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The Effect of Investment in Elderly Care on Hospitalization: Evidence from Norway

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Abstract

In this paper we look at how investment in long-term care for elderly affects their hospital use. We use an action plan – the HPE – which increased the number of long-term care units in Norway by almost 60 percent. With an imputation-based event study methodology we exploit the variation in timing of the plan in different municipalities to establish a causal connection between more long-term care units and hospitalization. This is the main contribution of our thesis, and to our knowledge we are the first to estimate the causal effect of LTC on hospitalization in Norway.

We find that municipalities investing in elderly care, by increasing their number of long-term care units, saw an increase in hospitalization compared to those that did not. As a result, the cost of elderly care increases beyond what the investment implies. For the population aged 80 and above our estimates suggest that the number of bed-days in hospital per person increased by around 50 percent in the period following the HPE. The number of overnight stays increased by around 60 percent. These findings seem to partly be explained by a reduction in mortality rates. Our estimates are robust using several robustness checks.

Keywords – NHH, master thesis, economics, event study, LTC, hospitalization, elderly care

Acronyms

ATT	Average Treatment Effect on Treated
HPE	Action Plan for Elderly Care
LTC	Long-Term Care
NSD	Norwegian Centre for Research Data
OLS	Ordinary Least Squares
OVB	Omitted Variable Bias
SSB	Statistics Norway
TWFE	Two-Way Fixed Effect

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1 Introduction

Ageing populations are a great concern to policy makers in all western societies. People living longer is inherently a good thing, a result of economic and social development. However, it also raises a great challenge; how can we provide satisfactory health and care services to an ever-increasing share of elderly? Today, in Norway, the share of the population aged 80 and above is 4.5 percent, by 2060 this share is expected to be 12 percent (Ministry of Finance, 2021). The proportion of the population aged 67–79 is also projected to grow. In total this means that the dependency ratio will increase, causing problems for the provision of health services, both in relation to financing and the number of health workers needed.[1]

Elderly people receive more health services than the rest of the population, and the cost of these services are increasing. Using demographic data from the OECD, Kotlikoff and Hagist (2006) estimate that the average governmental health expenditure in Norway is one and a half times as large per person in the age group 65–74 compared to a person in the age group 50–64. Further, people aged 80 and above demand more than three times as much state funding as those aged 50–64. This, together with the ageing population, will triple the government's expenses related to health services in Norway between 2005–2050, causing health services to represent 25 percent of GDP in 2050.[2] Hagist and Kotlikoff found that in 1970–2002, ageing contributed to approximately 12 percent of the increase in health expenses in Norway, while increased cost per patient accounted for 88 percent.

While many suggestions on how to reduce costs related to health care have been made, we want to study the fiscal implications concerning the organization of elderly care. We do this by looking at whether long-term care, hereby referred to as LTC, is an alternative to expensive hospitalization among the elderly. LTC is day-to-day help with activities such as washing and dressing, household chores like cooking and cleaning, in addition to some forms of medical care. Hospitals, on the other hand, provide medical and surgical treatment, but also nursing and care of the sick. Hospital use in recent years has increased, both due to changing demographics, but also because of more advanced treatment and complex health issues. At the same time, care tasks have been transferred from hospitals

[1]Dependency ratio: share of elderly (67+) compared to share of working population

[2]If the development in the period before 2005 continues until 2050.

to LTC institutions, which in theory should reduce hospitalization. Studies from the United Kingdom and Spain have found that more LTC reduces the use of somatic hospitals among the elderly, and thus lowers their associated hospitalization costs. We will study if similar effects can be observed in Norway, as the topic has not been thoroughly examined in a causal way. We ask the following research question:

"Can more long-term care reduce hospitalization among the elderly in Norway?"

Combining several data sources, including data from The Housing Bank, Norwegian Centre for Research Data (NSD) and Statistics Norway (SSB), we look at the development in hospitalization through the number of bed-days and the number of overnight stays.[3,4] *Bed-days* are defined as the total number of days spent in bed at a somatic hospital. *Overnight stays* are the number of admissions to somatic hospitals where the patient stayed overnight. To see how these variables are affected by LTC we use the Action Plan for Elderly Care, *Handlingsplan for eldreomsorgen*, hereby referred to as HPE. The HPE was a government subsidized build-out of LTC services in Norway between 1998–2003. Using an event study methodology, we can exploit the differences in timing of the HPE between municipalities to find a causal effect.

We find that more LTC in fact increases hospital use for those aged 80+ in Norway. Following the HPE, municipalities investing in more LTC units saw a 50 percent increase in the number of bed-days in hospital per person 80+, compared to a municipality not investing in more LTC. The number of overnight stays per person 80+ increased by around 60 percent. For those aged 67–79 our results are less clear, but they suggest a modest increase in hospitalization following investment in LTC.

This paper is structured as follows: First, in section 2, we give a brief overview of the cost differences between LTC and hospitalization, and present relevant research on the connection between them. In section 3, we present the use and organization of LTC and hospitals in Norway. Section 4 provides an overview of the data and descriptive statistics. In section 5, we explain the event study method. Lastly, in section 6, we present the results of our analyses of how more LTC affects hospitalization as well as discussing these findings and their limitations, before we arrive at a conclusion in section 7.

[3]The Housing Bank (Husbanken) is the government agency responsible for Norwegian housing politics

[4]Part of the data used in this thesis is extracted from NSD – *Norwegian Centre for Research Data Commune Database AS*. NSD is not responsible for the analysis, or the interpretations presented here.

2 Literature Review

In 2017 a general bed-day in hospital imposed a cost to the government of NOK 8,000, while a bed-day in a nursing home had a cost of NOK 3,000. Theory suggests that more LTC reduces the need for expensive hospital care. Thus, increasing the amount of LTC may have important cost-saving implications for the government. In the following sections we will take a closer look at the cost differences between LTC and hospital care and review the current literature on the effect of LTC on hospitalization.

While LTC entails many forms of care, both formal and informal, our analysis is restricted to LTC provided in retirement homes, nursing homes and residential care homes. Retirement homes are dedicated to those who can manage their personal care and hygiene, but who are not able, or do not want, to live at home. Nursing homes, on the other hand, provide more extensive care, even medical care, and are dedicated to those who cannot manage with just home-based services. In residential care homes the users have their own apartment with access to home-based services. Home-based services are nursing and care services, as well as domestic help, provided in the home of the user. In our analysis, we will not look at home-based care explicitly, beyond what occurs in residential care homes. A description of organization of LTC and hospitals in Norway is presented in section 3.

2.1 Cost Differences

LTC and hospitals differ in the way they are funded. Hospitals are funded through taxes, and patients do not have to pay when accessing this service. On the other hand, LTC is partly funded by user payments. In residential care homes, the users pay rent, as well as paying for their home-based services. However, most of the cost related to home-based services are covered through taxes. Despite the user payment in LTC the government covered 90 percent of the costs in 2008 according to numbers from SSB; it is thus not clear that LTC is the cheaper option for the government (SSB, 2021a).

Estimates suggests that the annual costs for the government related to hospital stays are at least 17 percent higher than for LTC.[5] Numbers from SSB (2021b) show that the

[5]LTC in this case includes home-based services.

government had NOK 61 billion in expenses related to overnight stays and day visits in hospitals in 2008. Expenses related to nursing homes and home-based nursing were NOK 52 billion. If we look at the entire specialized health service the annual expense for the government was NOK 95 billion in 2008, making it around 83 percent more costly than LTC (SSB, 2021b). Another estimate says that in total 67 percent of health expenditure in Norway was related to hospitals and ambulatory care in 2017, while LTC facilities accounted for 16 percent (Michas, 2021).

The cost of LTC can be difficult to pin down, as it depends on the type of service provided. Nursing homes are more costly to operate than residential care homes, even when the residential care homes have 24-hour care (Hjelmbrekke et al., 2011). SSB estimates a corrected gross operational cost per institutional unit in municipalities of NOK 1,555 per day in 2002 (SSB, 2018). In 2016, the cost had grown to NOK 3000 per day.[6]

When comparing the cost of a bed-day in nursing homes and hospitals we find that hospitals are 260 percent more expensive. This number, however, depends on the estimation method. The Norwegian Medicines Agency estimates that a general bed-day in hospital had a cost of NOK 8,000 in 2017 (The Norwegian Medicines Agency, 2020). This is 5,000 more than the 3,000 per institutional unit in 2016. In another study they found an overall average in 2017 of NOK 17,000 per bed-day. If we instead look at a day in the intensive care unit, hospitals become even more costly. In the period 1997–1999 a day in intensive care had a cost of NOK 19,378, corresponding to 105,961 per stay (Flaatten and Kvåle, 2003).[7]

These numbers show that hospitals are more expensive to operate than LTC, both overall and in per patient or per bed terms. Most cost numbers on LTC are related to nursing homes, but as we will present later, nursing homes are more expensive than residential care homes, so the cost differences between hospitals and LTC could be even greater. Consequently, if LTC could substitute some of the hospital use among the elderly this would reduce government expenses.

[6]Costs in institutional units varies depending on the level of care needed and type of unit, this is an average.

[7]Measured in 2001 prices.

2.2 Effects of LTC on Hospitalization

2.2.1 Health and Cost Implications of LTC

More LTC has positive health effects, such as reduced depression rates and increased sense of autonomy, according to research from the United Kingdom. These studies and experiences are from residential care homes and target people above pension age with a certain care need. They report various health gains such as: reduced loneliness and depression, higher perceived mental health and quality of life, lower death rate in the period following the move to the facility, more interactions with others, increased autonomy, and enhanced self-care (Berrington, 2017; Housing LIN, 2019; Institute of Public Care, 2012; McLaren and Hakim, 2003; Wood, 2017). Such health benefits can reduce hospitalization. The topic is not thoroughly studied in Norway, but we would expect to see similar effects here.

In addition to health effects, increased use of residential care homes can have important cost-saving effects for municipalities when it is used as an alternative to nursing homes. Residential care homes are assumed to be able to postpone or replace the more expensive institutional care in Norway (Daatland, 2014). Further evidence from the United Kingdom suggests that residential care homes and home-based care, particularly regarding elderly with need for extended care and support, provide significant cost-benefits (Housing LIN, 2019; McLaren and Hakim, 2003).

LTC, in later years, has taken over responsibilities from hospitals and increased the care level. Elderly care in the period 1990–2010 was characterized by an increase in nursing and residential care home units, and a substantial reduction in retirement home units (Brevik, 2010). The shift from retirement homes to nursing homes should lead to less hospitalization as nursing homes are able to care and treat patients in a way that retirement homes cannot. This is due to nursing homes having higher skilled staff who provide care and treatment that previously took place in a hospital. Indeed, many patients who previously received palliative care at a hospital, now receive this in the municipal health service (Vaksvik, 2017). Brevik (2010) argues that if municipalities take over responsibilities from hospitals, especially when it comes to elderly patients, it must also be the case that the resource use in a particular municipality will affect that municipality's need for hospital services. That is, municipalities who spend a lot on LTC should need less hospitalization.

Given the discussion above, sufficient LTC coverage, and particularly enough institutional units is essential for reducing unnecessary hospitalization. Through several Norwegian studies, it is estimated that 400,000 bed-days in hospitals could be avoided if municipalities had better service offers (Ministry of Health and Care Services, 2009, p. 42). These studies include two doctoral dissertations, an expert panel assessment, and a study of acute admissions to Norwegian hospitals. The study on acute admissions showed that of the 415,000 acute admissions in 2007, as many as 110,000 could have been avoided with readily available information concerning the patient and appropriate care alternatives at the municipality level (Ministry of Health and Care Services, 2009, p. 42). With the average length of stay being five days, this could amount to around 550,000 bed-days per year.

While the numbers above refer to the entire population, an analysis from the University of Oslo conducted by Terje P. Hagen found that in 2007 there was a reduction in acute admission to hospital for those aged 80 and above, related to municipalities with a high institutional service offer (Hagen, 2009). In addition, long-term bed-days in hospitals for those over 80 are reduced when municipalities have high coverage rate from nursing homes and good general practitioner coverage. The coverage rate is the ratio of 24-hour care units to the number of people aged 80 and above. However, these reductions only represent a small part of the total use of bed-days in hospitals.

What are the possible cost implications of low LTC coverage in municipalities? In 2002 it is estimated that 88,823 bed-days came from patients already deemed ready for discharge (Kalseth et al., 2004).[8] It would cost NOK 125 million for the municipalities to take care of these patients, while the associated cost of keeping them in hospitals is NOK 288 million. Municipalities would thus care for these patients for less than half the cost. If municipalities took care of the yearly estimated 400,000 avoidable bed-days, and costs benefits were the same as for patients ready for discharge, the government could save around NOK 750 million annually by transferring patients from hospitals to municipalities.

[8]This relates for patients ready for discharge who are not healthy enough to go home.

2.2.2 Previous Studies on the Effect of LTC on Hospitalization

Several studies from different countries find a decrease in hospital use with more home care. In a meta-analysis by Hughes et al. (1997) this effect is estimated to be a reduction of 1.4–3.3 percent for those who receive home care compared to those who do not. While we are not looking at home care explicitly in this thesis it is essential for the care provided in residential care homes. If people living at home experience reduced hospitalization with more home care, those who live in residential care homes and receive home-based care should see similar effects. However, it must be noted that the population living in residential care homes could be sicker than those receiving care in their own homes.

A Norwegian study finds a small but significant connection between more LTC and hospitalization. Using the entire population over the age of 66 in the period 2002–2006 the study concludes that there is a weak positive link between LTC and the number of bed-days (Deraas et al., 2011). Using age and sex stratification, and adjustments for several confounders, the study revealed a weakly positive and statistically significant relationship between LTC and hospitalization rates for women aged 67–79 and all men. For women aged 80 and above, there was a weak and negative relationship, but it neither statistically significant nor clinically important. However, the study uses linear regression, which is not especially well suited to estimate causal effects, in particular due to omitted variable bias (OVB). We hope to improve this in our study as the event study methodology is much better at dealing with OVB and other limitations of linear regression.

Another study by Condelius et al. (2008) was not able to establish a connection between municipal care and hospitalization. The researchers used linear regression on a sample of 4,907 people aged 65+ from four municipalities in southern Sweden. They examined the number of planned and acute hospital admissions during a year and its relation to municipal care, outpatient care, multi-morbidity, age and sex, making comparisons between those having one, two, and three or more hospital stays during a year and between those with and without municipal care services. Descriptive statistics show that those receiving municipal care more often had three or more hospital stays yearly, and a longer mean length of stay. The researchers found this to be a result of municipal care recipients being older and with higher degrees of multi-morbidity than their comparison group. However, the researchers were not able to establish a connection between municipal care and hospitalization through linear regression analyses. They conclude that further investigations are needed.

By contrast, a causal analysis from Spain by Costa-Font et al. (2018) find a significant and negative relationship between LTC and hospitalization. The researchers state that in their preferred estimates, hospital costs were reduced by 11 percent due to increased LTC coverage. The study is published in *Journal of Health Economics* and utilize a reform that increased LTC availability through subsidization in Spain to see if this affected hospitalization. Using a differences-in-differences approach they find robust evidence that the reform led to a reduction in hospital admission and utilization among those receiving a care-giving allowance. Other beneficiaries received publicly funded home care, this was also associated with a reduction in hospitalization, although weaker. Moreover, five years after its implementation, the subsidies were reduced, which increased the length of stay and number of admissions in hospitals, further strengthening the case for a causal connection. This study is the most reliable source we have on the connection between LTC and hospitalization, and the one closest in methodology to our study.

2.3 Implications for our Study

The literature is not clear on how LTC for elderly affects hospitalization, but the most reliable estimates suggest a negative connection. In addition, there has been a shift in the division of tasks, such that LTC services are responsible for care and treatment previously administered by hospitals. Low coverage rate in municipalities can hinder the division of task and cause unnecessary hospitalization. This can be very costly for the government. It thus seems that expanding LTC services should reduce hospitalization and the cost associated with caring for the elderly.

Through our analysis we will try to establish a causal connection between LTC and hospitalization and see if it has the expected negative effect in Norway. This is the main contribution of our thesis, and to our knowledge we are the first to estimate the causal effect of LTC on hospitalization in Norway. We use the natural experiment provided by the HPE to do this and hope to add to the very limited literature on the effect of the HPE. Though several reports evaluate the action plan, few have investigated its implications in a broader perspective. The main exception is a causal analysis by Løken et al. (2014), it looks at how the HPE affected labour supply decisions of grown children. We hope to add to this limited literature with our analysis on hospitalization. In addition, we explore the effect the HPE had on mortality rates and moving patterns. We also use our estimates to comment on the cost impact and possible policy implications.

