The impact of co-payments for nursing home care on use, health, and welfare

EsCHER Working Paper No. 2021002
November 2021

Marianne Tenand, PhD
Pieter Bakx, PhD
Bram Wouterse, PhD

Title
The impact of co-payments for nursing home care on use, health, and welfare

Authors
Marianne Tenand, PhD, Netherlands Bureau for Economic Policy Analysis (CPB)
Pieter Bakx, PhD, Erasmus School of Health Policy & Management (ESHPM)
Bram Wouterse, PhD, ESHPM

Corresponding author: Marianne Tenand, CPB, email: m.tenand@cpb.nl

Keywords
Population ageing, long-term care, nursing home, co-payments, price sensitivity, moral hazard

JEL classification
C24; D12; I18; J14

Cite as
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

Erasmus Centre for Health Economics Rotterdam (EsCHER) is part of Erasmus University Rotterdam.
Want to know more about EsCHER? Visit www.eur.nl/escher
Want to contact EsCHER? E-mail escher@eur.nl

Interested in more EsCHER Working Papers? Download from www.eur.nl/escher/research/workingpapers

The impact of co-payments for nursing home care on use, health, and welfare

Marianne Tenand* Pieter Bakx† Bram Wouterse‡

November 2021

Abstract

Nursing home residents often have to pay substantial co-payments. We assess the impact of these co-payments, leveraging a Dutch reform. An increase in the monthly co-payment induced users to postpone permanent nursing home admissions. The resulting savings were only partially offset by increases in home care use. There were no overall effects on mortality nor on children's care use and income. While the change in the monthly payment was modest, average lifetime payments increased substantially. The welfare loss due to the increased financial risk for potential users likely outweighs the gains associated with the reduction in publicly financed care.

Key-words: Population ageing, long-term care, nursing home, co-payments, price sensitivity, moral hazard.

JEL codes: C24; D12; I18; J14.

*Corresponding author. Contact: m.tenand@cpb.nl. CPB Netherlands Bureau for Economic Policy Analysis, The Hague; Erasmus School of Health Policy & Management (ESHPM), Erasmus University Rotterdam, Rotterdam, the Netherlands.

†Erasmus School of Health Policy & Management (ESHPM), Erasmus University, Rotterdam, the Netherlands.

‡Erasmus School of Health Policy & Management (ESHPM), Erasmus University, Rotterdam, the Netherlands.

Access to and use of individual-level data

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. Data use and publication of the results are in compliance with the European privacy legislation (GDPR, May 25$^{\text{th}}$, 2018).

Acknowledgements

The authors are grateful to Rob Aalbers, Jan Boone, Jeannette Brosig-Koch, Laura Keohane, Ana Moura, Marielle Non, Raun van Oijen, Sylvain Pichetti, Simon Rabaté, Minke Remmerswaal, Anne-Fleur Roos, Suraj Upadhyay as well as to the participants of the 2021 International Health Economics Association Congress, the 2021 Conference of the American Society of Health Economists, 2020 Journées des Économistes de la Santé Francais, the 2020 European Economic Association Congress, the 2020 IZA/University of Duisburg-Essen Workshop on The Economics of Long-Term Care and Healthy Aging, the 2020 Irdes-Dauphine workshop on applied health economics and policy evaluation, the 2020 Netspar International Pension Workshop, the 2020 Lowlands Health Economics Study Group, the CPB Research Seminar (The Hague), the LEGOS seminar at Paris Dauphine University, the internal Health Economics seminar (ESE, Rotterdam) and of the Long-Term Care Focus Group (ESHPM, Rotterdam) for their valuable comments. They are grateful to Judith Bom for her input to the early stages of this article, Francisca Vargas Lopes for preliminary work on the co-payment schedule as well as Walter Popken (CAK) for his assistance in collecting information about the co-payment schedule. The authors thank Annemarie van der Zeeuw for assistance with editing.

Funding

Access to the data was in part funded by the Open Data Infrastructure for Social Science and Economic Innovations (ODISSEI). The authors acknowledge additional financial support from the Network for Studies on Pensions, Aging and Retirement (Netspar) within the project "Optimal saving and insurance for old age: the role of public long-term care insurance". Tenand acknowledges funding from the European Union's Horizon 2020 research and innovation programme under the Marie Sklodowska-Curie grant agreement No 844314 (LTCpolicies).

1 Introduction

Out-of-pocket payments for nursing home care are a considerable financial risk for older people. Virtually all developed countries, including the U.S., have some sort of public scheme to pay for nursing home care (Hashiguchi & Llena-Nozal, 2020), but most of these only cover a part of the full costs (Colombo *et al.*, 2011). Nursing home residents have to pay co-payments[1] that can add up to tens of thousands of euros per year (Muir, 2017) and thus limit the financial protection offered by public programs considerably. For instance, five percent of older people in the U.S. will have lifetime spending of at least $47,000 on co-payments for long-term care (Hurd *et al.*, 2017).

The main economic motivation for levying co-payments is that they may reduce unwarranted use of care (i.e. moral hazard) by shifting a part of the marginal price to the user.[2] Yet, it is not that obvious that co-payments actually reduce use. It is often suggested that "no one wants to go to a nursing home" (Hitchcock, 2015). If an admission is really an option of last resort that is only used by severely disabled individuals, there is little scope for moral hazard. While a large body of literature has documented how financial incentives for patients affect health care use (e.g. Brot-Goldberg *et al.* (2017); Einav & Finkelstein (2018)) and, to a smaller extent, home care use (Pezzin *et al.*, 1996; Stabile *et al.*, 2006; Rapp *et al.*, 2011; Roquebert & Tenand, 2017; Non, 2017; Konetzka *et al.*, 2019), evidence on the price sensitivity of demand for nursing home care is relatively scarce, possibly because of limited availability of data on prices and co-payments (Konetzka *et al.*, 2019; Takahashi, 2020).

