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 $700 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 a 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 recognized the benefits of privatization for some time. 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, improving 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. In several countries, including Germany and Sweden, there are ongoing discussions or actions to privatize healthcare providers, sparking considerable controversy (Bergman et al. 2016; Knutsson and Tyrefors 2022).[1]

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 (Cutler and Gruber 1996).[2] If government care complements Medicaid, then privatization could

1. Germany has privatized more than 100 hospitals since the 1990s (Heimeshoff, Schreyögg, and Tiemann 2014). England introduced legislation in 2012 to encourage the private delivery of healthcare (Goodair and Reeves 2022). Sweden implemented privatization in multiple segments during 1990–2013 with mixed effects (Dahlgren 2014). Madrid, Spain, attempted to privatize hospitals, but the proposal had to be scrapped due to protests by unions. The provincial governments of Quebec and Ontario in Canada are reportedly considering privatizing hospitals. The city of Detroit privatized its public health department in 2012 but reversed course following the Covid-19 pandemic.

2. According to the Centers for Medicare and Medicaid Services (CMS), Medicaid had 79 million enrollees as of

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. This question is pressing, as our findings suggest higher privatization rates in states that mandated greater expansions in Medicaid. Finally, privatization is known to affect employment and wages in both privatized and competing firms (La Porta and López-de-Silanes 1999; Arnold 2022). Given that the hospital sector employs more than 6.5 million people in the US, comparable in size to the entire construction sector, and more government employees than any other sector besides education, privatization could have large consequences for workers.[3] To our knowledge, this paper provides the first causal estimates to inform these policy debates.

This paper quantifies the effects of approximately 260 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 surveys, and vital statistics microdata from the National Center for Health Statistics (NCHS). These rich data enable us to comprehensively examine changes in hospital performance, such as profitability and patient volume, as well as outcomes for key stakeholders, including employees, patients, and the local community. We complement these sources with publicly available files from Medicare cost reports and the US Census.

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 the privatized hospitals do not differ from the comparison group before the transition but experience 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.

November 2023. US population was estimated at 336 million in November 2023 by the Census Bureau.

3. Source: 2019 Quarterly Census of Employment and Wages and Current Employment statistics from the Bureau of Labor Services (BLS). The construction sector, NAICS code 23, employed about 7 million people in 2019.

A key purpose of privatizing hospitals is to improve their profitability so that they become financially sustainable without government subsidies. Before privatization, treated hospitals were unprofitable with an average operating margin of -4% (of revenue). Their 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; Schulman and Milstein 2019).[4] Medicaid patients experience nearly 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. We also find evidence to support cream-skimming on other dimensions, such as decreasing relatively unprofitable services and admitting patients of observably lower complexity. Second, privatized hospitals differentially increase their list prices, also known as charges, by 7%. Although these are not the negotiated prices hospitals receive from private insurers, they are often used as the basis for negotiations (Cooper et al. 2019). Hospitals also bill uninsured and some other types of patients using list prices directly.

We find consistent evidence of lower quality of care and reduced access to care for some patient groups, consistent with theoretical predictions (Shleifer 1998). We detect an approximately 3% increase in 30-day mortality rates among Medicare patients 65 years and older in the privatized hospital, which implies an increase of 3.6 deaths at the average treated hospital. 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 aggregate 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 suggestive evidence of an increase of 5 deaths per 100,000 people aged 55 to 64 years in treated markets. This estimate is statistically insignificant, but implies an additional 1.7 deaths in the average market. Nonparametric analysis reveals a strikingly linear relationship between market-level changes in Medicaid admissions and deaths, suggesting that

4. Using data from the nationally representative Medical Expenditure Panel Survey (MEPS) over 2000–2019, we find that Medicaid and self-pay mean inpatient reimbursement rates are 69% and 44%, respectively, of the mean Medicare rate.

the Medicaid admissions avoided were likely of high medical value. If we consider the increase in near-elderly deaths, the average privatization leads to an increase of 5.3 deaths 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.[5] In fact, we know of only a few relevant studies even outside economics, such as Ramamonjiarivelo et al. (2016) and 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 confirm the trade-off between efficiency and care quality hypothesized at the beginning, but also enable us to comment on cost effectiveness. Our estimates imply that the average privatization generates $0.8 million in savings for the government per additional death in the short term (2.7/3.6). This is our main estimate, and under different assumptions, we estimate an upper bound of $1.2 million. 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 threshold value of statistical life estimate (VSL) of 10 million used by federal government agencies to assess the cost effectiveness of new policies (HHS 2017; Viscusi 2018). Hence, the average hospital privatization would not be deemed cost effective under federal guidelines.

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: in 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 patients, 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 2023).

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). National spending patterns for Medicaid and Medicare cor-

5. 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).

roborate the idea that Medicaid beneficiaries face additional barriers to using care. Spending per enrollee has grown nearly twice as fast for Medicare beneficiaries relative to Medicaid during 2000–19. Similarly, the growth in Medicaid hospital admissions (18% over 2000–19) is substantially lower than the 38% we would predict based on quasi-experimental estimates of the effects of Medicaid coverage on hospital use.[6] Our results imply that hospital privatization is another factor suppressing utilization by Medicaid recipients and can explain about 20% (4%/20%) of this gap in markets that have experienced privatizations. Although we have begun to shed light on this important issue, more work is needed.

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.[7] 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. 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 Americans). Unless otherwise noted, we will use nonfederal and public hospitals interchangeably.

We note two interesting patterns in hospital ownership. First, states vary enormously in their dependence on public hospitals. Pennsylvania has only 4% of beds in state or local government hospitals, while 44% of Alabama's hospital beds are in such hospitals. This variation is even

6. Medicaid enrollment grew 38% between 2000–19, from 15.7% to 21.7%, per data from the Kaiser Family Foundation. Applying the estimated effect of gaining Medicaid on hospital use of 100% reported by Duggan, Gupta, and Jackson (2022), we predict an increase of 38% in hospital stays.

7. The AHA survey reports hospital "control", which could be recorded as one of nonprofit, for-profit, or government. Control and ownership are identical, 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.

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. Texas and Alabama have higher public hospital shares than Illinois and Pennsylvania. Similarly, the state's rural share of population does not explain public provision: New Hampshire, Maine, South Dakota, North Dakota and Vermont are among the most rural states in the US and have the lowest shares of public hospital capacity.[8] This puzzling heterogeneity suggests that there is little consensus across states on the appropriate scope for public delivery of hospital services and deserves greater research scrutiny.

2.2 The tradeoffs in privatization

There is 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, the start of our sample period, 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.[9] These amounts indicate the 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. 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 relevant to the hospital sector. First, insurer contracts with hospitals are

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

9. 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.

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 (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 by 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 tradeoff 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. However, as discussed earlier, evidence is currently very limited. This paper aims to fill this gap.

2.3 Government provision versus coverage of hospital care

With the perspective of the tradeoffs involved in hospital privatization, we now describe trends in privatization in the US over the last few decades. 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, 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%.[10] 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, state and local governments viewed the expansion of Medicaid coverage as an alternative means to ensure access to care, making it easier to justify the privatization of public hospitals. To further investigate this hypothesis, we formally test whether there is a negative correlation between government control of hospitals and 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 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 an 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 abso-

10. 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.

lute 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.

2.4 Hospital privatization in the US

The number of hospitals in the US under government control has declined through two channels. First, local governments relinquished operational control of the hospital to a private firm, which we call privatization. This accounts for 85% of the decrease in the number of government hospitals. Second, public hospitals closed operations entirely or turned to only outpatient care. We identified 258 and 41 cases of privatizations and closures over 2000–18, respectively. To put this figure in context, consider that of the 1,060 public hospitals present in our sample in 1999, nearly a quarter were privatized within 20 years.

We classified privatization deals according to their key features. 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 deals 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. Following Hart, Shleifer, and Vishny (1997), we define the control type according to which entity has operational control.

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. It is unclear how much incentive the private operators have to improve the hospital's profitability under these arrangements and whether they have the autonomy to focus on more profitable patients and services or to cut staff. The press reports did not suggest constraints on their ability to make such changes.

Private operators enjoy substantially more operational control over the hospital in the second group of deals. This group contains three types of deal structure. 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 operators are significantly more likely to be involved in the second group of deals than in the first. They are involved in more than 40% of the deals that grant more control but less than 20% of the deals that grant less control.

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 volume for three payers: Medicare, Medicaid, and a residual group ("Other") is largely made up of privately insured and uninsured patients. We cannot separately observe the numbers of uninsured and privately insured patients in the AHA data, but we do so using other datasets described below. We examine the total full-time equivalent (FTE) employed staff and the effects on different staff categories (physicians, nurses, and others). To avoid potential bias due to the substantial skew in outcomes across hospitals of different sizes, we transform the variables in the regression analyses. In the case of revenue, expenses, and patient volume, we use logs. In the case of labor inputs, we normalize by adjusted admissions, which incorporate both inpatient admissions and outpatient visits.[11]

11. The AHA reports adjusted admissions, which we use directly. These are typically computed by adding to hospital admissions the number of outpatient visits, scaled by the ratio of outpatient charges to inpatient charges (Schmitt 2017).

We identify the privatization of government-owned hospitals using a multi-step process, following previous studies on changes in hospital ownership (**andreyeva2023**; Schmitt 2017; Cooper et al. 2019; Prager and Schmitt 2021). 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) when studying 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 administrative data on hospitals in 5 states (CA, FL, IN, MN, and WA), which allow us to observe 27 privatizations during 2007–2018, approximately 10% of the total number of privatizations studied using AHA data. In the case of Florida, Indiana, and Washington, we have access to comprehensive hospital discharge data. In the case of California and Minnesota, we use annual reports on hospital patient volume. 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.

Medicare claims

The AHA data do not allow studying changes in a hospital's patient mix or health outcomes. 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 Medicaid and Medicare 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. Second, we test for changes in hospital chargemaster rates after privatization while controlling for changes in patient risk. This provides some insight into hospital billing practices. Third, we examine changes in patient mortality rates to test for causal effects on hospital quality. Section B.3 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.4 provides more details.

Supplementary data

We supplement the main data sources with information from publicly available files as needed. We source data on hospital revenue from the Healthcare Cost Reporting Information System (HCRIS), more commonly known as Medicare cost reports. We obtain nationally representative mean hospital reimbursement rates by payer from the Medical Expenditure Panel Survey (MEPS), described in more detail in Section B.5. All monetary values are deflated to 2019 dollars. 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).

3.2 Descriptive evidence

Table 2 describes the AHA analysis 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 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 imposed this distance requirement to mitigate the potential for spillover contamination.[12] Com-

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

paring 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 public payers. 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.

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, Georgia, Louisiana, Indiana, and Minnesota 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 5-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 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%, respec-

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.

tively. 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

Our goal is to quantify the causal effects of privatization on affected public hospitals and their patients, as well as on the markets they serve. 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 pretrends, 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 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_{mt}, a vector of market attributes that change over time, including population-level unemployment, poverty, and uninsurance rates for the county in which the treated hospital is located. We do not include time-varying hospital-level covariates (e.g., bed capacity, services offered) in the models since most such attributes would plausibly be affected by the privatization. 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.

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

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

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 length of stay in the hospital or mortality after discharge. This allows us to include patient covariates to control for differences in risk between patients. Similarly, we estimate an equivalent model on market-level data to quantify the effects of privatization on the local market.

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. 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 suggests little or no differential pretrends and relatively large changes in trends 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. We also exclude the year of privatization (year zero) since it represents partial treatment and adds to measurement error. We assess the robustness of the estimates from the primary specification to using alternate modeling assumptions (including weighting), estimators, sample construction rules, and comparison groups.

5 Effects on the privatized hospital

5.1 Finances

A key 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_{mt} in Panel A, while Panel B presents the corresponding results obtained by including controls at the county-year level for population, percent in poverty, percent

unemployed, and percent uninsured. 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 like 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 $485 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% level 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% 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 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). The 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 priva-

tization, 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 $255) 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 healthy 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 the public mission, 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 simply reflect a shift toward outpatient care following privatization. We test this conjecture and fail to detect an accompanying increase in outpatient

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.

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 implies, 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 also reduces the capacity of beds in a proportionate way or operates the hospital at a lower occupancy. Using the same research design, we estimate a 8% decrease in bed capacity after privatization, while patient volume per bed remains stable. For brevity, we do not present these results. Therefore, it appears that private management downsizes the hospital soon after they assume control and decreases patient volume accordingly.

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 Medicaid admissions decline by nearly 15%, Medicare admissions only 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 represented 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 lower reimbursement rates (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 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 pa-

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 68/307 or 30% of the estimated decrease in admissions.

tient 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 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 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 (SDiD) estimator (Arkhangelsky et al. 2021). SDiD constructs a weighted average of observed control units to generate a synthetic control unit for each treated cohort of hospitals. 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), SDiD also addresses concerns due to staggered treatment. In the case of a staggered treatment setting, this method does not permit producing 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]

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.

16. SDiD uses time-varying weights to generate the synthetic control trend corresponding to each treated cohort. Therefore, it is not possible to average synthetic control values across different cohorts without invalidating the design. We use the *sdid* command in STATA with the placebo inference option (200 replications) to implement the estimator.

