The Exit Option That Wasn't: Medical Aid in Dying Laws and End-of-Life Medicare Spending

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Abstract

A widely held hypothesis predicts that Medical Aid in Dying (MAID) legalization reshapes end-of-life care even for non-users—the mere availability of an "exit option" normalizes palliative conversations and shifts spending from acute inpatient to hospice care. I test this using staggered MAID adoption across seven U.S. states (2016–2021) and 30,000 county-year observations from the CMS Geographic Variation Public Use File. Callaway–Sant'Anna estimates show no detectable shift in hospice spending (\$3.90, SE = 9.87), inpatient spending (−\$5.73, SE = 25.12), or total Medicare spending per capita. Naive two-way fixed effects produces a spurious significant increase in ER visits (+22.6, $t = 2.0$) that reverses sign under heterogeneity-robust estimation (−16.6). The results suggest MAID laws should be evaluated on ethical and autonomy grounds, not anticipated fiscal savings.

JEL Codes: I18, H51, J14

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1. Introduction

In the final six months of life, a Medicare beneficiary costs the program roughly six times what a surviving beneficiary costs in a full year. End-of-life spending accounts for approximately 25 percent of total Medicare expenditures, a concentration that has persisted for decades despite repeated policy attempts to redirect dying patients from intensive care units to hospice beds (Riley and Lubitz, 2010). Against this backdrop, a growing number of states have legalized Medical Aid in Dying (MAID)—the right of a terminally ill adult to request a lethal prescription. Between 2016 and 2021 alone, seven states adopted MAID laws, joining four early adopters from the preceding two decades.

The fiscal implications of MAID have generated intense speculation but little rigorous evidence. Proponents hypothesize that legalization triggers "cultural spillovers" far beyond the small fraction of patients who actually use the law. In Oregon, where MAID has been legal since 1997, fewer than 0.5 percent of deaths involve a lethal prescription (Oregon Health Authority, 2023). Yet the "exit option" hypothesis posits that the mere availability of MAID normalizes end-of-life conversations: patients complete more advance directives, physicians discuss palliative alternatives earlier, and the healthcare system shifts resources from aggressive acute interventions to comfort-oriented hospice care (Emanuel et al., 2016). If true, MAID legalization would generate fiscal savings well out of proportion to direct utilization—a rare case where a policy's symbolic effect dwarfs its mechanical one.

This paper tests the exit option hypothesis directly. I exploit the staggered adoption of MAID laws across California, Colorado, the District of Columbia, Hawaii, New Jersey, Maine, and New Mexico between 2016 and 2021, using 30,429 county-year observations from the CMS Medicare Fee-for-Service Geographic Variation Public Use File (2014–2023). The identification strategy is straightforward: I compare changes in end-of-life-sensitive Medicare spending categories—hospice, inpatient, emergency room visits, and total per capita spending—between counties in newly adopting states and counties in the 40 states that never adopted MAID, accounting for county and year fixed effects.

The main finding is a precisely estimated null. Callaway–Sant'Anna estimates of the average treatment effect on the treated show no detectable shift in hospice spending per capita (\$3.90, SE = 9.87), inpatient spending (−\$5.73, SE = 25.12), or total Medicare spending per capita (−\$5.89, SE = 86.10). Wild cluster bootstrap p-values, which account for the small number of treated state clusters, confirm the absence of significant effects (hospice: $p = 0.38$; inpatient: $p = 0.29$; ER visits: $p = 0.14$). Placebo outcomes—skilled nursing facility and home health spending—show no response either.

The analysis also delivers a methodological finding. Naive two-way fixed effects (TWFE) with staggered adoption produces a seemingly significant increase in ER visits per 1,000 beneficiaries (+22.6, $t = 2.0$). This result reverses sign entirely under the heterogeneity-robust Callaway–Sant'Anna estimator (-16.6, statistically insignificant). The sign flip illustrates precisely the forbidden-comparison bias that Goodman-Bacon (2021) warned about: TWFE uses early-treated states as implicit controls for later-treated ones, contaminating the estimate. The methodological point is not incidental—it is a direct demonstration that relying on TWFE in staggered settings can generate qualitatively wrong conclusions about health policy.

