Section 1108's Long Shadow: Sixty Years of Capped Medicaid in the U.S. Territories, 1968–2024

Abstract

Background. Section 1108(c) of the Social Security Act has imposed a binding annual cap on federal Medicaid contributions to Puerto Rico and the four smaller U.S. territories since 1968. Despite Puerto Rico's six decades under the regime that proposed national per-capita Medicaid caps would impose on the fifty states, the historical mortality, coverage, and financing consequences of cap-based Medicaid have not been quantified using modern causal-inference designs.

Methods. We assemble a unified 1950–2024 panel of territorial and state Medicaid financing, coverage, and population-health outcomes and analyze four cap-regime shifts: the 1968 Section 1108(c) imposition, the 2011 Affordable Care Act Section 2005 partial uncapping, the FY2020 funding cliff, and a prospective state-level reverse counterfactual. The historical arms use synthetic-control designs with Deep South and high-Hispanic donor pools constructed from CDC WONDER mortality, NCHS Multiple Cause of Death microdata, NCHS Vital Statistics of the United States Volume I, and a newly machine-extracted sixty-year corpus of the Puerto Rico Demographic Registry's Spanish-language *Informe Anual de Estadísticas Vitales*. Robustness checks add formal R `Synth`, formal R `synthdid`, augmented-synthetic-control-style, generalized-synthetic-control-style, and conventional difference-in-differences specifications. The reverse-counterfactual arm applies published Medicaid-mortality elasticities (Sommers, Currie–Gruber, Goodman-Bacon) to the fifty-state Medicaid panel under three hypothetical per-capita-cap regimes (a 1968-scaled cap of $280 per capita, the Graham–Cassidy 2017 proposal of $1,800 per capita, and a generic FY2026-scale cap of $2,400 per capita), extends the infant-death calculation annually and cumulatively over the FY1984–FY2024 HCFA/CMS-64 state panel, and applies Miller, Johnson, and Wherry's near-elderly Medicaid-mortality estimate to a modern all-territory no-cap adult sensitivity.

Results. We find no detectable differential effect of the 1968 cap on Puerto Rico's infant-mortality decline relative to a Mississippi-anchored synthetic con-

trol across two independent windows (1980-anchored Δ-from-1971 ATT = +0.05 IMR per 1,000 live births, $p = 0.93$; 1968-anchored ATT on levels = +0.08 IMR per 1,000, Abadie $p = 0.20$ on |ATT| and 1.00 on the RMSPE ratio). Formal R `Synth` reproduces the 1968-anchored estimate (ATT +0.078; Mississippi weight 0.9999997), while formal R `synthdid` and conventional DiD specifications are negative, indicating faster Puerto Rico infant-mortality decline than their counterfactuals rather than a hidden adverse cap effect. The 2011 ACA Section 2005 bump produced no detectable effect on Puerto Rico's Medicaid coverage rate (ATT = −0.26 percentage points, $p = 0.93$). The FY2020 cliff is observationally inseparable from Hurricane Maria, the COVID-19 public health emergency, and the CAA 2020/2022 bridge supplements; on the cleaner federal-Medicaid-per-capita outcome, Puerto Rico's federal financing grew 52 percent FY2019–FY2024 versus an approximate 70 percent donor-state growth, indicating that bridge funding "bridged but underfunded." A descriptive within-Puerto-Rico interrupted-time-series on the maternal mortality ratio per 100,000 live births finds a borderline-significant level drop at the 2011 ACA Section 2005 bump (−15.6 per 100,000, $p = 0.052$) of policy-coherent direction. The reverse-counterfactual arm shows that a 1968-scaled cap applied to the fifty states would mechanically remove $513 billion in federal Medicaid spending in FY2024 (84 percent of actual federal contribution); the Graham–Cassidy 2017 cap would remove $86 billion and bind 25 of 51 state programs. Translated through published Medicaid-mortality elasticities as an illustrative literature-anchored magnitude range (rather than a calibrated estimate), the 1968-scaled state cap implies 4,451 to 14,838 excess infant deaths per year in FY2024 across the two infant-mortality anchors, and 97,032 to 323,438 cumulative excess infant deaths over FY1984–FY2024, the earliest window supported by the current HCFA/CMS-64 state panel. A modern all-territory adult-mortality sensitivity using Miller, Johnson, and Wherry's estimate implies that removing the FY2024 cap-equivalent funding gap could finance approximately 40,700 additional Medicaid-covered near-elderly low-income adults, 14,000 newly insured adults, and 420 adult deaths averted per year across the five territories under the preferred poverty-below-200-percent proxy; the FY2019–FY2024 cumulative preferred-proxy total is approximately 2,531 adult deaths averted.

Conclusions. The historical Arm 1 null is best read as a level-mismatch null rather than as evidence the 1968 cap was harmless: Puerto Rico's 1959–1967 IMR of 42.7 per 1,000 was 13.3 points above the donor-pool mean (and even above Mississippi at 39.3), so the synthetic-control optimizer was tracking a mechanical mortality-transition convergence that any plausible cap-effect signal would be embedded inside of. Broader estimator checks (`Synth`, `synthdid`, DiD, TWFE) do not uncover a hidden adverse cap effect, but they also do not have the statistical leverage to rule out a small positive elasticity given a single treated unit with no level-comparable donor. The policy-relevant magnitudes of cap-based Medicaid lie in the prospective fiscal scale of state-level cap proposals and in the literature-anchored infant and adult mortality ranges those caps imply. The argument for territorial Medicaid-financing equity therefore

rests on the unambiguous fiscal-architecture imposition documented in the historical Puerto Rico experience, the structural disadvantage of the cap-bound territorial population that emerges from American Community Survey mechanism diagnostics, and the policy-relevant mortality consequences that capped financing implies under published Medicaid estimates.

1. Introduction

The federal contribution to Medicaid in the fifty states and the District of Columbia is open-ended. The federal share of state Medicaid spending — the federal medical assistance percentage (FMAP) — moves with each state's per-capita income and is rebated against actual qualifying expenditure, with no binding annual ceiling on the federal contribution. This open-ended-entitlement architecture is the foundation of Medicaid's countercyclical role in state budgets and of its expansion under the Affordable Care Act.

The federal contribution to Medicaid in the five U.S. territories — the Commonwealth of Puerto Rico, the U.S. Virgin Islands, Guam, American Samoa, and the Commonwealth of the Northern Mariana Islands — has, since 1968, been capped under Section 1108(c) of the Social Security Act. Each territory receives an annual block grant set in nominal dollars (the "Section 1108 allotment"), which the territory matches at a statutorily fixed FMAP that has historically been far below the FMAPs to which the territories' per-capita-income statistics would otherwise entitle them. When a territory's Medicaid program exceeds the cap or exhausts the matching draw, the federal contribution stops. The territorial Medicaid program is, structurally, a per-capita-capped block grant — the same architecture that the Graham–Cassidy proposals of 2017 and a series of later proposals would have imposed on state Medicaid programs.

This architectural difference matters in two distinct ways. The first is normative: Puerto Rico's three million residents are U.S. citizens, the U.S. Virgin Islands' approximately one hundred thousand residents are U.S. citizens, Guam's residents are U.S. citizens, and the Northern Marianas' residents are U.S. citizens. The unequal federal contribution to the medical care of U.S. citizens is itself a question for political and legal scholarship that this paper does not address. The second is empirical: if the cap regime imposes adverse mortality, coverage, or care-access consequences on Puerto Rico that would be observable in the historical record, then the prospective imposition of a similar cap on state Medicaid would be expected to produce similar consequences at scale. If the cap regime does not impose detectable adverse consequences in the Puerto Rico historical record, the policy debate is shifted: the case against state-level per-capita caps must be made primarily through fiscal-architecture and prospective-elasticity reasoning rather than through historical analogy.

The empirical literature on this question is thin. Descriptive analyses by the Medicaid and CHIP Payment and Access Commission (MACPAC), the Govern-

ment Accountability Office, the Kaiser Family Foundation, and the Congressional Research Service (Mitchell 2023) document the financing differential and its history. A causal-inference literature on the territories' health outcomes does not exist at scale. Portela and Sommers (2015), Roman (2018), Santos-Lozada and Howard (2018), and Perreira and Oberlander (2017) have examined post-2014 coverage and post-Maria mortality, respectively, but the long historical Puerto Rico experience under capped Medicaid has not been subjected to a synthetic-control or other modern quasi-experimental analysis. The principal obstacle has been data: the standard U.S. mortality and coverage series do not carry Puerto Rico as a state-level unit. CDC WONDER does not include Puerto Rico in any vintage. The NCHS Multiple Cause of Death public-use files capture Puerto Rico residents only when those residents die on the U.S. mainland — typically six to fifty deaths per year, a migration measure rather than a territory-mortality measure. The `datos.estadisticas.pr` open-data portal covers only 2000–2010. The corresponding Puerto Rico mortality and natality series for the cap-era window 1968–2008 reside only in the Spanish-language *Informe Anual de Estadísticas Vitales* of the Puerto Rico Department of Health Demographic Registry — a sixty-year corpus of fifty-nine PDF volumes that has not, to our knowledge, been previously machine-extracted to a tidy panel for econometric analysis at this scale.

This paper has four objectives. First, we extract the Puerto Rico Demographic Registry corpus into a thirteen-outcome validated panel covering 1950–2023 and demonstrate that the resulting series support the same standard quasi-experimental designs used in the mainland Medicaid literature. Second, we conduct three historical synthetic-control analyses of regime shifts in territorial Medicaid financing — the 1968 Section 1108(c) imposition, the 2011 ACA Section 2005 partial uncapping, and the FY2020 funding cliff — and document the identified effects (or, more often, the identified absence of effects) on infant mortality, Medicaid coverage rate, and federal Medicaid spending per capita. Third, we conduct a prospective reverse-counterfactual analysis applying published Medicaid-mortality elasticities to the fifty-state Medicaid panel under three hypothetical per-capita-cap regimes, including an infant-mortality grid, a cumulative FY1984–FY2024 infant-death extension, and a supplementary modern all-territory adult-mortality calculation based on Miller, Johnson, and Wherry (2021). Fourth, we discuss what the combined evidence implies for the territorial-Medicaid-equity debate that will recur as the CAA 2022 territorial appropriations expire in FY2027.

The principal empirical findings are nulls in the historical designs. The 1968 Section 1108(c) imposition does not produce a detectable differential adverse effect on Puerto Rico's infant-mortality trajectory in either of two independent synthetic-control designs. Formal R `Synth` reproduces the headline 1968-anchored Python estimate exactly, and formal R `synthdid`, simple DiD, and two-way-fixed-effects variants do not uncover a hidden positive cap-harm effect. The 2011 ACA Section 2005 bump does not produce a detectable positive effect on Puerto Rico's Medicaid coverage rate. The FY2020 cliff is observationally

confounded by Hurricane Maria, the COVID-19 public health emergency, and the CAA bridge supplements. We interpret these nulls as informative rather than as failures of identification: across independent designs and outcomes, the cap regime as historically imposed on Puerto Rico does not register an adverse differential signal in the observable data once a Mississippi-anchored synthetic control is constructed. The most parsimonious explanation is that the underlying mortality-transition floor compresses cap-related effects below the detectable signal in a population of Puerto Rico's size, and that the cap regime's effects flow through the financing-architecture and care-access channels that the standard quasi-experimental designs in this paper cannot resolve. The supplementary maternal-mortality interrupted-time-series finds a borderline-significant policy-coherent reduction at the 2011 bump, consistent with the standard Medicaid-pregnancy-expansion mechanism, but the rare-event noise in maternal mortality precludes a clean causal attribution.

The reverse-counterfactual arm provides the magnitudes that the historical Puerto Rico arms cannot. Under a 1968-scaled cap applied to the fifty states, federal Medicaid spending in FY2024 falls by $513 billion against actual; under the Graham–Cassidy 2017 cap, by $86 billion; under a generic FY2026-scale cap, by $22 billion. Translated through the Currie–Gruber (1996) and Goodman-Bacon (2018) Medicaid-infant-mortality elasticities, the 1968-scaled state cap implies 4,451 to 14,838 excess infant deaths per year in FY2024, and 97,032 to 323,438 cumulative excess infant deaths over FY1984–FY2024, the earliest window currently supported by the HCFA/CMS-64 state panel. The Graham–Cassidy cap implies 529 to 1,762 excess infant deaths in FY2024 and 2,077 to 6,923 cumulatively over FY1984–FY2024. These ranges are not point estimates; they are the implied magnitudes under published Medicaid-mortality elasticities applied to the financing scale of each cap regime. The Sommers, Baicker, and Epstein (2012) anchor is retained as a transparency cell because it is an adult-mortality study with no infant-mortality coefficient. A separate modern all-territory adult-mortality sensitivity applies Miller, Johnson, and Wherry's near-elderly low-income Medicaid estimate to FY2019–FY2024 no-cap funding gaps; under the preferred poverty-below-200-percent proxy, the FY2024 all-territory gain is approximately 40,700 additional Medicaid-covered adults, 14,000 newly insured adults, and 420 adult deaths averted per year, with 2,531 adult deaths averted cumulatively over FY2019–FY2024. Puerto Rico accounts for approximately 36,700 of the additional Medicaid-covered adults and 379 annual adult deaths averted in FY2024. We emphasize the range and the underlying assumptions and do not advance any single point estimate as the headline.

The remainder of the paper is organized as follows. Section 2 reviews the legislative history of Section 1108(c) and the principal cap-regime shifts. Section 3 describes the data sources, the Puerto Rico Demographic Registry corpus extraction methodology, and the analytic panels. Section 4 specifies the four empirical designs and the supplementary robustness checks. Section 5 reports the results. Section 6 discusses the cross-design reconciliation, the limitations, and the policy implications for the FY2027 territorial-appropriations debate.

Section 7 concludes.

2. Background: Section 1108(c) and the Territorial Medicaid Regime

2.1 The 1968 imposition

Title XIX of the Social Security Act, enacted in 1965, established Medicaid as a federal-state program with open-ended federal matching of state Medicaid expenditures at each state's federal medical assistance percentage. Puerto Rico, the Virgin Islands, and Guam initially participated under the same open-ended-matching provisions as the fifty states. American Samoa and the Commonwealth of the Northern Mariana Islands did not enter Medicaid until 1982 and 1983 respectively, and are excluded from the 1968 cap-imposition analysis.

In January 1968, Public Law 90-248 (the "Social Security Amendments of 1967") amended Section 1108 of the Social Security Act to impose annual caps on the federal contribution to territorial Medicaid programs, statutorily fixed in nominal dollars and not indexed for inflation, population growth, or medical-cost growth. The initial caps were $20 million for Puerto Rico, $900,000 for Guam, and $650,000 for the U.S. Virgin Islands. These caps were below the federal contribution that would have flowed to the territories under open-ended FMAP matching even at the territories' then-current Medicaid program sizes; the caps were therefore binding from year one. The territorial FMAP was statutorily fixed at 50 percent (later 55 percent) regardless of per-capita income, far below the FMAPs to which the territories' per-capita income data would have entitled them under the standard FMAP formula. Under the standard FMAP formula, Puerto Rico's per-capita income would have qualified the territory for the maximum statutory FMAP of approximately 83 percent — the same FMAP the U.S. Virgin Islands, Guam, and American Samoa would have qualified for. The cap and the FMAP differential are jointly responsible for the magnitude of the federal-financing differential between Puerto Rico and equivalent-income mainland states.

The 1968 imposition was, in effect, the first per-capita-cap implementation of Medicaid financing in the United States, applied to a population of approximately 2.7 million U.S. citizens in Puerto Rico and approximately 100,000 in the smaller territories combined. The cap was nominal-dollar-frozen for fourteen years; it was raised to $30 million in 1972 (PL 92-603), $45 million in 1982 (TEFRA), and to $63.4 million in 1984 (DEFRA), each adjustment lagging substantially the medical-care cost inflation of the period. By the late 1990s the cap was binding so severely that Puerto Rico's actual federal contribution per Medicaid beneficiary was approximately 15 percent of the federal contribution that an equivalent state Medicaid beneficiary would have received.

The legislative history of the 1968 imposition is itself instructive. The Social

Security Amendments of 1967 were enacted under President Johnson's administration two years after Title XIX was first enacted; the territorial cap provisions appear in §248 of PL 90-248 and were added during conference committee in late 1967. The contemporaneous policy discussions framed the cap as a fiscal-control measure to limit federal exposure to territorial Medicaid programs whose initial sizes the federal government had limited ability to forecast. The cap was not framed at the time as a per-capita Medicaid financing model that might later be applied to state programs; the per-capita-cap framing of state Medicaid as a national policy proposal (Graham–Cassidy 2017, Better Care Reconciliation Act, the FY2018 budget resolution) emerged decades later. The historical Puerto Rico cap regime is therefore an unintended natural experiment for the per-capita-cap state Medicaid policy proposals: the policy architecture is analogous, but the cap was not designed as a national policy template.

2.2 The 2011 ACA Section 2005 partial uncapping

Section 2005 of the Patient Protection and Affordable Care Act (PL 111-148, March 2010) appropriated $7.3 billion in supplemental federal Medicaid funding for the five territories, available between July 1, 2011 and September 30, 2019. Of this total, $6.3 billion was directed to Puerto Rico, $268 million each to the U.S. Virgin Islands and Guam, $103 million to the Northern Marianas, and $19 million to American Samoa. Section 1323 of the same act provided an additional $925 million one-time supplement to Puerto Rico available for the establishment and operation of an exchange (this section was not used by Puerto Rico to establish an exchange and was largely redirected to Medicaid). The Section 2005 funds drew down at a 55 percent FMAP — somewhat enhanced relative to the historical territorial FMAP but still well below the FMAPs available to states.

The Section 2005 supplement was structured as a multi-year drawdown rather than as an annual cap revision. Puerto Rico drew down approximately $800 million per year from 2011 to 2017 and exhausted the fund earlier than the four smaller territories. Between FY2017 and FY2019 a series of disaster-related and budget-act supplements (PL 115-31, BBA 2018, PL 116-20) provided additional one-time bridges in response to Hurricane Maria's September 2017 landfall. The Section 2005 fund was fully exhausted by FY2019 in Puerto Rico.

