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ABSTRACT

Governments often concurrently deploy multiple policy instruments to tackle a common objective, yet researchers typically analyze each policy's impacts independently. We study interactions across policies and their implications within the context of efforts to reduce Medicare-financed home health services. We consider two geographically targeted policies: strike force prosecutions of suspected fraud and moratoria on the entry of new home health agencies. Depending on location and time, we observe either both, one, or neither policy in place. Individually, each policy reduced home health use substantially, and was well-targeted to places with higher treatment effects. Although we estimate only a modest interaction between the two policies, we find that optimally allocating them across the areas that received at least one policy could have increased their total impact by about 20% relative to the observed placement. Our exercise highlights the potential gains from coordination across different policy instruments pursuing similar objectives.

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1 Introduction

Governments often deploy multiple policies with a common objective. For example, carbon pricing, technology subsidies, and performance standards are all applied on (sometimes the same) firms in an effort to reduce greenhouse gases. Likewise, wage subsidies, job training programs, and earned income tax credits (EITC) may be applied to (sometimes the same) unemployed, low-wage workers to try to boost their employment. A key focus of economic analysis is to compare alternative policy instruments to determine which is most effective and should be prioritized (Hendren and Sprung-Keyser 2020).

Yet such analyses typically contrast effects of different policies that have been estimated in isolation, without considering how related policies may interact with each other and affect the optimal deployment of each. We study this issue in the context of the United States Medicare program, which in the early 2000s rolled out a series of policies designed to rein in the rapid growth of home health care and the accompanying concerns over waste, fraud, and abuse (e.g., OIG 2016; CMS 2021; Lee and Skinner 2021; HHS & DOJ 2022).

Home health looms large in the lives of the elderly. It consists of services provided by licensed medical providers at the patient's residence, delivered to patients who are considered healthy and independent enough to live at home, but sufficiently sick and immobile that they cannot travel to receive the medical care they need. In 2019, one-in-twelve Medicare enrollees aged 65 and over – and one-in-five of those aged 85 and over – used Medicare-financed home health. That year, Medicare spent \$20 billion on home health, accounting for almost one-third of its spending on all post-acute care (MedPAC 2022).

Home health occupies an nebulous position on the spectrum from medical inputs to consumption goods. It includes many services – such as physical therapy or help with home chores – that could also benefit fully healthy individuals, increasing the potential for waste, fraud, and abuse beyond that of typical medical care which, for most healthy people, has little direct utility value. Accordingly, over the last half century, the US government has engaged in a series of Medicare policy reforms designed to better target home health to those who most need it.[1] In the time series, these policies are associated with dramatic swings in the amount of Medicare-financed home health care use (see Figure 1), suggesting that policy can have a first-order impact on it.

We focus on two key, geographically-targeted policies deployed by the same government agencies in the second decade of the 21st century: strike force prosecutions of suspected fraud, introduced between 2009 and 2018, and moratoria on the entry of new home health agencies, introduced in 2013 and 2016. Using data on a random 20% sample of Medicare claims from 2004 through 2019, we exploit variation in the timing and spatial application of these policies to estimate their impacts across patients – in isolation as well as when deployed together – and to consider the counterfactual

[1] So too have most other high-income countries, which exhibit substantial heterogeneity in their home health policies and, presumably relatedly, the prevalence of home health (Gruber et al. 2023).

performance of alternative geographic targeting of the policies.

Crucially, depending on the location and time period, we observe counties with no policy, one of the policies but not the other, or both policies in place. This provides a rare opportunity to study the impacts – both individually and in combination – of multiple policies with a common objective. At one extreme, if the policies target very different types of patients or care, they may operate largely independently of each other and their combined impact may be close to the sum of their individual effects. At the other extreme, the two policies might be perfect substitutes, so that applying them in the same county would only achieve the maximum impact across either policy in isolation. Naturally, where reality lies in between these extremes will be consequential for the optimal placement of these policies.

A key empirical challenge is that, not surprisingly (and perhaps reassuringly), these policies were targeted at areas with large amounts of suspected waste based on their high and rapidly growing rates of home health use. Thus, while there is clear evidence of policy impacts on home health use – a standard event study analysis displays a visible break and reversal in trend coincident with each policy's introduction – quantifying policy effects, either in isolation or in combination, requires that we model this endogeneity. We do so by applying a county-specific de-trending procedure to the outcomes. We then allow for flexible interactions between the two policies, and also allow for heterogeneous treatment effects of each policy, individually and jointly, across observable characteristics of patients.

Our estimates allow us to compare reductions in home health under observed and counterfactual placements of the policies. We document substantial heterogeneity across patients in treatment effects, with larger (in absolute value) treatment effects for patients who are older and sicker. Our counterfactual findings indicate that the geographically-focused strike forces and moratoria were targeted at places with higher-than-average treatment effects of these policies. While our estimates suggest that the interaction between the two policies is not large, we also find that optimal placement of these policies across the areas that received at least one policy could have increased their overall impact by about 20% relative to their actual, observed placement. Our analysis highlights the potential gains from coordination across different policy instruments pursuing similar objectives. Our general approach may be usable in other, similar contexts where multiple policies are deployed with a common objective.

Our paper contributes to a growing literature analyzing the optimal targeting of policies. This literature has explored optimal targeting on observables (e.g., Kitagawa and Tetenov 2018; Athey and Wager 2021; Johnson et al. 2023; Haushofer et al. 2025), on unobservables (e.g., Einav et al. 2022; Ito et al. 2023), and on both (Ida et al. 2022). A key theme has been the importance of targeting policies based on predicted treatment effects (the $\hat{\beta}$'s or "slopes") rather than the more easily-observed predicted outcomes (the $\hat{y}$'s or "levels"). Our exercise highlights for this literature that optimal targeting should also consider *incremental*, rather than gross, treatment effects when multiple policies are deployed in service of a common objective.

Our specific application tackles a central topic in health economics and health policy: how to effectively combat the substantial waste in the US health-care system, with waste commonly defined as medical spending that provides little or no benefit to patient health. Health-care spending poses one of the largest fiscal challenges facing the public sector in the US, accounting for almost one-fifth of the economy, with half of that spending financed by taxpayers (Himmelstein and Woolhandler 2016). There is widespread consensus that a large portion of that spending is waste (e.g., Orszag 2009; McGinnis et al. 2013; Shrank et al. 2019), yet little agreement on how to design policies to reduce waste without also reducing patient access to valuable care (e.g., Cutler 2010; Doyle et al. 2017). Our paper contributes to a literature that has sought to identify and combat waste in health care spending in a variety of domains, including over-billing (Fang and Gong 2017), excessive administrative costs (Dunn et al. 2024), excess payments to medical device suppliers (Ji 2025), and unnecessary hospital spending (Shi 2024), among other areas.

Finally, our paper also relates to the literature on health care fraud and policies designed to combat it. In the context of home health, O'Malley et al. (2023) document the diffusion of home health fraud across home health agencies, while Kim and Norton (2015) study the impact of the national outlier payment cap on home health utilization.[2] Outside of home health, recent papers have studied the impact of whistle-blower filings against health-care providers under the False Claims Act (Howard and McCarthy 2021; Leder-Luis 2025), policies designed to combat fraudulent use of Medicare-financed ambulances as taxi services for patients' regular dialysis treatments (Eliason et al. 2025), and the use of revenue caps and anti-fraud litigation in reducing use of Medicare-financed hospice (Gruber et al. 2025).

The rest of the paper proceeds as follows. Section 2 describes our setting, and Section 3 describes our data. Section 4 documents the impact of each of the two policies, individually. Section 5 estimates interactions across these policies and uses the estimates to analyze the performance of counterfactual policy placements. The final section concludes.

2 Setting

The aging of the US population has brought with it a rapid growth in the use of paid long-term care. Long-term care includes care in an institutional setting (typically a nursing home) as well as care received in the home by paid caregivers.[3] Both types of care may be provided as part of rehabilitation and palliative services following a hospital stay or acute illness, or they may be

[2]There is also a literature that examines the impact of policies that reduce home health use on downstream outcomes, such as substitution to skilled nursing facility (Kemper 1988; McKnight 2006), and fails to find any evidence of such substitution. As we discuss in more detail below, we are not well-powered to examine downstream consequences of the policy-induced home health reductions we estimate. However, consistent with this prior literature, our evidence does not point to obvious, quantitatively important substitution to skilled nursing facilities.

[3]It also includes informal, unpaid care received in the home, often provided by relatives.

focused on managing more chronic conditions.

About two-thirds of long-term care is publicly financed. Medicare – the federal public health insurance program for the elderly and disabled – pays for a little over half of that care. Medicaid – the joint federal-state public health insurance program for low-income individuals – pays for the rest (CRS 2021; Gruber and McGarry 2023). Medicare coverage is limited to individuals who need long-term care following an acute illness and are expected to recover; it is not designed for patients who are expected to need on-going assistance. Medicaid, by contrast, will cover patients with chronic needs, including basic custodial help with activities of daily living.

In 2019 (the end of our study period), institutional care accounted for about three-fifths of spending on paid long-term care, and home health accounted for the remaining two-fifths (CMS 2023b). Home health is designed to treat or manage an illness, injury, or medical condition, and encompasses a range of services, including skilled nursing, physiotherapy, speech-language pathology, occupational therapy, and home health aide services (Sengupta et al. 2022). Upon a referral from a physician, a patient typically contacts a home health agency (HHA) that serves their area in order to schedule the home visit(s), during which service is provided.

2.1 Medicare home health

Medicare imposes eligibility requirements on both the patient and the provider of home health. For a Medicare enrollee to qualify for Medicare home health, they must have difficulty leaving the home without considerable effort, and they must require part-time or intermittent skilled nursing care. An eligible health-care provider must certify this need initially, and re-certify it at least every 60 days (42 CFR 424.22 2020). The certification (and re-certification) must detail the patient's condition and needs, as well as an estimated frequency and duration of needed home health services. Unlike the vast majority of Medicare-covered services, home health is free for the patient, without any patient cost sharing.[4] The combination of the lack of monetary cost (to the patient) and the risk-free aspects of home health services make home health a potentially attractive target for fraud (MedPAC 2017).

To provide service to Medicare patients, a home health agency must be primarily engaged in providing skilled nursing services and therapeutic services, and employ at least one physician and one registered professional nurse to oversee its services. The agency must also be licensed in the state or locality in which it operates; as a result, a given home health agency tends to operate within a single state, as each state has its own rules for licensure of home health agencies (Famakinwa 2023). To become Medicare-certified, agencies must submit a Medicare enrollment application, which is reviewed and approved by Medicare (CMS 2020). By the end of our sample period (2019), the home health industry included approximately 1.4 million employees, who were working for over

[4] An exception is any durable medical equipment or biologic drugs that are provided in connection with the home health visits (CMS 2022a; CMS 2023a), for which the "standard" 20% coinsurance rate applies.

11,000, predominantly for-profit home health agencies (HHAs) (BLS 2019; CDC 2020).

Since October 2000, Medicare has paid HHAs based on a prospective payment system. During our study period, payments were based on 60-day episodes, with the base rate adjusted for the patient's geographic location and 153 possible "resource groups" that capture the clinical and functional status of the patient as well as their expected service needs. In 2019, the national base rate for a 60-day episode was approximately $3,000 (MedPAC 2017; Medicare Learning Network 2019). An episode may qualify, ex-post, for additional, outlier payments if episode costs exceed a fixed dollar threshold amount above their expected cost; in such cases, the agency receives 80% of the estimated costs over the threshold (MedPAC 2017).

2.2 Medicare home health policy reforms

As evident in Figure 1, Medicare home health use has experienced significant fluctuations over the last half-century that coincide with specific changes in Medicare payment policies. There was a steep increase in the share of Medicare patients using home health immediately following a 1988 class action lawsuit that clarified that patients were allowed to receive home health services for stable needs; Medicare coverage did not require the patient's condition to be improving (Liu et al. 1999). The increase in use – and concomitant increase in Medicare home health spending – ultimately prompted Congress to reform the home health payment system in the Balanced Budget Act of 1997. This 1997 act changed the reimbursement of home health agencies from cost-based reimbursement with a per-visit cap to first an Interim Payment System (IPS), from 1997 to 2000, and then to the Home Health Prospective Payment System, from 2000 to the present. The IPS lowered the per-visit cap and also introduced an agency-level annual per-patient cap, resulting in a steep decline in the total number of home health payments as well as the number of visits per home health patient (McCall et al. 2001; Liu et al. 2003; McKnight 2006; McGinnis et al. 2013). However, following the implementation of the current prospective payment system based on 60-day episodes in 2000, Figure 1 shows that home health utilization began to rapidly increase. This prompted yet another round of policy reforms during our 2004-2019 study period.

We focus on two policy efforts that were selectively applied to specific geographic areas at different points in time between 2009 and 2018: strike forces and moratoria on new home health agencies. Both represented joint efforts between the Department of Justice (DOJ) and the Department of Health and Human Services' Office of the Inspector General (OIG), and both aimed to reduce perceived fraud and abuse in home health. In 2010, CMS also implemented a nationwide outlier payments cap that stipulated that outlier payments could not make up more than 10% of total Medicare home health payments to an agency in a given year (MedPAC 2019) and that contributed to reductions in subsequent home health use (Kim and Norton 2015).

Strike Forces. Strike forces were designed to detect and prosecute suspected home health fraud, a suspicion fueled by the dramatic growth in home health spending during the early 2000s as well as its concentration in particular regions.[5] One key suspect area was Miami-Dade county in Florida, where home health spending per beneficiary quadrupled between 2002 and 2008. Another suspect area was certain counties in Texas that likewise exhibited high growth in spending (MedPAC 2010b; Lee and Skinner 2021). Home health fraud can take different forms, but the majority of cases pursued by the OIG and the DOJ involved some combination of billing for excessive and unnecessary care, providing care to ineligible patients, and billing for services that were never provided (HHS & DOJ 2010; HHS & DOJ 2012). While it is difficult to accurately measure the government costs of prosecuting home health through Strike Forces, our rough back-of-the-envelope calculation (see Appendix A) suggests that each Strike Force spends about $10 million per year targeting suspected home health fraud.

The first strike force started in 2007 in the Southern District of Florida (which includes Miami), and initially focused on fraud in durable medical equipment before shifting its focus to home health fraud in 2009.[6] Over the next nine years, strike forces targeting home health were launched in 11 other federal judicial districts, chosen based on aberrant Medicare claims or other intelligence (HHS & DOJ 2010; HHS & DOJ 2011). Each Strike Force team combined the investigative and analytical resources of multiple agencies, and brought forward successful cases to federal district courts (HHS & DOJ 2010; HHS & DOJ 2011). By 2019, Strike Force teams operating in the 12 districts had filed over 2,800 cases for about $3.5 billion in losses (Office of the Inspector General 2020). Appendix Table OA.1 gives the timing and location of each strike force targeting home health that opened during our study period.

Moratoria. Moratoria on the entry of new home health agencies were motivated by the observation that the number of home health agencies was rapidly increasing, especially in Texas, Florida, and Michigan (MedPAC 2010a). The first wave of moratoria was introduced in 2013 in selected counties that encompassed large cities in Florida (Miami and Fort Lauderdale), Texas (Dallas and Houston), and the Midwest (Chicago and Detroit). In 2016, the moratoria expanded to include all counties in the states of Illinois, Texas, Florida, and Michigan (CMS 2016). Appendix Table OA.2 gives the timing and location of each moratorium on home health agency entry enacted during our study period.

[5] The use of Strike Forces in Medicare has not been limited to home health. During our study period, home health comprised of about half of the cases pursued by Strike Forces (DOJ 2013; DOJ 2016; HHS & DOJ 2019.)

[6] For this reason, we use 2009 as the start date for our analysis of the strike force.

3 Data

Data sources. We primarily rely on two administrative data sets from Medicare, covering the 2004-2019 period. The first is the annual Master Beneficiary summary file, which covers the universe of Traditional Medicare enrollees and contains information on patient demographics – including zip code of residence, race, and gender – as well as the number of comorbidities each enrollee has that year and summary measures for each enrollee's annual health-care utilization and spending, overall and in separate categories (including home health and skilled nursing facilities (SNFs)).

The second data source is health-care claims from a 20% random sample of Traditional Medicare beneficiaries. These claims provide more granular data on episode-level home health claims, including the start and end dates of each episode, the number of visits and services provided, and the specific HHA that provided the care.

