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Lifting the Burden: Formal Care of the Elderly and Labor Supply of Adult Children¹

Katrine V. Løken, Shelly Lundberg, and Julie Riise

Abstract

We use a reform in the federal funding of care for the elderly in Norway to examine the effects of formal care expansion on the labor supply decisions of middle-aged children. We find a consistent and significant negative impact of formal care expansion on insured work absences for the adult daughters of single elderly parents. This effect is particularly strong for daughters with no siblings, who are also more likely to exceed earnings thresholds after the reform. We find no impacts of the reform on daughters' mobility or parental health, and no effects on adult sons.

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I. Introduction

Rapidly aging populations pose 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 perhaps also 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 (publicly 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. The elderly 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 extent to which formal care services expanded across localities.

We estimate reduced form 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 than in municipalities with more extensive home-care services. 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. The first stage effects on relative increases in coverage are in line with the intentions of the reform, with coverage converging post-reform. Our main sample is cross sectional and consists of daughters with no siblings and 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. Similar analyses for adult sons yield no significant effects of the reform.

We explore a number of different outcomes of this reform for both adult children and elderly parents. First, the labor supply or location decisions of adult children may be affected if the increased supply of formal care substitutes for (or is complementary with) family-provided 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 or mobility 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. Only-child 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 or on the health of the parents.

Our results are robust to an extensive battery of specification checks, including exclusion of the largest cities and the smallest rural communities, 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.

Insured sickness absences require a doctor's certification and our results are consistent 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. 2012). 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 an important consequence 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. More broadly, these results highlight the work-family conflicts that persist for women even in states with advanced social welfare policies and egalitarian gender norms. This aspect of our results is likely to generalize to other countries (like the United States) with high levels of female labor force participation and limited availability of paid care leave.

II. 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 from 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 European 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, and this includes 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 negative 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. 2012). 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. (2012) 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 be complementary with 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 only modest decreases in informal care (Ettner 1995; Pezzin et al. 1996; White-Means and Rubin 2004; Stabile et al. 2006), while 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, we examine the impact of a formal care expansion that focused on home-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. Our expectation is that, on average, an increase in government-provided care will substitute for some of the informal care provided by adult children and enable them to increase their labor supply, potentially at both the extensive and intensive margins. Administrative data on work hours is not available, but we can examine employment and disability pension receipt at the extensive margin and earnings and spells of insured sickness absence at the intensive margin of labor supply. The prevalence of sickness absence and disability status will, of course, depend upon the health of potential care providers, but may also respond to other incentives to reduce work hours. Reduced reliance on informal care, which requires geographic proximity, may also increase the residential mobility of children, and we are able to identify cross-municipality moves in the data. We take advantage of a reform that equalized formal care coverage across municipalities to estimate reduced form models of the labor market responses of daughters (and sons) of elderly parents before and after the reform.¹

III. Background

III.A 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 expenditures on elder care increased by more than 200 percent. Most of the expansion was in the form of support for home-care services (which includes care in both private homes and assisted-living facilities); there was a small decrease in the number of institutional care (nursing home) slots during the same period.

Historically, government responsibility for elder care has been divided between local 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.

III.B 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 government that included grants to municipalities to expand the capacity of the health care system to deliver home-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 percent 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-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. A desire to preserve the autonomy of the elderly by enabling them to live in their own homes as long as possible contributed to the focus on home-care. This option also 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.

All municipalities could in principle apply for the investment grants but since increased investments implied increased operating costs for new spaces, there is evidence

suggesting that those municipalities with the lowest pre-reform coverage were more likely to apply for the grant and that municipalities with more elderly residents took 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-care coverage (the proportion of the population aged 80 or more receiving any home-based care) on pre-reform municipality characteristics (Table 1) showing that pre-reform coverage and the age distribution of the population, but not income, 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 absolute changes in home-care coverage rates between 1993/1996 and 2001/2005 for the population aged 80 and over for municipalities with different pre-reform coverage. 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. After the reform, however, there is convergence consistent with the announced goal of the municipal grants program.

Institutional care coverage follows a very different pattern. Pre-reform, the municipalities with higher home-care coverage had only slightly lower rates of institutional coverage than municipalities with lower coverage (in contrast to relatively large differences in home-care), and as can be seen in Figure 2, there is hardly any difference in the post-reform (modestly declining) trend. This suggests that home-care expansion did not come at the expense of institutional care, and is consistent with the government's stated strategy of emphasizing home-care in combating coverage discrepancies across municipalities (Daatland and Veenstra 2012).

