Psychiatric Medication Use at the Medicaid Eligibility Threshold in States with Separate CHIP

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Abstract

In states operating a separate Children's Health Insurance Program (CHIP), an income threshold separates Medicaid eligibility from CHIP eligibility. Public eligibility continues at that boundary, but coverage terms can change. I estimate local-linear regression discontinuities in past-12-month psychiatric-medication use using the 2016–2019, 2021–2022, and 2024 National Survey of Children's Health. The sample includes all children ages 6–17 in states without a pre-pandemic 12-month Medicaid coverage guarantee. The estimated share using psychiatric medication is 9.7 percentage points higher just above than below the threshold before the pandemic, 16.9 points higher during the federal Medicaid enrollment freeze, and 7.5 points higher in 2024. After adjustment for five outcomes, only the pre-pandemic difference is statistically significant at 5 percent. Before and during the freeze, estimates are larger among children with a current mental, emotional, developmental, or behavioral diagnosis and near zero among those without one. Uninsurance does not change detectably in either period. Program enrollment is unobserved, so the discontinuities are not effects of moving from Medicaid to separate CHIP. Cross-period differences do not identify effects of the enrollment freeze or the 2024 continuous-eligibility mandate.

Keywords Medicaid, Children's Health Insurance Program, continuous eligibility, regression discontinuity, psychiatric medication, insurance eligibility

JEL classification I13, I18, I11, H51

1. Introduction

About one in eight U.S. children has a diagnosed mental or behavioral condition (Currie, 2025). For low-income children with such a condition, a state's income threshold can determine which public program they qualify for. Below it, a child qualifies for Medicaid. Above it, in states that run a separate Children's Health Insurance Program (CHIP), the child qualifies for that program instead. Both programs cover behavioral-health services, and a child whose family earns its way across the threshold remains eligible for public coverage. On paper, public eligibility continues at the threshold. That continuity can make changes in coverage terms easy to overlook.

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At the threshold, the terms of coverage can change even when public eligibility does not. Medicaid carries the federal Early and Periodic Screening, Diagnostic, and Treatment entitlement. Title XXI does not require a separate CHIP program to provide that entitlement. Its state plan defines the benefit package. The programs can differ in how they pay providers and which clinicians are in network. Cost sharing and prior authorization can differ too. None of that appears in whether a child is insured. All of it could matter for whether a child keeps seeing a therapist or leaves an appointment with a prescription. So the question is whether treatment shifts at the threshold. This paper takes up one part of that shift, psychiatric-medication use.

I estimate reduced-form local-linear regression discontinuities (RDs) at the threshold using the National Survey of Children's Health (NSCH) (U.S. Census Bureau, 2025b). The running variable subtracts the survey-year school-age Medicaid limit published by the Kaiser Family Foundation (KFF) from prior-calendar-year reported family income (Kaiser Family Foundation, 2016–2024). Both are measured as percentages of the federal poverty level. Zero marks the point at which reported income equals that limit. The design compares children whose reported income falls just above and below the state-year Medicaid eligibility threshold. The identifying assumption is that expected outcomes would otherwise vary smoothly with income through the threshold. But the NSCH does not report actual program enrollment. The estimand is therefore an outcome difference at the income-policy boundary, not the effect of enrolling in separate CHIP instead of Medicaid.

Three blocks of years carry three different enrollment regimes. The years 2016–2019 come before the pandemic, 2021–2022 fall under the federal Medicaid enrollment freeze, and 2024 is the first year of the federal continuous-eligibility mandate. These enrollment protections can change whether an income crossing leads to a transition between programs. Each period gets its own estimate. The periods also differ in pandemic conditions and unwinding activity. Differences across periods are therefore descriptive and do not identify the effects of the freeze or the mandate. The primary sample includes all children ages 6–17 in states where children's Medicaid did not guarantee 12 months of coverage before the pandemic. Those are the states whose Medicaid enrollment protection changes across all three periods. Analyses among children with and without a current mental, emotional, developmental, or behavioral (MEDB) diagnosis assess where the medication discontinuity is concentrated.

Estimated medication use rises at the threshold. Among all children, estimated medication use is 9.7 percentage points higher just above the threshold before the pandemic and 16.9 points higher during the freeze. The 2024 estimate is 7.5 points and is imprecise. In a secondary analysis of children with a current MEDB diagnosis, the corresponding estimates are 32.8, 43.8, and 12.6 points. Estimates among children without a current diagnosis are near zero before and during the freeze. Attention-deficit/hyperactivity disorder (ADHD) prevalence rises at the threshold before the pandemic and during the freeze, and diagnosed children carry more conditions during the freeze. The first two estimates for diagnosed children may therefore partly reflect who enters the subgroup rather than how they are treated. Of the three all-child medication estimates, only the pre-pandemic estimate remains statistically distinguishable from zero after adjusting for

multiple testing across psychiatric medication, counseling, Medicaid/CHIP coverage, private-only coverage, and uninsured status. Uninsurance does not change detectably before or during the freeze.

Eligibility discontinuities have been used before to study children's public coverage, and what sets this paper apart is where the threshold sits. Work in this journal has estimated coverage and utilization responses at Medicaid thresholds (De La Mata, 2012) and at coverage cliffs (Koh and Park, 2025), and related work follows Medicaid expansions into mental-health treatment and prescribing (Ortega, 2023). Those designs study margins at which coverage itself changes. The margin here sits inside statutory public eligibility, where eligibility continues even though coverage terms can differ. Before the pandemic and during the freeze, I estimate positive medication-use discontinuities while uninsurance does not change detectably. And I follow the same threshold across three enrollment regimes, a comparison made possible by two federal rule changes within eight years. Closest to this paper's data, Eliason et al. (2025c) use the same survey to compare states by their pre-pandemic continuous-eligibility status. That comparison runs across states, and this one runs across an income threshold inside them.

This paper also contributes to work on treatment composition and switching costs. Mental-health treatment differs between children with Medicaid and private coverage (Ali et al., 2019). Among adults treated for depression in employer claims, antidepressant use was higher under preferred provider organization (PPO) plans and mental-health carve-outs and with higher psychotherapy copayments (Berndt et al., 1997). Changing health plans or doctors can also impose switching costs. Evidence on those margins comes from Politzer (2025) and Dahl and Forbes (2023). I bring these questions to a public-eligibility threshold. Before the pandemic, psychiatric-medication use rises at that threshold while counseling use shows no detectable change.

One possible explanation is that medication and therapy differ in how easily they continue when coverage terms change. A prescription can travel well. A new clinician may renew it, and the same pharmacy may fill it after a change in plan. A relationship with a therapist may travel less easily because it depends on that clinician remaining available and in network. If crossing the threshold makes therapy harder to continue than medication, medication use should be higher just above it. Estimated medication discontinuities are more consistently positive for children with anxiety or depression than for children with ADHD but without anxiety or depression, although the analysis does not formally compare the groups. The survey does not track provider continuity, and its counseling measure cannot separate psychotherapy from medication-management visits. The pattern is consistent with this explanation, but the data cannot test it directly.

