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THE IMPACT OF PRIVATIZATION:
EVIDENCE FROM THE HOSPITAL SECTOR

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The Impact of Privatization: Evidence from the Hospital Sector
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ABSTRACT

Privatization has been shown to increase the growth and profitability of government-owned firms. However, the effects on consumers have been understudied. We study potential trade- offs in the US hospital sector, where government control of capacity declined by 42% over 1983–2019. Private operators may improve hospitals' financial viability and reduce the need for subsidies, but a focus on profitability may adversely affect access for unprofitable low-income patients and care quality. Combining multiple patient- and hospital-level administrative datasets with national hospital survey data, we study 258 hospital privatizations during the 2000–2018 period. Private operators increase profitability through a reduction in employment and an increase in the mean revenue per patient. The latter is achieved by cream-skimming more profitable patients and services, as well as by increasing prices. However, we detect an increase in mortality rates among elderly (aged 65+) patients, suggesting a decline in care quality. We also find a decrease in aggregate admissions of low-income patients at the market level and an increase in mortality among the near-elderly (ages 55–64), particularly in low-income markets. Overall, we estimate that the 258 hospital privatizations led to approximately 920 additional deaths per year and $694 million in annual savings for local governments by the end of our study period.

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1 Introduction

When should governments rather than private firms provide goods and services? This question has long intrigued economists, yet a consensus remains elusive (Shleifer 1998). Meanwhile, privatization is an important global phenomenon, with nearly one trillion dollars raised through the sale of government assets between 2013 and 2016 (Megginson 2017). Empirical evidence generally suggests that privatization improves the efficiency and growth of government-owned firms (Ehrlich et al. 1994; World Bank 1995). However, its effects on consumers have been understudied (Megginson and Netter 2001; Galiani, Gertler, and Schargrodsky 2005). This is a key limitation, since the privatization debate now centers on the delivery of social services, traditionally managed by governments (Stiglitz 2005).

Economists have long recognized the benefits of privatization. Government enterprises often struggle with misaligned employee incentives, soft budget constraints, and political interference (Shleifer and Vishny 1994; Sheshinski and López-Calva 2003). Private management can address these agency problems and improve profitability and growth, which may also benefit consumers, especially in industries with sufficient competition and minimal market failures (Vickers and Yarrow 1991). However, in markets with imperfections, government enterprises might better serve consumer welfare by setting prices or quantities that reflect social marginal benefits (La Porta and López-de-Silanes 1999). These issues are particularly acute in healthcare, where incomplete contracts, clinical discretion, and limited competition can lead profit-driven firms to reduce quality of care or exclude less profitable patients. Hart, Shleifer, and Vishny (1997) hypothesize these responses by private contractors as a form of shirking on non-contractible quality.

Privatization of hospitals raises several critical policy concerns. The primary trade-off involves the potential for increased efficiency and reduced subsidies against the risk of diminished care quality and access for vulnerable populations. Despite the absence of research to help policymakers assess this trade-off, local governments have rapidly privatized hospitals over the past few decades. Data from the American Hospital Association (AHA) indicate a 42% reduction in government-controlled hospital capacity from 1983 to 2019. Another key issue is whether government care provision complements or substitutes Medicaid, the means-tested program that is now the largest health insurer in the US by enrollment, covering nearly one in four Americans.[1] If government care complements Medicaid, then privatization could undermine the effectiveness of this vital social safety net. Although there is abundant evidence on the beneficial effects of Medicaid coverage on access to care and health outcomes (Currie and Gruber 1996; Finkelstein et al. 2012; Goldin, Lurie, and McCubbin 2021; Miller, Johnson, and Wherry 2021), little is known about how the decline in government hospitals impacts Medicaid's effectiveness. Finally, hospital care is the largest segment of the US healthcare industry and accounts for $1.4 trillion in spending, much of it tax-funded. It employs more than 7.1 million people, comparable in size to the entire construction

1. According to the Centers for Medicare and Medicaid Services (CMS), Medicaid had 79 million enrollees as of November 2023, while Medicare insured 67 million individuals. US population was estimated at 336 million in November 2023 by the Census Bureau.

sector, and more government employees than any other sector besides education.[2] Privatization could greatly affect the performance of this vital sector of the economy.

These debates are currently being played out in several states and cities in the US. For example, the state of Connecticut is currently investigating the potential for privatization to reduce subsidies for the only state-owned hospital there, prompting strident criticism due to concerns about the impact on low-income patients and employees (Cummings 2024; Phaneuf 2024). These debates are not limited to the US. In several countries, including Germany and Sweden, there are ongoing discussions or actions to privatize healthcare providers, sparking considerable controversy (Dahlgren 2014; Heimeshoff, Schreyögg, and Tiemann 2014; Knutsson and Tyrefors 2022). To our knowledge, this paper provides the first causal estimates to inform these policy debates.

This paper quantifies the effects of 258 privatizations of nonfederal government hospitals that occurred between 2000 and 2018. We identify privatizations by manually validating ownership information recorded in annual national surveys of hospitals by the AHA. Our comprehensive data includes medical claims for the universe of Medicare fee-for-service (FFS) beneficiaries, hospital discharge data and annual reports from five states, AHA survey data, and vital statistics microdata from the National Center for Health Statistics (NCHS). These rich data enable us to comprehensively examine changes in hospital strategies to improve profitability and the accompanying changes in patient volume and quality of care. We complement these sources with publicly available files from Medicare cost reports and the US Census Bureau.

We employ a staggered difference-in-differences research design to estimate the effects of privatization on the treated hospital and on the market where the hospital is located. This follows the approach used by recent studies that examined privatization (Galiani, Gertler, and Schargrodsky 2005; Arnold 2022) as well as the organization of healthcare markets (Eliason et al. 2020; Craig, Grennan, and Swanson 2021). Government hospitals that did not experience a change in ownership during our sample period serve as the comparison group. To study outcomes at the market level, we compare trends for markets that experience a privatization with those of markets that remain unaffected throughout the sample period.

Although our research design is standard in this literature, we recognize that privatizations are not randomly assigned. We therefore take a number of precautions to probe the validity of our estimation strategy. We examine dynamic effects around the year of privatization and find that our key outcome variables did not change differentially from the comparison group before the transition but experienced an immediate and persistent shift after privatization. We perform a large number of robustness checks, including controlling for differences in local economic activity, specification checks, using alternate estimators that address bias in staggered designs, relaxing sample construction rules, and matching. The estimates are qualitatively similar in all cases.

A key purpose of privatizing hospitals is to improve their profitability so that they become financially sustainable without government subsidies. In the year before privatization, treated

2. Source: Hospital spending reported in Table 2 of the National Health Expenditure Accounts, 2022. Current Employment statistics from the Bureau of Labor Services (BLS). The construction sector, NAICS code 23, employed about 8.2 million people in June 2024.

hospitals were unprofitable with an average operating deficit of 4% of revenue. Public hospitals' profitability suffered compared to private hospitals primarily due to a lower mean revenue per patient, even though they had lower personnel and total operating costs per patient at baseline. We find that private owners improve performance exactly on this dimension: the mean revenue per patient increases by about 6%, alone sufficient to make a modest surplus. We also detect a substantial reduction in personnel spending, driven by a reduction in the number of full-time equivalent employees per patient. Overall, our baseline estimate implies that the average privatization generates $2.7 million in savings and tax revenue for the local government.

We document two strategic actions that help explain the increase in mean revenue per patient. First, hospitals "cream-skim" more profitable patients along multiple dimensions. Medicaid and self-pay or uninsured patients, who pay much lower rates than other payers, experience a disproportionate decline in stays (Horwitz 2005; Frakt 2011). Medicaid patients experience a 15% reduction in volume. Data from five large states indicate that uninsured patients experience substantially greater decline. In contrast, we detect small and statistically insignificant changes in stays for Medicare and privately insured patients. Changes in the payer mix account for 30% of the increase in revenue per patient. Our analysis also suggests that hospitals may be disproportionately decreasing admissions of less profitable payers by eliminating less profitable service lines. For example, we find a large decrease in obstetric admissions on the extensive margin. Second, privatized hospitals differentially increase their list prices, also known as charges, by 7%. These often form the basis of price negotiations with private insurers and affect vulnerable patients such as the uninsured. Changes in payer mix and list prices reinforce each other and can cumulatively explain up to 50% of the increase in mean reimbursement per patient.

However, we also find evidence of lower quality of care and reduced access to care for some groups of patients, consistent with theoretical predictions (Shleifer 1998). We detect an approximately 3% increase in 30-day mortality rates among Medicare FFS patients 65 years and older in the privatized hospital. This impact on FFS patients alone implies an increase of 3.6 deaths and 19 life-years lost (LYL) per privatization per year. This is a conservative estimate, since we cannot incorporate the effect on Medicare beneficiaries in managed care plans, who are not observed in our data.[3]

To quantify the effect on access to care, we examine changes in hospital admissions at the market level by comparing markets experiencing a privatization with those unaffected during the sample period. We detect approximately a 4% decrease in aggregate Medicaid admissions, the only payer to experience an overall decline. There is substantial heterogeneity in this estimate across different types of markets. Specifically, we detect more than a 10% decline in Medicaid admissions in markets with more than the median hospital concentration or poverty rate. Applying the same research design to vital statistics data, we find a statistically insignificant 0.5% increase in deaths of people 55 to 64 years of age in treated markets. Although imprecisely estimated,

3. If we apply the effects estimated for FFS patients onto the Medicare patients in managed care plans, we predict another 2.3 deaths and 12.4 LYL per privatization per year.

several patterns in the data assure us of a causal link with privatization. Nonparametric analysis reveals a strikingly linear relationship between the decrease in Medicaid admissions and the increase in deaths after privatization. We also find much greater effects among subgroups that have more exposure to privatization. The point estimate suggests an additional 1.7 deaths and 23.4 LYL among the near-elderly per hospital privatization. Taking together the effects on FFS patients and near-elderly residents, the average privatization leads to an increase of 5.3 deaths and 42.4 LYL per privatization per year.

This paper makes three contributions. To our knowledge, we are the first to obtain nationally representative estimates of the causal effects of hospital privatization in the US, adding to the broader privatization literature in economics.[4] In fact, we know of only a few relevant studies even outside economics, such as Ramamonjiarivelo et al. (2020). They study privatizations in an earlier period and document improved hospital profitability. However, they do not study operational changes, access to care, or impacts on health. Our results not only empirically document the effects on operations and care quality in detail but also enable us to concretely quantify the trade-off. Our estimates imply that the average privatization generates approximately $0.8 million (2.7/3.6) in savings for the government per additional death, or $141,000 (2.7 mn/19) per LYL. This is our main estimate, and under different assumptions, we estimate an upper bound of approximately $1.2 million per death or $220,000 per LYL. These estimates hew close to our analysis and do not incorporate long-term benefits (e.g., avoided pension obligations) or costs (e.g., worsening health effects). These are well below the thresholds for the value of a statistical life (VSL) of approximately 10 million or the value of a statistical life year (VSLY) of $369,000 stipulated by the U.S. Department of Health and Human Services (HHS) to assess the cost effectiveness of new policies (HHS 2017; Kniesner and Viscusi 2019).

A large literature has studied the effects of ownership structure on performance. Within healthcare, these studies have typically focused on the differences in objectives and performance between for-profit and nonprofit firms (Duggan 2000; Sloan et al. 2001; Malani, Philipson, and David 2003; Gaynor, Ho, and Town 2015). Knutsson and Tyrefors (2022) and Chan, Card, and Taylor (2023) quantify the difference in quality of care between government and private providers among ambulances and hospitals, respectively. Both studies find that government providers produce superior survival outcomes. We add to this nascent strand of the literature. Our results support their conclusion, as we find that converting a hospital to private control worsens survival among Medicare patients. Furthermore, we highlight the differences in operational strategies between public hospitals and their private counterparts, such as cream-skimming of profitable services and payers, increasing list prices, and reducing labor inputs. The results on cream-skimming reinforce similar findings from the nursing home sector, where providers can choose between Medicaid and more lucrative alternatives (Gandhi 2020; Werbeck, Wübker, and Ziebarth 2021; Hackmann, Pohl, and Ziebarth 2024).

4. Although there is extensive work on deregulation in sectors such as airlines, telecommunications, and electricity, the evidence on privatization in the US is thin (Lopez-de-Salanes, Shleifer, and Vishny 1997; Morrison and Winston 2010; Levin and Tadelis 2010; Davis and Wolfram 2012; Borenstein and Bushnell 2015; Howell et al. 2022).

Third, our results highlight the complementarity of Medicaid and government provision of care in designing an effective social safety net. When government hospitals privatize, Medicaid and uninsured patients find it more difficult to access hospital care. Previous studies have hypothesized that the low level of Medicaid reimbursement rates discourages providers (Garthwaite 2012; Alexander and Schnell 2024). The national utilization and spending patterns for Medicaid and Medicare corroborate the idea that Medicaid beneficiaries face additional barriers to using care. Spending per enrollee increased by almost 3% per year for Medicare enrollees during 2000–19, while it decreased by 0.2% for Medicaid enrollees. Compared to the levels in 1999, hospital utilization per Medicaid enrollee was 11 percent lower than that for Medicare enrollees in 2019. Our market-level results imply that hospital privatization is one of the factors that differentially suppress Medicaid utilization and can explain about 35% (4%/11%) of this gap in the markets that experienced privatizations. Given the magnitude of changes in hospital ownership during our study period and the plausible further changes in the years ahead, more work is needed to understand both the causes and consequences of hospital privatizations.

The paper proceeds as follows. Section 2 provides the necessary background about hospital ownership and privatization. We describe our data sources in Section 3, and our empirical strategy in Section 4. We present the estimated effects on privatized hospitals in Section 5. We similarly examine the effects on the affected markets in Section 6. Section 7 presents the effects on health. Section 8 discusses the implications of our results and Section 9 concludes.

2 Background

2.1 Hospital ownership

There is substantial heterogeneity in the ownership mix of hospitals across different geographies.[5] This is true not only of the share of publicly owned hospitals in a market but also of the type of privately owned hospital (nonprofit or for-profit). Table 1 highlights this variation and presents the shares of bed capacity of four different types of owners (public nonfederal, public federal, private nonprofit, and private for-profit) for a selected set of six large states with at least 100 hospitals in 2019 (AL, CA, TX, GA, IL, and PA). We also present the corresponding national means and standard deviations in column 7. The columns are ordered in descending order of the nonfederal public share of hospitals. For completeness, Table A.1 in the appendix presents the corresponding values of nonfederal public share of bed capacity for all states, as in 2019. In these tables and throughout the paper, we choose to focus on nonfederal public hospitals, since these usually serve the local community and are more comparable to private hospitals than federal hospitals, which mostly cater to military veterans or other designated populations (e.g., Native

5. The AHA survey reports hospital "control," which could be recorded as one of nonprofit, for-profit, or government. Control and ownership are typically synonymous, except for the small number of cases where the owner outsources managerial control or leases the property to a firm with a different organizational structure. There are some cases, as we shall discuss below, where the government *owns* the hospital, but it is *controlled* by a private company. Unless specified otherwise, our focus is on the entity with managerial control.

Americans).

We note two interesting patterns in hospital ownership. First, states vary enormously in their dependence on public hospitals. Pennsylvania has only 4% of its beds in state or local government hospitals, while 44% of Alabama's hospital beds are in such hospitals. This variation is even greater if we consider small states (Wyoming and Vermont have 71% and 2%, respectively). Second, the observed patterns are not easily explained. For example, the share of public hospitals does not track states' preferences over the size of government or the rural-urban split. Alabama has a higher public hospital share than Illinois. Similarly, the state's rural share of population does not explain public provision: Vermont and Maine are among the most rural states in the US but also have among the lowest shares of public hospital capacity.[6] Higher shares of hospital beds under government control in states that otherwise favor limited government foreshadows more waves of privatization in the future. Hence, the role of the government in the delivery of hospital care deserves greater research scrutiny.

2.2 The trade-offs in privatization

There is an extensive theoretical and empirical literature on the costs and benefits of government ownership of firms, as well as on the effects of privatizing government-owned enterprises (Vickers and Yarrow 1991; Megginson and Netter 2001). Shleifer (1998) summarizes the theoretical arguments for and against privatization. The main argument in favor is to alleviate agency problems with government employees and managers. Agency problems can arise through several channels, including political interference, soft budget constraints, and poor performance management practices. Another argument is that private firms can ease capital and credit constraints, thus enabling faster growth (Ehrlich et al. 1994). By resolving these frictions, private owners can improve the growth and profitability of formerly government-owned firms. This rewards not just the firm's managers and new shareholders, but potentially consumers as well, as shown by Galiani, Gertler, and Schargrodsky (2005) in the case of water utilities.

Greater efficiency is certainly desirable in the case of government hospitals. Data from the Census Bureau survey of local governments show that in 2000, local governments spent approximately 20% more on hospitals than they received in revenue. This deficit is also sizeable in absolute terms since it represents about 8% of total spending on social services.[7] These amounts indicate the substantial financial burden imposed on local governments due to hospital operations. Privatization offers a plausible path to reduce subsidies and free up funds for other priorities. Local politicians often have direct oversight and control over public hospitals, raising the specter of political interference that can lead to inefficient operations. Managers in public hospitals may have limited operational flexibility due to greater unionization. Appendix Figure A.1 presents the share of unionized employees in public and private hospitals over 1995–2019 using data from

6. State rural share of population: https://www.icip.iastate.edu/tables/population/urban-pct-states.

7. Authors' calculations using data obtained from the 2000 state and local government finances report, Table 1. Available at https://www.census.gov/data/datasets/2000/econ/local/public-use-datasets.html. This amount reflects spending on hospitals owned by local governments only and includes specialized hospitals.

the Current Population Survey (CPS). Unionization is consistently about twice as likely in public hospitals. Private owners can improve operating efficiency by reducing the number of hospital employees per patient, a strategy often implemented after privatization (Arnold 2022). This has the potential to save substantial costs since personnel spending constitutes approximately half of the total operating costs of the average government hospital.

However, Shleifer (1998) cautions that privatization may also harm consumers. Among the possible reasons, two are especially relevant to the hospital sector. First, insurer contracts with hospitals are incomplete since they cannot precisely stipulate and enforce exactly how they would like hospitals to behave. As a result, profit-driven private operators have an incentive to reduce costs more than is socially optimal (Hart, Shleifer, and Vishny 1997). Shleifer (1998) specifically offers the following example, "private hospitals may refuse to treat patients on whom hospitals generally lose money." Another example could be cutting back on staff in a way that reduces care quality. In theory, nonprofit hospitals can alleviate this concern if their managers are sufficiently mission-driven and more willing to accept unprofitable patients (Garthwaite, Gross, and Notowidigdo 2018; Eggleston 2024). However, previous empirical studies mostly find that nonprofit and for-profit hospitals respond similarly to financial incentives, suggesting that nonprofit hospitals are unlikely to resolve this concern (Duggan 2000; Sloan et al. 2001).

Second, hospital markets tend to be local and highly concentrated on average. Andreyeva et al. (2024) report that the mean Herfindahl Hirschman Index (HHI) for hospital markets was nearly 3,000 in 2000, well above the federal government's threshold for "highly concentrated" (DOJ 2010), and increased to about 4,000 by 2020. Most hospital stays originate in the emergency department, where hospital choice is shaped primarily by distance or travel time, not quality (McClellan, McNeil, and Newhouse 1994). In such a scenario, Shleifer (1998) argues that the privatized hospital can reduce cost and quality without worrying about consumers punishing them by switching to other providers. Privatization may also spur a greater response from the remaining hospitals if the market is more concentrated, as they will perceive a greater exposure to its effects. For example, consider a market in which one hospital privatizes out of 2 versus another in which one privatizes out of 6. The remaining hospital in the first market will expect a greater proportional influx of unprofitable patients, relative to the remaining 5 in the second market, who will expect to share the effects jointly. Similarly, the lone competitor will also fear a greater loss of its lucrative patients to the privatized facility. Negative responses from competitors could therefore reinforce and exacerbate the adverse effects of privatization in concentrated markets. Alternatively, if hospitals in concentrated markets have higher profitability due to market power, such spillover effects may not arise. Hence, the net effect of concentration remains an empirical question.

In summary, privatization represents a trade-off between operational improvements and lower subsidies, on one hand, versus a decrease in quality or access to care for some vulnerable groups, on the other. Causal evidence on both aspects is crucial for well-informed policy making. This debate is currently ongoing in Connecticut, where the state has engaged a consultant to investigate options, including privatization, to reduce its subsidy burden on a public hospital. However, this has sparked criticism due to concerns about the possible adverse effects on hospital employees and low-income patients, whom the hospital treats disproportionately (Cummings 2024; Phaneuf 2024). However, as discussed earlier, the existing evidence is inadequate to resolve such debates. This paper aims to fill this gap.

2.3 Hospital privatization in the US

Nonfederal governments have increasingly relinquished operational control of hospitals to private firms, a phenomenon we call privatization. We identify and study 258 privatizations during the 2000–18 period. To put this figure in context, consider that of the 1,060 public hospitals operating in 1999, nearly a quarter were privatized within 20 years. This tool is used mainly by local governments. In our sample, only 14 of the 258 privatizations, just over 5%, involved state-owned hospitals. The remainder involve facilities owned by counties (94), cities (33), or special-purpose hospital districts (117). The latter are similar to school districts in that they span multiple towns or cities within a county and tax constituents to fund and deliver health care services. Therefore, most privatization decisions are taken by county executives, governing boards of hospital districts, or city mayors.[8] In addition, public hospitals also closed operations entirely or turned to outpatient care only. We identify 41 closures, but do not study them.

Our review of the news coverage of these transactions suggests that there is significant heterogeneity in the motivations of the government sponsors. However, two drivers appear to be important for privatization. One is to reduce government subsidies devoted to hospital care while continuing to offer hospital services. The other is ideological and stems from the belief that private firms operate hospitals more efficiently than the government without compromising quality or access to care. In contrast, the hospitals targeted for closure appear to be consistently in poor financial condition or struggling to attract patients.

We also find significant heterogeneity in the structure of privatization deals, which we classify on two key dimensions. We did not have access to the contracts between governments and private firms and relied on press releases and independent reporting for this purpose. Table A.2 presents the distribution of the different types of deal represented in our sample and whether the new operator is organized as a for-profit or a nonprofit. As the table shows, privatization can manifest itself in numerous forms, and one could argue that every case has some unique features. We find that hospitals were brought under for-profit control in 28% of deals.

The private firm's operational control over the hospital after the transition varies in a continuum across different types of deal structures, ranging from limited control (short-term concessions) to complete control (ownership of all hospital assets). Section B.1 of the appendix provides details on the different ways in which governments transfer hospital control. To simplify exposition, we group deals into two categories representing less and more private control.

8. In some states, these officials are elected directly by citizens, while in other states they are appointed by the state legislature or governor. Hospital districts are constituted under state statute and therefore their structure and objectives vary across states.

The first group accounts for nearly 60% of all deals and represents less control for the private operator. The government retains ownership of assets, but outsources operational and managerial control to a private contractor. This structure was preferred to outright sales in some states (e.g., Florida) because the sale of government hospitals required legislative approval, a lengthy and uncertain process (Needleman, Chollet, and Lamphere 1997). The most common deal structure in this group was for the government to find a hospital management firm that would operate the hospital in return for a fixed monthly fee. We refer to this as "contract management." In another common approach, the government transfers operational control to a private company specially incorporated to run the hospital. The government agency continues to oversee the new entity.

Private operators enjoy substantially more operational control over the hospital in the second group of deals. This group contains three types of deals. The first is an outright sale of all hospital assets to the contractor. We assume that the new owners operate the hospital to maximize their own objectives, as they would any of their existing hospitals. The second approach is for the government to award a long-term lease (usually more than 15 years), giving the contractor more autonomy to make changes to the buildings and equipment, as well as day-to-day operational control. A third related approach that also involves a long-term transfer of control along with autonomy over the assets is for the contractor to enter into a joint venture with the government. Interestingly, for-profit firms are involved in more than 40% of the deals that grant more control, but less than 20% of the deals that grant less control, suggesting a preference for the ability to make more far-reaching operational changes.

2.4 Government provision versus coverage of hospital care

Figure 1 presents national trends related to government involvement in hospital care over 1983–2019, compiled using annual data from the AHA. Panel (a) shows that the share of hospital beds in nonfederal government hospitals declined from 27% in 1983 to 17% in 2019, a drop of nearly 40%. If we include ownership by the federal government in this calculation, the share decreased from 36% to 21%, more than a 40% decrease. There is a parallel, though slightly smaller, decline in the share of hospital employees working at public hospitals. In general, public hospitals have consistently declined in importance during this period.

In stark contrast, public insurance coverage of hospital care has grown rapidly during the same period. Figure 1 Panel (b) plots the trend in the share of patients covered by the two main public insurance programs at nonfederal hospitals. Medicaid, the means-tested public insurance program, more than doubled its share of hospital patients from 10% in 1983 to 22% in 2019. This is not surprising since Medicaid coverage eligibility has been expanded through several federal and state policy initiatives during this period. The share of Medicare, the public insurance program for the elderly, also increased from 32% to 45%.[9] Unlike Medicaid, eligibility for this program has been relatively stable and a large part of the increase is due to aging of the population. Perhaps,

9. The AHA includes both FFS and Medicare Advantage (MA) patients in its tally of Medicare patients. Analogously, Medicaid volume also includes patients in managed care plans.

local governments view the expansion of Medicaid coverage as an alternative means of ensuring access to care, making it easier to justify the privatization of public hospitals. Consistent with this hypothesis, Table A.1 shows that 7 of the 10 states with the highest shares of public hospital beds, typically those that favor limited government, had not expanded Medicaid under the ACA as of 2019. In contrast, eight of the 10 states with the lowest shares of public hospital beds had expanded Medicaid.

To further investigate this hypothesis, we formally test whether there is a negative correlation between the change in government control of hospitals and the corresponding change in Medicaid coverage of patients at the state level.

We estimate the association between state-level changes in Medicaid's share of nonfederal hospital patients (ΔM_{st}) and the corresponding changes in the public, nonfederal share of hospital bed capacity (ΔP_{st}) over four periods – 1983–1991, 1992–2000, 2001–2009, and 2010–18 – using the following model, stacking all four periods together:

$$\Delta P_{st} = \alpha_t + \gamma \Delta M_{st} + \xi_{st}. \tag{1}$$

γ is the coefficient of interest in this model and captures the within-state correlation between changes in Medicaid coverage and public hospital capacity. We weight each cell by the respective state population to account for the heterogeneity in size across states. We obtain a statistically significant estimate of -0.41 (0.11) for γ, which implies that an increase in Medicaid share of 10 percentage points (pp) in a state is associated with a decrease in the government's share of bed capacity in that state of about 4 pp. Recall that the national share of nonfederal public hospitals dropped by about 10 pp during this period; hence this effect size is economically meaningful. This estimate is not causal. However, it is consistent with the hypothesis that local government officials may view expanded eligibility for Medicaid as a substitute for government hospital capacity. If this is truly the case, then relinquishing control of hospitals via privatization is a potentially efficient policy response, since the government can reduce spending without harming consumers.

If, instead, private hospitals are reluctant to admit Medicaid (and uninsured) patients, as suggested by empirical patterns, then hospital privatization will exacerbate access challenges for this vulnerable group. In other words, instead of being substitutes, government hospitals could be a necessary complement for Medicaid to be effective. This conjecture is consistent with the patterns observed in national data on hospital use. Using CMS data for 1999–2019, the Appendix Figure A.2 presents hospital use per beneficiary for Medicare and Medicaid enrollees. Since the absolute rates of hospital use are very different between the two groups, and to focus on the percent change over time, we normalize utilization relative to levels in 1999. The figure shows that both groups have decreased hospital use during this period, likely driven by changes in medical technology and payment design. More importantly, we find that, relative to their respective utilization rates in 1999, utilization for Medicaid enrollees dropped 11 percent more than it did for Medicare beneficiaries. Several factors, such as changes in beneficiary risk and the use of preventive care between Medicaid and Medicare, could explain these differential trends. Our analysis will quantify whether the decrease in government care delivery, specifically through privatization, also contributed to this relative decline.

3 Data and descriptive evidence

3.1 Data sources and sample construction

We have compiled data from multiple federal, state, and proprietary sources with complementary strengths and weaknesses. We discuss the main data sources and their application below.

American Hospital Association surveys

We use annual surveys of hospitals from the AHA for the years 1995–2019 to source information on hospital attributes such as ownership type and location, and performance on patient volume, operating costs, and employment. We study inpatient volume by payer and in aggregate. Specifically, we observe inpatient volume for three payers: Medicare, Medicaid, and a residual group ("Other"), which is largely made up of privately insured and uninsured patients and contributes approximately 35% of patients in government hospitals. We cannot separately observe the number of hospital stays by uninsured and privately insured patients in the AHA data, but we do so using other datasets described below. We study changes in aggregate patient volume using a standard measure, "adjusted admissions", which is reported by the AHA and incorporates both inpatient and outpatient care (Schmitt 2017). Adjusted admissions are calculated by adding to hospital stays the number of outpatient visits scaled by the ratio of outpatient charges to inpatient charges to account for their lower resource intensity. We examine the total full-time equivalent (FTE) employed staff and the effects on different staff categories (physicians, nurses, and others).

We identify the privatization of government hospitals using a multi-step process, following previous studies on changes in hospital ownership (Schmitt 2017; Cooper et al. 2019; Prager and Schmitt 2021; Andreyeva et al. 2024). We first infer a change in control type if the value reported in the AHA survey changes from public one year to private the next, which yields 358 privatizations of public hospitals during 2000–18. However, previous studies have noted the prevalence of false positives when naively following this approach and have implemented a second step that involves validation of the naive list through internet searches and proprietary datasets. We similarly validate the inferred privatizations by examining the annual summary of change files from the AHA, news articles, press releases, and hospital websites; and confirm the changes against proprietary databases such as the American Hospital Directory (AHD), which tracks hospital ownership over time. If we cannot confirm a privatization, we drop the relevant hospital from the sample for our baseline model. In several cases, manual validation also helps to correct the year of privatization. Using this approach, we validate 258 privatizations, which implies a false positive rate of 28% in our sample (100/358), similar to the 30% rate reported by Schmitt (2017) who also used AHA data to study hospital mergers. Section B.2 describes other details of the sample construction.

