The Impact Of Privatization: Evidence From The Hospital Sector*

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Abstract

Government ownership in the U.S. hospital sector, which accounts for 5.4% of GDP, has steadily declined for decades. A key driver has been the privatization of hospitals owned by local governments. Theory predicts that privatization will reduce government subsidies, but may impose costs on other stakeholders. We study these trade-offs empirically by leveraging all 254 privatizations that occurred between 2001 and 2018. Privatization increases hospital profitability, eliminating the need for subsidies. However, it also reduces access for poor patients and increases mortality among elderly patients. On average, privatization generates approximately $0.8 million in local government 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 priorities (La Porta and López-de-Silanes 1999).

Hart, Shleifer, and Vishny (1997) formalize this intuition using a stylized economic model which predicts that, relative to government operation, a private contractor will reduce costs and 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 hurt consumers. These insights are relevant to hospitals, as the average hospital is a highly complex organization and government contracts with private firms are likely to be incomplete. Thus, on the one hand, privatization is expected to 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 local governments' spending 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 in that same year. Improved profitability could therefore free up funds for other priorities. The magnitude of the increase in profitability, the underlying mechanisms, and whether it eliminates subsidies are empirical questions.

On the other hand, excessive 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 reduce the quality of care.

Despite the absence of research to help assess this trade-off, local governments throughout much of the U.S. have 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 future. For example, the state of Connecticut is currently investigating the potential for privatization to reduce subsidies for its only state-owned hospital, 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 could greatly affect the performance of this vital sector of the economy. Hospital care is the largest segment of the U.S. health care industry and accounts for $1.5 trillion in spending, approximately 50% of which is tax funded. It employs more than 7.2 million people, comparable to the construction sector.[2] But this debate is not limited to the U.S. In several countries, including Germany, Sweden, and China, there are ongoing discussions or actions to privatize health care providers (Dahlgren 2014; Heimeshoff, Schreyögg, and Tiemann 2014; Liu et al. 2020; 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. In approximately 40% of the instances, the private partner exercises managerial control over day-to-day operations, authorized by short-term contracts. In the remaining deals, the private firm also gains long-run financial autonomy and control over all assets. We identify privatizations by manually validating changes in control recorded in annual national surveys of hospitals by the AHA. Our comprehensive data includes medical claims for the universe of elderly fee-for-service (FFS) beneficiaries covered by Medicare, hospital discharge data or annual reports from five states, confidential national vital statistics microdata, and AHA annual surveys. We supplement these with data from Medicare cost reports, the U.S. Census Bureau, and several other public and proprietary sources.

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) as well as the organization of health care 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 effects at the market

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

2. Source: Hospital spending and source of funds reported in Table 4 of the National Health Expenditure Accounts, 2023. Current Employment statistics from BLS. The construction sector, NAICS code 23, employed about 8.3 million people in June 2025.

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 several 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 $2.5 million, or 4% of revenue. Private control increases reimbursements per bed by about 9% following privatization. However, these per-unit revenue increases are offset by a decline in patient volume, as we discuss below, and aggregate revenue remains unaffected. Aggregate profitability increases because operating costs decline by about 9%, driven by lower spending on personnel. These savings are sufficiently large so that the average privatization eliminates the deficit and need for government subsidy.

However, the improvement in hospital profitability is partly achieved by imposing costs on patients, and more generally, on consumers. The first channel through which this occurs is a reduction in access to care for less profitable, lower income patients. Hospital patients in the U.S. have different types of insurance coverage that offer widely varying reimbursements for the same service. Using nationally representative survey data, we find that Medicare, the public insurance program for the elderly, and private insurers pay hospitals about 45% and 60% higher rates, respectively, than the amount paid by Medicaid, the public insurance program for low-income or disabled individuals. Uninsured patients pay even lower amounts on average than Medicaid patients. Therefore, hospitals have a financial incentive to avoid lower paying Medicaid and uninsured patients. Following privatization, we find that hospitals disproportionately decrease admissions of Medicaid and uninsured patients. In contrast, we detect small and statistically insignificant changes in stays for Medicare and privately insured patients.[3] We also detect a 4% net decrease in aggregate Medicaid admissions at the market level following a privatization, which we interpret as a decrease in access to care. A greater decrease in Medicaid admissions is associ-

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).

ated with a greater increase in the mortality rate of individuals aged 55–64. This pattern argues against the notion that avoided hospital stays were superfluous.

Second, privatization leads to lower hospital quality on average. Using detailed Medicare claims data, we detect an approximately 3% average increase in 30-day mortality rates, considered the gold standard measure of hospital quality, among Medicare FFS patients 65 years and older in the privatized hospital. This impact on FFS patients alone implies 3.9 additional deaths and 20.9 life-years lost due to the average privatization per year. 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 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, such as obstetrics, which cater primarily to Medicaid and uninsured patients. Privatized hospitals also differentially increase their list prices, known as charges. These often form the basis of price negotiations with private insurers and apply to uninsured patients. Finally, we show that privatized hospitals reduce care inputs, including length of stay in the hospital and staff availability. The latter helps explain the decrease in operating costs. The patterns of reductions in staff 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. Prior studies have linked such changes in staff to elevated patient mortality (Friedrich and Hackmann 2021; Aghamolla et al. 2024). Privatized hospitals experience a large increase in CEO turnover immediately after entering private control, which may facilitate the implementation of these wide-ranging operational changes.

These average effects mask considerable heterogeneity depending on the type of acquirer and its level of control. The reductions in personnel and consequently, in operating costs, are achieved only in the (approximately 45% of) deals in which the acquirer is a large hospital system with complete financial responsibility for the facility. This pattern suggests that imposing a hard budget constraint on the acquirer may motivate cost reductions, but the acquirer needs to be of a minimum scale for privatization to generate savings. However, the transfer of managerial control alone, regardless of the acquirer's size, generates access and mortality effects.

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 estimates imply that the average privatization generates approximately $0.8 million in savings for the local government per additional death, or $150,000 per life-year lost. This is not a comprehensive welfare estimate since some of the

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-Silanes, Shleifer, and Vishny 1997; Morrison and Winston 2010; Levin and Tadelis 2010; Davis and Wolfram 2012; Borenstein and Bushnell 2015; Howell et al. 2022).

savings represent a reduction in access to care or higher prices for consumers, whose harms are not incorporated in the denominator. However, it offers a useful benchmark to policymakers considering alternatives to privatization.

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 health care, these studies have generally focused on the differences 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).

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 describes heterogeneity in the effects by the type of deal, acquirer, or geography. 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 Variation in government provision of hospital care

There is substantial heterogeneity in hospital control across different geographies.[6] This is true not only of the share of hospitals that are publicly owned in a market but also of the type of privately owned hospital (nonprofit or for-profit). Table A.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 all states and DC in 2019. The states are ordered in descending order of the nonfederal public share of hospital beds. In this table 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 strongly suggests there will be more waves of privatization in the future. Hence, the role of government in the delivery of hospital care deserves greater research scrutiny.

2.2 Decline in government provision 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 similar, though 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

6. The AHA survey reports hospital "control," which could be recorded as 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.

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%.[8] Unlike Medicaid, eligibility for this program has been relatively stable and a large part of the increase is due to aging of the population.

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.[9] Recall that the national share of nonfederal public hospitals dropped by about 10 percentage points during this period; hence this effect size is economically meaningful. While not causal, the negative and statistically significant association is consistent with the hypothesis that local policymakers may view public health insurance expansions as a substitute for public provision of hospital care.

2.3 Hospital privatization and closure

Privatization

Much of the decline in nonfederal public hospitals has occurred because state and local governments have increasingly relinquished operational control of hospitals to private partners. For expositional simplicity, we refer to these private partners as "acquirers" although they do not necessarily purchase the facility in all cases. We identify and study 254 instances of hospital privatization during the 2001–18 period. To put this figure in context, it implies that a quarter of the 1,010 public hospitals in our sample in 2000 were privatized in 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.[10]

Our review of the news coverage of privatizations suggests that two drivers appear to be important for privatization. One is to reduce government subsidies devoted to hospital care while

8. 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.

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

10. 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.

continuing to offer hospital services, as opposed to closing the facility. The other is ideological and stems from the belief that governments should not operate hospitals and that they are better left to private sector experts.

Privatization can change the economic environment for government hospitals in three important ways. First, a private firm with objectives distinct from those of the local government takes control of daily operations at the hospital. As we discuss in the next section, there is heterogeneity in both acquirer type (e.g., for-profit status) and their revenue stream from privatization. In some cases, the acquirer receives a contractually fixed amount per year for operating the facility while in other cases it retains a portion or all of the profits. That being said, it is reasonable to assume that the acquirers are more profit oriented than local governments on average. The change in firm objectives will affect decisions related to staffing, the mix of services offered, clinical care protocols, and patient mix. These levers are typically under the control of the new management regardless of the type of privatization pursued.

Second, flowing from the change in firm objectives, the acquirer may change key performance metrics for top management, including linking some part of their compensation to meeting operational or financial targets. This is a common practice in the private sector to align managerial objectives with those of the firm (Prendergast 1999). As we will show later, acquirers also often change the hospital CEO when they take over. Changing top management likely further reduces resistance to the change being sought by the acquirer.

Third, if the acquirer assumes financial control of the facility, such as in a long-term lease or purchase arrangement, then it can take decisions with potentially long-run consequences such as investing in new equipment and facilities, issuing new debt, selling assets, and making deeper cuts to hospital staff. On the other hand, governments typically end most subsidies for hospital operations in such arrangements. Hence, the acquirer has both a stronger incentive and greater ability in such situations to move the hospital toward financial sustainability, and potentially to maximize profit.

Closure

Although not a focus of this paper, it is worth briefly considering closure as an alternate policy tool. Governments could choose to shut down hospital operations instead of privatizing if the status quo is not sustainable. However, we find that only 36 government hospitals, or 3.5% of the facilities in 2000, were closed during the same period.[11] As we show later, the hospitals that were closed had lower bed capacity, served substantially fewer patients, and were deeply unprofitable relative to the facilities that were privatized. Hence, governments appear very reluctant to shut hospitals, and they exercise this option only for small and poorly performing facilities.

11. We define a closure as occurring when a hospital shuts both inpatient and outpatient operations and they remain shut through 2019. If a hospital shuts but reopens in a few years, or moves to another location, or changes its name because of an acquisition, we do not consider that a closure.

2.4 Types of privatization arrangements

Table 1 describes the types of deal structures and acquirers we observe in the data. We briefly describe their features and implications for the acquirer's incentives.

Degree of private control

Table 1 Panel A classifies deals into five groups based on the deal structure and the implied level of control the acquirer enjoys. We did not have access to the contracts between governments and acquirers and relied on press releases and independent reporting for this classification. We list the categories approximately in increasing order of private control, i.e., the acquirer has least control in a "public hospital incorporating" arrangement, and the most control in a sale. For each deal type, we also report the breakdown by nonprofit and for-profit acquirer type. For brevity, we do not delve into the specifics of each deal type here. Appendix B.1 provides details along with illustrative examples.

The first three categories, contributing 43% of all deals, share a common feature that the government retains ownership of the land, other assets, and debt liability. The acquirer controls day-to-day operations and can press the levers discussed above to achieve its objectives, including making changes to top management and their incentives. However, the acquirer lacks meaningful financial control. The most common deal type in this set is for the government to outsource operations to a hospital management firm. We refer to this as contract management.

The latter two deal types, long-term lease and sale, give the acquirer meaningful financial control in addition to the ability to influence operations and managerial incentives. The partner enjoys a long-term planning horizon, is free to decide on capital investments, and is the residual claimant on profits. Interestingly, while nonprofit firms partner with governments in arrangements with less or more control in roughly equal proportion, for-profit acquirers enjoy more control in nearly 75% of their deals.

Partner organization type

Table 1 Panel B presents the number of deals associated with different types of partners based on whether they are independent hospital operators or systems, and in the latter case, the system size at the time of the deal. We supplement AHA data with manual searches to assess the organization type and scale of the partner for each deal. Approximately a quarter of the partners are small firms that only operate the focal hospital after the deal and they are mostly found in arrangements with less control such as public hospital incorporating and contract management.

We classify partners organized as systems into small (fewer than 5 facilities at the time of the deal), regional (5 or more facilities but span less than 4 states), and national (5 or more facilities and span 4 or more states). Although these definitions are somewhat arbitrary, they are intuitive and produce reasonable results.[12] Small, regional, and national systems are linked to, respectively, 2.7, 9, and 36 hospitals on average, and are therefore differentiated in both size and geographic

12. We use the threshold of 4 states to designate national status because approximately 10% of multihospital systems span 4 or more states in the full AHA sample, so these firms constitute the top decile in terms of geographic range.

range. Governments partner with regional or national systems in over 60% of the deals, suggesting that larger partners offer more attractive terms or inspire more confidence. Andreyeva et al. (2024) show that, in the case of acquisitions of private hospitals, larger acquirers obtain larger cost reductions at the target hospital.

Heterogeneity in treatment effects

We formally test heterogeneity in treatment effects on these and other dimensions in Section 7, after presenting the main results. Intuitively, we expect to find a greater increase in the privatized hospital's profitability when the acquirer has financial control or when the acquirer enjoys greater economies of scale.

2.5 Conceptual framework

We use the framework of 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. It generates predictions on the 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. Contract incompleteness 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 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 health care 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 for hospitals since they negotiate prices with private insurers. 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 or can carefully shift the hospital's focus toward more lucrative services, if they are sufficiently motivated. In the longer run, they can improve the hospital's quality and reputation to strengthen their bargaining leverage. Hence, we hypothesize that privatization will lead to an increase in revenue and 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 excessive cost cutting in the context of the hospital sector leads to two additional hypotheses. First, 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, based on their payer type or reason for admission. Our analysis of data from the Medical Expenditure Panel Survey (MEPS) shows that Medicare and private insurers pay hospitals about 45% and 60% higher rates, respectively, than Medicaid. Uninsured patients pay even lower amounts on average than Medicaid patients. 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. A reduction in the volume of lower-income patients, by itself, need not imply worse health outcomes. We therefore test for both changes in access to care and changes in health indicators for the affected population. A worsening of health along with a decrease in hospital care would suggest that avoided hospital stays had clinical value.

Second, cost reductions at the hospital may outweigh quality improvements, in which case the net quality of care may decline. Hospital care is labor intensive, with personnel spending contributing more than half of total operating cost, according to AHA data. Hence, staff reductions offer a path to 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, versus harmful effects to consumers arising from a decrease in access and/or quality. 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.

3 Data and descriptive evidence

3.1 Data sources and sample construction

We compile data from multiple federal, state, and proprietary sources with complementary strengths. 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 (American Hospital Association 2025). We study inpatient volume by payer and in aggregate. 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 summing hospital stays and the number of outpatient visits weighted by the ratio of outpatient charges to inpatient charges to account for their lower resource intensity. We also examine effects on hospital leadership (the CEO) and staff employment.

We identify privatizations 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 of news articles, press releases, hospital websites, and proprietary datasets 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 our baseline sample. In several cases, manual validation also helps to correct the year of privatization. This yields 254 validated 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 hospital-year panel. 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.

The local government may choose to close the hospital instead of privatizing it or continuing operations. We use an analogous methodology and identify 36 government hospitals that closed during the same period. Of these, 5 hospitals were privatized before they eventually shut down. We use this information to briefly analyze the effects of government hospital closures, which we discuss in Section 8.

Administrative data from select states

As discussed in Section 2.5, 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 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 (HCUP 2026; CA-HCAI 2026; MN-DOH 2026; IN-DOH 2026). Collectively, we observe 27 privatizations between 2008–2018 in these 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. This allows us to observe the universe of hospital stays across all payer types. In the case of California and Minnesota, we use annual reports, which allow us to observe patient volume for each payer type. 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 for 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 (ResDAC 2026). We observe hospital stays for all FFS patients nationwide during this period. In a typical year, there are about 30 million Medicare FFS recipients, which account for a substantial share of all hospital stays given the disproportionate use of hospitals by the elderly. Since this sample starts five years later than the AHA sample, we are able to study 50 fewer privatizations when we impose the same sample construction rules. Medicare data allow us to test for changes in the observed health risk of admitted patients, as we can use a patient's complete health care utilization history to develop risk indicators. We limit our analysis to patients aged 65 years and older, who represent the primary beneficiary group within

Medicare.[13] 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 use multiple supplementary data sources to obtain additional outcomes of interest or covariates that are not available in the main datasets discussed above. We obtain data on hospital revenue and contract labor use from the Healthcare Cost Reporting Information System (HCRIS), more commonly known as Medicare cost reports (CMS 2022). To our knowledge, this is the only national source of data on hospital revenue.[14] 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.7.1, respectively.

We include a rich vector of time-varying market-level covariates in our regression models, obtained from a mix of public use and proprietary sources. We discuss the covariate choices in more detail in Section 4 in the context of our research design. They include measures to control for differences across markets in the economic environment (population, poverty, unemployment, and uninsurance rates), preference for fewer government services (proxied by the vote shares for Republican candidates in U.S. House elections and Affordable Care Act (ACA) Medicaid expansion status), local government financial health, and the level of municipal debt. Data on economic indicators are obtained from the U.S. Census Bureau and the Bureau of Labor Statistics. Section B.7 describes the sources for the remaining covariates and variable construction for these measures in detail.

Variable construction

Since there is substantial heterogeneity in size across hospitals, we follow the previous literature and examine changes in outcomes either in logarithmic points or after scaling them by beds or the number of patients (Schmitt 2017; Garthwaite, Gross, and Notowidigdo 2018). Industry analysts also prefer per-unit measures to assess and report operational and financial performance (KaufmanHall 2025). In the case of hospital financial measures (revenue and costs), values vary tremendously between hospitals even after scaling by hospital size, and so we log transform these after scaling by hospital size. For completeness, we also show results pertaining to log finances without scaling by bed capacity. Throughout, all monetary values are adjusted for inflation and are expressed in 2019 dollars.

13. According to the Kaiser Family Foundation, in 2019, approximately 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.

14. Revenue values reported in HCRIS have a high degree of concordance with the corresponding values in administrative reports from California and Minnesota, which are more detailed, more likely to be audited, and generally considered of high quality.

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* privatized hospital. This group comprises our primary comparison group. We impose this distance requirement to mitigate the potential for spillover contamination.[15] Comparing these columns shows 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, suggesting that they may have been leaner than the comparison group prior to the change of control. Privatized hospitals were in slightly better financial condition at the beginning of our study compared to the remaining public hospitals.

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 were much less likely to be located in a rural county, 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 full-time equivalent 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.

Table A.2 explores the differences between government hospitals that are privatized and those that shut down. It also presents values from the start of the sample period and can be directly compared against Table 2. We assign the government hospitals in columns 1 and 2 of Table 2 to one of four mutually exclusive groups: (1) privatized but did not close; (2) privatized and closed; (3) closed without privatizing; and (4) neither. The table makes it evident that the facilities targeted for closure (col. 2 and 3) were already very different in 2000 from the vast majority of those that were privatized (col. 1) as well as the residual group that experienced neither change (col. 4). These two groups are relatively similar to each other. The hospitals that closed were much smaller, had lower occupancy rates, served a higher share of less profitable Medicaid patients, had substantially fewer staff per bed, and were extremely unprofitable. In contrast, privatized hospitals had healthier financial performance and greater staff availability. Overall, it appears that local governments shut down hospitals rarely, and usually only small and very unprofitable

15. 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.

facilities. Therefore, the most likely counterfactual for most privatized facilities is that they would continue operating under the government as before.

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. Privatization is widespread: 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 and the passage of the ACA – there were 15.4 privatizations 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 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 204 privatized hospitals and 776 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.[16] 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 claims data.

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

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

the average treatment effect on the treated (ATT) using a staggered difference-in-differences (D-D) research design. 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.[17]

In addition to presenting evidence on pre-trends for all outcomes, we take several steps to strengthen the research design. First, we make the sample more homogeneous by excluding facilities classified as specialized (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, for completeness, we retain the 31 comparison hospitals that closed during the sample period, even though they may not be very comparable. This introduces an econometric challenge of zero values of patient volume in a log model (Chen and Roth 2024). Our preferred approach is to include observations for these hospitals as long as they have nonzero patient volume. We get qualitatively similar results with a Poisson maximum likelihood modeling approach that includes observations for closed hospitals with zero values through 2019. Fourth, to ensure that the estimated pre-trends are not partially driven by compositional shifts, we require that privatized hospitals are observed for a minimum of five years prior to the transition. Finally, we focus on a 5-year window around privatization. We find an estimated effect more credible if privatized hospitals deviate from the comparison group within this window 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 occurs 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 are overall very similar.

Equation 1 below presents our baseline model. Y_{ht} denotes the outcome 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 unobserved selection. 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-

17. A desirable alternate approach would be an instrumental variable design. We carefully explored several candidate instruments based on local political preferences and government finances. However, no combination of instruments produced a sufficiently strong first stage.

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.

We test the sensitivity of the estimates to including a vector of time-varying covariates X_{mt}. These are market- and state-level attributes that help control for differences in economic, political, and administrative factors plausibly correlated with the decision to privatize and the outcomes of interest. It comprises measures of voter preference for small government (Republican vote share in U.S. House elections and state Medicaid expansion under the ACA); local government financial measures (ratio of total revenue to expenses and per-capita surplus amount); municipal debt issued in the year (log total amount and an indicator for any health-specific debt); and county-level economic indicators (population, unemployment, poverty, and uninsurance rates). Finally, we also include a measure for the county's exposure to privatization, the cumulative fraction of privatized government hospitals in other counties in the same hospital referral region. This measure helps control for differences in unobserved local factors that lead to privatization.

