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Effects of essential caregiver policies on COVID-19 and non-COVID-19 deaths in nursing homes

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Abstract

Federal authorities banned nursing home visitation in the early days of the coronavirus disease 2019 (COVID-19) pandemic. However, there was growing concern that physical isolation may have unintended harms on nursing home residents. Thus, nursing homes and policymakers faced a tradeoff between minimizing COVID-19 outbreaks and limiting the unintended harms. Between June 2020 and January 2021, 17 states implemented Essential Caregiver policies (ECPs) allowing nursing home visitation by designated family members or friends under controlled circumstances. Using the Nursing Home COVID-19 Public File and other relevant data, we analyze the effects of ECPs on deaths among nursing home residents. We exploit variation in the existence of ECPs across states and over time, finding that these policies effectively reduce both non-COVID-19 and COVID-19 deaths, resulting in a decrease in total deaths. These effects are larger for states that implemented policies mandatorily or without restrictions, indicating a dose-response relationship. These policies reduce non-COVID-19 deaths in facilities with higher quality or staffing levels, while reducing COVID-19 deaths in facilities with lower quality or staffing levels. Our findings support the use and expansion of ECPs to balance resident safety and the need for social interaction and informal care during future pandemics.

KEYWORDS
COVID-19, difference-in-differences, essential caregiver, nursing homes residents

JEL CLASSIFICATION
H75, I12, I18

1 | INTRODUCTION

The coronavirus disease 2019 (COVID-19) pandemic has taken a disproportionate toll on nursing home residents and staff (The New York Times, 2020). As of April 23, 2023, COVID-19 caused more than 166,000 deaths in nursing homes, accounting for at least 15% of all COVID-19 deaths in the US (Centers for Medicare & Medicaid Services, 2023). This share was even more devastating in the early days of the pandemic—nearly half of all COVID-19 deaths nationally were associated with nursing home residents and staff (Chidambaram, 2022). Nursing homes were referred to "death pits" in a New York Times article published in spring 2020 due to seemingly uncontrollable COVID-19 spread within them (Stockman et al., 2020).

As the COVID-19 tragedy in nursing homes began, mitigation policies understandably focused on protecting residents' physical safety. The Centers for Medicare & Medicaid Services (CMS) put strict bans on visitation by family and friends, along with cessation of social activities within the nursing home.[1] Eventually, however, there was growing concern about the unintended consequences (or accompanying harms) of these mitigation policies, specifically that physical isolation of nursing home residents was taking a toll on both their emotional and physical health. One study of Connecticut nursing homes found that residents exhibited increased depressive symptoms, incontinence, and weight loss after the implementation of visitor bans (Simonetti et al., 2020). Another study found that nursing homes with fewer COVID-19 deaths per resident had higher rates of non-COVID deaths during the same time (Cronin & Evans, 2022). Such findings suggest that nursing homes that most effectively isolated residents may have had the most severe unintended consequences of that isolation. Thus, nursing homes and policymakers faced a potential tradeoff between strict COVID-19 infection control and the more general well-being of residents.

In recognition of the toll that social isolation was taking on residents, at varying times between June 2020 and January 2021, 17 states implemented Essential Caregiver policies (ECPs; see Figure 1). These policies authorized nursing homes to bypass CMS guidelines to allow each resident one or two designated "essential caregivers," who were allowed to visit the resident regularly to provide care and social support. The essential caregivers were subject to regular screening and training in COVID-19 prevention, similar to requirements for staff. These policies allowed residents to have access to regular social interaction with family (or a designated friend) and also to benefit from additional care that the visitor might provide while facilities were short-staffed. Although these policies received positive reviews from families and advocates and present a potentially desirable compromise moving forward—for the rest of the COVID-19 pandemic and for future infectious disease outbreaks—they have not been rigorously evaluated.

In this paper, we examine the effects of ECPs on COVID-19 and non-COVID-19 deaths for nursing home residents in the U.S. To estimate plausibly causal effects, we employ a difference-in-differences model, exploiting the fact that ECPs were implemented in 17 states, at staggered times, and not at all in others. To address potential biases associated with the canonical two-way fixed effects approach in cases where treatment times are staggered, we implement the estimator proposed in Callaway and Sant'Anna (2021) as our preferred approach; we also present two-way fixed-effects models and the estimator proposed in Sun and Abraham (2021) as alternate estimates of ECPs for comparison.

In addition to contributing to the broader economics literature on nursing home quality, we contribute to several strands of the literature. First, our work contributes to a relatively sparse economics literature on nursing homes during the COVID-19 pandemic. It is most closely related to Cronin and Evans (2022), whose main result is consistent with the medical and health services literature on COVID-19 in nursing homes: that high quality was not associated with the ability to prevent COVID-19 from entering a nursing home, but somewhat reduced transmission and deaths once an outbreak started (Gorges & Konetzka, 2020; Konetzka et al., 2021). However, the secondary and novel finding of Cronin and Evans (2022) was that high-quality nursing homes experienced higher non-COVID-19 deaths and total deaths, ostensibly because they were over-emphasizing resident protection from COVID-19 and not considering the unintended mental and physical harms of isolation. This finding is consistent with a prior study that reported social isolation and loneliness increase the likelihood of mortality by 29% and 26%, respectively (Holt-Lunstad et al., 2015). Melo (2022) further explores the mechanism underlying the unintended consequences of nursing home isolation measures, assessing the effects of reduction in visitors using cell phone tracking data, finding that isolation measures implemented in nursing homes significantly increased non-COVID deaths and total deaths after the pandemic. Nonetheless, the empirical strategy used in the paper does not explicitly address concerns about potential endogeneity, nor does it test plausible solutions to this tradeoff between safety and social interaction. Our paper takes the next step in this sequence, assessing whether the restoration of social interaction ameliorated those harmful effects using a more rigorous approach. In so doing, we add evidence to the credibility of that explanation, while also providing a direct assessment of a policy intended to address the problem.

Second, our work also contributes to the literature regarding the relationship between informal care and formal care in long-term care settings. Several prior studies (Bolin et al., 2008; Pickard, 2012; Van Houtven & Norton, 2004) find that informal care is a substitute for long-term care such as nursing home care and home health care once endogeneity is addressed. Utilizing survey data from 11 European countries, Bonsang (2009) compares the effect of informal care on skilled and unskilled home care, finding that informal care also complements skilled home care, although weakly. More

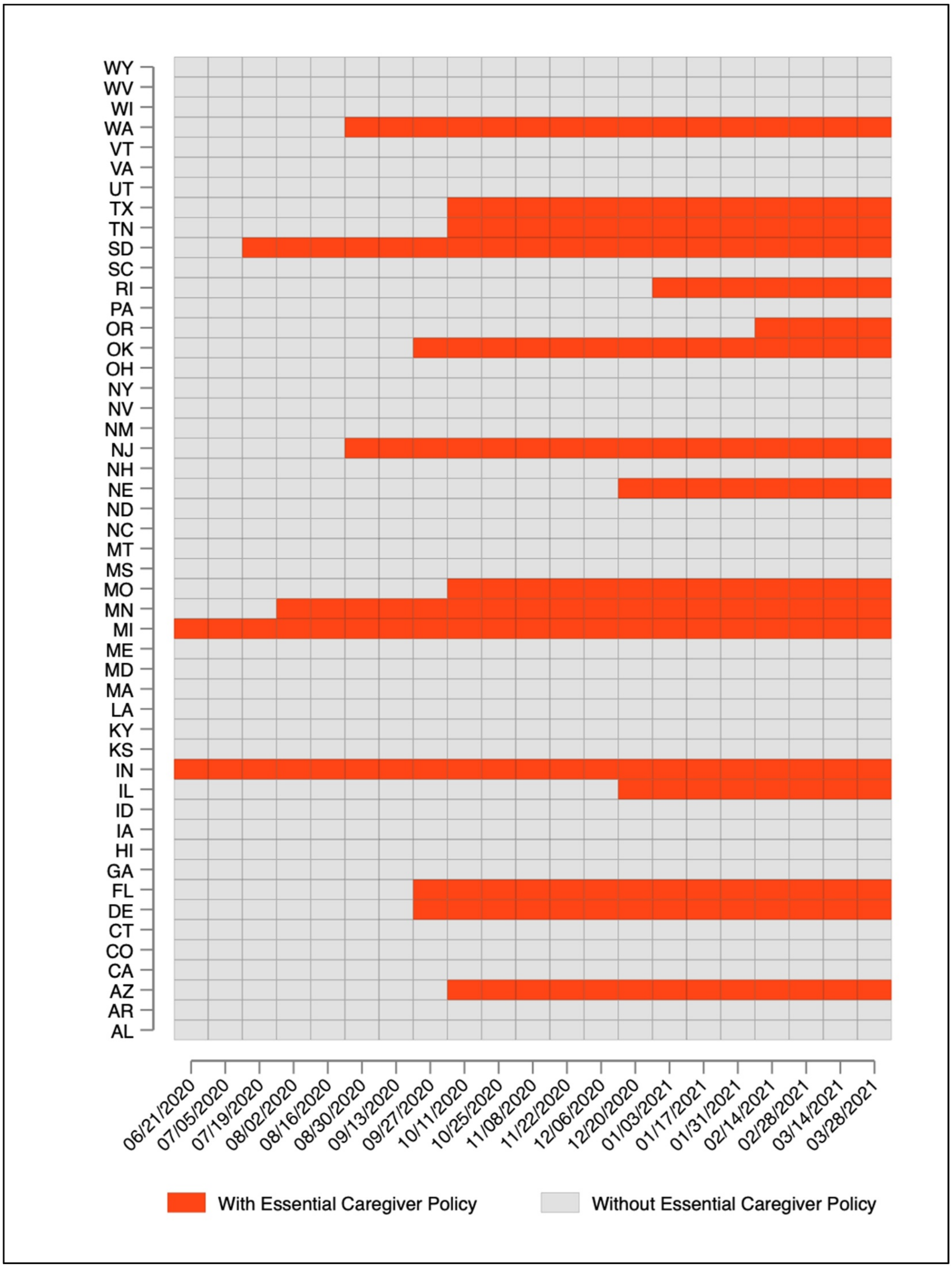


FIGURE 1 Distribution of implementation timing of Essential Caregiver policies (ECPs). (1) Dates on the *x*-axis reflect the end date of each 2-week period. (2) Implementation dates of ECPs in each state are collected from the executive orders or regulatory guidance issued by the governor or state public health authorities.

recently, Coe and Werner (2022) analyze the role of family caregivers in residential care facilities and nursing homes. Using data from the Health and Retirement Study and the National Health and Aging Trends Study, they find that informal care is common among older adults with functional limitations in residential care facilities and nursing homes. By examining whether the restoration of informal care (assuming Essential Caregivers provide some care as the policies intended) had a positive effect on the health outcomes of nursing home residents, we provide direct evidence for the importance of informal care provided by family members in the lives of nursing home residents.