We investigate the effects of co-payments on nursing home care by leveraging a reform in the Netherlands in 2013. This reform increased co-payments substantially for some individuals, while others were not affected. Dutch nursing home residents pay a co-payment, which depends on their income and financial wealth. In 2013, the proportion of financial wealth that is taken into account in the computation of the co-payment increased. As the first €25,000 of financial wealth are exempted from the calculation and many Dutch older people have little financial wealth, the reform affected the co-payments for nursing home care only for a subset of the population.

We implement a difference-in-differences (DiD) approach to compare changes in nursing home admission rates across groups that are affected differently by the 2013

[1] Co-payments are explicit user charges; programs may also feature other forms of cost sharing, such as means-testing or partial subsidies. We refer to any kind of out-of-pocket payment associated with care as a co-payment in the remainder of this introduction.

[2] A second economic argument that is used for co-payments is that they reduce the welfare losses from public financing.

reform. We use administrative data on nursing home eligibility and use available for the whole population. We focus on singles who become eligible for permanent nursing home care. Using individual income and wealth data, we can identify the effects on nursing home use, substitution with home care and medical care, total health care costs, mortality as well as on the health care and income of children

We make four contributions to the existing literature. First, the Dutch setting enables us to isolate the deliberate demand-side response from other potential effects. The Netherlands has universal, comprehensive social long-term care insurance and private long-term care insurance is absent. Paying a higher co-payment (or being richer) does not provide users with higher quality, a larger choice set or lower waiting times. Also, payments received by nursing homes from the social insurance scheme are not tied to the income level of the residents nor to the co-payment that they pay.

Second, we can rule out that the demand-side response is caused by a reduction in the access to care, and thus that the reform induced an efficiency loss through this specific channel. The value of long-term care insurance not only lies in consumption smoothing but also in providing financial access. Insurance entails an (implicit) income transfer to individuals in need of care, providing access to nursing home care that they could otherwise not afford (Nyman (1999); also see Konetzka *et al.* (2019); Bakx *et al.* (2015)). When an increase in the private price leads to a decrease in use, this is generally seen as evidence of ex-post moral hazard (see Aron-Dine *et al.* (2013); Finkelstein (2014)); most empirical studies, however, fail to make a distinction between what Nyman (1999) calls *adverse* moral hazard and *positive* moral hazard (the latter arising because of the implicit income transfer). The design of the co-payment in the Netherlands ensures that nursing home care is affordable for everyone. Individuals pay a capped monthly co-payment. The cap depends on an individual's income and wealth. Although the reform increased this cap considerably for some, co-payments therefore have remained within the financial means of care users.

Third, the extensive administrative data allows us to go beyond the effects on nursing home use, and identify effects on other outcomes: home care, medical care, mortality and effects on potential informal caregivers. These effects are important in order to assess whether the co-payment reform was optimal from a societal perspective. As Baicker *et al.* (2015) argue, individuals can make mistakes or lack information when assessing the health benefits of a care option (behavioral hazard). For instance, Chandra *et al.* (2021) find that even modest increases in the private price leads individuals to reduce their use of evidently life saving medication. Cogni-

tive constraints seem especially important in the case of long-term care where many prospective users suffer from dementia or other mental conditions (Chandra *et al.*, 2020), which may affect their ability to choose the optimal alternative when making complex decisions. In addition, there may be externalities, as individuals may not take into account how their choice affects public expenditures on other types of care (medical care, home care) and how it affects potential informal caregivers.

Fourth, we focus on financial incentives that intend to limit or postpone permanent nursing home admissions. Most work from the U.S. focuses on short-term stays in post-acute care. Although this type of nursing home care is very important in the U.S., it plays a much smaller role in many other countries (Bom, 2021). Behavioral responses of individuals in need of post-acute care (relatively independent individuals who suffer from a severe but often temporary health shock) probably differ from those of individuals with severe disabilities who are considering a permanent admission. The financial incentives also have a different purpose in both settings: whereas incentives in post-acute care are used to reduce the duration of treatment by incentivizing quick discharges, incentives in permanent nursing home care intend to postpone admissions for as long as possible. The effect of co-payments on postponing nursing home entry is of great policy relevance as many countries are implementing ageing-in-place policies in an attempt to limit the growing costs of nursing home care (Colombo *et al.*, 2011) .

Most studies on the price sensitivity of nursing home care come from the U.S. Grabowski & Gruber (2007) and Konetzka *et al.* (2019) have the most similar design to our study: they exploit features of nursing home care financing causing exogenous changes in co-payments. Grabowski & Gruber (2007) rely on variation in the generosity of public, means-tested coverage (Medicaid) of nursing expenditures between and within states. Konetzka *et al.* (2019) study the impact of private long-term care insurance, which reduces the out-of-pocket price, on permanent nursing home use, instrumenting insurance status by tax deductions. In both cases, the institutional setup means that, unlike in our setting, they cannot disentangle between simultaneous supply-side and demand-side responses to such changes. Neither of the studies find a significant effect of the change on the use of nursing home care. Hackmann & Pohl (2018) use a different source of variation in price. They exploit the differential rates for privately-paid and Medicaid-paid stays. They focus on post-acute care and find that individuals respond to lower cost-sharing on institutional care by extending their stay instead of transitioning back to the community.

