

Supplemental Appendix

Overlapping Policy Interventions: Evidence from Home Health

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A The cost of strike forces

As a collaborative effort between multiple branches of the US government, the Medicare Fraud Strike Force is quite expensive to maintain, but there is no readily available report on costs of the Strike forces. To estimate strike forces' annual spending on home health, we use the annual reports from 1997 through 2020 published by the US Department of Health and Human Services (HHS) and Department of Justice (DOJ) that report updates on the "Health Care Fraud and Abuse Control Program" (HCFAC). This program includes all of the HHS and DOJ's joint efforts to combat health care fraud, including the Strike Forces (HHS & DOJ 2010). Each report lists spending at a national level, as well as descriptions of each initiative under HCFAC. The spending is itemized under divisions of each department, but there is no finer data on how much was allocated to each specific initiative.

Appendix Table OA.18 provides an example of how funding to HCFAC is reported each year. We define Strike-Force spending to be the sum of total spending under the following categories: HHS Office of Inspector General, HHS Centers for Medicare and Medicaid Services, DOJ United States Attorneys, DOJ Criminal Division, and DOJ Federal Bureau of Investigation (FBI).³²

Appendix Figure OA.11a displays the amount of strike force spending in each year between 1997 and 2020 and also notes the years in which each home health strike force began (see Table OA.1). Appendix Figure OA.11b plots the first-differences of this annual spending. With the exception of 2014-2015, strike force spending grows very slowly over time in years without new strike force openings. We therefore estimate the annual spending for a new strike force office (or a group of offices when several come in in the same year) from the change in spending in the year it opens. For example, the difference between relevant spending in 2007 and relevant spending in 2006 would yield the approximate annual spending in operating the Strike Force office in Southern Florida, which opened in 2007. Likewise, the difference in relevant spending between 2008 and 2009 would indicate the approximate annual cost of operating strike forces in the Eastern District of Michigan, the Southern District of Texas, the Eastern District of New York, the Middle District of Louisiana and the Middle District of Florida, all of which opened in 2009. On average, we estimate that spending was about \$350 million per year across all 12 strike force offices, or about \$29 million per year per operating office.

Note, however, that the strike forces combat more than home health fraud; they also investigate and prosecute cases of fraud in Durable Medical Equipment, IV infusion therapy, and other areas. To approximate the share of a strike force annual spending that is devoted to combating home

³²We selected this category based on the following statement from the 2009 report: "Each Medicare Strike Force combines data analysis capabilities of CMS and the investigative resources of the FBI and the HHS/OIG with the prosecutorial resources of the DOJ Criminal Division, Fraud Section and the U.S. Attorneys' Offices." Note that the FBI is not always listed as a category in the HCFAC reports (for example, it is not listed in Appendix Table OA.18). We interpret this to mean that the FBI received zero funding in these years.

health fraud only, we utilize the DOJ’s Justice News website, which contains a database of press releases related to DOJ prosecutions. Specifically, for every year between 2009 and 2019, we search the website for the keywords “home health HEAT” and “HEAT”, which stands for Health Care Fraud Prevention and Enforcement Action Team. In total there were 1,025 press releases about HEAT and 364 of them (35.5%) mentioned home health. We therefore assume that roughly 35.5% of a strike force’s spending, or about \$10 million per year per strike force is dedicated to home health.

B Robustness

We explored the robustness of the estimated impacts of individual policies in Figure 5 and Appendix Table OA.4 on a number of dimensions.

Matching Instead of the de-trending approach we use in the baseline, we implement a standard, nearest neighbor matching approach for each outcome (as in e.g. Hjort et al. 2017; Fenizia and Saggio 2024) as an alternative way to control for pre-trends. For this analysis, we continue to define the set of treatment counties as those that received either or both Strike Force or Moratorium at any point during the study period, and control counties as those that have didn’t received either policy during the study period. We further divide the treated counties into groups based on the year they received their first policy; this results in seven groups: $g \in \{2009, 2010, 2011, 2013, 2014, 2016, 2018\}$.

We match each treated county in a given group g with the nearest control county based on the growth in outcome from year $g-5$ to $g-1$; thus the control for a given county can vary depending on the outcome analyzed. We select the nearest control based on the growth in the outcome variable, from the full set of controls with replacement. This allows us to produce a county-year dataset with the treatment counties and the control counties to which they are matched to; because controls are drawn with replacement, not all control counties will be included in the final matched sample, and some control counties will be duplicated.

We then estimate the following regression on the matched sample:

$$y_{ct} = \alpha_c + \gamma_{g(c) \times t} + \sum_{r^{SF} \neq 0} \beta_{r^{SF}} SF_{cr^{SF}} + \sum_{r^M \neq 0} \theta_{r^M} M_{cr^M} + \epsilon_{ct} \quad (8)$$

where α_c is a county fixed effect and $g(c)$ represents the group that the treatment-control match pair belongs to. Thus, $\gamma_{g(c) \times t}$ represents fixed effects for the full set of combinations of calendar year t and group $g(c)$. $SF_{cr^{SF}}$ are indicators that the Strike Force is in effect in county c in relative year r . M_{cr^M} are the analogous indicators for the moratoria. The regression is weighted by the number of Medicare patients aged 65+ in 2006. Standard errors are clustered at the district level.

Appendix Table OA.9 reports the results. For comparison, Panel A replicates the baseline results from Appendix Table OA.4. The corresponding event studies are shown in Appendix Figure OA.12. The first three rows of Panel B report the baseline matching estimates, which are similar to, although slightly smaller, than the baseline results using the detrending approach. Looking at the middle column (1–4+), introducing a strike force reduces home health visits by about 11.8% (standard error = 4.6%) and payments by about 7.1% (standard error = 3.6%), with essentially no effect on the share of patients using home health (point estimate close to zero). Moratoria show slightly larger impacts, reducing home health visits by about 14.0% (standard error = 4.6%),

payments by about 16.9% (standard error = 4.1%), and utilization by about 10.4% (standard error = 2.5%).

In subsequent rows of Panel B, we further probe robustness to how the matched sample is defined. Specifically, restricting matches to controls with at least 150 patients, restricting to matches with where both treatment and control counties have at least 150 patients, and dropping matches that have a $> 5\%$ difference between treatment and controls in based on the matching variable.³³ Results are quite similar across these alternative matching procedures.

Poisson Regression Appendix Table OA.10 Panel B shows robustness to estimating a Poisson regression (so that, unlike in our baseline specification in Panel A the counties that ever have a year with zero utilization are now included - recall footnote 14). Focusing on the middle column (1-4+), we see that on average, introducing a strike force into the county reduces home health visits by about 12.3% (standard error = 3.5%) and home health payments by about 10.6% (standard error = 2.5%), but a statistically insignificant reduction on the share of patients using home health services (point estimate of 3.1%). The impact of imposing a moratorium in the county is comparable; we estimate that on average the moratoria reduce home health visits by about 12.3% (standard error = 5.8%), home health spending by about 21.4% (standard error = 6.8%), and the share of patients using home health by about 14.9% (standard error = 6.2%). These results confirm the baseline findings: both policies significantly reduce home health utilization.

Callaway and Sant’Anna Estimator Appendix Table OA.10 Panel C reports estimates using the average treatment effect estimator of Callaway and Sant’Anna (2021), which accommodates staggered adoption and heterogeneous treatment effects across groups and over time.³⁴ Focusing on the middle column (1-4+), we find that introducing a strike force reduces home health visits by about 18.5% (standard error = 5.6%) and payments by about 15.0% (standard error = 4.9%), with a smaller and statistically weaker reduction in the share of patients using home health of about 5.6% (standard error = 3.1%). For moratoria, the effects are larger: we estimate that on average they reduce home health visits by about 20.6% (standard error = 7.6%), payments by about 24.0% (standard error = 6.4%), and the share of patients using home health by about 15.1% (standard error = 4.1%). These results again align with the baseline, indicating that both policies substantially reduce home health utilization.

Jackknife robustness To further probe whether our estimates are driven by a particular district, we conduct a series of jackknife exercises, re-estimating the baseline specification while sequentially excluding each district from the sample. Tables OA.11 and OA.12 report the results. The first column of each table reproduces the baseline “all districts” estimate for comparison.

³³This involves dropping about 4% of enrollee-weighted counties when analyzing home health payments, 3% for the ‘share used home health’, and 0.3% for home health visits.

³⁴Since the Callaway and Sant’Anna (2021) estimator is based on analyzing only one policy, it assigns units to treatment cohorts based on the year they are exposed to this single policy. However, our setting involves two distinct policy interventions. Therefore, we estimate two separate regressions using the Callaway and Sant’Anna (2021) framework, each designed to estimate the impact of one policy while controlling for the other. Thus when we estimate the impact of the Strike Force we assign units to treatment cohorts based on the year that they were assigned to the Strike Force, and control for whether the Moratorium is in effect in each county-year in the data, and vice versa for when we estimate the Moratoria.

Across both tables, the pattern of results remains consistent: both policies led to reductions in visits, payments, and utilization, regardless of which district is excluded. The magnitudes vary somewhat—for example, excluding Eastern Michigan or Southern Texas yields slightly larger effects on payments—but the overall order of magnitude and statistical significance remain stable. This indicates that our findings are not being driven by any single district.

C Examining potential substitution to nursing home care

One of the goals of Medicare coverage of home health is to obviate the need for enrollees to use more expensive (and presumably less desirable) Medicare services such as nursing home care (MedPAC 2020). Medicare covers short-term nursing home stays for patients recovering from a surgical procedure or acute health event (such as a stroke); these nursing homes are called skilled nursing facilities (SNFs). Importantly, not all patients eligible for Medicare-covered home care would in fact be eligible for Medicare-covered SNF care. In order to be eligible for Medicare coverage, a SNF stay must start within 30 days of the patient being discharged from the hospital after a hospitalization that lasted for at least three days (CMS 2022b). Medicare-covered home care, however, does not require a recent prior hospital stay. To approximate this in the data, we define an enrollee-year as “SNF eligible” if they had at least one inpatient hospital stay during the year, and “SNF ineligible” otherwise; we define a SNF stay or a home health episode as SNF-eligible if the stay or episode begins within 30 days of a qualifying inpatient stay.

Appendix Table OA.13 presents descriptive statistics on the relationship between SNF and home health use in 2006. Panel A column (1) shows that 6% of Medicare enrollees used a skilled nursing facility (SNF) in 2006, compared to 10% who used home health. Columns (2) and (3) look separately at enrollees we were and were not SNF-eligible in 2006. About 17 percent of enrollees were SNF-eligible. Our classification appears to be reasonable; about 34% of SNF-eligible enrollees used a SNF in 2006, while only about 1% of enrollees we classify as non SNF eligible did so.

Panel B shows average enrollee home health and SNF use, overall and broken down by use that is and is not SNF eligible. Once again, the results suggest our classification is reasonable: essentially all Medicare-covered SNF use is by SNF-eligible patients.³⁵ Strikingly, it also indicates that only 52 percent of patients who use Medicare-financed home health (accounting for 38 percent of Medicare home health payments) would be eligible for Medicare-financed SNF stays.³⁶

When we re-estimate equation (4) separately for home health use that is and is not SNF-eligible, we find much larger (proportional) impacts of the policies on reducing non-SNF eligible home health care use than SNF-eligible home health care use, which suggests substitution to SNF care may be limited. Appendix Table OA.14 summarizes the estimates.³⁷

³⁵Specifically, among SNF-eligible enrollees, average SNF spending per enrollee is \$3,721 while among SNF ineligible enrollees it is \$68. Accounting for the fact that only 17 percent of enrollees are SNF-eligible, this suggests that 94 percent of Medicare SNF payments are to enrollees whom we have labeled as eligible for SNF Medicare reimbursement.

³⁶29 percent of SNF-eligible patients use home health, compared to only 7 percent of SNF-ineligible patients. Accounting for the fact that 17 percent of patients are SNF-eligible, we get that 52 percent of home health patients are SNF eligible. SNF-eligible patients also spend on average \$887 on home health, compared to \$336 on home health for SNF-ineligible patients; accounting for the fact that 17 percent of patients are SNF-eligible, this implies that 38 percent of home health payments are to SNF-eligible enrollees.

³⁷Appendix Figures OA.4 and OA.5 report the underlying event studies from estimating equation (1) for SNF-eligible and SNF-ineligible home care, respectively. Appendix Figures OA.6 and OA.7 report the underlying event studies from estimating equation (4), again for SNF-eligible and SNF-ineligible home care respectively. For these

To examine policy impacts on SNF use directly, we re-estimate equation (4) with SNF utilization measures (instead of home-health utilization measures) as the outcomes.³⁸ Appendix Table OA.16 summarizes the estimates.³⁹ Puzzlingly, the results suggest opposite signed effects of the strike force and the moratoria on SNF use. For example, column 2 suggests that, on average, there was a statistically significant 7% increase in SNF payments among SNF-eligible patients from the strike force, and a statistically significant 11% decrease in SNF payments among SNF-eligible patients from the moratoria. Overall we view these results as inconclusive.

