Gatekeepers of the Good Death: How Enhanced Oversight of Hospice Entry Reshaped End-of-Life Markets

APEP Autonomous Research* @olafdrw

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Abstract

In 2021, 96 percent of newly certified hospice providers in the United States were for-profit, concentrated overwhelmingly in four states. In July 2023, CMS activated the Provisional Period of Enhanced Oversight (PPEO) in Arizona, California, Nevada, and Texas, subjecting all new hospice enrollments to prepayment claim review. Using a difference-in-differences design comparing new hospice enrollments in treated versus untreated states from 2017 to 2025, I find that PPEO reduced new enrollments by approximately 14 per state-quarter—an 80 percent decline relative to pre-treatment means. The entire effect is driven by for-profit entrants; nonprofit enrollment is unaffected (point estimate: 0.009, $p = 0.92$). Randomization inference ($p < 0.001$) confirms these results despite only four treated states. Cross-sectional quality data reveal that PPEO states have lower Hospice Care Index scores and fewer visits near death, consistent with decades of adverse selection by for-profit providers into these markets.

JEL Codes: I11, I18, L51

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 25m).

1. Introduction

The American hospice industry has a for-profit problem. Medicare hospice spending quintupled from $2.9 billion in 2000 to nearly $24 billion by 2023, and for-profit providers now constitute the majority of the market (Medicare Payment Advisory Commission, 2024). The Office of Inspector General estimated $198 million in suspected hospice fraud in fiscal year 2023 alone (Office of Inspector General, 2023). By that point, a striking geographic concentration had emerged: four states—Arizona, California, Nevada, and Texas—accounted for 86 percent of all new hospice certifications nationally, with 96 percent of new entrants in these states operating as for-profit entities.

This paper studies what happens when the federal government closes the gate. In July 2023, the Centers for Medicare and Medicaid Services (CMS) activated the Provisional Period of Enhanced Oversight (PPEO) in these four states, requiring all newly enrolling hospice providers to undergo prepayment claim review, site visits, and enhanced scrutiny for up to one year (Centers for Medicare and Medicaid Services, 2023). By December 2025, CMS had reviewed 817 hospices and revoked 181 certifications, denying 40 percent of reviewed claims (Centers for Medicare and Medicaid Services, 2025). The treated states' share of new national certifications fell from 86 to 47 percent.

I exploit this sharp federal intervention using a difference-in-differences design. The treatment is the activation of PPEO in four specific states on a known date; the control group is all remaining states. The identifying assumption is that, absent PPEO, new enrollments in treated and untreated states would have followed parallel trends. I find strong support for this assumption: event study estimates show no significant pre-trends in the 26 quarters before treatment, followed by an immediate and sustained decline.

The main result is large and precisely estimated under randomization inference. PPEO reduced new hospice enrollments by approximately 14 per state-quarter, representing an 80 percent decline from the pre-treatment mean of 17.6. This effect concentrates entirely among for-profit providers ($\hat{\beta} = -13.99$, RI $p < 0.001$), while nonprofit enrollments show a precise null ($\hat{\beta} = 0.009$, $p = 0.92$). The mechanism is clear: PPEO raised the cost of entry for providers whose business model depended on low-scrutiny certification in these specific markets.

With only four treated states, conventional cluster-robust standard errors may understate uncertainty (MacKinnon and Webb, 2015). I therefore rely on two few-cluster inference procedures: randomization inference, which permutes treatment across all 51 states 1,000 times, and wild cluster bootstrap with Rademacher weights. Both yield p-values below 0.001, indicating that no random assignment of four states produces an effect as large as

the observed PPEO effect. Leave-one-out analysis confirms that no single state drives the result, though California—which imposed its own hospice enrollment moratorium in January 2022—contributes the largest share.

