Does Paid Family Leave Reduce Nursing Home Use? The California Experience

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Abstract

The intent of Paid Family Leave (PFL) is to make it financially easier for individuals to take time off from paid work to care for children and seriously ill family members. Given the linkages between care provided by family members and the usage of paid services, we examine whether California's PFL program influenced nursing home utilization in California during the 1999 to 2008 period. This is the first empirical study to examine the effects of PFL on long-term care patterns. Multivariate difference-in-difference estimates across alternative comparison groups provide consistent evidence that the implementation of PFL reduced the proportion of the elderly population in nursing homes by 0.5 to 0.7 percentage points. Our preferred estimate, employing an empirically-matched group of control states, finds that PFL reduced nursing home usage by about 0.65 percentage points. For California, this represents an 11 percent relative decline in elderly nursing home utilization.

INTRODUCTION

Conflicts between paid work and family life have become increasingly salient in the United States. Women comprise nearly half of the labor force (U.S. Bureau of Labor Statistics, 2015), a pattern accompanied by a rise in the number of employed adults who simultaneously provide care to family members (Wolff & Kasper, 2006). Population aging and improvements in life expectancy are likely to exacerbate this tension by increasing the demand for elder care. At the same time, declining fertility and high divorce rates complicate the division of caregiving responsibilities within families, further limiting individuals' capacities to manage this tradeoff.

While several industrialized nations offer statutory entitlement to Paid Family Leave (PFL) to help employees balance the demands of employment and family care, the United States does not. Until the Federal Family Medical Leave Act (FMLA) was enacted in 1993, the United States did not provide any right to family leave. The FMLA requires that employers provide 12 weeks of family leave to qualifying workers with a newborn or a sick child, spouse, or parent (U.S. Department of Labor, 2015). However, in contrast to several Organisation for Economic Co-operation and Development (OECD) countries, FMLA leave in the United States is *unpaid* and limited in duration (Yang & Gimm, 2013).

Despite the absence of a PFL law at the Federal level, some states currently require employers to offer such leave to eligible employees in order to care for a newborn or a seriously ill family member. California was the first to do so in 2004, allowing

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six weeks of PFL with 55 percent of usual pay replaced, up to $1,104 per week in 2015 (Bartel et al., 2014). Funding for this leave comes from a payroll tax levied on employees. All private sector workers, and some public sector employees, who have worked at least 300 hours during a base period five to 17 months before filing a claim, are eligible for the program, with no firm-size exemptions. There is a one-week waiting period before benefits begin, except for new mothers transitioning from California's Temporary Disability Insurance program (Baum & Ruhm, 2016). Since 2004, several other states have also adopted PFL laws. Washington passed a law providing workers with PFL after the birth or adoption of a child in 2007, but implementation has been delayed. New Jersey and Rhode Island implemented laws similar to California's in 2009 and 2014, respectively. Most recently, both New York (2016) and Washington, DC (2017) passed PFL laws.

The increasing momentum surrounding PFL policies calls for a comprehensive understanding of their consequences. Existing research has mainly focused on direct effects of family leave policies on employees and employers. For instance, studies provide consistent evidence that state-level PFL policies increase labor market attachment among new parents (Gault et al., 2014). Scholars have also concluded that firms incurred few costs but gained substantial benefits (in terms of staff morale and increased probability of employee's return to work) as a consequence of California's PFL program (Appelbaum & Milkman, 2011).

Relatively little attention has been paid to analyzing indirect effects of PFL programs. A few studies have examined health and socio-emotional benefits of PFL policy. Stearns (2015) found that paid maternity leave policy led to a reduction in low-weight births. Similarly, Lichtman-Sadot and Bell (2017) demonstrated improvements in health outcomes among California's elementary school children following the introduction of PFL. Gimm and Yang (2016) examined the effect of California's PFL law on mental and physical health outcomes of family caregivers but found no effect of the policy change. However, the impact of PFL policies on the utilization of either public or market-provided services, such as nursing home stays among older adults or day care for children, has not been studied.

In this paper, we evaluate whether the PFL program influenced aggregate nursing home use in California over the 1999 to 2008 period. We focus on nursing home care because it accounts for the largest proportion of long-term care (LTC) costs in the country. Medicaid is the primary payer for over 63 percent of nursing facility residents, some of who deplete their assets in order to become eligible for the program (Kaiser Commission on Medicaid and the Uninsured, 2013). Medicare, which mainly covers short stays following a hospitalization, accounts for approximately 15 percent of nursing home utilization. Not only is institutional care financially burdensome for individuals as well as state and Federal budgets, it is also widely unpopular, as most seniors prefer to receive LTC supports and services in their residences and communities (Kane & Kane, 2001).

Thus, ways to divert or delay individuals from entering nursing facilities remain a priority among policymakers and state officials. To shift the mix of publicly-funded LTC away from institutions, existing efforts have relied mainly on encouraging the growth of Home- and Community-Based Services (HCBS)—a goal often referred to as "rebalancing" state LTC systems (Gornick, Howes, & Braslow, 2012). As a result, relative spending on HCBS has increased from 18 percent of Medicaid LTC spending in 1995 to 51 percent in 2013 (Health Policy Brief, 2015). However, studies show that the growth of HCBS has achieved only modest success in reducing nursing home utilization (Weissert & Frederick, 2013).

To examine the effect of PFL on nursing home utilization, we use state-level panel data and employ a multivariate difference-in-differences (DD) approach. Because the "treatment" (California's PFL law) is not randomly assigned, we use an empirical method, cluster analysis, to construct a comparison group, and test the robustness of our findings using alternative comparison groups and placebo tests. We also use inferential tools found in Ferman and Pinto (2016) to deal with the problems posed by having only one treatment state as well as heteroscedastic errors in the DD regression.

The main contribution of this paper is that it analyzes an indirect and possibly unexpected consequence of PFL: aggregate nursing home usage. To our knowledge, there have been no formal evaluations of PFL policies on LTC outcomes in the United States. In view of advancing population aging and the high costs of nursing facility care, our results may have important implications for LTC financing in the country.

THEORETICAL FRAMEWORK

Broadly speaking, older people unable to address their care needs without assistance from others must either live in institutional facilities (i.e., a nursing home) or at home[1] while receiving assistance from some mix of "formal" (paid) care and "informal" (unpaid) care. Informal care includes care provided by family members and friends. PFL alters the circumstances by which an employee can take time off of work for purposes of caring for a family member. Whether this will change nursing home usage depends on whether the new policy changes the supply of family care, operating through both supply and demand effects in the labor market, and on the degree to which family care and nursing home care are substitutes.

Economic models of how individuals allocate their time between paid employment and caregiving have been developed by Johnson and Lo Sasso (2000), Van Houtven and Norton (2004), and others. In these models, the amount of parent care supplied by an employed worker (which may be zero) reflects many factors, notably the market wage, the prices and availability of alternative sources of care, the parent's care needs, and the child's preferences regarding how those needs are met. The recent empirical literature on informal caregiving generally supports a conclusion that caregivers work fewer hours than non-caregivers, particularly if their caring commitments are heavy (Lilly, Laporte, & Coyte, 2007; Van Houtven, Coe, & Skira, 2013). In addition, other studies have found that among surveyed employed caregivers, a majority are likely to make informal arrangements (arriving to work late or leaving work early or taking time off during the day) in order to accommodate caregiving demands (MetLife and National Alliance for Caregiving, 2006).

In the absence of PFL, a worker might carry out a plan to take unpaid leave in order to provide a limited amount of parent care, where that care is part of a more extensive program of care that is both anticipated and shared among family members or other providers. More likely, however, care provision in this context represents the worker's response to an unanticipated shock to the system, such as a catastrophic health event befalling the parent, or the failure of alternative suppliers of care to materialize.

The decision to take unpaid leave is driven in part by consideration of any associated loss of earnings. The introduction of PFL represents a treatment that compensates for the earnings losses faced by a worker considering whether to take time off to care for a family member. Specifically, it provides a temporary subsidy for zero hours of work. Other things being equal, this policy change is expected to increase leave-taking behavior among employees who, in the absence of PFL, would not have taken unpaid leave to care for dependents, and among those who would have otherwise taken a shorter leave than the duration guaranteed under the statutory PFL

[1] "Home," in this case, includes Assisted Living residences as well as other forms of private housing.

policy. Thus, supply-side considerations indicate that PFL should increase (or at least not decrease) the availability of family caregivers.