D Details on the causal forest method

Causal forests are a method for uncovering heterogeneity in causal effects. The method mirrors the random forest methodology: while random forests split data on covariates to minimize prediction error while guarding against over-fitting, causal forests likewise split data on covariates to maximize the difference in treatment effects across splits while guarding against over-fitting. Despite the name, causal forests themselves do not product causal effects: causality is still established by research design, whether it is through a randomized controlled trial, or a quasi-experimental design as in our setting. This section describes the steps we take to implement a causal forest in our setting.

To estimate the $\tau_i(a)$'s across all patients and policy combinations, we partition the data into 82 “causal forest treatment groups.” These groups are finer than the 25 theoretical policy combinations described in Section 5 and shown in Appendix Table OA.5. The 25 combinations arise from crossing five possible durations of a strike force ($\emptyset, 1, 2, 3, 4+$) with five possible durations of a moratorium ($\emptyset, 1, 2, 3, 4+$). In the data, however, these combinations appear in different *calendar years* and for different sets of counties depending on when the policy was introduced. For example, a county in its second year of a strike force in 2011 and a county in its second year of a strike force in 2014 both belong to the same policy combination (“strike force at year 2, no moratorium”), but they are treated as distinct causal forest groups because they occur in different calendar years and with different contemporaneous controls. Accounting for different calendar years yields 82 treatment groups in total (listed in Appendix Table OA.17). We then estimate heterogeneous treatment effects for each of these 82 groups—relative to the corresponding calendar year in the no-policy

analyses, following the approach for the main analysis (see footnote 14) we remove from the sample the counties that have zero utilization of a particular type (SNF-eligible home care or SNF-ineligible home care) in any study year; this means removing 0.8% of population-weighted counties when analyzing SNF-eligible home care and 0.5% of population-weighted counties when analyzing SNF-ineligible home care. For example, looking 4 plus years after implementation, we estimate that the strike force reduced SNF-ineligible home health payments (which are about 62 percent of total home health payments) by 17.7 log points (standard error = 10.5) compared to 7.4 log points (standard error = 2.8) among SNF-eligible home health payments; the corresponding numbers for the moratoria are a reduction in SNF-ineligible home health payments of 47.4 log points (standard error = 13.9) compared to a reduction in SNF-eligible home health payments of 11.1 log points (standard error = 3.9). In other words, only about 22% of the decline in home health use was SNF-eligible, which limits scope for substitution.

³⁸ Again for these analyses, following the approach for the main analysis (see footnote 14) we remove from the sample the 0.5% of population-weighted counties that have zero SNF utilization in any study year.

³⁹ Appendix Figures OA.8 and OA.9 report the underlying event studies from estimating equation (1) and equation (4), respectively; the outcome is a measure of SNF use. In this case, the results from estimating the standard event study in equation (1) and the de-trended event study in equation (4) are quite similar, as the policies were placed in areas with growing rates of home health use which was mostly uncorrelated with trends in SNF use. Indeed, Appendix Table OA.15 indicates that strike force use is very similar across counties that do and do not receive each policy.

control group—and then combine the estimates to estimate policy combination interactions in equation (7).

Specifically, in Section 4 we estimated event studies on the detrended and residualized county-year log outcomes $\widehat{\ln(y_{ct})} = \ln(y_{ct}) - \widehat{\ln(y_{ct})}$, where $\widehat{\ln(y_{ct})}$ was predicted based on the estimates from equation (3). For our analysis of heterogeneous treatment effects we construct an analogous de-trended and residualized patient-year level outcome:

$$\widetilde{y}_{it} = y_{it} / \exp(\widehat{\ln(y_{ct})}). \quad (9)$$

We then apply the causal forest framework of [Athey et al. \(2019\)](#) to these patient-level outcomes and estimate heterogeneous treatment effects separately for each of the 82 treatment groups relative to control.

We implement the causal forest using the `grf` package in R.⁴⁰ For each of the 82 treatment groups, we append the control group, which includes patients from the same calendar year as the treatment group but in counties that are never treated by either the strike force or the moratorium. The input data include the detrended outcomes, the treatment indicator, and five patient characteristics along which it generates heterogeneous treatment effects: age, sex, whether also enrolled in Medicaid, prior year’s Medicare spending, and the number of comorbidities the patient has that year. These covariates were chosen because they capture important heterogeneity, and also because there is good overlapping support between treatment and control. In other words, when splitting the sample based on combinations of these covariates, we can ensure that both treatment and control observations are represented in the subgroups, allowing comparison between treatment and control.

Specifically, we use the following algorithm:

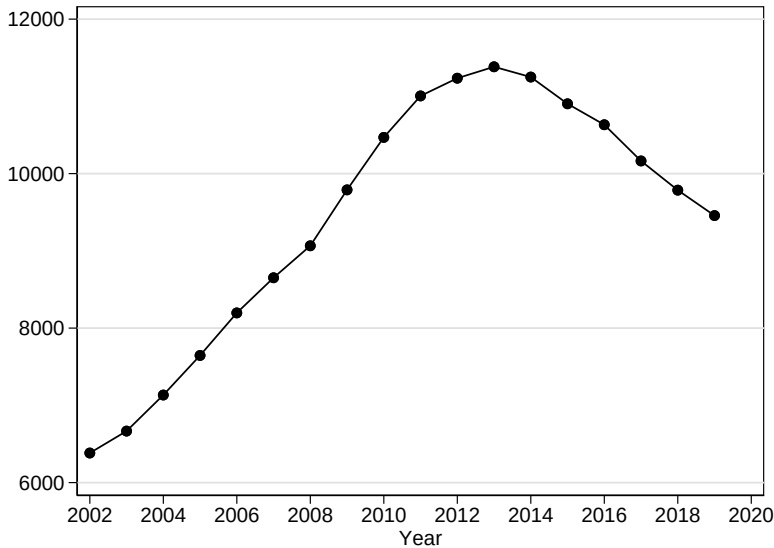
1. We randomly draw a subset of the control observations to use in the algorithm such that the size of the control group equals that of the treatment group. Equalizing the sizes of treatment and control improves computational efficiency.
2. We grow 250 trees. For each tree, we randomly separate the data into two halves: we grow the tree on the first half and estimate treatment effects using the second half. This follows the “honest causal forest” proposed by [Athey et al. \(2019\)](#): the same observation is not used to both search for heterogeneity and estimate it.
3. To grow each tree, the algorithm splits each node recursively. That is, it starts with the 50% sample used to grow the tree and tries up to $\sqrt{p} + 20$ covariates to split the sample at random, where p is the total number of covariates provided;⁴¹ for each covariate, it chooses the value which maximizes treatment effect heterogeneity across the resulting leaves. The covariate and value that results in the most heterogeneity is chosen for the split.
4. Each leaf continues to be split in this manner until each leaf cannot be split without creating a new set of leaves with fewer observations than our chosen “minimum node size”, which we define as 5% of the sample.
5. The same splits are then applied to the 50% holdout set, and treatment effects are calculated on each leaf.

⁴⁰For computational speed, we estimate the causal forest on a random 5% sample of our baseline sample.

⁴¹Since we only have five covariates, the algorithm searches over all covariates in our case.

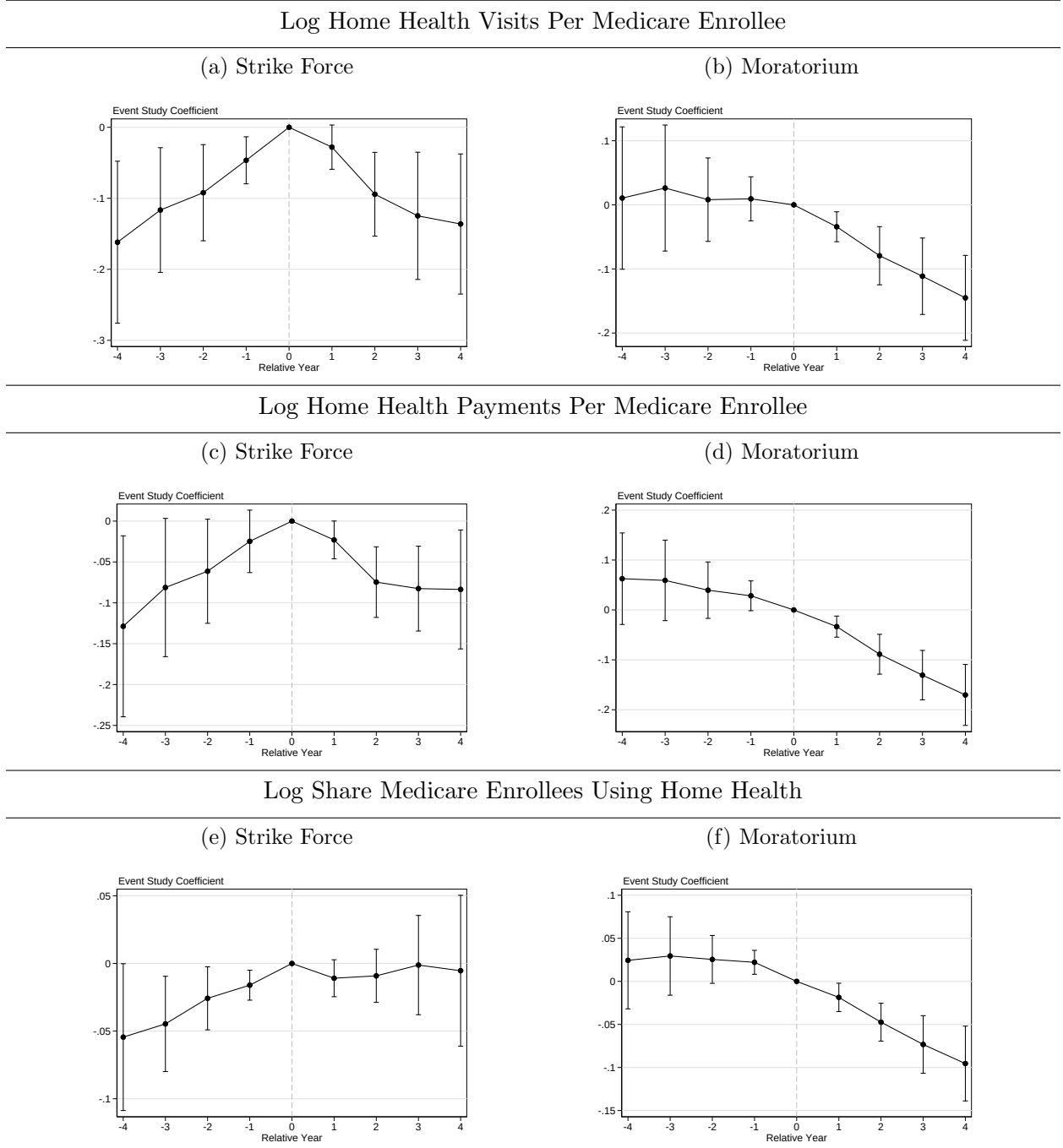
6. We repeat this process and take the average treatment effect for each individual across the 250 trees we have grown.
7. We apply the causal forest estimates to our full sample of observations in all 82 treatment groups and the control group, to generate counterfactual treatment effect predictions. The model is applied to out-of-sample individuals by matching observations with similar observable characteristics, similar to a nearest-neighbor matching.
8. Standard errors for each treatment effect are computed using the “noisy bootstrap” procedure proposed by [Sexton and Laake \(2009\)](#).

Figure OA.1: Home Health Agency Counts by Year, 2002-2019



Notes: This figure shows the number of home health agencies that served at least one patient in our sample in the corresponding year. The sample includes all beneficiaries enrolled in Traditional Medicare for the full calendar year in the 20% Medicare sample.

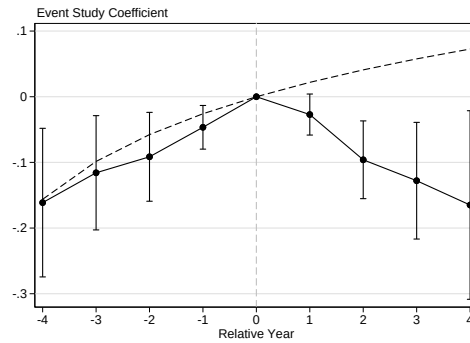
Figure OA.2: Standard Home Health Event Studies, with Years 5+ Binned



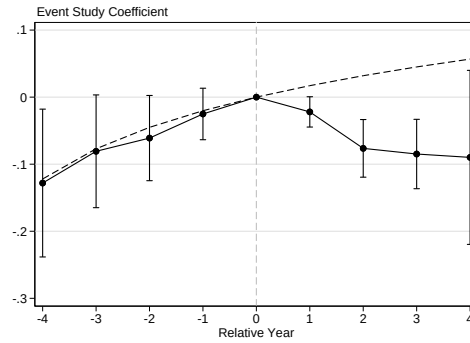
Notes: This figure shows estimates of $\beta_{r,SF}$ (left column) and $\theta_{r,M}$ (right column) from equation (1). The regressions are weighted by the number of Medicare enrollees (ages 65+) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 48,032$ county-years.

