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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 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. In general, we find no effects on long-term health outcomes.

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Keywords: Formal and informal eldercare, sickness absence, health.

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1 Introduction

With an aging population, the demand for care along with increasing health-care expenditures places an increasing burden on public finances. Informal care may both substitute for and complement publicly provided formal care; see e.g. Bonsang (2009), Bolin et al. (2008), and Van Houtven and Norton (2004). However, studies have shown that informal care may come at a cost to caregivers. For instance, care responsibilities can be a stressor as they often come in addition to other obligations, such as work and household chores, thus leading to reduced work hours or less leisure time (Vaage, 2000; Gautun and Hagen, 2010). Much of the care burden falls on close relatives, especially on the spouse or children of the person in need of care (Vaage, 2000; Jakobsson et al., 2012).

The relationship between care responsibilities and health has been well assessed; see Bauer and Sousa-Poza (2015) and Pinquart and Sörensen (2003, 2007) for reviews. However, the link is complex. On one hand, taking care of a frail parent can be rewarding (Toljamo et al., 2012). On the other, care responsibilities often involve physically demanding tasks, as well as mental strains over substantial periods of time, which may have negative effects on health both in the short and long run. Finally, time spent on caregiving may also supersede health-enhancing activities, such as physical exercise and other recreational activities.

The majority of the empirical evidence in this area documents negative correlations between health and caregiving. Moreover, a substantial share of the empirical evidence focuses on psychological aspects, such as stress and depression (Bauer and Sousa-Poza, 2015; Gautun and Hagen, 2010; Pinquart and Sörensen, 2003, 2007). Living with elevated stress levels or poor mental health over time has, in turn, been linked to reduced physical health, such as cardiovascular disease, hypertension, lung ailments, accidents, and suicide (Von Känel et al., 2008; Capistrant et al., 2012). Care responsibilities may also have direct and immediate impacts on physical health through physically demanding tasks (Pinquart and Sörensen, 2006). Finally, it may take time for the burden of caregiving to manifest as health adversities. The importance of assessing delayed health effects of caretaking has been highlighted by Coe and Van Houtven (2009), Leigh (2010), and Schmitz and Westphal (2017).

In this paper, we assess the long-term health implications of being an informal caregiver. Specifically, we assess the effects of increased formal elder care provision on long-term health outcomes for daughters of older, single parents. In addition, we replicate the results from Løken et al. (2017), who show that lifting the care burden for adult daughters led to a decrease in insured sickness absence from work. Here, we rely on new detailed register data to assess what underlying diagnoses drive the reduction in sickness

absences.

Despite a well assessed link between caregiving and health, much of the empirical literature suffers from a lack of causality. For instance, a large share of the available studies are based on cross-sectional data with small sample sizes, and often rely on subjective measures of health. In addition, several studies have poor or even lack reliable identification strategies. Also, there is little evidence using large-scale register data that rely on objective health outcomes. Therefore, the evidence for causal effects of caregiving remains limited, especially for long-term outcomes.

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 have endogenous uptake, too. 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 is a likely 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 high and low care responsibilities.

In this paper, we address 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, which enables us to estimate causal effects of increased formal care by comparing outcomes across municipalities with different levels of formal care expansions. We assess the sub-sample most likely to be affected by the reform: single-child daughters who have only one remaining parent who is at least 80 years old, and estimate causal effects of expanding publicly provided eldercare on short and long-term health outcomes for these daughters.

A similar approach was applied by (Løken et al., 2017) who used the reform to assess the effect of increased formal care on various labor supply outcomes for the same sub-sample of daughters. They show that the reform reduced certified sickness absence from work, thus, indicating that increased formal care positively affects the informal caregiver's health. However, sickness absence has been shown to be used as a means to

insure employees against a broad range of circumstances, besides own illness (Markussen et al., 2011; Carlsen, 2008; Gautun, 2008). Thus, we cannot rule out that the reduction in sickness absences is caused by reduced need for work absence due to reduced care obligations, rather than better health outcomes. In addition, sickness absence is observed only for those who are employed. An important group of informal caregivers are those with no or weak connections to the labor market. The contributions from this study, thus, lies in providing evidence on the health implications of increased formal care on long-term health outcomes, including health measures that are unrelated to labor market participation. In addition, we rely on novel register data that allow assessing what types of diagnoses are driving the reductions in sickness absences caused by the reform.

We start by showing that increased formal home-based care leads to reduced insured sickness absence from work in the short-run. This is in line with the findings from Løken et al. (2017). We supplement this finding by exploiting detailed data on underlying diagnoses and assess whether certain diagnoses drive the observed reduction in leaves of absence. Our results indicate that musculoskeletal and psychological disorders are the main drivers. This is an interesting finding because musculoskeletal and psychological disorders has both been associated with caregiving (Pinquart and Sörensen, 2006, 2007; Bauer and Sousa-Poza, 2015). Moreover, these diagnoses are particularly difficult to verify and, thus, vulnerable to misdiagnosis.

We then assess long-term health outcomes for the same sub-sample of individuals. Here, we employ rich and detailed register data on the utilization of primary health-care services and diagnoses related to caregiving in the literature.[1] Overall, our results show that long-term health is not much affected by increased formal care. More specifically, with one exception, we estimate no effects on the occurrence of a number of diagnoses identified in the literature as potentially affected by care responsibilities. The exception is an estimated 11 percent decrease in the risk of hypertension in the long run, which may be explained by lower stress levels in the sample of daughters who experienced a reduction in their care burden following the reform. However, this finding does not remain statistically significant after correcting for multiple hypothesis testing, and thus, needs to be interpreted with caution.

Finally, there might be heterogeneous effects in dimensions such as socioeconomic status (SES). However, it is not straightforward to hypothesize whether individuals with high or low SES are more affected by the care expansion assessed in this paper. On one hand, we might expect individuals with low SES to be more affected by the policy

[1]Importantly, as, for instance, the use of primary health-care services varies across the employed and the non-employed, we show that the expansion had no long-term effect on employment, or on death, or on the likelihood of receiving disability insurance.

change, as low SES individuals are more likely to take on a caregiver role (Schulz and Sherwood, 2008). On the other, we may also expect a strong reform response from high SES individuals as they may be more able to gain their parents access to the increased formal care capacity.[2] In general, being a caregiver can be especially stressful for low educated individuals as they might have less job flexibility with regard to e.g. working hours. At the same time, this group also has lower opportunity costs and may be more likely to reduce work hours due to caregiving responsibilities, making the higher educated group more likely to be in a situation that combines work and caregiving. However, in a meta-study Pinquart and Sörensen (2007) find no differences in the associations between caregiving and health across education. We add to the analysis by assessing differences in the effects for daughters with high and low education. In general, we find that there is not much difference in the effects of the care expansion across educational groups. The exceptions are a larger reduction in the probability of a sickness absence leave related to a psychological disorder for the group with low education, and that the estimated increase in the risk of hypertension in the long run seems to be driven by the group with high education.

The paper proceeds as follows. In Section 2, we review the related literature. In Section 3, we provide details on the institutional background and the reform. Section 4 describes the different sources of data, and Section 5 explains the empirical strategy. In Section 6, we discuss the results, and Section 7 concludes.

2 Related Literature

2.1 Informal caregivers

Several studies have documented that informal care of the elderly may to a certain extent substitute for formal care; see e.g Cutler and Sheiner (1994); Bonsang (2009); Stabile et al. (2006). However, the substitution rate seems to be weakening as the elderly individual in need of care becomes older or his or her health becomes more frail (Bonsang, 2009; Daatland et al., 2009; Bolin et al., 2008; Van Houtven and Norton, 2004), and some studies argue that when it comes to more severe care needs, such as nursing or inpatient hospital care, informal care is a complement to formal care; see Bonsang (2009) or Bolin et al. (2008). Nevertheless, with an aging population and an increasing demand for care, informal caregivers are 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 and Paulsen, 2009)–the amount of informal care is estimated to be of

[2] Fiva et al. (2014) argue that highly educated individuals (high SES) may be more able to navigate the public care provision bureaucracy to get proper care.

the same magnitude as the amount of the publicly provided formal care received by the elderly population (Berge et al., 2014).

Most informal care received by a frail elderly individual is provided by family members, usually the spouse or adult children (Vaage, 2000; Jakobsson et al., 2012). Informal care is, however, highly gendered. On the receiving end, mothers receive more care than fathers. However, this is likely because women tend to outlive their spouse, and thus, as men often die in a two-person household, women more often spend their last years widowed (Daatland et al., 2009). More notable is the gender difference in the provision of informal care: Female spouses provide more care than do male spouses, and daughters provide more help to their elderly parents than sons (Stark, 2005; Jakobsson et al., 2016).[3] Several potential explanations for these gender differences are given in the literature. For instance, women may have lower opportunity costs and/or a weaker connection to the labor market. A 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, and no, or a lower level, of formal care is assigned when daughters are present.

