Targeting Overuse of Home Health Care: Evidence from Multiple Policy Instruments

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Abstract

Multiple public policies are often concurrently employed to tackle a common objective, yet they are typically analyzed independently. We study the joint implementation of three very different policy instruments that have been deployed to reduce wasteful use of Medicare-financed home health care: geographically targeted strike force prosecutions of suspected fraud, moratoria on the entry of new home health agencies in specific counties, and a national cap on outlier payments received by home health agencies. We find that each policy reduced home health care use by an average annual 20 to 30 percent over five years, with no offsetting use of nursing home care. We examine potentially heterogeneous treatment effects across observable patient and provider characteristics in order to consider the counterfactual performance of alternative geographic targeting of the strike force and moratoria, holding their budgets fixed. We find that these policies were targeted to places with higher-than-average treatment effects, and that, in the absence of other policies, achieved about 80 percent of the savings from optimal targeting. However in the presence of the outlier payment cap which targeted similar places, optimal geographic targeting of these policies could have more than doubled the achieved savings. These findings underscore the value of coordination across policies pursuing similar objectives.

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1 Introduction

Governments often deploy multiple different policy instruments aimed at a common objective. 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 in practice, multiple related policies remain the norm, and economists have paid relatively less attention to how this can affect the optimal deployment of each. In this paper, we study this issue in the context of the U.S. Medicare program and the battery of different policy tools it has introduced in the first two decades of the 21st century in an effort to crack down on the rapid rise of Medicare-financed home health care and the related perception of substantial waste and fraud (Lee and Skinner 2021).

Medicare home health care looms large in the lives of the elderly. In 2019, one-in-twelve Medicare enrollees aged 65 and over - and one-in-five of those aged 85 and over - used home health care, producing $20 billion in annual spending Medicare home health care spending, or about 30 percent of total Medicare spending on post-acute care.[1] Yet home health care occupies an uncertain position, teetering on the boundary between health-promoting care and a utility-promoting consumption good. This was well-encapsulated by a recent government report on Medicare home health care (Medicare Payment Advisory Commission 2020):

> "Policymakers have long struggled to define the role of the home health benefit in Medicare... From the outset, there was a concern that setting a narrow policy could result in beneficiaries using other, more expensive services, while a policy that was too broad could lead to wasteful or ineffective use of the home health benefit."

Recognizing both the promise and peril of Medicare-financed home health care, the government has engaged in a series of Medicare policy reforms for home health over the last half century.[2] These have been temporally 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 home health care utilization.

In this paper we study three distinct Medicare home health care policies introduced between 2009 and 2016: (i) geographically-targeted Strike Forces introduced between 2009 and 2013 to detect and prosecute home health care fraud; (ii) geographically-targeted moratoria on the entry of new home health agencies in 2013 and 2016; and (iii) a a 2010 nationwide cap on the amount of outlier

[1] Home health care use is based on authors' calculations described below. Home health care spending is from Medicare Payment Advisory Commission (2021a,b).

[2] So too have most other high-income countries. These countries currently exhibit substantial heterogeneity in their public policies regarding coverage and reimbursement of home health care and, relatedly the prevalence of home health care. Indeed, the share of combined home care and nursing home care spending that is devoted to home health care varies considerably across high-income countries, ranging from under one-quarter in Spain to almost two-thirds in Japan; the United States occupies a middle ground at about two-fifths (Gruber et al. 2023b).

payments (for high spending patients) a home health agency could receive. Using the universe of Medicare claims data for two decades (1999-2019), we exploit variation in the timing and spatial application of these policies to estimate their impacts - both on average and heterogeneously across patients and providers - and to consider the counterfactual performance of alternative geographic targeting of the policies. Depending on the location and time period, anywhere from no policies to three policies were in effect. This provides a rare opportunity to study the impacts of multiple policies with a common objective, both individually and in different combinations.

The deployment of these different policies over similar time periods and populations provides us a rare opportunity to analyse their optimal allocation not only individually but in combination with each other.

We find that each policy has substantial and sustained effects on home health care use. These effects appear immediately and increase (in absolute value) over our 5-year post implementation study period. On average over their first five years we find that the Strike Forces - which are by far the most expensive to administer of the three policies - reduced home health care use per capita by about 30 percent and home health care payments per capita by about 20 percent, while the moratoria reduced both outcomes by about 20 percent and the outlier payment cap reduced use by about 20 percent and payments by about 7 percent.. The primary purpose of the Medicare home health care program is to provide care for patients who would otherwise need to be in a more expensive (and typically less preferred) nursing home (Lee and Skinner 2021). But consistent with the notion of substantial 'waste' in Medicare home health care, we find no substantive or statistical evidence that these large policy-induced reductions in home care are associated with any substitution toward nursing home care, either overall or for patients at high risk of using a nursing home.

In the second part of the paper we consider whether counterfactual geographic targeting of these policies could have substantially improved their efficacy either individually or in combination. To do so, we exploit recent advances in the use of causal forests to examine potentially heterogeneous treatment effects across observable characteristics of patients and home health agencies (e.g., Athey et al. 2019). We document substantial heterogeneity in treatment effects; for example, each of the policies tend to have larger (more negative) impacts on home health care use when applied to home health agencies that entered more recently or have more patients from the community.

Each of these policies targeted areas with large amounts of suspected waste based on their high (and growing) rates of home health care use. Our counterfactual findings indicate that the geographically focused strike forces and moratoria were also targeted at places with higher-than-average treatment effects of these policies. However we also find that in the presence of the outlier payment cap, optimal placement of these policies across geographies, holding their budgets fixed, could have more than doubled their combined impact relative to the impacts from where they are actually placed. The government's strategy of deploying the resource-intensive strike forces at high-

use, high-suspected fraud places (such as Miami and Los Angeles) is much closer to optimal in the absence of the outlier payment cap. In the presence of this payment reform technology which also effectively targets high use places, however, we find that it would have been much more effective to deploy the strike forces elsewhere. Our findings highlight the potential gains from coordination across different policy instruments pursuing similar objectives.

Our paper contributes to several related literatures. Most broadly, we contribute 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. 2022), unobservables (e.g., Einav et al. 2022; Ito et al. 2023), or both (Ida et al. 2022). A key theme has been the importance of targeting policies based on treatment effects (the $\hat{\beta}$'s or 'slopes') rather than the more easily-observed outcomes (the $\hat{y}$'s or 'levels'). Our findings underscore that, when considering the (frequent) problem of optimal deployment of multiple policies, a problem which to our knowledge has received considerably less research attention, optimal targeting should consider *incremental*, rather than gross, treatment effects.

Our specific application tackles a central topic in both health economics and health policy: how to effectively combat the substantial waste in the U.S. health care system, where waste is typically defined as medical spending that provides little or no benefit to patient health. Healthcare spending is one of the largest fiscal challenges facing the public sector in the United States, with health care spending accounting for almost one-fifth of the economy, and half of that spending taxpayer financed. There is widespread agreement that a large portion - perhaps one quarter or one-third - of healthcare spending is waste (e.g., Orszag 2009; McGinnis et al. 2013; Shrank et al. 2019). Yet there is little agreement on how to design policies to reduce wasteful spending without also reducing patient access to valuable care (e.g., Cutler 2010; Doyle et al. 2015). In this paper, we identify three such policies that each in the context of Medicare home health care, and explores how to optimally target these policies based on observable characteristics.

Finally, we contribute to existing work on Medicare home health care. Our findings add to the limited existing evidence - from a bespoke randomized experiment in the early 1980s and a 1997-2000 Medicare payment reform - (Kemper 1988; McKnight 2006) suggesting that Medicare-financed home health care dos not appear to substitute for nursing home care as it is intended. We complement these important analyses with sample sizes that provide more statistical power to detect potential substitution, and analysis of a more recent time period in which Medicare policy toward home care is very different, with potentially different implications for nursing home substitution, than in these earlier periods. In prior work on the policies we analyze O'Malley et al. (2023) document the role of patient-sharing across home health agencies in contributing to the spread of home health care fraud in areas targeted by strike forces, and Kim and Norton (2015) study the impact of the outlier payment cap on home health care utilization.[3]

[3]There is also a growing literature on policies designed to combat health care fraud outside of the home health care setting. Recent papers have studied the impact of whistle-blower filings against health care providers under the

The rest of the paper proceeds as follows. Section 2 describes our setting, including background information on home health care and the three policies we will study. Section 3 describes our data, sample and summary statistics. In Section 4 we estimate the impact of each of the three policies on home health care use, and analyze whether the induced reduction in home health care generates any substitution to nursing homes. Section 5 estimates heterogeneity in the impact of these policies across home health agencies and patient types, and uses these estimates to analyze the performance of counterfactual policy placements. The final section concludes.

2 Setting

2.1 Long-term care in the U.S.

The aging of the U.S. population has brought with it a rapid growth in the use of nursing home and home health care, most of which is publicly financed. The share of the U.S. population that is 65 and older has risen dramatically over the last 20 years - from about 12 percent in 2000 to about 17 percent in 2019 - and with it the share of GDP spent on long term care has also risen, from about 1.1 percent in 2000 to about 1.3 percent in 2019. Spending on long-term care is likely to increase further as the population continues to age; the OECD projects that by 2060 about one-quarter of the U.S. population will be over 65 (Gruber and McGarry 2023).

Long-term care includes care in an institutional setting (typically a nursing home) and care received in the home by paid caregivers.[4] In 2019, institutional care accounted for about three-fifths of spending on paid long-term care, with home health care accounting for the remaining two-fifths. (Center for Medicare & Medicaid Services 2023b)

About two-thirds of long-term care in the U.S. is publicly financed. Medicare - the federal public health insurance program for the elderly and disabled - pays for a little over half of that care, while Medicaid - the joint federal-state public health insurance program for low income individuals - pays for the rest; Medicare pays for a slightly larger share of publicly financed home health care (about 60 percent) (Gruber and McGarry 2023). Importantly, Medicare coverage for long-term care is limited to individuals who need 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.

False Claims Act (Leder-Luis forthcoming; Howard and McCarthy 2021), the relative efficacy of ex-post anti-fraud litigation and ex-ante regulation for combating fraudulent use of Medicare-financed ambulances as taxi services for patients' regular dialysis treatments (Eliason et al. 2021), and the use of revenue caps and anti-fraud litigation in reducing use of Medicare-financed hospice (Gruber et al. 2023a).

[4] It also includes informal, unpaid care received in the home, often provided by relatives. However, our focus is exclusively on paid care - either in a nursing home or at home. This type of care has direct fiscal consequences for public insurance programs, and is also substantially easier to measure. Nonetheless, the potential for informal care looms large in our analysis and in considerations of optimal policy design, as it represents a large potential margin of elasticity of paid home health care use.

2.2 Medicare coverage of home health

Medicare covers short-term nursing home stays for patients recovering from a surgical procedure or acute health event (such as a stroke). To be eligible for Medicare coverage, the nursing home stay must start within 30 days of the patient being discharged from the hospital after a hospitalization that lasted for at least three days (Center for Medicare & Medicaid Services 2022b). Medicare-covered home care, however, does not require a recent prior hospital stay. To qualify for home health care, a Medicare enrollee must have difficulty leaving the home without considerable effort and require part-time or intermittent skilled care; a health care provider must certify this need and re-certify it at least every 60 days. Medicare home health care encompasses a range of services delivered to patients in their homes to facilitate their recovery. This includes skilled nursing care, physical therapy, speech-language pathology services, occupational therapy, and home health aide services. Unlike most of Medicare-covered services, home health has no patient cost sharing, with the exception of a 20% coinsurance for any durable medical equipment or biologics provided in connection with the home health visits (Center for Medicare & Medicaid Services 2022a, Center for Medicare & Medicaid Services 2023a).

Medicare coverage of home health care offers the tantalizing prospect that through the provision of relatively inexpensive services in the home, patient health can be protected while they remain in their home rather than in a much more expensive – and usually less preferred – nursing home. At the same time, by subsidizing services that may be inherently appealing to many individuals, there is concern about substantial scope for use of home health care by patients who would otherwise not have used facility-based care and for whom the home care may provide little health benefit. As Lee and Skinner (2021) put it 'While the promise of [Medicare] home health care services is great—to keep frail people out of the hospital or nursing home by supporting them in their house—so also is the potential for fraud and "gray area" use of visits with little value to health.' As noted in Section 1, the explicit rationale of Medicare-financing for home health care is to reduce Medicare-financed nursing home use (or in principle other Medicare-financed care, of which nursing home use is the most natural margin of substitution). The potential savings are large - we estimate in our data below that covering a patient for a week in a nursing home costs Medicare over five times as much as covering them with home health care for a week - but so is the potential for substantial waste.

Somewhat surprisingly - both in light of this goal and given the natural possibility of such substitution - we have little evidence to date that Medicare home health care can reduce use of nursing home care. In the early 1980s, the channeling demonstration - a randomized controlled trial of the impact of providing more comprehensive case management and more coverage of community services on nursing home use - was explicitly designed by the Department of Health and Human Services to test this substitution hypothesis. Despite targeting an elderly and extremely frail population (the average age of the population served was 80, about 85 percent were restricted in their ability to perform activities of daily living and the remainder had multiple limitations to

instrumental activities of daily living) and successfully increasing the amount of formal community services used, this 10-site, 6,000-person intervention did not substantively reduce nursing home use over the 12-18 month period of the study (Assistant Secretary for Planning and Evaluation 1991) One reason that has been conjectured is the relatively low rates of nursing home use during the follow up period in the control group (about 13 to 14 percent); relatedly, the one site where the intervention produced meaningful substitution had substantially higher rates of nursing home use of about 50 percent in the subsequent year (Kemper 1988).

About two decades later, the dramatic, 30 percent decline in home health care use from 1997 - 2000 (see Figure 1) associated with the introduction of the Medicare Interim Prospective Payment System (IPPS) provided another opportunity to test the substitution hypothesis. Using variation across states in the generosity of the interim prospective payment rates relative to the historical fee-for-service payment rates, McKnight (2006) documented a substantial reduction in home health care use associated with IPPS; yet she found no corresponding increase in nursing home use or other adverse health consequences. These results again suggest little role for Medicare-covered home health care in reducing nursing home use, but they also leave open the possibility raised in response to the Channeling results that there could be meaningful substitution among the set of patients at risk for high rates of nursing home use. Such effects could have been quantitatively important for Medicare spending yet hard to detect in the data available for that time period; McKnight (2006)'s analysis uses the 1992-1999 Medicare Beneficiary Survey data on about 100,000 individuals, about 15 percent of whom reported some home health care use.

2.3 Medicare home health policy reforms

Over the past several decades, the utilization of home health care has experienced significant fluctuations that coincided with changes in Medicare payment policies (see Figure 1). Home health utilization witnessed a steep increase immediately following a 1998 class action lawsuit (Duggan vs Bowen) which prompted the Department of Health and Human Services to issue a clarification that allowed patients to receive home health services for stable needs, rather than requiring the patient's condition to be improving (Liu et al. 1999). Between 1990 and 1996, Medicare home health spending increased from $4 billion to $17 billion, raising concerns among policy makers over high levels of spending (Liu et al. 2003), and prompting Congress to reform the home health payment system in the Balanced Budget Act of 1997, first introducing the Interim Payment System (IPS) and later the Home Health Prospective Payment System (HH PPS). Prior to the IPS, home health agencies were reimbursed for their costs up to a per-visit cap. The IPS lowered the per-visit cap and additionally introduced an agency-level annual per-patient cap. The result was a steep decline in the number of visits per home health patient from 78 in 1997 to 46 in 1999 and a reduction in the total number of home health patients from 3.3 to 2.5 million (McCall et al. 2001).

In October 2000, the prospective payment system that is in place to this day was introduced;

home health care is prescribed in 60-day episodes, with the payment for the episode determined based on the patient's severity and geography (Medicare Payment Advisory Commission 2021b; Vontran and Gehne 2019); if a patient's care is exceptionally costly, the home health agency may receive an additional high-cost outlier payment on top of the regular payment (Medicare Payment Advisory Commission 2017). Following the implementation of PPS, utilization began to steadily increase throughout the 2000s (see Figure 1).

What followed were a series of efforts to reverse this increase in home health care use and that are the focus of our study. In particular we study three specific policies designed to combat rising home health care use and perceptions of wide-spread fraud and overuse: strike forces, moratoria on home health agency entry, and an outlier payment cap on home health agencies.[5] The Strike Forces were created specifically to combat what was perceived to be substantial amounts of prosecutable fraud; the other policies were designed to combat perceptions of waste or over-use more generally. The policy instruments vary widely in their design and, relatedly, their administrative costs, providing an opportunity to assess a range of possible types of interventions and to assess the relative efficacy of different instruments individually or in combination.

Strike Forces

The most resource-intensive of such efforts were the Strike Forces, joint efforts between the Department of Justice (DOJ) and the Department of Health and Human Services Office of the Inspector General (HHS-OIG) designed to detect and prosecute home health care fraud. The use of Strike Forces in Medicare has not been limited to home health care; they have also been used to prosecute Medicare fraud in areas such as durable medical equipment, IV infusion therapy and, by the tail end of our study period, prescription opioids (Department of Health & Human Services & Department of Justice 2019). During our study period, home health comprised about half or more of the cases pursued by Strike Forces (Department of Justice 2013, Department of Justice 2016). To our knowledge, we present the first empirical academic study of the impact of these Strike Forces.

Fraud in home health has been a major concern of the regulatory agencies, prompted by the dramatic growth in home health care starting in the early 2000s (see Figure 1), and its concentration in particular regions. In 2008, the five highest spending core-based statistical areas made up 20% of total home health spending. One suspect area was in Miami-Dade, Florida, where home health spending per beneficiary quadrupled between 2002 and 2008. Another suspect area included five

[5] Two other notable policies occurred during our study period, which ends in 2019. One was the pre-claims review requirement, first launched in August 1, 2016 in Illinois and suspended on April 1, 2017. Given the short time period it was in effect, we study the impact of the policy and discuss it briefly in Appendix B. The other policy was the introduction of a required face-to-face encounter between a physician and patient to document the patient's eligiblity for home care. This requirement was introduced in the second quarter of 2011 as part of the Affordable Care Act. According to a MedPAC report, the five states with the fastest-growing home health episode volume prior to 2011 experienced a 25 percent decline in volume between 2011 and 2019 (Medicare Payment Advisory Commission 2019). However, since the policy was introduced nationwide, it is difficult to isolate the causal impact of the face-to-face requirement from the myriad of concurrent changes.

counties, all located in Texas, in which the number of home health episodes exceeded the number of Medicare enrollees (Medicare Payment Advisory Commission 2010b, Lee and Skinner 2021). Home health fraud can take different forms, but the majority of cases pursued by the OIG and the DOJ involve one or more of the following activities: billing for excessive and unnecessary care, providing care to ineligible patients, and billing for services that were never provided. (Department of Health & Human Services & Department of Justice 2010, Department of Health & Human Services & Department of Justice 2012).