These supply-side predictions are supported in the empirical literature addressing both FMLA and PFL. Using data from the 1979 National Longitudinal Survey of Youth, Kerr (2016) finds that the FMLA increased both the probability of mother's leave-taking by 20 percentage points as well as the average leave length, by almost five weeks, across all states after the reform. Further, using Current Population Survey data, Rossin-Slater, Ruhm, and Waldfogel (2013) find that California's PFL law increased leave taking among new mothers by 3.2 weeks on average. Bartel et al. (2017) extend these results, finding that relative to the pre-treatment mean, new fathers in California are 46 percent more likely to be on leave when PFL is available. To our knowledge, Nizalova (2007) is the only study that examines the effect of paid leave on elder care. Focusing on European countries, she finds that the presence of a paid leave program increases the probability of being a caregiver among both male and female working adults with at least one parent or parent-in-law alive. Beyond paid leave, some studies also provide evidence that the use of other benefits, like flexible work arrangements, are positively correlated with transitions into elder caregiving among employed women caregivers (Chelsey & Moen, 2006; Fredriksen, 1996).

It is also necessary to consider the demand side of PFL impacts. California's PFL program is financed by a payroll tax levied on workers, which suggests that the unit costs of labor are not changed. Nevertheless, leave-taking may raise recruitment, training, and retention costs for employers, which could in turn induce a reduction in equilibrium employment levels. This, too, should result in an increase in the supply of potential family caregivers.

Whether increases in informal elder care reduce nursing home usage depends on the degree to which the two care sources are substitutes. Any consideration of the potential for this type of substitution must recognize the variety of situations for which nursing home care is used. Nursing home care usage is generally characterized in binary terms, using a contrast between (a) short-stay, post-acute care following hospital discharge and preceding a return to the community, largely funded by Medicare, and (b) long-stay care for chronic conditions, with admissions frequently from an at-home residential setting and often terminating in death, and with substantial Medicaid funding (Grabowski, 2010; Mor et al., 2010; Reschovsky, 1998). Post-acute nursing facility stays are quite short on average (27 days in 2008, according to Grabowski, 2010), suggesting that the six-week period of PFL offered by California's law could completely eliminate many such episodes. However, patients discharged from a hospital to a nursing home often have come from intensive-care units, and have medically complex conditions (Mor et al., 2010), diminishing the potential for family members to serve as an alternative source of care. Long-stay chronic-care patients, on the other hand, may need only "custodial care" (with little or no medical component) and might need only monitoring and supervision, which can more readily be provided at home with primary reliance on family members' efforts. While the average length of all Medicaid-funded nursing home stays is quite large—two years, according to Grabowski (2010)—end-of-life nursing home stays (regardless of funding source) are considerably shorter, with a median length of five months, and with 25 percent of such stays lasting only one month (Kelly et al., 2010).

Empirical research on the substitutability of nursing-home care and at-home informal care generally fails to address the distinctions between post-acute and chronic care needs, or of the origin (hospital vs. home) and endpoint (live discharge vs. death) of the care episodes. Nevertheless, available evidence suggests that the two care settings serve as substitutes. Lo Sasso and Johnson (2002) find that frequent help from children with basic personal care reduces the likelihood of nursing home use over a subsequent two-year period by about 60 percent for disabled Americans age 70 and older. Other studies using longitudinal data (Charles & Sevak, 2005) and instrumental variables to control for the endogeneity of formal and informal care (Van Houtven & Norton, 2004) also conclude that informal care reduces home health care use and delays nursing home entry.

Thus, both supply- and demand-side labor market considerations, together with the demonstrated substitutability of informal care for nursing home care, lead to a prediction that implementing PFL should reduce nursing home usage. Specifically, the assistance provided by an employee able to take family leave could shorten a period of post-acute institutionalization, or delay the beginning of what could become a lengthy period of institutionalization associated with progressive conditions such as Alzheimer's disease.

DATA AND MEASURES

We examine the relationship between PFL and nursing home utilization among the older population for the 50 states and Washington, DC, using longitudinal, state-level data collected from a number of sources. The data span calendar years 1999 to 2008. The panel ends in 2008 for two reasons: first, a consistent series of information on aggregate nursing home utilization is only available for the years 1999 to 2009. Second, while New Jersey passed its PFL law in 2008, it did not implement it until July 2009. This implies only a half-year of program exposure for New Jersey if the year 2009 is included in the data series. Because California's response would dominate our estimates of program impact in any case, we end our series in 2008 to facilitate a more straightforward interpretation of our results.

Aggregate data are typically used in research on the impacts of policy changes observed only at the state–year level (Cameron & Miller, 2015). While many studies begin with individual-level data and then aggregate to state-by-year combinations for inferential purposes, we begin with aggregate data. Our data cover more years (10) and states (all 50, plus DC), and with a near-absence of sampling variability, than would any individual-level data source of which we are aware. Several past studies also have used state-level data to evaluate the effect of family leave policies (Ruhm, 1998, 2000; Stearns, 2015), indicating its utility for informing the evidence base in this area.

Nursing home utilization, our outcome measure, equals the proportion of a state's older population that resides in a nursing home at any time during a calendar year. The numerator of this proportion is the unduplicated count of individuals age 65 or more that spend one or more nights in a nursing home during each year, obtained from the Center for Medicare and Medicaid Services (CMS) *Nursing Home Compendium* series for years 2000 (covering 1999) through 2009 (covering 2008). The reports are limited to Medicare- and Medicaid-certified nursing homes, which in 2004 represented 98.5 percent of all nursing facilities (and 98.8 percent of all nursing home beds).[2] The denominator of the proportion is the estimated state- and year-specific count of people age 65 and older as of July 1, taken from U.S. Bureau of the Census estimates (2015).

To elaborate on the numerator, the counts of nursing home residents over age 65 are taken from Minimum Data Set (MDS) assessments[3] and include all older adults in a nursing home at any point during the year. For an older adult assessed

[2] Centers for Disease Control and Prevention, National Center for Health Statistics, 2004 Facility Tables (http://www.cdc.gov/nchs/nnhs/facility_tables.htm), Table 1 (accessed 5/29/2015).
[3] See https://www.cms.gov/Research-Statistics-Data-and-Systems/Computer-Data-and-Systems/Minimum-Data-Set-3-0-Public-Reports/index.html.

more than once in the year, possibly in connection with different spells of nursing home residence, only a single assessment is used in the count. Because this measure includes individuals over age 65 in a nursing home at any point during the year, it accounts for both short- and long-stay nursing home episodes, therefore making our measure of nursing home utilization more comprehensive than point-in-time estimates. Moreover, point-in-time sampling of dynamic processes such as nursing home occupancy are known to over-represent longer-duration episodes, a phenomenon known as length-biased sampling (Cox, 1962). After 2010 the CMS data series changed to point-in-time sampling for counting nursing home residents.

Our analysis focuses on the effects of PFL. Between 1999 and 2008, the period covered by our data, only California enacted a PFL law. This law passed in 2002 but went into effect on July 1, 2004 (Employment Development Department, 2014). Thus, the variable representing the presence of PFL in California is coded "0" prior to 2004 and "1" beginning in 2005. The Ferman–Pinto estimator we use demands a clean separation of pre- and post-treatment periods. Because half of 2004 represents a pre-treatment period, and half falls into the post-treatment period, we exclude all cases for 2004 from our estimation. For all states other than California the "presence of PFL" variable is coded as "0" throughout.

Additionally, we have assembled data on several covariates that represent time-varying features of states' LTC environments, including state-level policies and other variables that potentially influence the supply of and demand for nursing home care (discussed below). Many of these state policy levers are associated with Medicaid because, with limited coverage in the private insurance market and few options under Medicare, state Medicaid programs are the primary payers for a majority of LTC services.[4] This set of covariates serves two distinct goals in our analysis: First, we use the covariates as criteria for identifying suitable groups to compare to California; second, conditional on additional tests for their exogeneity, we include selected time-varying covariates in our statistical model for estimating the effect of PFL on nursing home use.

States have historically sought to constrain the growth of the nursing home market through certificate of need (CON) and construction moratorium regulations. When a CON law is present, a nursing home must demonstrate a clinically legitimate rationale for additional beds to the state's CON board. A construction moratorium is even more stringent in that it effectively prevents any expansion within the nursing home sector. We represent these policies with a binary variable for whether a state had either a CON or moratorium in effect during a given year. Previous research indicates only limited effects of such policies on reducing nursing home utilization (Aykan, 2003; Grabowski & Gruber, 2007; Wallace et al., 1998), possibly because falling occupancy rates in nursing homes have meant that these constraints are not always binding (Grabowski, Ohsfeldt, & Morrisey, 2003).

