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ABSTRACT

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In this paper, we use a 1998 reform in the federal funding of local home-based care for the elderly in Norway to examine the effects of formal care expansion on the labor supply decisions and mobility of middle-aged children. Our main finding is a consistent and significant negative impact of formal care expansion on work absences longer than 2 weeks for the adult daughters of single elderly parents. This effect is particularly strong for daughters with no siblings, and this group is also more likely to exceed earnings thresholds after the reform. We find no impacts of the reform on daughter's mobility or parental health, and no effects on adult sons. Our results provide evidence of substitution between formal home-based care and informal care for the group that is most likely to respond to the parent's need for care – adult daughters with no siblings to share the burden of parental care. These results also highlight the importance of labor market institutions that provide flexibility in enabling women to balance home and work responsibilities.

JEL Classification: J14, J22

Keywords: formal and informal care, elderly, welfare state, women's career

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1 Introduction

Rapidly aging populations are posing public policy challenges to societies around the world. The fiscal and personal burdens that care of the elderly place on a working-age population that is shrinking in relative size are of particular concern to policy-makers. Expanding state care is expensive but may, if it substitutes for informal care provided by adult children, increase hours worked and labor force participation by middle-aged children and permit them to move in pursuit of labor market opportunities. This substitution, although it has potential implications for the quality of care, may partially offset the fiscal impact of public care responsibilities by increasing the tax base. It is important to understand the degree of substitution between formal (state-provided or purchased) and informal (family-provided) elder care, and the effect that care responsibilities have on both the labor market outcomes and mobility of adult children and the health of the elderly.

Estimating the causal effect of formal elder care on the labor market outcomes of adult children is difficult due to the endogeneity of formal care take-up. Parents who receive formal care are usually older and less healthy than those who do not, and comparing the behavior of their children does not take account of unobserved characteristics that affect both formal care choices and informal care responsibilities. Therefore, we use a 1998 reform in the federal funding of local home-based care for the elderly in Norway to examine the effects of an expansion of formal care availability on the labor supply decisions and mobility of middle-aged children. The goals of this reform included equalizing the availability of care services across municipalities, and it resulted in arguably exogenous variation in the degree to which formal care services expanded across localities.

We estimate difference in differences (DinD) models that exploit the differential post-reform availability of federal funds in municipalities with different pre-reform levels of care coverage. The federal grants program initiated in 1998 caused a larger expansion of home-care provision in municipalities that initially had low coverage rates. Since the actual expansion of care facilities in each municipality may be correlated with labor market conditions that also affect our outcome variable, we use the pre-reform coverage level as an indicator of the actual supply shock faced by the local authorities. To define treatment and control groups, municipalities are ordered according to their average level of home-based care coverage in 1993-1996, with municipalities below the median level of coverage classified as *treatment municipalities*, and municipalities with high pre-reform coverage considered *control municipalities*.

We show that the results are robust to different definitions of treatment and control municipalities, including an alternative specification in which we use linear pre-reform coverage as the indicator. The first stage effects on relative increases in coverage are in line with the intentions of the reform, with coverage converging post-reform and the average treatment-control gap almost disappearing in the longer term.

Our main sample is cross sectional and consists of daughters with only one surviving parent who is at least 80 years old. Since the primary caregiver for frail elderly who are married is usually the spouse (OECD, 2005; Kalwij et al., 2012), this restriction yields a sample of adult daughters who are more likely to have parental care responsibilities. We then narrow the focus further to daughters with no siblings to share this burden of care with.

We explore a number of different potential effects of this reform on both adult children and elderly parents. First, the labor supply or location decisions of adult children may be affected by the increased supply of formal care. Second, reform-related changes in the quality or intensity of care could affect the health of elderly parents. We find no evidence of extensive-margin labor supply responses to formal care expansion: there are no significant effects on employment, receipt of a disability pension, or migration to another municipality. We do, however, find significant positive impacts of the policy reform on the intensive margin of labor supply for daughters with single elderly parents. Daughters are more likely to pass administratively-set earnings thresholds, suggesting increases in hours of work for those already in the labor market, and there are strong and significant negative effects on daughters' use of insured sickness absence from work. We find no effects of formal care expansion on the labor supply of sons.

The common trend assumption for the DinD models is supported by a comparison of time series in all outcomes for the treatment and control communities. Our results are robust to an extensive battery of specification checks, including exclusion of the largest cities and the smallest rural communities, inclusion of extreme values of the initial coverage rates, and differential treatment of the policy transition period. We also run placebo tests using different groups of daughters less likely to be affected by the reform and find no effects with the exception of a negative, marginally significant effect on sickness absence for those with no living parents. Further analysis suggests that the latter result is driven by a set of women likely to be responsible for the care of an elderly parent-in-law. An additional placebo test changes the timing of the reform and provides further support for the common trend assumption.

Insured sickness absences require a doctor's certification and our results are con-

sistent with other evidence that, in Norway, this social insurance program is being used for reasons other than own diagnosed illness (Markussen et al., 2011; Fevang et al., 2011). A lack of temporal flexibility in employment (or the presence of large wage penalties for such flexibility) is particularly disadvantageous to women (Goldin, 2014) and sickness absence appears to act as an institutional source of such flexibility that permits many women to deal with domestic responsibilities related to elder care. Our findings indicate that the principal impact of Norway's expansion of formal care has been a reduced reliance on this source of work leave by adult women with an elderly parent.

2 Literature review

Most of the personal care received by disabled adults and the frail elderly is informal–provided by family, friends, and neighbors rather than by professional caregivers in the public sector or hired in the market (OECD, 2005). A recent U.S. survey found that 27 percent of adults reported caring for another adult in the preceding 12 months. The amount of time devoted to care varied with the needs of the recipient and the availability of other care providers. Half of caregivers reported spending 8 hours or less per week on care, while 11 percent spent more than 40 hours per week in caregiving activities (NAC and AARP, 2009). Spouses are the most important source of elder care, followed by adult children, though a study of the SHARE data found that other relatives and friends provide as much home care to the elderly as children (Kalwij et al., 2012).

Even in countries that provide comprehensive social services, such as Norway, time use data shows that spending time caring for elderly parents is very common, even for working adults. On an average work day in 2000, 8 percent of the working population spent an average of 1.2 hours taking care of a parent (Vaage, 2002). Among 45 to 65 year olds who have at least one parent still alive, 70 percent report that they combine work for pay and the provision of informal care to their parents (Gautun, 2008). Adult children may assist their elderly parents because formal (public) services are inadequate or incomplete, or because they place direct value on such interactions with their parents. Whatever the motivation, the time and energy devoted to taking care of elderly parents will take the place of other activities such as market work and leisure. Intensive caregiving of frail or disabled parents may impose high costs on the caregiver, including loss of employment, reduced wages, and restricted mobility.

The association between informal caregiving and labor market outcomes has been

extensively studied, but the likely selection of individuals with inferior labor market opportunities into care has made it difficult to establish causal effects. A recent survey, based primarily on studies using U.S. or U.K. data, found that caregivers were just as likely to be in the labor force as non-caregivers of the same age, once co-resident and very intensive providers of care were excluded, but that caregiving was associated with moderate reductions in work hours (Lilly et al., 2007). Informal caregivers who work appear to experience a wage penalty, all else equal (Carmichael and Charles, 2003; Heitmueller and Inglis, 2007). The conflict between work and care is also emphasized by Gautun and Hagen (2010), who report that employees are more likely to express a preference for reduced or flexible working hours when they have care obligations for elderly parents. A large literature chronicles the relationships between caregiving and other outcomes such as health, both physical and mental (see the review in Bianchi et al., 2012) and life satisfaction (Leigh, 2010). Care-related decreases in health and happiness may also have secondary impacts on employment.

The labor market consequences of informal caregiving may also vary across groups. The majority of carers are female, and several studies find that women are more likely than men to experience negative effects on labor market outcomes (Ettner, 1995, 1996; Heitmueller and Inglis, 2007), though these effects may be more persistent for male caregivers (Fevang et al., 2011). The intensity of caregiving is, not surprisingly, an important determinant of labor market costs (Lilly et al., 2007; Ettner, 1996), and Carmichael and Charles (2003) find that the impact of informal caregiving varies with the caregiver's initial level of attachment to the labor market.

Most existing studies rely on cross sectional data and, due to selection effects, probably overestimate the causal effects of caregiving on labor market outcomes. Exceptions include Leigh (2010), who uses panel data and finds that, though the initiation of caregiving has a modest negative impact on labor force participation, this effect is a fraction of the apparent association in the cross-section. Individual fixed-effect models of other labor market outcomes result in similarly small, or insignificant, effects of care. Spiess and Schneider (2003) find persistent effects of caregiving responsibilities on work hours in a fixed-effects model—initiating care results in reduced work hours, but terminating care does not increase hours. Fevang et al. (2011) use Norwegian register data to examine the employment rates of sons and daughters in the years immediately prior to a parent's death, when the demand for informal caregiving is likely to be high. They find decreases in employment and increased dependence on sickness insurance and other social security benefits during this period.

The effect of public provision of eldercare, or of subsidies for purchased formal care, on the employment and other labor market outcomes of their children will depend on the extent to which formal care substitutes for (or "crowds out") informal care. There may be considerable heterogeneity in these effects; for example, formal care expansions that focus on home-based assistance may have limited effects on informal care if they delay entry to nursing homes and other types of more-intensive institutional care. Policy changes such as expansions in public care and changes in reimbursement of market services have been used to examine these interactions between formal and informal care. Several studies have found that more generous public home-based care increases the probability that the elderly live independently and delay institutionalization (Pezzin et al., 1996; Orsini, 2010) and result in modest decreases in informal care (Ettner, 1995; Pezzin et al., 1996; White-Means and Rubin, 2004; Stabile et al., 2006), but other studies have found no evidence of crowding-out (Motel-Klingebiel et al., 2005; Christianson, 1988). Substitution between informal care and either home-based or institutional formal care is likely to depend upon the degree of disability of the care recipient. Bonsang (2009) distinguishes between skilled and unskilled formal care, and finds that informal care substitutes for unskilled formal care (with this substitutability declining as disability increases) but that informal care is a weak complement to skilled nursing care independently of the level of disability.

