CINCH
competence in competition + health
series

#2019/07

Norman Bannenberg, Oddvar Førland, Tor Iversen, Martin Karlsson and Henning Øien

# Preventive Home Visits

# Imprint



**CINCH SERIES**

CINCH – Health Economics Research Center
Weststadttürme, Berliner Platz 6-8
45127 Essen

www.cinch.uni-due.de
cinchseries@cinch-essen.de
Phone +49 (0) 201 183 - 3679
Fax +49 (0) 201 183 - 3716



ISSN 2199-8744 (online)

CINCH competence in competition + health series

#2019/07

Norman Bannenberg, Oddvar Førland, Tor Iversen, Martin Karlsson and Henning Øien

# Preventive Home Visits

UNIVERSITÄT DUISBURG ESSEN Open-Minded

Bundesministerium für Bildung und Forschung

rwi

Norman Bannenberg*, Oddvar Førland†, Tor Iversen‡,
Martin Karlsson§ and Henning Øien**

# Preventive Home Visits

## 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 8 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*

* *E-Mail: norman.bannenberg.@uni-due.de*
† *E-Mail: oddvar.forland@vid.no*
‡ *E-Mail: tor.iversen@medisin.uio.no*
§ *E-Mail: martin.karlsson@uni-due.de*
** *E-Mail: henning.oien@thi.no*
***Funding***: *Norman Bannenberg was financially supported by the European Investment Bank Institute: EIBURS Project "Demographic Change in the EU, the Oldest-Old and the Need for Innovative Models of More Efficient Elderly Care", and the Leibniz Association: Leibniz Science Campus Ruhr Project "Interdependence of Formal and Informal LTC Provision in Declining Regions". Henning Øien was financially supported by the Research Council of Norway: Project 256644 "A cross- sectoral approach to high quality health care transitions for older people".*
***Declarations of Interest:*** *none.*

# 1 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 variety difference-in-difference strategies, 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 18 per cent in nursing home care and 37 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 8 per cent, and mortality rates decline by 4 per cent in the years following the introduction of PHVs. All our results are robust to a battery of robustness checks. In addition, we show that the effects on health outcomes are stronger in municipalities with lower average incomes.

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

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

studied in RCTs. This alleviates concerns regarding external validity but also about selective compliance outside the controlled environment of an RCT. Second, we are able to follow subjects over a longer time period than most RCTs allow. 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 3 we present a simple theoretical model of the decision to introduce PHVs in municipalities. In Section 4 we present the various administrative data sources that we draw upon in our empirical analysis, and the estimation techniques we employ. Section 5 presents our empirical results – including analyses of effect heterogeneity and a number of robustness checks. Section 6 discusses the main results emerging from the analysis and Section 7 concludes.

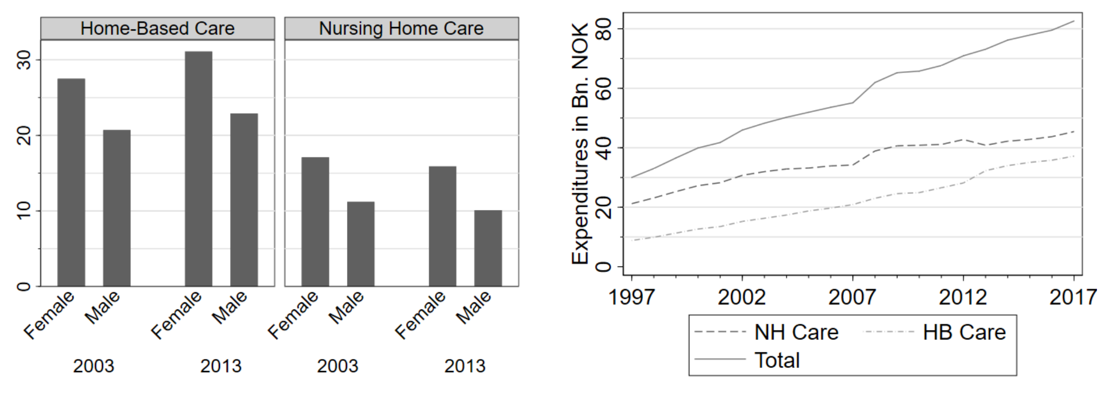
# 2 Institutional Background

## 2.1 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).




**(a)** Utilization by Gender and Setting (2003/2013)

**(b)** LTC Expenditures 1997–2017.

**Figure 1:** Long-Term Care in Norway

*Note*: Figure 1 (a) shows utilization rates for the 80+ population. *Source*: Statistics Norway (2019)

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.

## 2.2 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). The absence of specified preventive measures and targets groups in the Norwegian legislation may be seen as a consequence of a strong tradition in Norway of local self-determination and delegation of authority to the municipal level (Vike, 2017).

Within the tradition of local 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'. '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.

In Norway, 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 mu-

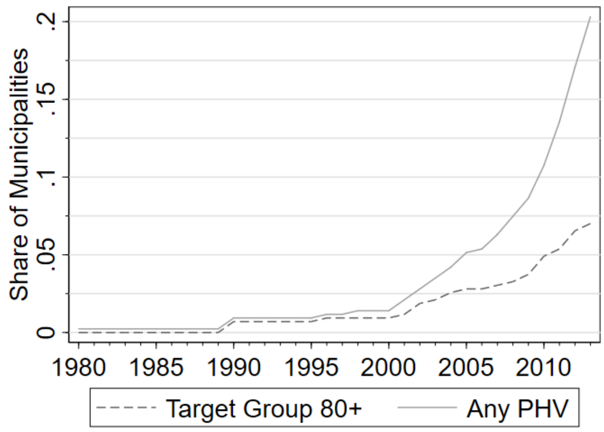
[2] Reablement programs provide early and intensive home-based rehabilitation after functional decline, cf. Section 5.4 below.

nicipalities. 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 governed policy by 2013. 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. Interestingly, 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.

**(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.

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).

# 3 Theory

In this section we sketch a simple theoretical model of the conditions for a PHV program being welfare improving. Our point of departure is a model by Dave and Kaestner (2009) which in turn is an adaptation of a model by Ehrlich and Becker (1972). We consider a consumer who can invest in prevention ($r$) in order to reduce the probability of becoming disabled ($\pi$). Disability enters the elementary utility function via the capability function $f(m)$ so that utility in a certain disability state $j$ equals

$$V\left(I(1-\tau)+f_j(H)-(1-q)L_j-(1-k)r\right) \tag{1}$$

where $I$ is income (which is assumed to be independent of disability), $\tau$ is a proportional tax rate, $f_j(H)$ is a concave function that represents how health translates into capabilities that contribute to utility, $q$ the government subsidy for long-term care, $L_j$ is the amount of long-term care consumed in state $j$, $k$ is the government subsidy for prevention and $r$ is the amount of preventive effort exerted. We allow for two disabled states, moderate and severe, and specify the following expected utility function:

$$\begin{aligned} EU = &(1-\pi(r))\,V_m\left(I(1-\tau)+f_m(H_m)-(1-q)L_m-(1-k)r\right) \\ &+\pi(r)\,V_s\left(I(1-\tau)+f_s(H_s)-(1-q)L_s-(1-k)r\right) \end{aligned}$$

where we realistically assume that $\frac{d\pi(r)}{dr} < 0$ so that preventive effort reduces the probability of becoming severely disabled. We further assume that the capabilities in the disabled states are given by $H_j = L_j - \theta_j \forall j \in \{m, s\}$, where $\theta_j$ is the underlying degree of disability (hence, $\theta_s > \theta_m$). We assume that the municipality runs a balanced budget given by

$$\tau I = rk + (1 - \pi(r)) qL_m + \pi(r) qL_s = rk + q\bar{L} \tag{2}$$

where $\bar{L}$ denote the per capita LTC costs.

We now solve the model first from the point of view of the individual and then from the point of view of the municipality. We present second order conditions and some additional results in Appendix A.1.

### 3.1 The Individual's Problem

The individual takes decisions in two stages. When their degree of disability has been realised, they decide on the amount of LTC to consume. This decision is thus dependent on the actual state of disability and given by the first order condition

$$\frac{\partial V_j}{\partial L_j} = \left[f'\left(H_j\right) - (1-q)\right] V_j'(\cdot) = 0. \tag{3}$$

Since $V_j'(\cdot) > 0$ for all values the argument, the FOC can only be satisfied if $f'\left(H_j\right) - (1-q) = 0$, which leads to the optimality criterion $L_j^* = (f')^{-1}(1-q) + \theta_j$. Thus, the demand for long-term care depends on the shape of the capability function $f$, it increases in the subsidy $q$ and the degree of disability. As expected, the subsidy $q$ leads to ex-post moral hazard so that an amount of care is consumed which is greater than the optimal level.

Next, we consider the prevention decision $r$. This leads to the first order condition

$$\frac{\partial EU}{\partial r} = -\pi'(V_m - V_s) - (1-k)\left[(1-\pi)V_m' + \pi V_s'\right] = 0. \tag{4}$$

The first term represents the utility gain from escaping disability, whereas the second term represents the costs of preventive effort. Clearly, the existence of a PHV program ($k > 0$) increases the amount of preventive effort. Whether this increased prevention represents a distortion or not will, however, depend on the amount of care consumed $L_j$: if excessive amounts of care are consumed, the PHV subsidy may be welfare-improving.

Appplying the implicit function theorem, we are able to study how the optimal choices $r^*$ and $L^*$ respond to some of the parameters of the model: the PHV subsidy $k$, the LTC subsidy $q$ and the degree of severe disability $\theta_s$.

$$\frac{dr^*}{dq} = -\frac{\frac{\partial^2 EU}{\partial r \partial q}}{\frac{\partial^2 EU}{\partial r^2}} = -\frac{-\pi' \left[L_m V_m' + L_s V_s'\right] + (1-k)\left[(1-\pi) L_m V_m'' + \pi L_s V_s''\right]}{\frac{\partial^2 EU}{\partial r^2}} < 0$$

$$\frac{dL_j^*}{dq} = -\frac{\frac{\partial^2 EU}{\partial L_j \partial q}}{\frac{\partial^2 EU}{\partial L_j^2}} = -\frac{V_j'}{f_j'' V_j'} = -\frac{1}{f_j''} > 0$$

$$\frac{dr^*}{dk} = -\frac{\frac{\partial^2 EU}{\partial r \partial k}}{\frac{\partial^2 EU}{\partial r^2}} = -\frac{-\pi' r \left[V_m' + V_s'\right] + (1-\pi) V_m' + \pi V_s' - (1-k)\left[(1-\pi) V_m'' + \pi V_s''\right]}{\frac{\partial^2 EU}{\partial r^2}} > 0$$

$$\frac{dL_j^*}{dk} = -\frac{\frac{\partial^2 EU}{\partial L_j^* \partial k}}{\frac{\partial^2 EU}{\partial \left(L_j^*\right)^2}} = 0$$

$$\frac{dr^*}{d\theta_s} = -\frac{\frac{\partial^2 EU}{\partial r \partial \theta_s}}{\frac{\partial^2 EU}{\partial r^2}} = -\frac{-\pi' f_s' V_s' + (1-k)\,\pi f_s' V_s''}{\frac{\partial^2 EU}{\partial r^2}} \gtreqless 0$$

$$\frac{dL_s^*}{d\theta_s} = -\frac{\frac{\partial^2 EU}{\partial L_s \partial \theta_s}}{\frac{\partial^2 EU}{\partial L_s^2}} = \frac{f_s'' V_s'}{f_s'' V_s'} = 1 > 0$$

Thus, a public subsidy for LTC unabmiguously increases demand for LTC ($\frac{dL_j^*}{dq} > 0$) and reduces the demand for prevention ($\frac{dr^*}{dq} < 0$). The PHV subsidy increases demand for prevention ($\frac{dr^*}{dk} > 0$) but does not affect the demand for LTC. Finally, the degree of severity $\theta_s$ increases demand for LTC – whereas the effect on prevention is ambiguous. The first term ($-\pi' f_s' V_s' > 0$) represents a utility gain from prevention: when the severely disabled state is worse, the utility gain of escaping it increases. The second term ($(1-k)\,\pi f_s' V_s'' < 0$) represents the perceived costs of prevention, which also increase when the severely disabled state gets worse.

In Appendix A.1 we also introduce the prevention effectiveness parameter $\beta$, which captures exactly how the probability of disability $\pi$ responds to prevention effort $r$. As it turns out, the effect this parameter has on preventive effort is also ambiguous.

### 3.2 The Municipality's Problem

We assume that the municipality's objective is to maximize welfare, conditional on the behavior of individual residents and on the LTC subsidy $q$ set by the federal government.[3] Inserting the municipality's budget constraint into the individual's objective function thus gives us the municipality's objective, which we denote $W$:

$$\begin{aligned} W = &(1-\pi) V_m \left( I(1-\tau) + f_m(H_m) - (1-q) L_m - (1-k) r - rk - q\bar{L} \right) \\ &+ \pi V_s \left( I(1-\tau) + f_s(H_s) - (1-q) L_s - (1-k) r - rk - q\bar{L} \right) \end{aligned}$$

Now, taking the derivatives with respect to the subsidy $k$, we get

$$\begin{aligned} \frac{dW}{dk} &= \frac{\partial W}{\partial k} + \frac{\partial W}{\partial r}\frac{dr^*}{dk} = \frac{\partial W}{\partial r}\frac{dr^*}{dk} \\ &= -\left(k + q\frac{d\bar{L}}{dr^*}\right)\frac{dr^*}{dk}\left[(1-\pi) V_m' + \pi V_s'\right] = 0 \end{aligned}$$

Since the term in the square bracket is positive by assumption, the FOC can only be satisfied if $\left(k + q\frac{d\bar{L}}{dr^*}\right) = 0$, which leads to the optimal subsidy of prevention given by

$$k^* = -q\frac{d\bar{L}}{dr^*}$$

Thus, the optimal subsidy is, first of all, dependent on the subsidy of long-term care $q$. If long-term care is paid completely out-of-pocket ($q = 0$), then the individual has correct incentives to engage in preventive effort, and a subsidy of such effort will not be welfare improving. Furthermore, responsiveness of per-capita care spending ($\bar{L}$) to preventive effort also justifies a higher subsidy.

[3] We do not model the national government's decision on $q$ but note that it represents a rather standard moral hazard problem, where the insurance motive calls for a large subsidy whereas the moral hazard problems call for some degree of cost-sharing. It is thus likely that the optimal solution will have $q \in (0, 1)$.

Next, consider how the subsidy depends on parameters representing severity ($\theta_s$) and effectiveness of prevention ($\beta$). Again, applying the implicit function theorem, we have

$$\frac{dk^*}{d\theta_s} = -\frac{\frac{\partial^2 W}{\partial k \partial \theta_s}}{\frac{\partial^2 W}{\partial k^2}} = -\frac{-q\pi' \frac{dL_s^*}{d\theta_s}}{\frac{\partial^2 W}{\partial k^2}} = -\frac{-q\pi'}{\frac{\partial^2 W}{\partial k^2}} > 0 \tag{5}$$

$$\frac{dk^*}{d\beta} = -\frac{\frac{\partial^2 W}{\partial k \partial \beta}}{\frac{\partial^2 W}{\partial k^2}} = -\frac{-q\frac{d\pi'}{d\beta}\left(L_s^* - L_m^*\right)}{\frac{\partial^2 W}{\partial k^2}} > 0 \tag{6}$$

Hence, even though we cannot sign unambiguously how individual behavior responds to these parameters, the optimal subsidy increases in both severity of disability and in the effectiveness of prevention.