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

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, which mechanically gives greater weight to hospitals with more patients. This allows us to include patient covariates to control for differences in patient mix across hospitals. We include a vector of patient demographics and a mortality risk index predicted using 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 estimate dynamic effects around privatization using 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 estimator (TWFE) in our baseline model due to its simplicity 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 assess the sensitivity of the baseline estimates to the use of 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 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 2.5, increase revenue as well. Hence, we begin our analysis by examining the effects on hospital finances. Table 3 reports D-D coefficients from 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 mitigates concerns about model misspecification and omitted variables. We prefer to focus on the estimates obtained without 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 a statistically significant 8.3% increase in log revenue per bed, which upon exponentiating implies an increase of 8.7% in revenue per bed. For uniformity, we exponentiate all log coefficients in the text, unless otherwise specified. 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 small and statistically insignificant decrease. 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 implies a statistically significant decline in personnel cost per bed of

6.5% (col. 3). This 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 a small statistically insignificant increase in costs elsewhere (col. 4). 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 8.7% increase in revenue alone is sufficient to enable these hospitals to generate a modest surplus. These results are not sensitive to our choice to scale by beds. We also estimate a companion set of models in which we express financial measures per contemporaneous adjusted admissions instead. These estimates represent the percent change in mean reimbursement or cost per patient. Figure A.2 and Table A.5 Panel A present the corresponding event study plots and point estimates, respectively. This approach produces similar estimated effects.

Our primary measures of hospital finances are scaled by beds or patients since they help normalize differences in size across facilities and better reflect changes in operational efficiency. However, one may also be interested in the effect of privatization on aggregate revenue and costs. This has the virtue of incorporating changes in beds or patient volume that occur following privatization. Table A.5 Panel B presents the corresponding results with aggregate log financial outcomes. In the year prior to privatization, the average treated hospital had an annual deficit of \$3.7 mn, which drops in magnitude to \$2.5 million after eliminating outlier values. The coefficient on operating costs (col. 2) implies a decrease of 9%, or \$5.7 mn. Hence, these results also imply that privatization helps hospitals generate a surplus. The decrease in costs is driven mostly by a decline in personnel spending, as in the main results. The key deviation from the per-bed results is that we detect no change in aggregate revenue. An increase in average reimbursement without an accompanying increase in aggregate revenue hints at an offsetting decrease in the number of patients served. The next section directly tests this hypothesis by examining the effects on patient volume.

5.2 Patient volume

This section investigates the effects of privatization on patient admissions. We first examine hospital-level effects, then market-level effects. 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 reports hospital-level effects on patient volume. Panel A presents results using the national sample of hospitals from the AHA. We present effects on total patient admissions as well as 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 coefficient on total patient admissions implies a decrease of 8.5% 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 to outpatient care after privatization. For example, the hospital may focus on elective surgeries and other services that can be provided in the outpatient setting. Alternatively, the hospital may change clinical protocols in the emergency department (ED) and discharge a greater share of these patients without admitting them. We test both these conjectures and fail to detect an accompanying increase in ED or outpatient care volume at privatized hospitals. Table A.6 columns 1 and 2 present the corresponding effects on the log of 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 and ED treatment as well. Figure A.3 Panels (a) and (b) present the corresponding event study plots that corroborate the D-D coefficients.

We then examine heterogeneity by payer. As noted earlier, reimbursement rates vary substantially across payers. Previous studies have suggested that Medicaid and uninsured patients are unprofitable to serve on average (Frakt 2011; Schulman and Milstein 2019). We examine heterogeneity in payer reimbursement 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. MEPS data show that Medicare and private insurers pay 45% and 60% more than Medicaid, respectively, while uninsured patients pay less. 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 over 14%, Medicare admissions decrease by an imprecisely estimated 5%. The decrease in the Other group is similar in magnitude to that of Medicaid. The event study plots in Figure 4 show that privatized hospitals did not trend differentially prior to the transition, which supports parallel trends. They are also 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, which suggests that the decline is not a transient phenomenon. 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 5 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 9.0% from 8.5%. 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, are more frequent among comparison hospitals. The closure rate during 2001–19 among comparison hospitals is 3.9% (31/802) while it is 2.0% (5/254) for privatized hospitals. While the difference on the extensive margin partly reflects baseline differences between the two groups, e.g., privatized hospitals are financially much stronger on average (see Table A.2), not accounting for it may overstate the decrease in patient volume due to privatization. We perform a sensitivity check in which we retain observations for closed hospitals in the sample and assign them zero volume. We estimate Poisson MLE models for this analysis, as recommended by Chen and Roth (2024). Reassuringly, these models also imply large and statistically significant decreases overall (7.7%) and in Medicaid admissions (10.7%). The effect on Other decreases in magnitude and loses precision (6.4%) but remains within two standard errors of the corresponding main estimate.

We hypothesize that declines in the Other group are driven by uninsured patients. We test this using detailed administrative data that we were able to obtain from five large states (California, Florida, Indiana, Minnesota, and Washington), together representing 27 privatizations. The small sample of privatized and nonprivatized hospitals necessitates an alternate modeling approach to ensure parallel trends between the two groups so we can credibly implement a D-D design. Therefore, we use the 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.[18] 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: privately 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

18. 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.

decrease in Other is mostly driven by a large drop of 37% in uninsured admissions. 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 to highlight the difference between the effects on the less lucrative payers, Medicaid and uninsured, and 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.

5.2.2 Admissions at the market level

Privatized hospitals 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 perceived as unprofitable, then other hospitals may be reluctant to offset the decline at the privatized hospital. For policymakers, 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 apply our research design 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 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 hospital referral region contains 18 hospitals. Consequently, we have greater statistical power to detect the market-level effects of a single privatization. At the same time, HSAs adequately capture a patient's hospital choices. Using Medicare claims data, we confirm that more than 70% of fee-for-service (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 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. As with the hospital-level analysis, 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.[19]

The key finding is that hospital privatization predominantly affects Medicaid patients, who experience a meaningful decline at the market level. In contrast, we estimate small positive effects on both Medicare and Other volume. The effect on Medicaid is -4.1 pp, slightly higher than what we would predict based on the privatized hospital's decline alone (21% of -14.4, or -3.0 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.

The average decline in aggregate Medicaid hospital stays may mask heterogeneity across markets. An Institute of Medicine report noted that hospitals located in markets with a higher proportion of residents in poverty have lower operating margins (Institute of Medicine 2003). Following from this, we hypothesize that the remaining hospitals in lower income markets will have less financial cushion to accommodate the spillover Medicaid (and uninsured) patients when a neighboring government 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 C presents the corresponding coefficients of interest from the triple difference model. The results clarify that privatization has little effect 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, the implied coefficient for markets with greater poverty

19. 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. We do not report these results in the interest of brevity.

is -3.0% (2.3 - 5.3 = -3.0), 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). 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 appears to play a role in exacerbating the effect of privatization on hospital access.

5.2.3 Interpreting the decrease in aggregate Medicaid admissions

To assess the value of avoided stays, 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 (NCHS 2023). For brevity, we describe this data set and analysis in detail in Appendix C. We briefly note here that we find a significant correlation between the two effects. Specifically, a larger decrease in Medicaid admissions following a privatization is associated with 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 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 excessive cost cutting.

5.3 Hospital quality

As discussed in Section 2.5, private operators may 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, and a comprehensive examination of the two is out of scope for this paper. We therefore focus 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).

Ideally, we would 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 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. To ensure five pretreatment years for all treated hospitals, we drop hospitals privatized over 2001–2004 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. The final sample for this analysis includes 204 privatized and 776 comparison hospitals.

We estimate a patient-level equivalent of 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. We control for demographics and a patient risk index, predicted using coefficients from a probit model of mortality at 30 days explained by demographics (age and sex), indicators for 30 comorbidities used to construct the Elixhauser score, and the history of health care utilization within the last 90 days (hospital stays and ED visits). This index parsimoniously controls for differences in mortality risk. To account for potential estimation error in the index, we bootstrap standard errors, including the first step probit estimation in the bootstrap loop.

Table 6 presents the corresponding coefficients. Panels A and B present coefficients from models without and with time varying market-level covariates, respectively. Panel A reports an increase in mortality of 0.33 percentage points across all FFS patients aged 65 or older, approximately 3% of the mean mortality rate. Controlling for differences between markets increases the magnitude of the coefficient. Figure 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. The estimates remain 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 examine 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 of ages between 65 and 80 versus those aged 81–99, 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, 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 the 0.33 pp estimate, we calculate that the average privatization in our sample leads to an increase of 3.9 deaths among FFS patients per year.[20] 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. 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 20.9 LYL (3.9 x 5.3) among Medicare FFS patients per hospital privatization per year.

This is a conservative estimate of deaths, and, consequently of LYL. Prior studies have often considered the effect on one-year mortality as the primary measure of hospital quality (e.g., Doyle Jr et al. 2015; Carroll 2023), which would double the number of implied deaths in our sample. It also excludes the effect on approximately one-third of Medicare patients who were enrolled in private Medicare Advantage plans. We do not observe hospital stays for these patients and the estimate for FFS patients may not apply to those in Medicare Advantage.

5.4 Robustness

We test robustness 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. The specification check models also do not include market 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 robustness to alternative specifications. Row IA presents coefficients obtained

20. The average privatized hospital served 1,273 fee-for-service patients at baseline (Table A.4). Allowing for a 5.2% decline in volume based on the main result in Table 4 Panel A, a 0.33% increase in mortality implies 3.9 additional deaths per year.

from regressions that weight hospitals by beds.[21] 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 compares hospitals within states. 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 are estimated in a previous step using data over 1996–2000, prior to the first privatization in our sample. Across all 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 address potential bias from 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.

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, 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 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 robustness to changing the sample of treated units. 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 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 balance 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

21. 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.

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. We assess balance using standardized differences (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 (1.6%), much lower than in the comparison group as a whole (3.9%). 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.

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

Administrative data from the selected 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 relatively unprofitable service line. 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 (except Minnesota, which does not report obstetrics volume separately) 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 reflects both 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.[22] We find that the probability of closure increases by 13.3% after privatization, which represents an increase of 70% relative to 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, the evidence above suggests that shifting away from less profitable services is a plausible strategy. Changes in service mix 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 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 only 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 this result interact with market-level Medicaid declines? Since more than 98% of births in the U.S. occurred in hospitals during this period (MacDorman and Declercq 2019), the 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.[23] We cannot test this directly using aggregate Medicaid births and non-births due to data limitations. However, these patterns imply that the decrease in care is borne by patients admitted for other services.

We provide 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 and Table A.5. Armed with the estimated effects on admissions by payer and the corresponding average re-

22. 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.

23. 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).

imbursement 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. Changes in the payer mix predict an increase in mean reimbursement from approximately \$12,560 to \$12,770 (1.7%), about 30% of the total increase.

6.2 Billing practices

We test whether billing practices change under private management. We first test for an increase in prices. Managers employed by the government may set prices inefficiently low 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. Ideally, we would observe prices negotiated with 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 ("charges") for Medicare FFS patients and test if these increase after privatization. Although list prices do not affect standard Medicare reimbursement rates, they affect prices for several types of patients. Private insurers often negotiate prices as a fraction of the charges (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, uninsured patients 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 6.8% 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.

We cannot directly estimate the contribution of an increase in the list price to the increase in mean reimbursement since we do not observe transaction prices. However, we 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 price to the share of hospital revenue contributed by patients affected by 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 1.3 percentage points, depending on the share of commercial insurance contracts based off 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 reimbursement rates incorporating the higher list price. We predict an increase in mean reimbursement of 2.2% to 2.9%.

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 additional comorbidities for patients so that they can be billed in a higher-paying category (known as a diagnosis-related group). We test 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 diagnosis-related group "weight" billed to Medicare, which determines the payment amount. We also do not detect an effect here. These results suggest that upcoding does not increase after privatization.

Alternate event study plots using the CS estimator are similar, shown in Figure A.10 Panels (a) and (b). Similarly, the results are robust to alternative specifications, as seen in Table A.11.

6.3 Care inputs

This 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 0.71 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 stays accounts for about half the decline in length of stay. Figure 8 Panels (a) and (b) present the event study plots, which corroborate the 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) present 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 labor inputs. Table 2 shows that personnel spending accounts for over 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.

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 use Bureau of Labor Statistics (BLS) data which sheds light on the national labor shares of different occupations in general medical and surgical hospitals in 2019 (Bureau of Labor Statistics 2019). We separately observe the occupation shares at local government and private hospitals and present the values in Table A.12.[24] 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. These data imply that more than half of the employees in the other group perform patient care roles. This includes roles in health care practitioner and technical, health care support, and community and social services.

There are some notable differences in the shares of key occupations between local public and private hospitals. Government hospitals employ relatively more physicians, but relatively fewer nurses (as defined by the AHA). If 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. We view staff availability as a mechanism to explain the changes in per-bed operating cost and patient mortality presented in Section 5 and hence examine effects on staff per bed or per patient. Table 11 presents the estimated effects on FTE staff employed per 100 contemporaneous beds. This measure includes both full-time and part-time employees. Column 1 presents the effect on the total staff. Total employment declines by 33 FTE per 100 beds. Compared to the mean level at baseline, this implies a decrease of 6%

24. This is the industry-occupation matrix data from the BLS. 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.

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 counterparts in private hospitals.[25] 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 find a 25% decrease in physician FTE. A decrease 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 corresponding event study plots. 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 is substitution toward contract labor. In that case, reductions in FTE may reflect reclassification 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 Medicare cost reports. The corresponding result is presented in Table 11 column 5. The coefficient is small 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.

These results are robust to scaling FTE by the number of patient admissions instead. 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. 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 columns 1–3 present the results from the usual set of robustness checks which show that the coefficients are not sensitive to the changes in the specifications, estimators, or sample construction.

6.4 Hospital leadership

How do the acquirers implement substantial operational changes so quickly? The management literature on mergers and acquisitions has extensively examined this question and one key channel appears to be to change top management. In a study of over 1,000 target firms after M

25. 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.

& A, Krug and Shill (2008) find that attrition among senior executives more than doubles in the first year after the deal and remains elevated over the next 10 years. New owners may replace incumbents with executives that they consider more competent or more aligned with their objectives. This conjecture has empirical support: Bertrand and Schoar (2003) document large variation in leadership styles across chief executive officers (CEOs) of private firms, and CEO fixed effects explain a large fraction of the variation in firm performance. The evidence on the effects of leadership in government firms is somewhat mixed. Muñoz and Otero (2025) show that a reform in Chile which replaced physician CEOs with executives with management training substantially improved quality of care. In contrast, Janke, Propper, and Sadun (2019) fail to detect changes in performance at public hospitals in England after a change in the hospital director. A potential explanation for the difference is that the reform in Chile replaced CEOs with external hires, while in England it rotated incumbents. Although an extensive examination of changes in hospital leadership is out of scope, we show that privatized hospitals experience higher CEO turnover in the years around the change in control.

Table 11 Column 6 presents the estimated average effect on the probability of a change in the hospital CEO in the 5 years after privatization. We detect an average increase of 4.8 percentage points against a baseline value of 24, implying a 20% increase. Figure 9 Panel (e) presents the corresponding event study plot and shows that the increase is concentrated in the first year after the transition, where we find nearly a 20 percentage point (80%) jump over the pre-treatment level.

Recall that our baseline models exclude the year of the transition from the analysis sample. This approach helps reduce the influence of transitory changes when examining other outcomes such as patient volume but it may inadvertently understate turnover if these changes begin in the year of the deal. The robustness checks in Table A.14 Column 4 help assess the importance of this omission. Across all the different checks, the coefficients remain similar in magnitude or larger than the baseline estimate. In row IIIB, where we retain all observations for the privatized hospital in the sample, including the year of the deal, we estimate a 6.4 percentage point average increase in CEO churn, 33% larger than the baseline estimate. Figure A.11 Panel (e) presents the event study plot using the CS estimator and confirms that there is a sharp increase of nearly 10 percentage points in CEO churn in the year of the deal as well. Overall, new owners replace top management relatively quickly, likely facilitating other operational changes.

7 Heterogeneity in treatment effects

This section tests theories related to the level of control the acquirer enjoys or its organization type. We examine heterogeneity in treatment effects using triple difference models, leveraging variation in the dimension of interest in addition to the usual D-D design. We interpret these results with caution, recognizing that deal types and acquirer organization are jointly determined along with the decision to privatize. Table 12 presents the associated results for a subset of key outcomes spanning hospital finances, patient volume, staff, and mortality among Medicare patients. The top row repeats the baseline D-D estimates for these outcomes to facilitate comparison. Additional results on heterogeneity are presented in Table A.15 and discussed briefly.

7.1 Financial control

When the acquirer purchases the hospital or enters into a long-term lease, it gains meaningful financial control over the hospital in addition to managerial control. Thus, studying the difference in effects between these two deal types and the rest allows us to assess the differential effect of the acquirer exercising financial control. This may affect operational decisions through two channels. First, the acquirer has greater ability to maximize profit. For example, the acquiring firm can make much deeper changes to staffing and procurement, which could manifest in greater changes in costs. It also has greater incentive to do so since it is the residual claimant on profits. Second, local government subsidies typically end in such cases, adding urgency to achieve financial sustainability and reinforcing the first channel.

Table 12 Panel A presents the corresponding coefficients. We present the D-D coefficients, which reflect the effects for 108 hospitals privatized in deals where the acquirer lacks financial control (see Table 1 Panel A1). The triple difference coefficient reflects the incremental effect for 146 hospitals privatized in deals where the acquirer also has financial control. The sharpest difference in effects between the two sets of deals is on operating cost (col. 3), where we find that total costs decrease only when the acquirer also has financial control. This appears to be driven by reductions in staff availability (col. 6), again only observed in the latter set of deals. The triple difference coefficients for both outcomes are precisely estimated. These results are consistent with the hypothesis that a hard budget constraint motivates acquirers to take unpleasant and politically risky actions like layoffs. With managerial control only, they primarily focus on increasing revenue per bed. We also find a larger increase in mortality rates when the acquirer has financial control, perhaps driven by the larger decline in staff availability. However, the triple difference coefficient on mortality is not statistically significant.

7.2 Acquirer organization type

Next, we examine whether the organization type of the acquirer is associated with significant differences in hospital outcomes after privatization. For brevity, we focus on two attributes that have received much attention in the prior literature: for-profit status and scale.

Although economists have extensively studied differences between for-profit and nonprofit hospital firms, the evidence is mixed. Older studies have tended to find little difference in profit orientation (e.g., Duggan 2000; Sloan et al. 2001). In contrast, more recent studies have found that nonprofits differentially act in ways consistent with mission orientation (Garthwaite, Gross, and Notowidigdo 2018; Gupta, La Forgia, and Sacarny 2024). Table 12 Panel B presents the corresponding results. We find limited differences in strategy between nonprofit and for-profit acquirers. The latter do obtain larger increases in revenue and decreases in costs, but the triple difference coefficients are small and insignificant. The main difference is on patient volume, where we find that for-profit acquirers increase total patient admissions rather than decrease them. The differential increase in admissions relative to nonprofits is statistically and economically significant. Overall, it appears that for-profit acquirers focus more on revenue and volume growth than on cutting costs. This pattern may partly reflect the fact that targets of for-profit acquirers had lower operating costs (and staff) per bed at baseline relative to those of nonprofit acquirers. Hence, it may have been more fruitful to focus operational changes towards growing revenue.

In a supplementary analysis, presented in Table A.15 Panel A, we report effects on for-profit acquirers separately for those owned by private equity (PE) firms versus not. Only 11 deals involve PE acquirers and so the corresponding triple difference coefficients are all imprecisely estimated. However, the patterns suggest that PE acquirers are much more focused on cost cutting than are non-PE acquirers. There is a large increase in mortality at hospitals acquired by PE firms, another example of a negative association between the effects on staffing and mortality.

We then examine heterogeneity by acquirer scale. In a study of over 100 acquisitions of independent hospitals by systems, Andreyeva et al. (2024) show that systems obtain cost savings at the target hospital mainly by decreasing employment and that the savings increase with the acquirer's scale. Since the cost savings in our setting share the same mechanism, we test for similar scale economies. Specifically, we test for differential effects when the acquirer is a large system, defined here as a system that owns or operates 5 or more hospitals at the time of the deal. Table 12 Panel C presents the corresponding results. Large systems are acquirers in about 160 of the 254 deals in our sample and, indeed large systems generate larger cost and staffing reductions. Table A.15 Panel B adds more color to this result by separately reporting the effects for regional (operating in < 4 states) and national (operating in ≥ 4 states) large acquirers.

Table 12 Panels A and C suggest that giving the acquirer financial control or partnering with a large hospital firm leads to qualitatively similar effects. It is worth noting that, empirically, these two attributes have substantial overlap. The acquirer is a large hospital firm *with* financial control in 115 deals, about 80% of all deals with financial control, and 70% of all deals involving large firms. In Figure A.12, we explore heterogeneity more granularly by estimating the effects separately for each of the four possible permutations of deal types, relative to the counterfactual of no privatization. For brevity, we report only four key outcomes. These estimates clarify that personnel reductions and cost savings are generated only when both attributes are present. In contrast, all four scenarios lead to a decrease in Medicaid admissions and an increase in mortality, although the coefficients are not always precisely estimated. Taken together, these results suggest that only a specific subset of privatization deals deliver cost savings, while access for low-income patients and quality of care decline even when governments transfer only managerial control.

7.3 Rural counties

Given the policy interest around diminishing access to hospital care in rural markets (GAO 2020), we examine heterogeneity by rural versus non-rural counties. Privatizations are nearly equally likely for government hospitals located in urban and rural counties. Table A.15 Panel

C presents the corresponding triple difference results. Privatization leads to larger decreases in patient volume in rural counties, particularly of Medicaid patients (20%). In addition, the effect on patient mortality is three times larger in rural counties (0.73 vs. 0.20 percentage points). Overall, rural counties disproportionately suffer the access and quality harms from privatization.

8 Discussion and Conclusion

Amid renewed debates over government efficiency, privatization is a promising solution but it can harm some stakeholders. This trade-off assumes greater significance in the case of government hospitals, where access to care, and in some cases, lives, are at stake. Despite extensive privatization of local public hospitals over the past two decades, this phenomenon has received little scrutiny. This paper provides the first comprehensive examination, to our knowledge, of all privatizations of nonfederal government hospitals in the U.S. over 2001–2018.

Privatization improves hospital profitability, shifting hospitals from losses to generating a modest surplus on average. Private management increases per-unit revenue and decreases aggregate costs substantially and therefore generates savings for taxpayers by eliminating the need for ongoing subsidy. However, improved profitability partly reflects reduced access to hospital care for low-income Medicaid and uninsured patients, higher prices, or lower staff availability. Hospital- and market-level results show that privatization leads to higher mortality among elderly Medicare FFS patients treated at the privatized facility.

