Crowding out in the safety net?

FQHC expansion and local health care markets

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Abstract

To what extent does government support for safety-net providers add care in underserved markets, and to what extent does it crowd out care from unsubsidized providers? I study this in the context of the United States' Federally Qualified Health Center (FQHC) program. Using a sample of primary care markets with subsidized FQHC entry during 2019–2024 and data on Medicaid claims by every provider in those markets, I estimate the extent to which entry of FQHCs crowds out other providers. Using a difference-in-differences design, I find that a $1 increase in FQHC billing is associated with a 37 cent reduction in billing by other primary-care providers. Patient counts show the same shift. On the extensive margin, for every FQHC entrant, there is an associated 0.198 increase in other primary-care providers that exit the Medicaid billing market. Lastly, an analysis of a longer county panel shows mortality reductions after FQHC entry, with significant effects concentrated in maternal/neonatal mortality and HIV mortality. This suggests that despite moderate crowd-out, the reallocation of care toward FQHCs may improve health outcomes through higher quality care.

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1 Introduction

Governments around the world heavily regulate and intervene in health care markets. Often, the goal of these interventions is to correct some market failure or to improve access to care as a form of redistribution.[1] As one example, in the United States, the federal government subsidizes both the demand side of the market, by insuring low-income individuals through Medicaid, and the supply side by subsidizing providers to serve those low-income populations. Understanding the impact of such interventions and how they interact with the private market is vital to designing effective policy.

In this paper, I study a large supply-side subsidy for primary care in the United States: support for Federally Qualified Health Centers (FQHCs). This program offers direct federal grants to a specific group of nonprofit providers (FQHCs) that offer care to "underserved" populations and geographies. These direct grants accounted for about thirty percent of total health-center revenue in 2017,[2] and this direct financial support is supplemented by enhanced Medicaid and Medicare payments, 340B drug pricing, and other regulatory benefits. The FQHC program was originally intended to ensure that low-income populations on Medicaid or without insurance have access to necessary primary care services.

This first version of the health center program began at the same time as Medicaid in 1965. But its scope has expanded significantly in the past twenty-five years. Beginning with the Bush administration's Health Centers Initiative from 2002 to 2007 and buoyed by increased funding from the Affordable Care Act in 2011, these providers are now major players in many primary care markets. In 2001, health centers treated about 10 million patients, and by 2022 they served 30.5 million.[3] In 2022, they served one out of every six Medicaid enrollees and one out of every five uninsured people.[4] This is all to say, FQHCs

[1] See Arrow (1963) for the classic summary on the role that economic phenomena like agency problems, asymmetric information, moral hazard, and incomplete insurance markets play in health care markets.

[2] Calculated using FQHC financials from HRSA's Uniform Data System (UDS). Also see Rosenbaum et al. (2019).

[3] Calculated using FQHC patient data from HRSA's Uniform Data System.

[4] See National Association of Community Health Centers (2024, Figure 2-1), based on 2022 UDS data and national population and enrollment totals.

have become a key aspect of the American health safety net.

This paper is the first attempt at measuring the equilibrium effects of FQHC expansion. I first combine annual rosters of FQHC sites with a dataset of Medicaid claims for every provider, procedure code, and month in 2018–2024. I aggregate these claims to a local primary care market level. The main sample compares 1,018 markets that receive federally-subsidized FQHC entry from 2019 to 2024, with 3,821 comparison markets that do not. Within each market, I measure Medicaid billing for different groups of providers and types of care over all seven years. I leverage a difference-in-differences design to estimate the impact of FQHC entry on Medicaid billing revenues and quantities. I use two estimators that account for heterogeneous entry timing and nonnegative spending and count outcomes: a stacked Poisson pseudo-maximum likelihood (PPML) estimator and an extended two-way fixed effect (ETWFE) estimator (Wooldridge, 2023). This allows me to measure when and where FQHCs add care, and when and where they displace care from other primary care providers. To ensure that treated and control markets are comparable, I include state-year fixed effects and use a coarsened exact matching (CEM) procedure to balance treated and control markets on pre-entry characteristics. I complement this intensive margin analysis by examining the entry and exit of Medicaid providers around FQHC entry. Lastly, I use the same design with a longer county panel to examine the effect of FQHC expansion on mortality outcomes.

As expected, upon subsidized FQHC entry, FQHC billing rises and other providers' billing falls. After FQHC entry, FQHC primary-care billing is 0.23 log points higher, relative to a pre-entry mean of $1.05 million per market. Primary-care billing by all other providers falls by 0.05 log points from a base of $2.24 million. Taken together, these estimates imply a crowd-out of about 37 cents for every dollar of FQHC billing. On the quantity side, beneficiary counts show a shift of a similar magnitude. On the extensive margin, I find that competing firm exits increase — an FQHC entry is associated with a 0.198 unit increase in competitor exits.

Crowd-out appears in all types of markets, but its estimated size depends on a few key market characteristics. I split the treated markets by the following pre-entry covariates: rurality, other providers' Medicaid primary-care billing per resident, physician supply, and whether the market already had an FQHC. Most of these characteristics are not associated with significant heterogeneity in crowd-out. The exception is the type of entry (Table 5) — that is, there is a significant difference between expansion in new markets versus markets where an existing FQHC is subsidized to deepen its presence with a new clinic. When an FQHC opens a site in a market not already served (394 markets), other providers' primary-care billing falls by 0.08 log points and crowd-out reaches 80 cents per FQHC dollar. When an organization adds a site in a market it already serves (330 markets), FQHC billing grows less (0.14 against 0.28 log points) and other providers' billing does not move (-0.01; $p = 0.03$ for the difference), so crowd-out is 13 cents. One reading of this is that a new site expansion in a market where an FQHC already exists mostly serves to redirect existing patients, while a site in a new market has to build its patient base from patients other providers already serve. This is the only significant contrast among the eight splits.

Downstream outcomes are consistent with reallocation and add little beyond it. Medicaid emergency-department billing in the hospital service areas around new sites falls by 0.02 log points, but this result is sensitive to specification and I take as only suggestive. Inpatient billing, hospital uncompensated care, and inpatient days from hospital cost reports do not move.

Lastly, in a longer county panel, I use the same difference-in-differences analysis to look at final health outcomes: specifically, the effect of FQHC entry on total mortality. I find that mortality falls after FQHC entry, especially for maternal/neonatal mortality and HIV/AIDS mortality. This mortality finding is consistent with previous literature, and it suggests that despite moderate crowd-out, the reallocation of care toward FQHCs may improve health outcomes through higher quality care.

This paper relates to three distinct literatures. The first literature evaluates health

centers directly. There has been relatively little work on the most recent set of expansions, but studies of the early rollout find that centers lowered infant mortality (Goldman and Grossman, 1988), raised birth weight, with evidence pointing to earlier prenatal care (Kose et al., 2026), and reduced mortality among elderly adults (Bailey and Goodman-Bacon, 2015). Work on the modern program's expansion has more often focused on patients and downstream use. Medicaid expansion raised coverage at existing centers, with service-use and measured-quality gains concentrated in rural centers (Cole et al., 2018); larger funding increases after ACA passage in Massachusetts increased the number of enrollees using FQHCs and reduced the number with non-emergent emergency visits (Myong et al., 2020). One study found that randomly assigning initial primary-care appointments at an FQHC cut emergency visits among undocumented New Yorkers by 23 percent (Sabety et al., 2023). These papers examine population health, patient access, and downstream utilization. They do not jointly decompose health-center activity, competing primary-care provider activity, and total activity within the same market. The contribution here is a clearer measurement of market-level impact and spillovers.

The second literature studies how providers respond to public financing. Medicare's introduction induced hospital entry and spending growth well beyond individual demand responses (Finkelstein, 2007), and more recent coverage expansions change provider entry and exit (Geddes and Schnell, 2023). Safety-net subsidies can also be captured rather than passed through. Duggan (2000) finds that California's disproportionate-share payments shifted Medicaid patients from public to private hospitals, while local governments reduced subsidies to public hospitals as their disproportionate-share revenue rose. Garthwaite et al. (2018) find that hospitals bear the cost of the uninsured as uncompensated care. Duggan et al. (2022) study Medicaid expansion in California and argue that it substituted for county indigent programs. The closest research analogue internationally is Atal et al. (2024), who study the entry of municipal pharmacies in Chile, which sold at a third of private prices yet took only 4 percent of private sales because convenience segmented the market. These

studies leave open how much subsidized supply adds to local market aggregates or displaces unsubsidized clinicians. This paper measures that.

The third literature explains why displacement is plausible in Medicaid primary care. Baker and Royalty (2000) study prenatal and delivery care and find that higher fees increased private physicians' service to poor patients, partly by shifting patients from public sites. Alexander and Schnell (2024) find that higher Medicaid physician payments increased access and service use and improved self-reported health, and Neprash et al. (2021) find Medicaid expansion shifted primary-care caseloads toward Medicaid without raising clinicians' total labor supply. The question also parallels classic studies of crowd-out of private coverage by public insurance (Cutler and Gruber, 1996; Gruber and Simon, 2008). It also bears on incidence: much of the spending on Oregon's Medicaid expansion was a transfer to providers of previously uncompensated care (Finkelstein et al., 2019), and grants and enhanced payments to health centers may work the same way.

Section 2 describes the FQHC program and its expansion. Section 3 describes the Medicaid claims data and other data sets I use for analysis, and Section 4 describes the estimation process and identifying assumptions. Section 5 presents results, and Section 6 draws out what these results imply for effective public policy.

2 Institutional Background

Program history and structure

The Community Health Center Program was originally authorized under Section 330 of the Public Health Service Act in 1965, with the first provider opening as the Columbia Point clinic in Boston in 1965. The program expanded in the later 1960s and 1970s, with funding stalled in the 1980s.[5] During these decades, it remained relatively small and focused on

[5]For detailed histories of the program, see Sardell (1988) and Lefkowitz (2007); Taylor (2004) provides a concise policy history through the early 2000s. The appendix summarizes this history and the program's growth.

treating the uninsured, especially children and pregnant mothers, in the most socioeconomically disadvantaged areas of the country. The Health Centers Consolidation Act of 1996 brought the program into its current form.

As it currently stands, the FQHC program is a bundle of subsidies given to providers that apply and meet a set of requirements.[6] A health center must be a public or nonprofit entity that serves a medically underserved area or population.[7] It must also offer a federally-defined set of primary, preventive, and enabling services, such as interpretation and transportation, and importantly, it must treat all residents of its service area regardless of insurance status or ability to pay. Charges must follow a sliding-scale fee schedule based only on income and family size: patients at or below the federal poverty level receive free care, and discounts phase out by 200 percent of poverty.[8]

In return for meeting these requirements, centers receive a significant direct federal grant and several payment and regulatory benefits. The grants (often called "Section 330" grants after the section of the Public Health Service Act that authorized them) are awarded and renewed through publicly-announced competitions run by the Health Resources and Services Administration (HRSA) at the Department of Health and Human Services (HHS). During 2019–2024, grantees reported about $5 billion per year in grant receipts, or roughly $160–170 per patient.[9] Medicaid and Medicare also pay FQHCs under separate prospective payment systems, which pay a bundled rate per encounter that is typically several times what Medicaid pays an unaffiliated practice for an office visit; the appendix describes these rules.

FQHCs also receive a few other regulatory benefits, such as eligibility to participate in the 340B drug discount program, eligibility for provider malpractice coverage through

[6] See Heisler (2017) for full details.

[7] Medically underserved areas (MUAs) are geographic areas that HRSA designates using an index of the primary-care physician-to-population ratio, the poverty rate, the share of residents aged 65 and over, and the infant mortality rate. In rural areas, a designation can cover one or more counties. In urban areas, it can cover a census tract or a group of tracts. Medically underserved populations are groups, such as migrant farmworkers or people experiencing homelessness, that face access barriers within an otherwise served area.

[8] One other governance aspect of the FQHC program is that a majority of the governing board at an FQHC must be held by current patients (Health Resources and Services Administration, 2018).

[9] Author's calculation from the Uniform Data System (UDS), the mandatory annual census of grantees described in Section 3.

the Federal Tort Claims Act, and automatic designation of their sites as Health Professional Shortage Areas, which allows them to host National Health Service Corps clinicians (Heisler, 2017). These benefits are tangential to the direct federal grant and constitute an additional implicit subsidy for FQHCs upon entry.[10][11]

FQHC expansion

The program is large and has grown rapidly. In 2021, the midpoint of the claims panel described below, there were roughly 14,000 FQHC service sites serving about 30 million patients. This is up from 5,600 sites and 13.7 million patients in 2005.[12] Growth has occurred both on the extensive margin, with new organizations entering the program, and on the intensive margin, with existing organizations adding new FQHC sites. This distinction will be relevant for the empirical analysis, as the two types of entry are estimated to have different effects on local markets.

Extensive margin entry occurs when a new organization applies for and receives a Section 330 New Access Point grant. Upon announcement of the grant, an FQHC has a period of eight months to open the new site and begin serving patients. These New Access Point grants are awarded through a competitive process based on analysis of a location's unmet-need index, and the competitions are held periodically as funding from Congress becomes available.[13] Intensive margin entry occurs when an existing organization applies for and

[10]The 340B program, created by the Veterans Health Care Act of 1992, requires drug manufacturers that participate in Medicaid to sell outpatient drugs to "covered entities" at statutorily discounted prices. Covered entities include health centers and look-alikes, along with certain hospitals and specialized clinics. A covered entity keeps the margin between the discounted acquisition price and the payer's reimbursement, and the statute directs these savings toward patient care.

[11]There is also a category of FQHCs called "look-alikes." A look-alike is an organization that HRSA certifies as meeting every program requirement without receiving a Section 330 grant; it qualifies for FQHC payment, 340B pricing, and Corps placements. Look-alikes are a small part of the program: just 170 reported to the UDS in 2025, emphasizing the importance of federal grants for allowing providers that follow the above requirements to stay in business (Health Resources and Services Administration, 2026b,a).

[12]The number of patients per grantee rose from 9,000 to 21,000 over the same period. Author's calculation from UDS data.

[13]In an analysis of the Grants.gov record, HRSA held them in fiscal years 2011, 2013, 2015, 2017, 2019, and 2025. The 2019 round offered about $50 million for up to 75 awards of up to $650,000 per year, and required each applicant to propose at least one new full-time site.

receives a Section 330 Service Expansion grant to open a new site. FQHCs also occasionally add sites without a grant, but they must still apply to HRSA for approval of the new site and demonstrate that it meets the program's requirements. Such additions are sometimes financed from the base grant, patient revenue, and one-time supplements such as the pandemic relief funds of 2020–2021.

The composition of patients at FQHCs has also changed over time. In recent years, 48 percent of FQHC patients were covered by Medicaid, 20 percent were uninsured, 20 percent had private coverage, and 11 percent had Medicare. This Medicaid share is the result of recent growth in the program, especially after state-level Medicaid expansions brought on by the Affordable Care Act in 2011. In 2005, for example, only 35 percent of patients had Medicaid and 40 percent were uninsured. Medicaid is thus now the largest revenue source, 44 percent of operating revenue in 2017 (Rosenbaum et al., 2019). The appendix plots the payer mix since 1996 and the FQHC share of Medicaid billing.

Figure 1 shows where these patients live. It maps UDS patients per 1,000 residents in 2020 by residence ZIP code. Use varies widely across local markets. Across much of the country, including most large metropolitan areas, ZIP codes record fewer than 100 patients per 1,000 residents. ZIP codes in central Appalachia, the Mississippi Delta, the Texas border, parts of the Mountain West and Pacific Northwest, northern New England, and the upper Great Lakes often exceed 500, so recorded health-center patients there amount to half or more of the resident population.

The expansion margin studied here is the FQHC service site. A single FQHC organization can operate many sites, so an additional site need not be a new organization or the first FQHC in its market. It is crucial to acknowledge that site placement is not random. For example, New Access Point scoring rewards documented unmet need, and scope changes may respond to local demand, workforce availability, and payer mix. The timing and location of additions can therefore track local conditions that also move Medicaid spending. The empirical strategy described in Section 4 attempts to address this by controlling for

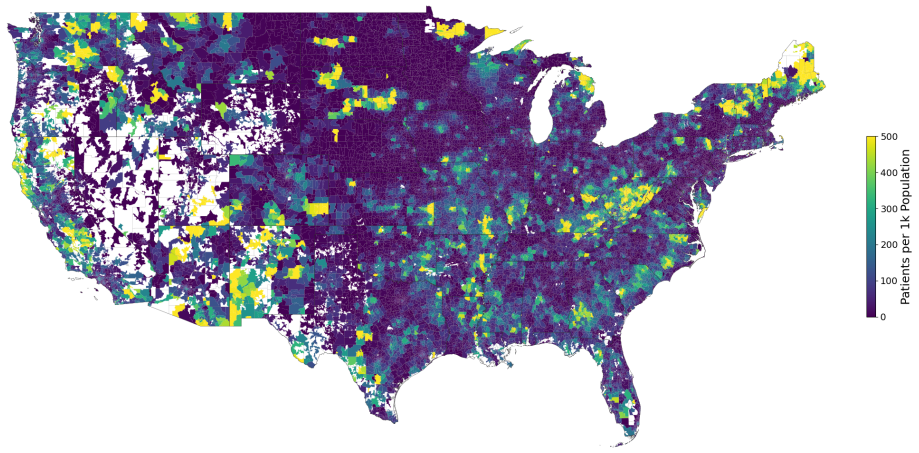


Figure 1: FQHC Patients per 1,000 Residents by ZIP Code, 2020

Notes: Patients that Section 330 grantees reported to the UDS by residence ZIP code in 2020, summed across grantees and divided by the ZIP code tabulation area's 2020 Census population. A patient seen by two grantees is counted twice. Grantees report ZIP codes with few of their patients only as a grouped category, which cannot be mapped. The color scale is capped at 500 patients per 1,000 residents; tabulation areas with no residents are blank. The map describes recorded use, not site locations. Source: HRSA Uniform Data System and the 2020 Decennial Census.

observable differences in markets and testing for pre-entry trend differences. But it cannot rule out every such correlation.

3 Data

Medicaid claims and FQHC identification

Medicaid market outcomes use the HHS *Medicaid Provider Spending* release, which reports Medicaid paid amounts, claim lines, and beneficiary counts by billing provider, servicing provider, procedure code, and month for calendar years 2018–2024. This dataset is new — it was released by the Department of Government Efficiency (DOGE) in late 2025, and it is the first time that Medicaid billing has been made available at the provider level for every state. This data covers all Medicaid fee-for-service claims, managed-care encounters, and CHIP activity, but it does not include insurer capitation payments. Thus, it measures the

provider billing portion of Medicaid spending rather than total Medicaid spending.[14] All paid amounts are deflated to 2010 dollars using the medical-services CPI.

Data on FQHCs come from a dataset obtained by FOIA from HRSA's Uniform Data System (UDS), as well as the public National Plan and Provider Enumeration System (NPPES). I identify FQHCs using linked UDS/NPPES health-center records, with provider taxonomies informing the classification. To identify Medicaid provider location, I use the ZIP code of the billing-provider NPI, with servicing-provider ZIP code used as a fallback. I construct a panel of claims outcomes for FQHCs, non-FQHC providers, and all providers, for all codes and a fixed primary-care code set. The primary-care code set follows the Medicare Shared Savings Program definition of primary care services (42 CFR 425.20), together with the Medicaid FQHC clinic encounter codes. Because some FQHCs operate multiple clinics and a new clinic cannot always be separated from an existing one, I do not examine outcomes for the clinic that opens.

I also look at billing outcomes for a narrower group, *non-FQHC primary-care providers*. I classify primary care providers by their annual Medicare specialty. Just as the Shared Savings Program denotes primary-care services, it also denotes primary-care specialties: family medicine, internal medicine, general practice, geriatric and pediatric medicine, and nurse practitioners and physician assistants without a specialized non-primary-care taxonomy. Hospitalists are excluded. I measure these providers' billing for all services and for primary care and split it into six provider facility types using NPPES categories: rural health clinics, urgent care, other primary-care clinics, office-based practices, hospital-billed primary care, and other organizations.[15]

In addition to the Medicaid billed amount for each provider group, care codes, and market-year described above, I also calculate two measures of quantity: claim lines and beneficiary-months. These outcomes provide an alternative measure of how FQHC entry

[14] In this dataset, cells with 11 or fewer beneficiaries or 11 or fewer claim lines are suppressed, so an absent cell need not mean zero care. Since I focus my analysis on the most common primary care encounter codes, very few providers are impacted by this censoring.