2. Institutional Setting

The analysis centers on the state-year income threshold where children's Medicaid eligibility ends and separate CHIP eligibility begins. Three statutory regimes govern enrollment protection during the analysis years. In 2016–2019, states chose whether to provide 12-month continuous eligibility (CE) in children's Medicaid. A federal condition then froze Medicaid disenrollment during 2021–2022, the coronavirus disease 2019 (COVID) period. In 2024, a federal 12-month CE mandate took effect nationwide. I omit 2020 and

2023 because the rules changed during both years, leaving neither year inside one regime. Table 1 compares statutory protection rules in Medicaid and separate CHIP across these periods.

Pre-period (2016–2019). In children's Medicaid, 12-month CE was a long-standing state option rather than a federal requirement. Once eligibility was determined, CE protected enrollment for 12 months even if family income changed. Twenty-four states guaranteed those 12 months to children ages 6–17. The same 24 states appear in every KFF survey from January 2016 through 2019, so a state's pre-pandemic status does not depend on which of those years defines it. Two states show why the age range belongs in the definition. Florida guaranteed 12 months only to children under 5, giving 6 months to those 5 and older, and Indiana only to children under 3 (Kaiser Family Foundation, 2016–2024). The classification therefore records a 12-month guarantee for school-age children, not the absence of any shorter or narrower protection.

COVID period (2021–2022). A federal condition replaced state choice during the pandemic. The Families First Coronavirus Response Act (FFCRA) made enhanced federal matching funds conditional on keeping Medicaid enrollees covered. Two features made that protection broader than any state CE policy. It reached every enrolled child, not only children in states that had elected CE, and it was tied to the public health emergency rather than to a fixed 12 months. The federal condition applied to Medicaid but not separate CHIP. FFCRA's condition lapsed on March 31, 2023, when the Medicaid "unwinding" began.

2024 period. The Consolidated Appropriations Act, 2023 (CAA 2023, § 5112) established nationwide 12-month CE for children in Medicaid and CHIP on January 1, 2024. In separate CHIP, that protection applies only while the child remains CHIP-eligible. In the mandate's first year, enrollment gains were largest in states that had no prior guarantee (Eliason et al., 2025a). The transition out of the freeze was not uniform in 2023. States unwound the pandemic freeze on their own schedules, and children in the states that lost the most Medicaid enrollment became more likely to pay cash for chronic medications (Chua et al., 2025). Losses were smaller where a state already guaranteed 12 months (Eliason et al., 2025b). Within 2024, what varies is only how long the federal guarantee has been in force.

Table 1. Statutory Enrollment-Protection Rules by Period and Program

Period	Children's Medicaid	Separate CHIP
Pre-period (2016–2019)	State option; no 12-month CE for ages 6–17 in the analysis states	No federal 12-month CE requirement
COVID period (2021–2022)	FFCRA funding condition protected enrolled children from disenrollment	No parallel federal FFCRA condition
2024 period	Federal 12-month CE	Federal 12-month CE while the child remains CHIP-eligible

Notes. CE denotes continuous eligibility, CHIP denotes Children's Health Insurance Program, COVID denotes coronavirus disease 2019, FFCRA denotes the Families First Coronavirus Response Act, and NSCH denotes National Survey of Children's Health. The table states program rules; the NSCH does not observe program enrollment. Medicaid and separate CHIP are mutually exclusive, so separate-CHIP CE cannot retain a child found eligible for Medicaid. The analysis states lacked 12-month Medicaid CE for ages 6–17 under their January 2016 rules; states with prior CE provide descriptive context only. Each period is estimated separately; cross-period differences do not identify an effect of CE.

The analysis focuses on states without a 12-month guarantee under their January 2016 rules because their enrollment protection changes across all three periods. I call them "historically non-CE" states. States with a prior guarantee serve as descriptive context only. Adoption was a deliberate policy choice, so differences between the two state groups can reflect both CE and the reasons states adopted it.

The Medicaid eligibility threshold is statutory. In a separate-CHIP state, children's Medicaid eligibility extends up to a state-year income limit, and separate CHIP eligibility begins above it. Federal law makes the two programs mutually exclusive. A separate CHIP may cover only a "targeted low-income child," a category that excludes any child found eligible for Medicaid (United States Code, 2024), and states must screen every CHIP applicant for Medicaid first (U.S. Code of Federal Regulations, 2026). The Medicaid eligibility threshold is therefore both Medicaid's upper bound and separate CHIP's lower bound. I keep state-years in which the separate program's ceiling sits at least 50 federal-poverty-level (FPL) percentage points above that threshold, so the income window contains one institutional boundary.

Medicaid and separate CHIP both cover behavioral-health services, but benefit rules, cost sharing, provider networks, and prior authorization can differ. Direct comparisons of prior-authorization requirements are limited (Supplementary Section S10.5). At each threshold included in the analysis, income-based public eligibility exists on both sides, with Medicaid below the threshold and separate CHIP above it. The insurance outcomes test whether reported coverage changes at the threshold, but they cannot distinguish Medicaid enrollment from separate CHIP enrollment.

In the pre-period, crossing the Medicaid eligibility threshold could change coverage terms, and the enrollment rules differed for income increases and decreases. At redetermination, eligibility rules directed a child above the threshold toward separate CHIP because Medicaid offered no 12-month guarantee in historically non-CE states. Separate CHIP could use a different provider network. A child who continued therapy might then need a new intake or prior authorization, while a pediatrician could continue the prescription. After family income fell below the threshold, the child became eligible for Medicaid. Federal law makes a Medicaid-eligible child ineligible for separate CHIP, so even a separate CHIP continuous-eligibility guarantee could not keep the child enrolled in that program. If the child returned to Medicaid, the federal Early and Periodic Screening, Diagnostic, and Treatment entitlement applied, but the provider network could change once more.

Predictions. H1 predicts a positive pre-period medication discontinuity among all children that is concentrated among children with a current MEDB diagnosis. It also predicts no corresponding discontinuity among children without a current diagnosis. H1 is stated for the pre-period because later federal enrollment protections change how an income crossing could translate into a program transition. H2 predicts a more consistently positive medication discontinuity for anxiety or depression than for ADHD without anxiety or depression. Because the subgroups share state clusters and I do not estimate the covariance between their estimates, I assess H2 as a sign pattern rather than a formal between-group contrast. H3 predicts no detectable discontinuity in uninsured status in any period. Cross-period comparisons remain descriptive

because the pandemic, unwinding, and federal rule changes separate the periods. Supplementary Section S3.2 maps each prediction to its empirical test.

3. Data

The analysis uses data from the National Survey of Children's Health (NSCH) for 2016–2019, 2021–2022, and 2024 (U.S. Census Bureau, 2025b). In each annual cross-section, a parent or caregiver reports information for one randomly selected child (U.S. Census Bureau, 2023). I omit 2020 and 2023 because their 12-month outcomes can span major policy changes. The primary population includes all children ages 6–17 in historically non-CE states. Using all children avoids defining the primary sample by a current diagnosis that could itself change at the threshold.

