labor market outcomes are measured using IRS tax returns, and health spending is measured using data from HCCI. We present the individual point estimates for each regression in Appendix Table A.4.

Figure 8: Placebo and Actual Estimates of the Relationship Between Health Spending and Deaths of Despair



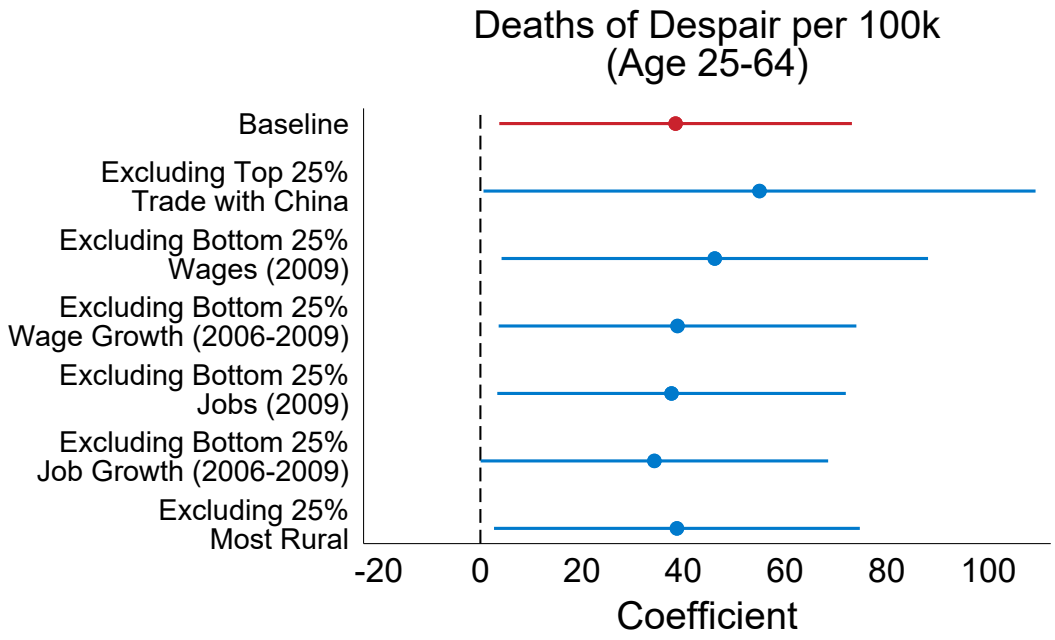
Note: The scatter plots present OLS regressions of the dependent variable against true and placebo simulated spending. The OLS regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers as described in Section 5.2. The standard errors for our estimates are clustered at the county level. The data used in the analysis is at the county-year level. The figure relies on health spending data from HCCI and mortality data from the CDC.

Figure 9: The Robustness of Mortality Estimates to Alternative Sources of Variation



Note: Horizontal lines are 95% confidence intervals. All models are 2SLS with simulated spending as the instrument. Regressions include county and year fixed effects. Rows 2, 3, and 4 contain a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level. Data is at the county-year level.

Figure 10: The Robustness of Mortality Estimates to Excluding Rural Counties, Counties Exposed to More Trade from China, and Counties with Lower Economic Performance



Note: Each row presents our 2SLS estimates of Equation (8) estimating the impact of increases in health spending on "deaths of despair" per 100,000 population. Dots are point estimates from our 2SLS; the lines represent the 95% confidence intervals for each regression. Row 1 presents our baseline estimates of Equation (8) from Table 5. Subsequent rows present 2SLS estimates of Equation (8) on mortality after excluding cohorts of counties from our analysis. In row 2, we exclude the 25% of counties that, according to data from Autor et al. (2013), were most exposed to trade from China from 2000 to 2007 based on data from Autor et al. (2013). In row 3, we exclude counties in the bottom 25% of wages nation-wide in 2009 according to the QCEW. In row 4, we exclude counties with the lowest wage growth from 2006 to 2009 according to the QCEW. In row 5, we exclude counties with the lowest employment rate in 2009 according to the QCEW. In row 6, we exclude counties with the biggest declines in employment from 2006 to 2009 according to the QCEW. In row 7, we exclude the 25% of the most rural counties in 2009 based on data from the Census. We measure spending using data from HCCI and labor market outcomes using tax returns from the IRS.

ONLINE APPENDICES

A Measuring Prices in the HCCI Data

Estimating post-merger price effects, as described in Section 5, requires a measure of each hospitals' price at a given point in time. 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

$$ln(p_{idht}) = \alpha_{ht} + \beta X_i + \pi_{dt} + \varepsilon_{idht}. \tag{9}$$

X_i includes controls for each patient's age and gender. $\pi_d t$ 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 (9) 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:

$$p_{ht}^{INDEX} = \hat{\alpha}_{ht} + \hat{\beta}\overline{X} + \hat{\pi}_{dt}\overline{dt}. \tag{10}$$

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 under 65 years old. We also limit our sample to individuals with a valid Diagnostic-Related Group (DRG) code, which we use to define the π_{dt} fixed effect.