### 3.3 Concluding Remarks

This section has made a number of points regarding the economic rationale for a PHV program. First, we have argued that the introduction of a PHV program may be welfare improving if there is a subsidy for LTC in place which leads to ex post moral hazard in the consumption of LTC. Second, we have shown that though the individual's response to policy parameters are unambiguous and go in the expected direction, it is theoretically unclear how individuals adjust their preventive effort to changes in severity of disability, and to the effectiveness of prevention effort. Third, we have shown the conditions under which the municipality would decide to introduce a PHV program in the current setting. This decision would depend on a number of factors. First, the greater the subsidy for LTC is, the stronger is the economic case for a PHV. However, the rationale for PHVs also depend on how responsive per-capita LTC expenditure is to preventive effort. Accordingly, the benefits of a PHV programme would be smaller in a municipality where there are small differences in LTC consumtion between different disability states. Likewise, a PHV program would be less attractive in municipalities where there is relatively little heterogeneity in disability among older people. Finally, a PHV program is obviously less attractive when the preventive effort is less effective in reducing the probability of disability.

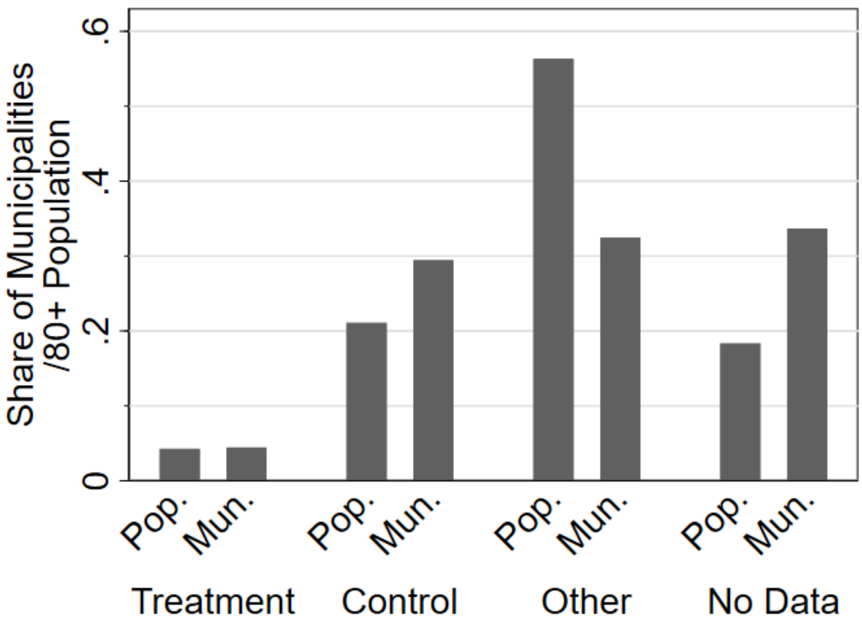



**Figure 3:** Share of Municipalities/80+ Population by Group

# 4 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.

## 4.1 Treatment Assignment

The information on preventive home visits in Norway is based on a survey of all 428 Norwegian municipalities carried out in 2013 (Førland and Skumsnes, 2014). 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.

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 30 municipalities (7.0% of all municipalities).

A municipality is included in the analysis if the outcome variable (see below) is observed in all periods from 1994 to 2014.[4] The treatment group is defined as the group of municipalities that introduced a PHV program after 1994 and before 2014 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 these cities are included.

The potential control group consists of 175 municipalities (40.9% of all municipalities) that neither introduced a PHV program, nor planned to introduce one.[5]

### 4.2 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*); utilization rates for nursing homes (*Utilization NH*) and home-based care (*Utilization HB*), respectively.[6]

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*). In the empirical analysis we take

[4] For expenditure variables, we require that the outcome is observed throughout the 2003–14 period.

[5] 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.

[6] *Expenditure NH* and *Expenditure HB* are observed in the period 2003-2014. In- and out-patient admissions attributable to mental health conditions are only observed in 2000 - 2014.

**Table 1:** Summary Statistics

| | Obs. | Mean | Std. Dev. | Min. | Max. |
|---|---|---|---|---|---|
| **Outcome Variables** | | | | | |
| Expenditure NH Care (in 1,000 NOK) | 1,704 | 7.1367 | 2.8847 | 1.9495 | 21.2235 |
| Expenditure HB Care (in 1,000 NOK) | 1,704 | 8.4814 | 3.6145 | 2.1781 | 44.2228 |
| Utilization NH Care | 2,877 | 14.7076 | 4.9082 | 0 | 55.6604 |
| Utilization HB Care | 2,877 | 37.1487 | 7.1152 | 4.6921 | 76.9531 |
| Hospital Admissions | 3,045 | 0.5980 | 0.1632 | 0.1357 | 2.7511 |
| Hospital Days | 3,045 | 3.6759 | 1.1411 | 0.7739 | 28.8773 |
| Mortality | 3,045 | 0.1126 | 0.0220 | 0.0390 | 0.3095 |
| Mental Health | 2,175 | 0.0137 | 0.0136 | 0 | 0.1270 |
| **Covariates** | | | | | |
| *Economic Indicators* | | | | | |
| Average Income (in 1,000 NOK) | 3,045 | 260.1885 | 77.4949 | 120.4000 | 587.7000 |
| Unemployment (Females) | 3,045 | 0.0193 | 0.0090 | 0.0018 | 0.1370 |
| Financial Capital (in 1,000 NOK) | 3,045 | 218.2279 | 112.8662 | 55.6000 | 1409.9994 |
| Real Capital (in 1,000 NOK) | 3,045 | 201.6377 | 84.1359 | 69.8000 | 624.2001 |
| Property Transfers | 3,045 | 0.0119 | 0.0052 | 0.0004 | 0.0450 |
| *Demographic Indicators* | | | | | |
| Share Females | 3,045 | 0.4993 | 0.0081 | 0.4489 | 0.5198 |
| Share Secondary or Higher Education | 3,045 | 0.6447 | 0.0666 | 0.3760 | 0.7840 |
| Live Births | 3,045 | 0.0115 | 0.0022 | 0.0022 | 0.0209 |
| Net Migration | 3,045 | 0.0029 | 0.0090 | −0.0839 | 0.0536 |
| Share European Immigrants | 3,045 | 0.0281 | 0.0181 | 0.0010 | 0.1852 |
| Share Non-European Immigrants | 3,045 | 0.0178 | 0.0131 | 0 | 0.0956 |
| Naturalizations | 3,045 | 0.0015 | 0.0012 | 0 | 0.0148 |
| Population Increase | 3,045 | 0.0045 | 0.0107 | −0.0709 | 0.0941 |
| Population Density | 3,045 | 57.7984 | 94.7880 | 0.3002 | 500.9767 |
| Marriages | 3,045 | 0.0043 | 0.0011 | 0 | 0.0110 |
| Divorces | 3,045 | 0.0020 | 0.0007 | 0 | 0.0063 |
| Share Living in Densely Populated Areas | 3,045 | 0.6272 | 0.2288 | 0 | 0.9965 |
| 80+ Population | 3,045 | 613.1579 | 522.2280 | 34 | 2548 |
| *Other* | | | | | |
| Vote Share Left-Wing Parties | 3,045 | 0.4492 | 0.1101 | 0.1707 | 0.8042 |
| Plantings | 3,045 | 0.0093 | 0.0112 | 0 | 0.0562 |

All values are weighted by pre-treatment average 80+ population. Utilization is measured in percent of the 80+ population, all other outcomes variables are measured as per capita of 80+ population. Expenditure variables are available for the period 2003–2014, and mental health information is available for 2000-2014. All covariates are expressed in per capita terms. *Financial Capital*, *Real Capital* and *Transfers* are are lagged by one year. *Source*: Statistics Norway (2019), Norwegian Centre for Research Data (2019), and Fiva et al. (2015).

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).[7]

[7] 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)

**Table 2:** Baseline Averages by Groups

| | Treatment | Control | Not Included |
|---|---|---|---|
| **Outcome Variables** | | | |
| Expenditure NH Care (in 1,000 NOK) | 5.9081 | 6.0826 | 6.4393 |
| Expenditure HB Care (in 1,000 NOK) | 4.9780 | 5.9709 | 5.0399 |
| Utilization NH Care | 17.7912 | 15.2094 | 16.1140 |
| Utilization HB Care | 36.7595 | 39.4726 | 37.7228 |
| Hospital Admissions | 0.5237 | 0.5577 | 0.5872 |
| Hospital Days | 3.3630 | 3.8253 | 4.5213 |
| Mortality | 0.1160 | 0.1244 | 0.1199 |
| Mental Health | 0.0093 | 0.0102 | 0.0228 |
| **Covariates** | | | |
| *Economic Indicators* | | | |
| Average Income (in 1,000 NOK) | 221.6995 | 210.9011 | 248.9195 |
| Unemployment (Females) | 0.0152 | 0.0174 | 0.0176 |
| Financial Capital (in 1,000 NOK) | 172.5849 | 148.5398 | 218.4140 |
| Real Capital (in 1,000 NOK) | 151.2670 | 140.8042 | 141.3711 |
| Property Transfers | 0.0123 | 0.0100 | 0.0132 |
| *Demographic Indicators* | | | |
| Share Females | 0.5004 | 0.5002 | 0.5076 |
| Share Secondary or Higher Education | 0.6532 | 0.6202 | 0.6801 |
| Live Births | 0.0121 | 0.0122 | 0.0136 |
| Net Migration | 0.0047 | 0.0030 | 0.0014 |
| Share European Immigrants | 0.0218 | 0.0205 | 0.0313 |
| Share Non-European Immigrants | 0.0112 | 0.0115 | 0.0367 |
| Naturalizations | 0.0011 | 0.0011 | 0.0029 |
| Population Increase | 0.0068 | 0.0045 | 0.0050 |
| Population Density | 50.2343 | 56.4076 | 460.5162 |
| Marriages | 0.0048 | 0.0050 | 0.0059 |
| Divorces | 0.0020 | 0.0019 | 0.0024 |
| Share Living in Densely Populated Areas | 0.6186 | 0.6168 | 0.8140 |
| 80+ Population | 490.5009 | 592.5662 | 7488.4568 |
| *Other* | | | |
| Vote Share Left-Wing Parties | 0.4149 | 0.4448 | 0.4324 |
| Plantings | 0.0107 | 0.0120 | 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 4.1, 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). Utilization is measured in percent of 80+ population, all other outcome variables are measured as per capita of 80+ population. Expenditure variables are available for the period 2003-2014, and mental health information is available for 2000–2014. *Source*: Statistics Norway (2019), Norwegian Centre for Research Data (2019), and Fiva et al. (2015).

### 4.3 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 corresponding 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. *Financial Capital* and *Real Capital* are expressed as NOK per capita and taken from the municipality's balance sheet of the previous year. *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 $km^2$. *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.

### 4.4 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 and without municipality fixed effects. Our main specifications are thus

$$Y_{mt} = \lambda_t + \beta \text{PHV}_m + \tau\left(\text{PHV}_m \times \text{Post}_{mt}\right) + X'_{mt}\gamma + \varepsilon_{mt} \tag{7}$$

$$Y_{mt} = \lambda_t + \mu_m + \tau\left(\text{PHV}_m \times \text{Post}_{mt}\right) + X'_{mt}\gamma + \varepsilon_{mt} \tag{8}$$

where $\text{Y}_{mt}$ indicates the outcome variable for municipality $m$ in year $t$, $\lambda$ is a set of year dummies, $\text{PHV}_m$ is a binary variable taking on the value 1 in case municipality $m$ belongs to the treatment group and 0 otherwise, the dummy $\text{Post}_{mt}$ equals 1 for each post-treatment year, $X'$ is a vector of covariates, and $\varepsilon_{mt}$ 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. $\mu$ is a set of municipality fixed effects.

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 $\tau$ 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. As we will show below, there is no evidence suggesting that conditioning makes the common trend assumption more likely to hold. Therefore, we control for a selective set of covariates in our main specifications, and test the sensitivity of results to the set of conditioning variables in a robustness check.

## 5 Results

### 5.1 Evidence Supporting Identification

**Balancing tests**. As a 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 three different specifications; the first is a simple DID specification defined in analogy with equation (7) but without covariates. Second, we adapt the fixed-effects specification in equation (8). Third, we estimate a fixed-effects specification with linear municipality-specific trends:

$$X^j_{mt} = \lambda_t + \mu_m + \mu_m \times t + \tau\left(\text{PHV}_m \times \text{Post}_{mt}\right) + \varepsilon_{mt}. \tag{9}$$

Results are presented in Table 3. The first column presents the correlation between treatment status and covariates measured at baseline, whereas the following three columns present estimates from the three 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 observations period is positively associated with baseline incomes, education levels, municipal assets, and a couple of other indicators. This is not 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.

Apparently, all covariates are balanced in the basic DID (column (1)) and Fixed Effects (column (2)) specifications. 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. For the specification with municipality-level trends (column (3)), the estimated "effects" are significant at the ten per cent level in two instances. This would not lead to the rejection of a hypothesis that the covariates are jointly unrelated with the treatment;[8] however, considering the even better performance of the two more parsimonius specifications, we conclude that these two specifications are most appropriate. We choose model (8) as our preferred specification as it also robust to unobserved time-invariant heterogeneity at the municipality level.

**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

[8]The $p$ value for 2 marginally significant (10%) results in 19 independent tests would equal 0.58.