The *strict sample* retains only state-years in which the state operates a separate CHIP program and its upper limit lies at least 50 FPL points above the Medicaid eligibility threshold. The 50-FPL gap keeps the analysis window from crossing a second institutional boundary. A transitioning state enters only in years when both requirements hold. Supplementary Section S11.1 documents which state-years enter under this rule. A sensitivity analysis adds Medicaid-expansion CHIP state-years, where the Medicaid eligibility threshold marks a different institutional margin (Supplementary Section S6.1).

The primary analysis drops records flagged by the Census Bureau as having an imputed family poverty ratio (`fpl_if == 1`) (U.S. Census Bureau, 2023). Dropping flagged records avoids using imputed poverty ratios to locate children relative to the Medicaid eligibility threshold. A sensitivity analysis retains flagged records, reconstructs the running variable with each of the six public-use FPL implicates, and combines estimates and robust variances with Rubin's rules (Supplementary Section S6.5).

NSCH variables do not share one reference date. In survey wave t, family income refers to calendar year $t-1$, psychiatric-medication use and mental-health treatment or counseling cover the 12 months before questionnaire completion, and insurance is current at completion. The primary running variable subtracts the KFF school-age Medicaid limit for survey year t from income reported for year $t-1$. Both are expressed as percentages of FPL. NSCH income does not measure statutory modified adjusted gross income (MAGI), and KFF thresholds may differ from operational program rules (Kaiser Family Foundation, 2016–2024). The running variable therefore approximates the eligibility margin. A sensitivity analysis uses the KFF limit for the income-reference year, including the 2015 limit for the 2016 survey wave.

Survey timing also complicates comparisons across policy periods. For the omitted 2020 wave, the 12-month outcomes can include months before and after the FFCRA enrollment condition took effect. The 2023 outcome windows can overlap the Medicaid unwinding, whose timing differed across states. The retained waves also differ in policy exposure. The 2021 and 2022 waves were fielded during the federal Medicaid enrollment freeze. The 2024 wave was fielded from June 2024 through February 2025 (U.S. Census Bureau, 2025a). Depending on response timing, its 12-month outcomes can include months before and after the federal continuous-eligibility mandate took effect and can overlap state unwinding activities.

The primary medication outcome comes from NSCH item K4Q23. It records whether the child used medication during the past 12 months because of difficulties with emotions, concentration, or behavior. Missing K4Q23 responses are omitted from the primary medication analysis. A measurement sensitivity expands the outcome to include current ADHD medication and current autism medication. That measure mixes a 12-month item with two current-use items. Each current-use item is skipped for children without the corresponding reported diagnosis, so the information available for the expanded measure differs across children.

Four companion outcomes join psychiatric medication in the primary all-child outcome family. Mental-health treatment or counseling is the behavioral-health companion. Medicaid/CHIP coverage and private-only coverage serve as coverage diagnostics. Uninsured status tests H3's prediction of no detectable discontinuity. Because I test five outcomes, I adjust their p-values within each period to limit false-positive findings. Supplementary Section S4 reports the question text and coding for every outcome, including the additional adequacy, access, cost, and general-utilization measures.

All children ages 6–17 form the primary population. Within that population, secondary analyses examine psychiatric-medication use and mental-health treatment or counseling among children with at least one current MEDB diagnosis. The MEDB group includes current ADHD, anxiety, depression, autism spectrum disorder, behavior or conduct problems, developmental delay, speech disorder, or Tourette syndrome. A separate specificity analysis estimates psychiatric-medication use among children without any current MEDB diagnosis. The two diagnosis-group medication analyses show where the all-child discontinuity is concentrated, but conditioning on diagnosis can change sample composition at the threshold.

The focal outcomes do not observe treatment substitution or movement between public programs. The medication item records neither the prescriber nor whether medication replaced psychotherapy. The counseling item combines psychotherapy with medication-management visits and lacks session counts, provider identity, or continuity. The public-coverage item combines Medicaid, Medicaid-expansion CHIP, and separate CHIP. It cannot reveal enrollment or switching between those programs. Private-only coverage and uninsured status identify broad coverage categories, not movement into or out of a specific public program. The data therefore provide no program-enrollment first stage and cannot identify substitution between psychotherapy and medication.

4. Empirical Strategy

Estimand. For each outcome, I estimate a reduced-form discontinuity at the state-year Medicaid eligibility threshold. The running variable subtracts the survey-year KFF school-age Medicaid limit from prior-calendar-year family income. Federal law makes Medicaid eligibility and separate CHIP eligibility mutually exclusive. The NSCH does not observe an administrative eligibility determination or distinguish Medicaid enrollment from separate CHIP enrollment. The estimand is therefore an outcome discontinuity at the reported-income policy boundary, not an enrollment effect comparing Medicaid with separate CHIP.

4.1. Estimation

I estimate each discontinuity with `rdrobust` using a local linear specification, a triangular kernel, survey weights, and robust bias-corrected inference (Calonico et al., 2014, 2020). I cluster standard errors by running-variable support point.[1] For each period-by-sample cell, I estimate

$$Y_{ist} = \alpha + \tau \cdot \mathbb{1}[X_{ist} \geq 0] + \beta_1 X_{ist} + \beta_2 X_{ist} \cdot \mathbb{1}[X_{ist} \geq 0] + Z'_{ist}\gamma + \mu_s + \nu_t + \varepsilon_{ist}. \tag{1}$$

Here, i, s, and t index children, states, and survey years, and Y_{ist} is the outcome. I define X_{ist} as family income minus the Medicaid eligibility threshold, with both expressed as percentages of FPL. The threshold indicator equals one when $X_{ist} \geq 0$, meaning family income is at or above the threshold. Covariates in Z_{ist} are race, sex, household language, highest education among reported adults, number of children in the household, and the responding caregiver's self-rated mental or emotional health. The terms μ_s and ν_t are state and survey-year fixed effects. The coefficient τ is the outcome discontinuity at the boundary.

The estimation bandwidth h sets the local linear window, while b sets the window used to estimate and correct its bias. The primary specification sets $h = b = 50$ FPL points so that neither window crosses the separate-CHIP upper limit in any retained state-year. A sensitivity keeps $h = 50$ but increases b to 70. That wider window crosses the upper limit in 47 of 111 historically non-CE state-years. I therefore do not use it as the primary model. Supplementary Section S11.3 reports the gap distribution.

The seven-specification grid includes an $h = b = 50$ model without demographic or state covariates and two $h = 30, b = 50$ models. One donut model removes observations strictly within ± 3 FPL while retaining $h = b = 50$. All multi-year models account for survey year. One replaces additive state and survey-year fixed effects with state-by-year fixed effects. For each estimate, N_h is the unweighted complete-case sample within the effect bandwidth. The state-cluster checks use 16, 16, and 15 states in the pre, COVID, and 2024 periods.

The primary RDs are period-specific estimates for all children ages 6–17 in historically non-CE states. Current-MEDB estimates are secondary analyses, while no-current-MEDB estimates provide a specificity check. I do not use changes across periods to identify effects of the enrollment freeze or the 2024 mandate. The periods also differ in pandemic conditions and Medicaid unwinding activity. Historically CE states provide descriptive context only. A causal difference-in-discontinuities interpretation would require the two state groups' discontinuities to follow parallel trends absent the federal policy changes. Selective CE adoption makes that assumption difficult to defend.