The first Strike Force started in 2007 in the Southern District of Florida (which includes Miami), initially focusing on fraud in durable medical equipment, before shifting its focus to home health fraud in 2009.[6] Over the next six years Strike Forces targeting home health care were launched in another nine federal judicial districts as part of the Heath Care Fraud Prevention and Enforcement Action Team (HEAT). The districts were chosen for Strike Forces based on where Medicare claims data revealed aberrant billing patterns or intelligence suggested potential fraud (Department of Health & Human Services & Department of Justice 2010, Department of Health & Human Services & Department of Justice 2011). Each Strike Force team brought together the investigative and analytic resources of multiple agencies in addition to the HHS and the DOJ, including the Federal Bureau of Investigation (FBI), the Defense Criminal Investigate Service (DCIS), the Federal Deposit Insurance Corporation Office of the Inspector General (FDIC-OIG), the Internal Revenue Services (IRS), among others. The DOJ Criminal Division's Fraud Section and the US Attorney's Offices (USAOs) brought forward cases in federal district courts (Department of Health & Human Services & Department of Justice 2010, Department of Health & Human Services & Department of Justice 2011). By the end of 2017, Strike Force teams operating the 10 districts had filed over 2,400 cases against about 600 defendants for about $2 billion in losses (Office of the Inspector General 2018).[7] It is difficult to exactly identify the government costs of prosecuting home health care through Strike Forces - both for the usual reasons of separating fixed and variable costs and because of the aggregated nature of reported government spending; however, our rough back of the envelope calculation in Appendix C suggests that each Strike Force spends about $120 million per year on home health care.

Figure 2a shows where and when strike forces were created in different districts. Appendix Table OA.1 presents more specifics on the dates of entry.

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

[7] Additional Strike Force teams were opened in Newark, NJ and Philadelphia, PA in August 2018, and in Orlando in November, 2018. However, since our sample period ends in 2019, we chose not to include these areas in our analysis. Washington, DC and the Appalachian Region also joined the Strike Force in 2018, but unlike the ones we study, they focused on corporate fraud and prescription opioids, respectively, rather than home health care and we excluded them from our analysis (Department of Health & Human Services & Department of Justice 2019).

Home health agency moratoria

The moratoria on new home health agencies were also joint efforts between the DOJ and HHS-OIG. HEAT also recommended moratoria on new home health agencies in particular counties where it felt that this was necessary for combating fraud (Center for Medicare & Medicaid Services 2013). Even though 99% of beneficiaries already lived in a zip code with a Medicare home health agency, the number of home health agencies continued to increase, with growth especially concentrated in the states of Texas, Florida, and Michigan. The rapid growth raised concerns over fraud and abuse (Medicare Payment Advisory Commission 2010a). Medicare introduced the first wave of moratoria in 2013 - right around the time of the last Strike Force that was introduced (in 2013 in the Eastern District of Louisiana) - in a variety of counties in Chicago, IL, Miami, FL, Houston, TX, Fort Lauderdale, FL, Detroit, MI, and Dallas, TX. In 2016, the moratoria expanded to include all counties in the states of Illinois, Texas, Florida, and Michigan (Center for Medicare & Medicaid Services 2016).

Figure 2b shows where and when home health agency moratoria were introduced in different counties. Appendix Table OA.2 presents more specifics on the dates of entry. Note too that there is a fair amount of overlap between where Strike Forces enter (Figure 2a) and moratoria (Figure 2b) enter. Specifically, we estimate that about 70 percent of Medicare-enrollee-weighted counties with a Strike Force received a moratorium, and vice versa, although as noted the moratoria came in after the Strike Forces. As a result, in the empirical analysis below we will analyze these two policies simultaneously.

Outlier payment caps

A national effort to reduce wasteful home health care spending was a 2010 reform that capped the outlier payments that home health agencies could receive. The policy, which was announced in 2009, stipulated that outlier payments could not make up more than 10% of total Medicare home health payments to the agency each year, above which the home health agencies would not receive further outlier payments. The reform was prompted by what Medicare described as "excessive growth in outlier payments in a few discrete areas of the country . . . where outlier payments to providers far exceed the national average and the 5 percent target for outlier payments . . . [and] the size of these statistical anomalies raised concerns about the medical necessity of the outlier episodes in some areas." (Medicare Payment Advisory Commission 2019). Since then, home health utilization has been on a steadily declining trajectory (see Figure 1). The effect of this outlier payment cap has been analyzed in prior literature: Kim and Norton (2015) exploits the variation in the proportion of outlier payments prior to the implementation of the cap to study how home health agencies adjusted the number of home health visits. The study finds a large reduction in the number of home health visits and that the effects are largest for relatively healthy patients.

Figure 2c shows the variation across U.S. counties in the share of home health care spending

in 2009 that would have been above the agency cap that was introduced in 2010 and presents summary statistics.[8] Only about 7 percent of (enrollee-weighted) counties and 11 percent of (home health care episode-weighted) HHAs had outlier payments above the cap in 2009; conditional on being above the cap, however, the average outlier payment share was 18 percent and 30 percent for counties and HHAs respectively (not shown).

3 Data and Summary Statistics

Our primary data is a 20% random sample of Traditional Medicare beneficiaries between 1999 and 2019. We limit to beneficiary-years enrolled in Medicare Parts A and B throughout the entire year; among other things, this excludes beneficiary-years enrolled in Medicare Advantage for all or part of the year. We use data from the Master Beneficiary summary file, which contains information on patient demographics - including zip code of residence, race, and gender - and summary measures of annual health care utilization and spending by separate categories, including home health care, skilled nursing facilities (SNFs), and inpatient hospital admissions. We also use more granular data on home health care claims, including the start and end dates of each episode of care, the specific agency that provided the care, and value of outlier payments in each episode. Finally, we obtain information on each home health agency - including its opening date and its ownership form - from the provider of service files.

Our primary outcomes of interest are home health care and SNF use and spending at the county-year level. For each type of care, we analyze three different measures of utilization, all on a per-capita basis:[9] an extensive margin measure of the share of patients who received any of that care that year, an intensive margin measure of the amount of care received (home health care visits or days in the SNF), and the amount of spending on that care.

We also find it useful to analyze these outcomes separately by several different measures of beneficiary type. First, we use the referral code variable in the home health care claims file to classify beneficiaries as either being 'from the community' (i.e. referred directly by a physician) or 'from the hospital' (the complement of that category, which includes patients admitted from any form of institutional care). We do this because beneficiaries coming from the community are both likely to be healthier and also, based on anecdotal description, more likely to be a source of fraudulent claims. This classification is only feasible for beneficiary-years with home health care claims.

Second, we take two approaches to classifying beneficiaries who are more vs. less likely to use a Medicare-covered SNF in the absence of home health care. We define home health care (or SNF) care as 'SNF qualifying' if the care starts being used by a patient within 30 days of their being discharged from an inpatient hospital stay that lasted at least three days. We also classify

[8] We discuss the data behind this in more detail in the next section.

[9] More specifically, we analyze each outcome per 100 Traditional Medicare enrollees.

beneficiaries based on their predicted SNF use. Specifically, we predict the number of days in a SNF that each patient would have in a 'training sample' of patients between 2001 and 2004 living outside of areas that ultimately became Strike Force districts; we make these restrictions to generate a measure of predicted SNF use before the major increase in home health care use (see Figure 1) and the related concerns about fraud. We generate the prediction by running a patient-year regression of SNF covered days on age (linearly), sex and race indicators, indicators for 27 chronic conditions, and prior year's Medicare spending broken into 13 categories that together encompass all Medicare Part A and B spending except for home health and SNF; we use the resulting estimates to predict a level of SNF utilization for all patient-years in the sample.

Summary statistics

As seen in Figure 1, the share of individuals using home health care use is rapidly rising over the 2000 to 2009 period, and then experiences a pronounced downturn, coinciding with the introduction of the (national) 2010 outlier cap policy as well as the initial introduction of Strike Forces and home health agency moratoria. Rates of home health care visits and spending per enrollee exhibit similar patterns (Appendix Figure OA.1).[10]

In 2019, 8.0% of Medicare beneficiaries (3.2 million) used Medicare-financed home health care services; the average patient receiving home health care had about 31 visits, spanning 1.9 60-day episodes. This care was provided by over 11,000 Medicare-approved home health agencies (or about 300 patients receiving home care per agency).

We estimate that covering a patient for a week in a SNF costs Medicare over five times as much as covering them with home health care for a week.On average over the time period in our data, a day in a SNF costs Medicare $360, and a home health care visit costs Medicare $143; on average a Medicare enrollee in home health care receives 3.3 visits per week. .

The first panel of Table 1 displays (population weighted) county means for each of the home-health care outcomes in 2007, prior to the introduction of the policies. The next panel shows the growth rate of these outcomes from 2003-2007. The first column shows results for all counties. On average in 2007, about 9 percent of patients used home health care, with (unconditional) average annual visits per capita of 3.1 and spending per capita of $420. About three-quarters of these visits came from patients from the community. These rates represent a substantial growth over the 2003-2007 period in all three measures.

The remaining columns show these outcomes separately for counties affected by different policies. Not surprisingly - indeed reassuringly - these policies do not appear to be randomly assigned. Counties that had Strike Forces or moratoria or a larger impact of the outlier payment cap had both higher rates of home health care use in 2007 and higher rates of growth in use between 2003

[10]Appendix Figure OA.2 shows that Medicare-covered SNF use also flattens out around a similar time. This appears to be due to the general decline in inpatient use around this time; unlike for home care, eligibility for Medicare coverage of SNF care requires a prior hospital stay.

and 2007 than counties that did not. We now turn to our empirical specifications which will take account of this non-random nature of the policy implementation.

4 Policy impacts on home health care and nursing home use

In this section, we evaluate the direct impacts of each of the three policies designed to reduce home health care use, as well as the impact of policy-induced home health care reductions on nursing home use. All of our analyses are at the county level. For home health care use, we analyze three main dependent variables: the log number of enrollees with home health care visits per 100 enrollees, the log number of home health care visits per 100 enrollees, and the log home health care spending per 100 enrollees.[11]. To examine substitution, we analyze the relationship between home health care visits per capita and SNF days per capita, or between health payments per capita and SNF payments per capita, using the policies to instrument for home health care utilization. A key feature that we will account for in our estimation approach is that, as discussed in the preceding section, many of these policies are - not surprisingly (and indeed, perhaps reassuringly) - not randomly assigned across the United States.

4.1 Policy Impacts on Home Health Care Use

4.1.1 Strike Forces and Moratoria

As discussed, because of the considerable overlap in the geographic placement of moratoria and Strike Forces (recall Figure 2) we analyze their impact together. We start with a standard county-year event study specification:

$$y_{ct} = \alpha_c + \tau_t + \sum_{r \neq -1} \beta_r SF_{cr} + \sum_{\tilde{r} \neq -1} \theta_{\tilde{r}} M_{c\tilde{r}} + \epsilon_{ct} \quad (1)$$

where y_{ct} denotes the outcome of interest in county c and year t, α_c denotes county fixed effects, and τ_t denotes calendar year fixed effects. SF_{cr} is an indicator variable that takes the value 1 if it is year r relative to the year a Strike Force opened in the district containing county c, and $M_{c\tilde{r}}$ is an indicator variable that takes the value 1 if it is year $\tilde{r}$ relative to the year a Moratorium was introduced in county c; both sets of indicators are always zero for counties that never received the intervention. For both sets of relative year indicators, we omit relative year -1 (the year prior to entry), and we include 'catch all' indicators for relative years -6 and earlier or relative years 6 and later . We weight each county-year by its 2006 Traditional Medicare enrollees, and cluster the

[11] We add one to all measures to avoid taking logs of zeros. In practice this affects less than 0.06% of the population-weighted county-years. And we show in robustness analysis below that the results are extremely similar if we instead estimate a Poisson specification for the home health care measures, as recommended by Chen and Roth (forthcoming)

standard errors at the district level, which are combinations of counties; there are 89 districts in our analysis.

As shown in Table 1, home health care use was growing more rapidly in the treatment counties prior to the Strike Force and Moratoria interventions. Therefore, as can be seen in Figure 3 estimation of equation 1 will show strong positive pre-trends in home health care use in the years prior to the introduction of the Strike Force (although not for the moratorium conditional on the Strike Force). We therefore allow for a logarithmic trend in the outcome variable in the absence of each policy. Specifically we estimate:

$$y_{ct} = \alpha_c + \tau_t + \sum_{r \notin \Omega} \beta_r SF_{cr} + \gamma \log(R_{cr}) + \sum_{\tilde{r} \notin \Omega} \theta_{\tilde{r}} M_{cr} + \phi \log(\tilde{R}_{c\tilde{r}}) + \epsilon_{ct} \quad (2)$$

where R_{cr} is a continuous variable taking the values of the year relative to the opening of a Strike Force office in county c and year t (this equals zero in all years for non-Strike Force areas), and $\tilde{R}_{c\tilde{r}}$ is defined analogously for the moratorium relative years; all other variables are defined the same way as in equation (1).[12] The set Ω denotes relative years -5 through -1; we omit indicator variables for the presence of strike forces or moratoria in those relative years in order to use those relative years to estimate the pre-period trend in outcomes in areas that subsequently receive these policies. In this specification, β_r now represents the effect of the Strike Force relative to a logarithmic trend in the outcome variable fitted in the five years prior to a Strike Force office opening. Likewise, $\theta_{\tilde{r}}$ now represents the effect of the moratoria relative to a logarithmic trend in the outcome variable fitted in the five years prior to a moratorium on HHA entry.

Figure 3 shows the role of accounting for these pre-trends for one of our outcome variables: log home health care visits per 100 enrolees. The solid lines show - separately for the Strike Force (panel a) and the Moratoria (panel b) the estimated impacts based on the standard event study from equation (1), while the dashed line shows the estimated pre-trends based on equation (2). The solid lines show a pronounced decline in home health care vists after the introduction of each policy. The dashed lines, however, indicate the presence of a substantial positive pre-trend for the Strike Force, although little if any pre-trend for the moratorium.[13] This suggests that failing to account for the endogenous placement of these policies in areas with rapidly growing home health care use could cause us to under-estimate their effects. The figures also suggest that the log-trend fits the pre-period growth well.

As a result, for our main estimating equations we will account for these trends by estimating:

[12] Note that we also add 7 to R_{cr} and to $\tilde{R}_{c\tilde{r}}$ so that their logs are never undefined (the earliest relative year in the data is -6).

[13] Note that the pre-trend for the Moratorium estimated in equation (2) is estimated conditional on the Strike Force moratoria. As shown earlier, both policies tended to be introduced in areas with rising home health care use, but there was also substantial geographic overlap in the introduction of these two policies, a point we will return to below when we consider optimal geographic allocation of these policies.

$$\tilde{y}_{ct} = \alpha + \sum_{r \neq -1} \beta_r SF_{cr} + \sum_{\tilde{r} \neq -1} \theta_{\tilde{r}} M_{c\tilde{r}} + \epsilon_{ct} \tag{3}$$

where $\tilde{y}_{ct} \equiv y_{ct} - \hat{y}_{ct}$, and $\hat{y}_{ct} = \hat{\alpha}_c + \hat{\tau}_t + \hat{\gamma} \log(R_{cr}) + \hat{\phi} \log(\tilde{R}_{c\tilde{r}})$ denotes the predicted outcome in the absence of the policy is the predicted outcome in the absence of the policy based on estimation of equation (2). In other words, equation (3) looks at the impact of the staggered implementation of the strike force (SF_{cr}) and moratoria ($M_{c\tilde{r}}$) across areas and years on $\tilde{y}_{ct}$, the outcome of interest residualized to account for county and year fixed effects, as well as pre-implementation outcome trends in places that receive a strike force or moratorium.

In what follows, we report results from estimating equations (3), however in Appendix Section D we show that results are very similar if we instead estimate the straightforward event study (1) and examine results relative to the estimated pre-trend from equation (2); these exercises also show that the logarithmic pre-trend tends to fit well across outcomes and policies.

Strike Force estimates

Figure 4 shows the estimated impacts of the Strike Force for all three outcomes, which are the estimates of β_r from equation (3). These estimates show a pronounced decline in all three measures of home health care use after the Strike Force office opens. This decline begins immediately in the year of Strike Force entry (relative year 0), and grows substantially over the five year period examined.

Table 2 summarizes these results. Specifically, for each of the three home health care outcomes, the top row of each panel summarizes the estimated average impact of the strike force in its year of entry, its first three years, and its first five years. These are based on linear combinations of the various estimates of β_r in equation (3). The results indicate that overall, home health utilization declined substantially as a result of Strike Force activity. For example, we estimate that home health visits per 100 enrollees decreased by about 29.2 percent (standard error = 4.1). Medicare home health care payments per capita declined by a similar amount (21.7 percent, standard error = 3.4). Interestingly, the effects of the Strike Force on the extensive margin—the share of patients in each county using home health care—are much more modest. We estimate that Strike Force offices decreased the share of patients receiving home health care by 7.6 percent (standard error = 2.5) on average in the five years following a Strike Force opening. This provides a preliminary indication that there may be room for improving the targeting of the Strike Force.

Moratoria estimates

Figure 5 shows the estimated impacts of the moratoria for the same three outcomes, which are the estimates of $\theta_{\tilde{r}}$ in equation (3).

Our estimates of $\theta_{\tilde{r}}$ for $\theta_{\tilde{r}} > 0$ indicate that the moratoria reduced home health care utilization on both the intensive and extensive margins. Table 2 indicates that on average in their first five years, the moratoria decreased the number of home health care visits per capita by about 16.9 percent (standard error = 5.9), decreased home health care spending per capita by 19.4 percent (standard error = 4.9), and the share of enrollees using home health by 12.7 percent (standard error = 2.3). These effects are comparable in magnitude to the Strike Force effects.

4.1.2 Outlier payment cap

The outlier payment cap was imposed on the entire country in 2010. However, we can measure the policy's impact by comparing counties where outlier payments were more or less common prior to the policy change, and hence more or less affected. Specifically, for each HHA j in the data, we define b_j to be the percentage of HHA j's total payments that came from outlier payments in 2009, the year before the cap was implemented. Then for each county c, we define b_c be a weighted average of b_j for all HHAs serving patients in county c, where the weights are the share of home health episodes received by patients in county c in 2009 that were provided by HHA j. Figure 2c showed the resulting geographic variation in b_c.