We limit our final analysis sample to government-owned nonfederal general acute care hospitals. We retain government hospitals that were treated (privatized) or did not experience a change in ownership during this period. The sample is an unbalanced panel at the hospital-year level. Figure A.3 presents a frequency distribution of the number of years we observe hospitals in the AHA. About 90% of the hospitals are observed for the maximum possible 25 years with similar patterns for the privatized and comparison hospitals.

Administrative data from select states

The AHA data do not allow us to observe changes in the service mix, which may help explain changes in patient volume. In addition, we cannot observe volume changes separately for privately insured and uninsured patients. To overcome these two limitations, we use more detailed administrative data on hospital care from select large states that experienced several privatizations during this period and share data for research purposes. We were able to obtain data from five states (CA, FL, IN, MN, and WA), of which Indiana and Minnesota are among the top 5 states by number of privatizations. Collectively, we observe 27 privatizations between 2008–2018 in this data, approximately 10% of the total number of privatizations studied using AHA data. In the case of Florida, Indiana, and Washington, we have access to detailed patient-level hospital discharge data. In the case of California and Minnesota, we use annual reports on total hospital patient volume by payer. In addition to examining the effect on total inpatient volume, we also study the effect on obstetric patients, as an example of changes for a relatively unprofitable service. Minnesota does not consistently report obstetric volume and therefore we perform this analysis using the other four states. We describe these data in detail in Section B.3.

Medicare claims

The AHA data do not allow studying changes in a hospital's patient mix or quality measures. The state discharge data are also limited for these applications, since we cannot observe a patient's utilization history prior to the hospital stay or their outcomes after discharge. To overcome these limitations, we use administrative claims data for the universe of FFS Medicare beneficiaries. These files were obtained from the Centers for Medicare and Medicaid Services (CMS) under a data use agreement and cover the period 2000–2019. We observe all hospital stays for FFS patients nationwide during this period. Since this sample starts five years later than the AHA sample, we are able to study 55 fewer privatizations when we impose the same sample construction rules. Medicare data allows us to test for changes in observed health risk of admitted patients, as we can use the complete history of a patient's health care utilization to develop risk indicators. We limit our analysis to patients aged 65 and older, who represent the primary beneficiary group within Medicare.[10] We test for changes in hospital chargemaster rates after privatization while controlling for changes in patient risk. This provides insight into hospital billing practices. A key benefit of this data is that it also records deaths that occur outside the hospital. We examine changes in

10. According to the Kaiser Family Foundation, in 2019, about 87% of Medicare recipients received coverage due to aging in. The remaining received coverage due to Social Security Disability Insurance or because they were diagnosed with end stage renal disease.

patient mortality rates to test the effect on hospital quality. Section B.4 describes the construction of this sample and the variables in more detail.

National vital statistics

We study changes in mortality rates at the market level using confidential Vital Statistics data for 1995–2019 obtained from NCHS (NCHS 2023). Each observation relates to the death of an individual and provides information on demographics (e.g., age and sex) and the cause of death. We observe the individual's county of residence and can accurately compute mortality rates for all counties in the US without any censoring for small counties. This enables us to test for the population-level effects of hospital privatization on mortality at the market level. Section B.5 provides more details.

Supplementary data

We supplement the main data sources with information from publicly available files. We source data on hospital revenue and use of contract labor from the Healthcare Cost Reporting Information System (HCRIS), more commonly known as Medicare cost reports. Both variables are not available in the AHA. We obtain nationally representative mean hospital reimbursement rates by payer from the Medical Expenditure Panel Survey (MEPS). We describe these data in more detail in Sections B.6 and B.7, respectively. Finally, we obtain information on market-level attributes, such as county-level population, poverty, unemployment, and uninsurance rates, from the US Census and the Bureau of Labor Services (BLS).

Variable construction

In general, we prefer to express outcome variables in levels because it facilitates interpretation of the results; however, several outcomes, particularly related to finances, vary tremendously between hospitals even after scaling by hospital size. To avoid potential bias due to the skew in outcomes, we log transform variables related to patient volume and finances in the regression analyses, with the latter outcomes transformed after scaling by hospital size. Staffing values exhibit less skew, and we retain these in levels after scaling by hospital size.[11] Our primary measure of hospital size for scaling purposes is contemporaneous adjusted admissions, but we also test sensitivity to using beds instead. The results are not sensitive to the choice of scaling by hospital admissions or beds, or to the functional form. Throughout, all monetary values are adjusted for inflation and are expressed in 2019 dollars.

3.2 Descriptive evidence

Table 2 describes the hospitals observed in the AHA sample. Across all columns, we present values from 1999, a year prior to the first privatization in our sample. Column 1 presents values for the 258 hospitals privatized (treated) during the sample period. Column 2 describes the 802

11. Total revenue, one of the key financial outcomes, has a coefficient of variation greater than twice that of total staff FTE even after scaling by adjusted admissions, illustrating the greater heterogeneity in financial outcomes.

remaining public hospitals that did not experience a change in ownership during this period and are located at least 15 miles from any privatized hospital. This group comprises our primary comparison group. We impose this distance requirement to mitigate the potential for spillover contamination.[12] Comparing the values in these two columns reveals that privatized hospitals had about 22% fewer beds than comparison hospitals, but were otherwise very similar: both types admitted about 35 patients per bed per year and about 65% of their patients were covered by the primary public payers Medicare and Medicaid. The privatized hospitals had about 15% lower labor intensity (FTE staff per 100 adjusted admissions) and 17% lower operating expenses per adjusted admission at baseline, which implies that they were leaner than the comparison group prior to the change in control. In general, privatized hospitals had better finances at the beginning of our study compared to the remaining public hospitals.

Column 3 presents the corresponding statistics on the 3,925 privately owned hospitals in the data. On almost all measures, private hospitals were noticeably different from their public counterparts. For example, they operated on a much larger scale with twice the number of beds as treated hospitals and discharged more patients per bed (40 versus 35). Public payers accounted for a lower share of their patients (58%). They had a similar labor intensity but higher operating costs per admission than privatized hospitals, suggesting a different cost structure. Hence, private hospitals differ substantially from public hospitals in important operational dimensions and are unlikely to offer a suitable counterfactual to privatized hospitals. Column 4 presents the corresponding statistics for all 4,985 hospitals in the sample. Since about 80% of the hospitals are privately owned and serve more patients, the aggregate statistics lean toward those of private hospitals.

Figure 2 describes the phenomenon of hospital privatization in the US over 2000–18. Panel (a) presents a heat map of the US based on the number of privatizations in the state. The states in the South and Midwest experienced the highest number of privatization events during this period. Texas, Minnesota, Georgia, Louisiana, and Indiana are the five states with the highest number of privatizations. However, privatization is a widespread phenomenon: more than 40 states experienced at least one and no state experienced more than 30. Panel (b) presents the number of privatizations in each year. There were at least 10 privatizations in each year from 2002 through 2017, and no single year accounts for more than 8% of the total number of privatizations. The trend of privatization accelerated following the Great Recession – there were about 16 conversions per year in 2009–2018 versus 12 per year over 2000–2009.

Tables A.3 and A.4 describe the five-state and Medicare samples at baseline, respectively. These samples are subsets of the AHA sample in terms of geography and time period covered. For ease of comparison, both tables follow the same format as Table 2. In the interest of brevity, we limit

12. This restriction drops only 32 potential control hospitals. The choice of 15 miles is somewhat arbitrary and trades off the need to isolate comparison hospitals from treated facilities against the desire to retain a larger share of potential comparison hospitals in the sample. According to our calculations, approximately 75% of Medicare hospital patients during 2000–2016 were treated at a hospital located within 15 miles of their home zip code, suggesting this is an appropriate threshold.

the discussion to a few notable points.

Both privatized and nonprivatized hospitals in the state sample have a higher bed capacity compared to the national average represented in the AHA. A key benefit of these data is the ability to granularly observe the payer type. The shares of Medicaid and Medicare patients are comparable to the national averages presented in Table 2. The remaining patients, which comprise the "Other" group in the AHA, can be allocated to three types of payers. Privately insured and uninsured patients account for the vast majority of patients in this group, 80% and 13%, respectively. A small proportion of patients are in neither category, such as workers' compensation and other government plans. We label these as "Miscellaneous."

Table A.4 shows that the Medicare sample contains 203 privatized hospitals and 769 public hospitals that did not experience a change in ownership during 2000–19. These correspond to the hospitals summarized in columns 1 and 2 of Table 2. Panel A shows that both groups are similar to their equivalents in the AHA sample in total admissions, bed capacity, and payer mix. Although privatized hospitals are smaller on average, they serve slightly more Medicare FFS patients than nonprivatized hospitals. Panel B presents mean values for the patient-level outcomes examined using Medicare data. These outcomes pertain to patient mix, intensity of treatment, billing, and mortality rates. In general, the privatized and comparison hospital have similar values.

4 Empirical Strategy

We leverage the 258 privatizations by state and local governments as natural experiments to quantify the average effect of the treatment, privatization, on affected hospitals and on the markets in which they are located. Our baseline models implement a staggered difference-in-differences (D-D) research design, following the recent literature on privatization and ownership in healthcare (Arnold 2022; Eliason et al. 2020). Government hospitals that did not experience a change in ownership during 2000–2018 constitute the comparison group, since they offer an intuitive counterfactual for privatized hospitals.[13] This design relies on the assumption that privatized and comparison hospitals would proceed along parallel trends in the absence of treatment. To facilitate estimating pre-trends, we require that we observe privatized hospitals for a minimum of five years prior to the transition. Since we use multiple data sets that span slightly different time periods and contain different groups of hospitals, we cannot study the same set of treated hospitals across all outcomes. However, we impose common rules when constructing the different samples and estimating regression models.

Equation 2 presents our baseline model. Y_{ht} denotes the outcome of interest for hospital h in market m in year t. We model the outcome as a function of hospital and year fixed effects, α_h and α_t, respectively. Recent studies of hospital closures have noted that markets experiencing closures had weak economic trends prior to closures (Alexander and Richards 2023; Chatterjee, Lin, and Venkataramani 2022). Hence, we test sensitivity to including covariates X_{hmt}, a vector

13. Hospitals that exit the sample are retained in the comparison group since this is a valid counterfactual to privatization.

of time-varying hospital, market, and state attributes, which comprises unemployment, poverty, and uninsurance rates for the county in which a hospital is located; county population; whether a hospital is a 340B provider; and an indicator for Medicaid expansion under the Affordable Care Act (ACA). The key regressor of interest, D_{ht}, is a time-varying indicator variable that is equal to one starting in the year the hospital is privatized and zero otherwise. Finally, ϵ_{ht} denotes unobserved time-varying factors. We cluster standard errors by hospital to account for the potential correlation of outcomes over time in the same hospital, which is the unit of treatment.

$$Y_{ht} = \alpha_h + \alpha_t + \beta\, D_{ht}\, [+X'_{hmt}\,\delta] + \epsilon_{ht}. \tag{2}$$

In our primary specifications, we estimate unweighted models, giving equal importance to all hospitals. We examine some outcomes by estimating an equivalent model at the patient level, such as patient complexity, length of stay in the hospital, and mortality after discharge. This allows us to include patient covariates to control for differences in risk between patients. Here, we include a vector comprising patient demographics, 30 Elixhauser risk flags based on the 90-day history of hospital inpatient and outpatient care, flags for a history of different types of hospital care and the reason for hospitalization. Section B.4 describes the patient covariates in more detail. When we quantify the market-level effects of privatization, we estimate an equivalent of the hospital model on data collapsed to the market level.

Although our approach is standard in this literature, we note that privatizations are not randomly assigned, nor are we aware of a credible quasi-experimental instrument for these changes in control. Hence, one should interpret the coefficient of interest, β, with caution. However, our specifications control for the most important potential confounders. For example, hospital fixed effects eliminate persistent unobserved differences between hospitals (and the markets they belong to), an important source of selection. Under the parallel trend assumption, β recovers the average treatment effect on treated units, which could be hospitals or markets, depending on the model. The treated hospitals differ from the average government hospital on several dimensions, most notably in size, and the results should be interpreted accordingly. We assess dynamic effects on the outcomes for treated units around the year of privatization by estimating the event study model in Equation 3 for each outcome.

$$Y_{ht} = \alpha_h + \alpha_t + \sum_{s\neq -1} \beta_s\, D_{h,t+s} + \epsilon_{ht}. \tag{3}$$

A lack of differential trends in the years prior to privatization is consistent with the identifying assumption. Reassuringly, the evidence that follows suggests little or no differential pre-trends and relatively large changes soon after privatization. We truncate the sample to five years before and after the year of privatization to focus on immediate changes in trajectory following the change in ownership, an approach common in such designs (e.g., Cooper et al. 2019, Eliason et al.

2020, and Einav, Finkelstein, and Mahoney 2023). We also exclude the year of privatization (year zero) since it represents partial treatment and adds to measurement error.

We prefer using the two-way fixed effects estimator in our baseline model due to its simplicity, transparency, and flexibility. However, we recognize the potential for bias due to heterogeneous treatment effects in a staggered treatment setting. We thoroughly assess the sensitivity of the baseline estimates to using alternative modeling assumptions, estimators, sample construction rules for treated hospitals, and choice of comparison groups. The alternative estimators proposed by De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2020), and Arkhangelsky et al. (2021) use different approaches to correct for potential bias in staggered treatment and help assess the importance of this concern. The first estimator also allows us to leverage the variation due to the 60 hospitals that experience the reverse of treatment, i.e., transition from private to public control. Reassuringly, all the robustness checks generate similar estimates and lead to qualitatively similar conclusions. Sections 5.4 and 6.3 present the results of these tests for hospital- and market-level analyses, respectively.

5 Effects on the privatized hospital

5.1 Finances

A frequent goal of privatization is to make public hospitals financially sustainable without the need for ongoing government subsidies. Hence, we begin our analysis by examining the effects on hospital finances. Table 3 presents the D-D coefficients obtained by estimating Equation 2 without including the covariate vector X_{hmt} in Panel A, while Panel B presents the corresponding results obtained by including the time-varying hospital, market, and state controls mentioned in Section 4. For brevity, we examine four outcomes. Column 1 presents the effects on the total revenue from patient care (inpatient and outpatient) after all discounts and adjustments. Columns 2, 3, and 4 present the effects on total operating expenses, personnel spending (including benefits), and all nonpersonnel expenses, respectively. We normalize revenue and expenses by contemporaneous adjusted admissions to account for potential changes in patient volume after privatization. We use the log of the normalized value rather than the level to mitigate the influence of outliers. Consequently, we interpret the coefficients as approximately estimating the percent change in mean revenue or cost per patient. Figure 3 presents the corresponding event study plots with the dynamic effects on each outcome around the transition.

As the table shows, the estimates are very similar whether we include market-level covariates or not. This is reassuring, since it mitigates the concern of model misspecification and omitted variables such as differences in the prevailing economic environment. We prefer to focus on the estimates obtained without including additional covariates as our primary results; hence, throughout the text, we will primarily discuss these estimates unless they meaningfully diverge between the two panels. We detect a nearly 6% increase in mean revenue per patient, which is statistically significant at the 5% level. Since the mean revenue per patient is about \$8,100, this implies about a $460 increase in the mean reimbursement. Figure 3 Panel (a) shows an increase in mean revenue in the year following privatization, and the increase remains consistent in the 5–10% range over the 5 years we track following the transition. Reassuringly, there is no evidence of a differential pre-trend at the privatized hospitals prior to the intervention.

Table 3 Panel A column 2 presents the effect on total operating expense per patient and indicates a modest 3.3% decline that is statistically insignificant. Figure 3 Panel (b) presents the corresponding event study, which confirms that there are no trends before or after privatization. Columns 3 and 4 unpack this result by presenting the effects on personnel and non-personnel costs, respectively. There is a large and statistically significant decline in average personnel cost per patient (col. 3). This measure includes spending on salaries and benefits, normalized by the total adjusted admissions. The coefficient implies a nearly 9% decline in personnel cost. This appears to be a moderate decrease, but is quite large when juxtaposed with the fact that privatized hospitals had lower personnel spend per patient at baseline than both private facilities and units in the comparison group (see Table 2). However, the decline in personnel spending is partially offset by small increases in costs elsewhere (col. 4). This latter coefficient is positive and statistically insignificant. Figure 3 Panels (c) and (d) present the corresponding event study plots that are consistent with the average effects implied by the D-D coefficients.

The pattern of effects on hospital finances is not sensitive to our choice to scale outcomes by adjusted admissions. As a sensitivity check, we estimate a companion set of models in which we express values per contemporaneous bed instead. Table A.5 presents the point estimates, which imply a slightly greater increase in revenue than before, but very similar results, in general. Figure A.4 presents the corresponding event study plots, which are similarly reassuring.

Overall, privatization meaningfully improves hospital profitability. In the year before privatization, treated hospitals had an operating margin of -$335 per patient or 4% of mean revenue. Therefore, the 6% increase in revenue alone is sufficient to enable these hospitals to generate a modest surplus. If we also include the 3% cost reduction (approximately $280) in this calculation, ignoring the statistical insignificance for a moment, we estimate an increase in operating margin of approximately $740 per patient or 9% of the mean revenue. Given the relatively low levels of operating cost per patient at baseline in the treated hospitals, it is intuitive that the new private management focuses on increasing mean revenue per patient to improve profitability.

5.2 Operations

This section examines the effect of privatization on a range of operational dimensions. There are two key goals. First, our objective is to determine and quantify, where possible, the role of different operational strategies in improving profitability. Unburdened by government control, we hypothesize that hospital managers achieve the increase in mean reimbursement by focusing on more lucrative payers and services in addition to charging higher prices. Private administrators may also have more experience with the practice of overstating patient risk to increase the reimbursement rate, also known as "upcoding." Similarly, cost reductions are likely achieved by reducing the share of unprofitable patients and services and decreasing inputs to care. The results in the previous section indicate a decrease in staffing, but there could also be reductions on other margins, such as the intensity of treatment. As part of these analyses, we test for a decrease in care for relatively unprofitable patients and services. This relates to our second goal, which is to examine whether privatization hurts consumers by reducing access to care or its quality.

5.2.1 *Patient volume*

Table 4 presents the corresponding D-D estimates of the effect of privatization on patient volume and payer mix at the privatized hospital. Panel A presents results using the national sample of hospitals from the AHA. We present the effects on total patient admissions as well as on the component admissions by payer to highlight potential heterogeneity in effects for patients accessing care through different payers. Columns 2–4 present results for patients covered by Medicaid, Medicare, and the residual group, which we refer to as "Other."[14] Figure 4 presents the corresponding event study plots obtained by estimating Equation 3. The total number of patient admissions to the privatized hospital decreases by 8.4% after privatization. This estimate is statistically significant at the 1% level and suggests a substantial contraction of the hospital's patient care services. Figure 4 Panel (a) presents the corresponding event study plot indicating a sharp and persistent decrease in volume following the transition.

The decrease in inpatient volume may reflect a shift toward outpatient care after privatization. We test this conjecture and fail to detect an accompanying increase in outpatient care at privatized hospitals. Table A.6 columns 1 and 2 present the corresponding effects on the log of Emergency Department (ED) and non-ED outpatient volumes, respectively. In both cases, we find statistically insignificant and negative coefficients, which suggests, if anything, a decrease in outpatient treatment. Figure A.5 Panels (a) and (b) present the corresponding event study plots that corroborate the D-D coefficients. We formally study the effect on total hospital care quantity by testing the effect on adjusted admissions, which incorporates both inpatient and outpatient volume. Table 4 column 6 presents the result, suggesting a 6% decrease in total hospital care. Figure 4 Panel (e) presents the corresponding event study.

We test whether the new management operates the hospital at a lower occupancy rate, which is consistent with a decrease in patient volume, but signals a lower level of efficiency. Using the same research design, we find that patient volume per bed remains stable after privatization, implying that private managers decrease patient volume while maintaining occupancy rates. For brevity, we do not present these results.

5.2.2 *Payer mix*

We then consider changes in the payer mix for admitted patients. Although inpatient volume appears to decline across all payers, the decline is not evenly felt by all patient groups. While admissions of low-income Medicaid patients decrease by nearly 15%, Medicare admissions only

14. The AHA does not disaggregate the "Other" group. However, as discussed in Section 3.2, detailed data from selected states reveal that privately insured and uninsured patients account for about 80% and 13% of this group, respectively.

decrease by about 5%, and the coefficient is statistically insignificant. Finally, we find a nearly 14% decrease in the Other group. Taken together, we infer that hospital privatization primarily affects non-Medicare patients. Although Medicaid represents only 20% of the patients at baseline in the average treated hospital, it accounts for 30% of the volume decline.[15] The event study plots in Figure 4 show that, relative to the public hospitals not treated, the privatized hospitals did not trend differentially on these outcomes prior to the transition year. This is reassuring and supports the parallel trends identifying assumption. In addition, the patterns are consistent with the coefficient magnitudes. For example, there is a noticeable discrete drop in Medicaid and Other volume in the year after the transition (Panels b and d). As indicated by the dynamic coefficients, the magnitude of the drop in Medicaid admissions persists for at least the five years we follow. This pattern suggests that the decline is not a transient phenomenon due to a one-time disruption in management. In contrast, there is little change in Medicare volume in privatized hospitals after the change (Panel c).

We hypothesize that the differential decline in Medicaid admissions is due to the lower reimbursement rates of this program (Frakt 2011; Schulman and Milstein 2019). We use patient-level hospital reimbursement data observed in the Medical Expenditure Panel Survey (MEPS) files to compute mean reimbursement rates by payer. Table A.7 Panel A column 1 presents the corresponding values, expressed in 2019 dollars. We calculate the overall baseline average reimbursement rate as a weighted average of the respective reimbursement rates for Medicaid, Medicare, and Other using the corresponding patient shares in column 2 as weights. The data confirm that Medicaid is less lucrative on average than Medicare and private insurers but pays more than the average uninsured patient. The mean unadjusted Medicare and private insurer rates are about 45% and 60% higher, respectively, than the amount paid by the average Medicaid patient. In contrast, uninsured, or self-pay, patients pay 35% less than the mean Medicaid rate. Hence, the differential decline in Medicaid is consistent with private hospital operators trying to increase the mean revenue per patient by focusing on more lucrative payers.

Similarly, we hypothesize that the decline in the Other group is disproportionately driven by uninsured patients. We test this hypothesis using more detailed data that we were able to obtain from five states (California, Florida, Indiana, Minnesota, and Washington), together representing 27 privatizations. We apply our baseline difference-in-differences research design to these data. The small sample of privatized and nonprivatized hospitals necessitates an alternate modeling approach to ensure parallel trends between the two groups. Therefore, we use the newly developed synthetic difference in differences estimator (SDiD) (Arkhangelsky et al. 2021). SDiD constructs a weighted average of observed control units to generate a synthetic control unit for each treated hospital. In addition, a second set of time-varying weights ensures that the synthetic controls trend in parallel with their matching treated units before treatment. Finally, like the estimators proposed by Callaway and Sant'Anna (2020) and De Chaisemartin and d'Haultfoeuille (2020),

15. If we apply the estimated percent declines for each payer to the corresponding mean volume at baseline, we predict declines of 93, 68, and 146 patients, respectively, for Medicaid, Medicare, and Others. Hence, Medicaid accounts for 93/307 or 30% of the estimated decrease in admissions.

SDiD also addresses concerns due to treatment effect heterogeneity in the presence of staggered treatment. A limitation of this method is that, in the case of staggered treatment, it does not produce conventional event study figures. However, following Arkhangelsky et al. (2021), we use randomization inference and present the distribution of placebo treatment effects relative to those estimated for privatized hospitals.[16] Section B.3 describes sample construction and methodology in more detail.

Table 4 Panel B columns 1–4 present results on the same outcomes as in Panel A and therefore allow for a comparison between the national and state samples. Reassuringly, we find very similar patterns in these states. The coefficients indicate a decrease in admissions across all payers, with a disproportionate decrease in Medicaid. Columns 5–7 disaggregate the effect on the Other group into three components: private insured, uninsured, and a small residual set of patients that do not belong to either, which we group under "Miscellaneous." These estimates imply that the decrease in Other is entirely driven by a large drop of 37% in uninsured admissions (exponentiating the coefficient). There is a small and statistically insignificant decrease in privately insured patients and an increase in the miscellaneous group, albeit off a small base.

Figure A.6 Panels (a)–(f) present the distributions of placebo treatment effects for admissions in total and by payer along with a dashed vertical line indicating the estimated effect for privatized hospitals. As expected in a valid design, the placebo distributions are symmetric and center around zero. The effects on Medicaid and uninsured volume for privatized hospitals are clearly outlier values, while those for other payers tend to fall within the corresponding placebo distributions.

We perform an additional exercise using SDiD to highlight the difference between the effects on the less lucrative payers, Medicaid and uninsured, and those on the remaining payers. We detect a 25% statistically significant decline in the sum of Medicaid and uninsured admissions at privatized hospitals. In contrast, we estimate a statistically insignificant decrease of 2% in pooled admissions for all other payers. Figure A.6 Panels (g) and (h) present the corresponding placebo distributions and estimated effects, respectively. Hence, privatization causes a shift in patient mix away from less lucrative payers.

Armed with the estimated effects on admissions by payer and the corresponding average reimbursement rates from MEPS, we quantify the impact of cream-skimming more lucrative payers on mean revenue per inpatient stay, assuming all else remains equal. Table A.7 Panel A columns 4–6 summarize these calculations. We apply the estimated percent effects on volume corresponding to Medicare, Medicaid, and Other obtained using the AHA sample and presented in column 4 to the baseline patient shares in column 2 and obtain the predicted patient shares following privatization (column 5). Analogously, we use the estimated percent effects on private, uninsured,

16. SDiD assigns a time invariant and a time-varying weight to each control unit to generate the synthetic control trend corresponding to each treated unit. The hospitals privatized in the same calendar year belong to the same treatment cohort. To produce an event study plot of average effects across treatment cohorts, one would have to average values of the treatment and synthetic controls across cohorts, which is not possible without ignoring the time-varying weights and therefore invalidating the design.

and miscellaneous volume obtained using the states sample to predict their shares following privatization. We then predict the resulting mean reimbursement rates for Other and overall due to changes in patient shares, which are presented in column 6. Based on these calculations, the change in the payer mix predicts an increase in overall mean reimbursement from approximately \$12,560 to \$12,770, an increase of 1.7%, which is approximately 30% of the total increase in mean revenue per patient discussed in Section 5.1.

5.2.3 *Service mix*

The rich data from the states also allow us to examine changes in hospital service mix after privatization. While an exhaustive analysis of hospital services is beyond the scope of this paper, we focus on obstetrics as an example of a service line widely perceived as relatively unprofitable. Using cross-sectional analyses, studies have found that privately owned hospitals are less likely to offer obstetric services than government hospitals (Horwitz 2005; Horwitz and Nichols 2022). Others have noted a monotonic decline in hospital obstetric capacity in recent decades due to closures (Fischer, Royer, and White 2024). We quantify the effect of privatization on obstetric admissions using the state data and the same methods discussed above.

Table 4 Panel C presents the associated results. Column 1 shows that there is a large decrease in obstetric patient admissions of 52% after privatization (exp(-0.738)-1). This effect includes changes on the extensive margin, such as closures, and reductions in volume on the intensive margin. We then examine these two channels separately. Column 2 presents the effect on the likelihood that an obstetric unit is closed in a given year, which we define as the obstetric share of total admissions falling to 2% or lower in a given year[17]. We find that the probability of closure increases by 12.5% after privatization, which represents an increase of 80% relative to the mean probability of closure at baseline. Column 3 presents the D-D coefficient for hospitals that are deemed to have open obstetric units throughout the sample period. The coefficient is highly imprecise, so we cannot rule out large changes in either direction. Figure A.7 presents the corresponding placebo distributions along with the estimates for the privatized hospitals.

The AHA reports total births in the hospital, allowing us to examine the effect on this service using a national sample, although with several limitations. Using the AHA sample with all 258 privatizations, we corroborate the results from the states and find a decline in total births of 20%, shown in column 4. Figure 4 Panel (f) presents the corresponding event study. Taken together, the results indicate a large decrease in obstetric volume that is disproportionately driven by changes on the extensive margin.

These results strongly suggest a shift in focus away from unprofitable services after privatization. We hypothesize that the change in the mix of services may also help explain how hospitals are able to disproportionately decrease admissions for low-income patients. Patient-level hospital discharge data from three states (FL, IN, and WA) help shed light on this channel. We find that 48% and 10% of the obstetric patients in the privatized hospitals were on Medicaid or unin-

17. We restrict this analysis to hospitals with an obstetric share greater than 2% in 2002, the first year we observe all hospitals in the state sample. Section B.3 provides additional details.

sured, respectively, before treatment. Back-of-the-envelope calculations imply that the decrease in obstetric admissions can account for approximately two-thirds and one-fourth of the decrease in Medicaid and uninsured admissions, respectively, in these states. They also explain 54% of the decrease in total admissions.[18] Therefore, closing obstetric wards is an important driver of the effects on patient volume and payer mix discussed previously.

In closing, we emphasize that since the analysis using data from the five selected states represents the effects for about 10% of privatized hospitals, we prefer to focus on their qualitative implications rather than the magnitudes of the coefficients.

5.2.4 *Patient complexity*

Hospitals can also select more profitable patients on other margins, such as complexity. If lower risk patients are more profitable since they use fewer care inputs but earn the hospital similar revenue due to prospective payment contracts, private management may try to attract such patients. We use patient-level claims data for Medicare fee-for-service (FFS) beneficiaries to test this hypothesis and apply the same research design and estimating equation used with the AHA data. One difference is that the Medicare claims sample begins in 2000 instead of 1995. Hence, to ensure that we observe five pretreatment years for all treated hospitals, we drop 55 hospitals privatized over 2000–2004 from the sample for this analysis. We limit the sample to patients 65 years and older and who were enrolled in Medicare Parts A and B for at least 3 months prior to hospital admission to make them more homogeneous and ensure that we can adequately document their risk. Section B.4 describes the data and sample construction in more detail. We estimate a patient-level equivalent of our baseline specification in Equation 2. Depending on the outcome of interest, we also include in the model a comprehensive vector of patient covariates to account for differences in risk, as described in Section 4.