Analysis sample and key variables. We restrict attention to beneficiary-years aged 65 or older, who were enrolled in Medicare Parts A and B throughout the entire year. This excludes Medicare beneficiaries who are only Medicare eligible due to disability or end-stage renal disease, as well as beneficiary-years who were enrolled in Medicare Advantage[7] for all or part of the year.[8]

Our outcomes of interest are measures of home health spending and use. We analyze these outcomes at the county-year level; the moratoria were targeted at counties, while the strike forces were targeted at a subset of the 89 federal districts, each of which is a collection of counties. To account for the vast heterogeneity in population across counties, we normalize each county-year outcome by the number of Traditional Medicare enrollees (hereafter "enrollees") in that county-year. Our primary outcome of interest is home health spending per enrollee in each county-year. We also analyze home health visits per enrollee (which is highly correlated with spending per enrollee), as well as an extensive margin measure of the share of enrollees in the county-year who received any home health that year. All monetary values are inflation-adjusted to 2019 using the CPI-U.

Summary statistics. HHAs cover a fairly large area, serving, on average over our sample period, patients across 5 counties and 20 zip codes. For this reason, the vast majority of patients have access to multiple HHAs, with the average zip code served by 5-7 distinct HHAs at a time, and the average county by almost 20.[9] The average agency employed about 20-25 direct care workers

[7]Medicare Advantage (also known as Medicare Part C) refers to Medicare health insurance plans offered by private insurance companies.

[8]We also exclude from the sample all home health care claims from home health agencies affiliated with Amedisys. A 2014 DOJ civil False Claims Act settlement alleged Medicare fraud occurring across numerous Amedisys care centers nationwide, but neither the press release nor the underlying complaint identifies the specific HHAs involved (U.S. Department of Justice 2014). Because the allegations pertain to the chain broadly—and the case resulted in a civil settlement rather than a criminal conviction—we cannot determine which individual agencies were implicated. To avoid misclassification, we drop all Amedisys HHAs.

[9]The HHA market is reasonably concentrated. The average patient resides in a zip code where the largest HHA has about a 50% Medicare patient market share, and the largest three HHAs account for more than 70% of the

(not counting administrative staff) each year. The national trend in home health use in the first two decades of the 21st century (recall Figure 1) is also reflected in counts of unique HHAs, which almost doubled in our data from just over 6,000 in the early 2000s to almost 12,000 by 2010, before gradually dropping to 9,000 by 2019 (Appendix Figure OA.1).

Figure 2 shows the geographic placement of strike forces and moratoria as of the end of our study period in 2019. About one-third of counties (weighted by 2006 Medicare enrollment) received at least one policy. In what follows, we will refer to the counties that received a strike force, a moratorium, or both by 2019 — i.e. the colored areas on the map — as "ever-treated" or treatment counties, and the rest as "never treated" or control counties. There is a high amount of overlap in the placement of policies within the ever-treated counties; among those that received at least one policy, about half of the enrollee-weighted counties received both policies. This will be a key focus of our analysis.

Figure 3 shows trends over our study period in annual home health use, separately for people in ever-treated counties (red line) and in never-treated counties (green line). It shows the share of Medicare enrollees using home health care, average home health visits per enrollee, and average home health payments per enrollee. All three measures grow rapidly prior to the introduction (in 2009) of the policies we study, with both the 2004 starting level and the subsequent growth rate much higher in ever-treated counties.[10] Our empirical strategy will therefore need to account for these differential levels and trends in treatment vs. control counties.

4 Studying individual policies

We begin by estimating the average effect of each policy, controlling for the other. This provides visual evidence that these policies had meaningful impacts before we move on to more flexible analyses that will allow for policy interactions. Of course, our impact estimates of each policy will reflect an average over instances when one policy is imposed in isolation and instances when the other policy is present. Estimating how policy effects vary across these cases will be the focus of the next section.

Basic event studies. We begin with a standard event study analysis exploiting variation across time and geography in the application of each policy. Specifically, we estimate:

$$\ln(y_{ct}) = \alpha_c + \lambda_t + \sum_{r^{SF} \neq 0} \beta_{r^{SF}} SF_{ct} + \sum_{r^{M} \neq 0} \theta_{r^{M}} M_{ct} + \epsilon_{ct}, \tag{1}$$

where α_c and λ_t denote county and calendar year fixed effects respectively, and r^{SF} and r^{M} denote

market.

[10] Appendix Table OA.3 shows that home health spending per home health patient and home health visits per home health patient also grew prior to 2009, and grew more rapidly in ever-treated counties.

the year relative to the first year of a strike force and moratorium (which we denote as relative year 1), respectively.[11] SF_{ct} and M_{ct} are binary indicators for a strike force or a moratorium being present in county c and in year t. We control for each policy in estimating the average impact of the other since, as seen in Figure 2, a given county may be treated by both policies.[12] We normalize effects to be zero for each policy in relative year 0, the year prior to implementation (i.e. we normalize the coefficients β_0 and θ_0 to 0). We bottom-code all relative years prior to year -5 as -5, and top-code all years after relative year 4 as 4; we include both "end point" indicators in the regression but only report coefficients between relatives years -4 and 4.[13] We estimate the model in logs to allow for proportional calendar time effects and policy effects across counties with very different pre-policy outcome levels.[14] In this and all subsequent analyses, we weight each county-year observation by the number of Medicare enrollees in the county in 2006, and we cluster the standard errors on the level of the district.

Figure 4 shows the results. Specifically, we show the estimated impact of the strike forces (in the left hand panels) and the moratoria (in the right hand panels) on the log of three outcomes: home health visits per Medicare enrollee (top panel), home health payments per Medicare enrollee (middle panel), and the share of Medicare enrollees using any home health (bottom panel).

The strike force estimates show a pronounced drop in visits and payments starting immediately in the policy's first year ($r = 1$), and a subsequent gradual drop over subsequent years.[15] Of course, what is also immediately clear in these figures is the equally pronounced upward trend in these outcomes *prior* to the strike force introduction. This is not surprising. As discussed, it reflects an explicit policy strategy of targeting areas where home health was not only high but also growing rapidly. While it is hard to look at the inverted-V pattern for home health visits and payments and believe that the strike force had no impact, it is difficult to assess the magnitude of the policy's impact without modeling how that pre-policy trend would have evolved absent the policy intervention. By the same token, the extensive margin results in the bottom left, which show a strong upward pre-strike-force trend that immediately flattens out, suggest impacts on the extensive margin as well, but the exact magnitude is again difficult to assess.

The moratoria estimates (right hand panels) also show some evidence of a break in trend after

[11]Specifically $r^{SF} \equiv t - t_0^{SF}(c)$ is the year relative to the year in which the strike force opened in county c, and $r^M \equiv t - t_0^M(c)$ is analogously the year relative to the year in which a moratorium was imposed in county c.

[12]This will be key in the next section for estimating policy interactions. For now, however, we simply estimate the average impact of each policy, conditional on the timing and targeting of the other policy.

[13]The period is chosen because a large number of counties received moratoria in 2016 (see Appendix Table OA.2) and our sample period ends in 2019, making year 4 the last year observed for many counties. We do not report the coefficient on relative year -5 given compositional effects that arise from the bottom coding of pre-policy years.

[14] We exclude approximately 0.4% of (population-weighted) counties in which there is no home health use in at least one study year. In robustness analysis below, we show that results are very similar if instead we include these counties and estimate a Poisson model.

[15]The confidence interval increases substantially in relative year 4 because it reflects the estimate for relative years 4 and later, and there are heterogeneous effects over time. Appendix Figure OA.2 shows that if we instead have a separate indicator for relative year 4 and then pool subsequent relative years 5 and later, the confidence interval on year 4 is substantially smaller.

implementation, but show much less of a pre-trend. This likely reflects the fact that about three-quarters of (enrollee-weighted) counties with a moratorium also had a strike force (see Figure 2), and in all such cases, the strike force predated the moratorium (see Appendix Tables OA.1 and OA.2). As a result, the controls for year relative to strike force in equation (1) are absorbing what might otherwise have been a pre-moratorium trend in Figure 4. This also suggests that accounting for endogenous placement of the *first* policy a county receives may be sufficient.

De-trending. We account for the endogenous nature of the first policy a county received with a "de-trending" approach. Its chief attraction relative to more standard approaches, such as a matching estimator in which treatment counties are matched to control counties experiencing similar rates of pre-policy growth, is that it allows us to view treatment as randomly assigned to the de-trended data; this in turn allows us to apply the causal forest estimator of Athey et al. (2019) in Section 5. In practice, as we will see in our robustness analysis below, our de-trending approach yields similar results to a more standard matching approach.

We begin with a "data pre-processing" step to account for county-specific trends in the intensity of home health utilization prior to a county's first receipt of either a strike force or a moratorium. This provides a parametric way of accounting for the endogeneity of the policies' placement illustrated in Figure 3. We will use this "data pre-processing" step for all subsequent analyses in the paper, unless otherwise noted.

For each treatment county, we define a variable r_{ct} that indicates the number of years before the county received its first policy.[16] We then limit the sample to the five county-years prior to the policy in each treatment county, and to all county-years for control counties. Using this sample, we estimate the following regression:

$$\ln(y_{ct}) = \alpha'_c + \lambda'_t + \gamma_c D_c ln(r_{ct}) + \eta_{ct}, \tag{2}$$

where y_{ct} is a given measure of home health utilization in county c in year t, α'_c and λ'_t are, respectively, county and year fixed effects, D_c is an indicator variable equal to 1 for all treatment counties, r_{ct} takes the value of 2 through 6, which correspond to year -4 through 0 before the county receives its first policy, and γ_c is a county-specific parameter. This regression thus allows for a county-specific, logarithmic pre-trend in each treatment county.[17] We use a (county-specific) logarithmic pre-trend rather than a more standard linear one to be conservative; once used "out of sample" for post-policy observations (see below), a concave pre-trend (relative to a linear one) leads to more measured predictions of counterfactual increases, and therefore to smaller estimates

[16] If the county received a strike force, the first policy is always a strike force, as no county received a moratorium before the strike force. However, for the approximately one-sixth of (enrollee weighted) ever-treated counties that only received a moratorium, the first policy will be the moratorium.

[17] Given the existence of county fixed-effects, the only role of the control counties in this regression is to identify the year effects, λ'_t, so that any pre-trend in the treatment counties is measured "on top" of any national time effects.

of the effect of the policy.[18]

We use the estimates from equation (2) to construct predicted outcomes, $\widehat{\ln(y_{ct})}$. Appendix Figure OA.3 shows the fit of the predicted trends relative to the basic event studies. We use these predictions to "residualize" the home health outcome in all treatment county-years:

$$\widetilde{\ln(y_{ct})} = \ln(y_{ct}) - \widehat{\ln(y_{ct})}. \tag{3}$$

We use the word "residualize" in quotes because, critically, we use equation (3) to calculate $\widetilde{\ln(y_{ct})}$ for all treatment county-years, not only for the original sample of pre-treatment county-year observations used to estimate equation (2). The post-policy years in treatment counties were not used in estimating equation (2) precisely because they were potentially impacted by the policies. Thus, the $\widetilde{\ln(y_{ct})}$ is based on the estimated county-specific concave pre-trend for each treatment county (as well as calendar year and county fixed effects); it reflects predicted home health use in county c and calendar year t absent the policy introduction, which is counterfactual for the out-of-sample post-policy years in treatment counties.

We then use the resultant panel of county-year outcomes $\widetilde{\ln(y_{ct})}$ for the treated counties to estimate an event study for the average impact of each policy:

$$\widetilde{\ln(y_{ct})} = \beta'_{r^{SF}} + \theta'_{r^{M}} + u_{ct}. \tag{4}$$

We estimate equation (4) on all treated county-years, normalizing the coefficients β'_0 and θ'_0 to 0. Recall that equation (2) already included year and county fixed effects, so it is redundant to include them in equation (4).

Figure 5 shows the results. We see a fairly substantial reduction in home health visits and home health payments per enrollee following the introduction of both the strike force (left hand panels) and moratorium (right hand panels). Once again, this reduction is fairly gradual and becomes more pronounced over time. Appendix Table OA.4 summarizes the estimates. Column (2) indicates that, on average over the post-policy period, introducing a strike force into the county reduces home health payments by about 9% (standard error = 3%) and home health visits by about 14% (standard error = 3.3%), but has no impact on the share of patients using home health services (point estimate of 0.7%, standard error = 3.1%). The impact of imposing a moratorium in the county is somewhat greater; we estimate that on average the moratoria reduce home health spending by about 22% (standard error = 6.8%), home health visits by about 15% (standard error = 6.5%), and the share of patients using home health by about 13% (standard error = 5.3%).

These results are generally robust across alternative specifications. In particular, we find similar results when, instead of de-trending, we use a standard matching procedure that matches each

[18]Conceptually, it also seems undesirable to assume a linear trend in which, absent policy intervention, home health would go on growing at the same rate "forever."

treatment county to control counties experiencing a similar rate of pre-policy growth. We also show robustness to a jack-knife style analysis in which we sequentially leave out different treatment strike force districts (to confirm that the results are not entirely driven by one outlier district, such as Miami), to estimating a Poisson model rather than one in logs, and to alternative two-way fixed effect estimators. Appendix B provides more detail.

A natural question is to what extent the substantial, policy-induced reductions in home health documented in Figure 5 are "offset" by increased use of nursing homes (SNFs). We explore this briefly in Appendix C. We estimate that only 38% of home health payments are for home care that would be eligible for Medicare coverage if it instead took place in a nursing home, and that over three-quarters of the policy-induced decline in home health payments was in home health use that would not be eligible for Medicare coverage if it took place in a SNF. This suggests that policy-induced substitution to SNF care may be limited. Directly investigating whether the policy-induced reductions in home health use lead to substitution to nursing home use is challenging for two reasons. First, the need to account for the policies' endogenous placement makes it harder to be confident in evidence for (or against) subtler "downstream" effects. Second, our statistical power to detect potential downstream effects from reduced home health use is fairly weak. These caveats not withstanding, consistent with prior work studying earlier Medicare home health reforms (Kemper 1988; McKnight 2006), our analysis in Appendix C does not uncover compelling evidence of substitution to nursing home care. We thus abstract from any potential adverse consequences of the policy-induced reduction in home health in the remainder of our analysis.

5 Interaction across policies

5.1 Approach

Key to our ability to analyze how the strike forces and moratoria may interact is that, as seen in Figure 2, some areas received only the strike force, some received only the moratorium, and some received both (or neither).[19] However, no place was treated simultaneously by both policies, and, importantly, the empirical evidence from Section 4 indicates that policy impacts increase over the first few years. Therefore, in order to distinguish the effects of policy interactions from the dynamic time pattern by which each policy affects the use of home health, we first estimate policy effects separately for each *policy combination* in the data – defined by the set of policies in place and the number of years each policy has been in place – and then combine these estimates to infer the key objects of interest.

[19] In addition, two nationwide policies were introduced during our study period: the 2010 outlier payment cap (described in Section 2) and a 2011 requirement that the physician or other practitioner have a face-to-face encounter to document the patient's eligibility within 90 days before or 30 days after the initiation of home health care (Skillman et al. 2016). We do not incorporate either into our policy interactions analysis because they are applied nationwide during essentially our entire post-treatment analysis; our analyses can therefore be thought of as examining how the strike force and moratoria interact in an environment where both of these other policies are in place.

Specifically, let $r^{SF} \in A^{SF} = \{\emptyset, 1, 2, 3, 4+\}$ be the number of years (if any, up to 4) a strike force has been in place in a county, and let $r^{M} \in A^{M} = \{\emptyset, 1, 2, 3, 4+\}$ likewise be the number of years (if any, up to 4) a moratorium has been in place in a county. We then let $a \in A = A^{SF} \times A^{M}$ denote a particular combination of policies, with A denoting the full set of potential combinations. This set is defined by whether a strike force exists and, if it does, how many years it has been since its introduction; and whether a moratorium exists and, if it does, how many years it has been since its introduction. There are 25 potential policy combinations, of which we observe 16 in our data (see Appendix Table OA.5).

We denote by $\tau(a)$ the average reduction in home health use under policy combination a relative to having no policies in effect. To estimate $\tau(a)$ we assume (as in the analysis of each policy separately in Section 4) that after the de-trending exercise in equations (2) and (3), it is reasonable to view treatment as randomly assigned. The key objects of interest are how, in years 4 and later after the policy introduction (i.e. in "steady state"), the $\tau(a)$'s vary between strike force only ($a = (4+, \emptyset)$), moratorium only ($a = (\emptyset, 4+)$), and both ($a = (4+, 4+)$).[20] However, we can make use of all the policy combinations we observe in the data, not merely those in years 4 and later, by imposing a parametric functional form on how policy impacts evolve over time; this allows us to also utilize the estimated reduction in home health care use from policy combinations prior to year 4.

