The Impact of Privatization: Evidence from the Hospital Sector*

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Abstract

Privatization has been shown to improve the efficiency and growth of public firms. However, the effects for consumers and workers are understudied. We study potential trade-offs in the hospital sector where private operators could deliver much-needed efficiency gains, but profit maximization could adversely affect some patients. Hospitals in the US have experienced significant privatization: public control of hospital capacity declined by 42% over 1983–2019. Across 257 transitions, we find that privatized hospitals downsize capacity and patient care, with low-income Medicaid patients experiencing the greatest decline of 14%. While other patients appear to be absorbed by neighboring hospitals, Medicaid patients experience an aggregate decline in utilization at the market-level, which we interpret as a decline in access to care. The decline in aggregate Medicaid utilization is greater in markets with greater poverty and hospital concentration levels, implying spillover responses by competing hospitals in these markets. Private owners lower operating costs by reducing labor intensity at the hospital, potentially improving efficiency.

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1 Introduction

Should governments deliver services themselves or outsource to private firms? Economists have long been interested in this question, with views spanning the whole spectrum of possibilities (Hayek, 1944; Lerner, 1944). While large privatization drives caught the public's attention in the 1980s and 1990s, it remains an important ongoing phenomenon, with nearly a trillion dollars raised over 2013–16 through the sale of government assets (Megginson, 2017).[1] The balance of empirical evidence suggests that privatization improves the efficiency and growth of government-owned firms (Ehrlich et al., 1994; World Bank, 1995). However, the effects on consumers have rarely been examined (Megginson and Netter, 2001; Estrin et al., 2009). This is a key limitation since the privatization debate now centers over social services, which have traditionally been provided directly by governments (Mehrotra and Delamonica, 2005; Stiglitz, 2005). Several countries have privatized or are contemplating the privatization of healthcare providers, however, it remains contentious (Bergman et al., 2016; Knutsson and Tyrefors, 2022).[2] This paper begins to fill this gap by studying the trade-off between greater efficiency and potential harm to consumers in the case of hospital privatization in the US.

Economic theory predicts that privatization could hurt consumers under certain circumstances. Hart, Shleifer and Vishny (1997) show that private firms may increase their surplus by cutting costs on dimensions which are not contractually stipulated or difficult to monitor. Managers in private firms may have high powered incentives to maximize profits, diverting their attention away from less salient objectives such as maintaining quality and access in the case of healthcare (Holmstrom and Milgrom, 1991). Hospitals are very complex firms and therefore difficult-to-monitor cost-

[1] The years 2015 and 2016 saw the highest annual privatization revenues recorded until then. China is the global leader in privatization, followed by the UK in (a distant) second place.

[2] Several developed countries have privatized or attempted to privatize healthcare delivery. Germany has privatized more than a hundred hospitals since the 1990s (Heimeshoff, Schreyögg and Tiemann, 2014). England introduced new legislation in 2012 to encourage private delivery of healthcare (Goodair and Reeves, 2022). Sweden implemented a multi-phase, multi-sector privatization over 1990–2013 with mixed effects (Dahlgren, 2014). Madrid, Spain attempted to privatize hospitals, but the proposal had to be scrapped due to protests by unions. The provincial governments of Quebec and Ontario in Canada are reportedly considering privatizing hospitals. The city of Detroit privatized its public health department in 2012 but reversed course following the Covid pandemic.

cutting and multi-tasking seem plausible as responses that may hurt consumer interests.

Our empirical exercise focuses on a single hospital response with important welfare implications for consumers: whether privatized hospitals avoid admitting financially unattractive patients for inpatient care. Profit maximizing hospitals could resort to such cream skimming since they are expected to provide homogeneous quality of care to patients that pay dramatically different rates. Cooper et al. (2019) report that Medicare, the public insurance program for the elderly, pays hospitals 45% lower prices than private insurers on average. Medicaid, the public program for low-income individuals, pays still lower. Indeed, the average private hospital has a lower proportion of Medicaid, uninsured, and other unprofitable patients than the average public hospital (Horwitz, 2005; Horwitz and Nichols, 2022). Privatization could therefore restrict access to hospital care for unprofitable patients who are also typically the most vulnerable, even as it makes the sector more efficient overall (Birdsall and Nellis, 2003).

Apart from being conceptually suited to study the trade-offs in privatization, the US hospital sector is empirically very important. First, it is a prominent sector of the economy – the largest segment within healthcare with over a trillion dollars in annual spending and employs over 6.5 million people, comparable in size to the entire Construction sector.[3] Second, the government is substantially involved in delivering hospital services. Public hospitals accounted for 21% and 22% of all hospital beds and employment in 2019, respectively. In fact, hospitals employ more government employees than any other sector except education.[4] Third, there has been significant hospital privatization over the last few decades. In 1983, public hospitals accounted for 36% and 32% of total bed capacity and employment, respectively (see Figure 1a). The number of public hospitals has declined through two broad channels: conversion to private managerial control or privatization and closure.[5] Both types of transitions have occurred at greater rates for public

[3]Source: 2019 Quarterly Census of Employment and Wages – The Bureau of Labor Services (BLS). The Construction sector, NAICS code 23, in 2019 employed about 7 million individuals.

[4]Source: Current Employment Statistics – The Bureau of Labor Services (BLS). Hospitals are third behind government administration and education.

[5]When a hospital stops providing inpatient care, we consider that to be a closure even if it continues to provide outpatient care such as maintaining an Emergency Department or physician clinics. These latter cases are sometimes called partial closures.

hospitals than they have for their private counterparts. Privatization is the dominant mechanism by far: there are more than 6 privatizations for every public hospital closure during our sample period.

The decline of government involvement in hospital care has received surprisingly little attention. To our knowledge, only a handful of studies have systematically examined hospital privatization (Needleman et al., 1999; Ramamonjiarivelo et al., 2016, 2020). In contrast, changes in public health insurance during the same period have been extensively investigated (Cutler and Gruber 1996; Currie and Gruber 1996; Finkelstein 2007; Clemens and Gottlieb 2014; Miller, Johnson and Wherry 2019; among many others). There appears to be little consensus among policymakers on the appropriate role of the government in hospital care provision. Table 1 highlights the variation in public ownership of hospital bed capacity across the states. 49% of hospital beds are at public hospitals in Alabama, compared to only 7% in Pennsylvania.

We examine the effects of all privatizations of non-federal public hospitals that occurred between 2000 and 2018. Our main data source is annual hospital surveys administered by the American Hospital Association (AHA). We identify public hospitals that experienced a change in ownership or managerial control as reported in the surveys. We manually validated each purported privatization, and confirmed 257. We also source data on our key outcomes – patient volume and hospital employment – and hospital attributes from this proprietary data. We complement the AHA surveys with publicly available sources like the Medicare cost reports and the US Census.

We employ a staggered difference-in-difference research design to estimate the effects of privatizations on the treated hospital, as well as spillovers on the market where the hospital is located. This follows the approach used by recent studies examining changes in the organization of healthcare markets (Eliason et al. 2020, Craig, Grennan and Swanson 2021), as well as on privatization (Arnold, forthcoming). We compare outcomes at the treated hospitals (markets) following the privatization with other public hospitals (markets) that did not experience a change in ownership through the end of the sample period. While the design is standard in this literature, we recognize that privatizations are not exogenously assigned. We therefore take a number of precautions to probe the validity of our estimation strategy. We examine dynamic effects around the year of the privatization and find that the privatized hospitals do not differ from the comparison group prior to the change, but experience an immediate and persistent shift following the transition. We also subject the estimates to a number of sensitivity and robustness checks, including controlling for market-level indicators of economic activity, using a matched subset of comparison hospitals, and correcting for potential bias due to the staggered design. The estimates are stable and qualitatively similar in all cases.

We begin by examining the impact on the hospital's total inpatient admissions as well as on the number of patients covered by different payers. We find that total volume decreases at the privatized hospital by 8.5% and does not recover to pre-privatization levels even after five years.[6] This decline is driven by a 14% reduction in the number of Medicaid patients, while in comparison, we find only small and statistically insignificant effects on Medicare patients. A limitation of the data is that we cannot observe the number of uninsured patients directly since they are reported as part of a residual group that also contains privately insured patients. We find a decline in the volume of this composite group, however, this estimate is sensitive to the choice of specification.

Previous studies have documented spillover effects of hospital entry and exit on the profitability and patient mix of their competitors (David et al. 2014, Garthwaite, Gross and Notowidigdo 2018). Given the relatively large decline in patient volume at the privatized hospital, we expect spillover effects in our setting as well. If competing hospitals adequately expand their patient volume in response (particularly for Medicaid beneficiaries), we should not find an aggregate decline in patient volume at the market-level. On the other hand, since Medicaid pays below the average cost of care, neighboring hospitals may not expand their Medicaid volume (Schulman and Milstein, 2019). Furthermore, the privatized hospital may compete more aggressively and cause a change in its competitors' patient volume and treatment intensity even absent any reallocation of patients.

We study aggregate market-level effects on patient volume, assuming that a market is treated when a public hospital is first privatized.[7] Medicaid is the only payer for which we find a negative

[6]We did not find a simultaneous increase in outpatient or Emergency Department (ED) volume to suggest a change in treatment style. The coefficient on outpatient volume is noisy, but has a negative sign, implying a decline.

[7]We define hospital markets using Health Service Areas (HSAs). These are collections of contiguous counties that were delineated by the US Census to define self-contained hospital markets. There are approximately 900 HSAs in the

effect on volume at the market-level (about 3%). This effect is imprecisely estimated, but implies no offset of the decline in Medicaid volume at the privatized hospital.[8] We interpret this decline as a reduction in access to hospital care for Medicaid patients.

We test heterogeneity in the effect on aggregate Medicaid volume across markets on two dimensions with important distributional and policy implications. First, we hypothesize a greater decline in Medicaid volume in lower income markets since the average hospital in such markets is under greater financial stress and has more limited capacity to absorb unprofitable patients. Our results indicate that the decline in Medicaid volume is entirely driven by markets with greater poverty, where we find a statistically significant 9% decline. This is well beyond the 'direct' effect of the privatized hospital, implying that competing hospitals reduce their own Medicaid admissions in response. Second, consistent with the prediction by Shleifer (1998), we hypothesize that the reduction in access for Medicaid patients will be magnified in more concentrated markets since greater market power cushions incumbents from the consequences of reputation loss associated with changes in care. The data also supports this hypothesis: more concentrated markets experience not only a large decline in Medicaid, but also a sizeable decline in total volume.

Changing the payer mix toward more lucrative patients after privatization presumably increases hospital profitability. In addition, the new private operator can use innovative managerial practices to reduce operating costs. Personnel costs account for about half of total hospital operating costs according to the AHA data. In the short-term therefore, private operators may aim to improve margins by reducing labor inputs (Boycko, Shleifer and Vishny 1996, Needleman et al. 1997). Accordingly, we test whether private operators reduce the intensity of labor inputs, which we measure using full-time equivalent (FTE) staff per 100 adjusted admissions.[9] We find a decrease of about 0.66 FTE, or 8% of the mean. There is no decline in physicians and nurses, although

US. We confirm they accurately reflect patient hospital choice – more than 70% of Medicare fee-for-service patients choose a hospital located in their HSA. Since there are thrice as many HSAs as HRRs, using this market definition allows us to retain more identifying variation.

[8] Privatized hospitals account for about 20% of total Market capacity on average. Hence, a 14% decline in Medicaid volume at the privatized hospital alone would predict a 2.8% decline in market-level volume if there was no counteracting response by other hospitals.

[9] The AHA reports adjusted admissions, which are computed by adding inpatient and outpatient volume, weighted by the respective charges. Results are similar if we normalize by beds instead.

they account for nearly 30% of total staff. The decrease is concentrated within support functions, both clinical (eg., technicians) and non-clinical (eg., maintenance, administration) in nature.[10] We obtain imprecise estimates on labor intensity at the market-level, though we can reject spillover effects on competing hospitals. We detect greater reductions in labor intensity in markets with lower levels of unionization, suggesting that union activity prevents deeper employment cuts. Our data does not allow us to test whether the reduction in labor intensity compromises hospital quality of care, though we believe this is an important area for future research.

This paper contributes to several distinct literatures. First, and most generally, it relates to the literature examining the role of government and the effects of privatization. This literature has typically found that privatization improves the efficiency and growth of formerly public-owned firms across a wide range of countries, sectors, and modes of implementation (Boycko, Shleifer and Vishny, 1997; López-de Silanes, Shleifer and Vishny, 1997; Shleifer, 1998; La Porta and López-de Silanes, 1999; Megginson and Netter, 2001; Savas, 2005). However, as noted previously, there is a paucity of evidence on the corresponding effects for consumers and workers, particularly from the US. The evidence on hospital privatization appears to be even more scant. Ramamonjiarivelo et al. (2016, 2020) study privatizations over 1997–2013 and document improved hospital profitability. However, they do not examine market-level effects on access or costs or the effects on employment.

We also contribute to the literature examining ownership in healthcare. Since Arrow (1963), economists have been interested in the need for and differential performance of non-profit versus for-profit firms. This literature has yet to reach a consensus since some studies have found that non-profits respond similarly to financial incentives as for-profits, while others have found significant differences (Norton and Staiger 1994; Duggan 2000; Sloan et al. 2001; Picone et al. 2002; Gaynor and Vogt 2003, Malani, Philipson and David 2003, Sloan et al. 2003; Horwitz and Nichols 2009; among many others). Our setting allows us to provide novel evidence on whether the absence of a public option affects access to hospital care. We test and are unable to reject the null of no difference in effects between a for-profit or non-profit firm controlling the hospital.

[10]We tested the effect on the use of contract staff and can reject an increase of more than 0.02 FTE. Hence, the decline in labor intensity cannot be explained by greater reliance on outsourcing.

Our study also contributes to an emerging literature that has linked the costs of un-insurance to the economics of providers. Garthwaite, Gross and Notowidigdo (2018) show that hospitals – particularly non-profit hospitals – bear the cost of un-insurance in their market in the form of un-compensated care. Dunn, Knepper and Dauda (2021) and Duggan, Gupta and Jackson (2022) find that public hospitals received a differentially greater boost in revenue due to the ACA mandated Medicaid expansion because they served more uninsured patients at baseline. Dranove, Garthwaite and Ody (2022) argue that supplemental payments (such as the Disproportionate Share program) are valuable in filling financial gaps for hospitals with large shares of Medicaid and uninsured patients. Our results are consistent with this theme and suggests that expanding Medicaid coverage alone without maintaining public providers may not be sufficient to ensure access to hospital care for low-income patients.

The paper proceeds as follows. Section 2 provides the necessary background about public hospital ownership, related trends, and privatizations. We follow with a description of the data in Section 3, and our empirical strategy in Section 4. We present the main results on the effects on hospital utilization and access in Section 5, including an examination of aggregate effects at the market level. We similarly examine effects on employment in Section 6. Finally, Section 7 concludes.

2 Background

2.1 Hospital ownership

There is substantial heterogeneity in the ownership mix of hospitals across different geographies.[11] This is true not only of the share of publicly owned hospitals in a market, but also the type

[11] Hospital ownership and 'control' are related. The AHA survey reports hospital *control*, which could be recorded as one of non-profit, for-profit, or government. Control and ownership are identical except in the small number of cases where the owner outsources managerial control or leases the property to a contractor who happens to have a different organization structure. Such contractors are invariably private firms, hence there are some cases, as we shall discuss below, where the government *owns* the hospital, but it is *controlled* by a private firm. We will use the two terms interchangeably unless specified otherwise.

of privately owned hospital (non-profit or for-profit). Table 1 highlights this variation and presents the shares of bed capacity of four different owner types (public non-federal, public federal, private non-profit, and private for-profit) for a selected set of 6 large states with at least 100 hospitals in 2019 (AL, CA, TX, GA, IL, and PA). We also present the corresponding national means and standard deviations in column 7. The columns are ordered in descending order of non-federal public share of hospitals. For completeness, Appendix Table A.1 presents the corresponding values of non-federal share of bed capacity for all states. In these tables and throughout the paper we choose to focus on non-federal 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 (eg., native Americans).