3 Institutional Background

The HPE increased the number of LTC units by almost 60 percent in the period 1998–2005. Our analysis will use this to see how more LTC affects hospitalization among the elderly. In this chapter we will present some background information relevant for our analysis. Section 3.1 is about LTC for elderly and how it was affected by the HPE, and Section 3.2 gives a short overview of the hospital use in Norway.

3.1 Long-Term Care for Elderly

After the Second World War, the government took on responsibility for LTC services, and municipalities were put in charge of provision. While this increased the amount of care services, municipality autonomy led to large geographical discrepancies in care coverage. In 1960, there were around 22,400 retirement and nursing homes units in Norway (Ministry of Health and Care Services, 2006). In the 1980s there was an extensive reorganization of services to ensure better prioritization and resource utilization. The reorganization was characterized by decentralization and coordination. Federal grants earmarked for elderly care were replaced by transfers to municipal budgets based on estimated need.[9] This gave municipalities freedom to allocate their resources as they saw fit, resulting in greater variation in service provision across municipalities (Ministry of Health and Social Affairs, 1997). By 1997 the expansion of LTC services had progressed considerably and there were around 48,000 retirement home, nursing home, and residential care home units.

In June 1997, the Norwegian parliament decided to use extraordinary governmental tools to increase quality and expand the capacity of long-term elderly care (The Housing Bank, 2004). This was done through an action plan – the HPE – implemented 1st of January 1998. The action plan aimed to strengthen home care services, build more residential care homes and nursing homes, ensure the availability of one-bed rooms, and improve the quality of old nursing homes (The Housing Bank, 2004). The financial instruments of the action plan were operating grants, investment grants and subsidies for interest and repayments on loans. In addition, the government used several measures to recruit personnel, as well as legal measures to enhance quality in services.

[9] *Need* in each municipality was estimated on the basis of demographics and income.

The most important tool in the HPE was investment grants. Our analysis will be based on investments in nursing homes and residential care homes through the HPE. To receive the grant, municipalities had to apply to the Housing Bank, and applications were granted based on fulfilment of formal requirements and advise from the County Executive and County Doctor (The Office of the Auditor General, 2004, p. 45). The HPE application period was 1998–2003. To ensure that poor municipalities with low coverage rate took part in the action plan, additional funds were given to municipalities with especially low coverage. The municipalities received a specific amount per new unit. While investment grants were available from 1994, they were larger during the HPE, and paired with a compensation grant that helped municipalities cover costs related to investment loans. Before the HPE municipalities could get NOK 100,000 per new residential care unit, and 150,000 per nursing home unit. Under HPE the corresponding amounts were NOK 740,000 and 830,000 (ECON, 2003).

Although, in theory, constrained by the total grant amount allocated by the parliament each year, the Housing Bank could give funds to all applications meeting the formal requirements in the first years as applications were scarce (The Office of the Auditor General, 2004, p. 48). Municipalities with good planning skills were able to build many projects early on. This meant that several large city municipalities with complex planning systems were not able to take part in the HPE at an early stage. Before 2001, municipalities built a lot more residential care homes compared to nursing homes due to lower operational costs.

Later, however, it was clear that one of the central goals of the HPE – to provide a more equal service across the country – was not met. Thus, guidelines were updated 1st of January 2001 to prioritize funds to municipalities with low coverage rates. In addition, the total grant amount was divided between counties to ensure more equal distribution, that is, county wise quotas were established (The Office of the Auditor General, 2004, p. 49).[10] This meant that the number of denied projects increased. However, many of these later received funds when the HPE was extended.

The original goal of the HPE was to build 24,400 new LTC units between 1998–2001. Some units would increase the standard of current facilities, giving an estimated net

[10]Division of funds between counties were based on county coverage rate

increase of 13,600 new units in the plan period. To ensure a more equal service offer, the goal was to increase the coverage rate to at least 25 percent in all municipalities. Later, the action plan was expanded with 14,000 extra units. The plan's time frame was also extended, with all grants given by the end of 2003, and building finished by 2005 (The Office of the Auditor General, 2004).[11]

Results

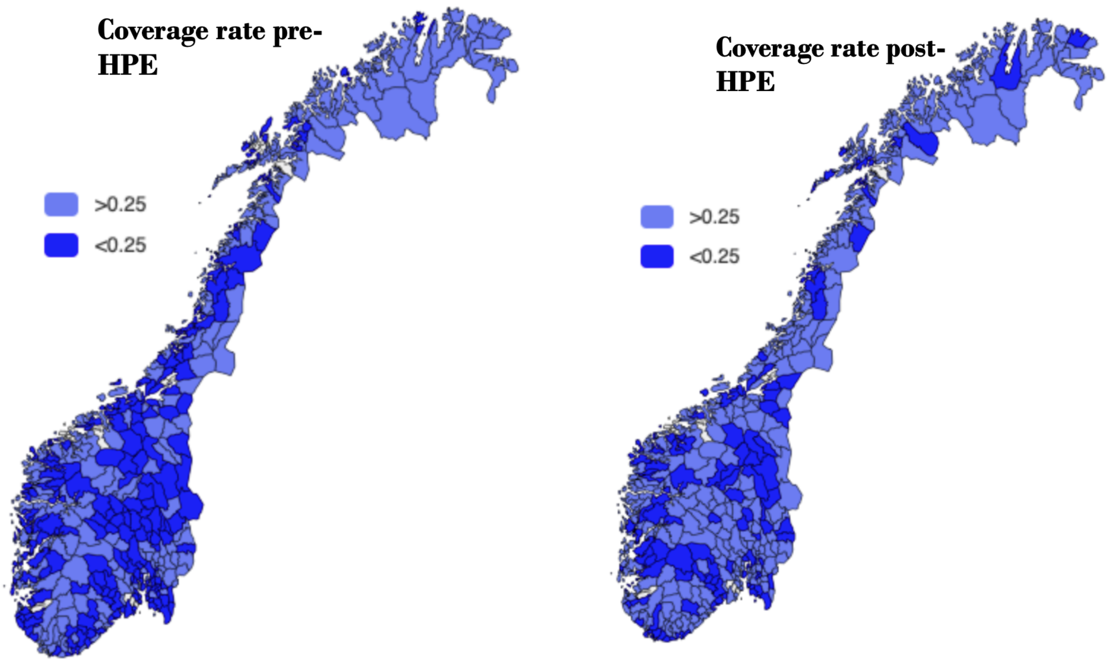
With investment grants seven times larger than before the HPE, applications for grants increased considerably in the period 1998–2003, and 38,608 nursing home and residential care home units received approval (The Housing Bank, 2004). The Office of the Auditor General estimates that the total cost for the municipalities was NOK 60.5 billion (The Office of the Auditor General, 2004, p. 46). The government covered approximately NOK 40 billion. In the period 1997 to 2004, the number of full-time equivalents increased by about 17,000, corresponding to an increase of 19 percent (Ministry of Health and Care Services, 2005). The increase of full-time equivalents is important to the extent that it ensures a good care offer in nursing and residential care homes. Of the 38,608 units approved a little more than 27,000 represents a net increase in the number of LTC units in Norway (The Office of the Auditor General, 2004, p. 57). This is an increase of 58 percent since 1997.

By the end of the HPE, coverage rates still differed significantly across municipalities, but the plan helped raise the minimum standard (Ministry of Social Affairs, 2002). In the period 1997–2001, the coverage rate for nursing homes, retirement homes and residential care homes increased from 25.9 percent to 29.4 percent (The Office of the Auditor General, 2004). However, these numbers are based on inclusion of some units not suitable for 24-hour care, and a 24-hour coverage rate of 25 percent is estimated to correspond to a 30 percent coverage rate using the numbers above.[12] The goal of at least 25 percent coverage rate was thus not entirely met. It is also worth noting that the increase in coverage rate is solely related to an increase in residential care homes. 26 percent of residential care home units for those over 67 years had 24-hour care in 2006 (Brevik, 2010). The coverage

[11] Not all buildings were completed in 2005, although that was the goal, the HPE was considered done in 2006.

[12] 24-hour care is provided in institutional based care, that is nursing homes and retirement homes. In addition, some residential care homes can fall under this category with appropriate follow-up from qualified personnel in the home-based services.

rate in municipalities pre and post HPE can also be seen in figure 3.1. The figure is made from our data; a description of the data set is provided in section 4. It confirms that fewer municipalities had below 25 percent coverage rate after the HPE, though there still were some, causing the goal of at least 25 percent coverage rate in all municipalities to not be met.[13]



(a) Coverage rate 1997

(b) Coverage rate 2006

Figure 3.1: Development in coverage rate

3.2 Hospitalization

Elderly people require significantly more hospitalization than the rest of the population, which comes in addition to their use of LTC services. It is the Ministry of Health and Care Services that has the overall responsibility for hospitals in Norway. Hospitals are costly to operate due to the highly trained staff and advanced equipment.

A person aged 80 or above had four more overnight stays, and six and a half times more bed-days, than a person in the age group 40–59 in 1999 (SSB, 2011). In addition to elderly consuming more hospital services, there has been a general increase in hospital

[13]Post-HPE the 25 percent coverage rate is defined as municipalities with 30 percent coverage rate, due to inclusion of units not suitable for 24-hour care. This also explain why some municipalities who initially had a coverage rate above 25 percent fall below that after the HPE.

use. Between 2000–2008 the number of admitted patients increased by 11 percent and the number of day-visits increased by 82 percent (Brevik, 2010). During the same period, bed-days have been reduced by 0.9 days per stay. Looking at the period 1990–2004, the average length of stay was reduced from 7.2 to 5.2 days (Johnsen, 2006).

During 1990–2010, hospitals have gone from being a treatment and care institution to specializing in treatment (Brevik, 2010). The increased use of hospitals is thus somewhat surprising considering the development in services provision. Many of the care tasks have been handed over to municipalities, such as palliative care, rehabilitation, and recreation. While there can be many reasons why the use of hospitals has not gone down, such as more complex clinical profiles, higher demand for quality in services, and an ageing population, this thesis aims to shed some light on this development.

4 Data

To investigate the effect of more investment in LTC on hospitalization we have constructed a panel data set. It combines data on investment with municipality statistics and relevant outcome variables. The data set entails annual data from 1994 to 2007 for all Norwegian municipalities. To construct the data set, we have utilized data from three main sources: data on investments through the HPE from the Housing Bank, population statistics from SSB, and municipality data from the Commune Database by NSD, including data on hospitalization.

The municipality structure in Norway has changed a lot between 2000–2020, and the data from the Housing Bank does not follow the structure from the period 1998–2002, but rather a later structure. Consequently, all other data have been altered to fit the structure from the Housing Bank, leaving us with 418 municipalities instead of the 435 Norway had in 2000. Importantly, this does not include merging any municipalities not taking part in the HPE with municipalities that did.

4.1 Data Sources

We have obtained data on applications for investment grants through the HPE from the Housing Bank.[14] It contains data on all applications for grants in the period 1998–2005. Each application receives a case number, and information about the application, such as date, applicant, and the amount/number of units applied for is registered. The Housing Bank process the application and decides if funds are granted or not. Lastly, the data includes a resolution date; the date when the application received the funds. Funds were transferred when the project was finished built and residents had moved in.

In our analysis, we disregard applications not receiving grants. This is because rejection of certain projects did not change which municipalities received investment grants. The municipalities not receiving any investment funds through the HPE never applied for any, those who got their applications rejected reapplied and got approval. The application process could be important for a discussion about anticipation effects. However, it seems

[14]We thank Andreas Fjelltoft and Hodan Adan at the Housing Bank for their help in providing us with this data.

highly unlikely that an elderly person knowing they will soon get a room at a nursing home or in a residential care home experience any change in their hospitalization needs before they move into their new homes.

Through NSD we have received access to the Commune Database. The database comprises statistics on central issues for Norwegian municipalities such as demographics, economy, politics, health, culture, infrastructure, and natural resources. Most of the data is facilitated and delivered by other data distributors, mainly SSB. Most importantly, the database gives us information about hospitalization for the inhabitants in Norwegian municipalities. We look at the development in hospitalization through the number of bed-days and the number of overnight stays. In addition, we use population data from SSB to change our dependent variables of interest from totals to per person measures.

The number of overnight stays tells us something about the frequency of hospital use, while the number of bed-days tells us more about the resource use in hospitals. Using these two measures should give a good overview of hospitalization. Since we are interested in the elderly population, we use four variables: bed-days per person for people aged 67–79, bed-days per person for people aged 80+, overnight stays per person for people aged 67–79 and overnight stays per person aged 80+. We choose to use per person measures to make municipalities more comparable. Norwegian municipalities differ greatly in size, and although we include municipality fixed effects in our analysis, this does not control for migration from small to larger municipalities during the HPE. Per person measures also makes it easier to interpret the results.

There are several other outcome variables that would be interesting to look at, particularly the resource use per person in hospitals. Unfortunately, we do not have access to this data at the municipality level. Nevertheless, using the data we have we can still give some estimates about the resource use.

In addition, we include data on full-time equivalents, municipality resources, and care units accessed through the Commune Database and provided by SSB. We also use data on cancer occurrences collected from the Municipal health statistics bank, which is provided by the Norwegian Institute of Public Health. Lastly, we include data on deaths collected from the Commune Database for the years 1994–2001 and provided by SSB. For the years

2002–2007 the data comes from Arkivverket.[15] The data on deaths includes age at death, municipality, and sex. We have combined this with data on the population at every age from the Commune Database provided by SSB to calculate mortality rates. Mortality rates are the number of deaths at a given age divided by the population at the same age.

Sample Restriction

We have a sample of 418 municipalities. However, small municipalities with few inhabitants are often subject to more random variation. In addition, with few elderly treatment effects can be difficult to establish as not all the elderly use LTC services, especially in the age group 67–79. We thus need an appropriate amount of elderly to ensure that we have enough LTC users to get reliable estimates. Therefore, we have chosen to restrict our analysis to only include municipalities with at least 150 people aged 80+. This leaves us with a sample of 271 municipalities.

4.2 Descriptive Statistics

In the period 1994–2007 there has been a slight decrease in the number of bed-days per person 67–79. This can be seen in figure 4.1. For those aged 80+ there has been an increase. For overnight stays there has been an increase in the number of stays per person for both age groups. This indicates that those between 67–79 more often require hospitalization, but each stay is getting shorter. Table 4.1 also confirms this picture. The table show mean, minimum, and maximum values for the outcome variables one year before the HPE and one year after.

Table 4.1: Municipality statistics pre and post HPE - Outcome variables

	Pre HPE 1997			Post HPE 2006		
	Mean	Min	Max	Mean	Min	Max
Beddays pp 67–79	2.60	1.08	4.94	2.53	0.35	9.82
Beddays pp 80+	3.44	0.82	7.45	4.01	0.83	14.09
Overnight stays pp 67–79	0.34	0.15	0.79	0.41	0.08	1.62
Overnight stays pp 80+	0.45	0.12	0.91	0.60	0.13	2.11

Note: This table shows descriptive statistics pre and post HPE for the outcome variables. Bed-days is the number of days spent in hospital during the course of a year. Overnight stays is the number of hospital stays lasting 24-hours or more during a year.

[15] We thank Ketil Rolland Hansen at Arkivverket for providing us with this data.