In this study of substitution between formal and informal care, we examine the impact of a formal care expansion that focused on home-based care on the labor market outcomes for children directly, rather than on their provision of informal care. High-quality administrative data enables us to link population cohorts of elderly parents with their adult children and their tax and social service records, so that we can examine a broad set of outcomes including employment, earnings, work absence, and residential mobility. We take advantage of a reform that equalized formal care coverage across municipalities to estimate difference-in-difference models of the labor market responses of daughters (and sons) of elderly parents before and after the reform.[1]

[1]Havnes and Mogstad (2011) use a similar strategy and an uneven expansion of child care services across Norwegian municipalities to examine the impact of formal child care on maternal labor supply.

3 Background & the reform

3.1 Formal care in Norway before the reform

In the mid-1960s, the foundations of a modern welfare state were being established in Norway. Relieving families from some of the burden of care for young, old, and disabled members was an important component of this transformation and in 1964 legal responsibility for care of the elderly in Norway was shifted from the family to the public sector.[2] During the 1970s, public resources devoted to elder care increased by more than 200%. Most of the expansion was in the form of home-based care (which includes care in both private homes and assisted-living facilities); there was a small decrease in number of institutional care (nursing home) slots during the same period.

Historically, government responsibility for elder care has been divided between municipalities, counties and central authorities, with the balance shifting during the past several decades. The decade of the 1980s was a period of decentralization, in which an increased focus on geographical and cultural diversity within Norway allowed municipalities to set their own priorities in the provision of many social services. A set of reforms in 1984 and 1988 transferred all responsibility for elder care, including health services and nursing home administration, to the local municipalities. Federal grants earmarked for elder care were replaced by transfers to municipal budgets based on estimated need (on the basis of demographics and income) in each municipality. With decentralization, the municipalities were free to allocate their budgets between different sectors, and the result was that local variation in elder care coverage increased (Norwegian Ministry of Health and Care Services, 1997). This variation, and later convergence, across municipalities will be important for our identification and forms the background for the reform in 1998.

3.2 The 1998 reform

The care needs of a growing elderly population exerted considerable pressure on municipal budgets by the mid-1990s, and coverage rates for both home-based and institutional care for the population aged 80 and above were declining. Also, the large discrepancies in care coverage that had developed across municipalities came to be seen as inequitable.[3] An action plan for the elderly was adopted by the federal gov-

[2]Information on the history of formal elder care in Norway is gathered from (Norwegian Ministry of Health and Care Services, 2005-2006).

[3]In 1997, coverage rates measured as number of inhabitants 80+ using formal care services at all during a year over all inhabitants 80+, ranged between 6% and 78% for home based services and

ernment that included grants to municipalities to expand the capacity of the health care system to deliver home-based care, beginning on January 1, 1998 (Norwegian Ministry of Health and Care Services, 1997). Care for elderly and disabled were to be integrated in the municipalities' programs, with a goal that all municipalities should be able to offer assistance 24/7 to at least 25% of those aged 80+. A more explicit goal was to increase the number of spaces in adapted apartments and institutions between 1998 and 2001, and to increase labor input in the sector nationwide by 6000 work years (Borge and Haraldsvik, 2006; Norwegian Ministry of Health and Care Services, 1997).

Most of the expansion in services took the form of home-based care provided in adapted apartments rather than institutional care in nursing homes, with an increased emphasis on providing medical treatment as well as practical assistance in the home. This option provided cost advantages, compared to institutions where a larger number of highly-qualified personnel are available at all hours, or to services provided to elderly living in private homes spread over relatively large areas, and also maintained more flexibility in service provision. A desire to preserve the autonomy of the elderly by enabling them to live in their own homes as long as possible also contributed to the focus on home-based care.

All municipalities could in principle apply for these investment grants but as long as increased investments implied increased operating expenses for new spaces, there is evidence suggesting that that those municipalities with the lowest pre-reform coverage (both in terms of number of spaces and the quality of spaces), were more likely to apply for the grant. In addition municipalities with higher local budgets and with a high share of elderly above age 80 did also take advantage of the reform to a larger degree than other municipalities (Borge and Haraldsvik, 2006). We confirm this in a regression of the growth in home-based care on pre-reform municipality characteristics (Table 1) showing that both pre-reform coverage and the age distribution of the population were important determinants of post-reform coverage growth.

Municipalities with the lowest pre-reform coverage, experienced the largest post-reform increases in home-care coverage rates as coverage rates converged in response to federal policy. Figure 1 shows the trends in home-based care coverage rates for the population 80+[4] for two groups of municipalities–those with pre-reform coverage rates below the median (treatment group) and municipalities with higher pre-reform

between 0 and 55% for institutional services.

[4]Coverage is defined as the number of individuals 80+ receiving home-based care over the total number of individuals 80+. Note that there is no differentiation on intensity of care in this measure, which is why the coverage rates are relatively high compared to the stated goal of the reform.

coverage (control group). The overall trend in coverage was negative before the reform, reflecting the failure of local service provision to keep pace with the increasing elderly population. There was a relatively large difference between control municipalities and treatment municipalities with respect to coverage rates in home-based services before the reform, with control municipalities providing home-based care to more than 40% of the elderly on average, while average coverage in the treatment municipalities was 34-35% around 1995.

After the reform, there is a clear pattern of convergence consistent with the announced goal of the municipal grants program. This is especially pronounced after 2000, when newly-built facilities were likely to be completed. The lower panel shows the difference between the treatment and control group with 95% confidence intervals. The difference in coverage between treatment and control municipalities is about 6 percentage points pre-reform. It starts converging 3 years after the reform, and stabilizes at almost full convergence by 2002. Figure 2 shows that the fraction of elderly increased over the period in both treatment and control municipalities, although slightly more in the treatment group. This means that without the reform the difference in home-based care between treatment and control municipalities could have diverged further.

Institutional care coverage in treatment and control municipalities follows a very different pattern. Figure 3 shows that control municipalities had slightly lower rates of institutional coverage than treatment municipalities (in contrast to the large discrepancies in home-based care), and there is no difference in the post-reform (modestly declining) trend. This pattern suggests that home care expansion in treatment localities did not come at the expense of institutional care, and is consistent with the government's stated strategy to emphasize home-based care in combating coverage discrepancies across municipalities (Daatland and Veenstra, 2012).

Although Figure 1 provides evidence that the reform was effective in its stated goal of equalizing access to care, this only captures one aspect of the reform; the increased availability of home-based care. There are other potential results of the reform, such as changes in the quality of home-based care or to employment in the eldercare sector. The observed reduced-form effects of the reform on parental and adult child outcomes thus may work through multiple channels. In Section 7 we discuss the possible mechanisms leading to changes in daughters' outcomes.

4 Empirical strategy

To estimate the effect of an expansion in the availability of public home-based care of the elderly on the labor market outcomes of their adult children, we apply a difference in differences (DinD) approach that exploits the differential increase in the availability of federal funds in municipalities with different pre-reform levels of formal care coverage. The federal grants program initiated in 1998 caused a larger expansion of home-care provision in municipalities that had initially low coverage rates.[5] We use the pre-reform coverage level as an instrument for the actual supply shock faced by the local authorities. We also check directly whether the reform had an effect on our sample's employment in the elder care sector.

To define treatment and control groups, municipalities are ordered according to their average level of home-based care coverage in 1993-1996. Municipalities with coverage rates below the median are classified *treatment municipalities*, and municipalities with coverage above the median are *control municipalities*. To make the treatment and comparison municipalities more similar on average we drop the municipalities with the 10% highest and lowest coverage pre-reform.[6] We compare the labor market outcomes of sons and daughters of elderly parents before and after the reform in treatment municipalities where, on average, federal funding of formal care for the elderly expanded a lot, with the change in these outcomes for children with parents in control municipalities that experienced smaller care coverage increases. The post-reform period is divided into a short term transition period from 1998 to 2000 when we expect to see smaller effects of the reform due to implementation delays, and a longer term post-reform period starting in 2001. The pre-reform year 1997 is dropped from the analysis, as there is evidence in some outcomes that municipalities may have responded to the announcement of the reform before grants became available[7].

The main regression model is the following:

$$Y_{it} = \alpha_1 + \alpha_2 Treat_i + \alpha_3 Short_t + \alpha_4 Long_t + \alpha_5(Treat_i * Short_t) + \alpha_6(Treat_i * Long_t) + \alpha_7 X_{it} + \epsilon_{it}$$

where i indexes the individual, and t, time. Y is the outcome(s), $Treat$ is 1 for

[5]For example, municipalities could expand elder care in response to a decline in female employment (though government documents do not mention this among the many possible reasons for expanding formal elder care).

[6]We include robustness checks for alternative sampling schemes and treatment specifications in section 6.

[7]We include robustness checks for alternative ways of treating 1997 in section 6.

individuals with an elderly parent in treated municipalities, 0 for individuals with an elderly parent in control municipalities, *Short* is 1 in 1998-2000, 0 otherwise, and *Long* is 1 in and after 2001, 0 before 2001. X is a set of control variables including year and municipality fixed effects, parent age and gender, dummies indicating whether parent and child are immigrants, child age, child education, child birth order and number of siblings. ε is an i.i.d. error term clustered at the municipality level. As in Baker et al. (2008) and Havnes and Mogstad (2011) we interpret α_5 and α_6 as the intention to treat effects, or the reduced form effects of the reform on outcomes Y on the short and long term respectively.

The DinD specification identifies the treatment effect as the post-reform change in the labor supply and migration behavior of adult children with an elderly parent in the treatment municipalities relative to the post-reform change for a matched population in the control municipalities. This controls for unobserved differences in the determinants of labor supply across municipalities and across years. Since municipal fixed-effects are included, municipal characteristics that are correlated both with the pre-reform level of elder care coverage and with child outcomes do not bias our results.

Appendix Table A1 provides a detailed comparison of the demographic, economic, fiscal, and political characteristics of treatment and control municipalities in 1997, the year before the reform. With the exception of the home-based care coverage rate, the average differences between these municipalities are small. Control municipality populations are slightly better-educated, more likely to be married, and more urban. Municipalities with higher care coverage rates do, however, have a higher share of socialist votes and are more likely to have a socialist mayor, suggesting that political factors may play a role in the divergence in social service provision. Per capita unrestricted budgets are about 15% higher in the control municipalities.

Our key identifying assumption is that the change in labor market outcomes for sons and daughters before and after the elder care reform would have been the same in treatment or control municipalities in the absence of the reform–that is, that the low elder care coverage in treatment municipalities is not a proxy for other unobserved determinants of labor market trends.[8] Figure 4 shows that trends in income, disposable unrestricted municipal resources, education levels, and employment rates are very similar in treatment and control municipalities, even before controlling for observables.

[8]The municipality of the parent defines treatment and control groups. However, nearly 90% of the adult children live in the same municipality as their parent.

