Living Longer, Caring Better:

Equality and efficiency in health and care at old age

Marlies Bär

Living Longer, Caring Better: Equality and efficiency in health and care at old age
Marlies Bär

Living Longer, Caring Better:

Equality and efficiency in health and care at old age

Marlies Bär

Colophon

Cover design by Robin Weijland, https://www.persoonlijkproefschrift.nl/

Printed by Optima Grafische Communicatie, https://ogc.nl/

Living Longer, Caring Better:
Equality and efficiency in health and care at old age

Langer leven, beter zorgen:
Gelijkheid en efficiëntie in gezondheid en zorg op oudere leeftijd

Thesis

to obtain the degree of Doctor from the
Erasmus University Rotterdam
by command of the
rector magnificus

Prof.dr. A.L. Bredenoord

and in accordance with the decision of the Doctorate Board.
The public defense shall be held on

Thursday 23 May 2024
at 15:30 hrs

by

Marlies Bär
born in Leeuwarden, the Netherlands.

Erasmus University Rotterdam

Doctoral Committee:

Promotor:	Prof.dr. E.K.A. van Doorslaer
Other members:	Prof.dr. F.T. Schut Prof.dr. M.P. Garcia Gomez Prof.dr. M. Lindeboom
Co-promotors:	Dr. B. Wouterse Dr. P.L.H. Bakx

Contents

Chapter 1

Introduction

Across the entire developed world, populations are ageing rapidly (Gruber et al., 2023). The proportion of the population aged 65 years and above increased globally from 7 percent in 2000 to 9 percent in 2020 and is expected to rise to 16 percent in 2050 (United Nations, 2022). In the Netherlands, one out of five individuals is currently aged 65 or older, which is anticipated to become a quarter in the next thirty years (OECD, 2023c). These developments stem not only from declines in fertility but also from decreasing mortality rates caused by continuous enhancements in population health (Bloom and Luca, 2016), particularly at older ages (OECD, 2023b). With more people surviving up to the age of 65 and beyond, health and care for the older population is becoming increasingly important.

This thesis contributes to the understanding of differences in health and care provision for the older population by examining: i) income-related inequality in mortality at older ages; and ii) efficiency in nursing home care.

Part I: Income-related inequality in mortality at older ages

Improvements in mortality have kept raising life expectancy in all OECD countries since the 1990s (OECD, 2023a), but not to the same extent across socioeconomic groups. Several studies report that the improvements in mortality over the last decades favoured the rich and have further widened the life expectancy gap by income (Banks et al., 2021; Chetty et al., 2016; Currie and Schwandt, 2016; Dahl et al., 2023). Even in a country like the Netherlands, which has an extensive welfare system with universal access to care that aims to reduce such inequalities, the increase in life expectancy at age 40 between 2005 and 2015 was twice as high among the richest 20 percent compared to the poorest 20 percent, contributing to a life expectancy gap of 9 years between these income groups in 2015 (Muns et al., 2018). Given that a long life is considered an important welfare indicator (Banks et al., 2021), such mortality-related inequalities are generally undesirable.

To move towards the long-standing worldwide goal of reducing mortality inequalities (Commission on Social Determinants of Health and World Health Organization, 2008), it is important to understand what drives these inequalities. This thesis provides insights into these drivers in three ways: i) by examining inequalities within different age-groups; ii) by providing descriptive evidence of the relative contribution of specific causes of death; and iii) by using the recent COVID-19 pandemic as a case for studying mortality disparities by income. Doing so, this thesis highlights the importance of focusing on mortality inequalities at older ages in research and policy.

Given that the recent improvements in mortality were more concentrated among older age groups (OECD, 2023b), it is important to understand the inequality patterns of this particular group. However, most studies focus on inequalities in life expectancy (Chetty et al., 2016; Muns et al., 2018), which is a composite measure of mortality rates at different ages. These studies do not reveal the, potentially very different, trends in inequality at different ages. Currie and Schwandt (2016) show that while mortality disparities by income at younger ages decreased, they increased among older ages in the United States between 1990 and 2010. Such diverging trends would not be visible when examining overall life expectancy. These underlying trends have not been examined much in the literature before because data on income, age and health outcomes, was previously only available through surveys; leaving studies examining age-specific trends to be underpowered due to small sample sizes. Nowadays administrative data sets encompass reliable population-wide information on individual level income, age and mortality. Chapter 2 of thesis uses such data to examine underlying age-trends behind the increasing life expectancy gap documented in other studies from the Netherlands (for example by Muns et al. (2018)).

To get a better sense of whether and how plausible disparities at older ages could be reduced, it is important to identify the potential determinants of the mortality gap by income and whether these determinants are avoidable. Potential determinants of increasing inequalities in mortality may include socioeconomic differences in resources to invest in health (e.g. income or education), circumstances (e.g. working conditions), health behaviors (e.g. smoking) and access to medical treatments and adoption of new technologies (Cutler et al., 2006, 2011). While some of these are inherently related to income, differences in health behaviors and access to medical treatments could be mitigated by appropriate policies. Chapter 2 examines this potential by identifying the contribution of potentially avoidable deaths through prevention and treatments in explaining changes in the mortality gap by income. This can provide valuable insights for crafting targeted policies aimed at reducing health disparities at older ages.

The recent COVID-19 pandemic serves as an interesting case for studying mortality disparities at older ages. First, because the pandemic had a large sudden impact on mortality, especially at older ages. In the Netherlands in 2020 and 2021, 40 thousand individuals died from COVID-19, of which 88 percent occurred among the 70+

year-old population (Statistics Netherlands, 2023d). Second, the pandemic affected the whole population. Yet, regardless of the virus not discriminating between income groups, the risk of dying from COVID-19 was 2.2 times higher in the poorest 20 percent compared to the richest 20 percent of the older population (Stoeldraijer et al., 2022). However, it is important to note that not all COVID-19 deaths contributed to more deaths than expected (i.e. excess deaths) (Statistics Netherlands and RIVM, 2022), and that poorer groups have a higher expected mortality rate in general based on mortality trends prior to the pandemic (Banks et al., 2021). Chapter 3 analyses income-related variation in excess mortality to study the pandemic's overall impact on mortality disparities at older ages and to examine whether pre-existing socioeconomic differences in underlying health shaped unequal mortality patterns during the pandemic.

Part II: Efficiency in nursing home care

Part II of this thesis focuses on nursing home care, which is an important sector as it constitutes a considerable part of formal care provision to the older population and the public budget. In the Netherlands, more than 50 percent of individuals aged 70 and over use nursing home care at some point during their lifetime, with average lifetime costs rising up to 254,000 euros per individual (Wouterse et al., 2022). Because nursing home care in the Netherlands is mainly publicly financed, these expenditures make up 2 percent of the Dutch GDP in 2019 (Bakx et al., 2023). These public expenditures on nursing home care are expected to rise further as the share of oldest age groups among the older individuals increases (Rijksinstituut voor Volksgezondheid en Milieu, 2020).

Insights into variation in how efficient the provision and allocation of nursing home care are can inform the policy debate about how to maintain access to high quality care without exhausting the workforce and public budgets while demand for nursing home care is rising. This thesis covers two types of (in)efficiencies in the nursing home care sector, namely efficient provision (Chapters 4 & 5) and efficient allocation (Chapter 6) of nursing home care. This distinction is important because examining each requires different research designs and generates different implications for policy.

Efficient provision of nursing home care

Efficiency in care provision means how well providers can convert inputs into relevant outputs (Jacobs et al., 2006). In other words, nursing home care provision is considered efficient when providers produce the highest level of outcomes given a fixed set of inputs, for example through optimising care processes as such that every additional staff member improves resident quality of life or health by as much as possible, or by choosing the optimal mix of nurses and medical doctors that maximises these resident outcomes. Identifying the highest attainable level of outcomes requires a comparison of all nursing home providers, controlling for input levels (such as budgets and staffing levels). Substantial variation in outcomes across providers conditional on input levels indicates a scope for enhancing efficiency, at least among providers with the worst outcomes. Documenting and explaining such variation in efficiency across nursing home providers is hence essential for determining whether and where efficiency gains are feasible. However, measuring efficiency proves challenging because not all nursing homes are comparable and there is very limited information about the outcomes produced by nursing home providers.

New causal methods to identify provider variation
Over the past three decades, researchers have employed different approaches to assess nursing home efficiency. Starting in the late 1980s, researchers adopted non-parametric methods from industry and hospitals (Kooreman, 1994; Nyman and Bricker, 1989). This involved constructing a production frontier based on providers achieving certain health outcomes with minimal inputs and measuring a provider's efficiency relative to the frontier. This approach, however, can only control for differences in the *observable* characteristics of residents across nursing homes (Arling and Daneman, 2002; Grosskopf and Valdmanis, 1993), while such differences may also be *unobserved*. Failing to account for all relevant case-mix differences leads to selection bias and unfair comparisons.

With the shift in emphasis to causal inference in economics and related fields, researchers have increasingly emphasised correcting for selection biases through natural experiments and quasi-experimental approaches in econometrics. Commonly applied methods use so-called exogenous shocks, such as lotteries, or policies that affect part of the population of interest to define comparable treatment and control groups. Because these methods require sufficient statistical power and therefore a sufficient number of people within each treatment group, they are generally used to estimate the impact of one treatment at a time. As a result, the focus of the nursing home literature shifted from measuring total differences in efficiency to identifying the impact of specific inputs, such as staffing (Friedrich and Hackmann, 2021; Lin, 2014) or ownership type (Grabowski et al., 2013; Gupta et al., 2021; Huang and Bowblis, 2018). Only recently, value-added methods have enabled the application of causal frameworks when studying the total difference in performance, even of smaller entities. These methods are used in the education literature to assess the performance of teachers based on student test scores (Angrist et al., 2016; Chetty et al., 2014; Deming, 2014; Kane and Staiger, 2008), and more recently in the evaluation of hospitals (Chandra et al., 2023; Doyle et al., 2019; Hull, 2020) and insurance plans (Abaluck et al., 2021) based on patient outcomes. Alongside a recent working paper by Einav et al. (2022) from the United States, Chapter 4 of this thesis is the first to adopt such a value-added method when analysing differences in efficiency across nursing home providers.

Using administrative data to measure resident outcomes
To gain insights into nursing home efficiency, comprehensive data on resident outcomes are imperative, yet these outcomes are typically not being measured. Apart from the United States, evaluations and documentation of nursing home performance indicators predominantly rely on inputs and care process metrics, while information on outcome-based indicators is often lacking (Barber et al., 2021). Nonetheless, this

information is crucial for measuring efficiency and performance in general, as indicators measuring the organisational structure or process component of quality, such as the use of psychotropic medicine, do not necessarily correlate with better outcomes (Bakx et al., 2020a; Werner et al., 2013).

Chapters 4 and 5 of this thesis demonstrate how the combination of multiple administrative data sources can be used to construct an outcome-based indicator based on the full population of nursing home residents. Both chapters use administrative data sources covering Dutch nursing home residents and organisation-level data on inputs and processes of nursing home care providers, which is a novel combination within the Netherlands. Previously, the organisation-level data was fragmented across different sources and unlinkable to individual-level information on residents. This thesis shows the result of an effort in bringing these sources together and integrating them with comprehensive administrative data on anonymised nursing home residents. This link contributes to more insights into nursing home variation in outcomes, without burdening care workers or recipients. In addition, it facilitates an examination of how resident outcomes relate to inputs (e.g. staffing) and processes of care (e.g. use of psychotropic medicine).

Efficient allocation of nursing home care

Efficient allocation of nursing home care requires scarce nursing home care to be optimally allocated to individuals who benefit the most from it so that it maximises welfare (Palmer and Torgerson, 1999). This entails, for example, that individuals with higher care needs – for whom the benefits or receiving nursing home care are the largest – should receive care in nursing homes and those with lower care needs in their own homes. In a setting where potential care users do not experience the costs, such as when public insurance covers these costs, additional policy measures may be needed to foster an efficient allocation.

Governments face challenges in addressing limited capacity in nursing homes and in promoting an efficient allocation of care among ageing populations. Waiting lists arise as a consequence of such limited capacity, yet it can also serve as a policy tool targeted at an optimal allocation of care. Waiting lists may aid in efficiently allocating care by incentivizing high-need individuals to seek care at a provider without a wait list (Iversen and Siciliani, 2011), or by empowering providers to prioritise high-need individuals from their wait list (Gravelle and Siciliani, 2008). However, in situations where waiting lists exist among all providers or when providers lack comprehensive information about the care needs of individuals on the wait list, waiting lists may sub-optimally delay access to nursing homes for the most severely ill (Arntzen et al., 2022; Leshno, 2022; Letterie, 2023).

To understand whether waiting lists are a useful policy tool for allocating scarce nursing home care, it is important to understand the effects of prolonged waiting times both on individuals on the wait list and on the broader healthcare system. However, studying this is challenging because waiting times are likely endogenous: individuals with worse expected outcomes wait shorter than individuals with better outcomes when provider prioritise efficiently. Prior studies that have addressed this issue have focused on the effects of waiting times for (mental) healthcare (Godøy et al., 2019; Moscelli et al., 2016; Nikolova et al., 2016; Prudon, 2023; Reichert and Jacobs, 2018). The consequences of waiting for nursing home admissions could be different because healthcare is aimed at improving health outcomes, while nursing homes focus on providing assistance with activities of daily living (Gruber et al., 2023). Chapter 6 of this thesis is the first study to examine the consequences of delayed admissions to nursing homes in a causal way.

Outline of this thesis

Part I: Income-related inequality in mortality at older ages
As outlined above, this thesis contributes to a better understanding of mortality differences among older individuals in the Netherlands, both before and during the COVID-19 pandemic. Chapter 2 studies mortality trends from 1996 to 2016, focusing on various age groups and poverty deciles within the Dutch population. This research is part of an international collaboration consisting of 11 countries hosted by the Institute of Fiscal Studies (Banks et al., 2021), of which a comparison piece is published in *Proceedings of the National Academy of Sciences (PNAS)* (Schwandt et al., 2021). Following Currie and Schwandt (2016), this chapter analyses the poverty gradient in mortality over time per sex and age-group by dividing the Dutch population into equally sized deciles based on regional poverty shares. Unlike studies merely examining overall life expectancy, this method can uncover contrasting inequality patterns at different ages.

Further, Chapter 2 delves into these inequality trends by decomposing them into the contributions of causes of death that are preventable, treatable, both or neither. This approach sheds light on whether the income-related disparities in mortality could potentially be avoided and to what extent they can be attributed to prevention or healthcare. These findings do not only provide insights into the potential determinants of income-related variation in mortality, which could inspire research directions and hypotheses, but also aid in informing policies aimed at reducing health-inequalities.

Building upon the work in Chapter 2, Chapter 3 focuses on mortality inequalities among the older population during the first year of the COVID-19 pandemic in 2020. During this period, the Dutch population experienced 15 thousand thousand more deaths than expected based on mortality trends from previous years (Statistics Netherlands and RIVM, 2022). Chapter 3 investigates whether these so-called excess deaths were equally distributed over income groups to examine whether the first year of the COVID-19 pandemic increased inequalities in mortality by income. This chapter uses a concentration index (Wagstaff et al., 1991) to identify inequalities in excess mortality and decomposes this into inequalities arising from additional COVID-19 deaths and those from unequal changes in other causes of death. This distinction is useful for understanding whether the pandemic's effect on mortality disparities at older ages is primarily driven by the direct consequences of COVID-19, and whether it is exacerbated or mitigated by COVID-19 displacing other causes of deaths or by the indirect impacts of the pandemic (e.g. lockdowns or delayed medical treatments).

Part II: Efficiency in nursing home care
The second part of this dissertation investigates efficiency in nursing home care. Chapters 4 and 5 focus on efficiency of the provision of nursing home care by examining the variation in health outcomes, specifically (excess) mortality and potentially avoidable hospitalisations, across nursing homes both prior to (Chapter 4) and during the COVID-19 pandemic (Chapter 5). Chapter 4 adopts a value-added approach and exploits exogenous variation in the geographical distance to nursing homes with varying performance levels. This method enables the estimation of a potential selection bias driven by unobserved case-mix differences in outcome-based indicators of nursing home performance. In addition, by linking the variation in outcomes to other nursing home characteristics related to structure and care processes, these chapters contribute to a more profound understanding of which inputs and care practices are related to efficient provision of nursing home care.

Chapter 6 focuses on efficiency of the allocation of nursing home care, specifically examining the consequences of delayed admissions to nursing homes resulting from waiting lists. While waiting lists may serve as an efficient way of allocating scarce care to individuals who benefit the most – i.e. by incentivizing high-need individuals to seek care at a provider without a wait list –, they can also create challenges. When waiting lists arise for all providers or when individuals' preferences are not in line with their actual care needs, it may lead to increased waiting times for high-need individuals, potentially creating undesirable outcomes. Chapter 6 investigates this by examining whether delayed admissions to nursing homes increase hospital utilisation. This examination delves further into the allocation of nursing home care and sheds light on the consequences of inefficiencies in the nursing home sector on the broader healthcare system.

Part I

Income-related inequality in mortality at older age

Chapter 2

Diverging mortality inequality trends among young and old in the Netherlands

With Bram Wouterse, Carlos Riumallo Herl, Tom Van Ourti & Eddy van Doorslaer

Published in Bär, M., Wouterse, B., Riumallo Herl, C., Van Ourti, T. and van Doorslaer, E. (2021), Diverging Mortality Inequality Trends among Young and Old in the Netherlands. *Fiscal Studies, 42*, 79-101.

Abstract

We analyse the trends in inequality in mortality across poverty groups at different ages over the period 1996–2016 in the Netherlands. In addition, we examine whether these trends are related to unequal changes in avoidable mortality, separated by preventable and treatable causes of death. We find that while inequalities in mortality have decreased at ages up to 65, inequalities increased for the oldest age groups. The decline in inequality at the younger ages can, to a large extent, be explained by a strong decrease of mortality from preventable and cardiovascular causes among the poor. The link between inequality and avoidable mortality at the oldest ages is less straightforward. The increasing inequality at old age might be the result of the inequalities shifting from the young to the older age groups, or of the rich benefiting more from the recent health (care) improvements than the poor.

2.1 Introduction

Life expectancy at birth in the Netherlands increased from 77.6 years in 1996 to 81.9 years in 2019 (OECD, 2020a). Although this development indicates substantial gains in the average chances of survival, it is unlikely that these were distributed equally. Survival improvements may have been different not only for groups that are socio-economically more and less advantaged, but also for younger and older age groups. Because (period) life expectancy is a cross-sectional summary measure composed of mortality probabilities at different ages, it does not reveal this heterogeneity in the mortality experience of different age and socio-economic groups.

The reduction of the persistent disparities in survival across socio-economic groups remains an important goal of health policy worldwide (Commission on Social Determinants of Health and World Health Organization, 2008). In the Netherlands, the overall progress in medical care and survival seems not to have led to a decrease in the inequality across socio-economic groups. In spite of the extensive welfare system and high access to care, with universal and comprehensive coverage (Doorslaer and Jones, 2004; van Doorslaer et al., 2006; OECD and European Observatory on Health Systems and Policies, 2019), socio-economic differences in survival remain a major cause for concern (Raad voor Volksgezondheid en Samenleving, 2020; Wetenschappelijke Raad voor het Regeringsbeleid, 2018). In fact, the Netherlands is one of the OECD countries with the lowest out-of-pocket expenditures as a share of total expenditures on health (OECD, 2021b). Yet, closing – or at least reducing – the gap in life expectancy between socio-economic groups continues to be an important policy aspiration (Health Holland, 2019). Along with addressing the socio-economic differences in underlying health behaviours, these are considered a crucial part of the prevention strategy agreed upon by the Dutch government, industry and other societal parties in the national 'prevention coalition'(Ministerie van Volksgezondheid, Welzijn en Sport, 2018).

Addressing inequalities in survival starts with properly measuring them. Earlier studies for the Netherlands generally focus on the gap in life expectancy between low- and high-educated groups. Most studies find large gaps in life expectancy at age 30 and older between the low and high educated (Kulhánová et al., 2014; Mackenbach and Nusselder, 2019; OECD and European Observatory on Health Systems and Policies, 2019). Moreover, several researchers find this absolute gap to be relatively stable over time (Statistics Netherlands, 2012; de Beer and van der Gaag, 2018; Mackenbach and Nusselder, 2019), although van Pieter van Baal and Nusselder (2016) forecast widening inequalities for the population aged 65 and over. A number of studies have focused on inequalities among income groups, using administrative data on individual (household) income. Kalwij et al. (2012) estimate a survival model for the Dutch elderly that depends, among other things, on income. They find a gap of 2.5 years in life expectancy at age 65 between low- and high-income individuals. Muns et al. (2018) analyse the trend of inequality in life expectancy at age 40 between income groups, following a similar approach as Chetty et al. (2016) for the United States. They find that the gap in life expectancy between the lowest and highest income quintiles increases by 1.8 years for men and 2 years for women between 2005 and 2015.

In this study, we examine the development of mortality by socio-economic group, gender and age from 1996 to 2016. We follow the approach developed by Currie and Schwandt (2016), focusing on the overall time trends across gender and age groups, and the differences within these groups across poverty deciles. We rank individuals based on the poverty share of the municipality they are living in. We extend the approach of Currie and Schwandt (2016) by investigating whether the trends in absolute inequality in mortality can be related to causes of death that are either treatable or preventable.

We contribute to the existing literature in several respects. First, we focus on the trends in inequality in mortality at different ages instead of the trend in overall life expectancy. Because life expectancy is a summary measure, the overall trend can mask possibly diverging trends across ages. The fact that earlier Dutch studies arrive at different conclusions on the inequality in life expectancy, depending on the starting age of their analysis, already indicates that age-specific trends might be diverging. Studying the trends in age-specific inequality also allows policymakers to better focus attention and efforts to reduce inequalities where most needed. Diverging trends in age-specific mortality might also be indicative of differences in health across birth cohorts. This would imply that the socio-economic disparities in current-period life expectancy are not representative for younger cohorts, as they are largely driven by the differences in current old-age mortality among older cohorts (Currie and Schwandt, 2016).

Second, we analyse socio-economic differences by income rather than by education, as in previous Dutch studies. Because we can precisely measure poverty shares in geographic areas (such as municipalities), this allows for a ranking of socio-economic groups based on a continuous measure. Education, however, is categorical, which results in less variation across the population and, hence, makes it more difficult to divide the population into (between- and within-country comparable) deciles. Furthermore, within-country comparability is hampered by changes in the relative sizes of educational groups over time (due to increases over time in the education level, particularly for women), while rankings based on poverty shares lead to similarly sized socio-economic groups in every year.

Third, we base our socio-economic ranking of individuals not on individual income, but on the poverty share within their municipalities. This has two practical advantages. First, it allows us to compare our results directly with those for other countries, where administrative data on individual income are not available. Second, it allows us to expand our analysis to younger age groups. Previous Dutch studies have excluded these age groups, either because they do not have an individual income or because their current income is not representative of their lifetime income. Moreover, an extensive series of studies has found that where people grow up and live is important for their economic and health outcomes (Chetty et al., 2016; Chetty and Hendren, 2018; de Jong et al., 2020), implying that the direct measurement of the relation between mortality and poverty at the municipality level has direct relevance on its own.

Finally, we examine whether trends in inequalities are driven by socio-economic differences in the development of particular causes of death. Kulhánová et al. (2014) find that educational inequalities in mortality in the Netherlands are mainly driven by cardiovascular diseases and particular types of cancer (including lung cancer), while for other types of cancer (colorectal, prostate and breast cancer) and external causes (for women) do not show such differences between education groups. Studies for other countries find that trends in inequality in mortality are driven by specific causes. For instance, Kallestrup-Lamb et al. (2020) find that the stagnation in mortality of Danish elderly women between 1985 and 1995 was largely caused by an increasing mortality from cancers and lung- and bronchus-related causes. Similarly, in a study among the Norwegian population for more recent years, namely 2005–2015, Kinge et al. (2019) find that the evolution of mortality from lung cancer, chronic obstructive pulmonary diseases and dementia varied most across income groups.

In this study, we examine trends in two groups of causes of death that can be classified as avoidable (Nolte and McKee, 2004), either by treatment or by prevention. Disaggregating the analysis of inequality trends by these two groups of causes of death can help to guide health-care policymakers to focus their inequality reduction efforts on lifestyle interventions (for prevention) and/or on more (equal) access to health care (for treatment).

We find that mortality decreased between 1996 and 2016 in all age groups and for all poverty groups. Mortality gradients by poverty in the age groups under 65 either decreased or remained stable. We observe the largest decrease in this gradient for men in the age group 20–49 and we demonstrate that this is mainly attributable to a large drop in mortality from potentially preventable causes among the poorest. By contrast, inequalities at older ages (>65) seem to have increased over the last decades, especially for women. Inequality among these older groups increased for all causes of death, either avoidable or not.

2.2 Data and methods

2.2.1 Data sources

We use three main sources of data to estimate mortality inequalities. To estimate the municipal age-specific mortality rates, we obtain the number of deaths and population size by municipality, age and gender using individual-level data from Statistics Netherlands from 1996 onwards. Information on the individuals' gender, date of birth (in months) and place of residence are obtained from the municipal records database (Gemeentelijke Basis Administratie or GBA in Dutch). Additionally, we obtain the date and primary cause of death from individual-level death registries. To construct municipal poverty shares, we use household-level income data from tax registries available from 2004 onwards. In the preceding years, we use information from the Regionale Inkomensverdeling (Statistics Netherlands, 2004) as individual-level income data are not available. For years without municipality-level poverty data (1996–1999), we use information from the nearest year (2000).

2.2.2 Poverty deciles

We divide the population into socio-economic groups using municipal poverty shares, that is, the share of households in a municipality with disposable household income below the poverty threshold. Grouping households in the same municipality using place of residence, we obtained the total number of households and the number of households with an income below the poverty threshold within a municipality. When calculating poverty shares, households that did not have a full year or had missing income are excluded, along with individuals living in student residences, institutions or nursing homes.

The poverty threshold, defined by (Statistics Netherlands, 2020b), is an absolute threshold adjusted for inflation for comparability over time. This threshold is widely used in the Netherlands to monitor the evolution of poverty (Sociaal Cultureel Planbureau/Statistics Netherlands, 2014). It is set as the fixed yearly income a single-person household received from social benefits in the year 1979.[1] To make the absolute threshold applicable to any type of household, we multiply household income by an equivalence factor depending on the household's size and composition (Statistics Netherlands, 2020a).

We calculated poverty shares for all 504 municipalities in the Netherlands. Municipalities are a sufficiently high level of aggregation to calculate a reliable poverty share. Additionally, many income- and health-related policies, such as the provision of home care and poverty policy, are decentralised at the municipality level. To ascertain that municipal changes in poverty deciles over time are not caused by municipality mergers, we keep the municipal borders from the year 2001 fixed over time.[2]

We divide the municipalities over ten deciles based on their poverty share and population size for 1996, 2005 and 2016 separately.[3,4] This allows municipalities to change deciles over time due to, for example, evolution of poverty or migration. As a result, for each year, we compare the 10 per cent of the population living in the least poor municipalities (decile 1) to the 10 per cent living in the poorest municipalities (decile 10). To achieve ten equally sized groups, each decile does not contain an equal number of municipalities, as the municipalities vary in population size. In addition, we split the largest three municipalities (Amsterdam, Rotterdam and The Hague) into three or four equally sized subgroups with equal mortality and poverty shares. In this way, we allow these municipalities to fall into two adjacent deciles (e.g. deciles 9 and 10) to better balance the bin sizes.

[1] In 1979, the purchasing power of social benefits was at its highest.

[2] There have been a notable number of mergers between municipalities between 1995 and 2016. In 1995, the Netherlands counted 633 municipalities. This number was reduced gradually to 390 in 2016 with the greatest reduction between 1996 and 1997 (Statistics Netherlands, 2020c)

[3] Note that municipal poverty shares from the year 2000 are used to divide the 1996 population into deciles.

[4] As robustness checks, instead of re-assigning the municipalities to a poverty decile each year separately, we ran the analyses keeping the poverty deciles fixed. Keeping the poverty deciles fixed at the poverty decile of either 1996, 2005 or 2016 does not affect our main results.

Table 2.1 provides descriptive statistics of each poverty decile in 2005. First, it shows that the poorest deciles contain fewer municipalities than the wealthier deciles. This results from large variations in population size across municipalities and from smaller municipalities generally having lower poverty shares. The latter can also be observed from Figure 2.1, which displays how municipal poverty shares are geographically distributed across municipalities in 2005. The map shows that poverty is mainly clustered in the northern provinces and in the larger municipalities (in terms of population size) such as Amsterdam and Rotterdam.

In addition, from Table 2.1 it follows that there is substantial variation in poverty shares across municipalities. For example, in 2005, poverty shares range from 4.1 to 18.5 per cent. Even though income inequality of households has remained relatively stable in the years before and after 2005 (Statistics Netherlands, 2016),[5] the gap in average poverty shares between municipalities in decile 1 and decile 10 decreased from 15 per cent (7 to 22 per cent) in 2000 to 13 per cent (5 to 18 per cent) in 2005 to 11 per cent (4 to 15 per cent) in 2016.

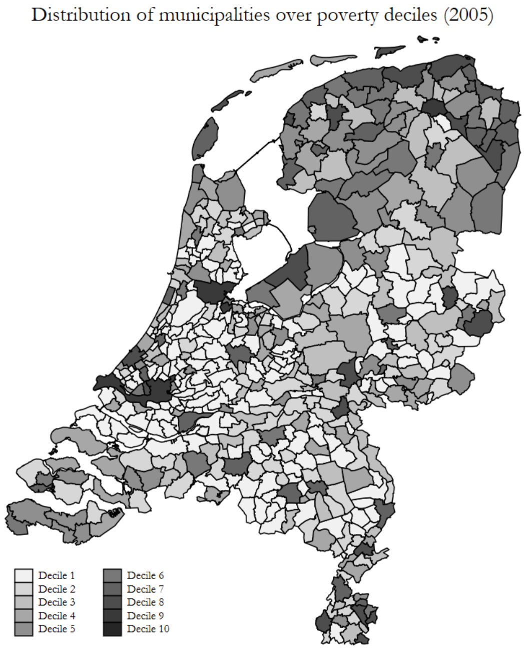
Table 2.1: Descriptive statistics (in year 2005) by poverty decile

	Poverty decile									
	1	2	3	4	5	6	7	8	9	10
Number of municipalities										
Total	104	84	79	66	50	42	29	27	22[a]	8[b]
Number of inhabitants per municipality (x 1,000)										
Total	1,632	1,635	1,639	1,655	1,578	1,651	1,626	1,721	1,631	1,510
Mean	15.7	19.5	20.7	25.1	31.6	39.3	56.1	63.8	74.2	188.8
Std. dev	9.8	15.8	12.2	18.9	27.6	32.6	50.1	69.5	56.2	0.7
Min	1.5	4.2	4.6	0.4	3.5	8.1	6.5	7.0	1.0	178.8
Max	48.0	132.8	78.6	106.5	155.7	139.8	176.5	276.3	158.2	196.8
Municipal poverty share (%)										
Mean	5.4	6.3	7.0	7.7	8.5	9.5	10.7	11.8	14.2	18.1
Std. dev	0.5	0.2	0.2	0.2	0.3	0.4	0.3	0.4	1.2	0.6
Min	4.1	6.0	6.7	7.3	8.1	8.9	10.1	11.3	12.6	16.8
Max	6.0	6.7	7.3	8.0	8.9	10.1	11.1	12.4	16.1	18.5
Age of population in decile in years										
Mean	38.4	38.9	39.0	39.0	39.3	38.9	38.5	38.3	38.9	37.5
Std. dev	22.6	22.6	22.6	22.7	22.6	22.8	22.4	22.2	22.3	21.6
Disposable household income (x 1,000 €)[c]										
Mean	23.1	22.7	22.1	21.5	21.7	20.7	20.3	20.3	19.7	20.2
Std. dev.	16.2	16.5	15.8	15.6	16.2	13.3	13.3	14.1	14.1	17.2
Median	20.4	20.0	19.5	19.1	19.1	18.5	18.2	18.0	17.2	17.2

[a] Decile 9 actually contains 20 municipalities as one is split into three.
[b] Decile 10 actually contains 3 municipalities as one is split into three and one into four.
[c] Disposable income is standardized with respect to household size.

[5] Measured by a Gini coefficient of approximately 0.29.

Figure 2.1: Municipalities in poverty deciles in the Netherlands (2005)



2.2.3 Mortality rates

We estimate age/gender-specific mortality rates using demographic administrative data by combining deaths and population within a municipality. We are also interested in trends for potentially avoidable causes. To investigate whether these trends can be attributed to treatable or preventable causes, we adopt a classification for avoidable mortality from OECD/Eurostat (2019). This list of causes of death, expressed in ICD-10 codes, combines classifications used in earlier analyses (Nolte and McKee, 2004, 2011; Eurostat, 2014; CIHI/Statistics Canada, 2018).

Using this classification, we assign deaths into four mutually exclusive groups: (1) treatable causes, (2) preventable causes, (3) causes that could be both preventable and treatable, and (4) other causes that are not assigned to one of the first three groups. More extensive definitions can be found in the introduction of this issue. Moreover, Table A1 in the online Appendix lists the causes of death with the largest increases or decreases in absolute deaths between 1996 and 2016 in the Netherlands.

Two remarks about this classification are worth making. First, it is focused on identifying causes of death that can likely be avoided (groups 1–3) and not on identifying a group of diseases that can certainly not be avoided (group 4). If the latter was intended, then we would expect no declining trend in mortality for this group at all, as these deaths should be unavoidable. However, as we shall see below, this is not the case. Second, the classification of deaths from avoidable causes is based on premature deaths before age 75 (OECD/Eurostat, 2019). This means that the estimates of the cause-specific trends for the oldest age group (80 and older) should be interpreted with caution.

2.2.4 Methods

We compare the poverty gradients of mortality by age and gender in the years 1996, 2005 and 2016. Following the approach of Currie and Schwandt (2016), we estimate age/gender-specific mortality rates by municipal poverty decile from 1996 onwards and we smooth them using three-year averages (1996–1998, 2005–2007 and 2016–2018). We use the following age groups: 0–4, 5–19, 20–49, 50–64, 65–79 and 80+. The mortality rates are standardised by age within each age/gender group.[6]

Our measure of absolute inequality within a particular age and gender group for a particular year is the slope coefficient of a regression of mortality on poverty decile (included as a continuous variable running from 1 to 10). This slope measures the average change in mortality when going from one poverty decile to the next (poorer) decile. Age/gender-specific inequality trends can be assessed by comparing slopes across years. We formally test for a difference in slope coefficients between 1996 and 2016. If this difference is positive, we interpret this as increasing absolute pro-rich mortality inequalities over time. In other words, if the poverty gradient becomes more positive over time, it means that, on average, the decline in mortality for the least-poor deciles (e.g. decile 1) is larger than for the poorest deciles (e.g. decile 10). We conduct these analyses for both total mortality and for each of the four cause-of-death groups.

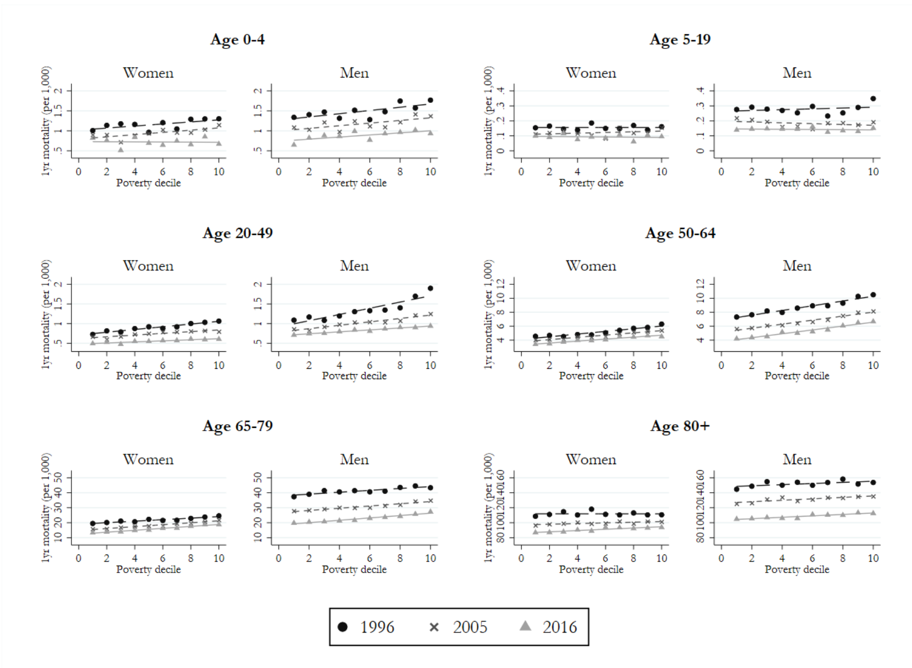
2.3 Results: mortality inequality by gender and age

The main results of our analyses are presented in Figure 2.2 and in Table 2.2. The slopes of the fitted regression lines, or the poverty gradients, in Figure 2.2 indicate the level of inequality. The estimated slope coefficients per year, which are quite precisely estimated from age group 20–49 onwards, and the corresponding intercepts can be retrieved from Table 2.2. Column 9 in Table 2 shows whether the slope has increased, decreased or remained stable between 1996 and 2016. From Figure 2.2, we see that for almost all age groups, mortality was higher among men, and in poorer groups. Moreover, in the last 20 years, mortality has substantially declined across all age/gender groups and for each poverty decile. Men experienced larger reductions in mortality, leading to a narrowing of the gender gap in mortality.

The poverty gradients do not show the same trends over time across age groups. In particular, we mostly find decreasing gradients below the age of 65 (that is, pro-poor improvements) and increasing pro-rich inequalities at older ages. In Figure 2.2 and

[6] The mortality rates of one-year age groups are weighted so that the age distribution within each age, gender and poverty group in each year matches the age distribution within the corresponding age and gender group (across all incomes) in 1995.

Figure 2.2: Poverty gradients in mortality by gender, agegroup and year



Note: Figure 2.2 plots one-year mortality rates (smoothed over three years) across poverty deciles by gender, age group and year. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The estimated intercepts and slope coefficients of the fitted regression lines can be found in Table 2.2

Table 2.2: Slope coefficients of fitted regression lines in Figure 2.2, including constants

	Intercept of regression line Figure 2.2			Poverty gradient/slope of regression lines Figure 2.2			Difference in poverty gradient/slopes		
	1996	2005	2016	1996	2005	2016	Δ 1996 2005	Δ 2005 2016	Δ 1996 2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Women									
Age 0-4	1.017	.793	.730	.026	.028	-.002	.002	-.030	-.028
	(.068)***	(.054)***	(.076)***	(.011)**	(.009)***	(.012)	(.014)	(.015)**	(.016)*
Age 5-19	.156	.109	.097	.000	.002	-.001	.002	-.003	-.001
	(.011)***	(.015)***	(.011)***	(.002)	(.002)	(.002)	(.003)	(.003)	(.003)
Age 20-49	.712	.622	.481	.035	.022	.014	-.013	-.008	-.021
	(.021)***	(.023)***	(.014)***	(.003)***	(.004)***	(.002)***	(.005)***	(.004)*	(.004)***
Age 50-64	4.094	3.750	3.295	.193	.162	.141	-.031	-.021	-.052
	(.143)***	(.083)***	(.083)***	(.023)***	(.013)***	(.013)***	(.027)	(.019)	(.027)*
Age 65-79	19.033	14.774	12.366	.501	.651	.647	.150	-.003	.147
	(.388)***	(.264)***	(.229)***	(.063)***	(.043)***	(.037)***	(.076)**	(.056)	(.073)**
Age 80+	111.862	97.083	86.148	-.007	.449	.848	.455	.399	.855
	(1.989)***	(.807)***	(.878)***	(.321)	(.130)***	(.141)***	(.346)	(.192)**	(.350)**
Men									
Age 0-4	1.267	.996	.740	.041	.033	.026	-.007	-.007	-.014
	(.084)***	(.073)***	(.057)***	(.014)***	(.012)***	(.009)***	(.018)	(.015)	(.016)
Age 5-19	.264	.199	.147	.003	-.003	-.001	-.006	.002	-.003
	(.022)***	(.015)***	(.008)***	(.004)	(.002)	(.001)	(.004)	(.003)	(.004)
Age 20-49	.916	.785	.693	.079	.043	.025	-.036	-.019	-.055
	(.079)***	(.028)***	(.012)***	(.013)***	(.005)***	(.002)***	(.013)***	(.005)***	(.013)***
Age 50-64	6.892	5.124	3.789	.340	.290	.278	-.050	-.011	-.061
	(.175)***	(.127)***	(.131)***	(.028)***	(.020)***	(.021)***	(.035)	(.029)	(.035)*
Age 65-79	37.808	26.692	18.444	.633	.747	.792	.114	.046	.160
	(.726)***	(.469)***	(.389)***	(.117)***	(.076)***	(.063)***	(.139)	(.098)	(.133)
Age 80+	147.790	126.081	103.441	.758	.965	.952	.207	-.013	.194
	(2.177)***	(1.390)***	(.915)***	(.351)**	(.224)***	(.147)***	(.416)	(.268)	(.381)

Notes: Columns 1 to 3 and 4 to 6 report the estimated intercepts and slope coefficients, respectively, from a regression of mortality on poverty deciles, as plotted in Figure 2.2, by sex, age-group and year. Columns 7 to 9 report differences in the estimated slope coefficients between each period. Standard errors are between brackets.
* statistically significant from zero at 10%; ** statistically significant from zero at 5%; *** statistically significant from zero at 1%.

Table 2.2, we observe greater mortality declines for the poorest decile than for the wealthiest in the youngest female age group (age group 0–4), for both men and women at prime age (20–49), and for both men and women aged 50–64, although only statistically significant at 10 per cent. At prime age, men experience a stronger decline in inequalities than women. Our results suggest that, over the last two decades, absolute inequalities in mortality have been decreasing for the youngest age groups.

By contrast, we observe the opposite pattern for the oldest age groups. Even though all poverty groups experienced large reductions in mortality, the reductions are larger for the wealthier deciles. For instance, mortality for women aged 80+ in the poorest group dropped from 111 per 1,000 in 1996 to 94 per 1,000 in 2016, in comparison to a decline from 109 to 87 per 1,000 in the wealthiest group. As a result, the gap in age- and gender-specific mortality rates between these groups increased, which is reflected in the rising poverty gradient (column 9 in Table 2.2). This rise is statistically significant only for women.

2.4 Results: trends by cause-of-death groups

Figure 2.3 and Table 2.3 present the results by the cause-of-death categories: preventable, treatable, both or other (i.e. not preventable/treatable). We first discuss overall trends in mortality by age and gender following from Figure 2.3. This figure shows the absolute changes (decreases if below zero) in cause-specific mortality between 1996 and 2016 for each poverty decile. In addition, the fitted regression lines in Figure 2.3 illustrate whether the absolute changes in mortality per cause-of-death category between 1996 and 2016 were larger for the poor, larger for the wealthy, or equally distributed across poverty deciles. From this, we infer whether the changes in mortality inequality observed in Section 2.3 can be attributed to unequal changes in mortality from either preventable, treatable, both or other causes. These attributions can also be derived from Table 2.3. Similar to Table 2.2, Table 2.3 presents the poverty gradients for the years 1996 and 2016, and the difference between them for each cause-of-death category separately (columns 3, 6, 9 and 12). Note that these estimated differences are equal to the slope coefficients of the fitted regression lines in Figure 2.3. We excluded the youngest two age groups from the cause-of-death analyses as the numbers of deaths per cause category are too small for a reliable analysis.

Figure 2.3 shows reductions in mortality between 1996 and 2016 for almost all cause-of-death categories and age groups. The largest drops occurred in deaths from both preventable and treatable causes, which mainly cover deaths from cardiovascular diseases. Additionally, we find that the decreases in mortality were larger for men than women in every age group and for each category, except for treatable mortality in the age groups 20–49 and 50–64. Another difference between men and women is the evolution of preventable mortality, which seems to be decreasing for men in all age groups and increasing for women in the oldest three age groups.

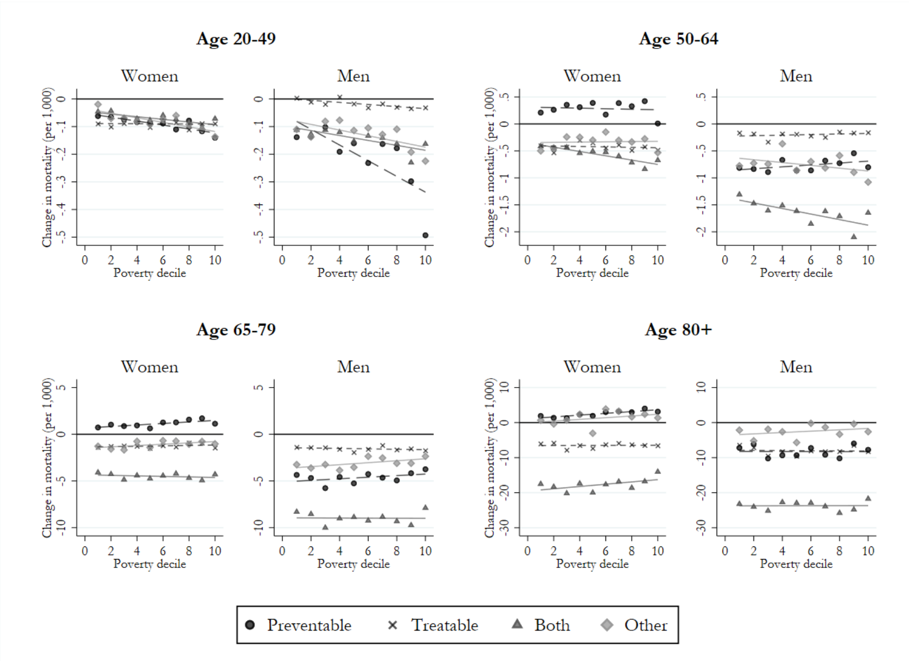
The results by poverty decile for the age group 20–49 indicate that, for both men and women, (almost) all cause-of-death categories that are statistically significantly, at 5 per cent, contributed to decreasing inequalities over time. For men in this age group, the decrease in preventable mortality between 1996 and 2016 was, in particular, much higher in poorer groups than in wealthier groups. This is reflected in a decrease in the poverty gradient of 0.028 (column 3 in Table 2.3), which can be interpreted as follows. The preventable mortality gap between the poorest and the least-poor decile fell by 0.28 deaths per 1,000 individuals between 1996 and 2016. Because this fall is larger than the decreases from the other cause-of-death categories (columns 6, 9 and 12), we argue that preventable mortality was the main contributor to the decrease in the gap in total mortality for men in the age group 20–49.

For the age group 50–64, however, the flattening of the poverty gradient of total mortality between 1996 and 2016 was mainly due to a decrease in mortality inequality from causes labelled both preventable and treatable (such as diabetes and cardiovascular diseases). In fact, the slope differences in Table 2.3 suggest that, for both men and women in the age group 50–64, only inequality in mortality from these causes statistically significantly decreased between 1996 and 2016. Furthermore, the magnitude of the difference in this category (–0.040 for women and –0.052 for men; see Table 2.3, column 9) is almost equal to difference of total mortality (–0.052 for women and –0.061 for men; see Table 2.2, column 9), indicating that the larger drop in deaths among the poor in this category drove the total decrease in mortality disparities in the age group 50–64.

There does not seem to be any dominant cause-of-death category driving the increasing inequalities in the oldest two age groups. For women in the age group 65–79, the rise in preventable mortality was higher among the poorest groups than among the wealthier groups. Along with increasing inequalities in mortality from other causes, this statistically significantly contributed to increasing inequalities in total mortality in this age group. For men, unequal changes in mortality from other (or potentially unavoidable causes) seem to have increased inequalities within this age group.

In addition, the results in Table 2.3 suggest that, for women in the oldest age group (80+), again unequal rises in preventable mortality contributed to increasing disparities at this age. However, also mortality from causes labelled both preventable and treatable and mortality from other causes seem to have played a role. Among men, it is mainly changes in mortality from other causes that are driving the results in column 9 of Table 2.2.

Figure 2.3: Change in mortality (1996–2016) across poverty deciles by gender, agegroup and cause of death



Note: This figure plots the difference in one-year mortality (smoothed over three years) between 1996 and 2016 across poverty deciles by cause-of-death category, gender and age group. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The slope coefficients of the fitted regression lines can be found in columns 3, 6, 9 and 12 in Table 2.3

Table 2.3: Poverty gradients in mortality by cause category, gender, age group and year

	Poverty gradients (slope coefficients fitted regression lines in Figures A1-A4) and differences in poverty gradients (equal to slope coefficients fitted regression lines Figure 2.2) by avoidable mortality category											
	Preventable			Treatable			Both (preventable and treatable)			Other		
	1996	2016	Δ 1996 2016	1996	2016	Δ 1996 2016	1996	2016	Δ 1996 2016	1996	2016	Δ 1996 2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Women												
Age 20-49	.015	.008	-.007	.001	.001	-.000	.006	.001	-.005	.011	.003	-.008
	(.001)***	(.001)***	(.002)***	(.001)	(.001)	(.002)	(.001)***	(.001)	(.002)***	(.003)***	(.001)**	(.003)***
Age 50-64	.070	.065	-.005	.013	.009	-.004	.059	.019	-.040	.046	.049	.003
	(.009)***	(.008)***	(.012)	(.007)*	(.004)**	(.008)	(.008)***	(.003)***	(.008)***	(.012)***	(.005)***	(.013)
Age 65-79	.148	.230	.081	.047	.069	.022	.145	.117	-.028	.158	.230	.071
	(.022)***	(.026)***	(.034)**	(.017)***	(.009)***	(.019)	(.040)***	(.010)***	(.041)	(.031)***	(.013)***	(.033)**
Age 80+	.113	.367	.254	.082	.089	.008	-.157	.170	.326	-.020	.221	.242
	(.050)**	(.039)***	(.063)***	(.075)	(.038)**	(.084)	(.154)	(.026)***	(.156)**	(.155)	(.112)**	(.191)
Men												
Age 20-49	.041	.013	-.028	.004	-.000	-.004	.013	.004	-.009	.018	.008	-.010
	(.007)***	(.002)***	(.007)***	(.001)***	(.001)	(.001)**	(.003)***	(.001)***	(.003)***	(.004)***	(.001)***	(.004)**
Age 50-64	.111	.129	.018	.008	.013	.005	.111	.059	-.052	.103	.077	-.026
	(.011)***	(.009)***	(.014)	(.005)*	(.004)***	(.006)	(.016)***	(.003)***	(.017)***	(.019)***	(.011)***	(.022)
Age 65-79	.182	.266	.085	.073	.051	-.022	.167	.161	-.006	.207	.312	.105
	(.050)***	(.029)***	(.058)	(.016)***	(.008)***	(.018)	(.063)***	(.019)***	(.066)	(.040)***	(.027)***	(.049)**
Age 80+	.253	.245	-.009	.066	.044	-.023	.251	.258	.007	.211	.403	.192
	(.145)*	(.067)***	(.160)	(.095)	(.037)	(.102)	(.176)	(.052)***	(.184)	(.141)	(.088)***	(.166)

Notes: Columns 1-2, 4-5, 7-8 and 10-11 report the estimated slope coefficients from a regression of mortality on poverty deciles, as plotted in Figures A1, A2, A3 and A4 in the Appendix, by cause category, sex, age-group and year. Columns 3, 6, 9 and 12 report the differences in these slope coefficients between 1996 and 2016. These differences are equal to the slope coefficients of the fitted regression lines plotted in Figure 2.3. Standard errors are between brackets.