Figure OA.3: Standard Event Studies and Predicted Trends for Estimates of Strike Force

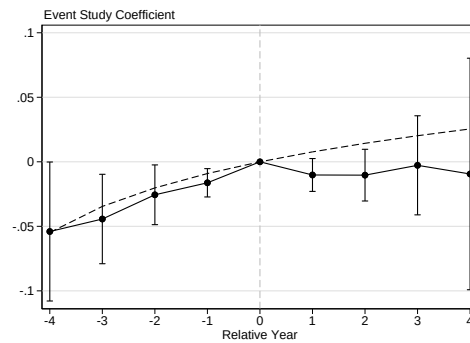
Log Home Health Visits Per Medicare Enrollee



Log Home Health Payments Per Medicare Enrollee

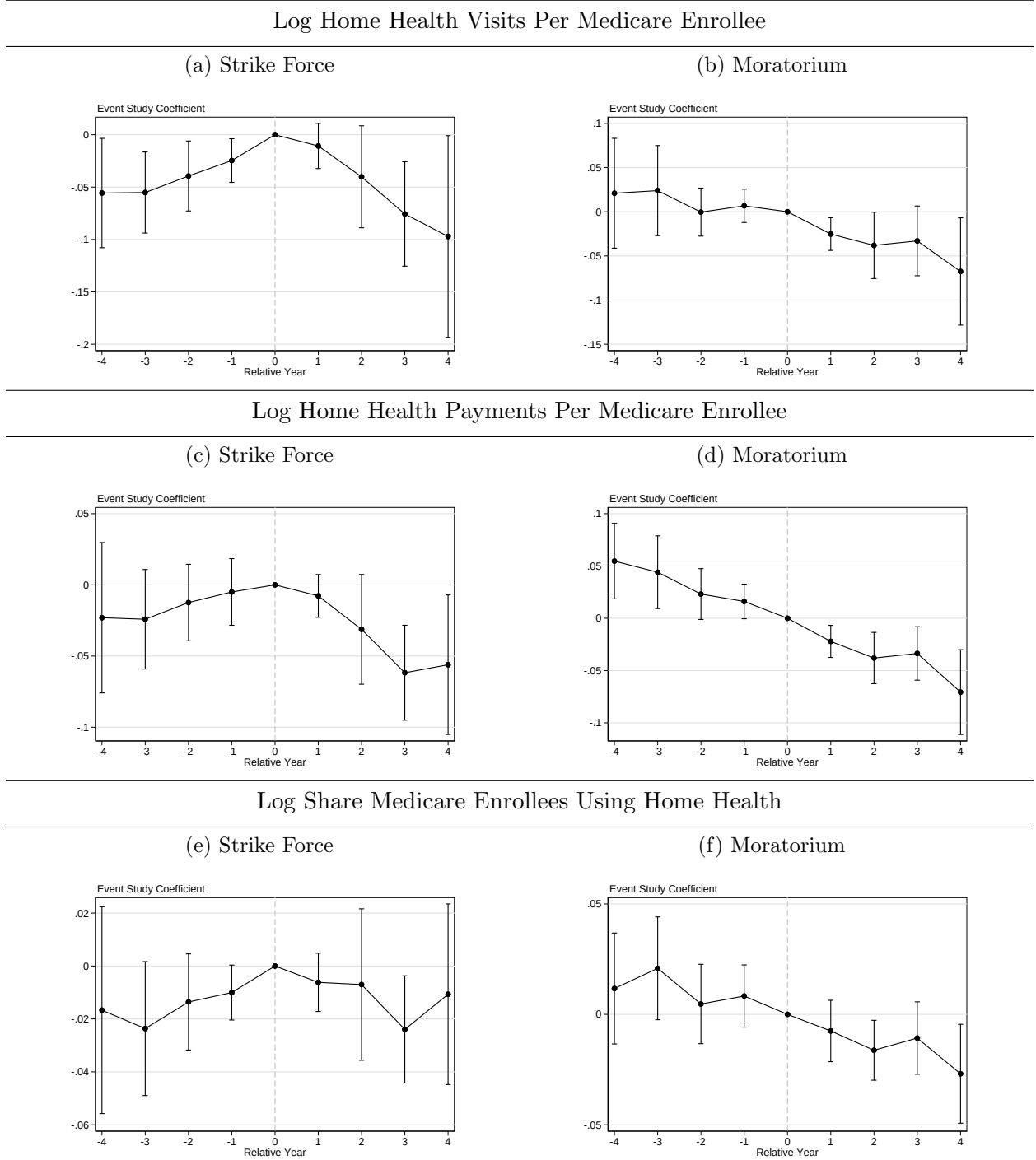


Log Share Medicare Enrollees Using Home Health



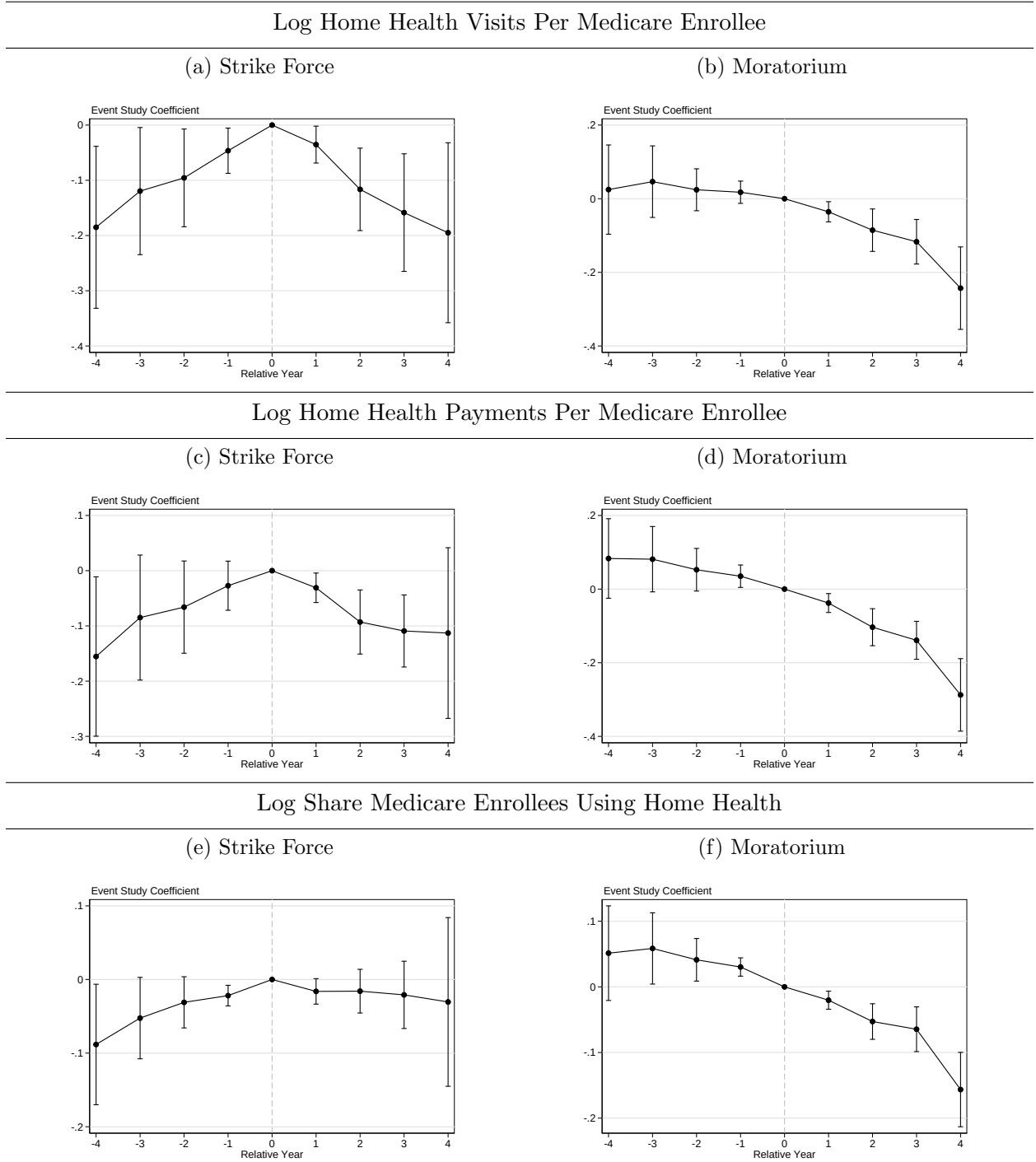
Notes: This figure plots the event-study coefficients and predicted pre-trends for the Strike Force estimates from equations (2) and (1). Regressions are weighted by the 2006 enrollee population. Vertical lines denote 95% confidence intervals clustered at the district level. The dashed line shows the pre-period logarithmic trend.

Figure OA.4: Standard Home Health Event Studies, limited to SNF-eligible Home Health



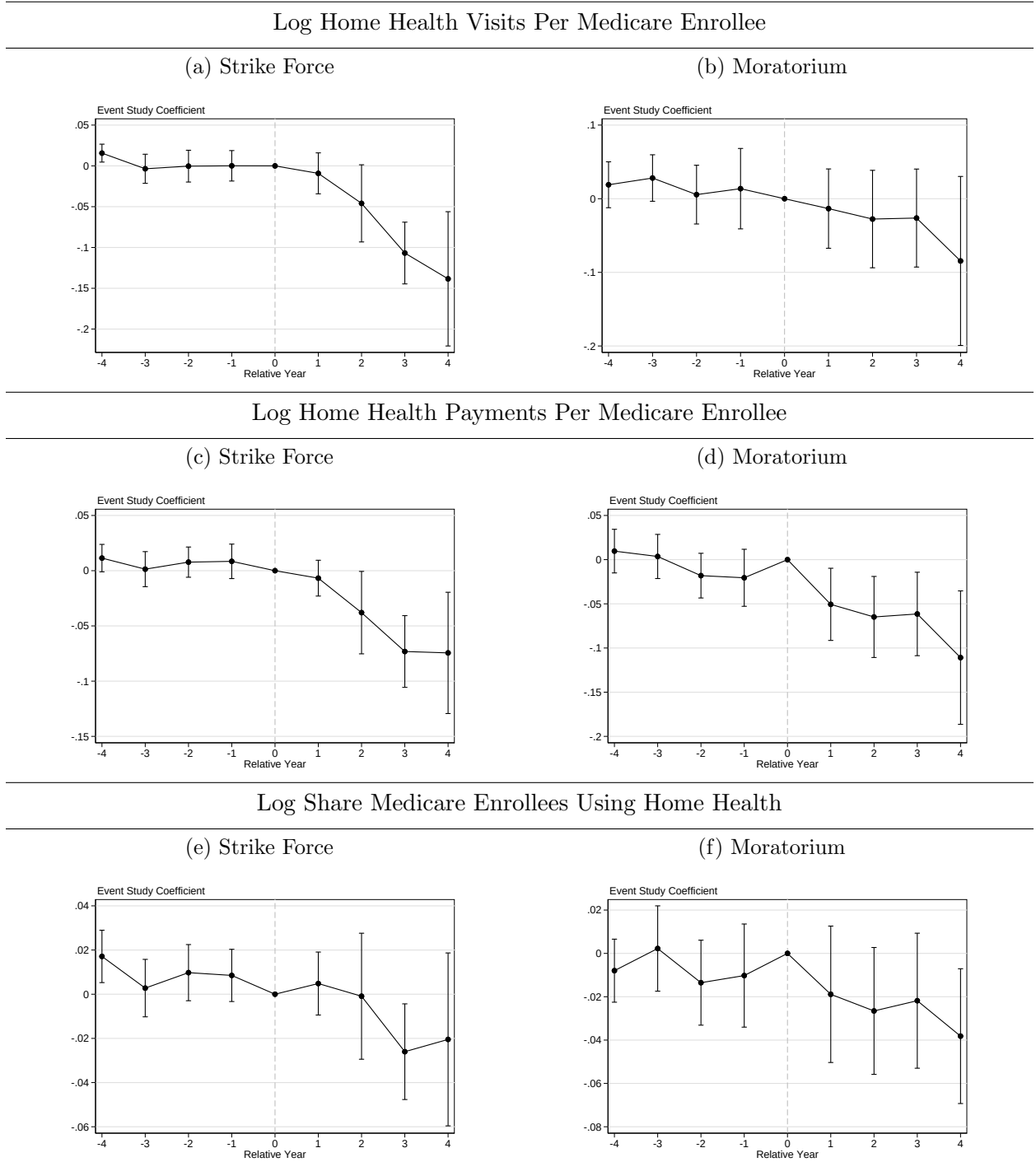
Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 1 with several measures of SNF eligible home health utilization as the outcome; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 45,712$ county-years.