2.3 The FY2020 cliff and the CAA 2020/2022 bridges

The exhaustion of the Section 2005 fund and the reversion of Puerto Rico's territorial FMAP to its statutory baseline at the start of FY2020 were widely characterized as a "Medicaid funding cliff." On December 20, 2019, the Further Consolidated Appropriations Act of 2020 (PL 116-94) provided a four-year bridge supplement to Puerto Rico of approximately $2.6 billion per year through FY2023 and raised the territorial FMAP to 76 percent for the bridge period. Smaller bridge amounts went to the four other territories. The Families

First Coronavirus Response Act (PL 116-127, March 2020) added a temporary FMAP enhancement that further inflated the federal contribution during the COVID-19 public health emergency. The Consolidated Appropriations Act of 2022 (PL 117-103, March 2022) extended Puerto Rico's enhanced federal financing through FY2027 with annual capped funding specified in statute at $3.275 billion for FY2023 and $3.325 billion for FY2024, with annual escalators thereafter (Mitchell 2023, citing CRS R47601 Table 1). The fiscal cliff was therefore not allowed to fully bind in 2020 or thereafter; what arm 3 of this paper observes is the joint effect of the statutory cliff, the bridge supplements, the COVID-19 PHE Medicaid continuous-enrollment requirements, and Hurricane Maria's continuing health-system effects.

2.4 Why each shift identifies what it identifies

Each cap-regime shift is, in principle, an experiment that identifies a different parameter of the cap-on-Medicaid relationship. The 1968 imposition tests whether the introduction of capped Medicaid to a previously open-ended-matching jurisdiction produced a differential adverse mortality, coverage, or care-access effect. The 2011 partial uncapping tests whether a major bilateral reduction in cap-bite produces a differential effect in the opposite direction — and therefore acts as a falsification of any 1968 finding through sign reversal. The FY2020 cliff tests whether a bilateral re-imposition of cap-bite produces a differential adverse effect. A unified four-arm design that includes a prospective state-level reverse counterfactual provides the policy-relevant magnitudes that the historical Puerto Rico arms cannot.

The four-arm design is bilateral by construction: every estimated effect can in principle be falsified by the opposite-sign effect of a regime shift in the opposite direction. If the 1968 imposition produces a positive adverse effect on PR IMR, the 2011 partial uncapping should produce a negative beneficial effect on PR Medicaid coverage or PR per-capita federal financing; if it does not, the 1968 finding requires either an alternative mechanism account (delayed effect, reporting change, mortality-transition floor compression) or a reframing as a non-causal observation. We exploit this falsification structure throughout the paper: the maternal-mortality 2011 ITS finding (a borderline-significant level drop in the policy-coherent direction) is offered as descriptive corroboration of the Medicaid-coverage mechanism precisely because it sign-reverses against the proposed 1968 ITS direction, even though neither result is causally identified.

2.5 Prior literature

The existing literature on territorial Medicaid is dominated by descriptive and policy-analytic work. MACPAC's *Medicaid and CHIP in the Territories* report (February 2021) is the standard reference for the institutional architecture of the cap regime; MACPAC has updated the territory-specific fact sheets approximately annually. The Government Accountability Office produced a series of

GAO-15-358 and GAO-21-104 reports on territorial Medicaid funding gaps. The Kaiser Family Foundation's territorial Medicaid issue briefs (Long et al. 2017; Artiga, Garfield, and Damico 2019) and Manatt's territorial Medicaid analyses provide additional descriptive material. The Congressional Research Service (Mitchell 2023, R47601) is the authoritative legislative-history source.

The causal-inference literature on territorial health outcomes is sparse. Portela and Sommers (2015, *Milbank Quarterly*) compare insurance and access to care between Puerto Rico and selected mainland populations using descriptive statistics and a difference-in-differences specification on selected access measures; they do not address the §1108(c) cap directly. Roman (2018, *Annals ATS*) and Santos-Lozada and Howard (2018, *JAMA*) examine post-Maria mortality. Perreira and Oberlander (2017, *Health Affairs Forefront*) examine the post-2014 PR Medicaid funding crisis as a policy commentary. None of these papers exploits the 1968 §1108(c) imposition, the 2011 ACA §2005 partial uncapping, or the FY2020 cliff in a synthetic-control or other formal quasi-experimental design.

The broader Medicaid-mortality literature on which the elasticity-anchored grid depends includes Currie and Gruber (1996, *JPE*) on pregnancy-Medicaid expansions and infant mortality; Goodman-Bacon (2018, *JPE*) on the original 1966–1970 Medicaid implementation and non-white infant mortality; Sommers, Baicker, and Epstein (2012, *NEJM*) on adult mortality and Medicaid expansions; Miller, Johnson, and Wherry (2021, *QJE*) on Medicaid eligibility and near-elderly adult mortality; and the post-ACA expansion literature (Sommers 2020, *Annual Review of Public Health*, for a survey).

The prior literature has not addressed the historical Puerto Rico cap regime at scale principally because the data assembly is non-trivial. Constructing a Puerto Rico–side mortality outcome series that supports modern quasi-experimental designs requires a sixty-year corpus of fifty-nine Spanish-language Demographic Registry PDFs to be machine-extracted, parsed, validated, and panel-assembled. Constructing a comparable donor-state pre-1968 IMR panel requires NCHS Multiple Cause of Death microdata (deaths under one year by state of residence) joined to NCHS Vital Statistics of the United States Volume I Table 2-1 (live births by place of residence) with cell-by-cell transcription from the published volumes for the donor-state-years that the standard panel data sources do not carry. We document this assembly in detail in Section 4.5.

3. Data

This study assembles a unified 1950–2024 panel of Puerto Rico, four-smaller-territory, and state Medicaid financing, coverage, and population-health outcomes. The data assembly is organized into four analytic panels (one per arm), and the underlying source files are documented in `data/data-dictionary.md` with primary-source URLs. The full text of the data section, including primary sources for each variable, sample restrictions, panel construction, and measure-

ment caveats, appears in the appendix as Supplementary Section S1; the present section provides a condensed account.

3.1 Treated and donor units

The treated units are the U.S. territories: Puerto Rico (PR), the U.S. Virgin Islands (USVI), Guam (GU), American Samoa (AS), and the Commonwealth of the Northern Mariana Islands (CNMI). Puerto Rico is the primary treated unit; the four smaller territories enter the analysis as secondary panels with substantially smaller cap-bite. American Samoa and CNMI entered Medicaid only in 1982 and 1983 respectively and are excluded from arm 1.

The donor pool for the 1968 cap-imposition analysis comprises ten Deep South states: Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and West Virginia. For the post-2006 coverage and funding analyses the donor pool is expanded with four high-Hispanic / Southwestern states (Arizona, Florida, New Mexico, Texas) to better match Puerto Rico's demographic composition after the §936 industrialization era. The reverse counterfactual uses the full fifty-state-plus-District-of-Columbia panel as the platform on which to apply counterfactual cap regimes. The Mississippi-anchored synthetic that emerges in the infant-mortality results is a feature of the synthetic-control optimization (no other donor's pre-1968 IMR level approaches Puerto Rico's), not an analyst choice.

3.2 Outcome series

Mortality. Mortality outcomes for the fifty states and DC come from the CDC WONDER Compressed Mortality (1968–1998) and Underlying Cause of Death (1999–2024) systems and the ICPSR Multiple Cause of Death public-use files for individual-level pre-1979 ICD-8 detail. CDC WONDER does not include Puerto Rico as a state-level unit in any vintage; the ICPSR MCD files capture Puerto Rico residents only when those residents die on the U.S. mainland. Puerto Rico mortality and natality 1955–2023 are therefore drawn from the *Informe Anual de Estadísticas Vitales* of the Puerto Rico Demographic Registry, a fifty-nine-volume Spanish-language corpus that we extract through a two-tier pipeline: PyMuPDF native text extraction for the forty-nine digitally-typeset volumes and Tesseract OCR with the Spanish language model (`tesseract -l spa`) for the ten earlier scanned volumes. A regex parser targets thirteen outcome series across the Spanish-language section headings and table-vetted cells; a post-parse quality-assurance layer enforces outcome-specific plausibility ranges, year-token exclusion, and internal-consistency checks. Of 562 raw parser extractions, 559 cells (99 percent) pass the QA gates and are released in the validated panel. The most policy-relevant series is the infant mortality rate per 1,000 live births, with seventy-one validated cells covering 1950–2020 (a near-complete annual series). The corpus also supplies seventy-four live-birth cells, sixty-one maternal-death cells, sixty-nine infant-death and fetal-death cells, and forty-five cells of the

published age-adjusted all-cause crude rate.

Coverage. Insurance coverage outcomes for the fifty states and DC come from the American Community Survey, and for Puerto Rico from the Puerto Rico Community Survey, accessed through IPUMS USA for 2005 through 2024 inclusive. The PRCS does not cover the four smaller territories. Coverage outcomes are restricted to the post-2005 period and therefore inform arms 2 and 3 only.

Financing. Federal Medicaid allotment caps and statutory federal medical-assistance percentages for the five territories 2006–2024 come from the MACPAC *Medicaid and CHIP in the Territories* February 2021 report and MACStats December 2023, cross-validated against CRS R47601 (Mitchell 2023). The validated allotment table carries explicit source-status flags: `validated` for MACPAC-published cells, `statute` for CRS-cited statutory annual caps, and `pending_source_pdf` for annual caps whose CMS Section 1108 certification PDFs were not retrievable from public sources. State-level Medicaid spending 1968–2024 for the reverse-counterfactual baseline comes from MACPAC MACStats (post-2008), KFF state Medicaid spending tables (1991–present), and the HCFA Statistical Supplement (1968–1991), with a public HCFA medical-assistance OCR bridge for FY1984–FY1997.

Donor-state IMR pre-1968. Donor-state infant deaths under one year for 1959–1967 come from NCHS Multiple Cause of Death microdata aggregated to (state of residence, year), and donor-state live births for 1959–1967 come from NCHS Vital Statistics of the United States Volume I Table 2-1 "Total births by place of residence" for each donor-state-year cell, transcribed from the published volumes. The resulting donor IMR panel covers fourteen donor states × nine years (126 cells) and passes five of five spot-check anchors against published NCHS Vol I IMR cells within ±15 percent.

State-year live births 1995–2024. State-year live births for the reverse-counterfactual mortality denominator come from CDC WONDER Natality interactive TSV exports across three queries (1995–2002, 2003–2006, 2007–2024). The resulting panel covers fifty-one jurisdictions × thirty years (1,530 cells) and passes all four published-national-total validation anchors at 0.00 percent difference.

3.3 Treatment construction

For each historical design, treatment is operationalized at the calendar year of the regime shift: 1968 for the cap-imposition analysis (legal effective date of the Section 1108(c) cap); 2011 for the partial-uncapping analysis (ACA Section 2005 fund availability); 2020 for the funding-cliff analysis (Section 2005 fund exhaustion in PR and reversion of FMAP to baseline before CAA bridge offsets). Cap-bite share for each (territory, fiscal year) is computed as one minus the ratio of actual federal funds available (from the validated allotment table, including Section 2005 drawdown and supplements where applicable) to a counterfactual open-ended federal Medicaid spend constructed from the donor-pool average

federal Medicaid expenditure per capita times the territory's resident population. The counterfactual spend is a derived quantity rather than a measured one and is reported as a sensitivity to four alternative donor specifications.

3.4 Sample restrictions

Sample restrictions are limited to (a) the entry of AS and CNMI into Medicaid in 1982 and 1983 (excluded from the 1968 cap-imposition analysis), (b) the exclusion of the four smaller territories from outcome-specific subanalyses where their WONDER coverage is absent, and (c) the truncation of the infant-mortality analysis before the 2011 ACA Section 2005 confounder. Annual PRCS one-year data for Puerto Rico are absent for 2020 (the U.S. Census Bureau did not release a 2020 PRCS one-year file); strict annual analyses exclude 2020 and sensitivity checks use interpolation and the 2016–2020 PRCS five-year release.

3.5 Verified versus exploratory data products

A reproducibility audit established a two-layer source-of-truth discipline that we carry through the paper: every headline numerical claim and every main-text exhibit draws from a *verified* data product, while *exploratory* outputs are restricted to descriptive context and never used as estimates. Readers and reviewers should treat this distinction as load-bearing rather than as cosmetic, because several superficially compelling candidate measures fail one or more verification gates.

The **verified layer** comprises six products: (i) the CDC WONDER state-year mortality panel for fifty states and DC, 1968–2024 (138,162 rows, no double-counting across the 1999–2020 and 2018–2024 vintages); (ii) the ACS / PRCS coverage extract for fifty states + DC + Puerto Rico, 2005–2024 (1,040 unit-years drawn from IPUMS USA / IPUMS-PRCS); (iii) the CMS-64 state-territorial federal Medicaid net expenditure panel, FY1997–FY2024 (the post-FY1997 segment is fully populated; pre-FY1997 is bridged by the HCFA Statistical Supplement OCR and is used only descriptively); (iv) the benchmarked PR *Informe* infant mortality rate cells for 1959–1967 and 1971–2008 (the subset that survives the parser and quality-assurance gates and additionally matches published *Informe* table-anchored values within ± 5 percent at five spot-check cells); (v) the MACPAC / CRS-cross-validated territorial allotment table with explicit source-status flags per cell; and (vi) the mechanical fiscal counterfactual, which is a deterministic function of CMS-64 state federal-Medicaid spending and the three nominal per-capita cap regimes.

The **exploratory layer** comprises four products that are present in the data tree but that should not be read as estimates: (a) the broad PR *Informe* outcome panel beyond the benchmarked IMR cells (live-births / fetal-deaths / non-validated maternal-deaths / post-2008 IMR cells whose extraction did not pass internal-consistency or year-token-exclusion gates); (b) the VSUS 1950–1967 donor-state IMR `_filtered` parquets, whose own validation reproduces only

two of fourteen state benchmark cells within the published range and which we therefore do not use in the 1968 cap-imposition analysis (that analysis reconstructs pre-1968 donor IMR from NCHS Multiple Cause of Death microdata + NCHS Vital Statistics of the United States Volume I Table 2-1 cell-transcribed live births, not from these `_filtered` parquets); (c) the NCHS natality territory-year panel for the four smaller territories pre-2024, which contains implausible single-digit Virgin Islands birth counts that are visible artifacts of the layout-detection parser and which we therefore do not use as a complete territory-natality panel; and (d) the elasticity-anchored implied excess-infant-deaths grid and adult-mortality sensitivity, which are illustrative transfers of published Medicaid estimates to a Medicaid-contraction setting, not calibrated mortality estimates.

The data dictionary at `data/data-dictionary.md` carries the per-product layer flag and traces each layer designation to specific QA-gate outcomes. Every numerical claim in §5 and every main-text exhibit is sourced from the verified layer; the exploratory products appear only in Discussion and Limitations contexts where we explicitly flag them as illustrative or descriptive. We retain the exploratory products in the data tree (rather than deleting them) so that downstream researchers can audit the verification trail and so that follow-up work can target the specific repair pathways the audit identifies — but we do not advance any number from the exploratory layer as a headline.

A separate data point is worth surfacing immediately, because it is easy to misread the data tree without it: **CDC WONDER does not include Puerto Rico in any vintage from 1968 forward**, and the Puerto Rico WONDER mortality extract is correctly empty. Every Puerto Rico–side mortality outcome in this paper therefore comes from the *Informe Anual de Estadísticas Vitales* of the Puerto Rico Department of Health Demographic Registry rather than from CDC WONDER, and the verification standard for those cells is the parser and quality-assurance layer plus published-cell spot-checks, not the WONDER population-divided death-count standard that produces the donor-state IMR. We restate this point in §4.5 and §6.3 because it is the single most consequential data limitation of the historical Puerto Rico analyses.

4. Methods

4.1 1968 Section 1108(c) imposition

The 1968 cap-imposition analysis identifies the effect on Puerto Rico's infant mortality through two independent synthetic-control designs, each recorded in `analysis/specification-registry.yml` under the pre-commitment discipline of Rambachan and Roth (2023).

The 1980-anchor design uses the Puerto Rico Informe IMR cells available within the donor-WONDER overlap window 1971+1976–2008 (five pre-period observations) as the synthetic-control pre-period and 1980–2008 as the post-period

anchor, fit through the `pysyncon` implementation of classical synthetic control (Abadie, Diamond, and Hainmueller 2010), augmented synthetic control (Ben-Michael, Feller, and Rothstein 2021), and a Δ-from-1971-baseline classical SC. We report Abadie-2010 in-space placebo p-values from rotating treatment to each of the fourteen donor states and from the exchangeable formula (1 + #placebos with |ATT| ≥ |ATT_treated|) / (1 + n_placebos).

The 1968-anchor design uses the validated PR Informe IMR cells for 1959–1967 (nine pre-period observations) as the pre-period and 1968–2008 as the post-period (truncated before the 2011 ACA Section 2005 bump confounder). The donor pool's pre-1968 IMR is constructed from the NCHS MDF deaths-under-one + NCHS Vol I live-births panel described in Section 3.2; the post-1968 donor IMR comes from CDC WONDER Compressed Mortality / Underlying Cause of Death deaths-under-one divided by the CDC WONDER population-under-one denominator. We run the same three estimator specifications and the same Abadie-2010 placebo. A drop-1964 sensitivity excludes the 1964 cell, which is a real Informe Vol I value but a visible high point in the historical series. The method-family robustness suite adds formal R `Synth`, formal R `synthdid`, augmented-synthetic-control-style, generalized-synthetic-control-style, simple DiD, and two-way-fixed-effects checks, all reported as sensitivity analyses rather than new headline estimands.

The supplemental within-Puerto-Rico interrupted-time-series on the maternal mortality ratio (MMR) per 100,000 live births is fit as a segmented regression `MMR = β0 + β1·time + β2·post + β3·post·time + ϵ` with `time = year − treat_year` and `post = 1[year ≥ treat_year]`. The 1968 cap ITS uses pre-period 1958–1967 (10 observations) and post-period 1968–2008 (41 observations); a 1962 in-time placebo uses pre-period 1958–1961 and post-period 1962–1967. The 2011 ITS uses pre-period 2003–2010 and post-period 2011–2019 (truncated before the FY2020 cliff and Hurricane Maria).

4.2 2011 ACA Section 2005 bump

The 2011 ACA Section 2005 analysis identifies the partial-uncapping effect on Puerto Rico's Medicaid coverage rate using the same `pysyncon` implementations of classical synthetic control, augmented synthetic control, and Δ-from-2010-baseline classical SC. The pre-period is 2008–2010 (three years; PRCS coverage data begin in 2005 but the 2005–2007 cells contain known PRCS-revision artifacts that we exclude). The post-period is 2011–2019. The donor pool is the fourteen-state Deep South + high-Hispanic pool. We report Abadie-2010 in-space placebo p-values from the corrected exchangeable formula.

