Preventive Home Visits

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Abstract

This paper evaluates the introduction of preventive home visits (PHV) for older people in Norway. Their purpose is to support autonomy and independence as well as preventing disability and nursing home admissions. We contribute to the literature by exploiting a natural experiment in Norwegian municipalities. Our results show that the introduction of a PHV program significantly changes the use of local public resources away from nursing homes, while increasing the utilization of home-based care. Further, PHVs lead to a decline in hospital admissions by 5 percent – whereas treatments for mental health conditions remain unaffected. Mortality is reduced by 4 percent in the age group 80 and above.

Keywords: preventive home visits, long-term care, natural experiment, primary prevention

JEL classification: C23; H75; I18; J14; J18

I. Introduction

The economics of long-term care (LTC) is receiving increasing attention both from the research community and from policy makers due to the belief that an aging population will greatly increase the demand for LTC services and become a large burden for public budgets. According to OECD projections, public spending on LTC across European countries can be expected to increase by as much as 70 per cent on average over the next four decades (OECD, 2016). This trend is mirrored by trends in private spending and in informal care provision, which are likely to follow a similar path. Although improvements in health among older people would decelerate this process – recent economic literature suggests that such improvements may have a limited impact on LTC expenditures (cf. Karlsson et al., 2018, for a review of that literature).

Considering this outlook, it is natural that policy makers around the world exhibit an increasing interest in preventive measures and interventions promoting the independence of older people. The 2015 *World report on ageing and health* identified prevention and early detection of chronic disease as one key focus area for public health in general and for the LTC sector in particular (WHO, 2015). This appeal to increased focus on prevention has been echoed by various governments. The U.S. National Prevention Strategy calls for an increased availability of clinical and community-based preventive services and home visiting programs (National Prevention Council, 2011) and the Affordable Care Act introduced annual preventive care visits for the older population (Chung et al., 2015).

However, the empirical evidence regarding many popular preventive and independence-promoting interventions is either weak, fragmented or completely missing. The studies that do exist are typically small and cover specific populations, which raises doubt about their external validity and scalability. Also, the outcomes considered are often very narrowly defined both in time and in scope. Thus, the lack of evidence regarding the effectiveness of preventive interventions for older people remain a notable gap in the literature (Mayo-Wilson et al., 2014; Moyer, 2012).

In this paper, we analyze the short-and long-term effects of a preventive home visit (PHV) program for older people, which was rolled out in Norwegian municipalities during the past two decades. The Norwegian PHV program has as its primary aim to promote independent living among older people, and in particular to prevent a decline in functional capacity and admissions into nursing home care (Tøien et al., 2014). The main component of the program is a visit by a health care worker – typically a nurse – who evaluates the older person's physical and mental health condition, and assesses the appropriateness of their home environment. The visit is followed by a recommendation in each individual case – including suggested solutions to problems that have been identified, and measures to prevent new problems from arising (van Haastregt et al., 2000).

Using a difference-in-difference strategy with municipality fixed effects, we estimate the effects of the program on a range of outcomes. In a first step, we assess whether the program has had the intended effect on resource allocation within the LTC sector. We find that the introduction of PHVs leads to a substitution of home care for nursing home care: the effect size is around 2 percentage points in the 80+ population (from a baseline of 19 per cent in nursing home care and 35 per cent in home-based care). However, this reduction in care intensity does not come at the expense of a deterioration in older people's health. Quite to the contrary, the indicators of health that are available suggest if anything an improvement in older people's health: hospital admissions are reduced by around 7 per cent, and mortality rates decline by almost 5 per cent in the years following the introduction of PHVs. All our results are robust to a battery of robustness checks.

Our paper contributes to several different strands of the literature. First, there is a large literature in economics studying the determinants of demand for and utilization of long-term care services. Most empirical studies in this literature either estimate the impact of one particular determinant on demand for services – e.g. income (Goda et al., 2011; Tsai, 2015) – whereas others consider a multitude of determinants and evaluate their quantitative importance (de Meijer et al., 2011; Balia and Brau, 2014). Hardly surprising, latter studies typically do confirm that disability is a key determinant of LTC utilization. However, they do not ask the more fundamental question of whether this risk factor is *malleable* – which is what we set out to do in this paper. Likewise, our paper contributes to the literature on substitutability between different types of health and long-term care services (Orsini, 2010; Bakx et al., 2015; Mommaerts, 2008; Costa-Font et al., 2016, 2018) by introducing an exogenous source of variation in the demand for those services.

Second, our paper does, in a broader sense, contribute to the literature on the returns to care technologies. In general, there is a strong case to be made for health care being an important driver of improvements in life expectancy since around 1935 (Catillon et al., 2018). The growing literature on early life health shows that a number of medical interventions can be highly cost effective, when considering their immediate and long-term effects on health and other outcomes (Bhalotra et al., 2016, 2017; Bharadwaj et al., 2013). For adults, results are more mixed, as some innovations have been found to be highly cost-effective (Duggan and Evans, 2008) whereas others deliver questionable returns (Skinner et al., 2006). This holds in particular for preventive measures. Screening programs for certain conditions have been found to be effective: Cutler (2008) attributes 35 per cent of the 1990–2004 reduction in cancer mortality to screening. However, recent research has highlighted that promising results found in RCTs might not scale to the overall population, considering the typically much lower compliance rates (Kim and Lee, 2017). General health-screening programs have been found to be of little value: Hackl et al. (2015) estimate that an Austrian program is associated with an increase in costs but not with any discernible improvements in health.[1] Kim et al. (2017) report similar findings for Korea and attribute the lack of impact to the very small behavioral responses to the program. On the other hand, it has been argued that evidence-based screening programs for older people may be effective (Chung et al., 2015). Our paper provides evidence suggesting that this may indeed be the case.

Third, we contribute to a large and diverse literature in medicine, in particular in geriatrics and nursing, on the effects of preventive home visits for older people. A large number of studies exist from a diverse set of countries such as the Netherlands (van Rossum et al., 1993), Switzerland (Stuck et al., 2000), Canada (Dalby et al., 2000), and Denmark (Kronborg et al., 2006) – and a wide range of different outcomes have been considered, such as mortality, quality of life, psychosocial functioning, falls and admission to hospital or LTC institutions. The typical study is conducted as an RCT with less than 1,000 participants in total. Despite a large number of studies using similar designs in similar settings, no consensus has emerged regarding the effectiveness of PHVs. A recent metastudy of 64 RCTs concluded that there is no consistent evidence of benefits for the range of outcomes considered (Mayo-Wilson et al., 2014). This finding contrasts to what has been reported in previous meta-studies, which report improvements in mortality, functional status and admissions into LTC (Huss et al., 2008; Elkan et al., 2001; Stuck et al., 2002). Our study makes two significant contributions to this literature: first, it is population-based and thus representative for a much larger population than those studied in RCTs. This alleviates concerns regarding external validity but also about selective

[1] However, some caution is required here: as (Hall, 2011) points out, health is not the only outcome of interest in an economic evaluation.

compliance outside the controlled environment of an RCT. Second, we are able to follow subjects over a longer time period than most RCTs allow: the follow-up period we consider is more than five years for all but the most recently introduced PHVs. This is of great importance since many public health interventions tend to have effects that fade in the long run.

The rest of the paper is organized as follows. In the next section, we give an overview of how long-term care to older people is organized in Norway, with particular focus on PHV and their introduction. In Section III. we present the various administrative data sources that we draw upon in our empirical analysis, and the estimation techniques we employ. Section IV. presents our empirical results – including the application of synthetic control approaches. Section V. concludes.

II. Institutional Background

A. LONG-TERM CARE IN NORWAY

In Norway, LTC is an integrated part of the extensive public health care system. In this system, services are universally available, predominately financed by general taxes and publicly provided (Magnussen et al., 2007; Karlsson et al., 2012). A fundamental ethical principle is that access to health and LTC services should be determined by health needs only (Ringard et al., 2013; Olsen, 2011).

The system is semi-centralized (Hagen and Kaarbøe, 2006). The central government determines the rules and regulations that define the legal bounds of public funding and provision, and the division of responsibility among government levels (Øien et al., 2012). Further, the central government is directly responsible for the funding and provision of specialized health care services. The responsibility of funding and provision of primary care services is decentralized to the municipalities – the lowest level of government – of which there are 422 as of 2019 (428 in 2013) in total. Among the primary care services are social as well as community health services provided to persons with LTC needs.