We parameterize the outlier cap as a binary treatment and define counties as "treated" if their outlier cap bite (b_c) is above the 75th percentile across counties. The choice of a binary treatment is for comparability with our strike force and moratoria analyses, and for the ease of extending to heterogeneous treatment effects in Section 5. In Appendix Section D, we report similar results using an analogous specification with continuous treatment by the size of the bite.

In the same spirit as our analyses of strike force and moratoria in equation (3), we estimate an event study of the impact of the outlier cap on the outcome of interest residualized to account for county and year fixed effects, as well as pre-implementation outcome trends in places that are 'treated' by the outlier cap. Thus our main estimating equation is

$$\tilde{y}_{ct} = \alpha + \sum_{t \neq -2009} \beta_t O_{ct} + \epsilon_{ct} \tag{4}$$

where variables are defined analogously as in the preceding equation (3), except indices are in calendar time because all areas are treated at once (2010). $O_{c(i,t)t}$ is an indicator for years relative to the beginning of the outlier cap in 2010 in "treated" counties. Appendix A provides the derivation of these equations in more detail. As with the other two policies, Appendix Section D shows that the results are very similar if we instead estimate a more straightforward event study version of the outlier cap and examine results relative to an estimated pre-trend; these exercises also show that the logarithmic pretrend also fits the outlier cap analysis well and that, as with the strike force, there is a substantial positive pre-trend in areas that are subsequently impacted by the outlier cap, so that failing to account for that would cause us to substantially under-estimate policy impacts.

Outlier payment cap estimates

Figure 6 plot the estimates of β_t in equation (4) for the three main home health care outcomes. Table 2 once again displays the average event study coefficients in the one, three, and five years after the policy is implemented.

As with the Strike Force and moratoria, the outlier cap had a large, negative impact on home health care utilization. For example, 2 indicates that five years after implementation, areas with higher outlier payment share saw a statistically significantly 17.2 percent (standard error = 2.3 percent) decline in home health care visits per 100 enrollees. Unlike the Strike Force and the home health agency moratoria, it had no discernible effect on the share of patients using home health care. However, unlike the Strike Force and moratoria which each reduced the share of Medicare enrollees who use home health care each year by about 10 percent, we estimate that the outlier cap had, not surprisingly, no impact on this extensive margin. On average in the five years after implementation, we estimate that it reduced the share of Medicare enrollees using home health care by a statistically insignificant 0.3 (standard error = 0.8) percent. This is not surprising given the nature of the policy which was aimed at reducing spending among high-spending patients rather than use among lower spending or marginal patients.

4.2 Home Care - Nursing Home Substitution

Since, as discussed in Section 2, the goal of Medicare-financed home health care is to use home health care as a substitute for patients who would otherwise be in more expensive (and presumably lower utility) nursing homes, a natural question is whether the substantial - 20 to 30 percent - policy-induced reductions in home health care result in substitution toward nursing home care. Overall we find no evidence that the reductions in home health care use produce substitution toward SNF use.

To begin, we use equations (3) and (4) to estimate the reduced form relationship between the policies and two measures of county-year SNF use: log covered SNF days per 100 enrollees and the log share of enrollees having any SNF-covered days. We find little evidence of any policy-induced increase in SNF use.[14]

To more directly investigate the substitution channel, we use the policy-induced declines in home health care use as instruments in a second stage examination of the relationship between SNF use and home health care use. Because the proportional functional form we have been using in estimating policy impacts seems appropriate for the first stage relationship between policies

[14]These results are displayed in Appendix Figures OA.3 and OA.4 and Appendix Table OA.4. The point estimates for the outlier cap show no substantive or statistically significant impact on either measures of SNF use, while the point estimates for the moratoria suggest that they are associated with a statistically significant *decline* in both measures of SNF use. Finally, while we find that the strike force was associated with a statistically significant 4% increase in the number of SNF covered days by years 4 and 5, it had no detectable impact on the share of enrollees using SNF.

and home health care use, we estimate a non-linear IV model where the first stage is a Poisson regression:

$$h_{ct} = \exp\left(\alpha_c + \tau_t + \sum_{r \notin \Omega} \beta_r SF_{cr} + \gamma \log(R_{cr}) + \sum_{\tilde{r} \notin \Omega} \theta_{\tilde{r}} M_{c\tilde{r}} + \phi \log(\tilde{R}_{c\tilde{r}}) + \sum_{t \notin \Omega} \delta_t b_{ct} + \psi \log(\bar{R}_t) b_c\right) \tag{5}$$

where h_{ct} denotes a measure of per-capita home health utilization and b_{ct} denotes the (continuous) outlier cap "bite" interacted with an indicator for it being year t.[15] $\bar{R}_t$ denotes years relative to the beginning of the outlier cap as a continuous variable. All other notation is consistent with the previous subsection.

We then use the predicted values from estimating equation (5) (denoted by $\hat{h}_{ct}$) in a second stage regression of the relationship between SNF use and home health care use:

$$s_{ct} = \alpha_c + \tau_t + \rho \hat{h}_{ct} + \gamma \log(R_{cr}) + \phi \log(\tilde{R}_{c\tilde{r}}) + \psi \log(\bar{R}_t) b_c + \epsilon_{ct} \tag{6}$$

where s_{ct} is a measure of per-capita SNF utilization, and we bootstrap the standard errors with 100 repetitions of the block bootstrap (i.e. resampling all county-years in 89 randomly selected districts with replacement).We also report results from a linear first stage relationship, which has the advantage of being identified solely off the instruments - rather than also the non-linear funtional form - but at the potential cost of a weaker first stage relationship; results are broadly similar

Substitution Estimates

Table 3 displays the results of our substitution analysis separately for the relationship between home health visits and SNF days (panel A) and home health spending and SNF spending (panel B). The first column of Table 3 displays estimates of ρ from equation (6) for the full sample. Using either a days or a payments measure of utilization, the OLS shows a positive and statistically significant association at the county-year level between home health care use and SNF use; this might of course reflect the fact that a less healthy population is likely to use more of both types of care. Consistent with this type of upward bias to the OLS, the IV relationship (either with the non-linear or linear first stage) is smaller, however it is never statistically significant and rather than negative point estimates which would be suggestive of substitution, the point estimates in column 1 tend to be positive. They are also quite small. For example, the IV Poisson model in Panel A column 1 indicates that one additional home health care visit increases SNF days by a

[15] We use the continuous outlier cap bite rather than the binary treatment indicators O_{ct} for additional power. As noted above, we obtain similar estimates for the impact of the outlier payment cap on home health care with the continuous and binary treatment measures.

statistically insignificant 0.004 days (standard error = 0.015); the 95 percent confidence interval allows us to rule out a decline in SNF days of more than 0.025 days per home health visit.

In columns (2) and (3) we try to detect substitution between home health care use and SNF use by narrowing in on subsets of the population for whom home health care use might be more likely to subsititute for SNF use. Column (2) limits our analysis to the top five percent of the population with the highest predicted use of SNFs; recall that only about 5 percent of Medicare enrollees use a SNF in a given year, compared to almost 10 percent of patients using home health care. Column (3) limits our first stage analysis to the approximately one-third of home health care use that is SNF-qualifying (i.e. occurs within 30 days of an inpatient stay lasting at least 3 days). The IV analyses now indicate a negative relationship between SNF use and home health care use, but are never statistically significant.

Finally, because the reduced form evidence in Figure OA.3 which suggests that the Strike Force and outlier payment caps show the most potential for substitution, we also show results in which we use only these two policies as instruments for home health care use, and include the moratorium variables ($\sum_{\tilde{r} \notin \Omega} \theta_{\tilde{r}} M_{c\tilde{r}}$) in the second stage relationship between SNF use and home health care use in equation (6). Specifically, in columns (4)-(6) of Table 3, we report estimates of ρ in the alternative second stage equation:

$$s_{ct} = \alpha_c + \tau_t + \rho \hat{h}_{ct} + \gamma \log(R_{cr}) + \sum_{\tilde{r} \notin \Omega} \theta_{\tilde{r}} M_{c\tilde{r}} + \phi \log(\tilde{R}_{c\tilde{r}}) + \psi \log(\bar{R}_t) b_c + \epsilon_{ct} \tag{7}$$

where controlling for the moratorium fixed effects in the second stage isolates variation induced from the Strike Force and outlier cap. Once again we see no evidence of substitution from home health care use to SNF use.

To better understand this lack of substitution, we also explored the impact of each policy on home health care use by two measures of patients' risk of SNF use. First, we look at impacts separately for patients that come from the community (and therefore are at low risk of Medicare-covered SNF use) and patients who come from the hospital (who are at higher risk); recall from Table 1 that about 80 percent of home health care visits are from patients who come from the community, and that this share is even higher in areas that were impacts by the policies we study. Second, we look at impacts separately for home health care use from patients who are 'SNF qualifying'. Appendix Figures OA.5 and OA.6 show the results for home health care by patient origin (from the community or not) and for home health care that is SNF qualifying or not, respectively, and Appendix Tables OA.5 and OA.6 summarize them. All three policies show substantial reductions in care by patients who are at low risk for using a SNF by either measure, which is consistent with our finding little evidence in support of the concern that policy-induced reductions in home health care use might produce a substitution toward SNF use. [16]

[16] By either measure, the moratorium appears to be the best targeted, with all of the reductions in home health care use concentrated among patients who come from the community or are not SNF qualifying. Likewise, the outlier

5 Counterfactual Policy Placement and Optimal Geographic Targeting

The results thus far have suggested that each of the three policies substantially reduced home health care use, with no evidence of offsetting substitution to nursing home care either overall or for patients at higher risk of using nursing homes. Given this evidence on policy efficacy, we now consider two types of counterfactuals. First, we consider how well-targeted the two geographically targeted policies - the strike forces and the moratoria - were individually by considering what would have happened if they were counterfactually expanded to other areas. Second, we ask whether - holding the budget constraint constant for each policy - we can improve targeting of these policies either indivdiually or in combination.

To do so, we first we estimate heterogeneous treatment effects across patients for each policy, and then apply these to counterfactual analyses of the application of the policies in different parts of the country in the presence or absence of other policies. An important caveat to the current results is that we are estimating heterogeneous treatment effects for each of the three *policies*; ultimately, however, we will estimate them for each of the seven possible policy *combinations*.

The counterfactual analysis involves two key assumptions. First, we assume that the relationship between observable patient and provider characteristics and treatment effects that we estimated where the policies were applied would also apply out of sample. Second, when considering the counterfactual impact of policy *combinations*, we assume that the combined impact of multiple policies on an individual is the maximum of the policy-specific treatment effects on an individual based on their observables; this is an extreme assumption that we will relax in future versions.

5.1 Heterogeneous treatment effects

We apply the causal forest framework of Athey et al. (2019) to estimate heterogeneous treatment effects of each of the policies on each of two outcomes: the number of home health visits per 100 enrollees and the home health care payments per 100 enrollees.

A primary dimension of treatment effect heterogeneity that we are interested in relates to attributes of home health agencies, since each of these policies was targeted at home health agencies . Of course, for the 90 percent of Medicare enrollees who do not use home health care, we do not observe which home health agency they would use if they (counterfactually) did use home health care. For each enrollee, therefore, we take a weighted average across home health agencies that were used by any patient in their zip code in 2006, with the weights corresponding to each home health agency's market share of home health care episodes for patients in that zip code.

cap policy appears to be the worst targeted by either measure; it has roughy proportional impacts on the use of home health care by patient origin, and roughly triple the impact on non-SNF qualifying care compared to SNF qualifying care, but by both measures it is less well-targeted than the Strike Force, which also reduces both types of care but more disproportionately reduces care by patients from the community or that is non-SNF qualifying.

We consider six observable attributes of home health agencies: the year that the agency was founded, the share of its patients that come from the community, the ratio between the number of patients served in 2009 and in 2004, and whether it is for-profit, non-profit, or government-owned. We expect bigger treatment effects for newer and growing home health agencies, home health agencies that service a greater share of patients from the community (i.e. less likely at risk of using a nursing home) and for-profit home health agencies. We also consider two patient attributes that are proxies for how sick they are: the patient's prior year Medicare spending and the number of comorbidities they have that year. We chose these characteristics based on qualitative information from our reading of the institutional setting and conversations with those involved in policy, as well as the empirical evidence on characteristics of areas targeted by policies.[17]

The causal forest algorithm uncovers heterogeneity in causal effects by optimally splitting the data along these chosen covariates in order to maximize the difference in treatment effects across splits, while guarding against over-fitting. Despite its name, a causal forest is not a tool for estimating causal effects per se; causality is still established by research design, whether it is through a randomized controlled trial, or a quasi-experimental design as in our setting. Appendix Section E provides more detail on how we implement the causal forest in our setting.

For each outcome, we grow 250 "trees" – a series of data splits along the set of attributes. For each tree, we follow the "honest causal forest" algorithm proposed by Athey et al. (2019) and use each observation either for prediction or for estimation, but never both. Specifically, for each tree, we split the sample into two equal halves. We grow the tree on the first half of the data, choosing the value of an attribute to split the data in a way that maximizes treatment effect heterogeneity across the resulting "leaves" – the resulting subsets, or end points of the tree. We continue to split each leaf in this manner until they cannot be split without creating a leaf with fewer than 10,000 observations.[18] We then implement the same set of splits in the second half of the data and calculate the treatment effects on each leaf. The treatment effect for each patient is the average effect taken across the 250 trees. We obtain standard errors following the "noisy bootstrap" procedure described in Sexton and Laake (2009).

Estimates

We detect heterogeneity in policy impacts with the standard deviation of the estimates between two and ten times the mean (Appendix Table OA.7). The bivariate correlations between estimated treatment effects and characteristics of patients and providers are consistent with intuition: the

[17] We ignore patient characteristics that don't vary much across area - such as gender and age - as well as race and ethnicity which does vary substantially across areas of the U.S. but may also be spuriously related to treatment effects. In particular, as noted earlier, Miami was an early and large source of home health care fraud, and it also has a large share of Hispanic patients.

[18] This is our chosen "minimum node size" for the algorithm. Due to limited computing power, we fixed the minimum node size to 10,000 based on results from our simulations. In an ideal setting, it would be chosen by cross validation.

treatment effect tends to be larger (more negative) for home health agencies that were created more recently, have more patients from the community, have had larger recent growth in patient volume; they are also larger for patients with higher spending and more comorbidities (Appendix Table OA.9).[19]

To compare the relative effectiveness of the three policies, Figures 7 and 8 report the cumulative distribution functions of the pairwise differences between treatment effects across enrollees for a given two policies for, respectively, home health visits and home health payments. The results indicate that the strike force dominates in effectiveness (although given that it is the most administratively expensive, that does not mean in dominates in cost-effectiveness). Specifically, compared with the moratorium it has a larger (i.e. more negative) treatment for 100% of enrollees when looking at impacts on home health care visits, and 80% of enrollees when looking at impacts on home health care payments; compared with the outlier cap, it has a larger impact for 80 to 90% of enrollees, depending on the outcome. The outlier cap is also somewhat more effective than the moratoria: it has a larger treatment effect for 50% to 80% of enrollees, depending on the outcome of interest.6

5.2 Counterfactual Policy Placement

We use the estimated treatment effects for counterfactual analysis of the policies to other geographic areas. Since the outlier cap is already applied nationally - and does not have a natural geographic targeting - we focus on the placement of the other two policies.

Table 4 considers the per-capita impact of the strike force and the moratoria under different specifications and samples. As a benchmark of comparison, the first row reports the homogeneous effect that we estimated in Section 4 using equation (3). The second row reports the average treatment effect estimated from the causal forest in areas that were affected by each policy. Reassuringly, the average homogeneous effect and the heterogeneous effects are quite comparable within each policy, despite the very different specifications used. The third row reports the treatment effects if the policy were counterfactually implemented in the unaffected areas. Comparing the second and third row, we see that for both policies and for both outcomes, implementing the policy in the unaffected area generates a smaller treatment effect than in the affected area; in other words, the areas chosen for policy implementation had higher average treatment effects than the areas that were not chosen. This suggests some degree of effective targeting (relative to choosing areas at random) but leaves open the question of whether alternative geographic targeting could have generated substantial improvements.

To investigate this, Table 5 considers savings (i.e. reductions in home health care payments per capita) under alternative applications of the moratorium and strike force policies. We consider

[19] Appendix Table OA.8 reports the importance weights on different covariates.For each of the three policies, lagged patient spending is by far the most important predictor of treatment effect heterogeneity, with average entry year of home health agencies in the patient's zip code and patient comorbidities also playing an important role.

two different scenarios, one in which the outlier cap is implemented nationwide (columns (1) and (3)) and one in which it is not implemented (column (2) and (4)).[20] For each scenario, we consider the incremental per capita savings we can generate by adding the strike force, the moratoria, or both. In the left panel, we consider combinations of the strike force and the moratorium in the counties where they actually took place; in the right panel, we consider counterfactual placements of policies to counties that would maximize savings per capita, holding the number of affected individuals constant. Finally, column (5) reports the estimated cost of each policy. The outlier cap has no direct cost (in fact, it generates direct savings by reducing reimbursements). We also expect the moratoria to have little direct cost. In contrast, the strike force is resource-intensive and we estimate that it costs $9.61 per capita per year at its current scale. We describe the methods we used to calculate the cost of the strike force in Appendix Section C.

Consider first the actual placement of policies with and without the outlier cap (columns 1 and 2). In both cases, as the results thus far would lead us to expect, the impact of the strike force is substantially larger than that of the moratorium. The last row shows that the combined impact of the two policies is lower (by about 20 percent) than the sum of their two effects; this qualitative result is mechanically driven by our assumption that in the presence of multiple policies the combined impact is the maximum of the policy-specific treatment effects, but the magnitude is nonetheless interesting. Likewise, comparing columns (1) and (2), we see that policy impacts are substantially higher in the absence of the outlier cap. For example, without the outlier cap, the impact of the actual strike force placement is $44.17 per capita compared to $24.04; again the qualitative result is mechanical but the quantitative estimates are informative.

The analysis of optimal placement (columns (3) and (4)) indicate that, holding the scale of the strike force and moratoria constant, optimal geographic placement can achieve substantial savings relative to the actual placement in the presence of the outlier cap, but that these gains are much smaller in the absence of the outlier cap. For example, in the presence of the outlier cap, the impact of actual placement of the strike force (column (1)) generates savings of $24.04 per capita, while optimal placement (column (3)) would increase those savings by $20.5 per capita to $44.55 per capita, almost doubling the strike force impact. Moreover compared to the actual placement of both the strike force and moratoria (which generates $30.05 savings per capita), optimal placement of both policies more than doubles the amount of savings to $68.6 per capita. However, in the absence of the outlier cap, the gains from optimal placement (column 4) relative to actual placement (column 2) are substantially lower. In other words, the strike force and moratoria - which originally began prior to the 2010 introduction of the outlier cap - appear to be reasonably well targeted, but once the outlier cap is introduced they are not.