5 Data

Our data is based on administrative registers provided by Statistics Norway, and cover the entire resident population of Norway from 1993 to 2006. For each year, we have individual demographic information (including gender, month of birth, place of birth, and marital status), socioeconomic data (including years of education, sector of work, earnings, sickness absence and disability insurance), and municipality of residence. The data contains unique identifiers that makes it possible to match children to their elderly parents and their siblings. In addition we have a separate source of municipality data from the Norwegian Social Science Data providing information on the use of different types of elder care[9] from 1993 onwards and population by age for all municipalities across time. The coverage and reliability of Norwegian registry data are considered to be exceptional, and received the highest rating in a data quality assessment conducted by Atkinson et al. (1995).

Our sample is cross sectional and consists of men and women with only one surviving parent who is at least 80 years old. Since the primary caregiver for frail elderly who are married is usually the spouse, this restriction yields a sample of adult children who are more likely to be presented with parental care responsibilities. Although we present results for both men and women, we focus our discussion and robustness checks on the sample of daughters.

Our measures of labor supply include the adult child's annual income and indicators of labor supply and insured absences from work (sickness absence). Earnings are measured as total gross pension-qualifying earnings reported in the tax registry. These measures are not top-coded and include labor earnings, taxable sick benefits, unemployment benefits, and parental leave payments. The market work dummy is set equal to one if an individual earns more than two times the administratively-set (annually-adjusted) minimum gainful activity thresholds.[10] We also study alternative income cut-offs, to capture intensive margins of working. The sickness absence variable is a dummy set equal to one for individuals who have received public benefits (requiring physician authorization) for a work absence of at least two weeks. We also examine days of insured absence (in addition to those two weeks), and both receipt and days of sickness absence conditional on employment. One mechanism for labor

[9]The available data on elder care use are the number of receivers, regardless of the intensity, of each type of elder care - at the municipality level. We are not able to trace individual receivers or for example the exact number of hours of care each municipality has provided, only the number of users in each municipality.

[10]The minimum gainful activity level is in Norway referred to as "G". Our indication of work related activity was income>2G and in 2006 this represented approximately US 20,000.

force withdrawal, particularly for older workers, is to apply for a disability pension, and we define a dummy variable indicating whether an individual has been granted a disability pension during a year. Finally, we construct an indicator of whether the daughter is employed in the elder care sector[11] to examine sector-specific shifts in employment opportunities.

Other important outcomes that may be affected by the reform include adult child mobility–whether he or she is able to move away from a parent–and parental health. Our mobility measure is a dummy variable indicating whether the adult child resides in a different municipality than in the previous year. The only measure related to parental health that is available is date of death, and we use a measure of whether the parent dies within the next year (relative to year of observation). The pre-reform averages for all the outcome variables are reported in the first column of the respective tables in the results section.

6 Results

Table 2 reports the intention to treat effects from a difference-in-differences model for our broadest sample: daughters who have only one living parent, at least 80 years old. The short term ITT is the effect for the period 1998-2000 and the long-term ITT is the effect for the period 2001-2005 (all compared to the pre-reform period of 1993-1996).[12] There is a strong effect of the policy change on the fraction of the population 80+ receiving home-based care at all in the long-term, but no significant impact in the short-term. In the treatment municipalities parents are 5 percentage points (from a base of 30%) more likely to receive home-based care after the reform compared to those in control municipalities. This supports institutional descriptions of the reform that report a lag in the actual expansion of home based care services. Concentrating on daughters we do not find significant effects of the reform either in the short-run nor the long-run on most of the outcome variables, including employment, earnings, and disability pension receipt. There is some reduction in sickness absence rates in the long-run, but this effect is statistically significant only at a 10% level.

If elder care responsibilities restrict the residential mobility of adult children, then

[11]Sector of work is registered in NACE 5 codes. We construct the indicator based on all NACE 5 codes that are related to the elder care sector in the municipalities. Typical examples are nurses or assistants working either at institutions or providing home based services, and administrators. Health services provided to elderly at hospitals or with their primary physicians are not included in the index.

[12]Our main results are robust to both the inclusion of 1997 as a pre-reform year and to treating 1997 as part of the first short term effect (see Table 8).

the reform may allow them to move away and pursue other opportunities in the labor or marriage markets. The rates of mobility (year-to-year changes in municipality of residence) of this group are low–only 3% of the sample has moved five years after the first year of observation. This could imply that effects are hard to detect, but the last rows in Table 2 show that the effects are very precisely estimated zeros.

One possible reason for these weak effects of the reform is that care responsibilities are shared among siblings. In Table 3 we report the same DinD models estimated on a subsample of adult daughters with no siblings (about 18% of the full sample). Here we observe a substantial and highly significant response on one intensive margin – working daughters are less likely to experience insured sickness absences (of more than 16 days) after the reform. This reduction is 2 percentage points from a base of 16% or, conditional on working, 3.2 percentage points from a base of 19% , and translates into about 3.4 days fewer insured days absent from work in a year. There are thus very substantial responses to the reform for a subsample of women that are perhaps the most likely to be engaged in eldercare, and these responses are also evident in the raw data. Figure 5 shows, based on the same subsample, the upward trends in sickness absence rates for treatment and control municipalities in the first panel and the differences between treatment and control municipalities (including 95 % confidence intervals) in the lower panel. The figure shows that sickness absence rates before the reform in treatment and control municipalities are very similar, but that the rates for the treated group are consistently lower than those of the control group in the long term post-reform window.

There are no significant impacts of the reform on measures of employment, earnings, disability pension receipt or residential mobility. Figures 6-7 shows the trends for these outcomes in addition to the other sickness absence variables. Though there appear to be no significant labor supply responses on the extensive margin, Norwegian caregivers report in surveys that they adjust on the intensive margin by reducing working hours (Gautun, 2008). There is no direct measure of hours worked in our data, but Table 4 examines possible movements along the earnings distribution instead, using the government-set gainful activity levels. In the period studied, the mean earnings from a full time job in Norway correspond to 6G, for women somewhere between 5G and 6G, while a 75-80% position, which is very common in female dominated professions (e.g. nursing, teaching), would pay on average 4G (Hansen and Skoglund, 2003). There appears to be no response to the reform at the lower end of the earnings distribution, but the probability of earnings corresponding to a full-time job and to the typical female job increases in the long-term post-reform period.

In summary, the expansion of elder care in the treatment municipalities does not seem to prompt discrete responses from daughters such as moving to another municipality or entering the labor market, but for the subsample of daughters without siblings it influences labor supply at the intensive margin for those already working. For sons, we are not able to trace any effects of the reform (Table 5). For the remaining outcomes and robustness checks, we restrict our focus to the subsample for whom the reform seems most relevant–daughters without siblings.

As discussed above, the greater expansion of formal elder care in treatment municipalities could have a mechanical effect, simply by providing new employment possibilities for treated daughters[13] and thus increasing their hours worked (as we find no effects on labor force participation per se). Around 7% of the sample works in the elder care sector before the reform and if changes in sector-specific employment opportunities are relevant to these adult daughters, we should see a change in their concentration in that sector. Figure 8 shows no apparent relationship between the expansion of the formal elder care in treatment municipalities and labor market participation in the elder care sector, although there is some variation over time unrelated to the reform. DinD estimation with employment in the elder care sector as an outcome variable confirms this; we are not able to trace any changes in sectoral employment to the reform.[14]

In Table 6 we examine the possibility that the reform could have affected parental health, and ultimately mortality. The effect of increased formal care is ambiguous a priori, as it could lead to better access to medical care but also less informal interaction with children (which may yield health benefits). Using the exact date of death, we look at the probability of dying one year after the year of observation, and find no significant effects, neither in the shorter window after the reform, nor in the longer window. The intention to treat effect is a rather precisely estimated zero from a base death rate of 9%. Figure 9 is also consistent with no effect of the reform on parents' probability of dying.

6.1 Robustness Checks

Table 7 reports DinD results for potential placebo samples of daughters whose care responsibilities are expected to be relatively low–those with both parents alive, no

[13]Although the sample is not restricted to daughters living in the same municipality as their parent, around 90% of the only child daughters in our sample actually do so.

[14]Unfortunately 7 % is too few to conduct separate analysis for those working in the elder care sector pre-reform.

living parents and with one living parent who is relatively young (age 60-74). For these groups, there are no significant effects of the reform on any of the outcomes. The coefficient in the sickness absence equation for the daughters with no living parents is marginally significant at the 10% level and is not significantly different from our main effect in Table 2. We examine whether some of these daughters may be care providers for parents-in-law, and indeed find that the effect may be driven by the subsample where the daughters have a single parent-in-law. If we drop those having a single elderly parent-in-law from the sample of daughters without parents, there are no longer any significant effects of the reform[15].

Table 8 reports the results of different specifications where we exclude various parts of our sample and the results are remarkably robust. The estimated effects of the reform are very similar when we drop either the three largest cities in Norway or the extreme rural municipalities. The effects are ass similar when we include the municipalities with highest 10% and lowest 10% pre-reform coverage rates. In Table 9 we perform analyses including 1997 (omitted in the previous models). The first column includes 1997 as a pre-reform year, the second includes 1997 as a post-reform year. Excluding 1997 does not drive any of the main results, with the effects being very similar to the baseline, for both types of inclusion of this particular year. Table 10 shows that dividing the sample into control and treatment municipalities at the mean rather than the median of pre-reform coverage is not driving our results and that a linear specification of coverage gives very similar results.[16]

Finally, Table 11 reports the results of a placebo specification that places the reform in the pre-period. The negative results here support the common pre-reform trend assumption required for a valid DinD.

7 Discussion and Conclusion

Using variation across municipalities in the impact of a Norwegian reform of federal funding for care of the elderly, we find robust evidence that the labor supply of middle-aged daughters with single elderly parents and no siblings is affected at the intensive margin by expansions in public home-based care. Previous estimates of the degree of substitution between informal care and home-based formal care have been mixed, but our results provide support for such substitution, in particular for a group of adult

[15]Results available on request

[16]The results of the linear specification, unlike the discrete model, are sensitive to the inclusion of the tails of the coverage distribution.

women with potentially large care responsibilities. Labor supply adjustments came in two forms: as a reduction in doctor-certified sickness absence and as an increased probability of working longer hours (measured as the probability of reaching higher income thresholds). We found no effects of the expansion in formal elder care on the labor supply of sons, the geographic mobility of adult children, the health of the elderly, or extensive margin labor supply decisions of daughters.