Outside the U.S., Kim & Lim (2015) use a regression discontinuity design that exploits jumps in public long-term care insurance benefits caused by eligibility cut-

offs in South Korea. They find that long-term care use responds to (changes in) public benefits for home care and institutional care. However, like the US studies, Kim & Lim (2015) cannot separate the effects of the price increase for nursing home care from other contemporaneous changes (the more generous home care benefit in their case). Lin & Imanaka (2020) study the effects of an increase in co-payments for long-term care in Japan using a difference-in-difference setting. They find no effect on nursing home care use. However, they do not specify which types of nursing home admission they consider. Finally, Fu & Noguchi (2019) estimate a -0.1 price elasticity of demand for long-term care by comparing individuals in Japan who have to pay a 10% co-payment to others who do not, using propensity score matching.

We find that the Dutch co-payment reform reduced the use of nursing home care of the treated by 4.4 days on average, within 12 months following first eligibility. Although this effect might seem small, it is economically significant when compared to the modest (average) increase in the marginal price of nursing home care: the median monthly price increase was €200. Users postponed nursing home use by 0.8 days per 100-euro increase in the monthly price. In addition, we document limited increases in a range of monetary and non-monetary costs potentially associated with a delayed admission. Increases in expenditures on home care and medical care offset the savings generated by the reduction in nursing home use only to a small extent. Furthermore, the co-payment did not increase overall mortality. There were no negative effects on the outcomes of adult children - who are the main source of informal care for single older adults.

Average lifetime payments among potential users affected by the reform increased by €4,845. Depending on the marginal costs of public funds, this shift from the government to users may represent a welfare gain for society. However, the reform also led to a reduction in the value of insurance. Although the increase in the monthly co-payment was modest, older individuals do face an increased risk of having to pay substantially higher lifetime co-payments when needing care for many years. The welfare loss due to this risk (€3,512 in our preferred estimate) likely outweighs the welfare gains associated with the reduction in moral hazard and government spending.

These results mean that, contrary to what is often conjectured, demand-side financial incentives do play a role in the timing of permanent nursing home stays, even in a country like the Netherlands with comprehensive universal coverage. Changes in the marginal private price, even when relatively moderate, can limit nursing home care use and foster ageing in place. However, relatively small changes in the marginal price still substantially increase lifetime co-payments and financial risk.

For co-payments to be not only *effective* at postponing an admission, but also *efficient*, policy makers need to combine sufficiently high financial incentives at the margin of nursing home entry with strong financial protection of long-stay users.

2 Long-term care in the Netherlands

2.1 Social insurance for long-term care

During the study period, all long-term care except for domestic help was funded through a single social insurance scheme (AWBZ) (Schut *et al.*, 2013). Social long-term care insurance provides universal, comprehensive coverage. This extensive coverage means that the Netherlands is one of the top spenders on long-term care worldwide. In 2014, 5.3% of the 65+ population lives in an institution (OECD, 2020), which is one of the highest rates in the OECD. Privately funded alternatives were virtually absent during the study period (Hussem *et al.*, 2020).

The use of nursing home care is constrained in two ways. First, users pay co-payments (see Section 2.2). Second, people have to be assessed to become *eligible* for long-term care. This assessment is done by the independent assessment agency CIZ (*Centrum Indicatiestelling Zorg*) based on the applicant's needs.[3] The assessor decides on the eligibility for a care setting (home care or institutional care) and the types and the intensity of care. Someone who is eligible for nursing home care may either choose to enter a nursing home, receive an equivalent package of in-kind care services at their home instead, or receive long-term care vouchers.[4] However, someone who is eligible for home care can only choose to receive in-kind home care or vouchers and cannot opt for a nursing home admission. There are no other major constraints to use: during the study period, waiting lists for nursing home care were almost absent (College voor Zorgverzekeringen, 2013).

Applicants who are eligible for permanent institutional care are assigned a care package reflecting the severity and the types of needs. These care packages correspond to 3 to 32 hours of nursing care, personal care, and guidance per week. They are provided in two types of permanent institutional care facilities for older individuals: assisted-living facilities and nursing homes. Individuals are eligible for the former when their care needs are relatively low (care packages 1 to 3) and for the latter when their care needs are relatively high (care packages 4 to 8).[5] For each

[3] Assessors should not take into account other factors, such as the applicant's income or supply constraints, as explained in Tenand *et al.* (2020).

[4] CIZ assessors play no role in care provision after someone has received the eligibility decision.

[5] Alternatively, someone may be eligible to a post-acute rehabilitation facility to recover from a hospitalization

resident, nursing homes are paid a per-diem rate that only depends on the patient's care package, meaning that nursing homes have no incentive to attract patients with specific socio-economic characteristics. To reduce the number of older individuals using institutional care, the rules for institutional care eligibility were made stricter in 2013 and further so in 2014. Specifically, assisted-living facility use was no longer funded for new applications, while rules for nursing home care were unchanged.

