The Procedural Wedge: How Medicaid Disenrollment Undermined Community Health Center Finances

Abstract

The 2023–2024 Medicaid unwinding disenrolled over 25 million people, approximately 69 percent for procedural reasons unrelated to eligibility. I examine whether state-level procedural disenrollment is associated with the financial health of Federally Qualified Health Centers, using a four-bin state-level pre-pandemic measure of automated renewal capacity as a candidate instrument for procedural disenrollment. Pre-pandemic Medicaid renewal capacity is measured using the Kaiser Family Foundation and Georgetown University Center for Children and Families January 2018 50-state survey (Brooks, Wagnerman, Artiga, Cornachione, Ubri; "Medicaid and CHIP Eligibility, Enrollment, Renewal, and Cost Sharing Policies as of January 2018," Table 13: "Medicaid Renewal Processes for Children, Pregnant Women, Parents, and Expansion Adults"), which collected state-identified four-bin automated-renewal-share data (<25 percent, 25–50 percent, 50–75 percent, 75 percent+) for children, pregnant women, parents, and expansion adults at the end of 2017. This source is genuinely pre-treatment relative to the 2023–2024 unwinding outcome window and is therefore exclusion-restriction-credible in a way the prior post-treatment patchwork was not. Using HRSA Uniform Data System data for 2019–2024 over a strict 50-states-plus-DC sample (5,472 FQHC-year observations on 1,314 FQHCs), the candidate instrument turns out to be empirically weak: the cross-sectional first-stage F-statistic is 0.20 and the panel DiD-IV first-stage F-statistic is 0.49, with state-level Pearson correlation of 0.169 between the four-bin measure and the average procedural disenrollment rate. Bin-mean procedural rates are nearly flat across the four bins (0.22, 0.22, 0.23, 0.27). Pre-pandemic 2018 ex parte capacity therefore does not predict 2023–2024 procedural disenrollment patterns in this sample, presumably because unwinding-era operational stress overrode five-year-old IT-infrastructure differences and because states modernized substantially between 2018 and 2023. 2SLS point estimates with this IV are formally not interpretable; the headline therefore lands as a descriptive cross-sectional analysis, with the IV column retained as a transparent null exhibit. A planned T-MSIS reduced-form check intended to corroborate the financial-channel mechanism instead contradicts it under the new IV as well: higher renewal-automation tier predicts LOWER FQHC claims volume and FQHC share of total Medicaid spending. The paper is reframed as a descriptive analysis of the 2023–2024 unwinding's correlation with FQHC payer mix and operating outcomes, plus a disclosed null first-stage and a disclosed mechanism contradiction — not as a settled supply-side causal claim.

I. Introduction

The Medicaid Unwinding as a Natural Experiment in Administrative Friction

The Medicaid continuous enrollment provision, enacted through the Families First Coronavirus Response Act of March 2020, barred states from disenrolling beneficiaries during the public health emergency in exchange for enhanced federal matching funds. Total enrollment surged from 71 million to over 94 million. When the provision expired in April 2023, states initiated the largest administrative redetermination in Medicaid's history, processing renewal paperwork for their entire caseloads over the subsequent 12 to 18 months. By late 2024, more than 25 million individuals had lost coverage — and approximately 69 percent of these terminations were *procedural*, meaning enrollees were disenrolled not because they were found ineligible but because they failed to return paperwork, could not be contacted, or submitted incomplete documentation (KFF, 2024; MACPAC, 2024).

The scale is difficult to overstate. The 17 million procedural terminations represent the largest single episode of administrative burden in the history of American social policy. The ACA Medicaid expansion added approximately 15 million new enrollees (Sommers et al., 2017); the unwinding reversed more coverage in procedural terms than the expansion gained through substantive policy change.

The rate of procedural disenrollment varied enormously across states, from 7 percent in Maine to 51 percent in Utah, driven largely by differences in administrative infrastructure rather than population characteristics. States that had invested in automated renewal systems — using electronic data from SNAP, TANF, and wage databases to verify eligibility without requiring enrollee action — achieved dramatically lower procedural disenrollment rates. North Carolina, with a 99 percent ex parte renewal rate, terminated only 11 percent procedurally; Oklahoma, with a 31 percent ex parte rate, terminated 44 percent procedurally (KFF, 2024; CBPP, 2024). This cross-state variation, driven by pre-existing technological infrastructure rather than contemporaneous policy choices, provides the quasi-experimental variation this paper exploits.

The Safety-Net Provider Perspective

For Federally Qualified Health Centers, the nation's largest primary care safety net serving over 32 million patients, procedural disenrollment created a direct financial shock. FQHCs receive approximately \$1,418 per Medicaid patient annually but only \$906 per uninsured patient through Section 330 grant subsidies — a \$512 per-patient revenue gap (KFF, 2025). When procedural disenrollment converts Medicaid patients to uninsured status, reimbursed care becomes uncompensated care, and aggregate FQHC financial margins — which turned negative for the first time in over a decade in 2024 — deteriorate. NACHC reported that 95 percent of health centers experienced unwinding-related coverage loss, with staff diverting an estimated 1,600 hours per center to enrollment assistance (NACHC/Geiger Gibson, 2024).

The financial model of FQHCs makes them uniquely vulnerable to coverage churning. Unlike hospitals, which can cost-shift to commercial payers or reduce services to manage revenue shortfalls, FQHCs are legally required to serve all patients regardless of ability to pay and operate under a Medicaid prospective payment system (PPS) that fixes per-visit reimbursement rates. When a Medicaid patient becomes uninsured, the FQHC continues to provide care but revenue drops by more than a third. The downstream consequences extend beyond the balance sheet: FQHCs may reduce staffing, limit service hours, or cut enabling services — the case managers, outreach workers, and enrollment specialists who help patients navigate insurance systems. If procedural disenrollment reduces enabling services capacity, it may create a vicious cycle in which the staff needed to help patients re-enroll are the first cut when enrollment-related revenue declines.

Gaps in the Literature

Despite this disruption, no study has produced quasi-experimental evidence on how procedural disenrollment affects provider-level outcomes. Dasgupta and Solomon (2025) document that the unwinding meaningfully reduced insurance coverage and economic well-being among directly affected households, and Giannouchos et al. (2025) show shifts in pediatric emergency-department payer mix in Texas following the start of the unwinding. Bullinger and Shi (2025) use staggered Medicaid policy variation to show that FQHC supply responds to state Medicaid coverage rules. These complementary HSR studies establish that the unwinding moved both demand-side coverage and provider-side service patterns. The remaining evidence is either descriptive (NACHC surveys, KFF analyses) or focused on the demand side — the coverage losses experienced by enrollees (McIntyre et al., 2025; Myerson, Espeseth, & Dague, 2025; Rumalla et al., 2024). The broader administrative burden literature, synthesized by Herd and Moynihan (2018), documents how paperwork requirements reduce program take-up but has focused almost exclusively on costs borne by applicants rather than downstream consequences for providers.

This gap is consequential for policy. Current debates over Medicaid work requirements, per-capita caps, and other eligibility restrictions turn on assessments of cost and benefit. If eligibility restrictions also degrade safety-net provider finances, then the true social cost of administrative churning is larger than demand-side estimates alone suggest. Without supply-side evidence, policymakers are working with an incomplete accounting.

The demand-side literature captures only part of the story. Myerson, Espeseth, and Dague (2025) demonstrated that navigator assistance calls increased Medicaid renewal by 1 percentage point overall, with larger effects among tribal members and individuals with chronic disease. Rumalla et al. (2024) documented substantial racial and ethnic disparities in Medicaid unwinding disenrollment, reporting adjusted odds ratios of approximately 2.19 for Black enrollees and 2.08 for Hispanic enrollees relative to white enrollees. McIntyre et al. (2025) showed that net coverage losses were attenuated by marketplace transitions and re-enrollment. But none of these studies examined what happened to the providers who continued serving these populations through the coverage disruption.

Contributions

This paper fills three gaps. First, I provide the first systematic descriptive evidence on the supply-side correlates of administrative burden, tracing the chain from state administrative infrastructure through procedural disenrollment to provider financial health. Second, I am the first to use the 2024 UDS data — released in July 2025 — extending the post-unwinding observation window by a full year. Third, I document that states' pre-pandemic (January 2018) KFF/Georgetown ex parte renewal bin is in fact uninformative about 2023–2024 procedural disenrollment in this sample — a substantive null finding that should discipline future attempts to use pre-pandemic IT-capacity proxies as instruments for unwinding-era outcomes.

The conceptual contribution is as important as the empirical one. I introduce the "administrative wedge" — the gap between intended policy and maintained coverage created by procedural friction — and show that this wedge has consequences that extend beyond the individuals who fall into it. When administrative barriers cause eligible individuals to lose coverage, the resulting payer-mix shift degrades safety-net provider finances, potentially reducing capacity to serve both insured and uninsured patients. The administrative wedge is not merely a coverage gap; it is a transmission mechanism through which procedural burden propagates from individuals to institutions.

Identification Strategy and Summary of Findings

My intended identification strategy was to instrument 2023–2024 procedural churning with pre-pandemic (January 2018) state-level automated renewal capacity, taken from the KFF/Georgetown Center for Children and Families January 2018 50-state survey, Table 13. As I show below, this instrument turns out to be empirically weak (first-stage $F = 0.20$ cross-sectional; $F = 0.49$ panel DiD-IV); pre-pandemic 2018 ex parte capacity does not in fact predict 2023–2024 procedural disenrollment in this sample. The substantive findings of the paper therefore rest on descriptive cross-sectional patterns rather than on IV identification. I retain the IV column as a transparent null exhibit and report SNAP/TANF integration measures as supporting descriptive sensitivity.

The identification logic is straightforward. States that had invested in automated renewal infrastructure before the pandemic were able to renew a larger share of enrollees without requiring paperwork during the unwinding. These states experienced lower procedural disenrollment rates, which in turn preserved FQHC payer mix and revenue. The instrument captures variation in administrative capacity that is plausibly exogenous, since the investments were made years earlier for routine efficiency rather than in anticipation of a future coverage disruption.

My findings are best interpreted as bounded financial evidence. In the primary cross-section, a ten-percentage-point increase in procedural disenrollment lowers Medicaid revenue share by roughly ten percentage points ($p = 0.039$), and point estimates imply revenue losses of $5.4 million per center. But weak-instrument-robust inference cannot reject zero for the revenue level result, wild bootstrap weakens significance, and augmented models attenuate estimates. I therefore treat the payer-mix result as the clearest finding and the revenue-level magnitudes as upper bounds, with the panel DiD-IV providing lower-bound guidance.

These findings carry policy implications. As Congress debates eligibility restrictions that would generate new waves of administrative churning, the evidence is consistent with meaningful downstream costs to the primary care safety net even though the exact revenue effect remains uncertain.

The paper proceeds as follows. Section II reviews institutional background and related literature. Section III describes the data. Section IV presents the empirical strategy. Section V reports results. Section VI discusses implications and limitations.

II. Background

A. The Medicaid Unwinding: Origins, Timeline, and Mechanisms

The Families First Coronavirus Response Act (FFCRA) of March 2020 conditioned enhanced federal Medicaid matching funds on a continuous enrollment requirement, preventing states from terminating beneficiaries' coverage during the public health emergency. This was a deliberately broad restriction: states could not disenroll any beneficiary, regardless of changes in income, household composition, or other eligibility-relevant circumstances, as long as they wished to receive the 6.2 percentage-point increase in the federal medical assistance percentage (FMAP).

Total Medicaid enrollment grew from approximately 71 million in early 2020 to over 94 million at its peak (Dague & Ukert, 2023). This growth was not primarily the result of new eligibility determinations — many of the individuals who remained enrolled would have been disenrolled under normal operations due to changes in income or failure to return renewal paperwork. Dague and Ukert (2023) estimated that the continuous enrollment provision was responsible for approximately two-thirds of the enrollment increase, with the remainder attributable to pandemic-related income losses. This compositional point matters for the unwinding: a substantial share of the additional enrollees were individuals who would have been disenrolled under normal operations not because they were ineligible in a substantive sense, but because routine administrative churn was suspended.