The relatively large effects of an expansion of formal eldercare on insured absences from work which are formally restricted to own diagnosed illnesses deserves further consideration. There are two possible mechanisms that may be driving this outcome: 1) the burden of informal care may have negative effects on the caretaker's mental and/or physical health, and 2) sick leave may be used, with or without the knowing cooperation of the physician in order to free time that can be spent on care-taking.

An association between own health and the burden of caretaking has been extensively documented (Bianchi et al., 2012), but a causal relationship has been difficult to establish. Surveys in Norway indicate that mature caretakers of elderly parents experience deteriorating health, which they ascribe to the burden of care-taking (Gautun, 2008). Paid leave for own illnesses or disability in Norway is generous, but the same is not true of caretaking leave. If elderly parents or other close family members (except children) are sick and in need of care, employers are in general only obliged to grant *unpaid* leave up to a maximum of ten days per year.[17]

The relative size of the two channels could vary between group, and we speculated that higher educated daughters may have jobs with more temporal flexibility and perhaps less need for sickness absence. The reults in Table 12 do not support this, though the reform does have a somewhat stronger effect on the probability that less-educated daugthers reach the earnings threshold of 4G, which would correspond to a 75% of a full time position.

The doctor's certification requirement for insured sickness absence is intended to prevent the fraudulent use of this leave, but several studies confirm that this program is used for purposes other than own illness. In interviews, Norwegian general practitioners / family doctors reveal that they sometimes certify that employees are sick when this is not strictly true (Carlsen, 2008), and a study based on very rich registry data, Markussen et al. (2011) concludes that "... the sickness absence insurance system in Norway has developed into a more general "justified absence" insurance system, where physicians certify sickness to help employees cope with a difficult life situation." Markussen et al. (2011) document a number of patterns that suggest con-

[17]Parents are each entitled to 10 days fully paid sickness leave to take of their sick children.

siderable subjectivity in physicians' absence certification practices, including substantial differences in apparent strictness among doctors and an association of traumatic personal events such as marriage dissolution and family deaths with insured work absences. Health problems such as musculoskeletal and mental disorders are both very difficult to verify and subject to reasonable disagreement among physicians as to the efficacy of time off work as a treatment. Social norms may also play an important role in explaining variation in absenteeism across workplaces that do not seem to be explained by worker sorting and a lower propensity for sickness absence among older workers. Survey data from a representative sample of mature Norwegians also indicates that sickness absence is a relative common way to cope with the need for more flexible working hours when parents are elderly and in need of care (Gautun, 2008).

We cannot, based on our analysis, distinguish between the alternative mechanisms that may be driving the association between an expansion of formal eldercare and a reduction in sickness absence. Relief from caregiving responsibilities may be improving the health of adult daughters, or their need to use a social welfare program designed to provide insurance against own illness to informally acquire caregiving leave may be alleviated. In combination with other studies of the use of sickness absence in Norway, it is reasonable to think that expanding public eldercare has reduced the need for some women to find enough flexibility in their work schedules to provide parental care when needed through a program designed for other purposes. Our results provide additional evidence that women's social roles often demand a degree of flexibility in work schedules that are not readily accommodated by current workplace or social institutions (Goldin 2014). We also find that formal care of the elderly can substitute, to some extent, for the informal caregiving demands that conflict with rigid work schedules.

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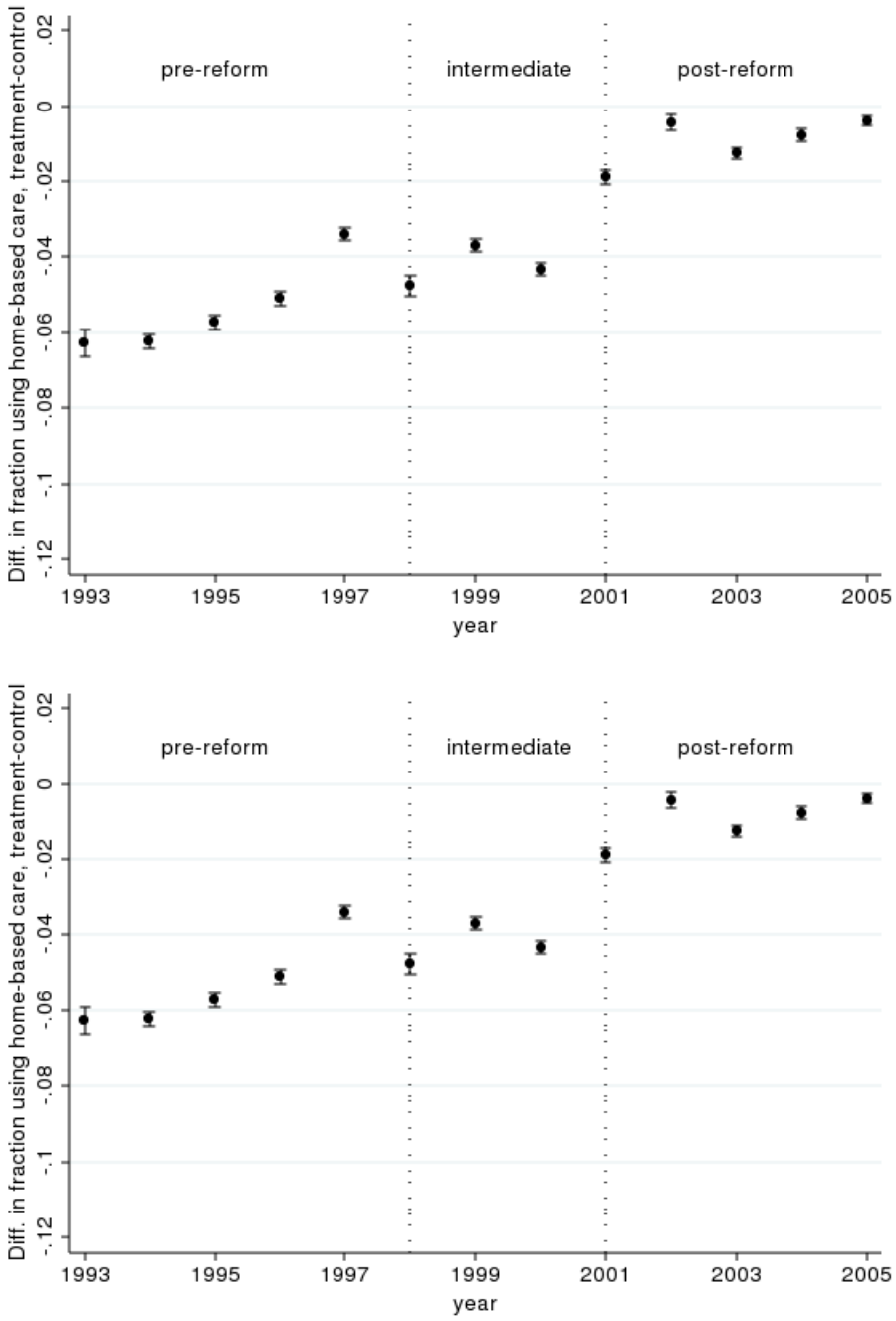

Figure 1: Home based care (at home or in adapted facilities)

Notes: The first panel graphs the fraction using home based care in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in fraction using home-based care between treatment and control municipalities, including the 95 % confidence interval.

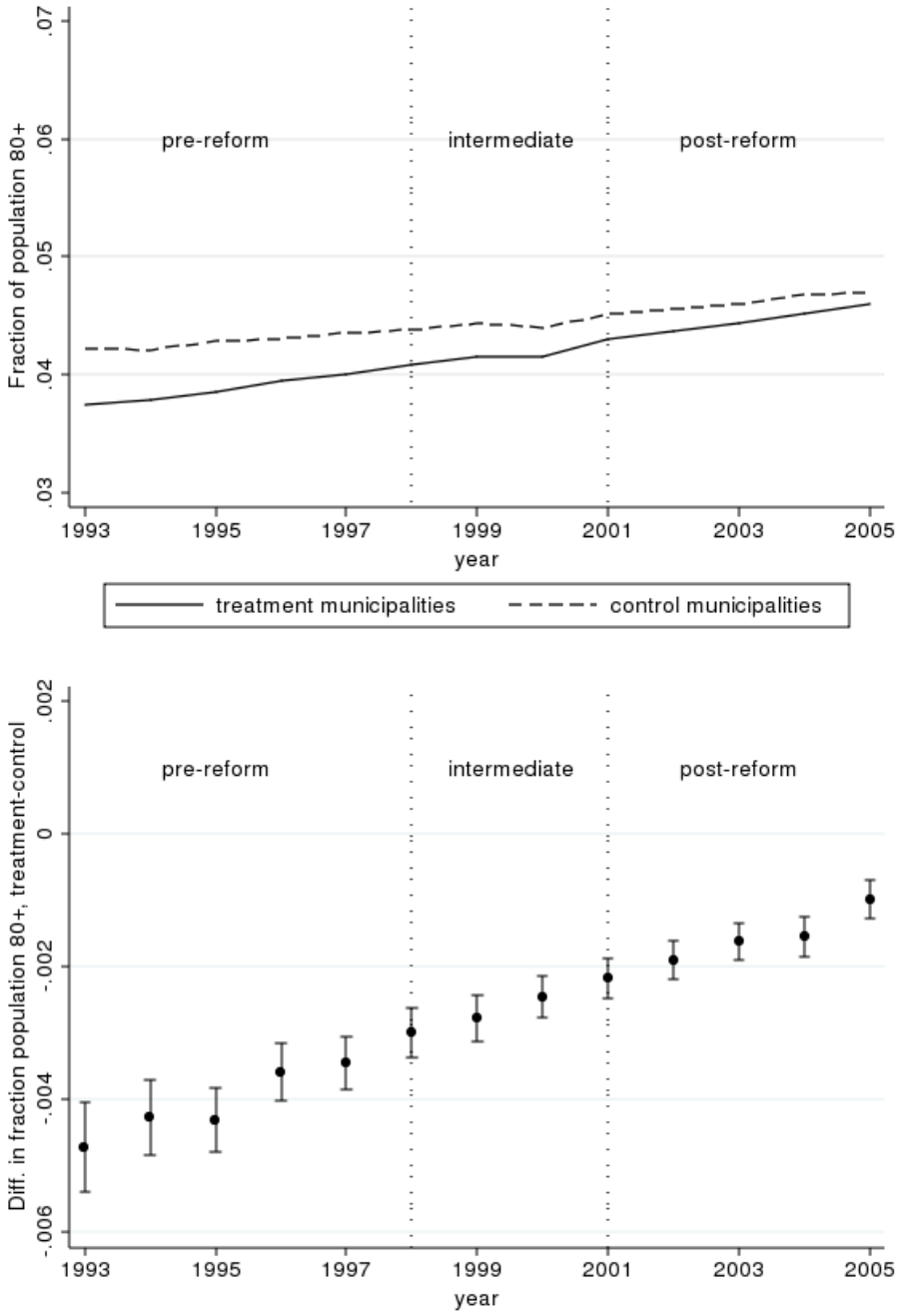

Figure 2: Fraction Population 80+

Notes: The first panel graphs the fraction of the population aged 80 and above in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in population aged 80 and above between treatment and control municipalities including the 95 % confidence interval.

