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Formal care of the elderly and health outcomes among adult daughters

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Abstract

Health-care expenditures and the demand for caregiving are increasing concerns for policy makers. Although informal care to a certain extent may substitute for costly formal care, providing informal care may come at a cost to caregivers in terms of their own health. However, evidence of causal effects of care responsibilities on health is limited, especially for long-term outcomes. In this paper, we estimate short and long-term effects of a formal care expansion for the elderly on the health of their middle-aged daughters. We exploit a reform in the federal funding of formal care for Norwegian municipalities that caused a greater expansion of home care provision in municipalities that initially had lower coverage rates. We find that expanding formal care reduced sickness absence in the short run, primarily due to reduced absences related to musculoskeletal and psychological disorders. The reduction in sickness absence is concentrated among workers with little work flexibility (e.g., shift workers), whereas there are no effects for workers with more flexible jobs. We were unable to detect effects on long-term health and healthcare utilization outcomes. Our results imply that sickness absence uptake could be hiding a need for more flexibility around work for people with caregiving responsibilities.

KEYWORDS
formal and informal eldercare, health, health care utilization, sickness absence

JEL CLASSIFICATION
I10, J14, J22, J38

1 | INTRODUCTION

With an aging population, the demand for care along with increasing health-care expenditures place an increasing burden on public finances. Informal care may substitute and complement publicly provided formal care (Bolin et al., 2008; Bonsang, 2009; Van Houtven & Norton, 2004). Therefore, informal caregivers constitute a critical national resource. Even in Norway—a country with a large public sector and one of the highest spending levels on publicly provided eldercare in Europe (Huseby & Paulsen, 2009)—the amount of man hours of informal care provided is estimated to be of the same magnitude as that of publicly provided formal care (Berge et al., 2014). Nevertheless, informal care may come at a cost for caregivers; care responsibilities can be a stressor as they often come in addition to other obligations such as work or household chores, or they can have

direct mental or physical health implications for the caregiver (Bauer & Sousa-Poza, 2015; Pinquart & Sörensen, 2003, 2007). Caregiving could also supersede labor market earnings and opportunities.

While taking care of a frail parent can be rewarding (Toljamo et al., 2012), its physical and psychological demands may have negative effects on health in both the short and long run (Capistrant et al., 2012; Kivimäki & Steptoe, 2018; McEwen, 1998; Pinquart & Sörensen, 2006; Von Känel et al., 2008). Caregivers tend to neglect physical exercise, eating healthy, or other practices associated with a healthy lifestyle (Bauer & Sousa-Poza, 2015; Capistrant et al., 2012). Care responsibilities may also have direct and immediate impacts on physical health through physically demanding tasks (Bauer & Sousa-Poza, 2015; Pinquart & Sörensen, 2006). Having caregiving responsibilities has been used as a model for studying chronic stress (Schulz & Sherwood, 2008). Living with elevated stress levels or poor mental health over time has, in turn, been linked to reduced physical health manifesting as, for example, cardiovascular disease, hypertension, lung ailments, accidents, and suicide (Capistrant et al., 2012; Kivimäki & Steptoe, 2018; McEwen, 1998; Pinquart & Sörensen, 2006; Von Känel et al., 2008).

Estimating causal effects of reduced care responsibilities on health is difficult for several reasons. First, formal care uptake is endogenous. Elderly individuals who receive formal care typically have a higher need for care. They may be older and less healthy than those not receiving formal care. Health has a genetic component, as well as a strong connection to lifestyle which may also be inherited. Therefore, simply comparing children with different care responsibilities for their elderly parents may lead to biased results. Second, caregiving may also have endogenous uptake. Individuals with poor relations to the labor market and lower opportunity costs are more likely to provide care. For instance, Mentzakis et al. (2009) find a negative correlation between health and the likelihood of providing care, and further, being employed is found to reduce the willingness to provide care (Carmichael et al., 2010). Therefore, poor health could be a cause of being a caregiver, and comparing health outcomes for caregivers to non-caregivers likely provides biased results. Finally, poor relations to the labor market may also be related to poor health in itself, which may further bias results obtained by comparing individuals with different levels of care responsibilities.

Løken et al. (2017) avoid the endogeneity problem by exploiting a reform introduced in 1998 which aimed at equalizing the availability of care services across Norwegian municipalities. This reform led to an arguably exogenous regional variation in the expansion of formal care services, enabling the estimation of causal effects of increased formal care by comparing outcomes across municipalities with different levels of formal care expansions. They assess the sub-sample most likely to be affected by the reform: daughters with no siblings and only one remaining parent who is at least 80 years old, and estimate causal effects of expanding publicly provided eldercare on various labor market outcomes, including sickness absence. They find that increasing formal care leads to reduced sickness absence in the short run.

Our study builds on the finding by Løken et al. (2017). Reduced sickness absence is beneficial on its own. The cost of sickness absence is about 68 billion NOK (7 billion USD) each year, and even small reductions in sickness absences can lead to considerable savings. If reduced sickness absence implies improved health for the individual, the benefits of increasing formal care could be substantial; improved health has intrinsic value for the caregivers, and it can lead to reduced health care utilization long term, which further increases savings.

However, the decline in sickness absences could also be driven by a reduced need for work absences due to lower care obligations. Sickness absence has been shown to be used as a means to insure employees against a broad range of circumstances, besides own illness (Carlsen, 2008; Gautun, 2008; Markussen et al., 2011), and employees with care responsibilities are more likely to prefer reduced or flexible working hours compared to other workers (Gautun & Hagen, 2010). In this analysis, we explore whether increased formal care has an effect on caregiver's health or if the result by Løken et al. (2017) can be explained by time constraints and a reduced need for work flexibility. Specifically, we apply difference-in-differences models and compare short and long-term health and healthcare utilization outcomes for daughters with parents living in municipalities with different levels of formal care expansions.

Our study differs from Løken et al. (2017) in several regards. First, we assess long term effects. There is a scarcity of studies estimating the long-term effects of caregiving on health (Leigh, 2010; Schmitz & Westphal, 2017; Stöckel & Bom, 2022). Second, while Løken et al. (2017) only had access to sickness absence spells lasting longer than 16 days, our study includes all certified absence spells in Norway. This is important because almost half of the certified sickness absence spells are shorter than 16 days. Third, we exploit access to novel register data that includes the reason (diagnoses) for all certified sickness absence in Norway. These data allow us to study what types of absence that decline when formal care increases, and thus whether reduced care obligations only affects health and/or if it also reduces the need for work flexibility. Fourth, we compare outcomes across workers with different levels of flexibility regarding work time and place (e.g., shift-workers compared to office workers), and those with versus those without small children (and thus care obligations) at home. If effects emerge primarily for the more time constrained groups, this supports the interpretation that the decrease in sickness absences is not driven by health alone. Fifth, we explore health effects for the whole population and not only for the employed. An important group of informal caregivers are those with no or weak connections to the labor market. Studies of sickness absence, such as Løken et al. (2017), can only

assess health effects for those who are employed. In our study of long- term outcomes, we analyze health effects for the whole population, including the non-employed.

This study contributes to a meager literature on the causal effects of caregiving on health, especially by exploring longer-term effects and using population-based register data. The majority of the existing literature documents negative correlations between health and caregiving, and focuses on psychological aspects, such as stress and depression (see Bauer and Sousa-Poza (2015) and Pinquart and Sörensen (2003, 2007) for reviews). A few studies account for the endogeneity of providing care, most of which use propensity score matching (see e.g., Schmitz and Westphal (2015)) or instrumental variables (see e.g., Coe and Van Houtven (2009); Do et al. (2015)), and most are based on survey data. In general, these studies find negative impacts on mental health and mixed results on physical health (see Bom et al. (2019) for a review). Several authors argue that because it takes time for the health effects to manifest, it is critical to assess long-term outcomes (Coe & Van Houtven, 2009; Leigh, 2010; Schmitz & Westphal, 2017). Yet, to our knowledge, only three studies have studied such long term effects. Schmitz and Westphal (2017) and Stöckel and Bom (2022) find lasting negative effects for up to 3–5 years, whereas De Zwart et al. (2017) found short term effects that did not last. All three studies analyzed health outcomes in the mental health domain and relied on propensity score matching and instrumental variables to identify the causal effects.

We start by showing that increased formal home-based care leads to reduced insured sickness absence from work in the short-run and that musculoskeletal and psychological disorders are the main drivers of this reduction. This is an interesting finding because both have been shown to be among the disorders or health problems most strongly correlated with caregiving (Bauer & Sousa-Poza, 2015; Pinquart & Sörensen, 2006, 2007). At the same time, these diagnoses are particularly difficult to verify and can therefore be expected for absence due to time constraints, difficulties with everyday chores or exhausted care capacity. We go on to show that increased formal care has no impact on the sickness absence for people in jobs that are flexible with regards to place and time, whereas for those in comparably non-flexible jobs, that is, jobs where you have to be present at a specific time and place to conduct your work tasks, we find substantial effects. Our findings indicate that the reduction in sickness absence is caused by a reduced need for time off work, rather than improved health alone. We found no differences in effects across those with or without children, with our without a partner/spouse or across levels of education. Finally, we were not able to detect any long term health effects for the caregivers, which is in line with our finding that time and flexibility are important drivers for why sickness absence are reduced when formal care increases.

The paper proceeds as follows. In Section 2, we present details on elder care in Norway and the reform. Section 3 describes the different sources of data, and Section 4 explains the empirical strategy. In Section 5, we present the results, and in Section 6 we discuss our findings. Section 7 concludes.

2 | BACKGROUND

2.1 | Norwegian caregivers

Norway is fairly comparable to other OECD countries with regards to the fraction of the population that provides informal care. The fraction of the adult population that reports to *provide informal care regularly* in Norway is 14%.[1] The average across OECD countries providing informal care *at least weekly* is 13% for people aged 50 and over and among the countries for which data exist (OECD, 2021). While the numbers are very similar, they are not perfectly comparable. In addition to the difference in the framing of the question (written in italics above), the OECD numbers are based on an older population (50+). Denmark and Sweden, countries that are similar to Norway in several regards, are also close to the OECD average, which strengthens our belief that the fraction of informal caregivers is comparable.

2.2 | The 1998 reform

We exploit variation in the expansion of formal elder care caused by the action plan for elder care services implemented in 1998.[2] This action plan was implemented as a response to declining coverage rates of home-based and institutionalized care for the elderly caused by a rapidly growing elderly population during the 1990s, and to the large differences in home-based care coverage rates across municipalities.[3] These differences were seen as inequitable, and they were a consequence of shifting the responsibility for eldercare services from the central authorities to the municipalities starting in the 1980s. This shift implied that earmarked grants for elder care were replaced by transfers to municipal budgets based on demographics, income, and

estimated needs in the municipality. Consequently, municipalities were free to allocate their budgets with close to full discretion, resulting in large differences in the coverage rates of home-care services across municipalities.