The LTC services the municipalities are required to provide can be broadly divided into nursing and home-based care services. Nursing homes are medical institutions with around-the-clock skilled nursing and care services. They are regulated with respect to staffing and service levels. The service must include all necessary health and care services, board and lodging. Home-based care includes home nursing, practical home help and community housing.

Home nursing is a skilled nursing service provided to dependent persons living in their own homes or in community housing (Fjørtoft, 2012; Øien, 2014). Home helpers provide help with instrumental activities of daily living such as cooking and cleaning. Community houses are adapted for persons in need of LTC, and are predominately for persons who are no longer able to live independently at home, but are not (yet) in need of nursing home care (Hagen et al., 2011; Øien, 2014). Individuals, or any person acting on behalf of an individual, must submit an application to the municipality to receive LTC services. Municipalities are restricted to allocating services according to health needs and independently of socioeconomic status and potential informal care provided by relatives (Ringard et al., 2013; Jakobsson et al., 2016).

The responsibility of municipalities to pay for LTC services is extensive. In 2010, public LTC expenses comprised 3.2 percent of GDP, which makes LTC the largest municipal sector in terms of share of total municipal spending (Hagen et al., 2011). Norway is among the countries in the OECD that spends most on LTC as a share of GDP (Francesca et al., 2011). The large economic burden of LTC and the concern that an aging population will increase future demand for LTC, have led policy makers at different levels to focus more on measures that can prevent and postpone care needs. One such measure is preventive home visits.

B. PREVENTIVE HOME VISITS

According to Norwegian legislation, municipalities are required to offer its citizens health-promoting and disease prevention services (§3-2 *Helse- og omsorgstjenesteloven*, LOV-2011-06-24-30 C.F.R. (2012)). The requirement is implemented by providing information, advice and guidance. The legislation is general in its nature. With the exception of public health centres for children and their parents and school health care, it does not specify what measures to take, or for what target populations. Although the central government recommends municipalities to organize PHVs for older adults (Norwegian Ministry of Health and Care Services, 2016), it is up to each municipality to decide whether they will use their resources for PHV or for other preventive measures. This is in contrast to the neighbour country Denmark where PHV for older adults has been a mandatory offer since 1996 (borger.dk, 2017; Førland and Skumsnes, 2017b).

Exploiting their self-determination, the municipalities have chosen different prevention strategies for the older population. A wide range of initiatives have been considered, such as establishing arenas for social participation; active partnership with families and the voluntary sector; early detections of health problems; and establishment of early efforts to solve such problems. Community work, group-based activities, and individual-based efforts like PHV and reablement[2] are typical examples (Førland and Skumsnes, 2017b). All these local programs can be categorized under the concepts of 'productive ageing' and 'active ageing'.[3]

A few municipalities introduced PHV already at the beginning of the 1990's. These efforts were initiated by dedicated professionals, most often nurses, occupation therapists, physiotherapists and social workers who were inspired by Danish colleagues and municipalities. Together with the United Kingdom, Denmark has been a pioneer in this field in Europe ever since the 1960's (Tøien, 2019). Nevertheless, it was only in the years after 2004 that the diffusion of PHV in Norway took off, with a growth from 8 to 24 percent during the 2004-13 period. Roughly 47 percent of the introductions occurred after 2010 (Førland et al., 2015). The municipalities where PHVs were not established reported a lack of resources and a shortage of qualified staff as the most prominent reasons not to have established a programme – whereas only few municipalities reported not seeing a need for PHVs (Førland and Skumsnes, 2014). Figure 2a shows the location of PHV programs in 2013 and Figure 2b shows their spread over time.

The period of rapid growth in PHV initiatives was also characterised by a consolidation of the networks of devoted professionals in the municipalities. For example, annual national conferences were established, which contributed to promoting the new preventive concept. This in turn increased the interest of national authorities. The national government announced in 2013 that it was going to oblige the municipalities to introduce PHV programs (Norwegian Government, 2013). In the same year, the Ministry of Health and Care Services established a three-year national plan to develop methods and models for PHV, and in 2016, they issued a circular with a request to implement the concept (Norwegian Ministry of Health and Care Services, 2016).

Thus, the first twenty years of PHVs in Norway were characterised by a gradual shift from an entrepreneurial and local bottom-up implementation to a more centralized and top-down

[2]Reablement programs provide early and intensive home-based rehabilitation after functional decline, cf. Section C. below.

[3]'Productive ageing' refers to the potential for economically productive activities of older people (Bass et al., 1993) and 'active ageing' to 'the process of optimizing opportunities for health, participation and security in order to enhance quality of life as people age' (WHO, 2015, 2002). Both concepts have been prominent in the policy discourses of Western countries since the late 1990's, promoting the adoption of healthy lifestyles, an extended work life, later retirement and being active after retirement.

governed policy by 2013.[4] The motivation for PHVs also gradually shifted from a perspective focusing on older people's quality of life, to a view of PHV programs as a contribution to a sustainable welfare state.

PHV is an outreach service. The municipalities distribute information about PHV to every individual in a target group, and ask whether the individual would accept a PHV or not (Førland and Skumsnes, 2017a). This aspect makes PHV different from other municipal care services, in which individuals must submit an application to the municipality to get access to services.Within the national guidelines, the municipalities have a great degree of freedom in how they design their PHV programs. Despite the discretion, the programs are homogeneous in several crucial aspects. All the PHVs we study have the explicit aim of preventing nursing home admissions and reducing the need for formal home-based care (Førland et al., 2015; Førland and Skumsnes, 2017a). Another common goal is early detection of health problems by improving health literacy in the older population. The PHVs are always conducted by a health care worker with extensive work experience and a tertiary degree; typically a nurse. The overwhelming majority of municipalities offer home visits either to all residents in the target group, typically all 80-year-olds in the local community, or to all members of that group who are currently not using any LTC service. The remainder typically narrows down eligibility to individuals for whom there is an indication (from GPs or elsewhere) that their condition might deteriorate. The home visit normally lasts 60-80 minutes and is conducted as a health-focused conversation. The topics covered in that interview may vary at the local level – but in general, the same topics are covered almost everywhere: physical health, living arrangements, safety at home, nutrition, social networks, aids, and activities (Førland and Skumsnes, 2014).

Eligible individuals are informed by the municipality per mail and phone. On average, 52 per cent of eligible individuals take up the offer of a PHV; however, the takeup rate varies quite substantially across municipalities. In around a third of cases, the PHV results in a measure of some kind – like a referral to a health care worker or adaptation of the person's home (Førland and Skumsnes, 2014).

[4] In addition, the diffusion of PHV programs reflect a growing influence of ideological trends related to concepts like preventive action; active care; active ageing; healthy ageing; autonomous ageing; and productive ageing in Norwegian municipalities (Helse- og omsorgsdepartementet, 2009, 2013, 2015). It also coincided with growing worries about the challenges brought by population ageing for the sustainability of the welfare state.

C. REABLEMENT PROGRAMS

The results of our empirical analysis may not be interpreted as causal if there are other interventions which correlate with the introduction of of PHVs. As it turns out, reablement programs represent an obvious candidate for such a confounding intervention: they have similar aims as PHVs and were introduced in the same period.

Reablement programs have been implemented in Norwegian municipalities since 2010. The programs address people with a variety of health challenges and have partially been funded by grants from the central government (Førland and Skumsnes, 2016). Langeland et al. (2016) and Langeland et al. (2019) describe and evaluate the reablement programs. The average age of recipients is 78 years and the most common health challenges are fracture and dizziness/balance problems. A reablement intervetion normally lasts for 4-10 weeks and the primary focus is to establish a dialogue to identify activities in which the individual would like to perform better. The intervention is targeted towards achieving these activity goals identified by recipients and professionals, and measures are specified in a rehabilitation plan. A multidisciplinary team consisting most often of an auxiliary nurse, physiotherapist, occupational therapist, nurse and home helper encourage participation and stimulate daily training for the participants, including performing their daily tasks themselves. Langeland et al. (2019) report from a multi-center, clinical controlled trial involving 47 municipalities in Norway. They conclude that six months into the program, reablement seems to be a more effective rehabilitation service for persons with functional decline than traditional home-based services. After 12 months, the differences between the treatment group and the control group fade.

A survey among the municipalities in 2017 showed that 210 municipalities offered a reablement program in 2017. Among the municipalities with a reablement program, 81 municipalities also offered PHV and 23 of those municipalities introduced the prevention program with a target group of individuals aged 80 and above. Hence, there is some overlap between the two programs.