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 summarizes 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, 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, we estimate that the overall mean reimbursement increases from approximately $12,560 to $12,770, an increase of 1.7 pp. Hence, cream-skimming more lucrative payers can explain about 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 admitting less than 5% of the obstetric patients observed in the hospital in 2003, the first year we observe all hospitals. 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. Together, the results indicate that the decrease in obstetric volume is completely 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 uninsured, respectively, before treatment. Simple back-of-the-envelope calculations imply that the decrease in obstetric admissions can account for approximately two-thirds and a quarter of the decrease in Medicaid and uninsured admissions, respectively, in these states. They also explain 54% of the decrease in total admissions.[17] Therefore, closing obstetric wards is an important driver of the effects on patient volume and payer mix discussed previously.

In closing, we would like to emphasize that since the analysis using the state data 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.3 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.

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

17. A 52% decrease in obstetric admissions implies 533 fewer cases against a baseline of 1024 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 1713 (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.

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 columns 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 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 are consistent with upcoding, since patients with lower complexity were billed at the same level as before.

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). Finally, uninsured and insured out-of-network patients 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.

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, implying that reducing employee count is the primary channel to decrease personnel spending rather than reducing employee compensation.[18] 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]

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, 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.

We also test the possibility whether the decline in employed staff is partially offset by an increase in the use of contract labor after privatization. This is crucial since it affects how we in-

18. In results not summarized here, we estimate a statistically insignificant 3% decline in compensation per employee at the privatized hospital.

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

terpret the decline in employment discussed above. 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. 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 therefore 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. As with the results on hospital finances, we assess the sensitivity of these estimates to scaling the outcomes by the contemporaneous number of beds instead of adjusted admissions. Table A.9 presents the corresponding estimates. The results are qualitatively similar, driven largely by the "Other" group, and suggest a slightly smaller decline in labor intensity than the estimates discussed above (7% vs. 8%). Figure A.9 presents the corresponding event study plots, which are consistent with the point estimates.

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 were also able to reduce 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 reduced about 70 FTE (13.5% of baseline) in the five years following the change in control, of which 55% was due to more efficient use of labor. One caveat in interpreting the decrease in labor intensity as an improvement in productivity is the simultaneous decrease in patient complexity and intensity of treatment.

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 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 de-

crease 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 increasing revenue 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 at acquired facilities by centralizing personnel, particularly in administrative and support functions (Andreyeva et al. 2024). Systems are the 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 (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 since the coefficients remain within two standard errors of the baseline estimates across all checks.

Panel I tests robustness to alternate specifications. Row IA presents coefficients obtained from regressions weighting hospitals by beds.[21] 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 including state-by-year fixed effects. This ensures we compare privatized hospitals to 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 trend for each hospital. These trends were estimated in a previous step using data over 1995–1999, prior to the

21. For this exercise, we hold bed capacity fixed. 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.

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) andDe Chaisemartin and d'Haultfoeuille (2020), respectively. These correct for staggered designs in different ways and estimate the weighted average treatment on the treated. 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 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 be interpreted as patient reallocation 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. There are 910 HSAs in the US and the average HSA 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 fee-for-service patients choose a hospital located in the same HSA as their home zipcode. Second, HSA borders follow county boundaries, which allows us to directly map 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 706 remaining markets form the comparison group.[22] 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 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,

22. We considered imposing a non-neighbor rule for comparison markets to mitigate the potential for spillovers. But such a rule would nearly eliminate all potential untreated markets in the same states as the treated markets, which was very unappealing.

such as poverty and unemployment. The average treated market contains 5.4 hospitals, of which 1.3 or 24% 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 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.

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, approximately what we would predict based on the privatized hospital's decline alone (24% of -14.9, or -3.6 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 trend is 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 hospitals since local hospital markets were already concentrated at the start of the sample period and became even more concentrated over time. The mean Herfindahl Hirschman Index (HHI) in 2000 across hospital markets was nearly 3,000, well over the federal government's threshold for "highly concentrated" (DOJ 2010), and increased to about 4,000 by 2020 (**andreyeva2023**). Patients in more concentrated markets will have fewer options and cannot adequately punish poor service or quality of care (Shleifer 1998). Hence, in more concentrated markets the new management may decrease Medicaid volume to a greater extent to improve profitability. We accordingly test for differential decline in patient volume in more concentrated markets. We designate treated markets as more concentrated if their HHI was above the median value across 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 greater share of the market (44% vs. 24%) 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. It is possible that the remaining hospitals in these markets 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 have Medicaid coverage (Gruber 2008). These disparities are also observed across markets with different levels of income. The uninsured disproportionately reside 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 greater proportion of residents in poverty have lower operating margins, capacity and quality of care. Consistent with this concern, Pagán and Pauly (2006) find that even insured individuals report greater unmet medical needs in lower income markets. 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 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). In contrast, the effects on Medicare and Other admissions are small and statistically insignificant.

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 itself. 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 across all markets, while Panel C presents the results from a heterogeneity test described below. The columns present effects on total FTE and its component groups, physicians, nurses, and "Other". Qualitatively, these results also suggest a decline in employment in Other. However, we are under-powered to reject the null hypothesis against effects of this magnitude. For example, we can only reject the null against a decrease of more than 0.21 FTE at the market level, which significantly exceeds the effect we would expect based on the direct effect at the privatized hospital alone (24% of 0.57 = 0.14 FTE).

As with market-level effects on utilization, the average effect on labor intensity can mask heterogeneity across different types of markets. Previous studies have noted the influential role of unions in preventing employment losses (Atanassov and Kim 2009). Powerful unions may prevent privatizations in the first place (Shleifer 1998). We test the hypothesis that the private operator is able to implement greater staff cuts in markets where unions are less influential. We measure union prevalence using state-year level data on the share of unionized employees in the private services sector.[23] We assign markets with below-median values of this measure (about 4%) in 1999 to weak union presence. We then implement a triple difference specification testing for differential effects in markets with weaker unions. Table A.12 panel C presents the corresponding results. Consistent with the hypothesis that unions prevent large employment cuts, we find a greater reduction in market-level labor intensity in markets with weaker unions. The difference between markets with low versus high union presence tends to be statistically significant across outcomes. Taking the case of total FTE, the aggregate effect in low-union presence markets is -0.2 FTE per 100 adjusted admissions (0.13 - 0.33 = -0.2). The direct effect on employment at the privatized hospital can account for about half of this aggregate effect.[24] This suggests that competing hospitals in these markets also reduce employment in response and these spillovers contribute the remaining half. This result suggests that government labor unions also provide some protection against job losses to those employed at private firms in their industry.

23. The measure of unionization by state was computed using the data maintained by Hirsch and Macpherson (2022) at www.unionstats.com. They combine multiple data sources, but mainly rely on the CPS.

24. In unreported results, we find an effect of -0.48 FTE at the privatized hospital in these markets, and they account for about 22% of the market. Hence, the direct effect alone predicts a reduction of 0.22 x 0.48 = 0.11 FTE at the market-level.

6.3 Robustness

Appendix Table A.13 presents the results of 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 that consistently experiences 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 or suffer disruptions in their treatment plan. Quality of care and health are multidimensional objects, and a comprehensive examination of the two could be a separate paper on its own. 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 commonly accepted value of a statistical life (VSL) estimates from the literature 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. Panel A of Table 10 presents the corresponding coefficients. The top row presents coefficients from a model that includes patient covariates to control for differences in patient risk. The bottom row also includes time-varying markets covariates. Row A1 reports a small increase in mortality of 0.33 percentage points across all FFS patients aged 65 or more, 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 supports the interpretation of an increase in mortality. There is an immediate increase in mortality following privatization which persists for the 5 year duration. 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). A limitation of this approach is the decrease in sample size, as this group contributes only about a quarter of all patients. Column 2 presents the corresponding results and shows a statistically significant increase of slightly larger magnitude than that reported for all patients. Since the baseline mortality rate for this group is higher than the overall average, the relative increase is similar. Figure 8 Panel (b) presents the corresponding event study plot. Back-of-the-envelope calculations using our preferred estimate of a 0.33 pp increase imply that the average privatization in our sample leads to an increase of 3.6 deaths among Medicare fee-for-service patients per year.[25]

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 disproportionately 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 Medicare patient groups.

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

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.

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.

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; 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.[26] Therefore, this approach recovers the "intent-to-treat" effect of privatization. To maximize statistical power and our ability to detect an effect on mortality, we limit the sample to those aged between 55 and 64 years. 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 Current Population Survey (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 residing in the affected market, defined by the HSA. We find a positive but statistically insignificant effect of about 5 deaths per 100,000, representing a 0.5% increase relative to baseline. The average treated market had about 42,160 individuals in this age group in 2000, and hence this estimate implies an increase of 2.2 deaths per year. Since treated markets experienced 1.3 privatizations on average, this implies about 1.7 additional deaths per privatization. However, since the average effect is estimated with imprecision, we consider three more pieces of evidence to corroborate this interpretation.

First, we test whether there is an association between the effects on market-level Medicaid hospital admissions and mortality. In a first step, 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 using the baseline market-level model. We therefore obtain two series of estimates with 204 observations each, one for each treated market. In the second step, we regress the effect on Medicaid 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.

To summarize this analysis, we present binned scatter plots in Figure 8. We limit these tests to the payers for which we detect a decrease in hospital admissions. 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

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 is likely to be much lower for people younger than 55 years.

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, suggesting that a linear fit is a reasonable approximation. 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. This is approximately 70% of the estimated effect on mortality. Hence, the decline in access to hospital care for Medicaid patients appears to be an important channel for the increase in mortality among the near-elderly. Similarly, Panel (d) shows an association between the effect on mortality and changes in Other admissions. The slope coefficient is even larger than in the case of Medicaid and significant at the 10% level. The relative imprecision in this relationship may be driven by the fact that Other admissions include both private and uninsured patients who experience very different effects, as shown in Section 5.2.2. Together, these patterns strengthen the interpretation that a reduction in hospital access leads to greater mortality rates

If the average effect on mortality at the market level is causally related to privatization, the effect should be greater among subgroups that suffer 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, which increases to 18.8 when we adjust for differences in market factors. This represents approximately 2% of the baseline mortality rate. Appendix Table A.15 presents additional results on mortality effects 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.

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 showed 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 columns 3 and 4 present 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 implies an increase in mortality of approximately 39 per 100,000, or 4% of the baseline mortality rate. This result is statistically significant at the 10% level and suggests that publicly owned hospitals serve a vital social function in lower-income markets.

Our estimated effects of privatization on mortality, whether in the affected hospital, county or market, are smaller in magnitude than the effects reported in previous studies of 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, our estimated effects 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 the immediate effects we consider. 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 subsidy 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. Applying a recently estimated tax rate of 2.1% of revenue to hospitals in our sample, we estimate the incremental tax revenue to be $1.2 million per for-profit hospital, or 0.3 million from the average privatization (Rosenbaum et al. 2015). The net savings for the government from the average privatization 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 the 56% 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 costs incurred in terms of lives lost due to privatization. Since our estimate of the increase in deaths among 55-64 year olds in the affected market is imprecise, we focus on the additional deaths among Medicare patients in the average privatization as our central estimate. As discussed in Section 7.1, we estimate an average of 3.6 deaths for Medicare patients 65 years and older. An upper bound estimate includes deaths in the community, 1.7, and is therefore 5.3. 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 additional death (2.7 / 3.6). The lower bound to this estimate also considers deaths in the community and is about $0.5 mn. On the other hand, the upper bound includes surplus revenue and ignores deaths in the community and is about $1.2 mn per death. Even the upper bound estimate is well below the threshold value of a statistical life (VSL) of $10 million recommended by the US Department of Health and Human Services and other federal agencies to consider a new policy to be cost-effective (HHS 2017; Viscusi 2018).

The second policy debate centers on the design of the social safety net. Specifically, if the government is expanding Medicaid coverage, does it also need to provide hospital care? National statistics on hospital utilization and spending on Medicaid are consistent with the notion that Medicaid beneficiaries face additional barriers to accessing care. For example, although Medicaid enrollment has grown faster than that of Medicare over 2000–19 (61% versus 55%), aggregate spending on Medicaid has grown at a much lower pace than for Medicare (106% versus 140%). Adjusting for the differential growth in enrollment enhances the difference – spending per enrollee has grown nearly twice as fast for Medicare than for Medicaid on average during this period (2.9% per year vs. 1.5%).[27]

The growth in aggregate hospital utilization by Medicaid patients (about 18% over 2000–19) appears low compared to predictions using estimates, taken from the literature, of the percent increase in hospital utilization after gaining Medicaid coverage. Estimates span a wide range, starting as low as 30% reported by the Oregon Health Insurance Experiment, up to 280%, reported by a quasi-experimental study of the Medicaid expansion mandated by the Affordable Care Act (Finkelstein et al. 2012; Dunn, Knepper, and Dauda 2021). The Oregon experiment involved a relatively small expansion in Medicaid, and its effect may understate the general equilibrium effects on utilization expected following the ACA. We prefer the estimate of 100% reported by Duggan, Gupta, and Jackson (2022) using data from California, which reflects general equilibrium effects and falls near the middle of this range. Applying this estimate to the observed growth in Medicaid enrollment (38%, reported by the Kaiser Family Foundation), we expect a 38% increase in Medicaid hospital utilization. Hence, the observed growth in Medicaid hospital utilization is about 20 percentage points (pp) lower than what estimates from the literature predict. Our results imply that privatization can explain about 4 pp, or about 20% of this gap in the markets exposed to

27. Medicaid enrollment information sourced from Kaiser Family Foundation which provides granular data by state. Medicare enrollment and spending information on both programs sourced from the National Health Expenditure Tables, 2020. We deflate spending to 2019 dollars using the CPIU reported by the BLS.

privatization, assuming that the affected markets are representative of the average market.