Table 5 presents the corresponding estimated effects. We present estimates from models excluding and including market covariates in Panels A and B, respectively. We begin by examining changes in the share of high-risk patients using two complementary measures of patient complexity. These results are presented in columns 1 and 2. The outcome in column 1 is an index of patient complexity, the predicted probability of mortality within 30 days of discharge from the hospital. This is predicted using coefficients from a probit model of 30-day mortality explained by demographics, comorbidities, and history of healthcare utilization within the last 90 days. The estimate implies a statistically significant 0.16 percentage point (pp) decrease in mortality risk, 1–2% of the mean mortality risk in this sample.

The second measure of complexity is directly observed in the data and does not use prior diagnoses. Hence, it is not susceptible to changes in coding in the privatized hospital. This is

18. A 52% decrease in obstetric admissions implies 533 fewer cases against a baseline of 1,024 patients (Table 4C). We then assume that the decrease in obstetric admissions affects different payer groups in proportion to their share of volume. For example, since Medicaid contributes 48% of obstetric admissions, it also accounts for 48% of the reduction, 48% x 533 = 256 fewer patients. The total decrease in Medicaid volume in these states is 22% compared to a baseline volume of 1,713 (not presented), or 377 patients. Hence, the shift away from obstetrics explains 256/377 = 68% of the decrease in Medicaid admissions in these states. Equivalent calculations are performed for other groups.

an indicator of a hospital stay in the last 30 days for a "nondeferrable" condition. These highly emergent conditions were defined by Doyle Jr et al. (2015) and signal a need for more intense treatment and a higher incidence of adverse outcomes than the average admission. In our sample, patients with a previous hospitalization for one of these conditions have nearly twice the 30-day mortality rate (17.4% vs. 9.6%) and stay about 0.5 additional days in the hospital on average than patients without it. Hence, this is an excellent indicator of complexity using an alternative source of variation. The estimates in column 2 imply a reduction of 0.7 pp in the probability of having a prior nondeferrable hospital stay, approximately 5% of the mean prevalence. Figure 5 Panels (a) and (b) present the corresponding event study plots corresponding to these outcomes. Both figures are reassuring on the lack of differential trends prior to privatization and suggest an immediate and persistent decline in patient complexity following privatization.

5.2.5 *Treatment intensity*

Hospitals can leverage gray zones in clinical guidelines and discharge patients sooner in order to reduce operating costs. We test this hypothesis by examining the effect on the duration of hospitalization, controlling for changes in patient risk. Table 5 column 3 examines the effect on log length of stay. The estimate implies a statistically significant 1.7% decrease. Column 4 shows that patients are now about 6% more likely to be discharged in less than 2 days (i.e., same day as admission or the next day). This increase in "short" admissions accounts for about half the estimated decrease in length of stay. Figure 5 Panels (c) and (d) present the corresponding event study plots, which corroborate the estimated D-D coefficients. These results are consistent with the above hypothesis and we return to this finding when we discuss the effects on patient mortality.

5.2.6 *Billing practices*

This section tests the use of two channels to increase mean reimbursement. First, we test whether hospitals upcode patient risk after privatization. Since we detected a decrease in the complexity of Medicare patients, we test whether Medicare was billed a commensurately lower amount for their care. Table 5 column 5 presents the corresponding results. The coefficients are positive, but small and statistically insignificant. We can rule out a decline in average payment of more than 2.5%. Similarly, in results not reported here, we find no change in the mean DRG weight, which determines most of the amount billed for the stay. These results imply that patients with lower complexity were billed at the same level as before, a form of upcoding.

We also test whether the hospital increases its list prices or "charges" after privatization. Although hospital charges do not affect standard Medicare reimbursements, increasing charges is an effective strategy to increase hospital prices for some group of patients. For example, Medicare "outlier" payments for very costly stays increase one-for-one with an increase in charges (Gupta, La Forgia, and Sacarny 2024). Private insurers routinely negotiate reimbursement rates with hospitals as a fraction of the charges billed for the stay (Cooper et al. 2019; Weber et al. 2021). Finally, patients who are not insured or receive care out-of-network are also billed the list price (Bai and Anderson 2016). Table 5 column 6 presents the estimated effect on log charges and implies an increase of about 7%. Figure 5 Panel (f) presents the corresponding event study plot which shows a flat pre-trend with a clear increase following the change in control.

We cannot directly estimate the contribution of the price channel to the increase in mean reimbursement in our sample, since we do not know which patients' prices are affected. However, we can provide a range using assumptions based on the literature. These calculations are summarized in Table A.7 Panel B. Previous studies have documented that the share of commercial insurance spending based on list price contracts during this period varied from approximately 20% (Cooper et al. 2019) to 50% (Dorn 2024). We apply these percent amounts to the share of hospital revenue contributed by patients affected by a change in list prices, assumed to include private insurance, uninsured, and miscellaneous categories. At baseline, these groups account for 35% of patient volume and 37% of total revenue. Columns 1 and 2 show that the increase in list prices, holding payer shares *fixed*, implies an increase of 0.5 to 1.3 percentage points in mean reimbursement.

However, this approach understates the importance of the price channel, since changes in payer shares and list prices reinforce each other. For example, hospitals both increase the share of admissions of privately insured patients and the prices they charge them. Panel B columns 4 and 5 present the mean reimbursement values obtained if we apply the post-treatment payer shares for private, uninsured, and miscellaneous (see Panel A column 5) to the mean reimbursement rates incorporating the increase in list price. We predict an increase in mean reimbursement of 2.2 to 2.9 percentage points. Hence, changes in payer mix and list prices can cumulatively explain up to approximately 50% of the increase in mean reimbursement.

5.2.7 *Employment*

Table 6 presents the estimated effects on employment normalized by hospital adjusted admissions. Column 1 presents the effect on the number of full-time equivalent (FTE) staff employed by the hospital, which includes both full-time and part-time employees. These are expressed per 100 adjusted admissions for ease of exposition. We find an economically meaningful reduction in total employment of 0.57 FTE per 100 admissions. Compared to the pre-privatization mean, this implies a decrease of 8% in labor intensity. In results not summarized here, we estimate a statistically insignificant 3% decrease in mean compensation at the privatized hospital. The lack of an effect on compensation differs from the general pattern of changes following privatization (Arnold 2022). However, it is intuitive in the case of hospitals, as government hospital employees already received lower wages at the beginning of the sample period than their counterparts in private hospitals.[19] Hence, our results suggest that the decrease in personnel spending reported in Section 5.1 is driven by a decrease in employment.

Although nurses account for 26% of the total staff, we do not detect any reduction in nurse FTE per patient. In contrast, physicians make up a small part of employed staff, but decrease by 30% relative to their strength.[20] The reduction in employment is driven mainly by the residual group,

19. See Table 2. The mean personnel expense in privatized hospitals in 1999 was 390.9/7.7 = $50,800. In contrast, in private hospitals it was 464.6/7.5=$62,000.

20. The figures here only account for employed physicians, such as hospitalists. However, for much of the sample pe-

here referred to as "Other." This group is disproportionately affected, since it represents 70% of the total FTE but contributes more than 90% of the decrease in labor intensity. This is a diverse group and includes staff performing patient care (e.g., technicians), back office (e.g., accounting), and managerial roles. If the decline in employment is partially or fully offset by an increase in contract staff, it implies that the new management is just changing how it contracts with workers. Therefore, we also test for an increase in the use of contract labor. However, the result in column 5 is close to zero and statistically insignificant. We can rule out an increase in contract staff of more than 0.01 FTE per 100 patients (-0.01 + 2 x 0.01), which would offset less than 2% of the estimated decline in employment. We conclude that private management truly decreases labor intensity.

Figure 6 presents the event study plots corresponding to each of these outcomes, except contract staff, where we find no effect and exclude for brevity. The dynamic coefficients are consistent with the D-D estimates presented in Table 6. There is a noticeable decline in total physician and other FTEs per 100 adjusted admissions in the year following privatization, and it persists over the next five years.[21]

We use our estimated effects to help put into perspective the change in labor inputs following privatization. The average treated hospital had 7,025 adjusted admissions per year before privatizing, which decrease by 6%, implying a reduction of approximately 420 cases per year. If labor intensity were kept constant at 7.4 FTE per 100 admissions (per Table 6), this alone would merit a reduction of 31 FTE (7.4 x 4.2 = 31.1) for the average privatized hospital. However, private operators also decrease labor intensity. For the average privatized hospital, this implies an additional decrease of 38 FTE (0.57 x (7025-420)/100 = 37.6). Hence, the average privatized hospital reduces about 70 FTE (13.5% of the mean) in the five years following the change in control. One caveat in interpreting the decrease in labor intensity as an improvement in productivity is the simultaneous decrease in patient complexity and length of stay.

5.3 Heterogeneity in treatment effects

This section tests three theories related to the type of management or organization that controls the hospital after privatization, which we refer to as the acquirer for brevity, although these deals often do not involve a change in ownership. We examine heterogeneity in treatment effects using triple difference models, leveraging variation in the nature of the acquirer or how much control it has over the hospital. In general, we do not find strong evidence of heterogeneity, since the triple difference coefficients tend to be statistically insignificant. Hence, we do not formally present the results and instead provide a brief summary.

First, we test whether acquirers make more extensive operational changes when they have a

riod, hospitals typically did not employ physicians directly, and this explains the low number of employed physicians.

21. These results are not sensitive to the choice of expressing FTE in levels or logs. We present results using log FTE per admission instead in Table A.8 and the corresponding event study plots are in Figure A.8. The results are also not sensitive to scaling FTE using admissions or beds. We present an alternate set of results using FTE scaled by the contemporaneous number of beds in Table A.9. Figure A.9 presents the corresponding event study plots. The results in both checks are qualitatively and quantitatively similar to those of the baseline model.

larger claim on hospital profits after privatization, as theory predicts. We find mixed evidence on this front. In some outcomes, like employment and personnel spending, we do find a larger decrease in deals conferring more control (e.g., buyouts or joint ventures). However, results related to patient volume and revenue do not follow a consistent pattern.

Second, we assess whether the changes effected by for-profit acquirers differ from those effected by nonprofits in a manner consistent with profit maximization. We find that for-profit acquirers obtain a greater increase in both mean revenue per patient and total admissions than nonprofits. The differential increase in admissions relative to nonprofits, 22 percentage points, is both statistically and economically significant. In contrast, the differences on employment and expenses are relatively muted, suggesting that for-profits more aggressively focus on growth than on cutting expenses.

Third, we assess whether privatization leads to a greater decrease in costs when the acquirer is a hospital system, i.e., it owns multiple facilities. Previous studies have shown that systems achieve greater cost reductions in acquired facilities by centralizing personnel, particularly in administrative and support functions (Andreyeva et al. 2024). Systems are acquirers in about 80 of the 258 deals in our sample, and we test if the effects of privatization differ in these deals relative to the remaining cases where the hospital remains independent. We do find much greater decreases in personnel expenses and employment when the acquirer is a system, qualitatively supporting the hypothesis. However, the coefficients are imprecisely estimated.

5.4 Robustness

We test the robustness of the main results presented above to different modeling assumptions and important validity concerns. Table 7 presents the corresponding results on finances, patient admissions (from the AHA), and employment in columns 1–3, 4–7, and 8–10, respectively. The top row repeats the estimates from the baseline model without market covariates for ease of comparison. Across all robustness checks, the models do not include market-level covariates. The results are collectively very reassuring, as the coefficients remain within two standard errors of the baseline estimates across all checks.

Panel I tests the robustness to alternate specifications. Row IA presents coefficients obtained from regressions that weight hospitals by beds.[22] This approach gives more weight to larger privatized hospitals. The effects on employment and personnel expenses increase in magnitude, implying that larger hospitals make greater employment cuts after privatization. Row IB presents estimates from a more flexible model that includes state-by-year fixed effects. This ensures that we compare privatized hospitals with comparison units in the same state. The effects on employment are attenuated, but the effects on other outcomes are similar to the baseline. Row IC tests whether the estimates are robust to relaxing the parallel trends assumption assumed in the baseline model. We follow Bhuller et al. (2013) and estimate D-D models that include a hospital-specific linear

22. For treated hospitals, we use the mean of pre-period beds, i.e., the mean of beds in the five years prior to privatization. For control hospitals, we use the number of beds in 1999 or the first year we observe that hospital.

trend for each hospital. These trends were estimated in a previous step using data over 1995–1999, prior to the first privatization in our sample. The estimates from this model are qualitatively similar to the baseline.

Panel II presents results using alternate estimators, which address the limitations of two-way fixed effect models when used in staggered treatment designs. Rows IIA and IIB report the corresponding coefficients of estimators proposed by Callaway and Sant'Anna (2020) and De Chaisemartin and d'Haultfoeuille (2020), respectively. These correct for potential biases due to staggered treatment in different ways and estimate the weighted average treatment on the treated. The latter estimator has an added advantage that it allows us to also leverage the 60 deals where private hospitals transition to government control, i.e., experience the reverse of privatization. Row IIC presents the coefficients generated by the synthetic difference-in-differences estimator. Reassuringly, all three sets of estimates are qualitatively similar to each other and to the baseline coefficient values.

Panel III tests the robustness to changing sample construction rules with respect to privatized hospitals. Row IIIA assesses the importance of reducing the imbalance in the panel for privatized hospitals. We limit the sample to privatized hospitals that we can follow for at least five years. The results remain virtually unchanged. The sample in row IIIB retains all observations for the treated units, instead of censoring them at +/- 5 years around the year of privatization. We also retain data from the year of the privatization (year zero). The effect on revenue diminishes, but other coefficients are similar to the baseline.

Panel IV tests the robustness to varying the comparison group. Table 2 shows that the comparison hospitals differ noticeably from the privatized hospitals in some dimensions, such as the number of beds. Although our research design does not require that treated and comparison hospitals be balanced in the levels of attributes or outcomes, this imbalance could signal unobserved differences in other dimensions that could potentially bias the estimates. Therefore, we assess the sensitivity of the main results to using a matched subset of the comparison group that more closely resembles the treated hospitals. We use 1:1 propensity score matching to identify a single comparison hospital for each treated hospital without replacement. Appendix C.1 describes the matching exercise in detail. Table A.10 presents evidence on the balance between privatized and comparison hospitals, before and after the matching. Following the previous literature, we calculate standardized differences to quantify improvement in balance (Schmitt 2017). Standardized difference values frequently exceed 0.2 in the unmatched sample, but are always below 0.1 in the matched sample, which is considered a benchmark of good balance (Austin 2011). Table 7 Panel IV row A presents the DD coefficients obtained using the matched sample, which are qualitatively very similar to the main estimates. In row IVB, we retain 110 additional hospitals in the comparison group that were recorded in the AHA data as switching between public and private control (and potentially back to public) in transitions that could not be validated. The estimates remain nearly unchanged.

We perform similar robustness checks for the results obtained using Medicare claims data, with two differences. We omit the specification check of weighting hospitals by beds, since the models are estimated at the patient level, and therefore implicitly give more weightage to larger hospitals anyway. We cannot use the alternate estimators presented in Panel II that require data to be collapsed to the level of treatment. The results are presented in columns 1–6 of Table A.11, which is organized similarly to Table 7. Reassuringly, the coefficient magnitudes remain within two standard errors of the baseline values.

6 Effects on the market

6.1 Hospital admissions

We find that public hospitals persistently admit fewer patients after privatization, and the decline is unevenly felt by patients covered by different payers. From a policymaker's perspective, the result assumes more significance if privatization causes an aggregate decline in utilization at the market-level; otherwise this could simply reflect a reallocation of patients within the market. Being forced to choose a different hospital in the same market could potentially be harmful if the new hospital is of lower quality than the privatized hospital, but it may also improve outcomes in the opposite scenario. However, a decline in admissions at the market level suggests that Medicaid and uninsured patients do not receive the care they need or have to travel to other markets to receive it. If these patients are perceived as unprofitable or undesirable, then other hospitals may be reluctant to offset the decline at the privatized hospital.

To shed light on this concern, we adapt our research design and implement it at the market level, which we define using Health Service Areas (HSAs). These were originally delineated by the US Census for the same purpose as Hospital Referral Regions (HRRs) developed by the Dartmouth Atlas group and have been used to study hospital markets (Makuc et al. 1991; Ho and Hamilton 2000; Petek 2022). HSAs have two appealing properties for our analysis. First, they are moderately sized. The average HSA in our sample contains about five hospitals. In contrast, the average HRR contains 18 hospitals. Consequently, we have greater statistical power to detect the market-level effects of a single privatization. At the same time, HSAs adequately capture a patient's hospital choices. Using Medicare claims data, we confirm that more than 70% of FFS patients choose a hospital located in the same HSA as their home zipcode. Second, HSA borders follow county boundaries, which allows us to directly link county attributes and outcomes (such as mortality) to HSAs.

To implement our analysis at the market level, we consider the 204 markets containing privatized hospitals as "treated," while the 725 remaining markets form the comparison group.[23] A market is considered treated when it first experiences a privatization (42 of the 204 markets experienced more than one privatization event) and is assumed to be treated through the end of

23. Imposing a non-neighbor rule for comparison markets to mitigate the potential for spillovers nearly eliminates all potential untreated markets in the same states as the treated markets, which is very unappealing. Hence, we do not impose such a rule.

the sample. We estimate an unweighted market-year-level model equivalent to that presented in Equation 2.

Table 8 describes the market-level analysis sample. Columns 1 and 2 are equivalent to the corresponding columns in Table 2. We also present some market-level economic characteristics, such as poverty and unemployment. The average treated market contains 6.1 hospitals, of which 1.3 or 21% are treated during the sample period. Market-level bed counts, payer mix, and the economic indicators are as expected based on the hospital-level averages. Comparison markets are slightly smaller in size and have slightly better economic indicators on average (e.g., lower poverty and unemployment).

Table 9 presents the estimated effects on hospital admissions at the market level, calculated as the sum of admissions across all hospitals located in the market. Since markets are quite heterogeneous, we model the effects on log patient volume. The columns present effects on total volume and by payer. Panels A and B present the average effects from specifications without and with time-varying covariates, respectively. Including market-level covariates tends to magnify the point estimates but leads to similar interpretations; hence, we continue to focus on the estimates without covariates. Column 1 presents estimates on total volume and reports a 0.4 percentage point (pp) decrease. However, we are under-powered to statistically detect an effect of this magnitude at conventional levels of significance.[24]

The key finding is that the decrease in volume at the market level is entirely driven by Medicaid, since we estimate positive effects on both Medicare and Other volume. Medicaid patients, on the other hand, experience a meaningful decline in admissions at the market level after privatization. The effect on Medicaid is -3.8 pp, slightly more than what we would predict based on the privatized hospital's decline alone (21% of -14.9, or -3.1 pp). This estimate suggests no offsetting responses by local hospitals. The coefficient is noisily estimated, so we cannot reject the null hypothesis of no change in Medicaid volume, although it is larger in magnitude and statistically significant at the 5% level when we control for differences in economic and social factors between markets. Figure 7 presents the corresponding event study plots for these outcomes. The estimated dynamic effects are consistent with the coefficients discussed above. Medicaid is the only payer for which the coefficients are consistently negative after privatization.

We examine heterogeneity in the effects on aggregate patient volume across markets along two policy-relevant dimensions, the first of which is the level of concentration in the local hospital market. Vickers and Yarrow (1991) note in their comprehensive review that privatization does not appear to increase productivity and growth when markets are not competitive. This is a highly pertinent issue in the case of hospital markets and can exacerbate the effect on admissions for unprofitable patients depending on the response of competing hospitals, as discussed in Section 2.2. We therefore test for a differential effect on patient volume in more concentrated markets. We designate treated markets as more concentrated if their HHI was above the median value across

24. We also estimate an imprecise decrease in log total adjusted admissions at the market level. The coefficient is -0.01 with a standard error of 0.012. Hence, the qualitative pattern remains similar even if outpatient visits are included in the count of admissions. We do not report these results in the interest of brevity.

all treated markets in 1999. We estimate triple difference models, comparing trends for both types of treated markets to all comparison markets.

Table 9 Panel C presents the corresponding results. For brevity, we present results only from models without including market covariates. The results imply that the effects of privatization differ dramatically in markets with low versus high levels of concentration. Utilization does not decline in competitive markets and even increases slightly, although we do not detect a statistically significant increase for Medicaid patients. There is a sharp decline in the aggregate volume of 5.2 pp in concentrated markets (4.3-9.5 = -5.2). Although volume declines in more concentrated markets across all payers, the decline is most pronounced for Medicaid patients at -11.2% versus -4.8% for the next most affected payer, Other. In results not reported here, we investigate the determinants of the larger decline in Medicaid volume in concentrated markets, relative to the average effect. We find that privatized hospitals experience a slightly larger decline in concentrated markets (16.5% vs. 15% overall). Privatized hospitals also contribute a larger share of the market (44% vs. 21%) in these markets. Hence, the decline in the privatized hospital predicts an aggregate decline of about 7.3% (44% x 16.5%) assuming no response from the remaining hospitals. These results imply that a substantial fraction of the aggregate decline cannot be explained by the actions of the privatized hospitals alone. The remaining hospitals in these markets likely also reduce Medicaid admissions when they are exposed to a privatization.

Next, we investigate heterogeneity across markets at different levels of affluence. Households with lower income levels are far more likely to be uninsured or to have Medicaid coverage (Gruber 2008). The uninsured reside disproportionately in communities with relatively low median household income (Institute of Medicine 2003). The Institute of Medicine report also noted that hospitals located in markets with a higher proportion of residents in poverty have lower operating margins. We hypothesize that the remaining hospitals in lower income markets will have less financial cushion to accommodate more Medicaid patients when a neighboring hospital is privatized. Hence, privatizations will lead to a greater aggregate decline in Medicaid patient volume in markets with above-median poverty rates. We test this hypothesis using a triple difference model.

Table 9 Panel D presents the corresponding coefficients of interest from the triple difference model. The results clarify that privatizations barely register in markets with below-median poverty rates. All D-D coefficients, which estimate the effects for low-poverty markets, are positive, small, and statistically insignificant. In contrast, markets with greater poverty experience an aggregate decline in patient volume of 2.7 pp (2 - 4.7 = -2.7), which is marginally significant. This is driven primarily by a large and statistically significant decrease in Medicaid volume of 11.8 pp (4.2 - 16 = -11.8). As in the case of concentrated markets, the aggregate decrease in Medicaid here cannot be explained by the direct effect on the privatized hospital alone. In a companion set of results not reported here, we find a qualitatively similar pattern of a differential decrease in admissions in markets with hospitals that had lower profit margins at the beginning of the sample period. Hence, the limited financial cushion of competing hospitals plays a role in exacerbating the effect of privatization on hospital access.

6.2 Employment

Previous studies on privatization have found spillover effects of privatization on market-level wages. Arnold (2022) studies privatization in Brazil and finds substantial spillover effects on mean wages at exposed firms in the market. The aggregate decline in wages is nearly three times that predicted based on the effect on the privatized firm alone. However, we do not find a direct effect on wages at the privatized hospital. We do find an effect on employment and, therefore, focus our attention on the aggregate effect on employment in the local hospital sector.

Table A.12 presents the corresponding effects on the FTEs employed in hospitals at the market level per 100 adjusted admissions. Panels A and B present the average effects obtained using models that do not include market covariates or include them, respectively. The columns present effects on total FTE and its component groups, physicians, nurses, and the residual "Other" group. The coefficients are small and statistically insignificant across all categories, suggesting that there is no detectable effect on employment on average.

6.3 Robustness

Appendix Table A.13 columns 1–8 present robustness checks on the baseline estimated effects on market-level hospital admissions (in logs), and employment. We implement the same checks as we did for the corresponding effects on the privatized hospital (Table 7), and therefore the table is organized similarly. Our main focus in this exercise is on the robustness of the decline in Medicaid admissions. Reassuringly, we find the pattern observed in the baseline estimates repeating consistently across all checks. Medicaid is the only payer for which we consistently estimate a decrease in volume.

7 Effects on mortality

This section investigates whether hospital privatization affects health outcomes. There are at least two potential channels. First, changes in care inputs, such as decreased length of stay and availability of staff, particularly physicians, could worsen the quality of care for patients treated in the hospital. Second, a decrease in access to hospital care could also adversely affect residents of the local community if they are forced to travel further for urgent medical care, suffer disruptions in their treatment plan, or experience crowding at the remaining facilities in the market. Quality of care and health are multidimensional objects, and a comprehensive examination of the two could warrant a separate paper. We therefore focus narrowly on the effects on mortality, an unambiguously bad outcome and one that is observed with little measurement error. Another advantage is that we can use values of a statistical life (VSL) estimates stipulated by government agencies to assess the monetary impact of increased mortality should we detect an effect.

7.1 Hospital quality

The economics literature has frequently studied short-term mortality as a key quality metric for hospital care (Chandra et al. 2016). Specifically, the probability of death 30 days after discharge from the hospital, or 30-day mortality, features prominently as a performance metric in Medicare's quality incentive program for hospitals (Norton et al. 2018). We estimate our baseline model in Equation 2 using the patient-level Medicare claims data, with 30-day mortality as the main outcome of interest. The model also includes patient covariates to control for differences in observed risk, as described in Section 4. Panel A of Table 10 presents the corresponding coefficients. The top and bottom rows present coefficients from models without and with time varying market-level covariates, respectively. Row A1 reports an increase in mortality of 0.33 percentage points across all FFS patients aged 65 or older, approximately 3% of the mean mortality rate. Controlling for differences between markets increases the magnitude of the coefficient. Figure 8 Panel (a) presents the corresponding event study and shows an immediate increase in mortality following privatization that persists for five years. We find similar qualitative effects if we study the effect on mortality at longer time horizons after discharge. Table A.14 Panel A presents the corresponding effects on mortality at 30, 60, 90, 180, and 365 days after discharge. Throughout, the estimated effect on mortality remains between 2-3% of the baseline mortality rate.

Due to concerns about potentially unobserved changes in patient risk, previous studies have preferred to focus on mortality rates for patients admitted with acute nondeferrable conditions (Card, Dobkin, and Maestas 2009). This group contributes only about a quarter of FFS patients and hence we do not prefer this approach, but we investigate the sensitivity to limiting the sample to these patients. Column 2 of Table 10 Panel A presents the corresponding results and shows a statistically significant increase of slightly larger magnitude than that reported for all patients but similar in percent terms. Figure 8 Panel (b) presents the corresponding event study plot and corroborates the D-D estimate.

We briefly investigate heterogeneity in the effect on mortality for different types of patients. Columns 3 and 4 of Table 10 Panel A present the results separately for patients aged 65–80 and 80+, respectively. Since older patients are more frail and sensitive to changes in quality of care, we expect a greater effect on their mortality. The results are consistent with this hypothesis and show that the older group experiences a higher relative increase in mortality (3% vs. 2%). Columns 5 and 6 present the effects separately for patients who receive medical treatment versus surgical procedures, respectively. We find a greater increase in mortality for patients receiving medical treatment, although the effects are similar compared to their corresponding baseline mortality rates. Table A.14 Panel B presents the effect on 30-day mortality for patients belonging to different major diagnostic categories (MDC). We report results separately for the top 5 MDCs: circulatory, respiratory, digestive, musculoskeletal, and kidney disease. Together, these five groups contribute nearly 70% of the total patient volume. We find greater effects for patients in the categories of circulatory, digestive, and kidney diseases. However, in general, we conclude that the increase in mortality is not driven by a specific demographic or disease group; rather, it is experienced by most of the FFS patient groups.

Using our preferred estimate of a 0.33 pp increase, we calculate that the average privatization in our sample leads to an increase of 3.6 deaths among FFS patients per year.[25] In addition to estimating the number of lives lost, we also provide an estimate of the number of life-years lost (LYL) for FFS patients. We can then perform an alternate comparison in terms of net savings to the government per LYL instead. This approach may be preferable, since elderly Medicare patients lose fewer years of life than the average person in the population. We follow the approach used by Gaynor, Moreno-Serra, and Propper (2013), who estimated LYL due to changes in hospital mortality rates in England. At baseline, the average age of death among FFS patients in privatized hospitals in our sample is approximately 82 years. Using data sourced from standard life tables, we calculate that the weighted average LYL for these patients is 8.9 years (CDC 2014). This calculation adjusts for age and sex of elderly FFS hospital patients, but does not account for their likely elevated mortality risk. To arrive at a more conservative estimate, we leverage the insight from Deryugina et al. (2019) that life expectancy for elderly Medicare beneficiaries is 40% lower after accounting for comorbidities. Therefore, we arrive at an estimate of 5.3 years lost per death and a total of 19 LYL (3.6 x 5.3) among FFS patients per hospital privatization per year.

7.2 Market level

We now test whether a decline in access to hospital care or a disruption in continuity of treatment leads to detectable mortality effects among people who reside in the affected market. Some local residents are directly affected because they receive care in the hospital and are affected by the decline in quality of care. The analysis in the previous section quantifies this effect for Medicare FFS patients 65 years and older. A second group is affected because they cannot access care in the hospital and have to travel further for care or experience a disruption in their treatment. A third group is indirectly affected due to the potential crowding in the remaining hospitals in the market. Examining mortality at the market level allows us to estimate the total effect across all three channels. We apply our market-level difference in differences research design to vital statistics microdata which allow us to observe the universe of deaths in the US during 1995–2019.

We caution that this test has limited power to detect an effect since only a small proportion of the population in the market is potentially affected by hospital privatization in any year. There are at least three reasons: Only a small fraction of people need inpatient care in a year[26]; the privatized hospital is typically only one of the six that serve the market; and, based on the results of Section 5.2, we hypothesize that the access effect is felt primarily by Medicaid beneficiaries and the uninsured. However, data constraints prevent us from focusing on lower income decedents directly. Therefore, this approach recovers an "intent-to-treat" effect. To maximize statistical power, we

25. The average privatized hospital served 1,136 fee-for-service patients at baseline (Table A.4). Adjusting for a 4.9% decline in volume (Table 4 Panel A), a 0.33% increase in mortality implies 3.6 additional deaths per year.

