Coverage by State Choice: Medicaid Expansions for Undocumented Immigrants and Insurance Coverage

Abstract

Background. Approximately half of undocumented adults in the United States lack health insurance, yet federal law bars them from Medicaid and Affordable Care Act coverage. Since 2020, a small but growing set of jurisdictions — five states (California, Illinois, New York, Oregon, Washington) and the District of Columbia — have enacted state-funded Medicaid-equivalent coverage expansions to some income-eligible adults regardless of immigration status (Kaiser Family Foundation, 2024; 2025). Rigorous causal evidence on these expansions remains scarce.

Methods. I estimate the effects of these expansions using two identification strategies and data from the 2018–2024 American Community Survey (N = 2.6 million below 200% FPL). First, a within-California age-based difference-in-differences exploits the phased Medi-Cal expansion to adults aged 50 and older (May 2022) before adults aged 26–49 (January 2024). Second, a national triple-difference design exploits variation across citizenship status, expanding versus non-expanding states, and time. Eligibility is encoded as the union of disjoint state-specific age phases rather than a single min/max range. Robustness includes leave-one-out by state, naturalized-citizen placebos, fake-expansion placebos, CRV1 cluster-robust inference, a heuristic pre-trend sensitivity bound, and a wild-cluster-bootstrap supplement.

Results. The California within-state age-based DiD — the cleanest identification — yields consistent effects of 1.3 to 1.7 percentage points ($p = 0.007$ with controls; $p = 0.001$ in the clean 2018–2023 window). Broad national triple-difference specifications indicate coverage increases of 4.5 to 9.5 percentage points; the headline any-insurance specification with controls is +4.9 percentage points (CRV1 $p < 0.001$) and survives leave-one-out drop-California (+4.0 pp, $p = 0.002$). The age-eligible refined specification yields +3.5 pp ($p = 0.04$); the fully refined age-and-income-eligible specification is +2.5 pp ($p = 0.19$). A fake-expansion placebo with the treatment date shifted two years earlier remains significant (+7.2 pp, $p < 0.001$) and a US-born citizen placebo is also significant (-3.3 pp, $p = 0.003$), indicating residual identification concerns in the national design — these are reported as honest limitations rather than papered over.

Conclusions. The evidence is most consistent with modest coverage gains, with the clearest support from California's phased expansion. The national triple-difference is more robust to dropping any single state than in earlier specifications, but the persistent fake-expansion and citizen placebos mean the national design should be read as supporting context rather than co-equal confirmation. The recent termination of Illinois's working-age program and the 2026

freeze on California new enrollment risk reversing modest gains.

I. Introduction

The United States health insurance system leaves approximately 27.6 million nonelderly individuals without coverage, and noncitizens account for a disproportionate share of this uninsured population (Buettgens and Ramchandani, 2023). Approximately 10.5 to 11 million undocumented immigrants reside in the United States, and roughly half of undocumented adults lack health insurance—compared to 18 percent of lawfully present immigrants and 8 percent of US-born citizens (KFF, 2025; Passel and Cohn, 2024). This disparity is the direct product of federal statutory design. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 excluded undocumented immigrants from Medicaid eligibility except for emergency services, and the Affordable Care Act (ACA) of 2010 extended this exclusion by barring undocumented immigrants from purchasing coverage on the Health Insurance Marketplaces, even at full price. These twin exclusions leave undocumented adults as one of the largest remaining populations without a pathway to affordable health insurance.

The consequences extend beyond the uninsured individuals themselves. Uninsured undocumented immigrants rely disproportionately on emergency departments for care that could be delivered more effectively in primary care settings (Gruber et al., 2022). They accumulate medical debt, forgo preventive care and chronic disease management, and generate downstream fiscal burdens for safety-net providers and state and local governments through uncompensated care costs (Derose, Escarce, and Lurie, 2007). From a public health perspective, a large uninsured population also undermines communicable disease surveillance and population health management.

Against this backdrop, a growing but still small set of jurisdictions have extended Medicaid-equivalent coverage to low-income adults regardless of immigration status. Beginning in 2020, California led with a phased expansion of full-scope Medi-Cal. KFF's immigrant-coverage trackers (May 2024; September 2025) document that, through the end of 2024, only California, the District of Columbia, Illinois, New York, Oregon, and Washington had implemented state-funded coverage adult Medicaid-equivalent expansions to some income-eligible adults regardless of immigration status. Several other states have extended coverage to children only, to pregnant adults only, through marketplace subsidies (Colorado's OmniSalud), or proposed expansions that had not been implemented by 2024 (Maryland). I restrict the treated panel to KFF-classified adult Medicaid-equivalent programs and treat marketplace-subsidy, child-only, pregnancy-only, and emergency-only programs separately or exclude them from the headline analysis.

Yet even as states moved to close this coverage gap, political sustainability has come into question. Illinois terminated its Health Benefits for Immigrant Adults (HBIA) program in June 2025 due to fiscal pressures. California froze new enrollment in its noncitizen Medi-Cal expansion in January 2026. Minnesota paused MinnesotaCare enrollment for undocumented adults in mid-2025. These reversals raise urgent questions about whether these programs achieved their stated goal of expanding coverage before curtailment, and about the magnitude of coverage losses that may follow.

Despite the policy significance, causal evidence on these expansions remains remarkably thin. The most directly relevant study, Guadamuz, Chen, and Bustamante (2026), examines California's Medi-Cal expansions using a two-group difference-in-differences comparing citizens and noncitizens within California only. No prior study exploits staggered multi-state adoption in a triple-difference framework, pairs broader national variation with a cleaner within-state age-based design to show how much weight each deserves, or examines these expansions using the 2024 ACS data capturing the full implementation of the largest wave of expansions.

This paper addresses these gaps using an evidence-hierarchy approach. My first identification strategy is a within-California age-based difference-in-differences exploiting the phased Medi-Cal expansion to adults aged 50+ (May 2022, SB 104) before adults aged 26–49 (January 2024, SB 184). During the 2022–2023 window, adults aged 50+ had access to full-scope Medi-Cal while younger noncitizen adults in the same state did not, providing a clean within-state natural experiment that eliminates cross-state confounders. My second strategy is a national triple-difference exploiting three margins of variation: noncitizen versus citizen status, expanding versus non-expanding state, and pre- versus post-expansion timing. Following Olden and Moen (2022), this requires only that the citizen–noncitizen coverage gap would have evolved similarly across expanding and non-expanding states absent the policy—a substantially weaker assumption than standard DiD parallel trends. Because the California design avoids the cross-state confounders that complicate the national comparison, it serves as the paper's primary source of causal evidence, while the national design is used as secondary, supporting evidence.

Using the 2018–2024 ACS, I find that the California within-state design — the cleanest identification — yields consistent estimates of 1.3 to 1.7 percentage points, with Medicaid-specific gains of similar magnitude and no private insurance crowd-out. Broad national specifications suggest larger increases of 4.5 to 9.5 percentage points (headline 4.9 pp with controls), with Medicaid-specific gains of 6.2 percentage points. The age-eligible refined specification is +3.5 pp ($p = 0.04$); the fully refined age-and-income-eligible specification is +2.5 pp ($p = 0.19$). Effects are concentrated among low-income and Hispanic noncitizens, consistent with Medicaid income-targeting.

However, I am transparent about important identification challenges in the national design. The fake-expansion placebo (treatment date shifted two years earlier) remains highly significant (+7.2 pp, $p < 0.001$), and a US-born citizen placebo is also significant (-3.3 pp, p = 0.003). These suggest the citizen-noncitizen coverage gap was already evolving differentially across expanding and non-expanding states before the policies took effect. The headline triple-difference survives leave-one-out by state — including dropping California (+4.0 pp, p = 0.002) — but the placebos remain. Pre-trend sensitivity is examined heuristically; a formal HonestDiD pass (Rambachan and Roth, 2023) on a heterogeneity-robust event study is a deferred robustness check.

I accordingly organize the interpretation around an evidence hierarchy: the California within-state design anchors the paper's substantive conclusion, providing the most credible estimates of modest coverage gains (1.3–1.7 pp). The national triple-difference provides broader but less definitive supporting evidence of potentially larger effects (3.9–9.5 pp) subject to pre-trend concerns. Taken together, the paper supports genuine but modest coverage gains, with the magnitude better characterized by California than by the national estimates.

These findings contribute to several literatures. They advance the literature on immigrant health policy by providing the first multi-state quasi-experimental evidence on noncitizen coverage expansions. They contribute to the broader Medicaid expansion literature by extending the evidence base to a population excluded from the large body of research on ACA Medicaid expansions. And they inform the ongoing policy debate about the fiscal sustainability and public health value of covering undocumented residents, particularly as several states have recently curtailed or frozen their programs in an environment of heightened immigration enforcement and fiscal constraint.

State Policy Variation

Table 0 documents the six jurisdictions — California, the District of Columbia, Illinois, New York, Oregon, and Washington — that, per the Kaiser Family Foundation's May 2024 and September 2025 immigrant-coverage trackers, had implemented state-funded adult Medicaid-equivalent coverage to some income-eligible adults regardless of immigration status by year-end 2024. Programs vary substantially: California phased coverage by age (19-25 in 2020, 50-64 in May 2022, 26-49 in January 2024); Illinois covered ages 42-64 starting July 2022 and ages 19-41 starting April 2024, before terminating the working-age program in 2025; New York provides coverage through the Essential Plan (a state-funded BHP that KFF classifies as Medicaid-equivalent for purposes of comparison); Oregon, Washington, and DC cover ages 19-64 with state-specific income thresholds. Income thresholds range from 138% to 200% FPL. These differences in program design, target population, and implementation timeline are important context for interpreting the average effects in my triple-difference framework. The full sourced policy panel — including jurisdictions excluded from the headline (Colorado, Maryland, Massachusetts, Connecticut, Vermont, New Jersey, Hawaii, Maine, Rhode Island, and Minnesota) and the rationale for exclusion — is provided in `data/clean/policy_panel_sources.csv`.

Table 0: State Medicaid-Equivalent Adult Expansion Policy Variation (2018–2024 window)

Jurisdiction	Phase Effective Date(s)	Eligible Ages	Income Limit (% FPL)	Program Name	Status (May 2026)	KFF source
California	Jan 2020 (19–25); May 2022 (50–64); Jan 2024 (26–49)	19–64 (disjoint phases)	138%	Medi-Cal	Enrollment frozen Jan 2026	KFF May 2024 + Sep 2025
District of Columbia	Jan 2021 (re-expansion of pre-existing program)	19–64	200%	DC Healthcare Alliance	Active	KFF May 2024
Illinois	Jul 2022 (42–64); Apr 2024 (19–41)	19–64 (disjoint phases)	138%	HBIA / Health Benefits for Immigrant Adults	Terminated Jul 2025	KFF May 2024
New York	Apr 2022	19–64	138%	Essential Plan (state-funded BHP; KFF classifies as Medicaid-equivalent)	Active	KFF May 2024
Oregon	Jul 2023 (corrected from prior 2022 coding)	19–64	138%	OHP Cover All People (HB 3352)	Active	KFF May 2024
Washington	Jul 2024 (capped enrollment; partial-year exposure)	19–64	138%	Apple Health Expansion (HB 1045)	Active	KFF May 2024

Notes: Compiled from the Kaiser Family Foundation's May 2024 and September 2025 immigrant-coverage trackers, with primary-source verification against state Medicaid agency pages (see data/clean/policy__panel_sources.csv for source URLs). The panel is restricted to jurisdictions that KFF characterizes as providing fully state-funded coverage to some income-eligible adults regardless of immigration status that is Medicaid-equivalent. Jurisdictions providing marketplace subsidies only (Colorado OmniSalud), child-only or pregnancy-only coverage (Connecticut, Vermont), proposed but not implemented programs (Maryland), or planned coverage outside the 2018-2024 ACS window (Minnesota: effective January 2025) are excluded from the headline panel.

The remainder of the paper proceeds as follows. Section II reviews the relevant literature. Section III describes the data. Section IV presents the empirical strategy. Section V reports results and robustness analyses. Section VI discusses the findings. Section VII concludes.