Specifically, we parametrize the effect of the strike force with the function $f(\alpha, R_{SF})$ and the effect of the moratorium with $g(\beta, R_M)$, as follows:

$$f(\alpha, r^{SF}) = \begin{cases} \alpha_1 + \alpha_2 r^{SF} & \text{if } r^{SF} \in \{1, 2, 3, 4+\} \\ 0 & \text{if } r^{SF} = \emptyset \end{cases} \tag{5}$$

and

$$g(\beta, r^{M}) = \begin{cases} \beta_1 + \beta_2 r^{M} & \text{if } r^{M} \in \{1, 2, 3, 4+\} \\ 0 & \text{if } r^{M} = \emptyset \end{cases}. \tag{6}$$

That is, α_1 and β_1 capture the initial effects of, respectively, the strike force and moratorium, and α_2 and β_2 capture how the effects change over time. We then estimate the following equation using nonlinear least squares:

$$-\tau\left(r^{\mathrm{SF}}, r^{\mathrm{M}}\right) = \max\{f(\alpha, r^{\mathrm{SF}}), g(\beta, r^{\mathrm{M}})\} + \delta \min\{f(\alpha, r^{\mathrm{SF}}), g(\beta, r^{\mathrm{M}})\}, \tag{7}$$

where $\delta \in [0, 1]$ captures the extent to which the two policies are substitutes or additively separable. When $\delta = 0$, the second component of the expression drops out, and the total treatment effect equals the greater of the two policy effects; in other words, the incremental effect of the less effective policy

[20] We assume, as in Section 4, that after 4 years in existence, each policy effect reaches its long-run impact.

is zero. When $\delta = 1$, the total treatment effect equals the sum of the two policy effects; in other words, the effects of the strike force and the moratorium are additively separable.

While we could in principle estimate equations (5), (6), and (7) at the county-year level, in practice we allow the estimated effects of each policy combination to vary by enrollee-type, where enrollee-type is a vector of enrollee covariates z_i. Intuitively, if the strike forces and moratoria impact the same type of enrollees (as defined by z_i), it is more likely that the policies will be substitutes (δ closer to zero), while if they impact very different types of enrollee, they are more likely to be additively separable (δ closer to 1). Additionally, incorporating enrollee-level heterogeneity allows us to account for potential differences in the patient population across counties receiving different policy combinations a, and hence potentially heterogeneous treatment effects across counties.[21]

To estimate heterogeneous treatment effects, we apply the causal forest framework of Athey et al. (2019). Specifically, we allow $\tau_i(a)$ to vary across enrollees[22] based on their demographics – age, sex, and whether they are enrolled in Medicaid – and their health – proxied by their Medicare spending in the prior year and the number of comorbidities the enrollee has that year.[23] These covariates form the vector z_i that defines an enrollee-type. The causal forest algorithm optimally splits the data along these chosen covariates in order to maximize the difference in treatment effects across splits, while guarding against over-fitting. Appendix D provides more detail on how we implement the causal forest.

We then estimate equations (5), (6), and (7) separately for each observed combination of enrollee-type (as defined by z_i).[24] Because we will estimate heterogeneous treatment effects across z_i, we are able to observe different combinations of R_{SF} and R_M for the same enrollee type. For every z_i, therefore, the resulting estimates allow us to generate separate estimates of τ for each policy combination a observed in the data; only one of these corresponds to the a that enrollee i actually experienced, which we refer to as the "factual" estimate, while the others correspond to predicted treatment effects under counterfactual policy combinations, estimated based on other enrollees who experienced that policy combination and have the same z_i.

[21]Otherwise, given that policies are not randomly assigned across counties with different underlying populations, we might mistakenly attribute the existence of heterogeneous treatment effects of policies across types of individuals to interaction effects between policies. If, for example, treatment effects of the individual policies were larger (smaller) in places that received both policies, we could infer that the two policies were complements (substitutes), when they are in fact additively separable.

[22]We refer to each enrollee-year as an individual i since the covariates used to estimate heterogeneous treatment effects are time-varying. In practice, of course, a given enrollee can appear across multiple years as distinct enrollees.

[23]We do not use an enrollee's geographic location as an input into the causal forest since our goal is counterfactual analyses of policy combinations across counties. In the same vein, we not include race or ethnicity in the individual demographics on which we examine heterogeneity because this can be highly correlated with place, in particular the first strike force location – Miami – has a very high share of enrollees who are Hispanic.

[24]As discussed in Appendix D, for computational speed, we estimate the causal forest on a random 5% sample of patient-years in our baseline sample. Moreover, when we estimate the parametric model in equations (5) through (7), we restrict to a random 0.1% of our baseline sample. A small subsample can be taken without loss of statistical accuracy as long as the full distribution of patient attributes is well-represented in the sample.

5.2 Estimates

To simplify the exposition, we focus on a single outcome – home health payments per Medicare enrollee – which is the most relevant one from the perspective of Medicare finances. Figure 6 reports the within-policy combination distribution of estimated "factual" treatment effects ($\tau_i(a)$) experienced by the enrollees who received a given policy combination. The distribution is bi-modal. The standard deviation of the estimates is 81% of the mean, suggesting considerable heterogeneity in treatment effects across enrollees treated with a given policy combination. When examining the importance weights on different covariates, the number of comorbidities is by far the most important, followed by Medicare spending from the prior year; demographics contribute relatively little to the estimates (Appendix Table OA.6). Appendix Table OA.7 reports the within-policy combination bivariate correlations between the $\tau_i(a)$'s and patient characteristics. These are mostly intuitive: treatment effects tend to be larger (more negative) for patients who are older, with higher spending, and more comorbidities. They are also larger for women.

We use the full set of $\tau_i(a)$'s to estimate the parametric model in equation (7) for each treated enrollee, as defined by their type z_i.[25] Table 1 reports the mean and standard deviation of the parameter estimates across enrollees. Notably, the average δ is 0.81, suggesting that, on average, the two policies are closer to being additively separable (i.e. $\delta = 1$) than substitutes (i.e. $\delta = 0$); but there is also considerable heterogeneity across enrollees, with a standard deviation in δ_i (across enrollees) of 0.27. Figure 7 shows the full distribution of δ across enrollees. For about 65% of enrollees the two policies are perfectly additively separable ($\delta = 1$), and for about 5% they are perfect substitutes ($\delta = 0$); for the remaining 30% of enrollees the policies are somewhere in between ($0 < \delta < 1$).

Table 2 uses the estimates from equation (7) to consider the average impact of the policies – individually and jointly – on different sets of enrollees defined by the policies their county received (as shown in the columns). The first row reports average home health spending per enrollee without either policy. The remaining rows show the estimated spending reduction under various policy combinations after those policies have been in place for 4 or more years. These impacts can vary across counties because of heterogeneity in the enrollee population.

In Panel A, a comparison of the first two columns shows that the policies were, broadly speaking, well targeted: spending reductions from the policies in the "ever treated" counties (column (2)) are substantially larger than those in the "never treated" counties (column (1)). For example, the average impact of the strike force on ever-treated counties is to reduce home health payments by \$218 per enrollee, compared to \$125 in never-treated counties; the corresponding estimates for moratoria are a reduction of \$162 in ever-treated counties compared to \$94 in never-treated

[25] For each enrollee, we have one factual treatment effect based on the policy combination a they experienced and counterfactual treatment effects for other policy combinations which are assigned by looking for another enrollee with the same vector of observables z_i who experienced each other policy combination. We then use these treatment effects for each a to estimate the parametric model separately for each treated enrollee.

counties.[26] By contrast, there is comparatively less variation within ever-treated counties in the impact of each policy across counties that received only one policy, only the other, or both. Columns (3) through (5) of Panel A show that the impact of the strike force ranges from \$212 to \$234 across those three groups; the corresponding range for the impact of moratoria is \$158 to \$175.

Panel B shows the estimated reduction in payments from applying both policies jointly. It shows estimates under three different assumptions about their interactions: using our average estimate of $\delta = 0.81$, assuming perfect substitutes ($\delta = 0$), and assuming additive separability ($\delta = 1$). Once again, effects are substantially larger in ever-treated counties than in never-treated counties. For example, under our estimated δ, applying both policies to the ever-treated counties produces an average decline in per enrollee home health spending of \$366, compared to \$210 in never-treated counties. This average reduction would have been much smaller if the policies were perfect substitutes (\$221 instead of \$366) and only slightly larger (\$380 instead of \$366) if the policies were additively separable .

5.3 Optimal policy targeting and placement

We use our estimates to investigate the optimal geographic placement of the policies. Specifically, we hold fixed at the observed levels the number of enrollees treated by each policy, but vary which counties are treated. In other words, when assigning a given policy across counties, we use the number of actually treated enrollees (by that policy) as our "budget," and assign counties to the policy until the number of enrollees runs out. Table 3 reports the reduction in home health payments under three scenarios (shown in the three different columns): random placement, actual observed placement, and optimal placement. Note that unlike in Table 2, not all counties within each column receive a policy, since this counterfactual analysis respects the "budget constraint" of the observed number of enrollees treated by each policy.

Panel A considers re-assigning policies across the ever-treated counties. The first two rows consider, separately, the placement of only the strike force or only the moratorium. They indicate that, within the ever-treated counties, the actual placement of the strike force or the moratorium individually achieved home health spending reductions that were comparable to random placement (\$60 and \$37, respectively, from observed placements compared to \$61 and \$36 from random placement.[27] Optimal placement of the policies, however, could have generated reductions of \$69 for the strike force and \$46 for the moratorium, or about 15% and 25% more than observed. The

[26] The estimates of individual policy impacts on "ever treated" counties need not be the same as those reported in Section 4, where we did not allow for heterogeneous effects across enrollees or interaction between policies. For example, we estimate here that deploying the strike force alone in ever-treated counties would reduce home health payments by about 40% in year 4 or later (\$218/\$579), whereas in Section 4 we estimated that the reduction would be about 13% (Appendix Table OA.4). Likewise, for moratoria alone, here we estimate about a 30% reduction in payments (\$162/\$579), compared to about 38% in Appendix Table OA.4.

[27] Note that, unlike in Table 2 where every county in a given column experienced the treatment indicated by a given row, here we are considering the average impact of the treatment in a given row across a group of counties, only some of which actually receive the treatment.

similarity of results between observed and random placement in Panel A reflects the fact that, as seen in Table 2, most of the variation in treatment effects is between the ever-treated counties and never-treated counties, rather than within the ever-treated counties. Thus, when we consider possible re-assignments of policies across all counties (Panel B), the observed placement of each individual policy does much better than random. For example, the reduction in home health payments of $37 per enrollee under the observed moratoria placement is noticeably higher than under random placement across all counties ($27) but also far below the optimal placement ($53).

The bottom rows of each panel reports effects from implementing both policies. Here, we define optimal placement as the optimal sequential placement of policies: we first look for counties that would have the highest treatment effect under the strike force and assign them to the strike force. We then assign the moratorium to maximize the combined treatment effect, taking the placement of the strike force as given. This procedure is similar to the situation faced by the agencies in practice; after the initial implementation of the strike force, the agencies then decided if and where to place the moratoria (see Appendix Table OA.1 and OA.2). Once again, Panel A shows that, within the ever-treated counties, observed placement and random placement achieve similar reductions in home health spending per enrollee ($95.3 compared to $94.6). However, optimal placement across ever-treated counties would have resulted in a 17% greater reduction in spending than actual placement ($112 compared to $95). When we go further out of sample to explore optimal placement across all counties in Panel B, we estimate that optimal placement of the two policies could have achieved about a 35% greater reduction in spending ($129 compared to $95) than the observed placement.

Appendix Table OA.8 summarizes how the distribution of policies changes between observed and optimal placement; Appendix Figure OA.10 shows the optimal placements on a map. If we consider optimal placement among ever-treated counties (Panel A), about 20% of these enrollee-weighted counties now receive no policy, and about 35% optimally receive the same policy combination that they actually received. When we broaden the scope of our analysis to consider optimal placement across all counties, now about half of the (enrollee-weighted) ever-treated counties would optimally receive no policy; moreover, about 15% of the (enrollee-weighted) never-treated counties would now receive a policy.[28]

6 Conclusion

We studied two policies that were deployed within the same decade to reduce perceived waste in Medicare-financed home health: strike forces and moratoria on entry of new home health agencies. We estimate substantial heterogeneity across patients in policy impacts, as well as reduced efficacy from one policy if the other is already in place. On average, we find that the policies were each

[28]Interestingly, Appendix Table OA.8 also indicates that optimal placement does not involve any county receiving only the moratorium. This result continues to hold even if instead of defining optimal placement by first assigning strike forces and then moratoria, we first assign the moratoria and then the strike forces (see Appendix Table OA.19).

reasonably well-targeted to places with higher than average treatment effects. However, within the areas that received at least one policy, reshuffling the allocation of policies could have increased their impact by about 20%. Moreover, going out of sample to consider counties that never received a policy, we estimate that optimal placement could increase their impact even more, by about 35%. Our findings suggest that, to maximize the impact of policy efforts, policymakers should move beyond evaluating each intervention in isolation and instead consider how policies interact and their incremental effects in combination. Optimal deployment that accounts for these interactions can lead to greater policy impacts and more effective use of public resources.

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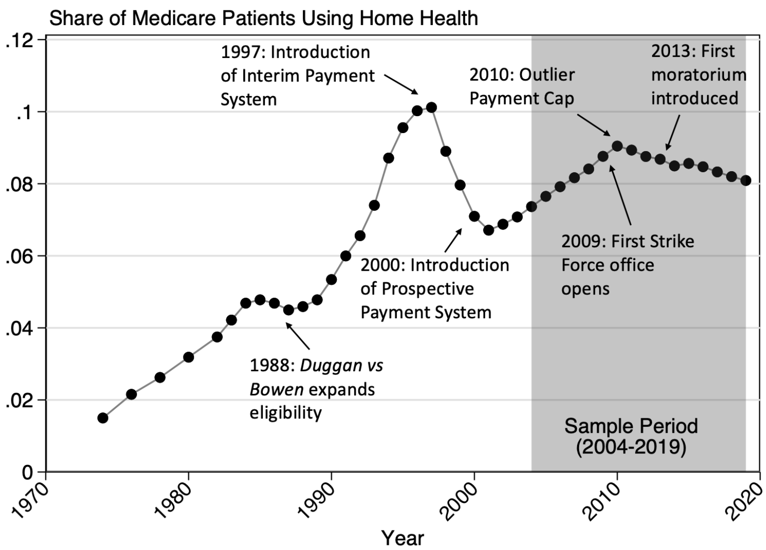
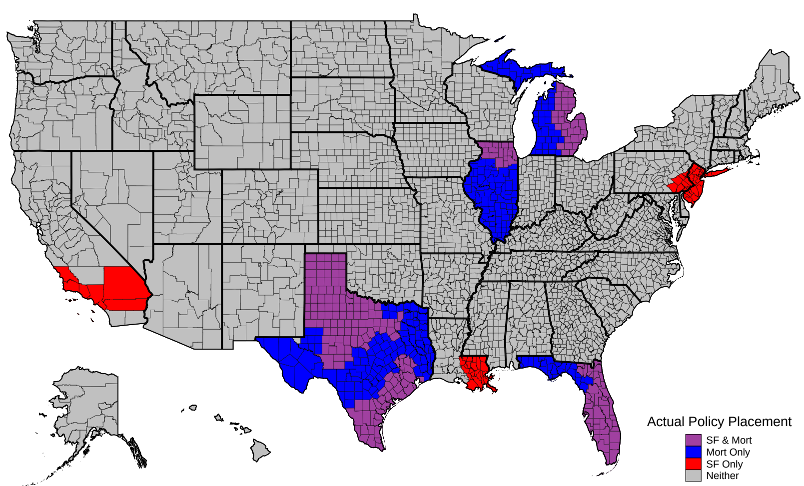
Figure 1: Share of Medicare Patients Receiving Home Health Care, 1974-2019



Notes: This figure displays trends in the share of Medicare enrollees receiving Medicare-financed home health care each year between 1974 and 2019. Data for 1974 through 1998 is from the CMS 2001 Statistical Supplement, while data for 1999 and beyond is from the 20% sample of Medicare claims data. The shaded area reflects the observation period used for this paper's empirical analysis.