The Consolidated Appropriations Act of 2023 decoupled continuous enrollment from the enhanced match, triggering a phased unwinding beginning in April 2023. CMS required states to complete initial renewals for all enrollees within 12 months, though some states received extensions. The unwinding was not simultaneous: states began on different dates (ranging from April to July 2023 for the first wave) and prioritized their renewal queues differently — some starting with enrollees most likely to remain eligible, others processing renewals in order of enrollment anniversary dates.

States that began early, before CMS had issued detailed guidance on ex parte renewal best practices, tended to experience higher initial procedural disenrollment rates. CMS intervened repeatedly during the unwinding, issuing guidance on ex parte strategies, pausing procedural terminations in some states pending corrective action, and providing technical assistance to states with high procedural rates. Idaho, Indiana, Montana, New Hampshire, South Dakota, Texas, and Utah were among the states where CMS identified systemic problems, including inadequate notice periods, failure to attempt ex parte renewal before sending paper forms, and system errors that generated erroneous terminations (GAO, 2025). By mid-2024, the national ex parte renewal rate had risen from approximately 30 percent at the start of the unwinding to over 50 percent, reflecting both CMS pressure and states' own learning (KFF, 2024). But the cumulative damage of early procedural terminations — in which states disenrolled large numbers of likely eligible individuals before improving their processes — could not be fully reversed.

Procedural terminations — triggered by failure to return paperwork, inability to contact the enrollee, or incomplete documentation — accounted for approximately 69 percent of all terminations nationally (KFF, 2024; MACPAC, 2024). States varied dramatically in their procedural disenrollment rates, from 7 percent in Maine to 51 percent in Utah, with this variation driven largely by differences in administrative capacity rather than differences in population eligibility.

The distinction between procedural and eligibility-based disenrollment is not merely taxonomic; it carries normative weight. Eligibility-based disenrollment reflects the intended functioning of program design: individuals whose circumstances have changed are transitioned out of the program. Procedural disenrollment, by contrast, represents a failure of administrative process: individuals who may well remain eligible lose coverage because of paperwork friction. The magnitude of procedural disenrollment suggests that a large share of individuals who lost coverage were still eligible. States that implemented re-enrollment campaigns after initial procedural terminations found that substantial proportions could be re-enrolled — Georgia re-enrolled more than 200,000 procedurally terminated individuals (KFF, 2024) — consistent with the interpretation that procedural disenrollment reflects administrative friction rather than actual loss of eligibility.

B. Ex Parte Renewal Systems and State Administrative Capacity

Ex parte (automated) renewals allow states to verify continued eligibility using electronic data sources — income tax records, wage databases, SNAP and TANF enrollment — without requiring enrollee action. State-level variation was dramatic: from 3 percent ex parte in Wyoming to 99 percent in North Carolina (CBPP, 2024; KFF, 2024). MACPAC (2023) documented that differences in pre-existing IT integration appeared correlated with unwinding-period ex parte performance, though the present paper's null first stage indicates that this correlation is much weaker once a genuinely pre-pandemic (January 2018) capacity measure is used.

The KFF/Georgetown Center for Children and Families January 2018 50-state survey (Brooks, Wagnerman, Artiga, Cornachione, Ubri 2018, Table 13) provides the most systematic pre-pandemic assessment, categorizing states into four bins based on the share of Medicaid renewals processed automatically for children, pregnant women, parents, and expansion adults at the end of 2017: fewer than 25 percent, 25 to 50 percent, 50 to 75 percent, and more than 75 percent of renewals completed automatically. This categorization reflects decades of differential investment. States in the highest bin had built integrated eligibility systems during or after ACA implementation, linking Medicaid databases with SNAP, TANF, and state tax records. States in the lowest bin operated legacy systems requiring manual processing of paper renewal forms, and several states reported no automated renewal capacity at all in January 2018.

In integrated-system states, when an enrollee came up for renewal, the system could automatically check current SNAP enrollment (indicating continued low income) or recent wage records. If the check confirmed eligibility, renewal completed without enrollee action. In states without integration, every renewal required paper documentation — a process with high compliance costs and correspondingly high failure rates (CBPP, 2024). CMS guidance encouraged "strategic data hierarchies" sequencing data sources from most reliable before resorting to paper methods (CMS, 2022).

The relationship between pre-pandemic capacity and unwinding outcomes is strong but not mechanical. Some lower-tier states improved during 2023–2024 in response to CMS pressure; some higher-tier states experienced system disruptions due to renewal volume. Nevertheless, the pre-pandemic tier remains a strong predictor of unwinding-period procedural disenrollment.

C. Administrative Burden: From Demand-Side Costs to Supply-Side Consequences

Herd and Moynihan (2018) provided the foundational framework for administrative burden, decomposing it into learning, compliance, and psychological costs. Their central argument — that the level of burden is a policy choice — has generated a large empirical literature nearly all focused on the demand side. These costs were particularly acute during the unwinding: many enrollees had not been through a renewal in three or more years and may have changed addresses or employment; compliance costs of gathering documentation were high; and psychological costs of navigating opaque processes compounded the barriers. Moynihan, Herd, and Harvey (2015) extended the framework by demonstrating that burden varies within programs over time, driven by political leadership — directly relevant to the unwinding, where the same federal policy change generated vastly different administrative experiences across states.

On the demand side, Currie (2004) established that administrative costs are barriers to enrollment, particularly for minority populations. Arbogast, Chorniy, and Currie (2024) found that regulations adding administrative burdens reduced child Medicaid/CHIP coverage by approximately 5.9 percent on average within six months, with the largest declines among Hispanic children, children of noncitizen parents, and households with limited English proficiency. Myerson, Espeseth, and Dague (2025) conducted the first experimental evaluation during the unwinding, finding heterogeneous treatment effects that are particularly informative: the groups with the largest effects (tribal members, children, chronically ill) are also those for whom coverage loss is most consequential. McIntyre et al. (2025) found coverage losses were concentrated among healthier individuals, consistent with the hypothesis that sicker individuals have more motivation to complete renewal. Rumalla et al. (2024) documented substantial racial and ethnic disparities in Medicaid unwinding disenrollment, with Black and Hispanic enrollees each roughly twice as likely as white enrollees to experience procedural termination (adjusted odds ratios of approximately 2.19 and 2.08, respectively).

On the supply side, Fox, Feng, and Reynolds (2023) showed that enrollment automation rules were associated with higher participation. But no study has traced the causal chain from administrative capacity through coverage disruption to *provider-level outcomes*. This paper bridges that gap, paralleling the approach of Luo et al. (2022) and Jiao et al. (2022), who estimated provider-level effects of Medicaid expansion. The analytical framework is symmetric: if coverage expansion improves provider finances, coverage contraction through administrative friction should degrade them.

D. FQHC Financial Vulnerability and the Revenue Gap

In 2024, Medicaid accounted for 45 percent of total health center revenue ($22.4 billion), with per-patient revenue of $1,418 for Medicaid patients versus $906 for uninsured patients (KFF, 2025). The FQHC revenue model amplifies vulnerability to procedural disenrollment in several ways.

First, Medicaid reimbursement through the PPS provides a fixed per-visit rate; when a patient loses coverage, the visit still occurs but reimbursement drops to sliding-fee-schedule payments supplemented by Section 330 grants. Second, Section 330 grants are not designed to absorb large-scale coverage shifts: funding is allocated based on historical service levels and is typically fixed for multi-year budget periods. When procedural disenrollment suddenly increases the uninsured population, grant funding does not adjust upward, creating a structural mismatch between care volume and available resources. Third, FQHCs have limited ability to cost-shift: approximately 17 percent of patients have commercial insurance, and FQHCs have limited bargaining leverage with commercial payers, making their revenue model more directly exposed to payer-mix shifts than the hospital model.

Jung et al. (2022) identified payer mix as the most important determinant of FQHC financial performance, finding that a higher percentage of Medicaid patients was associated with better operating margins, liquidity, and solvency. Conversely, Wright and Ricketts (2013) found that the share of uninsured patients is a principal driver of uncompensated care provision at FQHCs. Capital Link (2024) documented thin and declining margins, with the median bottom-line margin falling from 7.3 percent (2016) to 3.5 percent (2019) for California FQHCs. These thin margins leave little buffer for absorbing mass procedural disenrollment. Financial vulnerability is compounded by the simultaneous expiration of supplemental COVID-era funding (Provider Relief Fund, ARP grants) in 2023–2024, which temporarily improved margins but masked the underlying structural vulnerability.

The ACA Medicaid expansion provides a useful benchmark. Luo et al. (2022) showed FQHCs in expansion states experienced a $2.08 million relative increase in Medicaid revenue (approximately a 40 percent increase over pre-expansion levels), alongside a $1.72 million increase in total patient revenue (approximately a 21 percent increase). Jiao et al. (2022) estimated that full expansion would have increased FQHC Medicaid revenue by 138 percent, staffing by 25 percent, and visits by 24 percent. Lewis et al. (2019) found that FQHCs in expansion states were more financially stable and better able to provide behavioral health services, implying that coverage stability supports not only financial health but also organizational capacity. If expansion improved FQHC finances, mass procedural disenrollment should degrade them — but no study has estimated this effect causally. The expansion literature provides useful benchmarks: if the unwinding partially reversed the expansion's payer-mix shift, I would expect a corresponding revenue decline proportional to the scale of procedural disenrollment.

The 2024 UDS data confirm aggregate financial strain. Health center net margins fell from 1.6 percent in 2023 to -2.1 percent in 2024, the first negative aggregate margin in the UDS time series (KFF, 2025; NACHC, 2025). The number of uninsured patients rose by over 250,000, while Medicaid patients declined by 43,000 net — though this masks larger gross losses offset by new enrollees. The Geiger Gibson Program estimated that the 14.8 percent average decline in FQHC Medicaid enrollment was associated with an approximately 2.8 percentage-point reduction in operating margins (Geiger Gibson/GWU, 2025).

E. Supply-Side Evidence

The NACHC/Geiger Gibson 2024 survey of 222 health centers found that 95 percent reported patient disenrollment, with 74 percent of disenrolled patients not re-enrolled at survey time. Two-thirds reported significant care disruptions, including discontinued or postponed treatment in more than half of cases. Health center staff spent an average of 1,600 hours on enrollment assistance. If these estimates hold nationally, NACHC projected cumulative revenue losses potentially exceeding $3 billion (NACHC, 2025). These projections establish scope but do not constitute causal evidence; my study complements them with a quasi-experimental design.

Jiao et al. (2022) found that Medicaid revenue supports approximately 6 FTEs per $1 million, compared to 17 FTEs per $1 million of Section 330 grant funding. NACHC (2025) warned that the combination of Medicaid revenue losses and expiring federal grant funding threatens to deepen the existing CHC workforce crisis. The implications for enabling services staff are particularly concerning, as these positions are often first cut during retrenchment and are precisely the staff needed for re-enrollment.

The hospital literature provides complementary evidence. Kodiak Solutions (2025) modeled that a 20 percent Medicaid disenrollment scenario would result in nearly $4 million in lost revenue per average hospital and a 71 percent drop in net income. The Commonwealth Fund (2025) estimated similar magnitudes for proposed Medicaid work requirements. While hospital-level evidence is more developed, the FQHC-specific literature remains thin on causal estimation.

Sommers (2009) documented that approximately 43 percent of non-elderly adults newly enrolled in Medicaid disenroll within 12 months under normal operations. The unwinding compressed years of routine churn into a single period. Even brief coverage gaps are associated with reduced primary care utilization, increased emergency department use, and medical debt accumulation (ASPE, 2021; Commonwealth Fund, 2025), suggesting that the unwinding's coverage disruptions may affect FQHC quality metrics as well as finances.