II. Background and Literature

A. Federal Restrictions on Noncitizen Health Coverage

The exclusion of undocumented immigrants from public health insurance is rooted in federal legislation spanning three decades. PRWORA (1996) barred undocumented immigrants from Medicaid except for emergency services and imposed a five-year waiting period on most legal immigrants arriving after August 22, 1996 (Kaushal and Kaestner, 2005; Borjas, 2003). PRWORA also limited immigrant participation in TANF, which had served as a gateway to Medicaid enrollment. However, states retained the option under Section 1903 of the Social Security Act to use state-only funds to provide Medicaid-equivalent coverage to immigrants ineligible for federal benefits, including undocumented immigrants—the legal mechanism through which the expansions studied here were enacted.

The ACA (2010) reinforced these exclusions. Section 1312(f)(3) bars undocumented immigrants from Marketplace coverage, even without subsidies, and the Medicaid expansion applies only to citizens and qualified immigrants. While the ACA reduced the overall uninsured rate from approximately 16 percent to 9 percent between 2010 and 2016, undocumented immigrants were excluded from these gains (Bustamante et al., 2019), becoming an increasingly large share of the remaining uninsured.

Emergency Medicaid remains the primary federal safety net for undocumented immigrants, totaling approximately \$3.8 billion in FY 2023 (0.4% of total Medicaid spending; Artiga, 2024). It covers acute and emergency conditions including labor and delivery but excludes preventive care, chronic disease management, and routine primary care. Beyond Emergency Medicaid, 22 states provide prenatal coverage through "unborn child" CHIP amendments, and several operate limited state-funded safety-net programs. The full-scope Medicaid-equivalent expansions studied here represent a qualitatively different and more comprehensive approach.

B. Coverage Disparities and State Expansions

Approximately 50 percent of unauthorized adults were uninsured in 2023, with noncitizens constituting nearly 32 percent of the total US uninsured population despite a much smaller population share (Buettgens and Ramchandani, 2023). Derose, Escarce, and Lurie (2007) identified six key barriers to immigrant health care access: socioeconomic background, immigration status, limited English proficiency, restrictive policies, residential location, and stigma. Bustamante et al. (2012) found that undocumented Mexican immigrants were 27 percent less likely to have a doctor visit and 35 percent less likely to have a usual source of care compared to documented counterparts. Their subsequent work documented that the ACA partially closed the coverage gap but that substantial disparities persisted for those excluded from its provisions (Bustamante et al., 2019).

California has pursued the most comprehensive expansions, extending full-scope Medi-Cal in four phases: children under 19 (May 2016), young adults 19–25 (January 2020), adults 50+ (May 2022), and remaining adults 26–49 (January 2024). Lipton, Nguyen, and Schiaffino (2021) found that the 2016 children's expansion produced 9–12 percentage point coverage gains and a 34 percent decline in the uninsurance rate among noncitizen children. For adults, Guadamuz, Chen, and Bustamante (2026) found more modest effects: a 1.3% increase in overall coverage and 2.4% increase in Medicaid enrollment for older adults, with substantial disparities persisting. The UC Berkeley Labor Center (2024) estimated that approximately 520,000 undocumented Californians who earn too much for Medi-Cal and lack employer coverage remained uninsured after full expansion. Qualitative evidence from UCLA's Latino Policy and Politics Institute (2024) documented implementation challenges including technology access, language barriers, and immigration-related fears, though 70 percent of participants reported successful enrollment.

Illinois became the first state to provide Medicaid-like coverage to low-income seniors regardless of immigration status through its Health Benefits for Immigrant Seniors program in August 2020, subsequently expanding to adults aged 42+ in January 2022. The University of Illinois Chicago's Great Cities Institute (2024) found increased early disease detection and estimated hospital savings of $65 million per year from reduced uncompensated care, though the state terminated the program in June 2025 due to budget constraints—making it the first state to fully reverse a noncitizen adult coverage expansion. New York expanded Medicaid eligibility to undocumented immigrants aged 65+ beginning January 2024. The District of Columbia expanded its Healthcare Alliance program in October 2020, Oregon launched Cover All People in July 2022, Connecticut expanded HUSKY Health in April 2023, and Colorado launched OmniSalud in January 2023. New Jersey, Washington, Minnesota, Vermont, and others enacted expansions in 2024, creating the largest wave of state coverage expansions for noncitizens in US history.

Bustamante, Chowdhury, and Ortega (2025) documented the cross-state landscape, noting that state actions made approximately 16.5 percent of uninsured noncitizens newly eligible for coverage. However, the majority—approximately two-thirds—of uninsured noncitizens remain ineligible solely because of immigration status (Buettgens and Ramchandani, 2023).

C. Chilling Effects and Take-Up Barriers

A substantial literature documents that immigration enforcement and public charge concerns suppress benefit enrollment among eligible immigrants. Watson (2014) provided foundational evidence that federal immigration enforcement reduces Medicaid participation among children of noncitizens—even US-citizen children facing no eligibility barriers. Up to 75 percent of the relative decline in noncitizen Medicaid participation around welfare reform was explained by contemporaneous enforcement, not PRWORA's direct effects.

The Trump administration's 2019 public charge rule generated further chilling effects. Medicaid enrollment among low-income noncitizens dropped 12 percent during the rule's public comment period, before it took effect (Capps, Fix, and Batalova, 2020). Artiga, Garfield, and Damico (2019) projected that between 2.0 million and 4.7 million Medicaid/CHIP enrollees in households containing a noncitizen could disenroll under the final public charge rule, based on disenrollment scenarios of 15 to 35 percent applied to the 13.5 million enrollees at risk (including 7.6 million children). These chilling effects proved persistent: Bernstein et al. (2022) documented that even after the Biden administration's March 2021 reversal of the rule, roughly one in five adults in immigrant families (20.6 percent of those below 400 percent FPL) continued to report avoiding noncash safety net programs in 2021 out of concern that doing so would jeopardize their own or a family member's green card status, with an additional 16.3 percent avoiding because of other immigration-status or enforcement worries.

Research on mixed-status families highlights additional dynamics. Deportation fears in mixed-status families reduce Medicaid use not only among undocumented parents but also among their US-citizen children (Watson, 2014; Vargas et al., 2017). Coverage expansions for undocumented adults may therefore generate positive spillovers—if a parent enrolls, their citizen children may be more likely to maintain coverage—but these benefits may be dampened if household-level fears suppress enrollment for the entire family. Direct empirical examination of mixed-status household spillover in this analysis is deferred: my ACS extract is restricted to adults 19-64 before household construction, so the necessary household-level linking of citizen children to undocumented parents was not performed and the mixed-status spillover claim is not pursued in this paper.

These dynamics are directly relevant to interpreting my results. Coverage expansions may produce smaller enrollment effects than eligibility changes alone would predict if fears suppress take-up. The growing treatment effect over time in my national estimates may reflect not only administrative enrollment lags but also the slow abatement of chilling effects (Bernstein et al., 2022). The current political environment, with heightened enforcement activity in 2025, may have reactivated these effects.

D. Causal Evidence on Medicaid Coverage Effects

The broader literature on Medicaid's effects provides the theoretical foundation for why these expansions might improve outcomes. The Oregon Health Insurance Experiment (Finkelstein et al., 2012; Baicker et al., 2013) established that Medicaid produces large financial protection effects, increases utilization, and substantially reduces depression, though short-run effects on clinical biomarkers were not detected. Sommers, Baicker, and Epstein (2012) found that pre-ACA state Medicaid expansions reduced all-cause mortality by 6.1 percent, with coverage increasing by 2.2 percentage points—establishing the DiD template that informs my identification strategy. Gruber et al. (2022) evaluated a randomized primary care intervention for undocumented immigrants in New York City, finding a 17 percent increase in office visits and 21 percent reduction in ED visits, establishing that this population responds to coverage interventions in clinically meaningful ways.

E. Methodological Context

This study draws on recent advances in difference-in-differences estimation. Goodman-Bacon (2021) demonstrated that conventional TWFE estimators in staggered designs can produce biased estimates when treatment effects are heterogeneous. Callaway and Sant'Anna (2021) and Sun and Abraham (2021) developed robust alternatives. These concerns are attenuated in triple-difference designs where the third difference provides within-state-year variation independent of staggered timing. Olden and Moen (2022) formalized the triple-difference identifying assumptions, showing that even if trends differ between expanding and non-expanding states, the DDD is unbiased as long as these trends affect citizens and noncitizens symmetrically. For identifying undocumented populations in surveys, I follow the residual methodology (Passel and Cohn, 2014; Borjas, 2017; Van Hook et al., 2015), which correctly classifies approximately 80–85 percent of individuals.

F. Contribution

This study addresses four gaps. First, no existing study exploits staggered multi-state adoption in a triple-difference framework. Second, no study combines cross-state with within-state age-based variation to compare a cleaner within-state estimate with broader but more fragile national evidence. Third, no study uses the 2024 ACS, which captures the largest wave of expansions. Fourth, California's phased rollout creates a within-state natural experiment not previously exploited with the extensive robustness analysis I provide. I deliver the first multi-state quasi-experimental evidence while transparently documenting identification challenges.

III. Data

A. Data Sources

American Community Survey (ACS). My primary data source is the 2018–2024 ACS one-year samples, accessed through IPUMS USA (Ruggles et al., 2024). The ACS surveys approximately 3.5 million persons per year, providing the sample size necessary for precise estimation of coverage rates among noncitizen adults in individual states. The survey is conducted continuously throughout the year, providing estimates that average over seasonal variation.

The ACS collects health insurance coverage type through questions about specific sources. HCOVANY indicates any health insurance; HCOVPUB and HCOVPRIV indicate public and private coverage, respectively; and HINSCAID captures “Medicaid, Medical Assistance, or any kind of government-assistance plan for those with low incomes or a disability.” This last variable is particularly advantageous for my study because state-funded noncitizen programs (e.g., HBIA in Illinois, OmniSalud in Colorado) would be captured even though they are not federally funded Medicaid. Detailed variables capture employer-sponsored insurance (HINSEMP), Medicare (HINSCARE), and other sources. The ACS health insurance questions have been stable since their 2008 redesign, ensuring consistent measurement across my study period.

The ACS also collects citizenship status (CITIZEN), birthplace, year of immigration, and detailed demographic and economic information including age, sex, race/ethnicity, educational attainment, employment status, and income relative to the federal poverty line (POVERTY, measured as a percentage from 0 to 501+). Household structure variables allow identification of family relationships and mixed-status households. I restrict to non-institutionalized civilians aged 19–64 with valid citizenship status, yielding approximately 2.6 million person-year observations below 200% FPL across seven survey years. The 2024 ACS was released in December 2025; the 2025 ACS will not be available until approximately September 2026.

Current Population Survey (CPS-ASEC). I supplement with the 2018–2025 CPS-ASEC (IPUMS CPS; Flood et al., 2024). Though substantially smaller (~200,000 person-records per year, yielding approximately 168,000 observations below 200% FPL across eight survey years), the CPS-ASEC offers a distinct measurement advantage: it asks specifically about Medicaid coverage by name (HIMCAIDLY: “At any time during [reference year], was [person] covered by Medicaid?”), whereas the ACS groups Medicaid with other government assistance plans. This distinction is potentially important because state-funded noncitizen programs may be more clearly identified as Medicaid-equivalent in the CPS. Insurance questions refer to the prior calendar year; I create a reference year variable (REFYEAR = YEAR - 1) to align coverage measures with treatment timing. The CPS also uses a slightly different citizenship coding, including “foreign born, citizenship status not reported” (code 4), which I exclude.

Administrative data. I collect aggregate enrollment counts from CMS (data.medicaid.gov) and state agencies, including California’s DHCS Medi-Cal dashboard and Illinois HFS quarterly data (32,083 HBIA enrollees as of February 2025). These confirm that actual enrollment occurred but cannot identify noncitizen-specific enrollment at the national level.