Several studies have focused on the effects of care responsibilities on various labor market outcomes, and findings include reduced work hours (Schmitz and Westphal, 2017; Bauer and Sousa-Poza, 2015; Kotsadam, 2012; Gautun and Hagen, 2010), lower earnings (Løken et al., 2017; Schmitz and Westphal, 2017; Heitmueller and Inglis, 2007), and a weaker connection to the labor market in general (Kotsadam, 2012; Leigh, 2010; Lilly et al., 2007).[4] Most notable for the present study, Løken et al. (2017) find that expanding formal care in Norway led to reduced sickness absence for daughters of single parents aged 80 and older. These results were especially strong for the subsample of daughters with no siblings, which is the group most likely to have a higher burden of care. There are two possible mechanisms for why caregiving affects sickness absence. First, as further elaborated in the next section, being a caregiver can have direct and indirect health implications. Second, a sickness absence can be used as a means of being granted paid leave from work to gain flexibility to provide care for a frail parent. Although all sickness absences longer than three days must be certified by a physician to prevent fraudulent use of leave, several studies confirm that this program has been used for other reasons other than an individual's sickness. Markussen et al. (2011) conclude that the sickness

[3]Interestingly, in a survey conducted by Jakobsson et al. (2012), men were more likely than women to report that they think family should provide care.

[4]Allthough some studies find that the labor market effects hold only for the more intensive caregivers (Lilly et al., 2007; Kotsadam, 2012), this is not true for all studies.

insurance system in Norway is a more general absence system, where physicians help individuals cope with demanding life situations by certifying sickness absences. This conclusion has been confirmed by interviews with physicians (Carlsen, 2008) and by survey data (Gautun, 2008).

2.2 Health Effects of Caregiving

A vast amount of empirical evidence documents negative correlations between caregiving and various health outcomes. Bauer and Sousa-Poza (2015) and Pinquart and Sörensen (2007) provide reviews of this literature and note that the focus is often on various psychological aspects, or mental health, such as stress, depression, anxiety, and dementia, of which depression is the most common outcome. Bauer and Sousa-Poza (2015) argue that caregiving could have mental health consequences because caregiving is time-consuming and often difficult to combine with work and family duties, and because caring for close relatives may induce negative emotions due to compassion and fear of loss. Moreover, Schulz and Sherwood (2008) note that caregiving is seen as having all the features of a chronic stress experience, as caregiving is usually accompanied by high levels of unpredictability. Having caregiving responsibilities has even been used as a model for studying chronic stress (Schulz and Sherwood, 2008).

In addition to the documented effects on psychological aspects, caregiving can affect physical health. This link could be explained through the mental health channel, as living with depression, or poor mental health in general, over substantial periods of time may manifest as physical health problems. Elevated stress over a longer time span has, for instance, been linked to hypertension and cardiovascular disease in the long run (Kivimäki and Steptoe, 2018; Pinquart and Sörensen, 2006; Capistrant et al., 2012; McEwen, 1998). In addition, care responsibilities may have direct effects on physical health. Some studies link care responsibilities directly to an increased risk of hypertension and cardiovascular disease (Von Känel et al., 2008; Capistrant et al., 2012). Bauer and Sousa-Poza (2015) argue that caregiving often includes physically demanding tasks over longer periods of time that in turn, may cause musculoskeletal injuries and aggravation of arthritis and other chronic conditions. Having care responsibilities can also require physical effort, especially in the case where the care recipient develops behavioral problems (Pinquart and Sörensen, 2006). Finally, caregivers tend to neglect physical exercise, eating healthy, or other factors associated with a healthy lifestyle (Bauer and Sousa-Poza, 2015; Capistrant et al., 2012).

Despite the well-assessed link between caregiving and health, there are several drawbacks with the existing literature. The evidence on physical health effects is still very limited, especially for more objective health outcomes. The majority of studies apply

survey data, where the representativeness of the sample and the fact that the health outcomes can be highly contextual are potential issues. Importantly, as discussed in the introduction, the endogeneity problem due to selection into caregiving is an issue seldom addressed (Leigh, 2010).

Among the evidence that accounts for endogeneity, Coe and Van Houtven (2009) find that long-term caregiving reduces self-rated health and increases depressive symptoms. The increase in depressive symptoms is persistent, and the long-term consequences are larger than the immediate consequences. The negative effect of caregiving on depression is supported, for instance, by Bauer and Sousa-Poza (2015). Schmitz and Westphal (2015) find short-term effects of care-giving on mental health but no effect on physical health. Finally, Do et al. (2015) apply parent-in-law's health limitations as instrumental variables and find negative effects on pain, self-rated health, and out-of-pocket health-care spending for daughter-in-law caregivers.

Gender differences are found in the health effects of caregiving. Female caregivers report higher rates of depression compared to men (Bauer and Sousa-Poza, 2015). However, this is partly due to women being far more exposed through longer and more intense care (Pinquart and Sörensen, 2006).

Finally, little is known about persistence or the long-term health effects of caregiving. Coe and Van Houtven (2009) argue that it takes some time for the effects of caregiving on health to manifest, and several studies stress the importance of assessing long-term outcomes (Leigh, 2010; Schmitz and Westphal, 2017).

In sum, theoretical and empirical studies have provided links between caregiving and health outcomes. This study contributes to the literature by assessing long-term effects on a number of these outcomes, in particular, musculoskeletal diagnoses, cardiovascular disease, psychological disorders in general, depression, hypertension, and lifestyle disease. Our identification strategy is based on arguably exogenous variation in caregiving following a formal care expansion. We rely on unique, high-quality individual-level register data that cover the entire Norwegian population. Further, in contrast to a large share of this literature, we apply objective health outcomes, measured as the diagnosis provided by a physician.

3 Formal Care in Norway and the 1998 Reform

With the emergence of the modern Norwegian welfare state in the 1960s, public responsibility for care of the young, frail, and elderly was expanded. The legal responsibility for elder care shifted from the family to the public sector in 1964, and during the 1970s, public expenditures on elder care more than doubled. Most of this expansion was in the form of support for home-care services, either in private homes or in adapted facilities.

Responsibility for elder care services gradually shifted from the central authorities to the municipalities during the 1980s. This was a period of decentralization, and municipalities were granted more autonomy in the provision of services to their elderly population. This 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.[5]

During the mid 1990s, the need for care expanded rapidly due to a growing elderly population resulting in declining coverage rates of home-based and institutionalized care for the elderly aged 80 and older. At the same time, the large differences in home-based care coverage rates across municipalities were seen as inequitable. This resulted in an action plan for elder care services.[6] The plan was implemented in January 1998, with a four-year implementation period. The main purpose of the action plan was to increase capacity in buildings and personnel, and to create a more equitable level of care services across municipalities (Brevik, 2001).[7] Explicit goals of the action plan included that all municipalities should be able to provide 24/7 assistance to at least 25 percent of their population aged 80 and older. Specifically, the plan was to increase the number of spaces in adapted apartments and institutions, and to increase the number of labor input in the home-based care sector nationwide by 6000 work years (Borge and 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 in 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, there is evidence suggesting that the municipalities with the lowest home-based care coverage rates were more likely to apply (Borge and 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, the share of the population aged 80 or older or aged 67 or older,

[5]Although municipalities have nearly full discretionary control over their budgets, certain amounts of public services have to be provided as all Norwegian citizens have a statutory right to basic welfare services regardless of where in the country they live.

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

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

respectively, and the municipal budget.

In Figure 1, we show that the levels of home-based care coverage rates are converging and almost levelling off in the post-reform period. Moreover, we show 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 A3, 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.

The 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 and Veenstra, 2012). However, Figure 1 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.[8] The reduced-form effects of the care expansion, thus, are likely to work through all of these channels.

4 Data

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.[9] 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. The sample must be seen as repeated cross sections as 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.

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

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

4.1 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 annum in the time period 1993-2014. Our treatment variable is the fraction of individuals aged 80 and older who receive formal home-based care, i.e., home-based care provided by the municipality.[10] 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.

4.2 Individual-Level Data

We apply rich, individual-level register data provided by Statistics Norway (SSB), which covers 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 benefits. 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.

4.2.1 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. A physician's certification is required for all sickness absence spells lasting longer than the allowed number of self-reported days.[11] 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.[12] 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

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

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

[12] Workers who are frequently absent may need certification from day 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.[13] Being employed is defined as having a 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.[14]

All absence certifications should be followed by at least one diagnosis reported by the physician who issued the certificate.[15] In addition to the indicator for sickness absence at all, we construct separate indicators for absences related to the two largest diagnoses categories following a sickness absence: namely musculoskeletal disorders (MSD), and psychological disorders. 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.

4.2.2 Long-Term Health Outcomes

For the long-term health 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 entail 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 assess long-term effects of caregiving on health, we lag the health outcomes by 11 years. As the health data are available for nine years (2006-2014), lagging the data by 11 years ensures that we have enough observations in the pre- and post-reform periods.[16] As illustrated in Figure A2 in the Appendix, this means that, for instance, for individuals in the sample in year 1995 we measure long-term health outcomes in 2006. For individuals in the sample in year 1996, long-term health outcomes are measured in 2007, and so on.