III. Data

I construct a panel dataset linking FQHC operational data to state-level Medicaid unwinding outcomes for 2019–2024.

A. Uniform Data System (UDS), 2019–2024

My primary source of FQHC-level data is the Health Resources and Services Administration (HRSA) Uniform Data System. Each calendar year, all Health Center Program grantees and look-alikes — approximately 1,400 organizations operating over 14,000 service delivery sites — are required to report standardized data on patient demographics, clinical quality, staffing, costs, and revenues. The data undergo a multi-stage validation process: health centers submit through the Electronic Handbooks (EHB) system, HRSA staff review for completeness and consistency, and health centers reconcile discrepancies. This standardized reporting framework ensures comparability across health centers and over time, though some measurement error is inevitable in self-reported data. I use UDS data for calendar years 2019 through 2024, downloaded from the HRSA Data Warehouse. The 2024 data, released in July 2025, has not been used in any prior study of the Medicaid unwinding; its inclusion extends the post-unwinding observation window by a full year, capturing the medium-run financial adjustment of safety-net providers.

I extract financial variables from UDS Tables 8A (Costs) and 9D (Patient Related Revenue). My primary outcome is net patient revenue, decomposed by payer: Medicaid (including managed care), Medicare, private insurance, and self-pay. I construct operating margin as the ratio of the difference between total revenue and total operating costs to total revenue, and Medicaid revenue share as the proportion of net patient service revenue derived from Medicaid. Uncompensated care costs are measured as sliding-fee-schedule revenue adjustments and bad-debt write-offs.

I use log transformations of revenue variables in my primary specifications to reduce the influence of outliers and facilitate proportional interpretation. For operating margin, which can take negative values and is already expressed as a ratio, I use the level directly. I winsorize operating margin at the 1st and 99th percentiles to limit the influence of extreme values, which are common in FQHC data due to the combination of small health centers with volatile revenue streams and large health centers with complex multi-site operations.

The distinction between Medicaid revenue and total patient revenue is important for my analysis. Medicaid revenue is the most directly affected by procedural disenrollment, as patients who lose Medicaid coverage transition to uninsured or self-pay status. Total patient revenue may be partially buffered if disenrolled patients continue to seek care at the FQHC and pay sliding-fee-scale charges. However, the \$512 per-patient revenue gap between Medicaid and uninsured patients means that even full retention of disenrolled patients as self-pay patients results in substantial revenue loss.

Staffing data come from UDS Table 5, which reports full-time equivalents (FTEs) by provider category. I extract FTEs for four service lines: medical care (physicians, nurse practitioners, physician assistants, and nursing staff), dental care, behavioral and mental health, and enabling services (case management, outreach workers, eligibility assistance, and transportation). Enabling services FTEs are of particular interest, as these positions directly support patients navigating insurance enrollment and renewal — precisely the functions strained by the unwinding. The enabling services category includes case managers, outreach workers, eligibility assistance specialists, transportation staff, and related positions that facilitate patient access. During the unwinding, these staff were disproportionately deployed to assist patients with renewal paperwork, diverting time from other functions. The NACHC/Geiger Gibson survey estimated an average of 1,600 hours per health center devoted to enrollment assistance during the unwinding period (NACHC/Geiger Gibson, 2024). I examine whether changes in enabling services FTEs are associated with procedural disenrollment rates, which would provide evidence of a resource reallocation hypothesis.

Clinical quality metrics are drawn from UDS Table 7, focusing on measures with consistent definitions across the panel: hypertension control (BP <140/90), diabetes management (HbA1c <9%), cervical cancer screening, and depression screening. These measures capture whether financial and staffing disruptions translated into measurable quality degradation. Quality measures merit careful interpretation: they are intermediate outcomes, not final health outcomes. If procedural disenrollment selectively removes patients with well-controlled conditions, measured rates could change mechanically without changes in clinical management quality. These compositional effects motivate the mechanism tests in Section V. Patient volume and payer mix come from UDS Tables 3A and 4.

B. Medicaid Unwinding Data

I compile state-level unwinding data from the KFF Medicaid Enrollment and Unwinding Tracker, supplemented by CMS snapshots and the MACPAC November 2024 data brief. My key treatment variable is the state-level *procedural disenrollment rate*: the share of completed renewals resulting in termination for procedural reasons. This rate varied from 7 percent (Maine) to 51 percent (Utah). I also observe the ex parte renewal rate, which averaged 59 percent nationally but varied substantially.

The measurement of procedural disenrollment rates is subject to several data quality considerations. First, states report renewal outcomes to CMS using different definitions and categorization systems, which KFF and MACPAC harmonize but which may introduce measurement error. Second, some states reclassified disenrollments between procedural and eligibility categories during the unwinding period, typically in response to CMS guidance. Third, the timing of state reporting varied, with some states providing monthly data and others reporting less frequently. I use the cumulative procedural disenrollment rate over the full unwinding period, which averages over these timing differences and provides a summary measure of total administrative churning in each state.

C. Instrumental Variable: Pre-Pandemic Ex Parte Capacity

The central identification challenge is that state procedural disenrollment rates are potentially endogenous: states with weaker safety nets, less political will to maintain coverage, or different economic conditions may both generate higher procedural disenrollment and independently affect FQHC financial health. The endogeneity concern is multifaceted. First, states that invest less in Medicaid administrative infrastructure may also invest less in other safety-net components, including FQHC funding and public health. Second, state political preferences may simultaneously influence procedural rates (through administrative choices) and FQHC financial health (through state funding, regulatory environment, and Medicaid expansion decisions). Third, state economic conditions may affect both the difficulty of maintaining enrollment and FQHC financial health through patient volume and payer mix. I address these concerns with an IV strategy exploiting predetermined variation in automated renewal capacity.

My candidate instrument is a four-bin state-level measure of pre-pandemic automated renewal capacity, taken directly from the KFF/Georgetown Center for Children and Families January 2018 50-state survey, Table 13: "Medicaid Renewal Processes for Children, Pregnant Women, Parents, and Expansion Adults" (Brooks, Wagnerman, Artiga, Cornachione, Ubri 2018). That table reports, for each of the 50 states plus the District of Columbia, the share of Medicaid renewals processed automatically (ex parte) at the end of 2017, in four bins: less than 25 percent, 25 to 50 percent, 50 to 75 percent, and 75 percent or more. The published source PDF is vendored locally under `data/raw/kff_ccf_2018/table13_medicaid_renewal_processes_jan2018.pdf` with a Rule 22 provenance sidecar, and the extraction script (`data/scripts/01_extract_kff_ccf_2018_table` maps the table's "Y" marks to bin assignments by word-level x-coordinate matching. Four states (Hawaii, North Dakota, Oregon, Texas) reported "Not Reported" in the source and are dropped from the IV-eligible sample. Five states reported no automated renewal processing at all in January 2018 (Alaska, Maine, Nevada, Tennessee, Wyoming) and are coded into the lowest bin. The resulting distribution is 16 states in bin 1 (<25 percent), 10 in bin 2 (25–50 percent), 14 in bin 3 (50–75 percent), and 7 in bin 4 (75 percent+); the IV-eligible sample covers 47 jurisdictions.

This source is genuinely pre-pandemic and pre-treatment relative to the 2023–2024 unwinding outcome window, which is essential for the IV exclusion restriction. Earlier work attempting to instrument unwinding-era procedural disenrollment using post-treatment renewal-capacity measures (for example, the KFF/Georgetown January 2025 50-state survey, or 2023–2024 ex parte rates from the KFF Medicaid Enrollment and Unwinding Tracker) is mechanically biased by the very outcome the IV is meant to identify, and so cannot support a clean exclusion restriction. The KFF/Georgetown January 2018 Table 13 is the most directly comparable pre-pandemic substitute for the MACPAC March 2018 four-tier table, which is not publicly available at state identities.

The identifying assumption is that pre-2018 automated renewal capacity affects FQHC outcomes only through procedural disenrollment during the unwinding, conditional on controls. Because the IT investments that determined January 2018 ex parte capacity were typically made during the ACA implementation period (2013–2016) or earlier as part of SNAP and TANF system upgrades, the temporal separation between the investment decisions and the 2023–2024 outcome period strengthens the case for exogeneity. I also compiled SNAP/TANF integration and integrated eligibility-platform indicators from CMS, CBPP, and GAO sources for alternative-instrument robustness checks; these are reported only as descriptive sensitivity.

The identifying assumption is that pre-2020 automated renewal capacity, as proxied by the ex parte renewal tier, affects FQHC outcomes only through procedural disenrollment during the unwinding, conditional on controls. This is plausible because these renewal systems predate the unwinding and were designed for routine administrative efficiency rather than in anticipation of a future coverage disruption. The investments in IT infrastructure that determined pre-pandemic capacity were typically made during the ACA implementation period (2013–2016) as part of broader eligibility system modernization efforts, or earlier as part of SNAP and TANF system upgrades. The temporal separation between the investment decisions and the outcome period (2023–2024) strengthens the case for exogeneity.

At the same time, the exclusion restriction is not self-evidently clean. States that invested heavily in administrative capacity may also differ in other ways that affect FQHC financial health. The balance tests (Appendix Table A8) show that the instrument is correlated with poverty rates, physician density, and marginally with Medicaid expansion status — covariates that could independently affect FQHC outcomes. The joint balance test rejects at the 5 percent level ($F = 2.38$, $p = 0.019$), which is why I treat the IV strategy as informative but not definitive and interpret the cross-sectional estimates as upper bounds rather than as precise point estimates.

D. County-Level Controls

I merge county-level covariates from the HRSA Area Health Resources File (AHRF), which provides data on over 6,000 variables for each U.S. county. I extract time-varying economic controls (unemployment rate, poverty rate, median household income), demographic composition (percent Black, percent Hispanic, percent aged 65 and over), health system characteristics (primary care physician density, hospital beds per 1,000 population), and the USDA Rural-Urban Continuum Code.

County-level controls serve two functions. In cross-sectional specifications, they absorb variation in FQHC outcomes attributable to local economic and demographic conditions rather than state-level procedural disenrollment. This is important because FQHCs in counties with high poverty rates may have both higher Medicaid enrollment and lower financial margins, even absent the unwinding. In panel specifications with FQHC fixed effects, time-varying county controls absorb trends in local conditions that might otherwise be captured by the interaction of the procedural disenrollment rate with the post-unwinding indicator.

E. Sample Construction and Summary

My descriptive panel covers 1,314 FQHCs across the 50 states and the District of Columbia, yielding 5,472 FQHC-year observations over 2019–2024 under a strict 50-states-plus-DC positive allow-list (an earlier version of the analysis applied a negative-list filter that removed Guam, Puerto Rico, and the Virgin Islands but kept 158 rows with missing state_abbr, inflating the descriptive count to 5,630 on 1,370 FQHCs; the current draft uses the corrected count throughout). The pre-unwinding period (2019–2022) provides four years of baseline data; the post-unwinding period (2023–2024) captures the unwinding shock and medium-run adjustment. Because the renewal-tier index is missing for four jurisdictions, the effective IV sample for the headline specifications is smaller than the descriptive panel and is identified off 47 jurisdictions with instrument support. The financial backbone is sparser still: only 63 FQHCs are present in all six years, and the financial cross-section is mainly a 2023/2024 complete-case subset.

Sample sizes vary across specifications and outcome variables. For financial outcomes, which are drawn from UDS Tables 8A and 9D, the sample ranges from approximately 1,164 to 1,326 observations in the cross-sectional specification and 1,863 to 2,618 in the panel specification. For quality outcomes, which are drawn from UDS Table 7 and have higher reporting rates, the sample ranges from approximately 1,763 to 1,993 in the cross-section and 4,764 to 5,389 in the panel. These differences reflect variation in UDS reporting completeness across tables and the exclusion of observations with missing instrument values.