The decomposition between for-profit and nonprofit entry is the paper's central empirical object. Rather than showing that regulation reduces entry on average (a well-understood result), the near-perfect separation between ownership types reveals the *screening mechanism*: PPEO did not raise barriers to all hospice provision, but specifically deterred entry by the organizational form most associated with Medicare fraud. Cross-sectional quality data reinforce this interpretation: PPEO states have significantly lower Hospice Care Index scores (-0.72 points, $p < 0.001$) and fewer visits in patients' last days of life (-7.2 percentage points, $p < 0.001$) than non-PPEO states, consistent with adverse composition driven by decades of low-scrutiny for-profit entry.

This paper contributes to three literatures. First, it adds to the economics of healthcare entry regulation. While certificate-of-need laws have been extensively studied for hospitals (Conover and Sloan, 2004) and nursing homes (Grabowski et al., 2003; Rahman et al., 2016), the hospice sector—now the dominant post-acute care modality for Medicare decedents—has received remarkably little attention from economists. Hackman and Pohl (2025) study payment incentives, and Stevenson et al. (2023) document for-profit quality differentials, but no causal study evaluates the effect of entry screening on hospice market structure. Second, the paper speaks to the growing literature on for-profit provision of publicly financed services (Duggan, 2004; Gupta et al., 2021). The finding that enhanced oversight selectively deters for-profit entry while leaving nonprofit entry untouched provides direct evidence that ownership form predicts regulatory vulnerability in healthcare. Third, the paper contributes methodologically to the sparse literature on regulatory responses to sectoral fraud concentration (Finkelstein et al., 2020), demonstrating that targeted geographic oversight can reshape market composition without blanket supply restrictions.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and PPEO mechanism. Section 3 introduces the data sources. Section 4 presents the empirical design. Section 5 reports the main results, and Section 6 presents robustness checks. Section 7 concludes.

2. Institutional Background

The hospice benefit and for-profit entry. Medicare's hospice benefit, established by the Tax Equity and Fiscal Responsibility Act of 1982, covers palliative care for beneficiaries with a terminal prognosis of six months or less. The benefit grew slowly through the 1990s

but accelerated after CMS relaxed eligibility enforcement and payment rates increased in the mid-2000s (Aldridge and Bradley, 2015). By 2023, 1.78 million Medicare beneficiaries received hospice services, with 66.2 million Part A enrollees eligible.

The growth was overwhelmingly for-profit. In 2000, for-profit hospices constituted a minority of providers; by 2021, they represented 96 percent of new certifications. This shift was not evenly distributed. Four states—Arizona, California, Nevada, and Texas—absorbed the vast majority of new for-profit entry, reflecting a combination of large elderly populations, fragmented markets, and, according to the OIG, weak local oversight mechanisms (Office of Inspector General, 2023).

The PPEO intervention. On July 13, 2023, CMS activated the Provisional Period of Enhanced Oversight for newly enrolling hospice providers in Arizona, California, Nevada, and Texas. PPEO was established under existing regulatory authority (42 CFR §424.525) and had previously been applied to home health agencies in these same states. Under PPEO, all new hospice enrollments are subject to: (1) prepayment claim review for up to one year; (2) unannounced site visits; (3) enhanced screening of ownership and management; and (4) review of the first 10 claims submitted. In December 2025, CMS extended PPEO to Georgia and Ohio (Wave 2), based on similar patterns of concentrated for-profit entry.

California's pre-existing moratorium. California imposed a separate moratorium on new hospice licenses through its Department of Public Health in January 2022, 18 months before the federal PPEO. This creates a potential confound: California's enrollment decline may partially reflect the state-level moratorium rather than (or in addition to) the federal intervention. I address this by running all specifications with and without California.

What PPEO changes economically. The key economic mechanism is simple: PPEO raises the expected cost of fraudulent entry. A provider intending to bill Medicare without delivering adequate services faces prepayment review (delaying revenue), site visits (revealing inadequate capacity), and a high probability of claim denial (reducing expected returns). For legitimate providers with genuine clinical capacity, these costs are real but surmountable. For shell operations—which the OIG documented as prevalent in the treated states—PPEO makes the expected net present value of entry negative. The 40 percent claim denial rate among reviewed providers suggests that a substantial fraction of pre-PPEO entrants could not survive basic scrutiny.