[15] Full details of the provider-type classification are given in the appendix.

impacts primary care markets. But there are important caveats to note: these quantity measures are not perfect. For one, claim lines are not total encounters, and secondly, beneficiary-months are not unique patients. In the current Medicaid claims data, I am unable to measure either total encounters or unique patient flows between organizations. I report the two counts as a check on the spending results.

Defining local healthcare markets

To study local markets for primary care, I use primary-care service areas (PCSAs) as the geographic unit. PCSAs are defined by the Dartmouth Atlas of Health Care team, composed of groups of 2010 census tracts in which most residents receive primary care from inside that area.[16] I map Medicaid provider claims' ZIP codes to PCSAs. There are a total of 6,814 PCSAs, with an average 2018 population of 47,540 (median 22,136). Within each PCSA, I use Medicare FQHC cost-report rosters and Uniform Data System (UDS) tracking to identify the number of FQHC primary care providers in each PCSA and the year of each site's first entry. One important caveat is the fact that some organizations may open in the calendar year prior to the fiscal year in which they issue a cost report, which means that there could be some measurement error in the timing of exposure. I give further details of the data processing and roster construction in the appendix.

Treatment is defined as the entry of an FQHC into a PCSA during calendar years 2019–2024. To isolate the impact of funding for a new FQHC, I focus on PCSAs with a single FQHC entry, i.e., I exclude the small share of markets that gain more than one site in the same year, as well as markets that lose a site in any given year. Control markets are defined as those whose FQHC count remains stable throughout the 2018–2024 time period, as well as exhibiting no change in FQHC count up to three years beforehand, so that potentially dynamic responses do not sit in the counterfactual. The main sample is therefore a balanced

[16] In some follow-up analyses on hospital outcomes, I use hospital service areas (HSAs), similarly defined as geographic areas in which most residents get inpatient and emergency care in that area. For precise definitions, see the Dartmouth Atlas of Health Care.

panel of PCSAs with a single FQHC entry and stable-count PCSAs, observed for seven years. This includes 1,018 treated PCSAs (435 rural and 583 urban) and 3,821 comparison PCSAs: this means the total panel consists of 33,873 PCSA-years.[17] One alternative sample appears in the appendix, which I call a "window" sample. This is an unbalanced panel with every FQHC entry counted as a treatment event, truncated at any other entries or exits.

I use American Community Survey (ACS) and Census data on census tract-level demographics to describe PCSA demographics for heterogeneity analyses. For example, I classify a PCSA as urban when more than half its population lies in metropolitan counties.[18] The main-text spending tables report each outcome's mean across treated markets in the year before their own FQHC entry, in millions of 2010 dollars.

Other data sources

Provider counts, entries, and exits are built using Medicaid claims. A provider is active in a PCSA-year when it bills primary-care claim lines there, and exits are dated to the last active year. I track reorganizations that keep a firm's clinicians together under a new name. Exits are defined for 2018–2023 and entries for 2019–2024. To examine potential spillovers outside the primary care market, I use CMS hospital cost reports for 2012–2022, aggregated to hospital service areas (HSAs). These cost reports include hospital uncompensated-care costs, inpatient days, and total Medicaid revenue.

To measure health impacts, I use the Institute for Health Metrics and Evaluation's modeled county mortality estimates for 2000–2019. This IHME data calculates age-standardized deaths per 100,000 by cause from NCHS death certificates. Since this data is only available at the county-level, I identify county-level FQHC entry over this longer time period using the same FQHC roster approach as above.[19] IHME-measured outcomes are all-cause mor-

[17] Hospital outcomes use hospital service areas (HSAs) under the same classification rules as PCSAs. This leaves us with 685 treated HSAs (331 rural and 354 urban) and 1,463 comparison HSAs.

[18] Metropolitan county classification comes from the 2018 Area Health Resources File.

[19] Specifically, I use the CMS Provider of Services files for 2000–2004 and the Area Health Resources File for 2005–2020. Since the latter dataset begins in 2005, I splice the two sources together to create a continuous

tality and eight cause-specific rates. These include mortality causes plausibly influenced by primary care like cardiovascular disease, maternal/neonatal mortality, and chronic respiratory rates. I also examine causes that are less plausibly influenced by primary care, like unintentional injuries, for use in falsification tests.

4 Empirical Strategy

To estimate the effect of FQHC entry on Medicaid billing, I use a difference-in-differences design that compares treated markets with stable-count comparison markets. I use two estimators that account for heterogeneous entry timing and nonnegative spending and count outcomes: a stacked Poisson pseudo-maximum likelihood (PPML) estimator and an extended two-way fixed effect (ETWFE) estimator (Wooldridge, 2023). The ETWFE model is my preferred specification, but the results from the two estimators are qualitatively similar. I describe the ETWFE model below, and the PPML estimator is described in the appendix.

To further ensure the comparability of treated and control markets, I use a variety of techniques. First, all specifications include state-by-year fixed effects, which control for state-level Medicaid policy shocks (e.g., eligibility expansion or rate changes). Second, I run robustness specifications where I perform coarsened exact matching (CEM) on pre-entry market characteristics: rurality, population, the number of FQHC sites, primary care physicians per capita, and baseline Medicaid primary-care billing per resident. This ensures that markets are similar in measured pre-entry characteristics.

Poisson ETWFE model

Let g_i denote the year of PCSA i's new FQHC entry, with $g_i = \infty$ for control markets, and let Y_{it} be an outcome: paid amounts, claim lines, or beneficiary-months for one provider group and service set. I trace the response to entry using the Poisson extended two-way

county-level FQHC count history. A robustness analysis confirms that results do not change if I only focus on the 2005–2020 AHRF-only comparison.

fixed-effects (ETWFE) estimator of Wooldridge (2023). With $D_{igs} = \mathbf{1}\{g_i = g,\, t = s\}$,

$$\mathrm{E}[Y_{it} \mid \cdot] = \exp\Big(\alpha_{g_i} + \lambda_t + X_{it}'\gamma + \sum_{g} \sum_{s \neq g-1} \theta_{g,s} D_{igs}\Big). \tag{1}$$

The cohort effects α_{g_i} absorb fixed differences across entry cohorts, and control markets identify the year effects λ_t. Each cohort's year before entry is the omitted cell. The main specification adds state-by-year fixed effects and log population covariates; the matched specification also reweights the comparison markets by their CEM weights. Because this is an exponential model, the outcome enters in levels. Each $\theta_{g,s}$ measures a cohort-year response in log points, and we can interpret $100(e^{\theta_{g,s}} - 1)$ as a corresponding percentage change.

The model allows responses to vary across entry cohorts and calendar years. As is standard in staggered entry difference-in-differences literature, I aggregate the $\theta_{g,s}$ in two ways. The first is a pooled post-treatment estimate, aggregating across cohorts and years with weights proportional to treated observations. The second is an event-study path, which reindexes the cohort-year effects to event times $h = t - g$. In the event study, I limit to three years around FQHC entry in a market, from $h = -3$ through $h = +3$, weighting cohorts by their treated observations at each horizon. Cohort composition changes across horizons, so the paths are not changes for a fixed set of markets. The two displayed leads show pre-entry movement relative to control markets, and the appendix reports joint tests of them for every outcome. Figure intervals are pointwise 95 percent intervals clustered by PCSA. As another robustness analysis, in Appendix Table A9 I fit a synthetic difference-in-differences model (Arkhangelsky et al., 2021), so that treated pre-entry paths match the control markets'.

Identification and interpretation

These crowd-out estimates are causal if, absent the site, treated markets' outcomes would have followed the same proportional path as the control markets, conditional on the fixed

effects and covariates, and if markets do not respond before the site appears. This is not guaranteed, but I argue that it is likely to hold in this case. For one, it is not sufficient to note that treatment and control markets are different - we need their counterfactual proportional paths to diverge. It is true that FQHC openings respond to local need – New Access Point scoring rewards documented unmet need, and scope changes respond to local demand, workforce, and payer mix. But I can match markets on these characteristics and include state-by-year fixed effects, which remove common state policy shocks, including Medicaid expansions or rate changes. The pre-trends of the event studies are the main check, and Section 5 reads them outcome by outcome. Hospital outcomes from the Medicaid claims use HSAs as markets with the same specification.

In the raw data, FQHC spending F and non-FQHC spending N sum to market spending T. Proportional effects estimated separately for each do not satisfy that identity, and neither adding coefficients nor applying each to its pre-entry mean recovers a decomposition, because the three fits weight markets by their own outcome. For a pooled coefficient β, the tables report the pre-entry mean times $e^{\beta} - 1$ as an implied level effect. I read market expansion from the total's own estimate and crowd-out from the non-FQHC estimate, and I use one benchmark to connect them. If FQHCs bill a share s_F of pre-entry market spending and their billing rises by $e^{\beta_F} - 1$ with no displacement, market spending rises by $s_F(e^{\beta_F} - 1)$; a total-spending interval that excludes this number is evidence that part of the FQHC gain was offset elsewhere.

The mortality analysis uses county-level geographies, identifying FQHC entry from 2001–2019 as treatment, and state-clustered inference. The mortality outcomes are measured in deaths per 100,000, so instead of the Poisson ETWFE model, I use the linear staggered-entry difference-in-differences model from Callaway and Sant'Anna (2021). As with the Medicaid outcomes, the identifying assumption is that absent FQHC entry, treated counties would have followed the same path as control counties. Again, I include state-by-year fixed effects and log population as covariates.

5 Results

FQHC billing, other providers, and market totals

Table 1 reports pooled post-entry estimates for each primary outcome. As expected, FQHC billing rises. For all services the estimate is 0.22 log points, on a pre-entry mean of $1.40 million per market; for primary care it is 0.23 on $1.05 million. Both estimates reject a null of zero at the conventional 5 percent level. Applied to the pre-entry mean, the primary-care estimate implies about $0.28 million more FQHC primary-care billing per market-year. These outcomes include incumbent FQHCs as well as the new site, so they measure growth in the total subsidized sector rather than just the new entrant's billing. The matched-controls column repeats each fit with the comparison markets matched entry markets' 2018 characteristics: FQHC primary-care billing rises 0.267 log points, non-FQHC primary-care billing falls 0.071 (SE = 0.036), and the primary-care total rises 0.075 (SE = 0.034), so the matched comparison implies a crowd-out of about 49 cents per FQHC dollar.

Table 1: Medicaid Billing After FQHC Entry

	Estimate	Matched controls	Treated mean at $h = -1$	Implied level effect
All-services paid				
FQHCs	+0.222*** (0.044)	+0.223*** (0.052)	1.40	+0.35
Non-FQHCs, all	-0.021 (0.017)	-0.007 (0.026)	22.29	-0.47
Non-FQHCs, primary care providers	-0.025 (0.025)	-0.008 (0.041)	2.52	-0.06
All providers	-0.015 (0.017)	+0.027 (0.020)	23.69	-0.35
Primary-care services paid				
FQHCs	+0.235*** (0.047)	+0.267*** (0.062)	1.05	+0.28
Non-FQHCs, all	-0.046** (0.020)	-0.071** (0.036)	2.24	-0.10
Non-FQHCs, primary care providers	-0.042** (0.020)	-0.037 (0.035)	1.48	-0.06
All providers	-0.001 (0.018)	+0.075** (0.034)	3.29	-0.00
Hospital-based services paid (HSA)				
Emergency department	-0.023 (0.018)	-0.016 (0.023)	1.78	-0.04
Inpatient and observation	-0.040 (0.087)	-0.014 (0.064)	0.27	-0.01

Notes: Post-entry coefficients in log points, with standard errors clustered by PCSA (by HSA for the hospital rows) in parentheses. Includes controls for stack-by-state-by-year effects and log population; Appendix Table A2 reports the same outcomes beside the specification without them. Matched controls: the same fit with the comparison PCSAs reweighted by coarsened exact matching on 2018 characteristics (metropolitan status, the FQHC site count in four bins, and terciles of population and primary-care billing per resident, 54 strata); 50 treated PCSAs in strata without a comparison market are dropped, the weighted comparison arm has an effective size of about 470 PCSAs, and the largest weight is 43.4. The hospital rows apply the same matching to the HSA sample (51 strata); 37 treated HSAs and 14 comparison HSAs in strata without the other arm are dropped, the weighted comparison arm has an effective size of about 140 HSAs, and the largest weight is 87.2. PCSA rows use a sample of 1,018 treated and 3,821 comparison PCSAs (33,873 PCSA-years). Hospital rows use the HSA sample of 685 treated and 1,463 comparison HSAs (15,036 HSA-years). The treated mean is the outcome in millions of 2010 dollars across treated markets in the year before their FQHC entry. The implied level effect multiplies that mean by $e^{\beta} - 1$; it is a scale illustration, and separate fits need not add across rows. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Primary-care billing by other providers falls. The non-FQHC estimate is −0.05 log points (SE = 0.02), on a base of $2.24 million, or roughly $0.10 million per market-year. Non-FQHC primary-care providers, whose primary-care billing is $1.48 million before entry, decline by 0.04 log points. All-service billing by non-FQHCs, which is ten times primary-care billing and dominated by services FQHCs do not supply, changes by −0.02 log points, in an interval that includes zero.

Market totals do not change detectably. Total primary-care billing changes by −0.00 log points (SE = 0.02); the 95 percent interval runs from −0.04 to 0.03 log points, and the all-service interval from −0.05 to 0.02. The benchmark of Section 4 shows what these intervals exclude. FQHCs billed $1.05 million of the $3.29 million in primary-care billing before entry, a share of about 31.8 percent. Applying the FQHC coefficient to this share implies a no-displacement increase of 8.41 percent. This exceeds the upper confidence limit for market-total spending, expressed as a percentage change.

Dynamics

Figures 2 and 3 trace the event study response of equation (1). Appendix Figures A4 and A5 show the same paths without controls on the same vertical scales, and Appendix Figures A7 and A8 show them with the comparison markets reweighted by the matching weights of Table 1. FQHC primary-care billing rises after entry, reaching 0.45 log points at the third year.

Relative to the year before entry, primary-care billing by other providers increases by -0.02 log points within three years. The total primary-care coefficient by three years out is 0.05. For total primary-care billing, the joint test for significant pre-trends has a p-value of $p = 0.489$. For primary-care billing by non-FQHC primary-care providers, it gives $p = 0.113$, and for primary-care billing by all non-FQHC providers $p = 0.609$. In all cases, the pre-trend tests fail to reject the null at conventional levels. This does not guarantee that the parallel trends identifying assumption holds, but it does provide some suggestive reassurance that

the treated and control markets are moving similarly prior to entry.

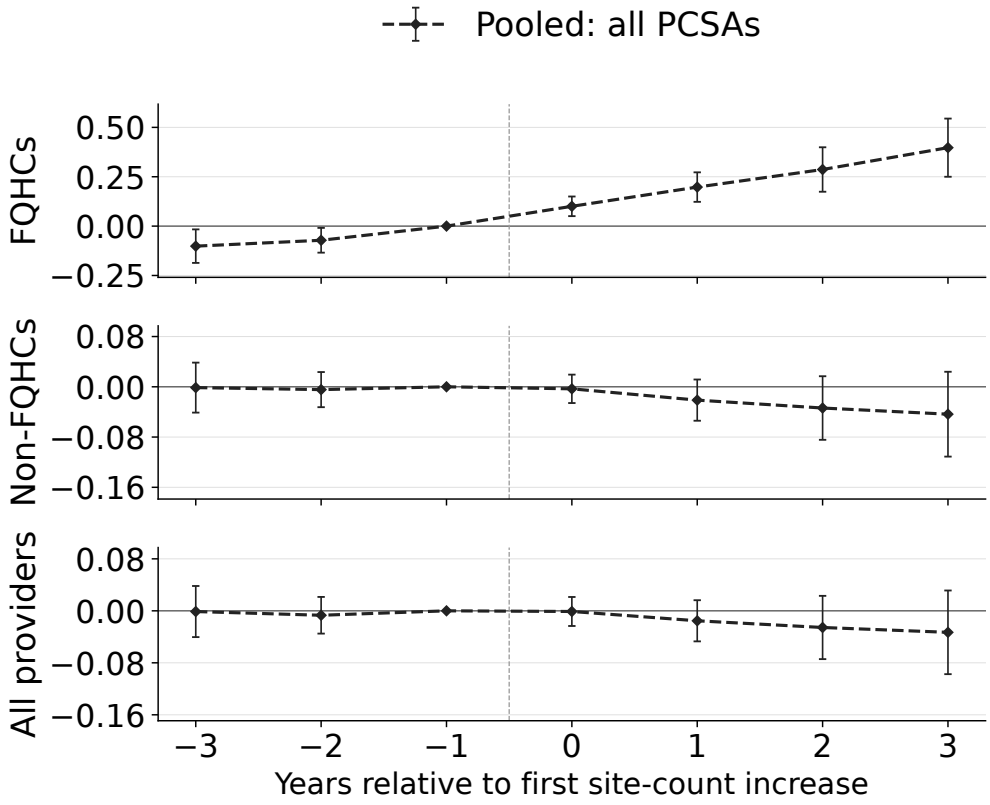


Figure 2: All-Service Medicaid Billing Around FQHC Entry

Notes: Poisson event-study coefficients from equation (1), fit with cohort effects, state-by-year effects, log population, for FQHC, non-FQHC, and all-provider paid amounts. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. Appendix Figure A4 shows the fit without state-by-year effects and log population on the same vertical scales. Rural and urban fits are in Table 3.

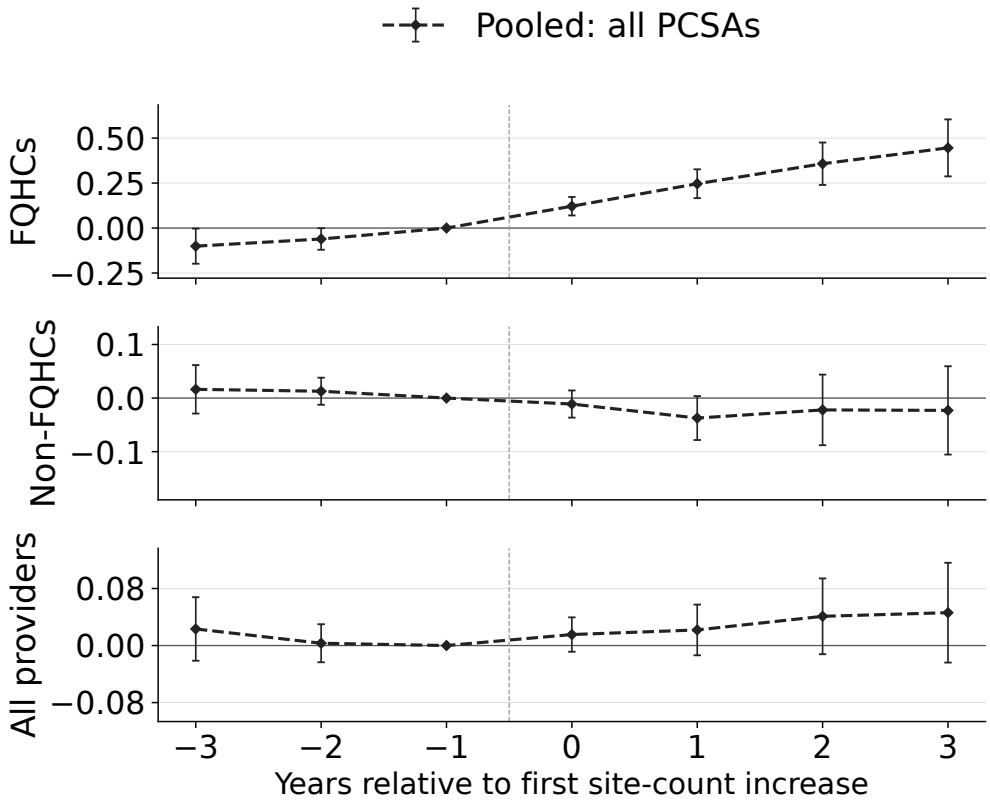


Figure 3: Primary-Care Medicaid Billing Around FQHC Entry

Notes: Poisson event-study coefficients for paid amounts on the primary-care service codes defined in the appendix (the Medicare Shared Savings Program code list plus its Medicaid FQHC encounter and preventive-visit analogues). Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. Appendix Figure A5 shows the fit without state-by-year effects and log population on the same vertical scales. Rural and urban fits are in Table 3.