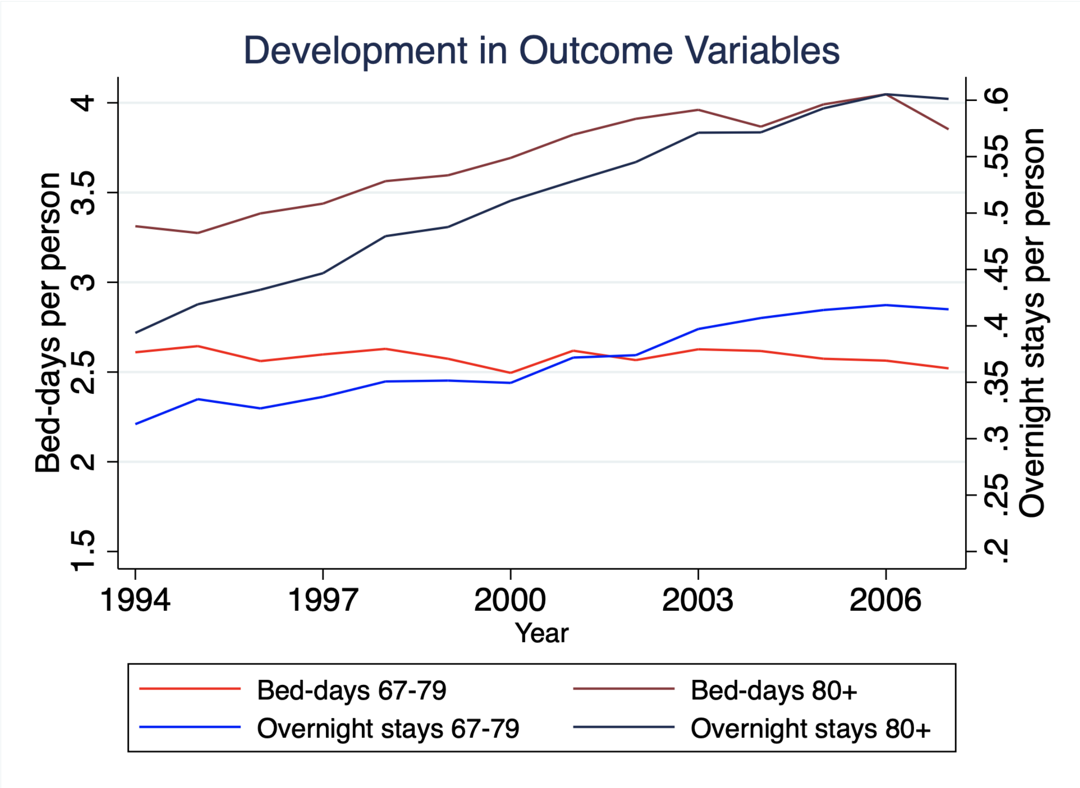


Figure 4.1: Development in hospitalization measured by bed-days and overnights stays 1994–2007

During the course of the HPE, the coverage rate increased from 27.5 percent to 35.7 percent, as shown in table 4.2. This differs from the rates presented in section 3.1 because we calculate a rate for each municipality while the estimate presented in section 3.1 was across the entire country. Our numbers confirm that the HPE helped raise minimum standards as the minimum coverage rate increased, but not all municipalities were able to reach a 25 percent coverage rate. Table 4.2 also show that the number of full-time equivalents per person increased during the HPE, and most notably the minimum value has increased strongly. Demographics have been relatively stable for both age groups during the HPE.

Table 4.2: Municipality statistics pre and post HPE - Independent variables

	Pre HPE 1997			Post HPE 2006		
	Mean	Min	Max	Mean	Min	Max
Coverage rate	0.275	0.000	1.261	0.357	0.137	1.411
Full-time equivalents pp 67–79	0.20	0.11	0.67	0.33	0.14	1.22
Full-time equivalents pp 80+	0.48	0.28	1.42	0.60	0.29	2.00
Population share 67–79	0.11	0.05	0.19	0.10	0.04	0.16
Population share 80+	0.05	0.01	0.09	0.06	0.02	0.10

Note: This table shows descriptive statistics for coverage rate, the number of full-time equivalents in the nursing and care sector, and the share of elderly in the population, pre and post reform.

Of the 418 municipalities, 410 took part in the HPE, but their treatment uptake differed. For those taking part the average municipality invested in 0.106 units per person 67–79 and 0.25 units per person over 80. This is shown in table 4.3. The municipality with the largest treatment uptake built 1.17 units per person over 80. There is also great variation in the number of new full-time equivalents. While some municipalities have had a decrease in the number of full-time equivalents, most have seen an increase. The two bottom rows part of table 4.3 gives an overview over when municipalities first applied for investment funds through the HPE and when they first received these. The first municipality applied in early January 1998, while there was one municipality that did not apply until October 2003. The first project was completed in January 1998. The last grants were handed out in December 2007.

Table 4.3: Treatment statistics

	Mean	Min	Max
Units pp 67–79	0.106	0.006	0.491
Units pp 80+	0.250	0.014	1.174
Increase in workers pp 67–79	0.086	-0.216	1.005
Increase in workers pp 80+	0.207	-0.582	1.890
First application	5/02/1999	1/08/1998	10/01/2003
First payout	12/22/2000	1/16/1998	12/14/2007

Note: This table shows treatment uptake in all municipalities. The first two rows it shows the number of new units per person over 67 and 80 for the municipalities taking part in the HPE. Units per person is based on population size pre HPE (1997). Row 3–4 show increase in full-time equivalents per person between 1997 and 2005. Lastly, row 5–6 show when municipalities first applied for investment funds and when they first received these.

5 Empirical Strategy

5.1 Event Study

In this thesis, we want to examine if there is a causal connection between investments in LTC and hospitalization among the elderly. To find the true causal impact, it would be necessary to observe the world in two states; one where municipalities invest more in LTC and one where they do not. Observing a municipality in both states is only possible if a parallel universe exists, or there is a way to reverse time. A more realistic alternative is to implement a randomized trial where half of the municipalities are randomly drawn to take part in a scheme that increases LTC investments, and the other half is not. This experiment is ideal to find causality, but it seems unlikely that policymakers would implement it. To identify the causal effect, we exploit the natural experiment of the HPE, where timing of investment differs across municipalities.

We use an event study methodology that utilizes the fact that municipalities received funding for investments in nursing homes and residential care homes through the HPE at different points in time. An event study is an extension of the more known differences-in-differences method. It has commonly been applied in finance to measure abnormal stock returns, but in later years the technique has gained merits in quasi-experimental applications in economics.[16] Event study works, in our case, by letting municipalities that have not yet received treatment, or never receive treatment, act as counterfactuals for those already treated. Identification is based on municipalities obtaining similar effects a given number of years after they each received treatment. We index time relative to when treatment occurs.

In recent years, several significant contributions have been made on correct estimation and inference of event studies in economics; see for example Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfæuille (2020), and Sun and Abraham (2020). New insight is being found on how to deal with important limitations such as underidentification and heteroskedasticity. Kirill Borusyak, Xavier Jaravel, and Jann Spiess (2021), make an important contribution by deriving an efficient estimator which is robust to treatment

[16] Event studies in finance usually exploit time-series data, while the economics version uses panel data. Thus, the two applications have different properties.

effect heterogeneity, as well as several other attractive features. We follow the method applied in their paper.

We start by describing a standard event study setup following the notation and terminology of Borusyak et al. (2021).[17] The objective is to estimate the causal effect of a binary treatment D_{it} on an outcome Y_{it}, using a panel with i units observed over t periods. Each unit i receives treatment at a unit specific time E_i, and stays treated thereafter: $D_{it} = \mathbf{1}[K_{it} \geq 0]$. Where $K_{it} = t - E_i$ is the number of periods since treatment took place. Never-treated units are denoted $E_i = \infty$. The conventional event study is implemented using two-way fixed effect (TWFE) regression. An example of such a specification is:

$$Y_{it} = \tilde{\alpha}_i + \tilde{\beta}_t + \sum_{\substack{h=-a \\ h\neq -1}}^{b-1} \tau_h \mathbf{1}[K_{it} = h] + \tau_{b+}\mathbf{1}[K_{it} \geq b] + \tilde{\varepsilon}_{it} \tag{5.1}$$

The unit and period fixed effects are represented by $\tilde{\alpha}_i$ and $\tilde{\beta}_t$, and $\tilde{\varepsilon}_{it}$ is the error term. The model follows a dynamic structure, including leads and lags that allow treatment effects to differ over a given horizon h. The number of included leads and lags are $a \geq 0$ and $b \geq 0$. The first lead is dropped as a normalization. τ is the treatment effect, and this model features binning of endpoints when τ_{b+}, meaning that treatment effects at longer horizons are binned.

While there is much to learn form a model such as the one above, it requires strong assumptions to yield a causal effect when using a TWFE estimation method. For example, it imposes a restricted model for causal effects which can lead to spurious identification in a dynamic structure. Several other limitations are discussed in Borusyak et al. (2021). To avoid these problems, we instead use imputation-based estimation as proposed by Borusyak et al. The regression model is still specified as equation (5.1), but the estimation method is different. In particular, the imputation-based method works by fitting the unit, $\hat{\alpha}_i$, and time, $\hat{\beta}_t$, fixed effects using untreated observations only, these are then used to obtain the untreated potential outcomes which are used to estimate a treatment effect for the treated observations: $\hat{\tau}_{it} = Y_{it} - \hat{\alpha}_i - \hat{\beta}_t$.[18] Lastly, the estimated treatment effects are weighed into an average in line with the target estimator.

[17]There is great heterogeneity in implementation of event study designs, thus the model we specify here might differ from other models within event study.

[18]Untreated observations include all pre-treatment periods in addition to never-treated units.

5.2 Regression Model for the Impact of more LTC on Hospitalization

As a starting point, we will follow the model described in section 5.1 to estimate the impact of more LTC on hospitalization among the elderly. All models are estimated for each of our outcome variables. We group all N observations, $it \in \Omega$, into treated $\Omega_1 = \{it \in \Omega : D_{it} = 1\}$ observations of size N_1, and untreated $\Omega_0 = \{it \in \Omega : D_{it} = 0\}$ observations of size N_0. The period-t potential outcome for unit i if it is never treated is denoted $Y_{it}(0)$. The model we want to estimate is:

$$Y_{it} = \widetilde{Mun}_i + \widetilde{Year}_t + \sum \tau_h \mathbf{1}[K_{it} = h] + \tilde{\varepsilon}_{it}, \tag{5.2}$$

where $\widetilde{Mun}_i$ are municipality fixed effects accounting for unobserved, municipality-level and time-invariant confounders. $\widetilde{Year}_t$ are year fixed effects, and catches year-to-year changes, such as business cycles, which affect all municipalities. To estimate this, we follow three steps. We start by estimating the unit and time fixed effects using the untreated observations:

$$Y_{it}(0) = \widetilde{Mun}_i + \widetilde{Year}_t + \tilde{\varepsilon}_{it} \tag{5.3}$$

Next, these fitted estimates are used to estimate the treatment effects:

$$\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}(0), \quad it \in \Omega_1 \tag{5.4}$$

These treatment effects are then averaged to find an estimate of the average treatment effect on the treated (ATT):

$$\hat{\tau}_w = \sum_{it \in \Omega_1} w_{it} \hat{\tau}_{it} \tag{5.5}$$

We include dynamic treatment effects, as such we want the ATT h years after treatment for a given $h \geq 0 : w_{it} = \mathbf{1}[K_{it} = h]/[\Omega_{1,h}]$ for $\Omega_{1,h} = \{it : K_{it} = h\}$. This implies that we will get h different estimates of the ATT, as well as an estimate for the year when treatment happens. In our analysis we use $h = 5$. We will also sum the treatment effects across h to find the total treatment effect in the five years after treatment. We do not need to omit any periods as a reference point with imputation-based estimation, the treatment effect is the difference between the treated and non-treated.

In this simple setup, treatment is defined as being switched *on* the year a municipality first receive grants. This means that we do not take any considerations regarding the differences in intensity of treatment or other differences between municipalities. Models taking these issues into account will be presented later in the chapter.

5.2.1 Effect Window

An important issue is the choice of the number of leads and lags in the model, the so-called effect window. The effect window will have implications for both inference and validity of the model. Since we are using annual data, a lead or a lag represents a year. Including leads and lags allow the treatment effect to vary over time, that is, the treatment effect can be different right after treatment and three years later. We say that a model is dynamic when it includes leads and lags. The alternative is a static model where we estimate one treatment effect for the whole post-treatment period. As presented above, we use a dynamic model, since we suspect that the effect of LTC on hospitalization will grow stronger over time. This is mainly due to treatment intensity increasing with time, as municipalities invested in several projects during the HPE. In addition, a person moving into a nursing home will perhaps not see a difference in hospital use the first year, but after a while, when the care given is adjusted for that person's needs, the effect might grow stronger.

Choosing the number of leads and lags can be important for the estimation results. Borusyak et al. (2021) make a distinction between leads and pre-treatment estimates. Leads are used when there are anticipatory effects and can be different from zero. Pre-treatment estimates are estimates for periods where we do not suspect treatment effects and should thus be zero. We include three pre-treatment period estimates, but no leads as we do not suspect any anticipatory treatment effects. We could have included more pre-treatment periods, but Borusyak et al. (2021) suggests that using all available periods is not a good idea with a small never-treated group like we have in our data set.

We have included five lags in our model in addition to the treatment period $K_{it} = 0$. At most we could have included nine lags as the first treatments happened in 1998 and our data set spans until 2007. We believe that our chosen number of lags give a good picture of the treatment effects, and effects further into the future are more likely to be biased by other factors. The choice of effect window also has implications for the identification of the model. A particular concern in event studies is underidentification, however, by restricting the treatment effects the problem of underidentification become smaller (Schmidheiny and Siegloch, 2019). The presence of never-treated units also helps with this problem.

5.3 Weighing Estimates

5.3.1 Treatment Intensity

Model 5.2 estimates an ATT across all included horizons. The choice of estimator could be changed by including estimation weights. For example, we could be interested in seeing how the treatment effect depends on the degree of treatment. To do this, we weigh the estimated treatment effects based on treatment intensity. As a measure of treatment intensity, we use the number of new units per person aged 80 and above in each municipality. This means that the treatment effect in municipalities with a high number of units per person are given a larger weight than the treatment effect in municipalities that built fewer units. This is also a form of sensitivity analysis, as stronger treatment should imply stronger treatment effect, and thus weighing based on treatment intensity will give stronger results.

5.3.2 Municipality Size

Another useful estimation weight is municipality size. There are great differences in the population size of Norwegian municipalities. While Oslo has more than half a million inhabitants, Utsira has less than 200. Small municipalities experience more year-to-year random variation than larger municipalities and weighing based on size could give more consistent results. The obvious drawback of this is that we put less weight on potentially useful variation in small municipalities. Since municipality size in terms of population varies greatly, municipalities are divided into seven categories ranging from 1 to 7 based on their population. The largest municipalities are thus seven times more important in the regression than the smallest municipalities. Categorization is based on the total population; the grouping is shown in table A1.1 in the appendix.

5.3.3 Initial Coverage Rate

Municipalities with different service offers before the HPE might react differently to treatment. More specifically, we would think that municipalities with low initial coverage rate experience stronger treatment effects. We calculate the coverage rate for 1997 and categorize municipalities in groups from 1 to 6. Here, 6 represents municipalities with the lowest initial coverage, and 1 those with the highest. How the groups are defined is shown in table A1.2 in the appendix. The categorization is done to make treatment effects in municipalities with the lowest initial coverage count the most.

5.4 Control Variables

Under *ceteris paribus* conditions, our estimates have a causal interpretation. Ceteris paribus means "other things equal", that is, we want to compare municipalities identical in every way except for their partition in the HPE to see how LTC affects hospitalization. However, municipalities are inherently different, they differ in demographics, governance, and many other ways. Control variables are a way to manage such differences and make municipalities comparable. Failure to include sufficient controls can lead to OVB. The bias is caused by the model over- or underestimating the effect of a factor due to the missing variable(s).

Fortunately, the event study methodology can help with the OVB without making it necessary to include many controls. A properly specified event study should not be prone to OVB. We only use control variables to check if our method sufficiently eliminates OVB. We include data on resource use in municipalities and cancer occurrences as controls. Including these in our analysis does not cause any significant changes to our estimated coefficients or standard errors. It thus seems that the event study method can control for unobserved factors in a satisfactory way, and we are not too concerned about OVB.

Another worry is that the way new LTC units affect hospitalization depend on the level of care provided. However, since the investment grants in the HPE was paired with operational grants municipalities were made able to provide adequate care. As mentioned in section 4.2, the number of full-time equivalents per person increased during the HPE, showing that municipalities should have the necessary staff available.

5.5 Identifying Assumptions

To identify τ_w Borusyak et al. (2021) consider four assumptions: the general model for untreated observations, no anticipation effects, a model for causal effects, and homoscedastic errors. Borusyak et al. (2021) define the first assumption as this: "For all $it \in \Omega$, $Y_{it}(0) = A_{it}'\lambda_i + X_{it}'\delta + \varepsilon_{it}$, where λ_i is a vector of unit-specific nuisance parameters, A_{it} and X_{it} are known non-stochastic vectors, and $E[\varepsilon_{it}] = 0$". Together with the no anticipation effects assumption: $Y_{it} = Y_{it}(0)$ for all $it \in \Omega$, the general model assumption can be tested using pre-treatment data. The assumptions are fulfilled if pre-treatment estimates are zero. It is possible to relax the no anticipatory effect assumption, but it is not necessary in our case as we do not believe there to be any.

The third assumption imposes a model for causal effects. However, as a "null model" no restrictions are imposed. This makes sense when the treatment effect structure is *ex ante* unknown (Borusyak et al., 2021). However, if economic theory suggests restrictions to treatment effects, they should be included to increase estimation power. We do not include restrictions to treatment effect heterogeneity as we do not have any indications that we should do so. In addition, Borusyak et al. (2021) include a homoscedastic error assumption for efficient estimation. This is a strong assumption, but it can be relaxed. As a standard when working with panel data clustering of standard errors is used to help fulfil this assumption.

