Are Nonprofit Hospitals Mission-Committed?

Julia Elena Gonzalez*, Victor Kilanko†, and Ryan Phillip Quandt‡

Cicero Institute

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Abstract

A nonprofit hospital is, in theory, an organization that weighs patients' health alongside its own surplus. Since the 2000s, successive reports have found nonprofits provide slightly more charity care than for-profits, which raises the question of whether the nonprofit form carries an objective of its own or only changes the conditions under which returns are sought. We use the staggered Medicaid expansions, a common shock that freed hospitals of part of their charity-care burden, to answer it. Expansion cut nonprofit charity care by roughly a third, which is no more than coverage substitution accounts for. Under an observable-selection assumption, nonprofits kept 0.8 percentage points more charity care (about 25 percent) than the same hospitals would have under for-profit ownership, and no more than they would have under public ownership. On IRS Schedule H forms, the freed resources partly moved to other community benefit categories: a fifth returned as subsidized care, education and research absorbed more, and between two-fifths and five-sixths reappeared in no category. In the three expansion states with charity-care spending floors, charity care did not fall. Nonprofit hospitals display a commitment their status obliges, not a mission weight that operates apart from it.

Keywords: Nonprofit Hospitals, Charity Care, Community Benefit, Payer Mix

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*Data Analyst, email.
†Economist, email.
‡Chief Economist & Director of Research, email, website: quandtify.com.

1 Introduction

A nonprofit hospital is, in theory, an organization that weighs patients' health alongside its own surplus. It treats patients who cannot pay and, at a cost to its margin, funds services that benefit patients without financial gain for the hospital. If that objective governs its spending, a nonprofit hospital will continue to provide uncompensated care, i.e., charity care, even if there is a decrease in the need for it and will direct the resources it frees to other services that benefit patients. A hospital that instead pursues returns under the conditions its tax status permits will, while upholding the required conditions, otherwise orient toward where it expects a return.

In practice, the record favors the latter description. Nonprofit hospitals devote only about 0.6 percentage points more of their operating expenses to charity care than for-profit hospitals, against an exemption worth $24.6 billion in 2011 (or 5.9% of total expenses (Herring et al., 2018)) and upwards of $37 billion by 2021 (Plummer et al., 2024).[1] They further devote about 1.5 percentage points less of total expenses toward charity care than for-profit hospitals, and four in five nonprofit hospitals spend less on charity care than they receive in tax benefits (Zare and Anderson, 2024; Bai et al., 2021). Congress heard testimony in 2025 that the shortfall exceeds $25 billion a year, and the bill considered at the time by representatives conditioned exemption on meeting a community benefit standard which, among other elements, included spending at least the value of the exemption on charity care, training and research, and facility improvements, much as the proposals in 2006 and 2007 would have required a minimum level of charity care.[2] This evidence is descriptive and it brings to the surface the question underlying the policy debate as whether or not nonprofit hospitals advance mission at least as much as they optimize returns, or whether nonprofit status only changes the conditions under which returns are sought.

The Medicaid expansions that followed from the Affordable Care Act supply a test with which to evaluate nonprofit behavior and motives. Expansion set the income limit to qualify for Medicaid to 138% of the federal poverty level, while previously only low-income, categor-

[1] Congressional Budget Office, "Nonprofit Hospitals and the Provision of Community Benefits," December 2006; Rosenbaum et al. (2015).

[2] Congressional Research Service, "Nonprofit Hospitals, Tax Benefits, and Charity Care," IF13192, March 30, 2026, citing H.R. 3019; testimony of Christopher M. Whaley before the House Committee on Ways and Means, Subcommittee on Oversight, September 16, 2025. The earlier proposals are described in the remarks of IRS Commissioner Steven T. Miller, January 12, 2009.

ically eligible groups could qualify. Consequently, when a state adopted Medicaid expansion, many patients who had previously relied on charity care from hospitals became Medicaid patients, and the hospitals that had previously absorbed the cost for these patients' care were now largely reimbursed for that coverage. An examination of what hospitals do with that windfall reveals how integral charity care is to the mission of that particular hospital in the first place.

The expansion shock is common to every hospital within a state opting into Medicaid expansion. Across states, the timing of expansion roll out varied, starting in 2014 with the first and largest wave of 26 state adopters. The shock applies to hospitals across a state regardless of ownership status, so we compare the nonprofit response with that of for-profit hospitals, which faced the same shock without an exemption or the obligations attached to it, and with that of public hospitals, which are exempt by their status and carry a charity-care obligation but stand outside the community-benefit standard that governs nonprofits.

Prior work has used the expansions to document the decline in nonprofit charity care and to track community-benefit categories after it, finding the savings offset by unreimbursed Medicaid care and little added direct community spending (Dranove et al., 2016; Young et al., 2018; Kanter et al., 2020; Moran, 2024). That work studies nonprofits alone, so it can show that charity care fell and where reported spending moved, but not whether ownership made any difference to either, which is the question the exemption raises. This paper makes four contributions to it. First, it estimates the ownership effect as a causal contrast: not the difference between nonprofit and for-profit responses, which confounds ownership with the different hospitals and markets the two types occupy, but the nonprofit response minus the response the same hospitals would have had under for-profit ownership (Caron, 2025), and it extends the same comparison to public hospitals, which share the obligation but not the standard. Second, it traces the windfall along a gradient the framework defines, from direct patient aid to private-return uses, on three margins: whether a category is reported, how much, and in dollars. We do this with staggered-adoption estimators that the two-period designs of earlier work do not use and with an accounting of how much of the freed charity care reappears anywhere on Schedule H. Third, it exploits the variation in the obligation itself: three expansion states had charity-care spending floors in force before 2014 and their hospitals' response to the same shock differs from the rest in the way a binding obligation predicts. Fourth, the selection a balanced hospital panel imposes is tested with

a preregistered hazard analysis whose remedy was named before the result was seen rather than assumed away.

We assemble two hospital panels. The first is a balanced hospital–year panel of Medicare Cost Reports for 3,495 hospitals over 2011–2023, from which we estimate the effect of expansion on charity care, uncompensated care, payer mix, and margins separately by ownership type, using the staggered-adoption group–time framework of Callaway and Sant'Anna (2021) with the doubly robust score of Sant'Anna and Zhao (2020). The second is a filer–year panel of IRS Form 990 Schedule H returns for 1,250 single-facility nonprofit organizations over 2011–2022, on which the same estimator traces where the freed resources move across the community-benefit categories the exemption counts.

Consistent with prior work, expansion reduced nonprofit hospitals' charity care by roughly a third: the charity-care payer mix falls by 0.9 to 1.1 percentage points against a pre-expansion mean of 3.1, charity-care cost as a share of net revenue by 39 to 64 percent, and bad debt by about a fifth, in every sample and specification and with flat pre-expansion leads. The decline is no larger than coverage substitution can account for: the Medicaid payer mix rises by 2.9 to 3.8 points over the same columns, and the Medicare and commercial mixes are null once covariates enter. Net margins rise by 1.7 points in the preferred specification, and the estimate is bounded between a seventh of the freed uncompensated-care cost and more than the whole of it.

Ownership shaped how far charity care fell. Under the observable-subgroup-selection assumption, nonprofit hospitals reduced their charity-care payer mix by 0.8 percentage points less than the same hospitals would have under for-profit ownership, a quarter of the pre-expansion level, with a 95 percent interval of 0.3 to 1.3 points; the plain difference between the nonprofit and for-profit responses, reported beside it, is not distinguishable from zero in the preferred specification, for a reason the paper traces to the for-profit arm's covariate model rather than to ownership. Against public hospitals the premium disappears: nonprofits kept no more charity care than the same hospitals would have under public ownership (-0.25 points, s.e. 0.30). The nonprofit premium is therefore a premium over the profit-maximizing form alone, and the pattern points to what nonprofit and public hospitals share and for-profit hospitals lack: a status that carries a charity-care obligation.

The freed resources did not return to patients in the way a mission-weighted hospital would spend them. Net subsidized services, the direct-aid category on which the framework's

mission test rests, do not move in logs in either design; in dollars they rise by about a fifth of the charity decline, among filers already providing them. Community health improvement gains reporters but few dollars. Education, research, and community-building spending rises by 0.32 log points and absorbs about a sixth of the charity decline once the largest one percent of filer-years is trimmed, and about half without the trim; most of it is health-professions education, and most of the dollars sit in large filers. Between two-fifths and five-sixths of the charity decline reappears in no Schedule H category.

The within-nonprofit decline survives post-treatment violations of parallel trends larger than any observed before treatment, though not linear extrapolation of the pre-trend, which three pre-periods cannot pin down. Balanced-panel membership is treatment-affected, but the remedy leaves the charity decline intact and Lee bounds exclude zero. The ownership contrast is examined under every treatment of the covariates the bad-controls literature proposes. The Schedule H results do not depend on the log transform, and their extensive-margin and dollar decompositions are reported in full.

Read together, the results say that the charity-care commitment nonprofit hospitals display is the one their exempt status obliges, not a mission weight that operates apart from it. Three expansion states had charity or indigent-care spending floors in force before 2014, and among their 130 treated filers charity care did not fall on the log margin, the reporting response to expansion seen elsewhere on education and community health improvement is absent, and the only response is on the subsidized-services lines that count toward a floor. Three states cannot separate a floor from state differences, but the evidence points the same way as the two ownership contrasts and the allocation of the windfall. Where the obligation is broad, the windfall goes where the obligation counts it: to categories that carry a private return to the hospital and to services whose cost is reimbursed rather than borne. Where the obligation is narrow, charity care is kept.

Section 2 sets the institutional background and the three literatures the paper joins: ownership and hospital behavior; charity care after expansion; and hospitals as insurers of last resort. Section 3 states the two-type framework that organizes the hypotheses. Sections 4 and 5 describe the panels and the estimators, Section 6 the results, Section 7 the robustness and sensitivity checks, and Section 8 what the pattern implies for the design of the exemption's obligations. Section 9 concludes.

2 Background

Medicaid expansion enlarges the public safety net by reimbursing care that would otherwise be uncompensated or self-pay. The reduction of this cost burden acts as a natural experiment for examining how ownership status affects their reallocation of the financial slack. Since nonprofit hospitals provide community benefit, at least as required by their tax-exempt status, we expect these hospitals to reallocate funds differently than for-profit hospitals. However, prior literature is mixed on this hypothesis.

2.1 Institutional Background

The nonprofit designation for hospitals confers federal, state, and local tax advantages in exchange for serving public purposes. Historically, tax exemption was tied closely to the provision of charity care until the IRS adopted the wider "community benefit" standard, which allowed hospitals to satisfy their obligations through a wider set of activities ranging from direct patient aid (charity care and subsidized services) to investments in education, research, capital that may benefit the hospital at least as much as the community it serves.[3] Under this standard, a nonprofit hospital can respond to an expansion of public reimbursement by shifting resources toward other reportable community-benefit categories or retain the gains in the form of higher operating margins.

By the early 2000s, policymakers questioned whether tax-exempt hospitals were providing benefits commensurate with the value of their exemptions. The Senate Finance Committee published a summary of ten nonprofit hospital responses to questions, in which the committee chair expressed concern over the provisions of community benefit.[4] Three months after, the Congressional Budget Office released a report documenting that nonprofit hospitals provided only slightly more charity care with the provision concentrated among a small share of nonprofits.[5]

There were two major federal responses. Beginning in tax year 2008, nonprofit hospitals were required to report detailed community-benefit spending on IRS Form 990, Schedule H.

[3] See IRS Revenue Ruling 69-545.

[4] See the Senate Finance Committee's Memorandum, "Grassley Releases Non-profit Hospital Responses, Expresses Concern Over Shortfalls in Charity Care, Community Benefit," Sept. 12, 2006.

[5] See the Congressional Budget Office's report, "Nonprofit Hospitals and the Provision of Community Benefit," Dec. 6, 2006.

The Affordable Care Act (ACA) subsequently imposed new federal obligations on nonprofit hospitals through section 501(r), including requirements related to community health needs assessments, financial assistance policies, emergency care, and billing and collection practices. However, a report in 2020 by the Government Accountability Office continues to flag concerns with the "community benefit" designation.[6]

The ACA did not impose a national minimum level or charity care or community-benefit spending. Thus, expansion increased public reimbursement while leaving nonprofit hospitals discretion over how to adjust their spending portfolios. As a result, Medicaid expansion is a common financial shock to nonprofit and for-profit hospitals operating in expansion states, but only nonprofits receive tax exemptions and face the reporting and conduct obligations tied to tax-exempt status.

2.2 Literature Review

This paper contributes to three related literatures: how ownership shapes hospital decision-making and responses to fiscal incentives; how nonprofit hospitals provide charity care and respond to payer-mix and reimbursement changes; and the "hospital of last resort" view of distributional consequences for the uninsured. No prior work jointly estimates the nonprofit–for-profit contrast under a common coverage shock and traces, within nonprofits, where freed resources move along the community-benefit gradient.

The first literature examines how hospital ownership shapes organizational objectives and, in turn, costs, quality, and responses to financial incentives. Motivated in part by early-2000s policy debates over the scope and justification of nonprofit tax exemption, this work documents systematic ownership differences in the cost and quality of care (Sloan et al., 2001), responsiveness to changes in reimbursement and other financial incentives (Duggan, 2000), and nonprofit hospitals' strategic responses to for-profit entry and competition (Horwitz and Nichols, 2009; Moran, 2025). A central takeaway is that nonprofit status does not mechanically imply mission-maximizing behavior: in a canonical study, Duggan (2000) finds that private nonprofit hospitals responded to public subsidies in ways that closely resembled for-profit behavior, including retaining a substantial share of the windfall through increased financial assets. But Duggan studies California's disproportionate-share payments, which is

[6]See Government Accountability Office report, "Opportunities Exist to Improve Oversight of Hospitals' Tax-Exempt Status," Sept. 2020.

a different lever, in an era before Schedule H, so it cannot observe the community-benefit reallocation we study. Closer to our setting, Richards et al. (2025) finds that hospitals funnel gains from ACA into IT spending. A related strand emphasizes that hospitals of all ownership types tend to reoptimize their service mix toward financially attractive lines by using cross-subsidization across departments to sustain or expand profitable services while limiting exposure to persistently loss-making ones (Horwitz, 2005; David et al., 2014).

A second literature documents that nonprofit hospital charity care declined following ACA Medicaid expansions (Nikpay et al., 2015; Dranove et al., 2016; Sachs, 2019). Building on these trends, Moran (2024) examines whether expansions in the public safety net—both Medicaid expansions and growth in publicly funded community health centers—crowd out nonprofit hospitals' charity care. Using IRS Form 990 Schedule H filings, the paper reports sizable reductions in charity care after Medicaid expansion and little evidence that hospitals adjust financial-assistance policy income thresholds in response to either shock. These contributions clarify that post-expansion declines in charity care are driven primarily by reductions in demand and that hospitals do not respond with policy changes that could maintain demand. This paper builds on Moran (2024) by including a for-profit contrast and examining other subcategories within community benefit, both of which let us test whether nonprofits respond distinctively, rather than characterize the nonprofit decline in isolation.

A third literature concerns the notion that hospitals function as residual insurers of last resort, absorbing costs when local insurance coverage is incomplete and reallocating resources when uncompensated-care burdens rise. In a prominent contribution, Garthwaite et al. (2018) estimates a large marginal uncompensated-care burden associated with increases in local uninsurance, which implies that a higher uninsured share mechanically compresses hospital margins absent offsetting subsidies or pricing responses. Related work shows that supply-side shocks, such as nearby hospital closures, can further concentrate uncompensated-care demand onto remaining providers, intensifying financial pressure and potentially inducing changes in service provision and spending patterns (Hodgson et al., 2015; Song and Saghafian, 2019; Carroll and Chang, 2025). This literature predicts that a coverage expansion that reduces uninsurance should relax an important financial constraint faced by hospitals with first-order implications for margins and for the allocation of resources away from uncompensated care. However, these papers do not examine whether the last-resort role is intrinsic or demand-conditional.

All three groups of literature circle the question of whether nonprofit ownership produces behavior distinct from that of for-profit hospitals. ACA expansion and the IRS 990 Schedule H allow us to ask whether the changes we observe reflect a mechanical equilibrium response to expansion or a persistent mission commitment. We identify the ownership effect cleanly by comparing nonprofit hospitals to for-profit hospitals under a common shock. We then examine other categories of community benefit to determine where freed resources are allocated along the spectrum from direct patient aid to institution-building investments that a for-profit would likely also make. These let us assess whether nonprofits' safety-net role is an intrinsic commitment or merely conditional on need.

3 Conceptual Framework

To formalize the mechanism tested here, we sketch a two-type hospital economy. The aim is to isolate financial responses that would differentiate hospitals of differing ownership statuses and, among nonprofit hospitals, the extent to which these decisions evince mission commitment: on one end is direct patient aid, while institutional investments are on the other end. This gradient is relevant to the mandates placed on nonprofit hospitals in exchange for their tax-exempt status.

To begin, define two ownership types for hospital j, where $j \in \{\text{FP}, \text{NP}\}$. Within hospital j, discharges are by payer: either private Q_p, Medicaid Q_m, or charity care for the uninsured Q^c. Revenues differ by discharge type with prices for the privately insured at P_p, for Medicaid recipients at P_m, and 0 for the uninsured. Total discharges are $Q \equiv Q_p + Q_m + Q^c$, and $C(Q)$ is the cost of treating them. Net income is revenue less cost, $\pi = P_p Q_p + P_m Q_m - C(Q)$, where charity discharges Q^c enter cost but earn no revenue. The net income depends on the composition of discharges across types at their respective prices less the costs to the hospital of those discharges: $\pi = P_p Q_p + P_m Q_m - C(Q)$.

Assume the for-profit hospitals simply maximize profits. Nonprofit hospitals maximize $U = \pi + \alpha D$, where D is spending directed at direct patient need, which couples both charity care and subsidized services, and $\alpha \geq 0$ is the board's mission weight. Assume nonprofit hospitals also receive T, a transfer via tax exemptions. The policy benchmark is whether $D \geq T$, or direct patient benefit is at least the value of their tax exemption to the public. "Community benefit" is a larger IRS tax category that includes institutional investments,

such as professional development, research and education, and capital investments. While these may benefit a hospital's community, we adopt a stricter benchmark since the aforementioned benefits are less tangible, and so less obvious evidence of a hospital's mission commitment.

Medicaid expansion converts fraction $\phi \in (0,1)$ of charity discharges to Medicaid, such that $\tilde{Q}^c = (1-\phi)Q^c, \qquad \tilde{Q}_m = Q_m + \phi Q^c, \qquad Q_p$ unchanged. We assume that the number of discharges and the prices remain constant; the composition of discharges shifts. Hospital incomes rise by the additional revenue generated through $W \equiv \tilde{Q}_m P_m - Q_m P_m$. The decline in measured charity care spending can be written, $\Delta S \approx -\bar{c}\phi Q^c$, where $\bar{c}$ is the average cost per charity discharge. This decline is mechanically due to the shift in covered patients. The shock is common to both hospital ownership types.

Hospitals allocate the windfall W from the shock between two uses: direct patient need D or private-return uses R. Direct patient need involves care for the uninsured or underinsured through charity care or subsidized care. Private-return uses are either retained surplus or institutional-building investments (education, research, capital), both of which are available across hospital types. Private-return uses yield the same marginal value ρ to both types. For-profit hospitals will allocate W to R. Nonprofit hospitals allocate W to D iff $\alpha > \rho$, else they mimic the for-profit hospitals.

The analysis that follows thus examines whether nonprofit hospitals' safety-net role is an intrinsic commitment ($\alpha > \rho$) or merely demand-conditional and firm-like ($\alpha \leq \rho$). We address this question jointly: first by estimating the contrast between ownership types, and then by examining the within-nonprofit gradient. Both are required because the within-ownership charity decline is ambiguous on its own: the windfall could be redirected to subsidized services or to other community-benefit categories that satisfy the exemption without directly aiding patients. The latter category satisfies the legal conditions for tax exemption.

4 Data

We draw on two hospital panels, related in subject but separate in construction. The first is a hospital–year panel of Medicare Cost Report filings covering all ownership types, on which we estimate the ownership contrast. The second is a filer–year panel of IRS Form 990 Schedule H returns covering nonprofit organizations only, on which we test where freed

resources move along the community-benefit gradient of Section 3. This section describes each source, the sample each analysis uses, and the outcomes and covariates the estimates are built on. Variable derivations, the Schedule H category anatomy, and the cleaning rules are in Appendix A.1.