III. Data and Method

We use a range of different administrative datasets to estimate the impact of PHVs. In this section we describe the datasets used, provide some descriptives, and present our empirical strategy. Our unit of analysis is a municipality, and therefore all the datasets consist of annual observations at the municipality level. Summary statistics for our analysis sample are provided in Table 1. In Table 2 we show variable means at baseline by treatment assignment.

A. TREATMENT ASSIGNMENT

The information on preventive home visits in Norway is based on three surveys of Norwegian municipalities, carried out in 2013, 2019 and 2020. The first survey (Førland and Skumsnes, 2014), which was conducted in 2013 and included all 428 municipalities, provides the bulk of the treatment assignment data. A total number of 386 municipalities (90.2% of all) answered the questionnaire and the answers of 378 municipalities (88.3% of all) are available. More than one fifth of all Norwegian municipalities (21.7%, 93 municipalities) stated to have already introduced a preventive home visits program in or before 2013. A follow-up survey covered the situation between 2013-19, and as a complement to that survey, the authors surveyed all municipalities with missing or conflicting information in 2020.

In order to get a homogeneous treatment group with regard to the components of the treatment, we focus on municipalities which implemented PHVs targeting the oldest old (80+) population. This definition includes municipalities that offer their services exclusively to all residents at and above 80 years, as well as those municipalities where the service is only provided to individuals aged 80+ and only on demand. This group consists of 39 municipalities (9.1% of all municipalities).

A municipality is included in the analysis if the outcome variable (see below) is observed in all periods from 1994 to 2017.[5] The treatment group is defined as the group of municipalities that introduced a PHV program after 1994 and before 2017 so that each municipality is observed in at least one period before and one period after the introduction of the treatment. Further, a treatment group municipality is excluded from the analysis if its population is larger than the largest control group municipality. This additional restriction is necessary for several reasons: first, the largest cities in Norway have LTC systems which are decentralized to district units, and thus the actual treatment assignment at the individual level cannot be recovered. In addition, all the largest cities are in the treatment group and these cities differ significantly from the other municipalities in terms of economic performance as well as demographic characteristics and thus we will not achieve covariate balance between treatment and control groups if

[5]For expenditure variables, we require that the outcome is observed throughout the 2003–17 period, and for mental health admissions the relevant period is 2001–17.

these cities are included. Our final treatment group consists of 24 municipalities which introduced a PHV program between 2001 and 2015, where about half of the introductions occurred before 2011. In three cases, the programs were abolished before 2017.

The potential control group consists of 160 municipalities (37.4% of all municipalities) that neither introduced a PHV program, nor planned to introduce one.[6]

B. OUTCOME VARIABLES

In the empirical analysis, we consider two types of outcome variables, all of which are defined for the oldest old (80+) population at the municipality level. The first group of outcomes consists of variables which represent resource use in the LTC sector: real per-capita expenditure on nursing homes (*Expenditure NH*) and for home-based care services (*Expenditure HB*) for the total population; utilization rates for nursing homes (*Utilization NH*) and home-based care (*Utilization HB*), respectively.[7]

The second group of outcomes capture the extent to which the PHVs had the desired effects on older people's health: the number of hospital admissions (*Hospital Admissions*) and hospital days (*Hospital Days*) per capita; as well as age-adjusted mortality rates (*Mortality*) and hospital admissions due to mental health problems (*Mental Health*).[8] In the empirical analysis we take the natural logarithm of these outcomes. The hospital outcomes were collected from the Norwegian Patient Registry (NPR). We defined mental health admissions as all in- and out-patient admissions where the main diagnosis code is in the range F00-69 (ICD10).[9]

C. COVARIATES

Our analysis sample includes of a number of covariates, which are also measured at the municipality level. They are provided by Statistics Norway (Statistics Norway, 2019), Norwegian Centre For Research Data's regional data base (Norwegian Centre for Research Data, 2019), and the Local Government Dataset by Fiva et al. (2015). These covariates can be grouped into three categories, *economic indicators*, *demographic indicators*, and *political/other indicators*; the cor-

[6] The exact definition of the treatment and control groups depends on data availability and the observed period so both groups can actually be smaller in the actual analysis.

[7] *Expenditure NH* and *Expenditure HB* are observed in the period 2003-2017.

[8] In- and out-patient admissions attributable to mental health conditions are only observed in 2000 - 2017.

[9] The data from NPR is aggregated to the municipality level under the ethics approval granted by the Regional Ethics Committee South East, Norway (ref. 2013/796b and 2016/1687) and the Norwegian Data Inspectorate (ref. 13/00993 and 17/00115)

responding summary statistics and baseline averages can be found in the lower parts of Tables 1 and 2.

Among the economic indicators, *Average Income* denotes average gross income (wages, pensions and capital income) of all residents 17 years and older, and *Unemployment (Females)* is the share of unemployed females aged 15 to 74 in the total female population aged 15 to 74. *Property Transfers* is the lagged number of dwelling property transfers, defined as the number of transactions per capita.

The variable *Share Females* captures the share of females in the total population, and *Share Secondary or Higher Education* is measured as the proportion of graduates in the population aged 16 and above. *Live Births* is the total number of live births per capita. Three variables on migration are considered, *Net Migration*, *Share European Immigrants*, and *Share Non-European Immigrants*. The first is defined as immigration minus emigration divided by total population size, and the latter are the shares of immigrants from specific areas in the total population. *Naturalizations* is also measured as the number of citizenships granted per capita. *Population Increase* is the proportional increase in the municipality population in a year, and we define *Population Density* as population per square kilometer. *Marriages* and *Divorces* are measured as per capita rates, and *Share Living in Densely Populated Areas* denotes the number of inhabitants in densely populated areas divided by the total municipality population.

In the last category, the variable *Vote Share Left-Wing Parties* is defined as the share of valid votes for a left-wing party among all valid votes in the last municipal election. *Plantings* captures forest planting measured in 1,000 plants per capita.

We also include a dummy variable taking on the value one if the municipality has a reablement program in a certain year.

D. METHOD

In our main analysis, we estimate the effect of the PHV program on various municipality-level outcome variables using a difference-in-differences (DID) strategy with municipality fixed effects and county-level linear trends. Our main specification is thus

$$Y_{mct} = \lambda_t + \mu_m + \rho_c \times t + \tau \left(\text{PHV}_m \times \text{Post}_{mt} \right) + X'_{mt}\gamma + \varepsilon_{mct} \tag{1}$$

where Y_{mt} indicates the outcome variable for municipality m in year t, λ, μ and ρ are sets of year, municipality and county dummies, PHV_m is a binary variable taking on the value 1 in case municipality m belongs to the treatment group and 0 otherwise, the dummy Post_{mt} equals 1 for each post-treatment year, X' is a vector of covariates, and ε_{mt} is the error term. The standard errors are clustered at the municipality level and the regressions are weighted by the size of the local 80+ population.

The identifying assumption is that in the absence of a PHV program, the trajectory of the outcome Y_{mt} would have been parallel to the corresponding trends in the control group. If the identifying assumption holds, the DID estimate τ represents the causal effect of the introduction of a PHV program in the treated municipalities (ATT).

There are two main rationales for including covariates X'_{mt} in a DID design. First, the common time trend assumption may only hold after conditioning on some control variables. Second, the inclusion of covariates with explanatory power can improve the precision of estimates. Therefore, we control for a selective set of covariates in our main specifications in order to maximize the corresponding treatment coefficient's precision.

IV. Results

A. EVIDENCE SUPPORTING IDENTIFICATION

A.1. BALANCING TESTS. As a first assessment of whether covariates are balanced between treated and control municipalities, we calculate the standardized difference d, defined as

$$d^j = \frac{\bar{X}_1^j - \bar{X}_0^j}{\sqrt{\frac{(s_1^j)^2 + (s_2^j)^2}{2}}}, \tag{2}$$

where $\bar{X}_1$ and $\bar{X}_0$ are the covariate's treatment and control group averages in the pre-2001 period, and s_1^2 and s_0^2 are the corresponding sample variances. The standardized differences has the advantage of being independent of sample sizes (Austin, 2009).

