ESSAYS IN HEALTH ECONOMICS

by

Markus B. Bjoerkheim
A Dissertation
Submitted to the
Graduate Faculty
of
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in Partial Fulfillment of
The Requirements for the Degree
of
Doctor of Philosophy
Economics

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Essays in Health Economics

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at George Mason University

By

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Dedication

I dedicate this work to my loving wife Heather, the ultimate teammate, and to my parents, whom I miss dearly, but know would be proud if they were here today.

Acknowledgements

I would like to thank my dissertation committee, Drs. Alex Tabarrok, Thomas Stratmann, and Alison Cuellar for their work and support during my years at George Mason University. Alex's mentorship combined the idealism of pursuing ambitious projects with the encouragement to continue moving forward at times when I failed to meet my own expectations. Thomas for excellent feedback and for keeping me on my toes, and Alison for providing me with opportunities outside the economics department. I hope my work with the three of you will continue.

I am also grateful to a great number of people associated with the Economics Department and the Institute for Humane Studies for support during my studies. This is especially true for the late Professor Walter E. Williams who both provided support and served as an incredible role model that I am honored to have worked with. A special thanks goes to Mary Jackson; I wonder where we'd all be without you.

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Abstract

ESSAYS IN HEALTH ECONOMICS

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George Mason University, 2022

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This dissertation consists of three papers in health economics. The first paper estimates the effect of targeting oversight toward underperforming nursing homes on quality of care. The second, co-authored with Alex Tabarrok, examines the course of the pandemic in nursing homes focusing especially on whether nursing homes could have been better shielded, and the third asks whether pandemic-era expansions of unemployment programs discouraged work.

Certification requirements are a key policy lever to incentivize quality in the nursing home industry, but the effects of regulation have been hard to identify, in part because homes are all subject to the same regulatory requirements and sanctions are imposed in response to care failures. This paper studies the Special Focus Facility program (SFF), which aims to improve quality of care by targeting oversight toward some, but not all, of the industry's worst performing facilities. I leverage capacity constraints in a difference-in-difference framework and show that after two years treated facilities improve an additional 17% compared to untreated candidates, or about two fewer deficient practices. I find performance reverts back *after treatment ends*, which makes alternative explanations unlikely.

The death toll in nursing homes accounted for almost 30% of total COVID-19 deaths in the U.S. during 2020. We examine the course of the pandemic in nursing homes focusing especially on whether nursing homes could have been better shielded. Across all nursing homes the key predictor of infections and deaths was community spread, i.e., a factor outside of the control of nursing homes. We find that higher quality nursing homes, as measured by the CMS Five-Star Rating system, were not better able to protect their residents. Policy failures from the CDC and FDA, especially in the early stages of the pandemic, created extended wait times for COVID-19 tests which hampered attempts to isolate infectious residents and allowed outbreaks to grow larger and deadlier. But testing would have had to have been much greater to have had an appreciable effect on nursing home deaths once community spread became widespread. We find, however, that starting vaccinations just five weeks earlier could have saved on the order of 14,000 lives and starting them ten weeks earlier could have saved 40,000 lives.

The generosity and coverage of unemployment insurance increased dramatically with the March 2020 passing of the CARES Act. This paper investigates whether these expansions discouraged nurses and nurse aides from returning to work in the nursing home industry. Using variation from 24 states that withdrew from the programs in a difference-in-difference design, I find suggestive evidence the withdrawals reduced facility-reported labor shortages by between 0.5-2 percentage points (3-11%). Placebo tests using physicians, physician assistants, and advanced practice nurses find no effect. Placebo tests using states that intended to withdraw but were ordered to remain are inconclusive.

Chapter 1. Do Patients Benefit from Regulatory Stringency? Evidence from Targeted Nursing Homes

Markus Bjoerkheim
George Mason University

Abstract

Certification requirements are a key policy lever to incentivize quality in the nursing home industry, but the effects of regulation have been hard to identify, in part because homes are all subject to the same regulatory requirements and sanctions are imposed in response to care failures. This paper studies the Special Focus Facility program (SFF), which aims to improve quality of care by targeting oversight toward some, but not all, of the industry's worst performing facilities. I leverage capacity constraints in a difference-in-difference framework and show that after two years treated facilities improve an additional 17% compared to untreated candidates, or about two fewer deficient practices. I find performance reverts back *after treatment ends*, which makes alternative explanations unlikely.

Keywords: Regulation, Deficiencies, Minimum Quality Standards, Targeting, Nursing Homes
JEL Classification: I180, L510, K230

Introduction

Despite requirements to meet hundreds of minimum care standards, U.S. nursing homes have a history of quality concerns including inadequate staff-resident ratios and infection control practices. Standards are enforced through a deterrence model based on unannounced inspections, during which inspectors observe facility practices and interview staff and residents. Regulators have the power to fine, deny payment, or even decertify facilities who fail to meet minimum care standards.

During annual inspections, 20% of facilities are found to have care practices that inspectors deem as causing "actual harm" to residents, leading residents to have unsupervised falls and accidents, acquire preventable bedsores, and contract potentially dangerous infections (United States General Accounting Office, 1999). Care quality has also been found to persist in the same facilities over time (Grabowski and Castle, 2004; Walshe and Harrington, 2002). However, enforcement remains largely one-size-fits-all; facilities with proven records of excellence receive the same inspection frequency and scrutiny, as those with severe and persistent deficiencies.

When inspectors find quality issues, state health departments recommend enforcement actions, which the Center for Medicare and Medicaid Services (CMS) carries out. Health departments tend to recommend the least stringent sanction available, which in practice means that most facilities receive opportunities to correct problems before sanctions are imposed. A common observation has been that facilities correct problems "just in time" to avoid mandatory termination, only to have the same problem resurface on the next inspection, which suggests the underlying problems have not been addressed.

In a simple model of nursing home markets, firms face a trade-off between the quality of care they provide, which is costly, and profits, due to low competitive pressures in geographically segmented markets. Deterrence-based regulation aims to raise the cost of failing to meet minimum care standards by adding an expected regulatory cost term to the firm's optimization problem, which is the product of the probability of detection times the cost of the sanction if detected.

The only federal exception to the one-size-fits-all regulatory approach is the Special Focus Facility (SFF) program. CMS started the SFF program in 1998 with the goal of improving quality of care by targeting oversight toward some of the industry's worst performing facilities. The SFF program has since targeted over 1,000 facilities using a three-pronged approach to raise the expected cost of failing to meet minimum care standards.

First, by doubling the frequency of inspections to every six months, facilities face increased likelihood that any given care failure is discovered by regulators. Second, by requiring progressively stringent sanctions when deficiencies are discovered, the expected regulatory cost conditional on discovery, are raised as well. Third, the program applies additional scrutiny until facilities complete *two consecutive inspections* without any "harm-level" deficiencies, or, if it fails to achieve this in 2-3 years, risk decertification from Medicaid & Medicare programs, which in practice means the facility would shut down, imposing significant financial losses.

Since 1998, the SFF program has targeted over 1000 facilities and imposed swift, progressively harsh penalties, including threat of termination unless the facility completes two consecutive inspections without any findings of harm. This paper estimates the impact that has on the care quality provided to residents.

The current literature on enforcement of certification requirements provides mixed evidence for whether firms respond to regulatory stringency by improving care quality, but effects are hard to estimate causally because (a) homes are subject to the same requirements, (b), enforcement actions are endogenously related to quality, and, (c), the most commonly evaluated outcome, inspection results, could reflect both changes in quality but also changing stringency of state inspectors (Walshe, 2001). A final difficulty, (d), is that inspection results have a natural tendency for mean-reversion, which means that year-to-year within-facility changes can overstate the effects of enforcement actions without a credible counterfactual.

To overcome the identification challenge in (b), this paper leverages the program's capacity constraints, which are capped by state. The cap has meant that while each state has many poor performers, similar on observable characteristics and performance trajectories, the program can only target a fixed number of facilities at any given time, creating close-to-random variation in which firms are treated. I exploit this in a difference-in-difference framework to produce the first causal effects estimates of targeted enforcement on nursing home care standards and health outcomes.

To recover a causal estimate of the SFF program's *additional targeting,* I use data from a Freedom of Information Act request of facilities that were *candidates* for the program, but that were not targeted. Restricting comparisons to the most similar candidate facilities in the same state recovers 600 policy experiments (cohorts) of facilities who followed similar paths in the years leading up to treatment. Restricting comparisons to come from within state-years overcomes I, because facilities are subject to the state's same overall stringency, and come close to overcoming (d), because any mean-reversion tendency should be similar for treated and untreated facilities.

I combine generalized difference-in-difference specifications with Coarsened Exact Matching to further overcome (d) and find comparisons with parallel trends leading up to treatment. The average targeted facility improves significantly more than candidate counterparts; two years after treatment, the average targeted facility receives two fewer deficiencies, an additional 17% reduction relative to the average candidate, who improves 48%. These results support the hypothesis that additional oversight benefits patients.

The alternative hypothesis is that additional oversight does not benefit patients; treated facilities would have improved more than candidates irrespective of the SFF-program. This could only be due to a time-varying factor that starts affecting treated facilities (more), just after treatment is assigned. However, this alternative hypothesis gives a clear prediction: we expect targeted facilities to be unaffected when graduating from the program. I find clear evidence that performance deteriorates after treated facilities graduate the SFF program. When considered together, these two findings are hard to reconcile with alternative explanations; the omitted factor(s) would have to both *turn on* and then *off again* disproportionately for treated facilities *at the exact right times*. I hold that this is implausible considering that enrollment and graduation events happen across such vast sets of time-space combinations.

Another concern is whether the program has unintended consequences. There is a large literature documenting the difficulty of crafting regulatory interventions to raise quality of care, because quality is multi-dimensional, interventions that raise quality on one margin, will often be, at least partially, offset on other margins (Bowblis et al., 2012; Bowblis and Lucas, 2012; Chen and Grabowski, 2015; Konetzka et al., 2013). I find little evidence of this when breaking down survey scores into 15 detailed subcategories, which largely mirror the overall findings.

Section 2 summarizes the literature on nursing home enforcement and regulation, as well their impacts on quality of care. Section 3 describes the SFF program including changes to the program over time that are relevant to the empirical analysis. Section 4 describes the data and provides summary statistics. Section 5 and 6 describes the empirical framework and estimation techniques and interprets the results across various specifications and data sources. Section 7 concludes.

Literature Review

There is a large literature devoted to the documentation of quality of care in nursing homes, variables that are associated with differences in care, and policy aimed at incentivizing better care. I will review the literature that considers regulatory mechanisms to incentivize quality improvements, including two descriptive papers on the SFF program, and then review an important paper by Hackmann (2019) who argues for increasing Medicaid payment rates. Harrington and Carrillo (1999) were among the first to analyze nationwide deficiency trends for all U.S. nursing homes and found that the number of deficiencies declined by 44% and the number of firms with perfect survey scores doubled from 1991-97. While Harrington and Carrillo (1999) acknowledge studies demonstrating "innovative efforts to reduce the use of restraints, to improve incontinence care, and to make other improvements in care" they are ultimately skeptical of interpreting the decrease in deficiencies as improvements in quality, and offer alternative interpretations such as the industry learning to avoid detection and regulators becoming less vigorous over time. They also cite Toby Edelman, who argued enforcement had been watered down by allowing firms the opportunity to correct deficiencies before applying sanctions, and by changing interpretive guidance such as the term "widespread" to only cover violations that affect all residents in the facility. Walshe (2001) provides an early survey of the U.S. regulatory system.

> [t]he impact of regulation has not been much researched, in part perhaps because it presents several methodological challengl... [a]ltough numerous studies have examined the implementation of nursing home regulation and the management of regulatory arrangements, these reports are of limited help in determining what impact regulation has had on nursing home performance and the quality of nursing home care."

He proceeds to describe three methodological/identification challenges to estimate the impact of regulation on performance. First, since (virtually) all nursing homes are regulated, no control group exists to compare regulated homes to, which "means that one can really only study changes in quality over time and attempt to determine whether those changes can be attributed to regulatory interventions." Second, it is challenging to distinguish changes in quality from changes in the regulatory process, using available data that is itself the product of the regulatory process. Third, Walshe notes that the reliability, validity, completeness, and timeliness of the available data has been questioned and suggests caution is needed when analyzing survey data. Despite progress in the past 20 years since Walshe (2001) and Harrington and Carrillo (1999) the task of overcoming these identification and methodological challenges remain incomplete. One strain of literature focuses on the first point raised by Walshe; that since all firms are regulated, no firms can form a control group. This is probably true with respect to federal regulations on the *extensive margin*, as one cannot operate a federally certified nursing home without being subject to the federal participation requirements, However, it is clearly not true on various *intensive margins*.

Another strain of literature considers targeting survey resources toward low-performing facilities more specifically.

> "Federal and state survey efforts [should] focus more on providers that are chronically poor performers by surveying them more frequently than required for other facilities, increasing penalties for repeated violations of standards, and de-certifying persistently substandard providers"
> Wunderlich and Kohler (2001) Committee on Improving Quality in Long-Term Care Institute of Medicine

The idea was specifically addressed by Grabowski and Castle (2004) who are skeptical of whether enforcement will address the underlying causes of poor-quality care

> "Clearly, targeted efforts to penalize low-quality facilities may be effective in the short run, but this proposal raises broader questions in the long run as to whether the root causes of persistently low quality will be addressed. That is, if low Medicaid payment rates or a lack of consumer information are the underlying sources of persistent low quality, it is unclear that simply shutting down chronic offenders will address the larger problem. The low-quality nursing home may not persist in a highly regulated environment, but the presence of low-quality care might"
> Grabowski and Castle (2004)

Three articles are published specifically on the Special Focus Facility program. Castle and Engberg (2010) provide a descriptive examination where they compare the certification scores and MDS quality measures of facilities that participated in the program during 2007 with those of all other facilities. Castle and Engberg (2010) find that SFF facilities receive on average twice as many deficiencies (12.36 vs 6.91) and quality of care deficiencies (2.80 vs 1.50), as well as nine times as many deficiencies for placing residents under immediate jeopardy of health and safety (0.36 vs 0.04). They also find that residents in SFFs are prescribed more antipsychotic medications (30.80% vs 25%) and are more frequently put under physical restraints (7.1% vs 5.4%), and conclude that CMS succeeded in targeting facilities of poor quality during the year studied (2007).

Castle et al. (2010) examine whether SFF participation of one facility has spillover effects on other facilities in the same county. They use data from 2007-08 and compare quality provided by firms in a county where one firm had been enrolled in the SFF program to quality by firms in all other counties (excluding counties with only two firms, reducing the sample to 123 firms out of the 135 SFF firms in 2007). Castle et al. (2010) find little evidence of spillover effects of the SFF program. Of 22 quality outcomes, they find changes are significantly different in SFF counties on six; Urinary Tract Infections (UTIs) and pressure sores for high risk long-stay, low risk long-stay, and short stay residents improve disproportionately in SFF counties, however, total citations and quality of care citations both worsen (increase). As the authors note, the analysis is limited by only covering one year of SFF facilities. The estimates cannot be given a causal interpretation as quality in SFF-markets are likely to differ for reasons other than SFF assignment, a point the authors note.

States can have standards that are more strict than federal standards, or choose to interpret federal standards more stringently, thus providing another source of treatment and control groups. Bowblis and Lucas (2012) estimate the effects of different regulatory stringency and minimum staffing requirements across states and over time on survey deficiencies and facility-level deficiencies. Bowblis and Lucas (2012) find that because quality is multidimensional, improvements on one margin can come at the expense of deterioration on another. In particular, they find that higher direct care staffing requirements reduce the use of feeding tubes but increase the use of physical restraints.

Another strain of literature uses instrumental variable techniques to attempt to address endogeneity in regulatory stringency. Mukamel et al. (2012) instruments for statewide differences in regulatory stringency with area two of the Economic Freedom Index of North America of 2010; “Takings and Discriminatory Taxation,” and finds that stringency increase certain kinds of staff hours per resident (Certified Nurse Aides), but reduce others (Registered Nurses), leading to fewer pressure sores.

Miller and Mor (2008) review regulatory systems for long term care providers including nursing homes, assisted living facilities, home health agencies, and even daycare centers. They emphasize the tension between the regulator's role in policing standards versus that of consulting with providers to improve, and note that practices vary over time as well as both within and across states. Miller and Mor (2008) use Hurricane Katrina and publicly available data from St. Rita's Nursing Home in New Orleans as a case study of the possibility of using real-time data in identifying residents who were particularly vulnerable. They argue that the publicly available data showed residents had been virtually abandoned years prior to Katrina and that more aggressive oversight from regulators and state officials could have prevented the literal abandonment that followed, resulting in 34 resident drownings.[1]

Like the IOM report (Wunderlich and Kohler, 2001), Miller and Mor (2008) envision a "smarter" regulatory approach, in part based on the idea of more targeted enforcement. They note top performing providers deemed fully immersed in continuous quality improvement might instead be subject to state surveys every two to three years while providers that fail to make sufficient progress "might be required to undergo more frequent visits by state inspectors." They also favor more explicit incentives including "less/more frequent inspections, lower/higher fines and other penalties" to induce providers to improve quality, both "on their own" and through consulting Quality Improvement Organizations (QIOs).

[1] See Gruneir and Mor (2008) for a different perspective. Gruneir and Mor (2008) point out that the history of nursing home regulatory policy is characterized by a repeating cycle of public scandal, which garners attention until it is met by a regulatory crackdown. Notably, they write that "it may be the adversarial environment within which nursing homes operate that poses the largest barrier to quality improvement" and further voice broader concerns regarding the deterrence model as a channel for enacting quality improvement which they argue "pits the regulatory body against the industry and complicates the development of productive and responsive relationships between the two," which precludes more official involvement of other industry stakeholders such as CMS sponsored Quality Improvement Organizations (QIOs).

Hackmann (2019) builds a structural supply and demand model of the nursing home industry in Pennsylvania, estimates the parameters of this model, and simulates the effects of increased Medicaid reimbursement rates and competition. The model quantifies that residents value an additional skilled nurse at $133,000, which exceeds the annual costs of $83,000 (including wages and fringe benefits, both in 2002 dollars). He shows that staffing ratios are inefficiently low in 96% of nursing homes.[2] A simulation finds a 10% increase in the Medicaid reimbursement rate would increase nursing homes skilled nurse hours per resident by 8.7%. In comparison, a new public facility entering rural ma–kets - where gains from additional competition presumably are large as they tend to be served by a handful of facil–ties - would raise staffing ratios by less than 1%.[3]

Nursing Home Enforcement and the Special Focus Facility (SFF) Program

Nursing homes must provide care that meets federally imposed minimum care standards in order to receive Medicare and/or Medicaid payments. While most requirements are federal, enforcement is carried out separately by each state, typically through departments of health, which conducts surveys and certifies compliance or non-compliance.[4] Compliance with care standards is primarily enforced through unannounced inspections every 9-15 months, where an interdisciplinary survey team consisting of social workers, dietitians, pharmacists, rehab specialists, and at least one registered nurse (RN) investigate the home's care practices (CMS, 2018a).

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[2] While estimates are based on data from just one state, it is unlikely to drive results as Pennsylvania has a slightly higher Medicaid reimbursement rate than the national average.

[3] Hackmann's estimates also show that 45% of the increased reimbursements are kept as profits, while 55% are passed on to consumers through higher staffing ratios (or lower prices). Higher reimbursement rates also lead to a considerable market expansion effect which, in Pennsylvania, would increase the cost to taxpayers from 228 million (holding demand constant), to 331 million.

[4] Some states have additional (or more stringent) requirements than the federal government.

Examples of commonly violated standards, or deficiencies, include failure(s) to prevent avoidable accidents with adequate supervision or keep areas free from hazards (F-323), maintain an effective infection control program (F-441), or provide care that maintains the dignity and respect for each resident (F-241). All deficiencies are scored according to the severity of harm (or potential harm) posed to residents and the number of residents affected (or with potential to affect). The Social Security Act Section 1819I(2)(C) requires any nursing home that does not achieve substantial compliance within six months, defined as having a deficiency rated G or higher, to be terminated. The Scope & Severity Table is reproduced in Appendix to Chapter 1, Table A1.1.

The regulatory system for Nursing Homes was historically based on a more "informational and cooperative model" where surveys informed providers of failures to meet federal standards. The focus of these surveys was typically the physical environment and facility management. The Omnibus Budget Reconciliation Act of 1987 (Public Law 100–203) changed the focus to a deterrence model that uses penalties to deter firms from committing care failures (Harrington et al., 2004). This model remains in place today and is subject to ongoing debate among policy makers. To deter homes from going out of "substantial compliance" and encourage swift return for those that do, states recommend enforcement actions ranging from a directed plan of correction, Civil Money Penalties (CMPs), denial of payment for new admissions (DPNAs), or even termination of provider agreement (decertification) depending on the scope and severity of the deficiencies. CMS, who ultimately imposes the penalties, could in theory override the state's recommendation and impose harsher or softer penalties, but in practice this is rare.

The change to a deterrence model did not eliminate substandard quality care. Proponents argue this is due to states and CMS being overly lenient with enforcement. The average survey reveals about seven deficiencies, but 88% of all deficiencies are rated as posing "no actual harm, with potential for minimal harm", or "more than minimal harm that is not immediate jeopardy," which leads to a maximum possible fine of about $2000 (CMS, 2016). A common observation is that states frequently recommend the least stringent sanction available, and only after giving the facility an opportunity to correct. This opportunity allows the facility to correct the problem within a certain date, typically a couple months, without any penalty, and CMS rarely overrides a state's recommendation.

In the cases where fines are imposed, facilities automatically receive an offer of a 35% reduction if they accept the fine, further reducing potential deterrence effects. Therefore, it is not surprising that the threat of fines and penalties appear to have had limited deterrence effects if we consider a firm scenario where providing the required level of care is costly, and where competitive pressures alone might be insufficient to induce the firm to provide the minimum quality level.

In 2016, nursing home expenditures totaled 170 billion across 15,500 facilities, which amounts to about 5% of U.S. health care spending (Hackmann, 2019). A typical home generates about $10,000,000 in annual revenues and pays $0 fines.[5] In comparison, CMS has in recent years collected between 40 and 80 million annually from CMPs, with the largest penalty levied being approximately 1.25 million. Most facilities compete in narrowly defined local markets as the median resident chose a facility within four miles of her former residence (Hackmann, 2019). These institutional details suggest and empirical evidence supports the notion that, for a

[5] I abstract away from Payment Denials as they are more difficult to quantify.

fraction of nursing homes, the rational profit maximizing strategy (absent regulatory interventions like the SFF program) is to provide care that does not always meet the minimum standards laid out in Chapter 7 of the State Operations Manual (CMS, 2018a). Facilities would rationally expect that not all deficiencies will be discovered, and for those that are, facilities will be given opportunities to correct. Further discounts means that the expected sum of $P_{detection}$ x *Penalty* is not sufficient for the profit maximizing strategy to be to hire more staff or invest in expensive equipment.

This is what has been observed in practice. A portion of homes consistently fail standard surveys and improve just enough for their correction dates that the scope and severity get below G, which lets the facility stay certified, only for subsequent inspections to find more deficiencies, often in the same category. While the deficiency need not be from the exact same care failure, it remains the case that repeated deficiencies in the same category rated G or higher, suggests there are underlying systemic problems that have not been addressed. The SFF program was established in 1998 as part of the Nursing Home Oversight and Improvement Program with the goal of changing the incentives for facilities with persistent care failures, essentially raising the probability of detecting care failures and the penalties if failures are detected (CMS, 2004). This is put in place by inspecting facilities twice as often.
The program requires the state to enforce progressively strict penalties.[6]

Homes "graduate" from the program by showing substantial compliance on two consecutive inspections, which they are expected to do within 18-24 months. If a facility does not achieve this it will be notified that the next inspection will be its last chance to achieve substantial compliance, or it will be subject to termination. SFF slots open up when homes

[6] For more details about this progressive enforcement, see Figure 13 in the Appendix.

graduate or are terminated, at which point, the state is required to select a new facility from the candidate list within 21 days (CMS, 2017b). I argue these procedures create quasi-random variation in both which firms end up treated, and when treatment occurs.

The program currently targets 88 nursing homes, with a fixed number of program slots for each state ranging from one (29 states) to six in California and Texas.[7] CMS ranks facilities within each state based on a weighted average of its three most recent years of inspection scores, and designates the lowest ranking firms in each state as candidates for the program.[8] Each SFF slot has five candidate slots, so in a state with two SFF slots, the ten lowest ranked facilities are automatically candidates.

The size of the program and the distribution of entry and exit periods are shown in Figures 1.1-1.3. A feature of the program's design, continually enrolling, graduating, and terminating facilities, means that treatment periods are spread across time and facilities such that no particular periods can be driving the results. The size and geographic distribution of the SFF program has changed at various points, largely due budgetary considerations.

The number of program slots was 135 from February 2005 until October 2010, when this was raised to 167 (CMS, 2004; Casey and Toomey, 2019). The program was reduced to 48 in April 2013 when the Balanced Budget and Emergency Deficit Control Act, commonly known as the "sequester," went into effect. In May 2014 the program was increased to its current size of 88 facilities (CMS, 2004, 2013; Hamilton, 2014). Treated facilities have typically served a quarterly flow of between 5-15,000 residents over this period.

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[7] See the appendix Figure 12 for the program's current geographic distribution.
[8] The most recent year is weighted 50%, last year 33%, and two years prior 17%.

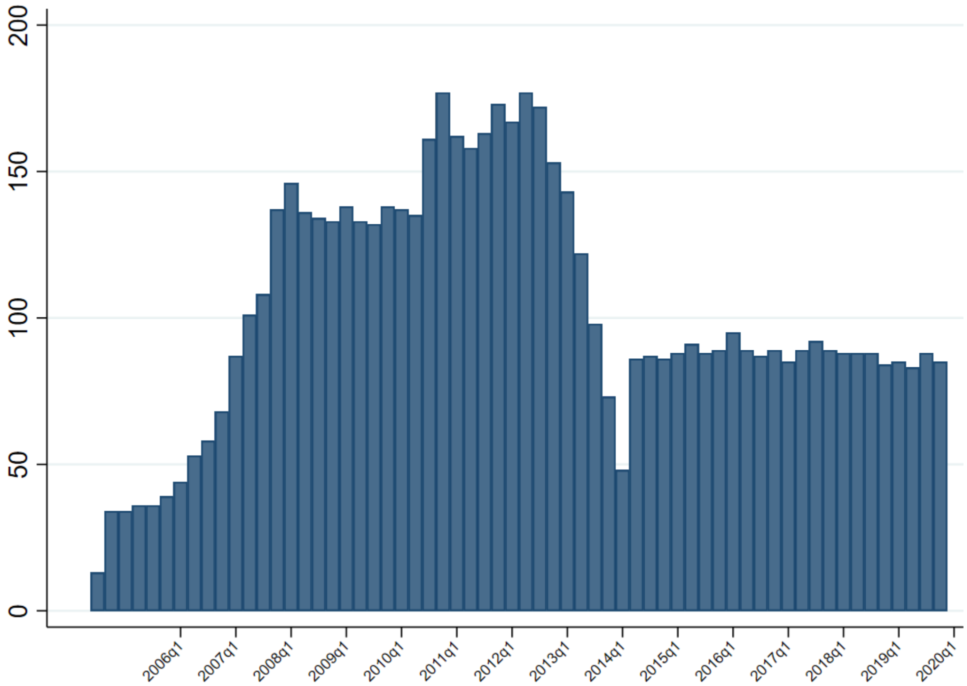
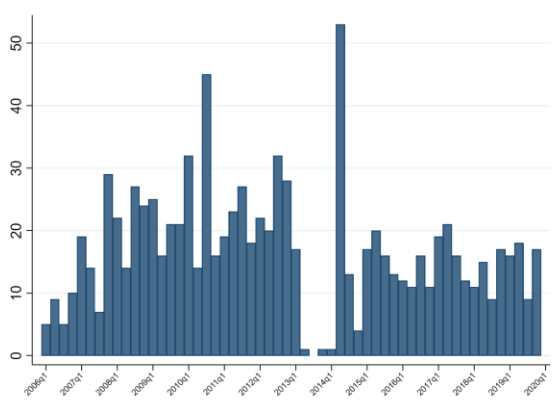


Figure 1.1. SFF Program Size



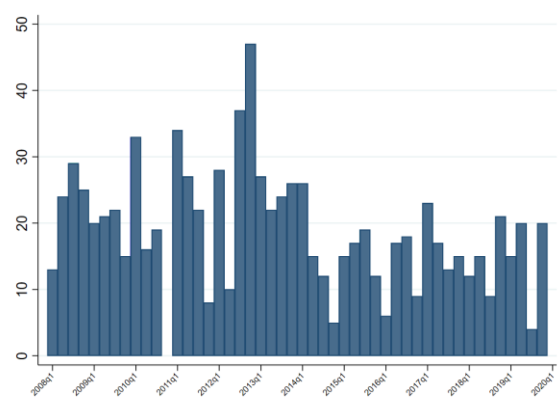


Figure 1.2. Enrollments **Figure 1.3. Exits**

Note: Figure 1.1 shows the size of the SFF program from 2008 through 2019. The number of slots grew from 135 to 151 in September 2010 and was reduced to 48 in March 2013 when budget cuts in the Balanced Budget and Emergency Deficit Control Act of 2013 (Sequester) went into effect. The number of slots was increased to 88 in May 2014, which remains in place today. Figures 1.2-1.3 show the quarterly number of entries and exits.

Data and Descriptive Statistics

Nursing Home Compare Firm and Quality Data

My primary data source is Nursing Home Compare (NHC), a website maintained by the Center for Medicare and Medicaid Services (CMS) that provides a comprehensive way to compare nursing homes on dimensions such as care practices, clinical outcomes, inspection results, and firm characteristics.[9] I investigate whether the SFF program benefits patients by looking at inspection results from 2004 through 2019.

Special Focus Facility Data

The SFF program was started in 1998, but CMS did not not make public which firms were enrolled until February 2008. CMS started releasing the current candidate facilities in June 2019 (Hamilton, 2008). I obtained monthly candidate lists from November 2010 to July 2019 through a Freedom of Information Act Request. During this period I observe close to 850 facilities that have been targeted by the program and about 3,000 facilities that have been candidates.[10]

Deficiencies and Inspection Scores

The primary way I measure the care provided is through the survey outcomes homes receive from standard health inspections. The inspection process and requirements for

[9] Because facilities receive about 70% of their revenues from Medicaid and/or Medicare payments, it is extremely rare for facilities to operate outside of these programs. As a result, we observe the universe of almost 16,000 nursing homes operating in the U.S.

[10] One potential concern is that facilities can receive new National Provider Identifiers (NPIs) during changes to certification status, ownership, or other legal changes. At best, this would make longitudinal analysis using NPIs noisy. But if, for instance, below average firms strategically shut down and re-open, this could bias the analysis. To overcome this potential problem, I utilize a facility-identifier crosswalk produced by the Long Term Care Focus group at Brown University, which allows continued tracking of facilities from 2000-2017. I use address and geographic information to link facilities that received new identifiers in 2018-19 (LTC Focus, 2017). This does not appear to be a significant driver; only ten facilities appear to have been treated under different NPIs.

participation are standardized and have only undergone minor changes between 1995 and 2019, making longitudinal analysis of nationwide comprehensive results available for each certified facility.[11] While surveyors might not catch all deficiencies, deficiencies are generally found to be a representative floor of care failures, because facilities have the opportunity to contest deficiencies, thus providing an incentive to eliminate frivolous deficiencies.