4.1 Sources

Our primary source is the National Academy for State Health Policy's (NASHP) aggregation of Medicare Cost Reports, filed annually with the Centers for Medicare & Medicaid Services on Form CMS-2552-10. All Medicare-participating hospitals must file, and the reports carry the charity-care, bad-debt, operating-expense, payer-mix, and capacity measures our outcomes are built from. Ownership is classified from the Provider Control Type Code. Two limits bear on interpretation. Reported charity care varies with institutional accounting practice and omits informal, non-billed uncompensated care. And the Cost Reports record no community-benefit spending outside charity care and bad debt, which is why the reallocation question requires a second source.

The second source is IRS Form 990 Schedule H, parsed and hosted by the National Center for Charitable Statistics and filed by nonprofit hospital organizations since tax year 2008. Schedule H is the only nationally consistent, category-level record of nonprofit community-benefit spending, which enables us to observe the gradient of service provision. Its limits are structural: filings are self-reported and subject to differing interpretations of the Schedule H definitions; coverage extends only to e-filers; and reporting occurs at the tax-filer (EIN) level rather than the facility level, so activity at multi-hospital systems is blurred. We address the last by restricting the panel to organizations operating a single hospital facility.

The two panels are not merged. No official crosswalk maps Employer Identification Numbers to CMS Certification Numbers; matches can be built by hospital name and address, as others have done for cross-sectional comparisons (Herring et al., 2018; Bai et al., 2021), but we do not construct one, because neither test in this paper requires it. The ownership contrast is estimated on the hospital–year panel, where for-profit hospitals are observed; the reallocation gradient on the filer–year panel, where only nonprofits file. No hospital is matched across the two, and no inference in this paper requires it.

4.2 The estimation samples

We begin with all hospitals in the NASHP release for 2011–2023 ($N = 5{,}119$). Because the design requires a consistent unit observed over the full window, we retain hospitals present in every year, which yields a balanced panel of 4,012; we then drop hospitals with incomplete key financial or demographic values (3,991); and finally, because the ownership comparison requires a stable ownership assignment, we remove hospitals that change ownership type during the period. The analytic panel is 3,495 hospitals: 2,203 nonprofit and 503 for-profit. Every hospital is observed in each of the thirteen years.

The main results compare nonprofit and for-profit hospitals, excluding public hospitals. One reason for this exclusion is that a public hospital has neither a profit motive to mimic nor a tax exemption to justify. Its charity care is financed by appropriation and local subsidy rather than out of an operating margin, and so does not face the allocation decision conceptualized above. Moreover, public hospitals' exit from the balanced filing panel is itself strongly related to expansion (hazard ratio 0.20, $p < 0.001$, see Table V): their exit masks the elevated nonprofit hazard in the pooled figure. Still, whether public ownership sustains charity care better than nonprofit or for-profit hospitals is the natural question when distinguishing whether the tax exemption or the ownership type drives the results. Either the tax exemption or ownership itself may be the remedy, and examining the response of public hospitals allows us to answer this question.

The IRS 990, Schedule H, panel comprises 1,250 single-facility nonprofit filers observed over tax years 2011–2022, or 14,063 filer–years. We restrict filers in the fifty states and the District of Columbia, retain the latest return when a filer–year appears more than once, and exclude filers with more than two missing years between their first and last filings, or fewer than ten filings in total. Appendix A.1 gives the remaining construction rules.

Both samples are restricted in two further ways. First, 2020 is excluded from every estimate: the pandemic year alters the payer-mix and expenditures for reasons unrelated to expansion. Second, we retain as treated only those adoption cohorts observed at least three periods before adoption and four periods from adoption onwards, which is what the group–time estimator requires to identify a pre-trend and a four-year post-period. In the NASHP-based sample, these restrictions result in the 2014, 2015, 2016, 2017, and 2019 treated cohorts; in the shorter Form 990 panel, 2014, 2015, and 2016 are used. One implication is that, since our timing cohorts are skewed toward states that expand Medicaid closer to the

Affordable Care Act, we estimate the effect of early rather than later adopters.

In our main estimates, hospitals in states adopting after our timing cohorts are assigned to the untreated group. In the NASHP panel 487 hospitals (245 nonprofits) join the 884 hospitals (410 nonprofits) in states that never adopt. The preferred specification identifies from not-yet-treated expanders and drops untreated units so that these hospitals do not enter. The inclusive specification uses the untreated group as controls as well. Three of the five later timing cohorts adopt within the observation window. We report both specifications throughout, and Appendix Tables A.II and A.III locate the divergence between them. Appendix Table A.VIII re-estimates such that each late cohort includes only pre-treatment observations so that those hospitals serve as not-yet-treated controls. Section 7 discusses these results.

Two reasons favor narrowing the control group to exclude the later timing cohorts. First, states adopting after 2019 did so under fiscal and political conditions unlike those of the 2014–2016 wave, so their untreated trajectory is a weaker counterfactual for early adopters than a not-yet-treated cohort is, and Table I shows they are not interchangeable with never-adopting states as they differ more from them than either differs from the treated group on three of five measures: on the Medicaid payer mix, hospitals in in-window states are distinct from both other groups (normalized differences of 0.41 and 0.43) while those two are indistinguishable from each other (0.01); on the charity-care payer mix after-window states are the lowest (0.027) and never-adopting states the highest (0.037), with in-window states between them, so the largest gap in the table (-0.36) is the one between the two untreated groups rather than either gap involving the treated. Reporting the untreated hospitals in a single column would average that gap away and report the result as balance. Second, later cohorts' post-treatment years fall primarily between 2021 and 2023, the years in which the pandemic's effects on payer mix and utilization are least separable from expansion's effects. We therefore report the exclusive specification as the headline beside the inclusive specification.

Requiring a hospital's presence in every year conditions the panel on survival, which is a post-treatment outcome if expansion itself affects hospital entry or exit. We test this by estimating the hazard ratio and find that the nonprofit exit hazard is elevated (1.98, $p = 0.025$) even though the pooled hazard is not. To remedy, we report differential-attrition tests, inverse-probability weighting, and Lee et al. (2009)'s trimming bounds in Table V and

discuss these results in Section 7.

The 990 Schedule H outcomes are constructed to match the gradient between direct patient aid and private return: net subsidized services and community health improvement are direct patient aid (s^D); health-professions education, research, physical improvements, economic development, and workforce development together are private-return uses (s^R).

Table I reports pre-treatment (2011-2013) means for the 2,203 nonprofit hospitals in the analytic panel. The 1,548 hospitals in states adopting inside the identifying window are separated from the 245 in states adopting outside it and the 410 hospitals in states that never adopt. We calculate the normalized differences between these groups for evidence of selection effects between them. On the Medicaid payer mix, the margin the policy mechanically changes, hospitals in in-window states are distinct from both other groups (normalized differences of 0.41 and 0.43). States that adopt outside the identifying window do not differ in Medicaid payer mix from states that never adopt. For charity care payer mix and poverty rates, states that adopt after the window contrast with states that never adopt, while the other comparisons do not substantively differ. States that adopt after the window again differ from the other groups with respect to unemployment. These patterns motivate our preference for not-yet-treated expanders as the primary comparison group. Appendix Table A.II shows the imbalance against never-adopting states is concentrated in the for-profit sector, where those hospitals face far higher local uninsurance.

4.3 Covariate Selection

The adjusted specifications condition on four county-level covariates from the American Community Survey five-year estimates: the uninsured rate, the unemployment rate, the nonwhite population share, and median household income. They were chosen for their clear interpretation and their direct links to hospitals' financial environment and to states' expansion timing, and every difference-in-differences table reports estimates with and without them. Appendix A.1 describes the linkage.[7]

[7]Before estimation we screened nineteen hospital and county characteristics with two cross-validated LASSO regressions on pre-expansion data, each repeated over 100 fold assignments. The screens select sparsely and inconsistently: for nonprofit charity-care payer mix they retain the county high-school completion rate and nonwhite share, for for-profits nothing, and for adoption timing no main effect in 98 of 100 draws. We keep the fixed set of four because the headline is insensitive to the choice (-0.0089 with no covariates, -0.0107 with the four, -0.0095 with the screened pair) and because a set that differs by outcome

5 Methods

Our empirical strategy has two parts. The first part estimates, separately for nonprofit and for-profit hospitals, the effect of Medicaid expansion on charity care, uncompensated care, payer mix, and margins, using Callaway and Sant'Anna (2021)'s staggered adoption framework with the doubly robust score of Sant'Anna and Zhao (2020). These within-ownership estimates establish how large the shock was, how much of the response was mechanical, and whether the windfall shows up in the margins. The second part contrasts ownership responses, which examines α-versus-ρ, or whether the intrinsic commitment of nonprofit hospitals overcomes demand-conditioned spending. We estimate the nonprofit effect minus the effect the same hospitals would have had under for-profit ownership, following Caron (2025). Since the causal estimand assumes observable subgroup selection, that is, that our covariates account for all variables contributing to nonprofit hospital ownership's sensitivity to expansion, we report the difference between the nonprofit and for-profit effect beside it. The within-nonprofit estimator is then applied to the IRS 990 Schedule H panel to estimate the reallocation gradient.

5.1 Within-Ownership Effects

To define the causal estimand for within-ownership effects, let G_i denote the year in which hospital i's state first implemented expansion, let $Q_i \in \{1, 0\}$ denote nonprofit or for-profit ownership, which is fixed in the panel, let X_i denote the four county-level covariates described below, and let Y_{it}^k be outcome k in year t. Retained timing cohorts are observed at least three years before and four years from adoption in the 2011-2023 window with 2020 removed: in the NASHP data, 2014, 2015, 2016, 2017, and 2019 timing cohorts; in the 990 Schedule H dataset, 2014, 2015, and 2016. Hospitals in a state that adopted outside that window or in a state that never adopted is coded as untreated. Government hospitals are excluded from our main analyses. Periods are indexed consecutively with 2020 omitted.

For ownership type q, let $Y_{it}^k(g)$ denote the potential outcome if hospital i first faced expansion in year g so that $Y_{it}^k(1)$ and $Y_{it}^k(0)$ are the treated and untreated potential outcome,

and by ownership type would make the ownership contrast a difference between two differently specified arms.

respectively. The cohort-time average treatment effect on the treated is

$$ATT_q^k(g,t) \equiv \mathbb{E}\big[Y_{it}^k(g) - Y_{it}^k(0) \mid G_i = g,\ Q_i = q\big], \qquad t \geq g, \tag{1}$$

the effect of expansion for hospitals of type q in cohort g observed in year t.

Identification of $ATT_q^k(g,t)$ depends on the following assumptions (Callaway and Sant'Anna, 2021). First, adoption is staggered and absorbing: once a state expands, it remains expanded. Second, there is no anticipation, so that $Y_{it}^k = Y_{it}^k(0)$ for every $t < G_i$. The estimator is run with zero anticipation and the pre-expansion dynamic estimates serve as a diagnostic. Third, untreated trends are parallel, conditional on X_i, between treated hospitals and hospitals of the same ownership type that have not expanded. Writing $C_{g,t} = 1\{G_i > t\}$ for the not-yet-treated comparison group,

$$\begin{aligned}&\mathbb{E}\big[Y_{it}^k(0) - Y_{i,g-1}^k(0) \mid G_i = g,\ Q_i = q,\ X_i\big] \\ &\quad = \mathbb{E}\big[Y_{it}^k(0) - Y_{i,g-1}^k(0) \mid C_{g,t} = 1,\ Q_i = q,\ X_i\big], \qquad t \geq g.\end{aligned} \tag{2}$$

This parallel trends assumption is the weakest among staggered difference-in-difference strategies because it only requires that trends be parallel relative to the year prior to treatment. Equation 2 shows our main specification, the exclusive specification, which uses not-yet-treated states to estimate the counterfactual. We compare these estimates with a specification that includes states that never expand, called the inclusive specification below. A fourth assumption for identification is overlap: within each (g,t) cell the conditional probability of belonging to cohort g is bounded away from one. With covariates and a thin cohort cell the propensity model can separate perfectly, in which case the doubly robust estimator is not identified. This separation occurs for the government arm with controls (see Appendix Table A.IX, where the column is dropped). The final assumption is no interference: a hospital's potential outcomes do not depend on another state's expansion timing.

We estimate $ATT_q^k(g,t)$ with the doubly robust score of Sant'Anna and Zhao (2020). Let $\Delta Y_{it}^k \equiv Y_{it}^k - Y_{i,g-1}^k$, let $G_{ig} \equiv 1\{G_i = g\}$, and define

$$\begin{aligned}p_g(X_i) &\equiv \Pr(G_i = g \mid G_i = g \text{ or } C_{g,t} = 1,\ Q_i = q,\ X_i), \\ m_{g,t}^k(X_i) &\equiv \mathbb{E}\big[\Delta Y_{it}^k \mid C_{g,t} = 1,\ Q_i = q,\ X_i\big].\end{aligned} \tag{3}$$

Then

$$ATT^k_{q,dr}(g,t) = \mathbb{E}\left[\left(\frac{G_{ig}}{\mathbb{E}[G_{ig}]} - \frac{\frac{p_g(X_i)C_{g,t}}{1-p_g(X_i)}}{\mathbb{E}\left[\frac{p_g(X_i)C_{g,t}}{1-p_g(X_i)}\right]}\right)\left(\Delta Y^k_{it} - m^k_{g,t}(X_i)\right)\right]. \tag{4}$$

The estimator is consistent if either the propensity score or the comparison-group outcome regression is correctly specified, and efficient when both are. The propensity score is a logit and the outcome regression is linear; both are fit within the (g,t) cell. The covariates are standardized within each estimation sample. These covariates are taken at their pre-expansion values: for every post-treatment cell the design matrix is built from the base period $g-1$, so no post-treatment value enters the estimator. The base period also varies: post-treatment effects are measured from $g-1$, while each pretreatment estimate compares a year with the year before it.

The cohort-time effects are aggregated in two ways. The overall effect reported in every results table averages $ATT^k_q(g,t)$ over each retained cohort's four post-adoption periods, $e = t-g \in \{0,1,2,3\}$, with cohort-size weights. The dynamic estimates (or event-study-style results) average over cohorts at each horizon $e \in \{-3,\ldots,3\}$, again with cohort-size weights.

Since expansion is assigned at the state level, all inference clusters by state. Standard errors are calculated by a multiplier bootstrap on the influence function with Mammen two-point weights and 2,000 draws. The standard error is taken as the interquartile range of the draws divided by 1.349 so that a few extreme draws cannot inflate it. One set of draws serves every estimate in a table. Dynamic results report simultaneous sup-t bands from the same draws. Three diagnostics accompany the estimates. Table II reports a joint Wald test that the three pre-expansion leads are zero. These tests are computed twice: under the conventional assumption that the leads are independent, and using the covariance of the bootstrap draws across leads. Figure II reports the relative-magnitude sensitivity bounds of Rambachan and Roth (2023) for the headline estimate. Pre-treatment estimates are also reported as diagnostics for the parallel trends assumption, not as tests of them (Roth, 2022).

5.2 The Ownership Contrast

Whether expansion altered hospital behavior within each ownership type allows us to examine how significant Medicaid coverage expanded, how much of the expansion was mechanical, and whether the windfall shows up in the margins. The next question is whether ownership itself shapes how hospitals use the fiscal relief of an expanded safety net. Two estimands bear on the latter question.

The first estimand is the difference in the two ownership-specific effects,

$$DATT^k(g,t) \equiv ATT_1^k(g,t) - ATT_0^k(g,t), \tag{5}$$

each arm identified under the conditional parallel-trends assumption of equation (2) within its own ownership type. Since for-profit hospitals are affected by expansion, a conventional triple difference design will not recover the nonprofit effect (Ortiz-Villavicencio and Sant'Anna, 2025; Caron, 2025). Separately, the difference in equation 5 is a difference in responses, not the causal effect of ownership: Each arm is an average over its own population, so the difference compares the nonprofit response at nonprofits' characteristics with the for-profit response at for-profits'; and where hospitals with a given size, system affiliation, or payer mix respond differently to expansion regardless of their ownership, that heterogeneity is correlated with ownership and enters the difference (Caron, 2025).

The second estimand, and the one we report in our main results, is the causal contrast of Caron (2025). Let $Y_{it}(g;q)$ denote the potential outcome under expansion cohort g and ownership q. Then

$$\begin{aligned} CDATT^k(g,t) \equiv \mathbb{E}\big[Y_{it}^k(g;1) - Y_{it}^k(0;1) \mid G_i = g,\ Q_i = 1\big] \\ - \mathbb{E}\big[Y_{it}^k(g;0) - Y_{it}^k(0;0) \mid G_i = g,\ Q_i = 1\big]. \end{aligned} \tag{6}$$

is the effect of expansion on hospitals that are nonprofits, minus the effect those same hospitals would have witnessed under for-profit ownership. The first term is the estimated nonprofit effect; the second is the counterfactual that is never observed for any nonprofit hospital.

Following Caron (2025)'s Assumption 7, we assume observable subgroup selection: conditional on the covariates X_i, the response a hospital would have under for-profit ownership

does not depend on whether the hospital is nonprofit or for-profit. Formally,

$$\begin{aligned}\mathbb{E}\big[Y_{it}^{k}(g;0) - Y_{it}^{k}(0;0) \mid G_i = g,\ Q_i = 1,\ X_i\big] \\ = \mathbb{E}\big[Y_{it}^{k}(g;0) - Y_{it}^{k}(0;0) \mid G_i = g,\ Q_i = 0,\ X_i\big]\,.\end{aligned} \tag{7}$$

This assumption is only as credible as the covariates it conditions on, so X_i is wider here than in the within-ownership estimates. The prior covariates hold local-market conditions fixed but nothing about the hospital itself, and the channels through which ownership types differ run through the hospital: for-profit hospitals are smaller, more often system-affiliated, and carry a different baseline payer mix. The causal contrast thus conditions on three additional pre-treatment hospital characteristics measured at the period prior to treatment, $g-1$: log bed size, system membership, and the Medicaid payer mix. We only include three because the comparison for-profit cells are small and each added regressor costs a degree of freedom in an outcome regression fit within that group.

The observable subgroup selection assumption entails that sorting into nonprofit status is unrelated to a hospital's sensitivity to expansion once the covariates are held fixed. It allows nonprofit and for-profit hospitals to differ in their responses, in their covariates, and in their outcomes; it rules out the difference in the for-profit-type response between the two groups that the covariates do not capture. While the assumption is untestable, its sensitivity to the conditioning set is testable and Appendix Table A.V reports the contrast between the county covariates alone and the expanded set of covariates that include hospital characteristics. The two estimands relate by

$$\begin{aligned}DATT(g,t) = CDATT(g,t) + \big(&\mathbb{E}[Y_{it}(g;0) - Y_{it}(0;0) \mid G_i = g,\ Q_i = 1] \\ &- \mathbb{E}[Y_{it}(g;0) - Y_{it}(0;0) \mid G_i = g,\ Q_i = 0]\big),\end{aligned} \tag{8}$$

and the second term is the selection term, which equation (7) sets to zero conditional on X_i. Reporting the difference in effects beside the causal contrast shows the observed difference in responses the assumption attributes to ownership. Appendix Table A.V reports the selection term itself, with its standard error, on the same cells.