To unpack what is going on, Table 6 summarizes the average characteristics of districts actually targeted by the strike forces (column 1), and the characteristics of the counties that optimally

[20] Neither scenario is fully factual or counterfactual, as the strike force and moratoria came in in some places before the nationwide imposition of the outlier cap, and in some places after.

targeted by the strike force in the absence (column 2) and presence (column 3) of the outlier cap. It shows that absent the outlier cap, the characteristics of the actual and optimal target districts are fairly similar, but that once the outlier cap is in place, the optimal targeting concentrated on areas with substantially lower home health care utilization and spending levels and growth. In other words, many of the places the Strike Forces were investigating were in fact areas with high outlier payments; the introduction of the outlier payment cap technology means that the strike force could be more effectively deployed to lower spending areas, leaving the outlier cap to do much of the strike force's work in the higher spending areas.

Figure 9 shows the difference between the optimal and actual placement of the strike force in the absence (panel a) and presence (panel b) of the outlier cap. Relative to the actual placement, it denotes areas where optimal placement involves the loss of a strike force in red, areas where optimal placement involves the gain of a strike force in green, and areas where optimal placement continues to have a strike force in blue. The results indicate that absent the outlier cap far more districts optimally retain a strike force than in its presence. Strikingly, Miami-Dade county, which is the first place to have received a strike force and is the poster child for home health care fraud (O'Malley et al. 2023) does not optimally receive a strike force once the outlier cap is in place (panel b); however when that strike force was created in 2009 the outlier cap was not in place and it is optimally allocated a strike force (panel b). This is because Miami-Dade had one of the highest share of outlier payments and hence once of the largest impacts of the outlier cap policy. Such findings underscore the importance of considering how optimal use of scarce resources may vary in the shadow of other complementary policies and the value of policy coordination within and across agencies pursuing similar (broad) objectives.

6 Conclusion

[Conclusion to be added]

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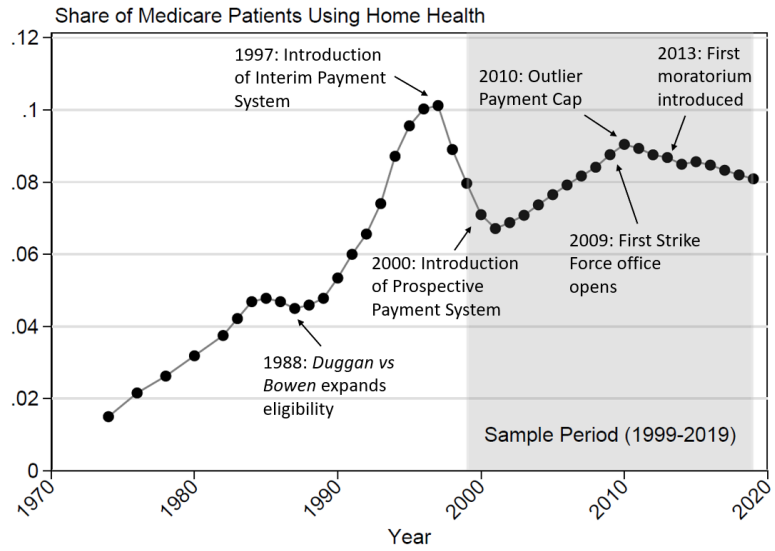
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Figures

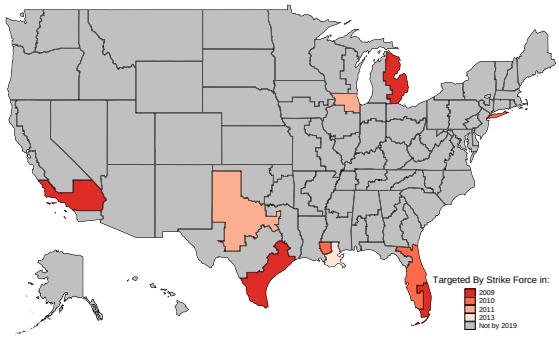
Figure 1: Share of Medicare Patients Receiving Home Health Care, 1974-2019



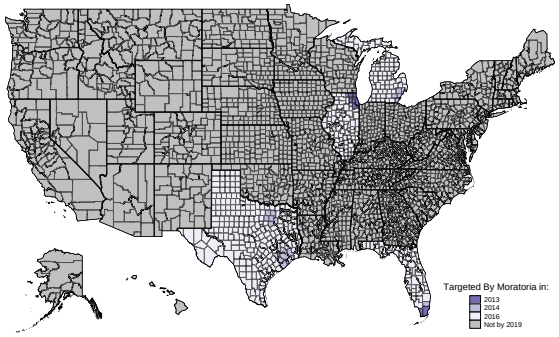
Notes: This figure displays trends in the share of Medicare enrollees receiving Medicare-financed home health care each year between 1974 and 2019. Data for 1974 through 1998 is from the CMS 2001 Statistical Supplement, while data for 1999 and beyond is from the 20% sample of Medicare claims data.

Figure 2: Geographic Variation in Policy Instruments

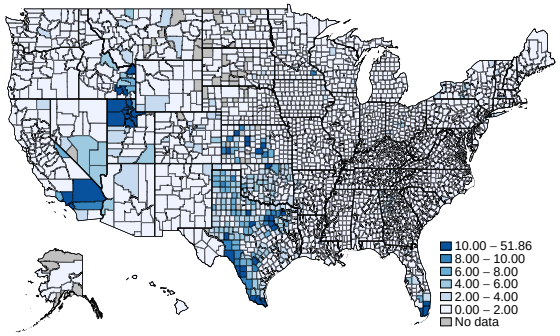
(a) Districts With Strike Force Offices



(b) Counties With Moratoria on HHA Entry



(c) Distribution of Outlier Payment Shares

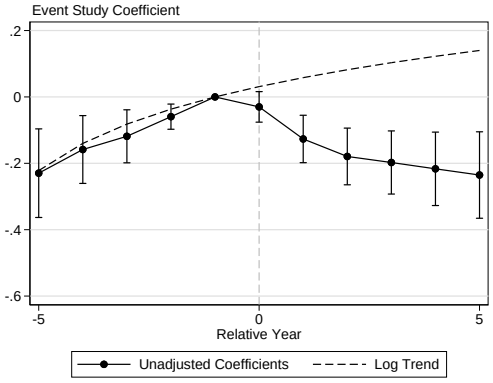


			Above 10%:	
	Mean	75th Pctile	Share	Mean
All	2.19	1.57	6.6%	18.34
SF or M	5.40	7.07	21.3%	18.60
Neither SF or M	0.93	0.86	0.8%	15.50

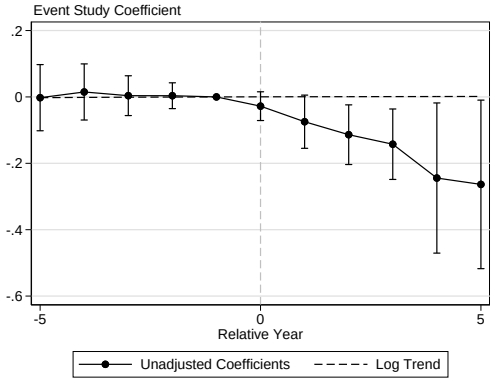
Notes: Panel (a) maps judicial districts in the United States, highlighting those with Strike Force offices operating by 2015. The legend gives the opening date of the office in each district; more recent openings have darker shades. Panel (b) maps counties in the United States, highlighting those that had moratoria on home health agency entry. The legend gives start year of the moratorium in each county. Panel (c) maps the percentage of payments that were outlier payments in 2009 across counties and provides summary statistics for these shares at the HHA and county levels. To aggregate the outlier payment share from the HHA level to the county level, we averaged the outlier payment shares for each HHA serving patients in each county, weighting by the share of home health episodes in the county that each HHA was responsible for. The distribution across counties is weighted by the number of Traditional Medicare enrollees living there in 2006. The sample size is 3,256 counties.

Figure 3: Strike Force and Moratorium Event Studies, With Trend

(a) Strike Force



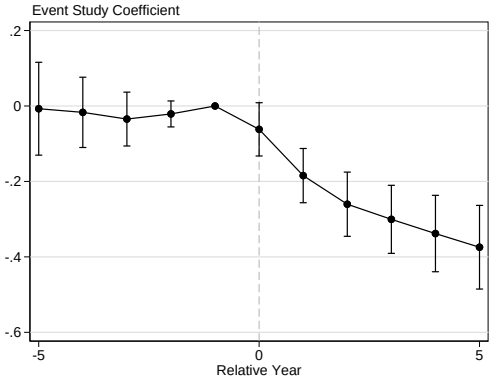
(b) Moratorium



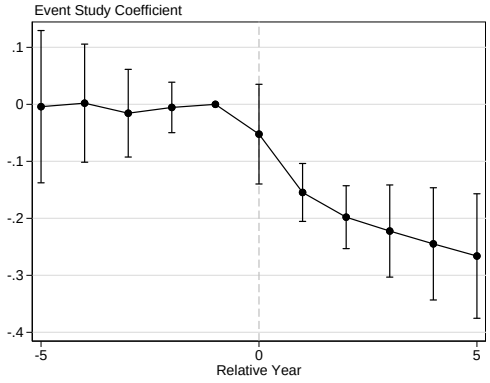
Notes: this figure shows event study estimates of β_r (Panel A) and $\theta_{\tilde{r}}$ (Panel B) in equation (1) with log HH visits per 100 enrollees as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. The dashed line represents the logarithmic trend in the outcome variable fitted according to equation (2). The sample size is 63,552 county-years.

Figure 4: Strike Force Effects on Log Home Health Care Utilization

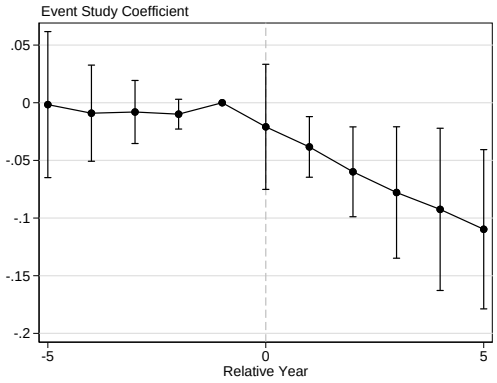
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



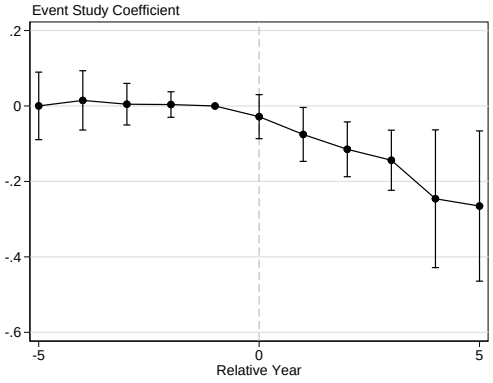
(c) Enrollees Using Home Health Per 100



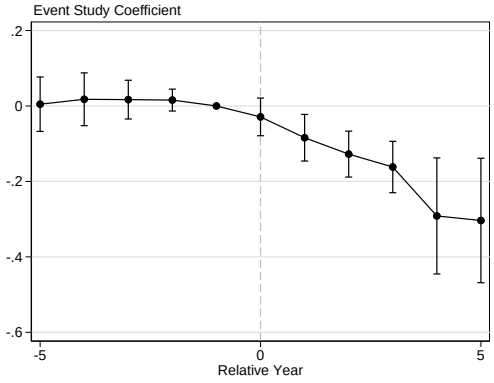
Notes: this figure shows event study estimates of β_r in equation (3) with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. The sample size is 63,552 county-years.

Figure 5: Moratorium Effects on Log Home Health Care Utilization

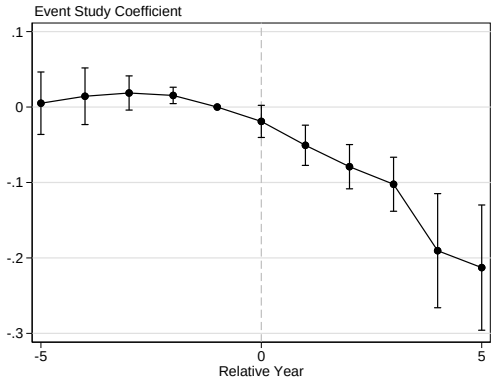
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



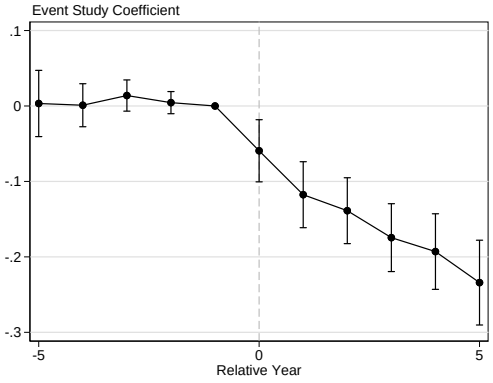
(c) Enrollees Using Home Health Per 100



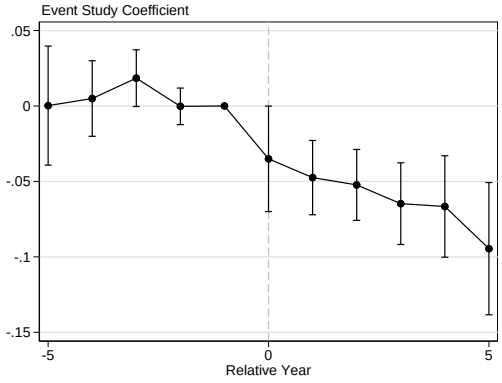
Notes: this figure shows event study estimates of $\theta_{\tilde{r}}$ in equation (3) with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. The sample size is 63,552 county-years.

Figure 6: Outlier Payment Cap Effects on Log Home Health Care Utilization

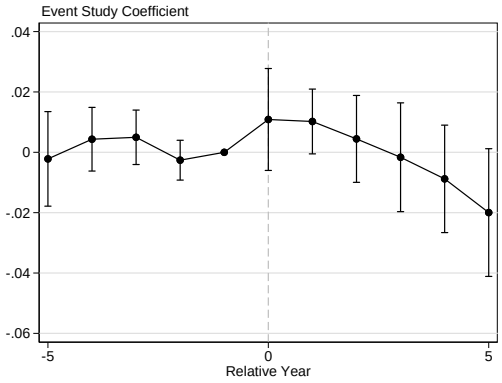
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



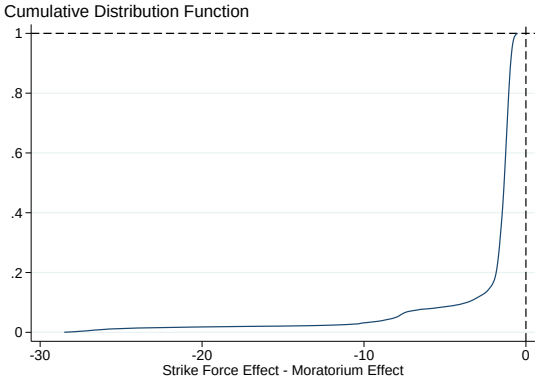
(c) Enrollees Using Home Health Per 100



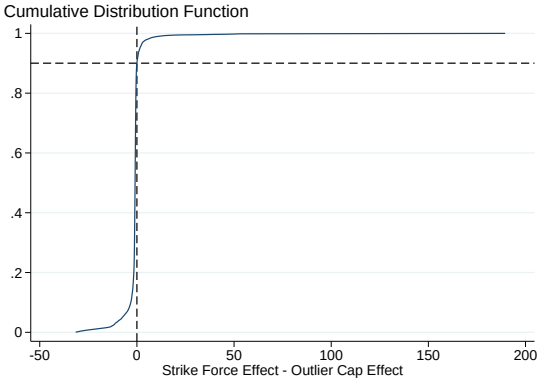
Notes: this figure shows event study estimates of β_r in equation 4 with several measures of home health utilization per 100 enrollees as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. The sample size is 63,552 county-years.

Figure 7: Cumulative Distribution Function of Treatment Effect Differences: HH Visits

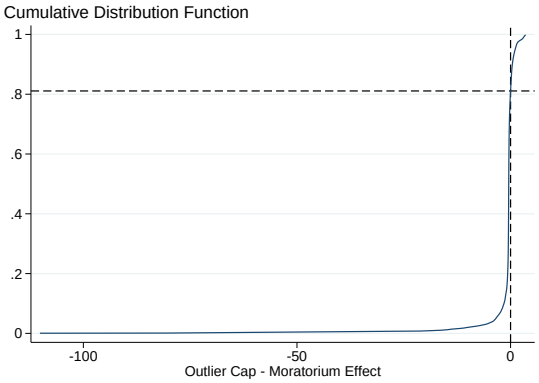
(a) Strike Force - Moratorium



(b) Strike Force - Outlier Cap



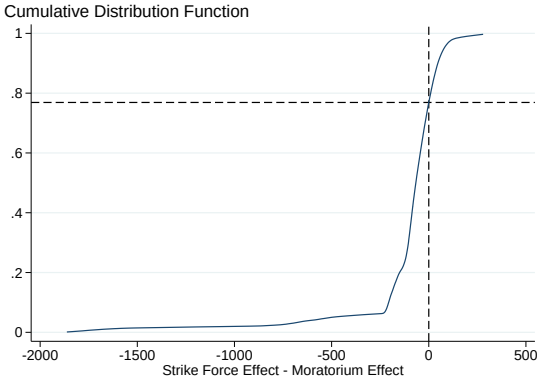
(c) Outlier Cap - Moratorium



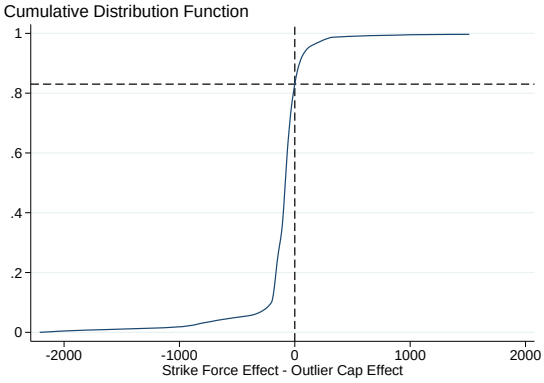
Notes: This figure displays the cumulative distribution function of the pairwise differences between treatment effects on HH visits for each policy. The outlier cap treatment effects are scaled by the ratio of the outlier cap in each patient's county of residence to the average bite in the counties above the 75th percentile (i.e. those defined to be treated in the causal forest). The sample size is 6,495,865 patient-years.