**Table 3:** Balancing Tests

| | Baseline (1) | DID (2) | DID-FE (3) | DID-FE + Trends (4) |
|---|---|---|---|---|
| Unemployment (Females) | −0.0031* | 0.0006 | 0.0011 | −0.0001 |
| | (0.0017) | (0.0012) | (0.0012) | (0.0012) |
| Share Females | −0.0005 | 0.0003 | −0.0005 | −0.0011 |
| | (0.0017) | (0.0013) | (0.0009) | (0.0007) |
| Share Secondary or Higher Education | 0.0343** | 0.0036 | −0.0018 | 0.0021 |
| | (0.0155) | (0.0098) | (0.0029) | (0.0014) |
| Vote Share Left-Wing Parties | −0.0477* | −0.0152 | 0.0139 | −0.0004 |
| | (0.0264) | (0.0206) | (0.0116) | (0.0085) |
| Live Births | 0.0001 | 0.0001 | 0.0001 | −0.0004 |
| | (0.0003) | (0.0003) | (0.0002) | (0.0003) |
| Real Capital | 0.0668* | 0.0113 | 0.0317 | 0.0063 |
| | (0.0359) | (0.0399) | (0.0300) | (0.0345) |
| Financial Capital | 0.1374*** | 0.0397 | −0.0259 | −0.0092 |
| | (0.0488) | (0.0366) | (0.0238) | (0.0186) |
| Property Transfers | 0.0020** | 0.0008 | 0.0008 | −0.0005 |
| | (0.0010) | (0.0014) | (0.0006) | (0.0009) |
| Share European Immigrants | 0.0009 | 0.0032 | 0.0040 | 0.0010 |
| | (0.0021) | (0.0025) | (0.0033) | (0.0020) |
| Share Non-European Immigrants | −0.0002 | −0.0003 | 0.0001 | −0.0003 |
| | (0.0015) | (0.0018) | (0.0017) | (0.0007) |
| Average Income | 0.0445* | −0.0045 | −0.0014 | −0.0032 |
| | (0.0265) | (0.0192) | (0.0080) | (0.0058) |
| Population Increase | 0.0024 | −0.0016 | −0.0006 | −0.0030* |
| | (0.0019) | (0.0020) | (0.0012) | (0.0017) |
| Divorces | 0.0000 | 0.0000 | 0.0000 | 0.0000 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Marriages | 0.0000 | 0.0002 | 0.0001 | 0.0001 |
| | (0.0002) | (0.0002) | (0.0001) | (0.0002) |
| Naturalizations | −0.0001 | 0.0000 | −0.0001 | 0.0001 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Plantings | −0.0007 | −0.0033 | 0.0006 | 0.0005 |
| | (0.0049) | (0.0027) | (0.0011) | (0.0011) |
| Share Living in Densely Populated Areas | 0.0050 | −0.0093 | 0.0047 | 0.0026 |
| | (0.0484) | (0.0384) | (0.0081) | (0.0047) |
| Net Migration | 0.0020 | −0.0019 | −0.0011 | −0.0027* |
| | (0.0016) | (0.0016) | (0.0011) | (0.0016) |
| Population Density | −6.4635 | −5.4528 | 1.7322 | 0.6477 |
| | (20.0261) | (10.3020) | (3.3803) | (0.9450) |
| 80+ Population | −93.0577 | −46.9433 | −7.7702 | 14.5319 |
| | (106.4595) | (53.9647) | (23.8213) | (10.4597) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | | | ✓ | ✓ |
| *Municipality-Level Trends* | | | | ✓ |

*Baseline* denotes the coefficient of regressing the specific covariate on the treatment group indicator in the common pre-treatment period 1994-2000. Estimates indicate the coefficients of the post-treatment variable in a regression of the specific covariate on the post-treatment indicator (and a treatment group indicator in the Difference-in-Differences 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

study graphs specified as

$$Y_{mt} = \lambda_t + \mu_m + PHV\_Intro_{mt}\tau + X'_{mt}\gamma + \varepsilon_{mt} \tag{10}$$

where $Y$ indicates the respective outcome variable for municipality $m$ in year $t$, $\lambda_t$ and $\mu_m$ are sets of year- and municipality-fixed effects, and $PHV\_Intro_{mt}$ specifies a set of lags and leads regarding the year of PHV introduction with $\tau$ as corresponding coefficient vector. Regressions are weighted by the 80+ population and standard errors are clustered at the municipality level. In case the pre-treatment coefficients in $\tau$ 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 A1 and A2. In each graph, we label coefficients as 'balanced' if all treated units contribute to them.

Almost all pre-treatment coefficients are insignificant; only Figures A2a and A2d have one significant pre-treatment coefficient each. Since we are estimating 40 pre-treatment parameters, this finding is consistent with the common time trend assumption.

**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 two specifications

$$Y_{mt} = \lambda_t + \beta \text{PHV}_m + \delta_1 t + \delta_2 \text{PHV}_m \times t + X'_{mt}\gamma + \varepsilon_{mt}, \tag{11}$$

$$Y_{mt} = \lambda_t + \mu_m + \delta_1 t + \delta_2 \text{PHV}_m \times t + X'_{mt}\gamma + \varepsilon_{mt} \tag{12}$$

for the common pre-treatment period. According to the results presented in Table 4, none of the outcomes exhibits significantly deviating trends. We conclude that the common time trend assumption appears to be plausible.

### 5.2 Main Results

Next, we turn to results from a regression analysis according to specifications (7) and (8). 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 two percentage points whereas the utiliza-

**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) |
| **Basic DID** | | | | | | | | |
| Year | 0.0351*** | 0.0901*** | −0.2574* | 1.6176*** | 0.0109* | 0.0083 | −0.0073 | 0.0016*** |
| | (0.0047) | (0.0094) | (0.1551) | (0.4152) | (0.0061) | (0.0075) | (0.0091) | (0.0006) |
| Treated × Year | −0.0082 | −0.0021 | 0.2323 | 0.1028 | −0.0016 | −0.0093 | −0.0023 | −0.0014 |
| | (0.0096) | (0.0136) | (0.2062) | (0.2831) | (0.0122) | (0.0093) | (0.0064) | (0.0009) |
| **Fixed Effects** | | | | | | | | |
| Year | 0.0194 | 0.1003*** | −0.0126 | 1.5039* | −0.0046 | 0.0043 | −0.0239 | 0.0016*** |
| | (0.0292) | (0.0220) | (0.2718) | (0.7808) | (0.0150) | (0.0167) | (0.0176) | (0.0006) |
| Treated × Year | −0.0076 | −0.0099 | 0.2319 | 0.0948 | −0.0047 | −0.0117 | −0.0004 | −0.0014 |
| | (0.0103) | (0.0111) | (0.1962) | (0.2982) | (0.0108) | (0.0083) | (0.0066) | (0.0009) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Covariates* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| $N^{Treatment}$ | 15 | 15 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 846 | 846 | 959 | 959 | 1015 | 1015 | 1015 | 568 |

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) and 1994-2000 for all other outcomes are included. *Treated* indicates whether a municipality belongs to the treatment group.

**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) |
| **Basic DID** | | | | | | | | |
| Post-Treat. | 0.0556* | 0.0421 | −2.2887* | 3.2194** | −0.0897* | −0.0381 | −0.0441*** | 0.0017 |
| | (0.0297) | (0.0476) | (1.2386) | (1.4432) | (0.0470) | (0.0757) | (0.0146) | (0.0020) |
| **Fixed Effects** | | | | | | | | |
| Post-Treat. | −0.0262 | −0.0019 | −1.9008** | 1.6210* | −0.0838** | −0.0926 | −0.0359*** | 0.0019 |
| | (0.0207) | (0.0299) | (0.8260) | (0.8395) | (0.0423) | (0.0668) | (0.0123) | (0.0021) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Covariates* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 1704 | 1704 | 2877 | 2877 | 3045 | 3045 | 3045 | 2175 |
| Multiple Testing p-Value (DiD) | 0.2994 | 0.6248 | 0.1737 | 0.0639 | 0.1737 | 0.6347 | 0.0060 | 0.6208 |
| Multiple Testing p-Value (FE) | 0.5888 | 0.9441 | 0.0719 | 0.1317 | 0.1257 | 0.1697 | 0.0120 | 0.3992 |

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. *Multiple Testing p-Value* denotes the Romano and Wolf (2005a) p-values for the fixed effects an difference-in-differences approach, respectively, using 500 repetitions.

tion of home-based care increases by roughly the same amount. In relative terms, the reduction in nursing home use corresponds to a ten-percent decline compared to baseline. There is some evidence of a reduction in nursing home expenditure in the long term (cf. Figure A1a) but this effect does not attain statistical significance at conventional levels. The effects on home-based care expenditure are uniformly 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 presented in columns (5)-(8). We find evidence suggesting reductions in hospital admissions: the reduction corresponds to an eight-percent decline. Also mortality rates decline by about 4 percent. On the other hand, the effect on mental health admissions is positive at 0.2%. This effect is insignificant but precise, so that we can rule out improvements by more than 0.2%.

Since we test 8 hypotheses for each specification, 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 for the nursing home utilization variables – which however is still statistically significant at the ten per cent level. For mortality the results remain significant at the five per cent level, whereas the effects on home-based care utilization and hospital admissions are a bit more sensitive to the correction for multiple testing.

### 5.3 Effect Heterogeneity

In the previous analysis we focused on the average effects of the prevention program. We now study effect heterogeneity across different types of municipalities.

**Municipality Characteristics**. In a first set of heterogeneity analyses, we consider municipality characteristics that help us to understand what segments of the older population are particularly likely to benefit. We decided to focus on three such municipality-level variables: *Average Income*, *Share Secondary or Higher Education*, and *Population Density*. To analyze effect heterogeneity, we center the specific variables around the annual mean within the treatment group and include an interaction with the post-treatment variable in order to evaluate how the treatment effects vary in case the specific variable deviates from the mean. The corresponding results can be found in Table 6.

There is some evidence that the treatment effect on some variables varies with *average income*. In municipalities with higher average income, a PHV introduction appears to increase home-based care expenditure whereas it has no significant effect for municipalities with an average income around the annual treatment group mean. The results suggest that a one per cent increase in municipal income leads to a PHV effect of 0.6 per cent. Further, the significant reductions in hospital admissions and mortality appear to be especially pronounced in low-income municipalities.

Our results also indicate that in municipalities with a *highly educated population*, preventive home visits are especially effective in increasing home-based care utilization. This may

**Table 6:** Heterogeneity by Municipality Characteristics

| | Expenditure | | Utilization | | Hospital | | Mortality | Mental Health |
|---|---|---|---|---|---|---|---|---|
| | NH Care | HB Care | NH Care | HB Care | Admissions | Days | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Income** | | | | | | | | |
| Post-Treat. | −0.0268 | 0.0028 | −1.9554** | 1.6267* | −0.0797** | −0.0931 | −0.0386*** | 0.0019 |
| | (0.0202) | (0.0258) | (0.7970) | (0.8536) | (0.0405) | (0.0685) | (0.0143) | (0.0021) |
| Income | −0.0014 | 0.0010 | −0.0072 | 0.0267 | 0.0001 | 0.0044 | 0.0012 | 0.0002 |
| | (0.0029) | (0.0026) | (0.0420) | (0.0605) | (0.0023) | (0.0029) | (0.0014) | (0.0002) |
| Post-Treat. × Income | 0.0007 | 0.0056* | −0.0717 | 0.0286 | 0.0047** | 0.0030 | 0.0046** | 0.0000 |
| | (0.0015) | (0.0032) | (0.0722) | (0.0630) | (0.0022) | (0.0042) | (0.0020) | (0.0002) |
| **Education** | | | | | | | | |
| Post-Treat. | −0.0245 | −0.0032 | −1.8713** | 1.3451 | −0.0798** | −0.0968 | −0.0329*** | 0.0018 |
| | (0.0212) | (0.0306) | (0.8179) | (0.8274) | (0.0389) | (0.0652) | (0.0127) | (0.0020) |
| Education | 0.0096 | 0.0045 | 0.0182 | 0.0030 | 0.0014 | −0.0010 | 0.0069* | 0.0006 |
| | (0.0118) | (0.0079) | (0.1655) | (0.1920) | (0.0081) | (0.0088) | (0.0039) | (0.0006) |
| Post-Treat. × Education | −0.0017 | 0.0065 | −0.0344 | 0.2829* | −0.0028 | 0.0007 | 0.0015 | 0.0004 |
| | (0.0032) | (0.0055) | (0.1342) | (0.1581) | (0.0060) | (0.0104) | (0.0027) | (0.0003) |
| **Population Density** | | | | | | | | |
| Post-Treat. | −0.0225 | −0.0001 | −1.8756** | 1.7244** | −0.0818* | −0.0956 | −0.0416*** | 0.0019 |
| | (0.0210) | (0.0276) | (0.8190) | (0.8016) | (0.0430) | (0.0673) | (0.0133) | (0.0022) |
| Pop. Density | −0.0013 | 0.0001 | 0.0226 | −0.0199 | −0.0002 | −0.0013 | −0.0001 | 0.0000 |
| | (0.0021) | (0.0015) | (0.0178) | (0.0283) | (0.0014) | (0.0029) | (0.0006) | (0.0001) |
| Post-Treat. × Pop. Density | 0.0002 | 0.0005** | −0.0013 | 0.0102 | 0.0002 | 0.0002 | 0.0001 | 0.0000 |
| | (0.0002) | (0.0002) | (0.0079) | (0.0069) | (0.0003) | (0.0006) | (0.0001) | (0.0000) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Covariates* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 1704 | 1704 | 2877 | 2877 | 3045 | 3045 | 3045 | 2175 |

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. *Income*, *Education*, and *Pop. Density* correspond to the variables *Average Income*×100, *Share Secondary or Higher Education*×100, as well as *Population Density* centered around the annual treatment group means.

be a demand-side effect, to the extent that well-educated individuals – be it the PHV recipient themselves or their children – are better at articulating their demands to the visiting health care professional. Considering that the standard deviation in our education measure (share of population with secondary or higher education ×100) equals 6.66, having a one standard deviation larger share of highly educated individuals is associated with an increase in utilization comparable to the average effect.

Finally, we note that there is little evidence of heterogeneity with regard to *population density*: only for home-based care expenditure do we note a larger increase in densely populated municipalities. Since there are no parallel differences in the utilization effects, this effect on costs may be driven by the higher wages in densely populated areas.

**Treatment Intensity**. As a proxy for treatment intensity within the PHV program, we exploit information on the average length of home visits (available for all municipalities but one). The corresponding distribution is shown in Figure 4. This allows us to determine how the treatment effect depends on visit duration. Table 7 presents the results; *Post-Treat.* denotes the treatment

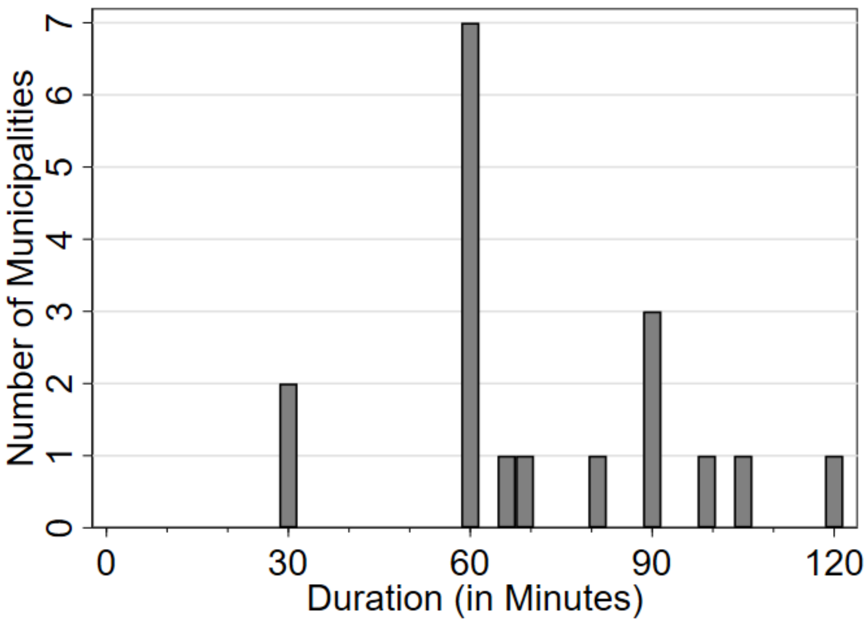



**Figure 4:** Average Visit Duration

effect at the lowest average length of 30 minutes, and *Post-Treat.* $\times$ *Duration* is the effect of each additional minute.

**Table 7:** Treatment Intensity

| | 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. | 0.0058 | −0.1438* | −2.7145* | −0.7242 | −0.0907 | −0.3139*** | −0.0517** | 0.0060 |
| | (0.0525) | (0.0761) | (1.4704) | (1.1472) | (0.0616) | (0.0703) | (0.0260) | (0.0040) |
| Post-Treat. $\times$ Duration | −0.0006 | 0.0032*** | 0.0331 | 0.0496** | 0.0002 | 0.0054** | 0.0000 | −0.0001 |
| | (0.0010) | (0.0012) | (0.0257) | (0.0208) | (0.0019) | (0.0027) | (0.0007) | (0.0001) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Covariates* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 17 | 17 | 18 | 18 | 18 | 18 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 1704 | 1704 | 2856 | 2856 | 3024 | 3024 | 3024 | 2160 |

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Municipality-level clustered standard errors 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. *Duration* denotes the average visits length (in 2013) in minutes beyond the minimum length of 30 minutes.