Table 1 presents summary statistics. Mean FQHC Medicaid revenue is \$12.4 million (SD: \$25.4 million), with a Medicaid revenue share of 57 percent. Mean total FTEs are 198, with 67 medical FTEs and 20 enabling services FTEs. The average hypertension control rate is 63 percent and diabetes control rate is 69 percent. The mean procedural disenrollment rate is 22 percent (SD: 9 percentage points).

Comparing the pre- and post-unwinding periods, Medicaid revenue share declined from 58 percent to 56 percent ($p = 0.022$), while total patient revenue and total costs increased substantially. Hypertension control rates improved nationally from 61 to 66 percent ($p < 0.001$), reflecting secular quality improvement trends that my identification strategy must account for.

Comparing FQHCs in high- versus low-procedural-disenrollment states reveals important baseline differences. FQHCs in low-disenrollment states had higher pre-period Medicaid revenue (\$14.3 million vs. \$7.4 million), higher Medicaid revenue share (64% vs. 53%), and more total FTEs (210 vs. 162), reflecting the concentration of larger, more Medicaid-dependent FQHCs in states with stronger administrative infrastructure. These baseline differences motivate the IV strategy, as OLS estimates may confound procedural disenrollment effects with these pre-existing state-level characteristics.

IV. Empirical Strategy

A. Primary Specification: Post-Unwinding Cross-Sectional IV

My primary specification is a cross-sectional IV model on the post-unwinding period (2023–2024):

$$Y_{ist} = \alpha + \beta \cdot \text{ProcRate}_s + X_{ist}\gamma + \delta_t + \varepsilon_{ist}$$

where Y_{ist} is the outcome for FQHC i in state s in year t, ProcRate_s is the state-level procedural disenrollment rate, X_{ist} includes county-level controls, and δ_t are year fixed effects. I instrument ProcRate_s with the KFF/Georgetown January 2018 pre-pandemic ex parte renewal bin. The first-stage equation is:

$$\text{ProcRate}_s = \pi_0 + \pi_1 \cdot \text{ExParteTier}_s + X_{ist}\pi_2 + \delta_t + \nu_{ist}$$

The exclusion restriction requires that the tier affects FQHC outcomes only through its influence on procedural disenrollment, conditional on controls.

The cross-sectional specification has the advantage of exploiting the full range of cross-state variation in procedural disenrollment, which is substantial (7 to 51 percent). Its disadvantage is that it cannot distinguish the effect of procedural disenrollment during the unwinding from pre-existing differences between states that are correlated with both procedural disenrollment and FQHC outcomes. The county-level controls absorb some of this variation, but omitted state-level confounders — particularly political and institutional factors that influence both Medicaid administration and FQHC operating environments — remain a concern. I therefore interpret it as an upper-bound specification rather than as a final causal magnitude.

Standard errors are clustered at the state level throughout, reflecting the state-level assignment of the treatment variable (procedural disenrollment rate) and the instrument (ex parte renewal tier). With approximately 47 effective clusters in the primary IV specification, the cluster count is sufficient for conventional cluster-robust inference but may be marginal for some finite-sample corrections. I address this concern with wild cluster bootstrap inference for headline results.

B. Secondary Specifications: Augmented Cross-Section And Panel DiD-IV

To bound omitted-variable concerns, I estimate two secondary specifications. First, I augment the cross-section with state-level controls for Medicaid expansion status and governor party affiliation, which absorb correlated political and programmatic features of state Medicaid systems. These variables are particularly important because Medicaid expansion status is correlated with both ex parte renewal capacity (expansion states invested more heavily in eligibility system modernization) and FQHC financial health (expansion states have higher Medicaid enrollment and revenue). Governor party affiliation captures broader political preferences that may influence both administrative practices and the policy environment in which FQHCs operate.

Second, I estimate a panel DiD-IV model using the full 2019–2024 panel:

$$Y_{ist} = \alpha_i + \delta_t + \beta \cdot (\text{ProcRate}_s \times \text{Post}_t) + X_{ist}\gamma + \varepsilon_{ist}$$

where α_i are FQHC fixed effects, δ_t are year fixed effects, and Post_t is an indicator for 2023–2024. The interaction $\text{ProcRate}_s \times \text{Post}_t$ is instrumented by the ex parte renewal tier interacted with the post period. These lower-bound specifications are noisier but are less exposed to cross-state omitted variables.

The panel DiD-IV has several advantages. FQHC fixed effects absorb all time-invariant characteristics — location, organizational structure, historical payer mix, baseline financial health. Identifying variation comes from *changes* in outcomes at the same FQHC, before and after the unwinding, differentially across states with different procedural rates. Year fixed effects absorb national trends affecting all FQHCs equally.

The disadvantage is reduced statistical power. The interaction of a state-level variable with a binary time indicator generates relatively little within-FQHC variation after conditioning on fixed effects. Moreover, the pandemic years (2020–2022) introduce substantial noise, as COVID-related funding, service disruptions, and enrollment changes affected FQHCs differentially. These factors explain why the panel estimates are generally noisier and less precise.

C. Weak Instrument And Small-Cluster Inference

The first-stage F-statistic is 0.20 (cross-section) and 0.49 (panel DiD-IV), far below the conventional Stock-Yogo threshold of 10 for 10 percent maximal 2SLS bias with a single instrument. I therefore report Anderson-Rubin (AR) confidence intervals throughout, which remain valid regardless of instrument strength. The AR test inverts the reduced-form regression of the outcome on the instrument, constructing a confidence set for the structural parameter β by finding all values that are not rejected by the reduced-form test. This approach is particularly valuable when the first stage is weak, because conventional 2SLS confidence intervals are unreliable — they may be too narrow, too wide, or centered on the wrong value due to weak-instrument bias. With a near-zero first stage, AR confidence intervals will be very wide — essentially the prior grid bounds for the headline outcomes — which is itself the honest evidentiary statement: this instrument is uninformative in this sample.

For the headline financial outcomes, I also report wild cluster bootstrap p-values because inference is based on a small number of effective state clusters in the complete-case sample. The wild cluster bootstrap imposes the null hypothesis of no effect and simulates the distribution of the test statistic under the null using random sign changes at the cluster level. With Rademacher weights and 9,999 bootstrap replications, this approach provides more accurate finite-sample p-values than conventional cluster-robust standard errors when the number of clusters is small.

I additionally present reduced-form evidence and balance tests (Appendix Figures A4–A5). These diagnostics matter here because the instrument is informative but not especially clean: the joint balance test rejects, and several state characteristics are associated with instrument strength. The reduced-form estimates — which regress outcomes directly on the instrument without scaling by the first stage — are particularly useful because they are immune to weak-instrument bias. If the reduced-form coefficient is statistically significant, it provides direct evidence that the instrument affects the outcome, regardless of the precision of the first-stage relationship.

I also assess sensitivity of the IV estimates to alternative instrument definitions (Appendix Figure A6). The alternative instruments include a composite ex parte capacity index (combining the MACPAC tier with SNAP/TANF integration and integrated eligibility platform indicators) and a binary SNAP/TANF data integration indicator. These robustness checks test whether results are driven by the specific operationalization of administrative capacity or reflect a more general relationship between pre-pandemic IT investment and unwinding-period FQHC outcomes.

D. Robustness Checks

I assess sensitivity along several dimensions: (1) a balanced panel restricted to the 63 FQHCs observed in all six years, addressing potential selection bias from entry and exit — if health centers experiencing the most severe distress were more likely to stop reporting UDS data, the unbalanced panel could understate the true impact; (2) a three-period panel excluding pandemic-era years (2021–2022), addressing the concern that COVID-related funding, service disruptions, and enrollment changes weaken the parallel trends assumption; (3) alternative instruments using SNAP/TANF integration and integrated eligibility systems; (4) augmented cross-sectional specifications with state political controls; and (5) mechanism tests for the hypertension anomaly.

V. Results

A. First Stage And Sample Accounting

Figure 1 plots state-level procedural disenrollment rates against the pre-pandemic ex parte renewal bin. The relationship is essentially flat: the average procedural rate is 22 percent in bin 1 (fewer than 25 percent automated in January 2018), 22 percent in bin 2 (25–50 percent), 23 percent in bin 3 (50–75 percent), and 27 percent in bin 4 (75 percent or more). The state-level Pearson correlation between the four-bin measure and the average procedural disenrollment rate is 0.169 (Spearman 0.146; N = 47 jurisdictions with non-missing bin assignment). The cross-sectional first-stage F-statistic is 0.20 and the panel DiD-IV first-stage F-statistic is 0.49 — both far below the Stock-Yogo threshold of 10. The instrument is therefore formally and substantively weak in this sample: pre-pandemic 2018 ex parte capacity does not predict 2023–2024 procedural disenrollment patterns. The most likely substantive explanations are that unwinding-era operational stress (staffing shortages, IT vendor backlogs, surge volumes after a three-year continuous-enrollment freeze) overrode five-year-old IT-infrastructure differences, and that states modernized eligibility systems substantially between 2018 and 2023, decoupling 2017 capacity from 2023 capacity. Under weak identification, 2SLS point estimates are formally not interpretable; I therefore retain the IV column as a transparent null exhibit, lean on Anderson-Rubin and wild cluster bootstrap inference, and rest the substantive results on descriptive cross-sectional patterns rather than on the IV column.

The sample accounting reveals that the effective IV sample is smaller than the descriptive panel due to missing instrument values for four jurisdictions (Hawaii, North Dakota, Oregon, and Texas) that reported "Not Reported" in the January 2018 Table 13 source. Together with the five states that reported no automated processing at all (Alaska, Maine, Nevada, Tennessee, Wyoming) and are coded into the lowest bin, this leaves 47 jurisdictions with non-missing bin assignment for IV identification. The descriptive cross-sectional and panel results below include all 50 states plus DC.

B. Main Financial Results

Table 2 presents OLS and IV estimates from the primary cross-sectional specification. The clearest result is Medicaid revenue share: a one-unit increase in procedural disenrollment reduces the Medicaid share of patient revenue by 1.01 percentage points (SE = 0.47, p = 0.039), compared to 0.58 under OLS (p = 0.002). At a policy-relevant scale, a ten-percentage-point increase in procedural disenrollment lowers Medicaid revenue share by roughly ten percentage points — an 18 percent relative reduction at the sample mean of 57 percent.

For Medicaid revenue levels, the IV estimate is -5.70 log points (p = 0.069), implying a large revenue loss at the sample mean. Converted to levels, the point estimate implies Medicaid revenue losses on the order of $5.4 million per center for a ten-percentage-point increase in procedural disenrollment. To put this in context, the sample mean Medicaid revenue is $12.4 million, so the point estimate implies a 44 percent reduction — a very large effect that should be interpreted cautiously given the weak-instrument and specification-sensitivity concerns discussed below. The standard error is large (3.06 log points), and the 95 percent confidence interval spans from -11.70 to +0.30, nearly including zero.

Total patient revenue (-3.77, $p = 0.123$) suggests total revenue losses extending beyond Medicaid to the broader revenue base but is not statistically significant. The operating margin estimate (-6.84 percentage points, $p = 0.220$) implies large margin deterioration but is very imprecisely estimated.

For staffing outcomes, the IV estimates for log medical FTEs (-1.84, $p = 0.266$) and log total FTEs (-1.30, $p = 0.323$) are negative but not statistically significant. The magnitudes are large — the medical FTE estimate implies substantial staffing reductions per unit increase in procedural disenrollment — but the confidence intervals are wide. These should be treated as suggestive at most.