With $N_i = 1\{Q_i = 1\}$, $F_i = 1 - N_i$, and $C_{g,t}$ the comparison group, four joint propensities $\pi_{g1}(X_i) = \Pr(G_i = g, N_i = 1 \mid X_i)$, π_{g0}, π_{c1}, and π_{c0} are estimated by a multinomial logit over the four cells, and three outcome regressions are estimated linearly in each population:

$m_c^1(X_i)$ for comparison nonprofits, $m_g^0(X_i)$ for treated for-profits, and $m_c^0(X_i)$ for comparison for-profits. Treated for-profits and both comparison groups are reweighted to the covariate distribution of treated nonprofits by the odds π_{g1}/π_{g0}, π_{g1}/π_{c1}, and π_{g1}/π_{c0}, and the doubly robust score

$$\begin{aligned} CDATT_{dr}(g,t) = \mathbb{E}\big[w_1\big\{\Delta Y_{it} - m_c^1(X_i) - \big(m_g^0(X_i) - m_c^0(X_i)\big)\big\} \\ - w_3\big\{\Delta Y_{it} - m_c^1(X_i)\big\} - w_2\big\{\Delta Y_{it} - m_g^0(X_i)\big\} + w_4\big\{\Delta Y_{it} - m_c^0(X_i)\big\}\big], \end{aligned} \tag{9}$$

with w_1 the treated-nonprofit indicator and w_2, w_3, w_4 the normalized reweighting terms for treated for-profits, comparison nonprofits, and comparison for-profits. The estimator is consistent if the joint propensity model is correctly specified or if the three outcome regressions are, and efficient when both are (Caron, 2025).[8] The contrast is aggregated with treated-nonprofit weights over the cells in which all four groups are observed. Appendix Table A.VI reports the contrast cell by cell, a contrast we return to in Section 6.

Standard errors for the ownership contrast, the difference in effects, and the selection term come from the same state-clustered multiplier bootstrap as the within-ownership estimates (Callaway and Sant'Anna, 2021), with a single set of draws applied to every arm. Because the nonprofit and for-profit arms share cohorts, covariates, and clusters, their estimation errors are correlated; the joint procedure preserves that covariance, which the analytic $\sqrt{SE_{NP}^2 + SE_{FP}^2}$ would discard.

5.3 The Reallocation gradient

The IRS "community benefit" category is wider than charity care and nonprofit hospitals may reallocate slack funds to other community benefits. Our reallocation test applies the within-nonprofit estimator of equation (4) to the 990 Schedule H panel, one category at a time, with the categories arranged by the partition between direct patient need and private return uses (Section 3). Each outcome is $\log(1+\$)$. Since several categories have substantial mass at zero, these coefficients are not scale-free proportional effects and are not read as percentage changes (Chen and Roth, 2024). Appendix Table A.I re-estimates every row under the inverse hyperbolic sine and as a linear probability model for any reporting, which agree with the log specification in sign and locate the margin on which each category moves. The

[8] The weighting-only and regression-only versions are reported in Appendix Table A.V.

retained cohorts are 2014, 2015, and 2016, and we mirror the split comparison group between not-yet-treated filers in expansion states and never-treated states from before. Inference is the state-clustered multiplier bootstrap, described above.

6 Results

To determine whether nonprofit hospitals' safety-net role reflects an intrinsic commitment or demand-conditional behavior, we report the within-ownership effects, then the ownership contrast, then the reallocation gradient. The headline finding is a split verdict. On charity care itself, nonprofit hospitals kept more than the same hospitals would have under for-profit ownership, that is, about a quarter of the pre-expansion level, under the observable-selection assumption of Section 5. On the resources expansion freed, the evidence divides by category and by comparison group: the pre-committed mission quantity, net subsidized services, does not move in either design, while in the not-yet-treated comparison both a direct-aid category, community health improvement, and the private-return uses of education, research, and development rise, and in the inclusive comparison nothing does. On the gradient, then, nonprofits' commitment is not the unambiguous one their retention of charity care suggests.

6.1 Within-ownership estimates

Medicaid expansion reduced nonprofit hospitals' charity care by roughly a third: the charity-care payer mix falls by 0.9 to 1.1 percentage points against a pre-expansion mean of 3.1 (Table II, Panel A), in every sample and specification, with flat pre-expansion leads. The decline is no larger than coverage substitution can account for: the Medicaid payer mix rises by 2.9 to 3.8 points over the same columns, while the Medicare and commercial mixes, which expansion should not touch, do not move (Panel B). The windfall does not surface as profit: net margins rise by 1.7 points, an estimate below what the design can detect, and log net revenue fails its pre-trend test and cannot carry the margin question (Panel C). We take the three panels in turn.

Every uncompensated-care estimate in Table II, Panel A, is precisely estimated (all $p < 0.01$), and the joint Wald tests of the three pre-expansion leads do not reject in any specification. The charity-care payer mix falls by 0.89 to 1.08 percentage points, 29 to 35

percent of its pre-expansion mean, and charity-care cost as a share of net patient revenue by 0.96 to 1.55 points, 39 to 64 percent. Both measure care the hospital provided without expecting payment. Bad debt and uninsured cost, by contrast, is the amount the hospital billed and did not collect; it falls by 0.33 to 0.63 points, 13 to 24 percent, in the same specifications. Summed within each specification, the two cost shares fall by 1.29 to 2.08 points of net revenue, 26 to 41 percent of their combined base. The pattern matches the prior literature on expansion and uncompensated care (Nikpay et al., 2015; Dranove et al., 2016; Sachs, 2019; Moran, 2025), and it is the one the framework predicts for a coverage shock regardless of ownership.

Panel B examines whether the coverage shock reached nonprofit hospitals and whether it displaced other payers. The Medicaid payer mix rises by 2.9 to 3.8 percentage points, 19 to 26 percent of its pre-expansion mean, in every specification (all $p < 0.001$). The rise exceeds the charity-care decline of Panel A by two to three points, so the movement of formerly uninsured patients onto Medicaid is more than sufficient to account for that decline. The two placebo mixes are null in the covariate-adjusted specifications: Medicare moves by -0.4 and $+0.7$ points and commercial by -1.2 and -0.2, with p-values between 0.30 and 0.80. Two unadjusted cells reject at the 1 percent level, Medicare with not-yet-treated controls (-0.7 points) and commercial with never-treated controls (-0.8). They disappear once covariates enter.

Panel C asks whether the windfall is retained as surplus. Net margins rise by 1.7 percentage points in the pre-committed specification (column (2); $p = 0.021$; 95 percent interval 0.3 to 3.2 points), 35 percent of the 4.9-point base, and by 1.1 points with never-treated controls (column (4); $p = 0.099$); the two unadjusted columns show no change. The evidence for a retained margin is confined to the covariate-adjusted specifications and is imprecise where it appears. Against the 2.1 points of net revenue that the same specification frees from charity care and bad debt, the interval runs from about a seventh of that cost retained as margin to more than the whole of it. The panel thus bounds the retained share without fixing it. The two subsections that follow ask where the remainder went: whether ownership changed how much charity care was kept, and which community-benefit categories absorbed what

was not.[9]

6.2 Ownership effect estimates

Under the observable-subgroup-selection assumption of Section 5, ownership shaped how far charity care fell: nonprofit hospitals reduced their charity-care payer mix by 0.8 percentage points less than the same hospitals would have under for-profit ownership (Table III, Panel D, column 3; 95 percent interval 0.3 to 1.3), about a quarter of the pre-expansion level. The result is confined to charity care: the contrast on charity-care cost is smaller and imprecise, and on bad debt it is zero. The plain difference between the nonprofit and for-profit effects (Panel C), which makes no such assumption, is positive in three of the four specifications. We take the within-ownership effects first (Panels A and B), then the two contrasts.

Panel B reports the within-ownership estimates for for-profit hospitals, with the nonprofit estimates of Table II repeated in Panel A for reference. For-profit hospitals entered the period with less charity care than nonprofits: a charity-care payer mix of 2.5 percent against 3.1 (17 percent lower), charity-care cost of 2.1 percent of net revenue against 2.4 (12 percent lower), and bad debt 1.5 percent higher. Their charity-care payer mix falls by 1.26 to 2.13 percentage points (50 to 84 percent) in the three specifications that reject zero (all $p < 0.001$). The specification with the exclusive sample and covariates gives -0.71 points (28 percent; $p = 0.203$) with a standard error nearly three times the unadjusted one; it is the cell in which the four-covariate propensity model separates on 208 for-profit hospitals, and we set it aside. Charity-care cost falls by 1.07 to 2.02 points (50 to 94 percent) and rejects zero in all four specifications ($p = 0.043$ with not-yet-treated controls and covariates, $p < 0.001$ otherwise). Bad debt and uninsured cost falls by 0.49 to 0.84 points (19 to 32 percent), precisely without covariates ($p < 0.001$) and imprecisely with them ($p = 0.114$ and 0.293). In percentage points, then, the for-profit declines match or exceed the nonprofit ones despite the lower for-profit base, so in proportional terms for-profits cut charity care by more. Were expansion's effect purely the mechanical conversion of charity patients to Medicaid, common to both types, each type's decline would scale with its charity exposure; that it does not is

[9]Log net patient revenue turns out to be unrevealing. The only cell that rejects, a 2.3 percent decline with not-yet-treated controls and no covariates ($p = 0.008$), is also the one that fails both pre-trend tests, and the covariate-adjusted cells have standard errors of 9 to 11 log points.

the gap the next two panels take up, first as the raw difference in effects and then as the part of that difference attributable to ownership once the hospitals' observable differences are held fixed.

Panel C differences the two within-ownership estimates. The difference is not the causal effect of ownership, for two reasons. Each arm is averaged over its own population, so the difference compares the nonprofit response at nonprofits' characteristics with the for-profit response at for-profits'. And any sensitivity to expansion that runs through those characteristics rather than through ownership enters the difference as if it were an ownership effect. Read as a description, the raw gap is positive in eight of twelve cells, which means the nonprofit decline is the smaller one, and rejects zero at the 5 percent level in four, all with never-treated controls: charity-care payer mix by 0.78 and 1.10 percentage points (25 and 36 percent; $p = 0.024$ and 0.032), charity-care cost by 0.80 points (33 percent; $p = 0.046$), and bad debt by 0.51 points (20 percent; $p = 0.003$). The four negative cells are the three outcomes in the preferred specification, column 2, none of which rejects (charity-care payer mix -0.36 points, $p = 0.509$), and bad debt with never-treated controls and covariates, which is zero. Column (2) inherits the for-profit cell set aside above, in which the covariate-adjusted propensity model separates, so its null is a property of that arm rather than evidence that ownership made no difference. The panel suggests that nonprofits' charity care decline is smaller, but not by how much, nor can these estimates isolate how much of the gap is ownership rather than characteristics correlated with ownership.

Panel D asks the ownership question directly: how much charity care would nonprofit hospitals have provided had they been for-profit? The answer rests on the observable-subgroup-selection assumption of Section 5, conditional on the seven covariates, which is why the panel has no unadjusted columns. Every contrast but bad debt with not-yet-treated controls is positive. Nonprofits' charity-care payer mix is 0.82 and 0.78 percentage points higher (27 and 25 percent) than it would have been under for-profit ownership ($p < 0.001$ and $p = 0.057$), and their charity-care cost 0.58 and 0.48 points higher (24 and 20 percent), though only marginally in the first case ($p = 0.074$) and not in the second. Bad debt and uninsured cost shows no contrast and switches sign. On the same cells and covariates, the difference in responses is 0.89 and 0.98 points, so the selection term the assumption removes is 0.07 and 0.20 points and not distinguishable from zero (Appendix Table A.V). Nearly all of the observed gap is attributed to ownership. The contrast is not evenly spread:

Appendix Table A.VI shows that the 2014 cohort carries 86 percent of its weight, that it is zero in the adoption year and grows to 1.6 points by the third year after, and that it lies between 0.50 and 0.97 points under every leave-one-state-out re-estimation. So while part of the windfall appears in net margins, nonprofit hospitals also kept more charity care than the same hospitals would have as for-profits. That is evidence of mission commitment on the charity-care margin. What remains to be seen is whether the resources the mechanical decline freed were reallocated within community benefit, and to which categories.

6.3 Reallocation estimates

On the Schedule H panel the mission test is null: net subsidized services do not move in either design (Table IV). Whether any community-benefit category rose depends on the comparison group. With not-yet-treated filers as controls, spending on health-professions education and on community health improvement services rises; with never-adopting filers included, no category does. The freed resources therefore do not reappear as direct patient aid and the panel does not locate where the remainder went. Retention at the upper end of the margin interval in Table II is one possibility that the two results leave open.

We favor the unadjusted columns on the Schedule H panel for three reasons. First, the LASSO screens of Appendix Table A.XVI run on the Schedule H outcomes, select none of the covariates for charity care, health professions education, and community health improvement, so the covariates have no demonstrated claim on these outcomes' pre-expansion trends. The covariates reach the filer panel through a ZIP-to-county link rather than the geocoded county assignment of the hospital panel, which is a cruder attachment. And with not-yet-treated filers as controls the comparison cells are small enough that reweighting on the covariates collapses them: the 43 comparison filers in the 2015 cells carry an effective size of five to nine after weighting with a single filer bearing a third of the weight. This lack of overlap is why Column (3) of Table IV flips signs and inflates magnitudes on rows the other three columns hold near zero. The covariate-adjusted inclusive column does not share the overlap problem but carries standard errors two to three times larger than the unadjusted estimates. Thus we focus below on the unadjusted estimates.

The Schedule H panel corroborates the Cost Report decline on a different measure and sample. Financial assistance at cost falls by 0.57 log points with not-yet-treated controls and

0.49 with never-adopting filers included, roughly 43 and 39 percent (Table IV). Since fewer than two percent of filer-years report zero charity care, this is the one row of the table for which the proportional reading holds. Those figures sit within the 39 to 64 percent decline in charity-care cost as a share of net revenue in Table II and above the 29 to 35 percent decline in the charity-care payer mix, on a panel of single-facility nonprofit filers that shares no hospital with the Cost Report sample by construction. The same question as before returns on this panel: Where were the resources the mechanical decline freed were reallocated? To answer, we partition the Schedule H categories into direct patient aid and private-return uses so that net subsidized services serves as the mission test.

The direct-aid categories give a mixed answer to where the freed resources went. In dollars, subsidized services net of offsetting revenue rise by \$0.19 to \$0.26 million per filer-year with not-yet-treated controls ($p = 0.026$ and 0.004, Appendix Table A.XII), about a fifth of the \$1.1 million decline in charity care, and the increase is among filers already providing such services: the share reporting any is unchanged (Appendix Table A.XI), which is why the $\log(1 + \$)$ estimate in Table IV is null. Gross subsidized expense rises by more (\$1.2 to \$1.4 million) but is matched by offsetting revenue, so most of the added subsidized care was reimbursed, by Medicaid among other payers, rather than absorbed as loss. Community health improvement services gain reporters, 5.6 percentage points of filers, and rise 0.55 log points, but add little in dollars (\$0.02 to \$0.11 million, not distinguishable from zero). The Part II community-building items do not move on any margin.

The private-return categories move on every margin but one. Education, research, and community building rises by 0.32 log points with not-yet-treated controls ($p = 0.006$), an estimate that stays between 0.23 and 0.61 under every leave-one-state-out re-estimation. Since 28 percent of filer-years report nothing in the category, the coefficient is not a proportional effect, and the movement is substantially filers beginning to report: the share reporting any such spending rises 5.2 percentage points (Appendix Table A.XI). In dollars the aggregate rises by \$0.60 million per filer-year on a \$6.50 million base, or 9 percent, and by \$0.17 million once the largest one percent of filer-years is winsorized (Appendix Table A.XII), so most of the dollar increase sits in a few large filers. The increase is human-capital investment. Health-professions education, 70 percent of the aggregate's pre-expansion dollars, rises 0.45 log points ($p = 0.007$; range 0.39 to 0.63 across state drops), gains 5.8 points of reporters, and adds \$0.38 million raw and \$0.13 million winsorized. Research, workforce development,

physical improvements, and economic development are small categories whose log estimates are positive but imprecise (p between 0.10 and 0.22), and whose increases, where present, are almost entirely new reporters of amounts near zero.

How much of the freed charity care reappears elsewhere on Schedule H? Estimating the sum of the non-charity categories as a single outcome (Appendix Table A.XII) gives, with not-yet-treated controls, an increase of $0.39 million per filer-year once the largest one percent of filer-years is winsorized (s.e. 0.12) against a charity-care decline of $1.05 million, an offset of 38 percent with a 95 percent interval of 16 to 59 percent. Without the trim the offset is 84 percent ($0.93 million against $1.11 million) but its interval runs from 15 percent to more than the whole decline because a few large filers carry most of the untrimmed increase. Of the trimmed offset, net subsidized services account for about half ($0.19 million) and education, research, and community building for about two-fifths ($0.17 million). Community health improvement and the Part II items add nothing in dollars. With never-adopting filers as the comparison the same point estimates appear, 18 percent trimmed and 66 percent untrimmed, but neither is distinguishable from zero.

On mission commitment, the direct-aid response is modest: roughly a fifth of the freed charity dollars returns as subsidized care, among filers already providing it, while a comparable share goes to human-capital investment whose benefit to patients is indirect, and the reimbursed expansion of subsidized services is not counted as mission spending because it is not borne as loss. On the windfall, most of it is unaccounted for in other community benefit categories: between two-fifths and five-sixths of the charity decline does not reappear in any Schedule H category on the trimmed estimates, and the decline itself understates the windfall since Medicaid revenue rose on patients who were never charity cases (Table II, Panel B). Where the remainder went—retained margin, whose interval in Table II admits a large share or spending outside the schedule's categories—is a question this panel cannot settle and Section 8 takes up.

7 Robustness & Sensitivity

The preceding section established three results. Nonprofit hospitals reduced charity care by 29 to 35 percent of its pre-expansion level across every sample and specification. Coverage substitution can account for the whole of this decline. Part of the resulting windfall appears

as retained margin, but only in the covariate-adjusted specification and imprecisely: the interval runs from about a seventh of the freed uncompensated-care cost to more than the whole of it. Under the observable-selection assumption, the decline would have been about 0.8 percentage points larger had the same hospitals been for-profit. The windfall does not move cleanly into other community-benefit categories: roughly a fifth of the freed dollars returns as subsidized care, a comparable share goes to human-capital investment, and between two-fifths and five-sixths of the charity care decline appears in no Schedule H category. This section tests the results' assumptions.

Several robustness checks have already been reported. The joint pre-trend tests in Table II do not reject for the uncompensated-care outcomes under either the independence or the covariance-aware form, and reject decisively for log net patient revenue. The Medicare and commercial payer-mix placebos of the same table are null in the covariate-adjusted specifications. The Schedule H estimates carry their leave-one-state-out ranges in Table IV, and Appendix Tables A.XI and A.XII report the extensive margin and the dollar changes behind the log coefficients. The ownership contrast is accompanied by its weighting-only and regression-only versions, the conditioning set that uses county covariates, the selection term, and its cohort-by-event-time decomposition (Tables A.V and A.VI).

7.1 Nonprofit hospitals reduced charity care

The within-ownership estimates rest on parallel trends, which no test can establish: flat pre-expansion leads cohere with the assumption but also with a violation that begins at adoption. Following Rambachan and Roth (2023), we ask how large a post-treatment violation the charity-care result would survive. Figure II bounds the violation relative to the largest deviation observed before treatment. The original 95 percent interval for the charity-care payer mix is -1.51 to -0.71 percentage points, and the robust confidence set first admits zero at $\bar{M} \approx 1.11$: the estimate survives a post-treatment departure from parallel trends larger than any deviation observed in the pre-period, the conventional benchmark in this framework. Appendix Figure A.I imposes the less forgiving smoothness restriction, which extrapolates the estimated pre-trend slope forward. The slope is -0.00063 per period, accounting for about -0.0016 of the -0.0111 effect, and the robust set at exact linear extrapolation is -1.77 to -0.00 points, so the sign survives but the breakdown value is

effectively zero. The reason is not a visible pre-trend but the length of the pre-period: with three pre-expansion event times and one normalized, two coefficients are all a smoothness restriction has to pin down a slope. The within-ownership charity decline is therefore robust to violations of the size actually observed before treatment and fragile to the assumption that a pre-trend can be extrapolated linearly.

Requiring a hospital to file in every year conditions the panel on survival, which is a post-treatment outcome if expansion changed who exits. Table V tests whether this condition influences our result on the unbalanced universe of 4,119 hospitals, or 58,163 hospital-years at risk, with a discrete-time hazard of leaving the filing panel stacked by adoption cohort. The pooled hazard ratio is 0.80 ($p = 0.18$), but it masks an elevated nonprofit exit hazard of 1.98 ($p = 0.025$) behind a sharp governmental decline of 0.20. When the 2015 timing cohort is dropped, the exit hazard falls to 1.59 ($p = 0.15$) and its joint pre-trend test does not reject ($p = 0.53$). Panel B reestimates the ATTs with attrition weights and Lee et al. (2009) trimming bounds. The elevated conditional hazard does not produce differential net attrition as treated nonprofits leave at 15.4 percent against 16.9 percent for comparisons ($p = 0.56$). Attrition weights move the proxy estimate from -0.0142 to -0.0141. Lee et al. (2009) trimming bounds exclude zero (-0.0179 to -0.0162). While the proxy's magnitudes are not comparable to the main results, the within-nonprofit charity decline is not an artifact of who stayed in the panel.