As an indirect test of the identifying assumptions, and as a device to choose the appropriate specification, we provide balancing tests for a number of municipality-level variables which are unlikely to be affected by the intervention. We consider two different specifications; the first is a simple DID specification with municipality fixed effects:

$$X^j_{mt} = \lambda_t + \mu_m + \tau\,(\mathrm{PHV}_m \times \mathrm{Post}_{mt}) + \varepsilon_{mt}. \tag{3}$$

Second, we use specification (1) with county-level linear trends:

$$X^j_{mct} = \lambda_t + \mu_m + \rho_c \times t + \tau\,(\mathrm{PHV}_m \times \mathrm{Post}_{mt}) + \varepsilon_{mct}. \tag{4}$$

We estimate 19 coefficients per specification. Hence, some coefficients may turn out significant even when the identifying assumption is satisfied. To account for this multiple testing scenario, we provide adjusted p values as proposed by Romano and Wolf (Romano and Wolf (2005a) and Romano and Wolf (2005b)).

Results are presented in Table 3. The first column presents the standardized differences measured for the 1994-2000 period, whereas the following four columns present estimates from the two regression specifications mentioned above. Cleary, PHV programs are not introduced at random: treatment status correlates with a number of variables that capture the economic conditions in a municipality: having a PHV program at the end of the observation period is positively associated with baseline incomes; education levels; unemployment; and a couple of other indicators. This is not necessarily an issue for our analysis, since it only requires a common time trend. Nevertheless, we conduct robustness checks using synthetic control groups, which ensure that the control group is similar also in terms of levels.

Most covariates are balanced in the basic DID-FE (columns (2) and (3)) specification and for the specification with county-level trends (columns (4) and (5)), the estimated "effect" are significant at the five per cent level for only one variable: net migration. Accordingly, the presence of a PHV coincides with a reduction in the net migration rate by 0.2 percentage points. Since we are conducting 19 tests in total, this is fairly compelling evidence that there are no other events at the municipality level that coincide with the PHV intervention. This conclusion is also supported by the Romano-Wolf p values, which adjust for the multiple hypothesis testing. Due to its better performance, we choose model (1) with regional trends as our preferred specification.

A.2. EVENT STUDIES. A key identifying assumption is that in the absence of treatment, the treated and control municipalities would have followed a common time trend. It is thus important to compare the treatment and control groups' time trends before the treatment. We estimate event study graphs specified as

$$Y_{mt} = \lambda_t + \mu_m + \sum_{s=t_{\min}-t_{0m}}^{t_{\max}-t_{0m}} \tau_s \mathbb{1}\left(t = t_{0m} + s\right) \mathrm{PHV}_m + X'_{mt}\gamma + \varepsilon_{mt} \tag{5}$$

where Y indicates the respective outcome variable for municipality m in year t, λ_t and μ_m are sets of year- and municipality-fixed effects, and t_{0m} specifies the PHV introduction year with τ_s as corresponding coefficients. Regressions are weighted by the 80+ population and standard errors are clustered at the municipality level. In case the pre-treatment coefficients $\tau_s \ \forall s < 0$ are close to zero, it appears plausible that treatment and control group municipalities would follow a common trend in the absence of treatment. The graphs are presented in Figures 3 and 4. In each graph, we label coefficients as 'balanced' if all treated units contribute to them.

Almost all pre-treatment coefficients are insignificant; only Figure 4a has one significant pre-treatment coefficient. Since we are estimating 40 pre-treatment parameters, this finding is consistent with the common time trend assumption.

A.3. PRE-TRENDS. In order to further gauge the plausibility of the common time trend assumption, we formally tested whether the treated municipalities had deviating linear pre-trends by estimating the specification

$$Y_{mt} = \lambda_t + \mu_m + \delta_1 t + \delta_2 \mathrm{PHV}_m \times t + X'_{mt}\gamma + \varepsilon_{mt} \tag{6}$$

for the common pre-treatment period. According to the results presented in Table 4, only *Mental Health* seems to exhibit significantly deviating trends. We conclude that in general, the common time trend assumption appears to be plausible.

B. MAIN RESULTS

Next, we turn to results from a regression analysis according to specification (1). Results for all outcome variables are provided in Table 5. Results for outcomes representing resource allocation within the LTC sector are presented under the headings 'Expenditure' and 'Utilization'. We find clear evidence suggesting that the PHVs had the intended effect on these variables: nursing home utilization is reduced by 1.4 percentage points whereas the utilization of home-based care hardly increases. In relative terms, the reduction in nursing home use corresponds to a seven-percent decline compared to baseline. There is some evidence of an increase in nursing home expenditure (cf. Figure 3a) but this effect does not attain statistical significance at conventional levels. The effect on home-based care expenditure is small and insignificant. This absence of effects on home-based care costs may either reflect a more efficient selection of older people into care services, or imply that the average user would receive less intense care after the introduction of a PHV program.

Results for the outcome variables representing older people's health are shown in columns (5)-(8). We find significant reductions in hospital admissions and hospital days corresponding to seven- and eleven-percent declines, respectively. Mortality rates significantly decline by almost 5 percent. The effect on mental health admissions is also negative at -0.2%. This effect is insignificant but precise, so that we can rule out a deterioration by more than 0.1%.

Our data sample includes the entire target population, the number of municipalities is relatively small, and some of the outcome variables have limited distributions. For this reason, the basis for standard statistical inference based on random sampling may be challenged. As an alternative, we conduct design-based inference based on the treatment assignment mechanism (Abadie et al., 2017). For the tests, we randomly assign both treatment group membership and year of treatment, estimate the DID parameters, and calculate the t statistics (cf. Fischer et al., ming; MacKinnon et al., 2016). This is repeated 10,000 times to obtain the distributions of the test statistics. The size of the placebo treatment group is restricted to be equal to that of the actual treatment group and the random treatment year distributions are identical as well. This is obtained by randomly sorting all municipalities, assigning for example the first 19 municipalities of the random order to the treatment group and all others to the control group, and then assigning a treatment year of for example 2001 to the first placebo treatment group municipality, 2002 to the second and so on until the structure of treated municipalities and treatment years matches to the original case. In the bottom panel of Table 5 we show the two-sided p values indicating the probability of exceeding t-statistics.

The results according to this alternative basis for statistical inference are very similar to those previously reported. The effects on nursing home care utilization and hospital days remain significant at the 10 and 5 per cent level, respectively. Also the effect on hospital admissions and mortality remain highly significant even at the 1%-level. Thus, randomization inference would in general lead to the same conclusions as traditional statistical inference.

Since we test 8 hypotheses, we further provide adjusted p values according to the approach introduced by Romano and Wolf (Romano and Wolf (2005a) and Romano and Wolf (2005b)). This leads to slightly increased p values as compared to traditional inference; however all previously significant estimates remain significant at some level.

Goodman-Bacon (2018) shows that in difference-in-difference designs with units receiving treatment at different times, the coefficient of interest is a weighted average of all possible two-group/two-period estimators. This might result in biased estimates if effects vary over time. Table 6 decomposes the DID estimate into a treated-untreated comparison and a comparison of treatment units with different introduction times (and a residual part) following the approach of Goodman-Bacon (2018).[10] Our results indicate that the DID estimate is mainly determined by the treated-untreated comparisons, which account for 70-80% of the estimated coefficient in each individual case. In case of the utilization outcomes, the treatment effects appear to be biased towards zero such that our estimates might actually understate the true effects. However, bias due to different introduction times is probably not a major concern in our analysis.

C. REABLEMENT PROGRAMS

Next we test whether the presence of a reablement program moderates the relationship between PHV programs and the outcomes we consider. Table 7 contains the estimates of the PHV effect on our outcome variables, taking the reablement programs and an interaction effect of both programs into account.

The main coefficients remain mostly unaffected by the inclusion of the reablement program introduction. Only the effects on long-term care utilization increase (in absolute terms). Moreover, there is evidence that a combination of PHV and reablement programs lead to a decrease in home-based care utilization of about 3 percentage points.

D. SYNTHETIC CONTROL METHODS

Our balancing tests in Table 3 and our tests of pre-trends presented in Table 4 consistently lend support to the main identification strategy. The event studies presented in Figures 3 and 4 paint a similar picture. Nevertheless, our control group consists of a large and diverse set of municipalities, and therefore we also investigate whether a better comparison group may

[10] As the approach does not allow for units that abolish the treatment, we exclude municipalities which have abolished the PHV program.

be derived using the synthetic control group method introduced by Abadie and Gardeazabal (2003) and formalized by Abadie et al. (2010). The main idea of the method is to construct for each of the N^T treated units a control unit that is a weighted average of the complete pool of N^C controls under the conditions that weights w_j for $j = N^T + 1, \dots, N^T + N^C$ are non-negative and sum to one.