9 Conclusion

Privatization can improve the profitability and growth of public firms, but may hurt some stakeholders. This trade-off assumes greater significance in the case of hospital care, which has unique challenges and has experienced substantial privatization. Yet this phenomenon has largely been ignored by researchers. We provide novel evidence from the privatizations of public hospitals in the US over 2000–2018. We confirm that privatization improves hospital profitability sufficiently so that the hospitals transition from being loss-making to generating a modest surplus.

The main channel to improve profitability is by increasing mean revenue per patient. This is partly achieved by cream skimming more profitable patients along multiple margins. One margin is to disproportionately reduce the intake of Medicaid and uninsured patients. This strategy has larger implications beyond the financial health of the privatized hospital since we detect a decline in aggregate Medicaid volume at the market level, raising the concern of a reduction in access to care for vulnerable low-income patients. The decline in access is much greater in certain types of markets, such as those with high poverty rates and high concentration levels. We also detect an increase in all-cause mortality among 55–64 year olds in high-poverty markets following privatization, consistent with the argument that public hospitals perform a crucial safety-net role. Overall, our results imply that privatization is quantitatively important in explaining why growth in Medicaid utilization and spending has lagged growth in enrollment.

There are several avenues for future research. Our main analysis relied on hospital- or county-level data, since we wanted to characterize the effects of privatization nationally. Our analysis using hospital discharge data begins to shed light on how privatization shifts focus away from uninsured patients and select service lines, but more investigation is warranted into changes in admission practices and hospital quality. Researchers with access to detailed patient-level claims data, perhaps focused on narrower geographies, can make progress on these questions. Future studies can use more granular employment data to examine labor and wage dynamics for workers in markets experiencing privatizations. These inputs are needed for a comprehensive welfare analysis of privatization.

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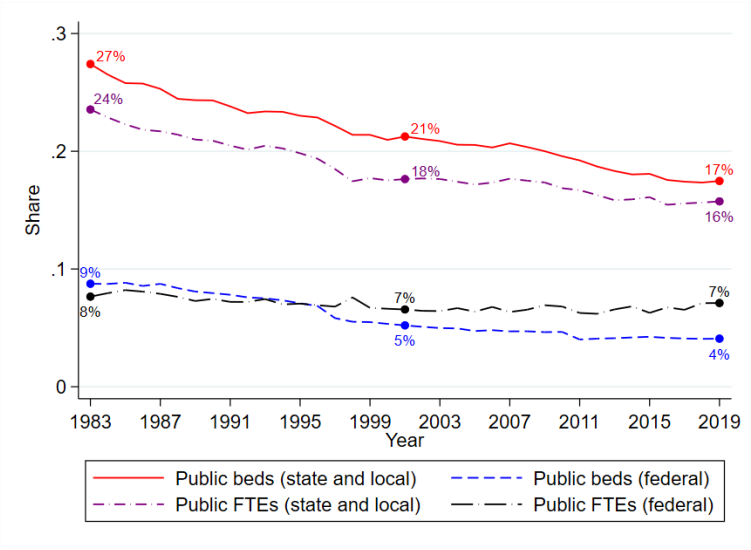
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(a) As provider



(b) As payer

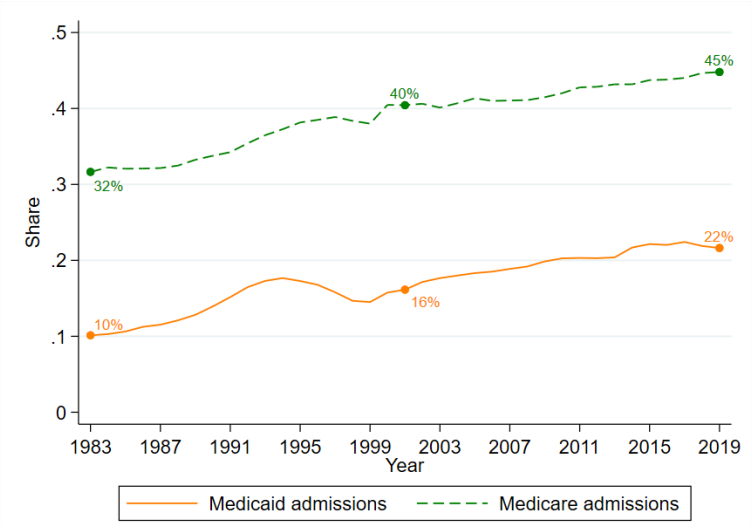
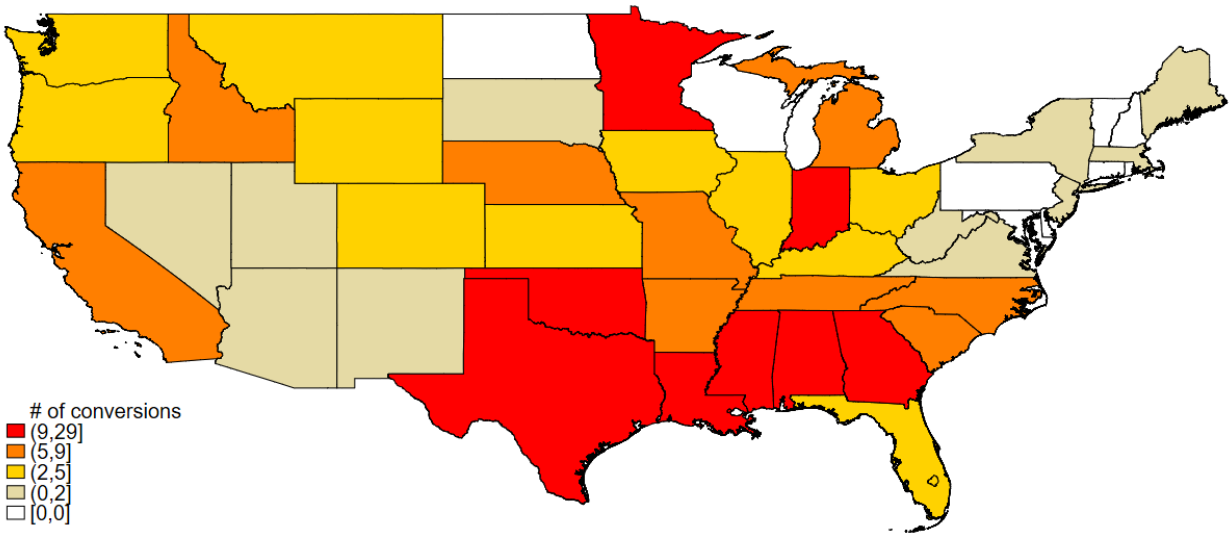


Figure 1: Government role in hospital care

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 lines, respectively) and by public, federal hospitals (blue and black 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

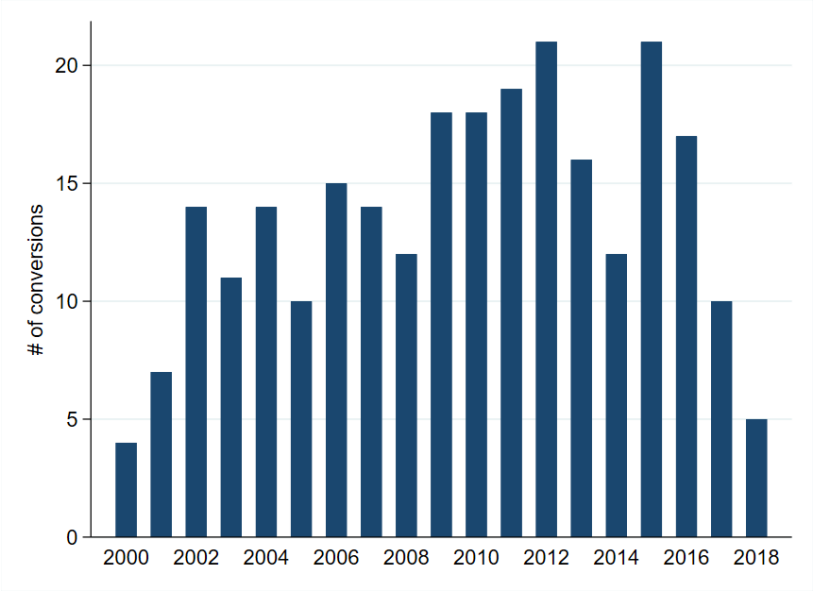


Figure 2: Privatizations

Note: The figure presents the distribution of nonfederal, public-hospital privatizations in our analysis sample during 2000–18. We restricted 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 but are included in the sample and experienced 4 and 1 conversions, respectively. We manually validated each privatization.

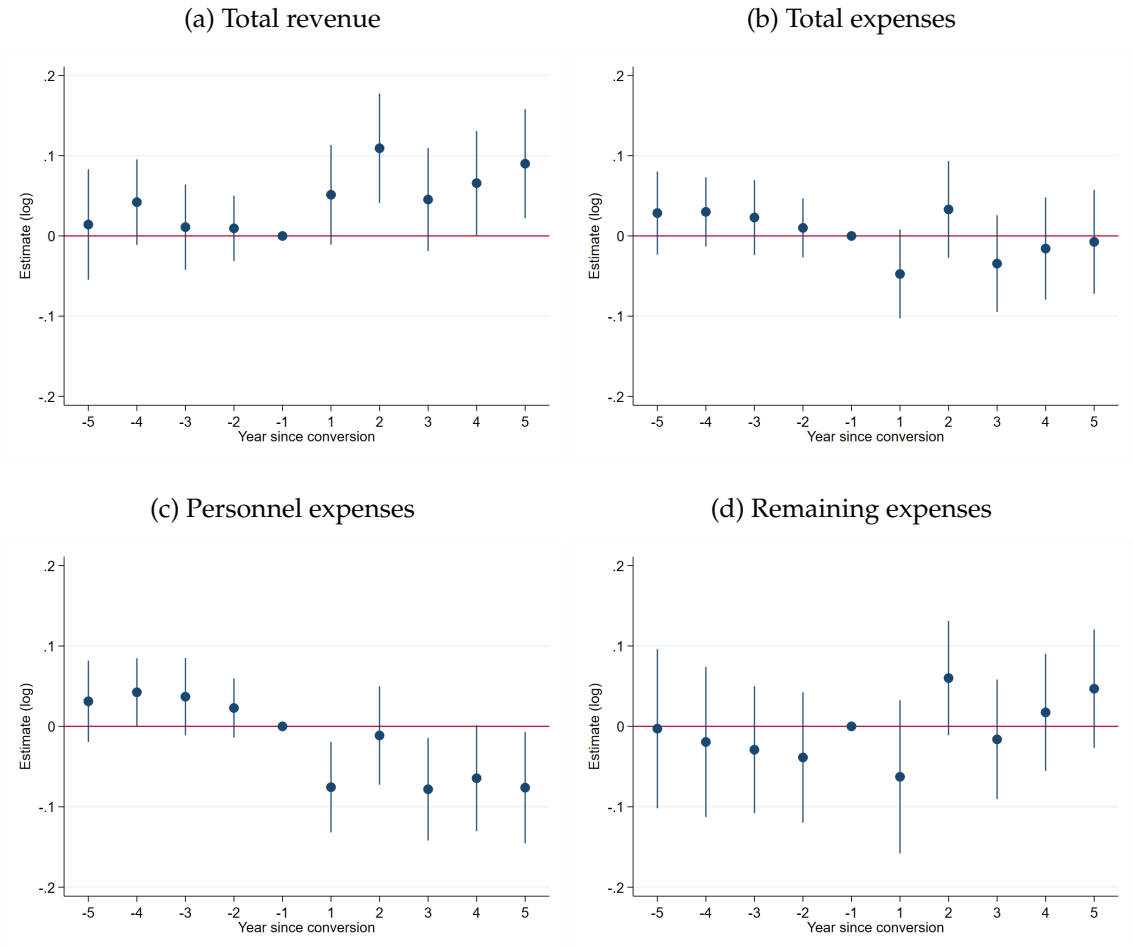


Figure 3: Effects on log finances (per patient)

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 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.

