Parental Leave Programs, Nurse Shortages, and Patient Health

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Abstract

Nurses define the largest health profession in most OECD countries and play a critical role in the delivery of health care services. At the same time, there is a growing concern about potential nurse shortages in several high-income countries as the population and the nursing work force ages. We investigate the effects of a nurse shortage induced by a parental leave program on patient health in Denmark. We find that the program reduced the number of employed nurses in hospitals and nursing homes by 12% and 11% respectively and led to an average increase in mortality in nursing homes by 12.5% for the population aged 65 and older.

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1 Introduction

Nurses define the largest health profession in most OECD countries (see OECD (2011)). In the U.S., for example, there were 2.8 million employed registered nurses in 2014, about 14.6% of total employment in the health care sector.[1] Nurses play a critical role in the delivery of health care services in hospital, primary, and nursing home care and are important to the success of emerging patient-oriented care models. At the same time, there is a growing concern about potential nurse shortages in many high-income countries as the population and the nurse workforce ages (see Buchan, Duffield, and Jordan (2015)). How potential nurse shortages affect the delivery of care and patient health is of central policy relevance as several countries consider education and immigration programs to address the growing demand for this profession.

In this paper, we take advantage of a natural experiment in Denmark to provide new evidence on the link between nurse shortages and patient health. In 1994, the Danish government introduced a federally funded parental leave program, which offered parents the opportunity to take up to one year of absence per child aged 0-8. Combining administrative employer-employee data with death registry data, we are able to quantify the effects of the parental leave program on leave taking, net employment, and patient mortality in hospitals, nursing homes, and other health providers.

In addition to providing new evidence on a key policy question, we also contribute to the literature on parental leave programs. Over the past 50 years, parental leave programs have become a prevalent and important feature of labor markets in developed countries (see Dahl et al. (2013)). Despite their economic and social relevance, the existing empirical evidence on costs and benefits remains mixed and incomplete. Previous studies have vastly focused on the labor market effects for affected parents and on outcomes of children. Yet employer consequences have largely been ignored.

In this paper, we argue that publicly funded parental leave programs can have significant negative externalities for employers and ultimately consumers if the programs affect imperfectly substitutable employees that are in limited supply. This applies to employees who hold firm- or industry-specific human capital and occupations that require a licensed degree in particular. The specifics of the parental leave program may amplify the employment effects. Employers are required to guarantee the same position for the returning leave taker. Therefore, the employer may leave the position vacant if she cannot find a temporary replacement. In practice, this effect is most pronounced in female-dominated occupations because the vast majority of leave takers are female. Therefore, we evaluate this externality in a particularly important context since more than 97% of Danish nurses are female.

Our empirical analysis starts out by quantifying the effects of the parental leave program

[1] See http://www.bls.gov/iag/tgs/iag62.htm.

on labor force participation. Using Danish administrative employer-employee matched data for the years 1990-2000, we are able to distinguish between eligible and ineligible health care professionals following the introduction of the parental leave program. We estimate separate take-up probabilities for doctors, nurses, and nursing assistants. Doctors in our empirical analysis hold an M.D., nurses hold a B.Sc., and nursing assistants have completed up to 24 months of practical training. Our findings indicate that a large number of eligible nurses take advantage of the parental leave program. The fraction of previously employed nurses with 0-1 year old children on leave, for example, increases by 15 to 20 percentage points following the introduction of the parental leave program. The effects are slightly smaller for nursing assistants who took a leave of absence more frequently even before the reform. In contrast, we only find small employment changes for doctors.

Despite the substantial program take up rates for both nurses and nursing assistants, the net employment effects for health care providers are very different. Based on aggregate employment data, we see no change in net employment for nursing assistants in hospitals or nursing homes. Health care providers simply replace leaving nursing assistants by hiring additional nursing assistants from other occupations and by hiring newly trained nursing assistants. This is not true for nurses. The aggregate data indicate a substantial decline in nurse employment in hospitals and nursing homes. To provide further evidence on the substantial net employment effects for nurses, we exploit variation in the effects of the parental leave program across counties, which mark separate, locally integrated health care systems. We exploit variation in leave-taking behavior based on differences in the demographic composition across counties. Our results corroborate the evidence from the aggregate data: health care providers are unable to substitute nurses on parental leave. In fact, we observe an even stronger decline in net employment than predicted by individual parental leave taking without replacement. This evidence is consistent with longer absences from the labor market following parental leave and with negative spillover effects on co-workers that induce further exit of employees. We estimate that the parental leave program reduced the stock of working nurses by 12% and 11% in hospitals and nursing homes respectively.

Finally, we turn to the effects on health care delivery. Specifically, we use death register data to estimate the effect of nurse shortages on patient mortality in different health sectors. We complement the mortality data with information from the population register and from hospitalization records to account for differences in the distribution of risk factors in the population across counties. Our analysis first focuses on the population aged 65 and older and finds a significant increase in resident mortality rates in nursing homes by 0.2 percentage points, which corresponds to an increase by 12.5% compared to the pre-reform mortality rate. We then examine the subpopulation aged 85 and older in more detail because the share of nursing home residents in this group is expected to be much higher and hence they will be more exposed to the nurse shortage. The parental leave policy led to an average increase in mortality in nursing homes by 1.1 percentage points or 12.8% for individuals aged 85 and older. This increase fully accounts for the significant increase in total mortality for this subpopulation at the county level that we estimate as a result of nurse shortages in nursing homes. In particular, we find that cardiovascular and respiratory deaths, as well as degenerative brain diseases increase significantly among nursing home patients in response to the nurse shortage. On the other hand, we do not find an increase in the one-year mortality for hospital patients. Yet our results show that readmission rates and average annual time spent in the hospital increase with nurse shortages in hospitals. Data on social benefits reveal that hospitals that face a higher share of eligible nurses are less likely to allow extended leave of more than 26 weeks in order to mitigate the shortage. Overall, our results suggest that staffing needs for skilled nurses are a particular concern in nursing homes and of growing importance as the population ages and the demand for long term care services increases.

Our analysis contributes to the following two strands of literature. First, we contribute to the literature on the role of nurses in the health production function. Gruber and Kleiner (2012) study the effect of nurse strikes on patient health using data from the State of New York and find a large increase in in-hospital mortality. One important difference between our and their research design is that we can quantify the medium-term effects of reductions in nurse staffing ratios on patient health, which allows health care providers to respond to the staffing shortfalls. Therefore, we think that our findings are more informative regarding the effects of nurse shortages on patient health. Propper and Van Reenen (2010) investigate the mortality effects of flat wage regulations in the UK. In their context, wages are relatively low in urban markets, which leads to low nurse staffing ratios in hospitals and higher mortality outcomes among heart attack patients. Stevens et al. (2014) analyze the counter-cyclical mortality fluctuations in the US and argue that nursing homes have an easier time hiring personnel in economic downturns, which ultimately leads to lower nursing home mortality rates in mortality rates in recessions. Finally, Lin (2014) studies the effects of nurse staffing ratios on the quality of care in the U.S. nursing home industry using variation in minimum staffing regulations. One advantage of our research design is that we are able to quantify differences in health outcomes across health care sectors (e.g., hospitals and nursing homes) within a given market. This allows us to identify the most pressing staffing needs by comparing the health effects between sectors and markets.

Second, we contribute to the literature on the effects of parental leave programs. Previous studies have investigated the costs and benefits of maternity leave programs, but the empirical evidence remains mixed. On one hand, several studies find positive effects on health, educational, and earning outcomes for children (see e.g. Ruhm (2000), Rossin (2011), Carneiro, Løken, and Salvanes (2011)) and positive or only slightly negative effects on parental labor force participation (see e.g. Ruhm (1998), Waldfogel, Higuchi, and Abe (1999), Baker and Milligan (2008), and Schönberg and Ludsteck (2014)). On the other hand, other studies do not find evidence for positive health or educational attainments for children (see e.g., Rasmussen (2010), Liu and Skans (2010), Dustmann and Schönberg (2012), and Dahl et al. (2013)) and suggest negative net employment effects (see Dahl et al. (2013)). Our findings provide new evidence on unintended negative consequences of parental leave programs if employers struggle with finding adequate substitutes. We find large negative net employment effects for nurses that even exceed the predicted reform effects that would occur if employees were to take an entire year of absence and employers were not able to replace any leaver on net. The large negative finding is in contrast to the existing literature, which typically finds that endogenous labor supply responses of eligible parents mitigate the negative net employment effects.

The remainder of the paper is organized as follows. Section 2 provides institutional background of the Danish health care sector and the policy reform that we study. In Section 3 we describe our econometric approach. In Section 4, we discuss the data and we present our empirical findings in Section 5. Finally, we conclude in Section 6.

2 Institutional Background

This section discusses important regulatory features of the Danish health care sector as well as the parental leave program. The goal of this section is to provide relevant background information to allow for a discussion of external validity and to motivate the empirical analysis at the regional (county) level. For additional details on the Danish health care sector see K. M. Pedersen, Christiansen, and Bech (2005).

2.1 The Danish Health Care Sector

Similar to other Scandinavian countries, Denmark's heath care system applies the "Beveridge" model. Health care expenses are primarily tax financed and most health care providers are publicly owned. In contrast to most other European countries, however, the Danish health care system is decentralized. For the period we study, the country was divided into 13 counties and three municipalities with county status, which are politically responsible for the financing and the delivery of key health care services including the health care delivery in hospitals and primary care practices.[2] Long term care services, including nursing home services, are organized at an even more granular level: the municipality level.[3] Overall, this suggests that health care systems are vastly integrated at the county level. Moreover, counties constitute separate labor markets for health care workers. 20% of nurses and 16% of nursing assistants switch jobs every year, but only 2% of assistants and 4% of nurses start a job in a different county. 88% of nurses and nursing assistants live and work in the same county and this share

[2]See Figure 13 in the Appendix.

[3]The number of municipalities was reduced from 271 in our sample period to 100 in 2007. This reform reduced the number of regions from 16 to only 5.

Table 1: Elderly Demographics and Nursing Homes in Denmark and the US

	Denmark	U.S.
NH beds/ per 1k elderly[(a)]	48	53
Fraction elderly in nursing home[(a)]	4%	5%
Fraction of population older than 65[(a)]	15.4%	12.7%
Fraction of population older than 80[(a)]	3.9%	2.9%
Average age in nursing home	82.2[(b)]	82.2[(c)]
Avg Nursing home size in beds[(d)]	36.3	82
Nurse to resident ratio	0.32	0.25

Sources: (a) Ribbe et al. (1997) for the year 1993, (b) Nursing home survey for Denmark 1994, Statistics Denmark, (c) Nursing home survey for Pennsylvania, US 1996, (d) Ribbe et al. (1997) for the year 1993 and own calculations.

increases to 96% when excluding the counties related to the capital city.[4] Our empirical analysis takes advantage of these institutional and geographic features and explores variation in the effects of the parental leave program across counties.

Our main findings, discussed below, indicate a substantial increase in nursing home mortalities because of the negative nurse employment effects of the parental leave program. To put these findings into perspective, we compare elderly demographics and nursing home characteristics between Denmark and the U.S. in Table 1. Both the share of elderly people in the population and the average age and share of elderly people living in nursing homes are very similar in Denmark and in the US in 1993. This similarity in nursing home relevance is observed towards the end of a transition away from institutional care and towards community based care in Denmark between 1983 and 1995. Ribbe et al. (1997) report that in 1993, there are 1,075 nursing homes in Denmark offering 39,000 beds. Their study implies that the average nursing home size in the U.S. is about twice as large as in Denmark.[5] We also find a generally higher nurse to resident staffing ratio in Denmark of 0.32 in 1993, which exceeds a comparable ratio in the U.S. by about 28%.[6]

[4]These figures are for the period 1990-1993 before the reform. We introduce the data in more detail in section 4 below.

[5]For the US, Ribbe et al. (1997) report 21,000 nursing homes and 256.6 million*12.7% elderly people. Based on the first row in Table 1, this implies 53/1000*(256.6 million *12.7%)=1.7 million beds. Hence, about 82 beds per nursing home.

[6]Based on the estimated elderly fraction in nursing homes from Table 1, we conclude that the Danish nurse staffing ratio in nursing homes equals 11,000 nurses divided by 4% of 850,000 elderly people: 11,000/(0.04*850,000)=0.32. We use data from Long Term Focus from the U.S. to construct the average number of skilled nurse hours per resident day. Considering registered and licensed practical nurses, we find a staffing ratio of 1.44 hours per resident day. Assuming that nurses work 2,080 hours per year (52 40h weeks), we find a nurse to resident ratio of 1.44*365/2,080=0.25.

2.2 Parental Leave Programs in Denmark

Denmark has a long tradition of maternity leave support going back more than 100 years, see Rasmussen (2010). More recently and following the increase in female labor force participation during the 1970s, policymakers raised the generosity of parental leave programs to improve the compatibility of work and family life. In 1980, Denmark introduced a maternity leave program which guaranteed mothers a 14-week leave after birth with generous income dependent support.[7] In addition, mothers were offered a 4-week maternity leave prior to birth and could not be fired while pregnant or on maternity leave. Fathers were not eligible for subsidized parental leave. In 1985, Denmark extended the post-birth leave time to 24 weeks but offered the additional 10 weeks as a parental leave time, to be taken by the mother or the father.[8] In addition, fathers were offered a compensated 2 week leave immediately after childbirth.