Surveyors review the home's compliance with minimum care standards and issue deficiencies when standards are not met. Each standard, referred to as F-Tags, are assigned a score ranging from 0 to 175 points according to the scope and severity of the violation with more points indicating more residents affected or a more severe potential for harm. This scoring system, commonly referred to as the Scope/Severity Table, is reproduced in the Appendix to Chapter 1 as Table A1.1. The most natural way to evaluate performance is to analyze the overall deficiencies and scores received during each inspection.

Deficiencies are reported with the exact date of issuance, while most other data sources, including the home's SFF status, are generally available on a quarterly frequency. To most accurately capture both short- and long-term impacts of the program, I aggregate the survey scores to the quarter in which they were issued. This means that on average homes will only have one quarter with a new survey score each year, and will have missing values for the other quarters. Second, in the rare event that a home has a perfect survey, the deficiency score could be missing rather than 0, thus overstating the average scores. This is unlikely to be of concern for two reasons. Facilities receive on average 6-7 deficiencies and a score between 40 and 50,

[11] CMS issued a comprehensive overhaul of survey and participation requirements in 2016 to be implemented in three phases; the first phase was implemented in November, 2016 but added only minor changes (CMS, 2016). Phase 2, implemented in November, 2017, updated the survey process itself including renumbering and reclassifying the F-Tags, and made the survey itself computer-based, etc. CMS (2017a) I believe these changes are small enough that they have little impact even when inspections occur across survey methods.

while homes enrolled in the SFF program on average receive twice as many deficiencies and scores that are more than twice as high. Earlier work has shown that about 10% of facilities, the very best in the industry, receive perfect scores Harrington et al. (2000). It is therefore unlikely that facilities that were ever associated with the SFF program would receive a perfect score.

Effects of Targeted Oversight on Nursing Home Quality

To evaluate the effects of targeted enforcement on care standards and health outcomes, I estimate a series of generalized difference-in-difference models. The treatment starts at different times for different facilities and also has variable duration as facilities take different numbers of inspections to meet (or fail to meet) graduation requirements. To see this graphically, see Figures 1.1-1.3. I combine this difference-in-difference framework with the Coarsened Exact Matching (CEM) technique developed by Iacus et al. (2012). This identification strategy has been used when there is (a) variation in treatment timing, (b), a limited number of untreated units that can serve as plausible controls, and (c), the researcher wants to allow some control units to serve as controls for multiple treated units. Similar identification and estimation strategies have been used by Jeon and Pohl (2017) and Rellstab et al. (2019).

Sample Selection of Treatment and Control Cohorts

I start with the problem state policy makers face when a Nursing Home has left the SFF program either due to meeting the “graduation” requirements or due to termination. The state policy maker must now enroll a new facility in no more than 21 calendar days, and must choose this facility from the most recent candidate list provided by CMS (CMS, 2017b).[12] This event occurs in 608 unique “state-quarters” from 2011Q1-2019Q3.

[12] For more information about how this is carried out in practice, see Figure 14 in the Appendix for a “Model Letter” informing newly selected facilities of their enrollment.

I group each treated facility into cohorts based on the state-quarter of the “enrollment-event” and let the pool of facilities eligible for the corresponding control group be those that were on the candidate list during the time of the event, but that were not chosen. I restrict the control groups to facilities that entered no more than two quarters before or after the treated facilities. To make the samples more homogeneous, I also restrict the control groups facilities that were not treated in the past two years. Note that a small minority of facilities can fit the criteria for multiple cohorts, for instance if a state has multiple consecutive quarters with enrollment-events. When this occurs the facility is duplicated (and given an additional identifier), which means the analysis includes 608 cohorts where each cohort is an unbalanced panel of a total of 622 treated and 2,720 untreated facilities.

Difference-in-Differences

To estimate a difference-in-differences model, I define an event-time indicator *k*, which takes a value of the number of years the facility is away from an enrollment-event q^k, and interact this with an indicator, D, that is 1 for the facilities that will end up treated. However, when treated facilities leave the program, D is coded 0.[13] The coefficient on βD^k can therefore be interpreted as the difference-in-differences, *k* years relative to the treatment-event, except for k = 4 which bin together inspections that happen four or more years after enrollment for the minority of facilities that stay in the program for extended periods. Because facilities are selected based on three years of compliance history and typically graduate from the program in 12-24 months, we are most interested in the estimated β’s forI= −4, ... , k = 2. The main estimating equation is

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[13] This is done to prevent $\beta^{k=1,\dots,k=4}$ to be confounded by treatment reversals, which are estimated in a separate section.

$$Y_{it} = \sum_{k=-15}^{4} \gamma^k q_{it}^k + \sum_{k=-15}^{4} \beta^k D_{it} q_{it}^k + [\alpha_i + \lambda_t + \delta_{st}] + \epsilon_{it} \quad (1)$$

where the first summation term captures the relative inspection-years for all facilities, while the second summation term is the one of interest and captures the effect of treatment *k* inspection-years relative to treatment assignment. The bracketed terms are used in various combinations, and include fixed effects for each facility, α_i, quarter, λ_t, as well as state-by-year indicators, δ_{st}. Equation (1) is estimated using the within transformation and ordinary least squares.[14]

Facility indicators are used to absorb differences across facilities that are constant over time, state-by-year indicators to absorb statewide changes in enforcement stringency, and quarterly time indicators to absorb nationwide shocks that are common to all facilities.
My preferred specifications do not control for the facility's occupancy rate or staff per resident, due to the concern that these are endogenous. The standard errors are clustered on the facility level to account for within-facility serial correlation, but note that alternative clustering strategies such as by cohort had little impact.

For β^k to have a causal interpretation in (1), it has to be plausible that quality among firms *not chosen* for the program, are an unbiased approximation of what would have happened to quality among chosen firms, had they not been chosen — the standard parallel trends assumption. This assumption is fundamentally untestable, but I follow common practice and test whether the path of quality evolved similarly prior to enrollment by inspecting the β^{k}'s prior to treatment.

[14] Note that some facilities serve as control units in multiple cohorts. In order to construct the relative-time variables these are duplicated and given "temporary" identifiers.

Figures 1.4 and 1.6 plot the average outcomes for treated and control facilities. Figures 1.5 and 1.7 are weighted based on the Coarsened Exact Matching (CEM) procedure described below. The main conclusion is that candidates who end up treated follow similar paths as those who don't from seven years before until to two years before treatment. Some candidates who end up treated have especially pronounced deterioration just prior to treatment, but this is driven by a small number of outliers.

I apply the Coarsened Exact Matching (CEM) technique put forth by (Iacus et al., 2012) to make the treatment and control groups even more homogeneous. CEM applies an exact-matching algorithm that temporarily coarsens pre-treatment variables provided by the user, and assigns units to different strata according to each possible combination of these (coarsened) pre-treatment variables. Within each strata, CEM calculates weights that balance the empirical distribution of each strata, where treated units receive weights = 1. Treated or untreated units that don't meet the common support of any strata are given weights = 0.

I match facilities based on the outcomes at $k = -3$, $k = -2$ and $k = -1$. This drops a small number of facilities, depending on which outcome is examined. For the regressions on deficiencies (scores) 24 treated (14) and 625 (188) control units are dropped because they were outside the common support area. Summary statistics from the last period before treatment are shown in Table 1.1, which shows that treated and control facilities are comparable in terms of pre-treatment outcomes, patient mix, and firm characteristics even before matching. It is further clear that the matching procedure is able to find units that are even more comparable on pre-treatment outcomes, without distorting the balance on other variables that could influence care quality.

Table 1.1. Descriptive Statistics Last Inspection Before Treatment

	Control mean	Treated mean	Matched Control mean	Matched Treated mean
Health Score	146.2	184.6	182.7	181.4
Deficiencies (#)	12.6	14.3	13.6	14.3
Age	76.7	76.4	76.6	76.3
Black (%)	23.1	22.3	24.0	22.0
Hispanic (%)	6.28	5.29	6.55	5.31
Female (%)	63.5	63.2	63.5	63.1
Acuity Index NCMI (0-4)	1.14	1.14	1.14	1.14
Medicaid (%)	67.7	69.2	67.9	69.2
Medicare (%)	12.1	12.3	12.1	12.3
For profit (%)	0.81	0.85	0.83	0.85
Residents (#)	97.1	95.3	98.1	95.0
Beds (#)	121.0	123.1	122.2	123.1
Rehospitalization Rate	0.20	0.20	0.20	0.20
Successful Discharge Rate	0.45	0.46	0.44	0.46

Note: This table compares treated and control units on inspection results, resident, and facility characteristics the last period before treatment.

I then plot the average outcomes by treatment status, where the plots on the right are weighted. The CEM-weights make treated and control units follow each other even more closely at $k = -3$ $k = -2$ and $k = -1$, which we expected since these were used for matching. However, it is reassuring that this procedure does not seem to have come at the expense of fundamentally changing the paths from $k = -7$ to $k = -4$. This reinforces the premise of the research design that there are facilities for which treatment appears conditionally random. Following common practice we can then test for pre-treatment differences in trends by inspecting the eslated $\beta^{-7},\ldots,\beta^{-1}$'s and 95% confidence intervals from estimating (1) while applying the discussed CEM-weights.

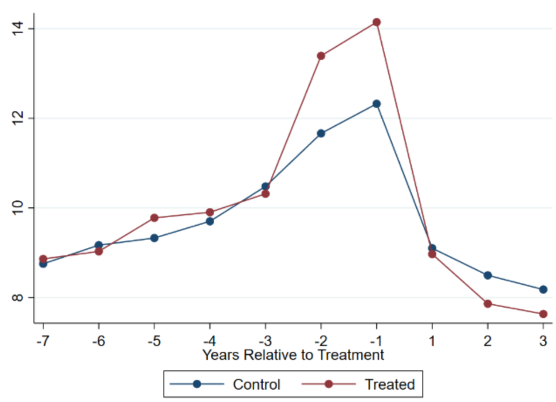

Figure 1.4. Health Deficiencies

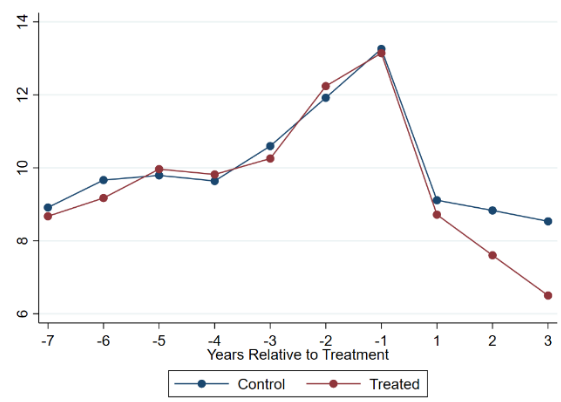

Figure 1.5. Health Deficiencies CEM-Weighted

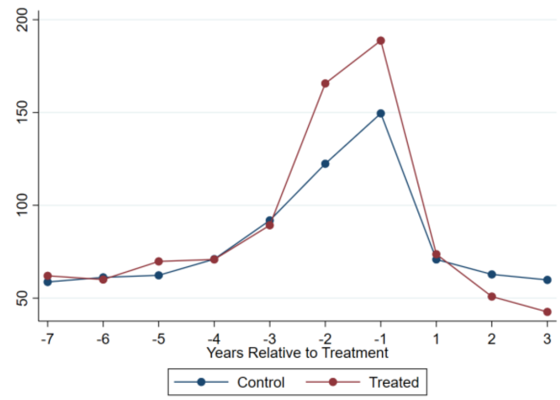

Figure 1.7. Health Score

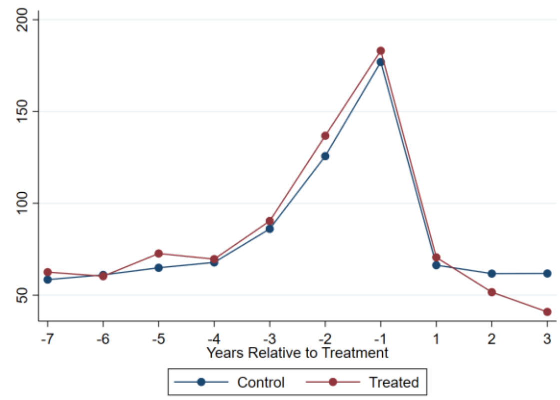

Figure 1.6. Health Score CEM-Weighted

Note: These figures plot the average outcomes for treated (red) and untreated (blue) facilities. Figures on the left are raw averages, while figures on the right are weighted by the weights from the CEM matching procedure. A lower score or fewer number of deficiencies represents a better survey result.

Effects of Enforcement on Deficiencies and Inspection Scores

Figures 1.8 and 1.9 plot the θ^ks from (1) on the health survey score for four health surveys prior to, and three following, the enrollment event showing two clear patterns. First, treated and control facilities are on similar paths leading up the (placebo) treatment. The parallel trend assumption appears to be credible, as both treated and untreated facilities experience a similar large performance swing leading up to treatment.[15] The second pattern is that treated facilities improve survey results more than untreated candidates.

Some researchers have cautioned against matching on pre-treatment outcomes while simultaneously using unit fixed effects to difference out permanent differences Chab´e-Ferret

[15] As a rough measure of this, the coefficients on γ^k, which captures the shared event-time between treated and untreated facilities, are typically 10 times larger during the periods leading up treatment compared to the θ^ks.

(2017). I therefore include various specifications in Tables 1.2 and 1.3, and pay most attention to the results in columns 4 and 5, which include the CEM weights.

The estimated treatment effect indicates that treatment caused an additional improvement of approximately 1-2 deficiencies, or an additional 7-20%, for facilities enrolled 1-3 years after treatment began, compared to untreated candidates. The difference is typically statistically significant by the third year, i.e., it appears to get larger the longer facilities stay in the program. This is surprising because facilities that remain in the program three years after enrollment are those that have not yet been able to graduate, and thus one might think this selection would lead the coefficient to shrink.

The estimates on the overall health score are somewhat noisy, but tell a similar story. Treated facilities receive on average 27 fewer survey points (S.E. = 15.1) three years after treatment, or an additional improvement of about 15%.

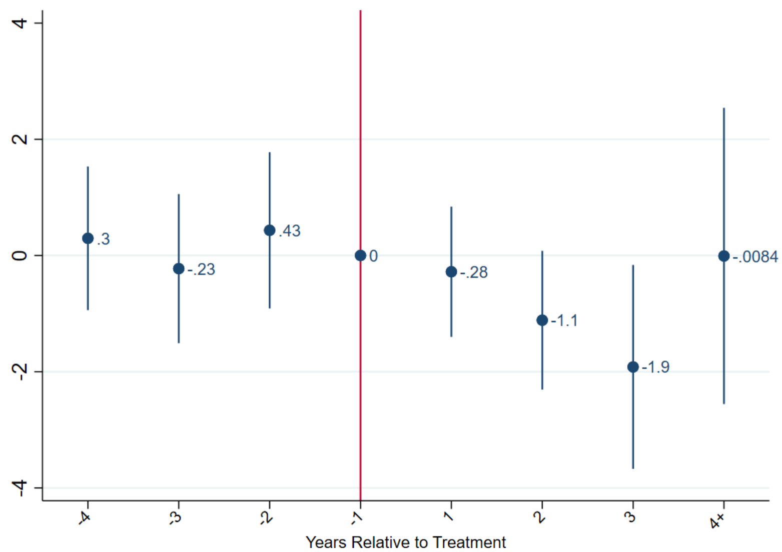

Figure 1.8. Treatment Effect on Health Deficiencies

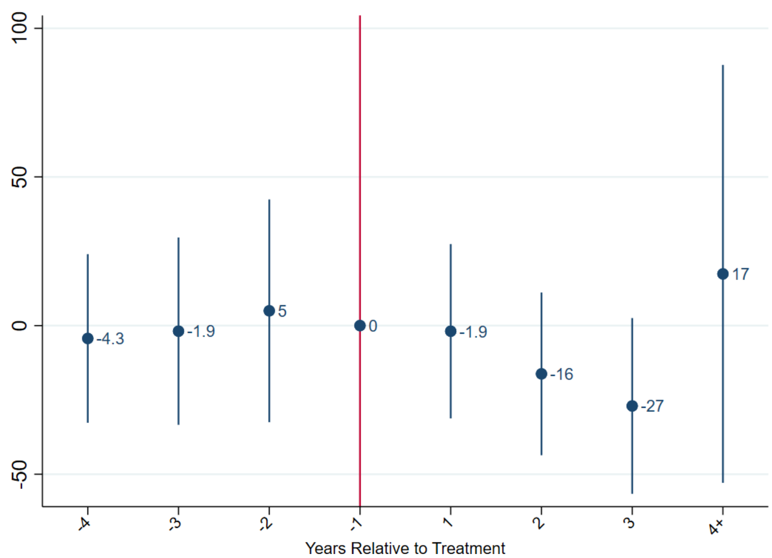

Figure 1.9. Treatment Effect on Health Scores

Note: These figures plot event-study difference-in-difference coefficients on health survey scores and deficiencies from (1). A lower score/number of deficiencies represents a better survey result.

Table 1.2. Main Results for SFF Program on Deficiencies and Health Scores

	(1) Deficiencies b/se	(2) Deficiencies b/se	(3) Deficiencies b/se	(4) Deficiencies b/se	(5) Deficiencies b/se
k=-4	-0.21	0.34	0.24	0.30	0.24
	0.40	0.41	0.40	0.63	0.62
k=-3	0.15	0.34	0.25	-0.23	-0.12
	0.42	0.42	0.40	0.65	0.65
k=-2	1.15**	1.16**	1.09**	0.43	0.31
	0.43	0.43	0.42	0.69	0.68
k=-1	0	0	0	0	0
	.	.	.	.	.
k=1	-1.27***	-0.96**	-1.02**	-0.28	-0.59
	0.34	0.34	0.33	0.57	0.55
k=2	-2.20***	-1.62***	-1.82***	-1.11	-1.97**
	0.39	0.39	0.39	0.61	0.60
k=3	-3.38***	-2.70***	-3.17***	-1.92*	-3.99***
	0.65	0.66	0.65	0.89	0.94
k=4+	-2.38**	-1.40	-1.13	-0.0084	-1.75
	0.82	0.81	0.73	1.30	1.26
Facility	Yes	Yes	Yes	No	Yes
CEM	No	No	No	Yes	Yes
Quarterly	No	Yes	No	No	No
State x Year	No	No	Yes	No	No
Mean of Y	9.35	9.35	9.35	9.72	9.72
N	63759	63759	63752	32310	32309

Note: Standard errors are clustered on the facility level. * 0.05 ** 0.01 *** 0.001

Table 1.3. Main Results for SFF Program on Deficiencies and Health Scores

	(1) Health Score b/se	(2) Health Score b/se	(3) Health Score b/se	(4) Health Score b/se	(5) Health Score b/se
k=-4	-14.6	-8.8	-10.9	-4.3	-3.3
	9.8	9.8	9.7	14.5	14.4
k=-3	-12.3	-9.5	-11.9	-1.9	-1.3
	10.5	10.6	10.4	16.1	16.1
k=-2	26.4*	25.7*	23.7	5.0	2.6
	12.7	12.7	12.6	19.1	19.1
k=-1	0.0	0.0	0.0	0.0	0.0
	.	.	.	.	.
k=1	-25.2**	-20.9*	-23.0*	-1.9	-3.8
	9.1	9.2	9.1	15.0	14.9
k=2	-36.0***	-27.6**	-31.7***	-16.2	-22.3
	8.8	8.9	9.1	14.0	13.9
k=3	-52.5***	-43.1***	-49.1***	-27.0	-46.2**
	9.9	10.2	10.5	15.1	15.6
k=4+	-29.8*	-22.5	-18.8	17.4	2.7
	14.9	14.8	13.4	35.8	40.8
Facility	Yes	Yes	Yes	No	Yes
CEM	No	No	No	Yes	Yes
Quarterly	No	Yes	No	No	No
State x Year	No	No	Yes	No	No
Mean of Y	75.1	75.1	75.1	78.6	78.6
N	63759.0	63759.0	63752.0	37729.0	37728.0

Note: Standard errors are clustered on the facility level. * 0.05 ** 0.01 *** 0.001

Treatment Reversals

The results discussed above indicate that the SFF program causes targeted facilities to improve certification results more than untreated candidates, but it remains possible that targeted facilities would have improved more, irrespective of the SFF program. Given the regression specification in (1), this could only be due to some time-invariant factor that starts affecting targeted facilities (more) just as the facility becomes targeted. This could happen if treated facilities, who before the matching procedure receive slightly worse survey results, decide on their own and irrespective of the SFF program to improve care standards. While the goal of the matching procedure was to eliminate this possibility, it remains possible that this was not achieved.

This and other alternative explanations predict that facilities will be unaffected by treatment ending. I test this by estimating

$$Y_{it} = \tau_{it} Reversal + \alpha_i + [\kappa_c + \lambda_t + \delta_{st}] + \sum_{k=-17}^{3} \gamma^k q^k_{it} + \sum_{k=-17}^{3} \beta^k D_i q^k_{it} + \varepsilon_{it} \qquad (2)$$

where every variable is the same as before but where I now include an indicator, Reversal, that turns on for treated facilities when treatment reverses, and is zero otherwise. I code Reversal to zero for untreated facilities so that the fixed effects still control for previously mentioned shocks. I no longer code the relative time as missing after treatment reverses. The results from (2) are found below in Tables 1.4-1.5. I also estimate a version of (2) where Reversal is coded as years from graduation events, which is plotted in Figures 1.10-1.11. Both specifications indicate that treatment reversals are followed by significant performance deterioration that are of comparable size to the preceding two years of differential improvements.

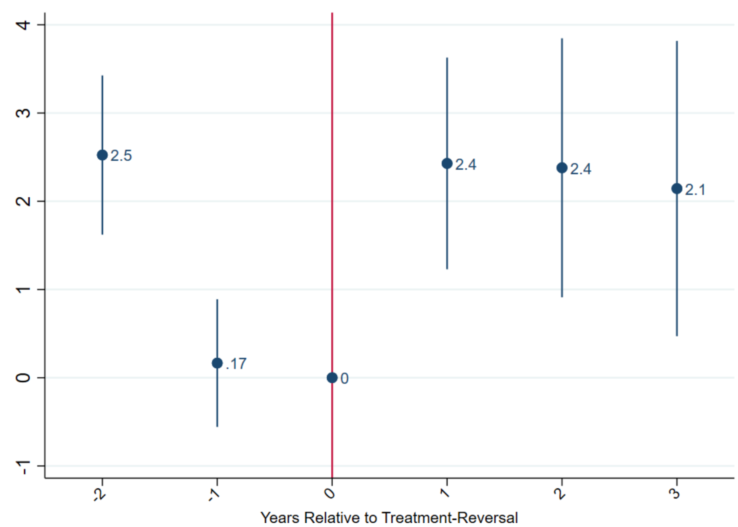

Figure 1.10. Treatment Reversal Effect on Deficiencies

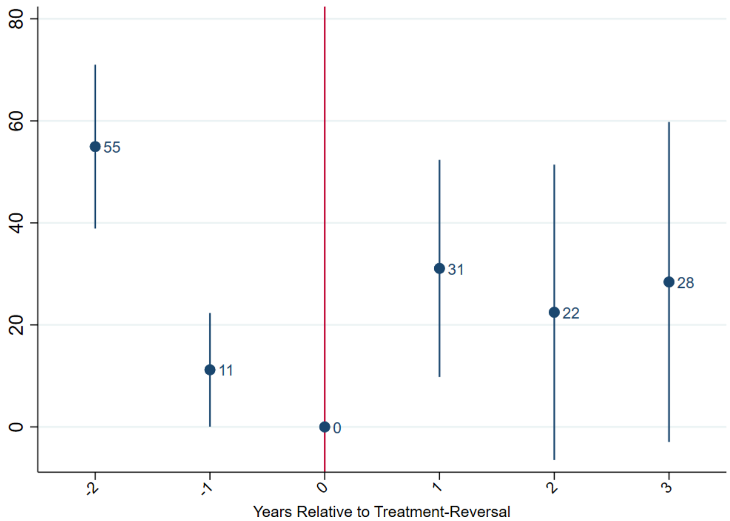

Figure 1.11. Treatment Reversal Effect on Health Score

Note: Figure above plot event-study coefficients from regressing health survey deficiencies and scores on treatment reversals (2). A lower number represents a better survey result.

Table 1.4. Effect of Treatment Reversal on Health Scores

	(1) Health Score	(2) Health Score	(3) Health Score	(4) Health Score	(5) Health Score
Treatment Reversal	21.66*	20.49*	23.03**	22.47**	21.69**
	(11.31)	(11.56)	(11.58)	(10.76)	(10.95)
Facility	Yes	Yes	Yes	Yes	Yes
Quarterly	No	Yes	Yes	No	Yes
Cohort	No	No	Yes	No	No
State x Year	No	No	No	Yes	Yes
N	52166	52166	52166	52157	52157
Mean of Dep. Variable	74.50	74.50	74.50	74.51	74.51

Standard errors in parentheses
$* p < 0.10$, $^{**} p < 0.05$, $^{***} p < 0.01$

Table 1.5. Effect of Treatment Reversal on Deficiencies

	(1) Deficiencies	(2) Deficiencies	(3) Deficiencies	(4) Deficiencies	(5) Deficiencies
Treatment Reversal	2.291***	2.122***	2.243***	2.385***	2.390***
	(0.633)	(0.646)	(0.654)	(0.599)	(0.600)
Facility	Yes	Yes	Yes	Yes	Yes
Quarterly	No	Yes	Yes	No	Yes
Cohort	No	No	Yes	No	No
State x Year	No	No	No	Yes	Yes
N	52166	52166	52166	52157	52157
Mean of Dep. Variable	9.497	9.497	9.497	9.497	9.497

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Discussion

This paper investigated whether a program that provides targeted enforcement of under-performing nursing homes causes the homes to improve care practices and ultimately the clinical outcomes of their residents. I find multiple sources of evidence consistent with this hypothesis. Facilities that get additional oversight improve more than comparable facilities that don't. Facilities that receive *more* treatment appear to improve more, and finally, improvements revert after treatment ends, a set of findings that are hard to reconcile with explanations other than oversight being the cause.

A recent policy proposal has called for a major expansion of the program from about 0.5% to 5% of the industry. While this paper did not formally test whether the treatment effect vary with changes in the size of the program, it is plausible that expanding the program to more facilities, at least over some range, would benefit the residents of those facilities. The results on treatment reversals suggests policy makers consider extending the length of the program, rather than just the size.

An important caveat exists in that the evidence produced here comes primarily from facilities targeted by the program that continued operating. This is around 90% of facilities. This paper cannot answer whether the residents of facilities that were terminated, as many as 10% of facilities, benefitted. But it is worth noting that given the substantial costs of moving, care quality would likely have to be dramatically better for this group to come out on top. This could be explored with resident-level data in future research.

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Appendix to Chapter 1

Table A1.1. Health Inspection Score: Weights for Different Types of Deficiencies

Table 1
Health Inspection Score: Weights for Different Types of Deficiencies

Severity	Scope		
	Isolated	**Pattern**	**Widespread**
Immediate jeopardy to resident health or safety	**J** 50 points* (75 points)	**K** 100 points* (125 points)	**L** 150 points* (175 points)
Actual harm that is not immediate jeopardy	**G** 20 points	**H** 35 points (40 points)	**I** 45 points (50 points)
No actual harm with potential for more than minimal harm that is not immediate jeopardy	**D** 4 points	**E** 8 points	**F** 16 points (20 points)
No actual harm with potential for minimal harm	**A** 0 point	**B** 0 points	**C** 0 points

Note: Figures in parentheses indicate points for deficiencies that are for substandard quality of care.

Shaded cells denote deficiency scope/severity levels that constitute substandard quality of care. See the Electronic Code of Federal Regulations (https://www.ecfr.gov/cgi-bin/text-idx?SID=9c4d022241818fef427dc79565aba4b5&mc=true&node=pt42.5.488&rgn=div5#se42.5.488_1301) for a definition of substandard quality of care.

* If the status of the deficiency is "past non-compliance" and the severity is Immediate Jeopardy, then points associated with a 'G-level' deficiency (i.e., 20 points) are assigned.

Source: Centers for Medicare & Medicaid Services

The number of SFF slots and candidates list for each State (effective May 1, 2014).

State	Required SFF Slots	Size of Candidate List	State	Required SFF Slots	Size of Candidate List
Alabama	1	5	Montana	1	5
Alaska	-	-	Nebraska	1	5
Arizona	1	5	Nevada	1	5
Arkansas	1	5	New Hampshire	1	5
California	6	30	New Jersey	2	10
Colorado	1	5	New Mexico	1	5
Connecticut	1	5	New York	3	15
Delaware	1	5	North Carolina	2	10
District of Columbia	-	-	North Dakota	1	5
Florida	3	15	Ohio	5	20
Georgia	2	10	Oklahoma	2	10
Hawaii	1	5	Oregon	1	5
Idaho	1	5	Pennsylvania	4	20
Illinois	4	20	Rhode Island	1	5
Indiana	3	15	South Carolina	1	5
Iowa	2	10	South Dakota	1	5
Kansas	2	10	Tennessee	2	10
Kentucky	1	5	Texas	6	30
Louisiana	1	5	Utah	1	5
Maine	1	5	Vermont	1	5
Maryland	1	5	Virginia	1	5
Massachusetts	2	10	Washington	1	5
Michigan	2	10	West Virginia	1	5
Minnesota	2	10	Wisconsin	2	10
Mississippi	1	5	Wyoming	1	5
Missouri	3	15	Total	88	435

Figure A1.1. Program Distribution

Progressive Enforcement Table

Surveys After SFF Selection	No Deficiencies cited at a Scope & Severity of "F" or Greater	Deficiencies at "F" or above (no improvement)	Immediate Jeopardy
1st Standard Survey	Complete 2nd Standard Survey	Immediately recommend remedy (CMP or DPNA at a minimum)	Recommend remedy and proceed to termination if not corrected.
2nd Standard Survey	Graduate (if 2 surveys with no deficiencies above "E")	Recommend more stringent remedy. Must be in substantial compliance at 6 months or face termination.	Recommend remedy and proceed to termination if not corrected.
3rd Standard Survey	If a facility has deficiencies at E or below on the 3rd Standard Survey after selection (but is not able to graduate due to findings at F or above on 2nd Standard Survey or LSC deficiencies greater than F), Schedule 4th Standard Survey.	If a facility has deficiencies at G or above at the 3rd Standard Survey, Triage- (1) Schedule a 4th standard survey or (2) Issue a termination notice	Recommend remedy and proceed to termination if not corrected.
4th Standard Survey	Graduate (if 2 consecutive surveys with no deficiencies above "E")	Triage - either (1) schedule 5th standard survey, or (2) issue a termination notice	Recommend remedy and proceed to termination if not corrected.
5th Standard Survey	Graduate (if 2 consecutive surveys with no deficiencies above "E")	Issue termination notice (timing may be extended but not beyond statutory timeframes).	Recommend remedy and proceed to termination if not corrected.