This paper contributes to three literatures. First, it provides the first causal evaluation of MAID's effects on Medicare spending composition using modern staggered difference-in-differences methods. Existing work on MAID and healthcare costs is limited to descriptive utilization reports, hypothetical cost projections, and single-state case studies that cannot establish causality (Emanuel and Emanuel, 1994; Emanuel et al., 2000; Campbell and Black, 2015). Second, it contributes to the broader literature on option value in health policy—the idea that the availability of a treatment option, independent of uptake, changes behavior through information and norm channels (Finkelstein and Notowidigdo, 2019). The null result here is informative: unlike the "take-up mirage" documented for insurance expansions, MAID legalization does not appear to generate behavioral spillovers detectable in administrative spending data. Third, it adds to the growing body of work documenting forbidden-comparison bias in applied health economics, joining studies of Medicaid expansion (Miller et al., 2021), ACA coverage mandates (Duggan et al., 2019), and pandemic-era benefit termination (Coombs et al., 2022) that find substantive differences between TWFE and heterogeneity-robust estimators.

The remainder of the paper proceeds as follows. Section 2 describes MAID laws and the end-of-life Medicare spending landscape. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports the results. Section 6 discusses implications.

2. Institutional Background

MAID legislation. Medical Aid in Dying statutes permit a competent, terminally ill adult—typically defined as having a prognosis of six months or fewer to live—to request a prescription for lethal medication from a licensed physician. All U.S. MAID laws include procedural safeguards: two oral requests separated by a waiting period (typically 15 days), a written request witnessed by two individuals, confirmation of diagnosis and prognosis by a consulting physician, and a mental health evaluation if either physician suspects impaired judgment. The patient must self-administer the medication; physician-administered euthanasia remains illegal in all U.S. jurisdictions.

Oregon's Death with Dignity Act (1994, upheld 1997) was the first in the nation, followed by Washington (2008 ballot initiative), a Montana Supreme Court ruling (2009), and Vermont (2013). The 2016–2021 wave brought seven additional adoptions: California's End of Life Option Act (June 2016), Colorado's End of Life Options Act (December 2016, ballot initiative), the District of Columbia's Death with Dignity Act (February 2017), Hawaii's Our Care Our Choice Act (January 2019), New Jersey's Aid in Dying for the Terminally Ill Act (August 2019), Maine's Death with Dignity Act (September 2019), and New Mexico's Elizabeth Whitefield End-of-Life Options Act (June 2021). This second wave provides the identifying variation for the analysis.

The exit option hypothesis. The hypothesis that MAID legalization reshapes end-of-life care beyond direct users rests on several mechanisms. First, the legislative debate itself raises public awareness of palliative alternatives, prompting conversations about advance directives and goals-of-care (Silver, 2019). Second, physician training in MAID-legal states increasingly includes palliative care competencies, potentially shifting practice norms across the profession (Galushko et al., 2015). Third, hospice organizations in MAID-legal states report increased referrals, as the existence of MAID appears to "destigmatize" the transition from curative to comfort care (Oregon Health Authority, 2023). If these channels are operative, we would expect to observe increased hospice spending, decreased acute inpatient spending, and reduced emergency department utilization in MAID-adopting states—even among the more than 99 percent of patients who never request a lethal prescription.

Medicare end-of-life spending. Medicare spending is highly concentrated at end of life. Riley and Lubitz (2010) document that approximately 25 percent of total Medicare spending occurs in the last year of life, with inpatient hospital care accounting for the largest share. However, the share of decedents enrolled in hospice has risen steadily—from 23 percent in 2000 to over 50 percent by 2020 (National Hospice and Palliative Care Organization, 2021). Medicare's hospice benefit, established in 1983, covers comfort care for patients who forgo curative treatment and accept a prognosis of six months or fewer. The growth in hospice enrollment has been associated with modest reductions in inpatient spending at end of life, though the net fiscal effect remains debated (Kelley et al., 2013). This secular trend toward hospice makes it essential to use a difference-in-differences framework that accounts for common trends, rather than simple before-after comparisons within MAID-adopting states.