Figure OA.5: Standard Home Health Event Studies, limited to SNF-ineligible Home Health



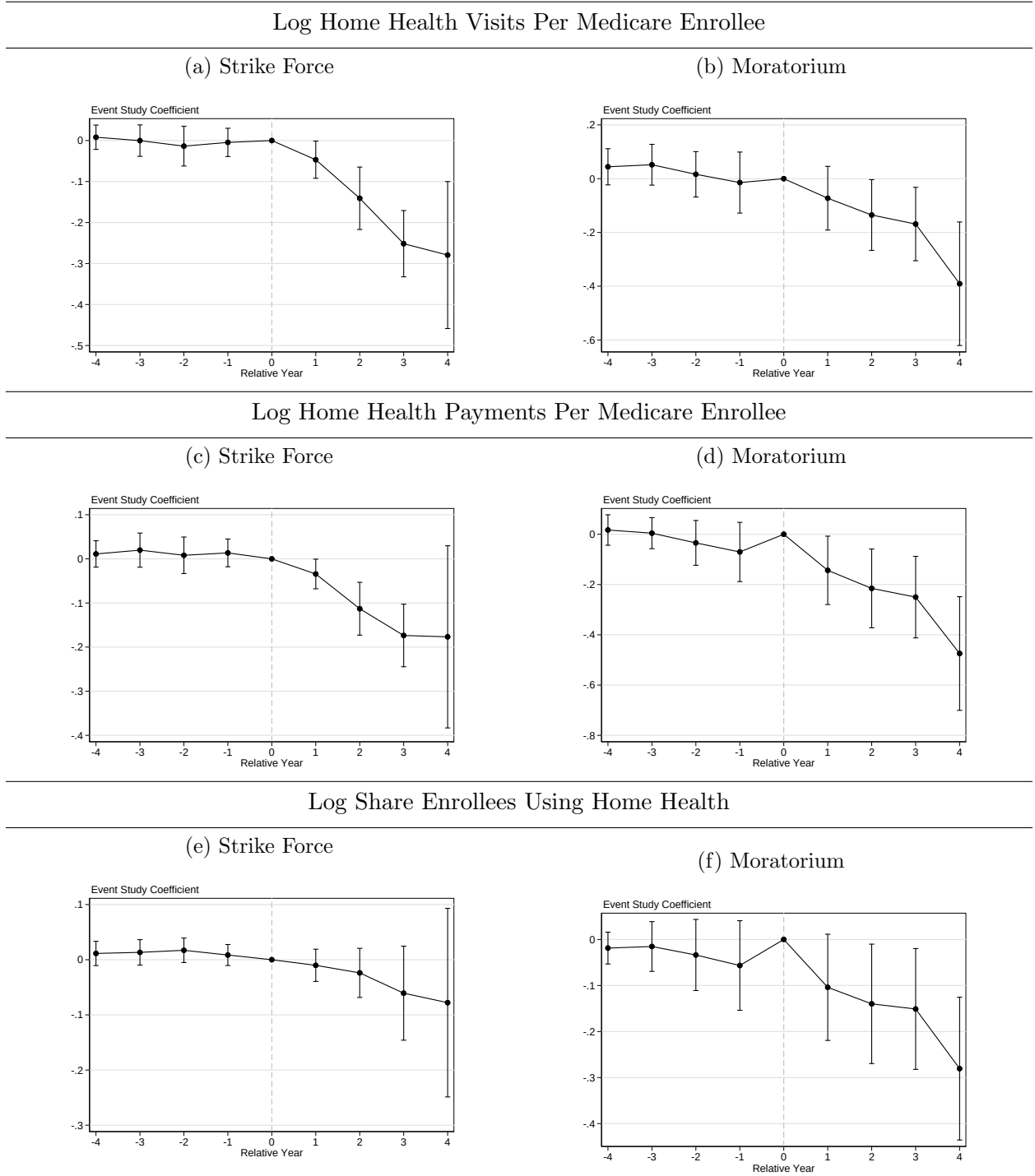
Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 1 with several measures of SNF ineligible home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 46,502$ county-years.

Figure OA.6: Detrended Home Health Event Studies, limited to SNF-eligible Home Health



Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 4 with several measures of SNF-eligible home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 8,720$ county-years.

Figure OA.7: Detrended Home Health Event Studies, limited to SNF-ineligible Home Health

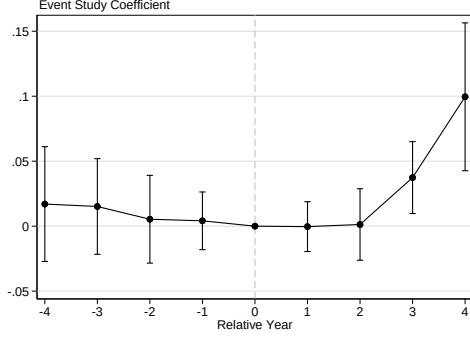


Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 4 with several measures of SNF-ineligible home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 8,927$ county-years.

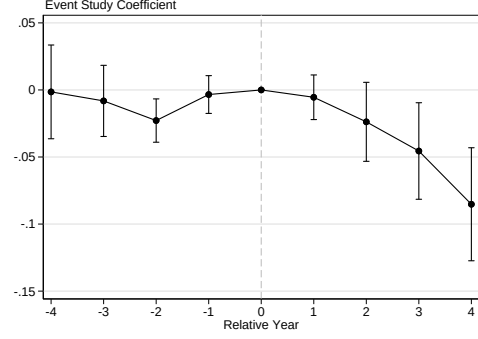
Figure OA.8: Standard SNF Event Studies

Log SNF Days Per Medicare Enrollee

(a) Strike Force

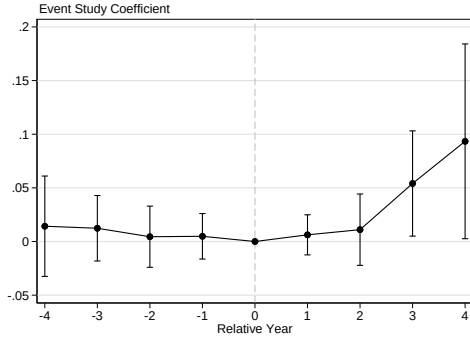


(b) Moratorium

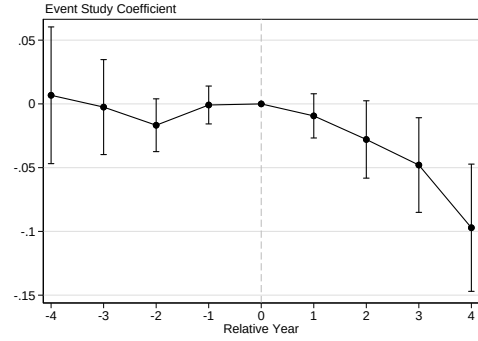


Log SNF Payments Per Medicare Enrollee

(c) Strike Force

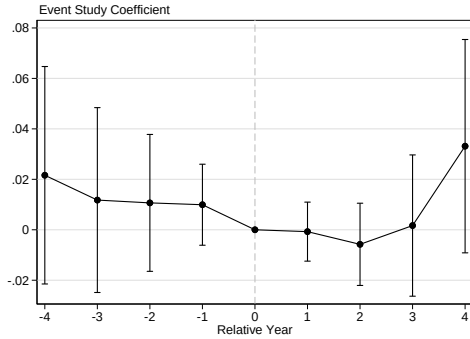


(d) Moratorium

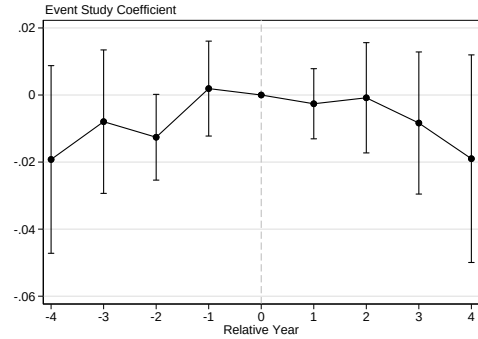


Log Share Medicare Enrollees Using SNF

(e) Strike Force



(f) Moratorium

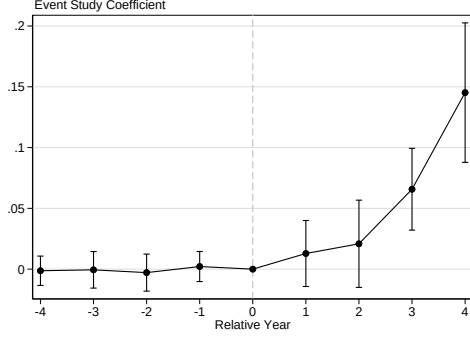


Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 1 with several measures of SNF utilization as the outcome. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 48,224$ county-years.

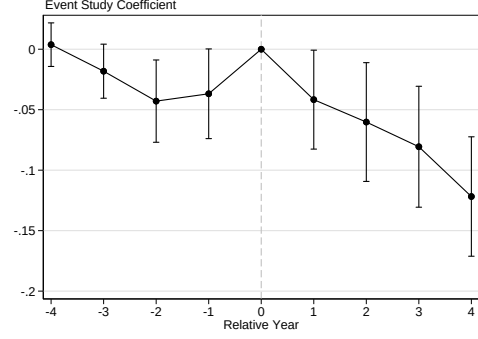
Figure OA.9: Detrended SNF Event Studies