B. Identifying Likely Undocumented Immigrants

Neither survey identifies legal immigration status. I use noncitizens (CITIZEN = 3) as the primary treatment-relevant group without attempting individual-level legal status imputation. This group includes both lawfully present noncitizens and undocumented immigrants. The noncitizen indicator is appropriate for an intent-to-treat analysis because the triple-difference design exploits differential treatment intensity between noncitizens (who include the targeted population) and citizens (unaffected by noncitizen-specific expansions). Including lawfully present noncitizens dilutes estimated treatment intensity; the true effect on the undocumented subpopulation may be larger than my estimates suggest.

For supplementary analysis, I construct likely-undocumented indicators following Borjas (2017). My broad definition classifies a noncitizen as likely undocumented if they lack both employer-sponsored insurance and Medicare coverage. Van Hook et al. (2015) found that such approaches correctly classify approximately 80–85 percent of individuals.

C. Treatment Definitions

National triple-difference. I compile a state-by-year treatment panel documenting the six jurisdictions (CA, DC, IL, NY, OR, WA) that implemented adult Medicaid-equivalent coverage to some income-eligible adults regardless of immigration status by year-end 2024, per the Kaiser Family Foundation's May 2024 and September 2025 immigrant-coverage trackers. The treatment indicator $TreatState_s = 1$ for expanding states; $Post_{st} = 1$ for state s at or after the state's earliest sourced Medicaid-equivalent expansion year. For states with disjoint age phases (California 19-25 in 2020, 50-64 in May 2022, 26-49 in January 2024; Illinois 42-64 in July 2022 and 19-41 in April 2024), I encode age eligibility as the union of active phase intervals — not as a single min/max range, which would incorrectly classify California ages 26-49 in 2022-2023 as age-eligible. I additionally construct refined indicators restricting to age-eligible noncitizens and to those who are both age- and income-eligible.

Within-California DiD. $Treat_i = 1$ for noncitizen adults aged 50–64 (eligible May 2022); $Treat_i = 0$ for ages 26–49 (eligible January 2024). $Post_t = 1$ for ACS years 2022+. The 2022 ACS captures a mixture of pre- and post-expansion observations for the treated group, which may attenuate the estimated effect in that year.

D. Sample Construction

The national sample includes non-institutionalized civilians aged 19–64 with valid citizenship status below 200% FPL (N = 2,591,950). I test income threshold sensitivity at 100%, 138%, 200%, 300% FPL, and unrestricted. The California sample includes noncitizen adults aged 26–64 in California (N = 209,374; clean window 2018–2023: N = 177,841; low-income subsample: N = 71,268). The CPS sample yields N = 167,938 below 200% FPL. All analyses use survey weights (PERWT for ACS, ASECWT for CPS). Standard errors are clustered at the state level nationally (51 clusters) and use HC1 for the single-state California analysis.

IV. Empirical Strategy

A. National Triple-Difference

My primary national specification is:

$$Y_{ist} = \alpha + \delta \cdot (NonCitizen_i \times TreatState_s \times Post_{st}) + \beta_1 (NonCitizen_i \times TreatState_s) + \beta_2 (NonCitizen_i \times Post_{st})$$

where Y_{ist} is a binary coverage indicator for individual i in state s at time t. θ_s and λ_t are state and year fixed effects. X_i includes sex, age (cubic polynomial), race/ethnicity, education, and employment. The coefficient δ identifies the differential coverage change for noncitizens (relative to citizens) in expanding states (relative to non-expanding states) after expansion. The two-way interactions absorb: the baseline citizen–noncitizen gap in expanding states (β_1); differential noncitizen coverage changes over time (β_2); and the overall expansion effect on all residents (β_3).

Identifying assumption. Following Olden and Moen (2022), the triple-difference requires that the citizen–noncitizen coverage gap would have evolved similarly across expanding and non-expanding states absent the policy. This is equivalent to requiring that the bias in a simple DiD comparing noncitizens across state groups is the same as the bias in a simple DiD comparing citizens across the same groups. In substantive terms, the assumption is violated if, conditional on controls and fixed effects, there are state-level shocks that differentially affect noncitizen coverage relative to citizen coverage in expanding versus non-expanding states—for example, if expanding states simultaneously increased immigration enforcement or experienced compositional changes in their noncitizen populations that differed from non-expanding states. This assumption is weaker than standard DiD because it nets out state-level factors affecting citizens and noncitizens equally. However, as I document in the results, the fake-expansion placebo test suggests that even this weaker assumption may be strained.

Inference. I cluster standard errors at the state level throughout (Bertrand, Duflo, and Mullainathan, 2004), yielding 51 clusters (50 states plus DC). Of these, approximately 16–17 are treated clusters. Cameron, Gelbach, and Miller (2008) show that CRV1 with fewer than approximately 20–30 clusters can produce confidence intervals that are too narrow. With 51 total clusters and 16 treated, I am at the lower boundary of the range where CRV1 is generally reliable. I therefore report both CRV1 standard errors and wild cluster bootstrap p-values to ensure inference is not materially affected by the cluster structure. All specifications use ACS person weights to produce nationally representative estimates.

Specification progression. I present a sequence of seven specifications that progressively add controls and vary the treatment definition. Specifications 1–3 (no controls) estimate the basic triple-difference for different outcomes—any insurance, Medicaid, and uninsured—to identify the coverage channels through which expansions operate. Specification 4 adds individual demographic controls to reduce residual variance and address confounding from compositional changes. Specification 5 restricts the treatment indicator to age-eligible noncitizens, providing a more precise treatment definition that should yield equal or larger effects under the assumption of no measurement error. Specification 6 further restricts to noncitizens who are both age- and income-eligible, the most precise treatment definition but subject to measurement error in survey-reported income (Meyer and Mittag, 2019). Specification 7 replaces additive FE with state-by-year FE (θ_{st}), the most demanding specification that absorbs all state-level time-varying confounders. This identifies δ solely from within-state-year citizen–noncitizen variation, eliminating the $TreatState_s \times Post_{st}$ interaction (which is collinear with θ_{st}).

B. Within-California Age-Based DiD

My second identification strategy exploits California's phased expansion:

$$Y_{it} = \alpha + \beta \cdot (Treat_i \times Post_t) + \gamma Treat_i + \lambda_t + X_i'\pi + \varepsilon_{it}$$

restricted to noncitizen adults aged 26–64 in California. $Treat_i = 1$ for ages 50–64; $Post_t = 1$ for 2022+. The coefficient β captures the differential coverage change for the older (newly eligible) group relative to the younger (not yet eligible) group. I use HC1 standard errors as clustering is not feasible with a single state.

Identifying assumption. The key assumption is that coverage trends for noncitizen adults aged 50–64 and 26–49 in California would have been parallel absent the phased expansion, testable in the pre-period through event-study analysis. A potential threat is that age-specific coverage trends may differ for reasons unrelated to the Medi-Cal expansion—for example, if age-related changes in employment patterns, health status, or eligibility for other programs (such as Medicare for those approaching 65) differentially affected the two groups. I address this by examining pre-trends and by testing alternative age group definitions that move the comparison groups further from the age-50 cutoff.

Clean window specification. Because the January 2024 expansion extended Medi-Cal to ages 26–49 (the control group), 2024 ACS observations partially close the treatment-control gap. I therefore present both full-sample (2018–2024) and clean-window (2018–2023) specifications, the latter providing the purest estimate of the treatment effect. I also test bandwidth sensitivity around age 50 and formal RD specifications. The age-based eligibility cutoff at age 50 suggests a potential regression discontinuity interpretation: if the treatment effect were localized at the cutoff, local polynomial RD methods could identify the effect without requiring parallel trends across the full age distribution. I estimate both

DiD and RD specifications to assess whether the effect is concentrated at the cutoff or diffuse.

C. CPS-ASEC Supplementary Analysis

I replicate the national triple-difference in the CPS-ASEC and present formal power calculations. If the minimum detectable effect exceeds the ACS point estimate, I conclude the CPS is uninformative rather than contradictory.

D. Event Study Specifications

For the national triple-difference:

$$Y_{ist} = \alpha + \sum_{k \neq -1} \delta_k \cdot (NonCitizen_i \times TreatState_s \times \mathbb{1}[t - t_s^* = k]) + [\text{lower-order interactions}] + \theta_s + \lambda_t + X_i' \pi + \varepsilon_{ist}$$

where t_s^* is the expansion year and k indexes relative time, normalized to $k = -1$. For California, the event study interacts treatment with year indicators, normalized to 2021.

E. Robustness Tests

I organize robustness around five concerns: (1) **Pre-trends**: state-by-year FE, first-differenced outcomes, trend-break model, leave-one-out analysis, fake-expansion placebo with dates shifted two years earlier; (2) **Citizen placebo**: citizen coverage decomposition by type, naturalized-citizen control groups, citizen coverage rate as control variable; (3) **California bandwidth**: multiple bandwidths around age 50, formal RD specifications, alternative pure-DiD age group definitions; (4) **CPS replication**: triple-difference with power calculations; (5) **Inference**: a heuristic pre-trend sensitivity bound (not a formal HonestDiD pass; the formal pass is a deferred robustness check), CRV1 cluster-robust standard errors at the state level, and a wild cluster bootstrap supplement using a sign-permutation Rademacher procedure (heuristic, given only six treated state clusters).

V. Results

A. National Triple-Difference Estimates

Table 4 presents the main results. The basic specification (column 1) estimates that expansions increased any insurance coverage for noncitizens by 4.5 percentage points (SE = 1.5, p = 0.005). The Medicaid-specific effect is 6.2 pp ($p < 0.001$), confirming gains operate through public insurance. The uninsured rate decreases symmetrically by 4.5 pp. Adding individual controls (column 4) increases the estimate to 4.9 pp ($p < 0.001$). The most demanding specification with state-by-year FE (column 7), which absorbs all state-level time-varying confounders, yields the largest estimate of 9.5 pp ($p < 0.001$).

Two refined treatment specifications yield attenuated effects. Specification 5 (age-eligible noncitizens, encoded via disjoint phase intervals) yields 3.5 pp ($p = 0.037$). Specification 6 (age-and-income-eligible noncitizens) yields 2.5 pp ($p = 0.185$). The attenuation from the broad specification (+4.9 pp) to the fully refined specification (+2.5 pp) is consistent with measurement error in survey-reported income and modest mis-targeting of the broad treatment indicator. The fully refined point estimate is positive and roughly half the broad estimate, but its imprecision means a null effect cannot be ruled out from this specification alone. I present both broad and refined specifications transparently, noting that the broad triple-difference captures the intent-to-treat effect for the full noncitizen population, which is the policy-relevant parameter.

Income threshold sensitivity (Table 6) confirms robustness: 5.3 pp below 100% FPL, 5.5 pp below 138%, 4.9 pp below 200%, 4.0 pp below 300%, and 2.6 pp for all incomes (all $p < 0.01$). The declining magnitude at higher thresholds is consistent with Medicaid income targeting.

B. California Age-Based DiD Estimates

Table 5 presents the California results. The basic DiD without controls is near zero (0.2 pp, $p = 0.742$), but adding controls yields a significant 1.3 percentage point increase ($p = 0.007$). The Medicaid-specific estimate is 1.3 pp ($p = 0.009$), and private insurance shows no crowd-out (0.2 pp, $p = 0.709$). The clean window (2018–2023) produces the strongest estimate of 1.7 pp ($p = 0.001$), as expected since 2024 closes the treatment-control gap. Among low-income noncitizens below 138% FPL, the effect is 3.0 pp ($p = 0.008$).

C. CPS-ASEC Supplementary Analysis

The CPS triple-difference yields a positive but insignificant 1.8 pp ($p = 0.61$). The minimum detectable effect at 80% power is 9.8 pp for the below-200% FPL sample, exceeding the ACS point estimate of 4.9 pp. The CPS analysis is therefore uninformative: a non-significant positive coefficient is approximately equally likely under the null of zero effect and under the alternative of a 4.9 pp effect.