To help summarize the tradeoff from the perspective of a local government contemplating privatization, we calculate the ratio of dollar savings for government per year from privatizing per additional death or life-year lost (LYL). This ratio summarizes key elements of the tradeoff in hospital privatization and could be a useful summary statistic to compare to the effects of other policies regarding government hospitals.

Privatized hospitals in our sample had an average deficit of $2.5 million in the year before privatization, but experience a decrease of $5.7 million in operating cost. This is sufficient to eliminate the deficit at most privatized hospitals and is the key financial benefit to the local government. In the approximately 15% of cases where a government hospital is purchased by a for-profit acquirer, there is also the prospect of additional property tax revenue for the local government. However, it is small relative to the deficit so we ignore it for simplicity.[26] As described in Section 5.3, we estimate 3.9 additional deaths and 20.9 LYL for FFS patients 65 years and older per privatization. We use a simulation approach to estimate the distribution of the ratio of savings to deaths and LYL, which we describe in Appendix B.9. Privatization yields an average of $0.8 million per year in saving to government per death, with a 95% confidence interval of ($0.36, $2.45). The corresponding estimate of average savings per LYL is approximately $150,000 ($70,000, $460,000).[27]

26. Rosenbaum et al. (2015) calculate that local property taxes contribute one-sixth of the total tax revenue from hospitals, or about 0.75% of hospital revenue (Exhibits 1 and B3). This would imply an additional $\approx \$65,000$ in incremental property taxes (0.75% x 15% x 57.7 mn) on average against a $2.5 million deficit.

27. The average values of the ratios are slightly skewed due to outlier cases. Median values of the distributions are,

This is not a comprehensive estimate of the welfare effect of privatization since it narrowly focuses on savings and deaths. It represents only the saving to the local government relative to the counterfactual of continuing to operate the hospital. Furthermore, some of the harmful effects for consumers that arise due to the savings, such as reduced access to care and higher prices, are not incorporated in the denominator. Here, it is worth noting that the balance of evidence in prior literature indicates that patient health outcomes (including for Medicaid patients) marginally improve after obstetric unit closures, likely due to reallocation toward better quality providers (Fischer, Royer, and White 2024; Dranove, Gaynor, and Geddes 2025). Hence, the welfare costs of reduced access may be mitigated by improved quality care for reallocated patients.

An open question is whether privatization is less harmful to patient health than closure. This is difficult to answer since, as we described previously, closures of government hospitals are rare and seem limited to small and near-defunct facilities. An analogous market-level analysis of closures, the details of which are described in Appendix B.10, suggests little effect on patient volume, regardless of payer (see Table A.16 and event study plots in Figure A.13). Similarly, we find no increase in death rates of the local population after a closure (see Table C.2). If anything, the estimates suggest that mortality decreases after a closure, perhaps because the facilities selected for closure were of low quality or had fewer resources. This would be consistent with broader evidence of no long-run changes or improvements in health on average following closures of hospitals and nursing homes (Petek 2022; Chandra, Dalton, and Staiger 2023; Olenski 2023).

Previous studies have shown that rural hospital closures are harmful, however. For example, Carroll (2023) finds that closures of critical access hospitals (CAH) lead to a 4% decrease in admissions and 8% higher mortality among Medicare patients with certain conditions. As discussed in Section 7, privatizations in rural counties are also more harmful than those of urban facilities. We detect a 14% decrease in total volume and a 6% increase in mortality of Medicare patients in such cases. Overall, this brief review suggests that closing a public hospital may not be more harmful than privatizing it, although an exhaustive analysis of closures merits a separate study.

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. 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. These inputs are needed for a comprehensive welfare analysis of hospital 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 social services that involve similar trade-offs between improving efficiency and serving unprofitable customers.

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respectively, \$0.65 million per death and \$120,000 per LYL.

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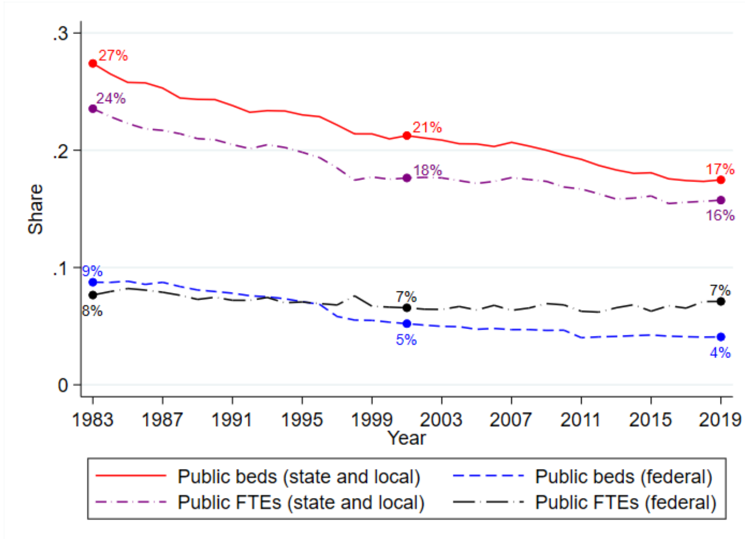
(a) As provider



(b) As payer

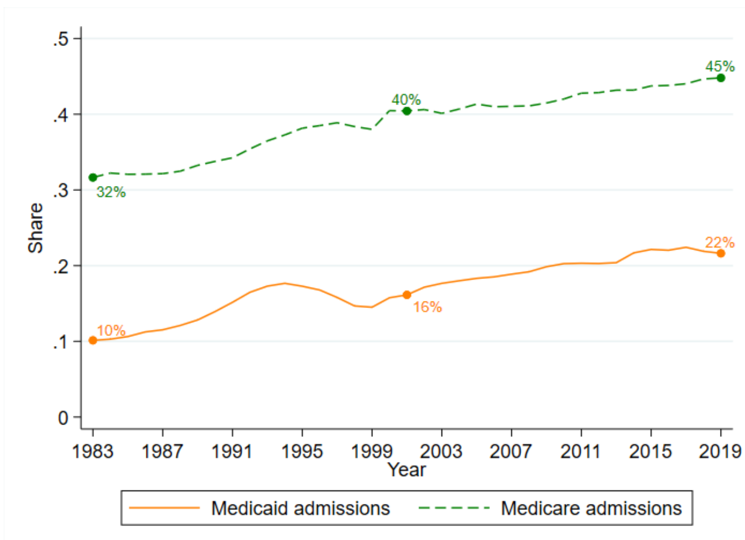
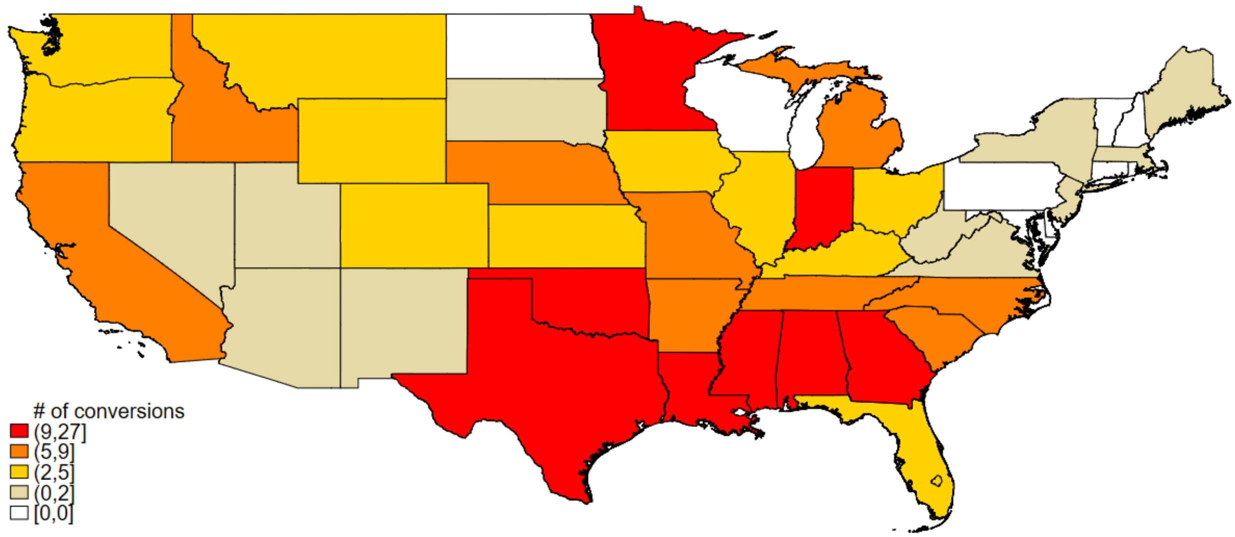


Figure 1: Government role in hospital care

Note: The figure presents overall shares in the 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

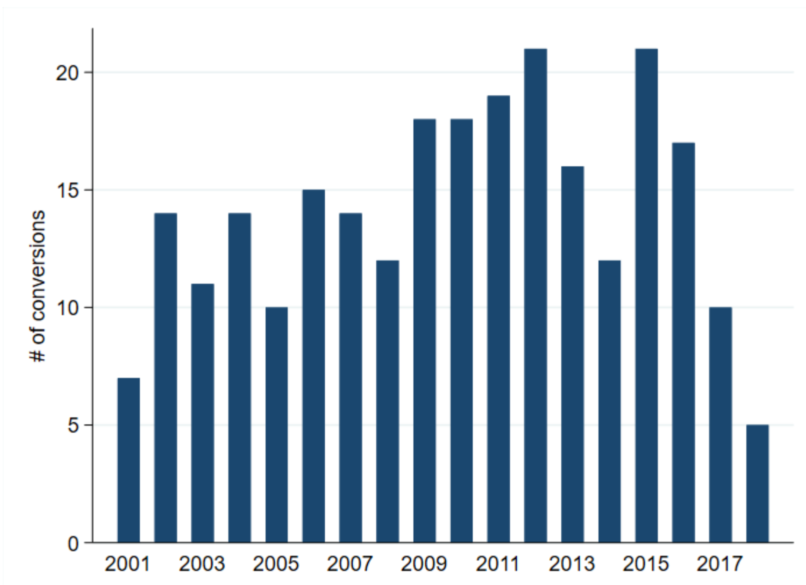


Figure 2: Privatizations

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 privatizations, respectively.

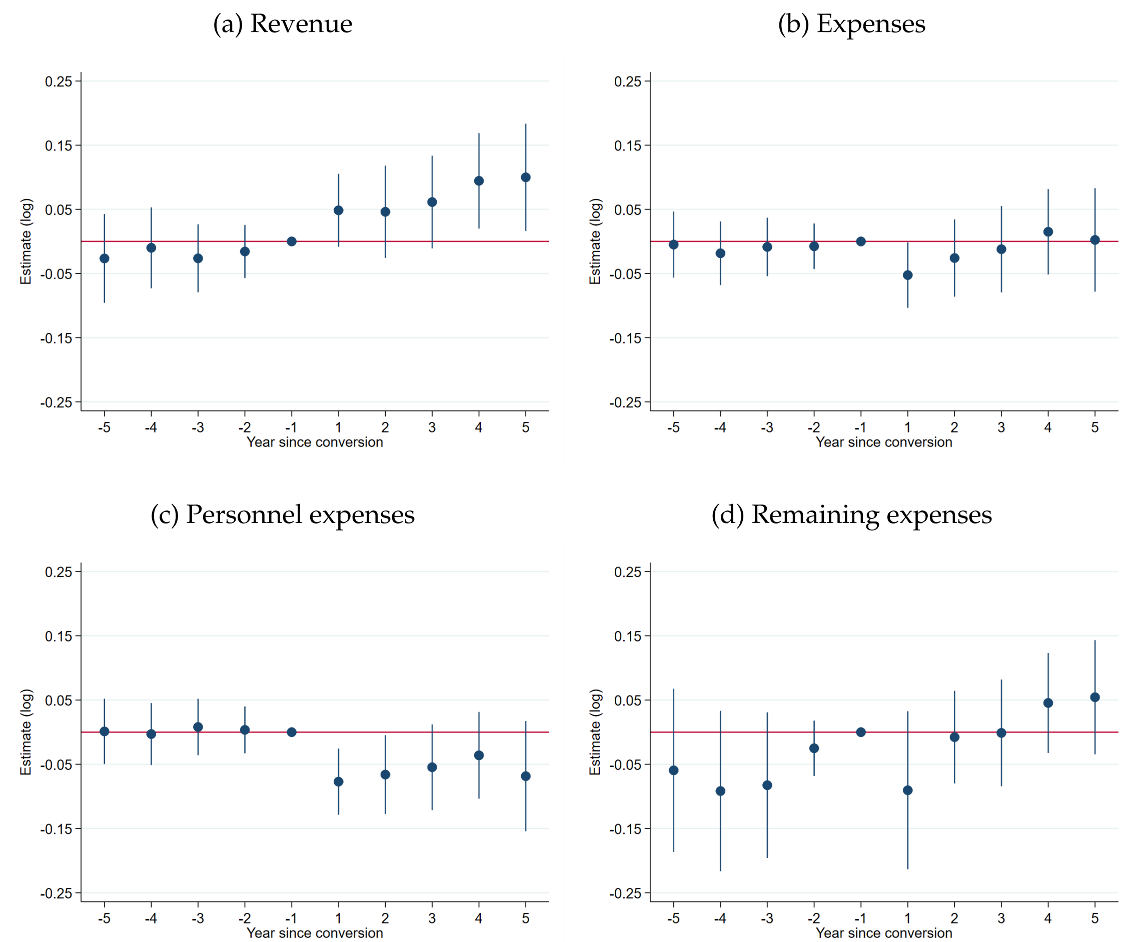
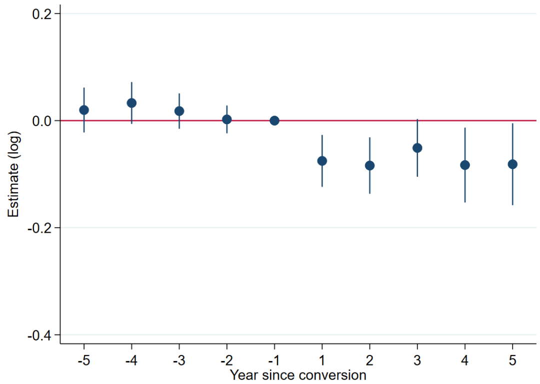


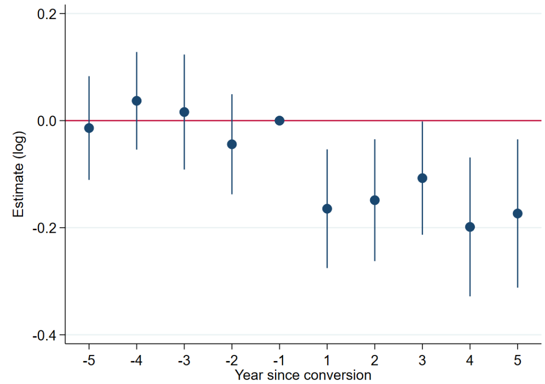
Figure 3: (log) Finances per bed

<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 and non-personnel ("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.

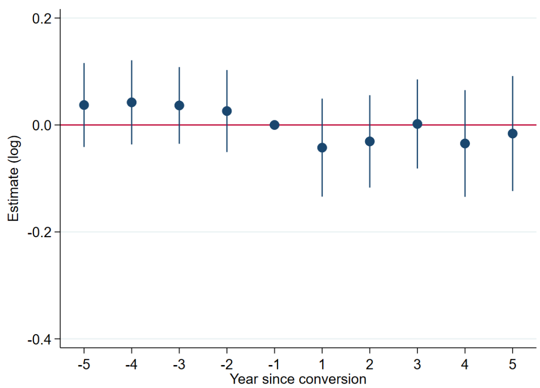
(a) Total inpatient



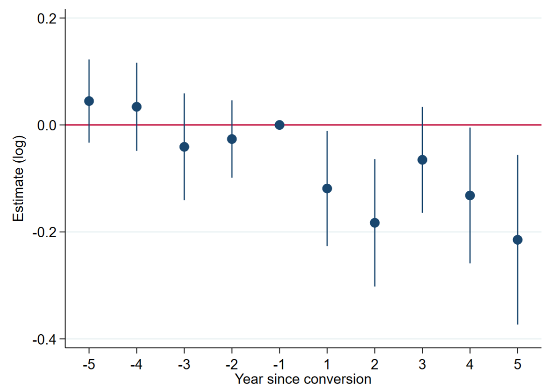
(b) Medicaid



(c) Medicare



(d) Other



(e) Adjusted admissions

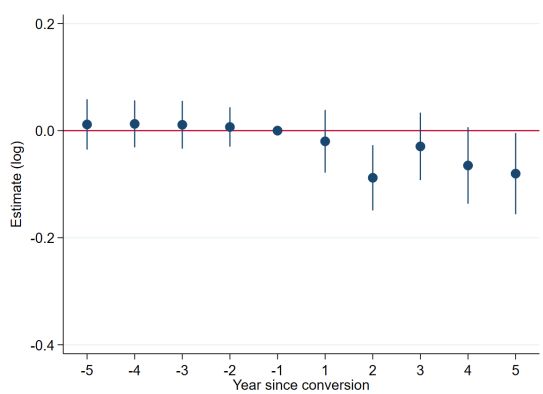


Figure 4: Patient (log) volume

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 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.

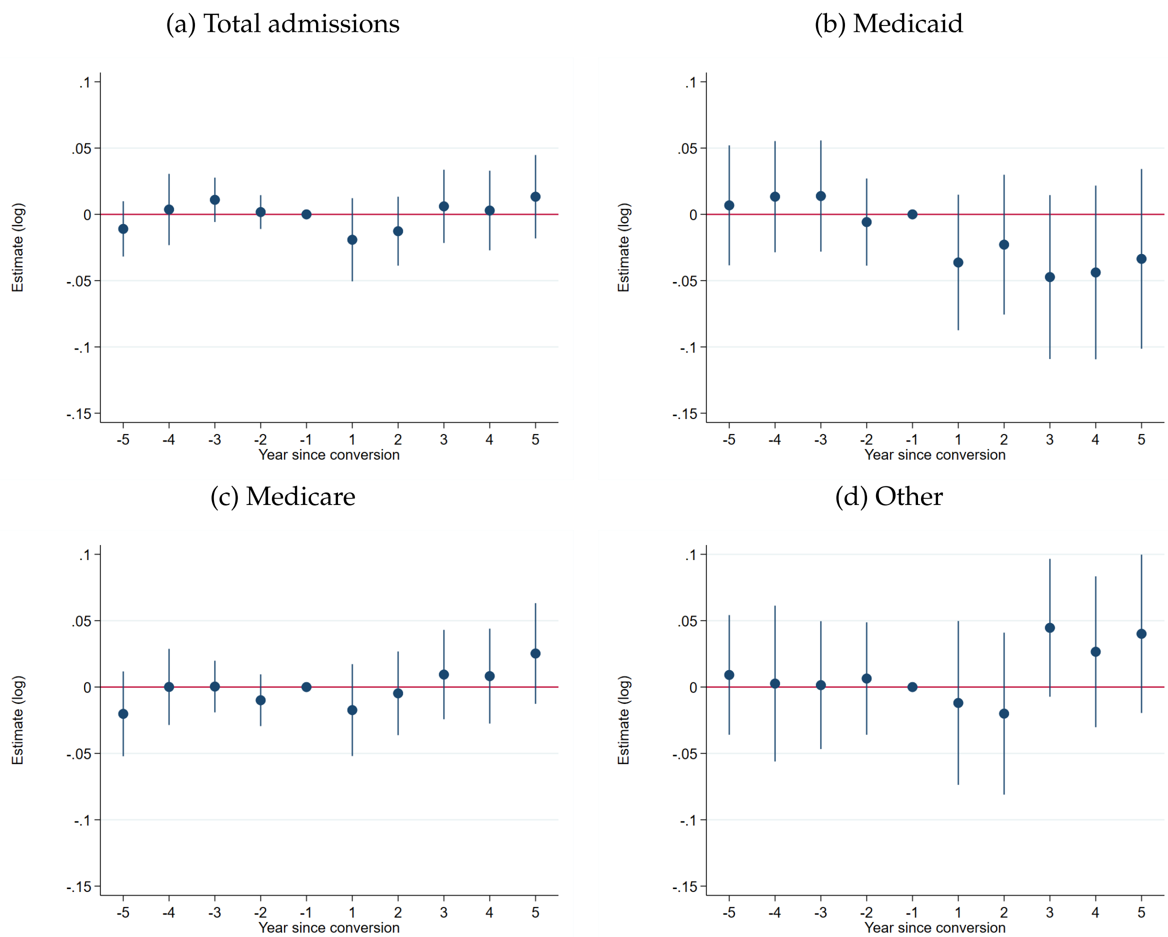
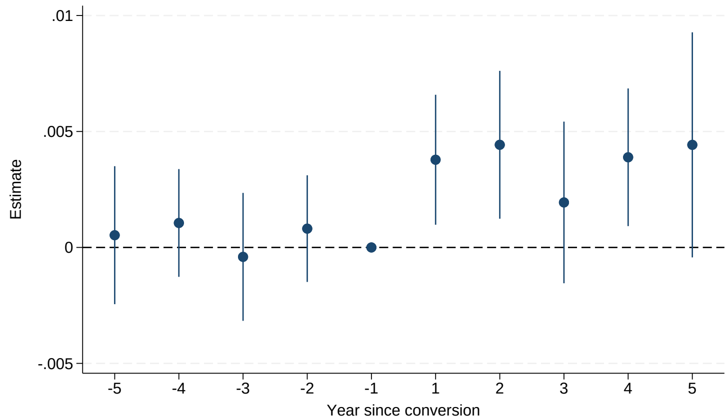