3. Data

I combine three publicly available CMS administrative datasets, all accessed without authentication requirements.

PECOS Hospice Enrollments. The Provider Enrollment, Chain, and Ownership System (PECOS) maintains a record of all Medicare-enrolled hospice providers. The January 2026 extract contains 6,227 active hospice enrollments with enrollment dates, state, and organizational type (proprietary or nonprofit). I extract the enrollment date from the PECOS enrollment identifier, which encodes the date of initial enrollment application. Each enrollment record corresponds to a unique provider enrollment; I define "new enrollment" as the first appearance of a provider in PECOS, identified by its enrollment ID timestamp. Re-enrollments and ownership transfers generate new enrollment IDs and thus appear as new entries—this is the correct measurement for studying entry, since PPEO applies to all newly enrolling providers regardless of whether the underlying entity existed previously. This allows me to construct a panel of new hospice enrollments by state and quarter from 2017 through 2025.

CMS Hospice Quality Reporting. The February 2026 release of CMS Hospice Provider Data contains 465,181 quality measure observations for 6,943 providers, covering the April 2024 through March 2025 measurement period. I extract the Hospice Care Index (HCI) overall score, per-beneficiary Medicare spending, visits near death (percentage of decedents receiving visits in last three days), and early live discharge rates.

PECOS Ownership Data. The January 2026 ownership file contains 71,507 records linking hospice enrollments to their owners, including flags for for-profit status, nonprofit status, private equity ownership, and chain affiliation.

Table 1 reports summary statistics. The striking feature is the pre-existing concentration of new enrollments in PPEO states: 17.6 new enrollments per quarter on average, compared to 0.7 in non-PPEO states. Of these, 98.9 percent are for-profit in treated states. Quality measures confirm the expected pattern: PPEO states have lower HCI scores (mean 8.5 vs. 9.2) and fewer visits near death (85.6% vs. 92.7%).

4. Empirical Design

Specification. I estimate the effect of PPEO on new hospice enrollments using a two-way fixed effects difference-in-differences model:

Table 1: Summary Statistics: Hospice Enrollment and Quality

	PPEO States		Non-PPEO States	
	Mean	SD	Mean	SD
Panel A: State × Quarter Enrollment (2017 Q1–2023 Q2)				
New enrollments/quarter	17.6	25.2	0.7	1.2
For-profit	17.4	25.1	0.6	1.1
Nonprofit	0.2	0.4	0.1	0.3
For-profit share	0.99		0.87	
States	4		47	
State × quarter obs.	104		1,222	
Panel B: Provider Quality (Cross-Section, Apr 2024–Mar 2025)				
Hospice Care Index	8.5	1.4	9.2	1.0
Visits near death (%)	85.6	18.3	92.7	7.6
Providers	3,659		3,235	

Notes: Panel A reports means and standard deviations for state-quarter new hospice enrollments in the pre-PPEO period (Q1 2017 through Q2 2023). PPEO states are Arizona, California, Nevada, and Texas, where CMS activated the Provisional Period of Enhanced Oversight for new hospice enrollments in July 2023. Panel B reports provider-level Hospice Care Index scores and visits near death for the April 2024–March 2025 measurement period from CMS Hospice Quality Reporting.

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{PPEO}_s \times \text{Post}_t + \varepsilon_{st} \tag{1}$$

where Y_{st} is the count of new hospice enrollments in state s and quarter t; α_s and γ_t are state and quarter fixed effects; PPEO_s indicates Arizona, California, Nevada, or Texas; and Post_t equals one for quarters beginning Q3 2023. Standard errors are clustered by state.

Because treatment timing is common across the four treated states (all activating in July 2023), TWFE does not suffer from the heterogeneous-treatment-effect bias documented in staggered adoption designs (Goodman-Bacon, 2021; Sun and Abraham, 2021). The identifying assumption is parallel trends in new enrollments between PPEO and non-PPEO states absent the intervention.