Claim lines and beneficiaries

The previous analysis uses total Medicaid paid amounts as an outcome. This metric is a combination of both prices and quantities. Since an FQHC encounter is often paid more than what Medicaid pays an office visit, it is helpful to also examine measures of quantities alone. Table 2 therefore repeats the primary-care estimates for two counts. Primary-care claim lines rise 0.18 log points at FQHCs, fall 0.03 at other providers, and change by 0.01 (SE = 0.01) market-wide. Beneficiary-months with a primary-care claim rise 0.16 log points at FQHCs, fall 0.03 at other providers, and change by 0.00 (SE = 0.01) market-wide. The counts carry the caveats of Section 3: FQHC claim lines and beneficiary-months include the unpaid detail lines that accompany an encounter, and beneficiary-months do not necessarily count unique patients.

Table 2: Primary-Care Claim Lines and Beneficiary-Months After FQHC Entry

	Estimate	Treated mean at $h = -1$ (000s)
Primary-care claim lines		
FQHCs	+0.183*** (0.034)	17.1
Non-FQHCs, all	-0.027* (0.014)	58.3
Non-FQHCs, primary care providers	-0.014 (0.017)	40.2
All providers	+0.005 (0.014)	75.4
Primary-care beneficiary-months		
FQHCs	+0.160*** (0.029)	13.4
Non-FQHCs, all	-0.026* (0.014)	48.4
Non-FQHCs, primary care providers	-0.006 (0.017)	34.0
All providers	+0.000 (0.013)	61.8

Notes: Coefficients in log points, with PCSA-clustered standard errors in parentheses; the specification matches Table 1, and Appendix Table A7 reports both specifications. Claim lines count lines on the primary-care service codes. Beneficiary-months sum the monthly beneficiary counts of the provider-procedure cells on those codes; they are not unique patients. Treated means are in thousands per market in the year before FQHC entry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Organizations

One reason that competitor billing can fall is because incumbent practices bill less, but it could also be that they stop billing Medicaid primary care in the market altogether.

Figure 4 shows this extensive margin. It plots Callaway–Sant'Anna event-study estimates for the number of non-FQHC organizations whose Medicaid primary-care billing in a PCSA ends in a given year. Exits move little relative to the reference year before the site addition and rise in the years after it. The average post-entry effect is 0.198 additional exits per market-year (SE = 0.071) on a pre-entry mean of 1.09, about a fifth of the pre-entry rate. Exits that leave Medicaid billing altogether, rather than relocating to another market, rise by 0.133 (SE = 0.060) on a mean of 0.94, so they account for most of the rise.

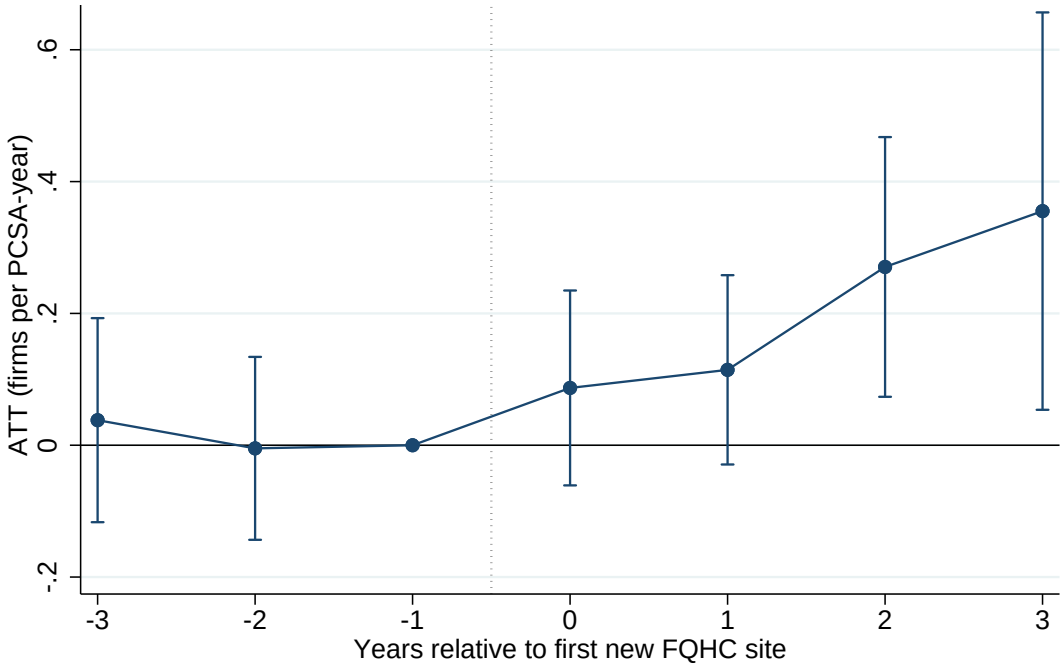


Figure 4: Non-FQHC Organization Exits from Medicaid Primary-Care Billing

Notes: Callaway–Sant'Anna event-study coefficients, in exits per PCSA-year, for the number of non-FQHC organizations whose Medicaid primary-care billing in the PCSA ends in that year. Sample of 630 PCSAs with one net site addition over 2018–2024 on the repaired site roster, against the 3,821 stable-count PCSAs of the clean comparison pool; not-yet-treated markets also serve as comparisons, and $h = -1$ is the reference. Exits are defined for 2018–2023, so the 2024 cohort enters only as a not-yet-treated comparison. An organization is active in a PCSA-year when it bills at least ten primary-care claim lines there; renamings that keep a firm's clinicians together are bridged and conversions to FQHC status are excluded. Bars are 95 percent PCSA-clustered intervals.

The stock of billing organizations moves less than billing does. The number of non-FQHC organizations billing Medicaid primary care in a market falls by 0.03 log points (SE = 0.01), on a pre-entry mean of 14.41 organizations, and entries change by −0.02. The stock decline is smaller than the billing decline, so most of the lost competitor billing comes from practices

that keep billing. These are exits from observed Medicaid primary-care billing rather than verified closures: my data cleaning pipeline accounts for documented renamings and mergers, but it is possible that clinicians may continue under an unlinked organization.

The event studies expose the design's main uncertainty: pre-entry movement differs across outcomes. At the 5 percent level, the joint test for pre-trends give a p-value of does not reject for the primary-care total and does not reject for primary-care billing by non-FQHC primary-care providers. Appendix Table A10 reports the full set. One robustness exercise is to choose comparison markets whose pre-entry paths match the treated markets' paths. Appendix Table A9 reweights the control markets with synthetic difference-in-differences weights and refits the stacked model: each primary-care coefficient is slightly reduced in magnitude, FQHC billing still rises, non-FQHC primary-care billing still falls, and the market-total interval still includes zero.

Heterogeneity in results

Table 3 reports separate rural and urban fits. Rural FQHCs respond proportionally more from much smaller bases: primary-care billing rises 0.42 log points on a pre-entry mean of \$0.31 million, against 0.24 on \$1.60 million in urban markets. Rural competitors also lose more: non-FQHC primary-care billing falls 0.05 log points in rural markets and 0.05 in urban ones. The rural primary-care total is 0.05 log points (SE = 0.03), and the urban total -0.01. The rural total is less precise, and the estimates come from separate fits rather than a formal test of rural–urban differences.

Table 3: Medicaid Billing After FQHC Entry in Rural and Urban Markets

	Rural PCSAs		Urban PCSAs	
	Estimate	Treated mean at $h = -1$	Estimate	Treated mean at $h = -1$
All-services paid				
FQHCs	+0.361*** (0.090)	0.43	+0.238*** (0.049)	2.12
Non-FQHCs, all	-0.044* (0.023)	7.31	-0.015 (0.019)	33.47
Non-FQHCs, primary care providers	-0.044 (0.029)	1.17	-0.024 (0.032)	3.53
All providers	-0.022 (0.022)	7.74	-0.010 (0.018)	35.59
Primary-care services paid				
FQHCs	+0.424*** (0.081)	0.31	+0.235*** (0.053)	1.60
Non-FQHCs, all	-0.049 (0.033)	1.01	-0.045* (0.024)	3.17
Non-FQHCs, primary care providers	-0.048* (0.024)	0.74	-0.046* (0.026)	2.03
All providers	+0.054* (0.030)	1.32	-0.008 (0.022)	4.76
Hospital-based services paid (HSA)				
Emergency department	-0.056** (0.026)	0.65	-0.025 (0.023)	2.84
Inpatient and observation	+0.018 (0.108)	0.04	-0.054 (0.097)	0.49

Notes: Coefficients in log points with PCSA-clustered (HSA-clustered for hospital rows) standard errors. The specification, outcomes, and means follow Table 1; the implied level column is omitted, and Appendix Table A3 reports both specifications. Urban PCSAs have more than half their population in metropolitan counties under the 2018 AHRF classification. Rural sample: 435 treated and 1,582 comparison PCSAs (14,119 PCSA-years); urban: 583 treated and 2,239 comparison PCSAs (19,754 PCSA-years). Hospital rows use 331 rural and 354 urban treated HSAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Figure A3 shows the paths for non-FQHC primary-care providers, including the pre-entry movement discussed above, and Appendix Figures A6 and A9 show them without controls and with matched controls. Table 4 splits competitor billing into six provider types. Office-based practices are the largest component, with primary-care billing of $0.88 million before entry and an estimate of −0.03 log points (SE = 0.03). Other primary-care clinics decline by 0.09 log points on a smaller base. Rural health clinics show 0.00 (SE = 0.04). Separate fits need not add to the aggregate competitor effect.

Appendix Table A5 repeats the split for rural and urban markets. Office-based practices, the largest type in both, change little in either. The rural decline is concentrated in hospital-billed primary care, which falls 0.55 log points from a pre-entry mean of only $0.03 million; the urban estimate for the same type is −0.05. Other primary-care clinics change by −0.08 log points in rural and −0.15 in urban markets, and rural health clinics in rural markets by 0.02 (SE = 0.04). These are separate fits of thin outcomes, and most rural type-level

intervals are wide.

Table 4: Competitor Billing by Provider Type After FQHC Entry

	Estimate	Treated mean at $h = -1$	Implied level effect
Primary-care competitor paid amounts by provider type			
Rural health clinics	+0.000 (0.040)	0.15	+0.00
Urgent care	+0.001 (0.101)	0.10	+0.00
Other primary-care clinics	-0.089 (0.066)	0.11	-0.01
Office-based practices	-0.027 (0.025)	0.88	-0.02
Hospital-billed	-0.124 (0.094)	0.07	-0.01
Other organizations	-0.058 (0.058)	0.17	-0.01
All-service competitor paid amounts by provider type			
Rural health clinics	-0.002 (0.041)	0.16	-0.00
Urgent care	-0.170 (0.134)	0.13	-0.02
Other primary-care clinics	+0.053 (0.075)	0.26	+0.01
Office-based practices	-0.028 (0.028)	1.33	-0.04
Hospital-billed	-0.023 (0.051)	0.24	-0.01
Other organizations	-0.047 (0.079)	0.40	-0.02

Notes: Coefficients in log points for non-FQHC primary-care providers' paid amounts split by the billing location's provider type, with PCSA-clustered standard errors in parentheses. Primary-care rows use the primary-care service codes; all-service rows use every HCPCS code. The six types partition each aggregate competitor measure in levels. Sample, specification, means, implied levels, and inference match Table 1; Appendix Table A4 reports both specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 re-estimates the FQHC and non-FQHC primary-care rows of Table 1 within strata of pre-entry market characteristics, each stratum against its own stable controls, and reports a crowd-out share for each: the implied non-FQHC level change over the implied FQHC change. Across all markets the share is 37 percent with controls. It does not differ detectably by rurality, pre-entry non-FQHC primary-care billing per resident, physician supply, prior FQHC presence, the state Medicaid fee level, or Medicaid expansion status. It does differ by the type of entry. When a grantee enters a market it did not already serve (394 markets), non-FQHC primary-care billing changes by -0.082 log points and the share is 80 percent. When a grantee adds a site in a market it already serves (330 markets), FQHC billing grows by 0.136 log points against 0.283, non-FQHC billing changes by -0.011 ($p = 0.03$ for the difference, from the tests in Appendix Table A8), and the share is 13 percent. The strata are separate fits, and the entry-type strata share the control pool.

Table 5: Crowd-Out by Pre-Entry Market Characteristics

	Treated PCSAs	Controls: FQHCs	Controls: Non-FQHCs, all	Crowd-out share (%)
All markets	1,018	+0.235*** (0.047)	-0.046** (0.020)	37
Rural and urban markets				
Rural	435	+0.424*** (0.081)	-0.049 (0.033)	29
Urban	583	+0.235*** (0.053)	-0.045* (0.024)	33
2018 non-FQHC primary-care paid per resident (terciles)				
Low	339	+0.297*** (0.100)	-0.044 (0.033)	9
Middle	339	+0.388*** (0.083)	-0.108*** (0.030)	38
High	340	+0.219** (0.104)	-0.040 (0.026)	54
2011 primary-care physician FTE per 100k (terciles)				
Low	339	+0.254** (0.108)	-0.077*** (0.028)	39
Middle	339	+0.071 (0.117)	-0.034 (0.027)	91‡
High	340	+0.380*** (0.059)	-0.083** (0.035)	43
FQHC sites in 2018				
First site	361	+0.820*** (0.202)	-0.098*** (0.025)	77
Additional site	657	+0.173*** (0.049)	-0.035 (0.037)	33
Medicaid-to-Medicare primary-care fee index, 2019 (terciles)				
Low	352	+0.391*** (0.090)	-0.084*** (0.031)	55
Middle	344	+0.195*** (0.057)	-0.025 (0.035)	45
High	305	+0.181** (0.075)	-0.026 (0.041)	15
Medicaid expansion by January 2019				
Expanded	639	+0.221*** (0.051)	-0.044* (0.026)	31
Not expanded	264	+0.357** (0.170)	-0.078** (0.033)	78
Type of expansion (all stable controls in every stratum)				
Own-market expansion	330	+0.136** (0.055)	-0.011 (0.025)	13
Grantee, new market	394	+0.283*** (0.077)	-0.082*** (0.022)	80
New organization	197	+0.323*** (0.079)	-0.032 (0.026)	28
Mixed	97	+0.285*** (0.098)	-0.073 (0.073)	32

Notes: Coefficients in log points on FQHC and non-FQHC primary-care paid amounts within strata of pre-entry market characteristics, with PCSA-clustered standard errors in parentheses. The specification matches Table 1; Appendix Table A8 reports both specifications, defines the strata, and tests the differences between strata. Each stratum uses its own stable controls, except the entry types, which share the control pool; cut points are the treated markets' terciles. Crowd-out share: minus the implied non-FQHC level change over the implied FQHC change, each the treated pre-entry mean times $e^{\beta} - 1$; ‡ marks a point estimate whose FQHC coefficient's 95 percent interval includes zero, so the ratio is imprecisely determined. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Hospital spillovers

If the additional primary care provided by FQHCs prevents acute care, then it could spill over to other providers. Specifically, I check if hospital use falls after entry. The hospital rows of Table 1 show Medicaid emergency-department billing in the HSAs around new sites falling by 0.02 log points (SE = 0.02), on a pre-entry mean of $1.78 million. Emergency claim lines change by −0.01 log points (SE = 0.02), so the billing decline may reflect prices or coding rather than fewer visits. Emergency billing, however, was drifting down before entry. Inpatient and observation billing changes by −0.04 log points, with a wide interval. Matching the comparison HSAs moves neither row: emergency-department billing changes by −0.016 log points (SE = 0.023) and inpatient and observation billing by −0.014 (SE = 0.064).

The all-payer cost-report outcomes in Table 6 do not establish a hospital response either: uncompensated-care cost changes by −$1.00 million per HSA-year (SE = $1.27 million) and inpatient days by +334 (SE = 438), intervals that allow meaningful changes in either direction. Thus, the hospital cost-report evidence neither establishes spillovers nor rules them out.

Table 6: Hospital Finances and Utilization After FQHC Entry

Outcome (per HSA, 'all' sample)	Callaway–Sant'Anna	BJS imputation
Hospital uncompensated care	$-1.00 ($1.27)	$-0.21 ($0.56)
Hospital charity care (% opex)	-0.237 (0.181)	-0.292* (0.162)
Hospital inpatient days	334 (438)	343 (463)
Hospital Medicaid paid (T-MSIS)	$0.096 ($0.100)	$0.082 ($0.105)
Number of hospitals	0.009 (0.009)	0.005 (0.010)

Notes: FQHC entry effects on hospital outcomes, with Dartmouth HSA markets. Cost-report rows cover 2012–2022; the Medicaid-paid row uses the shorter claims window. CS uses not-yet-treated comparison units. ATT with standard errors in parentheses, clustered by HSA. Spending in millions of dollars; charity care in percentage points of operating expense. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Mortality

Without individual Medicaid claims data, I cannot measure individual-level health impacts of FQHC expansion. But I can measure aggregate impacts, testing whether FQHC entry into a geographic area is followed by lower mortality. I use the Institute for Health Metrics and Evaluation (IHME) county-level age-standardized mortality rates for 2000–2019 (GBD US Health Disparities Collaborators, 2023). Since these data go back to 2000, they provide a longer panel for examining health outcomes than the Medicaid claims, allowing me to use more FQHC entry events. Note, however, that the IHME data are only at the county level, so I am no longer using the PCSA or HSA markets in previous analyses. I look at the impact on all-cause mortality and eight common cause-specific mortality measures, as well as "ambulatory care sensitive conditions" (ACSC) mortality, which is a composite of the cause-specific measures that primary care can prevent or manage.

Treatment is once again FQHC entry, defined as the first observed increase in the FQHC count within a county or county-equivalent. The sample includes 3,078 units: 1,825 first treated during 2001–2019 and 1,253 untreated through 2019. I use the staggered difference-in-differences estimator from Callaway and Sant'Anna (2021) to compare changes in each treated cohort with changes among not-yet-treated units, relative to the year before expansion. Standard errors are clustered by state.[20]

Table 7 reports average post-expansion estimates, after an outcome-specific linear trend is removed from the event-study estimates; Figure 5 plots the adjusted paths. All-cause mortality changes by -0.600 deaths per 100,000 (SE $= 0.664$) and ACSC mortality by -0.482 (SE $= 0.385$), neither distinguishable from zero. The appendix describes the adjustment and the propagation of uncertainty in the fitted trend.

Among the cause-specific outcomes, chronic respiratory and maternal/neonatal mortality decline, at -0.255 (SE $= 0.114$) and -0.023 (SE $= 0.008$) deaths per 100,000. HIV/STD

[20]Site counts combine CMS Provider of Services data for 2000–2004 with AHRF data for 2005–2020, aggregated to counties before defining treatment. The appendix reports event-time support and an AHRF-only treatment-history comparison.

mortality also declines (-0.062, SE $= 0.018$). The estimates for cardiovascular disease, cancer, diabetes/CKD, and the two injury measures are not distinguishable from zero at the 5 percent level.

These cause-specific declines are suggestive of health gains in conditions that primary care manages directly, with two cautions. First, a causal reading of the estimates requires the pre-treatment trend to have continued in the absence of expansion. Second, the inference conditions on IHME's modeled mortality rates, which may not fully capture the true underlying mortality trends. The evidence therefore points to modest cause-specific gains without establishing an aggregate mortality effect.