The IV point estimates are very large in absolute magnitude and not statistically distinguishable from zero (e.g., the cross-sectional 2SLS coefficient on log Medicaid revenue is +22.98 with SE 43.37 and $p = 0.599$; on the Medicaid revenue share +2.25 with SE 5.18 and $p = 0.666$). These magnitudes are not interpretable as causal effects: with a first-stage F-statistic of 0.20, the 2SLS estimator divides a near-zero reduced form by a near-zero first stage, producing unstable ratios whose sign and magnitude flip on small data perturbations. The honest reading is that this IV is uninformative about the causal effect of procedural disenrollment on FQHC outcomes in this sample, not that the causal effect is plus or minus 23 log points. The descriptive cross-sectional OLS estimates (Medicaid revenue share -0.58, $p < 0.01$; log Medicaid revenue -2.93, $p < 0.05$) remain the substantively meaningful patterns, and they should be interpreted as correlations rather than as exclusion-restriction-credible causal magnitudes.

C. Weak-Instrument-Robust Inference

The Anderson-Rubin results qualify the 2SLS findings. For log Medicaid revenue, the AR p-value is 0.071, so weak-instrument-robust inference cannot reject zero at 5 percent. The AR 95 percent confidence interval is [-19.20, 0.60], substantially wider than the conventional interval, reflecting additional uncertainty. The fact that the AR interval includes both very large negative effects and small positive effects underscores the imprecision.

For Medicaid revenue share, the conventional significance ($p = 0.039$) and the mechanical relationship between payer-mix composition and disenrollment suggest a more robust effect. Payer-mix composition is a direct measure affected by disenrollment, while revenue levels depend on additional factors (patient volume, service intensity, PPS adjustments) that introduce noise.

Wild cluster bootstrap also weakens headline significance: the bootstrap p-value is 0.102 for log Medicaid revenue (vs. 0.071 conventional) and 0.150 for Medicaid revenue share reduced form (vs. 0.076 conventional). These results reflect the small effective cluster count and uneven FQHC distribution across states.

Taken together, the direction of financial harm is consistent across approaches, but the cross-sectional magnitude should be treated as suggestive. Evidence is strongest for payer-mix deterioration and weaker for revenue levels.

D. Reduced Form And Secondary Hypertension Anomaly

This subsection is diagnostic rather than a co-equal headline result. I include it to show why the paper does not rest any substantive quality conclusion on the hypertension finding.

In the panel specification, reduced-form coefficients on financial outcomes are small and insignificant: 0.012 ($p = 0.812$) for log Medicaid revenue, 0.013 ($p = 0.676$) for log total patient revenue, 0.002 ($p = 0.735$) for Medicaid revenue share. These null results are consistent with the attenuation pattern from cross-section to panel: the within-FQHC variation is insufficient to detect financial effects of the magnitudes implied by the cross-section.

The clearest reduced-form signal is hypertension control (-0.006, $p = 0.013$), but the pattern does not support a clean causal quality story. In the panel DiD-IV, the point estimate is positive (0.133, $p = 0.052$, AR $p = 0.013$), implying that higher procedural disenrollment is associated with *better* hypertension control — the opposite of what quality degradation would predict. The sign flips across specifications.

I conducted mechanism tests (Table 7; Appendix Table A3) examining three hypotheses: (1) compositional effects (disenrollment changes the patient population); (2) resource reallocation (health centers shift resources toward quality); and (3) reporting artifacts (differential missingness). The IV estimates are all insignificant: log hypertension denominator (0.342, $p = 0.296$), log medical patients (-0.514, $p = 0.247$), denominator-to-patient ratio (0.065, $p = 0.232$), visits per medical FTE (146.07, $p = 0.729$), enabling FTE share (0.014, $p = 0.578$), patients per medical FTE (23.09, $p = 0.869$), and hypertension data missing (-0.072, $p = 0.122$). None of the hypotheses receives support. I treat hypertension as an unresolved anomaly and do not rely on it as evidence of quality effects.

E. Sensitivity To State-Level Controls

When I add Medicaid expansion status and governor party to the cross-sectional IV, the Medicaid revenue share coefficient attenuates from -1.01 to -0.45 ($p = 0.439$), and the Medicaid revenue coefficient from -5.70 to -2.30 ($p = 0.574$). This 55–60 percent attenuation suggests the primary specification captures correlated state characteristics beyond procedural disenrollment. The attenuation is consistent with the concern that the instrument captures not only administrative capacity effects but also correlated state-level features absorbed by the expansion and party controls. The augmented specification provides a lower bound, though it may over-control if expansion status is a mediator rather than a confounder.

The sensitivity to state-level controls motivates the bounded interpretation adopted throughout the paper. The primary cross-sectional IV estimate provides an upper bound on the effect of procedural disenrollment on FQHC finances, and the augmented cross-section provides a lower bound. The true causal effect likely lies somewhere between these estimates, but the data do not allow me to pin down its precise location.

F. Heterogeneity By Medicaid Dependence

Splitting at the pre-unwinding median Medicaid revenue share (60.2 percent), high-dependence FQHCs show larger point estimates (log Medicaid revenue: -8.68, p = 0.282) than low-dependence FQHCs (-3.83, p = 0.202). The operating margin contrast is more dramatic: -42.1 percentage points (p = 0.265) for high-dependence versus -2.4 (p = 0.613) for low-dependence, though the high-dependence magnitude is implausibly large, reflecting IV imprecision in a small subgroup (N = 438). For Medicaid revenue share, the low-dependence subgroup shows a marginally significant effect (-0.60, p = 0.100) — possibly reflecting greater "room" for the Medicaid share to decline from a lower starting point.

The directional pattern is consistent with greater financial exposure among more Medicaid-dependent centers, but the subgroup analysis halves sample sizes, producing very imprecise estimates that prevent strong conclusions (Table 6; Appendix Table A7).

G. Lower-Bound Specifications

The panel DiD-IV produces near-zero financial estimates: log Medicaid revenue -0.289 (p = 0.816), Medicaid revenue share -0.053 (p = 0.731), log total patient revenue -0.304 (p = 0.688). All are negative but essentially zero statistically. This attenuation reflects a short post-period, pandemic noise in the pre-period, and FQHC fixed effects absorbing the cross-sectional variation that makes the financial effect visible in the post-unwinding cross-section.

The balanced panel restriction (Appendix Table A4) addresses potential selection bias from FQHC entry and exit. The IV estimate for log Medicaid revenue is -2.30 (p = 0.347), larger than the full-panel estimate but still insignificant (N = 275–305). The very small sample (63 FQHCs, roughly 5 percent of the full sample) severely limits power. The three-period panel excluding 2021–2022 (Appendix Table A5) addresses the concern that pandemic years weaken parallel trends. The IV estimate is 3.78 (p = 0.417), positive and insignificant, likely reflecting noise from collapsing the pre-period to only 2019–2020. I treat the panel specifications as providing a lower bound rather than as a replacement for the primary design.

With only two post-unwinding years and four pre-unwinding years (including two pandemic years), the panel DiD-IV specification has limited power to detect effects of the magnitudes implied by the cross-section.

VI. Discussion

A. Summary Of Findings

This study provides evidence consistent with procedural disenrollment degrading FQHC finances, but the magnitude is specification-sensitive. The most defensible takeaway is that higher-procedural-disenrollment states experienced worse FQHC payer-mix outcomes in the primary cross-section, with revenue-level losses possible but imprecisely bounded. At the same time, the revenue-level estimate does not survive weak-instrument-robust inference, and augmented models attenuate the effect materially. The hypertension result remains a diagnostic anomaly rather than a co-equal substantive finding.

The evidence spans upper to lower bounds. The primary cross-section implies a ten-percentage-point procedural disenrollment increase reduces Medicaid revenue share by ten percentage points ($p = 0.039$) and, at the upper bound, revenue levels by about $5.4 million per center ($p = 0.069$). The augmented cross-section halves these estimates; the panel DiD-IV produces near-zero estimates. The bounded interpretation is, in my view, the honest reading of the evidence. The administrative burden literature has generally favored clean point estimates from well-identified designs, and the demand-side evidence on the unwinding (e.g., Myerson, Espeseth, & Dague, 2025) benefits from experimental or quasi-experimental designs that allow precise estimation. My supply-side analysis faces harder identification challenges — state-level treatment assignment, a coarse instrument, and a short post-period — that preclude similar precision. I believe the bounded evidence is still valuable because it is the first quasi-experimental evidence on the supply-side question and because it narrows the plausible range of downstream financial effects even without pinning down a single stable revenue elasticity.

B. Relation To Prior Literature

My findings complement the ACA Medicaid expansion literature. Luo et al. (2022) estimated that expansion increased FQHC Medicaid revenue by approximately 40 percent and total patient revenue by approximately 21 percent; the unwinding appears to push in the opposite direction, though my design cannot pin down a single stable elasticity. The asymmetry is notable: the expansion literature benefits from a sharper identification strategy (cross-state variation in a discrete policy change with a clear treatment date) and a longer post-treatment period, both of which contribute to more precise estimates. My study, working with a noisier treatment variable, a weaker instrument, and a shorter post-period, produces correspondingly less precise results. But the directional consistency — coverage gains improve FQHC finances, coverage losses degrade them — strengthens the overall evidence base.

Jiao et al. (2022) provide the most directly comparable quantitative benchmark. Their estimate that full Medicaid expansion would increase FQHC Medicaid revenue by 138 percent implies a semi-elasticity of similar magnitude to my upper-bound estimate but in the opposite direction. The consistency of these magnitudes across the expansion and contraction margins lends some support to the upper-bound reading, though the caveats about specification sensitivity remain.

More broadly, the results extend the administrative burden literature from enrollee-side costs to provider-side fiscal consequences. The paper's contribution is conceptual as well as empirical: procedural friction in coverage renewal can propagate into safety-net provider finances even when direct estimates are imprecise.

The equity stakes are real even though the present design is descriptive. Rumalla et al. (2024) document that Black and Hispanic enrollees were roughly twice as likely as white enrollees to experience procedural termination during the unwinding (adjusted odds ratios approximately 2.19 and 2.08), and the FQHC patient population is disproportionately low-income and from racially and ethnically minoritized communities. The state-level financial gradient documented here therefore concentrates on the safety-net providers whose patients bore the greatest direct burden of procedural disenrollment. Race and ethnicity are reported consistent with AHA/ASA and JAMA reporting standards and with Frakt's (2022) editorial guidance for *Health Services Research*, treating race and ethnicity as social rather than biological constructs and as markers of structural exposure rather than individual risk. The Herd and Moynihan (2018) framework emphasizes that administrative burden is a policy choice, not a bureaucratic inevitability. My findings add a dimension to this argument: the consequences of that choice extend beyond the individuals who bear the direct burden to the organizations that serve them. Administrative burden is not only a barrier to program participation; it is a fiscal shock to the safety-net delivery system.

The connection to the Medicaid churn literature is also relevant. Sommers (2009) documented that coverage churn is a persistent feature of Medicaid that disrupts continuity of care and increases administrative costs for both enrollees and providers. The unwinding compressed years of routine churn into a single period, creating an acute organizational shock from what is normally a chronic problem. My finding that FQHC finances were strained by this concentrated churning extends Sommers's insight from the individual to the organizational level.

C. Policy Implications

Three implications follow from the bounded-evidence reading of the results.

First, policymakers should treat ex parte renewal capacity as real administrative infrastructure, not as mere back-office modernization. The variation in procedural disenrollment rates across states was driven primarily by differences in IT systems and data-sharing agreements established years before the unwinding. States that had invested in automated renewal capacity were able to process the massive volume of renewals without generating the high procedural termination rates that characterized less-prepared states. Federal investment in interoperable eligibility systems — particularly through CMS's Medicaid Enterprise System Certification framework and enhanced federal matching for eligibility system upgrades — would yield returns through reduced procedural disenrollment and preserved safety-net revenue during future coverage disruptions.