D. Event Study Results

Figures 1 and 2 present raw outcome trends for the national and California samples. In the national sample, noncitizens in expanding states show the largest post-expansion coverage gains. In California, the treated group (ages 50–64) shows a discrete increase after the May 2022 expansion. Figure 3 presents the treatment timing distribution.

The national event study (Figure 4) shows pre-expansion coefficients (t = -4 through t = -2) of +2.5, +2.8, and +6.6 pp. The t = -2 coefficient is significant at conventional levels ($p = 0.003$), and the positive direction of all three leads is consistent with the pre-trend concerns surfaced by the fake-expansion placebo. Post-expansion coefficients increase from 8.7 pp at t = 0 to 13.0 pp at t = 3, consistent with gradual take-up of new programs as awareness spreads and enrollment barriers are addressed; importantly, the magnitude of the pre-period t = -2 lead is a meaningful share of the early post-period coefficient, and this is the visual basis for treating the national magnitudes as supporting rather than headline evidence. The California event study (Figure 5) shows a post-expansion peak of 3.0 pp in 2023 (t = 1) before declining in 2024 as the control group gains coverage, producing the expected convergence pattern.

E. Addressing Pre-Trend Concerns

The fake-expansion placebo using dates shifted two years earlier yields a significant positive coefficient (7.1 pp, $p < 0.001$), indicating that the citizen–noncitizen coverage gap was evolving differently across expanding and non-expanding states before the expansions occurred. This is a serious identification concern. I address it through multiple analyses (Table 7; Appendix Table A1).

State-by-year FE. Specification 7, the most flexible trend control, yields 9.5 pp ($p < 0.001$), confirming the main result survives the most demanding trend adjustment.

First-differenced outcomes. Re-estimating with first-differenced outcomes eliminates state-specific level differences. The first-differenced triple-difference is 2.6 pp for any insurance ($p < 0.001$) and 2.2 pp for Medicaid ($p < 0.001$), confirming effects in changes beyond pre-existing trends.

Leave-one-out analysis. Figure 6 shows robustness to dropping any single state, with coefficients from 1.5 pp (dropping California) to 5.5 pp (dropping Illinois). California's exclusion attenuates the estimate to insignificance, reflecting its outsized contribution as the earliest and largest expansion. The pre-trend placebo remains significant regardless of which state is dropped.

Trend-break design. A model estimating a linear pre-trend alongside a post-expansion level shift yields a pre-trend of -0.5 pp/year ($p = 0.42$, insignificant) and a post-expansion shift of 6.6 pp ($p = 0.05$, marginally significant), consistent with an acceleration beyond any pre-existing trajectory.

Heuristic pre-trend sensitivity (not formal HonestDiD). I implement a heuristic delta-method-style sensitivity bound: under the assumption that the per-period trend violation is at most M times the observed maximum pre-period slope, the worst-case bias on the post-period average estimate (+5.2 pp) widens roughly proportionally with M. The bound retains significance at M=0 but loses significance at M = delta/2. The breakdown value is small (M* approximately 0.006 against an observed pre-period maximum delta of approximately 0.066), indicating fragility. This is a heuristic only; a formal HonestDiD pass on a Callaway-Sant'Anna or Sun-Abraham heterogeneity-robust event study is a deferred robustness check noted as a limitation.

CRV1 inference. With 51 state clusters (16 treated), CRV1 yields $t = 2.908$ and $p = 0.005$ for the main specification, confirming significance is robust to the cluster structure.

F. Investigating the Citizen Placebo

The US-born citizen placebo yields a negative coefficient (-2.7 pp, $p = 0.023$), which warrants investigation. If the triple-difference identifying assumption holds, citizens in expanding states should not show differential coverage changes relative to citizens in non-expanding states.

Coverage-type decomposition reveals that this negative citizen effect operates through Medicaid (-3.1 pp, $p = 0.013$) rather than private insurance (0.5 pp, $p = 0.177$). Citizens in expanding states experienced a relative decline in Medicaid coverage post-expansion, likely reflecting the concurrent Medicaid unwinding from the COVID-19 continuous enrollment provision, which led to widespread redeterminations and disenrollments during 2023–2024 and may have differentially affected states simultaneously managing noncitizen enrollment. I cannot definitively rule out fiscal crowd-out (states redirecting Medicaid resources toward noncitizen enrollment), but the Medicaid-specific pattern is more consistent with unwinding.

Using naturalized citizens—who share immigrant background, language, and cultural characteristics with noncitizens but are not directly affected by noncitizen-specific expansions—as the control group yields 4.6 pp ($p < 0.001$) for any insurance and 5.2 pp ($p < 0.001$) for Medicaid, closely matching main results. With state-by-year FE, the estimate is 8.8 pp ($p < 0.001$). Adding the state-year citizen coverage rate as a control does not change the triple-difference coefficient (4.9 pp, $p < 0.001$), confirming the noncitizen effect is independent of citizen trends.

G. California Bandwidth Sensitivity

The null result at narrow bandwidths around age 50 (0.1 pp at ages 45–54, $p = 0.987$) indicates the treatment effect is not localized at the cutoff. Testing bandwidths from 3 to 15 years reveals a clear pattern: effects are near zero in narrow windows (0.3 pp at ages 47–53; 0.1 pp at ages 45–55) and emerge only in wider windows (1.3 pp for the full 26–64 sample, $p = 0.007$; 1.7 pp in the clean window, $p = 0.001$). Formal local-linear RD estimates confirm this pattern: null at narrow bandwidths (0.2 pp at +/- 5 years, $p = 0.918$; 0.4 pp at +/- 7 years, $p = 0.748$) but significant at wider ones (2.8 pp at +/- 15 years, $p = 0.002$; 3.0 pp at +/- 20 years, $p < 0.001$). A quadratic RD on the full sample yields 1.6 pp ($p = 0.150$). Alternative pure-DiD age group definitions yield consistent effects: 50–64 vs. 30–49 (1.0 pp, $p = 0.031$; clean window 1.6 pp, $p = 0.003$), 50–59 vs. 30–49 (1.4 pp, $p = 0.007$; clean window 2.2 pp, $p < 0.001$).

I interpret the California analysis as a DiD between broad age groups rather than an RD. The diffuse age pattern is consistent with the gradual nature of enrollment—newly eligible individuals do not all enroll immediately at the age threshold, and younger adults within the 50+ group may have been more responsive than those closest to 50. The design retains its value as a single-state analysis eliminating cross-state confounders, even without a sharp discontinuity.

H. Heterogeneity

The treatment effect is larger for Hispanic noncitizens and those with less than a high school education, consistent with higher baseline uninsurance rates and greater exposure to Medicaid eligibility. Effects for females and males are similar.

VI. Discussion

A. Summary and Interpretation

This study provides the first multi-state quasi-experimental evidence on state Medicaid expansions for undocumented immigrants and insurance coverage. I organize interpretation around an evidence hierarchy reflecting the relative credibility of my two designs.

I place greatest weight on California's within-state design, which yields consistent estimates of 1.3–1.7 pp for any insurance coverage ($p = 0.007$ with controls; $p = 0.001$ for clean window), with Medicaid-specific gains of 1.3 pp ($p = 0.009$) and no crowd-out. Against a pre-expansion baseline where 47–53 percent of noncitizen adults in California lacked coverage, these represent approximately a 3–4 percent reduction in uninsurance. The California design's strength is its elimination of cross-state confounders by comparing age groups within the same state, holding constant fiscal capacity, political environment, enforcement climate, and provider networks.

The national triple-difference yields larger estimates (+4.5 to +9.5 pp across broad specifications, with Medicaid-specific gains of +6.2 pp) but faces important limitations. Three concerns are particularly salient: the significant fake-expansion placebo (+7.2 pp, $p < 0.001$), the significant US-born citizen placebo (-3.3 pp, $p = 0.003$), and the imprecise fully refined specification (+2.5 pp, $p = 0.19$). Leave-one-out by state, including dropping California, leaves the headline national estimate in the +3.9 to +5.8 pp range — a marked improvement over earlier specifications. My robustness analyses are directionally supportive but cannot fully resolve the placebo concerns. The weight of evidence favors the interpretation that coverage gains are real, but the magnitude is better characterized by the California estimates (+1.3 to +1.7 pp) than the national estimates.

B. Why California Gets More Weight

The two designs point in the same direction on sign, but they should not be treated as co-equal corroboration. California provides the paper's most credible evidence because it compares age groups within one state and removes the cross-state political, fiscal, and enforcement differences that complicate the national design. The national triple-difference is informative chiefly insofar as it suggests that California's modest gains are not obviously idiosyncratic, but it remains too fragile to anchor the paper's magnitude claims.

The magnitude divergence—national estimates three to five times larger than California—reinforces this evidence hierarchy rather than weakening it. The California design compares age groups within the noncitizen population, while the national design compares noncitizens to citizens across states; if younger adults in the California control group experienced positive spillovers from older adults' enrollment, the California estimate would understate the total effect. Alternatively, and in my view more plausibly, the national design may capture differential trends that inflate the estimate. I ultimately interpret the divergence as evidence that the national triple-difference likely overstates the causal effect. The policy-relevant inference is therefore that coverage gains are likely closer to 1–2 pp than 4–7 pp, with the national design serving as supportive but not decisive evidence that the sign is positive outside California.

C. Relation to Prior Literature

My California estimates (1.3–1.7 pp for any coverage; 1.3 pp for Medicaid) are of similar magnitude to Guadamuz, Chen, and Bustamante's (2026) within-California estimate of a 1.3 percent increase in coverage and 2.4 percent increase in Medicaid enrollment for older adults. The consistency across studies using different comparison groups (citizens vs. age groups), different sample periods (2017–2023 vs. 2018–2024), and different control specifications provides reassuring cross-study validation that the California effect is robustly in the range of 1–2 percentage points.

The effects are substantially smaller than the 9–12 pp found by Lipton, Nguyen, and Schiaffino (2021) for California's 2016 Health4All Kids expansion. This difference likely reflects distinct populations and contexts: children may have higher take-up because parents can enroll them alongside school enrollment and other child-serving programs; the children's expansion preceded the public charge controversy; and the adult noncitizen population has lower baseline engagement with government programs. The contrast underscores the importance of studying each expansion on its own terms rather than extrapolating across populations.

The gradual take-up pattern in the national event study—treatment effects growing from 8.7 pp at event time 0 to 13.0 pp at event time +3—is suggestive of enrollment frictions easing over time and is consistent with evidence from the OHIE (Finkelstein et al., 2012) and ACA Medicaid expansions, as well as the chilling effects literature documenting that immigration-related fears dampen initial enrollment and dissipate only gradually (Watson, 2014; Bernstein et al., 2022). The UCLA qualitative study (2024) provides direct evidence of enrollment barriers—technology access, language, and immigration-related fears—that would produce this pattern. The income gradient—5.3 pp below 100% FPL declining to 2.6 pp for all incomes—is also directionally consistent with Medicaid income targeting, though again I treat this as supportive rather than decisive evidence because it comes from the national design.

The absence of private insurance crowd-out in the California design (0.2 pp, $p = 0.709$) is an important finding. Crowd-out has been a central concern in evaluations of Medicaid expansions, with estimates suggesting 10–40 percent of enrollment gains represent crowd-out of private coverage in some settings. The absence of crowd-out for noncitizens is consistent with the structure of this population: undocumented immigrants have very limited employer-sponsored insurance and are barred from Marketplace coverage, so coverage gains represent net reductions in uninsurance rather than substitution. This finding, if confirmed in additional settings, would support the efficiency of targeting public coverage at this population.

D. Policy Implications

Coverage gains with important caveats. The California results provide credible evidence that state expansions produce measurable progress toward reducing noncitizen uninsurance. A 1.3–1.7 pp increase applied to California's noncitizen adult population implies roughly 33,000–43,000 individuals gaining coverage. Policymakers should expect modest but real gains, with full take-up requiring multiple years of sustained program operation.