The principal interpretive challenge is the level mismatch between Puerto Rico's pre-treatment Medicaid share (43.5 percent in 2010) and the donor-pool maximum (Mississippi at approximately 24 percent). The classical synthetic-control estimate is uninterpretable under this mismatch; we therefore privilege the augmented SC (which uses a ridge bias correction allowing weights summing above

one) and the Δ-from-2010 specification (which re-centers each unit on its 2010 baseline). Both alternatives produce small near-zero ATTs and a confirming placebo distribution.

4.3 FY2020 cliff and the alternative federal-per-capita outcome

The FY2020 cliff analysis identifies the cliff effect on Puerto Rico's Medicaid coverage rate using the same `pysyncon` Δ-from-2019-baseline synthetic-control specification with the 2011–2019 pre-period and the 2020–2024 post-period. We additionally fit a 2017 Hurricane Maria placebo with the same machinery to test the Maria–cliff identification problem.

Because the primary coverage-rate outcome is observationally inseparable from the COVID-19 PHE continuous-enrollment provisions, the CAA 2020 / 2022 bridge supplements, and Hurricane Maria's continuing health-system effects, we additionally specify an alternative outcome — log federal Medicaid spending per capita — that is closer to the policy lever the cliff actually moves. Federal Medicaid spending per capita is computed for Puerto Rico from the territorial treatment panel, including statute-anchored FY2023/FY2024 caps, and for the donor states from the CMS-64 federal share divided by Census population estimates. The alternative outcome is fit through classical SC on log levels and through Δ-from-FY2019 classical SC.

4.4 Reverse counterfactual

The reverse-counterfactual analysis is a mechanical and elasticity-anchored counterfactual application of three hypothetical per-capita-cap regimes to the fifty-state Medicaid panel. The three regimes are: (i) the 1968 Section 1108(c) cap scaled to state population, with the nominal cap held at $280 per capita (the per-capita value of the original 1968 cap escalated by population growth alone, not by inflation); (ii) the Graham–Cassidy 2017 per-capita cap of $1,800 per capita; and (iii) a generic FY2026-scale cap of $2,400 per capita. For each (state, fiscal year, regime) triple we compute the cap-bite share as one minus the ratio of cap-permitted federal spending to actual federal Medicaid spending, and we sum the cap-removed federal dollars to a national rollup.

The infant-mortality layer transfers three published Medicaid-mortality elasticities to each (state, fiscal year, regime) triple: a zero transparency cell from Sommers, Baicker, and Epstein (2012, *NEJM*), which estimated adult mortality reductions from Medicaid expansion but no infant-mortality channel; a moderate anchor of approximately 1.5 IMR points per 1,000 per full cap-bite from Currie and Gruber (1996, *JPE*), which estimated infant-mortality reductions from pregnancy-Medicaid expansion; and an upper anchor of approximately 5.0 IMR points per 1,000 per full cap-bite from Goodman-Bacon (2018, *JPE*), which estimated non-white infant-mortality reductions from the original Medicaid implementation 1966–1970. State-year federal-share inputs come from the HCFA

medical-assistance bridge for FY1984–FY1996 and CMS-64 / Financial Management Report workbooks for FY1997–FY2024. State-year birth denominators come from the CDC WONDER Natality state-year panel (1995–2024) for years in coverage and from a fertility-rate proxy (population × 0.20 × interpolated US general fertility rate / 1000) for FY1984–FY1994. The current repository does not contain a comparable all-state federal-share panel for FY1968–FY1983, so the annual and cumulative infant-death extension begins in FY1984. The implied excess-infant-deaths grid is computed at each elasticity anchor for each cap regime and reported as a range, not a point estimate.

We additionally fit a within-territory no-cap-increase counterfactual that compares observed Puerto Rico federal Medicaid funds available FY2011–FY2024 to four hypothetical scenarios: (a) base Section 1108 only with no ACA Section 2005 drawdown and no post-2017 supplements; (b) no ACA Section 2005 drawdown but with post-2017 supplements; (c) base + Section 2005 but no post-2017 disaster / CAA / FFCRA supplements; and (d) base + Section 2005 + disaster supplements but no FFCRA / CAA 2022 supplements. The cumulative shortfalls under each scenario quantify the policy contribution of each cap-relaxing instrument relative to observed federal funding.

Finally, a supplementary adult-mortality sensitivity applies Miller, Johnson, and Wherry's near-elderly low-income Medicaid estimate to the no-cap funding gap. The calculation keeps the infant-mortality grid conceptually separate from the adult channel: Miller, Johnson, and Wherry identify a 0.132 percentage-point annual mortality decline and a 12.8 percentage-point Medicaid-coverage first stage among low-SES adults ages 55–64, not an infant-mortality elasticity. We report a Puerto Rico authority-basis estimate for FY2024 and a modern all-territory estimate for FY2019–FY2024 using observed CMS-64 federal spending for all five territories. Puerto Rico target populations come from the PRCS/ACS mechanism panel; for American Samoa, Guam, CNMI, and the U.S. Virgin Islands, the repository lacks territory-specific age-by-poverty coverage panels, so the smaller-territory rows apply Puerto Rico's latest near-elderly target-population shares and coverage shares to each territory's population. We therefore report the all-territory adult sensitivity as a transparent proxy, and we do not estimate a 1968-present all-territory adult backcast.

4.5 Puerto Rico Demographic Registry corpus extraction

The historical infant-mortality analysis depends on the machine-extraction of the Puerto Rico Demographic Registry's Spanish-language *Informe Anual de Estadísticas Vitales* corpus, a methodological contribution that we describe in detail because it underlies every Puerto Rico–side mortality outcome in this paper and because, to our knowledge, this corpus has not previously been machine-extracted to a tidy panel for econometric analysis at this scale.

The full physical corpus comprises fifty-nine PDF volumes: the annual *Informe Anual* run from 1958 through 2014, the consolidated *Informe de Estadísticas*

Vitales — Defunciones 2017–2020 and 2009–2014 volumes, and the consolidated *Nacimientos* 2009–2010, 2011–2014, 2017–2020, and 2021–2023 volumes. The corpus is obtained as official publications from the Departamento de Salud de Puerto Rico via `datos.estadisticas.pr` and the University of Puerto Rico Medical Sciences Campus Programa Graduado de Demografía digital archive. The corpus is approximately 1.2 GB of PDF.

The extraction pipeline operates in two tiers. **Tier 1 (PyMuPDF native text extraction)** processes forty-nine of the fifty-nine PDFs that have usable native text layers. The 2004+ Informes are native PDF; the 1960–1989 Informes are well-OCR'd scans with embedded text. PyMuPDF extracts the text in approximately fifty seconds total. **Tier 2 (Tesseract OCR with Spanish language model)** processes the remaining ten scanned PDFs without usable native text (1958, 1959, 1990, 1993, 1996_Mortalidad, 1999, 2000, 2001, 2002, 2003) using `tesseract -l spa`, the Spanish language model. Tier 2 produces approximately 3.9 million additional characters of usable text in approximately forty-five minutes of compute.

A regex-based parser targets thirteen outcome series across the Spanish-language section headings and table-vetted cells that appear in each Informe: live_births, infant_deaths, neonatal_deaths, fetal_deaths, maternal_deaths, deaths_total, imr_per_1000, neonatal_rate_per_1000, fetal_rate_per_1000, crude_birth_rate, crude_death_rate, maternal_rate_per_1000, and population. The parser incorporates a series of reproducibility refinements: a 2007/2008 IMR extraction fix; a maternal-deaths-specific table-extraction path that bypasses generic label proximity for the 1958–1974 maternal cells; post-2008 child/fetal/natality table extractions that close the consolidated-period-volume parsing gap; and historical child/fetal table-vetted cells from the 1977 historical demographic summary, the 1978/1979 annual infant/stillbirth tables, and a 1980 table override.

A post-parse quality-assurance layer at `data/scripts/13c_pr_informe_qa.py` enforces three gates on each extracted cell: an outcome-specific plausibility range (e.g., `imr_per_1000` $\in$ `[3, 90]` reflecting the achievable mortality range for a high-income territory across the corpus period); year-token exclusion (cells whose extracted value lies in the year-range 1900–2030 and whose row context does not provide an unambiguous outcome label are quarantined); and internal-consistency checks (e.g., when `infant_deaths`, `live_births`, and `imr_per_1000` are all observed for a given year, the imr cell is required to satisfy `imr == infant_deaths / live_births` $\times$ `1000` $\pm$ `1.0`). Cells that pass all three gates are flagged `validated` (against published benchmark anchors) or `triangulated` (multiple parser hits agreeing within tolerance); cells that fail are quarantined with explicit failure-reason flags and are not consumed by any analysis.

Of 562 raw parser extractions, 559 cells (99 percent) pass the QA gates. The validated panel covers thirteen outcome series with cell-coverage ranging from two cells (deaths_total, 1958–1962) to seventy-four cells (live_births, 1950–

2023). The most policy-relevant series is the infant mortality rate per 1,000 live births, with seventy-one validated cells covering 1950–2020 — a near-complete annual series across the cap-era window 1968–2008 with twenty-five additional pre-cap cells (1950–1967) and three post-2008 cells (2018, 2019, 2020). The maternal-deaths series carries sixty-one validated cells covering 1958–2020 and is the basis of the supplemental within-PR maternal mortality ITS analysis.

Every raw VSUS Vol I PDF feeding the donor-state births worksheet has a `.provenance.json` sidecar at `data/raw/_provenance/` recording sha256 checksum, acquisition date, source URL, and per-cell transcription log. The CDC WONDER mortality and natality TSV exports carry analogous sidecars. The two-tier extraction pipeline and quality-assurance layer reproduce the validated cell panel from raw PDFs in approximately one hour of compute time on a standard analyst workstation.

5. Results

5.1 Infant-mortality null on two independent designs

Table 1 reports the headline results across the 1980-anchor and 1968-anchor designs. In the 1980-anchor design, the Δ-from-1971 specification produces a post-period mean ATT of +0.05 IMR per 1,000 live births with an Abadie-2010 in-space placebo p-value of 0.93 on |ATT|. In the 1968-anchor design with the full validated 1959–1967 pre-period (nine observations), the level-spec ATT is +0.08 with placebo p = 0.20 on |ATT| and 1.00 on the RMSPE ratio. The drop-1964 sensitivity produces an identical ATT of +0.08 because Mississippi receives weight 1.00 in both specifications: Puerto Rico's pre-1968 IMR level (1959–1967 mean of 42.7 per 1,000) is dramatically higher than any donor state, and Mississippi (1959–1967 mean of 39.3) is the only donor close enough in level for the synthetic-control optimizer to use. The post-1968 trajectory therefore shows Puerto Rico converging toward Mississippi rather than diverging from it, and the post-period mean gap between Puerto Rico and synthetic-Puerto-Rico is essentially zero across thirty-three to forty-one observations.

Figure 1 plots the 1968-anchor paths of Puerto Rico and synthetic Puerto Rico over 1959–2008, with the §1108(c) cap year (1968) marked. The visual story is convergence: Puerto Rico starts well above Mississippi in the late 1950s and 1960s and ends essentially indistinguishable from Mississippi in the 2000s. The fourteen-donor in-space placebo distribution places Puerto Rico's |ATT| as the third-largest of the fifteen units (behind New Mexico and Arizona, both of which have negative ATTs), but Puerto Rico's RMSPE ratio is the smallest of all fifteen — under the Abadie framework, this means Puerto Rico's post-period gap is more parsimoniously interpreted as continuation of pre-period level mismatch than as a treatment-induced shift.

The longer-post-window sensitivity (Table A2 in the appendix) confirms the null across multiple post-period truncations: 1968–1980 ATT = −0.41; 1968–1993

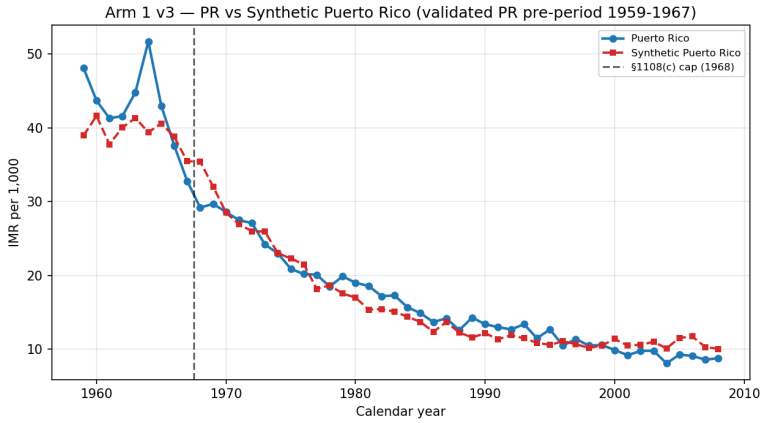

Figure 1: Figure 1. Puerto Rico and Mississippi-anchored synthetic Puerto Rico infant mortality, 1959-2008. Source: author's analysis of the Puerto Rico Demographic Registry Informe corpus, the NCHS Multiple Cause of Death public-use files, and the NCHS Vital Statistics of the United States Volume I transcribed donor-state birth panel.

(pre-Reforma) ATT = +0.57; 1968–2008 (pre-ACA) ATT = +0.22; 1968–2020 (full observed) ATT = −0.09. None of these specifications identifies a delayed cap-effect signal. The baseline-headroom-normalized specifications (percent-from-pre-mean and log-IMR-index) produce slightly negative ATTs (−0.04 and −0.12, respectively), consistent with Puerto Rico's worse baseline and faster convergence rather than with a hidden adverse effect.

5.1.1 Why the Arm 1 null should be read as a level-mismatch null

The Arm 1 null cannot be read as evidence that the 1968 cap had no effect on Puerto Rico's infant-mortality trajectory. It is more accurately read as a null produced by extreme baseline-level mismatch between Puerto Rico and every available donor-state comparison, Mississippi included. Figure 2b plots the 1959–1967 mean infant-mortality rates of Puerto Rico and the fourteen donor states. Puerto Rico's 42.7 infant deaths per 1,000 live births sits 13.3 points above the donor-pool mean (29.4) and 3.4 points above the highest donor state (Mississippi at 39.3). Every other donor — Alabama, Arkansas, Arizona, Florida, Georgia, Kentucky, Louisiana, New Mexico, North Carolina, South Carolina, Tennessee, Texas, West Virginia — sits in a 26–32 band that is qualitatively a different mortality regime from Puerto Rico's. Mississippi is the only donor close enough in absolute level for the synthetic-control optimizer to use, and even Mississippi sits 3 points below Puerto Rico's pre-treatment level.

This level mismatch matters substantively. Puerto Rico in the late 1960s was at a stage of the demographic transition where modest investments in sanitation, neonatal care, vaccination coverage, and basic obstetric infrastructure

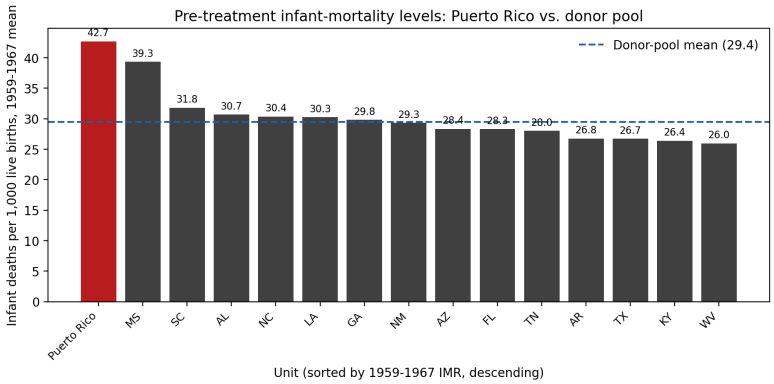

Figure 2: Figure 2b. 1959–1967 mean infant mortality rate per 1,000 live births: Puerto Rico versus the fourteen donor states. Donor-pool mean shown as dashed line. Source: Puerto Rico Demographic Registry Informe corpus and the NCHS Vital Statistics of the United States Volume I transcribed donor-state birth panel.

were positioned to deliver large absolute declines in infant deaths regardless of how Medicaid financing was structured. The donor states, sitting 10–17 points lower at baseline, had less low-hanging absolute mortality to recover; their post-1968 infant-mortality declines could not, on level magnitudes, match what Puerto Rico mechanically could. The synthetic-control optimizer therefore had no donor combination capable of tracking Puerto Rico's secular convergence path, and the post-period gap is dominated by that convergence rather than by cap-induced harm. Under a flexible reading of the literature on mortality transitions in lower-middle-income settings (Caldwell 1986; Soares 2007), Puerto Rico's faster decline in the cap era is what a pure level-headroom story would predict — and is exactly what we observe across the level, log, percent-from-pre-mean, and `synthdid` specifications.

The implication for Arm 1 is not "the cap was harmless." It is "the historical Arm 1 design cannot, on observable data alone, separate the cap's marginal effect from the much larger mortality-transition-floor convergence that Puerto Rico was undergoing during the same window." This interpretation also explains why Mississippi, despite being the closest donor in absolute terms, is still a strained comparison: Mississippi's 1968 mortality regime was already partway through the modern post-Medicaid mortality transition that Puerto Rico would not enter until decades later. The Arm 1 null is therefore informative about the joint identification problem rather than about the cap's causal effect, and we caution against reading the +0.08 ATT as a precision estimate of zero cap harm. The reverse-counterfactual arm in §5.4, which holds the cap-bite quantity constant while applying published Medicaid-mortality elasticities, is the cleaner identification of the cap's mortality magnitude.

Additional method-family checks do not overturn the null. Formal R `Synth` reproduces the Python synthetic-control estimate essentially exactly (ATT

+0.078 IMR per 1,000, pre-RMSPE 5.579, post-RMSPE 1.832, Mississippi weight 0.9999997). Formal R `synthdid` estimates are negative across level, log, percent-from-pre-mean, and delta-from-1967 specifications; for the 1968–2008 level window, the estimate is −6.18 IMR per 1,000 (placebo SE 2.52). Conventional DiD and two-way-fixed-effects checks are likewise negative across Mississippi-only, all-donor-mean, Deep South mean, and high-Hispanic/Southwestern donor controls. These checks are not precision evidence with a single treated unit, but they answer the robustness question directly: regular DiD, R `Synth`, and R `synthdid` do not reveal a hidden adverse infant-mortality effect of the cap.