The synthetic control group method is largely data driven, but one modeling choice is involved in the selection of pre-treatment variables for the calculation of weights; this is a potentially important choice since it is not possible to include the entire set of pre-treament observations in the presence of covariates (Kaul et al., 2015). We consider three different strategies in what follows: one which is based on each pre-treatment realization of the outcome; one which is based on the first and the last available pre-treatment year and on all covariates; and one which is only based on covariates.

After the weights are determined, synthetic control outcomes Y_{mt}^{SC} are constructed for each treated unit by multiplying each donor municipality's outcome with the municipality-specific weight:

$$Y_{mt}^{SC} = \sum_{j \in N^C} w_j^m Y_{jt}. \tag{7}$$

The post-treatment differences between treatment units' and synthetic controls' outcomes $\Delta_{mt} = Y_{mt} - Y_{mt}^{SC}$ are averaged, where the differences are weighted by the specific treatment unit's 80+ population size and the number of post-treatment observations. Then we obtain the causal estimate τ^{SC} as

$$\tau^{SC} = \sum_{m \in N^T} \sum_{s=t_{0m}}^{T} \Delta_{ms} \frac{\mathrm{p}_m^{80}}{\sum_{m \in N^T} \mathrm{p}_m^{80} \times (T - t_{0m})}, \tag{8}$$

where p_m^{80} denotes the average population aged 80+ in municipality m over the 1994-2000 period. Table 8 contains the results as well as the corresponding root mean squared prediction errors (RMSPEs) defined as

$$RMSPE = \sqrt{\sum_{m \in N^T} \sum_{s=t_{min}}^{t_{0m}-1} \Delta_{ms}^2 \frac{\mathrm{p}_m^{80}}{\sum_{m \in N^T} \mathrm{p}_m^{80} \times (t_{0m} - 1 - t_{min})}}. \tag{9}$$

Inference is conducted by randomly assigning treatment status (just as presented above in the regression case), finding synthetic control units for each placebo-treatment unit and determining the placebo estimates weighted by the 80+ population. This is repeated 100 times for each outcome and each specification.

All three specifications provide similar results which do not differ significantly from our main results. We interpret these results as confirmation that our initial control group is appropriate and that our estimates in Table 5 therefore capture the effects of introducing a PHV program.

V. Conclusion

In this study, we evaluated whether the introduction of a preventive home visits program in the Norwegian LTC sector was effective in two senses: first, if it had the intended effect on utilization of LTC services; and second, if there is evidence suggesting it also improved older people's health.

Concerning the first point, resource use in the LTC sector, our results unambiguously show that the introduction of PHVs was associated with a shift away from institutional care, with a small increase in the utilization of home-based services. The reduction of nursing home use corresponds to around 7 percent of the baseline. The reduced reliance on institutional care is not visible in public spending on LTC.

The program also appears to have improved older people's general health: hospital admissions are reduced by 7 percent in the 80+ population and there is a corresponding reduction in average hospital days of 11 percent. Mortality among the oldest old is significantly reduced by PHV; the magnitude of the effect is equivalent to a reduction by almost 5 percent. This might not seem like a large effect, but it is of course remarkable if a relatively low-cost preventive program can impact old-age mortality at such a rate.

By comparing costs and benefits, we might obtain an impression of whether PHV programs are welfare improving. Typically, PHV lasts 60 - 80 minutes. Langeland et al. (2016) find that cost per hour of qualified health personnel in the home services is on average about NOK 500, corresponding to € 51 with an exchange rate of 9.80 NOK/Euro. Including travel time, the total costs amount to € 102 for 120 minutes. Neither effects on nursing home expenditures nor expenditures on home based services are identified in our study. Langeland et al. (2016) report that the cost of an average hospital day is approximately € 500. A reduction of 0.05 hospital days would then be priced at € 25. A rough estimate of the net costs of PHV is thus € 77. This seems to be a very reasonable price to pay for an estimated 4 % mortality decline. We conclude that PHV in the setting we have studied, is likely to be socially efficient.

However, as the actual implementations of the programs are quite heterogeneous, it remains unclear which program types are the most beneficial. A further unanswered but interesting aspect is what the impacts of the substitution between nursing home and home based case on older people's mental health and life satisfaction are. The only outcome we were able to consider was admissions related to mental health problems, and for that variable, treatment effects in excess of 0.5 per cent in either direction can be ruled out. This is clearly still quite inconclusive regarding less extreme outcomes in this domain; however, our estimate does rule out a large deterioiration in mental health triggered by PHVs.

We can think of several mechanisms that are involved in mediating the effects of PHV. An old person who experience PHV will receive more knowledge about available prevention technology. Prevention effort will then be more effective than previously. It may also be that PHV convinces the old person to do more preventive effort to maintain health and independence. A third mechanism is that the nurse who does the PHV detects that the old person has certain health problems that need attention from the home based care service or health service to delay or prevent nursing home and hospital admission in the future. A further empirical exploration of the mechanisms involved in creating the effects of PHV is left for future research.

Finally, an important limitation of our study is that we have no information on informal care. It is possible, though unlikely, that the PHV affected older people's health through changes in the provision of informal care.

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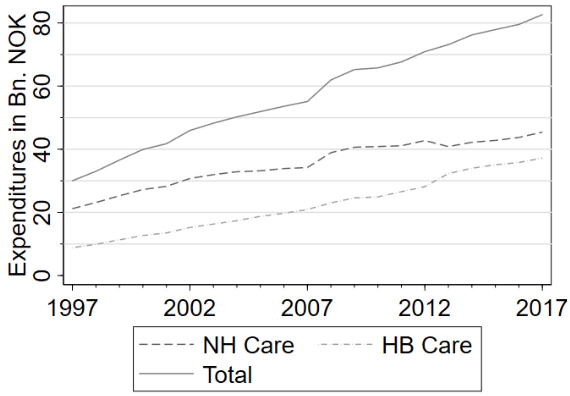
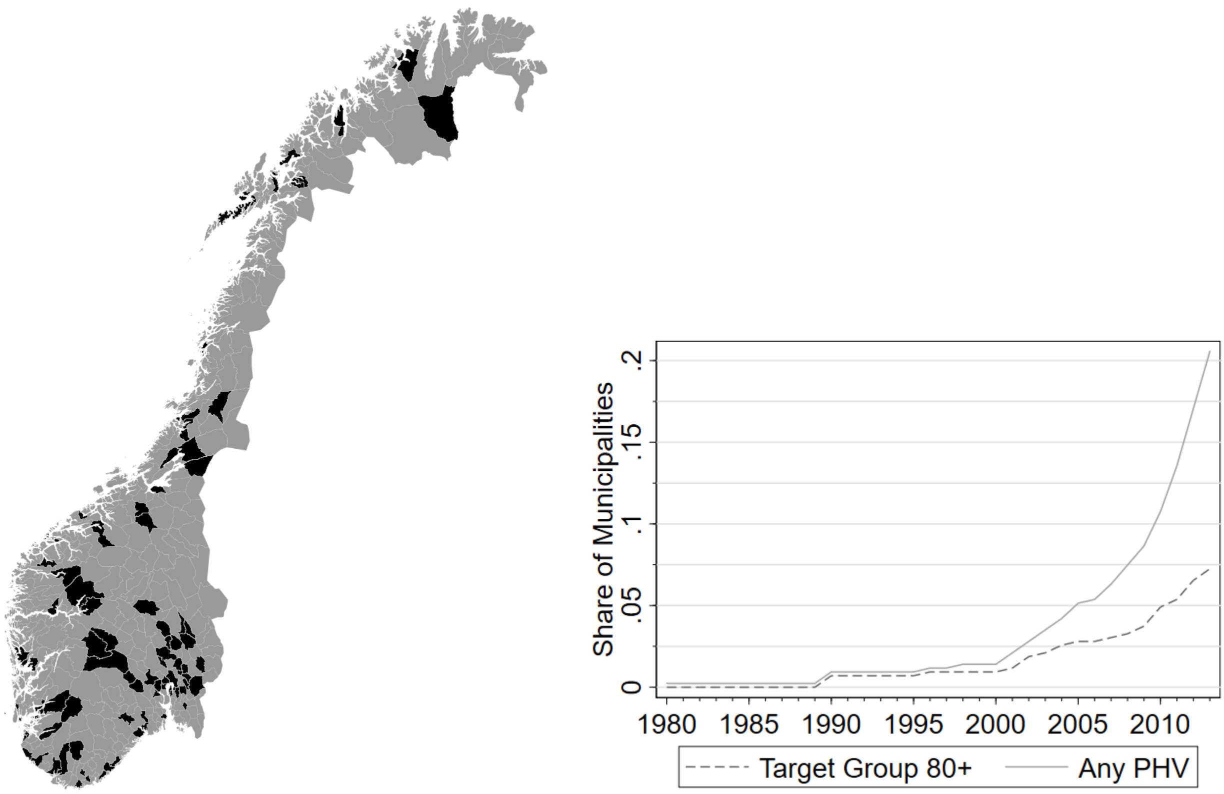
Appendix



(a) LTC Expenditures 1997–2017.