For the outpatient sample, we apply similar restrictions, limiting our sample to patients under 65. To ensure the prices we measure are complete — not payments negotiated as a bundl 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 π_{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 Premiums in the Form 5500

The Form 5500 is a regulatory filing required of all employers that offer a benefit plan to at least 100 employees. 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 a county-year measure of average premiums.

B.1 Sample Construction and Premium Measurement

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. To deal with these quality issues, we implement a series of data restrictions and cleaning steps described below.

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

As noted above, employers can change in both absolute size and health plan enrollment from year to year. We therefore standardize premiums using the total number of plan participants to compute average premiums per person per year. This ensures that the premium fluctuations we observe are related to changes in price, rather than fluctuations in firm size. This standardization also lends itself to more straightforward comparisons with our health spending estimates, which are calculated on a per person, per year basis.

B.2 Constructing a County-Year Premium Index

A key feature of the employer sponsored insurance market is that premiums vary widely across employers due to variation in customized plan design and employees' consumption patterns (Craig, 2022). A range of data quality issues may make it difficult to observe a premium observation in any given year. Moreover, employer panels may be incomplete if firms enter or exit the market during our sample period, experience idiosyncratic shocks to employer size that place them below filing thresholds, or fail to comply with filing requirements in a given year.

As a result, the raw average of premiums across employers in a county will be a function of which employers are observed in each year. Measurement error driven by compositional changes in the mix of employers will attenuate our estimates of passthrough. Although it would be possible to limit the sample to a balanced panel of employers, such a restriction would meaningfully reduce our sample size and limit the number of counties we could represent.

Instead, we construct a county-year premium index that adjusts for employer-specific differences in plan design and health care utilization. This exercise is conceptually similar to the price-index adjustments described in Appendix A, where instead of adjusting for the mix of services provided at a hospital, we adjust the county-year premium index for the mix of employers in the data. For employer i in county c in year t, we regress premiums on a county-year fixed effect (θ_{ct}), controlling for an employer fixed effect (θ_i):

$$log(prem_{ict}) = \theta_{ct} + \theta_i + \varepsilon_{ict}$$

We then take average, adjusted premiums to be the re-transformed estimates of θ_{ct}:

$$prem_{ct}^{INDEX} = e^{\hat{\theta}_{ct}+\overline{\theta}_i}$$

where $\overline{\theta}_i$, re-scales our index values relative to average premiums across employers in the data.

C Additional Tables and Figures

Table A.1: OLS Regression of Health Spending on Labor Market Outcomes: Without Instrumenting using Simulated Spending

	Log Income **(1)**	Share Unemployed **(2)**	Self-Employed **(3)**	Log Withholding **(4)**
Log Health Spending per Beneficiary	0.000	0.002	0.001	-0.001
	(0.012)	(0.006)	(0.017)	(0.013)
Mean	$30,314	0.12	0.11	$3,346
Observations	17,100	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are OLS estimates of logged health spending on labor market outcomes. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. The data are measured at the county-year level. We measure labor market outcomes using tax returns from the IRS. Health spending is measured using data from HCCI. The point estimates report the impact of a log point (100%) increase in health spending.

Table A.2: Labor Market Outcomes for Health Care and Non-Health Care Workers: Robustness using QCEW

	Log Wages			Share Unemployed		
	Overall	Non-Healthcare Workers	Healthcare Workers	Overall	Non-Healthcare Workers	Healthcare Workers
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Log Healthcare Spending per Beneficiary	-0.471**	-0.792***	0.563	0.134*	0.153**	-0.019
	(0.217)	(0.248)	(3.064)	(0.080)	(0.071)	(0.036)
10% Spending Increase ($499)	-$963	-$1248	$ 276	0.013	0.015	-0.002
Observations	17,100	17,100	17,100	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are 2SLS estimates of Equations (7) and (8) with simulated spending as the instrument. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. We measure labor market outcomes using the QCEW and health spending via HCCI. All data are measured at the county-year level. The point estimates report the impact of a log point (100%) increase in county-level health spending.

Table A.3: The Impact of Changes in Health Spending on Labor Market Outcomes: Excluding individuals without retirement benefits

	Log Income **(1)**	Share Unemployed **(2)**	Self-Employed **(3)**	Log Withholding **(4)**
Log Health Spending per Beneficiary				
Mean 10% Spending Increase ($499)				
Observations	17,100	17,100	17,100	17,100

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models are 2SLS estimates of Equations (7) and (8) with simulated spending as the instrument. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. The data are measured at the county-year level. Insurance premiums are measured using data from the DOL 5500 forms. Premium results are limited to the 1,255 of 1,710 counties in our analytic sample with premium data available. Actuarial values are measured using data from HCCI. We measure labor market outcomes using tax returns from the IRS. Health spending is measured using data from HCCI. The point estimates report the impact of a log point (100%) increase in health spending.