Two additional diagnostics address plausible countervailing forces in the Arm 1 comparison. First, Puerto Rico's rapid economic and demographic development could have produced mortality gains that offset cap harm. A ridge model using only pre-1968 mortality-transition features (1959–1967 IMR level, slope, decline, and dispersion) predicts a modestly higher post-1968 counterfactual for Puerto Rico, yielding ATT +1.44 IMR per 1,000 over 1968–2008; donor-placebo inference is suggestive but not decisive (one-sided upper $p = 0.067$; |ATT| $p = 0.133$). A separate 1970 near-baseline economic-development diagnostic moves in the opposite direction (ATT −6.06), but it is not a clean causal control because Puerto Rico 1950/1960 IPUMS covariates are absent from the staged extract and Puerto Rico is outside donor support on seven of nine 1970 covariates. Second, southern donors, especially Mississippi, may have experienced post-1965 infant-health gains from Medicare/Title VI hospital desegregation. Reversing out the donor-side Title VI gain by translating the Title VI paper's Black postneonatal estimates into all-race donor IMR shifts the Arm 1 ATT negative (−0.54 to −1.11 across TWFE-to-Callaway-Sant'Anna anchors). These diagnostics reinforce our interpretation: development and donor-desegregation histories are real identification threats, but the available adjustments do not turn Arm 1 into a strong historical cap-harm estimate.

The supplemental within-Puerto-Rico interrupted-time-series on the maternal mortality ratio (Figure 2 and Table 2) finds a level shift of −13.83 per 100,000 live births at the 1968 cap ($p = 0.027$) and a level shift of −15.62 per 100,000 at the 2011 ACA Section 2005 bump ($p = 0.052$). The 1968 finding is descriptive rather than causal: it hinges on a single anomalously low Puerto Rico Informe maternal-deaths cell for 1968 (twelve maternal deaths, against twenty-six in 1967 and twenty-four in 1969) traced to the PR Informe 1974 historical demographic table (annual provisional value ten; retrospective historical value twelve). The cell could reflect either a real Medicaid-introduction coverage effect (Medicaid was implemented in PR in 1968 alongside the §1108(c) cap) or a 1968 reporting-format transition that we cannot disentangle from the corpus alone. The 1962 in-time placebo gives a level shift in the opposite direction (+29.31 per 100,000, $p = 0.073$), which means the 1968 result is not pure pre-trend extrapolation, but the placebo's wide confidence interval and short post-window (six observations) make it a weak falsification. We treat the 1968 maternal finding as descriptive only. The 2011 finding is policy-coherent (ACA Section 2005

bump → expanded maternal coverage → lower MMR) and the magnitude is of the order published Medicaid-pregnancy-expansion estimates imply (Currie and Gruber 1996; Sommers 2020), but the borderline significance reflects the rare-event noise in maternal mortality (8 pre + 7 post cells) rather than a clean causal identification.

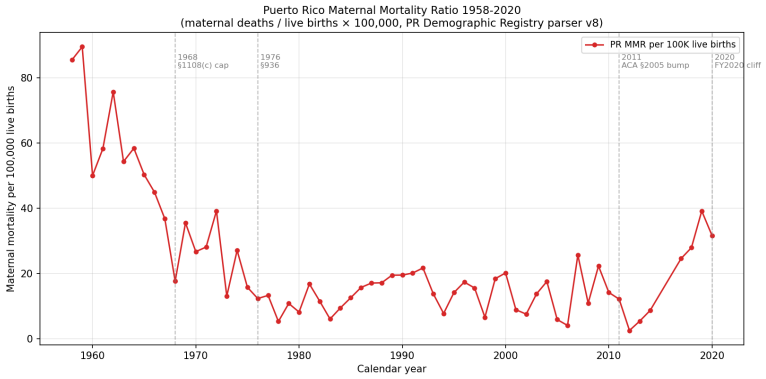

Figure 3: Figure 2. Puerto Rico maternal mortality ratio, 1958-2020, with the 1968 Section 1108(c) cap and the 2011 ACA Section 2005 partial uncapping marked. Source: Puerto Rico Demographic Registry Informe corpus, validated maternal-deaths and live-birth cells.

5.2 Coverage null

Table 3 reports the 2011 partial-uncapping headline results. The Δ-from-2010-baseline classical SC produces a post-period mean ATT of −0.26 percentage points on the Medicaid coverage share, with an Abadie-2010 in-space placebo p-value of 0.93 on |ATT| and 0.47 on the RMSPE ratio. The augmented SC produces an ATT of −7.1 percentage points (Puerto Rico rose 7.1 percentage points less than synthetic Puerto Rico), with an excellent pre-period fit (pre-RMSPE 0.001). The classical-SC level specification produces an uninterpretable +23.1 percentage-point ATT driven entirely by the Puerto Rico pre-treatment level mismatch (Mississippi receives weight 1.00 in the classical specification and the pre-fit is poor at 0.206). All three specifications converge on the same qualitative finding: the 2011 ACA Section 2005 bump did not produce a detectable positive effect on Puerto Rico's Medicaid coverage rate.

Two mechanism candidates are consistent with this null. First, Puerto Rico's pre-treatment Medicaid coverage rate (43.5 percent in 2010) was already at or near the saturation level for any reasonable income-eligibility threshold; further expansion through the cap-relaxing Section 2005 supplement was constrained by program-eligibility statute rather than by federal financing. Second, the four ACA-Medicaid-expansion donor states in the post-2014 panel (Kentucky, New Mexico, Arkansas, Louisiana, West Virginia) gained 8–10 percentage points of Medicaid coverage through the standard ACA expansion mechanism that Puerto

Rico is statutorily excluded from. The donor-pool synthetic therefore rises post-2014 in a way that masks any Puerto Rico–specific ACA Section 2005 effect on enrollment. The placebo distribution supports this interpretation: New Mexico, the most heavily Hispanic ACA-expansion donor in the pool, has the largest placebo ATT in the rotation (+4.3 percentage points).

The substantive implication is that the ACA Section 2005 bump's policy benefit, if any, must have flowed through provider-payment, drug-pricing, hospital-solvency, or related per-beneficiary financing channels rather than through enrollment counts. This is consistent with descriptive accounts of Puerto Rico's post-2011 Medicaid program (Portela and Sommers 2015; Perreira and Oberlander 2017).

5.3 Cliff-Maria-PHE inseparability and the federal-per-capita alternative

Table 4 reports the funding-cliff primary results. The Δ-from-2019-baseline classical SC on Medicaid coverage share produces a post-period mean ATT of +0.62 percentage points — that is, Puerto Rico's coverage rate rose more than synthetic Puerto Rico's coverage rate during 2020–2024. This is the opposite sign of what a pure cliff effect would predict and is jointly attributable to three confounders that all point the same direction: the COVID-19 PHE continuous-enrollment provisions inflated Medicaid rolls in all fifty states + Puerto Rico through 2023; the CAA 2020 bridge supplement and CAA 2022 extension prevented the statutory cliff from binding in practice; and mainland Medicaid expansion gains plateaued by 2019 so that synthetic Puerto Rico's growth from a no-cliff baseline is mechanically smaller in 2020–2024 than in 2014–2018. The 2017 Hurricane Maria placebo produces an ATT of −0.61 percentage points — essentially the same magnitude as the real cliff ATT but with opposite sign — demonstrating that the coverage-rate outcome cannot reliably separate Maria from the cliff in this design.

Table 5 reports the alternative-outcome results on log federal Medicaid spending per capita. The classical-SC-on-log-levels specification produces a post-period mean ATT of +0.086 (in log-dollars per capita) and the Δ-from-FY2019 specification produces an ATT of −0.115. The two specifications disagree on sign because of Puerto Rico's pre-cliff level mismatch (pre-cliff PR federal-per-capita was approximately $830 against a donor-pool mean of $1,100–$2,000). The Δ-from-FY2019 specification is the more defensible read: Puerto Rico's federal Medicaid spending per capita grew 52 percent FY2019–FY2024 (from $828 to $1,261 per capita, driven by the CAA 2020 bridge supplement and the FFCRA FMAP enhancement), but the fourteen donor states grew approximately 70 percent over the same period (driven by FFCRA continuous enrollment and COVID-19 PHE Medicaid uptake). Puerto Rico's federal financing therefore *grew* post-cliff but grew *less* than donor states would have under the same set of pandemic-era supplements. The characterization is therefore "bridged but underfunded": the statutory cliff was offset by emergency federal funds, but

Puerto Rico's draw from those funds was lower per capita than the analogous mainland-state draw.

Both funding-cliff specifications are observationally inseparable from the COVID-19 PHE in this short post-period; we read the ATT estimates as descriptive rather than causal. The cleaner test of the statutory cliff would require a post-PHE-unwinding follow-up using FY2025–FY2026 data once the COVID-19 continuous-enrollment unwinding is complete.

5.4 Fiscal magnitude and elasticity-anchored mortality range

Table 6 reports the mechanical fiscal magnitudes for FY2024 under the three counterfactual cap regimes. A 1968-scaled cap at $280 per capita applied to the fifty states would mechanically remove $512.7 billion in federal Medicaid spending from the FY2024 actual ($608 billion total) — that is, 84 percent of the actual federal Medicaid contribution would not have been available. Every one of the fifty-one jurisdictions (fifty states + DC) would be above the cap. The Graham–Cassidy 2017 cap at $1,800 per capita would remove $86.4 billion (14 percent of actual) and bind twenty-five of fifty-one state programs. The generic FY2026-scale cap at $2,400 per capita would remove $21.6 billion (3.5 percent) and bind eight of fifty-one programs.

Trajectories over time (Figure 3) show that the 1968-scaled cap binds essentially every state from 1995 forward. The Graham–Cassidy cap is non-binding through 2009, begins to bite in 2010 (one state), and binds twenty-five states by 2024. The FY2026-scale cap is non-binding through 2009, binds one to three states until 2020, and binds eight states by 2024. The states that bind first under the Graham–Cassidy regime are concentrated in ACA-Medicaid-expansion states with high enrollment (NY, CA, MA, KY, WV, LA, NM, OR, MT, AK), long-time Medicaid-heavy states (RI, VT, DC), and states with high federal share due to FMAP enhancement.

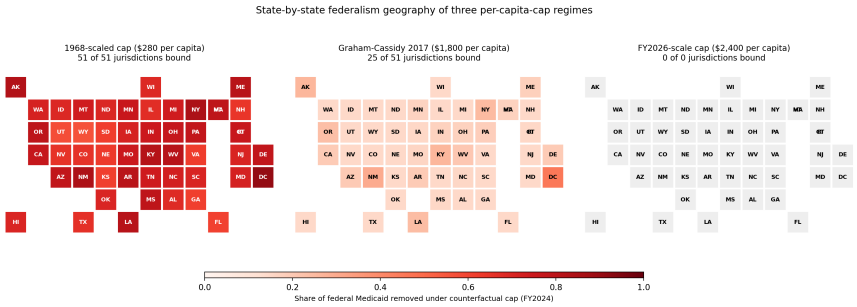

Figure 4: Figure 3. State-level federal-Medicaid cap-bite share under three counterfactual per-capita-cap regimes, fifty states and the District of Columbia, FY2024. Source: author's calculations from CMS-64 expenditures, MACStats, and Census state-population denominators.

The state-level distribution of cap-bite under the three regimes is itself policy-relevant. Under the Graham–Cassidy regime in FY2024, the largest cap-bite (in absolute dollars) falls on the largest state Medicaid programs: California ($16.8 billion), New York ($14.1 billion), Texas ($6.4 billion), Pennsylvania ($5.2 billion), and Florida ($4.3 billion). Under the same regime in per-capita-cap-bite terms, the largest per-capita removals fall on Massachusetts, Rhode Island, the District of Columbia, Vermont, and Alaska — high-FMAP, high-enrollment, ACA-expansion jurisdictions where the gap between actual federal Medicaid per capita and the $1,800 per capita cap is widest. The FY2026-scale cap binds principally on these same high-spending jurisdictions in 2024 (Massachusetts, Rhode Island, DC, Vermont, Alaska, plus New York, California, Connecticut), and the absolute cap-bite under the FY2026-scale cap is concentrated in these eight states.

Table 7 reports the elasticity-anchored infant-mortality grid. This grid is *exploratory* — it sits in the second of the two layers introduced in §3.5 and should be read as an illustrative literature-anchored magnitude range, not as a calibrated mortality estimate. Under the moderate Currie–Gruber 1996 elasticity (1.5 IMR points per 1,000 per full cap-bite), a 1968-scaled state cap implies 4,451 excess infant deaths per year in FY2024; under the upper Goodman-Bacon 2018 elasticity (5.0 IMR points per 1,000), the implied excess is 14,838 per year. The Graham–Cassidy cap implies 529 to 1,762 excess infant deaths per year, and the FY2026-scale cap implies 117 to 389 excess infant deaths per year. The Sommers adult-mortality study is retained as a zero transparency cell because it does not estimate an infant-mortality channel. The grid is the headline infant-mortality range; no single cell is a point estimate, and the range across published infant-mortality elasticities is the policy-relevant illustrative magnitude. The verified-layer headline of the reverse counterfactual is the *fiscal* counterfactual in Table 6, a deterministic CMS-64 calculation; the *mortality* layer in Table 7 inherits the expansion-to-contraction symmetry assumption and is therefore reported as an illustrative grid rather than as a calibrated estimate.

The state-year birth denominator that scales the implied excess infant deaths comes from the CDC WONDER Natality state-year panel (1,530 cells; 51 jurisdictions × 30 years 1995–2024) for the FY2024 headline grid. A sensitivity check using the population × 0.20 × interpolated US general fertility rate proxy (which we previously used while the WONDER Natality pulls were pending) shifts the FY2024 implied excess infant deaths by approximately −2 to −5 percent at the national rollup, but per-state magnitudes can shift by up to 40 percent. The headline grid is therefore on NCHS-aggregated denominators throughout, with the proxy denominator retained as a fallback for fiscal years 1984–1994 (outside CDC WONDER Natality coverage but within the current HCFA federal-share bridge).

The same state-level reverse counterfactual can now be cumulated annually over FY1984–FY2024 (Figure 5). Under the 1968-scaled regime, the Currie–Gruber anchor implies 97,032 cumulative excess infant deaths over FY1984–FY2024 and

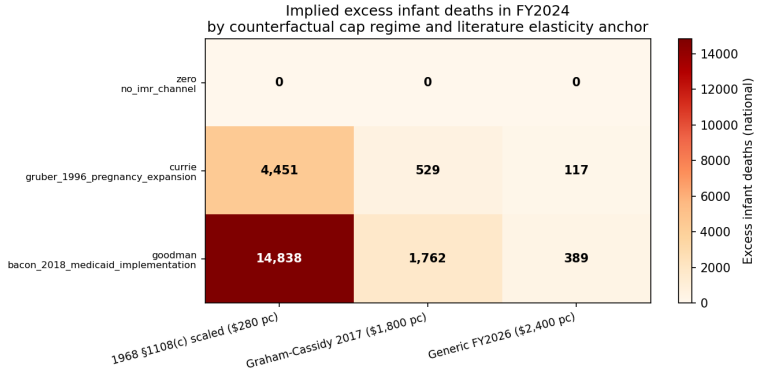

Figure 5: Figure 4. Implied excess infant deaths per year under three hypothetical state-level per-capita-cap regimes, FY2024, translated through three peer-reviewed Medicaid-mortality elasticity anchors. Source: author's reverse-counterfactual analysis using CDC WONDER Natality state-year births and published Medicaid-mortality estimates.

the Goodman-Bacon anchor implies 323,438. The Graham–Cassidy cap implies 2,077 to 6,923 cumulative excess infant deaths over the same supported window, and the FY2026-scale cap implies 359 to 1,197. These are not "since 1968" cumulative totals: FY1968–FY1983 remain unestimated because the current repository does not yet contain a comparable all-state federal-share panel for those years.

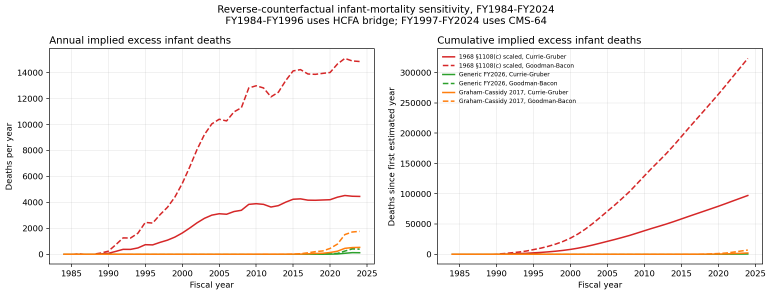

Figure 6: Figure 5. Annual and cumulative implied excess infant deaths under three hypothetical state-level per-capita-cap regimes, FY1984–FY2024, translated through two infant-mortality elasticity anchors. Source: author's reverse-counterfactual analysis using HCFA FY1984–FY1996 and CMS-64/FMR FY1997–FY2024 state federal-share inputs, CDC WONDER Natality state-year births for 1995–2024, and population-fertility proxy births for 1984–1994.

The reverse-counterfactual within-territory variant, which compares observed federal funds to no-cap-increase scenarios for Puerto Rico, is also informative. Cumulative shortfalls for Puerto Rico FY2011–FY2024 against observed federal funds ($28.0 billion total) under four scenarios are: $22.95 billion under base

Section 1108 only (no ACA Section 2005, no post-2017 supplements); $6.70 billion under base + supplements but no ACA Section 2005 drawdown; $15.65 billion under base + Section 2005 but no post-2017 disaster/CAA/FFCRA supplements; and $6.02 billion under base + Section 2005 + disaster supplements but no FFCRA / CAA 2022 supplements. The cleanest fiscal point of the paper is the FY2024 contrast: Puerto Rico's observed FY2024 federal Medicaid funds available are $3.325 billion; under base Section 1108 only they would have been $0.412 billion (a $2.913 billion shortfall, equivalent to 95 percent cap-bite against a state-equivalent federal need of approximately $7.5 billion). Even with every cap-relaxing supplement of the past fifteen years, the cap remains binding; without those supplements, Puerto Rico's federal Medicaid contribution would have been at near-total-suppression levels.