We find that ECPs effectively reduce both non-COVID-19 deaths and COVID-19 deaths, resulting in a decrease in total deaths. These effects are mainly driven by state ECPs that are mandatory or implemented without restrictions. Our findings also suggest that the beneficial effects of ECPs are heterogeneous across nursing home types. Specifically, these policies significantly reduce non-COVID-19 deaths in high-quality facilities and highly staffed facilities, while resulting in a decrease in COVID-19 deaths in facilities with lower quality or staffing levels. To our knowledge, this is the first study to provide rigorous evidence to assess the effects of ECPs on COVID-19 and non-COVID-19 deaths for nursing home residents, informing the tradeoffs between protecting nursing home residents from infectious disease and maintaining necessary social interaction for nursing home residents. For policymakers more directly, our findings support the use of ECPs by more states to balance risks during future infectious disease outbreaks.

2 | BACKGROUND ON ESSENTIAL CAREGIVER POLICIES

In response to CMS visitation restrictions at the onset of the COVID-19 pandemic, 17 states implemented ECPs.[2] Four states implemented their ECPs through executive orders or emergency rules,[3] requiring adoption, while most state policies were issued as regulatory guidance from public health authorities. Nursing homes within the latter set of states could choose whether or not to establish an Essential Caregiver program. Even in these non-mandatory states, these policies were enthusiastically welcomed and supported by families of nursing home residents and their advocates (Markowitz, 2020). The policies varied in their timing, implementation, and alignment with evolving federal guidelines. States defined key parts of the policies, including the eligibility and role of essential caregivers, infection control protocols, and, in some states, restrictions on program activity based on COVID-19 prevalence. Essential caregivers were required to apply for these programs, schedule their visits, and follow infection control measures including regular COVID-19 testing, masking, and restricted movement within the nursing home, although social distancing was not required between residents and their essential caregivers. Some states stipulated that the designated individual had to be already providing care to the resident at the onset of the pandemic to be eligible for essential caregiver status. Other key decisions, such as whether to allow visitation during active outbreaks (for states without related restrictions), were generally left to individual facilities.

All state policies were introduced after May 2020, when CMS reintroduced limited visitation conditional on low or decreasing COVID-19 prevalence in facilities and their surrounding communities. Although these guidelines provided the potential for visitation, high COVID-19 prevalence at the time meant that it was still effectively not allowed under federal guidelines. Most state policies simply bypassed these guidelines by distinguishing Essential Caregivers from general visitors or expanding the definition of compassionate care visitation, but a handful of states did restrict ECP activity with conditions modeled after CMS guidelines, that is, based on community or facility prevalence of COVID-19.[4] Most states did not allow essential caregivers to visit residents who were confirmed or suspected to have COVID-19.[5]

States establishing ECPs varied widely in their geographic location and political characteristics; there is no obvious pattern as to why some states established these policies while others did not. Indiana, Mississippi, Minnesota, New Jersey, Florida, Delaware, Washington, and Tennessee ended their ECPs and deferred to CMS guidelines by the time national visitation bans were lifted in March 2021.

There are little data available for directly assessing the facility-level adoption of ECPs. However, several qualitative studies indicate strong support for ECPs among nursing home administrators and families of nursing home residents. Using semi-structured qualitative interviews, Hathaway et al. (2023) found that ECPs were viewed favorably by long-term care facility administrators and were recognized as a valuable tool for balancing the psychosocial needs of residents and families with the health risks posed by COVID-19 infections. Similarly, in a Delphi panel consisting of post-acute and long-term care experts in clinical medicine, administration, and patient care advocacy, 79% of respondents agreed that the nursing home should consider a designated caregiver an essential member of the health care team (Bergman et al., 2020). Furthermore, a survey conducted by Human Rights Watch, based on a non-representative sample of 564 relatives of long-term care facility patients from 45 states (not all with ECPs) between October 2020 and March 2021, revealed that 55% of the respondents answered "Yes" when asked whether they inquired with the nursing home about becoming a compassionate or essential caregiver for their loved one. Among those who answered positively, 28% were granted compassionate or essential caregiver status by the nursing homes (Mills, 2021). These findings provide evidence that ECPs were well-received by nursing home administrators and families of nursing home residents, suggesting a probable increase in nursing home visitation following the implementation of these policies.

3 | CONCEPTUAL FRAMEWORK

We conceptualize the primary decision-maker in this context as the state (the governor or state public health authorities), who must choose nursing home visitation policies in order to maximize the well-being of nursing home residents; in this case, we approximate resident well-being through the short-term probability of death. When making a decision about whether or not to institute an ECP, state policy makers have to consider both direct and indirect threats posed by COVID-19 to nursing home resident survival. On one hand, the spread of COVID-19 in nursing homes poses a direct and immediate threat to the lives of nursing home residents. The nursing home population is composed of older adults with a high percentage of people that have cognitive impairment. Prior studies have found that older adults with impaired cognitive function are associated with a greater risk of death from COVID-19 infection (Damayanthi et al., 2021; Panagiotou et al., 2021). As COVID-19 is spread through proximity to infected individuals, allowing visitors into the nursing home can be assumed to increase the probability of COVID-19 deaths among residents; this was the motivation behind federal guidance to ban visitors. At the same time, the relationship between visitors and the probability of death is fraught with uncertainty. Research has established that facility size and community COVID-19 prevalence were the strongest predictors of nursing home COVID-19 deaths and that staff traffic in and out of facilities played a major role (Konetzka et al., 2021); no research has established the additional risk posed by visitation. Visitation may also pose little or no transmission risk if managed with limitations and precautions.

On the other hand, the cessation of visitation and other types of social interaction are also hypothesized to affect the probability of resident death, indirectly through the emotional and physical toll on resident health or through a reduction in supplemental care from family and friends (Coe & Werner, 2022), leading to more non-COVID-19 deaths. Additionally, prior studies have shown that both staff size (number of unique employees working daily) and nursing home staff networks (staff members working in multiple nursing homes) strongly predict nursing home COVID-19 outcomes (Chen et al., 2021; McGarry et al., 2021; Shi et al., 2020). The cessation of visitation might result in nursing home staff assuming additional hands-on care responsibilities previously undertaken by family caregivers, which could potentially contribute to an increase in COVID-19 deaths. In this case, increasing visitation is hypothesized to lead to a *decrease* in the probability of total deaths. This relationship is also subject to uncertainty. While these indirect effects have been posited and some supporting evidence has been generated (Cronin & Evans, 2022), the specific role of allowing visitation has not been tested.

State policymakers thus face two competing consequences of decisions to expand visitation of nursing home residents during a pandemic: a possible increase in deaths due to transmission of the disease at hand and a possible decrease in deaths due to the emotional and physical benefits of social interaction with, and additional care from, family and friends. The theoretical welfare effects of expanding visitation are ambiguous. Because of this theoretical ambiguity, state perceptions of the tradeoffs may have differed, and states made different decisions about whether to expand visitation through Essential Caregivers policies. The net effects on resident well-being are an empirical question, one that our study is intended to answer.

We expect some heterogeneity in the effects by key nursing home and state policy attributes. First, just as high-quality nursing homes may have over-emphasized prevention of COVID-19, the quality of the nursing home may influence the effectiveness of resuming visitation through ECPs through several mechanisms. First, high-quality nursing homes may see larger reductions in non-COVID-19 deaths because they had higher levels of this outcome to reduce. Second, high-quality nursing homes generally have higher staffing levels, and higher staffing levels may facilitate the ability to comply with state policies (Konetzka et al., 2015, 2022). For example, highly staffed facilities may be better able to provide enough visitation opportunities and to manage visits in a safe manner. Meanwhile, the implementation of ECPs may help to reduce COVID-19 deaths in nursing homes with lower staffing levels. These facilities typically employ a higher ratio of part-time staff, who often work across multiple facilities, thereby increasing the risk of COVID-19 transmission. The additional hands-on care and monitoring provided by essential caregivers may help alleviate this situation by reducing the interaction between nursing home staff and residents. Nonprofit status, often associated with higher quality and higher staffing (Grabowski et al., 2013), may also be associated with a stronger mission to improve less tangible aspects of resident well-being such as quality of life, potentially leading to more effective implementation of ECPs. At the same time, nonprofit status and high quality may not be completely synonymous. While the evidence is mixed, one prior study of the association between nursing home profit status and COVID-19 outcomes found that for-profit nursing homes were associated with fewer cases and better access to personal protective equipment (PPE) and testing (Gandhi et al., 2020). It is thus possible that for-profit nursing homes may have similar or even better performance in preventing potential COVID-19 outbreaks in the presence of ECPs.