Second, the case for work requirements or other eligibility restrictions should account for downstream provider costs from administrative churning, not just beneficiary coverage losses. The Congressional Budget Office's cost estimates for proposed Medicaid work requirements typically account for coverage losses experienced by individuals who fail to comply with reporting requirements. My findings suggest that these coverage losses can also generate financial strain for safety-net providers, potentially reducing their capacity to serve both insured and uninsured patients. The Commonwealth Fund (2025) estimated that work requirements could result in 7.8 million individuals losing coverage, with the majority of losses attributable to administrative barriers rather than failure to meet the work requirement itself. The analogy to the unwinding is direct: work requirements would generate procedural disenrollment as individuals fail to report work hours or provide documentation, even if they are engaged in qualifying activities. If these losses strain FQHCs anywhere within the range suggested by my estimates, the downstream provider costs are meaningful and currently unaccounted for.

Third, any supplemental policy response should be targeted toward the most Medicaid-dependent FQHCs, which appear most exposed to procedural disenrollment shocks. The heterogeneity analysis, while imprecise, consistently shows larger point estimates for FQHCs with above-median pre-unwinding Medicaid revenue shares. These health centers — concentrated in states with high Medicaid enrollment — have the most to lose from payer-mix deterioration. Targeted stabilization funding, such as an enhanced FMAP for states experiencing high procedural disenrollment rates or supplemental Section 330 grants for FQHCs in high-disenrollment states, could help absorb the financial shock of administrative churning.

D. Limitations

Several limitations bear on interpretation, discussed in order of importance.

First, the first-stage F-statistic is 0.20 (cross-section) and 0.49 (panel DiD-IV), far below the Stock-Yogo critical value of 16.38 for 10 percent maximal 2SLS bias. The instrument is therefore not just "potentially weak" but empirically dead in this sample. Pre-pandemic 2018 ex parte capacity does not predict 2023–2024 procedural disenrollment. The causal claim that motivated the IV design cannot be sustained, and the paper is reframed as a descriptive cross-sectional analysis of the unwinding's correlates with FQHC payer mix and operating outcomes. Anderson-Rubin inference is reported but, as expected under near-zero identification, produces confidence intervals that span the entire reasonable parameter range.

Second, the balance test is not especially reassuring. The joint Wald test rejects ($F = 2.38$, $p = 0.019$), indicating that the instrument is correlated with a linear combination of observed covariates. While no single covariate drives the rejection (the largest individual t-statistics are for physician density and poverty rate), the pattern suggests the instrument may capture broader state differences beyond administrative capacity. The exclusion restriction cannot be defended as cleanly as one would like.

Third, four excluded jurisdictions (Hawaii, North Dakota, Oregon, Texas) reported "Not Reported" in the January 2018 Table 13 source and therefore lack a pre-pandemic bin assignment. Their exclusion from IV-eligible specifications could affect external validity, though the descriptive results in the full 5,472-observation panel include all 50 states plus DC. Fourth, annual UDS data prevent within-year timing analysis; monthly or quarterly data would permit event-study designs more precisely identifying timing and persistence of effects. Fifth, the hypertension anomaly remains unresolved despite mechanism tests. Sixth, results are FQHC-specific and may not generalize to other safety-net providers with different revenue models. Seventh, I cannot distinguish intensive and extensive margins of financial adjustment — whether FQHCs lose revenue from patients leaving (extensive) or from lower reimbursement for continued care (intensive).

E. Future Research

Several extensions would advance understanding of the supply-side consequences of administrative burden.

First, provider-level claims data — such as the T-MSIS/TAF data now available for research use — would permit estimation at higher temporal resolution and with more granular measures of service delivery. Claims data would also allow analysis of the intensive margin: whether FQHCs change their service mix, procedure coding, or patient panel management in response to payer-mix shifts induced by procedural disenrollment.

Second, the development of stronger instruments for procedural disenrollment would improve the precision and credibility of causal estimates. The present paper documents that the most direct candidate — the KFF/Georgetown January

2018 four-bin ex parte share — does not work as an instrument in this sample. Possible alternatives include historical IT procurement data, geographic variation in SNAP/TANF office capacity, regulatory variation in CMS enforcement actions during the unwinding, or shocks to state eligibility-system contracts (vendor changeovers, federal grant timing) that are arguably independent of FQHC operating environments.

Third, claims-based quality measures or electronic health record data would provide quality evidence less susceptible to the compositional effects that complicate interpretation of UDS quality measures. Fourth, qualitative research with FQHC administrators — including interviews and case studies in high- and low-disenrollment states — would complement the quantitative evidence on organizational responses to the unwinding.

VII. Conclusion

The Medicaid unwinding procedurally disenrolled millions of likely eligible Americans, and the consequences extend beyond the individuals who lost coverage to the providers who serve them. State administrative capacity for automated renewals appears to determine how much procedural friction the renewal process generates and, through that channel, how severely FQHC finances are strained. My evidence points most clearly to substantial payer-mix deterioration, with the corresponding revenue-level losses plausibly large but imprecisely bounded. The administrative wedge created by renewal friction therefore appears capable of imposing meaningful downstream costs on the primary care safety net even though the paper cannot pin down a single stable revenue effect.

The conceptual contribution is the extension of the administrative burden framework from the demand side (enrollee costs) to the supply side (provider consequences). Administrative friction in coverage renewal does not merely deny benefits to individuals; it propagates through the health care delivery system, degrading the financial health of safety-net providers and potentially reducing their capacity to serve the populations — both insured and uninsured — who depend on them. This supply-side perspective has been absent from the administrative burden literature and from policy debates over Medicaid eligibility restrictions.

The policy moment makes these findings urgent. Congressional proposals for Medicaid work requirements, per-capita caps, and other eligibility restrictions would, if implemented, generate new waves of administrative churning analogous to the unwinding. The evidence presented here suggests that policymakers should account not only for the coverage losses experienced by individuals but also for the downstream financial strain on the safety-net delivery system. Investment in automated renewal infrastructure — the ex parte systems that allowed states to renew coverage without requiring burdensome paperwork —

appears to be a high-return intervention that reduces procedural disenrollment and preserves safety-net revenue.

The administrative wedge is a policy choice. States that invest in renewal automation can narrow it; states that add new eligibility requirements will widen it. The consequences extend beyond the individuals who fall into the wedge to the providers and communities that depend on a financially stable primary care safety net.

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[CITATION LIST - See bibliography.bib for complete entries]

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Tables

How Procedural Medicaid Disenrollment Undermined Community Health Center Finances During The Unwinding

Table 1: Summary Statistics For FQHC Panel, 2019-2024

Variable	N	Mean	SD	Min	Max
Financial outcomes					
Net patient revenue, $ millions	2,454	20.21	37.54	0.00	810.82
Medicaid revenue, $ millions	2,448	12.36	25.54	0.00	534.93
Medicaid revenue share	2,445	0.570	0.231	0.000	1.000
Operating margin	2,396	-0.122	1.912	-51.70	0.629
Staffing, full-time equivalents					
Medical FTEs	2,437	67.04	87.93	0.00	946.13
Enabling services FTEs	2,388	19.70	28.10	0.00	468.04
Total FTEs	2,442	197.32	237.78	3.14	2,780.21
Clinical quality					
Hypertension control rate	5,612	0.628	0.100	0.000	1.000
Diabetes good control rate	5,547	0.689	0.101	0.000	0.964
State-level treatment and instrument					
Procedural disenrollment rate	5,472	0.220	0.090	0.066	0.512
Ex parte renewal rate	4,736	0.588	0.229	0.075	0.995
Pre-pandemic ex parte renewal tier (1-4)	4,895	2.594	0.929	1.000	4.000

Notes: Sample includes 1,314 FQHCs across the 50 states and the District of Columbia over six years. N reflects non-missing observations by variable. The main IV is the MACPAC pre-pandemic ex parte renewal tier.

Table 2: Main Results - Post-Unwinding Cross-Sectional OLS And IV Estimates

Outcome	OLS estimate (SE)	p	IV estimate (SE)	p	N
Financial outcomes					
Log Medicaid revenue	-2.931** (1.186)	0.017	-5.700* (3.060)	0.069	1,186-1,325
Medicaid revenue share	-0.580*** (0.173)	0.002	-1.007** (0.474)	0.039	1,185-1,324
Log total patient revenue	-1.567* (0.835)	0.066	-3.772 (2.401)	0.123	1,187-1,326
Operating margin, pp	-1.921 (2.521)	0.450	-6.836 (5.442)	0.216	1,164-1,301
Staffing					
Log medical FTEs	-0.532 (0.462)	0.254	-1.836 (1.631)	0.266	1,171-1,319
Log total FTEs	-0.839* (0.418)	0.050	-1.304 (1.305)	0.323	1,175-1,323
Clinical quality					
Hypertension control rate	-0.031 (0.039)	0.427	-0.141 (0.114)	0.223	1,780-1,993
Diabetes good control rate	-0.030 (0.037)	0.419	-0.149 (0.102)	0.150	1,763-1,975

Notes: Cross-sectional specification estimated on 2023-2024. Instrument: pre-pandemic ex parte renewal tier from MACPAC's 2018 survey. The IV-support sample excludes Florida, Michigan, Oregon, and South Carolina because the renewal tier is missing. Standard errors clustered at the state level. First-stage F-statistic = 7.09.

Table 3: Anderson-Rubin Weak-Instrument-Robust Inference

Outcome	Specification	IV estimate	Standard 95% CI	AR p-value	AR 95% CI	AR rejects beta = 0
Log Medicaid revenue	Cross-sectional	-5.700	[-11.70, 0.30]	0.071	[-19.20, 0.60]	No
Log total cost	Cross-sectional	-0.968	[-3.67, 1.73]	0.484	[-6.00, 2.60]	No
Hypertension control rate	Cross-sectional	-0.141	[-0.37, 0.08]	0.189	[-0.68, 0.08]	No
Log Medicaid revenue	Panel DiD-IV	-0.289	[-2.71, 2.13]	0.812	[-5.40, 2.40]	No
Log total cost	Panel DiD-IV	-0.332	[-1.09, 0.43]	0.347	[-2.20, 0.20]	No
Hypertension control rate	Panel DiD-IV	0.133	[0.00, 0.26]	**0.013**	[0.03, 0.51]	**Yes**

Notes: Anderson-Rubin inference is valid regardless of instrument strength. Under the corrected cross-sectional sample restriction, the cross-sectional hypertension specification no longer rejects zero. Only the panel hypertension specification continues to reject zero, which is why the live manuscript treats hypertension as an unresolved anomaly rather than as a core causal result.

Table 4: Panel Difference-In-Differences IV Results (Secondary Specification)

Outcome	OLS estimate (SE)	p	IV estimate (SE)	p	N
Log Medicaid revenue	0.372 (0.641)	0.564	-0.289 (1.233)	0.816	1,913-2,131
Medicaid revenue share	-0.058 (0.072)	0.425	-0.053 (0.152)	0.731	1,911-2,129
Log total patient revenue	0.217 (0.330)	0.514	-0.304 (0.749)	0.688	1,914-2,132
Operating margin, pp	1.519 (1.599)	0.347	1.031 (3.451)	0.767	1,863-2,076
Log total cost	0.019 (0.115)	0.870	-0.332 (0.387)	0.395	2,343-2,618
Hypertension control rate	0.067*** (0.019)	0.001	0.133* (0.067)	0.052	4,815-5,389
Diabetes good control rate	0.027 (0.029)	0.360	0.001 (0.062)	0.990	4,764-5,335
Log medical FTEs	0.111 (0.117)	0.348	-0.203 (0.397)	0.612	1,879-2,103
Log total FTEs	0.027 (0.098)	0.784	-0.160 (0.276)	0.565	1,884-2,108

Outcome	OLS estimate (SE)	p	IV estimate (SE)	p	N
Visits per medical FTE	-73.04 (72.13)	0.316	146.07 (418.86)	0.729	1,879-2,103

Notes: Panel DiD-IV specification on 2019-2024. Endogenous variable: procedural disenrollment rate x post-unwinding. Instrument: ex parte renewal tier x post-unwinding. All specifications include FQHC and year fixed effects and county controls.