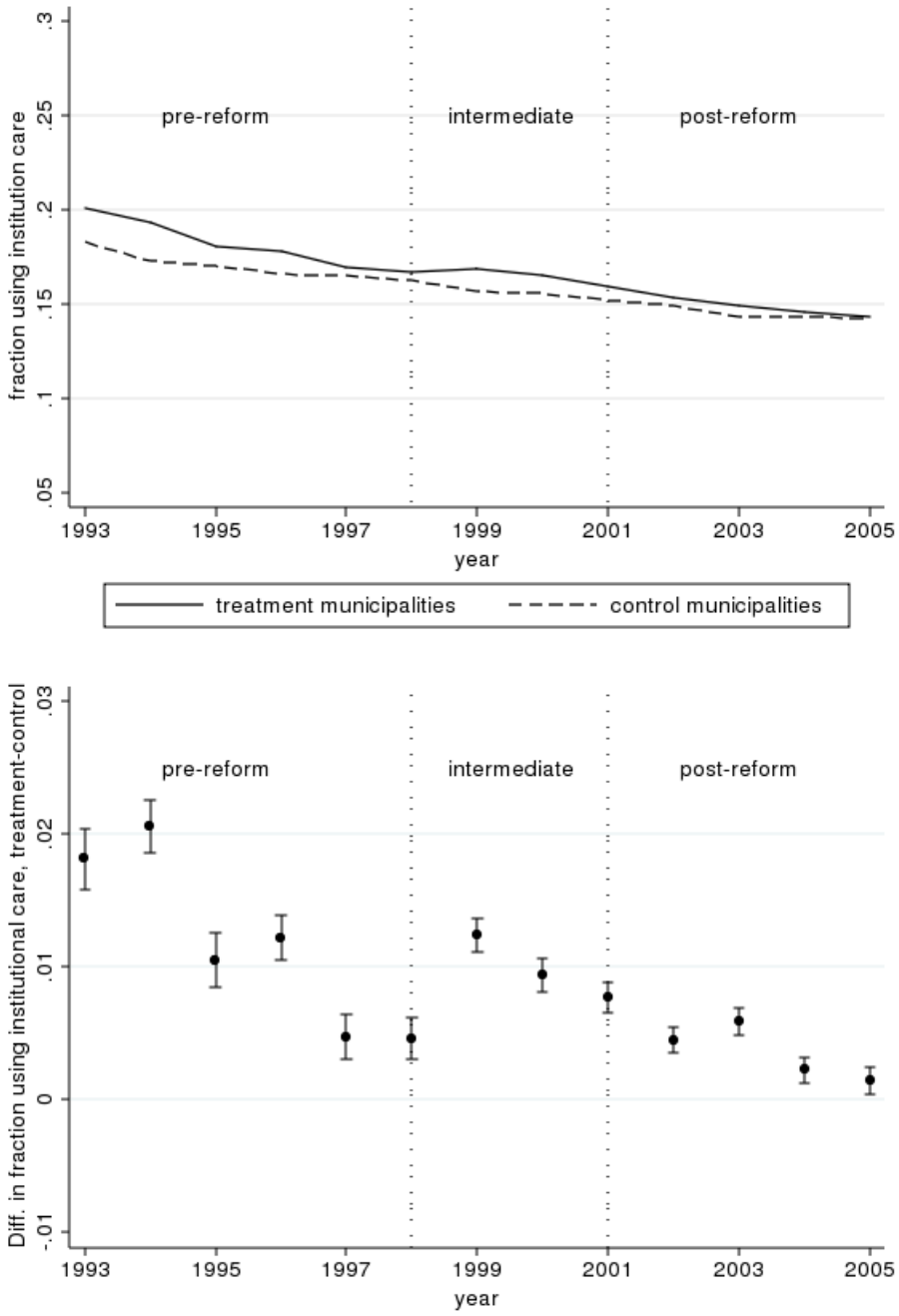

Figure 3: Institution based care (nursing homes)

Notes: The first panel graphs the fraction using institutional care in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in fraction using institutional care between treatment and control municipalities, including the 95 % confidence interval.

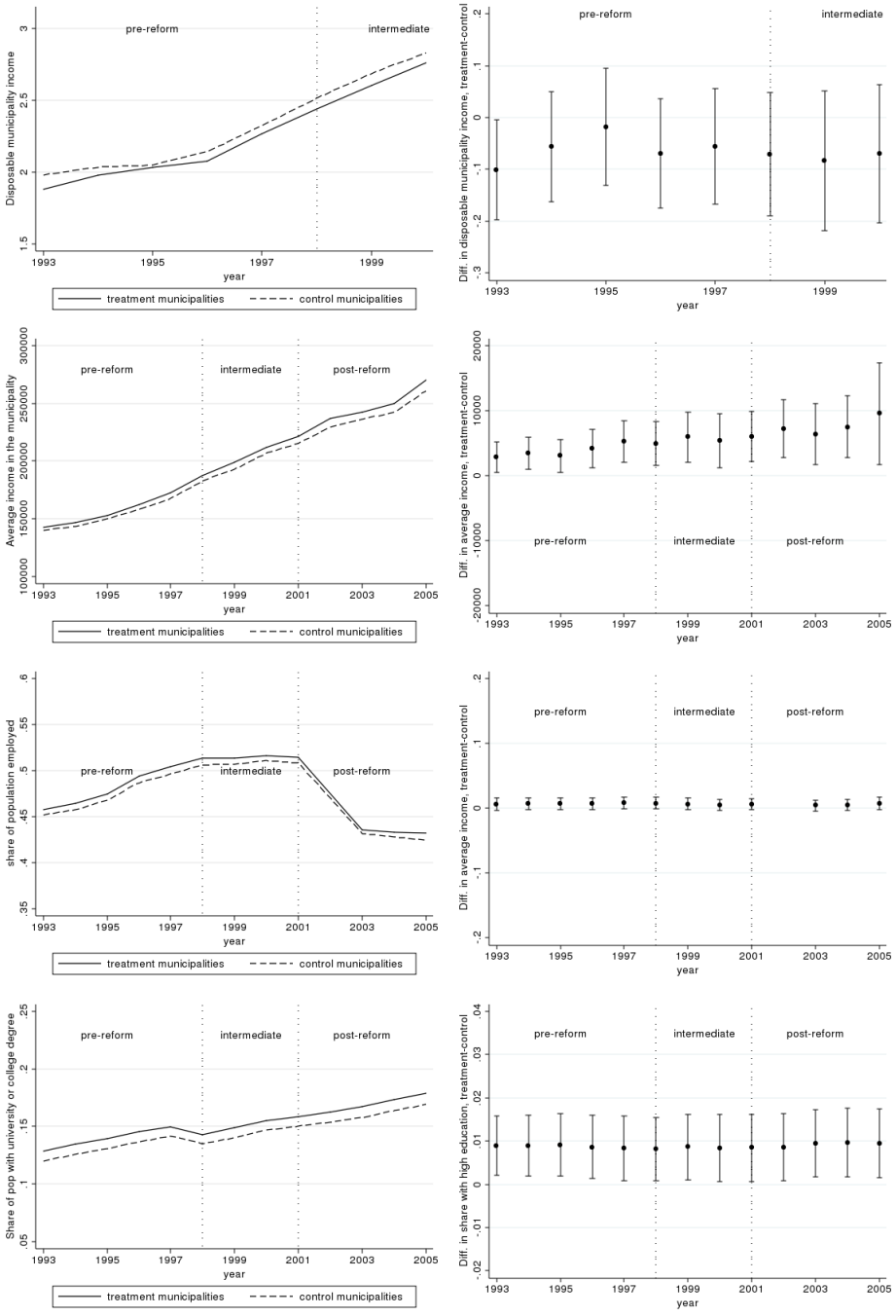

Figure 4: Municipality outcomes over time - treatment and control municipalities

Notes: The panels to the left graph outcomes in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The panels to the right show the differences per year in outcomes between treatment and control municipalities, including the 95 % confidence interval. Outcomes are from the top down: Disposable unrestricted municipality income from 1992-2000, average income in the municipality, the share of 16 + employed, and the share with a university or college degree.