The estimates indicate that that home-based care expenditure increases with visit duration. Hospital days are significantly reduced by short visits but the effect declines in case of longer visits. Nursing home utilization and mortality effects are both independent of duration.

### 5.4 Reablement Programs

The above exposition has shown that the introduction of PHV programs is associated with a number of changes in treated municipalities. We have also presented an array of results supporting the identification strategy. However, our results may nevertheless 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 2015 showed that 84 municipalities offered a reablement program in 2015. In contrast to PHV programs, the introduction of reablement programs appears to be strongly related to both demographic and economic municipal characteristics. Table A2 performs a number of balancing tests. Column (1) shows how the existence of a reablement program correlates with baseline municipality characteristics. It shows that municipalities with better educated populations, higher incomes and greater population density are more likely to introduce such programs. This difference in levels does not pose a threat to identification. However, the subsequent columns of Table A2 show that also the *introduction*

**Table 8:** Effect of PHV on Reablement Program Introduction

| | Introduction of Reablement Program | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post-Treat. | 0.0178<br>(0.0346) | 0.0169<br>(0.0348) | 0.0196<br>(0.0424) | 0.0071<br>(0.0352) | 0.0266<br>(0.0581) | 0.0176<br>(0.0488) |
| $N^{Treatment}$ | 19 | 19 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 126 | 126 | 126 | 126 |
| Obs. | 3045 | 3045 | 3045 | 3045 | 3045 | 3045 |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | | | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Level Trends* | | | | | ✓ | ✓ |
| *Covariates* | | ✓ | | ✓ | | ✓ |

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Estimates indicate the coefficients of the post-PHV introduction variable in a regression of a reablement program introduction dummy on the post-treatment indicator (and a treatment group indicator in the Difference-in-Differences specification). Covariates are selected in order to maximize the corresponding treatment coefficient's precision. Standard errors clustered at the municipality level in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

of reablement programs is correlated with a number of changes in local characteristics: population density and real capital are correlated with the introduction of a program in each of the specifications we consider. We conclude that an evaluation of reablement programs is impossible with the current research design. But we may consider it as an outcome in the analysis, and we may analyse the extent to which PHV and reablement programs are complementary.

Among the municipalities with a reablement program, 35 municipalities also offered PHV and 5 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.

**Spillover Effects**. As a first step, we test whether municipalities with a PHV program are more likely to introduce a reablement program. The results in Table 8 do not suggest that preventive home visits make the introduction of a reablement program more or less likely – and this holds across a range of speficiations. Estimates are small and insignificant throughout.

**Effect Heterogeneity**. Next we test whether the presence of a reablement program moderates the relationship between PHV programs and the outcomes we consider. Table 9 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. Moreover, there is no evidence of a significant impact of reablement programs or the interaction term on our outcomes.

**Table 9:** Effect Heterogeneity in the Presence of 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 | −0.0339 | −0.0024 | −1.9774** | 1.8158** | −0.0874* | −0.1002 | −0.0359*** | 0.0011 |
| | (0.0207) | (0.0318) | (0.8450) | (0.8657) | (0.0446) | (0.0685) | (0.0125) | (0.0021) |
| Reablement | −0.0869 | −0.0030 | −0.7719 | −0.9486 | 0.0088 | −0.0484 | 0.0021 | −0.0016 |
| | (0.0568) | (0.0272) | (0.6585) | (0.9079) | (0.0318) | (0.0528) | (0.0266) | (0.0029) |
| PHV × Reablement | 0.0750 | 0.0400 | 0.8630 | −1.4106 | 0.0391 | 0.0946 | 0.0233 | 0.0084 |
| | (0.0583) | (0.0536) | (1.4927) | (1.3048) | (0.0592) | (0.0810) | (0.0391) | (0.0058) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Covariates* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 1704 | 1704 | 2877 | 2877 | 3045 | 3045 | 3045 | 2175 |

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.

## 5.5 Robustness Checks

We expose our results to a number of robustness checks. First we assess the sensitivity of results to the inclusion and exclusion of control variables. Then we implement synthetic control methods. Third, we replace statistical inference with design-based inference (Abadie et al., 2017). Finally, we empirically assess the sensitivity of our results to unobserved confounders, using the method proposed by (Oster, 2019). This may be seen as the counterpart of the analysis of reablement programs in section 5.4, which assessed the sensitivity to the inclusion of an observed confounder.

### 5.5.1 Sensitivity to the set of Control Variables

As reported above, we picked the set of conditioning variables selectively in order to achieve maximum precision. We now check how results are affected by altering the set of conditioning variables. Results are presented in Appendix Table A1. We contrast two extreme cases – one with no covariates, and one including all covariates – with our main specifications. For our preferred fixed-effects specification, the set of conditioning variables does not matter – even though our specification with selected covariates sometimes exhibits slightly better precision in the estimates.

### 5.5.2 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 Appendix A.2 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 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 $j = N^T + 1, \ldots, N^T + N^C$ 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 only on pre-treatment realizations of the outcome; one which is based on selected pre-treatment realizations of the outcome (namely from the first and the last available pre-treatment year as well as one period in-between) and on all covariates; and one which is only based on covariates.

After the weights are determined, synthetic controls are constructed for each treated unit by multiplying each donor municipality's outcome and covariates with the municipality-specific weight. The balance of outcomes and controls for different specifications is presented in Appendix Table A3. The first two columns of the table compare the treatment group to the entire donor pool, and the following three columns show the variable means for our three synthetic controls. Clearly, the method improves the balance of pre-treatment outcomes in the majority of cases. For the health-related outcomes, it appears that in particular the specification combining pre-treatment outcomes and covariates produces a better control group than the entire donor pool.

Our main specification (8) can be estimated in a sample of all treated units and the same number of synthetic controls. As before, all regressions are weighted by pre-treatment average 80+ population. Table 10 contains the corresponding results.

**Table 10:** 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. | $-0.0055$ | 0.0076 | $-1.8028^{*}$ | 1.5682 | $-0.0540$ | $-0.1167$ | $-0.0389^{**}$ | 0.0021 |
| | (0.0192) | (0.0425) | (0.9317) | (0.9714) | (0.0470) | (0.0727) | (0.0186) | (0.0021) |
| **Matching on Covariates and Selected Pre-Treatment Outcomes** | | | | | | | | |
| Post-Treat. | 0.0072 | 0.0017 | $-1.7671^{*}$ | $1.7085^{*}$ | $-0.0772$ | $-0.1243^{*}$ | $-0.0466^{**}$ | 0.0032 |
| | (0.0255) | (0.0340) | (0.9364) | (1.0080) | (0.0473) | (0.0748) | (0.0191) | (0.0021) |
| **Matching on Covariates** | | | | | | | | |
| Post-Treat. | 0.0271 | $-0.0039$ | $-2.0154^{**}$ | $2.0851^{**}$ | $-0.0923^{**}$ | $-0.1247^{*}$ | $-0.0644^{***}$ | $0.0036^{*}$ |
| | (0.0300) | (0.0290) | (0.9203) | (0.9615) | (0.0466) | (0.0693) | (0.0204) | (0.0020) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Synth.\,Control}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| Obs. | 384 | 384 | 756 | 756 | 798 | 798 | 798 | 798 |

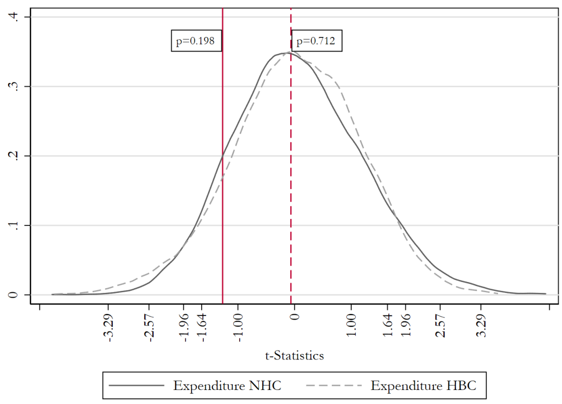
*Matching on Covariates* indicates creating synthetic controls based on pre-treatment covariate averages and *Matching on Pre-Treatment Outcomes* denotes creating the synthetic controls using pre-intervention outcomes. *Matching on Covariates and Selected Pre-Treatment Outcomes* matches on all covariates as well as selected pre-treatment outcomes. All regressions are weighted by the average population aged 80+ in the period 1994-2000. Municipality-level clustered standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. *Baseline* denotes the year 2000 treatment group outcome average (2003 for expenditure variables), and $N^{Treatment}$ and $N^{Synth.\,Control}$ are the number of treatment and synthetic control group municipalities, respectively. *Obs.* is the total number of observations used for estimation.

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.

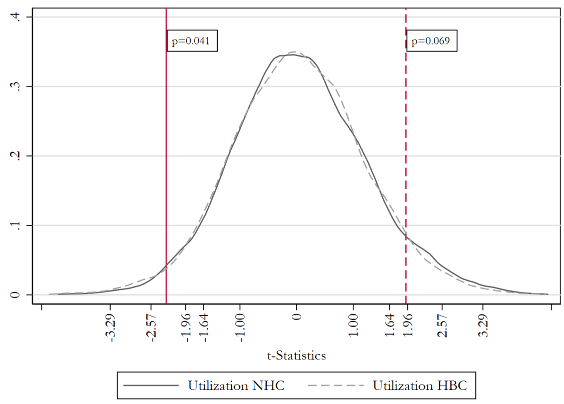
### 5.5.3 Design-Based Inference

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., 2017; MacKinnon et al., 2016). This is repeated 10,000 times to obtain the distributions of the test statistics. The size of the random treatment group is restricted to be equal to the real 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. Figure 5 shows the distribution of the $t$ statistics (vertical line indicates the original $t$ statistics) as well as the two-sided $p$ values indicating the probability of exceeding t-statistics.

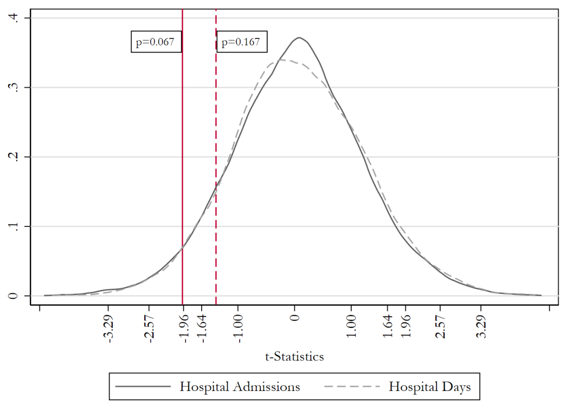
The results according to this alternative basis for statistical inference are very similar to those previously reported. The effects on utilization – of nursing homes and home-based care (Figure 5b) – remain significant at the 10 per cent level. Also the effects on hospital admissions and mortality remain significant. Thus, randomization inference would in general lead to the same conclusions as traditional statistical inference.



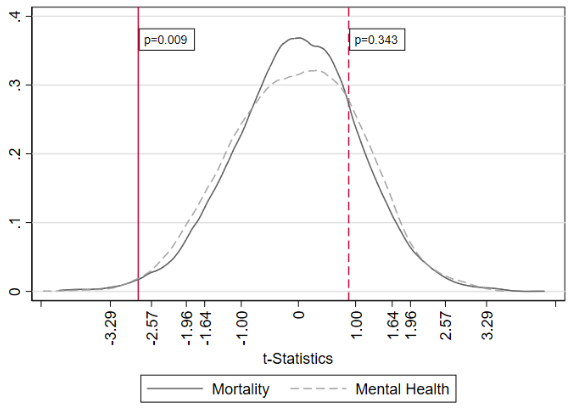

**(a)** LTC Expenditures




**(b)** LTC Utilization




**(c)** Hospital Admissions/Days




**(d)** Mortality/Mental Health

**Figure 5:** Randomization Inference

*Note*: All figures show $t$ statistics distributions of coefficient $\tau$ from model (8) obtained by randomizing the post-treatment variable with 10,000 repetitions. Vertical lines indicate the actual $t$ statistics.

### 5.5.4 Sensitivity Analysis

A key identifying assumption underlying the analysis is that there is no other relevant change in the treated municipalities that coincides with the introduction of PHV programs. We have shown in section 5.4 that the other main intervention happening in Norwegian municipalities

**Table 11:** Sensitivity Analysis: Oster (2019)

| | $\tilde{\tau}$<br>(1) | $\tau^*$<br>(2) | $\delta^*$<br>(3) |
|---|---|---|---|
| Expenditure NH Care | −0.0226 | 0.0178 | 0.6235 |
| Expenditure HB Care | 0.0030 | 0.0905 | −0.0441 |
| Utilization NH Care | −1.8577 | −0.3313 | 1.1939 |
| Utilization HB Care | 1.7853 | 1.5031 | 2.8707 |
| Hospital Admissions | −0.0814 | −0.0727 | 3.8040 |
| Hospital Days | −0.0865 | −0.0622 | 2.4233 |
| Mortality | −0.0433 | −0.0396 | 5.5639 |
| Mental Health | 0.0022 | 0.0065 | −0.6828 |

$\tilde{\tau}$ is the estimates from our preferred specification and $\tau^*$ is the estimated "true" treatment effect assuming proportional selection and $\delta = 1$ (cf. equations (16) and (17) in the appendix). $\delta^*$ denotes the importance of unobserved confounders that would lead to a treatment coefficient of zero.

in the same time period – reablement programs – are unrelated to the introduction of PHV programs. We now assess the extent to which *unobserved* confounders may bias our estimates, using two methods that have been proposed in the literature.

**Oster (2019).** The approach by Oster (2019) is based on the logic that observed confounders $X$ represent a natural benchmark for assessing the potential impact of unobserved confounders $U$. In particular, it is assumed that there is proportionality in the selectivity given by observed and unobserved confounders, defined as $\delta \frac{\sigma_{XT}}{\sigma_X^2} = \frac{\sigma_{UT}}{\sigma_U^2}$ where $\sigma_{iT}$ denotes the covariance between confounder $i \in (X, U)$ and the treatment variable. A situation with $|\delta| = 1$ would imply that unobserved confounders are equally important sources of selection bias as observed confounders, whereas $|\delta| < 1$ indicates less and $|\delta| > 1$ indicates more importance.

This proportional selection relationship allows to conduct two types of sensitivity tests: the first one directly calculates the bias based on given values for $\delta$ and the maximum $R$-squared that would result by including the full set of observed and unobserved confounders, and the second one provides a bound for $\delta$, i.e., the value of $\delta$ is calculated for which the treatment coefficient $\tau$ would equal zero. We provide an exact derivation of these two estimates in Appendix A.6.1. The main results coming out of the analysis are presented in Table 11.

According to the results, our estimated effects appear to be rather robust. The $\tau^*$ coefficients for home-based care utilization, hospital admissions, and hospital days are still comparable to our main results. However, the effect on nursing home care decreases to 18% of its initial size and the coefficient for nursing home expenditure even switches signs. Interpreting the $\delta^*$ leads to a similar conclusion: except for the expenditure outcomes, for all variables the

**Table 12:** Sensitivity Analysis: Ichino et al. (2008) - Summary

| | Expenditure | | Utilization | | Hospital | | Mortality | Mental Health |
|---|---|---|---|---|---|---|---|---|
| | NH Care | HB Care | NH Care | HB Care | Admissions | Days | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Share Significant | 0.00 | 0.00 | 0.88 | 0.12 | 0.20 | 0.00 | 0.64 | 0.00 |
| Share Stable Sign | 1.00 | 0.44 | 1.00 | 0.96 | 1.00 | 1.00 | 0.96 | 0.68 |
| Repetitions | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 |

*Share Significant* denotes the share of $t$ statistics above 1.64, and *Share Stable Sign* is the share of $t$ statistics which have the same sign as the original estimate.

unobserved confounders would have to be much more important than the observed ones in order to produce a treatment effect equal to zero.