* statistically significant from zero at 10%; ** statistically significant from zero at 5%; *** statistically significant from zero at 1%.

2.5 Discussion

Our study has documented substantial improvements in all-cause mortality in the Netherlands from 1996 to 2016. We find that age- and gender-specific mortality rates decreased for all groups between 1996 and 2016 regardless of municipal poverty share. While all groups benefited, improvements were largest among men, and particularly for those over the age of 65, thereby leading to a reduction in the gender gap in mortality. However, these mean mortality improvements mask very different inequality trends across age groups. Our results paint a different picture for the under-65s and the over-65s. On the one hand, absolute disparities in mortality decreased among those younger than 65 (that is, larger declines in mortality occurred in the poorest groups). On the other hand, poverty-related inequalities in mortality rates increased among older age groups: larger mortality drops were observed in the better-off deciles with lower poverty shares.

Our results for under-65s indicate that poverty gradients of mortality declined for the youngest age group (0–4), those in the prime age group (20–49) and those close to retirement (50–64). The decline in the gradient was generally greater for women, except for the prime age group in which the reduction among men dominated that among women. This shows that even though the gender gap in mortality was reduced from 1996 to 2016, poverty gaps in mortality did not fall as much for men as for women. We observed no change in disparities for the children/adolescent group (5–19), which is not surprising as it is the age range with lowest mortality rates across the life cycle. Among those aged 65 and older, the inequality rise was present for all age groups, but largest among women aged 80 and older, and imprecisely estimated for men older than 80. Taken together, our findings suggest that trends in mortality inequality over time differ across age groups.

Our results are in part in line with those from Currie and Schwandt (2016) in the United States between 1990 and 2010: they found a strong reduction in mortality inequalities for individuals below the age of 20, but rising inequalities for individuals at older ages. However, where the ‘good news’ on inequality reduction in the United States derives from these young groups only, we also find reductions in inequalities for those in prime age and near retirement. This highlights that, in contrast to the United States, Dutch mortality inequalities improved across a much wider range of pre-retirement ages.

We further analyse the poverty gradients of mortality by potentially avoidable causes of death (preventable, treatable, both) and other causes of death that are, according to the OECD/Eurostat (2019) definition, not classified as avoidable. Deaths below the age of 20 were too infrequent for a meaningful analysis of changes in the poverty gradients by causes of deaths. Analysis for those between 20 and 64, however, revealed that the improvements in all-cause mortality gradients were driven by all four categories, but mostly by trends in preventable mortality and by both preventable and treatable causes of death. Especially, the contribution of preventable mortality is worth noting as these causes of death did not show the largest average mortality drop between 1996 and 2016 (see the estimated intercepts in Table A2 in the online Appendix). Among older ages, the role of the avoidable causes of death was less straightforward as the 'other' category played a dominant role, but women nevertheless experienced a substantial increase in the poverty gradient due to preventable causes of death.

Overall, the analysis by cause of death points to the importance of lifestyle interventions and cardiovascular treatments for those below the age of 65. The gender difference in the contribution of preventable (and treatable) causes of death for older ages is in line with differences in the life-cycle profile of lifestyle, such as, for instance, smoking prevalence: in the considered period, this was still increasing among women but declining or stable among men. Another potential factor is the faster growing access to preventive cardiovascular medication amongst men (Mackenbach et al., 2011). We speculate that the larger mortality reductions among women in low-poverty versus high-poverty groups are related to the importance of different phases of lifestyle prevalence for men and women.

The finding of differential results for age groups implies that any composite population measure, such as life expectancy, could fail to unravel the more nuanced patterns that may have occurred in different age groups. Previous studies for the Netherlands have not been able to distinguish these patterns due to their use of a general population summary measure. Additionally, our use of poverty shares allows us to maintain equally sized deciles across the study period, which is not feasible for studies relying on education as a socio-economic status stratifier (Kulhánová et al., 2014; Mackenbach and Nusselder, 2019). Finally, another strength of the study is the use of administrative records for the whole population of the Netherlands, which provides a more representative estimate than previous studies that used samples.

There are some limitations to acknowledge. First, as inequalities are likely to exist not only between, but also within municipalities, we expect that aggregation to these geographical units may underestimate inequalities observed at a lower geographical, or individual, level. Moreover, because municipalities with fewer inhabitants are generally less poor, we expect a stronger underestimation of inequalities among poorer municipalities. Second, our analysis of mortality gradients by cause of death, which relies on the OECD classification of avoidable mortality, is less valid for deaths occurring after the age of 75. This limitation may, to some extent, explain why we find no clearly discernible pattern amongst the oldest age group (OECD/Eurostat, 2019). Given the limitations of this (inevitably somewhat arbitrary) classification scheme, it is difficult to fully explain the observed 75+ mortality patterns. One possibility is that death beyond the age of 75 was mostly random in 1996, but that it has become more socio-economically patterned since then. Another possible reason for the non-discernible pattern could be that the relative importance of causes of death changes for this older age group. In any case, the limitation of this classification scheme may be more severe for deaths after the age of 75 and it hampers the interpretation of those results.

A third limitation of our study is the focus on absolute inequalities. While our study provides strong evidence of how absolute inequalities in mortality have evolved, the picture is likely to be different for relative inequalities. In fact, it has been argued recently that aiming for a reduction of relative inequalities is very difficult when overall rates of mortality are declining (Mackenbach et al., 2016). As such, the focus on absolute inequalities in mortality may avoid this 'inconvenient truth', which to many has also appeared as a 'frustration'. As one colleague eloquently put it recently: 'When we focus on absolute inequalities, tackling health inequalities will no longer be "swimming against the current", but will be like "riding the waves". (Mackenbach, 2020)' As such, our focus on absolute inequalities may be considered a strength, not a limitation.

2.6 Conclusion

Overall, our findings highlight that while the Netherlands has witnessed important improvements in overall mortality – and therefore life expectancy – between 1996 and 2016, not all age and gender groups have benefited equally, and neither did they all experience the same changes in the mortality–poverty association. We show that large reductions in mortality inequalities have occurred for those below the age of 65. Our findings suggest that most of those improvements are probably linked to both health care and lifestyle improvements trickling down to the poorer classes, as suggested by the differential mortality gradients by cause of death. By contrast, mortality inequalities for the oldest group have grown. This highlights the fact that either there has been a shift of inequalities from younger to older ages (that is, a survival effect) or, regardless of the accessibility of the Dutch health care system, older individuals in wealthier areas of the Netherlands have benefited more from health improvements. If the latter explanation holds, it provides a strong motivation for further research to understand what has caused these improvements concentrated among the wealthier, and how to make systematic adjustments in order to enable equivalent mortality reductions in the less-advantaged socio-economic groups.

Chapter 3

Has COVID-19 increased inequality in mortality by income in the Netherlands?

With Bram Wouterse, Joana Geisler and Eddy van Doorslaer

Published in Wouterse B., Geisler, J., Bär, M. and van Doorslaer, E. (2023), Has COVID-19 increased inequality in mortality by income in the Netherlands? *Journal of Epidemiology & Community Health, 77*, 244-251.

Abstract

Background In the Netherlands in 2020, COVID-19 deaths were more concentrated among individuals with a lower income. At the same time, COVID-19 was a new cause that also displaced some deaths from other causes, potentially reducing income-related inequality in non-COVID deaths. Our aim is to estimate the impact of the COVID-19 pandemic on the income-related inequality in total mortality and decompose this into the inequality in COVID-attributed deaths and changes in the inequality in non-COVID causes.

Methods We estimate excess deaths (observed minus trend-predicted deaths) by sex, age and income group for the Netherlands in 2020. Using a measure of income-related inequality (the concentration index), we decompose the inequality in total excess mortality into COVID-19 versus non-COVID causes.

Results Cause-attributed COVID-19 mortality exceeded total excess mortality by 12% for the 65–79 age group and by about 35% for 80+ in the Netherlands in 2020, implying a decrease in the number of non-COVID deaths compared with what was predicted. The income-related inequality in all-cause mortality was higher than predicted. This increase in inequality resulted from the combination of COVID-19 mortality, which was more unequally distributed than predicted total mortality, and the inequality in non-COVID causes, which was less unequal than predicted.

Conclusion The COVID-19 pandemic has led to an increase in income-related inequality in all-cause mortality. Non-COVID mortality was less unequally distributed than expected due to displacement of other causes by COVID-19 and the potentially unequal broader societal impact of the pandemic.

3.1 Introduction

Reports from across the world suggest that existing inequalities (Banks et al., 2021) in mortality between socioeconomic groups have been exacerbated by the COVID-19 pandemic (Yaya et al., 2020; Bambra et al., 2020; Baqui et al., 2020; Drefahl et al., 2020; Arceo-Gomez et al., 2022). Also in the Netherlands in the first 4 months of the pandemic, the risk of dying from COVID-19 in 2020 for people over 70 was twice the risk for the lowest income quintile compared with the highest income quintile (Stoeldraijer et al., 2022).

The way in which pre-existing socioeconomic inequalities in health have shaped inequalities in total mortality during the COVID-19 pandemic is still unclear. On the one hand, the poorer mean health of individuals in lower socioeconomic groups may make them more susceptible to adverse health outcomes, deriving from the incidence of COVID-19 itself and resulting from the societal response to the virus such as delays in care provision (Kontopantelis et al., 2022). On the other hand, the concentration of prior health conditions related to COVID-19 mortality, such as overweight, diabetes, chronic obstructive pulmonary disease (COPD) and heart disease (Jordan et al., 2020), among groups with a lower socioeconomic status (Arceo-Gomez et al., 2022; Sepulveda and Brooker, 2021) may have reduced the inequality in deaths by other causes: a relatively large share of individuals with a low socioeconomic status who died from COVID-19 would otherwise have died in the same period from another cause.

The aim of our study is to estimate the impact of the COVID-19 pandemic on the income-related inequality in total mortality and decompose this impact into the inequality in COVID-attributed deaths and changes in the inequality in non-COVID causes. We do this for the Netherlands in 2020 using individual linked microdata for the entire population, measuring income-related inequality using a concentration index.

3.2 Data and methods

3.2.1 Data sources and sample

We aggregate data on deaths and population counts per age, sex and income group for the years 2015 up to 2020 based on individual-level data for the entire population made available by Statistics Netherlands. Our data come from three administrative sources that can be linked through the (anonymised) citizen service number (BSN): (1) sex and date of birth and death of individuals from municipal registries; (2) cause of death (COD) from death registries; and (3) household income data from tax registries. We restricted our attention to four age-sex groups (men and women at ages 65–79 and 80+) that together account for the large majority (94%) of COVID-19-related deaths in the Netherlands. Although the relative inequality in COVID-19 mortality below the age of 65 might be substantial, the absolute number of COVID-19 deaths in that age group is too low (1054 in total) to expect any meaningful effects on the inequality in total mortality.

3.2.2 Deaths by cause

Data on causes of death were derived from the death certificates. The causes of death reported on the certificates are converted into International Classification of Diseases 10th Revision (ICD-10) codes by Statistics Netherlands, based on WHO guidelines (WHO, 2020). The underlying cause may be difficult to identify for patients with a COVID-19 infection and comorbidities like COPD. However, by performing body autopsies, Elezkurtaj et al. (2021) found that the majority of such patients indeed died from lung damage caused by COVID-19 rather than by other comorbidities. Our data only contain the underlying cause and not the other causes from which the underlying cause was inferred.

We classify deaths as resulting from COVID-19 using ICD-10 codes U07.1 (virus identified) and U07.2 (virus not identified, but probable or suspected) based on primary COD data. About 13% of classified COVID-19 deaths are based on the latter code.

The non-COVID-19 causes of death are divided into circulatory, respiratory, cancer, mental and other causes. We chose these four plus 'other' because they have been identified as being affected by the pandemic (Kontopantelis et al., 2022, 2021).

3.2.3 Income groups

We use disposable annual household income (total household income net of taxes and income transfers, adjusted for household size) for 2019 to determine income groups. Those living in the Netherlands in 2020 and without an observation of full-year income in 2019 are excluded. For every age-sex group and year, we rank all individuals by their household income and divide them into 20 groups — or ventiles - of equal size. To control for potential (within age-sex group) differences in the age composition across income groups and over time, we use population weights based on 1 year age groups on 1 January 2020 within each age-sex group when calculating mortality.

3.2.4 Measurement of mortality

To determine what the mortality in 2020 would have been in the absence of COVID-19, we compute trend-predicted mortality probabilities ($M^{pred}_{a,s,i,2020}$) in 2020 by estimating linear trends for each age, sex and income group for the years 2015–2019, and then predict the mortality probabilities in 2020. By subtracting trend-predicted from observed total mortality, we obtain an estimate of total excess mortality for each age a, sex s and income i group: $M^{exc}_{a,s,i} = M^{obs}_{a,s,i} - M^{pred}_{a,s,i}$. Total excess mortality is the sum of COVID-19 mortality ($M^{cov}_{a,s,i}$) and excess mortality in all J other causes ($\sum_{j=i}^{J}\left[M^{obs}_{a,s,i,j} - M^{pred}_{a,s,i,j}\right]$).

This gives the decomposition that we will use throughout the paper (also see Appendix

Figure B1):

$$M^{obs}_{a,s,i} = M^{pred}_{a,s,i} + M^{cov}_{a,s,i} + \sum_{j=i}^{J} \left[M^{obs}_{a,s,i,j} - M^{pred}_{a,s,i,j} \right]. \tag{3.1}$$

Excess mortality in other causes can be either negative or positive. If total non-COVID excess mortality is negative, this suggests that COVID-19 has partly displaced other causes: individuals who died from COVID-19 would otherwise have died from another cause in 2020. However, the pandemic, and the containment policies following it, may also have influenced other cause mortality indirectly through other channels, like delayed non-COVID care in hospitals (Birkmeyer et al., 2020).

3.2.5 Measurement of income-related inequality in mortality

To measure the degree of relative inequality in mortality by income, we use the concentration index which is based on the association between mortality and income rank (Mackenbach and Kunst, 1997; Wagstaff et al., 1991). This measure is proportional to the relative index of inequality (Wagstaff et al., 1991). The concentration index is defined as (twice) the area between the concentration curve, which depicts the cumulative distribution of deaths as a function of the cumulative proportion of the population ranked by income, and the line of perfect equality. The concentration index C lies between -1 and 1 and is negative when mortality is more concentrated among the lower income groups and positive in the opposite situation. The concentration indices are computed using the CONINDEX command in STATA (O'Donnell et al., 2016).

To quantify how relative inequality has changed during the first year of the COVID-19 pandemic we compare the concentration index of observed total mortality to that of predicted mortality. We also compare the inequality in predicted mortality to the inequality in COVID-19 mortality to assess whether COVID mortality is more, or less, unequally distributed than total mortality in prepandemic times.

3.2.6 Decomposition of inequality by COD

Using Equation (3.1), we can decompose the concentration index for observed mortality into a weighted sum of its underlying components:

$$\underbrace{C(M^{obs}_{a,s}) - W^{pred}_{a,s} C(M^{pred}_{a,s})}_{\text{Inequality in excess mortality in 2020}} = \underbrace{W^{cov}_{a,s} C(M^{cov}_{a,s})}_{\text{Inequality contribution of COVID}} + \underbrace{\sum_{j=1}^{J} [CIC(M_{a,s})^{j}]}_{\text{Inequality contribution of other causes}} \tag{3.2}$$

with $CIC(M^{j}_{a,s}) = w^{obs}_{a,s} C(M^{obs}_{a,s}) - w^{pred}_{a,s} C(M^{pred}_{a,s})$ and the weights w for each component being the shares of deaths relative to the total number of observed deaths.

The left-hand side term measures the inequality in excess mortality in 2020, or, in other words, how much more unequal mortality has become compared with what was expected based on the trend. The right-hand side shows the contributions to inequality in excess mortality as the weighted sum of inequality in COVID-19 mortality and the inequality contributions of each of the J causes of death. The last term is sum of the 'inequality contributions' of each COD. The decomposition allows us to quantify the impact of COVID-19 on the inequality in total mortality and the extent to which other causes have lowered or raised it. The derivation of Equation (3.2) is found in supplemental appendix B.

3.3 Results

3.3.1 Mortality trends

Figure 3.1 shows the observed total mortality probabilities (per 1,000) for 2020 by age group (65–79; 80+) and sex, and compares these to 2020 trend predictions based on observed mortality in 2015–2019. For all groups, observed total mortality in 2020 is above the trend-predicted levels and outside of the 95% confidence interval of the prediction. The absolute difference between observed and predicted mortality — the excess mortality probability — is larger for older age groups.

Figure 3.1 also shows non-COVID-19 mortality in 2020. Mortality from other causes always lies below the predicted mortality probability, especially in the older age group.

Figure B2 in supplemental appendix B shows the observed, trend-predicted and non-COVID mortality probabilities in 2020 for each age-sex group and income ventile. Appendix tables B1 - B4 report the underlying observed and predicted mortality probabilities. For all groups, the observed mortality exceeds predicted mortality, indicating that all groups suffered from increased mortality due to the pandemic. Moreover, mortality from non-COVID causes generally lies below trend-predicted mortality.

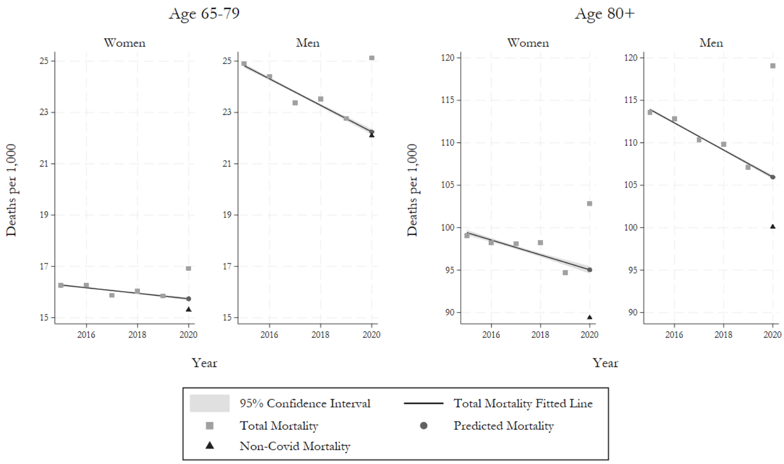
3.3.2 Excess mortality by COD

Figure 3.2 shows how observed mortality in 2020 for each cause differs from the trend-based prediction, by age group and sex. For men and women aged 65–79, excess deaths consist mostly of COVID-19 deaths and—to a lesser extent—of circulatory disease causes, while we observe negative excess deaths from respiratory and mental causes. For those over 80, observed deaths for all non-COVID-19 causes are below the predicted levels.

3.3.3 COVID-19 and income-related inequality in mortality

Figure 3.3 shows the concentration curves for predicted all-cause mortality and for COVID-19 mortality in 2020, again by age-sex group. All curves lie above the diagonal (i.e., mortality is more concentrated among the poor). However, COVID-19

Figure 3.1



Note: Mortality trends before and during 2020, women and men. The y-axis represents mortality probabilities in terms of deaths per 1000: the number of individuals who died during the year as a share of the individuals at the start of the year. The x-axis is the years, and the solid line is the estimated trend based on the observed mortality probabilities between 2015 and 2019 (including 95% confidence intervals), the squares are observed total mortality probabilities in 2015–2020, the circle is trend-predicted mortality and the triangle is non-COVID mortality in 2020. If observed total mortality (square) in 2020 exceeds trend-predicted (triangle) mortality, we conclude that there is excess mortality. If at the same time non-COVID mortality (triangle) is lower than trend-predicted (circle) mortality, this signals substitution.

mortality probabilities deviate further from the 45-degree line than predicted mortality probabilities. This indicates that COVID-19-attributed deaths are more concentrated among the lower income groups than predicted all-cause deaths. Second, relative inequality in both predicted and COVID-19 mortality is higher for the younger (under 80) than for the older (over 80) age groups.

3.3.4 Decomposing the inequality consequences of the emergence of COVID-19 mortality

The CIs reported in the top row of Table 3.1 confirm that the pandemic has contributed to an increase in mortality inequality: the *observed* all-cause mortality is more unequally distributed than the *predicted* all-cause mortality. This implies that excess mortality in 2020 resulted in a more unequal distribution of deaths than predicted (see the CI of excess mortality in Table 3.1, which is more negative than the CI of predicted all-cause mortality).

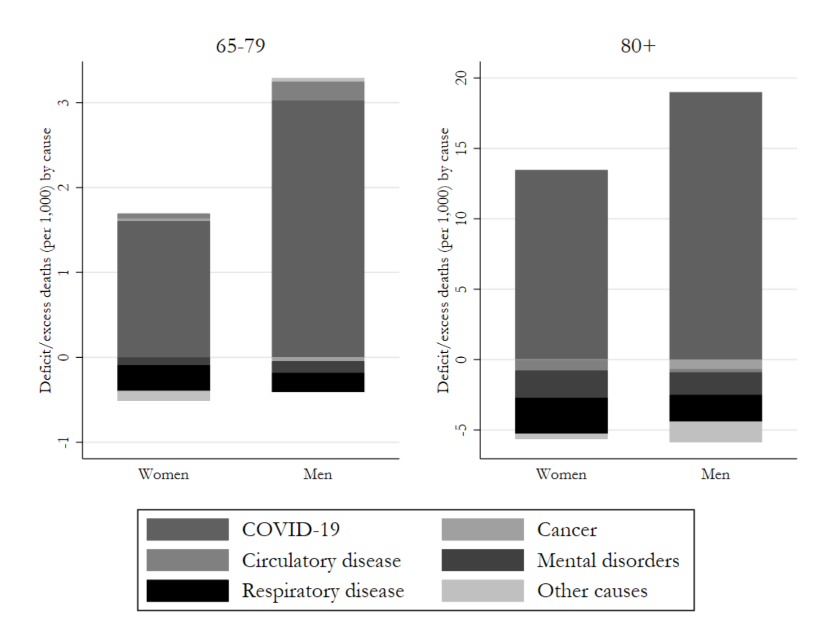
In Table 3.1, total inequality in excess mortality is decomposed into the inequality contribution of the new cause (COVID-19) and the inequality contribution of excess mortality for non-COVID causes. Supplemental appendix table B5 provides more detailed

Table 3.1: Decomposition of excess mortality by cause of death

	Women		Men	
	65–79	80+	65–79	80+
CI observed total mortality	–0.192	–0.0966	–0.176	–0.0792
CI predicted mortality	–0.189	–0.0911	–0.165	–0.0723
CI excess mortality	–0.238	–0.164	–0.256	–0.135
SE	0.106	0.0639	0.0506	0.031
Share of total observed deaths	0.0698	0.076	0.115	0.110
Contribution to CI total mortality	–0.0166	–0.0124	–0.0293	–0.0149
COVID-19				
CI observed mortality	–0.326	–0.164	–0.236	–0.143
Share of total observed deaths	0.096	0.131	0.122	0.159
Contribution to CI excess mortality	–0.031	–0.0215	–0.0284	–0.0227
Circulatory diseases				
CI predicted mortality	–0.232	–0.0749	–0.209	–0.0672
CI observed mortality	–0.224	–0.0754	–0.187	–0.0673
Share of total observed deaths	0.175	0.258	0.205	0.245
Contribution to CI excess mortality	0.000553	0.000404	0.00273	0.0000997
Respiratory diseases				
CI predicted mortality	–0.334	–0.146	–0.312	–0.154
CI observed mortality	–0.323	–0.141	–0.323	–0.128
Share of total observed deaths	0.072	0.058	0.063	0.076
Contribution to CI excess mortality	0.00681	0.00396	0.00212	0.00441
Mental disorder mortality				
CI predicted mortality	–0.278	–0.142	–0.259	–0.117
CI observed mortality	–0.263	–0.134	–0.294	–0.106
Share of total observed deaths	0.040	0.119	0.030	0.077
Cause-specific contribution to CI	0.00211	0.00355	0.000377	0.00237
Cancer mortality				
CI predicted mortality	–0.107	–0.034	–0.0987	–0.0297
CI observed mortality	–0.103	–0.0253	–0.109	–0.0351
Share of total observed deaths	0.411	0.145	0.38	0.213
Cause-specific contribution to CI	0.00149	0.00128	–0.00364	–0.000973
Other mortality				
CI predicted mortality	–0.233	–0.0951	–0.18	–0.0681
CI observed mortality	–0.224	–0.0968	–0.191	–0.0635
Share of total observed deaths	0.205	0.29	0.2	0.229
Cause-specific contribution to CI	0.00346	–0.000133	–0.00251	0.0019

Notes: This table summarises the decomposition of total mortality inequality in 2020 into causes of death categories. Mortality inequality is measured using the concentration index (CI). Deaths are divided into six causes: COVID-19, circulatory deaths, deaths from mental disorders, cancer deaths, respiratory disease deaths and other causes. For the full table, see Table B5 in the supplemental appendix.

Figure 3.2



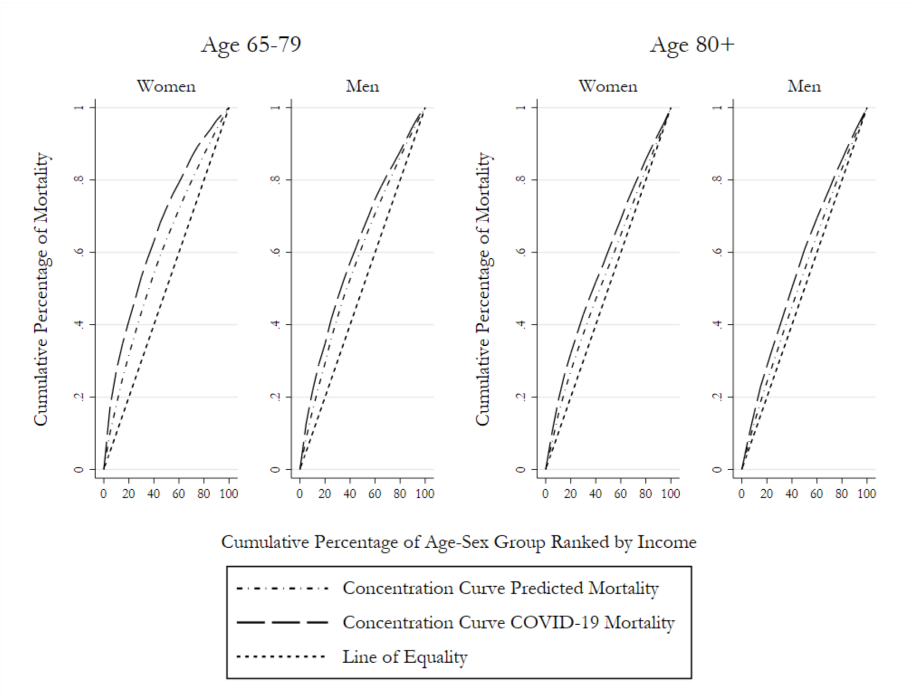
Note: Excess mortality decomposition by causes of death, by four age-sex groups. This figure illustrates the deficit/excess in deaths by causes in 2020 in the Netherlands. For circulatory disease, respiratory disease, cancer, mental disorders and remaining causes, the deficit/excess in mortality is calculated as the difference between predicted and observed values. Since predicted mortality for COVID-19 is zero, all COVID-19 deaths are excess.

results. Four observations can be made. First, in all demographic groups, COVID-19 mortality is more concentrated among lower income groups (concentration indices more negative) than predicted all-cause mortality.

Second, while an important COD for all demographic groups, COVID-19 deaths represent only a minor share of all deaths (between 9.5% and 16%), which implies that the impact of inequality in COVID-19 mortality on inequality in all-cause mortality is limited.

Third, inequality in observed mortality from specific non-COVID causes is often smaller than predicted (the CI is less negative). For the causes for which observed deaths are lower than predicted (e.g., respiratory diseases and mental disorders, see Figure 3.2), this implies that the lower income groups experienced a larger reduction in deaths from these causes (compared with predicted) than the higher income groups. For example, the CI for predicted deaths from respiratory diseases for men in the over 80 group is −0.154, while the CI for observed deaths is −0.128 indicating a less unequal distribution stemming from the fact that a relatively large number of deaths from respiratory

Figure 3.3



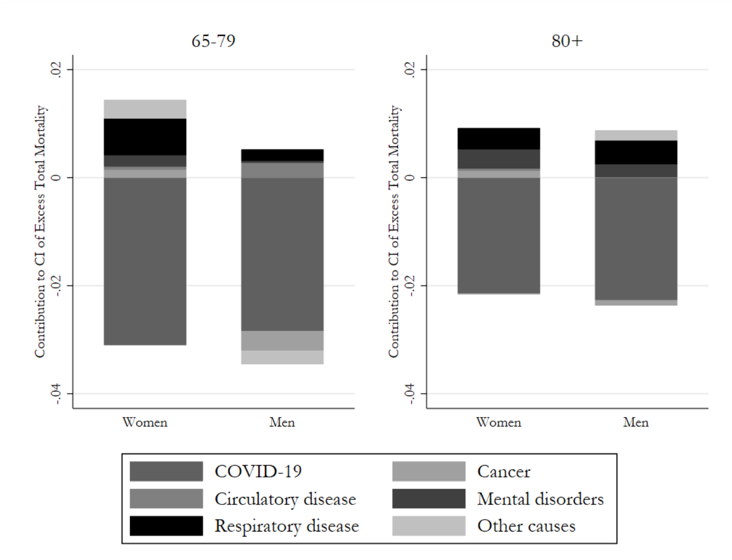
Note: Concentration curves of predicted and COVID-19 mortality. The concentration curves underlie the concentration indices (twice the area under the concentration curve), which are used to measure income-related inequalities for each of the age-sex groups. The x-axes represent the cumulative percentage of the population (ranked by income ventile). The y-axis represents the cumulative percentage of either predicted mortality (blue line) or COVID-19 mortality (red line). A concentration curve above the 45-degree line (green)—or line of equality—implies that mortality is more concentrated among the lower income ventiles, and a curve further away from the 45-degree line implies a higher degree of inequality.

diseases were displaced among the lower income groups.

Fourth, inequality in total (all-cause) excess mortality is a weighted combination of the inequality in the new cause COVID-19 and the inequality in (negative) excess mortality in non-COVID causes. Figure 3.4 shows the inequality contribution (CIC) of each cause as a percentage of the inequality in total excess mortality. The contribution of COVID-19 to the CI in excess mortality is generally larger than 100%, indicating a pro-poor distribution of (negative) excess mortality in other causes. Respiratory diseases and mental disorders (and cancer for the under 80 group) have the largest negative weights, indicating that the reductions in the inequality in these causes have the largest moderating impact on the inequality in total excess mortality.

For three of the demographic groups, the results are very similar: all other causes than COVID-19 show a positive inequality contribution to excess mortality, meaning that

Figure 3.4



Note: Cause of death contributions to inequality in excess mortality. This figure shows the weighted percentage contributions of six causes of death to the inequality in excess mortality. Inequality is measured using a concentration index of mortality by income and each contribution is the product of the weight (shares) and the concentration for mortality of each cause of death. The percentage is calculated by taking the ratio between the contribution of each cause and excess mortality.

the inequality in deaths from these causes is lower (i.e., a less negative concentration index) than predicted. There are only two instances where other causes have added to the inequality-increasing effect of COVID-19: cancer deaths for men in both age groups and other causes for men aged 65–79. Finally, only for men aged 65–79, the largest inequality-enhancing cause (after COVID-19) is circulatory diseases.

3.4 Discussion

In the Netherlands, COVID-19 mortality exceeds excess mortality in 2020 for all sex, age and income groups. Depending on the age group, the number of excess deaths is between 5% and 42% lower than the number of COVID-19 deaths, which implies an equally large decrease in deaths from non-COVID causes such as respiratory diseases and mental disorders. COVID-19 deaths are strongly concentrated among the low-income groups. At the same time, deaths from non-COVID causes are often less unequal than predicted.

The difference between excess and cause-attributed COVID-19 mortality

First, our results suggest that the net effect of the COVID-19 pandemic on mortality was smaller than the number of people registered as dying from COVID-19. This finding is in line with the numbers reported by Statistics Netherlands for the entire population: in 2020, over 20 000 COVID-19 deaths were registered in the death certificates while the number of excess deaths was estimated at 15,000 (Statistics Netherlands, 2022b).

However, findings for other countries concerning the difference between excess mortality and cause-attributed COVID-19 mortality are mixed (WHO, 2022; Wang et al., 2022; Stang et al., 2020; Woolf et al., 2021). These differences could in part reflect real differences in prior health of the population and in the societal responses to the pandemic. A direct comparison is difficult, however, because of differences in the time periods and the definition of COVID-19 mortality used in these studies.

The pandemic's impact on mortality inequality

Second, the relative inequality in COVID-19 mortality favouring the rich is considerably larger than inequality in total mortality in non-pandemic years. Bearing in mind the methodological challenges of cross-study comparisons, similar socioeconomic gradients in COVID-19 mortality were reported by some, studying either inequality in COVID-19 deaths or in excess mortality (Davies et al., 2021; Brant et al., 2020; Krieger et al., 2020; Decoster et al., 2021), although others have found the COVID-19 gradients to be broadly similar to all-cause gradients (Barnard et al., 2021).

Using the concentration index enables us to quantify these differences in relative inequality: the negative values of the concentration index for COVID-19 are between 30% and 80% larger (ie, more negative) than those for predicted total mortality.

Third, the inequality in deaths from non-COVID causes is often lower than predicted. The most likely explanation is that displacement of other causes by COVID-19 occurred more often among the lower income groups. However, other (indirect) effects of the pandemic (eg, delayed treatments, lockdowns) might also have had an impact. The reduction in inequality in non-COVID causes varies by sex and age: for all groups, except men between ages 65 and 79, the largest decrease in inequality (in terms of the contribution to inequality in all-cause excess mortality) is for respiratory diseases and mental disorders. For men aged 65–79, the largest decrease in inequality is observed for circulatory diseases. For men in both age groups, an increase in inequality is observed for cancer deaths. The differences in the impact of changes in non-COVID mortality on inequality across demographic groups may arise for several reasons that we cannot explore: differences in underlying conditions, in the ability and efforts to protect oneself from COVID-19 or in the effects of the societal response to the pandemic.

Studies for other countries have found diverging socioeconomic gradients for deaths from non-COVID causes during the pandemic (Stang et al., 2020; Woolf et al., 2021; Brant et al., 2020; Michelozzi et al., 2020). There is limited comparability to these other studies due to the methodological issues in terms of observation period, identification of COVID-19 deaths and differences across countries in the mortality trends prior to 2020. A relatively similar study by Kontopantelis et al. (2021) has estimated excess deaths due to COVID-19 and other causes in England and Wales by sex and geographical region during the COVID-19 pandemic in 2020. Like us, they find negative excess deaths in the 65+ age group for other respiratory causes and (for most groups) for cancer. Unlike us, they find positive excess mortality for cardiovascular disease and other causes. They also find, like our results, that more deprived quintiles report higher rates of excess deaths.

Limitations

The distinction between cause-attributed COVID-19 mortality and excess mortality that we use throughout the paper relies on the quality of the COD coding. While there have been serious efforts to internationally harmonise the COD registrations, it is not impossible that in practice some differences across countries remain. We followed the European Centre for Disease Prevention and Control recommendation by including both test-confirmed and suspected cases of COVID-19 in mortality statistics. The registration of COVID-19 deaths throughout the year may have depended on the testing policy: while severe/hospitalised cases and individuals in certain professions were tested from the start of the pandemic, testing only became available for the full population after 1 June (McDonald et al., 2022; Hoekman et al., 2020). Similarly, estimates of excess mortality depend crucially on the predictions based on historic trends in deaths. Methodological differences in estimating these trends have been shown to impact the estimates of excess mortality during the COVID-19 pandemic (Barnard et al., 2021). Although our approach could also be applied to other countries, differences in the reporting and coding of causes of deaths will complicate comparisons.

Moreover, we only observe the main COD and not the other contributory causes. In (part of) the cases where COVID-19 resulted in the sequence leading to death, the contributory causes could well be the ones we find to be displaced by COVID-19 in our analysis.

Our analysis is limited to the first year (2020) of the pandemic, as at the time of the research data for 2021 were not yet available. It is quite likely that the effects on inequality may have been different in the second year of the pandemic, when the roll-out and uptake of both tests and vaccines, and the predominance of different variants of COVID-19 may have had a different impact on mortality inequality.

Finally, we have not considered other short-term or long-term effects of a COVID-19 infection on health beyond mortality. More specifically, we have used annual mortality in 2020 as our main outcome, which ignores any long-term effects on mortality. Alternatively, one could estimate the years of life lost due to COVID-19 across income groups taking the prior health status of those who died of COVID-19 into account.

3.5 Conclusion

We find that the COVID-19 pandemic was not the ‘Great Unequalizer’ (Alsan et al., 2021), but it did further increase the already long-existing inequalities in mortality by income in the Netherlands. To some extent, COVID-19 deaths have displaced deaths from other causes that were expected to be distributed unequally, but the displacement was not enough to leave the total socioeconomic inequality unaltered: total mortality inequality (encompassing all causes including COVID-19) was still larger than expected based on historic trends. This finding highlights the importance of equity concerns for next pandemics. If future pandemic response policies are to avoid such a rise in inequality, greater investments in the pro-poor targeting of testing, vaccination and other interventions may be required.

Part II

Efficiency in nursing home care

Chapter 4

Estimating the health value added by nursing homes

With Pieter Bakx, Bram Wouterse & Eddy van Doorslaer

Published in Bär, M., Bakx, P., Wouterse, B., & van Doorslaer, E. (2022). Estimating the health value added by nursing homes. *Journal of Economic Behavior & Organization, 203*, 1-23.

Abstract

Measuring performance in healthcare remains a challenge. The use of health outcomes rather than structure and process indicators is considered as the way forward, but outcome-based results risk being biased by selection. Accounting for such selection bias is more difficult in settings with small-sized providers and low chances of resident health improvement, like in the case of nursing homes. In this paper we (i) measure the health outcomes of Dutch nursing homes in terms of mortality and avoidable hospitalizations among residents, (ii) we adopt a novel approach to test for selection bias and (iii) we examine the relationship between outcomes and other nursing home quality indicators and characteristics. Using administrative data from more than 110,000 residents, we estimate the performance of the 849 largest nursing homes in the Netherlands in the period 2015-2019. Controlling for an extensive set of observable case-mix variables, we first test for the presence of selection bias using a distance-based instrumental variable. We do not find any evidence for such a structural bias. While the wide confidence intervals of the estimates display considerable imprecision, our results do reveal substantial differences between top and bottom performing nursing homes. Because the outcome-based estimates turn out to be only weakly correlated with other quality indicators, we conclude that our mortality and avoidable hospitalization-based indicators provide important complementary information. When small sample issues and case-mix differences are adequately accounted for, outcome-based indicators can provide useful policy guidance for quality improvement in nursing homes.

4.1 Introduction

Continued increasing demand and limited supply in nursing home care might reduce incentives for nursing homes to improve quality (Ching et al., 2015; Nyman, 1988). To stimulate quality improvements it is therefore important to inform consumers and policy makers by evaluating their performance on a regular basis. While outcome-based measures are commonly applied to enhance performance in other types of healthcare institutions, like in pay-for-performance schemes for hospitals, nursing home care is still primarily evaluated on the basis of structure (e.g. staffing) or processes of care (e.g. use of psychotropic drugs) in most countries (Barber et al., 2021). Since the relationship between structure, processes and outcomes of care is far from straightforward (Donabedian, 2003), it is meaningful to complement such indicators with outcome-based measures. The challenges in doing so for nursing homes are that appropriate outcomes are less easily defined, one has to rely on self-reported measures and small sample sizes, and that - similar to other sectors - it is uncertain whether quality differences persist after correction for observable case-mix differences.

In this paper, we study whether residents' health outcomes may be used to evaluate performance of nursing homes. We examine i) how much variation in health outcomes there is across nursing homes; ii) whether this can be attributed to differences in performance rather than to differences in unobserved resident characteristics; and iii) the association of structure and process-based quality indicators with those. We use administrative data from over 110,000 nursing home admissions in the Netherlands linked to data on mortality and avoidable hospitalizations and background characteristics, to estimate the health-value added of each of the 849 largest Dutch nursing homes. The identification of these nursing-home-specific effects is complicated by the fact that residents with high or low unobserved health might self-select into particular homes. We address this econometric challenge by testing whether our case-mix corrected estimates can accurately predict the outcomes for (quasi-)randomly admitted residents (i.e. those admitted to the nursing home closest to their prior residence[1]). Finally, we correlate the outcome-based performance estimates to other quality indicators to verify whether structure and process indicators can explain variation in outcomes to improve understanding of the potential mechanisms involved.

Our paper makes the following contributions to the literature. First, it extends the economic value-added literature by demonstrating that by estimating a forecast coefficient when exploiting exogenous variation in provider choice, the value-added framework can still be meaningfully employed to test for the presence of selection bias, even when sufficient power to include individual instruments for each provider is lacking.[2]

[1]Geographical distance is an important determinant of nursing home choice and unlikely to be related to outcomes (other than through nursing home choice). Since Newhouse and McClellan (1998), this instrument has been used in many other settings in health and nursing home care (Cornell et al., 2019; Geweke et al., 2003; Gowrisankaran and Town, 1999; Grabowski et al., 2013; Helsø et al., 2019; Huang and Bowblis, 2018).

[2]This alternative test also stems from the education literature (Angrist et al., 2016; Chetty et al., 2014; Deming, 2014; Kane and Staiger, 2008), and has more recently been applied to the healthcare sector (Abal-

We apply the value-added framework to evaluate the presence of selection bias in performance on outcomes of relatively small entities like nursing homes. The existing value-added literature mainly focuses on larger organizations, like schools, hospitals, skilled nursing facilities or insurance plans, for which there is sufficient exogenous variation to estimate the causal impact and bias for each entity separately (Abaluck et al., 2021; Angrist et al., 2016; Chetty et al., 2014; Deming, 2014; Einav et al., 2022; Helsø et al., 2019; Kane and Staiger, 2008). As in Abaluck et al. (2021), we use the estimated forecast coefficient to examine whether our case-mix corrected outcome scores accurately predict causal variation in individual-level health outcomes.

Second, we contribute to the broader (health) economics literature by examining the predictive validity of case-mixed corrected outcome indicators like mortality and (avoidable) hospital (re)admission rates as measures of quality in the long-term care sector. Prior evidence from the hospital sector is not equivocal: some studies suggest that unobservable patient differences may generate misleading quality estimates (Gowrisankaran and Town, 1999; Hull, 2020), others that risk-adjusted outcomes do provide useful quality information (Doyle et al., 2019). These results cannot directly be transferred to the long-term care setting, because of its focus on preventing health deterioration rather than on improving health. The same holds for studies of Skilled Nursing Facilities in the U.S. (Einav et al., 2022; Rahman et al., 2016), where the hospital plays a more prominent role in choosing a facility, and many admissions are short-stays aimed at a discharges back to the community.

Third, we contribute to a better understanding of health outcomes across nursing homes by taking unobserved selection into account. The causal nursing home literature so far has only considered impacts on outcomes of one – often binary – characteristic at a time, like staffing levels, the presence of dementia special care units or ownership (Friedrich and Hackmann, 2021; Grabowski et al., 2013; Gupta et al., 2021; Huang and Bowblis, 2018; Joyce et al., 2018; Lin, 2014). However, since these characteristics are often strongly correlated, it is difficult to isolate the impact of a single characteristic, even with exogenous variation at the individual level (Konetzka et al., 2019). In contrast, we analyze the total variation in outcomes that can causally be attributed to provider differences. Prior research documenting overall differences in outcomes across nursing homes either does not take selection on unobservables into account (see for example Arling et al. (2007); Wouterse et al. (2023a)), or it focuses on short stays in (U.S.) Skilled Nursing Facilities (see for example Einav et al. (2022); Rahman et al. (2016)). It is essential to know the extent to which selection bias drives total observed variation in outcomes across nursing homes (without attributing it to one characteristic), e.g. for providing valid quality information to consumers, for making fair comparisons of nursing homes' relative performance or for assessing returns on public healthcare spending (OECD and European Commission, 2013).

We find meaningful variation in mortality and hospitalization rates across Dutch nursing homes. We show the value of estimating performance using administrative data

uck et al., 2021; Helsø et al., 2019).

which allows for controlling for a large range of resident characteristics. After extensive case-mix correction, we find that the five percent best-performing nursing homes have a 7 and 14 percentage points lower mortality and avoidable hospitalization rate compared to the worst performing ones. The results from our selection bias test demonstrate that this variation in outcomes is not attributable to unobservable heterogeneity in resident characteristics. Our findings suggest that outcomes are weakly correlated with only a small subset of process and structure indicators.

4.2 Background

4.2.1 Nursing homes in the Netherlands

Nursing homes may serve two groups. First, they serve residents - or clients[3] - who need long-term institutional care and who, once admitted, typically stay there for the remainder of their life. Second, they may serve clients who are discharged from the hospital for a (limited) period of rehabilitation care or post-acute care.[4] In the Netherlands and elsewhere, there is a clear distinction between these two. In this study, we focus on the first group; long-term institutional stays.[5]

For this group, the Netherlands has comprehensive social long-term care (LTC) insurance that pays for 99.9% of total nursing home care expenditures (Statistics Netherlands, 2017). Nursing home care, including costs for room and board, is covered by the insurance for the entire population. Nursing home recipients pay a relatively low co-payment that covers 11% of total expenditures (Rijksoverheid, 2017). The co-payment depends on the recipient's income and wealth but not on the type of care received or the nursing home chosen (Tenand et al., 2021). This makes the Dutch nursing home care accessible.

Elders need to apply for eligibility for a nursing home admission, which is granted if someone needs supervision or care around the clock. This eligibility decision is made by an independent government agency (CIZ). CIZ also decides on the care package which indicates the intensity of nursing home care that the recipient is eligible for.[6]

Elders who are eligible for a nursing home admission may choose any nursing home with availability for the desired care intensity package. The waiting time in each of the regions in the Netherlands (during our study period) is limited: virtually all eligible elders can move to a nursing home within the 6-week period that is set as the norm by the government (NZa, 2021). However, some elders choose to delay their admission

[3]The terms nursing home residents and clients are used interchangeably throughout this paper.

[4]In the US, this care may be provided in skilled nursing facilities.

[5]Some Dutch nursing homes also offer day-care for elders who live at home or (short-term) rehabilitation care. Elders receiving these types of nursing home care are not included in this study.

[6]Residents with lower care intensity (ZZP 4) need intensive support and extensive care, with dementia care (ZZP 5) need a protective living facility with intensive dementia care, with higher care intensity (ZZP 6) need a protective living facility with intensive support and care, with highest care intensity (ZZP 7 and 8) need a protective living facility with very intensive care and treatment or support (CIZ, nd). A resident's care intensity package may change during his/her stay in a nursing home.

until their preferred nursing home has an opening and are then put on a nursing home-specific waiting list while they temporarily live in another nursing home or receive substitute home care.

All providers are private entities that are not allowed to make profit (Barber et al., 2021).[7] Nursing homes receive a per diem price per client up to a budget ceiling that are negotiated with regional single-payers who contract long-term care providers. These prices are specific for each care intensity package and are constrained by a maximum price set by the government (Barber et al., 2021).

There are several measures in place to stimulate the provision of high-quality care. First, since 2017, nursing home budgets are supplemented by a subsidy for quality improvements. To receive this additional subsidy, nursing homes submit a quality improvement plan. Second, the Healthcare Inspectorate monitors quality of care, e.g. through unannounced visits. Its quality reports are published. Third, nursing homes are required to provide information about processes of care to the government, which is published online. Finally, nursing homes are obliged to facilitate residents and their relatives to report their satisfaction with the nursing home. Almost all providers do this through a public website called Zorgkaart Nederland. These online ratings are intended to assist (relatives of) future nursing home residents in selecting a nursing home.

4.2.2 Measuring nursing home performance

Nursing home quality is multidimensional and can be classified into three dimensions, namely structure, processes, and outcomes (Donabedian, 2003). In most countries, nursing home quality measures focus on the structure and process dimension of quality of care (Barber et al., 2021). Yet, Mor et al. (2003) and Werner et al. (2013) show that nursing homes that perform well on structure and process-based quality measures do not necessarily improve (health-related) outcomes of their residents. To provide a comprehensive set of quality information it is thus worthwhile to complement the widely used structure and process-based measures with information on outcomes. This subsection discusses prior work on outcome measurement in the nursing home sector and highlights two themes: the use of mortality and avoidable hospitalizations as outcomes measures and why there may be a selection bias in performance indicators using such outcomes.