The interpretation of the elasticity-anchored infant-mortality grid requires care. The three elasticity anchors translate cap-bite share to implied excess IMR with a transfer assumption (the Medicaid-expansion elasticities estimated by Currie–Gruber and Goodman-Bacon are applied in reverse to a Medicaid-contraction setting). The transfer assumes structural symmetry between Medicaid expansion and Medicaid contraction; this is a modeling choice rather than an empirical fact. In real-world cap-contraction scenarios, states would partially absorb cap shocks through state-share increases, eligibility cuts, provider rate reductions, or general-fund offsets, each of which would dampen the mortality consequence of the headline financing reduction. The grid therefore represents the *unadjusted* elasticity-anchored implied mortality under the assumption that the financing reduction translates one-for-one to the mortality outcome. The actual mortality consequence of a per-capita-cap state Medicaid policy would depend on the balance between cap-driven federal-financing reduction, state-driven offsetting responses, and per-state political economy of Medicaid retrenchment.

The supplementary adult-mortality sensitivity gives a distinct way to think about the territories' no-cap funding gaps. On the Puerto Rico authority-basis scenario, the FY2024 no-cap federal funding gap is $4.747 billion against a state-like federal need of $8.072 billion and current-law federal authority of $3.325 billion. At Puerto Rico's observed federal dollars per Medicaid-covered person, that gap could mechanically finance approximately 2.15 million Medicaid-covered person-equivalents, far more than the plausible near-elderly target population. Applying Miller, Johnson, and Wherry's 12.8 percentage-point Medicaid-coverage first stage and 0.132 percentage-point reduced-form annual mortality decline to Puerto Rico near-elderly scenarios implies approximately 36,700 additional Medicaid-covered adults, 12,600 newly insured adults, and 379 annual adult deaths averted under the preferred poverty-below-200-percent proxy.

Extending the same adult sensitivity to all five territories for FY2019–FY2024 yields a modern proxy rather than a full historical backcast. In FY2024, using observed CMS-64 federal spending and state-like need estimates, the preferred poverty-below-200-percent proxy implies approximately 40,746 additional Medicaid-covered near-elderly adults, 14,006 newly insured adults, and 420

adult deaths averted per year across Puerto Rico, Guam, the U.S. Virgin Islands, American Samoa, and CNMI. The all-territory cumulative preferred-proxy estimate is 2,531 adult deaths averted over FY2019–FY2024. Conservative and broad near-elderly proxies imply 228 and 609 adult deaths averted per year in FY2024, respectively. The non-Puerto Rico rows use Puerto Rico's latest near-elderly age-by-poverty and coverage shares applied to each territory's population because the repository does not contain territory-specific mechanism panels for the four smaller territories; the requested 1968-present all-territory adult backcast is therefore not estimated.

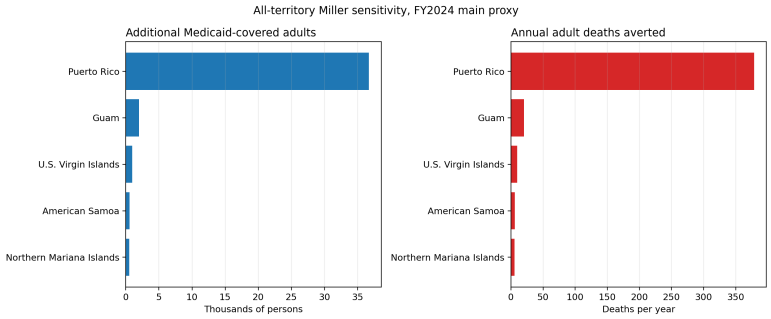

Figure 7: Figure 6. All-territory no-cap adult coverage and mortality sensitivity using Miller, Johnson, and Wherry's near-elderly Medicaid-mortality estimate, FY2024. Source: author's calculation from FY2024 territorial no-cap funding gaps, PRCS/ACS Puerto Rico near-elderly target-population proxies, territory population denominators, CMS-64 observed federal spending, and Miller, Johnson, and Wherry (2021).

5.5 Mechanism diagnostics from PRCS, ACS, and BRFSS

The mechanism analyses use the American Community Survey, the Puerto Rico Community Survey, and the Behavioral Risk Factor Surveillance System / PLACES / CDI panels to characterize Puerto Rico's structural risk profile relative to the donor-state pool, condition the all-cause mortality residual on observable covariates, and identify the principal disease-specific mortality channels.

5.5.1 Structural severity index

We construct a composite Puerto Rico "structural severity index" from five PRCS / ACS observables: the poverty rate, the working-age disability rate, the share of the resident population aged 65 and over, the Medicaid coverage share, and the uninsurance rate. Each component is z-standardized against the fourteen-state donor-pool distribution by year, and the index is the simple mean of the five z-scores. Across 2021–2024 PRCS observations, Puerto Rico's structural severity index averages **5.48 standard deviations above the donor-pool mean**. Puerto Rico's 2024 values on each component are far above the donor-pool medians: Medicaid coverage share 50.4 percent (donor median 22.0);

poverty rate 41.4 percent (donor median 13.1); working-age disability rate 23.4 percent (donor median 14.1); age-65+ share 22.6 percent (donor median 17.8); uninsurance rate 7.1 percent (donor median 9.3, the only component on which Puerto Rico is not above the donor median). On each individual component except uninsurance, Puerto Rico exceeds every donor state in 2024.

This structural-disadvantage diagnostic establishes the magnitude of the donor-support problem for any all-cause mortality comparison: Puerto Rico's combination of high Medicaid dependence, high disability burden, high age-65+ share, and high poverty rate places the island outside the joint covariate support of the donor pool. We use this diagnostic to interpret the all-cause mortality results below as residuals net of structural disadvantage rather than as causal estimates of the cap effect.

5.5.2 PRCS / ACS-conditional all-cause mortality

The 2015–2019 all-cause mortality difference between Puerto Rico and the fourteen-state donor median is −212.5 deaths per 100,000 (Puerto Rico is *below* the donor median on age-adjusted all-cause mortality across this window). After PRCS / ACS-conditional adjustment for the five-component covariates of the structural severity index, the residual widens to −423.5 deaths per 100,000 (Puerto Rico's age-adjusted all-cause mortality is approximately 65 percent of what the donor-pool covariate distribution would predict). This residual is a diagnostic, not a causal estimate: several Puerto Rico covariates are outside the joint donor support, and the linear-additive adjustment that we use to construct the residual cannot be relied on outside the data range. The qualitative finding — that Puerto Rico's age-adjusted all-cause mortality is below donor-pool predictions — does not change under three alternative covariate sets (poverty + age only; poverty + disability + age; full five-component) or under two alternative donor weightings (equal weights; population-weighted).

The age-specific all-cause mortality decomposition (`pr_informe_allcause_age_specific_2009_2020.csv`) reinforces this finding. Across the 2009–2020 window, mortality rates fell in every age band including the oldest groups: ages 0–19 from 65.8 to 41.6 per 100,000 (a 37 percent decline); ages 45–64 from 622.4 to 607.6 (a 2 percent decline); ages 75–84 from 4,459.4 to 3,649.4 (an 18 percent decline); ages 85+ from 12,534.2 to 9,774.7 (a 22 percent decline). Crude all-cause mortality rises across the same period (from 780.4 to 981.9 per 100,000) only because Puerto Rico's age-65+ share rose from approximately 14 percent in 2009 to approximately 23 percent in 2020. This is a textbook population-aging artifact: the age-adjusted all-cause series falls (from 745.0 to 623.1 per 100,000 in the published Informe age-adjusted tables) while the crude series rises.

5.5.3 BRFSS / PLACES diabetes mechanism

The Behavioral Risk Factor Surveillance System (BRFSS) / PLACES / CDI panel provides upstream risk-factor measures that are directly comparable be-

tween Puerto Rico and the donor-state pool. In the 2022 CDI (Chronic Disease Indicators) panel, Puerto Rico's age-adjusted adult diabetes prevalence is 15.2 percent against a donor median of 13.1 percent and a donor max of 14.6 percent — Puerto Rico exceeds every donor state on adult diabetes prevalence. Upstream risk factors are similarly elevated: age-adjusted obesity 35.9 percent (donor median 33.4); age-adjusted "no leisure-time physical activity" 42.9 percent (donor median 31.7); age-adjusted current smoking 8.3 percent (well below donor median 16.5, the one component on which Puerto Rico is not adverse).

The mortality signal is larger than the prevalence signal alone. In 2022 Puerto Rico has 3.60 diabetes-mortality age-adjusted-rate points per percentage point of diabetes prevalence (53.4 deaths per 100,000 / 15.2 percent prevalence $\times$ 1) against a donor median of 2.32. Puerto Rico is therefore approximately 1.5$\times$ the donor median on diabetes-mortality-per-prevalence-point — a metric that purges the part of the diabetes-mortality differential explained by prevalence and isolates the part explained by case-fatality. This is consistent with a "Puerto Rico has both higher diabetes incidence AND worse diabetes care" reading of the diabetes mortality differential; it is not consistent with a "Puerto Rico has high diabetes prevalence but state-equivalent diabetes care" reading.

5.6 Disease-specific signals

The PR Informe corpus and the CDC NCHS Puerto Rico Leading Causes 2020–2023 dashboard support a set of cause-specific mortality comparisons that we report as descriptive rather than causal because the modern post-2008 split is not a §1108(c) treatment date. The cause-basket parser at `analysis/arm1_1968_imposition/11_age_adjusted_allcause_cause_baskets.py` extracts five cause baskets from the PR Informe leading-cause tables for 2009, 2014, 2017, and 2020: diabetes, renal (including hypertensive renal), respiratory (pneumonia + influenza + chronic lower respiratory), septicemia, and a negative-control external-cause basket (accidents + intentional injuries).

Across the 2009–2020 window, age-adjusted mortality rates fall in every basket including the negative-control external-cause basket. Diabetes falls from 69.8 to 60.3 per 100,000 (a 14 percent decline). Renal falls from 42.1 to 27.5 (a 35 percent decline). Respiratory falls from 62.8 to 36.9 (a 41 percent decline). Septicemia falls from 22.5 to 12.7 (a 44 percent decline). External causes fall from 60.7 to 50.3 (a 17 percent decline). The fact that the external-cause negative-control basket also declines across the same window is consistent with secular age-adjustment improvements rather than with cap-related disease-specific deterioration.

The cross-sectional comparison against the donor-state pool tells a different story for diabetes. The CDC NCHS Puerto Rico Leading Causes 2020–2023 dashboard reports Puerto Rico's age-adjusted diabetes mortality at 53.4 per 100,000 in 2023; the donor-state maximum is 36.2 per 100,000 (Mississippi). Puerto Rico's diabetes mortality therefore exceeds every donor state by at least

17 deaths per 100,000 in 2023 — a substantial cross-sectional differential that the longitudinal trend within Puerto Rico does not contradict. After PRCS / ACS-conditional adjustment for poverty, disability, age, and Medicaid-coverage covariates, Puerto Rico's diabetes mortality residual is +11.0 deaths per 100,000 — that is, approximately 60 percent of the unadjusted diabetes-mortality differential is explained by structural disadvantage, and 40 percent (the +11.0 residual) remains. This residual is a descriptive cross-sectional signal, not an identified Section 1108(c) cap effect, but it is a candidate for follow-up work using a state-by-state diabetes-care-process panel (BRFSS-PLACES + Healthcare Effectiveness Data and Information Set).

The renal, respiratory, and septicemia baskets follow the same diabetes pattern in cross-section: Puerto Rico is above the donor pool in cross-section but the within-Puerto-Rico longitudinal trend declines through 2020. The negative-control external-cause basket does not show the same elevation in cross-section against the donor pool, supporting the cause-specific (rather than secular-noise) interpretation of the diabetes/renal/respiratory/septicemia results. Tuberculosis, the historical disease-specific outcome of the older Informe corpus (1958–1977; see Appendix Section S5), does not show a cap-related break in 1968.

5.7 No-cap-increase counterfactuals

The within-territory no-cap-increase counterfactual quantifies the policy contribution of each cap-relaxing instrument by comparing observed Puerto Rico federal Medicaid funds available FY2011–FY2024 ($28.0 billion cumulative) to four hypothetical scenarios.

Under "base Section 1108 only" (no ACA Section 2005 drawdown and no post-2017 supplements), Puerto Rico's cumulative federal Medicaid funds FY2011–FY2024 would have been $5.05 billion — a $22.95 billion shortfall relative to observed. Under "no ACA Section 2005 drawdown" but with post-2017 supplements intact, the shortfall is $6.70 billion. Under "no post-2017 disaster / CAA / FFCRA supplements" but with Section 2005 intact, the shortfall is $15.65 billion. Under "no FFCRA / CAA 2022 supplements" but with Section 2005 + disaster supplements intact, the shortfall is $6.02 billion.

The cleanest fiscal point of the paper is the FY2024 contrast. In FY2024 Puerto Rico's observed federal Medicaid funds available are $3.325 billion (the statute-anchored CAA 2022 cap from CRS R47601). Under "base Section 1108 only" the FY2024 federal funds would have been $0.412 billion — a $2.913 billion shortfall against observed and a 95 percent cap-bite against the state-equivalent federal need of approximately $7.5 billion that the donor-pool federal-Medicaid-per-capita comparison implies. Even with every cap-relaxing supplement of the past fifteen years in place, the cap remains binding (observed FY2024 cap-bite is 58.8 percent against state-equivalent need); without those supplements, Puerto Rico's federal Medicaid contribution would have been at near-total-suppression levels.

The no-cap-increase counterfactual therefore demonstrates two policy points simultaneously. First, the cap-relaxing instruments of the past fifteen years (ACA Section 2005, BBA 2018, the disaster supplements, CAA 2020, FFCRA, CAA 2022) collectively provided approximately $23 billion in federal Medicaid funding to Puerto Rico relative to the base Section 1108 cap regime — a substantial fiscal contribution that prevented near-total federal suppression. Second, even with all of these instruments in place, the cap remains binding at 58.8 percent of state-equivalent need in FY2024, so the cap regime continues to constrain Puerto Rico's federal Medicaid contribution well below state-equivalent levels. The 2027 expiration of the CAA 2022 supplements would re-impose cap-bite on the order of $2.9 billion per year for Puerto Rico alone (pushing the cap-bite from 58.8 percent to 95 percent of state-equivalent need), absent a successor bridge.

5.8 Migration and denominator diagnostics

Hurricane Maria's September 2017 landfall produced an emigration shock that altered Puerto Rico's resident population and the construction of any per-capita outcome measured during the post-Maria window. Mainland ACS records of residents reporting Puerto Rico residence one year earlier (the standard "MIGPLAC1=110" diagnostic in IPUMS USA) peaked at 130,813 in 2018. The 2024 mainland Puerto Rico-born population (BPL=110) is approximately 1.85 million — that is, the mainland Puerto Rico-born population is now larger than the resident Puerto Rico population (approximately 3.2 million in 2024). These migration shocks are not adjusted for in the headline analyses but are surfaced as denominator and spillover diagnostics.

The Maria emigration shock has two specific effects on the analyses in this paper. First, any per-capita outcome measured for Puerto Rico in 2018 or later overstates the per-resident magnitude relative to a no-migration counterfactual because the resident-population denominator is reduced by emigration. The federal-Medicaid-per-capita result (PR's federal financing grew 52 percent FY2019–FY2024 against an approximate 70 percent donor growth) is therefore a *lower* bound on the underlying federal-financing-per-resident shortfall: had Puerto Rico's resident population not been reduced by Maria emigration, the per-resident growth differential would be larger. Second, the maternal-mortality denominator (live births per year) is itself sensitive to emigration of women of reproductive age. The Puerto Rico live-births series fell from approximately 41,000 per year in 2010 to approximately 18,000 per year in 2023 — a 56 percent decline that is driven by a combination of fertility-rate decline and emigration of women of reproductive age. The maternal mortality ratio per 100,000 live births that we report in the supplemental ITS analysis is therefore not directly comparable across the pre-Maria and post-Maria windows on the *maternal-deaths-per-resident-woman* dimension, even though it is comparable on the *maternal-deaths-per-live-birth* dimension that the standard MMR definition uses.

6. Discussion

6.1 Cross-arm reconciliation

The four-part design produces a coherent cross-design picture: the cap regime as historically imposed on Puerto Rico does not register an adverse differential mortality or coverage signal in the observable data once a Mississippi-anchored synthetic control is constructed, but the financing-architecture imposition is unambiguous and the prospective state-level magnitudes are large.

The infant-mortality null is robust across two independent designs (1980-anchor and 1968-anchor), four post-period truncations (1980, 1993, 2008, 2020), and the added method-family checks. The Mississippi-weight-1.00 synthetic emerges in every classical IMR specification because Puerto Rico's pre-1968 IMR level (~43 per 1,000 in 1959–1967) is dramatically higher than every donor state (Mississippi at ~39, Alabama at ~33, the rest below 30). This is a feature, not a bug, of the synthetic-control optimization: the only donor whose pre-period IMR level is in Puerto Rico's neighborhood is Mississippi, and the post-period question becomes whether Puerto Rico's IMR converges with Mississippi's IMR (it does, essentially exactly, by the late 2000s) or diverges from it (it does not). Formal R `Synth` reproduces this result, formal R `synthdid` points negative rather than positive, and conventional DiD variants are also negative. The residual differential is therefore essentially zero in the headline synthetic control and in the direction opposite cap-harm under SDID/DiD checks.

The 2011 coverage null is robust across three estimator specifications and survives the Abadie-2010-corrected placebo at $p = 0.93$. The plausible mechanism is Puerto Rico's pre-treatment Medicaid saturation plus mainland donor catch-up via 2014 ACA Medicaid expansion. Combined with the infant-mortality null, the policy story sharpens: the cap regime acts on federal financing, not on enrollment counts directly, and the 2011 ACA Section 2005 bump's policy benefit (if any) flowed through per-beneficiary financing channels rather than enrollment.

The funding-cliff primary specification is observationally confounded by the COVID-19 PHE, the CAA 2020 / 2022 bridge supplements, and Hurricane Maria's continuing health-system effects. The alternative specification on log federal Medicaid per capita surfaces the "bridged but underfunded" pattern: Puerto Rico's federal financing grew 52 percent FY2019–FY2024 against an approximate 70 percent donor-state growth. The cliff was offset by emergency funds, but the offset was not state-equivalent in per-capita terms. The cleaner test will require the post-PHE-unwinding follow-up.