[13] 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.

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

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

[16] As a robustness check, we include an alternative where we lag the health outcomes by 10 years.

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, which can manifest 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.

5 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.[17] 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, more specifically, in the years 1993-1996. As noted in Section 3, municipalities with lower pre-reform coverage rates experienced a larger expansion in home-based care coverage from the pre- to the post-reform period. Importantly, we rely on informal care responsibilities being related to the amount of formal care provided in the municipalities.[18]

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:

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

[17] Our empirical strategy follows the strategy applied in Løken et al. (2017).

[18] This relationship, i.e., that individuals in municipalities with higher levels of formal care are less inclined to provide informal care, is confirmed by Jakobsson et al. (2012).

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 to 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.[19]

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 supported for extreme values of pre-reform coverage levels, we drop municipalities with extreme pre-reform coverage levels, defined as lower than the 10th percentile and higher than the 90th percentile.[20]

5.1 Multiple Hypothesis Testing

When testing a number of outcomes that are potentially correlated, and are 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 we correct for multiple hypothesis testing. For each outcome in our main tables, we include the p-value obtained using the stepwise 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.

5.2 Differences by Socioeconomic Status

When estimating the average effect on the entire group of daughters affected by the expansion, difference in responses across important subgroups may be concealed. Of particular interest in this setting is 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 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 had less than 10 years of education pre-reform

[19]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.

[20]The exclusion of these outliers is supported by Løken et al. (2017).

(measured in 1987).

5.3 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. Specifically, we start by excluding large cities and the most rural municipalities that might be very different from the rest of the sample. Further, 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. Moreover, 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.

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:

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

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. Specifically, 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 not to be affected by the care expansion. Here, we consider daughters with no siblings where both parents are deceased and those with younger parents (age 60-72). We exploit these various degrees of treatment exposure as a means to further ensure that the estimated effects stem from the care expansion.

Finally, our choice for the number of lags for the long-term health outcomes is restricted by the fact that only nine years of data are available, and that we need at least some years both before and after the reform to estimate the effects. However, it is not obvious by exactly how many years we should lag the outcomes, and this choice may potentially affect the results. For the final robustness check, we therefore provide results where the health outcomes have an alternative lag, namely, 10 years instead of 11 years.

6 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. All 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.[21] We start by presenting results for the short-term effects of the coverage expansion on sickness absence, and assess whether the effect on sickness absence is driven by a specific diagnosis group. We then assess the long-run health outcomes for the same sample. Next, we discuss heterogeneity by education, and finally, we discuss the robustness of the results.

6.1 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 a sickness absence at all. 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, namely, the two largest 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 towards 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. 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, i.e., 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

[21] An alternative version of the figures is to graph the absolute change from the pre- to the post-reform period in the outcome considered against the pre-reform coverage level. Although we do show this continuous version for the first stage figure, to avoid including too many graphs, we show only the binary version of the graphs, i.e., where we split municipalities into treatment and control groups, for the rest of the outcomes. The binary figures have the advantage of a more intuitive representation of the development of the outcomes, and the differences across municipalities with higher and lower pre-reform coverage levels. Additionally, the figures give us a way of visually inspecting the pre-trends.

first column) is statistically significant at the 5 percent level for both the phase-in period and the post-reform period. Specifically, we estimate that a 10 percentage point lower pre-reform coverage rate is related to a 14 percent reduction in the likelihood of a sickness absence among adult single-child daughters (a 3.0 percentage points reduction from a mean of around 21 percent).[22] As expected, the estimates are larger for the Post-ITT than for the Phase-in ITT effect.

As previously discussed, there are reasons to believe that a sickness absence is granted both due to own health and to gain flexibility to provide care for a frail parent. In this setting, it is therefore interesting to assess whether the reduction in sick leave is driven by a specific underlying diagnosis. We investigate this question in the following four columns of Table 2. Here, 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 percentage points from a mean of 9.2 percent, which translates into an effect of about 27 percent 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 percent reduction (a 1.1 percentage points reduction from a mean of 2.95 percent). 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.

[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 A5 and Table A14 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). These data are available only from 1995. Moreover, as our long-term health outcomes are available for only nine years, we need to limit the period under analysis further compared to Løken et al. (2017), and are left with the years 1995-2003.

[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.

6.2 Long-Term Health Outcomes

The graphical presentations of results for the long-term health outcomes are presented in Figure 3. As previously explained, for this part of the analysis the health outcomes are measured 11 years ahead. [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 3. 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 2.4 percentage points from a mean of 79.7 percent, which translates into an effect of 3 percent.[25] In the remaining columns of Table 3, we investigate the likelihood of the occurrence of specific diagnoses identified in the literature as potentially affected by caregiving responsibilities. Overall, we find little evidence of long-term health being affected by the care expansion. Specifically, we find no effects on the likelihood of the broad groups of MSD, psychological disorders and cardiovascular diseases, or on the more specific categories of depression and lifestyle related diseases. However, we find a statistically significant negative impact of the care expansion on the likelihood of having a hypertension diagnosis. The estimated effect is statistically significant at the 10 percent level for the Phase-in ITT and at the 5 percent level for the Post-ITT. The effect is also larger for the latter estimate. The Post-ITT shows a reduction in the likelihood of hypertension of almost 11 percent, a 2 percentage point decrease from a mean of 19 percent. However, when correcting for multiple hypothesis testing the calculated Romano-Wolf p-value becomes 0.235, and thus, the effect is not statistically significant at any conventional level.

6.3 Heterogeneity by Education

We then assess heterogeneity in the outcomes among daughters with different levels of education. In Tables 4 and 5, we present the effects estimated using Equation 1, where we include interaction terms between the treatment variables and an indicator for low education. The results for sickness absence and related diagnoses remains fairly similar

[24] We retain the same baseline sample of adult daughters. However, the number of observations differs slightly as we do not condition on employment, and we drop individuals who were dead in the years in which health outcomes are measured in this part of the analysis.

[25] In Table A4, we show that we find 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, e.g., as employed individuals need a physician to certify sickness absences.

to those in the main analysis for both educational groups. The effect for the group with high education is represented by the $PostITT$ and the effect for the low educated group is represented by the sum of the $PostITT$ and the $post \times loweduc$ (the p-value represents the joint statistical significance of the Post-ITT and the interaction term).

For the results for sickness absence the estimated reduction in the overall probability of an absence and in the probability of leaves of absence related to MSD and psychological disorders remains and is statistically significant for both educational groups (the p-value for the joint statistical significance of the Post-ITT and the interaction term is 0.095). The only statistically significant difference is that we estimate a slightly larger decrease in leaves of absence related to psychological disorders for the group with low education. While the estimated effect of 1 percentage point is still negative and statistically significant at the 5 percent level for the group with high education, we estimate a 1.3 percentage point decrease for the group with low education.

The heterogeneity results for the long-term health outcomes are presented in Table 5. We estimate a slight decrease in the probability of a primary care consultation for the group with low education, in contrast to the small increase for this outcome for the full sample. However, this effect is very close to zero (-0.2 percentage points).[26] The heterogeneity analysis further shows that the care expansion increased the risk of developing a depression diagnosis, which is not revealed in the main analysis. Specifically, we estimate a 0.2 percentage point increase for the group with low education.[27] Finally, the heterogeneity analysis shows that the estimated effect on hypertension seems to be driven by the group with higher education as we find an effect of 2 percentage points for this group (the same as in the full sample), whereas the p-value for the joint significance of the Post-ITT and the interaction term is 0.11 yielding a not statistically significant effect for the low education group.

6.4 Robustness

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). Finally,

[26] The table also shows a slight increase in the number of primary care consultations. Though this increase of 0.02 consultations is statistically significant, it is also fairly close to zero considering the average number of consultations is about 3.6.

[27] Albeit rather small, this finding contrasts with the existing empirical evidence in the literature on the health effects of having care responsibilities.

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.

The specification checks for sickness absence spells are shown in Table A5, and for the long-term health outcomes, results are found in Table A6. The estimated decrease in the probability of sickness absences remains relatively robust, except in column 2 where we exclude large cities. Here, the estimate is 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, 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.

Next, we estimate the reform effects on alternative samples: all daughters and 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 Tables A7 and A8. 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. The results for the long-term health outcomes for the alternative samples, presented in Table A8, provide the same picture. Here, too, we find no effects on the sample of sons with no siblings. For all daughters, we estimate statistically significant, yet smaller, effects of the care expansions on the likelihood of a primary health-care consultation and on the likelihood of a hypertension diagnosis 11 years later.[28] On average, this group of daughters is likely to have a smaller care burden than the daughters with no siblings, i.e., lower treatment intensity. Thus, finding results that vary with the potential treatment intensity reassures us that the observed effects stem from reduced caregiving.