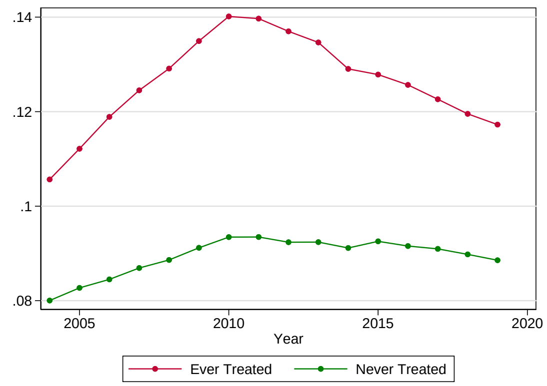
Figure 2: Geographic Variation in Policy Instruments



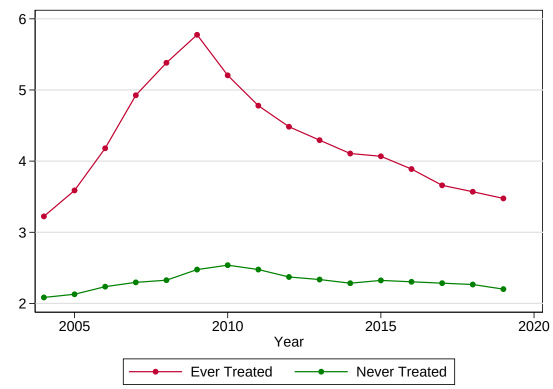
Notes: This figure maps counties in the United States according to which policy combination they received by 2019. 33.9% of enrollee-weighted counties received some policy. Of those, 34.0% received only the SF, 18.8% received only the M and 47.2% received both.

Figure 3: Home Health Utilization Over Time By Treatment

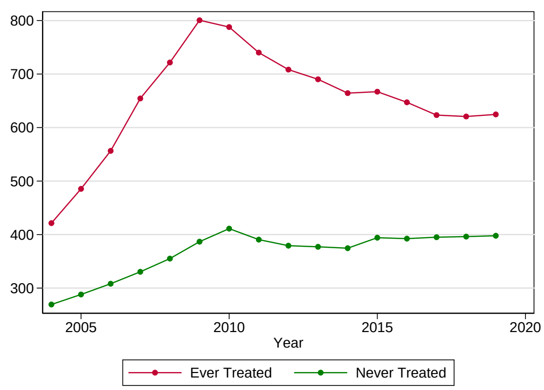
(a) Share of Medicare Enrollees Using Home Health



(b) Home Health Visits Per Medicare Enrollee



(c) Home Health Payments Per Medicare Enrollee

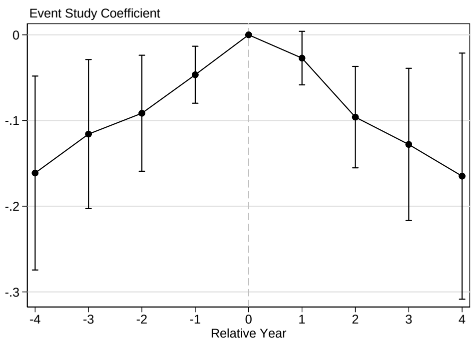


Notes: This figure displays various home health utilization measures over time by whether a county was ever-treated or never-treated by the strike force and moratorium. Home health payments is reported in 2019 dollars. The sample consists of all 65+ aged patients enrolled in Medicare for all 12 months of the year.

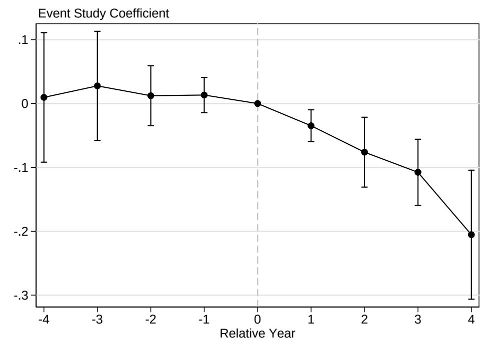
Figure 4: Standard Event Studies

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

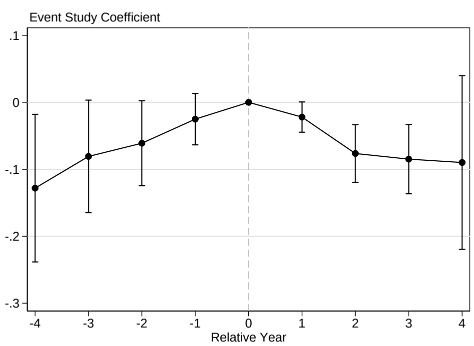


(b) Moratorium

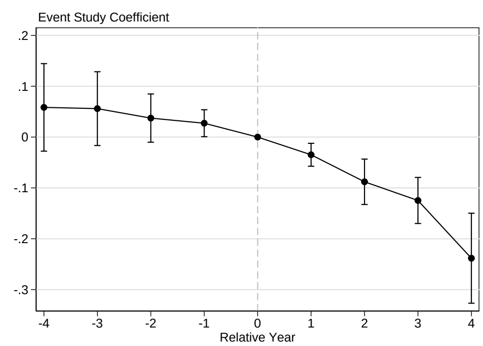


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

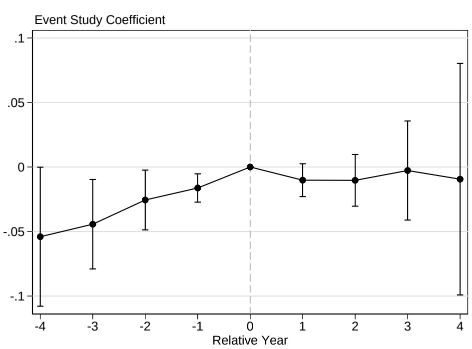


(d) Moratorium

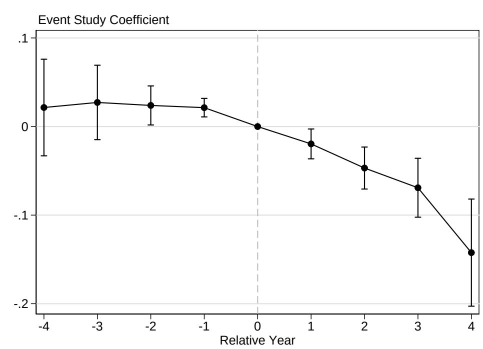


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



(f) Moratorium

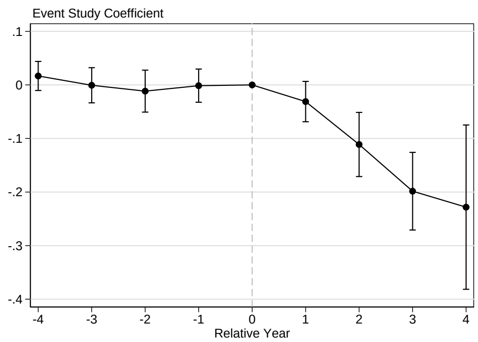


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation (1) with several measures of home health utilization as the outcome. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $48,032$ county-years.

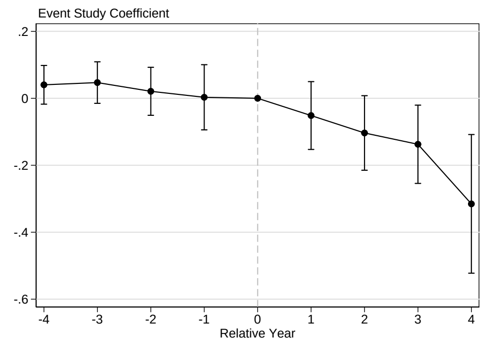
Figure 5: Detrended Event Studies

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

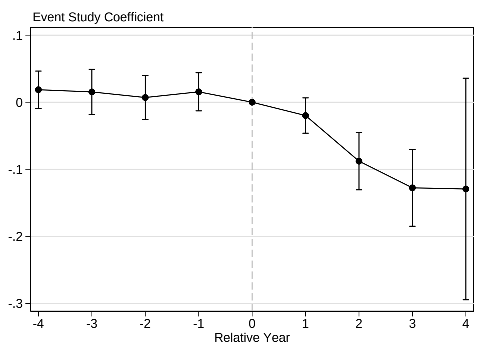


(b) Moratorium

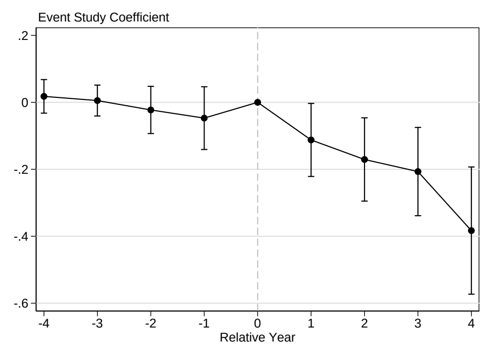


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

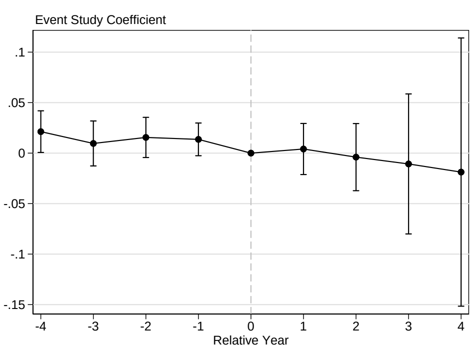


(d) Moratorium

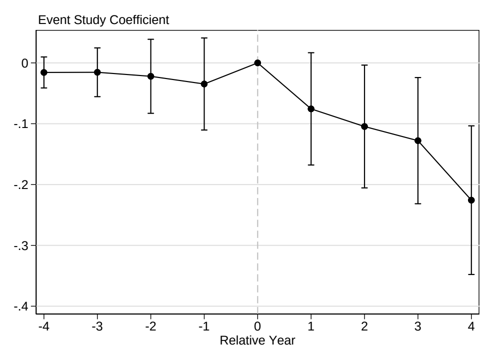


Log Share Medicare Enrollees Using Home Health

(e) Strike Force

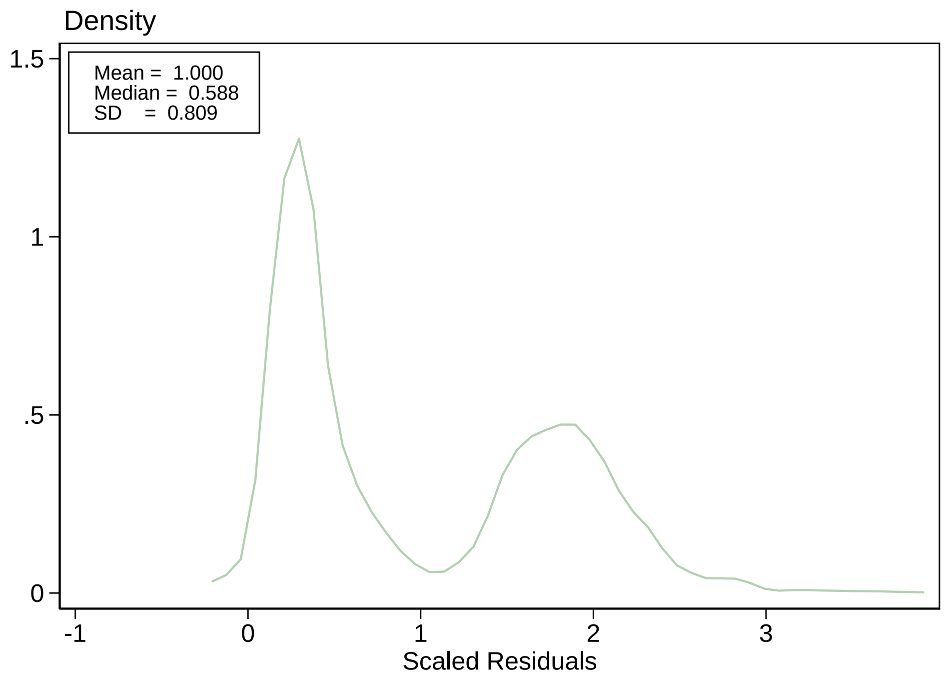


(f) Moratorium



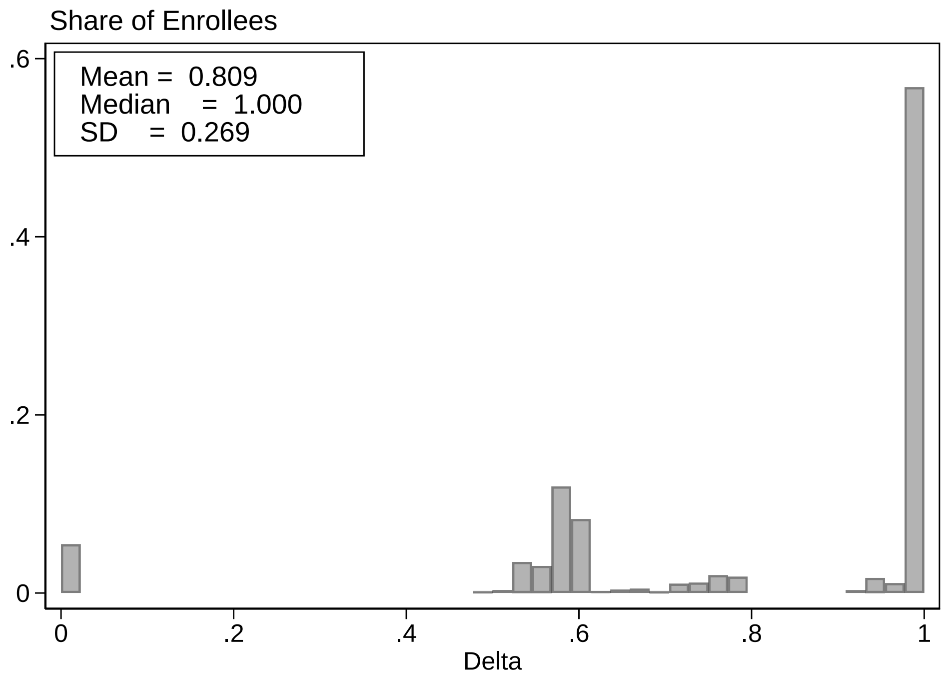
Notes: This figure shows estimates of $\beta'_{r^{SF}}$ (left column) and $\theta'_{r^{M}}$ (right column) from equation (4) with several detrended measures of home health utilization as the outcome. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. $N = 8,992$ county-years.

Figure 6: Distribution of τ



Notes: Figure reports the within-policy combination a distribution in treatment effects across the enrollee-years that actually received that policy combination. It is obtained by taking the estimates $\tau_i(a)$ for each enrollee-year i and policy combination that enrollee-year received (a) and residualizing out policy combination fixed effects. The figure then plots the density of $\left(\widetilde{\tau_i(a)} + \overline{\tau(a)}\right)/\overline{\tau(a)}$, where $\widetilde{\tau_i(a)}$ is the residualized treatment effect and $\overline{\tau(a)}$ is the average estimate in policy group a. An Epanechnikov kernel (bandwidth $= 0.1$) is used for smoothing.

Figure 7: Distribution of δ's for Home Health Payments



Notes: Figure shows the distribution of δ from the parametric model (equation (7)).

Table 1: Parametric Model Estimates

	Mean	Std. Deviation
α_1	0.328	0.243
α_2	0.010	0.009
β_1	0.110	0.062
β_2	0.041	0.028
δ	0.809	0.269

Notes: Table reports the results from estimating equation (7). Specifically, we report the mean and standard deviation of the enrollee level estimates.

Table 2: Reductions in Per Enrollee Home Health Spending Under Observed and Counterfactual Policy Placements

	Never Treated (1)	Ever Treated (2)	SF Only (3)	M Only (4)	SF and M (5)
1. Baseline Spending in absence of policies	360.5	578.9	563.8	647.2	561.1
Panel A. Spending reduction from applying each policy individually					
2. Strike Force Only	125.1	218.1	212.7	234.0	215.4
3. Moratorium Only	94.2	162.2	158.1	175.4	159.6
Panel B. Spending reduction from applying both Strike Force and Moratorium					
4. Estimated $\delta = 0.81$	210.2	366.4	356.8	393.4	362.1
5. $\delta = 0$	127.2	221.3	215.8	237.8	218.4
6. $\delta = 1$	219.3	380.3	370.8	409.4	375.0
Share of counties (enrollee-weighted)	0.67	0.33	0.11	0.06	0.15

Notes: Table displays estimated reduction in home health spending per enrollee after 4 or more years of different policies (shown in the rows) on different sets of enrollees defined by the policies their county received (shown in the columns). The baseline (mean with no treatment) is estimated by taking the average over $\exp(\hat{\tau}_t + \hat{\alpha_c})$ from equation (2), where the average is taken over the enrollee-years used in that column's sample. The subsequent rows of this table show predicted reductions in spending from different policy combinations. The bottom row reports the share of enrollee-weighted counties in each column. The columns of this table show different sets of enrollees (defined by the policies their county received), over which the predicted effects are averaged.

