The Impact Of Privatization: Evidence From The Hospital Sector*

Mark Duggan, Stanford University and NBER
Atul Gupta, University of Pennsylvania and NBER
Emilie Jackson, Michigan State University
Zachary Templeton, University of Pennsylvania

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Abstract

Government ownership in the U.S. hospital sector, which accounts for 5.3% of U.S. GDP, has steadily declined for decades. A key driver has been the privatization of hospitals owned by local governments. Theory predicts that privatization will improve hospital profitability, but may be socially inefficient. We test these predictions empirically by leveraging all 254 privatizations that occurred between 2001 and 2018. Privatization increases hospital profitability, eliminating the need for subsidies. However, we also find a reduction in access for Medicaid patients and an increase in mortality among elderly Medicare patients. On average, privatization generates $0.6 million in savings per additional death.

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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). Debates on the efficiency of government spending in the U.S. often highlight privatization as a potential solution (Simon 2012; Durkee 2024). It 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 suggests that privatization improves the efficiency and growth of government-owned firms (World Bank 1995; Dewenter and Malatesta 2001). However, its effects on consumers have been understudied (Megginson and Netter 2001; Galiani, Gertler, and Schargrodsky 2005). This is a key limitation, since the privatization debate now centers on the delivery of social services, traditionally managed by governments (Stiglitz 2005).

Economists have long recognized the benefits of privatization. Government enterprises often struggle with misaligned employee incentives, soft budget constraints, and political interference (Shleifer and Vishny 1994; Sheshinski and López-Calva 2003). Private management can address these agency problems and improve profitability and growth, which may also benefit consumers, especially in industries with sufficient competition and minimal market failures (Vickers and Yarrow 1991). However, in markets with imperfections, government enterprises might better serve consumer welfare by setting prices or quantities that reflect social marginal benefits (La Porta and López-de-Silanes 1999).

Hart, Shleifer, and Vishny (1997) formalize this intuition using a stylized economic model. The model considers the optimal responses of a manager employed by the government versus when she is a private contractor. The model predicts that private contractors will reduce costs and therefore improve financial efficiency. However, it also cautions that if the government's contract is incomplete, private managers are incentivized to cut costs on noncontractible or nonenforceable tasks, which may be socially inefficient. These insights are very relevant to the hospital setting, as the average hospital is a highly complex organization, and it is inconceivable for the government to specify in their contract the level of care inputs and desired treatments in detail.

Thus, this suggests a key trade-off in the decision to privatize hospitals. On the one hand, theory unambiguously predicts that privatization will improve the profitability of government hospitals. This is a significant temptation, as public hospitals typically lose money and their operations are sustained by taxpayer subsidies. Total costs at hospitals owned by local governments exceeded revenue by $17 billion in 2019, according to the Census Bureau's survey of state and local government finances. This amount represents 35% of the spending of local governments on housing and community development, 54% of the spending on jails and other correctional facilities and 69% of the spending on their legal and judicial systems. Improved financial efficiency could, therefore, help free up funds for other priorities. The magnitude of the increase in profitability, the underlying mechanisms through which this is achieved, and whether it is sufficient to eliminate subsidies on average are empirical questions.

On the other hand, theory's prediction of socially inefficient cost cutting in the context of the hospital sector leads to two potential concerns. First, private management may shift the hospital's focus toward more lucrative patients, which may reduce access to care for unprofitable or less profitable patients who are typically drawn from the most vulnerable segments of society. This concern has long been voiced by previous studies (Shleifer 1998), and is consistent with cross-sectional data showing that government hospitals are more likely to offer unprofitable services than their private counterparts (Horwitz 2005; Horwitz and Nichols 2022). Second, private management may excessively reduce costly care inputs, such as staff, which may inadvertently reduce the quality of care.

Despite the absence of research to help policymakers assess this trade-off, local governments throughout much of the U.S. 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 (as a share of total capacity) from 1983 to 2019. Even so, in 2019, there were more than 900 state and local government hospitals and more government employees worked in hospitals than in any other sector except education.[1] These statistics foreshadow more privatization in the foreseeable future. For example, the state of Connecticut is currently investigating the potential for privatization to reduce subsidies for the only state-owned hospital there, prompting strident criticism due to concerns about the impact on low-income patients and employees (Cummings 2024; Phaneuf 2024). The continuation of the hospital privatization trend described above could greatly affect the performance of this vital sector of the economy. Hospital care is the largest segment of the U.S. healthcare industry and accounts for $1.4 trillion in spending, approximately 50% of which is tax-funded. It employs more than 7.1 million people, comparable in size to the entire construction sector.[2] But this debate is not limited to the U.S. In several countries, including Germany and Sweden, there are ongoing discussions or actions to privatize healthcare providers, sparking considerable controversy (Dahlgren 2014; Heimeshoff, Schreyögg, and Tiemann 2014; Knutsson and Tyrefors 2022).

This paper empirically examines the trade-offs in privatization by leveraging all 254 privatizations of nonfederal government hospitals that occurred between 2001 and 2018. We identify privatizations by manually validating changes in managerial control 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, confidential national vital statistics microdata, and AHA annual surveys. We complement these sources with publicly available files from Medicare cost reports and the U.S. Census Bureau.

We employ a staggered difference-in-differences research design to estimate the effects of privatization on the treated hospital and on the market where the hospital is located. This follows the approach used by studies that examined privatization (Galiani, Gertler, and Schargrodsky 2005)

1. Source: Current Employment statistics from the Bureau of Labor Services (BLS).

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

as well as the organization of healthcare markets (Cooper et al. 2019; 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 spillovers at the market level, we compare trends for markets that experience one or more hospital privatization(s) with those of markets with no privatization throughout the sample period.

Although our research design is standard in this literature, we recognize that privatizations are not randomly assigned. As we show, privatized hospitals differ from other government hospitals at baseline. This is not a violation of the parallel trends assumption, but is important to consider when interpreting our results. We take a number of precautions to probe the validity of our estimation strategy in addition to examining dynamic effects around the year of privatization. We perform a large number of robustness checks, including controlling for differences in local economic activity, relaxing key sample restrictions, using a matched subset of comparison hospitals that closely resemble the privatized hospitals at baseline, and several others. The estimates are qualitatively similar in all cases. Recent studies have demonstrated the potential for bias in two-way fixed effects estimators and event studies in staggered treatment designs (Goodman-Bacon 2021; Sun and Abraham 2021). To assess the importance of this concern, we present a full set of alternate average estimates and event study plots using the estimator proposed by Callaway and Sant'Anna (2020). This approach produces similar results and leads to the same conclusions.

Guided by theory, we first examine the effect of privatization on hospital profitability. In the year before privatization, treated hospitals were unprofitable with an average operating deficit of 3% of revenue. Public hospitals' profitability suffered compared to private hospitals, primarily due to a lower average reimbursement rate, even though they had lower personnel and total operating costs. We find that private control improves performance exactly on this dimension: the mean revenue per bed increases by an average of about 8% following privatization, which is sufficient to make a modest surplus. We also detect a substantial reduction in personnel spending. Overall, our baseline estimate implies that the average privatization generates $2 million in savings and tax revenue per year for the government.

However, we also find evidence that the improvement in hospital profitability carries social costs. Following privatization, hospitals disproportionately decrease admissions of low-income Medicaid and uninsured patients, from whom they receive much lower reimbursements than from other payers (Frakt 2011). In contrast, we detect small and statistically insignificant changes in stays for Medicare and privately insured patients, from whom hospitals receive more generous reimbursements.[3] When we examine changes in hospital admissions at the market level, we detect approximately a 4% net decrease in aggregate Medicaid admissions, which we interpret as a decrease in access to care. Markets that experience a greater decrease in Medicaid admissions also experience a greater increase in deaths among 55–64 aged individuals, a sign that this decrease in care may be socially inefficient. In addition, we find evidence of a reduction in quality of care at

3. This is consistent with prior work that has highlighted that providers are responsive to reimbursement rates (Clemens and Gottlieb 2014; Alexander and Schnell 2024).

the privatized hospital. Using the detailed Medicare claims data, we detect an approximately 3% average increase in 30-day mortality rates among Medicare FFS patients 65 years and older in the privatized hospital. This impact on FFS patients alone implies an increase of 3.4 deaths and 18.4 life-years lost (LYL) due to the average privatization per year. Even if we assume that the mortality effects are limited to elderly patients, this represents a conservative estimate since we cannot incorporate the effect on Medicare beneficiaries enrolled in private managed care plans, who are not observed in our data.

We then document several mechanisms that help to explain the main results discussed above. We show that the changes in payer mix may be driven by a shift away from less profitable services. As a case study of a service known to be less profitable, we show that privatized hospitals severely restrict obstetric services, mainly by closing maternity wards. Since a majority of obstetric patients in the sample are covered by Medicaid or are not insured, this change disproportionately affects these groups. Second, privatized hospitals differentially increase their list prices, also known as charges. These often form the basis of price negotiations with private insurers and affect vulnerable patients such as the uninsured. Finally, we show that privatized hospitals reduce care inputs for FFS patients. Leveraging the detailed Medicare claims, we find that patients are discharged sooner from the hospital. We also detect a decrease in full-time equivalent (FTE) staff that can fully explain the reduction in personnel spending mentioned previously. The patterns are consistent with a deliberate shift in staffing toward the mix of occupations found at private hospitals, which have lower availability of physicians, social workers, and counselors than in public hospitals. This finding is significant because studies have linked such changes in staff to elevated mortality (Friedrich and Hackmann 2021; Aghamolla et al. 2024).

This paper makes two contributions. To our knowledge, we are the first to obtain nationally representative estimates of the causal effects of hospital privatization in the U.S., adding to the broader privatization literature in economics.[4] In fact, we know of only a few relevant studies even outside economics, such as Ramamonjiarivelo et al. (2020). They study privatizations in an earlier period and document improved hospital profitability. However, they do not study operational changes, access to care, or impacts on health. Our results not only empirically document the effects on operations and care quality in detail but also enable us to concretely quantify the trade-off. Our estimates imply that the average privatization generates approximately \$0.6 million (2 mn/3.4) in savings for the government per additional death, or \$110,000 (2 mn/18.4) per LYL. This is our main estimate, and under different assumptions, we estimate an upper bound of approximately \$1.26 million per death or \$236,000 per LYL. These estimates hew close to our analysis and do not incorporate several potential factors of interest. A common benchmark estimate of the value of a life-year (VSLY), expressed in 2019 dollars, is approximately \$150,000 per LYL (Cutler 2004). The estimated savings from privatization may exceed this benchmark. However, the federal government uses a much higher standard. The U.S. Department of Health and Human

4. Although there is extensive work on deregulation in sectors such as airlines, telecommunications, and electricity, the evidence on privatization in the U.S. 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).

Services (HHS) stipulates a value of a statistical life (VSL) of approximately 10 million and a VSLY of $369,000 (HHS 2017; Kniesner and Viscusi 2019), which comfortably exceed our estimates.

A much larger complementary literature has studied the effects of ownership structure on firm performance at a point in time (rather than the effects of changes in ownership within a firm as in the current study).[5] Within healthcare, these studies have generally focused on the differences in objectives and performance between for-profit and nonprofit firms (Duggan 2000; Sloan et al. 2001; Malani, Philipson, and David 2003; Gaynor, Ho, and Town 2015). Knutsson and Tyrefors (2022) and Chan, Card, and Taylor (2023) quantify the difference in quality of care between government and private providers among ambulances and hospitals, respectively. Both studies leverage plausibly exogenous variation in patient assignment and find that government providers produce survival outcomes that are superior to their privately owned counterparts. The latter considers the performance of federal hospitals operated by the U.S. Department of Veterans Affairs that treat only military veterans rather than the broader set of patients treated by the public hospitals in our analysis sample.

We add to this strand of the literature by investigating the effects of privatization-induced changes in ownership on performance at the same hospital. Of course, the average difference in performance at the same hospital before and after privatization need not be the same as the average difference in performance between all public and private hospitals. Our results are nevertheless consistent with the conclusions of Chan, Card, and Taylor (2023), as we find that converting a hospital from public to private control worsens survival among Medicare patients. Furthermore, we highlight the differences in operational strategies between public hospitals and their private counterparts, such as cream-skimming of profitable services and payers and reducing labor inputs. The results on cream-skimming reinforce similar findings from the nursing home sector, where providers can choose between Medicaid and more lucrative alternatives (Gandhi 2020; Werbeck, Wübker, and Ziebarth 2021; Hackmann, Pohl, and Ziebarth 2024).

Our results take on additional significance when one considers the considerable variation that currently exists across states in the share of hospitals that are controlled by state and local governments. For example, more than 40 percent of hospital beds in Wyoming, Alabama, and Mississippi are in facilities owned and operated by state and local governments versus less than 4 percent in Pennsylvania, North Dakota, and Vermont (Table A.1). It seems plausible that this variation is at least partly attributable to policymakers' uncertainty about the consequences of public versus private ownership generally and of hospital privatization specifically.

The paper proceeds as follows. Section 2 provides the necessary background about hospital control, including a discussion of predictions from economic theory. We describe our data sources in Section 3, and our empirical strategy in Section 4. We present the effects on the main outcomes of interest in Section 5, followed by evidence on potential mechanisms in Section 6. Section 7 discusses the implications of our results and Section 8 concludes.

5. Recent related research has also considered the effect of competition from public firms on the behavior of private firms (Atal et al. 2024) and the effects of granting managers of public firms greater autonomy (Kala 2024).

2 Background

2.1 Hospital control

There is substantial heterogeneity in the mix of hospital control types across different geographies.[6] 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 states (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 2019. In these tables and throughout the paper, we choose to focus on nonfederal public hospitals, since these usually serve the local community and are more comparable to private hospitals than federal hospitals, which mostly cater to military veterans or other designated populations (e.g., Native Americans).

We note two interesting patterns in hospital ownership. First, states vary enormously in their dependence on public hospitals. Pennsylvania has only 4% of its beds in state or local government hospitals, while 44% of Alabama's hospital beds are in such hospitals. This variation is even greater if we consider small states (Wyoming and Vermont have 71% and 2%, respectively). Second, the observed patterns are not easily explained. The share of public hospitals does not track states' preferences over the size of government. For example, Alabama has a higher share of public hospitals than Illinois. Similarly, the state's rural share of population does not explain public provision: Vermont and Maine are among the most rural states in the U.S. but also have among the lowest shares of public hospital capacity.[7] Higher shares of hospital beds under government control in states that otherwise favor limited government foreshadows more waves of privatization in the future. Hence, the role of the government in the delivery of hospital care deserves greater research scrutiny.

2.2 Conceptual framework

This section uses the economic framework developed in Hart, Shleifer, and Vishny (1997) to generate testable hypotheses about the effects of privatizing government hospitals. Hart, Shleifer, and Vishny (1997) propose a general stylized model in which a manager chooses the optimal effort on two dimensions: to reduce costs and improve quality. The model generates predictions on the

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

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

optimal levels of effort of the manager as a government employee versus as a private contractor. Following prior literature, the model assumes that a manager has weaker incentives to innovate on both cost and quality as a government employee (Kornai 1986; Laffont and Tirole 1993). In addition, they make the novel assumption that while the government controls actions by its employees, it cannot write a contract to completely specify all the tasks it would like a contractor to perform or all the standards that need to be met. Incompleteness of the contract implies that a private contractor does not internalize the potential harm to consumers from reducing activities (and avoiding costs) that are not specified or enforceable. This assumption is highly plausible in the hospital setting. The average hospital in our sample has hundreds of employees across different categories, offers scores of services, and serves thousands of patients. It is inconceivable for the government (or insurers) to specify in detail the level of care inputs and desired treatment styles in their contract.

Under these assumptions, the model predicts that a private contractor optimally chooses more effort than a government manager to reduce cost and improve quality. Furthermore, a private contractor will reduce costs more than is socially optimal due to the incompleteness of its contract. Therefore, the model unambiguously predicts that private management reduces operating costs. However, the effect of private management on quality is ambiguous and depends on whether quality innovations are offset by excessive cost cutting. The model suggests that when the potential for harm to consumers from cost cutting is high and the payoff from quality innovation is limited, public control is likely to be superior. The first condition is satisfied in the case of hospital care. The second condition may not be because quality innovation by healthcare providers can produce significant benefits for patients. However, due to information frictions (i.e., consumers cannot accurately observe quality) and low average levels of competition in the hospital sector, it is not clear whether private managers are adequately incentivized to invest in improving quality.

The original model did not consider the possibility that the manager could also innovate to increase revenue. However, this is an important margin in the case of hospitals. Following the same rationale as for cost and quality, a manager employed by the government also has weaker incentives to maximize revenue than if she is under private control. For example, senior executives can push for higher prices during negotiations with private insurers or can carefully shift the hospital's focus toward more lucrative services, if they are sufficiently motivated to make the necessary effort. Hence, we hypothesize that privatization will lead to an increase in revenue in addition to a reduction in costs. Whether the increase in profitability is sufficient to eliminate the need for subsidies is an empirical question.

Interpreting the prediction of socially inefficient cost cutting in the context of the hospital sector leads to two additional hypotheses. The hospital may avoid groups of patients that are expected to be less profitable or unprofitable. Undesirable groups could be targeted in multiple ways, for example, depending on their type of payer or reason for admission. Our analysis of data from the Medical Expenditure Panel Survey (MEPS) shows that state-run Medicaid programs pay very low reimbursement rates relative to Medicare and private insurers. Uninsured patients pay even less than Medicaid on average. Hence, private management may systematically try to reduce admissions for indigent patients, a possibility highlighted by the previous literature. In his review of privatization, Shleifer (1998) cautions that "private hospitals may refuse to treat patients on whom hospitals generally lose money." Since government hospitals disproportionately serve indigent patients (Horwitz 2005), a change in focus after privatization could disrupt access to care for these vulnerable patients. However, finding a decrease in the volume of lower income patients alone is not sufficient to deem it socially inefficient. We test for both changes in access to care and in health indicators of the affected population. A worsening of health along with the decrease in hospital care would suggest social inefficiency.

The effect of privatization on access to care will also depend on the response of the remaining hospitals in the market, which in turn may depend on the level of concentration (Vickers and Yarrow 1991). Hospital markets tend to be local and 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. Privatization may spur a greater response from competing hospitals in more concentrated markets, 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 proportional loss of its lucrative patients to the newly private-run hospital. Negative responses from competitors could therefore reinforce and exacerbate the adverse effects of privatization on access. We therefore examine heterogeneity in the access effect by the level of concentration in the market.

Finally, cost reductions at the hospital may outweigh quality improvements and the net quality of care may decline. Hospital care is highly labor intensive, with personnel spending contributing more than half of the total operating cost, according to AHA data. Hence, staff reductions offer a path to relatively quick and significant cost savings. Previous studies have shown that a reduction in the availability of staff, including people in roles beyond nurses and physicians, worsens patient health outcomes (Friedrich and Hackmann 2021; Andreyeva et al. 2024). Other studies have shown that changes in staff availability can bring about changes in treatment protocols, such as shorter stays, which worsen patient outcomes (Aghamolla et al. 2024). Again, finding a worsening in quality along with a reduction in relevant costs would be consistent with excessive cost cutting.

In summary, privatization represents a trade-off between improved profitability and lower subsidies, on one hand, versus harmful effects to consumers arising from a decrease in access and/or quality, on the other. Causal evidence on both sides is crucial for well-informed policy making. Guided by the theory, our main outcomes of interest are hospital profitability, patient volume, which helps to assess the effect on access, and health indicators. We then explore potential mechanisms that may help explain these changes.

2.3 Hospital privatization in the U.S.

State and local governments in the U.S. have increasingly relinquished operational control of hospitals to private firms. We identify and study 254 instances of hospital privatization during the 2001–18 period. To put this figure in context, consider that of the 1,010 public hospitals in our sample in 2000, a quarter were privatized within 19 years. This tool is used mainly by local governments. In our sample, only 14 of the 254 privatizations, just over 5%, involved state-owned hospitals. The remainder involve facilities owned by counties (92), cities (33), or special-purpose hospital districts (115). The latter are similar to school districts in that they span multiple towns or cities within a county and tax constituents to fund and deliver health care services. Therefore, most privatization decisions are taken by county executives, governing boards of hospital districts, or city mayors.[8]

Our review of the news coverage of privatizations suggests that there is significant heterogeneity in the motivations behind these transitions. However, two drivers appear to be important for privatization. One is to reduce government subsidies devoted to hospital care while continuing to offer hospital services. The other is ideological and stems from the belief that private firms operate hospitals more efficiently than the government without compromising quality or access to care.

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

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

The first category 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 outsource operations to a hospital management firm. 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.

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

Private operators enjoy substantially more operational control over the hospital in the second group of deals. This group contains three types of deals. The first is an outright sale of all hospital assets. The second approach is for the government to award a long-term lease, giving the contractor more autonomy to make changes to the buildings and equipment. Third, the private operator enters into a joint venture with the government to jointly own and operate the hospital. Interestingly, for-profit firms are involved in more than 40% of the deals that grant more control, but less than 20% of the deals that grant less control, suggesting a preference for the ability to make more far-reaching operational changes.

2.4 Government provision versus coverage of hospital care

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

In stark contrast, public insurance coverage of hospital care has grown rapidly during the same period. Figure 1 Panel (b) plots the trend in the share of patients covered by the two main public insurance programs at nonfederal hospitals. Medicaid, the means-tested public insurance program, more than doubled its share of hospital patients from 10% in 1983 to 22% in 2019. This is not surprising since Medicaid coverage eligibility has been expanded through several federal and state policy initiatives during this period. The share of Medicare, the public insurance program for the elderly, also increased from 32% to 45%.[9] Unlike Medicaid, eligibility for this program has been relatively stable and a large part of the increase is due to aging of the population. Perhaps, local governments view the expansion of Medicaid coverage as an alternative means of ensuring access to care, making it easier to justify the privatization of public hospitals. Consistent with this hypothesis, Table A.1 shows that 7 of the 10 states with the highest shares of public hospital beds, typically those that favor limited government, had not expanded Medicaid under the ACA as of 2019. In contrast, eight of the 10 states with the lowest shares of public hospital beds had expanded Medicaid.

The negative association between Medicaid share of hospital admissions and public hospital share of bed capacity is also present within-state over time. Using a panel data analysis, we find that an increase in Medicaid share of 10 percentage points predicts a decrease in public hospital share of approximately 4 percentage points.[10] Recall that the national share of nonfederal pub-

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

10. 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:

lic hospitals dropped by about 10 percentage points during this period; hence this effect size is economically meaningful. This estimate should not be interpreted as a causal effect. However, it is consistent with the hypothesis that local policymakers may view Medicaid expansions (by the state or federal governments) as a substitute for government provision of hospital care.

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 1996–2019 to source information on hospital attributes such as ownership type and location, and performance on patient volume, operating costs, and employment. We study inpatient volume by payer and in aggregate. Specifically, we observe inpatient volume for three payers: Medicare, Medicaid, and a residual group ("Other"), which is largely made up of privately insured and uninsured patients and contributes approximately 35% of patients in government hospitals. We cannot separately observe the number of hospital stays by uninsured and privately insured patients in the AHA data, but we do so using other datasets described below. We study changes in aggregate patient care using a standard measure, "adjusted admissions", which is reported by the AHA and incorporates both inpatient and outpatient care (Schmitt 2017). Adjusted admissions are calculated by adding to hospital stays the number of outpatient visits scaled by the ratio of outpatient charges to inpatient charges to account for their lower resource intensity. We examine the total FTE employed staff and the effects on different staff categories (physicians, nurses, and others).

We identify the privatization of government hospitals using a multi-step process, following previous studies on changes in hospital ownership (Schmitt 2017; Cooper et al. 2019; Prager and Schmitt 2021). 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 355 privatizations of public hospitals during 2001–18. 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

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

this approach, we validate 254 privatizations, which implies a false positive rate of 28% in our sample (101/355), similar to the 30% rate reported by Schmitt (2017) who used AHA data to study hospital mergers. Section B.2 describes other details of the sample construction.

We limit our final analysis sample to government-owned nonfederal general acute care hospitals. We retain government hospitals that were treated (privatized) or did not experience a change in ownership during this period. The sample is an unbalanced panel at the hospital-year level. Figure A.1 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

As discussed in Section 2.2, one of our key hypotheses is that private management will avoid admitting less profitable patients to improve profitability. To test this hypothesis, we would like to observe admissions granularly by payer and service line. However, AHA data do not report admissions by service line, nor separate private and uninsured admissions. To overcome these two limitations, we use more detailed administrative data on hospital care from select large states that experienced several privatizations during this period and share data for research purposes. We were able to obtain data from five states (CA, FL, IN, MN, and WA), of which Indiana and Minnesota are among the top 5 states by number of privatizations. Collectively, we observe 27 privatizations between 2008–2018 in this data, approximately 10% of the total number of privatizations studied using AHA data. In the case of Florida, Indiana, and Washington, we have access to detailed patient-level hospital discharge data. In the case of California and Minnesota, we use annual reports on total hospital patient volume by payer. In addition to examining the effect on total inpatient volume, we also study the effect on obstetric patients, as an example of changes for a relatively unprofitable service. Minnesota does not consistently report obstetric volume and therefore we perform this analysis using the other four states. We describe these data in detail in Section B.3.