Motivated at least in parts by high unemployment rates, the newly elected social democratic Danish government introduced several policies in 1993 that became effective in 1994 and were aimed at rotating the workforce, see Westergaard-Nielsen (2002). Most importantly, the government introduced an educational, a sabbatical, and a parental leave program.[9] The hope was that these programs would give unemployed people the opportunity to fill the opening positions and to gain valuable work experience. The analysis in Westergaard-Nielsen (2002) suggests that the parental leave program had the largest overall impact on labor market participation, while the sabbatical and educational leave had much lower take-up rates.[10] Our approach uses eligibility rules of the parental leave program to analyze the effects of this program.

The parental leave program offers a parent the opportunity of taking up to one year of absence if the child is aged 8 or younger.[11] The program guarantees job security[12] and offers a compensation of 80 percent of unemployment benefits, see Jensen (2000). Unemployment benefits equal 90% of previous wages up to a maximum of \$463[13] per week, see

[7] The minimum of 90% of income and DKK 2,008 (about \$335 in 1983-1985) per week. Most mothers received 90% of their salary, so the income effects of leave taking were relatively minor, see Rasmussen (2010).

[8] In 1984, maternity leave was first raised to 20 weeks. The additional 6 weeks were designed as parental leave.

[9] In addition, transition pay for unemployed workers between 50 and 60 was offered over 1992-1995. The offer included 82% of the highest UI benefits if the worker leaves the labor force into early retirement and further reduced the available worker pool.

[10] Using data on social benefit receipts 1995-1999, we find that these leave programs jointly account for 23 percent of total paid leave time among nurses, while parental leave accounts for 77 percent, see Appendix Table 11. Moreover, a large share of education leave among nurses is due to participation in short term continuing training, see Figure 14, and both education and sabbatical leave always require employer approval.

[11] The program guaranteed 26 weeks, see Jensen (2000), to employed employees but employees could take up to one year conditional upon employer support, see K. M. Pedersen, Christiansen, and Bech (2005).

[12] At least for publicly employed individuals, the vast majority of individuals in our sample, see Pylkkänen and Smith (2003).

[13] DKK 2,940 at an annual average exchange rate of 6.35 DKK per USD in 1994.

Westergaard-Nielsen (2002). Soon after the reform, policy makers noticed that the reform led to a “bottleneck” problem in the public sector in particular, where licensed professionals could not be replaced that easily (e.g., teachers and nurses). As a result, policymakers gradually cut back on the generosity of the program. Guaranteed coverage length was reduced in 1995 for children older than 1 year of age. Benefits were reduced to 70 percent of unemployment benefits in 1995 and subsequently to 60 percent in 1997 before the program was abolished in 2002 in the context of a comprehensive reform of the parental leave policies, see K. M. Pedersen, Christiansen, and Bech (2005) and Andersen and J. J. Pedersen (2007).

3 Empirical Strategy

In this section, we develop a simple empirical strategy that allows us to quantify the effects of the parental leave program on labor force participation and patient health outcomes. We conduct the main empirical analysis in two steps. First, we investigate parental leave take-up at the health worker-year level. Building on this evidence we next explore the implications of the reform on net employment and health outcomes by county and health sector over time. As mentioned in Section 2.1, counties represent integrated health care systems as well as separated labor markets. Therefore, we analyze differences in net employment changes and changes in mortality rates following differences in aggregate parental leave take-up across counties.

3.1 Program Take-up at the Worker Level

To quantify the take-up of the parental leave program, we employ a simple difference-in-differences estimator over the sample of individuals that were employed in the previous year. Our methodology compares changes in employment from before to after the reform between eligible and non-eligible parents. Our take-up regression is of the following form:

$$Y_{ita} = \alpha + \sum_{a=0}^{8} \alpha_a \cdot 1(ageCH_a) + \alpha_{post} \cdot Post_t + \sum_{a=0}^{8} \beta_a \cdot 1(ageCH_a) \cdot Post_t + \epsilon_{it} \ . \tag{1}$$

Here the dependent variable, Y_{ita}, is an indicator variable that takes the value 1 if person i, whose youngest child is a years old, is not employed in year t. $1(ageCH_a)$ refers to a series of indicator variables that take the value 1 if the youngest child is of age a. Post is an indicator variable that takes the value 1 for post-reform years. Our key parameters of interest are β_0-β_8, which indicate the take-up effects for eligible parents.

We estimate equation 1 for different sample populations. First, we separately investigate the immediate take-up effects for doctors, nurses, and nursing assistants by focusing on the sample years 1993 and 1994. In a separate set of regressions, we investigate the effects of the program in 1995 compared to 1993. These effects may be smaller, as the program became less generous over time and parents can only once take advantage for an eligible child.

3.2 County Level Analysis

To investigate the effects of the parental leave program on net employment and patient health, we aggregate the analysis to the county-health-sector-year level. Using our take-up parameter estimates, we are able to predict the total number of health care employees on parental leave at the county-health-sector-year level.[14] We cross-multiply the respective take-up probabilities by the number of eligible workers in any given health-sector, county, and year. In our baseline analysis, we use the estimated take-up probabilities in 1995, which are smaller than the immediate take-up probabilities and relatively stable over the following years. We refer to these as steady state take-up probabilities. We cross-multiply these steady state probabilities with the number of eligible employees in the last pre-reform year 1993, to generate a time-invariant measure for the aggregate effect of the parental leave program at the county-health sector level. We choose a time-invariant summary measure because it is not affected by endogenous job-transitions in the post-reform years. Finally, we divide the predicted number of employees on parental leave by the total number (eligible and ineligible) of employed nurses in 1993 at the county-health sector level, and refer to this measure as "Exposure",

$$\text{Exposure} = \frac{\text{Eligible nurses*estimated take-up-probability}}{\text{All nurses}}.$$

Based on these exposure measures, we estimate the effect of exposure on outcomes by year similar to Finkelstein (2007) and Clemens and Gottlieb (2014) according to

$$Y_{kt}^s = \mu_k^s + \mu_t^s + log\left(pop_{kt}\right) + \sum_{t=1990}^{2000} \lambda_t^s \cdot Exposure_k^s + u_{kt}^s \ . \tag{2}$$

Here, Y_{kt}^s denotes log employment or mortality rates in county k, year t, and health sector s. We distinguish between three sectors: nursing homes, hospitals, and all other industries combined. We are primarily interested in the coefficients $\{\lambda_t^s\}$ that show the pattern of the outcome variable over time across counties with different exposure to the reform. Hence, any structural break of the λ's around the reform year 1993 will be attributed to the reform effect. We include log population as well as county fixed effects μ_k^s and year fixed effects μ_t^s as controls. This specification allows graphical inspection of potential pre-trends across counties and will guide the subsequent difference-in-difference estimation of the average reform effect

[14] To be precise, we estimate the probability of previous employees to be on leave by the end of November, irrespective of the actual length of the leave period.

on employment and mortality rates.

4 Data

In this section, we discuss the key data sets and variables that we employ in the empirical analysis. Our main data sources are administrative matched employer-employee data from the Danish integrated database for labor market research (IDA) and the Danish Register of Causes of Death. We use individual level matched data to estimate take-up rates of the parental leave program. We then use the labor market data to measure annual employment by county and health sector over the period 1990-2000. We complement this information with annual mortality rates by county-sector for the elderly population from the death register. In order to illustrate the underlying mechanisms, we provide additional evidence on leave taking using social benefits data and we further analyze health outcomes for hospital patients using detailed information from the Danish Patient Register.

4.1 Labor-Market Data

IDA covers the universe of firms and workers in Denmark over 1980-2011. The data contain information about primary employment in November each year, including plant and firm identifiers, location and industry of the establishment, balance sheet information of firms and detailed worker characteristics such as gender, age, education, experience, tenure, hourly wages and annual earnings as well as household characteristics such as municipality of residence, marital status, number of children and the age of the youngest child in the household. The latter will be particularly useful to measure eligibility of workers for the parental leave program since only parents with children aged 8 years or younger can apply for these benefits.[15]

The education variable reports the highest degree that a person has achieved from schooling, vocational training or university education. In particular, the variable contains detailed categories of health workers that allow us to distinguish medical doctors, nurses and nursing assistants. We define doctors as all individuals with an M.D. or Ph.D. in medicine. Nurses are defined as all individuals with a Bachelor degree or equivalent training of theory and clinical practice in nursing or midwifery, as well as nurses that completed additional specialization training as home nurses, health visitors, head nurses, nurse teachers or participated in post-graduate training (Nursing diploma, Master in Nursing Science).[16] Finally, we define

[15] From 1995, social benefits records report the beginning and end date of parental leave and other welfare receipts. We use these data to complement our annual employment indicator in November and to analyze timing and duration of leave.

[16] Nursing is a licensed profession in Denmark and only workers authorized by the National Board of Health can practice as nurses. The Act on Certified Nurses 1933 establishes "Sygeplejerske" (certified nurse) as a reserved title.

unskilled nurses or nursing assistants as social and health care aides and health care assistants with 14 months of theoretical and practical training.[17]

Next, we use industry information of plants to identify hospitals and nursing homes. Our definition of nursing homes includes residential institutions for the elderly and for adults with disabilities. We summarize all other employment as the outside sector.[18] Moreover, workers without establishment affiliation in a given year are reported as unemployed or non-participating. Because of the wide age range in the data, this group of individuals includes young workers in training and retired individuals as well.

Figure 1 reports aggregate trends for employment of nurses and nursing assistants in different health care sectors and the private sector over time. Nurses primarily work in hospitals and their employment share in hospitals increases over time, whereas a large and increasing share of nursing assistants work in nursing homes. The aggregate trend for nurses shows a striking drop in employment in all sectors in 1994 that is consistent with a labor supply shock from the leave program. In contrast, the change in employment is less pronounced for nursing assistants, where the structural break is most visible outside of hospitals and nursing homes. One interpretation of these trends is that nursing assistants in the health sector are easily substitutable from the private sector and therefore we do not observe a structural change in aggregate health care employment. Health care providers simply replace leaving nursing assistants by hiring additional nursing assistants from other occupations and by hiring newly trained nursing assistants. As a result, our analysis will mainly focus on the labor supply shock for skilled nurses.

4.2 Mortality Data

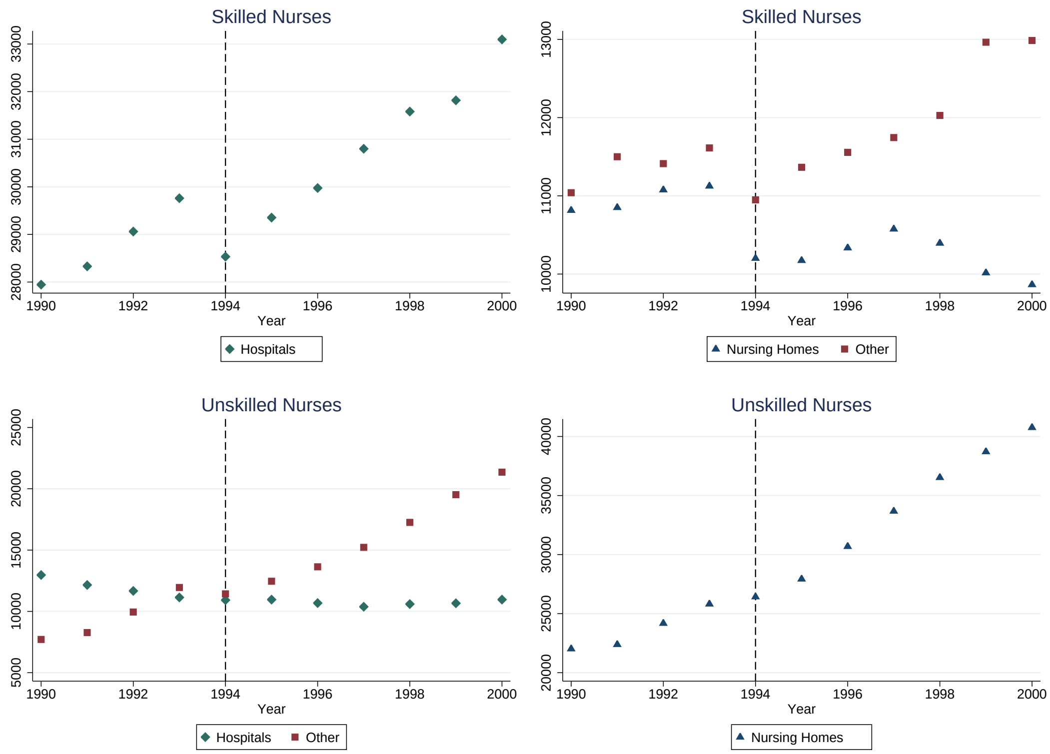
We focus on mortality rates in the elderly population. To this end, we merge demographic information for the entire population of Denmark from the person file of IDA (IDAP) with mortality data from the Danish Register of Causes of Death at the person level for the years 1991-2000. The death register provides information on the death date as well as the location of death, which allows us to distinguish between mortality originating from a hospital, a nursing home, and their home.[19] For our baseline analysis, we construct annual mortality rates at the county-death-location-year level for two population groups: the elderly aged 65 and older

[17]The relevant professions include “Plejer”, “Social- og sundhedshjaelper”, “Sygehjaelper”, “Plejehjemsassistent”, “Social- og sundhedsassistent”.

[18]There are three structural changes in industry classifications in Denmark over the time period that we study, these changes occur in 1993, 2003 and 2007. We define health sectors sufficiently broadly to be able to provide a consistent definition of institutions over time. This prevents us from separately measuring other health care providers such as physicians, home nursing, and midwifery. We rely on imputing industry information for a share of plants before 1993 but the time series of employment in different sectors do not suggest that this is a major concern.