**Ichino et al. (2008).** As an alternative to the method suggested by Oster (2019) we also consider an approach introduced by Ichino et al. (2008). This approach is based on the assumption that the unconfoundedness assumption – which is sufficient to achieve identification – is violated when conditioning only on observed covariates $X$, but that it holds if $X$ is augmented by an unobserved confounder $X$. The method then proceeds to put reasonable restrictions on the distribution of that unobserved confounder. In our case, this involves assuming that the confounder is correlated with the treatment variable and that it tends to bias estimates toward zero. We then vary the degree of correlation with the treatment variable and the outcome, and assess the robustness of results. We provide the details of the method in Appendix A.6.2, and the full set of estimates are provided in Appendix Table A5. Table 12 gives a summary: it shows the proportion of simulations which deliver significant estimates, and the proportion of estimated effects that have the same sign as in our baseline specification.

Nursing home care utilization and mortality exhibit a considerable robustness even to severe confounders. The estimates in case of home-based care utilization and hospital admissions lose significance for mild confounders. Although the effects of PHV on home-based care expenditure and hospital days are not significant, the sensitivity analysis results indicate that effects in the opposite direction are very unlikely.

## 6 Discussion

The most robust result coming out of our analysis is that PHV programs reduced mortality in the 80+ population. According to our preferred specification, this reduction amounts to 3.6% from a baseline of 11.9%. It thus corresponds to a reduction of the mortality rate by 0.4

percentage points. Given that the total 80+ population in the treated municipalities was around 7.200 at baseline, this corresponds to 29 avoided deaths per year.

Further results that are remarkably robust are the finding that PHVs lead to a substitution of home-based care for nursing home care and a reduction in hospital care utilization. This raises the question as to whether these other outcomes are mediating the effects of PHVs on mortality – or if they simply pick up an improvement in health driven by the PHV. A full-fledged mediation analysis goes beyond the scope of this paper, but we present some descriptives that may help understanding the relationships between different outcomes. In Figure 6, we present estimates of associations based on a regression of the type

$$y_{kmt} = \mu_m + \Gamma Y_{m,t-1} + \epsilon_{mt} \tag{13}$$

where $Y_{m,t-1}$ is a vector of outcomes, including the lagged value of the dependent variable, $y_{km,t-1}$. Each arrow in Figure 6 represents a parameter in $\Gamma$ and thus represents the association between the outcome and a lag of another outcome.[9]

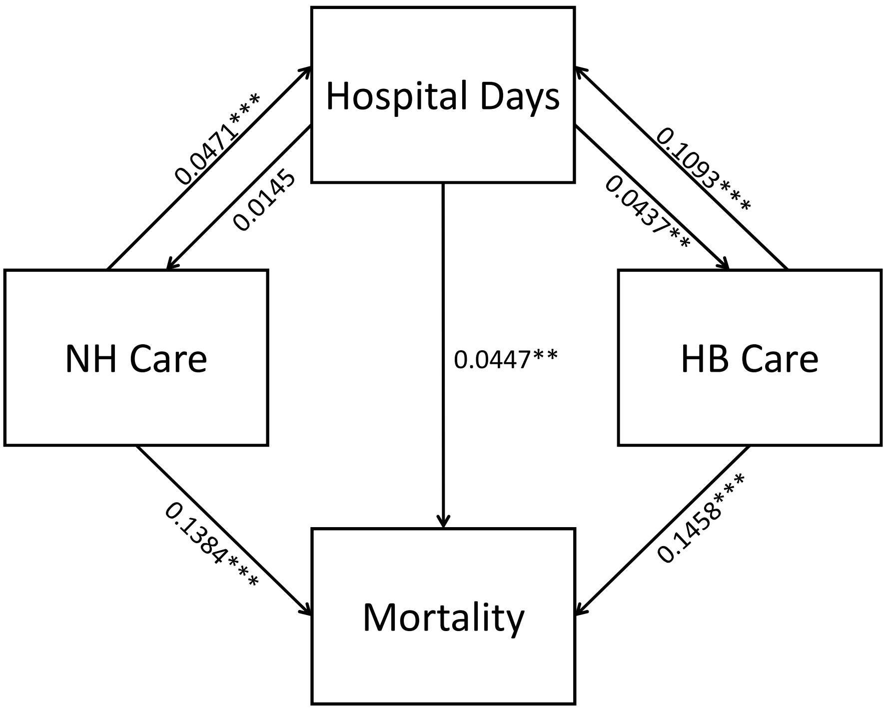



**Figure 6:** Associations between Outcomes

[9] We do not include hospital admissions since the hospital day variable is the product of admissions and average length of stay.

Despite the serious limitation that the estimates in the figure are descriptive, it may be informative regarding the sequence of events that ultimately lead to a reduction in mortality. That LTC utilization mediates the relationship between PHVs and health outcomes seems unlikely for a number of reasons. First, the association between LTC utilization and hospital care is stronger for home-based LTC. Hence, a shift from nursing homes to home-based care would ceteris paribus generate an increase in hospital days. Second, the association between LTC utilization and mortality is fairly similar for both types of care. Therefore, a shift from nursing home to home-based care should not have an impact on mortality according to these descriptive estimates. Third, if conditional on baseline health the substitution of home-based care for nursing home care leads to a reduction in mortality, there is serious misallocation at the outset in the sense that some residents of nursing homes are harmed by being there. While this is a theoretical possibility, we conclude that it seems more likely that PHVs lead to an improvement in health which in turn affects a number of endpoints – such as hospital admissions, LTC utilization, and mortality.

The only result which is not directly consistent with an improvement in older people's health is our – in most cases insignificant – estimate for hospital admissions due to mental health problems. But our preferred specification rules out improvements in mental health admissions by more than 0.2% – which is in stark contrast to the estimate for general hospital admissions, which drop by 8%. If admissions due to mental health problems increase, there are at least three possible mechanisms. First, PHVs may have a detrimental effect on the recipients' mental health. Second, PHVs may lead to improved detection of mental health problems. Third, to the extent that nursing homes provide similar services as hospitals regarding mental health needs, a reduction in nursing home utilization will have mechanical effects on the hospital admissions. Our data do not allow us to distinguish between these three mechanisms, but a direct effect of PHVs on mental health seems unlikely given that all indicators of physical health improve. We posit that the mechanical effect also seems implausible, since individuals who receive treatment in nursing homes for mental health conditions are unlikely to be those that change utilization patterns after the introduction of PHVs. This leaves better detection as the most promising candidate.

# 7 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 corresponding increase in the utilization of home-based services. The reduction of nursing home use corresponds to around 10 percent of the baseline. The reduced reliance on institutional care is not visible in public spending on LTC (even though we note a slight decline in the long term). Since costs are expressed per capita, a possible reason is that nursing home patients are sicker than before PHV was introduced.

The program also appears to have improved older people's general health: hospital admissions are significantly reduced by 8 percent in the 80+ population. There is a corresponding reduction in average hospital days, even though it fails to reach statistical significance at conventional levels. Also mortality among the oldest old is reduced by PHV; the magnitude of the effect is equivalent to a reduction by 4 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. In Section 2.2, we described that a typical 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. Our preliminary analysis suggests that PHVs might have increased the number of hospital admissions due to mental health issues. It remains unclear, however, whether this effect is driven by better detection or whether mental health actually deteriorated as a consequence of the program. We have argued that the former interpretation appears more plausible, but we are not able to rule out other mechanisms.

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.

## Acknowledgements

We thank Joan Costa-i-Font, Hendrik Schmitz, Hans Henrik Sievertsen and Jordi Kalseth for excellent comments on previous drafts. We would like to thank participants at the 12$^{th}$ European Conference on Health Economics, the 19$^{th}$ Norwegian National Health Economics Conference, the 2019 meeting of the German Health Economics Association dggö, the 2018 and 2019 Essen Health Conference, the 39$^{th}$ Nordic Health Economists' Study Group Meeting, the 4$^{th}$ IRDES-DAUPHINE Workshop on Applied Health Economics and Policy Evaluation, the Ageing and Long-Term Care Policy in China Workshop, and the 34$^{th}$ Annual Congress of the European Economic Association.

## References


Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2017). Sampling-based vs. design-based uncertainty in regression analysis. *arXiv preprint arXiv:1706.01778*.

Abadie, A., A. Diamond, and J. Hainmueller (2010). Synthetic control methods for comparative case studies: Estimating the effect of californias tobacco control program. *Journal of the American statistical Association 105*(490), 493–505.

Abadie, A. and J. Gardeazabal (2003). The economic costs of conflict: A case study of the basque country. *American economic review 93*(1), 113–132.

Bakx, P., C. De Meijer, F. Schut, and E. Van Doorslaer (2015). Going formal or informal, who cares? the influence of public long-term care insurance. *Health economics 24*(6), 631–643.

Balia, S. and R. Brau (2014). A country for old men? long-term home care utilization in europe. *Health Economics 23*, 1185–1212.

Bass, S. A., F. G. Caro, and Y.-P. Chen (1993). *Achieving a productive aging society*. Auburn House.

Bhalotra, S., M. Karlsson, and T. Nilsson (2017). Infant health and longevity: Evidence from a historical intervention in sweden. *Journal of the European Economic Association 15*(5), 1101–1157.

Bhalotra, S. R., M. Karlsson, T. Nilsson, N. Schwarz, et al. (2016). Infant health, cognitive performance and earnings: Evidence from inception of the welfare state in sweden. Technical report, Institute for the Study of Labor (IZA).

Bharadwaj, P., K. V. Løken, and C. Neilson (2013). Early life health interventions and academic achievement. *American Economic Review 103*(5), 1862–91.

borger.dk (2017). Tilbud om forebyggende hjemmebesg.

Catillon, M., D. Cutler, and T. Getzen (2018). Two hundred years of health and medical care: The importance of medical care for life expectancy gains. Technical report, National Bureau of Economic Research.

Chung, S., L. I. Lesser, D. S. Lauderdale, N. E. Johns, L. P. Palaniappan, and H. S. Luft (2015). Medicare annual preventive care visits: use increased among fee-for-service patients, but many do not participate. *Health Affairs 34*(1), 11–20.

Costa-Font, J., S. Jimenez-Martin, and C. Vilaplana (2018). Does long-term care subsidization reduce hospital admissions and utilization? *Journal of health economics 58*, 43–66.

Costa-Font, J., M. Karlsson, and H. Øien (2016). Careful in the crisis? determinants of older people's informal care receipt in crisis-struck european countries. *Health Economics 25*(S2), 25–42.

Cutler, D. M. (2008). Are we finally winning the war on cancer? *Journal of Economic Perspectives 22*(4), 3–26.

Dalby, D. M., J. W. Sellors, F. D. Fraser, C. Fraser, C. van Ineveld, and M. Howard (2000). Effect of Preventive Home Visits by a Nurse on the Outcomes of Frail Elderly People. *Canadian Medical Association Journal 162*, 497–500.

Dave, D. and R. Kaestner (2009). Health insurance and ex ante moral hazard: evidence from medicare. *International journal of health care finance and economics 9*(4), 367.

de Meijer, C., M. Koopmanschap, T. Bago d'Uva, and E. van Doorslaer (2011). Determinants of long-term care spending: Age, time to death or disability? *Journal of Health Economics 30*, 425–438.

Duggan, M. G. and W. N. Evans (2008). Estimating the impact of medical innovation: a case study of hiv antiretroviral treatments. In *Forum for Health Economics & Policy*, Volume 11. De Gruyter.

Ehrlich, I. and G. S. Becker (1972). Market insurance, self-insurance, and self-protection. *Journal of political Economy 80*(4), 623–648.

Elkan, R., D. Kendrick, M. Dewey, M. Hewitt, J. Robinson, M. Blair, D. Williams, and K. Brummell (2001). Effectiveness of Home Based Support for Older People: Systematic Review and Meta-Analysis. *BMJ 323*, 1–9.

Fischer, M., M. Karlsson, T. Nilsson, and N. Schwarz (2017). The long-term effects of long terms: Compulsory schooling reforms in sweden. Technical report, Ruhr Economic Papers.

Fiva, J. H., A. H. Halse, and G. J. Natvik (2015). Local government dataset.

Fjørtoft, A.-K. (2012). *Hjemmesykepleie: ansvar, utfordringer og muligheter*. Fagbokforlaget.

Førland, O. and R. Skumsnes (2014). Forebyggende hjemmebesøk til eldre i norge: Resultater fra en landsomfattende kommuneundersøkelse.

Førland, O. and R. Skumsnes (2016). *Hverdagsrehabilitering–En oppsummering av kunnskap*. Senter for omsorgsforskning.

Førland, O. and R. Skumsnes (2017a). Forebyggende og helsefremmende hjemmebesøk til eldre-en oppsummering av kunnskap.

Førland, O. and R. Skumsnes (2017b). Landsforeningen for ansatte i sundhedsfremmende forebyggende hjemmebesg.

Førland, O., R. Skumsnes, S. Teigen, and B. Folkestad (2015). Forebyggende hjemmebesøk til eldre. utbredelse, diffusjonsprosesser og spredning.

Francesca, C., L.-N. Ana, M. Jérôme, and T. Frits (2011). *OECD health policy studies help wanted? Providing and paying for long-term care: providing and paying for long-term care*, Volume 2011. OECD Publishing.

Goda, G. S., E. Golberstein, and D. C. Grabowski (2011). Income and the utilization of long-term care services: Evidence from the social security benefit notch. *Journal of Health Economics 30*, 719–729.

Hackl, F., M. Halla, M. Hummer, and G. J. Pruckner (2015). The effectiveness of health screening. *Health economics 24*(8), 913–935.

Hagen, T. P. and O. M. Kaarbøe (2006). The norwegian hospital reform of 2002: central government takes over ownership of public hospitals. *Health policy 76*(3), 320–333.

Hagen, T. P., K. Negera Amayu, G. Godager, T. Iversen, and H. Øien (2011). Utviklingen i kommunenes helse-og omsorgstjenester 1986-2010.

Hall, J. (2011). Disease Prevention, Health Care, and Economics. In *The Oxford Handbook of Health Economics*, pp. 555–577. Oxford University Press.

Huss, A., A. E. Stuck, L. Z. Rubenstein, M. Egger, and K. M. Clough-Gorr (2008). Multidimensional Preventive Home Visit Programs for Community-Dwelling Older Adults: A Systematic Review and Meta-Analysis of Randomized Controlled Trials. *Journal of Gerontology 63A*, 298–307.

Ichino, A., F. Mealli, and T. Nannicini (2008). From temporary help jobs to permanent employment: what can we learn from matching estimators and their sensitivity? *Journal of applied econometrics 23*(3), 305–327.

Jakobsson, N., A. Kotsadam, A. Syse, and H. Øien (2016). Gender bias in public long-term care? a survey experiment among care managers. *Journal of Economic Behavior & Organization 131*, 126–138.

Karlsson, M., T. Iversen, and H. ien (2018, 10). Aging and health care costs.

Karlsson, M., T. Iversen, and H. Øien (2012). Scandinavian long-term care financing. In *Financing long-term care in Europe*, pp. 254–278. Springer.

Kaul, A., S. Klößner, G. Pfeifer, and M. Schieler (2015). Synthetic control methods: Never use all pre-intervention outcomes together with covariates. *MPRA Paper* (83790).