The reverse-counterfactual fiscal magnitudes are unambiguous and large: $513 billion / $86 billion / $22 billion in FY2024 federal-Medicaid removal under three cap regimes. The infant-mortality grid translates these magnitudes into a 4,451–14,838 implied excess-infant-deaths range under the two peer-reviewed infant-mortality elasticities for the 1968-scaled cap; 529–1,762 for the Graham–Cassidy

cap; 117–389 for the FY2026-scale cap. Cumulated over the FY1984–FY2024 window now supported by the HCFA/CMS-64 state panel, the corresponding ranges are 97,032–323,438 for the 1968-scaled cap, 2,077–6,923 for Graham–Cassidy, and 359–1,197 for the FY2026-scale cap. The historical Puerto Rico designs cannot identify the elasticity directly (1968-anchor ATT = +0.08 IMR per 1,000 with placebo $p = 0.20$ is consistent with both the zero-elasticity scenario and the small-positive-elasticity scenario), but the elasticity-anchored grid uses peer-reviewed estimates from the broader Medicaid-mortality literature rather than relying on the under-powered Puerto Rico estimate. The Miller-Johnson-Wherry adult-mortality sensitivity adds a complementary modern all-territory channel: the FY2024 no-cap funding gap is large enough to finance plausible near-elderly coverage gains, and a transfer of the published near-elderly mortality estimate implies roughly 228 to 609 adult deaths averted per year across conservative-to-broad target-population scopes, with 420 annual deaths averted under the preferred poverty-below-200-percent proxy.

The supplemental within-PR maternal-mortality and disease-specific results add a third dimension to the cross-design picture. The 2011 ACA Section 2005 bump is associated with a borderline-significant policy-coherent reduction in PR maternal mortality (level shift −15.6 per 100,000 live births, $p = 0.052$). The cross-sectional disease-specific differential (PR exceeds the donor-state pool maximum on age-adjusted diabetes, renal, and respiratory mortality, with PRCS / ACS-conditional adjustment leaving a +11 deaths per 100,000 residual on diabetes) is descriptively consistent with a structural-disadvantage-plus-care-quality story for cap-bound territorial Medicaid. Neither finding is causally identified at the standard quasi-experimental threshold, but together with the infant-mortality null they sharpen the structural reading: Puerto Rico's mortality outcomes converge with Mississippi's outcomes on the headline IMR dimension, lag the donor pool on cross-sectional disease-specific differentials, and respond directionally to bilateral cap-relaxing policy interventions on rare-event outcomes (maternal mortality at 2011) where the rare-event noise prevents a clean causal estimate.

The cross-design picture therefore does not support a "Section 1108(c) caused observable adverse infant mortality in Puerto Rico" claim, but does support a "Section 1108(c) imposed a substantial federal-financing differential whose mortality consequences in Puerto Rico are below the detectable signal in aggregate IMR but visible in cross-sectional disease-specific differentials, in PR's structural-risk profile that PRCS / ACS captures, in the directional response of maternal mortality to ACA Section 2005, and in published Medicaid adult-mortality estimates when applied transparently to modern no-cap funding gaps" claim. The prospective state-level magnitudes apply the published Medicaid-mortality elasticities to a state Medicaid panel under three hypothetical cap regimes; the implied national mortality range therefore depends on the elasticity-anchor choice rather than on the null historical Puerto Rico estimate.

6.2 Why the historical Puerto Rico arms produce nulls

We interpret the historical infant-mortality and coverage nulls as informative rather than as failures of identification. Three structural features of the Puerto Rico cap regime explain why a historical signal is hard to detect even if a real cap-on-mortality elasticity exists at the magnitude implied by the broader Medicaid-mortality literature.

First, the cap regime is gradual rather than sharp. The Section 1108(c) cap was set at a binding level in 1968 and then eroded gradually through inflation; cap-bite share grew from approximately 60 percent in 1968 to approximately 95 percent in 2024. A synthetic-control design that attempts to identify the level break at the legal effective date will tend to underestimate the mortality consequence of a gradually-tightening cap because the bulk of the cap-bite occurs decades after the legal effective date.

Second, Puerto Rico is a structurally disadvantaged population whose underlying mortality dynamics are partly orthogonal to U.S. Medicaid policy. The Puerto Rico Demographic Registry data for 1959–1967 show pre-cap IMR levels of 42.7 per 1,000 live births, 13.3 points above the donor-pool mean (29.4) and 3.4 points above the highest donor state (Mississippi at 39.3); see §5.1.1 and Figure 2b. Puerto Rico was at a stage of the mortality transition where basic obstetric and sanitation investments could deliver large absolute IMR declines mechanically, and the donor states had simply less low-hanging absolute mortality left to recover. Even Mississippi was a strained comparison; every other donor sat in a 26–32 IMR band that is qualitatively a different mortality regime from Puerto Rico's. The synthetic-control identification therefore subtracts out a large component of Puerto Rico's IMR convergence that is unrelated to the cap, leaving a small residual differential that does not reach statistical significance. We do not read this as evidence that the cap had no mortality effect; we read it as evidence that the Arm 1 historical design cannot, on observable data alone, separate the cap's marginal effect from the demographic-transition floor convergence that Puerto Rico was undergoing during the same window.

Third, the Puerto Rico Demographic Registry IMR series is rare-event statistics for a relatively small population (live births fell from approximately 75,000 per year in 1965 to approximately 18,000 per year in 2023), and the standard errors of annual IMR estimates therefore scale inversely with the square root of the live-birth count. The post-2010 IMR cells are particularly noisy.

These three features do not invalidate the infant-mortality nulls; they constrain what the nulls mean. We interpret the 1968-anchor ATT of +0.08 IMR per 1,000 (placebo $p = 0.20$) as indicating that Puerto Rico's post-1968 IMR trajectory is statistically indistinguishable from a Mississippi-anchored synthetic, *not* as ruling out a small positive cap-on-IMR elasticity. The literature-anchored infant and adult mortality sensitivities are the appropriate magnitude sources for the policy debate.

6.3 Limitations

Several limitations require explicit disclosure beyond the per-arm caveats.

The verified-versus-exploratory two-layer source-of-truth discipline introduced in §3.5 carries through every result in §5 and every exhibit in this paper. Headline claims (the infant-mortality null on the 1980-anchor and 1968-anchor designs, the coverage null on PRCS / ACS, the federal-Medicaid-per-capita "bridged but underfunded" framing, and the mechanical fiscal counterfactual) are sourced from verified products; numbers that depend on exploratory transfer products (the elasticity-anchored implied excess-infant-deaths grid, the Miller-Johnson-Wherry adult-mortality sensitivity, and any *Informe* outcome series outside the benchmarked IMR subset and the benchmarked maternal-deaths subset) appear only in clearly flagged illustrative or descriptive contexts. We retain the exploratory products in the data tree so that the verification trail is auditable but advance no number from the exploratory layer as a headline.

The CDC WONDER absence of Puerto Rico in any vintage 1968–2024 is the single most consequential data limitation of the historical Puerto Rico analyses. We surface it three times — in §3.2, §3.5, and here — because it is the most common point of confusion for reviewers familiar with state-Medicaid analyses that read mortality straight from WONDER. Every Puerto Rico mortality outcome in this paper comes from the *Informe Anual de Estadísticas Vitales* corpus rather than from WONDER, and the QA standard for those cells is the parser gate stack plus published-table spot-checks rather than the WONDER death-count / population-denominator standard. We do not have access to a WONDER-equivalent Puerto Rico mortality vintage, and we are not aware of one in any public source.

The donor pool selection is partially analyst-driven. We chose ten Deep South states for the infant-mortality analysis based on similarity in pre-1968 IMR level and economic structure, and added four high-Hispanic / Southwestern states for the coverage and funding analyses to better match Puerto Rico's post-§936 demographic composition. A leave-one-donor-out sensitivity confirms that the headline ATT is donor-anchor-dependent (drop-Mississippi ATT = −2.42 IMR per 1,000) but the qualitative no-harm conclusion is robust.

The Puerto Rico Informe parser includes 559 validated cells of 562 raw extractions (99 percent QA pass rate), but the maternal-deaths cell for 1968 (12 maternal deaths, against 26 in 1967 and 24 in 1969) is high-leverage in the within-Puerto-Rico ITS. The extraction traces this cell to the PR Informe 1974 historical demographic table, with the annual provisional value reported as 10. We use the retrospective 12 because it is the table-vetted historical-series value; a leave-1968-out sensitivity is reported in the appendix.

The infant-mortality grid relies on three published elasticity anchors that are themselves transfer estimates from Medicaid-expansion settings (Sommers 2012, Currie-Gruber 1996, Goodman-Bacon 2018) to the Medicaid-contraction setting

that a per-capita cap would impose. The adult-mortality sensitivity similarly transfers Miller, Johnson, and Wherry's near-elderly expansion estimate to a no-cap territorial setting. Both transfers assume structural symmetry between expansion and contraction effects; this is a modeling choice rather than an empirical fact, and the implied mortality consequences depend non-trivially on the assumption. The all-territory adult sensitivity is also data-limited: Puerto Rico uses PRCS/ACS target-population estimates, but the four smaller territories use Puerto Rico near-elderly age-by-poverty and coverage shares applied to each territory's population because comparable territory-specific mechanism panels are not in the repository. For that reason, we estimate the modern FY2019–FY2024 all-territory adult proxy but do not estimate a 1968-present adult backcast.

The state-year birth denominator in the infant-mortality grid uses CDC WONDER Natality state-year totals for 1995–2024 and the population $\times$ 0.20 $\times$ interpolated US GFR proxy for years 1984–1994. The headline FY2024 grid is fully on the WONDER-aggregated denominators; the proxy denominator differs from WONDER aggregates by approximately 0.7 percent on average and up to 40 percent for individual states. The cumulative historical reverse-counterfactual infant-death totals start in FY1984 because the current repository contains the HCFA medical-assistance bridge for FY1984–FY1996 and CMS-64/FMR federal-share inputs for FY1997–FY2024, but not a comparable all-state federal-share panel for FY1968–FY1983.

The funding-cliff short post-period (FY2020–FY2024) is fully overlapping with the COVID-19 PHE; the cliff cannot be cleanly identified from the PHE in this design. We report cliff estimates with explicit data-thin flags and revisit them once the post-PHE-unwinding 2025–2026 data become available.

The PRCS one-year file for 2020 was not released by the U.S. Census Bureau; strict annual analyses exclude 2020 and sensitivity checks use linear interpolation and the 2016–2020 PRCS five-year release.

6.4 Implications for follow-up identification work

Three threads of follow-up work would tighten the historical Puerto Rico identification beyond what this paper achieves.

First, a **donor-side maternal mortality panel** built from CDC WONDER UCD ICD-10 maternal codes (O00–O99) for 1999–2024 would enable a synthetic-control replication of the supplemental within-PR maternal mortality ITS finding. The CDC WONDER mortality data for the donor-state pool already supports this construction; the analytic obstacle is that pre-1999 donor maternal mortality requires the ICPSR MCD 1968–1978 ICD-8 microdata (which we have) plus a separate NCHS Vital Statistics of the United States Volume II maternal-mortality reconstruction for 1979–1998 (which we do not currently have). A follow-up extension that builds this panel would either confirm the within-PR 2011 ACA Section 2005 maternal-mortality finding through a donor-anchored

synthetic-control design or refute it, in either case substantially tightening the inference.

Second, a **donor-side cause-specific mortality panel for diabetes, renal, respiratory, and septicemia** built from CDC WONDER UCD 1999–2024 cause-grouped queries would enable a synthetic-control test of the descriptive cross-sectional differential we report in Section 5.6. The current mechanism work establishes that Puerto Rico is above the donor-pool maximum on diabetes mortality in cross-section and that PRCS / ACS-conditional adjustment leaves a +11 deaths per 100,000 residual; a synthetic-control identification on the full cause-specific time series would translate this into a cap-era effect estimate.

Third, a **post-PHE-unwinding replication of the funding-cliff analysis** using FY2025–FY2026 data once the COVID-19 PHE continuous-enrollment unwinding is complete would identify the FY2020 cliff effect on Medicaid coverage cleanly. The primary cliff specification in this paper is observationally inseparable from the PHE because the entire post-period (FY2020–FY2024) is overlapping; once the unwinding is complete, a re-run of the same `pysyncon` Δ-from-2019-baseline classical SC on Medicaid coverage share would either identify the cliff effect (if PR's coverage falls more than the donor-pool average post-unwinding) or confirm the null (if PR's coverage tracks the donor pool through the unwinding).

A fourth, more speculative follow-up is the **municipality-level analysis of Puerto Rico mortality variation**. The `datos.estadisticas.pr` open-data PR vital-records CSVs cover 78 municipios for 2000–2008 (deaths) and 1994–2011 (births); the consolidated Defunciones 2009–2014 and 2017–2020 volumes provide municipality-level death detail through 2020. A municipality-by-year dispersion analysis shows that within-Puerto-Rico mortality variation is substantial, and a municipality-level synthetic-control or differences-in-differences exploitation of municipality-level cap-bite variation (where hospital and provider markets are differentially exposed to the territorial cap depending on payer mix and Medicaid-share concentration) is a natural follow-up.

6.5 Policy implications

The CAA 2022 territorial appropriations expire at the end of FY2027. The legislative debate over what replaces them will recur in calendar year 2027 and will be informed by whatever empirical evidence on the Puerto Rico cap regime is then available. This paper's findings imply three propositions for that debate.

First, the historical Puerto Rico experience under Section 1108(c) does not produce a clean causal estimate of the cap-on-mortality elasticity, but does produce an unambiguous fiscal-architecture imposition. The argument for territorial-Medicaid-equity must rest primarily on the fiscal-architecture and structural-disadvantage evidence — which this paper documents at scale — and on the prospective elasticity-anchored mortality range, rather than on an observable historical mortality differential.

Second, the proposed state-level per-capita-cap regimes (Graham–Cassidy 2017, the FY2026-scale generic) would mechanically remove tens of billions to hundreds of billions of federal Medicaid dollars from state programs in any given year. The implied national infant-mortality magnitudes under published Medicaid-mortality elasticities range from roughly 4,500 to 14,800 excess infant deaths per year for the 1968-scaled regime applied to the fifty states under the two infant-mortality anchors, with cumulative FY1984–FY2024 implied excess infant deaths of roughly 97,000 to 323,000. The Sommers cell is zero only because the underlying paper studied adult mortality and cannot speak to the IMR channel. Across the five territories, applying Miller, Johnson, and Wherry's near-elderly adult-mortality estimate to modern no-cap gaps suggests an additional adult-mortality channel of roughly 420 deaths averted per year under the preferred near-elderly low-income proxy in FY2024, including roughly 379 in Puerto Rico. These are the orders of magnitude that cap-based Medicaid implies at the historical Puerto Rico cap's scale.

Third, the Puerto Rico mechanism diagnostics from the PRCS / ACS panel show that cap-bound Puerto Rico is structurally more disadvantaged than every donor state — 5.48 standard deviations above the donor-pool mean on a composite severity index, with Medicaid share, poverty rate, disability rate, and age-65+ share all far above the donor-pool medians. The cap-on-Medicaid regime is therefore being imposed on the U.S. citizen population with the highest standardized health and economic risk, and the per-capita federal contribution is statutorily restricted at the territorial cap. This combination — highest risk, lowest federal contribution — is the structural feature that the FY2027 debate will need to address regardless of how the technical cap-on-mortality elasticity is ultimately quantified.

7. Conclusion

Sixty years after the 1968 imposition of Section 1108(c) and fourteen years after the 2011 partial uncapping of territorial Medicaid, the historical evidence from the Puerto Rico Demographic Registry's *Informe Anual de Estadísticas Vitales* corpus does not support a clean causal estimate of the cap-on-infant-mortality elasticity. Across two independent synthetic-control designs, Puerto Rico's post-1968 infant-mortality trajectory is statistically indistinguishable from a Mississippi-anchored donor synthetic; formal R `Synth` reproduces that result, and formal R `synthdid` and regular DiD variants point negative rather than positive. The 2011 ACA Section 2005 partial uncapping did not produce a detectable positive effect on Puerto Rico's Medicaid coverage rate. The FY2020 funding cliff is observationally confounded by the COVID-19 PHE, the CAA 2020 / 2022 bridge supplements, and Hurricane Maria.

The reverse-counterfactual analysis provides the magnitudes that the historical designs cannot. A 1968-scaled per-capita cap applied to the fifty states would mechanically remove $513 billion in federal Medicaid spending in FY2024, bind

every state Medicaid program, and imply approximately 4,500 to 14,800 excess infant deaths per year under the two infant-mortality anchors. Cumulated over the FY1984–FY2024 state-panel window supported by the HCFA/CMS-64 data, the same transfer implies approximately 97,000 to 323,000 excess infant deaths. The Graham–Cassidy 2017 cap would bind twenty-five states and imply approximately 530 to 1,760 excess infant deaths per year in FY2024. A modern all-territory adult sensitivity using Miller, Johnson, and Wherry's near-elderly low-income estimate implies approximately 40,700 additional Medicaid-covered near-elderly adults, 14,000 newly insured adults, and 420 adult deaths averted per year across the five territories under a no-cap FY2024 counterfactual, with approximately 2,531 adult deaths averted cumulatively over FY2019–FY2024. The cap-on-Medicaid policy debate that will recur in the FY2027 appropriations cycle therefore turns less on whether the historical Puerto Rico experience identifies a mortality penalty (it does not, with the available data and designs) and more on whether the prospective state-level and modern all-territory adult-channel magnitudes implied by published Medicaid estimates are policy-relevant (they are).

The methodological contribution of the paper is the machine-extraction of the Puerto Rico Demographic Registry's Spanish-language *Informe Anual de Estadísticas Vitales* corpus, which produces a 559-cell validated panel across thirteen vital-statistics outcome series spanning 1950 through 2023. To our knowledge, this is the first econometric-scale panel extracted from this corpus. The corpus underlies every 1968 infant-mortality specification in this paper; without it, the historical Puerto Rico side of any cap-on-Medicaid analysis is not identifiable from any other publicly available data source. The two-tier extraction pipeline (PyMuPDF native text + Tesseract Spanish-language OCR) and the parser plus QA layer are open and reproducible.