Table 3: Reductions in Per Enrollee Home Health Spending under Random, Observed, and Optimal Placements

	Random Placement	Observed Placement	Optimal Placement
Panel A. Ever-treated counties			
Implementing Strike Force only	61.0	60.2	69.3
Implementing Moratorium only	36.1	37.2	46.2
Implementing both policies (estimated δ)	94.6	95.3	111.7
Panel B. All counties			
Implementing Strike Force only	44.5	60.2	80.8
Implementing Moratorium only	26.8	37.2	52.5
Implementing both policies (estimated δ)	70.7	95.3	129.3

Notes: Table shows reductions in per enrollee home health spending after 4 or more years from different placements (shown across the columns) of a fixed number of enrollees treated by each policy. The "observed placement" reflects the actual placement of each policy within the ever treated counties. In Panel A, the pool of counties considered for random/optimal placement is all counties that were ever treated in reality, while in Panel B, it is all counties in the United States. Averages are weighted by the number of enrollees in a county in 2006. For 'Random Placement,' results shown are the average of 50 random allocations across counties.

Online Appendix

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 the 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).[29]

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 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%

[29] We 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.

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:

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

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 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 = 4.5%) 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 = 4.7%), 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.[30] 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 12.5% (standard error = 3.5%) and home health payments by about 10.7% (standard error = 2.6%), 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 12.5% (standard error = 5.8%), home health spending by about 21.5% (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.[31] 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 = 3.2%). For moratoria, the effects are larger: we estimate that on average they reduce home health visits by about 20.6% (standard error = 11.5%), payments by about 24.0% (standard error = 11.7%), and the share of patients using home health by about 15.1% (standard error = 6.4%). 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.

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.

[30] This 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.

[31] Since 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.

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 20 percent of enrollees were SNF-eligible. Our classification appears to be reasonable; about 30% 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.[32] Strikingly, it also indicates that only half 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.[33]

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.[34]

[32] Specifically, among SNF-eligible enrollees, average SNF spending per enrollee is $3,141 while among SNF ineligible enrollees it is $71. Accounting for the fact that only 20 percent of enrollees are SNF-eligible, this suggests that 93 percent of Medicare SNF payments are to enrollees whom we have labeled as eligible for SNF Medicare reimbursement.

[33] 25 percent of SNF-eligible patients use home health, compared to only 7 percent of SNF-ineligible patients. Accounting for the fact that 20 percent of patients are SNF-eligible, we get that 50 percent of home health patients are SNF eligible. SNF-eligible patients also spend on average $747 on home health, compared to $352 on home health for SNF-ineligible patients; accounting for the fact that 20 percent of patients are SNF-eligible, this implies that 38 percent of home health payments are to SNF-eligible enrollees.

[34] Appendix 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 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-ineligibile 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 25.9 log points (standard error = 7.3) 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 38.3 log points (standard error = 10.7) compared to a

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.[35] Appendix Table OA.16 summarizes the estimates.[36] 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 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 $\widetilde{\ln(y_{ct})} = \ln(y_{ct}) - \widehat{\ln(y_{ct})}$, where $\widehat{\log(y_{ct})}$ 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:

---

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.

[35] 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.

[36] 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.

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

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.[37] 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;[38] 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.

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

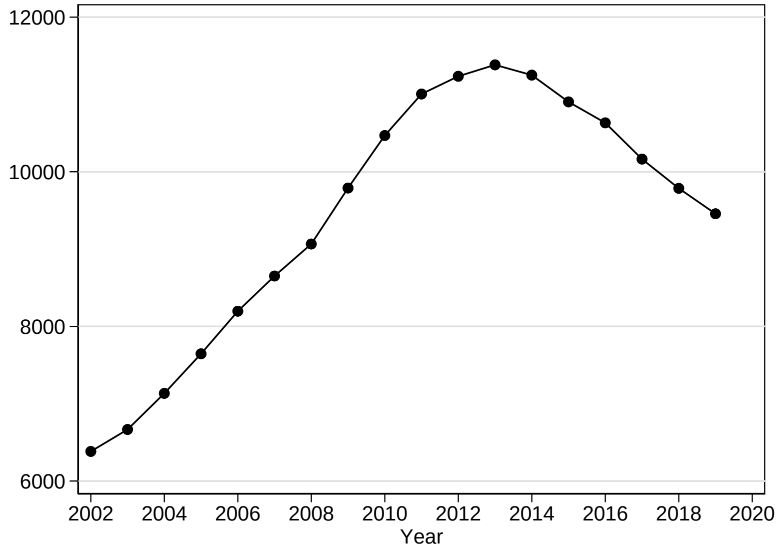
[37] For computational speed, we estimate the causal forest on a random 5% sample of our baseline sample.

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

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

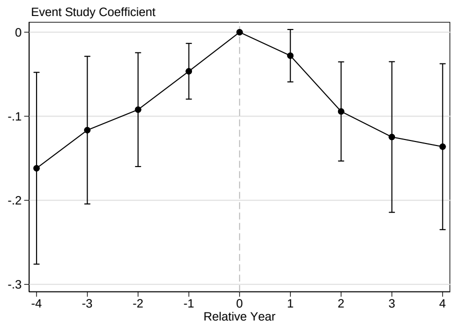


Notes: This figure shows the number of home health agencies that served at least one patient in our sample in the corresponding year. The sample includes all beneficiaries enrolled in Traditional Medicare for the full calendar year in the 20% Medicare sample.

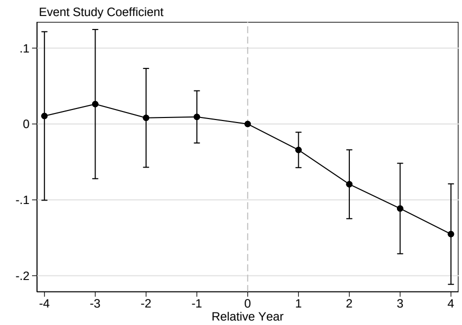
Figure OA.2: Standard Home Health Event Studies, with Years 5+ Binned

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

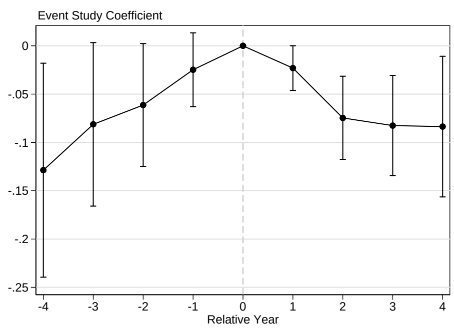


(b) Moratorium

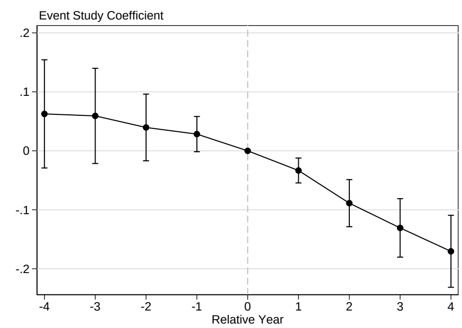


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

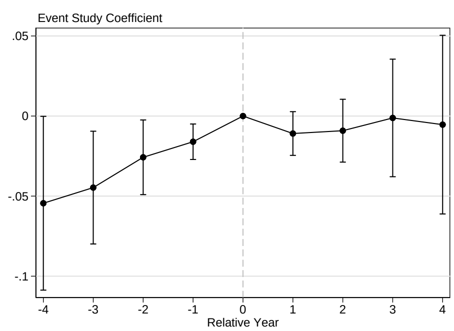


(d) Moratorium

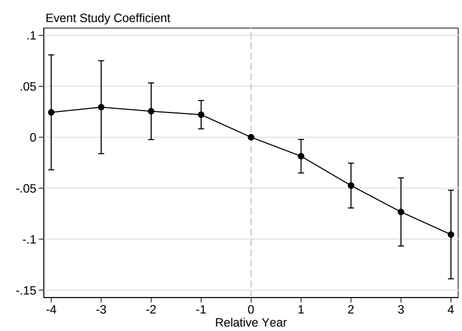


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



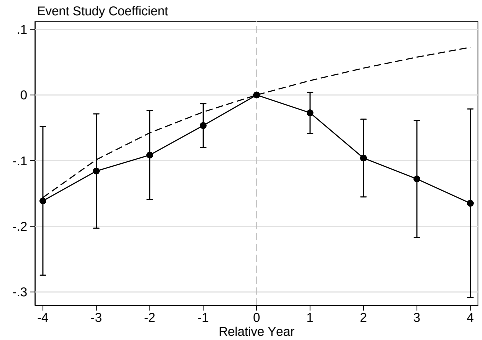
(f) Moratorium



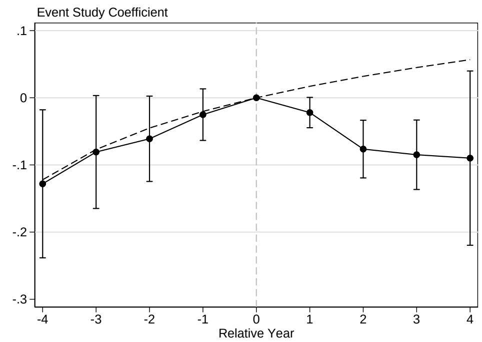
Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation (1). The regressions are weighted by the number of Medicare enrollees (ages 65+) in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district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

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

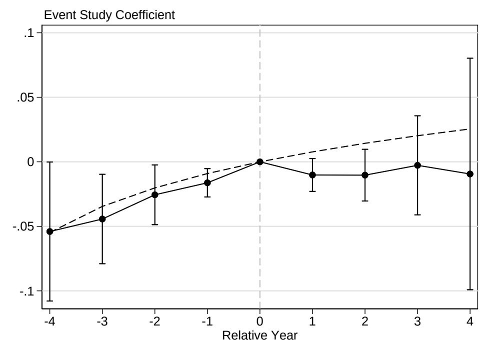
Log Home Health Visits Per Medicare Enrollee



Log Home Health Payments Per Medicare Enrollee



Log Share Medicare Enrollees Using Home Health

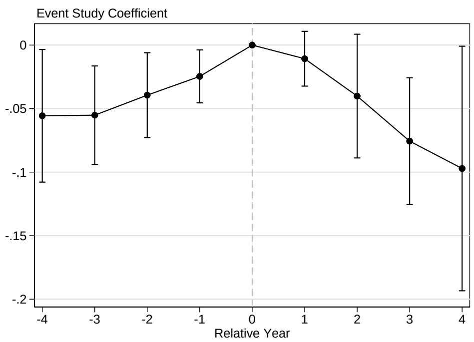


Notes: This figure plots the event-study coefficients and predicted pre-trends for the Strike Force estimates from equations (2) and (1). Regressions are weighted by the 2006 enrollee population. Vertical lines denote 95% confidence intervals clustered at the district level. The dashed line shows the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

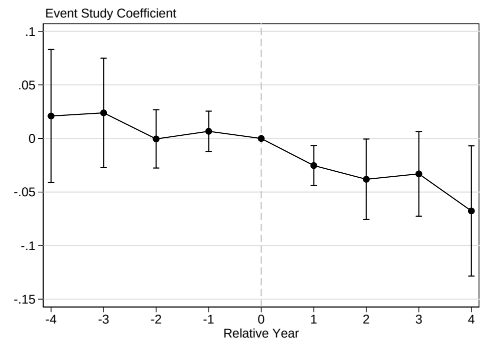
Figure OA.4: Standard Home Health Event Studies, limited to SNF-eligible Home Health

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

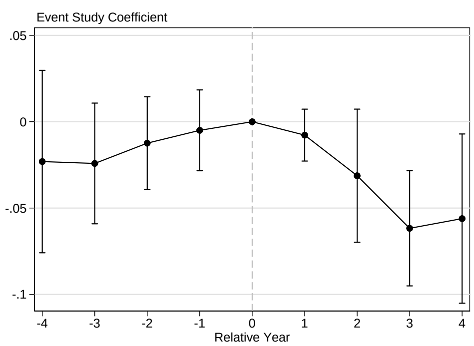


(b) Moratorium

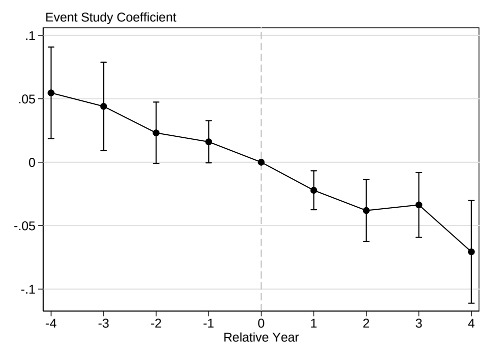


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

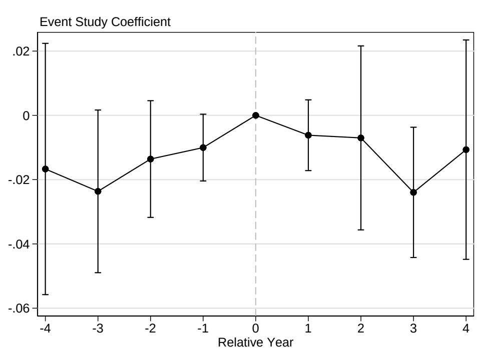


(d) Moratorium

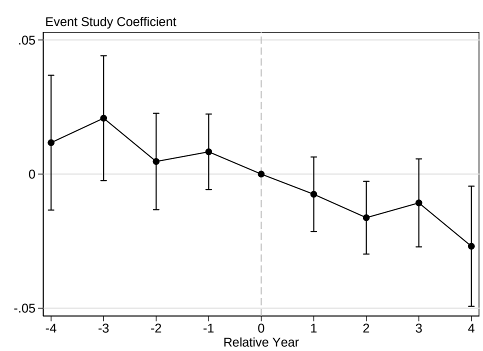


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



(f) Moratorium

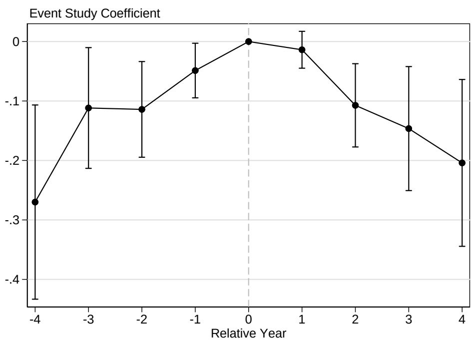


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 1 with several measures of SNF eligible home health utilization as the outcome; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 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.