We note two interesting patterns in hospital ownership. First, states vary tremendously in their reliance on public hospitals. Pennsylvania has only 4% of beds at such hospitals, while 44% of Alabama hospital beds are at state or local government hospitals. This variation is even greater if we consider small states (Wyoming and Vermont have 71% and 2%, respectively). Second, the share of public hospitals doesn't necessarily track states' preferences over the size of government. For example, Texas and Alabama have higher public hospital shares than do Illinois and Pennsylvania. Of the 10 states with the greatest public shares of hospital capacity, only one (Washington) was more liberal than the average in 2018, and three were among the most conservative (Alabama, Mississippi, and Louisiana).[12] We interpret this heterogeneity as a sign that there is little consensus across states on the appropriate scope for public delivery of hospital services.

2.2 Government and hospital care

Figure 1 presents national trends related to government involvement in hospital care over 1983–2019, all sourced from the American Hospital Association annual survey data. Panel (a) shows that the share of hospital beds at publicly owned hospitals (non-federal) declined from 27% in 1983 to 17% in 2019, a drop of nearly 40%. If we include in this calculation ownership by the federal

[12] Source: Gallup poll on political preferences, 2018. See https://news.gallup.com/poll/247016/conservatives-greatly-outnumber-liberals-states.aspx.

government, the share declined from 36% to 21%, more than a 40% decrease. There is a parallel, though slightly smaller, decline in the share of hospital employees working at public hospitals. Overall, public hospitals have consistently declined in importance over this period, though the decline was steeper in the 1980s and 1990s.

This pattern of declining public provision of hospital care is in stark contrast to the expansion of public insurance coverage for hospital care over the same period. Figure 1 panel (b) plots the trend in the share of patients covered by the two major public insurance programs at non-federal hospitals. Medicaid, the means-tested public insurance program *doubled* its share of hospital patients from 10% in 1983 to 22% in 2019. This is not surprising since Medicaid coverage has been expanded through several federal and state policy initiatives over this period, extending eligibility to an ever increasing share of the population. The share of Medicare – the public insurance program for the elderly – also increased from 32% to 45%.[13] Unlike Medicaid, eligibility for this program has been relatively stable and a large part of the increase is likely due to the ageing of the population. By the end of the sample period, the two public insurance plans collectively sponsored care for about *two-thirds* of all hospital patients, an increase of more than 60% relative to 1983.

The dramatic decline in government provision of hospital care is not part of a wider trend of a shrinking role in the provision of other services considered to be important for social welfare. For example, government provision of education services – a sector with similar market failures and policy considerations as healthcare – have remained remarkably stable over this same time frame. In contemporaneous time series for the share of high school and higher education (associate degree or higher) in public institutions, the share of students at public high schools has actually *increased* slightly, while the share of students at public degree granting institutions has decreased by 5%.

Historically, a key justification for public ownership of hospitals has been to provide last-resort access to necessary care for vulnerable individuals who may be shunned by private hospitals because they lack insurance coverage or are otherwise unprofitable. Perhaps state and local governments viewed the expansion of Medicaid coverage as an alternate means to ensuring access

[13] For all our analyses, Medicare includes patients on Traditional Medicare (TM) and Medicare Advantage (MA).

to care, making it easier to justify the privatization or divestiture of public hospitals. While the national time series on public hospital control and public insurance coverage of hospital patients are negatively correlated, we formally tested whether this correlation also exists at the state-level.

We estimate the association between state-level changes in Medicaid's share of hospital patients (ΔM_{st}) and the corresponding changes in the public non-federal share of hospital bed capacity (ΔP_{st}) over four periods – 1983–91, 1992–2000, 2001–09, and 2010–18 – using the following stacked model:

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

β is the coefficient of interest in this model and captures the within-state correlation between changes in Medicaid coverage and public hospital capacity. We weight each cell by the respective state population to account for the heterogeneity in size across states. We obtain a statistically significant estimate of -0.41 (0.11) for γ, implying that an increase in Medicaid share of 10 percentage points (pp) in a state is associated with a decline in public share of bed capacity in that state of about 4 pp. Recall that the national share of non-federal public hospitals dropped by about 10 pp over this period, hence this effect size is economically meaningful. We emphasize that this estimate represents a *correlation* and does not meet the bar for causality. However, it is consistent with the hypothesis that local and state government officials may view the expanded eligibility for Medicaid as an acceptable substitute for public hospitals to ensure access to care.

2.3 Privatization

The previous section documented the significant decline in public control of hospitals in the US. The reduction in the number of hospitals under government control occurred through two channels. More than 85% of the decline during the period we study was due to privatization – local governments relinquished operational control of the hospital to a private firm. The remainder occurred through outright closures of public hospitals or their conversion to solely providing outpatient care. During our sample period, we identified 257 and 41 cases of each, respectively.

In addition to manually validating all purported privatization deals, we also developed a taxonomy for types of privatization based on their key features. We did not have access to the contracts between the governments and the private firms and relied on press releases and independent reporting from the period around the transaction. Appendix Table A.2 presents the distribution of the different types of deals represented in our sample, and whether the new operator is organized as a for-profit or non-profit. As the table shows, privatization can manifest in several different forms and one could argue that every deal has some unique features. We find hospitals were brought under both non-profit and for-profit control, with the latter accounting for about a quarter of the deals.

The private firm's operational control over the hospital after the transition varies in a continuum across different types of deal structures, ranging from very limited control (short-term contracts) to complete control (ownership of all hospital assets). Appendix Section B.1 provides details on the different ways in which governments transfer hospital control. To simplify exposition, we group deals into two categories – about equal in size – with less and more private control. Following Hart, Shleifer and Vishny (1997), we don't rely on ownership of the assets alone to define control, rather we focus on operational control.

In the first group the government retains ownership of all assets, but outsources operational and managerial control to a private contractor. This structure was preferred to outright sales in some states (eg., Florida) because certain hospital sales required legislative approval, a lengthy and uncertain process (Needleman et al., 1997). The most common deal structure in this group was for the government to find a hospital management firm that would operate the hospital in return for a fixed monthly fee. We refer to this as 'contract management.' In another common approach, the government transfers operational control to a private firm specially incorporated to operate the hospital. The government agency continues to have governance oversight over the new entity. It is unclear how much incentive the private operators have to improve the hospital's profitability under these arrangements and whether they have the autonomy to focus on more profitable patients and services or cut staff. In general, we did not find language in the press reports suggesting the operators had constraints on their ability to make such changes, however we emphasize that we did not have access to the contracts.

Private operators enjoy substantially more operational control over the hospital in the second group of deals. This group has three types of deal structures. The first is an outright sale of all hospital assets to the contractor. We assume the new owners operate the hospital to maximize their own objectives, as they would any of their existing hospitals. The second approach is for the government to award a long-term lease (usually more than 15 years), giving the contractor more autonomy to make changes to the buildings and other assets as well as day-to-day operational control. A third, related approach that also involves a long-term transfer of control along with autonomy over the assets is for the contractor to enter in a joint venture with the government. Intuitively, we anticipate greater impacts of privatization in the second group of deals. We test this hypothesis when we examine the effects in Sections 5 and 6.

3 Data and descriptive evidence

3.1 Data sources and sample construction

Our main data source comprises annual surveys of hospitals from the American Hospital Association (AHA). Our primary analysis relies on AHA files for the years 1995–2019. We use the AHA files to source both our key outcomes variables (patient volume and employment), information on hospital attributes such as ownership (public or private), size, and location. As discussed above, we exclude federal hospitals from our main analyses since they are not funded by state or local governments and typically cater to a distinct set of patients (such as veterans or native Americans) rather than the local community at large. Our focus is on hospitals owned by a state, county, city, or by a hospital district.[14] A non-trivial number of government owned hospitals specialize in

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

psychiatric or rehabilitation care. In addition to being highly specialized, they are often reimbursed in a distinct way from community hospitals. We exclude these from our analysis sample as well and focus on non-federal general acute care hospitals.[15]

Since the treatment of interest is the privatization of publicly owned hospitals, we took several steps to minimize measurement error in identifying hospital ownership transitions. We started by inferring changes in owner type if the value reported on the AHA survey changes from one year to the next. This naive approach yielded a total of 353 privatizations of public hospitals over 2000–18. We manually validated these implied changes in ownership by examining the annual summary of change files from the AHA, news articles, press releases, hospital websites, and confirming the changes against proprietary databases such as the American Hospital Directory (AHD) which tracks hospital ownership over time. If we were not able to confirm a privatization, we assumed the hospital continued under public ownership in that instance. In several cases, the external data also helped us correct the year of privatization. Using this approach, we validated 257 privatizations, about 73% of the number implied by the raw data. Our analysis focuses on these transitions. The final sample contains public hospitals that were either treated (privatized) or that did not experience a change in ownership.[16]

The key outcomes are measures of patient volume and hospital employment. We study patient volume by payer and in aggregate. Specifically, we observe volume for three payers: Medicare, Medicaid, and a residual group ('Others') that is largely composed of privately insured and uninsured patients. A limitation of the AHA data is that we cannot separately observe volume for uninsured and privately insured patients.[17] We similarly examine total full-time equivalent (FTE)

[15] We identify general acute care hospitals using AHA's primary service code of 10, which are "general medical and surgical" hospitals. While it is rare for hospitals to switch service codes over the course of our sample period, we include all hospitals whose most common service code is general medical and surgical. The predominant service code among excluded public hospitals is psychiatric.

[16] We cannot rule out the possibility of false negatives – public hospitals that were privatized but this transition was not reported to the AHA. We believe this is very unlikely since it would mean the change is not reported over multiple years, not just a one-time event. We conducted random checks and did not find any. This measurement error, if it exists, will tend to bias effects toward zero. We also validated 46 transitions of privately owned hospitals to public ownership during this period. We excluded these hospitals from the sample.

[17] To our knowledge, there is no national data that can do better. It is possible to use uncompensated care costs reported in the Medicare cost reports to *impute* the share of uninsured patients, but this approach is feasible only after 2010.

employed staff and the effects on different components (physicians, nurses, administrative). We also rely on the Medicare cost reports to examine labor inputs since they provide more granular information than the AHA on this aspect. For example, we can observe the number of FTE contract staff at the hospital as well, allowing us to test whether the new management outsources staff. To circumvent potential bias due to the substantial skew in outcomes across hospitals of different sizes, we transform the variables. In the case of patient volume, we use logs rather than the level. In the case of labor inputs, we normalize by adjusted admissions, which include outpatient visits.[18]

Our final analysis sample is an unbalanced panel at the hospital-year level. Figure A.1 presents a frequency distribution of the number of years we observe hospitals in the sample. About 90% of the hospitals are observed for the maximum possible 25 years. The patterns are nearly identical for the privatized and comparison hospitals.

We supplement the AHA data with information on market-level attributes, such as county-level population, poverty, unemployment, and uninsurance rates from publicly available data sources like the US Census and the Bureau of Labor Services (BLS).

3.2 Descriptive evidence

Table 2 describes the hospital-level analysis sample. Across all columns, we present values from 1999, a year prior to the first privatization in our sample. Column 1 presents values for the 257 hospitals that would be privatized (treated) during the sample period. Column 2 describes the 766 remaining public hospitals that did not experience a change in ownership during this period and are located at least 15 miles away from any privatized hospital. This group comprises our primary comparison group. We imposed this distance requirement to mitigate the potential for spillover contamination.[19] Comparing values in these two columns reveals that privatized hospitals had

[18] Adjusted admissions are preferred over using inpatient volume alone since they also account for outpatient care which has rapidly grown over time. The AHA reports adjusted admissions and we use them directly. These are typically computed by scaling outpatient volume by the ratio of outpatient charges to inpatient charges (Schmitt, 2017). We also test sensitivity to normalizing by the contemporaneous number of hospital beds instead of by adjusted admissions. The results are qualitatively similar.

[19] This restriction drops only about xx potential control hospitals. The choice of 15 miles is somewhat arbitrary and trades off the need to isolate comparison hospitals from treated facilities against the desire to retain a larger share

about 20% fewer beds, but were otherwise very similar: both types admitted about 35 patients per bed per year and were largely reliant on public payers (about 65%). Privatized hospitals had about 14% lower labor intensity and operating expenses per adjusted admission at baseline, implying they were already leaner than the comparison group prior to the change in control.

Column 3 presents the corresponding statistics on the 4,392 privately owned hospitals in the data. On almost all measures, private hospitals were noticeably different than their public counterparts. For example, they operated at much greater scale with twice the number of beds as the treated hospitals and discharged more patients per bed (39 versus 35). Public payers accounted for a lower share of their patients (58%). They had similar labor intensity but higher operating costs per admission than the privatized hospitals, suggesting a different cost structure. Hence, private hospitals differ substantially from public hospitals on important operational dimensions and are unlikely to offer a suitable counterfactual to the privatized hospitals. Column 4 presents the corresponding statistics for all 5,415 hospitals in the sample. Since 80% of the hospitals are privately owned and they serve more patients, the aggregate statistics lean towards those for private hospitals.

Figure 2 describes the phenomenon of hospital privatization in the US over 2000–18. Panel (a) presents a heat map of the US based on the number of privatizations in the state. Privatization was widespread across the country with more than 40 states having at least one. States in the South and Midwest experienced the most number of privatization events during this period. Texas, Georgia, Louisiana, Indiana, and Minnesota are the five states with the most privatizations. Relative to the extant number of public hospitals, Louisiana and Indiana privatized a much greater share of their public hospitals than any other state. Panel (b) presents the number of privatizations in each year. There were at least 10 privatizations in each year from 2002 through 2017, suggesting that the estimated treatment effects will not be dominated by a specific sub-period. Similarly, no single year accounts for more than 8% of the total number of privatizations. The trend of privatization accelerated following the great recession – there were about 16 conversions per year in 2009–18

of potential comparison hospitals in the sample. We found that about 75% of Medicare patients over 2000–16 were treated at a hospital located within 15 miles of their home zip code, suggesting this is an appropriate threshold.

versus 12 per year over 2000–09.

4 Empirical Strategy

Our goal is to quantify the causal effects of privatization on public hospitals and on the markets they are located in. Our baseline models implement a staggered difference-in-differences (D-D) research design, following the recent literature on hospital ownership (Dafny et al., 2019; Craig et al., 2021). We study privatizations executed over 2000–2018 so we observe each treated hospital for 5 years before and at least 1 year following the privatization. Public hospitals that did not experience a change in ownership constitute the comparison group, and they offer an intuitive counterfactual for privatized hospitals.[20] Their performance trends are not contaminated by previous treatments, a complication with using already privatized (treated) hospitals as controls.

Equation 2 below presents our baseline model. Y_{ht} denotes the outcome of interest for hospital h in year t. We model the outcome as a function of hospital and year fixed effects, α_h and α_t, respectively. Recent studies of hospital closures have noted that markets experiencing closures had weak economic trends prior to the closures (Alexander and Richards, 2021; Chatterjee et al., 2022). Hence, we test sensitivity to including covariates X_{mt}, a vector of time-varying market attributes including population level, unemployment, poverty, and uninsurance rates for the county in which the treated hospital is located. We do not include time-varying hospital-level covariates (eg. bed capacity, services offered) in the models since most such attributes would plausibly be affected by the privatization. The key regressor of interest, D_{ht}, is a time-varying indicator variable that is equal to one starting in the year the hospital is privatized and zero otherwise. Finally, ϵ_{ht} denotes unobserved time varying factors. We cluster standard errors by hospital to account for the potential correlation of outcomes over time at the same hospital, which is the unit of treatment.

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

[20]Hospitals that exit are retained in the comparison group since this is a valid counterfactual to privatization.

While our approach is standard in this literature, we note that privatizations are not randomly assigned, nor are we aware of credible quasi-experimental instruments for changes in hospital ownership. Hence, one should interpret the coefficient of interest, β, with caution. However, our specifications control for the most important potential confounders. For example, hospital fixed effects eliminate persistent unobserved differences between hospitals (and the markets they belong to), an important source of selection. Under the assumption that the privatized and comparison hospitals would have evolved on parallel trends in the absence of the transaction, β recovers the average treatment effect on the treated hospitals. We assess dynamic effects on treated hospital outcomes around the year of the privatization by estimating the event study model in Equation 3 for each outcome.