[19]Mortality rates in nursing homes are recorded from 1991. This is why we restrict the sample period for mortality outcomes to 1991-2000.

Figure 1: Employment of Nurses and Nursing Assistants in Denmark



and the elderly aged 85 and older. To construct county-specific mortality rates we use the most recent address information from the population register.

We address potential differences in mortality risks between counties and over time in additional robustness checks. To this end, we combine age and gender information from IDAP with information on inpatient acute care hospitalizations from the Danish National Patient Register. Specifically, we calculate the number and the total length of this and the previous year's hospital visits for each elderly person. To leverage the rich demographic and health utilization information in our analysis we proceed in three steps. First, we regress a mortality indicator variable on age-gender fixed effects, county-year fixed effects, as well as current and last year's length and number of hospital visits at the person-year level. Second, we keep the predicted county-year fixed effects which capture differences in mortalities between counties and over time conditional on differences in the mortality risks as measured by the age-gender composition and the frequency of hospitalizations. Finally, we use these residualized mortality rates in our following county-year level regression analysis.

5 Results

In this section, we provide graphical and regression-based evidence on parental leave take-up and the subsequent effects on employment and health care delivery. We start with an analysis of program take-up at the worker level. Next, we investigate the aggregate effects on net employment at the county-health-care-sector level. Finally, we provide estimates of the effects on mortality rates across counties and health sectors.

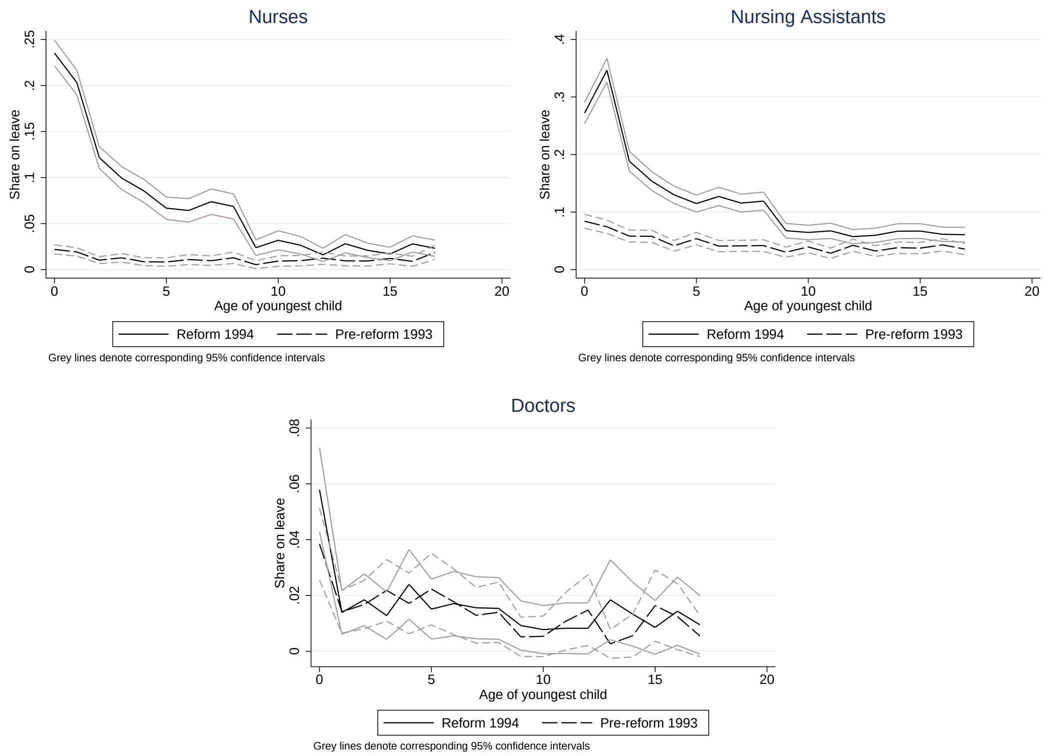
5.1 Program Participation

We first analyze the immediate take-up rates of the parental leave program. Figure 2 displays the fraction of leave takers for different health care workers by the age of their youngest child. Leave takers are defined as workers who were employed in the previous year but are non-participating in the current year. The black dashed line documents the fraction of leavers in the pre-reform year 1993, while the solid line shows the fraction of workers that take one year off in the first post-reform year 1994.[20]

For both nurses and nursing assistants, we find that eligible parents, parents with a child aged 8 or younger, are much more likely to be on leave in the post-reform year in particular if they have young children aged 0 or 1. We attribute the differential effects for parents with children aged 8 or younger to the introduction of the parental leave program. Quantitatively, we interpret the vertical difference between the solid and the dashed line, relative to the difference for children aged 9 or older, as the program's take up effect. The effects are substantial. The evidence suggests that the fraction of nurses with a child aged 1 or younger that take one year of absence increases by about 20 percentage points from 3 percent in 1993 to about 23 percent in 1994. We also notice some considerable bunching for children aged 6-8. This is reasonable given that this is the last chance for parents with an 8-year-old child to take advantage of the program. The pattern is qualitatively similar for nursing assistants, but the take-up rates are smaller because nursing assistants were on average more likely to take a year of absence in the pre-reform period. Second, leave taking among nursing assistants also increases for employees who are not eligible for parental leave, suggesting that nursing assistants are more likely to take advantage of the education and or sabbatical program that was also introduced in 1994 but does not condition on the age of the child. Yet these changes in leave taking are small compared to the increase in leave taking for young parents. We further find that medical doctors in general do not take a year of absence following the birth of a new child. Facing different career dynamics, doctors may risk potential career advancements if they take a leave of absence or they might have better access to child care facilities for

[20] All figures pool both male and female employees. Separate analysis by gender reveals that a large share of the effect is driven by mothers, whereas fathers do not usually participate in long-term leave taking. Note that 95% of nursing assistants and 97% of nurses in the labor market are female over the 1990s. For doctors, the share of women steadily increases from 28% in 1990 to 36% in 2000.

Figure 2: Immediate Program Take-Up



example. We present the analogous regression results in columns (1)-(3) of Table 2, which confirms the evidence from the graphical inspection.

We next turn to the take-up rates in the following years to describe take-up in steady state. The immediate program take-up includes considerable bunching around the age threshold for program eligibility. We expect these initial effects to fade out over subsequent years because many parents with older children have already taken advantage of the program in previous years. Figure 3 illustrates the convergence of take-up rates after the immediate surge in program participation in 1994 to what we consider "steady state" levels in the years 1995 and 1996. The left panel provides evidence for nurses and the right panel presents analogous evidence for nursing assistants respectively. Compared to immediate program take-up, the evidence suggests smaller steady-state take-up rates for parents with children aged 2 or older. We provide the analogous regression-based evidence for 1995 in column (4)-(6) of Table 2.

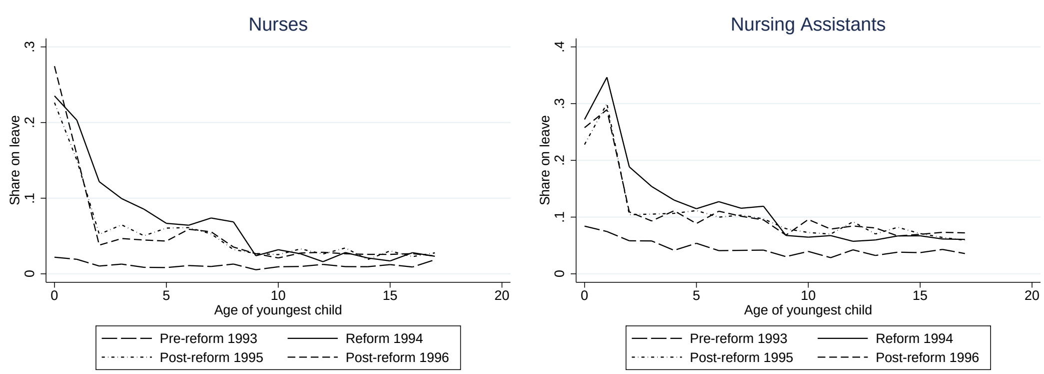
In sum, we find large reform effects on individual leave taking behavior for nurses and nursing assistants. Yet nursing assistants are more likely to take a leave of absence before the reform and their aggregate time trends in Figure 1 show smooth trends across health care sectors before and after the reform, suggesting that the labor supply shock might be alleviated by the drop in private sector employment. In contrast, nurses show the highest leave take-up rates and the aggregate employment trend illustrates a strong increase in non-participation of

Table 2: Program Take-Up

	(1) Nurses	(2) Assistants	(3) Doctors	(4) Nurses	(5) Assistants	(6) Doctors
	1993 vs 1994			1993 vs 1995		
Age0 x Post-Reform	0.2000***	0.1607***	0.0173**	0.1877***	0.1068***	0.0161**
	(0.006)	(0.010)	(0.007)	(0.006)	(0.009)	(0.007)
Age1 x Post-Reform	0.1706***	0.2445***	-0.0024	0.1142***	0.1885***	0.0126*
	(0.006)	(0.010)	(0.007)	(0.006)	(0.010)	(0.007)
Age2 x Post-Reform	0.0982***	0.1029***	-0.0004	0.0253***	0.0090	-0.0072
	(0.007)	(0.010)	(0.007)	(0.006)	(0.011)	(0.007)
Age3 x Post-Reform	0.0734***	0.0686***	-0.0112	0.0352***	0.0102	0.0003
	(0.007)	(0.010)	(0.008)	(0.007)	(0.010)	(0.008)
Age4 x Post-Reform	0.0636***	0.0611***	0.0047	0.0251***	0.0281***	-0.0060
	(0.008)	(0.010)	(0.009)	(0.007)	(0.010)	(0.008)
Age5 x Post-Reform	0.0452***	0.0336***	-0.0093	0.0355***	0.0206**	-0.0060
	(0.008)	(0.010)	(0.009)	(0.008)	(0.010)	(0.009)
Age6 x Post-Reform	0.0401***	0.0589***	-0.0028	0.0335***	0.0219**	-0.0061
	(0.009)	(0.011)	(0.009)	(0.008)	(0.010)	(0.009)
Age7 x Post-Reform	0.0508***	0.0469***	0.0006	0.0264***	0.0251**	0.0007
	(0.009)	(0.011)	(0.009)	(0.008)	(0.010)	(0.009)
Age8 x Post-Reform	0.0425***	0.0499***	-0.0007	0.0025	0.0179*	-0.0023
	(0.009)	(0.011)	(0.009)	(0.008)	(0.011)	(0.009)
Post-Reform	0.0132***	0.0274***	0.0021	0.0167***	0.0371***	-0.0002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	58,532	52,517	17,358	56,363	50,445	17,420
R-squared	0.088	0.064	0.008	0.073	0.043	0.008

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Figure 3: “Steady State” Program Take-Up



nurses in 1994 that goes along with a visible decrease in employment of nurses in all sectors. As a result, we expect the largest labor supply shock from the reform for nurses and focus our subsequent analysis on these workers.

5.2 Aggregate Effects on Employment

This section estimates aggregate effects of the parental leave program on employment of nurses across counties and health care sectors by using variation in parental leave take-up between counties, which coordinate and finance the delivery of health care services independently.

Following the strategy outlined in Section 3.2, we calculate the expected fraction of leavers among nurses by county and health care sector, and refer to this measure as parental leave "exposure". Specifically, we construct a time-invariant exposure measure that builds on the demographic composition of the last pre-reform year 1993 and the steady-state take-up probabilities for nurses from 1995, see column 4 of Table 2. We interpret this exposure measure as the effect of the parental leave program on the fraction of nurses on parental leave. This interpretation builds on the idea that nurses return to their employer after one year of parental leave. We will come back to this assumption below.

Table 3 shows that the average exposure for total employment across counties is 2.5% and there is considerable variation across counties. Average exposure is higher in hospitals (3.2%) than in nursing homes (1.6%), but the dispersion across counties is larger in the nursing home sector. A county with one standard deviation higher exposure expects to lose 11.5% more nurses in hospitals or 28.2% more nurses in nursing homes than the average county.