In terms of state-level heterogeneity, ECPs were mandatorily enforced in four states, which should lead to significantly higher adoption rates relative to states with non-mandatory policies. In non-mandatory states, some facilities could be reluctant to adopt ECPs due to concerns about the additional burden of facilitating visits and uncertainties over COVID outcomes. The effect of ECPs on death outcomes are therefore expected to be greater for states with mandatory policies. At the same time, some states constrained nursing homes visitation more than others. As noted above, in some states Essential Caregiver visits were not allowed if the community prevalence of COVID-19 exceeded established levels or if the facility itself had a current or recent outbreak. Nursing homes in these "restricted" states may be less likely to fully benefit from expanded visitation. In addition, as implementation of Essential Caregiver programs was not always mandatory, nursing homes in states with restrictions may have been less likely to implement the programs at all, given the extra burden of following COVID-19 rates and starting and stopping the program accordingly. Thus, we expect the effects of ECPs to be smaller in states with restrictions. On the other hand, if the restrictions were effective in preventing COVID-19 outbreaks, we might expect fewer COVID-19 deaths in these states.

Finally, as community COVID-19 prevalence has been found to be one of the strongest predictors for COVID-19 deaths in nursing homes (Gorges & Konetzka, 2021; Konetzka et al., 2021), the effectiveness of an Essential Caregiver program may be associated with time-varying community COVID-19 prevalence. Visits may simply be riskier during times of higher prevalence, or COVID-19 prevention may take temporary precedence over visits, or nursing home staff may be unable to do both well at the same time. Therefore, the effects of ECPs may be heterogeneous within nursing homes over time by community COVID-19 prevalence level.

It is important to note that when ECPs were passed by states, nursing homes, families, and residents may not have experienced effects immediately. Although policies were likely anticipated, it might take time for nursing homes to develop their Essential Caregiver programs according to state guidelines and to prepare for visitations. At the same time, families and friends would need to first apply for the essential caregiver designation and then schedule their visits. Furthermore, it may have taken time for social interaction and additional care provided by essential caregivers to generate observable effects on the well-being of residents, especially when approximating the well-being of residents through the short-term probability of death. Thus, we expect a lag in time between state adoption of ECPs and effects on resident outcomes. However, it is unclear a priori how much of a time lag to expect.

4 | METHODS

4.1 | Data sources

This study draws from multiple publicly available data sets. Our primary data source is the Nursing Home COVID-19 Public File released by CMS, which contains weekly data on COVID-19 cases and deaths reported by nursing homes (Centers for Medicare & Medicaid Services, 2023). Beginning on May 24, 2020, all nursing homes certified for Medicaid and/or Medicare were required by CMS to report weekly COVID-19 data to the Centers for Disease Control and Prevention (CDC) National Healthcare Safety Network (NHSN), Long Term Care Facility (LTCF) COVID-19 Module. We supplement this facility-week level data set with facility-level data on nursing home characteristics (e.g., number of certified beds) from the LTCFocus database (LTCFocus is sponsored by the National Institute on Aging (1P01AG027296) through a cooperative agreement with the Brown University School of Public Health, n.d.) and the Nursing Home Compare (NHC) archives. We then merge these facility-week-level observations with county-level socioeconomic characteristics (e.g., median household income) from the American Community Survey 2015–2019 (5-Year Estimates) data set. In addition, we use the county-day level data on COVID-19 cases released by the New York Times, based on reports from state and local health agencies, to calculate the county-week level community prevalence of COVID-19 (The New York Times, 2022).

4.2 | Main study sample

We start by including all NHSN weekly reports that pass the CMS quality assurance checks with week ending dates between June 7, 2020, and March 28, 2021.[6] We limit the sample to weekly reports before March 28, 2021, as CMS released revised guidance for nursing home visitation on March 10, 2021, effectively opening visitation in most circumstances, making it difficult to identify effects of ECPs after this date. We further exclude data from the first week of June 2020 due to concerns about data quality and consistency; reporting of deaths prior to June in the first reporting period was optional and it is not clear which facilities reported those earlier deaths and which did not. We build on the minimal data quality checks from CMS by excluding weekly reports of nursing homes that reported negative non-COVID-19 deaths, negative non-occupied beds, weekly new deaths that are greater than number of occupied beds, and cumulative deaths that are greater than cumulative residents served. Finally, we exclude observations with missing information on non-COVID-19 deaths, COVID-19 deaths, nursing home characteristics, and county-level socioeconomic characteristics.

Because the weekly data are inherently noisy, we combine every two consecutive weeks into a 2-week period and aggregate the data set to the facility-period level. The final study sample includes 231,941 unique facility-period level observations of 13,149 nursing homes.

4.3 | Study variables

4.3.1 | Treatment variable: Essential Caregiver policies indicator

The explanatory variable of interest is whether a nursing home is in a state with an ECP. We adopt an "intent to treat" approach, categorizing all nursing homes in states with ECPs as treated, regardless of whether that nursing home actually adopted an Essential Caregiver program. In this sense, we are estimating the effects of the state policy, not the effects of facility-level adoption. This is also necessary in that for states without mandatory policies, there is no data source documenting which nursing homes adopted the programs and which did not in states that allowed it. We create a dichotomous indicator that equals 1 if a nursing home is in a state with an ECP before or on the first day of the current 2-week period and 0 if a nursing home is in a state without an ECP in that period. In the study sample, there are 92,772 observations (40.00%) with the ECP indicator equal to 1, and 139,169 observations (60.00%) with the ECP indicator equal to 0 (see Table 1).

4.3.2 | Dependent variables

We examine the impact of ECPs on three outcomes: new COVID-19 deaths, new non-COVID-19 deaths, and new total deaths (incident deaths for each 2-week observation). First, we obtain new COVID-19 deaths and all-cause deaths from

TABLE 1 Summary of Essential Caregiver policy status.

	N (facility-periods)	Percent
Without essential caregiver policy	139,169	60.00
With essential caregiver policy		
- Cohort 1 (06/21/2020)	13,559	5.85
- Cohort 2 (07/19/2020)	1625	0.70
- Cohort 3 (08/02/2020)	5542	2.39
- Cohort 4 (08/30/2020)	8644	3.73
- Cohort 5 (09/27/2020)	15,946	6.88
- Cohort 6 (10/11/2020)	30,830	13.29
- Cohort 7 (12/20/2020)	13,363	5.76
- Cohort 8 (01/03/2021)	1316	0.57
- Cohort 9 (02/14/2021)	1947	0.84
- Subtotal	92,772	40.00
Total	231,941	100

Note: (1) The summary is on facility-period (2 weeks) level. (2) Cohort is defined based on the sequence of Essential Caregiver policy implementation in different states. (3) Dates in parentheses are the end date of each 2-week period during which an Essential Caregiver policy was implemented.

the NHSN weekly reports. Second, we derive new non-COVID-19 deaths by subtracting COVID-19 deaths from all-cause deaths. Finally, we scale new COVID-19 deaths, new non-COVID-19 deaths, and new total (all-cause) deaths to the size of the facility by dividing by the total number of occupied beds and then multiplying by 1000 (Miller et al., 2021). Because these outcomes are skewed and contain zeroes, we transform them (see details below) for use in our analysis.

4.3.3 | Other variables

We include facility level and facility-period level nursing home characteristics as control variables in all regressions, including number of certified beds, NHC 5-star ratings (overall, quality, and staffing), adjusted nursing (nurse aide, licensed practical nurse, registered nurse, and total) hours per resident-day, facility-reported indicators of staffing shortages (nursing, clinical, and aide), type of ownership, hospital-based status, chain membership, acuity index (a measure of prevalence and severity of functional limitations and cognitive impairment among a facility's residents), percentage of residents with Medicaid coverage, and percentage of residents with Medicare coverage for their stay. We also include county-level socioeconomic status (median household income, percent of county population of age 65 and over, percent of county population that is Black, and percent of county population that is Hispanic) and community prevalence of COVID-19 (measured as the number of confirmed COVID-19 cases per 1000 population in the county, excluding cases among nursing home residents). As nursing homes in states without ECPs had experienced higher rates of COVID-19 deaths and non-COVID deaths prior to the study period (see Table 2), which may leave a relatively healthier population in these facilities, we control for cumulative COVID-19 deaths, COVID-19 cases, and non-COVID-19 deaths lagged by two periods in all regressions. In addition, we include facility fixed effects and period fixed effects in all estimations to account for unobserved heterogeneity across facilities and over time. While we include all variables described above to summarize the study sample and as potential covariates in all regressions, some variables (chain membership, acuity index, percentage of residents with Medicaid coverage, percentage of residents with Medicare coverage, and variables of county-level socioeconomic status) are omitted in regressions as they do not vary over time in our study sample.

The covariates in the baseline period (06/08/2020–06/21/2020) are summarized in Table 2. Nursing homes in states with ECPs are more likely to be government-owned, are less likely to be hospital-based, are more likely to be in counties with a higher percentage of Hispanic population, are more likely to be in counties with lower levels of median household income, and have higher cumulative COVID-19 cases, higher cumulative COVID-19 deaths, and higher cumulative non-COVID-19 deaths. Other covariates are generally similar between nursing homes in states with and without ECPs.

4.4 | Empirical strategy

We use a difference-in-differences regression framework to examine the impact of ECPs on facility-period level non-COVID-19, COVID-19, and total deaths, employing multiple estimators for transparency and comparison. The main source of our identifying variation is whether states adopt ECPs for nursing homes during the study period. The identifying assumption in this research design is the parallel-trends assumption that nursing homes in states with ECPs and nursing homes in states without ECPs would have similar trends in non-COVID-19, COVID-19, and total deaths in the absence of passage of ECPs. Although inherently untestable, a standard way to assess the credibility of this assumption is to examine whether trends are parallel prior to implementation. We plot the trends in unadjusted non-COVID-19, COVID-19, and total deaths by ECP status in Figure 2 to visually assess the parallel-trends assumption. The trends in unadjusted non-COVID-19, COVID-19, and total deaths satisfy the parallel-trends assumption reasonably well. We also test the parallel-trends assumption for the transformed outcomes more rigorously by plotting the trends in covariates-adjusted outcomes by state (see Appendix Figures A2–A4) and in our preferred event study design using the Callaway and Sant'Anna (2021) estimator, described later.