Table A.4: The Effect of Rising Health Spending on Labor Market Outcomes Across Individuals at Different Positions in the Income Distribution

Decile	Log Income (1)	Share Unemployed (2)
First	-0.412***	0.107***
	(0.153)	(0.037)
Second	-0.019	0.240***
	(0.075)	(0.065)
Third	-0.061	0.292***
	(0.062)	(0.077)
Fourth	-0.080	0.212***
	(0.053)	(0.066)
Fifth	-0.062	0.081
	(0.044)	(0.053)
Sixth	-0.068*	0.025
	(0.039)	(0.047)
Seventh	-0.064*	0.046
	(0.037)	(0.043)
Eighth	-0.051	0.059
	(0.037)	(0.039)
Ninth	-0.076*	0.096**
	(0.041)	(0.039)
Tenth	-0.110**	0.045
	(0.052)	(0.032)
Uncategorized	-0.103**	-0.002
	(0.181)	(0.021)

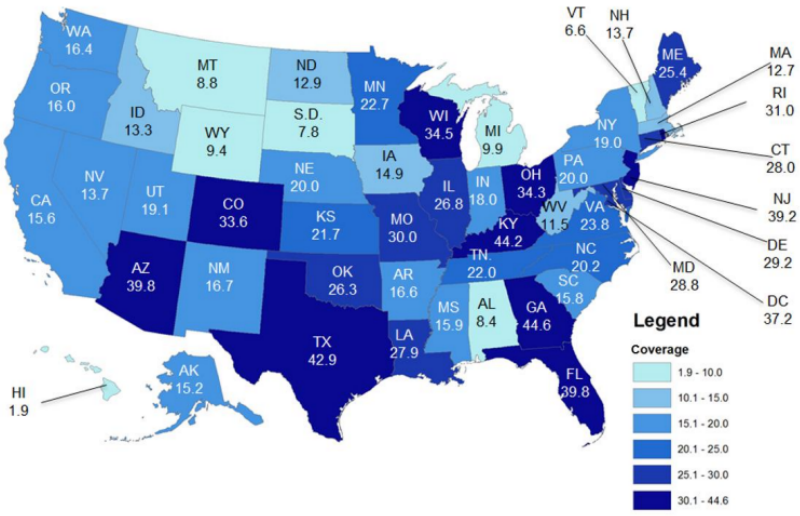
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The deciles are defined locally based on 3-digit zip code. Individuals are categorized into deciles in each year based on their income in the prior year. Labor market data comes from IRS tax returns and health spending data comes from HCCI.

Table A.5: The Impact of Changes in Health Spending on Deaths of Despair by Category

	Deaths per 100k					
	Suicides	Accidental Poisonings	Poisoning of Un-Determined Intent	Other Un-Determined Intent	Prescription Drug Overdose	Liver Disease
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
IV Effect	-1.114	41.557***	-5.412*	-0.011	-0.360*	-2.273
	(7.165)	(10.856)	(2.808)	(0.945)	(0.189)	(5.776)
Mean	12.366	12.748	1.134	0.193	0.008	8.048
10% Spending Increase ($499)	-0.0111	4.156	-0.541	-0.001	-0.036	-0.227
Observations	17,090	17,090	17,090	17,090	17,090	17,090

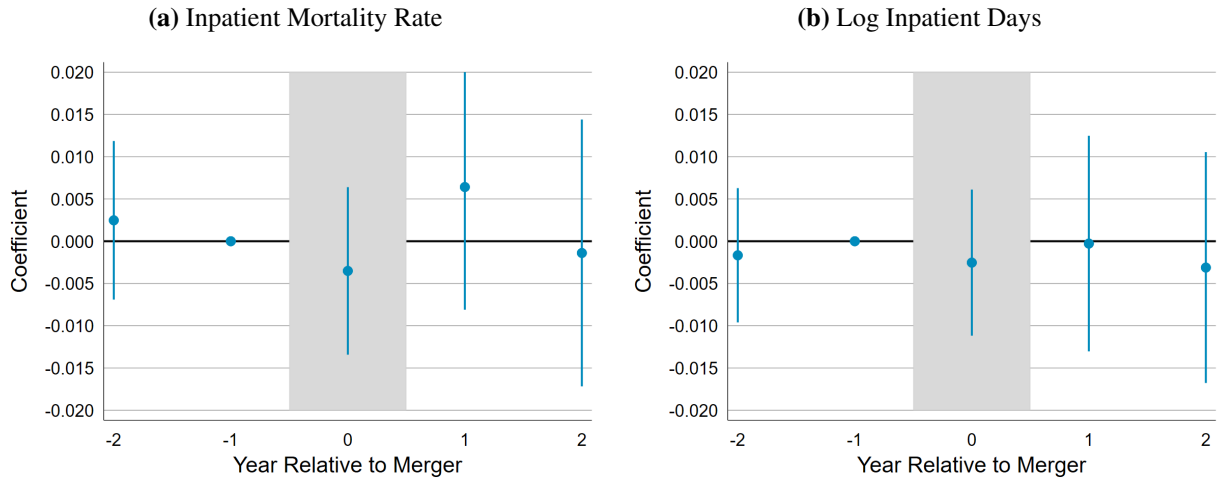
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Here we present estimates of "deaths of despair" per 100,000 population, broken down by cause. All data are measured at the county-year level. All regressions include county and year fixed effects, as well as a control to isolate variation in prices driven by price effects and timing of hospital mergers, as described in Section 5.2. Standard errors are clustered at the county level and included in parentheses. Data is at the county-year level. Our mortality measures are constructed using data from the CDC, and health spending data comes from HCCI. The point estimates report the impact of a log point (100%) increase in county-level health spending.