Figure A1.2. Progressive Enforcement

Appendix A

MODEL LETTER TO PROVIDER SELECTED AS A "SPECIAL FOCUS FACILITY"

IMPORTANT NOTICE – PLEASE READ CAREFULLY

(Date)

Nursing Home Administrator Name
Facility Name
Address
City, State, ZIP Code

Dear (Nursing Home Administrator)

Because of your facility's poor compliance history for the past three years, you have been selected as a Special Focus Facility (SFF) program. The purpose of this letter is to notify you of this designation and to explain what this designation means for your nursing home.

What Does This Mean?
You will be subject to two standard surveys per year instead of the one required by law. You can expect that we will be closely monitoring your facility with the desire that your facility can attain and maintain compliance.

How Does A Facility Get Removed From the SFF?
A nursing home may be removed from the SFF program when it demonstrates at two standard surveys that it has no deficiencies cited at a scope and severity level of "F" or greater and no intervening complaint-related cited at "F" or greater. A nursing home may also be removed through a termination action if it fails to make significant improvements in the 24 months (3 standard surveys) following its selection as a SFF.

Robust Enforcement for Lack of Significant Progress: CMS will impose an immediate sanction on a SFF that fails to achieve and maintain significant progress in correcting deficiencies on the first and each subsequent standard survey after a facility becomes a SFF. Enforcement sanctions will be of increasing severity. These will include a Civil Money Penalty and/or a Denial of Payment for New Admissions.

If, after 24 months and four surveys subsequent to being selected as a SFF, you fail to have made significant progress, a notice of termination from participation in Medicare and Medicaid will be issued. CMS will consider a facility's status and progress as a SFF in setting a reasonable assurance period before a home can reapply to participate in Medicare.

Can This Be Appealed?
Your selection as a SFF cannot be appealed. However, you still have the right to informal dispute resolution (see 42 Code of Federal Regulations §488.331) and to appeal the

Figure A1.3. Selection Letter Page 1

noncompliance that led to a remedy through an Administrative Law Judge of the Department of Health and Human Services. Specific requirements for requesting a formal hearing are contained in the notice of the imposition of the remedy.

It is our intent that you take the designation of a special focus facility seriously. We can help. We can refer you to helpful resources, including help from the (Name of State Quality Improvement Organization).

We are also sending a copy of this notice to (name of nursing home owner) and (name of mortgagee) to give them notice of the designation of SFF for your facility.

If you have any questions, please contact (name, title, address, phone number, fax number and e-mail address of appropriate survey agency official.)

Sincerely yours,

(Name and Title)

Cc: CMS Regional Office
(Name of Quality Improvement Organization)
(Name of Owner)
(Name of Mortgagee, if applicable)

Figure A1.4. Selection Letter Page 2

Chapter 2. COVID in the Nursing Homes: The US Experience

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Abstract

The death toll in nursing homes accounted for almost 30% of total COVID-19 deaths in the U.S. during 2020. We examine the course of the pandemic in nursing homes focusing especially on whether nursing homes could have been better shielded. Across all nursing homes the key predictor of infections and deaths was community spread, i.e., a factor outside of the control of nursing homes. We find that higher quality nursing homes, as measured by the CMS Five-Star Rating system, were not better able to protect their residents. Policy failures from the CDC and FDA, especially in the early stages of the pandemic, created extended wait times for COVID-19 tests which hampered attempts to isolate infectious residents and allowed outbreaks to grow larger and deadlier. But testing would have had to have been much greater to have had an appreciable effect on nursing home deaths once community spread became widespread. We find, however, that starting vaccinations just five weeks earlier could have saved on the order of 14,000 lives and starting them ten weeks earlier could have saved 40,000 lives.

Introduction

Nursing homes were the epicenter of the pandemic. The outbreak at Life Care Center, a nursing home in the suburbs of Seattle, was the first glimpse of the risks posed by the virus to the country's 15,436 nursing homes and their 1.3 million residents. [16] A cluster of respiratory illness started in mid-February of 2020, prompting a full investigation by the Centers for Disease Control and Prevention (C[DC]). By March 9th a major outbreak was undeniable; 111 COVID cases were identified including 81 of the facility's 130 residents (62%), 17 staff, and 13 visitors. By the end of March, 48% of the infected residents (39/81) had died (Cornwell, 2020; Healy and Kovaleski, 2020).

Nursing homes were like tinder boxes for communicable disease. The average age was 78, the typical nursing home resident also had multiple risk factors and preexisting comorbidities; 77% of residents had diagnosed high blood pressure, 29% were obese, and 23% had congestive heart failure, all factors associated with higher risk for severe illness and death.[17] Moreover, close contact between staff and residents was unavoidable because nearly 90% of residents need daily help with activities like eating and getting out of bed.[18]

Few nursing homes were able to avoid the virus. Between Jan 1, 2020 and Jan 3, 2021, around the time the first vaccinations started having an effect, 92% of nursing homes had

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[16] This is not quite right. As Carter Mecher had pointed out in a Feb. 20 "Red Dawn" email the passengers on the Diamond Princess cruise ship, although mobile and in relatively good health, were quite elderly and not dissimilar from many nursing home and resident care residents. For the Red Dawn emails see Lipton, Eric. 2020. "The 'Red Dawn' Emails: 8 Key Exchanges on the Faltering Response to the Coronavirus." *The New York Times*, April 11, 2020, sec. U.S. https://www.nytimes.com/2020/04/11/us/politics/coronavirus-red-dawn-emails-trump.html.

[17] See CDC List of Underlying Medical Conditions Associated with Higher Risk for Severe COVID-19, available at https://www.cdc.gov/coronavirus/2019-ncov/science/science-briefs/underlying-evidence-table.html

[18] LTC Focus Public Use Data from the National Institute on Aging (P01 AG027296) and Brown University School of Public Health. Available at www.ltcfocus.org.

experienced at least one resident case and 75% had one or more deaths. 553,660 residents had tested positive, as well as 474,195 of the roughly 1.5 million staff members.[19] Overall there were 107,413 confirmed COVID deaths in nursing homes and recent research shows substantial underreporting in the first half of 2020, bringing the estimated death count in nursing homes closer to 124,000, almost a third of all COVID deaths (380,272) in 2020.[20]

Figure 2.1 shows total deaths and nursing home deaths and the weekly share of nursing home deaths from May 2020 to August 2021. Until vaccine distribution began, nursing home deaths were 25-30% of total deaths. Vaccine distribution began in mid-December with priority given to nursing homes. The vaccines reduced nursing home deaths dramatically along with nursing home deaths as a share of total deaths which fell from about 30% in mid-December to approximately 5% by March of 2021.

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[19] U.S. Bureau of Labor Statistics (BLS), Division of Occupational Employment Statistics. May 2020 National Industry-Specific Occupational Employment and Wage Estimates. Available at https://www.bls.gov/oes/current/naics4_623100.htm.

[20] On confirmed nursing home deaths see CMS Nursing Home COVID-19 Public File, https://data.cms.gov/covid-19/covid-19-nursing-home-data. Note that the nursing home resident population turns over in a year so the total population moving through nursing homes is larger than the average population. See
(Shen et al., 2021) for information on undercounts, and CDC COVID Data Tracker, https://covid.cdc.gov/covid-data-tracker/ for total deaths.

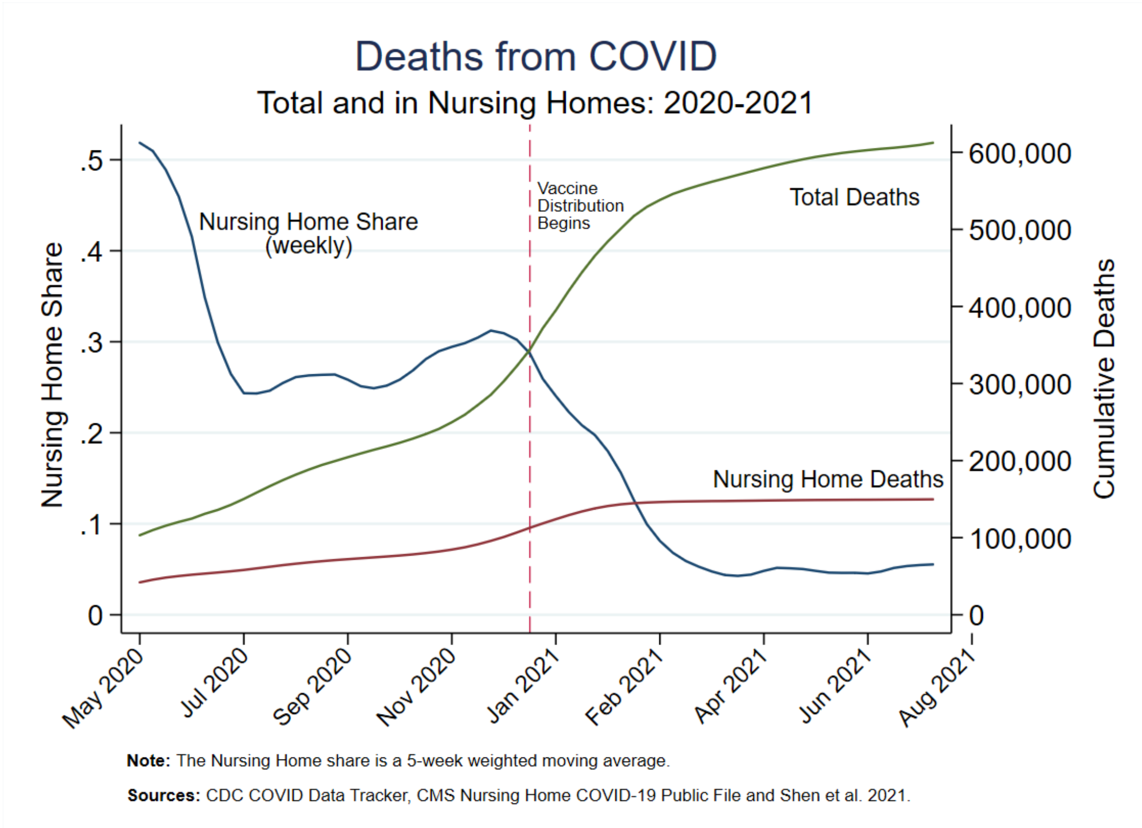

Figure 2.1. Deaths from COVID: Total and in Nursing Homes: 2020-2021

The outbreak in King County confirmed the potential for tragedy. On March 18, the CDC warned "Substantial morbidity and mortality might be averted if all long-term care facilities take steps now to prevent exposure of their residents to COVID-19. The underlying health conditions and advanced age of many long-term care facility residents and the shared location of patients in one facility places these persons at risk for severe morbidity and death" (McMichael et al., 2020).

Thus, the risks to nursing home residents were acknowledged at the time and in fact many steps were taken to protect nursing home residents. While mistakes were made, a topic we'll return to, the devastation in nursing homes happened despite the allocation of significant resources, both public and private, to protect nursing home residents.

In what follows we examine in greater detail the course of the pandemic in nursing homes focusing especially on where policy failed or might have been improved. We also ask whether some nursing homes performed better than others and if so what lessons are to be learned. Did quality certification or regulation, for example, predict nursing home success. Could the nursing homes have been better isolated from the pandemic, protecting the elderly while lifting restrictions on the young as some commentators—most notably the Great Barrington Declaration—advocated?

Isolation and Testing

The nursing homes were an ideal place for using testing as a public health (prophylactic) measure but that wouldn't come until much later. In the early months, it was difficult to test anyone. SARS-CoV-II testing was delayed in the United States due to a series of failures and policy actions by the CDC and the FDA. The initial test developed by the CDC was botched by contamination due to a failure of CDC labs to follow standard operating procedures (Gottlieb, 2021).

A single failure should not have been critical but instead of encouraging and aiding private test suppliers to enter the market the CDC and the FDA essentially monopolized the market. The CDC, for example, stated that only the CDC could operate its test and they refused to provide virus samples to test manufacturers (Gottlieb, 2021). The FDA also issued guidance requiring manufacturers to have SARS-CoV-II tests pre-approved, a new "emergency requirement" that flouted the long-held understanding that laboratory developed tests did not require FDA pre-approval (Gottlieb, 2021; Clement and Tribe, 2015).

As a result, in the entire month of February the CDC managed to test fewer than 4000 samples. During the same time period, German manufacturers had produced and shipped hundreds of thousands of test kits (Gottlieb, 2021). The failure to ramp-up testing—which could only be done with the involvement of the large private labs—had cascading consequences.

With so few tests available, the CDC issued stringent guidelines on who could be tested—essentially restricting testing to symptomatics with a close connection to China or a confirmed case—at a time when it was already clear that asymptomatic transmission was possible and likely common. The failure to test meant that the spread of the virus was invisible to policy makers, including the CDC itself. Scott Gottlieb (2021, p. 132) writes:

> The [CDC] took deliberate steps to enforce guidelines that would make sure it didn't receive more samples than its single lab could handle. In late March, the CDC went so far as to edit an article that was slated for publication in a science journal, to remove a passage inserted by a Washington State public health official that called for widespread testing at senior assisted-living facilities. That statement encouraged more testing than the CDC was prepared to allow or was able to handle at the time.
>
> ...Clinicians and local health officials would later say that they often had to press CDC officials for days to get the agency to accept a sample from a patient that doctors suspected of having COVID.

Limiting testing meant that by the time a facility had a positive test, the virus had often already spread throughout the facility. Recall the Life Care Center outbreak mentioned in the Introduction. The outbreak started mid-February, with multiple residents getting severely ill, including hospitalizations at least as early as February 24, but since there was no connection to China, COVID tests were not approved until February 27 when the interim guidance for testing changed to include unexplained respiratory illness. Thus, the first positive test of a person with no connection to a previous COVID case or China was on February 28.[21]

Another example of this is Canterbury Health and Rehab, a 190-bed facility outside of Richmond, VA, where a resident was confirmed positive on March 19. Even after the CDC gave symptomatic patients in long-term care facilities Priority 2 status for testing on March 24, no residents met the requirements for testing because Virginia also required there to be "no alternative diagnosis" before COVID tests would be approved.[22] Thus, clinicians were required to test for influenza, other respiratory infections, and even run x-rays before testing for COVID. Despite a willing test supplier and pleas from medical directors to the state's Governor, two weeks went by from the index case until mass testing was done, at which point 92 of the 160 residents tested positive. Fifty-four residents, more than half of the positive cases, were asymptomatic at the time of the test, but symptoms would soon appear as approximately 50 residents died over the next few weeks in what at the time was one of the country's deadliest outbreaks.[23]

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[21] For guidance pre February 27, see https://web.archive.org/web/20200227031026/www.cdc.gov/coronavirus/2019-nCoV/hcp/clinical-criteria.html and for post Feb 27, see https://web.archive.org/web/20200228190044/www.cdc.gov/coronavirus/2019-nCoV/hcp/clinical-criteria.html

[22] Virginia Guidance on Testing as of March 20, 2020. https://www.vdh.virginia.gov/content/uploads/sites/182/2020/03/VDH-Updated-Guidance-on-COVID19-Testing_FINAL.pdf .

[23] For data on the Canterbury outbreak see the COVID Tracking Project, https://covidtracking.com/, which reports 49 deaths and 154 cases on the first available report, dated July 9, 2020. It is unclear why the facility only reported 2 deaths and 102 cases to CMS as of May 24, 2020, but the figures from the COVID Tracking Project align with media reports from the time, see for instance, AP News "11,000 deaths: Ravaged nursing homes plead for more testing" which reported 49 deaths on April 24, https://apnews.com/e34b42d996968cf9fa0ef85697418b01. For an interview with Jim Wright, Canterbury's Medical Director at the time, see "'Every day I grieve': A deadly COVID outbreak at Canterbury Rehabilitation changed long-term care", by Michael Martz, Richmond Times-Dispatch, March 19, 2021, available at https://richmond.com/news/state-and-regional/govt-and-politics/every-day-i-

Canterbury is also an example where shortages of personal protective equipment and delays from policy makers likely contributed to the death toll. Despite well-known shortages, it took until May for FEMA to start shipping PPE to nursing homes, shipments that often didn't arrive until June or July, and reports surfaced of facilities receiving faulty, expired, or otherwise unusable equipment. Worse, the shipments did not include N95 masks (FEMA, 2020; McGarry et al., 2020; Rau, 2020).

On March 27, the CDC investigative team released another MMWR report from a neighboring facility of Life Care Center, where an outbreak had developed despite visitor restrictions, twice daily assessments of residents, and fever screening staff before every shift. The report concluded "Symptom-based screening in SNFs could fail to identify approximately half of residents with COVID-19."

CDC guidelines continued to limit testing for nursing home residents to those with symptoms, even after nursing home residents were made high priority on April 27.[24] While CMS recommended weekly testing of all staff and residents on May 18, supply constraints meant that in practice, testing remained limited to those with symptoms or facilities with known outbreaks (CMS, 2020).

In the absence of testing, isolation became necessary. On March 4, 2020, the Center for Medicare and Medicaid Services (CMS) issued guidance to screen people entering, isolate potentially infectious residents, and suspend non-emergency health inspections. On March 13 the nursing homes were ordered to lock down completely by canceling group activities,

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grieve-a-deadly-covid-outbreak-at-canterbury-rehabilitation-changed-long-term-care/article_3eba6f1d-fb40-5184-9a19-50b75da6f547.html

[24] For CDC Test criteria as of April 27, 2020, see web.archive.org/web/20200428234951/https://www.cdc.gov/coronavirus/2019-nCoV/hcp/clinical-criteria.html.

communal dining, and prohibit entry from non-essential personnel and visitors, except on a case-by-case basis for end-of-life situations.[25] Cell phone data in Figure 2.2 suggests that entries to nursing homes started falling in February and continued to plummet in March and April as visitation restrictions and stay-at-home orders were imposed.[26]

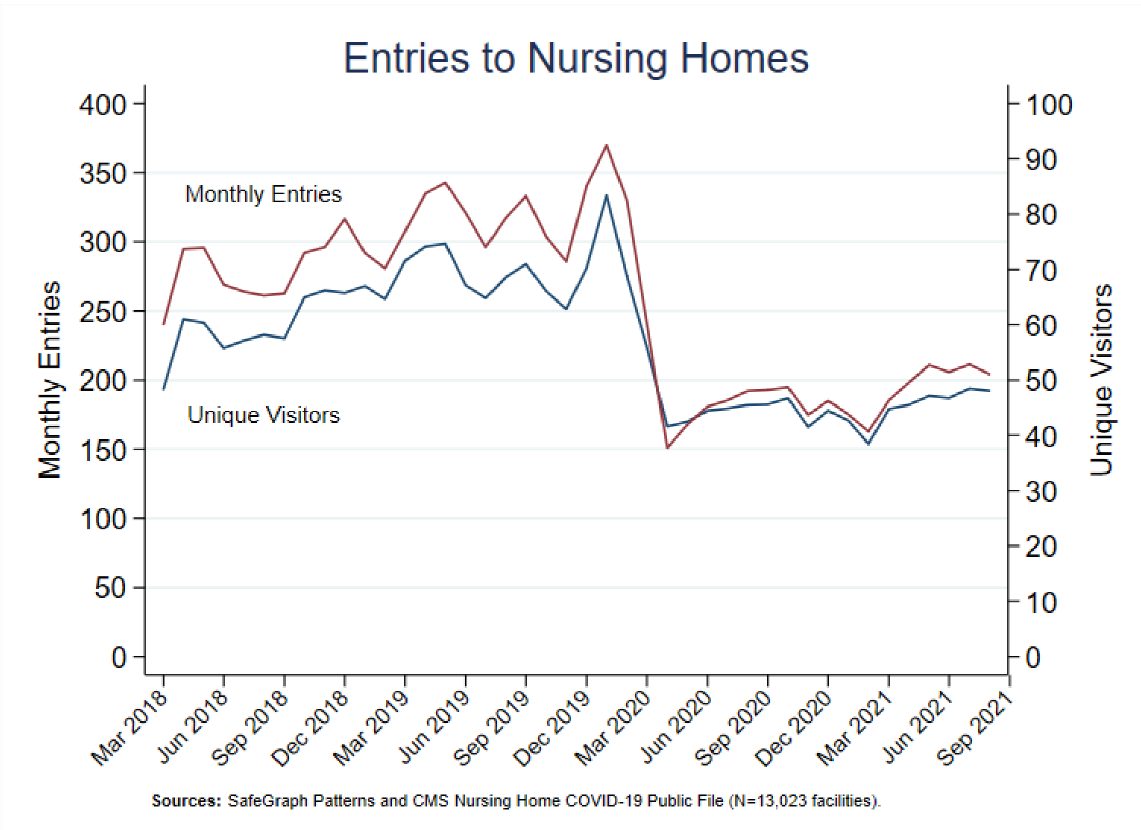

Figure 2.2. Entries to Nursing Homes

[25] It is worth noting here that while the CDC didn't recommend face masks for use in public until April 3, CMS recommended them be made "available and accessible" in facility entrances, waiting rooms, patient check-ins of nursing homes on March 4, and required visitors to wear them starting on March 13. For more on this guidance see CMS Memo, Guidance for Infection Control and Prevention Coronavirus Disease 2019 of (COVID-19) in Nursing Homes, QSO-20-14-NH, March 4, and the revised version that includes visitation from March 13, 2020, available at www.cms.gov/files/document/qso-20-14-nh-revised.pdf. For more on the suspension of health inspections see CMS Memo: Suspension of Survey Activities (CMS, 2020).

[26] Some of the initial drop reflects fewer post-acute care admissions, as most elective surgeries were put on hold.

It is ironic, given the goal of isolation, that one of the few groups allowed to enter nursing homes during this period were COVID-19 patients who were discharged from hospitals to free up hospital capacity. Nursing home operators were reluctant to admit patients without knowing whether they were still infectious but were often required to admit COVID patients. On March 25, New York controversially required nursing homes to admit medically stable COVID patients, an order that also prohibited homes from requiring a test before admission.[27] New Jersey, Pennsylvania, and Michigan soon followed suit.

It's unknown how many additional COVID cases were created by sending discharged patients to nursing homes. The incubation time of the virus suggests that few patients would still have been infectious, and thus the admissions were mostly resulting from, rather than contributing to the nursing home outbreaks in these states.[28] Nevertheless, admitting anyone, let alone a COVID patient, to the tinderbox of nursing homes carried risk. It seems likely that more could have done to isolate these patients, either temporarily in facilities like the Javits Center and the USNS Comfort that went largely unused, or in designated "COVID-only" nursing homes, an approach that was attempted in Massachusetts (Dafny and Lee, 2020) and a handful

[27] New York State Department of Health, "Advisory: Hospital Discharges and Admissions to Nursing Homes", March 25, 2020, available at https://dmna.ny.gov/covid19/docs/all/DOH_COVID19%20_NHAdmissionsReadmissions_%20032520.pdf

[28] For analysis of the New York admission requirements see "Factors associated with Nursing Home Infections and Fatalities in New York State During the COVID-19 Global Health Crisis", New York State Department of Health, February 11, 2021, available at www.health.ny.gov/press/releases/2020/docs/nh_factors_report.pdf.

It has been pointed out that the New York Department of Health advisory did not technically require nursing homes to admit COVID-patients, but it is clear from reporting and the language in the advisory that it was interpreted that way. The language stated "No resident shall be denied re-admission or admission to [a nursing home] solely based on a confirmed or suspected diagnosis of COVID-19. [Nursing homes] are prohibited from requiring a hospitalized resident who is determined medically stable to be tested for COVID-19 prior to admission or readmission". For reporting see Kaiser Health News, "Is Cuomo Directive to Blame for Nursing Home COVID Deaths, as US Official Claims?" available at www.khn.org/news/is-cuomo-directive-to-blame-for-nursing-home-covid-deaths-as-us-official-claims/.

other states (Connecticut, New Mexico, Rhode Island, Utah, and Florida).[29] Hotel occupancy rates in 2020 hit all-time lows and many could also have been repurposed during the emergency.[30]

Instead, these patients were spread widely; by May 24, 2020, when CMS first posted the nursing home COVID-19 data, at least 3,518 nursing homes (23% of facilities nationwide) had admitted one or more of the 27,455 previously hospitalized COVID-patients. In New York and New Jersey the same figures are 52 and 66 percent, respectively.[31]

Visitation remained highly restricted. While CMS introduced flexibility based on local conditions in May 2020, about half of states banned visits outright as late as June, and 8 continued through October. When states did allow visits they were limited to outdoor settings, designated areas with strict infection protocols, or to essential caregivers. By late April of 2021, guidance on visitation had been mostly normalized but cell phone data suggest nursing homes remained socially isolated throughout the pandemic.[32]

Isolation likely helped to avoid some infections but would likely have worked much better when combined with testing. Testing, however, continued to be very restricted, allowing even known outbreaks to grow larger and more deadly.

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[29] For more see National Governors Association, "State Actions Addressing COVID-19 in Long-Term Care Facilities," October 20, 2020, available at www.nga.org/wp-content/uploads/2020/06/State-Actions-Addressing-COVID-19-in-Long-Term-Care-Facilities.pdf .

[30] On hotel occupancy rates see https://www.npr.org/2021/01/27/960384171/2020-was-the-worst-year-ever-for-u-s-hotels-heres-whats-next

[31] Other notable states include Massachusetts (64%), Connecticut (59%), New York (52%), and the District of Columbia (63%).

[32] For details see CMS Memo's QSO-20-3--NH, QSO-20-39-NH, and QSO-20-39-NH-Revised available at https://www.cms.gov/files/document/qso-20-39-nh-revised.pdf .

The Surprising Failure of Rapid Testing

When testing did happen, its impact was limited by long wait times. As late as the week of August 16 when tests were nominally available, only 3% of facilities reported wait times of less than a day, about a third reported 1-2 days, while more than half said tests took 3 to 7 days, and 10% reported more than 7 days. In effect rendering a large portion of tests virtually useless.[33]

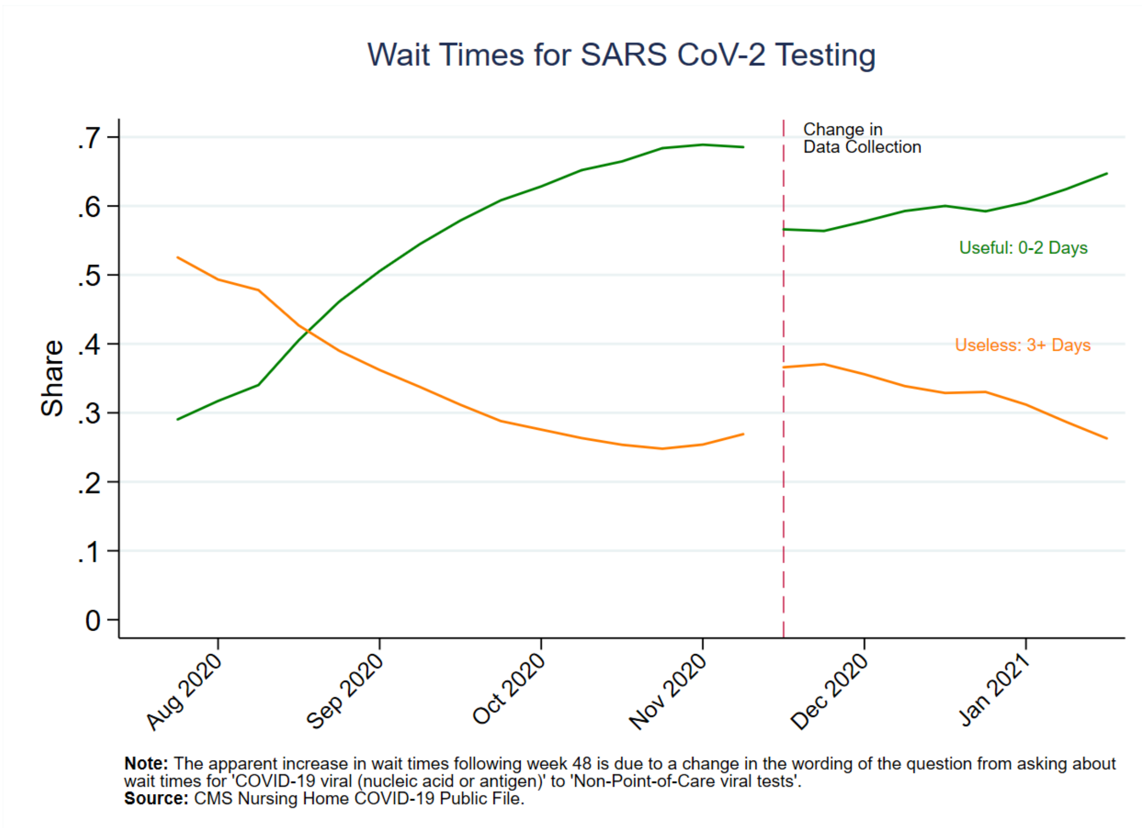

Figure 2.3. Wait Times for SARS CoV-2 Testing

[33] From the CMS Covid-19 Public File for the week ending August 16, the first time this question was included. 403 facilities responded less than 1 day, 4,069 facilities 1-2 days, 8,406 facilities 3-7 days, and 1,363 facilities reported average wait-times of more than 7 days.

Slow testing was supposed to be fixed by rapid antigen tests which could return results in 15 minutes. In July the Department of Health and Human Services (HHS) started sending Quidel Sofia and BD Veritor point-of-care devices to give every nursing home rapid antigen test capability, and in late August HHS purchased the entire lot of Abbott's 150 million BinaxNOW kits and started shipping these to states, including about 8 million that went directly to nursing homes and assisted living facilities.

On August 25, 2020, CMS required facilities to test all staff and residents immediately in the event of a positive case, and retest every 3-7 days until no new cases were identified. CMS also required staff (but not residents) to be tested routinely based on the county's positivity rate, which in effect required most facilities to test their staff at least weekly.[34]

Unfortunately, the requirements and point-of-care tests did not quickly turn the tide on testing, even though two thirds of nursing homes had test capability by the middle of September. Figure 2.4 shows weekly test volumes in nursing homes by recipient (staff or resident), and also breaks out the volume of point-of-care tests for staff and residents separately. Unfortunately we don't have data on lab tests prior to late November, but we do know that by December nursing homes reported weekly totals of nearly 3 million tests, enough to test all staff and residents weekly.

It took until late November-December before nursing homes were running a million weekly antigen tests, and even then, they ran more of the slower, more expensive lab tests.

[34] Cite: QSO-20-38-NH. The minimum frequency required was once a month at a positivity rate below 5%, once a week at a positivity rate between 5-10%, and twice a week if the positivity rate was above 10%.

Why weren't the rapid antigen tests used much more frequently? The explanation is not entirely clear, though we can list some possibilities.[35]

The initial impact of the rapid antigen test rollout was confusion as major states including California, New York, and Pennsylvania already required health care workers to be tested regularly using PCR tests, required certain antigen results be confirmed with PCR, or did not have data collection procedures for antigen tests which added administrative burden.[36]

The difference between rapid antigen tests as public health tests and PCR tests for diagnostic purposes wasn't properly understood early on. Nevada, for example, briefly halted the use of RATs all-together on October 2nd, after PCR testing confirmed just 16 of 39 positive antigen tests, suggesting a false-positive (23/39) rate of nearly 60%.[37] It was less remarked on, however, that Nevada has run 3,725 antigen tests with 3665 coming back negative–thus of potentially considerable information value. Gans et al. (2022) find that the rate of false positives from antigen tests is very low when measured (as it should be) against the number of people screened.