One of the four covariates is a direct outcome of the policy: expansion lowers the county uninsured rate, and the others move with it (Appendix Table A.XIII: the uninsured rate falls 1.3 percentage points, a tenth of its mean, unemployment 0.5 points, and the nonwhite share and median income rise slightly). Conditioning on their post-treatment values would absorb part of the effect, and dropping them would abandon the parallel-trends assumption (Caetano et al., 2026). The main tables take the first remedy Caetano and Callaway propose, conditioning on base-period values, which suffices under a covariate-unconfoundedness condition their pre-test can assess. The test rejects for the uninsured rate ($p = 0.012$) and the nonwhite share ($p = 0.025$) and passes for unemployment and income. Appendix Tables A.XIV and A.XV re-estimate every hospital–year outcome under their second remedy: imputing the covariate path each treated hospital would have followed absent expansion by linear imputation and by a doubly robust estimator with parametric and random-forest nuisance functions. The within-nonprofit charity decline lies between -0.0107 and -0.0112

under every treatment of the covariates. The for-profit estimate does not: it moves from -0.0071 under base-period conditioning to between -0.0135 and -0.0184 under covariate unconfoundedness, and the difference in effects from -0.0036 to between $+0.0036$ and $+0.0317$. The ownership contrast is sensitive to covariates, which we return to in the next subsection.

The four covariates were chosen for interpretability rather than selected by the data, so the results could depend on the choice. Appendix Table A.XVIII re-estimates every outcome that the LASSO screens of Section 4 retained at least one characteristic for, under its screened set, beside the no-covariate and four-covariate columns. Within nonprofits every estimate moves within a standard error of the four-covariate column: charity-care payer mix is -0.0095 (0.0018) against -0.0107. The for-profit charity estimate is the exception, returning to -0.0144 (0.0023) from -0.0071 (0.0056) under the screened set, so that the difference in effects is $+0.0049$ (0.0024) with not-yet-treated controls and $+0.0083$ (0.0032) with never-treated included. Coupled with the previous check, this locates the one negative contrast in the paper, column (3) of Table III, Panel C, in the for-profit arm's four-covariate propensity model, the cell in which that model separates on 208 hospitals. Under every other treatment of the covariates the difference in effects is positive. The causal contrast of Panel D, which reweights with a four-group model and adds the hospital characteristics, does not depend on that cell.

The reduced-form declines are not comparable across outcomes or comparison groups whose coverage take-up differs, and they say nothing about the size of the response per newly covered patient. Appendix Table A.IV scales each uncompensated-care effect by the first-stage effect on the Medicaid payer mix, using expansion as the instrument for coverage. Both legs are estimated on an identical sample with the same multiplier draws, and the interval reported is an Anderson–Rubin set that is valid whatever the strength of the first stage ($F = 36.3$ with not-yet-treated controls, 13.5 with never-treated included). Each percentage point of Medicaid coverage reduces the charity-care payer mix by 0.28 points (interval -0.43 to -0.18), charity-care cost by 0.41 points of net revenue, and bad debt by 0.14. All six sets exclude zero and the never-treated sets are wider but overlap. The ratio adds one assumption to the design: expansion moves uncompensated care only through coverage and the commercial placebo in Table II is its direct test. Under staggered adoption and heterogeneous effects the ratio is a device for putting the reduced form on a per-unit-of-coverage scale rather than a separately identified structural parameter (de Chaisemartin

and D'Haultfœuille, 2018). Because the two payer mixes share a denominator, the first row also has an accounting reading: the charity decline accounts for 28 percent of the Medicaid increase; the remainder comes from commercial, Medicare, and self-pay categories. In other words, Medicaid expansion enrolled substantially more patients than the charity pool contained.

The main results exclude the 789 governmental hospitals, which leaves open whether public ownership sustains charity care better than either private form. Appendix Table A.IX re-estimates the within-ownership effects for government hospitals. Public hospitals cut charity care at least as much as nonprofits: the charity-care payer mix falls by −0.0151 (0.0045) without covariates, −0.0098 (0.0034) with them, and −0.0083 (0.0031) with never-treated controls, on a pre-treatment base of 0.0291, while the Medicaid payer mix rises by 0.034 to 0.053. The governmental arm rests almost entirely on the 2014 cohort, 301 of its 368 retained hospitals, so it is a statement about the first adoption wave. And its covariate-adjusted specification is fragile: the charity-cost estimate swings from −0.062 (0.035) to +0.013 (0.013) with covariates, the unadjusted commercial placebo rejects (−0.038, 0.012), and with never-treated controls and covariates the propensity model separates perfectly in the thin cohort cells, so that column is omitted.

7.2 Nonprofit hospitals reduced charity care less than they would have as for-profits

The difference in ownership effects changes sign between samples, from −0.0036 with not-yet-treated controls to +0.0110 with never-treated states included (Table III, Panel C). This change could reflect a pre-existing ownership gap in never-expansion states rather than a treatment response. Appendix Table A.II shows that never-expansion states differ from expansion states more for for-profit hospitals than for nonprofits and that for-profits are roughly twice as prevalent in never-expansion states. Appendix Table A.III decomposes the two-way fixed-effects analogue of each design to examine whether the divergence is entering through that pretreatment difference: the within-treated comparisons are identical across designs by construction and the treated-versus-never-treated block carries 88.8 percent of the identifying weight for for-profits against 77.0 percent for nonprofits with a much larger for-profit estimate (−0.0247 against −0.0141). A placebo contrast estimated on pre-

treatment years alone is approximately zero under both designs, so the divergence is not a prior gap. The divergence is identified off never-expansion for-profits in high-uninsurance markets, which is the reason we prefer the exclusive sample.

The retention rule assigns cohorts adopting after 2019 to the untreated group, which drops them from the not-yet-treated design and, in the inclusive design, admits 268 nonprofit hospital-years that are in fact post-adoption. Appendix Table A.VIII instead gives each late cohort its true adoption year and keeps only its pre-adoption observations, so those hospitals serve as not-yet-treated controls and never as treated units; the nonprofit sample rises from 1,548 to 1,793 hospitals and the for-profit from 208 to 292. The within-nonprofit charity decline strengthens slightly, to −0.0113 without covariates and −0.0116 with them, and the for-profit decline to −0.0131 and −0.0188. The difference in effects moves from −0.0036 to +0.0072 (0.0072) in the covariate-adjusted column and from +0.0037 to +0.0019 (0.0030) without covariates, a sign change on the covariate-adjusted contrast that leaves neither estimate distinguishable from zero. The causal contrast of Panel D was not re-estimated on this design; its cell structure changes with the control pool, and we report the difference in effects only.

If tax exemption rather than ownership form drives the private contrast, the nonprofit-versus-governmental difference should be smaller than the nonprofit-versus-for-profit one. Appendix Table A.X reports the difference in effects between the nonprofit and public hospitals with the same joint bootstrap as Table III. On the charity-care payer mix it is +0.0062 (0.0035) without covariates, −0.0009 (0.0038) with them, and −0.0025 (0.0031) with never-treated controls: no stable difference in either direction. The two cells that reject, that is, charity-care cost with covariates (−0.0280, 0.0125) and bad debt with covariates (−0.0151, 0.0051), inherit the governmental arm's covariate instability just described. Appendix Table A.VII estimates the causal contrast of Panel D with governmental hospitals as the comparison subgroup, under the same assumption and covariates. The estimate answers a question Table III raises: nonprofit hospitals kept no more charity care than the same hospitals would have under public ownership, −0.0025 (0.0030) with not-yet-treated controls and +0.0002 (0.0033) with never-treated included. The nonprofit retention of charity care documented in Table III is therefore a premium over the profit-maximizing form, not over every alternative to it.

7.3 The freed resources do not reappear as direct patient aid

The Schedule H outcomes are $\log(1 + \$)$ over distributions with substantial mass at zero, and Chen and Roth (2024) show that such coefficients depend on the units the dollars are measured in and are not proportional effects. Appendix Table A.I re-estimates every row of Table IV's not-yet-treated column under the inverse hyperbolic sine and as a linear probability model for any reporting. The two concave transforms agree throughout—charity care -0.575 against -0.569, education, research, and community building 0.355 against 0.319—so nothing hinges on the particular transform.

The linear probability model locates the charity care decline on the intensive margin: the probability of reporting any financial assistance is unchanged (-0.009, 0.008), so filers report less rather than stop. The private-return increase is substantially extensive: the probability of reporting any education, research, or community-building spending rises 5.2 percentage points (0.008). Appendix Tables A.XI and A.XII, reported with the results, carry the same decomposition to every category and both designs, and show that the dollar changes are concentrated in large filers while the log and reporting changes are concentrated in small ones. The gradient reading in Section 6 is stated on those margins separately for that reason, and its dependence on the comparison group is a feature of the design, not of the transform: no transform produces a private-return increase with never-adopting filers as the comparison.

8 Discussion

A single verdict on whether nonprofit hospitals are mission committed is not supported by the results. The verdict differs by margin. The decline in charity care is the mechanical consequence of coverage: the payer mix and the cost share fall by roughly a third and by 39 to 64 percent, no more than the rise in Medicaid coverage can account for. On that margin nonprofits behaved as if $\alpha > \rho$, keeping 0.8 percentage points more charity care, a quarter of the pre-expansion level, than the same hospitals would have under for-profit ownership; however, they maintained charity care no more than they would have under public ownership, so the premium is over the profit-maximizing form, not over every alternative to it. On the windfall the framework's corner solution fails in both directions: a fifth of the freed dollars returned as subsidized care, two-fifths went to education and research, and between two-

fifths and five-sixths of the decline reappears in no Schedule H category. Margins rose in the covariate-adjusted specifications by an amount the design bounds only between a seventh of the freed cost and more than the whole of it. An interior split of this kind is not a prediction the framework can generate for a representative hospital; instead, it is what a population of hospitals with different mission weights would produce, most of them below ρ on the windfall margin and enough of them above it on the charity-care margin to leave the retention premium the ownership contrast finds.

The pattern across the two ownership contrasts points to the obligations attached to exempt status rather than to a mission weight that operates independently of them. Nonprofit hospitals kept more charity care than the same hospitals would have under for-profit ownership, and no more than they would have under public ownership; what nonprofit and public hospitals share, and for-profit hospitals lack, is a status that carries a spending obligation. For nonprofits that obligation is the IRS community-benefit standard, which counts education, research, and community building toward exemption alongside direct patient aid, and which sets no floor on any category. The allocation of the windfall follows the standard's shape. When expansion lowered the demand for charity care, a fifth of the freed dollars returned as subsidized care, two-fifths went to categories the standard counts but patients do not directly receive, and the reporting response was concentrated in those same categories. A hospital maximizing $\pi + \alpha D$ with α above ρ would have put the windfall into direct aid; a hospital satisfying a standard that counts education as community benefit had no reason to.

If the breadth of the federal standard is what permits that allocation, a narrower obligation should produce a different one, and three expansion states let us look. Illinois, Pennsylvania, and Nevada had charity or indigent-care spending floors in force before 2014, expressed as a share of revenue or of the value of the exemption, so that a fall in the demand for charity care does not release a hospital from the floor. Table VI re-estimates the Schedule H responses for the 130 treated filers in those states, with every other filer retained as a not-yet-treated control, beside the 695 treated filers in the other expansion states. Three differences appear. Charity care did not fall among filers under a floor on the log margin, -0.10 log points against -0.58 elsewhere, and fell by less in dollars, $-\$0.6$ million against $-\$1.1$ million, though the dollar estimates are too imprecise to distinguish. The reporting response to expansion seen elsewhere on education and community health improvement is absent under

a floor. And the one reporting response filers under a floor do show is on the two subsidized-services lines, net and gross, the Schedule H items that count toward such floors, where the share reporting any rises by seven to eight percentage points with no measurable change in what incumbents spend. This is the most direct evidence that the constraint governs the allocation: where the obligation is a floor on charity-type care, charity care is kept and the categories that absorb the windfall elsewhere do not move.

Two implications bear on the design of the exemption's obligations. The first concerns the breadth of the community-benefit standard. Because the standard counts education, research, and community building alongside direct patient aid, a hospital that receives a windfall satisfies it without adding to direct aid, and the categories the windfall moved into here are those the standard counts but patients do not directly receive. Where the obligation is instead a floor on charity-type care, as in the three states examined, charity care was retained and those categories did not move. If the object of exemption is care for patients who cannot pay, the results favor an obligation stated in those terms. The same results caution that a narrower obligation changed what hospitals reported against before it changed what they spent since the floor-state response on subsidized services is a reporting response. The second concerns what the broad categories contain. The paper's decomposition shows the education, research, and community-building aggregate is almost entirely health-professions education and research, so the definitional question is whether training and research, which carry a private return to the hospital, belong in a measure of benefit to the community at all. That is a question of policy rather than of evidence, and the results only show that it is where the windfall went.

9 Conclusion

Tax exemption subsidizes a nonprofit hospital's mission on the premise that the hospital would spend on patients' health what it could otherwise keep as surplus. The record has long given reason to doubt that the subsidy buys what it pays for, but that record is descriptive, and it cannot say whether the nonprofit form carries an objective of its own or only sets different conditions under which returns are sought.

The Medicaid expansions put the question to a test: a common shock that freed hospitals of part of their charity-care burden, arriving in different states in different years. We used

it to estimate, for the first time as a causal contrast, how much more charity care nonprofit hospitals kept than the same hospitals would have under for-profit ownership, and whether they kept more than they would have under public ownership; to trace, within nonprofits and on three margins, where the freed resources moved along the gradient from direct patient aid to private-return uses; to exploit the one source of variation in the obligation itself, the state spending floors in force before expansion; and to test rather than assume the selection a balanced panel imposes.

The answer the tests return is consistent across them. Nonprofits kept about a quarter more charity care than they would have as for-profits and no more than they would have as public hospitals. The freed resources went where the community-benefit standard counts them, to education and research and to reimbursed services, with a fifth returning as subsidized care and between two-fifths and five-sixths reappearing nowhere on Schedule H. Where the obligation was a floor on charity-type care, charity care was kept and the other categories did not move. The commitment nonprofit hospitals display is the one their status obliges, not a mission weight that operates apart from it.

Several limits bound our answer. The within-nonprofit decline, robust to violations of parallel trends as large as any observed before treatment, is fragile to linear extrapolation of a pre-trend that three pre-periods cannot pin down. The ownership contrast rests on the 2014 cohort in the years after adoption, is estimated with enough precision to reject zero but not to distinguish a quarter of baseline from a tenth, and reads as an ownership effect only under observable subgroup selection, an assumption that no test can confirm. The Schedule H gradient is identified in the preferred design off two adoption cohorts' first two years, its reallocation rows do not survive a comparison with never-adopting filers, whose charity care fell by nearly as much, and part of what moved on it is reporting rather than spending, since the categories that rose gained reporters more than dollars. The floor evidence rests on three states and 130 filers and cannot separate a floor from the state that imposed it. And our two panels are not linked, so no hospital's full ledger is observed.

Future research could leverage the spending floors that Oregon imposed from 2021, and the floors Texas and Utah maintained in states that never expanded, to offer more variation in the obligation than three expansion states do. Researchers could also leverage changes to ownership status or plausibly exogenous shocks to healthcare demand in an area to further test how ownership status changes hospitals' responses. A design observing the value of each

hospital's exemption directly is the natural next test of the obligation reading. Patient-level data would show whether the uninsured who remained after expansion received less charity care, as a decline concentrated on the intensive margin suggests.

Each limit names the study that would lift it. Schedule H filers have been matched to Medicare provider numbers by name and address for cross-sectional comparisons of exemption value and community benefit (Herring et al., 2018; Bai et al., 2021); applying such a match to a staggered-adoption design would put the ownership contrast and the gradient on the same hospitals, so that the charity decline measured on the Cost Report and the community-benefit changes on Schedule H become entries in one ledger, the share of the windfall reappearing nowhere can be estimated hospital by hospital, and the hospital characteristics that condition the ownership contrast can condition the gradient. The spending floors that Oregon imposed from fiscal 2022, whose first phase has been studied but whose floor has not (Santos et al., 2025), and the floors Texas and Utah maintain in states that never expanded, offer more variation in the obligation than three expansion states do. Conversions to nonprofit status do not raise charity care (Beniwal et al., 2026); whether they change the composition of community benefit is the open question on that margin. The value of the exemption has been compared with community benefit in the cross-section; a design around a change in that value, such as Illinois's 2012 statute conditioning the property-tax exemption on charity spending, is the natural next test of the obligation reading. And patient-level data would show whether the uninsured who remained after expansion received less charity care, as a decline concentrated on the intensive margin suggests; the policy-side evidence that hospitals did not tighten eligibility (Bai et al., 2022; Moran, 2025) makes realized receipt the question.

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10 Tables

TABLE I
SAMPLE AND PRE-TREATMENT BALANCE, NONPROFIT HOSPITALS

	Nonprofit hospitals, by adoption timing			Normalized difference		
	(1) Adopt in window	(2) Adopt after window	(3) Never adopt	(4) (1)–(2)	(5) (1)–(3)	(6) (2)–(3)
Charity-care payer mix	0.031	0.027	0.037	0.13	−0.23	−0.36
	(0.027)	(0.024)	(0.032)			
Charity-care cost (share of net revenue)	0.024	0.024	0.032	0.02	−0.15	−0.19
	(0.044)	(0.023)	(0.055)			
Medicaid payer mix	0.147	0.119	0.119	0.41	0.43	0.01
	(0.075)	(0.055)	(0.050)			
Poverty rate (ACS, percent)	18.30	17.38	19.44	0.12	−0.14	−0.26
	(8.44)	(7.53)	(8.37)			
Unemployment rate (ACS, percent)	9.85	8.13	9.44	0.43	0.10	−0.35
	(4.13)	(3.83)	(3.72)			
Hospitals	1,548	245	410			
Hospital-years, 2011–2013	4,659	735	1,233			
Adoption cohorts	2014–2019	2020–2025	—			

Notes. This table reports means over hospital-years 2011–2013, with standard deviations in parentheses, for the nonprofit hospitals in the balanced hospital–year panel, grouped by the year their state's Medicaid expansion took effect. The panel is built from Medicare Cost Reports (CMS Form 2552-10) as compiled by the National Academy for State Health Policy, covers 2011–2023 with 2020 dropped, and keeps hospitals that file in every retained year; ownership is the Cost Report's provider control type. Column (1) is hospitals in states that adopted in 2014, 2015, 2016, 2017, or 2019, the cohorts observed at least three years before and four years from adoption; column (2) is hospitals in states that adopted in 2020 or later, which the estimator treats as untreated; column (3) is hospitals in states that had not adopted by the end of the panel. Columns (4)–(6) are normalized differences between the indicated columns, $(\bar{x}_1 - \bar{x}_2)/\sqrt{(s_1^2 + s_2^2)/2}$. Payer mixes are the share of the hospital's gross patient charges attributable to charity care or to Medicaid; charity-care cost is charity care at cost as a share of net patient revenue. Poverty and unemployment rates are county-level estimates from the American Community Survey five-year series, assigned to each hospital by geocoding its address to a county. The not-yet-treated specification of Table II is estimated on the column (1) hospitals alone; the never-treated-included specification adds the hospitals in columns (2) and (3). Pre-treatment means of the remaining outcomes of Table II appear in the control-mean column of that table.