Figure A.1: HCCI Data Coverage Rates by State



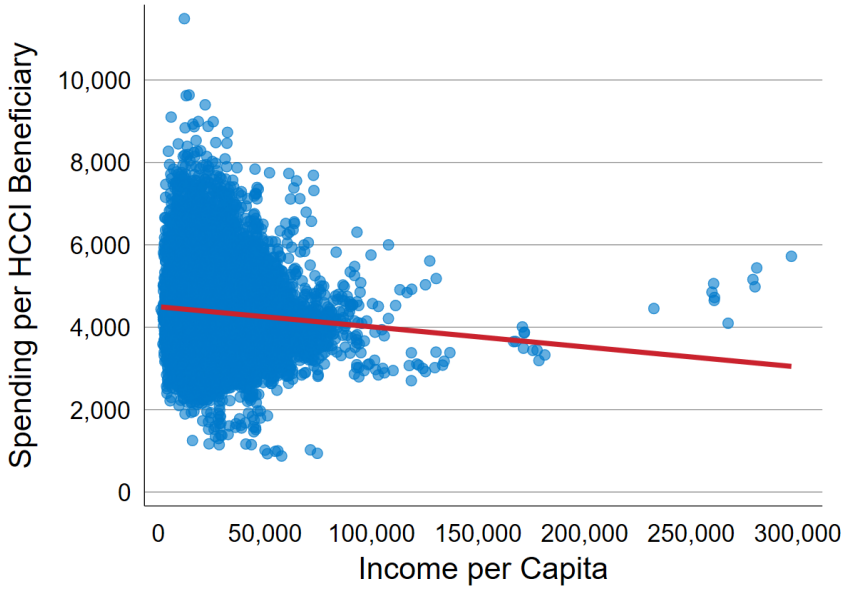
Note: Coverage rates represent the number of HCCI lives enrolled divided by total number of insured individuals in the state. Coverage rates were calculated using 2011 HCCI enrollment data. State-wide insurance coverage totals were derived from the American Community Survey for 2011. All numbers are reported in percentages.

Figure A.2: Quality and Quantity Event Studies



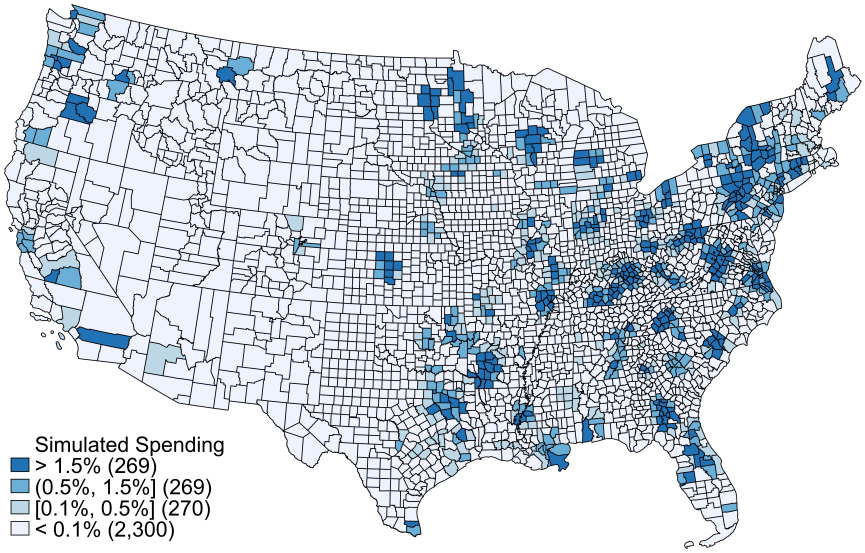
Note: Figures contain event studies of hospitals' inpatient mortality rates and total volume, measured relative to the year of a hospital merger as specified in Equation (1). Vertical lines display 95% confidence intervals based on standard errors clustered at the hospital level.