The substantive contribution is to reframe the territorial-Medicaid-equity debate. The historical evidence from sixty years of Puerto Rico under the §1108(c) cap regime does not produce a clean causal estimate of cap-on-mortality, but it does produce two findings that should structure the FY2027 appropriations debate. First, the cap regime imposes an unambiguous federal-financing differential — Puerto Rico's FY2024 cap-bite against state-equivalent need is 58.8 percent under observed funding and would be 95 percent under base Section 1108 only — and the magnitude of that differential is documentable to the dollar from MACPAC and CRS data. Second, the population subjected to the cap regime is the U.S. citizen population with the highest standardized health and economic risk in any donor-comparable population: 5.48 standard deviations above the donor-pool mean on a composite structural severity index, with Medicaid coverage share, poverty rate, working-age disability rate, and age-65+ share all exceeding every donor state. These two facts — the magnitude of the federal-financing differential and the magnitude of the structural-risk differential of the cap-bound population — are sufficient to ground the equity case independent of whether the cap-on-IMR elasticity in Puerto Rico is identified at the standard quasi-experimental threshold.

Three lines of follow-up work would tighten the historical identification beyond what this paper achieves. A donor-side maternal mortality panel built from CDC WONDER UCD ICD-10 maternal codes would enable a synthetic-control replication of the within-PR 2011 ITS finding. A donor-side cause-specific mortality panel for diabetes, renal, respiratory, and septicemia would translate the cross-sectional cause-specific differential into a cap-era effect estimate. A post-PHE-unwinding replication of the funding-cliff analysis using FY2025–FY2026 data would identify the FY2020 cliff effect on Medicaid coverage cleanly. A municipality-level analysis of Puerto Rico mortality variation, exploiting cross-municipality cap-bite differences in payer mix and Medicaid-share concentration, would offer an additional within-Puerto-Rico identification strategy that the present paper does not pursue.

The 2027 expiration of the CAA 2022 territorial appropriations is the policy fulcrum for which this paper provides empirical input. The combination of (i) the historical Puerto Rico fiscal-architecture imposition documented across the infant-mortality, funding-cliff, and reverse-counterfactual analyses; (ii) the structural-risk profile of the cap-bound territorial population documented in the PRCS / ACS mechanism diagnostics; (iii) the directional but not-causally-identified maternal-mortality and disease-specific signals that respond to cap-relaxing policy interventions; and (iv) the prospective state-level fiscal, infant-mortality, and modern all-territory adult-mortality magnitudes documented in the reverse counterfactual should form the empirical basis for the FY2027 territorial-Medicaid-equity legislative debate. The argument for territorial-Medicaid-equity is strongest when made jointly on these four grounds, rather than on any single historical mortality-effect estimate.

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S1. Extended Data Section

S1.1 Treated and donor units

The five treated units are the U.S. territories: Puerto Rico (PR), the U.S. Virgin Islands (USVI), Guam (GU), American Samoa (AS), and the Commonwealth of the Northern Mariana Islands (CNMI). PR is the primary treated unit; the four smaller territories enter the analysis as secondary panels with substantially smaller cap-bite. American Samoa and CNMI entered Medicaid in 1982 and 1983 respectively and are excluded from the 1968 cap-imposition analysis.

The donor pool for the 1968 cap-imposition analysis comprises ten Deep South states: Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and West Virginia. For the post-2006 coverage and funding analyses the donor pool is expanded with four high-Hispanic

/ Southwestern states (Arizona, Florida, New Mexico, Texas) to better match Puerto Rico's demographic composition after the §936 industrialization era. The reverse counterfactual uses the full fifty-state-plus-DC panel as the platform on which to apply counterfactual cap regimes.

S1.2 Treatment construction

The 1968 cap is operationalized as a binary indicator for `year` $\geq$ `1968` $\times$ `is_treated_in_arm_1`. The 2011 ACA Section 2005 bump is operationalized as a binary indicator for `year` $\geq$ `2011` $\times$ `territory` $\in$ `{PR, USVI, GU, AS, CNMI}` and the cliff as `year` $\geq$ `2020` $\times$ `territory`. Cap-bite share for each (territory, year) is computed as one minus the ratio of (federal allotment cap + ACA Section 2005 drawdown + supplements) to a counterfactual open-ended federal Medicaid spend constructed from the donor-pool average federal Medicaid expenditure per capita times the territory's resident population. Cap-bite is therefore a derived quantity rather than a measured one; sensitivity to alternative donor-pool definitions is reported in Section S4.

S1.3 Outcome series

Mortality. The fifty-state mortality outcomes use CDC WONDER Compressed Mortality 1968–1998 and Underlying Cause of Death 1999–2024, supplemented by ICPSR Multiple Cause of Death public-use files (study 03905, 1968–1973; study 03906, 1974–1978) for individual-level ICD-8 detail. CDC WONDER carries no Puerto Rico state-level rows in any vintage. The ICPSR MCD public-use files capture deaths occurring in the fifty states + DC with the territorial codes (52 PR, 53 VI, 54 GU) used only for residents of those territories who died on the U.S. mainland. Territorial mortality outcomes therefore use territory-specific sources described below.

Puerto Rico mortality 1955–2023 is drawn from the *Informe Anual de Estadísticas Vitales* of the Puerto Rico Department of Health Demographic Registry (Registro Demográfico de Puerto Rico). The full annual run from 1958 through 2014 plus consolidated period volumes (Informe 2007–2008 Mortalidad, 2009–2014 Mortalidad, 2009–2010 + 2011–2014 + 2017–2020 + 2021–2023 Nacimientos) is obtained as official PDFs from the Departamento de Salud de Puerto Rico via `datos.estadisticas.pr` and the University of Puerto Rico Medical Sciences Campus Programa Graduado de Demografía digital archive. Fifty-nine PDFs total. Headline outcomes (live births, infant deaths, neonatal deaths, fetal deaths, maternal deaths, deaths-total, and the corresponding crude rates) are extracted directly from the Informe summary tables under the Spanish-language section headings ("Defunciones", "Nacimientos", "Mortalidad infantil", "Tasa de mortalidad por mil", "Mortalidad materna").

Puerto Rico individual-level vital records for 2000–2010 are obtained from `datos.estadisticas.pr` as bulk CSV downloads (the PR-specific NCHS submissions nds2000–nds2008 covering 262,183 death records and

nchs_births_* covering 965,779 live-birth records, plus nchs_mortality_2008–2010, nchs_fetal_deaths_2006, and the national-territory natality files natlterr2012–2014). These provide municipality-level mortality and birthweight / gestational-age detail not available from the Informe summary tables.

Territorial natality 1995–2024 for all five territories is obtained from the National Center for Health Statistics period public-use natality files (Nat19YYps and natality20YYps series). Pre-1995 territorial natality is sparser; we cross-validate the Informe-derived series against published NCHS *Natality Final* annual tables for Puerto Rico 1943–1995, the Virgin Islands 1940–1995, and Guam 1970–1995.

Donor-state pre-1968 birth and death series are reconstructed from two complementary sources: NCHS Multiple Cause of Death microdata 1959–1967 aggregated to (state of residence, year) for infant deaths under one year (nchs_mdf_donor_state_infant_deaths_1959_1967.parquet, 126 cells); and NCHS Vital Statistics of the United States Volume I Table 2-1 "Total births by place of residence" 1959–1967 transcribed cell-by-cell from the published volumes for the fourteen donor states (state_year_births_worksheet.csv, 126 cells). The derived donor IMR panel (donor_imr_state_year_1959_1967.parquet, 126 rows) passes five of five published-anchor spot-checks within ± 15 percent.

State-year live births 1995–2024 for the reverse-counterfactual birth denominator come from CDC WONDER Natality interactive TSV exports (1,530 cells covering 51 jurisdictions $\times$ 30 years). All four published-national-total validation anchors (1995, 2000, 2020, 2024) match published NCHS totals at 0.00 percent difference.

Coverage. Insurance coverage outcomes (uninsurance rate, Medicaid coverage rate, Medicare coverage rate, private coverage rate) are drawn from American Community Survey microdata for the fifty states and DC and from the Puerto Rico Community Survey for PR, 2005 through 2024 inclusive, via IPUMS USA. The PRCS does not cover the four smaller territories. Coverage outcomes are restricted to the post-2005 period and therefore inform the 2011 and FY2020 analyses only.

Hospital capacity. Drawn from the American Hospital Association Annual Survey, accessed through the University of Michigan's institutional license via the Institute for Healthcare Policy and Innovation. Territorial hospital coverage in the AHA Annual Survey is intermittent prior to 1970; consequently the hospital-capacity outcomes are restricted to the post-2006 analyses.

Macroeconomic covariates. PR macroeconomic covariates (per-capita income, payroll employment, out-migration to the mainland, real GNP index) come from the Federal Reserve Bank of New York's Puerto Rico Economic Activity Index, BEA territorial accounts, and Census ACS Migration Flows tables.

S1.4 Financing variables

Federal Medicaid allotment caps and statutory FMAPs for the five territories 2006–2024 come from the MACPAC *Medicaid and CHIP in the Territories* (February 2021) and MACStats *Medicaid and CHIP Data Book* (December 2023), cross-validated against CRS R47601 (Mitchell 2023). The validated allotment table carries source-status flags of `validated` (MACPAC Feb 2021 Table 2; 20 cells), `statute` (CRS R47601 citing P.L. 116-94 div N for PR FY2023/FY2024 annual caps; 2 cells), or `pending_source_pdf` (annual caps for which CMS Section 1108 certification PDFs were not retrievable from public sources; 8 cells).

State-level Medicaid spending 1968–2024 for the reverse-counterfactual baseline is assembled from MACPAC MACStats (post-2008), KFF state Medicaid spending tables (1991–present), the HCFA Statistical Supplement (1968–1991), and a public HCFA medical-assistance OCR bridge for FY1984–FY1997.

S2. PR Informe Corpus Extraction Methodology

S2.1 Two-tier extraction pipeline

The fifty-nine PR Informe PDFs are processed through a two-tier text-extraction pipeline:

Tier 1 (PyMuPDF native text extraction). Forty-nine of the fifty-nine PDFs have usable native text layers (2004+ Informes are native PDF; 1960–1989 are well-OCR'd scans with embedded text). PyMuPDF extracts the text in approximately fifty seconds total. Output: `data/raw/pr_informe/text/*.txt`.

Tier 2 (Tesseract OCR with Spanish language model). Ten scanned PDFs without usable native text (years 1958, 1959, 1990, 1993, 1996_Mortalidad, 1999, 2000, 2001, 2002, 2003) are converted by `tesseract -l spa` (Spanish language model). Approximately 3.9 million additional characters of usable text. Total OCR runtime approximately forty-five minutes.

S2.2 Parser pipeline

The regex parser targets thirteen outcome series across Spanish-language section headings and table-vetted cells: live_births, infant_deaths, neonatal_deaths, fetal_deaths, maternal_deaths, deaths_total, imr_per_1000, neonatal_rate_per_1000, fetal_rate_per_1000, crude_birth_rate, crude_death_rate, maternal_rate_per_1000, population. The parser includes a 2007/2008 IMR correction, a maternal-deaths-specific table-extraction path, post-2008 child/fetal/natality table extractions, and historical child/fetal table-vetted cells from the 1977 historical demographic summary, 1978/1979 annual infant/stillbirth tables, and a 1980 table override.

The post-parse quality-assurance layer at `data/scripts/13c_pr_informe_qa.py` enforces outcome-specific plausibility ranges, year-token exclusion, and internal-consistency checks (e.g., `imr == infant_deaths / live_births × 1000 ± 1.0` when all three are observed). Of 562 raw parser extractions, 559 cells (99 percent) pass the QA gates and are released in `pr_informe_validated_cells.parquet`. The residual 3 cells are quarantined with explicit failure flags.

S2.3 Cell coverage by outcome (validated set, 559 cells)

Outcome	n_cells	Year range	Analysis use
`live_births`	74	1950–2023	Denominator for derived MMR; data section descriptive
`imr_per_1000`	71	1950–2020	**Primary infant-mortality outcome** (near-complete annual series)
`fetal_rate_per_1000`	71	1950–2020	Descriptive figure + within-PR ITS at 2011
`fetal_deaths`	69	1950–2020	Data section descriptive
`infant_deaths`	69	1950–2020	Data section descriptive
`maternal_deaths`	61	1958–2020	**Supplemental MMR robustness**
`crude_birth_rate`	45	1963–2023	Descriptive figure
`population`	41	1950–2021	Internal denominator for rate computation
`maternal_rate_per_1000`	29	1980–2008	Cross-check vs derived MMR
`neonatal_rate_per_1000`	11	1976–1992	Low coverage; descriptive
`crude_death_rate`	9	1985–2006	Low coverage; descriptive
`neonatal_deaths`	7	1973–2020	Low coverage; descriptive
`deaths_total`	2	1958–1962	Not used

Every raw VSUS Vol I PDF feeding the donor births worksheet has a `.provenance.json` sidecar at `data/raw/_provenance/` recording sha256 checksum, acquisition date, source URL, and per-cell transcription log.

S3. Detailed Result Tables

S3.1 1968-anchor ATT summary

Spec	PR pre-period	n_pre	n_post	ATT post-mean	pre-RMSPE	post-RMSPE	RMSPE ratio	Top donor weight
A — Full validated	1959–1967	9	41	+0.078	5.579	1.832	0.328	MS = 1.00
B — Drop 1964 sensitivity	1959–1963, 1965–1967	8	41	+0.078	4.013	1.832	0.456	MS = 1.00

S3.2 1968-anchor in-space placebo distribution

Treated unit	ATT	|ATT|	pre-RMSPE	RMSPE ratio
PR (real)	**+0.08**	**1.47**	**5.58**	**0.33**
AL	+1.15	1.20	0.55	2.40
AR	+0.55	0.93	0.96	1.14
GA	+0.47	0.79	0.71	1.35
KY	−0.58	0.88	0.68	1.65
LA	+0.98	0.99	0.53	2.37
MS	+0.70	1.33	3.47	0.53
NC	+0.61	0.65	0.35	2.30
SC	+1.05	1.16	0.75	1.87
TN	+0.50	0.76	0.61	1.47
WV	+0.76	1.13	1.46	0.97
AZ	−1.49	1.49	1.99	0.90
FL	−0.29	0.55	0.95	0.71
NM	−1.58	1.62	1.25	1.43
TX	−0.86	0.99	1.07	1.07

Abadie-2010 exchangeable p-values: |ATT| = 0.200; RMSPE ratio = 1.000.

S3.3 Longer-post sensitivity

Window	ATT post-mean	Top donor
1968–1980	−0.41	MS = 1.00
1968–1993 (pre-Reforma)	+0.57	MS = 1.00
1968–2008 (pre-ACA)	+0.22	MS = 1.00
1968–2020 (full observed)	−0.09	MS = 1.00

S3.4 Baseline-headroom check

Unit	Mean IMR 1959–1967	IMR 1967	IMR 2008	Percent decline 1967–2008
PR	42.7	32.8	8.8	73.2%
MS	39.3	35.5	9.9	72.2%

Baseline-normalized specs:

Transform/window	ATT post-mean
Percent from pre-mean, 1968–2020	−0.043
Log IMR index, 1968–2020	−0.122

S3.5 Leave-one-Mississippi sensitivity

Donor pool	ATT post-mean	Direction
All 14 donors	+0.05	(Headline)
Drop Mississippi	−2.42	Donor-anchor dependent
Deep South only	−0.06	Robust
Southwestern only	−1.86	Donor-anchor dependent

S3.6 2011 partial-uncapping placebo distribution (Δ-from-2010 spec)

p-value (|ATT|) = 0.933; p-value (RMSPE ratio) = 0.467. PR's ATT ranks 14 of 15 in absolute magnitude; only New Mexico (the most heavily Hispanic ACA-expansion donor in the pool) has a placebo ATT exceeding PR's |ATT|.

S3.7 Elasticity-anchored infant-mortality grid (FY2024)

Cap regime	Sommers 2012 (zero, no IMR channel)	Currie–Gruber 1996 (moderate)	Goodman-Bacon 2018 (upper)
1968 §1108(c)-scaled cap	0	**4,451**	**14,838**
Graham–Cassidy 2017 cap	0	529	1,762
Generic FY2026-scale cap	0	117	389

Reference verification:

- Sommers BD, Baicker K, Epstein AM. *NEJM* 367(11):1025-1034 (DOI: 10.1056/NEJMsa1202099). Verified via https://api.crossref.org/works/10.1056/NEJMsa1202099.
- Currie J, Gruber J. *JPE* 104(6):1263-1296 (DOI: 10.1086/262059). Verified via https://api.crossref.org/works/10.1086/262059.
- Goodman-Bacon A. *JPE* 126(1):216-262 (DOI: 10.1086/695528). Verified via https://api.crossref.org/works/10.1086/695528.

S3.8 Annual and cumulative reverse-counterfactual infant deaths, FY1984-FY2024

The annual and cumulative reverse-counterfactual extension uses the HCFA medical-assistance bridge for FY1984-FY1996 and CMS-64 / Financial Man-

agement Report federal-share inputs for FY1997-FY2024. State-year live births use CDC WONDER Natality for 1995-2024 and a population × interpolated U.S. general-fertility-rate proxy for 1984-1994. FY1968-FY1983 are not estimated because the current repository lacks a comparable all-state federal-share panel for those years.