6 Empirical Analysis

In this section we present the results of our empirical analysis, discuss robustness checks, limitations, and the implications of our results.

Overall, we find that the HPE has led to a substantial increase in hospitalization, both in terms of number of bed-days and overnight stays per person for those aged 80+. For those aged 67–79 the results are more mixed, but there seem to be a slight increase. Using four different estimators we find that our results are robust.

6.1 Bed-days 67–79

Figure 6.1 shows the result of estimating equation (5.2) using bed-days per person 67–79 as our outcome variable. It is an event study plot showing the point estimates and a 95 percent confidence interval, and it suggests a slight increase in the number of bed-days the first years after treatment. Five years after treatment the effect is negative. The treatment effect estimator is the ATT. To identify a causal effect in our case the key is pre-trends not statistically different from zero. Looking at the confidence interval we see that none of the pre-treatment point estimates are statistically different from zero using a 5 percent significance level, but they do show a slight downward trend which we should keep in mind.

To learn more about the estimates we look at table 6.1. Column (1) shows the estimates from figure 6.1. Column (2) weighs treatment effects based on treatment intensity. Column (3) weighs treatment effects by municipality size. Column (4) uses weights based on coverage rate before treatment. Event study plots for column (2)–(4) are presented in figure A2.1 in the appendix, all models have similar pre-treatment estimates. In column (1) we see that none of the point estimates are statistically significant if we use a 10 percent significance level. The same goes for the models presented in the other columns. Looking at the summed treatment effect, the total effect when all lags are summed up, column (1) suggests that treatment increased bed-days with 0.05 days per person. As the average municipality had a pre-treatment mean of 2.59 bed-days per person this corresponds to a 2 percent increase over the 6-year period.

Column (2) suggests a stronger treatment effect with an increase of 0.21, or 8.1 percent. Implying that higher treatment intensity strengthens the treatment effect. This result helps give credibility to our analysis, showing that treatment does matter for hospitalization. Column (3) on the other hand shows a decrease in bed-days of 0.06, suggesting that larger municipalities perhaps react differently to treatment. This could be partly explained by larger municipalities having weaker treatment intensity. Column (4) suggests an increase almost identical to column (1), implying that the initial coverage rate does not matter for how municipalities are affected by treatment. This also gives credibility to our method, as it looks like we can separate the treatment effect from the inherent differences between municipalities.

As mentioned, none of the results are statistically significant so we cannot trust the point estimates. The pre-treatment estimates show a downward trend but are not different from zero. The estimates imply that treatment is associated with a weak increase in the number of bed-days. This is somewhat surprising as we were expecting to see a decrease, still we cannot state that there was an increase due to the high standard errors. However, we have reason to rule out that more LTC cause a strong reduction in bed-days for those aged 67–79.

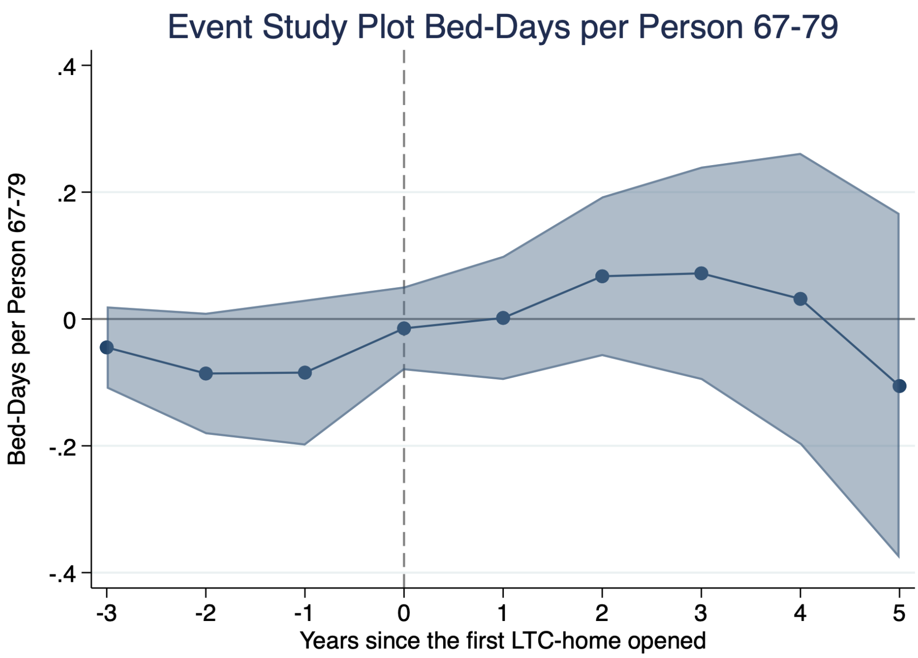


Figure 6.1: Event study plot with bed-days per person aged 67–79 as outcome

Table 6.1: Regressions for the effect of more LTC on number of bed-days per person in age group 67–79

	(1) Simple	(2) With TI	(3) With Mun Size	(4) With 1997 CR
Years after treatment:				
Year 0	-0.015	-0.000	-0.028	-0.022
	(0.034)	(0.035)	(0.032)	(0.034)
Year 1	0.002	0.017	-0.022	-0.006
	(0.050)	(0.053)	(0.047)	(0.051)
Year 2	0.067	0.099	0.048	0.069
	(0.064)	(0.067)	(0.061)	(0.065)
Year 3	0.072	0.116	0.068	0.063
	(0.086)	(0.092)	(0.081)	(0.085)
Year 4	0.032	0.057	0.006	0.036
	(0.117)	(0.122)	(0.113)	(0.120)
Year 5	-0.106	-0.083	-0.137	-0.090
	(0.139)	(0.140)	(0.139)	(0.141)
Sum TE	0.052	0.205	-0.064	0.049
	(0.431)	(0.442)	(0.408)	(0.434)
Pre-treatment mean	2.586	2.586	2.586	2.586
N	3382	3382	3382	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of bed-days per person 67–79. Column 1 is a simple imputation, column 2 includes weighting of results based on treatment intensity. Column 3 weighs result based on municipality size. Column 4 weighs results based on coverage rate in 1997, where municipalities with initial low coverage are given larger weight. Sum TE shows the sum of treatment effects (sum of the lags). Pre-treatment mean is the average number of bed-days per person 67–79 in year 0. All standard errors are clustered at the municipality level.

6.2 Bed-days 80+

In figure 6.2 bed-days per person 80+ is shown to increase progressively in the years after treatment. This increase is statistically significant at a 5 percent level 3–5 years after treatment. The pre-treatment estimates are close to zero and give credibility to this result. In table 6.2, column (1), the summed treatment effect is estimated to be an increase of 2.01 bed-days per person, indicating a 51 percent increase compared to the pre-treatment mean. This result is statistically significant at a 1 percent level. Column (2), (3) and (4) show a similar picture, and their event study plots are shown in figure A2.2 in the appendix. The summed treatment effect is a bit stronger in column (2), suggesting an increase of 60 percent. Column (3) shows a slightly weaker increase of 42 percent, and the estimates in column (4) are very similar to those in column (1). Treatment effects thus seem stronger with higher treatment intensity, and weaker for large municipalities. All models show increasing treatment effects as we move further away from the treatment date. This is reasonable as treatment is based on the first nursing home or residential care home the municipality opened through the HPE, but most municipalities opened several in the years after as well, such that treatment intensity increases over time.

With the satisfactory pre-treatment estimates and the high statistical significance we have good reason to trust these estimates. We can rule out treatment leading to a decrease in the number of bed-days for those aged 80+.

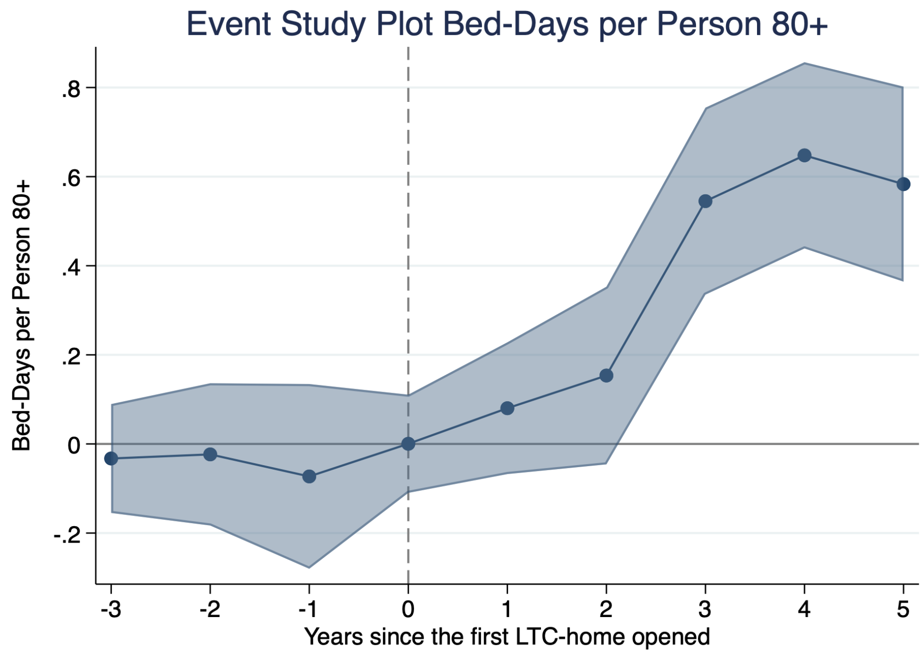


Figure 6.2: Event study plot with bed-days per person aged 80+ as outcome

Table 6.2: Regressions for the effect of more LTC on number of bed-days in age group 80+

	(1) Simple	(2) With TI	(3) With Mun Size	(4) With 1997 CR
Years after treatment:				
Year 0	0.000	0.043	-0.022	-0.010
	(0.056)	(0.060)	(0.056)	(0.056)
Year 1	0.080	0.138*	0.000	0.056
	(0.075)	(0.083)	(0.076)	(0.076)
Year 2	0.154	0.203*	0.054	0.127
	(0.102)	(0.115)	(0.103)	(0.104)
Year 3	0.545***	0.626***	0.490***	0.525***
	(0.107)	(0.118)	(0.111)	(0.108)
Year 4	0.648***	0.714***	0.602***	0.644***
	(0.107)	(0.128)	(0.108)	(0.111)
Year 5	0.583***	0.659***	0.509***	0.575***
	(0.112)	(0.108)	(0.091)	(0.123)
Sum TE	2.010***	2.382***	1.634***	1.917***
	(0.443)	(0.490)	(0.433)	(0.453)
Pre-treatment mean	3.915	3.915	3.915	3.915
N	3382	3382	3382	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of bed-days per person 80+. Column 1 is a simple imputation, column 2 includes weighting of results based on treatment intensity. Column 3 weighs result based on municipality size. Column 4 weighs results based on coverage rate in 1997, where municipalities with initial low coverage are given larger weight. Sum TE shows the sum of treatment effects (sum of the lags). Pre-treatment mean is the average number of bed-days per person 80+ in year 0. All standard errors are clustered at the municipality level.

6.3 Overnight Stays 67–79

Figure 6.3 shows quite similar results as figure 6.1. Using overnight stays per person between 67–79 as the outcome variable the figure suggest a weak and insignificant increase in the number of stays after treatment. The pre-treatment estimates are slightly negative but not different from zero. Looking at table 6.3 we see no significant point estimates in any of the columns. The summed treatment effect is positive for all models. In column (1) the estimated treatment effect is 0.02, corresponding to a 5.5 percent increase over six years in the number of stays compared to the pre-treatment mean. As in the previous cases, the effect is slightly stronger in column (2) compared to column (1), weaker in column (3), and very similar in column (4). Event study plots for the models in column (2)–(4) are shown in figure A2.3 in the appendix.

We have a hard time drawing conclusions from these result as they are not statistically significant, the estimated effect is also small in magnitude, suggesting only a slight increase. The downward trend in the pre-treatment estimates must also be considered. Just like in section 6.1 we can only conclude that we do not find a strong decrease in the number of stays. Our estimates suggest a modest increase, but we cannot be sure as they are not statistically different from zero.

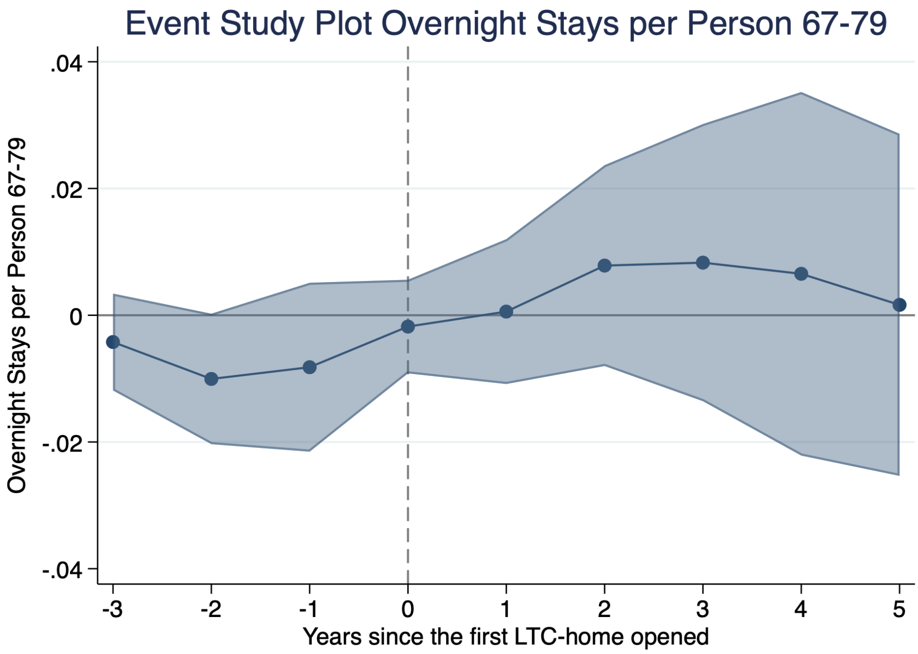


Figure 6.3: Event study plot with overnight stays per person aged 67–79 as outcome

Table 6.3: Regressions for the effect of more LTC on number of overnight stays per person in age group 67–79

	(1) Simple	(2) With TI	(3) With Mun Size	(4) With 1997 CR
Years after treatment:				
Year 0	-0.002	-0.000	-0.003	-0.002
	(0.004)	(0.004)	(0.003)	(0.004)
Year 1	0.001	0.003	-0.001	-0.000
	(0.006)	(0.006)	(0.006)	(0.006)
Year 2	0.008	0.010	0.008	0.008
	(0.008)	(0.009)	(0.008)	(0.008)
Year 3	0.008	0.011	0.008	0.008
	(0.011)	(0.012)	(0.011)	(0.011)
Year 4	0.007	0.010	0.003	0.007
	(0.015)	(0.016)	(0.015)	(0.015)
Year 5	0.002	0.003	-0.004	0.004
	(0.014)	(0.014)	(0.014)	(0.014)
Sum TE	0.023	0.037	0.010	0.024
	(0.051)	(0.054)	(0.049)	(0.051)
Pre-treatment mean	0.358	0.358	0.358	0.358
N	3382	3382	3382	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of overnight stays per person 67–79. Column 1 is a simple imputation, column 2 includes weighting of results based on treatment intensity. Column 3 weighs result based on municipality size. Column 4 weighs results based on coverage rate in 1997, where municipalities with initial low coverage are given larger weight. Sum TE shows the sum of treatment effects (sum of the lags). Pre-treatment mean is the average number of overnight stays per person 67–79 in year 0. All standard errors are clustered at the municipality level.

6.4 Overnight Stays 80+

Lastly, figure 6.4 shows estimates using the number of overnight stays per person 80+ as the outcome variable. The figure is quite similar to figure 6.2 with pre-treatment estimates very close to zero and a strong and progressive increase in the number of stays after treatment. Looking at table 6.4, column (1), the average number of overnight stays per person for those 80+ increased by 0.32 stays for municipalities taking part in the HPE. This corresponds to a 62 percent increase over six years as the average municipality had 0.52 overnight stays per person aged 80+ before treatment. This result is significant at a 1 percent level. In column (2), (3), and (4) the estimates are similar, with the same slightly stronger result in column (2), weaker in column (3) and similar in (4) as we have seen with the other outcomes. All summed treatment effects are statistically significant at a 1 percent level. Event study plots for column (2)–(4) are shown in figure A2.4 in the appendix.