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

A lack of differential trends in the years prior to the acquisition is consistent with the identifying assumption. Reassuringly, the evidence suggests relatively large changes in trends following privatization that cannot be explained by pre-trends, if any. We truncate the sample to 5 years before and after the year of privatization to focus on immediate changes in trajectory following the change in ownership. We also exclude the year of privatization (year zero) since it represents partial treatment. In our primary specifications, we estimate unweighted models, thus giving equal importance to all hospitals. Section 5.4 presents results from multiple checks where we assess robustness to using alternate modeling assumptions (including weighting) and specifications.

5 Privatization and hospital volume

5.1 Hospital-level effects

We begin by presenting the estimated effects on patient volume at the treated hospitals following the privatization. Table 3 presents the D-D coefficients obtained by estimating Equation 2 without including the covariate vector X (Panel A) and including controls at the county-year level for population, percent in poverty, percent unemployed, and percent uninsured (Panel B), respectively. We present the effects on total patient admissions as well as on the components 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 Other payers, which include private and uninsured patients.[21] Since the outcome in these models is log patient volume, we interpret the coefficients as approximately estimating the percent change in volume.

As the table shows, the estimates are very similar whether we include market-level covariates or not. This is reassuring since it mitigates the concern of model mis-specification and omitted variables like differences in the prevailing economic environment. We prefer to focus on the estimates obtained without including additional covariates as our primary results, hence throughout the text we will primarily discuss these estimates. Total patient admissions at the privatized hospital decline by about 8.5% following privatization. This estimate is statistically significant at the 1% level and suggests a substantial contraction of the hospital's patient care services. More importantly, the decline is not evenly felt by all patient groups. While Medicaid admissions decline by about 14.5%, Medicare admissions only decline by 5%, and are marginally significant. Finally, we find a 13.4% decrease in Other admissions, which includes uninsured admissions. Taken together, we infer that hospital privatization primarily affects non-Medicare patients.

Figure 3 presents the corresponding event study plots obtained by estimating Equation 3. The

[21] Medicaid and Medicare include those on managed care plans, e.g., Medicare Advantage. The 'Other' group is mostly composed of privately insured and self-pay patients. It also includes patients covered by small payers like government employee plans and workers' compensation. Unfortunately, the AHA survey does not provide a breakdown of Others.

figures show that, relative to the non-treated public hospitals, privatized hospitals were not trending differentially on these outcomes prior to the year of the transition. This is reassuring and supports the parallel trends identifying assumption. Further, the patterns are consistent with the coefficient magnitudes presented in Table 3. 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 admissions persists for at least the 5 years we follow. This pattern suggests the decline is not a transient phenomenon due to a one-off disruption in management. In contrast, there is little change in Medicare volume at privatized hospitals following the change (panel c).

We conducted an additional check to probe the effect on Medicare admissions using patient-level claims data on hospital utilization by Traditional Medicare (TM) patients, obtained from the Centers for Medicaid and Medicare Services (CMS). There are two noteworthy differences in this sample relative to our main sample drawn from the AHA. First, the volume observed in claims differs from the Medicare volume we observe in the AHA since it does not include utilization by Medicare beneficiaries on managed care, known as Medicare Advantage (MA). Second, the claims data spans 2000–2017 and we can therefore include slightly fewer privatizations in this analysis. We implement the same research design and present the results in Appendix Table A.3. We find a decline in TM admissions (Panel A row 1) that is not robust to using a matched sample (Panel B), nor to correcting for the staggered nature of the D-D, thus bolstering the conclusion that Medicare patient utilization is unaffected by privatization.[22]

Is the decline in patient volume due to lower bed occupancy or a broader decline in operational capacity? The former implies a reduction in operating efficiency – a surprising outcome of privatization – while the latter could reflect a strategic decision by the new management to improve finances. To answer this question, we consider the effect on total volume per bed, where beds are updated contemporaneously to account for changes in capacity. Appendix Figure A.2 presents the

[22]We also examine the effects separately for dual eligible and non-dual eligible TM patients and find a slightly larger decline among dual eligible patients in the baseline specification. However, the effects for both groups tend to be non-robust and are statistically indistinguishable.

dynamic effects on total patient volume per bed, obtained by estimating Equation 3. The figure shows a flat trend in total volume per bed following privatization, supporting the latter explanation. The corresponding D-D estimate is economically small and statistically insignificant: -0.3 patients per bed against a mean of 35, with a standard error of 0.73.

The decline in inpatient volume could be partially explained by a change in treatment style if hospitals prefer to treat more cases as outpatients following privatization. We test this conjecture and fail to detect an accompanying increase in outpatient care at the privatized hospitals. Appendix Table A.4 columns 1 and 2 present the corresponding effects on total Emergency Department (ED) and non-ED outpatient logged volumes, respectively. In both cases we find statistically insignificant and negative coefficients, implying, if anything, a decline in outpatient treatment. The coefficients are noisily estimated so we cannot rule out modest increases in outpatient volume. Appendix Figure A.3 panels (a) and (b) present the corresponding event study plots which support the interpretation of no changes in outpatient volume.

5.2 Privatization incentives and performance

Having discussed the average effects of privatization on hospital volume, we now briefly examine heterogeneity in treatment effects across deal types which plausibly confer different incentives or ability to maximize profits from the hospital.

In our setting, an important dimension of the deal is the extent of the private firm's control over the hospital. As we discussed in Section 2, in about half the deals the private firm has operational control over the hospital – either it owns the hospital or it has entered a long-term lease or joint venture to operate it. In the remaining deals, the firm is either on a short-term contract, acting under managerial supervision of the government, or the modality is undetermined. Hart, Shleifer and Vishny (1997) theorize that when the private firm has 'residual control rights' it has a greater incentive and ability to cut costs. In our setting, we assume the contractor has residual control rights in the first group of deals, while it lacks control or has less control in the second group of deals. We estimate a triple difference model, including an interaction of the deals with residual control with the post-deal indicator. To the extent there is measurement error in deal classification, it will bias us against finding distinctive effects.

Appendix Table A.6 presents the corresponding main DD and triple difference coefficients. Columns 1–4 present the effects on the same measures of patient volume as discussed previously. Panel A presents the baseline coefficients for ease of comparison. Panel B presents the triple difference results by residual control. The coefficients fit a pattern of greater reduction in volume in deals where the private firm has greater control. «Add more details once we have results from modified test»

The healthcare sector is somewhat unusual in the prominent role of non-profit firms. This has long interested economists and the differential behavior of non-profits versus for-profits has been consistently studied over the years. Economic theory predicts that for-profit contractors will cut costs more than non-profits will since their managers have high-powered financial incentives in comparison. For the same reason, for-profit firms will also deliver more growth. In our setting, this implies for-profit buyers will disproportionately reduce Medicaid volume relative to non-profit firms. However, the effect on total volume is unclear and depends on whether for-profits are able to sufficiently improve profit margins, for example, by increasing prices charged to privately insured patients. In this case they may increase total volume even as they cut Medicaid volume.

The triple difference coefficients testing this hypothesis are presented in Table A.6 Panel C. While some of the coefficients are imprecisely estimated, the pattern qualitatively supports the hypothesis stated above. We find for-profit buyers are able to increase total volume by 6.6%, contrasted against non-profit buyers who experience a 14.4% decline in total volume. The difference is statistically significant. In fact, for-profit buyers increase the volume of all payers except for Medicaid. The difference between the effect on Medicaid and total volume is greater in the case of for-profit buyers than in the case of non-profits. However, the absolute decline in Medicaid volume is lower in the case of for-profit buyers, suggesting their impact on access to care is smaller. Ultimately, due to the imprecision of the estimates, we do not emphasize these results.

5.3 Market-level effects

The results in the previous two sections showed that public hospitals persistently admit fewer patients following privatization, and the decline is uneven across patients covered by different payers. This naturally leads to the question whether privatization causes an aggregate decline in utilization at the market-level or are the patients served by neighboring hospitals instead? Medicaid patients appear to be one of the negatively affected groups. If these patients are perceived as unprofitable or undesirable, then alternate hospitals may be reluctant to step in as well. The implications for policymakers turn on the answer to this question. A reallocation to a different hospital could potentially be harmful if the new hospital is further away or of worse quality than the privatized hospital, but at the same time it may also be an improvement if the public hospital was of lower quality. However, a reduction in access to care implies Medicaid (and perhaps other) patients are unambiguously worse off following a privatization.

To shed light on this concern, we adapt our research design and implement it at the market-level, which we define using Health Service Areas (HSAs). These were originally delineated by the US Census in a similar fashion and for the same purpose as the more commonly used Hospital Referral Regions (HRRs), developed by the Dartmouth Atlas group. We prefer to use HSAs for two reasons. First, they are smaller in size – there are about 930 HSAs against 306 HRRs. The average HSA has about 6 hospitals (including both public and private owned), while the average HRR contains about 18. Hence, we will have greater statistical power to detect the market-level effects of a single hospital's privatization when we use a more granular market definition. At the same time, HSAs adequately capture patient hospital choice decisions.[23] Second, their borders follow county boundaries, while those of HRRs do not. This allows us to directly map the time-varying county-level characteristics to HSAs.

To implement our analysis at the market-level, we tag the 203 markets in which privatized hospitals are located as treated, while the 730 remaining markets form the comparison group.[24]

[23] Using Medicare claims data, we confirm that more than 70% of TM patients choose a hospital located in the same HSA as their residence zipcode. The corresponding number for HRRs is about 80%.

[24] We considered imposing a non-neighbor rule for comparison markets to mitigate the potential for spillovers. But

We then estimate an unweighted market-year level model equivalent to that presented in Equation 2. A market is considered treated when it first experiences a privatization during our sample period, and is then considered treated through the end of the sample (42 of the 203 markets experienced more than one privatization event). Table 4 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 has 6 hospitals, out of which 1.3 or 21% are treated during the period. Market-level bed counts, payer mix, and economic indicators are as one would expect based on the hospital-level averages. Comparison markets are slightly smaller in size and have slightly better economic indicators on average (eg., lower poverty and unemployment).

Table 5 presents the estimated effects on market-level patient volume, with log of volume as the outcome. The columns present effects on total volume and by payer, respectively. Panels A and B present the average effects from specifications without and with the time-varying controls, respectively. Including market controls tends to magnify the point estimates but leads to similar interpretations, hence we focus on the estimates without controls. Column 1 presents estimates on total volume and reports a 0.7 percentage point (pp) decline in admissions across all hospitals in the market. The direct effect at the treated hospitals was an 8.4% decline in admissions. Since treated hospitals are about 20% of total market capacity on average, we would expect a 1.7 pp decrease in total admissions at the market level based on the direct effect alone (20% of 8.4 pp). Hence, the point estimate we obtain suggests the presence of some offsetting responses by other hospitals in the same market. However, we are under-powered to statistically detect an effect of this magnitude at conventional levels of significance, nor can we reject the hypothesis that our estimate differs from 1.7 pp.

The key finding is that Medicaid is the only payer for which we estimate a negative effect on volume at the market-level. While the point estimates for Medicare and Others are positive and close to zero, the effect on Medicaid is -3 pp – approximately what we would predict based

such a rule would nearly eliminate all potential untreated markets in the same states as the treated markets. It was unappealing to have the comparison group be restricted to an almost disjoint set of states.

on the privatized hospital's decline alone (20% of -14 pp, or -2.8pp). Hence, the point estimate suggests no offsetting responses by other local hospitals for Medicaid patients. The coefficient is noisily estimated so we cannot reject the null hypothesis of no decline in Medicaid volume, although it is larger and significant at the 10% level when we include controls. Figure 4 presents the corresponding event study plots for these outcomes. The estimated dynamic effects are consistent with the coefficients discussed above. Medicaid is the only payer for which the trend appears to be consistently negative following privatization.

Heterogeneity

The average effect across all markets may mask heterogeneity in treatment effects across different types of markets, and the possibility that some markets may experience larger effects. This has implications for policy since we may want to avoid privatization in certain types of markets if they are likely to have significant undesirable effects for consumers. We draw on the institutional setting of healthcare markets and predictions from the privatization literature to guide our investigation of heterogeneous effects.

We first explore potential heterogeneity across markets with greater poverty since hospitals in markets with greater poverty levels are likely to have fewer resources to offset the decline in patient volume at the privatized hospital. Disparities in payer mix and profitability are evident between hospitals in markets with low versus high poverty rates. Medicaid contributed 18% of patients for hospitals in 1999 in markets with poverty greater than median, while the corresponding figure was 13% at hospitals in markets with poverty lower than the median. The average hospital operating margin in markets with above median poverty rate was 0.4%, less than half that in markets with poverty below the median (1%). We hypothesize that the aggregate decline in patient volume will therefore be larger in markets with greater poverty rates. In fact, since the competing hospitals may be losing much-needed privately insured patients to the newly privatized hospital, they may reduce their own Medicaid admissions in response. Hence, the aggregate impact on Medicaid volume at the market-level may even exceed what one would predict based on the direct effect of privatization. To test whether the impacts differ in markets with greater poverty, we estimate a triple difference specification including an additional term interacting the privatization indicator with an indicator for being above the median poverty rate.

Table 5 Panel C presents the estimated coefficients of interest from the triple difference model without including covariates. The results clarify that privatizations barely register in markets with below-median poverty rates. All D-D coefficients, which estimate the effects for low poverty markets, are positive, small, and statistically insignificant. In contrast, markets with greater poverty rates experience an aggregate decline in patient volume of 2.3 pp (0.9 - 3.2 = -2.3), driven mostly by a large and statistically significant decline in Medicaid volume of 9.4 pp (3.3 - 12.7 = -9.4). In results not presented here we confirmed that privatized hospitals downsize in both types of markets. The point estimate for the effect on hospital-level volume is not statistically distinguishable in the two types of markets. The contrasting effects on Medicaid volume in these two groups of markets suggests that neighboring hospitals in wealthier markets are able to offset the decline of hospital operations following privatization. However, not only does this offsetting mechanism not operate in lower income markets, but also the neighboring hospitals appear to reduce their own intake of Medicaid patients.

Shleifer (1998) hypothesizes that privatization will have less beneficial effects in more concentrated markets since consumers have fewer outside options and therefore market forces cannot discipline the new managers at these firms. This is a highly pertinent issue in the case of hospitals since local hospital markets are concentrated on average – the mean Herfindahl Hirschman Index (HHI) in 2000 was nearly 3,000, well over the federal government's threshold for being "highly concentrated" (DOJ, 2010). The mean HHI further increased to about 4,000 by 2020.[25] We test this hypothesis in our setting by examining if the negative effect on utilization is greater in more concentrated markets. We estimate triple difference models where we include an interaction term between treatment and a concentration level greater than the median in 1999. We note that highly

[25] We computed these HHI values using hospital bed shares recorded in the AHA and hospital referral regions to define hospital markets. Since HSAs are smaller and have fewer hospitals, the mean HHI would be greater if we used HSAs to define hospital markets.

concentrated markets do partially overlap with high poverty markets (54 markets are above-median on both dimensions), but overall, the two groups appear quite different. Concentrated markets are not as economically disadvantaged, having similar poverty, Medicaid, and uninsurance levels as the average treated market. Concentrated markets are also smaller and have half the number of hospitals as the average treated market. Hence, the remaining hospitals are more 'exposed' to privatization than in markets with greater poverty. We therefore expect greater aggregate effects in concentrated markets.

Table 5 Panel D presents the corresponding results from the triple difference model. The results imply that the effects of privatization are diametrically opposed in markets with low versus high concentration. Utilization does not decline in competitive markets, and even increases a bit, though even in this case there is no increase for Medicaid patients. There is a sharp decline in utilization in concentrated markets, with a 6 pp decline in aggregate volume (4.5-10.5=-6), nearly three times that in high-poverty markets. However, in this case, the decline in utilization appears to be more widespread and affects all patients regardless of payer.