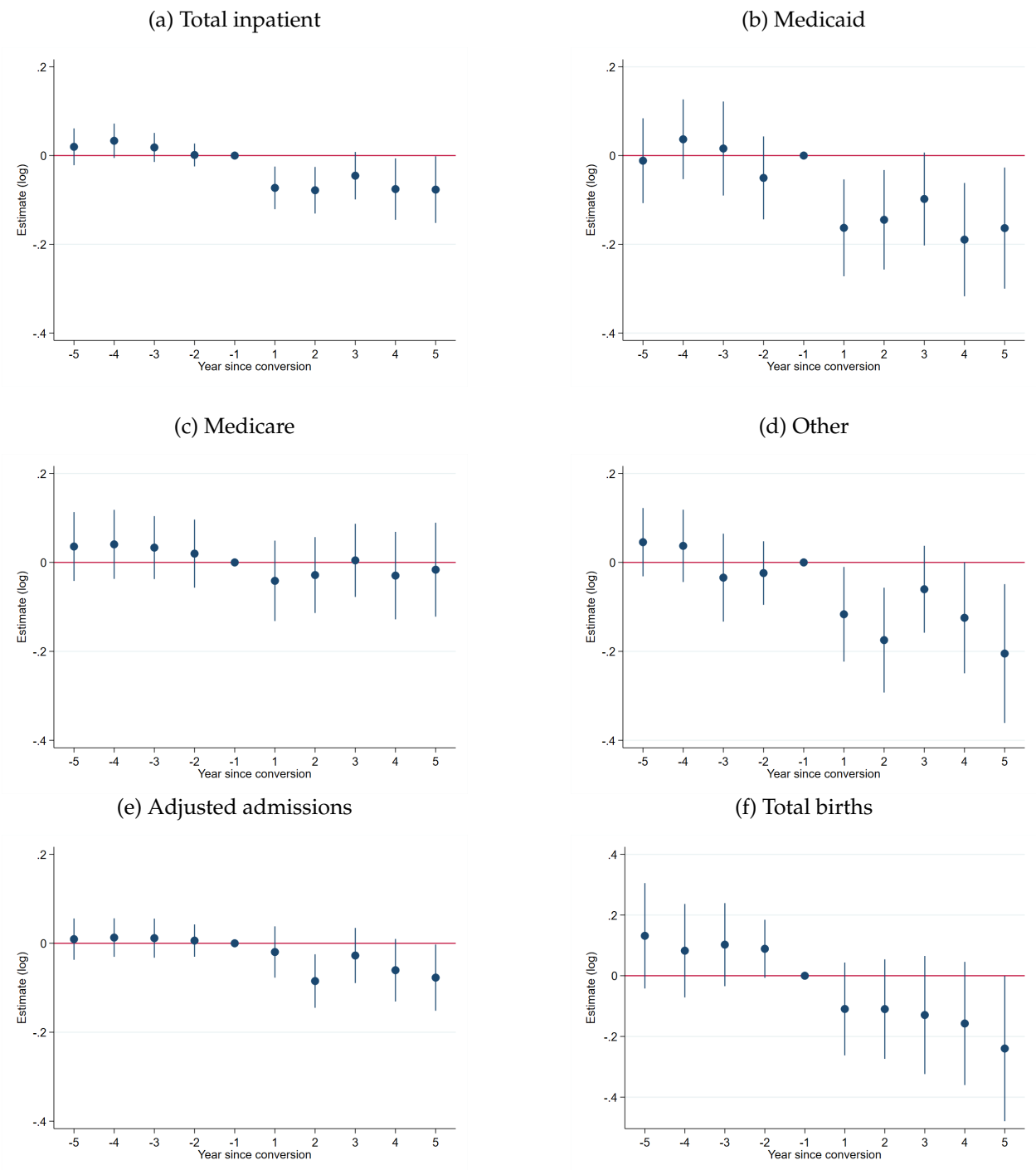


Figure 4: Effects on patient (log) volume

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. 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 refer to hospital admissions not covered by Medicaid or Medicare and mostly comprise privately insured and uninsured patients. Panel (e) presents the effect on adjusted admissions, which includes both inpatient and outpatient volume, and therefore 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.

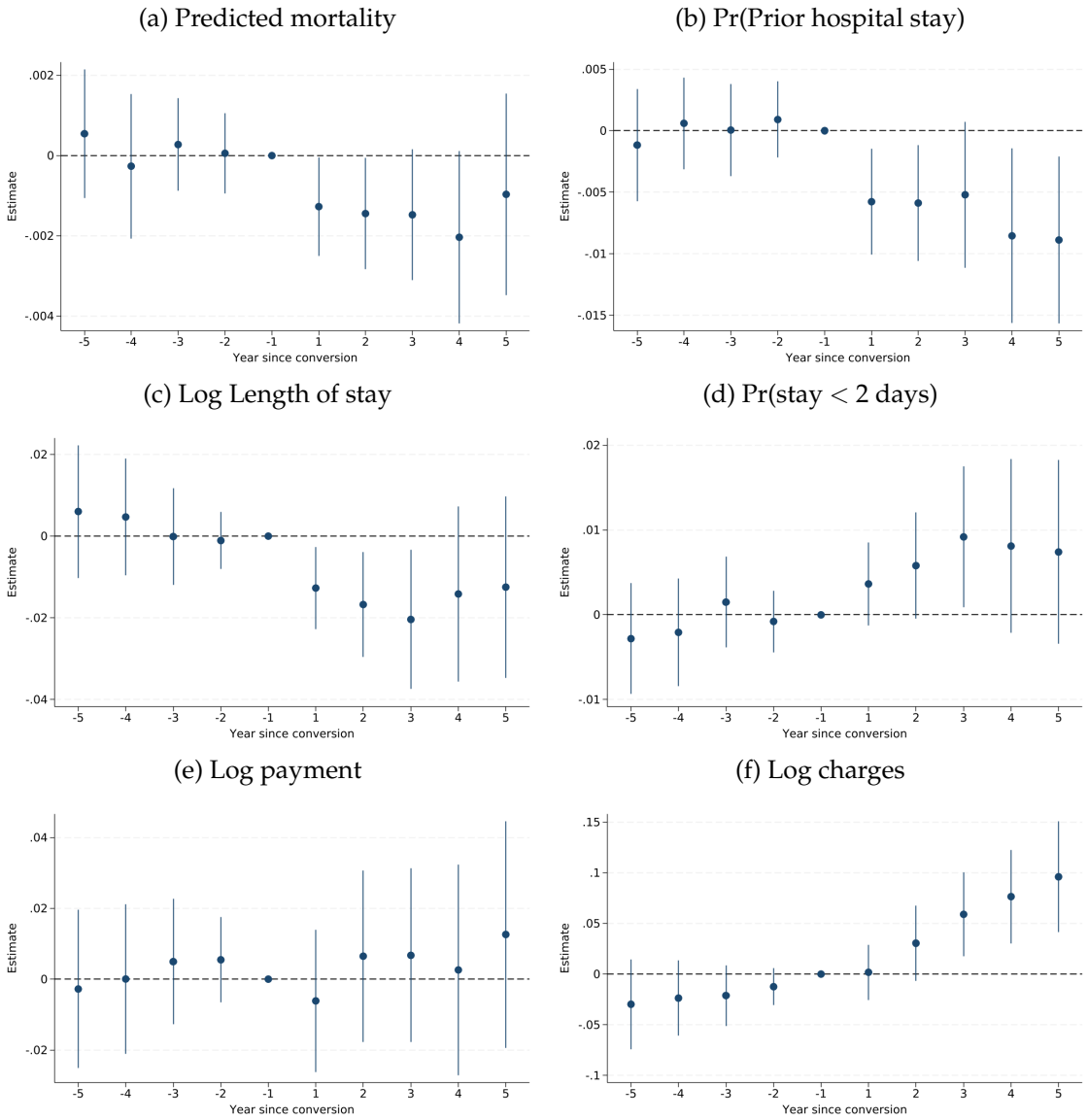


Figure 5: Effects on Medicare fee-for-service patients

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 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. 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.

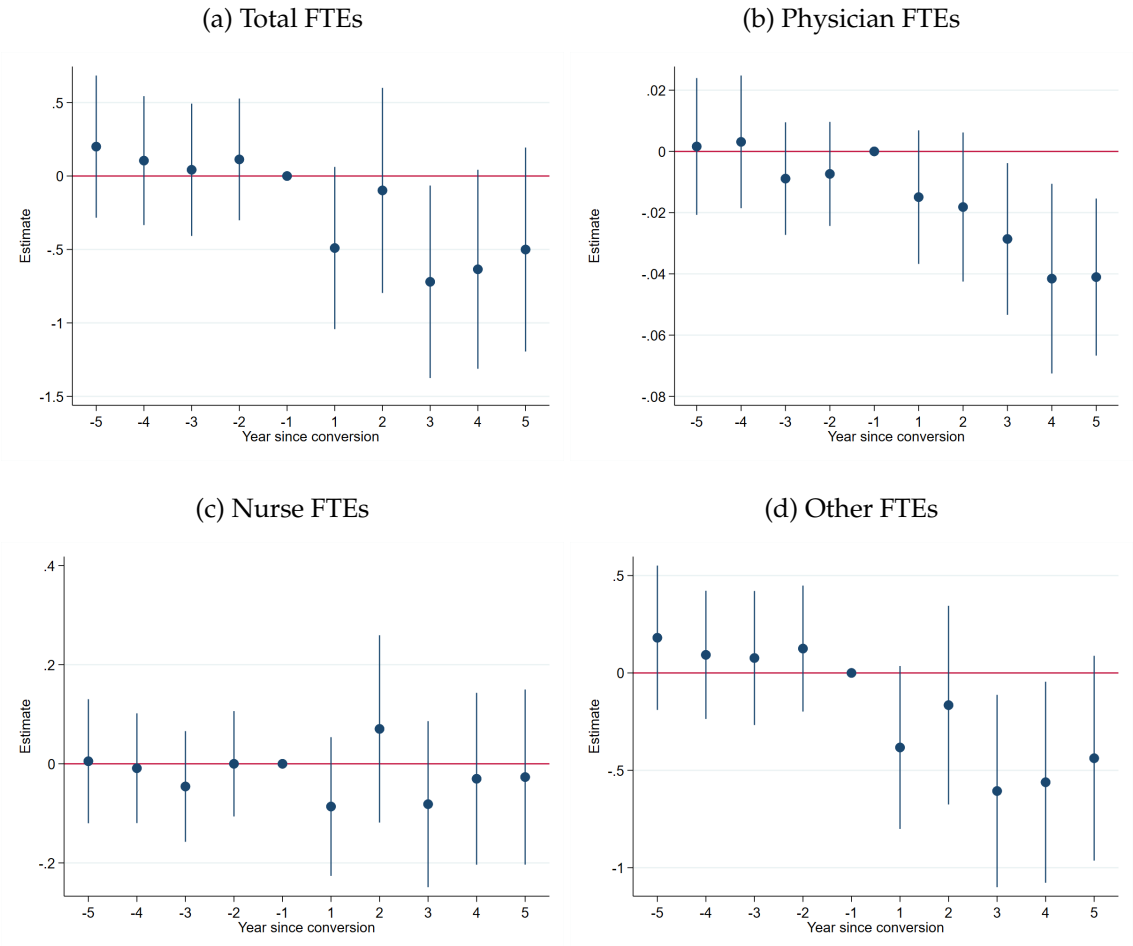


Figure 6: Effects on staff (per 100 adjusted admissions)

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 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.9 presents the corresponding plots obtained when we normalize staff FTE by 100 beds instead.

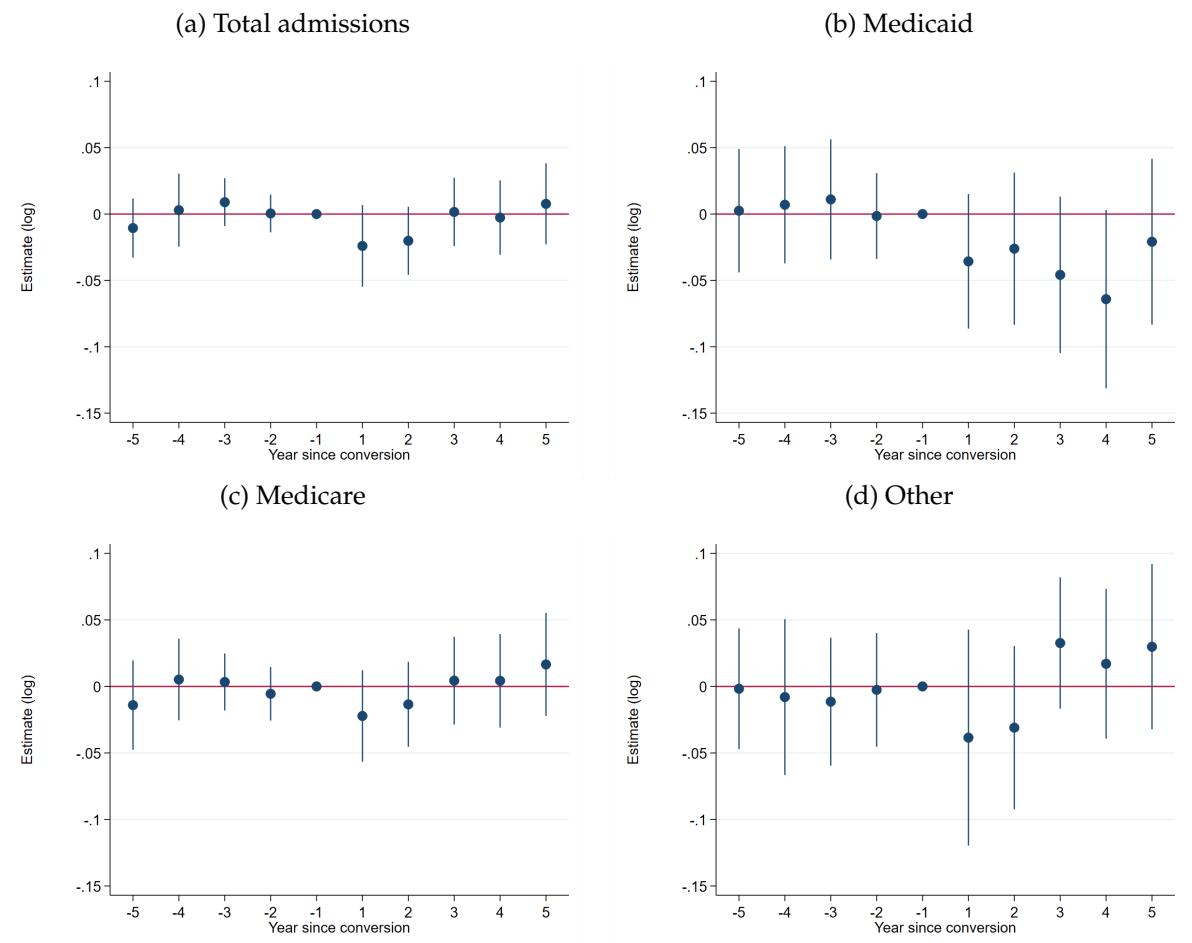


Figure 7: Effects on market-level (log) volume

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), as 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 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.