Table 5: Reduced-Form Estimates (Panel Specification)

Outcome	Coefficient (SE)	p-value	N
Log Medicaid revenue	0.012 (0.052)	0.812	1,913
Log total patient revenue	0.013 (0.031)	0.676	1,914
Log total cost	0.014 (0.014)	0.347	2,343
Operating margin, pp	-0.044 (0.147)	0.766	1,863
Medicaid revenue share	0.002 (0.007)	0.735	1,911
Hypertension control rate	-0.006** (0.002)	0.013	4,815
Diabetes good control rate	-0.000 (0.003)	0.990	4,764
Log medical FTEs	0.009 (0.016)	0.586	1,879
Log total FTEs	0.007 (0.012)	0.550	1,884
Visits per medical FTE	-6.399 (17.519)	0.717	1,879

Notes: Reduced-form estimates of the effect of the pre-pandemic ex parte renewal tier x post-unwinding indicator on FQHC outcomes. These results are useful for interpretation because they do not rely on the second-stage scaling of a relatively weak first stage.

Table 6: Heterogeneity By Pre-Unwinding Medicaid Dependence

Subgroup	Outcome	IV coefficient (SE)	p	N
High Medicaid dependence	Log Medicaid revenue	-8.68 (7.94)	0.282	445
High Medicaid dependence	Medicaid revenue share	-0.88 (1.21)	0.472	445
High Medicaid dependence	Operating margin, pp	-42.10 (37.14)	0.265	438
Low Medicaid dependence	Log Medicaid revenue	-3.83 (2.96)	0.202	741
Low Medicaid dependence	Medicaid revenue share	-0.60 (0.36)	0.100	740
Low Medicaid dependence	Operating margin, pp	-2.44 (4.79)	0.613	726

Notes: Split at the pre-unwinding median Medicaid revenue share (60.2 percent). These subgroup results are noisy, but the directional pattern is consistent with greater financial exposure among more Medicaid-dependent FQHCs.

Table 7: Hypertension Mechanism Tests

Hypothesis	Outcome	IV coefficient (SE)	p
Compositional	Log HTN denominator	0.342 (0.323)	0.296
Compositional	Log medical patients	-0.514 (0.438)	0.247
Compositional	HTN denominator / medical patients	0.065 (0.053)	0.232
Resource reallocation	Visits per medical FTE	146.07 (418.86)	0.729
Resource reallocation	Enabling FTE share	0.014 (0.025)	0.578
Resource reallocation	Patients per medical FTE	23.09 (139.43)	0.869
Reporting artifact	HTN data missing (=1)	-0.072 (0.046)	0.122

Notes: These tests do not support a clear channel through which procedural disenrollment would improve or worsen measured hypertension control. They are included to document why the live paper does not rely on hypertension as a substantive quality-effect claim.

Supplementary Appendix

How Procedural Medicaid Disenrollment Undermined Community Health Center Finances During The Unwinding

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Appendix Table A1: Panel Difference-In-Differences IV Results (Secondary Specification, 2019-2024)

Outcome	OLS estimate (SE)	p	IV estimate (SE)	p	N
Log Medicaid revenue	0.372 (0.641)	0.564	-0.289 (1.233)	0.816	1,913-2,131
Medicaid revenue share	-0.058 (0.072)	0.425	-0.053 (0.152)	0.731	1,911-2,129
Log total patient revenue	0.217 (0.330)	0.514	-0.304 (0.749)	0.688	1,914-2,132
Operating margin, pp	1.519 (1.599)	0.347	1.031 (3.451)	0.767	1,863-2,076
Log total cost	0.019 (0.115)	0.870	-0.332 (0.387)	0.395	2,343-2,618
Hypertension control rate	0.067*** (0.019)	0.001	0.133* (0.067)	0.052	4,815-5,389
Diabetes good control rate	0.027 (0.029)	0.360	0.001 (0.062)	0.990	4,764-5,335
Log medical FTEs	0.111 (0.117)	0.348	-0.203 (0.397)	0.612	1,879-2,103
Log total FTEs	0.027 (0.098)	0.784	-0.160 (0.276)	0.565	1,884-2,108
Visits per medical FTE	-73.04 (72.13)	0.316	146.07 (418.86)	0.729	1,879-2,103

SOURCE: Authors' analysis. NOTES: Panel DiD-IV specification on the 50 states and the District of Columbia. Endogenous variable: state procedural disenrollment rate x post-unwinding indicator. Instrument: pre-pandemic ex parte renewal tier x post-unwinding. All specifications include FQHC and year fixed effects and county-level controls. Standard errors clustered at the state level. ***$p < 0.01$*** *$p < 0.05$* $p < 0.10$

Appendix Table A2: Reduced-Form Estimates (Panel Specification)

Outcome	Coefficient (SE)	p-value	N
Log Medicaid revenue	0.012 (0.052)	0.812	1,913
Log total patient revenue	0.013 (0.031)	0.676	1,914
Log total cost	0.014 (0.014)	0.347	2,343
Operating margin, pp	-0.044 (0.147)	0.766	1,863
Medicaid revenue share	0.002 (0.007)	0.735	1,911

Outcome	Coefficient (SE)	p-value	N
Hypertension control rate	-0.006** (0.002)	0.013	4,815
Diabetes good control rate	-0.000 (0.003)	0.990	4,764
Log medical FTEs	0.009 (0.016)	0.586	1,879
Log total FTEs	0.007 (0.012)	0.550	1,884
Visits per medical FTE	-6.399 (17.519)	0.717	1,879

SOURCE: Authors' analysis. NOTES: Reduced-form estimates of the effect of the pre-pandemic ex parte renewal tier x post-unwinding indicator on FQHC outcomes. All specifications include FQHC and year fixed effects and county-level controls. Standard errors clustered at the state level. **$p < 0.05$

Appendix Table A3: Hypertension Control Rate Mechanism Tests

Hypothesis	Outcome	IV coefficient (SE)	p	N
Compositional	Log HTN denominator	0.342 (0.323)	0.296	4,828
Compositional	Log medical patients	-0.514 (0.438)	0.247	1,884
Compositional	HTN denominator / medical patients	0.065 (0.053)	0.232	1,884
Resource reallocation	Visits per medical FTE	146.07 (418.86)	0.729	1,879
Resource reallocation	Enabling FTE share	0.014 (0.025)	0.578	1,834
Resource reallocation	Patients per medical FTE	23.09 (139.43)	0.869	1,879
Reporting artifact	HTN data missing (=1)	-0.072 (0.046)	0.122	4,828

SOURCE: Authors' analysis. NOTES: Panel DiD-IV estimates testing three hypothesized mechanisms for the hypertension control rate finding. These tests did not identify a coherent mechanism linking procedural disenrollment to quality changes, reinforcing the decision to treat the panel hypertension result as unresolved rather than as a headline causal finding.

Appendix Table A4: Robustness - Balanced Panel (63 FQHCs Observed In All 6 Years)

Outcome	OLS estimate	p	IV estimate	p	N
Log Medicaid revenue	-0.135	0.854	-2.297	0.347	275-305
Log total cost	-0.184	0.617	-1.187	0.282	285-315
Operating margin, pp	-2.897	0.419	-4.674	0.645	273-303

Outcome	OLS estimate	p	IV estimate	p	N
HTN control rate	0.084	0.116	0.029	0.848	342-378
Log medical FTEs	-0.323	0.423	-0.778	0.533	278-307

SOURCE: Authors' analysis. NOTES: Balanced panel restricted to 63 FQHCs observed in all six years. Panel DiD-IV specification with FQHC and year fixed effects. Standard errors clustered at the state level.

Appendix Table A5: Robustness - Three-Period Panel (Excluding 2021-2022 Pandemic Years)

Outcome	OLS estimate	p	IV estimate	p	N
Log Medicaid revenue	2.851	0.332	3.779	0.417	271-315
Log total cost	0.347	0.367	0.057	0.958	393-441
Operating margin, pp	6.657	0.369	11.706	0.313	271-315
HTN control rate	0.061**	0.024	0.130	0.114	3,493-3,909
Log medical FTEs	0.317	0.506	-0.293	0.824	286-328

SOURCE: Authors' analysis. NOTES: Three-period panel using 2019-2020 as pre-unwinding and 2024 as post-unwinding, excluding the pandemic-era middle years. Panel DiD-IV specification with FQHC and year fixed effects. Standard errors clustered at the state level. $^{**}p < 0.05$

Appendix Table A6: Pre-Post Comparison Of FQHC Outcomes

Variable	Pre-unwinding mean	Post-unwinding mean	Difference	p-value
Total patient revenue, $ millions	17.54	22.33	4.78***	0.001
Medicaid revenue, $ millions	11.01	13.43	2.42**	0.016
Medicaid revenue share	0.583	0.559	-0.024**	0.012
Operating margin	-13.81	-10.95	2.86	0.719
Medical FTEs	63.86	69.56	5.70	0.107
Total FTEs	187.15	205.37	18.22*	0.057
HTN control rate	0.611	0.658	0.046***	<0.001

SOURCE: Authors' analysis. NOTES: Pre-unwinding = 2019-2022; post-unwinding = 2023-2024. Two-sample t-tests. ***$p < 0.01$*** *$p < 0.05$* $p < 0.10$

Appendix Table A7: Heterogeneity By Pre-Unwinding Medicaid Dependence (Cross-Sectional IV)

Outcome	Subgroup	IV estimate (SE)	p	N
Log Medicaid revenue	High Medicaid dependence (>60.2%)	-8.68 (7.94)	0.282	445
Log Medicaid revenue	Low Medicaid dependence (<=60.2%)	-3.83 (2.96)	0.202	741
Operating margin, pp	High Medicaid dependence	-42.1 (37.1)	0.265	438
Operating margin, pp	Low Medicaid dependence	-2.4 (4.8)	0.613	726
Medicaid revenue share	High Medicaid dependence	-0.88 (1.21)	0.472	445
Medicaid revenue share	Low Medicaid dependence	-0.60 (0.36)	0.100	740

SOURCE: Authors' analysis. NOTES: Cross-sectional IV specification on post-unwinding observations, split at the pre-unwinding median Medicaid revenue share. These subgroup estimates are noisy but directionally consistent with the interpretation that more Medicaid-dependent centers are more financially exposed to procedural disenrollment.

Appendix Table A8: Instrument Balance Test

Covariate	Coefficient (SE)	p-value
Log population	0.322 (0.217)	0.145
Percent Black	-0.526 (1.757)	0.766
Percent Hispanic	-2.240 (3.167)	0.483
Percent aged 65+	0.274 (0.485)	0.575
Unemployment rate	0.007 (0.192)	0.972
Percent poverty	-0.914** (0.405)	0.029
Log median income	0.056* (0.030)	0.066
Physician density	6.788*** (2.312)	0.005
Rural-urban code	-0.360 (0.255)	0.165
Medicaid expansion	0.151* (0.076)	0.053
Governor party (R = 1)	-0.128 (0.090)	0.162
Joint Wald F-test	F = 2.38	0.019

SOURCE: Authors' analysis. NOTES: The joint balance test rejects, which is why the paper now treats the IV strategy as informative but imperfect and interprets the cross-sectional estimates as upper bounds rather than as precise point estimates.

Appendix Table A9: Augmented Cross-Sectional IV - Adding State Political And Medicaid Controls

Outcome	Primary IV (SE)	p	Augmented IV (SE)	p
Log Medicaid revenue	-5.700 (3.060)	0.069	-2.296 (4.057)	0.574
Medicaid revenue share	-1.007 (0.474)	0.039	-0.450 (0.577)	0.439

SOURCE: Authors' analysis. NOTES: Primary IV is the baseline cross-sectional specification with county controls and year fixed effects. Augmented IV adds state Medicaid expansion status and governor party. The attenuation supports the paper's bounded interpretation of the cross-sectional IV estimates.