The growing inequalities between municipalities led to the implementation of the action plan for eldercare. The plan was implemented in January 1998, with a 4-year implementation period. The main purpose of the action plan was to increase capacity in adapted buildings and personnel, and to create a more equitable level of care services across municipalities (Brevik, 2001).[4] Explicit goals of the action plan included that all municipalities should be able to provide 24/7 assistance to at least 25% of their population aged 80 and older. Specifically, the plan aimed to increase (1) the number of spaces in adapted apartments and institutions and (2) the labor input in the home-based care sector nationwide by 6000 work years (Borge & Haraldsvik, 2006).

There was a strong desire to preserve the autonomy of the care recipients, and most of the expansion in care services took the form of home-care in adapted facilities, rather than nursing homes. This also had cost advantages compared to institutionalized services, as in the latter there is 24/7 access to highly qualified personnel (Løken et al., 2017). The plan was implemented by the municipalities through federal grants. Although all municipalities in principle could apply for the grants, evidence suggests that the municipalities with the lowest home-based care coverage rates were more likely to apply (Borge & Haraldsvik, 2006). We confirm this in a regression of growth in home-based care coverage rates on municipality characteristics in Table 1. We show that municipalities with the lowest pre-reform home-based coverage rates experienced the largest post-reform increase in home-based care coverage. This regression also confirms that there was no relation between the increase in coverage rates and pre-reform institutionalized care coverage, municipal budget or the share of the population aged 80 or older or aged 67 or older, respectively.

In Figure 1, we show that the levels of home-based care coverage rates are converging and almost leveling off in the post-reform period. There is a slight convergence across the municipalities also in the years leading up to the reform, however, there is a substantial increase in home based care in municipalities with low-pre reform coverage rates relative to municipalities with higher coverage rates after the implementation period. During this period, there was an increase in the fraction of the population 80+ in all municipalities (see Figure A1). This implies that, in absence of the reform, there could have been a reduction in coverage rates in the treatment municipalities, as is seen in the control municipalities. The lower graph in Figure 1 shows that the level of pre-reform coverage rates of home-based care is a strong predictor of absolute change in coverage rates from the pre-reform period to the post reform period.

In Figure A1, the top graph shows that the coverage level for institutionalized care remained fairly constant across the time period. The same holds for the fraction of the population aged 80 and older in the bottom graph. This is important because we might worry that the change in home-based care coverage rates was offset by changes in institutionalized care, or that individuals who are in need of care might move to municipalities that apply for grants to improve care provision. Additionally, we compare pre-reform characteristics across municipalities with high and low pre-reform coverage rates in Table A1, and conclude that the municipalities are similar on most measures. One notable difference is the tendency that municipalities with lower pre-reform coverage have larger populations.

While descriptive results discussed above confirm the government's stated strategy of emphasizing home-based care compared to nursing homes in combating coverage discrepancies across municipalities (Daatland & Veenstra, 2012), Figure 1

TABLE 1 Post-reform growth in home care coverage rates

	Absolute change in home care coverage rates (post - pre)	
	(1)	**(2)**
(−) Homecare coverage rate, pop. 80+ (1993–1997) (scaled by 10)	0.068*** (0.0070)	0.078*** (0.0042)
(−) Inst. based care coverage rate, pop. 80+ (1993–1997) (scaled by 10)	0.001 (0.0075)	0.003 (0.0069)
Share of pop 67 years+	0.070 (0.1999)	0.032 (0.2013)
Share of pop 80 years+	−0.056 (0.5544)	0.040 (0.5572)
Munic. unrestricted budget per capita	0.000 (0.0000)	−0.000 (0.0000)
Exclude extremes	Yes	No
Observations	347	435

Note: The (−) in front of homebased and institution based care means that these controls enter with negative values. Hence, the coefficients for these controls are interpreted as, for example, in col. 2, that a 10 percentage points *lower* home care coverage rate in the pre-reform period is related to a 7.8 percent increase in the coverage rate from the pre- to the post-reform period. The pre-reform period is defined as 1993–1996, while the post-reform period is defined as 2001–2003 (we define the period 1998–2000 as a phase-in period). Extremes are defined as municipalities with pre-reform home care coverage rates falling below the 10th or above the 90th percentiles in the pre-reform homecare coverage distribution. Robust standard errors in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

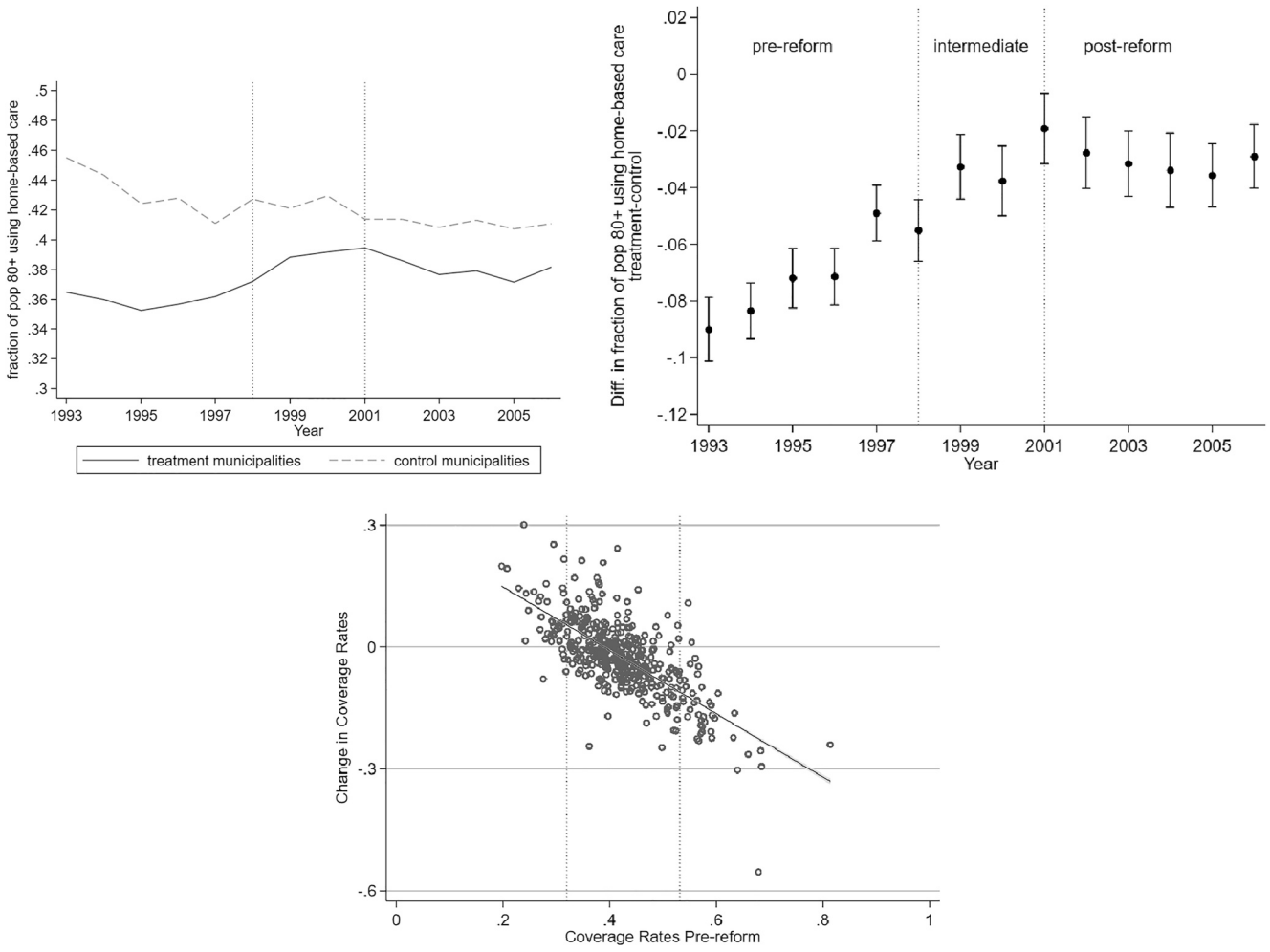

FIGURE 1 Home Based Care—Coverage Rates. Home based care is defined as care at home or in adapted facilities. In the first figure, treatment is defined as falling below the median home based care coverage rate in the pre-reform period, defined as 1993–1996. The dotted vertical lines represent the reform year 1998, and the year 2001. The period 1998–2000 represents the phase-in period. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded. The top left figure graphs the fraction of the 80+ population using home based care in treated municipalities (solid line) and control municipalities (dashed line) over the period 1993–2005. The top right figure shows the difference per year in the fraction of the 80+ population using home based care in treated versus control municipalities. The lower figure graphs the absolute change in coverage rates against the pre-reform coverage rates. In this figure, the dotted vertical lines indicate the 10th and 90th percentiles. Municipality list from the year 2000 ($N = 435$).

captures only the aspect of the reform associated with increasing the proportion of elderly receiving care. The reform may have affected other aspects, such as improved quality of the care provided or more hours of care for those already receiving formal care.[5] The reduced-form effects of the care expansion are thus likely to work through all of these channels.

3 | DATA AND SAMPLE

3.1 | Sample

Much of the care responsibility falls on close relatives, especially on the spouse or children (Jakobsson et al., 2012; Vaage, 2000). Studies using time use surveys show that spending time caring for elderly parents is common (Vaage, 2000), and that employees with caretaking responsibilities are more likely to prefer reduced or flexible working hours (Gautun & Hagen, 2010). Female spouses and daughters are found to provide more care than male spouses or sons do (Jakobsson et al., 2016; Stark, 2005), and two explanations for these gender differences are given in the literature. The first states that women may have lower opportunity costs and/or a weaker connection to the labor market than men. The second explanation relates to the way formal care services

are allocated. Stark (2005) and Jakobsson et al. (2016) note that public service managers often have gender-differentiated expectations for the amount of informal care that will be provided. In particular, informal care from daughters is seen as a closer substitute for formal care than informal care from sons. We therefore assess the health effects of informal caregiving among daughters. Specifically, we assess the effects of increased formal elder care provision on short and long-term health and healthcare utilization outcomes for daughters of elderly, single parents.

We define the baseline sample as adult daughters with no siblings and only one surviving parent aged 80 years or older in each year of the time period 1995–2003.[6] This particular sample of daughters is a group expected to have a greater care burden for an elderly parent, and thus, the group most likely to experience a reduction in care responsibilities caused by the reform.[7] The sample must be seen as an unbalanced panel where new daughters enter every year as the remaining parent turns 80 years old, or as a parent becomes a widow(er), provided he or she is 80 or older. Similarly, some daughters drop out of the sample due to the death of the remaining parent. The full sample of daughters with one parent still alive is 96,996 individuals. When we restrict the sample to daughters without siblings, the sample is reduced to 18,261 individuals.