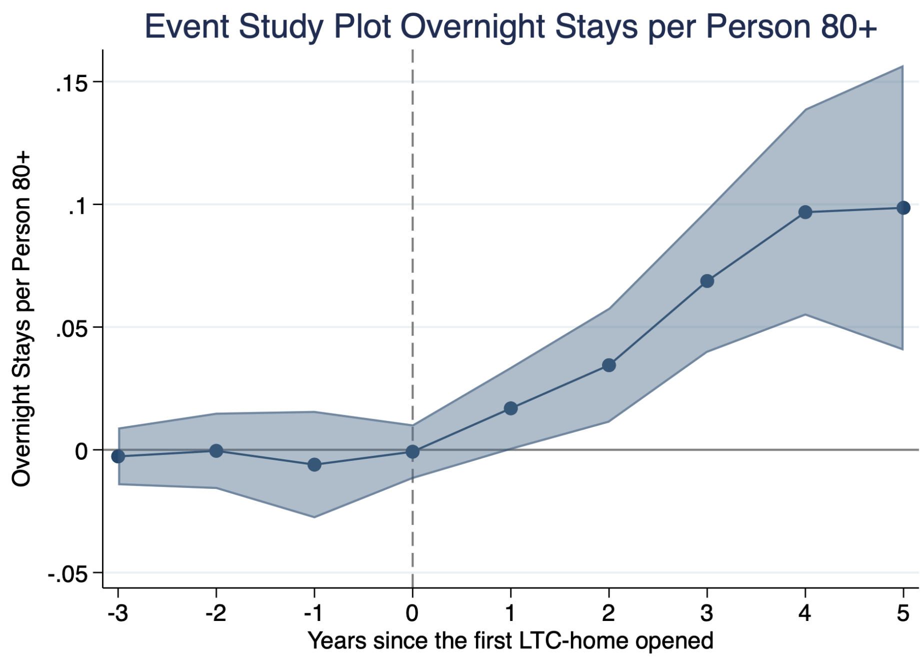


Figure 6.4: Event study plot with overnight stays per person aged 80+ as outcome

Table 6.4: Regressions for the effect of more LTC on number of overnight stays per person in age group 80+

	(1) Simple	(2) With TI	(3) With Mun Size	(4) With 1997 CR
Years after treatment:				
Year 0	-0.001	0.001	-0.001	-0.002
	(0.006)	(0.006)	(0.006)	(0.006)
Year 1	0.017*	0.022**	0.011	0.015*
	(0.009)	(0.009)	(0.008)	(0.009)
Year 2	0.034***	0.037***	0.029**	0.033***
	(0.012)	(0.013)	(0.012)	(0.012)
Year 3	0.069***	0.075***	0.065***	0.068***
	(0.015)	(0.017)	(0.014)	(0.015)
Year 4	0.097***	0.108***	0.094***	0.097***
	(0.022)	(0.024)	(0.020)	(0.022)
Year 5	0.099***	0.105***	0.091***	0.101***
	(0.030)	(0.029)	(0.028)	(0.031)
Sum TE	0.315***	0.348***	0.289***	0.312***
	(0.079)	(0.082)	(0.072)	(0.079)
Pre-treatment mean	0.519	0.519	0.519	0.519
N	3382	3382	3382	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of overnight stays per person 80+. Column 1 is a simple imputation, column 2 includes weighting of results based on treatment intensity. Column 3 weighs result based on municipality size. Column 4 weighs results based on coverage rate in 1997, where municipalities with initial low coverage are given larger weight. Sum TE shows the sum of treatment effects (sum of the lags). Pre-treatment mean is the average number of overnight stays per person 80+ in year 0. All standard errors are clustered at the municipality level.

6.5 Discussion of Results

The literature review in chapter 2 suggests that more LTC should reduce the use of hospitals. For those aged 80+ our estimates show a strong increase in hospitalization in the years following treatment through the HPE, both in terms of the number of bed-days and the number of stays. This indicates that more LTC do not cause a reduction in hospital use as expected, quite the opposite. The results differ slightly across the models with different weights, showing that the estimates are robust across the four estimators. As such, and for simplicity, we prefer the estimates from the simple model with no weights. Below we present three possible explanations for these surprising results, and then we use data to try and see if these explanations are plausible.

Theories

Firstly, our result could be caused by changes in longevity. As suggested by the literature LTC has potential health benefits and could even reduce the death rate. The care provided in LTC might cause users to live longer as they are enabled to live with more complex clinical profiles. In addition, increased longevity increase hospitalization by default as people require more care when they live longer.[19] If this is paired with more complex clinical profiles it could further increase hospital use.

Secondly, it could be that the threshold for sending users of LTC facilities to hospital is low. If this is the case more LTC would automatically increase hospitalization. The close follow-up from care staff can see users sent to hospital earlier and for smaller issues. This in turn could be connected to an increase in longevity, as early detection of e.g. cancer could make the cancer treatable instead of deadly. Treatable diseases, in most cases, require more hospitalization than deadly diseases.

Thirdly, increased LTC could affect moving patterns and thus change where the hospital use is registered. Higher availability of LTC may keep elderly in their home municipality instead of moving to other municipalities to access LTC. This would mean that those now staying in their home municipality are registered there when accessing hospitals, making the hospital use increase in that municipality. While we do use per person outcome

[19] Given that an increase in longevity partly cause an increase in the number of sick years in line with a dynamic equilibrium hypothesis, see Ministry of Finance (2017).

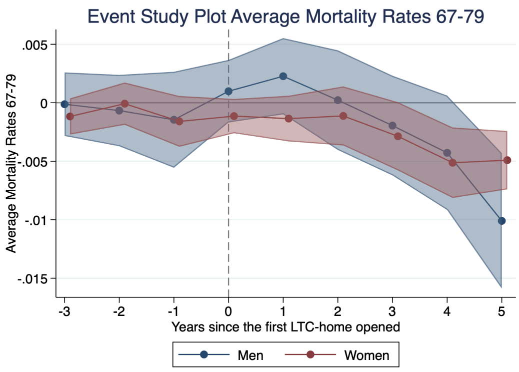
variables which should control for this, there could be differences in where the hospital use is registered causing parts of the increase in hospital use per person we find.

In addition to these suggestions on why more LTC increase hospitalization, there are several factors affecting the strength of this result. As shown in section 6.1–6.4 the treatment intensity and municipality size influence the estimates. The increase in hospitalization is stronger with high degrees of treatment, and for smaller municipalities. We have also done some simple analyses showing that municipalities with a high share of people receiving disability benefits see a stronger increase in hospitalization. The same goes for municipalities with more general practitioners than the average municipality. This might seem contradictory, but it is important to bear in mind that general practitioners are responsible for most hospital referrals. We also see a slight increase in bed-days for municipalities located far away from a hospital, suggesting that the long distance keep patients in hospital longer.

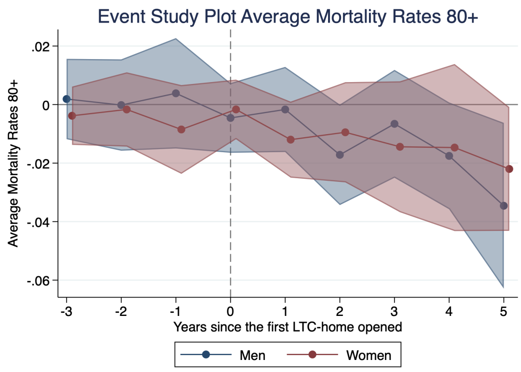
Taking theory to data

To check if longevity increases with more LTC we estimate equation (5.2) using mortality rates as outcome. More specifically we use average mortality rates 67–79 and average mortality rates 80+ for men and women. The average mortality rate 67–79 is the average likelihood to die a given year when you are aged between 67–79. The estimates suggest that more LTC reduces morality rates. The event study plots are shown in figure 6.5 (a) and (b). For men 67–79 the reduction is not statistically significant, this can be seen in table A3.1 in the appendix. For women 67–79 the reduction is significant at a 1 percent level, with an average treatment effect of -0.003, corresponding to a decrease of almost 14 percent. For men 80+ the average treatment effect is -0.014, suggesting a decrease of 7 percent, this result is significant at a 5 percent level. Lastly, the estimates suggest a decrease of 6 percent for women 80+, this result is almost statistically significant at a 10 percent level with a p-value of 0.107. The pre-treatment effects shown in figure 6.5 move a bit up and down but are not different from zero. To sum up our estimates are a bit noisy but suggest that more LTC might have reduce mortality, this is in line with the research from the United Kingdom presented earlier.

The decrease in mortality is an interesting result, however, we do not believe that the estimated increase in hospitalization can be explained by this alone. It could be working together with a low threshold for sending LTC users to hospital, but we unable to test this using the data we have available. What we do know, is that in later years there has been increased attention on cooperation between LTC and hospitals to reduce unnecessary hospitalization through the Coordination Reform of 2012 (Ministry of Health and Care Services, 2009). There has also been established interim solutions like municipal emergency day care units which became mandatory for all municipalities in 2016 (Faiz, 2019).[20] This indicates that too frequent hospitalization of LTC users is a concern for policy makers.



(a) Men and women 67–79



(b) Men and women 80+

Figure 6.5: Event study plot average mortality rates

Lastly, we investigate the issue of moving patterns. We use the number of inhabitants aged 80+ as the outcome variable in model (5.2) to see if this number was affected by treatment through the HPE. The event study plot is shown in figure 6.6, and indicates that the number of elderly in municipalities taking part in the HPE increased after treatment. This is confirmed in table A4.1 in the appendix. However, we also see that the pre-treatment effects are positive and different from zero so we cannot trust these estimates. Our model seem to be unable to separate between the general increase in the number of elderly and treatment effects. Despite this, it is an indication that moving patterns could play a part in increasing hospitalization, but further analysis is needed to conclude on this.

[20] In Norwegian: kommunal akutt døgnenhet (KAD).

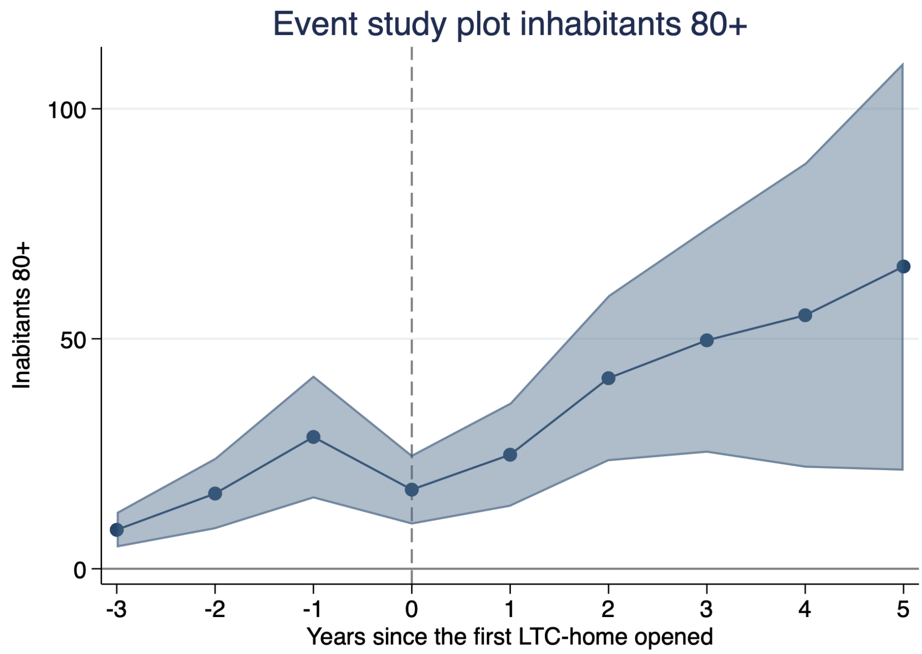


Figure 6.6: Event study plot inhabitants 80+

Conclusion

The surprising increase in hospitalization can partly be explained by a reduction in mortality rates, though we must be careful to conclude as the estimates are a bit noisy. It seems from our analyses that elderly in LTC have less likelihood of dying, perhaps because they receive care enabling them to live with more complex clinical profiles. This in turn could increase hospital use among the elderly as their diseases frequently require more specialized medical attention than what LTC provides. The effect is perhaps made stronger by a low threshold for sending LTC users to hospital. In addition, there could be moving pattern effects, but we are not able to confirm this with our data.

6.6 Robustness Checks

In addition to checking significance and pre-treatment effects for the estimates in section 6.1–6.4 we also need to check how sensitive they are to changes in the model and sample. We do this to check if our results rely critically on some assumptions and restrictions, or if they are similar across several environments. When doing the robustness checks we compare our estimates to the simple imputation model from column (1) in table 6.1–6.4 as they are our preferred estimates.

6.6.1 Changing Method and Assumption

As previously mentioned, event studies have seen several important contributions in the later years when it comes to correct estimation and inference. Different papers use different estimation methods, with different assumptions. A good way to test the sensitivity of our results is thus to see how they are affected by changes in method. Using the methods proposed by Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfæuille (2020), Sun and Abraham (2020), as well as a TWFE ordinary least squares (OLS) method we see how these estimates compare to ours that are based on Borusyak et al. (2021). The set-up we use to compared the different methods is provided by Borusyak et al. (2021).

Figure A5.1–A5.3 in the appendix show estimates for three of our outcome variables using these five different methods. The estimates for overnight stays per person 80+ is shown here in figure 6.7 as an example. All four figures show that the imputation-based estimates following Borusyak et al. are quite similar to de Chaisemartin and D'Haultfœuille, and Callaway and Sant'Anna, while Sun and Abraham, and the TWFE OLS give weaker results. This sensitivity analysis gives us reason to believe that our results are robust across several models.

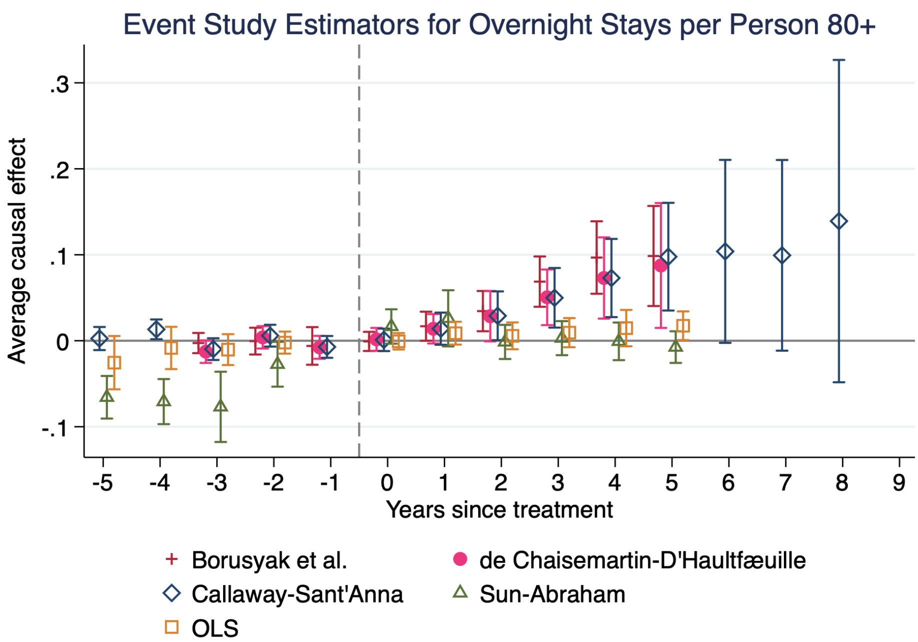
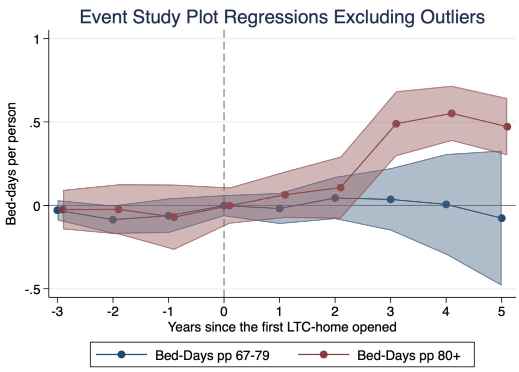


Figure 6.7: Sensitivity analysis using different event study estimators and specifications for the outcome overnight stays per person 80+

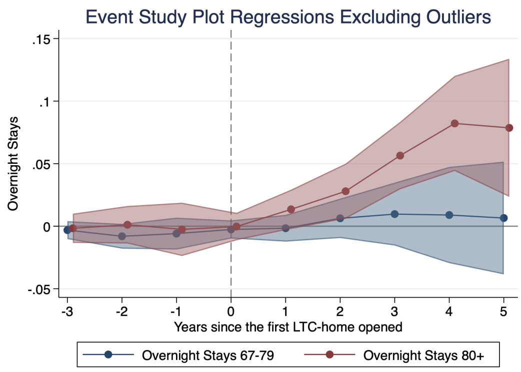
6.6.2 Excluding Outliers

We also need to check if our results are driven by a few extreme outliers. To do this we exclude observations smaller than the 1 percentile and larger than the 99 percentile for all four outcome variables. Results of the exclusion using equation (5.2) on all four outcome variables are shown in figure 6.8. Estimates are also shown in table A5.1 in the appendix. For bed-days per person 67–79 the estimates excluding outliers are now slightly negative, this is concerning compared to the positive result in section 6.1. However, the result is not statistically significant in any of the cases, and column (3) in table 6.1 also showed a negative treatment effect, so we are not to worried by these results. For bed-days per person 80+ excluding outliers strengthens the significance of the results but weakens the magnitude. In section 6.2 the total treatment effect was 52 percent, here it is 43 percent. For overnight stays 67–79 the estimates are similar to those in section 6.3, both in terms of magnitude and significance. With overnight stays per person 80+ the effect of excluding outliers is slightly weaker treatment effects, in section 6.4 it was 62 percent, here it is 50 percent. The pre-treatment effects for all outcomes are satisfactory.