2.2 The co-payment schedule

In the aggregate, co-payments are limited in the Netherlands: in 2012, they represented 8% of total spending on long-term care (Schut *et al.*, 2013; Hashiguchi & Llena-Nozal, 2020). At the individual level, co-payments are set as a function of the user's financial resources. There are rebates for users with specific circumstances, in particular those who have not reached the statutory retirement age, or those who live with a partner. Co-payments are paid to the office that administers the contributions to and payouts from the social long-term care insurance scheme rather than directly to the providers. Potential users can estimate their co-payments using an official online simulator.[6]

A nursing home resident is charged the same co-payment whichever nursing home they enter. Moreover, the co-payment does not depend on the type or intensity of the care received in the nursing home. Nursing home residents are subject to either a *low-rate* co-payment or a *high-rate* co-payment. Each of these is determined by specific schedules and ceilings. The low-rate co-payment applies during the first six months of a stay and for individuals who still have a partner living at home, while the high-rate co-payment applies to all other residents.

If an individual's income and wealth are very low, the high-rate co-payment is set to zero. Furthermore, co-payments are capped: no nursing home resident pays more than €2,250 of per month (2014 value - high-rate copayment). Co-payments are computed on a monthly basis.

When receiving home care, individuals are subject to a maximum co-payment that is proportional to the number of hours of care they receive. The home-care co-payment is capped and the cap depends on the individual's income and wealth, such that an individual pays the minimum of this cap and the maximum co-payment.[7] In 2011, the median annual co-payment for all-year nursing home residents was

or to a hospice providing palliative care in the final months of the life. These facilities are used for temporary admissions, which are not studied in this article.

[6] https://www.hetcak.nl/zelf-regelen/eigen-bijdrage-rekenhulp.

[7] In Tenand *et al.* (2021) (https://doi.org/10.25397/eur.16866442), we provide the formulas and additional information on the co-payment schedules.

€7,635, equivalent to 56% of their available income (Bakx *et al.*, 2020); the median annual co-payment for home care was €185. In general, for an equivalent package of care, co-payments are lower if the care is received at home rather than in a nursing home.

2.3 Income and wealth definitions and the 2013 reform

Co-payments for nursing home care and for home care are both a function of total income, which consists of household earnings, plus the income derived from any financial assets and real estate, excluding the net value of the own house (henceforth: wealth).[8] To calculate the income derived from wealth, the Tax Office uses a flat-rate approach: a fixed return on wealth of 4% is used. A part of the wealth is excluded from this calculation (the first €21,000 for singles in 2012), which avoids wealth taxation of those with very low assets.

In 2012, a reform was announced: next to the 4% of wealth included through the total income measure, an additional 8% of wealth was included in the computation of co-payments. The reform was part of a larger set of budgetary measures, but with no other simultaneous changes to the co-payment schedule or immediate impacts on long-term care provision. These measures were based on an agreement reached on April 26$^{\text{th}}$ between a number of opposition parties and the remaining coalition partners after the fall of the government on April 21$^{\text{st}}$.[9] As the reform was not part of the original government plans, anticipation effects before April 2012 are unlikely. The agreement was sent to the Parliament on May 23$^{\text{rd}}$, 2012.[10] The reform was implemented on January 1$^{\text{st}}$, 2013.

2.4 Who is affected by the reform?

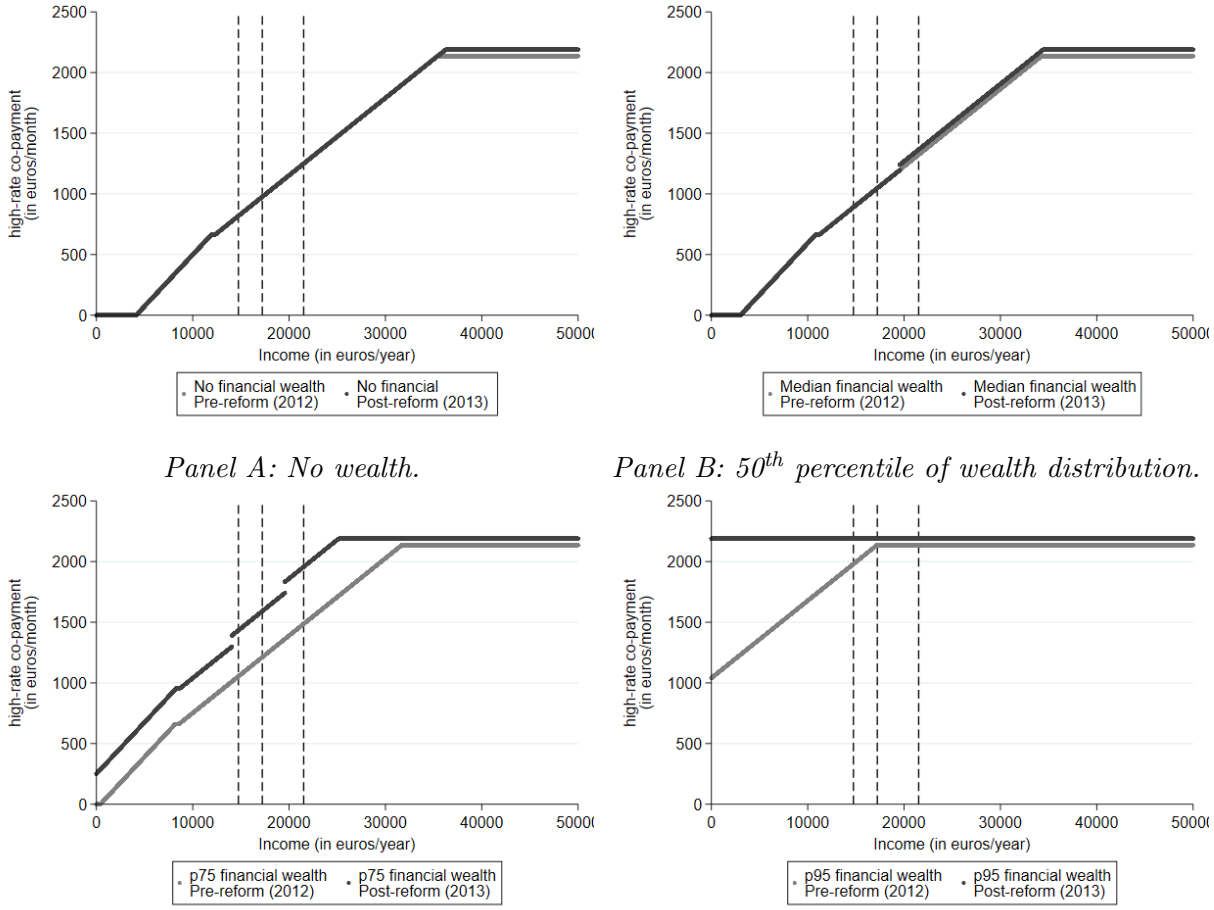
The reform increased co-payments for individuals with positive taxable wealth. Figure 1 shows how the reform affects the monthly high-rate co-payment, depending on the income level and for four different levels of wealth.