26. According to nationally representative survey data from the Health and Retirement Study (HRS) covering 2000–2018, about 25% of people 55 years and older experience a hospital stay over a two-year period. The proportion will be much lower for younger people.

limit the sample to those between 55 and 64 years of age. This group has relatively high hospitalization and mortality rates, while also having a high share of Medicaid and uninsured individuals. According to data from the CPS, about 20% of people aged 55–64 were covered by Medicaid or had no insurance in 2000 and 2019. In contrast, people 65 years and older enjoy nearly universal coverage through Medicare. Following similar rationale, studies on the aggregate mortality effects of the Affordable Care Act also focused on this age group (Black et al. 2019; Miller, Johnson, and Wherry 2021).

Table 10 Panel B column 1 presents the estimated effect on all-cause mortality for people aged 55–64 years residing in the affected market, defined by the HSA. We find an increase of about 5 deaths per 100,000, 0.5% of baseline mortality for this age group. However, this estimate is not statistically significant at conventional levels. Three patterns in the data corroborate the interpretation that this represents a causal effect of privatization. First, we find that the effect on mortality increases as Medicaid and Other hospital admissions in the market decrease. We estimate market-specific D-D effects on hospital admissions and near-elderly mortality for each treated market by comparing its trend with that for all comparison markets. We then regress the effect on admissions on the corresponding effect on mortality. We weight each market by its population of 55–64 year olds in 1999 to give more importance to larger markets and mitigate noise. To mitigate the influence of outlier values, we drop 2% outlier markets with the lowest and highest effects on patient volume, respectively. Finally, we bootstrap standard errors over both steps to account for the estimation error in the first step.

We present binned scatter plots in Figure 8. Panels (c) and (d) present the correlation between the effect on mortality (Y-axis) and the effect on Medicaid and Other admissions, respectively, on the X-axis. We present mean values in decile bins as nonparametric evidence and overlay a linear fit from the OLS model estimated on the underlying market-level estimates. The figures also mention the corresponding slope coefficients estimated by OLS and their bootstrapped standard errors. Panel (c) shows a clear downward sloping pattern, i.e., markets that experience a greater decline in Medicaid hospital admissions also experience a larger increase in near-elderly mortality. The pattern is remarkably linear across deciles. The slope coefficient is statistically significant and implies that a 4% decline in aggregate Medicaid volume, approximately what we estimate on average, predicts 3.6 more deaths per 100,000. Hence, the decline in Medicaid admissions can explain about 70% of the total increase in mortality. Similarly, Panel (d) shows an association between the effect on mortality and changes in Other admissions. The slope coefficient is even greater than in the case of Medicaid and is significant at the 10% level.

Intuitively, the effect on mortality should be greater among subgroups of the population that have greater exposure to treatment. This principle motivates our next two tests. We examine whether people located closer to the hospital experience greater effects. We do not observe the decedent's zipcode, so we cannot condition on distance directly. Instead, we estimate the effect on mortality separately for people living in the same county as the privatized hospital and those living in the remaining counties of the affected HSA. In both cases, the comparison group remains the same, which is the unaffected HSAs. Table 10 Panel B columns 2 and 3 present the corresponding results. These results confirm that the average mortality effect in the HSA is driven entirely by people living in the same county as the privatized hospital, which we call the affected county for brevity. These individuals experience an increase of 17.3 deaths per 100,000. This represents approximately 2% of the baseline mortality rate. Table A.15 presents additional results on mortality for people residing in the affected county. Panel A shows the effects for different age groups. We estimate positive effects among people 55 years and older, which is intuitive because these groups are more likely to use hospital care and are more sensitive to changes in access or quality. Panel B provides a breakdown of the effects by cause of death. The highest percent increases in mortality rate are for people dying of diabetes, liver and kidney, and respiratory diseases. Hence, the increase in mortality is not limited to people dying from urgent factors.

The third test leverages the variation in the poverty rate across markets. Lower income markets have a greater share of Medicaid and uninsured residents, who are more likely to use government hospitals. Moreover, in Section 6.1 we show that these markets experience a greater decrease in Medicaid admissions after privatization. Therefore, we expect a greater increase in mortality in affected counties with higher poverty rates. We estimate a triple difference model to test this hypothesis. Table 10 Panel B column 4 presents the results of this model. They show that the average effect on mortality in the affected counties reported above is primarily driven by those located within lower income markets. The coefficient, statistically significant at the 10% level, implies an increase in mortality of approximately 39 per 100,000, or 4% of baseline mortality. This result suggests that publicly owned hospitals serve a vital social function in lower-income markets.

We use the average estimate at the market level to calculate the lives lost among the near-elderly. The average treated market had about 42,160 individuals in this age group in the year prior to treatment, and hence this estimate implies an increase of 2.2 deaths per year. Since treated markets experienced 1.3 privatizations on average, this further implies 1.7 additional deaths per privatization. To obtain an estimate of LYL, we follow the same approach we used in the case of FFS patients. Standard life tables suggest an average life expectancy of 23.1 for people aged 60 years (CDC 2014). To be conservative in our assessment, we again assume that affected people are at a higher mortality risk than the average person of the same age. Following Deryugina et al. (2019), we scale this down by 40% to 13.9 years. Therefore, we estimate 23.4 LYL (1.7 x 13.9) among the near-elderly following the average privatization.

Our estimated effects of privatization on mortality, whether in the affected hospital or market, are smaller in magnitude than the effects documented due to sharper shocks to healthcare access. For example, Carroll (2023) finds an 8% increase in mortality among Medicare beneficiaries in rural markets when hospitals close. Miller, Johnson, and Wherry (2021) report a 9% decline in mortality among low-income individuals aged 55–64 years in Medicaid expansion states following the implementation of the ACA. Hence, these estimates are plausible in magnitude.

8 Discussion

This section ties together the estimated effects on hospital finances, admission volume, and patient health in a cost-benefit analysis to make more concrete the policy trade-off involved in hospital privatization. This analysis incorporates the channels studied in our empirical analysis, and we caution the reader that it does not account for many potential channels not captured in the empirics. This is particularly relevant for effects that will materialize over the long term. For example, privatization likely substantially reduces future pension obligations for the local government, but we cannot quantify this benefit. At the same time, long-term health impacts can also differ substantially from our estimates. For the reader's convenience, we summarize the computation of each cost and benefit amount in Table A.16.

The first policy question centers on the trade-off embodied in the privatization of any government enterprise: improved efficiency at the cost of lower quality, which, in the case of hospitals, includes reduced access. Our results support both sides of the debate. The average privatized hospital in our sample had a deficit of $2.4 million in the year before privatization (Table A.16 Panel B). Our results imply that this deficit is eliminated by privatization and forms the core of the financial benefit to the local government. Privatization also generates tax revenue in 28% of the cases where the hospital is acquired or run by a for-profit firm. Following Rosenbaum et al. (2015), we apply a nonfederal tax rate of 2.1% of revenue and estimate that incremental tax revenue is $1.2 million per for-profit hospital, or $0.3 million from the average privatization. Net savings for the government from the average privatization including incremental tax revenue, is therefore $2.7 million per year and is our central estimate of the benefit. We also consider an upper bound estimate, assuming that the entire increase in hospital surplus flows to the government in 56% of the deals where the private partner has less control. This is unlikely to satisfy the private partner's participation constraint, hence we consider it an upper bound. This increases the benefit amount to $4.2 million.

Next, we estimate the mortality cost incurred in terms of the lives or life-years lost due to privatization. We focus on the additional deaths among FFS hospital patients as our central estimate since it is precisely estimated. As described in Section 7.1, we estimate 3.6 additional deaths and 19 LYL for FFS patients 65 years and older. This probably underestimates the effect on mortality, as it does not consider the effects on Medicare beneficiaries enrolled in private Medicare Advantage plans. We do not observe hospital stays for these patients and therefore cannot directly estimate the effects for this group. If we extrapolate the effects estimated for FFS patients to all Medicare patients, we predict 5.9 total deaths and 31.4 LYL. To be conservative, we limit our upper bound of the effect on mortality to the effect on FFS patients and the imprecisely estimated effect on near-elderly residents described in Section 7.2. These two sum up to 5.3 deaths and 42.4 LYL. The calculations underlying these estimates are summarized in Table A.16 Panel C.

Our central estimate of the net savings to the government from privatization is approximately $0.8 mn per death (2.7 / 3.6) or $141,000 per LYL. The lower bound to this estimate also considers deaths among near-elderly residents in the community and is about \$0.5 mn per death or \$63,000 per LYL. The upper bound estimate includes surplus revenue, but only considers mortality for FFS patients and is about \$1.2 mn per death or \$220,000 per LYL. Whether we compare savings to lost lives or life years, even the upper bound estimates are below the reference values of approximately \$10 mn per life or \$369,000 per life year stipulated by HHS to assess cost effectiveness (HHS 2017; Kniesner and Viscusi 2019).

The second policy debate centers on the design of the social safety net. Specifically, if the government expands insurance coverage, does it also need to subsidize hospital care? National statistics on hospital utilization and spending are consistent with the notion that Medicaid beneficiaries face additional barriers to accessing care. According to data from the National Health Expenditure tables (NHE 2020), although Medicaid enrollment grew much faster than Medicare during 2000–19 (112% versus 55%, Table 22), inflation-adjusted spending on Medicaid grew at a *lower* pace than for Medicare (106% versus 140%, Table 3). Adjusting for the differential growth in enrollment enhances the difference – spending per enrollee increased approximately 3% per year for Medicare, while it *decreased* by 0.2% for Medicaid on average during this period. As shown in Figure A.2, compared to the respective levels in 1999, hospital utilization per beneficiary was 11 percent lower in 2019 for Medicaid relative to Medicare. Our market-level results on Medicaid admissions imply that privatization can explain about 4% or about 35% of this gap in the treated markets. Hence, privatization is a notable barrier to access for Medicaid beneficiaries, although there are likely several others as well. Medicaid coverage in its present form does not adequately substitute for the loss of government care delivery.

9 Conclusion

Privatization can improve the profitability and growth of government companies, but it can hurt some stakeholders. This trade-off assumes greater significance in the case of hospital care, which has unique challenges and has experienced substantial privatization in the US. However, this phenomenon has been largely ignored by researchers. We provide novel evidence from the privatizations of 258 government hospitals in the US over 2000–2018. We confirm that privatization improves hospital profitability sufficiently so that hospitals transition from loss-making to generating a modest surplus. The main channel to improve profitability is to increase the mean revenue per patient. Hospitals also reduce employment and personnel spending. Privatization therefore generates savings for state and local governments.

However, the improvement in finances comes at the cost of reduced access to hospital care for low-income patients who are often unprofitable for hospitals to serve. We show that hospitals disproportionately reduce admissions of low-income Medicaid and uninsured patients after privatization. We also detect a decline in aggregate Medicaid admissions at the market level, which implies that other hospitals do not offset the loss of government hospital capacity. Privatization can explain a small but nontrivial share of the gap in hospital utilization per beneficiary between

Medicaid and Medicare. In addition to a decrease in access, we also find evidence of a decrease in quality of care in the form of higher mortality rates among elderly Medicare patients. We also find strongly suggestive evidence of an increase in mortality among near-elderly residents of the affected market, with much larger effects in lower income markets. Our estimates imply that, on average, the savings generated per death or life-year lost do not meet the corresponding benchmarks set by the federal government.

Several avenues remain for future research on this topic. Although we document changes in some service lines and an increase in mortality rates, more investigation is warranted on changes in admission practices and other dimensions of hospital quality, particularly for non-Medicare patients. Researchers with access to all-payer claims data, perhaps focused on narrower geographies, can make progress on these questions. These inputs are needed for a comprehensive welfare analysis of privatization. Although our results imply that Medicaid coverage in its present form does not adequately substitute for government care delivery, they do not rule out the possibility that a reformed Medicaid program, for example, one that offers higher reimbursements, could do so. A necessary parameter to answer this question is the responsiveness of hospital admissions to changes in reimbursements, an estimation that was beyond the scope of this paper. Armed with these estimates, researchers can quantify whether a combination of privatization and Medicaid reform improves consumer welfare relative to privatization alone. Similarly, other dimensions of reform could also be explored.

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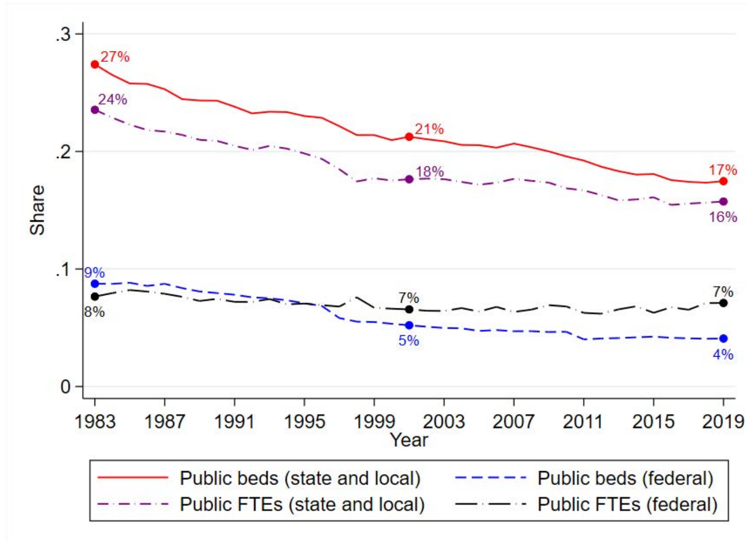
(a) As provider



(b) As payer

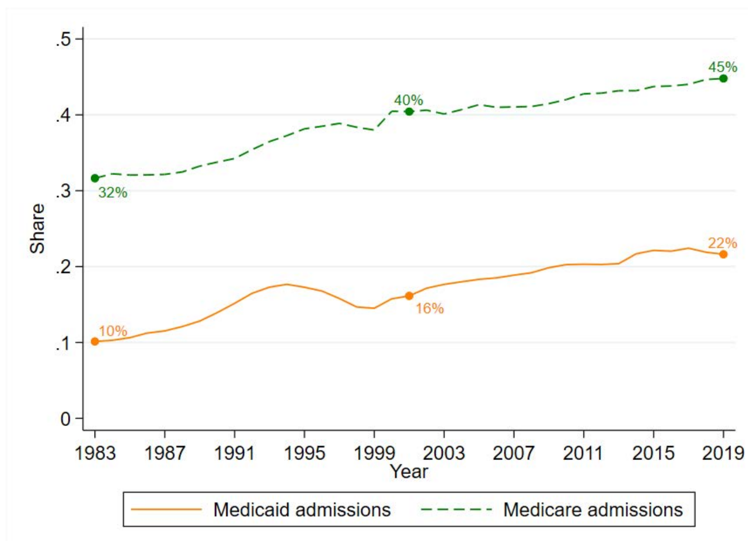
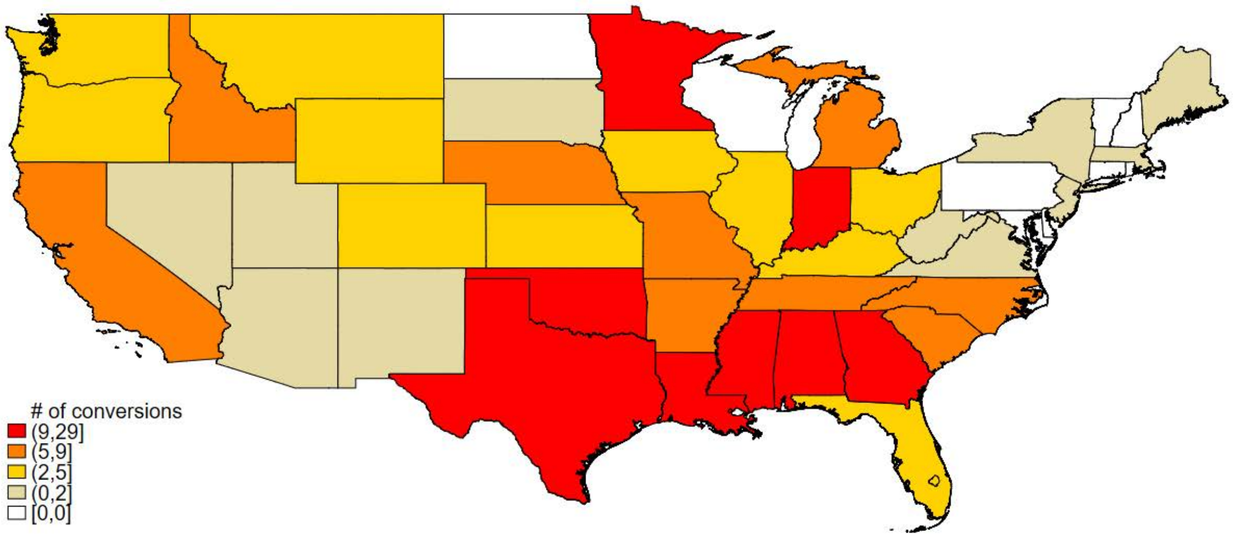


Figure 1: Government role in hospital care

Note: The figure presents overall shares in the US from 1983 through 2019 using American Hospital Association (AHA) survey data. Non-general-acute-care hospitals were included in the sample for share calculations. In Panel (a), we plot the share of total beds and full-time equivalent employees (FTEs) contributed by public, nonfederal hospitals (red and purple dashed lines, respectively) and by public, federal hospitals (blue and black dashed lines, respectively). In Panel (b), the share of Medicaid admissions is given by the orange solid line; the share of Medicare admissions is given by the green dashed line. For Panel (b), the denominator comprises all nonfederal hospitals present in the survey in each year.

(a) By state



(b) By year

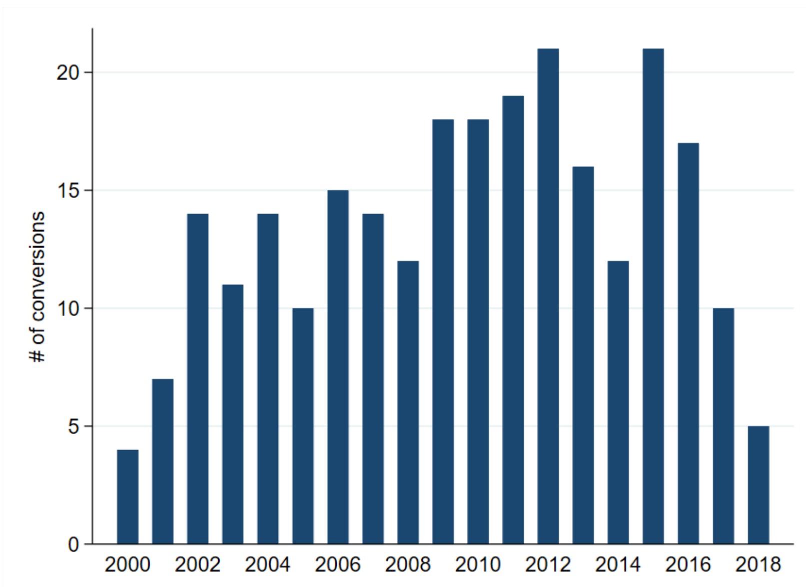


Figure 2: Privatizations

Note: The figure presents the distribution of nonfederal, public-hospital privatizations in our final analysis sample during 2000–18. We restrict the sample to general-acute-care hospitals. Panels (a) and (b) present the distribution by state and by year, respectively. Hawaii and Alaska are not pictured in Panel (a) but are included in the sample and experienced 4 and 1 conversions, respectively.

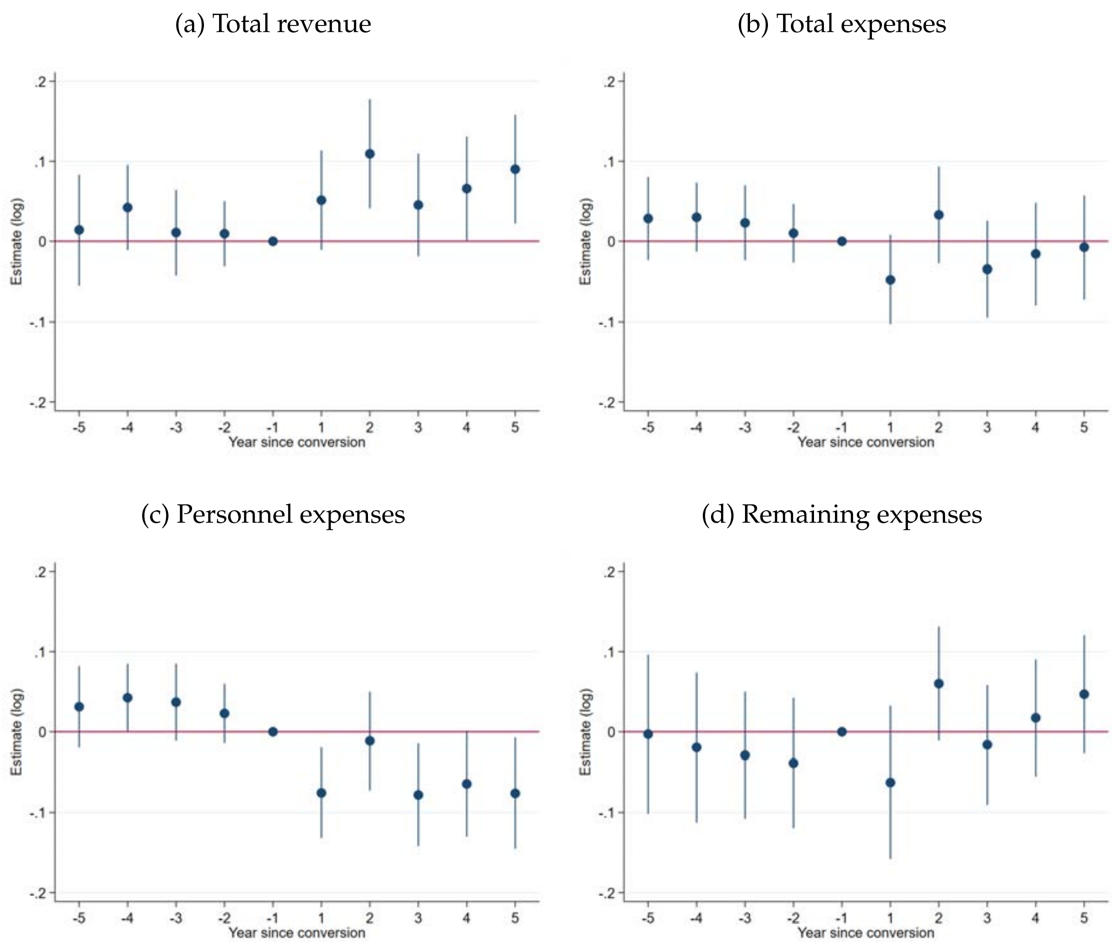


Figure 3: Effects on log finances (per patient)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group consists of hospitals that remain under government control throughout our sample period. The outcomes in Panels (a) and (b) are total revenue (from Medicare cost reports) and total expenses (from AHA), respectively. The total expenses comprise personnel expenses and remaining expenses, shown in Panels (c) and (d), respectively. All outcomes are normalized by contemporaneous adjusted admissions and are presented in logs. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Figure A.4 presents the corresponding plots obtained when we normalize outcomes by the contemporaneous number of beds instead. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital.

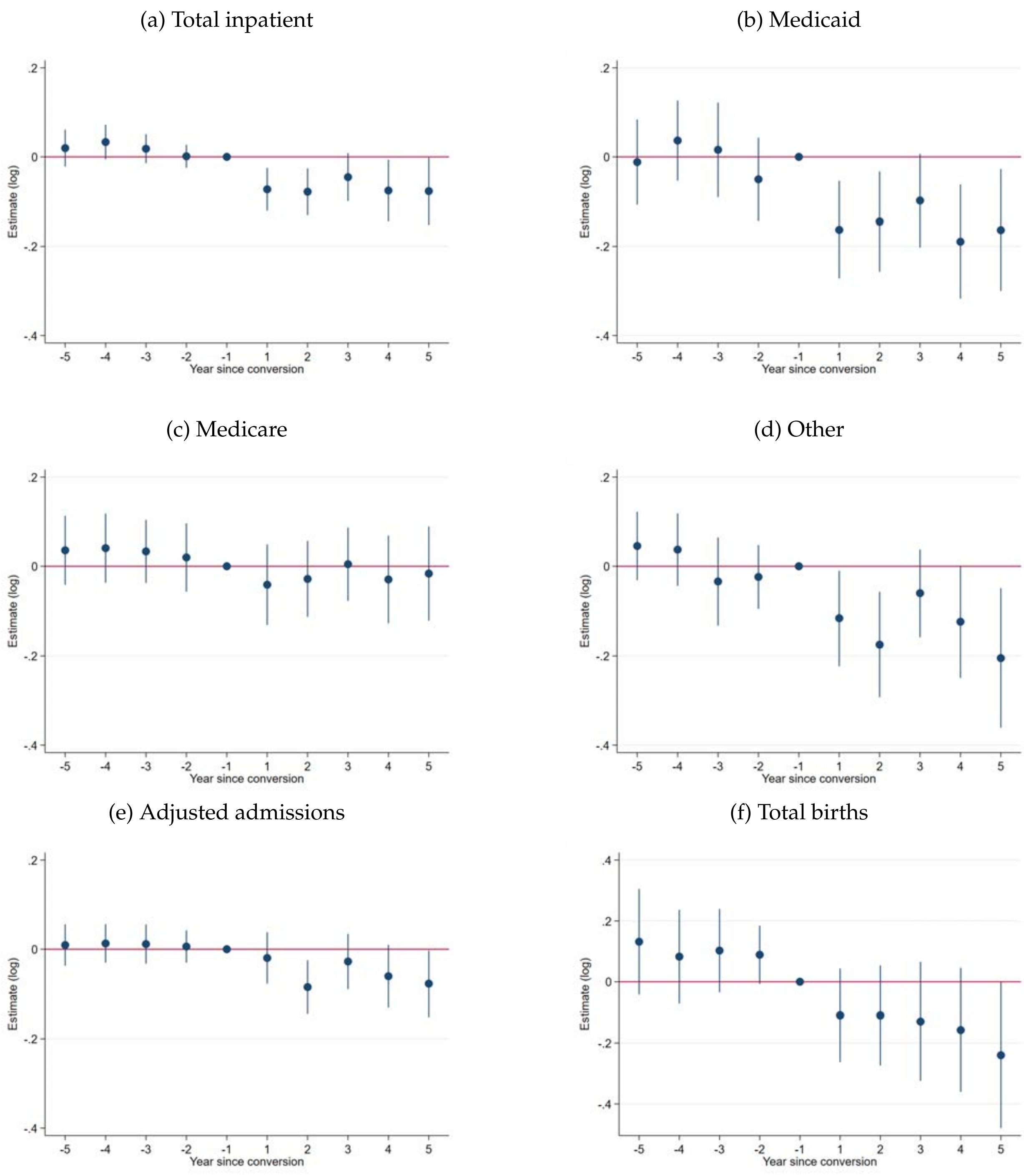


Figure 4: Effects on patient (log) volume

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level patient volume data from the AHA. The comparison group consists of hospitals that remain under government control throughout our sample period. The outcomes are log total inpatient, Medicaid, Medicare, and "Other" admissions in Panels (a), (b), (c), and (d), respectively. Other admissions refers to hospital admissions not covered by Medicaid or Medicare and mainly comprises privately insured and uninsured patients. Panel (e) presents the effect on adjusted admissions, which include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Therefore, it approximates total hospital care volume. Panel (f) presents the effect on total births (excluding fetal deaths). Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital.