Table II
Within-Nonprofit Effects of Medicaid Expansion

		Not-yet-treated controls		Never-treated included		Pre-trend Wald p	
	Control Mean	(1)	(2)	(3)	(4)	Independent	Cov. Aware
A. Uncompensated care							
Charity-care payer mix	0.0306	−0.0089	−0.0107	−0.0108	−0.0104	0.885	0.739
		(0.0019)	(0.0019)	(0.0019)	(0.0020)		
Charity-care cost (share of net revenue)	0.0244	−0.0107	−0.0155	−0.0096	−0.0109	0.512	0.471
		(0.0026)	(0.0035)	(0.0019)	(0.0035)		
Bad debt and uninsured (share of net revenue)	0.0259	−0.0047	−0.0053	−0.0033	−0.0063	0.398	0.337
		(0.0009)	(0.0017)	(0.0012)	(0.0022)		
B. Coverage substitution and payer-mix placebos							
Medicaid payer mix (first stage)	0.1465	0.0360	0.0381	0.0319	0.0286	0.995	0.993
		(0.0047)	(0.0063)	(0.0042)	(0.0078)		
Medicare payer mix (placebo)	0.3251	−0.0072	−0.0038	−0.0040	0.0067	0.420	0.400
		(0.0026)	(0.0149)	(0.0033)	(0.0089)		
Commercial payer mix (placebo)	0.4155	−0.0040	−0.0116	−0.0080	−0.0025	0.164	0.096
		(0.0054)	(0.0113)	(0.0031)	(0.0073)		
C. Financial outcomes							
Net profit margin	0.0494	0.0025	0.0172	0.0004	0.0109	0.215	0.413
		(0.0043)	(0.0074)	(0.0034)	(0.0066)		
Log net patient revenue	18.5717	−0.0226	0.0685	−0.0080	−0.0944	0.022	0.000
		(0.0085)	(0.0901)	(0.0075)	(0.1143)		
Covariates			x		x		
Observations		11,617	11,575	19,495	19,453		

Notes. Each cell in columns (1)–(4) reports the average effect of Medicaid expansion on treated nonprofit hospitals from a Callaway and Sant'Anna (2021) staggered difference-in-differences estimated with the doubly robust score of Sant'Anna and Zhao (2020) and aggregated over the four years from adoption with treated-unit weights; standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. The sample is the nonprofit hospitals of Table I, drawn from the Medicare Cost Reports for 2011–2023 with 2020 dropped; a state's cohort is the year its expansion took effect, and cohorts observed fewer than three years before or four years from adoption are treated as untreated. Columns (1)–(2) use not-yet-treated hospitals in adopting states as the comparison group; columns (3)–(4) also admit hospitals in states adopting after 2019 and in never-adopting states. Columns (2) and (4), marked x in the Covariates row, condition on four county-level covariates at their values in the year before adoption: the uninsured rate, the unemployment rate, the nonwhite population share, and median household income, from the American Community Survey five-year estimates linked to each hospital by its geocoded county. The control mean is the 2011–2013 mean of the outcome among the hospitals serving as comparisons in column (1). The last two columns report the p-value of a Wald test that the three pre-adoption event-time coefficients of the column (2) specification are jointly zero, computed first as if the leads were independent and then with the bootstrap covariance across leads. Payer mixes are the share of gross patient charges attributable to the named payer; charity-care cost and bad debt and uninsured cost are at cost as shares of net patient revenue; net profit margin is net income as a share of total revenue; log net patient revenue is in log points. All outcomes but the last are shares, so estimates are in proportion units. Observations are hospital-years in each estimation sample.

Table III
Ownership Contrast: Nonprofit versus For-Profit Hospitals

		Not-yet-treated		Never-treated included	
	Control Mean	(1)	(2)	(3)	(4)
A. Nonprofit ATT					
Charity-care payer mix	0.0306	−0.0089	−0.0107	−0.0108	−0.0104
		(0.0019)	(0.0019)	(0.0019)	(0.0020)
Charity-care cost (share of net revenue)	0.0244	−0.0107	−0.0155	−0.0096	−0.0109
		(0.0026)	(0.0035)	(0.0019)	(0.0035)
Bad debt and uninsured (share of net revenue)	0.0259	−0.0047	−0.0053	−0.0033	−0.0063
		(0.0009)	(0.0017)	(0.0012)	(0.0022)
B. For-profit ATT					
Charity-care payer mix	0.0253	−0.0126	−0.0071	−0.0186	−0.0213
		(0.0021)	(0.0056)	(0.0031)	(0.0046)
Charity-care cost (share of net revenue)	0.0214	−0.0162	−0.0107	−0.0175	−0.0202
		(0.0030)	(0.0053)	(0.0037)	(0.0048)
Bad debt and uninsured (share of net revenue)	0.0263	−0.0078	−0.0049	−0.0084	−0.0055
		(0.0024)	(0.0031)	(0.0015)	(0.0052)
C. Difference in effects (DATT = nonprofit − for-profit)					
Charity-care payer mix	0.0306	0.0037	−0.0036	0.0078	0.0110
		[0.000, 0.008]	[−0.014, 0.007]	[0.001, 0.015]	[0.001, 0.021]
Charity-care cost (share of net revenue)	0.0244	0.0054	−0.0048	0.0080	0.0093
		[−0.001, 0.011]	[−0.015, 0.005]	[0.000, 0.016]	[−0.003, 0.022]
Bad debt and uninsured (share of net revenue)	0.0259	0.0030	−0.0004	0.0051	−0.0009
		[−0.002, 0.008]	[−0.006, 0.005]	[0.002, 0.008]	[−0.012, 0.011]
D. Causal ownership contrast (CDATT)					
Charity-care payer mix	0.0306	—	0.0082	—	0.0078
			[0.003, 0.013]		[0.000, 0.016]
Charity-care cost (share of net revenue)	0.0244	—	0.0058	—	0.0048
			[−0.001, 0.012]		[−0.006, 0.015]
Bad debt and uninsured (share of net revenue)	0.0259	—	−0.0023	—	0.0015
			[−0.007, 0.002]		[−0.002, 0.005]
Covariates			x		x

Notes. Panels A and B report, for nonprofit and for-profit hospitals separately, the average effect of Medicaid expansion from the estimator, sample rules, comparison groups, covariates, and bootstrap described in the notes to Table II; Panel A repeats Panel A of that table. The for-profit sample is the 503 for-profit hospitals of the balanced panel, 208 of them in the retained adoption cohorts. Panel C is the nonprofit estimate minus the for-profit estimate in the same column. Panel D is the causal ownership contrast of Caron (2025): the nonprofit effect minus the effect the same hospitals would have had under for-profit ownership, identified under the additional assumption that, conditional on covariates, the for-profit response to expansion does not depend on whether a hospital is in fact nonprofit. It conditions on the four county covariates of Table II and on three hospital characteristics measured in the year before adoption, log bed size, membership in a hospital system, and the Medicaid payer mix, and is estimated with a doubly robust score in which a four-group multinomial propensity model reweights treated for-profit hospitals and both comparison groups to the covariate distribution of treated nonprofit hospitals and linear outcome regressions correct for misspecified weights. It is defined only in the covariate columns, hence the dashes in columns (1) and (3), and only on cohort–year cells in which treated and comparison hospitals of both types are observed. Brackets in Panels C and D are 95 percent confidence intervals from a joint state-clustered multiplier bootstrap with 2,000 draws in which both ownership arms are perturbed by the same draws. The control mean is the 2011–2013 mean among the comparison hospitals of the ownership type named in the panel heading; Panels C and D report the nonprofit mean. Outcomes are defined in the notes to Table II. Appendix Table A.V reports the components of Panel D and Appendix Table A.VI its cohort–year cells.

Table IV
The Reallocation Gradient: IRS Form 990 Schedule H

	Pre-treatment	Not-yet-treated controls		Never-treated included		Share at zero	Leave-one-state-out (ATT)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Source: the freed obligation							
Charity care (financial assistance)	\$3.82M	−0.5692	0.0666	−0.4944	−0.6734	0.021	[−0.597, −0.544]
		(0.0545)	(0.1734)	(0.0905)	(0.2388)		
Discretionary direct patient aid (s^D): the mission test							
Subsidized services, net (Line 7g)	\$1.69M	0.0801	0.1949	0.0028	−0.6342	0.498	—
		(0.2371)	(0.4638)	(0.1972)	(0.5662)		
Gross subsidized expense	\$5.43M	0.3142	1.0315	0.0233	−0.7158	0.492	[0.222, 0.359]
		(0.1883)	(0.5102)	(0.1952)	(0.6097)		
Less: direct offsetting revenue	\$3.73M	0.0854	0.5197	0.0833	−0.9767	0.609	—
		(0.1545)	(0.6529)	(0.1827)	(0.6169)		
Community health improvement and building	\$1.12M	0.3100	3.2523	0.1048	0.0284	0.160	[0.036, 1.363]
		(0.2462)	(1.3089)	(0.1543)	(0.3104)		
Private-return uses (s^R)							
Education, research, and community building	\$6.50M	0.3189	0.0116	−0.0299	−0.4359	0.313	[0.229, 0.605]
		(0.1159)	(0.9566)	(0.1437)	(0.4201)		
Health-professions education	\$4.52M	0.4480	0.1057	0.0960	−0.0548	0.386	[0.394, 0.631]
		(0.1648)	(0.8172)	(0.1609)	(0.3935)		
Research	\$1.87M	0.1553	−0.1459	−0.0560	−0.4492	0.842	—
		(0.1183)	(0.5626)	(0.1349)	(0.3236)		
Workforce development	\$59K	0.1994	0.5729	−0.1831	−0.3295	0.766	—
		(0.1205)	(0.4109)	(0.1382)	(0.2362)		
Physical improvements and housing	\$32K	0.1450	0.5468	0.0640	0.0286	0.928	—
		(0.1017)	(0.1911)	(0.0979)	(0.1271)		
Economic development	\$22K	0.1530	0.3181	0.0146	0.0733	0.843	—
		(0.1244)	(0.2031)	(0.1332)	(0.2586)		
Memorandum							
Medicaid-funded programs (Line 7b)	\$26.42M	0.1245	2.0236	0.1151	0.1241	0.086	—
		(0.2754)	(1.8756)	(0.1112)	(0.3539)		
Covariates			x		x		
Observations		5,858	5,858	10,194	10,194		

Notes. Each cell in columns (2)–(5) reports the average effect of Medicaid expansion on $\log(1 + \$)$ of the category's reported spending, from the Callaway and Sant'Anna (2021) estimator described in the notes to Table II, aggregated over the four years from adoption; standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. The sample is a filer–year panel of IRS Form 990 Schedule H returns, from the National Center for Charitable Statistics e-file data, for 1,250 nonprofit organizations operating a single hospital facility, tax years 2011–2022; the retained adoption cohorts are 2014, 2015, and 2016. Column (1) is the 2011–2013 mean of the category in dollars among filers in the retained cohorts. Columns (2)–(3) use not-yet-treated filers in adopting states as the comparison group; columns (4)–(5) also admit filers in never-adopting states and in states adopting after 2019. Columns (3) and (5), marked x, condition on the four county covariates of Table II, linked to each filer through the county containing the largest share of its ZIP code's land area (Census ZIP Code Tabulation Area relationship file). Column (6) is the share of all filer-years in the panel reporting exactly zero in the category. Column (7) is the range of the column (2) point estimate across re-estimations that drop one of the 32 sample states at a time; a dash marks a row for which it was not computed. Categories follow Schedule H: charity care is financial assistance at cost (Part I, line 7a); subsidized services, net, is line 7g net of the direct offsetting revenue reported against it, with the two components shown beneath; community health improvement and building is line 7e plus Part II lines 3–7; education, research, and community building is health-professions education (line 7f), research (line 7h), and the Part II physical improvements and housing, economic development, and workforce development lines, each shown beneath; Medicaid-funded programs is line 7b. The labels s^D and s^R are the direct-aid and private-return margins of Section 3. Observations are filer-years.

TABLE V
TREATMENT-AFFECTED PANEL SELECTION: DIAGNOSTIC AND REMEDY

	Hazard ratio (1)	p-value (2)	Number of exit events (3)	States (4)
A. Is balanced-panel membership treatment-affected?				
Pooled	0.80	0.176	511	41
Governmental	0.20	0.000	36	35
Nonprofit	1.98	0.025	155	41
For-profit	0.88	0.526	187	35
nonprofit, dropping the 2015 cohort	1.59	0.150	106	41
Joint pre-trend Wald p			0.53	
Hospital-years at risk (pooled model)			58,163	
B. Charity ATT under attrition (two-period proxy for the CS-DiD)				
Unweighted proxy ATT			−0.0142	
Inverse-probability-weighted proxy ATT			−0.0141	
Net differential attrition	treated 0.154 vs. comparison 0.169 ($p = 0.56$)			
Lee (2009) trimming bounds	[−0.0179, −0.0162]			

Notes. Panel A reports hazard ratios from a discrete-time hazard model of a hospital's departure from the Cost Report filing panel, estimated on the unbalanced universe of 5,119 Cost Report hospitals (58,163 hospital-years at risk) stacked by adoption cohort; the hazard ratio is the multiplicative effect of the state's expansion on the hazard of departure. Column (1) is the hazard ratio; column (2) its p-value, from a state pairs-cluster bootstrap with 499 draws for the pooled and nonprofit rows and from a cluster-robust z statistic for the governmental and for-profit rows; column (3) is the number of departures the model is fit on; column (4) the number of states contributing hospital-years. The pooled row fits all ownership types together, the ownership rows fit the model on each type alone, and the final nonprofit row drops the 2015 cohort. The joint pre-trend Wald p tests that the pre-adoption coefficients of the pooled model are zero. Panel B re-estimates the nonprofit charity-care payer-mix effect in a two-period two-way fixed-effects difference-in-differences on the unbalanced panel, a proxy for the staggered estimator of Table II that is not comparable to it in magnitude: unweighted, then reweighted by the inverse of each hospital's estimated probability of remaining in the panel. Net differential attrition is the share of treated and of comparison hospitals leaving the panel, with the p-value of their difference. The Lee (2009) bounds trim the group with the lower attrition rate by the attrition difference and report the resulting range of the proxy estimate. Data are the Medicare Cost Reports described in the notes to Table I.

TABLE VI
SCHEDULE H RESPONSES UNDER STATE SPENDING FLOORS

	Any reporting		log(1 + $)		Dollars (millions)	
	Floor (1)	Other (2)	Floor (3)	Other (4)	Floor (5)	Other (6)
Charity care (financial assistance)	0.009	−0.009	−0.103	−0.581	−0.622	−1.073
	(0.011)	(0.008)	(0.174)	(0.097)	(0.400)	(0.277)
Subsidized services, net (Line 7g)	0.068	0.001	0.679	−0.125	−0.201	0.208
	(0.039)	(0.024)	(0.573)	(0.320)	(0.355)	(0.114)
Gross subsidized expense	0.079	0.015	0.858	0.099	−0.984	1.302
	(0.037)	(0.022)	(0.602)	(0.297)	(1.168)	(0.340)
Community health improvement services (Line 7e)	−0.012	0.067	−0.358	0.704	−0.385	0.073
	(0.035)	(0.018)	(0.413)	(0.238)	(0.173)	(0.072)
Education, research, and community building	−0.041	0.076	−0.901	0.580	−0.544	0.204
	(0.047)	(0.016)	(0.707)	(0.233)	(1.308)	(0.215)
Treated filers	130 / 695		130 / 695		130 / 695	

Notes. Each cell reports the effect of Medicaid expansion on the named Schedule H category from the estimator, sample rules, and retained cohorts of Table IV, without covariates and with not-yet-treated controls, with the treated group restricted to filers in expansion states that had a charity-care or indigent-care spending floor in force before 2014 (Floor columns) or to filers in the other expansion states (Other columns). The floor states are Illinois, which requires spending equal to the value of the property-tax exemption on services to low-income and underserved residents; Pennsylvania, under the Institutions of Purely Public Charity Act; and Nevada, which requires 0.6 percent of revenue for indigent care. Texas and Utah also have floors but did not expand within the window, and Oregon's floor binds only from 2021, so none enters a treated group. In every column each retained-cohort filer outside the treated group keeps only its pre-adoption observations, so it serves as a not-yet-treated comparison and contributes no treated observations, as in Appendix Table A.VIII. Columns (1)–(2) are the change in the probability of reporting any positive amount, in proportion units; columns (3)–(4) the change in log(1 + $); columns (5)–(6) the change in millions of nominal dollars per filer-year, winsorized at the 99th percentile of filer-years. Standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. Treated filers number 130 under a floor (66 in Illinois, 59 in Pennsylvania, 5 in Nevada) and 695 elsewhere. Categories are defined in the notes to Table IV.

11 Figures

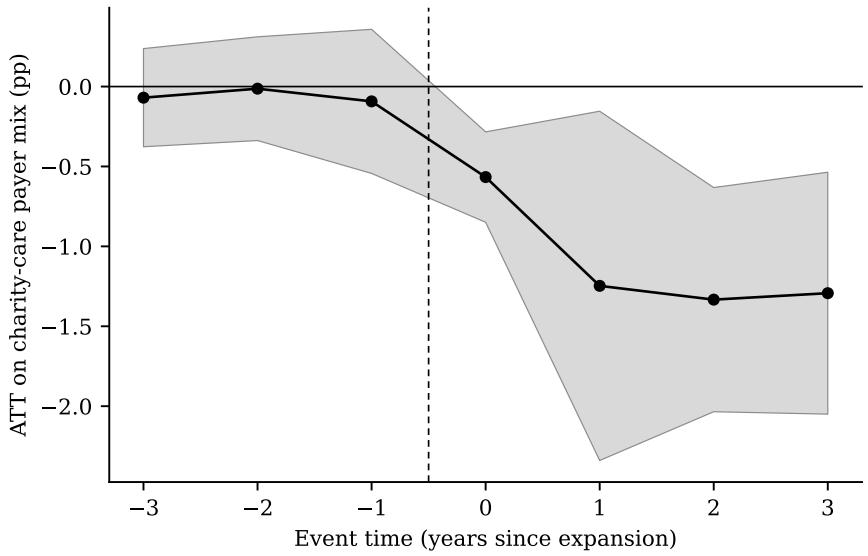


FIGURE I

CHARITY-CARE PAYER MIX: EVENT STUDY, WITHIN NONPROFITS

This figure plots event-time coefficients from the column (2) specification of Table II: the Callaway and Sant'Anna (2021) effect of Medicaid expansion on nonprofit hospitals' charity-care payer mix, with not-yet-treated hospitals in adopting states as the comparison group and the four county covariates, aggregated by years since adoption. Event time 0 is the year the state's expansion took effect; coefficients are in percentage points of gross patient charges. Markers are point estimates; the shaded region is a 95 percent simultaneous confidence band from the state-clustered multiplier bootstrap (critical value 2.434). Pre-adoption coefficients use the preceding year as the base period, so no lead is normalized to zero; the dashed vertical line separates pre- and post-adoption years. Data are the Medicare Cost Reports described in the notes to Table I.

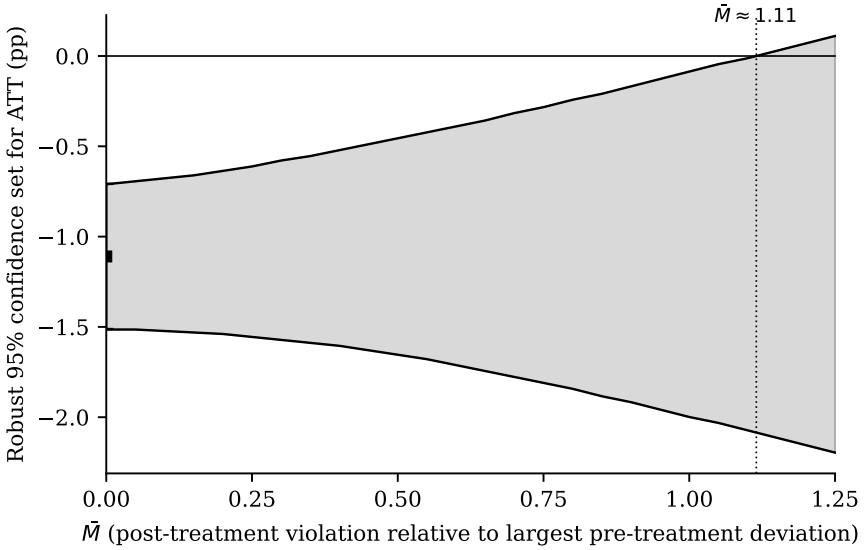


FIGURE II

SENSITIVITY OF THE CHARITY EFFECT TO VIOLATIONS OF PARALLEL TRENDS

This figure plots the Rambachan and Roth (2023) relative-magnitudes sensitivity of the post-adoption effect on nonprofit hospitals' charity-care payer mix from the column (2) specification of Table II. The horizontal axis is $\bar{M}$, the largest permitted post-adoption violation of parallel trends expressed as a multiple of the largest pre-adoption deviation between treated and comparison hospitals; the shaded region is the robust 95 percent confidence set for the average post-adoption effect at each $\bar{M}$, in percentage points; the square at $\bar{M} = 0$ is the conventional estimate. The dotted vertical line marks the breakdown value, the smallest $\bar{M}$ at which the robust set includes zero. The event study underlying the figure is estimated with a universal base period, the year before adoption, as the relative-magnitudes restriction requires, with state-clustered standard errors. Appendix Figure A.I reports the companion smoothness restriction. Data are the Medicare Cost Reports described in the notes to Table I.