Figure 5: Market-level (log) volume

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

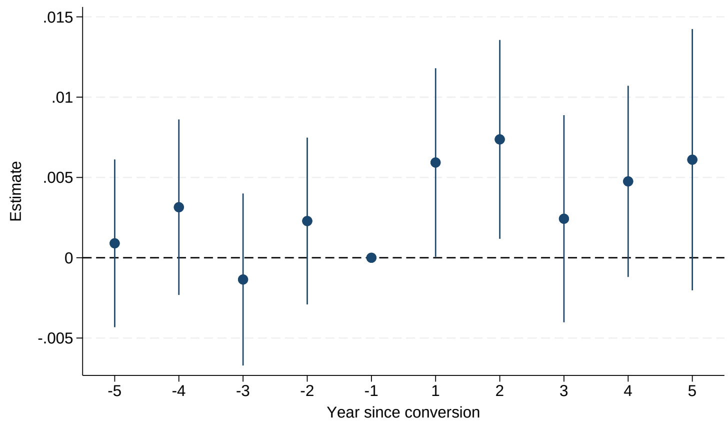
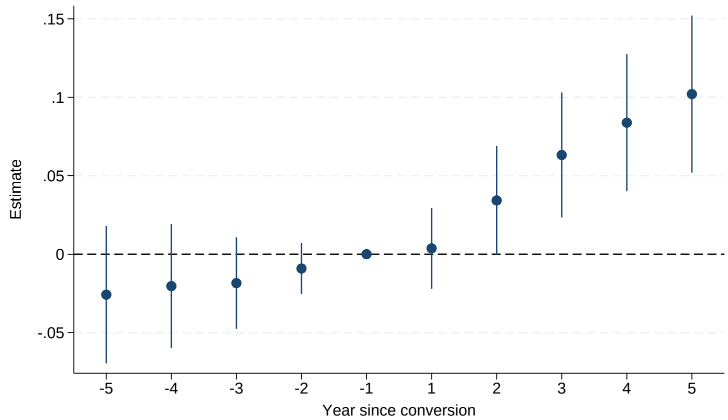


Figure 6: Hospital mortality rates

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. This sample excludes 50 hospitals that privatized prior to 2005 (so that we observe at least five pretreatment years) or were missing. 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 patient demographics and a mortality risk index, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital and bootstrapped to account for the first step construction of the risk index. 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

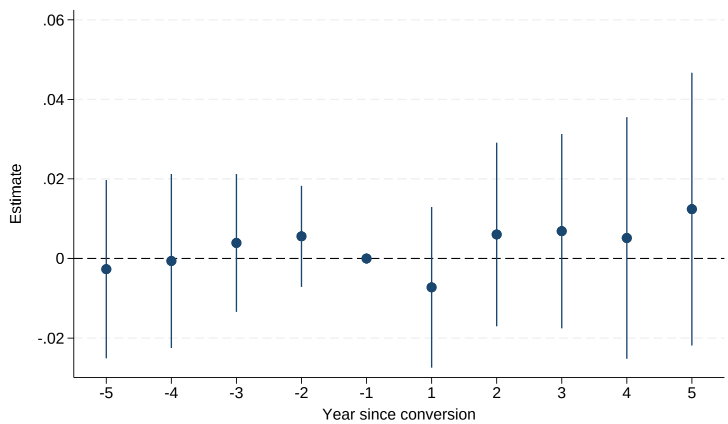
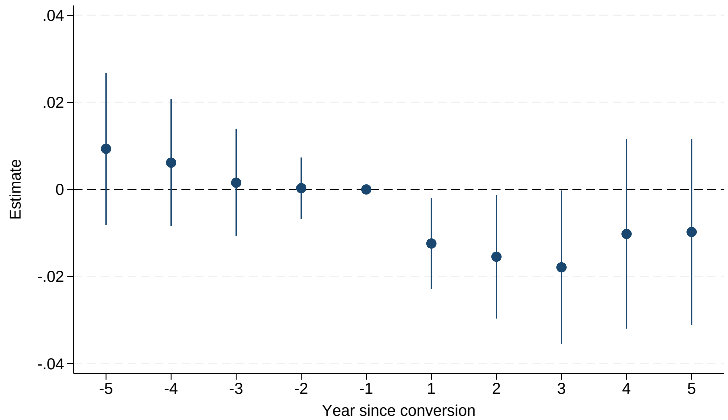


Figure 7: Billing practices

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. This sample excludes 50 hospitals that privatized prior to 2005 (so that we observe at least five pretreatment years) or were missing. 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 patient demographics and a mortality risk index, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital and bootstrapped to account for the first step construction of the risk index. 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)

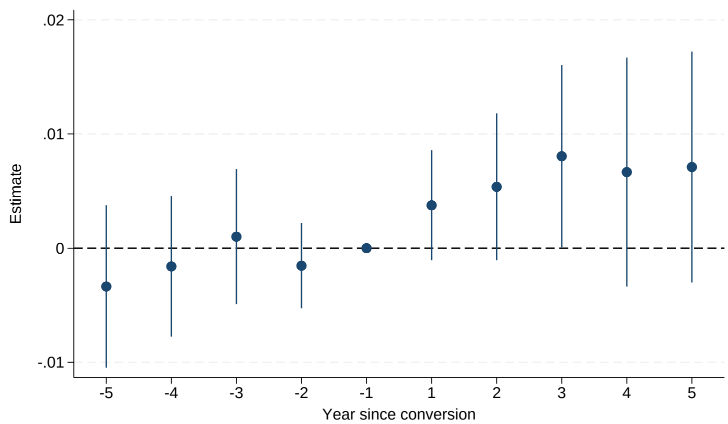


Figure 8: Patient length of stay

Note: The figure presents event study plots obtained by estimating Equation 2 on patient-level Medicare fee-for-service (FFS) claims data. This sample excludes 50 hospitals that privatized prior to 2005 (so that we observe at least five pretreatment years) or were missing. 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 patient demographics and a mortality risk index, as described in Section 4. The error bars present 95% confidence intervals. Standard errors are clustered by hospital and bootstrapped to account for the first step construction of the risk index. 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.

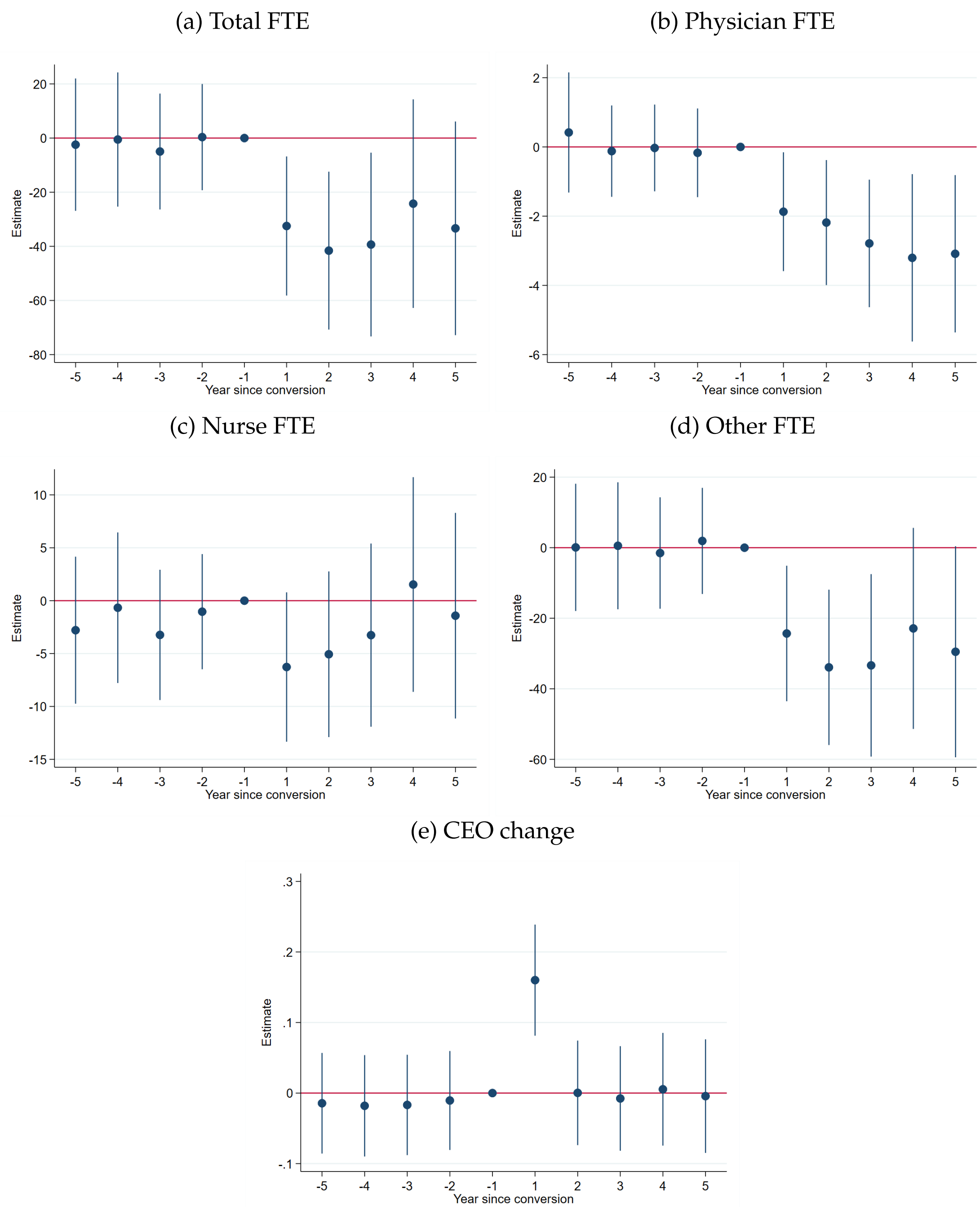


Figure 9: Staff availability and leadership churn

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, other (all remaining) FTEs, and the probability of a change in the hospital CEO in Panels (a), (b), (c), (d), and (e), respectively. FTE 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: Types of privatization arrangements

	(1) Non-profit	(2) For-profit	(3) Total
A: Deal type			
A1: Managerial control only			
Public hospital incorporating	38	3	41
Contract management	45	16	61
Joint venture or merger	5	1	6
A2: Managerial and Financial control			
Long-term lease	41	17	58
Sale	51	37	88
Total	180	74	254
B: Acquirer type			
Independent	47	14	61
Small (<5 hosp)	26	10	36
Regional	78	20	98
National	29	30	59
Total	180	74	254
C: Rural county			
Rural	94	40	134
Urban	86	34	120

Notes: This table describes the types of privatization arrangements 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 assigns deal types into those in which the acquirer has only managerial control (A1) or both managerial and financial control (A2). Public hospital incorporating refers to cases where a new private firm was incorporated–subject to oversight by the previous government owners–specifically to operate the hospital. In contract management, the private firm operates the hospital under a short-term contract, typically without taking on financial risk or making substantial capital investment. Joint ventures or mergers involve a private-public partnership with the government retaining some ownership and/or operational control. Long-term leases are typically of more than 10 years duration and the acquirer takes on financial risk and makes capital investments. Sales involve a complete transfer of operational control and all physical assets. Appendix B.1 describes these categories and provides examples. Panel B classifies deals according to the system status, size, and geographic coverage of the acquirer involved in the deal. Regional systems had 5 or more facilities but spanned ≤ 3 states at the time of the deal. National systems had 5 or more facilities and spanned 4 states or more at the time of the deal. Panel C classifies based on whether the hospital's county is rural or not per the USDA rural classification system. Appendix B.2 describes the rural flag in more detail.

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
% Nonprofit	0.0	0.0	79.0	62.1
% Rural	52.8	61.0	21.8	29.8
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	398	459	445
	(167)	(205)	(201)	(202)
# 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. % Rural refers to the percentage of hospitals located in counties classified as rural using the USDA's Rural–Urban Continuum Codes. "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: (log) Finances per bed

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
A: No controls	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	0.083	-0.009	-0.064	0.047
	(0.031)	(0.029)	(0.029)	(0.044)
Obs	16,589	16,589	16,589	16,589
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 begin 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 the same specification including time-varying market-level controls as described in Section 4. Some observations are missing covariates, resulting in a smaller sample in Panel B. 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 additional estimates related to hospital finances.

Table 4: Patient (log) volume

	(1)	(2)	(3)	(4)	(5)		
A: AHA	Total	Medicaid	Medicare	Other	Adjusted		
A1: No controls	-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	-0.098	-0.173	-0.066	-0.150	-0.072		
	(0.028)	(0.042)	(0.030)	(0.044)	(0.026)		
Obs	19,234	19,233	19,233	19,233	19,234		
Mean outcome (t-1)	3,038	622	1,361	1,054	7,087		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
B: States	Total	Medicaid	Medicare	Other	Private	Uninsured	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" comprises 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 the same specification including time-varying market-level covariates described in Section 4. Panel A2 has fewer observations since some market-level covariates are missing. 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: Market-level (log) volume

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: No controls				
DD	-0.004	-0.042	0.009	0.010
	(0.015)	(0.025)	(0.016)	(0.022)
Obs	19,288	19,288	19,288	19,288
B: Market controls				
DD	-0.017	-0.048	-0.006	-0.007
	(0.015)	(0.024)	(0.015)	(0.022)
Obs	18,449	18,449	18,449	18,449
C: Heterogeneity by market poverty				
DD	0.023	0.038	0.034	0.015
	(0.022)	(0.032)	(0.021)	(0.032)
x 1(> med. poverty)	-0.053	-0.158	-0.050	-0.010
	(0.027)	(0.045)	(0.030)	(0.043)
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 the same specification including time-varying, HSA-level controls as described in Section 4. Panel B has fewer observations since some covariates are missing. Panel C presents the corresponding results from a triple difference specification including 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: Hospital mortality rates

	(1) All	(2) Non-deferrable	(3) Age≤ 80	(4) Age> 80	(5) Medical	(6) Surgical
A: Patient controls						
DD	0.0033	0.0044	0.0017	0.0050	0.0035	0.0020
	(0.0013)	(0.0018)	(0.0013)	(0.0017)	(0.0015)	(0.0016)
B: Patient and market controls						
DD	0.0034	0.0046	0.0014	0.0056	0.0033	0.0029
	(0.0014)	(0.0021)	(0.0014)	(0.0018)	(0.0015)	(0.0020)
Mean outcome (t-1)	0.118	0.175	0.089	0.152	0.130	0.070
Observations	12,993,422	3,155,736	7,328,068	5,665,354	10,038,198	2,854,692

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 which includes patient demographics and a mortality risk index, 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 bootstrapped to account for the first step construction of the risk index.

Table 7: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Finances			Volume			Market-level	Hospital
	Revenue	Tot. Exp.	Pers. Exp.	Total	Medicaid	Other	Medicaid	Mortality
Baseline	0.083	-0.009	-0.063	-0.089	-0.156	-0.142	-0.042	0.0033
	(0.032)	(0.029)	(0.029)	(0.028)	(0.043)	(0.044)	(0.025)	(0.0013)
I: Specification checks								
A. Weighting by beds	0.104	-0.014	-0.056	-0.089	-0.164	-0.107	-0.010	-
	(0.032)	(0.042)	(0.032)	(0.029)	(0.045)	(0.049)	(0.017)	-
B. State-year FEs	0.128	0.017	-0.035	-0.104	-0.159	-0.178	-0.036	0.0031
	(0.031)	(0.028)	(0.028)	(0.030)	(0.044)	(0.046)	(0.025)	(0.0016)
C. Incl. pre-trend	0.065	-0.020	-0.078	-0.103	-0.167	-0.150	-0.078	0.0037
	(0.034)	(0.031)	(0.031)	(0.029)	(0.046)	(0.047)	(0.025)	(0.0012)
II: Alternate estimators								
A. CS estimator	0.059	-0.022	-0.063	-0.075	-0.152	-0.144	-0.039	0.0034
	(0.028)	(0.026)	(0.027)	(0.023)	(0.047)	(0.045)	(0.023)	(0.0013)
B. DCDH estimator	0.039	-0.011	-0.054	-0.061	-0.127	-0.115	-0.039	0.0032
	(0.025)	(0.024)	(0.025)	(0.023)	(0.042)	(0.047)	(0.021)	(0.0013)
III: Alternate samples: Treated group								
A. Balanced panel	0.121	0.016	-0.036	-0.064	-0.155	-0.098	-0.042	0.0030
	(0.034)	(0.032)	(0.032)	(0.031)	(0.047)	(0.049)	(0.026)	(0.0015)
B. All treated obs	0.033	-0.045	-0.104	-0.082	-0.148	-0.151	-0.037	0.0015
	(0.033)	(0.031)	(0.031)	(0.032)	(0.048)	(0.044)	(0.031)	(0.0014)
IV: Alternate samples: Comparison group								
A. Matched sample	0.084	-0.015	-0.056	-0.060	-0.149	-0.102	-0.058	0.0015
	(0.035)	(0.031)	(0.031)	(0.029)	(0.049)	(0.051)	(0.027)	(0.0019)
B. Switchers included	0.089	-0.006	-0.059	-0.086	-0.154	-0.139	-	0.0033
	(0.032)	(0.029)	(0.029)	(0.028)	(0.042)	(0.044)	-	(0.0011)

Notes: The table shows the results of robustness checks for the results using the AHA sample presented in Tables 3, 4A, and 6, respectively. For brevity, we do not present results for outcomes where we do not detect effects. Standard errors in column 8 are obtained by bootstrapping to account for the first step probit. IA uses static hospital beds to weight hospitals. IB includes state×year fixed effects and time-varying market controls. 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. IIA presents the Callaway and Sant'Anna (2020) and IIB the De Chaisemartin and d'Haultfoeuille (2020) estimators. Both models are estimated on a sample that includes data from the year of privatization. IIIA keeps only treated hospitals that we observe for five years before and after the transition. IIIB uses all treated observations, including those from the year of privatization and those beyond the five-year window around privatization. IVA presents results estimated on a matched subsample. IVB includes additional comparison hospitals that switch between public and private. See Section 5.4 for additional details.

Table 8: Access to 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: Billing practices

	(1) Log(charges)	(2) Log(payment)
A: Patient controls		
DD	0.0659	0.0024
	(0.0220)	(0.0142)
B: Patient and market controls		
DD	0.0685	-0.0200
	(0.0254)	(0.0155)
Mean outcome (t-1)	30,418	8,770
Observations	12,992,843	12,858,718

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. This sample excludes 50 hospitals that privatized prior to 2005 (so that we observe at least five pretreatment years) or were missing. 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 demographics and a mortality risk index, 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 bootstrapped to account for the first step construction of the risk index.

Table 10: Patient length of stay

	(1) Log(length of stay)	(2) Pr(stay< 2 days)
A: Patient controls		
DD	-0.0171	0.0071
	(0.0068)	(0.0027)
B: Patient and market controls		
DD	-0.0207	0.0095
	(0.0079)	(0.0033)
Mean outcome (t-1)	5.68	0.13
Observations	12,993,422	12,993,422

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. This sample excludes 50 hospitals that privatized prior to 2005 (so that we observe at least five pretreatment years) or were missing. 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 demographics and a mortality risk index, 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 bootstrapped to account for the first construction of the risk index.

Table 11: Staff availability and leadership churn

	(1) Total	(2) Physician	(3) Nurse	(4) Other	(5) Contract	(6) CEO change
A: No controls						
DD	-33.0	-2.6	-1.7	-29.1	0.1	0.048
	(12.9)	(0.8)	(3.3)	(9.7)	(1.4)	(0.018)
Obs	20,387	20,387	20,387	20,387	8,693	20,387
B: Market controls						
DD	-38.4	-2.9	-3.0	-32.8	-0.2	0.048
	(13.2)	(0.8)	(3.3)	(9.8)	(1.4)	(0.018)
Obs	19,234	19,234	19,234	19,234	8,536	19,234
Mean outcome (t-1)	513.9	10.3	139.0	364.1	13.6	0.24

Notes: The table presents effects on hospital leadership and staff availability at the privatized hospitals, obtained by estimating Equation 1 on hospital-year level data. Column 1 presents the effect on 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 the effect on contract FTEs, which come from Medicare cost reports and include management and patient care staff. Column 6 presents the effect on the probability of change in the hospital CEO. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from the same specification including time-varying market-level controls as described in Section 4. Panel B has fewer observations since some covariates are missing. 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 for outcomes in columns 1–5 where staff FTE counts are scaled by 100 adjusted admissions instead.

Table 12: Heterogeneity in treatment effects

	(1) Treated Hospitals	(2) Finances Revenue	(3) Tot. Exp.	(4) Volume Total	(5) Medicaid	(6) Staff FTE/bed	(7) Mortality 30-day
Baseline							
DD	254	0.083	-0.006	-0.089	-0.156	-33.041	0.0033
		(0.032)	(0.026)	(0.028)	(0.043)	(12.871)	(0.0013)
A: More private control							
DD	108	0.151	0.062	-0.106	-0.179	6.030	0.0021
		(0.044)	(0.038)	(0.045)	(0.059)	(16.499)	(0.0024)
x 1(Sale/Lease)	146	-0.115	-0.119	0.030	0.040	-68.386	0.0016
		(0.061)	(0.050)	(0.056)	(0.082)	(23.967)	(0.0027)
B: For-profit partner							
DD	180	0.080	0.002	-0.138	-0.186	-32.954	0.0030
		(0.037)	(0.031)	(0.032)	(0.052)	(15.804)	(0.0016)
x 1(For-profit)	74	0.009	-0.027	0.164	0.101	-0.288	0.0006
		(0.070)	(0.056)	(0.058)	(0.084)	(25.312)	(0.0027)
C: Large system partner							
DD	97	0.150	0.078	-0.090	-0.184	-5.273	0.0024
		(0.054)	(0.042)	(0.045)	(0.065)	(15.984)	(0.0028)
x 1(System >= 5)	157	-0.106	-0.137	0.001	0.046	-45.116	0.0011
		(0.065)	(0.052)	(0.056)	(0.083)	(23.483)	(0.0030)
Observations		16,829	20,387	20,387	20,386	20,387	12,993,422

Notes: This table presents results on heterogeneity in treatment effects of privatization on key outcomes of interest in hospital finances (revenue and total expenses), inpatient volume (total and Medicaid), staff total FTE/bed, and 30-day mortality rate. Column 1 presents the number of treated hospitals corresponding to different specifications of interest. All models exclude time-varying covariates. The top row presents the baseline D-D coefficient to facilitate comparison. Panel A presents results from a triple difference model studying the differential effect for deals involving a sale or long-term lease, which give managerial and financial control to the private partner. Panel B studies the differential effect on hospitals privatized to for-profit partners. Panel C studies the differential effect on hospitals privatized to hospital system partners owning 5 or more hospitals at the time of acquisition. Standard errors are in parentheses and are clustered by hospital. The standard errors in column 7 are bootstrapped to account for the first step construction of the risk index. Table A.15 presents results from additional heterogeneity tests.