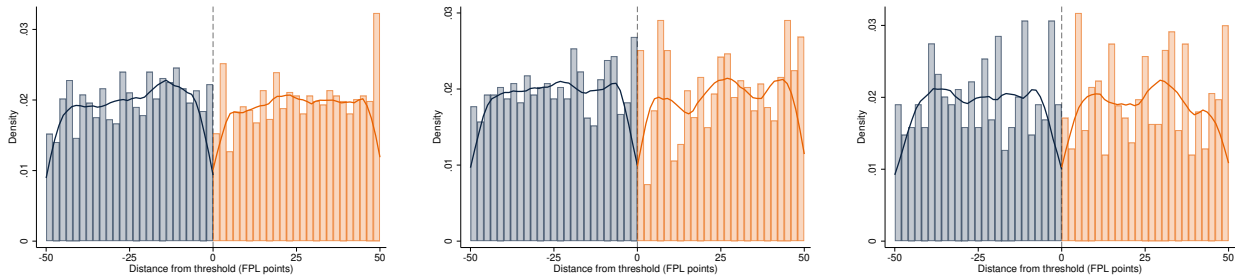
I apply Benjamini–Hochberg and Holm adjustments separately within each period. The five-outcome family includes psychiatric medication, counseling, Medicaid/CHIP coverage, private-only coverage, and

[1] Robust bias correction estimates and removes the leading local-polynomial approximation bias. Its standard error accounts for the additional variability from estimating that bias. I use separate inference checks for the discrete running variable and the small number of state clusters.

uninsured status. Benjamini–Hochberg controls the false discovery rate, while Holm controls the family-wise error rate.

4.2. Identification

Identification requires the conditional means of potential outcomes to vary smoothly through the Medicaid eligibility threshold (Hahn et al., 2001; Imbens and Lemieux, 2008). I assess this implication using the running-variable density and 18 measured covariates in the all-child population. Figure 1 shows the density evidence. Density tests do not reject continuity in any period. None of the balance tests rejects at 5 percent before the pandemic or during COVID. One rejects in 2024 and concerns the residual household-language category. Four balance measures describe the responding caregiver's current mental health, so the panel includes contemporaneous characteristics. These checks do not rule out changes in unobserved determinants.



(A) Pre (2016–2019). $T = 0.77$, $p = 0.44$ (B) COVID (2021–2022). $T = -0.96$, $p = 0.34$ (C) 2024. $T = 0.27$, $p = 0.79$

Figure 1. Running-variable density at the Medicaid eligibility threshold among all children in historically non-CE states.

Notes. The running variable is family income minus the state-year Medicaid threshold, both measured in FPL points. Histograms use 2-FPL bins. Solid curves are unweighted, side-specific kernel-density estimates within ±50 FPL. T is the robust `rddensity` statistic (Cattaneo et al., 2020). The strict sample includes all children ages 6–17 in historically non-CE separate-CHIP state-years with at least 50 FPL points before the next cutoff. These states did not guarantee 12 months of children's Medicaid coverage as of January 2016. CE denotes continuous eligibility, FPL denotes federal poverty level, and COVID denotes coronavirus disease 2019.

Observation of the psychiatric-medication item also matters for identification. Within the estimation window, the item is observed for more than 98 percent of children in every period. Before the pandemic, the estimated response discontinuity is 6.0 percentage points, and the robust test rejects continuity of item response at the 5 percent level. The corresponding COVID and 2024 response discontinuities are 2.5 and 3.7 points, and neither robust test rejects at that level. To assess sensitivity to missing responses, I re-estimate the medication discontinuity twice, once coding all missing responses as no medication and once coding them all as medication.

Two features complicate inference. The integer-coded running variable has many mass points (Lee and Card, 2008; Kolesár and Rothe, 2018), and each period contains only 15 or 16 states. In the primary analysis, I cluster robust bias-corrected standard errors by running-variable support point. I report state-clustered

inference as a sensitivity check for within-state dependence. Because the number of state clusters is small, I apply a state wild bootstrap to the conventional local-linear estimate, with details in Supplementary Section S13. That section also reports fixed-bandwidth honest intervals from a separate estimator (Armstrong and Kolesár, 2020).

4.3. Identification Limits

The running variable measures reported family income relative to KFF's school-age Medicaid limit. It only approximates the eligibility margin because NSCH income refers to the prior calendar year and does not reproduce MAGI, while KFF thresholds can differ from operational program rules. I assess the timing mismatch with income-reference-year thresholds. The cutoff is therefore a reported-income policy boundary, not an administrative eligibility determination. Supplementary Section S6.4 reports the timing sensitivity, and Supplementary Section S11.2 provides documentary context for selected KFF thresholds.

The all-child population avoids conditioning the primary estimate on diagnosis. The current-MEDB secondary analyses do condition on diagnosis. The diagnosed subgroup can differ across the threshold because both diagnosis prevalence and the mix of diagnoses can change. These estimates therefore compare outcomes among the diagnosed children observed on each side. They do not compare a diagnosed population with the same composition on both sides. The current-MEDB and no-current-MEDB groups are drawn from the same state clusters. I do not estimate the covariance between the two estimates, so I do not formally test whether their discontinuities differ.

The design estimates local outcome discontinuities but does not identify the pathways that produce them. NSCH combines Medicaid and CHIP coverage and observes each sampled child only once. It does not report program-specific enrollment, plan changes, provider continuity, or treatment histories. Longitudinal records tracking each child's public program, providers, and treatment history would be needed to determine whether the discontinuities reflect a program transition, provider disruption, or another process. Supplementary Section S10.4 summarizes the related identification limits and diagnostic evidence.

5. Results

5.1. Primary Medication Discontinuities

Table 2 reports the five all-child outcomes and the two secondary outcomes for children with a current MEDB diagnosis. Figure 2 displays the medication estimates. Before the pandemic, medication use is 9.7 percentage points higher just above the threshold. The 95 percent confidence interval runs from 2.4 to 17.0 points. Both the Benjamini–Hochberg and Holm adjusted p-values are 0.047. During the enrollment freeze, the estimate is 16.9 points, with a confidence interval from 3.4 to 30.4 points. Both adjusted p-values are 0.071. The 2024 estimate is 7.5 points and does not reject zero at the 5 percent level.