A Appendix

A.1 IRS 990, Schedule H

Form 990 Schedule H is filed by 501(c)(3) organizations operating at least one hospital facility and documents each organization's community-benefit activities and policies.

Part I, "Financial assistance and means-tested government programs," reports (a) financial assistance at cost, (b) Medicaid, and (c) costs of other means-tested government programs. Its "Other benefits" block reports (e) community health improvement services and community benefit operations, (f) health professions education, (g) subsidized health services, (h) research, (i) cash and in-kind contributions for community benefits, and (j) other benefits. Rows (d) and (k) sum their respective subparts.

Part II reports community-building activities in nine categories: (1) physical improvements and housing, (2) economic development, (3) community support, (4) environmental improvements, (5) leadership development and training for community members, (6) coalition building, (7) community health improvement advocacy, (8) workforce development, and (9) other. For each, the return lists (a) number of activities or programs, (b) persons served, (c) total community-building expense, (d) direct offsetting revenue, (e) net community-building expense, and (f) percent of total expense. Fields (a) and (b) are optional and we do not use them: they are missing for 80–96 percent of filer–years across categories.

Part III covers bad debt expense, Medicare costs, and collection policies. Part V reports the individual hospital facilities an organization operates, including facility count, name, address, state, license number, and characteristics such as whether the facility is a general medical and surgical, children's, teaching, critical access, or research facility. The IRS definitions for every category are in the Instructions for Schedule H (Form 990).

Constructed community-benefit categories

Our outcome categories aggregate Schedule H line items to match the gradient of Section 3 rather than the IRS's reporting order. Table IV prints them in that order.

Construct	Schedule H source
The freed obligation	
Charity care (financial assistance)	Part I, line 7a
Discretionary direct patient aid (s^D)	
Subsidized services, gross	Part I, line 7g
less direct offsetting revenue	Part I, line 7g offset field
Community health improvement and building	Part I, line 7e + Part II lines 3–7
Private-return uses (s^R)	
Education, research, and community building	Part I, lines 7f, 7h + Part II lines 1, 2, 8
Memorandum	
Medicaid-funded programs	Part I, line 7b

The private-return aggregate is dominated by its first two components: health-professions education is 70 percent of its pre-treatment dollar base and research a further 29 percent, so physical improvements, economic development, and workforce development together account for under two percent. Table IV therefore reports all five components individually as well as the aggregate.

Construction of the Schedule H panel

Because Schedule H is filed at the Employer Identification Number level and no public crosswalk maps EINs to CMS Certification Numbers, we restrict attention to the hospital facilities reported on Schedule H and limit the sample to nonprofit organizations operating a single hospital facility in a known state. Where multiple returns exist for the same filer–year we retain the latest. We confine the sample to the fifty states and the District of Columbia, dropping U.S. territories and filers with international facilities, and keep only facilities with a standardized five- or nine-digit ZIP code, padding four- and eight-digit codes in northeastern states; ZIP codes are truncated to five digits for covariate linkage. To limit coverage gaps we exclude filers with more than two missing years between their first and last observed filing, and filers with fewer than ten filings in total.

Hospital–county assignment

The hospital–year panel is linked to county-level American Community Survey five-year estimates. Each hospital was geocoded against the Census geocoder and assigned a provenance tier: an exact street match from the batch geocoder (84.3 percent of hospitals), a street match from the single-address geocoder (2.1 percent), a 2023 Gazetteer city centroid reverse-geocoded to county (12.5 percent), or — for 40 hospitals, 1.1 percent — no verified match. The last group is flagged in the covariate file and can be dropped as a robustness check. Covariates are drawn from ACS tables S2701 (coverage), B03002 (race), B17001 (poverty), B23025 (unemployment), and B19013 (income). Two features of the series are handled explicitly rather than assumed: the S2701 column layout changes at 2015, and the five-year series carries no health-insurance table for 2011, which is backfilled from the 2012 five-year estimate and flagged.

A.2 Additional Figures and Tables

TABLE A.I
FUNCTIONAL-FORM SENSITIVITY OF THE SCHEDULE H RESULTS

	ATT under alternative transforms			Share at
	$\log(1+\$)$ (1)	asinh($) (2)	$\mathbf{1}\{\$>0\}$ (3)	zero (4)
Charity care (financial assistance)	−0.5692	−0.5752	−0.0087	0.018
	(0.0545)	(0.0547)	(0.0078)	
Subsidized services, net	0.0801	0.0913	0.0159	0.487
	(0.2371)	(0.2493)	(0.0181)	
Gross subsidized expense	0.3142	0.3354	0.0306	0.479
	(0.1883)	(0.1972)	(0.0139)	
Community health improvement and building	0.3100	0.3321	0.0320	0.146
	(0.2462)	(0.2590)	(0.0212)	
Education, research, and community building	0.3189	0.3549	0.0520	0.285
	(0.1159)	(0.1184)	(0.0079)	
Health-professions education	0.4480	0.4884	0.0583	0.355
	(0.1648)	(0.1748)	(0.0179)	
Research	0.1553	0.1685	0.0190	0.813
	(0.1183)	(0.1264)	(0.0104)	
Workforce development	0.1994	0.2153	0.0231	0.762
	(0.1205)	(0.1282)	(0.0122)	
Medicaid-funded programs	0.1245	0.1228	−0.0024	0.099
	(0.2754)	(0.2884)	(0.0187)	

Notes. Each cell in columns (1)–(3) reports the effect of Medicaid expansion on the named Schedule H category from the estimator, sample, not-yet-treated comparison group, and bootstrap of column (2) of Table IV, without covariates; only the transformation of the dollar outcome differs. Column (1) is $\log(1+\$)$, as in Table IV; column (2) the inverse hyperbolic sine of dollars; column (3) a linear probability model for an indicator that the filer reports any positive amount in the category, so its estimates are changes in the probability of reporting, in proportion units. Column (4) is the share of 2011–2013 filer-years in the sample reporting exactly zero. Standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. Categories are defined in the notes to Table IV.

Table A.II
Composition of Never-Expansion States, by Ownership

	Nonprofit hospitals			For-profit hospitals		
	Ever expansion	Never expansion	Norm. diff.	Ever expansion	Never expansion	Norm. diff.
A. Pre-treatment composition, 2011–2013						
Uninsured rate (ACS, Nonwhite share (ACS, Unemployment rate (ACS, Median household income ()	52,280	47,910	0.36	51,748	48,906	0.24
Charity-care payer mix	0.0301	0.0372	−0.24	0.0234	0.0314	−0.28
Charity-care cost (share)	0.0243	0.0316	−0.15	0.0199	0.0257	−0.16
Medicaid payer mix	0.1428	0.1189	0.38	0.1506	0.1292	0.28
Net profit margin	0.0529	0.0501	0.03	0.0895	0.0627	0.17
B. Ownership mix of the hospital population						
Share of hospitals: nonprofit	0.687 vs. 0.464			difference 0.223		
Share of hospitals: for-profit	0.112 vs. 0.239			difference −0.127		
Share of hospitals: government	0.201 vs. 0.298			difference −0.096		
C. Pre-period placebo ownership contrast (2011–2013 only)						
Charity-care payer mix, EXCL	nonprofit −0.0026, for-profit −0.0019, placebo DATT −0.0007					
Charity-care payer mix, INCL	nonprofit 0.0071, for-profit 0.0080, placebo DATT −0.0008					
Charity-care cost (share), EXCL	nonprofit −0.0035, for-profit −0.0026, placebo DATT −0.0009					
Charity-care cost (share), INCL	nonprofit 0.0073, for-profit 0.0057, placebo DATT 0.0016					

Notes. Panel A reports 2011–2013 means for hospitals in states that expanded Medicaid at any point in the panel (ever expansion) and in states that had not expanded by its end (never expansion), separately for nonprofit and for-profit hospitals, with the normalized difference $(\bar{x}_1 - \bar{x}_2)/\sqrt{(s_1^2 + s_2^2)/2}$ between them. Uninsured rate, nonwhite share, unemployment rate, and median household income are the county-level ACS covariates of Table II; the remaining rows are Cost Report outcomes defined in the notes to that table. Panel B reports the ownership composition of the Cost Report hospital population in ever-expansion and never-expansion states (first and second figures) and the difference between them. Panel C re-estimates the nonprofit and for-profit effects and their difference (DATT) on the 2011–2013 observations alone, before any state had expanded, under the not-yet-treated (EXCL) and never-treated-included (INCL) comparison groups of Table III; a valid design implies values of zero. Data are the Medicare Cost Reports and ACS estimates described in the notes to Table I.

TABLE A.III
GOODMAN–BACON DECOMPOSITION BY OWNERSHIP AND COMPARISON GROUP

	Weight	Weighted-average 2×2 estimate	Overall TWFE
	(1)	(2)	(3)
Nonprofit hospitals, not-yet-treated controls			
Earlier- vs later-treated	0.3618	−0.00924	−0.00972
Later- vs earlier-treated	0.6382	−0.00999	
Nonprofit hospitals, never-treated included			
Earlier- vs later-treated	0.0833	−0.00924	−0.01305
Later- vs earlier-treated	0.1469	−0.00999	
Treated vs never-treated	0.7698	−0.01405	
For-profit hospitals, not-yet-treated controls			
Earlier- vs later-treated	0.3597	−0.01036	−0.01188
Later- vs earlier-treated	0.6403	−0.01273	
For-profit hospitals, never-treated included			
Earlier- vs later-treated	0.0404	−0.01036	−0.02327
Later- vs earlier-treated	0.0718	−0.01273	
Treated vs never-treated	0.8878	−0.02471	

Notes. This table reports the Goodman-Bacon (2021) decomposition of the two-way fixed-effects estimate of the effect of expansion on the charity-care payer mix, run separately for nonprofit and for-profit hospitals and for each comparison group of Table III. Each row is one type of 2×2 comparison: earlier-treated cohorts against later-treated cohorts while the latter are still untreated, later-treated cohorts against earlier-treated cohorts after the latter are treated, and treated hospitals against never-treated hospitals. Column (1) is the type's share of the identifying weight, column (2) the weighted average of its 2×2 estimates, and column (3) the overall two-way fixed-effects estimate, the weight-weighted sum of column (2). The hospital samples, outcome, and cohort definitions are those of Table III; the never-treated block pools never-adopting states with states adopting after the identifying window, as in the never-treated-included columns of that table.

Table A.IV
Uncompensated Care per Percentage Point of Medicaid Coverage

	Reduced form (expansion $\to Y$) (1)	First stage (expansion $\to M$) (2)	Wald ratio $\theta = \mathrm{ATT}_Y/\mathrm{ATT}_M$ (3)	95% Anderson–Rubin interval (4)
A. Not-yet-treated hospitals in expansion states as controls				
Charity-care payer mix	−0.0107	0.0381	−0.279	[−0.426, −0.179]
	(0.0019)	(0.0063)	(0.058)	
Charity-care cost (share of net revenue)	−0.0155	0.0381	−0.407	[−0.636, −0.231]
	(0.0035)	(0.0063)	(0.095)	
Bad debt and uninsured (share of net revenue)	−0.0053	0.0381	−0.139	[−0.241, −0.055]
	(0.0017)	(0.0063)	(0.046)	
First-stage F	36.3	Observations 11,575	Hospitals 1,542	
B. Never-expansion states admitted as additional controls				
Charity-care payer mix	−0.0104	0.0286	−0.363	[−0.722, −0.212]
	(0.0020)	(0.0078)	(0.102)	
Charity-care cost (share of net revenue)	−0.0109	0.0286	−0.380	[−0.823, −0.142]
	(0.0035)	(0.0078)	(0.142)	
Bad debt and uninsured (share of net revenue)	−0.0063	0.0286	−0.221	[−0.462, −0.077]
	(0.0022)	(0.0078)	(0.080)	
First-stage F	13.5	Observations 19,453	Hospitals 2,197	

Notes. Column (1) reports the effect of Medicaid expansion on the named uncompensated-care outcome and column (2) the effect on the Medicaid payer mix, both from the covariate-adjusted nonprofit specification of Table II (column (2) of that table in Panel A, column (4) in Panel B); standard errors in parentheses are from the state-clustered multiplier bootstrap. Column (3) is the ratio of the two, the change in the outcome per one-percentage-point increase in the Medicaid payer mix, with a delta-method standard error in parentheses. Column (4) is a 95 percent Anderson–Rubin confidence set for the ratio, obtained by inverting the test of $\mathrm{ATT}_Y - \theta_0\,\mathrm{ATT}_M = 0$ over a grid of θ_0 using the same bootstrap draws for both effects, so the set is valid whatever the strength of the first stage. Both effects are estimated on the identical sample, whose hospital-years, hospitals, and first-stage F statistic are given beneath each panel. Outcomes are defined in the notes to Table II.

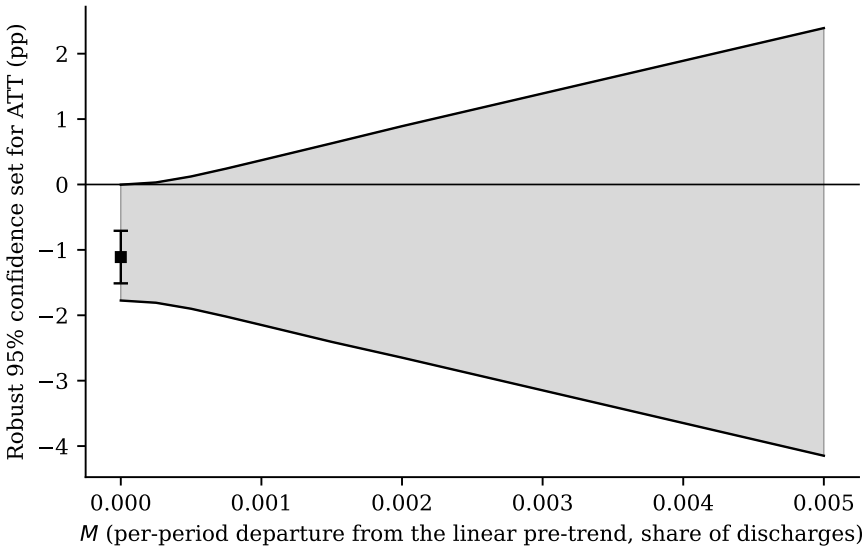


FIGURE A.I

SENSITIVITY TO DEPARTURES FROM THE LINEAR PRE-TREND

This figure plots the Rambachan and Roth (2023) smoothness sensitivity of the same post-adoption effect as Figure II. The restriction extrapolates the pre-adoption differential trend linearly and bounds how far the counterfactual may depart from that line: the horizontal axis is M, the largest permitted change between consecutive years in the slope of the differential trend, in units of the outcome (share of gross patient charges); $M = 0$ is exact linear extrapolation. The shaded region is the robust 95 percent confidence set at each M, in percentage points; the square and bar at the left are the conventional estimate and its 95 percent interval. The underlying event study, standard errors, sample, and specification are those of Figure II.

Table A.V
The Causal Ownership Contrast

	Not-yet-treated controls			Never-treated included		
	CDATT (1)	DATT (2)	Selection (3)	CDATT (4)	DATT (5)	Selection (6)
A. Doubly robust						
Charity-care payer mix	0.0082	0.0089	0.0007	0.0078	0.0098	0.0020
	(0.0024)	(0.0075)	(0.0066)	(0.0041)	(0.0079)	(0.0062)
Charity-care cost (share of net revenue)	0.0058	0.0071	0.0013	0.0048	0.0100	0.0052
	(0.0032)	(0.0063)	(0.0052)	(0.0053)	(0.0092)	(0.0062)
Bad debt and uninsured (share of net revenue)	−0.0023	0.0005	0.0028	0.0015	0.0038	0.0022
	(0.0022)	(0.0033)	(0.0026)	(0.0020)	(0.0023)	(0.0022)
B. CDATT by estimator component						
	IPW only	Regression only		IPW only	Regression only	
Charity-care payer mix	0.0064	0.0084		0.0074	0.0070	
	(0.0110)	(0.0022)		(0.0061)	(0.0028)	
Charity-care cost (share of net revenue)	0.0067	0.0078		0.0054	0.0060	
	(0.0082)	(0.0025)		(0.0049)	(0.0042)	
Bad debt and uninsured (share of net revenue)	−0.0018	−0.0010		0.0039	0.0037	
	(0.0060)	(0.0010)		(0.0027)	(0.0012)	
C. County covariates only (no hospital characteristics)						
	CDATT	DATT	Selection	CDATT	DATT	Selection
Charity-care payer mix	0.0054	0.0068	0.0015	0.0058	0.0095	0.0037
	(0.0022)	(0.0049)	(0.0047)	(0.0037)	(0.0083)	(0.0067)
Charity-care cost (share of net revenue)	0.0042	0.0072	0.0031	0.0027	0.0093	0.0066
	(0.0025)	(0.0054)	(0.0050)	(0.0044)	(0.0092)	(0.0066)
Bad debt and uninsured (share of net revenue)	−0.0043	0.0002	0.0045	0.0018	0.0048	0.0030
	(0.0019)	(0.0026)	(0.0021)	(0.0019)	(0.0022)	(0.0019)
Identified (g, t) cells		10			16	

Notes. Columns (1) and (4) report the causal ownership contrast of Caron (2025) described in the notes to Table III, Panel D: the nonprofit effect of Medicaid expansion minus the effect the same hospitals would have had under for-profit ownership, for the three uncompensated-care outcomes of that table, with not-yet-treated controls (columns (1)–(3)) and with never-treated states included (columns (4)–(6)). Columns (2) and (5) are the nonprofit effect minus the for-profit effect estimated on the same cohort–year cells, and columns (3) and (6) the difference between the two, the selection term of the decomposition in Section 5. Panel A conditions on the four county covariates of Table II and on log bed size, system membership, and the Medicaid payer mix in the year before adoption, and uses the doubly robust score. Panel B reports the contrast from the propensity weights alone (IPW only) and from the outcome regressions alone (Regression only). Panel C repeats Panel A conditioning on the four county covariates only. Effects are aggregated with treated-nonprofit weights over the cells in which all four groups are observed, whose number is given at the foot; cells without not-yet-treated for-profit hospitals, every cell of the 2019 cohort and the later cells of the 2017 cohort, are not identified in the not-yet-treated design. Propensities are trimmed at 0.005. Standard errors in parentheses are from the joint state-clustered multiplier bootstrap of Table III, so the three columns of each block are jointly bootstrapped. Appendix Table A.VI reports the cells.

TABLE A.VI
THE CAUSAL OWNERSHIP CONTRAST BY COHORT AND EVENT TIME

	$e = 0$	$e = 1$	$e = 2$	$e = 3$	Cohort
2014	−0.0001	0.0084	0.0139	0.0164	0.0096
	(0.0027)	(0.0035)	(0.0042)	(0.0055)	(0.0029)
	$w = 21.5\%$, ESS 30	$w = 21.5\%$, ESS 17	$w = 21.5\%$, ESS 13	$w = 21.5\%$, ESS 8	$w = 86.1\%$
2015	−0.0016	−0.0003	−0.0040	−0.0056	−0.0028
	(0.0038)	(0.0053)	(0.0036)	(0.0045)	(0.0053)
	$w = 3.3\%$, ESS 9	$w = 3.3\%$, ESS 8	$w = 3.3\%$, ESS 7	$w = 3.3\%$, ESS 7	$w = 13.1\%$
2017	0.0233	0.0344	—	—	0.0289
	(0.0161)	(0.0235)			(0.0198)
	$w = 0.4\%$, ESS 2	$w = 0.4\%$, ESS 2			$w = 0.8\%$
Event time	0.0001	0.0077	0.0115	0.0135	0.0082
	(0.0021)	(0.0033)	(0.0036)	(0.0048)	(0.0024)

Notes. Each cell reports the causal ownership contrast of Table III, Panel D, column (2) — charity-care payer mix, not-yet-treated controls, the four county covariates and three hospital characteristics — for adoption cohort g (rows) at event time $e = t - g$ (columns), with the joint state-clustered bootstrap standard error in parentheses and, beneath it, the cell's share w of the treated-nonprofit weight in the aggregate and the Kish effective sample size (ESS) of the reweighted comparison for-profit hospitals. The last column aggregates each cohort over its event times and the last row each event time over cohorts; the lower-right cell is the estimate reported in Table III. Cells are identified only where treated and comparison hospitals of both types are observed, so the 2016 cohort (three treated for-profit hospitals), the 2019 cohort (no later for-profit cohort), and event times 2 and 3 of the 2017 cohort do not appear. Estimates are in proportion units of gross patient charges.