Although Figure 1 indicates that the reform was effective in its stated goal of

equalizing access to care, this only captures one aspect of the reform; the increased proportion of the elderly population receiving any home-care. The increased funding associated with the reform was expected to influence care on other margins, such as improvements in the quality of home-care, changes along the intensive margin through increased hours for those already receiving care, and expanded employment in the eldercare sector. This means that an instrumental variables approach is inappropriate: the observed reduced-form effects of the reform on parental and adult child outcomes may work through multiple channels and no data is available on care quality or care hours. In Section 7 we discuss the possible mechanisms leading to changes in daughters' outcomes.

IV. Empirical strategy

To estimate the effect of an expansion on the availability of public home-care of the elderly on the labor market outcomes of their adult children, we apply a reduced form model 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. We use the pre-reform coverage level as a pre-determined indicator of the intensity of municipal response to the reform. The key outcomes we examine are indicators of labor supply (including employment, earnings, and sickness absence), residential mobility of adult children, and parental health.

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 and construction delays, and a longer term post-reform period starting in 2001 when projects were well established and funding levels had increased. Since the reform was much-

discussed in advance of implementation, the pre-reform year 1997 is dropped from the analysis.⁴

The main regression model is the following:

$$(1) Y_{it} = \alpha_1 + \alpha_2 Short_t + \alpha_3 Long_t + \alpha_4 (PreCoverage_i * Short_t) + \alpha_5 (PreCoverage_i * Long_t) + \alpha_6 X_{it} + c_{it}$$

where i indexes the individual, and t , time. Y represents child labor supply and other outcomes, $PreCoverage$ is the pre-reform coverage rate in the municipality of the elderly parent of individual i , $Short$ 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 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.⁵ As in Baker et al. (2008) and Haves and Mogstad (2011) we interpret α_4 and α_5 as the intention to treat effects, or the reduced form effects of the reform on outcomes Y in the short and long term respectively.⁶ This particular reduced form specification, where pre-reform coverage is a continuous variable, assumes a linear relationship between the outcome variable(s) and pre-reform coverage.⁷

The reform led to care expansions at both the intensive and extensive margins and to improvements in care quality but we observe only extensive margin (coverage) changes. To the extent that municipalities with high pre-reform coverage invested disproportionately in quality improvements and hour expansions, our estimates of the reform's impact will be biased towards zero. Our model 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 adult child outcomes do not bias our results.

Appendix 1 provides a detailed comparison of the demographic, economic, fiscal, and political characteristics of municipalities with high and low pre-reform coverage, defined as municipalities with pre-reform coverage above and below the median respectively, in the year before the reform. With the exception of the home-care coverage rate, the average differences between these municipalities are small. High coverage municipality populations are slightly better-educated, less likely to be married, and more urban. Municipalities with higher 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 percent higher in the high coverage municipalities, though differences in income and employment are trivial.

Our key identifying assumption is that the change in labor market outcomes for adult children before and after the elder care reform would have been the same in municipalities with high and low pre-reform coverage in the absence of the reform—that is, that the elder care coverage is not a proxy for other unobserved determinants of labor market trends or responses to other reforms implemented in the same period. Appendix 2 lists relevant reforms from the same period and briefly explains why they are not likely to have affected outcomes for our sample of middle-aged women. In addition, we find that other trends over this period, including changes in income, disposable unrestricted municipal resources, education levels, and employment rates, were not correlated with pre-reform coverage levels.⁸

V. 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 contain unique identifiers that make it possible to match children to their 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 care⁹ 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 main sample is cross sectional and consists of men and women with only one surviving parent who is at least 80 years old. New individuals enter each year as their parent turns 80 while some drop out of the dataset due to deaths. Since the primary caregiver for frail elderly who are married is usually the spouse, the sample restriction to single surviving parents 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, with and without siblings, we focus our discussion and robustness checks on the sample of daughters who are only children.