We link the sample of daughters to a number of different background characteristics and outcomes using each person's unique identifier number and the following registers: the population register which contain date of birth, municipality of residence (by year) and the unique identifier number for their parents, the education database, taxable income register, employment register, register on uptake of welfare (FD-trygd), data on sickness absence and related diagnoses from the Norwegian Labor and Welfare Service (NAV), and health records from the Control and Distribution of Health Reimbursement database (KUHR).[8] In addition, using the municipality of residence, we merge municipality level data from the regional database provided by the Norwegian Social Science Data (NSD). The different data sources and outcomes are presented in more detail in the next section.

3.2 | Individual-level data

We apply rich, individual-level register data provided by Statistics Norway (SSB) covering the entire resident population over the period 1993–2014. These registers include demographic information, such as year of birth, gender, immigration status, municipality of residence, and socioeconomic data, such as education and earnings. In addition, we have information on every individual's income and use of different welfare benefits, including disability insurance. Using unique identifiers, we are able to link siblings and parents to their children, and to link individuals to data from relevant health registers, explained in more detail below.

3.3 | Sickness absence and related diagnoses

Following the National Insurance Act (Folketrygdloven, 1997), all employees are entitled to sickness benefits to compensate for the loss of labor income due to illness or injury. To assess the impact of caregiving on sickness absence and the underlying diagnoses, we apply records of leaves of absence reported to the Norwegian Labor and Welfare Service (NAV). These data contain every sickness absence spell certified by a physician and their related diagnoses from 1995 to 2014. The absence spell is recorded from the first day of a certified absence.[9] A physician's certification is required for all sickness absence spells lasting longer than the allowed number of self-reported days.[10] As a general rule, workers are entitled to at least three self-reported absence days per leave, but in some workplaces workers are entitled to up to eight days.[11] Each sickness absence spell can last for a maximum of 12 months with full wage compensation up to a certain ceiling. Our measure of sickness absence is a binary indicator equal to one if a person has at least one spell of certified absence during the year. We condition sickness absence on employment, which means we drop individuals who are not eligible for reimbursed sickness absence.[12] Being employed is defined as having labor income exceeding the basic amount (G). The G levels are administratively set and adjusted each quarter of the year, and used in the national social security system to determine old-age pensions and eligibility for disability and unemployment benefits.[13]

All absence certifications should be followed by at least one diagnosis reported by the physician who issued the certificate.[14] In addition to the indicator for any sickness absence, we construct separate indicators for absences related to the two largest diagnoses categories following a sickness absence: namely musculoskeletal disorders (MSD), and psychological disorders.[15] We group all other diagnoses into a rest category, and finally, we keep as a separate category all leaves of absence with a missing diagnosis.

3.4 | Long-term health and health care utilization outcomes

For the long-term health and health care utilization outcomes, we use detailed register data from the Control and Distribution of Health Reimbursement database (KUHR), available for the years 2006–2014. These registers cover all Norwegian citizens' utilization of primary health-care services and contain administrative records of all reimbursements claimed by primary physicians. All Norwegian citizens belong to a specific primary physician's list, and each physician is responsible for providing primary health care services to patients that belong to his or her list. As a requirement in the physician's payment scheme, the physician is obliged to report to the national claims database (KUHR) on all services provided and actions taken after every consultation, including the main symptom or diagnosis, referrals and certification of sick leaves. Physicians are required to include at least one diagnosis code per consultation to be reimbursed for the services provided (Sørensen et al., 2016). Classification of main symptoms and diagnoses follow the International Classification of Primary Care (ICPC-02).

To measure utilization of primary health-care services, we record the yearly likelihood of consulting a primary physician and the number of consultations per year. As primary physician consultations, we include visits to the emergency room and the causality clinics. As discussed in the literature review, caregiving has been linked to a range of health outcomes, as well as to poor lifestyle choices that can result in poor health. We assess the likelihood of having a record of any of the following broad diagnosis categories: MSD, psychological, and cardiovascular disorders. Further, we look at the more specific diagnoses of hypertension and depression. Finally, we measure lifestyle related disorders as having any of the following diagnoses: non-insulin-dependent diabetes (type 2), overweight or obesity, or substance abuse, that being either alcohol, tobacco, medication or drug abuse. The exact construction by ICPC-02 codes that go into each outcome is listed in Table A2 in the appendix.

3.5 | Types of jobs

Types of jobs are coded according to the International Standard of Industrial Classification (ISIC) rev.3. These data are from the employment register, and the ISIC code is locked to the job a person had in 1998. Workers assumed to have little flexibility are those with codes: 0–64 (agriculture, hunting, forestry, fishing; mining and quarrying; manufacturing; electricity, gas, and water supply; construction; wholesale, retail, and repair shops; hotels and restaurants; transport, storage, and communications), 80–89 (education; health and social work), 90 (sewage, refuse disposal, sanitation), 93 (other service activities), and 95 (activities in private households). Workers with more flexibility are those with codes: 65–75 (finance, insurance; real estate, renting, business activities, public administration), 91 (activities of membership organizations), 92 (recreational/cultural activities).

3.6 | Municipality-level data

To obtain information on the type and extent of formal care on the municipality level, we use annual data on elder care from the regional database provided by the Norwegian Social Science Data (NSD). These data provide information on the number of users of different types of elder care in each municipality per year in the time period 1993–2014. Our treatment variable is the fraction of individuals aged 80 and older who receive formal home-based care, that is, home-based care provided by the municipality.[16] From the regional database, we also assess the fraction of individuals aged 80 or older living in care institutions in each municipality each year. We link individuals to municipality data based on their mother's (or the living parent's) municipality of residence in 1993.

4 | EMPIRICAL STRATEGY

To estimate the effects of the expansion in formal care for the elderly on health outcomes for their adult children, we use a reduced form model that exploits the differential increase in the allocation of federal funds across municipalities caused by the 1998 reform. We compare individual outcomes across municipalities that experienced different levels of expansion in formal care. As a predetermined indicator of the intensity of the municipal response to the reform, we use the average level of home care coverage measured before the reform: in the years 1993–1996. As noted in Section 2, municipalities with lower pre-reform coverage rates experienced a larger expansion in home-based care coverage from the pre- to the post-reform period.

We rely on informal care responsibilities being related to the amount of formal care provided in the municipalities. This is supported by for example, Jakobsson et al. (2012).

4.1 | Short-term effects

We split the post-1998 period into a phase-in period, defined as the period 1998–2000, and a post-reform period, defined as 2001–2003. In the phase-in period, we expect smaller effects due to delays in the implementation of the reform, whereas in the post-reform period, funding levels had increased, and home-based care coverage rates had almost converged between treatment and control municipalities. We estimate the following regression[17]:

$$\begin{aligned} Y_{it} &= \alpha_0 + \alpha_1 PreCoverage_i + \alpha_2 Phasein_t + \alpha_3 Post_t + \alpha_4 (PreCoverage_i \times Phasein_t) \\ &+ \alpha_5 (PreCoverage_i \times Post_t) + \mathbf{X}'_{it}\delta + \mu_{it} \end{aligned}, \quad (1)$$

where Y is the outcome of interest at time t for individual i. *Phasein* indicates the transition period, defined as 1998–2000, while *Post* indicates the post-reform period, 2001–2003. *PreCoverage* represents the treatment intensity of the municipality in which the elderly parent of individual i lived in 1993. Finally, X is a set of control variables including municipality fixed effects, child age and education, parent age and gender, and immigrant status for both parent and child.[18]

The coefficients of interest, α_4 and α_5, are interpreted as the intention to treat effect of the policy change in the phase-in period and the post-reform period, respectively. In addition to care expansion at the extensive margin, it is likely that the reform led to care expansion at the intensive margin and to quality improvements, which we are not able to observe. The key identifying assumption is that the change in outcomes from the pre- to post-period would have been the same across municipalities with different levels of pre-reform coverage rates in the absence of the reform. Our approach assumes a linear relationship between pre-reform care coverage rates and the outcomes of interest. As the linearity assumption is not well supported for extreme values of pre-reform coverage levels, we drop municipalities with coverage levels lower than the 10th percentile and higher than the 90th percentile.[19,20]

4.2 | Long term effects

As our data on health care utilization is available only from 2006, there is no meaningful pre-period for the long term outcomes. We therefore take a different approach to estimate the long term effects. We apply the same source of variation in formal care as above–that is, municipalities with lower pre-reform coverage rates experienced a larger increase in formal care compared to those with lower pre-reform coverage rates–but here we also take advantage of the variation across daughters based on when the (lone) parent turns 80. Daughters with parents in municipalities with low-pre reform coverage rates, who had a parent turn 80 before the reform, are assumed to be exposed to less formal care compared to daughters with parents in the same municipality but with a parent that turns 80 after the reform, and also compared to daughters with parents turning 80 in either period in municipalities with high pre-reform coverage rates. We then compare health outcomes 11 years after a parent turned 80 among daughters with parents turning 80 years in different years and across different municipalities.

We estimate the same regression specification as in Equation (1); however, the variables that goes into the equation differ. For the long term outcomes, Y_{it} is health care utilization or diagnosis at time $t + 11$ for individual i. *Phasein* indicates that the parent became 80 years during the transition period, defined as 1998–2000, while *Post* indicates that a parent became 80 years in the post-reform period, 2001–2003. $PreCoverage_i$ represents the treatment intensity of the municipality in which the elderly parent of individual i lived in 1993. Finally, X is a set of control variables including municipality fixed effects, child age and education, parent birth year and gender, and immigrant status for both parent and child.

The coefficients of interest, α_4 and α_5, captures the intention to treat effect of publicly provided formal care on long term health care utilization and the likelihood of specific diagnoses related to caregiving in the long term.

4.3 | Effect heterogeneity

In addition to studying long term effects, the main aim of this paper is to explore whether the reductions in sickness absence can be ascribed only to effects on caregiver's health or if it can result from the need for more flexibility in every day life. We

therefore divide the working daughters into two groups, one with little flexibility with regard to work time and place, such as shift workers, and another that is assumed to have more flexibility in terms of work hours and physical presence, such as office workers. The groups are coded using the ISIC rev.3 described in more detail in Section 3.2.3.[21]

Second, daughters who have children living at home might be particularly vulnerable to time and/or care capacity constraints. We therefore compare outcomes for daughters with children of kindergarten or primary school age (0–15 years in 1998) to daughters without small children.

Finally, when estimating the average effect on the entire group of daughters affected by the expansion, differences in responses across important subgroups may be concealed. Of particular interest in this setting are the differences in response across groups with different SES. We use education level as a proxy for SES, and investigate differences in the response to the expansion of elder care across daughters with high and low education.

We present all heterogeneity results group wise, but we preform a formal test by interacting both ($PreCoverage_i \times Phasein_t$) and ($PreCoverage_i \times Post_t$) in Equation (1) with an indicator equal to 1 if individual i has a non flexible job; has a small child; has a partner; or has less than 10 years of education. All indicators for group are measured pre-reform.