States also vary with respect to the Medicaid reimbursement rates paid to nursing homes. When the Medicaid rate is below the private pay rate, nursing home administrators may be reluctant to admit patients already on Medicaid or those that seem likely to become Medicaid-eligible during their stay (Harrington Meyer, 2001; Sloan, Picone, & Hoerger, 1997). We control for the state's average daily Medicaid reimbursement as well as the average private pay rate for nursing home providers. While early research on the effect of reimbursement policies found that an increase in the "Medicaid discount" (the difference between private pay and reimbursement

[4] Sources for these variables are provided in Appendix A. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

rates) lowered the probability of nursing home use (Cutler & Sheiner, 1994), more recent work has found only small effects (Grabowski & Gruber, 2007).

State policies can also influence the demand for Medicaid-funded LTC services. States can expand Medicaid eligibility, and thereby increase the demand for nursing home care, by instituting a "Medically Needy" option that permits individuals to "spend down" their income to qualify for Medicaid; eligibility in such states also requires that individuals' assets fall below the state's Medicaid asset standard.[5] We control for this using a binary variable for the presence of a Medically Needy provision for nursing homes.[6] While earlier work has found that the presence of Medically Needy programs increases the probability of nursing home use (Cutler & Sheiner, 1994), more recent research finds no effect on nursing home utilization (Grabowski & Gruber, 2007).

We also include factors that may indirectly influence nursing home utilization by altering the availability of community-based services and informal care. As discussed previously, if paid home care substitutes for nursing facility care, then it may be hypothesized that factors that positively influence the availability of home care might reduce the demand for institutional care. Almost all states cover some form of HCBS under a number of optional Medicaid programs. The Personal Care Services (PCS) State Plan is one such program that allows states to cover supportive services to individuals who are eligible for Medicaid and require help with Activities of Daily Living. Beyond PCS, states may also apply for HCBS waivers (or §1915[c] waivers) that allow them to offer a broad range of services possibly targeted to specific groups or those within a limited geographic area in a given state. Two dummy variables indicate whether the state's Medicaid Plan includes a PCS State Plan option, and whether the state has an "Aged" or "Aged/Disabled" 1915(c) HCBS waiver.[7] While there is some evidence that individuals living in states that offer services through HCBS waivers are more likely to receive formal home care (Aykan, 2003), recent studies have generally found that the growth of HCBS has only modestly reduced nursing home utilization (Weissert & Frederick, 2013).

Several states have adopted family leave provisions broader in scope than those mandated by the federal FMLA. Expanding FMLA provisions may increase the attractiveness of informal care provision. Among these provisions are extending FMLA coverage to workers in businesses with fewer than 50 employees, providing leave for a longer period of time, or by allowing a more inclusive definition of "family," i.e., covering step-parents and parents-in-law. In nearly every case, the states that have adopted these provisions did so prior to 1999. Only one of these provisions—expanding the definition of family to include parents-in-law—exhibited changes during the 1999 to 2008 period, allowing it to be included in our statistical model. (See Appendix B for the timing of each type of FMLA expansion across states.)

Other key variables include median hourly wage for personal care aides, which can influence both the supply of and the demand for nursing home use. Further, medical practice patterns and hospital discharge practices are known to differ substantially across states (Mor et al., 2010), and to be reflected in both overall levels of nursing

[5] See the Centers for Medicare and Medicaid Services' "Medicare.gov" website at https://www.medicare.gov/your-medicare-costs/help-paying-costs/medicaid/medicaid.html.
[6] The binary indicator is coded as "1" for those states that have a Medically Needy provision for nursing homes only. States that have no Medically Needy provision for nursing homes or a Medically Needy provision for both nursing homes and home health agencies are coded as "0."
[7] Almost all states currently have at least one 1915(c) waiver that focuses on older adults. For the purposes of temporal variation over the study period, our indicator variable for HCBS waivers captures the presence of at least two separate waivers focused on older adults within a given state–year.

home usage and in the mix of long- and short-stay nursing home episodes. We control for these sources of variation using two variables, Skilled Nursing Facility (SNF) days and Home Health visits, both of which are expressed in per-capita terms and are limited to Medicare-covered services.

Time-varying economic characteristics include a measure of state-level fiscal stringency (the size of the state's reserve funds, expressed as the percentage of fiscal year the reserves could cover), per capita income, and the poverty rate for the child population; the latter is included to account for possible competing claims on public resources. Finally, with regard to demographic characteristics, we account for the proportion of female population among the state's 65-and-older population, the proportion of the 65-and-older population in the state that is "oldest old" (i.e., 85 or older), and the proportions of state populations that are black, Hispanic, or of some other racial group.

METHODS

We use a DD regression model for estimating the effect of California's PFL law. The DD estimator contrasts changes in nursing home utilization in California before and after the enactment of its PFL policy to the corresponding changes in nursing home utilization in a set of comparison-group states. Our basic DD model is of the form

$$Y_{st} = \gamma + \delta PFL_{st} + X_{st}\beta + \tau(t - 1999) + \theta_t + \alpha_s + \varepsilon_{st} \tag{1}$$

where s indexes states and t indexes years (1999, . . . , 2008 but excluding 2004). We regress the proportion of elderly in nursing homes in state s and year t (Y_{st}) on a treatment variable that indicates that a PFL law has been implemented in state s and year t, as well as an array of other time-varying, state-specific policy, economic and demographic variables (X_{st}). This specification assumes the existence of common time trends (represented by τ) in California and the comparison-group states in the absence of the treatment, net of the effects of any time-varying covariates. Equation (1) also includes state (α_s) and calendar-year (θ_t) fixed effects (the latter beginning in 2001).

Apart from its inclusion of the trend and the time-varying covariates, equation (1) is equivalent to the simplest form of the widely-used basic DD regression, which incorporates a "treated unit" indicator, a "post" or "treated period" indicator, and an interaction of the two. In the canonical form of the model, the coefficient on the interaction of "treated" and "post" provides a direct estimate of the DD treatment effect. With our single treated state, the state-level fixed effect for California corresponds to the "treated unit" indicator, while the year dummies for 2005 to 2008 absorb the "treated period" effect. Our *PFL* dummy implicitly interacts the California dummy with the sum of the 2005 to 2008 year dummies, producing an estimate of the treatment effect.

Note also that equation (1), expressed in terms of aggregated state–year observations, is equivalent to an individual-level linear probability model of the outcome "any nursing home occupancy in year t," if a complete census of the 65-and-older population were available for analysis, and provided that all covariates were measured at the state level (e.g., rather than including a dummy variable indicating female individuals, controlling at the individual level for the proportion of females in the state population). While we do not have data on individual nursing home occupancy, the numerators of our outcome variable come from complete censuses of 65-and-older nursing home residents in each state–year combination. Thus, our aggregated data agree with what would be obtained if the necessary individual-level data were aggregated to the state–year level. Accordingly, we estimate equation (1) using weighted least squares, using weights proportional to the size of the 65-and-older population in each state–year combination.

Valid inferences about program effects based on nonexperimental data rest on an assumption that the expected value of the outcome variable in the comparison group during the treated period, net of all systematically controlled factors, represents the counterfactual for the treated units during that same period in the absence of the treatment. The fixed effects and time-varying covariates are included in order to account for the influence of time-invariant and time-varying observed factors, while the time trend is included in order to account for the influence of common time-varying but unobserved factors. The assumption—that the trend in California would not have changed, relative to that in the comparison states, in the post-treatment period in the absence of the treatment—cannot, strictly speaking, be tested. However, the assumption can be made more reasonable through the careful selection of a comparison group.

Selecting Comparison Groups

In our situation, with a single treated state, the potential applicability of empirical *matching* approaches (such as the propensity score method) to construct the control group is severely constrained as it would provide us with just one matched comparison-group state. Instead, we use clustering techniques—*data* clustering, not to be confused with clustering of regression errors—to identify a set of homogenous groups, each containing one or more states that are similar to each other with regard to a given set of observed attributes. Clustering techniques are widely used in applications such as taxonomy, market research, genetic analysis, and the targeting of medical and health-service interventions, and have also been used to select comparison groups for purposes of estimating treatment effects with nonexperimental data (e.g., Peck, 2005; Weitzman, Silver, & Dillman, 2002). While previous studies on the effect of PFL have used observations from all states other than California (Bartel et al., 2017; Lichtman-Sadot & Bell 2017), or the three next-largest states, to define the comparison group (Rossin-Slater, Ruhm, & Waldfogel, 2013), cluster analysis allows for the creation of a comparison group based on empirical similarity criteria, thus making it less reliant on subjective choices.