Using mortality and avoidable hospitalizations as outcomes measures
According to Gupta et al. (2021) and McClellan and Staiger (1999) mortality has become the ”gold-standard” for measuring quality in the health economics literature. Several extensive literature reviews indicate that reduced risk of mortality is associated with higher well-being of older persons (Chida and Steptoe, 2008; Martín-María et al., 2017), which makes it a good candidate for measuring nursing home outcomes. Likewise, a hospital stay can not only be costly but, more importantly, is also found to be traumatic, uncomfortable and disorienting for nursing home residents (Grabowski

[7] There is a small but increasing number of for-profit nursing homes (Bos et al., 2020; Hussem et al., 2020). These nursing homes are not included in the analysis of this paper.

et al., 2007; Ouslander et al., 2000). We believe that both mortality and potentially avoidable hospitalizations are undesirable and that nursing homes with lower mortality and lower avoidable hospitalizations – all else equal – are performing better.[8]

Research shows that variation in such health outcomes can at least to some extent be attributed to factors influenced by the nursing home. For example, Cornell et al. (2019) show that residents admitted to Skilled Nursing Facilities with higher STAR ratings have lower mortality and fewer hospitalizations. Other channels through which nursing homes are found to affect outcomes – like hospitalizations and mortality – are staffing levels, private equity ownership, nonprofit status and the presence of a dementia special care unit (Grabowski et al., 2013; Gupta et al., 2021; Joyce et al., 2018; Friedrich and Hackmann, 2021). Therefore, we would expect at least some variation in terms of these outcomes across nursing homes.

The outcomes that we measure are restricted to the health domain. Ideally, we would measure outcomes that go beyond health, like individual level wellbeing and quality of life. However, routinely measuring these on a large scale in such a vulnerable population is not feasible. The main advantages of using mortality and avoidable hospitalizations as outcomes are that they are not self-reported, available for the full population and not prone to measurement error. Furthermore, the econometric issues that we deal with apply to all outcome measures, making this study a relevant illustration of nursing home performance measurement problems more generally. As discussed in the previous two paragraphs, mortality and avoidable hospitalizations likely capture sufficiently relevant aspects of nursing home performance to be indicative of other types of relevant outcomes.

Selection bias in the nursing home setting

Variation in outcomes may be driven by selection bias. There are several reasons why non-random selection could occur in the nursing home setting. First, some nursing homes may selectively attract a certain type of clients. For-profit nursing homes might, for example, have an incentive to attract more profitable or less costly clients, especially when they are close to their full capacity (Gandhi, 2023; He and Konetzka, 2015). Second, different types of individuals may choose a nursing home based on different criteria, which could cause performance measures to be either positively or negatively biased. On the one hand, elders (or their family members) who consider themselves more likely to be more severely ill, and more dependent on care services, may be more inclined to choose a nursing home that has a reputation to deliver higher quality care. On the other hand, elders who are more severely ill may be less able to "shop around" for quality care, especially after a sudden impairment and end up choosing a nursing home with no waiting list, instead of one with higher perceived

[8]As some hospitalizations may be unavoidable, we focus on those that are potentially avoidable. We do not focus on potentially avoidable causes of death as most classifications of avoidable causes of death are based on premature mortality, defined as dying before the age of 70 (OECD, 2009). Since our sample is restricted to those aged 70 and older, applying such a classification would be inappropriate. Additionally, we do not expect that euthanasia has a large contribution to differences in mortality across nursing homes since euthanasia occurred only 286 times (i.e. 1 percent, according to the number of deaths in our sample in the same year) in total in nursing homes in 2017 (Heins et al., 2019)

quality (Castle, 2003; Schmitz and Stroka-Wetsch, 2020). Also, prospective residents who are more responsive to quality might be wealthier and better educated (Bensnes and Huitfeldt, 2021) or have better informal networks. Elders with such an advantageous environment can generally be expected to have a better (unobserved) health status, and they may also be more responsive to quality indicators when choosing other types of healthcare providers (Bensnes and Huitfeldt, 2021; Cornell et al., 2019). In sum, some degree of nursing home selection may be expected, but it seems hard to predict the direction of any bias a priori.

The literature on selection bias in nursing home outcome measures is limited and, like most research on nursing home quality (Lippi Bruni et al., 2019), generally focuses on Skilled Nursing Facilities (SNFs) in the United States.[9] Arling et al. (2007) demonstrate that shifts occur in SNF quality rankings when more observable differences in client characteristics are accounted for. This indicates that there may also be some selection on observable characteristics of SNF clients. However, how much selection does remain when many observable characteristics are already accounted for? Rahman et al. (2016) report wide variation in risk-adjusted re-hospitalization rates across SNFs (i.e. 15 percentage points between the five percent best and worst performing facilities). They show that these rates are an accurate prediction of the re-hospitalization risk of individuals admitted to these SNFs a few years later, suggesting that variation in risk-adjusted re-hospitalization rates is not driven by selection on unobservables. In contrast, Einav et al. (2022), do find evidence of selection bias driven by unobservably healthier clients being more likely to be admitted to SNFs that generate larger health improvements.

It is still an open question how any of the prior results on selection bias from the U.S. SNF may have relevance for the Dutch nursing home setting. Although both types of facilities offer institutional care mainly to older clients who cannot live at home yet or anymore, the Dutch system, like in many other developed countries, almost exclusively concerns on long-stays rather than short-stays and has a much more comprehensive social system for long term care (Barber et al., 2021). On the one hand, the role of selection likely plays a more prominent role in the U.S. setting due to financial incentives to admit short-stay non-Medicaid patients (see also Gandhi (2023)). Moreover, the focus on nonprofit nursing homes - which forms the largest part of all nursing homes in the Netherlands (Bos et al., 2020) – may induce a smaller role of non-random selection since nonprofit nursing homes may be less inclined to selectively attract healthier clients. On the other hand, the choice process may be more selective for long-stays since it requires the decision on where to reside until death (Bom, 2021), compared to where to stay for 26 days – the average length of stay in post-acute care in the U.S. (Cornell et al., 2019).

[9]When looking at the hospital setting, the evidence on this topic is mixed: where some studies find evidence for non-random selection in hospital outcomes (Geweke et al., 2003; Gowrisankaran and Town, 1999; Hull, 2020), others find that selection bias only plays a minor role (Doyle et al., 2019; Helsø et al., 2019).

4.3 Data

4.3.1 Sample selection

Nursing home residents

We use administrative data provided by Statistics Netherlands encompassing the full Dutch population (more detailed information about the data sources can be found in Appendix C). Our sample consists of individuals who were admitted to a nursing home for the first time between January 2015 and July 2019.[10] We use individual-level information on provider codes and addresses from the municipal registry, to link 87 percent of the 2015-2017 population to the nursing homes that they were admitted to.[11] We complement this sample by individuals who entered a nursing home in 2018 to July 2019 that we could match to nursing homes using information on addresses only. As a first step to make the resident populations across nursing homes more comparable, we dropped 9,438 (7%) admissions of residents whose age was younger than 70 at the time of admission[12], followed by 3,627 (3%) admissions for individuals for whom we have missing data on background characteristics. Our final study sample includes 119,699 nursing home residents in the mortality analyses, and 83,056 residents in the hospitalization analyses: data on hospitalizations is only available until December 2017.[13]

Nursing homes

Our data contains an anonymized provider code, but not the location. We do observe where individuals live and therefore identify nursing home locations by an address on which at least 5 individuals receive care within the same time period provided by the same provider (based on the provider code). Nursing home facilities belonging to the same chain organization can use the same provider code, but are distinguished using the address information. We use the provider codes combined with postal codes to link the information on quality indicators. Descriptive statistics about the quality indicators of the included nursing homes can be found in Appendix Table C1.

We include the 849 largest nursing home facilities, with at least 50 new admissions during the entire study period, in our main analysis.[14] Figure 4.1 shows the variation

[10] We focus on people with care packages 4-8, which are for long-term nursing home stays. That is, we exclude people who are eligible for palliative care (care package 10) or geriatric rehabilitation (care package 9).

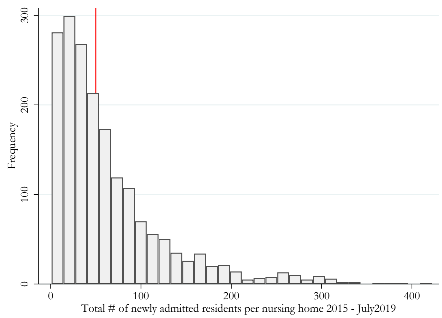
[11] In 31,688 cases, address information was missing and the provider code belonged to multiple nursing home facilities within an organization. For this group, we imputed to which nursing home facility the resident was admitted using admission data of the nearest neighbour with the same provider code and non-missing address information.

[12] Although this is a significant share of our sample, being admitted to a nursing home before the age of 70 is a rare event (i.e. between 0 to 0.5 percent depending on the age-group) in the Netherlands.

[13] We did not exclude residents who switched nursing homes during the study period because this may underestimate variation in performances, as they may switch from low to high quality homes.

[14] Individuals who are admitted to nursing homes with fewer admissions are generally in better health (i.e. lower care needs, lower healthcare expenditures - results available upon request), which may have implications for our results. However, we follow (Einav et al., 2022) by excluding the smaller nursing

Figure 4.1: Total number of newly admitted admissions per nursing home



Notes: This figure shows the total number of admissions per nursing home on the x-axis and the frequency, representing the number of nursing homes, on the y-axis. All nursing homes to the right of the red vertical line are included in the main analyses.

in size, in terms of the number of newly admitted residents across nursing homes. To ensure sufficient statistical power, we do not attempt to estimate performance for the nursing homes with fewer admissions (to the left side of the red line in Figure 4.1). The 21 percent of residents who are admitted to one of these 1,008 smaller nursing home facilities are included in the reference category in the analyses.

4.3.2 Health outcome measures

We focus on two outcomes, namely mortality and avoidable hospitalization. We define hospitalizations as potentially avoidable if they are related to a main diagnosis that could have been prevented or treated in the nursing home. For example, hospitalizations resulting from falls in nursing homes may be preventable by hip protectors (Vu et al., 2006) or by adaptations to the environment like an optimized light design for residents with cataract or height adjusted chairs (Becker and Rapp, 2010). The diagnoses that we classify as such (see also C2 in the Appendix) are based on two studies on ambulatory care-sensitive hospitalizations for the elders population (Carter, 2003; Walker et al., 2009), to which we add hospitalizations due to falls and fractures, wounds and rehabilitation as potentially preventable or treatable in a nursing home- thus being potentially avoidable. More than half of the avoidable hospitalizations are due to falls and fractures, about 8 percent to pneumonia, 6 percent to asthma and COPD, 5 percent to rehabilitation and 4 percent to kidney or urinary tract infections (Appendix Table C2).

We construct binary outcomes (equal to one if the individual died or had an avoidable hospitalization within 180 days after admission) as our main dependent variables to

homes to obtain more reliable estimates.

limit the influence of right censoring.[15,16] The 180-day cut-off is somewhat arbitrary, but in line with the nursing home literature (Cornell et al., 2019; Intrator et al., 2004; Vossius et al., 2018). Additionally, Kaplan-Meier survival curves in Figures C1a and C1a in the Appendix confirm that most variation in time until death and until an avoidable hospitalization occurs in the first half year after nursing home admission. The robustness checks examine the sensitivity of our results to the use of different cut-offs. In our sample, the average 180-day mortality and avoidable hospitalization rates are 21 and 13 percent respectively (Table 4.1).[17,18]

4.3.3 Case-mix controls

We control for observable differences in nursing home residents' characteristics by including an extensive set of case-mix controls in our analyses: age at admission; gender; whether someone lives in an rural municipality, defined by an average of at least one thousand addresses per square kilometer; yearly disposable household income, standardized by household size; wealth from assets and savings; and a comprehensive set of proxies for health: whether the person visited the hospital within 30 days prior to nursing home admission (also to account for potential hospital re-admissions); the Charlson comorbidity index based on 12 comorbidities like dementia, cancer and pulmonary diseases[19]; the number of different types of medicine consumed; and healthcare expenditures from the year before nursing home admission. Furthermore, we include care needs as measured by the care intensity package as determined by the independent eligibility assessment agency. An overview and more extensive explanation of all covariates can be found in Appendix Table C4.

[15]While mortality is already a binary event by nature, we could measure the hospital outcome as the number of avoidable hospitalizations. However, as this may be influenced by re-admissions, we use the count in our robustness tests only.

[16]As data on mortality is available up until 2019 and only 7 percent of individuals left the nursing home before their death, we are not concerned about right-censoring in this outcome measure. Hospitalizations are not accounted for censoring from deaths.

[17]The 180-day mortality rate is similar to that in skilled nursing facilities in the U.S. (Cornell et al., 2019) and slightly higher than in care homes in Norway (Vossius et al., 2018).

[18]Table C3 shows that, of the 23,165 residents who had at least one hospitalization within half a year after nursing home admission, 37 percent experienced a hospitalization that was potentially avoidable. Both this percentage and the 180-day hospitalization rate are higher in comparison to other studies (Carter, 2003; Intrator et al., 2004; Walker et al., 2009), likely resulting from the inclusion of falls and fractures as an avoidable cause.

[19]The Charlson comorbidity index is an indicator for disease burden and/or a predictor of mortality (Sundararajan et al., 2004). We use the updated version constructed by (Quan et al., 2011) which reflects a weighted score based on 12 comorbidities, among which dementia, diabetes and cancer.

Table 4.1: Case-mix controls by outcome

	Died within 180 days after nursing home admission					Had an avoidable hospitalization within 180 days after nursing home admission				
	All	No (79.3%)	Yes (20.7%)	Difference Yes (-) No	se	All	No (87.1%)	Yes (12.9%)	Difference Yes (-) No	se
Women (%)	0.649	0.674	0.553	-0.121***	(0.003)	0.65	0.649	0.663	0.014***	(0.005)
Age	85.027	84.786	85.951	1.166***	(0.046)	84.948	85.054	84.229	-0.826***	(0.066)
Care intensity[1] (%)										
Lower	0.205	0.227	0.122	-0.104***	(0.003)	0.202	0.197	0.239	0.043***	(0.004)
Dementia	0.49	0.498	0.457	-0.041***	(0.004)	0.459	0.475	0.35	-0.125***	(0.005)
Higher	0.294	0.264	0.407	0.143***	(0.003)	0.326	0.315	0.4	0.085***	(0.005)
Highest	0.011	0.011	0.014	0.003***	(0.001)	0.013	0.013	0.011	-0.003**	(0.001)
Healthcare expenditures	1,347	13,437	13,553	117	(165)	10,290	9,819	13,477	3,658***	(173)
Wealth	83,185	81,992	87,760	5,768.**	(2793)	85,895	87,366	75,954	-11,412**	(4,445)
Std. household income	21,914	21,901	21,966	65	(75)	21,811	21,816	21,777	-39	(107)
Number of medicine	8.766	8.514	9.731	1.217***	(0.035)	8.82	8.637	10.057	1.42***	(0.051)
Charlson score	0.672	0.537	1.192	0.656***	(0.010)	0.703	0.687	0.816	0.129***	(0.016)
Hospital in last month (%)	0.169	0.142	0.272	0.130***	(0.003)	0.191	0.184	0.244	0.061***	(0.004)
Rural (%)	0.297	0.297	0.299	0.002	(0.003)	0.302	0.305	0.284	-0.021***	(0.005)
Year (%)										
2015	0.25	0.248	0.258	0.010***	(0.003)	0.361	0.361	0.361	0.000	(0.005)
2016	0.31	0.301	0.344	0.043***	(0.003)	0.447	0.441	0.485	0.044***	(0.005)
2017	0.26	0.259	0.266	0.008**	(0.003)	0.192	0.198	0.154	-0.044***	(0.004)
2018	0.115	0.122	0.087	-0.036***	(0.002)					
2019	0.064	0.069	0.044	-0.025***	(0.002)					
N	119,699	94,933	24,766			83,056	72,354	10,702		

This table presents the averages or shares (%) of each case-mix control variable by the mortality and avoidable hospitalization outcome including differences between those for whom the outcome equals one and zero; Age, care intensity, rural and year at moment of nursing home admission; healthcare expenditures, wealth, std. household income, number of medicine and Charlson score from the (calendar) year before admission; Standard errors (se) between brackets.

* Difference is statistically significant at 10 percent; ** at 5 percent; *** at 1 percent.

[1] Lower - intensive support and extensive care; Dementia - protective living facility with intensive dementia care; Higher - protective living facility with intensive support and care; Highest - protective living facility with very intensive care and treatment or support.

Table 4.1 shows how these covariates vary across the health outcomes of individuals. Older male residents, those receiving a higher care intensity package, those who use more medication, those with a higher Charlson comorbidity index or those who visited the hospital within 30 days before nursing home admission, have a higher probability of dying within the next half year. On the other hand, residents who experienced an avoidable hospitalization within 180 days after admission are, on average, younger and enter the nursing home receiving lower care intensity. This implies that healthier individuals (i.e. younger with lower care needs) are more likely to be admitted to a hospital. Nonetheless, as we control for differences in underlying health across nursing homes, we interpret a higher risk of avoidable hospitalizations an undesirable outcome.

4.4 Empirical strategy

4.4.1 Observed performance

To quantify the effect of a nursing home j on the probability of an adverse health outcome, we use a linear value-added framework:

$$\mathrm{E}(Y_i|H_i = j) = \gamma X_i + \delta_j + \rho_i, \tag{4.1}$$

where Y_i is the outcome for individual i conditional on being admitted to nursing home j. The expected outcome depends on an individual's observed characteristics X_i, which include proxies for prior health, an unobserved individual component ρ_i, and a nursing home specific effect δ_j.[20] δ_j is the nursing home level estimate of interest: the value-added of the nursing home, i.e. the nursing home's impact on the outcome under the condition of exogenous nursing home choice. We assume that the nursing home impact is additive and homogeneous across residents.

We estimate a linear probability model using an ordinary least squares regression (with robust standard errors) to obtain each nursing home's performance on the two health outcomes.[21] The estimation equation is as follows:

$$Y_i = \alpha_0 + \gamma X_i + \sum_{j=1}^{J} \delta_j H_{ij} + \rho_i, \tag{4.2}$$

where Y_i is a zero-mean dichotomous outcome variable for individual i - e.g. mortality - and X_i are the individual level case-mix controls. α_0 represents the reference category which includes all individuals that were admitted to one of the smaller nursing homes. H_{ij} is a dummy variable that equals one if individual i is admitted to nursing home j

[20] Our value-added model deviates from the classical ones in the sense that we include proxies for individual's health as right-hand side variables instead of the individual's outcome Y_i prior to admission. The latter is simply not possible given the nature of our outcome variables.

[21] Estimating the same specification with a logit or random effects generates estimates that highly correlate (> 0.99) with the ones from the OLS procedure.

$(j = 1, 2, .., J)$. The estimated parameter $\hat{\delta}_j$ reflects the nursing home j's effect on the outcome – or the nursing home's value added.

Our estimates $\hat{\delta}_j$, especially those for small nursing homes, are surrounded by sampling imprecision. Like Angrist et al. (2017); Abaluck et al. (2021); Chetty and Hendren (2018); Kane and Staiger (2008), we therefore account for the statistical noise in our value-added estimates by applying a standard empirical Bayes correction (Morris, 1983). The empirical Bayes estimator of δ_j is weighted average of the precisely estimated grand mean (the average outcome across all nursing homes) and the imprecisely estimated nursing-home-specific OLS estimate $\hat{\delta}_j$, where the weight of the latter is proportional to its estimation error. As the estimation error is greater for small homes, the shrinkage is larger for these homes than for large homes.[22] We use these empirical Bayes estimates when evaluating total variation in performance in Section 4.5.1 and when correlating performance on other quality indicators in Section 4.6.

4.4.2 Testing for selection bias

Individuals are not randomly assigned to nursing homes, but are to a large extent free to choose the home that they prefer. We might therefore be worried about selection bias.The question we want to address is, after we correct for observable differences in individuals' characteristics X_i, are the estimates of observed performance $\hat{\delta}_j$ biased by unobserved individual differences? This is the case if there is a correlation between and individual's unobserved health and the performance - i.e. value added - of the nursing home he or she goes to (i.e. a correlation between ρ_i and H_{ij} in Equation 4.2).The bias can be either positive or negative, as preferences for nursing home quality can be both positively or negatively correlated to (unobserved) individual health (see Section 4.2.2).

The standard way of dealing with selection bias is to focus on plausibly exogenous variation in nursing home choice and exploit this variation, using instrumental variable analysis, to obtain (causal) estimates if δ_j. In our case, this would entail instrumenting each of the $J = 849$ nursing home dummies in Equation (4.2), which requires at least 849 instrumental variables to obtain a just or over-identified model. Although this can be done in some settings (see Gowrisankaran and Town (1999); Hull (2020) using such an approach for hospital care), in the setting of nursing home care, with many small-sized providers, this is not feasible because the lack of power likely causes a many weak instruments problem (Angrist et al., 2016).[23]

[22] We use the following estimator $\delta_j^{EB} = \tau_j \hat{\delta}_j + (1 - \tau_j)\bar{\delta}_j$, where $\hat{\delta}_j$ is obtained by estimating Equation (4.2), $\bar{\delta}_j$ is equal to the average of $\hat{\delta}_j$ across all nursing homes, and the shrinkage factor τ_j is equal to $\frac{\sigma_\delta^2}{\sigma_\delta^2 + se_\delta^2}$ with σ^2 being the between nursing home variation minus the average noise and se_δ^2 being equal to the within nursing home variation. Under the assumption that the nursing-home specific effects are independent, this estimator is equivalent to an empirical Bayes estimate of the nursing home specific effects given that both the prior and likelihood function come from a normal distribution (Angrist et al., 2017; Chetty and Hendren, 2018; Kane and Staiger, 2008; Morris, 1983).

[23] In a recent working paper, Einav et al. (2022) estimate the added health value of skilled nursing facilities in the U.S., using a control-function approach (which is quite similar in spirit to an IV-approach) to

Instead of trying to obtain a 'causal' IV-estimate for each nursing home, we test ex-post whether the observed performance measures $\hat{\delta}_j$ – estimated by Equation (4.2) - are biased (see also Abaluck et al. (2021); Angrist et al. (2016); Chetty et al. (2014); Deming (2014); Helsø et al. (2019)). The intuition behind the test is as follows. Suppose we already estimated the case-mix corrected – or value-added - scores from Equation (4.2) and then afterwards could randomly assign a new group of individuals over the J nursing homes. If the estimated performance scores $\hat{\delta}_j$ would be unbiased, then these scores would perfectly predict the (average) outcomes for the randomly assigned group. We could run the following regression on the sample of randomly assigned individuals:

$$Y_i = \gamma X_i + \lambda \hat{\delta}_{ij} + \varepsilon_i, \tag{4.3}$$

with $\hat{\delta}_{ij}$ the estimated performance score of the nursing home to which individual i has been assigned to. This regression would provide a simple test of (average) selection bias based on the forecast coefficient λ: if the values of $\hat{\delta}_{ij}$ represent (on average) the true causal effects of nursing homes on the outcome, then λ should be equal to one.[24]). If, on the other hand, the estimates suffer from selection bias then λ will be either smaller or larger than one. If there is a positive correlation between unobserved health and nursing home quality (healthier clients are more likely to choose better nursing homes) then λ will be larger than one. We then overestimate nursing homes' performance, in the sense that the observed performance is better than true performance for high quality homes and lower than true performance for low quality homes. If the correlation between unobserved health and nursing home quality is negative, then the observed performance is an underestimation of the true effect.

4.4.3 Instrumental variable approach

In practice, we cannot randomly assign a group of clients over the different homes, and thus have to rely on quasi-exogenous variation instead. If there is a subgroup within our population for which it is credible that nursing home choice is not related to expected outcomes, then we can use this group to perform a test similar to that for the imaginary randomly assigned group in Equation (4.3).

The source of variation we exploit is geographical distance from a client's home to a nursing home. Distance is an important driver of nursing home choice. Both earlier and more recent literature report distance to be a strong, if not the dominant, driver of nursing home choice (Castle, 2003; Gadbois et al., 2017; Hackmann, 2019; Schmitz and Stroka-Wetsch, 2020; Shugarman and Brown, 2006). As a result, location-based instruments are used widely to predict provider choice both in and beyond the nursing home literature (Einav et al., 2022; Gandhi, 2023; Gowrisankaran and Town, 1999; Grabowski et al., 2013; Hull, 2020; Newhouse and McClellan, 1998). Moreover, a

correct for potential selection on unobserved health. The average number of treated patients in these facilities, aimed at rehabilitation, is substantially higher than in the permanent residential homes we investigate. Also, Einav et al. (2022) seem to use a relatively restrictive model for patient choice.

[24] Although $\lambda = 1$ implies that there is no bias on average across nursing homes, this does not rule out the possibility that the scores of some specific homes are biased (see Angrist et al. (2017); Hull (2020)

location-based instrument is unlikely to be related to the unobserved component of the individual's outcome as regional differences in health are expected to be small in our setting, especially since we control for an extensive set of health proxies at the individual level.

To implement the forecast test using quasi-exogenous variation in nursing home choice based on distance, we perform an IV using two-stage least squares (similar to Abaluck et al. (2021); Deming (2014); Helsø et al. (2019)). In the first stage, we predict the observed performance score $\hat{\delta}_{ij}$ of the nursing home j that individual i actually goes to using the observed performance score $\hat{\delta}_{ij}^{Closest}$ of the nursing home that is closest to individual's i former residence:

$$\hat{\delta}_{ij} = \beta_0 + \mu X_i + \theta \hat{\delta}_{ij}^{Closest} + \varphi_i. \tag{4.4}$$

In the second stage, we use the first stage predictions of the performance score $\tilde{\delta}_{ij}$ to examine the effect of the nursing home performance scores on the outcomes (only) for individuals who move to a nursing home because it is the closest to their prior home. We do this by regressing individuals' outcome on the first-stage prediction of the performance score:

$$Y_i = \alpha_0 + \gamma X_i + \lambda \tilde{\delta}_{ij} + \varepsilon_i, \tag{4.5}$$

If our instrument is valid (the performance of a nursing home in uncorrelated with the unobserved health of the clients that live closest to it) [25], the interpretation of the forecast coefficient $\hat{\lambda}$ is the same as in Equation (4.3); if $\hat{\delta}_j$ is an unbiased estimate of the true effect of a nursing home on clients' outcomes, it should (on average) perfectly predict the outcomes for clients who go a particular home solely because it's the closest to their prior residence. A forecast coefficient that is not equal to one then signals that observed performance is, on average, biased.

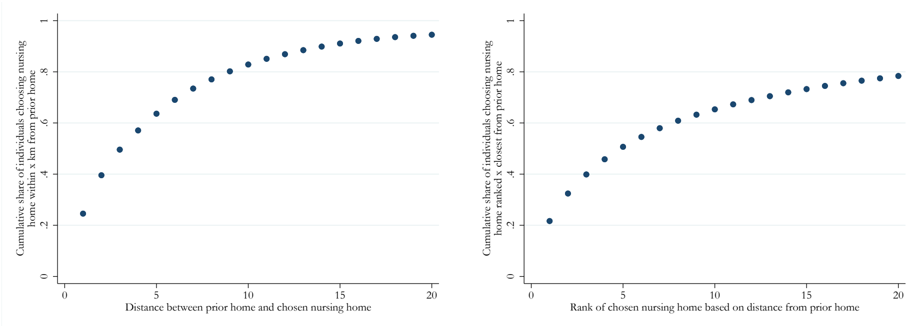
4.4.4 Instrumental variable assumptions

We first reflect on the assumptions that must hold to interpret $\hat{\lambda}$ as the impact of nursing home performance on a random individual's outcome. Two of these assumptions of the IV approach are that the instrumental variable – in our case performance of the closest nursing home - is (i) relevant and (ii) valid. A third condition is monotonicity.[26] We

[25] We further have to assume that the effect of $\hat{\delta}_j$ is homogeneous (i.e. constant for all individuals). Lambda is also affected by how compliers are distributed over the range of values that the treatment variable takes. If this distribution is not the same as in the full sample and if there are heterogeneous treatment effects, a deviation in lambda from one might not only be caused by selection bias (i.e. selection of low-mortality patients into low-mortality nursing homes) but also by systematic variation in the distribution of compliers over heterogeneous treatment effects. Our LATE would then deviate from one not because of a bias, but due to the high mortality estimates getting a lower weight due to lower compliance on that side of the distribution. We argue that this is unlikely to be an issue in our setting as almost every nursing home – irrespective of its performance – is the closest one to at least 20 people in our sample and is (in 99% of the cases) chosen by at least 5 percent of those. In other words, the distribution of the compliers over the treatment variable is likely the same as the distribution of the entire study population.

[26] This condition has received limited attention in other studies using the value-added framework, partly due to the nature of their instruments (Angrist et al., 2017; Chetty et al., 2014).

Figure 4.2: Relationship between nursing home choice and distance



(a) Distance in kilometers

(b) Ranked by distance; 1 is the closest

Notes: Figure (a) shows the share of individuals being admitted to a nursing home within x kilometers from the individual's prior home. The x-axis represents the kilometers (rounded down) between one's prior home to admission and the chosen nursing home. Figure (b) shows the share of individuals being admitted to the closest nursing home from the individual's prior home. The x-axis indicates whether the chosen nursing home was the closest (at x = 1) or the second closest (x =2) and so forth. In both figures the y-axis states the cumulative share of the full sample.

discuss the weak monotonicity assumption, which is sufficient for causal interpretation (Frandsen et al., 2019), in Appendix C. In the following subsections we pay more attention to why the relevance and validity assumption are likely to hold in this setting.

Relevance

The instrument is relevant if it has strong predictive power for nursing home choice. Prior studies argue and show that travel distance is the most important determinant of nursing home choice (Castle, 2003; Gadbois et al., 2017; Gandhi, 2023; Hackmann, 2019; Schmitz and Stroka-Wetsch, 2020; Shugarman and Brown, 2006). In our setting, we therefore expect that, all else equal, individuals prefer a nursing home that is closer to their prior home. Figure 4.2a confirms that most residents choose a nursing home that is close to their prior home: more than 60 percent of our sample is admitted to a nursing home within 5 kilometers from his or her prior home. Figure 4.2b shows that 21 percent chooses the nursing home that is closest to their former home. This suggests that the instrument is likely to be relevant.

The results from the first stage regression (Equation (4.4)) confirm that the instrument is strong. Table 4.2 shows that the first stage coefficient is economically and statistically significant. The partial F-statistics, which are equal to 7,189 and 18,545, both affirm the relevance of our instrument for both outcome variables (Staiger and Stock, 1997).[27]

[27] The reported F-statistics are extremely high. This is not surprising since the instrument directly links to the endogenous variable and our sample size is relatively large.

Table 4.2: First stage results

	Endogenous variable: Performance of the chosen nursing home ($\tilde{\delta}_i$)	
	Mortality	Avoidable hospitalization
Instrumental variable: Performance of the closest nursing home ($\hat{\delta}_{ij}^{Closest}$)		
Coefficient (θ)	0.315***	0.497***
	(0.004)	(0.004)
(Partial) F-statistic	7,189	18,545
Covariates	Yes	Yes
N	94,905	65,265

This table reports the estimated coefficient, standard error and partial F-statistic from the first stage (Equation (4.4)) which is a linear regression of endogenous performance of the chosen nursing on performance of the chosen nursing home.
Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.
The sample is restricted to those that are admitted to one of the 849 largest nursing homes.

Validity

The instrument is valid if the unobserved health and other characteristics of individuals (ε_i in Equation (4.5)) are not correlated with the performance of the closest nursing home. That is, performance of the closest nursing home should only be related to an individual's outcome through choice. The validity assumption could be violated if unobservably (un)healthy clients are systematically located closer to the same nursing homes – keeping all individual observable characteristics fixed. This might be the case if better nursing homes are more likely to be located closer to prior homes of individuals with better underlying health (Helsø et al., 2019). This could be an important concern as previous research shows that, at least in the US, high quality nursing homes are more likely to be located in – or closer to - wealthier areas (Konetzka et al., 2015).[28]

We expect the influence of this issue to be limited in our setting since we include individual level and precisely measured covariates like income and wealth as covariates when estimating performance.[29] Furthermore, (Cornell et al., 2019; Rahman et al.,

[28] In spite of this, distance-related instrumental variables are frequently used to correct for non-random selection into hospitals and nursing homes (Cornell et al., 2019; Geweke et al., 2003; Gowrisankaran and Town, 1999; Grabowski et al., 2013; Helsø et al., 2019; Huang and Bowblis, 2018; Newhouse and McClellan, 1998).

[29] The likelihood and severity of violations of the validity assumption is small in the Dutch context for several other reasons. First, financial constraints do not play a role when choosing a nursing home, because

2016) show that including zip code fixed effects limits the influence of regional differences that may be related to unobserved health and living close to a well-performing nursing home. In two robustness tests in Section 4.5.4, we therefore include neighbourhood characteristics as controls and neighbourhood fixed effects. We find that these additions do not change our main results.[30] This could either imply that the location of well performing nursing homes is not related to the health of neighbouring individuals in the Netherlands, or that our extensive set of covariates – encompassing individual level socio-economic indicators and proxies for the individuals' health – already controls for regional variation in health.

4.5 Results

4.5.1 Observed performance

Figure 4.3 presents the observed performance estimates ($\hat{\delta}_j$ from Equation (4.2) after shrinkage) on the y-axis for all 849 largest nursing homes in groups of ten, ranked by their performance.[31] There is a 7 percentage point difference in performance on mortality and a 14 percentage point difference in avoidable hospitalizations between the five percent best-performing and five percent worst-performing nursing homes. However, the wide 95% confidence intervals show that the individual estimates are imprecisely estimated, related to the relatively small size of most homes. The imprecision of our estimates does not facilitate the interpretation of observed differences, i.e. it remains hard to ascertain whether these are driven by true differences in performance or by imprecision.

4.5.2 Test for selection bias

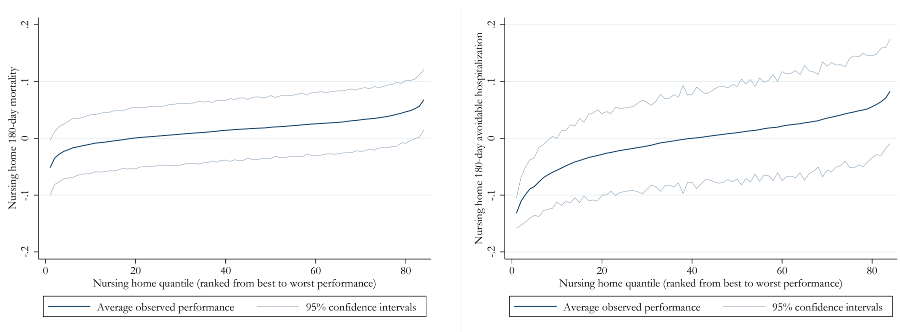
As discussed throughout the paper, the variation observed in the figures above may be driven by unobserved heterogeneity in resident characteristics across nursing homes. In this subsection we present the results for our test for such a selection bias. More specifically, after obtaining predicted performance in the first stage (see also Equation (4.4) and Table 4.2), we obtain an estimate for the forecast coefficient through Equation (4.5). We test whether the estimated forecast coefficient $\hat{\lambda}$ is equal to one. If we fail to reject this test, we interpret our estimated performance estimates as unbiased on average.

the co-payment is the same in all nursing homes. This means that selection related to socioeconomic status is likely much more limited than in the US and many other countries. Second, elders are unlikely to select their place of residence (where they lived prior to nursing home admission) according to the performance of the nursing homes since most elders have lived in the same neighborhood for many years before they enter a nursing home (Diepstraten et al., 2020).

[30] We do not include neighbourhood fixed effects in our main specification because the small number of people per neighbourhood moving to a nursing home minimizes the within-neighbourhood variation in which of the nursing homes is the closest one. This significantly reduces the power of the first stage, which in turn decreases the precision of our forecast coefficient.

[31] Estimates are published in groups of ten as, for privacy reasons, the results for individual nursing homes cannot be published.

Figure 4.3: Observed performance



(a) Mortality

(b) Avoidable hospitalization

Notes: This figure displays the nursing home performances on 180-day mortality (a) and avoidable hospitalizations (b). We present estimated performance (by Equation (4.2)) after empirical Bayes shrinkage. Nursing homes are ranked on their performances and subsequently divided into 84 equally sized groups of 10 to 11 nursing homes. The x-axis represent these nursing home groups. The y-axis indicates average observed performance of each of these groups and its confidence intervals, which are calculated based on the standard error from a randomly chosen nursing home within each group.

The forecast coefficients $\hat{\lambda}$ are economically and statistically not significantly different from one (Table 4.3). The estimated forecast coefficients deviate only minimally from one: choosing a nursing home with an above average mortality of 2 percentage points instead of one of 1 percentage point, increases the risk of dying by 1.07 percentage points.[32] These minor deviations from one are for both outcomes not statistically significant different (p = .408 and p = .104). This implies that observed performance is, on average, unbiased and is likely to predict true differences in performance across nursing homes.

4.5.3 Subgroup analysis

One of the assumptions of the value-added model is that nursing home performance scores are homogeneous across residents (see Section 4.4.3). When estimating the relevance of observed performance for clients with different care needs, we shed light on whether this assumption is plausible: i.e. is observed performance representative for all clients. For this, we use the forecast coefficient estimated through Equation (4.5) replacing predicted performance by observed performance of the chosen nursing home to compare deviations of $\hat{\lambda}$ from one for the different subgroups. This test provides insights on whether observed performance is more informative for specific groups, which also is a relevant question on itself.

[32] To compare, the absolute deviation of the forecast coefficient from one lies within the ones found in studies on hospital performance including extensive controls by Helsø et al. (2019) ($\hat{\lambda} = 0.956$) and Hull (2020) ($\hat{\lambda} = 1.086$).

Table 4.3: Forecast coefficient

	Individual level outcome (Y_i)	
	Mortality	Avoidable hospitalization
Predicted performance of the chosen nursing home ($\tilde{\delta}_{ij}$)		
Forecast coefficient ($\hat{\lambda}$)	1.067***	1.070***
	(0.085)	(0.042)
Forecast bias test ($\hat{\lambda} = 1$)		
χ^2 statistic	0.68	2.64
p-value	0.408	0.104
Covariates	Yes	Yes
N	94,905	65,265

This table reports the regression results from the second stage (from Equation (4.5)), which estimates the impact of predicted performance of the chosen nursing home on the individual level outcome, either mortality or avoidable hospitalization. The test statistic report the χ^2 statistic and the p-value when testing $\hat{\lambda} = 1$. Standard errors are reported between parentheses and p-values between (squared) brackets.
Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.
The sample is restricted to those that are admitted to one of the 849 largest nursing homes.

The regression results in Table C5 in the Appendix show that the estimates on both outcomes are predictive for residents of all care needs, but for some more accurately than for others. We find that the variation in observed performance on mortality is (on average) slightly overestimated ($\hat{\lambda} < 1$) for individuals with lower and the highest care needs. Although these results suggest that the estimates on outcomes are somewhat heterogeneous across care need groups, this does not affect the IV result from Section 4.5.2.[33]

4.5.4 Robustness

We examine the robustness of our results with two additional sets of checks: (i) by including larger sets of controls; and, (ii) by using different definitions for our health outcome measures. We inspect how they correlate with our baseline estimates and whether the result of no structural bias (from Section 4.5.2) is robust to these adjustments.

[33] From additional instrumental variable analyses by subgroup, we observe that performances on both outcomes are unbiased for clients of all care needs, except for performance on avoidable hospitalizations for people with higher care needs. Nevertheless, in this case the 1 lies only just outside of the 95% confidence bounds of $\hat{\lambda}$. These results are available upon request.

First, in Section 4.4.4 we reflected on the validity of our instrumental variables. We mentioned that any systematic differences in unobserved health that are related to the location of someone's prior home are threats to this validity. Therefore, in two robustness checks, we include either neighbourhood characteristics that might be related to someone's health as additional control variables (i.e. average property (house) value, average household income and the share of households living below the poverty threshold at the neighbourhood level as measures of neighbourhood living standards) or include neighbourhood fixed effects. Columns 1-2 in Table 4.4 demonstrate that our results are to a large extent robust to the inclusion of these covariates and neighbourhood fixed effects: performance estimates from both models are highly correlated with our baseline estimates. Additionally, at least with 95% certainty, we cannot reject that the forecast coefficients $\hat{\lambda}$ are equal to one when including neighbourhood controls or fixed effects.[34]

Second, by estimating different specifications of our outcome measures, we verify whether they are sensitive to how they are defined. If performance varies across different stages of admissions, e.g. between the first 90 and 180 days, having a strict cut-off within the outcome measure may not be appropriate. Nevertheless, we find that performance on our main outcomes is highly correlated – with correlations of at least 0.8 – with those of the other specifications in columns 3-5 in Table 4.4. This also holds for using only falls and fractures instead of all avoidable hospitalizations as an outcome. However, we find a statistically significant bias in observed performance on one-year mortality, discouraging the usage of this outcome as a quality measure.

[34] One thing to note is that the forecast coefficient in the model including neighbourhood fixed effects (Column 2) is imprecisely estimated. The fact that $\hat{\lambda}$ is only weakly to not statistically significant from zero for both outcomes implies that the within neighbourhood variation in nursing home performance is not predictive for an individual's health outcome, likely because there are relatively few nursing homes per neighbourhood.

Table 4.4: Robustness tests

	Neighbourhood controls		Other outcome specifications		
	Charact-eristics (1)	Fixed effects (2)	90 days (3)	365 days (4)	# days alive (5)
			Outcome = mortality		
A. Correlation baseline estimates	.999	.881	.830	.792	-.915
B. Forecast coefficient ($\hat{\lambda}$)	1.067***	1.143*	1.102***	1.221***	1.091***
– performance chosen NH ($\tilde{\delta}_{ij}$)	(.086)	(.632)	(.087)	(.083)	(.081)
C. Forecast bias test ($\hat{\lambda} = 1$)					
χ^2 statistic	.65	.05	1.46	7.32***	1.34
p-value	.420	.815	.227	.007	.247
Covariates	Yes	Yes	Yes	Yes	Yes
N (individuals)	83,501	94,605	94,905	88,619	94,905
			Outcome = avoidable hospitalization		
A. Correlation baseline estimates	.9996	.805	.898	.770	.953
B. Forecast coefficient ($\hat{\lambda}$)	1.076***	.683	1.065***	1.041***	1.042***
– performance chosen NH ($\tilde{\delta}_{ij}$)	(.044)	(.684)	(.053)	(.062)	(.049)
C. Forecast bias test ($\hat{\lambda} = 1$)					
χ^2 statistic	2.85*	.22	1.46	.44	.70
p-value	.091	.639	.227	.510	.402
Covariates	Yes	Yes	Yes	Yes	Yes
N (individuals)	64,004	64,992	65,265	65,265	65,265

Panel A shows how our baseline performance estimates correlate to the ones specified in each of the columns. Panel B reports the forecast coefficient, which is equal to the coefficient of predicted performance of the chosen nursing home (by the first-stage regression) in a regression with the individual level outcome as a dependent variable. Panel C tests whether the forecast coefficient $\hat{\lambda}$ is different from zero.

Column 1 includes average property (house) value, average household income and the share of households living below the poverty threshold at the neighbourhood level as additional control variables. Column 2 includes neighbourhood fixed effects. In the remaining columns, the outcome variables are specified respectively as 3) the occurrence of an adverse health outcome within 90 days; 4) within 365 days for mortality and being hospitalized due to a fall or fracture for avoidable hospitalization; 5) the number of days alive within 180 days after admission for mortality and the number of avoidable hospitalizations within 180 days.

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

Column 1 excludes individuals from very small neighbourhoods, as there is no data available for those. Estimates in Panel A are correlated to estimates obtained from this sample (excl. small neighbourhoods), but with the baseline model (no neighbourhood characteristics). Column 2 excludes individuals for whom the neighbourhood could not be identified. Column 4 for the mortality outcome excludes those admitted to a nursing home from January 2019 onward.

4.6 Correlations with quality indicators

In this section we examine to what extent quality indicators on other dimensions – like process and structure – can explain observed variation in outcomes. We explore the association of observed performance to publicly available measures of nursing home quality that are often used in comparisons (Castle and Ferguson, 2010; Spilsbury et al., 2011).[35] We use these results to evaluate whether performance on outcomes could complement the available indicators based on the other dimensions of quality.

4.6.1 Process and outcome quality indicators

The nursing home mortality scores are positively correlated with high levels of psychotropic medicine use which is a process-based indicator of low quality that is reported by the Dutch Health and Youth Care Inspectorate (Table 4.5). 180-day mortality in nursing homes in which all clients use psychotropic medicines is 2 percentage points higher compared to nursing homes in which none of its clients uses psychotropic medicine. Although the coefficient is rather small, the sign of the correlation is in line with the medical literature (Bronskill et al., 2009). Psychotropic medicine use may be related to mortality through side effects that may be more harmful to an older population, like diarrhea (Lindsey, 2009) and delirium. On the other hand, the relationships may also be confounded by (unobserved) other types of nursing home quality. Phillips et al. (2018) argue for example that the number of registered nurse hours is one of the main drivers of antipsychotic medication use among nursing home residents, which may in turn affect mortality through other channels or processes.

Moreover, we find that nursing homes with high rates of avoidable hospitalizations (low quality) have lower pressure sores rates (high quality). At first sight, this correlation may appear to be opposite to what one would expect. However, the negative association with pressure sores may well be a result of residents spending a relatively long time in their nursing home beds, which increases the risk of pressure sores. However, at the same time, spending a lot of time in bed could prevent nursing home residents from falling, which is one of the main contributors of avoidable hospitalizations. In that case, the negative associations are plausible, although it may raise questions about the interpretation of the avoidable hospitalization outcome. Other possible explanations are that pressure sores may be under-reported in the bottom performing nursing homes (Kaltenthaler et al., 2001), or that the (uncorrected) variation in pressure sores is driven by case-mix differences which causes the relationship with performance to be negative if those in worse health (pressure sores ↑) are more likely to be admitted to better performing nursing homes (avoidable hospitalizations ↓).

[35] Almost every characteristic that we consider is an average over multiple years between 2015 and 2018. The descriptive statistics and a more elaborate description of these characteristics can be found in Appendix Table C1.

Table 4.5: Bi-variate regression with quality measures

	Mortality		Avoidable hospitalizations		
	(1)	(2)	(3)	(4)	(5)
Regression:	Coefficient	Constant	Coefficient	Constant	N
I. Psych. medicine use	0.020***	0.007***	-0.015	0.001	705
($\downarrow$= *better*)	(0.005)	(0.002)	(0.010)	(0.004)	
II. Physical restraint use	0.008*	0.012***	-0.002	-0.004	719
($\downarrow$= *better*)	(0.004)	(0.001)	(0.009)	(0.003)	
III. Pressure sores	-0.005	0.015***	-0.098**	0.000	693
($\downarrow$= *better*)	(0.019)	(0.001)	(0.039)	(0.002)	
IV. Online rating	-0.000	0.015**	0.001	-0.010	709
($1 = worst; 10 = best$)	(0.001)	(0.007)	(0.002)	(0.015)	

Regression results of eight (2×4 (I-IV)) separate bi-variate regressions at the nursing home level with either performance on mortality (column 1-2) or avoidable hospitalization (column 3-4) as dependent variables. It uses the performance estimates obtained in Equation (4.2) after shrinkage. Nursing home characteristics are also at the nursing home facility level. Descriptive statistics of nursing home characteristics can be found in Appendix Table C1

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

4.6.2 Structure quality indicators

Table 4.6 presents associations between nursing home characteristics – or structure characteristics – and their mortality and avoidable hospitalizations outcomes. Most estimated coefficients are relatively close to zero. This may mean that, although the multivariate regression includes various observed characteristics[36], the results may be confounded by (unobserved) other types of nursing home quality, such as managerial quality, which may have offsetting effects: it may reduce the number of staff or higher educated nurses by empowering nurse aids, which in turn could improve outcomes (Barry et al., 2005). Yet, the very weak correlations may also imply that existing structure-based quality indicators do not accurately capture variation in performance on the health outcomes that we measure.

Nevertheless, there are some structure quality indicators that show a somewhat stronger association with performance on outcomes. For instance, we find that a relatively long waiting list is (weakly) negatively associated with nursing home mortality: having a one standard deviation larger waiting list to client ratio (of 12 percent) is associated with a 0.28 percentage points lower mortality rate. Caution is warranted as this association may be due to reverse causality; when nursing home mortality is low, turnover

[36]When examining correlations of the same characteristics in bi-variate regressions, we find very similar results. The only difference is that staff absenteeism becomes statistically significant at 10 percent when excluding the other characteristics as covariates.

of clients is also low, which may in turn result in longer waiting lists. On the other hand, even in a situation in which mortality rates are not publicly available – as in the Netherlands – there may also be some perception of quality that makes nursing homes with lower mortality more popular.

Our results do not provide evidence for strong relationships between various staffing indicators and performance on health outcomes. This may seem surprising since some studies report that adverse outcomes are related to, for example, lower (registered) nurse employment (Friedrich and Hackmann, 2021; Lin, 2014) and higher nurse turnover (Antwi and Bowblis, 2018). However, the findings from a literature review (see Spilsbury et al. (2011)) suggest that the evidence on this topic has been contradictory. Our results do indicate that nursing homes with larger shares of specialists – like geriatricians and psychologists – relative to total staffing are likely to have lower mortality. Although this relationship is statistically significant, in economic terms it is relatively weak.

Finally, the reported coefficients in Table 4.6 suggest that the size of nursing home organizations is linked to performance on avoidable hospitalizations. Keeping the other observed characteristics fixed, an organization with 6 additional facilities (equal to one standard deviation) is associated with a 0.6 percentage points higher avoidable hospitalization rate. This implies that nursing homes that belong to a larger (chain) organization score worse on avoidable hospitalization performance. This finding is in line with quantitative evidence from the United States (Grabowski et al., 2016; You et al., 2016), who argue that this relationship could be explained by chain targeted nursing homes being of lower quality because of, for example, a poor financial situation, both before and after acquisition. Qualitative evidence suggests that differences between low and high hospitalization nursing homes are related to how the staff approaches the decision to hospitalize (Cohen et al., 2017). Nursing homes with low rates generally make this decision case-by-case, whereas those with higher rates are more likely to approach it as an algorithmic process. The decision process may well be related to whether the nursing home belongs to a non-chain organization since they are characterized by having more autonomous staff (Kruzich, 2005), being more flexible in care provision (Lucas et al., 2007) and since staff may have a more personal relationship with the residents.