Kim, H., S. Lee, and W. Lim (2017). Knowing is not half the battle: Impacts of the national health screening program in korea. *IZA DP* (10650).

Kim, H. B. and S.-m. Lee (2017). When public health intervention is not successful: Cost sharing, crowd-out, and selection in korea's national cancer screening program. *Journal of health economics 53*, 100–116.

Kronborg, C., M. Vass, J. Lauridsen, and K. Avlund (2006). Cost Effectiveness of Preventive Home Visits to the Elderly. *European Journal of Health Economics 7*, 238–246.

Langeland, E., O. Førland, E. Aas, A. Birkeland, B. Folkestad, I. Kjeken, F. F. Jacobsen, and H. Tuntland (2016). Modeller for hverdagsrehabilitering - en følgeevaluering i norske kommuner. effekter for brukerne og gevinster for kommunene? Technical report, Senter for omsorgsforskning.

Langeland, E., H. Tuntland, B. Folkestad, O. Førland, F. Jacobsen, and I. Kjeken (2019). A multicenter investigation of reablement in norway: a clinical controlled trial. *BMC geriatrics 19*(1), 29.

MacKinnon, J. G., M. D. Webb, et al. (2016). Randomization inference for differences-in-differences with few treated clusters. Technical report, Carleton University, Department of Economics.

Magnussen, J., T. P. Hagen, and O. M. Kaarboe (2007). Centralized or decentralized? a case study of norwegian hospital reform. *Social science & medicine 64*(10), 2129–2137.

Mayo-Wilson, E., S. Grant, J. Burton, A. Parson, K. Underhill, and P. Montgomery (2014). Preventive Home Visits for Mortality, Morbidity, and Institutionalization in Older Adults: A Systematic Review and Meta-Analysis. *PLoS ONE 9*.

Mommaerts, C. (2008). Are coresidence and nursing homes substitutes? evidence from medicaid spend-down provisions. *Journal of Health Economics 59*, 125–138.

Moyer, V. A. (2012). Prevention of falls in community-dwelling older adults: Us preventive services task force recommendation statement. *Annals of internal medicine 157*(3), 197–204.

National Prevention Council (2011). National prevention strategy.

Norwegian Centre for Research Data (2019). Nsd's regional database. https://nsd.no/nsd/english/regionaldata.html.

Norwegian Government (2013). Politisk plattform for en regjering utgtt av hyre og fremskrittspartiet.

Norwegian Ministry of Health and Care Services (2016). Rundskriv om forebyggende hjemmebesk i kommunene (rundskriv i-2/2016). https://www.regjeringen.no/contentassets/92fac736a57b48b0a60f9bf04acdad5b/rundskriv-i-2-2016-om-forebyggende-hjemmebesok-i-kommunene.pdf.

OECD (2016). *Health at a Glance: Europe 2016: State of Health in the EU Cycle*. OECD.

Øien, H. (2014). Do local governments respond to (perverse) financial incentives in long-term care funding schemes? *The BE Journal of Economic Analysis & Policy 14*(2), 525–549.

Øien, H., M. Karlsson, and T. Iversen (2012). The impact of financial incentives on the composition of long-term care in norway. *Applied Economic Perspectives and Policy 34*(2), 258–274.

Olsen, J. A. (2011). Concepts of equity and fairness in health and health care. *Oxford Handbook of Health Economics*.

Orsini, C. (2010). Changing the way the elderly live: Evidence from the home health care market in the united states. *Journal of Public Economics 94*, 142–152.

Oster, E. (2019). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics 37*(2), 187–204.

Ringard, Å., A. Sagan, S. I. Sperre, and A. K. Lindahl (2013). Norway: health system review. *Health systems in transition 15*(8), 1–162.

Romano, J. P. and M. Wolf (2005a). Exact and approximate stepdown methods for multiple hypothesis testing. *Journal of the American Statistical Association 100*(469), 94–108.

Romano, J. P. and M. Wolf (2005b). Stepwise multiple testing as formalized data snooping. *Econometrica 73*(4), 1237–1282.

Rosenbaum, P. R. and D. B. Rubin (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika 70*(1), 41–55.

Skinner, J. S., D. O. Staiger, and E. S. Fisher (2006). Is technological change in medicine always worth it? the case of acute myocardial infarction. *Health affairs 25*(2), w34–w47.

Statistics Norway (2019). Kostra (local authorities statereporting system). https://www.ssb.no/en/offentlig-sektor/kostra.

Stuck, A. E., M. Egger, A. Hammer, C. E. Minder, and J. C. Beck (2002). Home Visits to Prevent Nursing Home Admission and Functional Decline in Elderly People: Systematic Review and Meta-Regression Analysis. *JAMA: The Journal of the American Medical Association 287*, 1022–1028.

Stuck, A. E., C. E. Minder, I. Peter-Wüest, G. Gillmann, C. Egli, A. Kesselring, R. E. Leu, and J. Beck (2000). A Randomized Trial of In-Home Visits for Disability Prevention in Community-Dwelling Older People at Low and High Risk for Nursing Home Admission. *Archives of Internal Medicine 160*, 977–986.

Tøien, M. (2019). *An exploration of how long-term preventive home visits affect older persons health and possibility for a good life in their own homes. Users and service-providers perspectives*. Ph. D. thesis, University of Oslo.

Tøien, M., M. Heggelund, and L. Fagerström (2014). How Do Older Persons Understand the Purpose and Relevance of Preventive Home Visits? A Study of Experiences after a First Visit. *Nursind Research and Practice*.

Tsai, Y. (2015). Social security income and the utilization of home care: Evidence from the social security notch. *Journal of Health Economics 43*, 45–55.

van Haastregt, J. C. M., J. P. M. Diederiks, E. van Rossum, L. P. de Witte, and H. F. J. M. Crebolder (2000). Effects of Preventie Home Visits to Elderly People Living in the Community: Systematic Review. *BMJ 320*, 754–758.

van Rossum, E., C. M. A. Frederiks, H. Philipsen, K. Portengen, J. Wiskerke, and P. Knipschild (1993). Effects of Preventive Home Visits to Elderly People. *BMJ 307*, 27–32.

Vike, H. (2017). *Politics and Bureaucracy in the Norwegian Welfare State: An Anthropological Approach*. Palgrave Macmillan.

WHO (2002). *Active ageing: A policy framework*. WHO.

WHO (2015). *World report on ageing and health*. World Health Organization.

# A Appendix

## A.1 Theoretical Model

In this appendix we provide some additional results related to the theoretical model presented in Section 3.

**The Individual's Problem**. The second order conditions for the individual's problem are given as:

$$\frac{\partial^2 EU}{\partial L_j^2} = f_j'' V_j' < 0$$
$$\frac{\partial^2 EU}{\partial r^2} = -\pi'' \left(V_m - V_s\right) + 2\left(1-k\right)\pi' \left(V_m' - V_s'\right) + \left(1-k\right)^2 \left[\left(1-\pi\right) V_m'' + \pi V_s''\right] < 0$$

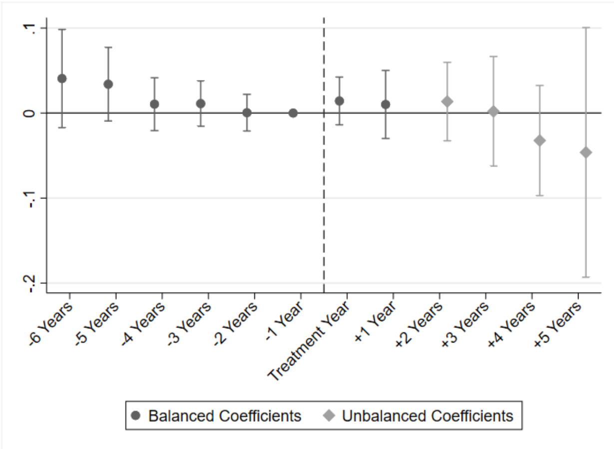
In addition, the various cross-derivatives of the FOCs are all equal to zero, which facilitates the determination of the individual's decision to various parameters.

In order to assess the effects of effectiveness of prevention, we now specify a functional form for $\pi\left(r\right)$. We assume that $\pi\left(r\right) = Ar^{-\beta}$ for some $\beta > 0$. The parameter $\beta$ represents the elasticity of the probability of disability with regard to prevention effort: $\frac{d\pi}{d\beta}\frac{\beta}{\pi} = -\beta$. Henceforth we call it '*effectiveness of prevention*'. Applying again the implicit function theorem, we get

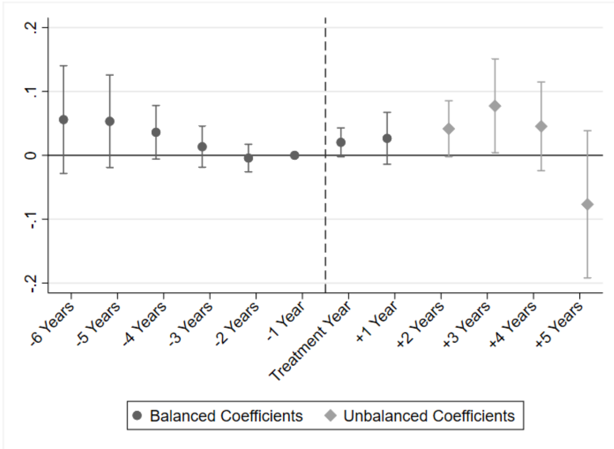
$$\frac{dr^*}{d\beta} = -\frac{\frac{\partial^2 EU}{\partial r \partial \beta}}{\frac{\partial^2 EU}{\partial r^2}} = -\frac{-\frac{\partial^2 \pi}{\partial r \partial \beta}\left(V_m - V_s\right) + \left(1-k\right)\frac{\partial \pi}{\partial \beta}\left(V_m' - V_s'\right)}{\frac{\partial^2 EU}{\partial r^2}}$$
$$= -\frac{-\left(1-k\right)\frac{\pi}{\beta}\left(V_m' - V_s'\right) + \left(1-k\right)V_m'\left(\frac{1}{\beta} - \ln r\right)}{\frac{\partial^2 EU}{\partial r^2}}$$

The first term in the numerator needs to be positive to satisfy the first order condition for $r$ (cf. equation 4 in Section 3). The sign of the second term depends on the sign of $\left(\frac{1}{\beta} - \ln r\right)$, and this term also determines whether an increase in $\beta$ increases the marginal effect of prevention effort: $\frac{d^2\pi}{drd\beta} = -\frac{\pi}{r}\left(1 - \beta \ln r\right)$. In the case that increases in $\beta$ improve the effectiveness of prevention, $\left(\frac{1}{\beta} - \ln r\right) > 0$ so that the entire numerator is positive, implying that the optimal effort $r$ is increasing in $\beta$.

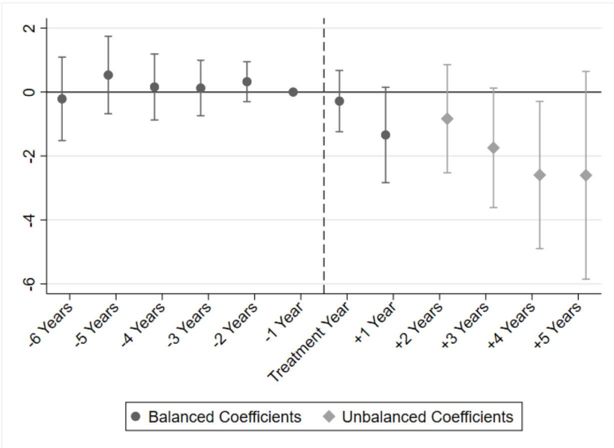
## A.2 Event Study Graphs



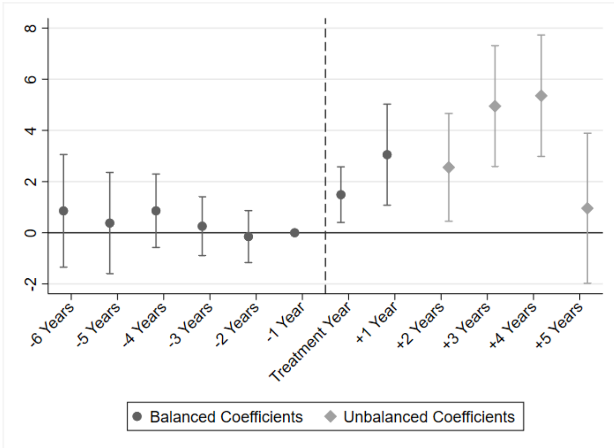

**(a)** Expenditure NH Care




**(b)** Expenditure HB Care



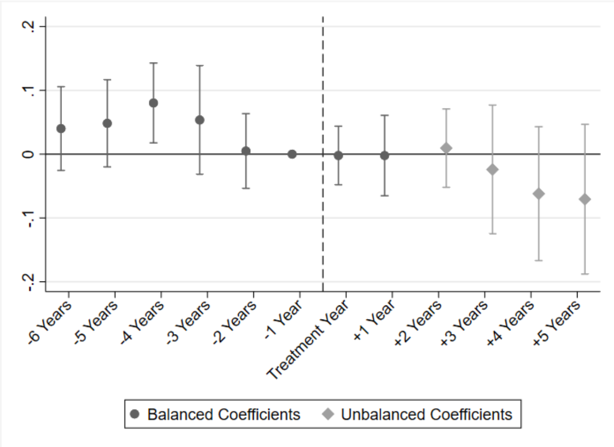

**(c)** Utilization NH Care



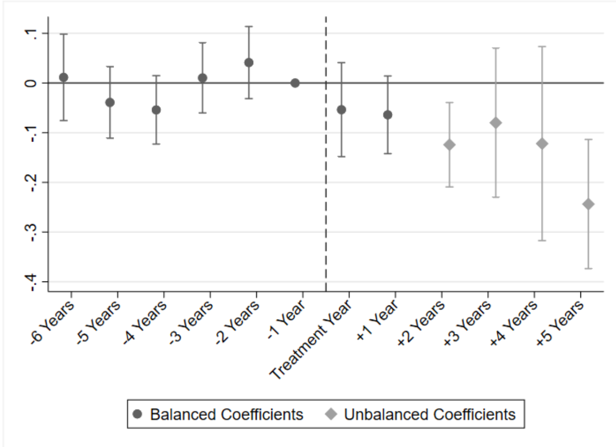

**(d)** Utilization HB Care

**Figure A1:** Event Study Graphs – LTC Expenditure/Utilization

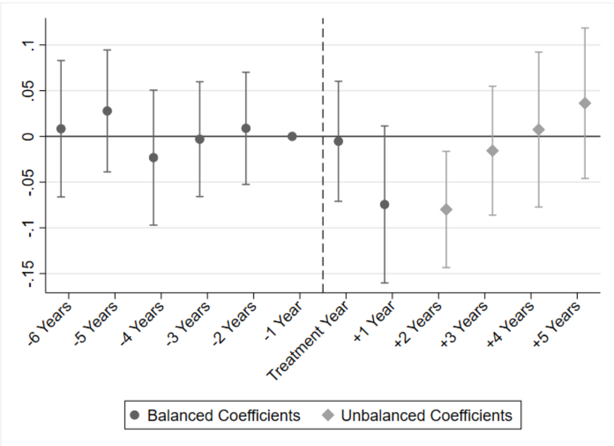
*Note*: Figures show the parameters $\tau$ from model (10). *Unbalanced Coefficients* indicate coefficients estimated on an incomplete set of treatment group municipalities due to insufficient post-treatment observations. Vertical lines correspond to 90% confidence intervals.