Figure A.3: Spending Per HCCI Beneficiary and Income Per Capita



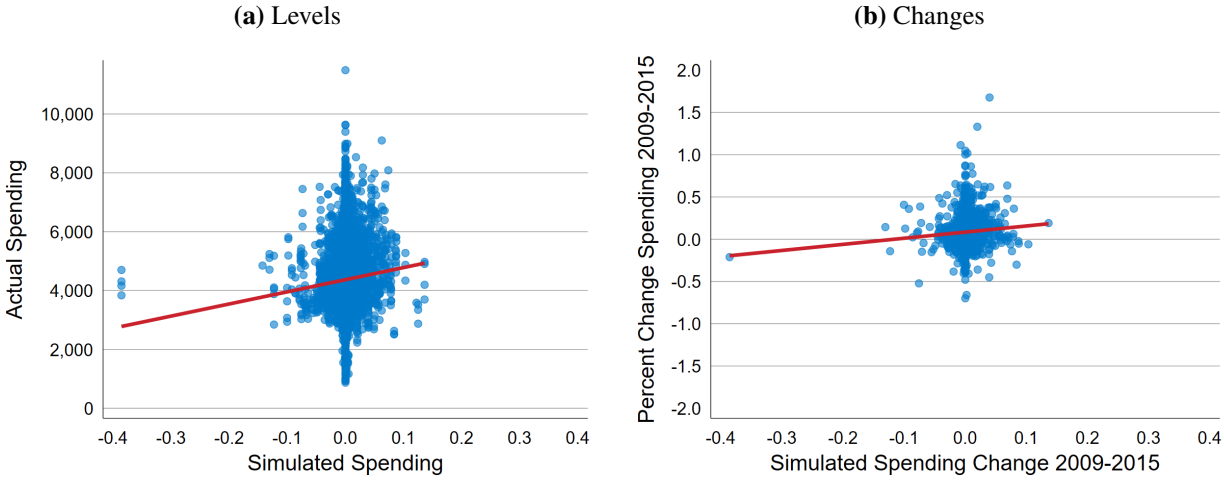
Note: This scatter plot show the relationship between income per capita and actual spending per beneficiary from 2008 to 2017. Spending is measured using data from HCCI. Income per capital is measured using data from QCEW.

Figure A.4: Simulated Spending 2015



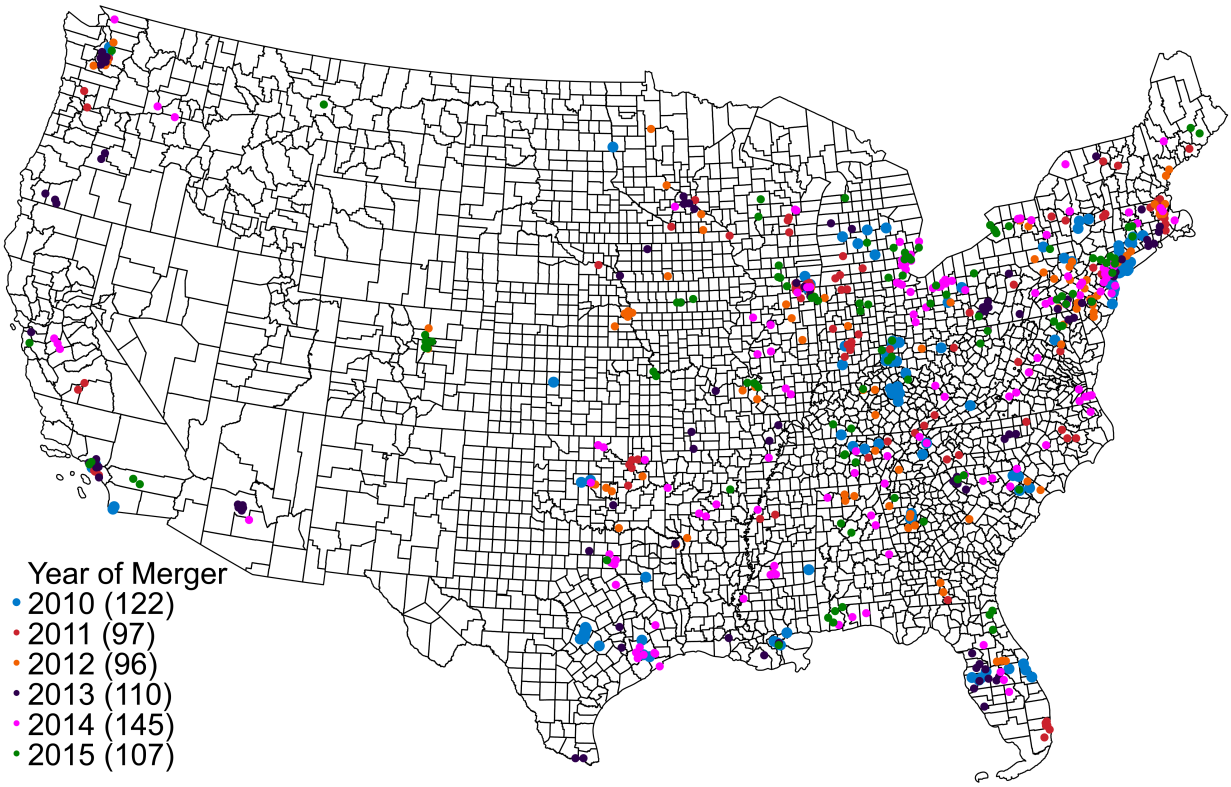
Note: Map presents simulated spending estimates constructed according to Equations (3) and (4). Simulated spending estimates represent the percent change in spending per insured beneficiary in each county in 2015, assuming quantities fixed at 2008-2009 levels. Hospital mergers are measured using augmented data from the American Hospital Association's Annual Survey of Hospitals. Health spending shares and post-merger price effects are generated using data from HCCI.