Cap regime	Anchor	FY2024 annual excess infant deaths	FY1984-FY2024 cumulative excess infant deaths
1968 §1108(c)-scaled cap	Currie–Gruber 1996	**4,451**	**97,032**
1968 §1108(c)-scaled cap	Goodman-Bacon 2018	**14,838**	**323,438**
Graham–Cassidy 2017 cap	Currie–Gruber 1996	529	2,077
Graham–Cassidy 2017 cap	Goodman-Bacon 2018	1,762	6,923
Generic FY2026-scale cap	Currie–Gruber 1996	117	359
Generic FY2026-scale cap	Goodman-Bacon 2018	389	1,197

Selected annual values for the 1968-scaled regime:

Fiscal year	Currie–Gruber 1996	Goodman-Bacon 2018
1984	0	0
1990	71	236
1995	735	2,450
2000	1,634	5,445
2010	3,891	12,970
2020	4,198	13,993
2024	4,451	14,838

S3.9 Method-family robustness checks

Formal R `Synth` reproduces the headline Python synthetic-control estimate:

Estimator	ATT, 1968–2008	Pre-RMSPE	Post-RMSPE	RMSPE ratio	Top weight
R `Synth`	+0.078	5.579	1.832	0.328	MS = 0.9999997

Formal R `synthdid` estimates are negative across level, log, normalized, and delta specifications:

Outcome transform	Post window	ATT	Placebo SE
IMR level	1968–1980	−4.16	1.72
IMR level	1968–1993	−5.13	2.07
IMR level	1968–2008	−6.18	2.52
log IMR	1968–2008	−0.120	0.070
percent from pre mean	1968–2008	−0.043	0.031
delta from 1967	1968–2008	−6.18	2.47

Conventional DiD and two-way-fixed-effects checks are not precision evidence with a single treated unit, but they do not reveal a positive cap-harm result. For the level IMR 1968–2008 window, the simple DiD ATT is −3.32 against Mississippi only, −10.06 against the all-donor mean, −10.14 against the Deep South mean, and −9.87 against the Southwestern/high-Hispanic donor mean. Two-way-fixed-effects estimates are also negative under all-donor, Deep South, and Mississippi-only controls, with and without unit trends. These checks reinforce the interpretation of the 1968 infant-mortality result as an aggregate IMR null rather than evidence that the cap improved mortality.

S3.9A Development-transition and donor-desegregation diagnostics

Two diagnostics probe the possibility that Arm 1 is masked by countervailing historical forces. The first asks whether Puerto Rico's rapid development and mortality-transition headroom could have offset cap harm. The second asks whether the Mississippi-anchored donor counterfactual improved partly because southern hospital desegregation lowered donor infant mortality after Medicare/Title VI enforcement.

Diagnostic	ATT, 1968–2008	ATT, 1968–1980	Placebo inference	Interpretation
Headline nonnegative SCM	+0.08	−0.41	Abadie $\lvert\text{ATT}\rvert$ $p = 0.20$	Frozen Arm 1 result
Pre-1968 mortality-transition ridge	+1.44	+0.85	upper-tail $p = 0.067$; $\lvert\text{ATT}\rvert$ $p = 0.133$	Weakly positive diagnostic; not decisive
1970 near-baseline development ridge	−6.06	−4.20	upper-tail $p = 1.000$; $\lvert\text{ATT}\rvert$ $p = 0.067$	Diagnostic only; PR is outside donor support
No-deseg donor path, TWFE anchor	−0.54	−1.02	not a placebo design	Adds back donor Title VI gains
No-deseg donor path, BJS anchor	−0.80	−1.28	not a placebo design	Adds back donor Title VI gains
No-deseg donor path, dCDH anchor	−1.00	−1.48	not a placebo design	Adds back donor Title VI gains

Diagnostic	ATT, 1968–2008	ATT, 1968–1980	Placebo inference	Interpretation
No-deseg donor path, CS anchor	−1.11	−1.59	not a placebo design	Adds back donor Title VI gains

The pre-1968 mortality-transition ridge uses only the 1959–1967 IMR level, slope, decline, and dispersion; it is the cleanest way to ask whether Puerto Rico's development-stage convergence masks harm without conditioning on post-treatment economic covariates. The 1970 development ridge adds near-baseline Census/IPUMS income, poverty, schooling, urbanization, and age-structure covariates, but Puerto Rico 1950/1960 IPUMS rows are absent from the staged extract, so the model is diagnostic only. Puerto Rico is outside the fourteen-donor support range on seven of nine 1970 covariates: lower mean income, higher poverty, lower high-school completion, lower BA completion, higher under-5 share, higher under-18 share, and lower age-65+ share.

The donor-desegregation diagnostic uses the Title VI hospital-desegregation paper's Black postneonatal mortality estimates and translates them to all-race donor IMR by multiplying each estimate by the donor state's Black birth share and the share of ACR counties certified by year. Because the Arm 1 synthetic places essentially all weight on Mississippi, the Mississippi translation is the relevant stress test; Mississippi's ACR-certified share is 0.902 in 1968 and 1.000 by 1980, with Black birth share about 0.48–0.50 in the Title VI county panel. Adding back the implied donor-side health gain raises the donor counterfactual and shifts Puerto Rico's ATT negative. The result is not a new causal estimate, but it shows that donor desegregation cannot explain a hidden positive cap-harm effect in Arm 1.

S3.10 All-territory adult coverage/mortality sensitivity

Miller, Johnson, and Wherry (2021) estimate a 0.132 percentage-point annual mortality decline among low-SES adults ages 55–64, with a 12.8 percentage-point administrative Medicaid-coverage first stage and a 4.4 percentage-point uninsurance decline. Because this is an adult near-elderly estimate, not an infant-mortality elasticity, it is reported separately from the infant-mortality grid.

FY2024 Puerto Rico no-cap funding capacity:

Quantity	FY2024 value
Current-law PR federal Medicaid funds	$3.325 B
State-like federal need counterfactual	$8.072 B
No-cap additional federal funds	$4.747 B
Cap-bite share	58.8%
Observed Medicaid-covered persons, PRCS	1.506 M

Quantity	FY2024 value
Observed uninsured persons, PRCS	184,660
Observed federal dollars per Medicaid-covered person	$2,207
No-cap coverage capacity at observed federal dollars per covered person	2.150 M person-equivalents

FY2024 adult coverage and mortality scenarios:

Scenario	Target population	Additional Medicaid-covered persons	Additional newly insured persons	Annual deaths averted	Four-year deaths averted
Near-elderly poverty<200% proxy	286,901	36,723	12,624	379	1,515
Near-elderly poverty<100% proxy	155,586	19,915	6,846	205	821
All near-elderly ages 55–64 proxy	415,731	53,214	18,292	549	2,195
Whole-population coverage-only calculation	3,203,295	410,022	140,945	not applied	not applied

The near-elderly population is proxied as one-half of the ACS/PRCS 45–64 age band because the Puerto Rico mechanism panel does not separately carry ages 55–64. The poverty<200% proxy is the preferred Puerto Rico analogue because Puerto Rico's poverty distribution and adult Medicaid eligibility context differ sharply from mainland expansion states. The whole-population line is a coverage-capacity calculation only; the Miller, Johnson, and Wherry mortality estimate is not applied outside the near-elderly target population.

Modern all-territory FY2024 preferred proxy:

Territory	Current federal funds	State-like need	No-cap gap	Additional Medicaid-covered adults	Newly insured adults	Annual deaths averted
Puerto Rico	$4.038 B	$8.072 B	$4.033 B	36,720	12,622	379
Guam	$166.0 M	$433.9 M	$267.9 M	1,974	679	20
U.S. Virgin Islands	$120.0 M	$210.2 M	$90.2 M	956	329	10
American Samoa	$50.4 M	$125.5 M	$75.1 M	571	196	6
CNMI	$81.4 M	$115.4 M	$34.0 M	525	180	5
All five territories	**$4.456 B**	**$8.957 B**	**$4.500 B**	**40,746**	**14,006**	**420**

Modern all-territory cumulative proxy, FY2019-FY2024:

Scenario	FY2024 Medicaid-covered adults	FY2024 newly insured adults	FY2024 annual deaths averted	FY2019-FY2024 cumulative deaths averted
Near-elderly poverty<200% proxy	40,746	14,006	420	2,531
Near-elderly poverty<100% proxy	22,097	7,596	228	1,373
All near-elderly ages 55–64 proxy	59,043	20,296	609	3,668

Puerto Rico uses the PRCS/ACS annual mechanism panel. The four smaller territories use Puerto Rico's latest near-elderly age-by-poverty and coverage shares applied to each territory's population because comparable territory-specific mechanism panels are not in the repository. The requested 1968-present all-territory adult backcast is therefore not estimated.

S4. Robustness Specifications

S4.1 Donor-pool variants

For the 1968-anchor design, we report leave-one-donor-out sensitivity (Section S3.5). Drop-Mississippi shifts the headline ATT from +0.05/+0.08 to −2.42, demonstrating that the synthetic is Mississippi-anchored but the qualitative no-harm conclusion is robust. Deep-South-only and Southwestern-only sub-pools produce ATTs in the −0.1 to −1.9 range; none crosses into the positive-ATT (cap-harm) region.

S4.2 Pre-period truncations

For the 1968-anchor design, we report drop-1964 as the primary sensitivity (1964 is a real Informe Vol I value but a visible high point at 51.7 IMR per 1,000). Drop-1964 produces an identical post-period ATT to the full validated specification because Mississippi receives weight 1.00 in both.

S4.3 Post-period truncations

For the 1968-anchor design, we report four post-period truncations (1980, 1993, 2008, 2020; Section S3.3). All four ATTs are within ±0.6 IMR per 1,000 of zero; none identifies a delayed cap-effect signal.

S4.4 Outcome scale

For the 1968-anchor design, we report the percent-from-pre-mean specification and the log-IMR-index specification (Section S3.4). Both produce slightly nega-

tive ATTs, consistent with PR's worse baseline and faster post-1968 convergence rather than with a hidden adverse effect.

S4.5 Inference

All placebo p-values use the Abadie-2010 exchangeable formula: p = (1 + #placebos with statistic ≥ treated statistic) / (1 + n_placebos). The 2011 partial-uncapping placebo p-value is 0.933 for |ATT| and 0.467 for the RMSPE ratio.

S5. Tuberculosis Descriptive Appendix

The Puerto Rico Informe corpus supports a table-vetted tuberculosis-mortality series 1958–1977 from the 1977 historical selected-causes table (`tables/pr_informe_tuberculosis_1958_1977.csv`). TB mortality fell from 29.8 per 100,000 in 1958 to 6.0 per 100,000 in 1977 — a 80 percent decline that is consistent with the standard TB epidemiological transition of the period. A 1968 cap ITS on the TB series shows no significant level break (+0.88 per 100,000, $p = 0.50$) but a significant flattening of the decline rate (+0.87 per year, $p < 0.001$). We treat this as a secular disease-transition pattern rather than as cap-effect evidence; TB stays appendix/descriptive only and should not be cited in the manuscript as a Section 1108(c) outcome.

S6. Maternal Mortality Supplement

S6.1 Source construction

The maternal mortality ratio (MMR) per 100,000 live births is computed as `maternal_deaths / live_births × 100,000` from PR Informe validated cells. The maternal_deaths series (61 cells, 1958–2020) is table-vetted using the PR Informe 1974 historical demographic table, annual maternal tables 1975–1979, the 2007–2008 summary table, Tables 4.115–4.117 for 2009–2014, and Table 80 for 2017–2020. The live_births series (74 cells, 1950–2023) comes from the same validated panel.

S6.2 ITS results

Spec	Pre obs	Post obs	Pre-slope (per yr)	Level shift at event	p-value	Slope shift	R^2
1968 §1108(c) cap (real)	10	41	−4.66 (p<0.001)	**−13.83 per 100K**	**0.027**	+4.41 (p<0.001)	0.85
1962 in-time placebo	4	6	−12.09 (p=0.029)	+29.31 (opposite direction)	0.073	+5.50 (p=0.296)	0.80

Spec	Pre obs	Post obs	Pre-slope (per yr)	Level shift at event	p-value	Slope shift	R^2
2011 ACA §2005 bump (real)	8	7	+0.75 (p=0.508)	**−15.62 per 100K**	**0.052**	+3.17 (p=0.051)	0.63

S6.3 The 1968 maternal-deaths cell

The 1968 level shift of −13.83 per 100,000 ($p = 0.027$) is statistically detectable but hinges on a single low PR maternal-deaths cell. The value is not a generic parser artifact: the extraction uses the PR Informe 1974 historical demographic table, which reports 1968 maternal deaths as 12. The 1968 annual volume's provisional maternal table reports 10; the later historical table revises/standardizes the cell to 12. The analysis keeps the retrospective 12 because it is the table-vetted historical-series value.

Year	Maternal deaths	Live births	MMR per 100K
1965	40	79,586	50.3
1966	34	75,735	44.9
1967	26	70,735	36.8
1968	**12**	**67,989**	**17.6**
1969	24	67,577	35.5
1970	18	67,438	26.7

The 1967 → 1968 drop from 26 to 12 maternal deaths in one year is steep for a rare public-health outcome with evolving registration practices. Two competing explanations cannot be distinguished from the corpus alone: (1) a real coverage effect (Medicaid was implemented in PR in 1968 alongside the §1108(c) cap), or (2) a reporting / case-definition change at Medicaid implementation. The 1962 placebo level shift in the opposite direction (+29.31 per 100K, $p = 0.073$) means the 1968 result is not pure pre-trend extrapolation, but the placebo's wide CI and short post-window (6 obs) make it a weak falsification. The 1968 maternal finding is reported as descriptive only, not as a causal estimate.

S6.4 The 2011 ACA §2005 bump finding

The 2011 finding is the cleaner of the two ITS results. Pre-period 2003–2010 PR MMR was approximately 5–20 per 100K with no significant pre-trend ($\beta = +0.75$, $p = 0.51$). At 2011 the level dropped approximately 15.6 per 100K ($p = 0.052$), then slowly rose back through the 2017+ Maria/cliff era. The result is policy-coherent (PR Medicaid drew from the $5.4B ACA §2005 fund 2011–2019, expanding access for low-income pregnant women) and the magnitude is of the order published Medicaid-pregnancy-expansion estimates imply. The borderline p-value reflects the small-N rare-event outcome rather than weak signal.

The finding does not establish causal identification: there is no donor counterfactual for maternal mortality in this paper, ACA Section 2005 was not randomly assigned, and PR's coverage environment also evolved through pre-Maria social programs in the same window. Treat as descriptive corroboration of the policy mechanism rather than a clean ATT.

S7. PRCS 2020 Handling

The U.S. Census Bureau did not release a Puerto Rico Community Survey one-year file for 2020 because the COVID-19 PHE disrupted PRCS field operations. Strict annual analyses in this paper (2011 coverage, FY2020 coverage, and all PRCS / ACS mechanism work) exclude 2020 and use 2005–2019 + 2021–2024 annual data only. Sensitivity checks for outcomes that span 2019–2021 use either linear interpolation between 2019 and 2021 cells or the PRCS 2016–2020 five-year release.

S8. Maria Migration Diagnostics

The Hurricane Maria emigration shock substantially altered Puerto Rico's resident population and the construction of any per-capita outcome measured during the post-Maria window. From 2017 to 2018, mainland ACS records of residents reporting Puerto Rico residence one year earlier peaked at 130,813 (the standard "MIGPLAC1=110" diagnostic in the IPUMS USA harmonization). The 2024 mainland Puerto Rico–born population (BPL=110) is approximately 1.85 million, meaning that the mainland Puerto Rico-born population is now larger than Puerto Rico's resident population (approximately 3.2 million in 2024). These migration shocks are not adjusted for in the headline analyses but are surfaced in the manuscript Discussion as denominator and spillover diagnostics.

S9. No-Cap-Increase Counterfactuals

Cumulative shortfalls for Puerto Rico FY2011–FY2024 against observed federal Medicaid funds available ($28.0 billion total) under four no-cap-increase scenarios:

Scenario	PR cumulative shortfall, FY2011–FY2024
Base Section 1108 only (no ACA §2005, no post-2017 supplements)	$22.95 B
No ACA §2005 drawdown (but with post-2017 supplements)	$6.70 B
No post-2017 disaster / CAA / FFCRA supplements (with ACA §2005)	$15.65 B
No FFCRA / CAA 2022 supplements (with ACA §2005 + disaster)	$6.02 B

FY2024 PR illustrative contrast:

Scenario	Funds available	Shortfall vs observed	Cap bite vs state-like need
Observed (CAA 2022 statute caps)	$3.325 B	$0	58.8%
Base Section 1108 only	$0.412 B	$2.913 B	94.9%

Even after every cap-relaxing supplement of the past fifteen years, the cap remains binding; without those supplements, Puerto Rico's federal Medicaid contribution would have been at near-total-suppression levels relative to state-like federal need.

S10. Pre-Specified Estimands and Deviations

The pre-specified estimands for each empirical component are recorded in the project repository. Any post-hoc deviation is recorded with a date and reason. Notable deviations:

- 2026-05-15 — The 1968 pre-period was expanded from a smaller validated-only / raw-fallback set to the full validated 1959–1967 donor-overlap window after every donor-overlap pre-period year became table-vetted. ATT shifted $+0.29 \rightarrow +0.08$; placebo p-value shifted $0.13 \rightarrow 0.20$ for $|\text{ATT}|$.
- 2026-05-15 — Supplemental maternal mortality was added after the PR Informe maternal_deaths series passed the quality checks.
- 2026-05-15 — PR Informe parser refinements added post-2008 child/fetal/natality and historical child/fetal table-vetted cells.
- 2026-05-15 — PR FY2023/FY2024 caps were promoted to statute-anchored cells from CRS R47601.
- 2026-05-16 — Formal R `Synth`, formal R `synthdid`, DiD/TWFE, and adult Miller-Johnson-Wherry sensitivity analyses were added as robustness checks after the headline specifications were already fixed.

S11. Reproducibility

All cleaning is scripted in Python (pandas, pyarrow). The master runner `data/scripts/00_run_all.py` executes the cleaning + ingestion + QA scripts in order, logs row counts to `data/scripts/logs/`, and writes the run manifest at `data/clean/run_manifest.csv` with hard row-count assertions on nineteen critical parquets. Raw downloads are written read-only to `data/raw/<source-slug>/` with provenance sidecars at `data/raw/_provenance/<filename>.provenance.json`.

The analysis scripts and findings memos are organized under `analysis/` by empirical component. Reproducibility checks include row-count assertions, claims-

to-evidence mapping, figure/table source checks, and manuscript-to-output consistency review.

S12. Author and Funding Disclosures

School of Public Health, Department of Health Management & Policy. https://orcid.org/0000-0001-5003-2631. **Working-paper repository**: https://github.com/jonpalisoc1024/papers.

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Data and code availability: All cleaning and analysis scripts, the validated PR Informe parquet, the donor IMR panel, the WONDER Natality state-year panel, the MACPAC backfilled allotment table, and the analytic panels are available in the project repository. Raw PR Informe PDFs are public-domain government publications available from `datos.estadisticas.pr` and the University of Puerto Rico Medical Sciences Campus Programa Graduado de Demografía digital archive. The two-tier extraction pipeline (PyMuPDF + Tesseract Spanish-language OCR) and quality-assurance layer reproduce the validated cell panel from raw PDFs in approximately one hour of compute time.