We start by estimating the canonical two-way fixed effects model of the following form:

$$m(Y_i) = \alpha \text{ECP}_{it} + \beta\prime X_{it} + \tau_i + \gamma_t + \epsilon_{it},$$

TABLE 2 Baseline nursing home characteristics by ECP status.

	All nursing homes	Nursing homes without ECP	Nursing homes with ECP
	N = 12,868	N = 7675	N = 5193
Number of certified beds, mean (SD)	108.11 (60.43)	107.67 (65.51)	108.75 (52.04)
NHC overall 5-star rating, mean (SD)	3.17 (1.42)	3.18 (1.41)	3.15 (1.42)
NHC quality 5-star rating, mean (SD)	3.69 (1.24)	3.72 (1.24)	3.66 (1.24)
NHC staffing 5-star rating, mean (SD)	2.95 (1.18)	2.96 (1.13)	2.92 (1.25)
Adjusted nurse aide hours per resident per day, mean (SD)	2.30 (0.54)	2.31 (0.53)	2.30 (0.56)
Adjusted LPN hours per resident per day, mean (SD)	0.87 (0.33)	0.89 (0.34)	0.84 (0.32)
Adjusted RN hours per resident per day, mean (SD)	0.69 (0.44)	0.69 (0.43)	0.69 (0.45)
Adjusted total nurse hours per resident per day, mean (SD)	3.85 (0.84)	3.87 (0.83)	3.82 (0.86)
Shortage of nursing staff, N (%)	1851 (14.38%)	1096 (14.28%)	755 (14.54%)
Shortage of clinical staff, N (%)	321 (2.49%)	182 (2.37%)	139 (2.68%)
Shortage of aides, N (%)	2168 (16.85%)	1235 (16.09%)	933 (17.97%)
Ownership, N (%)			
- For profit	9077 (70.54%)	5493 (71.57%)	3584 (69.02%)
- Government	788 (6.12%)	362 (4.72%)	426 (8.20%)
- Nonprofit	3003 (23.34%)	1820 (23.71%)	1183 (22.78%)
Hospital-based, N (%)	471 (3.66%)	354 (4.61%)	117 (2.25%)
Chain membership, N (%)	7542 (58.61%)	4423 (57.63%)	3119 (60.06%)
Acuity index, mean (SD)	12.15 (1.74)	12.22 (1.83)	12.04 (1.60)
Percent Medicaid, mean (SD)	0.61 (0.23)	0.61 (0.23)	0.60 (0.22)
Percent Medicare, mean (SD)	0.12 (0.12)	0.12 (0.12)	0.13 (0.12)
Percent of county population of age 65 and over, mean (SD)	0.17 (0.04)	0.17 (0.04)	0.17 (0.05)
Percent of county population of Black, mean (SD)	0.11 (0.13)	0.12 (0.15)	0.10 (0.10)
Percent of county population of Hispanic, mean (SD)	0.14 (0.15)	0.12 (0.14)	0.15 (0.16)
Median household income of county, mean (SD)	63,102 (17,261)	64,136 (18,851)	61,573 (14,463)
County-level weekly new COVID-19 cases per 1000 people, mean (SD)	5.87 (6.68)	5.94 (6.46)	5.77 (6.99)
Cumulative COVID-19 cases per 1000 residents, mean (SD)	99.39 (239.81)	109.17 (247.79)	84.95 (226.75)
Cumulative COVID-19 deaths per 1000 residents, mean (SD)	27.73 (73.23)	30.23 (76.75)	24.05 (67.54)
Cumulative non-COVID-19 deaths per 1000 residents, mean (SD)	59.17 (84.30)	61.77 (86.83)	55.32 (80.28)

Note: (1) The summary is based on nursing home characteristics in the first 2-week period (06/08/2020–06/21/2020). (2) NHC star rating: Nursing Home Compare star rating, which ranges from 1 to 5, with 1 star indicating lowest rating and 5 stars indicating highest rating; ECP, Essential Caregiver policy; LPN, licensed practical nurse; RN, registered nurse.

where i indexes the nursing home and t indexes the 2-week period. Y_i is one of the dependent variables (new non-COVID-19 deaths per 1000 residents, new COVID-19 deaths per 1000 residents, or new total deaths per 1000 residents) defined above. Because the distributions of dependent variables are right-skewed and contain zero values (see Appendix Figure A1), we follow Chen and Roth (2023) to transform the dependent variables through the following steps: (1) Normalizing the non-zero values of Y so that 1 corresponds to the value of the minimum non-zero Y in the data. Therefore, we have $Y_{\text{norm}} = \frac{Y}{\min(Y>0)}$ for $Y > 0$. (2) Subsequently, we perform the transformation as $m(Y) = \log(Y_{\text{norm}})$ for $Y > 0$ and $m(0) = 0$ for $Y = 0$. Thereby, we treat all zero values of Y as equivalent to the minimum non-zero value of Y in the sample. This specification ignores the extensive margin change between zero and the minimum