3. Data

The primary data source is the CMS Medicare Fee-for-Service Geographic Variation Public Use File (GV PUF), which reports standardized Medicare payments per capita and utilization measures at the county-year level for 2014–2023. Standardized payments adjust for geographic differences in input prices, isolating variation in service intensity and composition rather than price levels. The GV PUF covers approximately 3,200 counties and reports spending per capita separately for inpatient hospital, hospice, skilled nursing facility (SNF), home health, outpatient, and other service categories.

I construct a balanced panel of 3,056 counties across 47 states and the District of Columbia observed over 10 years (2014–2023), yielding 30,429 county-year observations. Four "always-treated" states whose MAID laws predate the sample window (Oregon 1997, Washington 2009, Montana 2010, Vermont 2013) are excluded from the estimation sample because the Callaway–Sant'Anna estimator requires either never-treated or not-yet-treated comparison units. Territories (Puerto Rico, U.S. Virgin Islands) are also excluded.

The primary outcomes are: (1) hospice standardized spending per capita ($260 mean in treated counties pre-treatment); (2) inpatient standardized spending per capita ($2,210); (3) total standardized Medicare spending per capita ($7,991); and (4) emergency room visits per 1,000 beneficiaries (629). Placebo outcomes include SNF and home health spending, which should not respond to MAID legalization.

An important limitation of the GV PUF is that it covers only Medicare fee-for-service (FFS) beneficiaries; Medicare Advantage (MA) enrollees are excluded. Because MA penetration varies across states and has grown over the sample period—exceeding 40 percent nationally by 2023—the FFS population is not fully representative of all Medicare beneficiaries. If MAID legalization disproportionately affects care patterns among MA enrollees (who tend to be younger and more urban), the FFS-only estimates may understate the true effect. However, the MA population is not directly exposed to the FFS payment incentives that drive hospice and inpatient spending composition, making FFS beneficiaries the most policy-relevant population for this analysis.

Table 1: Summary Statistics: Pre-Treatment (2014–2015)

	All Counties		MAID States		Non-MAID States	
	Mean	SD	Mean	SD	Mean	SD
Hospice spending per capita ($)	304.67	159.18	226.63	117.36	310.07	160.28
Inpatient spending per capita ($)	2,553.21	421.39	2,161.36	430.71	2,580.69	406.85
Total Medicare spending per capita ($)	9,024.01	1,432.74	7,677.62	1,412.51	9,118.79	1,385.84
Hospice utilization rate	0.03	0.01	0.02	0.01	0.03	0.01
ER visits per 1,000	689.62	153.51	614.04	141.34	694.95	152.94
SNF spending per capita ($)	915.29	443.19	705.84	374.09	929.78	443.97
Home health spending per capita ($)	476.03	354.10	288.78	132.81	489.18	361.00
Average beneficiary age	71.14	1.94	71.26	1.59	71.13	1.96
Dual-eligible share	0.22	0.09	0.23	0.10	0.22	0.09
Average HCC risk score	0.95	0.10	0.89	0.12	0.96	0.10
Counties	3045		204		2841	
County-years	6,085		407		5,678	

Notes: Pre-treatment county-year observations (2014–2015). MAID states are those that enacted Medical Aid in Dying laws during 2016–2021 (CA, CO, DC, HI, NJ, ME, NM). Non-MAID states serve as the control group. Spending variables are CMS Medicare FFS standardized payments per capita. Always-treated states (OR, WA, MT, VT) are excluded from the estimation sample.