Our measures of labor supply include the adult child's annual earnings, employment status and insured absences from work (sickness absence). Earnings are measured as total gross pension-qualifying employment-related income 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 employment dummy is based on a categorical variable indicating hours worked. If an individual is registered as having a

positive amount of working hours, the employment dummy is set equal to one. Due to the broad categories of the working hour variable, it is not well suited to study adjustments at the intensive margin. We therefore study alternative income cut-offs, to capture intensive margins of working. We use the national minimum gainful activity level often referred to as “G” to define thresholds. The G levels are administratively set and used by the national social security system to define labor market status, and to determine eligibility for unemployment benefits as well as disability and old age pension.¹⁰ The sickness absence variable is a dummy set equal to one for individuals who have received public benefits for a work absence of at least 16 days (requiring physician authorization). We also examine days of insured absence (in addition to those 16 days), and both receipt and days of sickness absence conditional on employment. One mechanism for labor force withdrawal, particularly for older frail workers, is to apply for a disability pension, and we define a dummy variable indicating whether an individual has received disability pension during a year. Finally, we construct an indicator of whether the daughter is employed in the elder care sector for a robustness check on the effects of the reform on sector-specific shifts in employment opportunities.¹¹

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) as our health outcome. Pre-reform averages for all the outcome variables are reported in the first column of the respective tables in the results section.

VI. Results

In Table 2 we report the reduced form estimates, or intention to treat effects (ITTs), for our main subsample of adult daughters with no siblings (about 18 percent of the full sample of daughters). 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). There is a strong effect of the policy change on the fraction of the population aged 80+ receiving home-care at all in the long-term, but no significant impact in the short-term. This supports institutional descriptions of the reform that report a lag in the actual expansion of home-care services.

There are no significant impacts of the reform in the long-term on our measures of employment, earnings or disability pension receipt, implying no impact on the extensive margin of labor supply.¹² On the intensive margin however, we do see some significant responses. In particular, we observe a substantial and highly significant response on spells of insured sickness absence: working daughters are less likely to experience any spells of sickness absences (of more than 16 days) after the reform. This reduction is 2.6 percentage points from a base of 13 percent or, conditional on working, 3.3 percentage points from a base of 16 percent, and translates to 6.3 days fewer insured days absent from work in a year if the parent lives in a municipality with a care coverage rate that was 10 percentage points lower pre-reform. There are thus very substantial responses to the reform for a subsample of women who are the most likely to be engaged in eldercare. These responses are also evident in the raw data. Figure 3 shows, based on the same subsample, the upward trends in sickness absence rates; all municipalities have higher absence rates after the reform. This increase is lower, however, in those municipalities that had low coverage pre-reform.¹³

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). Since there is not a good measure of hours worked in our data, we examine possible movements along the earnings distribution, using the government-set gainful activity levels. In the period studied, the mean earnings in a full time male job in Norway is about 6G, while mean earnings for full-time working women is closer to 5G. A 75-80 percent position, which is very common in female-dominated professions like nursing and teaching, would pay on average 4G (Hansen and Skoglund 2003). There appears to be a response to the reform at the higher end of the earnings distribution, and the probability of earnings corresponding to a full-time job and to the typical female job increases in the long-term post-reform period.

If elder care responsibilities restrict the residential mobility of adult children, then 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—before the reform only 1 percent of the sample moved during the last year and this does not seem to change as a result of the reform. 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.

In summary, the expansion of elder care does not seem to prompt discrete responses from daughters, such as moving to another municipality or entering the labor market, but for the sample of daughters without siblings it influences labor supply at the intensive margin for those already working. For sons without siblings and daughters with siblings, we do not see significant effects of the reform (Table 3) except for small effects on daughters' earnings. For the remaining outcomes and robustness checks, we restrict our focus to the

subsample for which the reform seems most relevant; daughters without siblings.

We finally 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 within 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 percent.

VI.A Robustness Checks

The expansion of formal elder care could have had a mechanical effect on daughters' labor supply by providing new employment possibilities and thus increasing their hours worked (as we find no effects on labor force participation per se). About 7 percent 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. Though there is an increased probability of being employed in the elder care sector after the reform, we find no relationship between pre-reform coverage and employment in the elder care sector in our sample of daughters.

Table 4 reports long term ITTs for potential placebo samples of single child daughters whose care responsibilities are expected to be relatively low—those with both parents alive, no 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, with one exception. The coefficient in the sickness absence equation for working daughters

with no living parents is marginally significant at the 10 percent 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 find some evidence that this is the case. 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.¹⁴

Table 5 reports the results of different specifications where we expand and shrink our sample, and the results are mainly robust.¹⁵ 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 smaller although in the same direction when we include the municipalities with highest and lowest 10 percent pre-reform coverage rates. The linear specification is no longer a good approximation as is apparent in Figure 3 where the 95 percent confidence intervals increase dramatically outside the dotted lines that indicate the 10 percent lowest and highest pre-reform coverage rates. In columns 4 and 5 in Table 5 we perform analyses including 1997 (omitted in the previous models). The first column includes 1997 as a pre-reform year while the second includes 1997 as a post-reform year. The estimates are not significantly different across specification, though they are smaller and only days absent is significant when 1997 is included in the pre-reform period. The last two columns in Table 5 show that a discrete model, where we divide the sample into control and treatment municipalities based on pre-reform coverage and run a difference in difference model (DinD), gives very similar results to the ones we get from our main linear specification, whether the treatment cut-off is at the median or the mean of the pre-reform coverage distribution. The results of the discrete model, unlike the linear specification, are not sensitive to the inclusion of the tails of the coverage distribution or to the inclusion of 1997 in the pre-reform period (Lundberg et al. 2014).