Table 4.6: Multi-variate regression with nursing home characteristics

	Mortality (1)	Avoidable hospitalization (2)
Facility level		
Number of people on waiting list	-0.023*	0.008
	(0.013)	(0.020)
Number of clients	-0.000	-0.005
	(0.002)	(0.004)
Organisation level		
Number of facilities	0.000	0.001**
	(0.000)	(0.000)
Operating profit margin	-0.000	0.001
	(0.000)	(0.001)
Solvency ratio	0.000	0.000
	(0.000)	(0.000)
Liquidity	0.000	-0.000
	(0.000)	(0.000)
FTE per client	0.009	0.005
	(0.006)	(0.017)
Percentage high educated nurses	0.004	-0.000
	(0.008)	(0.012)
Percentage specialists	-0.070**	-0.137
	(0.028)	(0.125)
Staff turnover	-0.027	-0.010
	(0.018)	(0.047)
Staff absenteeism	-0.039	0.287
	(0.115)	(0.251)
Expenditures on external staff	0.036	-0.003
	(0.029)	(0.063)
Constant	0.014	-0.004
	(0.014)	(0.025)
R-squared	0.036	0.053
N (facilities)	540	540
n (clusters = organizations)	177	177

Regression results of two multivariate regressions with either nursing home specific performance on mortality (column 1) or avoidable hospitalization (column 2) as dependent variables. It uses the performance estimates obtained in Equation (4.2) after shrinkage. Nursing home characteristics are either at the nursing home facility level or at the organisation level, which are copied to all facilities within the same organisation. Descriptive statistics of nursing home characteristics can be found in Appendix Table C1

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

4.7 Conclusion and discussion

As the quality of care in nursing homes has been the subject of vigorous public debate for decades, the sector might benefit from improved performance measurement based on health outcomes to complement the often-used structure and process measures (Barber et al., 2021; OECD, 2005; OECD and European Commission, 2013). However, quality estimates tend to rely on self-reported outcomes, be based on a small sample of residents, and be hampered by selection bias. The question is whether these challenges can be addressed and how outcome information can be used best to evaluate performance.

We have addressed the following three questions in this paper. First, how large is the variation in health outcomes across nursing homes? Second, to what extent can this variation be attributed to differences in performance of the nursing homes rather than to unobservable differences in case-mix? Third, is there any relationship with quality indicators based on structure and processes? If such existing indicators to a large extent explain variation in health outcomes, then complementing information on structure and processes with information on outcomes may be of less importance.

We use detailed administrative data to estimate variation in performance on outcomes - mortality and avoidable hospitalization risk – when correcting for observable case-mix differences. In addition, we apply a novel test developed in the value-added literature to examine the role of selection bias in nursing home outcomes. Finally, we examine how these outcomes relate to nursing home characteristics on other dimensions.

After controlling for differences in case-mix, we find substantial heterogeneity in clients' health outcomes across Dutch nursing homes. Due to the small population sizes, the estimates are relatively imprecise, but we can statistically distinguish top performers from bottom performers. We find that the probability of dying or being hospitalized within 180 days after admission is 7 to 14 percentage points higher in the five percent worst performing nursing homes compared to the best. The variation in the avoidable hospitalization outcome is comparable to the variation in rehospitalization rates of Skilled Nursing Facilities in the United States reported by (Rahman et al., 2016), 2016. Moreover, we do not find that unobserved heterogeneity in client characteristics due to non-random selection into nursing homes leads to biased performance estimates. The correlation with other indicators of provider quality is limited, indicating that outcome-based estimates supplement existing process and structure indicators.

Although our findings suggests that nursing homes vary in terms of outcomes, the imprecision in the point estimates is large compared to the observed differences. This means that even when using detailed data and noise-reducing methods like empirical Bayes, it remains difficult to measure variation in outcomes of small-scale providers. As a consequence, differentiating between nursing homes based on performance on outcomes, for example when benchmarking and in pay-for-performance schemes, should be executed with caution, especially when considering the non-extremes.

Our results imply that the observed variation in nursing home outcomes unbiasedly predicts variation in causal mortality and avoidable hospitalization performance. This is important, as it means that case-mix adjustment based on observable characteristics is sufficient for measuring nursing home performance based on outcomes, at least on average. However, selection bias may still be an issue in other settings: this study only includes non-profit nursing homes and things may be different in other institutional settings, e.g. for-profit nursing homes may have stronger incentives to attract healthier clients (Gandhi, 2023). Also the exclusion of nursing homes with fewer than 50 admissions is important: since our descriptive results show that there is at least some selection into larger vs smaller nursing homes based on observables, there might also be selection based on unobserved characteristics into these smaller homes.

While our results may be seen as reassuring with respect to selection at the aggregate level, they do not imply that observed performance is unbiased for every nursing home separately. Any selection bias due to unobserved heterogeneity may cancel out if it happens to be in the negative direction for some nursing homes and in the positive one for others. While the average bias could then be zero, it may still result into misclassification of certain high performers as some of the lowest, as was observed in the case of hospitals (Geweke et al., 2003; Gowrisankaran and Town, 1999; Hull, 2020). However, given that estimating a bias for individual entities requires sufficient statistical power, this investigation was not feasible in our setting with small providers. The question of whether case-mix adjusted performance on outcomes can be used to promote quality improvements through pay-for-performance incentives remains thus an open question.

All in all, our results suggest that in designing policies to improve the quality of nursing home care, such as public reporting of quality, the dashboards should be expanded with outcome measures. This is especially important in the long-term care sector for which expenditures are expected to grow but for which information on outcomes is limited. Having such additional information is useful, both for nursing homes themselves to identify where improvements may be achievable, and for users aiming to make more informed choices. As our findings suggest that there is no detectable selection bias at the aggregate level, directing users to the nursing homes with the best observable case-mix adjusted outcomes could generate positive benefits in terms of health.

Chapter 6

Do delayed admissions to nursing homes increase hospital use?

With Pieter Bakx, Nigel Rice, Rita Santos, Luigi Siciliani & Bram Wouterse

Abstract

We study negative spillovers of delayed nursing home admissions on hospitalisations. Using Dutch administrative data at the individual level, we exploit plausibly exogenous variation in within-region congestion for admission to nursing homes to account for a potential selection bias in delays. Our instrumental-variable approach suggests that delaying a nursing home admission by one month increases the risk of an urgent hospitalisation by 1.4 percentage points (11% of the mean urgent hospitalisation probability). The effect is twice as large for individuals with dementia. We find similar effects when we restrict hospitalisations to falls and document an increase in the number of days spent in a hospital conditional on a hospital visit. This implies that the returns of policies and investments targeted at improving access to nursing homes can spill over to the hospital sector.

6.1 Introduction

Ensuring timely access to health care and long-term care is a major policy concern in many OECD countries (OECD, 2021a). Population ageing and limited public funds likely generate increases in excess demands and thus limit access further. These concerns are particularly acute in the long-term care sector, which is labour intensive and predominantly provides care to the older population (OECD, 2020b), which mean the amount of excess demand may increase rapidly in the future. Understanding intersectoral spillovers of excess demand for long-term care is important for decisions on the optimal allocation of public funds across sectors and setting appropriate incentives for healthcare providers and patients.

We study spillovers of excess demand for long-term care to the hospital care sector. Excess demand for long-term care may create spillovers when less or less timely long-term care may lead to health problems that require medical care, such as emergency department visits caused by falls or malnutrition (Crawford et al., 2021; Serrano-Alarcón et al., 2022).[1] Such spillovers impose a cost on society through the value of a health loss and the additional cost of treating it.

Specifically, we evaluate if delayed nursing home admissions generate a negative externality to the hospital sector through urgent hospitalisations. Whether delayed admissions increase health care use is an empirical question. Delayed admissions are to a large extent driven by waiting lists, which may be a successful way of triage if they allocate scarce care to those who need it the most by incentivizing individuals who benefit most from care to seek care at an alternative provider with no waiting list. However, this is not necessarily the case in long-term care for three reasons. First, people with severe cognitive impairment or other limitations may underestimate the benefits of receiving care (Finkelstein and Notowidigdo, 2019). Second, if waiting times are stochastic, individuals with low care needs may switch to low-demand providers which increases the waiting times for everyone, including people with high care needs (Leshno, 2022). Third, nursing home care providers decide on who is prioritized and may have an incentive to admit healthier individuals to reduce costs or workload, especially when the nursing home is close to full capacity (Gandhi, 2023; He and Konetzka, 2015).

We exploit administrative data for the full population from the Netherlands on hospitalisations and nursing home admissions in 2015-2019. For each individual, we measure how long individuals wait before accessing a nursing home, and whether they are hospitalised urgently (according to the physician in place) within a year. To address non-random selection into delays, we adopt an instrumental variables approach. In line with (Godøy et al., 2019; Hoe, 2022; Prudon, 2023), we instrument individual's delay to access a nursing home with congestion within the region of residence. Congestion is measured by the average delay of other individuals with similar care needs who enter

[1] Spillovers could also occur if patients waiting for appropriate care receive health care that is an imperfect substitute, for example when hospitals keep patients longer than necessary because all nursing homes are full (Gaughan et al., 2015; Moura, 2022).

the waiting list for a nursing home admission around the same time. We show that these fluctuations strongly predict the individual's delay and explain that the timing of eligibility is plausibly exogenous.

We find that individuals who experience longer delays to be admitted to a nursing home are more likely to be hospitalised. More precisely, delaying a nursing home admission by one additional month increases the probability of an urgent hospitalisations by 1.4 percentage points, equal to 11 percent of the average urgent hospitalisation rate. This effect is mostly concentrated amongst individuals with moderate dementia care needs and those who were hospitalised following a fall. We also explore possible underlying mechanisms and show that our finding is likely to be explained by delays causing a shorter exposure to the protective environment of a nursing home rather than health deterioration while waiting for a place in a nursing home.

We make two contributions to the literature. First, we contribute to the literature (reviewed in more detail below) on spillover effects between health and long-term care. Previous work on this topic focused on the spillovers of limited availability driven by spending cuts, additional supply of care teams, cost-sharing or eligibility requirements (Bakx et al., 2020b; Crawford et al., 2021; Forder, 2009; Kim and Lim, 2015; Moura, 2022; Serrano-Alarcón et al., 2022; Tenand et al., 2021). Our paper is the first to focus on delays, which are a consequence of triage through waiting list. This differs from rationing through cost-sharing or eligibility requirements as i) it is both demand and supply driven; ii) the supply side plays a role in the allocation of care; and iii) they can vary over time and across regions. Understanding the consequences of delays is important as they are a natural consequence of excess demand when prices are regulated, which is the case in virtually all countries (Barber et al., 2021).

Second, we contribute to the literature on the causal effect of delayed treatments by focusing on the long-term care sector and by evaluating the consequences in another sector. Prior studies investigated the effect of delayed health care (e.g. longer waiting times for coronary bypass surgery, hip replacement surgery or psychotic treatments) on health outcomes (Moscelli et al., 2016; Nikolova et al., 2016; Reichert and Jacobs, 2018), or disability insurance on labour market outcomes (Bolhaar et al., 2019; Deshpande and Li, 2019). Unlike the literature mentioned above, we look at outcomes that are not a direct consequence of the treatment itself. There are two other studies by Godøy et al. (2019) and Prudon (2023) who investigate spillover effects of waiting times for orthopedic surgeries and mental health care on the labour market. Our study focuses on the impact of delays for long-term care on health care use. This is particularly relevant because these sectors share common resource, such as nurses and public budgets, and treat the same patients but are typically organized and financed separately.

6.2 Background

6.2.1 Related literature

Our study contributes to the broader literature on spillover effects within and across sectors. For example, within the health sector, Pinchbeck (2019) provides evidence that improved access to primary care can reduce hospital emergency visits in England, therefore generating a positive spillover from primary to secondary health care. Within the long-term care sector, there is extensive literature investigating the interface between informal care (e.g. provided by a spouse) and formal care (e.g. provided by a professional or care provider). For example, Kim and Lim (2015) find that subsidies to formal home care reduced informal care provision. In the opposite direction, there is evidence that informal care provision reduces nursing home and home care use (Charles and Sevak, 2005; Van Houtven and Norton, 2004).

Our focus is specifically on the interface and possible spillover effects between health and formal long-term care on which there is a growing literature within a causal framework.[2] The relationship can go in both directions: from post-acute health care to nursing homes (Einav et al., 2021; Eliason et al., 2018) or from nursing homes to health care (Bakx et al., 2020b; Gaughan et al., 2015; Moura, 2022; Serrano-Alarcón et al., 2022). For example, entry and reimbursement rules in the United States post-acute care sector are found to reduce nursing home care utilization. Einav et al. (2021) find that the entry of post-acute care hospitals can substitute care provided in nursing homes and creates inefficiencies as care provided in hospitals is more costly. Similarly, Eliason et al. (2018) shows that post-acute care providers respond to financial reimbursement incentives by discharging patients later to nursing home facilities.

We focus on spillovers from care provided by nursing homes to the health care sector. Related studies use regional variation to examine the effect of long-term care on health care utilisation. For example, Gaughan et al. (2015) and Moura (2022) exploit regional variations in the supply of nursing home care in the United Kingdom and Portugal and find that it increases hospital length of stay. Other studies consider differences in public long-term care spending cuts or expansions across countries (Costa-i Font and Vilaplana-Prieto, 2022) and across smaller geographical units (Crawford et al., 2021; Forder, 2009). Their findings generally support the evidence of spillovers from long-term care to health care, documenting negative effects of long-term care spending on emergency department utilization and health care spending, though Crawford et al. (2021) find no effect on inpatient admissions or outpatient visits. There are a few other studies in which spillovers are identified using the introduction of long-term care insurance (Feng et al., 2020) or plausibly exogenous variation in eligibility for long-term care insurance benefits (Bakx et al., 2020b; Kim and Lim, 2015; Serrano-Alarcón et al., 2022). The latter three studies compare individuals whose access to long-term care insurance benefit is determined by the leniency of a randomly assigned eligibility assessor (Bakx et al., 2020b; Serrano-Alarcón et al., 2022) or by an eligibility threshold

[2] See Spiers et al. (2019) for an overview including non-causal studies.

(Kim and Lim, 2015). All studies find evidence of spillovers from long-term care to (urgent) health care use or spending. Still, the impact of the actual use of nursing home care and the mechanisms behind the spillovers reported remains unexplored. We contribute to this evidence by focusing on delayed admissions to nursing homes in the context of the Netherlands.

6.2.2 Institutional context

Organization and financing of formal long-term care in the Netherlands
Nursing home care is organized and financed through the universal Long-term Care Act (in Dutch: Wet langdurige zorg). This care is coordinated by regional single-payers which contract providers, with whom they negotiate volume caps and prices.

Providers are private organizations. These organizations are subject to regulation. For example, they should adhere to certain quality standards, and they are prohibited from making a profit (Bakx et al., 2021). There is a small, but increasing, number of for-profit providers. Capacity is fixed in the short run because of personnel and real estate shortages and the contract requirements set by the regional single-payers are seen as an obstacle to enter the market (ACM, 2021).

Providers receive a per diem rate, which is adjusted for the intensity of care, but not for the income or wealth of residents or other factors.

Individuals apply for insurance benefits at an independent agency. The main eligibility criterion is that the applicant requires round-the-clock supervision and/or care.[3] The agency decides on i) whether the applicant meets the eligibility criteria; and ii) the intensity of care that the person is eligible to receive, which is referred to as a care profile.[4]

Recipients pay a relatively low co-payment, which is dependent on their income, wealth (Tenand et al., 2021) and whether they receive care in the nursing home or in their own home, but the co-payment does not depend on the provider from whom they receive care. This means there are no price differences across providers for the care recipient.

Individuals who are eligible for insurance benefits choose between: i) in-kind nursing home care; ii) in-kind care at home (in Dutch: Modulair Pakket Thuis or Volledig Pakket Thuis); or iii) a voucher to contract a provider themselves (in Dutch: Persoonsgebonden Budget). Given that the latter group would never use their voucher for an admission to a regular nursing home, we focus on recipients of in-kind care (93% of all eligible in 2020 (Statistics Netherlands, 2023c,b)), for whom all costs are covered, including room and board in case of a nursing home admission.

In the Netherlands, like in various other OECD countries, nursing home care mainly refers to long-term institutionalized care, which is medicalised and typically serves an

[3]The eligibility process is depicted in Figure E1 in the Appendix.
[4]The decision should be exclusively driven by care needs, and hence not be affected by waiting lists (Bakx et al., 2021).

older population with multiple and complex (mental) health conditions, such as heart disease and dementia (de Bienassis et al., 2020; SCP, 2021). Most nursing homes organisations employ medical staff which take over most primary and medical care services, except for hospital visits. Similar as in the United States, some nursing homes also provide short-term rehabilitation or post-acute care at separate departments or locations. These individuals are financed through another scheme which means we do not observe them in our data. Nonetheless, the majority of nursing home residents in the Netherlands move permanently, implying that most people who are admitted to a nursing home stay there for the remainder of their life (Bom, 2021).

The alternative to a nursing home admission is to receive formal care at home. This may involve regular visits of a care professional, washing and dressing and other daily tasks, depending on what the individual needs (SCP, 2019b). For individuals who are eligible for long-term care insurance benefits who choose to receive care at home, this home care can be (and is often) complemented by resources and medical care services from two other schemes, such as the provision of a wheelchair, visits to the general practitioner and medication.[5] This implies that people who receive in-kind care at home may be more likely to consume services beyond the Long-term Care Act compared to people who receive in-kind nursing home care.

Delays in access to nursing homes
People who are eligible to receive care paid for through the Long-term Care Act can choose any provider that is available at that point. Preferences for where to receive care - either in at home or in a nursing home - and choice of provider are given to their regional care office, who can connect them to their preferred provider. If the recipient's preferred provider has no bed available, the recipient has two options: i) either temporarily receive in-kind care at home while waiting for an available bed; or ii) be immediately admitted to another nursing home. In both cases, they may be placed on the waiting list for a bed at their preferred location. If there are no beds available at any provider within the recipient's region of residence, this alternative provider can be located in a different region.[6] Some individuals applied for eligibility out of precaution and choose to wait and receive care in their own home, even if there is a bed available at their preferred provider.

Management of waiting lists for nursing homes
The waiting lists are managed by nursing home providers and monitored by the regional care offices. Nursing homes decide which recipients to admit based on the time of application and level of urgency (Hanning and van Vliet, 2016). The regional care offices monitor the waiting lists with a focus on preventing eligible individuals experiencing a very long waiting time. The maximum acceptable waiting time for nursing home care – set by a group of representatives consisting of care providers and insurers

[5]The Social Support Act (Wmo) organizes and finances assistance in daily living, such as help with cleaning or groceries. This care is organized by municipalities(Bakx et al., 2021). In addition, the Health Insurance Act, covers medical care, such as visits at the general practitioner or hospital, medication. Both schemes have universal coverage and also provide home care to individuals who are not eligible for a nursing home admission.

[6]In practice, it never happens that all nursing home beds in the Netherlands are occupied.

– during our study period was 1.5 months (NZa, 2021). However, given excess demand, this target became infeasible and the maximum wait has been increased to 6 up to 12 months in 2021, depending on whether they wait for a preferred provider (12 months) or not (6 months) (Actiz, nd). In 2022, 11 percent of the individuals on the waiting list waited longer than the maximum period of 12 months (Zorgverzekeraars Nederland, 2023).

6.3 Data

6.3.1 Data sources

We use administrative data at the individual and household level from Statistics Netherlands. To identify delays of nursing home admissions, we link data on an individual's eligibility status required to access a nursing home (including the effective date) from the Central assessment agency (CIZ) to data on long-term care use from the Central administration office for long-term care insurance (CAK). At the individual level, we also link data on inpatient hospital admissions (including date and diagnoses) from all hospitals in the Netherlands from Dutch Hospital Data. Linkages are based on an exact match using (pseudonymized) identifiers for each individual. More information about the data and corresponding sources can be found in Appendix Table E4.

6.3.2 Study population

The study population consists of individuals who became eligible to receive care in a nursing home between 1 April 2015 and 31 December 2018.[7] Because individuals have been assessed as being eligible and hence require round-the-clock supervision or care, this population is well suited to analyse the impact of postponing nursing home admissions, as lack of round-the-clock supervision could, for this group, lead to adverse health outcomes.

We restrict the study population in five ways (see also Appendix Table E3 for the exact numbers). First, we create a more homogeneous population by removing those who purchase nursing home care via personal budgets before admission[8], and those who

[7] We restrict to 1 April 2015 to be able to construct the instrumental variable (which is based on people who were eligible up to 3 months before). We restrict to 31 December 2018 to observe hospitalisations within one year after eligibility but prior to the pandemic.

[8] Instead of receiving in-kind services, one could also opt for receiving an earmarked personal budget (PGB) to self-purchase long-term care services, for instance at a for-profit provider. This scheme is increasingly being used, but it only constitutes 7 percent of all care recipients within the Long-term Care Act in 2019 (Statistics Netherlands, 2022a). Because we do not have full information about the care that these individuals use, they are excluded from this study. In some cases, in 2017 approximately five thousand, individuals utilize the in-kind scheme for formal home care from the Long-term Care Act to receive nursing home care at a for-profit facilities and pay room and board out of pocket (Hussem et al., 2020). We do not identify these individuals, and they are therefore (mis)classified delaying their nursing home admission. If anything, we expect this would bias our results to zero, and our estimate would be a lower bound of the true effect since these individuals may benefit from receiving care in a nursing home while they are reported to be waiting at home.

are at younger than 65 years at eligibility. Second, we exclude those who delayed their admission by more than one year, as this likely reflects a strong preference to receive care at home instead of in the nursing home. Third, we drop those admitted out of a critical situation, identified by receiving their eligibility while in a hospital, also to limit the influence of hospital re-admissions. Fourth and most notably, to equalize the exposure to hospitalisations to one year, we exclude those who died within one year after eligibility. We examine whether selection on survival affects our results in Section 6.5.5. To focus on permanent nursing home admissions only, we exclude individuals if they (temporarily) moved out of the nursing home after their first admission and within one year after eligibility. Finally, we drop individuals with missing data on the covariates. This results in a sample of 72,762 individuals.

6.3.3 Care profiles

Because nursing home residents constitute a particularly heterogeneous group, our sample is split into three groups, defined by a (combination of) care profile(s) (in Dutch: Zorgzwaartepakket - ZZP). A care profile is set by an independent agency when granting eligibility and reflects the individual's care needs. We use the care profiles that grant access to admission to a nursing home and make three groups (see also Appendix Table E2).[9] First, we differentiate between people with moderate vs high care needs.[10] Then, among the people with moderate care needs, we split between people who require care for dementia or related conditions and people who require care for all other somatic morbidities. We make this additional split because providers generally have separate departments (and therefore separate waiting lists) for these two groups because of their different needs: whereas people with dementia need space to wonder around and protection to prevent them from walking away, people with physical impairments require assistance with getting out of bed, washing, eating and drinking. The high-need group also includes people with dementia or related conditions, but they only make up for 4 of all high-need individuals.

6.3.4 Measuring delays

Our variable of interest is the delay in nursing home admission. As shown in Figure 6.1, we define this as the number of days between the date on which the central assessment agency decides that an individual is eligible for admission to a nursing home and the (first) admission to the nursing home (Basis Wettenbestand, 2018).[11,12] The date of admission to the nursing home is the date when an individual starts paying co-payments for receiving institutionalized care within the Long-term Care Act. For example, if

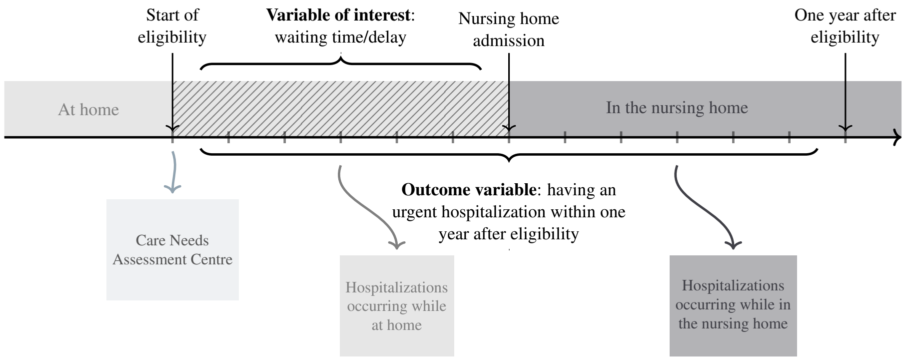
[9] Individuals eligible for rehabilitative or palliative care are excluded.

[10] We do not have a low-need group as all individuals who are granted eligibility require a minimum level of care needs.

[11] In some cases, for example, if eligibility is received in the hospital or another crisis situation, eligibility can be adjusted retrospectively to the date of the request. To limit the influence of such adjustments, we drop those who received their eligibility while staying in the hospital.

[12] In practice, people can have multiple eligibility assessments, for example, when they require additional care and move to another care profile. We focus only on an individual's first positive assessment.

Figure 6.1: Timeline for someone admitted to a nursing home six months after eligibility



Notes: Figure depicts an example of someone admitted to a nursing home within 6 months after eligibility. The figure shows that: i) delays to nursing home admissions are measured by the number of days between the start of eligibility to receive care in a nursing home and the admission to a nursing home; and ii) outcomes are measured over a one-year period after the start of eligibility, and can be observed either during the delay period at home or in the nursing home.

someone's eligibility decision is made on January 1st 2017, and they start incurring costs for receiving nursing home care on July 1st 2017, their delay is 181 days. As mentioned in the previous paragraph, we drop all individuals who delay by more than one year, which is the maximum acceptable waiting time when waiting for a preferred provider (Actiz, nd). We cannot observe whether an individual delays a nursing home admission because of preferences to receive care in at home or because there are no beds available at the preferred provider.

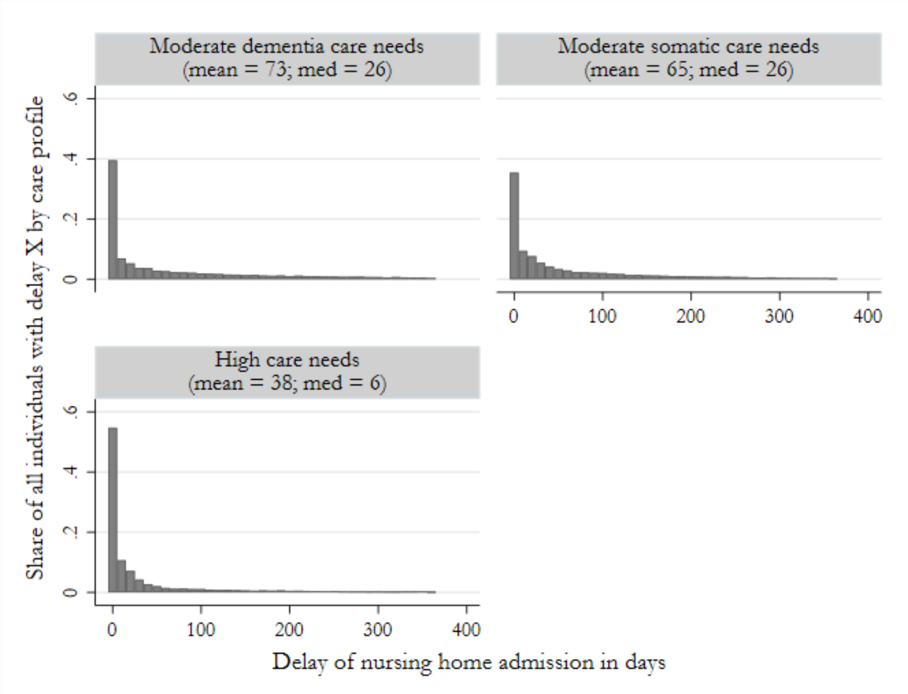
Figure 6.2 shows that the distributions of delays are right-skewed for all three care profiles: most people have no or short waits. While many individuals experience delays of less than 10 days (between 35% and 55%), a significant proportion (22% to 43%) have delays of more than 42 days (i.e. the maximum acceptable waiting time of 6 weeks). There is heterogeneity in delays across care profiles, with those with the highest care needs (Higher care profile) having the shortest delays.

6.3.5 Urgent hospitalisations

Our outcome of interest is the occurrence of an urgent hospitalisation. A hospitalisation is classified as urgent if the hospitalisation occurred within 24 hours following a decision to admit from a physician. More than 90 percent of urgent hospitalisations in our sample in 2018 are redirected from the emergency department.[13] The most common (main) diagnoses for the first urgent hospitalisation are: hip fractures (15%), heart failures (8.9%), pneumonia (8.0%), disorders of the urinary system (including

[13] We only observe information on re-directions from the emergency department in the year 2018.

Figure 6.2: Distribution of delays in nursing home admissions by care profile



Notes: Figure shows the distribution of delays by care profile. The x-axis represent the number of days of delay for a nursing home admission in bins of 10 days. The y-axis represent the share of all individuals with the specific care profile that delayed by the number of days within each of the bins.

urinary tract infections) (6.9%), and stroke (4.6%) (the 20 most common diagnoses are reported in Appendix Table E5). Focusing on urgent hospitalisations only limits the influence of possible anticipation effects (for example, associated with planned care) that might also influence the timing of a nursing home admission.

To evaluate the impact of nursing home admission delays on hospitalisation risk, we measure whether each individual in our sample had an urgent hospitalisation within one year following eligibility for a nursing home (see also Figure 6.1). This implies that for most individuals, the period covers both the time spent at home while waiting for a place in the nursing home and the time spent in the nursing home since admission to the nursing home. Some individuals will be admitted directly to a nursing home after eligibility, that is, they have a waiting time of zero days.

6.3.6 Control variables

We control for various proxies for an individual's underlying health to limit the potential bias occurring from differences in health between people who delay and people who do not (or briefly) delay. Table E4 provides a full overview of all control variables and definitions.

To control for demographics, we include sex and age at eligibility (in 5-year age groups). Additionally, we correct for underlying health using information on the individual's care profile and prior care utilisation. These include: i) care profile at eligibility, either dementia, less intense or more intense care needs; ii) healthcare expenditures for primary and hospital care services covered by the basic insurance package in the calendar year prior to eligibility for nursing home care (i.e. defined at the start of our observation period); iii) an indicator for whether an individual was hospitalized in the month before eligibility; iv) 17 dummies for Charlson co-morbidities (see also Sundararajan et al. 2004), based on diagnoses following a hospital visit in the year prior to eligibility; v) 18 dummies for relevant types of medicines consumed in the calendar year prior to eligibility, selected using Lasso[14]; and vi) an indicator for whether someone received eligibility status during the flu season. Furthermore, we include the individual's household wealth (excluding housing wealth) in the calendar year before eligibility and whether they were a house owner.[15]

Finally, we include year and regional fixed effects to account for differences across regions and years that could affect delayed admissions to nursing homes and hospital care use, for example, the supply of nurses. Regions are based on 31 regional care office areas within which long-term care is purchased and facilitated. They vary in terms of size and demographics, ranging from a population size of 180 thousand to 1.3 million with a proportion of 65 years and over between 74 and 80 percent (Statistics Netherlands, 2023a), and consequently face different levels of demand and supply for long-term care. For example, the northern regions have the lowest shares of individuals on waiting lists (less than 6 %), compared to some of the the south-eastern regions (more than 16 %) (see NZa 2021 for a greater discussion of differences in long-term care across these regions).

6.3.7 Descriptive statistics

Table 6.1 presents descriptive statistics for the full sample and separately by care profile. Across the whole sample, 68.6% are women, 54% are 80-89 years old, 4.4% had a hospitalization in the 30 days prior to eligibility. On average, the sample incurred €3,819 on hospital expenditures and €352 on general practitioners care in the calendar year prior to eligibility. About 34 percent of the sample owned a house in the calendar year prior to eligibility. One third received the eligibility status during the flu

[14]Selected from a linear model with our urgent hospitalisation outcome as a dependent variable and all 3-digit ATC-codes as explanatory variables.

[15]We also considered disposable household income, but wealth is more informative as a proxy of socioeconomic status for the age group we study(Spiers et al., 2022).

Table 6.1: Descriptive statistics of study sample, total and by care profile

	Full sample	By care profile		
		Moderate somatic care needs	Moderate dementia care needs	High care needs
Outcome: Urgent hospitalization (%)				
	15.9	20.0	12.9	18.1
Variable of interest: Delays (in days)				
	63.6	64.9	73.0	38.3
Instrumental variable: Average delays in region, period and care profile				
	52.5	54.6	60.6	29.2
Covariates (excl. medication and Charlson comorbidity dummies):				
Women (%)	68.6	72.5	67.9	65.1
Age-group (%)				
65-69 years	3.5	2.0	3.1	6.4
70-74 years	7.5	4.7	7.9	9.9
75-79 years	14.6	10.9	16.2	15.2
80-84 years	25.1	23.5	27.0	22.2
85-89 years	28.9	32.6	28.9	24
90-94 years	16.4	20.9	14.0	16.8
95+ years	4.1	5.3	2.9	5.5
Healthcare exp. on GP care (x1000€)	0.352	0.366	0.332	0.386
Healthcare exp. on hospital care (x1000€)	3.819	4.236	2.587	6.395
Hospitalization in last 30 days	4.4	4.8	3.3	6.3
Wealth (%)				
<€5,000	21.5	22.5	20.1	23.5
€5,000-€20,000	25.9	28.8	25.0	24.7
€20,000-€50,000	23.9	24.2	24.2	22.7
>€50,000	28.7	24.4	30.7	29.1
Home ownership (%)	34.4	28.6	37.4	34.4
Eligibility in flu season (%)	29.5	29.1	30.1	28.7
Year of eligibility (%)				
2015	16.5	15.7	16.6	17.3
2016	24.9	24.6	24.7	25.8
2017	27.5	28.1	27.5	26.8
2018	31.1	31.6	31.3	30.0
Observations	72,762	19,556	38,125	15,081
(%)	100	26.9	52.4	20.7

season, and the largest proportion of the sample (31 percent) in the year 2018. In addition, people who are about to enter a nursing home with dementia care needs constitute 52 percent of the sample and are generally a bit younger, more wealthy, and consumed less medical care prior to eligibility compared to the average.

6.4 Empirical Strategy

6.4.1 Baseline estimation

Our regression model is specified as the following linear probability model[16]:

$$Hosp_{irc} = \alpha + \beta Delay_{irc} + X_i\gamma + \nu_r + \rho_c + \varepsilon_{irc}, \tag{6.1}$$

where $Hosp_{irc}$ is a binary variable that represents whether individual i in region r with care profile c had at least one urgent hospitalisation within one year after eligibility, α is the constant, and ε_{irc} is an idiosyncratic error term. We are interested in the impact of $Delay_{irc}$, which is the number of days an individual's nursing home admission was delayed. The parameter of interest, β, represents the effect of a one-day delay in nursing home admission on the risk of having an urgent hospitalisation within one year following eligibility. A hospitalisation, within one year of eligibility, can occur while the individual is at his/her place of residence or already at the nursing home.

Since health can be both related to the urgency of the nursing home admission and hospital use, insufficient control for differences in health and need for health care may create bias in the key relationship of interest between delays and hospitalisation. Therefore, we include a vector of individual level controls X_i (see also Section 6.3.6). The impact of delays for nursing home admissions on hospital care use could also be biased by (unobserved) regional differences, for example, if there is regional variation in unobserved care needs, or in labor supply, which may both affect the long-term care and health care sectors. To control for such between region variation, we include a region fixed effect ν_r. The same holds for differences across care profiles, as these have different (health)care needs. At the same time, while formally there are no separate waiting lists, the allocation of nursing home beds is likely organized within care profiles, as they require different resources, such as a closed unit for people with dementia vs assistance with getting out of bed for others. To account for this, we include care profile fixed effects ρ_c and estimate the regression for the three care profiles separately.

6.4.2 Potential sources of endogeneity

The aim of this paper is to estimate the causal impact of delaying a nursing home admission on the risk of being hospitalised. Delays should therefore be exogenous to

[16]While our measure of outcome, hospitalisation, is binary, we prefer to specify the model as a linear probability model (LPM) to enable the use of two-stage least squares estimation to deal with the potential endogeneity of nursing home delay. Given the proportion of hospitalisations recorded does not fall close to 0 or 1 (the proportion of urgent hospitalisations range from 0.13 (moderate dementia care needs sample) to 0.20 (lower care needs sample) - see Table 6.1 - and that sample sizes are large, estimation from a LPM should approximate well estimates from a probit model.

other (unobserved) factors determining urgent hospital use. However, we identify two possible threats to identification.

First, both delays and the risk of hospitalisation may be driven by an individual's underlying health. We expect nursing home admissions to be more urgent for individuals who are most ill. On the demand side, sicker individuals may be less inclined to postpone their admission. On the supply side, providers may give priority to the most urgent cases. Both result in shorter delays. Unobserved health factors are likely to underestimate the causal estimate of the impact of delays on hospital care use if more severely ill clients are also more likely to be hospitalised. To mitigate these concerns, our empirical model includes a range of control variables related to patient health, such as healthcare expenditures in the past year, previous hospitalisations, prescribed medication, and a set of dummy variables for co-morbidities. We also estimate Equation (6.1) on sub-samples defined by care profiles.

Second, while delays in nursing home admissions may impact hospitalisations, hospitalisations may also impact delays. If someone is hospitalised and is assessed to be too frail to return home, they are put on an urgent waiting list to be admitted to an available nursing home as soon as possible (Zorgverzekeraars Nederland, 2023). This implies that a hospitalisation while waiting for a nursing home is likely to lead to a shorter waiting time, causing a downward bias in our estimate of interest.

6.4.3 Instrumental variable analysis

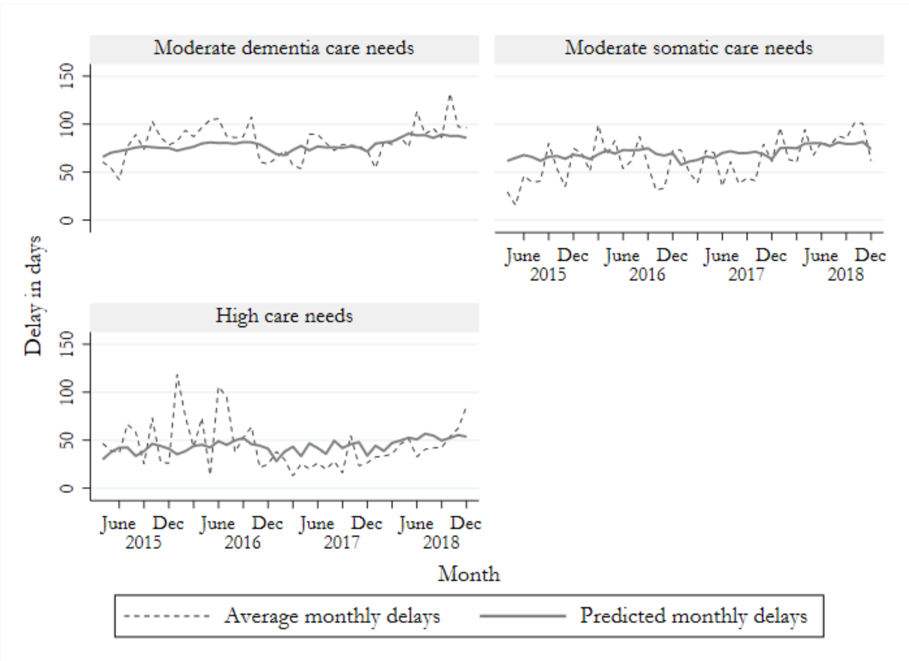
Defining the instrument
To account for potential endogeneity of the delay in nursing home admission, we follow Bensnes and Huitfeldt (2021), Godøy et al. (2019), Hoe (2022) and Prudon (2023) by exploiting plausibly quasi-random variation in congestion within regional markets over time. We define waiting list congestion as the average delay across all individuals, $j = 1, \ldots, J_i$, $(j \neq i)$ who became eligible in the time window starting 45 days before and ending 45 days after individual i's eligibility commenced, who reside in the same region, r, and have the same care profile, c, as individual i:

$$Congestion_{irc} = \frac{\sum_{j=1}^{J_i} Delay_j}{J_i}. \tag{6.2}$$

Variation in congestion is therefore derived from differences in the timing of eligibility, region of residence, and the care profile to which individual i belongs. We use the measure for congestion as an instrumental variable for the potentially endogenous nursing home delay in combination with region and care profile fixed effects in the first- and second-stage. With this, we exploit within region and care profile variation only.

Within regional variation in congestion (i.e. average delays) can be driven by (quasi-) random shocks in nursing home care demand or supply. The red line in Figure 6.3 shows how delays vary over time by care profile in the largest region in our sample (Utrecht). The differences between the red (actual average monthly delay) and the blue line (predicted monthly delay) illustrates shocks in congestion used for identification: the unexplained within region and care profile variation in delays over time. We observe that delays are in some periods systematically lower/higher than expected based on the characteristics of individuals entering the wait list (e.g. higher in September 2015 - September 2016 for people with moderate dementia care needs and lower in September 2016 - September 2017 for people with high care needs). Such systematic variations in delays arise, for instance, after a new nursing home facility enters the market, and if increasing salaries in other sectors create staff shortages in the nursing home market, and are expected to increase the delay of individuals who enter the wait list in this period.

Figure 6.3: Average and predicted delays for largest region in the sample



Notes: Figure shows for region with most observations in the sample (Utrecht) by care profile the actual average monthly delays (red thin line) and expected average monthly delays (blue thick line). Expected delays are predicted at the individual level using the coefficients of a regression with delays as a dependent variable and all covariates, year and influenza season fixed effects, and afterwards averaged over regions and care profiles.

First-stage equation

To obtain the causal impact of a delay to a nursing home admission on the risk of hospitalisation, we estimate a two-stage least squares (2SLS) regression with robust standard errors. The first stage exploits within regional variation in congestion by care profile as an instrumental variable such that:

$$Delay_{irc} = \lambda + \delta Congestion_{irc} + X_i\theta + \mu_r + \rho_c + \varepsilon_{irc}, \tag{6.3}$$

where $Delay_{irc}$ is the delay (in days) in the nursing home admission of individual i in region r with care profile c. $Congestion_{irc}$ represents the instrumental variable, defined in Equation (6.2). By including region and care profile fixed effects (μ_r and ρ_c), identification relies on within region and within care profile variation in congestion.

6.4.4 Assumptions

Identification requires the instrument satisfies the following three assumptions: i) waiting list congestion should strongly predict an individual's delay (relevance); ii) congestion levels should be exogenous within region, year and care profile groups (independence); and iii) nursing home congestion should affect hospital use only through delayed nursing home admissions (exclusion). There are two main threats to these identifying assumptions. First, shocks to the local health care economy that impact both nursing home congestion and hospital capacity. For example, a (sudden) shortage in nurse labour supply, could cause shocks in the hospital sector and affect the amount of care provided in nursing homes. This would violate the exclusion restriction if it affects hospitalisation rates. Given that we focus on urgent hospitalisations only for which such constraints are irrelevant, these shocks are unlikely to affect our outcome directly. In Appendix E we show there is no strong correlation between fluctuations in nursing home congestion and fluctuations in urgent hospitalisations among the population aged 65 years and over.[17]

A second threat is that the composition of individuals who receive eligibility may differ in times of congestion than when access is more readily available. This could happen if unhealthier people opt out of the system by seeking care in the for-profit market, or where individuals strategically apply for eligibility in advance to attain a more favourable place on the waiting list when congestion is particularly acute. We expect these factors to be of little influence since the for-profit market in the Netherlands is relatively small (the share of beds in the for profit market is estimated at less than 5 percent of all nursing home beds during our study period (SCP, 2019a)). In addition, if individuals behave strategically in times of congestion by applying for eligibility at an early stage than they would otherwise, then we would expect to observe the share of negative eligibility assessment results to vary with the level of congestion since assessments are based solely on the care needs of applicants (Bakx et al., 2021). In Section 6.5.2 and Appendix E we show that congestion is neither (strongly) related to the share of negative eligibility assessment results, nor to the share of people purchasing care with personal budgets (partly used for the for-profit sector), nor to observable proxies for individual health.

[17] The assumption would also be violated if there are specific time periods in the year in which congestion is especially high or low in both nursing homes and hospitals, such as in December or in the flu season. We therefore include becoming eligible during the flu season as an additional control, and include month-by-year fixed effects as a sensitivity test.

We estimate a local average treatment effect (LATE), which is driven by those who delay during periods of congestion (i.e. the compliers), for example, because they decide to wait for an available bed at their preferred provider. It excludes i) those who do not delay when there is congestion (i.e. never-takers), for example, because their admission is very urgent; and ii) those who delay when there is no congestion (i.e. always-takers), for example, because they would always prefer to postpone their admission to maintain independent living. We rule out defiers - individuals who delay when there is no congestion and do not delay when there is congestion. In the Section 6.5.2, we argue that the monotonicity assumption is likely to be satisfied.

6.5 Results

6.5.1 Main results

Full sample
Panel A of Table 6.2 presents the second stage results, $\hat{\beta}$, from Equation (6.1). Full sample results, presented in column (1), suggest that a one-day delay to a nursing home admission increases the probability of an urgent hospitalisation by 0.047 percentage points. The estimated coefficient is statistically significant at the 1 percent level and economically meaningful: individuals who delay by one additional month are 1.4 percentage points more likely to have an urgent hospitalisation within the year following eligibility. This is equivalent to a 11% increase relative to the mean and implies longer delays in accessing nursing home care increase urgent hospitalisations, at least at the extensive margin.

By care profile
To examine heterogeneity in the estimated effect, we differentiate between groups based on an individual's care profile at eligibility. This is useful for identifying groups that are particularly vulnerable to the consequences of delayed access to nursing home care. The results in columns (2) to (4) in Table 6.2 demonstrate that the estimated impact for the full sample (column (1)) is largely driven by people with dementia care needs. These constitute the largest group in our sample. For these individuals an additional month of delay increases the probability of at least one urgent hospitalisation within the year following eligibility by 3.1 percentage points. This represents a 24% increase relative to the mean for this group. While the effect size for individuals with high care needs is economically meaningful, it is too imprecisely estimated to neglect the possibility of a null effect. Even though the first stage results in columns (2) to (4) of Table 6.2 (Panel B) point to sufficiently strong first stages, particularly for the moderate dementia and high care needs groups, the second-stage estimated coefficients are somewhat imprecisely estimated. Accordingly, we interpret these results with some caution.

Delays having the largest effect on hospitalisations for individuals with dementia appears plausible. First, the disease profile and the appropriate treatment for mental health conditions such as dementia may be less obvious than for physical impairments.

Table 6.2: The effect of delayed nursing home admissions on urgent hospital use

	Full sample	By care profile		
		Moderate dementia care needs	Moderate somatic care needs	High care needs
	(1)	(2)	(3)	(4)
Panel A: Second stage result (outcome = urgent hospital use)				
$\widehat{Delay}$ (in days)	0.00047***	0.00101*	0.00013	0.00038
	(0.00018)	(0.00056)	(0.00084)	(0.00047)
Panel B: First stage result (endogenous var = delay in nursing home admission)				
Instrument: congestion	0.652***	0.335***	0.312***	0.676***
	(0.0282)	(0.0518)	(0.0599)	(0.0656)
F-statistic	534.0	41.8	27.1	106.2
Care profile fixed effects	Yes	No	No	No
Observations	72,762	38,125	19,556	15,081
Mean dept. var	0.1588	0.1291	0.1993	0.1813

All models include all covariates and year, region and care profile fixed effects. The reported F-statistic denotes the effective F statistic on the excluded instrument (see also Footnote 20).
Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

As a result, assessing the urgency of someone's nursing home admission (either by an individual, their relatives or a professional) may be more ambiguous for this group. Second, people with moderate somatic care needs mainly require assistance with daily activities like dressing and feeding, while people with dementia also require a protective environment. Extensive home care, consisting of regular visits to the care recipient, can more easily provide assistance with such daily activities, but may fail to create a consistent protective environment. Formal home care may, therefore, be more successful in substituting nursing home care to prevent hospitalisations for people requiring assistance with daily activities than for people who need round-the-clock supervision or care.

Comparison to OLS results

We report the results from a regression without accounting for selection bias from unobservables and reverse causality in Panel A of Appendix Table E6. The OLS-estimate for the full sample is 23% smaller compared to the IV-estimate reported in Table 6.2. This is in line with our expectations as we presumed a negative bias driven by: i) individuals with better underlying health likely having longer delays and a lower risk to be hospitalised; and ii) hospitalisations while waiting for a nursing home bed causing shorter delays (see also Section 6.4.2).

While the IV-estimate is larger than the OLS-estimate for the moderate dementia group, it is the other way around for individuals with moderate somatic and high care needs.

The difference could occur if the relationship between unobserved health and the hospitalisation risk or delayed admissions differs across care needs groups. Although we do not observe pronounced differences based on observable proxies for health (results available upon request), the decision to delay or not and the allocation of beds may differ between care profiles. Another potential explanation is that the group of compliers could alter between care profiles.

6.5.2 Assessing the instrumental variable assumptions

We argue that our instrument satisfies the assumptions required to obtain a causal estimate of delays on urgent hospitalisation by considering its relevance, the exclusion restriction and independence.[18]

Relevance
Panel B of Table 6.2 reports the first-stage results and provides support that variation in congestion strongly predicts individual level delays. Individuals who become eligible to receive nursing home care in a period in which the average delay (i.e. congestion) in their region for their care profile is one day longer on average wait 0.65 days longer to be admitted to a nursing home. The F-statistic to test the null hypothesis that the instrument has no effect on delays is large at 534 for the full sample estimates, and is far greater than conventional 'rule-of-thumb' values and more contemporary suggested values (for example, F as large as 105 to ensure a test with significance level 0.05, Lee et al. (2022)) to secure instrument relevance.[19,20] This suggests that our instrument is sufficiently strong.

Exclusion restriction
As discussed in Section 6.4.4, the exclusion restriction would be violated should shocks that affect nursing home congestion also affect hospital capacity, and therein hospitalisation rates. We cannot formally test this, but we can check whether within year, region and care profile variation in congestion correlates to within year and region variation in the number of urgent hospitalisations among all 65+ year-olds (see also Appendix E). The correlation equals -0.077, suggesting that variation in congestion in the nursing home sector is not strongly related to variation in urgent hospitalisations. Additionally, in Section 6.5.5 we show our results are robust to the inclusion of month-by-year fixed effects and hence not biased by seasonal differences.