4.4 | Multiple hypothesis testing

When testing a number of outcomes that are potentially correlated and estimated using the same source of variation, we may increase the risk of accepting an incorrect hypothesis. Therefore, we test whether the estimated effects remain statistically significant after correcting for multiple hypothesis testing. For each outcome in our main tables, we include the p-value obtained using the step-wise procedure described in Romano and Wolf (2005a,b, 2016). As outcomes occurring in the same table are conceptually similar, we test the outcomes in the same table simultaneously.

4.5 | Alternative specifications and robustness

To shed light on the robustness of our results, we perform a number of checks. As our first robustness check, we assess the sensitivity of the estimates to various specification checks. We start by excluding large cities and the most rural municipalities that might be very different from the rest of the sample. The large cities and most rural municipalities differ from the majority of municipalities in one regard of particular importance for this study: daughters, even more than sons, are likely to move away from small municipalities with few employment opportunities to larger municipalities with better labor market opportunities, in particular to the big cities. This implies that the caregiving "burden" in these municipalities is likely to be lower than in other municipalities as parents and daughters are less likely to live in close proximity, and therefore attenuate the estimates. Next, we include alternative ways of treating the year 1997 (the year before the reform), first by excluding 1997 altogether and second by including 1997 in the phase-in period. We also include a specification where we split the municipalities into treatment and control groups based on whether the municipalities fall above or below the median in the pre-reform coverage distribution. Finally, we include municipalities with particularly high or low pre-reform coverage rates.

Additionally, we re-estimate the effects using an alternative approach where we exclude the phase-in period and treat all years after the reform as post-years. This means estimating the following equation:

$$Y_{it} = \alpha_0 + \alpha_1 PreCoverage_i + \alpha_3 Post_t + \alpha_4 (PreCoverage_i \times Post_t) + \mathbf{X}'_{it}\delta + \mu_{it} \quad (2)$$

where post is defined as 1998–2003. Excluding the phase-in period gives us more post-reform years; however, it also means that the treatment exposure is smaller, on average, as we include individuals from the phase-in years. Thus, we expect the estimates of α_4 from Equation (2) to be smaller than the estimated post effects, α_5, using Equation (1).

We further estimate the reform effects on alternative samples and run two placebo tests. For the alternative samples we consider all daughters and sons with no siblings. These samples are assumed to have lower care burdens and thus, likely to be less affected by the care expansion. For placebo tests, we estimate the effects on samples we assume to be much less affected by the care expansion. Here, we consider daughters with no siblings where both parents are deceased, as well as daughters with younger parents (age 60–72). Importantly, as we cannot be certain that these daughters do not have care obligations that are also affected by the reform—-they might still care for parents-in-law or other relatives—-they are not perfect placebos.

5 | RESULTS

In the following sections, we present the results for the baseline sample of daughters with no siblings and only one living parent aged 80 or older. The results are presented graphically and in tables with the intention-to-treat (ITT) estimation results. For the graphical presentation we include figures where municipalities are split at the median of the pre-reform coverage rate distribution into a treatment and a comparison group. We start by presenting results for the short-term effects of the coverage expansion on sickness absence and whether the effect on sickness absence is driven by a specific diagnosis group. We then present results from the assessment of heterogeneity across groups, and, finally, we present the long-term health outcomes. The robustness of the results is discussed briefly, but the robustness results are placed in the appendix.

5.1 | Sickness absence and related diagnoses

The results for sickness absence and the diagnoses underlying the absence are presented graphically in Figure 2. The first graph shows the likelihood of having any sort of sickness absence. We see almost overlapping trends prior to the reform for municipalities with pre-reform coverage rates above (control) and below (treatment) the median. The graphs show a clear tendency toward divergence in the post-reform period. Figure 2 further shows the probability of absences related to specific diagnosis groups: MSD, and psychological disorders, a rest category containing all other diagnoses, as well as the group in which no diagnosis is specified. For these specific groups of absence spells, we see a clear tendency toward a reduction in absence related to MSD, and, although less clear, a tendency toward a reduction in spells related to psychological disorders in the treatment municipalities compared to the control municipalities. For the rest category and the spells with no specified diagnosis, there seems to be no effect of the care expansion.

Table 2 provides the corresponding ITT estimates for the sickness absence outcomes, using the specification in Equation (1). The regression results confirm the tendencies observed in the graphs in Figure 2. The phase-in ITT indicates the reform effect in the phase-in period, defined as 1998–2000, while Post-ITT indicates the effect in the post period, defined as 2001–2003, that is, where we expect stronger effects as the expansion of formal home-based care has had time to take effect. The estimated ITT effect on the likelihood of a sickness absence at all (the first column) is statistically significant at the 5% level for both the

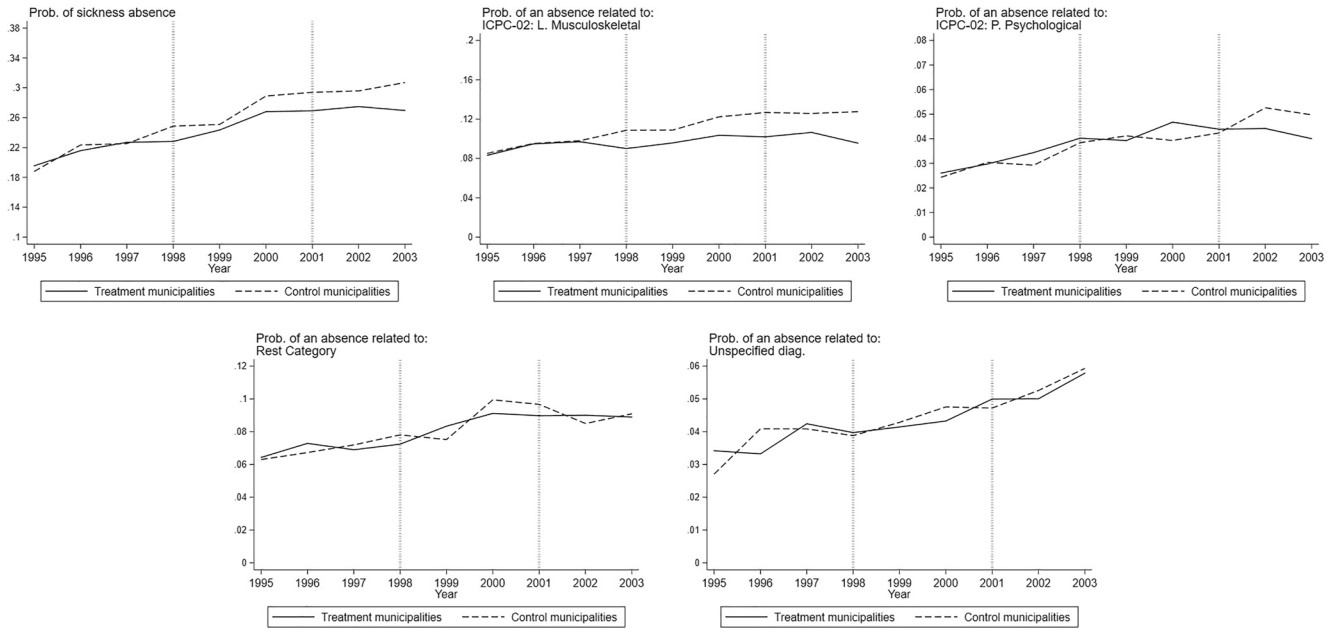

FIGURE 2 Probability of a Sickness Absence, 1995–2003. The graphs show the probability of a sickness absence spell (cond. on employment), and spells related to specific diagnoses for daughters with no siblings and with only one living parent at least 80 years old. Municipalities are split into treatment and control groups based on whether they fall below or above the median home based care coverage rate in the pre-reform period, defined as 1993–1996. The vertical lines represent the reform year 1998, and the year 2001. The period 1998–2000 represents the phase-in period. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded. Municipality list from the year 2000 ($N = 435$).

TABLE 2 Sickness absence—specific diagnosis categories

		Absence with specific diagnosis			
	Sickness absence at all	**Musculoskeletal diag.**	**Psychological diag.**	**All other diag.**	**Unspecified diag.**
Phasein ITT	−0.023**	−0.016*	−0.004	−0.000	−0.004
	(0.0116)	(0.0085)	(0.0040)	(0.0069)	(0.0050)
Post ITT	−0.030**	−0.025***	−0.011***	−0.004	−0.001
	(0.0146)	(0.0090)	(0.0040)	(0.0073)	(0.0048)
Mean	[0.214]	[0.0924]	[0.0295]	[0.0685]	[0.0368]
Romano-Wolf *p*-value	0.096	0.028	0.036	0.857	0.920
Obs.	57,753	57,753	57,753	57,753	57,753

Note: Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The Romano-Wolf *p*-value relates to the Post ITT coefficient, and is obtained using the Romano-Wolf stepwise procedure to correct for multiple hypothesis testing.

phase-in period and the post-reform period. Specifically, we estimate that a 10% point lower pre-reform coverage rate is related to a 14% reduction in the likelihood of a sickness absence among adult single-child daughters (a 3% point reduction from a mean of around 21%).[22] As expected, the estimates are larger for the Post-ITT than for the Phase-in ITT effect.

In the following four columns of Table 2 we investigate whether there are certain types of diagnoses that drive the sickness absence result. We find statistically significant reductions for the post-ITT in the probability of absences related to both MSD and psychological disorders, but no effect on absences related to other diagnoses (the rest category) or the groups of unspecified diagnoses.[23]

As discussed in the literature section, MSD and psychological disorders are health outcomes that are likely to be affected by caregiving. For the probability of having a sickness absence related to MSD, the long-term ITT effects are estimated to be −2.5% points from a mean of 9.2%, which translates into an effect of about 27% reduction in the likelihood of a sickness absence. Similarly, for the probability of an absence related to a psychological condition, our estimates translate into a 37% reduction (a 1.1% points reduction from a mean of 2.95%). Finally, as shown by the Romano-Wolf *p*-values provided in the table, the estimated effects on the probability of an absence at all, as well as the probability of leaves related to MSD and psychological disorders, are still statistically significant at conventional levels when the *p*-values are corrected for multiple hypothesis testing.

We also investigated more specific diagnoses by splitting up the broad MSD and psychological disorders categories into smaller groups but found no clear pattern of the sickness absence reduction being driven by any of these smaller groups. These results are shown in the Appendix Table A3.

5.2 | Heterogeneity across types of jobs, education, and life circumstances

Table 3 present the results for the heterogeneity analysis. The first two columns presents the sickness absence results for jobs with different levels of flexibility. We find no effects for those with flexible jobs, that is, jobs that typically do not require presence on site or strict hours. The point estimate is close to zero and not statistically significant. For those with less job flexibility, however, we find large and significant effects. The reduction in sickness absence in the post period is 3.7% points, which amounts to a reduction of 16%, and significant at the 5% level. The difference in effects between the two job types is statistically significant at the 5% level.