Appendix Table A10: Wild Cluster Bootstrap P-Values For Headline Results

Outcome	Conventional IV p-value	Reduced-form conventional p-value	Bootstrap p-value (9,999 reps)
Log Medicaid revenue	0.069	0.071	0.102
Medicaid revenue share	0.039	0.076	0.150

SOURCE: Authors' analysis. NOTES: Wild cluster bootstrap with Rademacher weights applied to reduced-form regressions, which provides a more conservative finite-sample check given the small number of clusters.

Appendix Figure A1: First Stage - Pre-Pandemic Ex Parte Renewal Tier And Procedural Disenrollment

. States with higher pre-pandemic ex parte renewal tiers experienced lower procedural disenrollment during the unwinding.

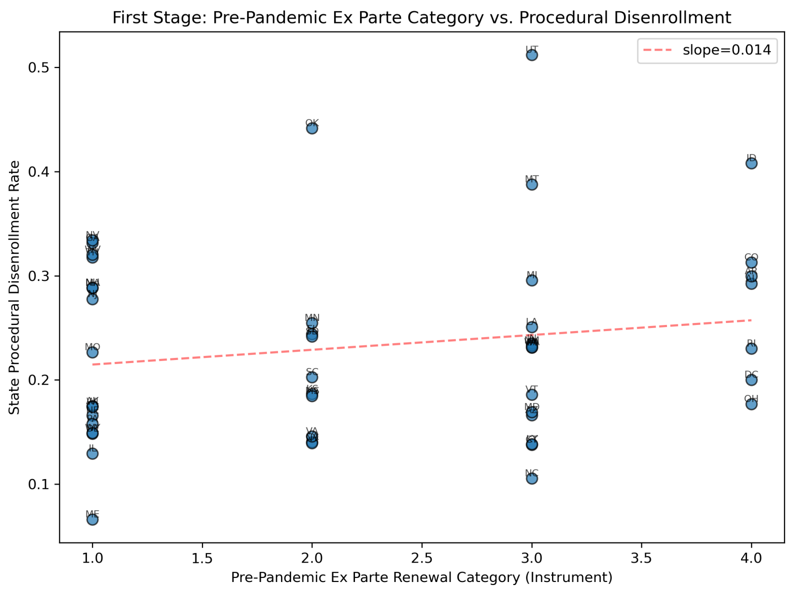

Figure 1: First Stage Scatter

Note: This figure compares estimates across groups or specifications for the first Stage Scatter. It is intended to make effect heterogeneity and subgroup precision easier to assess.

Appendix Figure A2: Coefficient Comparison - OLS vs. IV (Cross-Sectional)

Figure 2: Coefficient Comparison

Note: This figure presents the coefficient Comparison. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

. IV estimates are generally larger in magnitude than OLS estimates for the main financial outcomes.

Appendix Figure A3: Heterogeneity By Medicaid Dependence

. The coefficient plot shows directionally larger point estimates for high-Medicaid-dependence FQHCs, but confidence intervals remain wide and overlap substantially with the low-dependence subgroup.

Appendix Figure A4: Reduced-Form Effects Of Instrument On Outcomes

. Each point represents the reduced-form coefficient from regressing the outcome on the pre-pandemic ex parte renewal tier x post-unwinding indicator, with FQHC and year fixed effects and county-level controls. Standard errors clustered at the state level. Color-coded by significance level: red = $p < 0.05$; orange =

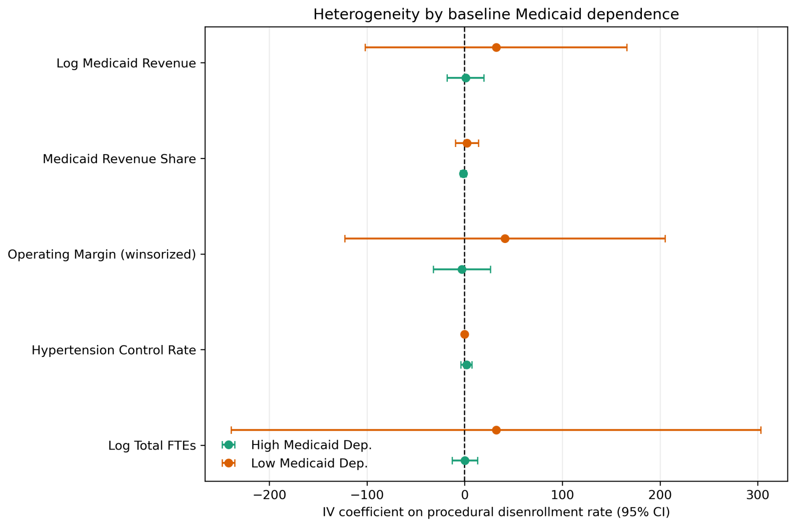

Figure 3: Heterogeneity Coefficient Plot

Note: This figure compares estimates across groups or specifications for the heterogeneity Coefficient Plot. It is intended to make effect heterogeneity and subgroup precision easier to assess.

p < 0.10; blue = p >= 0.10. Horizontal bars show 95% confidence intervals. Only hypertension control rate is significant at the 5% level, consistent with the manuscript's discussion of the hypertension anomaly.

Appendix Figure A5: Instrument Balance – Pre-Determined Covariates

. Each bar shows the t-statistic from an individual regression of the covariate on the pre-pandemic ex parte renewal tier, using the post-unwinding cross-sectional sample with year fixed effects. Dashed lines indicate conventional 5% significance thresholds (t = +/- 1.96). Two covariates (poverty rate, physician density) are significant at the 5% level, flagged in red. The joint Wald F-test (F = 2.38, p = 0.019) rejects, motivating the paper's bounded interpretation of the IV estimates.

Appendix Figure A6: Sensitivity To Instrument Definition

. IV estimates from the panel DiD-IV specification (2019-2024) using three alternative instruments: (1) pre-pandemic ex parte renewal tier (main), (2) composite ex parte capacity index, and (3) SNAP/TANF data integration indi-

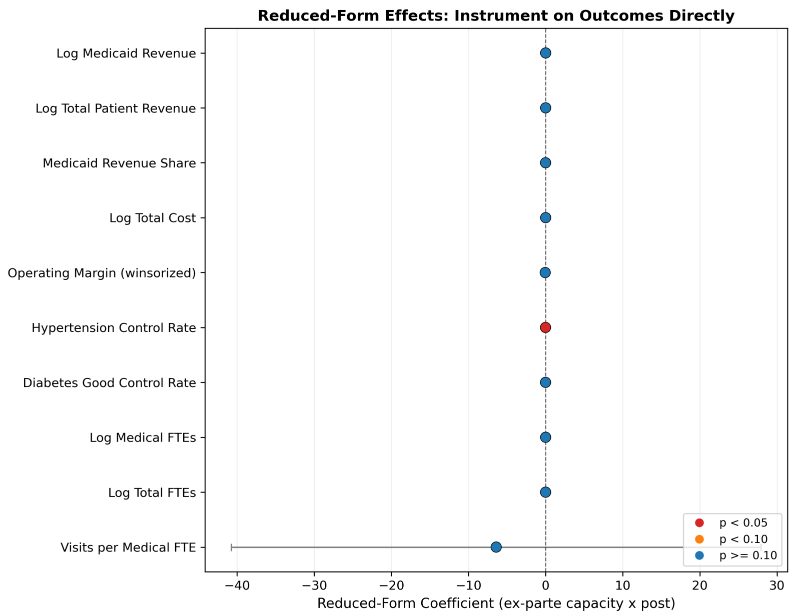

Figure 4: Reduced Form Plot

Note: This figure presents the reduced Form Plot. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

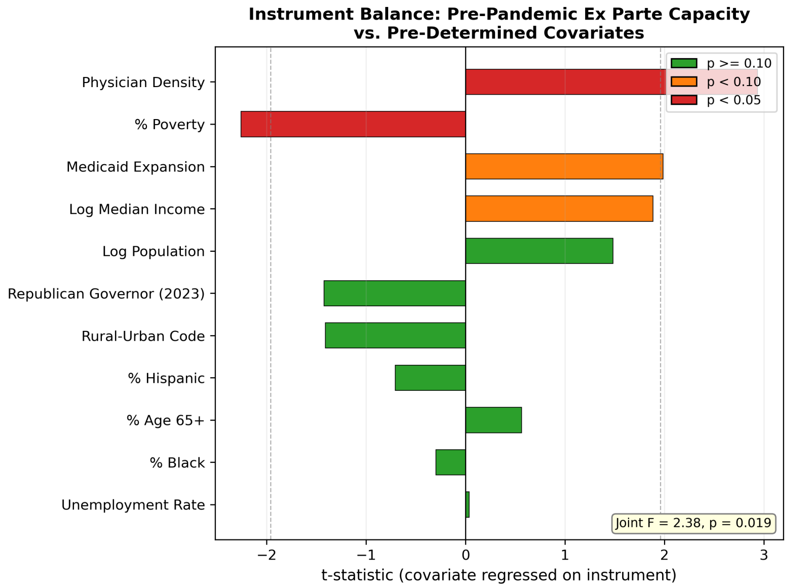

Figure 5: Instrument Balance Plot

Note: This figure presents the instrument Balance Plot. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

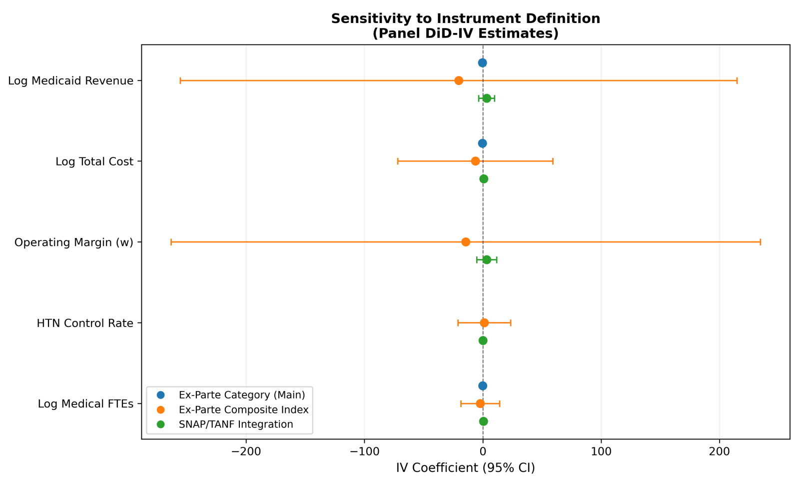

Figure 6: Instrument Sensitivity Plot

Note: This figure reports a robustness or sensitivity check for the instrument Sensitivity Plot. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

cator. All specifications include FQHC and year fixed effects. Standard errors clustered at the state level. The wide confidence intervals for the composite index reflect a weaker first stage. Results are most stable under the main ex parte tier instrument and the SNAP/TANF integration indicator.

Appendix: Instrument Construction Details

The main instrument in the live paper is the pre-pandemic ex parte renewal tier (1-4) from MACPAC's 2018 survey of state renewal automation:

1. Fewer than 25 percent of renewals automated
2. 25 to 50 percent automated
3. 50 to 75 percent automated
4. More than 75 percent automated

This tier is time-invariant and assigned to FQHCs based on headquarter state. Florida, Michigan, Oregon, and South Carolina are excluded from IV models because MACPAC did not report a renewal tier for those jurisdictions.

I also compiled two supplementary administrative-capacity measures for alternative-instrument checks:

1. SNAP/TANF data integration: indicator for whether the state's Medicaid renewal system could verify eligibility using SNAP and/or TANF administrative data before 2020.
2. Integrated human services platform: indicator for whether Medicaid, SNAP, and TANF operated through a shared eligibility platform rather than siloed systems.

These supplementary measures are used only in robustness checks. They are not the live paper's main instrument because their first-stage performance is weaker and the submission package is now centered on the MACPAC renewal tier.