Figure 1: Long-Term Care in Norway

Source: Statistics Norway (2019)

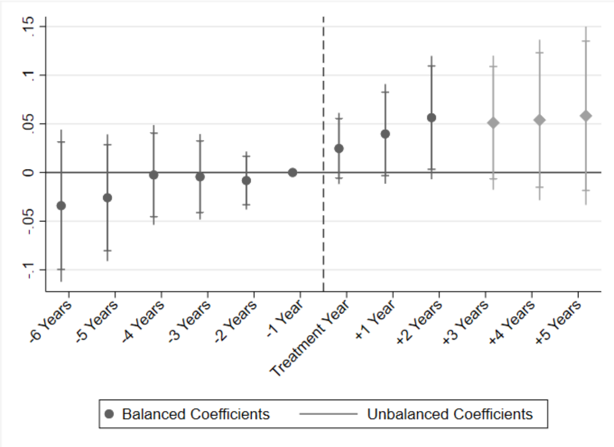


(a) Municipalities with PHV Program (2013)

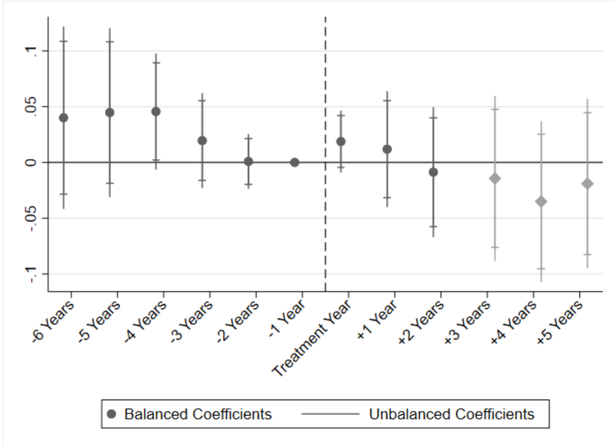
(b) Proportion of Municipalities with PHV

Figure 2: Preventive Home Visits in Norway

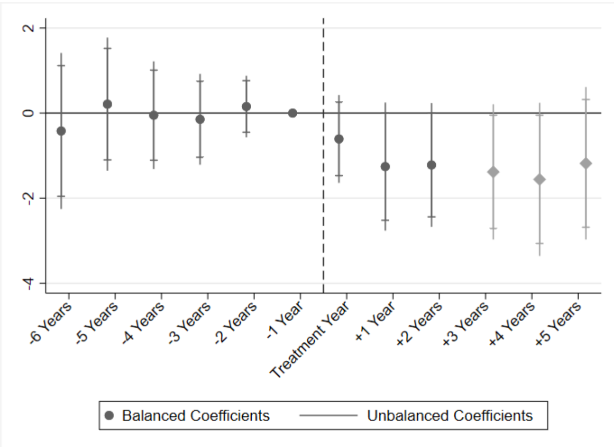
Note: Figure 2 (a) shows a map of all Norwegian municipalities in 2013. Municipalities that have introduced PHV in or before 2013 are highlighted in black. *Source:* Førland and Skumsnes (2014), own calculations.



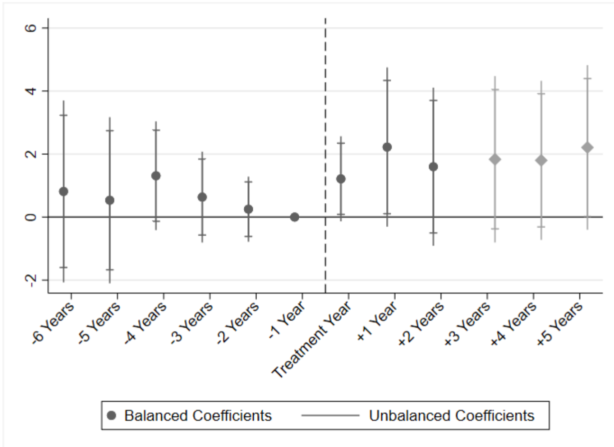
(a) Expenditure NH Care



(b) Expenditure HB Care



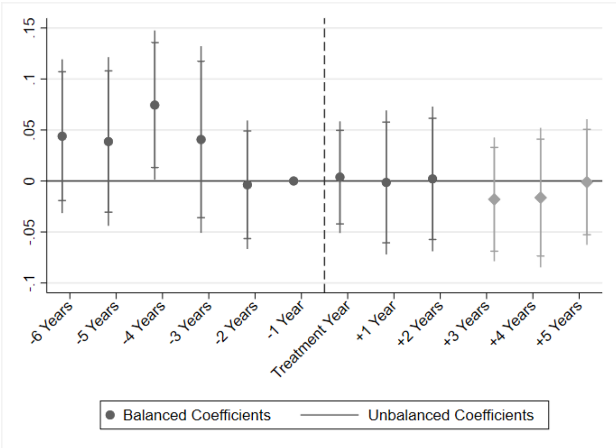
(c) Utilization NH Care



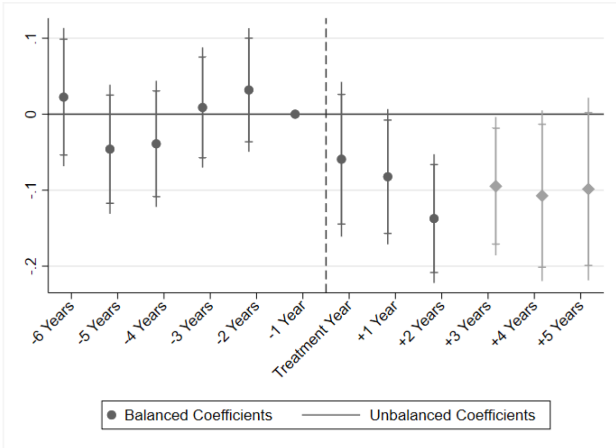
(d) Utilization HB Care

Figure 3: Event Study Graphs – LTC Expenditure/Utilization

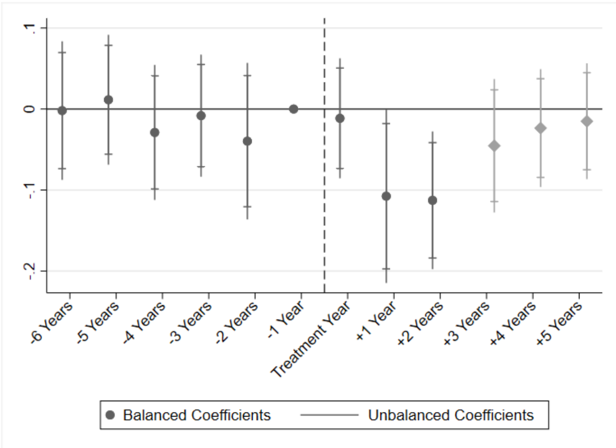
Note: Figures show the parameters τ_s from model (5). *Unbalanced Coefficients* indicate coefficients estimated on an incomplete set of treatment group municipalities due to insufficient post-treatment observations. Vertical lines correspond to 95% confidence intervals, horizontal lines denote 90% confidence intervals.



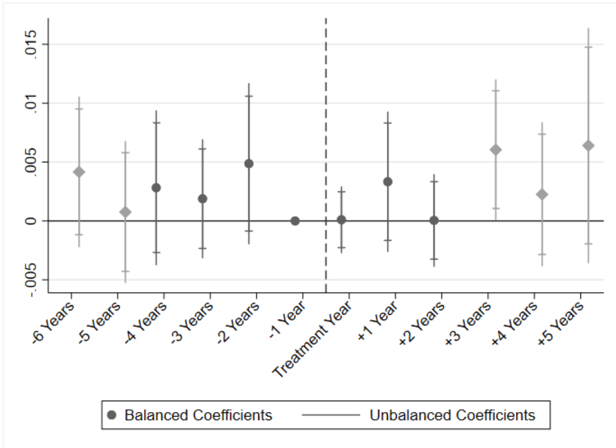
(a) Admissions



(b) Days



(c) Mortality



(d) Mental Health

Figure 4: Event Study Graphs – Hospital Care/Mortality

Note: Figures show the parameters τ_s from model (5). *Unbalanced Coefficients* indicate coefficients estimated on an incomplete set of treatment group municipalities due to insufficient post-treatment observations. Vertical lines correspond to 95% confidence intervals, horizontal lines denote 90% confidence intervals.