Figure 8: Cumulative Distribution Function of Treatment Effect Differences: HH Payments

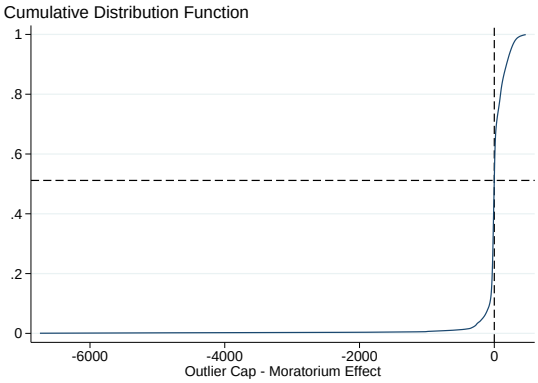
(a) Strike Force - Moratorium



(b) Strike Force - Outlier Cap



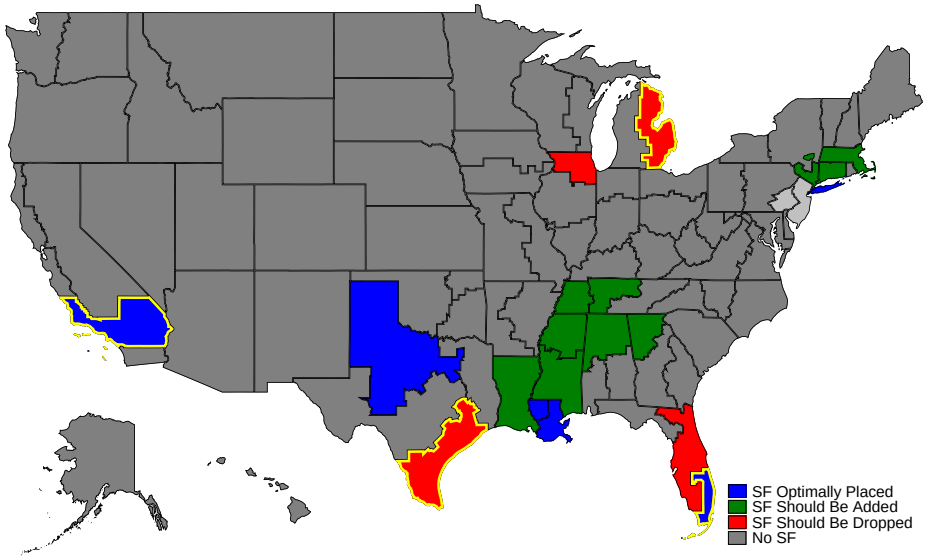
(c) Outlier Cap - Moratorium



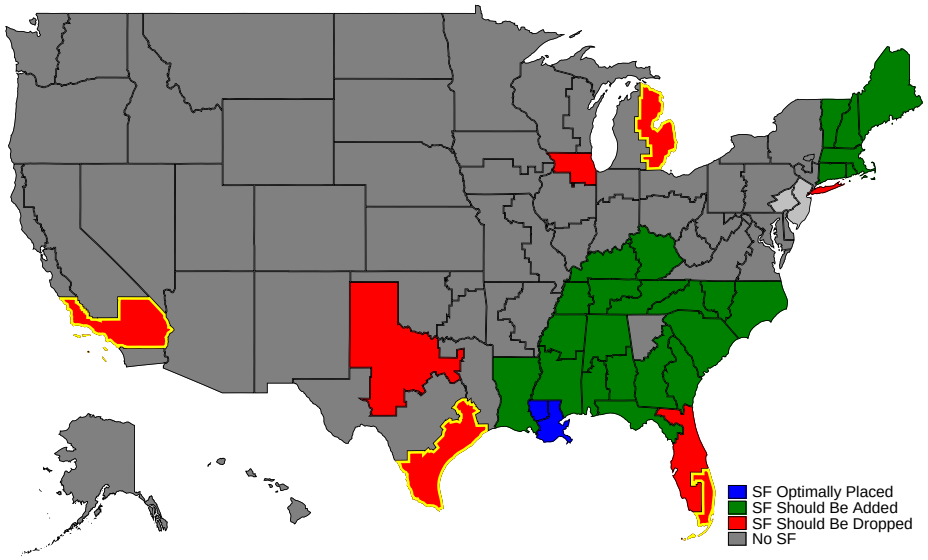
Notes: This figure displays the cumulative distribution function of the pairwise differences between treatment effects on HH payments for each policy. The outlier cap treatment effects are scaled by the ratio of the outlier cap in each patient's county of residence to the average bite in the counties above the 75th percentile (i.e. those defined to be treated in the causal forest). The sample size is 6,495,865 patient-years.

Figure 9: Optimal Placement of Strike Force With and Without Outlier Cap

(a) Without Outlier Cap



(b) With Outlier Cap



Notes: This figure maps the locations of the districts optimally targeted by Strike Force districts with and without the outlier cap (Panels A and B, respectively) relative to their actual placement. The districts outlined in yellow were targeted by Strike Force districts prior to 2010 (the implementation of the outlier cap).

Tables

Table 1: Summary Statistics

	All	Strike Force		Moratorium		Outlier Payment Share	
		Yes	No	Yes	No	≥75th Pctile	< 75th Pctile
	Panel A: Home Health Utilization						
County Average (2007)							
HH Visits Per Capita	3.08	5.76	2.27	5.69	2.31	4.82	2.08
From the Hospital	0.76	1.13	0.65	1.04	0.68	0.96	0.64
From the Community	2.32	4.64	1.62	4.65	1.63	3.86	1.44
HH Payments Per Capita	$419	$738	$322	$725	$328	$586	$323
Share of Patients Using HH	0.093	0.131	0.082	0.130	0.083	0.110	0.084
Average Change (2003-2007)							
HH Visits Per Capita	0.85	2.64	0.31	2.92	0.24	1.88	0.26
From the Hospital	0.13	0.31	0.07	0.30	0.08	0.23	0.07
From the Community	0.73	2.34	0.24	2.62	0.17	1.65	0.20
HH Payments Per Capita	$147	$349	$86	$382	$77	$246	$90
Share of Patients Using HH	0.014	0.030	0.009	0.036	0.008	0.020	0.011
	Panel B: SNF Utilization						
County Average (2007)							
SNF Covered Days Per Capita	2.01	2.20	1.95	2.24	1.94	2.12	1.94
SNF Payments Per Capita	$652	$752	$622	$721	$632	$709	$619
Share of Patients Using SNFs	0.051	0.052	0.050	0.053	0.050	0.052	0.050
Average Change (2003-2007)							
SNF Covered Days Per Capita	0.33	0.40	0.30	0.47	0.28	0.35	0.31
SNF Payments Per Capita	$223	$264	$211	$275	$208	$242	$213
Share of Patients Using SNFs	0.004	0.006	0.004	0.007	0.004	0.005	0.004
Number of Counties	3,177	273	2,904	506	2,671	803	2,374

Notes: This table displays average measures of home health utilization (Panel A) and SNF utilization (Panel B) across various counties as defined by each column title. Averages are weighted by each county's Traditional Medicare enrollees in 2007.

Table 2: Effect of Policies on Utilization of Home Health Care

	Average of Relative Period Coefficients:		
	1	1-3	1-5
Panel A: Log Home Health Visits Per Capita			
Strike Force	-0.185	-0.249	-0.292
	(0.037)	(0.040)	(0.041)
Moratorium	-0.075	-0.111	-0.169
	(0.036)	(0.037)	(0.059)
Outlier Cap	-0.118	-0.144	-0.172
	(0.022)	(0.022)	(0.023)
Panel B: Log Home Health Payments Per Capita			
Strike Force	-0.155	-0.192	-0.217
	(0.026)	(0.027)	(0.034)
Moratorium	-0.084	-0.125	-0.194
	(0.032)	(0.031)	(0.049)
Outlier Cap	-0.048	-0.055	-0.065
	(0.013)	(0.012)	(0.014)
Panel C: Log Share of Enrollees Using Home Health			
Strike Force	-0.038	-0.059	-0.076
	(0.013)	(0.019)	(0.025)
Moratorium	-0.051	-0.077	-0.127
	(0.014)	(0.015)	(0.023)
Outlier Cap	0.010	0.004	-0.003
	(0.006)	(0.007)	(0.008)

Notes: This table displays several averages of post-period coefficients assessing the impact of each policy on several measures of home health utilization: the β_r's from equation (3) for the Strike Force, the $\theta_{\tilde{r}}$'s from equation (3) for the moratorium, and the β_r's from equation (4) for the outlier cap. Standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level are displayed in parentheses below each estimate. The sample size is 3,177 counties.

Table 3: IV Estimates of HH to SNF Substitution

	Strike Force, Moratoria, and Outlier Cap			Strike Force and Outlier Cap		
	All Patients	High Predicted SNF Use	SNF Qualifiers	All Patients	High Predicted SNF Use	SNF Qualifiers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of HH Visits Per Capita on SNF Covered Days Per Capita						
OLS	0.0120	-0.0085	-0.1053	0.0093	-0.0082	-0.1128
	(0.0060)	(0.0075)	(0.0358)	(0.0055)	(0.0079)	(0.0337)
IV (Poisson)	0.0039	-0.0230	-0.0773	-0.0007	-0.0231	-0.1033
	(0.0146)	(0.0468)	(0.1334)	(0.0156)	(0.0549)	(0.1348)
IV (Linear)	0.0006	-0.0383	-0.0833	-0.0069	-0.0402	-0.1278
	(0.0095)	(0.0341)	(0.1526)	(0.0123)	(0.0383)	(0.1686)
F-S F Statistic	282.6	84.7	47.4	168.8	16.3	27.5
Panel B: Effect of HH Payments Per Capita on SNF Payments Per Capita						
OLS	0.0770	0.0735	-0.3406	0.0652	0.0823	-0.3538
	(0.0341)	(0.0545)	(0.1718)	(0.0354)	(0.0568)	(0.1729)
IV (Poisson)	0.0467	-0.1259	-0.6213	0.0113	-0.1107	-0.6859
	(0.0799)	(0.1985)	(0.4992)	(0.0919)	(0.2263)	(0.5135)
IV (Linear)	-0.0179	-0.4410	-1.5547	-0.1054	-0.5405	-2.2082
	(0.0820)	(0.3694)	(1.5826)	(0.1425)	(0.5127)	(2.0126)
F-S F Statistic	252.9	67.2	83.2	178.6	46.4	70.1
N	63,270	62,608	63,168	63,270	62,608	63,168

Notes: This table displays estimates of ρ in equation 6 (columns 1-3) and 7 (columns 4-6). The outcome and independent variables are given by the titles of each panel. Standard errors clustered at the district level are displayed in parentheses; they are computed via the block bootstrap with 100 repetitions (sampling at the district level) for the Poisson IV specification and analytically for the other specifications. For the IV estimates, the independent variable is instrumented using the first stage equation (5), estimated using either Poisson or linear regression as denoted by the row title. All regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006.

Table 4: Per Capita Effects of Policies on Affected vs Unaffected Areas

	Strike Force Effect On:		Moratorium Effect On:	
Counterfactual Area Applied	HH Visits	HH Payments	HH Visits	HH Payments
Homogeneous Effect	-2.82	$-246.50	-0.36	$-100.97
	(1.09)	($57.05)	(0.30)	($39.53)
Affected Areas	-2.96	$-196.00	-0.17	$-88.38
Unaffected Areas	-2.28	$-164.79	-0.05	$-55.68
Entire US	-2.43	$-171.82	-0.07	$-62.96

Notes: Rows report the average five-year impact of the policies based on alternative specifications and samples. The first row reports the estimate based on the county-year homogeneous effects baseline model estimated in equation (3), with two differences from what we have reported previously to make the results more directly comparable with the subsequent rows. First, we estimate the model with the outcomes in levels rather than logs and second we report the results from POST dummies rather than averaging five year indicators. The second row reports the the average treatment effect from the heterogeneous treatment effects estimation, assuming the policies were applied where they were actually applied. The final two rows display the average counterfactual treatment effects from the heterogenous treatment effects estimation had the policies in each column been counterfactually applied on the patient-years living in the areas given in each row. The sample size is 6,497,397 patient-years in the entire US, 1,464,279 patient-years living in Strike Force districts, and 1,446,527 patient-years living in moratorium counties.

Table 5: Per Capita Savings Under Actual Counterfactual Policies Implementations

	Actual Placement		Optimal Placement		
	Outlier Cap	No Outlier Cap	Outlier Cap	No Outlier Cap	Cost
Policy Regime	(1)	(2)	(3)	(4)	(5)
Baseline	$-68.48	$0.00	$-68.48	$0.00	$0.00
+ Strike Force	$-24.04	$-44.17	$-44.55	$-51.36	$9.61
+ Moratorium	$-12.98	$-19.68	$-17.52	$-24.46	$0.00
+ Strike Force, Moratorium	$-30.05	$-53.06	$-68.63	$-82.34	$9.61

Notes: This table displays the per capita savings of each policy regime given by the combination of the row and column titles. Columns (1) and (2) treat the location of the Strike Force and moratorium as fixed, using the true locations targeted by each policy. Columns (3) and (4) target the areas (districts for the Strike Force and counties for the moratorium) that maximize total savings using the algorithm described in Section 5.2. The sample size is 6,497,397 patient-years.

Table 6: Patient and HHA Characteristics Under Actual and Optimal Targeting of Strike Force

		Optimal	
	Actual	No Outlier Cap	Outlier Cap
Characteristic	(1)	(2)	(3)
2006 HH Visits Per Capita	4.70	4.74	2.96
2006 HH Payments Per Capita	$611	$565	$398
2009/2004 HH Visits Per Capita	1.75	1.45	1.26
2009/2004 HH Payments Per Capita	1.96	1.64	1.53
HHA Entry Year	1994	1986	1982
Share of Patients from Community	0.70	0.67	0.64
Share of For-Profit HHAs	0.79	0.66	0.55

Notes: This table displays levels and trends in average patient-level home health utilization, as well as average characteristics of HHAs in districts targeted by the Strike Force chosen in the actual setting (column 1) and the optimal setting with and without the outlier cap in place (columns 2 and 3). Averages in the first 4 rows are weighted by the number of patients in each county , while averages across HHAs in the last 3 rows are weighted by the episodes served in 2006.

Appendices

A Details on Outlier Cap Empirical Strategy

Similar to our analysis of the strike force and the moratorium, we start by considering the following equation:

$$y_{ct} = \alpha_c + \tau_t + \sum_{t \neq 2009} \beta_t O_{ct} + \epsilon_{ct} \tag{8}$$

where α_c represents county fixed effects, τ_t represents calendar year fixed effects, and O_{ct} represents indicators that take value 1 if it is year t and county c is "treated" (i.e. has a bite above the 75th percentile). We again weight each county-year by the 2006 Medicare enrollee population, and we cluster standard errors on the county.

We the follow the same three-step procedure to estimate a detrended version of the event study, again under the assumption that home health care use increases logarithmically over time. Specifically, we first fit the equation

$$y_{ct} = \alpha_c + \tau_t + \sum_{t \notin \Omega} \beta_t O_{ct} + \gamma \log(R_t) + \epsilon_{ct} \tag{9}$$

where R_t is a continuous variable taking the values of the year relative to the beginning of the outlier cap in the treated counties and zero otherwise; all other variables are defined the same way as in equation (8)[21]. Since Ω denotes the set of relative years between -5 and -1, β_t now represents the effect of the outlier cap relative to a logarithmic trend in the outcome variable fitted in the five years prior to the cap's implementation.

We then use the resulting estimates to predict $\hat{y}_{ct} = \hat{\alpha}_c + \hat{\tau}_t + \hat{\gamma} \log(R_t)$ and estimate the baseline specifications previously reported in equation (4):

$$\tilde{y}_{ct} = \alpha + \sum_{t \neq 2009} \beta_r O_{ct} + \epsilon_{ct} \tag{10}$$

where $\tilde{y}_{ct} \equiv y_{ct} - \hat{y}_{ct}$.

B Pre-claims review

A final policy we are able to study is a requirement for pre-claim authorization; specifically, a requirement that CMS review and approve patients for eligibility for home health prior to the agency receiving any payments. Key components of the program included requirements for home health agencies to submit documentation from a provider proving that the beneficiary receiving home health met the coverage standards when filing an initial request for payment. If an HHA failed to provide documentation, payments would be reduced by 25% (Medicare Payment Advisory Commission 2017, Medicare Payment Advisory Commission 2019).

This was first launched on August 1, 2016 in Illinois. The program was initially designed to expand to other states, including Florida, Massachusetts, and Michigan, but the program was

[21] As with equation (2), we bin indicators O_{ct} and values of R_t six years before the policy and six years after the policy; thus, we add 7 to ensure that its logarithm is always defined

suspended after eight months, on April 1, 2017 before it could expand.[22] A Government Accountability Office report finds that home health payments decreased by about $100 million during the months the program was in effect. The reduction came from both the reduced payments to agencies that failed to provide documentation, as well as from agencies that stopped providing care to patients who were either marginally or totally ineligible for Medicare home health benefits. (Medicare Payment Advisory Commission 2019).

To study the pre-claims review requirement that was in place in Illinois from August 1, 2016 through March 31, 2017, we analyze a county-month panel from August 2015 through August 2017 (i.e. a one-year window before and after the policy). We estimate:

$$y_{ct} = \alpha_c + \tau_t + \sum_{r \neq -1} \beta_r P_{cr} + \epsilon_{ct} \tag{11}$$

where α_c and τ_t denote county and calendar month fixed effects respectively. Here, P_{cr} that takes value 1 in the month r relative to the implentation of the pre-claims review in August 2016 in treated counties. The regressions are weighted by the number of Medicare enrollees in each county in 2016 and standard errors are clustered at the county level.

Appendix Figure OA.10 shows that the pre-claims review also had an immediate and substantial impact on home health care use. Moreover, although the policy was only in effect for 10 months, it indicates that the reduction in home health care use persists through the end of our analysis (approximately 5 months after the policy ended). The magnitudes are not directly comparable to the others, since we only look at the first 10 months (rather than 5 years) of the policy; still the results suggest a statistically significant decline in home health care visits per capita of about 10 percent, and about a 5 percent decline in the share of enrollees using home health care.

C Estimating the cost of the strike force

As a collaborative effort between multiple branches of the U.S. government, the Medicare Fraud Strike Force is quite expensive to maintain, but there is no readily available report of exactly how costly a Strike Force is.