We further perform placebo tests by estimating the effects on daughters with no

[28] An estimated increase in the risk of MSD also turns up as statistically significant at the 10 percent level for daughters with siblings. After correcting for multiple hypothesis testing, the effect is no longer statistically significant.

siblings where both parents are deceased and on those with younger parents (aged 60-72). The results for the placebo tests are presented in Tables A9 and A10. As seen in the first table, we find no statistically significant effects of the care expansion on the probability of sickness absence. For the long-term outcomes, we estimate a slightly significant increase in the risk of a cardiovascular disease and of a lifestyle-related disease for the group of daughters with no living parents. 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 two alternative approaches. First, we exclude the intermediate period and treat all years after the reform as post-reform years. Second, we lag the health outcomes by 10 years instead of 11. Results for the specification excluding the intermediate period are presented in Tables A11 and A12. We see that excluding the intermediate period 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. For the long-term health outcomes in Table A12, we no longer estimate the increase in the likelihood of a primary care consultation, as in the main results, but we still find a reduction in the risk of a hypertension diagnosis.

Results from the specification where we apply different lags to the health outcomes are shown in Table A13. Again, we find similar results as in the baseline specification. Overall, there is not much evidence pointing toward long-term health effects of the care expansion, except from a small increase in the probability of a primary care consultation and a decrease in the likelihood of a hypertension diagnosis (here 10 years after).

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

7 Discussion and Conclusion

Summing up the main findings of this paper, we show that expanding 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. This finding is in line with the previous findings in Løken et al. (2017). We expand this analysis and show that the decrease in

absences seems to be driven by leaves of absence related to MSD and psychological disorders. These findings remain statistically significant at conventional levels when correcting for multiple hypothesis testing, and they are robust to a range of specification checks. In addition, results for the placebo tests as well as when estimated on alternative samples increase our confidence in these results.

Although the results show that the reduction in leaves of absence is driven by MSD and psychological disorders, this does not necessarily imply reduced own illness alone. An early study of physicians' certifying practice in Norway suggests that the diagnosis stated on the absence certificate is a good reflection of a person's health (Tellnes et al., 1989), and the ICPC-2 coding of absence certifications conducted by Norwegian physicians has been judged by Brage et al. (1996) to be of satisfactory quality. However, several studies have documented that the sickness insurance system in Norway is used partly to insure employees against a broad range of circumstances, other than own illness (Markussen et al., 2011; Carlsen, 2008; Gautun, 2008). Moreover, in recent years there has been a tendency toward increased sickness absence certifications based on more subjective complaints and diffuse symptoms (Brage et al., 2013). The explanations provided include assertions that may imply that the diagnoses related to a sickness spell do not necessarily reflect individual health perfectly.[29]

In the second part of the analysis, we assess the effects of expanding publicly provided elder care on long-term health outcomes among the sample of daughters. Overall, we find little evidence of long-term health outcomes being affected by reducing the care burden. We investigate long-term effects on primary care consultations and the likelihood of having specific diagnoses detected in the literature as potentially affected by care responsibilities. These diagnoses are MSD, psychological, or cardiovascular disorders, depression, hypertension, and, finally, lifestyle related disorders. We find no statistically significant effects of the care expansion on the long-term health outcomes, except hypertension, where we estimate an 11 percent reduction in the risk of a hypertension diagnosis. To the extent that we can rely on this finding, it can be explained by lower stress levels following a reduced care burden.[30] Despite the well-established link between caregiving and health, we document no causal effects of caregiving on some of the most common health outcomes in this literature, such as depression, psychological disorders, and MSD in the longer term. As a closing comment, given that we estimate short-term effects

[29]The explanations discussed by Brage et al. (2013) include that a diagnosis given by a physician may have consequences for an individual, e.g., when purchasing health insurance, and that this makes physicians more careful in their diagnostic practice. A further explanation is that at the initial consultation the diagnosis given often relies primarily on the patients' own complaints, and that some physicians do not update the diagnoses later when the absence is expanded.

[30]The result on hypertension is robust to a range of specifications, but not to correcting for multiple hypothesis testing, which means that this result needs to be interpreted with caution.

on sickness absence, an explanation for why we find essentially no effects on long-term health outcomes could be that sickness absences have been used as a preventive measure to avoid severe illness in the future.

Conflict of Interest

Conflict of Interest: The authors declare that they have no conflict of interest.

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Figures

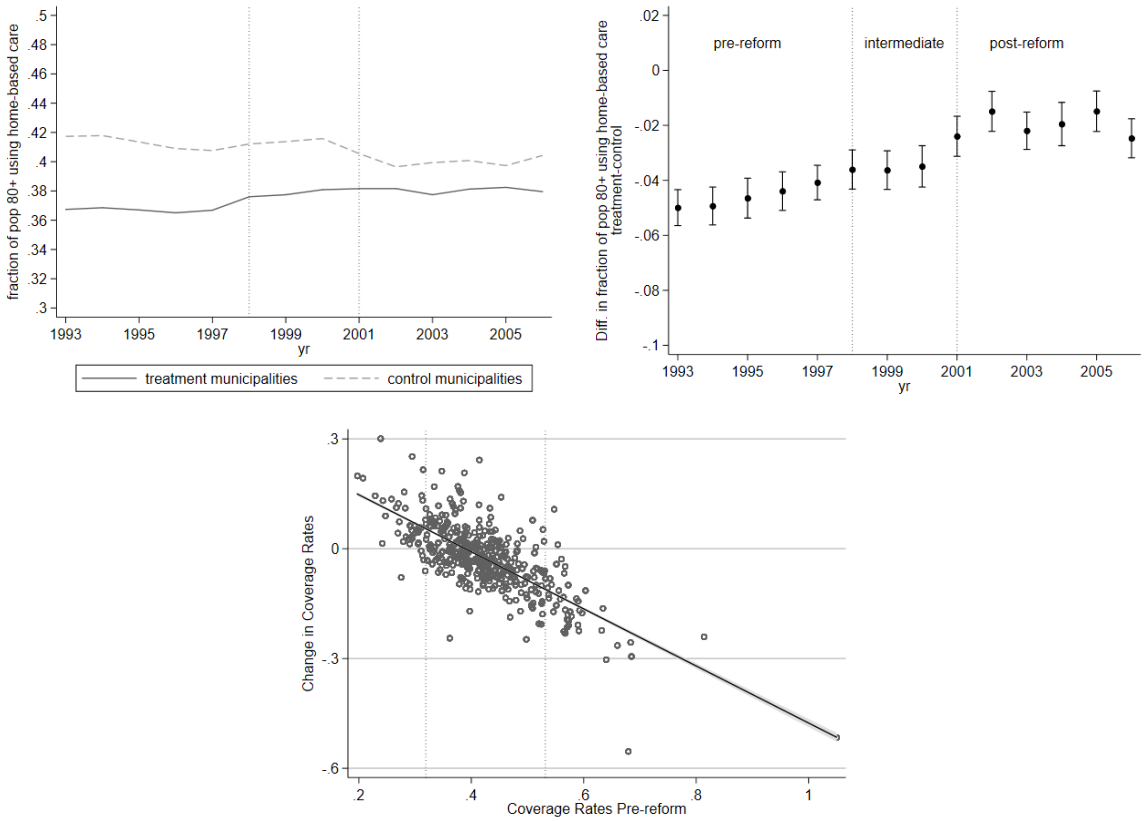

Figure 1: Home Based Care – Coverage Rates

Note: Home based care is defined as care at home or in adapted facilities. In **the first two figures**, 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 differences per year in the fraction of the 80+ population using home based care in treated vs. 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).