5.4 Robustness

We test the robustness of the main results to different modeling assumptions and important validity concerns. Table 9 presents the corresponding results for both hospital- and market-level volume in columns 1–4 and 5–8, respectively. Panel A presents the baseline estimates, without including time-varying covariates, for ease of comparison. Across all checks, the models do not include market-level covariates.

Panel B presents the coefficients obtained from regressions incorporating hospital bed capacity as weights.[26] This approach gives more weight to the changes at larger privatized hospitals. The estimates remain very similar in magnitude and statistical significance in the case of the hospital-level effects. At the market-level, giving more importance to larger markets results in a positive

[26]For this exercise, we hold bed capacity fixed. For treated hospitals we use the mean of pre-period beds, i.e., the mean of beds in the five years prior to privatization. For control hospitals we use the number of beds in 1999 or if the hospital was not in the sample in 1999 (rare), the first year we observe that hospital.

overall effect. This implies that the decline in Medicaid is greater in smaller markets.

Panel C tests whether the estimates are robust to allowing the privatized hospitals (markets) to progress on a differential linear trend. We estimate models including a linear trend interacted with an indicator for the treated units. Both sets of estimates remain qualitatively similar. The hospital-level results suggest that the estimated decline in Other patients is not robust to this change.

The recent econometric literature on differences-in-differences has shown that estimates obtained from staggered treatment designs may suffer from biases due to the use of treated groups as controls for future treated units. To assess the importance of this potential threat, we report coefficients from the estimator proposed by Callaway and Sant'Anna (2020) which corrects for staggered designs and computes the weighted average treatment on the treated. Panel D presents the corresponding coefficients which are remarkably similar to the baseline estimates.

The last two panels present results from the baseline specification estimated on two different sub-samples. Our main analysis sample allows an unbalanced panel in treatment time for the privatized hospitals. That is, while we are able to follow some hospitals for 5 years following privatization, we can follow others for as little as 1 year. We assess the importance of this imbalance by imposing the restriction that we should be able to follow all privatized units for 5 years. The results remain qualitatively similar. The only noteworthy point is that market-level effects on Medicare and Other volume become more positive.

Finally, we implement propensity score matching to identify a subset of the comparison hospital group that resembles the privatized hospitals on key attributes like bed capacity and patient volume in the years just prior to the transition. We use matching to identify a single comparison hospital (market) for each treated hospital (market) without replacement, and then estimate our baseline D-D specification without market covariates. We limit the data to years -5 through +5 around the year of privatization for both treated and control units. Appendix C.2 describes the matching exercise in more detail. Panel F presents the corresponding coefficients, which suggest slightly smaller effects on patient volume, except in the case of Medicaid. Appendix Figure A.4 presents the corresponding event studies on patient volume. Reassuringly, the patterns are qualitatively very similar to those obtained using the full sample.

6 Effects on operating costs

Privatization is likely to affect hospital operations along multiple dimensions. Studies have documented across a wide range of settings that the new private management usually reduces operating costs, and specifically reduces labor inputs and associated spending (Megginson and Netter, 2001). Table 2 shows that privatized hospitals already had substantially lower operating costs than the comparison group and private hospitals at baseline. Hence, in this setting there may not be much scope for further reducing costs. We examined the effect on total operating costs and could not reject the null hypothesis of no change. Since personnel spending accounts for about half of total operating costs, the contractor may reduce labor inputs to impact costs immediately. Since we find a reduction in hospital volume due to privatization, we examine the effect on labor intensity (i.e., FTE per 100 adjusted admissions) rather than on the level of labor inputs.

6.1 Direct effects

Table 7 presents the estimated effects on employed and contracted full-time equivalent (FTE) staff per 100 admissions. We find an economically meaningful reduction in total employment following privatization of 0.66 FTE per 100 admissions (Col. 1). Compared to the pre-privatization mean, this implies a decrease of 9% in labor intensity. Although nurses account for 27% of total staff, we do not detect any reduction in nurse intensity. The reduction is driven mainly by the residual group, referred here as 'Others.'[27] This group is disproportionately affected since it accounts for 70% of total FTE but contributes over 90% of the total reduction in labor intensity. This is a diverse group and includes patient care (eg., technicians), back office or overhead (eg., accounting), and managerial functions (eg., administrators). While a smaller component of the aggregate effect, the estimate on physicians suggests an economically meaningful reduction of 30% compared with

[27]The figures here only account for employed physicians, such as hospitalists. However, for much of the sample period, hospitals typically did not employ physicians directly and this explains the low number of employed physicians.

the pre-privatization mean.

We also test whether the decline in employed staff is partially offset by an increase in the use of contract labor following privatization. This is crucial since it affects how we interpret the decline in employment discussed above. If the decline in employment is partly or fully offset by an increase in contract labor, it implies that patient care is likely not affected, and the new management is just changing how it contracts with staff. However, the result in column 6 is near zero and statistically insignificant. We can rule out an increase in contract staff of more than 0.02 FTE per 100 cases, which would offset less than 2% of the decline in employment. We therefore interpret the results as implying a real reduction in labor intensity at the hospital. The average treated hospital had about 6,800 adjusted admissions per year at baseline, implying that privatization allowed the average hospital to serve the same number of patients with 45 fewer FTE (6800 x 0.66 / 100 = 44.9).

Figure 5 presents the event study plots corresponding to each of these labor variables. The dynamic coefficients are consistent with the D-D estimates presented in Table 7. There is a noticeable decline in physician, others, and overhead FTEs per bed in the year following privatization, and it persists or in some cases increases over the next 5 years. Appendix Table A.5 presents the corresponding estimates obtained using FTE normalized by contemporaneous beds instead. The results are qualitatively similar, largely driven by the other group, and suggest a slightly smaller decline in labor intensity than the estimates discussed above (6% vs. 9%). Appendix Figure A.6 presents the corresponding event study plots, which are consistent with the point estimates.

«Discuss heterogeneity by extent of control and for-profit vs. non-profit»

6.2 Market-level effects

Previous studies on privatization have found spillover effects of privatization on market-level wages. Arnold (forthcoming) studies privatization in Brazil and finds substantial spillover effects on mean wages at exposed firms in the market. The aggregate decline in wages is nearly three times what would be predicted based on the effect on the privatized firm alone. In our setting, however, public hospitals did not pay higher wages than their private counterparts at baseline, and hence there appears less room for a decline in wages following privatization. As discussed earlier, public hospitals also had fewer employed staff per patient, hence further reductions may not impact the employment decisions of competing hospitals in the same market.

Table 8 presents the corresponding effects on market-level hospital labor intensity obtained by applying our research design. Panels A and B present the estimates without and with including market covariates, respectively. The columns present effects on total FTE as well as on the same components studied in Table 7. The coefficients are small and statistically insignificant across all columns. We can reject a change of more than 0.18 FTE per patient at the market-level. This range includes the effect we would expect based on the direct effect alone (20% of 0.66 = 0.13 FTE).

Again, the average effect on labor intensity may mask heterogeneous effects across different types of markets. Previous studies have noted the influential role of unions in preventing employment losses following privatization «cites». Some have noted the importance of powerful unions in preventing privatization in the first place (Shleifer, 1998). We test the hypothesis that the contractor is able to implement greater cuts in markets where unions are less influential. We measure union prevalence using county-year level data on the share of employees in the private services sector. We assign hospital with below median values of this measure (about 12%) in 1999 as counties with weak union presence. We then implement a triple difference specification testing for differential effects in markets with weaker unions. Table 8 Panel C presents the corresponding results. Consistent with the hypothesis that unions may prevent large employment cuts, we find net negative effects on labor intensity at the market-level in markets with weaker unions (Col. 1). The net decline is what we would expect based on the changes at the privatized hospital alone and discussed above. However, the coefficient is not precisely estimated, perhaps due to the underlying measurement error in the union prevalence variable. In other words, the employment cuts are detectable at the aggregate level only in markets with low level of union presence. The results across different sub-groups are consistent with the patterns seen previously at the hospital-level.

7 Conclusion

This paper studies the privatizations of public hospitals in the US over 2000–2018. Our main finding is that hospitals downsize operations and reduce patient volume after transferring to private control. However, the decline in utilization is not evenly spread across patients. The decrease in the number of Medicaid patients is nearly twice as large as the total decline in patients. This raises the question whether patients experience a decline in access to care following privatizations, or are reallocated to other hospitals in the market. We find a decline in aggregate Medicaid volume at the market-level which is greater in markets with higher poverty rates or greater concentration levels. The aggregate decline in these markets is well beyond what would be predicted by the direct effect on the privatized hospital alone, implying spillover responses by competing hospitals. The new private hospital operators also reduce the intensity of labor inputs, particularly in administrative and support functions, suggesting greater efficiency. We also detect a decline in aggregate hospital labor intensity at the market-level in markets with low levels of unionization.

There are several avenues for future research. Since we wanted to characterize the effects of privatizations nationally, we were limited to using hospital-level data. The lack of granular data imposes several limitations. We cannot describe whether hospital utilization declined to a greater extent for certain services (eg., unprofitable services like psychiatric care). Researchers with access to patient-level data, perhaps focused on narrower geographies, can study whether the changes in service led to adverse effects on public health. Along similar lines, future studies can use more granular employment data to examine labor and wage dynamics for workers in the local hospital market. Understanding these aspects will be key to comprehensively quantify the welfare effects of privatization and inform policy interventions.

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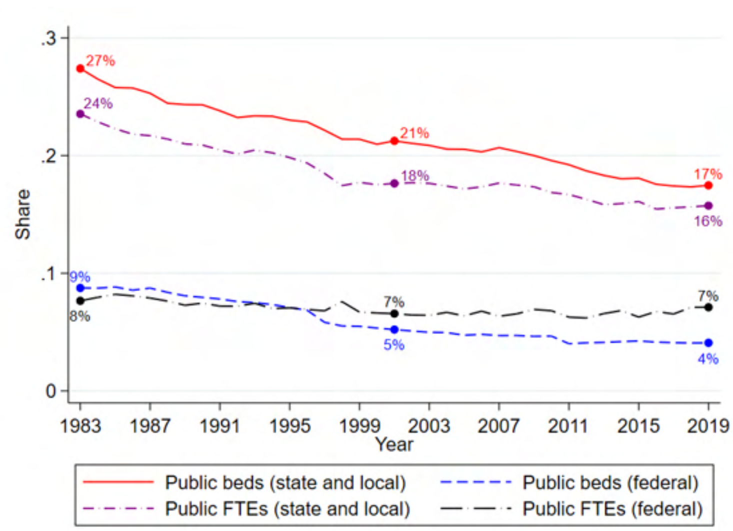
(a) As provider



(b) As payer

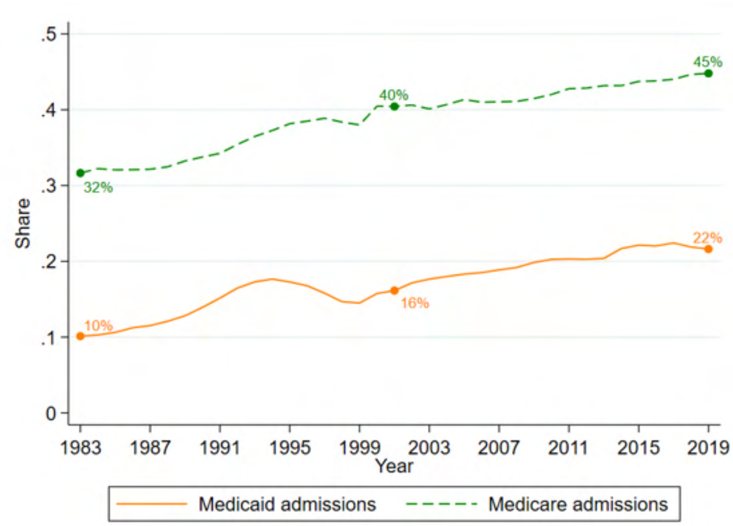
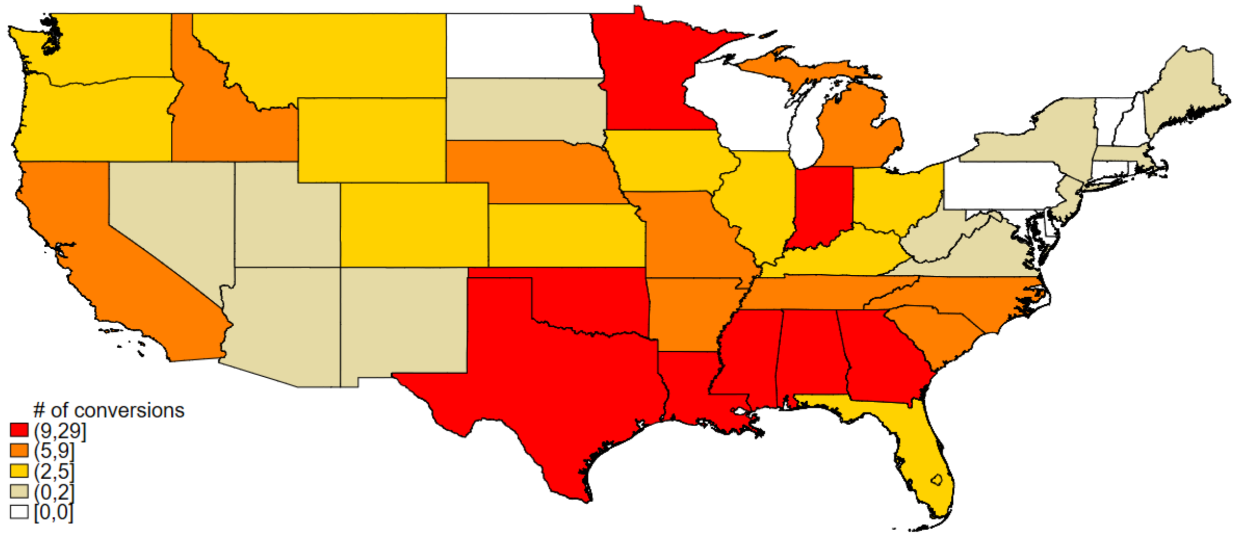


Figure 1: Government role in hospital care

Note: The figure presents overall shares in the US from 1983 through 2019 using American Hospital Association (AHA) survey data. In Panel A we plot the share of total beds contributed by public, non-federal hospitals with a red, solid line and the share of public, federal hospitals with a blue, dashed line. 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 non-federal hospitals present in the survey in each year.

(a) By state



(b) By year

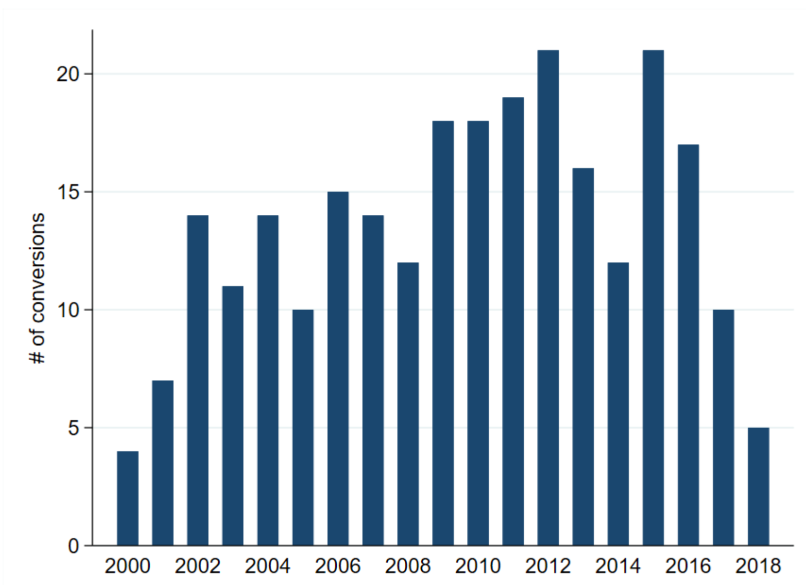
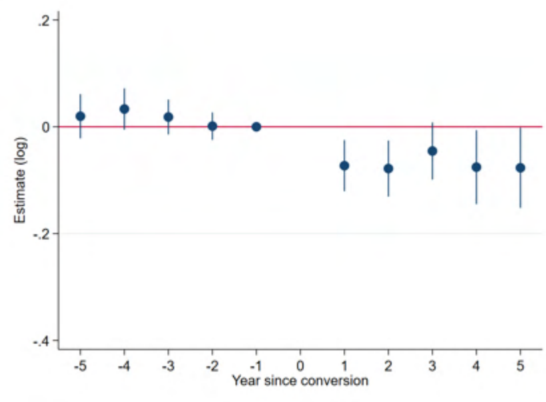


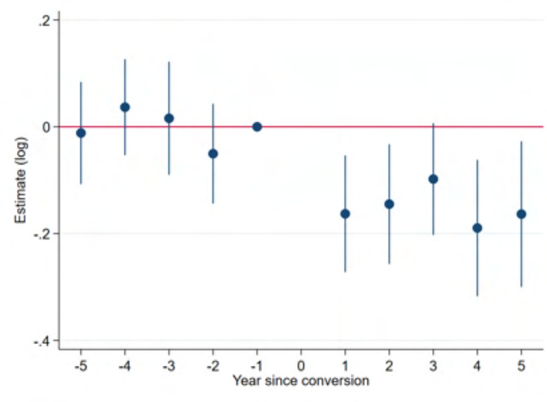
Figure 2: Privatizations

Note: The figure presents the distribution of non-federal public hospital privatizations during our sample period (2000–18). Panels (a) and (b) present the distribution by state and by year, respectively. Hawaii and Alaska are not pictured and include 4 and 1 conversions, respectively. We manually validated each conversion.