Another problem that slowed the use of RATs was that it wasn't understood that rapid antigen tests were tests of infectiousness rather than infection (Tabarrok, 2020; Mina, 2020). Thus some thought that the lower sensitivity of antigen relative to PCR tests would allow too

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[35] The CMS COVID-19 data does ask facilities for reasons for not testing. The responses essentially rule out reasons such as lack of personnel, supplies, PPE, uncertainty about reimbursement, and access to a laboratory.

[36] Other states were New Jersey, Connecticut and Massachusetts. For more on California's requirement see Coronavirus Disease 2019 (COVID-19) Mitigation Plan Recommendations for Testing of Health Care Personnel (HCP) and Residents at Skilled Nursing Facilities (SNF), California Department of Public Health, May 22, 2020, available at https://web.archive.org/web/20200629003518/https://www.cdph.ca.gov/Programs/CHCQ/LCP/Pages/AFL-20-53.aspx.

[37] "REMOVAL OF DIRECTIVE to Discontinue the Use of Antigen Testing in Skilled Nursing Facilities Until Further Notice", Nevada Department of Health and Human Services, October 9.

many false-negative individuals to enter facilities undetected but these concerns were likely misplaced if these individuals were past the point of infectiousness.

There was also some ambiguity as to whether the tests, which were granted Emergency Use Authorization “to test specimens from individuals who are suspected of COVID-19” could (legally) be used outside the tests authorization on asymptomatic individuals. This prompted CMS to notify facilities that it would exercise enforcement discretion and not penalize facilities for this on December 7, 2020.[38]

Another part of the answer of why rapid tests were not used more frequently is likely reimbursement policy, as Medicare (and sometimes Medicaid) would reimburse diagnostic tests for residents, including asymptomatic residents if the facility had an outbreak, but did not reimburse surveillance tests, or staff tests, even though these were mandated by states and CMS.[39] HHS paid and sent point-of-care rapid test devices to every nursing home, but didn’t fund (or subsidize) their use (beyond one round which was included with the devices). This was a missed opportunity and a likely consequence of a lack of unified decision making.

Similarly, health insurers were required to pay for diagnostic tests of (insured) workers who were symptomatic or had known exposure, but not surveillance tests. In late May, a stand-off erupted between New York’s health department, who issued a directive stating the tests were “medically necessary” and thus should be covered by insurance without cost-sharing, and insurers who claimed surveillance tests were akin to health-screenings like physicals and drug

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[38] For more on this, see www.cms.gov/files/document/clia-sars-cov-2-point-care-test-enforcement-discretion.pdf .
[39] For tests covered by Medicare, Medicaid, and some other sources see www.cms.gov/files/document/covid-medicare-payment-covid-19-viral-testing-flow-chart.pdf .

tests that employers routinely pay for.[40] Ultimately the homes themselves would often be responsible to pay for much of this testing, though states like Maryland and Minnesota paid for some, and about a dozen states deployed teams to help administer tests, sometimes involving the national guard.

As a result of these and other issues, the point-of-care devices were underutilized.[41] The BinaxNOW initiative, however, was an even greater failure. As of February 2021, at least 32 of the 150 million kits were collecting dust in state warehouses, approaching their expiration dates. Making matters worse, the actual figure is likely much larger, as only about half of states had submitted data.[42] Countries like Germany did pursue far more ambitious antigen strategies, aiming to supply facilities with enough rapid tests for every resident to be tested 20 times per month. While it is unclear how much these initiatives contributed to the lower fatality rates experienced among German nursing home residents, evidence from 382,017 tests run exclusively on asymptomatic individuals in Bavarian long term care facilities did identify 1,058 cases, leading Tischer et al., (2021) to note "that a number of infection outbreaks in Bavarian

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[40] For more on the conflict over who would pay for surveillance tests including some state policies see "Testing Nursing Home Workers Can Help Stop Coronavirus. But Who Should Pay?" New York Times, June 9, 2020, available at www.nytimes.com/2020/06/09/health/testing-coronavirus-nursing-homes-workers.html , and also New York State Department of Health, "Directive: COVID 19 Testing of Nursing Home and Adult Care Facility Personnel Deemed Medically Necessary", May 19, 2020, available at https://coronavirus.health.ny.gov/system/files/documents/2020/05/medicalnecessitydirective.pdf .

[41] For more see "Many Nursing Homes Shun Free COVID Testing Equipment", Wall Street Journal, Nov 7, 2020, available at www.wsj.com/articles/many-nursing-homes-shun-free-covid-19-testing-equipment-11604769383 .

[42] For more on the unused BinaxNOW kits, see "The U.S. Bought Rapid Covid-19 Tests to Help Control the Virus. Now Many Are Unused." Wall Street Journal, February 15, 2021, available at www.wsj.com/articles/the-u-s-bought-rapid-covid-19-tests-to-help-control-the-virus-now-many-are-unused-11613397601. The HHS announcement is available at www.hhs.gov/about/news/2020/07/14/trump-administration-announces-initiative-more-faster-covid-19-testing-nursing-homes.html

healthcare institutions may have been prevented based on the relatively inexpensive and fast antigen tests."

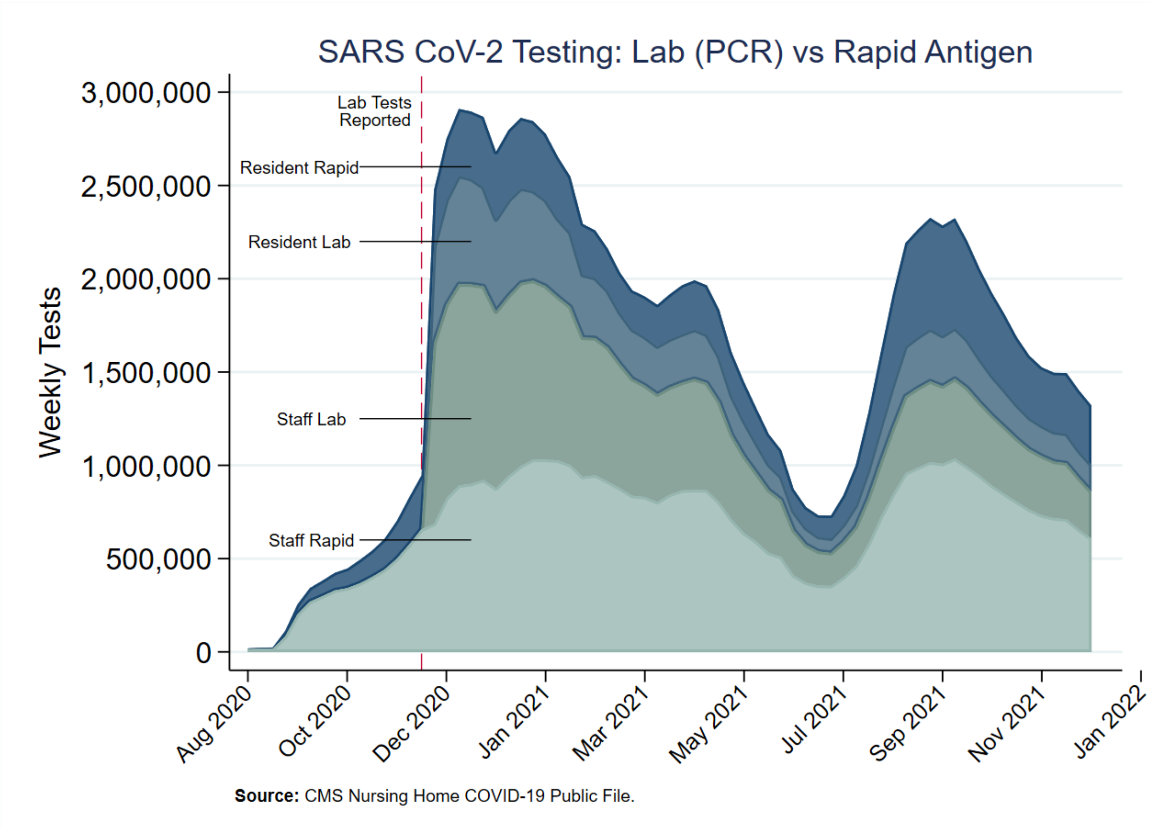

Figure 2.4. SARS CoV-2 Testing: Lab (PCR) vs Rapid Antigen

Could Focused Protection Have Worked?

A central premise of the Great Barrington Declaration (Kulldorff et al., 2020) is that protecting the vulnerable would have been possible through Focused Protection, while the virus spread at an inevitably faster rate, in surrounding communities.

> **How do we protect the elderly in nursing homes and other care settings?**
> "A focused protection strategy would include frequent testing of nursing home staff members that are not already immune, testing of visitors, and less staff rotation so that residents only interact with a limited number of staff people. COVID-19 infected

> individuals should not be sent to nursing homes, and all new residents should be tested. Sequestering of care home residents who have COVID-19 is also important."
>
> "By way of example, nursing homes should use staff with acquired immunity and perform frequent testing of other staff and all visitors. Staff rotation should be minimized."
>
> Great Barrington Declaration, October 4, 2020 [43]

A focused protection strategy which would have meant *fewer* community suppression strategies (fewer lockdowns, school closings, mandatory mask wearing etc.) We evaluate whether focused protection could have worked by looking at whether some nursing homes were in fact better able to protect their residents. If some nursing homes were successful at protecting their residents this suggests their approach *might* have been scaled. If there is little evidence of successful protection given substantial community suppression strategies, however, that suggests that focused protection would certainly not have worked.

We look primarily at two tests, whether higher quality nursing homes were better protected and whether some nursing homes were able to perform substantially better than would be suggested by their community infection rates.

5-Star Ratings and Quality Measures

If it was feasible to shield nursing homes from the virus, we would expect to see better outcomes among higher quality nursing homes. A natural place to look is therefore the CMS Five-Star Rating system, which rates facilities from 1 to 5 stars relative to facilities in the same state and is based on comprehensive data from annual health inspections, staff payrolls, and clinical quality measures from quarterly Minimum Data Set assessments. The rating system has been validated against other measures of quality such as mortality and hospital readmissions,

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[43] The first quote is from the Frequently Asked Questions section of the Great Barrington Declaration website, while the second is from the declaration itself. For more see www.gbdeclaration.org/ .

and thus serves our purposes as we are primarily interested in clinical outcomes (Cornell et al., 2019; Konetzka et al., 2020).[44] So, did higher quality homes have better COVID-19 outcomes?

Researchers rushed to answer this question in the early months of the pandemic. Konetzka et al. (2021), reviewed 16 studies that examined the relationship between the overall Five-Star Rating and facility-level COVID-19 outcomes, and surprisingly concluded "no practically meaningful or statistically significant relationship was found between the overall 5-star rating and COVID-19 outcomes."[45] However, they also noted important limitations including that most studies were conducted prior to the November-December surge, thereby missing a large portion of the cases and deaths. Many studies also failed to control for local disease prevalence and facility size, the most consistent predictors in the literature, and almost all studies used cross-sectional data, leading the authors to conclude "More work is needed to establish causal connections and assess temporal trends." We revisit this question with data on the universe of U.S. nursing homes and a year of additional data, relative to the most recent study reviewed by Konetzka et al.

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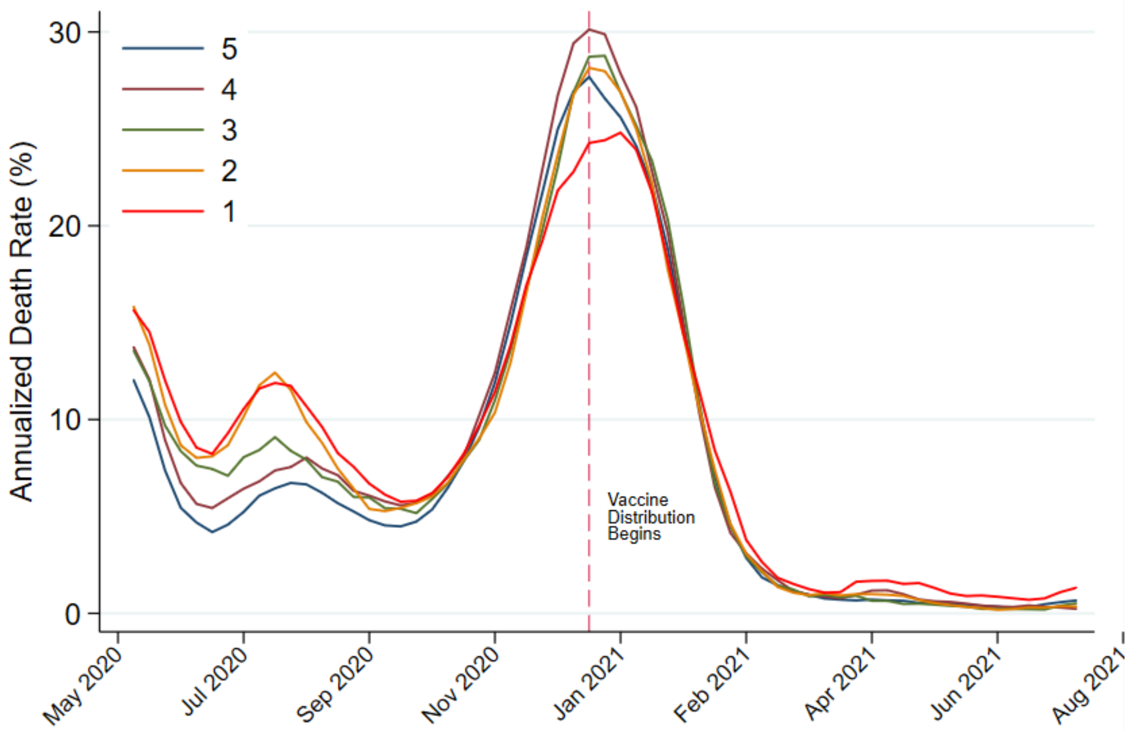
[44] The rating system has been criticized, among other things for overemphasizing clinical outcomes, relative to measures of subjective wellbeing/customer satisfaction, and for relying on facility-reported staff data. The first is less of a concern for us as we are primarily interested in clinical outcomes, and the second is no longer a concern after the staff measure was updated in 2018 with data based on auditable payrolls.

[45] The study with the most recent data ended in January 2021 (Williams et al., 2021), did find a modest negative (and statistically significant) relationship, however, Konetzka et al. point to several potential flaws with that particular study. First, the ratings used were from January 2021, which meant that performance during COVID could predict ratings, rather than the other way around. Second, the study "used an unusual denominator for their outcome measures: cumulative resident incidence and mortality" where "[t]he denominator for the cumulative measures was the resident census as of January 2021 rather than the typical baseline measure, with an offset term to account for average resident census starting only in May 2020." In effect, this meant that the outcomes would be inflated in facilities who experienced large drops in occupancy from deaths or drops in admissions prior to NHSN data collection which began May 2020. For more see page 4-5 of Konetzka et al., (2021).

Before analyzing this data we note that CMS required nursing homes to report weekly data on cases and deaths from May 24, 2020, but allowed voluntary reporting for the period prior to this. We also note that the testing requirements that were imposed in late August of 2020, and the vaccine distribution starting in December 2020 would all significantly impact the data generation process.

To explore this more we start by plotting unadjusted COVID-19 death rates by pre-pandemic star rating in Figure 2.5. These seem to paint a slightly different picture; unadjusted death rates followed ratings during the spring and summer of 2020, but seem mostly indistinguishable from the fall and winter of 2020, except perhaps for facilities with 1-star ratings, which surprisingly had the lowest death rates during the December-January peak.



Note: 3-week moving average of annualized COVID-19 death rate by (pre-pandemic) star rating.
Source: CMS Nursing Home COVID-19 Public File.

Figure 2.5. Unadjusted COVID-19 Death Rates by Pre-Pandemic Star Rating

We therefore split our data into four separate time periods; prior to May 24, when reporting was voluntary, between May 24 and August 30, 2020, when reporting was mandatory but testing had yet to be required, from August 30, to December 27, when testing was mandated, and from December 28, 2020, when the vaccine deployment began, until December 5, 2021.

COVID-19 cases and deaths counts are overdispersed (i.e. have a variance greater than their mean) and tend to have excess zeros relative to negative binomial or poisson distributions.[46] We note that positive counts and zeros are potentially different data generating processes, for instance, it may be that higher quality homes employ more staff, which raises the probability of introducing the virus to a facility, but that the higher quality staff follow infection protocols more closely, which reduces the chance it will spread within the facility. To model this, we run Zero-Inflated Negative Binomial models, which allow these processes to be different (Deb et al., 2017).

Our variable of interest is the overall pre-pandemic Five-Star rating, which, unlike other consumer ratings that might have bimodal distributions, come in five categories of similar proportions.[47] We control for factors outside the facility's control including the disease prevalence during each period (measured as the number of positive tests as a share of the county population), natural immunity prior to the period (measured as cumulative cases as a share of population), the county's Urban-Rural classification from the National Center for Health Statistics (six categories), socioeconomic factors using the county's Area Deprivation Index, and the facility's size (log number of beds). The model for 2021 also controls for the county's average vaccination rate during the period. Finally, the count portion includes an exposure term that is the log of the number of resident-weeks in the facility during each period, while the zero-portion is a logit model with the same control variables.[48]

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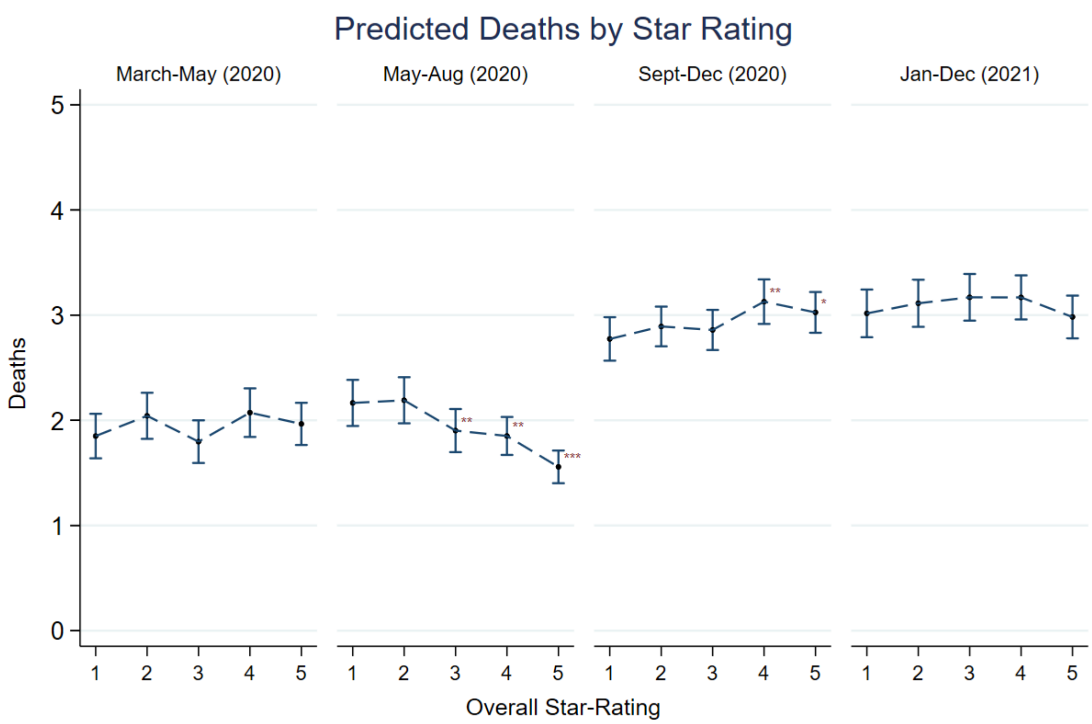
[46] For cases we observe between 10,012 (period 1) and 1,826 (period 4) facilities with zero cases, and for deaths we observe between 10,939 (period 1) and 5,892 (period 4) facilities with zero deaths. Our samples range from 14,008-14,860 facilities. We confirm the counts are overdispersed by noting that our models produce estimates of the negative binomial overdispersion parameter, alpha, ranging from 1.10-3.04 for cases, and 1.72-2.15 for deaths (values of alpha greater than 1 indicate overdispersion).

[47] 17% of facilities were rated 1 star, 20% rated 2 star, 18% rated 3 star, 21% rated 4 star, and 24% rated 5 star.

[48] We excluded approximately 1.5% of facilities for failing to meet the CMS data quality check 10% of weeks or more.

Detailed results of the count models are shown in Table A2.2 in the Appendix to Chapter 2, but for convenience in Figure 2.6 we plot predicted counts of deaths in each period by overall star rating. We also test whether deaths were different in facilities rated 2, 3, 4 and 5 star relative to those rated 1-star attach significance stars in the graph. We also conduct the same analysis for cases, which find similar results to that of deaths, and is included in the Appendix to Chapter 2.



Note: Adjusted predictions and 95% confidence intervals estimated using zero-inflated negative binomial regression of total deaths during each period on pre-pandemic star ratings. Significance stars show estimate is statistically different to homes rated 1-Stars, at 1% (***), 5% (**), and 10% (*) percent levels. Standard Errors clustered by county. Estimates adjusted for the log bedcount, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables.

Figure 2.6. Predicted Deaths by Star Rating

On balance, we find star ratings were not predictive of future deaths. In some periods deaths in five stars and 1-star facilities were similar, in others they were lower in 5-star facilities, in others higher. Our findings are therefore in line with the earlier conclusions by Konetzka et al.

The question then becomes whether the lack of relationship between ratings and COVID-19 outcomes is because it is so hard to shield a nursing home from COVID-19 that we don't observe much variation in COVID-19 outcomes at all, or whether star-ratings are simply measuring the wrong thing or are too gamed to be useful?[49]

The evidence we find suggests that higher quality nursing homes did take more actions to avoid COVID but these actions were mostly ineffectual, at least as far as we are able to measure statistically. For instance, higher rated facilities consistently invested in more testing. On average, facilities with 5- star ratings ran 0.93 Point-of-Care (antigen) tests per resident-week, compared to 0.76 for facilities rated 1-star. The difference is even greater for lab tests where 5-star rated facilities ran 0.94 tests per resident-week vs 0.57 for facilities rated 1-star. For more see Appendix to Chapter 2, Section A. It is possible that gains from more tests, better routines, compliance with care standards, and more available staff might simply be too small to measure, or offset one another (i.e. better practices are offset by the additional risk of more staff entering the facility (McGarry et al., 2021a), leading to no net gains.

Since quality ratings do not reliably predict COVID-19 outcomes we ask if any nursing homes were able to insulate residents from COVID-19, and what, if anything, can be learned from these facilities?

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[49] On gaming of ratings see Han et al., 2018; Ody-Brasier and Sharkey, 2019 and for a recent article on this topic see "Maggots, Rape and Yet Five Stars: How U.S. Ratings of Nursing Homes Mislead the Public." New York Times, March 13, 2021, available at www.nytimes.com/2021/03/13/business/nursing-homes-ratings-medicare-covid.html

Any Safe Islands in an Ocean of Disease?

Is there any evidence that some nursing homes were able to protect their residents substantially better than would be predicted by community infection rates? Prior to the vaccination campaign, community spread was found to consistently predict COVID-19 cases and deaths in nursing homes (Abrams et al., 2020; Konetzka et al., 2021), while, as we noted in the last section, nursing home quality ratings generally, do not. In Figures 2.7 and 2.8 we plot total case and death tolls in nursing homes (as a % of residents) against community spread (cases as a % of county population), up until February 28, 2021, and note that few facilities in high transmission counties managed to shield their residents.

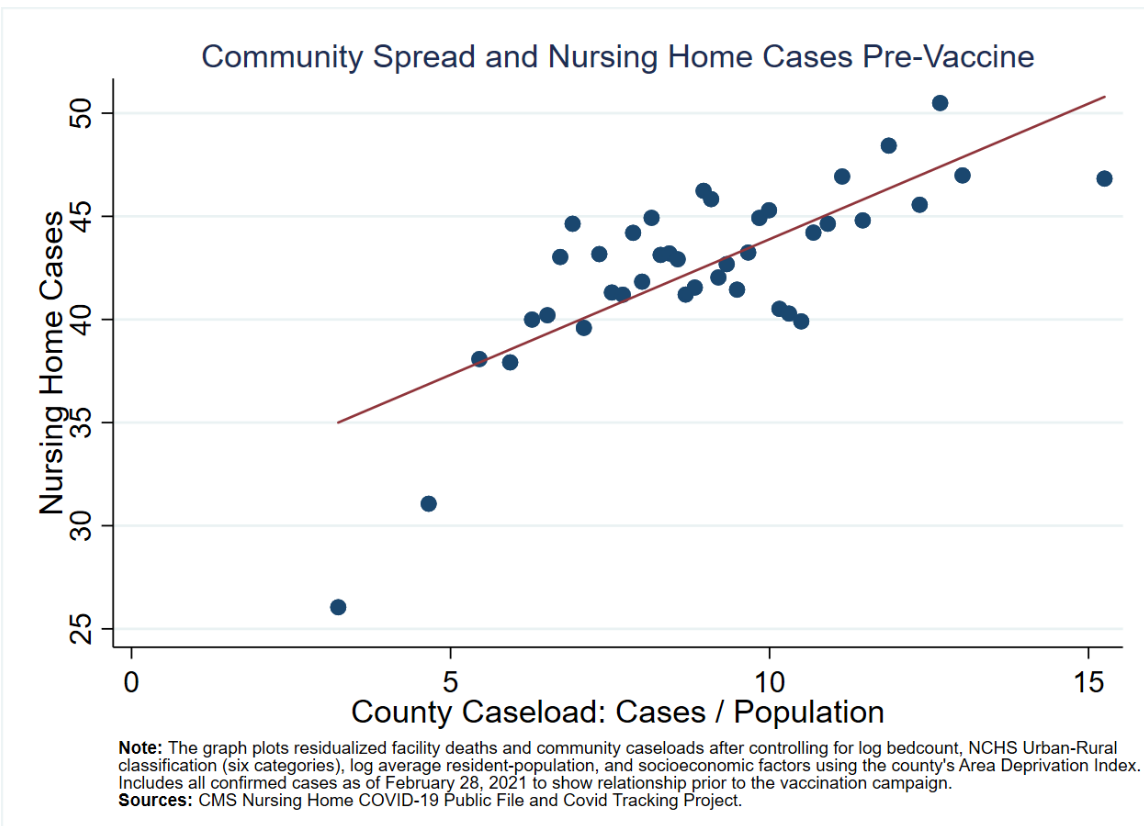

Figure 2.7. Community Spread and Nursing Home Cases Pre-Vaccine

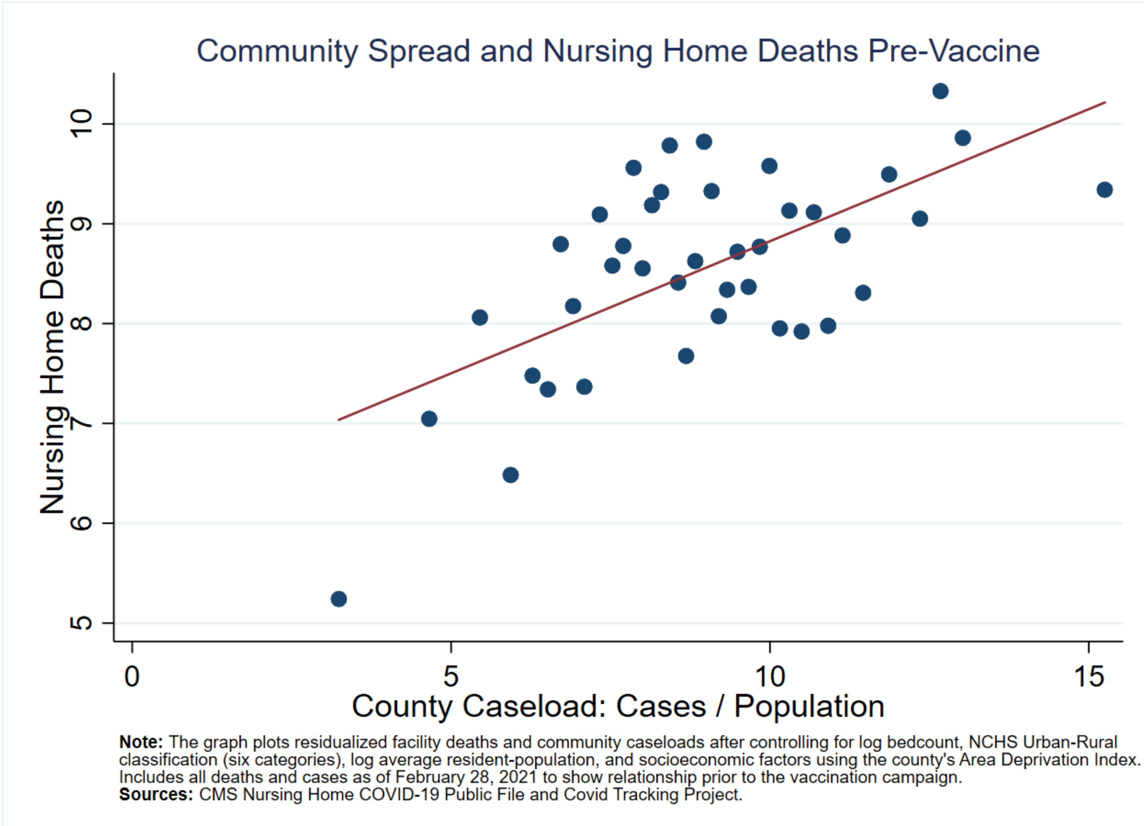

Figure 2.8. Community Spread and Nursing Home Deaths Pre-Vaccine

To try to get a more complete sense of whether there were islands of safety we turn to see if any facilities managed to keep their residents acceptably safe while being located in counties with high caseloads, and if so, what do they have in common? We recognize that this analysis exploratory and cannot be considered causal as we are selecting on the dependent variable.

The average U.S. nursing home is located in a county where, as of the end of the pre-vaccine period (up until February 28, 2021), cumulative cases as a share of the county population were 8.95%. We define “oceans of covid” as counties with caseloads in or above the 90th percentile, or 11.94%, with average caseloads of 13.5%. We then define safe islands, as facilities that managed to keep deaths below 2.32% of their residents, the 25th percentile, while located in an ocean of COVID. This level of safety is comparable to what one would expect from a year-long flu season where all facilities are exposed, the virus has an attack rate of 33%, and a case fatality rate of 6.5% (Lansbury et al., 2017). 248 facilities meet these criteria. We exclude 6 children's hospitals which have an average age lower than 50, leaving us with 242 "safe islands."[50]

For islands to provide any information we first have to rule out that their success can be attributed to a substantially different patient population or other factors that can't be replicated elsewhere. In Table 2.1 we compare pre-pandemic patient characteristics for "islands" relative to other facilities in "oceans of COVID" and the nationwide average. Islands have patients with comparable age, and gender distributions, slightly lower acuity levels and rates of incontinence, but higher rates of obesity. Overall these differences seem unlikely to explain their performance.

[50] Note that occasional missing data for some variables/sources will mean this figure will fluctuate somewhat.