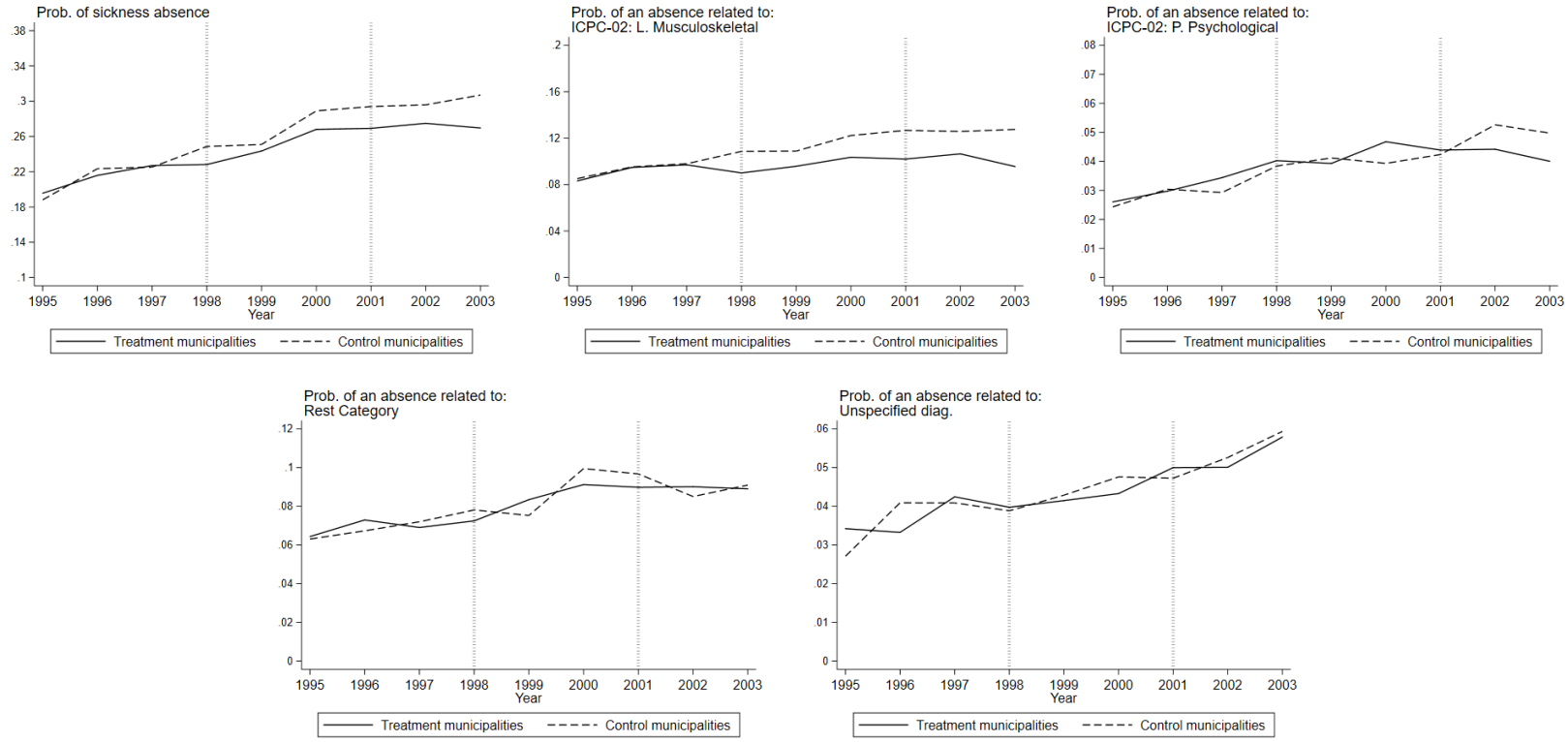

Figure 2: Probability of a Sickness Absence, 1995-2003

Note: The graphs show the probability of a sickness absence spell (cond. on employment), and spells realted 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).

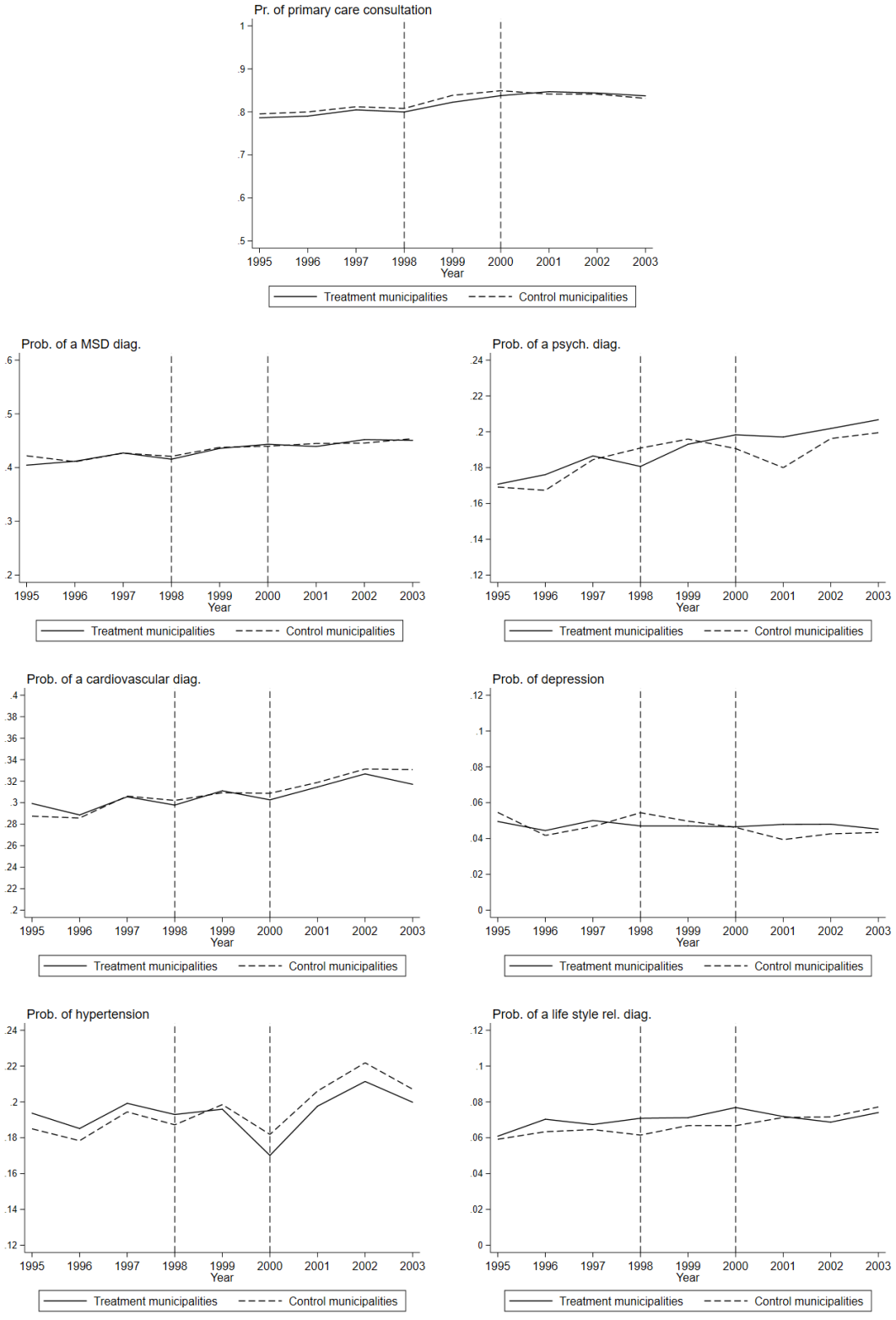

Figure 3: Long-Term Health Outcomes: Utilization of Primary Health Care Services

Note: 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).

Tables

Table 1: Post-Reform Growth in Home Care Coverage Rates

	Abs change in home care cov. rates (post - pre)	
	(1)	(2)
(-) Homecare coverage rate, pop. 80+ (1993-1997) (scaled by 10)	0.068***	0.078***
	(0.0070)	(0.0042)
(-) Inst. based care coverage rate, pop. 80+ (1993-1997) (scaled by 10)	0.001	0.003
	(0.0075)	(0.0069)
Share of pop 67yrs+	0.070	0.032
	(0.1999)	(0.2013)
Share of pop 80yrs+	-0.056	0.040
	(0.5544)	(0.5572)
Munic. unrestricted budget per capita	0.000	-0.000
	(0.0000)	(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, e.g. 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.

Table 2: Main Results: 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	[.214]	[.0924]	[.0295]	[.0685]	[.0368]
Romano-Wolf p-value	0.096	0.028	0.036	0.857	0.920
Obs.	57753	57753	57753	57753	57753

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.

Table 3: Main Results: Long-Term Health Outcomes

	Prim. care consultations:		Specific diagnoses (groups):					
	Prob.	No.	MSD	Psych.	Cardio.	Depr	Hyper-tension	Life style
Phasein ITT	0.004	-0.037	0.007	-0.007	-0.012	-0.002	-0.015*	0.008
	(0.0098)	(0.0940)	(0.0099)	(0.0089)	(0.0108)	(0.0050)	(0.0082)	(0.0060)
Post ITT	0.024**	0.058	0.017	0.009	-0.014	0.005	-0.020**	0.001
	(0.0110)	(0.1019)	(0.0121)	(0.0106)	(0.0118)	(0.0055)	(0.0100)	(0.0066)
Mean	[.797]	[3.62]	[.417]	[.177]	[.296]	[.0478]	[.19]	[.0649]
Romano-Wolf p-value	0.235	0.924	0.745	0.892	0.745	0.892	0.235	0.924
Obs.	73963	73963	73963	73963	73963	73963	73963	73963

Note: Health outcomes are lagged by 11 years. 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.