Medicare claims

AHA data do not allow the study of changes in treatment choices or quality of care. 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 Medicare FFS beneficiaries. These files were obtained from the Centers for Medicare and Medicaid Services (CMS) under a data use agreement and cover the period 2000–2019. We observe all hospital stays for FFS patients nationwide during this period. Since this sample starts five years later than the AHA sample, we are able to study 51 fewer privatizations when we impose the same sample construction rules. Medicare data allows us to test for changes in observed health risk of admitted patients, as we can use the complete history of a patient's health care utilization to develop risk indicators. We limit our analysis to patients aged 65 years and older, who represent the primary beneficiary group within

Medicare.[11] We test for changes in hospital list prices or "charges" after privatization while controlling for changes in patient risk. This provides insight into hospital billing practices. A key benefit of this data is that it also records deaths that occur outside of the hospital. We examine changes in patient mortality rates to test the effect on hospital quality. Section B.4 describes the construction of this sample and the variables in more detail.

Supplementary data

We supplement the main data sources with information from publicly available files. We source data on hospital revenue and use of contract labor from the Healthcare Cost Reporting Information System (HCRIS), more commonly known as Medicare cost reports. To our knowledge, this is the only national source of data on hospital revenue. We compare the revenue data reported in HCRIS with the corresponding values in detailed administrative reports from California and Minnesota and find a high degree of concordance. This is reassuring because state reports are more detailed, more likely to be audited, and generally considered of high quality.[12] We obtain nationally representative mean hospital reimbursement rates by payer from the MEPS. We describe these data in more detail in Sections B.5 and B.6, respectively. Finally, we obtain information on market-level attributes, such as county-level population, poverty, unemployment, and uninsurance rates, from the U.S. Census and the Bureau of Labor Services (BLS).

Variable construction

Since there is substantial heterogeneity in size across hospitals, we follow the previous literature and study the outcomes either in logarithmic points or after scaling them by bed capacity (Finkelstein 2007; Acemoglu and Finkelstein 2008). An exception to this principle is the outcomes related to hospital finances (revenue and costs). These values tend to vary tremendously between hospitals even after scaling by hospital size, and so we log transform these *after* scaling by hospital size. In robustness checks, we show that the results are not sensitive to scaling outcomes by adjusted admissions instead of beds. Throughout, all monetary values are adjusted for inflation and are expressed in 2019 dollars.

3.2 Descriptive evidence

Table 2 describes the hospitals observed in the AHA sample, which we consider the main analysis sample. Across all columns, we present values from 2000, a year prior to the first privatization in our sample. Column 1 presents values for the 254 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*

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

12. A regression of HCRIS values on the corresponding state values over 2003–19 returns a coefficient of 0.92 for all hospitals and 0.90 for government hospitals.

privatized hospital. This group comprises our primary comparison group. We impose this distance requirement to mitigate the potential for spillover contamination.[13] Comparing the values in these two columns reveals that privatized hospitals had about 21% fewer beds than comparison hospitals, but were otherwise very similar: both types admitted about 34 patients per bed per year and approximately 64% of their patients were covered by the primary public payers Medicare and Medicaid. The privatized hospitals already had about 5% lower operating expenses per bed at baseline, implying that they were leaner than the comparison group prior to the change of control. Privatized hospitals had better finances at the beginning of our study compared to the remaining public hospitals, suggesting that governments may find it easier to attract private partners for the better-run facilities.

Column 3 presents the corresponding statistics on the 3,867 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 34). Public payers accounted for a lower share of their patients (60%). They employed more FTE staff and had higher operating costs per bed 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 4,923 hospitals in the sample. Since about 80% of the hospitals are privately owned, the aggregate statistics lean toward those of private hospitals.

Figure 2 describes the phenomenon of hospital privatization in the U.S. over 2001–18. Panel (a) presents a heat map of the U.S. based on the number of privatizations in the state. The states in the South and Midwest experienced the highest number of privatization events during this period. Texas, Minnesota, Georgia, Louisiana, and Indiana are the five states with the highest number of privatizations. However, privatization is a widespread phenomenon: more than 40 states experienced at least one and no state experienced more than 30. Panel (b) presents the number of privatizations in each year. There were at least 10 privatizations in each year from 2002 through 2017, and no single year accounts for more than 8% of the total number of privatizations. The trend of privatization accelerated following the Great Recession – there were 15.4 conversions per year in 2010–2018 versus 12.8 per year over 2001–2009.

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

13. This restriction drops only 32 potential control hospitals. 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.

comparable to the national averages presented in Table 2. The remaining patients, which comprise the "Other" group in the AHA, can be allocated to three types of payers. Privately insured and uninsured patients account for the vast majority of patients in this group, 80% and 13%, respectively. A small proportion of patients are in neither category, such as workers' compensation and other government plans. We label these as "Miscellaneous."

Table A.4 shows that the Medicare sample contains 203 privatized hospitals and 767 nonprivatized public hospitals used as the comparison group. As discussed earlier, since this sample starts 5 years later than the AHA sample, we retain fewer privatized hospitals.[14] 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. In general, the privatized hospitals and the comparison group have similar mean values at baseline.

4 Empirical Strategy

Our goal is to estimate the causal effect of privatization on various stakeholders. We leverage the 254 privatization decisions by state and local governments as natural experiments to obtain the average treatment effect on the treated (ATT) using a staggered difference-in-differences (D-D) research design. We note that privatizations are not randomly assigned and therefore the ATT may differ from the effect of privatizing the average government hospital. Government hospitals that did not experience a change in ownership during 2001–2019 form the comparison group, since they intuitively form the pool of candidates for privatization. This approach follows the previous literature studying privatization and ownership (examples include Galiani, Gertler, and Schargrodsky 2005; Cooper et al. 2019; Eliason et al. 2020 and Arnold 2022), which has generally also used a D-D design. The identification assumption is that privatized and comparison hospitals would proceed along parallel trends in the absence of treatment.

In addition to presenting evidence on pre-trends for all outcomes, we take several steps to help make this assumption more plausible. First, we make the hospital sample more homogeneous by excluding facilities classified as specialized by the AHA (e.g., psychiatric, rehabilitation, etc.). There are virtually no privatizations among these groups during our sample period, so they would overwhelmingly fall into the comparison group. However, the patient mix and treatments at these hospitals can differ dramatically from those of general acute care hospitals. Second, we exclude hospitals located within 15 miles of any privatized facility to avoid the possibility of contamination by spillover patient flows. Third, we retain the 29 comparison hospitals that close during the sample period, as closure is a plausible counterfactual outcome to privatization. We have to then grapple with instances of zero values, which is problematic when using log models

14. We also lose a few comparison hospitals since they could not be cross-walked to the AHA. Some small comparison hospitals had to be dropped due to CMS disclosure restrictions.

(Chen and Roth 2024). Hence, our preferred approach is to include observations for these hospitals as long as they have nonzero patient volume. We get qualitatively similar results with an alternate approach that includes the observations with zero values for the remainder of the sample period. Fourth, in order to ensure that the estimated pre-trends can be interpreted without worrying about compositional shifts, we require that privatized hospitals are observed for a minimum of five years prior to the transition. Finally, we focus our estimation on a period of 5 years before and after the privatization. We find an estimated effect more credible if privatized hospitals deviate from the comparison group within this window of time without any pre-trends (Cooper et al. 2019; Eliason et al. 2020). We exclude data from the year of privatization in our baseline models, since it represents partial treatment (we do not know when the change occurred within a year) and hospitals may experience transient disruptions to care during the change in management which may introduce bias. We relax many of these restrictions in sensitivity checks and find that the coefficients remain unaffected.

Equation 1 below presents our baseline model. Y_{ht} denotes the outcome of interest for hospital h in market m in year t. We model the outcome as a function of hospital and year fixed effects, α_h and α_t, respectively. The use of hospital fixed effects eliminates persistent unobserved differences between hospitals (and the markets they belong to), an important source of selection. Recent studies of hospital closures have noted that markets experiencing closures had weak economic trends prior to closures (Alexander and Richards 2023; Chatterjee, Lin, and Venkataramani 2022). Hence, we test sensitivity to including covariates X_{hmt}, a vector of time-varying hospital, market, and state attributes, which comprises unemployment, poverty, and uninsurance rates for the county in which a hospital is located; county population; whether a hospital is a 340B provider; and an indicator for Medicaid expansion under the Affordable Care Act (ACA). The key regressor of interest, D_{ht}, is a time-varying indicator variable that is equal to one starting in the year the hospital is privatized and zero otherwise. Finally, ϵ_{ht} denotes unobserved time-varying factors. We cluster standard errors by hospital to account for the potential correlation of outcomes over time in the same hospital, which is the unit of treatment.

$$Y_{ht} = \alpha_h + \alpha_t + \beta\, D_{ht}\, [+X'_{hmt}\,\delta] + \epsilon_{ht}. \tag{1}$$

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

Under the parallel trends 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 2 for each outcome.

$$Y_{ht} = \alpha_h + \alpha_t + \sum_{s \neq -1} \beta_s \, D_{h,t+s} \, + \epsilon_{ht}. \tag{2}$$

A lack of differential trends in the years prior to privatization is consistent with the identifying assumption. Reassuringly, the evidence that follows suggests little or no differential pre-trends and relatively large changes soon after privatization on certain key outcomes.

We prefer to use the two-way fixed effects (TWFE) estimator in our baseline model due to its simplicity, transparency, and flexibility. However, recent econometric literature has demonstrated the potential for bias in TWFE estimates in a staggered treatment setting due to heterogeneous treatment effects (e.g., Goodman-Bacon 2021). We thoroughly assess the sensitivity of the baseline estimates to using alternative estimators proposed by De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2020). These estimators use different approaches to correct for potential bias in staggered treatment and help assess the importance of this concern. We also present event study plots for all outcomes using the Callaway-Sant'anna (henceforth, CS) estimator. Reassuringly, the numerous robustness checks generate similar estimates and lead to qualitatively similar conclusions.

5 Main results

5.1 Finances

Economic theory unambiguously predicts that private management should reduce costs and, as we note in Section 2.2, increase revenue as well. Hence, we begin our analysis by examining the effects on hospital finances. Table 3 presents the D-D coefficients obtained by estimating Equation 1 without including the covariate vector X_{hmt} in Panel A, while Panel B presents the corresponding results obtained by including the time-varying hospital, market, and state controls mentioned in Section 4. For brevity, we limit the analysis to four outcomes. Column 1 presents the effect on the mean revenue per bed. This measure includes revenue from inpatient and outpatient care and is net of all discounts and adjustments. Columns 2, 3, and 4 present the effects on mean operating expenses, personnel spending (including benefits), and all nonpersonnel expenses per bed, respectively. As noted earlier, 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 bed. Figure 3 presents the corresponding event study plots with the dynamic effects on each outcome around the transition.

As the table shows, the estimates are very similar whether we include market-level covariates or not. This is reassuring, since it mitigates the concern of model misspecification and omitted variables such as differences in the prevailing economic environment. We prefer to focus on the estimates obtained without including additional covariates as our primary results; hence, throughout the text, we will primarily discuss these estimates unless they meaningfully diverge between the two panels. First, in column 1 of Table 3 Panel A, we detect an 8% increase in mean revenue per bed, which is statistically significant at the 5% level. Since the mean revenue per bed is about $651,000, this implies an increase of about $52,000 per bed for the average privatized hospital. Figure 3 Panel (a) shows an increase in mean revenue in the year following privatization, and the increase slowly grows 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 bed and indicates a statistically insignificant decrease of 0.9%. Figure 3 Panel (b) presents the corresponding event study, which suggests a transient decline in the first year after the change in control, but no persistent decrease after privatization. Table 3 columns 3 and 4 unpack this result by presenting the effects on personnel and non-personnel costs, respectively. Personnel spending includes salaries and benefits. The coefficient indicates a statistically significant decline in average personnel cost per bed of 6% (col. 3). This appears to be a moderate decrease, but is quite large considering that privatized hospitals had lower personnel spending at baseline than the comparison group (see Table 2). This result signals that private management reduces labor inputs after taking over. We investigate the source of the decrease further in Section 6. The decline in personnel spending is partially offset by small increases in costs elsewhere (col. 4). This latter coefficient is positive and statistically insignificant. Figure 3 Panels (c) and (d) present the corresponding event study plots that are consistent with the average effects implied by the D-D coefficients.

Overall, privatization meaningfully improves hospital profitability. In the year before privatization, treated hospitals had an operating margin of -$18,100 per bed or 3% of mean revenue. Therefore, the 8% increase in revenue alone is sufficient to enable these hospitals to generate a modest surplus. If we also include the 0.9% reduction in cost in this calculation ($6,000), ignoring the statistical insignificance for a moment, we estimate an increase in operating margin of approximately $58,000 per bed or about 9% of the mean revenue. It is intuitive that the private management focuses on increasing mean reimbursements to improve profitability since privatized hospitals already had a competitive cost structure at baseline.

These results are not sensitive to our choice to scale financial measures by beds. As a sensitivity check, we estimate a companion set of models in which we express financial measures per contemporaneous adjusted admissions instead. This approach is also appealing because the estimates represent the percent change in mean reimbursement or cost per patient. Figure A.2 and Table A.5 present the corresponding event study plots and point estimates, respectively. This formulation of the outcome also produces similar estimated effects. For example, we detect a statistically significant increase in mean revenue per patient of about 6%.

5.2 Patient volume

This section investigates the effects of privatization on patient admissions. We first examine the effects at the privatized hospital and then test whether the aggregate hospital volume at the market level is affected. Since one of our main hypotheses is that private management may reduce admissions of indigent patients to improve profitability, we pay particular attention to admission volumes of uninsured and Medicaid patients.

5.2.1 Admissions at the privatized hospital

Table 4 presents the D-D estimates of the effect of privatization on patient volume 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, "Other." The total number of patient admissions to the privatized hospital decreases by 8.9% 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 presents the corresponding event study plots obtained by estimating Equation 2. Panel (a) indicates a sharp and persistent decrease in volume following the transition.

The decrease in inpatient volume may reflect a shift toward outpatient care after privatization. We test this conjecture and fail to detect an accompanying increase in outpatient care at privatized hospitals. Table A.6 columns 1 and 2 present the corresponding effects on the log of Emergency Department (ED) and non-ED outpatient volumes, respectively. In both cases, we find statistically insignificant and negative coefficients, which suggests, if anything, a decrease in outpatient treatment. Figure A.3 Panels (a) and (b) present the corresponding event study plots that corroborate the D-D coefficients.

We then consider heterogeneity in the effects by payer type. There is substantial heterogeneity in mean reimbursement rates across different payers, which we document using patient-level hospital reimbursement data from the MEPS. Table A.7 Panel A column 1 presents the corresponding values, expressed in 2019 dollars. We calculate the overall baseline average reimbursement rate as a weighted average of the respective reimbursement rates for Medicaid, Medicare, and Other using the corresponding patient shares in column 2 as weights. The data confirm that Medicaid is less lucrative on average than Medicare and private insurers but pays more than the average uninsured patient. The mean unadjusted Medicare and private insurer rates are about 45% and 60% higher, respectively, than the amount paid by the average Medicaid patient. In contrast, uninsured, or self-pay, patients pay 35% less than the mean Medicaid rate. Previous studies have suggested that Medicaid and uninsured patients are unprofitable to serve, on average (Frakt 2011; Schulman and Milstein 2019). Hence, if private management aims to increase profitability, as economic theory predicts, shifting the payer mix away from Medicaid and uninsured patients will help.

The results in Table 4 Panel A support this hypothesis. While admissions of low-income Medicaid patients decrease by 15.6%, Medicare admissions only decrease by about 5%, and the coefficient is statistically insignificant. We detect a 14% decrease in the Other group. Taken together, we infer that hospital privatization primarily affects non-Medicare patients. 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 the five years that 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 following privatization (Panel c). We also study the effect on adjusted admissions, which incorporate both inpatient and outpatient volumes. Table 4 Panel A column 6 presents the result, implying a 6% decrease in total hospital care. Figure 4 Panel (e) presents the corresponding event study.

We exclude the year of privatization from the sample in our preferred approach, as noted earlier. Regressions on samples that retain these data generate slightly larger decreases. For example, the effect on total volume increases to 9.6% from 8.9%. Our baseline approach drops hospital-year observations from the sample if they have zero volume. Hence, our results represent the effect on admissions on the intensive margin. An issue with this approach is that closures, while rare in the sample, occur disproportionately among comparison hospitals. The closure rate during 2001–19 among comparison hospitals is 3.6% while it is 0.4% for privatized hospitals. Focusing only on the intensive margin will lead to a more negative effect on volume than the total effect if privatization helps avoid closure. In Section 5.4, we assess this possibility by examining the closure rate for a subset of comparison hospitals matched to the privatized hospitals based on baseline attributes and find that they have similarly low closure rates, suggesting that the difference in closure reflects differences in baseline attributes. Nevertheless, to assess the importance of this concern, we perform a sensitivity check in which we retain observations for closed hospitals in the sample assigning them zero volume. Reassuringly, these models also imply large and statistically significant decreases in Medicaid admissions (10%) and other admissions (8%). These magnitudes are well within the confidence intervals of the main estimates. The effect on total admissions becomes statistically insignificant and close to zero (1.1%), because the effect on Medicare admissions is now marginally positive (1.3%). Overall, even under this conservative approach, we find that privatization leads to a large decrease in admissions for non-Medicare patients.

Similarly, we hypothesize that the decline in the Other group is likely driven by uninsured patients, while privately insured patients are relatively unaffected. We test this hypothesis using more detailed data that we were able to obtain from five large states (California, Florida, Indiana, Minnesota, and Washington), together representing 27 privatizations. We apply our baseline difference-in-differences research design to these data. The small sample of privatized and nonprivatized hospitals necessitates an alternate modeling approach to ensure parallel trends between the two groups. Therefore, we use the newly developed synthetic difference in differences estimator (SDID) (Arkhangelsky et al. 2021). SDID constructs a weighted average of observed control units to generate a synthetic control unit for each cohort of treated hospitals. In addition, a second set of time-varying weights is chosen to ensure that the synthetic controls trend in parallel with their matching treated units before treatment. A limitation of this method is that, in the case of staggered treatment, it does not produce a conventional event study plot that aggregates all treated units. 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.[15] Section B.3 describes sample construction and methodology in more detail.

Table 4 Panel B columns 1–4 present results on the same outcomes as in Panel A and therefore allow for a comparison between the national and state samples. Reassuringly, we find very similar patterns in these states. The coefficients indicate a decrease in admissions across all payers, with a disproportionate decrease in Medicaid. Columns 5–7 disaggregate the effect on the Other group into three components: private insured, uninsured, and a small residual set of patients that do not belong to either, which we group under "Miscellaneous." These estimates imply that the decrease in Other is mostly 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.4 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, the placebo distributions are symmetric and center around zero. The effects on Medicaid and uninsured volume for privatized hospitals are outliers, while those for other payers tend to fall within the corresponding placebo distributions.

We perform an additional exercise using SDID to highlight the difference between the effects on the less lucrative payers, Medicaid and uninsured, and those on the remaining payers. We detect a 25% statistically significant decline in the sum of Medicaid and uninsured admissions at privatized hospitals. In contrast, we estimate a statistically insignificant decrease of 2% in pooled admissions for all other payers. Figure A.4 Panels (g) and (h) present the corresponding placebo distributions and estimated effects, respectively. Hence, privatization causes a shift away from less lucrative payers, consistent with profit maximization.

5.2.2 Admissions at the market level

We showed that public hospitals, once privatized, persistently admit fewer patients and the decline is felt disproportionately by low-income patients. It is possible that other hospitals in the market respond by increasing their volume and offset this decrease. However, if these patients are

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

perceived as unprofitable or undesirable, then other hospitals may be reluctant to offset the decline at the privatized hospital. From a policymaker's perspective, the result assumes more significance if privatization causes an aggregate decline in utilization at the market level, suggesting greater difficulty in accessing hospital care.

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 U.S. Census for the same purpose as Hospital Referral Regions (HRRs) developed by the Dartmouth Atlas group and have been used to study hospital markets (Makuc et al. 1991; Ho and Hamilton 2000; Petek 2022). HSAs have two appealing properties for our analysis. First, they are moderately sized. The average HSA in our sample contains about five hospitals. In contrast, the average HRR contains 18 hospitals. Consequently, we have greater statistical power to detect the market-level effects of a single privatization. At the same time, HSAs adequately capture a patient's hospital choices. Using Medicare claims data, we confirm that more than 70% of FFS patients choose a hospital located in the same HSA as their home zipcode. Second, HSAs coincide with county boundaries, allowing us to link county attributes and outcomes to HSAs.

To implement our analysis at the market level, we consider the 202 markets containing privatized hospitals as "treated," while the 727 remaining markets form the comparison group.A market is considered treated when it first experiences a privatization (40 of the 202 markets experienced more than one privatization event) and is assumed to be treated until the end of the sample. We estimate an unweighted market-year-level model equivalent to that presented in Equation 1.

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

Table 5 presents the estimated effects on hospital admissions at the market level, calculated as the sum of admissions across all hospitals located in the market. Since markets are quite heterogeneous, we model the effects on log patient volume. The columns present effects on total volume and by payer. Panels A and B present the average effects from specifications without and with time-varying covariates, respectively. Including market-level covariates tends to magnify the point estimates but leads to similar interpretations; hence, we continue to focus on the estimates without covariates. Column 1 presents estimates on total volume and reports a 0.4 percentage point (pp) decrease. However, we are under-powered to statistically detect an effect of this magnitude at conventional levels of significance.[16]

The key finding is that hospital privatization at the market level appears to predominantly

16. We also estimate an imprecise decrease in log total adjusted admissions at the market level. The coefficient is -0.010 with a standard error of 0.012. Hence, the qualitative pattern remains similar even if outpatient visits are included in the admission count. We do not report these results in the interest of brevity.

affect Medicaid patients, who experience a meaningful decline. In contrast, we estimate small positive effects on both Medicare and Other volume. The effect on Medicaid is -4.2 pp, slightly more than what we would predict based on the privatized hospital's decline alone (21% of -15.6, or -3.3 pp). This estimate suggests that other local hospitals do not offset the decrease at the privatized hospital. 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 5 presents the corresponding event study plots for these outcomes. The estimated dynamic effects are consistent with the coefficients discussed above. Medicaid is the only payer for which the coefficients are consistently negative after privatization.

We examine heterogeneity in the effects on aggregate patient volume across markets along two policy-relevant dimensions, the first of which is the level of concentration in the local hospital market. Vickers and Yarrow (1991) note in their comprehensive review that privatization does not increase productivity and growth when markets are not competitive. This is a highly pertinent issue in the case of hospital markets and can exacerbate the effect on admissions for unprofitable patients depending on the response of competing hospitals, as discussed in Section 2.2. We therefore test for a differential effect on patient volume in more concentrated markets. We designate treated markets as more concentrated if their HHI was above the median value across all treated markets in 2000. We estimate triple difference models, comparing trends for both types of treated markets to all comparison markets.

Table 5 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 still do not detect a statistically significant increase for Medicaid patients. There is a sharp decline in the aggregate volume of 5.1% in concentrated markets (4.2-9.3 = -5.1). Although volume declines in more concentrated markets across all payers, the decline is most pronounced for Medicaid patients at -11.0% versus -4.6% 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. Although we do not find relatively larger declines in Medicaid admissions among privatized hospitals in concentrated markets, privatized hospitals contribute a larger share of Medicaid admissions in these markets (40% vs. 13%). The estimate of a 14.4% (exp(-0.156) - 1) decline in Medicaid admissions at privatized hospitals predicts an aggregate decline of about 5.8% (40% x 14.4%), assuming no response from the remaining hospitals. These results imply that a substantial fraction of the aggregate decrease in Medicaid cannot be explained by the actions of the privatized hospitals alone. The remaining hospitals in these markets likely also reduce Medicaid admissions when they are exposed to a privatization.

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

Table 5 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 3% (2.3 - 5.3 = -3), which is marginally significant. This is driven primarily by a large and statistically significant decrease in Medicaid volume of 12.0% (3.8 - 15.8 = -12.0). As in the case of concentrated markets, the aggregate decrease in Medicaid here cannot be explained by the direct effect on the privatized hospital alone.

In a companion set of results not reported here, we find a qualitatively similar pattern of a differential decrease in admissions in markets with hospitals that had lower profit margins at the beginning of the sample period. Hence, the limited financial cushion of competing hospitals plays a role in exacerbating the effect of privatization on hospital access.