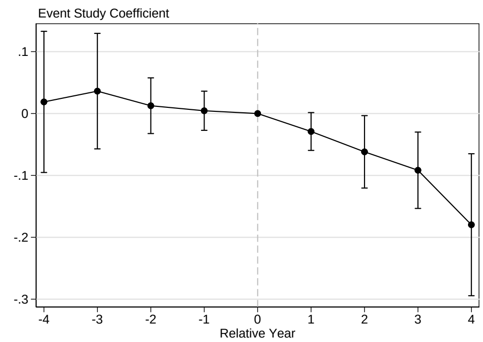
Figure OA.5: Standard Home Health Event Studies, limited to SNF-ineligible Home Health

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

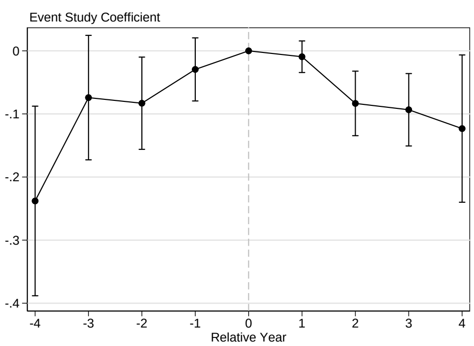


(b) Moratorium

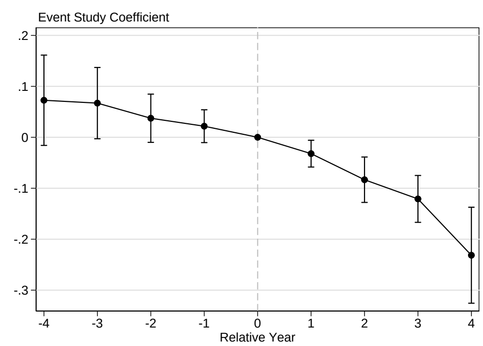


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

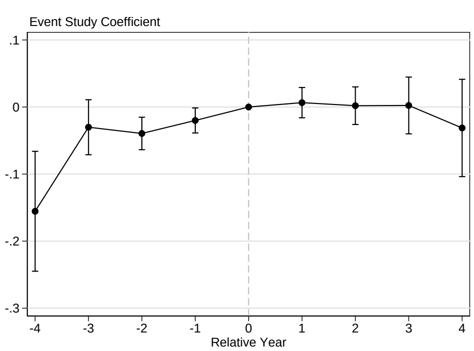


(d) Moratorium

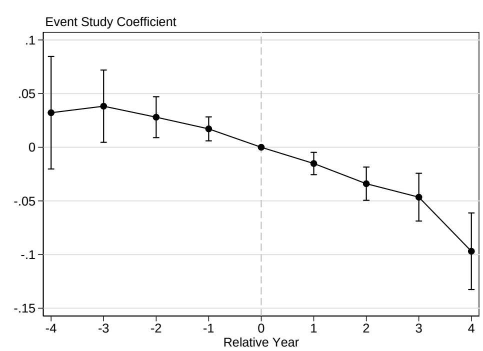


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



(f) Moratorium

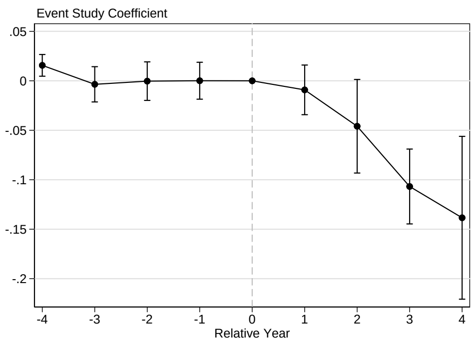


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 1 with several measures of SNF ineligible home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) 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.

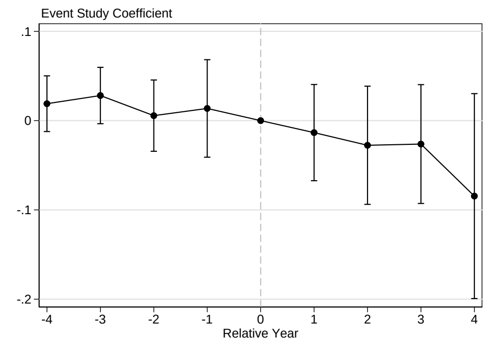
Figure OA.6: Detrended Home Health Event Studies, limited to SNF-eligible Home Health

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

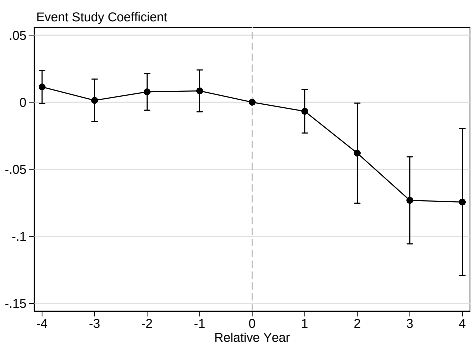


(b) Moratorium

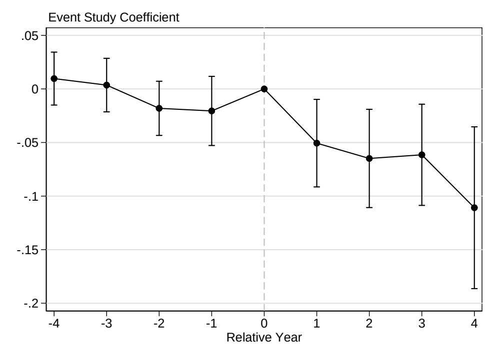


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

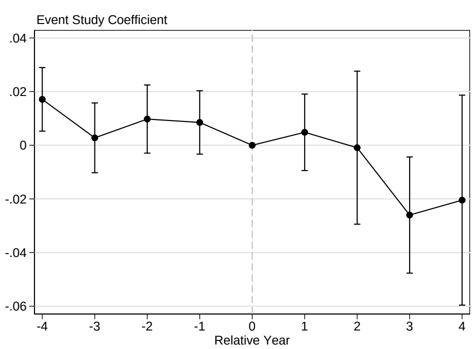


(d) Moratorium

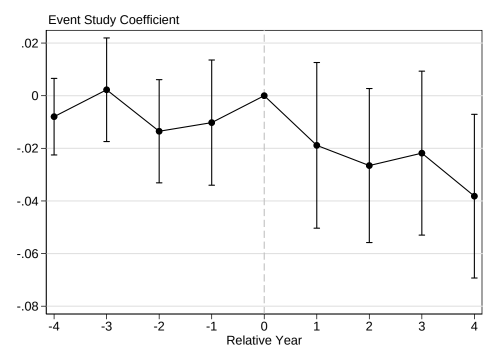


Log Share Medicare Enrollees Using Home Health

(e) Strike Force



(f) Moratorium

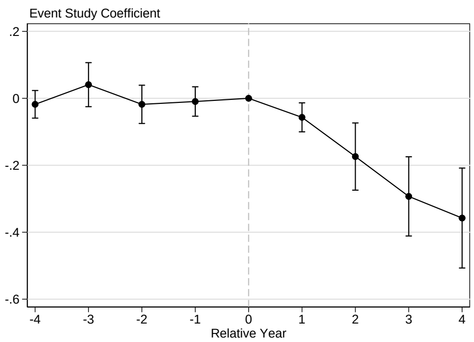


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 4 with several measures of SNF-eligibile home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) 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.

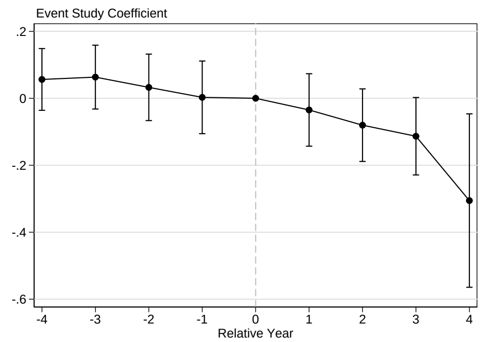
Figure OA.7: Detrended Home Health Event Studies, limited to SNF-ineligible Home Health

Log Home Health Visits Per Medicare Enrollee

(a) Strike Force

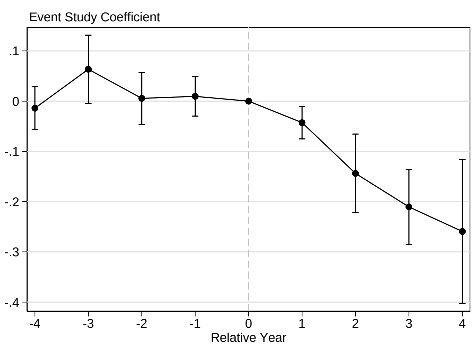


(b) Moratorium

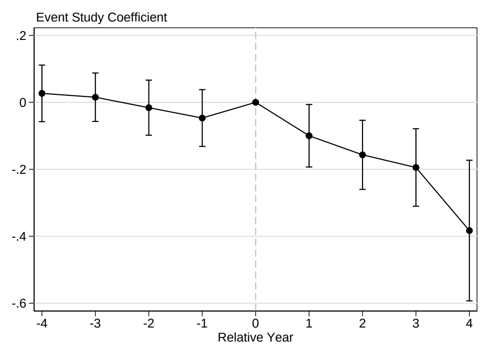


Log Home Health Payments Per Medicare Enrollee

(c) Strike Force

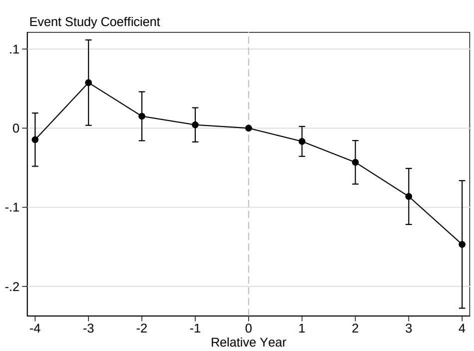


(d) Moratorium

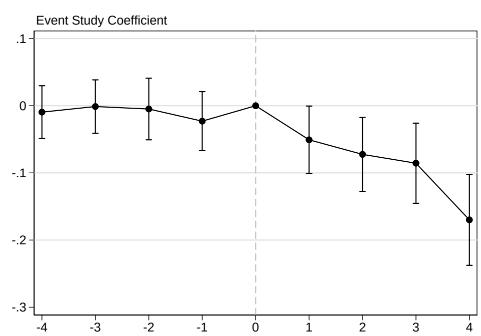


Log Share Enrollees Using Home Health

(e) Strike Force



(f) Moratorium

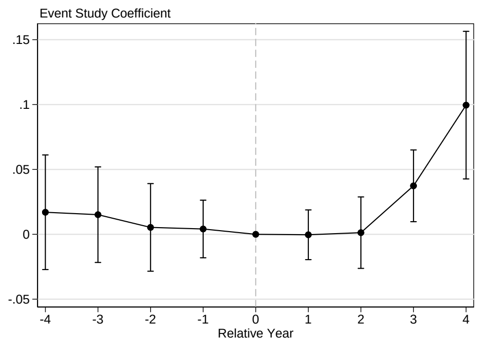


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 4 with several measures of SNF-ineligible home health utilization; see Appendix C for more details on the definition. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) 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.

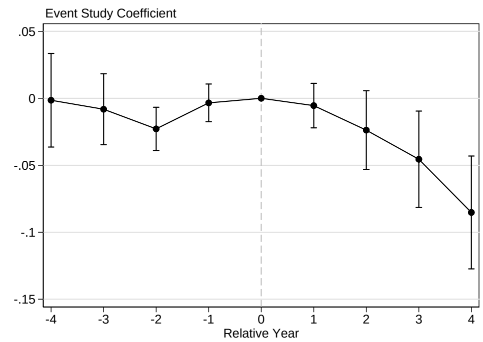
Figure OA.8: Standard SNF Event Studies

Log SNF Days Per Medicare Enrollee

(a) Strike Force

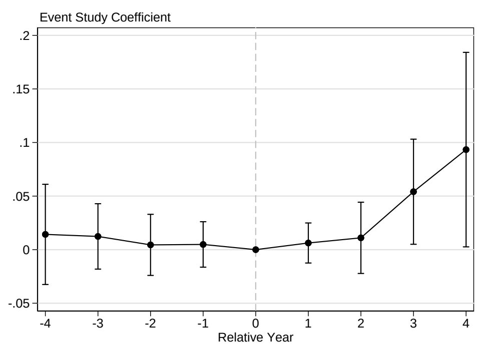


(b) Moratorium

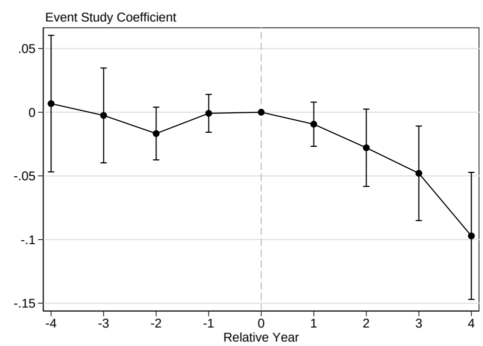


Log SNF Payments Per Medicare Enrollee

(c) Strike Force

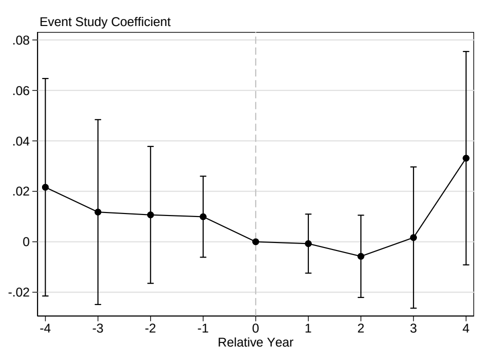


(d) Moratorium

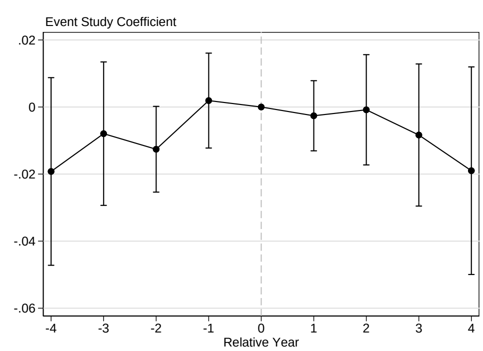


Log Share Medicare Enrollees Using SNF

(e) Strike Force



(f) Moratorium

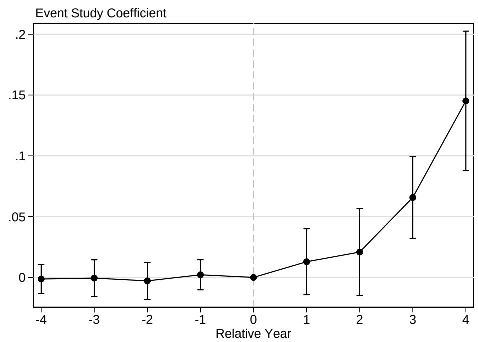


Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 1 with several measures of SNF utilization as the outcome. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence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

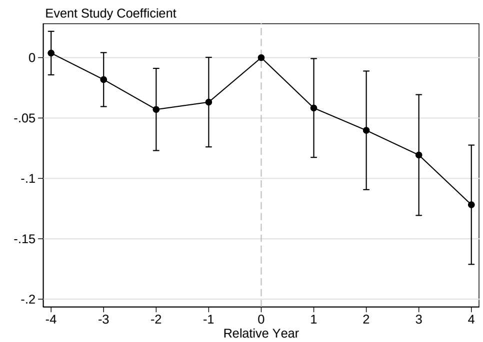
Figure OA.9: Detrended SNF Event Studies