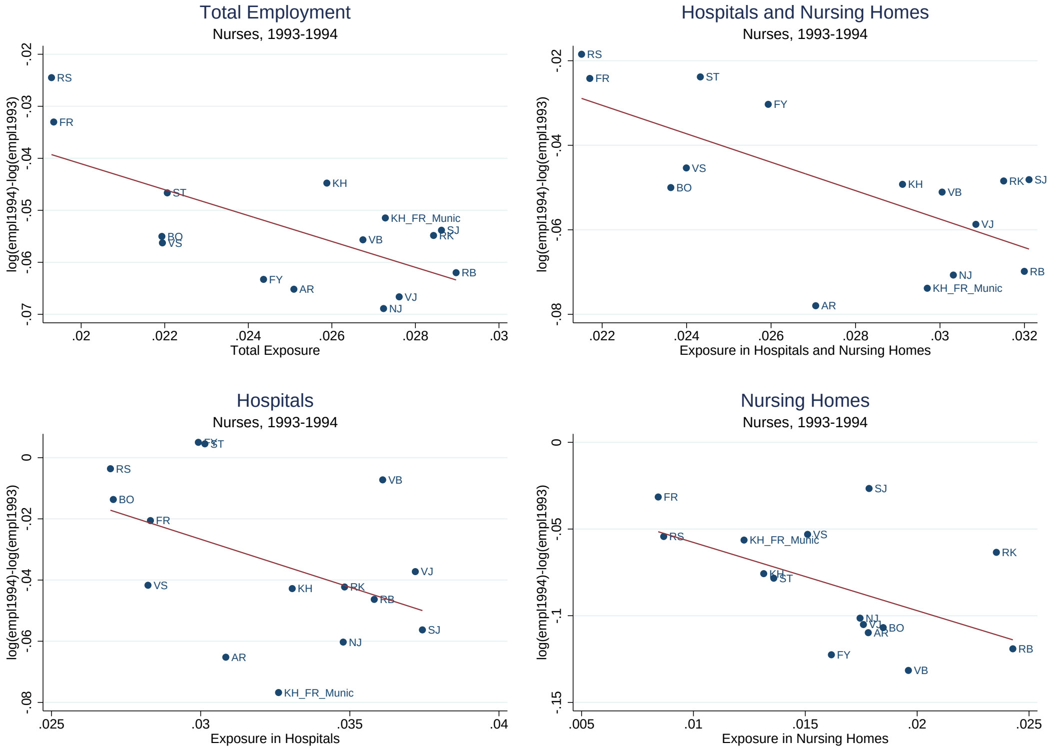
Table 3: Exposure for Nurses

	Total	Hosp and NH	Hosp	NH	Not Hosp
Mean	0.025	0.0276	0.0322	0.0163	0.0162
Sdev	0.0033	0.0038	0.0037	0.0046	0.0032
Min	0.0193	0.0215	0.0270	0.0084	0.0099
Max	0.029	0.0321	0.0374	0.0243	0.0212

We illustrate the relationship between reform exposure and immediate changes in employment after the reform in Figure 4. We compute the log change in employment by county between 1993 and 1994 for each health worker type and sector and plot these changes relative to exposure by county in 1993. There is a stronger decline in employment for counties with higher initial exposure to the reform. In fact, many counties with high exposure lose more than 5 percent of nurses in hospitals and more than 10 percent in nursing homes in the first year of the reform.

Figure 5 illustrates the estimates for the main specification in equation (2). The first row shows estimates using total employment and employment in both health sectors respectively. The second row shows employment effects of exposure separately for hospitals and nursing

Figure 4: Reform Exposure and Employment Change 1993-1994



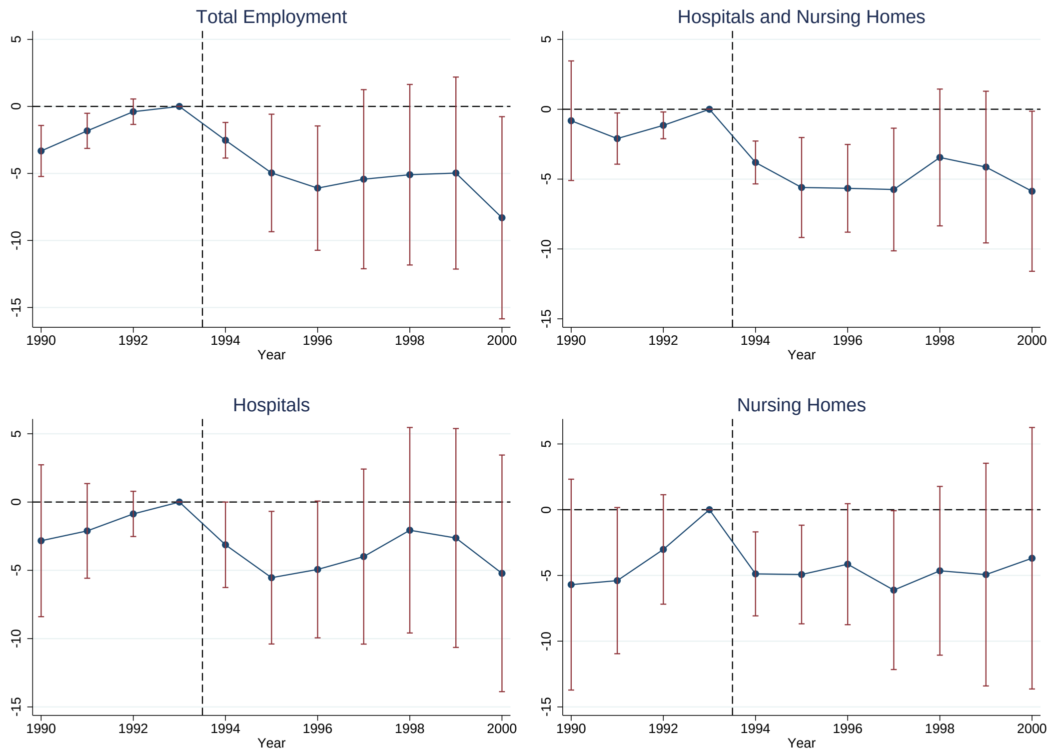
homes. For all specifications, there is an initial upward trend in the coefficient estimates, indicating that counties with higher exposure in 1993 grew faster in the years before the reform. This pre-trend is more pronounced at the individual health care sector level, in particular for nursing homes. All figures show a strong structural break in this growth path in 1994 when the reform starts and persistent effects in subsequent years.[21] The negative λ estimates in the post-reform years confirm the negative effect of exposure on net employment.

We report the net employment effects in Table 4. After analyzing reform effects on total employment in column (1), columns (2) to (4) of Table 4 consider the effect in more detail by looking at employment and exposure for different health sectors.

The first row of Table 4 reports the average effect of exposure on aggregate employment for nurses in the post-reform years. Specifically, we estimate a difference-in-differences regression model with separate county and year fixed effects for the years 1991-1996 and report the effect of exposure interacted with a post-reform indicator variable. The point estimates suggest negative net-employment effects for total employment as well as employment in nursing homes and hospitals. However, the point estimates are relatively small for hospitals and nursing

[21] All corresponding regressions are reported in Table 12 in the Appendix.

Figure 5: Lambda Estimates for Employment Effects by Sector Exposure



homes in particular when compared to the corresponding slope estimates from Figure 4 and the overall time-series variation reported in Figure 1.[22] This is largely because this specification does not account for the pre-trends illustrated in Figure 5. We augment the regression model in the second row by adding linear time trends by county that capture general trends in employment. The results of this exercise show a large negative effect of reform exposure on nurse employment. Pre-trends are more prevalent at the level of individual health care sectors and the coefficient estimates become larger and more precise after adding county trends. Using the estimates from the augmented specification and average exposure in the pre-reform year 1993 across counties, the bottom of Table 4 reports that total employment decreases by $0.025 \cdot 2.33 = 0.058$ log points. The coefficients for exposure to the reform by subsector are more pronounced than for total employment. Counties with higher exposure in hospitals or nursing homes have significantly lower employment of nurses in these sectors in the post-reform years than counties with lower exposure. One standard deviation higher exposure is associated with 1.4 log points lower employment in hospitals and 3.1 log points lower employment in nursing homes respectively. Overall, we find that the parental leave program reduced net

[22] For example, when scaled with the average exposure in nursing homes the point estimate suggests an employment reduction of 0.0163*1.88=3% which corresponds to about 3%*11,000=330 skilled nurses. The national trend indicated in Figure 1, on the other hand, suggest a decrease of about 1,000 skilled nurses.

employment in hospitals and nursing homes by 12% and 11% respectively, as indicated in the last row of Table 4. These estimates echo the preliminary findings outlined in the data section, see Figure 1.

Table 4: Net Employment Effects for Nurses

	(1) Total	(2) Hosp and NH	(3) Hosp	(4) NH
λ^{post}	-3.949**	-4.389***	-4.361*	-1.88
	[-.005,-7.893]	[-1.328,-7.449]	[.409,-9.131]	[3.135,-6.895]
λ^{post}_{trend}	-2.355	-4.034***	-3.952*	-6.698***
	[1.014,-5.724]	[-1.42,-6.649]	[.407,-8.311]	[-2.378,-11.018]
Δ_1	-2.918***	-4.96***	-4*	-7.904**
	[-1.187,-4.65]	[-2.633,-7.288]	[.266,-8.266]	[-.287,-15.521]
Δ_2	-6.785***	-7.696***	-7.651**	-10.325***
	[-1.649,-11.922]	[-3.147,-12.246]	[-.019,-15.283]	[-2.555,-18.095]
Δ_3	-9.416***	-6.479**	-7.768*	-9.844*
	[-4.44,-14.393]	[-.883,-12.075]	[1.413,-16.948]	[.8,-20.489]
Pre-Reform Value	52443	40886	29761	11125
Avg. Effect	-.059	-.111	-.127	-.109

The 95% confidence interval is displayed in brackets.
Standard errors are clustered at the county level.
* $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

The results suggest that the ability of hospitals and nursing homes to recruit nurses from other occupations is limited because the average effect of exposure in these health sectors is larger than for total employment. Further note that the point estimates for exposure are less than -1. A coefficient of minus 1 is an interesting benchmark because it indicates that nurses on parental leave reduce net employment one-to-one, suggesting that employers are unable to replace any leaver on net. A coefficient of less than one in absolute value would suggest that employers can at least partially replace nurses on parental leave, for example by reactivating nurses outside the labor force. The point estimates in Table 4 suggests the opposite, that employment decreases by more than the number of predicted leavers. For nursing homes, -1 lies outside the 95% confidence interval in the preferred specification. This finding is consistent with workers leaving for more than one period and negative spillover effects of leavers on nurses ineligible for the program. First, note that our estimates for take-up rates in Table 2 measure the probability of leave conditional on working in the previous year. Based on the maximum program duration of 12 months, we do not take longer duration of leave into account. If some workers decide to extend their leave beyond the program duration, this implies larger employment losses that are not captured by our exposure measure.[23] Second, some of the increase in leave probabilities for ineligible workers might be due to indirect

[23]In fact, we find that less than 70 percent of nurses who are eligible for parental leave and decide to take a leave of absence during the period 1994-2000 return to the same county and sector within five years. If 30 percent of leavers do not return, the stock of leavers after five years with an equal number of leavers per year increases by a factor of 2.5.

effects of the parental leave program. If co-workers suffer from leave taking of eligible nurses in terms of workload and hours for example, the program might induce other workers to leave, which is again consistent with our large exposure estimate.[24] Finally, note that we use leave taking behavior in 1995 to compute predicted leavers and exposure. This exposure measure understates the immediate reform outcomes and hence will lead to larger coefficient estimates to fit the large initial drop in employment.

The next three rows of Table 4 build on our primary regression (2) as illustrated in Figure 5 and report estimates for the average effect of the reform over $\tau = 1, 2, 3$ years taking pre-trends into account,

$$\Delta_{\tau}^{trend} = \frac{1}{\tau} \sum_{t=1}^{\tau} \left[(\lambda_{1993+t}^{s} - \lambda_{1993}^{s}) - (\lambda_{1993}^{s} - \lambda_{1993-t}^{s}) \right]. \tag{3}$$

Following the empirical approach in Finkelstein (2007), each summand in equation (3) compares the change in trends relative to the reference year 1993 for t periods before and after the reform. This specification implies that the pre-trends would have continued at the same rate after the reform. We find significant negative effects in all specifications, both for total employment and for sectoral employment changes, except for the one period effect in hospitals. Interestingly, the effects increase over time, indicating a cumulative reduction in employment that is consistent with low re-entry rates of leave takers and negative spill-over effects on co-workers. The size of the average effect over three years is larger than the exposure effects for the post-reform period estimated in the first two rows, which may indicate that the assumption of persistent pre-trends in (3) provides an upper bound to the effect.

In sum, we find large effects of the parental leave program on aggregate employment of nurses, in particular in counties and subsectors with high exposure to begin with.

5.3 Mortality

In the previous section, we have documented that the parental leave program led to a sizable reduction of the number of nurses working in hospitals and nursing homes. In this section, we provide evidence for adverse effects of reform-induced nurse shortages on patient mortality.