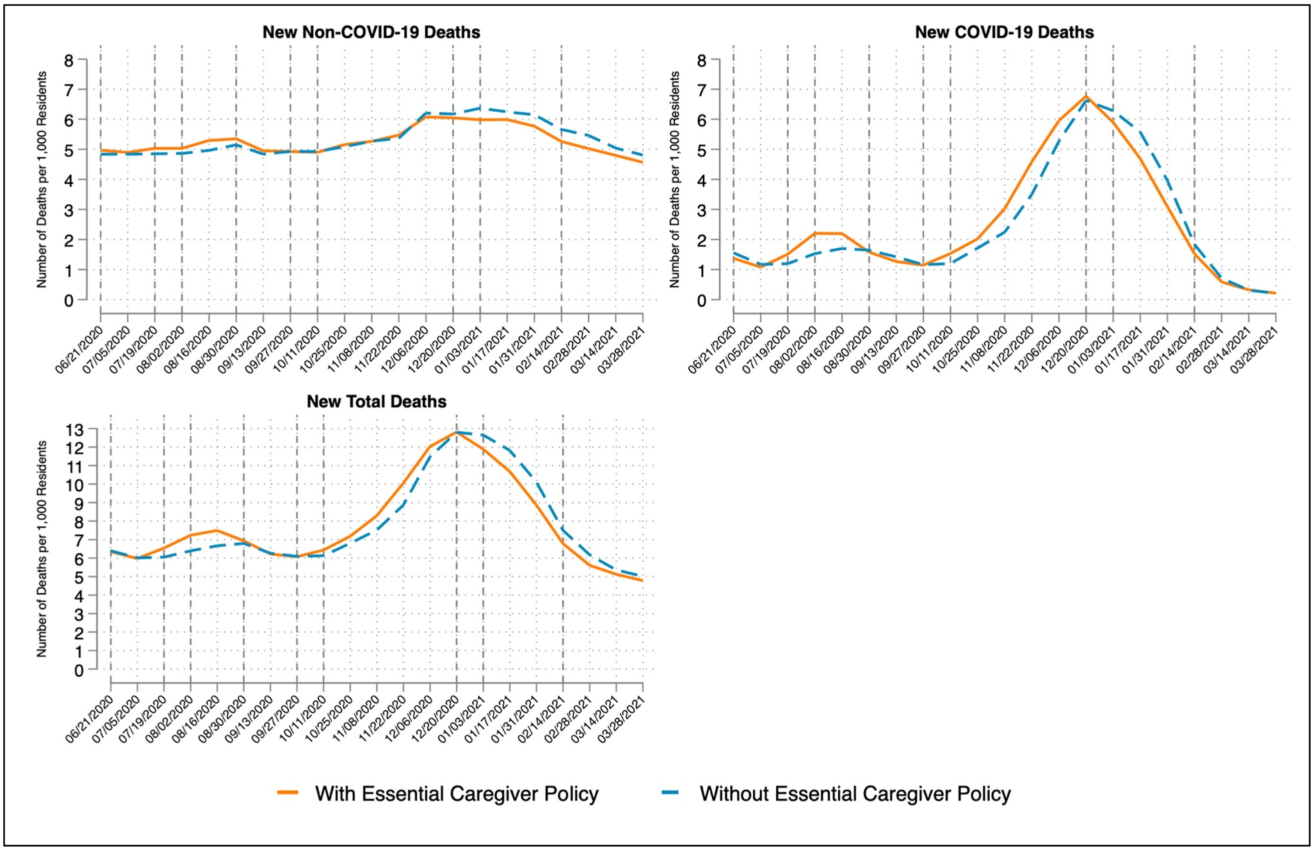


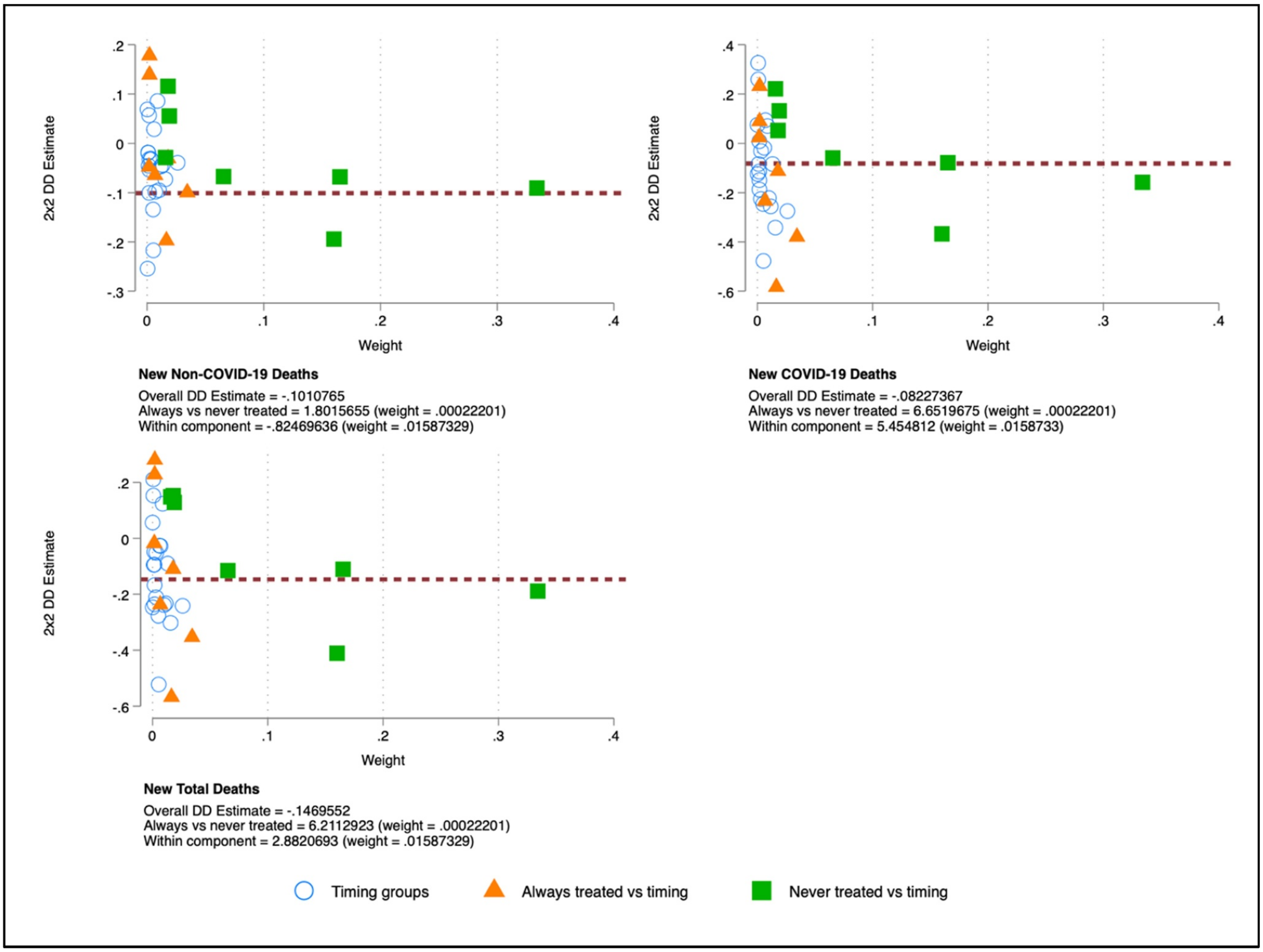
FIGURE 2 Unadjusted trends in new non-COVID-19 deaths, new COVID-19 deaths, and new total deaths in nursing homes by ECP status. (1) Dates on the x-axis representing the end date of each 2-week period. (2) The vertical reference lines reflect treatment timing (when an ECP was implemented) for each treatment cohort. ECP, Essential Caregiver policy.

non-zero values of Y when estimating the treatment effect, leading to a more conservative estimation.[7] This transformation yields two distinct benefits: Firstly, our estimated treatment effects are less heavily influenced by outcomes in the tail of the distribution. Second, by explicitly specifying how we handle a change in Y from 0 to the minimum non-zero value of Y, our estimated treatment effects remain unaffected by the units of dependent variables. Hence, we can interpret these estimates as approximate percentage changes.

The independent variable of interest is ECP_{it}, which equals 1 if a nursing home i is in a state with an ECP in period t. Vector X_{it} includes the facility-period level covariates described above. We also include facility fixed effects τ_i and period fixed effect γ_t to adjust for unobserved facility-specific and period-specific confounders. ϵ_{it} is a mean zero error term. We estimate this equation separately for each outcome.

Recent advances in econometric theory suggest that standard two-way fixed effects model can provide biased estimates when there is variation in treatment timing, as the estimate may capture heterogeneity and variation in the effect rather than the average treatment effect on the treated (e.g., Baker et al., 2022; Borusyak et al., 2024; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). More specifically, the use of earlier-treated units as controls for later-treated units will bias the treatment effect if the impact of earlier implementation grows or wanes over time.

Because our analysis is likely to be subject to this potential problem, we follow Goodman-Bacon (2021) to decompose our two-way fixed effect estimator into five component parts (timing groups, always-treated group vs. timing groups, always-treated group vs. never-treated group, timing groups vs. never-treated group, and within group) based on their ECP status to analyze how each component part contributes to the overall estimate. Our primary concern here is whether the estimate from timing groups, which contain earlier-treated units to serve as controls for later-treated units, significantly differs from the overall estimate, especially when it holds significant weight within the overall estimate.[8] We present the decomposition and weight for each component part in Figure 3.[9]



	Estimates on Non-COVID-19 Deaths	Estimates on COVID-19 Deaths	Estimates on Total Deaths	Total Weight
Timing groups	-0.0514	-0.1808	-0.1788	0.1255
Always vs Timing	-0.0876	-0.3153	-0.2968	0.0810
Never vs Timing	-0.0955	-0.1565	-0.1892	0.7774
Always vs Never	1.8016	6.6520	6.2113	0.0002
Within	-0.8247	5.4548	2.8821	0.0159

FIGURE 3 The Goodman-Bacon decomposition of the two-way fixed effects estimator in the balanced panel. (1) The Goodman-Bacon (2021) decomposition requires a balanced panel, we thus exclude observations of nursing homes with any missing weekly reports within the study period. (2) This figure presents 2 × 2 difference-in-differences comparisons for five components parts based on the implementation status of the Essential Caregiver policies. The five components parts are: timing groups (earlier-treated units vs. later-treated units and later-treated units vs. earlier-treated units), always-treated group versus timing groups, never-treated group versus timing groups, always-treated group versus never-treated group, and within group (comparisons between units with different predicted treatment status within the same timing group). The 2 × 2 comparisons of the always-treated group versus never-treated group and within groups are not presented in the diagnostic plot; however, they are associated with tiny weights and thus have little impact on the overall estimates. The *y*-axis plots the estimates for each pair and the *x*-axis plots the weights for each pair. The horizontal line shows the weighted average estimates. The table displays the estimated coefficients and weights for all 2 × 2 difference-in-differences comparisons. The weights are determined by the sample share and variance of the treatment dummy in each component part. (3) The log transformation outlined in the Empirical Strategy section is used for estimating the treatment effects for all three outcomes.

Next, we implement the Sun and Abraham (2021) estimator and the Callaway and Sant'Anna (2021) estimator to address potential bias from the two-way fixed effects estimator (Rios-Avila et al., 2022; Sun, 2021).[10] We estimate these estimators separately for each outcome.[11] To formally test the parallel-trends assumption, we estimate the dynamic event study effects of all periods using the Callaway and Sant'Anna (2021) estimator for each outcome (see Appendix Tables A2–A4). We also re-estimated the two-way fixed effects model in the restricted sample with the aim of drawing a more appropriate comparison between results across different estimators.

We consider the Callaway and Sant'Anna (2021) estimator to be the preferred model in our case, since it avoids problematic 2 × 2 difference-in-differences comparisons that violate the parallel-trends assumption and derives estimates under more general conditions. More specifically, the Callaway and Sant'Anna (2021) estimator first estimates the time-specific treatment effects for each treatment timing cohort, then generates the overall treatment effect by aggregating all time-specific treatment effects. Leveraging the improved doubly robust difference-in-differences estimator proposed by Sant'Anna and Zhao (2020), Callaway and Sant'Anna (2021) can better accommodates cases when the parallel trends assumption holds only after conditioning on covariates. However, given the evolving science and lack of consensus on which estimator is most appropriate in which cases, we present three estimators for the sake of transparency: the two-way fixed effects estimates, Sun and Abraham (2021), and Callaway and Sant'Anna (2021).

5 | RESULTS

5.1 | Main results

Estimated effects of ECPs on non-COVID-19, COVID-19, and total deaths in nursing homes from all three estimators are presented in Table 3. We report sample means of the three dependent variables across different samples in the last three rows. Column 1 contains estimated coefficients from the two-way fixed effects models in the full sample (a full set of regression results can be found in Appendix Table A13) and column 2 contains estimates from the same model in the restricted sample. The estimated coefficients in column 1 suggest the implementation of ECPs decreases new non-

TABLE 3 Effects of essential caregiver policies on new non-COVID-19 deaths, new COVID-19 deaths, and new total deaths in nursing homes.

	TWFE (1)	TWFE (restricted sample) (2)	SA (3)	CS (4)
New non-COVID-19 deaths per 1000 residents	−0.103**	−0.0984*	−0.0975**	−0.0918**
	(0.0491)	(0.0524)	(0.0411)	(0.0419)
New COVID-19 deaths per 1000 residents	−0.0765	−0.0552	−0.1034**	−0.1096**
	(0.0699)	(0.0738)	(0.0455)	(0.0548)
New total deaths per 1000 residents	−0.146*	−0.126	−0.1641***	−0.1625***
	(0.0868)	(0.0920)	(0.0616)	(0.0612)
Week FE	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
N	231,941	208,258	208,253	198,189
Mean of new non- COVID-19 deaths per 1000 residents	5.36	5.26	5.26	5.27
Mean of new COVID-19 deaths per 1000 residents	2.57	2.54	2.54	2.50
Mean of new total deaths per 1000 residents	7.93	7.80	7.80	7.77

Note: (1) TWFE: the two-way fixed effects OLS regression. Full regression results can be found in Appendix Table A13. (2) Restricted sample: a sub-sample excluding observations of nursing homes in states (Michigan, Indiana, South Dakota, and Minnesota) without sufficient data points in pre-treatment period for examining parallel-trends assumption and in state (Nebraska) which we observe clear violation of parallel-trends assumption. (3) SA: the Sun and Abraham (2021) estimator. The results are estimated in the restricted sample. (4) CS: the Callaway and Sant'Anna (2021) estimator. The results are estimated in the restricted sample. (5) The log transformation outlined in the Empirical Strategy section is used for estimating the treatment effects for all three outcomes. (6) Standard errors clustered at state level are included in parentheses. (7) *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

COVID-19 deaths by 9.79% (10.3 log points), new COVID-19 deaths by 7.36% (7.65 log points), and new total deaths by 13.58% (14.6 log points). However, the estimated effect on new COVID-19 deaths is not significantly different from zero. Similar estimated coefficients are reported in column 2 using the restricted sample, except that the estimated effect on new total death is not statistically significant. These estimates are meaningful but may be subject to the problems with two-way fixed-effects models described above.