Each year, the U.S. Department of Health and Human Services (HHS) and Department of Justice (DOJ) publish a report with 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 Force. Each report lists spending at a national level, as well as descriptions of each initiative under HCFAC.

Appendix Table OA.10 provides an example of how funding to HCFAC is reported each year. The spending is itemized under divisions of each department, but there is no finer data on how much was allocated to each specific initiative.

To determine what spending is "relevant" to the Medicare Fraud Strike Force, we use the following quote from the 2009 report:

[22] A similar program – the Review Choice Demonstration – was announced in 2018 for implementation in Illinois, Ohio, Texas, Florida, and North Carolina in June 2019, September 2019, March 2019, September 2021, and September 2021, respectively. However, the rollout for Texas, Florida, and North Carolina were significantly disrupted by the COVID-19 pandemic and the entire Review Choice Demonstration program was paused on March 30, 2020 (Medicare Payment Advisory Commission 2019).

"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."

Based on this quote, we define relevant spending to be the sum of spending under the following categories:

1. HHS Office of Inspector General
2. HHS Centers for Medicare and Medicaid Services
3. DOJ United States Attorneys
4. DOJ Criminal Division
5. DOJ Federal Bureau of Investigation

For each of these categories, we sum both mandatory and discretionary funding. 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.10. We interpret this to mean that the FBI received zero funding in these years.

Appendix Figure OA.11a displays the amount of "relevant" spending in each year between 1997 and 2020. With the exception of 2014-2015, relevant spending grows very slowly over time in years without new Strike Force openings. Under a "common trends" assumption in which the relevant spending level would have remained constant if a new Strike Force office did not open, we can impute how much Strike Force offices cost to operate by computing differences in relevant spending across years. For example, the difference between relevant spending in 2007 and relevant spending in 2006 would yield the approximate cost of operating the Strike Force office in Southern Florida, which opened in March 2007 and began focusing on home health in 2009. We plot these differences in Appendix Figure OA.11b and map them to cost estimates of Strike Force offices in Appendix Table OA.11.

Since we do not consider the Strike Force offices in New Jersey and Eastern Pennsylvania as part of our analysis, summing the costs of the remaining Strike Force offices results in an estimate of $332.2 million to operate each year. Note, however, that the Strike Force combats more than home health fraud; they also investigate and prosecute cases of fraud in Durable Medical Equipment, IV infusion therapy, and other areas. To obtain an estimate of how much the government spends in Strike Force operations to combat 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" only. In total there were 364 press releases on home health and HEAT and 1,025 total press releases about HEAT. In other words, we estimate that roughly 35.5% of Strike Force spending, or $118.0 million per year is dedicated to home health.

D Alternative event-study specification

In this section, we report estimates from equations (1) and equation (2) on the impact of the strike force and the moratoria. We also estimate an alternative event study specification with continuous treatment for the outlier payment cap.

Strike force estimates

Figure OA.7 shows the estimated impacts of the Strike Force for all three outcomes. The solid line in each figure corresponds to the estimates of β_r from estimating equation (1). These estimates show a pronounced decline in all three measures of home health care use after the Strike Force office opens. This decline begins immediately; it is visible even in the year of Strike Force entry (relative year 0), grows substantially in year 1, and continues to grow over the five year period examined. The estimates also indicate that prior to the opening of a Strike Force office, areas where a Strike Force office subsequently opened were experiencing rising home health care use relative to other areas where a Strike Force office either didn't open or opened later. As discussed, this is not surprising from a policy perspective – rapidly rising home health care use after 2000 was part of what prompted concerns about home health care fraud (Medicare Payment Advisory Commission 2015, 2021a), and we would expect that Strike Force offices would open first in the areas where such fraud was considered most likely to be occurring at large scale.

Nonetheless, this suggests that the estimated impact of the Strike Force based on the estimates of β_r from estimating equation (1) in Figure OA.7 will under-estimate the impact of the Strike Force because they fail to account for the fact that absent that intervention, home health care use would have likely continued to grow in those areas. The dashed line in Figure OA.7 visualizes the fitted trend by plotting $y = \gamma \log(R + 7)$ from equation (2), normalized to pass through $(-1, 0)$. As a result, the effect of the Strike Force as measured by the estimates of β_r in equation (2) is approximately the distance between the dashed and solid lines. Table OA.3 summarizes these results. Specifically, for each of the three home health care outcomes, the top row of each panel summarizes the estimated average impact of the strike force in its year of entry, its first three years, and its first five years. These are based on linear combinations of the various estimates of β_r in equation (2).

The results indicate that overall, home health utilization declined substantially as a result of Strike Force activity. For example, in Figure OA.7 panel (a) we estimate that, even without accounting for the fact that home health care visits would likely have increased in the absence of the Strike Force, home health visits per capita decreased by about 15 percent on average in the five years after the Strike Force operations began. This effect increases to 27.6 percent (standard error = 6.2) once we account for the pre-existing rising trend in visits (Table OA.3). Medicare home health care payments per capita declined by a similar amount (21.2 percent, standard error = 5.8). Interestingly, the effects of the Strike Force on the extensive margin—the share of patients in each county using home health care—are much more modest. Even after adjusting for trends in the pre-period, we estimate that Strike Force offices decreased the share of patients receiving home health care by a statistically insignificant 7.0 percent (standard error = 4.2) on average in the five years following a Strike Force opening. This provides a preliminary indication that the impacts of the Strike Force may not have been well-targeted.

Moratoria estimates

Figure OA.8 shows the estimated impacts of the moratoria for the same three outcomes. The solid lines in each figure now correspond to the estimates of $\theta_{\tilde{r}}$ in equation (1), while the dashed line visualizes the fitted trend by plotting $y = \phi \log(\tilde{R} + 7)$ from equation (2), normalized to pass through $(-1, 0)$.

For each outcome, our estimates of $\theta_{\tilde{r}}$ for $\tilde{r} < 0$ are small and statistically indistinguishable

from zero and, correspondingly, the fitted pre-trend line is essentially flat.[23] Our estimates of $\theta_{\tilde{r}}$ for $\theta_{\tilde{r}} > 0$ indicate that the moratoria reduced home health care utilization on both the intensive and extensive margins. Table OA.3 indicates that on average in their first five years, the moratoria decreased the number of home health care visits per capita by about 17.3 percent (standard error = 6.9) and decreased home health care spending per capita by 20.4 percent (standard error = 5.1). These effects are similar in magnitude to the Strike Force effects but whereas the Strike Force had no discernible effect on the extensive margin—the share of patients using home health—the moratoria on entry of new home health agencies had a significant effect on the share of patients receiving home health care; on average over its first five years, a moratorium decreased the share of patients using home health by a statistically significant 13.7 percent (standard error = 2.7).

Outlier payment cap estimates

The solid lines in Figure OA.9 plot the fitted values of β_r in equation (8) for the three main home health care outcomes. As with the Strike Force event study, we see pronounced pretrends. The dashed line in Figure OA.9 visualizes the fitted trend by plotting $y = \gamma \log(R_t)$ in equation (9), normalized to pass through $(-1, 0)$. As a result, the effect of the outlier cap as measured by the specification in equation (9) is approximately the distance between the dashed and solid lines. Table OA.3 once again displays the average event study coefficients relative to trend in the one, three, and five years after the policy is implemented.

As with the Strike Force and moratoria, the outlier cap had a large, negative impact on home health care utilization. Table OA.3 indicates that five years after implementation, the counties with an outlier cap bite above the 75th percentile had their home health visits per capita reduced by 17.6 percent (standard error = 3.5 percent) and their home health payments per capita reduced by 7.0 percent (standard error 2.6 percent). Like the Strike Force (and unlike the home health agency moratoria) it had no discernable effect on the share of patients using home health care. We estimate that on average in the five years after implementation, it reduced the share of Medicare enrollees using home health care by a statistically insignificant 0.4 (standard error = 1.1) percent. This is not surprising given the nature of the policy which was aimed at reducing spending among high-spending patients rather than use among lower spending or marginal patients.

E Details on the causal forest method

Causal forest is a method for uncovering heterogeneity in causal effects. The method mirrors the random forest methodology: while random forest splits the data repeatedly to minimize prediction error, causal forest splits the data to maximize the difference in treatment effects across splits. Despite the name, causal forest itself does not make the effects causal: 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 causal forest in our setting.

To apply a causal forest to our setting, we first difference out the set of fixed-effects and other covariates required for establishing causality in our event-study design, before assessing treatment

[23] As noted in Section 2.3, Moratoria came in starting in 2013, after the Strike Forces, so any parametric 'pre-trend' in home health care use prior to the moratoria will be partly absorbed by the Strike Force post-intervention relative year dummies.

effect heterogeneity along the eight attributes of interest. This appendix describes that process in detail.

E.1 Data Preparation

In its canonical form, the causal forest algorithm assumes randomly assigned treatment, and splits on a set of variables to maximize the difference across splits in the correlation between the treatment variable and the outcome variable, thereby characterizing the treatment effect heterogeneity. In our setting, we do not have randomly assigned treatment. Therefore, we must first difference out the set of fixed-effects and other covariates required for establishing causality in our event-study design, before assessing treatment effect heterogeneity along our variables of interest. Specifically, we difference out the county fixed effects, year fixed effects, and logarithmic trend in the relative year from from both the outcome variable and the treatment variable. We describe this step more formally below.

Strike Force and Moratorium For the Strike Force and moratorium, we first estimate

$$y_{it} = \alpha_{c(i,t)} + \tau_t + \sum_{r \notin \Omega} \beta_r SF_{c(i,t)r} + \gamma \log(R_{c(i,t)r}) + \sum_{\tilde{r} \notin \Omega} \theta_{\tilde{r}} M_{c(i,t)r} + \phi \log(\tilde{R}_{c(i,t)\tilde{r}}) + \epsilon_{it} \tag{12}$$

where y_{it} denotes patient i's home health utilization in year t. County and year fixed effects are denoted using $\alpha_{c(i,t)}$ and τ_t, respectively. $SF_{c(i,t)r}$ denotes a set of indicators for years relative to the opening of a Strike Force office in the county patient i lives in during year t. We use $R_{c(i,t)r}$ to denote the year relative to the opening of a Strike Force district in levels (set to zero for all non-Strike Force districts); this is shifted by seven since the earliest relative year in the data is -6 (all years prior to this are binned in a single indicator, while all years after relative year 6 are binned into a separate indicator). Similarly, $M_{c(i,t)r}$ denotes indicators for years relative to the beginning of a moratorium in county c, and $\tilde{R}_{c(i,t)\tilde{r}}$ denotes these relative years as a continuous variable. They are also binned separately for years prior to relative year -6 and after relative year 6 and shifted by 7 for the same reason. Ω denotes the set of relative years between -5 and -1, allowing each logarithmic trend to fit on the first five years before the policy.

Using the resulting estimates, we predict

$$\hat{y}_{it} = \hat{\alpha}_{c(i,t)} + \hat{\tau}_t + \hat{\gamma} \log(R_{c(i,t)r}) + \hat{\phi} \log(\tilde{R}_{c(i,t)\tilde{r}}) \tag{13}$$

To remove the logarithmic trends and the fixed effects from the outcome variable and estimate a homogeneous effect of the Strike Force and moratorium, we subtract $\hat{y}_{it}$ from the outcome and estimate

$$\tilde{y}_{it} = \alpha + \sum_{r \in -1 \cup \tilde{\Omega}} \beta_r SF_{c(i,t)r} + \beta \text{SF_POST}_{c(i,t)t} + \sum_{\tilde{r} \notin -1 \cup \tilde{\Omega}} \theta_{\tilde{r}} M_{c(i,t)\tilde{r}} + \theta \text{M_POST}_{c(i,t)t} + \epsilon_{it} \tag{14}$$

where $\tilde{y}_{it} \equiv y_{it} - \hat{y}_{it}$, $\text{SF_POST}_{c(i,t)t}$ is an indicator that takes value 1 in the five years post-treatment for Strike Force districts, and $\text{M_POST}_{c(i,t)t}$ is an indicator that takes value 1 in the five

years post-treatment for moratorium counties. As in the main text, $\tilde{\Omega}$ is a set containing relative years 1 through 5.[24]

The logic of our residualization process is as follows. By the Frisch-Waugh-Lovell theorem, β can be recovered as the coefficient of a bivariate regression of $\overline{y}_{it}$ on $\overline{\text{SF_POST}}_{c(i,t)t}$, where $\overline{y}_{it}$ is the residual from the regression

$$\tilde{y}_{it} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \beta_r SF_{c(i,t)r} + \sum_{\tilde{r} \notin -1 \cup \tilde{\Omega}} \theta_{\tilde{r}} M_{c(i,t)\tilde{r}} + \theta \text{M_POST}_{c(i,t)t} + \epsilon_{it} \tag{15}$$

and $\overline{\text{SF_POST}}_{c(i,t)t}$ is the residual from the regression

$$\text{SF_POST}_{c(i,t)t} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \beta_r SF_{c(i,t)r} + \sum_{\tilde{r} \notin -1 \cup \tilde{\Omega}} \theta_{\tilde{r}} M_{c(i,t)\tilde{r}} + \theta \text{M_POST}_{c(i,t)t} + \epsilon_{it} \tag{16}$$

Similarly, θ can be recovered as the coefficient of a bivariate regression of $\overline{\tilde{y}}_{it}$ on $\overline{\text{M_POST}}_{c(i,t)t}$, where $\overline{\tilde{y}}_{it}$ is the residual from the regression

$$\tilde{y}_{it} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \beta_r SF_{c(i,t)r} + \beta \text{SF_POST}_{c(i,t)t} + \sum_{\tilde{r} \notin -1 \cup \tilde{\Omega}} \theta_{\tilde{r}} M_{c(i,t)\tilde{r}} + \epsilon_{it} \tag{17}$$

and $\overline{\text{M_POST}}_{c(i,t)t}$ is the residual from the regression

$$\text{M_POST}_{c(i,t)t} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \beta_r SF_{c(i,t)r} + \beta \text{SF_POST}_{c(i,t)t} + \sum_{\tilde{r} \notin -1 \cup \tilde{\Omega}} \theta_{\tilde{r}} M_{c(i,t)\tilde{r}} + \epsilon_{it} \tag{18}$$

As a result, using $\overline{y}_{it}$ as the outcome variable with $\overline{\text{SF_POST}}_{c(i,t)t}$ as the treatment in the algorithm of Athey et al. (2019) allows us to obtain heterogeneous treatment effects of the Strike Force, while using $\overline{\tilde{y}}_{it}$ as the outcome variable with $\overline{\text{M_POST}}_{c(i,t)t}$ as the treatment allows us to obtain heterogeneous treatment effects of the moratorium.

Outlier Cap For the outlier cap, we first estimate

$$y_{it} = \alpha_{c(i,t)} + \tau_t + \sum_{r \notin \Omega} \phi_r O_{c(i,t)r} + \gamma \log(R_t) + \epsilon_{it} \tag{19}$$

where y_{it} is patient i's home health utilization in year t, $\alpha_{c(i,t)}$ denotes county fixed effects, and τ_t denotes year fixed effects. $O_{c(i,t)r}$ is an indicator for years relative to the beginning of the outlier cap in "treated" counties (those with an outlier cap "bite" above the unweighted 75th percentile). R_t denotes these relative years as a continuous variable, binned together for years prior to relative year -6 and separately for years after relative year 6, and set to zero for untreated counties. I use the resulting estimates to predict

$$\hat{y}_{it} = \hat{\alpha}_{c(i,t)} + \hat{\tau}_t + \hat{\gamma} \log(R_t) \tag{20}$$

and then estimate

[24] We have confirmed that the estimates using the five-year POST dummies are similar to the average five year effects shown in Table 2 estimated from the separate post indicator variables using equation 3.

$$\tilde{y}_{it} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \phi_r O_{c(i,t)r} + \phi \text{POST}_{c(i,t)t} + \epsilon_{it} \tag{21}$$

where $\tilde{y}_{it} \equiv y_{it} - \hat{y}_{it}$. Here, ϕ gives the homogeneous effect of the outlier cap. Again, the Frisch-Waugh-Lovell theorem tells us that ϕ can be obtained by the regression of $\overline{y}_{it}$ on $\overline{\text{POST}}_{c(i,t)t}$, where $\overline{y}_{it}$ is the residual from estimating

$$\tilde{y}_{it} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \phi_r O_{c(i,t)r} + \epsilon_{it} \tag{22}$$

and $\overline{\text{POST}}_{c(i,t)t}$ is the residual from estimating

$$\text{POST}_{c(i,t)t} = \alpha + \sum_{r \notin -1 \cup \tilde{\Omega}} \phi_r O_{c(i,t)r} + \epsilon_{it} \tag{23}$$

Using $\overline{y}_{it}$ as the outcome variable with $\overline{\text{POST}}_{c(i,t)t}$ as the treatment in the causal forest algorithm allows us to obtain heterogeneous treatment effects of the outlier cap.

E.2 Causal Forest Algorithm

We implement causal forest using the grf package in R, which takes the residualized outcome and residualized treatment as inputs, along with eight variables along which it generates heterogeneous treatment effects. These eight variables include six characteristics of HHAs in a patient's "choice set" (defined as HHAs in the patient's zip code) and two characteristics of the patient. The set of HHA characteristics include the average entry year, share of patients referred from the community (as opposed to the hospital), and the ratio between the number of patients in 2009 and in 2004, along with the share of HHAs in the zip code that are for-profit, non-profit, and government-owned. We aggreage these HHA-level characteristics to the zip code level by taking a weighted average where the weights are the share of episodes the HHA serves to patients in the zip code among all episodes in that zip code. The patient-level characteristics are lagged Medicare spending from the prior year and the number of comorbidities each patient has, defined as the number of chronic conditions they have in the Master Beneficiary Summary File.

For ease of notation, we index units by i only in this section. Let Y_i denote the outcome, W_i denote the treatment, and X_i denote the covariates. First, the algorithm marginalizes the covariates from both the outcome and treatment, estimating $\hat{Y}_i = E[Y_i|X_i]$ and $\hat{W}_i = E[W_i|X_i]$ to avoid confounding. We then use the following algorithm:

1. 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.

2. To grow each tree, the algorithm splits each node recursively. In other words, it starts with the 50% sample used to grow the tree. It then tries up to $\sqrt{p} + 20$ covariates to split the sample at random, where p is the total number of covariates provided[25]; for each covariate, it

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

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.

3. Each leaf continues to be split in this manner until each leaf cannot be split without creating a new set of leaves with fewer than 10,000 observations; this is our chosen "minimum node size".[26]

4. The same splits are then made on the 50% holdout set, and treatment effects are calculated on each leaf. All of the original patients falls in one of these leaves; they are assigned the treatment effect on the leaf they fall in.