Table 7: Mortality After FQHC Entry

Outcome	Treated mean at $t = -1$	Estimate
Cardiovascular	314.9	-0.273 (0.302)
Maternal / neonatal	4.8	-0.023*** (0.008)
HIV / STD	2.9	-0.062*** (0.018)
Chronic respiratory	73.8	-0.255** (0.114)
Cancer	234.8	-0.051 (0.160)
Diabetes / CKD	58.4	-0.025 (0.116)
Broad injury	70.1	-0.062 (0.086)
Transport injuries	24.4	+0.024 (0.053)
ACSC composite	447.2	-0.482 (0.385)
All-cause	949.1	-0.600 (0.664)

Notes: IHME modeled age-standardized deaths per 100,000. Treatment is FQHC entry, the first increase in the FQHC count. All 2001–2019 cohorts are retained at supported event times. Estimates average event years 0–5 after removing an outcome-specific linear pre-trend fitted to the pre-entry coefficients; cohort composition varies across event years. Appendix Table A12 reports the unadjusted estimates beside the adjusted ones. Parentheses contain standard errors clustered by state. The treated mean uses all 1,825 treated units at their own year -1. Stars denote nominal tests: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

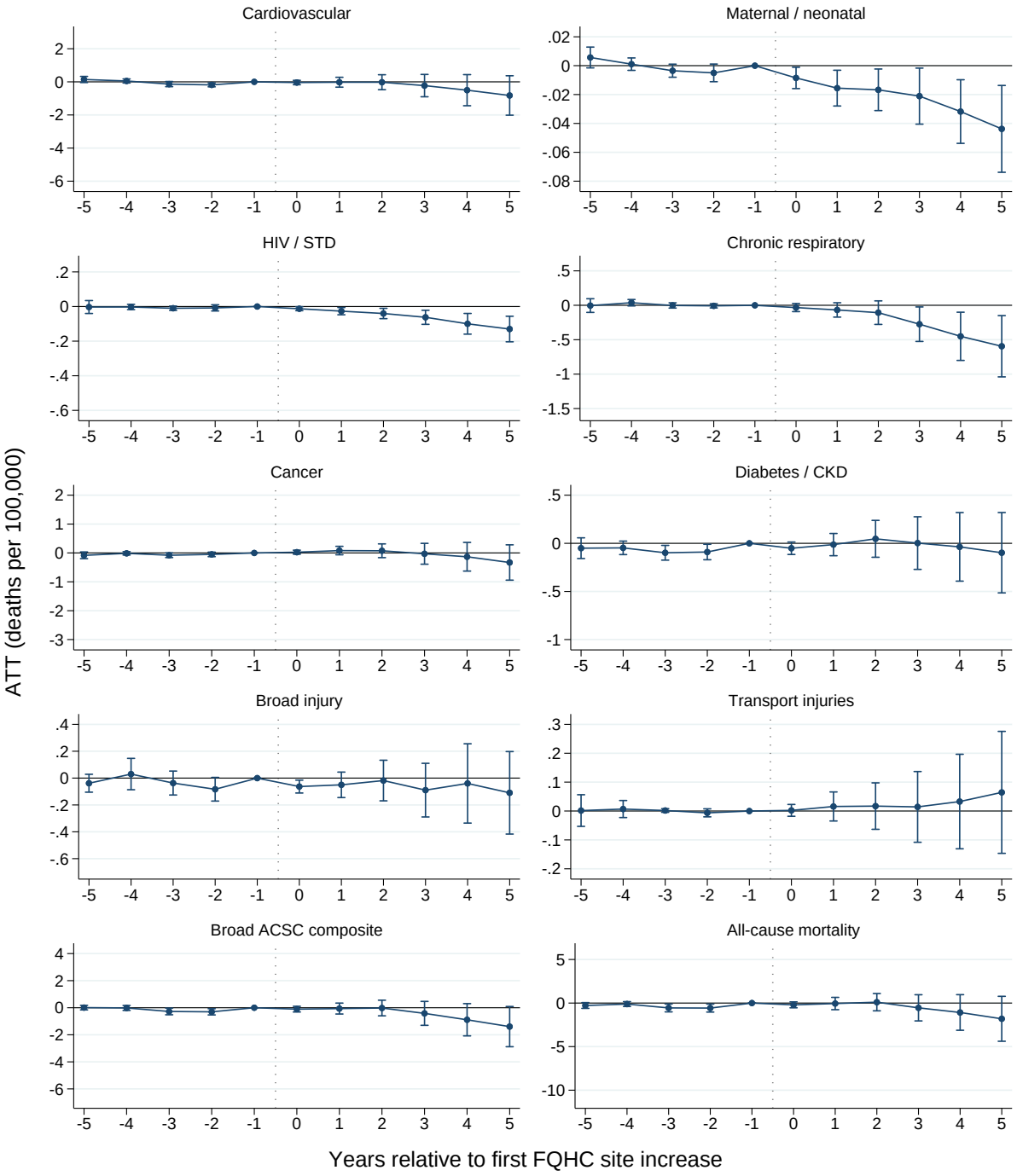


Figure 5: Mortality Event Studies

Notes: Each path is estimated after controlling for a linear pre-trend. Appendix Figure A10 shows the unadjusted paths. The combined POS/AHRF design retains all 2001–2019 cohorts at supported event times, relative to $h = -1$. Treated-unit support is 1,360 at $h = -5$, 1,825 at $h = 0$, and 1,477 at $h = 5$. Pointwise 95% intervals are clustered by state and condition on the modeled IHME outcomes.

6 Discussion

This paper examines whether a subsidy for safety-net primary care providers adds care to a local market or simply moves it around, and whether that change has downstream spillovers. The main estimates use a sample of 1,018 PCSAs with a first FQHC entry in 2019–2024 compared with 3,821 PCSAs that experience no change during the panel. FQHC Medicaid primary-care billing rises by 0.23 log points upon entry, while primary-care billing by other providers falls by 0.05. The adjustment also appears on the extensive margin: the count of billing organizations declines modestly, and exits from Medicaid primary-care billing rise by 0.198 organizations per market-year.

But the event studies leave the causal interpretation open. As expected, FQHC billing rises after subsidized entry, and estimates imply that this subsidized entry displaces some competitor billing. The primary-care market total rises only modestly and without statistical significance. And while tests for significant pre-trend coefficients fail to reject, event studies visually indicate a slight pre-trend in pre-entry years. A new FQHC site may replace care that would have persisted or sustain care in a market where other providers were retreating. Future work is needed to distinguish these counterfactuals. Reallocation of billing could be described by two mechanisms, and these data cannot yet separate them. The first is patient switching: Medicaid patients who previously saw office-based practices move to the new site, which can lower their travel or waiting costs without adding visits. The second is financing substitution: care that was uncompensated becomes a Medicaid-paid FQHC encounter.

A few policy implications follow. First, every encounter that moves from a standard Medicaid fee-schedule practice to an FQHC raises Medicaid spending without raising care, so part of the subsidy is a transfer to the center. But this transfer may have other purposes: for example, FQHCs treat many patients without insurance, and the subsidy may be intended to keep those patients in care. So we must be cautious about interpreting the welfare implications of the observed reallocation, since there is little evidence on how FQHC presence changes outcomes for uninsured patients. Second, the heterogeneity analyses suggest that

HRSA may want to prioritize expanding funding for existing FQHCs in the markets they already serve, rather than FQHC sites in new markets, since the latter seem to displace more care.

The downstream evidence is consistent with this reading. Medicaid emergency department billing in the hospital service areas around entering FQHCs falls, but this is insignificant and not robust to alternative specifications. Inpatient billing, hospital uncompensated care, and inpatient days do not move detectably. Rather, the strongest downstream evidence comes from the long county panel, which shows significant declines in some types of mortality after FQHC entry. This would be consistent with previous literature showing that early FQHC expansion improved infant and maternal health, as well as some evidence that FQHCs provide equal or higher-quality care than other providers that treat Medicaid patients (Goldman et al., 2012; Hicks et al., 2006).

One potential path forward is to leverage the fact that federal grants for new FQHC sites are awarded in competitions. The New Access Point program scores applicants and funds them in rank order until the appropriation is exhausted. Future work that can obtain the applicant scores and the funding cutoff can compare funded with unfunded applicants near the cutoff, creating more plausibly exogenous variation in entry. Future work could also utilize patient-level Medicaid claims to more directly observe whether patients billed by a new site are new to primary care or switched from a competitor. This would allow a more direct test of whether the new FQHCs are adding care or simply moving it around, as well as downstream health impacts.

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A Institutional details of the FQHC program

Program history

The federal health center program originated in the 1960s War on Poverty.[21] In 1965 the Office of Economic Opportunity (OEO) awarded the first two "neighborhood health center" demonstration grants: one to Tufts University for a clinic at the Columbia Point housing project in Boston, and a second, launched in 1967, to the rural Mississippi Delta community of Mound Bayou. The clinics drew on H. Jack Geiger's experience with community-oriented primary care in South Africa, combining clinical care with efforts to address social conditions.[22] The modern program embodies four related requirements: (i) clinics must be located in a designated medically underserved area or serve a medically underserved population; (ii) they must charge patients on a sliding scale and serve all comers regardless of insurance status; (iii) they must generally be governed by a board of directors of which at least a majority are active patients of the clinic; and (iv) they must provide a comprehensive set of primary, preventive, and enabling services rather than a narrow clinical menu (Health Resources and Services Administration, 2018).

In the early 1970s, before the OEO was dissolved, the health centers program moved to the Department of Health, Education, and Welfare. Congress authorized community and migrant health centers in 1975; separate authorities for people experiencing homelessness and residents of public housing were added later. The Health Centers Consolidation Act of 1996 combined these programs under Section 330 of the Public Health Service Act (Taylor, 2004).[23]

A second pivotal period came at the end of the 1980s, when Congress addressed a financing tension that had grown as Medicaid became the largest single payer at community

[21] The authoritative institutional history is Sardell (1988); an interview-based history is Lefkowitz (2007). Taylor (2004) provides a concise policy history through the early 2000s.

[22] Geiger (2016) recounts the founding of the Mound Bayou clinic. The National Library of Medicine's historical exhibit documents Geiger's work at the Pholela Health Center in South Africa.

[23] The migrant, homeless, and public-housing authorities are now codified at 42 U.S.C. §254b(g), (h), and (i), respectively.

health centers. Under standard Medicaid fee schedules, state payments to health centers often fell below the cost of providing care to Medicaid enrollees, forcing centers to cross-subsidize Medicaid services with their federal Section 330 grants, effectively diverting funds appropriated for the uninsured. The Omnibus Budget Reconciliation Act of 1989 created the "Federally Qualified Health Center" designation and required state Medicaid programs to reimburse FQHCs at their reasonable cost of providing covered services; OBRA 1990 extended the same cost-based reimbursement logic to Medicare.[24] Together these provisions defined the modern FQHC as the intersection of (a) a Section 330 grantee (or a clinic that meets all program requirements but receives no Section 330 grant, a so-called "look-alike"), and (b) an entity eligible for enhanced Medicaid and Medicare reimbursement. A reform in 2000 subsequently replaced cost-based Medicaid reimbursement with a prospective payment system indexed to the Medicare Economic Index, stabilizing payments while preserving the enhanced-rate structure that distinguishes FQHCs from other outpatient providers.[25] The resulting per-visit rates are typically several multiples of what the same Medicaid program pays an unaffiliated private physician for an office visit.

Two subsequent rounds of appropriations substantially increased the program's scale. Launched in fiscal year 2002, the Bush administration's Health Centers Initiative set a five-year goal of adding 1,200 new or expanded sites. Its initial aspiration to double patients served was later revised to an increase from 10.3 million in 2001 to 16 million by 2006 (Taylor, 2004). The Affordable Care Act then created the Community Health Center Fund (Section 10503), which established a multi-year mandatory appropriation on top of the program's discretionary base.[26] These expansions provide historical context for the longer county panel. The 2018–2024 Medicaid panel instead studies subsequent site-level growth: a single FQHC grantee typically operates multiple delivery sites, and new-site openings—rather than the

[24]Pub. L. 101–239, §6404; Pub. L. 101–508, §4161. The term "FQHC" originates in these statutes; prior to 1989 the program's grantees were generally known as community and migrant health centers.

[25]Benefits Improvement and Protection Act of 2000, §702, incorporated into Pub. L. 106–554.

[26]Rosenbaum et al. (2018) describe the post-ACA funding structure and the "funding cliff" dynamics created by short mandatory reauthorization windows.

founding of entirely new grantee organizations—account for most of the growth in access points since 2005.

The scale of the modern program is easiest to see in the UDS census. In 2021—the midpoint of the claims window studied below—1,373 Section 330 grantees operated roughly 14,000 sites and served 30.2 million patients. Medicaid is the dominant payer: 14.5 million patients (48 percent) were covered primarily by Medicaid and another 6.1 million (20 percent) were uninsured, with the remainder split between private coverage (20 percent) and Medicare (11 percent). Figure A1 plots the payer mix back to 1996: patient totals have roughly quadrupled since the late 1990s, the Medicaid share has risen from about a third of patients in the early 2000s to 47 percent in 2025, and the uninsured count fell with the ACA coverage expansions and has been roughly stable since. FQHCs billed roughly $6.0 billion in Medicaid provider spending in calendar year 2021, about 5 percent of all Medicaid provider-billed spending in the T-MSIS panel described in Section 3; since 2018 the FQHC share of claim lines has drifted up while the paid share has not. These totals exclude managed-care capitation payments.

FQHC payment rules

Because the main outcomes are Medicaid paid amounts, the FQHC payment rules matter for interpretation. Before 1989, state Medicaid fee schedules often paid health centers less than their cost of treating Medicaid enrollees, so centers used grant dollars intended for the uninsured to cover the shortfall. The Omnibus Budget Reconciliation Act of 1989 created the FQHC category and required states to reimburse these centers at reasonable cost, and the Benefits Improvement and Protection Act of 2000 replaced cost reimbursement with a prospective payment system (PPS).[27] Under PPS, Medicaid pays each FQHC a single bundled rate per face-to-face encounter, whatever services are delivered that day. The rate was set from each center's own average cost per visit in 1999–2000, is trended forward each

[27]Section 1902(bb) of the Social Security Act, added by Pub. L. 106–554, § 702. Section A gives the program's history.

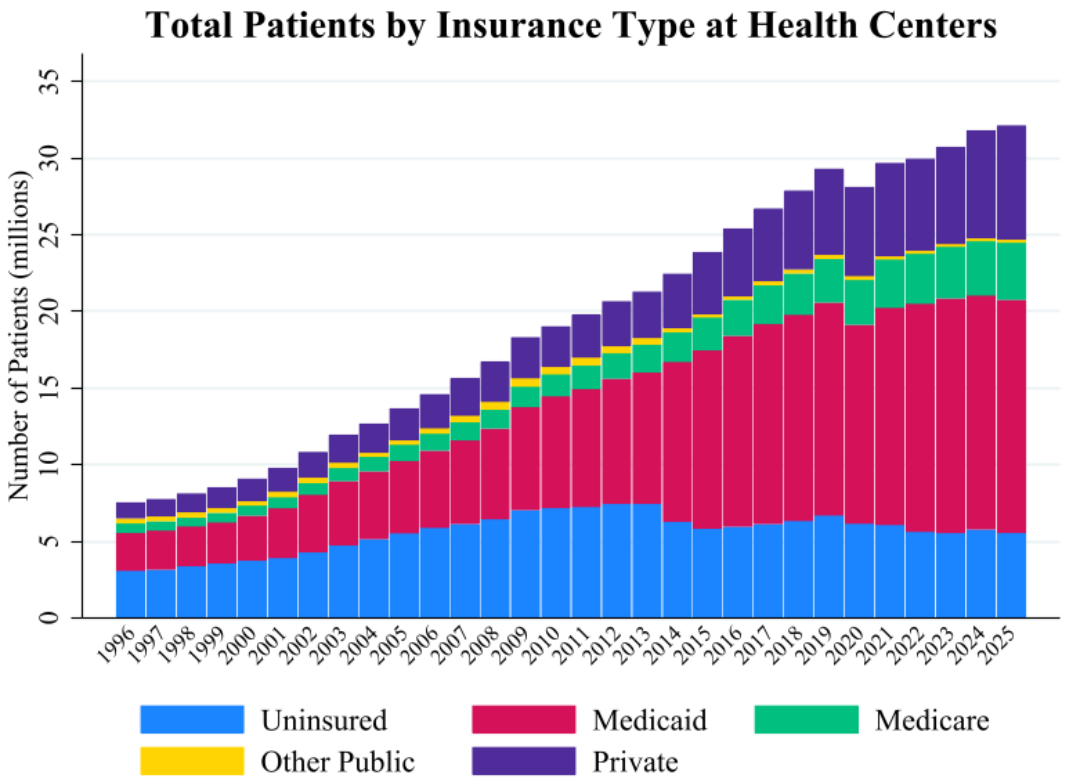


Figure A1: FQHC Patients by Primary Insurance, 1996–2025

Notes: Total patients at Section 330 grantees by primary insurance category, summed over grantees in the 50 states and DC reporting to the UDS in each year. Source: HRSA Uniform Data System.

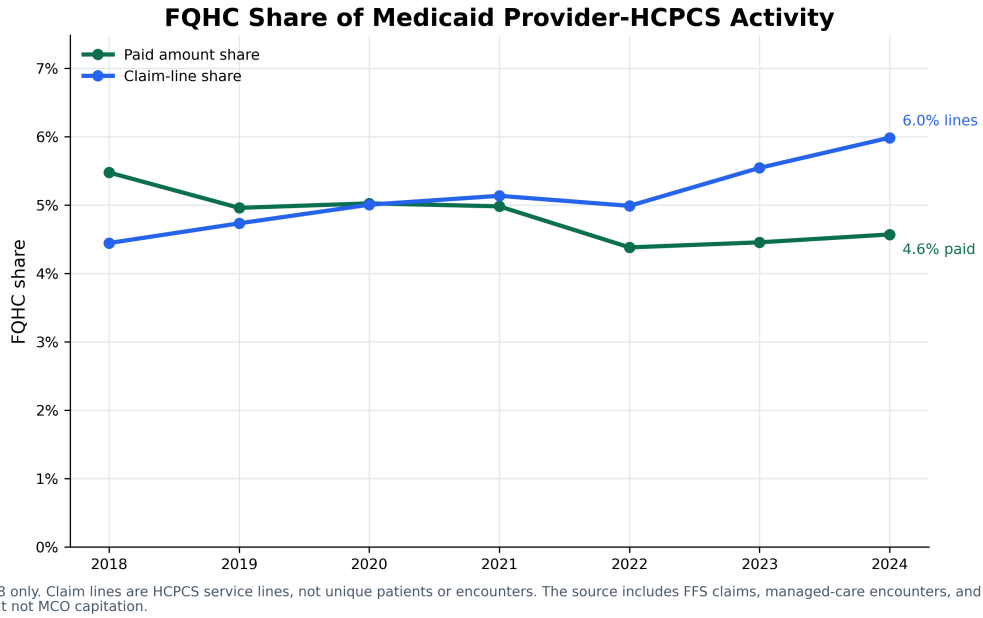


Figure A2: FQHC Share of Medicaid Provider Billing, 2018–2024

Notes: FQHC-billed share of Medicaid paid amounts and claim lines in the T-MSIS provider-procedure panel, lower 48 states. The source includes fee-for-service claims, managed-care encounters, and CHIP, but not managed-care capitation payments. Source: HHS Open Data *Medicaid Provider Spending by HCPCS* (U.S. Department of Health and Human Services, 2026).

year by the Medicare Economic Index, and is adjusted when the center's approved scope of services changes; new centers receive rates based on comparable centers. A state may instead adopt an alternative payment methodology with the center's agreement, provided it pays at least the PPS amount. In my inventory of state policies, 16 states use PPS only, 30 offer both PPS and an alternative method, and 5 rely on an alternative method. When a managed-care plan pays an FQHC less than its PPS rate, the state owes a wraparound payment for the difference, which may be settled per claim or through periodic reconciliation. Paid amounts recorded in claims data therefore need not equal a center's PPS revenue.