Table 2. Primary All-Child Outcomes and Secondary Outcomes among Children with a Current MEDB Diagnosis

Outcome	Pre (2016–19)	COVID (2021–22)	2024
All children ages 6–17			
Psychiatric medication use	0.097***	0.169**	0.075
	(0.037)	(0.069)	(0.063)
N_h	3,482	2,022	1,026
Mental-health treatment or counseling	−0.009	0.104*	−0.164
	(0.043)	(0.055)	(0.136)
N_h	3,512	2,054	1,036
Medicaid/CHIP coverage	0.107	−0.126	0.275**
	(0.083)	(0.078)	(0.140)
N_h	3,395	2,047	1,033
Private-only insurance coverage	−0.022	0.137*	−0.104
	(0.071)	(0.077)	(0.120)
N_h	3,466	2,020	1,022
Uninsured	−0.076	−0.006	−0.221**
	(0.051)	(0.045)	(0.107)
N_h	3,466	2,020	1,022
Children with a current MEDB diagnosis			
Psychiatric medication use	0.328***	0.438***	0.126
	(0.113)	(0.138)	(0.090)
N_h	998	653	354
Mental-health treatment or counseling	−0.157	0.211	−0.161*
	(0.117)	(0.141)	(0.095)
N_h	1,002	657	355

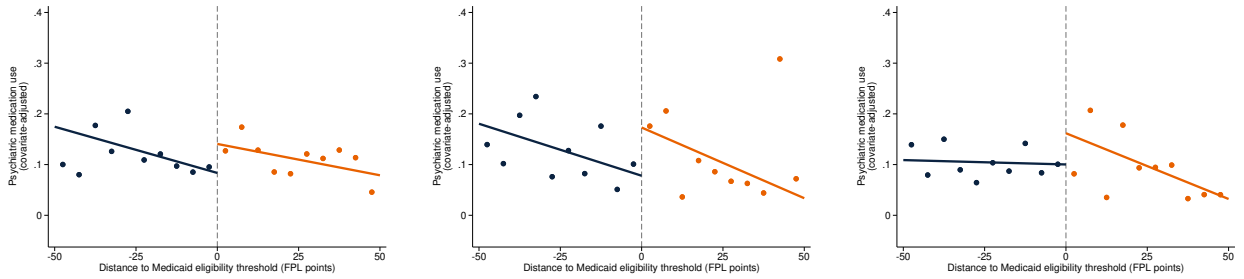
Notes. Entries are robust bias-corrected regression-discontinuity estimates. Robust standard errors clustered by running-variable support point appear in parentheses. N_h is the unweighted complete-case sample within the effect bandwidth. Models use survey weights, a triangular kernel, covariates, and $h = b = 50$ FPL points. The strict sample retains historically non-CE separate-CHIP state-years with at least 50 FPL points before the next cutoff. CHIP denotes Children's Health Insurance Program, COVID denotes coronavirus disease 2019, FPL denotes federal poverty level, and MEDB denotes a mental, emotional, developmental, or behavioral diagnosis. Stars use unadjusted robust p-values. Within the five-outcome all-child family, Benjamini–Hochberg/Holm adjusted p-values for psychiatric medication are 0.047/0.047, 0.071/0.071, and 0.295/0.684. In 2024, the corresponding values are 0.123/0.197 for Medicaid/CHIP and 0.123/0.193 for uninsured. Supplementary Section S9 reports the complete adjusted results. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3 compares support-point and state-clustered inference across the three populations. Supplementary Section S13.4 reports state wild-bootstrap inference for the conventional local-linear estimates rather than the bias-corrected estimates in the table. For all children, the support-point and state-clustered tests both reject zero at 5 percent before the pandemic. During COVID, the support-point test rejects zero at that level, while the state-clustered test narrowly misses it. The corresponding wild-bootstrap tests do not reject zero in either period. For children with a current MEDB diagnosis, the support-point and state-clustered tests both reject zero at 5 percent in the first two periods, while the wild bootstrap rejects only during COVID. For children without a current MEDB diagnosis, the support-point test, state-clustered test, and wild bootstrap find no evidence that the medication discontinuity differs from zero in any period. All three tests reject zero at 5 percent only for children with a current MEDB diagnosis during COVID. For all children, the three tests do not reach the same conclusion before the pandemic or during COVID. In 2024, none of the three tests finds evidence of a medication discontinuity in any population.

Table 3. Inference for Psychiatric-Medication Discontinuities by Population

Population	Period	$\hat{\tau}^{bc}$	Support p	State p	N_h
All children	Pre	0.097	0.009	0.032	3,482
	COVID	0.169	0.014	0.063	2,022
	2024	0.075	0.236	0.388	1,026
Current MEDB	Pre	0.328	0.004	0.010	998
	COVID	0.438	0.001	0.001	653
	2024	0.126	0.160	0.442	354
No current MEDB	Pre	−0.008	0.544	0.620	2,484
	COVID	−0.002	0.781	0.791	1,369
	2024	0.069	0.249	0.133	672

Notes. $\hat{\tau}^{bc}$ is the robust bias-corrected estimate under $h = b = 50$. Support and state columns report robust two-sided p-values under the indicated clustering level. N_h is the unweighted complete-case sample within the effect bandwidth. All rows use the past-12-month psychiatric-medication item. State counts are 16 before the pandemic and during COVID and 15 in 2024. Conventional estimates, state wild-bootstrap p-values, and wild-bootstrap confidence intervals appear in Supplementary Section S13.4. COVID denotes coronavirus disease 2019, and MEDB denotes a mental, emotional, developmental, or behavioral diagnosis.



(A) Pre (2016–2019) (B) COVID (2021–2022) (C) 2024

Figure 2. Psychiatric-medication use at the Medicaid eligibility threshold among all children in historically non-CE states.

Notes. Panels show survey-weighted means in 5-FPL bins and covariate-adjusted local-linear fits. The outcome records past-12-month medication use for difficulties with emotions, concentration, or behavior. Fits use a triangular kernel and $h = 50$ FPL points. Robust bias-corrected estimates under $h = b = 50$ are 0.097, 0.169, and 0.075. Non-CE states did not guarantee 12 months of children's Medicaid coverage as of January 2016. CE denotes continuous eligibility, FPL denotes federal poverty level, and COVID denotes coronavirus disease 2019.

5.2. Robustness and Population Scope

Across seven specification checks, the medication estimates remain positive in all three periods. They range from 8.7 to 10.8 percentage points before the pandemic and from 14.3 to 27.7 points during COVID. Six of the seven pre-period estimates and all seven COVID estimates reject zero at 5 percent. The 2024 range is 4.8 to 12.5 points, and none of those estimates rejects zero. The grid varies the bias-correction bandwidth, covariates, effect bandwidth, a three-FPL donut, and fixed effects. The wider bias-correction window remains a sensitivity because it can cross the separate-CHIP ceiling.

The medication estimates vary with the outcome definition and treatment of missing responses. The expanded medication measure gives estimates of 8.0, 19.4, and 11.1 percentage points. Coding every missing K4Q23 response as no medication gives 10.0, 17.1, and 8.2 points. Coding every missing response as medication reduces the estimates to 4.0, 14.5, and 4.6 points. The COVID estimates stay above 14 points under every measure and coding rule. Only the coding that treats every missing response as medication removes the pre-period rejection at 5 percent. No 2024 estimate rejects zero at that level.

Income-measurement checks preserve the same ordering across periods. Using income-reference-year thresholds gives estimates of 9.4, 16.1, and 7.5 percentage points. Restoring observations with imputed income and combining all six FPL implicates gives 8.6, 12.7, and 8.3 points. The Rubin-combined estimate rejects zero at 5 percent before the pandemic, narrowly misses that level during COVID, and does not reject in 2024. Year-specific estimates are positive in every retained survey year. During COVID, they are 16.8 points in 2021 and 13.9 points in 2022, so the pooled estimate is not driven by one survey wave.