VII. 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-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 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, deserve 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.¹⁶

The relative size of the two channels (sickness absence and reduced working hours) could vary between groups, and we speculated that higher educated daughters may have jobs with more temporal flexibility and perhaps less need for formal sickness absence. The results in Table 6 show that though the reform does seem to have a somewhat stronger effect on both sickness absence and reaching earnings thresholds for less-educated daughters, the differences between the two groups are not statistically significant.

The doctor's certification requirement for insured sickness absence is intended to prevent 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 considerable 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. In combination with other studies of the use of sickness absence in Norway, however, 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. We also cannot generalize these results beyond the Norwegian context, but adult children in the United States face many of the same challenges in arranging the care of elderly parents as do adult children in Norway; high levels of full-time female employment and limited availability of paid leave that can legitimately be used for this purpose. Formal care provided by the market may differ from government-provided formal care in its substitutability with informal care and will certainly introduce a strong interaction with family income in the American context. More broadly, however, 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 for this particular group of adult children in Norway, formal care of the elderly appears to substitute, to some extent, for the informal caregiving demands that conflict with rigid work schedules.

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Endnotes

i. We thank colleagues and seminar participants at several universities and conferences for valuable feedback and suggestions.

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.

2. Information on the history of formal elder care in Norway is gathered from (Norwegian Ministry of Health and Care Services 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 percent and 78 percent for home-care services and between 0 and 55percent for institutional services.

4. We include robustness checks for alternative ways of treating 1997 in section 6.

5. Given that the policy variation we use is at the municipality-year level, this could be an alternative clustering level. However, since the error term for a municipality is likely correlated over time, we use the more conservative clustering at the municipality level.

6. The long- and short-term effects reported in the tables change the signs of α_4 and α_5 and scale the coefficients by 10 to give a more intuitive interpretation.

7. In our main specification, we drop the municipalities with the highest and lowest pre-reform coverage, as the linearity assumption is not well-supported with these outliers included. In earlier work, we take a differences-in-differences approach, dividing the municipalities into treatment and control groups at median pre-reform coverage rates and find consistent results that are robust to the inclusion of outliers (Lundberg et al. 2014).

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

8. Correlation tests are available on request.

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

10. In 2006 1G represented approximately US \$10,000.

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. The effect on employment in the short run is significant. Since this is the only robust significant finding we have in the short term (the probability of earning more than 2 G is highly correlated with the employment dummy) we omit short-term estimates from subsequent tables. The short-term effect on employment remains in our robustness checks: this could be because the reform led to more rapid increases in care at the intensive margin, or it could be a chance significant effect among many outcomes.

13. The 95 percent confidence intervals in the figures are based on observations aggregated at the municipality level. Our regressions (which are based on individual observations and include various controls) therefore produce more precise estimates and are able to identify statistically significant effects.

14. Results are available on request.

15. We do not include the first stage results in this table since all estimates for all sub groups are similar to the main specification estimates in Table 2.

16. Parents are each entitled to 10 days of paid leave to take of their sick children.

Table 1

Post-reform growth in home-based care coverage

Absolute change in proportion of 80+ receiving any home-based care from 1993/96 to 2001/05	
Pre-reform home-care coverage (1993-1996)	-0.784*** (0.050)
Pre-reform institutional care coverage (1993-1996)	-0.047 (0.069)
Share of population >67 in 1997	-0.00002*** (7.27e-06)
Share of population >80 in 1997	0.00006** (0.00002)
Disposable municipality income in 1997	-0.003 (0.006)
Constant	0.329*** (0.027)
N (number of municipalities)	435

Notes. Robust standard errors in parentheses. *, **, and *** refer to statistical significance at the 10 percent, 5 percent, and 1 percent level respectively.