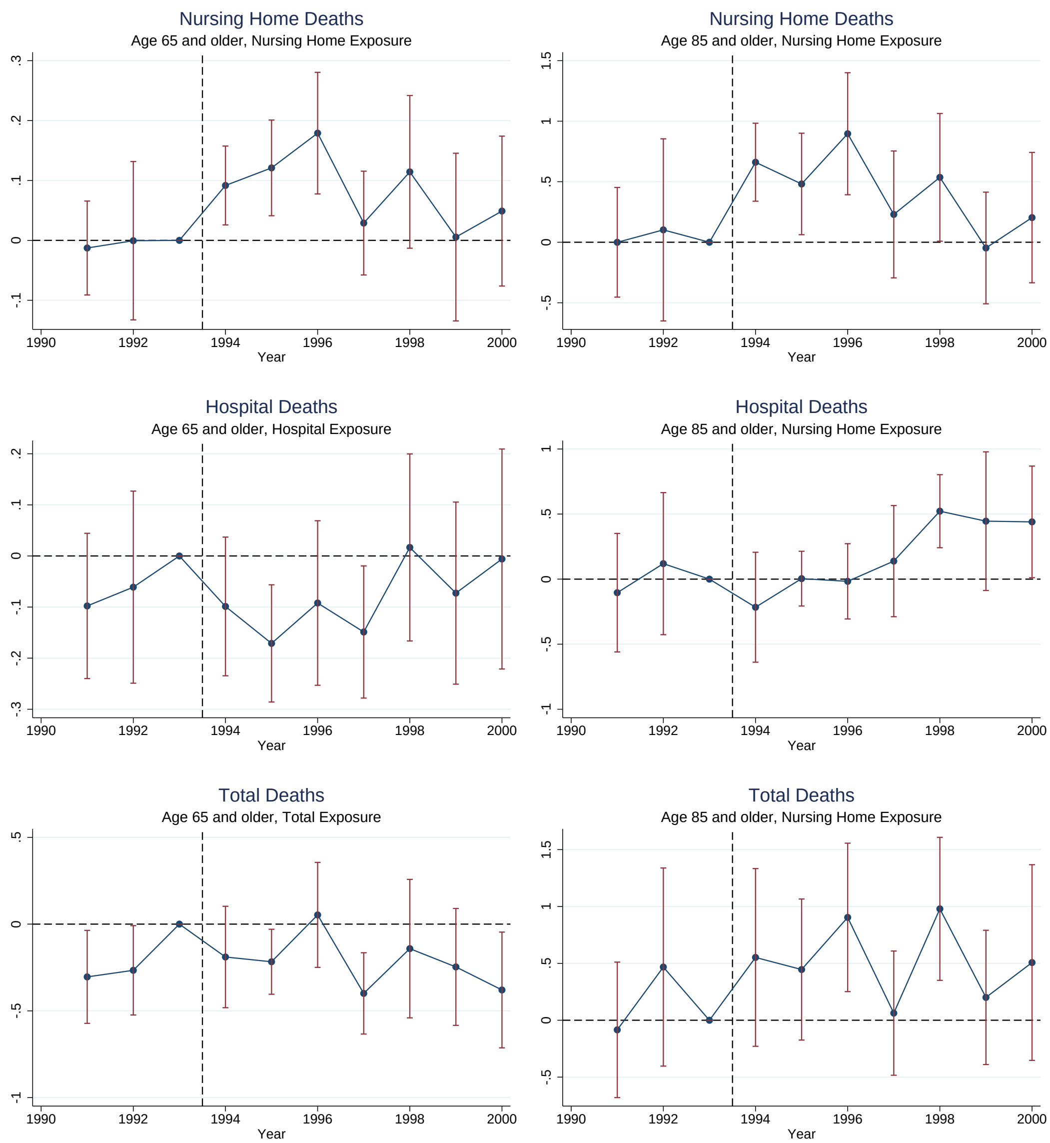
We illustrate the effects of the reform on mortality for the population aged 65 and older based on our main specification in equation (2) in the left panel of Figure 6.[25] The first figure shows the effect of exposure in nursing homes on mortality rates in nursing homes for this age group. There are no pre-trends of exposure on mortality rates, but there is a striking increase in mortality rates in the post-reform years. The figure suggests that the effects are even intensifying over time as more nurses leave the sector. The figure for hospital deaths suggests

[24] This hypothesis is testable using our matched data set and is part of our research agenda.

[25] We report the corresponding regression results in Appendix Table 13.

the opposite pattern, namely that mortality rates in hospitals decrease slightly in counties with higher exposure in hospitals after the reform. This pattern is consistent with internal reallocation in hospitals to focus on high risk patients or with patients being transferred to nursing homes for further treatment in response to shortages in the hospital sector. We further study these adjustment patterns and additional patient outcomes in hospitals below. The figure for total deaths among the population aged 65 and older does not suggest a structural break after 1993 as a function of total county-level exposure.

Figure 6: Lambda Estimates for Mortality Effects by Sector Exposure



We summarize the average reform effects for mortality among individuals aged 65 and older in Table 5. We estimate the effect of higher reform exposure for the time period 1991-1996 with county and year fixed effects as well as controls for county population. The measure of reform exposure for each specification corresponds to the aggregation level. Following the previous discussion, λ^{post} without trends in the first row is our preferred estimate of the average mortality effect.[26] Column (1) documents the small and insignificant change in total mortality among individuals aged 65 and older after the reform. Columns (3) and (5) provide evidence that mortality in nursing homes increases significantly for the three years after the reform, whereas for hospitals there is some evidence of a decline, which is however not statistically significant. The magnitude of the effect in nursing homes is sizable; as the bottom row of Table 5 documents, the point estimate of 0.14 together with an average exposure in nursing homes of 0.016 translates into an average increase in mortality rates by 0.2 percentage points. This is a 12.5% increase relative to the pre-reform mortality rate of 0.016. In hospitals, the point estimate indicates a mortality decline by 10.3% but this effect is not precisely estimated. Notice, that the nursing home (hospital) mortality rates are unconditional mortality rates: they do not condition on a nursing home (hospital) stay.

Table 5: Mortality Effects Across Health Sectors: Age 65 and older

	(1) Total	(2) Total	(3) Hosp	(4) Hosp	(5) NH	(6) NH
λ^{post}	.019	.116	-.079	-.051	$.14^{***}$	$.168^{***}$
	[-.196,.233]	[-.072,.303]	[.066,-.224]	[.049,-.152]	[.087,.193]	[.105,.231]
λ^{post}_{trend}	$-.241^{*}$	$-.238^{*}$	-.1	-.157	.005	.008
	[.041,-.522]	[.039,-.515]	[.13,-.33]	[.056,-.371]	[-.092,.102]	[-.089,.105]
Δ_1	-.19	-.183	-.099	-.13	$.092^{**}$	$.099^{***}$
	[.16,-.539]	[.141,-.506]	[.063,-.261]	[.032,-.292]	[.013,.17]	[.029,.169]
Δ_2	-.07	-.063	-.104	$-.109^{*}$	$.107^{***}$	$.128^{***}$
	[.124,-.264]	[.126,-.252]	[.049,-.258]	[.017,-.234]	[.051,.162]	[.067,.189]
Δ_3	.072	.068	-.068	-.061	$.135^{***}$	$.166^{***}$
	[-.126,.27]	[-.144,.28]	[.076,-.211]	[.045,-.167]	[.074,.196]	[.099,.232]
Add. Controls	✗	✓	✗	✓	✗	✓
Pre-Reform Value	.057	.061	.029	.031	.016	.017
Avg. Effect	0	.003	-.003	-.002	.002	.003

The 95% confidence interval is displayed in brackets.
Standard errors are clustered at the county level. In columns 2, 4, and 6 we added further controls including previous hospitalizations and age-gender fixed effects.
* $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

Since the demographic structure among the elderly might vary across counties, columns (2), (4) and (6) repeat the analysis taking risk factors of the county population such as previous hospitalizations and gender and age structure into account.[27] This specification strengthens

[26]Introducing county trends in the second row introduces noise in the estimates because the pre-reform patterns in mortality are very noisy. As a result, the mortality effects disappear or reverse sign in this specification.

[27]In particular, the model controls for age-gender fixed effects, as well as controls for the number of bed days in the hospital and fixed effects for the number of hospital visits in the current and previous year.

the results and implies an increase in nursing home mortality by 17.6%. In a county with one standard deviation higher exposure in nursing homes, mortality increases by an additional 4.5%.

Finally, we also estimate the average reform effect over $\tau = 1, 2, 3$ years without taking pre-trends into account,

$$\Delta_\tau = \frac{1}{\tau}\sum_{t=1}^{\tau}\left[\lambda^s_{1993+t} - \lambda^s_{1994-t}\right]. \tag{4}$$

The results for Δ_τ in Table 5 parallel the graphical evidence that suggests an increase in mortality in nursing homes over time. The effect in the first year after the reform is about 60% of the average effect over three years. This is consistent with the cumulative employment loss in nursing homes documented in Table (4). Intuitively, patients in nursing homes may suffer from an increased frequency of mistakes in medication or lack of supervision and assistance and these effects may be exacerbated as more nurses leave the sector.

The previous results suggest that exposure in nursing homes might create a bottleneck in health care delivery. In the next step, we further analyze the role of these shortages on mortality rates across counties. If patients are merely reallocated from hospitals to nursing homes, we would not expect total mortality effects in response to nurse shortages in nursing homes. The age structure of nursing home patients reported in Table 1 suggests that the population aged 65 and older may hide larger effects on older individuals who are more likely to be in a nursing home and who face higher baseline mortality rates. As a result, we now focus on the population aged 85 and older.

Table 6: Mortality Effects of Nurse Shortages in Nursing Homes: Age 85 and older

	(1) Total	(2) Total	(3) Hosp	(4) Hosp	(5) NH	(6) NH
λ^{post}	.485***	.702***	-.087	.036	.626***	.673***
	[.188,.781]	[.417,.987]	[.264,-.439]	[-.189,.262]	[.297,.956]	[.335,1.01]
λ^{post}_{trend}	.348	.33	-.24	-.231	.565*	.524*
	[-.678,1.374]	[-.571,1.231]	[.319,-.799]	[.257,-.72]	[-.034,1.163]	[-.058,1.106]
Δ_1	.552	.564	-.216	-.211	.661***	.644***
	[-.382,1.486]	[-.232,1.36]	[.289,-.721]	[.203,-.624]	[.276,1.046]	[.285,1.003]
Δ_2	.265	.423***	-.166	-.066	.52***	.542***
	[-.095,.625]	[.159,.688]	[.185,-.516]	[.205,-.337]	[.151,.889]	[.165,.918]
Δ_3	.506***	.708***	-.081	.032	.646***	.688***
	[.14,.872]	[.421,.995]	[.288,-.451]	[-.207,.271]	[.264,1.027]	[.317,1.059]
Add. Controls	✗	✓	✗	✓	✗	✓
Pre-Reform Value	.16	.165	.055	.058	.078	.079
Avg. Effect	.008	.011	-.001	.001	.01	.011

The 95% confidence interval is displayed in brackets.
Standard errors are clustered at the county level. In columns 2, 4, and 6 we added further controls including previous hospitalizations and age-gender fixed effects.
* $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

The right panel in Figure 6 provides graphical evidence for the mortality effects in nursing homes, hospitals and the overall county level as a function of reform exposure in nursing homes. First note that for this older age group, there is again a clear pattern of increased mortality in nursing homes in the post-reform years. Intuitively, the absolute size of the effects is magnified compared to individuals aged 65 and older. The corresponding average mortality effect λ^{post} reported in column (5) of Table 6 indicates an increase in nursing home mortality by 1 percentage point among the population aged 85 and older, which translates into a similar relative increase compared to pre-reform levels of 12.8% as for the age group 65 and older. Figure 6 further shows that exposure in nursing homes has no effect on mortality rates in hospitals. The point estimates are flat and close to zero for the three years before and after the reform. Finally, the figure for total deaths shows large positive point estimates for total mortality rates as a function of nursing home exposure. Similar to the pattern in nursing homes, total mortality rates at the county level gradually increase over 1994-1996 and the effect in 1996 is statistically significant. This result provides evidence against the hypothesis that the increase in nursing home mortalities merely reflects patient reallocation between hospitals and nursing homes. Table 6 shows that the size of the average effect on total mortality in the specification with additional controls in column (2) is equal to the effect for nursing homes in column (6). In general, staffing shortages in nursing homes may also lead to rationing of nursing home spots and increased mortality at home. But the previous finding suggests that it is the remaining nursing home residents who mainly suffer from increased mortality rates.

Figure 7: Mortality Effects of Nurse Shortages in Nursing Homes: Age 85 and older

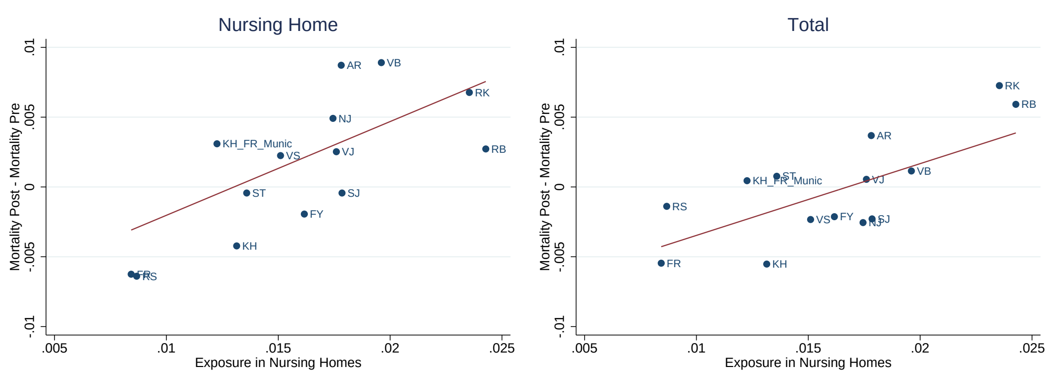


Figure 7 graphically summarizes the main results in this section. In this figure, we plot the average change in the mortality rate among the elderly aged 85 and older between post-reform years 1994-1996 and pre-reform years 1991-1993 against the exposure in nursing homes at the county level. The left figure shows a larger increase in nursing home mortality in counties with a higher exposure in the nursing home sector. The right figure shows that a higher nursing home exposure also raises the overall mortality rate in the respective county, which rules out pure patient reallocation across different health care sectors and home care and instead emphasizes the adverse effects of the reform on patient health in general.

Finally, we decompose the mortality effects in nursing homes for individuals age 85 and older in Figure 8, distinguishing cardiovascular and respiratory deaths, infections, cancer, and degenerative brain diseases. Table 7 reports the corresponding regression results. We find that the increase in nursing home mortality rates is mainly driven by cardiovascular deaths and an increase in brain diseases. These two categories account for two-thirds of the overall increase in mortality among the population age 85 and older. Brain diseases include dementia and senility and are most common among the oldest and weakest patients in nursing homes, suggesting a disproportionately large mortality effect for this group. However, the effect on cardiovascular related mortality is 75% larger which may include mortalities of overall healthier residents as well.