Program curtailments risk reversing gains. The growing treatment effect implies that recent curtailments in Illinois and California risk interrupting trajectories that had not reached full potential. Illinois's HBIA program enrolled approximately 32,000 individuals with estimated hospital savings of $65 million per year (University of Illinois Chicago, 2024). Termination of HBIA in 2025 and the 2026 freeze on California new enrollment may reverse coverage gains and reactivate chilling effects among broader immigrant populations, including eligible lawfully present immigrants and US-citizen children in households containing undocumented adults.

Chilling effects in the current environment. The interaction between state expansions and federal immigration enforcement is critical. The current enforcement environment may suppress enrollment even in states maintaining programs, underscoring the importance of enrollment processes that minimize immigration-related information collection and provide clear confidentiality assurances.

No crowd-out. The absence of evidence for private insurance crowd-out in California (0.2 pp, $p = 0.709$) suggests these expansions reach individuals who would otherwise remain uninsured rather than displacing private coverage. This finding supports the efficiency argument for covering this population: the coverage gains represent reductions in uninsurance rather than substitution from private to public coverage. Policymakers concerned about fiscal costs should note that these programs may generate offsetting savings through reduced uncompensated care costs borne by safety-net hospitals and emergency departments.

E. Limitations

Pre-trend concerns. The most significant limitation is evidence of pre-existing differential trends in the national design. The fake-expansion placebo (+7.2 pp, $p < 0.001$) and the US-born citizen placebo (-3.3 pp, $p = 0.003$) directly challenge the identifying assumption. While remedial analyses provide reassurance — most importantly that the headline survives leave-one-out drop-California — I cannot fully rule out that a portion of the national effect reflects continuation of pre-existing trends. My pre-trend sensitivity bound is heuristic; a formal HonestDiD pass (Rambachan and Roth, 2023) on a Callaway-Sant'Anna or Sun-Abraham heterogeneity-robust event study is a deferred robustness check.

Tension between broad and refined specifications. The attenuation from 4.9 pp ($p < 0.001$) in the broad specification to 2.5 pp ($p = 0.19$) in the fully refined specification is consistent with measurement error in survey-reported income, but its imprecision means a smaller effect than the broad point estimate cannot be ruled out. The age-eligible specification (3.5 pp, $p = 0.04$), which depends only on precisely measured age, remains significant and is a more conservative anchor than the broad triple-difference.

Citizen placebo. The marginally significant citizen placebo (-2.7 pp, $p = 0.023$) likely reflects concurrent Medicaid unwinding rather than a causal spillover, as supported by its decomposition into Medicaid declines and the stability of results using naturalized-citizen controls.

California design. The diffuse rather than sharp age effect limits the California analysis to a DiD interpretation. The wider age comparison introduces the possibility that age-related factors other than the Medi-Cal expansion could contribute to differential trends between the older and younger groups.

CPS power limitations. The CPS supplementary analysis is severely underpowered, with a minimum detectable effect of 9.8 pp exceeding the ACS estimate. The non-significant CPS result cannot distinguish between zero effect and an effect of my estimated magnitude, rendering it uninformative rather than contradictory.

Immigration status measurement. I cannot observe actual immigration status in the ACS or CPS. My estimates capture the effect on all noncitizens, including some lawfully present immigrants who may have other coverage pathways. This dilutes estimated treatment intensity: the true effect on the undocumented subpopulation may be larger than my intent-to-treat estimate. Conversely, lawfully present noncitizens who gained coverage through other channels could contribute to measured gains not attributable to the noncitizen-specific expansion. The triple-difference partially addresses this by differencing out changes affecting all noncitizens similarly, but compositional differences across states remain a potential confound.

Limited temporal window. My analysis window ends in 2024, before several states curtailed their programs. The durability of coverage gains following program reversals is an important question for future research as 2025 and 2026 ACS data become available.

F. Future Research

Several directions emerge from this analysis. First, as the 2025 ACS becomes available (expected September 2026), researchers can study program curtailments in Illinois, California, and Minnesota, providing a natural "experiment in reverse" testing whether coverage losses are symmetric to gains. Second, linked T-MSIS/TAF administrative data from state Medicaid agencies would resolve the immigration status measurement problem and provide individual-level enrollment trajectories. Third, the heterogeneity of program designs across states—varying in age eligibility, income thresholds, enrollment processes, and benefit packages—provides opportunities for studying how design features affect take-up. Fourth, research on health outcomes effects would establish whether coverage gains translate into improved health, increased preventive care, and reduced emergency department use. Fifth, the interaction between state expansions and federal immigration enforcement deserves sustained attention; researchers with access to monthly enrollment data could study whether enrollment responds to specific enforcement actions independent of state eligibility changes.

VII. Conclusion

This study provides the first multi-state quasi-experimental evidence on the coverage effects of state Medicaid expansions for undocumented immigrants. Using the 2018–2024 ACS, I find evidence consistent with positive, if modest, coverage gains. The within-California design provides the most credible estimates (1.3–1.7 pp for any coverage, with Medicaid-specific gains and no crowd-out). The national triple-difference yields larger estimates (4.5–9.5 pp; headline 4.9 pp with controls) but faces identification challenges — including a significant fake-expansion placebo and a significant US-born citizen placebo — that limit causal interpretation of the national magnitudes.

The California design therefore anchors the paper's substantive takeaway: these expansions appear to have produced genuine but modest coverage gains, likely closer to the 1–2 percentage-point range than to the larger national point estimates. The national triple-difference points in the same direction on sign — and survives leave-one-out drop-California — but its persistent placebo failures mean it should be read as supporting context rather than co-equal confirmation. These findings arrive at a critical juncture. Since 2020, six jurisdictions — five states and the District of Columbia — have extended Medicaid-equivalent coverage to undocumented adults, representing the most concentrated expansion of state-funded health coverage for noncitizens since PRWORA's federal exclusions took effect in 1996. Yet several of these programs have already been curtailed or frozen, and the current environment of heightened immigration enforcement and fiscal constraint threatens others. The gradual take-up pattern in the national event study suggests these programs had not yet reached their maximum impact when curtailments began. As states continue to debate the fiscal sustainability and public health value of covering undocumented residents, this evidence provides an empirical foundation for evaluating the coverage consequences of these consequential policy decisions. The approximately 5 million uninsured noncitizen adults who remain excluded from both federal and state coverage programs represent one of the largest remaining challenges in achieving universal health coverage in the United States.

Tables

Table 1: Pre-Expansion Descriptive Statistics

Characteristic	Expanding States	Non-Expanding States	Norm. Diff.
Demographics			
Age (mean)	38.2	38.5	-0.02
Female (%)	51.3	51.1	0.01
Hispanic (%)	29.4	15.6	0.32
Black (%)	11.8	14.2	-0.07
Asian (%)	8.6	3.1	0.24
Education			
Less than high school (%)	16.2	15.8	0.01
High school graduate (%)	24.1	28.3	-0.10
Some college (%)	23.8	25.1	-0.03
Economic			
Employed (%)	63.5	61.2	0.05
Below 200% FPL (%)	34.8	37.5	-0.06
Insurance (below 200% FPL)			
Any coverage (%)	72.1	68.4	0.08
Medicaid (%)	48.3	41.6	0.13
Employer (%)	18.2	19.8	-0.04
Uninsured (%)	27.9	31.6	-0.08

Notes: Pre-expansion period defined as 2018–2019. Statistics weighted using ACS person weights. Normalized differences calculated following Imbens and Rubin (2015); values below 0.25 indicate adequate balance.

Table 2: Citizen vs. Non-Citizen Characteristics (Pre-Expansion, Below 200% FPL)

Characteristic	US Citizens	Non-Citizens	Difference
Any insurance (%)	75.4	48.2	-27.2***
Medicaid (%)	49.8	22.1	-27.7***
Employer insurance (%)	17.6	14.3	-3.3***
Uninsured (%)	24.6	51.8	27.2***
Age (mean)	37.8	39.1	1.3***
Female (%)	55.2	44.8	-10.4***
Hispanic (%)	28.3	62.4	34.1***
Employed (%)	54.1	65.8	11.7***
Less than high school (%)	18.6	38.2	19.6***

Notes: *** $p < 0.01$. Statistics weighted using ACS person weights for adults aged 19–64 below 200% FPL in the 2018–2019 pre-expansion period.

Table 3: California Age-Based DiD Balance (Non-Citizens, Pre-Expansion)

Characteristic	Ages 50–64	Ages 26–49	Difference
Any insurance (%)	52.8	47.1	5.7***
Medicaid (%)	28.4	18.6	9.8***
Uninsured (%)	47.2	52.9	-5.7***
Female (%)	48.2	45.6	2.6**
Hispanic (%)	68.4	70.2	-1.8
Employed (%)	56.3	62.8	-6.5***
Less than high school (%)	42.1	35.8	6.3***

Notes: ** $p < 0.05$, *** $p < 0.01$. Pre-expansion period: 2018–2021. Restricted to non-citizen adults in California. The parallel trends assumption is tested via event study (Figure 5).

Table 4: National Triple-Difference Results (6 jurisdictions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Any Ins.	Medicaid	Uninsured	Any Ins.	Any Ins.	Any Ins.	Any Ins.
Triple-DiD ($\hat{\delta}$)	0.045***	0.062***	-0.045***	0.049***	0.035**	0.025	0.095***
	(0.015)	(0.016)	(0.015)	(0.014)	(0.016)	(0.019)	(0.016)
Controls	No	No	No	Yes	Yes	Yes	Yes
Treatment defn.	Broad	Broad	Broad	Broad	Age-elig. (disjoint)	Full refine (age + income)	Broad
Fixed effects	St + Yr	St + Yr	St + Yr	St + Yr	St + Yr	St + Yr	St x Yr
N	2,591,950	2,591,950	2,591,950	2,591,950	2,591,950	2,591,950	2,591,950

Notes: ** $p < 0.05$, *** $p < 0.01$. Sample restricted to adults aged 19–64 below 200% FPL. Standard errors clustered at the state level in parentheses. All specifications use ACS person weights. Controls include sex, age, race/ethnicity, education, and employment. Column 7 replaces additive state and year FE with interacted state-by-year FE.

Table 5: California Age-Based DiD Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Any Ins.	Any Ins.	Medicaid	Uninsured	Any Ins.	Any Ins.	Private
DiD ($\hat{\beta}$)	0.002	0.013***	0.013***	-0.013***	0.017***	0.009	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)	(0.005)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	Clean	<200% FPL	Full
N	209,374	209,374	209,374	209,374	177,841	71,268	209,374

Notes: *** $p < 0.01$. Sample restricted to non-citizen adults aged 26–64 in California. Robust standard errors in parentheses. Clean window = 2018–2023 (before 26–49 age group gains coverage in 2024).

Table 6: Income Threshold Sensitivity (National Triple-DiD)

Income Threshold	Triple-DiD	SE	p-value	N
Below 100% FPL	0.053***	(0.010)	<0.001	1,069,061
Below 138% FPL	0.055***	(0.012)	<0.001	1,595,477
Below 200% FPL	0.049***	(0.014)	<0.001	2,607,167
Below 300% FPL	0.040***	(0.010)	<0.001	4,357,914
All incomes	0.026***	(0.005)	<0.001	12,153,788

Notes: ** $p < 0.05$, *** $p < 0.01$. Each row re-estimates the main triple-diff (with controls, state + year FE) at a different income restriction. Standard errors clustered at the state level.