Before and during COVID, the medication discontinuity is concentrated among children with a current MEDB diagnosis. The estimates for that subgroup are 32.8 and 43.8 percentage points. The corresponding estimates for children without a current MEDB diagnosis are −0.8 and −0.2 points. In a common sample,

medication use can be divided into current-MEDB and no-current-MEDB components. Before rounding, the two component estimates sum exactly to the all-child estimate. The current-MEDB components are 10.4 and 17.0 points, while the no-current-MEDB components are −0.7 and −0.1 points. In 2024, the current-MEDB component is 2.1 points and the no-current-MEDB component is 5.3 points. Unlike in the earlier periods, the no-current-MEDB component is larger.

Conditioning on diagnosis introduces a separate composition concern. In the all-child sample, any current MEDB diagnosis changes by 5.9 points before the pandemic, 14.0 points during COVID, and −14.3 points in 2024. None of those estimates rejects zero at 5 percent. The data therefore do not show a clear change in the overall share of children with a current MEDB diagnosis at the threshold. Current ADHD diagnosis changes by 7.5, 17.1, and −1.1 points across the same periods. The first two estimates reject zero at 5 percent, while the 2024 estimate does not. These ADHD results raise the possibility that the diagnosed population differs across the threshold before and during COVID, even though the overall MEDB estimates do not reject zero. The remaining checks use the current-MEDB sample, an effect bandwidth of 50 FPL points, and a wider 70-point bias-correction bandwidth. During COVID, a count of current ADHD, depression, anxiety, behavior or conduct problems, and autism diagnoses rises by 0.70 at the threshold and rejects zero at 5 percent. Children with a current MEDB diagnosis just above the threshold are therefore estimated to have, on average, about 0.70 more of these diagnoses than those just below it. Adding that count to the medication model reduces the estimated discontinuity from 36.7 to 22.9 points. After adjustment, the estimate does not reject zero at 5 percent, although it is close to doing so. This reduction suggests that diagnosis composition may contribute to the medication estimate. The adjustment does not eliminate the selection concern because diagnosis itself may change at the threshold.

5.3. Coverage and Counseling Outcomes

Among all children, coverage outcomes do not show a precise shift before the pandemic. Medicaid/CHIP coverage rises by 10.7 percentage points, private-only coverage falls by 2.2 points, and uninsured status falls by 7.6 points. None of the three estimates rejects zero at 5 percent. During COVID, Medicaid/CHIP coverage falls by 12.6 points and private-only coverage rises by 13.7 points. Neither estimate rejects zero at 5 percent, although the private-only estimate rejects at 10 percent. Uninsured status changes by less than one point. Administrative evidence shows that Medicaid–CHIP transfers and CHIP premiums can shift coverage without necessarily raising uninsurance (Orzol et al., 2015; Nikolova and Stearns, 2014). The COVID signs fit that possibility, but the estimates are not precise enough to establish it.

Among all children, the 2024 coverage estimates differ from the earlier periods. Medicaid/CHIP coverage rises by 27.5 percentage points, private-only coverage falls by 10.4 points, and uninsured status falls by 22.1 points. The private-only estimate does not reject zero at 5 percent. The Medicaid/CHIP and uninsured estimates both reject zero at 5 percent before adjustment, but neither does after the Benjamini–Hochberg adjustment. Each 2024 estimate uses about 1,000 children, less than one-third of the corresponding pre-pandemic sample. The three directions are consistent with a shift toward public coverage above the threshold,

with lower private-only coverage and uninsurance. They do not establish crowd-out because the private-only estimate is imprecise and the data do not track children moving between coverage types. One of the 18 balance tests also rejects continuity at 5 percent in 2024, for a residual household-language category. The federal mandate guaranteed 12 months of enrollment in Medicaid and, subject to continued CHIP eligibility, in separate CHIP. It therefore created no clear continuous-eligibility contrast at the threshold in 2024. These estimates do not establish a coverage effect of the mandate. Uninsured status does not reject zero before the pandemic or during COVID, as H3 predicts, and in 2024 it rejects only before adjustment.

Among all children, counseling use is estimated to be 0.9 percentage points lower just above the threshold before the pandemic and 10.4 points higher during COVID. The 2024 estimate is 16.4 points lower. None of the three estimates rejects zero at 5 percent, although the COVID estimate rejects at 10 percent. Among children with a current MEDB diagnosis, the corresponding estimates are 15.7 points lower, 21.1 points higher, and 16.1 points lower. None rejects zero at 5 percent, although the 2024 estimate rejects at 10 percent. Taken together, these results do not show that crossing the threshold consistently increases or decreases counseling use. The counseling item combines psychotherapy and medication-management visits, so these results cannot identify substitution between treatment modes.

5.4. Period-Specific Interpretation

The all-child medication estimate is larger during COVID than before the pandemic or in 2024, but that difference does not identify an effect of the enrollment freeze. Therapy supply, telehealth, referral patterns, severity, and survey behavior changed during the pandemic (Golberstein et al., 2020; Hoffnung et al., 2021; Fegert et al., 2020). The similar all-child estimates in 2021 and 2022 show that the pooled pattern spans both retained waves. The larger COVID estimate among children with a current MEDB diagnosis may also reflect the diagnosis-composition change at the threshold.

The 2024 results remain unresolved. The all-child medication estimate is imprecise, and one of the 18 balance tests rejects continuity at 5 percent. One rejection among 18 tests can occur by chance, so it is a caution rather than evidence that children on the two sides differ systematically. Because medication use covers the past 12 months, the 2024 outcome can include months before the federal continuous-eligibility mandate and can overlap state unwinding activity. The Medicaid/CHIP and uninsured estimates do not reject zero after adjustment for multiple testing. Together, these limitations mean that the 2024 results cannot show whether the mandate changed medication use or coverage relative to either earlier period.

6. Mechanism Evidence and Alternative Explanations

Changing plans or physicians can impose costs even when insurance continues (Handel, 2013; Drake et al., 2022; Dahl and Forbes, 2023; Politzer, 2025). Medicare Part D offers a related public-program setting. Low-Income Subsidy enrollees can be reassigned while remaining covered, but the new plan can change formularies and utilization-management rules (Decarolis, 2015; Patel et al., 2013). Medicaid managed-care

plan exits can likewise disrupt provider and prescription use (Politzer, 2025). These studies do not establish what happens at the Medicaid income threshold. They show that continued coverage does not necessarily remove the costs of changing how care is delivered.

For pediatric behavioral health, the portability explanation is that prescriptions and psychotherapy may differ in how easily they continue when coverage terms change. A prescription can often continue through a new prescriber or the same pharmacy. Psychotherapy depends more heavily on continued access to a particular clinician. Changes in provider networks, contracts, credentialing, or authorization can interrupt that relationship. This explanation does not require separate CHIP to be uniformly more restrictive than Medicaid. It requires only that coverage terms differ at the threshold and that maintaining a relationship with a clinician is harder than continuing a prescription when those terms change.