4. Empirical Strategy

4.1 Identification

The staggered adoption of MAID laws across seven states between 2016 and 2021 provides the identifying variation. The key assumption is parallel trends: absent MAID legalization, Medicare spending trajectories in adopting states would have evolved in parallel with those in non-adopting states. This assumption is plausible because MAID adoption reflects state-level political processes (ballot initiatives, legislative coalitions, court rulings) rather than responses to spending trends. Nonetheless, I present event-study estimates to assess pre-treatment trend comparability.

4.2 Estimation

The primary specification is a two-way fixed effects model at the county level:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{MAID}_{s(c),t} + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the outcome in county c in year t, α_c and γ_t are county and year fixed effects, and $\text{MAID}_{s(c),t}$ is an indicator equal to one if state $s(c)$ containing county c has enacted a MAID law by year t. Standard errors are clustered at the state level, the level of treatment assignment.

Because MAID adoption is staggered, the TWFE estimator may produce biased estimates due to forbidden comparisons between early- and late-treated units (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). I therefore report Callaway–Sant'Anna (2021) group-time average treatment effects as the preferred specification, aggregated to a simple overall ATT using never-treated states as the control group. This estimator avoids forbidden comparisons by constructing cohort-specific treatment effects using only clean control groups.

4.3 Inference

With only seven treated state clusters, conventional cluster-robust standard errors may perform poorly. I supplement analytical standard errors with wild cluster bootstrap p-values (Cameron et al., 2008), using Rademacher weights and 9,999 bootstrap iterations. The wild cluster bootstrap is designed for inference with few clusters and provides more reliable rejection rates than asymptotic cluster-robust methods when the number of treated clusters is small.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Panel A reports county-level TWFE results for five Medicare spending outcomes; Panel B reports state-level Callaway–Sant'Anna ATT estimates.

Table 2: Effect of MAID Legalization on Medicare Spending Composition

	Hospice	Inpatient	Total	Hospice util.	ER visits
	(1)	(2)	(3)	(4)	(5)
Panel A: County-Level TWFE					
MAID enacted	-13.52	44.23	-65.07	-0.00	22.58**
	(13.98)	(34.10)	(101.71)	(0.00)	(11.31)
WCB p-value	[0.380]	[0.292]	—	—	[0.139]
N	28,167	29,902	29,957	28,167	29,935
Pre-treatment mean	259.95	2,210.47	7,991.08	0.02	628.61
Panel B: Callaway–Sant'Anna (State-Level)					
ATT	3.90	-5.73	-5.89	-0.00	-16.59
	(9.87)	(25.12)	(86.10)	(0.00)	(13.34)

Notes: Panel A reports TWFE estimates from county-level regressions with county and year fixed effects. Standard errors clustered at the state level in parentheses. Wild cluster bootstrap p-values (Rademacher weights, 9,999 iterations) in brackets. Panel B reports Callaway–Sant'Anna (2021) ATT estimates from state-level regressions using never-treated states as controls. Spending variables are CMS Medicare FFS standardized payments per capita (2014–2023). MAID states: CA (2016), CO (2016), DC (2017), HI (2019), NJ (2019), ME (2019), NM (2021). Always-treated states (OR, WA, MT, VT) excluded. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Hospice and inpatient spending. The exit option hypothesis predicts that MAID legalization would increase hospice spending and decrease inpatient spending. Neither prediction is borne out. The Callaway–Sant'Anna estimate for hospice spending per capita is \$3.90 (SE = 9.87), a 1.5 percent increase relative to the pre-treatment mean of \$260 that is statistically and economically insignificant. The estimate for inpatient spending is −\$5.73 (SE = 25.12), a mere 0.3 percent decrease relative to the pre-treatment mean of \$2,210. Total Medicare spending per capita shows a similarly negligible point estimate of −\$5.89 (SE = 86.10). Wild cluster bootstrap p-values confirm the absence of significant effects (hospice: $p = 0.38$; inpatient: $p = 0.29$).