Encounter rates are provider specific and vary widely. Among 337 centers in 15 states that publish center-level rates and that I could match to Medicare records, the median 2024 Medicaid base rate was $216 per encounter, with state medians ranging from $137 in Georgia to $350 in Washington.[28] Medicare's FQHC PPS, in place since October 2014,

[28]These states are not a random sample, multi-site centers are summarized by their median rate, and the comparison is of posted rates rather than realized payments per claim.

pays a national base rate scaled by a geographic adjustment factor. The base rate was $187 in 2023 and $196 in 2024, with a 34 percent add-on for new patients and wellness visits.[29] For comparison, the 2023 physician fee schedule paid about $91 for a level-three and $128 for a level-four established-patient office visit before geographic adjustment, and Medicaid fee-for-service physician fees averaged 67 percent of Medicare's for primary care in 2019 (Zuckerman et al., 2021). An FQHC encounter therefore pays roughly one and a half to two times a Medicare office visit and about three times a Medicaid office visit at an unaffiliated practice. In the Medicaid claims used in the paper, an FQHC encounter commonly appears as one paid line carrying the encounter code T1015, with the services delivered that day listed on separate detail lines that carry no payment. Paid amounts thus combine encounter counts with center-specific rates, which is one reason the paper interprets spending as recorded billing rather than as a quantity of care.

B Sample Construction Details

The PCSA estimation samples

The main sample is the all-openings sample described in Section 3 of the main text. It dates treatment at a PCSA's first FQHC-site increase during 2019–2024 and retains 1,018 PCSAs with complete 2018–2024 histories, no annual count decline, and no annual increase exceeding seven sites; 1,167 of the 6,814 PCSAs with complete histories fail these count-history restrictions. The comparison pool contains 3,821 PCSAs. It starts from the 4,629 PCSAs whose counts do not change during 2018–2024 and applies two screens. The first removes 605 markets whose site roster churns, a site added and another removed during 2019–2024, under either the cost-report roster (a site is a cost-report CCN at a standardized address) or the Medicare certification roster (a site is a certified CCN, active from its certification to its termination). The second removes 203 more that gained a site in 2016, 2017 or 2018, a net

[29] CMS Transmittals R11677CP and R12267CP (Centers for Medicare & Medicaid Services, 2022, 2023).

increase in the cost-report count or a certification in the market, because the event studies show FQHC activity still rising three years after entry, so a market that gained a site within three years of the first treatment cohort would carry an unfolding response into the counterfactual. Every treated market stays; the same pool results under both the cost-report and the certification dating of entry. Hospital service areas follow the same rules: 1,946 HSAs whose counts never change, less 383 churning and 100 recent-entry markets, leave 1,463. The balanced panel has 33,873 PCSA-years. The treated markets comprise 435 rural and 583 urban PCSAs; controls comprise 1,582 rural and 2,239 urban PCSAs. Every PCSA has positive population and a state identifier in every year, so covariate specifications use the full sample. Subsequent qualifying additions remain part of the treatment response. The all-openings label describes qualifying first increases; it does not identify verified physical openings or remove the full-history restrictions.

The window sample is an alternative treated sample. It includes 1,552 treated PCSAs with an increase of exactly one site during 2019–2024, observed from the previous count change (or 2018) through the year before the next change (or 2024). For multiple eligible increases, it selects the longest window, then the longer post-period, then the earlier event. Its 34,919 PCSA-years include 630 treated markets observed for all seven years, 470 with one pre-entry year, and 547 with one post-entry year. Changes outside the selected window can include declines or jumps above seven sites. It uses the same 3,821 comparison PCSAs. These restrictions change the population and treatment margin, as well as cohort support.

The main estimates use identical observations for FQHC, non-FQHC, competitor, and total activity, with no outcome-specific trimming. Proportional estimators can drop all-zero fixed-effect groups and separated observations; actual observation and cluster counts are recorded for every fit in the replication CSVs.

The site-entry panel from cost reports

The PCSA counts come from FQHC Medicare cost-report site rosters: legacy Form CMS-222-92 through FY2014 and Form CMS-224-14 Worksheet S-1 from FY2014 onward. The panel begins in FY2010; FY2009 is excluded because of incomplete coverage. I deduplicate main and additional service locations within organization-year by standardized street address and map service-location ZIP codes to Dartmouth PCSA v3.1. The resulting counts line up closely with the CMS PECOS enrollment snapshot at the practice-location level (correlation ≈ 0.98). A count increase may reflect an opening, reporting change, conversion, or reorganization.

The main analysis aligns cost-report fiscal years with Medicaid calendar years by their numeric year labels. Roster timing can lag operations: a site can be certified and bill before appearing on a later cost report. The certification-timing comparison re-dates treatment by the certification dates of newly certified sites (2019–2024 cohorts); the available results are similar under either timing. Neither timing rule independently validates all roster additions as physical openings.

The firm panel

For the firm exit and entry analysis, a firm is active in a PCSA-year if it has at least ten observed Medicaid claim lines on the primary-care service codes defined in Section C, the same code set as the spending measures, there. Gross entries and exits are counted with a one-year washout and lookahead, and exits are dated to the firm's last active year. Reorganizations that keep a firm's servicing clinicians together under a new name are bridged rather than counted as exits, and practice-to-FQHC conversions are excluded from the exit counts. The Callaway–Sant'Anna exit estimates in main-text Figure 4 use the single-addition sample: the 630 treated PCSAs with exactly one net new site, a balanced panel, against the same 3,821 comparison PCSAs.

C Medicaid Claims: Processing and provider classification

Source description

The market outcomes are built from the HHS Open Data *Medicaid Provider Spending* release, a T-MSIS-derived file of Medicaid paid amounts reported in monthly cells of billing NPI × servicing NPI × HCPCS code, calendar years 2018–2024. The source includes fee-for-service claims, managed-care encounters, and CHIP, but not managed-care capitation payments, so it measures provider-billed activity rather than total Medicaid spending. I aggregate the monthly cells to an annual provider-by-HCPCS file that retains paid dollars, claim lines, distinct claim months, and beneficiary-months; beneficiary counts cannot be deduplicated across monthly provider-procedure cells, so unique patients are not recoverable. The release suppresses monthly cells with fewer than 12 beneficiaries or 12 claim lines; absence from the released file is not evidence of zero underlying care. HCPCS codes are restricted to the valid five-character pattern, which drops a small number of corrupt cells flagged in the data audit. All dollar series are deflated to 2010 dollars with the medical-services CPI-U. The exact source version and checksum are recorded in the replication materials.

Assigning claims to providers and places

Each annual cell is assigned two provider identities. The *location NPI*—used for FQHC status and geography—is the billing NPI when it is a valid ten-digit NPI and the servicing NPI otherwise; the *specialty NPI*—used for individual clinician specialty—prefers the servicing NPI and falls back to the billing NPI. Cells with no valid NPI in either role are dropped. Location NPIs are placed at their NPPES practice-location ZIP code from the crosswalk snapshot, rather than a validated historical service address, and mapped to geographies by ZIP crosswalks: to counties by best-match assignment (each ZIP assigned to the county

receiving its largest address share), and to PCSAs and HSAs by the Dartmouth definitional one-to-one ZIP assignments.

Identifying FQHCs

An NPI carries the *headline FQHC flag* if it satisfies either of two criteria: (i) it appears in NPPES as an organization with the FQHC taxonomy (261QF0400X); or (ii) it matches a UDS grantee organization or service site in a purpose-built crosswalk, which links UDS records to NPIs through the 340B covered-entity registry's identifier chain and through deterministic name-and-ZIP and site-address matches against Medicare FQHC enrollment records, NPPES FQHC-taxonomy organizations, and a claims-side FQHC audit panel; only the crosswalk's high-confidence tier is retained, and any NPI matched ambiguously to more than one grantee or site is excluded. A *broad FQHC flag* adds NPIs that a claims-side audit identifies as FQHC-like: NPIs enrolled in Medicare as Part A FQHCs (provider types 00–04 in any Provider Enrollment file snapshot) and NPIs observed billing the core FQHC visit G codes (G0466–G0470). The headline flag defines the FQHC/non-FQHC split throughout the market analysis; the broad flag is used only to purge FQHC-like billers from the competitor measures below. Within headline FQHCs, the legacy `new` series identifies location NPIs whose first positive paid amount or claim-line count occurs after 2018; the `inc` series identifies those observed in 2018. This split is defined symmetrically in treated and control geographies. It measures first observed billing within a left-censored and suppressed source, not the opening date of a linked site. A new site can bill through an existing NPI. Both headline and broad FQHC flags are applied across the full period, so this classification also cannot by itself establish time-varying conversion dates.

Primary-care and emergency department services

Primary-care activity is measured on a fixed set of primary-care service codes anchored to the Medicare Shared Savings Program definition: 42 CFR 425.20 defines primary care services as

the HCPCS and revenue center codes designated under 42 CFR 425.400(c). The code list is the union of the codes designated for every performance year in 425.400(c)(1), namely office and outpatient evaluation-and-management visits 99201–99215 (99201 was deleted in 2021 but appears historically), nursing-facility, domiciliary, and home visits, prolonged-service add-ons, tobacco-cessation counseling, online digital evaluation and management, chronic, principal, and transitional care management, behavioral health integration, advance care planning, health risk assessment, Medicare wellness visits, alcohol and depression screening, virtual check-ins, and the codes added for 2024 through 2026, plus the COVID-19 public-health-emergency telephone visit codes of 425.400(c)(2). The list is held fixed over 2018–2024; codes that did not yet exist carry no claim lines. Two Medicaid translations of Medicare-specific components are added: FQHC and rural health clinic encounters, which the regulation designates by revenue center codes, enter through the Medicaid encounter code T1015, the FQHC visit codes G0466–G0470, and the FQHC care-management and telehealth codes G0511, G0512, G0071, and G2025; and the CPT preventive visits 99381–99397 stand in for the Medicare wellness visits. Hospital outpatient clinic visit code G0463 is excluded because the regulation designates it only for electing-teaching-amendment hospitals, which the source cannot identify, and conditions that require place-of-service fields are not applied. Visit counts are claim lines on these codes, and primary-care spending is paid dollars on them. Emergency department spending is paid dollars on ED evaluation-and-management codes 99281–99285 and type B ED visit codes G0380–G0384; facility ED revenue billed only under UB-04 revenue codes does not appear in the HCPCS-level source.

Substitute specialties and non-FQHC primary-care providers

The *substitute-specialty* measures restrict non-FQHC visit-code billing to cells whose specialty NPI is an individual primary-care clinician under the Medicare Shared Savings Program definitions in 42 CFR 425.20. A primary care physician is one with a primary care specialty designation of family practice, general practice, internal medicine, geriatric

medicine, or pediatric medicine; these Medicare designations are translated to the unspecialized NPPES family, general, internal, and pediatric medicine taxonomies plus geriatric medicine and the adolescent, adult, and bariatric family and internal medicine codes that have no separate Medicare specialty. Subspecialists with their own Medicare specialty designation, including cardiology, gastroenterology, hospitalists, and pediatric subspecialties, are not primary care, and neither are obstetrics and gynecology or certified nurse midwives. Non-physician ACO professionals, namely nurse practitioners, physician assistants, and clinical nurse specialists, count except for the psychiatric and mental-health, acute-care, critical-care, emergency, neonatal, and perioperative or surgical specializations.

The *non-FQHC primary-care provider* measures refine the substitute-specialty rule in two ways. First, all broad-flag FQHC-like NPIs are excluded, which removes FQHCs the headline crosswalk misses from the competitor side; billing by these broad-but-not-headline NPIs is tracked separately as the missed-FQHC mass. Second, a cell counts as competitor billing if the servicing clinician has a substitute specialty *or* the location NPI's primary NPPES taxonomy is a whitelisted competitor organization type: clinic/center codes for unspecified, primary-care, community-health, rural-health, urgent-care, migrant-health, health-service, and public-health clinics (261Q00000X, 261QP2300X, 261QC1500X, 261QR1300X, 261QU0200X, 261QM1000X, 261QH0100X, 261QP0904X, 261QP0905X); and organization or solo-practitioner NPIs enumerated under the same primary-care physician and practitioner taxonomies as the clinician rule. Mental-health, family-planning, dialysis, dental, hospital, and other non-whitelisted organization types are classified out; hospital-billed primary care still enters through the servicing-clinician rule when the servicing NPI carries a substitute specialty. Cells unclassifiable on both routes—no individual specialty match and no NPPES taxonomy for the location NPI—are tracked separately and amount to roughly 0.4 percent of competitor billing pooled across years; because such NPIs also lack a practice ZIP, they cannot be geolocated and do not enter the located panels. The primary-care competitor measures apply these rules on the visit-code universe; the total-Medicaid competitor mea-

sure applies the same biller rules to all HCPCS codes. Each competitor cell is also assigned one of six mutually exclusive provider types from the location NPI's primary taxonomy: rural health clinics (261QR1300X); urgent care (261QU0200X); other primary-care clinics (the remaining whitelisted clinic/center codes); office-based practices (location NPIs that themselves carry a primary-care physician or practitioner taxonomy); hospital-billed primary care (282* taxonomies, captured through the servicing clinician); and other organizations (all remaining organization types captured through the servicing clinician, such as multi-specialty clinics and agencies, plus NPIs without an NPPES record). The six types sum exactly to the aggregate competitor measures.

D Additional Results

County balance

The county balance comparisons describe the longer historical panel, not the PCSA claims estimation sample. Entry counties differ in baseline population and need. These level differences motivate scrutiny of entry timing but do not establish parallel trends.

E Market Estimates and Specification Comparisons

Pooled estimation

The pooled tables fit a common post-entry effect by stacked Poisson pseudo-maximum likelihood (PPML), adapting the stacked event-study design of Cengiz et al. (2019). For each first-increase cohort $g \in \{2019, \ldots, 2024\}$, I form a stack containing that cohort's treated PCSAs over 2018–2024 and every comparison-pool PCSA over 2018–2024. Treated PCSAs receive weight one. A comparison PCSA receives weight n_g/N in stack g, where n_g is the cohort's size and $N = 1,018$, so its weights sum to one across stacks and each stack's

Table A1: County Covariates by FQHC Entry Status, 2010–2020

	No Entry	New Entry	Diff	p-value
Population	32,626	179,257	-146,631	0.000
Per capita income (LAPI)	34,223	33,842	380	0.216
Median household income (SAIPE)	42,848	43,497	-649	0.092
Percent in poverty (SAIPE)	16.05	17.65	-1.60	0.000
Percent <65 uninsured (SAHIE)	18.63	18.40	0.23	0.249
SNAP recipients per 1k	142.47	169.23	-26.76	0.000
Hospitals per 1k	0.07	0.03	0.04	0.000
Hospital admissions per 1k	64.60	98.25	-33.65	0.000
Hospital beds per 1k	3.42	3.14	0.28	0.087
Primary care physicians per 1k	0.49	0.59	-0.10	0.000
Dentists per 1k	0.31	0.41	-0.10	0.000
Medicaid eligibles share	0.20	0.23	-0.03	0.000
HPSA: Primary care	1.18	1.30	-0.13	0.000
HPSA: Dental	1.09	1.33	-0.24	0.000
HPSA: Mental health	0.97	1.14	-0.16	0.000
N counties (No Entry)	1,732			
N counties (New Entry)	1,408			

Notes: Means for baseline year 2010 across all counties in the merged AHRF panel, before the estimation-sample filters described in the text; county counts here therefore exceed the estimation samples. Entry defined as any positive change in FQHC count between 2010 and 2020. Diff is the No-Entry mean minus the New-Entry mean. p-values are from two-sample t-tests.

comparison arm has the same share of the total as its treated arm. Pooling the stacks, I fit

$$\mathrm{E}[Y_{ist} \mid \cdot] = \exp\left(\alpha_{is} + \lambda_{st} + \beta D_{ist}\right), \tag{A1}$$

where s indexes stacks, α_{is} is a stack-by-PCSA effect, λ_{st} a stack-by-year effect, and D_{ist} equals one for treated PCSAs in and after their entry year. The controlled specification replaces the stack-by-year effects with stack-by-state-by-year effects and adds log population with a common coefficient. The coefficient β is a common post-entry effect in log points; $100(e^{\beta} - 1)$ is the implied percentage change. Standard errors cluster by PCSA across all of a PCSA's copies. I fit each specification on all PCSAs jointly and separately on rural and urban PCSAs; the pooled fit is not an average of the two.

The stack-by-PCSA effects absorb fixed differences across markets, and each cohort is compared only with never-changing markets, so later cohorts' pre-entry years do not serve as controls for earlier cohorts. Hospital claims outcomes apply this specification at the HSA, clustering by HSA.

Main-text tables with and without controls

Tables A2 through A8 repeat main-text Tables 1, 3, 4, 2, and 5, and Appendix Table A5, with both stacked PPML specifications side by side. Baseline fits carry stack-by-market and stack-by-year effects only. The Controls column is the main-text specification, with stack-by-state-by-year effects and log population. Samples, means, implied levels, and inference are as in the main text.

Table A2: Medicaid Billing After FQHC Entry, With and Without Controls

	Baseline	Controls	Treated mean at $h = -1$	Implied level effect
All-services paid				
FQHCs	+0.248*** (0.049)	+0.222*** (0.044)	1.40	+0.35
Non-FQHCs, all	-0.045 (0.029)	-0.021 (0.017)	22.29	-0.47
Non-FQHCs, primary care providers	-0.081** (0.033)	-0.025 (0.025)	2.52	-0.06
All providers	-0.042 (0.028)	-0.015 (0.017)	23.69	-0.35
Primary-care services paid				
FQHCs	+0.307*** (0.054)	+0.235*** (0.047)	1.05	+0.28
Non-FQHCs, all	-0.083*** (0.028)	-0.046** (0.020)	2.24	-0.10
Non-FQHCs, primary care providers	-0.087*** (0.029)	-0.042** (0.020)	1.48	-0.06
All providers	-0.023 (0.026)	-0.001 (0.018)	3.29	-0.00
Hospital-based services paid (HSA)				
Emergency department	-0.027 (0.041)	-0.023 (0.018)	1.78	-0.04
Inpatient and observation	-0.062 (0.083)	-0.040 (0.087)	0.27	-0.01

Notes: Post-entry coefficients in log points, with standard errors clustered by PCSA (by HSA for the hospital rows) in parentheses. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. PCSA rows use a sample of 1,018 treated and 3,821 comparison PCSAs (33,873 PCSA-years). Hospital rows use the HSA sample of 685 treated and 1,463 comparison HSAs (15,036 HSA-years). The treated mean is the outcome in millions of 2010 dollars across treated markets in the year before their FQHC entry. The implied level effect uses the Controls coefficient and multiplies that mean by $e^{\beta} - 1$; it is a scale illustration, and separate fits need not add across rows. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Medicaid Billing After FQHC Entry in Rural and Urban Markets, With and Without Controls

	Rural PCSAs			Urban PCSAs		
	Baseline	Controls	Treated mean at $h = -1$	Baseline	Controls	Treated mean at $h = -1$
All-services paid						
FQHCs	+0.236** (0.092)	+0.361*** (0.090)	0.43	+0.273*** (0.051)	+0.238*** (0.049)	2.12
Non-FQHCs, all	-0.097** (0.040)	-0.044* (0.023)	7.31	-0.036 (0.032)	-0.015 (0.019)	33.47
Non-FQHCs, primary care providers	-0.125*** (0.044)	-0.044 (0.029)	1.17	-0.076* (0.040)	-0.024 (0.032)	3.53
All providers	-0.081** (0.039)	-0.022 (0.022)	7.74	-0.035 (0.032)	-0.010 (0.018)	35.59
Primary-care services paid						
FQHCs	+0.356*** (0.089)	+0.424*** (0.081)	0.31	+0.335*** (0.061)	+0.235*** (0.053)	1.60
Non-FQHCs, all	-0.126*** (0.038)	-0.049 (0.033)	1.01	-0.074** (0.035)	-0.045* (0.024)	3.17
Non-FQHCs, primary care providers	-0.144*** (0.037)	-0.048* (0.024)	0.74	-0.073** (0.037)	-0.046* (0.026)	2.03
All providers	-0.015 (0.037)	+0.054* (0.030)	1.32	-0.025 (0.031)	-0.008 (0.022)	4.76
Hospital-based services paid (HSA)						
Emergency department	-0.091** (0.046)	-0.056** (0.026)	0.65	-0.014 (0.053)	-0.025 (0.023)	2.84
Inpatient and observation	-0.011 (0.123)	+0.018 (0.108)	0.04	-0.062 (0.088)	-0.054 (0.097)	0.49