Log SNF Days Per Medicare Enrollee

(a) Strike Force

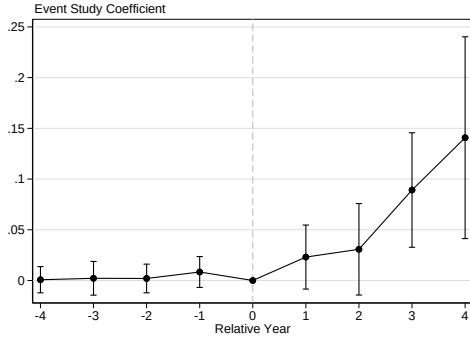


(b) Moratorium

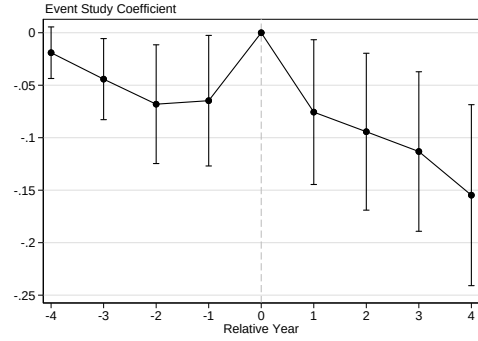


Log SNF Payments Per Medicare Enrollee

(c) Strike Force

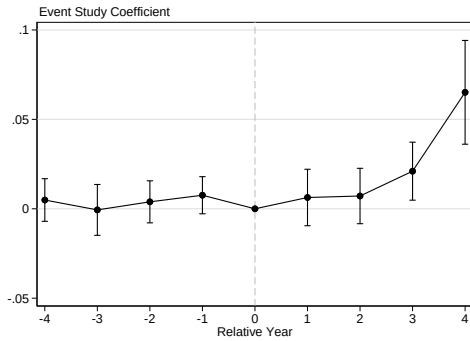


(d) Moratorium

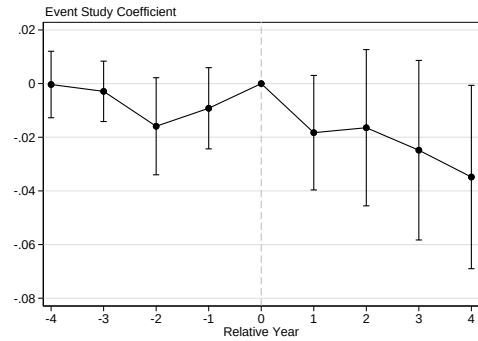


Log Share Enrollees Using SNF

(e) Strike Force



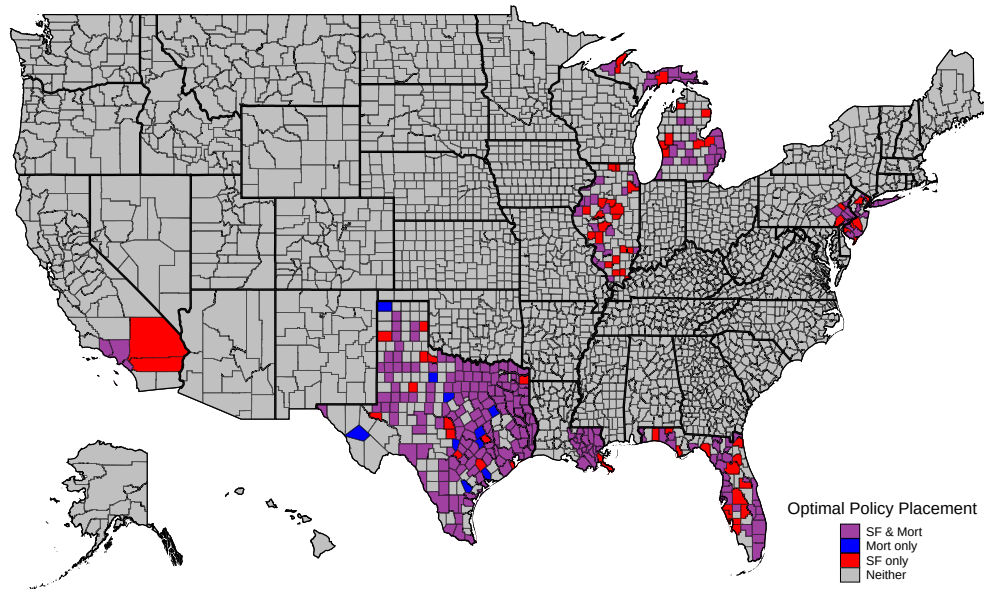
(f) Moratorium



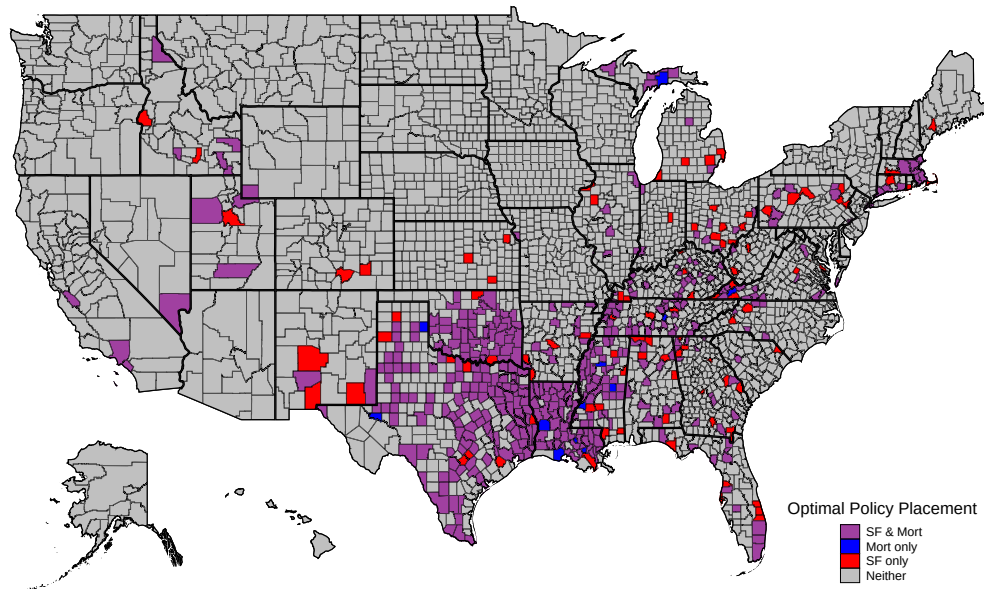
Notes: This figure shows estimates of $\beta_{r, SF}$ (left column) and $\theta_{r, M}$ (right column) from equation 4 with several detrended measures of SNF utilization. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 8,768$ county-years.

Figure OA.10: Optimal Placement of Policies

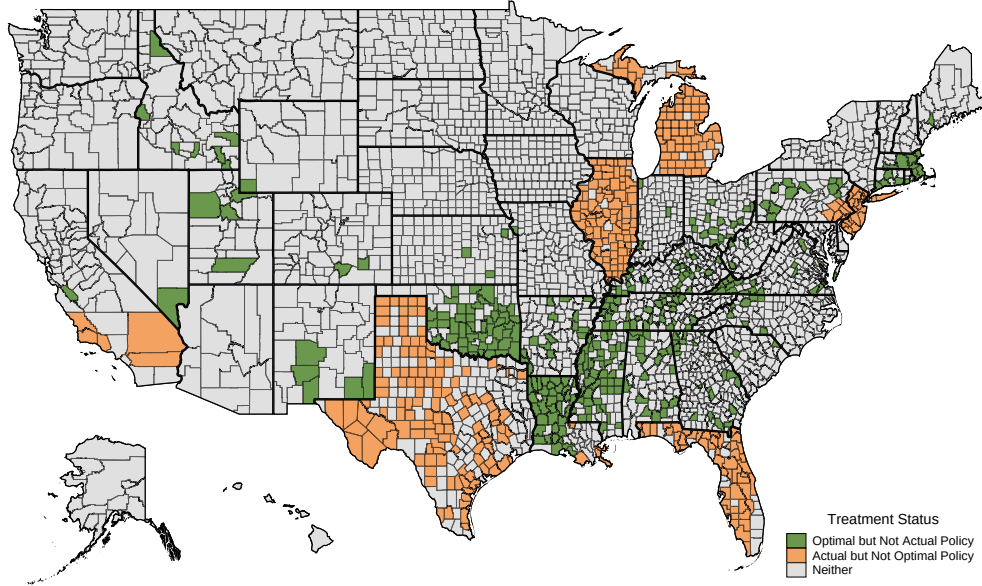
(a) Ever Treated Counties



(b) All Counties



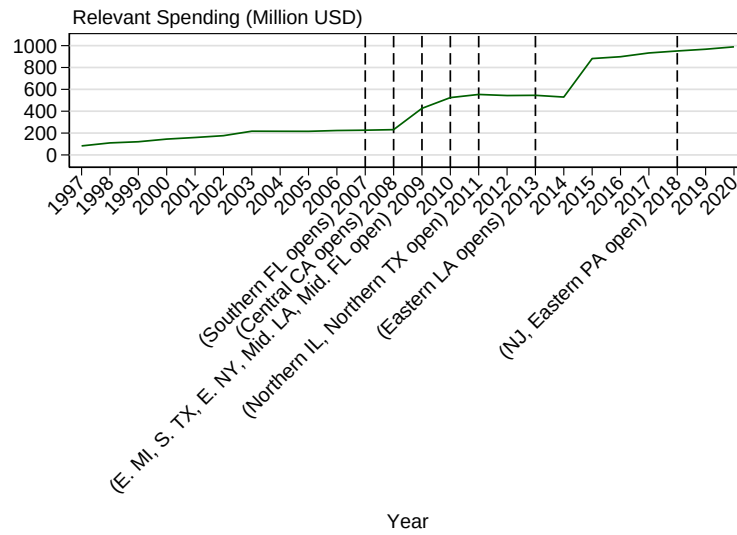
(c) All Counties: Changes in Treatment Status



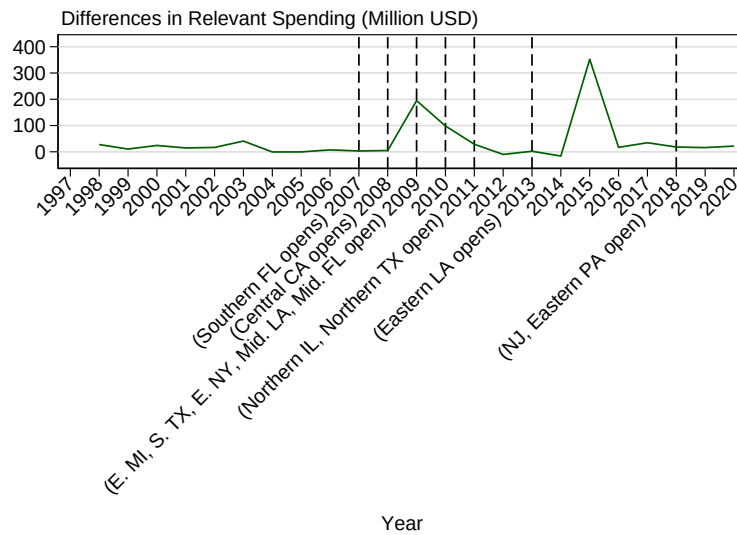
Notes: These figures map counties in the United States by which policy or policies they would receive under our optimal allocation procedure. In Panel (a), the pool of counties considered for optimal placement is counties that were ever treated in reality, while in Panel (b), it is all counties. Panel (c) considers all counties and indicates changes in policy treatment status between actual placement (as shown in Figure 2 and optimal placement; specifically it shows those that received at least one policy under actual placement but no policies under the optimal placement, and those that received at least one policy under optimal placement but no policies under actual placement.)

Figure OA.11: Annual Trends in Strike Force Spending

(a) Levels



(b) First Differences

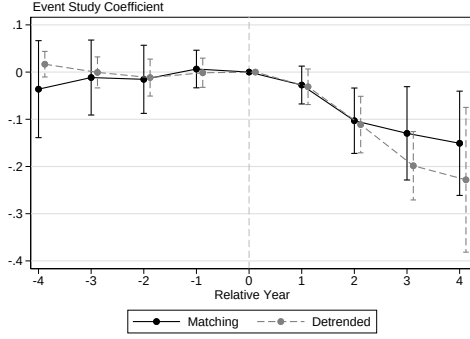


Notes: Panel a displays trends in annual, national spending on strike forces, using the sources and methods defined in Appendix A. Panel b displays the first differences in this spending.

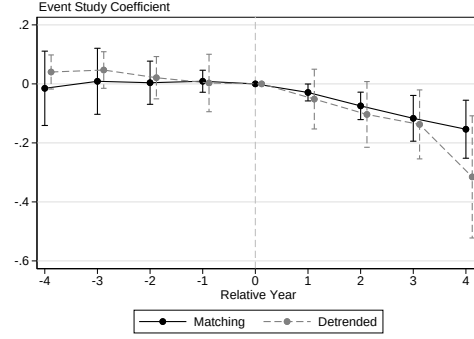
Figure OA.12: Matching Specification Event Studies

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

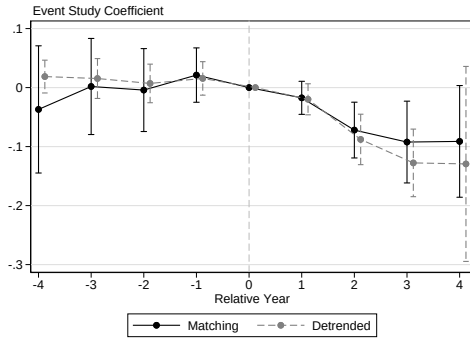


(b) Moratorium

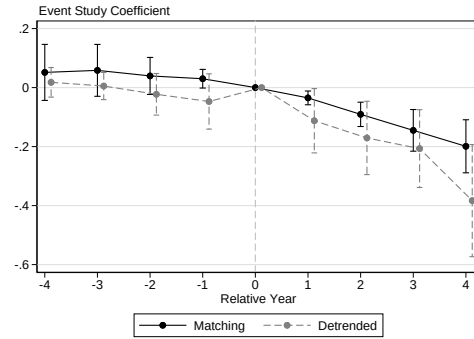


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

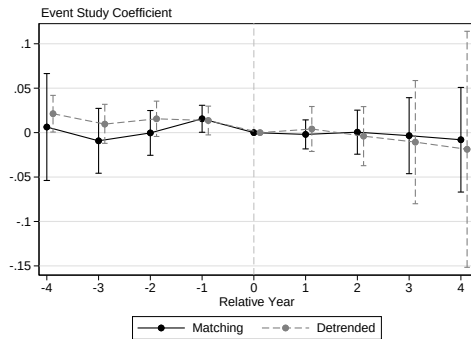


(d) Moratorium

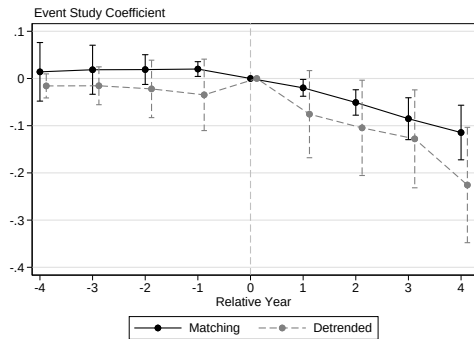


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



(f) Moratorium



Notes: This figure shows matching estimates from equation 8 (in black) and detrended estimates (in gray) from equation 4.

Table OA.1: Strike Force Office Openings

Federal Judicial District	City Contained	Strike Force Opening Date	Effective Date (if Different)
Southern District of Florida	Miami	March 2007	January 2009
Central District of California	Los Angeles	March 2008	January 2009
Eastern District of Michigan	Detroit	June 2009	-
Southern District of Texas	Houston	July 2009	-
Eastern District of New York	New York	December 2009	-
Middle District of Louisiana	Baton Rouge	December 2009	-
Middle District of Florida	Tampa	December 2009	-
Northern District of Texas	Dallas	February 2011	-
Northern District of Illinois	Chicago	February 2011	-
Eastern District of Louisiana	New Orleans	May 2013	-
District of New Jersey	Newark	August 2018	-
Eastern District of Pennsylvania	Philadelphia	August 2018	-

Notes: This table displays the location of each Strike Force office examined in our study, the date the office opened, and the date the office started targeting home health care agencies. These two dates are identical for all districts except for the Southern District of Florida and the Central District of California, where the focus was initially DME fraud when the offices opened, but shifted to home health fraud in 2009, which is the year we use in our analysis. (Office of the Inspector General 2018)

Table OA.2: HHA Entry Moratoria Start Dates

Counties Targeted	State	Moratorium Start Date
Cook, DuPage, Kane, Lake, McHenry, Will	Illinois	July 2013
Miami-Dade, Monroe	Florida	July 2013
Collin, Dallas, Denton, Ellis, Kaufman, Rockwall, Tarrant	Texas	January 2014
Macomb, Monroe, Oakland, Washtenaw, Wayne	Michigan	January 2014
Brazoria, Chambers, Fort Bend, Galveston, Harris, Liberty, Montgomery, Waller	Texas	January 2014
Broward	Florida	January 2014
Rest of Illinois	Illinois	June 2016
Rest of Texas	Texas	June 2016
Rest of Michigan	Michigan	June 2016
Rest of Florida	Florida	June 2016

Notes: This table displays the name and state of each county affected by a moratorium on home health entry, as well as the date on which the moratorium started (MedPAC 2010a; CMS 2016).