Table 1: Summary Statistics

	Obs.	Mean	Std. Dev.	Min.	Max.
Outcome Variables					
Expenditure NH Care (in 1,000 NOK)	2,115	8.0397	3.6254	1.9364	37.3091
Expenditure HB Care (in 1,000 NOK)	2,115	9.0728	3.5981	0.5796	32.7262
Utilization NH Care	3,240	15.0235	5.1169	0	88.2353
Utilization HB Care	3,096	36.7003	7.2362	4.6921	80.1653
Hospital Admissions	4,056	0.5846	0.1546	0.1357	2.7511
Hospital Days	4,056	3.5295	1.0584	0.4118	11.0625
Mortality	3,864	0.1102	0.0236	0.0181	0.3095
Mental Health	1,638	0.0113	0.0144	0	0.1270
Covariates					
Economic Indicators					
Average Income (in 1,000 NOK)	4,080	275.6733	88.4373	114.7000	783.2000
Unemployment (Females)	4,080	0.0171	0.0079	0	0.0925
Property Transfers	4,080	0.0116	0.0054	0	0.0450
Demographic Indicators					
Share Females	4,080	0.4980	0.0086	0.4354	0.5357
Share Secondary or Higher Education	4,080	0.6460	0.0695	0.3040	0.8020
Live Births	4,080	0.0111	0.0024	0.0015	0.0233
Net Migration	4,080	0.0024	0.0097	−0.0839	0.0568
Share European Immigrants	4,080	0.0319	0.0225	0	0.2324
Share Non-European Immigrants	4,080	0.0191	0.0146	0	0.0909
Naturalizations	4,080	0.0015	0.0013	0	0.0148
Population Increase	4,080	0.0032	0.0117	−0.2009	0.0941
Population Density	4,080	51.9914	89.2001	0.3002	513.3585
Marriages	4,080	0.0041	0.0012	0	0.0137
Divorces	4,080	0.0019	0.0008	0	0.0116
Share Living in Densely Populated Areas	4,080	0.5760	0.2519	0	1.0406
80+ Population	4,080	573.0062	542.9113	11	2548
Other					
Vote Share Left-Wing Parties	4,080	0.4402	0.1110	0.1032	0.8042
Plantings	4,080	0.0079	0.0102	0	0.0555

All values are weighted by pre-treatment average 80+ population. Expenditure variables are available for the period 2003–2017 and measured as per capita of total population. Utilization is measured in percent of the 80+ population, all other outcomes variables are measured as per capita of 80+ population. Mental health information is available for 2000-2017. All covariates are expressed in per capita terms. *Source*: Statistics Norway (2019), Norwegian Centre for Research Data (2019), and Fiva et al. (2015).

Table 2: Baseline Averages by Groups

	Treatment	Control	Not Included
Outcome Variables			
Expenditure NH Care (in 1,000 NOK)	6.1996	6.7526	6.5194
Expenditure HB Care (in 1,000 NOK)	4.9626	5.9610	5.0767
Utilization NH Care	19.2930	15.6730	16.1803
Utilization HB Care	34.8481	39.8174	38.2019
Hospital Admissions	0.5232	0.5455	0.5824
Hospital Days	3.3888	3.8177	4.4208
Mortality	0.1154	0.1252	0.1203
Mental Health	0.0043	0.0045	0.0168
Covariates			
Economic Indicators			
Average Income (in 1,000 NOK)	221.2783	206.7219	244.7706
Unemployment (Females)	0.0140	0.0161	0.0165
Property Transfers	0.0120	0.0093	0.0127
Demographic Indicators			
Share Females	0.4998	0.4996	0.5069
Share Secondary or Higher Education	0.6524	0.6098	0.6752
Live Births	0.0121	0.0121	0.0134
Net Migration	0.0050	0.0021	0.0015
Share European Immigrants	0.0218	0.0194	0.0304
Share Non-European Immigrants	0.0108	0.0104	0.0338
Naturalizations	0.0011	0.0011	0.0027
Population Increase	0.0071	0.0028	0.0048
Population Density	48.2177	49.2567	416.8127
Marriages	0.0048	0.0048	0.0058
Divorces	0.0020	0.0018	0.0023
Share Living in Densely Populated Areas	0.5933	0.5536	0.7870
80+ Population	461.3694	551.2377	6734.7133
Other			
Vote Share Left-Wing Parties	0.4078	0.4368	0.4343
Plantings	0.0090	0.0104	0.0085

All values are group-specific averages for the baseline period 2000 (2003 in case of expenditure variables) weighted by the pre-treatment average 80+ population. *Treatment* and *Control* are the treatment and control group municipalities as defined in subsection A., and *Not Included* contains all municipalities on which information is available but that are excluded from the analysis due to different constraints (largest five municipalities, municipalities with different PHV target group or PHV introduction planned, or not sufficient pre- or post-treatment periods available). Expenditure variables are available for the period 2003–2017 and measured as per capita of total population. Utilization is measured in percent of the 80+ population, all other outcomes variables are measured as per capita of 80+ population. Mental health information is available for 2000-2017. *Source*: Statistics Norway (2019), Norwegian Centre for Research Data (2019), and Fiva et al. (2015).

Table 3: Balancing Tests

	Std.Diff.	DID-FE	DID-FE MT p-Value	FE + County Trends	FE + County Trends MT p-Value
	(1)	(2)	(3)	(4)	(5)
Unemployment (Females)	−0.3376	0.0022**	0.3187	0.0012	0.7672
		(0.0011)		(0.0008)	
Share Females	−0.0327	−0.0001	0.9550	−0.0010	0.8362
		(0.0008)		(0.0007)	
Share Secondary or Higher Education	0.7068	−0.0069**	0.2967	−0.0029	0.9441
		(0.0034)		(0.0026)	
Vote Share Left-Wing Parties	−0.4108	0.0084	0.9191	0.0089	0.9740
		(0.0098)		(0.0101)	
Live Births	0.1113	0.0002	0.9191	−0.0002	0.9660
		(0.0002)		(0.0002)	
Property Transfers	0.6234	0.0006	0.8392	−0.0001	0.9980
		(0.0006)		(0.0005)	
Share European Immigrants	0.1925	0.0048	0.8142	0.0025	0.9740
		(0.0040)		(0.0031)	
Share Non-European Immigrants	0.0773	0.0015	0.9191	0.0001	0.9980
		(0.0019)		(0.0014)	
Average Income	0.3903	−0.0113	0.6254	−0.0062	0.9111
		(0.0078)		(0.0051)	
Population Increase	0.2767	−0.0008	0.9191	−0.0016	0.8561
		(0.0011)		(0.0012)	
Divorces	0.1140	−0.0001	0.8392	−0.0001	0.9590
		(0.0001)		(0.0001)	
Marriages	0.0951	0.0000	0.9550	0.0000	0.9980
		(0.0001)		(0.0001)	
Naturalizations	−0.0652	0.0001	0.9191	0.0001	0.9980
		(0.0001)		(0.0001)	
Plantings	−0.0723	−0.0004	0.9550	0.0003	0.9980
		(0.0010)		(0.0005)	
Share Living in Densely Populated Areas	0.1846	0.0148	0.5854	0.0094	0.9510
		(0.0097)		(0.0091)	
Net Migration	0.3000	−0.0018*	0.4515	−0.0023**	0.3257
		(0.0010)		(0.0011)	
Population Density	−0.0196	4.5372	0.8392	0.9533	0.9980
		(4.2643)		(2.9330)	
80+ Population	−0.2174	19.3089	0.9191	8.7425	0.9980
		(27.2498)		(21.8695)	
Reablement Program	0.0000	0.0626	0.8392	0.0307	0.9940
		(0.0583)		(0.0533)	

Std.Diff. indicates the standardized difference defined by 2. Estimates indicate the coefficients of the post-treatment variable in a regression of the specific covariate on the post-treatment indicator, year- and municipality-fixed effects (and linear county-level trends in the *FE + Reg. Trends* specification). All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *MT p Value* denotes Romano-Wolf (2005) p values using 1,000 repetitions.