[18]We also assume monotonicity, which in our setting means that individuals who delay when there is no congestion should also delay if they become eligible for nursing home care in another region or period when there is congestion. Appendix Figure E3 and E4 demonstrate that the relationship between congestion and individual delays is strictly positive for every level of congestion and subgroups of different care needs. Frandsen et al. (2023) argue that such findings at the group level are sufficient at least for a weaker monotonicity assumption as long as every individual complies to sufficient levels of congestion in sufficient region and time period combinations. According to the authors, under the other instrumental variable assumptions, the weaker monotonicity assumption is sufficient for a causal interpretation.

[19]See Lal et al. (2021) for a wider discussion.

[20]We report the effective F-statistic which is robust to heteroskedasticity and compare it to the Montiel Olea and Pflueger (2013) critical value of, in our case, 37.4 with a maximum bias of 5 percent and 23.1 with a maximum bias of 10 percent.

Independence
The independence assumption requires the composition of individuals (related to hospitalisation risk) to be independent of our instrument, nursing home congestion. We assess the plausibility of this assumption by examining the relationship between a broad set of individual characteristics and our measure of congestion. Table E7 in the Appendix demonstrates that the observed characteristics are unrelated to congestion (conditionally on year, region, care profile and influenza fixed effects, and on the other characteristics contained in Equation (6.1)). Almost all coefficients are not statistically different from zero at the 5 percent level. There are a few exceptions, but the magnitudes of these coefficients are relatively small when compared to the relationship with individual level delays (endogenous variable). This suggests that the observed characteristics of individuals who receive eligibility in times of high congestion are not different from those of individuals who receive it at times of low congestion.[21]

The test above does not rule out selection on unobserved characteristics. A part of the individuals' underlying health at baseline (eligibility) might not be measured by our proxies, but might be observable to the eligibility assessor. Hence, if individuals who apply for eligibility in times of congestion are in better (worse) unobservable health, we would expect the eligibility assessment result to be negative (positive) more often during these periods. However, we find no correlation ($\rho = 0.0020$) between congestion and the share of negative eligibility assessment results over time (results are reported in Table E) in the Appendix. This suggests that the individuals who apply for eligibility when there is congestion are not in (unobservably) better or worse health.

6.5.3 Characterising compliers

As shown in subsection 6.5.1, our IV-estimate exceeds the OLS-estimate. The difference can be attributed not only to a negative bias, but also to the composition of a Local Average Treatment Effect (LATE), particularly when the effect of delays on urgent hospitalisations for the group affected by the instrument (compliers) differs from the treatment effect among those who are not. To better understand whether the LATE is driven by treatment effects of individuals with specific characteristics, we estimate the first-stage for relevant subgroups separately. Examining the characteristics of compliers is a relevant policy question on itself, as it sheds light on which subgroups are most affected by the consequences of shortages of nursing home care.

Appendix Figure E4 shows the coefficient and 95 percent confidence intervals of the instrument congestion in the first stage regression using individual delays as an outcome for different sub-samples. Although most coefficients fall within each other's confidence bounds, there are some small differences. The coefficients are somewhat larger

[21] As an additional check, we examine the coefficient stability of the instrument by estimating the reduced-form regression with our urgent hospitalisation outcome on the instrument including and excluding our usual covariates (see also Appendix table E6). We find that the reduced-form coefficient is stable when including (observable) individual characteristics. This may give some confidence for that the relationship between our instrument and urgent hospitalisations is not confounded by selection, at least based on observable characteristics (Oster, 2019).

among individuals who had higher expenditures on GP and hospital care in the calendar year prior to receiving the eligibility status, and for those with a higher predicted probability to be hospitalized in the year after eligibility based on observed characteristics. On the other hand, delays of individuals who were hospitalised in the last 30 days before eligibility seem to be least affected by congestion, possibly because they would never delay as they likely require a very urgent admission to a nursing home.

This means that individuals with higher health care consumption, with the exception of those who were recently hospitalised, are more strongly affected by congestion and more representative in the group that drives the LATE. This is in line with our expectations as individuals in better health may always prefer to delay, and receive long-term care in their own home, irrespective of congestion in the market, while individuals in poorer health and with higher care needs will only delay when they need to.

6.5.4 Other outcomes

Hospitalisation related outcomes
In addition to examining the impact of delays on the probability of an urgent hospitalisation, we evaluate its impact on other related outcomes. The results are presented in Table 6.3. The first two columns show that the estimated coefficient for the probability of any hospitalisation within the year after eligibility is almost completely driven by urgent hospitalisations[22], and that the impact on the probability of non-urgent hospitalisations is economically small and statistically insignificant. If a longer exposure to nursing home care increases hospital care use because caregivers are more likely to notice health problems, we may expect a negative effect on non-urgent hospitalisations. We do not observe this.

We also consider the main diagnose for urgent hospitalisations, namely being hospitalised following a fall.[23] In our sample, 4.3% were hospitalised after suffering a fall within the year following eligibility. The estimated coefficient in column (3) of Table 6.3 shows that this risk was larger for those who delayed their nursing home admission by a longer period: delaying by an additional month increases the risk of such a hospitalisation by 0.5 percentage points ($= 0.00017 \times 30.5$), which is more than 10% of the average and explains about 30% of the effect on all urgent hospitalisations. Being admitted to a nursing home earlier may prevent falls due to the constant presence of a caregiver (Serrano-Alarcón et al., 2022).

Columns (4) and (5) show the impact of delays on the number of days spent in hospital (for urgent visits) within the year after eligibility. The result in column (4) captures both the impact on the extensive margin, and the intensive margin. The sample in column (5) is conditional on having an urgent hospitalisation, and measures the impact only on the intensive margin: do people who were urgently hospitalised remain longer in hospital if they delayed their nursing home admission? The results indicate that, conditional on being urgently hospitalised, delaying a nursing home admission by one

[22] Contrast estimates in columns (1) and (2) of Table 6.3 with our main estimate of 0.00047 in Table 6.2
[23] We use information on both primary and secondary diagnoses to identify falls.

additional month increases the length of stay for an urgent hospitalisation by 0.9 days ($= 0.02896 \times 30.5$). This could potentially be suggestive evidence of delays resulting in bed-blocking: the situation in which individuals remain longer in a hospital because they cannot be discharged to a nursing home. This would be consistent with the empirical literature (Gaughan et al., 2015; Moura, 2022). Nonetheless, this finding could also reflect a composition effect because people who delay might be hospitalised for different reasons, and hence require longer stays than people who do not delay.

Table 6.3: 2SLS results - Other outcomes

	Hospitalisation-related outcomes:				
	All hospitalisations	Non-urgent hospitalisations	Hospitalisation due to fall	# days in hospital (urgent)	# days in hospital (urgent) if urgent hospitalisation
	(1)	(2)	(3)	(4)	(5)
$\widehat{Delay}$ (in days)	0.00049**	0.00010	0.00017*	0.00819***	0.02896***
	(0.00019)	(0.00012)	(0.00009)	(0.00245)	(0.01109)
Observations	72,762	72,762	72,762	72,762	11,553
Mean dept. var	0.2030	0.0583	0.0433	1.3300	8.3768

2SLS results for other outcomes of interest. Outcomes are measured in the 365 days following eligibility; All models include all covariates and year and region fixed effects in both the first and second stage.
Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

6.5.5 Sensitivity checks

We test the robustness of our main results to three types of changes. First, we examine how sensitive our results are to the exclusion of individuals who were not living in a nursing home after one year following eligibility, either because they moved out or died within one year after eligibility. These individuals are excluded in the main analysis because we focus on permanent nursing home moves only and delays are directly affected by deaths. However, excluding individuals who died may also introduce a bias through a selective sample as survivors are likely in better health and therefore more likely to delay and less likely to be hospitalised. Nonetheless, we find that excluding this groups does not affect the main results as we find similar results when we include these individuals (see Column (1) of Appendix Table E8).

Second, to further analyse whether our results are driven by sudden shocks that affect both nursing home congestion and hospitalisations (violating the exclusion restriction), we include month-by-year fixed effects in our two-stage least squares regression. Our results are robust to this inclusion (Column (2) of Appendix Table E8).

Finally, we run the analyses using four different specifications of the instrumental variable, which in our main specification is the average delay of other individuals with the same care profile, living in the same care office region, receiving the eligibility status in the 46 days before and after the individual's own eligibility. We test the following adaptations: i) use smaller regions (i.e. municipalities); ii) use a narrower time window of 30 days instead of 46 days before and after the individual's own eligibility; iii) we shift the time window to 92 days *before* the individual's own eligibility; and iv) calculate a weighted average of delays using distances from the individual's place of residence to the other individuals' residences for who we calculate the average. Though the main result deviates slightly in terms of the magnitude, overall the result seems robust to all of these changes to our preferred specification (Column (3) - (6) of Appendix Table E8). The same holds for the analyses by care profile (results are available upon request).

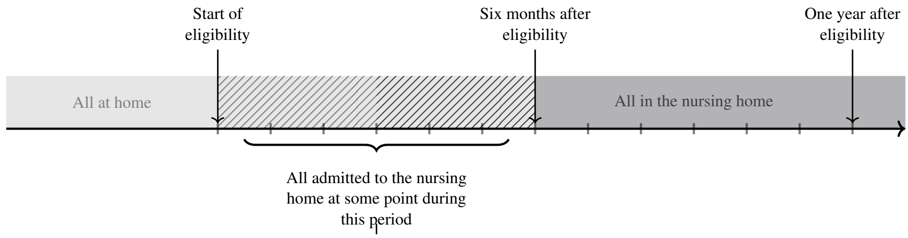
6.5.6 Hospitalisations at home vs in the nursing home

The impact of delaying a nursing home admission on hospital care utilisation may run via two channels. First, delaying a nursing home admission results in residing at home for a longer period, which increases the risk of an urgent hospitalisation if nursing homes act as a protective environment. This will occur when the risk of certain events or conditions that require medical services are lower in a nursing home, for example a fracture following a fall (Ouslander et al., 2010), or complications from an urinary tract infection (Grabowski et al., 2007). Second, delayed admissions may also increase the hospitalisation risk after the nursing home admission if an individual's health deteriorates faster in the community than it would otherwise do in a nursing home. For example, medication may be better monitored in a nursing home compared to at home. Conditional on similar underlying health at eligibility, individuals who delay their nursing home admission may be in poorer health once they are eventually admitted due to having experienced longer exposure to a potential (faster) deterioration in health and wellbeing.

Our main analysis measures a composite of the above two effects. The section below explores the extent to which the impact of delays leading the hospitalisations is driven by hospitalisations occurring at home or in the nursing home. We construct a new sample in which all individuals are admitted to a nursing home within six months following eligibility and everyone survives up to at least one year after eligibility. We then run the same analysis as previously for two different outcomes: i) urgent hospitalisations occurring within zero to six months following eligibility; and ii) for urgent hospitalisations occurring within six to twelve months following eligibility (see also Figure 6.4). While the first outcome measures hospitalisations occurring in ones own home and in the nursing home, the second outcome only measures those occurring in a nursing home as everyone is resident in a nursing home six months following eligibility by construction.

The results of Table 6.4 demonstrate that the impact of postponing a nursing home admission by one additional month (i.e. 30.5 days) increases the probability of an urgent hospitalisation within six months following eligibility by 2.5 percentage points

Figure 6.4: Timeline - differentiating between protection and deterioration effect



Notes: In this sub sample, everyone is admitted to a nursing home between the start of eligibility and 6 months after. Hospitalizations occurring within 0-6 months after **eligibility** capture both the protection and the deterioration effect, while those occurring within 6-12 months only capture the deterioration effect, assuming that protection does not accumulate over time.

(Column 1).[24] In the period following the initial six months, when everyone has been admitted to a nursing home, this impact decreases to 1 percentage point and is not statistically different from zero at the 10 percent level (Column 2). This implies that once admitted to a nursing home, delays have less impact on hospitalisations. This is despite individuals being slightly older, and hence more frail.[25] This suggests that nursing homes provide a more protective environment which is sufficient to counteract any deterioration in health caused through a delayed admission (together with an ageing effect). Hence, delayed admission to a nursing home largely impacts hospitalisations from their own home, and less so once an individual has been admitted into nursing facility.

[24]This result also shows that the direction of our estimate from the main specification in Table 6.2 is robust to choosing a different time frame.

[25]Another potential explanation is that individuals are in better health during the second period (6-12 months), as some will have recently been discharged from a hospital, potentially in better health than they were prior to the hospitalisation. However, when we run the same analysis, but excluding those with an urgent hospitalisation in the first period (1-6 months), the impact of a delay remains small (coefficient = 0.00026) and non-significant (p-value = 0.409).

Table 6.4: 2SLS results - Hospitalisations at home vs in a nursing home

	Outcome:	
	Urgent hospitalisation within 0-6 months (both at home and in a nursing home)	Urgent hospitalisation within 6-12 months (in a nursing home only)
	(1)	(2)
Second stage (outcome = urgent hospitalisation):		
$\widehat{Delay}$ (in days)	0.00081**	0.00032
	(0.00038)	(0.00032)
Covariates	Yes	Yes
Year fixed effects	Yes	Yes
Region fixed effects	Yes	Yes
Observations	63,330	63,330
Mean dept var	0.0990	0.0641

The coefficient of the instrument in the first stage equals 0.273 with a standard error of 0.015. The F-statistic of the excluded instrument equals 308.
All models include all covariates and year, region and care profile fixed effects.
Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

6.6 Discussion

Excess demand for long-term care can create spillovers to the hospital sector, e.g. if limited access to long-term care causes health problems which require medical care. Such spillovers are inefficient and impose a cost to society, for example if the additional expenditures of hospital treatments exceed those of an immediate nursing home stay. In this paper, we quantify these spillovers by examining the effect of delayed nursing home admissions, driven by excess demand, on hospital use.

We find that delays in nursing home admissions increase hospital care use: a one-month delay increases the probability to be hospitalised at least once within the year after eligibility by 1.4 percentage point (11%). This effect is notably large, especially considering that a substantial proportion of individuals experience delays exceeding three months. The effect is largest among individuals with dementia care needs, to a large extent (30%) driven by hospitalisations that occur after a fall, and is predominantly explained by hospitalisations that occur while waiting at home.

Our results reveal three signs of inefficiencies of delayed admissions to nursing homes. First, the falls suggest that they create additional health problems which, besides a burden on the individual, require costly medical care. Second, nursing home care may be imperfectly substituted by hospital care as, conditional on being hospitalised, people who delay their nursing home admission longer spend more days in the hospital. Third, while waiting lists in times of excess demand should ration out people with lower care needs for efficient allocation of care, we find evidence for the opposite. Our results suggest that people with high care needs are more likely to delay their admission to a nursing home longer in a period of congestion. The latter could be explained by people with high care needs being more likely to underestimate the benefits of an immediate admission (Finkelstein and Notowidigdo, 2019), or by the provider side being likely to be more selective in admitting healthier clients when they are close to full capacity (Gandhi, 2023; He and Konetzka, 2015).

These findings have several implications for policy. First, the evidence of spillovers suggests that improving timely access to nursing home care, for instance by expanding capacity, generates gains in the healthcare sector. These gains should be acknowledged in decisions about the allocation of scarce resources, such as budgets and labor, across sectors. Policy makers and care providers should aim to minimize the spillovers, for instance by improving the alternatives for nursing home care (like other types of assisted living facilities), improving care that is provided at home or by prioritizing vulnerable groups that may benefit most from nursing home care. The latter is important, at least in the Dutch context, as the current form of rationing through waiting lists, where waiting times fluctuate over time and care providers are in charge of the order in which people on the waiting list are admitted, seems ineffective in allocating care to those with high care needs in times of congestion.

Chapter 7

Discussion

Ageing populations raise policy concerns about the distribution of mortality gains and the provision of care to a population that is increasingly growing older. This thesis addresses these concerns by i) analysing income-related disparities in mortality; and ii) assessing the efficiency of nursing home care provision and allocation for the older population.

Main findings

Part I: Income-related inequality in mortality at older ages

Reducing inequalities in mortality has been a long-standing worldwide goal (Commission on Social Determinants of Health, 2018), but defining appropriate policies aimed at reducing these disparities requires insights into the drivers of these inequalities. Because mortality improvements have been shifting from younger to older ages (OECD, 2023b) and because the sheer number of people surviving up to ages 65 and over (United Nations, 2022), it is particularly important to obtain a better understanding of how and why disparities in the mortality of older age groups have changed over the last decades. The COVID-19 pandemic highlighted the relevance of older age groups when examining inequalities further as older and lower-income groups were most affected by the pandemic. Part I of this thesis analyses mortality disparities at older ages and contributes to a better understanding of its drivers, aiding in directing policies and new research areas.

Chapter 2 of this thesis shows that disparities in mortality at older ages by income have been increasing over the last two decades. While mortality has been falling in all income groups between 1996 and 2016 for ages 65 and over, the reductions were typically larger among the rich compared to the poor, particularly among 80+ women. Among this group the drop in mortality for the richest decile was almost 1.5 times as large as for the poorest decile. This implies that the recent improvements in mortality have increased disparities by income at older ages, which stands in contrast to the decreasing disparities at younger ages. In addition, the findings show that mortality (and its disparities) at older ages can to some extent be attributable to avoidable causes, meaning there is still room for improvement.

The COVID-19 pandemic exposed and increased existing health disparities at older ages. Chapter 3 shows that cause-attributed COVID-19 mortality was more concentrated among poorer groups, contributing to increasing inequalities in mortality at older ages by income in 2020. This contribution of unequal COVID-19 deaths was mitigated because COVID-19 deaths displaced deaths from other causes that are more prevalent among poorer groups. This suggests that the pandemic's impact on disparities at older ages is largely explained by underlying health differences across income groups. This underscores the fact that the already worse off were disproportionately affected negatively by the COVID pandemic, which is to be borne in mind when preparing for a next pandemic.

Part II: Efficiency in nursing home care

There are substantial efficiency differences in care provision across nursing home providers, as outlined in Chapters 4 and 5. These chapters document wide variation in health outcomes across nursing homes: even after adjusting for resident case-mix differences, the 5 percent best-performing nursing homes had a 7 percentage points lower 180-day mortality rate compared to the worst-performing ones in 2015-2019. This difference was exacerbated in 2020-2021 during the COVID-19 pandemic, in which the 20 percent nursing homes with lowest excess mortality had close to zero excess deaths, and the 20 percent with highest excess mortality had 6 per 100 more deaths than expected. The considerable variation in (excess) mortality and avoidable hospitalisations reflect large differences in the performance of nursing homes in preventing these adverse outcomes, indicating there is a scope for improving outcomes for the worst performing providers.

While adopting novel methods from the value-added literature (Abaluck et al., 2021; Angrist et al., 2017; Chetty et al., 2014; Deming, 2014; Kane and Staiger, 2008), Chapter 4 shows that mortality rates and avoidable hospitalisations at the nursing home level causally predict an individual's own outcomes. This implies that adjusting for observed resident characteristics is sufficient for correcting biases when measuring nursing home outcomes, at least at the average level. As value-added models correct for such individual-level characteristics, they are a useful tool for generating unbiased indicators based on health outcomes and could be used for quality reporting in the nursing home sector.

Nursing home performance as measured by resident outcomes is not fully captured by indicators related to the organisational structure and care processes in nursing homes. Chapters 4 and 5 reveal weak associations between publicly reported provider characteristics in the Netherlands and nursing home outcomes. While some staffing-related characteristics, such as the share of specialists (Chapter 4), spending on external staff and absenteeism (Chapter 5), appear to be statistically significantly related to outcomes, they lack economic relevance because they are very small. The weak associations suggest that the publicly reported information on structure and processes is insufficient for prospective residents wishing to select the nursing home with the best outcomes, and for regional purchasing offices whose responsibility it is to purchase high-quality care (Bakx et al., 2021).

Chapter 6 explores the consequences of delayed admissions to nursing homes as a consequence of prolonged waiting times. The findings show that individuals who enter a wait list during busy periods, have to wait longer to enter a nursing home, and as a result use more hospital care, predominantly due to falls and likely occurring at home. Individuals with dementia care needs are particularly vulnerable to the adverse consequences of prolonged delays. These findings suggest that timely access to a protective environment, such as a nursing home facility, induces positive outcomes which spill over to the healthcare sector.

Policy implications

Tackling income-related inequalities in mortality

Policymakers should actively invest in reducing mortality inequalities at older ages. Part I of this thesis highlights that: i) the largest inequalities (in absolute terms) are among 65+ year-olds; ii) the recent changes in mortality at older ages favoured the richer groups; and iii) these unequal changes can at least to some extent be attributable to avoidable causes. Nonetheless, there is little attention for reducing mortality inequalities at this particular age-group in public policy.

Policies targeted at improving health behaviour of poorer individuals may be effective in reducing mortality inequalities, also at older ages. Current policies related to health behaviours in the Netherlands, for example those outlined in the Preventieakkoord (Ministerie van Volksgezondheid, Welzijn en Sport, 2018), often concentrate on preventing unhealthy behaviours, such as increasing the starting age of smoking and drinking, primarily focusing on younger age groups. Although policies targeting early-life behaviour likely yield substantial long-term benefits, interventions aimed at improving the health behaviour of poorer older individuals can still play an important role in reducing inequalities. The findings in Chapter 2 underscore the relevance of health behaviours at older ages, especially among older women for whom mortality due to preventable causes has been increasing in the last decades. While these trends are possibly driven by increasing smoking rates among women in the past (Long et al., 2021), focusing smoking cessation interventions on poorer older women remains important since smoking cessation at older ages may still have beneficial survival effects (Gellert et al., 2012).

Addressing health-related inequalities among older ages requires a long-term proactive attitude from the government, as health disparities are persistent. Major events affecting health of older individuals, such as a pandemic – but potentially also increasing public expenditures aimed at reducing waiting times for various types of care (Mackenbach et al., 2011) –, have the potential to disproportionately harm poorer groups or benefit wealthier ones due to inherent health differences. It is therefore important to actively tailor policies towards poorer groups if the goal is to mitigate the consequences of major health shocks on inequalities. Examples of such include greater investments in the pro-poor targeting of testing and vaccinations during the COVID-19 pandemic, ensuring access to (health) care for the poor through the public system in times of shortages, or through paying more attention to the nursing home sector during health emergencies, like the COVID-19 pandemic. The latter is particularly important because nursing homes generally serve the poorer and most vulnerable individuals (Bom et al., 2020). Nonetheless, De Onderzoeksraad voor Veiligheid (2022) argued that the nursing home sector received low priority in the advise and decision making process during the outbreak of the COVID-19 pandemic in the Netherlands; a phenomenon that, according to (Werner et al., 2020), also manifested in the United States. Specifically targeting these most vulnerable groups in decision making could serve as a way towards less socioeconomic inequalities in longevity.

Insights for policy in nursing home care

Rapidly ageing populations raise concerns about the future organisation and financing of nursing home care (Barber et al., 2021; Gruber et al., 2023). Important questions that should be discussed in the policy debates in all countries, including the Netherlands, involve: should the increasing care needs of the ageing population be met at home or in a nursing home? How should one deal with scarcity of nursing home beds? How can nursing home providers be incentivized to produce improve outcomes? The findings of this thesis provide valuable insights into these questions.

Policies aimed at reducing use of nursing home care may have broader welfare implications

To mitigate the impact of rising care needs on the public budget and care workforce, recent policy changes and future ambitions in the Netherlands focus on reducing and discouraging the use of nursing home care. These changes include stricter eligibility requirements for institutional care, higher co-payments for nursing home care, a large reform of the long-term care system aimed at encouraging individuals to receive care at home, and potentially restricting supply by limiting the number of nursing home beds (Alders and Schut, 2019; Helder, 2022). Along the same lines, recently defined programs, such as the WOZO (Ministerie van Volksgezondheid, Welzijn en Sport, 2022), aim to separate care provision from housing arrangements (in Dutch: Scheiden wonen en zorg), for example through reforming the financing of institutional care (NZa, 2023).

Focusing on limiting nursing home care usage is a political decision, but policymakers should consider that such a choice can transfer the burden of increasing care needs to other sector(s). This thesis illustrates that delaying use of nursing home care can create spillover effects, leading to additional demand for hospital care services. Similarly, less access to nursing home care is found to increase demand for other healthcare subsectors – home care (Bakx et al., 2020b) and care provided by general practitioners (Forder et al., 2019) – but possibly also to the labour market through informal care provision (Kim and Lim, 2015) and the housing market. Ignoring these broader costs and benefits could result in an underestimation of policies' impacts on budgets and societal welfare.

To mitigate the negative externalities of policies aimed at reducing use of nursing home care, such policies should go hand in hand with with enhancing the alternatives to nursing home care. Diepstraten et al. (2020) show that this could be achieved by improving home environments through adjustments which can enhance the feasibility of receiving care at home. The development of new forms of housing arrangements within neighbourhoods in which older people with similar care needs live together and nurses can efficiently deliver home care could potentially also offer a viable solution. These strategies may aid in accommodating the increasing care needs among the older population while preserving scarce nursing home beds for the most vulnerable cases.

An efficient allocation of nursing home care is important when there is limited capacity
To optimally allocate scarce resources, various public bodies, such as NZa (2023), argue that the government should provide more guidance about which individuals should receive care in a nursing home, and which should receive care at home. This thesis underlines the importance of more guidance in how nursing home beds can be allocated to the most severe cases because limited access to nursing home care can have detrimental effects for specific groups, for instance among individuals with high dementia care needs.

Despite the policy efforts aimed at discouraging use of nursing home care, the increasing care needs and therefore preferences for receiving nursing home care causes waiting lists to increase (TNO, 2019). Because waiting lists are managed by nursing homes, the providers are responsible for the allocation of beds among individuals on their wait list. This may lead to sub-optimal outcomes if providers sub-optimally prioritise the less severely ill, possibly because they lack crucial information about the care needs of individuals on the wait list. To optimise the allocation of nursing home beds, the Dutch government could either define narrower and more detailed care profiles to improve the information about wait-listed individuals or reduce the responsibility for providers by organizing the allocation of nursing home care at a central point within regions. Another option would be to reduce demand by eliminating access to nursing home care for the lowest care profile (in Dutch: Zorgzwaartepakket 4) and with this increase the minimum level of care needs that grant entry to a nursing home, which is still relatively low in comparison to other countries (Bakx et al., 2023).

Nursing home quality indicators should include performance on resident outcomes
Enhancing efficiency in the provision of nursing home care would be an effective policy direction as it improves overall outcomes, while keeping the impact on public and individual budgets fixed (Rijksoverheid, 2023). This thesis shows that, while providers generally receive more or less the same per diem price, some providers produce better health outcomes for similar residents than others, meaning there is indeed

room for improving efficiency for some nursing homes. However, the lack of publicly available information in resident outcomes creates minimal external incentives for nursing homes to improve the outcomes of their residents.

The available information on nursing home quality primarily focuses on inputs and processes of care, while the link between these indicators and patient outcomes is rather weak (Chapters 4 and 5, Bakx et al. 2020a). This means that prospective users and regional care purchase offices can only select nursing homes based on indicators that do not necessarily contribute to better outcomes. Systematically collecting and reporting information on relevant outcomes in an accessible way – see for an example the US website Nursing Home Compare – and including information on outcomes in quality frameworks (in Dutch: Kwaliteitskader) can be helpful for regional offices who are responsible for purchasing high quality care and may encourage individuals to select a nursing home with better outcomes (Cornell et al., 2019; Perraillon et al., 2019b). This potentially incentivizes providers to improve resident outcomes for competitive advantage – though adverse provider incentives to selectively admit healthier residents should be considered (Perraillon et al., 2019a) through case-mix corrections, potentially by adopting value-added models as demonstrated in Chapter 4 of this thesis. Additionally, focusing on resident outcomes may mean that some of the process indicators no longer need to be collected. Registering such outcomes may be perceived as less of a burden for nursing staff, especially if they can be retrieved from existing administrative records.

Recommendations for future research

More research on the drivers of mortality disparities at older ages
To define effective policy strategies aimed at reducing disparities in mortality at older ages, it is important to understand the underlying factors driving these disparities. This thesis highlights that the increasing old-age mortality gap is to some extent attributable to avoidable causes, suggesting a potential for improvement. However, the actual drivers remain unclear. Insights could be further improved by examining whether specific behaviours, cohorts, medical advancements or policy developments contributed to the decreasing mortality rates of the older population, and whether these contributions varied by income.

This thesis highlights at least two potential avenues for further exploration. First, the findings in Chapter 2 indicate a potentially important role for preventable causes of deaths in explaining mortality disparities by income. Analysing the role of prevention in a causal way is complicated because of self-reporting biases and because the benefits of preventive behaviour (such as the mortality effects of smoking, exercising) may only become apparent much later in life. Nonetheless, the availability of supermarket scanner data on food purchases linked to regional income (Allcott et al., 2019; Oster, 2020), tracking data from wearable technologies (Handel and Kolstad, 2017) or longitudinal survey data on health behaviours linked to to registry data on deaths and healthcare use enables to study such topics over longer periods. Second, future research could focus on analysing cohorts rather than time trends alone (see for example Jokela 2014). This approach could offer a better understanding of whether the increasing disparities are driven by developments in medical care or social policies of the last two decades, or whether they reflect an ageing cohort with particularly unequal characteristics.

New data (linkages) required to study the full nursing home market
Access to linkable data on all aspects of the nursing home market is essential for answering a broader range of policy questions that researchers have not been able to investigate yet. Two notable examples are: i) should governments embrace, combat and/or intervene in the rise of private equity owned nursing homes? and ii) how should nursing homes and governments deal with labour market shortages, for instance through applying technological advancements? These questions require sufficient data on for-profit providers and the nursing home workforce, both of which are challenging to access and often lack linkages to other data sources in Netherlands and most other European countries.

The rise of private equity investments in nursing home care is sometimes seen as a partial solution to capacity problems. However, evidence from the United States shows that private equity ownership of nursing homes may have detrimental effects on the quality of care and outcomes of residents (Gupta et al., 2021; Grabowski et al., 2013). Nonetheless, the evidence on settings outside of the United States is limited due to a lack of available information on the for-profit nursing home market.

Like in many other public sectors, labour market shortages threaten the sustainability of care provision to the ageing population. These shortages may have drastic implications, especially in the nursing home sector: according to Friedrich and Hackmann (2021) and Stevens et al. (2015), lower nurse employment substantially increase mortality among nursing home residents. More insights into drivers and consequences of nurse shortages, including the mechanisms, may aid in shaping policies aimed at mitigating the negative effects on resident outcomes. Unfortunately, while detailed data at the nursing home staff member-level is in some cases available, for example through payroll information (Gandhi et al., 2021), it cannot always be linked to resident-level outcomes. Facilitating the link between comprehensive individual-level staff records, resident-level outcomes and characteristics, and provider-level information would provide researchers with the opportunity to contribute to the limited research on care workforces.

Bibliography

Abaluck, J., Bravo, M. C., Hull, P., and Starc, A. (2021). Mortality Effects and Choice Across Private Health Insurance Plans. *The Quarterly Journal of Economics*, 136(3):1557–1610.

ACM (2021). Onderzoek verpleeghuiszorg: De relatie tussen concentratiemaatstaven en de marktuitkomsten kwaliteit, toegankelijkheid en betaalbaarheid. Zaaknr. ACM/20/038360, Autoriteit Consument & Markt. Retrieved from https://www.acm.nl/sites/default/files/documents/bijlage-acm-rapport-onderzoek-verpleeghuiszorg.pdf.

Actiz (n.d.). Hoe staat het met: de wachtlijsten voor verpleeghuizen? Retrieved at 7 February, 2023, from https://www.actiz.nl/hoe-staat-het-met-de-wachtlijsten-voor-verpleeghuizen.

Alders, P. and Schut, F. T. (2019). Trends in ageing and ageing-in-place and the future market for institutional care: scenarios and policy implications. *Health Economics, Policy and Law*, 14(1):82–100.

Allcott, H., Diamond, R., Dubé, J.-P., Handbury, J., Rahkovsky, I., and Schnell, M. (2019). Food Deserts and the Causes of Nutritional Inequality*. *The Quarterly Journal of Economics*, 134(4):1793–1844.

Alsan, M., Chandra, A., and Simon, K. (2021). The great unequalizer: Initial health effects of covid-19 in the united states. *Journal of Economic Perspectives*, 35(3):25–46.

Angrist, J., Hull, P., Pathak, P., and Walters, C. (2016). Interpreting tests of school vam validity. *American Economic Review*, 106(5):388–92.

Angrist, J. D., Hull, P. D., Pathak, P. A., and Walters, C. R. (2017). Leveraging lotteries for school value-added: Testing and estimation. *The Quarterly Journal of Economics*, 132(2):871–919.

Antwi, Y. A. and Bowblis, J. R. (2018). The impact of nurse turnover on quality of care and mortality in nursing homes: Evidence from the great recession. *American Journal of Health Economics*, 4(2):131–163.

Arceo-Gomez, E. O., Campos-Vazquez, R. M., Esquivel, G., Alcaraz, E., Martinez, L. A., and Lopez, N. G. (2022). The income gradient in covid-19 mortality and hospitalisation: An observational study with social security administrative records in mexico. *The Lancet Regional Health - Americas*, 6:100115.

Arling, G. and Daneman, B. (2002). Nursing home case-mix reimbursement in mississippi and south dakota. *Health Services Research*, 37(2):377–395.

Arling, G., Lewis, T., Kane, R. L., Mueller, C., and Flood, S. (2007). Improving quality assessment through multilevel modeling: The case of nursing home compare. *Health Services Research*, 42(3):1177–1199.

Arntzen, R. J., Bekker, R., Smeekes, O. S., Buurman, B. M., Willems, H. C., Bhulai, S., and van der Mei, R. D. (2022). Reduced waiting times by preference-based allocation of patients to nursing homes. *Journal of the American Medical Directors Association*, 23(12):2010–2014.e1.

Atkins, J. L., Masoli, J. A. H., Delgado, J., Pilling, L. C., Kuo, C.-L., Kuchel, G. A., and Melzer, D. (2020). Preexisting Comorbidities Predicting COVID-19 and Mortality in the UK Biobank Community Cohort. *The Journals of Gerontology: Series A*, 75(11):2224–2230.

Bakx, P., Bär, M., Rellstab, S., and Wouterse, B. (2020a). Zicht op kwaliteit van verpleeghuiszorg? Eshpm onderzoeksrapport 2020.03, Erasmus School of Health Policy & Management (ESHPM), the Netherlands.

Bakx, P., Schut, E., and Wouterse, B. (2021). Pricing long-term care for older persons: Netherlands. Technical report, WHO, Geneva/OECD Publishing, Paris.

Bakx, P., van Doorslaer, E., and Wouterse, B. (2023). *Long-Term Care in the Netherlands*, book chapter 7. In Gruber and McGarry (2023).

Bakx, P., Wouterse, B., van Doorslaer, E., and Wong, A. (2020b). Better off at home? effects of nursing home eligibility on costs, hospitalizations and survival. *Journal of Health Economics*, 73:102354.

Bambra, C., Riordan, R., Ford, J., and Matthews, F. (2020). The covid-19 pandemic and health inequalities. *Journal of Epidemiology & Community Health*, 74(11):964–968.

Banks, J., Currie, J., Krutikova, S., Salvanes, K. G., and Schwandt, H. (2021). The evolution of mortality inequality in 11 oecd countries: Introduction*. *Fiscal Studies*, 42(1):9–23.

Baqui, P., Bica, I., Marra, V., Ercole, A., and van der Schaar, M. (2020). Ethnic and regional variations in hospital mortality from covid-19 in brazil: a cross-sectional observational study. *The Lancet Global Health*, 8(8):e1018–e1026.

Bär, M., Bakx, P., Wouterse, B., and van Doorslaer, E. (2022). Estimating the health value added by nursing homes. *Journal of Economic Behavior and Organization*, 203:1–23.

Barber, S. L., van Gool, K., Wise, S., Wood, M., Or, Z., Penneau, A., Milstein, R., Ikegami, N., Kwon, S., Bakx, P., et al. (2021). Pricing long-term care for older persons. World Health Organization.

Barnard, S., Fryers, P., Fitzpatrick, J., Fox, S., Waller, Z., Baker, A., Burton, P., Newton, J., Doyle, Y., and Goldblatt, P. (2021). Inequalities in excess premature mortality in england during the covid-19 pandemic: a cross-sectional analysis of cumulative excess mortality by area deprivation and ethnicity. *BMJ Open*, 11(12).

Barry, T., Brannon, D., and Mor, V. (2005). Nurse Aide Empowerment Strategies and Staff Stability: Effects on Nursing Home Resident Outcomes. *The Gerontologist*, 45(3):309–317.

Basis Wettenbestand (2018). Beleidsregels indicatiestelling Wlz 2018, Artikel 2.6.4. Ingangsdatum en geldigheidsduur. Retrieved at October 7, 2022, from http://wetten.overheid.nl/jci1.3:c:BWBR0040327&artikel=2.

Becker, C. and Rapp, K. (2010). Fall prevention in nursing homes. *Clinics in Geriatric Medicine*, 26(4):693–704. Falls and Their Prevention.

Bensnes, S. and Huitfeldt, I. (2021). Rumor has it: How do patients respond to patient-generated physician ratings? *Journal of Health Economics*, 76:102415.

Bielza, R., Sanz, J., Zambrana, F., Arias, E., Malmierca, E., Portillo, L., Thuissard, I. J., Lung, A., Neira, M., Moral, M., et al. (2021). Clinical characteristics, frailty, and mortality of residents with covid-19 in nursing homes of a region of madrid. *Journal of the American Medical Directors Association*, 22(2):245–252.e2.

Birkmeyer, J. D., Barnato, A., Birkmeyer, N., Bessler, R., and Skinner, J. (2020). The impact of the covid-19 pandemic on hospital admissions in the united states. *Health Affairs*, 39(11):2010–2017. PMID: 32970495.

Blackman, C., Farber, S., Feifer, R. A., Mor, V., and White, E. M. (2020). An illustration of sars-cov-2 dissemination within a skilled nursing facility using heat maps. *Journal of the American Geriatrics Society*, 68(10):2174–2178.

Bloom, D. and Luca, D. (2016). Chapter 1 - the global demography of aging: Facts, explanations, future. In Piggott, J. and Woodland, A., editors, *Handbook of the Economics of Population Aging*, volume 1, pages 3–56. North-Holland.

Bolhaar, J., Ketel, N., and van der Klaauw, B. (2019). Job search periods for welfare applicants: Evidence from a randomized experiment. *American Economic Journal: Applied Economics*, 11(1):92–125.

Bom, J. (2021). Formal and informal long-term care in an ageing society. Doctoral thesis, Erasmus University Rotterdam, the Netherlands.

Bom, J., Wouterse, B., Bakx, P., and Tenand, M. (2020). Grote verschillen in eigen bijdrage aan ouderenzorg. *Geron*, 22.

Bos, A., Kruse, F. M., and Jeurissen, P. P. T. (2020). For-profit nursing homes in the netherlands: What factors explain their rise? *International Journal of Health Services*, 50(4):431–443.

Brant, L. C. C., Nascimento, B. R., Teixeira, R. A., Lopes, M. A. C. Q., Malta, D. C., Oliveira, G. M. M., and Ribeiro, A. L. P. (2020). Excess of cardiovascular deaths during the covid-19 pandemic in brazilian capital cities. *Heart*, 106(24):1898–1905.

Bronskill, S. E., Rochon, P. A., Gill, S. S., Herrmann, N., Hillmer, M. P., Bell, C. M., Anderson, G. M., and Stukel, T. A. (2009). The relationship between variations in antipsychotic prescribing across nursing homes and short-term mortality: quality of care implications. *Medical Care*, 47(2):1000–1008.

Carter, M. W. (2003). Factors associated with ambulatory care - sensitive hospitalizations among nursing home residents. *Journal of Aging and Health*, 15(2):295–331.

Castle, N. G. (2003). Searching for and Selecting a Nursing Facility. *Medical Care Research and Review*, 60(2):223–247.

Castle, N. G. and Ferguson, J. C. (2010). What is nursing home quality and how is it measured? *Gerontologist*, 50(4):426–442.

Chandra, A., Dalton, M., and Staiger, D. O. (2023). Are hospital quality indicators causal? Working Paper 31789, National Bureau of Economic Research.

Charles, K. K. and Sevak, P. (2005). Can family caregiving substitute for nursing home care? *Journal of Health Economics*, 24(6):1174–1190.

Chetty, R., Friedman, J. N., and Rockoff, J. E. (2014). Measuring the impacts of teachers i: Evaluating bias in teacher value-added estimates. *American Economic Review*, 104(9):2593–2632.

Chetty, R. and Hendren, N. (2018). The impacts of neighborhoods on intergenerational mobility ii: County-level estimates. *The Quarterly Journal of Economics*, 133(3):1163–1228.

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., and Cutler, D. (2016). The association between income and life expectancy in the united states, 2001-2014. *Journal of the American Medical Association*, 315:1750–1766.

Chida, Y. and Steptoe, A. (2008). Positive Psychological Well-Being and Mortality: A Quantitative Review of Prospective Observational Studies:. *Psychosomatic Medicine*, 70(7):741–756.

Ching, A. T., Hayashi, F., and Wang, H. (2015). Quantifying the impacts of limited supply: the case of nursing homes. *International Economic Review*, 56(4):1291–1322.

CIHI/Statistics Canada (2018). *Indicator library: List of conditions for potentially avoidable mortality and mortality from preventable and treatable causes indicators*. Ottawa, ON: CIHI.

CIZ (n.d.). Zorgprofielen. Retrieved at April 29, 2020, from https://www.ciz.nl/zorgprofessional/over-de-wlz/zorgprofielen.

Cohen, A. B., Knobf, M. T., and Fried, T. R. (2017). Avoiding Hospitalizations From Nursing Homes for Potentially Burdensome Care: Results of a Qualitative Study. *JAMA Internal Medicine*, 177(1):137–139.

Commission on Social Determinants of Health (2018). *Closing the Gap in a Generation. Health Equity through the Social Determinants of Health*. Geneva: World Health Organization.

Commission on Social Determinants of Health and World Health Organization (2008). Closing the gap in a generation: health equity through action on the social determinants of health. Who/ier/csdh/08.1, World Health Organization.

Cornell, P. Y., Grabowski, D. C., Norton, E., and Rahman, M. (2019). Do report cards predict future quality? the case of skilled nursing facilities. *Journal of Health Economics*, 66:208–221.

Costa-Font, J., Jiménez Martin, S., and Viola, A. (2021). Fatal underfunding? explaining covid-19 mortality in spanish nursing homes. *Journal of Aging and Health*, 33(7-8):607–617.

Costa-i Font, J. and Vilaplana-Prieto, C. (2022). 'Investing' in care for old age? an examination of long-term care expenditure dynamics and its spillovers. Cesifo working paper no.9553, CESifo, Munich. Retrieved from https://www.cesifo.org/node/67860.

Crawford, R., Stoye, G., and Zaranko, B. (2021). Long-term care spending and hospital use among the older population in england. *Journal of Health Economics*, 78:102477.

Cronin, C. J. and Evans, W. N. (2022). Nursing home quality, covid-19 deaths, and excess mortality. *Journal of Health Economics*, 82:102592.

Currie, J. and Schwandt, H. (2016). Mortality inequality: The good news from a county-level approach. *Journal of Economic Perspectives*, 30(2):29–52.

Cutler, D., Deaton, A., and Lleras-Muney, A. (2006). The determinants of mortality. *Journal of Economic Perspectives*, 20(3):97–120.

Cutler, D. M., Lange, F., Meara, E., Richards-Shubik, S., and Ruhm, C. J. (2011). Rising educational gradients in mortality: The role of behavioral risk factors. *Journal of Health Economics*, 30(6):1174–1187.

Dahl, G. B., Kreiner, C. T., Nielsen, T. H., and Serena, B. L. (2023). Understanding the rise in life expectancy inequality. Review of Economics and Statistics, forthcoming.

Davies, B., Parkes, B. L., Bennett, J., Fecht, D., Blangiardo, M., Ezzati, M., and Elliott, P. (2021). Community factors and excess mortality in first wave of the covid-19 pandemic in england. *Nature communications*, 12(1):3755.

de Beer, J. and van der Gaag, N. (2018). Educational differences in longevity. *Demos: Bulletin on Population and Society*, 34(5):3–4.

de Bienassis, K., Llena-Nozal, A., and Klazinga, N. S. (2020). The economics of patient safety part iii: Long-term care. OECD Health Working Papers 121, OECD Publishing, Paris.

de Jong, G., Muilwijk-Vriend, S., and Ter Weel, B. (2020). In de stad ontgroeien lage inkomens hun inkomenspositie minder vaak. *Economisch Statistische Berichten*, 105(4790):460–463.

de Jonge, H. (2020a). Kamerstukken ii 2019/2020, 25295, 461. Technical report, Tweede Kamer der Staten-Generaal.

de Jonge, H. (2020b). Kamerstukken ii 2019/2020, 31765, 491. Technical report, Tweede Kamer der Staten-Generaal.

de Jonge, H. (2020c). Kamerstukken ii 2020/2021, 25295 nr. 769. Technical report, Tweede Kamer der Staten-Generaal.

De Onderzoeksraad voor Veiligheid (2022). *Aanpak coronacrisis: Deel 1: tot september 2020*. Den Haag: De Onderzoeksraad voor Veiligheid.

Decoster, A., Minten, T., and Spinnewijn, J. (2021). The income gradient in mortality during the covid-19 crisis: evidence from belgium. *The Journal of Economic Inequality*, 19(3):551–570.

Deming, D. J. (2014). Using school choice lotteries to test measures of school effectiveness. *American Economic Review*, 104(5):406–411.

Deshpande, M. and Li, Y. (2019). Who is screened out? application costs and the targeting of disability programs. *American Economic Journal: Economic Policy*, 11(4):213–48.

Diepstraten, M., Douven, R., and Wouterse, B. (2020). Can your house keep you out of a nursing home? *Health Economics*, 29(5):540–553.

Donabedian, A. (2003). *An Introduction to Quality Assurance in Health Care*. Oxford University Press.

Doorslaer, E. v. and Jones, A. M. (2004). Income-related inequality in health and health care in the european union. *Health Economics*, 13(7):605–608.

Doyle, J. J., Graves, J. A., and Gruber, J. (2019). Evaluating measures of hospital quality: Evidence from ambulance referral patterns. *The Review of Economics and Statistics*, 101(5):841–852.

Drefahl, S., Wallace, M., Mussino, E., Aradhya, S., Kolk, M., Brandén, M., Malmberg, B., and Andersson, G. (2020). A population-based cohort study of socio-demographic risk factors for covid-19 deaths in sweden. *Nature communications*, 11(1):5097.

Dyer, A. H., Fallon, A., Noonan, C., Dolphin, H., O'Farrelly, C., Bourke, N. M., O'Neill, D., and Kennelly, S. P. (2022). Managing the impact of covid-19 in nursing homes and long-term care facilities: An update. *Journal of the American Medical Directors Association*, 23(9):1590–1602.

Einav, L., Finkelstein, A., and Mahoney, N. (2021). Long-Term Care Hospitals: A Case Study in Waste. *The Review of Economics and Statistics*, pages 1–57.

Einav, L., Finkelstein, F., and Mahoney, N. (2022). Producing health: Measuring value added of nursing homes. Available at https://leinav.people.stanford.edu/research.

Elezkurtaj, S., Greuel, S., Ihlow, J., Michaelis, E. G., Bischoff, P., Kunze, C. A., Sinn, B. V., Gerhold, M., Hauptmann, K., Ingold-Heppner, B., et al. (2021). Causes of death and comorbidities in hospitalized patients with covid-19. *Scientific Reports*, 11(1):4263.

Eliason, P. J., Grieco, P. L. E., McDevitt, R. C., and Roberts, J. W. (2018). Strategic patient discharge: The case of long-term care hospitals. *American Economic Review*, 108(11):3232–65.

Eurostat (2014). Amenable and preventable deaths statistics. Retrieved from https://ec.europa.eu/eurostat/cache/metadata/Annexes/hlth_cdeath_esqrs_fi_an2.pdf.

Feng, J., Wang, Z., and Yu, Y. (2020). Does long-term care insurance reduce hospital utilization and medical expenditures? evidence from china. *Social Science & Medicine*, 258:113081.

Finkelstein, A. and Notowidigdo, M. J. (2019). Take-Up and Targeting: Experimental Evidence from SNAP*. *The Quarterly Journal of Economics*, 134(3):1505–1556.

Forder, J. (2009). Long-term care and hospital utilisation by older people: an analysis of substitution rates. *Health Economics*, 18(11):1322–1338.

Forder, J., Gousia, K., and Saloniki, E.-C. (2019). The impact of long-term care on primary care doctor consultations for people over 75 years. *The European Journal of Health Economics*, 20:375–387.

Frandsen, B., Lefgren, L., and Leslie, E. (2023). Judging judge fixed effects. *American Economic Review*, 113(1):253–77.

Frandsen, B. R., Lefgren, L. J., and Leslie, E. C. (2019). Judging judge fixed effects. NBER working paper 25528, National Bureau of Economic Research.

Friedrich, B. U. and Hackmann, M. B. (2021). The Returns to Nursing: Evidence from a Parental-Leave Program. *The Review of Economic Studies*, 88:2308—-2343.

Gadbois, E. A., Tyler, D. A., and Mor, V. (2017). Selecting a Skilled Nursing Facility for Postacute Care: Individual and Family Perspectives. *Journal of the American Geriatrics Society*, 65(11):2459–2465.

Gandhi, A. (2023). Picking your patients: Selective admissions in the nursing home industry. Available at SSRN: https://dx.doi.org/10.2139/ssrn.3613950.

Gandhi, A., Yu, H., and Grabowski, D. C. (2021). High nursing staff turnover in nursing homes offers important quality information. *Health Affairs*, 40(3):384–391.

Gaughan, J., Gravelle, H., and Siciliani, L. (2015). Testing the bed-blocking hypothesis: Does nursing and care home supply reduce delayed hospital discharges? *Health Economics*, 24(S1):32–44.

Gellert, C., Schöttker, B., and Brenner, H. (2012). Smoking and All-Cause Mortality in Older People: Systematic Review and Meta-analysis. *Archives of Internal Medicine*, 172(11):837–844.

Geweke, J., Gowrisankaran, G., and Town, R. J. (2003). Bayesian inference for hospital quality in a selection model. *Econometrica*, 71(4):1215–1238.