5.2.3 Interpreting the decrease in aggregate Medicaid admissions

As we noted in Section 2.2, finding a decrease in aggregate Medicaid patient volume does not necessarily imply that this change in hospital focus is socially inefficient. To help infer the value of the stays avoided, we examine the effect on health of the local population. If hospital stays avoided by privatization are clinically valuable, it may lead to an increase in mortality rates among the population segments most affected. We use national vital statistics microdata from the National Center for Health Statistics (NCHS) to examine the association between the effect on Medicaid admissions and on mortality at the market level. For brevity, we describe this data set and analysis in detail in Appendix C. We briefly note here that we detect a statistically significant correlation between the two effects. Specifically, markets that experience a larger decrease in Medicaid admissions following a privatization also experience a larger increase in mortality rates among 55–64 year old individuals. Figure C.1 presents the corresponding binned scatter plots showing the relationship, which is approximately linear. We focus on this age segment because they are more sensitive to changes in hospital access than younger groups and, at the same time, are more likely to have Medicaid as their primary insurer relative to people 65 years of age or older. This and other results in this analysis suggest that the hospital stays avoided by privatization have clinical value. Therefore, the aggregate decrease in Medicaid stays likely represents an example of socially inefficient cost cutting.

5.3 Hospital quality

As discussed in Section 2.2, private operators are incentivized to exploit incomplete contracts with insurers and regulators to reduce care inputs and maximize profits. This may reduce the quality of care provided at the hospital, and patient health may suffer. Quality of care and health are multidimensional objects, and a comprehensive examination of the two is out of scope for this paper. We therefore focus narrowly on the effects on mortality, an unambiguously bad outcome and one that is observed with little measurement error. The economic 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 would ideally like to examine the effect on mortality across all patients at the privatized hospitals. However, we can only perform this analysis for Medicare FFS patients, for whom we observe rich patient-level data. We apply our D-D research design and estimating equation to the claims data sample. One difference relative to the AHA is that the Medicare sample begins in 2000 instead of 1996. Hence, to ensure that we observe five pretreatment years for all treated hospitals, we drop 51 hospitals privatized over 2001–2004 from the sample for this analysis. We limit the sample to patients aged 65 to 99 years and who were enrolled in Medicare Parts A and B for at least 3 months prior to hospital admission to make them more homogeneous and ensure that we can adequately document their risk. Section B.4 describes the data and sample construction in more detail.

We estimate a patient-level equivalent of our baseline specification in Equation 1, with 30-day mortality as the main outcome of interest. We include in the model a comprehensive vector of patient covariates to account for differences in risk, as described in Section 4. In addition to controlling for demographics, we condition on a patient risk index, predicted probability of death within 30 days of discharge. This value is predicted using coefficients from a probit model of mortality at 30 days explained by demographics, comorbidities, and the history of healthcare utilization within the last 90 days. Panel A of Table 6 presents the corresponding coefficients. The top and bottom rows present coefficients from models without and with time varying market-level covariates, respectively. Row A1 reports an increase in mortality of 0.32 percentage points across all FFS patients aged 65 or older, approximately 3% of the mean mortality rate. Controlling for differences between markets increases the magnitude of the coefficient. Figure 6 Panel (a) presents the corresponding event study and shows an immediate increase in mortality following privatization that persists for five years. We continue to detect an increase if we study the effect on mortality at longer time horizons after discharge. Table A.9 Panel A presents the corresponding effects on mortality at 30, 60, 90, 180, and 365 days after discharge. Throughout, the estimated effect on mortality remains between 2-3% of the baseline mortality rate.

Due to concerns about potentially unobserved changes in patient risk, previous studies have preferred to focus on mortality rates for patients admitted with acute nondeferrable conditions

(Card, Dobkin, and Maestas 2009). This group contributes only about a quarter of FFS patients and hence we do not prefer this approach, but we investigate the sensitivity to limiting the sample to these patients. Column 2 of Table 6 Panel A presents the corresponding results and shows a statistically significant increase of slightly larger magnitude than that reported for all patients but similar in percent terms. Figure 6 Panel (b) presents the corresponding event study plot and corroborates the D-D estimate.

We briefly investigate heterogeneity in the effect on mortality for different types of patients. Columns 3 and 4 of Table 6 Panel A present the results separately for patients aged 65–80 and 80–99, respectively. Since older patients are more frail and sensitive to changes in quality of care, we expect a greater effect on their mortality. The results are consistent with this hypothesis and show that the older group experiences a higher relative increase in mortality (3% vs. 2%). Columns 5 and 6 present the effects separately for patients who receive medical treatment versus surgical procedures, respectively. We find a greater increase in mortality for patients receiving medical treatment, although the effects are of similar magnitudes in percent terms. Table A.9 Panel B presents the effect on 30-day mortality for patients belonging to different major diagnostic categories (MDC). We report results separately for the top 5 MDCs: circulatory, respiratory, digestive, musculoskeletal, and kidney disease. Together, these five groups contribute nearly 70% of the total patient volume. We find greater effects for patients in the categories of circulatory, digestive, and kidney diseases. However, in general, we conclude that the increase in mortality is not driven by a specific demographic or disease group; rather, it is experienced by most of the FFS patient groups.

Using our preferred estimate of a 0.32 pp increase, we calculate that the average privatization in our sample leads to an increase of 3.4 deaths among FFS patients per year.[17] In addition to estimating the number of lives lost, we also provide an estimate of the number of life-years lost (LYL) for FFS patients. This approach may be preferable, since elderly Medicare patients lose fewer years of life than the average person in the population. We follow the approach used by Gaynor, Moreno-Serra, and Propper (2013), who estimated LYL due to changes in hospital mortality rates in England. At baseline, the average age at death among FFS patients in privatized hospitals in our sample is approximately 82 years. Using data sourced from standard life tables, we calculate that the weighted average LYL for these patients is 8.9 years (CDC 2014). This calculation adjusts for age and sex of elderly FFS hospital patients, but does not account for their likely elevated mortality risk. To arrive at a more conservative estimate, we leverage the insight from Deryugina et al. (2019) that life expectancy for elderly Medicare beneficiaries is 40% lower after accounting for comorbidities. Therefore, we arrive at an estimate of 5.3 years lost per death and a total of 18.4 LYL (3.4 x 5.3) among Medicare FFS patients per hospital privatization per year.

17. The average privatized hospital served 1,133 fee-for-service patients at baseline (Table A.4). Adjusting for a 5.2% decline in volume (Table 4 Panel A), a 0.32% increase in mortality implies 3.4 additional deaths per year.

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. For brevity, we focus on the key variables where we detect a change following privatization. Columns 1–3, 4–8, and 9 present results on hospital finances, hospital- and market-level patient admissions from the AHA, and hospital mortality, respectively. Among market-level admission outcomes, we present results only for Medicaid. The top row repeats the estimates from the baseline model without market covariates for convenience. Across all robustness checks, the models do not include market-level covariates. The results are collectively very reassuring, as the coefficients remain within two standard errors of the baseline estimates across all checks.

Panel I tests the robustness to alternate specifications. Row IA presents coefficients obtained from regressions that weight hospitals by beds.[18] This approach gives more weight to larger privatized hospitals. We do not perform this check for mortality since the baseline model uses patient-level data and already gives higher weight to larger hospitals. Row IB presents estimates from a more flexible model that includes state-by-year fixed effects. This helps compare privatized hospitals with comparison units in the same state. 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 1996–2000, prior to the first privatization in our sample. Across all three checks, the estimates remain similar to the baseline.

Panel II presents results using alternate estimators, which address the limitations of TWFE models when used in staggered treatment designs. Rows IIA and IIB report the corresponding coefficients of estimators proposed by Callaway and Sant'Anna (2020) and De Chaisemartin and d'Haultfoeuille (2020), respectively. These correct for potential biases due to staggered treatment in different ways and estimate the weighted average treatment on the treated. In addition, to alleviate concerns about the potential impact of excluding data from the year of privatization, we retain these observations when deploying these estimators. Despite these changes, the estimates are very close in magnitude to the baseline values. Because we use synthetic difference-in-differences (SDID) to estimate volume effects for private and uninsured patients in the state discharge sample, we also examine the sensitivity to using SDID to study patient admissions in the national AHA sample. In results not presented formally, we find qualitatively similar results to the baseline TWFE. For example, we estimate statistically significant decreases of 12% and 10% in Medicaid and Other volume, respectively.

Recent studies have also raised concerns about the validity of event study plots obtained using TWFE in staggered treatment settings (Sun and Abraham 2021). Therefore, in addition to presenting alternate estimates of the ATT, we also present event study plots generated by the CS estimator. Figures A.5, A.6, A.7, and A.8 present the corresponding event study plots for hospital

18. 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 2000 or the first year we observe that hospital.

finances, hospital admissions, market admissions, and hospital mortality, respectively. In almost all cases, the dynamic effects obtained using the CS estimator are indistinguishable from those obtained using the baseline TWFE model. The figures also indicate that the trends for privatized hospitals usually start to deviate from those of the comparison group in the year of privatization itself, particularly in the case of admissions.

Panel III tests the robustness to changing sample construction rules with respect to privatized units, whether hospital or market. Row IIIA assesses the importance of reducing the imbalance in the panel for privatized units. We limit the sample to privatized units 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). These changes do not qualitatively affect the results.

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 the treated and comparison units be balanced in the levels of attributes or outcomes, this imbalance could signal unobserved differences 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 units. Although differences are less noticeable at the market level (see Table A.8), we also implement equivalent matching at the market level for completeness. We use 1:1 propensity score matching to identify a single comparison hospital (market) for each treated hospital (market) without replacement. Section B.8 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 D-D coefficients obtained using the matched sample, which are qualitatively similar to the main estimates.

The matched sample also helps to better assess the protective effect of privatization against hospital closure, since the matched comparison hospitals closely resemble the privatized hospitals in the year before treatment, and we can compare the probability of closure in both groups over a consistent duration after privatization. We find that the 5-year probability of closure is also rare in the matched comparison group (1.2%), much lower than in the comparison group as a whole (3.6%). A two-sample test of difference in means fails to reject the null hypothesis that closure rates are the same in the matched treated and comparison hospitals.

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. This exercise is not relevant for the market-level analysis, as we include patient counts for all hospitals located in the market regardless of "switcher" status.

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

Second, we assess whether the changes effected by for-profit acquirers differ from those effected by nonprofits in a manner consistent with profit maximization. We find that for-profit acquirers obtain a greater increase in both total admissions and mean revenue per patient than nonprofits. The differential increase in admissions relative to nonprofits, 22 percentage points, is both statistically and economically significant. In contrast, the difference on expenses is relatively muted, suggesting that for-profits focus more on growth than on cutting expenses. Overall, mixed evidence is consistent with the conclusions of previous studies that nonprofits in the hospital sector behave similarly to for-profits (Duggan 2000).

Third, we assess whether privatization leads to a greater decrease in costs when the acquirer is a hospital system, i.e., it owns multiple facilities. Previous studies have shown that systems achieve greater cost reductions in acquired facilities by centralizing personnel, particularly in administrative and support functions (Andreyeva et al. 2024). Systems are acquirers in about 80 of the 254 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 when the acquirer is a system, supporting the hypothesis. However, the coefficients are imprecisely estimated.

6 Mechanisms

This section presents evidence on changes in hospital operations that help explain the effects on key measures of profitability and patient care described in Section 5. Specifically, we show that the private management makes three types of operational changes consistent with profit maximization and predictions from theory.

6.1 Changes in service mix

The rich administrative data from the five states 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 in Section 5.2.1 to examine the effects on admissions of private and uninsured patients.

Table 8 presents the associated results. Column 1 presents the effect on log total obstetric admissions. We find a large decrease in admissions of 53.6% after privatization (exp(-0.768)-1). This effect includes changes on the extensive margin, such as maternity ward closures, and reductions in volume on the intensive margin. We then examine these two channels separately. Column 2 presents the effect on the likelihood that an obstetric unit is closed in a given year, which we define as the obstetric share of total admissions falling to 2% or lower in a given year.[19] We find that the probability of closure increases by 13.3% after privatization, which represents an increase of 70% relative to the mean probability of closure at baseline. Column 3 presents the D-D coefficient for hospitals that are deemed to have open obstetric units throughout the sample period. The coefficient is highly imprecise, so we cannot rule out large changes in either direction. Figure A.9 presents the corresponding placebo distributions with the estimated effect for the privatized hospitals overlaid using a dashed vertical line.

Since these estimates are obtained using data from four states, we prefer to focus on their qualitative implication rather than on the magnitudes of the coefficients. Closing obstetric services may not be as important in the entire sample as in these states. However, based on this evidence, the strategy of moving away from less profitable services appears plausible as a mechanism to shift the payer mix to more lucrative payers and improve profitability. We hypothesize that the change in the mix of services may be a key channel for hospitals to disproportionately decrease admissions for low-income patients. Patient-level hospital discharge data from three states (FL, IN, and WA) make this clear in the case of obstetrics. In these states, 48% and 10% of the obstetric patients in the affected hospitals were Medicaid or uninsured, respectively, before privatization. To put these magnitudes in perspective, note that these two groups contribute 19% and 6%, respectively, to all admissions in the state data sample (see Table 4B). Hence, closing maternity wards would disproportionately affect admissions for these two groups.

How does the decrease in obstetric admissions at privatized hospitals interact with our result in Section 5.2.2 that Medicaid admissions decline at the market level? Since more than 98% of births in the U.S. occurred in hospitals during this period (MacDorman and Declercq 2019), the

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

most likely consequence of maternity ward closures is that births are reallocated to other hospitals in the market. Therefore, we expect that the decrease in Medicaid admissions at the market level is driven by non-obstetric cases. The AHA reports total hospital admissions across all payers related to births, and we use this variable to partially test this hypothesis. Using our market-level research design, we find that total births in a treated HSA are not affected by privatization. In contrast, we detect a statistically significant decrease in non-birth admissions.[20] Unfortunately, we cannot perform this analysis specifically using aggregate Medicaid births and non-births due to data limitations. However, these patterns imply that the decrease in hospital care at the market level is borne by patients admitted for other services.

We conclude this section with a back-of-the-envelope calculation to assess the importance of the changes in the payer mix to the 6% increase in mean reimbursement per patient reported in Section 5.1. Armed with the estimated effects on admissions by payer and the corresponding average reimbursement rates from MEPS, we quantify the impact of cream-skimming more lucrative payers on mean revenue per inpatient stay, assuming all else remains equal. Table A.7 Panel A columns 4–6 summarize these calculations. We apply the estimated percent effects on volume corresponding to Medicare, Medicaid, and Other obtained using the AHA sample and presented in column 4 to the baseline patient shares in column 2 and obtain the predicted patient shares following privatization (column 5). Analogously, we use the estimated percent effects on private, uninsured, and miscellaneous volume obtained using the states sample to predict their shares of "Other" 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, changes in the payer mix collectively predict an increase in mean reimbursement per patient from approximately \$12,560 to \$12,770, an increase of 1.7%, which is approximately 30% of the increase in mean revenue per patient reported previously.

6.2 Billing practices

This section tests whether the hospital changes billing practices under new management. We first test for an increase in prices. Managers employed by the government may inefficiently set low prices due to agency problems or the cushion of a soft budget. Alternatively, they may optimally set lower prices than a private entity would because their objective is to maximize patient volume, not revenue. Hospitals negotiate prices with private insurers, and we expect the new private management to negotiate more aggressively and get higher rates. We would ideally like to test the effect on prices charged to private insurers. However, these data have historically been difficult to obtain for researchers, and we do not have access to them.

We observe hospital list prices or "charges" for Medicare FFS patients and test if these increase after privatization. Although list prices do not affect standard Medicare reimbursement rates, increasing them is an effective strategy to increase hospital prices for several types of pa-

20. We estimate a statistically significant decrease of 3% in total non-birth admissions (p-value 0.04) in affected markets after privatization. The corresponding effect on total births is a 0.4% statistically insignificant decrease (p-value 0.94).

tients. Private insurers routinely negotiate reimbursement rates with hospitals as a fraction of the charges billed for the stay (Weber et al. 2021). Previous studies have documented that the share of commercial insurance spending based on list price contracts during this period ranged from approximately 20% (Cooper et al. 2019) to 50% (Dorn 2024). Medicare "outlier" payments for very costly stays increase one-for-one with an increase in list prices (Gupta, La Forgia, and Sacarny 2024). Finally, patients who are not insured or receive care out-of-network are also billed the list price (Bai and Anderson 2016). Hence, an increase in list prices is strongly indicative of a broader effort to increase transaction prices.

Table 9 column 1 presents the estimated effect on log charges and implies an increase of about 6.5% in the average list price. Figure 7 Panel (a) presents the corresponding event study plot which shows a flat pre-trend with a clear increase after privatization that grows in magnitude during the follow-up period.

We cannot directly estimate the contribution of an increase in the list price to the increase in mean reimbursement presented in Section 5.1, since we do not observe transaction prices. However, we can provide a range using assumptions based on the previous literature. These calculations are summarized in Table A.7 Panel B. We apply the estimated increase in list prices to the share of hospital revenue contributed by patients affected by a change in list prices, assumed to include private insurance, uninsured, and miscellaneous categories. At baseline, these groups account for 35% of patient volume and 37% of total revenue. Columns 1 and 2 show that the increase in list prices, holding payer shares *fixed*, implies an increase in mean reimbursement ranging from 0.5 to 1.2 percentage points, depending on the share of commercial insurance contracts based off list prices.

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

Unlike private insurers, Medicare and Medicaid set prices unilaterally that are not usually linked to the hospital's management. However, hospitals may use a practice known as upcoding to increase the mean reimbursement from these insurers. This involves documenting more comorbidities for patients so that they can be billed in a higher-paying category (known as a DRG). We begin by testing for an increase in the average amount (in logs) billed to Medicare for the FFS patients in our sample. Table 9 column 2 presents the D-D coefficient, which is small and statistically insignificant. Consistent with this result, the event study plot in Figure 7 Panel (b) indicates that there is no change in the mean payment amount. In results not presented here, we also directly test for a change in the mean DRG "weight" billed to Medicare, which determines the payment amount. We also do not detect an effect here. Taken together, these results argue against the use of upcoding following privatization.

Alternate event study plots using the CS estimator are similar, shown in Figure A.10 Panels (a) and (b). Similarly, the coefficients are robust against the same specification and sample construction checks presented in Section 5.4, as seen in Table A.11 columns 1 and 2.

6.3 Care inputs

The section investigates two specific operational changes that help explain the deterioration in quality of care for Medicare FFS patients reported in Section 5.3.

6.3.1 Length of stay

Hospitals can leverage gray zones in clinical guidelines and discharge patients sooner to reduce operating costs. We test this hypothesis by examining the effect on the duration of hospitalization, controlling for observed changes in patient demographics and clinical risk. Table 10 column 1 examines the effect on log length of stay. The estimate implies a statistically significant decrease of 1.7%. Column 2 shows that patients are now 0.75 percentage points, or 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 shorter admissions accounts for about half the estimated decrease in length of stay. Figure 8 Panels (a) and (b) present the event study plots, which corroborate the estimated D-D coefficients. In sum, hospitals discharge Medicare FFS patients faster after privatization, reflecting a change in treatment protocols. Figure A.10 Panels (c) and (d) presents the corresponding event study plots obtained using the CS estimator. Table A.11 columns 3 and 4 present the associated robustness checks.

6.3.2 Staff availability

We now investigate the effect of privatization on hospital labor inputs. Table 2 shows that personnel spending accounts for slightly more than half of the total operating cost for hospitals, regardless of the type of ownership. Hence, reducing labor inputs offers a salient path to significant cost savings. We use data on hospital staff from AHA annual surveys for this exercise, which allows us to observe full-time equivalent (FTE) employment for coarse staff categories. Staff availability has been demonstrated as a key leading indicator of hospital quality in the literature (Friedrich and Hackmann 2021; Silver 2021). Note that there could be other changes to staffing that are not observed in our data but are also relevant to quality of care. For example, private management could put in place new compensation systems and incentives that lead to greater turnover of the staff. Alternatively, even if the number of staff employed remains unchanged, there could be a shift in the composition of workers within the same occupation toward lesser experienced or lower skilled individuals.

We observe FTE employed staff in three categories: physicians, nurses, and a residual group that we refer to as "Other." These groups represent 2%, 27% and 71%, respectively, of the total staff at privatized hospitals at baseline. The nurse category includes registered nurses (RN) and licensed practical nurses (LPN), but does not include nurse practitioners, nurse aides, or nurse assistants. The AHA folds the latter groups into the residual Other group. The relative coarseness of the AHA data is an unfortunate limitation in assessing which occupations may be affected by privatization and interpreting the results of this analysis.

To partially overcome this limitation, we collect data from the BLS, which sheds light on the national labor shares of different occupations in general medical and surgical hospitals in 2019. We separately observe the occupation shares at local government and private hospitals and present the values in Table A.12.[21] We organize the table so that the top two rows correspond to the physician and nurse categories of the AHA. Reassuringly, the AHA and BLS data closely correspond on labor shares. The remaining rows help disaggregate the other group of the AHA. We also mention the names of the two largest sub-occupation categories by labor share in each row. These data imply that more than half of the employees in the other group perform patient care roles. This includes roles in healthcare practitioner and technical (e.g., therapists), healthcare support (e.g., nursing assistants), and community and social services (e.g., social workers).

There are some notable differences in the shares of key occupations between local public and private hospitals. For example, physicians contribute 3.6% of the workforce at government hospitals versus 2.9% at private facilities, nearly 25% more. This difference is mainly due to the fact that public hospitals have a greater number of family and internal medicine physicians. In contrast, nurses (as defined by the AHA) constitute a smaller share of the average government hospital's employee base. If the new private management would like to shift the employee composition toward that of a private hospital, we would expect a decrease in physicians, but not in nurses. Among the occupations falling into the other group, government hospitals have greater shares of roles that directly affect patient care (e.g., nurse practitioners and social workers), but also those that are more clerical in nature (e.g., office and administrative support). Therefore, the effects of reductions in this group on patient health cannot be easily interpreted.

With this brief background, we discuss our results. Table 11 presents the estimated effects on FTE staff employed per 100 contemporaneous beds. This measure includes both full-time and part-time employees. We scale FTE counts by the number of beds to decrease heterogeneity and skewness in counts across hospitals. Column 1 presents the effect on the total staff. We find an economically meaningful reduction in total employment of 33 FTE per 100 beds. Compared to the mean level at baseline, this implies a decrease of 6% in the employed staff, essentially identical to the estimated decrease in personnel expenses per bed reported in Section 5.1. This congruence suggests that the decrease in personnel spending is largely driven by a decrease in employment. In results not summarized here, we estimate a statistically insignificant 3% decrease in mean salary expense per employee at privatized hospitals. This is not surprising since government hospital employees had lower mean salaries at the beginning of the sample period relative to their coun-

21. This is the industry-occupation matrix data from the BLS, available for 2023 at https://www.bls.gov/emp/tables/industry-occupation-matrix-industry.htm. We use the internet archive to get the corresponding data files for 2019, the last year of our sample. The earliest data available is for 2016, so we cannot document occupation shares as of 2000. We use the tables for general medical and surgical hospitals local and private, respectively.

terparts in private hospitals.[22] Since the mean number of beds at baseline for privatized hospitals is 93, our estimated effect implies a decrease of 31 FTE at the average hospital during the five years after privatization.

We also examine the effects on FTE in each of the three labor categories observed in the AHA. Consistent with the conjectures stated above based on the occupation shares data, we do not detect a reduction in nurse FTE but a relatively large decrease of 25% in physician FTE. A decrease in physician FTE of this magnitude would make the share of physicians comparable to that observed at private hospitals. In absolute terms, the main reduction in FTE is driven by the other group. This category represents 70% of the total FTE but contributes nearly 90% of the estimated decrease. Figure 9 presents the event study plots corresponding to each of these outcomes. The dynamic coefficients are consistent with the D-D estimates presented in Table 11. There is a noticeable decline in total, physician, and other FTEs per 100 hospital beds in the year following privatization, and it persists over the next five years.

A possible confounder in this analysis is if private management prefers to use contracted staff instead of full-time employees. In that case, what appears as a reduction in FTE may just represent a change in the nature of the contract without a decrease in care inputs. Therefore, we also test for an increase in the use of contract labor after privatization. We obtain data on contracted FTE at the hospital from HCRIS. The corresponding result is presented in Table 11 column 5. The coefficient is close to zero and statistically insignificant. We can rule out an increase in contract staff of more than 2.9 FTE per 100 beds (0.1 + 2 x 1.4), which would offset less than 10% of the estimated decline in employment. We conclude that private management truly decreases labor inputs.

The results on staff availability are not sensitive to scaling the FTE by the number of patient admissions instead of beds. We present the corresponding results in Table A.13. These coefficients imply relatively similar changes to the main estimates. For example, we detect a reduction of 0.54 FTE per 100 admissions, which represents a 7% decrease. The effects for the different components also align well with those in the main set of results. Hence, there is a decrease in staff availability regardless of whether we benchmark against hospital bed capacity or patients served. Figure A.11 presents an alternate set of event study plots using the CS estimator that are consistent with the TWFE plots. Table A.14 presents the results from the usual set of robustness checks and shows that the coefficients are not sensitive to a variety of changes.