Panel A of 1 shows that the reform had no effect on co-payments for users with no wealth: the pre-reform and post-reform schedules are exactly the same. For individuals with median wealth, the reform hardly affected cost sharing on nursing home care, regardless of their income (Panel B).

However, co-payments increased for individuals at the 75$^{\text{th}}$ wealth percentile (Panel C). This increase was larger for those with a low income; the cap kicks

[8] *Box 3: sparen en beleggen.*
[9] See https://www.parlement.com/id/vj1ped4lzuz8/kabinetscrisis_2012_de_catshuiscrisis.
[10] See https://www.tweedekamer.nl/kamerstukken/detail?id=2012Z10576&did=2012D22281.

Figure 1: Co-payment schedule for nursing home care before and after the 2013 reform depending on income, by level of financial wealth.



Panel A: No wealth. *Panel B: 50th percentile of wealth distribution.*

Panel C: 75th percentile of wealth distribution. *Panel D: 95th percentile of wealth distribution.*

NOTES: Authors' simulations. The grey curves are based on the co-payment rules and parameters for 2012, the black ones for 2013. Panel B: schedule for an individual at the 50$^{\text{th}}$ percentile of the wealth distribution (€28,000). Panel C: schedule for an individual at the 75$^{\text{th}}$ percentile of the wealth distribution (€92,000). Panel D: schedule for an individual at the 95$^{\text{th}}$ percentile of the wealth distribution (€455,000). The dashed vertical lines indicate the 25$^{\text{th}}$, 50$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of the income distribution. The wealth and income distributions refer to the distribution of financial wealth (per capita) and the distribution of available income (per consumption unit) as reported in tax year 2010 in the 65+ Dutch population alive in 2012. The notches visible in Panels B and C are due to rebates on taxable wealth that apply to individuals below some income thresholds and a certain wealth level. See the formulas in Tenand *et al.* (2021) (link). The increase in the co-payment cap, visible in all four panels, reflects the fact that the schedule parameter values are indexed every year.

in only for those with a relatively high income (€25,000 a year and higher, while the third quartile of the available income distribution was only around €22,000). Individuals in the 75$^{\text{th}}$ wealth percentile with a median income experienced an increase in the co-payment for a month in the nursing home of about €500 (from

€1,200 to €1,700). By contrast, only a small fraction of the 95$^{\text{th}}$ percentile of the wealth distribution (Panel D) experiences a change in co-payments. Those with high wealth and an income higher than the median income experienced no change in the co-payment because they already hit the cap under the pre-reform rules. However, those with high wealth and low income experienced a marked increase in the monthly co-payment, of up to €1,200.

To sum up, the 2013 reform led to an increase in the out-of-pocket price of nursing home care only for individuals with high wealth. Among those who were affected by the change, the effect was highly heterogeneous. Co-payments for home care also increased for individuals with sufficient wealth, to a much smaller extent than for nursing home care, and by a magnitude that depends not only on income and wealth but also on the number of hours of care used.

3 Theoretical model

3.1 A model of nursing home entry

To guide the empirical analysis and aid the interpretation of results, we introduce a theoretical model of nursing home entry. Individuals who become eligible can use nursing home care in each consecutive month $t = 1, .., T$ after eligibility. An individual i will do so when the private benefits from living in a nursing home are larger than the private costs.[11] We are interested in the effect of the private price of care on use. We label this price p_{it}. Furthermore, we capture the net difference between all other monetary and non-monetary costs and benefits in a single term u_{it}, the (monetized) net utility of living in a nursing home in period t. Thus, an individual uses nursing home care in period t when:

$$u_{it} > p_t \tag{1}$$

The type of care we investigate in this paper is permanent nursing home care: once individuals move to a nursing home, they (generally) stay there until they die.[12] This means that the relevant marginal decision for an individual is whether

[11] We abstract here from externalities and potential (health) effects of sub-optimal decision making, but do address these in the empirical analysis and when weighing the costs against the benefits of the co-payment reform.