Godøy, A., Haaland, F., Huitfeldt, I., and Votruba, M. (2019). Impacts of hospital wait time on patient health and labor supply. Discussion papers no. 919, Statistics Norway. Available from https://www.ssb.no/en/forskning/discussion-papers.

Gorges, R. J. and Konetzka, R. T. (2020). Staffing levels and covid-19 cases and outbreaks in u.s. nursing homes. *Journal of the American Geriatrics Society*, 68(11):2462–2466.

Gowrisankaran, G. and Town, R. J. (1999). Estimating the quality of care in hospitals using instrumental variables. *Journal of Health Economics*, 18:747–767.

Grabowski, D. C., Feng, Z., Hirth, R., Rahman, M., and Mor, V. (2013). Effect of nursing home ownership on the quality of post-acute care: An instrumental variables approach. *Journal of Health Economics*, 32(1):12–21.

Grabowski, D. C., Hirth, R. A., Intrator, O., Li, Y., Richardson, J., Stevenson, D. G., Zheng, Q., and Banaszak-Holl, J. (2016). Low-quality nursing homes were more likely than other nursing homes to be bought or sold by chains in 1993–2010. *Health Affairs*, 35(5):907–914.

Grabowski, D. C., O'Malley, A. J., and Barhydt, N. R. (2007). The costs and potential savings associated with nursing home hospitalizations. *Health Affairs*, 26(6):1753–1761.

Gravelle, H. and Siciliani, L. (2008). Is waiting-time prioritisation welfare improving? *Health Economics*, 17(2):167–184.

Grosskopf, S. and Valdmanis, V. (1993). Evaluating hospital performance with case-mix-adjusted outputs. *Medical Care*, 31(6):525–532.

Gruber, J. and McGarry, K. (2023). *Long-Term Care around the World*. University of Chicago Press.

Gruber, J., McGarry, K., and Hanzel, C. (2023). *Introduction to "International Comparisons of Long-Term Care"*, book chapter 1. In Gruber and McGarry (2023).

Gupta, A., Howell, S. T., Yannelis, C., and Gupta, A. (2021). Does private equity investment in healthcare benefit patients? evidence from nursing homes. NBER working paper 28474, National Bureau of Economic Research.

Hackmann, M. B. (2019). Incentivizing better quality of care: The role of medicaid and competition in the nursing home industry. *Amercian Economic Review*, 109(5):1684–1716.

Handel, B. and Kolstad, J. (2017). Wearable technologies and health behaviors: New data and new methods to understand population health. *American Economic Review*, 107(5):481–85.

Hanning, M. and van Vliet, N. (2016). Wachtlijsten in de Wlz: Wensen en behoeften van wachtenden. Technical Report NV/16/1873/wllz, Bureau HHM, Enschede. Retrieved from https://www.vgn.nl/system/files/article/file/Eindrapport%2BHHM%2Bwachtlijsten%2Bin%2Bde%2BWlz.pdf.

He, D. and Konetzka, R. T. (2015). Public Reporting and Demand Rationing: Evidence from the Nursing Home Industry. *Health Economics*, 24(11):1437–1451.

He, M., Li, Y., and Fang, F. (2020). Is there a link between nursing home reported quality and covid-19 cases? evidence from california skilled nursing facilities. *Journal of the American Medical Directors Association*, 21(7):905–908.

Health Holland (2019). *Kennis- en Innovatieagenda 2020–2023: Gezondheid en Zorg*. Utrecht: Health Holland.

Heins, M., Donker, G., Versteeg, S., and Korevaar, J. (2019). Ontwikkelingen in het aantal euthanasiegevallen en achterliggende factoren. Nivel, Utrecht, the Netherlands.

Helder, C. (2022). Kamerstuk 29389, nr. 111 - programma wonen, ondersteuning en zorg voor ouderen (wozo). Technical report, Tweede Kamer der Staten-Generaal.

Helsø, A. L., Pierri, N., and Wang, A. (2019). The economic impact of healthcare quality. IMF Working Paper No. 19/173.

Hinkema, M., Kompatscher, K., van Wissekerke, N. E., van Heumen, S., Balkuv, F., and Opdrachtgever Ministerie van Volksgezondheid, Sport en Welzijn (2020). Prognose capaciteitsontwikkeling verpleeghuiszorg fase ii. No. tno 2020 r11184, TNO.

Hoe, T. P. (2022). Does hospital crowding matter? evidence from trauma and orthopedics in england. *American Economic Journal: Economic Policy*, 14(2):231–62.

Hoekman, L. M., Smits, M. M. V., and Koolman, X. (2020). The dutch covid-19 approach: Regional differences in a small country. *Health Policy and Technology*, 9(4):613–622.

Huang, S. S. and Bowblis, J. R. (2018). The principal–agent problem and owner-managers: An instrumental variables application to nursing home quality. *Health Economics*, 27(11):1653–1669.

Hull, P. (2020). Estimating quality with quasi-experimental data. Available at https://about.peterhull.net/wp.

Hussem, A., Bakx, P., and Tenand, M. (2020). Publieke middelen voor private ouderenzorg. Netspar Design paper 143, Network for Studies on Pensions, Aging and Retirement.

IGJ (2020). Rapportage openstellen verpleeghuizen voor bezoek. Technical report, Inspectie Gezondheidszorg en Jeugd. Retrieved from https://www.igj.nl/publicaties/publicaties/2020/10/22/rapportage-openstellen-verpleeghuizen-voor-bezoek.

Imbens, G. W. and Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47(1):5–86.

Intrator, O., Zinn, J., and Mor, V. (2004). Nursing home characteristics and potentially preventable hospitalizations of long-stay residents. *Journal of the American Geriatrics Society*, 52(10):1730–1736.

Iversen, T. and Siciliani, L. (2011). 649 Non-Price Rationing and Waiting Times. In *The Oxford Handbook of Health Economics*. Oxford University Press.

Iyanda, A. E. and Boakye, K. A. (2022). A 2-year pandemic period analysis of facility and county-level characteristics of nursing home coronavirus deaths in the united states, january 1, 2020–december 18, 2021. *Geriatric Nursing*, 44:237–244.

Jacobs, R., Smith, P. C., and Street, A. (2006). *Measuring Efficiency in Health Care: Analytic Techniques and Health Policy*. Cambridge: Cambridge University Press.

Jokela, M. (2014). Are neighborhood health associations causal? a 10-year prospective cohort study with repeated measurements. *American journal of epidemiology*, 180(8):776–784.

Jordan, R. E., Adab, P., and Cheng, K. K. (2020). Covid-19: risk factors for severe disease and death. *BMJ*, 368.

Joyce, N. R., McGuire, T. G., Bartels, S. J., Mitchell, S. J., and Grabowski, D. C. (2018). The impact of dementia special care units on quality of care: An instrumental variables analysis. *Health Services Research*, 53(5):3657–3679.

Kallestrup-Lamb, M., Kjærgaard, S., and Rosenskjold, C. (2020). Insight into stagnating adult life expectancy: Analyzing cause of death patterns across socioeconomic groups. *Health Economics*, 29(12):1728–1743.

Kaltenthaler, E., Withfield, M., Walters, S., Akehurst, R., and Paisley, S. (2001). Uk, usa and canada: how do their pressure ulcer prevalence and incidence data compare? *Journal of Wound Care*, 10(1):530–535.

Kalwij, A., Alessie, R., and Knoef, M. (2012). The Association Between Individual Income and Remaining Life Expectancy at the Age of 65 in the Netherlands. *Demography*, 50:181–206.

Kane, T. J. and Staiger, D. O. (2008). Estimating teacher impacts on student achievement: An experimental evaluation. NBER working paper 14607, National Bureau of Economic Research.

Keith Chen, M., Chevalier, J. A., and Long, E. F. (2021). Nursing home staff networks and covid-19. *Proceedings of the National Academy of Sciences*, 118(1):e2015455118.

Khairat, S., Zalla, L. C., Adler-Milstein, J., and Kistler, C. E. (2021). U.s. nursing home quality ratings associated with covid-19 cases and deaths. *Journal of the American Medical Directors Association*, 22(10):2021–2025.e1.

Kim, H. B. and Lim, W. (2015). Long-term care insurance, informal care, and medical expenditures. *Journal of Public Economics*, 125:128–142.

Kinge, J. M., Modalsli, J. H., Øverland, S., Gjessing, H. K., Tollånes, M. C., Knudsen, A. K., Skirbekk, V., Strand, B. H., Håberg, S. E., and Vollset, S. E. (2019). Association of Household Income With Life Expectancy and Cause-Specific Mortality in Norway, 2005-2015. *JAMA*, 321(19):1916–1925.

Konetzka, R. T., White, E. M., Pralea, A., Grabowski, D. C., and Mor, V. (2021). A systematic review of long-term care facility characteristics associated with covid-19 outcomes. *Journal of the American Geriatrics Society*, 69(10):2766–2777.

Konetzka, R. T., Yang, F., and Werner, R. M. (2019). Use of instrumental variables for endogenous treatment at the provider level. *Health Economics*, 28(5):710–716.

Konetzka, T. R., Grabowski, D. C., Perraillon, M. C., and Werner, R. M. (2015). Nursing home 5-star rating system exacerbates disparities in quality, by payer source. *Health Affairs*, 34(5):819–827.

Kontopantelis, E., Mamas, M. A., Webb, R. T., Castro, A., Rutter, M. K., Gale, C. P., Ashcroft, D. M., Pierce, M., Abel, K. M., Price, G., Faivre-Finn, C., Van Spall, H. G., Graham, M. M., Morciano, M., Martin, G. P., and Doran, T. (2021). Excess deaths from covid-19 and other causes by region, neighbourhood deprivation level and place of death during the first 30 weeks of the pandemic in england and wales: A retrospective registry study. *The Lancet Regional Health - Europe*, 7:100144.

Kontopantelis, E., Mamas, M. A., Webb, R. T., Castro, A., Rutter, M. K., Gale, C. P., Ashcroft, D. M., Pierce, M., Abel, K. M., Price, G., Faivre-Finn, C., Van Spall, H. G. C., Graham, M. M., Morciano, M., Martin, G. P., Sutton, M., and Doran, T. (2022). Excess years of life lost to covid-19 and other causes of death by sex, neighbourhood deprivation, and region in england and wales during 2020: A registry-based study. *PLOS Medicine*, 19(2):1–20.

Kooreman, P. (1994). Nursing home care in the netherlands: a nonparametric efficiency analysis. *Journal of Health Economics*, 13(3):301–316.

Krieger, N., Waterman, P. D., and Chen, J. T. (2020). Covid-19 and overall mortality inequities in the surge in death rates by zip code characteristics: Massachusetts, january 1 to may 19, 2020. *American Journal of Public Health*, 110(12):1850–1852. PMID: 33058698.

Kruse, F., van Tol, L., Vrinzen, C., van der Woerd, O., and Jeurissen, P. (2020). The impact of covid-19 on long-term care in the netherlands: the second wave. Technical report, LTCcovid, International Long Term Care Policy Network, CPEC-LSE.

Kruzich, J. M. (2005). Ownership, chain affiliation, and administrator decision-making autonomy in long-term care facilities. *Administration in Social Work*, 29(1):5–24.

Kulhánová, I., Hoffmann, R., Eikemo, T. A., Menvielle, G., and Mackenbach, J. P. (2014). Educational inequalities in mortality by cause of death: first national data for the netherlands. *International journal of public health*, 59:687–696.

Lal, A., Lockhart, M., Xu, Y., and Zu, Z. (2021). How much should we trust instrumental variable estimates in political science? Practical advice based on over 60 replicated studies. Technical report, SSRN. Retrived from https://ssrn.com/abstract=3905329.

Lee, D. S., McCrary, J., Moreira, M. J., and Porter, J. (2022). Valid t-ratio inference for iv. *American Economic Review*, 112(10):3260–90.

Leshno, J. D. (2022). Dynamic matching in overloaded waiting lists. *American Economic Review*, 112(12):3876–3910.

Letterie, H. P. (2023). Essays on the regulation of long-term care in the netherlands. Phd thesis, Vrije Universiteit Amsterdam.

Li, Y., Temkin-Greener, H., Shan, G., and Cai, X. (2020). Covid-19 infections and deaths among connecticut nursing home residents: Facility correlates. *Journal of the American Geriatrics Society*, 68(9):1899–1906.

Lin, H. (2014). Revisiting the relationship between nurse staffing and quality of care in nursing homes: An instrumental variables approach. *Journal of Health Economics*, 27:13–24.

Lindsey, P. L. (2009). Psychotropic medication use among older adults: what all nurses need to know. *Journal of gerontological nursing*, 35(9):28–38.

Lippi Bruni, M., Mammi, I., and Verzulli, R. (2019). Quality in nursing homes. Oxford University Press.

Long, D., Mackenbach, J., Martikainen, P., Lundberg, O., Brønnum-Hansen, H., Bopp, M., Costa, G., Kovács, K., Leinsalu, M., Rodríguez-Sanz, M., et al. (2021). Smoking and inequalities in mortality in 11 European countries: a birth cohort analysis. *Population Health Metrics*, 19:1–9.

Lucas, J. A., Levin, C. A., Lowe, T. J., , Robertson, B., Akincigil, A., Sambamoorthi, U., Bilder, S., Paek, E. K., and Crystal, S. (2007). The relationship between organizational factors and resident satisfaction with nursing home care and life. *Journal of Aging & Social Policy*, 19(2):125–151.

Mackenbach, J. P. (2020). Re-thinking health inequalities. *European Journal of Public Health*, 30(4):615–615.

Mackenbach, J. P. and Kunst, A. E. (1997). Measuring the magnitude of socio-economic inequalities in health: An overview of available measures illustrated with two examples from europe. *Social Science & Medicine*, 44(6):757–771. Health Inequalities in Modern Societies and Beyond.

Mackenbach, J. P., Martikainen, P., Menvielle, G., and de Gelder, R. (2016). The arithmetic of reducing relative and absolute inequalities in health: a theoretical analysis illustrated with european mortality data. *Journal of Epidemiology & Community Health*, 70(7):730–736.

Mackenbach, J. P., Slobbe, L., Looman, C. W. N., van der Heide, A., Polder, J., and Garssen, J. (2011). Sharp upturn of life expectancy in the netherlands: effect of more health care for the elderly? *European journal of epidemiology*, 26:903–914.

Mackenbach, J. P., R.-V. J. R. and Nusselder, W. (2019). Opleiding, levensverwachting en pensioenleeftijd. Netspar Design paper 119, Network for Studies on Pensions, Aging and Retirement.

Martín-María, N., Miret, M., Caballero, F. F., Rico-Uribe, L. A., Steptoe, A., Chatterji, S., and Ayuso-Mateos, J. L. (2017). The Impact of Subjective Well-being on Mortality: A Meta-Analysis of Longitudinal Studies in the General Population. *Psychosomatic Medicine*, 79(5):565–575.

McClellan, M. and Staiger, D. (1999). The quality of health care providers. NBER working paper 7327, National Bureau of Economic Research.

McDonald, S. A., Lagerweij, G. R., de Boer, P., de Melker, H. E., Pijnacker, R., Mughini Gras, L., Kretzschmar, M. E., den Hartog, G., van Gageldonk-Lafeber, A. B., et al. (2022). The estimated disease burden of acute covid-19 in the netherlands in 2020, in disability-adjusted life-years. *European Journal of Epidemiology*, 37(10):1035–1047.

McGarry, B. E., Gandhi, A. D., Grabowski, D. C., and Barnett, M. L. (2021). Larger nursing home staff size linked to higher number of covid-19 cases in 2020. *Health Affairs*, 40(8):1261–1269.

Michelozzi, P., de'Donato, F., Scortichini, M., Pezzotti, P., Stafoggia, M., De Sario, M., Costa, G., Noccioli, F., Riccardo, F., Bella, A., et al. (2020). Temporal dynamics in total excess mortality and covid-19 deaths in italian cities. *BMC public health*, 20:1–8.

Ministerie van Volksgezondheid, Welzijn en Sport (2018). Nationaal preventieakkoord. Retrieved from https://www.rijksoverheid.nl/onderwerpen/gezondheid-en-preventie/documenten/convenanten/2018/11/23/nationaal-preventieakkoord.

Ministerie van Volksgezondheid, Welzijn en Sport (2022). WOZO programma wonen, ondersteuning en zorg voor ouderen. Retrieved from https://open.overheid.nl/documenten/ronl-63e851f8e45b8662a4d04b1d5832b98231d40670/pdf.

Montiel Olea, J. L. and Pflueger, C. (2013). A robust test for weak instruments. *Journal of Business & Economic Statistics*, 31(3):358–369.

Mor, V., Berg, K., Angelelli, J., Gifford, D., Morris, J., and Moore, T. (2003). The quality of quality measurement in U.S. nursing homes. *Gerontologist*, 43(SPEC. ISS. 2):37–46.

Morris, C. N. (1983). Parametric empirical bayes inference: Theory and applications. *Journal of the American Statistical Association*, 78(381):47–55.

Moscelli, G., Siciliani, L., and Tonei, V. (2016). Do waiting times affect health outcomes? evidence from coronary bypass. *Social Science & Medicine*, 161:151–159.

Moura, A. (2022). Do subsidized nursing homes and home care teams reduce hospital bed-blocking? evidence from portugal. *Journal of Health Economics*, 84:102640.

Muns, S., Knoef, M., and Van Soest, A. (2018). Verschil in levensverwachting tussen lage en hoge inkomens is verder gestegen. Me Judice, Retrieved from https://www.mejudice.nl/artikelen/detail/verschil-in-levensverwachting-tussen-lage-en-hoge-inkomens-is-verder-gestegen#:~:text=In%20Nederland%20is%20in%20tien,9%2C4%20jaar%20voor%20vrouwen.

Newhouse, J. P. and McClellan, M. (1998). Econometrics in outcomes research: The use of instrumental variables. *Annual Review of Public Health*, 19:17–34.

Nikolova, S., Harrison, M., and Sutton, M. (2016). The impact of waiting time on health gains from surgery: Evidence from a national patient-reported outcome dataset. *Health Economics*, 25(8):955–968.

Nivel (n.d.). Rol Nivel Peilstations - bijdragen aan weekcijfers en jaarcijfers ziekten en aandoeningen. https://www.nivel.nl/nl/nivel-zorgregistraties-eerste-lijn/

actuele-weekcijfers-aandoeningen-surveillance/nivel-peilstations-detailcijfers-aandoeningen, Retrieved at October 26, 2022.

Nolte, E. and McKee, M. (2004). *Does health care save lives? Avoidable mortality revisited*. The Nuffield Trust.

Nolte, E. and McKee, M. (2011). Variations in amenable mortality—trends in 16 high-income nations. *Health Policy*, 103(1):47–52.

Nyman, J. A. (1988). Excess demand, the percentage of medicaid patients, and the quality of nursing home care. *The Journal of Human Resources*, 23(1):76–92.

Nyman, J. A. and Bricker, D. L. (1989). Profit incentives and technical efficiency in the production of nursing home care. *The Review of Economics and Statistics*, 71(4):586–594.

NZa (2021). Monitor ouderenzorg - regionale verschillen (deel 1). Marktscan - monitor, Nederlandse Zorgautoriteit. Retrieved from https://puc.overheid.nl/doc/PUC_636235_22.

NZa (2023). Scheiden wonen en zorg in de verpleging en verzorging. Deel 1 - advies, Nederlandse Zorgautoriteit. Retrieved from https://open.overheid.nl/documenten/043e176f-bed2-4536-87bc-37d06e156f12/file.

O'Donnell, O., O'Neill, S., Ourti, T. V., and Walsh, B. (2016). Conindex: Estimation of concentration indices. *The Stata Journal*, 16(1):112–138. PMID: 27053927.

OECD (2005). Ensuring quality long-term care for older people. Policy brief, Organisation for Economic Co-operation and Development. Available at https://www.oecd.org/els/health-systems/Ensuring-quality-long-term-care-for-older-people.pdf.

OECD (2009). *Premature mortality, in Health at a Glance 2009: OECD Indicators*. OECD Publishing, Paris.

OECD (2020a). Life expectancy at birth (indicator). Retrieved from https://doi.org/10.1787/27e0fc9d-en.

OECD (2020b). *Who Cares? Attracting and Retaining Care Workers for the Elderly*. OECD Health Policy Studies, OECD Publishing, Paris.

OECD (2021a). *Health at a Glance 2021: OECD Indicators*. OECD Publishing, Paris.

OECD (2021b). Health expenditure and financing. retrieved from https://stats.oecd.org/Index.aspx?DataSetCode=SHA#.

OECD (2023a). Life expectancy at birth (indicator). Retrieved from https://stats.oecd.org/index.aspx?queryid=30114#.

OECD (2023b). *Mortality and the Provision of Retirement Income*. OECD Publishing, Paris.

OECD (2023c). Population statistics. retrieved from https://stats.oecd.org/Index.aspx?DataSetCode=HISTPOP.

OECD and European Commission (2013). *A Good Life in Old Age? Monitoring and Improving Quality in Long-term Care*. OECD Health Policy Studies, OECD Publishing, Paris.

OECD and European Observatory on Health Systems and Policies (2019). *Netherlands: Country Health Profile 2019, State of Health in the EU*. OECD Publishing/European Observatory on Health Systems and Policies, Paris/Brussels.

OECD/Eurostat (2019). Avoidable mortality: Oecd/eurostat lists of preventable and treatable causes of death (november 2019 version). Available at https://www.oecd.org/health/health-systems/Avoidable-mortality-2019-Joint-OECD-Eurostat-List-preventable-treatable-causes-o pdf.

Oosterhoff, M., Kouwenberg, L. H., Rotteveel, A. H., van Vliet, E. D., Stadhouders, N., de Wit, G. A., and van Giessen, A. (2023). Estimating the health impact of delayed elective care during the covid -19 pandemic in the netherlands. *Social Science and Medicine*, 320:115658.

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

Oster, E. (2020). Health recommendations and selection in health behaviors. *American Economic Review: Insights*, 2(2):143–60.

Ouslander, J. G., Lamb, G., Perloe, M., Givens, J. H., Kluge, L., Rutland, T., Atherly, A., and Saliba, D. (2010). Potentially avoidable hospitalizations of nursing home residents: Frequency, causes, and costs. *Journal of the American Geriatrics Society*, 58(4):627–635.

Ouslander, J. G., Weinberg, A. D., and Phillips, V. (2000). Inappropriate hospitalization of nursing facility residents: A symptom of a sick system of care for frail older people. *Journal of the American Geriatrics Society*, 48(2):230–231.

Palmer, S. and Torgerson, D. J. (1999). Definitions of efficiency. *BMJ*, 318(7191):1136.

Panagiotou, O. A., Kosar, C. M., White, E. M., Bantis, L. E., Yang, X., Santostefano, C. M., Feifer, R. A., Blackman, C., Rudolph, J. L., Gravenstein, S., and Mor, V. (2021). Risk factors associated with all-cause 30-day mortality in nursing home residents with covid-19. *JAMA Internal Medicine*, 181(4):439–448.

Perraillon, M. C., Brauner, D. J., and Konetzka, R. T. (2019a). Nursing home response to nursing home compare: The provider perspective. *Medical Care Research and Review*, 76(4):425–443.

Perraillon, M. C., Konetzka, R. T., He, D., and Werner, R. M. (2019b). Consumer response to composite ratings of nursing home quality. *American journal of health economics*, 5(2):165–190.

Phillips, L. J., Birtley, N. M., Petroski, G. F., Siem, C., and Rantz, M. (2018). An observational study of antipsychotic medication use among long-stay nursing home residents without qualifying diagnoses. *Journal of Psychiatric and Mental Health Nursing*, 25(8):463–474.

Pieter van Baal, Frederik Peters, J. M. and Nusselder, W. (2016). Forecasting differences in life expectancy by education. *Population Studies*, 70(2):201–216.

Pinchbeck, E. W. (2019). Convenient primary care and emergency hospital utilisation. *Journal of Health Economics*, 68:102242.

Prudon, R. (2023). Is delayed mental health treatment detrimental to employment? Available at: https://www.rogerprudon.com/.

Quan, H., Li, B., Couris, C. M., Fushimi, K., Graham, P., Hider, P., Januel, J.-M., and Sundararajan, V. (2011). Updating and Validating the Charlson Comorbidity Index and Score for Risk Adjustment in Hospital Discharge Abstracts Using Data From 6 Countries. *American Journal of Epidemiology*, 173(6):676–682.

Raad voor Volksgezondheid en Samenleving (2020). *Gezondheidsverschillen voorbij: complexe ongelijkheid is een zaak van ons allemaal*. Den Haag: Raad voor Volksgezondheid en Samenleving.

Rahman, M., Grabowski, D. C., Mor, V., and Norton, E. C. (2016). Is a skilled nursing facility's rehospitalization rate a valid quality measure? *Health Services Research*, 51(6):2158–2175.

Reichert, A. and Jacobs, R. (2018). The impact of waiting time on patient outcomes: Evidence from early intervention in psychosis services in england. *Health Economics*, 27(11):1772–1787.

Rijksinstituut voor Volksgezondheid en Milieu (2020). Toekomstverkenning zorguitgaven 2015-2060: Kwantitatief vooronderzoek in opdracht van de wetenschappelijke raad voor het regeringsbeleid. deel 1: toekomstprojecties. Rivm-rapport 2020-0059, Rijksinstituut voor Volksgezondheid en Milieu, Bilthoven.

Rijksoverheid (2017). Rijksbegroting. Government of the Netherlands. Available at https://www.rijksbegroting.nl/2017/verantwoording/jaarverslag,kst244411_29.html.

Rijksoverheid (2023). Niets doen is geen optie. Ibo ouderenzorg, Rijksoverheid. Retrieved from https://open.overheid.nl/documenten/b0d11071-3141-4b48-8c81-4e5b372427b3/file.

RIVM (2022). Covid-19 aantallen per gemeente per publicatiedatum. Retrieved from https://data.rivm.nl/meta/srv/eng/catalog.search;

jsessionid=7E3B438E1812BD5F2AE74734A9B43230#/metadata/5f6bc429-1596-490e-8618-1ed8fd768427 on September 2nd 2022.

Rutten, J. J., van Loon, A. M., van Kooten, J., van Buul, L. W., Joling, K. J., Smalbrugge, M., and Hertogh, C. M. (2020). Clinical suspicion of covid-19 in nursing home residents: Symptoms and mortality risk factors. *Journal of the American Medical Directors Association*, 21(12):1791–1797.

Schmitz, H. and Stroka-Wetsch, M. A. (2020). Determinants of nursing home choice: Does reported quality matter? *Health Economics*, 29(7):766–777.

Schols, J., Poot, E., Nieuwenhuizen, N., and Achterberg, W. (2020). Dealing with covid-19 in dutch nursing homes. *Journal of Nursing Home Research*, 6:30–4.

Schultze, A., Nightingale, E., Evans, D., Hulme, W., Rosello, A., Bates, C., Cockburn, J., MacKenna, B., Curtis, H. J., Morton, C. E., et al. (2022). Mortality among care home residents in england during the first and second waves of the covid-19 pandemic: an observational study of 4.3 million adults over the age of 65. *The Lancet Regional Health-Europe*, 14.

Schwandt, H., Currie, J., Bär, M., Banks, J., Bertoli, P., Bütikofer, A., Cattan, S., Zong-Ying Chao, B., Costa, C., González, L., Grembi, V., Huttunen, K., Karadakic, R., Kraftman, L., Krutikova, S., Lombardi, S., Redler, P., Riumallo-Herl, C., Rodríguez-González, A., Salvanes, K. G., Santana, P., Thuilliez, J., van Doorslaer, E., Van Ourti, T., Winter, J. K., Wouterse, B., and Wuppermann, A. (2021). Inequality in mortality between black and white americans by age, place, and cause and in comparison to europe, 1990 to 2018. *Proceedings of the National Academy of Sciences*, 118(40):e2104684118.

SCP (2019a). *Wonen met zorg: verkenning van particuliere woonzorg voor ouderen*. Sociaal en Cultureel Planbureau, Den Haag.

SCP (2019b). *Zorgen voor thuiswonende ouderen*. Sociaal en Cultureel Planbureau, Den Haag.

SCP (2021). *Het leven in een verpleeghuis*. Sociaal en Cultureel Planbureau, Den Haag.

Sepulveda, E. R. and Brooker, A.-S. (2021). Income inequality and covid-19 mortality: Age-stratified analysis of 22 oecd countries. *SSM - Population Health*, 16:100904.

Serrano-Alarcón, M., Hernández-Pizarro, H., López-Casasnovas, G., and Nicodemo, C. (2022). Effects of long-term care benefits on healthcare utilization in catalonia. *Journal of Health Economics*, 84:102645.

Shen, K., Loomer, L., Abrams, H., Grabowski, D. C., and Gandhi, A. (2021). Estimates of COVID-19 Cases and Deaths Among Nursing Home Residents Not Reported in Federal Data. *JAMA Network Open*, 4(9):e2122885–e2122885.

Shugarman, L. R. and Brown, J. A. (2006). Nursing home selection: How do consumers choose? volume i: Findings from focus groups of consumers and information intermediaries. Rand ccrporation, Office of Disability, Aging and Long-Term Care Policy Office of the Assistant Secretary for Planning and Evaluation U.S. Department of Health and Human Services, Washington D.C.

Sociaal Cultureel Planbureau/Statistics Netherlands (2014). *Armoedesignalement 2014*. Den Haag: Sociaal Cultureel Planbureau/Statistics Netherlands.

Spiers, G., Matthews, F. E., Moffatt, S., Barker, R., Jarvis, H., Stow, D., Kingston, A., and Hanratty, B. (2019). Does older adults' use of social care influence their healthcare utilisation? a systematic review of international evidence. *Health & Social Care in the Community*, 27(5):e651–e662.

Spiers, G. F., Liddle, J. E., Stow, D., Searle, B., Whitehead, I. O., Kingston, A., Moffatt, S., Matthews, F. E., and Hanratty, B. (2022). Measuring older people's socioeconomic position: a scoping review of studies of self-rated health, health service and social care use. *Journal of Epidemiology & Community Health*, 76(6):572–579.

Spilsbury, K., Hewitt, C., Stirk, L., and Bowman, C. (2011). The relationship between nurse staffing and quality of care in nursing homes: A systematic review. *International Journal of Nursing Studies*, 48(6):732–750.

Staiger, D. and Stock, J. H. (1997). Instrumental variables regression with weak instruments. *Econometrica*, 65(3).

Stang, A., Standl, F., Kowall, B., Brune, B., Böttcher, J., Brinkmann, M., Dittmer, U., and Jöckel, K.-H. (2020). Excess mortality due to covid-19 in germany. *Journal of Infection*, 81(5):797–801.

Statistics Netherlands (2004). Statline: Regionale inkomensverdeling 2000, kerncijfers. Retrieved from `https://opendata.cbs.nl/#/CBS/nl/dataset/70050ned/table?ts=1603787618593`.

Statistics Netherlands (2012). Kloof in levensverwachting tussen hoog-en laagopgeleiden blijft even groot. Retrieved from `https://www.cbs.nl/nl-nl/achtergrond/2012/06/kloof-in-levensverwachting-tussen-hoog-en-laagopgeleiden-blijft-even-groot`

Statistics Netherlands (2016). *Welvaart in Nederland 2016*. Den Haag: Centraal Bureau voor de Statistiek.

Statistics Netherlands (2017). Eigen betalingen zorg. Statistics Netherlands. Available at `https://www.cbs.nl/nl-nl/maatwerk/2017/07/eigen-betalingen-zorg`.

Statistics Netherlands (2020a). Begrippen: gestandaardiseerd inkomen.

Statistics Netherlands (2020b). Begrippen: lage-inkomensgrens.

Statistics Netherlands (2020c). Gemeentelijke indelingen per jaar.

Statistics Netherlands (2022a). Monitor langduring zorg, personen met indicatie naar gebruik wlz-zorg; indicatie, leveringsvorm, zzp. Retrieved at October 7, 2022, from https://opendata.cbs.nl/#/CBS/nl/dataset/84529NED/table?dl=2CAFA.

Statistics Netherlands (2022b). Over 19 thousand covid-19 deaths in 2021. Available at: https://www.cbs.nl/en-gb/news/2022/13/over-19-thousand-covid-19-deaths-in-2021.

Statistics Netherlands (2023a). Monitor langduring zorg, bevolking; huishouden en regio. Retrieved at September 5, 2023, from https://mlzopendata.cbs.nl/#/MLZ/nl/dataset/40015NED/table?dl=6E5A4.

Statistics Netherlands (2023b). Monitor langduring zorg, personen met een pgb; aantal zorgverleners, zorgzwaartepakket. Retrieved at July 27, 2023, from https://mlzopendata.cbs.nl/#/MLZ/nl/dataset/40063NED/table?dl=93DBC.

Statistics Netherlands (2023c). Monitor langduring zorg, personen met indicatie zorg met verblijf; zzp en regio. Retrieved at July 27, 2023, from https://mlzopendata.cbs.nl/#/MLZ/nl/dataset/40004NED/table?dl=93DBB.

Statistics Netherlands (2023d). Overledenen; doodsoorzaak (uitgebreide lijst), leeftijd, geslacht. Retrieved at October 27, 2023, from https://opendata.cbs.nl/statline/#/CBS/nl/dataset/7233/table?dl=99557.

Statistics Netherlands (n.d.). Definitions: Degree of urbanisation. Retrieved at October 26, 2021, from https://www.cbs.nl/en-gb/onze-diensten/methods/definitions/degree-of-urbanisation.

Statistics Netherlands and RIVM (2022). *Sterfte en oversterfte in 2020 en 2021 - Onderzoek door het CBS en het RIVM, in het kader van het ZonMw onderzoeksprogramma Oversterfte*. Centraal Bureau voor de Statistiek, Den Haag.

Stevens, A. H., Miller, D. L., Page, M. E., and Filipski, M. (2015). The best of times, the worst of times: Understanding pro-cyclical mortality. *American Economic Journal: Economic Policy*, 7(4):279–311.

Stoeldraijer, L., Kunst, A., Chilunga, F., and Harmsen, C. (2022). *Sociaal-demografische verschillen in COVID-19-sterfte in het eerste jaar van de coronapandemie*. Den Haag: Centraal Bureau Voor de Statistiek.

Sundararajan, V., Henderson, T., Perry, C., Muggivan, A., Quan, H., and Ghali, W. A. (2004). New icd-10 version of the charlson comorbidity index predicted in-hospital mortality. *Journal of Clinical Epidemiology*, 57:1288–1294.

Tenand, M., Bakx, P., and Wouterse, B. (2021). The impact of co-payments for nursing home care on use, health, and welfare. EsCHER Working Paper Series No. 2021002, Erasmus University Rotterdam. Available from: https://www.eur.nl/en/research/escher/research/working-papers.

TNO (2019). Prognose capaciteitsontwikkeling verpleeghuiszorg. Tno-rapport r12033, Nederlandse Organisatie voor Toegepast Natuurwetenschappelijk Onderzoek. Retrieved from https://www.rijksoverheid.nl/documenten/rapporten/2019/12/20/prognose-capaciteitsontwikkeling-verpleeghuiszorg.

United Nations (2022). World population prospects 2022: Summary of results. Un desa/pop/2022/tr/no. 3, United Nations Department of Economic and Social Affairs, Population Division.

Unruh, M. A., Yun, H., Zhang, Y., Braun, R. T., and Jung, H.-Y. (2020). Nursing home characteristics associated with covid-19 deaths in connecticut, new jersey, and new york. *Journal of the American Medical Directors Association*, 21(7):1001–1003.

van den Besselaar, J. H., Sikkema, R. S., Koene, F. M. H. P. A., van Buul, L. W., Oude Munnink, B. B., Frénay, I., Witt, R. T., Koopmans, M. P. G., Hertogh, C. M. P. M., and Buurman, B. M. (2021). Are presymptomatic sars-cov-2 infections in nursing home residents unrecognised symptomatic infections? sequence and metadata from weekly testing in an extensive nursing home outbreak. *Age and Ageing*, 50(5):1454–1463.

van Doorslaer, E., Masseria, C., and Koolman, X. (2006). Inequalities in access to medical care by income in developed countries. *CMAJ*, 174(2):177–183.

Van Houtven, C. H. and Norton, E. C. (2004). Informal care and health care use of older adults. *Journal of Health Economics*, 23(6):1159–1180.

van Reijen, R., Dierkx, R., Dauwerse, L., Drost, K., and Nonneman, M. (2022). Mortaliteit bij covid-19-vaccinatie in het verpleeghuis. *Tijdschrift voor Ouderengeneeskunde*, 3:2–12.

Vossius, C., Selbæk, G., Šaltytė Benth, J., and Bergh, S. (2018). Mortality in nursing home residents: a longitudinal study over three years. *PloS one*, 13(9):e0203480.

Vu, M. Q., Weintraub, N., and Rubenstein, L. Z. (2006). Falls in the nursing home: Are they preventable? *Journal of the American Medical Directors Association*, 7(3, Supplement):S53–S58. CME Digest.

V&VN (2020). Peiling v&vn: tekorten maskers houden aan, psychische druk hoog. Retrieved from https://www.venvn.nl/nieuws/peiling-v-vn-tekorten-maskers-houden-aan-psychische-druk-hoog/.

Wagstaff, A., Paci, P., and van Doorslaer, E. (1991). On the measurement of inequalities in health. *Social Science & Medicine*, 33(5):545–557.

Walker, J. D., Teare, G. F., Hogan, D. B., Lewis, S., and Maxwell, C. J. (2009). Identifying potentially avoidable hospital admissions from canadian long-term care facilities. *Medical Care*, 47(2):250–254.

Wang, H., Paulson, K. R., Pease, S. A., Watson, S., Comfort, H., Zheng, P., Aravkin, A. Y., Bisignano, C., Barber, R. M., Alam, T., et al. (2022). Estimating excess mortality due to the covid-19 pandemic: a systematic analysis of covid-19-related mortality, 2020–21. *The Lancet*, 399(10334):1513–1536.

Werner, R. M., Hoffman, A. K., and Coe, N. B. (2020). Long-term care policy after covid-19 — solving the nursing home crisis. *New England Journal of Medicine*, 383(10):903–905.

Werner, R. M., Rita, T., and Kim, M. (2013). Quality improvement under nursing home compare: The association between changes in process and outcome measures. *Medical Care*, 51(7):582–588.

Wetenschappelijke Raad voor het Regeringsbeleid (2018). *Van verschil naar potentieel. Een realistisch perspectief op de sociaaleconomische gezondheidsverschillen*. Den Haag: Wetenschappelijke Raad voor het Regeringsbeleid.

WHO (2020). International guidelines for certification and classification (coding) of covid-19 as cause of death. Technical report, World Health Organization.

WHO (2022). 14.9 million excess deaths associated with the covid-19 pandemic in 2020 and 2021. Available: https://www.who.int/news/item/05-05-2022-14.9-million-excess-deaths-were-associated-with-the-covid-19-pandemic-in-2020-and-2021.

Woolf, S. H., Chapman, D. A., Sabo, R. T., and Zimmerman, E. B. (2021). Excess deaths from covid-19 and other causes in the us, march 1, 2020, to january 2, 2021. *JAMA*, 325(17):1786–1789.

World Health Organization (2016). International statistical classification of diseases and related health problems 10th revision. Retrieved at May 23, 2023, from `https://icd.who.int/browse10/2016/en`.

World Health Organization (2023). Methods for estimating the excess mortality associated with the covid-19 pandemic. Technical document, Geneva: World Health Organization.

Wouterse, B., Bakx, P., and Wong, A. (2023a). Measuring nursing home performance using administrative data. *Medical Care Research and Review*, 80(2):187–204. PMID: 35872642.

Wouterse, B., Geisler, J., Bär, M., and van Doorslaer, E. (2023b). Has covid-19 increased inequality in mortality by income in the netherlands? *Journal of Epidemiology & Community Health*, 77(4):244–251.

Wouterse, B., Hussem, A., and Wong, A. (2022). The risk protection and redistribution effects of long-term care co-payments. *Journal of Risk and Insurance*, 89(1):161–186.

Yaya, S., Yeboah, H., Charles, C. H., Otu, A., and Labonte, R. (2020). Ethnic and racial disparities in covid-19-related deaths: counting the trees, hiding the forest. *BMJ Global Health*, 5(6).

You, K., Li, Y., Intrator, O., Stevenson, D., Hirth, R., Grabowski, D., and Banaszak-Holl, J. (2016). Do nursing home chain size and proprietary status affect experiences with care? *Medical care*, 54(3):229.

Zorgverzekeraars Nederland (2023). Wachtlijsten langdurige zorg. Retrieved at 14 April 2023 from https://www.zn.nl/zorgkantoren/wachtlijsten-langdurige-zorg.

Appendices per chapter

A Appendix - Diverging Mortality Inequality Trends among Young and Old in the Netherlands

Appendix Tables

Table A1: Causes with the largest changes between 1996 and 2016

	Cause of death (ISHMT classification) with largest absolute change between 1996 and 2016 by age-group, sex and avoidable mortality category.			
	Preventable	Treatable	Both	Other
Women				
Age 0-4	Other infectious and parasitic diseases (28.1%)	Disorders related to short gestation and low birth weight (35.7%)		Congenital malformations, deformations and chromosomal abnormalities (62.7%)
Age 5-19	Transport accidents (56.6%)*			Other malignant neoplasms (35.4%)
Age 20-49	Malignant neoplasms of trachea, bronchus and lung (25.2%)	Malignant neoplasm of breast (64.6%)	Cerebrovascular diseases (48.9%)	Other malignant neoplasms (26.1%)
Age 50-64	Malignant neoplasms of trachea, bronchus and lung (53.6%)	Malignant neoplasm of breast (49.5%)	Acute myocardial infarction (57.9%)	Other malignant neoplasms (18.1%)
Age 65-79	Malignant neoplasms of trachea, bronchus and lung (51.9%)	Pneumonia (29.3%)	Acute myocardial infarction (52.2%)	Other malignant neoplasms (15.7%)
Age 80+	External causes other than transport accidents (41.3%)*	Pneumonia (33.5%)	Acute myocardial infarction (49.6%)	Dementia (36.6%)
Men				
Age 0-4	Congenital malformations, deformations and chromosomal abnormalities (24.1%)	Disorders related to short gestation and low birth weight (49.0%)		Congenital malformations, deformations and chromosomal abnormalities (35.9%)
Age 5-19	Transport accidents (76.5%)*			Other malignant neoplasms (29.9%)
Age 20-49	Transport accidents (29.6%)*	Pneumonia (20.2%)	Acute myocardial infarction (65.5%)	Other malignant neoplasms (27.7%)
Age 50-64	Malignant neoplasms of trachea, bronchus and lung (33.5%)	Malignant neoplasm of colon, rectum and anus (23.0%)	Acute myocardial infarction (69.7%)	Conduction disorders and cardiac arrhythmias (19.3%)
Age 65-79	Chronic obstructive pulmonary disease and bronchiectasis (25.4%)	Pneumonia (27.4%)	Acute myocardial infarction (57.3%)	Other malignant neoplasms (22.4%)
Age 80+	External causes other than transport accidents (36.8%)*	Malignant neoplasm of colon, rectum and anus (21.3%)	Acute myocardial infarction (58.9%)	Dementia (26.0%)

Notes: This table presents the causes of death (using the ISHMT classification of ICD-10 codes) that experienced the greatest changes (either positive or negative) between 1996 and 2016 in terms of absolute number of deaths within each age-group, sex and cause of death category. The percentage between brackets indicates the share of the total absolute change in deaths that is explained by the given cause within the corresponding category, age-group and sex.
*ICD-10 chapters V, W, X and Y are not part of the ISHMT list and are therefore here classified using ICD-10 blocks.

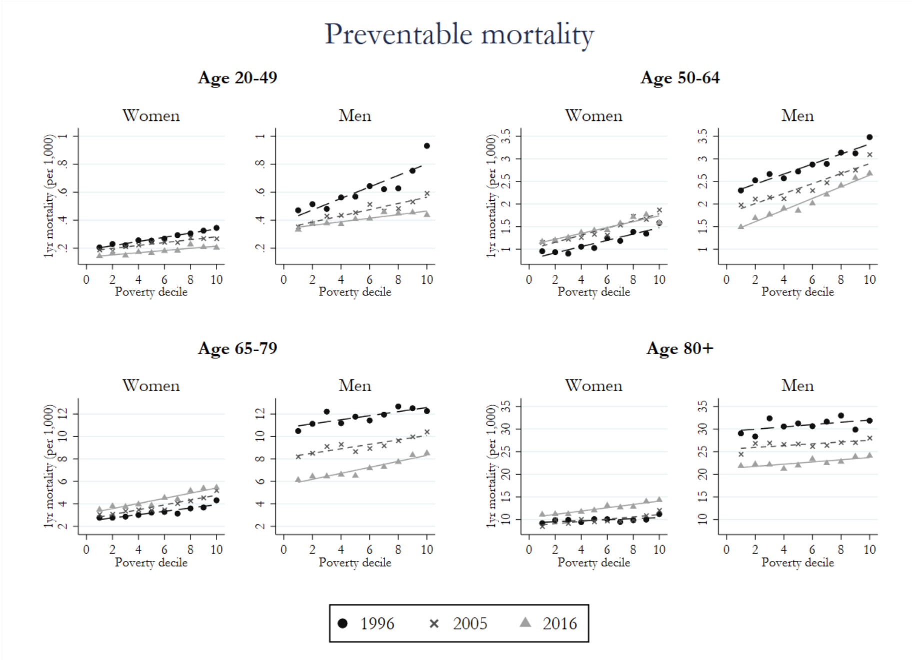
Table A2: Slope coefficients of fitted regression lines in Figure 2.3 including constants

	Poverty gradient of change in mortality between 1996 and 2016 (slope coefficients and intercepts from fitted regression lines in Figure 3)							
	Preventable		Treatable		Both		Other	
	Coeff (1)	Cons (2)	Coeff (3)	Cons (4)	Coeff (5)	Cons (6)	Coeff (7)	Cons (8)
Women								
Age 20-49	-.007	-.052	-.000	-.089	-.005	-.045	-.008	-.035
	(.001)***	(.009)***	(.001)	(.009)***	(.002)***	(.009)***	(.002)***	(.014)***
Age 50-64	-.005	.312	-.004	-.402	-.040	-.350	.003	-.346
	(.015)	(.090)***	(.008)	(.049)***	(.007)***	(.045)***	(.014)	(.088)***
Age 65-79	.081	.668	.022	-1.373	-.028	-4.342	.071	-1.490
	(.028)***	(.176)***	(.022)	(.134)***	(.033)	(.203)***	(.035)**	(.217)***
Age 80+	.254	1.114	.008	-6.547	.326	-19.533	.242	-.038
	(.054)***	(.332)***	(.075)	(.465)***	(.170)*	(1.054)***	(.215)	(1.334)
Men								
Age 20-49	-.028	-.053	-.004	.003	-.009	-.097	-.010	-.071
	(.009)***	(.054)	(.001)***	(.007)	(.003)***	(.016)***	(.004)**	(.025)***
Age 50-64	.018	-.867	.005	-.229	-.052	-1.360	-.026	-.610
	(.011)	(.071)***	(.006)	(.039)***	(.017)***	(.107)***	(.020)	(.124)***
Age 65-79	.085	-5.106	-.022	-1.425	-.006	-8.947	.105	-3.674
	(.060)	(.374)***	(.023)	(.143)***	(.075)	(.464)***	(.049)**	(.303)***
Age 80+	-.009	-8.210	-.023	-7.884	.007	-23.764	.192	-3.588
	(.187)	(1.159)***	(.127)	(.789)***	(.146)	(.907)***	(.201)	(1.249)***

Notes: Table A2 reports the estimated slope coefficients (Coeff) and intercepts (Cons) from a regression of the change in mortality between 1996 and 2016 on poverty deciles, as plotted in Figure 2.3, by cause category, sex and age-group. Standard errors are between brackets.
* statistically significant from zero at 10%; ** statistically significant from zero at 5%; *** statistically significant from zero at 1%.

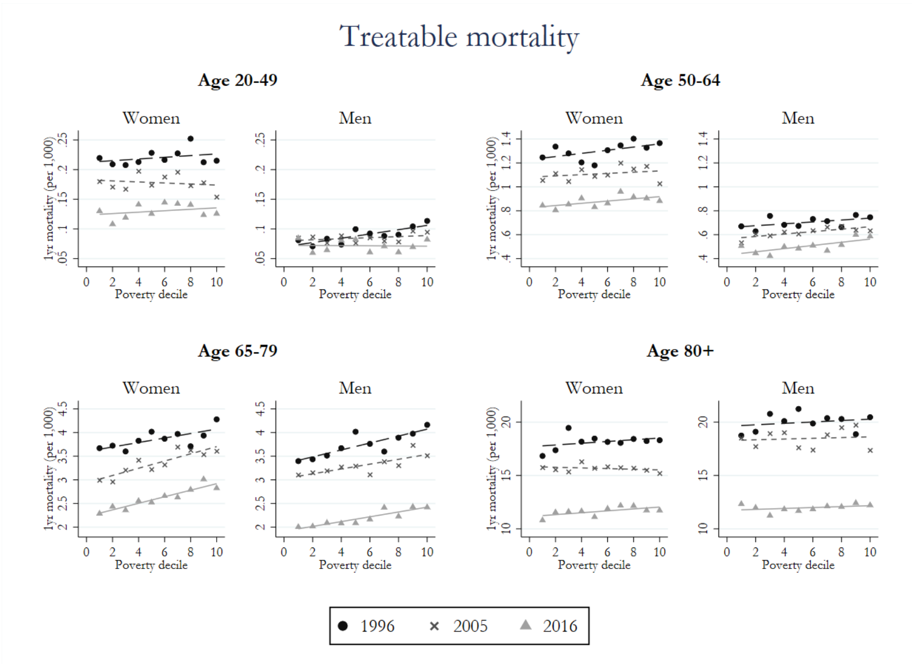
Appendix Figures

Figure A1: Poverty gradients in preventable mortality by gender, age group and year



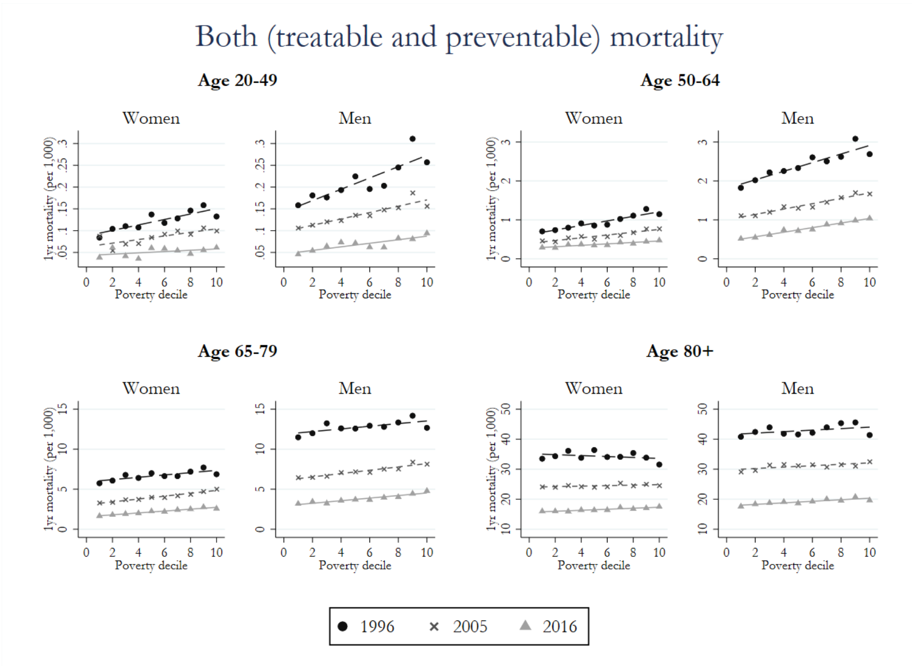
Note: This figure plots one-year mortality rates (smoothed over three years) from potentially preventable causes across poverty deciles by gender, age group and year. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The slope coefficients of the fitted regression lines can be found in columns 1–3 of Table 2.3.