The next two columns present the results for those with and without young children. While there are significant reductions in sickness absence only for the group without children, the point estimate in the post period (Post ITT) is larger for the group with children, and there are no statistically significant differences between the two groups. We find no statistically significant differences between those with and without a partner/spouse or between those with different educational levels. The point estimates are within the same range for all groups.

TABLE 3 Sickness absence—heterogeniety

	Not flexible	Flexible	Child	No child	Partner	No partner	Low education	High education
Phasein ITT	−0.025*	−0.004	0.011	−0.029**	−0.030**	0.002	−0.020	−0.037*
	(0.0139)	(0.0221)	(0.0319)	(0.0124)	(0.0128)	(0.0319)	(0.0136)	(0.0204)
Post ITT	−0.037**	−0.003	−0.039	−0.029*	−0.030	−0.025	−0.035**	−0.020
	(0.0180)	(0.0254)	(0.0325)	(0.0162)	(0.0187)	(0.0276)	(0.0158)	(0.0228)
Mean	[0.229]	[0.214]	[0.179]	[0.217]	[0.205]	[0.237]	[0.22]	[0.195]
P-val difference	0.033	0.033	0.559	0.559	0.218	0.218	0.034	0.034
Obs.	37,777	13,539	10,523	47,230	42,799	14,920	42,064	15,467

Note: Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. The p-value relates to the following interaction term: $(PreCoverage_i \times Post_t)$ in Equation (1) with an indicator equal to 1 if individual i has a non flexible job; has a small child; has a partner; or has less than 10 years of education.

5.3 | Long-term health and health care utilization outcomes

The graphical presentations of results for the long-term health and health care utilization outcomes are presented in Figure 3. As previously explained, for this part of the analysis the health outcomes are measured 11 years ahead. We retain the same baseline sample of adult daughters. However, we no longer need to condition on employment as these health outcomes are also available for those who are not employed.[24] In general, the graphical presentation provides no clear evidence of effects in any direction on the outcomes considered. However, we see some tendencies toward a reduction in the risk of hypertension in the treatment municipalities compared to the control municipalities.

The estimation results for the long-term outcomes are presented in Table 4. The Phase-in and Post-ITT are defined as above. The first column presents the reform effects on the probability of a primary care consultation. The point estimate shows an increase of 1.7% points, which is not statistically significant.[25] In the remaining columns of Table 4, we investigate the likelihood of the occurrence of specific diagnoses identified in the literature as potentially affected by caregiving responsibilities. Overall, we are not able to detect any evidence on long-term health of the care expansion. Specifically, we find no significant effects on the likelihood of the broad groups of MSD, psychological disorders and cardiovascular diseases, or on the more specific categories of depression, hypertension, and lifestyle related diseases.

We assess long-term health outcomes for the same sub groups as in Section 5.2. Results are presented in Table 5. Again, we were not able to find convincing evidence of long-term health being affected by the care expansion.

5.4 | Robustness

As described in Section 4, we perform a wide array of robustness checks and sensitivity analyses. A more detailed presentation of the robustness assessment and results is provided in the appendix. In short, we confirm that the results found in the main analysis are robust to various specifications, and we show that effect sizes differ in the expected direction according to the assumed treatment intensity, that is, when assessing the same outcomes for samples less likely to be affected by the reform, we find smaller or not significant effects. However, it should be noted that the coefficients in the placebo analysis are of the same magnitude as the main analysis, and that some of the effects are no longer significant when including municipalities with particularly high or low pre-reform coverage rates. Finally, we present a simple test of survivorship bias showing that there are no correlation between how long a daughter stays in the sample and pre-reform coverage rates and an interaction between pre-reform coverage rates and education (a proxy for health). These results are presented in appendix Table A10.

6 | DISCUSSION

As shown in this paper, increased formal elder care reduced sickness absence for daughters of elderly single parents.[26] Sickness absence is costly for employers and for public finances. Increasing formal care can therefore lead to savings.[27] In this paper we show that the reform in question led to a reduction in all certified sickness absence of 14%, 3% points. In order to translate

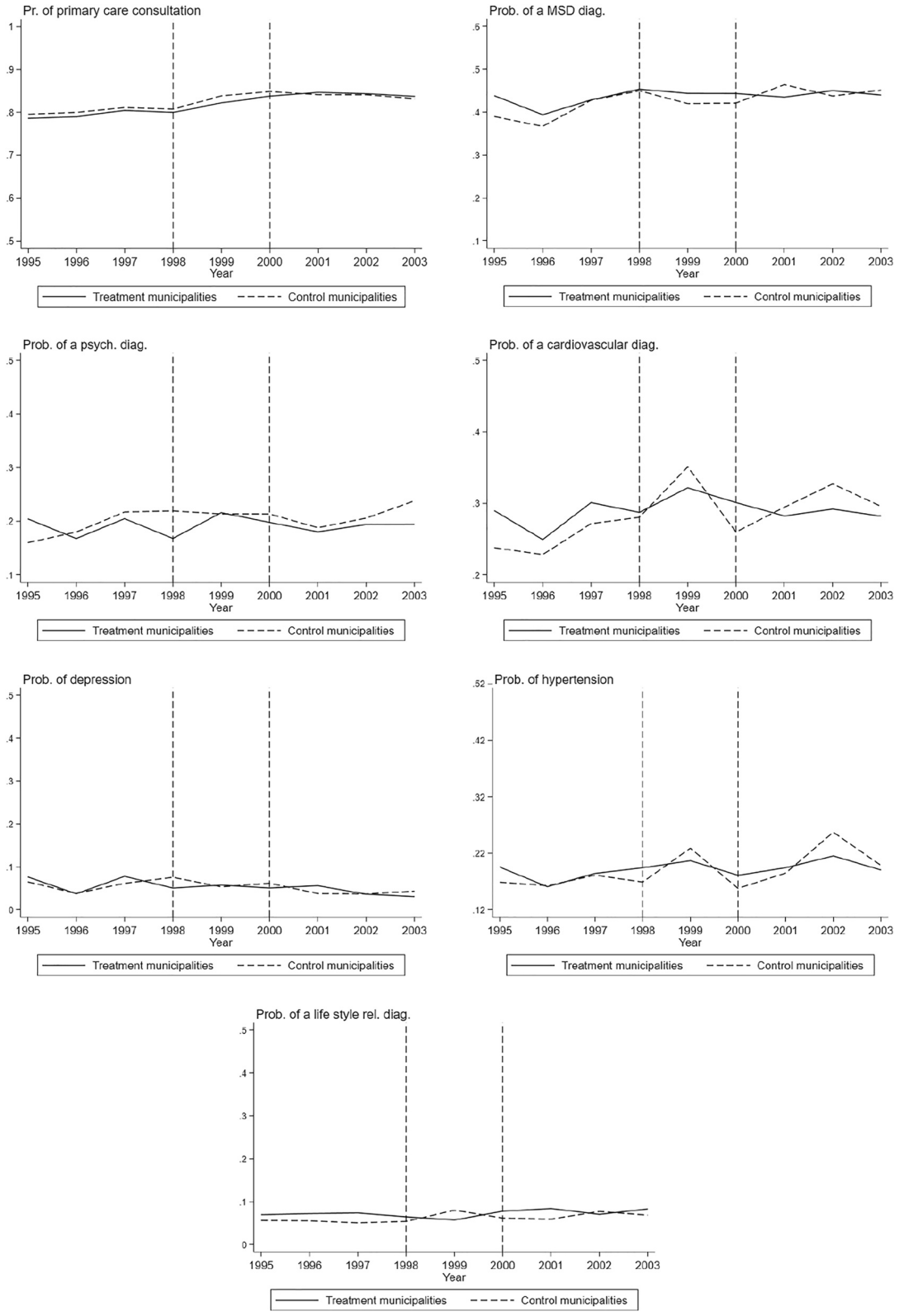

FIGURE 3 Long-Term Health Outcomes: Utilization of Primary Health Care Services. The graphs show the probability of a primary care consultation and consultations related to a specific diagnosis (lagged by 11 years) for daughters with no siblings and with only one living parent at least 80 years old. Municipalities are split into treatment and control groups based on whether they fall below or above the median home based care coverage rate in the pre-reform period, defined as 1993–1996. The vertical lines represent the reform year 1998, and the year 2001. The period 1998–2000 represents the phase-in period. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded. Municipality list from the year 2000 ($N = 435$).

TABLE 4 Long term outcomes

	Prim. care consultations		Specific diagnoses (groups)					
	Prob.	No.	MSD	Psych.	Cardio.	Depr	Hyper-tension	Life style
Phasein ITT	0.001	−0.025	−0.036	−0.038	0.011	−0.018	0.020	−0.007
	(0.0269)	(0.2596)	(0.0349)	(0.0233)	(0.0350)	(0.0166)	(0.0350)	(0.0210)
Post ITT	0.017	−0.386	−0.032	−0.018	−0.033	−0.008	−0.013	0.006
	(0.0300)	(0.2782)	(0.0355)	(0.0342)	(0.0287)	(0.0163)	(0.0252)	(0.0204)
N	6834	6834	6834	6834	6834	6834	6834	6834

Note: Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Standard errors clustered at the municipality level in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

TABLE 5 Long term outcomes—Heterogeniety

	MSD	Psych.	Cardio.	Depr	Hyper-tension	Life style
Flexible jobs						
Post ITT	0.111	−0.011	0.123	−0.042	0.116*	−0.012
	(0.1039)	(0.0767)	(0.0750)	(0.0450)	(0.0687)	(0.0463)
Non flexible jobs						
Post ITT	−0.082*	−0.011	−0.032	−0.003	−0.011	0.039
	(0.0458)	(0.0498)	(0.0377)	(0.0253)	(0.0317)	(0.0238)
Young children						
Post ITT	−0.075	0.070	0.040	−0.002	0.014	0.015
	(0.0973)	(0.0873)	(0.0889)	(0.0449)	(0.0834)	(0.0358)
Not young children						
Post ITT	−0.031	−0.052	−0.066*	−0.011	−0.037	0.009
	(0.0431)	(0.0391)	(0.0345)	(0.0181)	(0.0307)	(0.0241)
Partner						
Post ITT	−0.030	−0.011	−0.071*	−0.012	−0.029	0.007
	(0.0483)	(0.0368)	(0.0383)	(0.0191)	(0.0344)	(0.0226)
No partner						
Post ITT	−0.000	−0.001	0.066	0.025	0.013	−0.007
	(0.0812)	(0.0741)	(0.0699)	(0.0382)	(0.0495)	(0.0411)
High education						
Post ITT	−0.038	−0.003	−0.066	0.008	−0.104*	0.026
	(0.0975)	(0.0688)	(0.0657)	(0.0364)	(0.0593)	(0.0294)
Low education						
Post ITT	−0.030	−0.015	−0.035	0.000	0.002	0.001
	(0.0383)	(0.0350)	(0.0345)	(0.0196)	(0.0307)	(0.0246)

Note: Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Standard errors clustered at the municipality level in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

this result into actual savings, we can conduct the following simple calculations: first, we assume that our result has high external validity and impose the same effect on all women. We use country averages and apply the average earnings for full time equivalents in Norway between 2016 and 2019, which was 552,000 NOK for women. The mean of sickness absence during the same time period was 6.5%, which amounts to a yearly cost of sickness absence of about 36 billion NOK. Applying our effect estimate, the cost of sickness absence is reduced by 5 billion NOK. The reform increased the numbers of formal care workers with 12,000 full-time equivalents. Applying the average earnings from above, this amounts to a cost of 7 billion NOK. However, our results are likely to have poor external validity; we found no significant effects on sickness absence for all daughters or

for sons with no siblings. If we calculate savings using only our sample of daughters without siblings, we get the following: sickness absence for this group amounts to 20%, the average earnings (among workers) is 344,000 NOK, and they are 74,000 individuals. Thus, an estimate of the total cost of sickness absence for this group is 4.3 billion and the savings caused by the reform amounts to 800 million NOK.