Table 4: Heterogeneity by Education: Sickness Absence – Main Categories

	Prob. of sickness absence				
	At all	MSD	Psych	Rest category	Unspecified diag.
Post ITT	-0.031**	-0.027***	-0.010**	-0.004	-0.001
	(0.0147)	(0.0090)	(0.0040)	(0.0074)	(0.0048)
post * low educ	-0.002	0.004	-0.003**	-0.000	-0.001
	(0.0039)	(0.0027)	(0.0015)	(0.0021)	(0.0019)
Joint sign. p-value	.0953	.00442	.00442	.831	.766
Obs.	57531	57531	57531	57531	57531

Note: In the first row an indicator for Post (2001-2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The second row shows the coefficients where we interact Post with the negative pre-reform coverage and an indicator for low education, defined as education below 10 years. An interaction with the phase-in period (1999-2000) is included, but not shown in the 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 5: Heterogeneity by Education: Long-Term Health Outcomes

	Prim care consultations:		Specific diagnoses (groups):					
	Prob.	No.	MSD	Psych	Cardio.	Depr.	Hyper-tension	Life style
Post ITT	0.078	0.024**	0.018	0.009	-0.015	0.005	-0.020**	-0.000
	(0.1012)	(0.0110)	(0.0124)	(0.0106)	(0.0119)	(0.0056)	(0.0099)	(0.0067)
post * low educ	-0.080**	0.000	-0.004	-0.002	0.000	-0.003**	-0.002	0.002
	(0.0379)	(0.0023)	(0.0039)	(0.0024)	(0.0048)	(0.0014)	(0.0042)	(0.0021)
Joint sign. p-value	.105	.0784	.286	.433	.459	.0614	.112	.561
Obs.	73647	73647	73647	73647	73647	73647	73647	73647

Note: In the first row an indicator for Post (2001-2003) is interacted with the negative pre-reform coverage rate, scaled by 10. The second row shows the coefficients where we interact Post with the negative pre-reform coverage and an indicator for low education, defined as education below 10 years. An interaction with the phase-in period (1999-2000) is included, but not shown in the 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.

Appendix

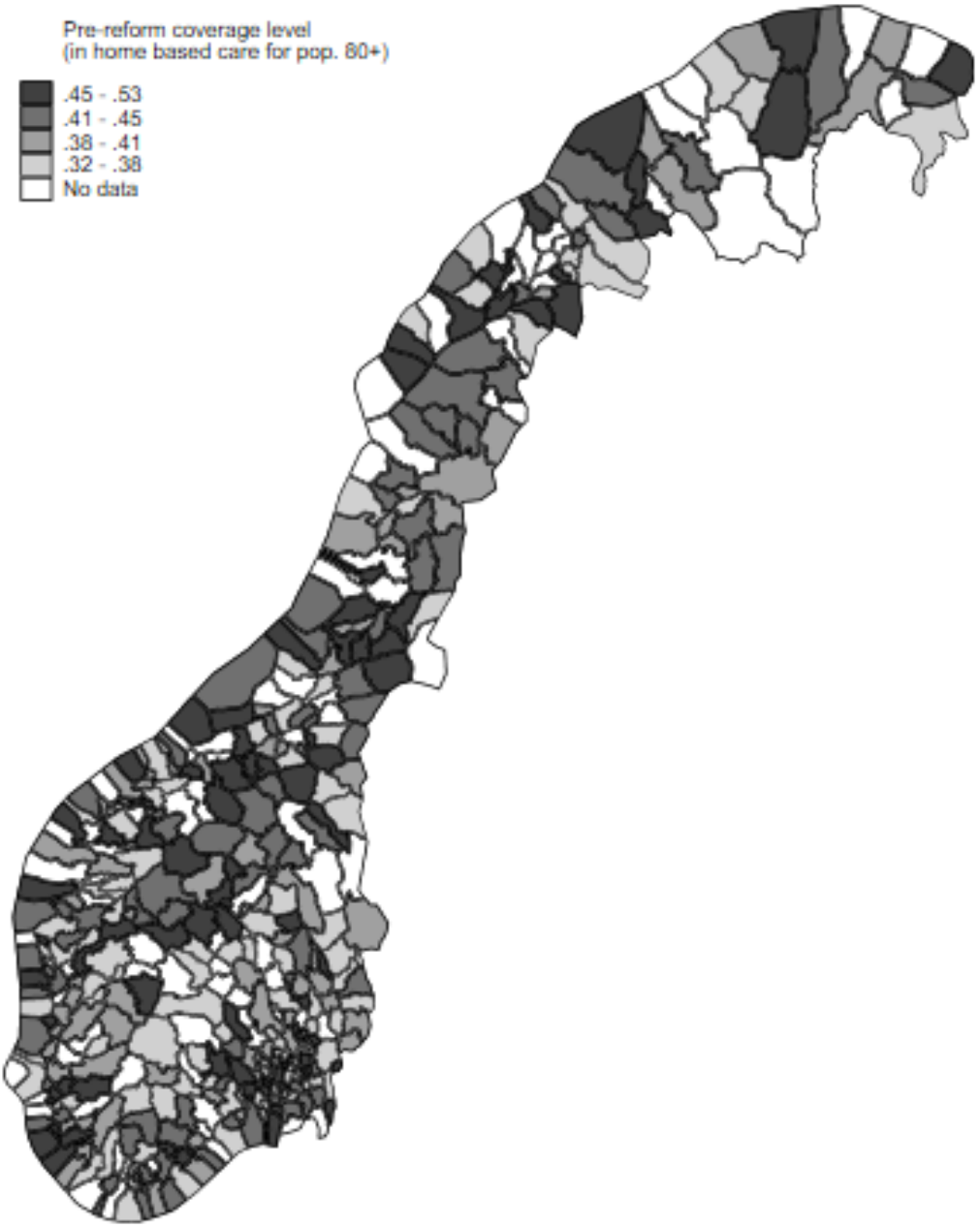

Figure A1: Regional Variation in Pre-Reform Coverage Rates

Note: The map shows variation in pre-reform home care coverage rates across municipalities. Municipalities not in the preferred sample are those with pre-reform home care coverage rates falling below the 10th or above the 90th percentiles of the pre-reform coverage distribution.

Table A1: Municipality Characteristics (1997 numbers)

	Municipalities with pre-reform home based care coverage:	
	Below median	Above median
Total population	12892	8849
	(39619)	(19397)
Share of pop 67yrs+	.159	.157
	(.0334)	(.0368)
Share of pop 80yrs+	.0483	.0486
	(.0146)	(.0156)
Share of males 16+ with compulsory schooling	.374	.386
	(.0764)	(.0789)
Share of females 16+ with compulsory schooling	.424	.435
	(.0732)	(.079)
Share of males 16+ with high school	.47	.473
	(.0507)	(.0531)
Share of females 16+ with high school	.413	.413
	(.0462)	(.0542)
Share of males 16+ with university degree	.11	.103
	(.0345)	(.0323)
Share of females 16+ with university degree	.14	.133
	(.0367)	(.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	22641	23233
	(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+)	.362	.411
	(.0661)	(.0892)
- Pre reform coverage rate (1993-1997)	.373	.459
	(.0257)	(.0334)
- Change from pre to post	.00893	-.0478
	(.0654)	(.0649)
Inst. care coverage rate (pop. 80+)	.193	.192
	(.0659)	(.0632)
- Pre reform coverage rate (1993-1997)	.214	.205
	(.0652)	(.0588)
- Change from pre to post	-.0422	-.0382
	(.0415)	(.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.

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	T07	Weight gain
related diseases	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.

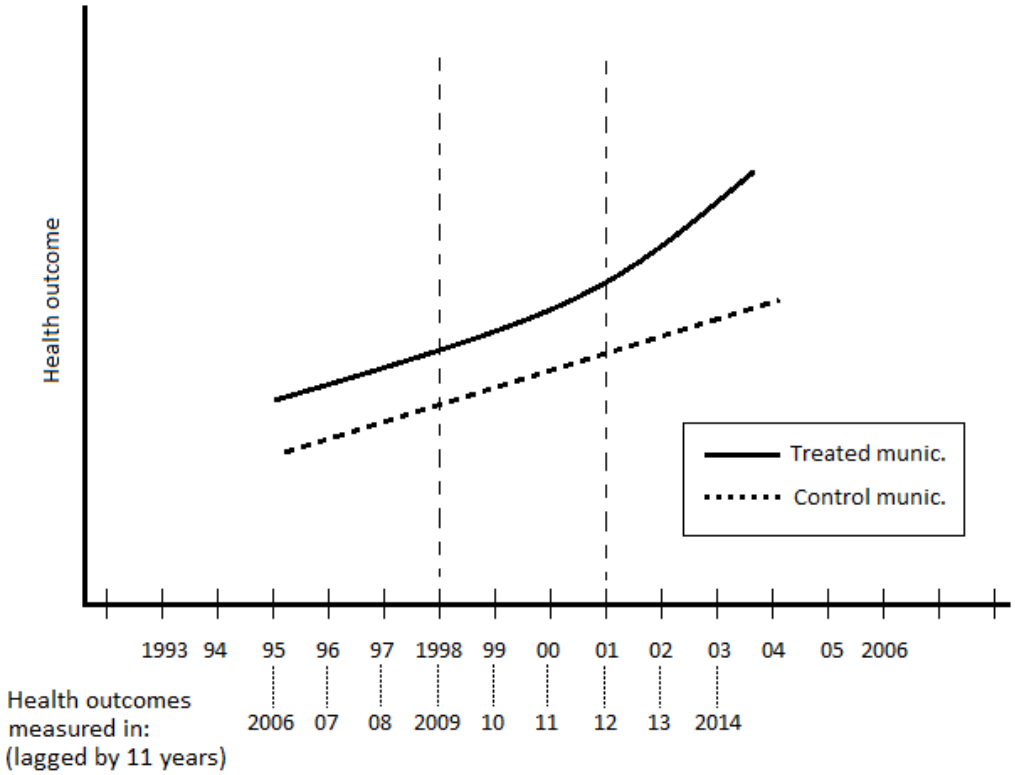

Figure A2: Illustration of the Long-Term Outcomes Design

Note: The figure is an illustration of how our long-term health outcomes are measured. In each year in the period 1995-2003 our sample entails daughters with no siblings who have only one living parent at least 80 years old in the particular year. Long-term health outcomes are measured 11 years later, e.g. for the 1995 sample, long-term outcomes are measured in 2006.