The purpose of clustering is to partition observations into non-overlapping groups such that within each group, units are similar to each other, while the units in different groups are dissimilar to each other (Kaufman & Rousseeuw, 2005). We use a large number of observables to identify clusters, including the pre-treatment (1999 to 2003) slope from a state-level regression of nursing home utilization on year—i.e., the pre-treatment trend—as well as the pre-treatment average of each of the 18 time-varying covariates previously described.

There are numerous clustering algorithms available, as well as an extensive set of formal criteria for choosing the appropriate number of distinct groups that can be found within a given data structure. We used the *hierarchical clustering algorithm* based on group centroids (i.e., within-group similarity is assessed using the distance of each observation from its group's centroid, and between-group dissimilarity is assessed by the distance between group centroids). Further, we determined the number of groups based on both substantive grounds (i.e., the number of states in the cluster that contains California) and a formal goodness-of-fit criterion, the well-established Duda–Hart $J_e(2)/J_e(1)$ statistic (Duda & Hart, 1973; Milligan & Cooper, 1985). For the clustering solution found in this way, we selected as our primary comparison group for California the other states also found in what we henceforth refer to as the "California cluster." The states in this cluster are objectively similar to each other in the high-dimensional space used in the clustering algorithm, which

Table 1. Groupings of states into homogeneous clusters and "family friendly" comparison group.

Cluster	States
1	CA,[a] DE, ME,[a] MA, MD, MN,[a] NH, NJ,[a] NM, ND,[a] OH, PA, RI,[a] VA, VT,[a] WA,[a] WI[a]
2	AL, AZ, AR, CO, FL, GA, ID, IL, IN, IA, KS, KY, MI, MO, MS, MT, NC, NE, NV,[a] OK, OR,[a] SC, SD, TN,[a] TX, UT, WV
3	HI[a]
4	WY
5	LA
6	CT,[a] NY[a]
7	DC[a]
8	AK

[a]In "family-friendly" comparison group.

is desirable from the perspective of making causal inferences. Importantly, we can still include all of the fixed- and time-varying elements of equation (1) in the DD regression that uses the states in this cluster. Time-varying exogenous covariates play the important role of reducing residual variances, lending greater precision to our estimated treatment impacts.[8]

In addition to this primary comparison group, we also construct two alternative comparison groups. The first alternative control group consists of states with laws that either exceed the minimum requirements of the Federal FMLA or that currently provide paid leave through PFL policies (Table 1 indicates which states are included in this group). The legislative initiatives of these states indicate their tendency to impose "family friendly" workplace policies, establishing their plausibility as control-group states. To the extent that passing a PFL law reflects otherwise unmeasured variables beyond those captured by fixed and time-varying elements included in the model, this collection of states should be similar with respect to those unobservables. About half the states in this alternative comparison group are also found in the California Cluster (see Table 1). The second alternative comparison group consists of *all* states (plus DC). Because this basis for grouping states ignores their pre-treatment trends in nursing home utilization, when we use these comparison groups, we also control for time trends at the cluster level, with clusters defined by the same algorithm as described in the previous section.

For both of these alternative groupings, our regression may not fit perfectly into a strict DD framework. Instead, by introducing cluster-specific trends we infer program impacts by differencing California's *displacement from trend* to the displacement from comparison clusters' trends, assuming that the displacement from trends found in untreated states represent a valid counterfactual for California.[9]

[8] Some studies have used the Synthetic Control Method (SCM) (Abadie, Diamond, & Hainmueller, 2010) to create a single comparison unit. However, the applicability of SCM is limited in our case given that we have only five pre-intervention periods. Having a sizable number of pre-intervention periods is important to ensure the credibility of the synthetic control unit (Abadie, Diamond, & Hainmueller, 2015) as SCM rules out the inclusion of state and year fixed effects as well as post-intervention values of time-varying covariates and the linear time trend. Successful applications of SCM have often used data with many more pre-treatment periods (Adabie, Diamond, & Hainmueller, 2010, 2015; Stearns, 2015).
[9] This approach to inferring program impacts is similar to the "comparative interrupted time series" designs used in many studies, e.g., Dee and Jacob (2011) or Rodgers, St. John, and Coleman (2005).

Inferring Treatment Effects

Beyond selecting the comparison group, another issue that arises when there is only a single treated group is the problem of inference (Cameron & Miller, 2015). In such a case, regression-based estimates of treatment impacts are inconsistent, because very little of the variation in the data is generated by the treated group, and confidence intervals based on conventional cluster-robust standard errors (CRSE) can substantially over-reject the null hypothesis (Conley & Taber, 2011). Moreover, given that the state populations represented in our aggregated data vary greatly in size, the residuals in equation (1) are likely to exhibit substantial heteroscedasticity.

Accordingly, we use a bootstrap estimator, developed by Ferman and Pinto (2016), to assess the statistical significance of our estimated PFL impacts. The technique entails first adjusting each state's residuals to eliminate heteroscedasticity, and then randomly resampling from linear combinations of each state's adjusted residuals, where the linear combination chosen represents the within-state differences—i.e., average post-treatment outcomes minus average pre-treatment outcomes—used in the DD estimator. The technique does not produce a test statistic, but the bootstrapped distribution of pseudo-treatment effects provides a basis for determining an empirical *P*-value for the estimate of the treatment effect (see Ferman & Pinto, 2016, for details). For all regression coefficients other than those representing PFL effects, we use robust standard errors clustered at the state level.

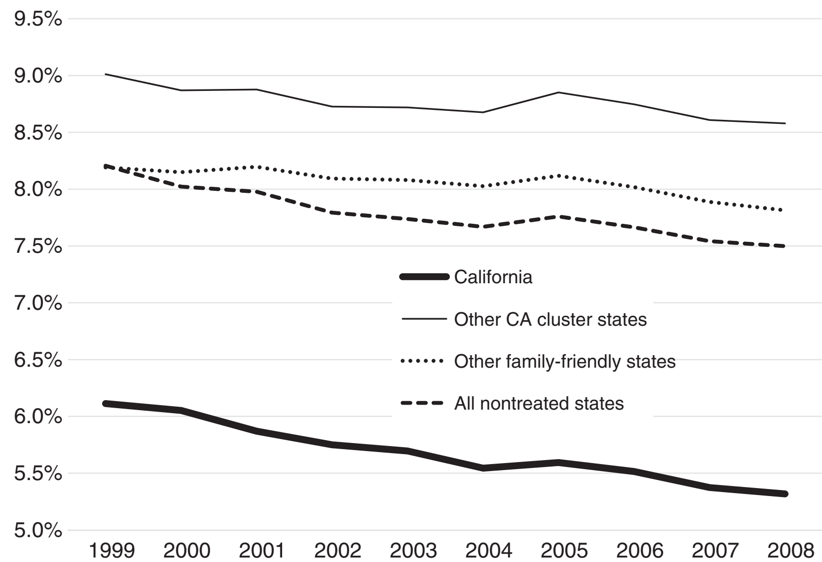
RESULTS

Clustering of Observations

The hierarchical clustering algorithm used with our data for 50 states plus Washington, DC, each of which was characterized by pre-treatment annual rates of change of nursing-home utilization as well as the pre-treatment means of 18 covariates, produced an 8-cluster solution. The states placed into each of the clusters are listed in Table 1. The cluster that contains California also includes 16 other states (DE, ME, MD, MA, MN, NH, NJ, NM, ND, OH, PA, RI, VT, VA, WA, and WI), while the other seven clusters range in size from 1 to 27 states. Although the clustering algorithm we used is not formally based on "nearest neighbor" matching, it happens that for each of the 17 states in the California cluster, their nearest neighbor (based on Euclidean distances in a 19-dimensional space) is also present in the cluster.

Trends in Nursing Home Utilization

In 1999, across the 50 states plus DC, 8.2 percent of people 65 and older spent at least some time as a nursing home resident during the year. Over the 10-year period of our data, this percentage fell slightly, reaching 7.3 percent in 2008. This represents an average annual decline of 0.1 percentage points in nursing home usage among the 65-and-older population. These annual figures are higher than the point-in-time nursing home residency figures often reported; for example, Census data show that 4.5 percent of those 65 and older were in nursing homes in 2000 (Hetzel & Smith, 2001), and 3.1 percent were in nursing homes in 2010 (Werner, 2011). As described previously, point-in-time data on a dynamic phenomenon such as nursing home occupancy produces a length-biased sample, with an over-representation of lengthy episodes. The substantial differences between our annual data and the point-in-time measures are indicative of the fact that a large fraction of nursing home residency episodes is relatively short. Nevertheless, the downward trend in our annual measure



Notes: "Other CA cluster" states are DE, MA, MD, ME, MN, ND, NH, NJ, NM, OH, PA, RI, VA, VT, WA, and WI.
"Other family-friendly states" are CT, DC, HI, ME, MN, ND, NJ, NV, NY, OR, RI, TN, VT, WA, and WI.