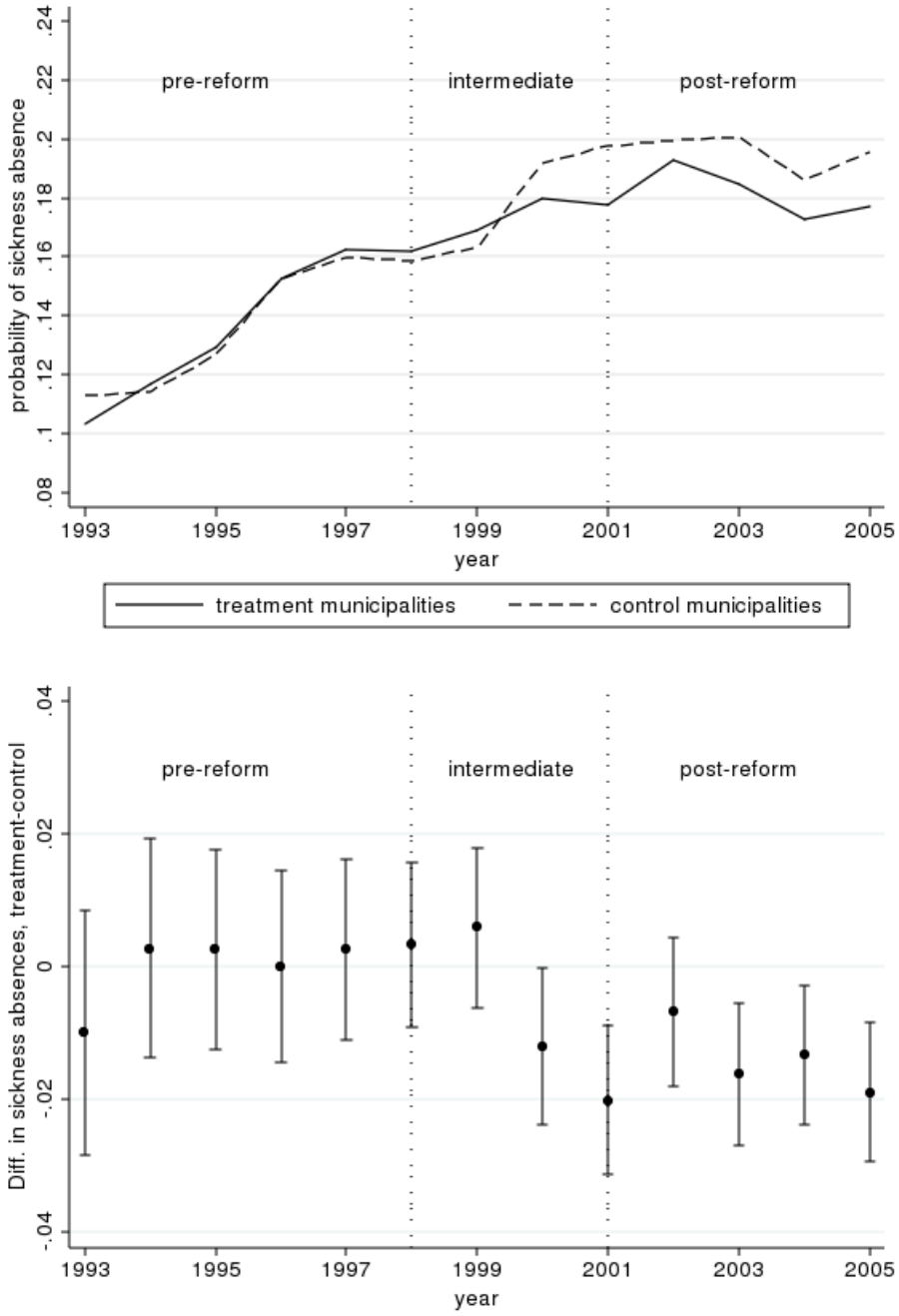

Figure 5: Sickness absence

Notes: The first panel graphs the probability of being absent in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in probability of being absent between treatment and control municipalities, including the 95 % confidence interval.

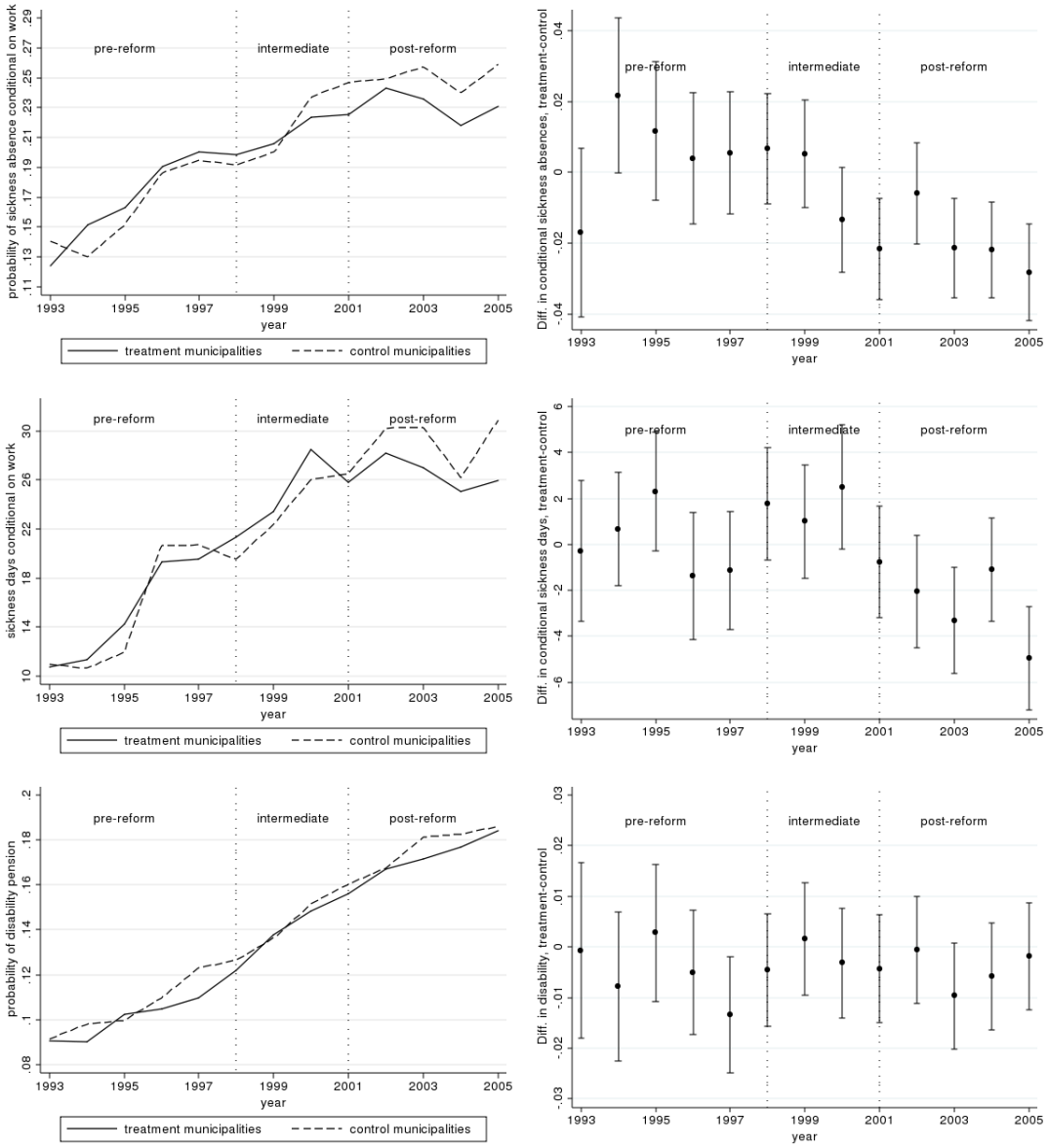

Figure 6: Other outcomes a)

Notes: The panels to the left graph the development in outcomes in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The panels to the right show the differences per year in outcomes between treatment and control municipalities, including the 95 % confidence interval. Outcomes are from the top down: The probability of sickness absence conditional on working, the number of days absent conditional on working, and the the probability of being on disability pension.

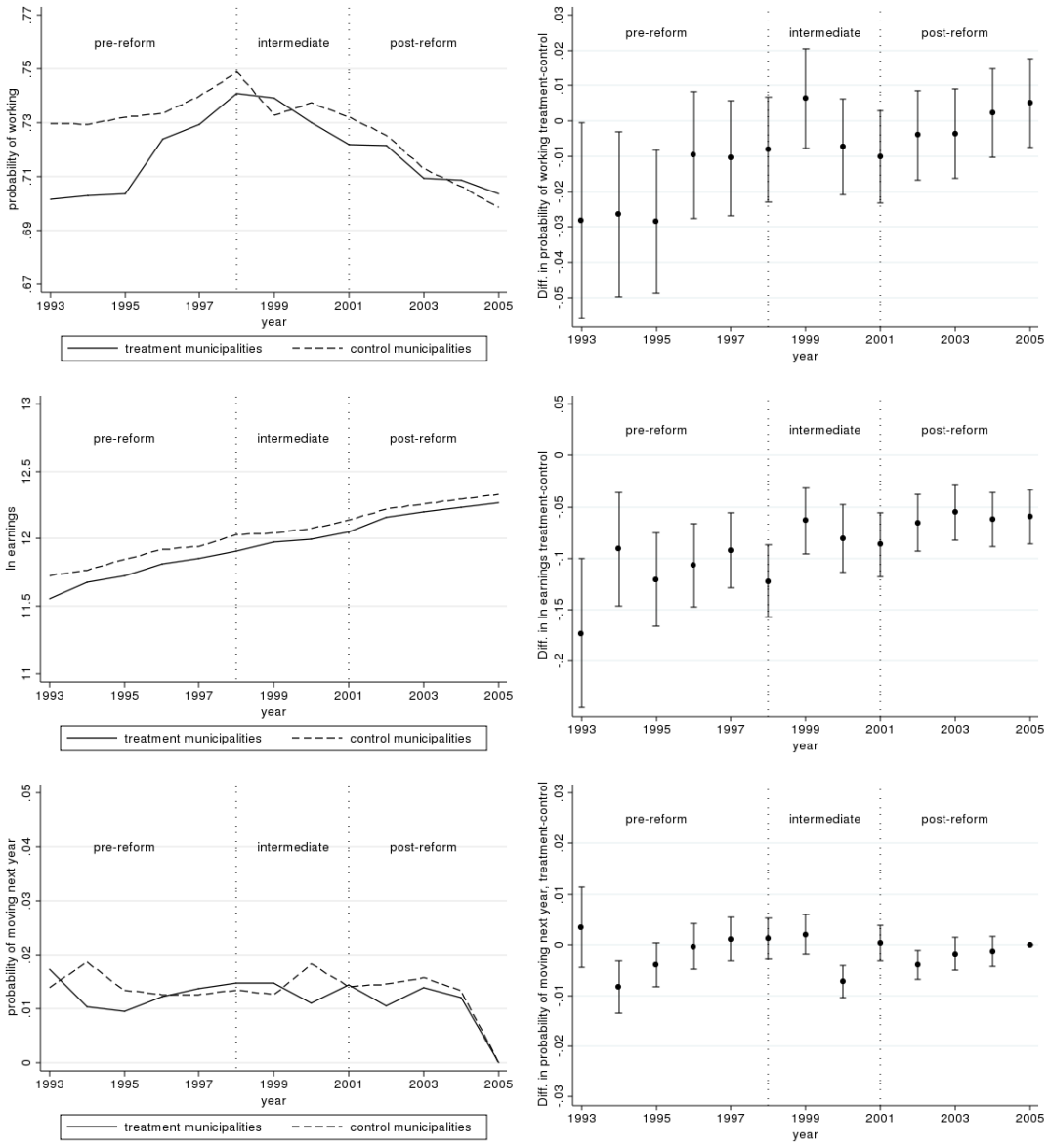

Figure 7: Other outcomes b)

Notes: The panels to the left graph outcomes in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The panels to the right show the differences per year in outcomes between treatment and control municipalities, including the 95 % confidence interval. Outcomes are from the top down: the probability of working, ln earnings, and the probability of moving next year.