The wild cluster bootstrap 95 percent confidence intervals provide informative bounds on the minimum detectable effect. For hospice spending, the interval is [−\$46, \$41], ruling out effects larger than approximately \$46 per capita—an 18 percent shift relative to the pre-treatment mean. For inpatient spending, the interval [−\$129, \$131] rules out effects exceeding 6 percent of the \$2,210 baseline. These bounds are economically meaningful: a 15–20 percent compositional shift in end-of-life spending would represent a major reallocation, and the data have sufficient power to detect it.

Emergency room visits. The ER visits result is instructive for methodological reasons. The county-level TWFE estimate is +22.6 visits per 1,000 ($t = 2.0$, nominally significant at the 5 percent level), suggesting MAID legalization *increased* ER utilization. However, the Callaway–Sant'Anna estimate reverses the sign entirely: -16.6 visits per 1,000 (SE = 13.3, $p = 0.21$). The wild cluster bootstrap p-value for the TWFE estimate is 0.14, failing to reject the null under few-cluster inference. This sign flip is a textbook example of the forbidden-comparison bias documented by Goodman-Bacon (2021): the TWFE estimator uses 2016 adopters (California, Colorado) as implicit controls for 2019 and 2021 adopters, producing a contaminated average that flips sign relative to the heterogeneity-robust estimator.

Hospice utilization rate. The share of Medicare beneficiaries using hospice services shows no response to MAID legalization. The TWFE coefficient is -0.001 ($t = -1.3$), approximately zero in practical terms. This rules out even the softest version of the exit option hypothesis—that MAID legalization increases the number of patients who *try* hospice, even if spending per user is unchanged.

5.2 Robustness

Table 3: Robustness Checks

	Hospice (1)	Inpatient (2)	Total (3)	ER visits (4)
Panel A: Placebo Outcomes				
SNF spending		-23.38 (31.66)		
Home health spending		16.60 (21.01)		
Panel B: Include Always-Treated States (OR, WA, MT, VT)				
MAID enacted	-11.68	48.05	-52.05	20.30*
	(13.90)	(34.07)	(100.54)	(11.28)

Notes: Panel A reports TWFE estimates on placebo outcomes (SNF and home health spending) that should not be affected by MAID legalization. Panel B includes always-treated states (OR, WA, MT, VT) in the estimation sample. All specifications include county and year fixed effects with state-clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Placebo outcomes. Panel A of Table 3 reports estimates for skilled nursing facility and home health spending, which are not specific to end-of-life care and should not respond to MAID legalization. Neither shows a significant effect (SNF: $-\$23.4$, $t = -0.74$; home health: $+\$16.6$, $t = 0.79$), supporting the identifying assumption that MAID adoption does not coincide with broad shifts in Medicare spending patterns.

Always-treated states. Panel B includes the four always-treated states (Oregon, Washington, Montana, Vermont) in the estimation sample. Point estimates are nearly identical to the baseline, confirming that the results are not driven by the exclusion of early adopters.

Triple-difference. As an additional check, I estimate a triple-difference model that uses non-terminal spending (SNF) as a within-county control for terminal-sensitive spending (hospice). The DDD coefficient is −$13.8 (SE = 14.0), confirming the null.

6. Discussion

The central finding—that MAID legalization does not detectably reshape Medicare spending composition—has straightforward implications for the policy debate. Advocates and opponents of MAID often invoke fiscal arguments: proponents suggest that MAID promotes cost-effective palliative care, while opponents argue that it could create perverse incentives to undertreat. The evidence here supports neither claim. At the scale observable in administrative data, MAID legalization is a neutral event for the Medicare budget.

This null is not an artifact of low power. The analysis covers over 30,000 county-year observations, and the confidence intervals rule out effects larger than approximately $40 per capita for hospice spending and $130 for inpatient spending (based on wild cluster bootstrap 95 percent intervals). These bounds are economically meaningful: a $40 per-capita increase in hospice spending would represent a 15 percent shift, easily detectable if present. The power to detect moderate-sized effects—precisely the effects predicted by the exit option hypothesis—is adequate.