TABLE A.VII
THE CAUSAL OWNERSHIP CONTRAST: NONPROFIT VERSUS GOVERNMENTAL HOSPITALS

	Not-yet-treated controls			Never-treated included		
	CDATT (1)	DATT (2)	Selection (3)	CDATT (4)	DATT (5)	Selection (6)
Charity-care payer mix	−0.0025	−0.0062	−0.0037	0.0002	−0.0022	−0.0024
	(0.0030)	(0.0034)		(0.0033)	(0.0037)	
Charity-care cost (share of net revenue)	0.0121	0.0099	−0.0022	−0.0052	−0.0069	−0.0016
	(0.0155)	(0.0239)		(0.0094)	(0.0062)	
Bad debt and uninsured (share of net revenue)	0.0025	−0.0100	−0.0125	−0.0018	0.0049	0.0067
	(0.0052)	(0.0071)		(0.0046)	(0.0034)	
Identified (g, t) cells		5			12	

Notes. Columns (1) and (4) report the causal contrast of Caron (2025) with governmental hospitals as the comparison ownership type: the nonprofit effect of Medicaid expansion minus the effect the same hospitals would have had under public ownership, conditional on the seven covariates of Table III, Panel D, under the same observable-subgroup-selection assumption, doubly robust score, treated-nonprofit weights, and joint state-clustered bootstrap. Columns (2) and (5) are the nonprofit effect minus the governmental effect on the same cohort–year cells, and columns (3) and (6) their difference, the selection term. Columns (1)–(3) use not-yet-treated controls and columns (4)–(6) also admit never-treated states. Cells are identified only where treated and comparison hospitals of both types are observed, whose number is given at the foot: with not-yet-treated controls these are the 2014 cohort through 2016 and the 2015 cohort through 2016, since the comparison governmental group falls to four hospitals from 2017 and the 2016, 2017, and 2019 cohorts have too few treated governmental hospitals to fit their outcome regressions; the reweighted comparison governmental group has an effective sample size between 5 and 22 in the identified cells. Governmental hospitals are defined in the notes to Appendix Table A.IX; outcomes in the notes to Table II.

TABLE A.VIII
LATE-ADOPTING COHORTS RETAINED AS CONTROLS

	Pre-treatment	No covariates		Covariates	
	(1) control mean	(2) Committed	(3) Controls	(4) Committed	(5) Controls
A. Nonprofit hospitals					
Charity-care payer mix	0.0306	−0.0089	−0.0113	−0.0107	−0.0116
		(0.0018)	(0.0019)	(0.0020)	(0.0019)
Charity-care cost (share of net revenue)	0.0244	−0.0107	−0.0112	−0.0155	−0.0125
		(0.0025)	(0.0022)	(0.0037)	(0.0043)
Uninsured and bad-debt cost	0.0259	−0.0047	−0.0039	−0.0053	−0.0076
		(0.0009)	(0.0011)	(0.0017)	(0.0017)
Medicaid payer mix	0.1465	0.0360	0.0350	0.0381	0.0314
		(0.0047)	(0.0039)	(0.0065)	(0.0069)
Commercial payer mix (placebo)	0.4155	−0.0040	−0.0107	−0.0116	−0.0037
		(0.0055)	(0.0031)	(0.0111)	(0.0071)
Log net patient revenue	18.5717	−0.0226	−0.0142	0.0685	−0.3288
		(0.0080)	(0.0096)	(0.0913)	(0.2015)
B. For-profit hospitals					
Charity-care payer mix	0.0253	−0.0126	−0.0131	−0.0071	−0.0188
		(0.0021)	(0.0026)	(0.0055)	(0.0068)
Charity-care cost (share of net revenue)	0.0214	−0.0162	−0.0127	−0.0107	−0.0145
		(0.0029)	(0.0045)	(0.0052)	(0.0102)
Uninsured and bad-debt cost	0.0263	−0.0078	−0.0088	−0.0049	−0.0107
		(0.0023)	(0.0016)	(0.0031)	(0.0047)
Medicaid payer mix	0.1633	0.0447	0.0523	0.0405	0.0467
		(0.0080)	(0.0081)	(0.0168)	(0.0140)
Commercial payer mix (placebo)	0.4125	−0.0010	−0.0136	−0.0124	−0.0181
		(0.0061)	(0.0052)	(0.0186)	(0.0167)
Log net patient revenue	18.7173	0.0128	0.0100	0.1772	0.2974
		(0.0188)	(0.0185)	(0.0875)	(0.1962)
C. Difference (nonprofit − for-profit)					
Charity-care payer mix	0.0306	0.0037	0.0019	−0.0036	0.0072
		(0.0021)	(0.0030)	(0.0053)	(0.0072)
		[0.0028]	[0.0032]	[0.0058]	[0.0070]
Charity-care cost (share of net revenue)	0.0244	0.0054	0.0015	−0.0048	0.0020
		(0.0031)	(0.0049)	(0.0050)	(0.0112)
		[0.0038]	[0.0050]	[0.0064]	[0.0110]
Uninsured and bad-debt cost	0.0259	0.0030	0.0049	−0.0004	0.0031
		(0.0022)	(0.0016)	(0.0027)	(0.0044)
		[0.0025]	[0.0020]	[0.0035]	[0.0050]
Medicaid payer mix	0.1465	−0.0087	−0.0173	−0.0023	−0.0153
		(0.0062)	(0.0064)	(0.0152)	(0.0132)
		[0.0093]	[0.0089]	[0.0180]	[0.0156]
Commercial payer mix (placebo)	0.4155	−0.0030	0.0029	0.0008	0.0144
		(0.0062)	(0.0055)	(0.0183)	(0.0197)
		[0.0083]	[0.0061]	[0.0217]	[0.0182]
Log net patient revenue	18.5717	−0.0354	−0.0241	−0.1086	−0.6262
		(0.0240)	(0.0206)	(0.1275)	(0.2027)
		[0.0205]	[0.0208]	[0.1265]	[0.2812]
Nonprofit hospitals		1,548	1,793	1,542	1,787
For-profit hospitals		208	292	208	292

Notes. Each cell reports the effect of Medicaid expansion from the estimator, sample rules, and not-yet-

Table A.IX
Nonprofit and Governmental Hospitals

	Pre-treatment	Not-yet-treated		Incl. never-treated
	(1) control mean	(2) No cov.	(3) Covariates	(4) No cov.
A. Nonprofit hospitals				
Charity-care payer mix	0.0306	−0.0089	−0.0107	−0.0108
		(0.0018)	(0.0020)	(0.0018)
Charity-care cost (share of net revenue)	0.0244	−0.0107	−0.0155	−0.0096
		(0.0025)	(0.0037)	(0.0019)
Uninsured and bad-debt cost	0.0259	−0.0047	−0.0053	−0.0033
		(0.0009)	(0.0017)	(0.0012)
Medicaid payer mix	0.1465	0.0360	0.0381	0.0319
		(0.0047)	(0.0065)	(0.0043)
Commercial payer mix (placebo)	0.4155	−0.0040	−0.0116	−0.0080
		(0.0055)	(0.0111)	(0.0031)
Log net patient revenue	18.5717	−0.0226	0.0685	−0.0080
		(0.0080)	(0.0913)	(0.0070)
B. Governmental hospitals				
Charity-care payer mix	0.0291	−0.0151	−0.0098	−0.0083
		(0.0045)	(0.0034)	(0.0031)
Charity-care cost (share of net revenue)	0.0120	−0.0616	0.0125	−0.0066
		(0.0345)	(0.0127)	(0.0056)
Uninsured and bad-debt cost	0.0432	−0.0079	0.0098	−0.0077
		(0.0028)	(0.0049)	(0.0031)
Medicaid payer mix	0.1661	0.0336	0.0528	0.0422
		(0.0132)	(0.0086)	(0.0074)
Commercial payer mix (placebo)	0.4000	−0.0384	−0.0164	−0.0049
		(0.0121)	(0.0103)	(0.0045)
Log net patient revenue	17.7299	0.0764	−0.0302	0.0228
		(0.0351)	(0.1891)	(0.0181)
Nonprofit hospitals		1,548	1,542	2,203
Governmental hospitals		368	367	789

Notes. Each cell reports the effect of Medicaid expansion from the estimator, sample rules, and bootstrap of Table II, estimated separately for nonprofit hospitals (Panel A, repeating that table) and governmental hospitals (Panel B), which enter no estimation sample in the main tables. Governmental hospitals are those whose Cost Report provider control type is a federal, state, county, city, or district government entity. Columns (2)–(3) use not-yet-treated hospitals in adopting states as the comparison group, column (3) with the four county covariates of Table II; column (4) also admits never-adopting states and states adopting after the window, without covariates. The never-treated-included specification with covariates is omitted because the propensity model separates in the thin governmental cohort cells and the doubly robust estimator is undefined. Column (1) is the 2011–2013 mean among the column (2) comparison hospitals of each type. Of the 368 governmental hospitals in retained cohorts, 301 are in the 2014 cohort, 36 in 2017, 18 in 2015, 9 in 2016, and 4 in 2019. Outcomes are defined in the notes to Table II; uninsured and bad-debt cost is the bad debt and uninsured outcome of that table.

TABLE A.X
NONPROFIT VERSUS GOVERNMENTAL HOSPITALS: TRIPLE DIFFERENCE

	Pre-treatment	Not-yet-treated		Incl. never-treated
	(1) control mean	(2) No cov.	(3) Covariates	(4) No cov.
Charity-care payer mix	0.0306	0.0062	−0.0009	−0.0025
		(0.0035)	(0.0038)	(0.0031)
		[0.0049]	[0.0039]	[0.0036]
Charity-care cost (share of net revenue)	0.0244	0.0508	−0.0280	−0.0030
		(0.0345)	(0.0125)	(0.0052)
		[0.0346]	[0.0132]	[0.0059]
Uninsured and bad-debt cost	0.0259	0.0031	−0.0151	0.0044
		(0.0026)	(0.0051)	(0.0029)
		[0.0030]	[0.0052]	[0.0033]
Medicaid payer mix	0.1465	0.0024	−0.0146	−0.0103
		(0.0104)	(0.0080)	(0.0064)
		[0.0140]	[0.0108]	[0.0086]
Commercial payer mix (placebo)	0.4155	0.0344	0.0048	−0.0031
		(0.0150)	(0.0147)	(0.0045)
		[0.0133]	[0.0151]	[0.0055]
Log net patient revenue	18.5717	−0.0990	0.0987	−0.0308
		(0.0260)	(0.2039)	(0.0175)
		[0.0360]	[0.2100]	[0.0194]

Notes. Each cell is the nonprofit estimate minus the governmental estimate from the corresponding column of Appendix Table A.IX, the governmental analogue of Panel C of Table III. Column (1) is the nonprofit pre-treatment comparison mean of that table. Standard errors in parentheses are from a joint state-clustered multiplier bootstrap in which both ownership arms are perturbed by the same draws; the bracketed figure beneath is the standard error under independence of the two arms, $\sqrt{SE_{NP}^2 + SE_G^2}$. Columns, samples, covariates, and the omitted specification are as in Appendix Table A.IX.

TABLE A.XI
THE EXTENSIVE MARGIN OF SCHEDULE H REPORTING

	Share at zero, 2011–2013 (1)	Not-yet-treated controls (2)	Never-treated included (3)
Source: the freed obligation			
Charity care (financial assistance)	0.018	−0.0087	−0.0109
		(0.0078)	(0.0045)
Discretionary direct patient aid (s^D)			
Subsidized services, net (Line 7g)	0.487	0.0159	0.0019
		(0.0181)	(0.0146)
Gross subsidized expense	0.479	0.0306	0.0016
		(0.0139)	(0.0150)
Community health improvement services (Line 7e)	0.173	0.0560	0.0100
		(0.0166)	(0.0165)
with Part II community-building items	0.146	0.0320	0.0072
		(0.0212)	(0.0134)
Private-return uses (s^R)			
Education, research, and community building	0.285	0.0520	0.0026
		(0.0079)	(0.0157)
Health-professions education	0.355	0.0583	0.0110
		(0.0179)	(0.0180)
Research	0.813	0.0190	−0.0059
		(0.0104)	(0.0121)
Workforce development	0.762	0.0231	−0.0160
		(0.0122)	(0.0149)
Physical improvements and housing	0.917	0.0255	0.0051
		(0.0123)	(0.0100)
Economic development	0.853	0.0046	0.0005
		(0.0178)	(0.0171)
Memorandum			
Medicaid-funded programs (Line 7b)	0.099	−0.0024	−0.0047
		(0.0187)	(0.0074)
Observations		5,858	10,194

Notes. Each cell in columns (2)–(3) reports the effect of Medicaid expansion on an indicator that the filer reports any positive amount in the category, from the estimator, sample rules, retained cohorts, and comparison groups of Table IV without covariates, so estimates are changes in the probability of reporting, in proportion units; standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. Column (2) uses not-yet-treated filers as the comparison group and reproduces column (3) of Appendix Table A.I; column (3) also admits never-adopting filers and filers in states adopting after the window. Column (1) is the share of 2011–2013 filer-years among treated filers reporting exactly zero. Categories follow the Schedule H lines given in the notes to Table IV; community health improvement services is Part I line 7e alone, shown also with the Part II community-building items (community support, environmental improvements, leadership development, coalition building, and health advocacy) added, which is the aggregate Table IV reports. Observations are filer-years.

Table A.XII
Schedule H Changes in Dollars per Filer-Year

	Pre-treatment	Not-yet-treated controls		Never-treated included	
	mean	Raw	Winsorized	Raw	Winsorized
	(1)	(2)	(3)	(4)	(5)
Source: the freed obligation					
Charity care (financial assistance)	3.82	−1.112	−1.050	−1.213	−1.124
		(0.254)	(0.235)	(0.342)	(0.274)
Discretionary direct patient aid (s^D)					
Subsidized services, net (Line 7g)	1.69	0.255	0.190	0.225	0.175
		(0.088)	(0.086)	(0.142)	(0.120)
Gross subsidized expense	5.43	1.351	1.164	0.427	0.481
		(0.236)	(0.221)	(0.396)	(0.250)
Less: direct offsetting revenue	3.73	1.097	0.873	0.194	0.200
		(0.204)	(0.182)	(0.315)	(0.177)
Community health improvement services (Line 7e)	1.00	0.112	0.021	0.068	0.011
		(0.097)	(0.033)	(0.102)	(0.038)
Part II community-building items	0.11	−0.040	−0.006	−0.055	−0.002
		(0.039)	(0.005)	(0.046)	(0.004)
Private-return uses (s^R)					
Education, research, and community building	6.50	0.604	0.166	0.558	0.067
		(0.221)	(0.069)	(0.376)	(0.138)
Health-professions education	4.52	0.378	0.125	0.443	0.105
		(0.156)	(0.069)	(0.273)	(0.110)
Sum of categories other than charity care					
Net subsidized + 7e + Part II + education	9.32	0.931	0.394	0.797	0.199
		(0.391)	(0.117)	(0.588)	(0.231)
Sum as share of the charity-care decline		84%	38%	66%	18%

Notes. Each cell in columns (2)–(5) reports the effect of Medicaid expansion on the category's reported amount in millions of nominal dollars per filer-year, from the estimator, sample rules, retained cohorts, and comparison groups of Table IV without covariates; standard errors in parentheses are from a state-clustered multiplier bootstrap with 500 draws. Columns (2)–(3) use not-yet-treated filers as the comparison group and columns (4)–(5) also admit never-adopting filers and filers in states adopting after the window. Columns (3) and (5) winsorize each category at the 99th percentile of filer-years before estimation. Column (1) is the 2011–2013 mean among treated filers, in millions. Categories follow the Schedule H lines given in the notes to Table IV; community health improvement services (Part I line 7e) and the Part II community-building items are shown separately. The sum row estimates the sum of net subsidized services, line 7e, the Part II items, and education, research, and community building as a single outcome, so it carries its own standard error; the final row divides that estimate by the charity-care estimate in the same column.

Table A.XIII
Are the Covariates Affected by Expansion? Covariates as Outcomes

	Pre-treatment	Post-expansion ATT		Percent of	Pre-trend
	mean	Estimate	(s.e.)	mean	Wald p
	(1)	(2)	(3)	(4)	(5)
A. Hospital-year panel, nonprofit hospitals					
Uninsured rate	12.68	−1.3269	(0.3022)	−10.5	0.012
Unemployment rate	8.89	−0.5437	(0.1606)	−6.1	0.558
Nonwhite share	25.10	0.2553	(0.0860)	1.0	0.025
Median household income	53,140	425	(155)	0.8	0.775
Adults 25 and over (share)	67.32	−0.1005	(0.0666)	−0.1	0.178
Bed size	168.78	1.3607	(0.6386)	0.8	0.621
Critical access hospital	0.26	—		—	—
Disability rate	13.06	0.0511	(0.0496)	0.4	0.004
High-school completion rate	86.80	−0.2220	(0.0926)	−0.3	0.105
Median home value	207,108	2,902	(4,250)	1.4	0.006
Population density	1,648	3	(18)	0.2	0.061
Public coverage rate	31.64	1.5870	(0.4522)	5.0	0.030
Public-transit commuting share	4.16	0.0300	(0.0314)	0.7	0.000
Veteran share	9.85	−0.0604	(0.0772)	−0.6	0.266
B. Hospital-year panel, for-profit hospitals					
Uninsured rate	15.59	−1.2133	(0.4365)	−7.8	0.248
Unemployment rate	9.80	−0.6899	(0.1936)	−7.0	0.177
Nonwhite share	37.61	0.1944	(0.0834)	0.5	0.345
Median household income	53,487	243	(257)	0.5	0.011
Adults 25 and over (share)	65.81	0.1138	(0.1137)	0.2	0.001
Bed size	180.00	1.3142	(1.3103)	0.7	0.030
Critical access hospital	0.03	—		—	—
Disability rate	12.80	0.4099	(0.0914)	3.2	0.002
High-school completion rate	84.14	−0.6073	(0.0788)	−0.7	0.365
Median home value	229,746	9,482	(8,340)	4.1	0.001
Population density	1,210	16	(9)	1.3	0.139
Public coverage rate	30.82	1.8629	(0.3563)	6.0	0.017
Public-transit commuting share	3.50	0.0701	(0.0680)	2.0	0.005
Veteran share	9.22	−0.0341	(0.0585)	−0.4	0.714
C. Schedule H panel					
Uninsured rate	12.67	−0.3821	(0.1511)	−3.0	0.000
Unemployment rate	8.84	−0.1956	(0.1367)	−2.2	0.550
Nonwhite share	23.93	−0.0170	(0.1475)	−0.1	0.751
Median household income	52,307	−4	(120)	0.0	0.046
Disability rate	13.35	0.0207	(0.0515)	0.2	0.028
High-school completion rate	86.69	0.0311	(0.1213)	0.0	0.082
Male share	49.47	0.0315	(0.0230)	0.1	0.079
Owner-occupied share	68.51	−0.2357	(0.0872)	−0.3	0.145
Public coverage rate	32.39	0.4599	(0.1386)	1.4	0.042

Notes. Each row treats one covariate as the outcome and reports the effect of Medicaid expansion on it from the estimator, sample rules, and not-yet-treated comparison group of Table II, without covariates, the diagnostic of Caetano, Callaway, Payne, and Sant'Anna (2026, Remark 6) for covariates that treatment itself affects. Column (1) is the 2011–2013 mean among treated units; column (2) the effect aggregated over the four years from adoption, with its state-clustered multiplier-bootstrap standard error in column (3); column (4) the effect as a percent of column (1); column (5) the p-value of a joint Wald test, using the bootstrap covariance across leads, that the three pre-adoption event-time coefficients are zero. Panels A and B are the nonprofit and for-profit hospital-year samples of Table III, Panel C the Schedule H filer sample of Table IV. Covariates are county-level rates from the American Community Survey five-year estimates, in percent except median household income and median home value (dollars) and population density, together with bed size, critical-access status, and system independence from the Cost Reports. A dash marks a covariate for which the effect is not estimated.