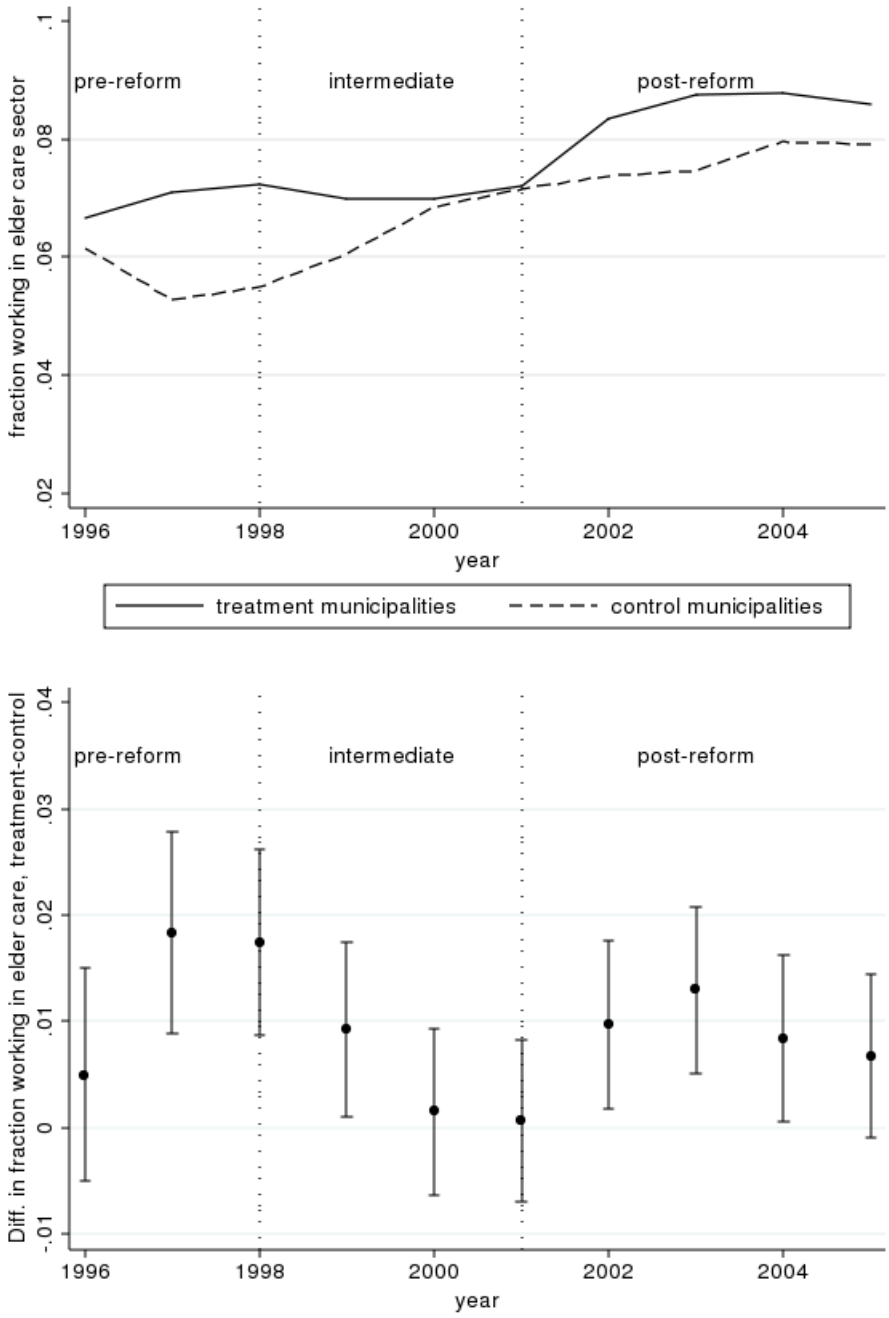

Figure 8: Daughter's employment in elder care sector

Notes: The first panel graphs the fraction working in elder care in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in fraction working in elder care between treatment and control municipalities, including the 95 % confidence interval.

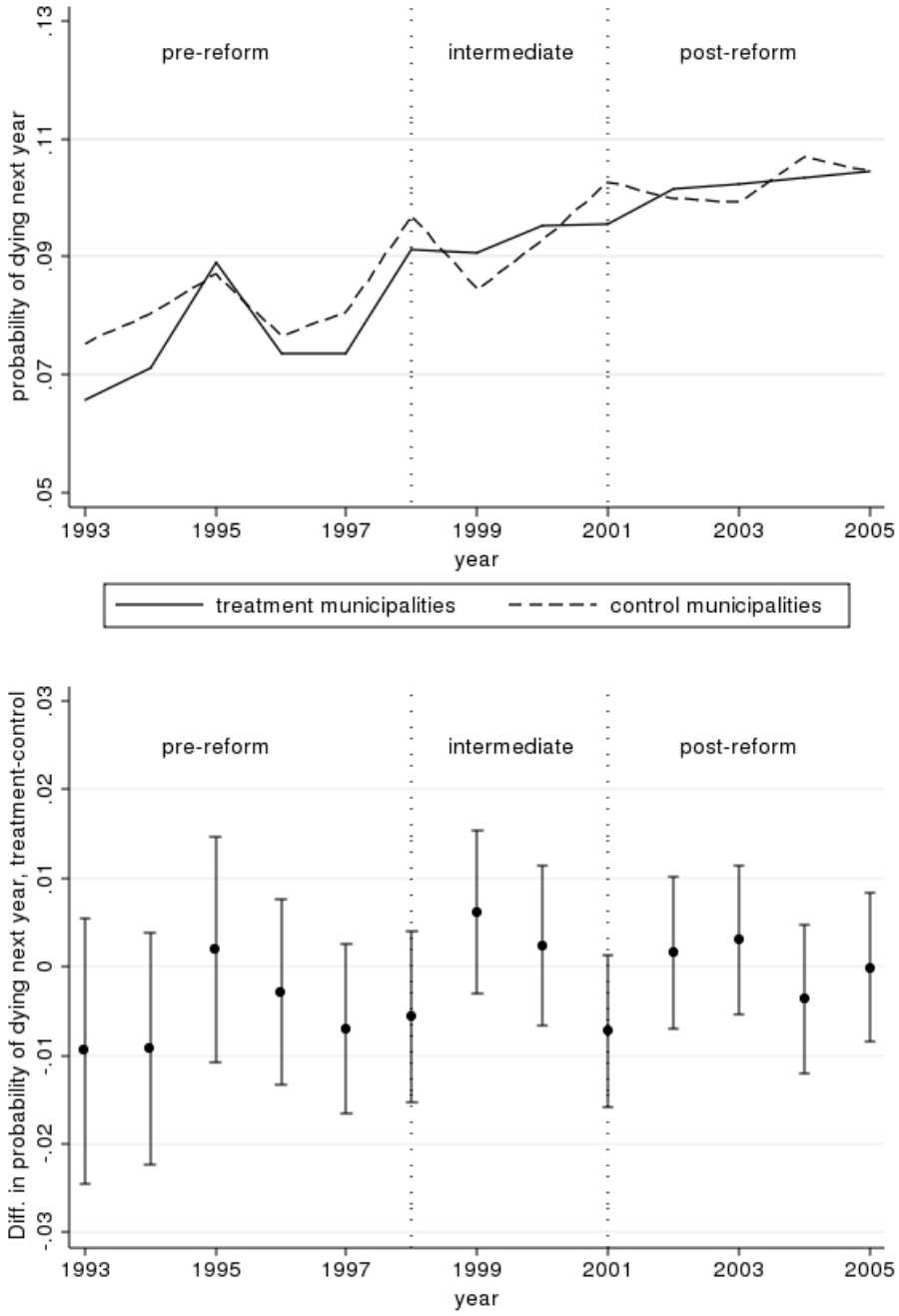

Figure 9: Parent's Health

Notes: The first panel graphs the probability of parent dying next year in treatment municipalities (solid line) and control municipalities (dashed line) over the period 1993-2005. We add a vertical line in 1998 (year of reform) and 2001 (expansion of reform). The second panel shows the differences per year in probability of parent dying next year between treatment and control municipalities, including the 95 % confidence interval.

Table 1: Predictors of post-reform growth in coverage

Growth in home-based care coverage (1999/2000)/(1996/1997)	
Home-based care coverage before the reform	-1.597***
(1996/1997)	(.229)
Institution care coverage before the reform	-.497***
(1996/1997)	(.170)
Share of population >67 in 1997	1.312**
	(.532)
Share of population >80 in 1997	-3.089**
	(1.264)
Disposable income	.029**
	(.014)
Constant	1.627*
	(.094)
N (municipalities)	431

*Note: Results from a standard linear regression. Standard errors clustered at municipality level in parentheses. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 2: Main outcomes DinD; Daughters

First stage:	Pre-reform Mean	Short term ITT	Long term ITT	Obs.
Home based care	.38	.014	.047***	416,330
80+		(.009)	(.007)	
Outcomes:				
Probability of	.74	-.005	-.000	416,330
working		(.007)	(.008)	
Ln earnings	11.9	.008	.016	367,064
		(.013)	(.016)	
Sickness absence	.16	-.003	-.008*	416,330
		(.005)	(.004)	
Sickness absence	.19	-.004	-.010*	308,459
(conditional on work)		(.005)	(.005)	
Days absent	20.6	.24	-1.03	308,459
(conditional on work)		(.80)	(.77)	
Disability pension	.12	.003	.004	416,330
		(.005)	(.007)	
Move to another	.014	-.001	-.000	415,201
municipality 1 year after		(.001)	(.001)	

*Note: Pre-reform means of the treatment municipalities are reported for comparison before the results from estimations. The first row thus shows that the probability of receiving home-based care rose almost five percentage points more in treatment municipalities in the long run than in control municipalities as a result of the reform. This represented an increase of almost 15% (from a pre-reform coverage of 38 %) . Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 3: Main outcomes DinD; Daughters, no siblings

First stage:	Pre-reform Mean	Short term ITT	Long term ITT	Obs.
Home based care	.38	.015*	.049***	74,526
80+		(.008)	(.007)	
Outcomes:				
Probability of	.73	.015	.014	74,526
working		(.013)	(.013)	
Ln earnings	11.9	.024	.042	64,211
		(.030)	(.030)	
Sickness absence	.16	-.006	-.021***	74,526
		(.007)	(.008)	
Sickness absence	.19	-.013	-.032***	53,972
(conditional on work)		(.009)	(.009)	
Days absent	20.4	.861	-3.399**	53,972
(conditional on work)		(1.531)	(1.397)	
Disability pension	.12	.000	-.001	74,526
		(.009)	(.011)	
Move to another	.012	-.001	.001	74,309
municipality 1 year after		(.002)	(.003)	