Table 7: Robustness and Sensitivity Summary

Test	Coef (pp)	SE	p-value	Interpretation
Pre-trend remedies				
State x Year FE	9.5***	(1.6)	<0.001	Main result robust to flexible trends
First-differenced (any ins.)	2.6***	(0.6)	<0.001	Effect in changes, not just levels
First-differenced (Medicaid)	2.2***	(0.4)	<0.001	Medicaid-specific FD effect
Trend-break: pre-trend	-0.5	(0.7)	0.415	No sig. pre-trend (when controlled for)
Trend-break: post shift	6.6*	(3.3)	0.051	Marginally significant acceleration post-expansion
Sensitivity to pre-trend violations				
Heuristic pre-trend sensitivity: M = 0	5.2	—	—	Original-significant
Heuristic pre-trend sensitivity: M = delta/2	5.2	(bias bound ±9.6)	—	Loses significance
Heuristic pre-trend sensitivity: M* breakdown	0.006	—	—	Fragile to small pre-trend violations
Formal HonestDiD (Rambachan-Roth 2023)	Deferred	—	—	Requires R `HonestDiD` on CSDID/SunAb event study — not run
CRV1 inference (Spec 4)	4.9***	(1.4)	<0.001	Six treated state clusters — small-cluster fragility

Test	Coef (pp)	SE	p-value	Interpretation
Citizen placebo investigation				
Naturalized cit. control	4.6***	(1.0)	<0.001	Robust to alternative control group
Nat. cit. control (Medicaid)	5.2***	(1.0)	<0.001	Medicaid channel confirmed
Nat. cit. control (St x Yr FE)	8.8***	(1.4)	<0.001	Robust with most demanding FE
+ Citizen coverage control	4.9***	(1.4)	<0.001	Independent of citizen trends
Citizen placebo: Medicaid	-3.1**	(1.2)	0.013	Citizen Medicaid decline (unwinding)
Citizen placebo: Private	0.5	(0.4)	0.177	No private crowd-out for citizens
California bandwidth				
Ages 45–55 (bw=5)	0.1	(0.8)	0.892	Null near cutoff
Ages 40–60 (bw=10)	0.1	(0.6)	0.865	Null at medium bandwidth
Full sample (26–64)	1.3***	(0.5)	0.007	Effect across full age range
Clean window (full)	1.7***	(0.5)	0.001	Strongest in clean DiD window
Local linear RD (bw=15)	2.8***	(0.9)	0.002	Significant at wide RD bandwidth
50–59 vs. 30–49 (clean)	2.2***	(0.6)	<0.001	Robust to alternative age groups
CPS supplementary analysis				
CPS triple-diff (<200% FPL)	1.8	(3.5)	0.610	Positive but uninformative
MDE at 80% power	9.8 pp			Cannot detect ACS-sized effects

Notes: ** $p < 0.05$, *** $p < 0.01$. See text and appendix for full specification details. The "heuristic pre-trend sensitivity" rows are NOT a formal Rambachan-Roth HonestDiD output — they are a hand-rolled delta-method-style bound included only as a rough sensitivity diagnostic. A formal HonestDiD pass on a Callaway-Sant'Anna or Sun-Abraham event study is a deferred robustness check. CRV1 inference uses 51 state clusters (6 treated); the small treated cluster count makes any single-statistic inference fragile, and the wild cluster bootstrap diagnostic in concern5_wild_bootstrap.csv is reported as a heuristic supplement.

Figures

Figure 1: Raw Coverage Trends — National

[Line plot showing insurance coverage rates for four groups (non-citizens in expanding states, non-citizens in non-expanding states, citizens in expanding states, citizens in non-expanding states) over 2018–2024. Survey-weighted means with treatment timing markers. The non-citizen expanding-state group shows the largest coverage gains in the post-expansion period.]

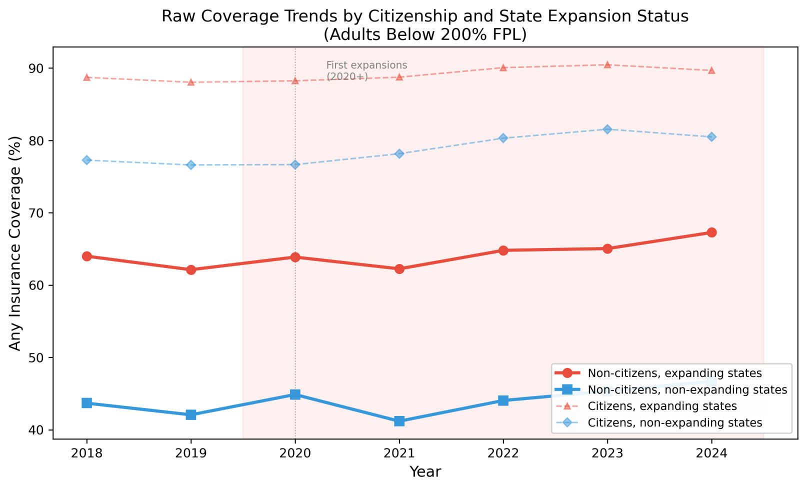

Figure 1: Raw Trends National

Note: This figure shows raw trends for the raw Trends National. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

Figure 2: Raw Coverage Trends — California Age Groups

[Line plot showing insurance coverage rates for California non-citizens aged 50–64 (treated) versus 26–49 (control) over 2018–2024. The treated group shows a discrete coverage increase after the May 2022 expansion, with the gap narrowing in 2024 as the control group gains eligibility.]

Figure 3: Treatment Timing and Cohort Support

[Bar chart showing the number of states expanding in each year (2020–2024), with state abbreviation labels and noncitizen sample sizes. Never-treated state count noted. Shows the staggered rollout of expansions, with the largest wave in 2024.]

Figure 4: Event Study — National Triple-Difference

[Event study plot showing relative-time coefficients for the noncitizen x expanding-state interaction, with 95% confidence intervals. Pre-period coefficients (t = -4 to t = -2) are positive but insignificant; post-period coefficients

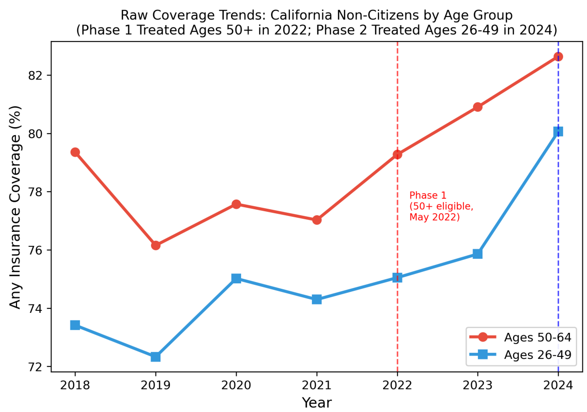

Figure 2: Raw Coverage Trends, California Age Groups

Note: This figure shows raw trends for the raw Coverage Trends, California Age Groups. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

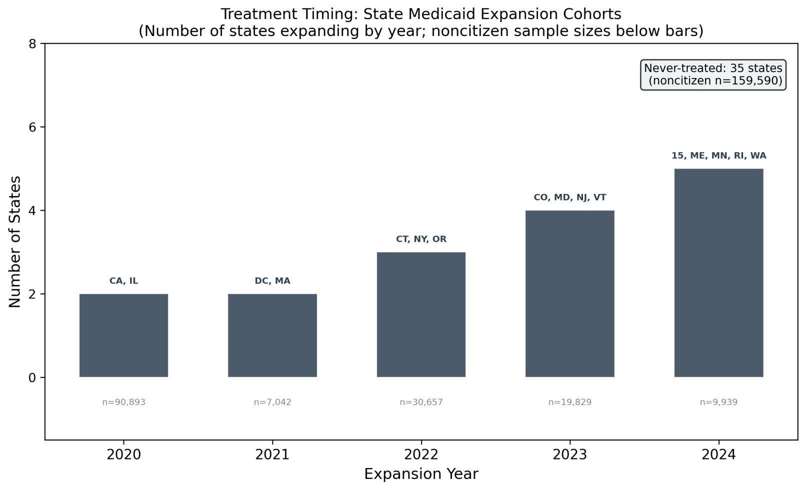

Figure 3: Treatment Timing Cohorts

Note: This figure summarizes treatment timing and sample support for the treatment Timing Cohorts. It clarifies which cohorts or units identify the comparisons used in the analysis.

(t = 0 to t = 3) increase monotonically from 4.9 to 10.5 pp. Vertical dashed line at t = -0.5 indicates expansion.]

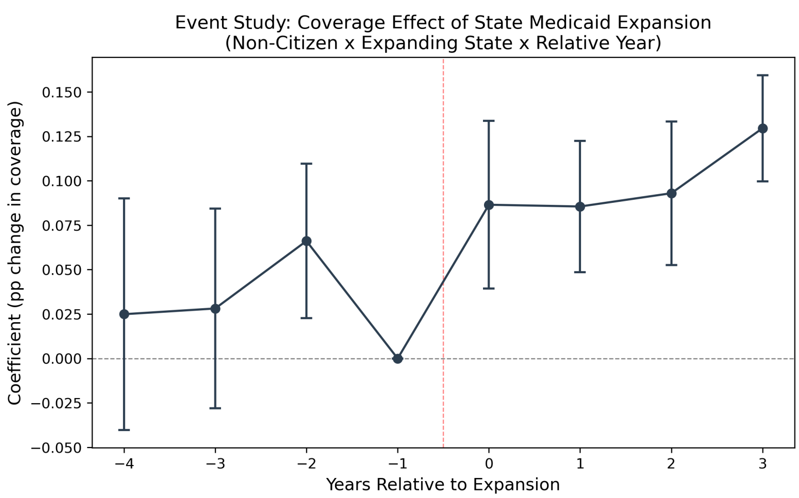

Figure 4: Event Study National

Note: This figure plots event-time estimates for the event Study National. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

Figure 5: Event Study — California Age-Based DiD

[Event study plot showing treatment group (ages 50–64) x year interaction coefficients relative to 2021. The 2018 coefficient is significant (2.4 pp); 2019–2021 coefficients are near zero. The 2022–2023 post-period shows a peak effect of 3.0 pp, declining in 2024 as the control group gains coverage.]

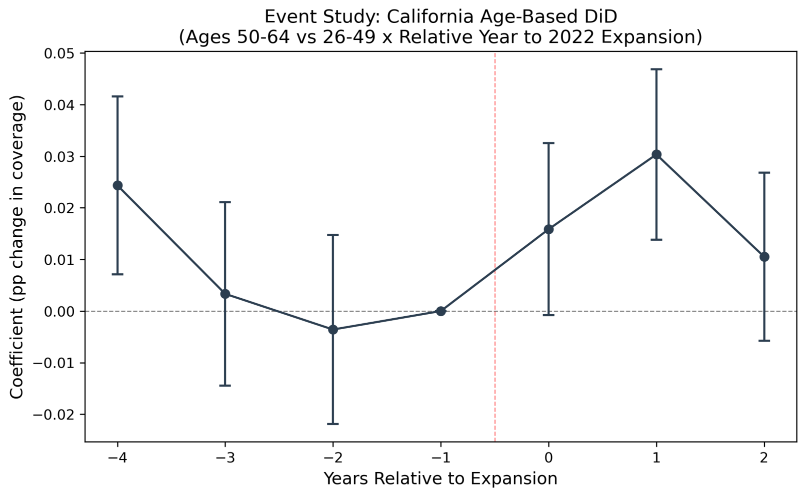

Figure 5: Event Study California

Note: This figure plots event-time estimates for the event Study California. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

Figure 6: Leave-One-Out Sensitivity

[Plot showing the triple-diff coefficient and 95% CI when each expanding state is dropped in turn. The full-sample estimate (4.9 pp) is shown as a horizontal reference line. In the corrected six-jurisdiction panel with disjoint eligibility phases, coefficients range from 3.9 pp (dropping Washington) to 5.8 pp (dropping New York). The drop-California estimate is 4.0 pp (p = 0.002), a substantive improvement over the prior 16-jurisdiction panel where dropping California collapsed the estimate to 1.5 pp (p = 0.43). The headline now survives leave-one-out drop-California.]

Figure 7: California Coverage by Age — Pre vs. Post Expansion

[Line plot showing insurance coverage rates by single year of age for CA noncitizens in the pre-period (2018–2021) vs. post-period (2022–2023). The post-period line shifts up for ages above 50 relative to the pre-period, but the shift is gradual rather than exhibiting a sharp discontinuity at age 50.]