A Additional figures and tables

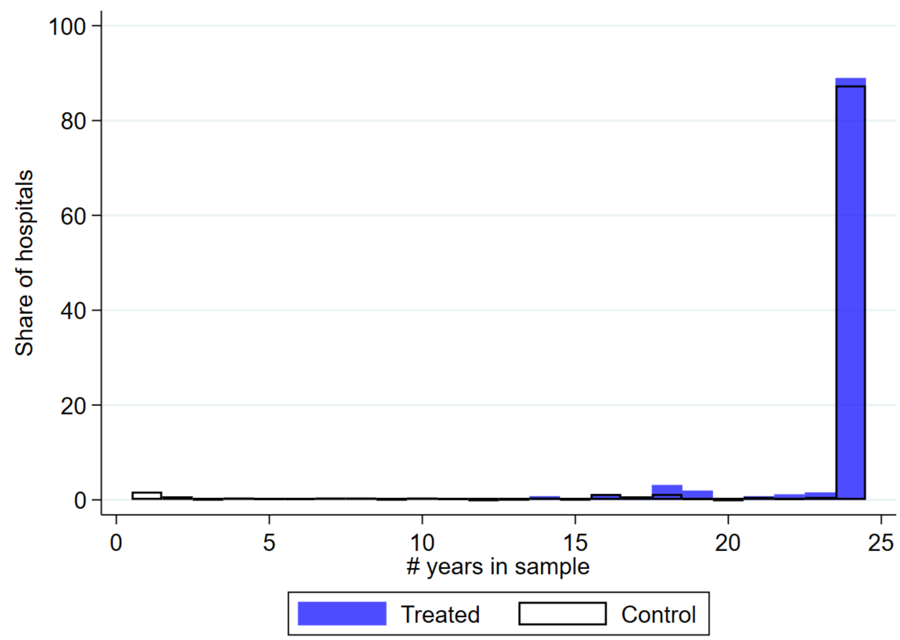


Figure A.1: Balance of hospital panel

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).

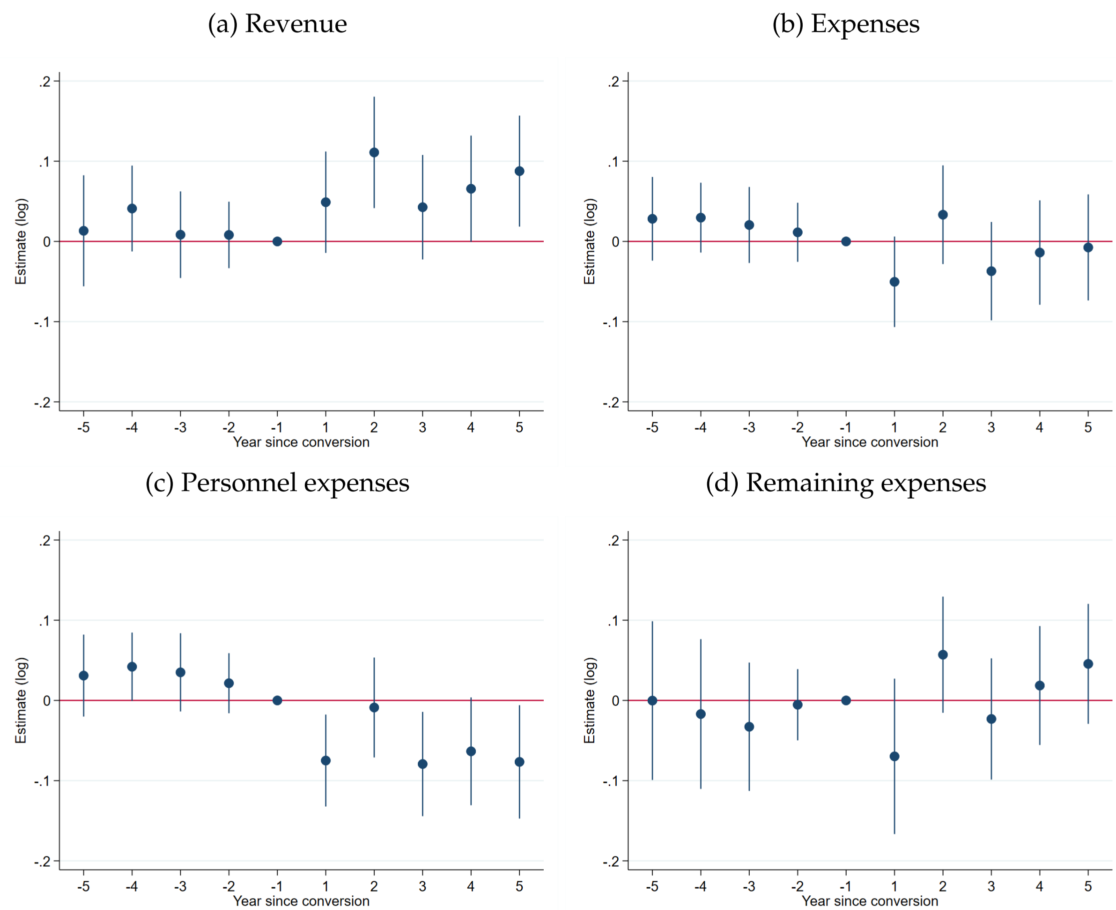


Figure A.2: (log) Finances per patient

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.

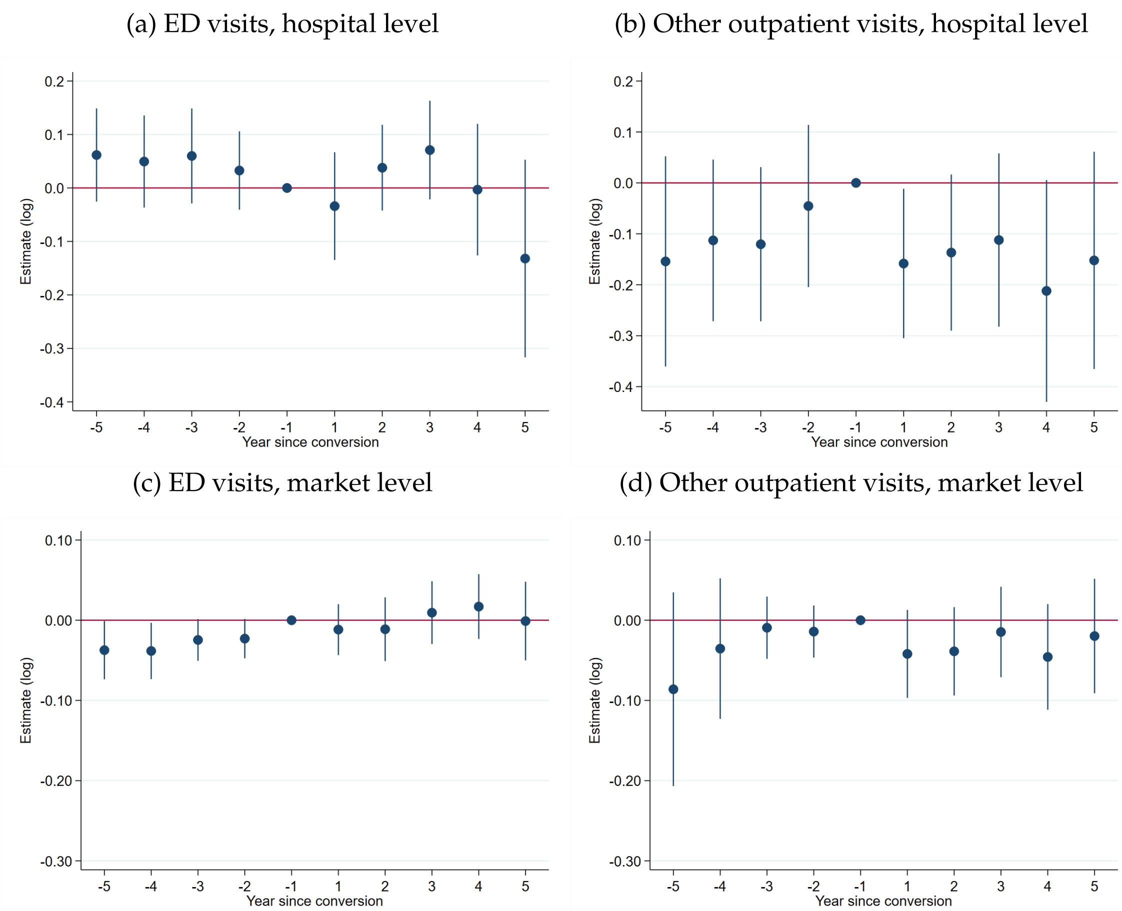


Figure A.3: ED and other outpatient visits

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).

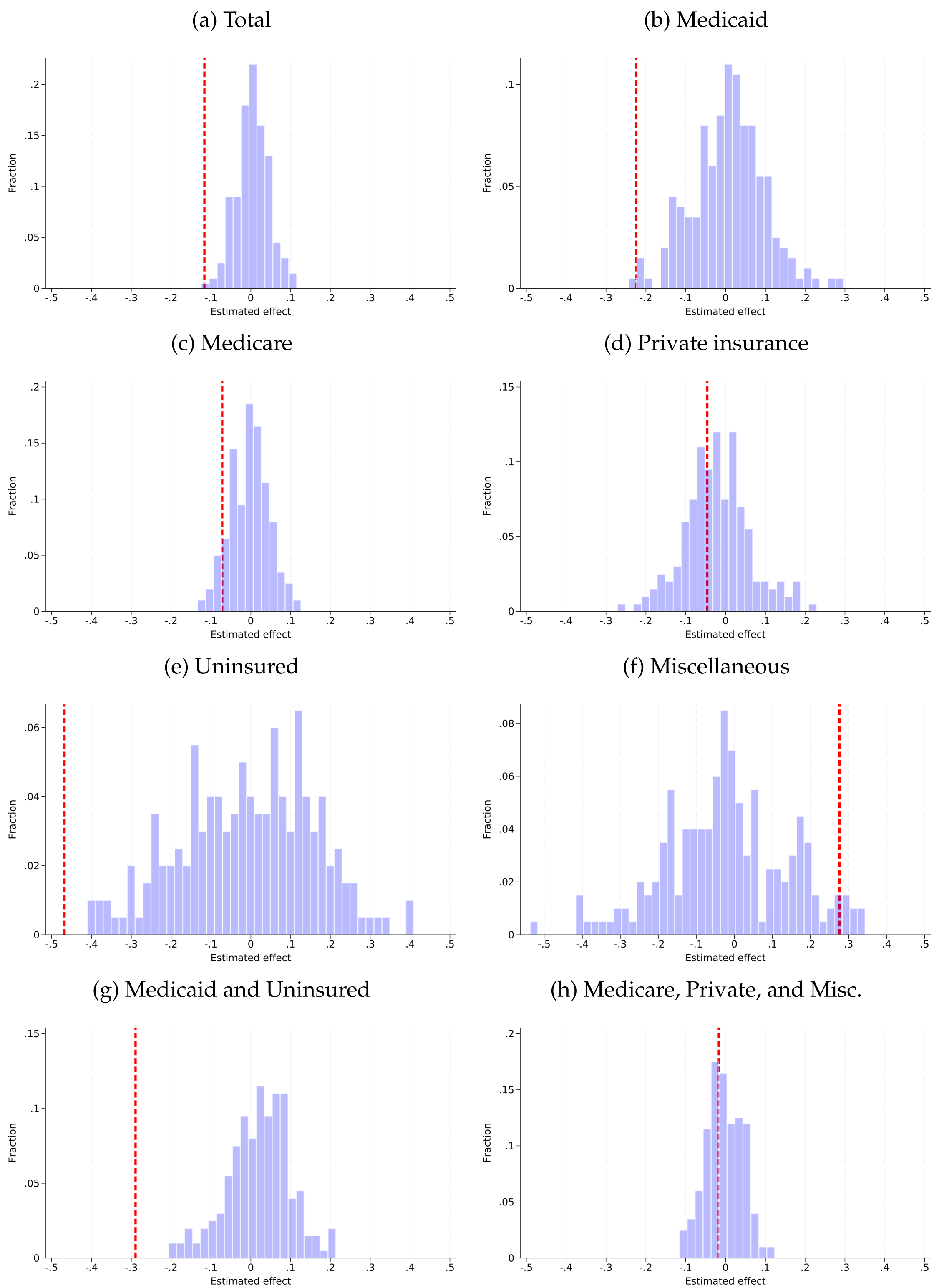


Figure A.4: Admissions by payer using state data

<u>Note:</u> 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 at least 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.

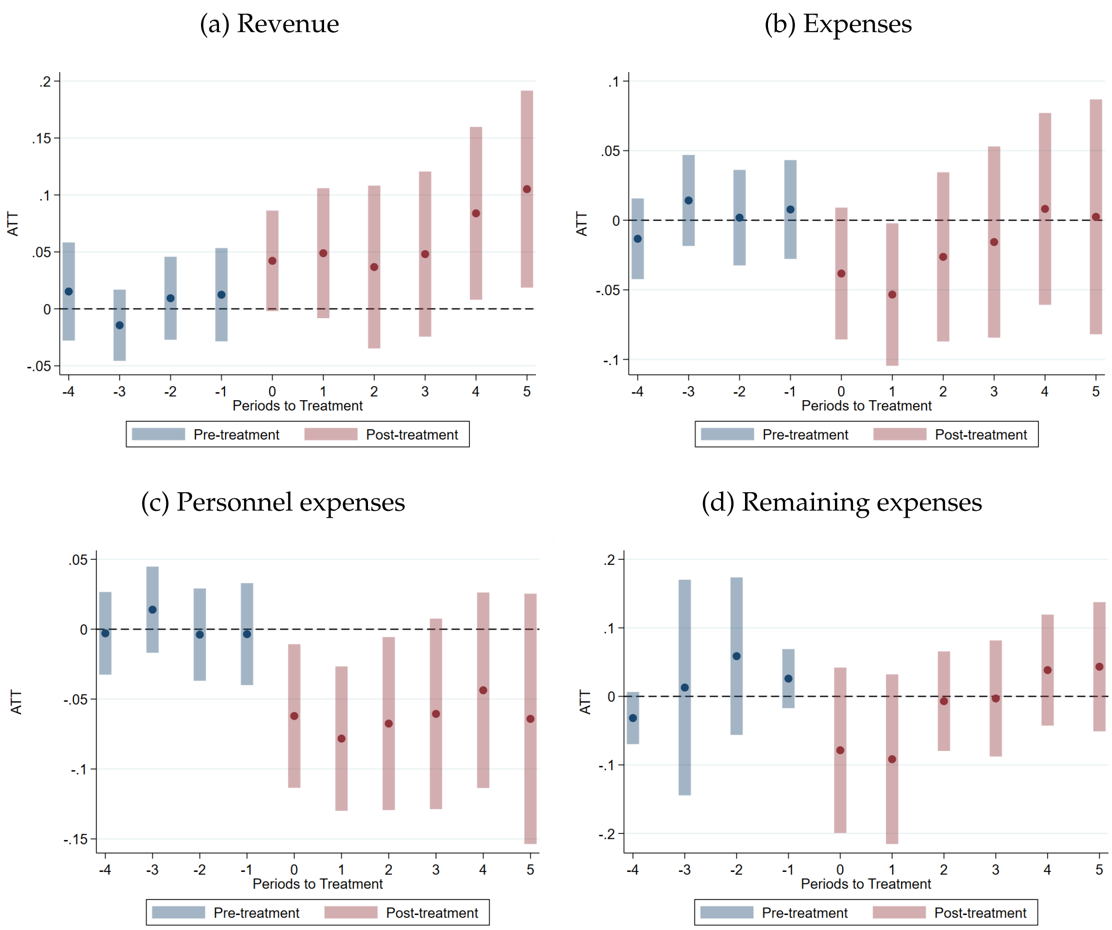


Figure A.5: (log) Finances per bed (Callaway-Sant'Anna)

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.

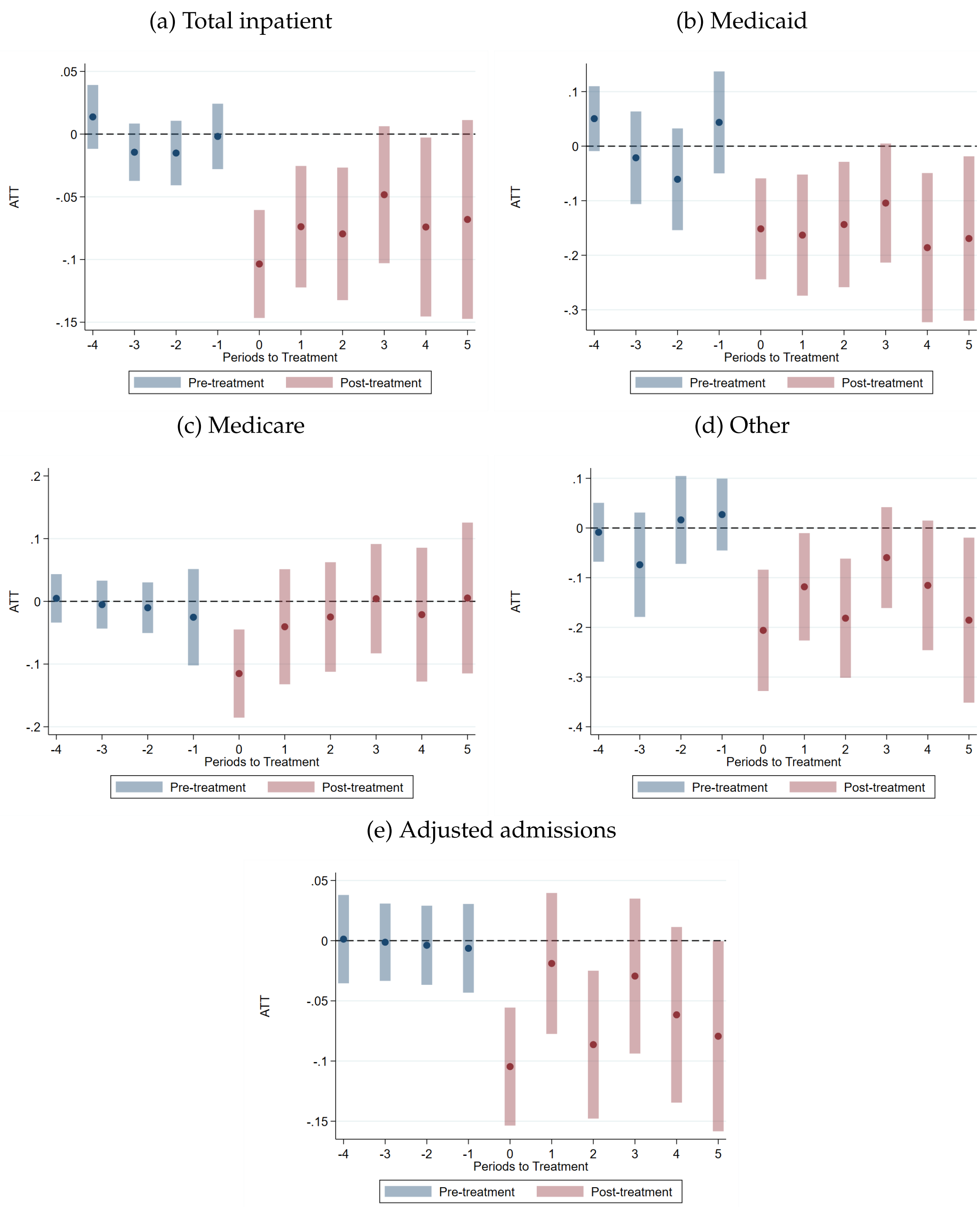


Figure A.6: Patient (log) volume (Callaway-Sant'Anna)

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

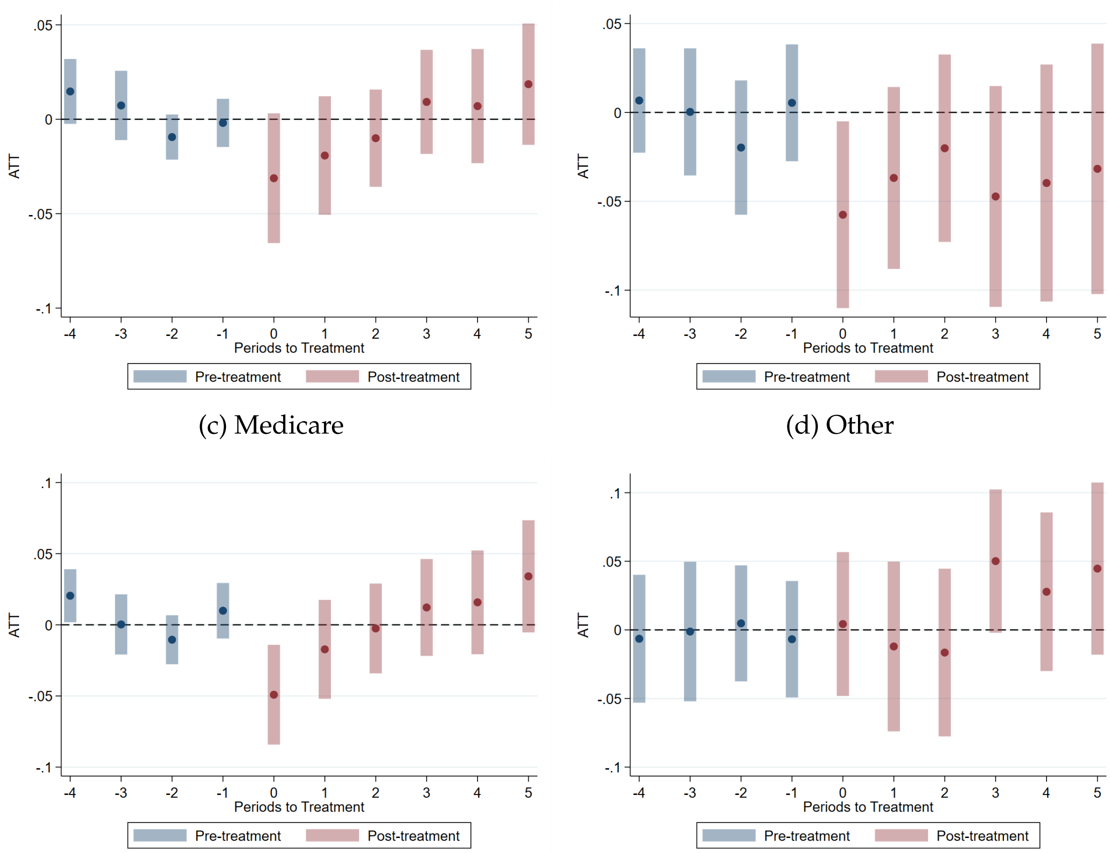
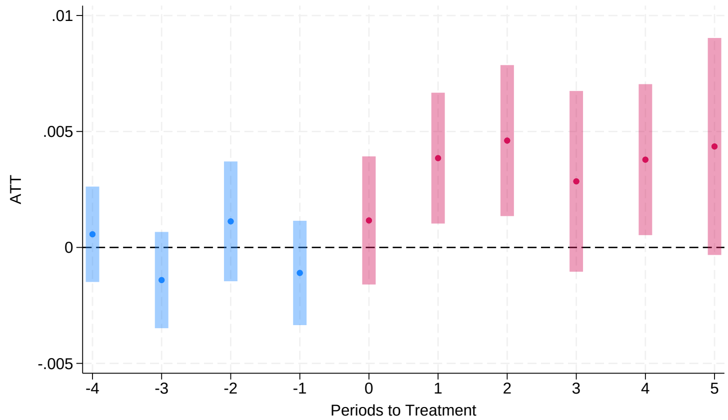