Figure 1. Trends in Nursing Home Utilization, California, and Various Comparison-Group States.

mirrors the downward trend that has been reported using earlier point-in-time measures (Bishop, 1999).

One way to judge the reasonableness of the common trends assumption is graphically. Figure 1 illustrates trends in the prevalence of nursing home utilization throughout the 1999 to 2008 period for California (solid heavy line) and the weighted average for the other 16 states in the California cluster (solid light line) as well as both alternative comparison groups. All four trend lines are similar in appearance. A visual inspection of the pre-treatment trends in the outcome variable suggests that California's trend closely parallels that of all comparison groups, but lies well below it. This difference in levels may be attributed to several underlying factors: California's per-capita supply of nursing home beds is among the lowest in the United States (Horowitz, Dickey, & Montalvo, 2003), and it has for many years been a leader in the development of alternatives to nursing home care. For example, in a study of trends in the availability of assisted living units among 13 states (Grabowski, Stevenson, & Cornell, 2012), California's capacity was slightly below Oregon's while well above that in the 11 remaining states.[10] The Program for All-Inclusive Care for the Elderly, an innovator in promoting community-based alternatives to nursing homes, originated in California (Hirth, Baskins, & Dever-Bumba, 2009). Also, California had by 2001 established the oldest and largest program of "consumer directed" community-based LTC care (Benjamin, 2001). Finally, California's

[10] Including the availability of Assisted Living units as a control variable would be relevant in our empirical specification. However, reliable across-states time-series data on Assisted Living units is not readily available.

Table 2. Summary statistics for three analytic samples; [a] *P*-values for exogeneity tests.

	CA cluster		"Family friendly"		All states		Exog. test
Variable	Mean[b]	SD	Mean	SD	Mean	SD	*P*-values[c]
Proportion in NH	0.079	0.018	0.073	0.018	0.076	0.017	N/A
Paid leave	0.127	0.334	0.144	0.352	0.048	0.214	N/A
Medically needy	0.259	0.440	0.163	0.370	0.151	0.358	[d]
CON/Moratorium	0.261	0.440	0.278	0.449	0.383	0.487	[d]
NH reimbursement rate	169.650	24.492	174.771	34.869	159.752	34.778	0.011
NH private pay rate	254.875	31.025	278.500	53.138	231.594	55.103	0.046
PCS option	0.664	0.474	0.868	0.339	0.614	0.487	[d]
Elderly 1915(c) waiver	1.018	0.133	1.015	0.134	1.116	0.379	[d]
In-laws in FMLA	0.186	0.391	0.282	0.452	0.094	0.292	[d]
PCA wages	8.142	0.666	8.224	0.561	7.693	0.836	0.087
SNF days per capita	1.783	0.398	1.709	0.416	1.717	0.418	0.965
HHA visits per capita	2.185	0.783	2.272	0.872	2.815	1.703	<0.001
General fund days	19.574	20.637	18.177	21.536	26.238	26.851	0.247
Per-cap. income/10,000	4.402	0.489	4.528	0.547	4.132	0.573	0.373
Child poverty rate	15.047	3.446	16.578	3.737	17.314	4.063	0.005
Percent female in 65+	58.218	1.126	58.038	1.394	58.077	1.395	0.134
Proportion oldest-old	0.131	0.012	0.132	0.014	0.127	0.014	0.814
Percent Black	6.533	4.555	6.456	5.832	8.285	6.062	0.072
Percent Hispanic	5.900	6.852	7.527	5.696	5.721	6.156	0.083
Percent other race/ethnicity	4.888	4.767	6.838	9.140	3.359	5.924	0.267

[a]Summary statistics for pooled (1999 to 2003 and 2005 to 2008) state–year samples.
[b]All summary statistics are weighted by size of age 65-plus population.
[c]Exogeneity tests based on all-states sample; *P*-values based on CRSEs.
[d]Time-invariant in California during the 1999 to 2008 period, and therefore not tested.

population has the largest share of non-native-born individuals of any state,[11] which likely contributes to a reduction in nursing home utilization there.

Figure 1 provides no apparent suggestion of a downward shift in California's trend line beginning in 2005, the first full year of PFL implementation; instead, it appears that in comparison-group states the post-2004 trends lie above the pre-2004 trends. Provided that the California cluster is, indeed, a valid comparison group, the graph suggests that in the absence of PFL, nursing home utilization in California would have been higher beginning in 2005. Moreover, any effects of the PFL law could be small in magnitude, or masked by the offsetting effects of other observed or unobserved time-varying factors.

Sample Characteristics

Summary statistics for all time-varying variables, pooled over all nine years included in our sample, are provided in Table 2. Means are shown for the California cluster, the set of 16 "family friendly" states (which also includes California), and the entire country. The full-sample means (50 states plus DC) are most representative of the country as a whole, while the two smaller samples reflect various selection criteria.

[11] *Source*: Authors' calculations based on U.S. Census Bureau's "State of residence in 2000 by state of birth: 2000" table (https://www.census.gov/population/www/cen2000/briefs/phc-t38/index.html, accessed 8/25/2016).

Many of the sample means, especially for demographic variables, are quite similar across samples. Others differ in predictable ways: The mean for "in-laws included in FMLA" is much higher in the family-friendly subsample than in either of the other two samples; both the California cluster and the family-friendly states have nursing home costs (i.e., the private-pay rate) that are higher than in the country as a whole, but they also have reimbursement rates that are somewhat higher than the country as a whole.

Exogeneity Tests

The time-varying covariates discussed previously, whose pre-treatment values were used to identify a set of homogeneous clusters containing one or more states, are all potentially relevant to nursing home utilization. However, if any of these covariates are themselves influenced by the implementation of PFL in the post-intervention period, they cannot be considered exogenous to the treatment, and must be excluded from the impact equation. Five of these covariates[12] were fixed within California during our study period, and are therefore conditionally independent of the treatment by definition. We conducted tests of the exogeneity of the remaining 13 covariates, treating each in turn as the dependent variable in a panel fixed-effects regression, including state and year fixed effects, state-specific trends, a "post" (2004) dummy variable, and a "treatment" dummy variable (i.e., California $\times$ post). Four of these covariates—the nursing home reimbursement rate, the private-pay rate for nursing homes, utilization of Home Health Agency visits per capita under Medicare, and the poverty rate for children—failed the test of exogeneity based on the *P*-value associated with the "treatment" variable (see the final column of Table 2). Although these inference tests, based on CRSE, are expected to over-reject the null hypothesis when there is a single treated unit, we adopted a conservative approach and concluded that none of these four covariates could be confidently viewed as exogenous. Furthermore, while we might expect that one of 13 tests might reject the null purely by chance, we have no basis for deciding which of the four offending variables could reasonably be retained, so—again taking a conservative approach—we decided to exclude all four from the PFL impact analysis.

Main Results

We present the results of several DD regressions in two parts. Given its centrality, we first focus on the estimated PFL effect using our preferred sample, the California cluster, along with a variant form of the model that tests the common trends assumption (Table 3). We then present findings using two alternative comparison groups (Table 4). All of these models also include the set of time-varying covariates that were retained after conducting the exogeneity tests previously described. Finally, we discuss the covariate effects for all three comparison groups (Table 5).

The first column in Table 3 (Model 1a) presents the estimated treatment effect when we use the other states in the California cluster as controls. Model 1a provides our preferred estimate of PFL effects, as it uses as controls only those states determined to be most similar to California based on observable characteristics, including the pre-treatment time trend. Conditioning on time-varying features of states' LTC environment, demographic and economic variables, a common time

[12] Specifically, the Medically Needy provision, presence of a CON/Moratorium provision, adoption of the PCSs Option, having an Older Adults 1915(c) waiver program, and the FMLA "in-laws" provision.

Table 3. Main results—DD model with homogenous cluster as comparison group.

Dependent variable: Proportion of elderly in nursing homes		
	Model 1a	Model 1b
PFL effect	−0.0065***	−0.0100***
|t| using CRSE	7.44	4.27
CRSE *P*-value	<0.0001	<0.0001
F–P *P*-value	<0.001	<0.001
Linear time trend	−0.004	−0.0037
CRSE *P*-value	<0.0001	<0.0001
Treatment leads		
CA * 2000		0.0010
|t| using CRSE		0.55
CA * 2001		−0.0015
|t| using CRSE		0.86
CA * 2002		−0.0019
|t| using CRSE		1.07
CA * 2003		−0.0032
|t| using CRSE		1.73
Sample size	153	153

Notes: CRSE refers to CRSE; F–P refers to inference based on Ferman–Pinto (2016). All models include additional controls for LTC state policy, economic, and demographic variables as well as state and year fixed effects. *P<0.05; **P<0.01; ***P<0.001.