Table 7: Mortality Effects by Cause of Death in Nursing Homes: Age 85 and older

	(1) All	(2) Cir	(3) Neop	(4) Inf	(5) Res	(6) Bra
λ^{post}	.626***	.256***	-.023	0	.074	.145**
	[.297,.956]	[.097,.415]	[.104,-.15]	[-.049,.05]	[-.047,.194]	[.016,.274]
λ^{post}_{trend}	.565*	.12	.13	-.004	.125	.172
	[-.034,1.163]	[-.308,.548]	[-.07,.33]	[.075,-.083]	[-.083,.332]	[-.054,.398]
Δ_1	.661***	.301*	.049	.014	.068	.141*
	[.276,1.046]	[-.038,.64]	[-.083,.181]	[-.043,.071]	[-.104,.239]	[-.021,.303]
Δ_2	.52***	.083	.043	-.007	.105*	.18***
	[.151,.889]	[-.177,.343]	[-.084,.169]	[.055,-.069]	[-.008,.219]	[.05,.31]
Δ_3	.646***	.263***	-.019	.001	.069	.15*
	[.264,1.027]	[.092,.434]	[.128,-.166]	[-.052,.053]	[-.076,.213]	[-.006,.306]
Pre-Reform Value	.078	.046	.007	0	.008	.007
Avg. Effect	.01	.004	0	0	.001	.002

The causes of death are: Cir-Circulatory, Neop-Neoplasms, Inf-Infections, Res-Respiratory, Bra-brain diseases.
The 95% confidence interval is displayed in brackets. Standard errors are clustered at the county level.
* $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

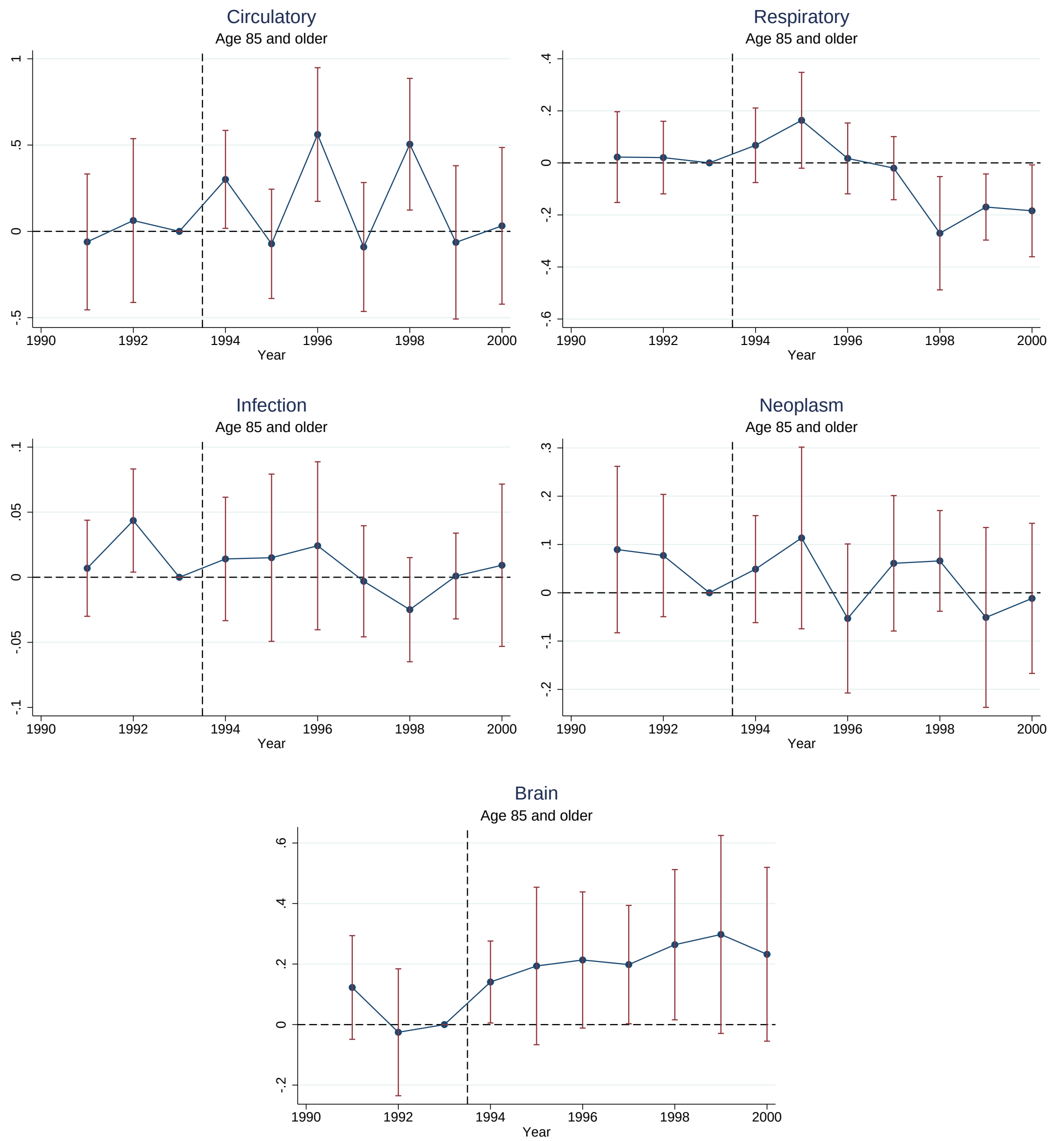
Finally, we find very similar effects regarding the cause of death when considering overall mortality at the county level. This suggests again that the increase in nursing home mortality does not primarily reflect patient reallocation between hospitals and nursing homes.[28]

5.4 Other health outcomes in hospitals

Based on the lack of mortality effects in hospitals, we also analyze whether hospital patients are affected at other margins. Using data from the Danish National Patient Register, we investigate the effects of the nurse shortage on the number of visits, the length of stay, the one-month readmission probability, the number of wait days, and the one-year mortality rate

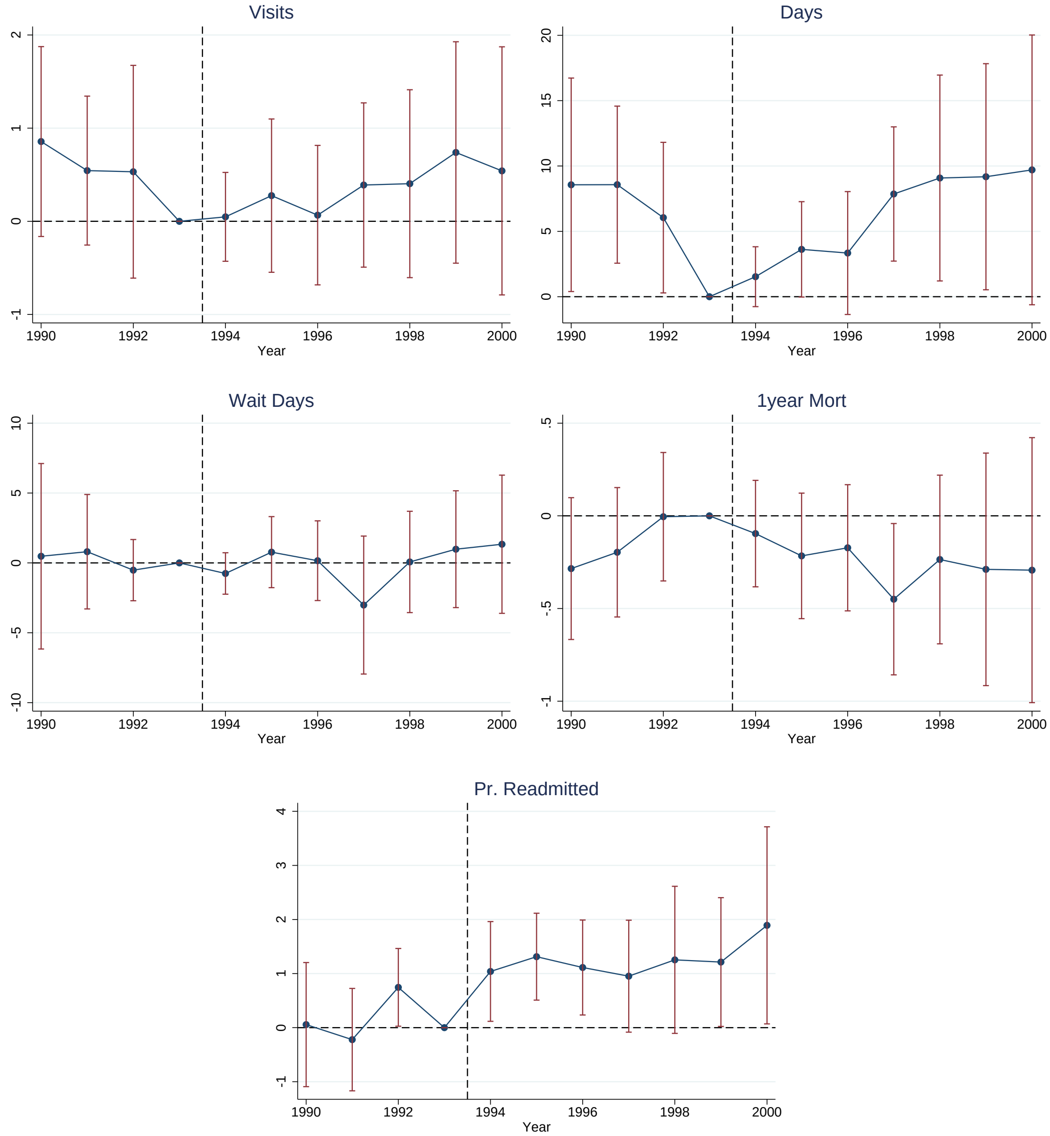
[28]The corresponding tables and figures are available upon request.

Figure 8: Lambda Estimates for Mortality Effects by Cause of Death in Nursing Homes



among inpatient hospital stays. We focus on patients at all ages but find qualitatively similar results when we restrict the analysis to elderly patients aged 65 and older. The corresponding graphs of the lambda estimates are illustrated in Figure 9. The graphs in the first row of the figure indicate a trend break in the number of hospital visits and the length of stay following the introduction of the parental leave program in 1994. Both measures trend downwards in the pre-reform years but start to increase in the post-reform years. We present the corresponding regression coefficients in the first two columns in Table 8. Taking pre-trends into account, foolowing the approach outlined in equation (3), we find a significant increase in the number of visits and the length of stay in the first three post-reform years as indicated in rows 3-5.

Figure 9: Lambda Estimates for Other Measures of Patient Outcomes in Hospitals



We neither find evidence for systematic changes in the number of wait days nor for the one-year mortality rate. However, we do find evidence for a significant increase in the one-month readmission rate as indicated by the fifth column of Table 8. One interpretation of these

Table 8: Other Measures of Patient Outcomes in Hospitals

	(1) Visits	(2) Days	(3) Wait Days	(4) 1year	(5) Readm
λ^{post}	-.229	-2.042	-.037	-.094	.98***
	[.771,-1.229]	[3.776,-7.86]	[3.927,-4.002]	[.159,-.348]	[.199,1.762]
λ^{post}_{trend}	.166	3.029**	-.122	-.184	.759
	[-.397,.729]	[.094,5.965]	[3.194,-3.438]	[.266,-.635]	[-.766,2.284]
Δ_1	.58	7.581*	-.756	-.096	1.039*
	[-1.009,2.169]	[-.538,15.7]	[1.015,-2.528]	[.247,-.439]	[-.063,2.141]
Δ_2	.7	9.888***	.266	-.154	.803**
	[-.547,1.947]	[1.938,17.837]	[-2.751,3.282]	[.124,-.431]	[.004,1.603]
Δ_3	.774	10.56***	-.037	-.094	.98***
	[-.497,2.045]	[1.986,19.134]	[4.048,-4.123]	[.167,-.356]	[.175,1.786]
Pre-Reform Value	.192	1.228	.381	.085	.169
Avg. Effect	.005	.097	-.001	-.003	.032

The 95% confidence interval is displayed in brackets.
Standard errors are clustered at the county level. The delta estimates in columns 1 and 2 take pre-trends into account.
* $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

findings is that the lack of nurses on staff has a negative effect on health care delivery in hospitals as indicated by higher readmission rates and longer recovery times as indicated by longer hospital stays.