Table OA.3: Summary Statistics

	All	Strike Force		Moratorium	
		Yes	No	Yes	No
County Average (2006)					
Home Health Visits per Medicare enrollee	2.90	4.34	2.36	4.70	2.38
Home Health Visits per Home Health Patient	27.07	31.58	25.35	32.60	25.47
Home Health Payments per Medicare enrollees	\$393.47	\$577.77	\$323.14	\$612.61	\$329.97
Home Health Payments per Home Health Patient	\$3792.91	\$4410.68	\$3557.16	\$4449.71	\$3602.61
Share of Patients using Home Health	0.10	0.12	0.09	0.13	0.09
Average Change (2004-2009)					
Home Health Visits per Medicare enrollee	1.11	2.63	0.53	3.43	0.43
Home Health Visits per Home Health Patient	3.22	7.90	1.43	10.46	1.12
Home Health Payments per Medicare enrollee	\$204.84	\$393.56	\$132.83	\$495.72	\$120.57
Home Health Payments per Home Health Patient	\$1055.24	\$1571.50	\$858.23	\$1893.87	\$812.26
Share of Patients using Home Health	0.02	0.03	0.01	0.04	0.01
Number of Counties	3,002	296	2,706	498	2,504

Notes: This table displays average measures of home health utilization across various counties as defined by each column title. Averages are weighted by each county's Medicare enrollees (ages 65 and older) in 2006.

Table OA.4: Effect of Individual Policies on Home Health Utilization

Avg. of Relative Year Coef.	1 (1)	1-4+ (2)	4+ (3)
A: Strike Force			
Log Home Health Visits per Medicare enrollee	-0.031 (0.019)	-0.142 (0.033)	-0.228 (0.078)
Log Home Health Payments per Medicare enrollee	-0.020 (0.013)	-0.091 (0.030)	-0.129 (0.084)
Log Used Home Health per Medicare enrollee	0.004 (0.013)	-0.007 (0.031)	-0.019 (0.068)
B: Moratorium			
Log Home Health Visits per Medicare enrollee	-0.051 (0.052)	-0.152 (0.065)	-0.315 (0.106)
Log Home Health Payments per Medicare enrollee	-0.112 (0.056)	-0.218 (0.068)	-0.383 (0.097)
Log Used Home Health per Medicare enrollee	-0.076 (0.047)	-0.133 (0.053)	-0.226 (0.062)

Notes: This table shows averages of post-period coefficients assessing the impact of the strike force (panel A) and the moratoria (panel B) on measures of home health utilization. Specifically, it reports the average (over various post-period years) of the estimates of the $\beta_{r,SF}$'s from equation 4 for the strike force, and the $\theta_{r,M}$'s for the moratorium. Standard errors clustered at the district level are displayed in parentheses below each estimate. The sample size is 8,992 county-years.

Table OA.5: Policy Combinations

	Relative Year	Moratorium				
		0	1	2	3	4+
Strike Force	0	—	2.53%	2.55%	2.53%	2.81%
	1	6.99%	0%	0%	0%	0%
	2	10.84%	0%	0%	0%	0%
	3	7.69%	1.18%	0%	0%	0%
	4+	32.67%	5.07%	6.10%	6.04%	13.00%

Notes: This table shows the share of (enrollee-weighted) county-years in each policy combination.

Table OA.6: Variable Importance - Home Health Payments

Lag Spending	# Comorbidities	Age	Sex	I(Dual)
0.141	0.455	0.088	0.019	0

Notes: The table reports the variable importance measure, a weighted sum of how many times a given variable was split on at each depth in the forest. Splits closer to the root receive higher weight, with the weight decreasing quadratically with depth. Variables with a higher value of variable importance contribute more to the forest's ability to detect meaningful heterogeneity in treatment effects across subgroups. Averages are taken across all 82 causal forest groups (see Appendix Table OA.17).

Table OA.7: Bivariate Coefficients From Regressing $\tau_i(a)$ on Enrollee Observables

Covariate	Estimate	SE
Age	-0.008	0.0000
I(Dual)	-0.115	0.0005
I(Female)	-0.047	0.0003
# Comorbidities	-0.085	0.0000
Lag Spending	-0.00000226	0.00000003

Notes: Table reports results from regressions of $\tau_i(a)$ on policy group (a) fixed effects and one of the covariates from the causal forest (age, dual Medicaid eligibility, sex, number of comorbidities, and lagged Medicare spending); each row in the table shows results from a regression on a different covariate.

Table OA.8: Distribution of policies under observed and optimal placement

	Optimal Placement			
	Strike Force Only	Moratorium Only	Strike Force and Moratorium	No Policy
Panel A. Ever-treated counties				
<i>Observed Placement</i>				
Strike Force Only (34%)	0.17	0.00	0.69	0.15
Moratorium Only (19%)	0.13	0.00	0.61	0.27
Strike Force and Moratorium (47%)	0.16	0.00	0.65	0.18
Ever-treated counties (100%)	0.16	0.00	0.66	0.19
Panel B. All counties				
<i>Observed Placement</i>				
Strike Force Only (12%)	0.01	0.00	0.41	0.57
Moratorium Only (6%)	0.05	0.01	0.42	0.52
Strike Force and Moratorium (16%)	0.10	0.00	0.45	0.45
No Policy (66%)	0.05	0.00	0.12	0.84
Ever-treated counties (34%)	0.06	0.00	0.43	0.51

Notes: Table presents the distribution of policies under observed and optimal placement. Panel A shows results for ever-treated counties and Panel B shows results for all counties. Each row contains the counties that actually received a given policy combination (their enrollee-weighted share of all counties in the panel is shown in parentheses), and the columns show what share of these counties would receive each policy combination under optimal assignment. Shares are weighted by the number of enrollees in a county in 2006.

Table OA.9: Effect of Individual Policies on Health Health Utilization, Robustness Analysis

Avg. of Relative Year Coef.	Strike Force			Moratorium		
	1 (1)	1-4+ (2)	4+ (3)	1 (4)	1-4+ (5)	4+ (6)
Panel A. Baseline Specification						
Log Home Health Visits per Medicare Enrollee	-0.031 (0.019)	-0.142 (0.033)	-0.228 (0.078)	-0.051 (0.052)	-0.152 (0.065)	-0.315 (0.106)
Log Home Health Payments per Medicare Enrollee	-0.020 (0.013)	-0.091 (0.030)	-0.129 (0.084)	-0.112 (0.056)	-0.218 (0.068)	-0.383 (0.097)
Log Share Enrollees Using Home Health	0.004 (0.013)	-0.007 (0.031)	-0.019 (0.068)	-0.076 (0.047)	-0.133 (0.053)	-0.226 (0.062)
Panel B. Matching Specifications						
<i>Baseline Matching Specification</i>						
Log Home Health Visits per Medicare Enrollee	-0.027 (0.020)	-0.118 (0.046)	-0.165 (0.074)	-0.029 (0.015)	-0.140 (0.046)	-0.239 (0.083)
Log Home Health Payments per Medicare Enrollee	-0.017 (0.014)	-0.071 (0.036)	-0.088 (0.065)	-0.035 (0.012)	-0.169 (0.041)	-0.288 (0.074)
Log Share Enrollees Using Home Health	-0.002 (0.008)	-0.003 (0.023)	-0.005 (0.043)	-0.019 (0.009)	-0.104 (0.025)	-0.182 (0.046)
<i>Matching with Control > 150 patients</i>						
Log Home Health Visits per Medicare Enrollee	-0.029 (0.020)	-0.118 (0.045)	-0.164 (0.073)	-0.029 (0.015)	-0.140 (0.046)	-0.239 (0.083)
Log Home Health Payments per Medicare Enrollee	-0.017 (0.014)	-0.071 (0.035)	-0.089 (0.065)	-0.035 (0.012)	-0.167 (0.041)	-0.285 (0.074)
Log Share Enrollees Using Home Health	-0.002 (0.008)	-0.003 (0.023)	-0.005 (0.042)	-0.019 (0.009)	-0.104 (0.025)	-0.183 (0.045)
<i>Matching with Treat & Control > 150 patients</i>						
Log Home Health Visits per Medicare Enrollee	-0.029 (0.020)	-0.120 (0.045)	-0.165 (0.073)	-0.030 (0.015)	-0.139 (0.047)	-0.238 (0.084)
Log Home Health Payments per Medicare Enrollee	-0.016 (0.014)	-0.076 (0.035)	-0.097 (0.066)	-0.035 (0.012)	-0.172 (0.041)	-0.294 (0.074)
Log Share Enrollees Using Home Health	-0.002 (0.008)	-0.003 (0.023)	-0.005 (0.042)	-0.020 (0.009)	-0.106 (0.025)	-0.185 (0.046)
<i>Drop Bad Matches</i>						
Log Home Health Visits per Medicare Enrollee	-0.028 (0.021)	-0.118 (0.047)	-0.166 (0.074)	-0.030 (0.015)	-0.139 (0.047)	-0.238 (0.083)
Log Home Health Payments per Medicare Enrollee	-0.017 (0.014)	-0.071 (0.036)	-0.088 (0.065)	-0.035 (0.012)	-0.169 (0.041)	-0.288 (0.074)
Log Share Enrollees Using Home Health	-0.003 (0.008)	-0.003 (0.023)	-0.006 (0.043)	-0.020 (0.009)	-0.104 (0.025)	-0.181 (0.046)

Notes: Table shows robustness of the estimates in Appendix Table OA.4 to alternative specifications. It reports averages of post-period coefficients assessing the impact of the strike force (column 1 through 3) and the moratoria (column 4 through 6) on measures of home health utilization. Panel A reports the baseline results from Appendix Table OA.4. Panel B presents the results from nearest neighbor matching estimates of equation (8). We show results from four different matching estimators. The first three rows show the results from our baseline matching procedure, while the second three rows shows an alternative with a restricted set of counties. The next three row shows the results of weighting the regression with the average 2006 enrollee population between the treatment county and its control match. Finally, the last three rows ('drop bad matches') shows the results of removing matches with a distance between treatment and control that is greater than 5%. Standard errors clustered at the district level are displayed in parentheses below each estimate.

Table OA.10: Effect of Individual Policies on Health Health Utilization, Robustness Analysis

Avg. of Relative Year Coef.	Strike Force			Moratorium		
	1 (1)	1-4+ (2)	4+ (3)	1 (4)	1-4+ (5)	4+ (6)
Panel A. Baseline Specification						
Log Home Health Visits per Medicare Enrollee	-0.031 (0.019)	-0.142 (0.033)	-0.228 (0.078)	-0.051 (0.052)	-0.152 (0.065)	-0.315 (0.106)
Log Home Health Payments per Medicare Enrollee	-0.020 (0.013)	-0.091 (0.030)	-0.129 (0.084)	-0.112 (0.056)	-0.218 (0.068)	-0.383 (0.097)
Log Share Enrollees Using Home Health	0.004 (0.013)	-0.007 (0.031)	-0.019 (0.068)	-0.076 (0.047)	-0.133 (0.053)	-0.226 (0.062)
Panel B. Poisson Regression						
Home Health Visits per Medicare Enrollee	-0.043 (0.025)	-0.123 (0.035)	-0.181 (0.064)	-0.039 (0.052)	-0.123 (0.058)	-0.254 (0.074)
Home Health Payments per Medicare Enrollee	-0.055 (0.022)	-0.106 (0.025)	-0.121 (0.067)	-0.123 (0.059)	-0.214 (0.068)	-0.352 (0.085)
Log Share Enrollees Using Home Health	-0.027 (0.015)	-0.031 (0.017)	-0.032 (0.051)	-0.095 (0.057)	-0.149 (0.062)	-0.236 (0.070)
Panel C. Callaway & Sant'Anna (2021)						
Log Home Health Visits per Medicare Enrollee	-0.027 (0.014)	-0.185 (0.056)	-0.269 (0.077)	-0.047 (0.023)	-0.206 (0.076)	-0.337 (0.117)
Log Home Health Payments per Medicare Enrollee	-0.021 (0.013)	-0.150 (0.049)	-0.219 (0.073)	-0.049 (0.018)	-0.240 (0.064)	-0.399 (0.100)
Log Share Enrollees Using Home Health	-0.010 (0.006)	-0.056 (0.031)	-0.090 (0.046)	-0.028 (0.013)	-0.151 (0.041)	-0.262 (0.072)

Notes: Table shows robustness of the estimates in Table OA.4 to alternative specifications. It reports averages of post-period coefficients assessing the impact of the strike force (column 1 through 3) and the moratoria (column 4 through 6) on measures of home health utilization. The baseline reports estimates of the $\beta_{r,SF}$'s from equation 4 for the strike force, and the $\theta_{r,M}$'s for the moratorium. Standard errors clustered at the district level are displayed in parentheses below each estimate. Panel A reports the baseline results from Appendix Table OA.4. Panel B shows estimates from the baseline specification using a Poisson regression. Panel C reports the results using the average treatment effect estimator of Callaway and Sant'Anna (2021).