Table 2.1. Pre-Pandemic Patient Characteristic Descriptive Statistics: Outliers vs. Nationwide Average

Descriptive Statistics: Outliers vs. Nationwide Average

	Islands		Oceans		Nationwide	
	mean	sd	mean	sd	mean	sd
Admissions from Acute Care Hospital (%)	79.4	16.1	79.9	15.6	84.4	13.4
Average Age	79.1	6.95	79.2	7.36	78.8	7.27
Activities of Daily Living (ADL) Score (0-28)	15.4	3.16	16.0	2.90	16.5	2.69
Case Mix Index	1.12	0.18	1.15	0.19	1.18	0.19
Female (%)	65.8	12.0	64.7	11.6	64.9	12.1
White (%)	84.3	19.7	81.4	22.3	79.0	22.1
Low Cognitive Impairment (%)	42.0	14.1	37.1	11.6	38.4	12.5
Moderate Cognitive Impairment (%)	53.2	13.0	55.8	12.6	54.4	12.5
Bladder Incontinence (%)	76.6	13.1	79.0	12.2	79.7	13.1
Bowel Incontinence (%)	57.6	16.1	61.6	16.0	64.7	15.4
High Blood Pressure (%)	75.8	10.8	77.2	10.7	76.8	10.6
Obese (%)	33.3	10.1	31.5	8.81	30.0	8.37
Hospitalizations per resident-year	2.15	1.04	2.19	0.98	2.37	1.06
Observations	216		994		13961	

This table compare pre-pandemic patient characteristics for outlier facilities relative to the nationwide average. Outliers are located in counties with high community spread, defined as having cases as share of population >11.94% (90th percentile), and successfully shielded their residents from COVID-19, defined as having fewer than 2.32% (25th percentile) of residents die from COVID-19, up until February 28, 2021.
Sources: CMS Nursing Home COVID-19 Public File, Covid Tracking Project, and LTC Focus/Brown University.

We therefore turn to see if their investment decisions and other facility characteristics will give an indication of what it would take to make Focused Protection work.

Table 2.2. Pre-Pandemic Patient Characteristic Descriptive Statistics for Key Variables: Outliers vs. Nationwide Average

Descriptive Statistics: Outliers vs. Nationwide Average

	Islands mean	Oceans mean	Nationwide mean
Residents Total COVID-19 Deaths	0.32	10.3	8.66
Residents Total Confirmed COVID-19	14.2	47.6	42.8
Residents Total Admissions COVID-19	6.90	18.5	17.4
Residents Total Non-COVID-19 Deaths	12.0	16.1	19.6
Staff Total Confirmed COVID-19	24.4	43.2	36.8
Weekly Resident Antigen Tests/Residents	0.34	0.30	0.25
Weekly Staff Antigen Tests/Residents	1.46	0.94	0.75
Weekly Resident Lab Tests/Residents	0.31	0.31	0.42
Weekly Staff Lab Tests/Residents	0.78	0.65	1.00
Shortage Nursing Staff (% of Weeks)	18.1	19.9	17.3
Shortage Clinical Staff (% of Weeks)	2.11	2.23	2.21
Shortage Nurse Aides (% of Weeks)	21.5	22.0	19.4
Ventilators available (#)	22.7	18.1	18.0
For profit facility (%)	58.1	65.0	70.5
Non-profit facility (%)	27.4	26.0	23.2
Government operated facility (%)	14.5	8.99	6.27
Number of All Beds	76.1	96.5	106.7
Occupancy rate (avg)	73.8	69.4	70.8
Facility age (years)	26.0	28.5	30.5
Hospital based (%)	10.4	5.30	3.85
Star-rating	3.32	3.12	3.17
County Case toll: Cases/Pop	13.5	13.6	8.88
County Death toll: Deaths/Pop	0.20	0.25	0.17
Area Deprivation Index (Nat'l Rank)	61.2	66.1	54.2
County Vaccination Rate	6.50	6.18	6.34
County Population	421663.5	321351.2	832341.3
Observations	242	1058	15075

This table compare outlier facilities to the nationwide average on key facility- and county-level variables. Outliers are located in counties with high community spread, defined as having cases as share of population >11.94% (90th percentile), and successfully shielded their residents from COVID-19, defined as having fewer than 2.32% (25th percentile) of residents die from COVID-19, up until February 28, 2021.
Sources: CMS Nursing Home COVID-19 Public File and Covid Tracking Project.

A few things stand out. If we account for the difference in size (76.6 vs 106.7 beds), outlier facilities report similar levels of staff cases as the national average (24.6 vs 36.8), so we can say that the shielding occurred not only with the virus surrounding them in the community, but at least as close as the facility's doorstep.

Interestingly, the successful outlier facilities (islands) ran more point-of-care (rapid) tests per resident-week than those who were not successful (1.46 vs .94 staff tests, and .34 vs .30 resident tests), and significantly more than the national average. However, they actually ran less PCR tests than the national average (.31 vs .42 staff tests, and .78 vs 1.00 resident tests). Workforce shortages were comparable across all groups, while islands were more likely to be hospital-based (10.4% vs 5.3% vs 3.8%), less likely to be run for-profit (58.1% vs 65% vs 70.5%), and admitted far fewer residents previously hospitalized for COVID (6.9 vs 18.5 vs 17.4). Islands also report having more ventilators available, but this is only reported by a very small % of facilities, so we would interpret this cautiously.

To further explore the differences we find in testing, COVID admissions, and hospital-base, we run separate regressions for each variable on total resident COVID-19 deaths as of February 28, 2021 with a fixed set of control variables. While the decision of how many tests to run, how to run them, and whether to admit patients previously hospitalized with COVID, are to some extent determined by the facilities themselves and could thus be biased, so we interpret with caution. However, before we discuss individual effect estimates, note that if we take each effect estimate at face value, the cumulative effect of the observed differences in testing, COVID admissions, and hospital base, would explain less than 1 (-0.76) deaths, of the roughly 10 death difference between the outlier facilities and the rest. In other words, most of the differences are due to factors we did not include in our analysis, unobserved factors, or simply luck.

Table 2.3. Average Marginal Effect on COVID-19 Deaths

Average Marginal Effect on COVID-19 Deaths.

	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)
Weekly Resident Antigen Tests/Residents	-1.54***					
	(.28)					
Weekly Staff Antigen Tests/Residents		-0.17				
		(.119)				
Weekly Resident Lab Tests/Residents			-1.12***			
			(.216)			
Weekly Staff Lab Tests/Residents				-0.04		
				(.0883)		
Residents Total Admissions COVID-19					0.04***	
					(.0023)	
Hospital based (%)						-0.03***
						(.00533)
Observations	15003	15003	14501	14501	15093	15093

Note: Each estimate (standard errors) is estimated using zero-inflated negative binomial regression of total COVID-19 resident-deaths as of February 28, 2021, on the variable of interest and a fixed set of controls. Estimates adjusted for the log number of beds, facility age (in years), NCHS Urban-Rural classification (six categories), local disease prevalence measured as cumulative COVID-19 cases as a share of the county population, and local socioeconomic factors using the county's Area Deprivation Index. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. Standard Errors clustered by county.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Taken at face-value but with the above limitations in mind, the results suggest that if nursing homes ran *one additional rapid test on every resident each week* (quadrupling their use), it would prevent 1.54 deaths over the 1-year period, while one additional weekly lab test for each resident (more than *doubling their use*) would prevent 1.12 deaths. To put this in perspective, nursing homes had on average 8.66 covid deaths, so to bring deaths down to 2-3, what we would expect from a hypothetical year-long influenza season, would require a large increase i74stimateg.

The estimates for both kinds of staff-tests were not statistically significant, perhaps because these were mandated on a surveillance basis by CMS (while resident-testing was only mandated for when symptomatic, in response to outbreaks, or known exposure). The estimate for COVID admissions is statistically significant, but suggests a facility would have to admit 25 former COVID patients (more than double the national average) for it to lead to an additional death.

Finally, the estimate for hospital base is statistically significant, and is consistent with the claim by Gottlieb (2021, p.300-301) that hospital based facilities did a better job at controlling the spread. However, the effect, estimated to -0.03 deaths, means it is probably not very economically relevant, as it implies 33 facilities would have to be brought up to the standard of hospital-based facilities, to prevent 1 death. Nationwide this corresponds to preventing 448 deaths if all of the 14,744 non-hospital based facilities were transformed prior to pandemic.

It is possible that our previous estimates, using the entire country, overestimate the cost of reducing deaths relative to a Great Barrington scenario where community spread is higher. We also re-run the same analysis restricted to the counties with high community spread. While the point-estimates grow, the cumulative effect of these variables remains modest, explaining about one and a quarter (1.22) of the difference of about 10 deaths between the oceans and the islands. For more on this see Appendix to Chapter 2, Section C.

Overall, this exercise suggests that a very large increase in the use of rapid antigen tests could have averted a significant number of nursing home deaths but the increase is so large as to be out-of-sample. Other countries did use rapid antigen tests at much higher levels but at least in the United States our judgment is that the focused protection strategy would certainly have resulted in more deaths outside of nursing homes and even in more deaths in nursing homes making the benefits of the strategy tenuous.

Finally, it’s important to note that many of the specific points of the focused protection strategy were either done or were moot. The points about frequent testing of visitors and isolating COVID-positive residents, for example, are essentially moot as CMS required isolating COVID-positive residents since March 2020, and, as we have documented, visitors were essentially banned nationwide for large periods, and there's little evidence to suggest they have come back since.

Sending COVID-19 infected individuals to nursing homes certainly posed a risk, as discussed earlier but many patients admitted from COVID hospitalizations were probably not infectious. Moreover, by June 9 states including California, New York, Florida and Pennsylvania required hospitals to test before discharge, and from August facilities nationwide were required to test anyone with symptoms or known exposure, so, at least from the fall of 2020, this issue would seem to be dealt with.[51]

The remaining proposals were to limit staff rotation, a point that was clear early on (Chen et al., 2021), and finally, that nursing homes ought to use staff that have already acquired natural immunity. It is frankly hard to imagine how this could have been done at scale, especially considering that at the time the declaration was signed, 16% of nursing homes already reported severe shortages for nurses and 19% for nurse aides.

So while we have highlighted areas where we believe more could have been done to protect nursing home residents, a balanced reading of the evidence would have to acknowledge that a significant portion of deaths in nursing homes happened while we *both* maintained a version of community lockdown and social distancing policies opposed by the GBD *and* focused protection. In other words, the United States implemented focused protection and it didn't

[51] The other 5 states were Alabama, Massachusetts, Michigan, New Mexico, and Oklahoma, covering about 35% of residents nationwide. For more on this, see "State Actions Addressing COVID-19 in Long Term Care Facilities" by the National Governors Association, available at www.nga.org/wp-content/uploads/2020/06/State-Actions-Addressing-COVID-19-in-Long-Term-Care-Facilities.pdf .

work. Moreover, as Tabarrok (2020) noted, the Great Barrington approach contained an internal contradiction–the goal was to free most of society from COVID restrictions by segregating the elderly but segregating the elderly would have been much more difficult the fewer the lockdowns, mask mandates, social distancing, and other restrictions imposed on the rest of society.

Vaccine Roll Out: Pharmacy Partnership for Long-Term Care

Operation Warp Speed (OWS) produced vaccines in record time but OWS was not in charge of approval or administration, so warp speed slowed to impulse power on November 20 when Pfizer submitted their application for Emergency Use Authorization (EUA) to the FDA. The Vaccines and Related Biological Products Advisory Committee (VRBPAC) met on December 10, 20 days later, to discuss the vaccine's safety and efficacy in individuals 16 years of age and older.[52] VRBPAC voted in favor and the FDA issued the EUA on December 11. Hope was in the air and HHS secretary Alex Azar told the press that every nursing home patient could be vaccinated by Christmas.

The reality proved different. Distribution of vaccines was initially held up in part because CVS and Walgreens insisted facilities collect written consent forms, a logistical hurdle when many nursing home residents need family members to decide on their behalf. Ultimately, the pharmacies allowed verbal consent from residents and emails/phone calls from family

[52] Specifically, whether it was "reasonable to believe that the Pfizer-BioNTech COVID-19 Vaccine may be effective in preventing COVID-19 in individuals 16 years of age and older," and if "the known and potential benefits of the Pfizer-BioNTech COVID-19 Vaccine outweigh its known and potential risks for use in individuals 16 years of age and older."

members, but by Christmas Eve, less than 25,000 residents had received their first dose.[53] Distribution did not really get going until early January.[54]

The vaccine undoubtedly saved many lives, but the slow start meant that it took until the middle of January before a significant portion of residents had received their first dose, and with another 2 weeks for immunity to develop, it is striking how much of the damage was already done by the time vaccine-acquired immunity developed for many in late January. Nursing home cases had fallen from their peak of 33,710 the week of December 20, to 17,002 the week of January 24, and 11,381 the week of January 31st.

[53] For more on this see www.washingtonpost.com/health/nursing-homes-covid-vaccine-consent-delays/2020/12/19/730ecd4a-3fd5-11eb-8bc0-ae155bee4aff_story.html .

[54] Note that the Pharmacy Partnership for Long-Term Care was also responsible for administering the vaccine to assisted living facilities and while nursing homes were generally prioritized, this was not always feasible (or desirable), for instance in cases where facilities offer both skilled nursing and assisted living. In our graphs we assume 90% of doses went to nursing homes in the first 6 weeks.

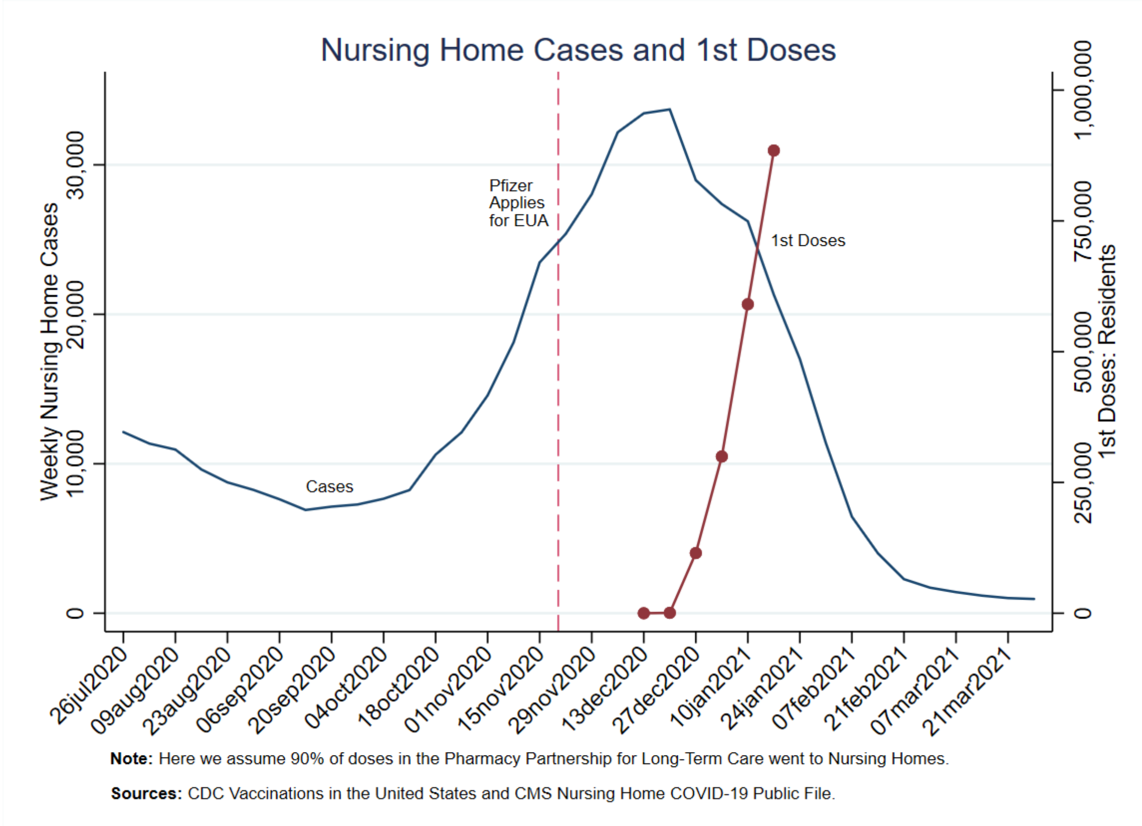

Figure 2.9. Nursing Home Cases and 1st Doses

The question we ask is how much of this illness and death could have been avoided with reasonable changes in the vaccine approval and administration process? We first consider an approach similar to the one discussed by Gottlieb (2021 p. 301) where VRBPAC convenes a day or two after EUA submission to consider a limited EUA for residents of nursing homes and other congregate settings–patients for whom it was already abundantly clear the known and potential benefits outweighed the known and potential risks. If this was pursued together with better coordination of the initial launch of the Pharmacy Partnership for Long-Term Care Program, it is entirely plausible to move administration up a total of 5 weeks.

The question becomes how nursing home cases would evolve with earlier vaccine administration. To get a sense of this, we create an estimate of natural immunity among nursing home residents, note how this relates to the growth rate in cases, and use it to inform us how cases might have evolved with earlier vaccinations.[55] We don't argue that the 3rd wave receded solely due to natural immunity, however, we do think it gives us a reasonable indication. Note for instance that cases peaked in nursing homes the week ending December 20, about 3 weeks earlier than the rest of the country, and, as can be seen in Figure 2.10, it is striking how many residents our estimates suggest were exposed to the virus in the nursing homes.

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[55] For any given week, the flow of residents acquiring immunity are those who contract the disease but do not die. Some difficulties include asymptomatic cases, lack of testing especially in the beginning, as well as residents that are tested while no longer infectious, to account for this we assume that on average there are 50% more cases than we observe. The stock of immune residents then equals that week's flow, plus some fraction of last week's stock, as natural deaths imply the stock decays. We use 0.5% per week, which we take from data on weekly non-covid deaths / population.

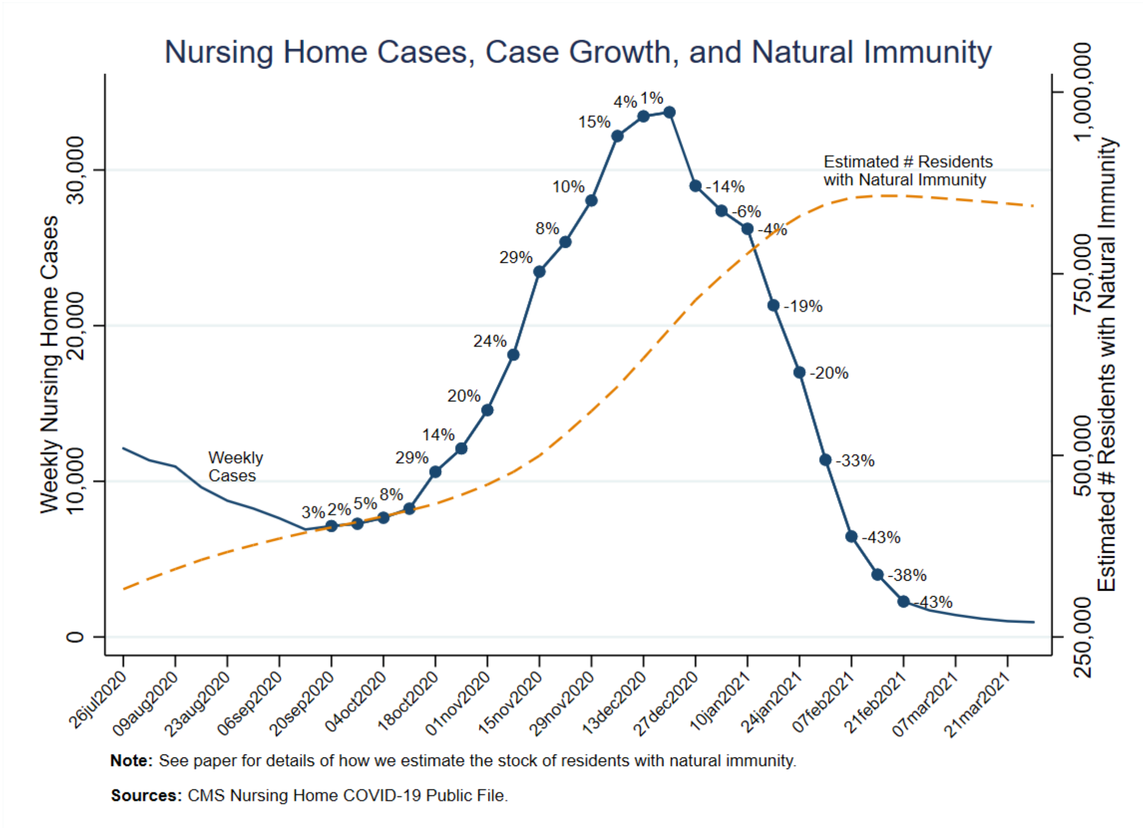

Figure 2.10. Nursing Home Cases, Case Growth, and Natural Immunity

From Figure 2.10 we note that cases grew exponentially through large parts of October, and then peaked the week of December 20, when an estimated 675,000 current residents had been exposed to the virus. Cases then started falling, at a rate that increased as vaccine-acquired immunity started to kick in towards the end of January, and continued to fall until it stabilized around 1,000 weekly cases mid-March. In comparison, prior to the vaccine, we never went below 6,900 weekly cases.

We take 675,000 as a rough estimate of the number of immune residents required for cases to peak. With some assumptions of efficacy and administration, we find that by moving vaccinations 5 weeks earlier, immunity would now reach this level on December 9.[56] We then assume cases would start to fall at the same rate as we observed, before the rate of decline further increases once the stock of immunized residents reaches 800-850,000. We approximate this by moving the growth rates forward one period the week of December 20. While this is somewhat arbitrary, we believe it is conservative given that the stock of immune residents would be growing much faster in this scenario, than what actually happened.

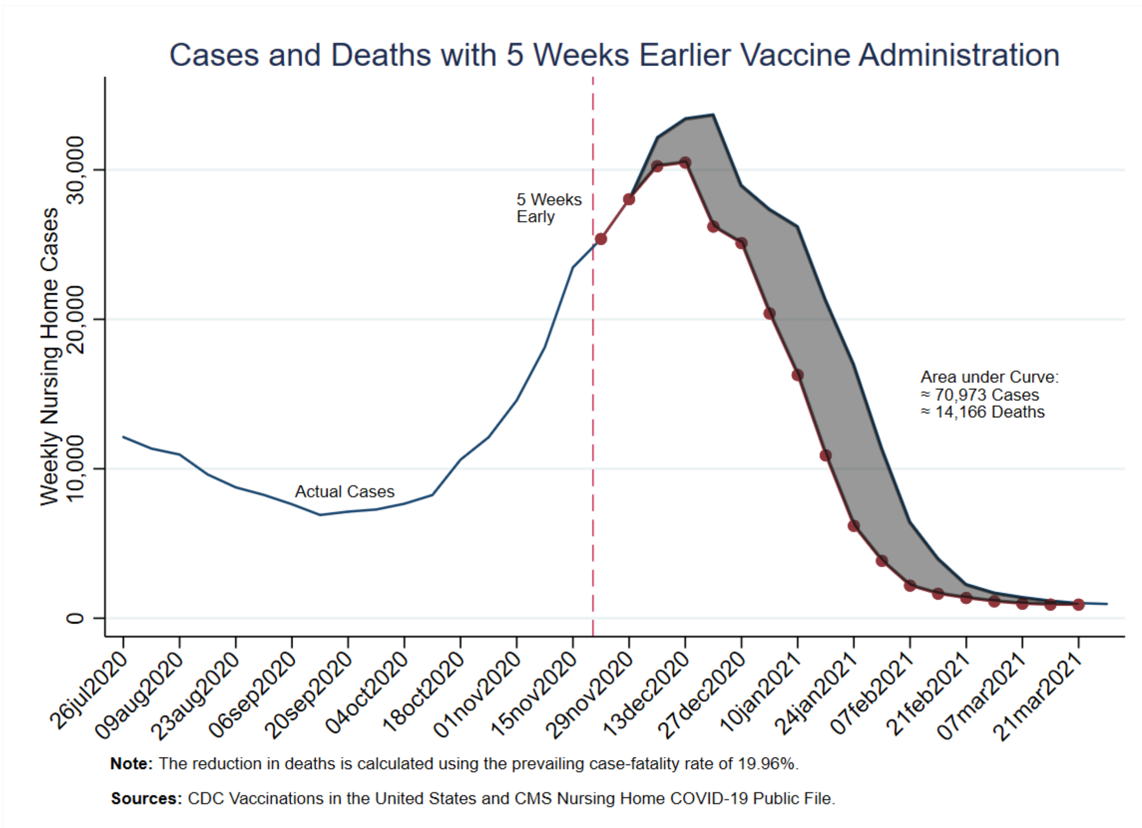

Figure 2.11. Cases and Deaths with 5 Weeks Earlier Vaccine Administration

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[56] Specifically, we assume 1st doses have zero effect until 14 days have passed, at which point they are 90% as effective as our measure of prior natural infection, which recall likely include some false positives as well as waning protection. We further assume doses are given equally to residents with and without prior exposure, and that vaccinating a resident with prior exposure effectively raises the stock of immune residents by 1/10th of a resident.

This exercise suggests moving the vaccine program up 5 weeks could have prevented 70,973 nursing home cases, which, at the prevailing case-fatality rate of 19.96%, would translate to about 14,166 fewer deaths. While we have noted several limitations of this approach, we think 14 thousand lives is a conservative estimate of the number of lives that could have been saved had this policy been carried out.

We now repeat the exercise with administration moved up 10 weeks. It's unlikely that testing could have concluded 10 weeks earlier but it's quite possible that nursing homes could have been offered vaccines 10 weeks earlier. Deborah Birx, the coordinator of the White House Coronavirus Task Force, forcefully advocated that nursing home residents should be given the option of being vaccinated earlier under a compassionate use authorization (Borrell, 2022). Many other treatments such as convalescent plasma were authorized under compassionate use procedures and there was more than enough vaccine available to vaccinate all nursing home residents.

Earlier vaccination has a larger potential impact as it could prevent more of the exponential growth we saw in November, but also because a given number of doses would do more to increase immunity when there is less natural immunity. As a first approximation we find the Birx plan would have prevented on the order of 200,000 nursing home cases and 40,000 nursing home deaths. To put that in perspective, the Birx plan would have reduced overall nursing home COVID deaths by almost 30% (using all CMS reported resident nursing home deaths as of December 5, 2021).[57]

[57] A final caution about these scenarios is that if an EUA was limited to nursing home residents only, and did not include staff members, we might overstate the benefits of moving administration up somewhat, as staff members were being vaccinated as part of the Pharmacy Partnership for Long-Term Care. The caveat to that is that staff vaccination rates, especially early on, were much lower than that for residents. As late as July 18, 2021, vaccination rates among Certified Nurse Aides working in nursing homes were still

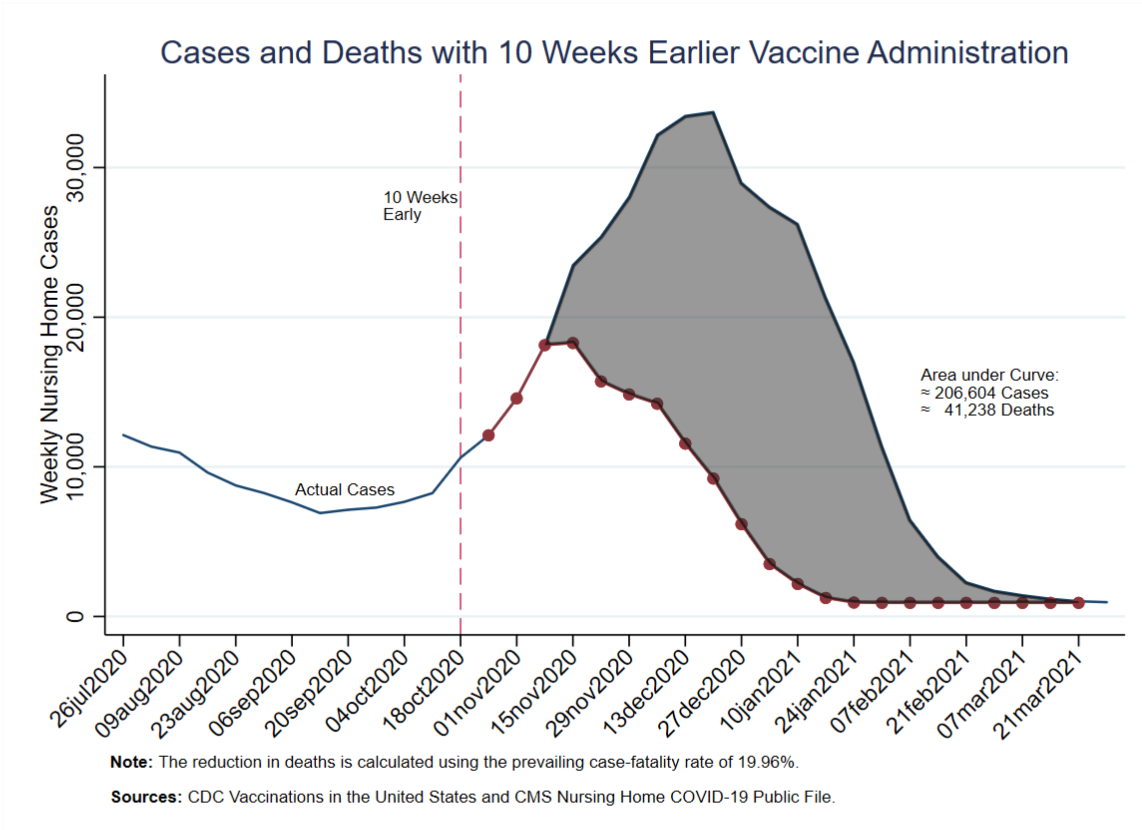

Figure 2.12. Cases and Deaths with 10 Weeks Earlier Vaccine Administration

When a virus is spreading exponentially, faster vaccine approval and administration can have enormous benefits, especially when the vaccine can be targeted to high-risk populations. Structuring our regulatory system towards speed and targeting it on high-risk populations, would far outweigh the other sacrifices we made for vulnerable nursing home residents, of which there were many.

below 50% nationwide (McGarry et al., 2021b), so for many types of direct care staff vaccine coverage was likely quite low in January and February.

Conclusions

It became clear early on that COVID was especially deadly to the aged and the infirm. In response, the United States implemented a policy of nursing home isolation and testing, in addition to extensive lockdowns and non-pharmaceutical interventions in society at-large. Judged by inputs, the policy was reasonably successful. Nursing homes were isolated and nursing home residents and staff were extensively tested. Nevertheless, judged by outputs, focused protection mostly failed. A large percentage of the total deaths from COVID in the United States came from nursing homes, especially in 2020.

Focused protection without extensive non-pharmaceutical interventions elsewhere would almost certainly have resulted in more deaths, both in nursing homes and elsewhere. Moreover, nursing homes were the ideal case for a strategy of focused protection. In a future pandemic it could be the young or the middle-aged who are most at risk, making focused protection more difficult and less likely to succeed.

Government policies could have been better but even the highest quality nursing homes, as measured by pre-COVID ratings, failed to offer much additional protection. If it existed, a successful strategy of focused protection was out-of-sample. The only exception was vaccines. Vaccines were by far the most successful intervention. A modest increase in the speed of vaccine distribution of five weeks would have saved on the order of 14 thousand lives and the Birx plan to offer vaccines on a compassionate-use basis could have saved 40 thousand lives.