The reform has a net benefit under the unlikely assumption of the same reduction for all workers. However, under the more plausible assumption that this effect only holds for our main sample, the cost outweighs the savings. There are, however, several potential benefits not accounted for in this analysis, the most obvious one being presumably increased care quality for the elderly. Other potential benefits related to informal caregivers may be parents spending more time with their kids; more time available for working might lead to higher performance at work and higher work satisfaction; and less stress and more time in general can lead to higher quality of life for informal caregivers. In addition, those who are care workers or who care for small children, can provide better care for patients and children when care obligations for parents are reduced. Almost 35% of female workers work part time. Reduced caregiving responsibilities could potentially allow more women to work full time.

The reduction in sickness absence could be caused by health improvement for the caregivers. Improved health has intrinsic value for the individuals, and is therefore a potential benefit of the reform. Improved health today can even reduce the need for health care in the longer run, adding to the benefits of providing increased public formal care. However, we find that there are likely no long-term health benefits of increasing formal care. Moreover, we show that the reduction in sickness absence only holds for those whose work requires that they are physically present at a certain time and place. For those with more flexible jobs, we find no reductions in sickness absence. This difference points to the sickness absence results being more about the need for more flexibility in everyday life, than actual health benefits. It has been shown that the sickness insurance system in Norway is partly used to insure employees against a broad range of circumstances other than own illness, such as the need for increased work flexibility (Carlsen, 2008; Gautun, 2008; Markussen et al., 2011).

Our interpretation is that the reduction in sickness absence is caused, at least in part, by reduced need for absence from work to provide care for frail parents. The policy implications of our results are that sickness absence uptake could be hiding a need for more flexibility around work for people with caregiving responsibilities. Certified sickness access is granted upon a consultation with a physician. However, there is a severe shortage of primary physicians in Norway, and physician's time is a particularly scarce resource. Thus, there might be other policies that are better suited to provide the needed flexibility for informal caregivers. Such arrangements could be more specifically oriented toward those who need more flexibility to avoid undermining the sickness absence system. Increasing formal eldercare is one such possible arrangement.

Our study is based on the assumption that formal and informal care are substitutes, at least in part. Several studies has shown that there is a link between formal care and informal care (Bolin et al., 2008; Bonsang, 2009; Van Houtven & Norton, 2004). Jakobsson et al. (2012) show that there is a negative correlation between the level of publicly provided elder care and the level of informal care provided by informal caregivers in Norway. While this link is not the primary concern of this study, our results point to a link between formal and informal care. We show that when formal care is increased, sickness absence is reduced, in particular for those who work in sectors with little work flexibility. The most likely channel through which formal care affects sickness absence is reduced informal care responsibilities. We could think of other channels such as the *family effect*. Bobinac et al. (2010) argues that there are two effects at play when analyzing the health effects of caregiving within families. The first effect is the *caregiving effect*, which is the effect on health of providing care. The second effect is the *family effect*. This is the health effect of having a family member with care needs. Because we care about people who are close to us, we are impacted by their health. If the parent becomes substantially healthier by having better access to formal care, we might expect their daughters to feel better and thus needing less sickness absence. However, since we only find effects for daughters in less flexible occupations, this is a highly unlikely mechanism. There is no reason to expect that daughters with these kinds of jobs should have a stronger family effect compared to daughters with more flexible jobs. Thus, our findings point toward that formal and informal care are substitutes.

The main strength of this study is the application of extremely rich register panel data in combination with an exogenous increase in formal care, and the assessment of long term health effects, all of which are limitations in the literature. Although the study by Løken et al. (2017) share some of the data and outcomes and were the first to apply the reform to analyze formal care expansion, this study have access to better data to study this problem (such as all sickness absence spells, including short spells, diagnoses on the sickness absence, and long term health outcomes). This enables us to explore the important question of whether reduced sickness absence is caused by improved health or if other factors, such as time constraints or work flexibility are likely drivers of this result. Other strengths include using data that cover the entire population (no selection bias in the data) and health outcomes that are not self-reported. As far as we know, our study is the first to do so for long-term health outcomes. Most studies in this field apply survey data, which typically has the disadvantage of small samples or selection bias, in addition to subjective measures of health. The latter implies that the measure of health can be highly contextual. We cannot exclude that

a caregiver rate own health in relation to that of the care recipient, which can be a source of measurement error that leads to bias. Moreover, most the studies in this field use propensity score matching or instrumental variables to identify the causal effect. The problem with the former is that is does not use exogenous variation in caregiving, thus we cannot be sure that there is no confounding, and the problem with many of the studies that use IV is that they use health shocks to family members or in-laws as instruments, and we cannot be certain that these shocks are exogenous to own health.

Some limitations of our study are important to note. First, in our short-term analysis, we can only investigate those in the labor force. An important group of caregivers are not employed. However, we do not have access to other health information besides sickness absence before 2006. Second, the results are sensitive to the exclusion of municipalities with particularly high or particularly low pre-reform coverage rates. They are not included in the main analysis as the linearity assumption is not well supported for these levels of coverage rates; however, we added them to the analysis as a robustness check. For some of the outcomes the point estimate was substantially lower and no longer statistically significant. Third, we are not able to separate out the *family effect* from our estimates, so the effects estimated in this study must be seen as a combination of the two effects. Fourth, when studying caregiving and health using only register data, a lot of information that is available in survey data is missed. In our study using register data, we can only identify the presence or sickness, such as cardiovascular disease or hypertension. These are severe health outcomes. A range of more moderate health effects could have been studied if we had had access to survey data. More importantly, data on informal care provision at the individual level are only available using survey data. Unfortunately, our data does not allow us to see who the caregivers are and who responded more strongly to increased formal eldercare. The lack of significant long-term effects must be seen in light of this. Access to this information would enable the estimation of a first stage and the local average treatment effect (LATE), as well as providing an informative descriptive analysis of which demographic group responds more strongly to increased formal elder care. Moreover, access to more detailed caregiving information would allow us to investigate the distribution of health effects along the intensive margins of caregiving, which would be beneficial for policy makers. Future research would benefit from access to more detailed caregiving information of large samples using more objective health data. Fifth, there can still be moderate effects in our long term analysis that we do not have enough power to detect. Finally, we found no effects for sons without siblings. Gender roles are changing and women are spending more time in paid employment now compared to in the beginning of 2000. Future studies of caregiving would therefore benefit from paying attention to gender differences.

7 | CONCLUSION

We expand on the analysis by Løken et al. (2017) and show that increased formal care of the elderly led to a decrease in the probability of sickness absence for single-child daughters with only one surviving parent aged 80 or older also when we include all absence spells in Norway, and that the decrease in absences is driven by leaves of absence related to MSD and psychological disorders. These are disorders that are more difficult to verify, and diagnoses that we would expect for leaves of absence that are not necessarily caused by health issues. We then show that the effects are concentrated among women who work in places where one needs to be physically present at a certain time and place to conduct the work task. For those who to a greater extent can work more flexible hours or from home, there are no effects. We found no effects across other groups, such as those with and without young children, those with and without partners and those with low compared to high education. Finally, we were unable to detect any impact of the formal care expansion on long-term health care utilization or on the presence of a range of diagnoses that have been identified in the literature as associated with providing informal care. All our results point to time constraints being an important reason why sickness absence is reduced when formal care increases.

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CONFLICT OF INTEREST

The authors declare that they have no conflict of interest.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from Statistics Norway. Restrictions apply to the availability of these data, which were used under license for this study. Data are available from ssb.no with the permission of Statistics Norway.

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ENDNOTES

[1] See Statistics Norway's statbank at https://www.ssb.no/statbank/table/08252/.

[2] In Norwegian: "Handlingsplan for Eldreomsorgen".

[3] A municipality is the smallest governing level in Norway, and the municipality's responsibilities include providing primary health care and old age welfare services to the municipality's citizens. Specialist health care is governed at a higher administrative level. In 1998 there were 435 municipalities in Norway.

[4] The plan also included goals for quality improvements in the home-based care and existing properties.

[5] No data are available on quality or hours of care. Thus, we are not able to separate between differing quality across municipalities.

[6] We define daughters with no siblings as daughters of mothers who have only one child.

[7] This is the same sub-sample applied by Løken et al. (2017).

[8] The data from NAV and KUHR were not available to Løken et al. (2017).

[9] Løken et al. (2017) only had access to certified sickness absence spells lasting longer than 16 days. Almost half of the absence spells are shorter than this.

[10] There is no available national record of self-reported sickness absence days.

[11] Workers who are frequently absent may need certification from day one.

[12] Although this choice could potentially affect the results, we expect that it is of little importance as Løken et al. (2017) estimate no effects of the care expansion on employment at the extensive margin.

[13] In 2006, 1 G represented approximately USD $10,000.

[14] Sickness absence diagnoses follow the International Classification of Primary Care system, 2nd Edition (ICPC-02).

[15] Table A2 gives an overview of which ICPC-02 are used to create the diagnosis groups.

[16] The data report only the number of individuals who receive care, not the amount of care each individual receives.

[17] This specification is very close to that of Løken et al. (2017).

[18] For some specifications in the robustness checks (where we estimate the effects for all daughters, with and without siblings), we include the child's birth order and the number of siblings as controls.

[19] See the lower panel of Figure 1. The exclusion of these outliers was also applied by Løken et al. (2017). As a sensitivity analysis, we show the results including these outliers (see appendix, Table A5).

[20] Daughters' moving behavior before and during the 1998 reform has been thoroughly covered by Løken et al. (2017). They show that the reform's effect on moving behavior was a fairly precisely estimated zero. Moreover, they show that the pre-reform rates of mobility (year-to-year changes in municipality of residence) for daughters was very low (1 percent during the last year before the reform) and did not change during the reform years.

[21] Educational levels are fairly equally distributed across the two groups. We also assess heterogeneity of the effects with respect to education below.