Figure A.5: Relationship Between Simulated Spending and Observed Spending



Note: Scatter plots show the relationship between spending per beneficiary and our simulated spending measure, as detailed in equations (3) and (4), and actual spending per beneficiary. Panel (a) plots the levels of observed and simulated spending from 2009 to 2015. Panel (b) plots the percent change in spending per beneficiary against the change in simulated spending from 2009 to 2015.

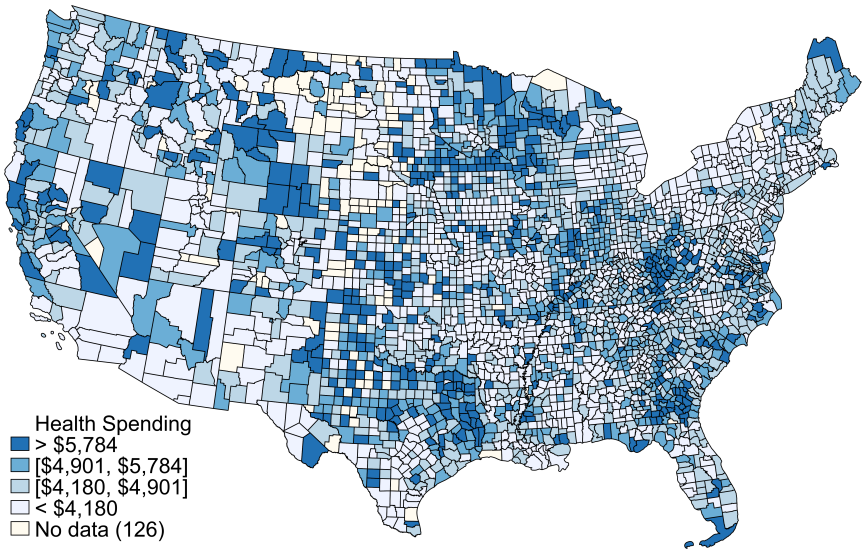
Figure A.6: The Timing of Hospital Mergers in the Estimation Sample



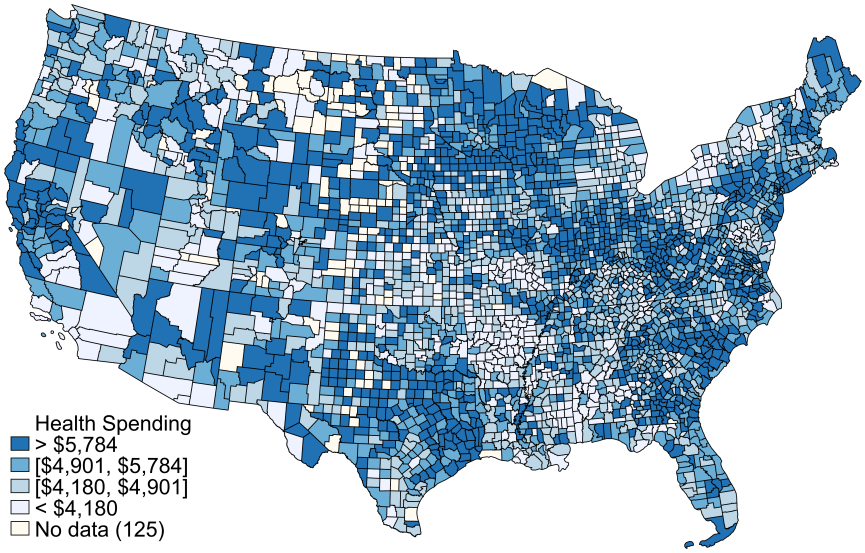
Note: The map shows the location of each merger, where each color indicates the year in which the merger occurred.