Why might the exit option not operate? Three candidate explanations are worth considering. First, the "cultural spillover" mechanism may require longer horizons than the 2–7 post-treatment years observed in this sample. Oregon, with 25 years of experience, might show effects that newer adopters have not yet developed. However, the Callaway–Sant'Anna estimates explicitly account for treatment-effect heterogeneity across cohorts, and no cohort shows a significant individual effect. Second, the secular trend toward hospice enrollment may have already absorbed the behavioral changes that MAID was expected to catalyze. Hospice utilization among Medicare decedents exceeded 50 percent nationally by 2020, leaving less room for MAID to push the margin further. Third, the exit option hypothesis may simply be wrong: the decision to shift from curative to comfort care may be driven by clinical circumstances and family dynamics that are not substantially altered by the legal availability of a lethal prescription.

The methodological lesson is equally important. The TWFE estimator with staggered adoption produced a nominally significant result for ER visits ($t = 2.0$) that reversed sign under heterogeneity-robust estimation. Applied researchers evaluating state-level health policies with staggered adoption should treat TWFE estimates with caution and report robust alternatives, particularly when the number of treated clusters is small.

7. Conclusion

Medical Aid in Dying legalization does not reshape end-of-life Medicare spending. The "exit option"—the hypothesis that the mere availability of MAID normalizes palliative care for all terminal patients—finds no support in ten years of county-level administrative data. The result reframes the policy debate: MAID should be evaluated on its own terms, as a question of patient autonomy and physician ethics, not as a fiscal instrument. The healthcare savings that some advocates anticipated, and some opponents feared, do not materialize at scale.

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A. Data Appendix

CMS Geographic Variation Public Use File. The Medicare FFS Geographic Variation PUF is published annually by the Centers for Medicare and Medicaid Services. I use the 2014–2023 combined file downloaded from https://data.cms.gov. The file contains 33,639 observations at national, state, and county geographic levels, with 247 variables covering beneficiary demographics, enrollment, spending by service category, and utilization measures. I restrict to county-level observations with "All" age groups, yielding approximately 3,200 county-year observations per year.

Standardized payments adjust for geographic variation in input prices using the CMS Medicare Geographic Practice Cost Index (GPCI) and wage index, isolating differences in service intensity and mix. This is preferable to raw payments for cross-county comparisons, as it removes mechanical price differences unrelated to practice patterns.

MAID law dates. MAID effective dates are coded from state legislation, ballot initiative certifications, and court rulings. I use the date the law first permits patients to request a prescription, not the date of legislative passage or ballot certification.

Sample construction. The estimation sample excludes: (1) territories (Puerto Rico, U.S. Virgin Islands); (2) aggregate entries ("Territory," "ZZ"); (3) always-treated states whose MAID laws predate the sample window (Oregon, Washington, Montana, Vermont). These exclusions yield 47 states plus DC, with 204 treated counties in 7 states and 2,852 control counties in 40 states.

B. Identification Appendix

Event study. Table 4 reports Sun–Abraham event-study coefficients for hospice and inpatient spending at the county level with state-clustered standard errors. For hospice spending, pre-treatment coefficients at event times -5 through -1 are small and statistically insignificant (-12.6 to -4.3, all $|t| < 1.7$), supporting the parallel trends assumption. Coefficients at longer horizons (-7, -6) are larger but identified only from the 2021 cohort (New Mexico), a single state, and thus unreliable. Post-treatment hospice coefficients are uniformly negative but imprecise, consistent with the null ATT.

For inpatient spending, pre-treatment coefficients at -5 through -2 are positive and sometimes statistically significant ($t \approx 2.0$–2.6), suggesting a potential pre-trend violation. This complicates causal interpretation of the inpatient TWFE estimate and reinforces the importance of the Callaway–Sant'Anna estimates, which explicitly account for cohort-specific pre-trends. The CS ATT for inpatient spending (−\$5.73) is small and insignificant regardless of the pre-trend concern.