5. The treatment effect for each patient is the average effect taken over the 250 trees we have grown.

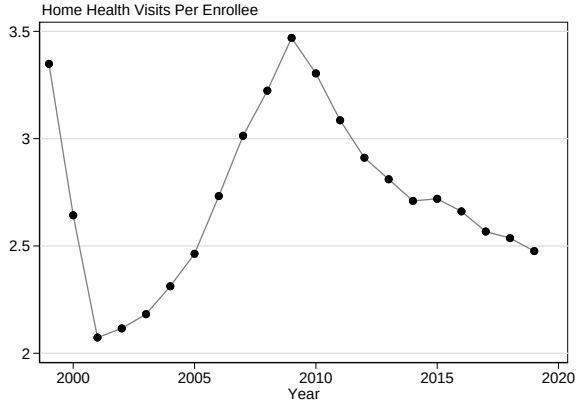
6. Standard errors for each treatment effect are computed using the "noisy bootstrap" procedure proposed by Sexton and Laake (2009).

[26] This was chosen via simulation. Ideally, it would be chosen via cross validation; however, we were constrained by our large sample size and limited computing resources.

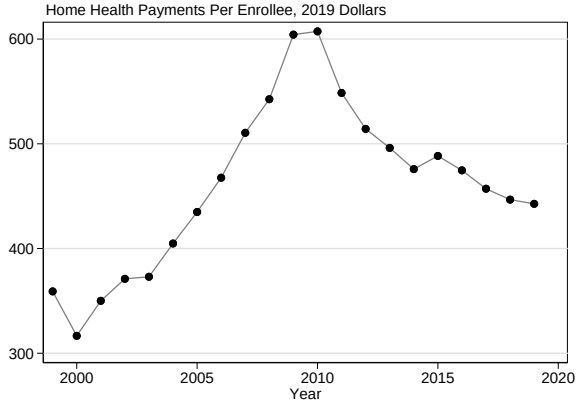
F Appendix Figures

Figure OA.1: Trends in HH Utilization, 1999-2019

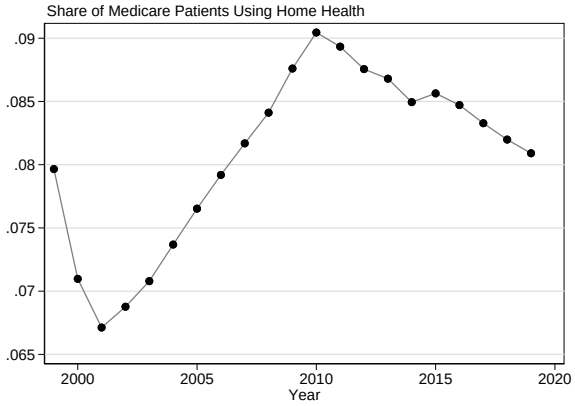
(a) Home Health Visits



(b) Home Health Payments



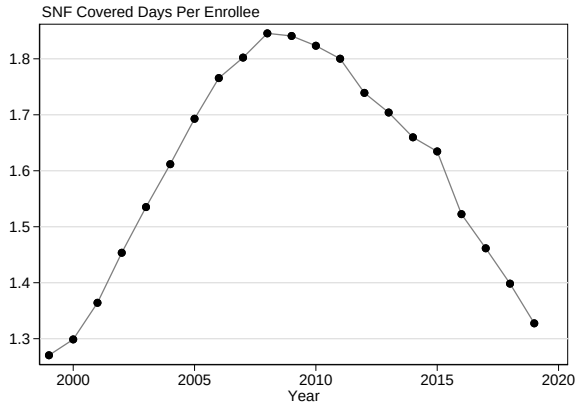
(c) Share of Patients Using Home Health



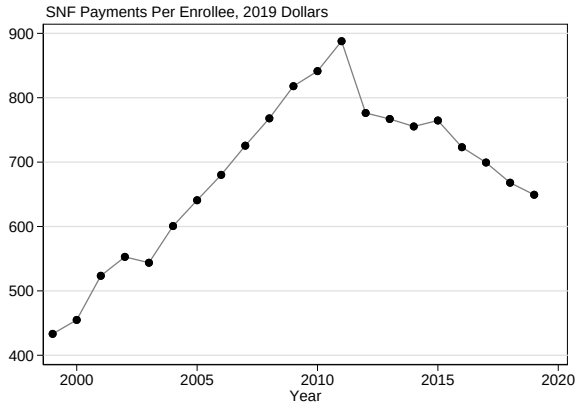
Notes: This figure displays the average level of home health utilization across the United States between 1999 and 2019 as measured by home health visits (Panel a), home health payments (Panel b), and the share of patients using home health (Panel c). The sample consists of all patients enrolled in Traditional Medicare for all 12 months of the year in the 20% claims files.

Figure OA.2: Trends in SNF Utilization, 1999-2019

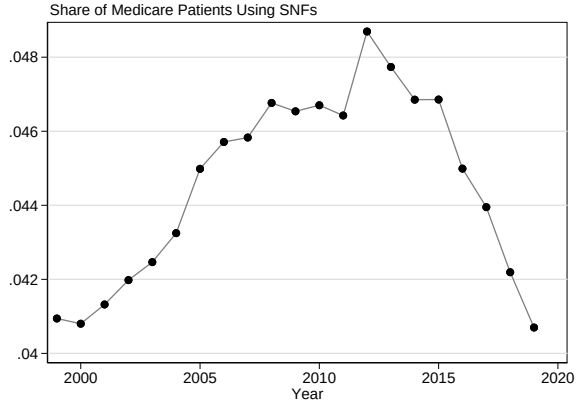
(a) SNF Covered Days



(b) SNF Payments



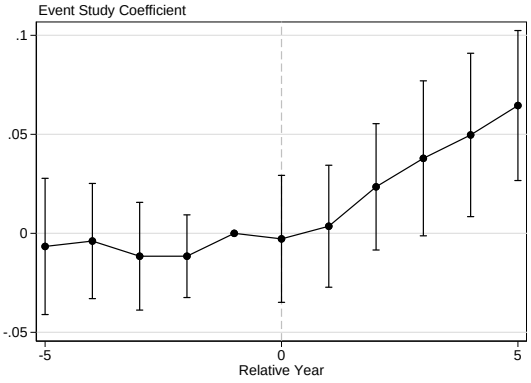
(c) Share of Patients Using SNFs



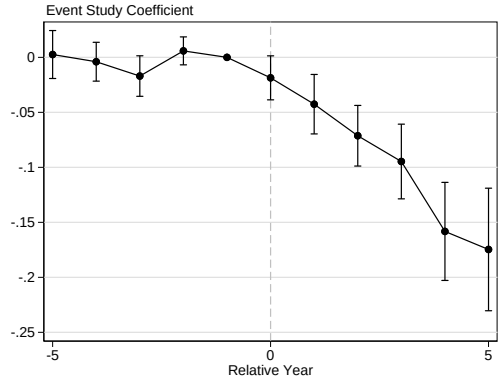
Notes: This figure displays the average level of SNF utilization across the United States between 1999 and 2019 as measured by SNF covered days (Panel a), SNF payments (Panel b), and the share of patients using SNFs (Panel c). The sample consists of all patients enrolled in Traditional Medicare for all 12 months of the year in the 20% claims files.

Figure OA.3: Impact of Policies on Log SNF Covered Days

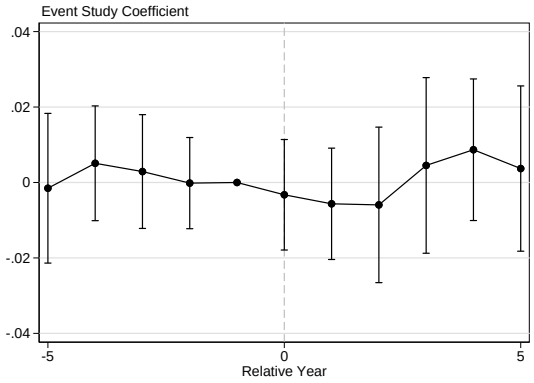
(a) Strike Force



(b) Moratorium



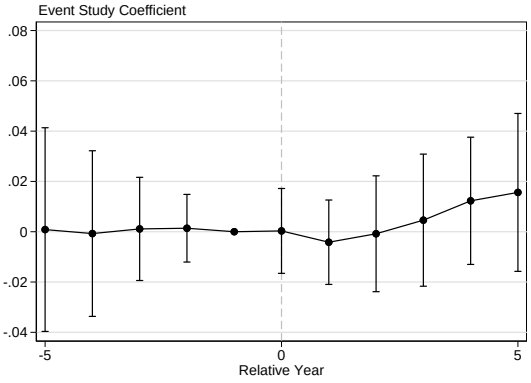
(c) Outlier Cap



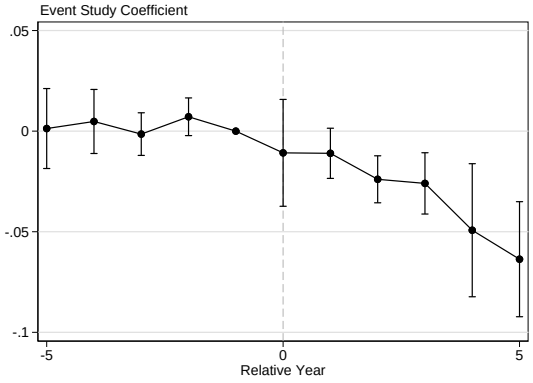
Notes: This figure displays event study estimates of the effects of each policy on log SNF covered days per 100 enrollees. Vertical lines denote 95% confidence intervals constructed using standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006.

Figure OA.4: Impact of Policies on Log Share of Enrollees Using SNFs

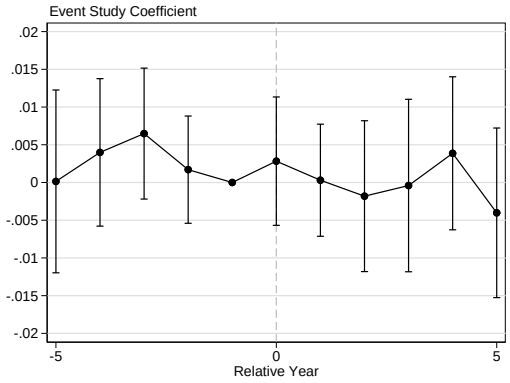
(a) Strike Force



(b) Moratorium



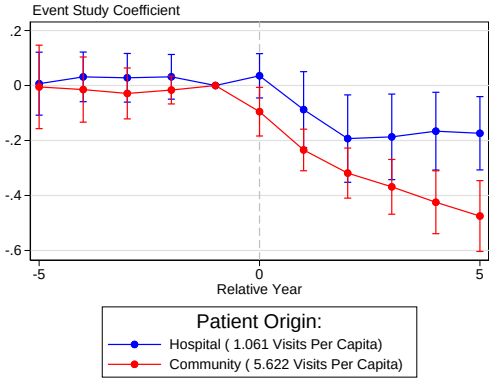
(c) Outlier Cap



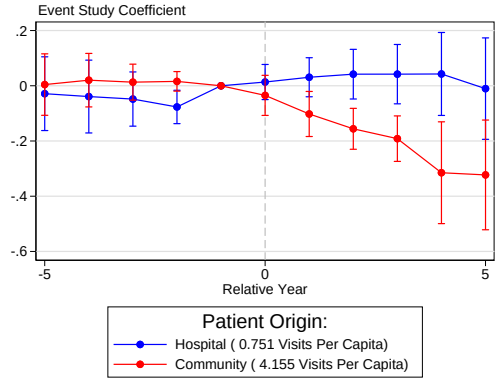
Notes: This figure displays event study estimates of the effects of each policy on the log share of Medicare enrollees using SNFs. Vertical lines denote 95% confidence intervals constructed using standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006.

Figure OA.5: Impact of Policies on Log Home Health Visits by Patient Origin

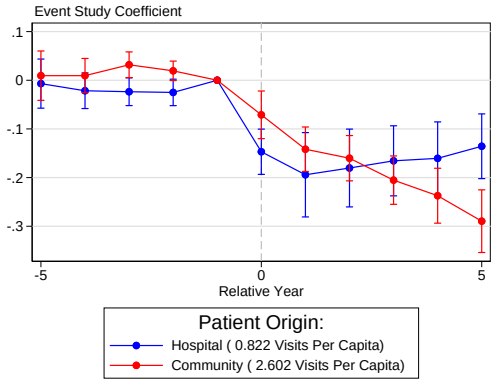
(a) Strike Force



(b) Moratorium



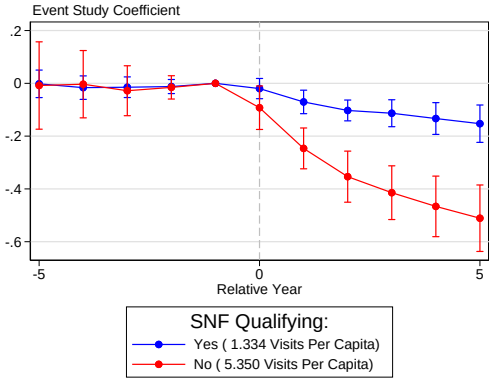
(c) Outlier Cap



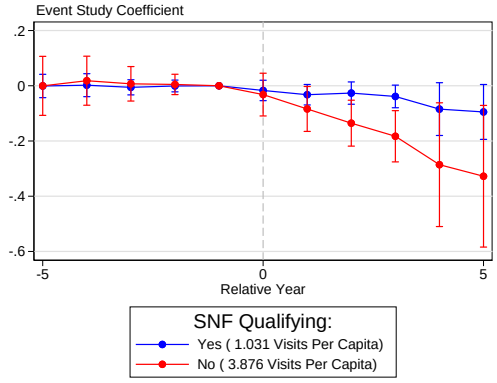
Notes: This figure displays event study estimates of the effects of each policy on home health visits per capita, separately for patients referred to HHAs from the community and from the hospital. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006.

Figure OA.6: Impact of Policies on Log Home Health Visits by SNF Qualifying Status

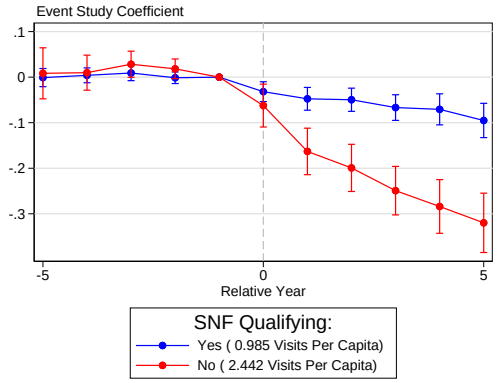
(a) Strike Force



(b) Moratorium



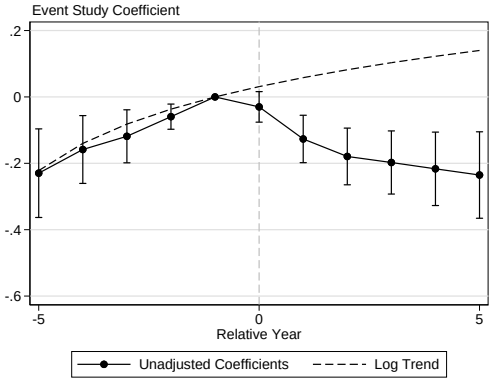
(c) Outlier Cap



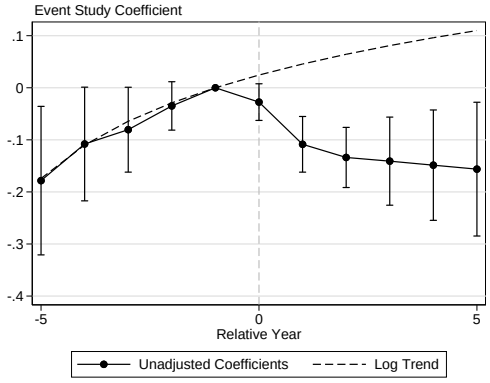
Notes: This figure displays event study estimates of the effects of each policy on home health visits per capita, separately for utilization occurring during episodes starting within 30 days of a discharge from an inpatient hospital stay lasting at least 3 days ("SNF qualifying") and otherwise. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006.

Figure OA.7: Strike Force Effects on Log Home Health Care Utilization

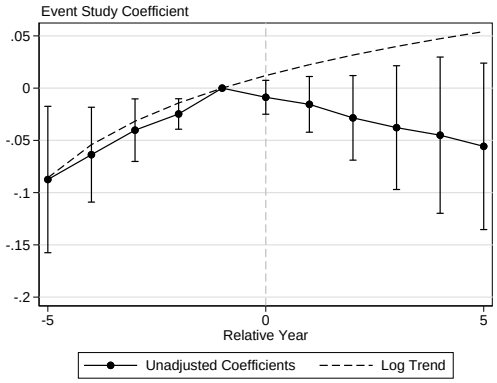
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



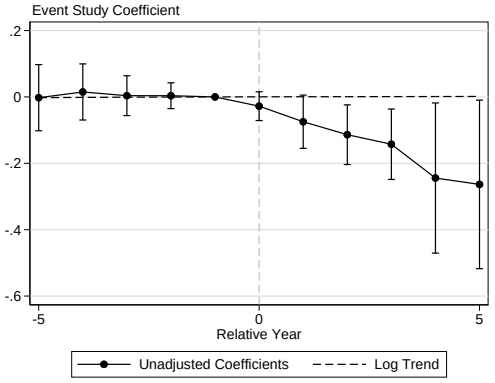
(c) Enrollees Using Home Health Per 100



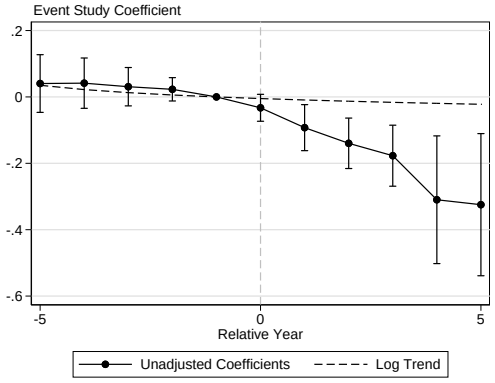
Notes: this figure shows event study estimates of β_r in equation (1) with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. The dashed line represents the logarithmic trend in the outcome variable fitted according to equation (2). The sample size is 63,552 county-years.