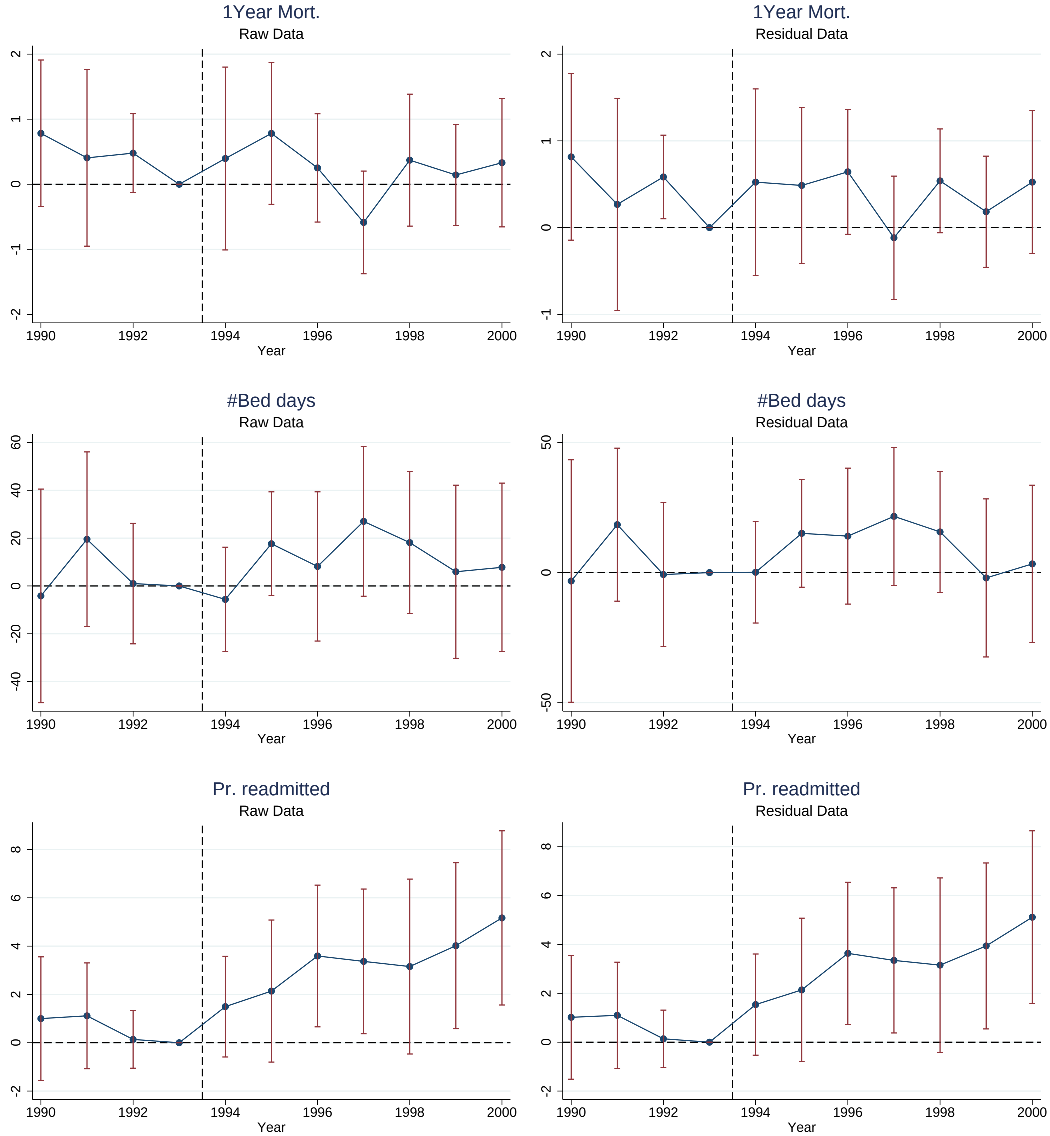
Table 9: Nurse Shortage and Newborns in Hospitals

	(1) Days	(2) Days	(3) Readm	(4) Readm	(5) 1year	(6) 1year
λ^{post}	-.113	3.851	1.991*	2.025*	.181	.267
	[25.11,-25.34]	[-21.09,28.78]	[-.13,4.12]	[-.11,4.16]	[-.46,.82]	[-.29,.82]
λ^{post}_{trend}	4.192	7.248	1.254	1.276	.594	.379
	[-32.29,40.67]	[-22.93,37.42]	[-2.23,4.73]	[-2.19,4.74]	[-1.87,3.06]	[-1.52,2.28]
Δ_1	-5.625	.129	1.495	1.538	.395	.524
	[20.45,-31.70]	[-23.19,23.45]	[-1.00,3.99]	[-.94,4.01]	[-1.28,2.08]	[-.76,1.81]
Δ_2	5.513	7.977	1.748	1.769	.349	.213
	[-19.88,30.91]	[-16.01,31.97]	[-.93,4.43]	[-.92,4.46]	[-.83,1.53]	[-.72,1.14]
Δ_3	-.113	3.851	1.991*	2.025*	.181	.267
	[25.88,-26.11]	[-21.84,29.54]	[-.20,4.18]	[-.17,4.22]	[-.48,.84]	[-.31,.84]
Add. Controls	✗	✓	✗	✓	✗	✓
Pre-Reform Value	4.756	4.953	.056	.06	.019	.021
Avg. Effect	-.004	.124	.064	.065	.006	.009

The 95% confidence interval is displayed in brackets. Standard errors are clustered at the county level.
In columns 2, 4, and 6 we flexibly control for the birth weight. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.001$

We also investigate the effects of nurse shortages on health care delivery in a more homogenous group of patients with highest policy relevance - newborn babies. In Figure 10, we

Figure 10: Lambda Estimates for Effects on Newborns



present graphical evidence on the effects of nurse shortages on the one-year mortality rate, the number of bed days, and the one-month readmission probability. We present the lambda coefficients based on the "raw data" in the left column and based on the "residual data" in the right column, which conditions on the babies' birth weights. We see a statistically significant increase in the one-month readmission rates as indicated by columns 3 and 4 in Table 9. We also find suggestive evidence for an increase in the number of bed days once we condition on the babies' birth weights as indicated by column 2 of Table 9. Overall, the presented evidence for newborns confirms the evidence for the entire hospital inpatient population presented above.

5.5 Mechanisms

In this section we explore the mechanisms behind the observed differences in mortality effects between hospitals and nursing homes. In particular, we investigate whether employers mitigate the effects of nurse shortages and how these efforts vary between health care providers.

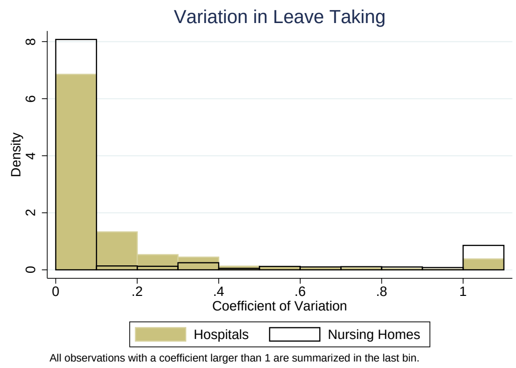
We first analyze whether hospitals and nursing homes differ systematically in terms of the staffing fluctuation that they face due to the reform. Better capacity management may be an important explanation why hospitals prevent adverse mortality effects. In order to compare fluctuations in leave taking across providers with different staffing levels, we construct the coefficient of variation for leave taking at the individual provider level based on the exact timing of leave taking reported by social benefit data for 1995-2000. Since both hospitals and nursing homes face strong seasonality in leave taking, we analyze capacity management of health care providers within months.[29] In particular, we take the standard deviation of leave takers per day within each month relative to the average monthly number of leave takers. Figure 11 shows the distribution of monthly coefficients of variation for hospitals and nursing homes, winsorized at 1.[30] Months without any inflow or outflow of leavers are common in nursing homes, but at the same time these providers face very high fluctuation in other months. Overall, this evidence suggests that hospitals are more likely to avoid peak shortages which might have large adverse effects.

Next, we further use the data on benefit spells to analyze whether health care providers differ in their influence on take-up and leave duration. To this end, we display the OLS regression coefficient of exposure on a leave taking indicator variable, that turns on if the eligible nurse is on leave in the given year, and a leave duration indicator variable among leave takers, that turns on if the leave exceeds 26 weeks, in odd and even columns of Table 10, respectively. The first two columns present the coefficients for the full sample of hospitals

[29] Appendix Figure 11 illustrates these seasonal differences in leave taking. In general, the number of nurses on leave peaks in June and July and decreases during the winter months.

[30] We focus on all individual providers with at least two skilled nurses and set the coefficient of variation to zero for months without leavers.

Figure 11: Parental Leave and Staffing Management

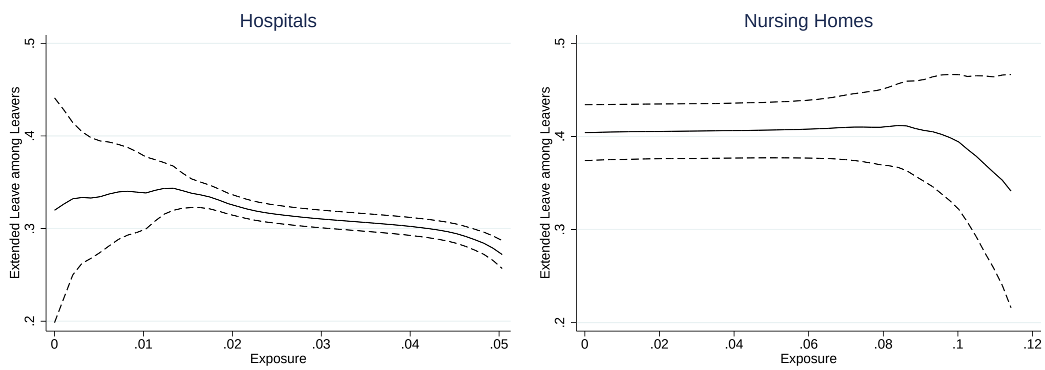


and nursing homes before we split the sample into hospitals, columns (3) and (4), and nursing homes, columns (5) and (6). The table shows two main results. First, both hospitals and nursing homes cannot affect the extensive margin of leave taking among eligible parents. Columns (1), (3) and (5) show that higher exposure does not affect the probability of leave taking among eligible parents. The parental leave reform entitled all parents with children up to 8 years of age to take 26 weeks of leave without employer approval. In line with the policy design, leave taking depends on worker preferences and cannot be influenced by employers. Second, columns (2), (4) and (6) analyze the intensive margin of leave taking among leavers. Even though parents are entitled to leave, employers can influence the duration of leave because extended leave of up to 52 weeks had to be approved by the employer. As a result, the probability of extended leave reflects employer preferences. We find that hospitals that are more exposed to the reform are more likely to prevent *extended* leave taking of their nurses, as indicated by the statistically significant negative coefficient in column 4. Yet while hospitals with average exposure reduce the probability of extended leave among leavers by 18.3 percentage points, there is no significant reduction in nursing homes with higher exposure. One explanation for the differences in behavior across providers could be that hospitals have higher bargaining power to retain their employees if necessary. As a result, hospitals may be better able to retain qualified workers and to stabilize the workforce overall. This adjustment may explain why mortality effects are largest in nursing homes that do not benefit from this type of employee management.

Table 10: Employer Approval for Extended Leave

These adjustment mechanisms in combination with the differences in mortality effects across hospitals and nursing homes suggest an important role for nonlinearities in the health production function. Specifically, we suspect that large exposures lead to disproportionately drastic negative consequences if left unaddressed. We revisit the employer efforts to mitigate the adverse staffing implications of large policy exposures in Figure 12, where we present nonparametric Epanechnikov kernel estimates and 95% confidence intervals for the conditional probability of extended leave among leavers. We find that the share of extended leave takers is much lower at providers with high exposure. Yet hospitals proportionately reduce the share of extended leave according to the overall fraction of leavers, whereas the share of extended leave in nursing homes is stable across a wide range of exposure and only declines for facilities with very high exposure levels.[31] Figure 12 also emphasizes the level differences in extended leave taking across hospitals and nursing homes, with hospitals avoiding extended leave much more frequently. In sum, providers with the largest exposure show the strongest endogenous response to prevent large nurse shortages and adverse health outcomes for patients. Hospitals are more effective in avoiding staffing fluctuations and in preventing extended leave and as a result the relationship between exposure and patient health outcomes is weaker and noisier for these providers.

Figure 12: Nonlinearities in Employer Responses



5.6 Discussion

Our empirical findings indicate that the parental leave program reduced the number of skilled nurses in nursing homes by 11% and increased the nursing home mortality among the elderly aged 65 and older by 12.5%. This implies a nurse-mortality elasticity in nursing homes of about 1.1. In absolute numbers, we find that the parental leave program reduced the number of skilled nurses by about 10.9%x$11,000$=$1,200$ nurses which raised the number of mortalities among the elderly aged 65 and older by 0.002x$850,000$ elderly people = $1,700$ deaths per year.

[31] In general, estimates of health effects at the provider level will be biased if endogenous adjustments are strongest at providers with highest exposure because this response leads to a nonlinear relationship between exposure and health outcomes. Our aggregation at the county level mitigates this potential bias at the provider level.

Overall, this suggests that skilled nurses play an important role in the delivery of care in the nursing home industry, which is a sector of growing importance as the population ages. This observation is consistent with evidence from Hackmann (2015), who finds that nursing home residents value the number of skilled nurses per resident. Using data from Pennsylvania, Hackmann (2015) estimates that residents jointly value an additional skilled nurse by about \$126,000 per year. Assuming that dying residents lose only one year of the residual life time and that residents only value life expectancy, we find an upper bound on the value of the last year of a nursing home resident of about \$126,000*1,200/1,700=\$89,000, which is in the ballpark of estimates from the literature. Cutler et al. (1997), for example, find a "quality-adjusted-life-year" (QALY) factor of 0.62 for an 85 year old person in 1990. This suggests a value of a year of life of about \$62,000 for an 85 year old based on a value of \$100,000 in the best possible health state.

6 Conclusion

Nurses make up the largest health profession in most OECD countries and play a critical role in the delivery of health care services in hospital, primary, and nursing home. At the same time, there is a growing concern about potential nurse shortages in several high-income countries as the population and the nursing workforce ages. Understanding how nurse shortages affect the delivery of care across markets and health care sectors is therefore of high policy relevance as countries consider different policies to address the growing demand for this profession.