Notes: Separate rural and urban stacked PPML fits; coefficients in log points with PCSA-clustered (HSA-clustered for hospital rows) standard errors in parentheses. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. The treated mean is the outcome in millions of 2010 dollars across treated markets in the year before their FQHC entry. Urban PCSAs have more than half their population in metropolitan counties under the 2018 AHRF classification. Rural sample: 435 treated and 1,582 comparison PCSAs (14,119 PCSA-years); urban: 583 treated and 2,239 comparison PCSAs (19,754 PCSA-years). Hospital rows use 331 rural and 354 urban treated HSAs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Competitor Billing by Provider Type After FQHC Entry, With and Without Controls

	Baseline	Controls	Treated mean at $h = -1$	Implied level effect
Primary-care competitor paid amounts by provider type				
Rural health clinics	-0.097** (0.049)	+0.000 (0.040)	0.15	+0.00
Urgent care	-0.075 (0.100)	+0.001 (0.101)	0.10	+0.00
Other primary-care clinics	-0.242** (0.099)	-0.089 (0.066)	0.11	-0.01
Office-based practices	-0.046 (0.039)	-0.027 (0.025)	0.88	-0.02
Hospital-billed	-0.381** (0.149)	-0.124 (0.094)	0.07	-0.01
Other organizations	-0.081 (0.079)	-0.058 (0.058)	0.17	-0.01
All-service competitor paid amounts by provider type				
Rural health clinics	-0.082 (0.050)	-0.002 (0.041)	0.16	-0.00
Urgent care	-0.213** (0.104)	-0.170 (0.134)	0.13	-0.02
Other primary-care clinics	-0.145* (0.087)	+0.053 (0.075)	0.26	+0.01
Office-based practices	-0.049 (0.042)	-0.028 (0.028)	1.33	-0.04
Hospital-billed	-0.205* (0.107)	-0.023 (0.051)	0.24	-0.01
Other organizations	-0.056 (0.090)	-0.047 (0.079)	0.40	-0.02

Notes: Coefficients in log points for non-FQHC primary-care providers' paid amounts split by the billing location's provider type, with PCSA-clustered standard errors in parentheses. Primary-care rows use the primary-care service codes; all-service rows use every HCPCS code. The six types partition each aggregate competitor measure in levels. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. Sample matches the PCSA rows of Table 1. The treated mean is the outcome in millions of 2010 dollars across treated markets in the year before their FQHC entry. The implied level effect uses the Controls coefficient and multiplies that mean by $e^{\beta} - 1$; it is a scale illustration, and separate fits need not add across rows. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Competitor Billing by Provider Type in Rural and Urban PCSAs

	Rural PCSAs		Urban PCSAs	
	Estimate	Treated mean at $h = -1$	Estimate	Treated mean at $h = -1$
Primary-care competitor paid amounts by provider type				
Rural health clinics	+0.023 (0.042)	0.25	-0.122 (0.099)	0.08
Urgent care	-0.280 (0.233)	0.02	+0.010 (0.114)	0.16
Other primary-care clinics	-0.080 (0.098)	0.07	-0.150 (0.093)	0.14
Office-based practices	-0.034 (0.034)	0.29	-0.033 (0.030)	1.32
Hospital-billed	-0.553** (0.255)	0.03	-0.049 (0.139)	0.09
Other organizations	+0.045 (0.090)	0.08	-0.059 (0.067)	0.24
All-service competitor paid amounts by provider type				
Rural health clinics	+0.025 (0.043)	0.27	-0.152 (0.101)	0.08
Urgent care	-0.398 (0.245)	0.02	-0.177 (0.151)	0.20
Other primary-care clinics	+0.030 (0.092)	0.12	+0.008 (0.102)	0.37
Office-based practices	-0.034 (0.039)	0.40	-0.034 (0.032)	2.02
Hospital-billed	-0.104 (0.074)	0.17	-0.020 (0.071)	0.29
Other organizations	-0.177 (0.129)	0.19	-0.042 (0.092)	0.57

Notes: Separate rural and urban fits of the competitor-by-type outcomes in Table 4; coefficients in log points with PCSA-clustered standard errors in parentheses. Samples, specification, and means match Table 3; Appendix Table A6 reports both specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Competitor Billing by Provider Type in Rural and Urban PCSAs, With and Without Controls

	Rural PCSAs			Urban PCSAs		
	Baseline	Controls	Treated mean at $h = -1$	Baseline	Controls	Treated mean at $h = -1$
Primary-care competitor paid amounts by provider type						
Rural health clinics	-0.102* (0.061)	+0.023 (0.042)	0.25	-0.089 (0.088)	-0.122 (0.099)	0.08
Urgent care	-0.035 (0.240)	-0.280 (0.233)	0.02	-0.063 (0.106)	+0.010 (0.114)	0.16
Other primary-care clinics	-0.189 (0.126)	-0.080 (0.098)	0.07	-0.232* (0.127)	-0.150 (0.093)	0.14
Office-based practices	-0.075 (0.054)	-0.034 (0.034)	0.29	-0.042 (0.045)	-0.033 (0.030)	1.32
Hospital-billed	-0.650*** (0.244)	-0.553** (0.255)	0.03	-0.095 (0.157)	-0.049 (0.139)	0.09
Other organizations	-0.045 (0.110)	+0.045 (0.090)	0.08	-0.062 (0.092)	-0.059 (0.067)	0.24
All-service competitor paid amounts by provider type						
Rural health clinics	-0.081 (0.061)	+0.025 (0.043)	0.27	-0.091 (0.091)	-0.152 (0.101)	0.08
Urgent care	-0.107 (0.264)	-0.398 (0.245)	0.02	-0.206* (0.110)	-0.177 (0.151)	0.20
Other primary-care clinics	-0.212 (0.135)	+0.030 (0.092)	0.12	-0.130 (0.105)	+0.008 (0.102)	0.37
Office-based practices	-0.006 (0.063)	-0.034 (0.039)	0.40	-0.056 (0.048)	-0.034 (0.032)	2.02
Hospital-billed	-0.402** (0.165)	-0.104 (0.074)	0.17	-0.083 (0.137)	-0.020 (0.071)	0.29
Other organizations	-0.051 (0.112)	-0.177 (0.129)	0.19	-0.035 (0.107)	-0.042 (0.092)	0.57

Notes: Separate rural and urban fits of the competitor-by-type outcomes in Table 4; coefficients in log points with PCSA-clustered standard errors in parentheses. Primary-care rows use the primary-care service codes; all-service rows use every HCPCS code. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. Urban PCSAs have more than half their population in metropolitan counties under the 2018 AHRF classification. Rural sample: 435 treated and 1,582 comparison PCSAs (14,119 PCSA-years); urban: 583 treated and 2,239 comparison PCSAs (19,754 PCSA-years). The treated mean is the outcome in millions of 2010 dollars across treated markets in the year before their FQHC entry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Primary-Care Claim Lines and Beneficiary-Months After FQHC Entry, With and Without Controls

	Baseline	Controls	Treated mean at $h = -1$ (000s)
Primary-care claim lines			
FQHCs	+0.276*** (0.050)	+0.183*** (0.034)	17.1
Non-FQHCs, all	-0.047* (0.024)	-0.027* (0.014)	58.3
Non-FQHCs, primary care providers	-0.057** (0.027)	-0.014 (0.017)	40.2
All providers	-0.002 (0.022)	+0.005 (0.014)	75.4
Primary-care beneficiary-months			
FQHCs	+0.220*** (0.039)	+0.160*** (0.029)	13.4
Non-FQHCs, all	-0.046** (0.024)	-0.026* (0.014)	48.4
Non-FQHCs, primary care providers	-0.050* (0.026)	-0.006 (0.017)	34.0
All providers	-0.009 (0.021)	+0.000 (0.013)	61.8

Notes: Coefficients in log points, with PCSA-clustered standard errors in parentheses. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. Sample matches the PCSA rows of Table 1. Claim lines count lines on the primary-care service codes. Beneficiary-months sum the monthly beneficiary counts of the provider-procedure cells on those codes; they are not unique patients. Treated means are in thousands per market in the year before FQHC entry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Crowd-Out by Pre-Entry Market Characteristics, With and Without Controls

		Baseline				Controls			
	Treated PCSAs	FQHCs	Non-FQHCs, all	Crowd-out share (%)	p	FQHCs	Non-FQHCs, all	Crowd-out share (%)	p
All markets	1,018	+0.307*** (0.054)	-0.083*** (0.028)	48		+0.235*** (0.047)	-0.046** (0.020)	37	
Rural and urban markets									
Rural	435	+0.356*** (0.089)	-0.126*** (0.038)	90	0.31	+0.424*** (0.081)	-0.049 (0.033)	29	0.92
Urban	583	+0.335*** (0.061)	-0.074** (0.035)	35		+0.235*** (0.053)	-0.045* (0.024)	33	
2018 non-FQHC primary-care paid per resident (terciles)									
Low	339	+0.418*** (0.103)	-0.027 (0.078)	4	0.97	+0.297*** (0.100)	-0.044 (0.033)	9	0.92
Middle	339	+0.417*** (0.099)	-0.099** (0.039)	32		+0.388*** (0.083)	-0.108*** (0.030)	38	
High	340	+0.183** (0.090)	-0.024 (0.034)	41		+0.219** (0.104)	-0.040 (0.026)	54	
2011 primary-care physician FTE per 100k (terciles)									
Low	339	+0.227* (0.118)	-0.108** (0.046)	n.d.‡	0.61	+0.254** (0.108)	-0.077*** (0.028)	39	0.90
Middle	339	+0.183** (0.088)	-0.095** (0.040)	90		+0.071 (0.117)	-0.034 (0.027)	n.d.‡	
High	340	+0.460*** (0.071)	-0.076* (0.044)	31		+0.380*** (0.059)	-0.083** (0.035)	43	
FQHC sites in 2018									
First site	361	+0.904*** (0.198)	-0.118*** (0.039)	80	0.09	+0.820*** (0.202)	-0.098*** (0.025)	77	0.15
Additional site	657	+0.277*** (0.055)	-0.020 (0.043)	11		+0.173*** (0.049)	-0.035 (0.037)	33	
2018 hospital share of non-FQHC provider paid									
None	804	+0.344*** (0.065)	-0.058** (0.029)	29	0.34	+0.265*** (0.063)	-0.045** (0.019)	31	0.08
Low	82	+0.369*** (0.127)	-0.123 (0.096)	55		+0.222 (0.197)	+0.028 (0.057)	n.d.‡	
High	82	-0.010 (0.157)	-0.125* (0.064)	n.d.‡		-0.094 (0.195)	-0.182** (0.076)	n.d.‡	
Medicaid-to-Medicare primary-care fee index, 2019 (terciles)									
Low	352	+0.656*** (0.132)	-0.129*** (0.049)	42	0.42	+0.391*** (0.090)	-0.084*** (0.031)	55	0.25
Middle	344	+0.314*** (0.066)	-0.035 (0.037)	37		+0.195*** (0.057)	-0.025 (0.035)	45	
High	305	+0.145** (0.070)	-0.071 (0.051)	53		+0.181** (0.075)	-0.026 (0.041)	15	
Medicaid expansion by January 2019									
Expanded	639	+0.279*** (0.056)	-0.082** (0.032)	43	0.58	+0.221*** (0.051)	-0.044* (0.026)	31	0.42
Not expanded	264	+0.589*** (0.221)	-0.121* (0.063)	64		+0.357** (0.170)	-0.078** (0.033)	78	
Type of expansion (all stable controls in every stratum)									
Own-market expansion	330	+0.211*** (0.058)	-0.018 (0.040)	13	0.06	+0.136** (0.055)	-0.011 (0.025)	13	0.03
Grantee, new market	394	+0.321*** (0.078)	-0.117*** (0.035)	98		+0.283*** (0.077)	-0.082*** (0.022)	80	
New organization	197	+0.443*** (0.105)	-0.077** (0.036)	44		+0.323*** (0.079)	-0.032 (0.026)	28	
Mixed	97	+0.419*** (0.096)	-0.208** (0.101)	53		+0.285*** (0.098)	-0.073 (0.073)	32	

Notes: Stacked PPML coefficients in log points on FQHC and non-FQHC primary-care paid amounts within strata of pre-entry market characteristics, with PCSA-clustered standard errors in parentheses. Baseline includes stack-by-market and stack-by-year effects. Controls includes stack-by-market and stack-by-state-by-year effects and log population. Crowd-out share: minus the implied non-FQHC level change over the implied FQHC change, each the treated pre-entry mean times $e^{\beta} - 1$; ‡ marks a point estimate whose FQHC coefficient's 95 percent interval includes zero, so the ratio is imprecisely determined. Each stratum is estimated with its own stable controls; cut points are the treated markets' terciles (hospital share: the median of positive treated shares), and a market missing a characteristic is dropped from that split only. The entry-type strata share the full control pool, since the type is an attribute of the entry rather than the market: own-market expansion is an existing grantee adding a site in a market it already serves; grantee, new market is an existing grantee's first site in the market; new organization is a new grantee; mixed is a first addition of more than one type. Non-FQHC primary-care paid per resident, the hospital share of non-FQHC primary-care-provider paid, and FQHC sites are measured in 2018; physician FTE per 100,000 residents in 2011; the fee index is the state's Medicaid-to-Medicare primary-care fee ratio in 2019; expansion is as of January 2019, with the 2020–2024 expansion states excluded. p: two-sided test of equal non-FQHC coefficients between the first and last strata of the split (rural against urban; low against high; first against additional site; none against high; expanded against not expanded) or, for the entry types, own-market expansion against a grantee's new market, ignoring the shared-control covariance. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Synthetic difference-in-differences

The stacked fits weight every comparison market equally within a stack. As a check on that choice, I refit equation (A1) with the comparison markets reweighted by synthetic difference-in-differences weights (Arkhangelsky et al., 2021). For each cohort stack, I fit unit weights ω_i on the stable-count markets and time weights λ_t on the pre-entry years to $\log(\text{paid}+1)$ with the Arkhangelsky et al. (2021) solver; the unit weights make the weighted comparison path track the cohort's mean pre-entry path, and the time weights pick the pre-entry years that predict the post-entry years. The 2019 cohort has one pre-entry year and keeps uniform weights. Treated market-years enter the Poisson likelihood with weight one, comparison market-years with weight $(n_g/N)\, N_C\, \omega_i$, and pre-entry years with weight $T_g^{\text{pre}}\lambda_t$, where N_C is the number of comparison markets and T_g^{pre} the cohort's number of pre-entry years. With uniform ω and λ these weights reduce to the stacked weights described under pooled estimation above, so the first column of Table A9 is the Baseline stacked PPML specification of Table A2 fit on this sample. The outcome stays in levels; only the weights come from the log scale. There are no covariates. Standard errors come from 200 bootstrap draws that resample markets with their full histories and refit the weights in every draw.

Reweighting attenuates every primary-care coefficient by one to one and a half standard errors and changes no sign. FQHC primary-care billing rises 0.26 log points with market and time weights against 0.31 with uniform weights; non-FQHC primary-care billing falls 0.05 (SE = 0.02) against 0.08; the estimate for non-FQHC primary-care providers is −0.05 (SE = 0.03) against −0.09, with an interval that includes zero; and the primary-care total is 0.01 (SE = 0.02) against −0.02. The implied crowd-out share, computed as in Table 5, is 48 percent with uniform weights and 37 percent with synthetic weights. These fits have no covariates, so the comparison is with the Baseline column rather than the main-text specification. The weighted comparison paths match the treated paths before entry by construction, so the fit says nothing about pre-trends; it shows that the estimates do not depend on the stable-count markets whose pre-entry paths diverge from the treated markets'.

The weights are generated from the same panel, and the identification and inference results for synthetic difference-in-differences in levels do not carry over to this Poisson fit, so I read the table as a sensitivity check rather than as an alternative estimator.

Table A9: Stacked PPML Estimates With Synthetic Difference-in-Differences Weights

	Uniform	Market weights	Market and time weights	Treated mean at $h = -1$
Primary-care services paid				
FQHCs	+0.307*** (0.051)	+0.220*** (0.054)	+0.256*** (0.045)	1.05
Non-FQHCs, all	-0.083*** (0.028)	-0.069** (0.028)	-0.052** (0.023)	2.24
Non-FQHCs, primary care providers	-0.087*** (0.032)	-0.071** (0.031)	-0.046* (0.026)	1.48
All providers	-0.023 (0.026)	-0.014 (0.026)	+0.006 (0.022)	3.29

Notes: Common post-entry coefficients in log points from equation (A1) on the all-openings sample (1,018 treated and 3,821 comparison PCSAs, 6 cohort stacks), with bootstrap standard errors in parentheses (200 draws resampling markets and refitting the weights; stars use the normal approximation). Uniform: the stack weights of the Baseline column of Table A2. Market weights: comparison markets reweighted by synthetic difference-in-differences unit weights fit to $\log(\text{paid} + 1)$ within each cohort stack. Market and time weights: the same with pre-entry years reweighted by the fitted time weights. No covariates. Treated means are in millions of 2010 dollars across treated markets in the year before FQHC entry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Event studies for non-FQHC primary-care providers

Figure A3 reports the controlled event-study paths for all-service and primary-care billing by non-FQHC primary-care providers.

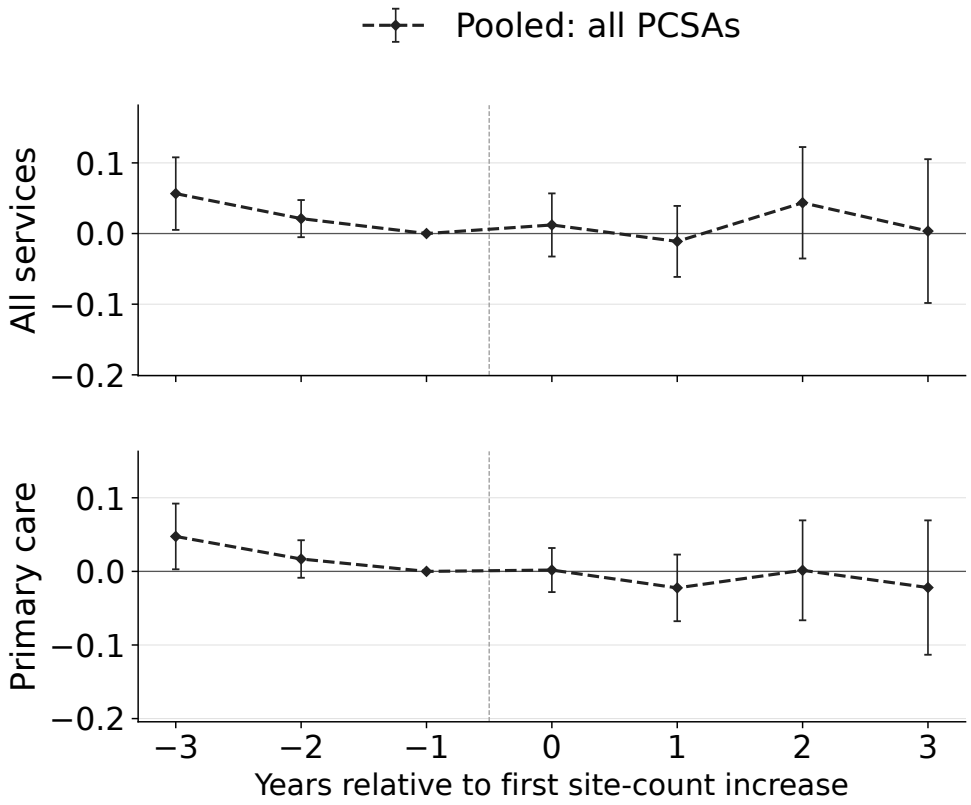


Figure A3: Billing by Non-FQHC Primary-Care Providers Around FQHC Entry

Notes: Poisson event-study coefficients for all-service and primary-care paid amounts among non-FQHC primary-care providers, the subset of non-FQHC providers defined in the appendix.

Event studies without controls and with matched controls

Figures A4, A5, and A6 repeat Figures 2, 3, and A3 without state-by-year effects and log population, so each fit carries only cohort and year effects and the stable-count comparison markets. Each panel keeps the vertical scale of its controlled counterpart. Table A10 reports the joint lead tests for both specifications.