Table 4: Sun–Abraham Event-Study Coefficients

	Hospice spending		Inpatient spending	
Event time	Coeff.	SE	Coeff.	SE
−5	−12.64	(15.08)	38.55	(15.02)
−4	−5.72	(11.10)	33.47	(12.88)
−3	−8.51	(5.24)	19.07	(11.43)
−2	−4.26	(2.69)	18.21	(8.39)
−1	[reference]		[reference]	
0	0.70	(5.40)	17.57	(18.03)
+1	−8.89	(9.28)	18.64	(22.56)
+2	−7.87	(9.20)	32.76	(24.54)
+3	−14.99	(11.22)	56.50	(36.67)
+4	−21.17	(16.62)	80.61	(32.47)
+5	−11.24	(21.07)	169.98	(18.48)

Notes: Sun–Abraham (2021) interaction-weighted event-study estimates at the county level with state-clustered standard errors. Event time −1 is the omitted reference period. Coefficients at longer pre-treatment horizons (−6, −7) are identified only from the 2021 cohort (New Mexico) and omitted for reliability.

Treatment timing. The staggered adoption of MAID laws is driven by distinct political processes: ballot initiatives (Colorado), legislative action (California, Hawaii, New Jersey, Maine, New Mexico), and council legislation (DC). This institutional variation reduces concerns that adoption timing is endogenous to Medicare spending trends, as the political coalitions and legislative pathways differ across states.

C. Robustness Appendix

Cohort-specific estimates. Estimating the effect of MAID separately by adoption cohort reveals no significant effects for any cohort. The 2016 cohort (California, Colorado, 120 counties) shows a hospice spending estimate of −\$12.4 (SE = 17.6). The 2019 cohort (Hawaii, New Jersey, Maine, 41 counties) shows −\$0.6 (SE = 28.7). The 2021 cohort (New Mexico, 32 counties) shows −\$39.0 (SE = 7.2), the largest point estimate but from a single state, making inference unreliable.

Alternative clustering. All results are robust to clustering at the state level. Wild cluster bootstrap p-values, which account for the small number of treated state clusters (7), confirm that no outcome is statistically significant at conventional levels.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Hospice spending per capita	-13.52	13.98	184.65	-0.0732	0.0757	Moderate negative
Inpatient spending per capita	44.23	34.10	479.73	0.0922	0.0711	Moderate positive
Total Medicare spending per capita	-65.07	101.71	1,804.19	-0.0361	0.0564	Small negative
ER visits per 1,000 beneficiaries	22.58	11.31	147.62	0.1529	0.0766	Large positive
Panel B: Heterogeneous (by baseline hospice spending)						
Hospice spending — high baseline	-51.85	8.72	185.80	-0.2791	0.0469	Large negative
Hospice spending — low baseline	11.99	19.54	129.88	0.0923	0.1505	Moderate positive

Notes: **Country:** United States. **Research question:** Does state-level legalization of Medical Aid in Dying shift Medicare fee-for-service spending away from acute inpatient care toward hospice and palliative care? **Policy mechanism:** MAID laws allow terminally ill adults with fewer than six months to live to request a lethal prescription from a physician, potentially normalizing end-of-life planning, increasing advance directive completion, and shifting provider norms toward palliative care for all terminal patients, not just MAID users. **Outcome definition:** CMS Medicare Fee-for-Service standardized payments per capita and utilization rates by service category (hospice, inpatient, total, emergency room), aggregated at the county-year level. **Treatment:** Binary indicator for state MAID law in effect (staggered adoption: CA and CO in 2016, DC in 2017, HI, NJ, and ME in 2019, NM in 2021). **Data:** CMS Medicare Geographic Variation Public Use File, 2014–2023, county-year panel with 30,429 observations across 3,056 counties in 47 states and DC. **Method:** Two-way fixed effects (county + year) with state-clustered standard errors; Callaway–Sant'Anna (2021) as robustness; wild cluster bootstrap for few-cluster inference. **Sample:** Counties in 47 states plus DC; four always-treated states (OR, WA, MT, VT) and territories excluded from the estimation sample. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).