Table 2

Main outcomes; Daughters, no siblings

	Pre-reform Mean	Short term ITT	Long term ITT	Observations
First stage:				
Home-care coverage 80+	0,38	0,017 -0,016	0,071*** -0,009	74456
Outcomes:				
Employment	0,72	0,035** -0,016	0,023 -0,017	74526
Ln earnings	11,77	0,039 -0,036	0,057 -0,039	64211
Prob. Earnings > 1G	0,81	0,026 -0,016	0,011 -0,017	74383
Prob. Earnings > 2G	0,75	0,042** -0,017	0,033* -0,018	74383
Prob. Earnings > 3G	0,63	0,019 -0,019	0,03 -0,02	74383
Prob. Earnings > 4G	0,5	0,021 -0,019	0,035* -0,02	74383
Prob. Earnings > 5G	0,33	0,028* -0,014	0,040** -0,018	74383
Prob. Earnings > 6G	0,17	0,016 -0,012	0,021 -0,014	74383
Sickness absence (at least one spell during past year)	0,13	-0,001 (0,011)	-0,026* (0,014)	74526
Sickness absence (cond. on work)	0,16	-0,006 -0,015	-0,033** -0,016	53972
Days absent (cond. on work)	14,69	-1,941 -2,333	-6,298*** -2,152	53972
Disability pension	0,1	-0,002 -0,014	-0,001 -0,018	74526
Move to another municipality (within next year)	0,01	0,000 -0,004	0,001 -0,003	74309
Parental death (within next year)	0,08	0,001 -0,008	0,005 -0,008	74526

Notes: The linear specification interacts the short and long term reform coefficients with

the pre-reform coverage. The interpretation is the effect of having 10 pp lower coverage pre-reform (which corresponds to a 7 pp 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 percent, 5 percent, and 1 percent level respectively.

Table 3

Alternative samples

	Sons, no siblings			All daughters		
	Pre-reform mean	Long term ITT	Observations	Pre-reform mean	Long term ITT	Observations
Employment	0,74	0,018 (0,014)	100700	0,73	0,011 (0,01)	416330
Ln earnings	12,3	0,035 (0,030)	92286	11,74	0,043** (0,019)	367064
Prob. Earnings > 1G	0,91	-0,003 (0,013)	100480	0,82	0,012 (0,009)	415594
Prob. Earnings > 2G	0,88	0,006 (0,014)	100480	0,75	0,017* (0,010)	415594
Prob. Earnings > 3G	0,84	0,011 (0,014)	100480	0,61	0,007 (0,012)	415594
Prob. Earnings > 4G	0,79	0,016 (0,014)	100480	0,48	0,014 (0,011)	415594
Prob. Earnings > 5G	0,69	0,007 (0,016)	100480	0,3	0,019* (0,011)	415594
Prob. Earnings > 6G	0,53	0,010 (0,017)	100480	0,14	0,013 (0,009)	415594
Sickness absence (at least one spell during past year)	0,12	-0,003 (0,010)	100700	0,14	-0,008 (0,006)	416330
Sickness absence (cond. on work)	0,12	0,016 (0,011)	74034	0,17	-0,012 (0,008)	308459
Days absent (cond. on work)	12,5	1,702 (1,606)	74034	15,6	-1,60 (1,061)	308459
Disability pension	0,08	0,004 (0,011)	100700	0,1	-0,000 (0,008)	416330
Move to another municipality (within next year)	0,014	0,001 (0,003)	100377	0,02	-0,000 (0,002)	415201

Notes: The linear specification interacts the short and long term reform coefficients with the pre-reform coverage. The interpretation is the effect of having 10pp lower coverage pre-reform (which corresponds to a 7 pp 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 percent, 5 percent, and 1 percent level respectively.

Table 4

Placebos

	Long term ITT; daughters, no siblings, key outcomes		
	Daughters with no living parents	Daughters with two living parents	Daughters with younger parent (60-72)
Employment	-0,005 (0,030)	-0.030 (0,021)	0,021 (0,030)
Ln earnings	-0,030 (0,050)	-0,014 (0,043)	-0,011 (0,049)
Sickness absence	-0,016 (0,017)	-0,014 (0,016)	-0,007 (0,015)
Sickness absence (cond. on work)	-0,038* (0,021)	0,000 (0,019)	-0,016 (0,020)
Days absent (cond. on work)	-2,065 (2,964)	0,620 (2,675)	-1,842 (3,093)
Disability pension	-0,007 (0,016)	0,012 (0,018)	-0,013 (0,016)
Move to another mun. 1 year after	0,002 (0,005)	0,001 (0,004)	0,004 (0,005)
No. of observations	66998	48079	41918

Note: The linear specification interacts the short and long term reform coefficients with the pre-reform coverage. The interpretation is the effect of having 10pp lower coverage pre-reform (which corresponds to a 7 pp 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 percent, 5 percent, and 1 percent level respectively.