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Appendix to Chapter 2

Section A

Table A2.1. Plotted Estimates from Figure 2.6

Predicted Resident COVID-19 Cases and Deaths by Star Rating.

	Cases March-May	Cases May-Aug	Cases Sept-Dec	Cases Post Vaccine	Deaths March-May	Deaths May-Aug	Deaths Sept-Dec	Deaths Post Vaccine
*	7.50	10.30	18.23	16.04	1.85	2.17	2.77	3.02
	(.401)	(.446)	(.526)	(.437)	(.108)	(.112)	(.105)	(.116)
**	7.62	9.56	18.67	16.14	2.04	2.19	2.89	3.11
	(.42)	(.384)	(.497)	(.373)	(.112)	(.112)	(.0962)	(.114)
***	6.91	8.85	19.11	16.94	1.80	1.90	2.86	3.17
	(.443)	(.403)	(.477)	(.419)	(.103)	(.105)	(.0975)	(.113)
****	6.97	8.73	18.88	16.63	2.07	1.85	3.13	3.17
	(.369)	(.371)	(.452)	(.366)	(.118)	(.092)	(.108)	(.107)
*****	6.22	7.67	17.60	15.88	1.97	1.56	3.03	2.98
	(.341)	(.352)	(.464)	(.336)	(.102)	(.0792)	(.0989)	(.104)
Observations	14008	14808	14860	14792	14008	14808	14860	14792

Note: Adjusted predictions and (95%) confidence intervals estimated using zero-inflated negative binomial regression of total cases and deaths during each period on pre-pandemic star ratings. Standard Errors clustered by county. Estimates adjusted for the log number of beds, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. The first (5th) column shows cases before May 24, 2020, the second (6th) between May 25 and August 30, 2020, the third (7th) between Sept. 1 and December 27, 2020, and the fourth (8th) from December 28, 2020, to December 5th, 2021.

Even when we consider the most generous hypothetical; a prospective resident choosing between a 1- and a 5-star rated facility, we find no effect prior to May 24, 2020 or from December 28, 2020 to December 5, 2021.

We find modest evidence of fewer deaths when we compare facilities rated 1- vs 3- to 5 star, in the May-August “mandatory-reporting, optional test” period. Our estimated differences (standard errors) for this period are -0.26 (.129) deaths for facilities rated 3-star, -0.31 (.138) for facilities rated 4-star, to -0.61 (.134) for facilities rated 5-star, relative to 1-star facilities (see Table A2.2a). The estimates are statistically significant at the 5% level for 3 and 4-star facilities, and at the 1% level for 5-star facilities.

Importantly, these differences were short lived. Our estimates for September-December (2020) finds that homes rated 4 and 5 stars actually had modestly *higher death counts* relative to those rated 1-star. Our estimates range from 0.35 (.154) additional deaths for facilities rated 4-stars, to 0.25 (.146) additional deaths for facilities rated 5 stars, relative to 1-star facilities during this period (see Table A2.2a). These estimates are statistically significant at the 5% level for 4-star facilities and at the 10% level for 5-star facilities.[58]

The lower death counts for higher rated facilities from May-August (2020), roughly cancel out the somewhat higher death counts from September to December. Considering we found no evidence of a relationship in the other two periods, we therefore conclude that, on

[58] The Special Focus Facility (SFF) program targets additional oversight toward some of the worst performing facilities in each state and have suppressed ratings on Nursing Home Compare, and were therefore not included in analyses by other authors such as (Williams et al.,). When we run the same analysis including SFF facilities and rate them as 1-star, we find the results are qualitatively the same (results not shown). For more on the SFF program see working paper “Do Patients Benefit from Regulatory Stringency? Evidence from Targeted Nursing Homes,'' Bjoerkheim 2021.

balance, higher quality homes (as measured by star ratings) did not provide consumers a reliable prediction of lower death counts from COVID-19.

Table A2.2a tests whether outcomes were different in facilities rated 2, 3, 4, and 5 stars compared to those rated 1-stars.

Table A2.2a. Resident COVID-19 Cases and Deaths Contrasted by Star Rating

Resident COVID-19 Cases and Deaths Contrasted by Star Rating.

	Cases March-May	Cases May-Aug	Cases Sept-Dec	Cases Post Vaccine	Deaths March-May	Deaths May-Aug	Deaths Sept-Dec	Deaths Post Vaccine
Rated 2 vs. 1	0.12	-0.74	0.44	0.10	0.19	0.03	0.12	0.10
	(.516)	(.503)	(.593)	(.518)	(.14)	(.14)	(.134)	(.149)
Rated 3 vs. 1	-0.59	-1.45	0.88	0.89	-0.05	-0.26	0.09	0.15
	(.492)	(.509)	(.618)	(.506)	(.133)	(.129)	(.14)	(.145)
Rated 4 vs. 1	-0.52	-1.57	0.65	0.59	0.22	-0.31	0.35	0.15
	(.48)	(.537)	(.645)	(.538)	(.154)	(.138)	(.154)	(.147)
Rated 5 vs. 1	-1.28	-2.63	-0.63	-0.16	0.12	-0.61	0.25	-0.03
	(.475)	(.507)	(.683)	(.503)	(.132)	(.134)	(.146)	(.153)

Note: Model estimated using zero-inflated negative binomial regression of total cases and deaths during each period on pre-pandemic star ratings. Standard Errors clustered by county. Estimates adjusted for the log number of beds, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. The first (5th) column shows cases before May 24, 2020, the second (6th) between May 25 and August 30, 2020, the third (7th) between Sept. 1 and December 27, 2020, and the fourth (8th) from December 28, 2020, to December 5th, 2021.

Table A2.2b: Alternative Specification: Incidence Interacted with Rating

Resident COVID-19 Deaths Contrasted by Star Rating, Alternative Specification: Incidence Interacted with Rating

	(1) Deaths March-May	(2) Deaths May-Aug	(3) Deaths Sept-Dec	(4) Deaths Post Vaccine
Rated 2 vs. 1	0.23	0.02	0.10	0.11
	(.15)	(.138)	(.133)	(.148)
Rated 3 vs. 1	-0.05	-0.25	0.04	0.16
	(.133)	(.13)	(.138)	(.145)
Rated 4 vs. 1	0.23	-0.31	0.31	0.16
	(.156)	(.138)	(.151)	(.146)
Rated 5 vs. 1	0.13	-0.64	0.20	-0.02
	(.133)	(.132)	(.144)	(.152)

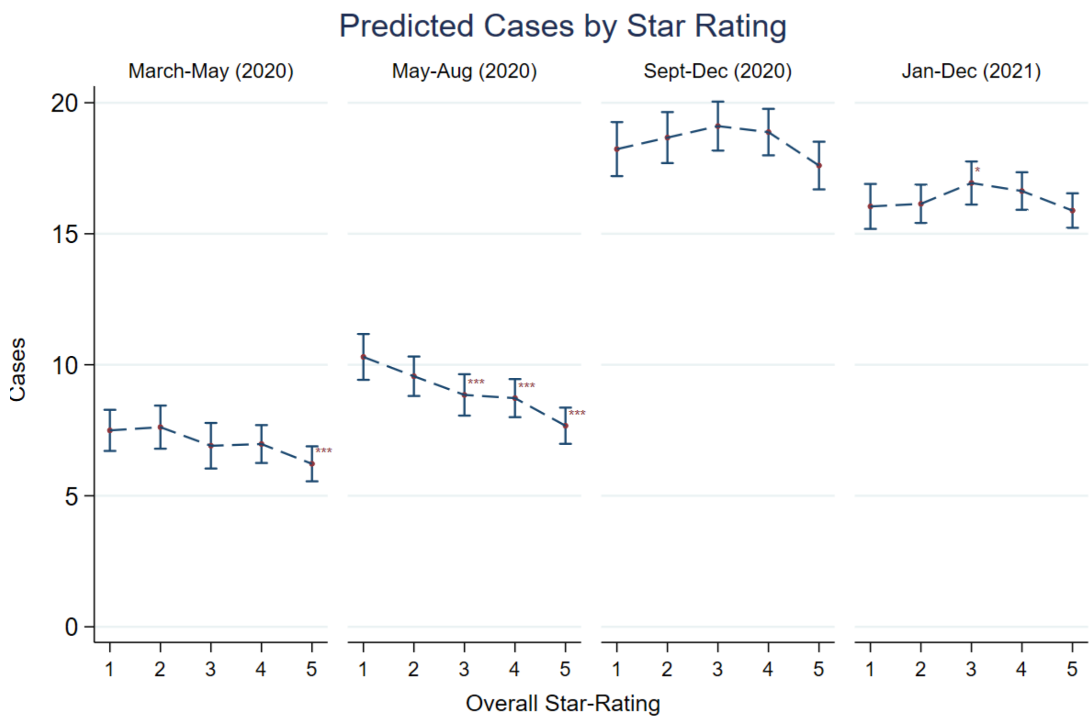
Note: Model estimated using zero-inflated negative binomial regression of total cases and deaths during each period on pre-pandemic star ratings. Standard Errors clustered by county. Estimates adjusted for the log number of beds, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. The first column shows cases before May 24, 2020, the second between May 25 and August 30, 2020, the third between Sept. 1 and December 27, 2020, and the fourth from December 28, 2020, to December 5th, 2021.

Table A2.2c: Alternative Specification: Include squared term of Incidence

Resident COVID-19 Deaths Contrasted by Star Rating, Alternative Specification: Include Incidence Squared

	(1) Deaths March-May	(2) Deaths May-Aug	(3) Deaths Sept-Dec	(4) Deaths Post Vaccine
Rated 2 vs. 1	0.19	0.02	0.12	0.10
	(.138)	(.14)	(.135)	(.149)
Rated 3 vs. 1	-0.04	-0.26	0.09	0.15
	(.13)	(.129)	(.141)	(.146)
Rated 4 vs. 1	0.21	-0.31	0.35	0.15
	(.15)	(.137)	(.154)	(.148)
Rated 5 vs. 1	0.09	-0.60	0.25	-0.03
	(.129)	(.132)	(.146)	(.153)

Note: Model estimated using zero-inflated negative binomial regression of total cases and deaths during each period on pre-pandemic star ratings. Standard Errors clustered by county. Estimates adjusted for the log number of beds, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), a squared term for local disease prevalence, and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. The first column shows cases before May 24, 2020, the second between May 25 and August 30, 2020, the third between Sept. 1 and December 27, 2020, and the fourth from December 28, 2020, to December 5th, 2021.



Note: Adjusted predictions and 95% confidence intervals estimated using zero-inflated negative binomial regression of total cases during each period on pre-pandemic star ratings. Significance stars show estimate is statistically different to homes rated 1-Stars, at 1% (***), 5% (**), and 10% (*) levels. Standard Errors clustered by county. Estimates adjusted for the log bedcount, NCHS Urban-Rural classification (six categories), local immunity prior to the period (measured as cumulative COVID-19 cases as a share of the county population), local disease prevalence (measured as cases as a share of the county population during the period), and local socioeconomic factors using the county's Area Deprivation Index. The estimates from 2021 also control for the county's average vaccination rate during the period. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables.

Figure A2.1. Predicted Cases by Star Rating

Section B

Below we plot average weekly tests per residents by test type and star rating. With the exception of resident-antigen tests (upper left graph), where there are virtually no differences, there's a clear pattern among the three other types of tests that higher rated facilities ran more tests. In fact, average test levels generally follow the ratings, i.e. 5-star levels are generally higher than 4-star, which are higher than 3-star rated facilities, etc.

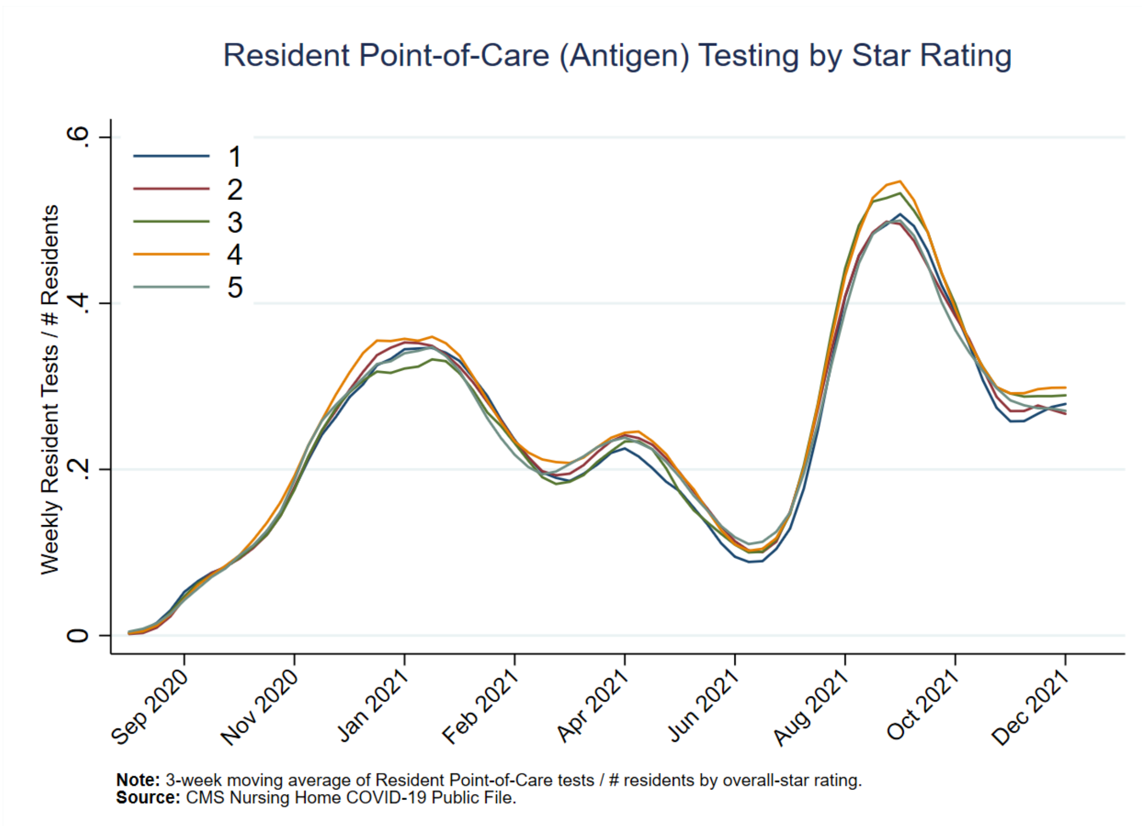


Figure A2.2. Resident Point-of-Care (Antigen) Testing by Star Rating

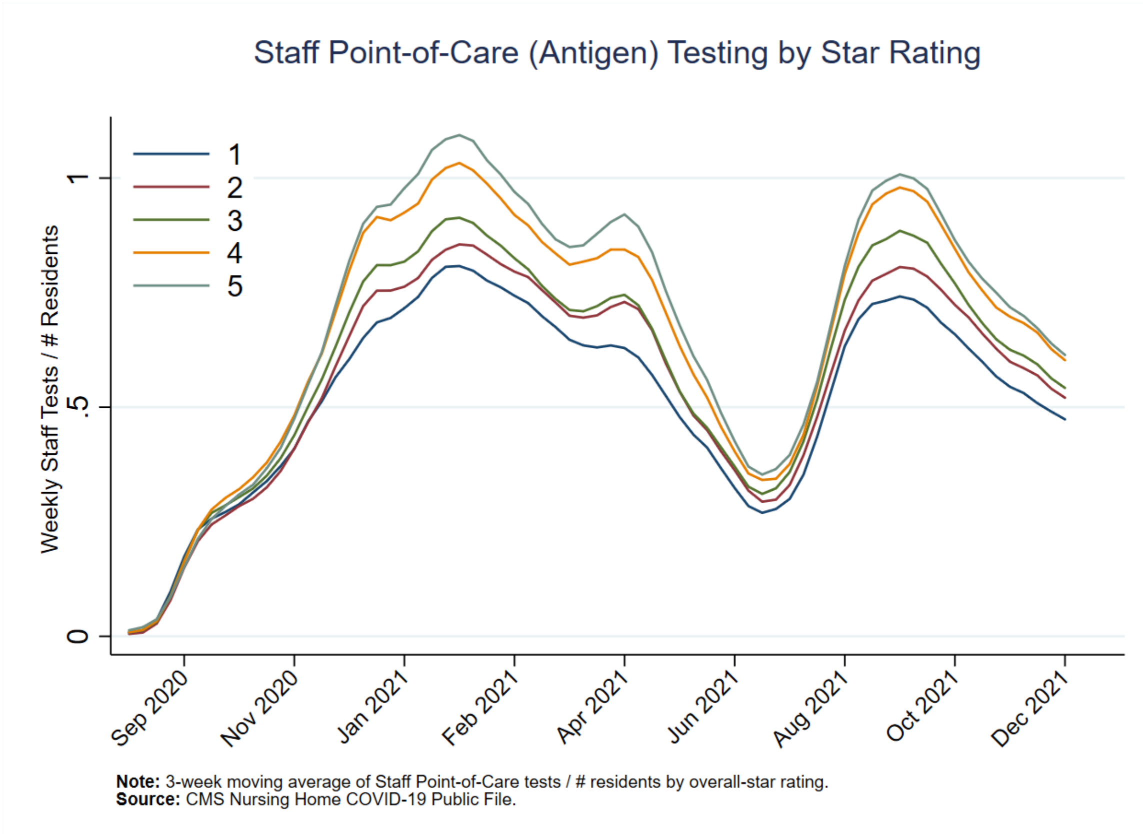


Figure A2.3. Staff Point-of-Care (Antigen) Testing by Star Rating

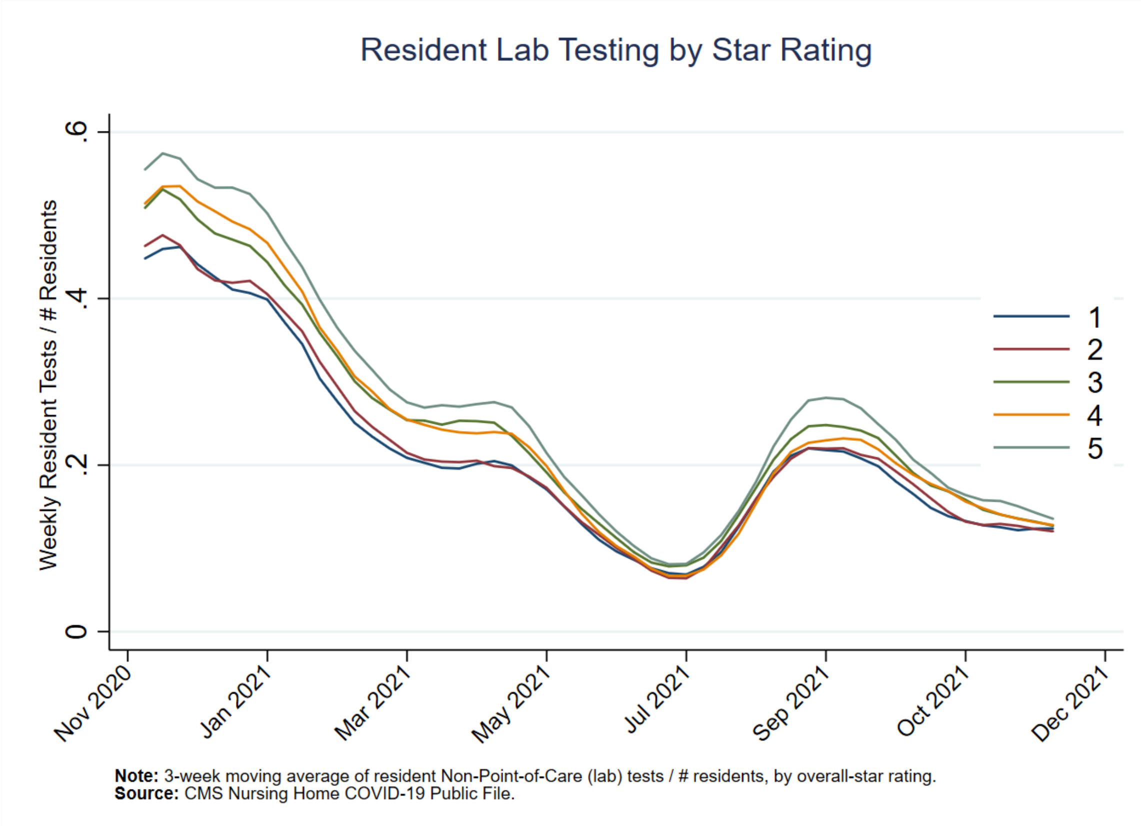


Figure A2.4. Resident Lab Testing by Star Rating

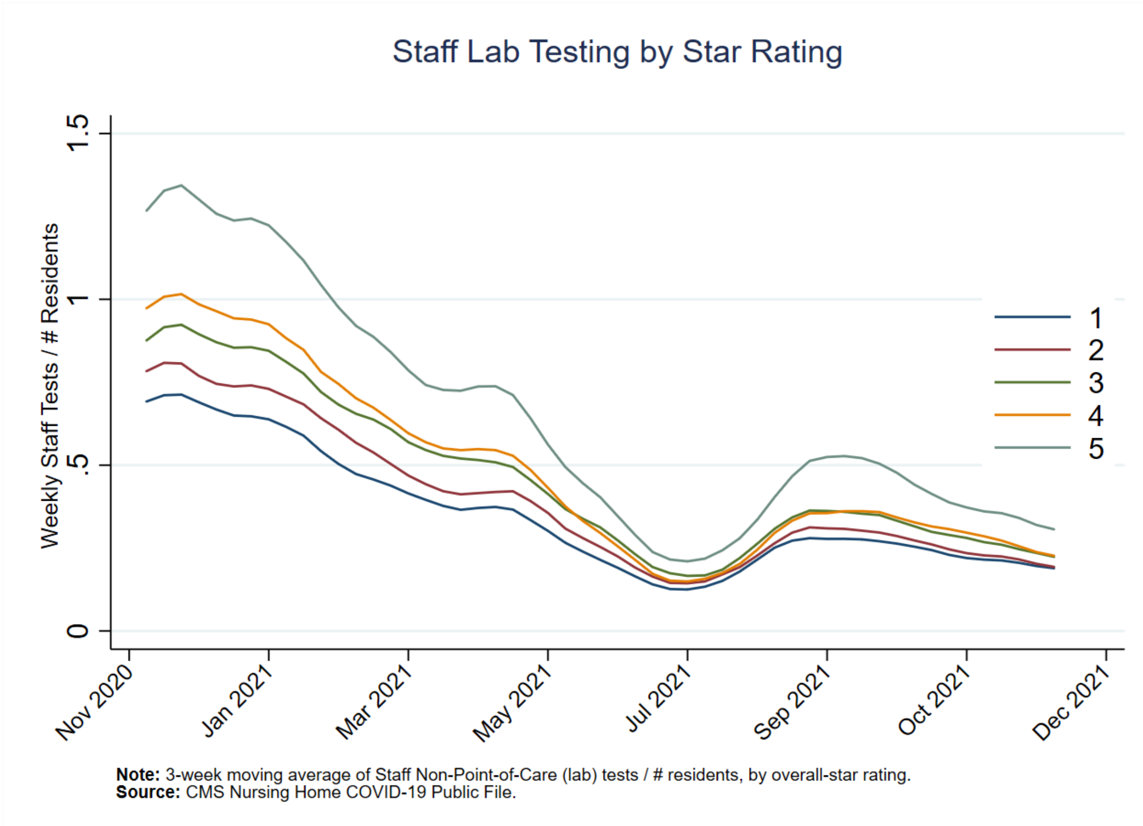


Figure A2.5. Staff Lab Testing by Star Rating

Section C

As it is possible that our main estimates, which uses the entire country, overestimates the cost of reducing deaths relative to a Great Barrington scenario where community spread is higher, we here re-run the same analysis restricted to the counties with high community spread.

Our estimate for one additional resident-antigen per week is now quite large, at -3.54 deaths it implies 2 additional antigen tests per resident each week would bring safety levels near that of a year-long flu-season, but keep in mind that this is outside-of-sample. We now find staff-antigen tests to be significant too, with one additional staff test per resident-week predicted to prevent 0.96 deaths. While the point-estimates did increase, the cumulative impact of these variables remains modest, explaining about one and a quarter (1.22) of the 10 deaths difference between the groups.

Table A2.4. Average Marginal Effect on COVID-19 Deaths

Average Marginal Effect on COVID-19 Deaths.

	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)	(dy/dx)
Weekly Resident Antigen Tests/Residents	-3.54***					
	(.852)					
Weekly Staff Antigen Tests/Residents		-0.96**				
		(.352)				
Weekly Resident Lab Tests/Residents			-0.37			
			(.668)			
Weekly Staff Lab Tests/Residents				0.28		
				(.282)		
Residents Total Admissions COVID-19					0.04***	
					(.00767)	
Hospital based (%)						-0.02
						(.00966)
Observations	1281	1281	1252	1252	1285	1285

Note: Each estimate (standard errors) is estimated using zero-inflated negative binomial regression of total COVID-19 resident-deaths as of February 28, 2021, on the variable of interest and a fixed set of controls. Estimates adjusted for the log number of beds, facility age (in years), NCHS Urban-Rural classification (six categories), local disease prevalence measured as cumulative COVID-19 cases as a share of the county population, and local socioeconomic factors using the county's Area Deprivation Index. The count portion include an offset term for the log number of resident-weeks. The zero-portion is a logit model with the same control variables. Standard Errors clustered by county.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 3. Did Expanding Unemployment Insurance Benefits Discourage Work During the Pandemic? Evidence from Nursing Homes

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Abstract

The generosity and coverage of unemployment insurance increased dramatically with the March 2020 passing of the CARES Act. This paper investigates whether these expansions discouraged nurses and nurse aides from returning to work in the nursing home industry. Using variation from 24 states that withdrew from the programs in a difference-in-difference design, I find suggestive evidence the withdrawals reduced facility-reported shortages for nurses and nurse aides by between 0.5-2 percentage points (3-11%). Event-study specifications using physicians, physician assistants, and advanced practice nurses find no effect. Placebo tests using states that intended to withdraw but were ordered to remain are inconclusive.

Introduction

Unemployment Insurance (UI) is the most important federal program for unemployed workers, providing temporary income support after qualifying job losses. Whether, and the extent to which, the unemployment insurance discourages workers from returning to work has generated considerable attention among economists and policy makers, but attempts to estimate this relationship have often been limited to relatively modest cross-state variation in benefit levels or duration (Meyer, 1990). The COVID-19 pandemic has provided an opportunity to re-examine this question.

The generosity and eligibility of Unemployment Insurance (UI) changed dramatically in the U.S. with the March 2020 passing of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, and subsequent pandemic relief bills. This paper starts an investigation into whether the increased generosity discouraged workers from taking jobs in the nursing home industry, focusing on the Federal Pandemic Unemployment Compensation (FPUC) program, a $300 weekly bonus that doubled replacement rates, and the Pandemic Emergency Unemployment Compensation (PEUC) program, which provided up to 49 weeks of additional UI benefits.

Nursing homes are in many ways an ideal place to study this question. Like other more familiar settings such as fast food chains, turnover in nursing homes exceed 100% annually (Gandhi et al., 2021), which means nursing homes are always hiring and changes in labor market conditions are quickly reflected in weekly data available for all U.S. nursing homes.

Further, nursing homes employ workers across the income distribution ranging from nurse aides and licensed practical nurses (LPNs), to registered nurses (RN's), physicians, and physician-assistants. While theory suggests a fixed dollar amount of $300 per week would have differential impact on each group, they face many of the same non-pecuniary costs, allowing within-facility comparisons not typically available in other data sources.

Finally, the question also has important implications for nursing home operators and policy makers concerned with staff shortages in nursing homes and impacts on resident health. A September 2021 survey by the American Health Care Association found that 71% of respondents reported unemployment benefits among the "biggest obstacles" preventing nursing homes from hiring more staff.[59] The health implications from foregoing alternative policy efforts if this is not actually driving staff shortages are likely significant, given the well-established contributions staff make for resident health outcomes.[60]

Using variation from 24 states that withdrew from CARES Act participation in the summer of 2021 in a difference-in-difference setting, I estimate the effects on nursing home staff shortages. Comparing states that withdrew to states that remained, event-study specifications suggest withdrawal from FPUC and PEUC reduced facility-reported labor shortages for nurse aides by almost 1 percentage point (5%), and 0.9 percentage points for nurses (5%), though these estimates are somewhat imprecise. Event-studies using physicians, physician assistants, and advanced practice nurses, workers for whom the variations in generosity induced by FPUC and PEUC would have minimal impact, find precisely estimated null effects.

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[59] For more on the survey see www.ahcancal.org/News-and-Communications/Fact-Sheets/FactSheets/Workforce-Survey-September2021.pdf

[60] See for instance Friedrich and Hackmann (2021) who studies a parental-leave program in Denmark that led to shortages for nurses in both hospitals and nursing homes and found that a 1% reduction in nurse (RN) employment increased resident mortality by 1.9%.

Placebo tests using Maryland and Indiana, states that announced their intention to withdraw but were forced to continue providing benefits by court orders, are inconclusive due to signs of pre-trends. The same is true for a similar placebo test using South Dakota.

I also estimate traditional fixed effects specifications with mixed results. While the estimated effects are in the expected direction, and generally larger than event-study specifications, only some specifications are significant. Comparing these estimates across different worker types does generally not conform to expectations of the differential impact the policies should have on different workers.

This paper adds to the emerging evidence surrounding the pandemic era unemployment programs, and their potential to discourage workers from returning to work. Holzer et al., (2021) found this among the general population using the Current Population Survey, and Coombs et al., (2021) found a similar result using bank transactions of low-income, credit constrained individuals.

The next section describes the CARES Act Unemployment Programs. Section 3 describes the data sources and provides summary statistics, while section 4 describes the results. Section 5 concludes. Placebo tests and other ancillary evidence is in the Appendix to Chapter 3.

CARES Act Unemployment Programs

Congress passed several temporary unemployment programs in response to the COVID-19 pandemic, first through the CARES Act of March 2020, and later extended them in the Consolidated Appropriations Act, and finally through the American Rescue Plan Act (ARPA) of

2021.[61] The programs were administered by the states but were funded entirely by the federal government (both benefits and administrative costs were covered).

The focus of this paper is the withdrawal of two such programs, the Federal Pandemic Unemployment Compensation program (FPUC), which at the time of the withdrawals supplemented UI recipients with $300 weekly (in addition to regular benefits), and the Pandemic Emergency Unemployment Compensation (PEUC), which provided extended UI benefits for an additional 49 weeks.[62]

[61] H.R.748 - 116th Congress (2019-2020): CARES Act, H.R.748, 116th Cong. (2020), www.congress.gov/bill/116th-congress/house-bill/748. H.R.133 - 116th Congress (2019-2020): Consolidated Appropriations Act, 2021, H.R.133, 116th Cong. (2020), www.congress.gov/bill/116th-congress/house-bill/133. H.R.1319 - 117th Congress (2021-2022): American Rescue Plan Act of 2021, H.R.1319, 117th Cong. (2021), www.congress.gov/bill/117th-congress/house-bill/1319.

[62] PEUC originally extended benefits for 13 weeks for recipients that exhausted regular UI benefits, however, the program was extended multiple times. By the time ARPA was passed, PEUC generally offered 49 additional weeks of coverage, and in some cases up to 53 weeks.