Table 4. Robustness checks—DD models with alternative comparison groups.

Dependent variable: Proportion of elderly in nursing homes				
	Model 2a Control group: “Family friendly” states (15 states)	Model 2b Control group: “Family friendly” states (15 states)	Model 3a Control group: All other states + DC (50 states)	Model 3b Control group: All other states + DC (50 states)
PFL effect	−0.0050***	−0.0139***	−0.0072***	−0.0140***
|t| using CRSE	4.50	6.02	4.85	5.50
CRSE *P*-value	<0.0001	<0.0001	<0.0001	<0.0001
F–P *P*-value	<0.001	<0.001	0.04	<0.001
Cluster-specific trend	Yes	Yes	Yes	Yes
Treatment leads				
CA * 2000		−0.0014		−0.0019
|t| using CRSE		1.08		1.19
CA * 2001		−0.0049***		−0.0051***
|t| using CRSE		3.54		3.15
CA * 2002		−0.0052***		−0.0062***
|t| using CRSE		3.45		3.62
CA * 2003		−0.0072***		−0.0076***
|t| using CRSE		4.40		4.01
Sample size	144	144	459	459

Notes: CRSE refers to CRSE; F–P refers to inference based on Ferman–Pinto (2016). All models include additional controls for LTC state policy, economic, and demographic variables as well as state and year fixed effects. *P<0.05; **P<0.01; ***P<0.001.

Table 5. Full specifications for Models 1a, 2a, and 3a.

Dependent variable: Proportion of elderly in nursing homes			
	Model 1a	Model 2a	Model 3a
PFL effect	−0.0065***	−0.005***	−0.0072***
	(7.44)	(4.50)	(4.85)
Medically needy	0.0060***	0.004***	0.0025*
	(5.51)	(3.13)	(2.43)
CON/Moratoria	−0.0015	−0.0013	0.0008
	(1.86)	(1.09)	(1.07)
PCSs option	0.0001	0.0036*	0.0021
	(0.08)	(2.41)	(1.63)
Older adults 1915(c) waiver	0.0026	−0.0007	0.0008
	(1.53)	(0.55)	(0.78)
"In-laws" allowed by state family leave policy	0.0011	0.0037**	−0.0028**
	(1.76)	(3.16)	(2.95)
PCA wage	−0.0005	0.0009	−0.0008
	(0.90)	(1.48)	(1.37)
SNF days per capita	0.017***	0.0149***	0.0045**
	(11.16)	(7.04)	(2.97)
General fund days (in 1000s)	0.0198*	0.0147	0.0024
	(2.13)	(1.93)	(0.34)
Real per capita income	0.0091***	0.0026	−0.0036*
	(5.02)	(1.32)	(2.02)
Proportion of female among older adults	−0.0012	0.0020	0.0018
	(−1.11)	(1.54)	(1.36)
Proportion of "oldest old" among older adults	0.3709***	0.1523	0.3620***
	(8.56)	(1.49)	(5.89)
Percent black	−0.0026***	0.0059***	−0.0021***
	(3.99)	(4.47)	(2.97)
Percent Hispanic	0.0013	0.0045**	0.0001
	(1.36)	(2.70)	(0.17)
Percent other	0.0017	−0.0032	0.0023*
	(1.74)	(1.62)	(2.42)
Sample size	153	144	459

Notes: |t| using CRSE in parentheses. Estimation also includes time trends for each cluster, state, and year fixed effects. *$P<0.05$; **$P<0.01$; ***$P<0.001$.

trend, and state and year fixed effects, we find that the implementation of PFL led to a statistically significant decrease in elderly nursing home utilization.

Specifically, PFL reduced the annual proportion of elders in nursing homes in California by 0.0065, about two-thirds of a percentage point. While this reduction may appear small in size, its relative magnitude is substantial when compared to baseline nursing home utilization levels. In 2003, the year prior to PFL implementation, 5.7 percent of California's older adults resided in nursing homes. Thus, our estimated PFL effect implies a relative decline of over 11 percent in the proportion of elderly in nursing homes in California. As expected, the *P*-value obtained by the Ferman–Pinto approach is well above that produced by CRSEs. However, even after adopting this more conservative approach to inference, our estimate is statistically significant at conventional levels.

Model 1b presents a formal test of the common trends assumption by allowing for "leads" of the PFL implementation in the estimation. We do this using interactions of pre-treatment year dummies (for 2000 to 2003) with the treatment variable (i.e., state indicator for California). None of the four treatment leads is significant, judging by the CRSE-based t-statistics. In this model the PFL effect increases to -0.0100 (i.e., it approximately doubles), but that represents a difference from the comparison-group trend rather than California's unique trend. A better estimate of the true PFL effect is given by the difference between the PFL coefficient and the CA*2003 estimate ($-0.0100 - [-0.0032] = -0.0068$), which is very close to the basic DD result (and which remains significant with $P<0.001$). This, in turn, provides evidence that the DD approach is valid, supporting our conclusion that California's PFL law caused a reduction in nursing home utilization among the elderly population.

Robustness Checks

We check for the robustness of our results by using two alternative control groups as well by conducting a direct placebo test. Table 4 presents the estimated treatment effects produced when these two alternative control groups are used. Model 2a includes California and 15 other states with generous family-friendly policies, and Model 3a presents the estimated treatment effect when all states are used as controls for California. Both models include the time-varying features of states' LTC environment, demographic and economic variables previously described, linear time trend as well as state and year fixed effects. Models 2a and 3a also include controls for cluster-specific time trends. By using all available data, the specification described in Model 3a provides for the largest sample size: 51 (states) $\times$ 9 (years) = 459 observations.

Similar to Model 1, in both alternative models (Table 4, columns 1 and 3), we find a decrease in elderly nursing home utilization as a result of PFL implementation. These decreases are statistically significant, based on CRSE as well as the more appropriate (and more conservative) Ferman–Pinto P-values. Specifically, after controlling for time-varying observed and time-invariant unobserved characteristics, PFL reduced the annual proportion of elders in nursing homes in California by about 0.5 to 0.72 percentage points in Models 2a and 3a, respectively. These robustness tests add confidence to our main finding (Model 1), the magnitude of which lies within this relatively tight range.

Columns two and four in Table 4 present results for the alternative specifications when pre-treatment leads (interaction of state indicator for California with pre-treatment years) are included in the equations. Unlike the results from our primary control group, both Models 2b and 3b in Table 4 provide evidence of statistically significant (based on CRSE) pre-treatment "effects" in California for three of the four pre-treatment years. It is possible for some of this response to be anticipatory in nature, as the PFL bill was passed in California in 2002 but not implemented until 2004. One source of anticipatory response could be that the momentum surrounding the passage of PFL in 2002 encouraged some private employers in California to offer paid leave as part of their benefits package in advance of the actual implementation of the law, thus raising the overall availability of paid leave in the state. Another possible anticipatory response may occur if family members, knowing that they will be able to use paid leave for informal care responsibilities in the future, change behavior and become more likely to use alternative sources of accumulated leave—vacation or sick days—to provide care to seriously ill older adults during the 2002 to 2003 period.

However, the above arguments do not address the issue of finding a statistically significant effect for California in 2001. It is possible that these pre-treatment effects are a result of not controlling for some unidentified change or event in California during these years. In addition, because the control groups in both alternative specifications are not as thoroughly matched—the family-friendly group is matched only on one unobservable dimension, and Model 3 includes all 50 states (and DC) in the control group—there is a heavy reliance on statistical controls to produce comparability. This makes it somewhat unsurprising to find pre-treatment effects. That said, it is encouraging to note that the magnitude of the PFL effect, when treatment leads are included in all three models, remains almost precisely the same. The unique PFL effect in both columns two and four in Table 4 is given by the difference between the PFL coefficient and the CA*2003 estimate for each specification ("Family friendly" Model: −0.0139 − [−0.0072] = −0.0067; All States Model: −0.0140 − [−0.0076] = −0.0064). Both these estimates are strongly comparable to the finding in column 2, Table 3, where the PFL estimate is −0.0068. Similarly, Bartel et al. (2017) also show that using a DD framework using all states that there were policy responses to PFL in terms of father's leave-taking increases in 2002 and 2003. Like us, they adopt an alternative estimation approach to resolve this analytical issue.