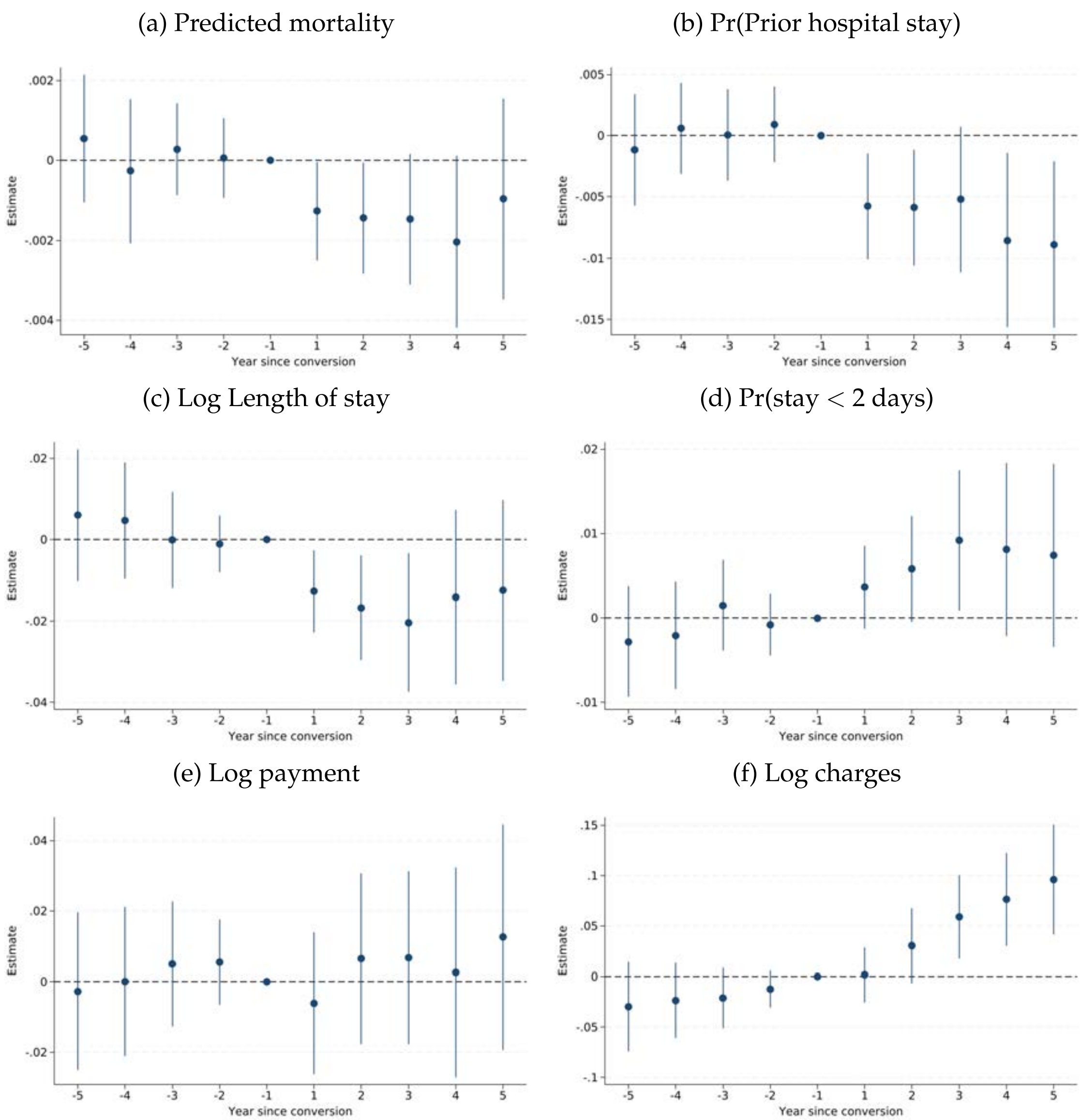
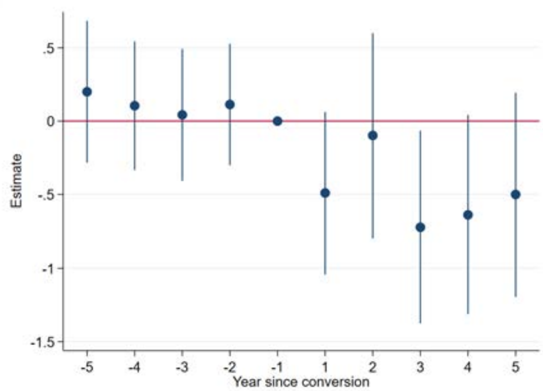


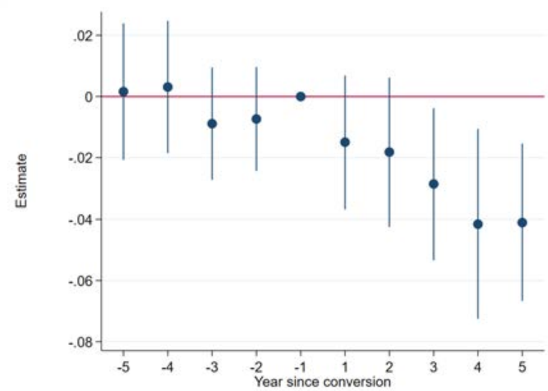
Figure 5: Effects on Medicare fee-for-service patients

Note: The figure presents event study plots obtained by estimating Equation 3 on patient-level Medicare fee-for-service (FFS) claims data. Consistent with our research design, we exclude 55 hospitals that privatized prior to 2005 for this analysis to ensure that we observe at least five pretreatment years for each privatized facility. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The comparison group consists of hospitals that remain under government control throughout our sample period. Patients are 65 years or older and enrolled in Medicare Parts A and B for at least 3 months prior to admission. The outcomes are: (a) predicted mortality within 30 days based on demographics, co-morbidities, and 90-day utilization history; (b) an indicator for the patient having a prior hospital stay in the last 90 days for a nondeferrable condition; (c) logarithm of length of stay; (d) an indicator for discharging the day of admission or the next day; (e) log of total Medicare payment amount; and (f) log of hospital charge for the stay. The latter two values are deflated to be in 2019 dollars. Except in the case of the first two outcomes, the models include a vector of patient demographics and risk attributes, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Table 5 presents the corresponding D-D coefficients and describes the outcome variables in more detail.

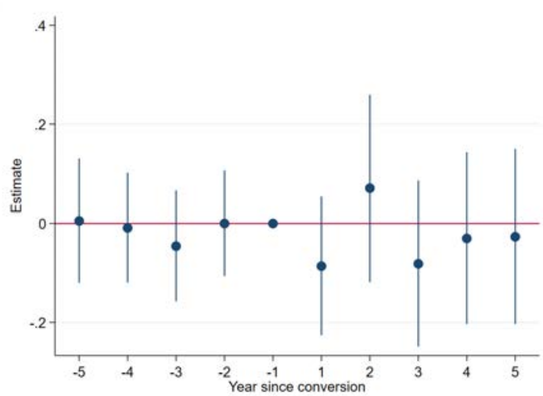
(a) Total FTEs



(b) Physician FTEs



(c) Nurse FTEs



(d) Other FTEs

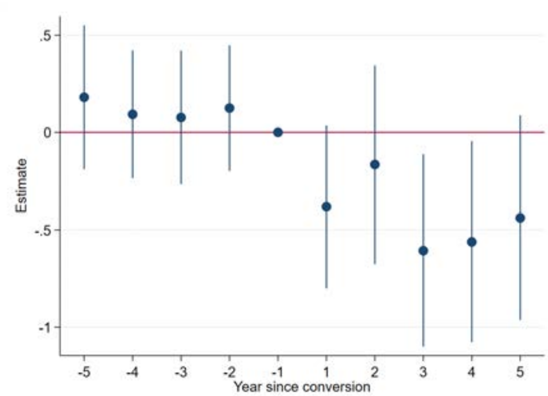


Figure 6: Effects on staff (per 100 adjusted admissions)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The control group consists of hospitals that remain under government control throughout our sample period. The outcomes are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other (all remaining) FTEs in Panels (a), (b), (c), and (d), respectively. We normalize the staff level so that it is expressed per 100 contemporaneous adjusted admissions. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure A.8 presents the corresponding plots obtained using the log of FTE per 100 admissions instead. Figure A.9 presents the corresponding plots obtained when we normalize staff FTE by 100 beds instead.

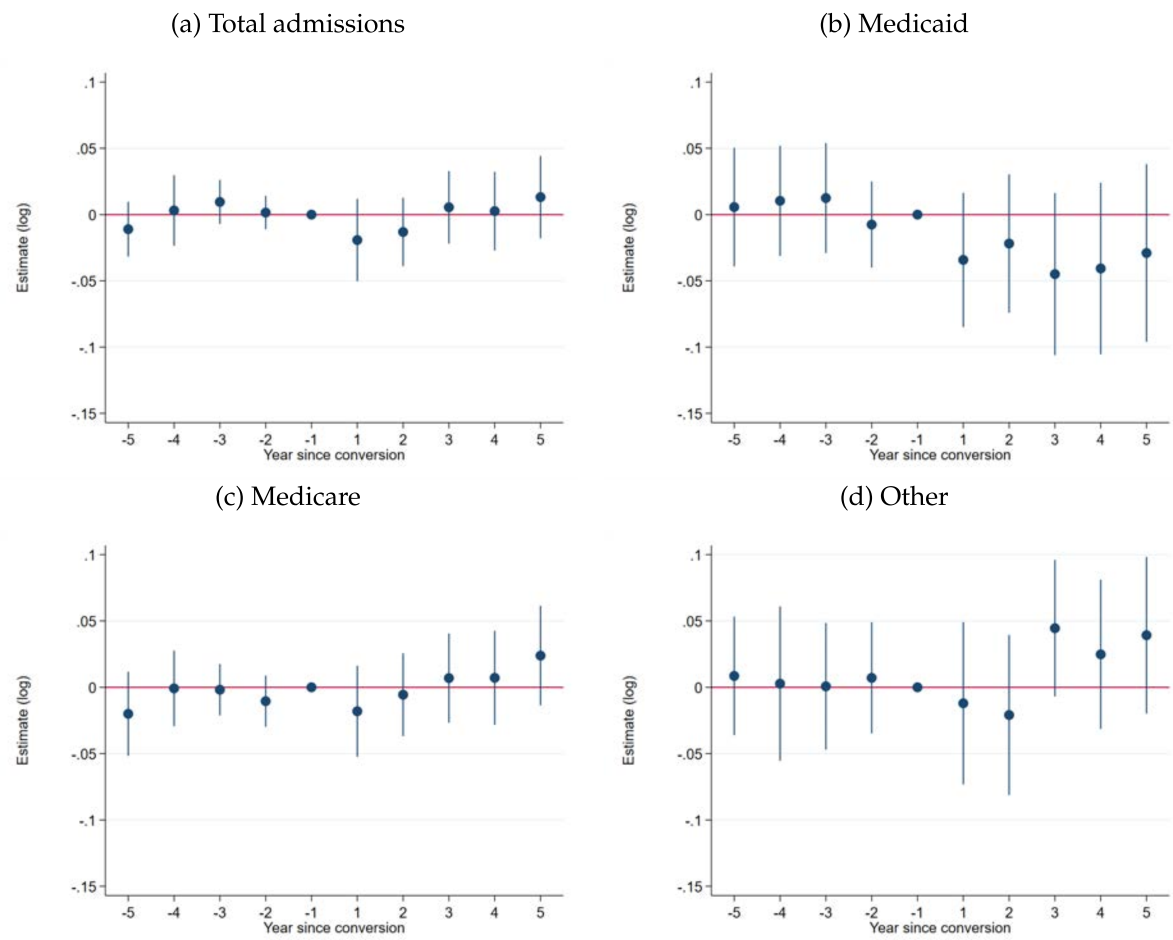


Figure 7: Effects on market-level (log) volume

Note: The figure presents event study plots obtained by estimating the market-level equivalent of Equation 3 on market-year level data. We define hospital markets using health service areas (HSA), described in Section 6. The outcomes are log total, Medicaid, Medicare, and Other admissions in Panels (a), (b), (c), and (d), respectively. "Other" admissions refers to hospital admissions not covered by Medicaid or Medicare and mainly comprises privately insured and uninsured patients. Year zero is the year a market experiences a privatization for the first time and is excluded for treated markets since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by HSA. The corresponding hospital-level figure on patient volume is presented in Figure 4.

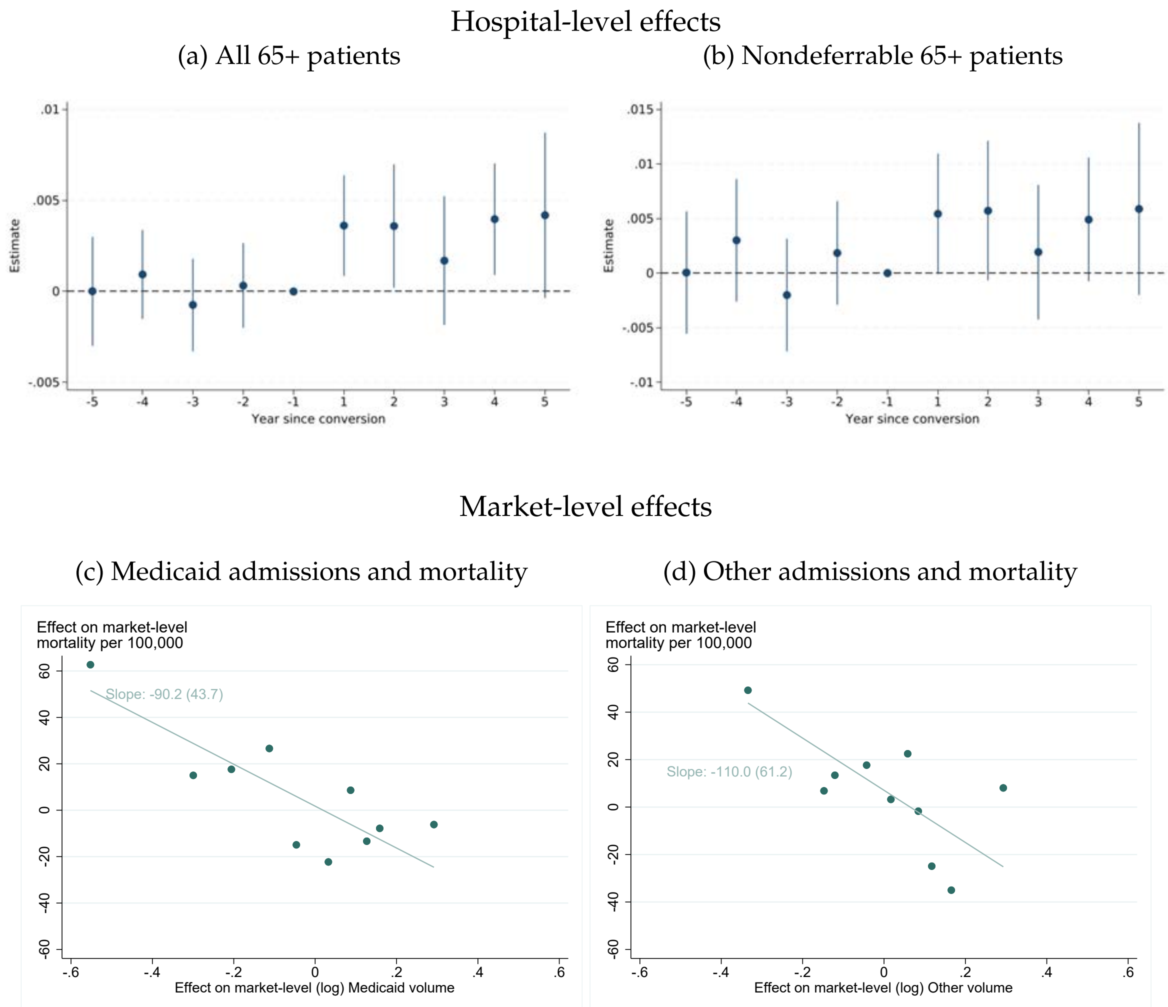


Figure 8: Effects on mortality

<u>Note:</u> The figure presents evidence on the effects of privatization on mortality. The top two panels present event study plots on the effects on 30-day mortality for Medicare fee-for-service (FFS) patients aged 65 or older, where the 30-day period starts on the day of discharge from the hospital. Panel (a) presents the effects for FFS patients regardless of condition, while Panel (b) presents the results specifically for patients admitted through the emergency department for nondeferrable conditions. The latter group is considered less susceptible to selection concerns and is identified following the approach in Doyle Jr et al. (2015). These results are obtained by estimating Equation 3 on patient-level Medicare claims data. The model includes a vector of patient demographics and risk attributes, as described in Section 4. The bottom two panels present evidence on the correlation between the effects of hospital privatization on market-level mortality rates among 55–64 year olds and on market-level volume for Medicaid (Panel c) and "Other" patients (Panel d), respectively, across the approximately 200 markets experiencing privatizations. Each panel presents a binned scatter plot of the effect of privatization on mortality rates among 55–64 year olds per 100,000 population (Y-axis) against the corresponding effect on aggregate hospital volume in logs (X-axis) in decile bins. For each of these outcomes, we first estimate each affected market's D-D coefficient on mortality and on patient volume by comparing its trends to those for the full set of comparison markets. The plots overlay lines of best-fit and slope coefficients from a linear regression using the underlying market-level estimates. Standard errors for slope coefficients are in parentheses; they are bootstrapped over both steps to account for estimation error in the first step, where we obtain market-specific D-D estimates.

Table 1: Shares of hospital beds by type of ownership for select states in 2019

	(1) AL	(2) CA	(3) TX	(4) GA	(5) IL	(6) PA	(7) US Overall
Public (nonfederal)	44.4	22.9	15.8	11.7	8.0	3.8	17.3
							(12.5)
Public (federal)	4.4	3.6	5.8	3.4	3.7	3.6	4.2
							(2.1)
Non-profit	23.4	56.8	37.1	71.5	80.8	79.3	62.9
							(19.2)
For-profit	27.8	16.8	41.3	13.4	7.5	13.3	15.6
							(12.4)
# hospitals	116	419	588	172	208	235	6,090

Notes: The table presents shares of hospital beds by type of ownership for select large states using American Hospital Association survey data (AHA) from 2019. We source this information using the "control" variable in the AHA and use the terms owner and control interchangeably since they are identical in most cases. The states are ordered in descending order of nonfederal public share, which is the top row. The states are selected to illustrate the range in shares of hospitals under different types of ownership. Appendix Table A.1 lists public (nonfederal) hospital bed shares for all states. Non-general-acute-care hospitals were included in the sample for share calculations. Column 7 shows mean shares for the overall US; standard deviations are shown in parentheses.

Table 2: Descriptive statistics

	(1) Privatized	(2) Remaining Public	(3) Private	(4) All
% public	100.0	100.0	0.0	21.3
% for-profit	0.0	0.0	21.1	16.6
% nonprofit	0.0	0.0	78.9	62.1
Admissions	3,095	4,154	7,461	6,703
	(4,404)	(6,917)	(7,702)	(7,587)
Beds	93	120	186	171
	(105)	(162)	(179)	(176)
% Medicaid adm	15.4	16.6	13.0	13.7
	(8.6)	(12.4)	(8.8)	(9.5)
% Medicare adm	49.2	47.4	44.5	45.2
	(15.6)	(16.8)	(13.1)	(13.9)
% other adm	35.4	36.1	42.4	41.0
	(14.4)	(14.0)	(13.9)	(14.2)
Total FTEs/100 adj adm	7.7	9.1	7.5	7.8
	(5.2)	(5.8)	(4.4)	(4.7)
Total revenue/adj adm	8,134	8,310	10,154	9,756
	(9,086)	(8,792)	(18,455)	(16,909)
Total expenses/adj adm	7,320	8,812	9,099	8,960
	(4,003)	(5,302)	(4,647)	(4,744)
Personnel expenses/adj adm	3,909	4,765	4,646	4,627
	(2,082)	(2,827)	(2,287)	(2,379)
# hospitals	258	802	3,925	4,985

Notes: The table presents descriptive statistics on the cross-section of hospitals in the AHA analysis sample as of 1999. In rare instances in which we do not observe a hospital in 1999, we use values from that hospital's first year in the data. Appendix B.2 describes the sample construction restrictions in detail. Column 1 describes government hospitals that privatized during the sample period. These comprise the treated units. Column 2 describes the comparison group, government hospitals that did not experience a change in ownership during this period. Column 3 describes all privately owned, nonprofit and for-profit hospitals that were not converted to government control during this period. Column 4 presents the corresponding values for the entire sample. "Other" admissions refers to hospital admissions not covered by Medicaid or Medicare and mostly comprises privately insured and uninsured patients. "adj adm" refers to adjusted admissions, which include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Total revenue is sourced from the Medicare cost reports (HCRIS) and is the only outcome variable in the table that is not sourced from the AHA. Standard deviations are shown in parentheses.

Table 3: Effects on log finances (per patient)

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
DD	0.057	-0.033	-0.086	0.024
	(0.026)	(0.024)	(0.024)	(0.036)
Obs	16,673	16,673	16,673	16,673
DD	0.089	-0.009	-0.061	0.046
	(0.026)	(0.024)	(0.024)	(0.036)
Obs	16,662	16,662	16,662	16,662
Mean outcome (t-1)	8,109	8,444	4,604	3,840

Notes: The table presents effects on revenue and expenses at the privatized hospitals, obtained by estimating Equation 2 on hospital-year level data. All outcomes are normalized by contemporaneous adjusted admissions and presented in logs. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Column 1 presents results for total revenue (inpatient plus outpatient revenue minus contractual allowances and discounts), obtained from Medicare cost reports. Column 2 presents results for total expenses, which comprises personnel expenses (column 3) and remaining expenses (column 4), all of which are obtained from the American Hospital Association survey. Because Medicare cost reports data begins two years after the start of our AHA sample and is missing for some hospitals, we drop any hospital-year observations with missing values for total revenue, which allows for the same sample across outcomes. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying hospital and county-level controls as described in Section 4. The mean values pertain to outcomes (in levels) at privatized hospitals in the year before privatization. Standard errors are clustered by hospital and are presented in parentheses. Table A.5 presents the corresponding point estimates obtained when we normalize outcomes by the contemporaneous number of beds instead.

Table 4: Effects on patient (log) volume, payer, and service mix

A: AHA payer volume	(1) Total	(2) Medicaid	(3) Medicare	(4) Other	(5) Adjusted
A1: No controls					
DD	-0.084	-0.149	-0.049	-0.138	-0.060
	(0.027)	(0.042)	(0.030)	(0.043)	(0.026)
Obs	20,998	20,997	20,997	20,997	20,998
A2: Market controls					
DD	-0.096	-0.170	-0.072	-0.139	-0.067
	(0.028)	(0.042)	(0.031)	(0.044)	(0.026)
Obs	19,385	19,384	19,384	19,384	19,385
Mean outcome (t-1)	3,014	617	1,351	1,046	7,025

B: State payer volume	(1) Total	(2) Medicaid	(3) Medicare	(4) Other	(5) Private	(6) Uninsured	(7) Miscellaneous
DD	-0.117	-0.224	-0.071	-0.061	-0.046	-0.468	0.277
	(0.041)	(0.091)	(0.048)	(0.071)	(0.082)	(0.167)	(0.163)
Obs	8,721	8,721	8,721	8,721	8,721	8,721	8,721
Mean outcome (t-1)	6,093	1,147	2,722	2,224	1,702	383	139

C: Obstetric volume	(1) Ob adm.	(2) Ob closure	(3) Ob adm. excluding clos.	(4) AHA births
DD	-0.768	0.133	0.287	-0.224
	(0.287)	(0.048)	(0.378)	(0.086)
Obs	5,746	5,746	5,627	20,998
Mean outcome (t-1)	1,024	0.188	1,642	336

Notes: The table presents estimated effects on patient volume in privatized hospitals obtained by estimating Equation 2 on hospital-year-level data. Panel A presents results using AHA data. Columns 1, 2, 3, and 4 present the effects on log total, Medicaid, Medicare, and other admissions, respectively. "Other" admissions refer to hospital admissions not covered by Medicaid or Medicare. Panel A1 reports coefficients from a two-way fixed-effects specification without covariates. Panel A2 reports coefficients from a specification that includes time-varying hospital and county-level covariates described in Section 4. Panel A2 has fewer observations since the market-level covariates are not available for 1995 and 1996. Panels B and C present results using data from five states (CA, FL, IN, MN, and WA) on inpatient volume and obstetric volume, respectively. We estimate synthetic difference-in-differences models using the "sdid" command with placebo inference using 200 replications. In Panel B, we also disaggregate "Other" into three groups: privately insured, uninsured, and miscellaneous (e.g., workers compensation), respectively. Panel C column 1 presents the total effect on obstetric inpatient volume. Columns 2 and 3 present the effects on the extensive and intensive margins, respectively. Panel C column 4 presents the effect on total births (excluding fetal deaths) using the usual AHA national sample. We include this in Panel C rather than in Panel A so that it is along with the results on obstetrics from the states sample. The mean values are calculated for privatized hospitals in the year before privatization. Standard errors are clustered by hospital and are presented in parentheses.

Table 5: Effects on patient complexity and billing

	(1) Pred. mortality	(2) Pr(prior stay)	(3) Log LOS	(4) Pr(stay<2 days)	(5) Log (payment)	(6) Log (charges)
A: Patient controls						
DD	-0.0016	-0.0067	-0.0174	0.0075	0.0014	0.0641
	(0.0007)	(0.0021)	(0.0067)	(0.0029)	(0.0135)	(0.0190)
B: Patient and mkt. controls						
DD	-0.0021	-0.0056	-0.0213	0.0083	0.0020	0.0506
	(0.0007)	(0.0021)	(0.0070)	(0.0030)	(0.0134)	(0.0206)
Mean outcome (t-1)	0.122	0.143	5.724	0.129	8,902	31,357
Observations	13,097,798	13,097,798	13,097,798	13,097,798	12,960,951	13,097,165

Notes: The table shows the results of various outcomes for Medicare fee-for-service (FFS) patients using regressions estimated using patient–level data during 2000–19. We use the same sample of hospitals as in the main analysis, other than dropping 55 hospitals privatized before 2005 to ensure that we can observe all privatized hospitals for at least 5 years prior to conversion. The sample is limited to Medicare FFS patients 65 years or older and enrolled in Parts A and B for a minimum of 3 months at admission. With the exception of the outcomes in columns 1 and 2 that study patient complexity, the results are from a specification that includes patient covariates, as described in Section 4. The model in Panel B also includes hospital and county-level covariates. The outcomes are as follows: (1) predicted 30-day mortality, obtained using a probit model on demographics, diagnosis codes, and past utilization; (2) an indicator of hospitalization in the last 30 days for a nondeferrable condition, as defined by Doyle Jr et al. (2015); (3) length of stay (in logs); (4) the probability of being discharged on the same or next day after admission; (5) log of total Medicare payment for the stay; and (6) log of hospital charges for the admission. Standard errors are clustered by hospital and are presented in parentheses.

Table 6: Effects on staff

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-0.57	-0.03	-0.02	-0.52	-0.01
	(0.26)	(0.01)	(0.06)	(0.19)	(0.01)
Obs	20,998	20,998	20,998	20,998	8,632
B: Market controls					
DD	-0.36	-0.02	0.02	-0.36	-0.02
	(0.26)	(0.01)	(0.07)	(0.19)	(0.01)
Obs	19,385	19,385	19,385	19,385	8,628
Mean outcome (t-1)	7.40	0.10	1.90	5.30	0.20

Notes: The table presents effects on full-time equivalent (FTE) employed staff at the privatized hospitals, obtained by estimating Equation 2 on hospital-year level data. Column 1 presents results for total FTE, which comprises physicians, nurses, and others (all remaining), presented in columns 2, 3, and 4, respectively. We normalize the number of FTEs so that it is expressed per 100 contemporaneous adjusted admissions. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Column 5 presents results for contract FTEs, which come from Medicare cost reports and include management and patient care staff. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a specification including time-varying hospital and county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to the outcomes (in levels) at privatized hospitals in the year before privatization. Standard errors are clustered by hospital. Table A.8 presents the corresponding results obtained when we use log of FTEs instead. Table A.9 presents the corresponding results obtained when we normalize staff FTE by 100 beds instead.

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Finances			Volume				Staff	
	Revenue	Tot. Exp.	Pers. Exp.	Total	Medicaid	Medicare	Other	Total	Physician	Other
Baseline	0.057	-0.033	-0.086	-0.084	-0.149	-0.049	-0.138	-0.57	-0.03	-0.52
	(0.026)	(0.024)	(0.024)	(0.027)	(0.042)	(0.030)	(0.043)	(0.26)	(0.01)	(0.19)
I: Specification checks										
A. Weighting by beds	0.036	-0.079	-0.122	-0.091	-0.165	-0.080	-0.111	-0.77	-0.04	-0.61
	(0.026)	(0.032)	(0.024)	(0.029)	(0.044)	(0.034)	(0.048)	(0.22)	(0.02)	(0.16)
B. State-year FEs	0.109	0.003	-0.050	-0.100	-0.154	-0.067	-0.176	-0.20	-0.02	-0.24
	(0.027)	(0.025)	(0.025)	(0.029)	(0.044)	(0.032)	(0.045)	(0.27)	(0.01)	(0.20)
C. Incl. pre-trend	0.037	-0.047	-0.102	-0.100	-0.171	-0.060	-0.156	-0.62	-0.04	-0.56
	(0.024)	(0.024)	(0.024)	(0.028)	(0.044)	(0.031)	(0.046)	(0.27)	(0.01)	(0.20)
II: Alternate estimators										
A. CS estimator	0.066	-0.017	-0.063	-0.064	-0.146	-0.015	-0.124	-0.52	-0.03	-0.45
	(0.028)	(0.026)	(0.027)	(0.026)	(0.053)	(0.044)	(0.048)	(0.27)	(0.01)	(0.20)
B. DCDH estimator	0.064	0.013	-0.031	-0.057	-0.122	-0.016	-0.110	-0.33	-0.02	-0.29
	(0.023)	(0.022)	(0.023)	(0.019)	(0.048)	(0.034)	(0.037)	(0.23)	(0.01)	(0.17)
C. Synthetic DiD	0.036	-0.047	-0.092	-0.024	-0.116	-0.023	-0.105	-0.93	-0.03	-0.72
	(0.029)	(0.023)	(0.023)	(0.027)	(0.045)	(0.040)	(0.039)	(0.28)	(0.01)	(0.22)
III: Alternate samples - treatment group										
A. Balanced panel	0.071	-0.020	-0.068	-0.059	-0.146	-0.027	-0.096	-0.56	-0.02	-0.51
	(0.029)	(0.029)	(0.029)	(0.031)	(0.046)	(0.034)	(0.048)	(0.28)	(0.01)	(0.21)
B. All treated obs	0.028	-0.044	-0.103	-0.076	-0.143	-0.058	-0.148	-0.68	-0.03	-0.56
	(0.026)	(0.022)	(0.023)	(0.032)	(0.048)	(0.035)	(0.043)	(0.25)	(0.01)	(0.18)
IV: Alternate samples - comparison group										
A. Matched sample	0.043	-0.056	-0.110	-0.027	-0.122	-0.018	-0.085	-0.82	-0.03	-0.69
	(0.031)	(0.028)	(0.028)	(0.029)	(0.051)	(0.040)	(0.049)	(0.31)	(0.01)	(0.23)
B. Switchers included	0.061	-0.032	-0.085	-0.083	-0.147	-0.047	-0.138	-0.59	-0.02	-0.53
	(0.026)	(0.024)	(0.024)	(0.027)	(0.042)	(0.030)	(0.043)	(0.26)	(0.01)	(0.20)

Notes: The table shows the results of robustness checks for the results using the AHA sample presented in Tables 3, 4A, and 6, respectively. For brevity, we do not present results for outcomes where we do not detect effects, such as non-personnel expenses and nurse employment. Row IA uses static hospital beds to weight hospitals. Row IB includes state×year fixed effects and time-varying hospital and county controls. Row IC includes hospital-specific trends that are first estimated using data from 1995–1999. This analysis uses 2000–2019 data while dropping privatizations in 2000 and 2001. Row IIA presents the Callaway and Sant'Anna (2020) estimator obtained using the *csdid* command. Row IIB presents the De Chaisemartin and d'Haultfoeuille (2020) estimator, implemented using the *did_multiplegt* command. This sample also includes the 60 hospitals that converted from private to government control, as the estimator can accommodate reverse treatment. We average the five estimated dynamic effects and calculate standard errors via 100 bootstrap replications. Row IIC uses the *sdid* command with 100 replications and placebo inference to obtain the synthetic D-D estimator. Row IIIA keeps only treated hospitals that we observe for five years before and after the transition, which primarily excludes treated hospitals that privatized after 2014. Row IIIB uses all treated observations, including those from the year of privatization and those beyond the five-year window around privatization (if available). Row IVA presents results estimated on a matched subsample using propensity score matching (see Section C.1 for details). Panel IVB includes additional comparison hospitals that switch between public and private and were omitted from the main sample. See Section 5.4 for additional details.