Log SNF Days Per Medicare Enrollee

(a) Strike Force

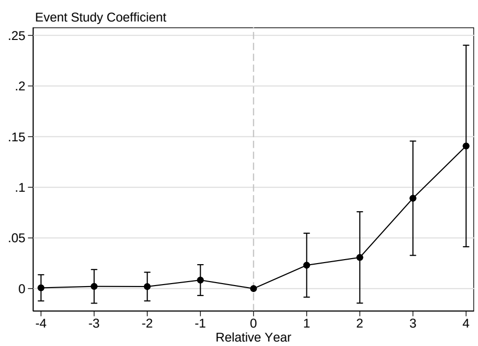


(b) Moratorium

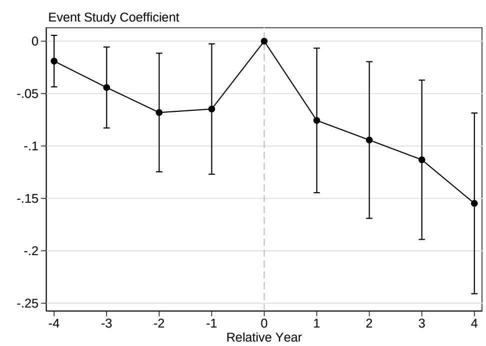


Log SNF Payments Per Medicare Enrollee

(c) Strike Force

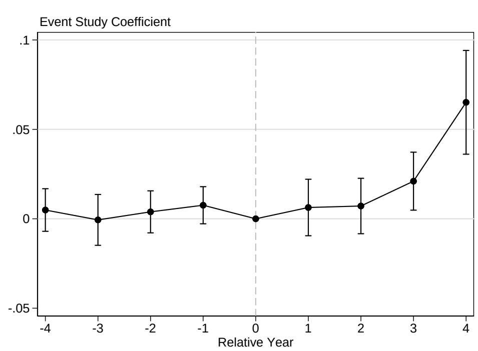


(d) Moratorium

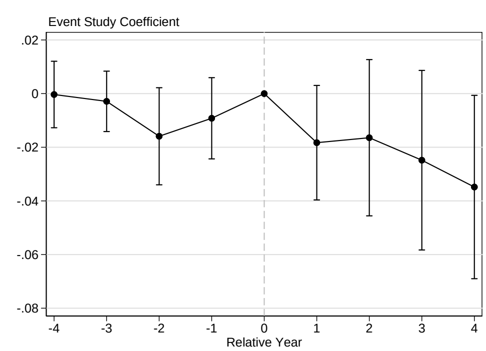


Log Share Enrollees Using SNF

(e) Strike Force



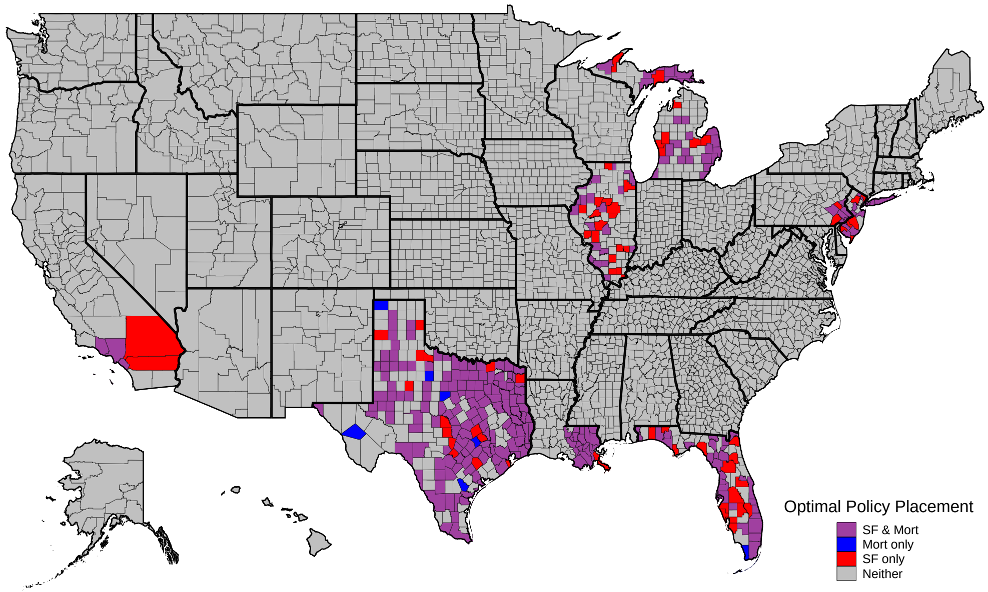
(f) Moratorium



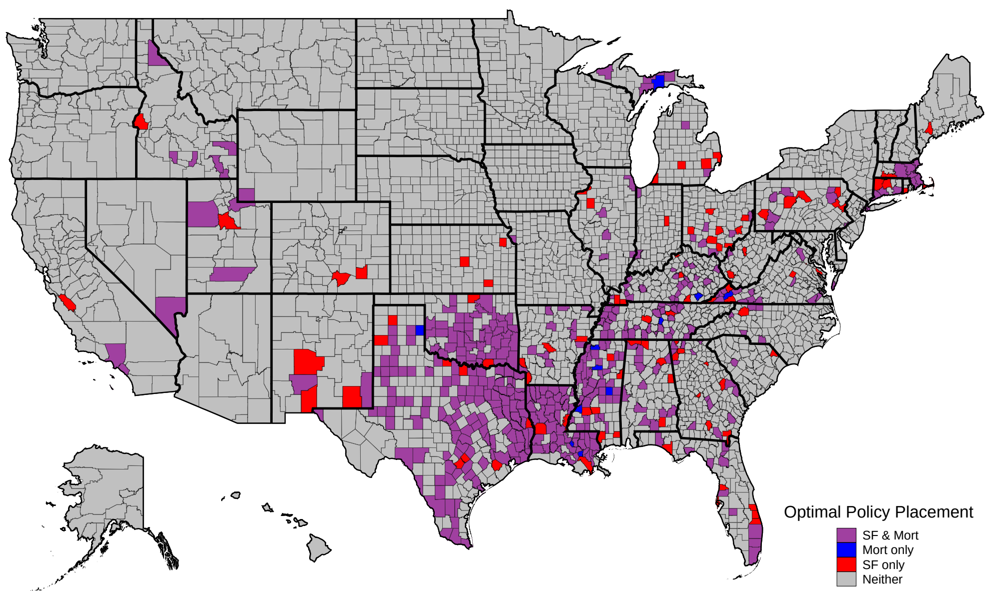
Notes: This figure shows estimates of β_{rSF} (left column) and θ_{rM} (right column) from equation 4 with several detrended measures of SNF utilization. The regressions are weighted by the number of Medicare enrollees (ages 65 and older) in each county in 2006. Vertical lines denote 95% confidence intervals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

Figure OA.10: Optimal Placement of Policies

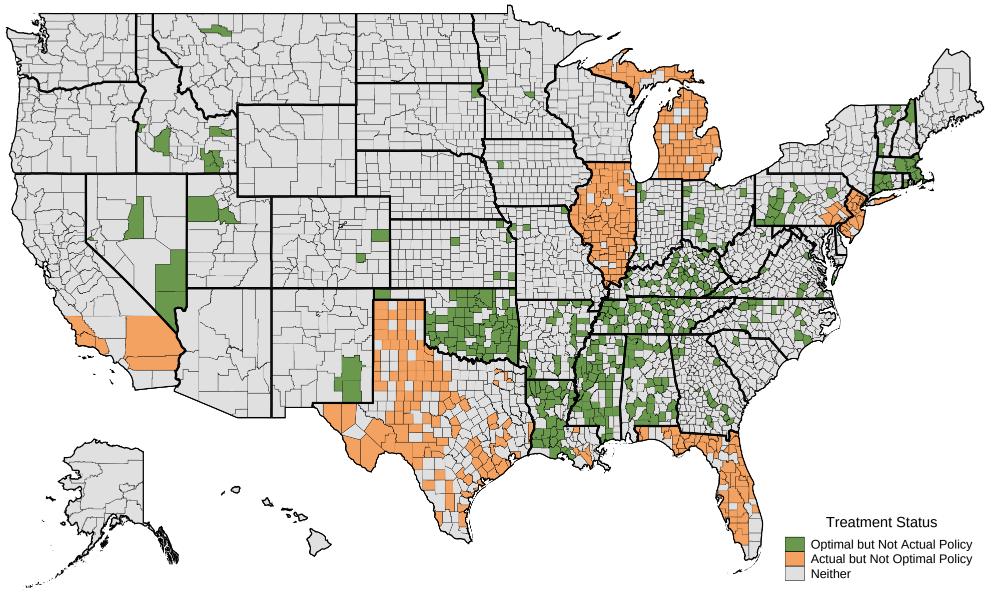
(a) Ever Treated Counties



(b) All Counties



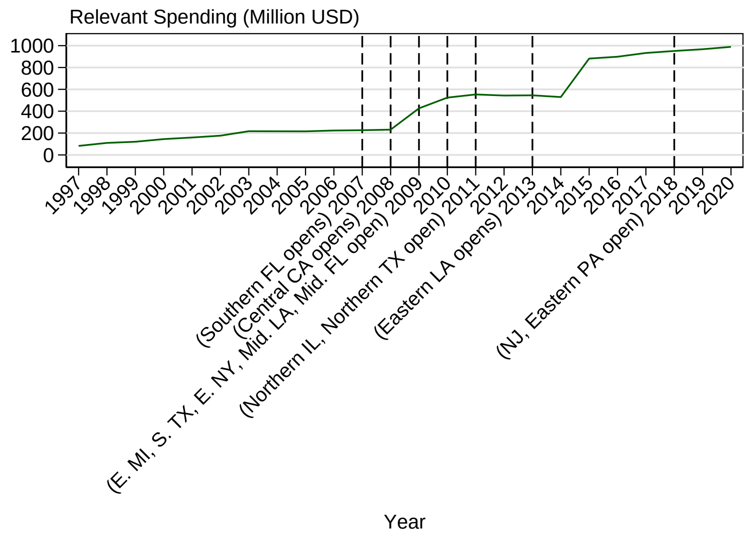
(c) All Counties: Changes in Treatment Status



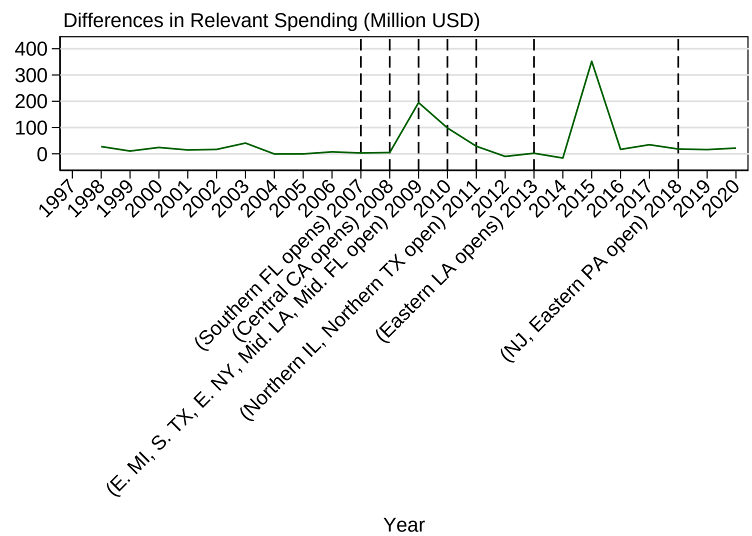
Notes: These figures map counties in the United States by which policy or policies they would receive under our optimal allocation procedure. In Panel (a), the pool of counties considered for optimal placement is counties that were ever treated in reality, while in Panel (b), it is all counties. Panel (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.

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	1-4+	4+
	(1)	(2)	(3)
		A: Strike Force	
Log Home Health Visits per Medicare enrollee	-0.031	-0.142	-0.228
	(0.019)	(0.033)	(0.078)
Log Home Health Payments per Medicare enrollee	-0.020	-0.091	-0.129
	(0.013)	(0.030)	(0.084)
Log Used Home Health per Medicare enrollee	0.004	-0.007	-0.019
	(0.013)	(0.031)	(0.068)
		B: Moratorium	
Log Home Health Visits per Medicare enrollee	-0.051	-0.152	-0.315
	(0.052)	(0.065)	(0.106)
Log Home Health Payments per Medicare enrollee	-0.112	-0.218	-0.383
	(0.056)	(0.068)	(0.097)
Log Used Home Health per Medicare enrollee	-0.076	-0.133	-0.226
	(0.047)	(0.053)	(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 β_{rSF}'s from equation 4 for the strike force, and the θ_{rM}'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

		Moratorium				
	Relative Year	0	1	2	3	4+
	0	—	2.53%	2.55%	2.53%	2.81%
	1	6.99%	0%	0%	0%	0%
Strike Force	2	10.84%	0%	0%	0%	0%
	3	7.69%	1.18%	0%	0%	0%
	4+	32.68%	5.07%	6.10%	6.05%	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)
0.142	0.452	0.088	0.020	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)	-0.102	0.0005
I(Female)	-0.047	0.0003
# Comorbidities	-0.084	0.0000
Lag Spending	-0.00000225	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				
Observed Placement				
Strike Force Only (34%)	0.17	0.00	0.69	0.15
Moratorium Only (19%)	0.13	0.00	0.60	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				
Observed Placement				
Strike Force Only (12%)	0.01	0.00	0.41	0.57
Moratorium Only (6%)	0.06	0.00	0.42	0.53
Strike Force and Moratorium (16%)	0.10	0.00	0.45	0.46
No Policy (66%)	0.04	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 Polices on Health Health Utilization, Robustness Analysis

Avg. of Relative Year Coef.	**Strike Force**			**Moratorium**		
	1	1–4+	4+	1	1–4+	4+
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Baseline Specification						
Log Home Health Visits per Medicare Enrollee	-0.031	-0.142	-0.228	-0.051	-0.152	-0.315
	(0.019)	(0.033)	(0.078)	(0.052)	(0.065)	(0.106)
Log Home Health Payments per Medicare Enrollee	-0.020	-0.091	-0.129	-0.112	-0.218	-0.383
	(0.013)	(0.030)	(0.084)	(0.056)	(0.068)	(0.097)
Log Share Enrollees Using Home Health	0.004	-0.007	-0.019	-0.076	-0.133	-0.226
	(0.013)	(0.031)	(0.068)	(0.047)	(0.053)	(0.062)
Panel B. Matching Specifications						
Baseline Matching Specification						
Log Home Health Visits per Medicare Enrollee	-0.027	-0.118	-0.165	-0.029	-0.140	-0.239
	(0.021)	(0.045)	(0.074)	(0.015)	(0.047)	(0.083)
Log Home Health Payments per Medicare Enrollee	-0.017	-0.071	-0.088	-0.035	-0.169	-0.288
	(0.014)	(0.036)	(0.065)	(0.012)	(0.041)	(0.074)
Log Share Enrollees Using Home Health	-0.002	-0.003	-0.005	-0.020	-0.104	-0.182
	(0.008)	(0.023)	(0.043)	(0.009)	(0.025)	(0.046)
Matching with Control > 150 patients						
Log Home Health Visits per Medicare Enrollee	-0.029	-0.119	-0.164	-0.029	-0.140	-0.240
	(0.020)	(0.045)	(0.073)	(0.015)	(0.046)	(0.083)
Log Home Health Payments per Medicare Enrollee	-0.017	-0.072	-0.089	-0.034	-0.167	-0.285
	(0.014)	(0.035)	(0.065)	(0.012)	(0.041)	(0.074)
Log Share Enrollees Using Home Health	-0.002	-0.003	-0.005	-0.020	-0.104	-0.183
	(0.008)	(0.023)	(0.042)	(0.009)	(0.025)	(0.045)
Matching with Treat & Control > 150 patients						
Log Home Health Visits per Medicare Enrollee	-0.029	-0.120	-0.165	-0.030	-0.139	-0.238
	(0.020)	(0.045)	(0.073)	(0.015)	(0.047)	(0.084)
Log Home Health Payments per Medicare Enrollee	-0.016	-0.076	-0.097	-0.035	-0.172	-0.294
	(0.014)	(0.035)	(0.066)	(0.012)	(0.041)	(0.074)
Log Share Enrollees Using Home Health	-0.002	-0.003	-0.005	-0.020	-0.105	-0.184
	(0.008)	(0.023)	(0.042)	(0.009)	(0.025)	(0.046)
Drop Bad Matches						
Log Home Health Visits per Medicare Enrollee	-0.028	-0.118	-0.166	-0.030	-0.140	-0.238
	(0.021)	(0.046)	(0.074)	(0.015)	(0.047)	(0.083)
Log Home Health Payments per Medicare Enrollee	-0.017	-0.072	-0.088	-0.035	-0.169	-0.288
	(0.014)	(0.036)	(0.065)	(0.012)	(0.041)	(0.074)
Log Share Enrollees Using Home Health	-0.002	-0.003	-0.006	-0.020	-0.104	-0.181
	(0.008)	(0.023)	(0.043)	(0.009)	(0.025)	(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 Polices on Health Health Utilization, Robustness Analysis

Avg. of Relative Year Coef.	**Strike Force**			**Moratorium**		
	1	1–4+	4+	1	1–4+	4+
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Baseline Specification						
Log Home Health Visits per Medicare Enrollee	-0.031	-0.142	-0.228	-0.051	-0.152	-0.315
	(0.019)	(0.033)	(0.078)	(0.052)	(0.065)	(0.106)
Log Home Health Payments per Medicare Enrollee	-0.020	-0.091	-0.129	-0.112	-0.218	-0.383
	(0.013)	(0.030)	(0.084)	(0.056)	(0.068)	(0.097)
Log Share Enrollees Using Home Health	0.004	-0.007	-0.019	-0.076	-0.133	-0.226
	(0.013)	(0.031)	(0.068)	(0.047)	(0.053)	(0.062)
Panel B. Poisson Regression						
Home Health Visits per Medicare Enrollee	-0.044	-0.125	-0.182	-0.041	-0.125	-0.254
	(0.025)	(0.035)	(0.064)	(0.052)	(0.058)	(0.074)
Home Health Payments per Medicare Enrollee	-0.056	-0.107	-0.122	-0.125	-0.215	-0.352
	(0.023)	(0.026)	(0.067)	(0.058)	(0.068)	(0.084)
Log Share Enrollees Using Home Health	-0.027	-0.031	-0.032	-0.095	-0.149	-0.236
	(0.015)	(0.017)	(0.051)	(0.057)	(0.062)	(0.070)
Panel C. Callaway & Sant'Anna (2021)						
Log Home Health Visits per Medicare Enrollee	-0.027	-0.185	-0.269	-0.047	-0.206	-0.337
	(0.012)	(0.056)	(0.077)	(0.030)	(0.115)	(0.186)
Log Home Health Payments per Medicare Enrollee	-0.021	-0.150	-0.219	-0.049	-0.240	-0.399
	(0.012)	(0.049)	(0.073)	(0.029)	(0.117)	(0.195)
Log Share Enrollees Using Home Health	-0.010	-0.056	-0.090	-0.028	-0.151	-0.262
	(0.007)	(0.032)	(0.047)	(0.019)	(0.064)	(0.113)