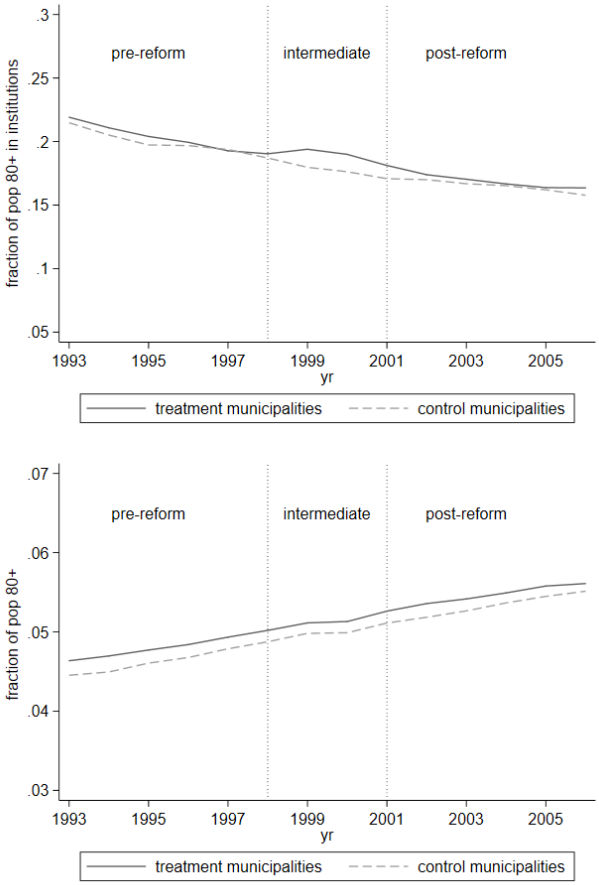

Figure A3: Other Municipality Characteristics

Note: 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).

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	[.00847]	[.0215]	[.0223]	[.019]	[.0615]	[.0145]
Obs.	57753	57753	57753	57753	57753	57753

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.

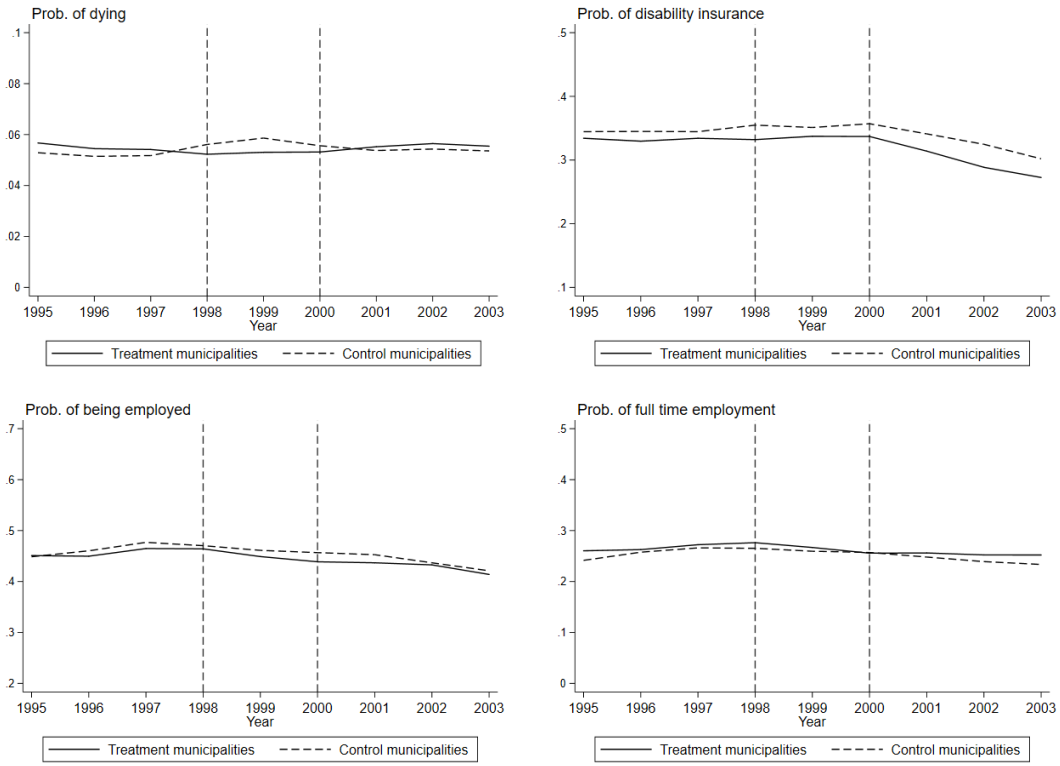

Figure A4: Additional Long-Term Outcomes
Work, disability, and mortality

Note: The graphs show results on long-term outcomes for daughters with no siblings and with only one living parent at least 80 years old. Treatment is defined as falling below the median home based care coverage rate in the pre-reform period, defined as 1993-1997. The vertical line represent the reform year 1998. 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 A4: Additional Long-Term Outcomes – Employment, Disability, and Mortality

	Probability of:			
	Dying	Disability insurance	Employment	Full time employment
Phasein ITT	-0.007	-0.004	0.004	0.001
	(0.0049)	(0.0104)	(0.0081)	(0.0091)
Post ITT	-0.002	-0.009	-0.003	-0.001
	(0.0067)	(0.0124)	(0.0104)	(0.0122)
Mean	[.0539]	[.337]	[.458]	[.262]
Obs.	78225	73963	73963	73963

Note: The table show estimated effects of expanding formal eldercare on long-term labor market outcomes. The outcomes are lagged by 11 years. 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	Excl. large cities	Excl. rural munic.	Drop 1997	Incl. 1997 as post	Split at median coverage
Sickness absence at all	-0.030**	-0.024	-0.036**	-0.033*	-0.030**	-0.031***
	(0.0146)	(0.0148)	(0.0155)	(0.0169)	(0.0146)	(0.0110)
Related Diagnoses:						
Musculoskeletal	-0.025***	-0.021**	-0.027***	-0.026**	-0.025***	-0.026***
	(0.0090)	(0.0092)	(0.0097)	(0.0106)	(0.0090)	(0.0064)
Psychological	-0.011***	-0.011***	-0.013***	-0.009**	-0.011***	-0.008**
	(0.0040)	(0.0044)	(0.0042)	(0.0043)	(0.0040)	(0.0037)
All other diagnoses	-0.004	-0.002	-0.007	-0.008	-0.004	-0.003
	(0.0073)	(0.0073)	(0.0077)	(0.0085)	(0.0073)	(0.0064)
No specified diagnosis	-0.001	0.000	-0.001	-0.000	-0.001	-0.001
	(0.0048)	(0.0053)	(0.0050)	(0.0053)	(0.0048)	(0.0038)
Obs.	57753	43292	52770	51878	57753	57753

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: Long-Term Health Outcomes

	Daughters, no siblings					
	Baseline	Excl. large cities	Excl. rural munic.	Drop 1997	Incl. 1997 as post	Split at median coverage
Primary health care cons.:						
Prob. of cons.	0.024**	0.029**	0.023*	0.029**	0.024**	0.012
	(0.0110)	(0.0114)	(0.0119)	(0.0129)	(0.0110)	(0.0101)
No. of cons.	0.058	0.116	0.028	0.065	0.058	-0.040
	(0.1019)	(0.1032)	(0.1084)	(0.1116)	(0.1019)	(0.0973)
Prim. health care cons. related to specific diagnosis groups:						
MSD	0.017	0.023*	0.010	0.017	0.017	0.002
	(0.0121)	(0.0121)	(0.0125)	(0.0135)	(0.0121)	(0.0110)
Psych.	0.009	0.013	0.002	0.007	0.009	0.006
	(0.0106)	(0.0112)	(0.0106)	(0.0122)	(0.0106)	(0.0096)
Cardiovascular	-0.014	-0.014	-0.018	-0.020	-0.014	-0.013
	(0.0118)	(0.0128)	(0.0126)	(0.0131)	(0.0118)	(0.0105)
Depression	0.005	0.003	0.003	0.008	0.005	0.004
	(0.0055)	(0.0058)	(0.0058)	(0.0059)	(0.0055)	(0.0049)
Hypertension	-0.020**	-0.022**	-0.021**	-0.025**	-0.020**	-0.016*
	(0.0100)	(0.0108)	(0.0105)	(0.0106)	(0.0100)	(0.0084)
Life style related	0.001	0.002	0.003	0.002	0.001	-0.004
	(0.0066)	(0.0071)	(0.0072)	(0.0071)	(0.0066)	(0.0054)
Obs.	73963	56474	67372	66427	73963	73963