Table 5

Robustness checks

	Long term ITT; Daughters, no siblings						
	Shrinking/expanding sample			Different treatment of 1997		Splitting coverage => DinD	
	Drop Oslo/ Bergen/ Trondheim	Drop rural municipalities	Include +/- 10 % of coverage distr.	Include 1997 pre- reform	Include 1997 post-reform	Split coverage at mean	Split coverage at median
Employment	0,018 (0,017)	0,029 (0,018)	0,008 (0,010)	0,018 (0,016)	0,022 (0,017)	-0,007 (0,013)	0,014 (0,013)
Ln earnings	0,045 (0,040)	0,049 (0,042)	-0,019 (0,024)	0,047 (0,035)	0,057 (0,039)	-0,011 (0,035)	0,042 (0,030)
Sickness absence	-0,025* (0,014)	-0,027* (0,015)	-0,008 (0,007)	-0,018 (0,012)	-0,026* (0,014)	-0,020* (0,011)	-0,021*** (0,008)
Sickness absence (cond. on work)	-0,031* (0,017)	-0,033* (0,018)	-0,011 (0,009)	-0,024 (0,014)	-0,033** (0,016)	-0,027** (0,013)	-0,032*** (0,009)
Days absent (cond. on work)	-6,241*** (2,168)	-5,207** (2,319)	-0,291 (1,18)	-4,615** (1,849)	-6,238** (2,154)	-4,950*** (1,43)	-3,493** (1,401)
Disability pension	-0,001 (0,018)	-0,006 (0,019)	-0,006 (0,009)	0,001 (0,016)	-0,002 (0,018)	-0,003 (0,014)	-0,001 (0,011)
Move to another mun. 1 year after	0,000 (0,003)	0,000 (0,004)	-0,001 (0,002)	0,002 (0,003)	0,001 (0,003)	0,002 (0,003)	0,001 (0,002)
No. of observations	58523	67772	93300	79558	79558	74526	74526

Notes: The linear specification interacts the short and long term reform coefficients with the pre-reform coverage. The interpretation is the effect of having 10 pp lower coverage pre-reform (which corresponds to a 7 pp 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 percent, 5 percent, and 1 percent level respectively. In the DiDs in the two last columns, municipalities are divided into treatment and control groups based on their average level of home-care coverage in 1993-1996; those with coverage rates below the mean/median are classified treatment municipalities, those with coverage above the mean/median are control municipalities.