Figure A.7: Market-level admissions (Callaway Sant'Anna)

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

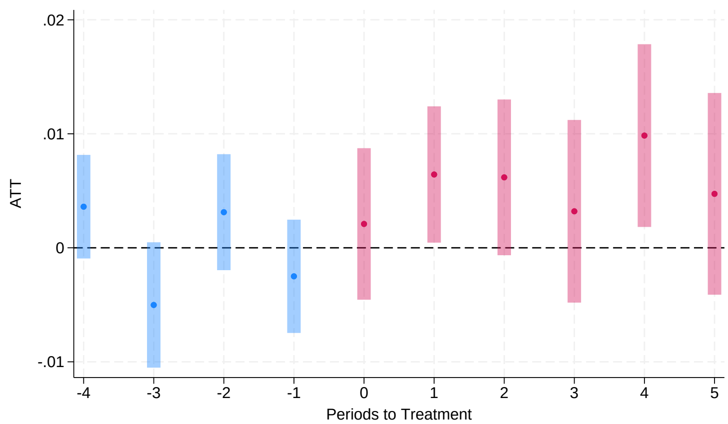
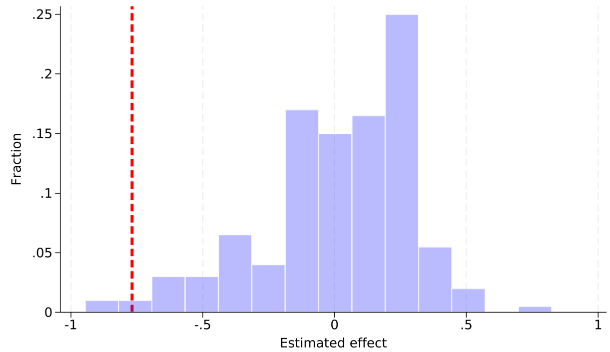


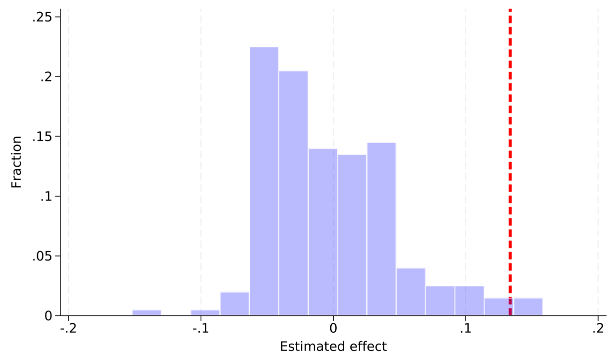
Figure A.8: Hospital mortality rates (Callaway Sant'Anna)

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 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

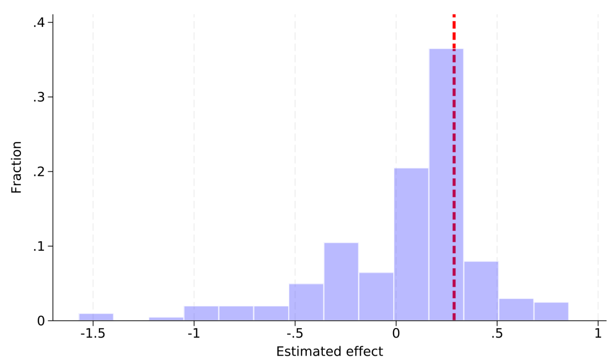


Figure A.9: Obstetric admissions

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.

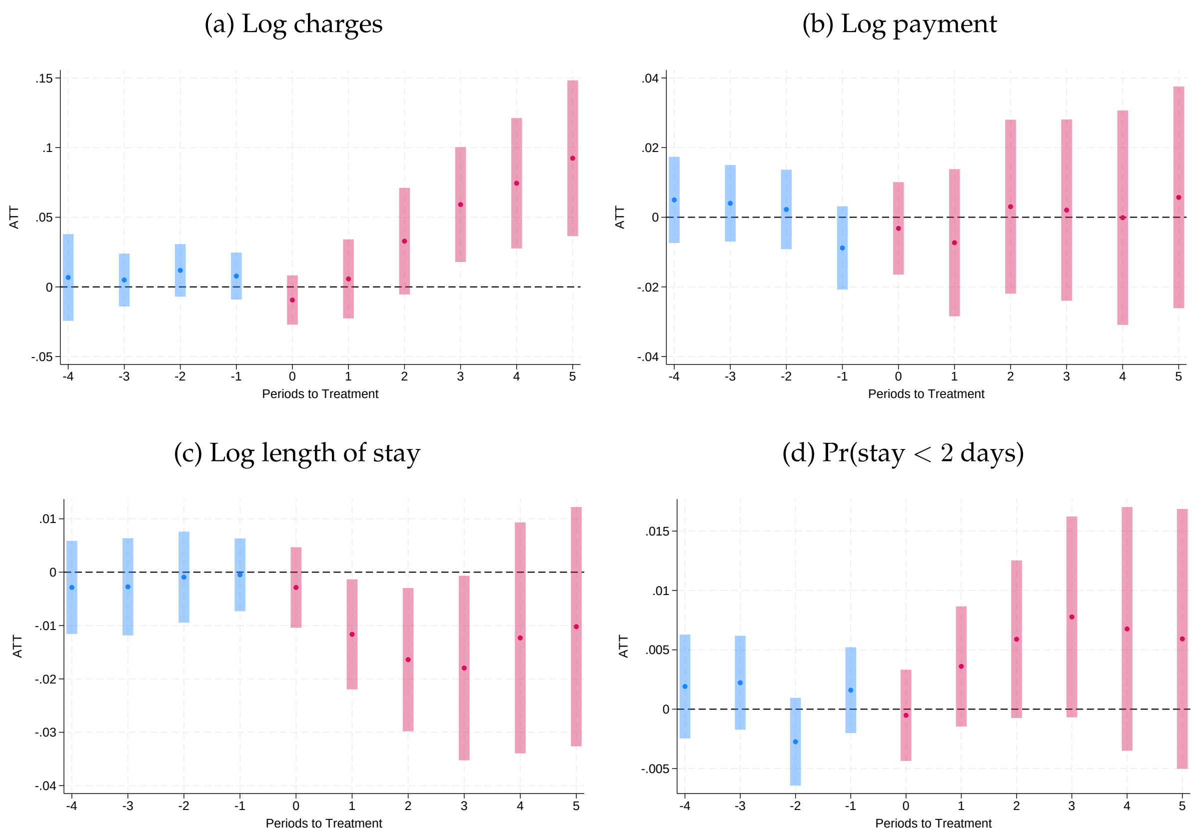
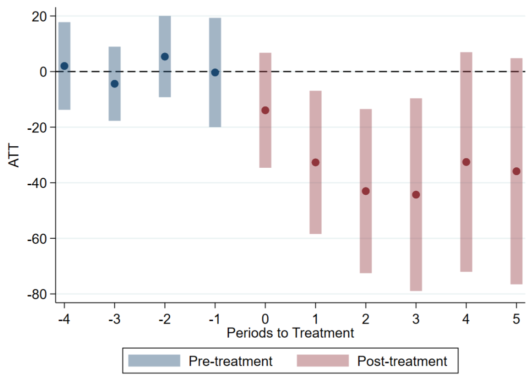


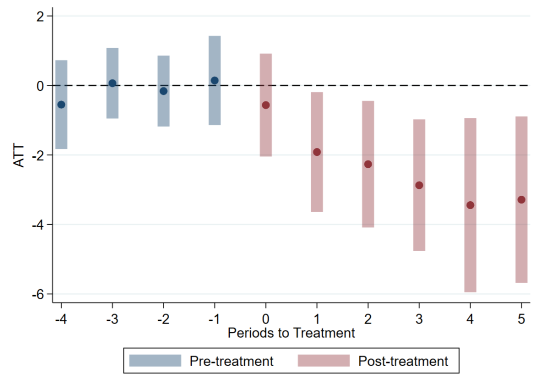
Figure A.10: Billing practices and patient length of stay (Callaway-Sant'Anna)

Note: 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.

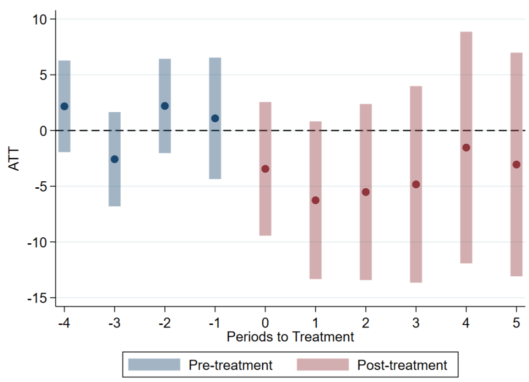
(a) Total FTE



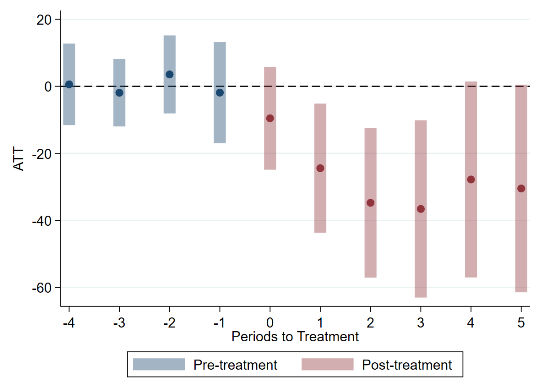
(b) Physician FTE



(c) Nurse FTE



(d) Other FTE



(e) CEO change

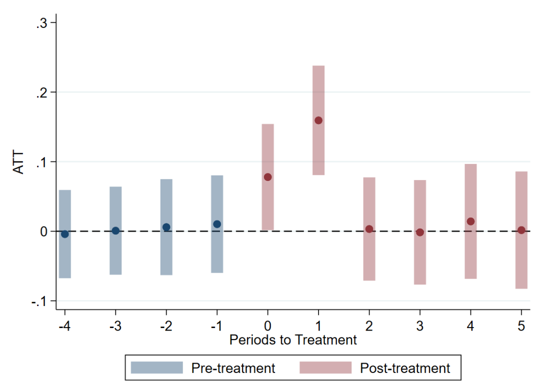


Figure A.11: Staff availability and leadership churn (Callaway-Sant'Anna)

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 and the probability of changing the hospital CEO, 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.

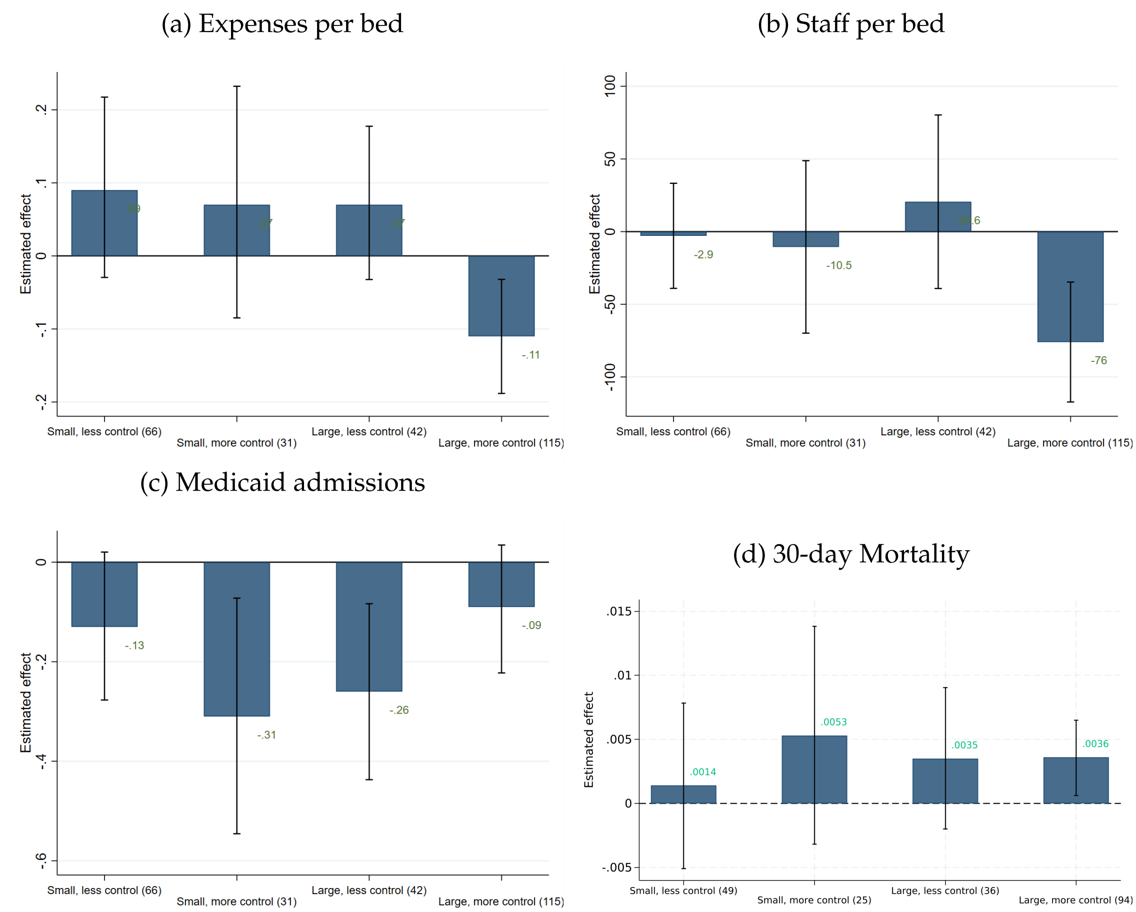


Figure A.12: Heterogeneous effects: Acquirer type and degree of control

Note: The figure presents bar plots of point estimates from a specification testing for heterogeneity in treatment effects across four types of deals relative to the comparison group: acquirer with fewer than 5 hospitals with managerial control, acquirer with fewer than 5 hospitals with managerial and financial control, acquirer with 5 or more hospitals but only managerial control, and finally acquirer with 5 or more hospitals with managerial and financial control. Standard errors are clustered by hospital. In Panel (d), the standard errors are obtained by bootstrapping over the first step construction of the risk index.

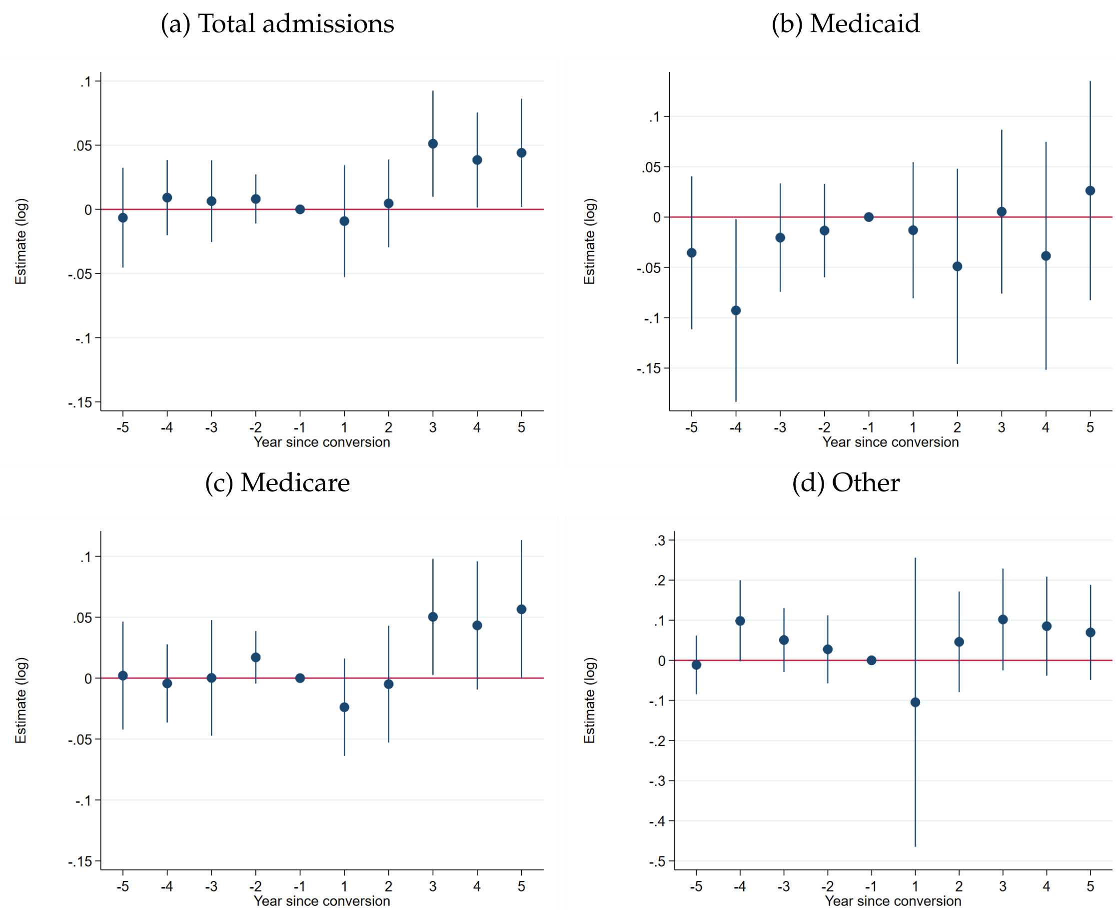


Figure A.13: Effects of hospital closures on patient volume

Note: The figure presents event study plots obtained by estimating Equation 2 on market-level data. The outcomes are log hospital inpatient admissions aggregated to the market level. Markets are defined using Health Service Areas, as described in Section 4. The 27 treated markets here each experienced one hospital closure between 2001 and 2018. The comparison group includes approximately 700 markets that did not experience a closure nor a privatization during the sample period. Year zero is the year of the closure and is omitted from the sample since it represents partial treatment. The error bars denote 95% confidence intervals. Standard errors are clustered by HSA.