Excluding outliers have some effects on our results, and for our most important results, bed-days per person 80+, and overnight stays 80+ treatment effects are slightly reduced. This indicates that extreme observations might cause a slight upward bias, but the main conclusion of a strong increase in hospitalization is still valid. We conclude that our results are adequately robust to extreme observations.



(a) Bed-days



(b) Overnight Stays

Figure 6.8: Event study plot excluding outliers

6.6.3 Restricting the Sample Differently

Our analysis restricts the sample to only include municipalities with at least 150 inhabitants aged 80+. In figure 6.9 and 6.10 we show how sensitive our estimates are to changes in this number. We include our original estimates with at least 150 municipalities, in addition to at least 100, at least 200, no restrictions, as well as estimates using only municipalities with less than 150 inhabitants 80+. The results are also shown in table A5.2–A5.5 in the appendix.

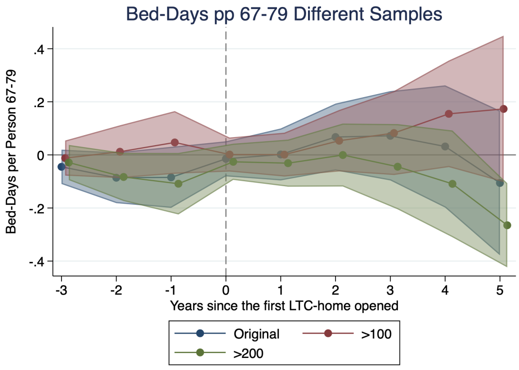
Compared to the original estimates, bed-days per person 67–79 shows a stronger positive increase when the sample is >100, and a slightly negative result when the sample is >200. With no restrictions the negative effect is stronger, and even more so if we only look at observations <150. This suggests that small municipalities and large municipalities are more prone to experience a decrease in hospitalization with treatment, while middle sized municipalities experience an increase. However, except for one estimate in year 5 none of the point estimates are statistically significant as shown in table A5.2.

For bed-days 80+, results are weaker, but still positive, when the sample is >100 and >200. With no restrictions it turns negative, and with <150 it is still negative, and stronger than with no restrictions. A few point estimates are statically significant, but for the summed treatment effect only the original estimates and >100 are statistically significant, where >100 is only significant at a 10 percent level.

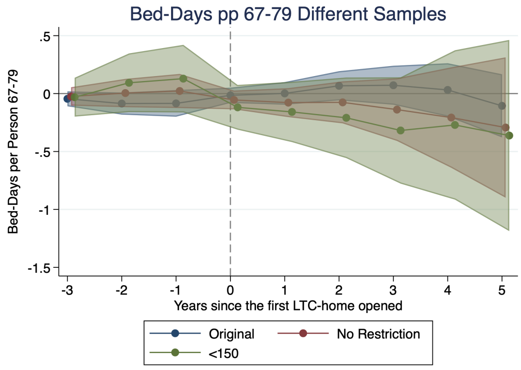
Looking at overnight stays 67–79 the differences when we change the restrictions are like those for bed-days 67–79. Only one of the yearly point estimates shown in table A5.4 is statistically significant.

Overnight stays 80+ shows slightly weaker but still positive and highly significant estimates when the sample is restricted to >100 and >200. With no restrictions the estimated summed treatment effect is positive but weaker and not statistically significant. Two of the yearly estimates are statistically significant. Looking at <150 we once again get a negative estimate, but very small in magnitude and not significant.

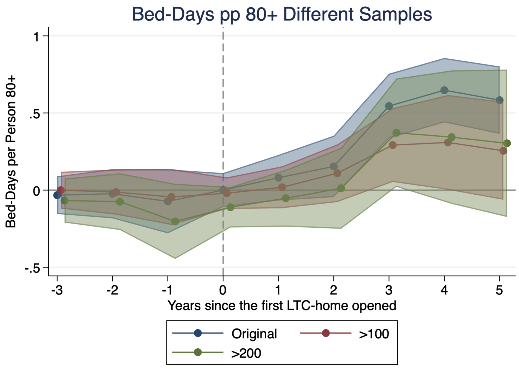
For all outcomes, small municipalities and large municipalities seem to react differently to treatment compared to middle sized municipalities. For the large municipalities this is not very surprising as the lower treatment intensity reduce treatment effects in these. On the



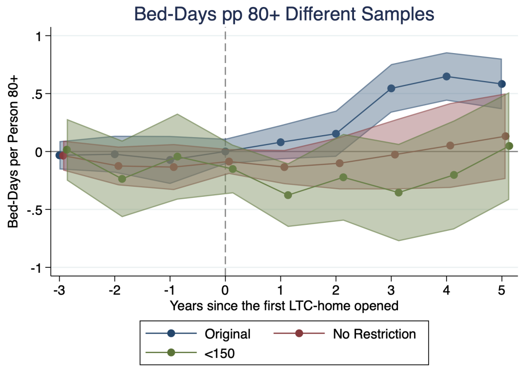
(a) Bed-days 67–79



(b) Bed-days 67–79



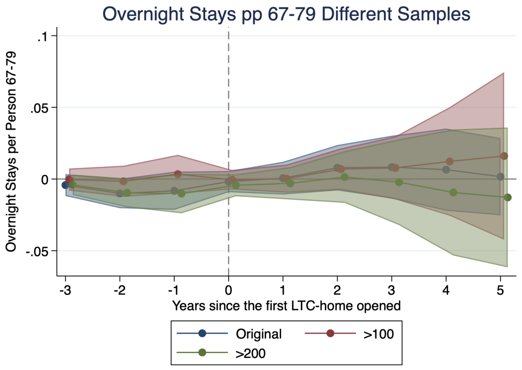
(c) Bed-days 80+



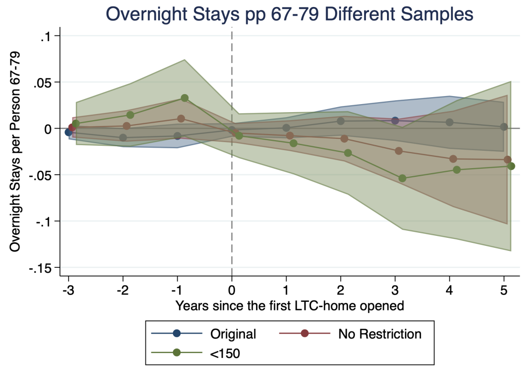
(d) Bed-days 80+

Figure 6.9: Event study plot bed-days different samples

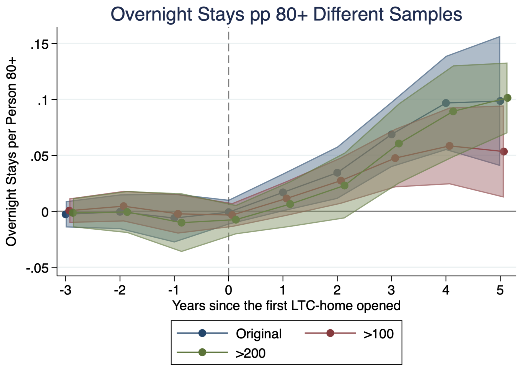
other hand, the small municipalities are worrisome as they seem to reduce hospital use with treatment, contrary to the effect in our original estimates. The main argument for restricting the sample to exclude small municipalities was to reduce the effect of potential random variation in these. It is a problem that their variation does not seem to be entirely random. Except for one yearly estimate, none of the point estimates are statistically different from zero for all outcomes with sample <150, however, all indicate a negative trend. We thus cannot conclude that it is all due to random variation, but we cannot reject it either. A possible explanation for the negative treatment effect could be initial coverage rate, which we will look at next.



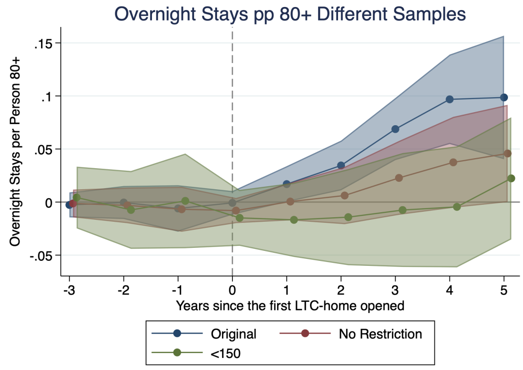
(a) Overnight Stays 67–79



(b) Overnight Stays 67–79



(c) Overnight Stays 80+



(d) Overnight Stays 80+

Figure 6.10: Event study plot overnight stays different samples

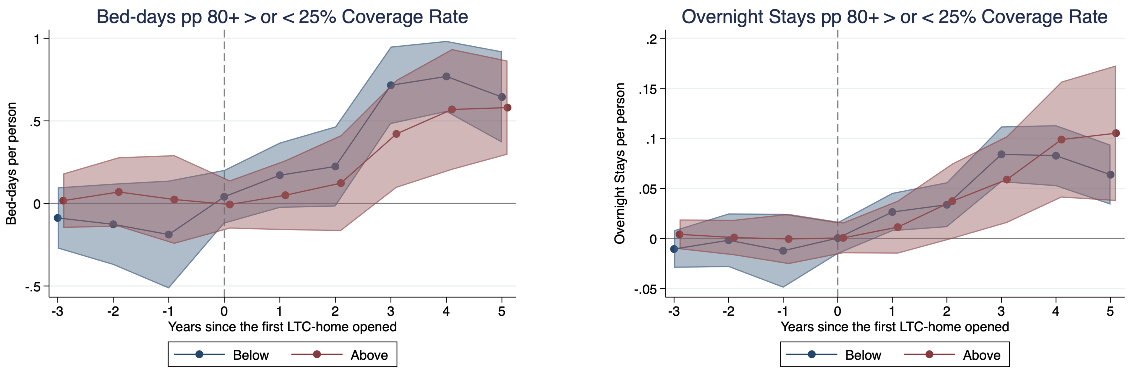
6.6.4 Initial Coverage Rate

With our original sample we look at how municipalities with above and below an initial coverage rate of 25 percent react to treatment. In figure 6.11 we show the results of estimating equation (5.2) for our 80+ outcome variables using two different samples. One sample consisting of municipalities with an initial coverage rate below 25 percent, and one for those with an initial coverage rate above 25 percent. By looking at the figures it seems that for those aged 80+ the treatment effect is stronger for municipalities with a low initial coverage rate.

The estimates in table A5.6 in the appendix confirm that bed-days per person 80+ increase more in municipalities with low initial coverage rate. A result which is statistically significant. As small municipalities on average have higher initial coverage rate than large municipalities this could help explain the weak treatment effect in these, however, it cannot explain that it becomes negative. The result is also helpful as a general robustness check, as standard economic theory of decreasing marginal utility suggests stronger treatment effects for those who initially had less LTC.

However, this picture changes if we look at overnight stays 80+. Table A5.7 in the appendix show that for overnight stays per person 80+ treatment effects are quite similar across coverage rates but slightly stronger for municipalities with above 25 percent initial coverage rate. Both summed treatment estimates are statistically significant.

The estimates for those aged 67–79 are also shown in table A5.6 and A5.7 in the appendix, and their event study plots are shown in figure A5.5. The number of bed-days per person 67–79 is estimated to react negatively on treatment with initial coverage below 25 percent, a result which is statistically significant. With coverage rate above 25 percent the effect is also negative but much weaker and not statistically significant. For overnight stays per person 67–79 the estimates are not statistically significant but suggests positive treatment effects that are slightly stronger for municipalities with above 25 percent coverage rate.



(a) Bed-days per person 80+ **(b)** Overnight Stays per person 80+

Figure 6.11: Event study plot coverage rate

6.6.5 Limitations

Regarding the Data Set

Our data set has several limitations. A main issue is data we wanted to include not being available at municipality level. A variable we would have liked to include is gender, to see if more LTC affects hospitalization differently for men and women. Further, we use data from somatic hospitals, but we do not make any distinctions between reasons for the hospitalization. In addition, we are only using two age groups, and particularly among the eldest we would suspect there to be differences within age cohorts so that several smaller age groups would have been interesting to look at.

Another limitation to our analysis is that the treatment effect is being based on the first opening date of a LTC home in a municipality. Municipalities opened several homes during the HPE, causing treatment intensity to increased over time. However, changing treatment to happen when the last LTC home opened does not significantly alter the results.

In our analysis we work with data on quantity, like the number of LTC units, the number of bed-days etc. We have not looked at the quality in the service provided and the effects of this. We work on the assumption that several tasks transferred from hospitals to LTC are performed at the same level of quality in both places. This might not be the case. A more thorough study would have included data on e.g. the education level of the care personnel to gain insight on this.

Lastly, we have seen in the literature section that there are large differences between nursing homes and residential care homes, both in terms of care provided and costs. Our data has not allowed us to separate the two in our analysis, however, we believe that this would have been informative because the differences in level of care provided could cause differences in how it affects hospitalization. In addition, we do not explicitly include home-based care, which is an important part of LTC, this also reduce our insight. However, not being able to separate home-based care in residential care homes and in private homes with the available data made us reluctant to include it as we would not be able to separate effects of institutional environments and care.

Regarding the Estimation Method

Since we are using panel data, we must be aware of serial correlation. Serial correlation is the tendency for observations to be correlated with previous observations. In our case we see clear trends in the development of hospitalization over time, as shown in figure 4.1. These trends and the fact that hospital use in a municipality one year most likely is correlated with hospital use the next year cause us to be concerned about serial correlation.

Serial correlation is a problem because it biases the estimated standard errors, which in turn cause problems for the inference. The most common way to deal with serial correlation is to cluster the standard errors. We have clustered all standard errors at the municipality level. This makes sense as it is the level of treatment assignment, as well as municipalities possibly being hit by individual shocks. We could also cluster at county-level, but the low number of counties, as well as treatment being assigned at municipality level, makes us prefer municipality-level clustering.

Lastly, we would like to mention the external validity of our study. Are our findings relevant in another context? In this case another context would mean for another country. We consider the external validity of our study to be quite low. The results are to a large extent contingent on the welfare system in Norway and the way we organize our health services. As such a study like ours might find similar results in the Nordic countries but are less likely to do so in other countries. We already know from the Spanish study by Costa-Font et al. (2018) that results are likely to differ.

6.7 Summary of Results

Our results can rule out a strong reduction in hospitalization due to more LTC. Our preferred estimates suggest that more LTC cause a significant increase in hospitalization for those aged 80+. This increase was around 50 percent over a six-year period for the number of bed-days per person, and around 60 percent for overnight stays per person. The pre-treatment effects indicates that our results have a causal interpretation. The estimated increase is a very strong result that moves in the opposite direction of what the literature suggests. For the age group 67–79 the estimates suggested a weak increase, but the results were not statistically different from zero. The results also indicate that the treatment effect is slightly stronger with increased treatment intensity, and slightly weaker for large municipalities, most likely due to weaker treatment intensity.

The strong increase can in part be attributed to a reduction in mortality rates, which might work together with a low threshold for sending LTC user to hospital. Moving pattern effects could also play a part in the increased hospitalization. However, more analyses are needed to pin down the exact reasons for the increase in hospitalization we find.