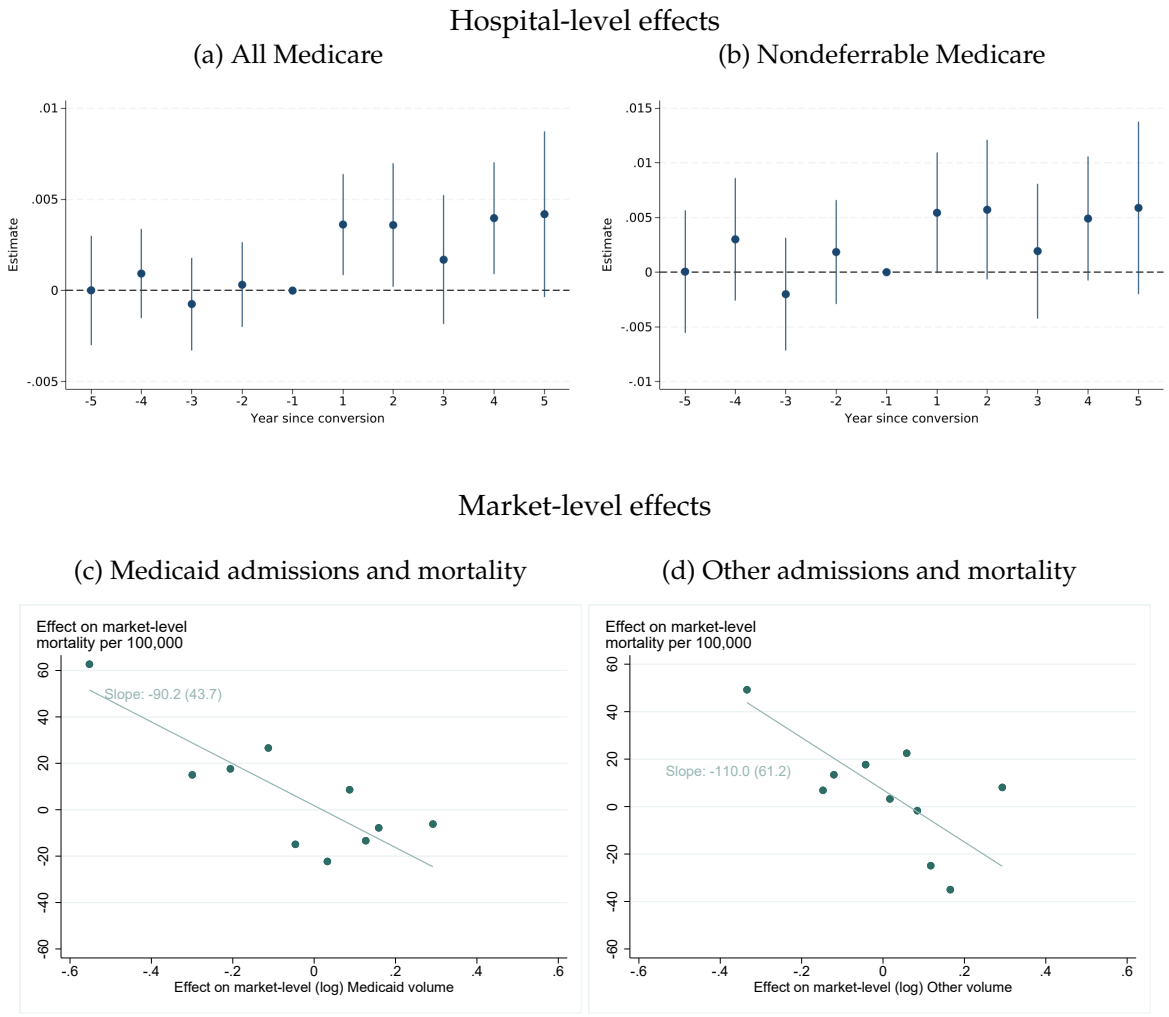


Figure 8: Effects on mortality

Note: 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 more after discharging from the hospital. Panel A presents the effects for all 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 (Doyle Jr et al. 2015). These results are obtained by estimating equation 3 on patient-level Medicare claims data. 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 coefficients on mortality and 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 ownership type for select states in 2019

	(1) AL	(2) CA	(3) TX	(4) GA	(5) IL	(6) PA	(7) US Overall
Public (non-federal)	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 ownership type for select states using American Hospital Association survey data from 2019. 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
% non-profit	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 refer to hospital admissions not covered by Medicaid or Medicare and mostly comprise privately insured and uninsured patients. 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, 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: State obstetrics 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 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 by payer 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: private insured, uninsured, and miscellaneous (e.g., workers compensation), respectively. Panel C column 1 presents the total effect on obstetrics 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 AHA data. 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 acquisition. The sample is limited to Medicare FFS patients 65 years or older and enrolled in Parts A and B for min. 3 months at admission. Panel A presents results from a specification that includes patient covariates. The model in panel B also includes market 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 comprise physicians, nurses, and others, presented in columns 2, 3, and 4, respectively. We normalize FTEs for the staff 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 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.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. The model in row IB includes state-year fixed effects and time-varying market controls. The model in 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. We average the five estimated dynamic effects and calculated 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 drops treated hospitals privatized after 2014 to ensure that we observe each privatized hospital for five years 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 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: Descriptive statistics (market-level)

	(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)	961.4	998.6	990.4
	(206.6)	(229.5)	(225.1)
All-cause mortality (ages 55–64)	1165.6	1110.9	1122.9
	(263.0)	(268.9)	(268.5)
# HSAs	204	725	929

Notes: The table presents descriptive statistics for the market-level sample, where markets are defined by Health Service Areas (HSAs). We use values from 1999 for most HSAs. In rare instances in which 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. Mortality is 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	(4) 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	1,032.6	1,028.3	1,032.6	

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 more. The outcome is 30-day mortality for hospitalized patients. The coefficients are obtained by estimating equation 2 on patient-level data. 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 other counties in the HSA, respectively. We estimate a triple difference specification (including an interaction term with an indicator for treated counties located in markets with above-median poverty) and report the double and triple difference coefficients in column 4. All regressions in panel B involve the same control group of HSAs that never experienced a privatization. Mean values are computed for privatized hospitals, affected markets, or counties in the year before treatment.

A Additional figures and tables

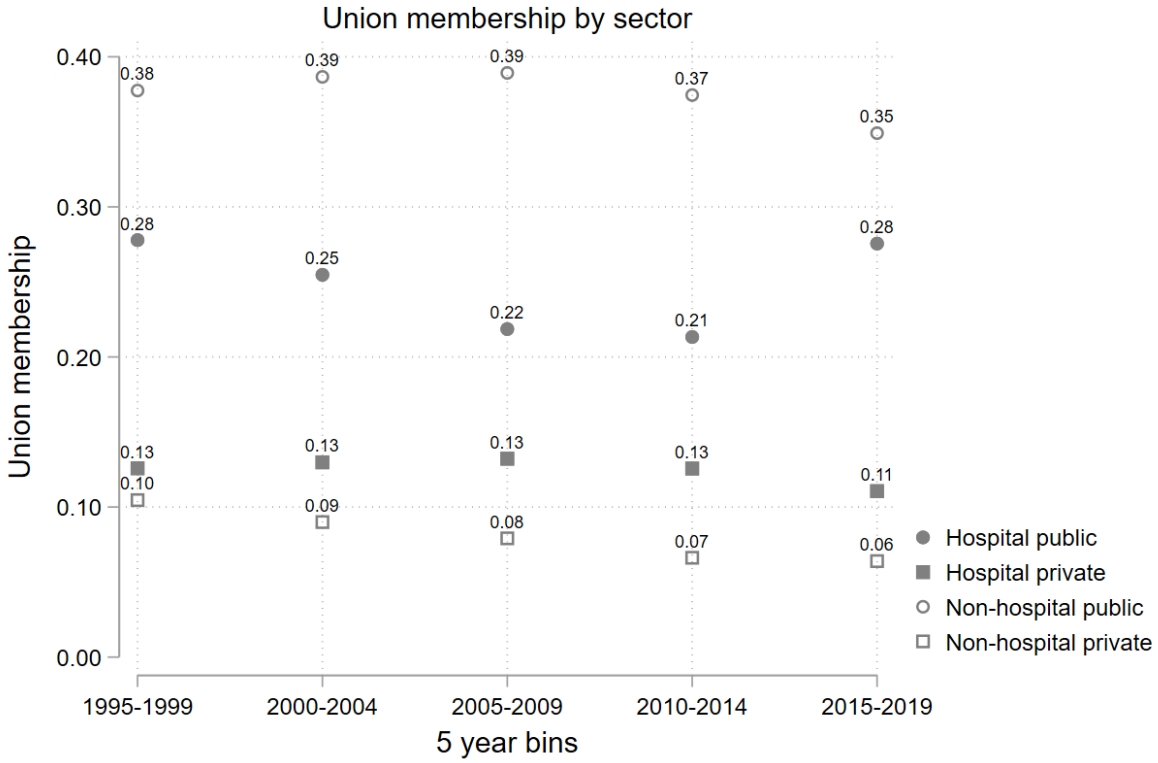


Figure A.1: Unionization among public and private employees

Note: 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 were sourced from the Annual Social and Economics Supplement of the Current Population Survey (ASC) over 1995–2019. The ASC includes individuals who are surveyed in the "earner study", which includes approximately one quarter of the CPS sample. We restricted 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 identified the hospital industry using the NAICS1990 code 831 and the NAICS1997 code 622.

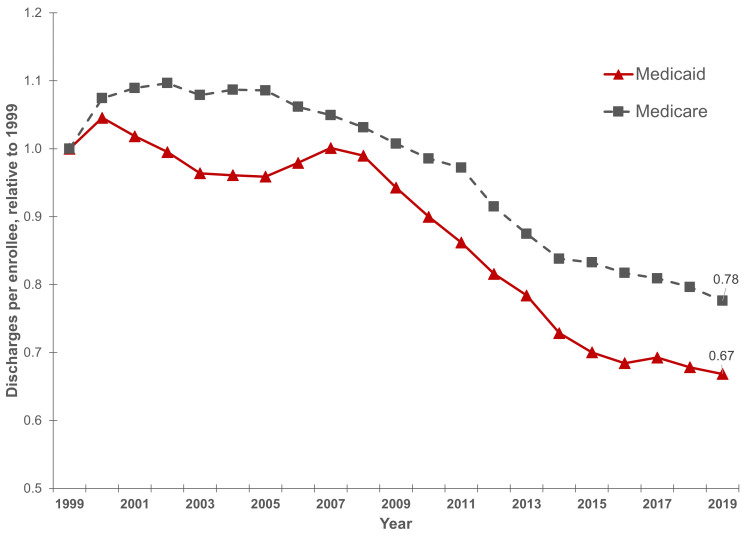


Figure A.2: Hospital utilization by Medicaid and Medicare beneficiaries

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

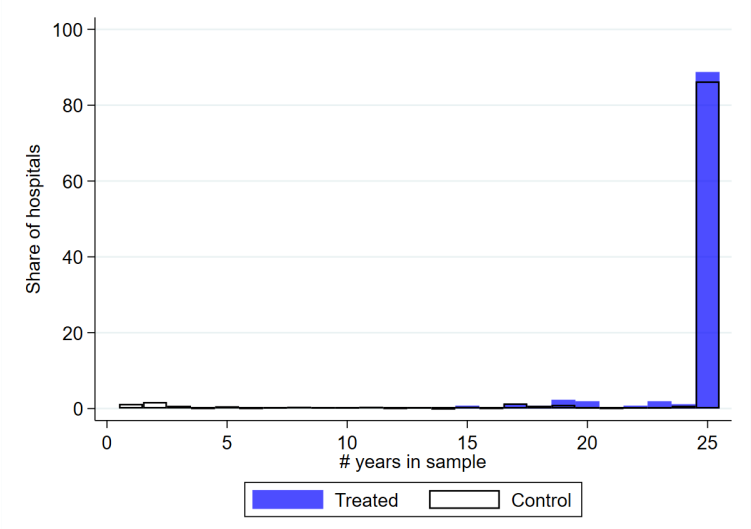


Figure A.3: Balance of hospital panel

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).