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 β_{rSF}'s from equation 4 for the strike force, and the θ_{rM}'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 Polices 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	-0.026	-0.044	-0.034	-0.033	-0.024	-0.031	-0.035	-0.028	-0.036	-0.022	-0.027	-0.027
	(0.019)	(0.021)	(0.023)	(0.020)	(0.021)	(0.019)	(0.019)	(0.023)	(0.020)	(0.019)	(0.018)	(0.022)	(0.019)
1–4+	-0.142	-0.123	-0.164	-0.148	-0.139	-0.129	-0.140	-0.148	-0.142	-0.164	-0.136	-0.146	-0.137
	(0.033)	(0.028)	(0.041)	(0.037)	(0.034)	(0.035)	(0.033)	(0.034)	(0.034)	(0.033)	(0.034)	(0.038)	(0.033)
4+	-0.228	-0.208	-0.299	-0.236	-0.208	-0.186	-0.223	-0.229	-0.228	-0.275	-0.228	-0.235	-0.221
	(0.078)	(0.076)	(0.083)	(0.088)	(0.077)	(0.073)	(0.079)	(0.078)	(0.078)	(0.087)	(0.081)	(0.083)	(0.078)
Log Home Health Payments per Medicare Enrollee													
Year 1	-0.020	-0.017	-0.031	-0.025	-0.021	-0.016	-0.019	-0.022	-0.015	-0.025	-0.018	-0.018	-0.016
	(0.013)	(0.015)	(0.017)	(0.014)	(0.015)	(0.015)	(0.013)	(0.016)	(0.014)	(0.014)	(0.014)	(0.015)	(0.013)
1–4+	-0.091	-0.080	-0.127	-0.092	-0.085	-0.094	-0.089	-0.095	-0.090	-0.105	-0.091	-0.090	-0.085
	(0.030)	(0.026)	(0.030)	(0.032)	(0.027)	(0.037)	(0.029)	(0.031)	(0.030)	(0.033)	(0.031)	(0.031)	(0.028)
4+	-0.129	-0.116	-0.235	-0.124	-0.101	-0.126	-0.122	-0.130	-0.129	-0.153	-0.131	-0.131	-0.120
	(0.084)	(0.084)	(0.063)	(0.090)	(0.075)	(0.102)	(0.084)	(0.085)	(0.084)	(0.100)	(0.088)	(0.088)	(0.083)
Log Share Enrollees Using Home Health													
Year 1	0.004	0.004	-0.010	0.001	0.003	0.007	0.005	0.008	0.008	0.002	0.005	0.004	0.006
	(0.013)	(0.013)	(0.010)	(0.013)	(0.013)	(0.015)	(0.013)	(0.014)	(0.013)	(0.015)	(0.013)	(0.012)	(0.013)
1–4+	-0.007	-0.008	-0.050	-0.006	-0.003	-0.004	-0.006	-0.005	-0.005	-0.010	-0.005	-0.003	-0.003
	(0.031)	(0.032)	(0.014)	(0.032)	(0.030)	(0.037)	(0.031)	(0.032)	(0.031)	(0.035)	(0.032)	(0.031)	(0.031)
4+	-0.019	-0.015	-0.109	-0.013	-0.001	-0.009	-0.015	-0.019	-0.019	-0.025	-0.018	-0.017	-0.014
	(0.068)	(0.070)	(0.031)	(0.071)	(0.064)	(0.079)	(0.068)	(0.068)	(0.068)	(0.078)	(0.070)	(0.070)	(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 β_{rSF}'s from equation 4. Standard errors clustered at the district level are displayed in parentheses below each estimate.

Table OA.12: Effect of Individual Polices 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	-0.027	-0.008	-0.054	-0.037	-0.077	-0.055	-0.051	-0.051	-0.079	-0.047	-0.057	-0.057
	(0.052)	(0.044)	(0.032)	(0.056)	(0.050)	(0.043)	(0.052)	(0.052)	(0.052)	(0.055)	(0.054)	(0.063)	(0.051)
1–4+	-0.152	-0.109	-0.101	-0.157	-0.146	-0.182	-0.156	-0.152	-0.152	-0.173	-0.145	-0.162	-0.157
	(0.065)	(0.050)	(0.046)	(0.072)	(0.067)	(0.056)	(0.065)	(0.066)	(0.066)	(0.075)	(0.069)	(0.079)	(0.065)
4+	-0.315	-0.223	-0.256	-0.325	-0.327	-0.351	-0.320	-0.315	-0.315	-0.328	-0.305	-0.338	-0.322
	(0.106)	(0.060)	(0.091)	(0.121)	(0.113)	(0.098)	(0.106)	(0.106)	(0.106)	(0.121)	(0.114)	(0.140)	(0.105)
Log Home Health Payments per Medicare Enrollee													
Year 1	-0.112	-0.103	-0.047	-0.109	-0.095	-0.113	-0.117	-0.112	-0.112	-0.129	-0.116	-0.115	-0.119
	(0.056)	(0.052)	(0.026)	(0.056)	(0.047)	(0.064)	(0.055)	(0.056)	(0.056)	(0.060)	(0.057)	(0.063)	(0.055)
1–4+	-0.218	-0.190	-0.142	-0.218	-0.208	-0.220	-0.223	-0.218	-0.218	-0.236	-0.221	-0.218	-0.225
	(0.068)	(0.060)	(0.036)	(0.071)	(0.064)	(0.077)	(0.068)	(0.068)	(0.068)	(0.076)	(0.071)	(0.076)	(0.067)
4+	-0.383	-0.316	-0.295	-0.384	-0.390	-0.386	-0.389	-0.383	-0.383	-0.403	-0.388	-0.385	-0.391
	(0.097)	(0.074)	(0.071)	(0.107)	(0.100)	(0.105)	(0.097)	(0.097)	(0.097)	(0.109)	(0.103)	(0.120)	(0.096)
Log Share Enrollees Using Home Health													
Year 1	-0.076	-0.080	-0.016	-0.071	-0.064	-0.081	-0.078	-0.076	-0.076	-0.087	-0.076	-0.074	-0.079
	(0.047)	(0.047)	(0.017)	(0.046)	(0.042)	(0.054)	(0.047)	(0.047)	(0.047)	(0.048)	(0.047)	(0.050)	(0.047)
1–4+	-0.133	-0.130	-0.068	-0.130	-0.125	-0.140	-0.136	-0.134	-0.134	-0.147	-0.134	-0.128	-0.137
	(0.053)	(0.053)	(0.017)	(0.053)	(0.049)	(0.060)	(0.053)	(0.053)	(0.053)	(0.055)	(0.053)	(0.054)	(0.052)
4+	-0.226	-0.208	-0.150	-0.222	-0.225	-0.233	-0.229	-0.226	-0.226	-0.242	-0.228	-0.218	-0.230
	(0.062)	(0.062)	(0.027)	(0.065)	(0.062)	(0.070)	(0.063)	(0.062)	(0.062)	(0.067)	(0.064)	(0.066)	(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	0.20	0.80
Share of Enrollees using SNF	0.06	0.29	0.011
Share of Enrollees using Home Health	0.10	0.25	0.07
Panel B: Utilization Outcomes			
SNF Days per Medicare enrollees	2.20	10.12	0.30
SNF Payments per Medicare enrollees	$673	$3141	$71
Home Health Visits per Medicare enrollees	2.90	4.95	2.74
Home Health Payments per Medicare enrollees	$393	$747	$352

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
Strike Force									
Log Home Health Visits per Medicare Enrollee	-0.031	-0.009	-0.057	-0.142	-0.075	-0.220	-0.228	-0.138	-0.358
	(0.019)	(0.013)	(0.022)	(0.033)	(0.018)	(0.044)	(0.078)	(0.042)	(0.076)
Log Home Health Payments per Medicare Enrollee	-0.020	-0.007	-0.043	-0.091	-0.048	-0.164	-0.129	-0.074	-0.259
	(0.013)	(0.008)	(0.017)	(0.030)	(0.012)	(0.032)	(0.084)	(0.028)	(0.073)
Log Share Enrollees Using Home Health	0.004	0.005	-0.017	-0.007	-0.011	-0.073	-0.019	-0.020	-0.147
	(0.013)	(0.007)	(0.010)	(0.031)	(0.010)	(0.017)	(0.068)	(0.020)	(0.041)
Moratorium									
Log Home Health Visits per Medicare Enrollee	-0.051	-0.013	-0.035	-0.152	-0.038	-0.133	-0.315	-0.085	-0.305
	(0.052)	(0.027)	(0.055)	(0.065)	(0.037)	(0.072)	(0.106)	(0.059)	(0.132)
Log Home Health Payments per Medicare Enrollee	-0.112	-0.051	-0.100	-0.218	-0.072	-0.209	-0.383	-0.111	-0.383
	(0.056)	(0.021)	(0.048)	(0.068)	(0.025)	(0.063)	(0.097)	(0.039)	(0.107)
Log Share Enrollees Using Home Health	-0.076	-0.019	-0.051	-0.133	-0.026	-0.095	-0.226	-0.038	-0.170
	(0.047)	(0.016)	(0.026)	(0.053)	(0.015)	(0.028)	(0.062)	(0.016)	(0.035)
Enrollee Share	1.000	0.203	0.797	1.000	0.203	0.797	1.000	0.203	0.797

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 β_{rSF} and θ_{rM} from equation 4, with strike force effects corresponding to β_{rSF} and moratorium effects to θ_{rM}. 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	0.29	0.28	0.29	0.27	0.29
SNF Days per Capita	10.12	10.60	9.94	10.21	10.10
SNF Payments per Capita	$3141	$3479	$3011	$3095	$3155

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	0.061	0.145
	(0.014)	(0.017)	(0.029)
Log SNF Payments per Medicare enrollee	0.023	0.071	0.141
	(0.016)	(0.026)	(0.051)
Log Used SNF per Medicare enrollee	0.006	0.025	0.065
	(0.008)	(0.009)	(0.015)
	B: Moratorium		
Log SNF Days per Medicare enrollee	-0.042	-0.076	-0.122
	(0.021)	(0.023)	(0.025)
Log SNF Payments per Medicare enrollee	-0.076	-0.109	-0.155
	(0.035)	(0.038)	(0.044)
Log Used SNF per Medicare enrollee	-0.018	-0.024	-0.035
	(0.011)	(0.014)	(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 20 percent of enrollees each year who would be SNF eligible. The averages reported use the β_{rSF}'s from equation 4 for the Strike Force, and the θ_{rM}'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	—	$R_{\mathrm{SF}} = 2, R_{\mathrm{M}} = 0$	94	3.89%
2	2010	2010	—	$R_{\mathrm{SF}} = 1, R_{\mathrm{M}} = 0$	49	2.61%
3	2011	2009	—	$R_{\mathrm{SF}} = 3, R_{\mathrm{M}} = 0$	94	3.96%
4	2011	2010	—	$R_{\mathrm{SF}} = 2, R_{\mathrm{M}} = 0$	49	2.62%
5	2011	2011	—	$R_{SF} = 1, R_{\mathrm{M}} = 0$	118	2.06%
6	2012	2009	—	$R_{\mathrm{SF}} = 4, R_{\mathrm{M}} = 0$	94	4.01%
7	2012	2010	—	$R_{\mathrm{SF}} = 3, R_{\mathrm{M}} = 0$	49	2.64%
8	2012	2011	—	$R_{\mathrm{SF}} = 2, R_{\mathrm{M}} = 0$	118	2.10%
9	2013	2009	2013	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 1$	2	0.20%
10	2013	2011	2013	$R_{\mathrm{SF}} = 3, R_{\mathrm{M}} = 1$	6	1.18%
11	2013	2009	—	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 0$	92	3.81%
12	2013	2010	—	$R_{\mathrm{SF}} = 4, R_{\mathrm{M}} = 0$	49	2.65%
13	2013	2011	—	$R_{\mathrm{SF}} = 3, R_{\mathrm{M}} = 0$	112	0.92%
14	2013	2013	—	$R_{\mathrm{SF}} = 1, R_{\mathrm{M}} = 0$	13	0.18%
15	2014	2009	2013	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 2$	2	0.19%
16	2014	2011	2013	$R_{\mathrm{SF}} = 4, R_{\mathrm{M}} = 2$	6	1.07%
17	2014	2009	2014	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 1$	13	1.21%
18	2014	2011	2014	$R_{\mathrm{SF}} = 4, R_{\mathrm{M}} = 1$	5	0.43%
19	2014	2009	—	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 0$	79	2.47%
20	2014	2010	—	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 0$	49	2.65%
21	2014	2011	—	$R_{\mathrm{SF}} = 4, R_{\mathrm{M}} = 0$	107	0.48%
22	2014	2013	—	$R_{\mathrm{SF}} = 2, R_{\mathrm{M}} = 0$	13	0.18%
23	2014	—	2014	$R_{\mathrm{SF}} = 0, R_{\mathrm{M}} = 1$	3	0.13%
24	2015	2009	2013	$R_{\mathrm{SF}} = 7, R_{\mathrm{M}} = 3$	2	0.18%
25	2015	2011	2013	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 3$	6	1.04%
26	2015	2009	2014	$R_{\mathrm{SF}} = 7, R_{\mathrm{M}} = 2$	13	1.18%
27	2015	2011	2014	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 2$	5	0.42%
28	2015	2009	—	$R_{\mathrm{SF}} = 7, R_{\mathrm{M}} = 0$	79	2.42%
29	2015	2010	—	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 0$	49	2.66%
30	2015	2011	—	$R_{\mathrm{SF}} = 5, R_{\mathrm{M}} = 0$	107	0.48%
31	2015	2013	—	$R_{\mathrm{SF}} = 3, R_{\mathrm{M}} = 0$	13	0.17%
32	2015	—	2014	$R_{\mathrm{SF}} = 0, R_{\mathrm{M}} = 2$	3	0.13%
33	2016	2009	2013	$R_{\mathrm{SF}} = 8, R_{\mathrm{M}} = 4$	2	0.18%
34	2016	2011	2013	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 4$	6	1.08%
35	2016	2009	2014	$R_{\mathrm{SF}} = 8, R_{\mathrm{M}} = 3$	13	1.20%
36	2016	2011	2014	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 3$	5	0.43%
37	2016	2009	2016	$R_{\mathrm{SF}} = 8, R_{\mathrm{M}} = 1$	71	1.01%
38	2016	2010	2016	$R_{\mathrm{SF}} = 7, R_{\mathrm{M}} = 1$	35	1.74%
39	2016	2011	2016	$R_{\mathrm{SF}} = 6, R_{\mathrm{M}} = 1$	107	0.48%
40	2016	2009	—	$R_{\mathrm{SF}} = 8, R_{\mathrm{M}} = 0$	8	1.53%
41	2016	2010	—	$R_{\mathrm{SF}} = 7, R_{\mathrm{M}} = 0$	14	1.00%

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

Table OA.18: Example HCFAC Cost Report

FY 2009 ALLOCATION OF HCFAC APPROPRIATION[5] (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	**$56.025**[7]	**$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				
Observed Placement				
Strike Force Only (34%)	0.16	0.00	0.69	0.15
Moratorium Only (19%)	0.12	0.01	0.60	0.27
Strike Force and Moratorium (47%)	0.16	0.00	0.65	0.19
Ever-treated counties (100%)	0.15	0.00	0.65	0.19
Panel B. All counties				
Observed Placement				
Strike Force Only (12%)	0.06	0.00	0.41	0.52
Moratorium Only (6%)	0.05	0.00	0.42	0.53
Strike Force and Moratorium (16%)	0.09	0.00	0.45	0.45
No Policy (66%)	0.05	0.00	0.11	0.84
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.