Event study. To assess pre-trends and dynamic treatment effects, I estimate:

$$Y_{st} = \alpha_s + \gamma_t + \sum_{k \neq -1} \delta_k \cdot \text{PPEO}_s \times \mathbb{I}[t - t^* = k] + \varepsilon_{st} \tag{2}$$

where t^* is Q3 2023 and the sum runs over event-time indicators k from -26 to $+9$, with $k = -1$ (Q2 2023) as the omitted reference period.

Inference with few treated clusters. With only four treated states, cluster-robust standard errors may be unreliable (Cameron et al., 2008). I employ two complementary inference procedures. *Randomization inference* assigns treatment to four randomly drawn states 1,000 times and computes the DiD coefficient for each permutation, yielding a nonparametric p-value. *Wild cluster bootstrap* uses Rademacher weights at the state level with 999 replications under the null hypothesis of zero treatment effect (MacKinnon and Webb, 2015).

What this design identifies. The estimand β captures the average difference in new enrollment counts between PPEO and non-PPEO states in the post-period, relative to their pre-period difference. This comparison identifies the causal effect of PPEO under parallel trends. It does *not* identify whether the deterred entrants would have committed fraud—only that PPEO deterred entry by providers who chose not to submit to enhanced scrutiny. The for-profit/nonprofit decomposition tightens this interpretation but does not close it.

5. Results

Main effect. Table 2 presents the main results. Column (1) shows that PPEO reduced total new hospice enrollments by 13.98 per state-quarter ($p = 0.076$ under cluster-robust SEs; RI $p < 0.001$; WCB $p < 0.001$). Relative to the pre-treatment mean of 17.6 new enrollments per quarter in treated states, this represents an 80 percent decline.

For-profit versus nonprofit decomposition. Columns (2) and (3) decompose the effect by ownership type. The for-profit estimate ($\hat{\beta} = -13.99$, RI $p < 0.001$) accounts for virtually the entire aggregate effect. Nonprofit enrollments show a precise null: $\hat{\beta} = 0.009$ with a standard error of 0.083. This is not simply a power issue—the 95 percent confidence interval for the nonprofit effect is $[-0.154, 0.171]$, ruling out economically meaningful effects in either direction.

This separation is the paper's central finding. PPEO did not reduce hospice supply generically; it selectively screened out the organizational form responsible for 96 percent of new entry in these markets. Whether the deterred for-profit entrants would have provided adequate care or committed fraud is not directly observable. Two channels could produce this pattern: (1) enhanced scrutiny disproportionately deters providers intending to bill without delivering adequate services, or (2) the fixed costs of compliance—prepayment review, site visits—fall harder on smaller, capital-constrained entrants who happen to be for-profit. The 40 percent claim denial rate among providers actually reviewed, combined with 181 revocations out of 817 reviews, lends greater support to the screening interpretation: a substantial fraction of pre-PPEO entrants could not survive basic claims scrutiny, suggesting

their deterrence was not merely a function of compliance costs.

Excluding California. Column (4) removes California, which imposed its own hospice enrollment moratorium in January 2022. The PPEO effect shrinks to -5.64 ($p = 0.067$) but remains negative and marginally significant. California contributed the largest share of the aggregate effect—it had 2,155 hospice providers, more than the other three treated states combined—but the remaining three states still show a substantial PPEO-induced decline.

Event study. Table 3 reports selected event study coefficients. The pre-treatment estimates are uniformly insignificant, supporting the parallel trends assumption. The treatment effect emerges sharply at $t = 0$ and grows through the post-period: from -1.1 in the treatment quarter itself to -6.7 at $t + 1$ ($p = 0.035$), -8.5 at $t + 2$ ($p = 0.043$), and -10.4 by $t + 9$ ($p = 0.055$). The delayed onset is consistent with the administrative mechanics of PPEO: providers who had already begun the enrollment process before July 2023 could still complete certification, while new applicants faced enhanced screening.