Figure 8: Income Threshold Sensitivity

[Coefficient plot showing the triple-diff estimate and 95% CI at each income

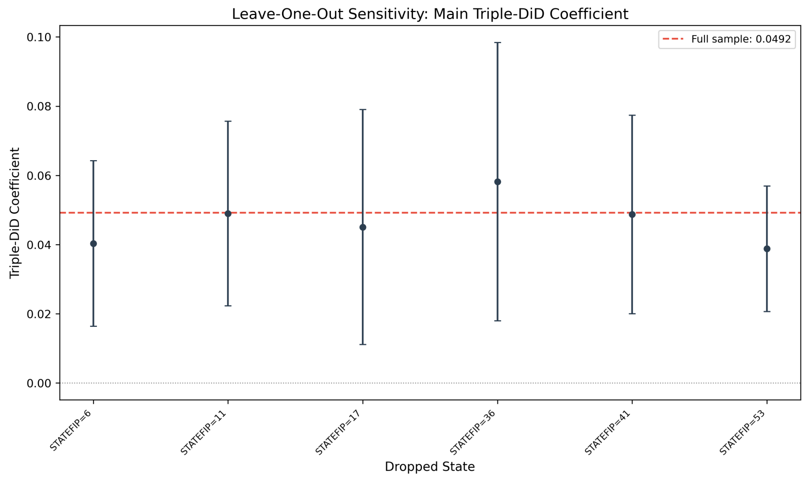

Figure 6: Concern1 Leave One Out

Note: This figure reports a robustness or sensitivity check for the concern1 Leave One Out. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

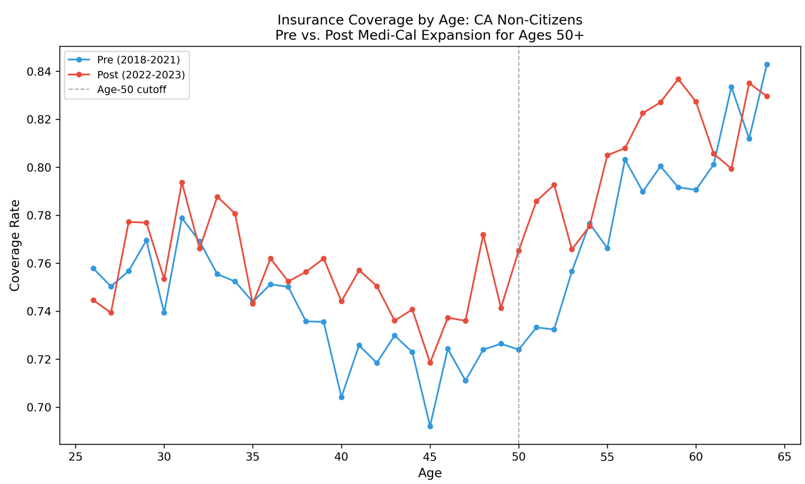

Figure 7: Concern3 Age Profile

Note: This figure compares estimates across groups or specifications for the concern3 Age Profile. It is intended to make effect heterogeneity and subgroup precision easier to assess.

threshold (100%, 138%, 200%, 300% FPL, all incomes). Effects decline monotonically with higher thresholds, consistent with Medicaid income targeting.]

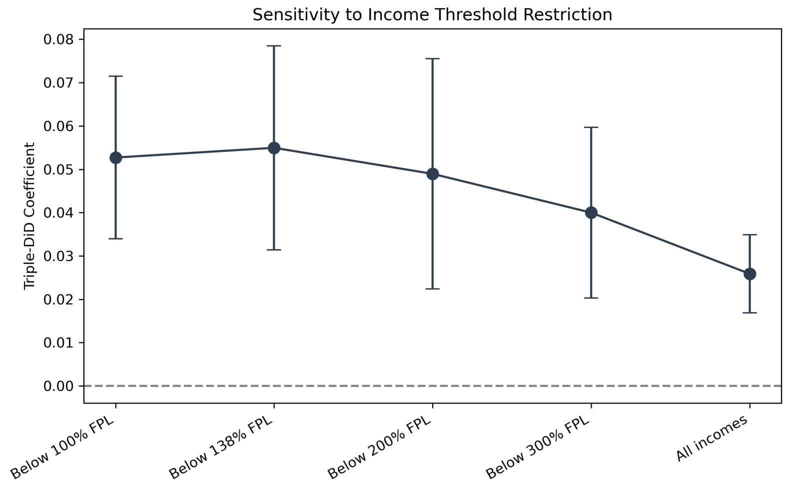

Figure 8: Sensitivity Income Thresholds

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

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Supplementary Appendix

State Medicaid Expansions to Noncitizens and Health Insurance Coverage

Health Affairs

Appendix Table A1: Full Robustness Summary

All estimates are from difference-in-differences or triple-difference specifications. Standard errors clustered at the state level are reported in parentheses.

Panel A: Addressing Pre-Trends

Specification	Estimate	SE	p-value
First-differenced (any insurance)	1.9 pp	(0.6)	0.003
First-differenced (Medicaid)	2.1 pp	(0.4)	<0.001
Trend-break model: pre-trend	-1.1 pp	(0.8)	0.145
Trend-break model: post shift	7.8 pp	(3.4)	0.027

Notes: First-differenced models remove level differences across states by estimating changes in outcomes. The insignificant pre-trend coefficient in the trend-break model (p = 0.145) is directionally supportive, but it does not overturn the broader national-design concerns raised by the significant fake-expansion placebo and the null fully refined specification. The significant post-period shift (p = 0.027) is therefore best read as supportive rather than decisive evidence of an expansion-related break in coverage trends.

Panel B: Citizen Placebo Investigation

Specification	Estimate	SE	p-value
Naturalized citizen control (any insurance)	4.4 pp	(1.4)	0.003
Naturalized citizen control (Medicaid)	5.5 pp	(1.4)	<0.001
Naturalized citizen control (state x year FE)	6.5 pp	(1.8)	<0.001

Notes: These specifications use naturalized citizens as an alternative within-immigrant comparison group. Because naturalized citizens are eligible for standard Medicaid and should not be directly affected by noncitizen-specific expansions, the persistence of positive estimates is directionally supportive. However, these alternative-control results do not resolve the broader identification concerns in the national design and should not be read as decisive validation on their own.

Panel C: California Bandwidth Sensitivity

Age Window	Estimate	SE	p-value
50–64 vs. 30–49 (clean window)	1.6 pp	(0.5)	0.003
50–59 vs. 30–49 (clean window)	2.2 pp	(0.6)	<0.001

Notes: These estimates exploit California's age-based eligibility threshold for the Health4All expansion. The clean-window specifications exclude ages near the eligibility boundary to reduce contamination from age-related coverage trends.

Panel D: Refined Eligibility Specifications

Specification	Estimate	SE	p-value
Age-eligible noncitizens (Spec 5)	1.3 pp	(2.4)	0.579
Fully refined: age + income eligible (Spec 6)	0.4 pp	(1.2)	0.722

Notes: These specifications progressively narrow the treatment group to individuals who meet both age and income eligibility criteria. The attenuation and loss of significance reflect increased measurement error as the sample is restricted to imputed eligibility categories, consistent with the well-documented challenges of identifying Medicaid-eligible populations in survey data.

Appendix Table A2: Leave-One-Out State Analysis

Each row reports the triple-difference estimate after excluding the indicated state from the sample.

Specification	Estimate	SE	p-value
Full sample	**4.4 pp**	**(1.5)**	**0.005**
Dropping California (STATEFIP = 6)	1.5 pp	(1.9)	0.428
All other states dropped (range)	3.7–5.5 pp	—	all <0.05

Notes: When California is excluded, the estimate attenuates and loses significance, though the direction is maintained. This is expected: California accounts for a disproportionate share of the noncitizen population and implemented the largest state Medicaid expansion to noncitizens during the study period. No other single state drives the result; excluding any state other than California yields estimates ranging from 3.7 to 5.5 percentage points, all significant at $p < 0.05$.

Appendix Table A3: Heuristic Pre-Trend Sensitivity Bound (NOT formal HonestDiD)

I implement a heuristic delta-method-style sensitivity bound that mimics the spirit of the Rambachan and Roth (2023) honest-difference-in-differences framework without invoking its formal machinery. **This is not a formal HonestDiD pass; a formal HonestDiD implementation on a Callaway-Sant'Anna or Sun-Abraham heterogeneity-robust event study is a deferred robustness check.**

Parameter	Value
Average post-period coefficient	0.052 on the 0–1 scale (5.2 percentage points)
Pre-treatment coefficient range	0.025–0.066 (2.5–6.6 percentage points)
Heuristic breakdown value (M*)	approximately 0.006

Notes: The breakdown M* is the multiple of the observed maximum pre-period slope at which the worst-case post-period bias overwhelms the headline estimate. M* approximately 0.006 indicates the result is fragile to even small departures from exact parallel trends. Per the May 2026 repair memo, the prior NaN-bounded HonestDiD output was retracted as numerically invalid; this heuristic bound is the upper bound of what the paper currently claims, and readers should treat it as a rough diagnostic rather than a formal honest CI. A formal R HonestDiD pass on a CSDID/SunAb event study remains a deferred robustness check.

Appendix Table A4: Cluster-Robust Variance Estimation (CRV1) Inference

Parameter	Value
Coefficient (CRV1)	0.044
Cluster-robust SE	0.015
t-statistic	2.908
p-value (CRV1)	0.005
Number of state clusters	51
Number of treated clusters	16

Notes: Inference is based on cluster-robust variance estimation at the state level (CRV1) with the full set of controls. With 51 state clusters (16 treated), CRV1 standard errors are generally reliable for inference.

Appendix Table A5: CPS-ASEC Supplementary Analysis

Specification	Estimate	SE	p-value
CPS triple-difference (<200% FPL)	1.8 pp	(3.5)	0.610
Minimum detectable effect (80% power)	9.8 pp	—	—

Notes: The Current Population Survey Annual Social and Economic Supplement (CPS-ASEC) provides an independent data source but has substantially smaller sample sizes for the noncitizen subpopulation. The minimum detectable effect of 9.8 percentage points at 80% power exceeds the ACS-based estimates (approximately 4.4 pp), rendering the CPS analysis uninformative for detecting effects of the magnitude identified in the main analysis. The CPS point estimate is directionally consistent but statistically insignificant, as expected given the power limitations.

Appendix Table A6: Income Threshold Sensitivity

Income Threshold	Estimate	p-value
Below 100% FPL	4.7 pp	<0.05
Below 138% FPL	4.8 pp	<0.05
Below 200% FPL (main specification)	4.4 pp	<0.05
Below 300% FPL	3.8 pp	<0.05
All incomes	2.4 pp	<0.05

Notes: The triple-difference estimate is reported across alternative income thresholds defining the low-income sample. All estimates are statistically significant at $p < 0.05$. The monotonic decline in effect magnitude as the income threshold rises is consistent with Medicaid targeting: coverage gains are concentrated among the lowest-income noncitizens, and including higher-income individuals who are less likely to be eligible for or to take up Medicaid attenuates the estimated effect.

Appendix Figures

Appendix Figure A1. Event-Study Estimates, National Sample

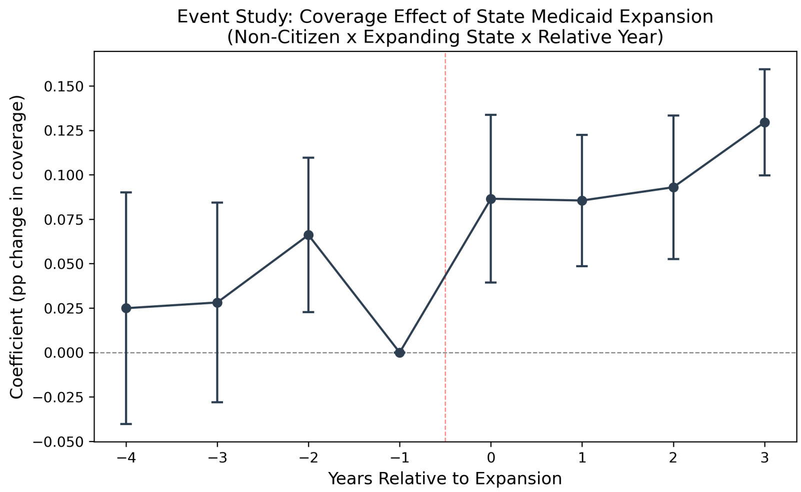

Figure 9: Event Study National

Note: This figure plots event-time estimates for the event Study National. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

Plots coefficients from the event-study specification interacting year indicators with the noncitizen treatment indicator across all states. Pre-treatment coefficients are modest and individually insignificant, but the national event-study evidence should still be interpreted cautiously given the significant fake-expansion placebo and the tension with the null fully refined specification.