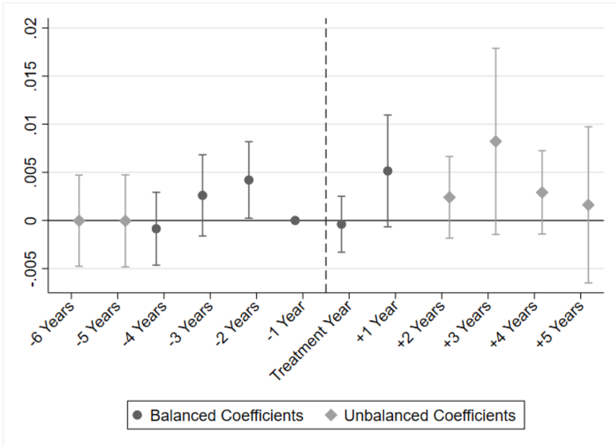

**(a)** Admissions




**(b)** Days




**(c)** Mortality




**(d)** Mental Health

**Figure A2:** Event Study Graphs – Hospital Care/Mortality

*Note*: Figures show the parameters $\tau$ from model (10). *Unbalanced Coefficients* indicate coefficients estimated on an incomplete set of treatment group municipalities due to insufficient post-treatment observations. Vertical lines correspond to 90% confidence intervals.

## A.3 Main Results - Alternative Specifications

**Table A1:** Results - Alternative Specifications

| | Expenditure | | Utilization | | Hospital | | Mortality | Mental Health |
|---|---|---|---|---|---|---|---|---|
| | NH Care | HB Care | NH Care | HB Care | Admissions | Days | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| BASIC DID | | | | | | | | |
| **No Covariates** | | | | | | | | |
| Post-Treat. | $0.0556^{*}$ | 0.0212 | −2.1100 | $3.0238^{*}$ | $-0.1008^{**}$ | −0.0499 | $-0.0495^{***}$ | 0.0017 |
| | (0.0297) | (0.0549) | (1.4013) | (1.7747) | (0.0501) | (0.0875) | (0.0190) | (0.0020) |
| **Selected Covariates (Main Specification)** | | | | | | | | |
| Post-Treat. | $0.0556^{*}$ | 0.0421 | $-2.2887^{*}$ | $3.2194^{**}$ | $-0.0897^{*}$ | −0.0381 | $-0.0441^{***}$ | 0.0017 |
| | (0.0297) | (0.0476) | (1.2386) | (1.4432) | (0.0470) | (0.0757) | (0.0146) | (0.0020) |
| **All Covariates** | | | | | | | | |
| Post-Treat. | 0.0505 | 0.0064 | −2.0451 | $2.7282^{*}$ | $-0.0942^{*}$ | −0.0449 | $-0.0398^{**}$ | 0.0028 |
| | (0.0443) | (0.0458) | (1.3741) | (1.5650) | (0.0523) | (0.0818) | (0.0164) | (0.0021) |
| FIXED EFFECTS | | | | | | | | |
| **No Covariates** | | | | | | | | |
| Post-Treat. | −0.0256 | −0.0037 | $-2.0077^{**}$ | $1.8070^{*}$ | $-0.0822^{*}$ | −0.0884 | $-0.0438^{***}$ | 0.0019 |
| | (0.0230) | (0.0317) | (0.9649) | (1.0040) | (0.0450) | (0.0723) | (0.0156) | (0.0021) |
| **Selected Covariates (Main Specification)** | | | | | | | | |
| Post-Treat. | −0.0262 | −0.0019 | $-1.9008^{**}$ | $1.6210^{*}$ | $-0.0838^{**}$ | −0.0926 | $-0.0359^{***}$ | 0.0019 |
| | (0.0207) | (0.0299) | (0.8260) | (0.8395) | (0.0423) | (0.0668) | (0.0123) | (0.0021) |
| **All Covariates** | | | | | | | | |
| Post-Treat. | −0.0226 | 0.0030 | $-1.8577^{**}$ | $1.7853^{*}$ | $-0.0814^{*}$ | −0.0865 | $-0.0433^{***}$ | 0.0022 |
| | (0.0253) | (0.0323) | (0.8978) | (0.9306) | (0.0442) | (0.0752) | (0.0131) | (0.0022) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Baseline | 5.9081 | 4.9780 | 17.7912 | 36.7595 | 0.5237 | 3.3630 | 0.1160 | 0.0093 |
| $N^{Treatment}$ | 16 | 16 | 18 | 18 | 19 | 19 | 19 | 19 |
| $N^{Control}$ | 126 | 126 | 119 | 119 | 126 | 126 | 126 | 126 |
| Obs. | 1704 | 1704 | 2877 | 2877 | 3045 | 3045 | 3045 | 2175 |

All regressions are weighted by the average population aged 80+ in the period 1994-2000. Standard errors clustered at the municipality level in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. *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.

## A.4 Reablement Programs - Balancing Tests

**Table A2:** Balancing Tests (Reablement Programs)

| | Baseline (1) | DID (2) | DID-FE (3) | DID-FE + Trends (4) |
|---|---|---|---|---|
| Unemployment (Females) | 0.0006 | 0.0005 | −0.0003 | 0.0005 |
| | (0.0010) | (0.0012) | (0.0010) | (0.0008) |
| Share Females | 0.0038** | −0.0021 | −0.0009 | −0.0003 |
| | (0.0016) | (0.0014) | (0.0006) | (0.0006) |
| Share Secondary or Higher Education | 0.0304*** | −0.0187** | −0.0069** | 0.0003 |
| | (0.0111) | (0.0092) | (0.0029) | (0.0015) |
| Vote Share Left-Wing Parties | 0.0262 | −0.0391 | 0.0051 | 0.0049 |
| | (0.0197) | (0.0239) | (0.0090) | (0.0060) |
| Live Births | 0.0005* | 0.0007* | 0.0000 | −0.0004** |
| | (0.0003) | (0.0004) | (0.0002) | (0.0002) |
| Real Capital | 0.0064 | 0.1253*** | 0.1151*** | 0.0612*** |
| | (0.0292) | (0.0377) | (0.0344) | (0.0223) |
| Financial Capital | −0.0169 | 0.0685 | 0.0451* | −0.0138 |
| | (0.0381) | (0.0529) | (0.0234) | (0.0187) |
| Property Transfers | 0.0024*** | 0.0020 | 0.0003 | −0.0005 |
| | (0.0007) | (0.0013) | (0.0006) | (0.0005) |
| Share European Immigrants | 0.0041** | 0.0073* | 0.0047* | 0.0013 |
| | (0.0019) | (0.0040) | (0.0027) | (0.0015) |
| Share Non-European Immigrants | 0.0053*** | 0.0054 | 0.0024 | −0.0007 |
| | (0.0017) | (0.0033) | (0.0018) | (0.0007) |
| Average Income | 0.0375*** | 0.0330 | −0.0071 | −0.0029 |
| | (0.0144) | (0.0215) | (0.0083) | (0.0046) |
| Population Increase | 0.0055*** | 0.0022 | −0.0019 | −0.0038** |
| | (0.0013) | (0.0017) | (0.0012) | (0.0017) |
| Divorces | 0.0002** | 0.0000 | −0.0002** | −0.0002** |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Marriages | 0.0005*** | 0.0004*** | −0.0001 | 0.0000 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Naturalizations | 0.0004** | 0.0002 | 0.0000 | −0.0001 |
| | (0.0002) | (0.0002) | (0.0001) | (0.0002) |
| Plantings | 0.0011 | −0.0033* | 0.0006 | −0.0018** |
| | (0.0022) | (0.0019) | (0.0007) | (0.0008) |
| Share Living in Densely Populated Areas | 0.2040*** | 0.0757** | 0.0034 | 0.0047 |
| | (0.0384) | (0.0344) | (0.0074) | (0.0055) |
| Net Migration | 0.0039*** | 0.0006 | −0.0019 | −0.0033** |
| | (0.0010) | (0.0013) | (0.0012) | (0.0016) |
| Population Density | 55.3549*** | 49.4775* | 11.1932*** | 1.2946* |
| | (17.0673) | (25.8996) | (4.3073) | (0.7615) |
| 80+ Population | 393.1709*** | 194.0962 | 71.3556*** | −14.0020 |
| | (121.2084) | (144.2490) | (19.9345) | (19.4267) |
| *Year-Fixed Effects* | ✓ | ✓ | ✓ | ✓ |
| *Municipality-Fixed Effects* | | | ✓ | ✓ |
| *Municipality-Level Trends* | | | | ✓ |

*Baseline* denotes the coefficient of regressing the specific covariate on the treatment group indicator in the common pre-treatment period 1994-2012. Estimates indicate the coefficients of the reablement post-treatment variable in a regression of the specific covariate on the post-treatment indicator (and a treatment group indicator in the Difference-in-Differences 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

## A.5 Synthetic Control Methods

**Table A3:** Baseline Averages by (Synthetic) Groups

| | Treatment | Control | Synth: Pre-Treatment | Synth: Covariates + Pre-Treatment | Synth: Covariates |
|---|---|---|---|---|---|
| **Outcome Variables** | | | | | |
| Expenditure NH Care | 5.9081 | 6.0826 | 6.0838 | 5.0483 | 4.8845 |
| Expenditure HB Care | 4.9780 | 5.9709 | 5.8603 | 5.4133 | 5.5369 |
| Utilization NH Care | 17.7912 | 15.2094 | 15.4736 | 13.9445 | 13.5504 |
| Utilization HB Care | 36.7595 | 39.4726 | 39.8823 | 37.7789 | 38.5110 |
| Hospital Admissions | 0.5237 | 0.5577 | 0.5569 | 0.5556 | 0.5520 |
| Hospital Days | 3.3630 | 3.8253 | 3.7977 | 3.7998 | 3.7733 |
| Mortality | 0.1160 | 0.1244 | 0.1228 | 0.1225 | 0.1242 |
| Mental Health | 0.0093 | 0.0102 | 0.0114 | 0.0092 | 0.0094 |
| **Covariates** | | | | | |
| *Economic Indicators* | | | | | |
| Average Income | 253.9176 | 242.7416 | 211.4613 | 222.0073 | 221.5236 |
| Unemployment (Females) | 0.0216 | 0.0231 | 0.0169 | 0.0155 | 0.0162 |
| Financial Capital | 204.7245 | 179.6868 | 148.4367 | 159.2539 | 159.0266 |
| Real Capital | 183.9998 | 172.1706 | 142.3743 | 148.6507 | 148.5578 |
| Property Transfers | 0.0141 | 0.0115 | 0.0100 | 0.0120 | 0.0119 |
| *Demographic Indicators* | | | | | |
| Share Females | 0.5015 | 0.5006 | 0.4992 | 0.5012 | 0.5014 |
| Share Secondary or Higher Education | 0.6701 | 0.6385 | 0.6232 | 0.6532 | 0.6486 |
| Live Births | 0.0118 | 0.0112 | 0.0123 | 0.0126 | 0.0124 |
| Net Migration | 0.0046 | 0.0005 | 0.0025 | 0.0062 | 0.0060 |
| Share European Immigrants | 0.0239 | 0.0215 | 0.0211 | 0.0233 | 0.0232 |
| Share Non-European Immigrants | 0.0155 | 0.0157 | 0.0110 | 0.0134 | 0.0129 |
| Naturalizations | 0.0011 | 0.0012 | 0.0011 | 0.0013 | 0.0013 |
| Population Increase | 0.0072 | 0.0016 | 0.0043 | 0.0094 | 0.0088 |
| Population Density | 52.1936 | 57.5330 | 54.2788 | 81.7750 | 79.7965 |
| Marriages | 0.0043 | 0.0042 | 0.0051 | 0.0052 | 0.0053 |
| Divorces | 0.0022 | 0.0022 | 0.0018 | 0.0021 | 0.0020 |
| Share Living in Densely Populated Areas | 0.6387 | 0.6322 | 0.5856 | 0.6732 | 0.6711 |
| *Other* | | | | | |
| 80+ Population | 523.6443 | 637.7554 | 449.7947 | 551.3040 | 622.1223 |
| Vote Share Left-Wing Parties | 0.4118 | 0.4588 | 0.4343 | 0.4254 | 0.4252 |

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 4.1, and *Synth* denotes the average for all synthetic control units within a specification. *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 + Pre-Treatment Outcomes* matches on all covariates as well as selected pre-treatment outcomes. Utilization is measured in percent of 80+ population, all other outcomes are measured as per capita of 80+ population. Expenditure variables are available for the period 2003-2014, and mental health information is available for 2000-2014. *Source*: Statistics Norway (2019), Norwegian Centre for Research Data (2019), and Fiva et al. (2015).

## A.6 Sensitivity Analysis

### A.6.1 Oster (2019)

For the calculations, we need to define the true model of $Y_{mt}$ as

$$Y_{mt} = \lambda_t + \mu_m + \tau\,(\text{PHV}_m \times \text{Post}_{mt}) + X'_{mt}\gamma_1 + U'_{mt}\gamma_2 + \varepsilon_{mt}, \tag{14}$$

**Table A4:** Sensitivity Analysis: Oster (2019)

| | $\hat{R}$ (1) | $\tilde{R}$ (2) | $R_{max}$ (3) | $\tilde{\tau}$ (4) | $\tau^*$ (5) | $\delta^*$ (6) |
|---|---|---|---|---|---|---|
| Expenditure NH Care | 0.4077 | 0.4446 | 0.8137 | −0.0226 | 0.0178 | 0.6235 |
| Expenditure HB Care | 0.7991 | 0.8049 | 0.8631 | 0.0030 | 0.0905 | −0.0441 |
| Utilization NH Care | 0.2728 | 0.3154 | 0.7406 | −1.8577 | −0.3313 | 1.1939 |
| Utilization HB Care | 0.0673 | 0.1457 | 0.9301 | 1.7853 | 1.5031 | 2.8707 |
| Hospital Admissions | 0.1643 | 0.1839 | 0.3796 | −0.0814 | −0.0727 | 3.8040 |
| Hospital Days | 0.1990 | 0.2500 | 0.7604 | −0.0865 | −0.0622 | 2.4233 |
| Mortality | 0.2435 | 0.3574 | 1 | −0.0433 | −0.0396 | 5.5639 |
| Mental Health | 0.0222 | 0.0394 | 0.2114 | 0.0022 | 0.0065 | −0.6828 |

$\hat{R}$, and $\tilde{R}$ denote the r-squared for the models without covariates, and with all observed confounders, respectively. $R_{max}$ is the hypothetical r-squared of a regression including both observed and unobserved confounders, and is defined as $R_{max} = \tilde{R} + (\tilde{R} - \hat{R}) \times 10$. $\tilde{\tau}$ and $\tau^*$ are the treatment effects according to (16) as well as (17) for $\delta = 1$, and $\delta^*$ denotes the importance of unobserved confounders that would lead to a treatment coefficient of zero.

whereas the model without confounders is given by

$$Y_{mt} = \hat{\lambda}_t + \hat{\mu}_m + \hat{\tau}\left(\text{PHV}_m \times \text{Post}_{mt}\right) + \hat{\varepsilon}_{mt}, \tag{15}$$

and with observable confounders we have

$$Y_{mt} = \tilde{\lambda}_t + \tilde{\mu}_m + \tilde{\tau}\left(\text{PHV}_m \times \text{Post}_{mt}\right) + X'_{mt}\tilde{\gamma}_1 + \tilde{\varepsilon}_{mt}. \tag{16}$$

The corresponding r-squared values are $R_{max}$, $\hat{R}$, and $\tilde{R}$.