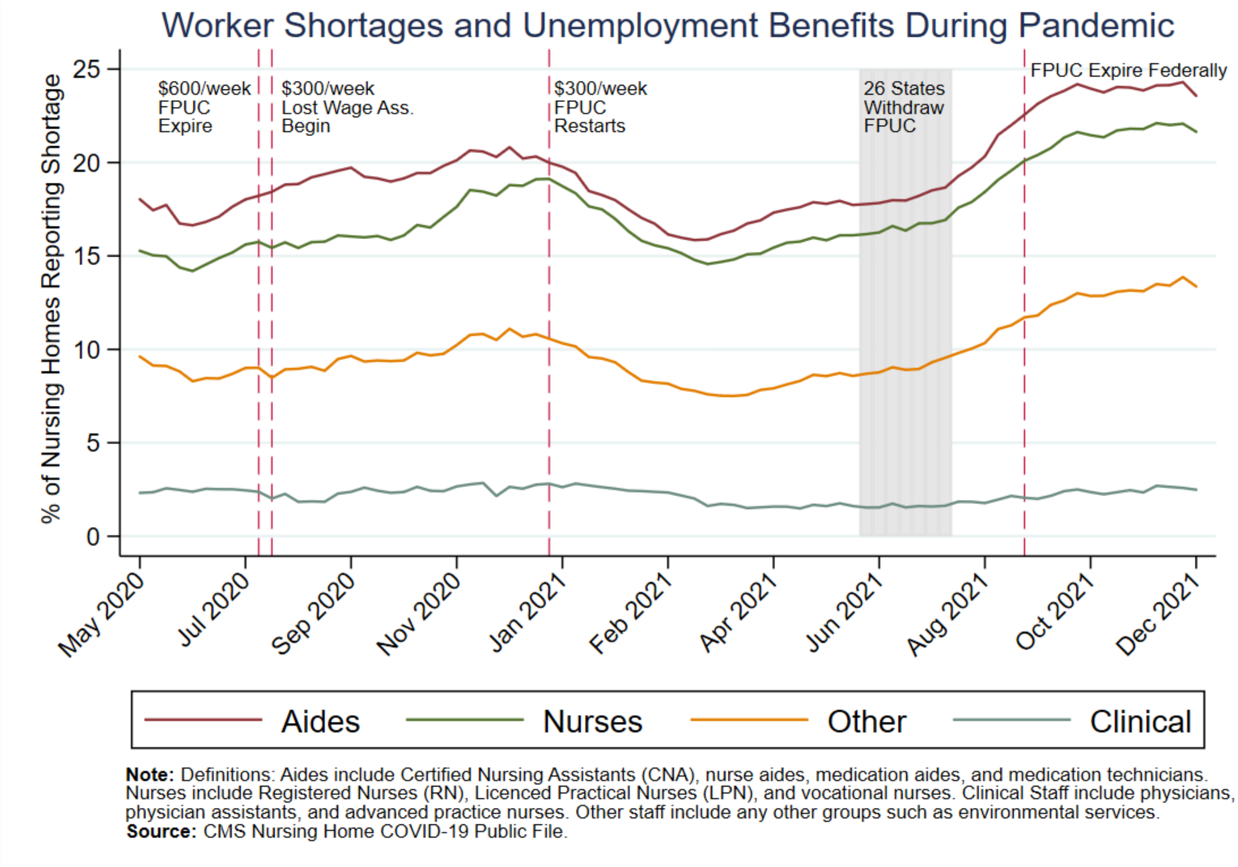


Figure 3.1. Raw data

The FPUC "bonus" was initially $600 weekly from March 27 through July 25, 2020. Starting August 8, 2020, the bonus was continued for an additional 6 weeks at a reduced rate of $300 per week through Lost Wages Supplemental Payment Assistance from the Federal Emergency Management Agency's Disaster Relief Fund. FPUC was reinstated with the passing of the Consolidated Appropriations Act on December 26, 2020 (still at $300 weekly) and was extended a final time when the American Rescue Plan (ARPA) passed in March 2021, until the first week of September 2021 when they expired nationally. These changes are displayed graphically in Figure 3.1 along with nationwide shortages by worker type.

The median replacement rate, the fraction of previous earnings a UI recipient received, more than tripled initially with the addition of a $600 weekly bonus, from around 40% before the pandemic, to 145% during April-July 2020 (Bateman, 2020; Ganong et al., 2020). A reasonable approximation is therefore that the $300 weekly bonus corresponded to roughly a doubling in the replacement rate.

While every state provided the additional $300 per week after the passing of ARPA, 26 states announced their withdrawals in short succession in the beginning of May 2021. States were required to give the Department of Labor 30-day notice before terminating the agreements, so termination dates range– from June 12 - August 3, 2021, with over 20 states terminating in June.

Two states, Maryland and Indiana, announced withdrawals, but was later mandated to continue paying benefits by court orders. I therefore exclude these two states from the main analysis but analyze them separately as placebo-events in the Appendix to Chapter 3.

There are several channels the withdrawals could influence labor force decisions through. First, because all 24 states withdrew from FPUC, weekly benefits for around 1 million current (and still eligible) recipients were reduced by $300, which could incentivize workers to return to work faster. Second, 20 of the 24 states that withdrew from FPUC also withdrew from PEUC, which meant extended benefits expired entirely for about 2 million workers, which could induce these workers to return faster (Coombs et al., 2021).

I focus on the withdrawals themselves, but it is worth noting that a state's announcement of the intention to withdraw could induce beneficiaries to change their behavior now, anticipating reduced benefit levels and durations in the future. If these anticipation effects were important, we would expect to see this reflected as a break in the pre-trends of event-study specifications around 4 weeks prior to the withdrawals. I find little evidence of this (see Figure 3.3-3.4), so I proceed defining the treatments using the withdrawal dates but note that future work could test this more formally using the announcement dates.

While these are likely the most important channels, I will also note that there were two additional UI programs that most states withdrew from at the same time, PUA, which provided UI benefits for workers not usually covered (independent contractors, gig-economy workers, etc.), and the mixed-earner program, which provided \$100 weekly to certain formerly self-employed individuals.[64] These are unlikely to be important as few independent contractors or self-employed individuals work in nursing homes, but I will still note this as a possible limitation, as the results could reflect these programs as well.[65]

Generally, UI does not cover individuals who voluntarily quit their job or get fired for cause. Therefore, if UI discourage workers from working in nursing homes, the primary channel would be through reduced hiring. While this would effectively limit the feasibility of studying this question in most industries, this is not the case for nursing homes. Recent research has shown that turnover among nursing home staff averages over 100% annually; ranging from 114% for Licensed Practical Nurses, 129% for Certified Nurse Aides, to 140% for Registered Nurses (Gandhi et al., 2021). Nursing homes are therefore constantly hiring and would thus

[64] The Pandemic Unemployment Assistance program (PUA) provided up to 75 weeks of UI benefits for individuals not otherwise eligible (self-employed, independent contractors, “gig-economy” workers, that were unable to work for certain COVID-19 specific reasons and were unable to telework). The Mixed Earner Unemployment Compensation provided an additional \$100 weekly for claimants that had \$5,000 in self-employment income in the most recent taxable year.

[65] Finally, I’ll note a few other details from the UI programs that changed during the pandemic. While UI recipients are traditionally required to actively search for a job, the CARES act required states to relax this requirement. Further, in 2020, most taxpayers who received UI benefits could exclude \$10,200 from their taxable income, but the tax treatment of UI benefits returned to normal in 2021.

experience disincentive effects relatively quickly. This turnover also limits the potential concern that treatment effects would vary significantly over time (Goodman-Bacon, 2018).

Data

The primary data source for this paper is the CMS Nursing Home COVID-19 Public File and I also use weekly county-level COVID-19 cases from The COVID Tracking Project (2021). The underlying nursing home data comes from the CDC Long Term Care Facility Module's section on Personnel and Staff Impact, reported weekly through the National Healthcare Safety Network (NHSN). The module asks "Does your organization have a shortage of staff and/or personnel?" and the respondent can indicate "Yes/No" for "Nursing Staff," "Clinical Staff," "Aide," and "Other staff or facility personnel." The form is reproduced below for simplicity.

NHSN
NATIONAL HEALTHCARE SAFETY NETWORK

OMB Approved
OMB No. 0920-1209
Exp. Date 01/31/2024
www.cdc.gov/nhsn

Does your organization have a shortage of staff and/or personnel?	
Staffing Shortage?	**Staff and Personnel Groups**
□ YES □ NO	**Nursing Staff:** registered nurse, licensed practical nurse, vocational nurse
□ YES □ NO	**Clinical Staff:** physician, physician assistant, advanced practice nurse
□ YES □ NO	**Aide:** certified nursing assistant, nurse aide, medication aide, and medication technician
□ YES □ NO	**Other staff or facility personnel**, regardless of clinical responsibility or resident contact not included in the categories above (for example, environmental services)

Figure 3.2. Staff Shortage Questions

CMS required all certified facilities to complete the module on a weekly basis, starting in May 2020. As this was a new requirement, the initial releases of the data did include some missing data and some inconsistencies. I therefore start the sample in July 2020. The shortage questions have since had a response rate above 98% and have not been subject to any changes in the wording or methodology. Following Callaway and Sant'Anna (2020), I end the sample the week of September 5, 2021, when the programs expire federally.

In Table 3.1 the module questions on shortages are matched to estimates of employment and wage statistics from the Bureau of Labor Statistics.

Table 3.1. CMS Questions and Occupational Employment and Wage Statistics

CMS Question	BLS Occupation	# of employees	Hourly wage (median)	Annual (mean)
Nurse Aides	Nursing Assistants	527,480	$14.48	$31,000
Nursing Staff	Registered nurse (RN)	143,250	$33.13	$72,090
Nursing Staff	Licensed Practical (LPN) and Vocational Nurses	199,760	$24.11	$51,200
Clinical Staff (physician, physician assistant, advanced practical nurse)	Physicians (family medicine, general internal medicine, and all other except pediatric)	410-33,087[66]	$81.04-$96.89	$157,760-$222,350
Other staff (ex. Environmental services)	Building and Grounds Cleaning and Maintenance	83,880	$12.34	$27,630

Sources: See Nursing Home COVID-19 Public File for the CMS questions and May 2020 National Industry-Specific Occupational Employment and Wage Estimates fo– NAICS 623100 - Nursing Care Facilities (Skilled Nursing Facilities), Bureau of Labor Statistics, for employment and wage figures for the nursing home industry. See www.bls.gov/oes/current/naics4_623100.htm#29-0000 for more.

Consider a Nurse Aide and a registered nurse (RN) in Nevada, a state with a statutory replacement rate of 52%, cap $469 weekly benefits, and which did not pass statewide hazard-pay or other incentives for nursing home workers. Prior to being laid off the workers earned the national average for their occupations ($31,000/year for the Nurse Aide and 72,000/year for the RN).

[66] Few physicians are employed directly by the nursing home, therefore the BLS estimate of 410 physicians is very low. Ryskina et al., (2017) report 33,087 physicians billed Medicare Part B for nursing home-based care in 2015, of which 6,857 were “nursing home specialists” who had 90% or more of claims relating to nursing home care.

If the nurse aide was laid off prior to the CARES Act, he or she would receive $310 weekly in UI benefits, 52% of the previous weekly earnings of $596. The $300 weekly bonus, which importantly did not count towards the $469 cap, means that after the CARES Act the nurse aide would receive $610 weekly in UI benefits, 102% of the previous $596 weekly earnings.

Now consider the same scenario for the RN. Before the CARES Act the RN would receive the cap of $469 per week, or 34% of her previous weekly earnings. The additional $300 weekly increases the RN's benefits so that she now receives $769, however, the increase is much smaller in percentage terms, and only replaces about 55% of her previous weekly earnings.

This simple example shows the differential impact the UI bonus payments could have for the incentives to return to work. We therefore hypothesize that state withdrawal should have the largest impact on nurse aides and less impact on nurses. Clinical staff (physicians, physician assistants etc.) function almost as placebos in that UI withdrawal should have very little, if any, impact on them.

Methods

As discussed I define the treatment as occurring when a state withdraws from the UI programs and estimate the effect of UI withdrawal using difference-in-differences ordinary least squares regressions with facility and week-fixed effects. The standard regression equation is included below (1), but I also run specifications that control for COVID-19 cases on the county level (covid cases per 1000 county residents), as these are outside the nursing homes control and have been shown to influence shortages (Xu et al., 2020), and event-study specifications, shown in equation (2).

The dependent variable Y_{it} is an indicator for whether facility i is reporting a shortage in week t, γ_i and λ_t are fixed effects for facilities and weeks, and ε_{it} is the error term. In (1) $Withdrawal$ is an indicator variable that "turns on" the first week the state's withdrawal is in effect. The β coefficient will therefore, assuming the parallel trend assumption holds, reflect the average effect of the change in generosity for recipients that are still eligible (FPUC), and the exhaustion of benefits for as many as 2 million recipients (PEUC). In (2), β_t traces out the dynamic difference-in-difference estimates relative to the last period before the withdrawal (i.e., the last pre-withdrawal period is omitted). Standard errors are clustered on the state-level, the level of the treatmenI.

$$\mathrm{Y_{it}} = \gamma_{\mathrm{i}} + \lambda_{\mathrm{t}} + \beta(\mathrm{Withdrawal_{it}}) + \varepsilon_{\mathrm{it}} \qquad 1)$$

$$\mathrm{Y_{it}} = \gamma_{\mathrm{i}} + \lambda_{\mathrm{t}} + \sum\nolimits_{\mathrm{k \neq Last\ Week\ Pre-Withdrawal}} \beta_{\mathrm{t}} \left(\mathrm{I_{t=k}} * \mathrm{Withdrawal_i}\right) + \varepsilon_{\mathrm{it}} \qquad 2)$$

Recent work in theoretical econometrics has shown that the kind of fixed effects regressions in (1) can be biased, for instance in cases where the treatment happens at different times for different units (Callaway and Sant'Anna, 2020; Goodman-Bacon, 2018). While there is some variation in the timing of the withdrawals, ranging from June 12-August 3, most withdrawals occur in June, so it seems unlikely this limited variation would play a large role, however, future work could improve this.

Withdraw states tended to have Republican governors, which could suggest facilities in these states are inherently different from the "remain" states. Table 3.2 compares facilities in withdraw and remain states just prior to the withdrawal announcements and finds some evidence of level differences between facilities in the two states. Note however, that these differences are generally small and most of these variables are constant within each facility over the time we observe them, and thus will be controlled for using facility-fixed effects.

Table 3.2. Descriptive Statistics Before Withdrawal Announcements

Descriptive Statistics Before Withdrawal Announcements

	Remain	Withdraw	B	SE
Shortage of Nursing Staff	14.4	17.8	-3.41***	0.62
Shortage of Clinical Staff	1.78	1.44	0.34	0.21
Shortage of Aides	16.4	19.7	-3.28***	0.65
Shortage of Other Staff	7.63	9.19	-1.55***	0.47
COVID-19 Cases per 1000 (County)	1.09	0.76	0.33***	0.0098
Hospital based (%)	4.53	3.64	0.89**	0.33
For profit facility (%)	69.7	72.6	-2.83***	0.76
Non-profit facility (%)	25.5	20.6	4.90***	0.71
Government operated facility (%)	4.73	6.80	-2.07***	0.39
Facility age (years)	34.0	27.0	6.97***	0.21
Number of All Beds	111.0	101.0	10.0***	1.00
Occupancy rate (%)	0.72	0.68	0.048***	0.0030
Observations	14304			

Note: This table compare facilities in withdraw and remain states on key variables (outcomes, resident, facility, and county characteristics) on May 2nd, 2021, the last available period before the first withdrawal announcement were made on May 4, 2021.

To assess the plausibility of the parallel-trends assumption we follow common practice and compare the trends in staff shortages for Withdraw and Remain states. I do this first using the raw-data and simple group averages, and then using pre-withdrawal coefficients from the "event-study" specifications in (2). The trends appear reasonably parallel through most of the pre-withdrawal periods so the parallel trend assumption appears justified.

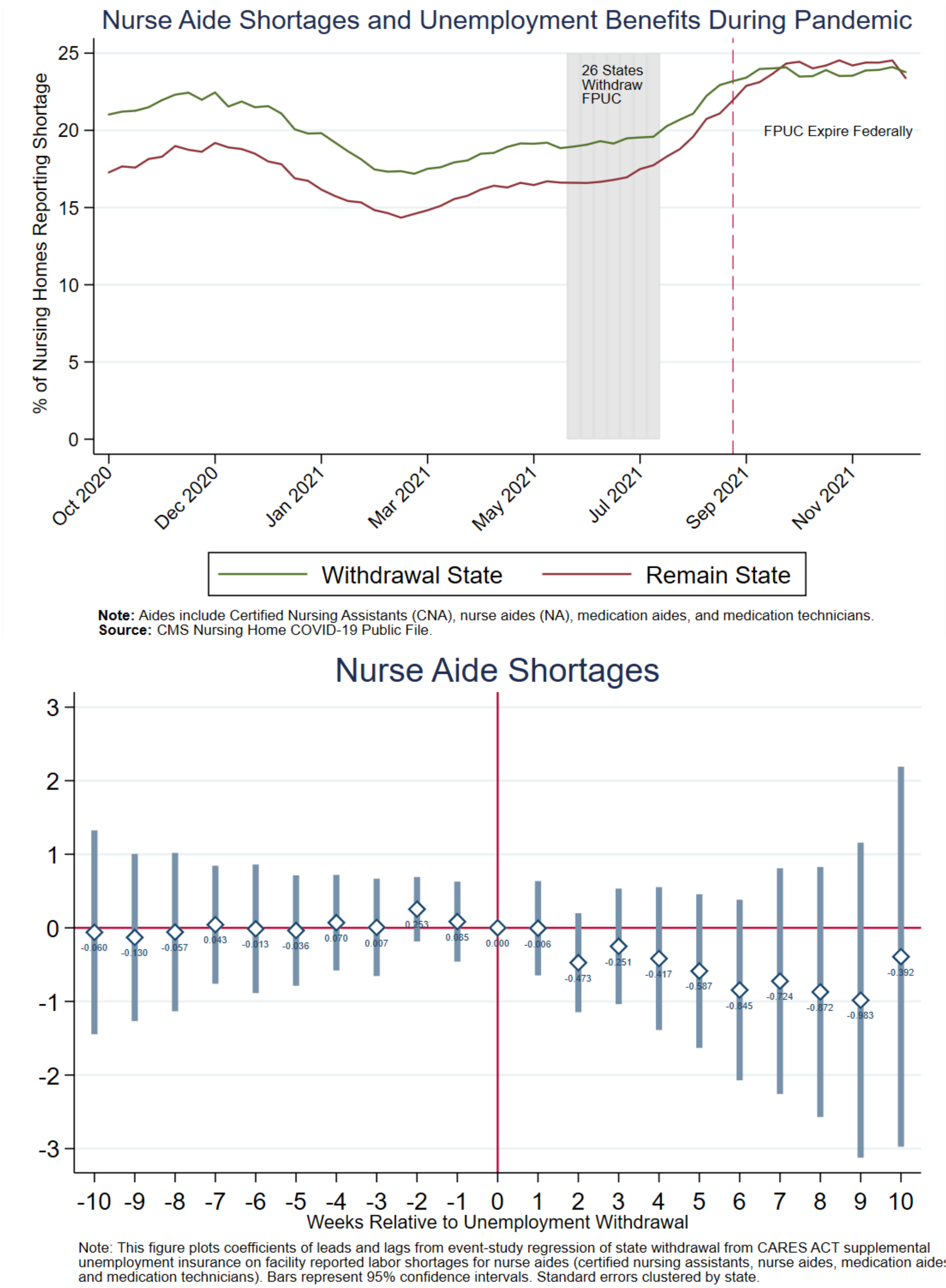
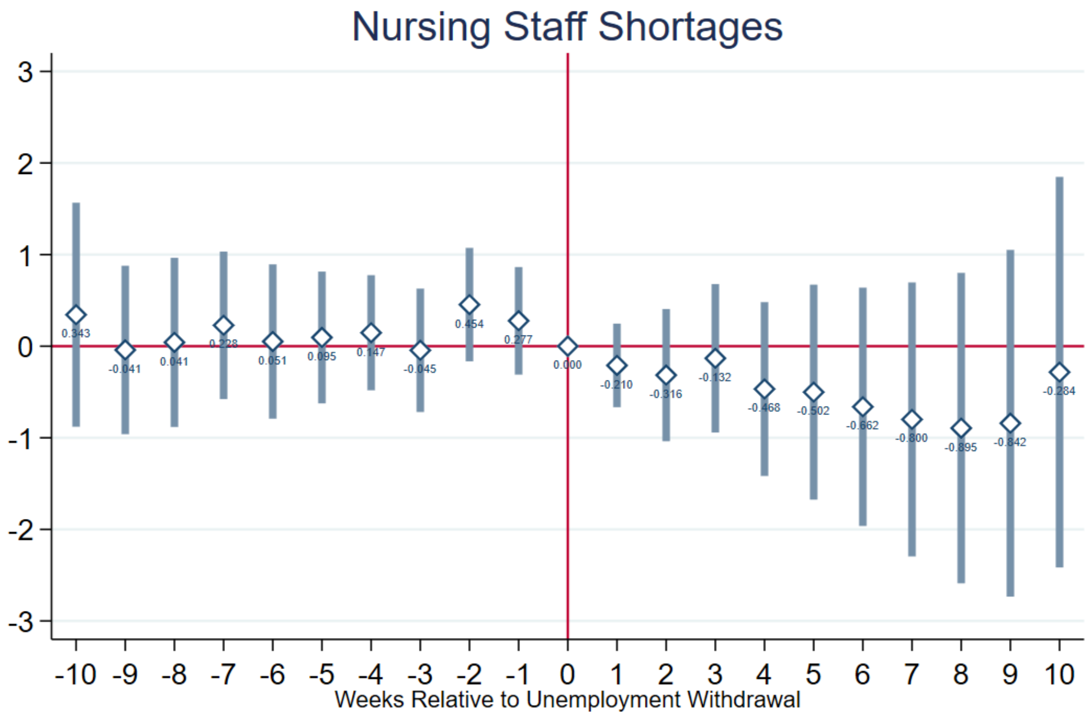
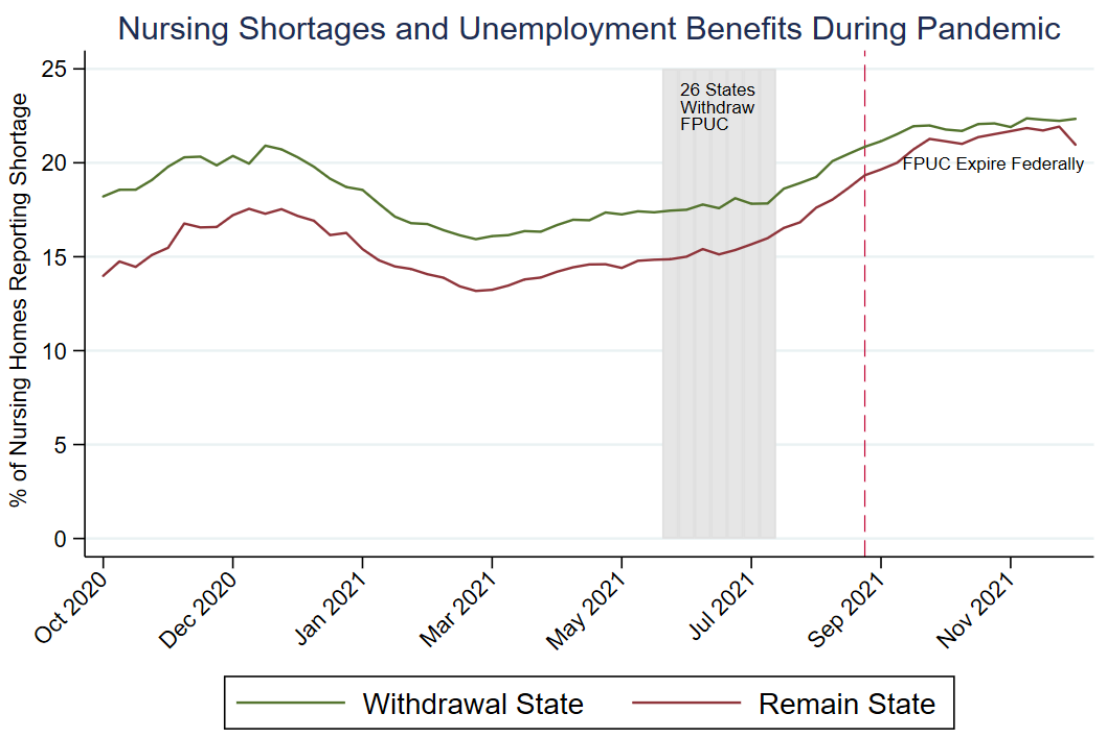


Figure 3.3. Nurse Aide Shortages: Raw data and event-study results



Note: This figure plots coefficients of leads and lags from event-study regression of state withdrawal from CARES ACT supplemental unemployment insurance on facility reported labor shortages for Nursing Staff (registered nurses, licensed practical nurses, and vocational nurses). Bars represent 95% confidence intervals. Standard errors clustered by state.



Note: Nurses include Registered Nurses (RN), Licenced Practical Nurses (LPN), and vocational nurses.
Source: CMS Nursing Home COVID-19 Public File.

Figure 3.4. Nursing Shortages: Raw data and event-study results

For the raw data for Clinical Staff see the Appendix to Chapter 3.

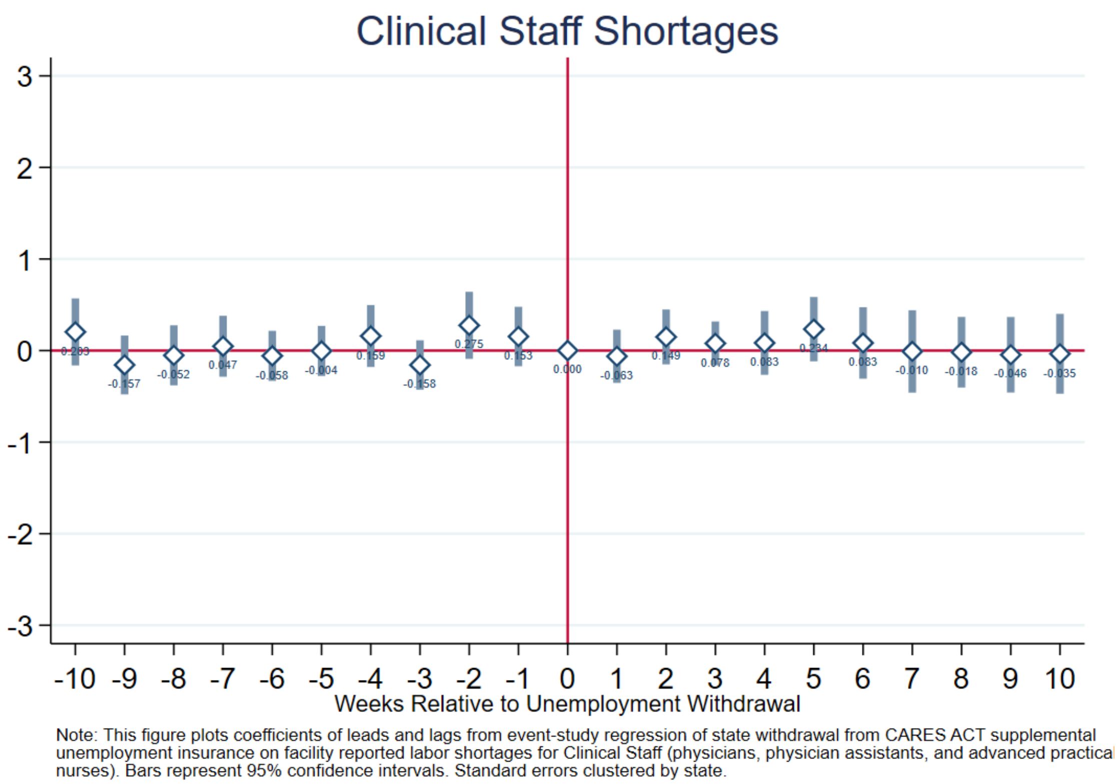


Figure 3.5. Clinical Staff Shortages

Results and Conclusion

The withdrawal coefficient for Nurse Aides is negative but insignificant (-1.331) in the plain two-way fixed effects specification. The coefficient is negative (-2.010) and significant at the 10% level in the specification that controls for the county's weekly COVID-19 cases per 1000 residents. The coefficient, -2.010, suggests early withdrawal reduced facility reported shortages for nurse aides by about 2 percentage points, or 10.6%.

Table 3.3. Effect of Unemployment Withdrawal on Nurse Aide Shortages

Table 2a. Effect of Unemployment Withdrawal on Nurse Aide Shortages

	(1) TWFE	(2) TWFE
UI Withdrawal	-1.331	-2.010^{+}
	(1.0685)	(1.1814)
Facility	Yes	Yes
Week	Yes	Yes
Controls	No	Yes
Mean of Dep. Variable	18.94	18.94
# Facilities	14,518	14,518
Observations	844,882	844,844

Note: Coefficients, Standard Errors, and Mean of Dependent Variable are multiplied by 100 for ease of interpretation. Standard Errors in parenthesis are clustered by state. Aides include Certified Nursing Assistants (CNA), nurse aides, medication aides, and medication technicians. Controls include the county's weekly COVID-19 cases per 1000 residents and lagged weekly cases.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Contrary to the hypotheses of differential impact from section 3, the results for nurses are very similar to those for nurse aides. The withdrawal coefficient for nurses is negative but insignificant (-1.202) in the two-way fixed effects specification and becomes significant at the 10% level in when we control for the county's COVID-19 cases. The coefficient, -1.807, suggests early withdrawal reduced facility reported shortages for nurses by 1.8 percentage points, or about 10.7%.

Table 3.4. Effect of Unemployment Withdrawal on Nurse Shortages

Table 2b. Effect of Unemployment Withdrawal on Nurse Shortages

	(1) TWFE	(2) TWFE
UI Withdrawal	-1.202	-1.807^{+}
	(1.0071)	(1.0739)
Facility	Yes	Yes
Week	Yes	Yes
Controls	No	Yes
Mean of Dep. Variable	16.86	16.86
# Facilities	14,518	14,518
Observations	844,884	844,846

Note: Coefficients, Standard Errors, and Mean of Dependent Variable are multiplied by 100 for ease of interpretation. Standard Errors in parenthesis are clustered by state. Nurses include Registered Nurses (RN), Licensed Practical Nurses (LPN), and vocational nurses. Controls include the county's weekly COVID-19 cases per 1000 residents and lagged weekly cases.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We then look at clinical staff, workers for whom the UI policies should have minimal impact, and therefore function as a placebo test. Here the two-way fixed effect and event-study specifications contradict one another. The withdrawal coefficient is negative and significant at the 1% level (-0.422) in the two-way fixed effects specification and becomes significant at the 0.1% level when we control for the county’s COVID-19 cases. The coefficients, ranging from -0.422 to -0.523, suggests early withdrawal reduced facility reported shortages for clinical staff by about half a percentage point, or about 23%. However, the event-study specifications for clinical staff (Figure 3.5) finds precisely estimated null-effects. This should be investigated further.