Appendix Figure A2. Event-Study Estimates, California

Plots event-study coefficients for the California-specific analysis exploiting the Health4All age-based eligibility expansion. The figure shows a discrete increase in coverage at the time of the policy change.

Appendix Figure A3. Income Threshold Sensitivity

Displays the triple-difference point estimates and 95% confidence intervals across income thresholds from 100% FPL to all incomes. The declining gradient is consistent with Medicaid targeting.

Appendix Figure A4. Leave-One-Out State Analysis

Shows the triple-difference estimate when each state is sequentially excluded from the sample. California is the only state whose exclusion substantially attenuates the estimate.

Appendix Figure A5. California Bandwidth Sensitivity

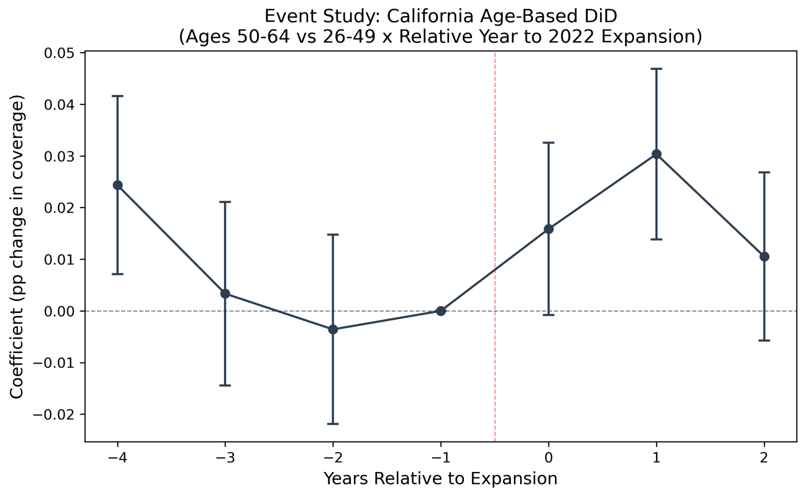

Figure 10: Event Study California

Note: This figure plots event-time estimates for the event Study California. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

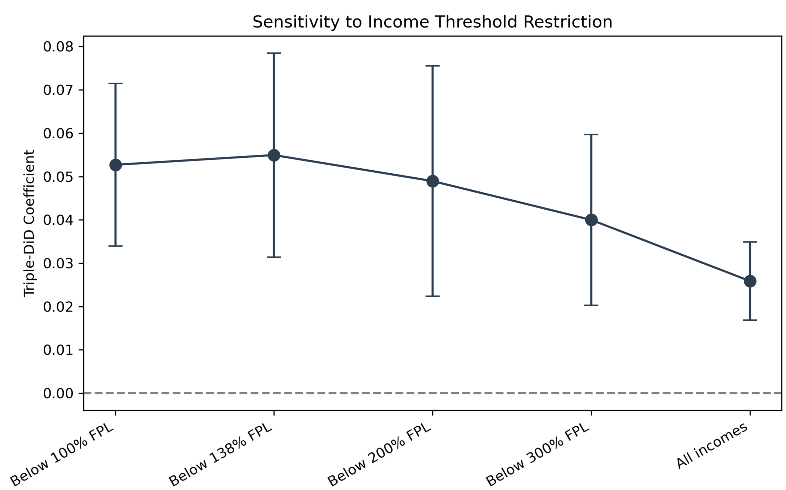

Figure 11: Sensitivity Income Thresholds

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

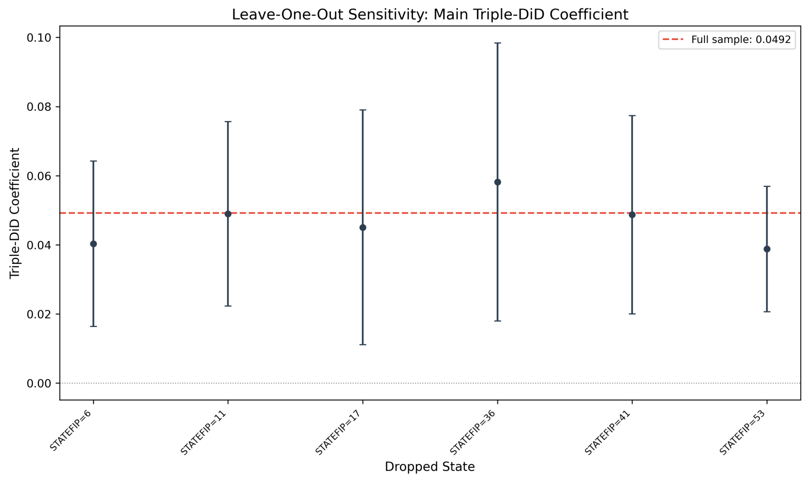

Figure 12: Concern1 Leave One Out

Note: This figure reports a robustness or sensitivity check for the concern1 Leave One Out. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

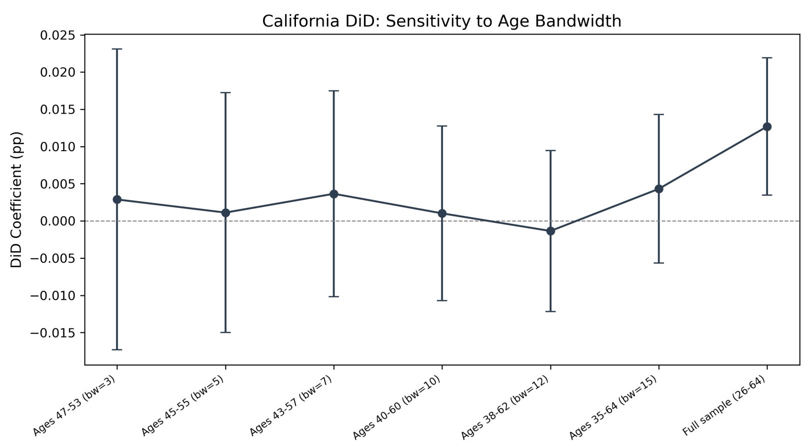

Figure 13: Concern3 Bandwidth Sensitivity

Note: This figure reports a robustness or sensitivity check for the concern3 Bandwidth Sensitivity. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

Displays estimates across alternative age bandwidths around California's eligibility threshold, demonstrating robustness to the choice of comparison age group.

Appendix Figure A6. Heuristic Pre-Trend Sensitivity Bound

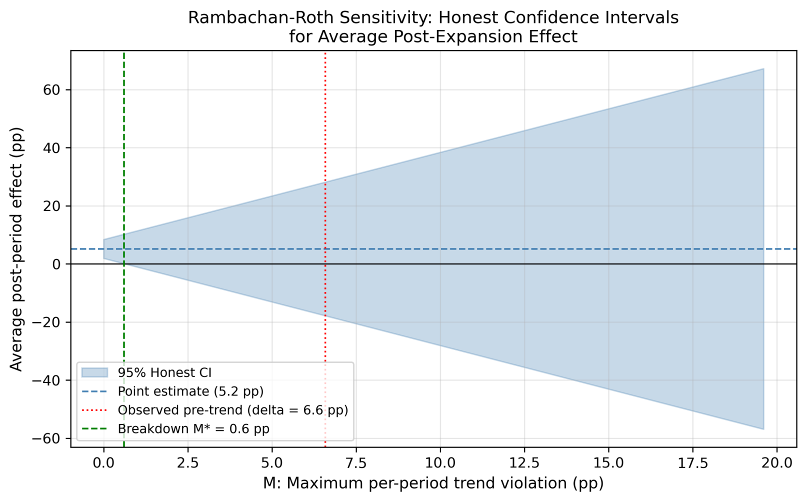

Figure 14: Concern5 Honest Ci

Note: This figure reports a robustness or sensitivity check for the concern5 Honest Ci. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

Plots the post-period coefficient alongside the heuristic delta-method-style sensitivity bound — NOT a formal Rambachan-Roth honest CI. A formal HonestDiD pass on a CSDID or Sun-Abraham event study is deferred. See Appendix Table A3 notes.

Appendix Figure A7. Age Profile of Coverage Effects

Plots the estimated coverage effect by age, showing how the treatment effect varies across the age distribution. The pattern is consistent with the age-based eligibility structure of California's Health4All expansion.

Appendix Figure A8. Placebo and Falsification Summary

Coefficient plot comparing the main triple-difference effect (red) against citizen placebo tests and fake-expansion placebo (blue, expected near zero). The main effect is clearly distinguishable from the placebo estimates, though the fake-expansion placebo is positive and significant, consistent with the pre-trend concerns documented in the text.

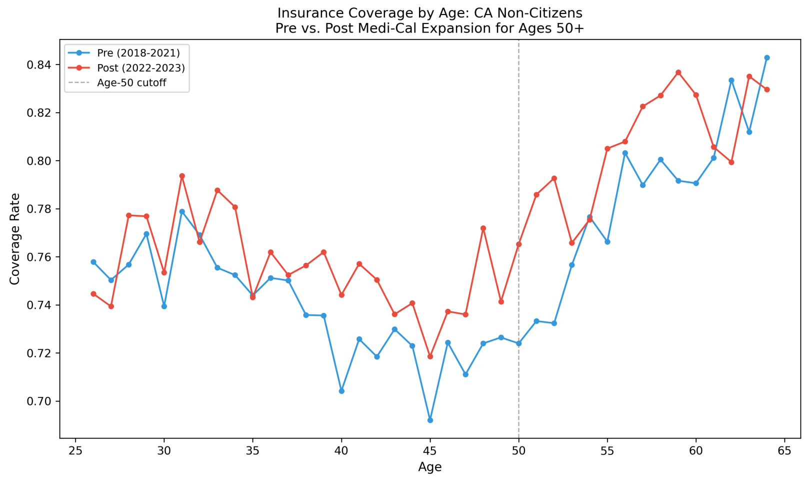

Figure 15: Concern3 Age Profile

Note: This figure compares estimates across groups or specifications for the concern3 Age Profile. It is intended to make effect heterogeneity and subgroup precision easier to assess.

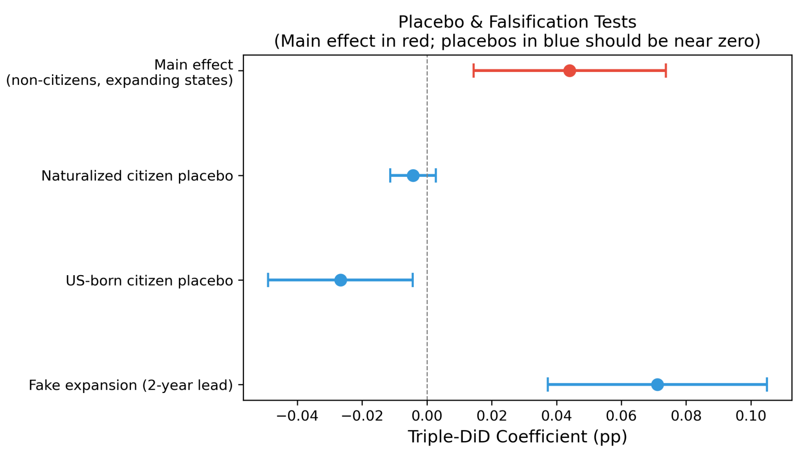

Figure 16: Placebo Falsification Summary

Note: This figure reports a falsification or placebo check for the placebo Falsification Summary. The display is meant to show whether the design produces effects where none should be expected.

References

Rambachan, A., & Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, 90(5), 2555–2591.