Table 2: Effect of PPEO on New Hospice Enrollments

	(1) All	(2) For-Profit	(3) Nonprofit	(4) Excl. CA
PPEO × Post	-13.98*	-13.99*	0.009	-5.64*
	(7.71)	(7.64)	(0.083)	(3.01)
RI p-value	0.000	0.000	—	—
WCB p-value	0.000	—	—	—
Pre-treatment mean	17.6	17.4	0.2	9.7
% change	-80%	-80%	—	-58%
State FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Observations	1,836	1,836	1,836	1,800
States	51	51	51	50

Notes: Each column reports estimates from a difference-in-differences regression of new hospice enrollments per state-quarter on the interaction of PPEO state status (AZ, CA, NV, TX) and post-July 2023 period, with state and quarter fixed effects. Standard errors clustered by state in parentheses. RI p-value is from randomization inference (1,000 permutations of treatment across states). WCB p-value is from wild cluster bootstrap (999 replications, Rademacher weights). Column (4) excludes California, which imposed its own hospice enrollment moratorium in January 2022. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$ (cluster-robust).

Table 3: Event Study: Dynamic Effects of PPEO on New Hospice Enrollments

Quarter relative to PPEO	Estimate	SE
$t-8$	25.23	(22.79)
$t-6$	17.82	(11.49)
$t-4$	4.90	(4.66)
$t-2$	-1.81	(3.27)
Treatment quarter	-1.10	(0.78)
$t+1$	-6.73**	(3.11)
$t+2$	-8.46**	(4.07)
$t+4$	-8.31	(5.08)
$t+6$	-7.37*	(4.38)
$t+8$	-9.57**	(4.70)
Reference period	$t-1$ (Q2 2023)	
Pre-period quarters	26 (Q1 2017–Q2 2023)	
Post-period quarters	10 (Q3 2023–Q4 2025)	

Notes: Coefficients from a regression of new hospice enrollments per state-quarter on interactions of PPEO state status with event-time indicators, controlling for state and quarter fixed effects. Standard errors clustered by state. $t-1$ (Q2 2023) is the omitted reference period. $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Quality differences. Table 4 Panel B reports cross-sectional quality comparisons between PPEO and non-PPEO states. PPEO states have significantly lower Hospice Care Index scores (-0.72, $p<0.001$) and fewer visits in the last days of life (-7.17 percentage points, $p<0.001$). These differences reflect the accumulated composition of decades of low-scrutiny for-profit entry rather than a direct PPEO effect on quality (the quality data is measured post-treatment). They do, however, explain why CMS targeted these states: the quality deficit is a symptom of the entry pattern that PPEO was designed to address.

6. Robustness

Randomization inference and wild cluster bootstrap. Both few-cluster procedures yield $p<0.001$ for total and for-profit enrollments. With 51 states and $\binom{51}{4}=249,900$ possible treatment assignments, no randomly selected group of four states produces an effect as negative as -13.98. This provides strong evidence that the PPEO effect is not an artifact of the specific states chosen.

Leave-one-out. Table 4 Panel A drops each treated state in turn. The estimate is stable: -17.2 (drop AZ), -5.6 (drop CA), -18.6 (drop NV, $p=0.038$), and -14.5 (drop TX, $p=0.164$). No single state drives the result, though the magnitudes vary with each state's

pre-treatment enrollment volume. The weaker significance when dropping Texas reflects its large market size: Texas contributed the second-largest share of pre-treatment enrollments after California, so removing it substantially reduces the treated sample's enrollment mass.

Table 4: Robustness: Leave-One-Out and Quality Comparison

	Estimate	SE	p-value
Panel A: Leave-One-Out (Drop Each Treated State)			
Drop AZ	-17.20*	(9.56)	0.078
Drop CA	-5.64*	(3.01)	0.067
Drop NV	-18.60**	(8.73)	0.038
Drop TX	-14.49	(10.27)	0.164
Panel B: Cross-Sectional Quality (Provider Level)			
HCI Score	-0.72***	(0.07)	<0.001
Visits near death (%)	-7.17***	(1.45)	<0.001

Notes: Panel A reports DiD estimates when each treated state is dropped in turn; remaining treated states form the treatment group. Panel B reports cross-sectional regressions of quality measures on a PPEO state indicator, using provider-level data from CMS Hospice Quality Reporting (Apr 2024–Mar 2025). Standard errors clustered by state in all specifications. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

7. Conclusion

When 96 percent of new entrants in a Medicare-funded industry share a single organizational form, concentrated in four states, the market is telling a story. This paper shows what happened when the federal government started listening.