Figure A.7: Health Spending Per Beneficiary in 2008 and 2017

(a) 2008

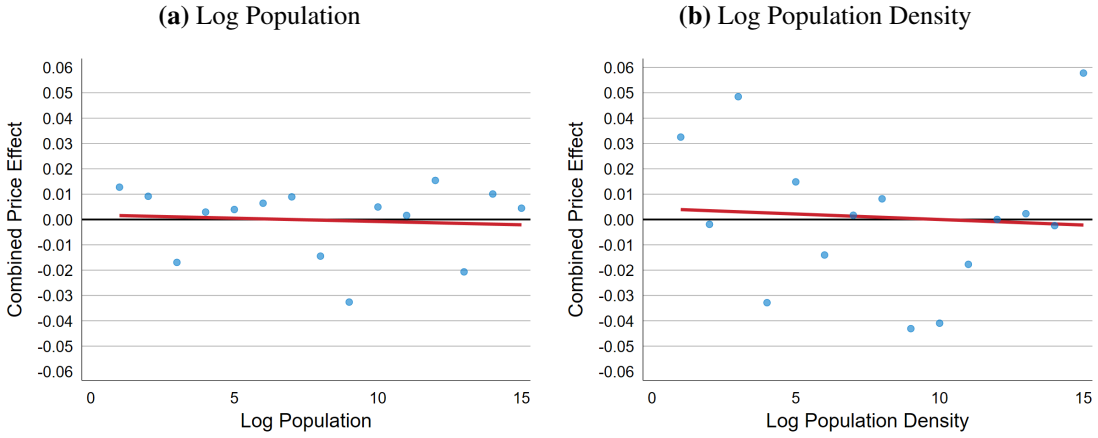


(b) 2017



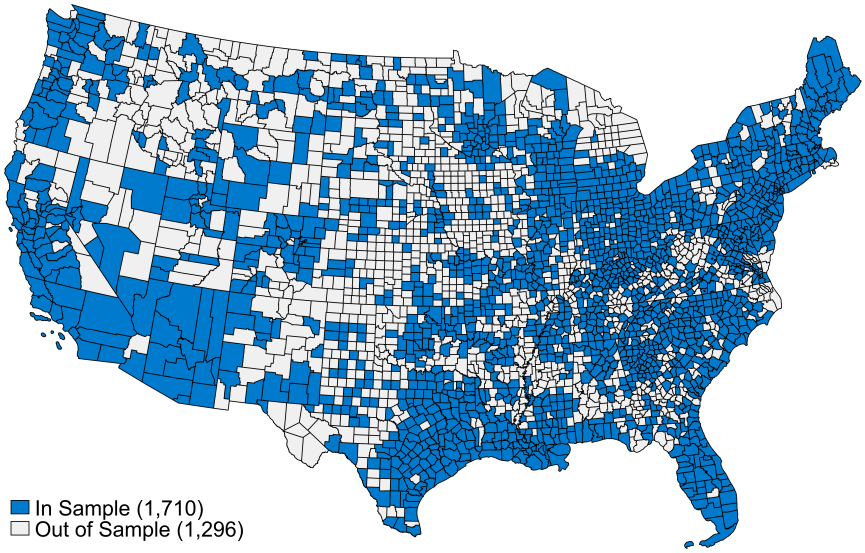
Note: This figure displays spending per HCCI beneficiary per county. Panel (a) shows 2008 and panel (b) shows 2017.

Figure A.8: Correlations Between County Characteristics and Combined Price Effects



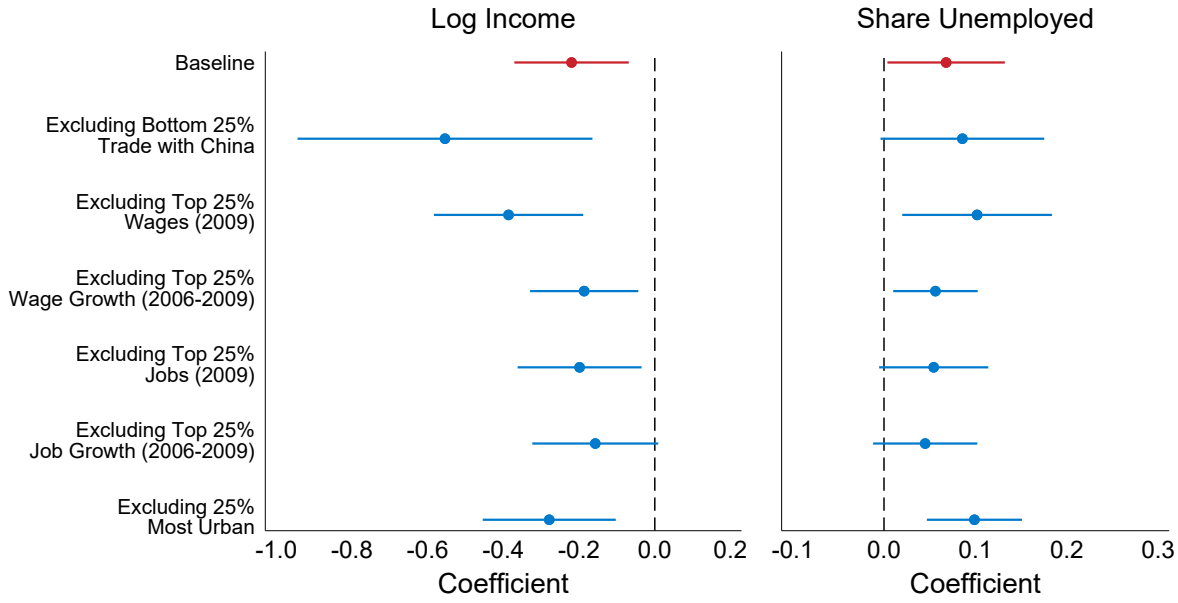
Note: Scatter plots show the relationship between the merger price effects λ_{eh} and characteristics of the merging hospital's county. Consistent with the simulated spending measure, whether the merger was above or below HSR thresholds has been residualized out of the merger price effects λ_{eh}. Panel (a) demonstrates that the estimated merger price effects are uncorrelated with population size. Panel (b) shows these effects are also uncorrelated with population density. The figure relies on population and population density data from the 2010 Census.