7 Discussion

This section ties together the estimated effects on hospital finances 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 potentially important channels not captured in the empirics. For example, privatization likely substantially reduces future pension obligations for the local

22. Table 2 indicates that the mean personnel expense per FTE in privatized hospitals in 2000 was $55,500. In contrast, in private hospitals it was $66,100.

government, but we cannot quantify this benefit. Since we do not study how local governments use the savings generated from privatization, we cannot account for potential benefits from the alternate use of these funds. Similarly, our empirical approach cannot estimate the cost of reduced access to care for Medicaid beneficiaries. For convenience, we explain the computation of each cost and benefit amount in Table A.15.

The average privatized hospital in our sample had a deficit of $1.7 million in the year before privatization (see Table A.15 Panel B). Our results imply that this deficit is eliminated by privatization and forms the core of the financial benefit to the local government. Privatization also generates tax revenue in 28% of the cases where the hospital is acquired or run by a for-profit firm. Following Rosenbaum et al. (2015), we apply a nonfederal tax rate of 2.1% of revenue to estimate that incremental tax revenue. Net savings for the government from the average privatization including incremental tax revenue is $2 million per year and is our central estimate of the benefit. We also consider an upper bound estimate, assuming that the entire increase in hospital surplus flows to the government in 56% of the deals where the private partner has less control. This is unlikely to satisfy the private partner's participation constraint, hence we consider it an upper bound. This increases the benefit amount to $4.3 million.

Next, we estimate the mortality cost incurred in terms of the lives or life-years lost due to privatization. As described in Section 5.3, we estimate 3.4 additional deaths and 18.4 LYL for FFS patients 65 years and older. This is a conservative estimate in multiple ways. Prior studies have often considered the effect on one-year mortality in such cost-benefit analyses (Doyle Jr et al. 2015), which would double the number of additional deaths implied by our estimates. However, we use the effect on 30-day mortality as our baseline estimate. Furthermore, this analysis does not consider the effects on Medicare beneficiaries enrolled in private Medicare Advantage plans. We do not observe hospital stays for these patients and therefore cannot directly estimate the effects for this group.

Our central estimate of the net savings to the government from privatization is approximately $0.6 mn per death (2 / 3.4) or $110,000 per LYL. The upper bound estimate includes surplus revenue and is about $1.26 mn per death or $236,000 per LYL. Unfortunately, there is little consensus on the appropriate VSL or VSLY estimate against which to benchmark these estimates. Previous studies often cite a benchmark VSLY of $100,000 proposed by Cutler (2004), approximately $150,000 in 2019 dollars. In contrast, federal agencies use higher reference values in their cost-effectiveness assessments of new policies. For example, the Department of Health and Human Services (HHS) stipulates a VSL of approximately $10 mn and a VSLY of $369,000 (HHS 2017; Kniesner and Viscusi 2019). Our results imply that hospital privatization may compare favorably with the standard set by Cutler (2004) but is unlikely to meet the thresholds used by HHS.

8 Conclusion

Amid renewed debates over improving efficiency in government, privatization is a promising solution, but it can harm some stakeholders. This trade-off assumes greater significance in the case of hospital care, which has unique challenges and has experienced substantial privatization in the U.S.. However, this phenomenon has been largely ignored by researchers. We provide novel evidence from the privatizations of all 254 nonfederal government hospitals in the U.S. over 2001–2018. We confirm that privatization improves hospital profitability sufficiently so that hospitals transition from loss-making to generating a modest surplus. The main channel to improve profitability is to increase the mean revenue per bed. Privatization therefore generates savings for state and local governments.

However, the improvement in finances comes partly at the cost of reduced access to hospital care for low-income patients who are often unprofitable for hospitals to serve. We show that hospitals disproportionately reduce admissions of low-income Medicaid and uninsured patients after privatization. We also detect a decline in aggregate Medicaid admissions at the market level, which implies that other hospitals do not offset the loss of government hospital capacity. In addition to a decrease in access, we also find evidence of a decrease in quality of care in the form of higher mortality rates among elderly Medicare FFS patients. Our estimates imply that, on average, the savings generated per life-year lost range between $110,000-236,000.

Several avenues remain for future research on this topic. Although we document changes in some service lines and an increase in mortality rates, more investigation is warranted on changes in admission practices and other dimensions of hospital quality, particularly for non-Medicare patients. Researchers with access to all-payer claims data, perhaps focused on narrower geographies, can make progress on these questions. More evidence is also needed on how local governments deploy the savings generated by privatization and the resulting benefits for local residents. These inputs are needed for a comprehensive welfare analysis of privatization. Although our results imply that Medicaid coverage in its present form does not adequately substitute for government care delivery, they do not rule out the possibility that a reformed Medicaid program, for example, one that offers higher reimbursements, could do so (Alexander and Schnell 2024). Additional research using individual-level longitudinal earnings and employment data could shed light on the short- and long-term effects for employees of public hospitals just prior to the privatization.

Finally, we hope that this work spurs similar investigations in other sectors. The qualitative conclusions of this study are relevant to the privatization of other loss-making government services that involve similar trade-offs between improving efficiency and serving vulnerable groups of society.

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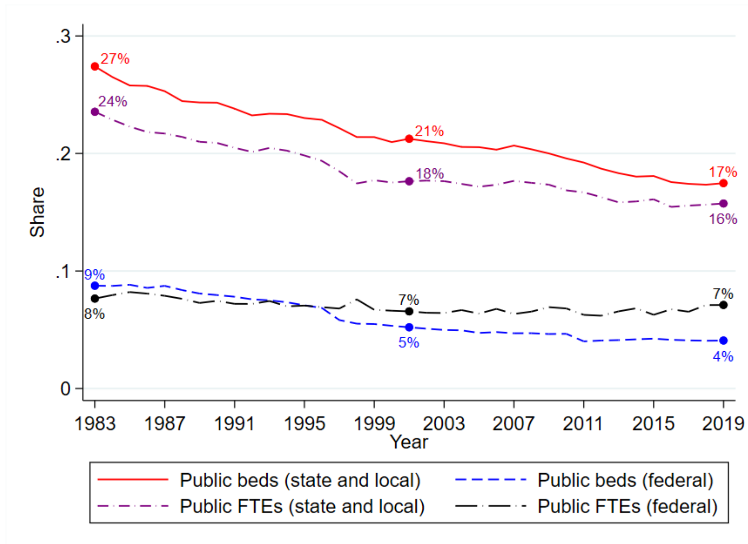
(a) As provider



(b) As payer

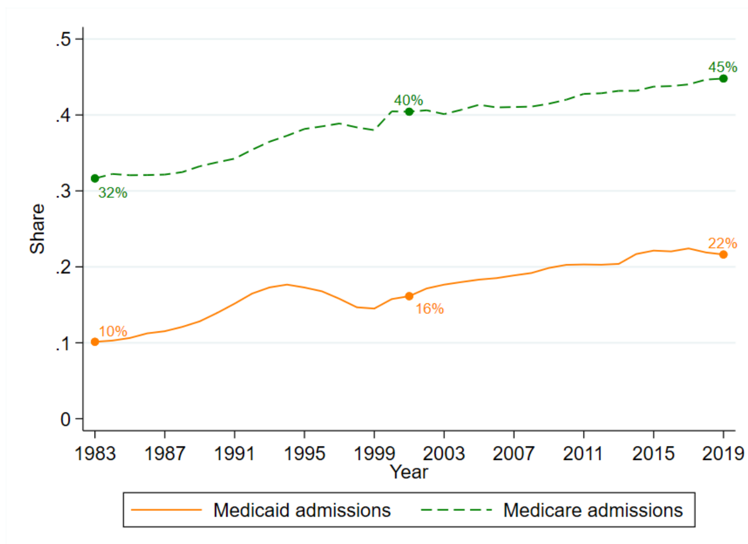
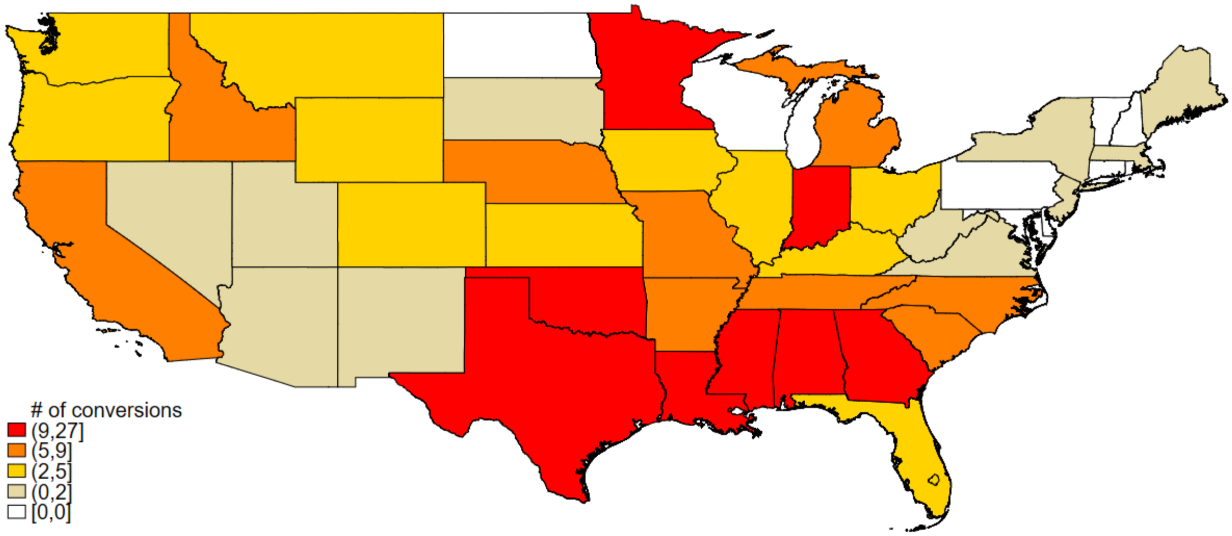


Figure 1: Government role in hospital care

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

(a) By state



(b) By year

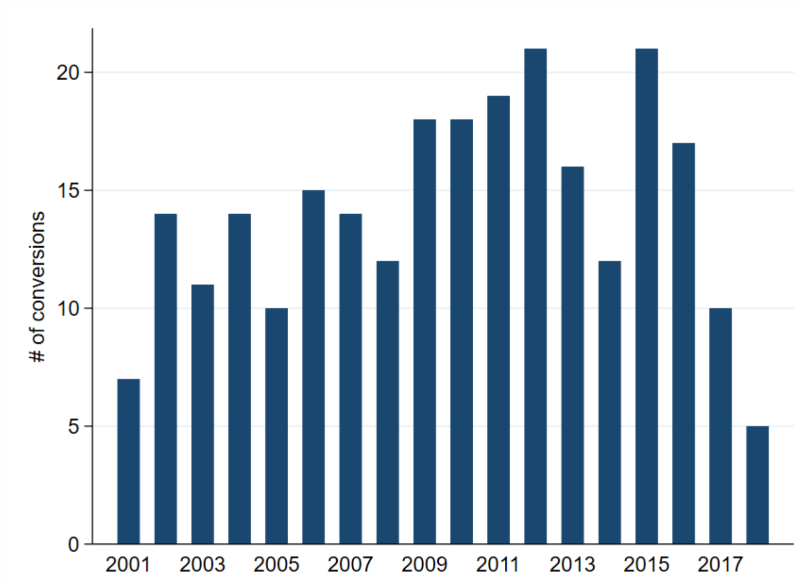


Figure 2: Privatizations

Note: The figure presents the distribution of nonfederal, public-hospital privatizations in our final analysis sample during 2001–18. We restrict the sample to general-acute-care hospitals. Panels (a) and (b) present the distribution by state and by year, respectively. Hawaii and Alaska are not pictured in Panel (a) but are included in the sample and experienced 4 and 1 conversions, respectively.

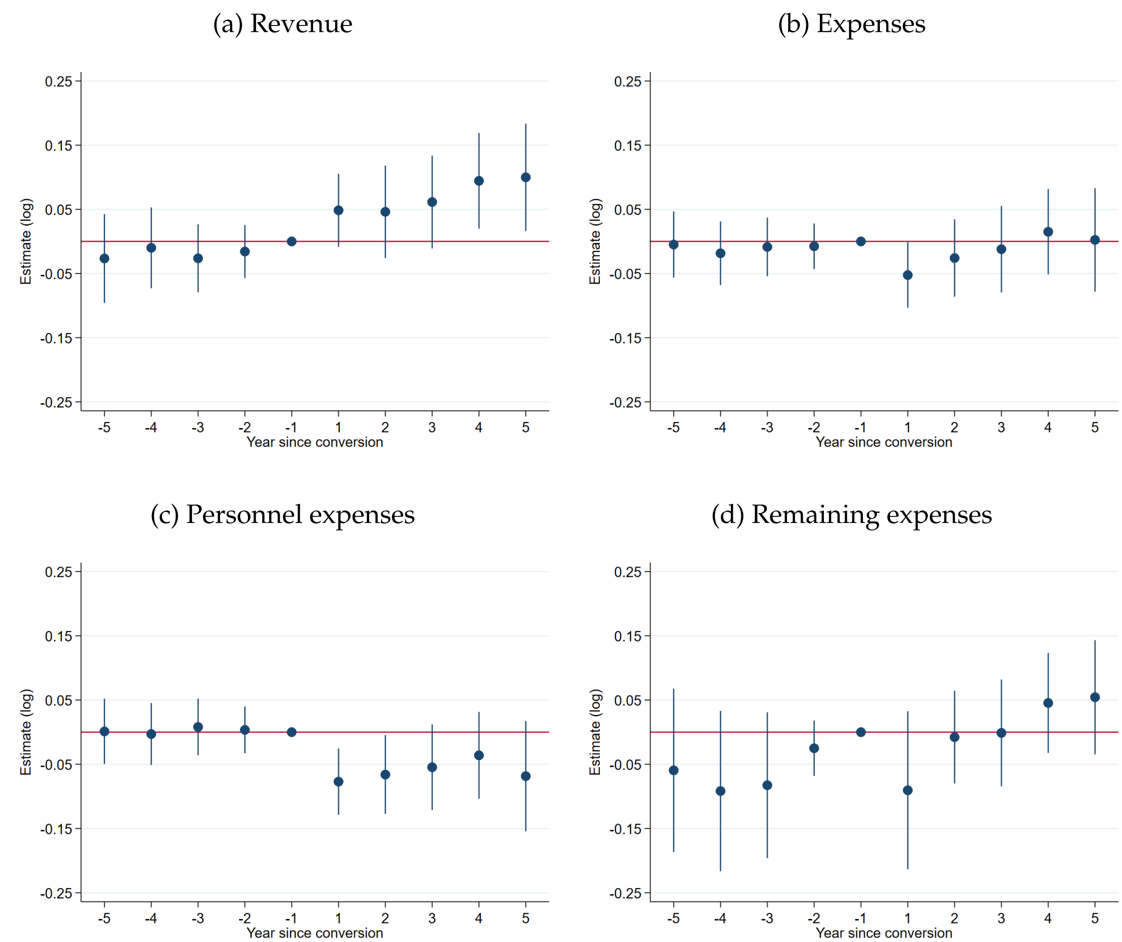


Figure 3: Effects on (log) finances per bed

<u>Note:</u> The figure presents event study plots obtained by estimating Equation 2 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 revenue (from Medicare cost reports) and expenses (from AHA), respectively. 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. 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 presents the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

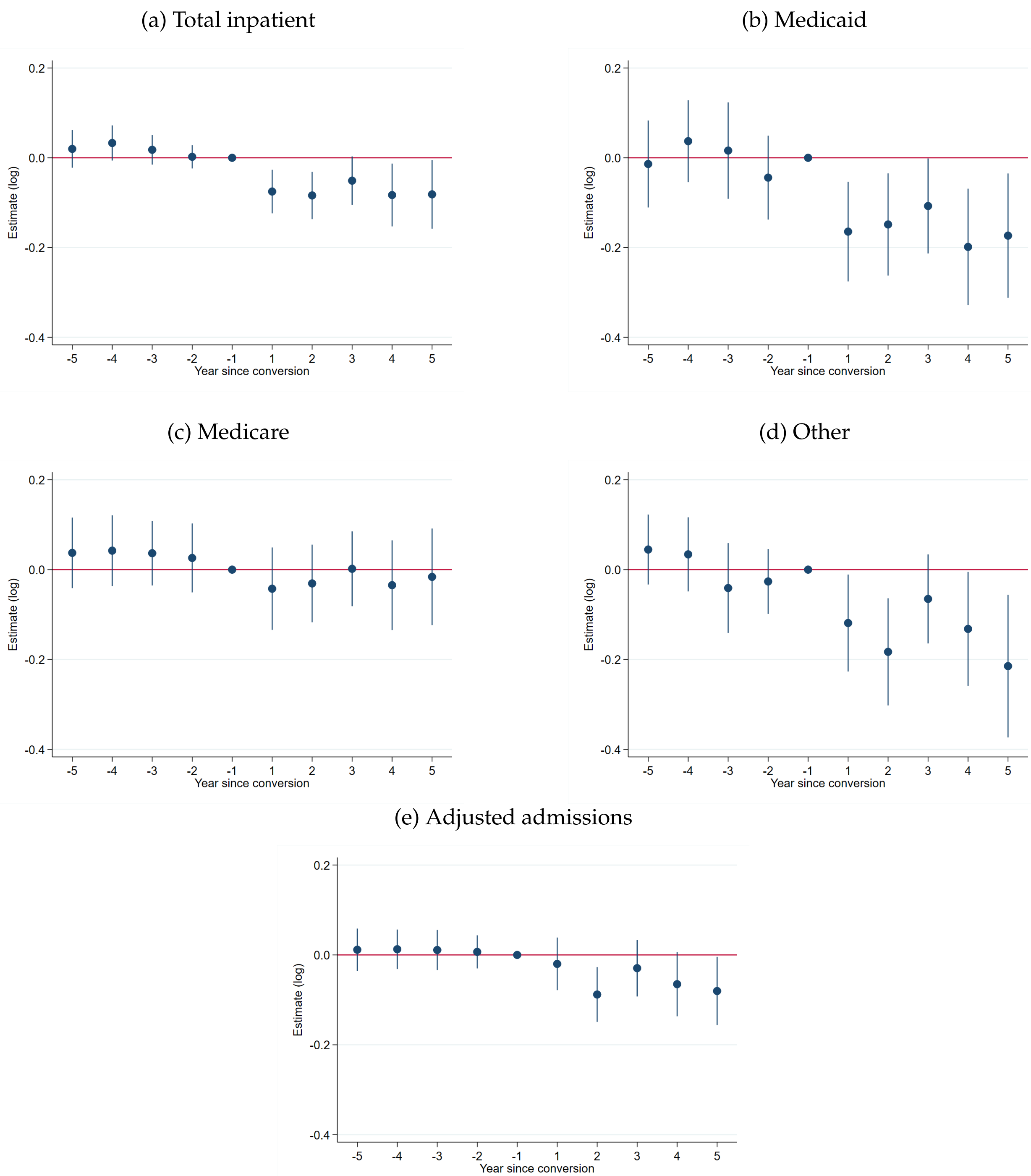


Figure 4: Effects on patient (log) volume

Note: The figure presents event study plots obtained by estimating Equation 2 on hospital-year level patient volume data from the AHA. The comparison group consists of hospitals that remain under government control throughout our sample period. The outcomes are log total inpatient, Medicaid, Medicare, and "Other" admissions in Panels (a), (b), (c), and (d), respectively. Other admissions refers to hospital admissions not covered by Medicaid or Medicare and mainly comprises privately insured and uninsured patients. Panel (e) presents the effect on adjusted admissions, which include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Therefore, it approximates total hospital care volume. 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.6 presents the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

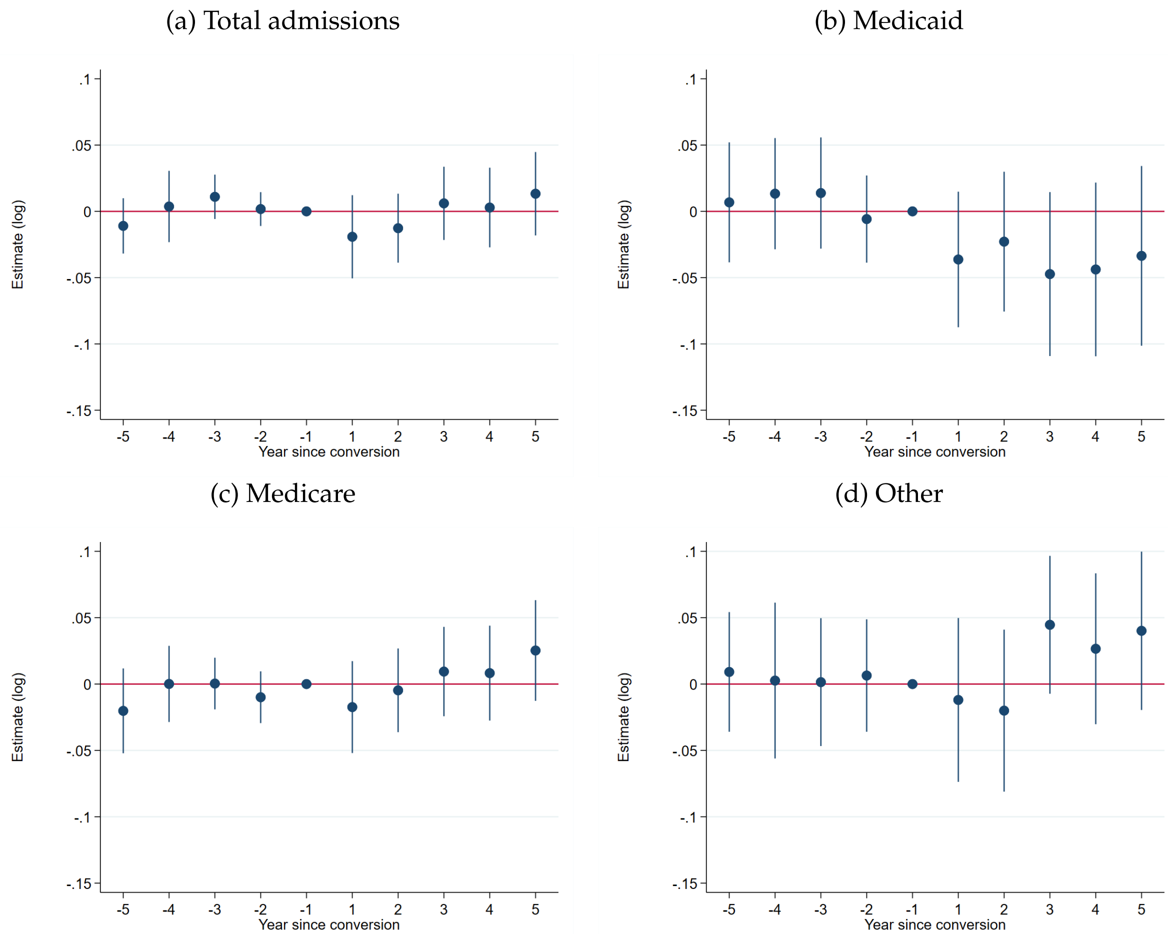
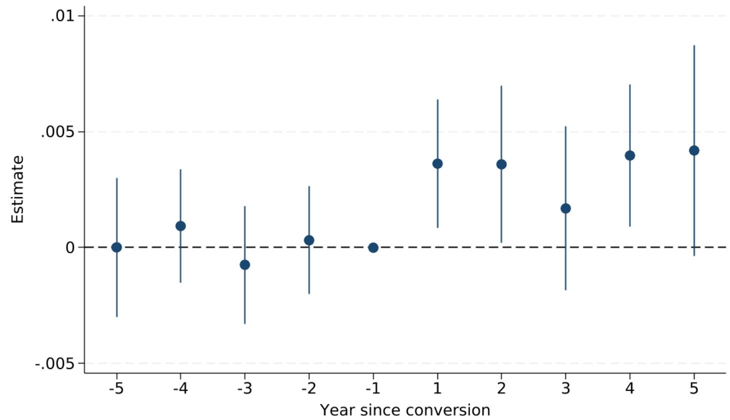


Figure 5: Effects on market-level (log) volume

Note: The figure presents event study plots obtained by estimating the market-level equivalent of Equation 2 on market-year level data. We define hospital markets using health service areas (HSA), described in Section 5.2.2. The outcomes are log total, Medicaid, Medicare, and Other admissions in Panels (a), (b), (c), and (d), respectively. “Other” admissions refers to hospital admissions not covered by Medicaid or Medicare and mainly comprises privately insured and uninsured patients. Year zero is the year a market experiences a privatization for the first time and is excluded for treated markets since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by HSA. Figure A.7 presents the corresponding event study plots obtained using the Callaway-Sant’anna estimator on a sample that includes data from year zero.