Figure A2: Poverty gradients in treatable mortality by gender, age group and year



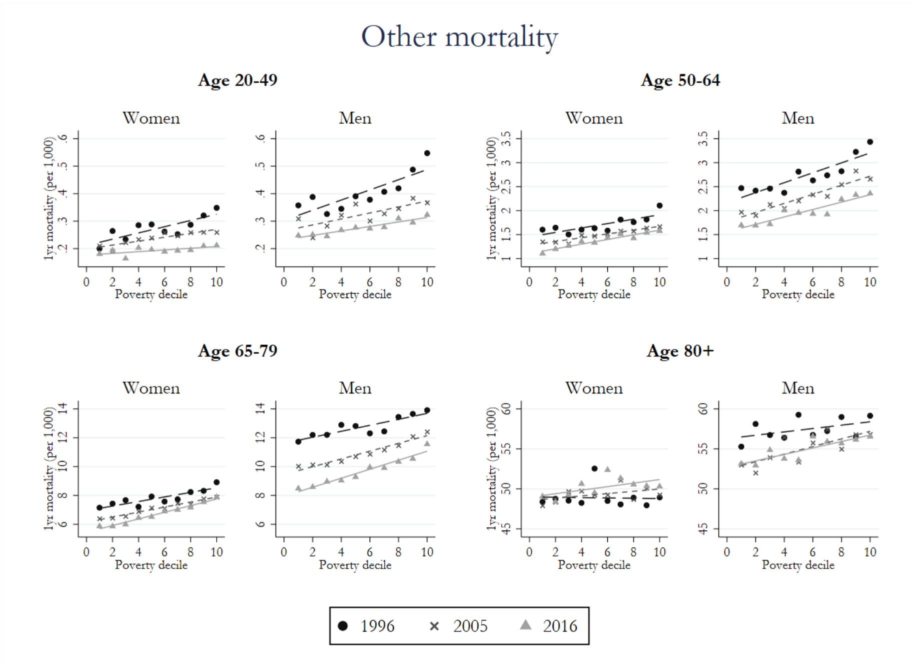
Note: This figure plots one-year mortality rates (smoothed over three years) from potentially treatable causes across poverty deciles by gender, age group and year. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The slope coefficients of the fitted regression lines can be found in columns 4–6 of Table 2.3.

Figure A3: Poverty gradients in preventable mortality by gender, age group and year



Note: This figure plots one-year mortality rates (smoothed over three years) from causes that could be both preventable and treatable across poverty deciles by gender, age group and year. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The slope coefficients of the fitted regression lines can be found in columns 7–9 of Table 2.3.

Figure A4: Poverty gradients in preventable mortality by gender, age group and year



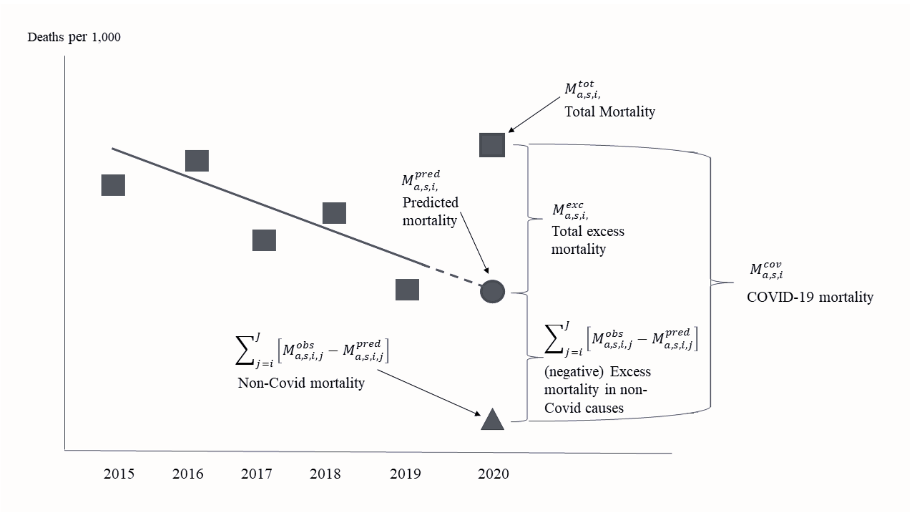
Note: This figure plots one-year mortality rates (smoothed over three years) from causes that are not labelled as preventable or treatable across poverty deciles by gender, age group and year. Poverty decile 1 contains 10 per cent of the population living in the wealthiest municipalities and decile 10 contains those living in the poorest municipalities. Mortality rates are age-adjusted using one-year age bins, keeping the age composition within each age group and gender similar to the one in 1995. The slope coefficients of the fitted regression lines can be found in columns 10–12 of Table 2.3.

B Appendix - Has COVID-19 increased inequality in mortality by income in the Netherlands?

Measurement of mortality

Crucial 2020 mortality concepts in our approach are (i) the trend-predicted mortality, (ii) total observed mortality, (iii) COVID-19 cause-of-death mortality, (iv) excess mortality, and (v) non-COVID mortality. Figure 1 shows a graphical exposition of how these concepts are related. In 2020, the observed total mortality probability can differ from the trend-predicted probability because of a new cause (COVID-19) or because the mortality probabilities for non-COVID causes have changed. To determine what the mortality in 2020 would have been in the absence of COVID-19, we compute trend-predicted mortality ($M^{pred}_{a,s,i,2020}$) in 2020 by estimating linear trends for each age-sex-income group for the years 2015-2019, and then predict the mortality probability in 2020.

Figure B1: Illustration of Mortality Concepts



Note: Figure B1 graphically illustrates the mortality concepts used in this paper using hypothetical data. The blue squares represent total observed mortality (in deaths per 1000) in 2015-2019, which are used to fit the time trend (solid line) to be extrapolated (dotted line) to obtain trend-predicted mortality in 2020 (circle). Excess mortality, defined as the difference between trend-predicted (circle) and observed mortality (blue-red square). The latter can be decomposed into (cause of death defined) COVID-19 and non-COVID mortality (triangle). We call the difference between COVID-19 and excess mortality substitute mortality, which is by definition equal to trend-predicted minus non-COVID mortality.

By subtracting trend-predicted from observed total mortality, we obtain an estimate of total excess mortality for each age (a), sex (s), and income (i) group: $M^{exc}_{a,s,i} = M^{obs}_{a,s,i} - M^{pred}_{a,s,i}$. It reflects how observed total mortality in 2020 differs from what would be expected from a simple linear age-, sex- and income-group-specific trend extrapolation.

Total excess mortality is the sum of Covid-19 mortality ($M^{cov}_{a,s,i}$) and excess mortality in all J other causes ($\sum_{j=i}^{J}\left[M^{obs}_{a,s,i,j} - M^{pred}_{a,s,i,j}\right]$). This gives the following decomposition of total mortality that we will use throughout the remainder of the paper:

$$M^{obs}_{a,s,i} = M^{pred}_{a,s,i} + M^{cov}_{a,s,i} + \sum_{j=i}^{J}\left[M^{obs}_{a,s,i,j} - M^{pred}_{a,s,i,j}\right]. \tag{1}$$

Decomposition of inequality by cause of death

To assess how the emergence of COVID-19 mortality has affected inequality in total mortality, and how this effect depends on differential substitution of non-COVID mortality across groups, we start from the identity in Equation (1) and decompose the inequality in observed total mortality (in 2020) as a weighted sum of the inequality measured in trend-predicted total mortality plus COVID-19 mortality plus excess mortality for all other causes. We use the convenient property of the concentration index that it can be decomposed into sources or factors. For any variable that can be written as a sum of components, the concentration index of the sum equals the weighted sum of the concentration indices of these factors or components, with the weights equal to the relative shares of each component:

$$\begin{aligned} C(M^{tot}_{a,s}) &= w^{pred}_{a,s}C(M^{pred}_{a,s}) + w^{cov}_{a,s}C(M^{cov}_{a,s}) \\ &+ \sum_{j=1}^{J}\left[w^{obs}_{a,s,j}C(M^{obs}_{a,s,j}) - w^{pred}_{a,s,j}C(M^{pred}_{a,s,j})\right], \end{aligned} \tag{2}$$

where $C(M^{tot}_{a,s})$ denotes the concentration index of the observed total mortality probability for age a and sex s. The other concentration indices are defined analogously for the trend-predicted total mortality probability $C(M^{pred}_{a,s})$, the COVID-19-attributed mortality probability $C(M^{cov}_{a,s})$, the non-COVID cause-specific observed and trend-predicted mortality probabilities, $C(M^{obs}_{a,s,j})$ and $C(M^{pred}_{a,s,j})$, with j denoting the cause. The weights of each component are based on the shares of deaths relative to the total number of observed deaths in an age-sex group in 2020: $w^{pred}_{a,s} = \frac{\sum_{i=1}^{20} M^{pred}_{a,s,i}}{\sum_{i=1}^{20} M^{tot}_{a,s,i}}$, $w^{cov}_{a,s} = \frac{\sum_{i=1}^{20} M^{cov}_{a,s,i}}{\sum_{i=1}^{20} M^{tot}_{a,s,i}}$, $w^{obs}_{a,s,j} = \frac{\sum_{i=1}^{20} M^{obs}_{a,s,i,j}}{\sum_{i=1}^{20} M^{tot}_{a,s,i}}$, and $w^{pred}_{a,s,j} = \frac{\sum_{i=1}^{20} M^{pred}_{a,s,i,j}}{\sum_{i=1}^{20} M^{tot}_{a,s,i}}$.

The last term of Equation (2) allows us to compare the relative inequality in predicted mortality to actual mortality for each cause, and assess to which extent (negative) excess mortality for each cause has contributed to an in- or decrease of inequality in total mortality. To ease the interpretation, and by labeling the Concentration Index Contribution of cause j as $CIC(M^{j}_{a,s}) = w^{obs}_{a,s}C(M^{obs}_{a,s}) - w^{pred}_{a,s}C(M^{pred}_{a,s})$, we can rewrite Equation (2) as:

$$\underbrace{C(M_{a,s}^{tot}) - w_{a,s}^{pred} C(M_{a,s}^{pred})}_{\text{Inequality in excess mortality in 2020}} = \underbrace{w_{a,s}^{cov} C(M_{a,s}^{cov})}_{\text{Inequality contribution of COVID}} + \underbrace{\sum_{j=1}^{J} [CIC(M_{a,s})^{j}]}_{\text{Inequality contribution of other causes}} \tag{3}$$

The left-hand side term measures the inequality in excess mortality in 2020, namely how much more unequal mortality has become compared to what was expected based on the trend. The right-hand side shows the contribution to the inequality in excess mortality as the weighted sum of inequality in COVID-19 mortality plus the inequality contributions of each of the J causes of death. The last term is now written as the (simple, unweighted) sum of the 'inequality contributions' of each cause of death. Note that these contributions can be positive or negative. Equation (3) is the identity used to disentangle the separate contributions of death causes to total observed inequality.

Appendix Tables

Table B1: Summary statistics for women aged 65-79 by income group

Income Group	All	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Total Mortality	0.017	0.036	0.026	0.023	0.021	0.021	0.021	0.019	0.017	0.016	0.015	0.014	0.014	0.014	0.013	0.012	0.012	0.011	0.011	0.011	0.010
Predicted Mortality	0.016 (0.014; 0.018)	0.032 (0.031; 0.033)	0.024 (0.023; 0.025)	0.024 (0.023; 0.025)	0.020 (0.019; 0.021)	0.019 (0.018; 0.02)	0.018 (0.017; 0.019)	0.017 (0.016; 0.018)	0.017 (0.016; 0.018)	0.015 (0.014; 0.016)	0.014 (0.013; 0.015)	0.013 (0.012; 0.014)	0.013 (0.012; 0.014)	0.013 (0.012; 0.014)	0.012 (0.011; 0.013)	0.012 (0.011; 0.013)	0.012 (0.011; 0.013)	0.010 (0.009; 0.011)	0.011 (0.01; 0.012)	0.010 (0.009; 0.011)	0.009 (0.008; 0.01)
Covid Mortality	0.002	0.006	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
Circulatory Mortality	0.003	0.006	0.005	0.004	0.004	0.004	0.004	0.003	0.003	0.003	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
Predicted Mortality	0.003 (0.002; 0.004)	0.006 (0.003; 0.009)	0.005 (0.002; 0.007)	0.005 (0.002; 0.008)	0.004 (0.001; 0.007)	0.003 (0.001; 0.006)	0.003 (0; 0.006)	0.003 (0; 0.006)	0.003 (0; 0.006)	0.003 (0; 0.006)	0.002 (0; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.002 (-0.001; 0.005)	0.001 (-0.001; 0.004)	0.001 (-0.002; 0.004)
Respiratory Mortality	0.001	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000
Predicted Mortality	0.002 (-0.001; 0.004)	0.004 (0; 0.008)	0.003 (-0.001; 0.007)	0.003 (-0.001; 0.007)	0.002 (-0.002; 0.006)	0.002 (-0.002; 0.006)	0.002 (-0.002; 0.006)	0.002 (-0.002; 0.006)	0.002 (-0.002; 0.006)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.001 (-0.003; 0.005)	0.000 (-0.003; 0.005)	0.000 (-0.003; 0.005)	0.000 (-0.003; 0.004)
Mental Mortality	0.001	0.003	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.001	0.000	0.000	0.001	0.000	0.000	0.001	0.001	0.001	0.000
Predicted Mortality	0.001 (0; 0.001)	0.003 (0.001; 0.005)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.001 (-0.001; 0.003)	0.001 (-0.001; 0.002)	0.001 (-0.001; 0.003)	0.000 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.000 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.000 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.001 (-0.001; 0.002)	0.000 (-0.001; 0.002)
Cancer Mortality	0.007	0.009	0.009	0.009	0.009	0.008	0.009	0.008	0.007	0.007	0.007	0.006	0.007	0.006	0.006	0.006	0.006	0.006	0.005	0.005	0.005
Predicted Mortality	0.007 (0.005; 0.009)	0.008 (0.006; 0.011)	0.009 (0.007; 0.011)	0.009 (0.007; 0.011)	0.008 (0.006; 0.011)	0.009 (0.006; 0.011)	0.008 (0.006; 0.011)	0.008 (0.005; 0.01)	0.008 (0.006; 0.01)	0.007 (0.004; 0.009)	0.007 (0.005; 0.009)	0.007 (0.004; 0.009)	0.007 (0.004; 0.009)	0.006 (0.004; 0.009)	0.006 (0.004; 0.009)	0.006 (0.003; 0.008)	0.006 (0.003; 0.008)	0.005 (0.003; 0.008)	0.006 (0.003; 0.008)	0.005 (0.003; 0.008)	0.005 (0.003; 0.007)
Other Mortality	0.004	0.010	0.005	0.005	0.004	0.004	0.004	0.004	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.002	0.002	0.002	0.002	0.002	0.002
Predicted Mortality	0.004 (0.002; 0.005)	0.011 (0.009; 0.012)	0.006 (0.004; 0.008)	0.005 (0.004; 0.007)	0.004 (0.003; 0.006)	0.004 (0.002; 0.006)	0.004 (0.002; 0.005)	0.004 (0.002; 0.006)	0.004 (0.002; 0.006)	0.003 (0.002; 0.005)	0.003 (0.001; 0.005)	0.002 (0.001; 0.004)	0.002 (0.001; 0.004)	0.003 (0.001; 0.005)	0.003 (0.001; 0.004)	0.003 (0.001; 0.004)	0.003 (0.001; 0.004)	0.002 (0; 0.004)	0.003 (0.001; 0.005)	0.002 (0.001; 0.004)	0.002 (0; 0.004)
Population	1305939	65317	65286	65340	65315	65235	65367	65258	65266	65360	65259	65280	65310	65302	65286	65290	65299	65287	65294	65295	65293

Notes: This table shows the summary statistics of the mortality probabilities by cause of death for females aged 65-79 by 20 income groups. The causes of death are divided into 6 categories: COVID-19, circulatory, respiratory, mental, cancer, other mortality. The predicted values for 2020 are based on 5-year trend probabilities. The 95% Confidence Interval of the prediction is given in parentheses. Population is counted on January 1st of 2020.

Table B2: Summary statistics for men aged 65-79 by income group

Income Group	All	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Total Mortality	0.023	0.046	0.039	0.035	0.032	0.034	0.028	0.028	0.025	0.025	0.022	0.023	0.021	0.020	0.019	0.020	0.019	0.018	0.017	0.016	0.015
Predicted Mortality	0.022	0.039	0.034	0.030	0.028	0.031	0.026	0.025	0.021	0.021	0.021	0.020	0.019	0.018	0.018	0.017	0.017	0.016	0.015	0.015	0.014
	(0.02; 0.024)	(0.038; 0.04)	(0.033; 0.035)	(0.029; 0.031)	(0.027; 0.029)	(0.03; 0.032)	(0.025; 0.027)	(0.024; 0.026)	(0.02; 0.022)	(0.02; 0.022)	(0.02; 0.022)	(0.019; 0.021)	(0.018; 0.02)	(0.017; 0.019)	(0.017; 0.019)	(0.016; 0.018)	(0.016; 0.018)	(0.015; 0.017)	(0.014; 0.016)	(0.014; 0.016)	(0.013; 0.015)
Covid Mortality	0.003	0.008	0.005	0.004	0.003	0.004	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.001
Circulatory Mortality	0.005	0.009	0.008	0.008	0.007	0.007	0.006	0.006	0.004	0.005	0.005	0.004	0.004	0.004	0.003	0.004	0.004	0.004	0.003	0.003	0.003
Predicted Mortality	0.005	0.009	0.008	0.007	0.007	0.007	0.006	0.005	0.005	0.004	0.004	0.004	0.004	0.004	0.004	0.003	0.003	0.003	0.003	0.003	0.003
	(0.004; 0.006)	(0.006; 0.012)	(0.005; 0.011)	(0.004; 0.01)	(0.004; 0.01)	(0.005; 0.01)	(0.003; 0.009)	(0.003; 0.008)	(0.002; 0.008)	(0.002; 0.007)	(0.002; 0.007)	(0.001; 0.007)	(0.001; 0.007)	(0.001; 0.007)	(0.001; 0.006)	(0.001; 0.006)	(0.001; 0.006)	(0; 0.006)	(0; 0.006)	(0; 0.006)	(0; 0.006)
Respiratory Mortality	0.002	0.004	0.003	0.003	0.002	0.003	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
Predicted Mortality	0.002	0.004	0.003	0.003	0.003	0.003	0.002	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(-0.001; 0.004)	(0; 0.008)	(-0.001; 0.007)	(-0.001; 0.007)	(-0.001; 0.007)	(-0.001; 0.007)	(-0.002; 0.006)	(-0.002; 0.006)	(-0.002; 0.006)	(-0.002; 0.006)	(-0.002; 0.006)	(-0.003; 0.006)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.003; 0.005)	(-0.004; 0.005)
Mental Mortality	0.001	0.002	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.000	0.000	0.000	0.000
Predicted Mortality	0.001	0.003	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.001	0.000	0.001	0.000
	(0; 0.002)	(0.001; 0.005)	(0; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.002)	(-0.001; 0.003)	(-0.001; 0.002)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.003)	(-0.001; 0.002)	(-0.001; 0.002)	(-0.002; 0.002)	(-0.001; 0.002)	(-0.001; 0.002)
Cancer Mortality	0.010	0.011	0.013	0.012	0.012	0.012	0.011	0.011	0.011	0.010	0.009	0.009	0.009	0.009	0.009	0.008	0.008	0.007	0.007	0.007	0.006
Predicted Mortality	0.010	0.011	0.013	0.012	0.011	0.013	0.011	0.011	0.010	0.010	0.010	0.009	0.009	0.008	0.008	0.008	0.008	0.008	0.008	0.008	0.006
	(0.008; 0.012)	(0.008; 0.013)	(0.011; 0.015)	(0.009; 0.014)	(0.009; 0.014)	(0.011; 0.015)	(0.009; 0.014)	(0.009; 0.014)	(0.007; 0.012)	(0.007; 0.012)	(0.008; 0.012)	(0.007; 0.012)	(0.007; 0.011)	(0.006; 0.011)	(0.006; 0.011)	(0.005; 0.01)	(0.006; 0.01)	(0.006; 0.011)	(0.005; 0.01)	(0.005; 0.01)	(0.004; 0.009)
Other Mortality	0.005	0.012	0.008	0.007	0.006	0.007	0.005	0.005	0.004	0.005	0.004	0.004	0.004	0.004	0.004	0.005	0.004	0.003	0.004	0.003	0.003
Predicted Mortality	0.005	0.012	0.008	0.007	0.006	0.006	0.005	0.005	0.004	0.005	0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.003	0.004	0.003
	(0.003; 0.007)	(0.01; 0.014)	(0.006; 0.01)	(0.005; 0.009)	(0.005; 0.008)	(0.004; 0.008)	(0.004; 0.007)	(0.003; 0.007)	(0.003; 0.006)	(0.003; 0.006)	(0.002; 0.006)	(0.003; 0.006)	(0.002; 0.006)	(0.002; 0.005)	(0.003; 0.006)	(0.002; 0.006)	(0.002; 0.006)	(0.002; 0.005)	(0.002; 0.005)	(0.002; 0.005)	(0.002; 0.005)
Population	1235379	61777	61765	61778	61799	61747	61754	61771	61794	61737	61809	61743	61757	61783	61770	61790	61746	61760	61764	61768	61767

Notes: This table shows the summary statistics of the mortality probabilities by cause of death for males aged 65-79 by 20 income groups. The causes of death are divided into 6 categories: COVID-19, circulatory, respiratory, mental, cancer, other mortality. The predicted values for 2020 are based on 5-year trend probabilities. The 95% Confidence Interval of the prediction is given in parentheses. Population is counted on January 1st of 2020.

Table B3: Summary statistics for women aged 80+ by income group

Income Group	All	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Total Mortality	0.098	0.161	0.154	0.128	0.117	0.108	0.105	0.103	0.096	0.091	0.093	0.095	0.095	0.093	0.092	0.090	0.088	0.088	0.088	0.080	0.093
Predicted Mortality	0.095	0.139	0.147	0.122	0.108	0.100	0.096	0.094	0.090	0.084	0.083	0.084	0.088	0.087	0.084	0.087	0.085	0.080	0.079	0.082	0.084
	(0.093; 0.097)	(0.138; 0.140)	(0.146; 0.148)	(0.121; 0.123)	(0.107; 0.109)	(0.099; 0.101)	(0.095; 0.097)	(0.093; 0.095)	(0.089; 0.091)	(0.083; 0.085)	(0.082; 0.084)	(0.083; 0.085)	(0.087; 0.089)	(0.086; 0.088)	(0.083; 0.085)	(0.086; 0.088)	(0.084; 0.086)	(0.079; 0.081)	(0.078; 0.080)	(0.081; 0.083)	(0.083; 0.085)
Covid Mortality	0.014	0.027	0.024	0.020	0.016	0.015	0.014	0.013	0.012	0.011	0.012	0.012	0.012	0.013	0.011	0.011	0.011	0.010	0.009	0.009	0.011
Circulatory Mortality	0.029	0.037	0.035	0.031	0.030	0.028	0.027	0.028	0.027	0.025	0.025	0.026	0.026	0.025	0.024	0.025	0.024	0.023	0.024	0.021	0.023
Predicted Mortality	0.027	0.037	0.037	0.033	0.030	0.031	0.028	0.027	0.027	0.025	0.025	0.027	0.025	0.025	0.025	0.025	0.025	0.023	0.026	0.022	0.024
	(0.027; 0.028)	(0.034; 0.040)	(0.034; 0.040)	(0.030; 0.035)	(0.027; 0.033)	(0.029; 0.034)	(0.025; 0.031)	(0.024; 0.030)	(0.024; 0.029)	(0.022; 0.028)	(0.022; 0.028)	(0.024; 0.030)	(0.023; 0.028)	(0.023; 0.028)	(0.022; 0.028)	(0.022; 0.028)	(0.022; 0.028)	(0.021; 0.026)	(0.023; 0.028)	(0.019; 0.025)	(0.021; 0.026)
Respiratory Mortality	0.009	0.009	0.010	0.008	0.007	0.007	0.007	0.006	0.006	0.005	0.006	0.005	0.006	0.006	0.005	0.005	0.004	0.005	0.004	0.004	0.005
Predicted Mortality	0.009	0.013	0.015	0.011	0.012	0.009	0.009	0.009	0.008	0.008	0.008	0.007	0.008	0.008	0.006	0.008	0.007	0.007	0.006	0.006	0.006
	(0.006; 0.011)	(0.009; 0.017)	(0.011; 0.019)	(0.007; 0.015)	(0.008; 0.016)	(0.005; 0.013)	(0.005; 0.013)	(0.005; 0.014)	(0.004; 0.013)	(0.004; 0.012)	(0.004; 0.012)	(0.003; 0.011)	(0.004; 0.012)	(0.004; 0.012)	(0.002; 0.010)	(0.004; 0.012)	(0.003; 0.011)	(0.003; 0.011)	(0.002; 0.010)	(0.002; 0.010)	(0.002; 0.010)
Mental Mortality	0.014	0.025	0.023	0.017	0.014	0.012	0.011	0.011	0.008	0.009	0.009	0.010	0.009	0.009	0.012	0.010	0.010	0.011	0.010	0.009	0.012
Predicted Mortality	0.014	0.029	0.030	0.021	0.015	0.014	0.014	0.012	0.011	0.009	0.009	0.010	0.012	0.011	0.012	0.013	0.012	0.012	0.009	0.013	0.014
	(0.013; 0.015)	(0.027; 0.030)	(0.028; 0.031)	(0.020; 0.023)	(0.013; 0.017)	(0.012; 0.016)	(0.012; 0.016)	(0.010; 0.014)	(0.009; 0.013)	(0.007; 0.011)	(0.007; 0.011)	(0.008; 0.012)	(0.010; 0.013)	(0.009; 0.013)	(0.010; 0.014)	(0.011; 0.015)	(0.010; 0.014)	(0.010; 0.014)	(0.008; 0.011)	(0.011; 0.015)	(0.013; 0.016)
Cancer Mortality	0.015	0.015	0.015	0.015	0.016	0.016	0.016	0.016	0.017	0.017	0.016	0.015	0.015	0.015	0.014	0.014	0.014	0.015	0.014	0.013	0.015
Predicted Mortality	0.015	0.014	0.016	0.017	0.017	0.016	0.015	0.017	0.017	0.016	0.016	0.015	0.016	0.015	0.015	0.014	0.013	0.013	0.014	0.014	0.012
	(0.013; 0.017)	(0.011; 0.016)	(0.013; 0.018)	(0.015; 0.019)	(0.015; 0.019)	(0.014; 0.018)	(0.013; 0.018)	(0.015; 0.019)	(0.014; 0.019)	(0.013; 0.018)	(0.013; 0.018)	(0.013; 0.017)	(0.013; 0.018)	(0.013; 0.018)	(0.012; 0.017)	(0.012; 0.016)	(0.011; 0.016)	(0.011; 0.015)	(0.011; 0.016)	(0.012; 0.017)	(0.010; 0.015)
Other Mortality	0.030	0.048	0.046	0.038	0.034	0.031	0.030	0.030	0.026	0.025	0.026	0.026	0.028	0.027	0.026	0.026	0.026	0.025	0.023	0.029	0.030
Predicted Mortality	0.030	0.047	0.049	0.040	0.035	0.029	0.030	0.028	0.027	0.027	0.025	0.025	0.027	0.027	0.026	0.028	0.028	0.025	0.025	0.027	0.028
	(0.029; 0.032)	(0.046; 0.049)	(0.047; 0.051)	(0.038; 0.042)	(0.033; 0.036)	(0.028; 0.031)	(0.028; 0.031)	(0.027; 0.030)	(0.026; 0.029)	(0.025; 0.028)	(0.024; 0.027)	(0.023; 0.027)	(0.025; 0.029)	(0.025; 0.029)	(0.025; 0.028)	(0.026; 0.029)	(0.027; 0.030)	(0.023; 0.027)	(0.023; 0.026)	(0.025; 0.029)	(0.026; 0.030)
Population	504593	25233	25233	25236	25219	25232	25263	25218	25231	25228	25214	25236	25213	25248	25227	25224	25223	25227	25230	25230	25228

Notes: This table shows the summary statistics of the mortality probabilities by cause of death for females aged 80+ by 20 income groups. The causes of death are divided into 6 categories: COVID-19, circulatory, respiratory, mental, cancer, other mortality. The predicted values for 2020 are based on 5-year trend probabilities. The 95% Confidence Interval of the prediction is given in parentheses. Population is counted on January 1st of 2020.

Table B4: Summary statistics for men aged 80+ by income group

Income Group	All	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Total Mortality	0.110	0.166	0.148	0.143	0.129	0.127	0.124	0.124	0.125	0.119	0.118	0.116	0.113	0.116	0.110	0.110	0.104	0.098	0.097	0.100	0.096
Predicted Mortality	0.106	0.144	0.131	0.122	0.115	0.111	0.109	0.113	0.108	0.108	0.107	0.102	0.103	0.101	0.100	0.099	0.091	0.095	0.087	0.086	0.089
	(0.104; 0.108)	(0.143; 0.145)	(0.130; 0.132)	(0.121; 0.123)	(0.114; 0.116)	(0.110; 0.112)	(0.108; 0.110)	(0.112; 0.114)	(0.107; 0.109)	(0.107; 0.109)	(0.106; 0.108)	(0.101; 0.103)	(0.102; 0.104)	(0.100; 0.102)	(0.099; 0.100)	(0.098; 0.100)	(0.090; 0.092)	(0.094; 0.096)	(0.086; 0.088)	(0.085; 0.087)	(0.088; 0.090)
Covid Mortality	0.019	0.032	0.028	0.027	0.021	0.021	0.020	0.021	0.021	0.020	0.021	0.017	0.017	0.015	0.015	0.016	0.014	0.015	0.014	0.013	0.013
Circulatory Mortality	0.032	0.039	0.035	0.031	0.031	0.031	0.031	0.030	0.031	0.032	0.028	0.029	0.029	0.030	0.030	0.027	0.027	0.024	0.023	0.023	0.024
Predicted Mortality	0.029	0.036	0.035	0.033	0.032	0.031	0.033	0.031	0.032	0.031	0.033	0.028	0.027	0.030	0.028	0.028	0.024	0.024	0.024	0.026	0.024
	(0.029; 0.03)	(0.033; 0.039)	(0.032; 0.038)	(0.03; 0.036)	(0.029; 0.035)	(0.028; 0.033)	(0.03; 0.035)	(0.028; 0.034)	(0.029; 0.035)	(0.028; 0.033)	(0.03; 0.036)	(0.025; 0.031)	(0.024; 0.03)	(0.027; 0.032)	(0.025; 0.03)	(0.025; 0.031)	(0.021; 0.027)	(0.021; 0.027)	(0.021; 0.027)	(0.023; 0.029)	(0.022; 0.027)
Respiratory Mortality	0.012	0.014	0.013	0.012	0.012	0.010	0.009	0.009	0.010	0.009	0.008	0.010	0.008	0.008	0.007	0.008	0.008	0.008	0.006	0.007	0.006
Predicted Mortality	0.011	0.017	0.016	0.016	0.015	0.013	0.010	0.014	0.012	0.011	0.012	0.012	0.010	0.008	0.010	0.008	0.008	0.009	0.006	0.007	0.007
	(0.009; 0.013)	(0.013; 0.021)	(0.012; 0.02)	(0.011; 0.02)	(0.01; 0.019)	(0.009; 0.017)	(0.006; 0.014)	(0.01; 0.018)	(0.008; 0.016)	(0.007; 0.015)	(0.008; 0.016)	(0.008; 0.016)	(0.006; 0.014)	(0.004; 0.013)	(0.006; 0.014)	(0.004; 0.012)	(0.004; 0.012)	(0.005; 0.014)	(0.002; 0.01)	(0.003; 0.011)	(0.003; 0.011)
Mental Mortality	0.011	0.018	0.012	0.011	0.011	0.009	0.009	0.009	0.008	0.008	0.009	0.009	0.008	0.009	0.008	0.008	0.009	0.007	0.007	0.008	0.007
Predicted Mortality	0.011	0.021	0.016	0.014	0.012	0.010	0.011	0.012	0.010	0.009	0.008	0.009	0.011	0.011	0.009	0.010	0.009	0.009	0.008	0.008	0.010
	(0.01; 0.011)	(0.019; 0.023)	(0.014; 0.018)	(0.012; 0.016)	(0.01; 0.014)	(0.008; 0.012)	(0.009; 0.012)	(0.01; 0.013)	(0.008; 0.012)	(0.007; 0.011)	(0.006; 0.01)	(0.007; 0.011)	(0.009; 0.013)	(0.009; 0.013)	(0.007; 0.011)	(0.008; 0.012)	(0.007; 0.011)	(0.007; 0.011)	(0.006; 0.01)	(0.006; 0.01)	(0.008; 0.012)
Cancer Mortality	0.027	0.023	0.025	0.031	0.027	0.029	0.027	0.027	0.029	0.024	0.027	0.027	0.024	0.027	0.024	0.026	0.023	0.021	0.024	0.022	0.023
Predicted Mortality	0.026	0.024	0.027	0.028	0.028	0.027	0.029	0.029	0.027	0.028	0.026	0.026	0.029	0.025	0.025	0.027	0.024	0.025	0.022	0.022	0.023
	(0.024; 0.028)	(0.022; 0.027)	(0.025; 0.029)	(0.025; 0.03)	(0.026; 0.03)	(0.025; 0.03)	(0.026; 0.031)	(0.027; 0.032)	(0.025; 0.029)	(0.026; 0.031)	(0.024; 0.029)	(0.023; 0.028)	(0.026; 0.031)	(0.023; 0.028)	(0.022; 0.027)	(0.025; 0.029)	(0.021; 0.026)	(0.023; 0.027)	(0.019; 0.024)	(0.021; 0.026)	(0.021; 0.025)
Other Mortality	0.028	0.040	0.035	0.032	0.027	0.027	0.028	0.029	0.026	0.026	0.026	0.025	0.028	0.027	0.026	0.026	0.024	0.024	0.023	0.026	0.023
Predicted Mortality	0.029	0.045	0.037	0.032	0.029	0.031	0.027	0.028	0.027	0.029	0.027	0.026	0.027	0.027	0.028	0.026	0.027	0.028	0.028	0.021	0.025
	(0.027; 0.03)	(0.043; 0.046)	(0.036; 0.039)	(0.03; 0.034)	(0.027; 0.031)	(0.029; 0.033)	(0.026; 0.029)	(0.026; 0.03)	(0.025; 0.029)	(0.028; 0.031)	(0.025; 0.029)	(0.025; 0.028)	(0.025; 0.028)	(0.025; 0.028)	(0.025; 0.029)	(0.027; 0.03)	(0.024; 0.028)	(0.025; 0.028)	(0.026; 0.029)	(0.027; 0.03)	(0.019; 0.023)
Population	324219	16212	16218	16208	16219	16211	16207	16218	16208	16208	16207	16210	16215	16214	16200	16216	16205	16212	16213	16208	16210

Notes: This table shows the summary statistics of the mortality probabilities by cause of death for males aged 80+ by 20 income groups. The causes of death are divided into 6 categories: COVID-19, circulatory, respiratory, mental, cancer, other mortality. The predicted values for 2020 are based on 5-year trend probabilities. The 95% Confidence Interval of the prediction is given in parentheses. Population is counted on January 1st of 2020.

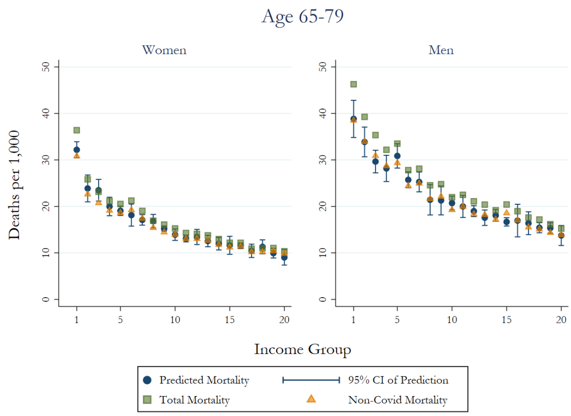
Table B5: Decomposition of excess mortality by cause of death

	Women		Men	
	65-79	80+	65-79	80+
CI Total Mortality	-0.192	-0.0966	-0.176	-0.0792
CI Predicted Mortality	-0.189	-0.0911	-0.165	-0.0723
CI Observed Excess Mortality	-0.238	-0.164	-0.256	-0.135
Std. Error	0.106	0.0639	0.0506	0.031
Weight Excess Mortality	0.0698	0.076	0.115	0.11
Contribution to CI Total Mortality	-0.0166	-0.0124	-0.0293	-0.0149
COVID-19				
Weight Observed	0.095	0.131	0.121	0.159
CI Observed mortality	-0.326	-0.164	-0.236	-0.143
Cause-specific Contribution to CI	-0.031	-0.022	-0.028	-0.023
Percentage contribution	187%	173%	97%	152%
Circulatory diseases				
Weight Predicted	0.17	0.266	0.196	0.247
CI Predicted Mortality	-0.232	-0.0749	-0.209	-0.0672
Weight Observed	0.173	0.259	0.205	0.245
CI Observed Mortality	-0.224	-0.0754	-0.187	-0.0673
Cause-specific Contribution to CI	0.000553	0.000404	0.00273	0.0000997
Percentage contribution	-3%	-3%	-9%	-1%
Respiratory diseases				
Weight Predicted	0.0889	0.0829	0.0714	0.0924
CI Prediction Mortality	-0.334	-0.146	-0.312	-0.154
Weight Observed	0.071	0.058	0.0624	0.0764
CI Observed Mortality	-0.323	-0.141	-0.323	-0.128
Cause-specific Contribution to CI	0.00681	0.00396	0.00212	0.00441
Percentage contribution	-41%	-32%	-7%	-30%
Mental Disorder Mortality				
Weight Predicted	0.0458	0.136	0.0352	0.0906
CI Predicted Mortality	-0.278	-0.142	-0.259	-0.117
Weight Observed	0.0403	0.118	0.0297	0.0771
CI Observed Mortality	-0.263	-0.134	-0.294	-0.106
Cause-specific Contribution to CI	0.00211	0.00355	0.000377	0.00237
Percentage contribution	-13%	-29%	-1%	-16%
Cancer Mortality				
Weight Predicted	0.412	0.146	0.382	0.219
CI Prediction Mortality	-0.107	-0.034	-0.0987	-0.0297
Weight Observed	0.413	0.146	0.38	0.213
CI Observed Mortality	-0.103	-0.0253	-0.109	-0.0351
Cause-specific Contribution to CI	0.00149	0.00128	-0.00364	-0.000973
Percentage contribution	-9%	-10%	12%	7%
Other Mortality				
Weight Predicted	21%	29%	0.201	0.241
CI Prediction Mortality	-0.233	-0.0951	-0.18	-0.0681
Weight Observed	0.207	0.289	0.203	0.229
CI Observed Mortality	-0.224	-0.0968	-0.191	-0.0635
Cause-specific Contribution to CI	0.00346	-0.000133	-0.00251	0.0019
Percentage contribution	-21%	1%	9%	-13%

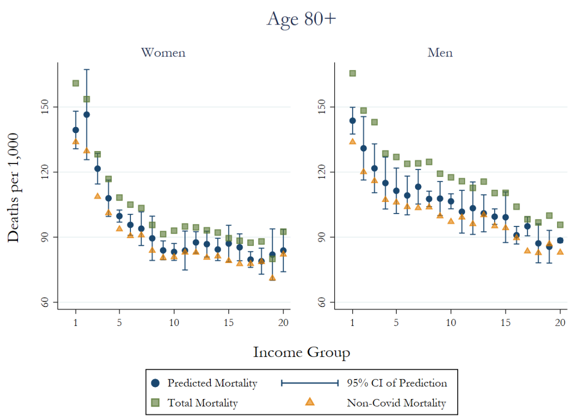
Notes: This table is the full Table 3.1. It shows the decomposition of total mortality inequality in 2020 into causes of death categories. Mortality inequality is measured using the Concentration Index. Deaths are divided into six causes: COVID-19, circulatory deaths, deaths from mental disorders, cancer deaths, respiratory disease deaths and other causes. The Concentration Index is calculated for each cause of death and its predicted probability. The contribution to total excess mortality is shown as the cause-specific contribution.

Appendix Figures

Figure B2: Total mortality, predicted mortality (with 95% confidence intervals) and non-COVID-19 mortality by 20 income ventiles, by four age-sex groups



(a)



(b)

Note: This Figure depicts the mortality income gradient in 2020 for total, predicted, and non-Covid mortality over 20 income groups in the Netherlands, with the y-axis symbolizing deaths per 1000 and the x-axis symbolizing the income group. Total mortality, represented by a square, is the mortality observed in 2020. Predicted mortality is calculated by age-sex-income group using 5-year trend probabilities and is shown as a circle. Non-Covid mortality is all mortality attributable to causes of death that are not COVID-19, represented by a triangle. Total mortality is above predicted mortality for 2020, while non-Covid mortality is below what would have been expected for 2020.

C Appendix - Estimating the health value added by nursing homes

Data sources
We use administrative data provided by Statistics Netherlands encompassing the full Dutch population. The data on individuals' nursing home care use and care needs, determined by the independent eligibility assessor, comes from the Dutch Central Administrative Office (CAK). We combine the anonymized provider codes and admission dates included in the nursing home use data with data on addresses from the mandatory municipal registry to link individuals to their chosen nursing home.

We obtain mortality data from the municipal registries, which includes the exact date of death. Data for our other outcome variable on hospital visits comes from NZa (Nederlandse Zorgautoriteit) and includes both ICD-10 and DBC (in Dutch: Diagnose Behandeling Combinatie) codes.

We also obtain age and sex from the municipal registries, and supplement this with the following data sources to define our other covariates: medicine consumption by ATC-code from SGZ (Gezondheid en Zorg); income and wealth data from tax registries; and healthcare expenditures from insurance data provided by Vektis.

Monotonicity assumption
An IV estimate can only be interpreted as a local average treatment effect (LATE) under the monotonicity assumption. Because our (non-binary) instrument has multiple values, we can interpret λ as a weighted sum of multiple LATEs defined by different pairs of values of the instrument (Imbens and Wooldridge, 2009). We can only interpret λ as LATE if the value of the treatment variable ($\hat{\delta}_{ij}$) is increasing (or decreasing) with every increment in the value of the instrument, and if the monotonicity assumption holds for any two values of the instrument.

In our case, monotonicity means that if a person chooses a low mortality nursing home far away because the closest nursing home has slightly higher-than-average mortality – e.g. 1 percentage point above average –, this person should also choose a low mortality nursing home if the closest nursing home has even higher mortality — e.g. 2 percentage points above the average. The same person should also choose a low mortality nursing home if it was to be the closest one.

While we cannot test this directly, we can do the following: first, we plot the average performance estimate of the chosen nursing home (the endogenous treatment variable) against the instrument. Figures C2a and C2a shows that the endogenous performance estimates on average monotonically increase with the instrument. Second, we follow Frandsen et al. (2019) and run the first-stage regressions for a wide range of subgroups. We find positive and statistically significant coefficients for all of them. This is suggestive evidence in favor for the weak monotonicity assumption, which is sufficient for causal interpretation (Frandsen et al., 2019).

Appendix Tables

Table C1: Descriptive statistics nursing home quality indicators

Variable	Description	Mean	Std. dev.	N
Facility level				
Psychotropic medicine use	Share of clients using psychotropic medicine	0.370	0.166	705
Physical restraint use	Share of clients to whom physical restraints are applied	0.303	0.184	719
Pressure sores	Share of clients with pressure sores	0.042	0.041	693
Online rating	Online rating according to Zorgkaart Nederland (1-10)	7.761	0.826	709
Number of people on waiting list	Number of people actively or passively waiting per nursing home	0.092	0.119	540
Number of clients	Number of clients	100.263	54.119	540
Organisation level				
Number of facilities	Number of facilities belonging to organization (= 1 if no chain)	6.921	6.104	177
Operating profit margin	Operating profit divided by total revenue multiplied by 100	1.376	2.603	177
Solvability	Equity-to-asset ratio: equity divided by total revenue multiplied by 100	38.290	15.299	177
Liquidity	Current ratio: current assets divided by current liabilities multiplied by 100	177.882	112.499	177
FTE per client	Care-related FTE per nursing home client	0.697	0.174	177
Percentage high educated nurses	Percentage of employed nurses that is high educated (in terms of FTE)	0.331	0.239	177
Percentage specialists	Percentage of care-related FTE that is specialized staff (i.e. geriatricians, medical doctors, psychologists and nurse practitioners)	0.026	0.031	177
Staff turnover	Staff turnover measured by outflow divided by average staffing (in terms of FTE)	0.143	0.063	177
Staff absenteeism	Percentage staff absenteeism	0.063	0.011	177
Expenditures on external staff	Percentage of salary expenses on external staffing	0.066	0.045	177

This table lists the nursing home quality indicators used in Section 4.6 in this paper. It reports a more elaborate description of each of the indicators, the mean, standard deviation and number of unique observations. Some indicators are reported at the facility level, while others are reported at the organisation level.

Table C2: Avoidable hospitalization diagnoses

Diagnoses	Percent
Anaemia	2.50
Angina pectoris	2.24
Asthma & COPD	6.32
Dehydration	0.44
Diabetes	3.59
Falls & fractures	52.82
Gastroenteritis	1.51
Grand mal seizure disorders	2.37
Hypertension	0.98
Infections skin	2.66
Infections other	1.25
Kidney or urinary tract infections	4.14
Pneumonia	8.16
Rehabilitation	5.02
Sepsis	3.14
Wounds	2.85
Total	(n = 10,702) 100

This table lists the diagnoses of hospitalizations that we identify as potentially avoidable, which is based on Carter (2003); Walker et al. (2009).

Table C3: Hospitalization by avoidable and type of stay

	Avoidable hospitalization		
Type	No (%)	Yes (%)	Total (%)
Outpatient stay	78.2	63.6	72.9
Inpatient stay - one-day	4.0	2.3	3.4
Inpatient stay - overnight	17.8	34.0	23.7
Total	(n = 18,968)	(n = 10,702)	(n = 29,670)

This table reports whether the hospitalization within 180 days after nursing home admission was an outpatient, a one-day inpatient stay or an overnight inpatient stay. It includes individuals who had at least one hospitalization within 180 days after nursing home admission and separates by those of which at least one of them was potentially avoidable or not.