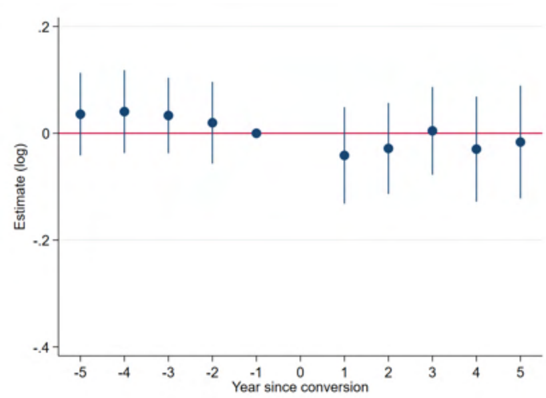
(a) Total admissions



(b) Medicaid



(c) Medicare



(d) Other

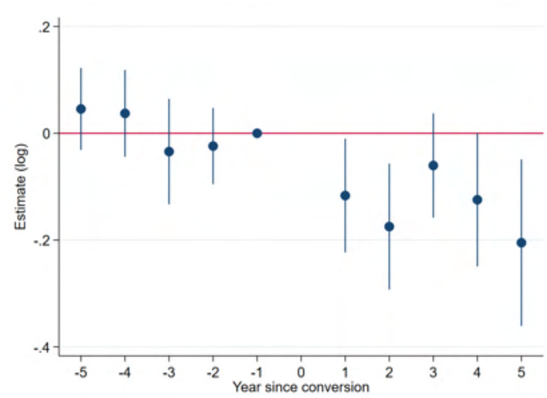


Figure 3: Effects on patient volume

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period and are not located within 15 miles of any treated hospital. The outcomes are logged total patient volume, Medicaid, Medicare, and other volume in panels (a), (b), (c), and (d), respectively. 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.

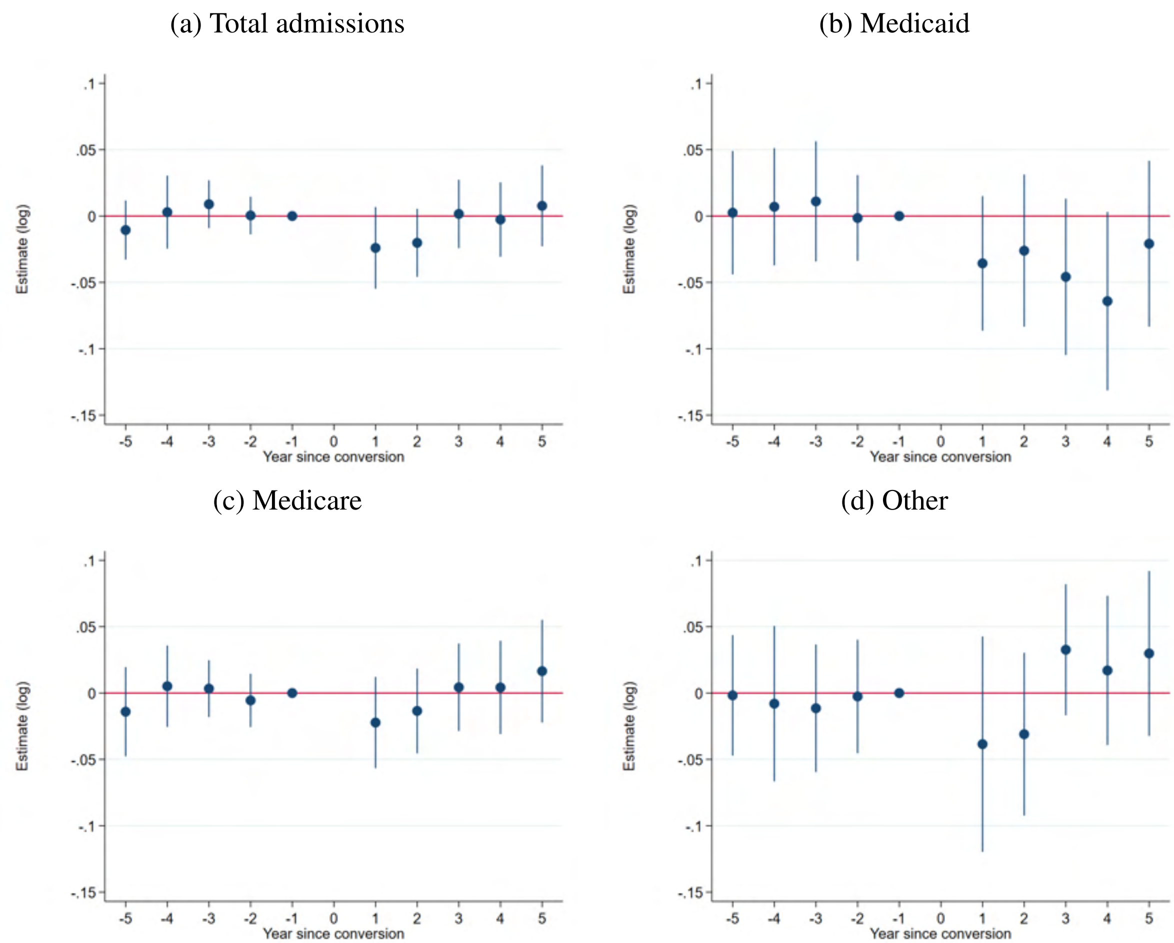
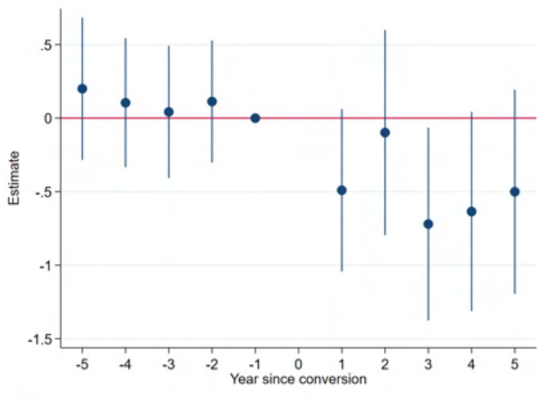


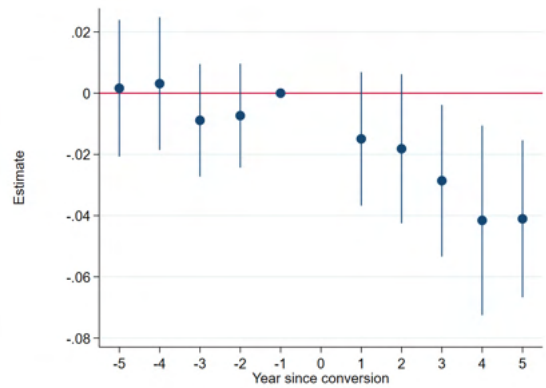
Figure 4: Effects on market volume

Note: The figure presents event study plots obtained by estimating the market-level equivalent of Equation 3 on market-year level data. We define hospital markets using Health Service Areas (HSAs), as described in Section 5.3. The outcomes are as indicated in the figure and are logged. Year zero is the year a market first experiences a privatization and is excluded from the data for treated markets since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by HSA.

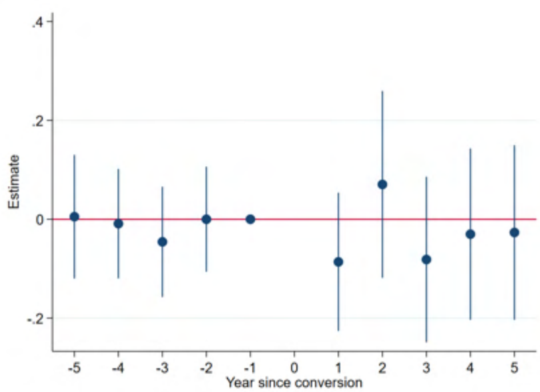
(a) Total FTEs



(b) Physician FTEs



(c) Nurse FTEs



(d) Other FTEs

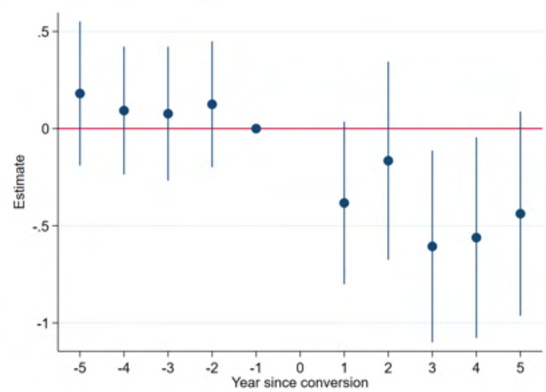


Figure 5: Effects on staff (per 100 adjusted admissions)

Note: The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period and are not located within 15 miles of any treated hospital. Outcomes from the AHA are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other FTEs in panels (a), (b), (c), and (d), respectively. Outcomes from HCRIS are overhead FTEs and contract FTEs in panels (e) and (f), respectively. We normalize the staff levels in each column by contemporaneous, adjusted admissions, which scales admissions by the ratio of outpatient to inpatient revenue. Year zero is the year of privatization and is excluded for the treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital.

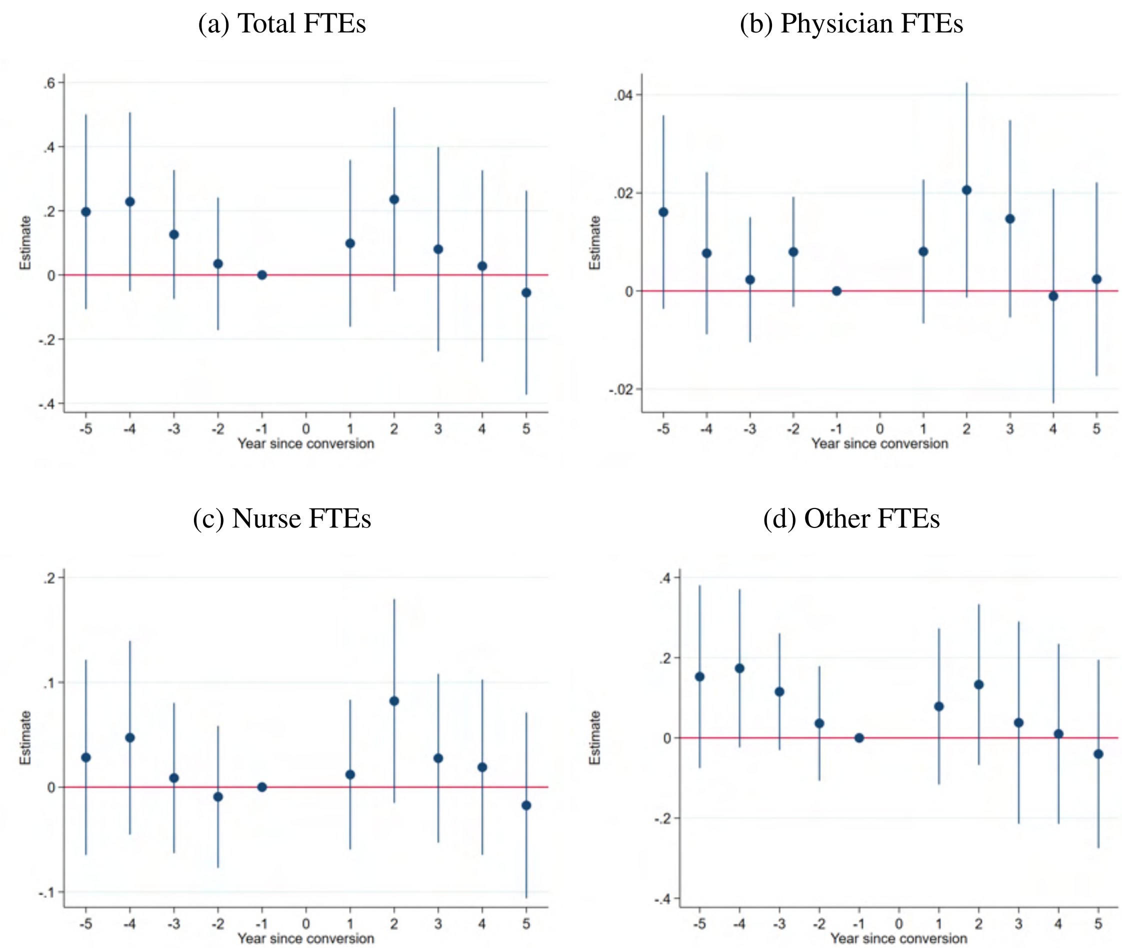


Figure 6: Effects on market staff (per 100 adjusted admissions)

Note: The figure presents event study plots obtained by estimating the market-level equivalent of Equation 3 on market-year level data. We define hospital markets using Health Service Areas (HSAs), as described in Section 5.3. The outcomes are as indicated in the figure and are normalized by 100 adjusted admissions. Year zero is the year a market first experiences a privatization and is excluded from the data for treated markets since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by HSA.

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

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

Notes: The table presents shares of hospital beds by ownership type for select states using American Hospital Association survey data from 2019. Appendix A.1 lists public (non-federal) hospital bed shares for all states. Column 7 shows mean shares for the overall US; standard deviations are shown in parentheses.

Table 2: Descriptive statistics

	(1) Privatized	(2) Remaining Public	(3) Private	(4) All
% public	100.0	100.0	0.0	21.3
% for-profit	0.0	0.0	21.1	16.6
% non-profit	0.0	0.0	78.9	62.1
Admissions	3,095	4,154	7,461	6,703
	(4,404)	(6,917)	(7,702)	(7,587)
Beds	93	120	186	171
	(105)	(162)	(179)	(176)
% Medicaid adm	15.4	16.6	13.0	13.7
	(8.6)	(12.4)	(8.8)	(9.5)
% Medicare adm	49.2	47.4	44.5	45.2
	(15.6)	(16.8)	(13.1)	(13.9)
% other adm	35.4	36.1	42.4	41.0
	(14.4)	(14.0)	(13.9)	(14.2)
Total FTEs/100 adj adm	8.1	9.7	8.0	8.3
	(9.2)	(9.8)	(15.1)	(14.1)
Total expenses/adj adm	7,685	9,460	9,752	9,598
	(7,175)	(9,547)	(16,389)	(15,132)
# hospitals	258	802	3,925	4,985

Notes: The table presents descriptive statistics on a cross-section of hospitals in the analysis sample. We use values from 1999 for most hospitals. In rare instances in which we do not observe a hospital in 1999, we use values from that hospital's first year in the data. Appendix C.1 describes the sample construction restrictions in detail. Column 1 describes the public hospitals privatized during the sample period. These comprise the treated units. Column 2 describes the primary comparison group: public hospitals that did not experience a change in ownership during this period and are more than 15 miles away from all treated hospitals. Column 3 describes all privately owned non-profit and for-profit hospitals that were not converted to public ownership during this period. Column 4 presents the corresponding values on the full sample. 'Other' admissions refers to hospital admissions not covered by Medicaid or Medicare and mostly comprises privately insured and uninsured patients. Standard deviations are shown in parentheses.