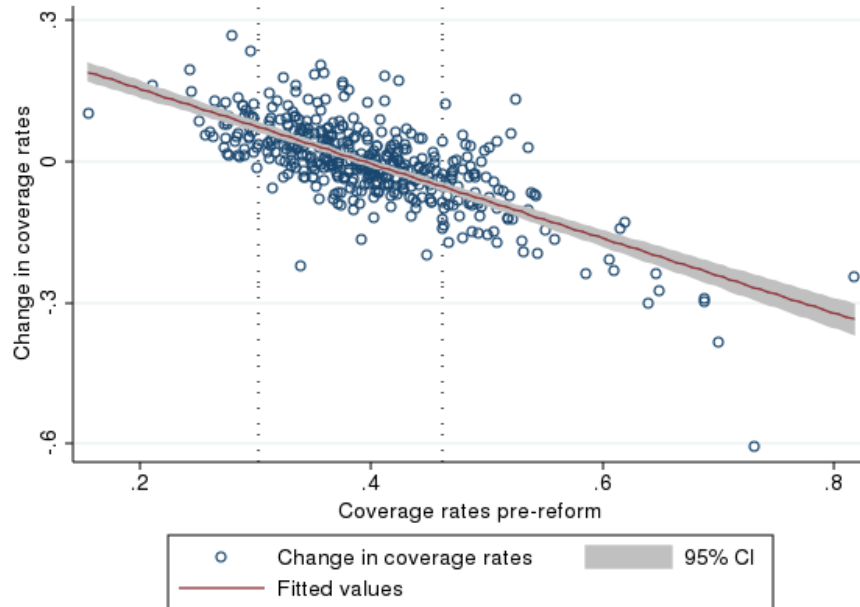
Table 6

Intensive margin outcomes for different education levels

	Daughters, no siblings			
	Low education (≤ 10 y)		High education (> 10 y)	
	Long term ITT	Observations	Long term ITT	Observations
Absence				
Sickness absence (at least one spell during past year)	-0,028* (0,017)	41369	-0,015 (0,019)	33157
Sickness absence (cond. on work)	-0,048** (0,022)	27432	-0,009 (0,022)	26540
Days absent (cond. on work)	-6,973* (3,858)	27432	-5.631** (2,503)	26540
Earnings/Working hours				
Prob. Earnings > 1G	0,017 -0,023	41310	-0,001 (0,025)	33073
Prob. Earnings > 2G	0,048* (0,027)	41310	0,006 (0,025)	33073
Prob. Earnings > 3G	0,050* (0,03)	41310	-0,000 (0,023)	33073
Prob. Earnings > 4G	0,033 (0,025)	41310	0,026 (0,029)	33073
Prob. Earnings > 5G	0,026 -0,02	41310	0,053 (0,034)	33073
Prob. Earnings > 6G	0,02 -0,013	41310	0,015 (0,032)	33073

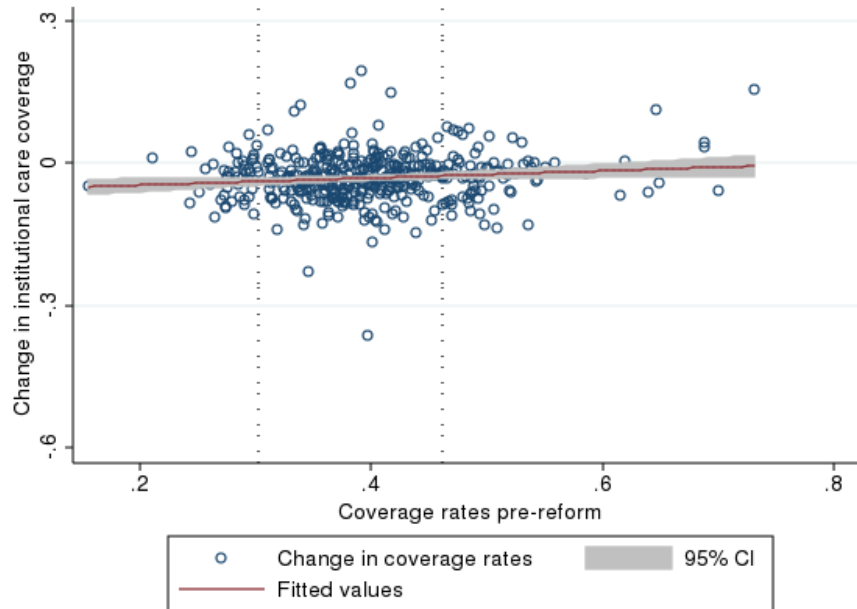
Note: The linear specification interacts the short and long term reform coefficients with the pre-reform coverage. The interpretation is the effect of having 10 pp lower coverage pre-reform (which corresponds to a 7 pp increase in long term coverage post reform) on various outcomes. Standard errors clustered at municipality level in brackets. *, **, and *** refer to statistical significance at the 10 percent, 5 percent, and 1 percent level respectively.