Table OA.11: Effect of Individual Policies on Health Health Utilization, Robustness of Strike Force Estimates to Dropping Districts

District	All Districts	Southern FL	Central CA	Eastern MI	Southern TX	Eastern NY	Middle LA	NJ	Eastern PA	Middle FL	Northern TX	Northern IL	Eastern LA
Log Home Health Visits per Medicare Enrollee													
Year 1	-0.031 (0.019)	-0.026 (0.021)	-0.044 (0.023)	-0.034 (0.020)	-0.033 (0.021)	-0.024 (0.019)	-0.031 (0.019)	-0.035 (0.023)	-0.028 (0.020)	-0.036 (0.019)	-0.022 (0.018)	-0.027 (0.022)	-0.027 (0.019)
1-4+	-0.142 (0.033)	-0.123 (0.028)	-0.164 (0.041)	-0.148 (0.037)	-0.139 (0.034)	-0.129 (0.035)	-0.140 (0.033)	-0.148 (0.034)	-0.142 (0.034)	-0.164 (0.033)	-0.136 (0.034)	-0.146 (0.038)	-0.137 (0.033)
4+	-0.228 (0.078)	-0.208 (0.076)	-0.299 (0.083)	-0.236 (0.088)	-0.208 (0.077)	-0.186 (0.073)	-0.223 (0.079)	-0.229 (0.078)	-0.228 (0.078)	-0.275 (0.087)	-0.228 (0.081)	-0.235 (0.083)	-0.221 (0.078)
Log Home Health Payments per Medicare Enrollee													
Year 1	-0.020 (0.013)	-0.017 (0.015)	-0.031 (0.017)	-0.025 (0.014)	-0.021 (0.015)	-0.016 (0.015)	-0.019 (0.013)	-0.022 (0.016)	-0.015 (0.014)	-0.025 (0.014)	-0.018 (0.014)	-0.018 (0.015)	-0.016 (0.013)
1-4+	-0.091 (0.030)	-0.080 (0.026)	-0.127 (0.030)	-0.092 (0.032)	-0.085 (0.027)	-0.094 (0.037)	-0.089 (0.029)	-0.095 (0.031)	-0.090 (0.030)	-0.105 (0.033)	-0.091 (0.031)	-0.090 (0.031)	-0.085 (0.028)
4+	-0.129 (0.084)	-0.116 (0.084)	-0.235 (0.063)	-0.124 (0.090)	-0.101 (0.075)	-0.126 (0.102)	-0.122 (0.084)	-0.130 (0.085)	-0.129 (0.084)	-0.153 (0.100)	-0.131 (0.088)	-0.131 (0.088)	-0.120 (0.083)
Log Share Enrollees Using Home Health													
Year 1	0.004 (0.013)	0.004 (0.013)	-0.010 (0.010)	0.001 (0.013)	0.003 (0.013)	0.007 (0.015)	0.005 (0.013)	0.008 (0.014)	0.008 (0.013)	0.002 (0.015)	0.005 (0.013)	0.004 (0.012)	0.006 (0.013)
1-4+	-0.007 (0.031)	-0.008 (0.032)	-0.050 (0.014)	-0.006 (0.032)	-0.003 (0.030)	-0.004 (0.037)	-0.006 (0.031)	-0.005 (0.032)	-0.005 (0.031)	-0.010 (0.035)	-0.005 (0.032)	-0.003 (0.031)	-0.003 (0.031)
4+	-0.019 (0.068)	-0.015 (0.070)	-0.109 (0.031)	-0.013 (0.071)	-0.001 (0.064)	-0.009 (0.079)	-0.015 (0.068)	-0.019 (0.068)	-0.019 (0.068)	-0.025 (0.078)	-0.018 (0.070)	-0.017 (0.070)	-0.014 (0.067)
N (county-years)	8992	8848	8880	8448	8320	8912	8848	8656	8848	8432	7488	8704	8784

Notes: This table shows robustness of the strike force estimates from Table OA.4 to dropping counties in the corresponding court district. The “all districts” column reports the baseline estimates from Table OA.4, and the other columns report the results of dropping particular court districts. Specifically, table reports averages of the $\beta_{r, SF}$ ’s from equation 4. Standard errors clustered at the district level are displayed in parentheses below each estimate.

Table OA.12: Effect of Individual Policies on Health Health Utilization, Robustness of Moratoria Estimates to Dropping Districts

District	All Districts	Southern FL	Central CA	Eastern MI	Southern TX	Eastern NY	Middle LA	NJ	Eastern PA	Middle FL	Northern TX	Northern IL	Eastern LA
Log Home Health Visits per Medicare Enrollee													
Year 1	-0.051 (0.052)	-0.027 (0.044)	-0.008 (0.032)	-0.054 (0.056)	-0.037 (0.050)	-0.077 (0.043)	-0.055 (0.052)	-0.051 (0.052)	-0.051 (0.052)	-0.079 (0.055)	-0.047 (0.054)	-0.057 (0.063)	-0.057 (0.051)
1-4+	-0.152 (0.065)	-0.109 (0.050)	-0.101 (0.046)	-0.157 (0.072)	-0.146 (0.067)	-0.182 (0.056)	-0.156 (0.065)	-0.152 (0.066)	-0.152 (0.066)	-0.173 (0.075)	-0.145 (0.069)	-0.162 (0.079)	-0.157 (0.065)
4+	-0.315 (0.106)	-0.223 (0.060)	-0.256 (0.091)	-0.325 (0.121)	-0.327 (0.113)	-0.351 (0.098)	-0.320 (0.106)	-0.315 (0.106)	-0.315 (0.106)	-0.328 (0.121)	-0.305 (0.114)	-0.338 (0.140)	-0.322 (0.105)
Log Home Health Payments per Medicare Enrollee													
Year 1	-0.112 (0.056)	-0.103 (0.052)	-0.047 (0.026)	-0.109 (0.056)	-0.095 (0.047)	-0.113 (0.064)	-0.117 (0.055)	-0.112 (0.056)	-0.112 (0.056)	-0.129 (0.060)	-0.116 (0.057)	-0.115 (0.063)	-0.119 (0.055)
1-4+	-0.218 (0.068)	-0.190 (0.060)	-0.142 (0.036)	-0.218 (0.071)	-0.208 (0.064)	-0.220 (0.077)	-0.223 (0.068)	-0.218 (0.068)	-0.218 (0.068)	-0.236 (0.076)	-0.221 (0.071)	-0.218 (0.076)	-0.225 (0.067)
4+	-0.383 (0.097)	-0.316 (0.074)	-0.295 (0.071)	-0.384 (0.107)	-0.390 (0.100)	-0.386 (0.105)	-0.389 (0.097)	-0.383 (0.097)	-0.383 (0.097)	-0.403 (0.109)	-0.388 (0.103)	-0.385 (0.120)	-0.391 (0.096)
Log Share Enrollees Using Home Health													
Year 1	-0.076 (0.047)	-0.080 (0.047)	-0.016 (0.017)	-0.071 (0.046)	-0.064 (0.042)	-0.081 (0.054)	-0.078 (0.047)	-0.076 (0.047)	-0.076 (0.047)	-0.087 (0.048)	-0.076 (0.047)	-0.074 (0.050)	-0.079 (0.047)
1-4+	-0.133 (0.053)	-0.130 (0.053)	-0.068 (0.017)	-0.130 (0.053)	-0.125 (0.049)	-0.140 (0.060)	-0.136 (0.053)	-0.134 (0.053)	-0.134 (0.053)	-0.147 (0.055)	-0.134 (0.053)	-0.128 (0.054)	-0.137 (0.052)
4+	-0.226 (0.062)	-0.208 (0.062)	-0.150 (0.027)	-0.222 (0.065)	-0.225 (0.062)	-0.233 (0.070)	-0.229 (0.063)	-0.226 (0.062)	-0.226 (0.062)	-0.242 (0.067)	-0.228 (0.064)	-0.218 (0.066)	-0.230 (0.062)
N (county-years)	8992	8848	8880	8448	8320	8912	8848	8656	8848	8432	7488	8704	8784

This table shows robustness of the moratoria estimates from Table OA.4 to dropping counties in the corresponding court district. The “all districts” column reports the baseline estimates from Table OA.4, and the other columns report the results of dropping particular court districts. Specifically, table reports averages of the θ_r ’s from equation 4. Standard errors clustered at the district level are displayed in parentheses below each estimate.

Table OA.13: 2006 Summary Statistics on SNF and Home Health Use, by SNF Eligibility

	All (1)	SNF-Eligible (2)	SNF-Ineligible (3)
Panel A: Share of Enrollees			
Share of Enrollees	1.00	0.17	0.83
Share of Enrollees using SNF	0.06	0.34	0.01
Share of Enrollees using Home Health	0.10	0.29	0.07
Panel B: Utilization Outcomes			
SNF Days per Medicare enrollees	2.20	12.02	0.28
SNF Payments per Medicare enrollees	\$673	\$3721	\$68
Home Health Visits per Medicare enrollees	2.90	5.88	2.62
Home Health Payments per Medicare enrollees	\$393	\$887	\$336

Notes: This table presents 2006 Medicare enrollee-level averages of SNF and home health utilization outcomes. Panel A reports the share of enrollees (overall, and by type of care that they use); columns (2) and (3) distinguish between enrollees who are SNF-eligible vs SNF-ineligible, with an enrollee defined as “SNF-eligible” if she has at least one inpatient hospital stay lasting three or more days during the year, and “SNF-ineligible” otherwise. Panel B reports the average enrollee’s SNF and home health use; here, columns (2) and (3) distinguish between care use that is and is not SNF-eligible, with SNF-eligible care limited to care that begins within 30 days of a qualifying inpatient stay. See Appendix C for more details.

Table OA.14: Effect of Individual Policies on Home Health Utilization by SNF Use Eligibility

Avg of Relative Year Coef.	1			1-4+			4+		
	All	SNF Eligible	SNF Ineligible	All	SNF Eligible	SNF Ineligible	All	SNF Eligible	SNF Ineligible
<i>Strike Force</i>									
Log Home Health Visits per Medicare Enrollee	-0.031 (0.019)	-0.009 (0.013)	-0.047 (0.023)	-0.142 (0.033)	-0.075 (0.018)	-0.180 (0.039)	-0.228 (0.078)	-0.138 (0.042)	-0.279 (0.051)
Log Home Health Payments per Medicare Enrollee	-0.020 (0.013)	-0.007 (0.008)	-0.034 (0.017)	-0.091 (0.030)	-0.048 (0.012)	-0.124 (0.038)	-0.129 (0.084)	-0.074 (0.028)	-0.177 (0.105)
Log Share Enrollees Using Home Health	0.004 (0.013)	0.005 (0.007)	-0.010 (0.015)	-0.007 (0.031)	-0.011 (0.010)	-0.043 (0.039)	-0.019 (0.068)	-0.020 (0.020)	-0.085 (0.087)
<i>Moratorium</i>									
Log Home Health Visits per Medicare Enrollee	-0.051 (0.052)	-0.013 (0.027)	-0.073 (0.060)	-0.152 (0.065)	-0.038 (0.037)	-0.192 (0.076)	-0.315 (0.106)	-0.085 (0.059)	-0.315 (0.117)
Log Home Health Payments per Medicare Enrollee	-0.112 (0.056)	-0.051 (0.021)	-0.143 (0.069)	-0.218 (0.068)	-0.072 (0.025)	-0.271 (0.084)	-0.383 (0.097)	-0.111 (0.039)	-0.474 (0.139)
Log Share Enrollees Using Home Health	-0.076 (0.047)	-0.019 (0.016)	-0.104 (0.059)	-0.133 (0.053)	-0.026 (0.015)	-0.169 (0.067)	-0.226 (0.062)	-0.038 (0.016)	-0.281 (0.075)
Enrollee Share	1.00	0.17	0.83	1.00	0.17	0.83	1.00	0.17	0.83

Notes: This table presents averages of post-period coefficients assessing the effects of the Strike Force and Moratorium policies on three measures of home health utilization: log home health visits per enrollee, log home health payments per enrollee, and log share using home health . The coefficients are computed using $\beta_{r,SF}$ and $\theta_{r,M}$ from equation 4, with strike force effects corresponding to $\beta_{r,SF}$ and moratorium effects to $\theta_{r,M}$. Standard errors, clustered at the district level, are shown in parentheses below each estimate. We show results for all care, and separately for SNF-eligible and non-SNF eligible care; see Appendix C for more detail on how these are defined. The sample includes 8,992 county-years for each Home Health outcome among all Home Health use. Among SNF-eligible use, the sample includes 8,720 county-years. Among SNF-ineligible use, the sample includes 8,927 county-years.

Table OA.15: Summary Statistics for SNF-Eligible Use

	All Counties	Strike Force		Moratorium	
		Yes	No	Yes	No
SNF Use (2006)					
Share of Patients using SNF	0.34	0.33	0.34	0.32	0.34
SNF Days per Capita	12.02	12.42	11.86	11.98	12.03
SNF Payments per Capita	\$3721	\$4073	\$3585	\$3627	\$3748

Notes: This table displays average measures of SNF utilization for those that are eligible for Medicare coverage across various subsets of counties as indicated by the column headings. “Share of Enrollees using SNF” refers to the proportion of county Medicare enrollees who used SNF among SNF-eligible enrollees. SNF-eligible utilization is an SNF use that occurred within 30 days following an inpatient hospital stay for 3 or more days. Averages are weighted by each county’s Medicare enrollees (ages 65 and older) in 2006.