Table 3.5. Effect of Unemployment Withdrawal on Shortages for Clinical Staff

Table 2c. Effect of Unemployment Withdrawal on Shortages for Clinical Staff

	(1) TWFE	(2) TWFE
UI Withdrawal	-0.422**	-0.523***
	(0.1517)	(0.1465)
Facility	Yes	Yes
Week	Yes	Yes
Controls	No	Yes
Mean of Dep. Variable	2.15	2.15
# Facilities	14,518	14,518
Observations	844,882	844,844

Note: Coefficients, Standard Errors, and Mean of Dependent Variable are multiplied by 100 for ease of interpretation. Standard Errors in parenthesis are clustered by state. Clinical Staff include physicians, physician assistants, and advanced practice nurses. Controls include the county's weekly COVID-19 cases per 1000 residents and lagged weekly cases.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Overall, the tests produced mixed evidence for the hypothesis that unemployment insurance is discouraging work in the nursing home industry. The effects that were seen using traditional two-way fixed-effects regressions were only marginally significant in certain specifications. Moreover, while theory suggests UI withdrawal should differentially impact workers with different salaries, I find little evidence of this when I compare results across worker types. For instance, the coefficients are very similar for nurse aides and nurses, and almost identical (10.6 vs 10.7%) when measured in percentage terms, despite large differences in average salaries.

A similar pattern is found when comparing nurse aides and nurses using event-study specifications, where I find suggestive evidence that withdrawal reduces shortages by almost 1 percentage point, or 5%, for both types of workers, 2 months after withdrawal. Though it is notable that the effects found using this method appears to be smaller, around half the size, of the two-way fixed effects estimates. Future work should explore this in more detail. While much work remains to be done in this area, the evidence at this point suggests expanded unemployment insurance may have played a role in increasing shortages for nurses and nurse aides.

However, contrary to a recent industry survey, the effect sizes observed here from a large policy experiment that cut replacement rates in half and eliminated benefits from almost 2 million recipients, does not suggest it is the generosity of unemployment insurance that is driving staff shortages in the nursing home industry.

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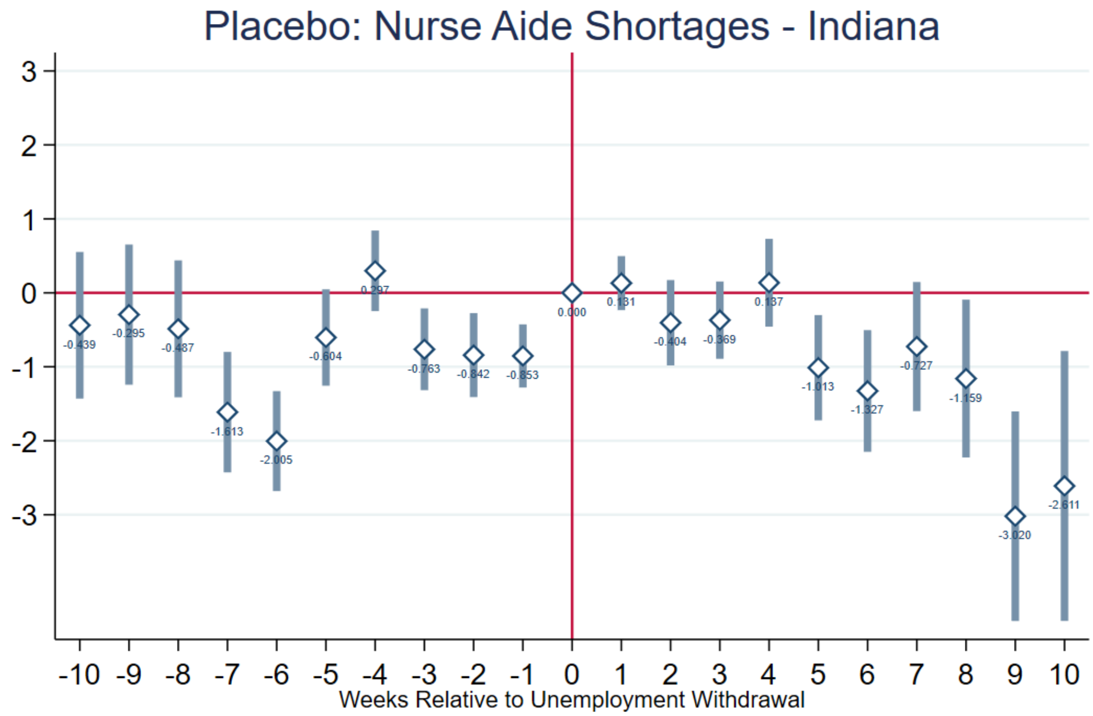
Appendix to Chapter 3

Placebo Tests using Maryland and Indiana

Below I plot event-study coefficients from Maryland and Indiana which announced withdrawal but were forced to continue operating the programs by state courts. In these specifications I have dropped the states that did withdraw, so the control group consists only of "remain" states. In Indiana, the state stopped issuing the additional UI benefits on June 19, then Judge John Hanley of Marion Superior Court ordered on June 25 that the state continue to pay the benefits, the state appealed, but the Indiana Court of Appeals denied the motion. Indiana started issuing the benefits again on July 16, including retroactive claims going back to June 26.

The hypothesis that UI discourage work predicts that $ß_t$ should be negative in the 3-4 weeks that payments were halted, and then bounce back, at least partially, once payments resume. This is generally the opposite pattern of the one we observe for both nurse aides and nurses, though trends leading up to the placebo withdrawal suggests caution in giving the estimates a causal interpretation, especially for nurse aides.



Note: This figure plots coefficients of leads and lags from event-study regression of state withdrawal from CARES ACT supplemental unemployment insurance on facility reported labor shortages for nurse aides (certified nursing assistants, nurse aides, medication aides and medication technicians). Bars represent 95% confidence intervals. Standard errors clustered by state.

Figure A3.1. Placebo: Nurse–Aide Shortages - Indiana

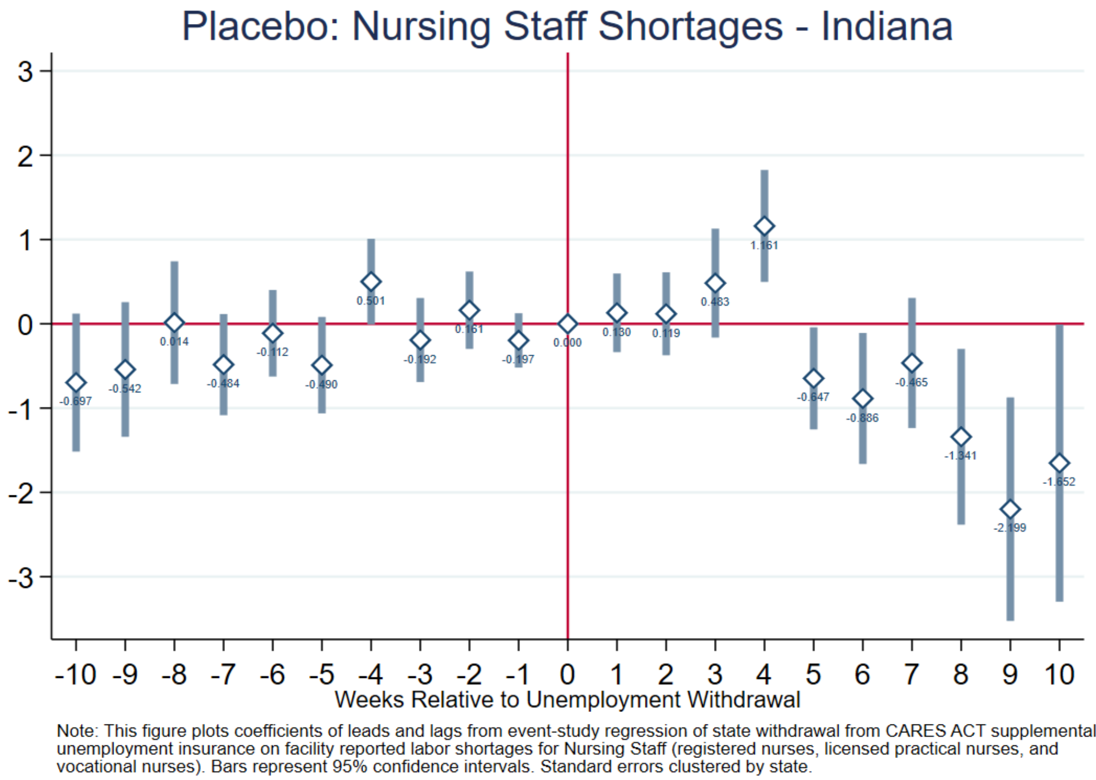
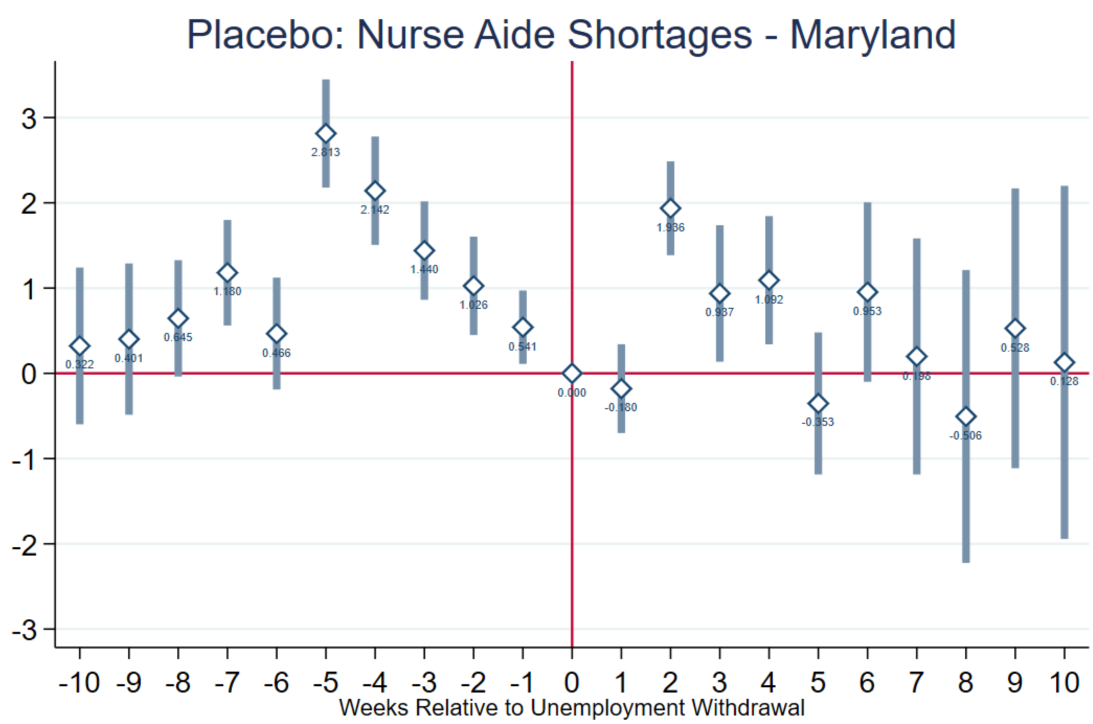


Figure A3.2. Placebo: Nursing –taff Shortages - Indiana

In Maryland, benefits were set to be terminated on July 3 but a court order on the same day required the state to continue paying the benefits. While there were additional lawsuits and appeals, the state complied with the requirement to continue paying the benefits, so Maryland continued payments without pause.

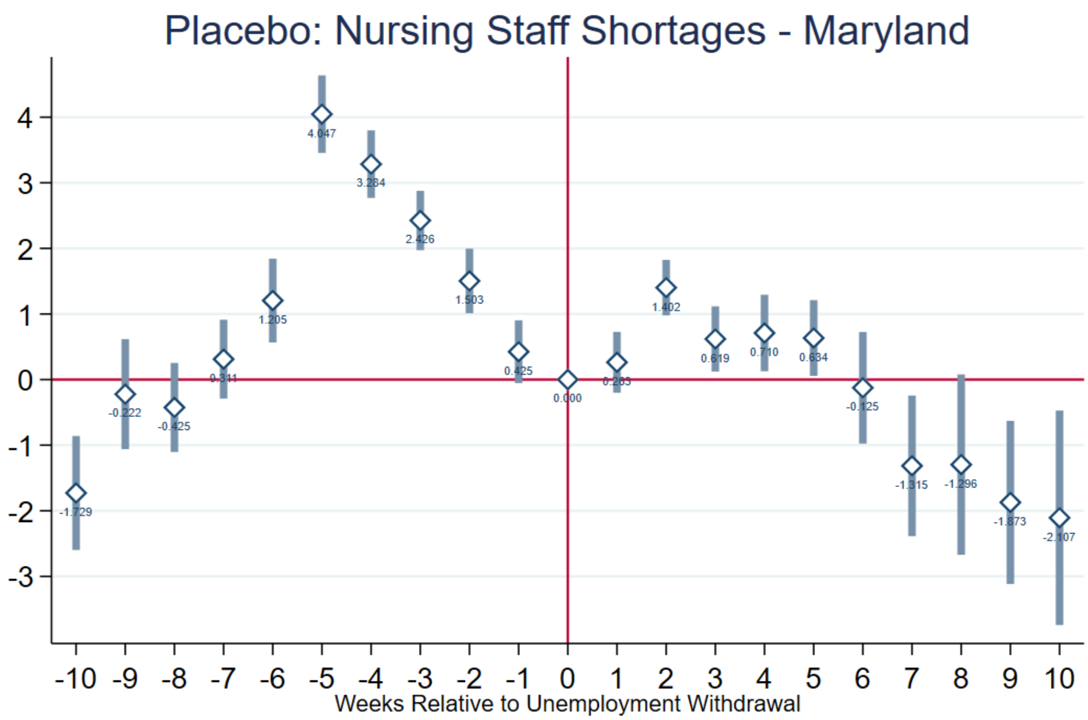
The hypothesis that UI discourages work predicts that we observe no effect for Maryland relative to the "remain" states, or that any pre-existing trend for facilities in Maryland would continue smoothly. This is also not what we observe. For nurse aides, the effect is, if anything, an increase in shortages 2-4 weeks after the withdrawal date, but this fades towards 0 after. For nurses we observe a fairly consistent (and statistically significant) increase in week 2-5, which then turns negative (and statistically significant) through week 7-10. However, both estimates should be interpreted cautiously as there was a large increase in shortages for both aides and nurses 5 weeks prior to the original withdrawal date, casting doubt on the parallel trend assumption.

It is worth noting that data in a small state like Maryland with only about 225 nursing homes will tend to be more noisy; the jump observed at t=-5, corresponds to differences of 5 and 6 facilities that report shortages for nurse aides and nurses, respectively.



Note: This figure plots coefficients of leads and lags from event-study regression of state withdrawal from CARES ACT supplemental unemployment insurance on facility reported labor shortages for nurse aides (certified nursing assistants, nurse aides, medication aides and medication technicians). Bars represent 95% confidence intervals. Standard errors clustered by state.

Figure A3.3. Placebo: Nurse Aide Shortages – Maryland



Note: This figure plots coefficients of leads and lags from event-study regression of state withdrawal from CARES ACT supplemental unemployment insurance on facility reported labor shortages for Nursing Staff (registered nurses, licensed practical nurses, and vocational nurses). Bars represent 95% confidence intervals. Standard errors clustered by state.

Figure A3.4. Placebo: Nursing Staff Shortages – Maryland

On balance, the placebo results from Indiana and Maryland are inconclusive. While estimates were generally not in line with the predictions that UI discourages work, it is not clear that we can learn anything conclusive from them due to the pre-trends observed.

South Dakota's FPUC Enrollment January 2021

Placebo: As discussed in section 2, South Dakota was the only state that did not provide a $300 weekly bonus through the Lost Wages program starting in August 2020, but started issuing $300 weekly the first week of January 2021 after the passing of the American Rescue Plan (ARPA). In theory, South Dakota could therefore provide another source of variation for testing the impact of FPUC on worker shortages.

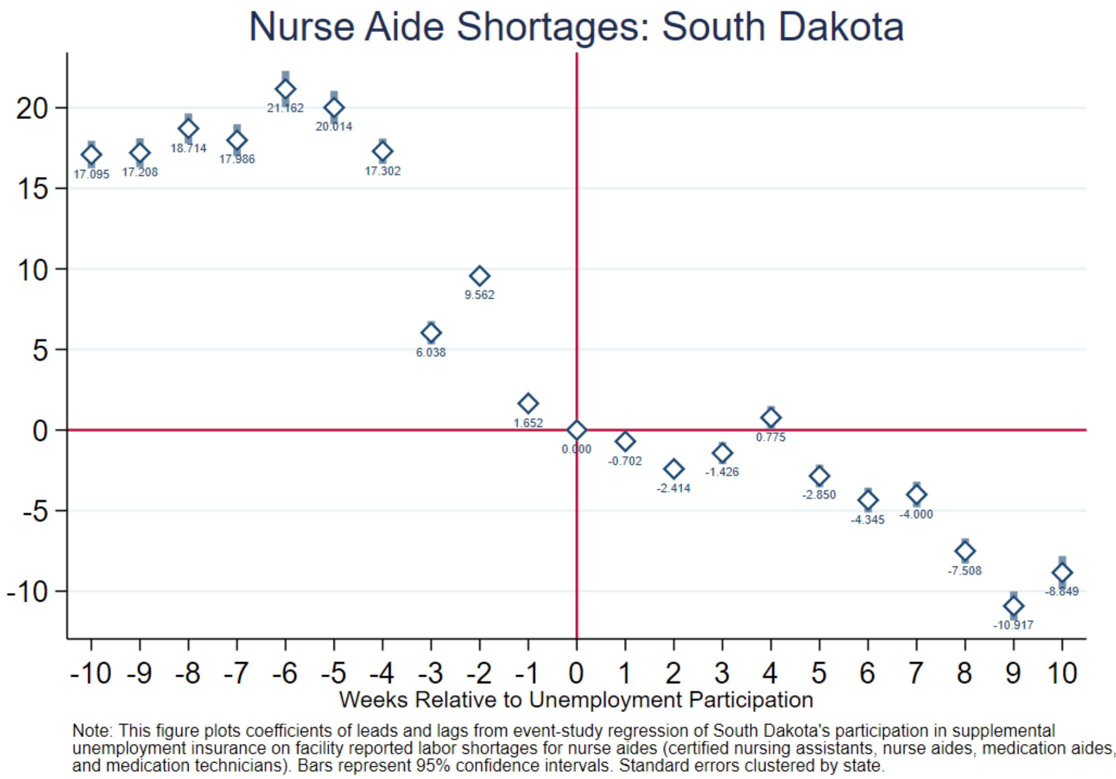
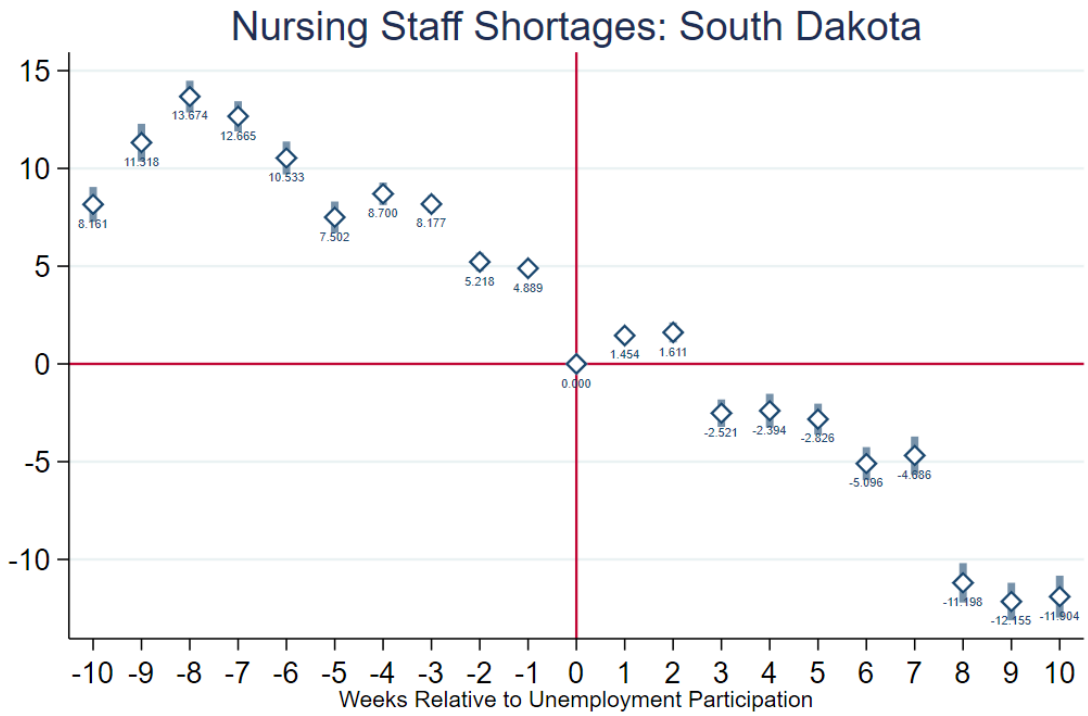


Figure A3.5. Nurse Aide Shortages: South Dakota



Note: This figure plots coefficients of leads and lags from event-study regression of South Dakota's participation in supplemental unemployment insurance on facility reported labor shortages for Nursing Staff (registered nurses, licensed practical nurses, and vocational nurses). Bars represent 95% confidence intervals. Standard errors clustered by state.

Figure A3.6. Nursing Staff Shortages: South Dakota

Raw Data for Clinical Staff

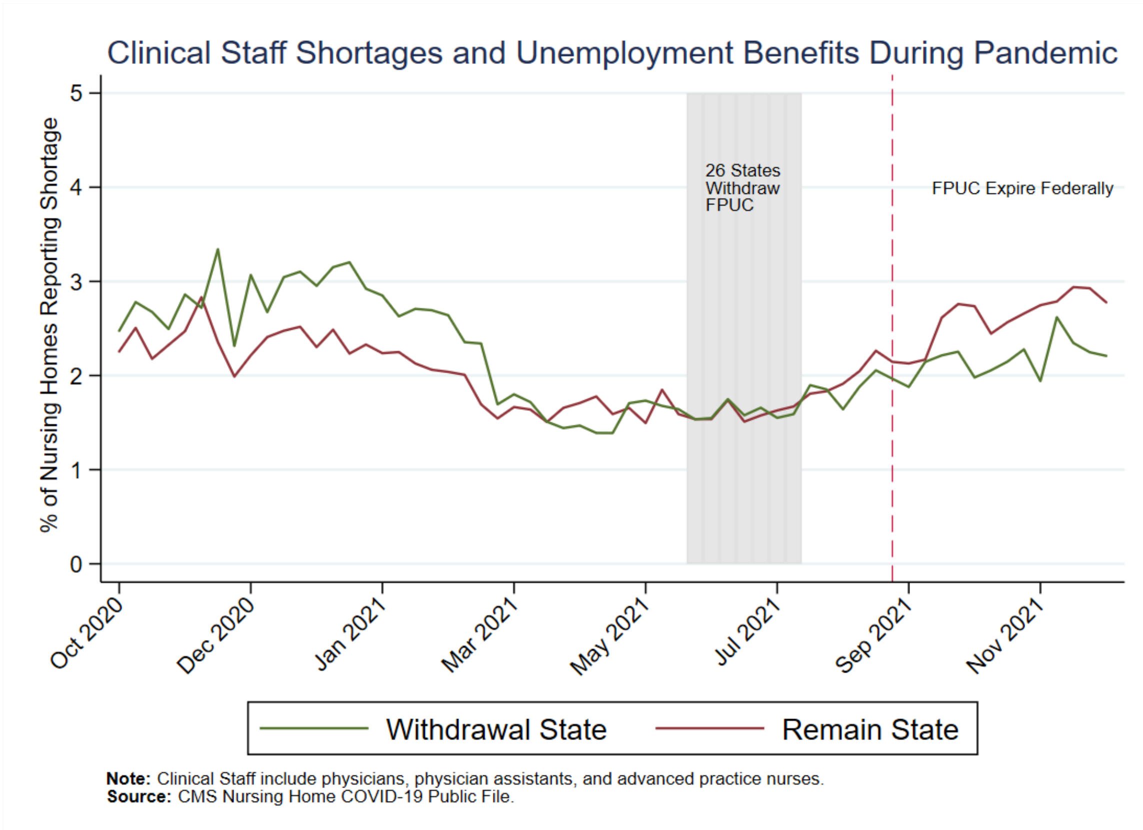


Figure A3.7. Clinical Staff Shortages and Unemployment Benefits During Pandemic

Raw Data, Event-Study Specification, and Regression Results for "Other Staff"

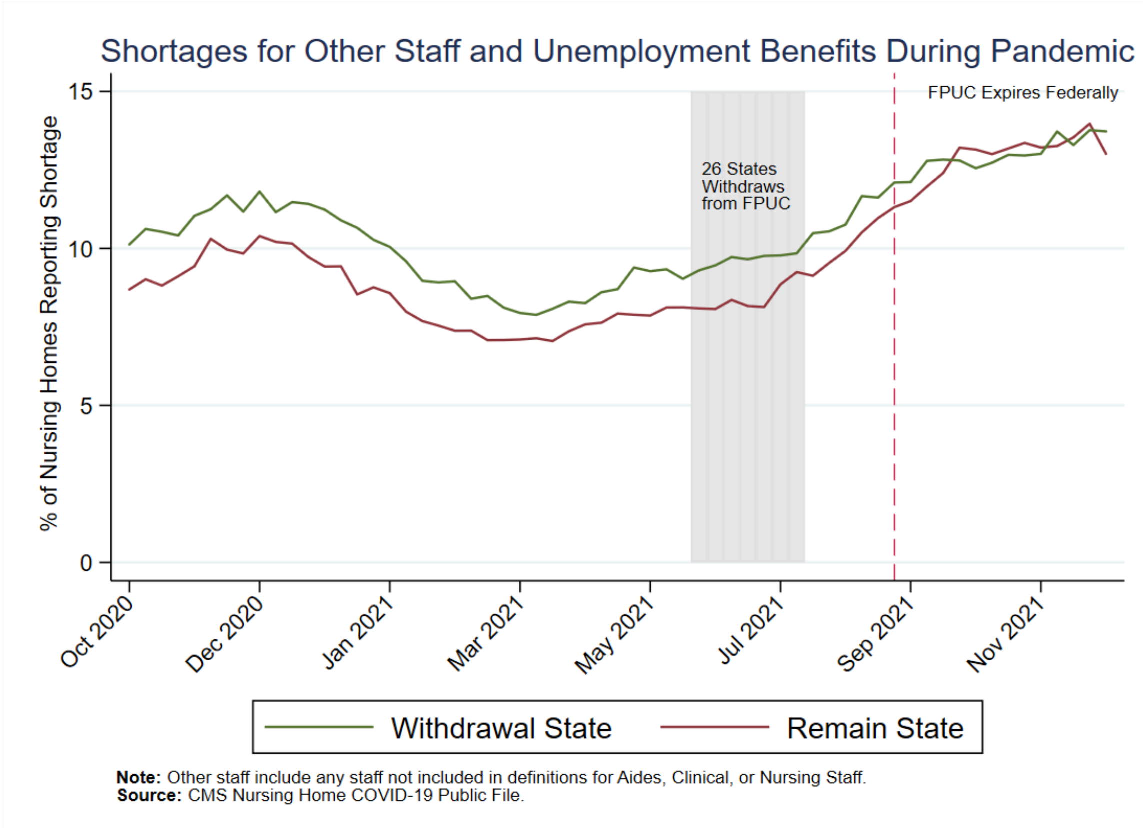


Figure A3.8. Shortages for Other Staff and Unemployment Benefits During Pandemic

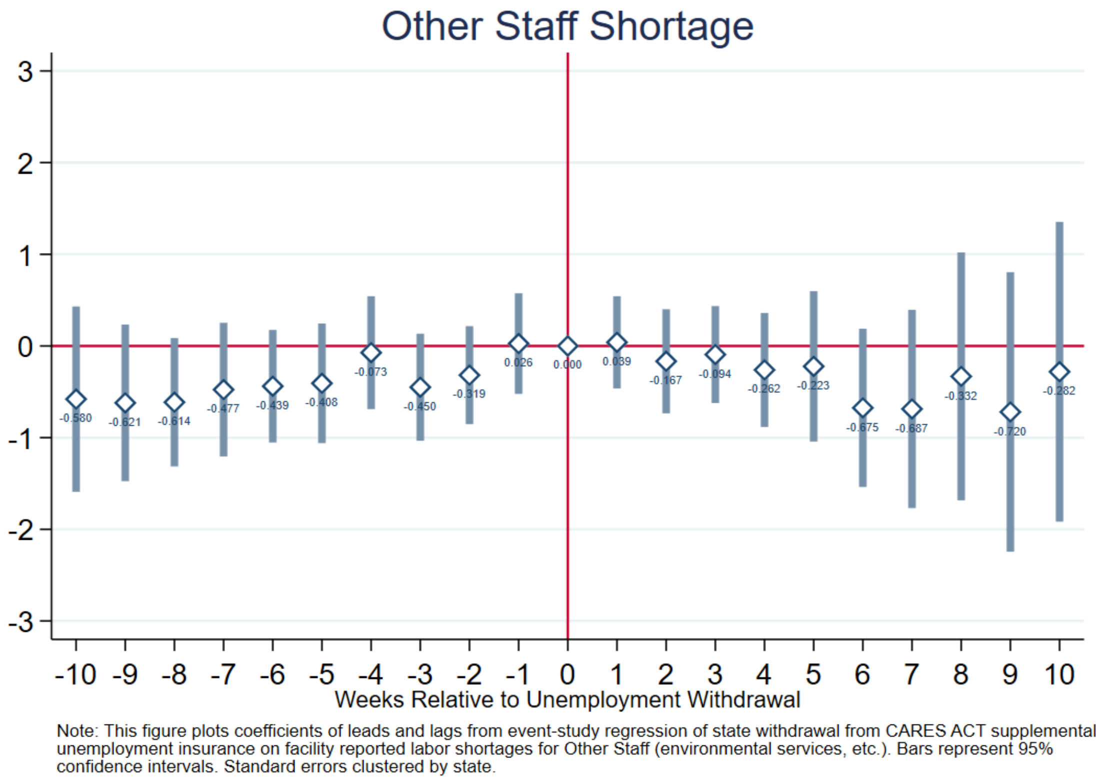


Figure A3.9. Other Staff Shortages

The results for other staff are not significant, but do follow the same pattern of being negative, and larger when controlling for the county's COVID-19 cases.

Table A3.1. Effect of Unemployment Withdrawal on Shortages for Other Staff

Table 2d. Effect of Unemployment Withdrawal on Shortages for Other Staff

	(1) TWFE	(2) TWFE
UI Withdrawal	-0.283	-0.697
	(0.7082)	(0.7780)
Facility	Yes	Yes
Week	Yes	Yes
Controls	No	Yes
Mean of Dep. Variable	9.43	9.43
# Facilities	14,518	14,518
Observations	844,866	844,828

Note: Coefficients, Standard Errors, and Mean of Dependent Variable are multiplied by 100 for ease of interpretation. Standard Errors in parenthesis are clustered by state. Other staff include any other staff not included in definitions for Aides, Nursing, or Clinical staff. Controls include the county's weekly COVID-19 cases per 1000 residents and lagged weekly cases.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Biography

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