Table A.XIV
Main Estimates under Alternative Treatments of the Covariates

	Linear imputation, four covariates				DR,
	Exclude	Include X_t	Pre-treatment X_{g-1}	Covariate unconfoundedness	base-period values
	(1)	(2)	(3)	(4)	(5)
A. Nonprofit hospitals					
Charity-care payer mix	−0.0090	−0.0102	−0.0112	−0.0112	−0.0107
	(0.0021)	(0.0098)	(0.0050)	(0.0048)	(0.0019)
Charity-care cost (share of net revenue)	−0.0108	−0.0125	−0.0125	−0.0126	−0.0155
	(0.0031)	(0.0114)	(0.0046)	(0.0046)	(0.0035)
Bad debt and uninsured (share of net revenue)	−0.0047	−0.0057	−0.0060	−0.0060	−0.0053
	(0.0011)	(0.0084)	(0.0064)	(0.0056)	(0.0017)
Medicaid payer mix	0.0361	0.0381	0.0369	0.0368	0.0381
	(0.0057)	(0.0178)	(0.0113)	(0.0117)	(0.0063)
Medicare payer mix	−0.0072	−0.0115	−0.0055	−0.0058	−0.0038
	(0.0028)	(0.0062)	(0.0061)	(0.0056)	(0.0149)
Commercial payer mix	−0.0041	0.0024	−0.0042	−0.0041	−0.0116
	(0.0078)	(0.0130)	(0.0179)	(0.0155)	(0.0113)
Net profit margin	0.0025	0.0042	0.0033	0.0033	0.0172
	(0.0058)	(0.0161)	(0.0099)	(0.0101)	(0.0074)
Log net patient revenue	−0.0224	0.0118	−0.0038	−0.0041	0.0685
	(0.0152)	(0.2214)	(0.0685)	(0.0715)	(0.0901)
B. For-profit hospitals					
Charity-care payer mix	−0.0126	−0.0419	−0.0172	−0.0174	−0.0071
	(0.0059)	(0.0252)	(0.0056)	(0.0053)	(0.0056)
Charity-care cost (share of net revenue)	−0.0162	−0.0373	−0.0195	−0.0200	−0.0107
	(0.0049)	(0.0267)	(0.0097)	(0.0099)	(0.0053)
Bad debt and uninsured (share of net revenue)	−0.0079	−0.0117	−0.0051	−0.0061	−0.0049
	(0.0045)	(0.0053)	(0.0034)	(0.0034)	(0.0031)
Medicaid payer mix	0.0444	0.0608	0.0476	0.0470	—
	(0.0097)	(0.0131)	(0.0158)	(0.0144)	
Medicare payer mix	−0.0089	−0.0328	−0.0077	−0.0126	—
	(0.0060)	(0.0233)	(0.0050)	(0.0081)	
Commercial payer mix	−0.0003	0.0097	−0.0017	−0.0091	—
	(0.0103)	(0.0199)	(0.0086)	(0.0140)	
Net profit margin	−0.0079	0.0204	−0.0173	−0.0201	—
	(0.0235)	(0.0397)	(0.0229)	(0.0231)	
Log net patient revenue	0.0130	0.1622	−0.0049	−0.0108	—
	(0.0440)	(0.1198)	(0.0440)	(0.0460)	
C. Difference (nonprofit − for-profit)					
Charity-care payer mix	0.0036	0.0317	0.0060	0.0062	−0.0036
	(0.0037)	(0.0167)	(0.0034)	(0.0035)	(0.0059)
Charity-care cost (share of net revenue)	0.0055	0.0248	0.0071	0.0075	−0.0048
	(0.0038)	(0.0150)	(0.0061)	(0.0060)	(0.0063)
Bad debt and uninsured (share of net revenue)	0.0033	0.0061	−0.0008	0.0001	−0.0004
	(0.0049)	(0.0053)	(0.0046)	(0.0037)	(0.0035)
Medicaid payer mix	−0.0083	−0.0227	−0.0106	−0.0102	—
	(0.0069)	(0.0288)	(0.0114)	(0.0130)	
Medicare payer mix	0.0017	0.0212	0.0022	0.0068	—
	(0.0057)	(0.0188)	(0.0061)	(0.0068)	
Commercial payer mix	−0.0038	−0.0072	−0.0025	0.0050	—
	(0.0065)	(0.0260)	(0.0224)	(0.0194)	
Net profit margin	0.0104	−0.0161	0.0206	0.0234	—
	(0.0267)	(0.0515)	(0.0254)	(0.0253)	
Log net patient revenue	−0.0353	−0.1504	0.0012	0.0068	—
	(0.0525)	(0.2852)	(0.0971)	(0.0986)	

Notes. Each cell reports the effect of Medicaid expansion from the sample rules, not-yet-treated comparison group, and four pre-committed covariates of Table II, under five treatments of the covariates. Columns (1)–(4) are the linear imputation estimators of Caetano, Callaway, Payne, and Sant'Anna (2026, Section 6.1), each built from the same cohort–year cells and aggregated with treated-unit weights: column (1) omits the covariates; column (2) conditions on their contemporaneous values X_t together with their values in the year before adoption, X_{g-1}; column (3) conditions on X_{g-1} only; column (4) replaces each treated hospital's post-adoption covariates with values imputed from not-yet-treated hospitals with the same base-period covariates and base-period outcome, their covariate unconfoundedness estimand. Column (5) reprints the doubly robust estimates of Tables II and III, which condition on base-period values with the doubly robust score rather than a linear outcome model; a dash marks an outcome those tables do not report. Standard errors in parentheses are from a state pairs-cluster bootstrap with 499 draws using the same draws across columns and ownership arms, so Panel C's are joint; column (5)'s are those of the source tables. Panels and outcomes are as in Table III.

Table A.XV
Doubly Robust Estimates under Covariate Unconfoundedness

	Covariate unconfoundedness		CS DR,
	Doubly robust, parametric (1)	Doubly robust, random forest (2)	base-period values (3)
A. Nonprofit hospitals			
Charity-care payer mix	−0.0112	−0.0108	−0.0107
	(0.0014)	(0.0014)	(0.0019)
Charity-care cost (share of net revenue)	−0.0126	−0.0128	−0.0155
	(0.0016)	(0.0018)	(0.0035)
Bad debt and uninsured (share of net revenue)	−0.0058	−0.0043	−0.0053
	(0.0011)	(0.0010)	(0.0017)
Medicaid payer mix	0.0365	0.0350	0.0381
	(0.0046)	(0.0049)	(0.0063)
Medicare payer mix	−0.0059	−0.0041	−0.0038
	(0.0026)	(0.0027)	(0.0149)
Commercial payer mix	−0.0037	−0.0054	−0.0116
	(0.0055)	(0.0052)	(0.0113)
Net profit margin	0.0033	0.0070	0.0172
	(0.0053)	(0.0054)	(0.0074)
Log net patient revenue	−0.0042	−0.0076	0.0685
	(0.0069)	(0.0118)	(0.0901)
B. For-profit hospitals			
Charity-care payer mix	−0.0184	−0.0135	−0.0071
	(0.0028)	(0.0015)	(0.0056)
Charity-care cost (share of net revenue)	−0.0204	−0.0152	−0.0107
	(0.0043)	(0.0028)	(0.0053)
Bad debt and uninsured (share of net revenue)	−0.0072	−0.0073	−0.0049
	(0.0031)	(0.0018)	(0.0031)
Medicaid payer mix	0.0460	0.0437	—
	(0.0103)	(0.0114)	
Medicare payer mix	−0.0109	−0.0056	—
	(0.0039)	(0.0072)	
Commercial payer mix	−0.0090	−0.0200	—
	(0.0089)	(0.0191)	
Net profit margin	−0.0193	−0.0088	—
	(0.0166)	(0.0116)	
Log net patient revenue	−0.0047	0.0047	—
	(0.0157)	(0.0166)	
C. Difference (nonprofit − for-profit), joint SE			
Charity-care payer mix	0.0071	0.0027	−0.0036
	(0.0030)	(0.0022)	(0.0059)
Charity-care cost (share of net revenue)	0.0078	0.0024	−0.0048
	(0.0047)	(0.0030)	(0.0063)
Bad debt and uninsured (share of net revenue)	0.0014	0.0031	−0.0004
	(0.0028)	(0.0017)	(0.0035)
Medicaid payer mix	−0.0095	−0.0087	—
	(0.0093)	(0.0090)	
Medicare payer mix	0.0050	0.0015	—
	(0.0047)	(0.0064)	
Commercial payer mix	0.0053	0.0146	—
	(0.0113)	(0.0253)	
Net profit margin	0.0225	0.0158	—
	(0.0137)	(0.0117)	
Log net patient revenue	0.0005	−0.0123	—
	(0.0192)	(0.0283)	

Notes. Each cell reports the effect of Medicaid expansion from the sample rules, not-yet-treated comparison group, and four pre-committed covariates of Table II, treating the covariates as ones expansion may itself affect. Columns (1)–(2) are the Neyman-orthogonal estimator of Caetano, Callaway, Payne, and Sant'Anna (2026, Section 6.2) under their covariate unconfoundedness assumption, with the base-period outcome as the auxiliary variable W: the covariate values a treated hospital would have had absent expansion are learned from not-yet-treated hospitals with the same base-period covariates and outcome, and the score is robust to misspecification of either the outcome models or the propensity score. Column (1) estimates the nuisance functions parametrically (linear outcome and odds models and a logit propensity, their Estimator 8); column (2) by random forests of 300 trees with minimum leaf size 5 and five-fold cross-fitting, with the nested pseudo-outcomes taken from out-of-bag predictions (their Estimator 9). Propensity scores are trimmed to $[0.01, 0.99]$. Effects are aggregated with treated-count weights over each cohort's four post-adoption years. Column (3) reprints the Callaway–Sant'Anna estimates of Tables II and III, which condition on base-period values only; a dash marks an outcome those tables do not report. Standard errors in parentheses sum the score's per-unit contributions by state and apply the multiplier draws used throughout, so Panel C's are joint. Panels and outcomes are as in Table III.

TABLE A.XVI
LASSO COVARIATE SCREENS ON THE REPAIRED COVARIATES

	Outcome-change screen		Timing screen
	Nonprofit (1)	For-profit (2)	(states) (3)
A. Charity-care payer mix: share of 100 draws selecting each covariate			
Adults 25 and over (share)	0.03	0.00	0.02
Bed size	0.00	0.00	0.00
Critical access hospital	0.00	0.00	0.00
Disability rate	0.05	0.00	0.00
High-school completion rate	1.00	0.00	0.00
Median household income	0.00	0.00	0.02
Independent hospital	0.05	0.00	0.00
Median home value	0.05	0.00	0.00
Nonwhite share	1.00	0.00	0.00
Owner-occupied share	0.00	0.00	0.00
Population density	0.00	0.00	0.00
Poverty rate	0.03	0.00	0.00
Private coverage rate	0.00	0.00	0.00
Public-transit commuting share	0.00	0.00	0.00
Public coverage rate	0.00	0.00	0.00
Renter-occupied share	0.05	0.00	0.00
Unemployment rate	0.03	0.00	0.00
Uninsured rate	0.03	0.00	0.00
Veteran share	0.03	0.00	0.00
Any interaction term	1.00	0.00	0.99
Units	2,197	503	41
B. Other hospital-year outcomes: covariates selected in at least half of the draws			
Charity-care cost (share of net revenue)	1	0	1
Bad debt and uninsured (share of net revenue)	4	0	0
Medicaid payer mix	1	0	0
Medicare payer mix	9	1	1
Commercial payer mix	9	0	0
Net profit margin	0	1	1
Log net patient revenue	0	3	0
C. Schedule H outcomes: covariates selected in at least half of the draws			
Charity care (financial assistance)	0	—	0
Subsidized services, net	1	—	0
Gross subsidized expense	1	—	0
Community health improvement	0	—	0
Community investment (aggregate)	0	—	0
Education, research, and community building	1	—	0
Health-professions education	0	—	0
Research	2	—	4
Medicaid-funded programs	0	—	0

Notes. This table reports two cross-validated LASSO screens run on pre-expansion data, each repeated over 100 random draws of the cross-validation folds. The outcome-change screen (columns (1)–(2)) regresses each unit's 2011–2013 change in the outcome on its 2013 values of the candidate covariates, those values interacted with quartile indicators of the unit's 2011–2013 mean outcome, and the indicators themselves, separately for nonprofit and for-profit hospitals; Schedule H filers are nonprofit only, hence the dashes in column (2) of Panel C. The timing screen (column (3)) is a weighted binomial LASSO of early (2014–2016) against late (2019 or later) adoption on state means of the same terms and their interactions with nonprofit status, among expansion states. The penalty is $\sqrt{\lambda_{\min}\lambda_{1se}}$, the geometric

TABLE A.XVII
COVARIATES SELECTED BY THE LASSO SCREENS, BY OUTCOME

	Adults 25 and over (share)	Bed size	Critical access hospital	Disability rate	High-school completion rate	Median home value	Median household income	Nonwhite share	Population density	Public coverage rate	Public-transit commuting share	Veteran share
A. Hospital-year outcomes												
Charity-care payer mix					x			x				
Charity-care cost (share of net revenue)							x		x			
Bad debt and uninsured (share of net revenue)					x		x				x	x
Medicaid payer mix						x						
Medicare payer mix	x	x	x		x							
Commercial payer mix		x	x	x		x						
Net profit margin						x	x					
Log net patient revenue				x		x				x		

	Disability rate	High-school completion rate	Male share	Owner-occupied share	Public coverage rate	Unemployment rate
B. Schedule H outcomes						
Charity care (financial assistance)						
Subsidized services, net			x			
Gross subsidized expense			x			
Community health improvement						
Community investment (aggregate)						
Education, research, and community building		x				
Health-professions education						
Research	x			x	x	x
Medicaid-funded programs						

Notes. An x marks a covariate whose main effect was selected in at least half of the 100 fold draws in any of the three screens of Appendix Table A.XVI, ranked by its mean selection share across the screens and capped at four per outcome, the size of the pre-committed set; the cap binds for the Medicare payer mix, the commercial payer mix, and research. A blank row means no covariate was retained for that outcome. Panel A lists the hospital-year outcomes of Table II and Panel B the Schedule H outcomes of Table IV. These sets are used only in Appendix Table A.XVIII; every table in the main text uses the four pre-committed covariates.

TABLE A.XVIII
MAIN ESTIMATES UNDER THE SCREENED COVARIATE SETS

	Not-yet-treated controls			Never-treated included		
	(1)	(2)	(3)	(4)	(5)	(6)
Covariates	none	four	screened	none	four	screened
A. Nonprofit hospitals						
Charity-care payer mix	−0.0089	−0.0107	−0.0095	−0.0108	−0.0104	−0.0107
	(0.0019)	(0.0019)	(0.0018)	(0.0019)	(0.0021)	(0.0019)
Charity-care cost (share of net revenue)	−0.0107	−0.0155	−0.0116	−0.0096	−0.0109	−0.0124
	(0.0025)	(0.0036)	(0.0026)	(0.0019)	(0.0036)	(0.0028)
Bad debt and uninsured (share of net revenue)	−0.0047	−0.0053	−0.0051	−0.0033	−0.0063	−0.0050
	(0.0009)	(0.0016)	(0.0011)	(0.0012)	(0.0023)	(0.0067)
Medicaid payer mix	0.0360	0.0381	0.0345	0.0319	0.0286	0.0296
	(0.0048)	(0.0065)	(0.0039)	(0.0044)	(0.0078)	(0.0038)
Medicare payer mix	−0.0072	−0.0038	−0.0084	−0.0040	0.0067	−0.0045
	(0.0025)	(0.0150)	(0.0046)	(0.0033)	(0.0088)	(0.0038)
Commercial payer mix	−0.0040	−0.0116	−0.0009	−0.0080	−0.0025	−0.0065
	(0.0054)	(0.0112)	(0.0044)	(0.0033)	(0.0069)	(0.0032)
Net profit margin	0.0025	0.0172	0.0017	0.0004	0.0109	−0.0002
	(0.0043)	(0.0075)	(0.0071)	(0.0035)	(0.0065)	(0.0046)
Log net patient revenue	−0.0226	0.0685	−0.0953	−0.0080	−0.0944	0.0519
	(0.0082)	(0.0905)	(0.0895)	(0.0070)	(0.1133)	(0.1184)
B. For-profit hospitals						
Charity-care payer mix	−0.0126	−0.0071	−0.0144	−0.0186	−0.0213	−0.0190
	(0.0020)	(0.0051)	(0.0023)	(0.0032)	(0.0047)	(0.0029)
Charity-care cost (share of net revenue)	−0.0162	−0.0107	−0.0170	−0.0175	−0.0202	−0.0211
	(0.0030)	(0.0050)	(0.0034)	(0.0040)	(0.0051)	(0.0039)
Bad debt and uninsured (share of net revenue)	−0.0078	−0.0049	−0.0082	−0.0084	−0.0055	−0.0108
	(0.0022)	(0.0031)	(0.0105)	(0.0015)	(0.0049)	(0.0058)
Medicaid payer mix	0.0447	0.0405	0.0363	0.0503	0.0428	0.0458
	(0.0086)	(0.0169)	(0.0077)	(0.0077)	(0.0160)	(0.0064)
Medicare payer mix	−0.0086	−0.0025	−0.0162	−0.0009	−0.0021	—
	(0.0058)	(0.0167)	(0.0043)	(0.0052)	(0.0127)	
Commercial payer mix	−0.0010	−0.0124	0.0175	−0.0128	−0.0081	—
	(0.0061)	(0.0177)	(0.0135)	(0.0045)	(0.0151)	
Net profit margin	−0.0081	0.0385	−0.0239	−0.0143	0.0054	−0.0246
	(0.0133)	(0.0249)	(0.0375)	(0.0147)	(0.0261)	(0.0214)
Log net patient revenue	0.0128	0.1772	−0.1238	−0.0124	0.0848	−0.0158
	(0.0186)	(0.0928)	(0.1489)	(0.0153)	(0.2060)	(0.1652)
C. Difference (nonprofit − for-profit), joint bootstrap SE						
Charity-care payer mix	0.0037	−0.0036	0.0049	0.0078	0.0110	0.0083
	(0.0022)	(0.0051)	(0.0024)	(0.0035)	(0.0053)	(0.0032)
Charity-care cost (share of net revenue)	0.0054	−0.0048	0.0055	0.0080	0.0093	0.0087
	(0.0032)	(0.0052)	(0.0034)	(0.0041)	(0.0063)	(0.0038)
Bad debt and uninsured (share of net revenue)	0.0030	−0.0004	0.0031	0.0051	−0.0009	0.0059
	(0.0022)	(0.0028)	(0.0096)	(0.0017)	(0.0057)	(0.0090)
Medicaid payer mix	−0.0087	−0.0023	−0.0019	−0.0184	−0.0142	−0.0162
	(0.0065)	(0.0154)	(0.0080)	(0.0060)	(0.0151)	(0.0052)
Medicare payer mix	0.0014	−0.0013	0.0078	−0.0031	0.0088	—
	(0.0055)	(0.0177)	(0.0055)	(0.0052)	(0.0143)	
Commercial payer mix	−0.0030	0.0008	−0.0185	0.0048	0.0056	—
	(0.0066)	(0.0180)	(0.0134)	(0.0051)	(0.0165)	
Net profit margin	0.0106	−0.0213	0.0256	0.0147	0.0055	0.0244
	(0.0151)	(0.0253)	(0.0341)	(0.0137)	(0.0267)	(0.0191)
Log net patient revenue	−0.0354	−0.1086	0.0285	0.0044	−0.1792	0.0677
	(0.0242)	(0.1326)	(0.1847)	(0.0168)	(0.2146)	(0.2236)
D. Schedule H, nonprofit filers (log points)						