[12] We simply assume this is the case when we derive the effective marginal price in the next section. We need this assumption to prevent the theoretical option that individuals leave the nursing home once they have to start paying reach the high co-payment. This is something we do not expect given the high care needs of residents and indeed, do not observe in the data. Potential ways to formalize this in the theoretical model are the inclusion of habit formation or sufficiently high transition costs when moving in and out of a nursing home.

to enter a nursing home now, or extend the period living at home by at least one month.

3.2 Effective net price

Thus far, we have not specified p_{it}. The price is influenced by two aspects specific to our context. First, the reform also affected the private price for home care. A large majority of individuals at the margin of using nursing home care already use home care (see figures in Section 6.4). Therefore, the relevant choice is whether to go to the nursing home now *or* stay at home longer using home care. To capture that the reform affected the private price of these two alternative types of care (and nothing else), we include the private price of home care in p_{it} rather than in u_{it}. That is, we define p_{it} as the net price: the difference between the private prices of nursing home care and of an equivalent amount of home care (Section 5.2 explains how this amount is determined).

Second, the co-payment for nursing home care has two price regimes, as explained in Section 2: a low-rate co-payment in the first 6 months of use and a high-rate co-payment for any use after that. This means that there is a difference between the *spot price* (the low co-payment paid in the first months of nursing home use) and the *effective marginal price* (the impact of entering the nursing home now, instead of a period later, on expected lifetime private payments). The effective marginal price is a weighted average of the net price p_{low} under the low co-payment and p_{high} under the high co-payment regime, where the weight depends on the probability of living long enough to be exposed to the high co-payment regime:

$$\mathrm{E}(p_{it}) = p_{low} + S_{it}^{t+6}(p_{high} - p_{low}), \tag{2}$$

where t now defined in months, for notational convenience, and $S_t^{t+\tau}$ is the probability of survival from t to $t+\tau$. If S_t^{t+6} is large, then the effective marginal price is almost equal to the high co-payment.

3.3 Empirical application

In the main empirical analysis, we estimate the average effect of the increase in the net price of nursing home care induced by the reform: $p_{it}^{post} > p_{it}^{pre}$, on the total number of months of care use. We can write this effect as:

$$\sum_{i=1}^{N}\sum_{t=0}^{T} S_{it}\left(\mathrm{I}[u_{it} > p_{it}^{post}] - \mathrm{I}[u_{it} > p_{it}^{pre}]\right), \tag{3}$$

where $I[.]$ is an indicator function. Furthermore, we assume that changes in nursing home entry do not affect survival (which we test empirically). Hence, we can interpret Equation (3) as the average delay in nursing home entry due to the reform. In addition, we estimate the effect of the reform on use in each individual month since eligibility.

For some individuals, delaying nursing home entry means not going to the nursing home at all. To identify this effect, we also estimate the change in the percentage of individuals that use any nursing home care.

Furthermore, we expect that individuals who are confronted with a larger increase in the net price due to the reform postpone their use more than individuals confronted with a smaller increase. Because we cannot identify whether individuals respond to the effective price or to the spot price, we model the relation between the response and the change in p^{high}, selecting the functional form of this relation based on the fit with the data.[13]

4 Data and sample

4.1 Administrative data

We use administrative register data on the *use* of long-term care from CAK. For each stay in an institution, we know the date of admission, the type of care to be received (elderly care, long-term psychiatric care or care for the handicapped), the care intensity and the date of discharge, if applicable.

We link these individual-level data to background information from other administrative data sets using pseudonymized individual and household identifiers. First, we link eligibility decisions from CIZ on *eligibility* for institutional care and home care. Second, we link information on the age, date of death (if applicable), gender, marital status, household composition, children who are alive, and municipality of residence from the population register (*Basisregistratie Personen*). Third, we add data from the Tax Office on household income and wealth as well as data about the children's employment and personal primary income. Fourth, we add claims data from the mandatory social health insurance scheme, which pays for the majority of spending on medical care in the Netherlands, collected by Vektis.

[13] As the low and high co-payments are strongly correlated, using the high price sufficiently captures the heterogeneity in treatment within the population. However, not being able to distinguish between responses to the spot price and the effective price means that we have to be careful in interpreting the size of the price-treatment interaction term and in policy recommendations regarding changes to the co-payment scheme.

4.2 Information on co-payment schedules

Our empirical strategy relies on the simulation of co-payments each individual would pay for institutional care and home care, based on individual data and legal rules. Two elements make us confident in our ability to approximate the price increase due to the reform in a satisfactory way. First, we retrieved information on the co-payment schedule from public CAK documentation and legislative work around the 2013 reform. When unsure about the value of specific parameters, we consulted a policy advisor from CAK. Second, co-payments and their increase with the 2013 reform mainly depend on specific tax concepts of income and wealth that we are able to proxy using administrative registers based on data from the Tax Office. We use data on income and wealth from two years earlier, following CAK rules.[14]

4.3 Study population: individuals eligible for nursing home care for the first time

We use four criteria to define the study population. First, we focus on on individuals who become eligible for a nursing home stay for a somatic or psycho-geriatric condition between January 2009 and December 2014.[15] Second, we focus only on those who become eligible *for the first time*. An individual is included if they were not eligible for or have used any institutional care, including not only nursing home care, but also care in assisted-living facilities, post-acute rehabilitation centers and hospices beforehand or at any point between 2009 and 2014. Third, we select individuals who are at least 66 years old, i.e. who have reached the statutory retirement age.[16] Fourth, we keep individuals who had no tax partner already as of two years before first eligibility, whom we call *singles*. We drop individuals in multiple-person households because the co-payments rules are different for them. In 2013, 63% of older adults admitted to a nursing home for the first time were singles. We end up

[14] In Appendix D.5, we conduct an assessment of the quality of the simulations. We use an external sample with observational data on co-payments. The results should be read with caution, as there are a number of reasons why the observed co-payments can diverge from the nursing home price we aim to simulate. Abstracting from this caveat, this comparison with these co-payments suggests that the simulations offer a good approximation of the co-payments paid for a majority of individuals, although the simulations and the observational data differ significantly for a minority.