(a) All 65+ patients



(b) Nondeferrable 65+ patients

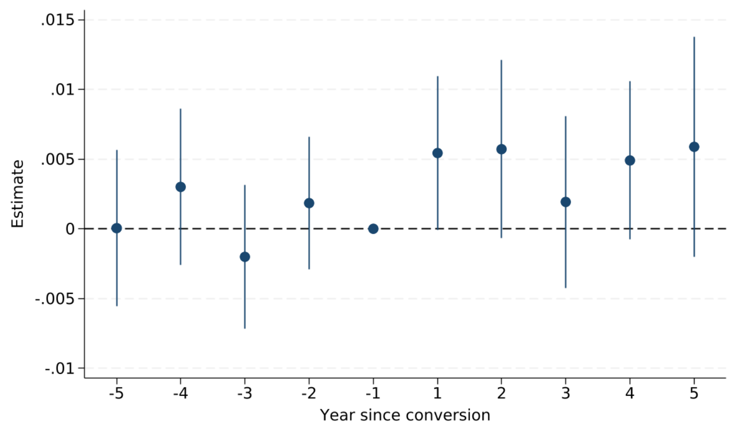
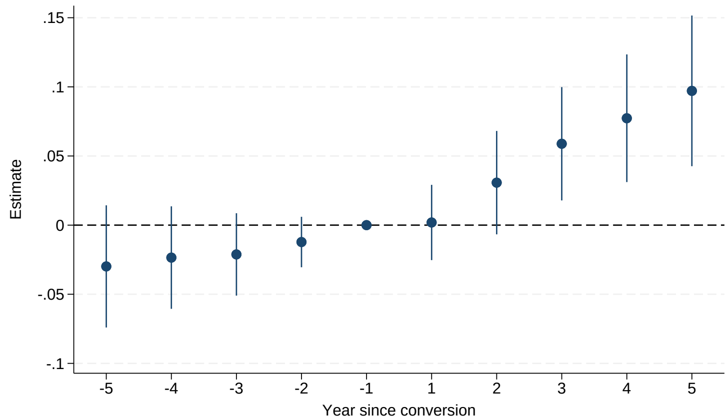


Figure 6: Effects on hospital mortality rates

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. Consistent with our research design, we exclude 51 hospitals that privatized prior to 2005 for this analysis to ensure that we observe at least five pretreatment years for each privatized facility. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The comparison group consists of hospitals that remain under government control throughout our sample period. Patients are aged 65–99 and enrolled in Medicare Parts A and B for at least 3 months prior to admission. The outcome is probability of death within 30 days of discharge from the hospital. Panel (a) presents the effects for all FFS patients regardless of condition, while Panel (b) presents the results specifically for patients admitted through the emergency department for nondeferrable conditions. The latter group is considered less susceptible to selection concerns and is identified following the approach in Doyle Jr et al. (2015). The model includes a vector of patient demographics and risk attributes, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure A.8 presents the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

(a) Log charges



(b) Log payment

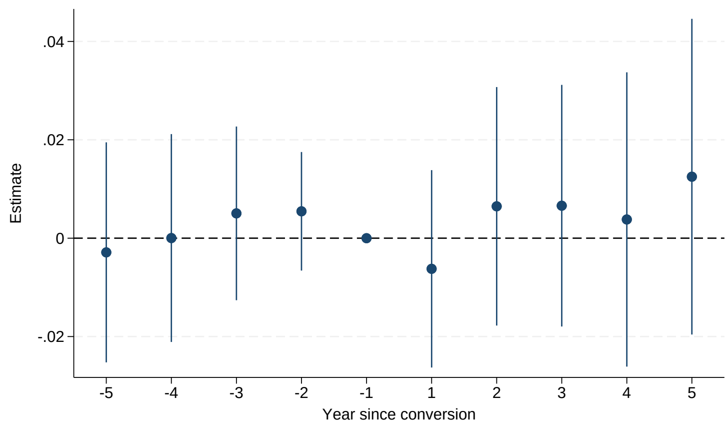
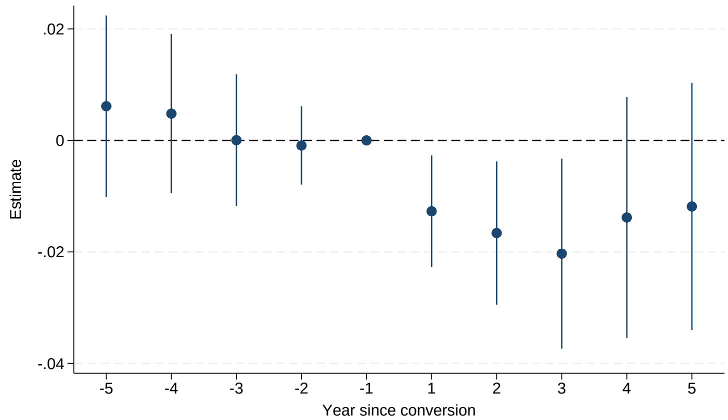


Figure 7: Effects on billing practices

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. Consistent with our research design, we exclude 51 hospitals that privatized prior to 2005 for this analysis to ensure that we observe at least five pretreatment years for each privatized facility. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The comparison group consists of hospitals that remain under government control throughout our sample period. Patients are aged 65–99 and enrolled in Medicare Parts A and B for at least 3 months prior to admission. The outcomes are: (a) log of hospital charges or list price for the stay and (b) log payment amount for the stay. Both variables are deflated to 2019 dollars. The models include a vector of patient demographics and risk attributes, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure A.10 Panels (a) and (b) present the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

(a) Log Length of stay



(b) Pr(stay $<$ 2 days)

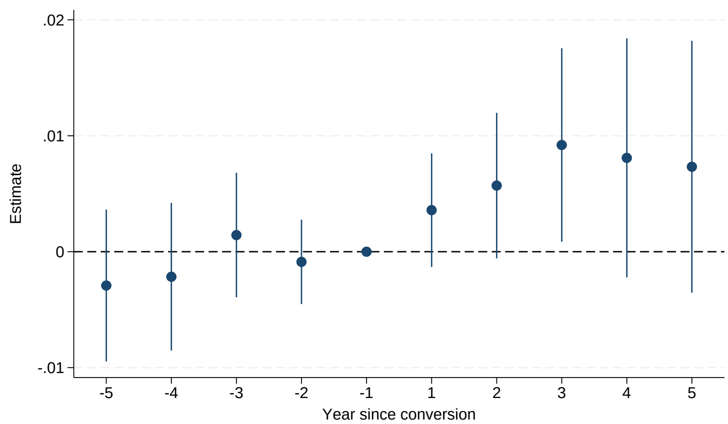


Figure 8: Effects on length of stay

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. Consistent with our research design, we exclude 51 hospitals that privatized prior to 2005 for this analysis to ensure that we observe at least five pretreatment years for each privatized facility. Year zero is the year of privatization and is excluded for treated hospitals since it represents partial treatment. The comparison group consists of hospitals that remain under government control throughout our sample period. Patients are aged 65–99 and enrolled in Medicare Parts A and B for at least 3 months prior to admission. The outcomes are: (a) log length of stay and (b) an indicator for discharging the day of admission or the next day. The models include a vector of patient demographics and risk attributes, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure A.10 Panels (c) and (d) present the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

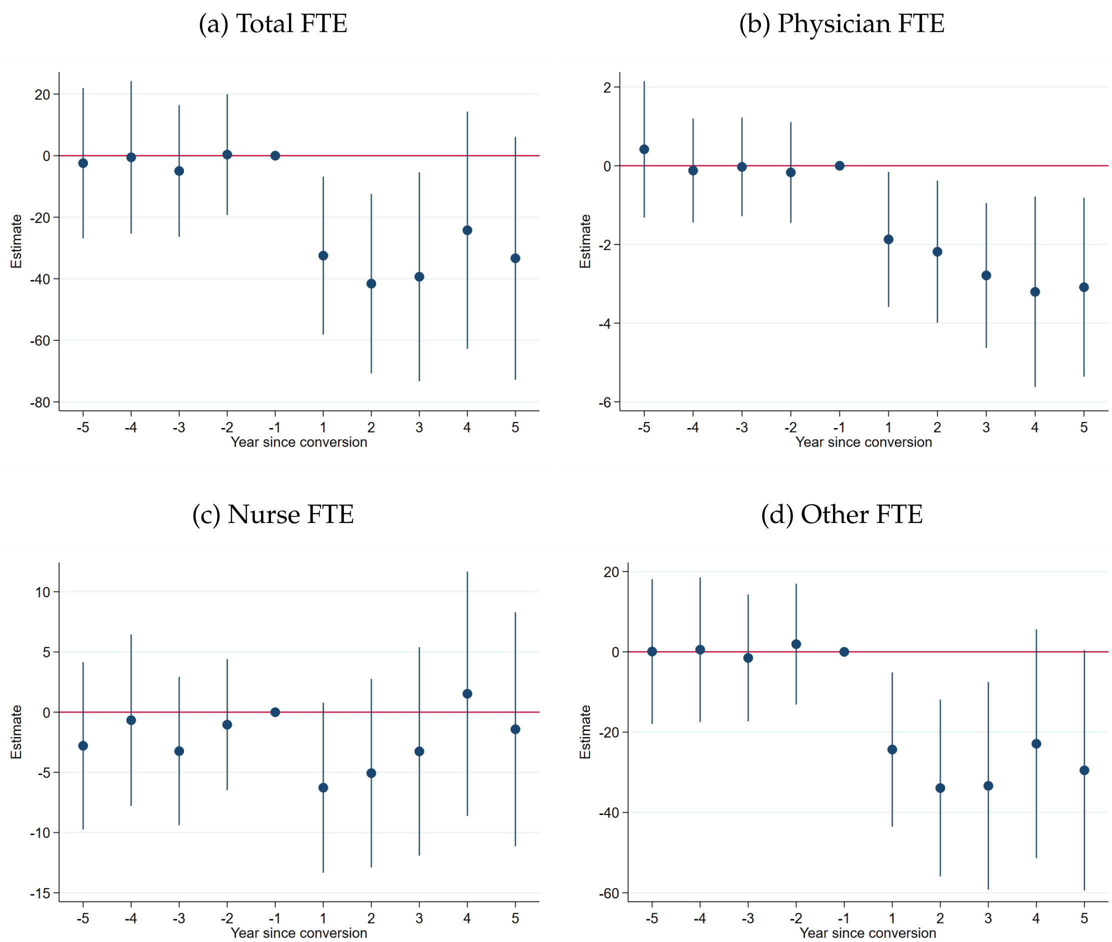


Figure 9: Effects on staff FTE per 100 beds

Note: The figure presents event study plots obtained by estimating Equation 2 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period. Outcomes are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other (all remaining) FTEs in Panels (a), (b), (c), and (d), respectively. All outcomes are normalized by the contemporaneous number of hospital beds and presented per 100 beds. Year zero is the year of privatization and is excluded for the treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital. Figure A.11 presents the corresponding event study plots obtained using the Callaway-Sant'anna estimator on a sample that includes data from year zero.

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

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

Notes: The table presents shares of hospital beds by type of ownership for select large states using American Hospital Association survey data (AHA) from 2019. We source this information using the "control" variable in the AHA and use the terms owner and control interchangeably since they are identical in most cases. The states are ordered in descending order of nonfederal public share, which is the top row. The states are selected to illustrate the range in shares of hospitals under different types of ownership. Appendix Table A.1 lists public (nonfederal) hospital bed shares for all states. Non-general-acute-care hospitals were included in the sample for share calculations. Column 7 shows mean shares for the overall U.S.; 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	21.5
% For-profit	0.0	0.0	21.0	16.5
% Non-profit	0.0	0.0	79.0	62.1
Beds	93	117	186	170
	(98)	(166)	(180)	(177)
Admissions	3,210	4,055	7,659	6,843
	(4,485)	(6,803)	(7,935)	(7,778)
% Medicaid adm	16.0	15.6	13.7	14.1
	(9.6)	(10.5)	(9.0)	(9.3)
% Medicare adm	49.5	49.3	46.3	46.9
	(13.9)	(15.8)	(13.3)	(13.8)
% Other adm	34.6	35.1	40.1	39.0
	(12.1)	(12.4)	(13.8)	(13.7)
Total revenue/bed	409,885	405,658	688,281	628,236
	(246,748)	(320,193)	(1,449,804)	(1,298,690)
Total expenses/bed	407,904	429,755	600,352	562,631
	(221,442)	(306,231)	(337,865)	(335,669)
Personnel expenses/bed	218,738	229,673	302,895	286,625
	(116,092)	(160,134)	(162,582)	(163,101)
Total FTEs/100 beds	394.4	398.0	458.5	445.4
	(167.1)	(204.9)	(201.3)	(201.9)
# Hospitals	254	802	3,867	4,923

Notes: The table presents descriptive statistics on the cross-section of hospitals in the AHA analysis sample as of 2000. In rare instances in which we do not observe a hospital in 2000, we use values from that hospital's first year in the data. Appendix B.2 describes the sample construction restrictions in detail. Column 1 describes government hospitals that privatized during the sample period. These comprise the treated units. Column 2 describes the comparison group, government hospitals that did not experience a change in ownership during this period. Column 3 describes all privately owned, nonprofit and for-profit hospitals that were not converted to government control during this period. Column 4 presents the corresponding values for the entire sample. "Other" admissions refers to hospital admissions not covered by Medicaid or Medicare and mostly comprises privately insured and uninsured patients. Total revenue is sourced from the Medicare cost reports (HCRIS) and is the only outcome variable in the table that is not sourced from the AHA. Standard deviations are shown in parentheses.

Table 3: Effects on (log) finances per bed

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
A: No controls				
DD	0.083	-0.009	-0.063	0.045
	(0.032)	(0.029)	(0.029)	(0.045)
Obs	16,829	16,829	16,829	16,829
B: Market controls				
DD	0.116	0.017	-0.036	0.066
	(0.032)	(0.029)	(0.029)	(0.045)
Obs	16,816	16,816	16,816	16,816
Mean outcome (t-1)	650,670	668,767	356,995	311,772

Notes: The table presents effects on revenue and expenses at the privatized hospitals, obtained by estimating Equation 1 on hospital-year level data. All outcomes are normalized by contemporaneous hospital beds and presented in logs. 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 one year after the start of our AHA sample and is missing for some hospitals, we drop any hospital-year observations with missing values for total revenue, which allows for the same sample across outcomes. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying hospital and county-level controls as described in Section 4. The mean values pertain to outcomes (in levels) at privatized hospitals in the year before privatization. Standard errors are clustered by hospital and are presented in parentheses. Table A.5 presents the corresponding point estimates obtained when we normalize outcomes by the contemporaneous number of adjusted admissions instead.

Table 4: Effects on patient (log) volume

A: AHA	(1) Total	(2) Medicaid	(3) Medicare	(4) Other	(5) Adjusted		
A1: No controls							
DD	-0.089	-0.156	-0.053	-0.142	-0.063		
	(0.028)	(0.043)	(0.030)	(0.044)	(0.026)		
Obs	20,387	20,386	20,386	20,386	20,387		
A2: Market controls							
DD	-0.101	-0.179	-0.078	-0.142	-0.070		
	(0.028)	(0.042)	(0.031)	(0.044)	(0.026)		
Obs	19,559	19,558	19,558	19,558	19,559		
Mean outcome (t-1)	3,038	622	1,361	1,054	7,087		
B: States	(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

Notes: The table presents estimated effects on patient volume in privatized hospitals obtained by estimating Equation 1 on hospital-year-level data. Panel A presents results using AHA data. Columns 1, 2, 3, and 4 present the effects on log total, Medicaid, Medicare, and other admissions, respectively. "Other" admissions refer to hospital admissions not covered by Medicaid or Medicare. Panel A1 reports coefficients from a two-way fixed-effects specification without covariates. Panel A2 reports coefficients from a specification that includes time-varying hospital and county-level covariates described in Section 4. Panel A2 has fewer observations since the market-level covariates are not available for 1996. Panel B presents results using data from five states (CA, FL, IN, MN, and WA) on inpatient volume . We estimate synthetic difference-in-differences models using the "sdid" command with placebo inference using 200 replications. In Panel B, we also disaggregate "Other" into three groups: privately insured, uninsured, and miscellaneous (e.g., workers compensation), respectively. 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 market-level (log) volume

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: No controls				
DD	-.004	-.042	.009	.010
	(.015)	(.025)	(.016)	(.022)
Obs	19,288	19,288	19,288	19,288
B: Market controls				
DD	-.021	-.056	-.011	-.011
	(.015)	(.024)	(.016)	(.022)
Obs	18,555	18,555	18,555	18,555
C: Heterogeneity by market HHI				
DD	.042	.024	.048	.064
	(.016)	(.022)	(.017)	(.017)
x 1(> med. HHI)	-.093	-.134	-.079	-.110
	(.027)	(.046)	(.030)	(.042)
D: Heterogeneity by market poverty				
DD	.023	.038	.034	.015
	(.022)	(.032)	(.021)	(.032)
x 1(> med. poverty)	-.053	-.158	-.050	-.010
	(.027)	(.045)	(.030)	(.043)
Mean outcome (t-1)	40,699	7,838	16,904	15,957

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 5.2.2. 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 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 2000 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 2000 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 6: Effects on hospital mortality rates

	(1) All patients	(2) Non-deferrable	(3) Age<80	(4) Age>80	(5) Medical	(6) Surgical
A: Patient controls						
DD	0.0032	0.0043	0.0019	0.0047	0.0035	0.0019
	(0.0012)	(0.0018)	(0.0013)	(0.0016)	(0.0014)	(0.0013)
B: Patient and mkt. controls						
DD	0.0038	0.0046	0.0022	0.0057	0.0040	0.0025
	(0.0013)	(0.0019)	(0.0013)	(0.0017)	(0.0015)	(0.0014)
Mean outcome (t-1)	0.118	0.176	0.089	0.152	0.130	0.071
Observations	13,017,104	3,168,233	7,368,823	5,648,281	10,030,657	2,885,706

Notes: The table presents hospital-level effects of privatization on mortality using claims data on the universe of Medicare fee-for-service patients aged 65–99. The outcome is 30-day mortality for patients, calculated from the date of discharge from the hospital. The coefficients are obtained by estimating Equation 1 on patient-level data and are adjusted for differences in patient risk, as described in Section 4. The different columns present the estimated effect on different samples: (1) all patients regardless of condition; (2) patients admitted through the emergency department for a nondeferrable condition, identified following Doyle Jr et al. (2015); (3) patients aged 65–80; (4) patients aged more than 80; (5) patients admitted for a "medical" major diagnostic category (MDC); and (6) patients admitted for a "surgical" MDC. A small fraction of patients could not be assigned an MDC. Standard errors are clustered by hospital and are presented in parentheses.

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Hospital finances			Hospital volume				Market-level	Hospital
	Revenue	Tot. Exp.	Pers. Exp.	Total	Medicaid	Medicare	Other	Medicaid	Mortality
Baseline	0.083	-0.009	-0.063	-0.089	-0.156	-0.053	-0.142	-0.042	0.0032
	(0.032)	(0.029)	(0.029)	(0.028)	(0.043)	(0.030)	(0.044)	(0.025)	(0.0012)
I: Specification checks									
A. Weighting by beds	0.104	-0.014	-0.056	-0.089	-0.164	-0.077	-0.107	-0.010	–
	(0.032)	(0.042)	(0.032)	(0.029)	(0.045)	(0.034)	(0.049)	(0.017)	–
B. State-year FEs	0.128	0.017	-0.035	-0.104	-0.159	-0.072	-0.178	-0.036	0.0032
	(0.031)	(0.028)	(0.028)	(0.030)	(0.044)	(0.032)	(0.046)	(0.025)	(0.001)
C. Incl. pre-trend	0.065	-0.020	-0.078	-0.103	-0.167	-0.059	-0.150	-0.078	0.0036
	(0.034)	(0.031)	(0.031)	(0.029)	(0.046)	(0.032)	(0.047)	(0.025)	(0.0.001)
II: Alternate estimators									
A. CS estimator	0.059	-0.022	-0.063	-0.075	-0.152	-0.034	-0.144	-0.039	0.0031
	(0.028)	(0.026)	(0.027)	(0.023)	(0.047)	(0.038)	(0.045)	(0.023)	(0.001)
B. DCDH estimator	0.039	-0.011	-0.054	-0.061	-0.127	-0.018	-0.115	-0.039	0.0030
	(0.025)	(0.024)	(0.025)	(0.023)	(0.042)	(0.030)	(0.047)	(0.021)	(0.001)
III: Alternate samples - treatment group									
A. Balanced panel	0.121	0.016	-0.036	-0.064	-0.155	-0.033	-0.098	-0.042	0.0029
	(0.034)	(0.032)	(0.032)	(0.031)	(0.047)	(0.034)	(0.049)	(0.026)	(0.001)
B. All treated obs	0.033	-0.045	-0.104	-0.082	-0.148	-0.063	-0.151	-0.037	0.0019
	(0.033)	(0.031)	(0.031)	(0.032)	(0.048)	(0.035)	(0.044)	(0.031)	(0.001)
IV: Alternate samples - comparison group									
A. Matched sample	0.084	-0.015	-0.056	-0.060	-0.149	-0.026	-0.102	-0.058	0.0016
	(0.035)	(0.031)	(0.031)	(0.029)	(0.049)	(0.036)	(0.051)	(0.027)	(0.002)
B. Switchers included	0.088	-0.006	-0.060	-0.085	-0.152	-0.050	-0.138	–	0.0034
	(0.032)	(0.029)	(0.029)	(0.028)	(0.042)	(0.030)	(0.044)	–	(0.001)

Notes: The table shows the results of robustness checks for the results using the AHA sample presented in Tables 3, 4A, and 11, respectively. For brevity, we do not present results for outcomes where we do not detect effects, such as non-personnel expenses and total market volume. Row IA uses static hospital beds to weight hospitals. Row IB includes state×year fixed effects and time-varying hospital and county controls. Row IC includes hospital-specific trends that are first estimated using data from 1996–2000. This analysis uses 2001–2019 data while dropping privatizations in 2001 and 2002. 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 report the average of five estimated dynamic effects and calculate standard errors via 100 bootstrap replications. Both models are estimated on a sample that includes data from the year of privatization. Row IIIA keeps only treated hospitals that we observe for five years before and after the transition, which primarily excludes treated hospitals that privatized after 2014. Row IIIB uses all treated observations, including those from the year of privatization and those beyond the five-year window around privatization (if available). Row IVA presents results estimated on a matched subsample using propensity score matching (see Section B.8 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: Effects on obstetric services

Obstetric volume	(1) Ob adm.	(2) Ob closure	(3) Ob adm. excluding clos.
DD	-0.768	0.133	0.287
	(0.287)	(0.048)	(0.378)
Obs	5,746	5,746	5,627
Mean outcome (t-1)	1,024	0.188	1,642

Notes: The table presents estimated effects on patient volume in privatized hospitals obtained by estimating Equation 1 on hospital-year-level data. We use data from four states (CA, FL, IN, and WA) on obstetric volume. Obstetric admissions information is not available in Minnesota and hence is taken from the remaining four states. We estimate synthetic difference-in-differences models using the "sdid" command with placebo inference using 200 replications. Column 1 presents the total effect on obstetric admissions. Columns 2 and 3 present the effects on the extensive and intensive margins, respectively. 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 9: Effects on billing practices

	(1) Log (charges)	(2) Log (payment)
A: Patient controls		
DD	0.0643	0.0022
	(0.019)	(0.013)
B: Patient and mkt. controls		
DD	0.0507	0.0093
	(0.021)	(0.013)
Mean outcome (t-1)	31,393	8,907
Observations	13,016,475	12,881,319

Notes: The table shows the effect on billing practices 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 51 hospitals privatized before 2005 to ensure that we can observe all privatized hospitals for at least 5 years prior to conversion. The sample is limited to Medicare FFS patients 65–99 years and enrolled in Parts A and B for a minimum of 3 months at admission. The results are from a specification that includes patient covariates, as described in Section 4. The model in Panel B also includes hospital and county-level covariates. The outcomes are as follows: (1) log charges (list price) and (2) log medicare payment. Both variables are deflated to 2019 dollars. Column 2 has fewer observations because some cases do not report positive payment amounts, which we exclude. Standard errors are clustered by hospital and are presented in parentheses.

Table 10: Effects on length of stay

	(1) Log (length of stay)	(2) Pr(stay $<$ 2 days)
A: Patient controls		
DD	-0.0173	0.0075
	(0.007)	(0.003)
B: Patient and mkt. controls		
DD	-0.0212	0.0083
	(0.007)	(0.003)
Mean outcome (t-1)	5.7	0.13
Observations	13,017,104	13,017,104

Notes: The table shows the effect on length of stay 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 51 hospitals privatized before 2005 to ensure that we can observe all privatized hospitals for at least 5 years prior to conversion. The sample is limited to Medicare FFS patients 65–99 years and enrolled in Parts A and B for a minimum of 3 months at admission. The results are from a specification that includes patient covariates, as described in Section 4. The model in Panel B also includes hospital and county-level covariates. The outcomes are as follows: (1) length of stay (in logs) and (2) the probability of being discharged on the same or next day after admission. Standard errors are clustered by hospital and are presented in parentheses.