Under proportional selection and for given values of $\delta$ and $R_{max}$, we obtain the true treatment coefficient by

$$\tau^* = \tilde{\tau} - \delta \frac{(\tilde{\tau} - \hat{\tau})(R_{max} - \tilde{R})}{(\tilde{R} - \hat{R})}. \tag{17}$$

We calculate this coefficient for $\delta = 1$ and for $R_{max} = \tilde{R} + \left(\tilde{R} - \hat{R}\right) \times 10$, so we assume that the unobserved confounders are as important as the observed confounders, and the unobserved $U$ explains ten times as much variation in $Y_{mt}$ as $X$.

In case of the bounding argument, the $\delta$ for which the treatment coefficient would equal zero is calculated as

$$\delta^* = \frac{\tilde{\tau}^2\left[(\tilde{R} - \hat{R})\right]^2[\hat{\tau} - \tilde{\tau}] + \tilde{\tau}\left[(\tilde{R} - \hat{R})\right]\left[\hat{\sigma}_Y^2\left[(\tilde{R} - \hat{R})\right]^2 + [\hat{\tau} - \tilde{\tau}]^2\left[(\tilde{R} - \hat{R})\right]\right]}{\tilde{\tau}^2\left[(\tilde{R} - \hat{R})\right]^2[\hat{\tau} - \tilde{\tau}] + \left[R_{max} - \tilde{R}\right][\hat{\tau} - \tilde{\tau}]\left[\hat{\sigma}_Y^2\left[(\tilde{R} - \hat{R})\right]^2 + [\hat{\tau} - \tilde{\tau}]^2\left[(\tilde{R} - \hat{R})\right]\right]}. \tag{18}$$

### A.6.2 Ichino et al. (2008)

As suggested by Rosenbaum and Rubin (1983), we may assume that unconfoundedness does not hold,

$$\left(Y(0), Y(1)\right) \not\perp\!\!\!\perp \text{PHV} \mid X, \tag{19}$$

but that it holds conditional on an unobserved binary confounder $U$. For example, this confounder may be thought of as another intervention with a similar target group which some municipalities introduced together with PHV, leading to the assumption

$$\left(Y(0), Y(1)\right) \perp\!\!\!\perp \text{PHV} \mid X, U. \tag{20}$$

In addition, it is assumed that $U$ is independent of $X$ and its distribution given by the probabilities $p_{ij} = Pr\left(U = 1 \mid \text{PHV} = i, Y^* = j\right)$. For simplicity, we consider $Y^*$ to equal 1 if $Y$ is larger than the mean, and 0 otherwise (cf. Ichino et al., 2008).

As countless combinations of $p_{ij}$ are possible, we implement a few restrictions suggested by Ichino et al. (2008) to evaluate only the most interesting probabilities. First, we require a positive correlation with the treatment variable, $p_{1.} - p_{0.} > 0$. Second, to evaluate how strong a counfounder must be to bias the treatment effect towards zero, we set $p_{01} - p_{00} > 0$ in case of a positive treatment effect.[10] Third, we define $p_{11} = p_{10}$, and fourth, we set for each year $Pr\left(U = 1 \mid year = t\right) = 0.3 + Pr\left(T = 1 \mid year = t\right)$.

The sensitivity analysis is conducted by varying the two parameters $s = p_{1.} - p_{0.}$ and $d^+ = p_{01} - p_{00}$ (or $d^- = p_{00} - p_{01}$ in case of negative treatment effects, respectively), generating a confounder according to these probabilities, re-estimating the model with the confounder $U$ included, and determining the $t$ statistic for the treatment coefficient. These steps are repeated 1,000 times for each $s$ and $d^+/d^-$ combination. A treatment effect is considered to be robust if the average $t$ statistic does not change its sign and remains beyond the critical values for mild confounders. The corresponding results can be found in Table A5; Table 12 contains a brief summary.

[10]Conversely, we impose $p_{00} - p_{01} > 0$ in the case of a negative treatment effect.

**Table A5:** Sensitivity Analysis – Ichino et al. (2008)

**(a)** Expenditure NH Care

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −1.24 | −1.21 | −1.19 | −1.16 | −1.13 |
| 0.2 | −1.21 | −1.15 | −1.08 | −1.01 | −0.94 |
| 0.3 | −1.17 | −1.08 | −0.98 | −0.87 | −0.76 |
| 0.4 | −1.14 | −1.00 | −0.87 | −0.73 | −0.57 |
| 0.5 | −1.10 | −0.93 | −0.76 | −0.58 | −0.39 |

**(b)** Expenditure HB Care

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −0.05 | −0.03 | 0.00 | 0.04 | 0.09 |
| 0.2 | −0.03 | 0.01 | 0.05 | 0.11 | 0.18 |
| 0.3 | −0.02 | 0.04 | 0.10 | 0.17 | 0.27 |
| 0.4 | 0.00 | 0.07 | 0.16 | 0.25 | 0.37 |
| 0.5 | 0.02 | 0.11 | 0.21 | 0.33 | 0.47 |

**(c)** Utilization NH Care

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −2.28 | −2.25 | −2.24 | −2.22 | −2.21 |
| 0.2 | −2.25 | −2.20 | −2.15 | −2.10 | −2.05 |
| 0.3 | −2.22 | −2.14 | −2.06 | −1.98 | −1.89 |
| 0.4 | −2.19 | −2.08 | −1.97 | −1.85 | −1.72 |
| 0.5 | −2.16 | −2.01 | −1.87 | −1.72 | −1.55 |

**(d)** Utilization HB Care

| | $d^+$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | 1.86 | 1.79 | 1.70 | 1.59 | 1.43 |
| 0.2 | 1.79 | 1.65 | 1.48 | 1.28 | 1.01 |
| 0.3 | 1.72 | 1.51 | 1.26 | 0.97 | 0.58 |
| 0.4 | 1.65 | 1.36 | 1.03 | 0.63 | 0.13 |
| 0.5 | 1.57 | 1.21 | 0.79 | 0.28 | −0.34 |

**(e)** Hospital Admissions

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −1.93 | −1.86 | −1.78 | −1.69 | −1.56 |
| 0.2 | −1.87 | −1.75 | −1.61 | −1.44 | −1.23 |
| 0.3 | −1.81 | −1.63 | −1.43 | −1.19 | −0.89 |
| 0.4 | −1.75 | −1.51 | −1.24 | −0.92 | −0.54 |
| 0.5 | −1.69 | −1.38 | −1.04 | −0.64 | −0.16 |

**(f)** Hospital Days

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −1.36 | −1.34 | −1.35 | −1.36 | −1.41 |
| 0.2 | −1.32 | −1.27 | −1.22 | −1.19 | −1.17 |
| 0.3 | −1.28 | −1.19 | −1.10 | −1.01 | −0.92 |
| 0.4 | −1.24 | −1.11 | −0.96 | −0.82 | −0.67 |
| 0.5 | −1.20 | −1.02 | −0.83 | −0.62 | −0.39 |

**(g)** Mortality

| | $d^-$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | −2.80 | −2.73 | −2.72 | −2.76 | −2.85 |
| 0.2 | −2.65 | −2.43 | −2.26 | −2.14 | −2.07 |
| 0.3 | −2.49 | −2.12 | −1.79 | −1.51 | −1.26 |
| 0.4 | −2.31 | −1.79 | −1.30 | −0.84 | −0.41 |
| 0.5 | −2.13 | −1.44 | −0.78 | −0.14 | 0.48 |

**(h)** Mental Health

| | $d^+$ | | | | |
|---|---|---|---|---|---|
| $s$ | 0.1 | 0.2 | 0.3 | 0.4 | 0.5 |
| 0.1 | 1.19 | 1.14 | 1.10 | 1.07 | 1.04 |
| 0.2 | 1.12 | 1.00 | 0.89 | 0.79 | 0.69 |
| 0.3 | 1.05 | 0.86 | 0.69 | 0.52 | 0.35 |
| 0.4 | 0.98 | 0.72 | 0.48 | 0.24 | 0.00 |
| 0.5 | 0.90 | 0.57 | 0.25 | −0.07 | −0.38 |

*Note*: Tables contain average $t$ statistics for $Pr\,(U = 1 \mid year = t) = 0.3 + Pr\,(T = 1 \mid year = t)$ and given values of $s$ and $d^+/d^-$ after 1,000 repetitions.

## CINCH working paper series

1 Halla, Martin and Martina Zweimüller**. Parental Responses to Early Human Capital Shocks:** Evidence from the Chernobyl Accident. CINCH 2014.

2 Aparicio, Ainhoa and Libertad González. **Newborn Health and the Business Cycle:** Is it Good to be born in Bad Times? CINCH 2014.

3 Robinson, Joshua J. **Sound Body, Sound Mind?:** Asymmetric and Symmetric Fetal Growth Restriction and Human Capital Development. CINCH 2014.

4 Bhalotra, Sonia, Martin Karlsson and Therese Nilsson. **Life Expectancy and Mother-Baby Interventions:** Evidence from A Historical Trial. CINCH 2014.

5 Goebel, Jan, Christian Krekel, Tim Tiefenbach and Nicolas R. Ziebarth. **Natural Disaster, Environmental Concerns, Well-Being and Policy Action:** The Case of Fukushima. CINCH 2014.

6 Avdic, Daniel, **A matter of life and death? Hospital Distance and Quality of Care:** Evidence from Emergency Hospital Closures and Myocardial Infarctions. CINCH 2015.

7 Costa-Font, Joan, Martin Karlsson and Henning Øien. **Informal Care and the Great Recession.** CINCH 2015.

8 Titus J. Galama and Hans van Kippersluis. **A Theory of Education and Health.** CINCH 2015.

9 Dahmann, Sarah. **How Does Education Improve Cognitive Skills?:** Instructional Time versus Timing of Instruction. CINCH 2015.

10 Dahmann, Sarah and Silke Anger. **The Impact of Education on Personality:** Evidence from a German High School Reform. CINCH 2015.

11 Carbone, Jared C. and Snorre Kverndokk. **Individual Investments in Education and Health.** CINCH 2015.

12 Zilic, Ivan. **Effect of forced displacement on health.** CINCH 2015.

13 De la Mata, Dolores and Carlos Felipe Gaviria. **Losing Health Insurance When Young:** Impacts on Usage of Medical Services and Health. CINCH 2015.

14 Tequame, Miron and Nyasha Tirivayi. **Higher education and fertility:** Evidence from a natural experiment in Ethiopia. CINCH 2015.

15 Aoki, Yu and Lualhati Santiago. **Fertility, Health and Education of UK Immigrants:** The Role of English Language Skills. CINCH 2015.

16 Rawlings, Samantha B., **Parental education and child health:** Evidence from an education reform in China. CINCH 2015.

17 Kamhöfer, Daniel A., Hendrik Schmitz and Matthias Westphal. **Heterogeneity in Marginal Non-monetary Returns to Higher Education.** CINCH 2015.

18 Ardila Brenøe, Anne and Ramona Molitor. **Birth Order and Health of Newborns:** What Can We Learn from Danish Registry Data? CINCH 2015.

19 Rossi, Pauline. **Strategic Choices in Polygamous Households:** Theory and Evidence from Senegal. CINCH 2016.

20 Clarke, Damian and Hanna Mühlrad. **The Impact of Abortion Legalization on Fertility and Maternal Mortality:** New Evidence from Mexico. CINCH 2016.

21 Jones, Lauren E. and Nicolas R. Ziebarth. **US Child Safety Seat Laws:** Are they Effective, and Who Complies? CINCH 2016.

22 Koppensteiner, Martin Foureaux and Jesse Matheson**. Access to Education and Teenage Pregnancy.** CINCH 2016.

23 Hofmann, Sarah M. and Andrea M. Mühlenweg**. Gatekeeping in German Primary Health Care –** Impacts on Coordination of Care, Quality Indicators and Ambulatory Costs. CINCH 2016.

24 Sandner, Malte. **Effects of Early Childhood Intervention on Fertility and Maternal Employment:** Evidence from a Randomized Controlled Trial. CINCH 2016.

25 Baird, Matthew, Lindsay Daugherty, and Krishna Kumar. **Improving Estimation of Labor Market Disequilibrium through Inclusion of Shortage Indicators.** CINCH 2017.

26 Bertoni, Marco, Giorgio Brunello and Gianluca Mazzarella. **Does postponing minimum retirement age improve healthy behaviors**

**before retirement?** Evidence from middle-aged Italian workers. CINCH 2017.

27 Berniell, Inés and Jan Bietenbeck. **The Effect of Working Hours on Health.** CINCH 2017.

28 Cronin, Christopher, Matthew Forsstrom, and Nicholas Papageorge. **Mental Health, Human Capital and Labor Market Outcomes.** CINCH 2017.

29 Kamhöfer, Daniel and Matthias Westphal. **Fertility Effects of College Education:** Evidence from the German Educationl Expansion. CINCH 2017.

30 Jones, John Bailey and Yue Li. **The Effects of Collecting Income Taxes on Social Security Benefits.** CINCH 2017.

31 Hofmann, Sarah and Andrea Mühlenweg. **Learning Intensity Effects in Students' Mental and Physical Health –** Evidence from a Large Scale Natural Experiment in Germany. CINCH 2017.

32 Vollmer, Sebastian and Juditha Wójcik. **The Long-term Consequences of the Global 1918 Influenza Pandemic:** A Systematic Analysis of 117 IPUMS International Census Data Sets. CINCH 2017.

33 Thamarapani, Dhanushka, Rockmore, Marc, and Willa Friedman. **The Educational and Fertility Effects of Sibling Deaths**. CINCH 2018.

34 Lemmon, Elizabeth. **Utilisation of personal care services in Scotland:** the influence of unpaid carers. CINCH 2018.

35 Avdic, Daniel, Büyükdurmus, Tugba, Moscelli, Giuseppe, Pilny, Adam, and Ieva Sriubaite**. Subjective and objective quality reporting and choice of hospital:** Evidence from maternal care services in Germany. CINCH 2018.

36 Hentschker, Corinna and Ansgar Wübker. **Quasi-experimental evidence on the effectiveness of heart attack treatment in Germany.** CINCH 2018.

37 Pasha, Mochamad, Rockmore, Marc, and Chih Ming Tan. **Early Life Exposure to Above Average Rainfall and Adult Mental Health**. CINCH 2018

38 Elsner, Benjamin and Florian Wozny. **The Human Capital Cost of Radiation:** Long-run Evidence from Exposure outside the Womb. CINCH 2019

39 de la Mata, Dolores and Carlos Felipe Gaviria Garcés. **Exposure to Pollution and Infant Health:** Evidence from Colombia. CINCH 2019

40 Besstremyannaya, Galina and Sergei Golovan. **Physicians' altruism in incentives contracts:** Medicare's quality race. CINCH 2019

**41** Hayen, Arthur P., Klein, Tobias J., and Martin Salm. **Does the framing of patient cost-sharing incentives matter?** The effects of deductibles vs. no-claim refunds. CINCH 2019

**42** Molina, Teresa. **Pollution, Ability, and Gender-Specific Responses to Shocks**. CINCH 2019

**43** Fischer, Martin, Gerdtham, Ulf-G, Heckley, Gawain, Karlsson, Martin, Kjellsson, Gustav, and Therese Nilsson. **Education and Health: Long-run Effects of Peers, Tracking and Years.** CINCH 2019

**44** Bannenberg, Norman, Førland, Oddvar, Iversen, Tor, Karlsson, Martin, and Henning Øien. **Preventive Home Visits.** CINCH 2019