[15] Going back further in time is not possible because information on eligibility is not available prior to 2009. We also do not to extend the analysis to more recent years as the entire long-term care system, including the eligibility processes, was substantially reformed in 2015 (Maarse & Jeurissen, 2016).

[16] Rebates for long-term care co-payments that users are entitled to depend on whether they have reached the statutory retirement age.

with a study population of 79,559 individuals.[17]

4.4 Outcome variables

We define two sets of outcomes. The first set consists of two measures of nursing home use: dummy Use_{it} indicates whether an individual i has used any nursing home care within the 12 months following the day of their first eligibility, which took place in month t. Second, $Duration_{it}$ equals the number of days spent in a nursing home within 12 months after t.[18] In Appendix D.4 we study whether the reform only affects use within the first 12 months by estimating the effects of the reform by month since eligibility.

The second set consists of outcomes that shed light on the broader impact of co-payments on social welfare. These include the 2-year survival of the person who is eligible for a nursing home admission, as well as their expenditure on medical care and home care in the first two consecutive *calendar* years after eligibility.[19] Moreover, we study a number of outcomes for the most likely potential caregivers: the children of the individual. For these children, we analyze the impact of the nursing home price change on their health and income, proxied by the average medical care expenditures, and the average primary income in the calendar year after first eligibility.

4.5 Descriptive statistics

Table I provides descriptive statistics on the study population. There is substantial variation with respect to income and wealth in the sample. About a quarter of the population is a home owner. Furthermore, the majority of individuals are women, aged between 80 and 95 years-old. 42% of the population are eligible for nursing home care because of a somatic condition, 23% have a psycho-geriatric condition (dementia), 35% has both. Most individuals are eligible for care packages 4 to 6. One-third is eligible for care package 5, which is for patients with marked symptoms of dementia. Becoming eligible for the first time and being assigned care packages 7 or 8, which indicate severe care needs, is extremely uncommon: most in-

[17] Appendix C provides further details on sample selection.

[18] When computing these variables, we exclude the stays in specialized institutions such as the psychiatric hospitals and the centers for the mentally handicapped. However, our outcomes include any use of institutional elderly care, encompassing (i) nursing home care but also (ii) stays in assisted living facilities, (iii) hospice care and (iv) rehabilitative care. These 4 types of care cannot be disentangled in the data prior to 2011. In our study population, stays of types (ii) to (iv) make about 7% of admissions. For simplicity, we refer to our outcomes as capturing *nursing home* care use.

[19] Medical care spending is recorded by calendar year.

dividuals become eligible for nursing home care before reaching such a deteriorated health and functional status. Finally, about half of the sample dies within the 2 calendar years after becoming eligible for nursing home care.

About 80% of those who become eligible ends up in a nursing home, yet only a small number enters the nursing home immediately after the eligibility decision. Figure 2 indicates the timing of a nursing home admissions. After 10 days, 21% of the study population has entered a nursing home. This proportion increases to 33% after 30 days, and then slowly increases before plateauing at around 74% after 11 months. This pattern may arise because individuals and their families can choose between a faster admission and an admission to the preferred nursing home. Furthermore, this figure shows that most of the admissions take place within one year after the eligibility decision, which suggests that we capture all effects within the period during which people are followed. The unconditional duration spent in a nursing home was 164 days, with a standard deviation of 139 days.

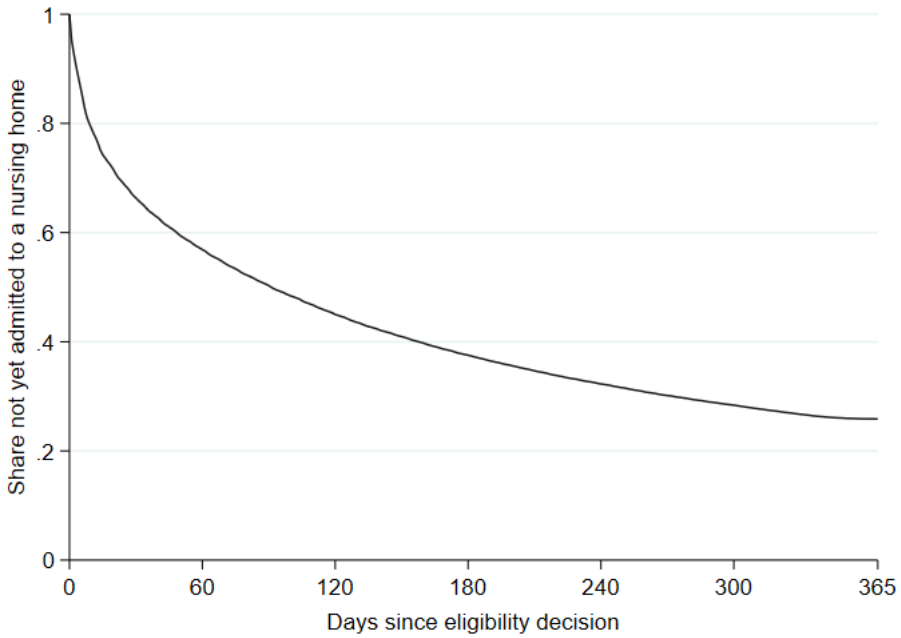
Table I: Descriptive statistics: Study population of first-time eligible.