In addition to these two alternative specifications, we also conducted a direct placebo test to validate the robustness of our DD results. For this test, we focused on the "All States" specification (Model 3a). Specifically, we excluded California from our dataset and iteratively assigned a false or placebo treatment to the remaining 50 states (and DC) beginning in 2005 (as 2004 is dropped from the analytic dataset). We ran the DD model 50 times and compared placebo PFL coefficients across states. The mean of these 50 placebo PFL effect estimates is −0.0005, very close to its theoretical value of zero (the SD of the 50 estimates is 0.0044, nearly 9 times their mean value). We found only three states (Kansas, Montana, and Washington) with a false "PFL effect" that was more negative than the California effect. The placebo tests thus suggest an approximate *P*-value of 0.06 based on the empirical distribution of treatment effects under the null hypothesis of no effect. This finding demonstrates that the California PFL estimate lies in the lower tail of the null distribution, and therefore the impact is unlikely to have occurred purely by chance.[13] A complete listing of the null-treatment estimates across all 50 states may be found in Appendix C.[14]

Other Covariates

In Table 5, we present the full set of regression results, including all covariates, for Models 1a, 2a, and 3a. Because it uses the largest sample, while also incorporating the full range of variability of the covariates—i.e., the distribution across all 50 states plus DC—our discussion emphasizes the all-states model (equation 3a). Cluster-robust t statistics are provided in parentheses. We do not use the Ferman–Pinto approach to inference for interpreting the statistical significance of covariates as there is no longer a single treated group.

With regard to factors that may influence the demand for nursing home use, we find that the presence of a Medically Needy or Spend Down provision leads to an increase in nursing home use. This is theoretically consistent as a more generous Medicaid eligibility standard is expected to positively influence the use of institutional care services. We also find that more generous state family leave laws—in particular, inclusion of in-laws in the definition of "parent"—lead to a decrease in nursing home use. This is consistent with previous

[13] When we conducted the placebo test for the Model 1a sample, we obtained the same result: One of 16 placebo PFL effects was slightly more negative than the estimated effect for California, suggesting an empirical *P*-value of 0.0625.
[14] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

evidence indicating the substitutability of informal care and nursing home use. It also lends support to the main finding of this paper on the impact of PFL policy in California.

We find no evidence of the impact of state policies theoretically predicted to influence supply of nursing home use, i.e., specifically the presence of CON/Moratoria. Among the economic factors considered, states with higher per capita income are likely to have lower nursing home use. From a demographic perspective, as expected, a higher proportion of oldest old is positively associated with nursing home use. Finally, a higher percentage of blacks is negatively associated with nursing home use. This is consistent with previous literature on the topic indicating that black elders use less institutional care than disabled white elders (Cagney & Agree, 1999; Hing & Bloom, 1990). On the other hand, a higher percentage of older minorities (in comparison to whites) is positively associated with nursing home use. This is consistent with recent research that finds Asians as one of the fastest growing minority groups among nursing home residents as a result of shifts in demographic trends (Feng et al., 2011).

Comparing across the three sets of results shown in Table 5, we see that for 11 of the 14 included covariates the coefficients are either consistent (i.e., agree in sign or in determination of statistical significance) or at least not contradictory. The exceptions include coefficients on the "in-laws" variable, per capita income, and percentage of black. These differences presumably arise from the criteria applied in selecting states for the California cluster (used in 1a) or the family-friendly comparison group (used in 2a). The consistency of PFL effects across samples, despite the few instances of inconsistency for these few covariates, strengthens our conclusion regarding the effect of PFL on nursing home usage.

DISCUSSION

Our regression-based analyses indicate that California's PFL program reduced nursing home occupancy among the 65-and-older population by 0.5 to 0.7 percentage points; our preferred estimate, employing an empirically-matched group of 16 states as controls, finds that PFL reduced nursing home usage by about 0.65 percentage points. When treatment leads are added to that model, the PFL estimate is preserved, and the common trends assumption underlying a strict DD framework is validated. Using an inferential approach that accounts for the fact that there is only one treatment state, while adjusting for heteroscedastic errors, we conclude that these treatment impacts are statistically significant.

With alternative comparison groups, in which comparability to California is admittedly less strong—requiring, in turn, heavier reliance on controls for covariates and divergent trends—we find remarkably consistent estimates of PFL effects, but also evidence of unexplained pre-treatment differences between California and other states. The robustness of our estimates to alternative specifications and the consistency of our inferential evidence (including a similar effect of unpaid but generous state-level FMLA policies on nursing home usage) add to our confidence in this finding. To our knowledge, this is the first study that empirically examines the relationship between paid leave and LTC outcomes.

While the size of the estimated reduction in nursing home usage may appear small, its relative magnitude is sizable, when compared to baseline nursing home utilization levels. The proportion of elderly in nursing homes in California during 2003, the year prior to the PFL implementation, was about 0.057. Evaluated at the 2003 nursing home utilization level, the range of PFL coefficient estimates implies a relative decline about 9 percent to as much as 13 percent in the proportion of elderly in nursing homes in California. In addition, our preferred estimate (−0.0065) is over

50 percent larger than the average annual change in elderly nursing home utilization in the California-cluster states prior to 2004 (−0.004).

By the nature of our outcome measure—the proportion of the population ever-resident in a nursing home during a calendar year—our estimate of the impact of PFL understates the true policy impact. In order for the proportion of the population with any nursing home experience during a year to decline, one of three things must happen: an episode must end earlier (i.e., in the prior year), or begin later (i.e., in the following year), or—for the relatively shorter spells that begin and end in the same calendar year—be entirely averted. Thus, any impacts of PFL that take the form of reducing the length of nursing home spells that remain in-progress at any point during the calendar year will be missed. However, the data necessary to develop a more sensitive measure (e.g., of person-days spent in nursing homes during a year) do not appear to be available.

Our estimated PFL impact is meaningful with respect to service use. The estimated number of people 65 and older in California in 2009 was 4,165,000 (U.S. Bureau of the Census, 2015). Using −0.005 as a measure of program impact (the low end of our range of estimates), our results imply that there would have been about 20,800 more nursing home residents among the 65-plus population that year in the absence of PFL. Based on an average of 96 beds per nursing facility in California (the figure for 2007 according to Houser, Fox-Garage, & Gibson, 2009), that many residents would completely fill 217 of California's 1,283 nursing homes. However, the great majority of the nursing-home episodes averted as a consequence of PFL are likely to be quite short on average. For example, if all of the estimated 20,800 averted spells were one month in length, and distributed uniformly over the year, they would represent the equivalent of about 18 full-to-average-capacity nursing homes. Therefore, the implementation of PFL might ultimately lead to a modest reduction in the state's nursing home bed usage.

The principal limitation of our study is that the implementation of PFL policies is not randomly assigned. In the absence of random assignment, we must be concerned about the possibility that states enact PFL laws in response to levels of, or trends in, the propensity of older people to reside in nursing homes, or other unobservables. We attempt to address this through a combination of including relevant time-varying variables (e.g., other aspects of state policy, population characteristics, and economic environment that might influence nursing home usage), statistical model (e.g., including state- and year-specific effects), and alternative configurations of comparison-group states. Reverse causality seems not to be an issue in this analysis: Nowhere have we encountered anyone arguing for paid leave legislation on the grounds that it would help alleviate LTC service use or costs.

Our analysis is also limited by the time period covered by our nursing home utilization data. The CMS reports we used to measure counts of nursing home residents by age changed their procedures for deriving these counts (from annual to point-in-time measures) starting in 2010. As a result, our data cover a period of time during which only one state falls into the treatment group, creating problems with respect to statistical inference. In future research, it would be desirable to verify our findings using larger sample sizes and over a longer time period post policy implementation.

We are also unable to demonstrate that the reduction in nursing home occupancy found in our analysis is directly linked to the caregiving efforts of employed family members (mainly adult children), although logically this seems to be the only mechanism that would connect PFL laws to nursing home usage. Both Kerr's (2016) and Bartel et al.'s (2017) studies show that enhanced family leave legislation leads to an increase in leave-taking among parents. These findings lend support to our claim that PFL, targeted towards workers with a family member needing care, could reduce formal care-service usage while increasing family care provision. More generally, our situation is analogous to that faced in several program-evaluation efforts. For example, Lichtman-Sadot and Bell (2017) propose that California's PFL law improves health outcomes among elementary school children through breastfeeding, greater parental care during infancy, and reduced prenatal stress; however these mechanisms are unobserved in their data and the authors rely on existing studies to propose these channels. Detailed individual- (and family-) level data on elderly people with care needs, and the employment status and caregiving behavior of their family members, would be needed to investigate these connections more directly.