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

State	(1) Public (nonfederal)	(2) Public (federal)	(3) Nonprofit	(4) For-profit	(5) # Hospitals	(6) Medicaid expansion
Wyoming	70.8	7.4	13.1	8.7	32	N
Alabama	44.4	4.4	23.4	27.8	116	N
Mississippi	40.7	5.8	31.9	21.6	112	N
Kansas	36.8	2.6	38.0	22.5	152	N
South Carolina	32.9	2.9	39.9	24.3	88	N
North Carolina	31.8	4.0	55.4	8.9	135	N
Iowa	29.8	3.0	65.0	2.2	123	Y
Washington	27.0	4.9	60.2	7.9	107	Y
Louisiana	26.1	1.4	46.2	26.3	200	Y
Idaho	25.2	2.3	47.4	25.0	52	N
New York	23.6	3.9	70.8	1.7	210	Y
Colorado	23.5	2.9	53.0	20.6	106	Y
California	22.9	3.6	56.8	16.8	419	Y
New Mexico	22.2	8.4	33.2	36.3	55	Y
Hawaii	22.1	4.2	73.7	0.0	28	Y
Virginia	20.1	6.0	55.3	18.6	123	Y
Oregon	19.8	3.3	72.9	4.0	65	Y
Oklahoma	19.4	4.0	49.5	27.1	146	N
Tennessee	19.0	3.8	47.8	29.3	132	N
Utah	18.6	1.9	47.3	32.1	59	N
Missouri	18.2	3.4	63.6	14.9	143	N
Indiana	17.5	2.0	64.7	15.8	161	Y
Florida	16.8	3.8	47.8	31.6	253	N
Texas	15.8	5.8	37.1	41.3	588	N
Alaska	14.6	4.2	58.1	23.1	26	Y
Minnesota	14.4	4.8	79.9	0.9	141	Y
Nevada	14.1	3.3	22.4	60.3	58	Y
Kentucky	13.7	2.3	67.6	16.4	121	Y
Nebraska	13.5	4.1	79.0	3.4	99	N
New Jersey	12.9	1.7	76.0	9.4	99	Y
Georgia	11.7	3.4	71.5	13.4	172	N
Ohio	11.3	4.5	75.1	9.1	224	Y
Arkansas	10.4	5.6	60.5	23.5	102	Y
Rhode Island	10.3	2.6	87.1	0.0	15	Y
Montana	10.1	3.6	80.6	5.7	66	Y
Connecticut	9.9	2.1	86.0	2.1	42	Y
West Virginia	9.3	11.0	63.8	15.9	61	Y
Maryland	8.5	8.0	83.1	0.4	62	Y
Massachusetts	8.2	5.7	69.2	16.9	102	Y
Illinois	8.0	3.7	80.8	7.5	208	Y
District Of Columbia	7.4	7.4	64.5	20.7	14	Y
Delaware	6.3	2.3	78.5	13.0	13	Y
Wisconsin	6.3	5.6	83.6	4.4	149	N
Arizona	6.2	6.0	61.4	26.4	110	Y
Michigan	6.2	2.3	79.9	11.6	165	Y
New Hampshire	5.5	1.3	74.1	19.1	31	Y
Maine	5.4	2.8	89.5	2.3	39	Y
South Dakota	4.4	9.1	82.1	4.3	64	N
Pennsylvania	3.8	3.6	79.3	13.3	235	Y
North Dakota	2.6	3.0	87.5	6.9	50	Y
Vermont	1.7	5.1	93.2	0.0	17	Y

Notes: The table presents shares of hospital beds contributed by different hospital control types 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 nonfederal shares. The total number of hospitals for each state is given in column 5. 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: Descriptive statistics, privatized versus closed hospitals

	(1) Privatized only	(2) Both	(3) Closed only	(4) Neither priv. nor closed
% Rural	51.8	100.0	51.6	61.3
Beds	94	35	78	119
	(98)	(9)	(61)	(168)
Admissions	3,253	1,066	1,977	4,139
	(4,519)	(464)	(2,394)	(6,910)
% Medicaid adm	16.0	12.3	18.9	15.5
	(9.7)	(8.4)	(11.6)	(10.4)
% Medicare adm	49.4	52.6	47.4	49.4
	(14.0)	(12.1)	(17.3)	(15.8)
% Other adm	34.6	35.0	33.7	35.1
	(12.1)	(14.3)	(12.5)	(12.5)
Total revenue/bed	412,725	213,025	208,781	410,896
	(247,051)	(129,848)	(177,895)	(321,561)
Total expenses/bed	410,777	264,847	369,113	432,193
	(222,370)	(98,220)	(324,492)	(305,444)
Personnel expenses/bed	220,334	139,234	182,541	231,568
	(116,535)	(48,957)	(139,224)	(160,708)
Total FTEs/100 beds	396	336	337	401
	(168)	(122)	(200)	(205)
# hospitals	249	5	31	771

Notes: The table presents descriptive statistics on four mutually exclusive groups of nonfederal government hospitals in our analysis sample as of 2000. The rows correspond exactly to the variables presented in Table 2. We compare hospitals that were privatized during 2001–18 but did not close in this period (col. 1); privatized and subsequently closed (col. 2); closed without privatizing (col. 3); and neither privatized nor closed during this period (col. 4). Columns 1 and 2 collectively correspond to column 1 and columns 3 and 4 collectively correspond to column 2, respectively, of Table 2.

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	100	115	111
	(105)	(164)	(153)
Admissions	3,565	4,020	3,925
	(4,807)	(6,813)	(6,445)
% Medicaid	16.1	15.5	15.6
	(9.7)	(10.5)	(10.4)
% Medicare	49.8	49.8	49.8
	(14.6)	(15.9)	(15.6)
% Other	34.0	34.7	34.5
	(12.2)	(12.7)	(12.6)
Medicare FFS	1,273	1,219	1,231
	(1,565)	(1,858)	(1,800)
B: Patient outcomes			
Mortality rate (30-day)	.116	.113	.114
	(.03)	(.037)	(.036)
Charges ($)	11,940	12,715	12,553
	(5,740)	(9,704)	(9,022)
Length of stay	5.377	5.294	5.311
	(1.093)	(1.325)	(1.28)
Pr(stay$<$ 2 days)	.151	.173	.168
	(.057)	(.074)	(.071)
# hospitals	204	776	980

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: Additional evidence on (log) hospital finances

	(1) Total revenue	(2) Total expenses	(3) Personnel expenses	(4) Remaining expenses
A: Per admission				
A1: No controls				
DD	0.057	-0.033	-0.085	0.013
	(0.026)	(0.024)	(0.024)	(0.035)
A2: Market controls				
DD	0.066	-0.025	-0.078	0.023
	(0.026)	(0.024)	(0.024)	(0.035)
Mean outcome ($)	8,158	8,498	4,633	3,864
B: Aggregate				
B1: No controls				
DD	-0.005	-0.097	-0.150	-0.041
	(0.022)	(0.021)	(0.022)	(0.045)
B2: Market controls				
DD	-0.003	-0.095	-0.149	-0.038
	(0.022)	(0.021)	(0.022)	(0.045)
Mean outcome ($ mn)	57.7	61.4	32.6	28.9

Notes: The table presents effects on revenue and expenses at the privatized hospitals, obtained by estimating Equation 1 on hospital-year level data. In Panel A, 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. Panel B presents results on aggregate outcomes expressed 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. In each panel, the top row reports coefficients from a two-way fixed effects specification with no covariates, and the bottom row reports coefficients from the same specification including time-varying market-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.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	-0.044	-0.077	0.018	-0.002
	(0.034)	(0.065)	(0.016)	(0.029)
Observations	19,234	19,234	18,449	18,449
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	13,568	13,841	14,077	14,360
Private insurance	15,122	15,427	15,122	15,427
Uninsured	6,009	6,130	6,009	6,130
Miscellaneous	15,359	15,669	15,359	15,669
Overall	12,621	12,716	12,832	12,927
% Increase in reimb.	0.5%	1.3%	2.2%	2.9%

Notes: The table presents results on the effects of changes in payer mix, list prices, or both on mean reimbursement. Panel A 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 privatization during 2001–18. Control HSAs do not have any privatizations 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	0.0033	0.0045	0.0055	0.0065	0.0077	
	(0.0013)	(0.0015)	(0.0016)	(0.0019)	(0.0022)	
A2: Patient and market controls						
DD	0.0034	0.0045	0.0057	0.0067	0.0084	
	(0.0014)	(0.0015)	(0.0016)	(0.0019)	(0.0021)	
Mean outcome (t-1)	0.118	0.156	0.183	0.241	0.322	
Observations	12,993,422	12,959,193	12,923,091	12,819,207	12,590,273	
B: By diagnostic category	(1) Circulatory	(2) Respiratory	(3) Digestive	(4) Musculoskeletal	(5) Kidney	(6) Miscellaneous
B1: Patient controls						
DD	0.0028	0.0020	0.0051	0.0019	0.0057	0.0038
	(0.0021)	(0.0024)	(0.0022)	(0.0016)	(0.0031)	(0.0017)
B2: Patient and market controls						
DD	0.0035	0.0034	0.0045	0.0040	0.0040	0.0032
	(0.0023)	(0.0027)	(0.0021)	(0.0018)	(0.0029)	(0.0021)
Mean outcome (t-1)	0.096	0.158	0.085	0.049	0.119	0.146
Observations	3,049,068	2,216,794	1,413,243	1,401,345	930,325	3,982,642

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 demographics and a mortality risk index 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 and are bootstrapped to account for the first step construction of the risk index.

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

	(1)	(2)	(3)	(4)	(5)
	All treated	All controls	Std. difference	Matched controls	Std. difference
# hospitals	254	802		254	
Beds	87	116	-0.18	94	-0.05
Total admissions	3,038	4,156	-0.16	3,164	-0.02
Medicaid admissions	622	1,057	-0.21	643	-0.02
Expenses (mn)	62	106	-0.22	64	-0.02
HSA population	577,057	695,743	-0.08	492,422	0.08
% in poverty (county)	16.8	15.8	0.16	16.9	-0.02
% unemployment (county)	7	6.3	0.24	7.1	-0.05

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	0.0659	0.0024	-0.0171	0.0071
	(0.0220)	(0.0142)	(0.0068)	(0.0027)
I: Specification checks				
A: State-year FEs	0.0826	-0.0159	-0.0213	0.0095
	(0.0234)	(0.0253)	(0.0110)	(0.0052)
B: Incl. pre-trend	0.0692	-0.0160	-0.0198	0.0045
	(0.0182)	(0.0136)	(0.0075)	(0.0034)
II: Alternate estimators				
A: CS estimator	0.0402	-0.0002	-0.0119	0.0048
	(0.0155)	(0.0109)	(0.0067)	(0.0032)
B: DCDH estimator	0.0314	-0.0012	-0.0122	0.0046
	(0.0144)	(0.0105)	(0.0060)	(0.0029)
III: Alternate samples: Treated group				
A: Balanced panel	0.0695	0.0019	-0.0121	0.0063
	(0.0219)	(0.0165)	(0.0077)	(0.0034)
B: All treated obs	0.0853	0.0049	-0.0150	0.0090
	(0.0217)	(0.0170)	(0.0077)	(0.0033)
IV: Alternate samples: Comparison group				
A: Matched sample	0.0548	0.0030	-0.0190	0.0079
	(0.0291)	(0.0169)	(0.0087)	(0.0040)
B: Switchers included	0.0734	0.0045	-0.0164	0.0069
	(0.0207)	(0.0126)	(0.0063)	(0.0029)

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. The models control for patient demographics and a mortality risk index, as described in Section 4. 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 Sant'Anna and de Chaisemartin d'Haultfoueille, DCDH in short. 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 IV tests the robustness to varying the comparison group. Row A presents results using a matched subsample, and the sample in row B includes hospitals that switch between public and private status. Standard errors are clustered by hospital and are bootstrapped to account for the first step construction of the risk index.

Table A.12: Hospital occupations in 2019

Occupation name	Share of employment Local	Private	Major sub-occupations
(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, obtained from the industry-occupation matrix data provided by the Bureau of Labor Statistics ((Bureau of Labor Statistics 2019). 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 health care practitioner and technical" together form the category, "health care 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: Staff availability (per patient)

	(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	-0.50	-0.03	0.00	-0.46	-0.01
	(0.26)	(0.01)	(0.07)	(0.19)	(0.01)
Observations	19,234	19,234	19,234	19,234	8,536
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 some market-level covariates are missing. 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 and leadership churn)

	(1)	(2)	(3)	(4)
	Total	Physician	Other	CEO change
Baseline	-33.0	-2.6	-29.1	0.048
	(12.9)	(0.8)	(9.7)	(0.018)
I: Specification checks				
A. Weighting by beds	-29.2	-4.6	-22.8	0.051
	(14.7)	(1.8)	(10.5)	(0.034)
B. State-year FEs	-23.1	-2.5	-21.9	0.045
	(13.1)	(0.8)	(9.9)	(0.018)
C. Incl. pre-trend	-35.3	-3.2	-31.1	0.041
	(14.2)	(0.9)	(10.7)	(0.020)
II: Alternate estimators				
A. CS estimator	-33.5	-2.3	-27.0	0.046
	(13.2)	(0.8)	(9.9)	(0.029)
B. DCDH estimator	-31.9	-1.9	-26.4	0.046
	(13.0)	(0.8)	(9.8)	(0.029)
III: Alternate samples: Treated group				
A. Balanced panel	-18.9	-2.2	-17.7	0.049
	(13.8)	(0.9)	(10.4)	(0.020)
B. All treated obs	-53.1	-3.2	-43.9	0.064
	(14.4)	(0.8)	(10.8)	(0.013)
IV: Alternate samples: Comparison group				
A. Matched sample	-23.5	-1.9	-23.4	0.060
	(14.1)	(0.9)	(10.5)	(0.022)
B. Switchers included	-31.7	-2.5	-27.9	0.049
	(12.8)	(0.8)	(9.6)	(0.018)

Notes: The table shows the results of robustness checks for the effects on staff availability per 100 beds and leadership churn 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: Heterogeneity in treatment effects

	(1) Treated Hospitals	(2) Finances Revenue	(3) Finances Tot. Exp.	(4) Volume Total	(5) Volume Medicaid	(6) Staff FTE/bed	(7) Mortality 30-day
Baseline							
DD	254	0.083	-0.006	-0.089	-0.156	-33.041	0.0033
		(0.032)	(0.026)	(0.028)	(0.043)	(12.871)	(0.0013)
A: Private equity partner							
Nonprofit	180	0.080	0.003	-0.138	-0.186	-32.926	0.0030
		(0.037)	(0.031)	(0.032)	(0.052)	(15.805)	(0.0016)
x 1(for-profit, not PE)	63	0.025	-0.005	0.165	0.090	9.367	-0.0007
		(0.074)	(0.058)	(0.064)	(0.091)	(26.464)	(0.0029)
x 1(for-profit, PE)	11	-0.097	-0.156	-0.010	0.078	-67.245	0.0065
		(0.170)	(0.144)	(0.087)	(0.151)	(56.937)	(0.0051)
B: National systems							
Ind. or small system	97	0.150	0.078	-0.090	-0.184	-5.275	0.0024
		(0.054)	(0.042)	(0.045)	(0.065)	(15.985)	(0.0028)
x 1(Regional system)	98	-0.115	-0.138	0.020	0.075	-41.049	0.0024
		(0.068)	(0.054)	(0.061)	(0.092)	(26.883)	(0.0030)
x 1(National system)	59	0.024	0.002	-0.048	-0.077	-10.600	-0.0035
		(0.085)	(0.069)	(0.071)	(0.112)	(36.549)	(0.0032)
C: Rural county							
DD	120	0.022	-0.078	-0.024	-0.103	-59.246	0.0020
		(0.049)	(0.039)	(0.035)	(0.058)	(19.540)	(0.0014)
x 1(Rural)	134	0.109	0.135	-0.122	-0.100	49.450	0.0053
		(0.062)	(0.050)	(0.053)	(0.082)	(24.851)	(0.0024)
Observations		16,829	20,387	20,387	20,386	20,387	12,993,422

Notes: This table presents additional results on heterogeneity in treatment effects of privatization on key outcomes of interest in hospital finances (revenue and total expenses), inpatient volume (total and Medicaid), staff total FTE/bed, and 30-day mortality rate. Column 1 presents the number of treated hospitals corresponding to different specifications of interest. All models exclude time-varying covariates. The top row presents the baseline D-D coefficient to facilitate comparison. Panel A presents results from a triple difference model studying the differential effect on hospitals privatized to for-profit partners, and within that, to private equity-owned partners. Panel B studies the differential effect on hospitals privatized to hospital system partners owning 5 or more hospitals at the time of acquisition, and within that, to acquirers that are regional (span < 4 states) or national systems (span ≥ 4 states). Panel C presents the differential effect for targets located in rural counties per the USDA classification system. Standard errors are clustered by hospital and in the case of mortality are bootstrapped to account for the first step construction of the risk index.

Table A.16: Effects of hospital closures

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: Full sample				
A1: No controls				
DD	0.021	0.018	0.019	0.000
	(0.023)	(0.036)	(0.026)	(0.068)
Obs	16,926	16,926	16,926	16,926
A2: Market controls				
DD	0.006	0.016	-0.003	-0.020
	(0.022)	(0.039)	(0.024)	(0.069)
Obs	16,154	16,154	16,154	16,154
B: Matched sample				
B1: No controls				
DD	0.013	0.061	-0.004	0.008
	(0.037)	(0.058)	(0.040)	(0.088)
Obs	870	870	870	870
B2: Market controls				
DD	-0.007	0.043	-0.029	-0.035
	(0.036)	(0.057)	(0.040)	(0.086)
Obs	843	843	843	843
Mean outcome (t-1)	78,826	17,267	30,338	31,221

Notes: The table presents estimated effects on log inpatient volume in markets which have experienced a hospital closure. The 27 treated markets here each experienced one hospital closure between 2001 and 2018. The comparison group includes approximately 700 markets that did not experience a closure nor a privatization during the sample period. Additional details are in Appendix B.10. Panels A and B present the coefficients obtained by estimating Equation 1 using the full sample or a matched subsample, respectively. Within each panel, we present results without and with time-varying market-level covariates, respectively. The mean values pertain to outcomes (in levels) in the treated markets in the year before privatization. Standard errors are clustered by market.

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 privatization 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 positives. Our final tally is 254.

Through these detailed reviews, we classify privatizations into five groups, described below and also listed in Table 1. The private operator typically has a short planning horizon and assumes little or no financial risk in the first three arrangements, while taking on partial or complete financial control and risk in the latter two arrangements. We provide a representative example for each type to help illustrate the differences between these deals.

- **Public hospital incorporating (41):** In the modal case, the public health system files for 501C3 nonprofit status ("incorporating"). The hospital enjoys independent management but is still under the oversight of the local government through representatives on its governance board. A key motivation for this change appears to be to relax credit constraints on the hospital since private nonprofits can raise debt faster and more easily than a government entities, which often have to get voter approval first.

  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.

- **Contract management (61):** Occurs when a private (corporation or health system) firm takes over the day-to-day management of a hospital for a pre-specified contract period. The government maintains control over the hospital's property, assets, and debts. The private firm often brings in new managerial leadership. The incentive structure of the private partner varies within this category. In some deals, the firm receives a fixed fee, while in others, it appears there may be some profit sharing.

  Example: Weiner Memorial Hospital (Marshall, MN) recorded a conversion in the AHA in 2009 from “City" to “other not-for-profit." Manual validation found that the hospital's board signed an agreement with Avera Health, a regional nonprofit hospital system in Sioux Falls, SD in *2004* to manage the hospital. The name of the hospital was changed in that year to Avera Marshall Regional Medical Center to signal Avera's management control. The hospital CEO also changed in 2004. In 2009, Avera bought the hospital from the city, which may explain why the AHA recorded this change only then. We consider 2004 as the year of privatization for our analysis.

- **Joint venture (5):** Occurs when one or more private (corporations or health systems) firms agree to enter officially into a joint venture with the local government authority, which results in a newly formed private firm to operate 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.

- **Long-term lease (58):** Occurs when a private corporation or health system takes over operational management of a hospital for an extended period of time (more than 10 years, and usually more than 15). The government entity maintains control over the hospital's property and debts. However, the private partner typically makes capital investments into the facility and takes on financial risk.

  Example: McCune-Brooks Regional Hospital (Carthage, 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. The hospital was renamed to Mercy McCune-Brooks Hospital following the change. Mercy eventually bought the hospital from the city.

- **Sale (88):** Occurs when there is a permanent transfer in the ownership and control of the property, assets, and debts of a hospital, from the local government owner to a private non-profit or for-profit firm. The latter clearly prefer to purchase hospitals over other types of arrangements, since 50% of all deals involving for-profit acquirers are sales, versus less than 30% of deals involving nonprofits.

  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. Incidentally, IASIS was backed by a private equity firm (TPG). IASIS eventually merged with Steward hospitals in 2017.

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 privatizations by starting with cases 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[28]. We also require that hospitals be listed as private for at least two "post"

28. We include the year 2000 to identify privatizations that occur in 2001. A very small number of hospitals enter the sample after 2000, another reason for imbalance in the sample.

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 privatization 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 privatizations.

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)
- 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.

We further classify hospitals' markets as rural or non-rural based on the county in which they are located. Specifically, we merge each hospital's county identifier to the U.S. Department of Agriculture's Rural–Urban Continuum Codes: RUCC (https://www.ers.usda.gov/data-products/rural-urban-continuum-codes). We define a rural binary variable equal to one if the hospital is located in a county classified as rural (RUCC 5–9) and zero otherwise, and we identify a hospital to be rural if it is identified as rural in most years. Our classification primarily relies on the 2013 RUCC, with the 1993 and 2003 versions used for the two counties not present in the 2013 codes. This approach provides a consistent measure of rural market status across all hospitals in our sample.

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 (NCI SEER 2023). 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.

Verifying and Cleaning Hospital CEO Names — The AHA surveys report the name and title of the hospital's principal officer in a field labeled "Name of Chief Administrator." A range of job titles such as Chief Executive Officer (CEO), Chief Administrative Officer (CAO), President, Administrator, and other variants occur in this field. For simplicity, we refer to all these roles as "CEO" throughout the analysis. We use this field to construct our hospital-year panel of CEO identifiers and define a CEO change as occurring in year t if the listed name in year t differs from the name in year $t-1$, after applying the cleaning and standardization procedures described below.

We first assess the reliability of the AHA data on CEO names. We cross-validate the reported CEO names from AHA data against publicly available IRS Form 990 data sourced from ProPublica's Nonprofit Explorer. We match a set of 50 hospitals that appear in both the AHA and IRS samples and manually compare the CEO names reported in each source across available years. We have in total of 570 matched hospital-year observations, with an average panel length of approximately 11 years per hospital. Based on IRS filings, we find that the AHA survey typically reports CEO transitions one year earlier than the IRS. To align the data, we systematically shift the AHA records forward by one year. After this adjustment, the two sources show strong consistency: in 94.16% of cases, the CEO name reported by the AHA also appears in the IRS Form 990 for the same year, either as CEO or as another key officer. Expanding the comparison window to include IRS filings within ± 1 year raises the match rate to 96.75%, and further to 96.92% when using a ± 3-year window.

Based on these exercises, we believe that the AHA's reporting of CEO names is sufficiently accurate for our analysis and is better suited for our purposes. While IRS Form 990s are commonly used to identify hospital leadership, they are available only for nonprofit hospitals, excluding government and for-profit facilities. Even among nonprofits, filings vary in availability and completeness. For some systems—particularly large or public hospitals—leadership information may appear under affiliated foundations or parent organizations, complicating the link between executives and individual facilities. Moreover, titles in IRS forms are often ambiguous, and prior to 2013, filings frequently omitted specific job titles, making it difficult to reliably identify CEOs. In contrast, the AHA panel offers broader coverage and a longer historical span specifically for one key officer.

We then performed a series of cleaning and standardization procedures on the AHA's CEO names:

- Standardize formatting by converting all names to lowercase, removing punctuation (e.g., periods, commas, hyphens), trimming excess whitespace, and excluding job titles that appear inconsistently (e.g., "CEO," "President," "Administrator")
- Perform within-hospital fuzzy matching to identify likely variations of the same individual (e.g., "Kris Doody," "Kris A. Doody," "Kris Doody-Chabre") using a Jaro-Winkler distance threshold of 0.8
- Check for common nickname variants (e.g., "William" vs. "Bill," "Robert" vs. "Bob") when fuzzy matches were close but not automatically linked
- Manually review matched name pairs to ensure that linked name pairs truly refer to the same individual

These procedures collapsed 777 name variants into 384 distinct individuals. Of these, 291 were further verified using credible external sources such as official hospital announcements and public profiles. We use all matched names from this process to standardize CEO names in our dataset, which results in 4,208 unique individuals across all hospitals across the years. This total comprises 1,323 distinct individuals for the 254 treated hospitals, 1,120 for the 254 matched control hospitals, and 2,115 for the 548 additional control hospitals.