[22] Although we use a slightly different sample and use sickness absence data from a different register, these results are in line with the results on sickness absence found in Løken et al. (2017). We replicate their result in Figure A2 and Table A11 applying the same source of sickness absence and the same period of analysis, 1993–2005, as they did. However, to obtain data on the diagnoses connected to each sickness absence spell, we need to use a different register for our analysis (the NAV Register).

[23] Testing for statistical differences in these estimates confirms that the estimate for MSD is not equal to the unspecified diagnoses group and the rest category. For absences related to a psychological condition, however, we are not able to reject that the estimates are statistically different for the two categories.

[24] We drop individuals who were dead in the years in which health outcomes are measured.

[25] In Table A4, we show that there were no effects on employment, disability, and mortality. These long-term outcomes are interesting in themselves, and important for this analysis as they are states that may affect the likelihood and regularity of primary care consultations, for example, as employed individuals need a physician to certify sickness absences.

[26] Confirming and expanding on the results by Løken et al. (2017).

[27] Depending on the cost benefit trade-off between providing more formal care and paying sickness absence, which, in Norway, the state does for all absence spells lasting more than 16 days.

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APPENDIX

Additional Tables and Graphs

TABLE A1 Municipality Characteristics (1997 numbers)

	Municipalities with pre-reform home based care coverage	
	Below median	**Above median**
Total population	12,892	8849
	(39,619)	(19,397)
Share of pop 67 years+	0.159	0.157
	(0.0334)	(0.0368)
Share of pop 80 years+	0.0483	0.0486
	(0.0146)	(0.0156)
Share of males 16+ with compulsory schooling	0.374	0.386
	(0.0764)	(0.0789)
Share of females 16+ with compulsory schooling	0.424	0.435
	(0.0732)	(0.079)
Share of males 16+ with high school	0.47	0.473
	(0.0507)	(0.0531)
Share of females 16+ with high school	0.413	0.413
	(0.0462)	(0.0542)

(Continues)

TABLE A1 (Continued)

	Municipalities with pre-reform home based care coverage	
	Below median	**Above median**
Share of males 16+ with university degree	0.11	0.103
	(0.0345)	(0.0323)
Share of females 16+ with university degree	0.14	0.133
	(0.0367)	(0.0341)
Share of working age pop. employed	96.3	96.1
	(1.9)	(1.9)
Private income (100,000 NOK)	6967	5674
	(5631)	(5198)
Munic. unrestricted budget per capita	22,641	23,233
	(7266)	(6356)
Centrality index	3.94	3.58
	(2.51)	(2.54)
Population density	4.29	3.92
	(2.75)	(2.87)
Home based care coverage rate (pop. 80+)	0.362	0.411
	(0.0661)	(0.0892)
Pre reform coverage rate (1993–1997)	0.373	0.459
	(0.0257)	(0.0334)
Change from pre to post	0.00893	−0.0478
	(0.0654)	(0.0649)
Inst. care coverage rate (pop. 80+)	0.193	0.192
	(0.0659)	(0.0632)
Pre reform coverage rate (1993–1997)	0.214	0.205
	(0.0652)	(0.0588)
Change from pre to post	−0.0422	−0.0382
	(0.0415)	(0.0521)
Observations	173	174

Note: The table shows mean values and standard deviations of municipality characteristics measures in 1997 when no other year is specified. Municipalities are divided into groups based on whether they fall below or above the median home based care coverage rate in the pre-reform period, defined as 1993–1996. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded.

Robustness checks

The results from the robustness checks are presented in the Appendix in Tables A5 and A6. In these tables, we include the baseline specification for comparison in the first column, and we show only the results for the post-ITT. We first exclude large cities and the most rural municipalities (in columns 2 and 3, respectively), and we then include alternative ways of treating the year 1997 (the year before the reform), first by excluding that year (column 4) and second by including 1997 as a phase-in year (column 5). In column 6, we include a specification where we split the municipalities into treatment and control groups based on whether the municipalities fall above or below the median in the pre-reform coverage distribution. Finally, in column 8, we include municipalities with particularly high or low pre-reform coverage rates.

The specification checks for sickness absence spells are shown in Table A5. The estimated decrease in the probability of sickness absences remains relatively robust, except in column 2 where we exclude large cities and in column 7 where municipalities with particularly high and low pre-reform coverage rates are excluded. In both these cases, the estimates are smaller, but the direction of the effect remains the same. The results are similar to the baseline specification for the other specification checks. The diagnoses that seem to be driving the results on sickness absence, MSD and psychological disorders remain robust throughout all specifications, while estimates for the other categories remain close to zero and are not statistically significant. For the long-term health outcomes, most specification checks show, as in the baseline specification, non-statistically significant effects. The estimated small increase in the likelihood of a primary health-care consultation is robust to most specifications,

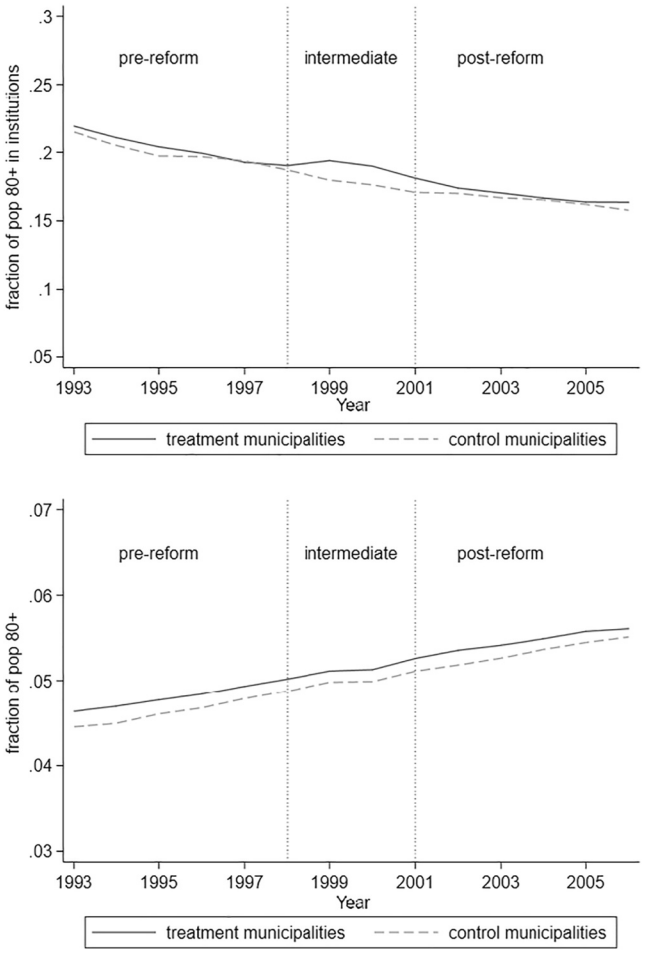

FIGURE A1 Other Municipality Characteristics. The figures show the development in coverage rates in institution based care, and in the fraction of population aged 80 and above. Institution based care is defined as care in nursing homes. Treatment is defined as falling below the median home based care coverage rate in the pre-reform period, defined as 1993–1996. The dotted vertical lines represent the reform year 1998, and the year 2001. The period 1998–2000 represents the phase-in period. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded. Municipality list from the year 2000 ($N = 435$)

except when we split the municipalities into treatment and control groups based on median pre-reform coverage in column 6. Last, the estimated decrease in the likelihood of a hypertension diagnosis remains statistically significant and about the same size throughout all the specification checks.

Table A6 shows results from the same specification checks as above, but separate for the sub groups assessed as heterogeneity analysis. The main outcome on sickness absence is considered in this table. Again we see that results are driven by individuals with little flexible jobs (first row). As for the main sample of daughters, we see that this result is robust to most of the specification checks, except where we exclude large cities and municipalities with particularly high or low pre-reform coverage rates.

Next, we estimate the reform effects on alternative samples: (i) all daughters and (ii) sons with no siblings. These samples are assumed to have lower or no care burdens, and thus, are less likely to be affected by the care expansion. The results for these alternative samples are shown in Table A7. We maintain the baseline results in the first column, while the results for all daughters and for sons with no siblings are presented in the following columns. Table A7 shows no effects on either sickness absence or on absence related to specific diagnoses for any of the groups assumed to have fewer care responsibilities.

We further preform placebo tests by estimating the effects on daughters with no siblings where both parents are deceased and on those with younger parents (aged 60–72). The results for the placebo tests are presented in Table A8. As seen in the table, we find no statistically significant effects of the care expansion on the probability of sickness absence. As noted by Løken et al. (2017), the group of daughters with no living parents may have care responsibilities for parents-in-law, and thus be affected by the care expansion. However, the direction on the estimated effect in the placebo test contradicts the direction we would expect given the findings in the literature.

Finally, we re-estimate the main effects using an alternative approach where we drop the phase-in period and treat all years following the reform as post-reform years. Results for the specification where we do not use a phase-in period are presented in Table A9. Here we treat all years following the reform as post-reform years. We see that this specification does not alter the conclusions from the main analysis. Though a little smaller, the point estimates for sickness absence, as well as for leaves of absence related to underlying diagnoses, are still statistically significant for absence at all and absences related to MSD and psychological disorders, and small and not statistically significant for the groups of unspecified diagnoses and the rest category. Smaller effects is what we would expect using this specification, and supports our main specification treating the first years following the reform as a phase-in period.

TABLE A2 ICPC-02 classification

Diagnosis	ICPC-2 code	ICPC-02 titles
Musculoskeletal (MSD)	Any code with prefix L	Chapter L: Musculoskeletal
Psychological	Any code with prefix P	Chapter P: Psychological
Cardiovascular	Any code with prefix K	Chapter K: Cardiovascular
Depression	P03	Feeling Depressed
	P73	Depressive disorder
Hypertension	K85	Elevated blood pressure
	K86	Hypertension uncomplicated
	K87	Hypertension complicated
Life style related diseases	T07	Weight gain
	T82	Obesity
	T83	Overweight
	T90	Diabetes non-insulin dependent
	P15	Chronic alcohol abuse
	P16	Acute alcohol abuse
	P17	Tobacco abuse
	P19	Medication abuse
	P19	Drug abuse

Note: The table shows the ICD-02 codes used to classify sickness absence spells related to specific diagnoses, and to construct the long-term health outcomes.