Using the Goodman-Bacon (2021) decomposition of the coefficients estimated using the two-way fixed effects model in a balanced panel (column 1 of Appendix Table A1) to explore potential biases in the two-way fixed effects estimate and to understand where the variation is coming from in our preferred estimator, Figure 3 shows that the estimated coefficients from the two-way fixed effects estimator are mainly driven by the estimated effects derived from 2 × 2 comparisons of the never-treated group versus timing groups. The 2 × 2 comparisons of timing groups and always-treated group versus timing groups produce large estimated coefficients in both directions, indicating potential violation of the parallel trends assumtion. Ideally, we would expect the three types of 2 × 2 comparisons shown in Figure 3 to yield similar estimates. The Goodman-Bacon (2021) decomposition suggests clear evidence of bias associated with the estimated effects from the two-way fixed effects model.

Columns 3–4 of Table 3 contain the estimated coefficients from the Sun and Abraham (2021) estimator and the Callaway and Sant'Anna (2021) estimator, respectively. Estimated coefficients from our preferred model, the Callaway and Sant'Anna (2021) model, suggest ECPs are associated with an 8.77% (9.18 log points) decrease in new non-COVID-19 deaths, a 10.38% (10.96 log points) decrease in new COVID-19 deaths, and a 15% (16.25 log points) decrease in total deaths. Similar results are obtained when using the Sun and Abraham (2021) estimator.

The dynamic event study effects of ECPs estimated from the Callaway and Sant'Anna (2021) estimator are graphically displayed in Figure 4. The estimated effects in most of the pre-treatment periods are not significantly different from zero for all three outcomes, suggesting no obvious violation of the parallel-trends assumption. In post-treatment periods, we find ECPs are estimated to reduce all three death outcomes with effect sizes increasing over time in the first half of the post-treatment period. The effects on COVID-19 deaths start to wane after post-treatment period 9, which coincides with the time when COVID-19 vaccination became available for nursing home residents and staff. These results are consistent with the overall estimates we present in Table 3. In addition, the estimated effects are relatively small for the first three post-treatment periods, which is consistent with our contention that nursing home residents were likely to experience the effects of ECPs with a time lag. We report estimated event study effects of ECPs in all pre- and post-treatment periods in Appendix Tables A2–A4. In Appendix Figure A5, we also present the overlay of event study plots estimated using all three estimators, the Callaway and Sant'Anna (2021) estimator, the Sun and Abraham (2021) estimator, and a dynamic version of the two-way fixed effects estimator, which shows the estimated dynamic effects are generally consistent across three estimators.

5.2 | Heterogeneity[12]

To test for heterogeneous effects of ECPs, we stratify the estimation using our preferred model, the Callaway and Sant'Anna (2021) model, by several key variables as described in our conceptual framework.[13] First, we stratify by nursing home quality level. We define facilities with baseline overall 5-star rating above the median (3-star) as high-quality nursing homes, and facilities rated 3-star or below at baseline as low-quality nursing homes.

Second, we stratify by nursing home baseline staffing level. We define facilities with case-mix adjusted total nurse staffing score of greater than 50 points as high-staffing nursing homes.[14] Facilities with a baseline case-mix adjusted total nurse staffing score smaller than or equal to 50 points are defined as low-staffing nursing homes. Third, we analyze whether there are differential responses between for-profit nursing homes and not-for-profit nursing homes.[15] Fourth, we stratify by whether the ECP was mandatory[16] and whether it was subject to restrictions. We define states with mandatory and non-mandatory ECPs as separate treatment groups in a secondary analysis. Similarly, we define an indicator of whether each state's ECP had restrictions,[17] and we separately consider the states with and without restrictions as the treatment group. Last, we analyze whether the effects of ECPs differ in time periods of high COVID-19 prevalence in the surrounding county.[18]

When stratifying by nursing home quality level, we find that the implementation of ECPs produces significant reduction in non-COVID-19 deaths and total deaths for nursing homes with higher quality, whereas we fail to find statistically significant effects on these outcomes for low-quality nursing homes. In contrast, we find that ECPs reduce COVID-19 deaths only for nursing homes with lower quality. These results are consistent with our hypotheses described in the conceptual framework. We present these estimates in columns 1–2 of Table 4.

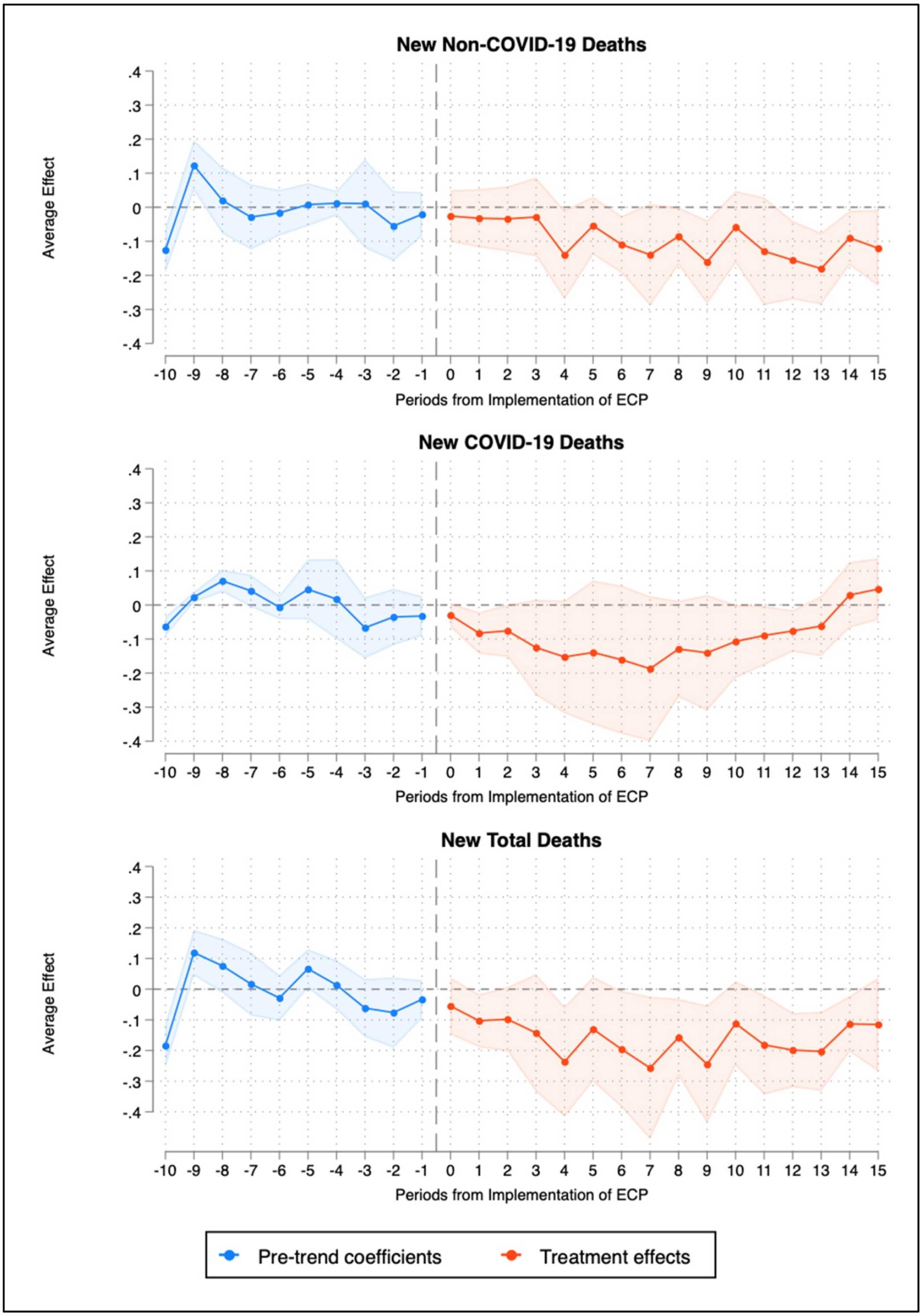


FIGURE 4 Adjusted dynamic event study effects of 12 pre- and post-treatment periods, estimated from the Callaway and Sant'Anna (2021) estimator. (1) This figure plots the Callaway and Sant'Anna (2021) event study estimates and 95% confidence intervals for relative-time periods from −10 to 15 around the implementation period of Essential Caregiver policy. (2) ECP, Essential Caregiver policy. (3) The log transformation outlined in the Empirical Strategy section is used for estimating the treatment effects for all three outcomes. (4) The shaded area represent 95% confidence intervals. Standard errors are clustered at state level.

TABLE 4 Effects of essential caregiver policies on new non-COVID-19 deaths, new COVID-19 deaths, and new total deaths in nursing homes, stratified by overall quality, staffing level, profit status, restriction status of essential caregiver policies, and mandatory status of essential caregiver policies.