Note: Health outcomes are lagged by 11 years. 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)
	[.214]	[.213]	[.154]
Related Diagnoses:			
Musculoskeletal	-0.025***	-0.002	-0.003
	(0.0090)	(0.0046)	(0.0058)
	[.0924]	[.0935]	[.0643]
Psychological	-0.011***	-0.002	0.001
	(0.0040)	(0.0022)	(0.0031)
	[.0295]	[.0287]	[.0162]
All other diagnoses	-0.004	-0.002	-0.003
	(0.0073)	(0.0030)	(0.0054)
	[.0685]	[.0684]	[.0538]
No specified diagnosis	-0.001	0.001	-0.001
	(0.0048)	(0.0022)	(0.0034)
	[.0368]	[.0356]	[.027]
Obs.	57753	290863	89576

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: Alternative Samples: Long-Term Health Outcomes

	Daughters		Sons
	No siblings	All	No siblings
Primary health care cons.:			
Prob. of cons.	0.024**	0.013*	-0.002
	(0.0110)	(0.0078)	(0.0104)
	[.797]	[.793]	[.715]
Number of cons.	0.058	0.032	-0.060
	(0.1019)	(0.0534)	(0.1103)
	[3.62]	[3.51]	[3.06]
Prim. health care cons. related to specific diagnosis groups:			
MSD	0.017	0.011*	-0.003
	(0.0121)	(0.0057)	(0.0095)
	[.417]	[.412]	[.297]
Psych.	0.009	0.001	0.000
	(0.0106)	(0.0050)	(0.0071)
	[.177]	[.169]	[.111]
Cardiovascular	-0.014	-0.002	-0.012
	(0.0118)	(0.0060)	(0.0112)
	[.296]	[.283]	[.343]
Depression	0.005	0.002	-0.000
	(0.0055)	(0.0028)	(0.0033)
	[.0478]	[.047]	[.0273]
Hypertension	-0.020**	-0.011**	-0.005
	(0.0100)	(0.0048)	(0.0089)
	[.19]	[.177]	[.187]
Life style related	0.001	-0.000	-0.000
	(0.0066)	(0.0027)	(0.0061)
	[.0649]	[.0586]	[.0877]
Obs.	73963	357934	97186

Note: Health outcomes are lagged by 11 years. 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: 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)
	[.228]	[.244]
Related Diagnoses:		
Musculoskeletal	0.003	-0.020
	(0.0078)	(0.0275)
	[.0977]	[.108]
Psychological	0.002	-0.006
	(0.0040)	(0.0176)
	[.0285]	[.0378]
All other diagnoses	-0.010	0.014
	(0.0079)	(0.0210)
	[.0777]	[.0797]
No specified diagnosis	-0.007	-0.021
	(0.0048)	(0.0143)
	[.0385]	[.035]
Obs.	70753	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 A10: Placebos: Long-Term Health Outcomes

	Daughters, no siblings	
	No living parents	Younger parents
Primary health care cons.:		
Prob. of cons.	0.006	-0.033
	(0.0114)	(0.0346)
	[.809]	[.781]
Number of cons.	0.044	-0.122
	(0.1113)	(0.3896)
	[3.93]	[3.64]
Prim. health care cons. related to specific diagnosis groups:		
MSD	0.009	0.004
	(0.0120)	(0.0412)
	[.426]	[.413]
Psych.	0.003	0.027
	(0.0090)	(0.0380)
	[.178]	[.213]
Cardiovascular	0.018*	-0.010
	(0.0109)	(0.0374)
	[.356]	[.261]
Depression	0.001	0.016
	(0.0046)	(0.0245)
	[.0482]	[.0621]
Hypertension	0.009	0.001
	(0.0103)	(0.0303)
	[.228]	[.172]
Life style related	0.013**	-0.018
	(0.0051)	(0.0231)
	[.076]	[.0754]
Obs.	105503	8144

Note: Health outcomes are lagged by 11 years. 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 A11: No Intermediate 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	[.214]	[.0924]	[.0295]	[.0685]	[.0368]
Obs.	57753	57753	57753	57753	57753

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 A12: No Intermediate Period: Long-Term Health Outcomes

	Prim. care consultations:		Specific diagnoses (groups):					
	Prob.	No.	MSD	Psych.	Cardio.	Depr	Hyper-tension	Life style
Post ITT	0.014	0.012	0.012	0.001	-0.013	0.001	-0.018**	0.005
	(0.0098)	(0.0921)	(0.0098)	(0.0090)	(0.0104)	(0.0048)	(0.0081)	(0.0057)
Mean	[.797]	[3.62]	[.417]	[.177]	[.296]	[.0478]	[.19]	[.0649]
Obs.	73963	73963	73963	73963	73963	73963	73963	73963

Note: Health outcomes are lagged by 11 years. 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 A13: Alternative Lags: Long-Term Health Outcomes – Primary Health Care Services

	Prim. care consultations:		Specific diagnoses (groups):					
	Prob.	No.	MSD	Psych.	Cardio.	Depr	Hyper-tension	Life style
Phasein ITT	0.013	-0.033	0.002	-0.010	-0.015	0.001	-0.007	0.002
	(0.0100)	(0.0884)	(0.0105)	(0.0094)	(0.0121)	(0.0051)	(0.0094)	(0.0059)
Post ITT	0.027**	0.029	0.016	0.007	-0.019	0.008	-0.017*	-0.001
	(0.0120)	(0.1112)	(0.0121)	(0.0109)	(0.0124)	(0.0056)	(0.0101)	(0.0070)
Mean	[.791]	[3.56]	[.41]	[.174]	[.279]	[.0477]	[.179]	[.0644]
Obs.	77324	77324	77324	77324	77324	77324	77324	77324

Note: The table show estimated effects on long-term health outcomes when outcomes are lagged by 10 years (we lag by 11 years in the main specification). 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.

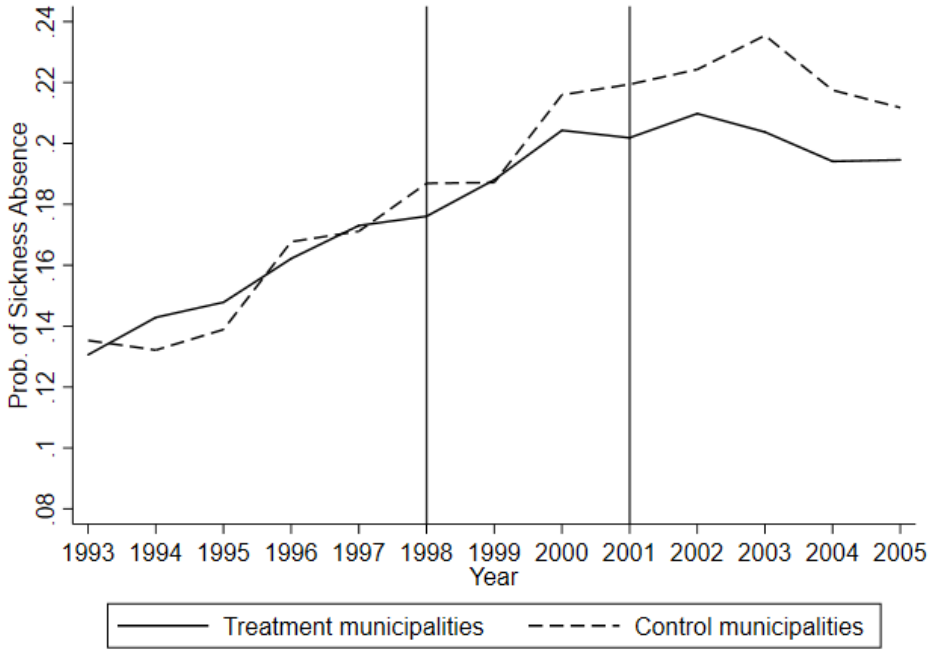

Figure A5: Replication Results on Sickness Absence (16 days+)

Note: 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 A14: 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)
	[.146]	[.194]	[20.4]
Obs.	101785	70948	70948

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.

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