Table 8: Market-level descriptive statistics

	(1) Treated HSAs	(2) Control HSAs	(3) Total
# treated hospitals	1.3	0.0	0.3
	(0.6)	(0.0)	(0.6)
Total hospitals	6.1	4.6	4.9
	(5.6)	(6.6)	(6.4)
Total admissions	37,723	31,641	32,977
	(59,193)	(81,333)	(77,030)
Total beds	976	805	842
	(1,444)	(1,962)	(1,861)
% Medicaid adm	15.6	14.1	14.4
	(6.3)	(6.7)	(6.6)
% Medicare adm	44.9	47.2	46.7
	(9.9)	(9.5)	(9.6)
% other adm	39.5	38.7	38.8
	(11.0)	(9.9)	(10.1)
Total FTEs/100 adj adm	7.2	7.2	7.2
	(2.3)	(3.3)	(3.1)
% in poverty	14.1	13.0	13.3
	(5.0)	(4.8)	(4.8)
% unemployment	4.9	4.7	4.8
	(2.3)	(2.4)	(2.4)
% uninsurance	20.6	19.1	19.4
	(6.0)	(5.7)	(5.8)
HHI (admissions)	4,574	5,565	5,347
	(2,434)	(2,831)	(2,778)
All-cause mortality (all ages)	974.7	1008.2	1000.8
	(210.7)	(230.5)	(226.6)
All-cause mortality (ages 55–64)	1084.7	1036.0	1046.7
	(253.5)	(247.5)	(249.5)
# HSAs	204	725	929

Notes: The table presents descriptive statistics for the market-level sample, where markets are defined by Health Service Areas (HSAs) defined by the US Census. We use values from 1999 for most HSAs. In rare instances where we do not observe an HSA in 1999, we use values from that HSA's first year in the data. The treated HSAs have at least one hospital that undergoes public to private conversion during 2000–18. Control HSAs do not have any conversions during our sample period. Values related to hospital care are sourced from the American Hospital Association survey. Mortality rates are sourced from national vital statistics and are reported as the number of deaths per 100,000 population of the respective age group. All rows present means and standard deviations (in parentheses).

Table 9: Effects on aggregate patient volume

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: No controls				
DD	-.004	-.038	.008	.009
	(.014)	(.024)	(.016)	(.022)
Obs	19985	19985	19985	19985
B: Market controls				
DD	-.021	-.053	-.012	-.011
	(.015)	(.024)	(.016)	(.022)
Obs	18522	18522	18522	18522
C: Heterogeneity by market HHI				
DD	.043	.034	.045	.065
	(.015)	(.022)	(.017)	(.017)
x 1(> med. HHI)	-.095	-.146	-.074	-.113
	(.027)	(.046)	(.029)	(.041)
D: Heterogeneity by market poverty				
DD	.020	.042	.029	.016
	(.021)	(.028)	(.021)	(.030)
x 1(> med. poverty)	-.047	-.160	-.042	-.014
	(.027)	(.045)	(.030)	(.042)
Mean outcome (t-1)	40,587	7,792	16,885	15,909

Notes: The table presents estimated effects on patient volume at the market level. We define markets using Health Service Areas (HSAs), as described in Section 6. Columns 1, 2, 3, and 4 present the effects on log total, Medicaid, Medicare, and other admissions, respectively. "Other" refers to hospital admissions not covered by Medicaid or Medicare and mostly comprises privately insured and uninsured patients. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a specification including time-varying, HSA-level controls: population, unemployment, uninsurance, and poverty rates. Other controls include the share of hospitals in the HSA that are 340B providers and a time-varying indicator for being located in a Medicaid expansion state. Panel B has fewer observations since the covariates are not available for 1995 and 1996. Panel C presents the corresponding results from a triple difference specification including an interaction term with an indicator for the market having a Herfindahl-Hirschman index (based on admission shares) in 1999 greater than the median among treated markets. Panel D is analogous to Panel C but instead includes an interaction term with an indicator for the market having a poverty rate in 1999 greater than the median. The mean values pertain to patient volume (in levels) in the treated markets in the year prior to privatization. Standard errors are clustered by HSA and are presented in parentheses.

Table 10: Effects on mortality

A: Hospital-level effect	(1) All patients	(2) Non-deferrable	(3) Age $65-80$	(4) Age>80	(5) Medical	(6) Surgical
A1: Patient controls						
DD	0.0033	0.0042	0.0019	0.0048	0.0036	0.0019
	(0.0012)	(0.0018)	(0.0013)	(0.0016)	(0.0014)	(0.0013)
A2: Patient and mkt. controls						
DD	0.0038	0.0045	0.0023	0.0057	0.0040	0.0024
	(0.0013)	(0.0019)	(0.0013)	(0.0017)	(0.0015)	(0.0013)
Mean outcome (t-1)	0.119	0.176	0.089	0.153	0.131	0.072
Observations	13,097,798	3,194,371	7,386,100	5,711,696	10,099,836	2,897,147

B: Market-level effect	(1) All	(2) Affected county	(3) Other counties	DD	(4) x 1(> med. poverty)
B1: No controls					
DD	5.2	17.3	-5.0	-2.2	38.8
	(6.6)	(11.7)	(8.4)	(13.6)	(22.9)
Obs	19,985	19,985	19,833	19,985	
B2: Market controls					
DD	6.5	18.8	-3.3	3.1	31.2
	(6.6)	(11.8)	(8.5)	(13.6)	(23.0)
Obs	18,522	18,522	18,371	18,522	
Mean outcome (t-1)	1,022.4	1,026.5	1,011.5	1,026.5	

Notes: The table presents evidence on the effects of privatization on mortality. Panel A presents hospital-level effects using claims data on the universe of Medicare fee-for-service patients aged 65 or older. The outcome is 30-day mortality for patients, calculated from the date of discharge from the hospital. The coefficients are obtained by estimating Equation 2 on patient-level data and are adjusted for differences in patient risk, as described in Section 4. The different columns present the estimated effect on different samples: (1) all patients regardless of condition; (2) patients admitted through the emergency department for a nondeferrable condition, identified following Doyle Jr et al. (2015); (3) patients aged 65–80; (4) patients aged more than 80; (5) patients admitted for a "medical" major diagnostic category (MDC); and (6) patients admitted for a "surgical" MDC. A small fraction of patients could not be assigned an MDC. Standard errors are clustered by hospital and are presented in parentheses. Panel B presents market-level effects on all-cause mortality (per 100,000) for ages 55–64. We define markets using Health Service Areas (HSAs), as described in Section 6. Panel B, column 1 presents mortality effects in which all counties that comprise an HSA are included. Panel B, columns 2 and 3 present mortality effects for counties in which a privatization occurred and all remaining counties in the HSA, respectively. 16 HSAs contain only a single county that is treated and hence the model in column 3 has fewer observations. Column 4 presents results from a triple difference version of column 2 which includes an interaction term with an indicator for treated counties located in markets with above-median poverty. All regressions in Panel B involve the same control group of HSAs that never experienced a privatization. The mean values are computed for privatized hospitals, affected markets, or counties in the year before treatment. For Panel B, column 4, the mean mortality rates (in the year before privatization) are 904.1 and 1,161.1 for below and above-median poverty treated counties, respectively. Rows A2 and B2 present results from models that include time-varying, HSA-level controls: population, unemployment, uninsurance, and poverty rates. Other controls include the share of hospitals in the HSA that are 340B providers and a time-varying indicator for being located in a Medicaid expansion state.

A Additional figures and tables

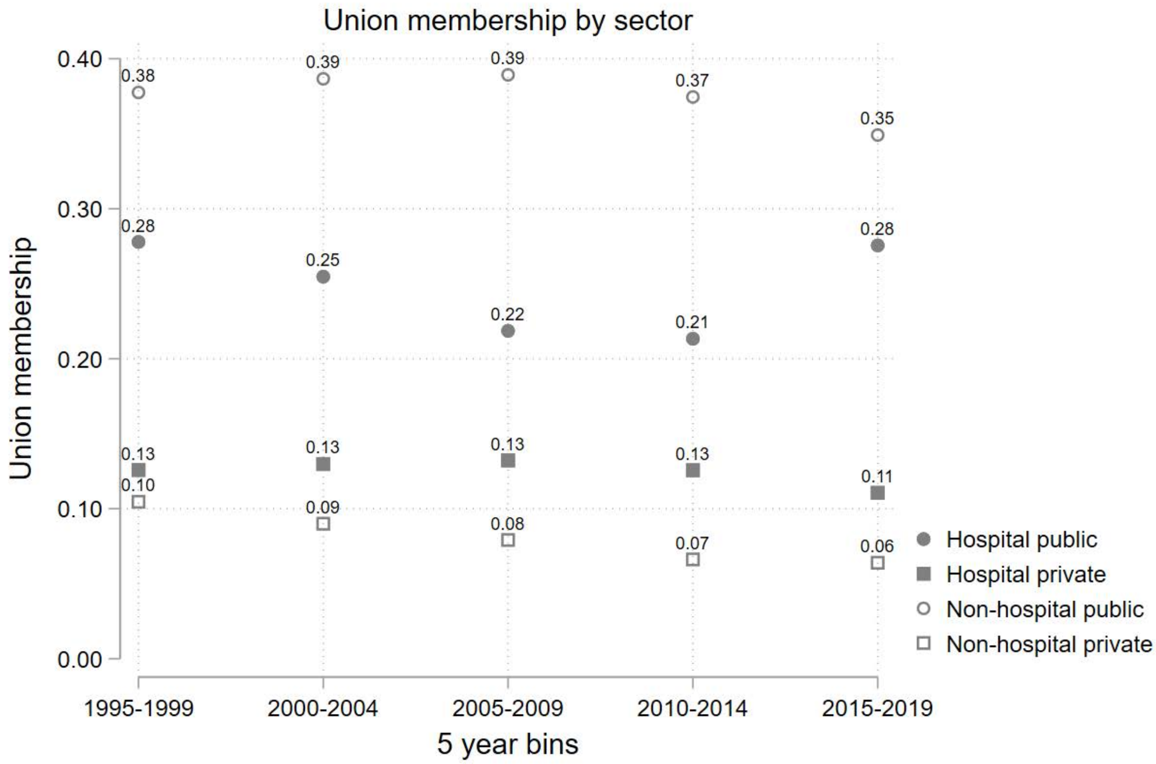


Figure A.1: Unionization among public and private employees

<u>Note:</u> The figure presents the mean share of unionized employees in government and private companies over time. We plot the values separately for employees of hospitals and all other firms within each group. The underlying data are sourced from the Annual Social and Economics Supplement of the Current Population Survey (ASEC) over 1995–2019. The ASEC includes individuals who are surveyed in the "earner study," which includes approximately one quarter of the CPS sample. We restrict the sample to include: individuals who are part of the earner study; employed: at work or has a job but not at work; older than 17 and younger than 66. Due to the small sample sizes, we pool data from five years to compute the means. Individuals are weighted by the corresponding population weights provided in the survey. We identify the hospital industry using the NAICS1990 code 831 and the NAICS1997 code 622.

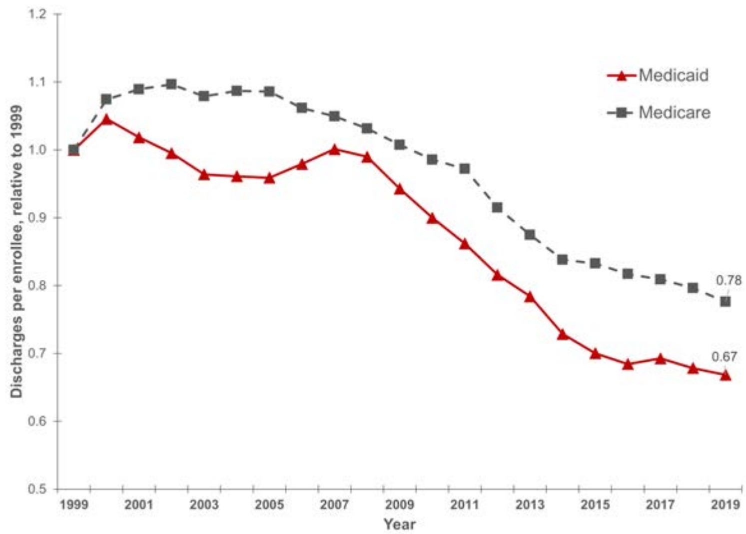


Figure A.2: Hospital utilization by Medicaid and Medicare beneficiaries

Note: The figure presents the trend in hospital stays per beneficiary for Medicaid and Medicare recipients during 1999–2019. Since the absolute levels differ between the two groups, and to focus attention on the relative changes over time, we normalize the number of stays per beneficiary in each group to its respective level in 1999. Hospital stay values are sourced from the National Health Expenditure Tables, while enrollment numbers are sourced from CMS.

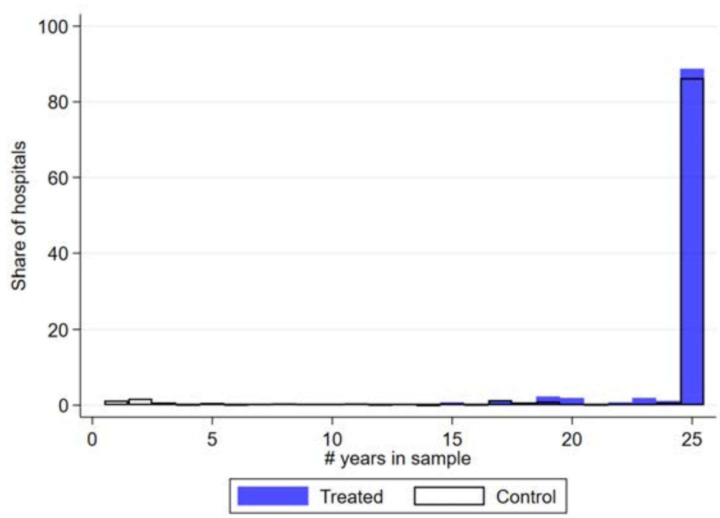


Figure A.3: Balance of hospital panel

Note: The figure presents a frequency distribution of the number of years a hospital is observed in the sample, separately for privatized (treated) and comparison hospitals. The maximum number of years possible is 25 (1995–2019).

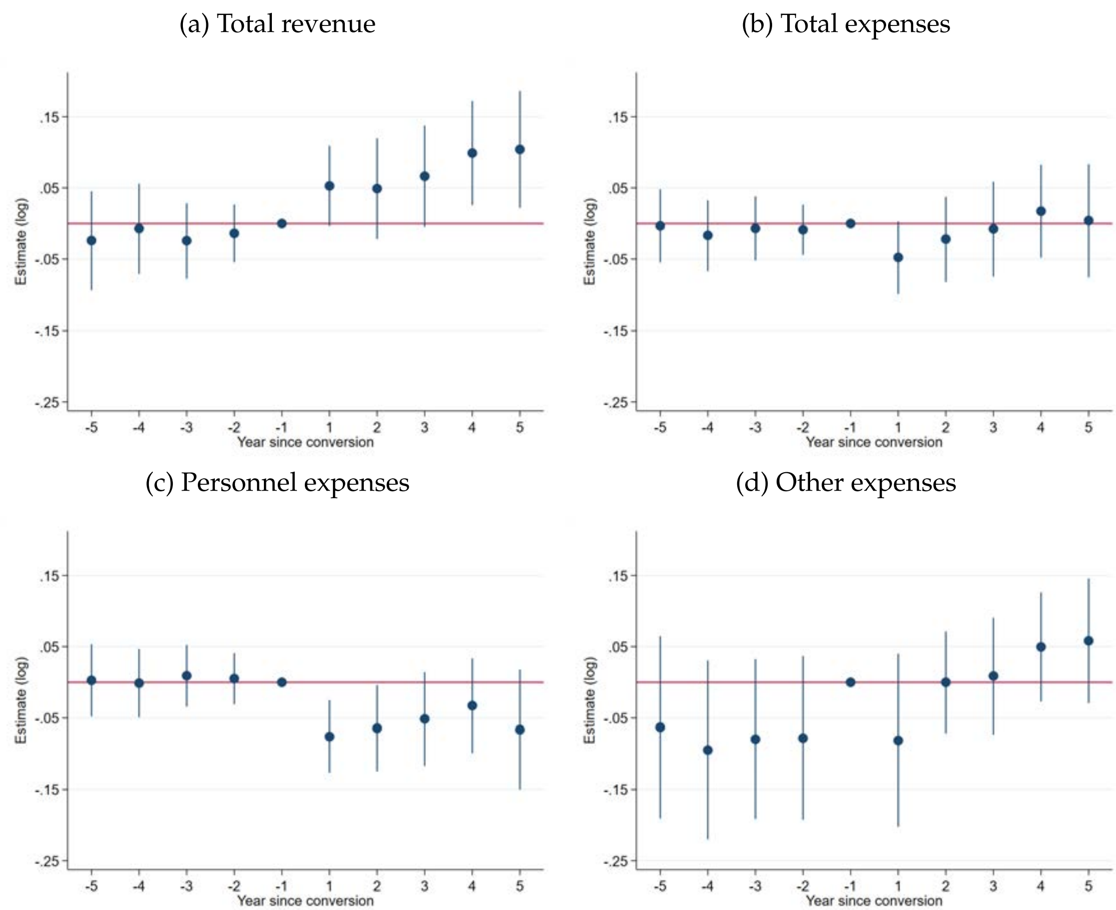
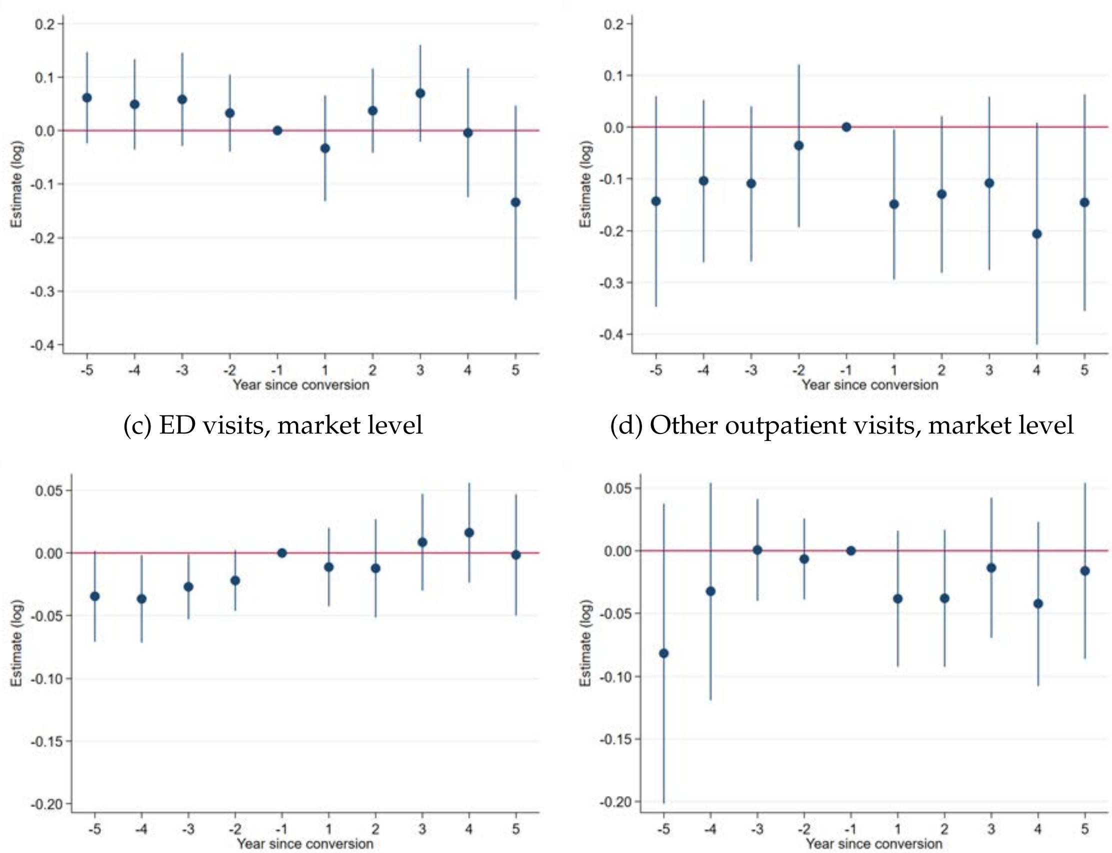


Figure A.4: Effects on log finances (per bed)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period. The outcomes in Panels (a) and (b) are total revenue (from Medicare cost reports) and total expenses (from AHA), respectively. Total expenses comprise personnel expenses and remaining expenses, shown in Panels (c) and (d), respectively. All four outcomes are normalized by contemporaneous number of hospital beds and presented in logs. Figure 3 presents the corresponding event study plots obtained when the outcomes are normalized by adjusted admissions instead. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital.

(a) ED visits, hospital level

(b) Other outpatient visits, hospital level



(c) ED visits, market level

(d) Other outpatient visits, market level

Figure A.5: Effects on ED and other outpatient visits

Note: The figure presents event study plots obtained by estimating Equation 3 on AHA data at the hospital-level (Panels (a) and (b)) and market-level (Panels (c) and (d)). The outcomes are log emergency department (ED) and non-ED, or other outpatient visits. Year zero is the year of privatization and is excluded for privatized hospitals, since it represents partial treatment. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital in Panels (a) and (b) and by market in Panels (c) and (d).

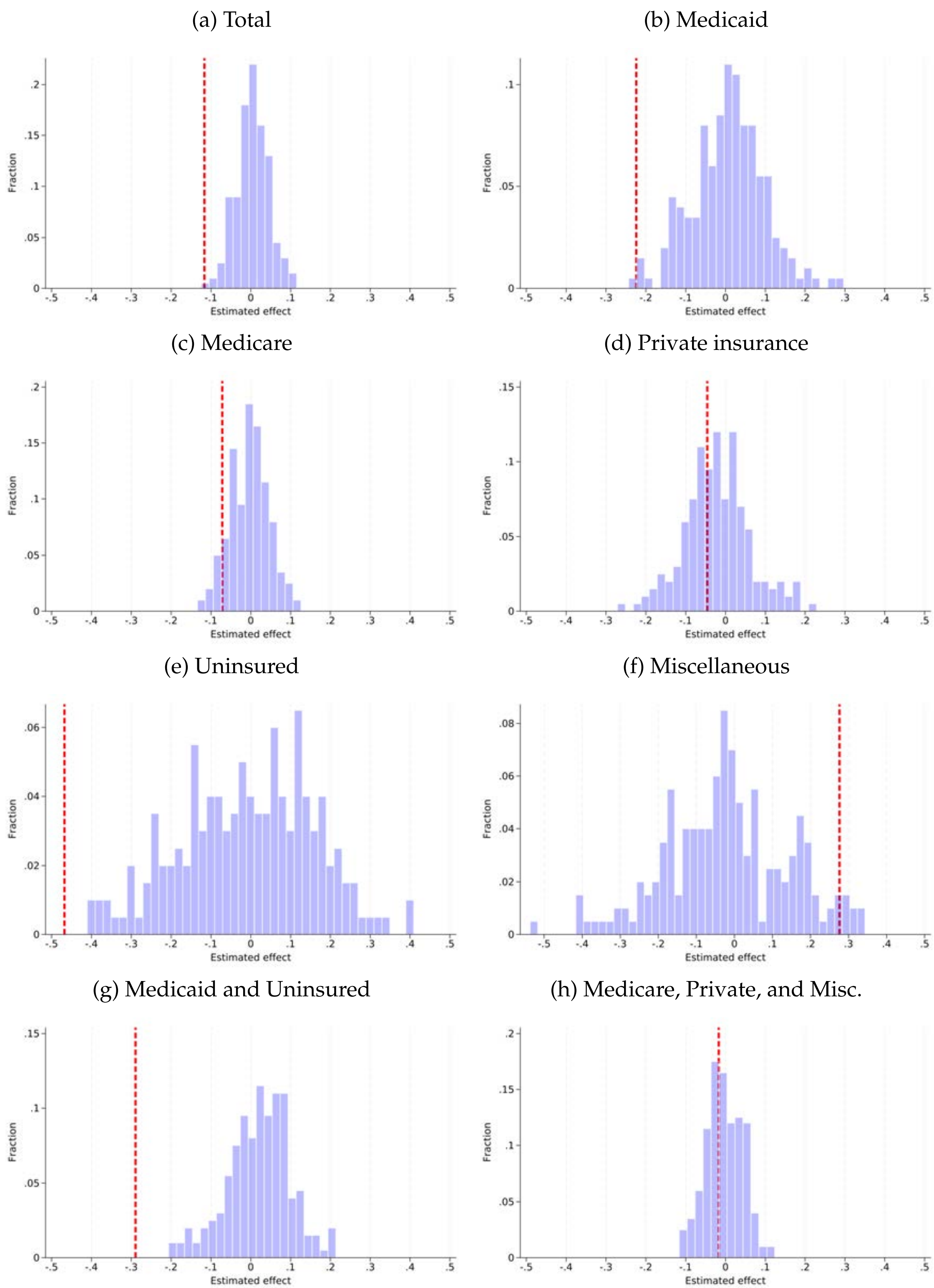
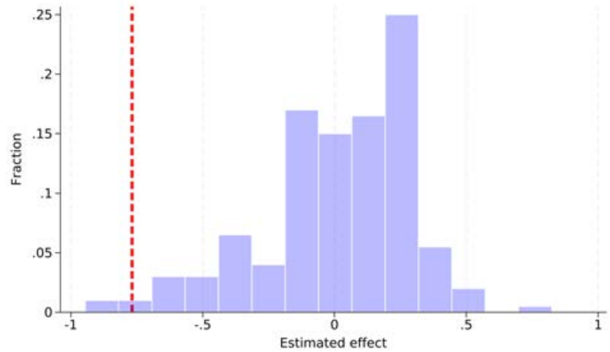


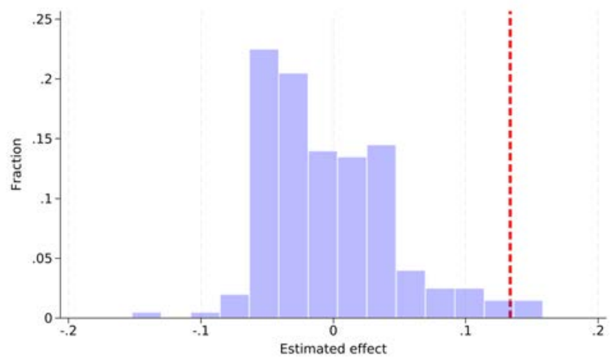
Figure A.6: Effects on admissions by payer using state data

Note: The figure presents distributions of estimated placebo effects on inpatient volume by payer using data during 2003–2019 from California, Florida, Indiana, Minnesota, and Washington. We limit the sample to privatizations occurring over 2008–18 in order to have min. 5 pre-treatment years for each event, as in the other analyses. This leads to a sample with 27 privatizations. We obtain the placebo estimates using the “sdid” command with the placebo inference option and 200 replications. The red vertical lines indicate the estimated effect for the privatized hospitals in these states. Section 5.2.2 provides more details.

(a) Obstetric admissions



(b) Obstetric closure



(c) Obstetric adm. excluding closure

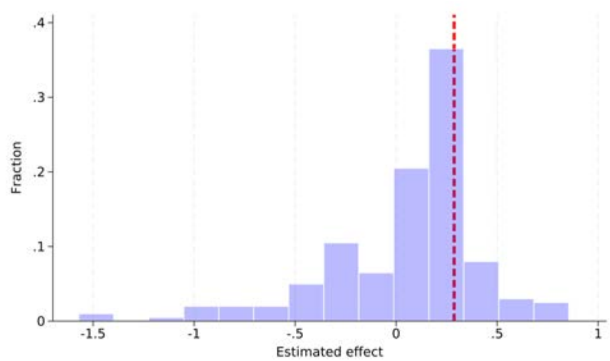


Figure A.7: Effects on obstetric admissions using state data

Note: The figure presents distributions of estimated placebo effects on obstetric admissions and closure using 2003–2019 data from California, Florida, Indiana, and Washington. Minnesota data was dropped because it only includes obstetric outcomes beginning in 2007. For this analysis we restrict to hospitals with greater than 2% obstetric share of admissions in 2002. We obtain the placebo estimates using the "sdid" command with the placebo inference option and 200 replications. The red vertical lines indicate the estimated effect for the privatized hospitals in these states. Section 5.2.2 provides more details.