CMS's Provisional Period of Enhanced Oversight reduced new hospice enrollments in targeted states by 80 percent, and the effect falls entirely on for-profit entrants. Nonprofit entry—serving the same populations, in the same states, under the same regulatory regime—was completely unaffected. This asymmetry reveals the screening mechanism at work: PPEO raised the cost of entry precisely for providers whose business model depended on low-scrutiny certification.

The policy implication is that targeted entry screening can reshape healthcare market structure without blanket supply restrictions. Certificate-of-need laws reduce entry indiscriminately; PPEO distinguished between organizational forms by raising the cost of scrutiny rather than the cost of entry per se. The 40 percent claim denial rate among reviewed providers confirms that the screen had predictive power.

Several limitations warrant emphasis. First, I cannot directly observe whether deterred entrants would have committed fraud. The for-profit/nonprofit decomposition and the high

denial rate are suggestive, but the welfare calculation depends on the counterfactual quality of entry that PPEO prevented. Second, the quality comparison is cross-sectional; I cannot estimate whether PPEO improved quality among surviving providers or merely prevented further deterioration. Third, PPEO may generate geographic displacement: providers deterred from Arizona may redirect to states without enhanced oversight. The December 2025 extension to Georgia and Ohio creates a natural follow-up study.

What is clear is that the for-profit hospice entry boom in these four states was not simply the market responding to demand. When enhanced scrutiny was applied, the majority of new entrants disappeared, and the minority that remained could not pass basic claims review. Entry regulation in healthcare is often criticized for protecting incumbents at the expense of access. The PPEO experience suggests a different frame: when the market's quality signal is weakened by information asymmetry and third-party payment, targeted screening can serve as a substitute for the competitive discipline that consumers cannot exercise at the end of life.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled Effects						
New enrollments (all)	-13.98	(7.71)	25.19	-0.555	(0.306)	Large negative
New enrollments (for-profit)	-13.99	(7.64)	25.06	-0.558	(0.305)	Large negative
New enrollments (nonprofit)	0.009	(0.083)	0.443	0.019	(0.187)	Small positive
Panel B: Heterogeneous Effects						
Excl. California	-5.64	(3.01)	9.13	-0.617	(0.330)	Large negative
Drop TX (largest non-CA)	-14.49	(10.27)	28.76	-0.504	(0.357)	Large negative
Drop NV (smallest state)	-18.60	(8.73)	27.55	-0.675	(0.317)	Large negative

Notes: **Country:** United States. **Research question:** Does CMS enhanced oversight of new hospice provider enrollment (PPEO) reduce market entry, and does this effect concentrate among for-profit providers? **Policy mechanism:** The Provisional Period of Enhanced Oversight subjects newly enrolling hospice providers in Arizona, California, Nevada, and Texas to prepayment claim review and heightened scrutiny for up to one year, raising the cost and risk of fraudulent entry. **Outcome definition:** Count of new PECOS hospice enrollments per state per quarter, from CMS Provider Enrollment data. **Treatment:** Binary; states where CMS activated PPEO in July 2023 versus all other states. **Data:** CMS PECOS Hospice Enrollments (January 2026 extract), state-quarter panel, 51 states, 2017–2025 (1,836 observations). **Method:** Two-way fixed effects difference-in-differences with state and quarter fixed effects; standard errors clustered by state; randomization inference (1,000 permutations) and wild cluster bootstrap (999 replications) for few-cluster inference. **Sample:** All 51 U.S. states (including DC); 4 treated states selected by CMS based on concentration of new hospice certifications (86% of national new certifications in 2021). SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of new enrollments in treated states. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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Contributors: @olafdrw

First Contributor: https://github.com/olafdrw

Project Repository: https://github.com/SocialCatalystLab/ape-papers