In this paper, we take advantage of a parental leave program in Denmark that led to a sizable reduction in the number of employed nurses, to provide new evidence on this important question. Using administrative employer-employee match data from Denmark, we find substantial take-up rates of the program for both nurses and nursing assistants. For example, the fraction of leave takers among previously employed nurses with 0-1 year old children increases by 15 to 20 percentage points following the introduction of the parental leave program. Despite the substantial program take up rates for both nurses and nursing assistants, the net employment effects for health care providers are very different. We do not find evidence for a meaningful change in net employment for nursing assistants in hospitals or nursing homes. Health care providers simply replace leaving nursing assistants by hiring additional nursing assistants from other occupations and by hiring newly trained nursing assistants. This is not true for nurses. The aggregate data indicate a substantial decline in nurse employment in hospitals and nursing homes. We estimate that the parental leave program effectively reduced the stock of working nurses in these health sectors by 11% and conclude that health care providers are unable to find replacements for nurses on parental leave. In fact, our estimated changes in net employment exceed the predicted reform effects that would occur if employees were to take an entire year of absence and employers were not able to replace any leaver on net.

Building on the substantial net reduction in nurse employment, we investigate the effects on mortality. We find that the parental leave program on average increases nursing home mortality by 12.5% among the population aged 65 and older and by 12.8% for the population aged 85 and older. We find some evidence for lower mortality rates in hospitals that suggest patient reallocation from hospitals to nursing homes in response to higher reform exposure. Yet the increase in nursing home mortality is not purely driven by a reallocation of patients. Instead, we show that the increase in nursing home mortality for age 85 and older maps one-to-one into an increase in total mortality, while leaving hospital mortality unaffected. Our findings suggest that staffing needs for skilled nurses are a particular concern in nursing homes and of growing importance as the population ages and the demand for long term care services increases.

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Appendix

Figure 13: Counties of Denmark 1970-2006

	Description	Our Code
1+2	Copenhagen and Frederiksberg Municipalities	CPH-FR Munic
3	Copenhagen County	KH
4	Frederiksborg County	FR
5	Roskilde County	RS
6	West Zealand County	VS
7	Storstrom County	ST
8	Funen County	FY
9	South Jutland County	SJ
10	Ribe County	RB
11	Vejle County	VJ
12	Ringkjobing County	RK
13	Viborg County	VB
14	North Jutland County	NJ
15	Aarhus County	AR
16	Bornholm	BO

Figure 14: Duration of Leave Benefits: Education and Parental Leave

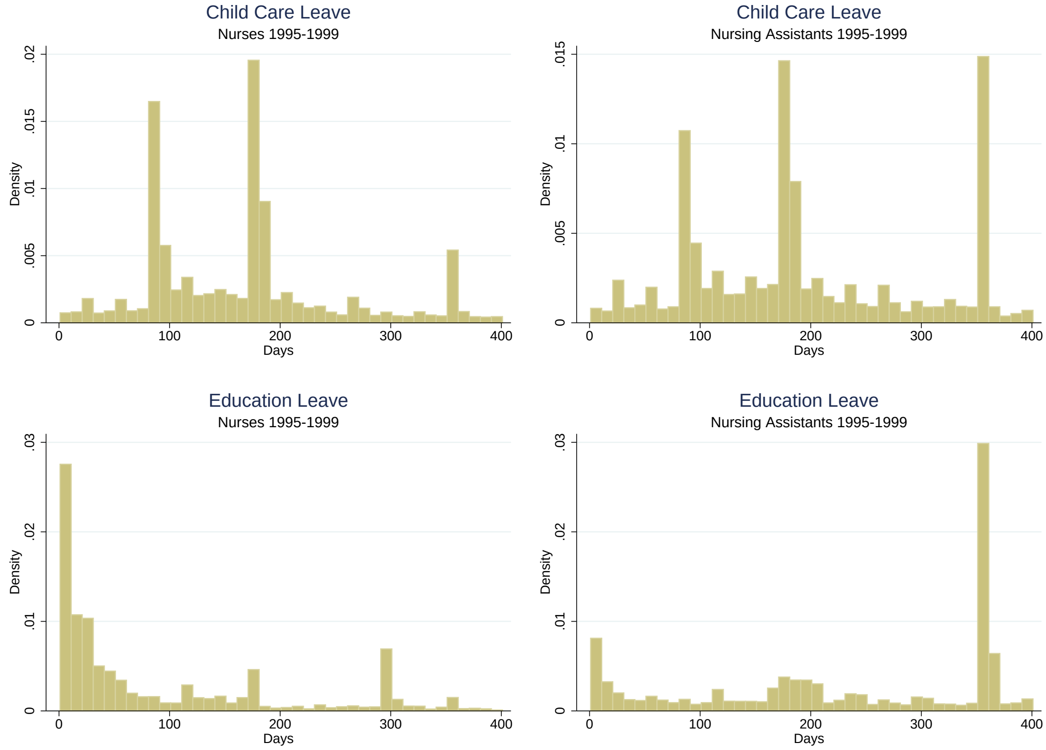

Figure 15: Timing of Parental Leave Taking

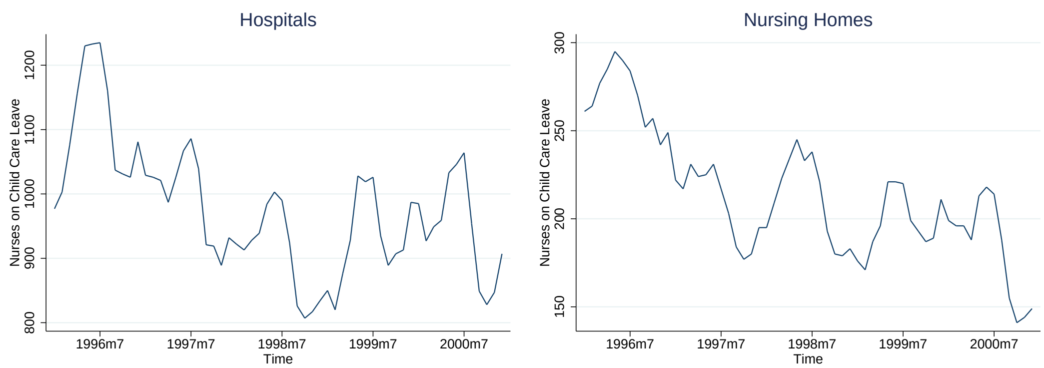

Table 11: Leave Program Take-Up

Number of full-time equivalent leavers among nurses			
	Parental leave	Education leave	Sabbatical leave
1995	2108	516	129
1996	1606	575	<10
1997	1296	517	<10
1998	1114	317	<5
1999	1133	282	<5
2000	1130	205	<5

Table 12: Regression Results for Employment

	(1) Total	(2) Hosp and NH	(3) Hosp	(4) NH
λ_{1990}	-3.322**	-0.820	-2.833	-5.699
	[-5.811,-0.834]	[-6.417,4.776]	[-10.10,4.438]	[-16.19,4.787]
λ_{1991}	-1.822**	-2.098*	-2.113	-5.395
	[-3.535,-0.110]	[-4.496,0.300]	[-6.644,2.419]	[-12.67,1.875]
λ_{1992}	-0.393	-1.152*	-0.869	-3.023
	[-1.635,0.848]	[-2.404,0.100]	[-3.035,1.296]	[-8.467,2.421]
λ_{1994}	-2.525***	-3.808***	-3.131	-4.881**
	[-4.263,-0.786]	[-5.816,-1.801]	[-7.220,0.959]	[-9.058,-0.704]
λ_{1995}	-4.963*	-5.599**	-5.538*	-4.929**
	[-10.69,0.768]	[-10.28,-0.920]	[-11.89,0.812]	[-9.835,-0.0240]
λ_{1996}	-6.094**	-5.658**	-4.935	-4.145
	[-12.16,-0.0279]	[-9.764,-1.553]	[-11.48,1.614]	[-10.16,1.873]
λ_{1997}	-5.429	-5.745**	-3.993	-6.117
	[-14.17,3.308]	[-11.48,-0.00469]	[-12.37,4.388]	[-14.02,1.784]
λ_{1998}	-5.095	-3.450	-2.064	-4.647
	[-13.90,3.712]	[-9.854,2.953]	[-11.90,7.775]	[-13.04,3.744]
λ_{1999}	-4.972	-4.138	-2.632	-4.936
	[-14.34,4.395]	[-11.24,2.961]	[-13.11,7.849]	[-16.01,6.141]
λ_{2000}	-8.304*	-5.868	-5.220	-3.692
	[-18.17,1.558]	[-13.36,1.624]	[-16.55,6.108]	[-16.70,9.313]
Controls				
N	165	165	165	165

95% confidence intervals in brackets

Note: Standard errors are clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Regression Results for Mortality: Age 65 and older

	(1) Tot	(2) Tot	(3) Hosp	(4) Hosp	(5) NH	(6) NH
λ_{1991}	-0.30*	-0.25	-0.098	-0.12	-0.013	-0.042
	[-0.65,0.046]	[-0.64,0.14]	[-0.28,0.088]	[-0.31,0.071]	[-0.12,0.090]	[-0.16,0.075]
λ_{1992}	-0.27	-0.29**	-0.061	-0.095	-0.00054	-0.022
	[-0.60,0.070]	[-0.53,-0.043]	[-0.31,0.18]	[-0.29,0.10]	[-0.17,0.17]	[-0.19,0.15]
λ_{1994}	-0.19	-0.18	-0.099	-0.13	0.092**	0.099**
	[-0.57,0.19]	[-0.54,0.17]	[-0.28,0.079]	[-0.31,0.048]	[0.0056,0.18]	[0.023,0.18]
λ_{1995}	-0.22*	-0.23**	-0.17**	-0.18**	0.12**	0.13**
	[-0.46,0.028]	[-0.42,-0.044]	[-0.32,-0.021]	[-0.34,-0.028]	[0.017,0.23]	[0.031,0.24]
λ_{1996}	0.053	0.079	-0.092	-0.085	0.18**	0.20***
	[-0.34,0.45]	[-0.31,0.47]	[-0.30,0.12]	[-0.28,0.11]	[0.046,0.31]	[0.069,0.33]
λ_{1997}	-0.40**	-0.37***	-0.15*	-0.20**	0.029	0.064
	[-0.71,-0.093]	[-0.62,-0.12]	[-0.32,0.020]	[-0.34,-0.052]	[-0.084,0.14]	[-0.053,0.18]
λ_{1998}	-0.14	-0.16	0.017	-0.039	0.11	0.15*
	[-0.66,0.38]	[-0.61,0.28]	[-0.22,0.26]	[-0.23,0.15]	[-0.052,0.28]	[-0.012,0.31]
λ_{1999}	-0.25	-0.26	-0.073	-0.17*	0.0055	0.050
	[-0.69,0.19]	[-0.67,0.15]	[-0.31,0.16]	[-0.35,0.0062]	[-0.18,0.19]	[-0.14,0.24]
λ_{2000}	-0.38*	-0.35*	-0.0059	-0.076	0.049	0.094
	[-0.82,0.057]	[-0.74,0.040]	[-0.29,0.28]	[-0.31,0.16]	[-0.11,0.21]	[-0.069,0.26]
Controls	✗	✓	✗	✓	✗	✓
N	150	150	150	150	150	150

95% confidence intervals in brackets

Note: Standard errors are clustered at the county level. In columns 2, 4, and 6 we added further controls including previous hospitalizations and age-gender fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Mortality by Nursing Home Exposure: Age 85 and older

	(1) Tot	(2) Tot	(3) Hosp	(4) Hosp	(5) NH	(6) NH
λ_{1991}	-0.084	-0.38	-0.10	-0.30	-0.00044	-0.080
	[-0.86,0.69]	[-1.24,0.49]	[-0.70,0.49]	[-0.80,0.20]	[-0.59,0.59]	[-0.71,0.56]
λ_{1992}	0.47	0.24	0.12	-0.038	0.10	0.049
	[-0.67,1.61]	[-0.82,1.31]	[-0.59,0.83]	[-0.63,0.55]	[-0.88,1.09]	[-0.91,1.01]
λ_{1994}	0.55	0.56	-0.22	-0.21	0.66***	0.64***
	[-0.47,1.57]	[-0.31,1.44]	[-0.77,0.34]	[-0.66,0.24]	[0.24,1.08]	[0.25,1.04]
λ_{1995}	0.45	0.52	0.0038	0.040	0.48*	0.49*
	[-0.37,1.26]	[-0.22,1.26]	[-0.27,0.28]	[-0.17,0.25]	[-0.067,1.03]	[-0.072,1.05]
λ_{1996}	0.90**	0.90**	-0.017	-0.069	0.90**	0.90**
	[0.051,1.76]	[0.13,1.68]	[-0.39,0.36]	[-0.36,0.22]	[0.24,1.56]	[0.23,1.58]
λ_{1997}	0.062	0.095	0.14	0.11	0.23	0.24
	[-0.65,0.78]	[-0.60,0.79]	[-0.42,0.70]	[-0.45,0.66]	[-0.46,0.92]	[-0.50,0.97]
λ_{1998}	0.98**	0.85**	0.52***	0.36**	0.54	0.51
	[0.16,1.80]	[0.032,1.66]	[0.16,0.89]	[0.053,0.68]	[-0.15,1.23]	[-0.18,1.20]
λ_{1999}	0.20	0.077	0.45	0.27	-0.047	-0.048
	[-0.57,0.97]	[-0.57,0.72]	[-0.25,1.14]	[-0.082,0.62]	[-0.65,0.56]	[-0.81,0.71]
λ_{2000}	0.51	0.18	0.44	0.11	0.20	0.17
	[-0.62,1.63]	[-0.72,1.07]	[-0.12,1.00]	[-0.10,0.31]	[-0.50,0.91]	[-0.59,0.93]
Controls	✗	✓	✗	✓	✗	✓
N	150	150	150	150	150	150

95% confidence intervals in brackets

Note: Standard errors are clustered at the county level. In columns 2, 4, and 6 we added further controls including previous hospitalizations and age-gender fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$