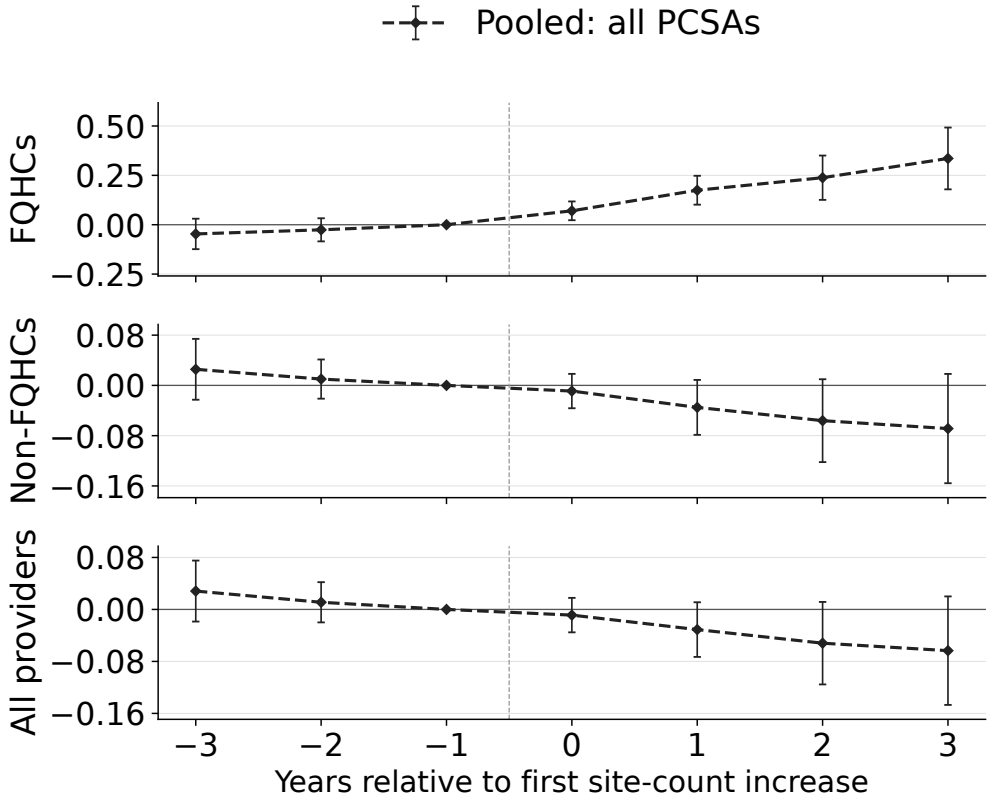


Figure A4: All-Service Medicaid Billing Around an FQHC Site Addition, Without Controls
Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort and year effects, for FQHC, non-FQHC, and all-provider paid amounts. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the counterpart to main-text Figure 2 without state-by-year effects and log population, using the same stable-count comparison markets and outcome-specific vertical scales.

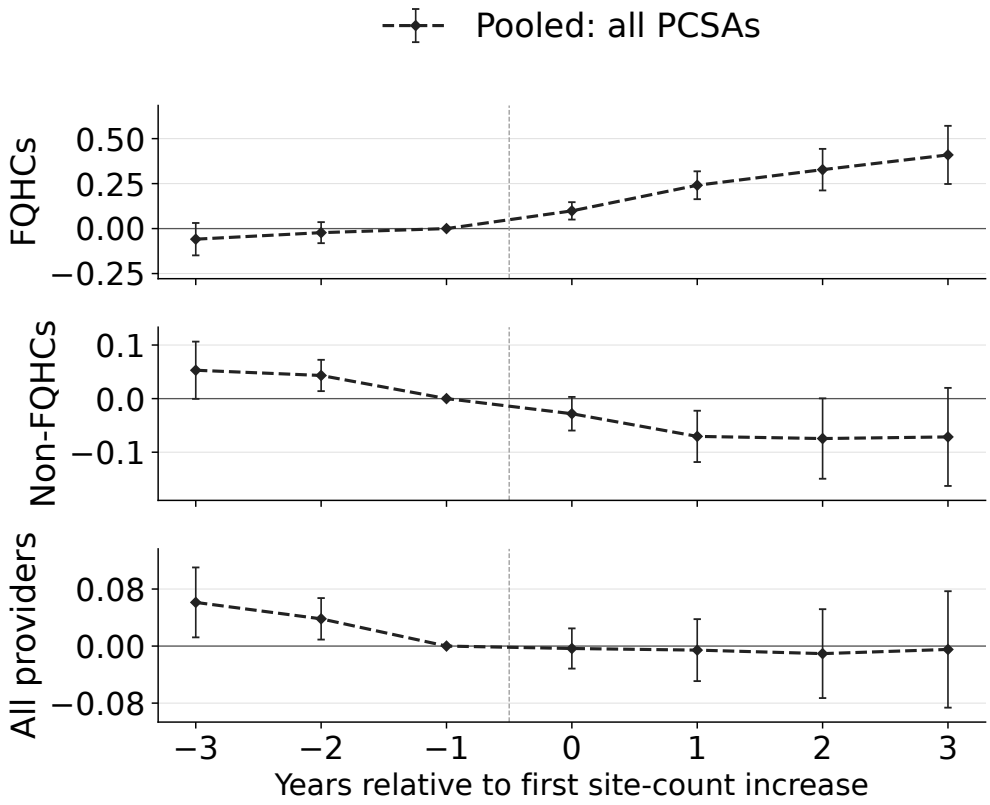


Figure A5: Primary-Care Medicaid Billing Around an FQHC Site Addition, Without Controls

Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort and year effects, for paid amounts on the primary-care service codes defined in the appendix (the Medicare Shared Savings Program code list plus its Medicaid FQHC encounter and preventive-visit analogues). Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the counterpart to main-text Figure 3 without state-by-year effects and log population, using the same stable-count comparison markets and outcome-specific vertical scales.

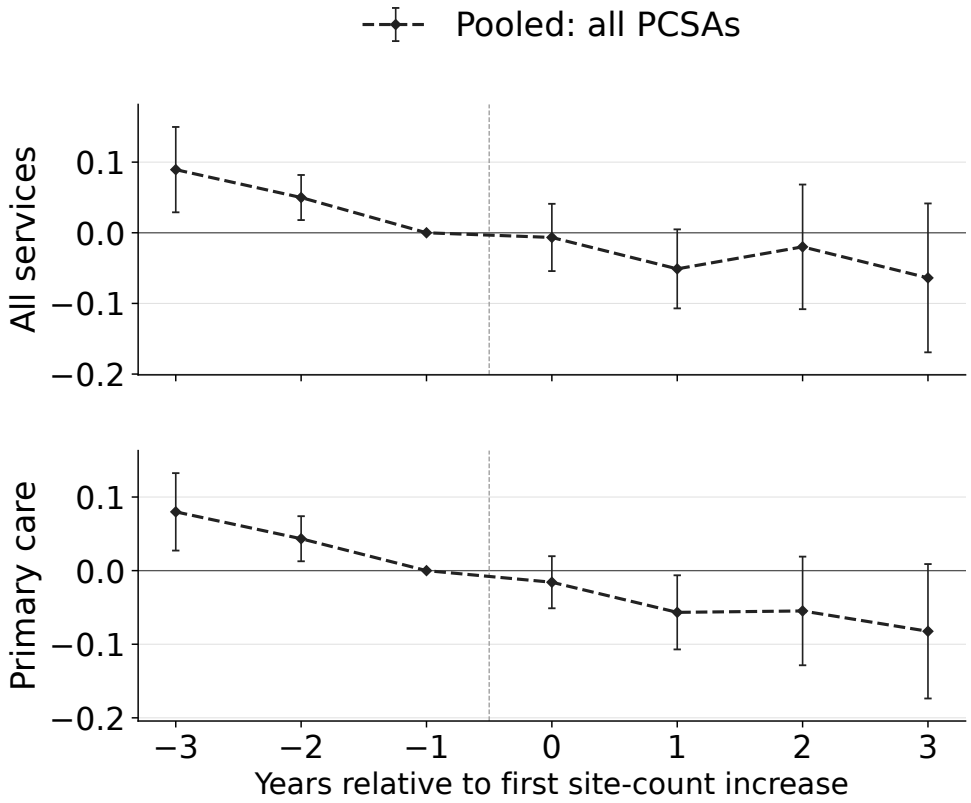


Figure A6: Billing by Non-FQHC Primary-Care Providers Around an FQHC Site Addition, Without Controls

Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort and year effects, for all-service and primary-care paid amounts among non-FQHC primary-care providers, the subset of non-FQHC providers defined in the appendix. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the counterpart to Appendix Figure A3 without state-by-year effects and log population, using the same stable-count comparison markets and outcome-specific vertical scales.

Figures A7, A8, and A9 repeat Figures 2, 3, and A3 with the comparison markets reweighted by the coarsened-exact-matching weights behind the matched-controls column of main-text Table 1: the fundamentals design, which matches on metropolitan status, the 2018 FQHC site count in four bins, and terciles of 2018 population and primary-care billing per resident (54 strata). The fits keep state-by-year effects and log population. Treated markets weigh one, so the treated-observation weighting of the cohort-year effects is unchanged; the 50 treated PCSAs in strata without a comparison market are dropped, and the weighted comparison arm has an effective size of about 470 PCSAs. Each panel keeps the vertical scale of its controlled counterpart, widened where the wider matched intervals extend beyond it. The paths keep their shape under matching. FQHC billing rises along the same path, with a somewhat steeper lead three years before entry and wider intervals, since the weighted comparison arm rests on fewer markets. Non-FQHC primary-care billing dips in the entry year and the year after by somewhat more than in the main-text figure. For non-FQHC primary-care providers, the upward pre-entry movement of the controlled figure is absent, and both paths are flat before and after entry. The primary-care total rises more than in the main-text figure in the second and third years after entry, from leads at or below zero rather than above it, and the all-service total drifts upward across the window, from below zero before entry to slightly above it after, where the main-text path is flat or slightly declining.

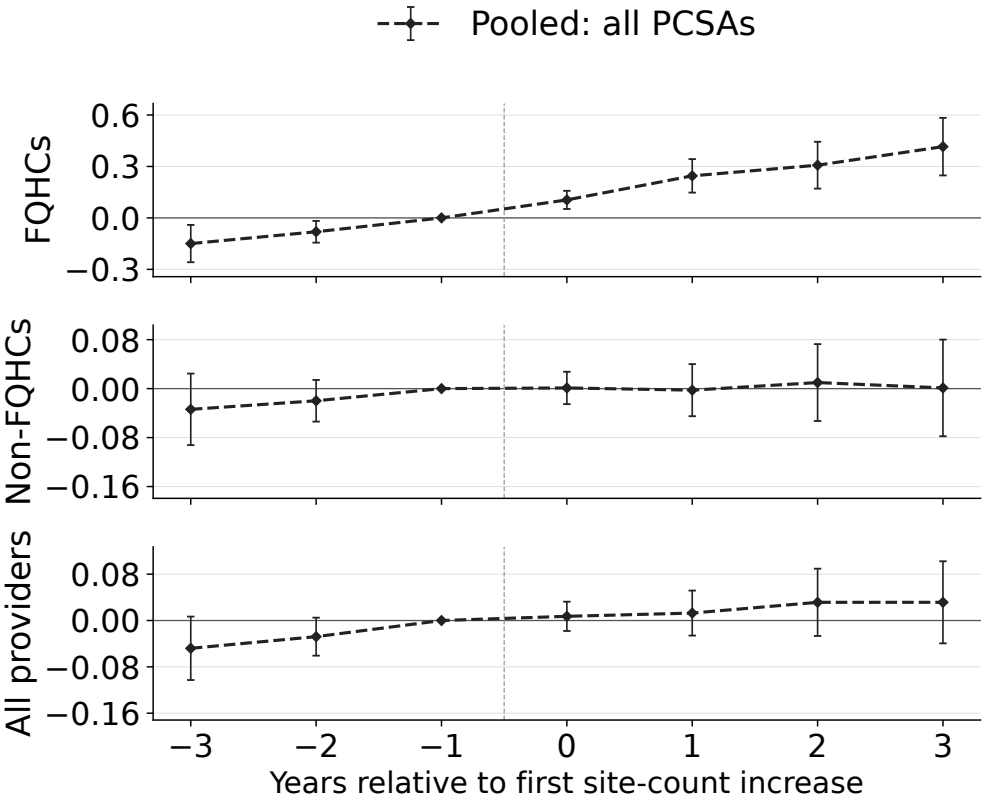


Figure A7: All-Service Medicaid Billing Around an FQHC Site Addition, Matched Controls

Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort effects, state-by-year effects, and log population, for FQHC, non-FQHC, and all-provider paid amounts. Comparison PCSAs are reweighted by coarsened exact matching on 2018 characteristics (metropolitan status, the FQHC site count in four bins, and terciles of population and primary-care billing per resident), as in main-text Table 1; treated PCSAs in strata without a comparison market are dropped. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the matched-controls counterpart to main-text Figure 2, on the same outcome-specific vertical scales, widened where the matched intervals extend beyond them.

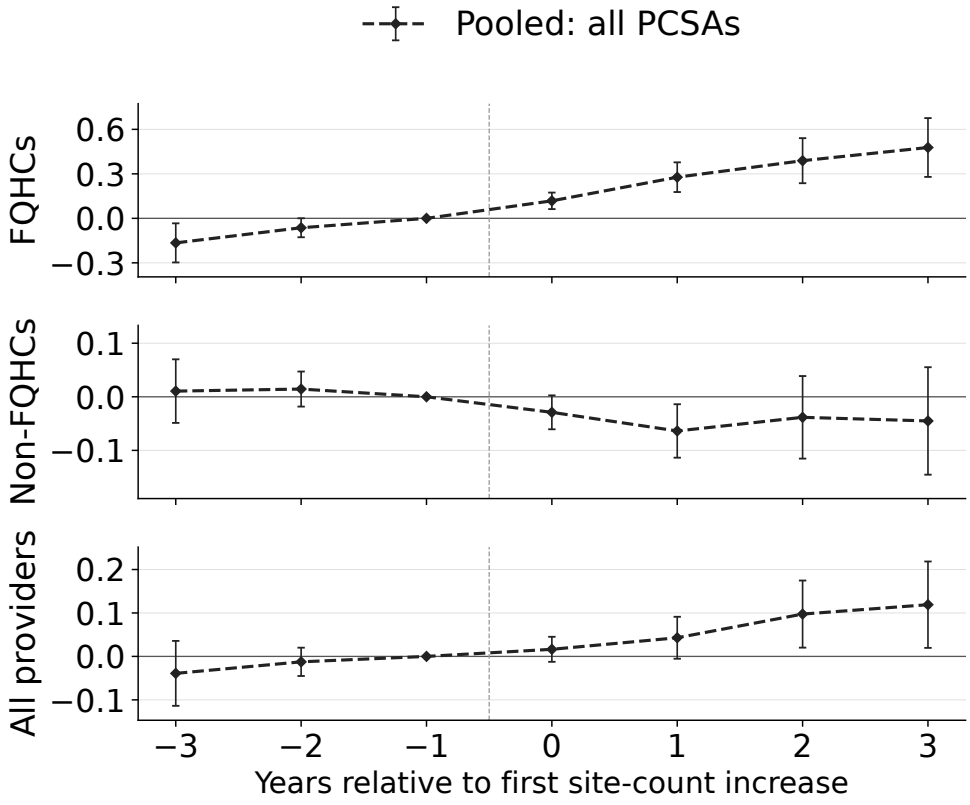


Figure A8: Primary-Care Medicaid Billing Around an FQHC Site Addition, Matched Controls

Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort effects, state-by-year effects, and log population, for paid amounts on the primary-care service codes defined in the appendix (the Medicare Shared Savings Program code list plus its Medicaid FQHC encounter and preventive-visit analogues). Comparison PCSAs are reweighted by coarsened exact matching on 2018 characteristics (metropolitan status, the FQHC site count in four bins, and terciles of population and primary-care billing per resident), as in main-text Table 1; treated PCSAs in strata without a comparison market are dropped. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the matched-controls counterpart to main-text Figure 3, on the same outcome-specific vertical scales, widened where the matched intervals extend beyond them.

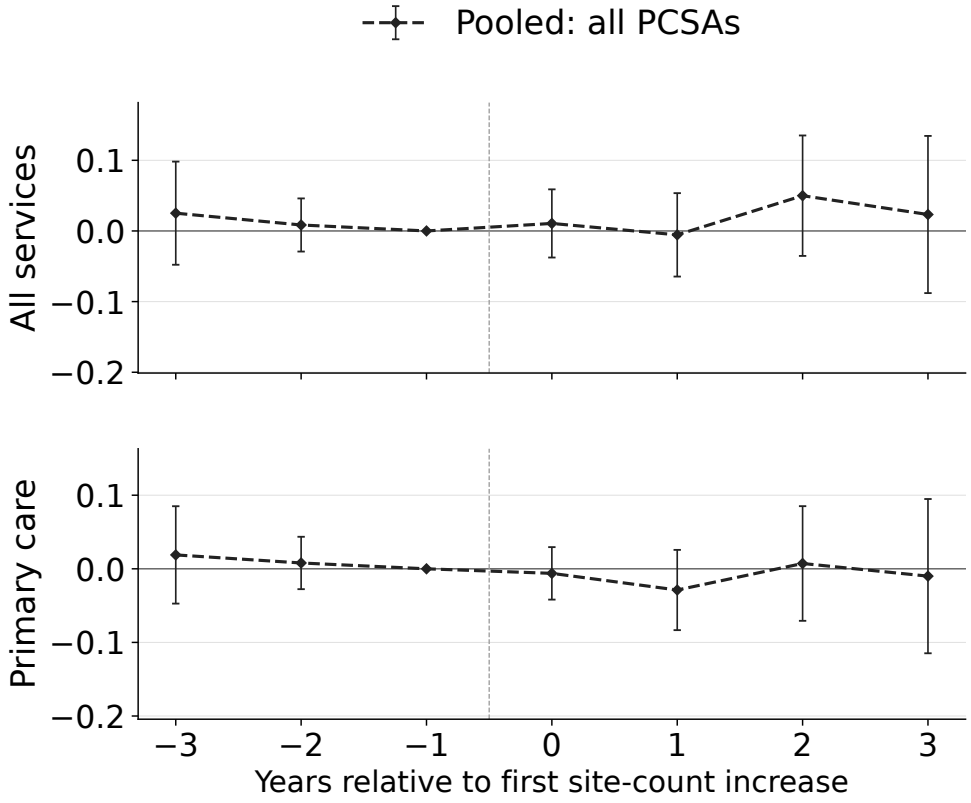


Figure A9: Billing by Non-FQHC Primary-Care Providers Around an FQHC Site Addition, Matched Controls

Notes: Poisson event-study coefficients from main-text equation (1), fit with cohort effects, state-by-year effects, and log population, for all-service and primary-care paid amounts among non-FQHC primary-care providers, the subset of non-FQHC providers defined in the appendix. Comparison PCSAs are reweighted by coarsened exact matching on 2018 characteristics (metropolitan status, the FQHC site count in four bins, and terciles of population and primary-care billing per resident), as in main-text Table 1; treated PCSAs in strata without a comparison market are dropped. Exact event times relative to FQHC entry; $h = -1$ is the reference and endpoints are not binned. Bars are pointwise 95 percent PCSA-clustered intervals. Cohort support changes across horizons. This is the matched-controls counterpart to Appendix Figure A3, on the same outcome-specific vertical scales, widened where the matched intervals extend beyond them.

Joint tests of the pre-entry coefficients

Table A10 reports the joint Wald test of the two displayed leads ($h = -3$ and $h = -2$) for each all-openings ETWFE outcome, stratum, and specification. In pooled estimates, the baseline primary-care total and both non-FQHC primary-care provider aggregates reject at the 5 percent level; the corresponding controlled tests do not. FQHC primary-care billing and all-service market totals do not reject in either pooled specification. The non-FQHC primary-care test also does not reject at the 5 percent level, although its baseline path slopes downward before entry. These are diagnostics of pre-entry movement, not proofs of parallel untreated trends, and the narrower subgroup fits can differ from the pooled tests.