Table OA.16: Effect of Individual Policies on SNF Utilization

Avg. of Relative Year Coef.	1	1-4+	4+
	(1)	(2)	(3)
A: Strike Force			
Log SNF Days per Medicare enrollee	0.013 (0.014)	0.061 (0.017)	0.145 (0.029)
Log SNF Payments per Medicare enrollee	0.023 (0.016)	0.071 (0.026)	0.141 (0.051)
Log Used SNF per Medicare enrollee	0.006 (0.008)	0.025 (0.009)	0.065 (0.015)
B: Moratorium			
Log SNF Days per Medicare enrollee	-0.042 (0.021)	-0.076 (0.023)	-0.122 (0.025)
Log SNF Payments per Medicare enrollee	-0.076 (0.035)	-0.109 (0.038)	-0.155 (0.044)
Log Used SNF per Medicare enrollee	-0.018 (0.011)	-0.024 (0.014)	-0.035 (0.017)

Notes: This table shows several averages of post-period coefficients assessing the impact of each policy on several measures of detrended SNF utilization. Analysis is restricted to the approximately 17 percent of enrollees each year who would be SNF eligible. The averages reported use the $\beta_{r, SF}$ ’s from equation 4 for the Strike Force, and the $\theta_{r, M}$ ’s from equation 4 for the moratorium. Standard errors clustered at the district level are displayed in parentheses below each estimate. The sample size is 8,768 county-years.

Table OA.17: Causal Forest Groups

Group	Calendar Year	Year Targeted by SF	Year Targeted by M	Relative Years	Treated Counties	Share of Treated Pop
1	2010	2009	—	$R_{\text{SF}} = 2, R_{\text{M}} = 0$	94	3.89%
2	2010	2010	—	$R_{\text{SF}} = 1, R_{\text{M}} = 0$	49	2.61%
3	2011	2009	—	$R_{\text{SF}} = 3, R_{\text{M}} = 0$	94	3.96%
4	2011	2010	—	$R_{\text{SF}} = 2, R_{\text{M}} = 0$	49	2.62%
5	2011	2011	—	$R_{\text{SF}} = 1, R_{\text{M}} = 0$	117	2.06%
6	2012	2009	—	$R_{\text{SF}} = 4, R_{\text{M}} = 0$	94	4.01%
7	2012	2010	—	$R_{\text{SF}} = 3, R_{\text{M}} = 0$	49	2.64%
8	2012	2011	—	$R_{\text{SF}} = 2, R_{\text{M}} = 0$	117	2.10%
9	2013	2009	2013	$R_{\text{SF}} = 5, R_{\text{M}} = 1$	2	0.20%
10	2013	2011	2013	$R_{\text{SF}} = 3, R_{\text{M}} = 1$	6	1.18%
11	2013	2009	—	$R_{\text{SF}} = 5, R_{\text{M}} = 0$	92	3.81%
12	2013	2010	—	$R_{\text{SF}} = 4, R_{\text{M}} = 0$	49	2.65%
13	2013	2011	—	$R_{\text{SF}} = 3, R_{\text{M}} = 0$	111	0.92%
14	2013	2013	—	$R_{\text{SF}} = 1, R_{\text{M}} = 0$	13	0.18%
15	2014	2009	2013	$R_{\text{SF}} = 6, R_{\text{M}} = 2$	2	0.19%
16	2014	2011	2013	$R_{\text{SF}} = 4, R_{\text{M}} = 2$	6	1.07%
17	2014	2009	2014	$R_{\text{SF}} = 6, R_{\text{M}} = 1$	13	1.21%
18	2014	2011	2014	$R_{\text{SF}} = 4, R_{\text{M}} = 1$	5	0.43%
19	2014	2009	—	$R_{\text{SF}} = 6, R_{\text{M}} = 0$	79	2.47%
20	2014	2010	—	$R_{\text{SF}} = 5, R_{\text{M}} = 0$	49	2.65%
21	2014	2011	—	$R_{\text{SF}} = 4, R_{\text{M}} = 0$	106	0.48%
22	2014	2013	—	$R_{\text{SF}} = 2, R_{\text{M}} = 0$	13	0.18%
23	2014	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 1$	3	0.13%
24	2015	2009	2013	$R_{\text{SF}} = 7, R_{\text{M}} = 3$	2	0.18%
25	2015	2011	2013	$R_{\text{SF}} = 5, R_{\text{M}} = 3$	6	1.04%
26	2015	2009	2014	$R_{\text{SF}} = 7, R_{\text{M}} = 2$	13	1.18%
27	2015	2011	2014	$R_{\text{SF}} = 5, R_{\text{M}} = 2$	5	0.42%
28	2015	2009	—	$R_{\text{SF}} = 7, R_{\text{M}} = 0$	79	2.42%
29	2015	2010	—	$R_{\text{SF}} = 6, R_{\text{M}} = 0$	49	2.66%
30	2015	2011	—	$R_{\text{SF}} = 5, R_{\text{M}} = 0$	106	0.48%
31	2015	2013	—	$R_{\text{SF}} = 3, R_{\text{M}} = 0$	13	0.17%
32	2015	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 2$	3	0.13%
33	2016	2009	2013	$R_{\text{SF}} = 8, R_{\text{M}} = 4$	2	0.18%
34	2016	2011	2013	$R_{\text{SF}} = 6, R_{\text{M}} = 4$	6	1.08%
35	2016	2009	2014	$R_{\text{SF}} = 8, R_{\text{M}} = 3$	13	1.20%
36	2016	2011	2014	$R_{\text{SF}} = 6, R_{\text{M}} = 3$	5	0.43%
37	2016	2009	2016	$R_{\text{SF}} = 8, R_{\text{M}} = 1$	71	1.01%
38	2016	2010	2016	$R_{\text{SF}} = 7, R_{\text{M}} = 1$	35	1.74%
39	2016	2011	2016	$R_{\text{SF}} = 6, R_{\text{M}} = 1$	107	0.48%
40	2016	2009	—	$R_{\text{SF}} = 8, R_{\text{M}} = 0$	8	1.53%
41	2016	2010	—	$R_{\text{SF}} = 7, R_{\text{M}} = 0$	14	1.00%

42	2016	2013	—	$R_{\text{SF}} = 4, R_{\text{M}} = 0$	13	0.18%
43	2016	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 3$	3	0.14%
44	2016	—	2016	$R_{\text{SF}} = 0, R_{\text{M}} = 1$	263	2.40%
45	2017	2009	2013	$R_{\text{SF}} = 9, R_{\text{M}} = 5$	2	0.17%
46	2017	2011	2013	$R_{\text{SF}} = 7, R_{\text{M}} = 5$	6	1.08%
47	2017	2009	2014	$R_{\text{SF}} = 9, R_{\text{M}} = 4$	13	1.20%
48	2017	2011	2014	$R_{\text{SF}} = 7, R_{\text{M}} = 4$	5	0.44%
49	2017	2009	2016	$R_{\text{SF}} = 9, R_{\text{M}} = 2$	71	1.01%
50	2017	2010	2016	$R_{\text{SF}} = 8, R_{\text{M}} = 2$	35	1.74%
51	2017	2011	2016	$R_{\text{SF}} = 7, R_{\text{M}} = 2$	106	0.49%
52	2017	2009	—	$R_{\text{SF}} = 9, R_{\text{M}} = 0$	8	1.57%
53	2017	2010	—	$R_{\text{SF}} = 8, R_{\text{M}} = 0$	14	1.02%
54	2017	2013	—	$R_{\text{SF}} = 5, R_{\text{M}} = 0$	13	0.18%
55	2017	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 4$	3	0.14%
56	2017	—	2016	$R_{\text{SF}} = 0, R_{\text{M}} = 2$	263	2.42%
57	2018	2009	2013	$R_{\text{SF}} = 10, R_{\text{M}} = 6$	2	0.17%
58	2018	2011	2013	$R_{\text{SF}} = 8, R_{\text{M}} = 6$	6	1.07%
59	2018	2009	2014	$R_{\text{SF}} = 10, R_{\text{M}} = 5$	13	1.17%
60	2018	2011	2014	$R_{\text{SF}} = 8, R_{\text{M}} = 5$	5	0.43%
61	2018	2009	2016	$R_{\text{SF}} = 10, R_{\text{M}} = 3$	71	0.97%
62	2018	2010	2016	$R_{\text{SF}} = 9, R_{\text{M}} = 3$	35	1.75%
63	2018	2011	2016	$R_{\text{SF}} = 8, R_{\text{M}} = 3$	106	0.48%
64	2018	2009	—	$R_{\text{SF}} = 10, R_{\text{M}} = 0$	8	1.60%
65	2018	2010	—	$R_{\text{SF}} = 9, R_{\text{M}} = 0$	14	1.02%
66	2018	2013	—	$R_{\text{SF}} = 6, R_{\text{M}} = 0$	13	0.17%
67	2018	2018	—	$R_{\text{SF}} = 1, R_{\text{M}} = 0$	30	2.14%
68	2018	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 5$	3	0.15%
69	2018	—	2016	$R_{\text{SF}} = 0, R_{\text{M}} = 3$	263	2.39%
70	2019	2009	2013	$R_{\text{SF}} = 11, R_{\text{M}} = 7$	2	0.17%
71	2019	2011	2013	$R_{\text{SF}} = 9, R_{\text{M}} = 7$	6	1.06%
72	2019	2009	2014	$R_{\text{SF}} = 11, R_{\text{M}} = 6$	13	1.17%
73	2019	2011	2014	$R_{\text{SF}} = 9, R_{\text{M}} = 6$	5	0.43%
74	2019	2009	2016	$R_{\text{SF}} = 11, R_{\text{M}} = 4$	71	0.95%
75	2019	2010	2016	$R_{\text{SF}} = 10, R_{\text{M}} = 4$	35	1.75%
76	2019	2011	2016	$R_{\text{SF}} = 9, R_{\text{M}} = 4$	107	0.48%
77	2019	2009	—	$R_{\text{SF}} = 11, R_{\text{M}} = 0$	8	1.59%
78	2019	2010	—	$R_{\text{SF}} = 10, R_{\text{M}} = 0$	14	1.03%
79	2019	2013	—	$R_{\text{SF}} = 7, R_{\text{M}} = 0$	13	0.16%
80	2019	2018	—	$R_{\text{SF}} = 2, R_{\text{M}} = 0$	30	2.05%
81	2019	—	2014	$R_{\text{SF}} = 0, R_{\text{M}} = 6$	3	0.15%
82	2019	—	2016	$R_{\text{SF}} = 0, R_{\text{M}} = 4$	264	2.37%

Table OA.18: Example HCFAC Cost Report

FY 2009 ALLOCATION OF HCFAC APPROPRIATION⁵ (Dollars in millions)			
Organization	Mandatory Allocation	Discretionary Allocation	Total Allocation
Department of Health and Human Services			
Office of Inspector General ⁶	\$177.205	\$18.967	\$196.172
Office of the General Counsel	5.714	0	5.714
Administration on Aging	3.200	0	3.200
Centers for Medicare & Medicaid Services (CMS)	24.979	160.066	185.045
Subtotal	\$211.097	\$179.033	\$390.130
Department of Justice			
United States Attorneys	\$33.800	\$9.023	\$42.823
Civil Division	\$15.069	\$8.237	\$23.306
Criminal Division	\$3.780	\$1.606	\$5.386
Civil Rights Division	\$2.376	\$0.100	\$2.476
Nursing Home and Elder Justice Initiative	\$1.000	\$0	\$1.000
Subtotal	\$56.025⁷	\$18.966	\$74.991
TOTAL	\$267.122	\$197.999	\$465.121

Notes: This table presents an example of funding estimates from the Health Care Fraud and Abuse Control Program, published by the U.S. Department of Health and Human Services and the Department of Justice each year. We use these reports to estimate a back-of-the-envelope cost for the Strike Force.

Table OA.19: Distribution of policies under observed and optimal placement, Moratorium First

	Optimal Placement			
	Strike Force Only	Moratorium Only	Strike Force and Moratorium	No Policy
Panel A. Ever-treated counties				
<i>Observed Placement</i>				
Strike Force Only (34%)	0.16	0.00	0.69	0.15
Moratorium Only (19%)	0.12	0.02	0.59	0.27
Strike Force and Moratorium (47%)	0.16	0.00	0.65	0.18
Ever-treated counties (100%)	0.16	0.01	0.65	0.19
Panel B. All counties				
<i>Observed Placement</i>				
Strike Force Only (12%)	0.06	0.00	0.41	0.52
Moratorium Only (6%)	0.05	0.01	0.42	0.52
Strike Force and Moratorium (16%)	0.10	0.00	0.45	0.45
No Policy (66%)	0.05	0.01	0.11	0.83
Ever-treated counties (34%)	0.08	0.00	0.43	0.49

Notes: This table replicates Table OA.8 but assigns moratorium, rather than strike force, first in the optimal placement.