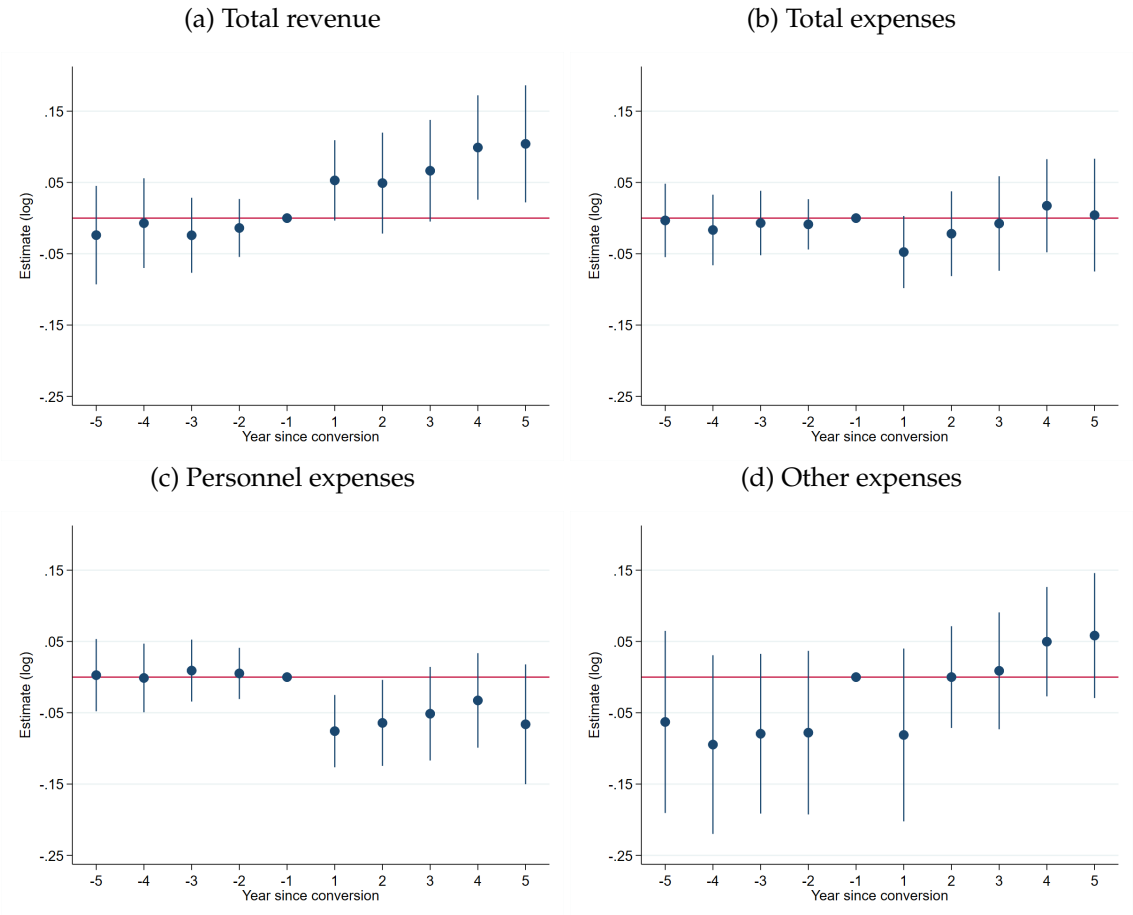


Figure A.4: Effects on log finances (per bed)

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. The 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.

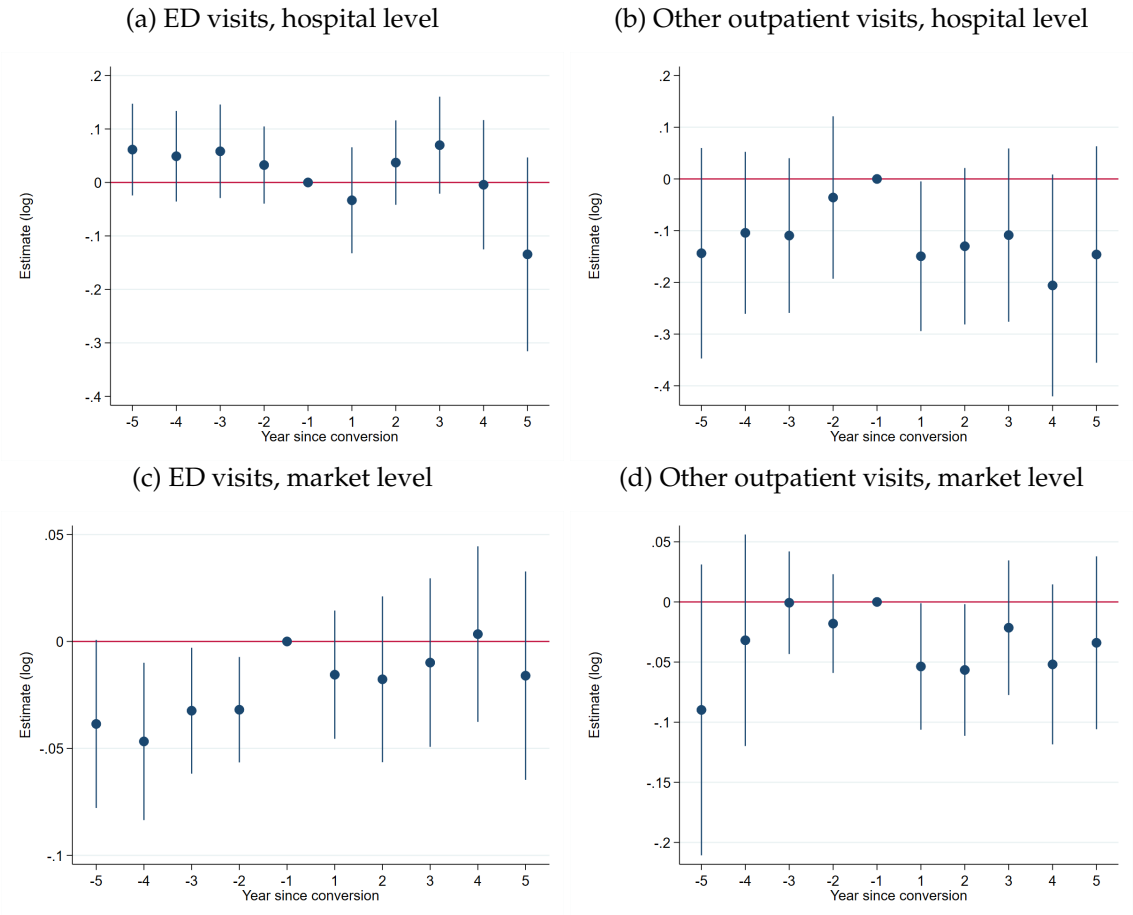


Figure A.5: Effects on ED and other outpatient visits

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 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).

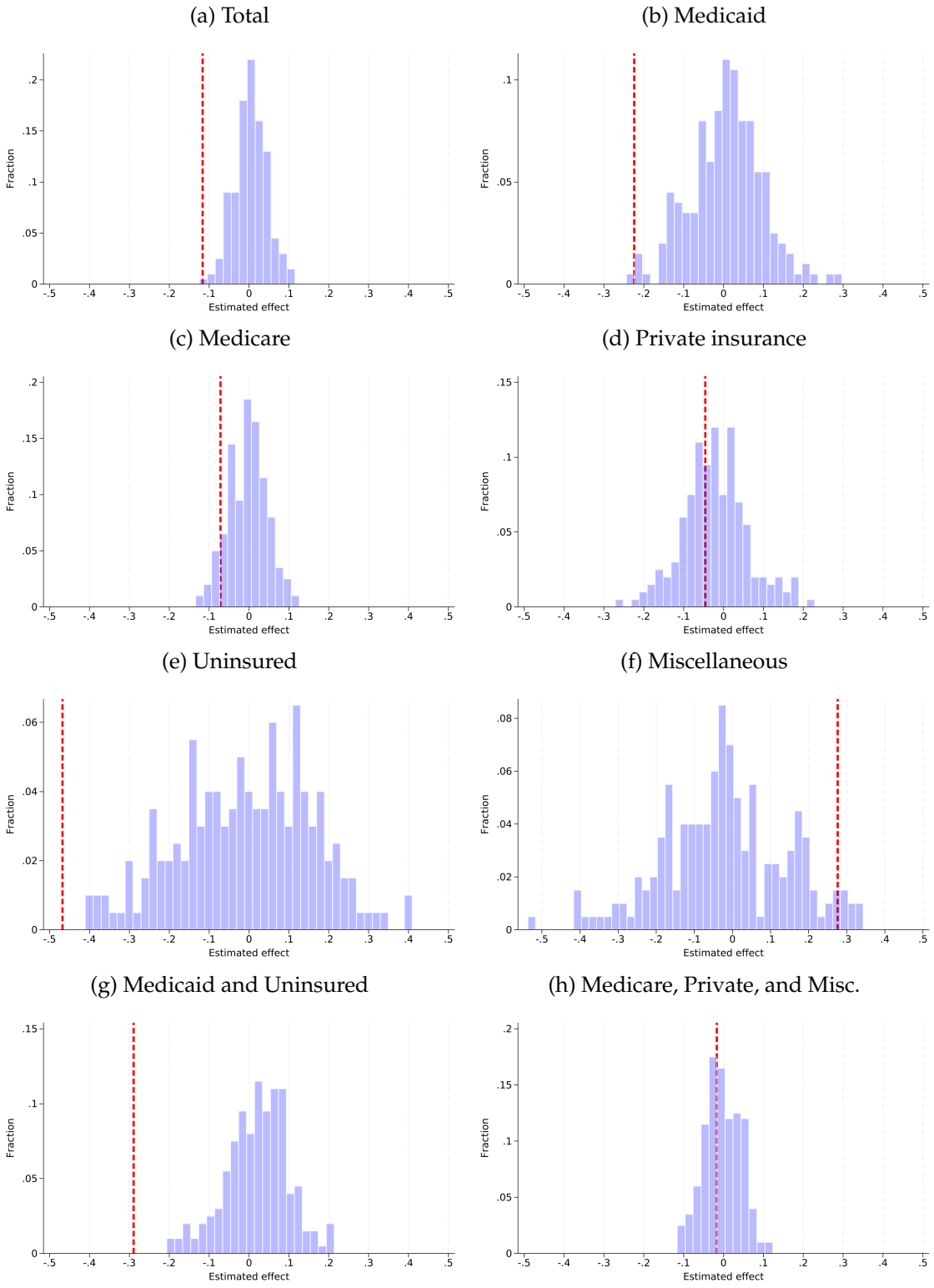
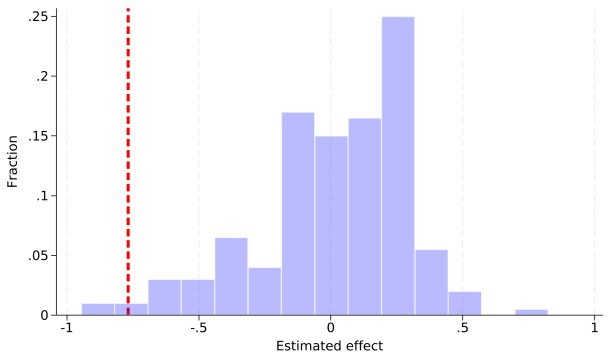


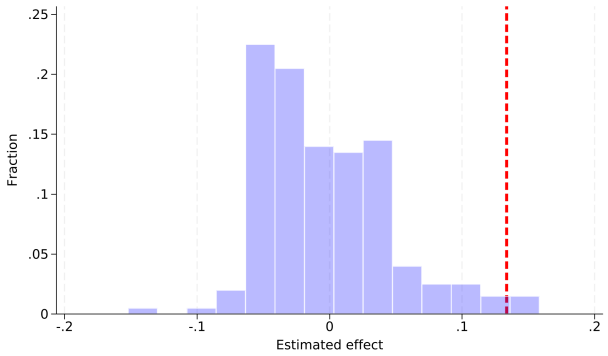
Figure A.6: Effects on admissions by payer using state data

Note: The figure presents distributions of estimated placebo effects on inpatient volume by payer using data from California, Florida, Indiana, Minnesota, and Washington. We obtained 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.

(a) Obstetric admissions



(b) Obstetric closure



(c) Obstetric adm. excluding closure

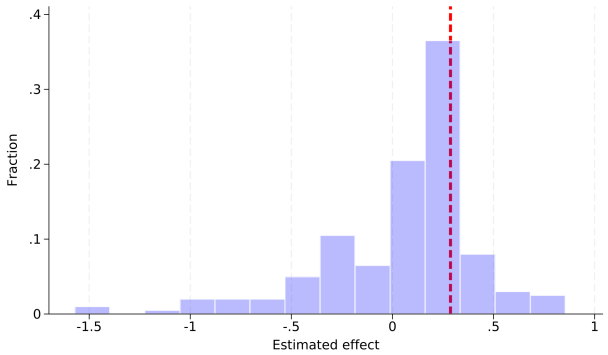


Figure A.7: Effects on obstetric admissions using state data

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 obtained 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.