	High-quality (1)	Low-quality (2)	High-staffing level (3)	Low-staffing level (4)	Not-for-profit (5)	For-profit (6)	ECP without restriction (7)	ECP with restriction (8)	Mandatory ECP (9)	Non-mandatory ECP (10)
New non-COVID-19 deaths per 1000 residents	−0.1041***	−0.0690	−0.0970**	−0.0675	−0.1088*	−0.0982**	−0.1117**	−0.0278	−0.1683***	−0.0815
	(0.0387)	(0.0634)	(0.0465)	(0.434)	(0.0617)	(0.0428)	(0.0480)	(0.0295)	(0.0343)	(0.0533)
New COVID-19 deaths per 1000 residents	−0.0858	−0.0599***	−0.1132	−0.0968***	−0.0889	−0.1064***	−0.1140*	−0.1155***	−0.1615*	−0.0512
	(0.0604)	(0.0213)	(0.0972)	(0.0228)	(0.0891)	(0.0379)	(0.0665)	(0.0282)	(0.0899)	(0.0437)
New total deaths per 1000 residents	−0.1536**	−0.1048	−0.1598*	0.1391***	−0.1813*	−0.1690***	−0.1777**	−0.1545***	−0.2658***	−0.1298**
	(0.0626)	(0.0765)	(0.0893)	(0.0442)	(0.1018)	(0.0517)	(0.0735)	(0.0332)	(0.0673)	(0.0546)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	93,508	113,434	102,372	104,514	45,086	150,152	206,658	178,569	186,416	198,811
Mean of new non-COVID-19 deaths per 1000 residents	5.79	4.82	5.43	5.09	6.37	4.94	5.19	5.34	5.20	5.32
Mean of new COVID-19 deaths per 1000 residents	2.50	2.57	2.46	2.61	2.54	2.53	2.40	2.44	2.40	2.44
Mean of new total deaths per 1000 residents	8.28	7.39	7.89	7.70	8.91	7.47	7.59	7.78	7.59	7.75

Note: (1) The results displayed are estimated using the Callaway and Sant'Anna (2021) estimator in the restricted sample. (2) In stratification analyses, some cohort-period groups of treatment cohort of which Essential Caregiver policies were implemented in the period ended on January 3, 2021 have number of records that are fewer than number of covariates being adjusted, which could cause model being overfitted. To circumvent this problem, we excluded all records of nursing homes of this treatment cohort (Rhode Island), which take up around 0.57% of the study sample. We repeated the main analysis on this sample and found similar results as the ones showed in Table 3. (3) High-quality nursing homes: facilities with overall 5-star rating above the median (3-star) at baseline period. Low-quality nursing homes: facilities rated 3-star or below at baseline period. (4) Nursing homes with high staffing level: facilities with case-mix adjusted total nurse staffing score greater than 50 points (adjusted total nurse hours per resident per day greater than 3.72) at baseline period. Nursing homes with low baseline staffing level: facilities with case-mix adjusted total nurse staffing score smaller than or equal to 50 points (adjusted total nurse hours per resident per day smaller than or equal to 3.72) at baseline period. (5) ECP, Essential Caregiver policy. (6) States implemented Essential Caregiver policies with restrictions: Delaware, Illinois, Indiana, Michigan, Oregon, South Dakota, Tennessee, and Washington. (7) States implemented mandatory Essential Caregiver policies: Delaware, Florida, South Dakota, and Texas. (8) The log transformation outlined in the Empirical Strategy section is used for estimating the treatment effects for all three outcomes. (9) Standard errors clustered at state level are included in parentheses. (10) $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Similar to the quality level stratification results, when stratifying by nursing home baseline staffing level, we find significant effects of ECPs on reducing non-COVID-19 deaths and total deaths for nursing homes with a higher baseline staffing level. However, in lower-staffed facilities, these policies produce a substantial decrease in COVID-19 deaths, consequently resulting in a reduction in total deaths. These results support our hypothesis that highly staffed facilities may be better able to provide enough visitation opportunities, while the additional care provided by essential caregivers may positively impact COVID-19 outcomes for residents in facilities with lower staffing level. These estimates are displayed in columns 3–4 of Table 4.

When stratifying our analysis by nursing home profit status, we find that the implementation of ECPs is associated with similar reductions in non-COVID-19 and total deaths between not-for-profit nursing homes and for-profit nursing homes. In contrast, we observe a significant reduction in COVID-19 deaths specifically for for-profit nursing homes. On one hand, these results are consistent with our hypothesis that for-profit nursing homes may have had better access to PPE and testing, which may strengthen their ability to control the spread of the virus. On the other hand, these findings do not strongly support our hypothesis that not-for-profit nursing homes might be able to implement ECPs more effectively. However, this inconsistency may also be attributed to the smaller sample size of not-for-profit nursing homes. These estimates are reported in columns 5–6 of Table 4.

Next, when stratifying our analysis by whether each state's ECP had restrictions, as expected if there is a dose-response type of effect, we find that policies without restrictions significantly reduce non-COVID-19 deaths and have a larger impact on decreasing total deaths. In contrast, policies with restrictions do not have a significant effect on non-COVID-19 deaths but effectively lower both COVID-19 deaths and total deaths. These estimates are reported in columns 7–8 of Table 4.

Similarly, we find that mandatory policies are associated with greater reductions in all three deaths outcomes, while the effects of non-mandatory policies are much smaller and only reach statistical significance for total deaths. As adoption rates are expected to be substantially higher in states with mandatory policies, these results reinforce the dose-response effect we found in the stratification by whether the policies had restrictions and provide stronger evidence for a causal relationship between ECPs and nursing home deaths. We present these estimates in columns 9–10 of Table 4.

Finally, we test if the effects of ECPs are heterogenous by community COVID-19 prevalence level by including an interaction term between the ECP indicator and community COVID-19 prevalence in the two-way fixed effects models. We find that the estimated coefficients associated with the interaction terms are not statistically significant for any of the three deaths outcomes (see Table A10 in Appendix). These results imply that the effects of ECPs on the deaths outcomes of nursing home residents are not heterogeneous by community COVID-19 prevalence level.

5.3 | Robustness checks

First, a common concern with analyses of state policies is unobserved confounding, in particular that implementation of the policies occurred at the same time as other policies that could affect COVID-19 outcomes in nursing homes. More specifically, our estimated results would be biased if there are unobserved state policies that are associated with both states' choice of implementing ECPs and trends in death outcomes of nursing home residents. Although inherently untestable, to mitigate this concern, we collect information on other COVID-19 mitigation policies that are potentially related and summarize the timing of their implementation.[19] These policies would be less likely to confound the relationship between ECPs and death outcomes if their dates of implementation are significantly different from those of ECPs. We summarize the implementation timing of other COVID-19 mitigation policies in Appendix Figure A6 to examine the existence of other, potentially confounding policies. We do not find that the start times of ECPs coincide with any other general mitigation policies across states.

Second, given that COVID-19 vaccination became available for nursing home staff and residents in some states as early as December 2020 (Sinha & Konetzka, 2022), we conducted a sensitivity analysis ending our study period as of January 3, 2021, in order to mitigate any potential confounding effects of vaccination on COVID-19 risks. In Appendix Table A11, we report the results of this sensitivity analysis. Compared to the estimated effects from our main analysis, the effect of ECPs on non-COVID-19 deaths estimated in the sub-sample is slightly smaller, while the estimated effect on COVID-19 deaths is marginally larger. As we have shown in Figure 4 that the dynamic event study effects of ECPs on non-COVID-19 deaths consistently increase over time, while the estimated effects on COVID-19 deaths start to wane in the last few post-treatment periods, the estimated effects found in this sub-sample are in line with our main analysis.

Finally, we also conduct a robustness check on the use of ordinary least squares (OLS) regressions. In our main specification, we treat dependent variables, the number of new deaths, as continuous variables and estimate the models using OLS regressions on transformed dependent variables. We make this choice because the alternative estimators that correct for mismeasurement in a difference-in-differences framework with staggered treatment timing are built upon OLS regressions, with unclear validity in other models. Addressing potential bias caused by staggered treatment timing was critical in this analysis. However, it may be more appropriate to treat these outcomes as count data since they are nonnegative integers with a skewed distribution. We thus test the sensitivity of our results to this choice using two-way fixed effects negative binomial regressions, which better accommodate count data with a large fraction of zeros, modeling raw (untransformed) death outcomes. These estimates, shown in Appendix Table A12, have the same direction as the estimates from the two-way fixed effects OLS regressions. The size of the estimated effects are larger for new non-COVID-19 deaths and new COVID-19 deaths, but smaller for new total deaths. The general similarity of these estimates suggests that the application of linear estimation to our outcomes does not result in substantial bias, although it may bias our magnitudes somewhat.

6 | DISCUSSION

In this analysis, we present empirical estimates of the effects of ECPs on new non-COVID-19, COVID-19, and total deaths for nursing home residents, using a difference-in-differences framework. We find that ECPs effectively reduce both non-COVID-19 deaths and COVID-19 deaths, thereby reducing total deaths for nursing home residents.

In addition to our preferred estimator, we conduct our analysis using multiple difference-in-differences estimators to address potential bias. Our findings are robust across these approaches, with some differences in magnitude.

In our study sample, nursing homes on average report 5.27 new non-COVID-19 deaths per 1000 residents, 2.50 new COVID-19 deaths per 1000 residents, and 7.77 new total deaths per 1000 residents in a 2-week period. The results estimated from our preferred model suggest that on average, ECPs prevent 8.77% new non-COVID-19 deaths, 10.38% new COVID-19 deaths, and 15% new total deaths in a 2-week period. These estimates imply that ECPs could prevent a meaningful proportion of new pandemic-era deaths in nursing homes during our study period. Importantly, the greater estimated effects we find for nursing homes in states with ECPs that are mandatory or implemented without restrictions demonstrate a clear *dose-response* relationship between the implementation of ECPs and lower deaths for nursing home residents, which supports plausible causality of the estimates.

In tests of heterogeneity, we find the estimated effect of ECPs on reducing non-COVID-19 deaths is driven by nursing homes with higher quality and nursing homes with higher staffing levels. Conversely, we find these policies help to decrease COVID-19 deaths, specifically for for-profit nursing homes and those with lower quality or staffing levels. On one hand, these findings suggest that high quality and high staffing levels are key to the effectiveness of ECPs in reducing non-COVID-19 deaths, perhaps because these facilities have the slack in resources and staffing to accommodate more visitation by essential caregivers. On the other hand, our findings suggest that the additional hands-on care and monitoring provided by essential caregivers play a critical role in maintaining the safety of residents in nursing homes with lower quality or staffing levels during the pandemic. This additional care might replace a meaningful portion of the services typically offered by nursing home staff, leading to a significant reduction in the daily interaction between nursing home residents and staff.