Figure A.9: Counties Included in Analysis



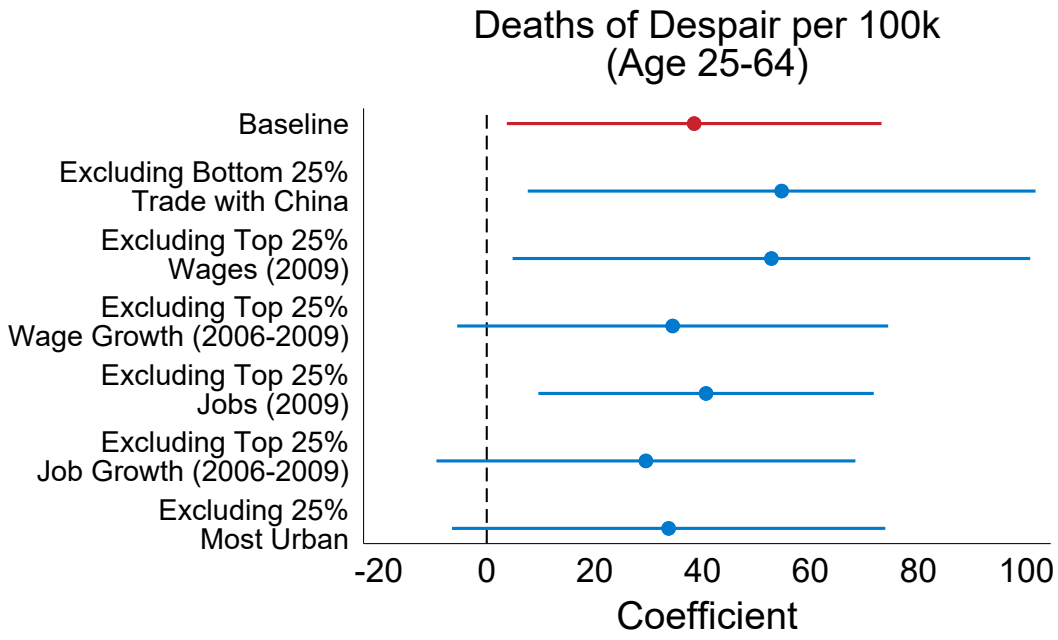
Note: This map presents the counties with over 500 HCCI beneficiaries that are included in our analytical sample. These 1,710 counties represent 96% of the population aged 25 to 64 in the continental United States.

Figure A.10: The Robustness of Labor Market Estimates to Excluding Urban Counties, Counties Exposed to Less Trade from China, and Counties with Higher Economic Performance



Note: Each row presents our 2SLS estimates of Equation (8) on labor market outcomes. Dots are point estimates from our 2SLS; the lines represent the 95% confidence intervals for each regression. Row 1 presents our baseline estimates of Equation (8) from Table 3. Subsequent rows present 2SLS estimates of Equation (8) on log income and the share unemployed after excluding cohorts of counties from our analysis. In row 2, we exclude the 25% of counties that, according to data from Autor et al. (2013), were least exposed to trade from China from 2000 to 2007 based on data from Autor et al. (2013). In row 3, we exclude counties in the top 25% of wages nation-wide in 2009 according to the QCEW. In row 4, we exclude counties with the highest wage growth from 2006 to 2009 according to the QCEW. In row 5, we exclude counties with the highest employment rate in 2009 according to the QCEW. In row 6, we exclude counties with the smallest declines in employment from 2006 to 2009 according to the QCEW. In row 7, we exclude the 25% of the most urban counties in 2009 based on data from the Census. We measure spending using data from HCCI and labor market outcomes using tax returns from the IRS.

Figure A.11: The Robustness of Mortality Estimates to Excluding Urban Counties, Counties Exposed to Less Trade from China, and Counties with Higher Economic Performance



Note: Each row presents our 2SLS estimates of Equation (8) on labor market outcomes. Dots are point estimates from our 2SLS; the lines represent the 95% confidence intervals for each regression. Row 1 presents our baseline estimates of Equation (8) from Table 3. Subsequent rows present 2SLS estimates of Equation (8) on log income and the share unemployed after excluding cohorts of counties from our analysis. In row 2, we exclude the 25% counties that, according to data from Autor et al. (2013), were least exposed to trade from China from 2000 to 2007 based on data from Autor et al. (2013). In row 3, we exclude counties in the top 25% of wages nation-wide in 2009 according to the QCEW. In row 4, we exclude counties with the highest wage growth from 2006 to 2009 according to the QCEW. In row 5, we exclude counties with the highest employment rate in 2009 according to the QCEW. In row 6, we exclude counties with the smallest declines in employment from 2006 to 2009 according to the QCEW. In row 7, we exclude the 25% of the most urban counties in 2009 based on data from the Census. We measure spending using data from HCCI and labor market outcomes using tax returns from the IRS.