While our estimates are consistent when using other estimation methods, we have some issues when it comes to restriction of the sample and outliers. Outliers seem to slightly upward bias our estimates for those aged 80+, indicating that treatment effects might be weaker, but still strong. More worrisome is the fact that our estimates seem to be highly dependent on the restriction of the sample, where small municipalities seem to drive the results. While we cannot explicitly rule out that this is not due to random variation, our estimates suggest that small municipalities have negative treatment effects. As we do not find any good explanations for these negative estimates, we say that the internal validity of our study has some problems, and we cannot state that our estimates transfer to small municipalities. However, overall, our estimates seem to be robust for all other municipalities, albeit with perhaps slightly weaker treatment effects than our preferred estimates suggest.

6.7.1 Implications

The number of bed-days is the main determinant of resource use in hospitals. While our estimates suggest a per person increase in bed-days of around 50 percent for the age group 80+, the true effect might be slightly smaller due to problems with the estimates. Nevertheless, we have good reason to believe there is an increase associated with more LTC, and as a consequence, health care costs will rise. As such, there does not seem to be a natural substitution between expensive hospitalization and cheaper LTC for the elderly. In fact, investing more in LTC increased the use of hospitals according to our analysis. If bed-days per person increase by 2 days i.e., 50 percent, for those aged 80+, and a bed-day in hospital has a cost of NOK 8,000 this amounts to a cost increase of almost NOK 3.7 billion given 230,000 people aged 80+. This is an additional cost of more LTC beyond the general hospitalization costs and LTC costs. While NOK 3.7 billion over the course of six years might not be much compared to the annual resource use in specialized health care, it is still a significant amount of money.

However, this does not vouch for less investments in LTC. It means that policy makers should determine the expansion of LTC on other factors than cost savings. Many individuals are dependent on an LTC offer, and their relatives might benefit from it as well, by for example taking away the burden of informal care, see Løken et al. (2014). There are many consequences of more LTC and this thesis has only looked at its effect on hospitalization and to some degree mortality rates. To make a policy decision, other factors should be considered, such as the economic value of reduced mortality rates. What we hope our results can contribute to is starting a debate on whether LTC has the intended effects, and if not, if there are more effective ways to organize elderly care. Government spending on elderly care is an important policy issue requiring considerable attention.

7 Conclusion

Our thesis adds to the small literature regarding investment in elderly care and its impact on hospitalization in the context of Norway. Our main contribution is a causal estimate of the effect of more LTC on hospital use. We find that more LTC caused an increase in hospitalization. Specifically, our preferred estimates suggest that more LTC increased bed-days and overnight stays per person with 50–60 percent over a six-year period for those aged 80+. For the age group 67–79, the estimates suggest a weak increase, but it is not statistically different from zero. The results also indicate that the treatment effect is slightly stronger with high treatment intensity, and slightly weaker for large municipalities.

Our results move in the opposite direction of what the existing literature suggests. From our analyses we find that this seems to be partly explained by a reduction in mortality rates. This is in line with previous research from the United Kingdom. The reduction in mortality rates could further increase hospitalization by working together with a low threshold for sending LTC users to hospital. These are interesting results requiring further investigation. In addition, we have indications that moving patterns might play a role in the increased hospitalization, but we are not able to confirm this. There seems to be factors we have not been able to identify that also drive our results. As our analysis moves in a different direction than what the literature suggests, and we are not fully able to explain why, we conclude that further research on the topic is needed.

Though our analysis suggests that more LTC has led to an increase in hospitalization, policy makers should not stop investing in LTC. Instead it indicates that investment in elderly care perhaps should be determined by other factors than cost savings. Policy makers could include factors like the economic value of reduced mortality rates when making such policy decisions.

When starting the work with this thesis we were motivated by trying to find a way to reduce government expenses related to elderly care. We hoped to contribute to this challenging policy issue. In the end our results did not go in the direction we originally imagined, and we could not make a calculation on how much the government can save in reduced hospital costs with more investments in LTC. Still, our estimated increase is no less important for policy makers than a reduction would have been. Quite the contrary, more LTC leading to increased hospitalization is a highly relevant result for policy makers, with substantial cost implications. We do hope that further analyses will be conducted within this topic to improve the grounds for decision-making.

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Appendix

A1 Categorization for Weighing of Treatment Effects

Group	Total Population Size
1	$\leq 2,000$
2	$> 2,000$ & $\leq 10,000$
3	$> 10,000$ & $\leq 20,000$
4	$> 20,000$ & $\leq 30,000$
5	$> 30,000$ & $\leq 45,000$
6	$> 45,000$ & $\leq 75,000$
7	$> 75,000$

Table A1.1: Categorization of population size

Group	Coverage Rate
1	> 0.5
2	≤ 0.5 & > 0.4
3	≤ 0.4 & > 0.3
4	≤ 0.3 & > 0.2
5	≤ 0.2 & > 0.1
6	≤ 0.1

Table A1.2: Categorization of coverage rate

A2 Event Study Plots Hospitalization

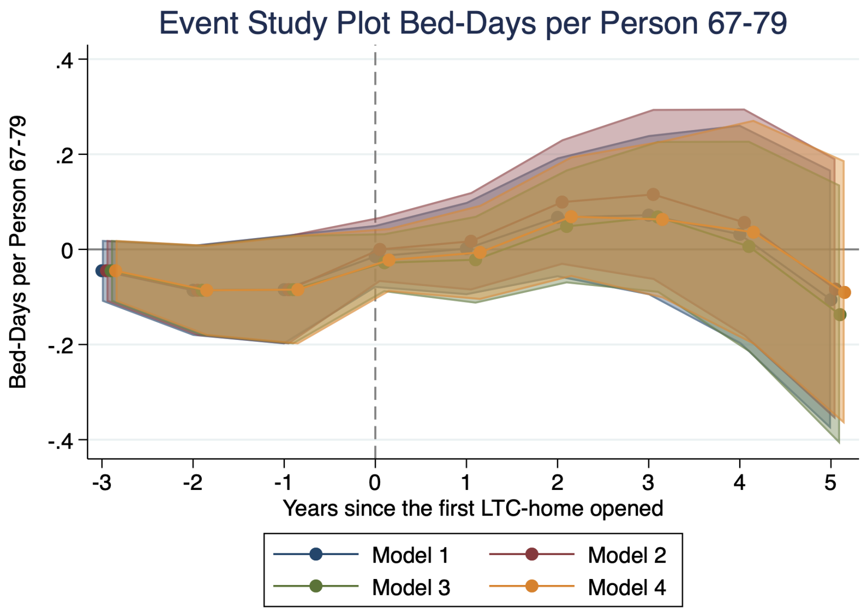


Figure A2.1: Event study plots all models bed-days per person 67–79

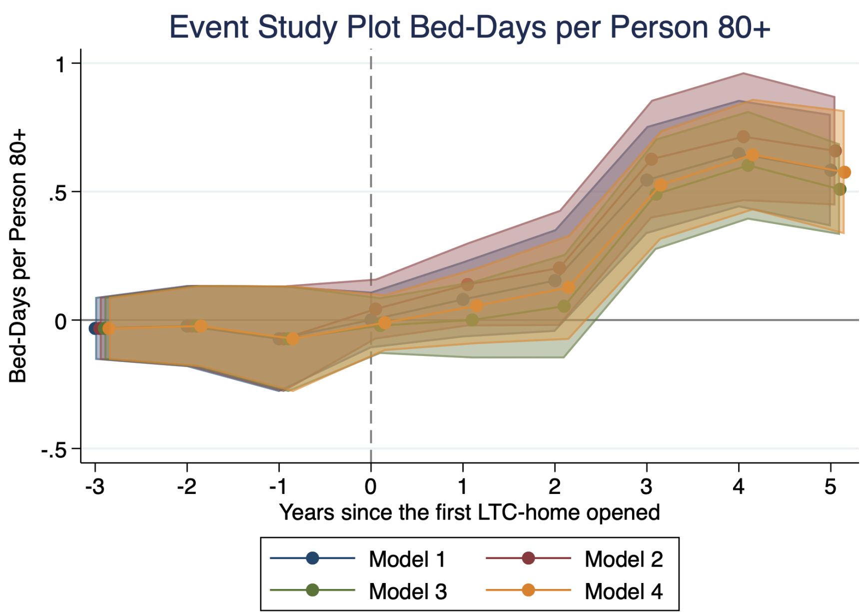


Figure A2.2: Event study plots all models bed-days per person 80+

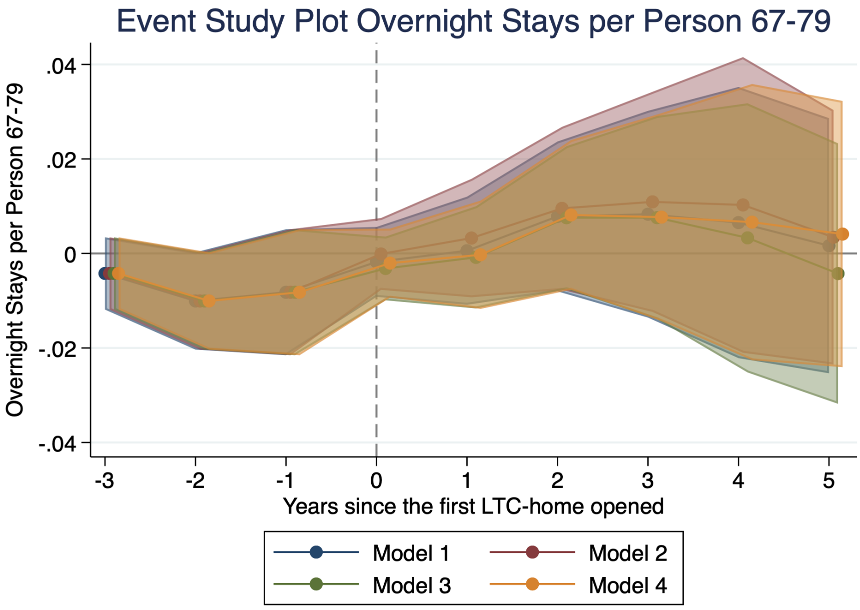


Figure A2.3: Event study plots all models overnight stays per person 67–79

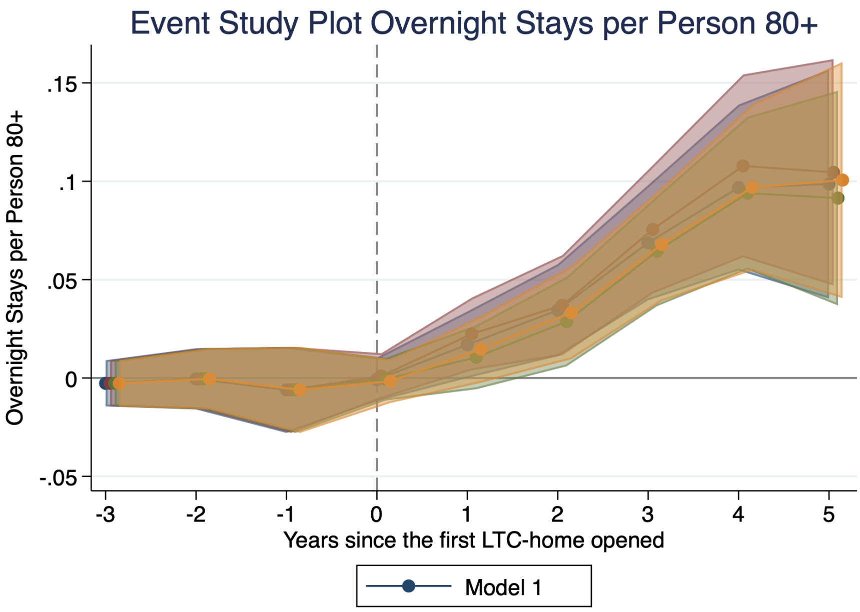


Figure A2.4: Event study plots all models overnight stays per person 80+

A3 Mortality Rates

Table A3.1: Regressions for the effect of more LTC on average mortality rates

	(1) Men 67-79	(2) Women 67-79	(3) Men 80+	(4) Women 80+
Years after treatment:				
Year 0	0.001	-0.001	-0.005	-0.002
	(0.001)	(0.001)	(0.006)	(0.005)
Year 1	0.002	-0.001	-0.002	-0.012*
	(0.002)	(0.001)	(0.007)	(0.007)
Year 2	0.000	-0.001	-0.017**	-0.009
	(0.002)	(0.001)	(0.009)	(0.009)
Year 3	-0.002	-0.003*	-0.007	-0.014
	(0.002)	(0.001)	(0.009)	(0.011)
Year 4	-0.004*	-0.005***	-0.018*	-0.015
	(0.002)	(0.002)	(0.009)	(0.015)
Year 5	-0.010***	-0.005***	-0.035**	-0.022**
	(0.003)	(0.001)	(0.014)	(0.011)
Average TE	-0.002	-0.003***	-0.014**	-0.012
	(0.002)	(0.001)	(0.007)	(0.008)
Pre-treatment mean	0.043	0.022	0.207	0.190
N	3382	3382	3382	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All models are simple imputations. Column 1 uses average mortality rate for men 67–79 as outcome variable. In column 2 the outcome variable is average mortality rate for women 67–79. Column 3 and 4 follow column 1 and 2 but for 80+. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

A4 Moving Patterns

Table A4.1: Regression with the number of inhabitants aged 80+ as outcome

	(1) Inhabitants 80+
Years after treatment:	
Year 0	17.203***
	(3.874)
Year 1	24.800***
	(5.768)
Year 2	41.434***
	(9.208)
Year 3	49.659***
	(12.464)
Year 4	55.119***
	(16.910)
Year 5	65.721***
	(22.654)
Sum TE	253.936***
	(67.641)
Pre-treatment mean	630.715
N	3382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The model is a simple imputation. The outcome is the number of people aged 80+ in municipalities. Sum TE shows the sum of treatment effects (sum of the lags). Standard errors are clustered at the municipality level.

A5 Robustness Checks

A5.1 Changing Method and Assumption

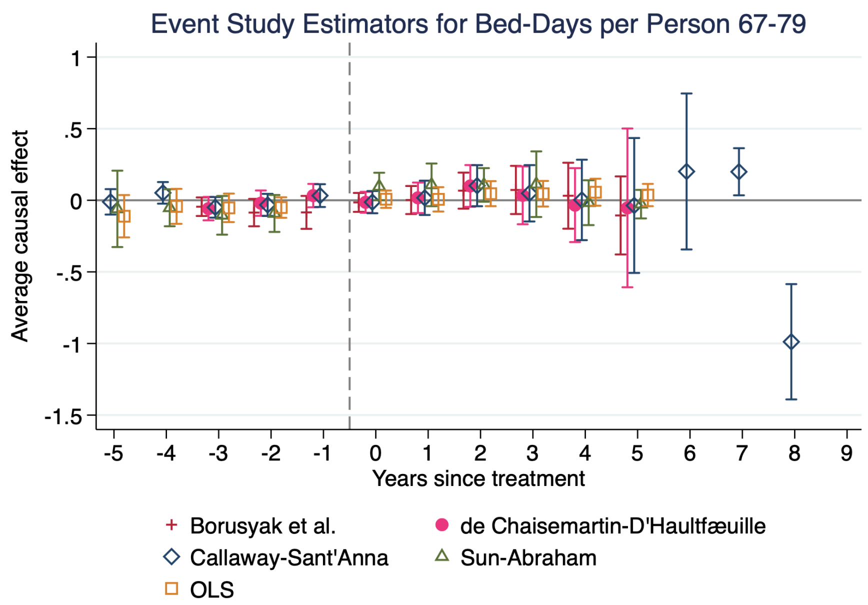


Figure A5.1: Sensitivity analysis using different event study estimators and specifications for the outcome bed-days per person 67–79

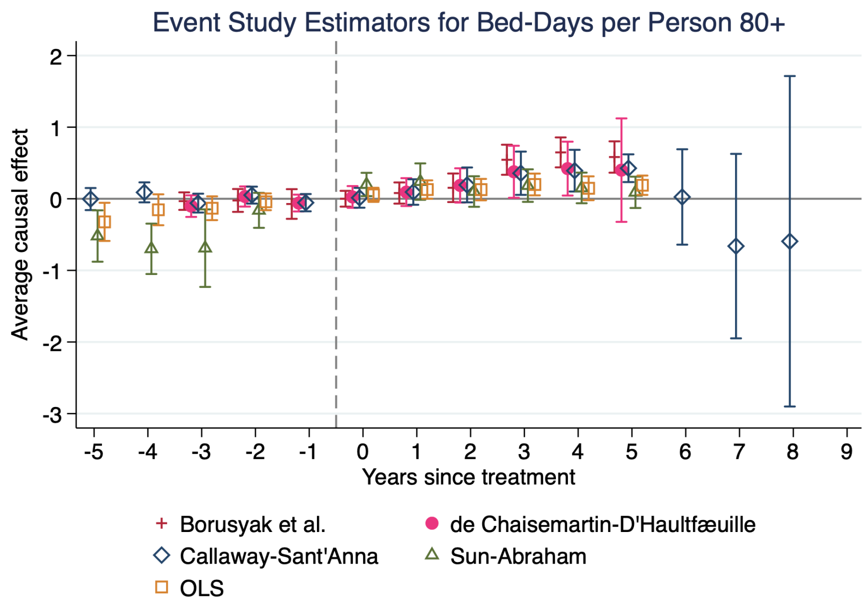


Figure A5.2: Sensitivity analysis using different event study estimators and specifications for the outcome bed-days per person 80+