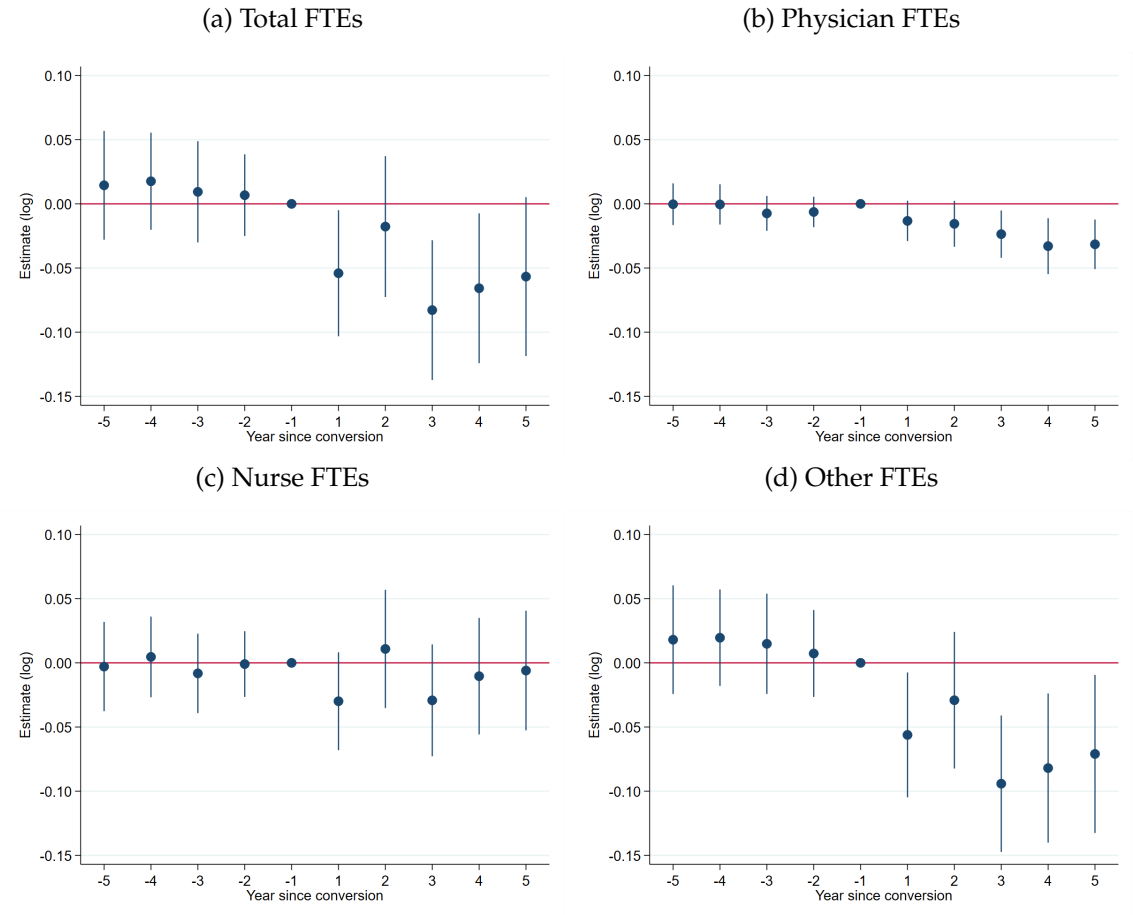


Figure A.8: Effects on staffing (log FTE per 100 admissions)

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 FTEs in panels (a), (b), (c), and (d), respectively. All outcomes are expressed per 100 contemporaneous adjusted admissions and presented in logs. 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.

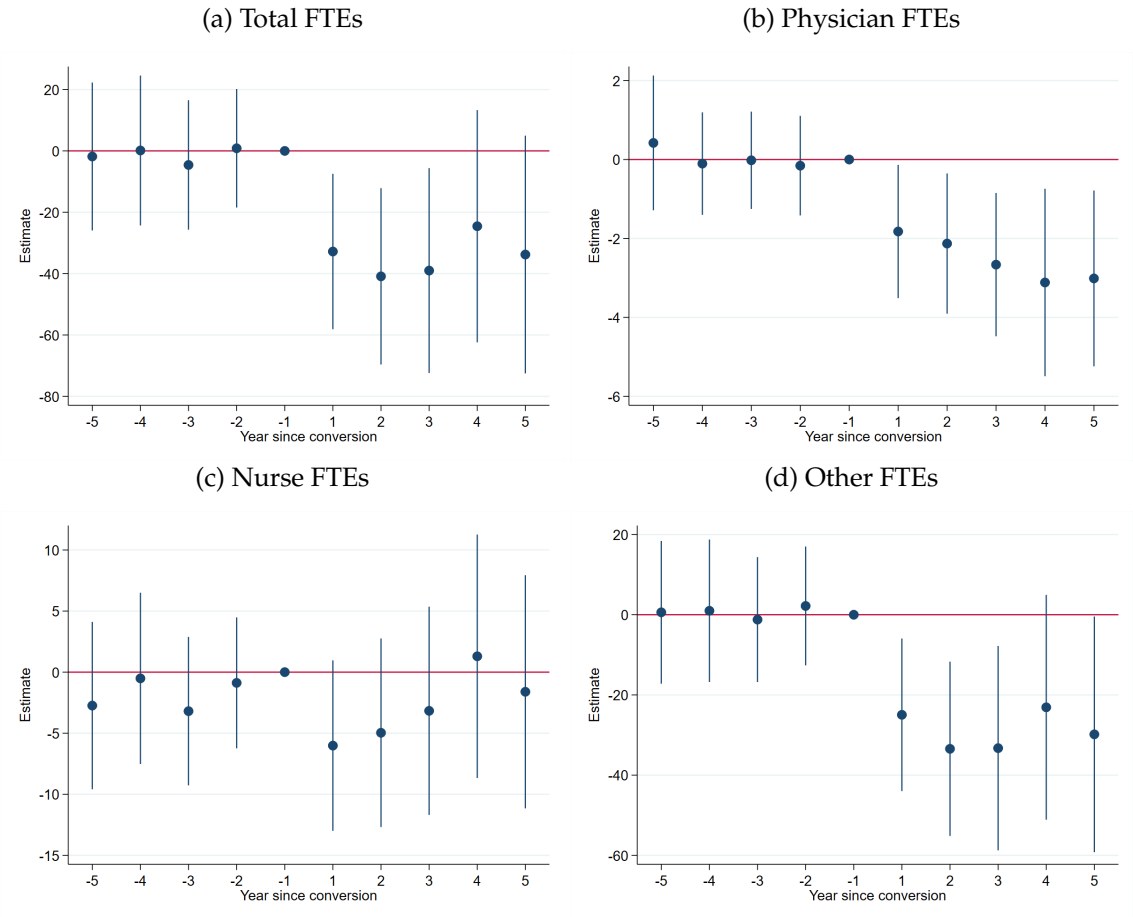


Figure A.9: Effects on staffing (FTE per 100 beds)

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 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.

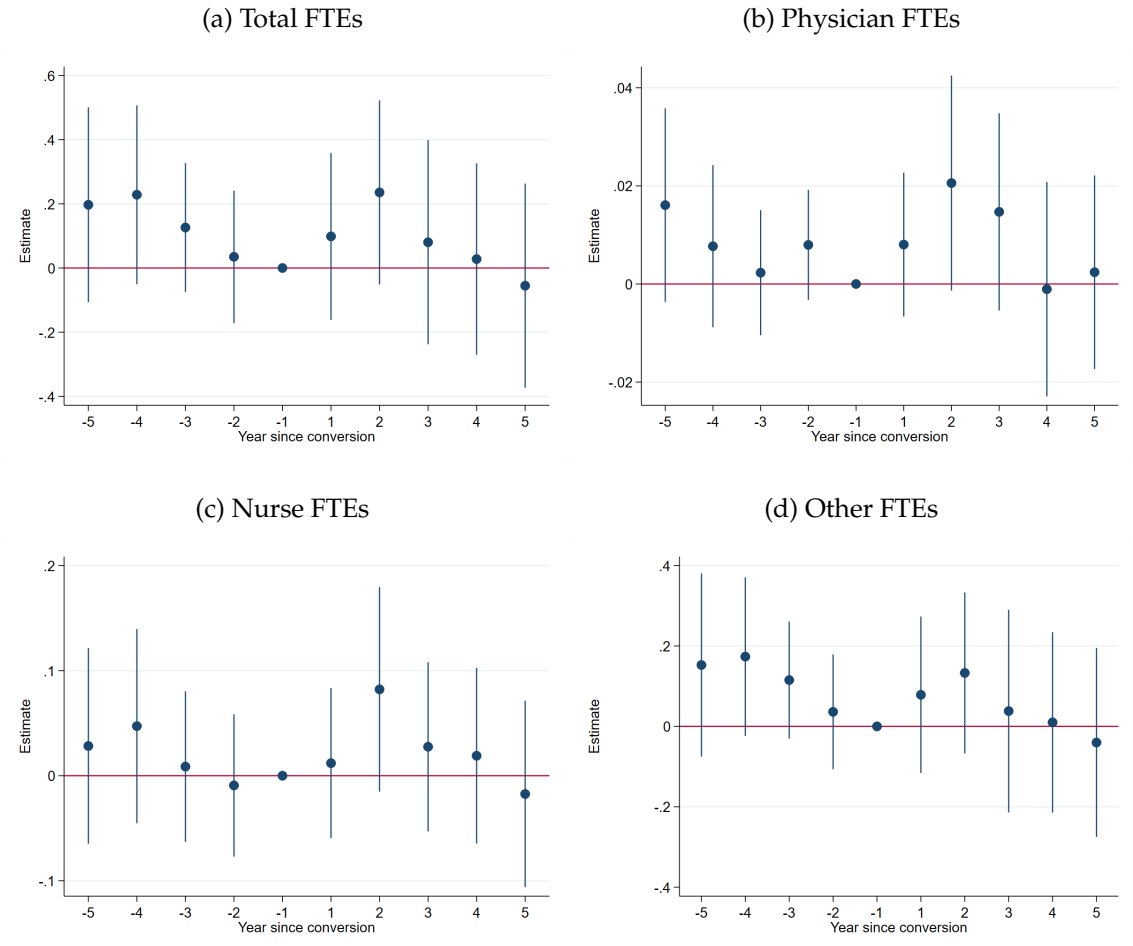
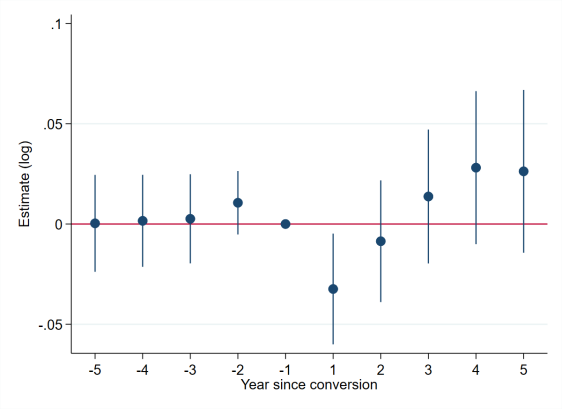


Figure A.10: Effects on market staff (per 100 adjusted admissions)

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. 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.

(a) Total admissions



(b) Medicaid

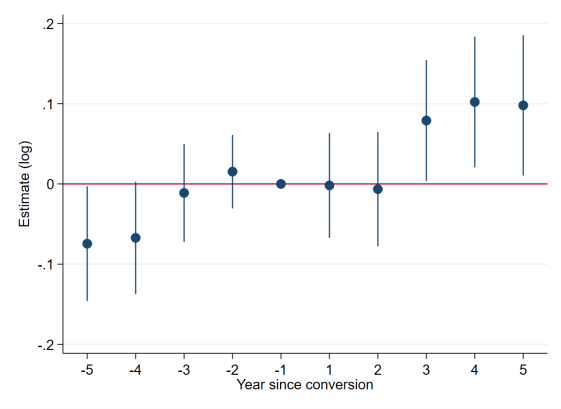


Figure A.11: Effects on patient (log) volume, independent hospitals acquired by systems

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. For this analysis, the treatment group is comprised of independent hospitals that are first acquired by a system sometime between 2000 and 2018. The control group consists of hospitals that remain independent throughout our sample period. The outcomes are log total and Medicaid admissions in panels (a) and (b), respectively. 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.