Table 11: Effects on staff FTE per 100 beds

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-33.0	-2.6	-1.7	-29.1	0.1
	(12.9)	(0.8)	(3.3)	(9.7)	(1.4)
Obs	20,387	20,387	20,387	20,387	8,693
B: Market controls					
DD	-24.2	-2.7	-0.0	-21.8	0.0
	(13.0)	(0.8)	(3.3)	(9.8)	(1.4)
Obs	19,559	19,559	19,559	19,559	8,687
Mean outcome (t-1)	513.9	10.3	139.0	364.1	13.6

Notes: The table presents effects on full-time equivalent (FTE) employed staff at the privatized hospitals, obtained by estimating Equation 1 on hospital-year level data. Column 1 presents results for total FTE, which comprises physicians, nurses, and others (all remaining), presented in columns 2, 3, and 4, respectively. We normalize the number of FTEs so that it is expressed per 100 contemporaneous hospital beds. Column 5 presents results for contract FTEs, which come from Medicare cost reports and include management and patient care staff. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a specification including time-varying hospital and county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 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.13 presents the corresponding results obtained when we normalize staff FTE by 100 adjusted admissions instead.

A Additional figures and tables

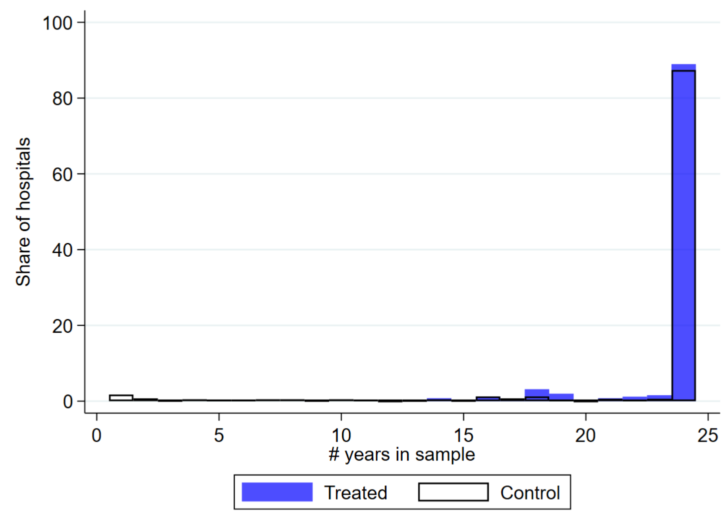


Figure A.1: Balance of hospital panel

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 24 (1996–2019).

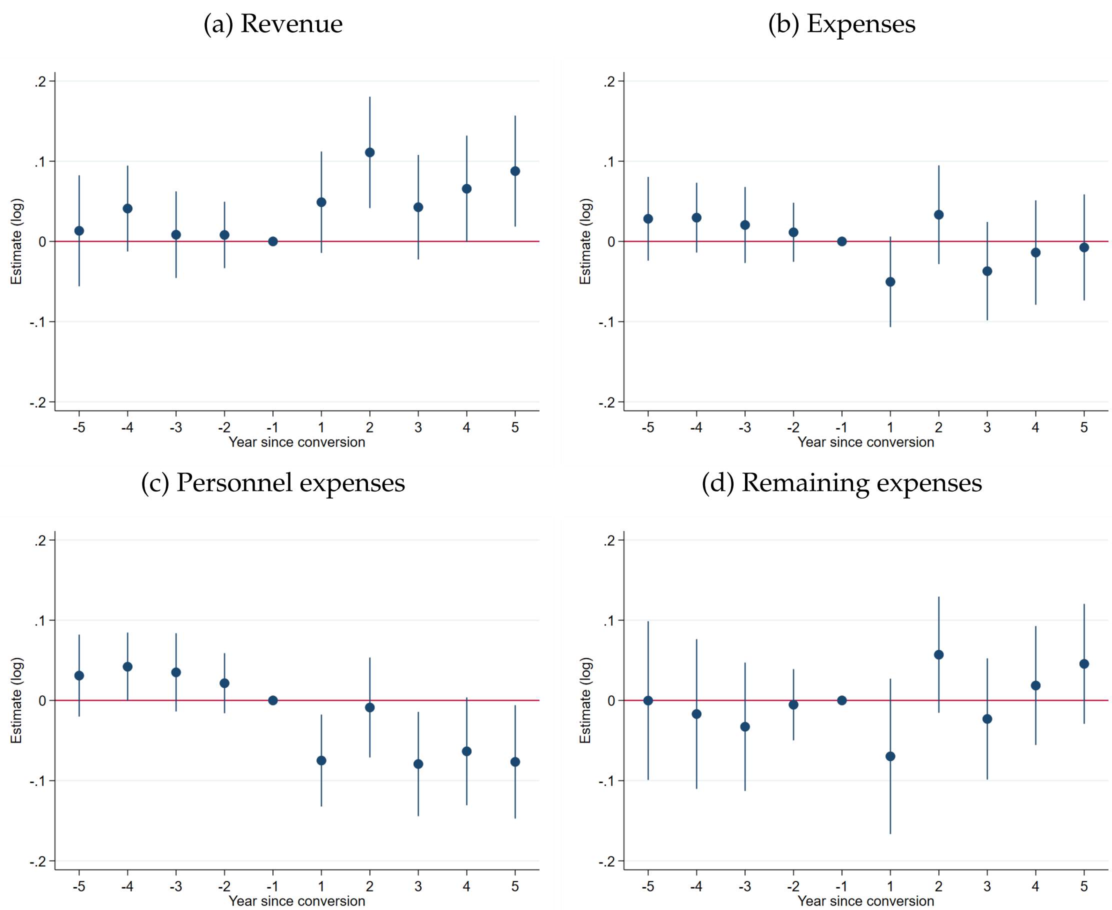


Figure A.2: Effects on (log) finances per patient

Note: The figure presents event study plots obtained by estimating Equation 2 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 revenue (from Medicare cost reports) and expenses (from AHA), respectively. Expenses comprise personnel expenses and remaining expenses, shown in Panels (c) and (d), respectively. All outcomes are normalized by contemporaneous adjusted admissions and then converted to logs. Adjusted admissions include both inpatient admissions and outpatient visits, with the latter scaled by their share of gross revenue. Figure 3 presents the corresponding event study plots obtained when the outcomes are normalized by hospital 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.

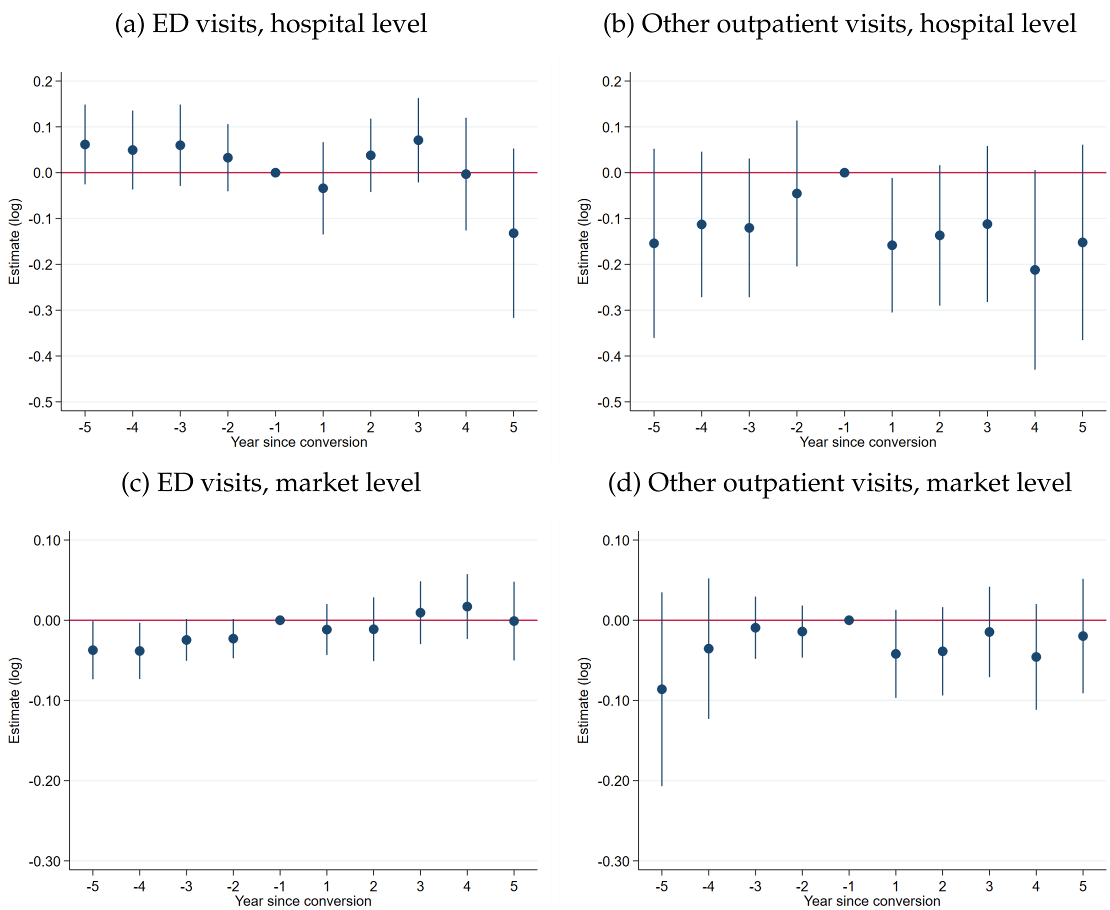


Figure A.3: Effects on ED and other outpatient visits

Note: The figure presents event study plots obtained by estimating Equation 2 on AHA data at the hospital-level (Panels (a) and (b)) and market-level (Panels (c) and (d)). The outcomes are log emergency department (ED) and non-ED, or other outpatient visits. Year zero is the year of privatization and is excluded for privatized hospitals, since it represents partial treatment. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital in Panels (a) and (b) and by market in Panels (c) and (d).

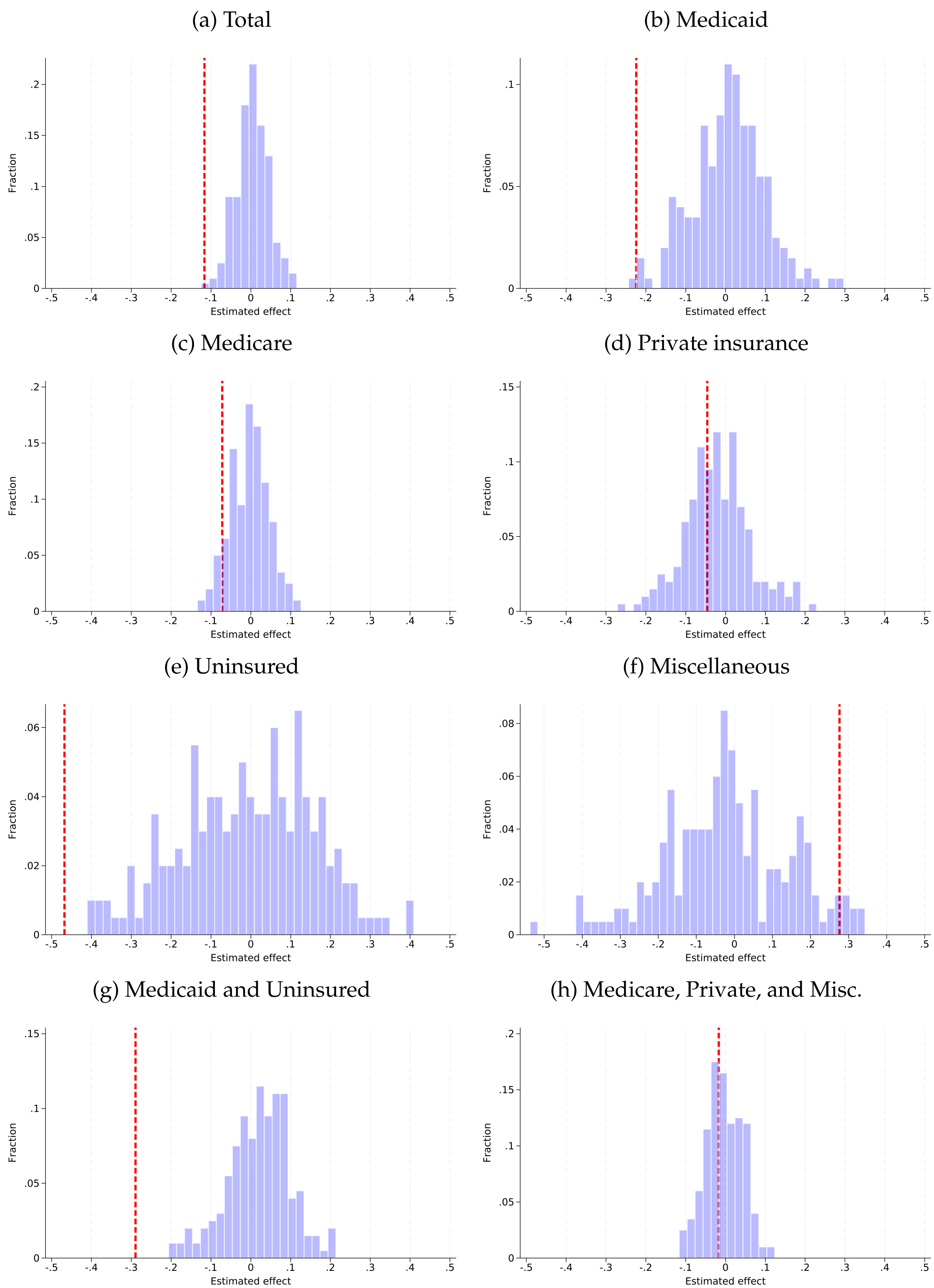


Figure A.4: Effects on admissions by payer using state data

Note: The figure presents distributions of estimated placebo effects on inpatient volume by payer using data during 2003–2019 from California, Florida, Indiana, Minnesota, and Washington. We limit the sample to privatizations occurring over 2008–18 in order to have min. 5 pre-treatment years for each event, as in the other analyses. This leads to a sample with 27 privatizations. We obtain the placebo estimates using the "sdid" command with the placebo inference option and 200 replications. The red vertical lines indicate the estimated effect for the privatized hospitals in these states. Section 5.2.1 provides more details.

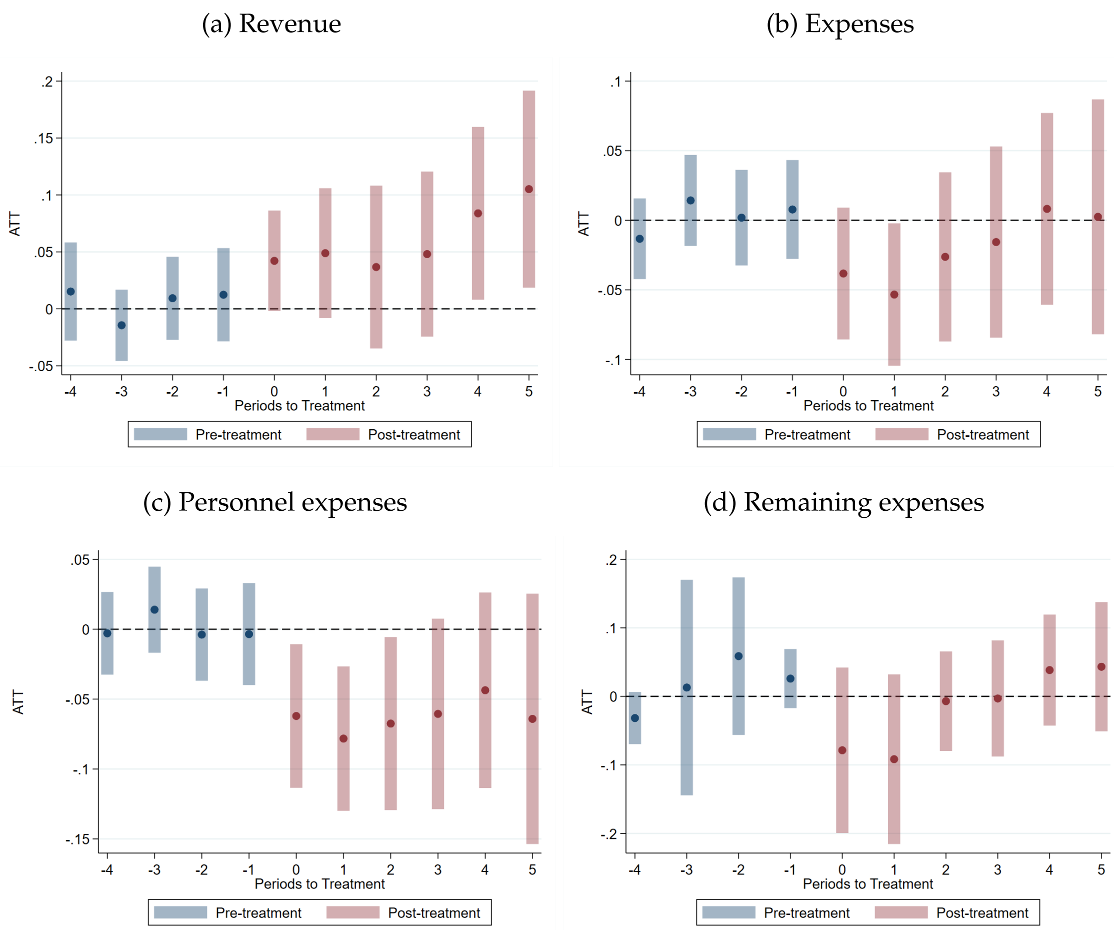


Figure A.5: Effects on (log) finances per bed (Callaway-Santanna)

Note: The figure presents alternate event study plots obtained using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcomes are financial measures per bed expressed in logs. We use never treated hospitals as the comparison group. The sample retains the year of privatization for treated hospitals, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

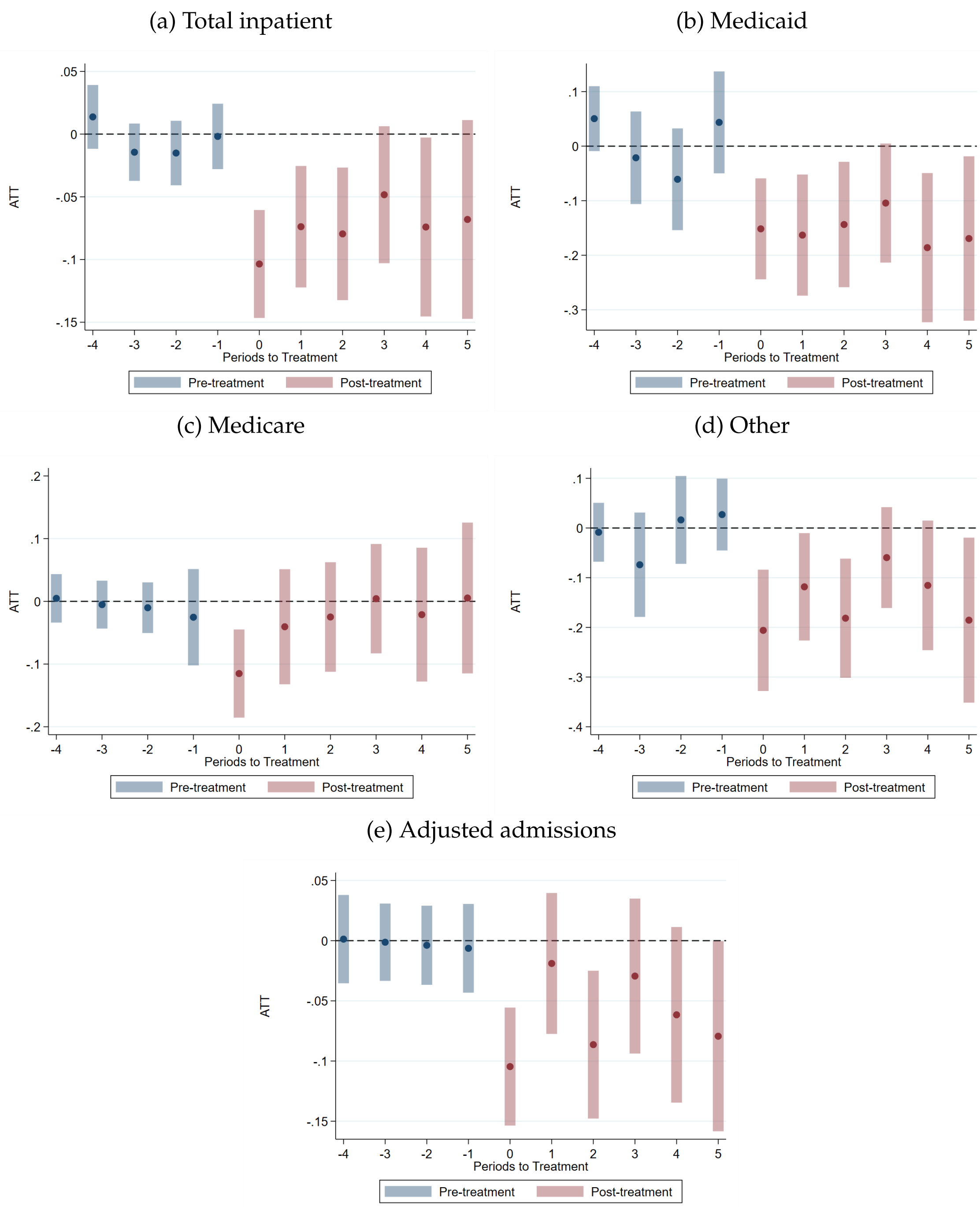
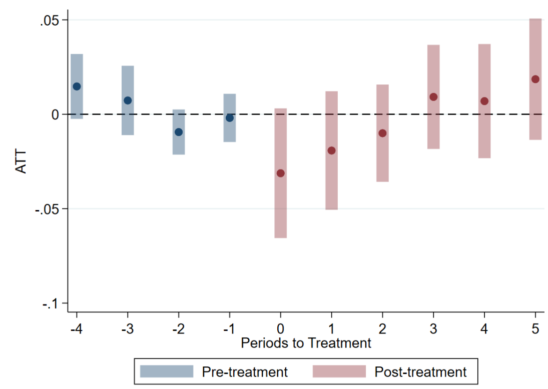


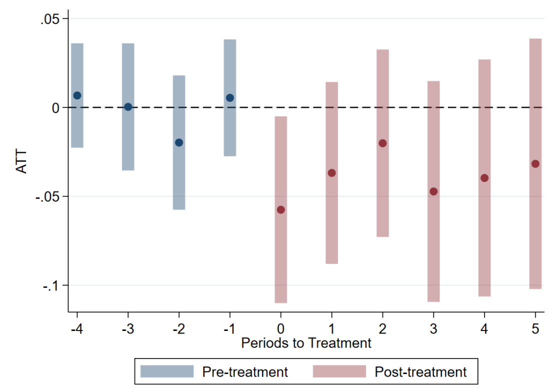
Figure A.6: Effects on patient (log) volume (Callaway-Santanna)

Note: The figure presents alternate event study plots obtained using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcomes are measures of hospital volume expressed in logs. We use never treated hospitals as the comparison group. The sample retains the year of privatization for treated hospitals, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

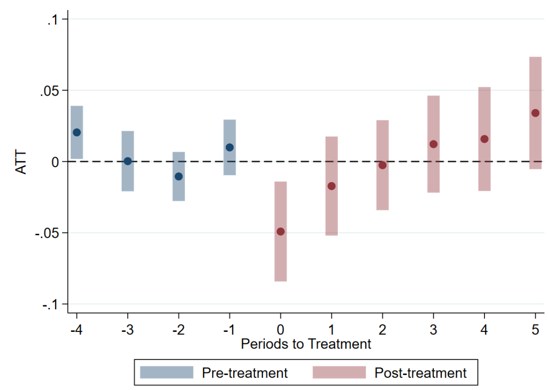
(a) Total admissions



(b) Medicaid



(c) Medicare



(d) Other

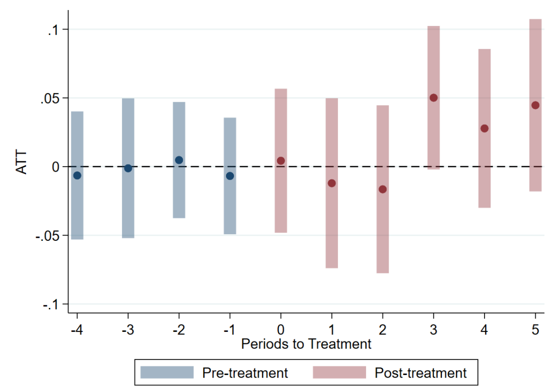
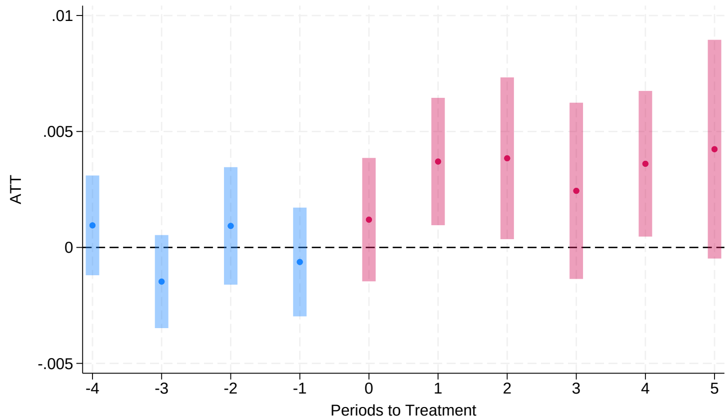


Figure A.7: Effects on market-level admissions (Callaway Santanna)

Note: The figure presents alternate event study plots obtained using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcomes are log hospital inpatient admissions aggregated to the market level. Markets are defined using Health Service Areas, as described in Section 4. We use never treated markets as the comparison group. The sample retains the year of privatization for treated markets, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by HSA.