More than one transition process could produce the same pattern observed at the threshold. Under a one-directional explanation, movement from the Medicaid pathway toward separate CHIP disrupts an existing therapy relationship, while movement in the other direction does not produce the same treatment response. Under a bidirectional explanation, changing programs in either direction can interrupt a relationship with a provider. The treatment response can still differ by direction because the destination programs may differ in provider networks, formularies, or authorization rules.

These transitions allow several possible treatment outcomes. Both psychotherapy and medication may continue; psychotherapy may be interrupted while medication continues; medication may be interrupted while psychotherapy continues; or both may be interrupted. Under the portability explanation, preserving a prescription is expected to be easier than preserving a relationship with a therapist. Provider disruption can therefore occur in both directions even when the medication response differs by direction. These are competing explanations for the same observed discontinuity, not separate effects identified by the RD.

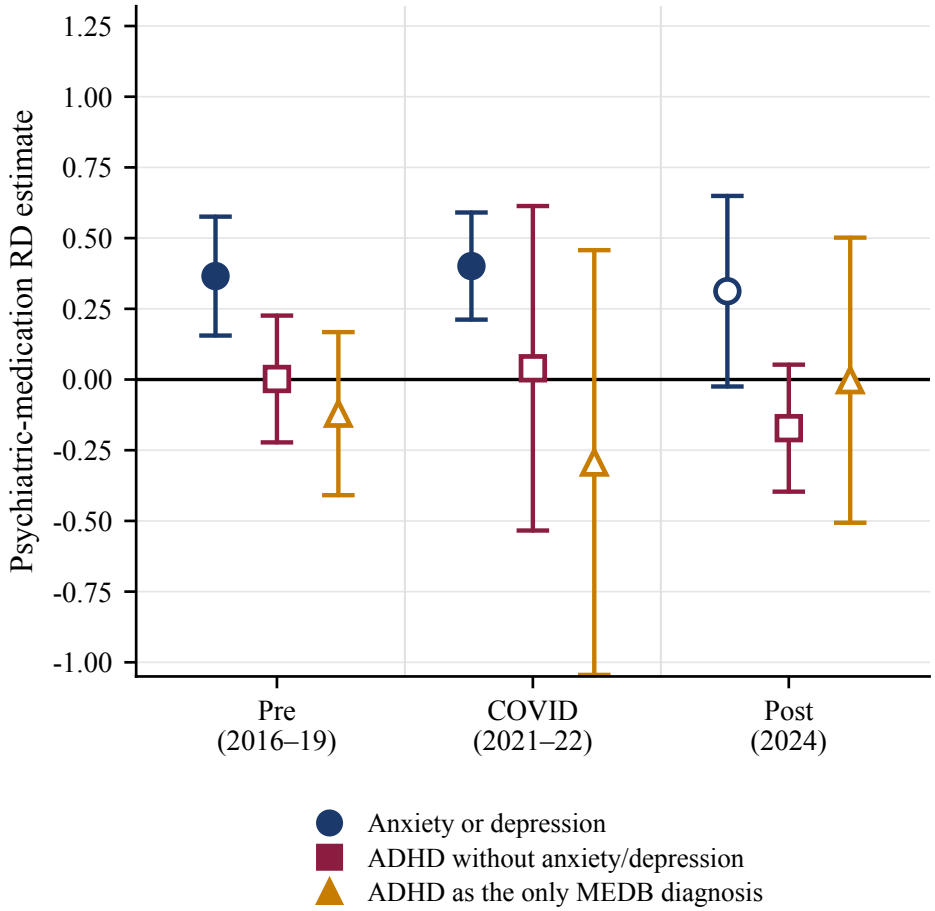
The NSCH cannot distinguish among these transition explanations or treatment outcomes. It does not record a child's previous coverage program or whether, when, or in which direction a transition occurred. Its counseling item also cannot separate psychotherapy visits from medication-management visits. The diagnosis-group analysis therefore asks only whether the estimates move in the directions predicted by the portability explanation. Even if they do, the analysis cannot identify a program switch or its direction. It also cannot show that a child lost access to a therapist or that medication replaced psychotherapy.

6.1. Treatment-Modality Heterogeneity

H2 compares children with anxiety or depression with two ADHD groups in Figure 3. The broader group has ADHD but neither anxiety nor depression; the narrower group has ADHD as its only current MEDB diagnosis. The diagnoses help distinguish groups for which guidelines support different treatment approaches, but they do not show which treatment an individual child received. Clinical guidelines list psychotherapy, medication, or combined care for anxiety and depression, with sequencing based on diagnosis and severity (Walter et al., 2020; Higa-McMillan et al., 2016; Walter et al., 2023). Medication is a guideline-supported

first-line option for school-aged children with ADHD (Wolraich et al., 2019). The portability explanation therefore predicts a more consistently positive medication discontinuity for anxiety or depression than for ADHD without either condition. The ADHD-only group provides a narrower check of the same prediction.

The diagnosis-group results follow the H2 sign pattern, but the evidence remains suggestive. The estimates for anxiety or depression are 36.6, 40.1, and 31.2 percentage points. Under the unadjusted support-point inference used in Figure 3, the pre-period and COVID estimates reject zero at the 1 percent level. In 2024, the estimate rejects at the 10 percent level but not at the 5 percent level. For ADHD without anxiety or depression, the estimates are 0.2, 4.0, and -17.2 points, while those for the more restrictive ADHD-only group are -12.1, -29.4, and -0.2 points. None of the estimates for either ADHD group rejects zero at the 10 percent level under the same inference. Among the ADHD groups, the 2024 cells contain 84 observations for ADHD without anxiety or depression and 33 for the nested ADHD-only group. These small ADHD samples make the 2024 estimates especially imprecise. Unlike the earlier periods, 2024 contains only a single survey wave, which further limits cross-period comparisons. Taken together, the medication discontinuity is consistently positive in the anxiety-or-depression group but not in either ADHD comparison group. This pattern provides suggestive support for H2 and the portability explanation. These results suggest that children whose care can involve psychotherapy may account for more of the overall medication increase at the threshold. Available data cannot determine that share because the diagnosis groups overlap and do not cover all children in the analysis. Because the estimates come from the same state clusters, a separate test is needed to determine whether the group effects differ.



(A) RD point estimates with 95% CIs

Effect-bandwidth sample	Pre	COVID	2024
Anxiety or depression	498	363	214
ADHD without anxiety or depression	275	168	84
ADHD as the only MEDB diagnosis	114	70	33

(B) Unweighted N_h

Figure 3. Psychiatric-medication discontinuities by diagnosis group in historically non-CE states.

Notes. Points are robust bias-corrected estimates from the primary $h = b = 50$ FPL-point specification. Bars are 95 percent confidence intervals. Standard errors are clustered by running-variable support point. ADHD denotes attention-deficit/hyperactivity disorder, COVID denotes coronavirus disease 2019, FPL denotes federal poverty level, MEDB denotes a mental, emotional, developmental, or behavioral diagnosis, and RD denotes regression discontinuity. The ADHD-only group is nested within the ADHD-without-anxiety-or-depression group. The 2024 ADHD cells contain 84 and 33 observations, respectively. Within-group estimates do not establish differences between groups. Supplementary Section S3.1 reports complete primary and $h = 50, b = 70$ sensitivity results.