Although it might be more challenging to allow visitation when in a COVID-19 hotspot, we find the effectiveness of ECPs in reducing non-COVID-19 deaths, COVID-19 deaths and total deaths are not affected by community COVID-19 prevalence. Yet, the beneficial effects of ECPs are driven by states that did not dictate to nursing homes when they may or may not allow caregivers to visit. Although we do not have data on facility-specific policies, we suspect that state restrictions did not just reduce visitation in risky times but effectively dissuaded some facilities from implementing visitation at all, given the additional need for monitoring and adherence to the restrictions. However, we note that the additional or reinforced COVID-19 mitigation measures implemented through ECPs may contribute to a significant reduction in total deaths, primarily through decreasing COVID-19 deaths, for nursing homes residents in states that implemented these policies with restrictions. These findings provide additional insights into the decrease in COVID-19 associated with the implementation of ECPs and suggest that these policies benefit nursing home residents even when implemented with some restrictions.

The findings from this study should be viewed with respect to several limitations. First, our analysis is based on facility-period level data, which does not include patient-level risk factors. Future research using patient-level claims and assessment data, accounting for patient-level clinical and demographic characteristics, is warranted to check the robustness of our findings to the use of microdata. Second, we use COVID-19 death counts that are self-reported by nursing homes and derive non-COVID-19 death counts based on those self-reported data. Considering variations in testing availability and practices, these death counts are subject to potential measurement error. We minimize this problem by only including records that pass CMS data assurance checks and exclude records of nursing homes reporting dubious COVID-19 death counts or total death counts, but misreporting errors may remain. Third, we examine only three outcomes, COVID-19 deaths, non-COVID-19 deaths, and total deaths, arguably the most extreme outcomes; data limitations preclude examining less extreme outcomes related to quality of life, such as mental health or satisfaction. However, our finding that ECPs affected deaths suggests that less extreme outcomes were also likely to be affected. Finally, with these data we are unable to examine the exact mechanisms for the positive effect of ECPs on reducing COVID-19 deaths. Future research with patient-level data or mixed-methods approaches could shed some light on this question.

In addition to shedding light on nursing home behavior during the COVID-19 pandemic, our results have important implications for policy and practice. Despite a strong argument for visitation restrictions at the outset of the pandemic, our study confirms the adverse effect of extreme isolation on nursing home residents found in prior research (Cronin & Evans, 2022; Holt-Lunstad et al., 2015; Melo, 2022) and furthermore provides support for a reasonable mitigation approach. Our findings suggest that nursing home residents derive substantial benefit from regular social interaction with their family members and friends as well as from the additional care they provide. This is consistent with prior research on the complementary effects of informal care for residents of long-term care facilities (Coe & Werner, 2022). Critically, we also show that the relaxation of visitor restrictions in a systematic and careful way does not increase the risk of COVID-19 deaths, suggesting the need for more nuanced nursing home visitation guidelines during potential future waves of COVID-19 and future unrelated outbreaks of infectious disease. In addition, our findings suggest that restrictions associated with ECPs should be carefully assessed, as we show evidence that the effectiveness of policies will not be weakened by high community COVID-19 prevalence, and nursing home residents benefit less from policies with blunt, state-level restrictions. The CMS, state health departments, and nursing homes themselves need to consider the tradeoffs between protecting nursing home residents from infectious disease and maintaining necessary social interaction for nursing home residents. Our analysis supports the use and expansion of ECPs as a successful model for finding that balance.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available in COVID-19 Nursing Home Data at https://data.cms.gov/covid-19/covid-19-nursing-home-data.

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ENDNOTES

[1] "Guidance for Infection Control and Prevention of Coronavirus Disease 2019 (COVID-19) in Nursing Homes (REVISED)" issued by the CMS on March 9, 2020. Available at: https://www.cms.gov/files/document/qso-20-14-nh-revised.pdf (Wright, 2020).

[2] At the height of federal visitation restrictions from March 2020 to March 2021, Arizona, Delaware, Florida, Illinois, Indiana, Michigan, Minnesota, Missouri, Nebraska, New Jersey, Oklahoma, Oregon, Rhode Island, South Dakota, Tennessee, Texas, and Washington implemented Essential Caregiver policies.

[3] The four states implemented their Essential Caregiver policies through executive orders or emergency rules are: Delaware, Florida, South Dakota, and Texas.

[4] Washington and Tennessee required a 28-day period since the last facility outbreak. South Dakota required a 14-day period since the last facility outbreak. While Illinois discouraged Essential Caregiver visitation during active facility outbreaks, it let facility administrators

make this determination on a case-by-case basis. Oregon required a county's COVID-19 risk level to be below "extreme risk (counties with >15,000 people: ≥10.0% percentage test positivity over previous 14 days; counties with 15,000–30,000 people: ≥60 COVID-19 cases over 14 days; counties with 30,000+ people: ≥200.0 of COVID-19 cases per 100,000 over 14 days)" for the Essential Caregiver policy to operate.

[5] Texas and Indiana allowed essential caregivers to visit residents who were confirmed or suspected to have COVID-19 in limited circumstances depending on the vaccination and infection history of the essential caregiver and/or resident.

[6] CMS flags reports that may be entered incorrectly, such as cumulative COVID-19 cases being entered as new COVID-19 cases and other obvious data entry errors, and we exclude these.

[7] To see this, we use $Y(1)$ and $Y(0)$ to denote the potential outcomes under treatment and control, respectively, and both of which are strictly positive. The average treatment effect on the treated (ATT) can be written as follow: $ATT = P(Y(1) > 0, Y(0) > 0)E_p[m(Y(1)) - m(Y(0))|Y(1) > 0, Y(0) > 0, D = 1] + P(Y(1) > 0, Y(0) = 0)E_p[m(Y(1)) - m(0)|Y(1) > 0, Y(0) = 0, D = 1] + P(Y(1) = 0, Y(0) > 0)E_p[m(0) - m(Y(0))|Y(1) = 0, Y(0) > 0, D = 1]$. The first term on the right-hand side represents the intensive margin and the other two terms represent the extensive margins. If $ATT > 0$, the second term is underestimated due to the use of transformation function $m(\cdot)$ and the third term is zero. Similarly, if $ATT < 0$, the third term is underestimated due to the use of transformation function $m(\cdot)$ and the second term is zero.

[8] See Appendix. Brief Summary of the Goodman-Bacon (2021) Decomposition.

[9] We conduct the Goodman-Bacon (2021) decomposition in a balanced panel, as it is a requirement for this technique, excluding observations of nursing homes with any missing weekly reports within the study period.

[10] See Appendix. Brief Summary of the Sun and Abraham (2021) Estimator and the Callaway and Sant'Anna (2021) Estimator.

[11] When implementing these estimators, we first exclude observations of nursing homes in states (Indiana and Michigan) that passed Essential Caregiver policies in the first month of our study period, since both estimators require always-treated units to be excluded from estimation. Additionally, we exclude observations of nursing homes in South Dakota and Minnesota, which implemented their Essential Caregiver policies in 2-week periods three and four, respectively, resulting insufficient data points in pre-treatment period for examining the parallel-trends assumption. Because the Sun and Abraham (2021) estimator has stricter requirements in cases where the parallel-trends assumption holds conditionally on covariates, we exclude observations of nursing homes in Nebraska, as clear violations of the parallel-trends in both non-COVID-19 deaths and COVID-19 deaths are observed (see Appendix Figures A2 and A3) for the last few pre-treatment periods. For the purpose of further reducing potential bias arising from unobserved differences between states with and without Essential Caregiver policies, we use not-yet-treated units as controls in these estimations.

[12] We report the dynamic event study results of the stratification analyses estimated from the Callaway and Sant'Anna (2021) model in Appendix Tables A5–A9. Across most of the pre-treatment periods, the estimated effects are not statistically significant, suggesting that the parallel-trends assumption holds reasonably well in these analyses.

[13] In stratification analyses, some cohort-period groups of treatment cohort 8 (cohort of Essential Caregiver policies implemented in the period ended on January 3, 2021) have number of records that are fewer than number of covariates being adjusted, which could cause model being overfitted. To circumvent this problem, we excluded all records of nursing homes of treatment cohort 8 (Rhode Island), which take up around 0.57% of the study sample. We repeated the main analysis on this sample and found similar results as the ones showed in Table 3.

[14] Those with adjusted total nurse hours per resident per day greater than 3.72 at baseline. The cut points for case-mix adjusted total nurse staffing score are defined in the Nursing Home Compare Technical User' Guide (https://www.cms.gov/medicare/provider-enrollment-and-certification/certificationandcomplianc/downloads/usersguide.pdf) prepared by CMS (CMS, 2021).

[15] We leave government-owned facilities out of this stratification, as they constitute a small minority of facilities and there is no clear hypothesis associated with them.

[16] Among the 17 states that implemented Essential Caregiver policies, four states enforced their policies mandatorily through executive orders or emergency rules, whereas policies in other states were issued as non-mandatory regulatory guidance.

[17] Eight states, including Delaware, Illinois, Indiana, Michigan, Oregon, South Dakota, Tennessee, and Washington, implemented their Essential Caregiver policies with restrictions related to the level of facility and community spread of COVID-19.

[18] Stratifying by community COVID-19 prevalence level will result in some tiny treatment cohort-period groups, which could lead to overfitting when using the Callaway and Sant'Anna (2021) model. As Callaway and Sant'Anna (2021) model does not allow for interaction terms, we employ the canonical two-way fixed model with an interaction between the Essential Caregiver policy indicator and community COVID-19 prevalence to test if there is any heterogeneous effect by community COVID-19 prevalence level.

[19] Although each of these policies is nuanced and incorporates different degrees of restriction, we classify them into binary categories according to whether any restriction was implemented. We consider any form of mandatory quarantine for travelers, large gatherings ban, non-essential business closure, stay at home order, or masking requirement as an active policy implementation.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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