Table 3: Effects on patient (log) volume

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: No controls				
DD	-.084	-.149	-.049	-.138
	(.027)	(.042)	(.030)	(.043)
Obs	20,998			
B: Market controls				
DD	-.090	-.157	-.056	-.142
	(.027)	(.042)	(.030)	(.043)
Obs	19,385			
Mean outcome (t-1)	3,014	617	1,351	1,046

Notes: The table presents estimated effects on patient volume at the privatized hospitals obtained by estimating Equation 2 on hospital-year level data. Columns 1, 2, 3, and 4 present the effects on total, Medicaid, Medicare, and the remaining ('Other') admissions, respectively. In all cases outcomes are logged. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to patient volume at privatized hospitals in the year prior to privatization. Standard errors are clustered by hospital and are presented in parentheses.

Table 4: Descriptive statistics (market-level)

	(1) Treated HSAs	(2) Control HSAs	(3) Total
# treated hospitals	1.3	0.0	0.3
	(0.6)	(0.0)	(0.6)
Total hospitals	5.4	4.3	4.6
	(5.1)	(6.4)	(6.1)
Total admissions	34,152	30,927	31,650
	(54,842)	(79,635)	(74,780)
Total beds	875	778	800
	(1,313)	(1,902)	(1,786)
% Medicaid adm	15.4	13.9	14.2
	(6.3)	(7.1)	(6.9)
% Medicare adm	45.2	47.1	46.7
	(10.2)	(9.6)	(9.8)
% other adm	39.3	39.0	39.1
	(11.5)	(10.0)	(10.4)
Total FTEs/100 adj adm	7.1	7.2	7.2
	(2.2)	(3.4)	(3.2)
% in poverty	14.1	12.9	13.2
	(5.0)	(4.7)	(4.8)
% unemployment	4.9	4.7	4.8
	(2.3)	(2.4)	(2.4)
% uninsurance	20.6	19.0	19.3
	(6.0)	(5.6)	(5.7)
HHI (admissions)	4,981	5,814	5,627
	(2,607)	(2,930)	(2,881)
# HSAs	204	706	910

Notes: The table presents descriptive statistics for the market-level sample, where markets are defined by Health Service Areas (HSAs). Treated HSAs have at least one hospital that undergoes public to private conversion during 2000–18. Control HSAs do not have any conversions during our sample period. All rows present means and standard deviations (in parentheses).

Table 5: Effects on aggregate patient (log) volume

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other
A: No controls				
DD	-.009	-.042	-.001	.004
	(.014)	(.026)	(.016)	(.022)
Obs	19,404			
B: Market controls				
DD	-.021	-.051	-.016	-.008
	(.014)	(.026)	(.015)	(.022)
Obs	17,983			
C: Heterogeneity by market poverty				
DD	.016	.032	.022	.019
	(.019)	(.031)	(.019)	(.027)
x 1(> med. poverty)	-.050	-.148	-.045	-.029
	(.027)	(.048)	(.029)	(.042)
D: Heterogeneity by market HHI				
DD	.043	.034	.042	.068
	(.016)	(.024)	(.017)	(.019)
x 1(> med. HHI)	-.105	-.154	-.088	-.128
	(.026)	(.048)	(.029)	(.041)
Mean outcome (t-1)	36,550	6,796	15,282	14,473

Notes: The table presents estimated effects on patient volume at the market-level obtained by estimating the market-level equivalent of Equation 2 on market-year data. We define markets using Health Service Areas (HSAs), as described in Section 5.3. Columns 1, 2, 3, and 4 present the effects on total, Medicaid, Medicare, and the remaining ('Other') admissions, respectively. In all cases outcomes are logged. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying HSA-level controls: population, unemployment, uninsurance, and poverty rates. Panel B has fewer observations since the covariates are not available for 1995 and 1996. Panel C presents the corresponding results from a triple difference specification including an interaction term with an indicator for the market having a poverty rate in 1999 greater than the median among treated markets. Panel D is analogous to panel C but instead includes an interaction term with an indicator for the market having a Herfindahl-Hirschman Index (based on admission shares) in 1999 greater than the median. The mean values pertain to patient volume in the treated markets in the year prior to privatization. Standard errors are clustered by HSA and are presented in parentheses.

Table 6: Patient volume robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hospital				Market			
	Total	Medicaid	Medicare	Other	Total	Medicaid	Medicare	Other
A. Baseline	-0.084	-0.149	-0.049	-0.138	-0.009	-0.042	-0.001	0.004
	(0.027)	(0.042)	(0.030)	(0.043)	(0.014)	(0.026)	(0.016)	(0.022)
B. Weighted by beds	-0.091	-0.165	-0.080	-0.111	0.026	0.0003	0.030	0.051
	(0.029)	(0.044)	(0.034)	(0.048)	(0.010)	(0.0185)	(0.012)	(0.014)
C. Treated group trend	-0.061	-0.119	-0.034	-0.062	-0.037	-0.035	-0.033	-0.051
	(0.030)	(0.058)	(0.047)	(0.066)	(0.016)	(0.031)	(0.020)	(0.040)
D. CS estimator	-0.064	-0.146	-0.015	-0.124	-0.005	-0.036	0.001	0.004
	(0.026)	(0.053)	(0.044)	(0.048)	(0.012)	(0.025)	(0.016)	(0.023)
Obs (panels A-D)	20,998	20,997	20,997	20,997	19,403	19,403	19,403	19,403
E. Balanced panel	-0.059	-0.146	-0.027	-0.096	-0.002	-0.043	0.006	0.012
	(0.031)	(0.046)	(0.034)	(0.048)	(0.015)	(0.027)	(0.017)	(0.022)
Obs	20,522	20,521	20,521	20,521	19,122	19,122	19,122	19,122
F. Matched sample	-0.055	-0.121	-0.033	-0.103	0.0004	-0.002	0.007	-0.001
	(0.029)	(0.051)	(0.041)	(0.050)	(0.0172)	(0.029)	(0.018)	(0.028)
Obs	4,921	4,921	4,921	4,921	4,120	4,120	4,120	4,120
G. All treated obs	-0.076	-0.143	-0.058	-0.148	0.016	-0.035	0.025	0.037
	(0.032)	(0.048)	(0.035)	(0.043)	(0.022)	(0.033)	(0.024)	(0.026)
Obs	24,844	24,843	24,843	24,843	22,535	22,535	22,535	22,535
H. Switchers included	-0.083	-0.147	-0.047	-0.138	-0.004	-0.038	0.008	0.009
	(0.027)	(0.042)	(0.030)	(0.043)	(0.014)	(0.024)	(0.016)	(0.022)
Obs	23,620	23,619	23,619	23,619	19,989	19,989	19,989	19,989

Notes: The table shows the results of robustness checks for the effects on patient volume estimated for the privatized hospitals and treated markets, given in Tables 3 and 5, respectively. For each outcome we present the baseline estimates in row A. Row B includes static hospital beds to weight hospitals or markets. Panel C uses the baseline specification including a linear trend interacted with an indicator for privatized hospitals or treated markets. Panel D presents coefficients from the Callaway and Sant'Anna (2020) estimator. Panel E drops treated hospitals privatized after 2014 (or markets treated after 2014) to ensure we observe each privatized hospital for 5 years before and after the transition. Panel F presents results estimated using a matched

Table 7: Effects on staff (FTE per 100 adjusted admissions)

	(1) Total	(2) MD	(3) Nurse	(4) Other	(5) Contract
A: No controls					
DD	-0.57	-0.03	-0.02	-0.52	-0.01
	(0.26)	(0.01)	(0.06)	(0.19)	(0.01)
Obs	20,998				8,632
B: Market controls					
DD	-0.52	-0.03	-0.01	-0.48	-0.01
	(0.26)	(0.01)	(0.07)	(0.19)	(0.01)
Obs	19,385				8,628
Mean outcome (t-1)	7.40	0.10	1.90	5.30	0.20

Notes: The table presents effects on full-time equivalent (FTE) employed staff per 100 adjusted admissions at the privatized hospitals, obtained by estimating Equation 2 on hospital-year level data. Column 1 presents results for total FTEs, which comprises of physician, nurse, and all others, presented in columns 2, 3, and 4, respectively. We normalize the staff levels in each column by contemporaneous, adjusted admissions, which scales admissions by the ratio of outpatient to inpatient revenue. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to FTEs per 100 adjusted admissions at privatized hospitals in the year prior to privatization. Standard errors are clustered by hospital and are presented in parentheses.

Table 8: Effects on aggregate staff (FTE per 100 adjusted admissions)

	(1) Total	(2) MD	(3) Nurse	(4) Other
A: No controls				
DD	-0.03	0.003	0.01	-0.05
	(0.12)	(0.007)	(0.03)	(0.09)
Obs	19,404			
B: Market controls				
DD	0.01	0.004	0.02	-0.01
	(0.11)	(0.007)	(0.03)	(0.08)
Obs	17,983			
C: Heterogeneity by union membership				
DD	0.13	0.01	0.02	0.09
	(0.16)	(0.01)	(0.04)	(0.12)
x 1(< med. union membership)	-0.33	-0.02	-0.03	-0.29
	(0.18)	(0.01)	(0.05)	(0.13)
Mean outcome (t-1)	6.59	0.13	1.92	4.54

Notes: The table presents estimated effects on full-time equivalent (FTE) employed staff per 100 adjusted admissions at the market-level obtained by estimating the market-level equivalent of Equation 2 on market-year data. We define markets using Health Service Areas (HSAs), as described in Section 5.3. Column 1 presents results for total FTEs, which comprises of physician, nurse, and all others, presented in columns 2, 3, and 4, respectively. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying HSA-level controls: population, unemployment, uninsurance, and poverty rates. Panel B has fewer observations since the covariates are not available for 1995 and 1996. Panel C presents the corresponding results from a triple difference specification including an interaction term with an indicator for markets located in states with rates of union membership (among private workers) in 1999 less than the median among states with treated markets. The mean values pertain to outcomes in treated markets in the year prior to privatization. Standard errors are clustered by HSA and are presented in parentheses.

Table 9: Staff robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hospital				Market			
	Total	MD	Nurse	Other	Total	MD	Nurse	Other
A. Baseline	-0.57	-0.03	-0.02	-0.52	-0.03	0.003	0.01	-0.05
	(0.26)	(0.01)	(0.06)	(0.19)	(0.12)	(0.007)	(0.03)	(0.09)
B. Weighted by beds	-0.77	-0.04	-0.10	-0.61	-0.09	-0.01	-0.02	-0.06
	(0.22)	(0.02)	(0.06)	(0.16)	(0.07)	(0.01)	(0.03)	(0.05)
C. Treated group trend	-0.29	-0.00	-0.03	-0.27	0.29	0.02	0.08	0.19
	(0.33)	(0.01)	(0.09)	(0.25)	(0.13)	(0.01)	(0.04)	(0.10)
D. CS estimator	-0.52	-0.03	-0.05	-0.45	0.05	0.01	0.01	0.03
	(0.27)	(0.01)	(0.07)	(0.20)	(0.13)	(0.01)	(0.04)	(0.09)
Obs (panels A-D)	20,998	20,998	20,998	20,998	19,403	19,403	19,403	19,403
E. Balanced panel	-0.56	-0.02	-0.02	-0.51	-0.02	0.005	0.01	-0.03
	(0.28)	(0.01)	(0.07)	(0.21)	(0.14)	(0.008)	(0.04)	(0.10)
Obs	20,522	20,522	20,522	20,522	19,122	19,122	19,122	19,122
F. Matched sample	-1.01	-0.02	-0.13	-0.83	0.05	0.01	0.07	-0.03
	(0.33)	(0.01)	(0.08)	(0.24)	(0.21)	(0.01)	(0.04)	(0.18)
Obs	4,921	4,921	4,921	4,921	4,120	4,120	4,120	4,120
G. All treated obs	-0.68	-0.03	-0.08	-0.56	-0.20	-0.004	-0.02	-0.17
	(0.25)	(0.01)	(0.06)	(0.18)	(0.17)	(0.008)	(0.04)	(0.13)
Obs	24,844	24,844	24,844	24,844	22,535	22,535	22,535	22,535
H. Switchers included	-0.59	-0.02	-0.03	-0.53	0.02	0.002	0.03	-0.004
	(0.26)	(0.01)	(0.07)	(0.20)	(0.09)	(0.007)	(0.03)	(0.070)
Obs	23,620	23,620	23,620	23,620	19,989	19,989	19,989	19,989

Notes: The table shows the results of robustness checks for the effects on full-time equivalent (FTE) employed staff per 100 adjusted admissions estimated for the privatized hospitals and treated markets, given in Tables 7 and 8, respectively. For each outcome we present the baseline estimates in row A. Row B includes static hospital beds to weight hospitals or markets. Panel C uses the baseline specification including a linear trend interacted with an indicator for privatized hospitals or treated markets. Panel D presents coefficients

A Additional figures and tables

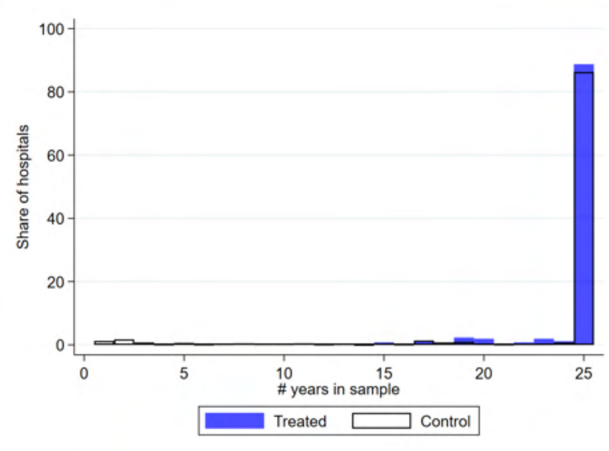


Figure A.1: Balance of hospital panel

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

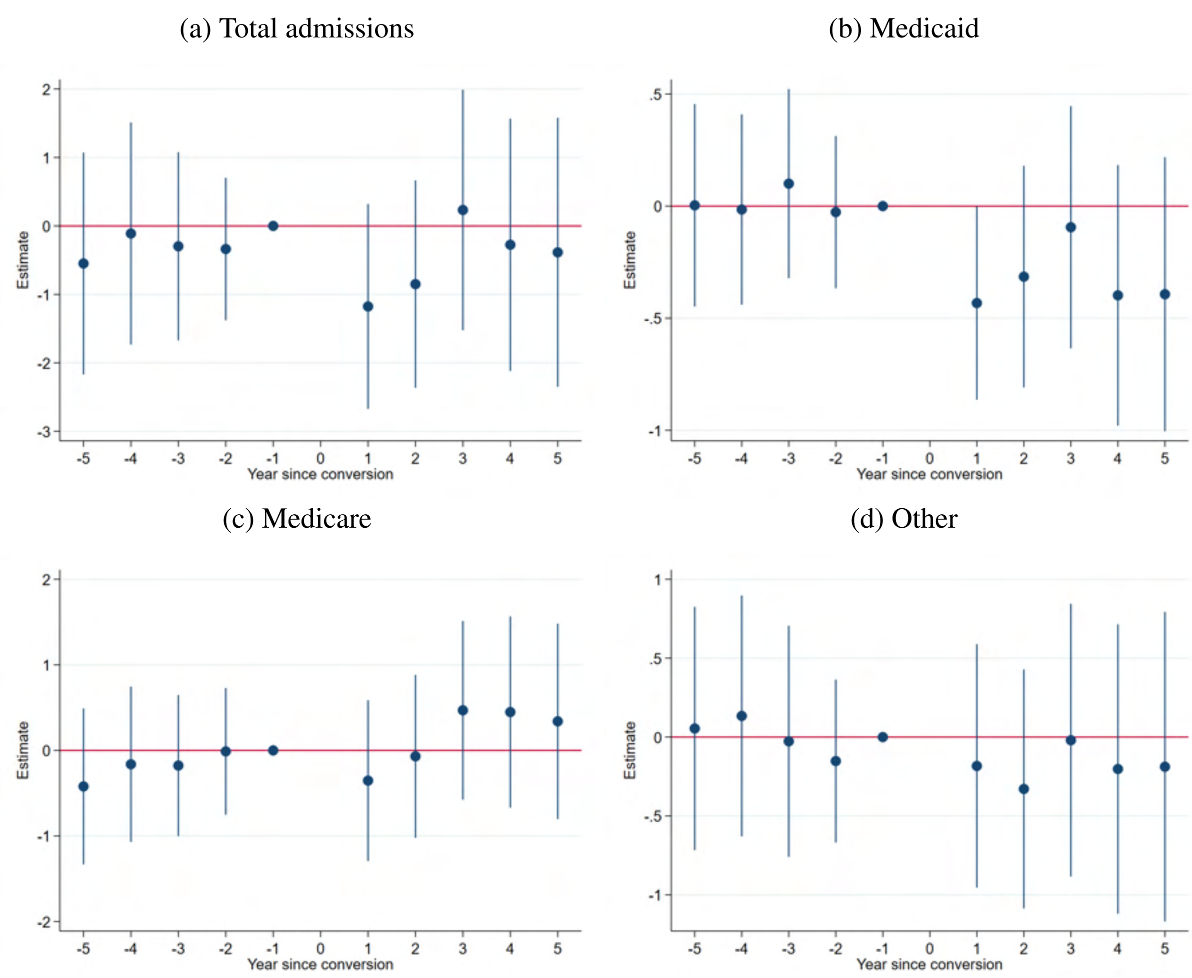


Figure A.2: Effects on patient volume per bed

Note: The figure presents dynamic effects on total volume per bed obtained by estimating Equation 3 on the analysis sample. Total patient volume is normalized by contemporaneous hospital beds. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