The ADHD-only definition remains narrow for a clinical reason. Clinical guidance for behavior or conduct problems, autism, and Tourette syndrome includes behavioral and pharmacologic options that depend on the condition and symptom burden (Steiner and Remsing, 2007; Riley et al., 2016; Hyman et al., 2020; Murphy et al., 2013; Pringsheim et al., 2019). Adding those diagnoses would broaden the treatment mix rather than simply increase the sample size. Keeping the comparison limited to ADHD preserves the intended treatment contrast, but the smaller sample produces less precise estimates.

6.2. Specificity and Alternative Explanations

Children without a current MEDB diagnosis face the same statutory threshold but rarely use psychiatric medication. Table 3 reports their medication estimates, and Supplementary Section S10.3 reports the full falsification grid. The medication discontinuities are -0.8, -0.2, and 6.9 percentage points, and none rejects zero at the 5 percent level. Because medication use is already rare in this group, it has little room to fall but can still rise at the threshold. The small negative pre-period estimate is consistent with H1's prediction that the medication discontinuity is concentrated among children with a current diagnosis. The COVID estimate shows the same near-zero pattern under a different enrollment regime. The 2024 estimate is positive but imprecise and does not reproduce the earlier pattern.

The portability explanation is not unique. Program-specific prescribing practices could change medication use even if children do not switch providers. Preferences associated with income or other unmeasured differences between children just above and below the threshold could also produce the pattern. The additional analyses narrow several of these explanations. Positive pre-period and COVID estimates among all children show that the findings do not arise only from limiting the sample to children with a current diagnosis. Near-zero pre-period and COVID estimates among children without a current diagnosis make a broad medication increase across both diagnosis groups less consistent with the evidence. Because medication use is uncommon in that group, however, those estimates cannot rule out a smaller increase. The COVID estimate also remains positive under the alternative medication measure and both missing-response codings. A single outcome definition or treatment of missing data is therefore less likely to explain the COVID finding. The imprecise 2024 results do not narrow the alternatives in the same way. The pre-period evidence also remains less secure. Its estimate weakens when every missing medication response is treated as medication. Before the pandemic, the robust test also finds a discontinuity in the medication-question response rate at the 5 percent level. This means that differences in who answered the medication question may explain part of the pre-period finding. Unmeasured differences between children on either side of the threshold could still produce an apparent discontinuity. If children changed programs, differences in formularies or prior-authorization rules could alter prescribing without a provider switch, or a network change could interrupt psychotherapy while medication continues. Because the current data do not track these changes for the same child, they cannot show whether any of these pathways operate. Future research could link family income and eligibility histories to actual program enrollment, plan rules, provider continuity, psychotherapy use, and medication fills for the same child. Combined with designs that isolate changes in coverage

rules or provider access, these records could test whether program transitions, prescribing restrictions, or psychotherapy disruptions precede and contribute to the medication increase.

7. Conclusions

Psychiatric-medication use changes locally at a reported-income policy boundary between Medicaid and separate CHIP eligibility. Among all children, medication use is estimated to be 9.7 percentage points higher just above the threshold before the pandemic and 16.9 points higher during the enrollment freeze. The corresponding 2024 estimate is 7.5 points and does not reject zero at the 5 percent level. The statistical evidence depends on how uncertainty is measured and how missing responses are handled. Tests clustered at income support points reject zero at the 5 percent level for the first two estimates, while state wild-bootstrap tests do not. After adjusting for the five outcomes tested together, only the pre-period medication estimate rejects zero at the 5 percent level under both adjustment methods. That pre-period estimate, however, no longer rejects zero when every missing medication response is treated as medication. The COVID estimate remains positive under every measurement and coding check, although its primary estimate does not reject zero under either multiple-testing adjustment. The 2024 result remains unresolved.

The diagnosis-group results show a different pattern across children with and without a current MEDB diagnosis, but they do not identify why medication use changes at the threshold. Among children with a current MEDB diagnosis, the medication estimates are 32.8 percentage points before the pandemic and 43.8 points during COVID. Both reject zero at the 5 percent level under support-point and state-clustered tests, but only the COVID estimate does so under the state wild bootstrap. The corresponding estimates among children without a current MEDB diagnosis are near zero and do not reject zero at the 5 percent level under any of the three tests. Before the pandemic, this contrast is consistent with H1's prediction that the discontinuity is concentrated among children with a current diagnosis. Smaller changes remain possible among children without a current diagnosis because medication use is rare in that group. During COVID, children in the current-MEDB group also had more recorded diagnoses just above the threshold, so the 43.8-point estimate may partly reflect which children enter that subgroup. Estimates for anxiety or depression are positive in all three periods, whereas estimates for both ADHD groups are smaller or negative. This sign pattern provides suggestive support for H2. It suggests that the medication increase is larger among children whose guideline-supported care more often includes psychotherapy than among the ADHD groups. Because the diagnosis groups overlap and were not formally compared, the estimates cannot establish that their effects differ. They also cannot distinguish a change in prescribing rules from a disruption of psychotherapy after a program or provider-network change.

The policy implication is narrow but important. Continued public eligibility may not be enough to preserve an ongoing course of behavioral-health care when coverage terms change. The medication results therefore motivate a broader question about the difference between eligibility on paper and the composition of care a child actually receives. The current data cannot answer that question because they do not observe

actual program enrollment, program transitions, or psychotherapy separately from medication-management visits. Two questions follow. First, does receiving coverage through Medicaid rather than separate CHIP change the balance between medication and psychotherapy? Second, how do continuous-eligibility policies change the frequency of transitions between public programs, and do those changes affect children's ability to retain providers and treatments? Answering these questions will require longitudinal data on actual enrollment, program transitions, provider continuity, psychotherapy, and medication, combined with designs that isolate changes in coverage rules. Such evidence could show which policies preserve insurance coverage and continuity in behavioral-health treatment.

Data Availability

The analysis uses public-use National Survey of Children's Health files (U.S. Census Bureau, 2025b) and KFF state-year thresholds (Kaiser Family Foundation, 2016–2024). A supplementary analysis also uses a registered-user IPUMS USA extract from the American Community Survey (Ruggles et al., 2025). The submission archive does not redistribute the registered-user extract. Users can recreate it from the documented sample and variable list under IPUMS terms. The archive contains analysis code, redistributable derived inputs, reference outputs, and instructions for results that require the extract. A permanent repository link will appear in the version of record.

AI-Use Statement

During revisions from May through August 2026, the author used Anthropic Claude and OpenAI Codex as writing, coding, and verification assistants. Exact build identifiers were not retained because the services changed during this period. The tools assisted with prose and LaTeX edits and with identifying limitations and robustness checks. They also helped draft and audit Stata and Python verification code. The author selected the analyses and wording, reviewed all code changes and outputs, and accepts full responsibility for the submitted content. No AI system is an author.

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