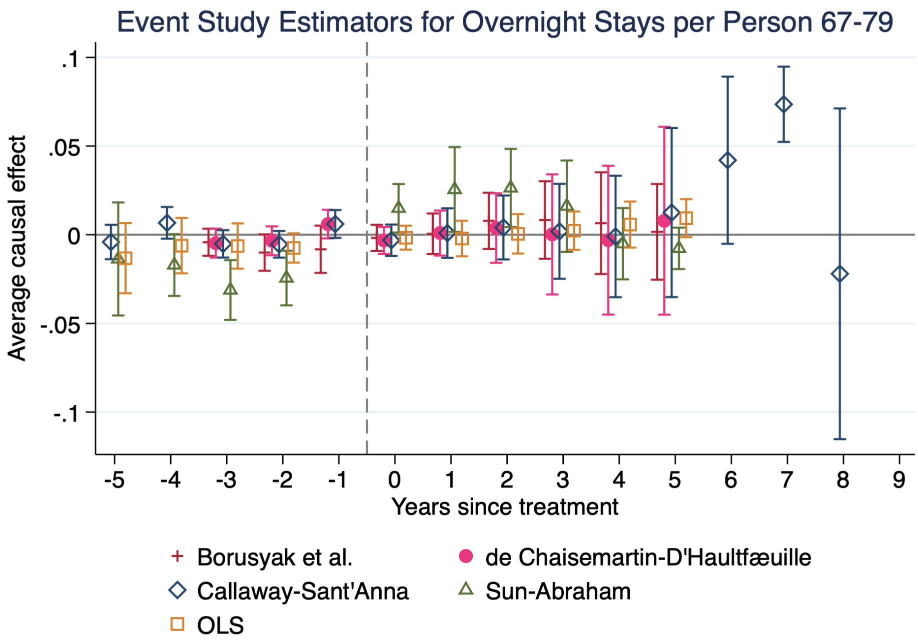


Figure A5.3: Sensitivity analysis using different event study estimators and specifications for the outcome overnight stays per person 67–79

A5.2 Excluding Outliers

Table A5.1: Regressions for all four Outcomes Excluding Outliers

	(1) Bed-Days pp 67–79	(2) Bed-Days pp 80+	(3) Overnight Stays pp 67–79	(4) Overnight Stays pp 80+
Years after treatment:				
Year 0	-0.002	-0.001	-0.003	-0.000
	(0.032)	(0.055)	(0.004)	(0.006)
Year 1	-0.019	0.064	-0.002	0.013*
	(0.048)	(0.071)	(0.005)	(0.008)
Year 2	0.044	0.107	0.006	0.028**
	(0.065)	(0.095)	(0.008)	(0.011)
Year 3	0.036	0.489***	0.010	0.056***
	(0.095)	(0.100)	(0.013)	(0.014)
Year 4	0.006	0.552***	0.009	0.082***
	(0.154)	(0.085)	(0.020)	(0.019)
Year 5	-0.077	0.472***	0.007	0.079***
	(0.207)	(0.088)	(0.023)	(0.028)
Sum TE	-0.011	1.681***	0.027	0.258***
	(0.538)	(0.378)	(0.065)	(0.072)
Pre-treatment mean	2.597	3.917	0.357	0.519
N	3318	3311	3318	3317

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sum TE shows the sum of treatment effects of models estimated with five lags. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

A5.3 Restricting the Sample Differently

Table A5.2: Regressions for the effect of more LTC on number of bed-days per person in age group 67–79, Different Samples

	(1) Original	(2) >100	(3) >200	(4) No Restriction	(5) <150
Years after treatment:					
Year 0	-0.015	0.001	-0.026	-0.055	-0.119
	(0.034)	(0.033)	(0.035)	(0.041)	(0.099)
Year 1	0.002	0.001	-0.031	-0.078	-0.158
	(0.050)	(0.042)	(0.045)	(0.064)	(0.133)
Year 2	0.067	0.053	-0.000	-0.076	-0.209
	(0.064)	(0.059)	(0.060)	(0.093)	(0.177)
Year 3	0.072	0.082	-0.045	-0.138	-0.318
	(0.086)	(0.081)	(0.082)	(0.138)	(0.234)
Year 4	0.032	0.155	-0.109	-0.207	-0.271
	(0.117)	(0.102)	(0.103)	(0.221)	(0.329)
Year 5	-0.106	0.173	-0.265***	-0.293	-0.362
	(0.139)	(0.141)	(0.081)	(0.309)	(0.421)
Sum TE	0.052	0.466	-0.476	-0.847	-1.437
	(0.431)	(0.381)	(0.345)	(0.808)	(1.274)
Pre-treatment mean	2.586	2.561	2.592	2.552	2.495
N	3382	4215	2665	5107	1643

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of bed-days per person 67–79. All models are simple imputations with different restrictions of the sample. Column 1 show the original sample which excludes municipalities with less than 150 persons aged between 67–79. The other column use different restrictions according to their name. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

Table A5.3: Regressions for the effect of more LTC on number of bed-days per person in age group 80+, Different Samples

	(1) Original	(2) >100	(3) >200	(4) No Restriction	(5) <150
Years after treatment:					
Year 0	0.000	-0.020	-0.111*	-0.086	-0.151
	(0.056)	(0.052)	(0.067)	(0.055)	(0.107)
Year 1	0.080	0.018	-0.053	-0.134*	-0.376***
	(0.075)	(0.069)	(0.094)	(0.075)	(0.140)
Year 2	0.154	0.110	0.012	-0.101	-0.222
	(0.102)	(0.095)	(0.134)	(0.115)	(0.191)
Year 3	0.545***	0.292**	0.372**	-0.026	-0.354*
	(0.107)	(0.122)	(0.179)	(0.154)	(0.214)
Year 4	0.648***	0.309**	0.343	0.052	-0.202
	(0.107)	(0.157)	(0.221)	(0.187)	(0.240)
Year 5	0.583***	0.255	0.304	0.132	0.048
	(0.112)	(0.162)	(0.244)	(0.188)	(0.237)
Sum TE	2.010***	0.964*	0.868	-0.164	-1.258
	(0.443)	(0.561)	(0.824)	(0.684)	(0.911)
Pre-treatment mean	3.915	3.813	3.983	3.752	3.402
N	3382	4215	2665	5107	1643

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of bed-days per person 80+. All models are simple imputations with different restrictions of the sample. Column 1 show the original sample which excludes municipalities with less than 150 persons aged between 80+. The other column use different restrictions according to their name. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

Table A5.4: Regressions for the effect of more LTC on number of overnight stays per person in age group 67–79, Different Samples

	(1) Original	(2) >100	(3) >200	(4) No Restriction	(5) <150
Years after treatment:					
year 0	-0.002	-0.000	-0.004	-0.005	-0.008
	(0.004)	(0.003)	(0.004)	(0.005)	(0.012)
Year 1	0.001	0.000	-0.003	-0.008	-0.016
	(0.006)	(0.005)	(0.006)	(0.008)	(0.017)
Year 2	0.008	0.007	0.001	-0.011	-0.027
	(0.008)	(0.007)	(0.009)	(0.013)	(0.023)
Year 3	0.008	0.008	-0.002	-0.024	-0.054*
	(0.011)	(0.011)	(0.015)	(0.018)	(0.028)
Year 4	0.007	0.012	-0.009	-0.033	-0.045
	(0.015)	(0.019)	(0.022)	(0.027)	(0.038)
Year 5	0.002	0.016	-0.013	-0.034	-0.041
	(0.014)	(0.030)	(0.025)	(0.036)	(0.047)
Sum TE	0.023	0.043	-0.030	-0.115	-0.190
	(0.051)	(0.068)	(0.075)	(0.099)	(0.151)
Pre-treatment mean	0.358	0.358	0.354	0.361	0.372
N	3382	4215	2665	5107	1643

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of overnight stays per person 67–79. All models are simple imputations with different restrictions of the sample. Column 1 show the original sample which excludes municipalities with less than 150 persons aged between 67–79. The other columns use different restrictions according to their name. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

Table A5.5: Regressions for the effect of more LTC on number of overnight stays per person in age group 80+, Different Samples

	(1) Original	(2) >100	(3) >200	(4) No Restriction	(5) <150
Years after treatment:					
Year 0	-0.001	-0.003	-0.007	-0.008	-0.015
	(0.006)	(0.005)	(0.007)	(0.006)	(0.013)
Year 1	0.017*	0.011	0.006	0.000	-0.017
	(0.009)	(0.008)	(0.010)	(0.009)	(0.018)
Year 2	0.034***	0.027***	0.023	0.006	-0.014
	(0.012)	(0.011)	(0.015)	(0.014)	(0.023)
Year 3	0.069***	0.048***	0.061***	0.023	-0.008
	(0.015)	(0.013)	(0.018)	(0.018)	(0.027)
Year 4	0.097***	0.058***	0.089***	0.037*	-0.005
	(0.022)	(0.018)	(0.021)	(0.022)	(0.029)
Year 5	0.099***	0.053**	0.101***	0.046*	0.022
	(0.030)	(0.021)	(0.016)	(0.023)	(0.029)
Sum TE	0.315***	0.195***	0.273***	0.105	-0.036
	(0.079)	(0.065)	(0.078)	(0.081)	(0.114)
Pre-treatment mean	0.519	0.513	0.523	0.513	0.504
N	3382	4215	2665	5107	1643

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The outcome variable is the number of overnight stays per person 80+. All models are simple imputations with different restrictions of the sample. Column 1 shows the original sample which excludes municipalities with less than 150 persons aged 80+. The other columns use different restrictions according to their name. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

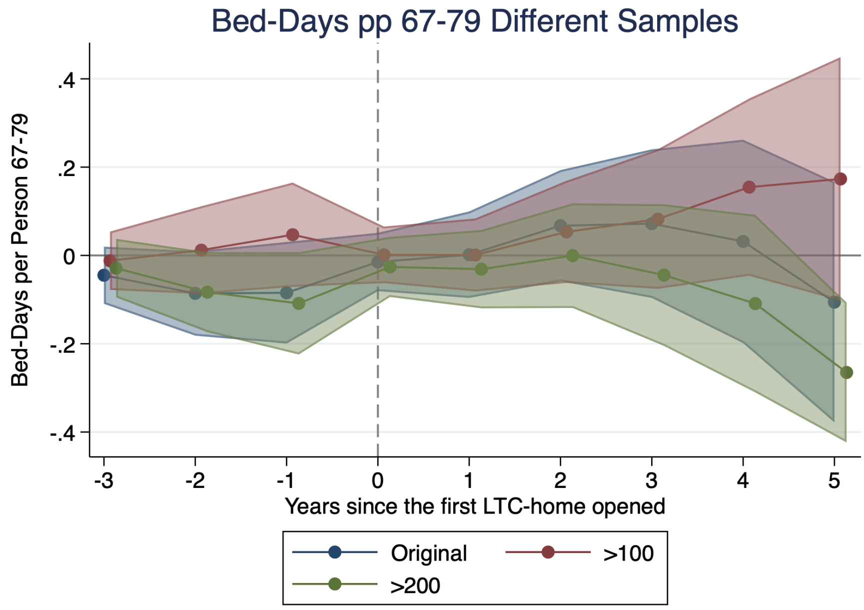
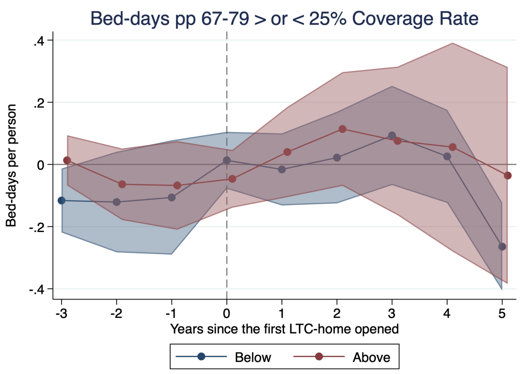
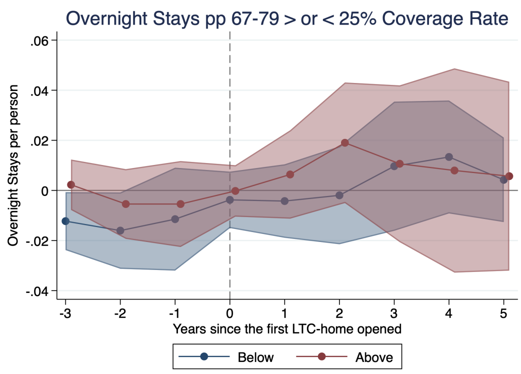


Figure A5.4: Event study plot with different sample sizes, using bed-days per person 67–79 as outcome

A5.4 Initial Coverage Rate



(a) Bed-days per person 67–79



(b) Overnight Stays per person 67–79

Figure A5.5: Event study plot coverage rate

Table A5.6: Regressions for municipalities with initial coverage rate below or above 25 percent, bed-days per person

	Below		Above	
	67–79	80+	67–79	80+
Years after treatment:				
Year 0	0.013	0.041	-0.046	-0.006
	(0.047)	(0.083)	(0.047)	(0.075)
Year 1	-0.016	0.171*	0.040	0.050
	(0.059)	(0.101)	(0.074)	(0.108)
Year 2	0.022	0.224*	0.114	0.123
	(0.075)	(0.124)	(0.093)	(0.148)
Year 3	0.093	0.716***	0.076	0.421**
	(0.081)	(0.120)	(0.122)	(0.167)
Year 4	0.026	0.769***	0.056	0.569***
	(0.076)	(0.110)	(0.171)	(0.187)
Year 5	-0.265***	0.645***	-0.036	0.580***
	(0.072)	(0.141)	(0.178)	(0.145)
Sum TE	-0.126	2.565***	0.204	1.737**
	(0.320)	(0.506)	(0.610)	(0.681)
Pre-treatment mean	2.670	3.922	2.517	3.916
N	1396	1396	1841	1841

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All models are simple imputations. The outcome is bed-days per person. Column 1 shows bed-days per person 67–79 for municipalities with initial coverage rate below 25 percent. Column 2 shows bed-days per person 80+ with initial coverage rate below 25 percent. Column 3 and 4 mirror 1 and 2 but for municipalities with initial coverage rate above or equal to 25 percent. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.

Table A5.7: Regressions for municipalities with initial coverage rate below or above 25 percent, overnight stays per person

	Below		Above	
	67–79	80+	67–79	80+
Years after treatment:				
Year 0	-0.004	0.000	-0.000	0.001
	(0.006)	(0.008)	(0.005)	(0.008)
Year 1	-0.004	0.026***	0.006	0.011
	(0.007)	(0.010)	(0.009)	(0.014)
Year 2	-0.002	0.034***	0.019	0.037*
	(0.010)	(0.011)	(0.012)	(0.019)
Year 3	0.010	0.084***	0.011	0.059***
	(0.013)	(0.014)	(0.016)	(0.022)
Year 4	0.013	0.083***	0.008	0.099***
	(0.011)	(0.016)	(0.021)	(0.030)
Year 5	0.004	0.064***	0.006	0.105***
	(0.009)	(0.015)	(0.019)	(0.035)
Sum TE	0.017	0.291***	0.050	0.312***
	(0.049)	(0.056)	(0.074)	(0.109)
Pre-treatment mean	0.364	0.519	0.352	0.519
N	1396	1396	1841	1841

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All models are simple imputations. The outcome is overnight stays per person. Column 1 shows overnight stays per person 67-79 for municipalities with initial coverage rate below 25 percent. Column 2 shows overnight stays per person 80+ with initial coverage rate below 25 percent. Column 3 and 4 mirror 1 and 2 but for municipalities with initial coverage rate above or equal to 25 percent. Sum TE shows the sum of treatment effects (sum of the lags). All standard errors are clustered at the municipality level.