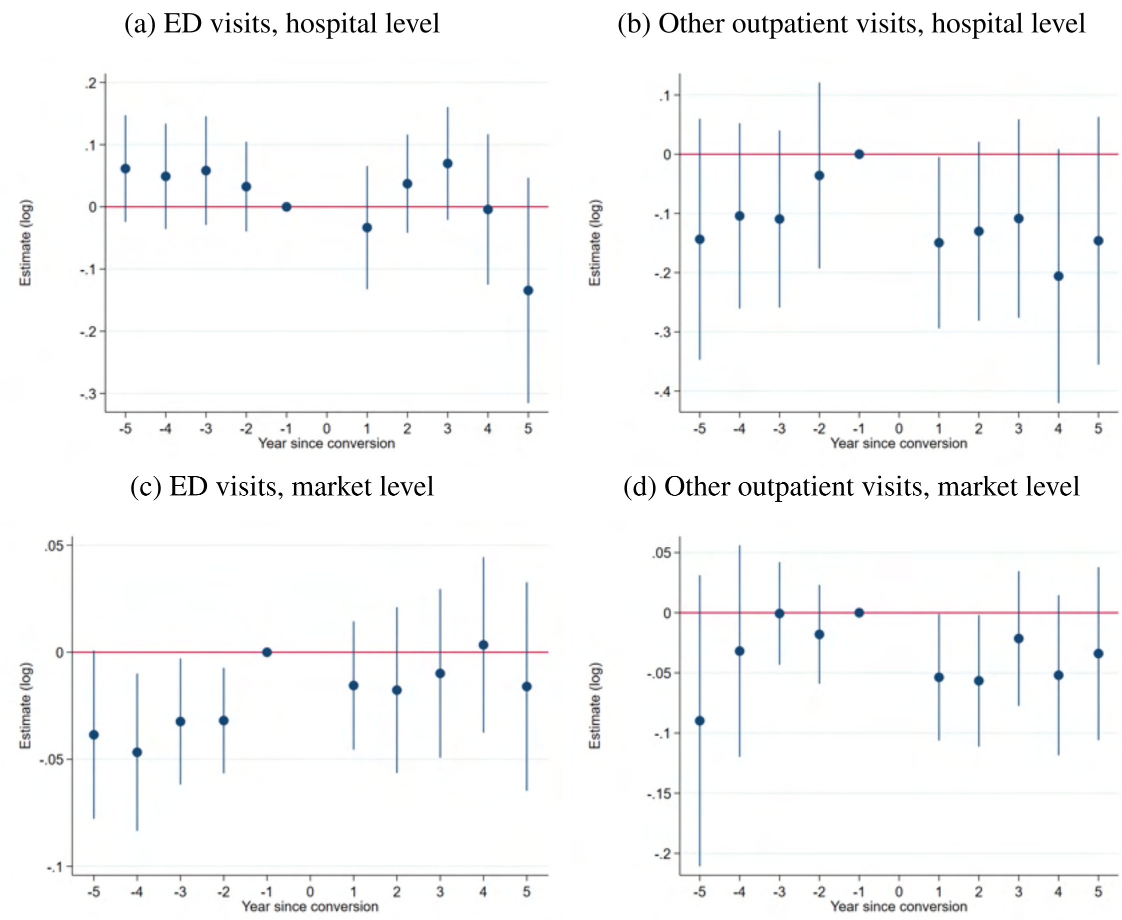


Figure A.3: Effects on ED and other outpatient visits

Note: The figure presents event study plots obtained by estimating Equation 3 on the hospital-year sample (panels (a) and (b)) and market-year sample (panels (c) and (d)). The outcomes are logged emergency department (ED) visits and other (non-ED) outpatient visits. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital in panels (a) and (b) and by market in panels (c) and (d).

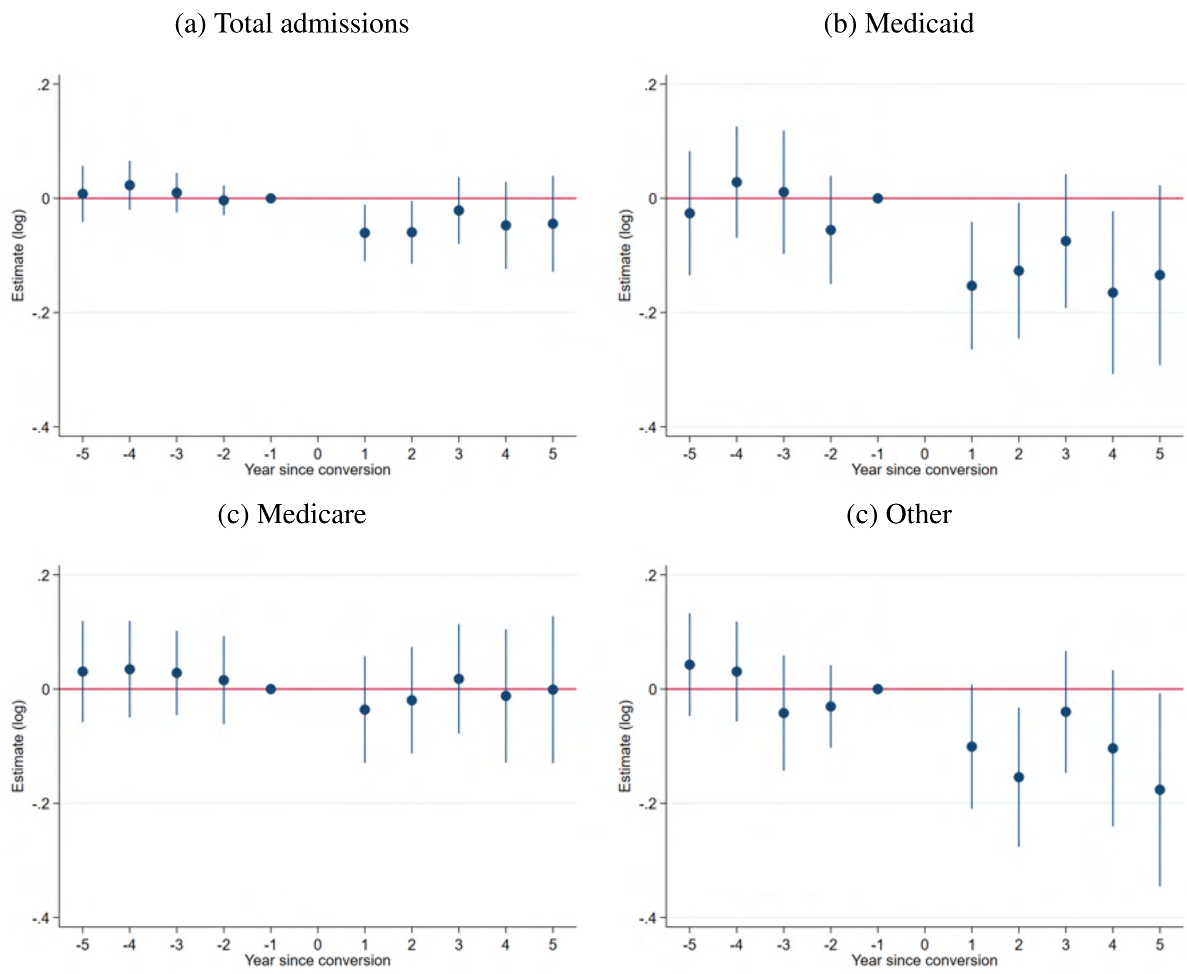
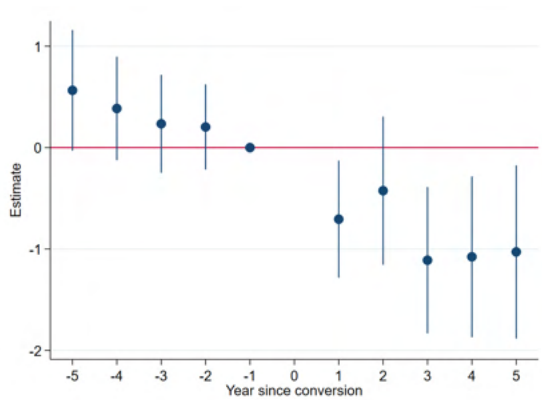


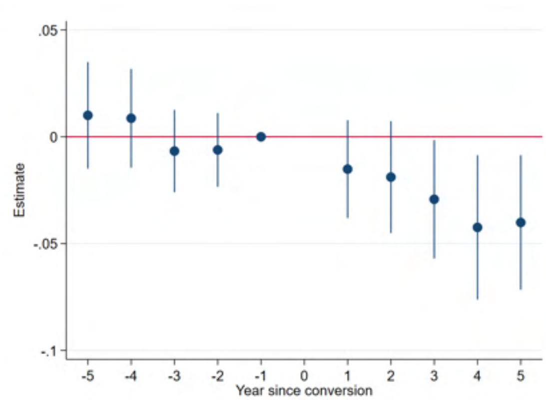
Figure A.4: Effects on patient volume using the matched sample

Note: The figure presents event study plots obtained by estimating Equation 3 on a matched subsample, where suitable matched comparison hospitals were identified using propensity score matching. We matched each privatized hospital to a single control hospital without replacement based on bed capacity, total and Medicaid volume, total expenses, and market attributes one to three years prior to privatization (see C.2 for more details). The outcomes are (logged) total, Medicaid, Medicare, and other patient volume, respectively. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

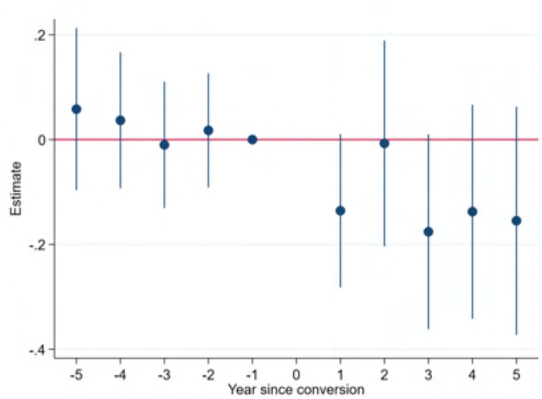
(a) Total FTEs



(b) Physician FTEs



(c) Nurse FTEs



(d) Other FTEs

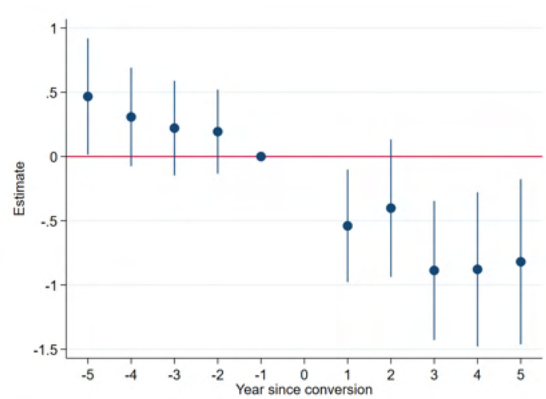


Figure A.5: Effects on staffing using the matched sample

Note: The figure presents event study plots obtained by estimating Equation 3 on a matched subsample, where suitable matched comparison hospitals were identified using propensity score matching. We matched each privatized hospital to a single control hospital without replacement based on bed capacity, total and Medicaid volume, total expenses, and market attributes one to three years prior to privatization (see C.2 for more details). The outcomes are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other FTEs in panels (a), (b), (c), and (d), respectively. All outcomes are normalized by contemporaneous adjusted admissions and presented per 100 adjusted admissions. The error bars denote 95% confidence intervals. Standard errors are clustered by hospital.

(a) Total FTEs (b) Physician FTEs (c) Nurse FTEs (d) Other FTEs

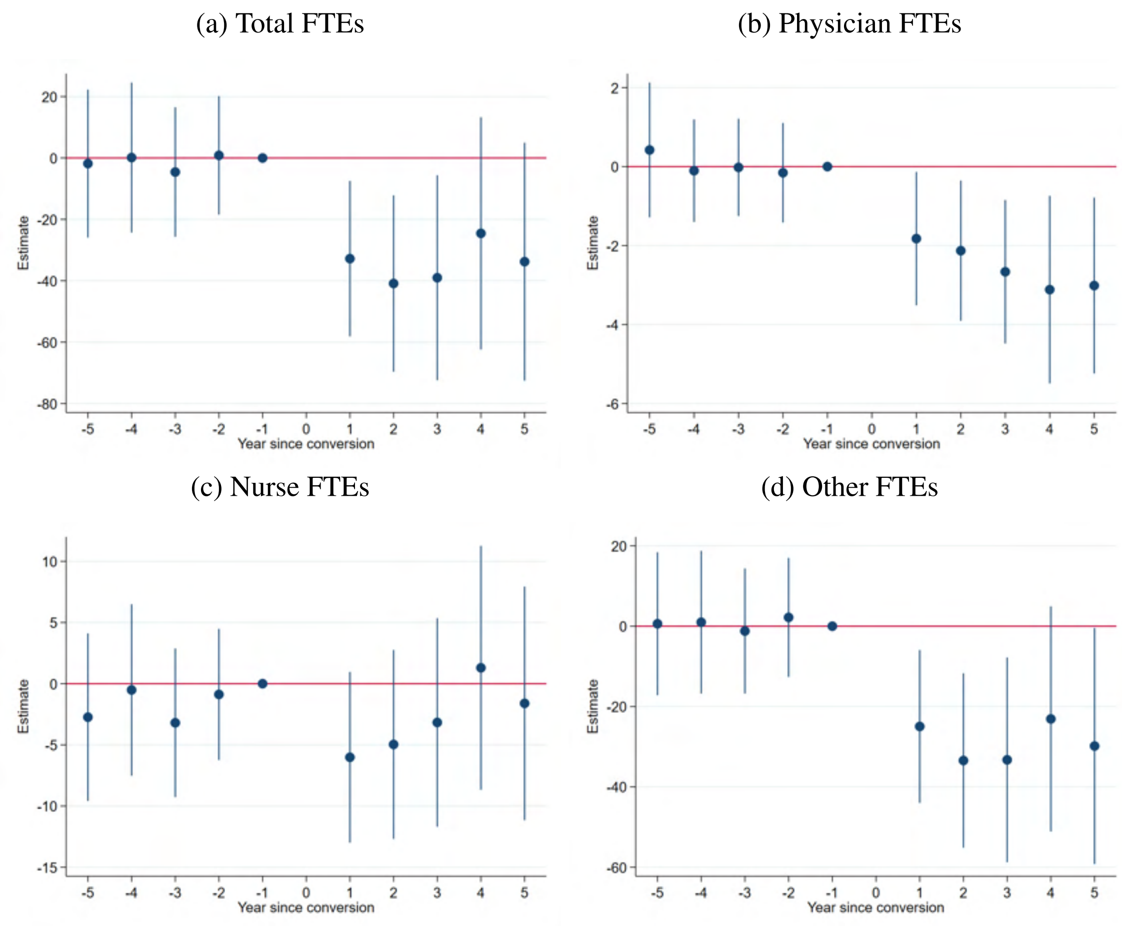


Figure A.6: Effects on staffing (FTE per 100 beds)

<u>Note:</u> The figure presents event study plots obtained by estimating Equation 3 on hospital-year level data. The comparison group is comprised of hospitals that remain public throughout our sample period and are not located within 15 miles of any treated hospital. Outcomes from the AHA are total full-time equivalent employees (FTEs), physician FTEs, nurse FTEs, and other FTEs in panels (a), (b), (c), and (d), respectively. All outcomes are normalized by the contemporaneous number of hospital beds and presented per 100 beds. Year zero is the year of privatization and is excluded for the treated hospitals since it represents partial treatment. The error bars present 95% confidence intervals. Standard errors are clustered by hospital.