Figure OA.8: Moratorium Effects on Log Home Health Care Utilization

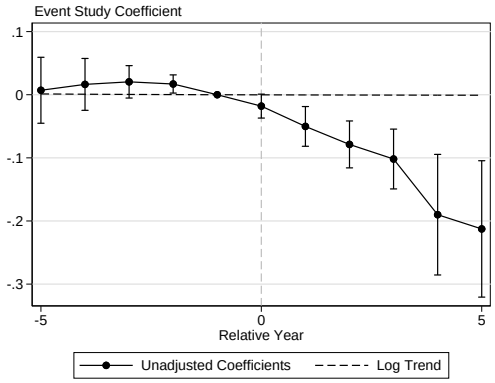
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



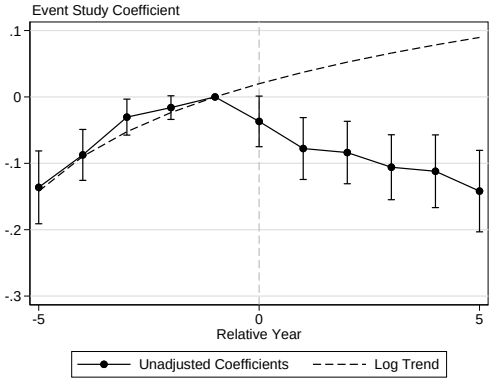
(c) Enrollees Using Home Health Per 100



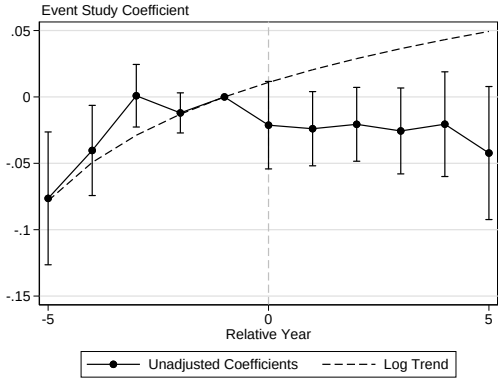
Notes: this figure shows event study estimates of $\theta_{\tilde{r}}$ in equation (1) with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the district level. The dashed line represents the logarithmic trend in the outcome variable fitted according to equation (2). The sample size is 63,552 county-years.

Figure OA.9: Outlier Payment Cap Effects on Log Home Health Care Utilization

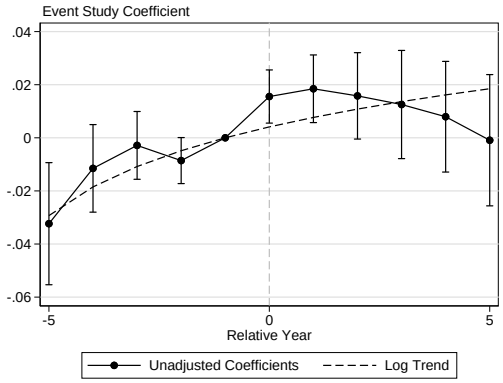
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



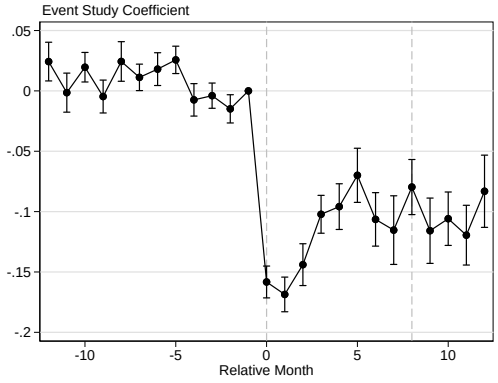
(c) Enrollees Using Home Health Per 100



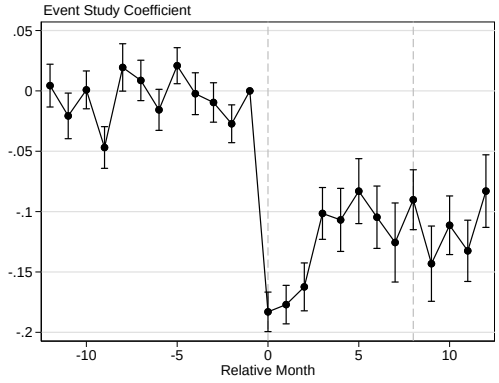
Notes: this figure shows event study estimates of β_r in equation (8) with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2006. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. The dashed line represents the logarithmic trend in the outcome variable fitted according to equation (9). The sample size is 63,552 county-years.

Figure OA.10: Pre-Claims Review Effects on Log Home Health Care Utilization

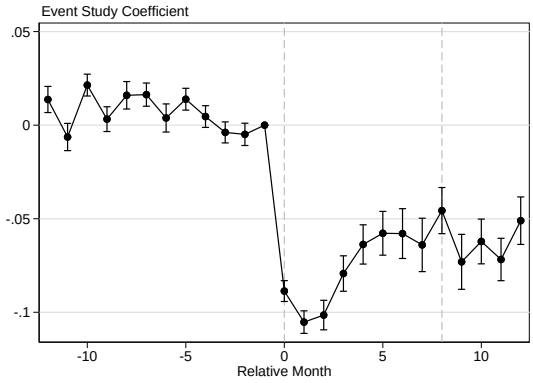
(a) Home Health Visits Per 100



(b) Home Health Payments Per 100



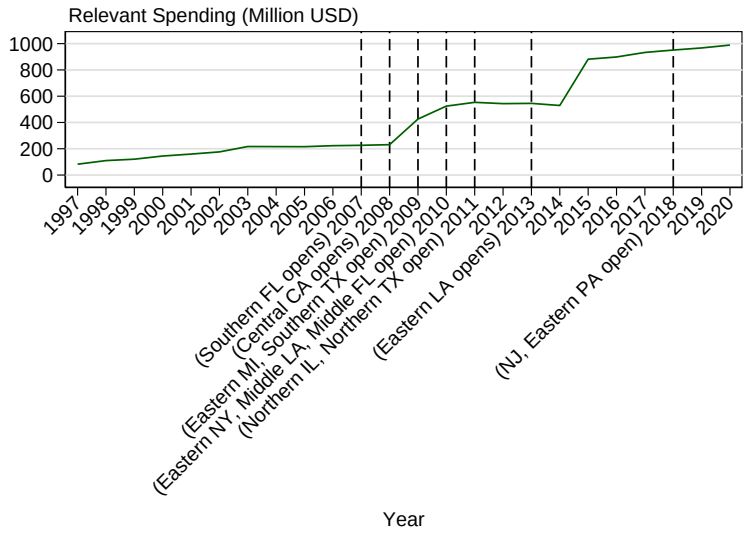
(c) Enrollees Using Home Health Per 100



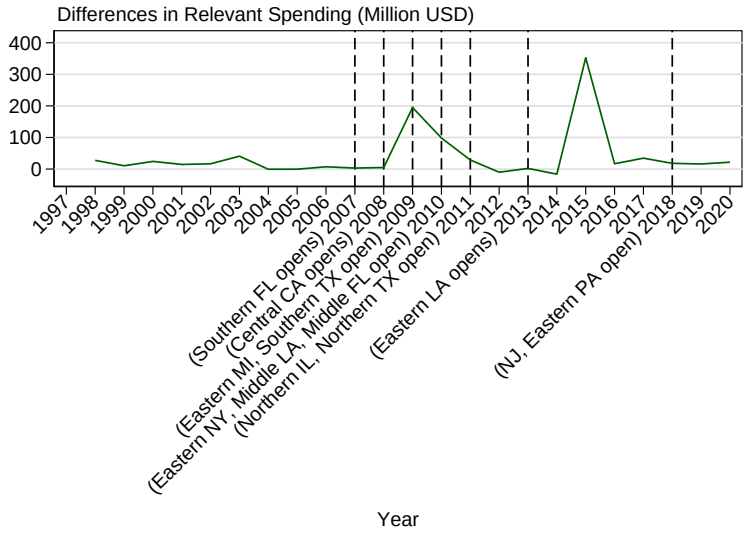
Notes: this figure shows event study estimates of (β_r) in equation 11 with several measures of per capita home health utilization as the outcome. The regressions are weighted by the number of Traditional Medicare enrollees in each county in 2016. The first gray dashed vertical line indicates the implementation of the policy in August 2016, while the second gray dashed line indicates the program's suspension in April 2017. Vertical lines denote 95% confidence intervals computed using standard errors clustered at the county level. The sample size is 79,392 county-months.

Figure OA.11: Spending Relevant to Strike Forces in HCFAC Reports

(a) Levels



(b) First Differences



Notes: Figure OA.11a displays trends in "relevant spending" used to estimate costs of the Strike Force, as defined in Appendix C. Figure OA.11b displays first differences in this spending.

G Appendix Tables

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	-

Notes: This table displays the location of each Strike Force office examined in our study, the date the office opened, and the date the strike force 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. There are two additional offices in the District of New Jersey and the Eastern District of Pennsylvania, which both opened in August 2018. Given that our sample period ends in 2019, we elected to exclude all counties in these districts from our sample.

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.

Table OA.3: Effect of Policies on Utilization of Home Health Care

	Average of Relative Period Coefficients:		
	1	1-3	1-5
Panel A: Log Home Health Visits Per Capita			
Strike Force	-0.168	-0.232	-0.276
	(0.041)	(0.052)	(0.062)
Moratorium	-0.080	-0.116	-0.173
	(0.043)	(0.045)	(0.069)
Outlier Cap	-0.122	-0.148	-0.176
	(0.030)	(0.032)	(0.035)
Panel B: Log Home Health Payments Per Capita			
Strike Force	-0.151	-0.186	-0.212
	(0.031)	(0.043)	(0.058)
Moratorium	-0.095	-0.135	-0.204
	(0.032)	(0.032)	(0.051)
Outlier Cap	-0.052	-0.060	-0.070
	(0.019)	(0.021)	(0.026)
Panel C: Log Share of Enrollees Using Home Health			
Strike Force	-0.034	-0.052	-0.070
	(0.020)	(0.032)	(0.042)
Moratorium	-0.061	-0.088	-0.137
	(0.014)	(0.016)	(0.027)
Outlier Cap	0.009	0.003	-0.004
	(0.007)	(0.009)	(0.011)

Notes: This table displays several averages of post-period coefficients assessing the impact of each policy on several measures of home health utilization: the β_r's from equation (2) for the Strike Force, the $\theta_{\tilde{r}}$'s from equation (2) for the moratorium, and the β_r's from equation (9) for the outlier cap. Standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level are displayed in parentheses below each estimate. The sample size is 3,177 counties.

Table OA.4: Effect of Policies on SNF Utilization

	Average of Relative Period Coefficients:		
	1	1-3	1-5
Panel A: Log SNF Covered Days			
Strike Force	0.004	0.022	0.036
	(0.016)	(0.017)	(0.017)
Moratorium	-0.043	-0.070	-0.108
	(0.014)	(0.014)	(0.016)
Outlier Cap	-0.006	-0.002	0.001
	(0.008)	(0.009)	(0.009)
Panel B: Log Share of Enrollees Using SNFs			
Strike Force	-0.004	-0.000	0.006
	(0.009)	(0.011)	(0.012)
Moratorium	-0.011	-0.020	-0.035
	(0.006)	(0.006)	(0.008)
Outlier Cap	0.000	-0.001	-0.000
	(0.004)	(0.004)	(0.004)

Notes: This table displays several averages of post-period coefficients assessing the impact of each policy on several measures of SNF utilization: the β_r's from equation (3) for the Strike Force, the $\theta_{\tilde{r}}$'s from equation (3) for the moratorium, and the β_r's from equation (4) for the outlier cap. Standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level are displayed in parentheses below each estimate. The sample size is 3,177 counties.

Table OA.5: Effect of Policies on Home Health Visits by Patient Origin

		Average of Relative Period Coefficients:		
	Patient Origin	1	1-3	1-5
Strike Force	Hospital	-0.088	-0.156	-0.161
		(0.070)	(0.075)	(0.069)
	Community	-0.234	-0.307	-0.364
		(0.039)	(0.042)	(0.045)
Moratorium	Hospital	0.031	0.038	0.030
		(0.036)	(0.045)	(0.058)
	Community	-0.102	-0.150	-0.218
		(0.042)	(0.039)	(0.061)
Outlier Cap	Hospital	-0.194	-0.180	-0.167
		(0.044)	(0.040)	(0.037)
	Community	-0.142	-0.169	-0.207
		(0.023)	(0.024)	(0.025)

Notes: This table displays several averages of post-period coefficients assessing the impact of each policy on log home health visits per capita from the hospital and from the community: the β_r's from equation (3) for the Strike Force, the $\theta_{\tilde{r}}$'s from equation (3) for the moratorium, and the β_r's from equation (4) for the outlier cap. Standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level are displayed in parentheses below each estimate. The sample size is 3,177 counties.

Table OA.6: Effect of Policies on Home Health Visits by SNF Qualifying Usage

		Average of Relative Period Coefficients:		
	SNF Qualifying	1	1-3	1-5
Strike Force	Yes	-0.071	-0.096	-0.115
		(0.023)	(0.021)	(0.023)
	No	-0.247	-0.338	-0.398
		(0.039)	(0.044)	(0.047)
Moratorium	Yes	-0.032	-0.033	-0.055
		(0.019)	(0.019)	(0.030)
	No	-0.084	-0.134	-0.203
		(0.042)	(0.043)	(0.072)
Outlier Cap	Yes	-0.048	-0.055	-0.066
		(0.013)	(0.013)	(0.014)
	No	-0.163	-0.204	-0.243
		(0.026)	(0.026)	(0.027)

Notes: This table displays several averages of post-period coefficients assessing the impact of each policy on log home health visits per capita occurring as part of episodes starting within 30 days of a discharge from an inpatient stay of at least 3 days ("SNF qualifying") or otherwise: the β_r's from equation (3) for the Strike Force, the $\theta_{\bar{r}}$'s from equation (3) for the moratorium, and the β_r's from equation (4) for the outlier cap. Standard errors clustered at the district (Strike Force and moratorium) or county (outlier cap) level are displayed in parentheses below each estimate. The sample size is 3,177 counties.

Table OA.7: Heterogeneous Treatment Effect Summary Statistics

	Effect on Visits			Effect on Payments		
Statistic	Strike Force	Moratorium	Outlier Cap	Strike Force	Moratorium	Outlier Cap
Mean	-2.431	-0.074	-1.175	-171.822	-62.963	-68.503
SD	4.418	0.847	6.849	309.045	106.407	452.367

Notes: This table displays the mean and standard deviation of the heterogeneous treatment effect of each policy on each measure of home health utilization. Since the effect of the outlier cap depends on the "bite" of policy, we scale each individual-level treatment effect by the ratio of the bite in the county the patient resides in to the average bite in the treated counties (i.e. the average among counties with a "bite" above the 75th percentile). The sample size is 6,495,865 patient-years.

Table OA.8: Causal Forest Variable Importance

	Effect on Visits			Effect on Payments		
Covariate	Strike Force	Moratorium	Outlier Cap	Strike Force	Moratorium	Outlier Cap
HHA Entry Year	0.251	0.179	0.247	0.088	0.204	0.110
Average Share of Patients From Community	0.081	0.053	0.085	0.080	0.032	0.124
Share of For-Profit HHAs	0.018	0.017	0.033	0.029	0.014	0.021
Share of Non-Profit HHAs	0.010	0.008	0.016	0.020	0.013	0.011
Share of Government HHAs	0.013	0.014	0.006	0.021	0.011	0.008
Average 2009/2004 HHA Patient Ratio	0.010	0.018	0.008	0.023	0.054	0.011
Lagged Patient Spending	0.554	0.525	0.542	0.651	0.505	0.661
Patient Comorbidities	0.063	0.185	0.063	0.088	0.166	0.054

Notes: This table displays importance weights for each of the eight covariates. Importance weights are defined as the share of splits made on each attribute.

Table OA.9: Correlation Between Treatment Effects and Covariates

	Effect on Visits			Effect on Payments		
Covariate	Strike Force	Moratorium	Outlier Cap	Strike Force	Moratorium	Outlier Cap
HHA Entry Year	-0.451	-0.084	-1.259	$15.09	$-28.35	$-54.08
	(0.113)	(0.017)	(0.515)	($4.94)	($2.29)	($23.53)
Average Share of Patients From Community	-0.757	-0.125	-1.098	$-27.02	$-22.75	$-49.35
	(0.077)	(0.011)	(0.413)	($3.74)	($2.29)	($19.02)
Share of For-Profit HHAs	-0.579	-0.110	-1.014	$-13.40	$-26.48	$-44.68
	(0.085)	(0.013)	(0.390)	($4.73)	($2.10)	($18.29)
Share of Non-Profit HHAs	0.572	0.108	0.966	$13.35	$25.89	$41.92
	(0.085)	(0.014)	(0.382)	($4.98)	($2.14)	($17.92)
Share of Government HHAs	0.029	0.009	0.200	$0.43	$2.39	$11.46
	(0.026)	(0.004)	(0.071)	($1.25)	($0.87)	($3.87)
Average 2009/2004 HHA Patient Ratio	-0.170	-0.027	-0.294	$-6.80	$-11.17	$-13.30
	(0.035)	(0.006)	(0.184)	($1.83)	($1.88)	($9.60)
Lagged Patient Spending	-3.194	-0.662	-1.615	$-226.57	$-66.09	$-116.84
	(0.149)	(0.016)	(0.592)	($10.88)	($2.73)	($42.21)
Patient Comorbidities	-1.630	-0.468	-0.891	$-97.37	$-57.91	$-42.50
	(0.069)	(0.010)	(0.343)	($4.73)	($1.89)	($17.43)

Notes: This table displays estimates of β in the bivariate regression $y_{it} = \alpha + \beta x_{it} + \epsilon_{it}$, where x_{it} is the (standardized) variable given by each row title and y_{it} is given by each column title. Standard errors clustered at the district level are displayed in parentheses. The sample size is 6,495,865 patient-years.

Table OA.10: 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.11: Yearly Strike Force Cost Estimates

Year	Strike Force Offices Open	Operation Cost (Millions)
2007	Southern FL	$3.138
2008	Southern FL, Central CA	$8.016
2009	Southern FL, Central CA, Eastern MI, Southern TX	$202.906
2010	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL	$301.287
2011	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX	$330.126
2012	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX	$330.126
2013	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA	$332.191
2014	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA	$332.191
2015	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA	$332.191
2016	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA	$332.191
2017	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA	$332.191
2018	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA, NJ, Eastern PA	$350.352
2019	Southern FL, Central CA, Eastern MI, Southern TX, Eastern NY, Middle LA, Middle FL, Northern IL, Northern TX, Eastern LA, NJ, Eastern PA	$350.352
Total		$3,537.258

Notes: This table displays back-of-the-envelope estimates of the cost of operating Medicare Fraud Strike Force offices by year. The methodology used to obtain these estimates is described in Appendix C.