Table 4: Test of Pre-Trends

	Expenditure		Utilization		Hospital		Mortality	Mental Health
	NH Care	HB Care	NH Care	HB Care	Admissions	Days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year	0.0177**	0.0988***	−0.3476	0.2825	0.0275*	0.0187***	−0.0002	0.0007
	(0.0081)	(0.0118)	(0.2813)	(0.2125)	(0.0167)	(0.0062)	(0.0132)	(0.0008)
Treated × Year	0.0029	−0.0086	0.0972	0.0062	−0.0054	−0.0144	−0.0093	−0.0009**
	(0.0080)	(0.0099)	(0.1921)	(0.3375)	(0.0128)	(0.0091)	(0.0058)	(0.0004)
Year-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Level Trends	✓	✓	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
$N^{Treatment}$	18	18	21	20	24	24	24	10
$N^{Control}$	122	122	114	109	145	145	137	78
Obs.	840	840	945	903	1183	1183	1127	704

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. $N^{Treatment}$ and $N^{Control}$ are the number of treatment and control group municipalities, respectively. *Obs.* is the total number of observations used for estimation. Only the common pre-treatment periods 2003-2008 for expenditure variables (one municipality excluded to obtain sufficient pre-treatment years), 2000-2007 for mental health (three municipalities excluded) and 1994-2000 for all other outcomes are included.

Table 5: Results

	Expenditure		Utilization		Hospital		Mortality	Mental Health
	NH Care	HB Care	NH Care	HB Care	Admissions	Days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Treat.	0.0302	−0.0288	−1.4079**	0.2145	−0.0715***	−0.1127***	−0.0476***	−0.0022
	(0.0324)	(0.0281)	(0.6411)	(0.9137)	(0.0217)	(0.0396)	(0.0127)	(0.0016)
Year-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Level Trends	✓	✓	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Baseline	6.1996	4.9626	19.2930	34.8481	0.5232	3.3888	0.1154	0.0043
$N^{Treatment}$	19	19	21	20	24	24	24	13
$N^{Control}$	122	122	114	109	145	145	137	78
Obs.	2115	2115	3240	3096	4056	4056	3864	1638
RI p-Value	0.4556	0.3875	0.0866	0.8466	0.0098	0.0285	0.0035	0.3395
Multiple Testing p-Value	0.6444	0.5744	0.0969	0.7812	0.0140	0.0260	0.0100	0.4176

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. Covariates are selected in order to maximize the corresponding treatment coefficient's precision. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Baseline* denotes the year 2000 average (2003 for expenditure variables) of the outcome in the treatment group, and $N^{Treatment}$ and $N^{Control}$ are the number of treatment and control group municipalities, respectively. *Obs.* is the total number of observations used for estimation. *RI p Value* corresponds to randomization inference p values for 10,000 repetitions and *Multiple Testing p Value* denotes Romano-Wolf (2005) p values using 1,000 repetitions.

Table 6: Goodman-Bacon Decomposition

	Expenditure		Utilization		Hospital		Mortality	Mental Health
	NH Care	HB Care	NH Care	HB Care	Admissions	Days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fixed Effects								
Post-Treat.	0.0258	−0.0255	−1.3885*	0.3875	−0.0723***	−0.1182***	−0.0471***	−0.0010
	(0.0376)	(0.0315)	(0.7150)	(1.0126)	(0.0234)	(0.0432)	(0.0123)	(0.0018)
Goodman-Bacon Decomposition								
Treat. vs. Untreat.	−0.0598	−0.0122	−2.8777	1.3089	−0.0809	−0.1173	−0.0601	0.0021
Share	0.7424	0.7377	0.7890	0.7741	0.7814	0.7819	0.7797	0.7261
Timing	−0.0189	0.0017	−2.4914	1.7219	−0.0887	−0.1910	−0.0311	−0.0008
Share	0.0544	0.0557	0.0651	0.0702	0.0678	0.0677	0.0681	0.0731
Year-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Level Trends	✓	✓	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Baseline	6.1079	4.6763	19.1919	35.2615	0.5204	3.3175	0.1142	0.0013
$N^{Treatment}$	16	16	18	18	21	21	21	10
$N^{Control}$	122	122	114	109	145	145	137	78
Obs.	2070	2070	3168	3048	3984	3984	3792	1584

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. Covariates are selected in order to maximize the corresponding treatment coefficient's precision. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Baseline* denotes the year 2000 average (2003 for expenditure variables) of the outcome in the treatment group, and $N^{Treatment}$ and $N^{Control}$ are the number of treatment and control group municipalities, respectively. *Obs.* is the total number of observations used for estimation. *Treat. vs. Untreat.* corresponds to treatment-control comparisons and *Timing* denotes comparisons of treatment units introducing the treatment at different times. *Share* is the weight of the specific factor in the treatment effect; the difference to 1 is the residual part. Three municipalities which abolished the program before 2017 are excluded.

Table 7: Reablement Programs

	Expenditure		Utilization		Hospital		Mortality	Mental Health
	NH Care	HB Care	NH Care	HB Care	Admissions	Days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PHV	0.0112	−0.0221	−1.6378**	0.9033	−0.0765***	−0.0993**	−0.0613***	−0.0009
	(0.0333)	(0.0282)	(0.6760)	(0.9392)	(0.0242)	(0.0395)	(0.0153)	(0.0016)
Reablement Program	−0.0768**	0.0243	−0.4876	0.7786	0.0098	0.0458	−0.0094	0.0003
	(0.0344)	(0.0198)	(0.5003)	(0.7101)	(0.0217)	(0.0294)	(0.0191)	(0.0024)
PHV × Reabl.	0.0877**	−0.0287	1.1590	−3.0545**	0.0081	−0.0536	0.0028	−0.0046
	(0.0401)	(0.0367)	(0.8498)	(1.2554)	(0.0478)	(0.0610)	(0.0237)	(0.0029)
Year-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
County-Level Trends	✓	✓	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Baseline	6.1996	4.9626	19.2930	34.8481	0.5232	3.3888	0.1154	0.0043
$N^{Treatment}$	19	19	21	20	24	24	24	13
$N^{Control}$	122	122	114	109	145	145	137	78
Obs.	2115	2115	3240	3096	4056	4056	3864	1638

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. Covariates are selected in order to maximize the corresponding treatment coefficient's precision. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Baseline* denotes the year 2000 treatment group outcome average (2003 for expenditure variables), and $N^{Treatment}$ and $N^{Control}$ are the number of treatment and control group municipalities, respectively. *Obs.* is the total number of observations used for estimation.

Table 8: Synthetic Control Method

	Expenditure		Utilization		Hospital		Mortality	Mental Health
	NH Care	HB Care	NH Care	HB Care	Admissions	Days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Matching on Pre-Treatment Outcomes								
Post-Treat.	−0.0562	0.0913	−3.3114***	0.7394	−0.0307	−0.1122*	−0.0618**	0.0049
RMSPE	0.0026	0.0641	1.2724	3.2610	0.0862	0.1001	0.0695	0.0046
Matching on Covariates and Selected Pre-Treatment Outcomes								
Post-Treat.	−0.0246	0.0323	−2.1296**	1.8828	−0.0712	−0.1039*	−0.0471**	0.0012
RMSPE	0.0918	0.1105	3.7691	7.2810	0.2364	0.2328	0.1846	0.0090
Matching on Covariates								
Post-Treat.	−0.0057	−0.1562	−1.8689	1.1635	−0.0831	−0.0660	−0.0482*	0.0037
RMSPE	0.2615	0.4227	6.1596	8.4123	0.2610	0.3040	0.1949	0.0130
Baseline	6.1967	4.9521	19.2930	34.8481	0.5232	3.3888	0.1154	0.0043
$N^{Treatment}$	18	18	21	20	24	24	24	10
$N^{Control}$	122	122	114	109	145	145	137	78
Obs.	2100	2100	3240	3096	4056	4056	3864	1584

Matching on Covariates indicates creating synthetic controls based on pre-treatment covariate averages and *Matching on Pre-Treatment Outcomes* denotes creating the synthetic controls using pre-intervention outcomes. *Matching on Covariates and Selected Pre-Treatment Outcomes* matches on all covariates as well as selected pre-treatment outcomes. Inference is conducted via randomization considering 100 repetitions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Baseline* denotes the year 2000 treatment group outcome average (2003 for expenditure variables), and $N^{Treatment}$ and $N^{Control}$ are the number of treatment and synthetic control group municipalities, respectively. *Obs.* is the total number of observations used for estimation. In case of expenditures and mental health, municipalities with introductions before 2008 are excluded to obtain sufficient pre-treatment years.