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

State	**Share**	**# Hospitals**
Wyoming	70.8	32
Alabama	44.4	116
Mississippi	40.7	112
Kansas	36.8	152
South Carolina	32.9	88
North Carolina	31.8	135
Iowa	29.8	123
Washington	27.0	107
Louisiana	26.1	200
Idaho	25.2	52
New York	23.6	210
Colorado	23.5	106
California	22.9	419
New Mexico	22.2	55
Hawaii	22.1	28
Virginia	20.1	123
Oregon	19.8	65
Oklahoma	19.4	146
Tennessee	19.0	132
Utah	18.6	59
Missouri	18.2	143
Indiana	17.5	161
Florida	16.8	253
Texas	15.8	588
Alaska	14.6	26
Minnesota	14.4	141

State	**Share**	**# Hospitals**
Nevada	14.1	58
Kentucky	13.7	121
Nebraska	13.5	99
New Jersey	12.9	99
Georgia	11.7	172
Ohio	11.3	224
Arkansas	10.4	102
Rhode Island	10.3	15
Montana	10.1	66
Connecticut	9.9	42
West Virginia	9.3	61
Maryland	8.5	62
Massachusetts	8.2	102
Illinois	8.0	208
District Of Columbia	7.4	14
Delaware	6.3	13
Wisconsin	6.3	149
Arizona	6.2	110
Michigan	6.2	165
New Hampshire	5.5	31
Maine	5.4	39
South Dakota	4.4	64
Pennsylvania	3.8	235
North Dakota	2.6	50
Vermont	1.7	17

Table A.2: Types of privatization deals

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

Notes: This table presents a breakdown of the privatization deals in our main analysis sample. These occur between 2000-2018. Columns 1 and 2 present the number of hospitals converted to private non-profit and for-profit, respectively. Non-transfer of ownership implies the government continued to own the real estate and buildings, but transferred operational control to the new private firm. This could be implemented in multiple ways as listed. Miscellaneous includes cases where a new private firm was incorporated subject to oversight by the previous government owners specifically to operate the hospital, and cases where the modality could not be identified. Transfer of ownership implies the government sold all hospital assets to the new private owner. Appendix B.1 describes these categories in more detail with examples.

Table A.3: Effects on Traditional Medicare patient volume

	(1) All	(2) Duals	(3) Non duals
A: Full sample			
1: Baseline	-0.083	-0.077	-0.091
	(0.030)	(0.035)	(0.029)
2: C-S	-0.042	-0.056	-0.038
	(0.031)	(0.034)	(0.033)
Mean	6.09	4.82	5.69
Observations	13,824	13,824	13,824
B: Matched sample			
1: Baseline	-0.023	-0.028	-0.028
	(0.038)	(0.044)	(0.037)
2: C-S	-0.002	-0.023	0.004
	(0.035)	(0.039)	(0.037)
Mean	6.20	4.96	5.79
Observations	3,893	3,893	3,893

Notes: This table presents effects on Traditional Medicare (TM) patient volume at the privatized hospitals, estimated using 100% Medicare fee-for-service inpatient claims data over 2000–17. The outcomes are logs of total TM volume, dual eligibles, and non-duals, respectively. Panels A and B present results on the full and matched samples, respectively. In each panel, rows 1 and 2 present results from the baseline two-way fixed effects and Callaway-Santanna models, respectively. These models have fewer observations since the claims data spans a shorter period than the AHA sample used in the main analysis. To ensure we have 2 years before and after every privatization, we limit treated units to hospitals privatized during 2002–15. Hence, these models include 215 privatized hospitals instead of the 257 used in the main analysis.

Table A.4: Effects on ED and other outpatient (log) volume

	(1)	(2)	(3)	(4)
	Hospital		Market	
	ED	Other Outpt	ED	Other Outpt
DD	-0.048	-0.069	0.018	-0.016
	(0.032)	(0.063)	(0.016)	(0.028)
Obs	20,998	20,998	19,404	19,404
DD	-0.043	-0.067	0.016	-0.010
	(0.032)	(0.063)	(0.016)	(0.028)
Obs	19,385	19,385	17,983	17,983
Mean outcome (t-1)	15,424	53,766	136,340	455,672

Notes: The table presents estimated effects on Emergency Department (ED) and non-ED outpatient (log) volume at the privatized hospital (cols. 1 and 2) and on the market experiencing privatization (cols. 3 and 4). Panels A and B presents coefficients obtained by estimating Equation 2 without and with the county-year controls, respectively. Standard errors are clustered by hospital or market, depending on the level of treatment.

Table A.5: Effects on staff (FTEs per 100 beds)

	(1) Total	(2) MD	(3) Nurse	(4) Other	(5) Contract
DD	-33.4	-2.5	-1.7	-29.5	0.16
	(12.7)	(0.8)	(3.2)	(9.5)	(1.35)
Obs	20,998				8,632
DD	-33.3	-2.6	-1.9	-29.1	0.12
	(12.7)	(0.8)	(3.2)	(9.5)	(1.35)
Obs	19,385				8,628
Mean outcome (t-1)	511.2	10.2	138.6	362.1	13.58

Notes: The table presents effects on full-time equivalent (FTE) employed staff per 100 beds at the privatized hospitals, obtained by estimating Equation 2 on hospital-year level data. Column 1 presents results for total FTEs, which comprises of physician, nurse, and all others, presented in columns 2, 3, and 4, respectively. The outcome for column 5 is contract FTEs, which are derived from HCRIS. We normalize the staff levels in each column by the contemporaneous number of hospitals beds to account for the possibility that privatized hospitals downsize following privatization. The staff inputs are presented per 100 beds, which is approximately the size of a public hospital in our sample. Panel A reports coefficients from a two-way fixed effects specification with no covariates. Panel B reports coefficients from a two-way fixed effects specification including time-varying county-level controls as described in Section 4. Panel B has fewer observations since the market-level covariates are not available for 1995 and 1996. The mean values pertain to patient volume at privatized hospitals in the year prior to privatization. Standard errors are clustered by hospital and are presented in parentheses.

Table A.6: Heterogeneity by type of privatization

	(1) Total	(2) Medicaid	(3) Medicare	(4) Other	(5) Total FTEs	(6) MD	(7) Nurse	(8) Other
A: Baseline								
DD	-.084	-.149	-.049	-.138	-0.57	-0.03	-0.02	-0.52
	(.027)	(.042)	(.030)	(.043)	(0.26)	(0.01)	(0.06)	(0.19)
B: Heterogeneity by extent of private control								
DD	-.104	-.184	-.047	-.168	-0.37	-0.02	0.02	-0.35
	(.035)	(.054)	(.040)	(.053)	(0.32)	(0.01)	(0.08)	(0.24)
x 1(more private control)	.046	.083	-.003	.070	-0.45	-0.01	-0.10	-0.38
	(.054)	(.082)	(.058)	(.088)	(0.51)	(0.02)	(0.13)	(0.38)
C: Heterogeneity by for-profit conversion								
DD	-.146	-.186	-.084	-.210	-0.67	-0.03	-0.05	-0.58
	(.032)	(.051)	(.036)	(.048)	(0.32)	(0.01)	(0.08)	(0.24)
x 1(for-profit)	.212	.128	.120	.244	0.34	0.03	0.10	0.20
	(.057)	(.084)	(.060)	(.098)	(0.48)	(0.02)	(0.13)	(0.36)
Obs	20,998	20,997	20,997	20,997	20,998	20,998	20,998	20,998

Notes:

B Data Appendix

B.1 Privatization taxonomy

We first identify cases of public hospitals that were converted to private control or that closed during our study period of 2000–18. There is no official source of such events and thus we utilized the AHA annual survey of hospitals files over this period. We infer a conversion when we observe a change in management control type from public (state, county, or city) to private (for-profit or non-profit). We infer a closure when a hospital disappears from the survey in the middle of the sample. We validate both conversions and closures using information recorded in the annual AHA Summary of Changes files, which explain each change in the AHA survey from the previous year. A criticism of the AHA is that small, rural hospitals sometimes do not feature in its surveys. To overcome this limitation, we also inferred closures using the Medicare Place of Service (POS) files that do not appear in the AHA. This process yields 381 conversions and 127 closures over 2001–16.

Further, we have devoted hundreds of hours to manually verify each conversion and closure by combing through hospital websites, news articles, and third-party sites such as the American Hospital Directory (AHD). Manual validation help us identify non-trivial numbers of false positive (160) and false negative (34) conversions. Our final tally of conversions is accordingly 257 (381 – 160 + 34).

Through these detailed reviews we have divided conversions into five categories. Four categories involve only a change in management and account for about 65% of all conversions, while the fifth is an outright sale of all assets. To the best of our knowledge, these aspects of hospital conversions have not been studied previously.

This appendix attempts to provide a flavor of the heterogeneous nature of public hospital conversions. We briefly describe some case studies to illustrate our categorization of conversions.

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

  Example: Glenwood Regional Medical Center (West Monroe, La) recorded a conversion in the AHA in 2006 from "hospital district or authority" to "other not-for-profit." Article in 2006 states that IASIS Healthcare(R) LLC announced the signing of a definitive agreement to acquire Glenwood Regional Medical Center from the Hospital Service District for approximately $82.5 million. Source: https://www.businesswire.com/news/home/20060721005223/en/IASIS-Healthcare-LLC-Announces-Agreement-Acquire-Northeast.

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

  Example: Mercy Hospital Lincoln (Troy, Mo) recorded a conversion in the AHA in 2015 from "County" to "other not-for-profit." Article in January 2015 states that "Under an agreement executed by both parties, Mercy will lease and manage 25-bed Lincoln County Medical Center beginning March 1." Source: https://www.beckershospitalreview.co m/hospital-transactions-and-valuation/lincoln-county-medical-center-joins-mercy-health.html

- **Long-term lease:** Occurs when a private (corporation or health system) authority takes control over day-to-day management of a hospital for an extended period of time (more than 15 years). The government entity maintains control over the hospital's property, assets, and debts. We consider this to be a management change only. Example: Mercy McCune-Brooks Hospital (Joplin, Mo) recorded a conversion in the AHA in 2012 from "city" to "church operated." Article published in 2012 states that "Mercy's 50-year lease of the city-owned hospital was approved by the Carthage City Council." Source: https://www.joplinglobe.com/news/local_news/new-year-brings-mccune-brooks-into-sisters-of-mercy-health-system/article_2aca1cb3-7a97-5538-b98e-b434fd2ef056.html.

- **Joint venture or merger:** Occurs when two private (corporations or health systems) authorities agree to merge or sign a joint venture, which results in a newly formed private authority to take over management of the hospital. We consider this to be a management change only.

  Example: Rice Memorial Hospital (Willmar, Mn) recorded a conversion in the AHA in 2018 from "city" to "other not-for-profit." Article published in 2017 states 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 will make a capital investment of $32 million in Rice Memorial Hospital over the next 10 years. Rice Memorial Hospital assets will continue to be owned by the City of Willmar." Source: https://www.centracare.com/blog/2017/december/carris-health-agreement-finalized/

- **Public hospital incorporating:** Occurs when a public health system files for 501c3 non-profit status ("incorporating"). We consider this to be a management change only.

  Example: Hutchinson Area Health Care (Hutchinson, Mn) recorded a conversion in 2008 from "city" to "other not-for-profit." In an article detailing the history of the Hutchinson's hospital and clinic, the article notes that in "January 2008: Hutchinson Area Health Care becomes its own private, nonprofit corporation and is no longer a part of the city of Hutchinson. Source: https://www.crowrivermedia.com/hutchinsonleader/news/local/hutchinson-health-and-healthpartners-become-one/article_7357cfee-04c4-5c62-b4c4-7b9dc22cd13b.html

C Methodology

C.1 Sample selection

To construct our analytic sample of control hospitals, we start with American Hospital Association (AHA) survey data for the years 1995 to 2019. In the raw data there are ∼6,200 hospitals per year and ∼8,400 unique hospitals over the sample period. We make the following sample restrictions:

- Drop hospitals whose most common AHA service code is not "general medical and surgical" (2,457 hospitals)
- Drop hospitals that on average report fewer than 10 beds (42 hospitals)
- Drop hospitals that are ever classified as federal government by the AHA (293 hospitals). These include military, Veterans Affairs, Indian Health Service, and Department of Justice hospitals
- Drop hospitals that are only classified as public (state and local) in some years of the sample period but not all. This group includes hospitals that are most commonly labeled as private (290 hospitals) and hospitals that are most commonly labeled as public (122 hospitals). This is a conservative restriction to ensure that our comparison group is comprised of non-converting, public hospitals
- Drop hospitals that are within 15 miles of at least one treated hospital (32 hospitals)

Our final, analytic sample consists of 802 control hospitals.

As discussed in Section 3.1, we created our list of public to private conversions by starting with conversions implied by changes in the AHA's control variable and then manually validating each conversion. From this process we identified 269 total conversions. From our manual validation, we found that two treated hospitals experience more than two conversions (i.e. public to private or private to public) over our sample period; we dropped these hospitals. Five hospitals were dropped that convert from private to public and back to private within our sample period. Finally, we dropped four treated hospitals whose most common AHA primary service code was not "general medical and surgical." Our final set of treated hospitals consists of 258 public to private conversions. We note that two treated hospitals experience a second conversion in which they convert back from private to public. For these two hospitals we drop observations on or after the second conversion.

C.2 Propensity score matching

In one of our robustness checks reported in Table 9, we apply propensity score matching (PSM) to our analytic sample to identify treated and control hospitals that are similar on pre-period observables. Specifically, we conduct one-to-one, nearest neighbor matching without replacement and estimate logit models to predict privatization with the following explanatory variables from t-1 to t-3 (where t denotes the year of privatization for a given treated hospital):

- # hospital beds
- Total admissions
- Medicaid admissions
- Total expenses
- % in poverty (measured at the county-year level)

- % unemployment (measured at the county-year level)
- Health Service Area population (only t-1; calculated by aggregating county-year population estimates)

We impose the restriction that propensity scores of matched pairs be in the same decile of the propensity score distribution. We apply PSM sequentially by first searching for similar control hospitals for hospitals that privatize in 2000, the first year of conversions in our data. Control hospitals that match to these privatizing hospitals are removed from the donor (control hospital) pool prior to searching for matches for hospitals that privatize in 2001. We continue this process for all 19 years of privatizations (2000–2018) and are able to match all 258 treated hospitals.

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