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



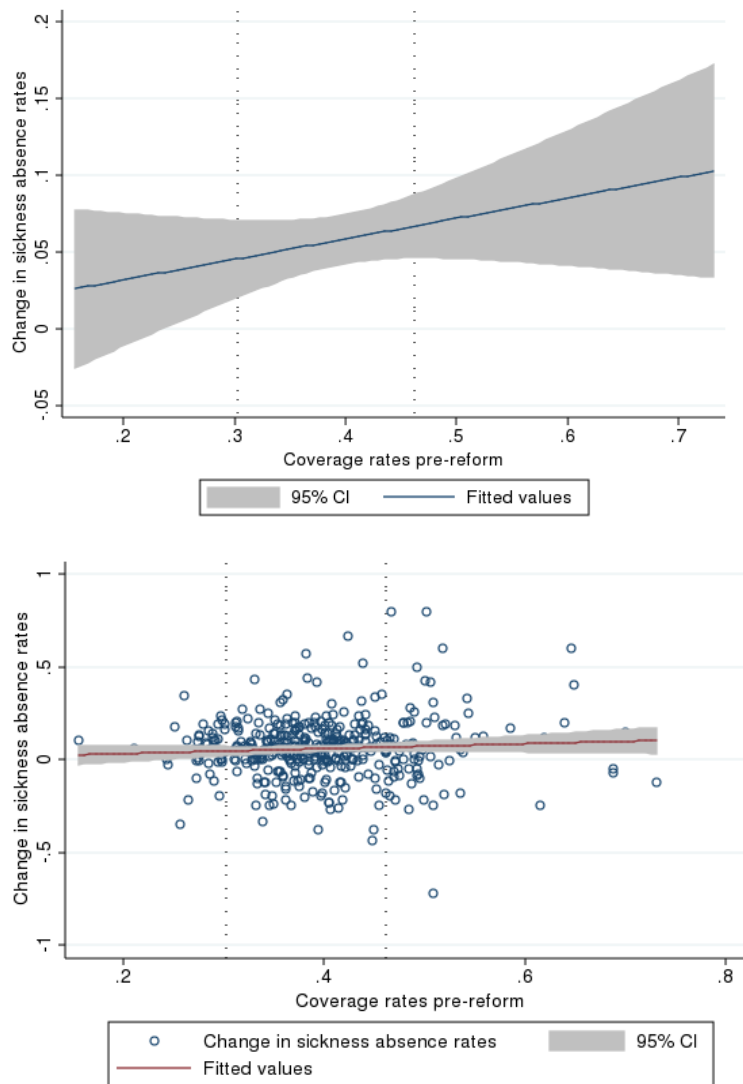
Notes: Pre-reform home-care coverage is graphed against the absolute change in home-care coverage rates from the pre-reform period 1993-1996 to the post-reform period 2001-2005. The dotted, vertical lines represent the 10th and 90th pre-reform coverage percentiles.

Figure 2: Institution care (nursing homes)



Notes: Pre-reform home-care coverage is graphed against the absolute change in institutional coverage rates from the pre-reform period 1993-1996 to the post-reform period 2001-2005. The dotted, vertical lines represent the 10th and 90th pre-reform coverage percentiles.

Figure 3: Change in probability of sickness absence



Notes: Pre-reform home-care coverage is graphed against the absolute change in probability of being absent (for a period longer than 16 days) from the pre-reform period 1993-1996 to the post-reform period 2001-2005. The dotted, vertical lines represent the 10th and 90th pre-reform coverage percentiles. The graphs are based on the sample of

single daughters. The figure on top includes all observations at the aggregate municipal level while the one below zooms in and shows only the fitted line and confidence intervals.

Appendix 1

Descriptive statistics for municipalities with low and high pre-reform coverage in 1997

	Pre-reform coverage<median		Pre-reform coverage>median	
	Mean	Standard deviation	Mean	Standard deviation
Population	9477	16146		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	0,08	0,269	0,076
females	0,362	0,078	0,35	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	0,08
- 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
Unrestricted 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	0,05
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	0,36	0,13	0,336

Appendix 2

Other potentially interesting reforms in the period 1997-2001

Reform	When	What	Potential effect (on single child daughters with one alive parent 80+)
Cash for care reform	July 1998	Universal reform providing families with children 1-3 years old with a cash transfer if they did not use subsidised child care	Some effects on labor supply of mothers have been identified in the literature. (Naz, 2004; Schøne, 2004; Hardoy et al., 2010; Naz, 2010; Drange, 2011) The daughters in our sample, with the exception of the placebo sample of daughters with younger parents, are more likely the grandparents of the affected children => if anything the reform should be expected to have a negative effect on labor market participation in the mentioned placebo sample, but we were not able to identify any effects at all on this sample.
Pension reform	January 1998	The minimum annual payment for single persons was raised by 17%, from 69360 NOK to 81360 NOK	Could potentially make it more attractive to retire for the group of daughters that were not strongly attached to the labor market => lower labor market participation in the lower end of the wage distribution could be a consequence. However, we observe no change on this threshold but rather an increased probability of being in a relatively high earnings category.
Work-encouraging reform targeted at single parents	January 1998	The maximum benefit period for support was substantially reduced, from 10 to 3 years	Increased labor market participation and earnings among single mothers have been identified in the literature (Mogstad et al., 2012). These mothers are however younger than the average mother in our sample, with one possible exception; the placebo sample of daughters with younger parents. In the placebo test however, we did not identify any effects for these daughters.
Disability reform	January 1998	Tightening the eligibility for disability pension for younger persons	This reform should not affect daughters in our sample, again with a possible exception of the placebo group with younger parents. Among these daughters the reform could lead to a reduction in disability retirement. However, we observe no change in the probability of entering disability retirement.