(a) All 65+ patients



(b) Nondeferrable 65+ patients

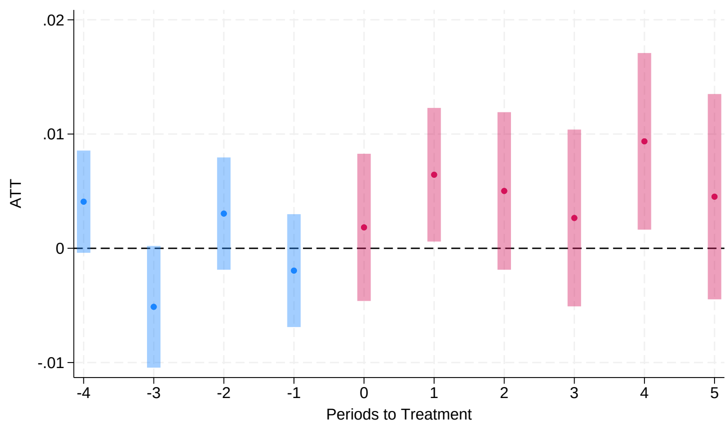
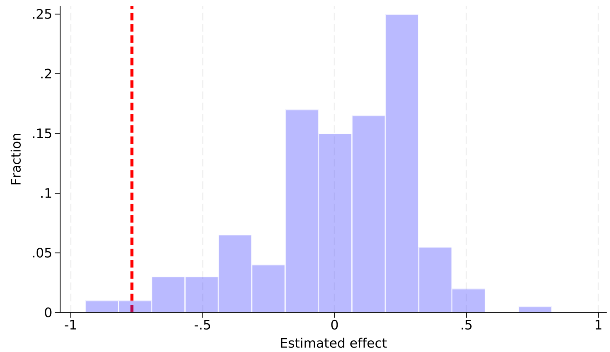


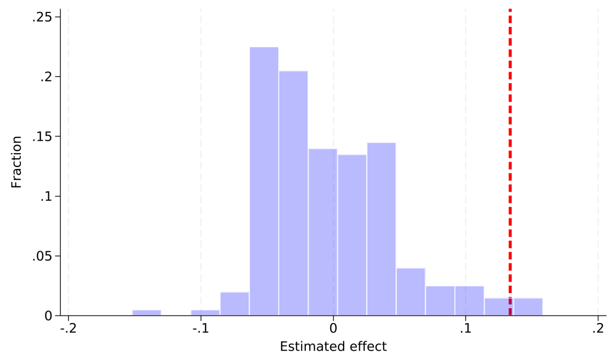
Figure A.8: Effects on mortality (Callaway Santanna)

<u>Note:</u> The figure presents alternate event study plots obtained using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcome is death at 30 days following discharge from the hospital. Panels (a) and (b) present the effects on mortality for all Medicare FFS patients aged 65-99 and those with nondeferrable admissions, respectively. We identify nondeferrable admissions following Doyle Jr et al. (2015). We use never treated hospitals as the comparison group. The sample retains the year of privatization for treated hospitals, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

(a) Obstetric admissions



(b) Obstetric closure



(c) Obstetric adm. excluding closure

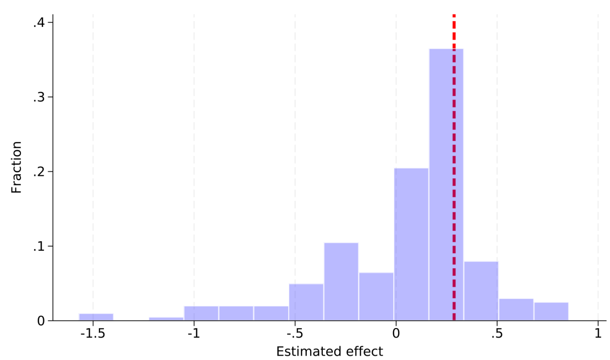


Figure A.9: Effects on obstetric admissions using state data

Note: The figure presents distributions of estimated placebo effects on obstetric admissions and closure using 2003–2019 data from California, Florida, Indiana, and Washington. Minnesota data was dropped because it only includes obstetric outcomes beginning in 2007. For this analysis we restrict to hospitals with greater than 2% obstetric share of admissions in 2002. We obtain the placebo estimates using the "sdid" command with the placebo inference option and 200 replications. The red vertical lines indicate the estimated effect for the privatized hospitals in these states. Section 6.1 provides more details.

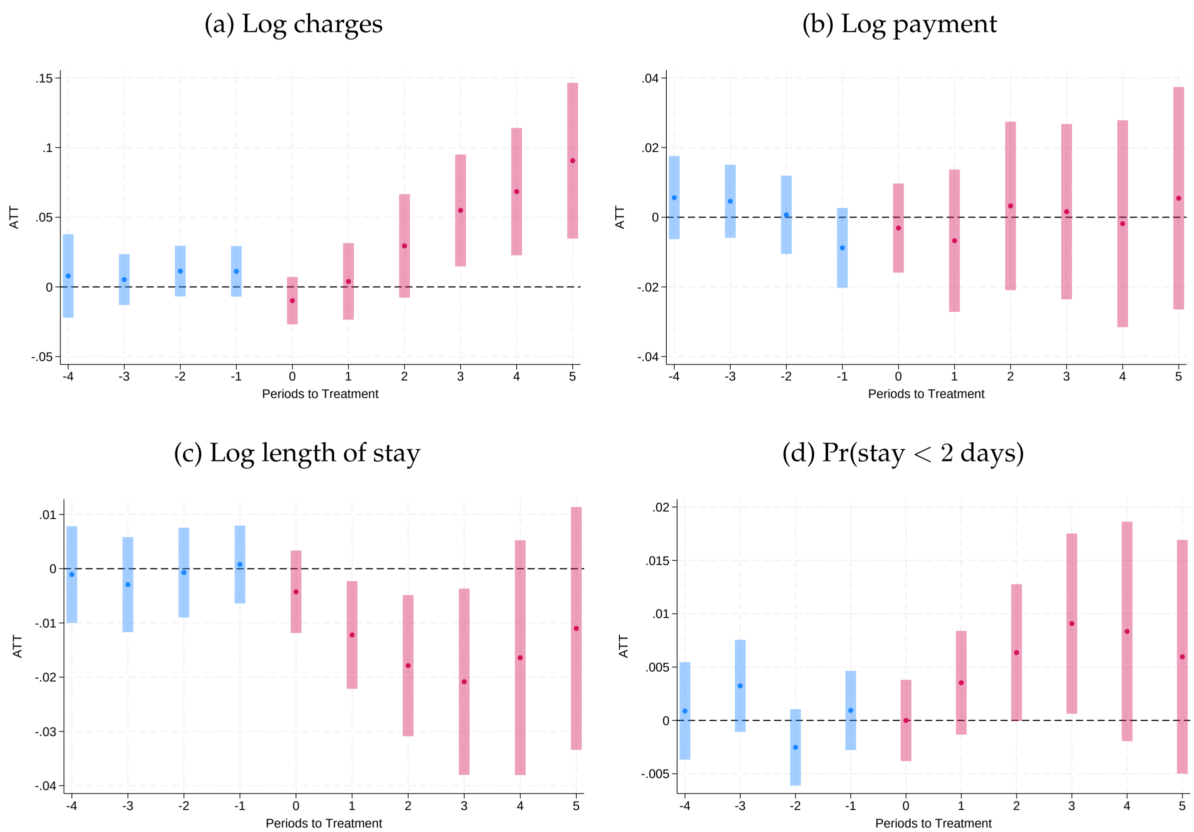


Figure A.10: Effects on billing and length of stay (Callaway-Santanna)

<u>Note:</u> The figure presents alternate event study plots obtained for Medicare FFS patients aged 65–99 using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcomes are a) log Medicare payment, b) log charges (list price), c) log length of stay, and d) probability that the stay is shorter than 2 days. We use never treated hospitals as the comparison group. The sample retains the year of privatization for treated hospitals, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

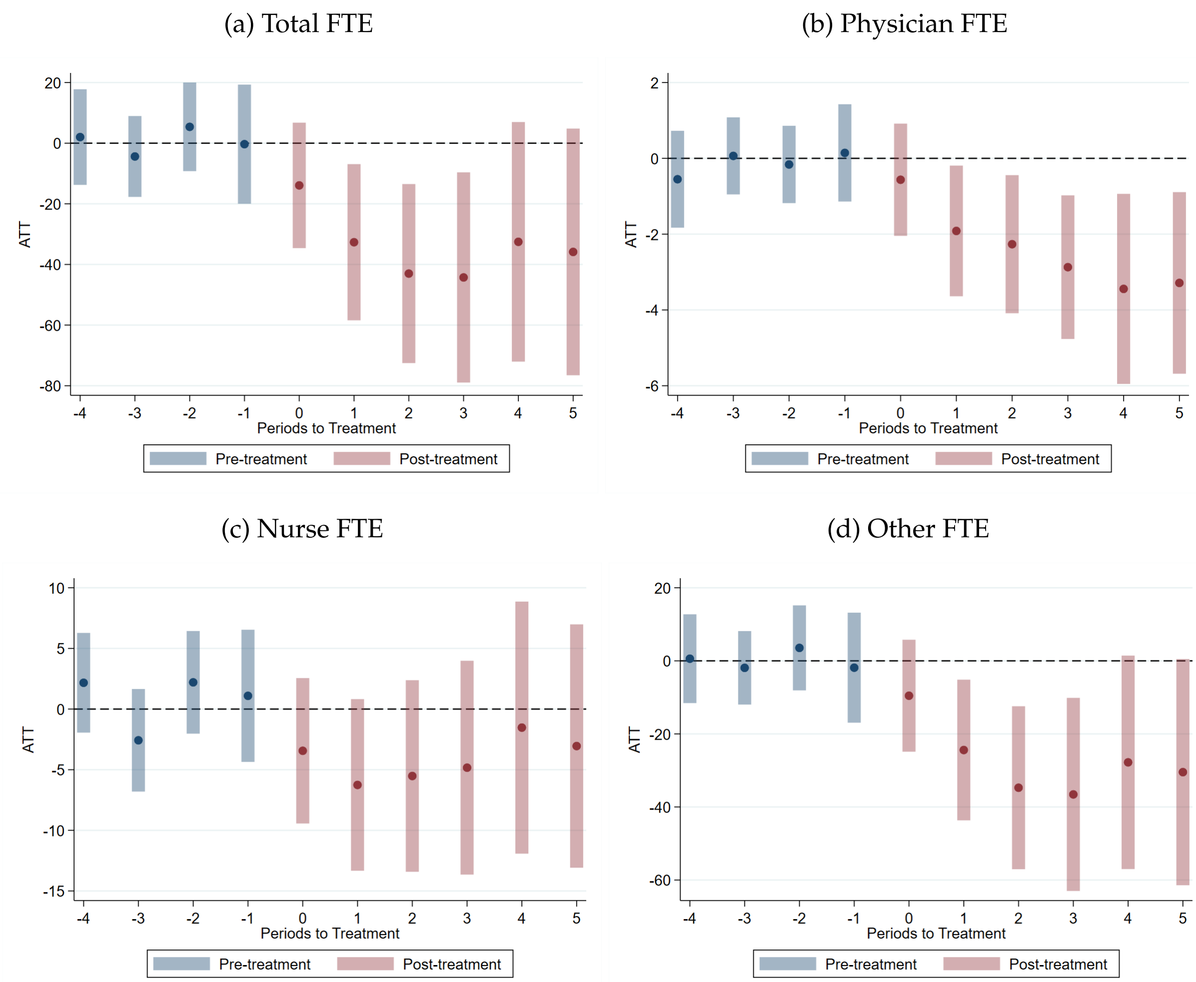


Figure A.11: Effects on staff FTE per 100 beds (Callaway-Santanna)

Note: The figure presents alternate event study plots obtained using the estimator proposed by Callaway and Sant'Anna (2020) and implemented by the command "csdid." The outcomes pertain to staff FTE per bed as observed in the AHA survey data. We use never treated hospitals as the comparison group. The sample retains the year of privatization for treated hospitals, thus also testing sensitivity to retaining the transition year. With 5 observations prior to treatment, this approach estimates only 4 dynamic coefficients. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

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

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

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

Table A.2: Characteristics of privatizations

	(1) Non-profit	(2) For-profit	(3) Total
A. Less control	117	24	141
- Contract Management	68	9	77
- Miscellaneous	49	15	64
B. More control	65	48	113
- Sale	36	33	69
- Lease/Joint venture	29	15	44
Total	182	72	254

Notes: This table presents characteristics of the types of privatization deals in our sample. These privatizations occur between 2001 and 2018. Columns 1 and 2 present the number of hospitals that converted to private nonprofit and for-profit, respectively. Panel A lists the modes that allow the private firm to have less control over hospital operations. In contract management, the private firm operates the hospital under a short-term contract. "Miscellaneous" includes cases where a new private firm was incorporated–subject to oversight by the previous government owners–specifically to operate the hospital and cases where the modality could not be identified. Panel B lists the modes of transfer that allowed the private firm more control over hospital operations. These include sale, lease, and joint ventures. Appendix B.1 describes these categories in more detail with examples.

Table A.3: Descriptive statistics, state sample

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

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

Table A.4: Descriptive statistics, Medicare sample

	(1) Privatized	(2) Not privatized	(3) All
A: Hospital attributes			
Beds	103	117	114
	(111)	(168)	(158)
Admissions	3,747	4,175	4,084
	(5,174)	(7,295)	(6,900)
%Medicaid	16.2	15.7	15.8
	(10.9)	(10.8)	(10.8)
%Medicare	49.5	49.7	49.7
	(14.3)	(15.8)	(15.5)
%Other	34.3	34.6	34.6
	(12.2)	(12.3)	(12.3)
Medicare FFS	1,133	996	1,025
	(1,502)	(1,484)	(1,488)
B: Patient outcomes			
Mortality rate (30-day)	0.115	0.114	0.114
	(0.03)	(0.04)	(0.04)
Charges ($)	12,061	12,690	12,558
	(5,866)	(9,628)	(8,969)
Length of stay	5.37	5.30	5.31
	(1.10)	(1.32)	(1.28)
Pr(stay$<$ 2 days)	0.152	0.172	0.168
	(0.06)	(0.07)	(0.07)
# Hospitals	203	767	970

Notes: The table presents descriptive statistics on hospitals and patients in the Medicare fee-for-service (FFS) claims sample. We present values from the first year the hospital appears in the sample, which is typically 2000. 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 aged 65–99 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 using the Medicare data. Standard deviations are shown in parentheses.

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

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
A: No controls				
DD	0.057	-0.033	-0.085	0.013
	(0.026)	(0.024)	(0.024)	(0.035)
Obs	16,829	16,829	16,829	16,829
B: Market controls				
DD	0.090	-0.008	-0.058	0.035
	(0.027)	(0.024)	(0.024)	(0.035)
Obs	16,816	16,816	16,816	16,816
Mean outcome (t-1)	8,158	4,633	8,498	3,864

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

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	-0.047	-0.067	0.024	-0.004
	(0.033)	(0.064)	(0.015)	(0.029)
Obs	20,387	20,387	19,288	19,288
B: Market controls				
DD	-0.040	-0.041	0.019	0.006
	(0.034)	(0.065)	(0.016)	(0.029)
Obs	19,559	19,559	18,555	18,555
Mean outcome (t-1)	15,526	54,409	152,477	505,800

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

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

A: Payer mix	(1) Mean amount ($/stay)	(2) Share of hospital stays Privatized (AHA)	(3) Privatized (states)	(4) Effect on volume %	(5) Predicted share of stays	(6) Predicted reimb. ($/stay)
1. Medicare	13,419	0.45	*	-0.052	0.47	13,419
2. Medicaid	9,269	0.21	*	-0.144	0.19	9,269
3. Other	13,385	0.35	*	-0.132	0.33	13,888
Private insurance	14,919	NA	0.28	-0.045	0.27	
Uninsured	5,928	NA	0.06	-0.374	0.04	
Miscellaneous	15,153	NA	0.02	0.319	0.03	
Overall	12,558	1.00	1.00		1.00	12,769
% Increase in reimb.						1.7%

B: List prices	Effect on list prices		Effect on volume and list prices	
%List price contracts	20% ($/stay)	50% ($/stay)	20% ($/stay)	50% ($/stay)
1. Medicare	13,419	13,419	13,419	13,419
2. Medicaid	9,269	9,269	9,269	9,269
3. Other	13,563	13,830	14,079	14,367
Private insurance	15,117	15,415	15,117	15,415
Uninsured	6,007	6,125	6,007	6,125
Miscellaneous	15,354	15,656	15,354	15,656
Overall	12,620	12,712	12,831	12,923
% Increase in reimb.	0.5%	1.2%	2.2%	2.9%

Notes: The table presents results on the effects of changes in payer mix, list prices, or both on mean reimbursement. Panel A walks the reader through the calculation of the predicted effect of changes in payer mix on mean reimbursement per patient. Column 1 presents mean reimbursement rates for hospital inpatient stays averaging across Medical Expenditure Panel Survey (MEPS) waves of 2000, 2005, 2010, 2015, and 2019. The mean values are expressed in 2019 dollars. Column 2 presents the shares of patients for Medicare, Medicaid, and "Other" for privatized hospitals calculated using AHA data in the year before treatment and reported in Table 4 Panel A. The AHA does not report volumes separately for the component groups within Other, therefore we denote these as NA. Column 3 is equivalent to column 2 but uses data from 5 states (CA, FL, IN, MN, and WA). These data, reported in Panel C of Table 4, are used here only to calculate the changes in shares for 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 (U.S. military insurance), and other government programs. The mean reimbursement for Other is calculated as a weighted average of private, uninsured, and miscellaneous, with the patient shares from the states data (using volumes reported in Table 4B) as weights. Column 4 presents the estimated percent effects on inpatient volume by payer. The values for Medicare, Medicaid, and Other reflect the exponentiated coefficients reported in Table 4 Panel A. The values for private, uninsured, and miscellaneous reflect the exponentiated coefficients reported in Panel B of the same table. Column 5 presents the predicted share of stays by payer that result when we apply the estimates in col. 4 to the corresponding baseline shares in cols. 2 (Medicare, Medicaid, and Other) or col. 3 (private, uninsured, and miscellaneous). Results from the states sample are used to quantify the shift in composition within Other, while results from the AHA sample are used to quantify the shift between Medicare, Medicaid, and Other. Column 6 presents the predicted reimbursement for Other and overall after incorporating the estimated changes in payer shares. Panel B walks the reader through the calculation of the predicted effect of changes in list price alone (cols. 1-2) and the combination of changes in payer mix and list price (cols. 3-4) on mean reimbursement per patient. We apply the estimated increase in list price, 6.6%, to the mean reimbursement of private, uninsured, and miscellaneous payers, scaled by the proportion of contracts that are based on list price. Following prior studies, we assume this proportion to range between 20% and 50%. Mean reimbursement for other is the weighted average calculated using the shares in Panel A col. 3 as weights. Columns 4 and 5 incorporate the changes in payer mix presented in Panel A col. 5. Hence, the same increases in list prices lead to a greater mean reimbursement for Other patients. The last row in both panels presents the % increase in mean reimbursement relative to the baseline value, $12,558.

Table A.8: Market-level descriptive statistics

	(1) Treated HSAs	(2) Control HSAs	(3) Total
# Treated hospitals	1.2	0.0	0.3
	(0.6)	(0.0)	(0.6)
Total hospitals	6.1	4.6	4.9
	(5.6)	(6.5)	(6.3)
Total admissions	38,771	32,432	33,811
	(61,561)	(82,754)	(78,647)
Total beds	979	804	842
	(1,436)	(1,940)	(1,843)
% Medicaid adm	16.1	14.6	14.9
	(7.1)	(6.9)	(7.0)
% Medicare adm	45.9	47.8	47.4
	(9.2)	(9.6)	(9.5)
% Other adm	38.0	37.6	37.7
	(9.7)	(9.6)	(9.6)
% In poverty	13.7	12.7	12.9
	(5.0)	(4.8)	(4.9)
% Unemployment	4.4	4.3	4.3
	(1.4)	(1.5)	(1.5)
% Uninsurance	20.6	19.1	19.4
	(6.0)	(5.7)	(5.8)
HHI (admissions)	4,614	5,610	5,393
	(2,451)	(2,859)	(2,805)
# HSAs	202	727	929

Notes: The table presents descriptive statistics for the market-level sample, where markets are defined by Health Service Areas (HSAs) defined by the U.S. Census. We use values from 2000 for most HSAs. In rare instances where we do not observe an HSA in 2000, 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 2001–18. Control HSAs do not have any conversions during our sample period. Values related to hospital care are sourced from the American Hospital Association survey. All rows present means and standard deviations (in parentheses).

Table A.9: 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	0.0032	0.0044	0.0053	0.0063	0.0071	
	(0.0012)	(0.0015)	(0.0016)	(0.0018)	(0.0022)	
A2: Patient and mkt. controls						
DD	0.0038	0.0051	0.0062	0.0074	0.0086	
	(0.0013)	(0.0015)	(0.0017)	(0.0020)	(0.0023)	
Mean outcome (t-1)	0.118	0.156	0.183	0.241	0.322	
Observations	13,017,104	12,982,284	12,945,540	12,839,814	12,607,345	
B: By diagnostic category	(1) Circulatory	(2) Respiratory	(3) Digestive	(4) Musculoskeletal	(5) Kidney	(6) Miscellaneous
B1: Patient controls						
DD	0.0027	0.0019	0.0046	0.0020	0.0051	0.0042
	(0.0019)	(0.0024)	(0.0021)	(0.0014)	(0.0028)	(0.0017)
B2: Patient and mkt. controls						
DD	0.0033	0.0036	0.0058	0.0027	0.0059	0.0048
	(0.0021)	(0.0026)	(0.0021)	(0.0013)	(0.0029)	(0.0018)
Mean outcome (t-1)	0.096	0.159	0.086	0.05	0.119	0.146
Observations	3,062,832	2,211,333	1,411,954	1,403,534	929,142	3,998,301

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 65+ patients at different durations from 30 days through 365 days following discharge from the index hospital stay. Since we observe death for beneficiaries through December 2019, we limit the sample to people discharging at progressively earlier dates as we extend the follow-up period. For example, to study 365-day mortality we stop at patients discharged on Dec 31 2018, while for 30-day mortality we include patients through Nov 30 2019. 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 1 on patient-level data. The model represented in row 1 of each panel includes patient covariates to control for observed differences across patients, as described in Section 4. The model in row 2 of each panel also includes time-varying market covariates. Standard errors are clustered by hospital.

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

	(1) All treated	(2) All controls	(3) Std. difference	(4) Matched controls	(5) Std. difference
# hospitals	254	802		254	
Beds	87	113	-.16	94	-.05
Total admissions	3,038	4,178	-.16	3,164	-.02
Medicaid admissions	622	1,039	-.20	643	-.02
Expenses (mn)	62	101	-.21	64	-.02
HSA population	577,057	684,072	-.07	492,422	.08
% in poverty (county)	16.8	15.8	.16	16.9	-.02
% unemployment (county)	7.0	6.3	.21	7.1	-.05

Notes: The table presents means for treated hospitals (col. 1, 254 in number), all comparison hospitals, (col. 2, 802), and matched comparison hospitals (col. 4, 254). We use 1:1 matching without replacement and describe the matching procedure in more detail in Section B.8. We present mean values for the variables used in propensity score matching. Col. 3 presents the standardized difference in means between the full sample of treated and comparison hospitals. We compute the standardized difference as the difference in means divided by the standard deviation of the pooled sample. Col. 5 presents the standardized difference in means in the matched sample. All means are computed in the year before privatization. In col. 2 we randomly assign privatization years to control hospitals, drawn from the empirical distribution of privatization years among the treated hospitals. In col. 4 each matched control hospital is assigned the same privatization year as its matched treated hospital counterpart.