(a) Total FTEs (b) Physician FTEs

(c) Nurse FTEs (d) Other FTEs

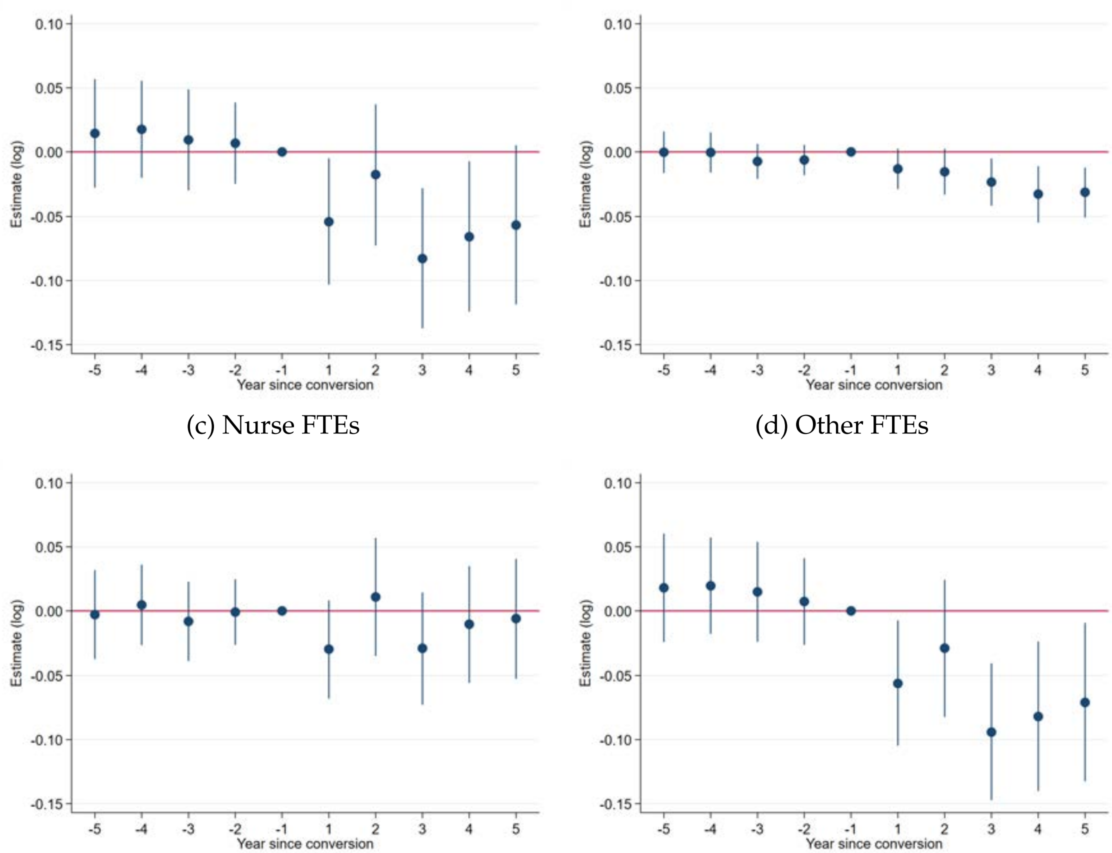


Figure A.8: Effects on staffing (log FTE per 100 admissions)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period. Outcomes are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other (all remaining) FTEs in Panels (a), (b), (c), and (d), respectively. All outcomes are expressed per 100 contemporaneous adjusted admissions and presented in logs. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Year zero is the year of privatization and is excluded for the treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure 6 presents the corresponding plots obtained when we use FTE per 100 adjusted admissions in levels.

(a) Total FTEs (b) Physician FTEs

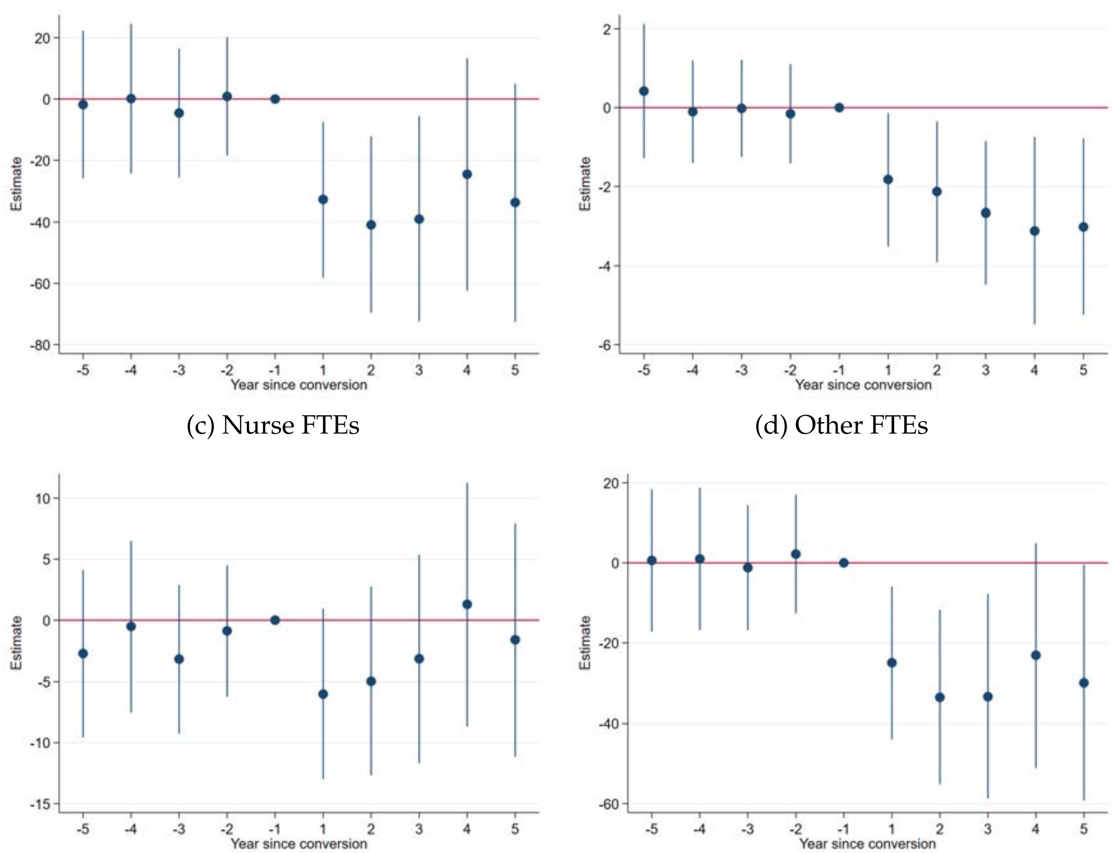
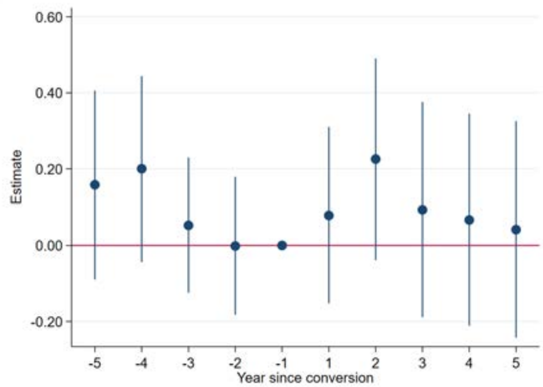


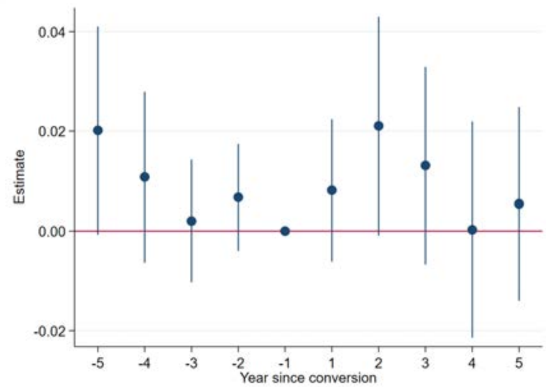
Figure A.9: Effects on staffing (FTE per 100 beds)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period. Outcomes are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other (all remaining) FTEs in Panels (a), (b), (c), and (d), respectively. All outcomes are normalized by the contemporaneous number of hospital beds and presented per 100 beds. Year zero is the year of privatization and is excluded for the treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure 6 presents the corresponding plots obtained when we normalize staff FTE by 100 adjusted admissions instead.

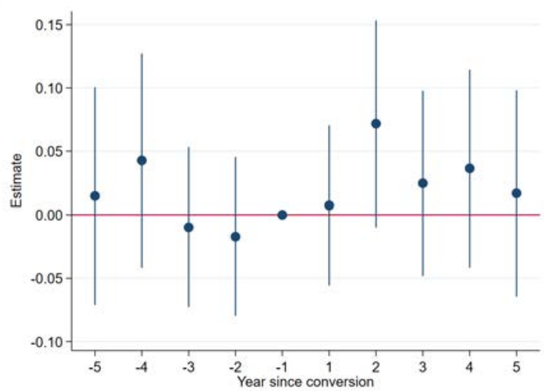
(a) Total FTEs



(b) Physician FTEs



(c) Nurse FTEs



(d) Other FTEs

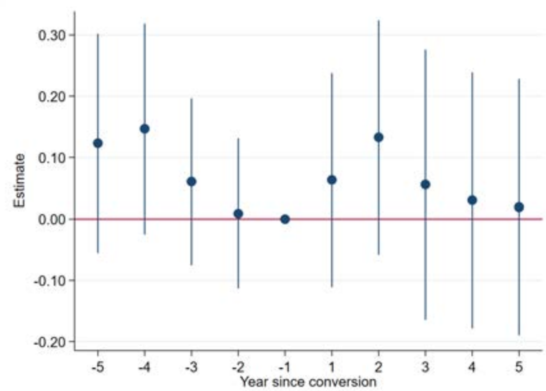


Figure A.10: Effects on market staff (per 100 adjusted admissions)

Note: The figure presents event study plots obtained by estimating the market-level equivalent of Equation 3 on market-year level data. We define hospital markets using Health Service Areas (HSAs), as described in Section 6. The outcomes are as indicated in the figure and are normalized by contemporaneous, adjusted admissions. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. We then multiply outcomes by 100 for ease of exposition. Year zero is the first year a privatization occurs in a given market and is excluded for treated markets since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by HSA.

Table A.1: Public hospital share of beds and Medicaid expansion status

State	Share	# Hospitals	Exp.	State	Share	# Hospitals	Exp.
Wyoming	70.8	32	N	Nevada	14.1	58	Y
Alabama	44.4	116	N	Kentucky	13.7	121	Y
Mississippi	40.7	112	N	Nebraska	13.5	99	N
Kansas	36.8	152	N	New Jersey	12.9	99	Y
South Carolina	32.9	88	N	Georgia	11.7	172	N
North Carolina	31.8	135	N	Ohio	11.3	224	Y
Iowa	29.8	123	Y	Arkansas	10.4	102	Y
Washington	27.0	107	Y	Rhode Island	10.3	15	Y
Louisiana	26.1	200	Y	Montana	10.1	66	Y
Idaho	25.2	52	N	Connecticut	9.9	42	Y
New York	23.6	210	Y	West Virginia	9.3	61	Y
Colorado	23.5	106	Y	Maryland	8.5	62	Y
California	22.9	419	Y	Massachusetts	8.2	102	Y
New Mexico	22.2	55	Y	Illinois	8.0	208	Y
Hawaii	22.1	28	Y	District Of Columbia	7.4	14	Y
Virginia	20.1	123	Y	Delaware	6.3	13	Y
Oregon	19.8	65	Y	Wisconsin	6.3	149	N
Oklahoma	19.4	146	N	Arizona	6.2	110	Y
Tennessee	19.0	132	N	Michigan	6.2	165	Y
Utah	18.6	59	N	New Hampshire	5.5	31	Y
Missouri	18.2	143	N	Maine	5.4	39	Y
Indiana	17.5	161	Y	South Dakota	4.4	64	N
Florida	16.8	253	N	Pennsylvania	3.8	235	Y
Texas	15.8	588	N	North Dakota	2.6	50	Y
Alaska	14.6	26	Y	Vermont	1.7	17	Y
Minnesota	14.4	141	Y				

Notes: The table presents public (nonfederal) shares of hospital beds for all 50 states and DC using data from the American Hospital Association survey of 2019. All hospitals, including non-general-acute-care hospitals, were included in share calculations. The states are listed in decreasing order of public shares. The total number of hospitals for each state is given in the third column. The last column indicates whether or not a state had expanded Medicaid coverage under the Affordable Care Act as of 2019, the last year in our sample.

Table A.2: Characteristics of privatizations

	(1) Non-profit	(2) For-profit	(3) Total
A. Less control	119	25	144
- Contract Management	70	10	80
- Miscellaneous	49	15	64
B. More control	66	48	114
- Sale	36	33	69
- Lease/Joint venture	30	15	45
Total	185	73	258

Notes: This table presents characteristics of the types of privatization deals in our sample. These privatizations occur between 2000 and 2018. Columns 1 and 2 present the number of hospitals that converted to private nonprofit and for-profit, respectively. Panel A lists the modes that allow the private firm to have less control over hospital operations. In contract management, the private firm operates the hospital under a short-term contract. "Miscellaneous" includes cases where a new private firm was incorporated–subject to oversight by the previous government owners–specifically to operate the hospital and cases where the modality could not be identified. Panel B lists the modes of transfer that allowed the private firm more control over hospital operations. These include sale, lease, and joint ventures. Appendix B.1 describes these categories in more detail with examples.

Table A.3: Descriptive statistics, state sample

	(1) Privatized	(2) Not privatized	(3) All
% Public	100.0	20.6	24.8
% For-profit	0.0	21.4	20.3
% Nonprofit	0.0	58.0	55.0
Admissions	6,182	10,758	10,517
	(6,291)	(10,229)	(10,108)
Beds	151	227	223
	(108)	(215)	(211)
% Medicaid	18.3	18.0	18.0
	(10.6)	(12.7)	(12.6)
% Medicare	45.5	43.4	43.5
	(13.4)	(14.0)	(13.9)
% Private	27.8	30.7	30.5
	(13.7)	(13.3)	(13.3)
% Uninsured	4.5	5.2	5.1
	(6.4)	(8.4)	(8.3)
% Miscellaneous	4.0	2.8	2.8
	(7.2)	(3.4)	(3.7)
Obstetric adm.	921	1,326	1,308
	(816)	(1,559)	(1,536)
# Hospitals	27	486	513

Notes: The table presents descriptive statistics on hospitals in the five states (CA, FL, IN, MN, and WA), which comprise the analysis sample represented in Table 4, Panels B and C. We use values from 2003, the first year of data in this sample. Column 1 describes government hospitals that privatized in or after 2008. These comprise the treated units. Column 2 describes the comparison group: government and private hospitals that did not experience ownership changes during the sample period. Column 3 presents the values for the full sample. "Miscellaneous" admissions refer to hospital admissions not classified as one of the other payer categories (e.g., workers' compensation). Obstetric admissions information is not available in Minnesota and hence is taken from the remaining four states. Standard deviations are shown in parentheses.

Table A.4: Descriptive statistics, Medicare sample

	(1) Privatized	(2) Not privatized	(3) All
A: Hospital attributes			
Admissions	3,747	4,134	4,053
	(5,174)	(7,268)	(6,880)
Beds	103	116	113
	(111)	(167)	(157)
%Medicaid	16.2	15.7	15.8
	(11)	(11)	(11)
%Medicare	49.5	49.7	49.6
	(14)	(16)	(16)
%Other	34.3	34.6	34.6
	(12)	(13)	(12)
Medicare FFS	1,136	990	1,020
	(1,504)	(1,482)	(1,487)
B: Patient outcomes			
Predicted mortality	0.113	0.112	0.112
	(0.01)	(0.02)	(0.02)
Pr(prior stay)	0.132	0.129	0.13
	(0.04)	(0.04)	(0.04)
Length of stay	5.374	5.291	5.308
	(1.11)	(1.32)	(1.28)
Pr(stay$\leq$ 2 days)	0.152	0.172	0.168
	(0.06)	(0.08)	(0.07)
Medicare payment ($)	6,048	6,857	6,688
	(2,268)	(4,100)	(3,804)
Charges ($)	12,059	12,640	12,519
	(5,863)	(9,628)	(8,973)
Mortality rate (30-day)	0.116	0.114	0.115
	(0.03)	(0.04)	(0.04)
# hospitals	203	769	972

Notes: The table presents descriptive statistics on hospitals and patients in the Medicare fee-for-service (FFS) claims sample. We present values from 2000, the first year of data in this sample. Column 1 describes government hospitals that privatized in or after 2005. These comprise the treated units. Column 2 describes the comparison group: government hospitals that did not experience ownership changes during the sample period. Column 3 presents the corresponding values for both sets of hospitals. Panel A describes hospital bed size, patient volume, and payer mix. Medicare FFS presents the number of Medicare FFS patients older than 65 in the claims sample. Other values in Panel A are obtained from AHA and are comparable to the corresponding values in Table 2. Panel B presents baseline mean values of the patient outcomes examined in Table 5 and Table 10 Panel A. Standard deviations are shown in parentheses.

Table A.5: Effects on log finances (per bed)

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
A: No controls				
DD	0.085	-0.007	-0.062	0.064
	(0.031)	(0.029)	(0.028)	(0.046)
Obs	16,673	16,673	16,673	16,673
B: Market controls				
DD	0.117	0.019	-0.036	0.086
	(0.032)	(0.028)	(0.028)	(0.047)
Obs	16,662	16,662	16,662	16,662
Mean outcome (t-1)	644,202	661,999	353,417	308,582

Notes: The table presents estimated effects on hospital finances obtained by estimating Equation 2 on hospital-year level data. The comparison group consists of hospitals that remain under government control throughout our sample period. The outcomes in columns 1 and 2 are total revenue (from Medicare cost reports) and total expenses (from AHA), respectively. Total expenses comprise personnel expenses and remaining expenses, shown in columns 3 and 4, respectively. All four variables are normalized by contemporaneous number of hospital beds and presented in logs. Table 3 presents the corresponding results when we normalize values by adjusted admissions instead. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. The mean values present the outcomes in 2019 dollars per bed.

Table A.6: Effects on ED and other outpatient (log) volume

	(1)	(2)	(3)	(4)
	Hospital		Market	
	ED	Other Outpt	ED	Other Outpt
A: No controls				
DD	-0.048	-0.069	0.023	-0.006
	(0.032)	(0.063)	(0.015)	(0.029)
Obs	20,998	20,998	19,985	19,985
B: Market controls				
DD	-0.040	-0.040	0.019	0.005
	(0.034)	(0.064)	(0.015)	(0.029)
Obs	19,385	19,385	18,522	18,522
Mean outcome (t-1)	15,424	53,766	151,923	504,456

Notes: The table presents estimated effects on the log emergency department (ED) and non-ED, or other outpatient volume at the privatized hospital (cols. 1 and 2) and the corresponding market-level effects (Cols. 3 and 4). Panels A and B present the coefficients obtained by estimating Equation 2 without and with time-varying covariates, respectively. The mean values pertain to outcomes (in levels) at treated hospitals or markets in the year before privatization. Standard errors are clustered by hospital or market, depending on the level of treatment.

Table A.7: Effects of changes in payer mix and list prices

A: Payer mix	(1) Mean amount ($/stay)	(2) Share of hospital stays Privatized (AHA)	(3) Privatized (states)	(4) Effect on volume %	(5) Predicted share of stays	(6) Predicted reimb. ($/stay)
1. Medicare	13,419	0.45	0.45	-0.048	0.47	13,419
2. Medicaid	9,269	0.20	0.19	-0.138	0.19	9,269
3. Other	13,385	0.35	0.37	-0.129	0.33	13,888
Private insurance	14,919	*	0.28	-0.045	0.26	
Uninsured	5,928	*	0.06	-0.374	0.04	
Miscellaneous	15,153	*	0.02	0.319	0.03	
Overall	12,558	1.00	1.00		1.00	12,767
% Increase in reimb.						1.7%

B: List prices	Effect on list prices		Effect on volume and list prices	
%List price contracts	20% ($/stay)	50% ($/stay)	20% ($/stay)	50% ($/stay)
1. Medicare	13,419	13,419	13,419	13,419
2. Medicaid	9,269	9,269	9,269	9,269
3. Other	13,570	13,847	14,079	14,367
Private insurance	15,125	15,434	15,125	15,434
Uninsured	6,010	6,132	6,010	6,132
Miscellaneous	15,362	15,676	15,362	15,676
Overall	12,622	12,718	12,831	12,927
% Increase in reimb.	0.5%	1.3%	2.2%	2.9%

Notes: The table presents results on the effects of changes in payer mix, list prices, or both on mean reimbursement. Panel A presents walks the reader through the calculation of the predicted effect of changes in payer mix on mean reimbursement per patient. Column 1 presents mean reimbursement rates for hospital inpatient stays averaging across Medical Expenditure Panel Survey (MEPS) waves of 2000, 2005, 2010, 2015, and 2019. The mean values are expressed in 2019 dollars. Column 2 presents the shares of patients for Medicare, Medicaid, and "Other" for privatized hospitals calculated using AHA data in the year before treatment and reported in Table 4 Panel A. The AHA does not report volume separately for the component groups within Other. Column 3 is equivalent to column 2 but uses data from 5 states (CA, FL, IN, MN, and WA). These data, reported in Panel C of Table 4, present patient shares granularly for the different component groups within Other. "Miscellaneous" is a residual category containing patients who are not Medicare, Medicaid, Private, or uninsured. This mainly includes patients covered by workers compensation, Veterans Affairs, TRICARE (US military insurance), and other government programs. The mean reimbursement for Other is calculated as a weighted average of private, uninsured, and miscellaneous, with the patient shares from the states data (using volumes reported in Table 4B) as weights. Column 4 presents the estimated percent effects on inpatient volume by payer. The values for Medicare, Medicaid, and Other reflect the exponentiated coefficients reported in Table 4 Panel A. The values for private, uninsured, and miscellaneous reflect the exponentiated coefficients reported in Panel B of the same table. Column 5 presents the predicted share of stays by payer that result when we apply the estimates in col. 4 to the corresponding baseline shares in cols. 2 (Medicare, Medicaid, and Other) or col. 3 (private, uninsured, and miscellaneous). Results from the states sample are used to quantify the shift in composition within Other, while results from the AHA sample are used to quantify the shift between Medicare, Medicaid, and Other. Column 6 presents the predicted reimbursement for Other and overall after incorporating the estimated changes in payer shares. Panel B walks the reader through the calculation of the predicted effect of changes in list price alone (cols. 1-2) and the combination of changes in payer mix and list price (cols. 3-4) on mean reimbursement per patient. We apply the estimated increase in list price, 6.9%, to the mean reimbursement of private, uninsured, and miscellaneous payers, scaled by the proportion of contracts that are based on list price. Following prior studies, we assume this proportion to range between 20% and 50%. Mean reimbursement for other is the weighted average calculated using the shares in Panel A col. 3 as weights. Columns 4 and 5 incorporate the changes in payer mix presented in Panel A col. 5. Hence, the same increases in list prices lead to a greater mean reimbursement for Other patients. The last row in both panels presents the % increase in mean reimbursement relative to the baseline value, $12,558.

Table A.8: Effects on log staff (per 100 admissions)

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-0.064	-0.019	-0.012	-0.077	-0.015
	(0.022)	(0.007)	(0.017)	(0.022)	(0.009)
Obs	20998	20998	20998	20998	8631
B: Market controls					
DD	-0.047	-0.017	-0.001	-0.059	-0.017
	(0.022)	(0.007)	(0.017)	(0.022)	(0.010)
Obs	19385	19385	19385	19385	8627
Mean outcome (t-1)	7.4	0.1	1.9	5.3	0.2

Note: The table presents effects on staff employment at the privatized hospitals, obtained by estimating Equation 2 on hospital-year level data. All outcomes are expressed per 100 contemporaneous adjusted admissions and presented in logs. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Column 1 presents results for total FTE, which comprises physicians, nurses, and others, presented in columns 2, 3, and 4, respectively. Column 5 presents results for contract FTEs, which come from Medicare cost reports and include management and patient care staff. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a specification including time-varying hospital and county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to the outcomes (in levels) at privatized hospitals in the year before privatization. Standard errors are clustered by hospital. Table 6 presents the corresponding results obtained when we use staff FTE in levels instead.

Table A.9: Effects on staff (per 100 beds)

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-33.4	-2.5	-1.7	-29.5	0.13
	(12.7)	(0.8)	(3.2)	(9.5)	(1.35)
Obs	20,998				8,631
B: Market controls					
DD	-25.0	-2.7	-0.2	-22.5	-0.002
	(12.9)	(0.8)	(3.3)	(9.6)	(1.360)
Obs	19,385				8,627
Mean outcome (t-1)	511.2	10.2	138.6	362.1	13.60

Notes: The table presents the effects on personnel expenses and full-time equivalent (FTE) staff employed at privatized hospitals, obtained by estimating Equation 2 on hospital-year-level data. Column 1 presents results for total FTEs, which comprises physician, nurse, and other staff, presented in columns 2, 3, and 4, respectively. We normalize staff FTEs in each column by 100 contemporaneous hospital beds, which is approximately the size of a public hospital in our sample. Column 5 presents the results for contract FTEs, which come from Medicare cost reports and include management and patient care staff. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports the coefficients of a two-way fixed effects specification that includes time-varying hospital and county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to outcomes at privatized hospitals in the year before privatization. Standard errors are clustered by hospital and are presented in parentheses. Table 6 presents the corresponding results obtained when we normalize staff FTE by 100 adjusted admissions instead.

Table A.10: Balance in the full and matched AHA samples

	(1) All treated	(2) All comparison	(3) Std. difference	(4) Matched comparison	(5) Std. difference
# hospitals	258	802		258	
Beds	87	119	-0.20	80	0.06
Total admissions	3,014	4,324	-0.18	2,598	0.09
Medicaid admissions	617	1,140	-0.24	539	0.07
Expenses (mn)	61	107	-0.24	54	0.07
HSA population	570,563	698,706	-0.09	562,126	0.01
% in poverty (county)	16.8	15.7	0.19	16.6	0.03
% Unemployment (county)	7.0	6.3	0.23	7.1	-0.06

Notes: The table presents means for treated hospitals (col. 1, 258 in number), all comparison hospitals, (col. 2, 802), and matched comparison hospitals (col. 4, 258). We use 1:1 matching without replacement and describe the matching procedure in more detail in Section C.1. We present mean values for the variables used in propensity score matching. Col. 3 presents the standardized difference in means between the full sample of treated and comparison hospitals. We compute the standardized difference as the difference in means divided by the standard deviation of the pooled sample. Col. 5 presents the standardized difference in means in the matched sample. All means are computed in the year before privatization. In col. 2 we randomly assign privatization years to control hospitals, drawn from the empirical distribution of privatization years among the treated hospitals. In col. 4 each matched control hospital is assigned the same privatization year as its matched treated hospital counterpart.

Table A.11: Robustness checks (Medicare patients)

	(1) Pred. mortality	(2) Pr(prior stay)	(3) Log LOS	(4) Pr(stay<2 days)	(5) Log(payment)	(6) Log (charges)	(7) Mortality
Baseline	-0.0016	-0.0067	-0.0174	0.0075	0.0020	0.0641	0.0033
	(0.0007)	(0.0021)	(0.0067)	(0.0029)	(0.0134)	(0.0190)	(0.0012)
I: Specification checks							
A: State-year f.e.	-0.0019	-0.0054	-0.0207	0.0072	0.0037	0.0514	0.0032
	(0.0007)	(0.0020)	(0.0064)	(0.0029)	(0.0122)	(0.0194)	(0.0014)
B: Incl. pre-trend	-0.0003	-0.0071	-0.0199	0.0056	-0.0207	0.0645	0.0037
	(0.0005)	(0.0022)	(0.0073)	(0.0032)	(0.0128)	(0.0189)	(0.0012)
II: Alternate samples, Treated group							
A: Balanced panel	-0.0015	-0.0082	-0.0125	0.0062	0.0022	0.0676	0.0029
	(0.0008)	(0.0024)	(0.0075)	(0.0033)	(0.0158)	(0.0219)	(0.0014)
B: No trimming	-0.0018	-0.0077	-0.0138	0.0079	0.0052	0.0826	0.0019
	(0.0008)	(0.0022)	(0.0079)	(0.0035)	(0.0159)	(0.0227)	(0.0013)
III: Alternate samples, Comparison group							
A: Matched sample	-0.0004	-0.0049	-0.0296	0.0098	0.0042	0.0274	0.0015
	(0.0008)	(0.0029)	(0.0088)	(0.0033)	(0.0159)	(0.0229)	(0.0018)
B: Include switchers	-0.0015	-0.0065	-0.0165	0.0073	0.0034	0.0715	0.0034
	(0.0007)	(0.0021)	(0.0066)	(0.0029)	(0.0132)	(0.0187)	(0.0012)

Notes: The table shows the results of robustness checks for the effects on Medicare fee-for-service patient complexity (cols. 1-2), treatment intensity (cols. 3-4), billing (cols. 5-6), and 30-day mortality (col. 7). These results are obtained by estimating patient-level models and correspond to the results in tables 5 and 10 Panel A (mortality). The outcomes are described in more detail in the notes to those tables. For each outcome, we first present the baseline estimates. Panel I presents results from two specification checks – including state-by-year fixed effects (A) and including a hospital-specific linear trend estimated on 2000–2003 data (B). We do not estimate weighted regressions, since patient-level models implicitly account for hospital size. Panel II presents results of checks using two different samples, one in which all treated hospitals are observed for 5 years after privatization (A), and the other in which we retain all observations for treated hospitals, including the year of privatization (B). Panel III tests the robustness to varying the comparison group. Row A presents results using a matched subsample identified using propensity score matching, and the sample in row B includes hospitals that switch between public and private status. Standard errors are clustered by hospital.

Table A.12: Effects on aggregate staff

	(1) Total	(2) Physician	(3) Nurse	(4) Other
A: No controls				
DD	0.02	0.00	0.03	-0.00
	(0.09)	(0.01)	(0.03)	(0.07)
Obs	19,985	19,985	19,985	19,985
B: Market controls				
DD	0.09	0.00	0.03	0.05
	(0.09)	(0.01)	(0.03)	(0.07)
Obs	18,522	18,522	18,522	18,522
Mean outcome (t-1)	6.61	0.13	1.92	4.56

Notes: The table presents effects on full-time equivalent (FTE) employed staff at the market level obtained by estimating the market-level equivalent of Equation 2 on market-year level data. We define markets using Health Service Areas (HSAs), as described in Section 6. Column 1 presents results for total FTEs, which comprises physicians, nurses, and all remaining "Other" staff, presented in columns 2, 3, and 4, respectively. We normalize staff FTEs in each column by 100 contemporaneous adjusted admissions. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Panel A reports coefficients from a two-way fixed effects specification with no covariates other than market and year fixed effects. Panel B reports coefficients from a two-way fixed effects specification that include time-varying, HSA-level controls: population, unemployment, uninsurance, and poverty rates. Other controls include the share of hospitals in the HSA that are 340B providers and a time-varying indicator for being located in a Medicaid expansion state. Panel B has fewer observations, since the covariates are not available for 1995 and 1996. The mean values pertain to the outcomes in the treated markets in the year prior before privatization. Standard errors are clustered by HSA and are presented in parentheses.