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 (HCUP 2026). 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-DOH 2026). 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 (CA-HCAI 2026). 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 (MN-DOH 2026). 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 (ResDAC 2026). 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 (CMS 2022). 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 Vital statistics microdata

We study changes in mortality rates at the market level using restricted-use 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.[29] 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.7 Miscellaneous datasets

This section provides more details on how we sourced data and calculated values for miscellaneous variables used in the analyses. These measures are typically covariates we include in the

29. The data and data dictionary can be found at https://seer.cancer.gov/popdata/. The SEER data are designed to provide more precise population estimates for years between censuses; see, e.g., Finkelstein et al. 2025; Ruhm 2015.

regression models to control for differences in economic and political factors between different counties.

B.7.1 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 (MEPS 2023). 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.7.2 Local government debt information

We constructed county-year measures of municipal debt issuance from the SDC Municipal Deals dataset provided by London Stock Exchange (LSEG) (LSEG Data & Analytics 2025). We accessed this data through a subscription provided by Wharton Research Data Services (WRDS). The raw file contains bond-level observations of all tax-exempt and taxable municipal securities from 1991 to 2025. Each issuer is uniquely identified by its name and state code. To ensure geographic comparability, we excluded state-level and national authorities as issuers and linked the remaining issuers to U.S. counties or county equivalents through a multi-stage crosswalk. Specifically, issuer names were first matched to Census geographic reference files—including county, county-subdivision, and place lists—within the same state using fuzzy string matching. Ambiguous or unmatched cases were then cross-validated through a structured ChatGPT API query, which returned candidate county names and indicators for multi-county coverage. When an issuer spanned multiple counties, we assigned it to the largest county by population to maintain a consistent one-to-one mapping between issuers and counties. This procedure successfully linked 47,862 issuers, representing approximately 99% of total par value, to county FIPS codes; the small number of unmatched issuers were excluded from the final dataset. We then aggregated the bond-level observations to the county–year level to form a county-equivalent panel. In particular, we constructed two key county-level variables: (1) an indicator for health-related bond issues (bonds whose purpose references health or hospital facilities), and (2) the total par amount of debt issued (in 2019 USD millions). These variables capture both the intensity and composition of local borrowing, particularly as it relates to health-sector investment and fiscal capacity.

Unfortunately, we were unable to include two key financial attributes (bond yield and credit ratings) in the analysis, as both variables exhibit severe missingness and outliers. Yield is reported in percentage points but contains numerous implausible values (e.g., yield is reported $\leq$ 100%). After reasonably trimming observations with yields above 20%, the yield remains missing for roughly half of all bonds. Six types of credit ratings from S&P, Moody's, and Fitch are available in the dataset, but each source exhibits extensive nonreporting. Even the two most complete measures, Moody's Long-Term Rating and S&P's Long-Term Rating, are missing for 83.50% and 83.36% of issues, respectively. The widespread absence of ratings is because many local bond issues are small in size and therefore never formally rated by credit agencies. Given this pervasive data-quality problem, we excluded both yield and rating variables.

B.7.3 House vote shares

We constructed measures of U.S. House vote shares using data from Dave Leip's United States Election Data, under the "House" tab for all election years (Leip 2025). For each county, we organize by county FIPS and year, and extract the democratic vote total, republican vote total, other vote total (3^{rd} party, etc.), total write-in votes, and total votes. Notably, we do not count write-in votes in the total vote number. We do not include any data for Alaska's counties as we were not able to find accurate measures within the raw data. As mentioned with the presidential variables, Broomfield, CO was created in November, 2001, therefore our data for Broomfield begins in 2002. We count votes for Shannon County and Oglala Lakota, just as we did for presidential votes. Clifton Forge, VA became part of Alleghany county in 2001, and we drop the votes for Clifton Forge, VA. With congressional elections, there were many instances where the election was uncontested, particularly in Oklahoma, Arkansas, and Louisiana. As a result of an election being uncontested, the total vote number is 0. In these cases, we linearly interpolate from the most recent contested elections to get an estimate of the vote totals. Using the Democratic vote total, Republican vote total, and total votes, we create the Republican share of total votes by dividing Republican votes by total votes. We linearly interpolate from congressional election years to show shares over time for non-election years.

B.7.4 Local government finances

We constructed a county-equivalent panel of local government public finances from the U.S. Census Bureau's Annual Survey of State and Local Government Finances and the quinquennial Census of Governments (US Census Bureau 2025)[30], harmonizing all records to a consistent county geography over 1997–2019 using the government ID–FIPS crosswalk.[31] For standard counties we take the county-government record directly; for consolidated city–counties (e.g., Jacksonville–Duval, Indianapolis–Marion, Denver, San Francisco, Broomfield, Nashville–Davidson, Baltimore City, St. Louis City, and the City of New York) we use the consolidated municipal record in lieu of a separate county entry. For states without functioning county governments—Connecticut, Rhode Island, and most Massachusetts counties after the late-1990s abolitions, we aggregate general-purpose municipal governments to the county boundary. For Connecticut we treat towns as the base units (adding the few independent, non-coterminous cities) and avoid double-counting consolidated city-towns[32], while in Massachusetts and Rhode Island we sum all cities and towns,

30. For 1997–2011, we extract total revenue and total expenditure from the historical tables `IndFinXXa.Txt` contained in `_IndFin_1967-2012.zip`. For 2012–2016, we compute total revenue, total expenditure and population from the Public Use Zip files `YEARFinEstDAT_pu.txt` (e.g., the 2016 release), following the Census classification code manual. For 2017-2019, we compute total revenue and total expenditure from the earliest published version of the Public Use Zip files `YEARFinEstDAT_pu.txt` (e.g., the 2019 release) via the Wayback Machine. Population is merged from `Fin_P(G)ID_YEAR.txt` in the same zip file.

31. We map governments id (used in 1997-2018's data) to county FIPS (used in 2019's data) using `GOVS_to_FIPS_Codes_State_&_County_2012.xls` in `_IndFin_1967-2012.zip`.

32. Beginning with the 2017 reference year, the Census Bureau's Annual Survey of State and Local Government Finances replaced county FIPS codes for Colorado with planning-region identifiers in its most recent data releases. To maintain a consistent county-level geography, we retrieved the earliest published version of the data via the Wayback Machine, which retains the original county codes.

which are mutually exclusive and collectively exhaustive. We adopt standard Census government identifiers to de-duplicate, reconcile name variants, and ensure a single observation per state–county–year.

The construction of fiscal variables total revenue and total expenditure strictly follow 2006 Census Classification Manual. Revenues are the sum of tax receipts, intergovernmental transfers, charges, utility, insurance trust and miscellaneous revenues; expenditures are the sum of total direct general outlays, including operations, capital, interest, assistance, and intergovernmental payments. For each county-year we form two analysis measures of county-level budget health from previous measurement of total revenue and total expenditure: (i) the revenue-to-expenditure ratio, rev/exp; and (ii) the surplus amount per capita, (rev − exp)/pop. Where municipal aggregation is required (e.g., CT/MA/RI or independent-city cases), we first sum revenues and expenditures across component governments within the county boundary and then compute ratios on these sums. To preserve panel continuity we fill within-county gaps in population and aggregate finance series using linear interpolation (with limited end-point extrapolation for edge years), and we conduct standard quality screens to exclude mechanically extreme ratios arising from near-zero denominators.

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)

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.

B.9 Savings estimates

We examine the effects of privatization on hospital finances and report the average effects on revenue and costs in Section 5.1. Later, in Section 5.3, we estimate the effect of privatization on mortality rates for Medicare fee-for-service patients aged over 65. This enables estimation of the average effect on deaths and life-years lost on average due to privatization. In Section 8, we calculate the ratio of these two estimates to arrive at the savings per additional death (or life-year lost) per year due to the average privatization. This section describes the methodology and assumptions to calculate these ratios and obtain their confidence intervals.

Numerator: We apply the estimated effect on log aggregate costs, reported in Table A.5 Panel B1 column 2, to the average hospital cost before privatization (61.4 mn), to obtain the estimated reduction in aggregate cost in millions of dollars: 5.7 (1.3). We then simulate 1,000 observations of a normally distributed variable with mean equal to the point estimate and standard deviation equal to its standard error. Under the assumption that the government's benefit is limited to eliminating the subsidy that was previously needed to offset the deficit (average \$2.53 mn), we cap the saving amount to this value. The cap is binding for most observations, and hence the resulting government savings variable has mean \$2.527 million (0.04).

Denominator: We need estimates of the additional deaths and life-years lost due to privatization. To obtain these, we must scale the estimated mortality effect by the average annual patient volume at privatized hospitals (1,273), reported in Table A.4. To be conservative, we adjust patient volume downward by the estimated effect on Medicare volume reported in Table 4 Panel A column 3, giving an estimated FFS volume of 1,207 (39). We draw 1,000 observations of two independent normally distributed variables, one which simulates the mortality estimate 0.0033 (0.0013) and the other which simulates the patient volume parameters mentioned above. The product of these two variables give us the estimate of additional deaths, with mean 3.9 and standard deviation 1.5. To arrive at the life-years lost, we scale the deaths variable by 5.34, as described in the text. The corresponding estimate is 20.9 (8.2).

Ratio: We assume that the estimates of savings and additional deaths or life-years lost are independently distributed. This simplifying assumption is consistent with the mixed evidence found in the heterogeneity tests in Section 7. For example, Figure A.12 shows similar increases in mortality across 4 different types of deals whereas the corresponding cost effects are dramatically different. Under this assumption, the savings per additional death (or life-year lost) can be calculated as the ratio of the two simulated variables described above. The resulting distributions have some extreme outlier values. To mitigate their influence, we winsorize at the 0.5% level, affecting a total of 10 observations. The resulting savings per death has mean \$ 0.8 million and we report the 2.5 and 97.5 centiles to define the 95% confidence interval. We use the equivalent approach for savings per life-year lost.

B.10 Hospital closure analysis

Section 8 briefly describes the effects of government hospital closures on patient volume and Appendix C.2 briefly describes the corresponding effects on mortality rates in the affected county. We use a difference-in-differences research design for this exercise, analogous to the market-level analysis of the effects of privatization. Markets that experience a closure are considered treated for this analysis, while those that do not experience a privatization nor a closure constitute the comparison group. As in the main analysis, we use health service areas (HSAs) to define markets. Standard errors are clustered by market.

We identify 31 hospitals that close without being privatized and another 5 hospitals that are privatized and then closed subsequently during the 2001–18 period. Since the goal is to examine the effects of closure, we focus only on the 31 hospitals for this analysis. The analysis is performed at the market level, as mentioned above. The 31 closed hospitals are located in 31 distinct markets, which is the starting set of treated markets. We drop 198 markets that experience a privatization during this period from the sample so that the comparison markets experience neither closure nor privatization. 4 markets experience both types of treatment, but in all cases the privatization occurs before or in the same year as the closure. Hence, we drop these from the sample as well. The final analysis sample has 27 treated markets that experience one closure each, and 698 comparison markets. The estimating equations are exactly equivalent to those used to examine the market-level effects of privatization on patient volume, except that the treatment of interest here is closure.

Figure A.13 presents the corresponding event study plots on the effects on inpatient volume (total and by payer). The figures suggest no decrease in market volume after a closure, in fact Medicare volume appears to increase. In particular, there is no effect on Medicaid volume after a closure, which contrasts with a noticeable decrease after a privatization. Table A.16 presents the corresponding point estimates. Panel A presents the effects using the full sample described above. None of the coefficients are statistically significant and several are positive in sign. Panel B presents the corresponding results from using a matched subsample, where we apply the same matching algorithm used in the main robustness checks to identify a subset of 27 comparison markets similar in attributes to the treated markets. The tradeoff is that the sample drops considerably in size since only 54 markets remain. Not surprisingly, the coefficients lack precision. Again, several are positive in sign, including for Medicaid.

As we note in the text, these patterns may partly reflect the fact that hospitals targeted for closure were small and served few patients even prior to closure (see Table A.2). So the ATT likely does not reflect the effect of closing the "average" government hospital.

We also use this research design to examine the effect on market-level mortality rates using CDC vital statistics data. The main difference here is that we only include the affected county within the treated HSA in order to maximize our chances of detecting an effect. Briefly, the coefficient patterns suggest a decline in mortality after closure rather than an increase. However, the event study plot does not imply a sharp or persistent change in mortality rates so we interpret these coefficients with caution.

C Market level Mortality

We test whether a decline in access to hospital care 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[33]; 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 in our primary analysis. 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 and closure on mortality at the market level. Section B.6 provides more details.

C.1 Effects of privatization

Table C.1 column 1 presents the estimated effect of privatization 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

33. 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.

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.

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 marginally significant and implies that a 4% decline in aggregate Medicaid volume, approximately what we estimate on average, predicts 3.2 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.

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 residing 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.

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 36 per 100,000, or 3.5% of baseline mortality. This result suggests that publicly owned hospitals serve a vital social function in lower-income markets.

C.2 Comparing the effects of privatizations and closures

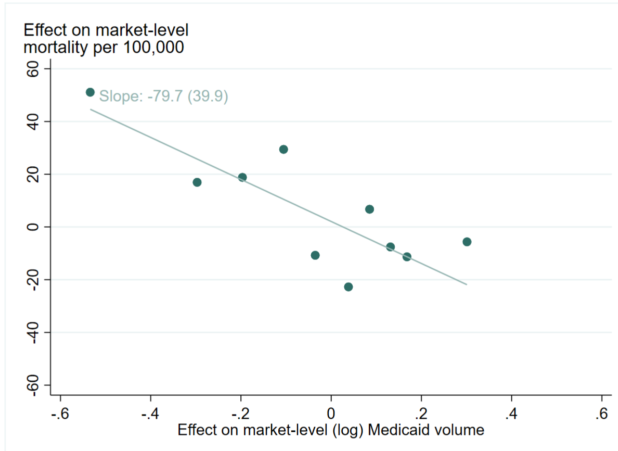
We examine the effects of closures of government hospitals on mortality rates in affected markets using an analogous research design. Recall that 36 government hospitals closed during our sample period, of which 5 were privatized before they closed. We focus on the remaining 31, which were located in 31 distinct markets. However, 4 markets experienced privatizations before they experienced a closure so it is difficult to disentangle these two effects for these markets. We therefore focus on the remaining 27 markets that experienced only closures, as our treated group. The preceding analysis of privatizations found that mortality effects are larger in magnitude among residents of the county in which the treated hospital is located. Hence, for this analysis also we focus on the affected county and exclude the remaining counties in the treated HSA. 698 HSAs experienced neither a closure nor a privatization during this period and they form the comparison group for this analysis. This section briefly describes these results and compares them to the corresponding effects of hospital privatization. Appendix B.10 provides more details on the sample construction and research design to examine the effects of closures.

Table C.2 presents the resulting coefficients. Panels A and B present the effects of privatization and closures, respectively. In each panel, the top row presents the D-D coefficients from the baseline specification without any market-level covariates. The bottom row presents coefficients adjusted for covariates. We will focus on the coefficients without covariate adjustment for brevity. Column 1 presents the effects on the mortality rate across all residents. The remaining columns present results for individuals of different age groups, which collectively span all ages. Mean mortality rates corresponding to the treated markets in the year prior to treatment are also presented and show that markets experiencing privatization and closure had similar levels of mortality overall and for all the age groups.

Comparing the coefficients in columns 1 in Panel A1 and B1 shows that privatization leads to a small increase in overall mortality, while closure leads to a decrease. Neither coefficient is precisely estimated, although the effect of closure is marginally significant. Figure C.2 presents the associated event study plots. Neither figure implies sharp or persistent changes in mortality rates after the treatment, explaining the lack of precision. However, the patterns of dynamic effects are directionally consistent with the regression coefficients. Examining the coefficients for specific age groups reveals some nuances. In the case of privatization, the overall effect appears to be driven by individuals age 55 and older, with the largest proportional effects among the 55–64 group. Younger individuals appear to be largely unaffected. In the case of closures, we obtain negative coefficients for all groups except those aged 55–64. The largest proportional effects are for individuals aged 15–34, who experience a nearly 10% decrease in mortality.

Taken together, the results suggest that hospital closures do not increase mortality rates for the local population and may in fact lead to declines. Since the event study plots are somewhat ambiguous, we interpret these coefficients with caution. That being said, these results are directionally consistent with prior, more general evidence on hospital closures that has also found neutral or positive mortality effects of closures, potentially since patients are reallocated to higher quality facilities (Petek 2022; Chandra, Dalton, and Staiger 2023). Olenski (2023) examines nursing home closures and comes to a similar conclusion there as well. Since the focus of this paper is on hospital privatization, we do not perform a deeper investigation of the effects of closures, which may merit a separate study and leave it as a task for future work.

(a) Medicaid admissions and mortality



(b) Other admissions and mortality

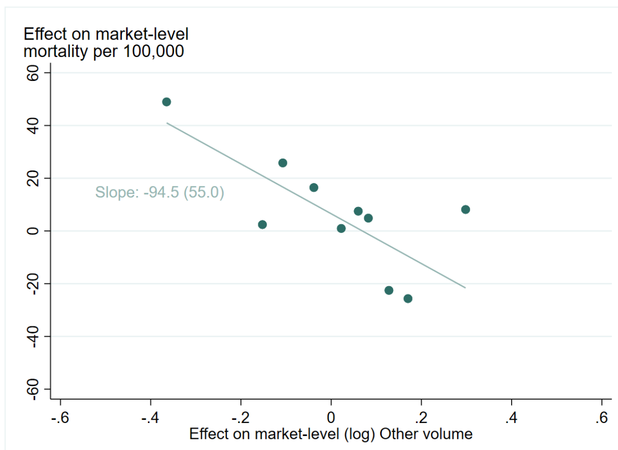
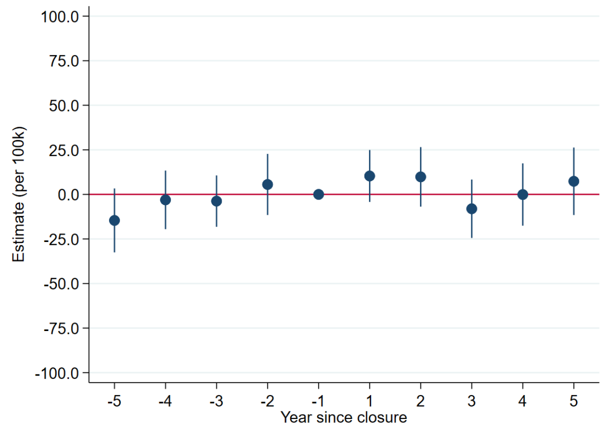


Figure C.1: Hospital admissions and market-level mortality

<u>Note:</u> 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.

(a) Privatizations



(b) Closures

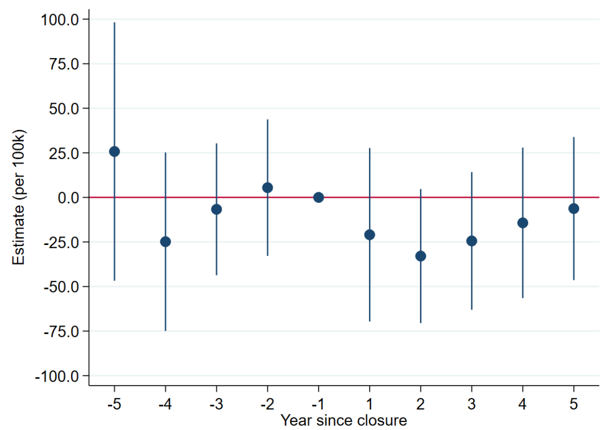


Figure C.2: Effects of privatization and closure on mortality rates

Note: The figure presents event study plots of dynamic effects in the event window around a hospital privatization (Panel a) or closure (Panel b). These coefficients pertain to the effects for the affected county and are estimated by comparing against unaffected health service areas (HSAs). Standard errors are clustered by HSA.

Table C.1: Effect on mortality

	(1)	(2)	(3)	(4)	
	All	Affected county	Other counties	DD	x 1(> med. poverty)
A: No controls					
DD	4.5	14.4	-5.1	-3.6	35.5
	(6.6)	(11.6)	(8.4)	(13.4)	(22.7)
Obs	19,288	19,288	19,136	19,288	
B: Market controls					
DD	6.2	16.5	-3.5	4.1	24.2
	(6.6)	(11.6)	(8.5)	(13.7)	(22.7)
Obs	18,449	18,449	18,298	18,449	
Mean outcome (t-1)	1,020.1	1,036.5	1,025.7	1,036.5	

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

	(1) All ages	(2) <15	(3) 15-34	(4) 35-54	(5) 55-64	(6) ≥65
A: Hospital privatizations						
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)
Observations	19,288					
A2: Market controls						
DD	7.7	-2.1	-2.7	-2.7	16.5	16.5
	(5.6)	(2.3)	(2.8)	(4.9)	(11.6)	(25.3)
Observations	18,449					
Mean outcome (t-1)	1054.2	71.1	123.5	387.8	1036.5	4923.4
B: Hospital closures						
B1: No controls						
DD	-20.0	-0.1	-13.9	-11.3	14.3	-125.4
	(10.6)	(6.4)	(6.9)	(15.0)	(23.0)	(59.2)
Observations	16,926					
B2: Market controls						
DD	-17.5	0.2	-12.5	-7.9	14.7	-123.3
	(11.0)	(6.7)	(6.9)	(14.5)	(22.9)	(62.7)
Observations	16,154					
Mean outcome (t-1)	1064.0	64.3	146.1	372.5	1192.7	4843.0

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 of hospital privatizations for all-cause mortality, split by mutually exclusive and exhaustive age groups. In Panel B, we show the corresponding effects of hospital closures on all-cause mortality. Appendix B.10 describes the sample construction and design in detail. Mean values are computed for treated counties in the year before treatment.

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