Table A.11: Robustness checks (billing and length of stay)

	(1) Log (charges)	(2) Log(payment)	(3) Log LOS	(4) Pr(stay<2 days)
Baseline	0.0643	0.0022	-0.0173	0.0075
	(0.019)	(0.013)	(0.007)	(0.003)
I: Specification checks				
A: State-year f.e.	0.0513	0.0037	-0.0207	0.0072
	(0.019)	(0.012)	(0.006)	(0.003)
B: Incl. pre-trend	0.0647	-0.0205	-0.0198	0.0056
	(0.019)	(0.013)	(0.007)	(0.003)
II: Alternate estimators				
A: Callaway Santanna	0.0370	-0.0004	-0.0138	0.0055
	(0.017)	(0.011)	(0.006)	(0.003)
B: DCDH	0.0284	-0.0014	-0.0142	0.0054
	(0.014)	(0.010)	(0.006)	(0.003)
III: Alternate samples, Treated group				
A: Balanced panel	0.0676	0.0021	-0.0125	0.0063
	(0.022)	(0.016)	(0.008)	(0.003)
B: No trimming	0.0823	0.0051	-0.0138	0.0079
	(0.023)	(0.016)	(0.008)	(0.004)
IV: Alternate samples, Comparison group				
A: Matched sample	0.0275	0.0041	-0.0296	0.0099
	(0.023)	(0.016)	(0.009)	(0.003)
B: Include switchers	0.0717	0.0036	-0.0164	0.0073
	(0.019)	(0.013)	(0.007)	(0.003)

Notes: The table shows the results of robustness checks for the effects on billing practices (columns 1-2) and length of stay (columns 3-4) using the Medicare fee-for-service patient sample. These results are obtained by estimating patient-level models and correspond to the results in Tables 9 and 10, respectively. The top row presents the baseline estimates for convenience. 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 alternate estimators - Callaway Santanna using the "csdid" command (A) and de Chaisemartin d'Haultfoueille, DCDH in short, using the "did_multiplegt" command (B). We estimate these results on samples that retain data from the year of privatization and only use never treated hospitals in the comparison group. Panel III presents results from samples with different restrictions, one in which all treated hospitals must be observed for 5 years after privatization (A), and the other in which we retain all observations for treated hospitals, including the year of privatization (B). Panel III tests the robustness to varying the comparison group. Row A presents results using a matched subsample identified using propensity score matching, and the sample in row B includes hospitals that switch between public and private status. Standard errors are clustered by hospital.

Table A.12: Hospital occupations in 2019

Occupation name	Share of employment		Major sub-occupations
	Local	Private	
(1)	(2)	(3)	(4)
Nurses	30.0	32.6	RNs, LPNs
Physicians	3.6	2.9	Family & general internal medicine, other non-pediatric
Other healthcare practitioner & technical	20.0	21.4	Therapists, lab technicians
Office and administrative support	13.2	11.5	Information and record clerks, secretaries
Healthcare support	12.5	12.5	Nursing assistants, medical assistants
Management	4.0	3.7	Medical and health service managers, operation specialty managers
Building and grounds cleaning	3.5	3.1	Maids and housekeeping, janitors
Community and social service	2.4	1.9	Social workers, counselors
Food preparation and serving	2.3	2.2	Cooks and food prep, food and beverage servers
Business and financial operations	2.2	2.2	Financial specialists, HR workers
All remaining	6.3	6.0	Computer occupations, maintenance & repair
Total	100.0	100.0	

Note: The table presents the share of national hospital employment for different occupations, as recorded by the Bureau of Labor Statistics (BLS). This is the industry-occupation matrix data, available for 2023 at https://www.bls.gov/emp/tables/industry-occupation-matrix-industry.htm. We use the internet archive to get the corresponding data files for 2019, the last year of our sample. The earliest data available appears to be for 2016, so we cannot document occupation shares as of 2000. We present occupation shares for local government and private hospitals separately, obtained from the tables 62210L and 62210P, respectively. We organize the occupations so that nurses and physicians correspond to the corresponding categories in the AHA data and are listed at the top. The remaining occupations are listed in descending order of labor share at government hospitals. Nurses, physicians, and "Other healthcare practitioner and technical" together form the category, "Healthcare practitioner and technical" with occupation code 29-0000. In column 4 we present the two largest sub-occupations within each occupation by labor share.

Table A.13: Effects on staff per 100 admissions

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-0.54	-0.03	-0.02	-0.49	-0.01
	(0.26)	(0.01)	(0.07)	(0.19)	(0.01)
Obs	20,387	20,387	20,387	20,387	8,693
B: Market controls					
DD	-0.34	-0.02	0.02	-0.34	-0.02
	(0.26)	(0.01)	(0.07)	(0.20)	(0.01)
Obs	19,559	19,559	19,559	19,559	8,687
Mean outcome (t-1)	7.40	0.10	1.90	5.30	0.20

Note: The table presents effects on staff employment at the privatized hospitals, obtained by estimating Equation 1 on hospital-year level data. All outcomes are 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 1 presents results for total FTE, which comprises physicians, nurses, and others, presented in columns 2, 3, and 4, respectively. Column 5 presents results for contract FTEs, which come from Medicare cost reports and include management and patient care staff. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a specification including time-varying hospital and county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1996. The mean values pertain to the outcomes at privatized hospitals in the year before privatization. Standard errors are clustered by hospital. Table 11 presents the corresponding results obtained when we scale staff by beds instead.

Table A.14: Robustness checks (staff availability)

	(1) Total	(2) Physician	(3) Other
Baseline	-33.04	-2.58	-29.08
	(12.87)	(0.77)	(9.67)
I: Specification checks			
A. Weighting by beds	-29.22	-4.59	-22.82
	(14.72)	(1.76)	(10.46)
B. State-year FEs	-23.09	-2.52	-21.92
	(13.07)	(0.81)	(9.92)
C. Incl. pre-trend	-35.28	-3.25	-31.06
	(14.19)	(0.87)	(10.66)
II: Alternate estimators			
A. CS estimator	-33.48	-2.32	-26.95
	(13.20)	(0.81)	(9.93)
B. DCDH estimator	-31.92	-1.93	-26.39
	(12.96)	(0.76)	(9.76)
III: Alternate samples - treatment group			
A. Balanced panel	-18.86	-2.20	-17.70
	(13.81)	(0.85)	(10.43)
B. All treated obs	-53.12	-3.16	-43.89
	(14.43)	(0.85)	(10.80)
IV: Alternate samples - comparison group			
A. Matched sample	-23.51	-1.92	-23.36
	(14.14)	(0.85)	(10.54)
B. Switchers included	-31.69	-2.48	-27.80
	(12.82)	(0.76)	(9.64)

Notes: The table shows the results of robustness checks for the effects on hospital FTE staff per 100 beds using the AHA sample. These results are obtained by estimating hospital-level models and correspond to the results in Table 11. The top row presents the baseline estimates for convenience. The panel structure and checks are identical to those presented in the main robustness table (Table 7). Standard errors are clustered by hospital.

Table A.15: Cost-benefit calculations

Item	**Value**	**Notes**
A. Baseline values:		
Beds	93	Table 2
Revenue/bed ($)	650670	Table 3
Cost/bed ($)	668767	Table 3
Deficit/bed ($)	18097	Cost - Revenue
Deals with less control	56%	Table A2
For-profit partner	28%	Table A2
B. Savings, per privatization per year		
B1. Deficit	1,683,021	93 beds x $18097/bed
B2. Increase in surplus		
Increase in revenue/bed	56,310	$650670 x 8.3% increase
Increase in revenue	5,236,845	93 beds x 56310 per bed
Reduction in cost/bed	6,046	$668767 x 0.9% decrease
Reduction in cost	562,284	93 beds x 6046 per bed
Gross increase in surplus	5,799,129	Additional revenue + cost savings
Net increase in surplus	4,116,108	Gross surplus - Deficit
B3. Additional tax funds		
Hospital tax rate	2.10%	Rosenbaum et al. 2015 ex. 4
Mean hospital revenue	60,512,310	7025x(1-6%) patients x ($8109+$462)
Incremental tax (FP only)	1,270,759	2.1% of revenue
Share of FP in deals	28%	Table A2
Expected tax	359,556	Incremental tax x FP share
Baseline net savings	2,042,577	B1 (deficit) + B3 (tax revenue)
Surplus in deals w less control	2,297,363	56% of net surplus
Upper bound estimate	4,339,939	Baseline + surplus revenue
C. Deaths, per privatization per year		
C1. Hospital mortality		
Medicare FFS patients	1133	Table A3; 65+ only
All Medicare patients	1873.5	Table A3
Volume reduction	-5.2%	Table 4 Panel A
Mortality effect	0.32%	Table 5 Panel A
Incremental deaths	3.44	1133*(1-5.3%) patients x 0.32%
Standard LYL	8.90	Life exp. using CDC life table 2010
Realistic LYL	5.34	8.9*(1-40%) Deryugina et al 2019
Realistic aggregate LYL	18.36	3.44 x 5.34
Extrapolated to all Medicare:		
Incremental deaths	5.69	1873.5 (1-5.2%) patients x 0.32%
LYL	30.36	5.69 x 5.34
D. Savings per death or per LYL		
D1. Baseline estimate:		
Savings per death ($mn)	0.59	$2mn /3.4 deaths
Savings per LYL ($)	111,244	$2mn /18.4 LYL
D2. Upper bound:		
Savings per death ($mn)	1.26	$4.3mn /3.4 deaths
Savings per LYL ($)	236,363	$4.3mn /18.4 LYL

Notes: The table explains the calculations used in the cost-benefit analysis discussed in Section 7. Panel A presents baseline values of patient volume, revenue, costs, etc. for the privatized hospitals before privatization. Panel B describes how we estimate the savings, gross and net surplus, and tax revenue generated from the average privatization. Panel C presents the additional deaths and life-years lost (LYL) due to the average privatization among Medicare patients at the hospital. We use the CDC life tables for 2010 to calculate the average years of life lost. For Medicare patients, we integrate life expectancy at each age using the observed distribution of age at death in our sample. To account for potential heightened mortality risk among decedents, we scale these estimates down by 40% following Deryugina et al. (2019). Panel D presents two estimates of the net savings per death and per LYL. Column 3 provides the rationale or source of the value used in the calculations.

B Data Appendix

B.1 Privatization taxonomy

We first identify cases of public hospitals that were converted to private control during our study period of 2001–18. There is no official source of such events, and thus we utilize the AHA annual survey files over this period. See Section B.2 for more details on how we construct our initial list of privatizations. We manually verify each conversion by combing through hospital websites, news articles, and third-party sites such as the American Hospital Directory. Manual validation helps identify nontrivial numbers of false positive conversions. Our final number of conversions is 254.

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

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

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

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

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

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

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

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

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

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

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

B.2 American Hospital Association annual surveys

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

Identifying privatizations — We create an initial list of public to private conversions by starting with conversions implied by changes in the control or system name variables in the AHA data. In the former case, we identify hospitals that in year $t-1$ are listed as public (state, county, city, city-county, or hospital district or authority) and in year t are listed as private (for-profit or nonprofit), for the years 2000–2018[23]. We also require that hospitals be listed as private for at least two "post" years, with the exception of privatizations in 2018. Using the system name variable, we identify hospitals with system name changes during the years 2000–2018. Specifically, we create a list of public and private health systems based on their names and then identify hospitals under public control and not part of a private system (i.e., part of a public system or not part of a system) for two years, and then subsequently listed as public control and part of a private system for two years. Furthermore, we implement the same sample restrictions made when creating our analytic sample, e.g., we drop privatizations of hospitals not considered "general medical and surgical". In addition, we limit our treated hospitals to those that experience only one conversion over our sample period.

Using the above approach, we identify 355 "naive" privatizations, which we then manually validate. Our validation yields 101 false positives, in which we do not find evidence in the public domain that a privatization occurred at a given hospital. This gives our final set of 254 public-to-private conversions.

Defining the control group of hospitals — We start with American Hospital Association (AHA) survey data for the years 1996 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" (2,388 hospitals)
- Drop hospitals that on average report fewer than 10 beds (44 hospitals)

23. We include the year 2000 to identify privatizations that occur in 2001.

- Drop hospitals that are ever classified as federal by the AHA (278 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 (264 hospitals) and hospitals that are most commonly labeled as public (112 hospitals). This is a conservative restriction to ensure that our comparison group is comprised of non-converting, public hospitals
- Drop hospitals that are within 15 miles of at least one treated hospital (32 hospitals)

The final AHA analysis sample contains 802 comparison hospitals.

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

B.3 State administrative data on hospitals and patients

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

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

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

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

B.4 Medicare fee-for-service claims

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

We use Medicare data to test the effects of privatization on patient complexity, treatment intensity, billing practices, and mortality by estimating models on patient-level data. We construct and use two measures of patient complexity for this analysis. We generate a predicted probability of 30-day mortality using a probit model based on patient demographics (gender, age, age squared), 30 Elixhauser risk flags based on the 90-day history of hospital inpatient and outpatient care, flags for utilization history of different types of care (hospital stay in the past 30 days, past 90 days, 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 matches perfectly the observed 30-day mortality risk in the prediction sample. We then use the estimated model coefficients to predict the mortality risk for all patients in the analysis sample. We use the same vector of patient covariates when testing for changes in treatment intensity, billing, and mortality.

B.5 Healthcare Cost Report Information System (HCRIS) data

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

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

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

B.6 Medical Expenditure Panel Survey (MEPS)

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

B.7 Vital statistics microdata

We study changes in mortality rates at the market level using confidential Vital Statistics data for 1996–2019 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 U.S. 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.

To calculate mortality rates at the market level, we combine individual-level mortality data from the CDC that span 1996 to 2019 with county-level population data from the National Cancer Institute Surveillance Epidemiology and End Results (SEER) program.[24]. We further merge population estimates from CDC Wonder for Hawaii for 1996–1999 since they were missing in SEER. We construct mortality and population counts for each HSA (for mortality events, we use the HSA of residence, not the HSA of occurrence) and year for six different age groups: all ages, <15, 15–34, 35–54, 55–64, and >65. We then calculate the death rates for each HSA-year-age group as $100,000$ x number of deaths / population.

B.8 Matching design

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

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

24. 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.

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 2001, 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 2002. We continue this process for all 18 years of privatizations (2001–2018) and are able to match all 254 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.

C Market level Mortality

We 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 Section 5.3 quantifies this effect for Medicare FFS patients 65 years and older. A second group is affected because they cannot access care in the hospital and have to travel further for care or experience a disruption in their treatment. A third group is indirectly affected due to the potential crowding in the remaining hospitals in the market. Examining mortality at the market level allows us to estimate the total effect across all three channels. We apply our market-level difference in differences research design to vital statistics microdata which allow us to observe the universe of deaths in the U.S. during 1996–2019.

We caution that this test has limited power to detect an effect since only a small proportion of the population in the market is potentially affected by hospital privatization in any year. There are at least three reasons: Only a small fraction of people need inpatient care in a year[25]; the privatized hospital is typically only one of the six that serve the market; and, based on the results of Section 5.2.1, we hypothesize that the access effect is felt primarily by Medicaid beneficiaries and the uninsured. However, data constraints prevent us from focusing on lower income decedents directly. Therefore, this approach recovers an "intent-to-treat" effect. To maximize statistical power, we limit the sample to those between 55 and 64 years of age. This group has relatively high hospitalization and mortality rates, while also having a high share of Medicaid and uninsured individuals. According to data from the CPS, about 20% of people aged 55–64 were covered by Medicaid or had no insurance in 2000 and 2019. In contrast, people 65 years and older enjoy nearly universal coverage through Medicare. Following similar rationale, studies on the aggregate mortality effects of the Affordable Care Act also focused on this age group (Black et al. 2019; Miller, Johnson, and Wherry 2021).

We utilize confidential Vital Statistics data for 1996–2019 obtained from NCHS (NCHS 2023) to calculate mortality rates at the market level. 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 U.S. 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.7 provides more details.

Table C.1 column 1 presents the estimated effect on all-cause mortality for people aged 55–64 years residing in the affected market, defined by the HSA. We find an increase of 4.5 deaths per 100,000, 0.4% of baseline mortality for this age group. However, this estimate is not statistically significant at conventional levels. Three patterns in the data corroborate the interpretation that this represents a causal effect of privatization. First, we find that the effect on mortality increases as Medicaid and Other hospital admissions in the market decrease. We estimate market-specific D-D effects on hospital admissions and near-elderly mortality for each treated market by comparing its trend with that for all comparison markets. We then regress the effect on admissions on the corresponding effect on mortality. We weight each market by its population of 55–64 year olds in 2010 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.

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

We present binned scatter plots in Figure C.1. Panels (a) and (b) present the correlation between the effect on mortality (Y-axis) and the effect on Medicaid and Other admissions, respectively, on the X-axis. We present mean values in decile bins as nonparametric evidence and overlay a linear fit from the OLS model estimated on the underlying market-level estimates. The figures also mention the corresponding slope coefficients estimated by OLS and their bootstrapped standard errors. Panel (a) shows a clear downward sloping pattern, i.e., markets that experience a greater decline in Medicaid hospital admissions also experience a larger increase in near-elderly mortality. The pattern is remarkably linear across deciles. The slope coefficient is statistically significant and implies that a 4% decline in aggregate Medicaid volume, approximately what we estimate on average, predicts 3.6 more deaths per 100,000. Hence, the decline in Medicaid admissions can explain about 70% of the total increase in mortality. Similarly, Panel (b) shows an association between the effect on mortality and changes in Other admissions. The slope coefficient is even greater than in the case of Medicaid and is significant at the 10% level.

Intuitively, the effect on mortality should be greater among subgroups of the population that are more exposed to privatization. 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 C.1 columns 2 and 3 present the corresponding results. These results confirm that the average mortality effect in the treated 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 14.4 deaths per 100,000. This represents 1.4% of the baseline mortality rate. Table C.2 presents additional results on mortality for people residing in the affected county. Panel A shows the effects for different age groups. We estimate positive effects among people 55 years and older, which is intuitive because these groups are more likely to use hospital care and are more sensitive to changes in access or quality. Panel B provides a breakdown of the effects by cause of death. The highest percent increases in mortality rate are for people dying of diabetes, liver and kidney, and respiratory diseases. Hence, the increase in mortality is not limited to people dying from urgent factors.

The third test leverages the variation in the poverty rate across markets. Lower income markets have a greater share of Medicaid and uninsured residents, who are more likely to use government hospitals. Moreover, in Section 5.2.2 we show that these markets experience a greater decrease in Medicaid admissions after privatization. Therefore, we expect a greater increase in mortality in affected counties with higher poverty rates. We estimate a triple difference model to test this hypothesis. Table C.1 column 4 presents the results of this model. They show that the average effect on mortality in the affected counties reported above is primarily driven by those located within lower-income markets. The coefficient, statistically significant at the 10% level, implies an increase in mortality of approximately 39 per 100,000, or 4% of baseline mortality. This result suggests that publicly owned hospitals serve a vital social function in lower-income markets.

We use the average estimate at the market level to calculate the lives lost among the near-elderly. The average treated market had about 42,400 individuals in this age group in the year prior to treatment, and hence this estimate implies an increase of 1.9 deaths per year. Since treated markets experienced 1.2 privatizations on average, this further implies 1.6 additional deaths per privatization. To obtain an estimate of LYL, we follow the same approach we used in the case of FFS patients. Standard life tables suggest an average life expectancy of 23.1 for people aged 60 years (CDC 2014). To be conservative in our assessment, we again assume that affected people are at a higher mortality risk than the average person of the same age. Following Deryugina et al. (2019), we scale this down by 40% to 13.9 years. Therefore, we estimate 22.2 LYL (1.6 x 13.9) among the near-elderly following the average privatization.

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

(a) Medicaid admissions and mortality (b) Other admissions and mortality

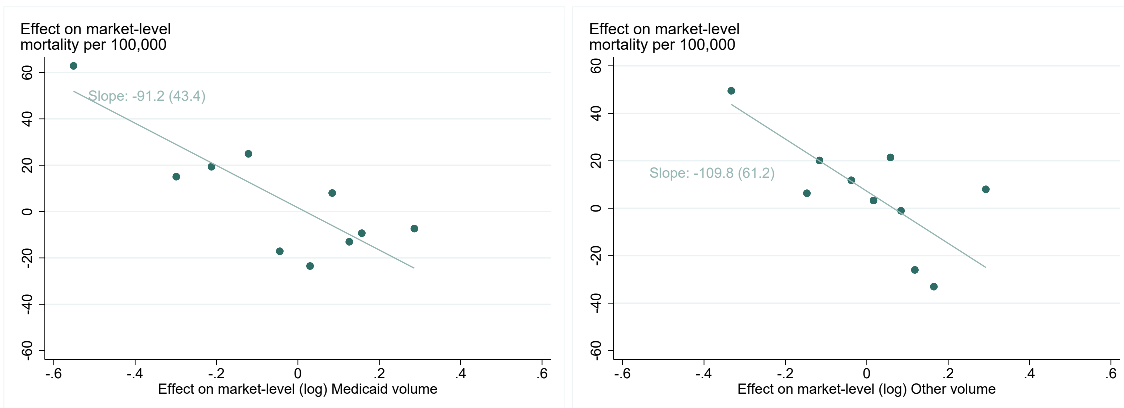


Figure C.1: Effect on market-level mortality

Note: The figure presents evidence on the effects of privatization on mortality at the market level. The 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 a) and "Other" patients (Panel b), respectively, across the approximately 200 markets experiencing privatizations. Each panel presents a binned scatter plot of the effect of privatization on mortality rates among 55–64 year olds per 100,000 population (Y-axis) against the corresponding effect on aggregate hospital volume in logs (X-axis) in decile bins. For each of these outcomes, we first estimate each affected market's D-D coefficient on mortality and on patient volume by comparing its trends to those for the full set of comparison markets. The plots overlay lines of best-fit and slope coefficients from a linear regression using the underlying market-level estimates. Standard errors for slope coefficients are in parentheses; they are bootstrapped over both steps to account for estimation error in the first step, where we obtain market-specific D-D estimates.

Table C.1: Effect on mortality

	(1) All	(2) Affected county	(3) Other counties	DD	(4) x 1(> med. poverty)
A: No controls					
DD	4.5	14.4	-5.1	-4.9	38.5
	(6.6)	(11.6)	(8.4)	(13.4)	(22.7)
Obs	19,288	19,288	19,136	19,288	
B: Market controls					
DD	6.3	16.4	-3.1	1.9	28.8
	(6.7)	(11.7)	(8.5)	(13.6)	(22.8)
Obs	18,555	18,555	18,404	18,555	
Mean outcome (t-1)	1,020.1	1,021.4	1,011.7	1,021.4	

Notes: This table presents the main results on market-level mortality for individuals aged 55–64 per 100,000. We define markets using Health Service Areas (HSAs), as described in Section 5.2.2. Mortality estimates are derived from Vital Statistics data from the NCHS. In column 1, treated units are HSAs that experienced one or more privatizations while comparison units did not experience a privatization. In column 2, we compare trends for the affected counties within the treated HSAs against the comparison HSAs. In column 3 we compare the unaffected counties within the treated HSAs against the comparison HSAs. In column 4, we present estimates from a triple difference model where we interact affected counties within the treated HSA with an indicator for being in an above-median poverty rate market. Mean values are computed for treated units in the year before treatment. Standard errors are clustered by HSA.

Table C.2: 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	7.2	-1.8	-3.0	-4.1	14.4	16.3
	(5.6)	(2.3)	(2.8)	(4.9)	(11.6)	(25.1)
Obs	19,288					
A2: Market controls						
DD	7.6	-1.4	-2.7	-1.7	16.4	23.1
	(5.6)	(2.3)	(2.8)	(4.9)	(11.7)	(25.2)
Obs	18,555					
Mean outcome (t-1)	1054.2	71.1	123.5	387.8	1036.5	4923.4

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	9.5	-8.8	4.7	4.3	3.5	1.2
	(5.6)	(5.8)	(2.5)	(2.3)	(2.2)	(5.5)
Obs	19,288					
B2: Market controls						
DD	10.8	-8.9	4.9	4.4	3.7	1.5
	(5.7)	(5.8)	(2.5)	(2.3)	(2.2)	(5.5)
Obs	18,555					
Mean outcome (t-1)	344.3	308.3	68.6	55.4	37.3	222.7

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 5.2.2. Mortality estimates are derived from Vital Statistics data from the NCHS. In all analyses, treated units are counties that experienced a privatization during the sample period, and control units are HSAs that never experienced a privatization. In Panel A, we show effects for all-cause mortality, split by mutually exclusive and exhaustive age groups. In Panel B, we show effects for ages 55-64 mortality, split by cause of death. To obtain these groups, we started with the ICD 39 cause recode groups provided in the data, which groups together similar ICD codes pertaining to cause of death. We then further aggregated these groups for ease of exposition. Mean values are computed for counties in the year before treatment.