CONCLUSION

Notwithstanding the limitations, California's paid leave policy does appear to have had the unanticipated consequence of reducing nursing home utilization, with potentially important LTC financing implications. This is especially significant in light of current proposals to impose caps on federal Medicaid payments. Such budgetary restrictions are likely to drastically reduce Medicaid spending, a large share of which goes toward LTC services and supports. It is also important to note that any nursing home cost savings that results from PFL must be weighed against possible cost increases, including administrative expenses accrued by employers, additional nonfinancial caregiving burden on informal caregivers (Miller, Allen, & Mor, 2009), and any offsetting increases in the use of paid home care services. On the other hand, these costs may reduce the opportunity cost of time among informal caregivers, which has been estimated to total $522 billion annually (Chari et al., 2015). As a growing share of Americans express support for paid family and medical leave (Horowitz et al., 2017), and more states implement PFL laws, or debate the adoption and extension of such laws, empirical evidence on a broad range of policy impacts—such as the LTC service use studied here—can inform these policy debates.

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APPENDIX A: DATA SOURCES

1. Medically Needy Program Provisions

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2. CON and Moratoria Restrictions

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3. Medicaid Nursing Home Reimbursement Rates

AARP Public Policy Institute. Across the States: Profiles of Long-Term Care, various years, are the source of “Medicaid payment rate for nursing facility care” for 1998, 2002, 2007, and 2011. Values for other years are interpolated.

4. Average Private-Pay Nursing Home Costs

Average daily private-pay rates for nursing homes by state and year for 2004 to 2010 were provided by Marc Cohen of LifePlans, Inc (personal communication, May 2014). Estimates for earlier years were obtained as predicted values for state-specific linear regressions of the private-pay rates on year, backwards-extrapolated to 1999.

5. PCSs State Plan Option

Kaiser Commission on Medicaid and the Uninsured. (2005 to 2009). Medicaid 1915(c) home and community-based service programs: Data update (Issue Paper).

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6. HCBS Waivers

Miller, N., Ramsland, S., & Harrington, C. (1999). Trends and issues in the Medicaid 1915(c) waiver program. Health Care Financing Review, 20, 139–160.

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7. "Family Friendly" Leave Policies

Lowering Firm-Size Threshold to Cover More Workers

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Broaden Definition of Family to Include Parents, In-Laws, and Step-Parents

Feinberg, Lynn. (2013). Keeping up with the times: Supporting family caregivers with workplace leave policies. AARP. Washington, DC. Retrieved September 2017 from http://www.aarp.org/content/dam/aarp/research/public_policy_institute/ltc/2013/fmla-insight-keeping-up-with-time-AARP-ppi-ltc.pdf.

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Provision of PFL or TDI

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8. Wages for Personal Care Workers

Center for Personal Assistance Services. (2011) U.S.—State chart book on wages for personal care aides, 2000–2010. San Francisco, CA: University of California. Retrieved August 30, 2017 http://nasuad.org/sites/nasuad/files/hcbs/files/159/7948/phichartbook.pdf.

9. Medicare-Covered SNF and Home Health Agency Services

Centers for Medicare & Medicaid Services. Medicare & Medicaid statistical supplement (annual editions). Retrieved August 30, 2017 from https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/MedicareMedicaidStatSupp/index.html. SNF covered days taken from Table 38; Home Health visits per enrollees taken from Table 48.

10. State Fiscal Constraints

Pew Charitable Trusts. Fiscal 50: State Trends and Analyses. Days' worth of General Fund expenditures in reserve. The Pew Charitable Trusts. Retrieved August 30, 2017 from http://www.pewtrusts.org/en/multimedia/data-visualizations/2014/fiscal-50#ind5.

11. Per Capita Incomes

Department of Commerce. (2015). Bureau of the Census. Statistical abstract of the United States (various issues). Retrieved August 30, 2017 from https://www.census.gov/prod/www/statistical_abstract.html.

12. Poverty Rates

U.S. Census Bureau. Small Area Estimates Branch. Tables of "Poverty and median income estimates—States" for years 1999 to 2009. Retrieved February 2013 from http://www.census.gov/did/www/saipe/data/statecounty/data/index.html.

13. Population Gender and Age Structure

1990s: (ST–99–8). Population Estimates for the U.S. Regions, Divisions, and States by 5-year Age. Groups and Sex: Time Series Estimates, July 1, 1990 to July 1, 1999 and April 1, 1990 Census Population Counts. Retrieval August 30, 2017 from https://www.census.gov/programs-surveys/popest/data/data-sets.html.

2000s: ST-EST00INT-AGESEX. Intercensal Estimates of the Resident Population by Single Year of Age and Sex for States and the United States: April 1, 2000 to July 1, 2010. Retrieval August 30, 2017 from https://www.census.gov/programs-surveys/popest/data/data-sets.html.

14. Racial/Ethnic Composition

U.S. National Center for Health Statistics. Bridged-Race intercensal population estimates for calculating vital rates, 1990–1999 file and Revised 2000–2009 files. Available at http://www.cdc.gov/nchs/nvss/bridged_race.htm.

APPENDIX B: "FAMILY FRIENDLY" STATES WITH LAWS EXCEEDING MINIMUM FMLA REQUIREMENTS

Table B1. "Family friendly" states with laws exceeding minimum FMLA requirements.

Lowers firm-size threshold		Definition of "family" includes parents-in-law		Definition of "family" includes step parents		Expands leave duration		Offers/passed PFL benefit	
ME	Before 1999	CA	Before 1999	CA	Before 1999	RI	Since 2012	CA	Since 2004
MN	Before 1999	CT	Before 1999	CT	Before 1999	TN[a]	Since 2005	NJ	Since 2009
OR	Before 1999	HI	Before 1999	HI	Before 1999			WA[a]	Passed 2007
VT	Before 1999	NJ	Before 1999	NJ	Before 1999			RI	Since 2014
DC	Before 1999	RI	Before 1999	NV	Before 1999			NY	Passed 2016
		VT	Before 1999	DC	Before 1999			DC	Passed 2017
		WI	Before 1999	ND	Before 1999				
		WA	Since 2002	WI	Before 1999				
		OR	Since 2005	MN	Before 1999				

[a]Only for child care.

APPENDIX C: DISTRIBUTION OF PLACEBO PFL EFFECTS

Table C1. Distribution of placebo PFL effects.

	Placebo "PFL" effect[a]	
	PFL coeff.	CRSE |t|
Alabama	0.0093	3.66
Alaska	−0.0010	0.28
Arizona	−0.0003	0.24
Arkansas	0.0008	0.50
California	−0.0072	4.85
Colorado	−0.0023	2.97
Connecticut	−0.0071	4.23
Delaware	0.0033	1.92
Florida	0.0063	5.38
Georgia	0.0075	4.99
Hawaii	−0.0031	3.35
Idaho	−0.0046	3.02
Illinois	−0.0050	5.00
Indiana	−0.0050	5.24
Iowa	−0.0014	1.63
Kansas	−0.0091	6.37
Kentucky	0.0024	2.34
Louisiana	0.0003	0.08
Maine	−0.0017	1.33
Maryland	−0.0008	0.38
Massachusetts	−0.0006	0.74
Michigan	0.0067	3.44
Minnesota	−0.0041	3.98
Mississippi	0.0030	1.61
Missouri	−0.0006	0.45
Montana	−0.0102	7.46
Nebraska	−0.0027	2.13
Nevada	−0.0064	2.86
New Hampshire	0.0022	1.75
New Jersey	0.0047	3.68
New Mexico	−0.0034	1.80
New York	0.0049	3.12
North Carolina	0.0046	3.68
North Dakota	−0.0007	0.31
Ohio	0.0011	1.15
Oklahoma	−0.0043	3.43
Oregon	0.0053	2.66
Pennsylvania	−0.0007	0.89
Rhode Island	−0.0023	1.44
South Carolina	−0.0005	0.50
South Dakota	−0.0009	0.73
Tennessee	−0.0054	5.19
Texas	−0.0027	1.81
Utah	0.0004	0.58

Table C1. Continued.

	Placebo "PFL" effect[a]	
	PFL coeff.	CRSE |t|
Vermont	0.0014	1.16
Virginia	0.0046	3.33
Washington	−0.0075	4.99
Washington DC	−0.0049	0.63
West Virginia	0.0014	1.08
Wisconsin	−0.0024	3.73
Wyoming	0.0059	2.11

[a]Actual PFL effect for California; false-PFL effect in all other cases (with CA removed from sample).