*Note: Pre-reform means of the treatment municipalities are reported for comparison before the results from estimations. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 4: More outcomes DinD; Daughters, no siblings

Outcomes:	Pre-reform	Short term ITT	Long term ITT	Obs.
Probability of working (more than 1G)	.80	.011 (.012)	.004 (.013)	74,383
Probability of working (more than 2G)	.75	.014 (.016)	.015 (.017)	74,383
Probability of working (more than 3G)	.64	.004 (.015)	.016 (.017)	74,383
Probability of working (more than 4G)	.52	.017 (.013)	.033** (.014)	74,383
Probability of working (more than 5G)	.34	.012 (.009)	.019 (.013)	74,383
Probability of working (more than 6G)	.18	.010 (.007)	.017** (.008)	74,383

*Note: Pre-reform means of the treatment municipalities are reported for comparison before the results from estimations. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 5: Main outcomes DinD; Sons, no siblings

Outcomes:	Pre-reform mean	Short term ITT	Long term ITT	Obs.
Probability of working	.75	.004	.003	100,700
		(.009)	(.009)	
Ln earnings	12.4	.000	-.014	92,286
		(.021)	(.026)	
Sickness absence	.12	-.006	-.001	100,700
		(.007)	(.006)	
Sickness absence (conditional on work)	.12	-.004	.008	74,034
		(.010)	(.007)	
Days absent (conditional on work)	12.5	-1.17	1.20	74,034
		(1.31)	(.97)	
Disability pension	.08	.004	.007	108,733
		(.005)	(.007)	
Move to another municipality 1 year after	.017	-.004*	-.002	99,530
		(.002)	(.002)	

*Note: Pre-reform means of the treatment municipalities are reported for comparison before the results from estimations. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 6: DinD; Parental health

	Pre-reform Mean	Short term ITT	Lon term ITT	Obs.
Outcomes:				
Dead within next year	.09	.004 (.006)	.003 (.007)	74,526

*Note: Pre-reform mean of the treatment municipalities is reported for comparison before the results from estimations. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 7: DinD; Placebo groups

	Daughters, no siblings					
	Daughters with no living parents		Daughters with two living parents		Daughters with younger parent (60-74)	
	Long term ITT	Obs.	Long term ITT	Obs.	Long term ITT	Obs.
Outcomes:						
Probability of working	-.016	67,579	-.013	48,497	-.006	65,109
	(.018)		(.014)		(.013)	
Ln earnings	-.047	55,408	.010	41,954	-.043	57,509
	(.039)		(.025)		(.027)	
Sickness absence	-.013	67,579	-.004	48,497	-.004	65,109
	(.011)		(.008)		(.007)	
Sickness absence (con. on work)	-.022*	45,617	.002	35,177	-.008	46,738
	(.012)		(.011)		(.009)	
Days absent (con. on work)	-.87	45,617	-.363	35,177	-1.25	46,738
	(1.8)		(1.73)		(1.40)	
Disability pension	.001	67,579	.011	48,497	-.003	65,109
	(.011)		(.013)		(.011)	
Move to another mun. 1 year after	-.001	67,383	-.000	48,321	.001	64,804
	(.004)		(.002)		(.002)	

*Note: Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 8: Robustness DinD a) shrinking/expanding sample

	Robustness tests; Daughters, no siblings					
	Drop Oslo/Bergen/Trondheim		Drop rural municipalities		Include +/- 10 % of coverage distribution	
	Long term ITT	Obs.	Long term ITT	Obs.	Long term ITT	Obs.
Outcomes:						
Probability of working	.020	58,523	.019	67,772	.013	93,300
	(.014)		(.014)		(.012)	
Ln earnings	.039	50,072	.050	58,332	.011	80,585
	(.034)		(.032)		(.029)	
Sickness absence	-.021*	58,523	-.023***	67,772	-.015**	93,300
	(.011)		(.008)		(.008)	
Sickness absence (con. on work)	-.033***	41,734	-.032***	49,321	-.023**	67,770
	(.013)		(.010)		(.009)	
Days absent (con. on work)	-4.78**	41,734	-2.64*	49,321	-1.52	67,770
	(1.56)		(1.35)		(1.22)	
Disability pension	-.005	58,523	-.005	67,772	-.003	93,300
	(.014)		(.011)		(.009)	
Move to another mun. 1 year after	.000	57,839	-.001	67,560	-.001	93,052
	(.003)		(.003)		(.002)	

*Note: Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 9: Robustness DinD b); different treatment of 1997

Robustness tests; Daughters, no siblings				
	Include 1997 as pre reform year		Include 1997 as post reform year	
	Long term ITT	Obs.	Long term ITT	Obs.
Outcomes:				
Probability of working	.011	79,558	.014	79,558
	(.011)		(.013)	
Ln earnings	.036	68,570	.042	68,570
	(.026)		(.030)	
Sickness absence	-.020***	79,558	-.021***	79,558
	(.007)		(.008)	
Sickness absence (conditional on work)	-.029***	57,684	-.031***	57,684
	(.009)		(.009)	
Days absent (conditional on work)	-2.76**	57,684	-3.36**	57,684
	(1.34)		(1.39)	
Disability pension	.002	79,558	-.001	79,558
	(.009)		(.011)	
Move to another municipality 1 year after	.001	79,325	.001	79,325
	(.002)		(.002)	

*Note: Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 10: Robustness DinD c) different measures of coverage

Robustness tests; Daughters, no siblings				
	Split coverage at mean		Linear coverage	
	Long term ITT	Obs.	Long term ITT	Obs.
Outcomes:				
Probability of working	-.007	74,526	.023	74,526
	(.013)		(.017)	
Ln earnings	-.009	64,211	.059	64,211
	(.035)		(.039)	
Sickness absence	-.020*	74,526	-.026*	74,526
	(.011)		(.013)	
Sickness absence (conditional on work)	-.027**	53,972	-.032*	53,972
	(.013)		(.016)	
Days absent (conditional on work)	-4.85***	53,972	-6.17**	53,972
	(1.42)		(2.14)	
Disability pension	-.003	74,526	-.001	74,526
	(.014)		(.018)	
Move to another municipality 1 year after	.005*	74,309	.001	74,309
	(.003)		(.003)	

*Note: The linear specification interacts the intermediate and long term reform coefficients with the pre-reform coverage. The interpretation is the effect of having 10 % lower coverage pre-reform (which corresponds to a 6 % increase in long term coverage post reform) on various outcomes. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 11: Placebo reform 1993-1996

Placebo reform, Daughters, no siblings		
	Reform: 1995, Window: 1993-1996	
	ITT	Obs.
Outcomes:		
Probability of working	.020	21,521
	(.014)	
Ln earnings	.041	18,881
	(.029)	
Sickness absence	.005	21,521
	(.010)	
Sickness absence (conditional on work)	.001	15,727
	(.013)	
Days absent (conditional on work)	.370	15,727
	(1.646)	
Disability pension	-.001	21,521
	(.008)	
Move to another municipality 1 year after	-.002	21,435
	(.003)	

*Note: Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Table 12: DinD - different education levels

Daughters, no siblings				
	Low education level (<=10y)		High education level (>10y)	
	Long term ITT	Obs.	Long term ITT	Obs.
Outcomes:				
Sickness absence	-.019*	41,369	-.023**	33,157
	(.011)		(.011)	
Sickness absence	-.036**	27,432	-.025**	26,540
(conditional on work)	(.014)		(.013)	
Days absent	-3.103	27,432	-3.864**	26,540
(conditional on work)	(2.420)		(1.608)	
Probability of	.022	41,310	-.019	33,073
working (more than 1G)	(.018)		(.016)	
Probability of	.036	41,310	-.017	33,073
working (more than 2G)	(.023)		(.018)	
Probability of	.037	41,310	-.019	33,073
working (more than 3G)	(.025)		(.017)	
Probability of	.050***	41,310	.002	33,073
working (more than 4G)	(.018)		(.019)	
Probability of	.018	41,310	.018	33,073
working (more than 5G)	(.015)		(.020)	
Probability of	.016*	41,310	.019	33,073
working (more than 6G)	(.009)		(.018)	

*Note: Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10%, 5%, and 1% level respectively.*

Appendix Figures and Tables

Table A1: Descriptive statistics for treatment and control municipalities in 1997

	Treatment		Control	
	Average	SD	Average	SD
Population	9477	(16146)	10723	(37450)
Share of population 67+	0.159	(0.035)	0.158	(0.039)
Share of population 80+	0.049	(0.015)	0.048	(0.016)
Share of 67+ emigrated	0.006	(0.004)	0.007	(0.004)
Share of 67+ immigrated	0.005	(0.003)	0.005	(0.003)
Share of population married	0.396	(0.028)	0.378	(0.038)
Share of population divorced	0.067	(0.018)	0.075	(0.023)
Share of population widowed	0.065	(0.014)	0.065	(0.014)
Education 9 years - males	0.279	(0.080)	0.269	(0.076)
- females	0.362	(0.078)	0.350	(0.074)
Education 12 years - males	0.493	(0.039)	0.481	(0.049)
- females	0.431	(0.027)	0.419	(0.038)
Education > 12 years - males	0.203	(0.086)	0.216	(0.080)
- females	0.183	(0.067)	0.198	(0.066)
Employment rate (share of population 16+ in work)	0.534	(0.051)	0.535	(0.516)
Average income NOK	187978.3	(28526.97)	190963.7	(26452.85)
Unsrestricted budget..? per capita (10 000 NOK)	1.944	(0.426)	2.268	(0.693)
Sentrality index	5.307	(2.177)	5.489	(2.134)
Share of population in densely populated areas	0.702	(0.263)	0.751	(0.269)
Coverage rate institutions for 80+	0.174	(0.045)	0.173	(0.050)
Coverage rate home based for 80+	0.339	(0.041)	0.428	(0.061)
Share of registered voters that participated in elections 1995	0.622	(0.047)	0.630	(0.057)
Socialist vote share 1995	0.248	(0.108)	0.381	(0.101)
Socialist mayor from 1996	0.286	(0.452)	0.318	(0.466)
Female mayor from 1996	0.153	(0.360)	0.130	(0.336)