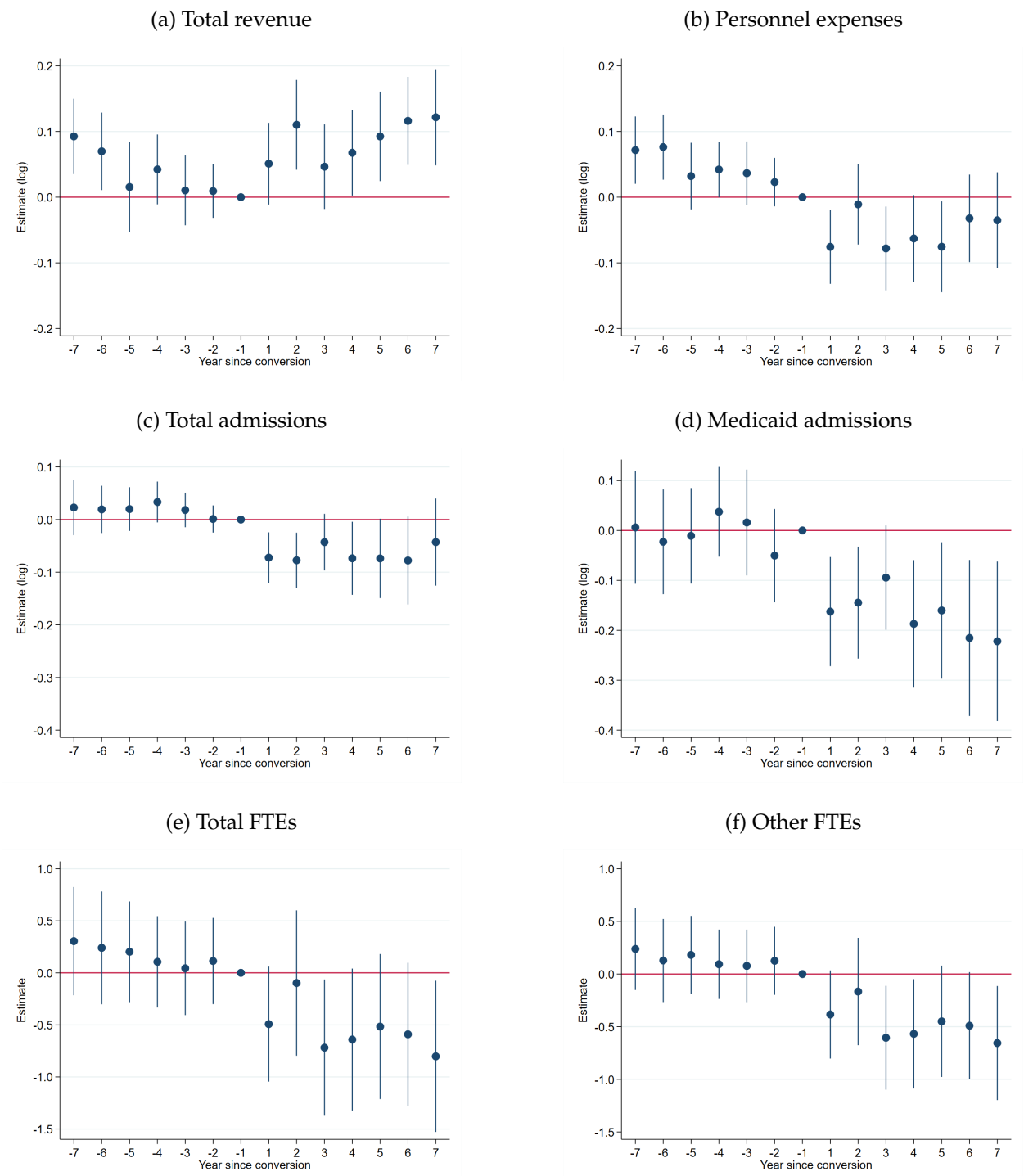


Figure A.12: Effects on selected hospital-level outcomes, expanded event time horizon

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. For treated hospitals we estimate coefficients for event times -7 to 7. We drop year zero and event times that fall outside of -7 to 7. The control group is comprised of hospitals that remain public throughout our sample period. The outcomes are log total revenue, log personnel expenses, log total admissions, log Medicaid admissions, total FTEs, and other FTEs in panels (a), (b), (c), (d), (e), and (f), respectively. As in the main analyses, finance and staffing variables are normalized by adjusted admissions. 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.

Table A.1: Public (nonfederal) hospital share of beds by state in 2019

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

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.

Table A.2: Characteristics of privatizations

A. Prior control	(1) State	(2) County	(3) City	(4) City-county	(5) Hospital district or authority	(6) Total
N of privatizations	14	84	33	10	117	258

B: Types of deals	(1) Non-profit	(2) For-profit	(3) Total
B1. Less control	119	25	144
- Contract Management	70	10	80
- Miscellaneous	49	15	64
B2. 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 privatizations in our main analysis sample, including the hospital's control type prior to privatization (panel A) and the types of privatization deals (panel B). These privatizations occur between 2000-2018. In panel A, we report the most common control value among the five years prior to privatization. Panel B, columns 1 and 2 present the number of hospitals that converted to private non-profit and for-profit, respectively. Panel B1 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 B2 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
% Non-profit	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 corresponding values for both sets of hospitals. "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 use 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 xx panel xx. 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. The 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	0.018	-0.016
	(0.032)	(0.063)	(0.016)	(0.028)
Obs	20,998	20,998	19,404	19,404
B: Market controls				
DD	-0.043	-0.067	0.016	-0.010
	(0.032)	(0.063)	(0.016)	(0.028)
Obs	19,385	19,385	17,983	17,983
Mean outcome (t-1)	15,424	53,766	136,340	455,672

Notes: The table presents estimated effects on the log emergency department (ED) and non-ED 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 prior to privatization. Standard errors are clustered by hospital or market, depending on the level of treatment.

Table A.7: Changes in payer mix and mean reimbursement

Payer	(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	same
2. Medicaid	9,269	0.20	0.19	-0.138	0.19	same
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

Notes: The table presents results on the effects of changing payer mix on mean reimbursement rates. It shows how we combine effects estimated using the AHA and states samples to predict the effect of cream-skimming on mean reimbursement per stay. Column 1 presents data on mean reimbursement rates for hospital inpatient stays obtained from the Medical Expenditure Panel Survey (MEPS) waves of 2000, 2005, 2010, 2015, and 2019. The mean values are expressed in 2019 dollars pooling data across the five survey waves. 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, which necessitates the use of data from a subgroup of states. Column 3 is equivalent to column 2 but uses data from the 5 states (CA, FL, IN, MN, and WA). These data, reported in Panel C of Table 4, allow us to observe 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 used 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). Therefore, 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.

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

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

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 comprise 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 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.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	0.16
	(12.7)	(0.8)	(3.2)	(9.5)	(1.35)
Obs	20,998				8,632
	.				
B: Market controls					
DD	-33.3	-2.6	-1.9	-29.1	0.12
	(12.7)	(0.8)	(3.2)	(9.5)	(1.35)
Obs	19,385				8,628
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 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 prior to 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 XX (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 (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
C: Heterogeneity by union membership				
DD	0.16	0.01	0.02	0.13
	(0.15)	(0.01)	(0.04)	(0.11)
x 1(< med. union membership)	-0.29	-0.01	0.00	-0.28
	(0.17)	(0.01)	(0.05)	(0.13)
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 FTE, 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 adjusted admissions. Adjusted admissions are admissions scaled by the ratio of outpatient to inpatient revenue. 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 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. Panel C presents the corresponding results from a triple difference specification including an interaction term with an indicator for markets located in states with rates of union membership (among private service workers) in 1999 less than the median among states with treated markets. The mean values pertain to the outcomes in the treated markets in the year prior to 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	5.21
	(0.014)	(0.024)	(0.016)	(0.022)	(0.09)	(0.01)	(0.03)	(0.07)	(6.58)
I: Specification checks									
A. Weighted by beds	0.025	-0.009	0.030	0.048	-0.08	-0.01	-0.01	-0.05	10.69
	(0.010)	(0.017)	(0.012)	(0.014)	(0.07)	(0.01)	(0.03)	(0.05)	(7.11)
B. State-year FEs	-0.012	-0.033	0.001	-0.015	0.08	-0.00	0.05	0.03	2.38
	(0.015)	(0.025)	(0.016)	(0.023)	(0.11)	(0.01)	(0.03)	(0.08)	(6.53)
C. Incl. pre-trend	-0.023	-0.071	-0.010	-0.004	0.01	0.00	0.04	-0.02	4.31
	(0.014)	(0.024)	(0.015)	(0.023)	(0.10)	(0.01)	(0.03)	(0.07)	(7.31)
II: Alternate estimators									
A. CS estimator	-0.000	-0.033	0.006	0.017	0.09	0.01	0.02	0.05	-6.23
	(0.012)	(0.025)	(0.016)	(0.022)	(0.12)	(0.01)	(0.03)	(0.09)	(9.58)
B. DCDH estimator	-0.006	-0.037	-0.004	0.014	0.13	0.01	0.03	0.08	-6.24
	(0.011)	(0.021)	(0.014)	(0.019)	(0.12)	(0.01)	(0.03)	(0.09)	(8.31)
C. Synthetic DiD	0.007	-0.028	0.019	0.042	-0.11	0.00	-0.01	-0.09	6.26
	(0.017)	(0.028)	(0.021)	(0.022)	(0.14)	(0.01)	(0.04)	(0.11)	(8.49)
III: Alternate samples - treated group									
A. Balanced panel	0.005	-0.037	0.017	0.022	0.07	0.00	0.04	0.03	4.49
	(0.015)	(0.026)	(0.017)	(0.021)	(0.10)	(0.01)	(0.03)	(0.07)	(7.30)
B. All treated obs	0.021	-0.031	0.031	0.044	-0.10	-0.00	-0.00	-0.09	5.42
	(0.022)	(0.031)	(0.023)	(0.025)	(0.11)	(0.01)	(0.03)	(0.09)	(7.05)
IV: Alternate samples - comparison group									
A. Matched sample	-0.016	-0.026	-0.006	-0.013	0.04	0.00	0.03	0.00	4.01
	(0.017)	(0.028)	(0.019)	(0.026)	(0.11)	(0.01)	(0.03)	(0.09)	(8.15)

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. 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. In row IC, we include a linear trend interacted with an indicator for treated markets. Row IIIA drops markets treated after 2014 to ensure that we observe each treated market for 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 xx. The model in row 2 of each panel also includes time-varying market covariates to control for differences in economic factors between markets. 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 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 ($)	-335	Table 3
B. Benefits:		
1. Revenue		
Increase in revenue/patient	5.70%	Table 3
Increase in revenue	3,051,992	7025x(1-6%) patients x 8109 x 5.7% per patient
2. Cost		
Reduction in cost/patient	3.30%	Table 3
Reduction in cost	1,839,939	7025x(1-6%) patients x 8444 x 3.3% per patient
Total increase in surplus	4,891,932	
Reduction in deficit	2,353,375	Lower bound; 7025x335
3. Additional taxes		
Hospital tax rate	4.50%	Rosenbaum et al. 2015 ex. 4
Incremental tax (FP)	2,546,807	7025x(1-6%) patients x (8109 x 1.057) per patient x 4.5%
Share of FP in deals	28%	Table A2
Expected tax	720,608	incremental tax x FP share
share to federal govt.	384,324	federal govt. share = 0.024/0.045
Total incremental funds	5,612,540	
C. Deaths		
4. Hospital mortality		
Medicare FFS patients	1136	Table A5; 65+ only
Volume reduction	4.90%	Table 4 panel A
Mortality effect	0.33%	Table 5
Incremental deaths	3.57	
5. Market-level mortality		
Mean population	42160	mean 55-64 popn. in treated markets
Mortality estimate	5.2	Table 9 panel A
Mortality effect/privatization	4	linearly scale 5.2 by 1.3 deals/mkt
Incremental deaths	1.69	4 * 42160 / 100,000
Total incremental deaths	5.25	
D. Revenue per death ($mn)		
Lower bound:	0.51	(deficit+non-federal tax)/total deaths
Central estimate:	1.07	total funds/total deaths
Upper bound:	1.57	total funds/medicare deaths

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, etc. for the privatized hospitals before privatization. Panel B describes how we estimate the incremental surplus and tax revenue generated from the average privatization. Panel C presents the additional deaths due to the average privatization, both among Medicare patients at the hospital and among 55-64 year old individuals in the community. 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 utilized the AHA annual survey files over this period. We infer a conversion when we observe a change in management control type from public (state, county, or city) to private (for-profit or nonprofit). We validate conversions using information recorded in the annual AHA summary of changes files, which explain each change in the AHA survey from the previous year. This naive approach yielded 354 public to private conversions. Next, we devoted hundreds of hours to manually verify each conversion by combing through hospital websites, news articles, and third-party sites such as the American Hospital Directory. Manual validation help us identify non-trivial numbers of false positive and false negative conversions. Our final number of conversions is 258.

Through these detailed reviews we classified privatizations into five groups, described below. We consider the first two as transitions where 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 across 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 since 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 by a hospital district.[28] Second, we exclude specialized hospitals such as psychiatric and rehabilitation facilities. In addition to being highly specialized, these hospitals are often reimbursed in a distinct way from community hospitals. Therefore, our final sample contains nonfederal, general acute care hospitals.

Identifying privatizations - We create our list of public to private conversions by starting with conversions implied by changes in the AHA's control variable and then manually validating each conversion. From this process we identify 269 total conversions. From our manual validation, we find that two treated hospitals experience more than two conversions (i.e., public to private or private to public) over our sample period; we drop these hospitals. Five hospitals are dropped that convert from private to public and back to private within our sample period. Finally, we drop four treated hospitals whose most common AHA primary service code was not "general medical and surgical." Our final set of treated hospitals consists of 258 public to private conversions. We note that two treated hospitals experience a second conversion in which they convert back from private to public. For these two hospitals we drop observations on or after the second conversion.

Comparison hospitals - We start with American Hospital Association (AHA) survey data for the years 1995 to 2019. In the raw data there are ∼6,200 hospitals per year and ∼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

28. Hospital districts are funded by taxpayers to own and operate public hospitals. These are mostly found in rural markets. They are typically comparable to a county in terms of size.

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

The final AHA analysis sample contains 802 comparison hospitals.

B.3 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 the 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), Elixhauser risk flags based on the 90-day history of hospital inpatient and outpatient care, the flags for utilization history of different types of care (hospital stay in the past 30 days, past 90 days, nondeferrable 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, 0.109, 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 we test for changes in treatment intensity, billing, and mortality.

B.4 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.[29]. We further merge in population estimates from CDC Wonder for Hawaii for 1995–1999. 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.5 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

29. 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.

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-period 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.