Table A10: Joint Tests of the Two Pre-Entry Coefficients: All-Openings Event Studies

	All PCSAs		Rural PCSAs		Urban PCSAs	
	Baseline	Controls	Baseline	Controls	Baseline	Controls
All-services paid						
FQHCs	0.473	0.037	0.504	0.123	0.536	0.043
Non-FQHCs, all	0.582	0.943	0.046	0.241	0.881	0.854
Non-FQHCs, primary care providers	0.004	0.096	0.001	0.000	0.012	0.218
All providers	0.496	0.866	0.056	0.362	0.782	0.809
Primary-care services paid						
FQHCs	0.439	0.088	0.795	0.405	0.387	0.065
Non-FQHCs, all	0.014	0.609	0.001	0.007	0.014	0.412
Non-FQHCs, primary care providers	0.006	0.113	0.000	0.000	0.047	0.454
All providers	0.020	0.489	0.019	0.037	0.031	0.806
Primary-care competitor paid amounts by provider type						
Rural health clinics	0.170	0.455	0.002	0.038	0.037	0.137
Urgent care	0.301	0.304	0.001	0.000	0.609	0.610
Other primary-care clinics	0.005	0.046	0.026	0.102	0.012	0.042
Office-based practices	0.268	0.727	0.385	0.731	0.348	0.790
Hospital-billed	0.085	0.178	0.018	0.045	0.006	0.013
Other organizations	0.637	0.965	0.000	0.000	0.359	0.251

Notes: p-values of PCSA-clustered Wald tests that the $h = -3$ and $h = -2$ coefficients of the Poisson event studies in main-text equation (1) are jointly zero relative to the year before FQHC entry ($h = -1$), on the all-openings sample, by stratum and specification. Baseline includes cohort and year effects. Controls includes cohort and state-by-year effects and log population. Provider-type rows use primary-care paid amounts.

Mortality design, event-time support, and treatment history

Table 7 and Figure 5 in the main text use IHME's modeled age-standardized mortality outcomes for 2000–2019 and the combined POS/AHRF count history. Raw CMS POS counts supply 2000–2004; AHRF supplies 2005–2020. Treatment is the first observed annual increase after summing component-county counts within IHME's stable geographic units. The analysis retains all first-increase cohorts in 2001–2019, including 2005 at the source boundary. Because a source difference at that boundary could affect the measured annual change, the AHRF-only comparison below is informative about treatment-history construction.

Of 3,079 complete IHME outcome units, 3,078 match the treatment panel and all enter estimation. The combined history identifies 1,825 treated units, including 465 first treated in 2001–2004, and 1,253 units untreated through 2019. Callaway–Sant'Anna outcome-regression estimates use not-yet-treated controls, a universal $h = -1$ reference, and zero anticipation. Inference aggregates unit influence functions to 51 state clusters, including DC. State clustering allows dependence within states; it does not absorb state-level confounding.

Table A11 reports which cohorts contribute at each event time. Earlier cohorts remain in every supported post-entry comparison, while later cohorts contribute shorter postperiods. Within an event year, cohort effects are weighted by cohort size. Static estimates equally average the six event-year effects at $h = 0, \ldots, 5$, so the contributing cohort mix changes across that average. The event paths are not binned at their endpoints.

Table A11: Mortality Event-Time Support: Combined POS/AHRF History

Event year	First cohort	Last cohort	Treated units
-5	2005	2019	1,360
-4	2004	2019	1,512
-3	2003	2019	1,656
-2	2002	2019	1,738
-1	2001	2019	1,825
0	2001	2019	1,825
1	2001	2018	1,778
2	2001	2017	1,723
3	2001	2016	1,648
4	2001	2015	1,565
5	2001	2014	1,477

Notes: Counts refer to treated stable IHME units with an observed outcome and reference year. All matched units remain in the group-time estimation panel. Event year -1 is normalized to zero rather than separately estimated. Not-yet-treated comparisons vary by cohort and calendar year.

Figure A10 reports the unadjusted baseline paths for the ten mortality outcomes in Table 7, reading in table order from left to right and top to bottom. Main-text Figure 5 shows their linear-trend-removed counterparts.

Linear pre-trend adjustment. The adjustment is applied separately to each outcome after estimating and aggregating the Callaway–Sant'Anna cohort-time effects. Let $\widehat{\boldsymbol{\theta}}$ contain the ten event-year estimates at $h = -5, \ldots, 5$, excluding the normalized reference $h = -1$. Let $\widehat{\Sigma}$ be their joint covariance matrix, constructed by summing the unit influence functions within states and taking the cross-product of those state sums divided by the squared number of units. Its diagonal reproduces the original state-clustered variances. The four pre-entry coefficients form $\widehat{\boldsymbol{\theta}}_{\text{pre}}$, with covariance $\widehat{\Sigma}_{\text{pre}}$.

I fit a generalized least squares (GLS) line to those four coefficients using $x = (-4, -3, -2, -1)'$, the values of $h + 1$ at $h = -5, \ldots, -2$:

$$\widehat{\gamma} = \left(x' \widehat{\Sigma}_{\text{pre}}^{-1} x \right)^{-1} x' \widehat{\Sigma}_{\text{pre}}^{-1} \widehat{\boldsymbol{\theta}}_{\text{pre}}, \qquad \widehat{\theta}_h^{\text{adj}} = \widehat{\theta}_h - \widehat{\gamma}(h + 1). \tag{A2}$$

The line has no free intercept: $h+1$ anchors it at zero in the $h=-1$ reference period. GLS uses both the precision of the pre-entry estimates and their correlations. Post-entry coefficients do not enter the slope fit. The fitted line is then subtracted at every event year, preserving the normalized reference.

To propagate slope uncertainty, write $\widehat{\gamma} = w'\widehat{\boldsymbol{\theta}}$, where w contains the GLS weights on the four pre-entry coefficients and zeros on the post-entry coefficients. With $z_h = h+1$ and $A = I - zw'$, the adjusted path and covariance are

$$\widehat{\boldsymbol{\theta}}^{\,\mathrm{adj}} = A\widehat{\boldsymbol{\theta}}, \qquad \widehat{\Sigma}^{\,\mathrm{adj}} = A\widehat{\Sigma}A'. \tag{A3}$$

This calculation includes the covariance between the fitted slope and each original coefficient, treating the estimated GLS weights as fixed. Pointwise intervals use $\widehat{\theta}_h^{\mathrm{adj}} \pm 1.96\sqrt{\widehat{\Sigma}_{hh}^{\mathrm{adj}}}$. The static adjusted ATT averages $h = 0, \ldots, 5$ equally and therefore equals the raw static ATT minus $3.5\widehat{\gamma}$; its variance is $L\widehat{\Sigma}^{\,\mathrm{adj}}L'$ if L is the row vector assigning weight $1/6$ to each post-entry coefficient and zero elsewhere. The implementation checks the static point-estimate identity.

This is a post-estimation sensitivity, not a re-estimation with unit- or cohort-specific trends. Because the cohorts represented in the leads differ from each other and from the postperiod, the fitted slope can reflect cohort composition as well as differential trends. Interpretation requires extrapolating that slope beyond entry. Separate diagnostics test whether the four raw leads are jointly zero, whether the fitted slope is zero, and whether the three remaining degrees of freedom reject a linear fit. For broad ACSC and all-cause mortality, the raw leads reject a joint zero ($p = 0.002$ and $p < 0.001$) and reject linearity around the fitted slope ($p = 0.035$ and $p = 0.008$). The confidence intervals account for estimation uncertainty, but do not account for uncertainty about the linear extrapolation assumption.

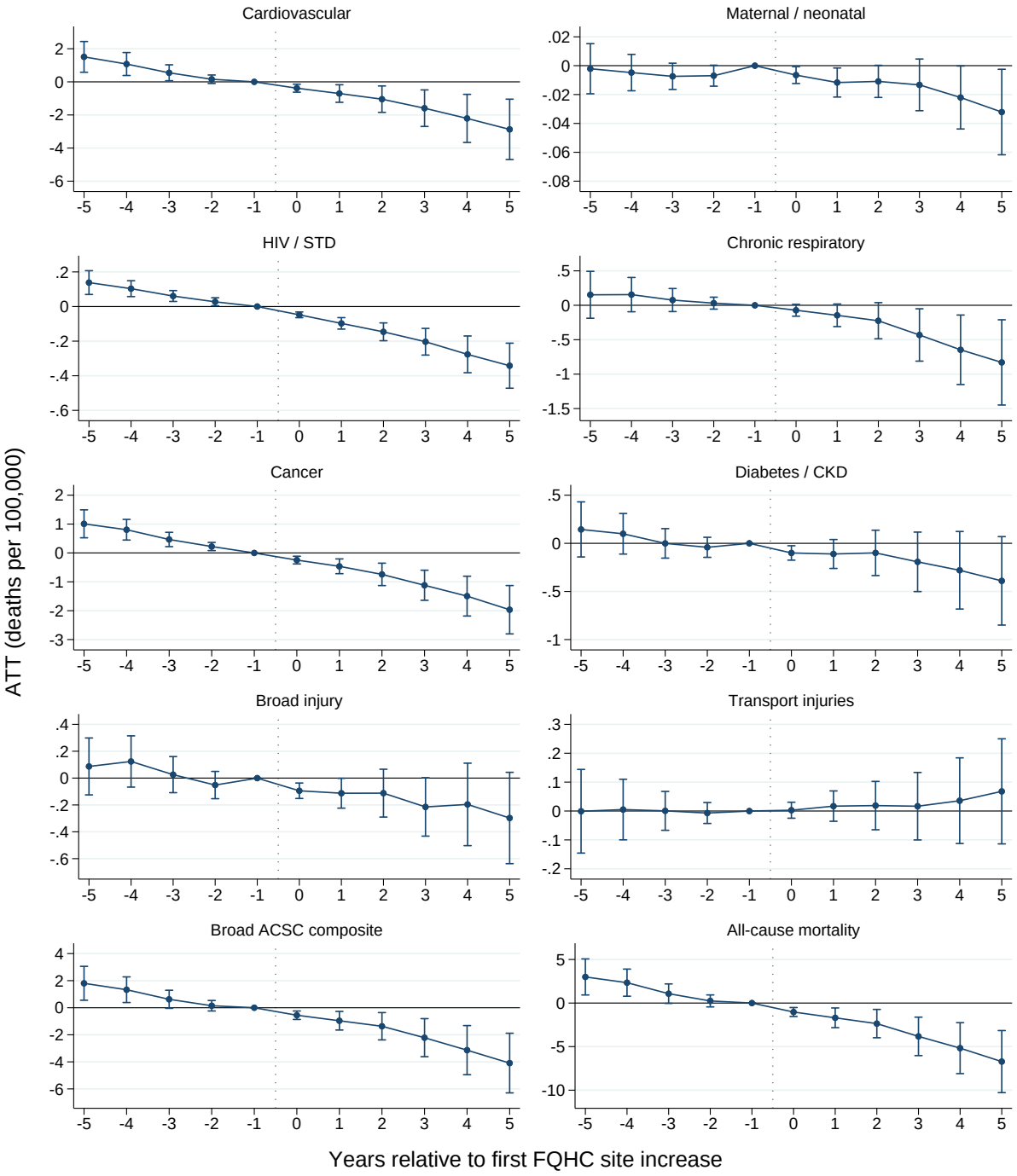


Figure A10: Mortality Event Studies Without Linear Trend Removal

Notes: These are the unadjusted counterparts to main-text Figure 5, on the same outcome-specific vertical scales. The combined POS/AHRF design retains all 2001–2019 cohorts at supported event times, relative to $h = -1$. Treated-unit support is 1,360 at $h = -5$, 1,825 at $h = 0$, and 1,477 at $h = 5$. Pointwise 95% intervals are clustered by state and condition on the modeled IHME outcomes.

Table A12: Mortality After FQHC Entry, With and Without Linear Trend Removal

Outcome	Treated mean at $t = -1$	Raw ATT	ATT
Cardiovascular	314.9	-1.467*** (0.498)	-0.273 (0.302)
Maternal / neonatal	4.8	-0.016** (0.007)	-0.023*** (0.008)
HIV / STD	2.9	-0.186*** (0.034)	-0.062*** (0.018)
Chronic respiratory	73.8	-0.391** (0.169)	-0.255** (0.114)
Cancer	234.8	-1.006*** (0.237)	-0.051 (0.160)
Diabetes / CKD	58.4	-0.195 (0.134)	-0.025 (0.116)
Broad injury	70.1	-0.171* (0.096)	-0.062 (0.086)
Transport injuries	24.4	+0.027 (0.049)	+0.024 (0.053)
ACSC composite	447.2	-2.054*** (0.619)	-0.482 (0.385)
All-cause	949.1	-3.467*** (0.995)	-0.600 (0.664)

Notes: IHME modeled age-standardized deaths per 100,000. Treatment is FQHC entry, the first increase in the FQHC count in the combined POS/AHRF history. All 2001–2019 cohorts are retained at supported event times. Raw ATT averages the unadjusted event-year 0–5 estimates; ATT removes an outcome-specific linear pre-trend fitted to the pre-entry coefficients, as in equation (A2). Cohort composition varies across event years. Parentheses contain standard errors clustered by state. The treated mean uses all 1,825 treated units at their own year −1. Stars denote nominal tests: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Respiratory and maternal/neonatal mortality remain nominally negative after adjustment, with joint-zero prepath p-values of 0.313 and 0.234. Their adjusted outcome-wise p-values are 0.026 and 0.005; Holm correction across the original family of 13 estimated outcomes (including three not displayed) raises these to 0.284 and 0.058. HIV/STD mortality has an adjusted Holm p-value of 0.010, but its raw prepath strongly rejects a joint zero. The summary files report both Holm and Bonferroni adjustments separately for the raw and detrended columns; table stars remain nominal. All inference conditions on IHME's modeled point estimates and excludes IHME posterior uncertainty.

AHRF-only treatment history. As a source sensitivity, I replace the separate POS counts for 2000–2004 with AHRF's published `F13320` history from archived ARF releases. The archived and later AHRF releases agree in their overlapping 2005–2008 cells. This alternative uses the same 3,078 geographic units, retains all 2001–2019 cohorts, and identifies 1,801 treated units and 1,277 units untreated through 2019. The count levels coincide across histories from 2005 onward, but differences before 2005 can alter first-increase timing, including the change measured at the 2005 boundary. The comparison is therefore a

treatment-history sensitivity, not an independent post-2005 source replication.

Table A13: Mortality Estimates Using AHRF-Only Treatment History

Outcome	Treated mean at $t = -1$	Raw ATT	ATT
Cardiovascular	314.7	-1.451*** (0.508)	-0.304 (0.294)
Maternal / neonatal	4.8	-0.014** (0.007)	-0.019** (0.008)
HIV / STD	3.0	-0.179*** (0.033)	-0.065*** (0.020)
Chronic respiratory	74.0	-0.353** (0.165)	-0.226** (0.106)
Cancer	234.8	-0.970*** (0.234)	-0.121 (0.162)
Diabetes / CKD	58.5	-0.168 (0.138)	-0.040 (0.119)
Broad injury	70.2	-0.169* (0.095)	-0.093 (0.080)
Transport injuries	24.4	+0.020 (0.048)	-0.015 (0.051)
ACSC composite	447.2	-1.971*** (0.636)	-0.566 (0.371)
All-cause	949.6	-3.335*** (1.009)	-0.886 (0.664)

Notes: IHME modeled age-standardized deaths per 100,000. Treatment is FQHC entry, the first increase in the FQHC count in the AHRF-only history. Raw ATT averages the unadjusted event-year 0–5 estimates; ATT removes an outcome-specific linear pre-trend fitted to the pre-entry coefficients. Cohort composition varies across event years. Parentheses contain standard errors clustered by state. The treated mean uses all 1,801 treated units at their own year -1. All first-increase cohorts in 2001–2019 are retained at observed event times; support is 1,304 treated units at $h = -5$, 1,801 at $h = 0$, and 1,447 at $h = 5$. Stars denote nominal tests: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The AHRF-only estimates have the same broad pattern: raw all-cause and ACSC mortality decline by 3.335 and 1.971 deaths per 100,000, while the detrended estimates are -0.886 (SE $= 0.664$) and -0.566 (SE $= 0.371$). Respiratory and maternal/neonatal mortality remain nominally negative after adjustment. These comparisons leave the interpretation dependent on differential trends and outcome measurement while showing that the full-cohort findings are similar across the two treatment histories.

F Conceptual Framework

FQHC expansion changes access to a local network of providers. For a primary-care episode, patient i chooses among available FQHC and non-FQHC providers and forgone care. Utility from provider j is

$$u_{ij} = v_{ij} - \alpha_i p_{ij}^{\text{oop}} - \tau_i d_{ij} - \kappa_i w_j - a_{ij} + \varepsilon_{ij}, \tag{A4}$$

where v_{ij} values expected care quality and continuity, p_{ij}^{oop} is the patient's payment, d_{ij} is travel distance, w_j is waiting time, and a_{ij} captures administrative barriers. The nonnegative coefficients value these costs; ε_{ij} captures idiosyncratic preferences. Providers' payer acceptance restricts the choice set. Additional FQHC locations or appointment capacity can lower these access costs in markets with existing FQHCs. Patients can benefit from shorter travel or waiting even when they switch providers without increasing utilization.

Providers choose capacity, quality, and payer acceptance, trading off operating surplus and patient benefit subject to financial viability. Appointment availability and waiting adjust to demand and capacity. Grants can finance fixed costs or relax budget constraints; reimbursement changes returns to covered services; enabling services reduce patients' access costs. Medicaid, uninsured, and other insured patients generate different revenues and need not face the same availability (Alexander and Schnell, 2024). Supplying negative-margin care requires a motive or constraint, such as a service mission or contractual obligation. With patient-oriented provider objectives, the quality response to competition can be ambiguous (Brekke et al., 2011). Other practices may contract, reallocate appointments across payers, or lose staff to FQHCs; local capacity rises only if staffing, hours, or productivity expand sufficiently. The site-count design does not separately identify the effects of grants and reimbursement.

A restricted benchmark clarifies displacement. With a fixed population, one encounter choice per patient, and unchanged attributes of existing alternatives, adding an FQHC option weakly reduces non-FQHC encounters and forgone care. Thus $\Delta Q_N \leq 0$ and $0 \leq \Delta Q_T \leq$

ΔQ_F, where $Q_T = Q_F + Q_N$ counts encounters across provider types. Provider adjustment and repeated or complementary services relax these restrictions. They also need not hold for paid amounts or claim lines. The empirical decomposition is therefore an accounting of recorded activity, not a direct measurement of patient switching. Three extensions motivate discriminating tests.

Complementarity and health investment. Primary care can generate diagnoses and referrals that increase other services before disease management reduces later complications. Health is a stock to which care contributes over time (Grossman, 1972). Complementarity predicts growth in clinically related services following increased primary care, potentially alongside later declines in avoidable acute care. Distinguishing it from substitution requires separate paths for substitute primary care and complementary services; the present primary-care code set cannot establish a referral channel. Patient-level sequences would provide stronger evidence than aggregate timing, and mortality need not respond within the claims window.

Organization and billing. A site-count increase can accompany a physical opening, conversion, or billing reorganization. Capacity expansion predicts additional clinical inputs or encounters; reclassification can shift billing between sectors while clinicians, locations, and care persist. Tests should link site events to continuing clinicians and organizations, retain unresolved attribution, and compare payments with code-standardized quantities and independently measured encounters. An organizational exit alone cannot establish lost capacity, and paid amounts per line cannot isolate reimbursement from service mix.

Financing and redistribution. Additional Medicaid billing can finance previously uncompensated care as well as new services. The incidence of safety-net funding can therefore extend to providers that previously absorbed care costs (Garthwaite et al., 2018; Finkelstein et al., 2019). Financing substitution predicts greater compensated activity and lower

uncompensated-care burdens without commensurate growth in all-payer encounters. Testing it requires joint evidence on payer mix, encounters, and uncompensated-care costs for comparable populations and periods. Welfare also depends on access, health, financial protection, resource costs, and net fiscal costs; provider payments alone do not measure it.

These mechanisms can coexist; the main paper does not identify their separate contributions.