Table A.13: Robustness checks (market-level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total	Medicaid	Medicare	Other	Total	Physician	Nurse	Other	Mortality
Baseline	-0.004	-0.038	0.008	0.009	0.02	0.00	0.03	-0.00	5.2
	(0.014)	(0.024)	(0.016)	(0.022)	(0.09)	(0.01)	(0.03)	(0.07)	(6.6)
I: Specification checks									
A. Weighted by beds	0.025	-0.009	0.030	0.048	-0.08	-0.01	-0.01	-0.05	10.7
	(0.010)	(0.017)	(0.012)	(0.014)	(0.07)	(0.01)	(0.03)	(0.05)	(7.1)
B. State-year FEs	-0.012	-0.033	0.001	-0.015	0.08	-0.00	0.05	0.03	2.4
	(0.015)	(0.025)	(0.016)	(0.023)	(0.11)	(0.01)	(0.03)	(0.08)	(6.5)
C. Incl. pre-trend	-0.023	-0.071	-0.010	-0.004	0.01	0.00	0.04	-0.02	4.3
	(0.014)	(0.024)	(0.015)	(0.023)	(0.10)	(0.01)	(0.03)	(0.07)	(7.3)
II: Alternate estimators									
A. CS estimator	-0.000	-0.033	0.006	0.017	0.09	0.01	0.02	0.05	-6.2
	(0.012)	(0.025)	(0.016)	(0.022)	(0.12)	(0.01)	(0.03)	(0.09)	(9.6)
B. DCDH estimator	-0.006	-0.037	-0.004	0.014	0.13	0.01	0.03	0.08	-6.2
	(0.011)	(0.021)	(0.014)	(0.019)	(0.12)	(0.01)	(0.03)	(0.09)	(8.3)
C. Synthetic DiD	0.007	-0.028	0.019	0.042	-0.11	0.00	-0.01	-0.09	6.3
	(0.017)	(0.028)	(0.021)	(0.022)	(0.14)	(0.01)	(0.04)	(0.11)	(8.5)
III: Alternate samples - treated group									
A. Balanced panel	0.005	-0.037	0.017	0.022	0.07	0.00	0.04	0.03	4.5
	(0.015)	(0.026)	(0.017)	(0.021)	(0.10)	(0.01)	(0.03)	(0.07)	(7.3)
B. All treated obs	0.021	-0.031	0.031	0.044	-0.10	-0.00	-0.00	-0.09	5.4
	(0.022)	(0.031)	(0.023)	(0.025)	(0.11)	(0.01)	(0.03)	(0.09)	(7.1)
IV: Alternate samples - comparison group									
A. Matched sample	-0.016	-0.026	-0.006	-0.013	0.04	0.00	0.03	0.00	4.0
	(0.017)	(0.028)	(0.019)	(0.026)	(0.11)	(0.01)	(0.03)	(0.09)	(8.1)

Notes: The table presents robustness checks for the effects on the volume of hospital patients in the market and the full-time equivalent (FTE) staff employed per 100 adjusted admissions, presented in Tables 9 and A.12, respectively. The table also includes robustness checks for the effects on all-cause mortality (per 100,000) for ages 55-64, presented in Table 10, Panel B, column 1. It follows the same format and presents the same checks as in Table 7, except that there is no equivalent for the check that includes switcher comparison hospitals, since all models include all hospitals present in the market. In row IA, we use static hospital beds to weight markets. The model in row IC includes market-specific trends that are first estimated using data from 1995–1999. This analysis uses 2000-–2019 data while dropping treated markets in 2000 and 2001. Row IIIA drops any treated market that we do not observe five years before and after the transition. Row IIIB uses all treated observations, including those from the year of privatization and those beyond the five-year window around privatization (if available). Row IVA presents results estimated using a matched subsample identified using propensity score matching. Standard errors are clustered by market and are presented in parentheses.

Table A.14: Additional results on mortality for Medicare patients

A: By duration	(1) 30-day	(2) 60-day	(3) 90-day	(4) 180-day	(5) 365-day
A1: Patient controls					
DD	0.0033	0.0045	0.0054	0.0063	0.0072
	(0.0012)	(0.0015)	(0.0016)	(0.0018)	(0.0021)
A2: Patient and mkt. controls					
DD	0.0038	0.0052	0.0063	0.0074	0.0088
	(0.0013)	(0.0015)	(0.0017)	(0.0020)	(0.0022)
Mean outcome (t-1)	0.119	0.157	0.184	0.242	0.323
Observations	13,097,798	13,097,798	13,097,798	13,097,798	13,097,798

B: By Diagnostic category	(1) Circulatory	(2) Respiratory	(3) Digestive	(4) Musculoskeletal	(5) Kidney	(6) Miscellaneous
B1: Patient controls						
DD	0.0027	0.0017	0.0048	0.0020	0.0053	0.0044
	(0.0019)	(0.0024)	(0.0021)	(0.0013)	(0.0028)	(0.0017)
B2: Patient and mkt. controls						
DD	0.0033	0.0034	0.0059	0.0027	0.0060	0.0049
	(0.0021)	(0.0026)	(0.0021)	(0.0013)	(0.0029)	(0.0018)
Mean outcome (t-1)	0.097	0.16	0.086	0.051	0.119	0.147
Observations	3,079,206	2,226,683	1,419,222	1,412,489	936,939	4,023,246

Notes: The table presents additional results on mortality for Medicare FFS patients using the Medicare claims data. Panel A presents the estimated average effect on mortality across all 65+ patients at different durations from 30 days through 365 days following discharge from the index hospital stay. Panel B presents the estimated effect on 30-day mortality for 65+ patients in the top 5 major diagnostic categories (MDCs) by volume in columns 1–5 and the effect for all remaining patients in column 6. The top 5 MDCs by volume in our sample are: circulatory system (MDC5), respiratory system (MDC4), digestive system (MDC6), musculoskeletal system and connective tissue (MDC8), kidney and urinary tract (MDC11). These 5 categories together contribute nearly 70% of total patient volume. A small fraction of patients could not be assigned to an MDC. All results were obtained by estimating Equation 2 on patient-level data. The model represented in row 1 of each panel includes patient covariates to control for observed differences across patients, as described in Section 4. The model in row 2 of each panel also includes time-varying market covariates. Standard errors are clustered by hospital.

Table A.15: Additional results on market-level mortality

A: All causes, by age group	(1) All ages	(2) <15	(3) 15-34	(4) 35-54	(5) 55-64	(6) ≥65
A1: No controls						
DD	8.4	-2.3	-2.5	-2.3	17.3	17.6
	(5.6)	(2.3)	(2.7)	(4.8)	(11.7)	(25.4)
Obs	19,985					
A2: Market controls						
DD	8.2	-2.2	-2.2	-0.3	18.8	22.9
	(5.6)	(2.3)	(2.8)	(4.8)	(11.8)	(25.2)
Obs	18,522					
Mean outcome (t-1)	1053.6	72.3	122.9	386.2	1032.6	4937.1

B: Ages 55–64, by cause of death	(1) Cancer	(2) Cardiovascular	(3) Respiratory	(4) Liver and kidney	(5) Diabetes	(6) Miscellaneous
B1: No controls						
DD	8.4	-6.3	4.5	4.4	4.8	1.4
	(5.6)	(6.0)	(2.5)	(2.3)	(2.5)	(5.4)
Obs	19,985					
B2: Market controls						
DD	9.7	-7.1	5.1	4.3	4.9	2.0
	(5.7)	(5.9)	(2.5)	(2.3)	(2.4)	(5.5)
Obs	18,522					
Mean outcome (t-1)	343.6	307.7	68.6	55.1	37.2	220.5

Notes: This table presents additional results on market-level mortality (per 100,000). We define markets using Health Service Areas (HSAs), as described in Section 6. Mortality estimates are derived from Vital Statistics data from the NCHS. In all analyses, treated units are counties that experienced a privatization during the sample period, and control units are HSAs that never experienced a privatization. In Panel A, we show effects for all-cause mortality, split by mutually exclusive and exhaustive age groups. In Panel B, we show effects for ages 55-64 mortality, split by cause of death. To obtain these groups, we started with the ICD 39 cause recode groups provided in the data, which groups together similar ICD codes pertaining to cause of death. We then further aggregated these groups for ease of exposition. Mean values are computed for counties in the year before treatment.

Table A.16: Cost-benefit calculations

Item	Value	Notes
A. Baseline values:		
Total patients	7025	Table 4 panel A
Volume effect	-6%	Table 4 panel A
Revenue/patient ($)	8109	Table 3
Cost/patient ($)	8444	Table 3
Deficit/patient ($)	-335	Table 3
Deals with less control	56%	Table A2
For-profit partner	28%	Table A2
B. Savings, per privatization per year		
B1. Reduction in deficit	2,353,375	7025 patients x $335/patient
B2. Increase in surplus		
Increase in revenue/patient	462	$8109 x 5.7% increase
Increase in revenue	3,052,224	7025x(1-6%) patients x 462 per patient
Reduction in cost/patient	279	$8444 x 3.3% decrease
Reduction in cost	1,840,078	7025x(1-6%) patients x 279 per patient
Gross increase in surplus	4,892,302	Additional revenue + cost savings
Net increase in surplus	2,680,130	7025x(1-6%) patients x (741-335) per patient
B3. Additional tax funds		
Nonfederal tax rate	2.10%	Rosenbaum et al. (2015) Ex. 4
Mean hospital revenue	56,600,005	7025x(1-6%) patients x ($8109+$462)
Incremental tax (FP only)	1,188,600	2.1% of revenue
Share of FP partner in deals	28%	Table A2
Expected tax	336,309	Incremental tax x FP share
Baseline net savings	2,689,684	B1 (reduction in deficit) + B3 (tax revenue)
Surplus in deals w less control	1,495,886	56% of total net surplus
Upper bound estimate	4,185,571	Baseline + surplus revenue
C. Deaths, per privatization per year		
C1. Hospital mortality		
Medicare FFS patients	1,136	Table A5; 65+ only
All Medicare patients	1,873.5	Table A5
Volume reduction	4.9%	Table 4 Panel A
Mortality effect	0.33%	Table 10 Panel A1 col. 1
Incremental deaths	3.57	1136x(1-4.9%) patients x 0.33%
Standard LYL	8.90	Life exp. using CDC life table 2010
Realistic LYL	5.34	8.9x(1-40%) Deryugina et al (2019)
Realistic aggregate LYL	19.04	3.57 x 5.34
Extrapolated to all Medicare:		
Incremental deaths	5.88	1873.5x(1-4.9%) patients x 0.33%
LYL	31.40	5.88 x 5.34
C2. Market-level mortality		
Mean population	42160	Mean 55-64 popn in treated markets
Effect per market	5.2	Table 10 Panel B1 col. 1
Effect per privatization	4	linearly scale 5.2 by 1.3 deals/mkt
Incremental deaths	1.69	4 x 42160 / 100,000
Standard LYL	23.10	Life exp. using CDC life table 2010
Realistic LYL	13.86	23.1x(1-40%) Deryugina et al (2019)
Realistic aggregate LYL	23.37	1.69 x 13.86
D. Savings per death or per LYL		
D1. Baseline estimate:		
Savings per death ($mn)	0.75	$2.69mn /3.57 deaths
Savings per LYL ($)	141,282	$2.69mn /19.04 LYL
D2. Upper bound:		
Savings per death ($mn)	1.17	$4.19mn /3.57 deaths
Savings per LYL ($)	219,857	$4.19mn /19.04 LYL
D3. Lower bound:		
Savings per death ($mn)	0.51	$2.69mn /(3.57+1.69) deaths
Savings per LYL ($)	63,419	$2.69mn /(19.04+23.37) LYL

Notes: The table explains the calculations used in the cost-benefit analysis discussed in Section 8. Panel A presents baseline values of patient volume, revenue, costs, etc. for the privatized hospitals before privatization. Panel B describes how we estimate the savings, gross and net surplus, and tax revenue generated from the average privatization. Panel C presents the additional deaths and life-years lost (LYL) due to the average privatization, both among Medicare patients at the hospital and among 55–64 year old individuals in the community. We use the CDC life tables for 2010 to calculate the average years of life lost. For Medicare patients, we integrate life expectancy at each age using the observed distribution of age at death in our sample. For market level deaths we use life expectancy at age 60. To account for potential heightened mortality risk among decedents, we scale these estimates down by 40% following Deryugina et al. (2019). Panel D presents three estimates of the net savings per death and per LYL. Column 3 provides the rationale or source of the value used in the calculations.

B Data Appendix

B.1 Privatization taxonomy

We first identify cases of public hospitals that were converted to private control during our study period of 2000–18. There is no official source of such events, and thus we utilize the AHA annual survey files over this period. See Section B.2 for more details on how we construct our initial list of privatizations. We manually verify each conversion by combing through hospital websites, news articles, and third-party sites such as the American Hospital Directory. Manual validation helps identify nontrivial numbers of false positive conversions. Our final number of conversions is 258.

Through these detailed reviews, we classify privatizations into five groups, described below. We consider the first two as transitions in which the private operator has less control over hospital operations, while the latter three afford greater control. We provide counts for each group in Table A.2. We provide an example for each type to help illustrate the differences between these deals.

- **Contract management:** Occurs when a private (corporation or health system) firm takes over the day-to-day management of a hospital. Government maintains control over the hospital's property, assets, and debts.

  Example: Mercy Hospital Lincoln (Troy, MO) recorded a conversion in the AHA in 2015 from "County" to "other not-for-profit." Manual validation noted that Mercy signed an agreement to lease and manage the facility beginning March 1, 2015.

- **Public hospital incorporating as a private firm:** Occurs when a public health system files for 501c3 nonprofit status ("incorporating").

  Example: Hutchinson Area Health Care (Hutchinson, MN) recorded a conversion in 2008 from "city" to "other not-for-profit." Manual validation noted that in January 2008 Hutchinson Area Health Care became its own private, nonprofit corporation and was no longer a part of the city of Hutchinson.

- **Sale:** Occurs when there is a permanent transfer in the ownership and control of the property, assets, and debts of a hospital, from government to a private corporation or hospital.

  Example: Glenwood Regional Medical Center (West Monroe, LA) recorded a conversion in the AHA in 2006 from "hospital district or authority" to "other not-for-profit." Manual validation noted that IASIS Healthcare LLC announced the signing of a definitive agreement to acquire Glenwood Regional Medical Center from the Hospital Service District for approximately $82.5 million.

- **Long-term lease:** Occurs when a private (corporation or health system) authority takes control over day-to-day management of a hospital for an extended period of time (more than 15 years). The government entity maintains control over the hospital's property, assets, and debts.

  Example: Mercy McCune-Brooks Hospital (Joplin, MO) recorded a conversion in the AHA in 2012 from "city" to "church operated." Manual validation noted that Mercy's 50-year lease of the city-owned hospital was approved by the Carthage City Council in 2012.

- **Joint venture:** Occurs when one or more private (corporations or health systems) firms agree to enter into a joint venture with the local government authority, which results in a newly formed private firm to take over management of the hospital.

Example: Rice Memorial Hospital (Willmar, MN) recorded a conversion in the AHA in 2018 from "city" to "other not-for-profit." Manual validation noted that Rice Memorial Hospital, ACMC Health and CentraCare Health signed the final agreement to establish Carris Health, a subsidiary of CentraCare Health, which is a not-for-profit health care system. Carris Health committed to make a capital investment of $32 million in Rice Memorial Hospital over the next 10 years. The hospital's assets would continue to be owned by the City.

B.2 American Hospital Association annual surveys

We exclude two types of hospitals from our analysis sample. First, we exclude federal hospitals because they typically cater to a distinct set of patients (such as veterans or Native Americans) rather than the local community at large. The government hospitals in our sample are owned by a state, county, city, or hospital district. Hospital districts are funded by taxpayers to own and operate public hospitals. Second, we exclude specialized hospitals such as psychiatric and rehabilitation facilities. In addition to being highly specialized, these hospitals are often reimbursed differently from community hospitals. Therefore, our final sample contains nonfederal, general acute care (GAC) hospitals. We identify GAC hospitals using the AHA's primary service code of 10, which are "general medical and surgical" hospitals. We include all hospitals whose most common service code is general medical and surgical.

Identifying privatizations — We create an initial list of public to private conversions by starting with conversions implied by changes in the control or system name variables in the AHA data. In the former case, we identify hospitals that in year $t-1$ are listed as public (state, county, city, city-county, or hospital district or authority) and in year t are listed as private (for-profit or nonprofit), for the years 2001–2018. We also require that hospitals be listed as private for at least two "post" years, with the exception of privatizations in 2018. Note that this approach captures potential privatizations that occurred starting in 2001; however, in our manual validation we discovered that some hospitals had an incorrect conversion year and actually privatized in 2000. Using the system name variable, we identify hospitals with system name changes during the years 2000–2018. Specifically, we create a list of public and private health systems based on their names and then identify hospitals under public control and not part of a private system (i.e., part of a public system or not part of a system) for two years, and then subsequently listed as public control and part of a private system for two years. Furthermore, we implement the same sample restrictions made when creating our analytic sample, e.g., we drop privatizations of hospitals not considered "general medical and surgical". In addition, we limit our treated hospitals to those that experience only one conversion over our sample period.

Using the above approach, we identify 358 "naive" privatizations, which we then manually validate. Our validation yields 100 false positives, in which we do not find evidence in the public domain that a privatization occurred at a given hospital. This gives our final set of 258 public-to-private conversions.

Defining the control group of hospitals — We start with American Hospital Association (AHA) survey data for the years 1995 to 2019. In the raw data, there are $\sim$6,200 hospitals per year and $\sim$8,400 unique hospitals over the sample period. We make the following sample restrictions:

- Drop hospitals whose most common AHA service code is not "general medical and surgical" (2,457 hospitals)
- Drop hospitals that on average report fewer than 10 beds (42 hospitals)

- Drop hospitals that are ever classified as federal government by the AHA (293 hospitals). These include military, Veterans Affairs, Indian Health Service, and Department of Justice hospitals

- Drop hospitals that are only classified as public (state and local) in some years of the sample period but not all. This group includes hospitals that are most commonly labeled as private (290 hospitals) and hospitals that are most commonly labeled as public (122 hospitals). This is a conservative restriction to ensure that our comparison group is comprised of non-converting, public hospitals

- Drop hospitals that are within 15 miles of at least one treated hospital (32 hospitals)

The final AHA analysis sample contains 802 comparison hospitals.

Constructing the market-level (HSA) sample — We define markets as Health Service Areas (HSAs) and use of the list of "NCI Modified" HSAs provided by the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (https://seer.cancer.gov/seerstat/variables/countyattribs/hsa.html). HSAs are single counties or collections of counties. Although there are about 950 HSAs in the US, 933 HSAs are represented among hospitals in the base AHA sample (using the county in which a hospital is located to merge HSAs). An additional four HSAs are (implicitly) dropped due to sample restrictions when constructing our hospital-level sample, e.g., keeping only general medical and surgical hospitals. This gives our final market sample of 929 HSAs.

B.3 State administrative data on hospitals and patients

To examine changes in service mix and disaggregate the "Other" payer group in AHA data, we use administrative data from select states. Our goal was to obtain data from large states that also experienced many privatizations. However, among the states that experienced the most privatizations during this period, many do not share data in a usable form (e.g., Georgia and Michigan do not release hospital IDs; Alabama, Oklahoma, and Idaho do not release data at all), price discharge data prohibitively (e.g., Texas), or do not release earlier years (e.g., Arkansas and Mississippi). We were able to obtain suitable data over 2003–2019 from five states: California, Florida, Indiana, Minnesota, and Washington. Of these, MN and IN ranked second and fifth, respectively, by the number of privatizations during our study period. TX is first, GA is third, and LA is fourth. We obtained data from LA but found it ill-suited for this analysis. We have detailed patient-level discharge data for FL, IN, and WA and annual hospital-level reports for CA and MN.

FL and WA share hospital discharge data through the Healthcare Cost and Utilization Project (HCUP) State Inpatient Databases (https://hcup-us.ahrq.gov/sidoverview.jsp). We use HCUP categories to assign hospitalizations based on the primary payer; we defined uninsured hospitalizations as those categorized as self-pay, no charge, or missing. We obtained hospital discharge data for IN from the Indiana Department of Health, Office of Data & Analytics. In a similar fashion to the HCUP data, we assign hospitalizations using the primary payer definitions in the data; uninsured is defined as either self-pay or other/unknown payer. Data for CA and MN come from detailed state reports on the number of discharges (by payer and type of hospitalization) at the hospital-year level. CA data comes from the Department of Health Care Access and Information (HCAI)'s hospital annual financial data reports, which are publicly available (https://data.chhs.ca.gov/dataset/hospital-annual-financial-data-selected-data-pivot-tables). Medicare, Medicaid, and private discharges are defined as the sum of traditional and managed care discharges, which are reported separately in the data. Uninsured discharges are defined as the sum of county and other indigent discharges. MN data comes from the Health Economics program of the Minnesota Department of Health. The data is not publicly available but is free and available on request. We define uninsured admissions as the self-pay payer category in the data.

Synthetic difference-in-differences — The comparison group of hospitals in analyses using the state data is also limited to these 5 states. This creates an issue with non-parallel trends in some of the outcomes of interest when we estimate the baseline DD model. To overcome this limitation, we apply the synthetic difference-in-differences (SDiD) estimator developed by Arkhangelsky et al. (2021) using the Stata command *sdid*. SDiD constructs synthetic control units using unit and time-period (pre-treatment) weights, with the goal of mirroring pre-treatment trends in outcomes among treated units and providing a suitable counterfactual. SDiD requires a balanced panel (in calendar time) and can accommodate staggered treatment. To calculate standard errors, we use the "placebo" option and 200 replications. For the analysis in Table 4, Panel B, we use a balanced panel of public and private hospitals from the previously mentioned five states for the period 2003–2019. We include private hospitals in the control group so that there are a sufficient number of hospitals with which to construct the synthetic controls. Hospitals that privatized prior to 2008 are dropped so that we observe at least five years prior to privatization, as in the main analysis with AHA data. In addition, we require that hospitals be present in the "base" AHA sample (i.e., hospitals in Table 2, Column 4; all of the above data sources have AHA IDs that allow merging) and have 10 or more uninsured hospitalizations per year between 2003–2007. We also drop two treated hospitals for which we observe outlier volume values in the year of privatization. Our final sample consists of 27 privatizations, 100 public controls, and 386 private controls.

For the analysis in Table 4, Panel C, we use the same data with the exception of MN, which only reports obstetric admissions beginning in 2007. In the FL, WA, and IN data, we define obstetric hospitalizations as those with an HCUP Clinical Classifications Software code between 176 and 196 based on the primary diagnosis ICD-9/10 code. In the CA data, we use nursery discharges as the number of obstetric hospitalizations. We drop any hospitals with an obstetric share of hospitalizations less than or equal to 2% in 2002. Obstetric closures are defined analogously as obstetric share dropping to 2% or below in a given year. The final sample for the obstetric analysis is a balanced panel of 338 hospitals, including 16 privatizations, 70 public controls, and 252 private controls.

B.4 Medicare fee-for-service claims

We access 100% Medicare claims and enrollment files at the National Bureau of Economic Research (NBER) through a data reuse agreement with CMS. We use data over 2000–2019 in our analysis, which approximately matches the period observed in the AHA annual survey and vital statistics datasets. The AHA files also mention the hospital CMS ID, which allows us to link the two datasets. We improve the crosswalk with manual validation to account for many-to-one links and changes in ownership. Thus, we identify the privatized and nonprivatized government hospitals of interest in Medicare claims. In our analysis using AHA data, we only include privatized hospitals that are observed for 5 years prior to treatment. To implement the same approach in the analysis using Medicare data, we limit the sample to 203 privatizations that occurred during 2005–18.

We use Medicare data to test the effects of privatization on patient complexity, treatment intensity, billing practices, and mortality by estimating models on patient-level data. We construct and use two measures of patient complexity for this analysis. We generate a predicted probability of

30-day mortality using a probit model based on patient demographics (gender, age, age squared), 30 Elixhauser risk flags based on the 90-day history of hospital inpatient and outpatient care, flags for utilization history of different types of care (hospital stay in the past 30 days, past 90 days, non-deferrable hospital stay in the past 30 days, and ED visit in the past 30 days) and the reason for hospitalization (flags for heart attack, pneumonia, stroke, and nondeferrable admission through the ED). In order to ensure that we observe sufficient claims history for each patient, we limit the sample to patients enrolled in Medicare Parts A and B for at least 3 months prior to admission. We estimate the probit model on data prior to any privatization, i.e., 2000–2004. The mean predicted mortality risk matches perfectly the observed 30-day mortality risk in the prediction sample. We then use the estimated model coefficients to predict the mortality risk for all patients in the analysis sample. We use the same vector of patient covariates when testing for changes in treatment intensity, billing, and mortality.

B.5 Vital statistics microdata

To calculate mortality rates at the market level, we combine individual-level mortality data from the CDC that span 1995 to 2019 with county-level population data from the National Cancer Institute's Surveillance Epidemiology and End Results (SEER) program.[27]. We further merge in population estimates from CDC Wonder for Hawaii for 1995–1999 since they were missing in SEER. We construct mortality and population counts for each HSA (for mortality events, we use the HSA of residence, not the HSA of occurrence) and year for six different age groups: all ages, $<$15, 15–34, 35–54, 55–64, and $>$65. We then calculate death rates for each HSA-year-age group as $100,000$ x number of deaths / population.

B.6 Healthcare Cost Report Information System (HCRIS) data

For total revenue and contract FTE variables, we use Healthcare Cost Report Information System (HCRIS) data from the Centers for Medicare and Medicaid Services (CMS) for the years 1997-2019. All Medicare-certified hospitals are required to submit an annual cost report to a Medicare Administrative Contractor; the data are publicly available for fiscal years 1996 onwards on CMS' website (https://www.cms.gov/data-research/statistics-trends-and-reports/cost-reports). Revenue variables come from Worksheet G-2 for both forms CMS-2552-96 and CMS-2552-10; we follow Lewis and Pflum (2017) in our cleaning steps. Total revenue is defined as the sum of gross inpatient revenue and gross outpatient revenue minus contractual allowances and discounts. Following Lewis and Pflum (2017), gross inpatient revenue is calculated as inpatient revenue minus gross ambulatory surgical center and hospice revenues. Gross outpatient revenue is defined analogously. Contractual allowances and discounts are found in Worksheet G-3. We use two-tailed winsorization at the 1% level among all hospitals in a given year to address outliers.

To construct the contract FTE variable, we follow the cleaning steps of Prager and Schmitt (2021), subsequently adopted by Andreyeva et al. (2024). Specifically, we sum the following contract labor variables from Worksheet S-3, Part II: top-level management and other management hours, physician Part A administrative hours, direct patient care hours, and contracted intern and resident hours. We convert to FTEs using a 40-hour work week and 52 weeks in a year. We set negative values, values outside the fifth and 95^{th} percentiles (among all hospitals in a given year), and values substantially different from the median within a hospital, to be missing. We then impute missing values by averaging non-missing values among adjacent years for a given hospital.

27. The data and data dictionary can be found at https://seer.cancer.gov/popdata/. The SEER data is designed to provide more precise population estimates for years between censuses, see e.g. Finkelstein et al. 2024; Ruhm 2015.

To align with other outcome definitions, we normalize total revenue and contract FTEs using adjusted admissions from AHA survey data.

B.7 Medical Expenditure Panel Survey (MEPS)

We obtained data on payer-specific mean reimbursement for inpatient care from the Medical Expenditure Panel Survey (MEPS) Hospital Inpatient Stays data. The MEPS captures the amounts paid to providers for all health care services used by the survey respondents. The MEPS has two features which make it well-suited for our purposes. First, it is designed to be a nationally representative survey. Second, the paid amounts are sourced directly from the providers so it does not rely on the recall accuracy of respondents. For these reasons, the MEPS has also previously been used to examine reimbursements for hospital care by payer (Hamavid et al. 2016). We used data for survey years 2000, 2005, 2010, 2015, and 2019 which span our analysis period. For the years 2000, 2005, 2010, and 2015, we pool expenditures paid by "other public" with Medicaid since "other public" are expenditures paid by Medicaid for non-Medicaid enrollees. For those same years, we pool expenditures by "other private" with private. For 2019, categories "other public" and "other private" are not reported anymore in the MEPS. For all years, we combine payments to doctors and payments to facilities and inflation-adjust expenditures using the CPIU series to reflect 2019 price levels. Lastly, we drop observations where all expenditures are equal to 0 as well as outliers with total payments below the 1^{st} and above the 99^{th} percentile for each year. We combine data over all 5 years to calculate the mean unadjusted reimbursement per hospital stay by payer.

C Methodology

C.1 Propensity score matching

In one of our robustness checks reported in Table 7, we apply propensity score matching (PSM) to our analytic sample to identify treated and control hospitals that are similar on pre-treatment observables. Specifically, we conduct one-to-one, nearest neighbor matching without replacement and estimate logit models to predict privatization with the following explanatory variables from T-1 to T-3 (where T denotes the year of privatization for a given treated hospital):

- # hospital beds
- Total admissions
- Medicaid admissions
- Total expenses
- % in poverty (measured at the county-year level)
- % unemployment (measured at the county-year level)
- Health Service Area population (only t-1; calculated by aggregating county-year population estimates)

We impose the restriction that propensity scores of matched pairs be in the same decile of the propensity score distribution (Diamond, Guren, and Tan 2020). Within this tolerance band, we assign the nearest neighbor as the match. We apply PSM sequentially by first searching for similar comparison hospitals for those that privatize in 2000, the first transaction year in our data. Control hospitals that match these privatizing hospitals are removed from the donor pool prior to searching for matches for hospitals that privatize in 2001. We continue this process for all 19 years of privatizations (2000–2018) and are able to match all 258 treated hospitals.

We also apply PSM to our market-level (HSA) sample using an analogous approach. The only difference is that we match the total number of hospitals in the market from t-1 to t-3, rather than the total number of hospital beds.