Variable	Explanation	Data source(s)
	Health outcomes Y_{ij}	
Mortality	Binary outcome variable that equals one if resident i died within 180 days after nursing home admission.	Data on the date of death is obtained from (municipal) death registries.
Avoidable hospitalization	Binary outcome variable that equals one if resident i had a potentially avoidable hospitalization. Diagnoses for which a hospitalization is perceived as potentially avoidable are listed in Table C2.	NZa (NederlandseZorgautoriteit) and including both ICD-10 and DBC (in Dutch: Diagnose Behandeling Combinatie) codes
	Case-mix controls X_i	
Women	Equals one if resident i is a woman	(Mandatory) municipal registries
Age	Age of at admission	Date of birth from (Mandatory) municipal registries
Care intensity	A care intensity package (in Dutch: Zorgzwaartepakket (ZZP)) is a proxy for the intensity of nursing home care that the recipient needs according to an independent care assessor from the Care Assessment Centre (CIZ). Residents with lower care (ZZP 4) intensity need intensive support and extensive care, with dementia care (ZZP 5) need a protective living facility with intensive dementia care, with higher care intensity need a protective living facility with intensive support and care, with the highest care intensity (ZZP 7 and 8) need a protective living facility with very intensive care and treatment or support (CIZ, nd).	Centraal Administratie Kantoor (CAK)
Healthcare expenditures	Yearly healthcare expenditures in logarithmic form from the calendar year before admission, obtained from claims data.	Insurance claims data provided by Vektis
Wealth	Wealth from assets and savings (excluding home equity) of the household in the calendar year prior to admission. Variable enters the control function in logarithmic form.	Tax office administration
Standardized household income	Yearly disposable household income in the calendar year before admission from tax registries, standardized for household size. Variable enters the control function in logarithmic form.	Tax office administration
Number of medicine	Is equal to the number of different types of medicine resident i received in the calendar year before admission. Types are distinguished based on ATC3 codes.	4-digit ATC-code consumption (either Yes/No) from Zorginstituut Nederland - SGZ (Gezondheid en Zorg)
		Continued on next page

Table C4 – continued from previous page

Variable	**Explanation**	Data source(s)
Charlson score	The Charlson comorbidity index is a weighted score based on 12 different comorbidities: congestive heart failure, dementia, pulmonary disease, connective tissue disorder, liver disease, diabetes, diabetes complications, paraplegia, renal disease, cancer, metastatic cancer, severe liver disease and HIV (Quan et al., 2011). Diagnoses are obtained from hospital visits in the 365 days before nursing home admission.	Date of admission and discharge and diagnose from Dutch Hospital Data
Hospital in last month	An indicator for whether the individual had an hospitalization within the last 30 days before the individual were admitted to a nursing home.	Date of admission and discharge from Dutch Hospital Data
Rural	This binary variable indicates whether the resident's prior home was in a rural (equals 1) or urban (equals 0) municipality. We categorize a municipality as urban if the area is at least moderately urbanised ($> 1{,}000$ addresses per square kilometer) (Statistics Netherlands, nd).	(Mandatory) municipal registries
Year	Indicates in which year the individual is admitted to a nursing home.	Centraal Administratie Kantoor (CAK)

Table C4: Variable specifications

Table C5: Subgroup analysis

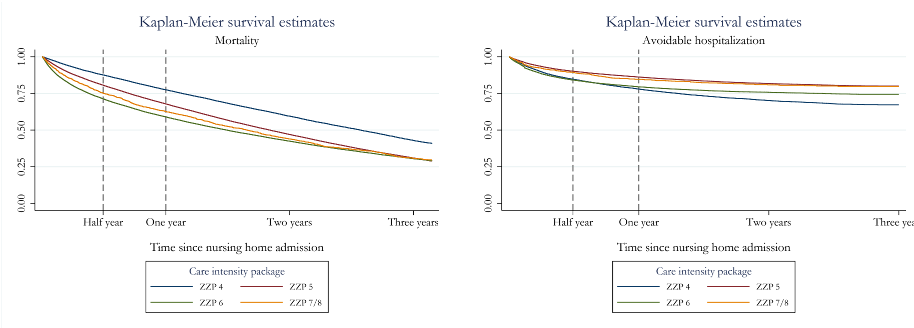
	Outcome: Mortality			
	By care intensity			
	Lower	Dementia	Higher	Highest
Performance chosen NH ($\hat{\delta}_{ij}$)				
Coefficient	0.774***	0.991***	1.135***	0.669**
	(0.053)	(0.038)	(0.055)	(0.279)
Constant	-0.461***	-0.521***	-0.310***	-0.935**
	(0.068)	(0.052)	(0.062)	(0.392)
Controls	Yes	Yes	Yes	Yes
R-squared	0.065	0.066	0.093	0.078
N	17,923	46,144	29,703	1,135
	Outcome: Avoidable hospitalization			
	By care intensity			
	Lower	Dementia	Higher	Highest
Performance chosen NH ($\hat{\delta}_{ij}$)				
Coefficient	1.004***	0.850***	1.155***	1.200***
	(0.053)	(0.033)	(0.043)	(0.216)
Constant	0.135	-0.061	0.429***	0.353
	(0.089)	(0.049)	(0.058)	(0.345)
Controls	Yes	Yes	Yes	Yes
R-squared	0.054	0.029	0.057	0.044
N	11,811	29,869	22,693	892

For this exercise we distinguish between four groups: (1) clients with a lower level of care intensity; (2) clients with a higher level of care intensity; (3) clients with the highest level of care intensity; and (4) clients with dementia care needs. These are identified by the care intensity package, as explained in Footnote [6] and Appendix Table C4. For each of these groups, we regress the individual level outcome – mortality or avoidable hospitalization – on the observed performance score of the nursing home to which the individual is admitted ($\hat{\delta}_{ij}$).

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

Appendix Figures

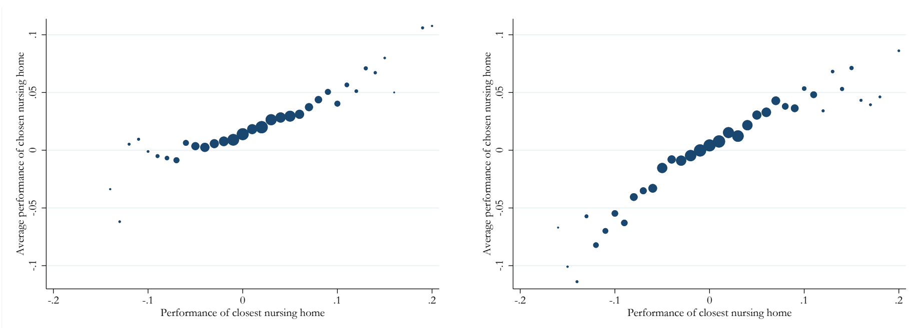
Figure C1: Kaplan-Meier survival curves by care intensity



(a) Time to death **(b)** Time without avoidable hospitalization

Notes: This figure shows Kaplan-Meier survival curves for time to death (mortality) and time to an avoidable hospitalization by care intensity package. Time to death for those who enter the nursing home with a higher care intensity is, on average, shorter than for those with a lower care intensity. On the other hand, those who have relatively high or dementia care needs, stay longer without having an avoidable hospitalization. This can partly be explained by this group dying earlier.

Figure C2: Non-parametric relationship instrument and endogenous variable



(a) Mortality **(b)** Avoidable hospitalization

Notes: This figure (nonparametrically) shows the relationship between performance of the chosen nursing home (i.e. the endogenous variable) and performance of the closest nursing home (i.e. the instrumental variable) for both outcomes: mortality (a) and avoidable hospitalization (b). The size of the data points reflect the group size on which the average is based.

E Appendix - Do delayed admissions to nursing homes increase hospital use?

Assessing the validity of the instrument
As discussed in Section 6.5.2, to assess the validity of our instrument we evaluate how fluctuations in average delays (congestion) vary with two other variables that represent threats to the exclusion or independence assumption. First, to examine if congestion would affect hospitalisations directly we look at the number of all hospitalisations among the full population of 65 years and older. Second, to evaluate whether the composition of individuals in terms of their care needs changes with congestion, we look at the percentage of applicants for long-term care who receive a negative eligibility assessments, which indicates lower care needs.

We first calculate the average delays by region, care profile and month-by-year. Then we remove the between care profile, region and year variation by using the residuals of a regression of the average delays on care profile, region and year fixed effects. This approach comes close to our identification strategy as our model specification includes care profile, region and year fixed effects. We do the same with the between region and year variation for the other two variables and correlate these to the previously obtained monthly variation in delays. The correlation coefficients are reported in Column 1 of Table E1. We repeat this process by care profile and report the coefficients in Column 2-4.

The results show that the monthly regional variation in delays is not strongly related to the variation in the number of urgent hospitalisations among all 65+ year-olds. The shocks that affect congestion in the nursing home market are therefore not expected to directly affect hospital care utilisation in general (exclusion restriction). Additionally, the weak correlations between monthly regional delays and negative eligibility assessments suggests that the individuals who apply for eligibility in times of high congestion are not systematically different in terms of their care needs (as observed by the independent care needs assessor) compared to those who applied when there is low congestion (independence).

Table E1: Correlations between monthly delays, urgent hospitalisations of all 65+ year-olds and negative eligibility assessments

	Monthly variation in delays per region, excluding care profile, region and year fixed effects			
	Full sample	By care profile		
		Moderate dementia care needs	Moderate somatic care needs	High care needs
	(1)	(2)	(3)	(4)
Monthly variation per region, excluding care profile, region and year fixed effects:				
Urgent hospitalisations 65+ all year-olds (N)	-0.0771	-0.0517	-0.1236	-0.0645
Negative eligibility assessment results (%)	0.0020	0.0165	0.0070	-0.0173
Observations	4185	1395	1395	1395

Table reports correlation coefficients between the number of urgent hospitalisations among the full population of 65+ year-olds and the share of negative eligibility assessment results.
The number of observations is equal to the number of regions (31 health care office regions) × number of periods (45 months-by-years) × care profiles (3 profiles).

Appendix Tables

Table E2: Care profile definitions

<table>
<tr><td rowspan="2"></td><td rowspan="2"></td><td colspan="2">Dementia or related condition</td></tr>
<tr><td>No</td><td>Yes</td></tr>
<tr><td rowspan="2">Care needs</td><td>Moderate</td><td>Moderate somatic care needs</td><td>Moderate dementia care needs</td></tr>
<tr><td>High</td><td colspan="2">High care needs</td></tr>
</table>

Table E3: Sample restrictions

	Sample size before exclusion	Number of individuals excluded	Percentage excluded (%)
Used personal budget to purchase care	150,900	2,551	1.7
Aged younger than 65 years	148,349	4,011	2.7
Delayed by more than one year	144,338	8,811	6.1
Received eligibility in hospital	135,527	13,375	9.9
Died within one year after eligibility	122,152	35,196	28.8
Moved out of nursing home within one year after eligibility	86,956	12,738	14.6
Missing information on covariates	74,218	1,456	2.0
Final sample size	72,762		

Variable	Explanation	Data source(s)
(Main) outcome variable		
Urgent hospitalization	Binary indicator that equals one if individual had an urgent hospitalization within one year after the start of eligibility. Hospitalizations are assessed as urgent if the hospitalization should be realized within 24 hours after the judgment of the physician.	Date of admission and discharge, diagnose and identifier for urgency from Dutch Hospital Data
Variable of interest		
Delay of the nursing home admission	Number of days between the start of eligibility and the start of the first nursing home admission	Start date of eligibility from Centrum Indicatiestelling Zorg (CIZ); Start and end date of LTC use, type and intensity of LTC use from Centraal Administratie Kantoor (CAK)
Covariates		
Care profile	A care profile (in Dutch: Zorgzwaartepakket (ZZP)) is a proxy for the intensity of nursing home care that the recipient needs according to an independent care assessor from the Care Assessment Centre (CIZ). We define three categories: residents with moderate dementia care needs (ZZP 5); with moderate somatic care needs (ZZP 4) ; and with high care needs (ZZP 6-8)	Care profile at start eligibility from Centrum Indicatiestelling Zorg (CIZ); and Care intensity at start of nursing home admission from Centraal Administratie Kantoor (CAK)
Woman	Equals one if resident *i* is a woman	(Mandatory) municipal registries
Age	Age at eligibility. In the analyses transformed to five-year age groups.	Date of birth from (mandatory) municipal registries; and date of eligibility from Centrum Indicatiestelling Zorg (CIZ)
Healthcare expenditures	Yearly healthcare expenditures in €1000 (within the basic insurance package) either on GP care or on hospital care in the calendar year prior to eligibility	From Health Insurers, facilitated by Vektis
Hospitalization in last 30 days	Binary indicator that equals one if individual was hospitalized in 30 days prior to eligibility	Date of admission and discharge from Dutch Hospital Data
Medicine consumption	(Prescribed) medicine consumed within the standard insurance package during the calendar year prior to the year of eligibility per ATC-code (3 digits). 18 relevant ATC-codes to include in analyses are selected using Lasso plugin estimators	4-digit ATC-code consumption (either Yes/No) from Zorginstituut Nederland
Charlson co-morbidities	17 dummies for co-morbidities that are generally used to calculate a Charlson comorbidity score (Sundararajan et al., 2004). Co-morbidities are identified using information on all hospitalizations in the year prior to eligibility	Date of admission and discharge and diagnose from Dutch Hospital Data

Continued on next page

Table E4 – continued from previous page

Variable	**Explanation**	**Data source(s)**
Household wealth	Total wealth of household, excluding the value of own property and mortgage, in the calendar year prior to eligibility, categorized: <5 thousand €; 5-20 thousand €; 20-50 thousand €; >50 thousand €	Tax office administration
Home ownership	Equals one if resident *i* owned a house at the end of the calendar year prior to eligibility	Tax office administration
Eligibility in flu season	Received eligibility status during the flu season, using flu starting week and period identified by Nivel (nd).	Start date of eligibility from Centrum Indicatiestelling Zorg (CIZ).

Table E4: Definitions of included variables and its source

Table E5: Most common diagnoses of urgent hospitalisations

	Full sample	By care profile		
		Moderate dementia care needs	Moderate somatic care needs	High care needs
ICD-10 block (World Health Organization, 2016)	(1)	(2)	(3)	(4)
Injuries to the hip and thigh	15.1	24.6	7.6	8.6
Other forms of heart disease (incl. heart failure)	8.9	5.7	13.1	8.7
Influenza and pneumonia	8.0	7.2	8.6	8.7
Other diseases of the urinary system	7.0	6.2	5.3	10.5
Cerebrovascular diseases (incl. stroke)	4.6	4.6	5.1	4.0
Chronic lower respiratory diseases	4.1	2.2	5.8	4.9
Injuries to the head	3.3	3.9	3.3	1.9
Symptoms and signs involving the circulatory and respiratory systems	3.2	2.2	4.9	2.8
General symptoms and signs	3.0	3.2	3.3	2.2
Episodic and paroxysmal disorders	2.6	2.0	2.7	3.3
Other diseases of intestines	2.4	2.0	2.2	3.3
Ischemic heart diseases	2.1	1.5	3.3	1.7
Complications of surgical and medical care, not elsewhere classified	1.8	1.3	1.7	2.7
Other bacterial diseases	1.7	1.5	1.3	2.6
Disorders of gallbladder, biliary tract and pancreas	1.7	1.4	1.8	1.9
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	1.4	2.1	1.1	0.7
Mental disorders due to known physiological conditions	1.4	2.1	1.1	0.7
Other diseases of the digestive system	1.4	1.3	1.5	1.6
Nutritional anemias	1.3	1.2	1.3	1.4
Metabolic disorders	1.3	1.1	1.4	1.3

Table reports share of twenty most common diagnoses for the full sample, and by care profile. When individuals had multiple hospitalisations, only the first main diagnose is used to construct this table.

Table E6: Ordinary least squares results

		By care profile		
	Full sample	Moderate dementia care needs	Moderate somatic care needs	High care needs
	(1)	(2)	(3)	(4)
Panel A: Ordinary least squares (including covariates):				
Delay (in days)	0.00036***	0.00037***	0.00021***	0.00056***
	(0.00002)	(0.00002)	(0.00003)	(0.00005)
Panel B: Reduced form includig all covariates (OLS):				
Congestion	0.00031**	0.00034*	0.00004	0.00026
	(0.00012)	(0.00019)	(0.00026)	(0.00032)
Panel C: Reduced form excluding health covariates (OLS)				
Congestion	0.00031**	0.00033*	-0.00007	0.00024
	(0.00012)	(0.00019)	(0.00027)	(0.00032)
Observations	72,762	38,125	19,556	15,081
Mean dept. var	0.1588	0.1291	0.1993	0.1813

All regressions show the results of an ordinary least squares regression with a binary indicator for whether the individual had at least one urgent hospitalization in the year after eligibility as an outcome. Panel A includes delays as an explanatory variable, and Panel B and C the instrument. All models include year, region and care profile fixed effects. Panel A and B include all covariates and Panel C only an indicator for whether someone received eligibility during a flu season.

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

Table E7: Relationship delays and congestion with covariates

	Delay		Congestion	
Woman	2.714***	(0.764)	-0.111	(0.105)
Age 65-69	1.159	(1.811)	0.182	(0.267)
Age 70-74	3.476***	(1.345)	-0.409**	(0.185)
Age 75-79	5.104***	(1.078)	-0.0224	(0.143)
Age 80-84	1.715*	(0.888)	-0.298**	(0.119)
Age 90-94	-4.177***	(0.989)	0.139	(0.138)
Age 95+	-8.838***	(1.622)	-0.299	(0.246)
HC exp. on GP care (x1000)	0.689	(1.487)	0.0217	(0.213)
HC exp. on hospital care (x1000)	-0.420***	(0.0352)	-0.00560	(0.00658)
Hospitalization in last 30days	-18.68***	(1.353)	0.0949	(0.231)
Wealth 5-20k	2.702***	(0.945)	0.0374	(0.131)
Wealth 20-50k	1.979**	(0.981)	0.0316	(0.136)
Wealth >50k	-0.224	(0.974)	0.0338	(0.135)
Home ownership	0.675	(0.731)	0.00444	(0.0995)
ATC_A02	1.790**	(0.742)	0.0434	(0.101)
ATC_A10	0.400	(0.833)	-0.158	(0.116)
ATC_B01	-0.193	(0.776)	-0.118	(0.107)
ATC_B03	-0.487	(0.950)	0.113	(0.133)
ATC_B05	1.053	(4.196)	-0.294	(0.677)
ATC_C01	0.0436	(0.965)	0.275**	(0.137)
ATC_C03	-1.142	(0.716)	0.0245	(0.101)
ATC_C07	0.371	(0.725)	0.201**	(0.1000)
ATC_C08	0.115	(0.794)	0.175	(0.112)
ATC_G04	0.276	(1.027)	-0.193	(0.143)
ATC_H02	1.032	(0.997)	0.255*	(0.143)
ATC_J01	0.830	(0.719)	-0.131	(0.0982)
ATC_L04	-4.280	(2.699)	-0.231	(0.410)
ATC_M04	-2.796	(1.702)	-0.165	(0.246)
ATC_N02	-1.779**	(0.817)	0.0669	(0.116)
ATC_N03	-0.853	(1.305)	-0.293	(0.190)
ATC_R03	-0.00518	(0.956)	0.153	(0.135)
ATC_V03	13.17**	(5.509)	0.0198	(0.811)
Congestive heart failure	-15.12***	(1.975)	0.0161	(0.336)
Peripheral vascular disease	-13.02***	(4.141)	0.670	(0.798)
Stroke	-31.57***	(0.914)	-0.243	(0.183)
Dementia	-29.59***	(6.834)	0.0349	(0.919)
Pulmonary disease	-5.073*	(2.609)	-0.727*	(0.420)
Connective tissue disorder	7.634	(11.26)	1.441	(1.757)
Peptic ulcer disease	-26.75***	(6.227)	-0.322	(1.117)
Liver disease	1.404	(15.38)	-0.822	(2.271)
Diabetes	-15.17***	(4.042)	0.714	(0.669)
Diabetes complications	8.027	(16.88)	0.383	(2.796)
Paraplegia	-31.99***	(7.910)	-2.076	(1.694)
Renal disease	-10.48*	(6.315)	1.593	(1.150)
Cancer	-9.989***	(2.753)	0.316	(0.433)
Metastic cancer	-14.83**	(6.774)	-1.398	(1.278)
Severe liver disease	-20.77	(16.96)	-1.685	(3.620)
Constant	82.87***	(3.163)	69.81***	(0.505)
Care profile fixed effects	Yes		Yes	
Year fixed effects	Yes		Yes	
Region fixed effects	Yes		Yes	
Influenza dummy	Yes		Yes	
F_test joint significance	37.82		1.317	
p_value	0		0.0694	
Observations	72762		72762	

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

Table E8: 2SLS results - Sensitivity tests

	Including incomplete observa-tions (e.g. deaths)	Add month-by-year fixed effects	Instrument definitions			
			Smaller re-gions (mu-nicipalities)	Narrower time win-dow (30 days before and after eligibility)	Change time win-dow to 92 days before eligibility	Weighted average based on inverse distance to other eligible individuals
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second stage result (outcome = urgent hospital use)						
$\widehat{Delay}$ (in days)	0.00051***	0.00045**	0.00054***	0.00039**	0.00070***	0.00060***
	(0.00017)	(0.00020)	(0.00017)	(0.00019)	(0.00019)	(0.00018)
Panel B: First stage result (endogenous var = delay in nursing home admission)						
Instrument: congestion	0.660***	0.610***	0.247***	0.535***	0.611***	0.292***
	(0.0195)	(0.0291)	(0.0111)	(0.0252)	(0.0278)	(0.0138)
F-statistic	823	440	495	449	485	449
Observations	119,444	72,762	71,708	72,762	72,648	72,757
Urgent hos-pitalisation rate	0.2017	0.1588				

Table reports first and second stage results of the main analyses with small corrections to analyse the robustness of the main results. Column (1) includes all previously deleted individuals because they either died or moved out of the nursing home within one year after eligibility. Column (2) includes month-by-year fixed effects in both the first and second stage regression. Columns (3) to (6) tests how robust the main result is to changes in the definition of the instrumental variable, namely using fluctuations in delays within smaller regions (i.e. municip-alities), using a narrower time window of 30 instead of 46 days before and after the individual's own eligibility, replacing the time window to include other individuals who received eligibility just before the individual's own and calculating a weighted average of delays by distances from the individual's place of residents to the other individuals' residents who have the same care profile and are eligible in the same region and period.

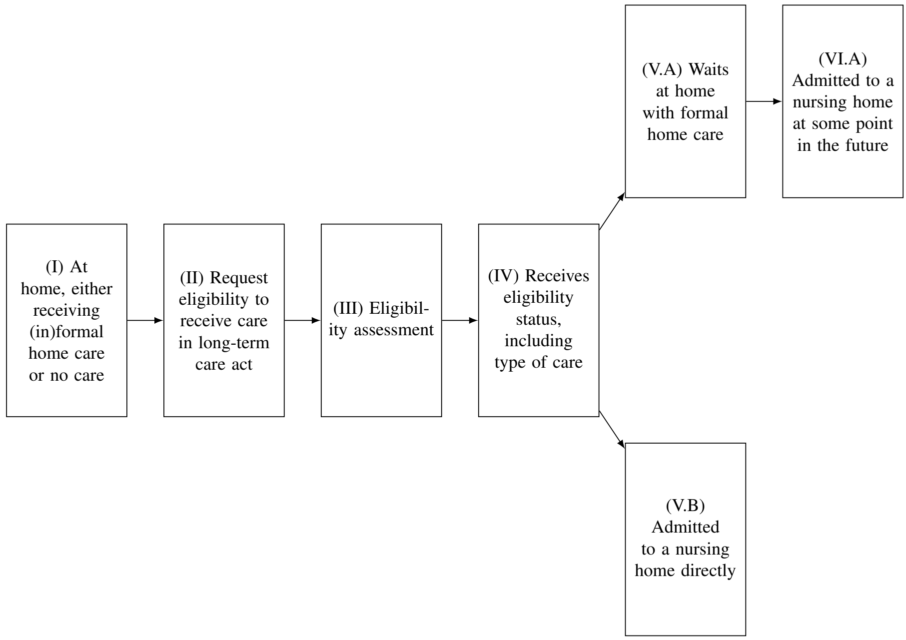
All first and second stage regressions include all covariates and care office region, year and care profile fixed effects.

Sample sizes slightly deviate between the instrument specifications due to the omission of very small municip-alities in Column (3), dropping observations who received eligibility on April 1 or 2 to construct an instrument using data from January 1 2015 of all eligible individuals 92 days before in Column (5) or with missing detailed address data.

Standard errors between brackets. *** Statistically significantly different from zero at 1 percent; ** at 5 percent; * at 10 percent.

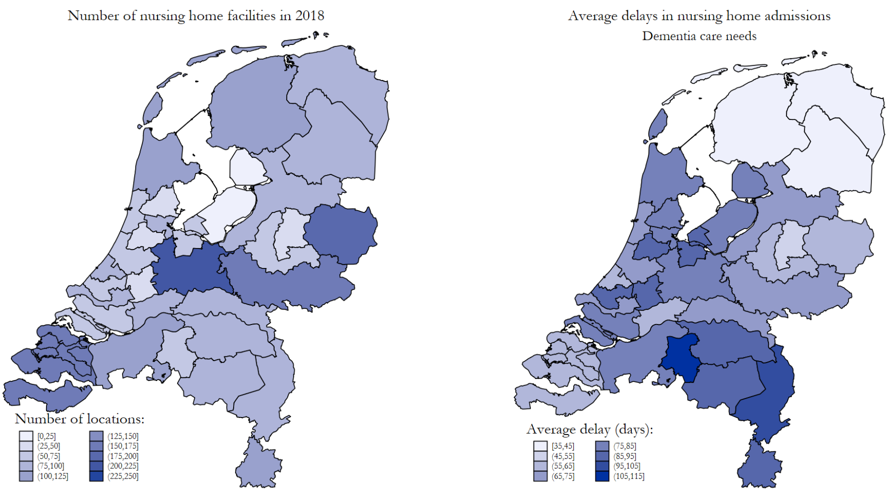
Appendix Figures

Figure E1: Process from eligibility to the nursing home admission



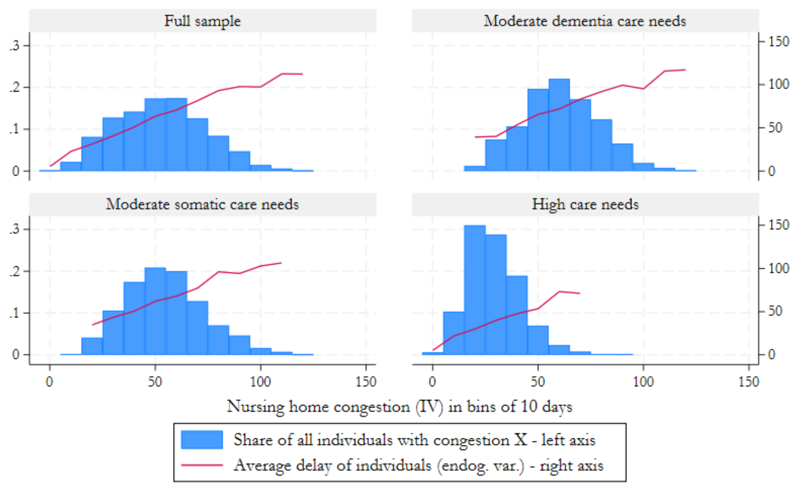
Notes: The Figure demonstrates the process of applying for an eligibility status up to the nursing home admission. Eligibility can also be requested by a physician if one requires a nursing home admission after a hospitalisation. The process of such an urgent admission slightly deviates from the process depicted in this Figure in which the recipient may first receive care in a crisis facility.

Figure E2: 31 care office regions (in Dutch: Zorgkantoorregio's)



Notes: Figure shows the variation across 31 regions in the number of nursing home facilities in 2018 (left) and the average delay among people with moderate dementia care needs (right). Data on the number of facilities comes from TNO (2019) and average delays from own calculations.

Figure E3: Congestion and average individual delays



Notes: Figure shows the distribution of the instrument (i.e. nursing home congestion) and the non-parametric relationship with individual delays (endogenous variable). The average delay for instrument level X is removed if it was based on fewer than 50 observations

Figure E4: First stage result by subgroup

Notes: Figure shows the first stage results for various sub-samples, composed using information on the covariates. Predicted hospitalisation quintiles are constructed by estimating a linear regression of urgent hospitalisations on all covariates, then predicting one's probability to be hospitalised using the estimated coefficients and subsequently dividing the sample into five quintiles from low to a high probability. The red dots depict the first-stage coefficient of the instrument congestion in a regression on endogenous individual delays as an outcome, with the 95 percent confidence intervals reported by the blue lines.

Acknowledgements per chapter

Chapter 2
We acknowledge support from the Initiative for Smarter Choices for Better Health funded by Erasmus University and the project 'Optimizing long-term care provision' funded by the Dutch Research Council (NWO). C. Riumallo Herl received funding from the European Union's Horizon 2020 research and innovation programme under the Marie Sklodowska Curie grant agreement No 707404. The results are based on our own calculations using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information, contact microdata@cbs.nl. We are grateful to Janet Currie, Sonya Krutikova, James Banks, Monica Costa Dias and the other participants of the IFS meetings on inequality in mortality for their valuable comments. We also thank Viola Angelini and the other participants of the 2021 Netspar Pension Day, and the participants at the SCBH Health Equity meeting for their comments.

Chapter 3
We thank Tim Doran and three anonymous reviewers for their valuable comments on an earlier draft of this paper. Data may be obtained from a third party and are not publicly available. The results presented in this article are based on calculations by the authors using non-public microdata from Statistics Netherlands (CBS). Under certain conditions and a confidentiality agreement, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl.

Chapter 4
We do not have any competing interests to report. We acknowledge support from the project 'Optimizing long-term care provision' funded by the Dutch Research Council (NWO) and Open Data Infrastructure for Social Science and Economic Innovations (ODISSEI). The results are based on our own calculations using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information, contact microdata@cbs.nl.

We are grateful to two anonymous reviewers, Rudy Douven, Raf van Gestel, Lucas Goossens, Anne Penneau, Marcelo Peraillon, Martin Salm, Willem Sas, Jannis Stöckel, Francisca Vargas Lopes, Cristina Vilaplana Prieto, as well as to the participants of the

2022 Nursing Home Research International Conference, the 2022 Dutch Assocation for Health Economics (Vereniging voor Gezondheidseconomie) conference, the 2021 APHEC workshop, the 2021 International Health Economics Association Congress, the 2021 Conference of the American Society of Health Economists, the 2021 Irdes-Dauphine Workshop on Applied Health Economics and Policy Evaluation, the 2021 Dutch Economists' Day, the 2020 European Health Economics Association Congress, the 2020 Lowlands Health Economics Study Group, the ESE/ESHPM PhD Research Symposium and of the Long-Term Care Focus Group (ESHPM, Rotterdam) for comments on earlier versions of this paper.

Chapter 5
This research is part of the program 'Oversterfte in Nederland 2020-2021', funded by ZonMw. The results are based on our own calculations using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information, contact microdata@cbs.nl. We are grateful to Maud ten Koppel, the participants of the lowlands Health Economists' Study Group conference 2023 and the International Health Economics Association conference 2023, and the members of UNO Amsterdam and the nursing home care professionals for their comments on earlier versions of the study.

Chapter 6
We acknowledge support from the project 'Optimizing long-term care provision' funded by the Dutch Research Council (NWO), Stichting Erasmus Trustfonds and Open Data Infrastructure for Social Science and Economic Innovations (ODISSEI). The results are based on our own calculations using non-public microdata from Statistics Netherlands. Under certain conditions, these microdata are accessible for statistical and scientific research. For further information, contact microdata@cbs.nl.

We are grateful to James Beveridge, Eddy van Doorslaer, Rudy Douven, Giovanni van Empel, David Madden, Henning Øien, Karinna Saxby, George Stoye, as well as to the participants of the Gateway to Global Aging Data Long-Term Care Seminar, the spring 2023 European Health Economics Association seminar series, the 2023 IRDES-Dauphine Workshop on Applied Health Economics and Policy Evaluation, the 2023 International Health Economics Association World Congress, the 2023 European Workshop on Econometrics and Health Economics, the 2023 Dutch Health Econometrics Workshop, the Health Economics and Data Group seminar at the University of York and the internal Health Economics seminar at Erasmus School of Economics for comments on earlier versions of this paper.

Summary

Populations are increasingly growing older across the entire developed world. While these ageing populations are partly a result of falling mortality rates at older ages, indicating improvements in overall population health, they also raise questions about the distribution of mortality improvements across socioeconomic groups. The recent COVID-19 pandemic further amplified these concerns as it had a large sudden impact on mortality, particularly affecting the old and poorer population. Beyond considerations about mortality inequality, ageing populations bring challenges regarding the sustainability of care provision for the growing oldest age groups. One of the central policy questions is how access to high quality care for the increasing number of older individuals can be ensured without exhausting the public budget or workforce. Enhancing the efficiency of care for the older population may be a potential way to address this issue. This thesis explores these areas by studying: i) income-related inequality in mortality at older ages; and ii) efficiency in nursing home care.

Part I: Income-related inequality in mortality at older ages
Overall survival at older ages has been improving over the last decades. However, some groups may have benefited more than others. For example, wealthier individuals may derive more advantages from medical advancements compared to those with lower incomes, who encounter more financial barriers in accessing such medical care. When individuals with higher incomes experience larger mortality improvements, it exacerbates the existing mortality gap. Such widening disparities are undesirable and reducing them has been a long-standing policy goal. Part I of this thesis offers insights into the trends and potential drivers of mortality inequality by income, which are key for designing strategies how to reduce them.

Chapter 2 of this thesis reveals diverging mortality inequality trends among the young and the old in the Netherlands between 1996 and 2016. At younger ages, mortality improvements – particularly those stemming from preventable or cardiovascular related causes of death – favoured individuals from poorer regions, thereby reducing the disparities in mortality, at least at the regional level. In contrast, among the 65+, mortality disparities by income increased. Notably, among 80+ women, the drop in mortality within the wealthiest decile was 1.5 times as large as the drop in the poorest decile. Whether these increasing inequalities at older ages can be attributed to avoid-

able causes of death is not straightforward. However, rising death rates from preventable causes among women explain part of the widening disparities. These findings highlight the importance of considering older age-groups and the potential role of preventive health behaviour in explaining disparities by income.

Chapter 3 further considers the older population, using the recent COVID-19 pandemic as a case to examine mortality inequalities by income. The pandemic constitutes a major unexpected shock to mortality at older ages, leading overall mortality among the 65+ year population in 2020 to be, on average, 4 percent higher than expected based on historic trends. While the pandemic affected the whole (older) population, cause-attributed COVID-19 deaths were more concentrated among poorer groups. Yet, the pandemic's impact on overall mortality disparities was mitigated by a reduction in deaths from other causes of death, which mainly occurred among poorer groups. This suggests that the unequal distribution of cause-attributed COVID-19 deaths mainly reflect pre-existing socioeconomic health disparities, as COVID-19 deaths likely displaced deaths that might have otherwise been driven by other causes. Nonetheless, the displacement of other causes was insufficient to prevent an overall increase in inequalities, resulting in total mortality inequality in 2020 exceeding the expected levels based on historic trends.

Part II: Efficiency in nursing home care

Ageing populations increase the demand for care services, continuing to rise (public) care expenditures and the burden on the workforce. These concerns are particularly acute in the nursing home sector due to it being expensive, labour intensive and mainly provided to the older population. Optimising the provision and allocation of nursing home care might mitigate the impact of increasing care needs on the public budget at workforce. The second part of this thesis provides new insights into the efficiency of provision and allocation of nursing home care by examining differences in outcomes across nursing home providers and studying the consequences of waiting times.

The findings in Chapters 4 and 5 document substantial differences in health outcomes across Dutch nursing homes. Even after adjusting for resident case-mix differences, the mortality probability in the top 5 percent nursing homes is 7 percentage points lower than that in the bottom 5 percent (Chapter 4). This difference extends to a 14 percentage points difference in the avoidable hospitalisation rate (Chapter 4) and a 10 percentage points difference in excess mortality during the COVID-19 pandemic (Chapter 5). Using exogenous variation in the proximity to varying performance levels, Chapter 4 shows that the variation in health outcomes across nursing homes is not driven by selection bias. The considerable variation in (excess) mortality and avoidable hospitalisations thus reflects large variability in the performance of nursing homes in preventing adverse health outcomes. This suggests there is a scope for improving outcomes, particularly among the lowest-performing providers.

The variation in resident outcomes before and during the COVID-19 pandemic is not captured by quality indicators related to the organisational structure and care processes of nursing homes. Chapters 4 and 5 reveal weak relationships between (excess) mortality and avoidable hospitalisation and various nursing home characteristics, including staffing indicators, care processes and user-reported online ratings. The weak relationships suggest that information on resident outcomes could serve as a useful complement to the publicly available information on nursing home characteristics. Adding information on outcomes to quality frameworks would provide additional insights for prospective residents or other users of quality information wishing to select a nursing home with good outcomes.

Chapter 6 indicates that inefficiencies in the allocation of nursing home beds can cause spillovers to the hospital sector. These spillovers can occur when individuals delay their admission to a nursing home due to long waiting times. Such a delay increases hospital utilisation: a one-month delay increases the probability to be urgently hospitalised by 1.4 percentage points (equivalent to 11 percent of the average urgent hospitalisation rate). The impact is primarily driven by individuals requiring dementia care, can for more than 30 percent be attributed to falls and disappears as soon as one is admitted to a nursing home. These findings suggest that timely access to a protective environment, such as a nursing home facility, induces positive outcomes which spill over to the healthcare sector. This implies that policies restricting access to nursing homes have consequences for the broader healthcare system, which should be considered when evaluating the impact of such policies on budgets and societal welfare.

Samenvatting

In de gehele ontwikkelde wereld worden mensen steeds ouder. Terwijl deze vergrijzing deels het gevolg is van dalende sterftecijfers op oudere leeftijd, wat wijst op verbeteringen in de algehele gezondheid van de bevolking, roepen ze ook vragen op over mogelijk ongelijk verdeelde verbeteringen in sterfte over sociaaleconomische groepen. De recente COVID-19 pandemie heeft deze zorgen verder versterkt, gezien de grote plotselinge impact op de sterfte met name onder ouderen en de armere bevolking. Naast overwegingen over ongelijkheid in sterfte brengt vergrijzing andere uitdagingen met zich mee die betrekking hebben tot de houdbaarheid van zorgverlening voor de groeiende oudste leeftijdsgroepen. Eén van de centrale beleidsvragen is hoe toegang tot kwalitatief goede zorg voor het toenemende aantal ouderen gewaarborgd kan worden zonder het publieke budget of personeel uit te putten. Het verbeteren van de efficiëntie van zorg voor de oudere bevolking kan een mogelijke manier zijn om dit probleem aan te pakken. Dit proefschrift verkent deze gebieden door te kijken naar: i) inkomensgerelateerde ongelijkheid in sterfte op oudere leeftijd; en ii) efficiëntie in verpleeghuiszorg.

Deel I: Inkomensgerelateerde ongelijkheid in sterfte op oudere leeftijd
Over het algemeen zijn de sterftecijfers op oudere leeftijd in de afgelopen decennia flink verbeterd. Sommige groepen hebben echter mogelijk meer geprofiteerd dan anderen. Bijvoorbeeld, welgestelde individuen profiteren mogelijk meer van medische vooruitgang in vergelijking met mensen met lagere inkomens, die relatief meer financiële barrières ondervinden bij het verkrijgen van (medische) zorg. Wanneer groepen met hogere inkomens grotere verbeteringen in sterfte ervaren wordt de bestaande kloof in sterfte verergerd. Dergelijke toenemende ongelijkheden zijn ongewenst en het verminderen ervan is al jaren een beleidsdoel. Deel I van dit proefschrift biedt inzicht in de trends en mogelijke drijfveren van ongelijkheid in sterfte naar inkomen. Deze inzichten zijn essentieel zijn voor het bepalen van strategieën om deze ongelijkheden te verminderen.

Hoofdstuk 2 in dit proefschrift onthult uiteenlopende trends in ongelijkheid in sterfte tussen jongeren en ouderen in Nederland tussen 1996 en 2016. Op jongere leeftijd waren sterfteverbeteringen - met name die voortkwamen uit vermijdbare of hart- en vaatziekten gerelateerde doodsoorzaken - voordeliger voor personen uit armere regio's, waardoor de ongelijkheid in sterfte verminderden, althans op regionaal niveau. Daarentegen nam onder de 65-plussers de ongelijkheid in sterfte naar inkomen toe. Onder vrouwen van 80 jaar en ouder was de daling in sterfte binnen het rijkste deciel zelfs 1,5 keer zo groot als de daling in het armste deciel. Of deze toenemende ongelijkheden op oudere leeftijd kunnen worden toegeschreven aan vermijdbare doodsoorzaken is niet eenduidig, maar toenemende sterfte door vermijdbare oorzaken bij vrouwen lijkt in ieder geval deels de groeiende ongelijkheden te verklaren. Deze bevindingen benadrukken het belang van het overwegen van oudere leeftijdsgroepen en de potentiële rol van preventief gezondheidsgedrag bij het verklaren van ongelijkheid in sterfte naar inkomen.

In hoofdstuk 3 ligt de focus vervolgens verder op de oudere bevolking en wordt de recente COVID-19 pandemie als casus gebruikt om ongelijkheid in sterfte naar inkomen te onderzoeken. Deze pandemie vormt een grote onverwachte schok in sterfte op oudere leeftijd, waarbij de algehele sterfte onder de 65-plussers in 2020 gemiddeld 4 procent hoger lag dan verwacht op basis van historische trends. Hoewel de pandemie de gehele (oudere) bevolking heeft getroffen, waren COVID-19 gerelateerde sterfgevallen meer geconcentreerd onder armere groepen. Toch werd het effect van de pandemie op de algehele ongelijkheid in sterfte verzacht door een afname van sterfgevallen door andere doodsoorzaken, die voornamelijk plaatsvond onder armere groepen. Dit suggereert dat de ongelijke verdeling van COVID-19 gerelateerde sterfgevallen voornamelijk een weerspiegeling is van bestaande sociaaleconomische gezondheidsverschillen, en dat COVID-19 gerelateerde sterfgevallen waarschijnlijk sterfgevallen hebben verdrongen die zonder pandemie door andere oorzaken zouden zijn veroorzaakt. De verdringing van andere oorzaken was echter onvoldoende om een algehele toename in ongelijkheid te voorkomen, wat in 2020 resulteerde in een totale ongelijkheid in sterfte die de verwachte niveaus op basis van historische trends overschreed.

Deel II: Efficiëntie in de verpleeghuiszorg

De vergrijzende bevolking verhoogt de vraag naar zorgdiensten, waardoor (publieke) uitgaven aan zorg blijven stijgen en de druk op de beroepsbevolking toeneemt. Deze zorgen zijn met name acuut in de verpleeghuissector vanwege de hoge kosten en intensieve arbeid die deze zorg vraagt en omdat deze voornamelijk wordt verleend aan oudere cliënten. Het optimaliseren van de verstrekking en toewijzing van verpleeghuiszorg kan de impact van toenemende zorgbehoeften op het publieke budget en de beroepsbevolking helpen verminderen. Het tweede deel van dit proefschrift biedt nieuwe inzichten over de efficiëntie van de verstrekking en toewijzing van verpleeghuiszorg door verschillen in uitkomsten tussen verpleeghuizen te onderzoeken en de gevolgen van wachttijden te bestuderen.

De bevindingen in hoofdstukken 4 en 5 tonen aanzienlijke verschillen aan in gezondheidsuitkomsten tussen Nederlandse verpleeghuizen. Zelfs na correctie voor verschillen in de case-mix van bewoners, is de kans op sterfte in de top 5 procent verpleeghuizen 7 procentpunten lager dan die in de onderste 5 procent (hoofdstuk 4). Het verschil tussen de top en onderste 5 procent voor vermijdbare ziekenhuisopnames is gelijk aan 14 procentpunten (hoofdstuk 4) en voor oversterfte tijdens de COVID-19 pandemie gelijk aan 10 procentpunten (hoofdstuk 5). Met behulp van exogene variatie in de nabijheid van verpleeghuizen met verschillende uitkomsten, laat hoofdstuk 4 zien dat de variatie in gezondheidsuitkomsten tussen verpleeghuizen niet wordt veroorzaakt door selectiebias. De aanzienlijke variatie in (over)sterfte en vermijdbare ziekenhuisopnames weerspiegelt dus een grote variabiliteit in de prestaties van verpleeghuizen bij het voorkomen van ongunstige gezondheidsresultaten. Dit suggereert dat er ruimte is voor verbetering van deze uitkomsten, met name onder de slechtst presterende aanbieders.

De variatie in uitkomsten van bewoners voor en tijdens de COVID-19 pandemie wordt niet volledig verklaard door kwaliteitsindicatoren gebaseerd op de organisatiestructuur en zorgprocessen van verpleeghuizen. Hoofdstukken 4 en 5 onthullen zwakke relaties tussen (over)sterfte en vermijdbare ziekenhuisopnames en verschillende kenmerken van verpleeghuizen, waaronder personeelsindicatoren, zorgprocessen en door gebruikers gerapporteerde online beoordelingen. De zwakke relaties suggereren dat informatie over bewonersuitkomsten een nuttige aanvulling kan zijn op de openbaar beschikbare informatie over de kenmerken van verpleeghuizen. Het toevoegen van informatie over uitkomsten aan kwaliteitskaders zou aanvullende inzichten bieden voor toekomstige bewoners of andere gebruikers van kwaliteitsinformatie die een verpleeghuis met goede uitkomsten willen selecteren.

De bevindingen in hoofdstuk 6 laten zien dat inefficiënties in de toewijzing van verpleeghuisbedden kunnen leiden tot negatieve gevolgen die overlopen naar de ziekenhuissector. Deze gevolgen kunnen optreden wanneer mensen hun opname in een verpleeghuis uitstellen vanwege lange wachttijden. Een dergelijke vertraging van een opname verhoogt het ziekenhuisgebruik: een vertraging van één maand verhoogt de kans op een urgente ziekenhuisopname met 1,4 procentpunten (gelijk aan 11 procent van het gemiddelde urgente ziekenhuisopnamepercentage). Het effect wordt voornamelijk veroorzaakt door personen die dementiezorg nodig hebben. Daarnaast kan meer dan 30 procent worden toegeschreven aan ziekenhuisopnames na een val en verdwijnt het negatieve effect van de vertraging zodra iemand wordt opgenomen in een verpleeghuis. Deze bevindingen suggereren dat tijdige toegang tot een beschermende omgeving, zoals een verpleeghuis, positieve resultaten teweegbrengt die doorstromen naar de bredere gezondheidszorgsector. Dit impliceert dat beleidsmaatregelen die de toegang tot verpleeghuizen beperken, gevolgen hebben voor het bredere gezondheidszorgsysteem. Deze gevolgen zullen in overweging genomen moeten worden bij het evalueren van de invloed van dergelijke beleidsmaatregelen op budgetten en maatschappelijk welzijn.

Portfolio

Academic publications	Year
Bär, M., Wouterse, B., Riumallo Herl, C., Van Ourti, T. and van Doorslaer, E. (2021), Diverging Mortality Inequality Trends among Young and Old in the Netherlands. *Fiscal Studies, 42*, 79-101	2021
Schwandt, H., Currie, J., Bär, M., Banks, J., Bertoli, P., Bütikofer, A., Cattan, S., Zong-Ying Chao, B., Costa, C., Gonzalez, L., Grembi, V., Huttunen, K., Karadakic, R., Kraftman, L., Krutikova, S., Lombardi, S., Redler, P., Riumallo Herl, CJ., Rodríguez-González, A., ... Wupperman, A. (2021). Inequality in Mortality between Black and White Americans by Age, Place, and Cause, and in Comparison to Europe, 1990-2018. Proceedings of the National Academy of Sciences of the United States of America, 118(40), Article e2104684118	2021
Bär, M., Bakx, P., Wouterse, B., & van Doorslaer, E. (2022), Estimating the health value added by nursing homes. *Journal of Economic Behavior & Organization, 203*, 1-23	2022
Wouterse B., Geisler, J., Bär, M. and van Doorslaer, E. (2023), Has COVID-19 increased inequality in mortality by income in the Netherlands? *Journal of Epidemiology & Community Health, 77*, 244-251	2023

Other professional publications	
Bakx, P., Bär, M., Rellstab, S., & Wouterse, B. (2020). *Zicht op kwaliteit van verpleeghuiszorg?* ESHPM Onderzoeksrapport 2020.03 ed. Erasmus School of Health Policy & Management (ESHPM)	2020
Bär, M., Vermeulen, W., Varkevisser, M. & Wouterse, B. (2022). Kiezen voor kwaliteit: feit of fictie? *VGE Bulletin, 39*(1), 15-18	2022

Training activities and courses	
Basic didactics *Risbo*	2019
Empirical policy evaluation in health *Swiss Society of Health Economics*	2020
Coachvaardigheden *TOP*	2020
The econometrics of panel data *ERIM*	2020
English academic writing for PhD candidates *EGSH*	2020
Digital research methods for textual data *EGSH*	2021
Communicating your research: Lessons from Bitescience *EGSH*	2021
How to supervise thesis students? *Risbo*	2022
MATLAB data skills & tools for the social sciences *EGSH*	2022

Presentations	
European Health Economics Association Conference (virtual)	2020
Lowlands Health Economics Study Group (virtual)	2020
American Society of Health Economists Conference (virtual)	2021
APHEC Workshop	2021
Dutch Economists Day	2021
International Health Economics Association World Congress (virtual)	2021
IRDES Workshop on Applied Health Economics and Policy Evaluation (virtual)	2021
Netspar International Pension Workshop (virtual)	2021
Dutch Association for Health Economists Congress	2022
Dutch Economists Day (panel member)	2022
European Workshop on Econometrics and Health Economics	2022
Health, Econometrics and Data Group seminar, University of York	2022
Nursing Home Research Conference	2022
Dutch Health Econometrics Workshop	2023
European Health Economics Association seminar series (virtual)	2023
European Workshop on Econometrics and Health Economics	2023
Gateway to Global Aging Data Long-Term Care Seminar (virtual)	2023
International Health Economics Association World Congress	2023
IRDES Workshop on Applied Health Economics and Policy Evaluation	2023
Lowlands Health Economics Study Group	2023

Teaching activities	
Economics of Health and Healthcare (MSc)	2019 -2021
Bachelor thesis coach (BSc)	2019 -2022
Zorgen voor Later (BSc)	2020 -2023
Public Health Economics (MSc)	2021 -2023
Master thesis supervisor (MSc)	2022 -2023

Grants	
ZonMw COVID-19 deelprogramma 'Oversterfte in Nederland 2020-2021' Project title: *Determinants of excess mortality rates in nursing homes*	2022
Erasmus Trustfonds Research Visit Grant Project title: *Nursing home waiting times and hospital outcomes*	2022

Other activities	
Young ESHPM board member	2021
Journal reviewing:	
Journal of Economic Behavior & Organization	2022
Health Economics	2023
Organiser internal events:	
CBS microdata focus groups	2022 -2023
Ask the editors	2022
Comfy to your next confy	2023
Research visit at the Center for Health Economics, University of York *Collaborators/hosts: Nigel Rice, Rita Santos & Luigi Siciliani*	2022

About the author

Marlies Bär (born in 1995, Leeuwarden, the Netherlands) started her academic career at the University of Groningen, Faculty of Economics and Business, obtaining a Bachelor's degree in Economics & Business Economics in 2017. Following this, she pursued a Master's degree in Health Economics at Erasmus School of Economics, Erasmus University Rotterdam. During her master's program, Marlies spent three months at Stellenbosch University in South Africa, where she conducted research for her Master thesis and also worked as a research assistant.

In 2019, Marlies commenced her PhD trajectory in Health Economics at Erasmus School of Health Policy & Management (ESHPM). Her doctoral research, focusing primarily on income-related inequalities in mortality and long-term care, is part of the NWO program 'Optimizing long-term care provision', resulting in this dissertation. Alongside her research, Marlies has been involved in teaching both BSc and MSc courses at ESHPM. Additionally, she served as a board member of Young ESHPM and spent 2 months as a visiting researcher at the Center for Health Economics at the University of York, United Kingdom.

Since January 2024, Marlies has worked as an assistant professor at ESHPM, where she continues her research and teaching in the field of Health Economics.