TABLE A3 Additional results on sickness absence—selected diagnosis categories

	Sickness absence (selected diagnoses)					
	P01-P29 psych. symptoms	P70-P99 psych. diag.	P03; P76 depression	L01-L29. MSD symptoms	L83-L95; L98; L99 MSD diag.	L72-L81; L96 MSD injury
Phasein ITT	−0.000	−0.004	0.000	−0.006	−0.005	−0.007**
	(0.0029)	(0.0031)	(0.0038)	(0.0040)	(0.0069)	(0.0030)
Post ITT	−0.006**	−0.006**	−0.005*	−0.007*	−0.014*	−0.007**
	(0.0027)	(0.0030)	(0.0032)	(0.0037)	(0.0074)	(0.0033)
Mean	[0.00847]	[0.0215]	[0.0223]	[0.019]	[0.0615]	[0.0145]
Obs.	57,753	57,753	57,753	57,753	57,753	57,753

Note: The table show estimated effects of expanding formal eldercare on sickness absence spells related to diagnoses broken down to more specific categories than in the main results. Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

TABLE A4 Additional long-term outcomes—employment, disability, and mortality

	Probability of			
	Dying	Disability insurance	Employment	Full time employment
Phasein ITT	−0.009	−0.058	0.024	0.039
	(0.0133)	(0.0403)	(0.0370)	(0.0304)
Post ITT	−0.014	−0.025	−0.005	−0.000
	(0.0172)	(0.0269)	(0.0308)	(0.0293)
N	7151	6834	6834	6834

Note: The table show estimated effects of expanding formal eldercare on long-term labor market outcomes. The outcomes are health outcomes 11 years after. Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

TABLE A5 Specification checks: Sickness absence—main diagnosis categories

	Daughters, no siblings						
	Baseline	**Excluding large cities**	**Excluding rural municipalities**	**Drop 1997**	**Including 1997 as post**	**Split at median coverage**	**Incl. extr. cov. rate**
Sickness absence at all	−0.030**	−0.024	−0.036**	−0.033*	−0.030**	−0.031***	−0.005
	(0.0146)	(0.0148)	(0.0155)	(0.0169)	(0.0146)	(0.0110)	(0.0082)
Related diagnoses:							
Musculoskeletal	−0.025***	−0.021**	−0.027***	−0.026**	−0.025***	−0.026***	−0.01*
	(0.0090)	(0.0092)	(0.0097)	(0.0106)	(0.0090)	(0.0064)	(0.0042)
Psychological	−0.011***	−0.011***	−0.013***	−0.009**	−0.011***	−0.008**	−0.007**
	(0.0040)	(0.0044)	(0.0042)	(0.0043)	(0.0040)	(0.0037)	(0.0027)
All other diagnoses	−0.004	−0.002	−0.007	−0.008	−0.004	−0.003	−0.005
	(0.0073)	(0.0073)	(0.0077)	(0.0085)	(0.0073)	(0.0064)	(0.0034)
No specified diagnosis	−0.001	0.000	−0.001	−0.000	−0.001	−0.001	0.000
	(0.0048)	(0.0053)	(0.0050)	(0.0053)	(0.0048)	(0.0038)	(0.0032)
Obs.	57,753	43,292	52,770	51,878	57,753	57,753	66,399

Note: Indicator for the Post period (2001–2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The estimates of the effect in the Phase-in period is excluded in this table. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE A6 Specification checks for sub groups: Sickness absence

	Daughters, no siblings						
Sub group:	**Baseline**	**Excluding large cities**	**Excluding rural municipalities**	**Drop 1997**	**Including 1997 as post**	**Split at median coverage**	**Incl. extr. cov. rate**
Not flexible	−0.037**	−0.028	−0.041**	−0.041**	−0.037**	−0.042***	−0.005
	(0.0180)	(0.0184)	(0.0194)	(0.0205)	(0.0180)	(0.0129)	(0.0113)
Flexible	−0.003	0.007	−0.014	−0.006	−0.003	−0.018	0.007
	(0.0254)	(0.0264)	(0.0253)	(0.0296)	(0.0254)	(0.0205)	(0.0154)
Child	−0.039	−0.052	−0.030	−0.018	−0.039	−0.006	0.006
	(0.0325)	(0.0347)	(0.0347)	(0.0463)	(0.0325)	(0.0253)	(0.0198)
No child	−0.029*	−0.021	−0.039**	−0.034*	−0.029*	−0.034***	−0.004
	(0.0162)	(0.0164)	(0.0172)	(0.0185)	(0.0162)	(0.0118)	(0.0095)
Partner	−0.030	−0.018	−0.036*	−0.032	−0.030	−0.036**	−0.004
	(0.0187)	(0.0175)	(0.0197)	(0.0218)	(0.0187)	(0.0140)	(0.0104)
No partner	−0.025	−0.037	−0.030	−0.025	−0.025	−0.018	0.002
	(0.0276)	(0.0303)	(0.0292)	(0.0308)	(0.0276)	(0.0251)	(0.0174)
Low educ.	−0.035**	−0.029*	−0.043**	−0.042**	−0.035**	−0.033***	−0.009
	(0.0158)	(0.0158)	(0.0170)	(0.0186)	(0.0158)	(0.0127)	(0.0102)
High educ.	−0.020	−0.016	−0.028	−0.009	−0.020	−0.028*	0.003
	(0.0228)	(0.0257)	(0.0229)	(0.0241)	(0.0228)	(0.0165)	(0.0135)
Obs.	15,467	10,828	14,468	13,945	15,467	15,467	17,666

Note: Indicator for the Post period (2001–2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The estimates of the effect in the Phase-in period is excluded in this table. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE A7 Alternative samples: Sickness absence—main categories

	Daughters		Sons
	No siblings	All	No siblings
Sickness absence at all	−0.030**	−0.004	−0.004
	(0.0146)	(0.0070)	(0.0090)
	[0.214]	[0.213]	[0.154]
Related diagnoses:			
Musculoskeletal	−0.025***	−0.002	−0.003
	(0.0090)	(0.0046)	(0.0058)
	[0.0924]	[0.0935]	[0.0643]
Psychological	−0.011***	−0.002	0.001
	(0.0040)	(0.0022)	(0.0031)
	[0.0295]	[0.0287]	[0.0162]
All other diagnoses	−0.004	−0.002	−0.003
	(0.0073)	(0.0030)	(0.0054)
	[0.0685]	[0.0684]	[0.0538]
No specified diagnosis	−0.001	0.001	−0.001
	(0.0048)	(0.0022)	(0.0034)
	[0.0368]	[0.0356]	[0.027]
Obs.	57,753	290,863	89,576

Note: Indicator for the Post period (2001–2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The estimates of the effect in the Phase-in period is excluded in this table. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE A8 Placebos: Sickness absence—main categories

	Daughters, no siblings	
	No living parents	Younger parents
Sickness absence at all	−0.017	−0.034
	(0.0122)	(0.0380)
	[0.228]	[0.244]
Related diagnoses:		
Musculoskeletal	0.003	−0.020
	(0.0078)	(0.0275)
	[0.0977]	[0.108]
Psychological	0.002	−0.006
	(0.0040)	(0.0176)
	[0.0285]	[0.0378]
All other diagnoses	−0.010	0.014
	(0.0079)	(0.0210)
	[0.0777]	[0.0797]
No specified diagnosis	−0.007	−0.021
	(0.0048)	(0.0143)
	[0.0385]	[0.035]
Obs.	70,753	6708

Note: Indicator for the Post period (2001–2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The estimates of the effect in the Phase-in period is excluded in this table. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE A9 No phase-in period: Sickness absence—main categories

		Absence with specific diagnosis			
	Sickness absence at all	**Musculoskeletal diag.**	**Psychological diag.**	**All other diag.**	**Unspecified diag.**
Post ITT	−0.027**	−0.021***	−0.007**	−0.002	−0.002
	(0.0113)	(0.0071)	(0.0034)	(0.0063)	(0.0042)
Mean	[0.214]	[0.0924]	[0.0295]	[0.0685]	[0.0368]
Obs.	57,753	57,753	57,753	57,753	57,753

Note: Post covers the entire post-reform period, 1998–2003, and is interacted with the negative pre-reform coverage rate, scaled by 10. The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

TABLE A10 Simple test of survivorship bias

	Year in sample
Low edu	0.110***
	(0.0149)
Treated	0.035
	(0.0571)
LOWedu*Treatment	0.001
	(0.0286)
Age	−0.063***
	(0.0017)
Mother birth year	−0.263***
	(0.0020)
Father birth year	−0.058***
	(0.0010)
Obs.	441,705

Note: Regression of years a daughter is in the sample. Standard errors clustered at the municipality level in parenthesis, *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

In sum, the robustness analysis shows that the results found in the main analysis are robust to most alternative specifications and reassuringly, that when assessing the same outcomes for samples less likely to be affected by the reform, we find essentially no effects.

Replication Results

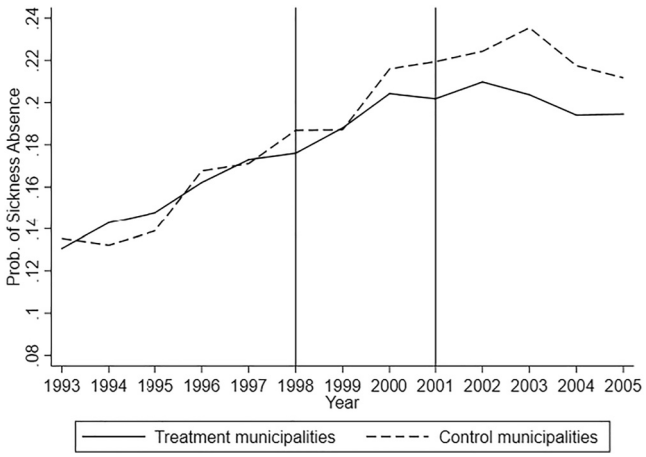

FIGURE A2 Replication Results on Sickness Absence (16 days+). The graph shows the replication of the sickness absence result for daughters with no siblings and with only one living parent at least 80 years old from Løken et al. (2017). Treatment is defined as falling below the median home based care coverage rate in the pre-reform period, defined as 1993–1996. The vertical lines represent the reform year 1998, and the year 2001. The period 1998–2001 represents the expansion period. Municipalities with pre-reform home care coverage rates falling below the 10th and above the 90th percentiles are excluded. Municipality list from year 2000 ($N = 435$)

TABLE A11 Replication Results on Sickness Absence (16 days+)

	Probability of sickness absence	Probability of sickness absence (cond. on work)	Number of sick days (cond. on work)
Main sample			
Daughters, no siblings			
Phasein ITT	−0.011	−0.020	−2.609
	(0.0101)	(0.0128)	(1.8557)
Post ITT	−0.024**	−0.028**	−2.695
	(0.0105)	(0.0139)	(2.0629)
	[0.146]	[0.194]	[20.4]
Obs.	101,785	70,948	70,948

Note: Sickness absence from the FD-trygd register. Indicators for the Phase-in (1998–2000) and the Post (2001–2003) period are interacted with the negative pre-reform coverage rate, scaled by 10. Note that the period indicators are called "short-term" and "long-term" in Løken et al. (2017). The estimates are interpreted as the effect of having a 10 percentage points lower pre-reform coverage rate, which corresponds to having a 7.8 percentage points increase in the coverage rate from the pre- to the post-reform period. Mean values (pre-reform) in brackets and standard errors clustered at the municipality level in parenthesis, $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.