Variable	Mean	Standard deviation
	(1)	(2)
Outcomes		
Any stay in a nursing home	0.805	
Time spent in a nursing home	163.888	138.693
2-year mortality	0.417	
6-month mortality	0.185	
Nursing home care expenditures[a]	41250.115	36985.607
Home care expenditures[a]	14436.247	19015.721
Medical care expenditures[a]	15565.059	21263.267
Hospital care expenditures[a]	7931.738	12483.386
Total care expenditures[a]	71251.422	41380.805
Children's median medical care expenditures[b]	2355.365	5527.761
Children's median income[c]	32533.400	35247.020
Covariates		
First eligibility: in 2009	0.130	
First eligibility: in 2010	0.153	
First eligibility: in 2011	0.153	
First eligibility: in 2012	0.154	
First eligibility: in 2013	0.196	
First eligibility: in 2014	0.214	
Home owner	0.261	
Disposable income	19237.571	9749.904
Total wealth (net)	142654.394	355250.420
Gender: woman	0.779	
Age: 65-74	0.080	
Age: 75-79	0.126	
Age: 79-84	0.252	
Age: 85-89	0.304	
Age: 90-94	0.184	
Age: 95+	0.053	
Number of children: 0	0.202	
Number of children: 1	0.152	
Number of children: 2	0.259	
Number of children: 3+	0.387	
Has a daughter	0.627	
Care package upon first eligibility: 4	0.381	
Care package upon first eligibility: 5	0.349	
Care package upon first eligibility: 6	0.243	
Care package upon first eligibility: 7	0.018	
Care package upon first eligibility: 8	0.009	
Eligibility because of dementia	0.229	
Eligibility because of somatic condition	0.417	
Eligibility because of both somatic condition and dementia	0.354	

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

NOTES TO TABLE I: Unless specified otherwise, computed for the full study population; [a]: Computed for the sub-population with health insurance claims in the year of their first eligibility and who become eligible for nursing home care prior to 2014; care expenditures are summed over the year of first eligibility and the following year (N=60,610); [b]: Computed for the sub-population with at least a child alive in the year following eligibility and for whom children's health-insurance claims could be retrieved (N=62,900); [c]: Computed for the sub-population with at least one child alive in the year following eligibility and for whom the children's income could be retrieved (N=62,830).
Stay and time spent in a nursing home: within the 12 months following first eligibility. Income: annual, of two years before, in current euros. Wealth: of two years before, in current euros. Medical care expenditures in 2013 constant euros. Home care expenditures are computed multiplying the hours of home care of each type times the hourly tariff for this type of care in 2013. nursing home care expenditures are computed multiplying the days spent in a nursing home or rehabilitative or palliative care facility with a given care package times the day tariff for this care package in 2013. Children's medical care expenditures and income: annual, in the year following eligibility, in current euros.

Figure 2: Timing of first nursing home admission following the day of first eligibility.



STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).
NOTES: Kaplan-Meier plot. Observations are censored on the date of death.

5 Empirical strategy

5.1 A difference-in-differences approach

We estimate the effect of the co-payment reform on the use of nursing home care. This reform (only) increased the net price p for an additional month in a nursing home for some individuals. A simple before-after comparison of co-payments and nursing home care use would provide a biased estimate of the causal effect because of time trends in nursing home care use (due to e.g. cultural or institutional shifts) and in the costs and benefits of living in a nursing home (e.g. the cost of living outside a nursing home may have increased). Therefore, we compare the change in nursing home use between individuals who were affected by the reform and those who were not.

5.2 Definition of the treatment group and of the treatment intensity

The treatment group consists of individuals whose net price for nursing home care increases as a result of the reform. To identify this group, we calculate the net price under the pre-reform co-payment rules (p_i^{pre}) and the post-reform rules (p_i^{post}) for each individual.[20] We do this by applying the co-payment rules explained in Section 2.2 to the information on the individual's income and wealth.

As explained in Section 3, the price depends on the expected survival until the first month under the high-rate regime for nursing home co-payments. As most individuals survive up to at least six months following first admission (Table I), we use the monthly high-rate co-payment (rather than the low-rate co-payment) as the price that individuals and their families take into account when deciding about the timing of the nursing home admission. To compute co-payments on home care (which depend on the care volume), we use the care package the individual is entitled to receive in the nursing home and retrieve the number of hours of personal care, nursing care, and guidance that the care package contains.[21] [22]

For individual i, the treatment intensity Δ_i is the difference between the post-

[20] The net price is defined as the difference between the monthly co-payments for nursing home care and for home care, cf. Section 3.

[21] The care packages are described in Appendix A.

[22] The results are robust to assuming that individuals trade off between a nursing home stay and staying at home with a *lower* (rather than equivalent) volume of home care. See Appendix D.1.

reform price and the pre-reform price (expressed here in hundred euros per month):

$$\Delta_i = (p_i^{post} - p_i^{pre})/100 \tag{4}$$

The treatment group consists of all individuals whose net price for nursing home care is higher under the post-reform than under the pre-reform rules:

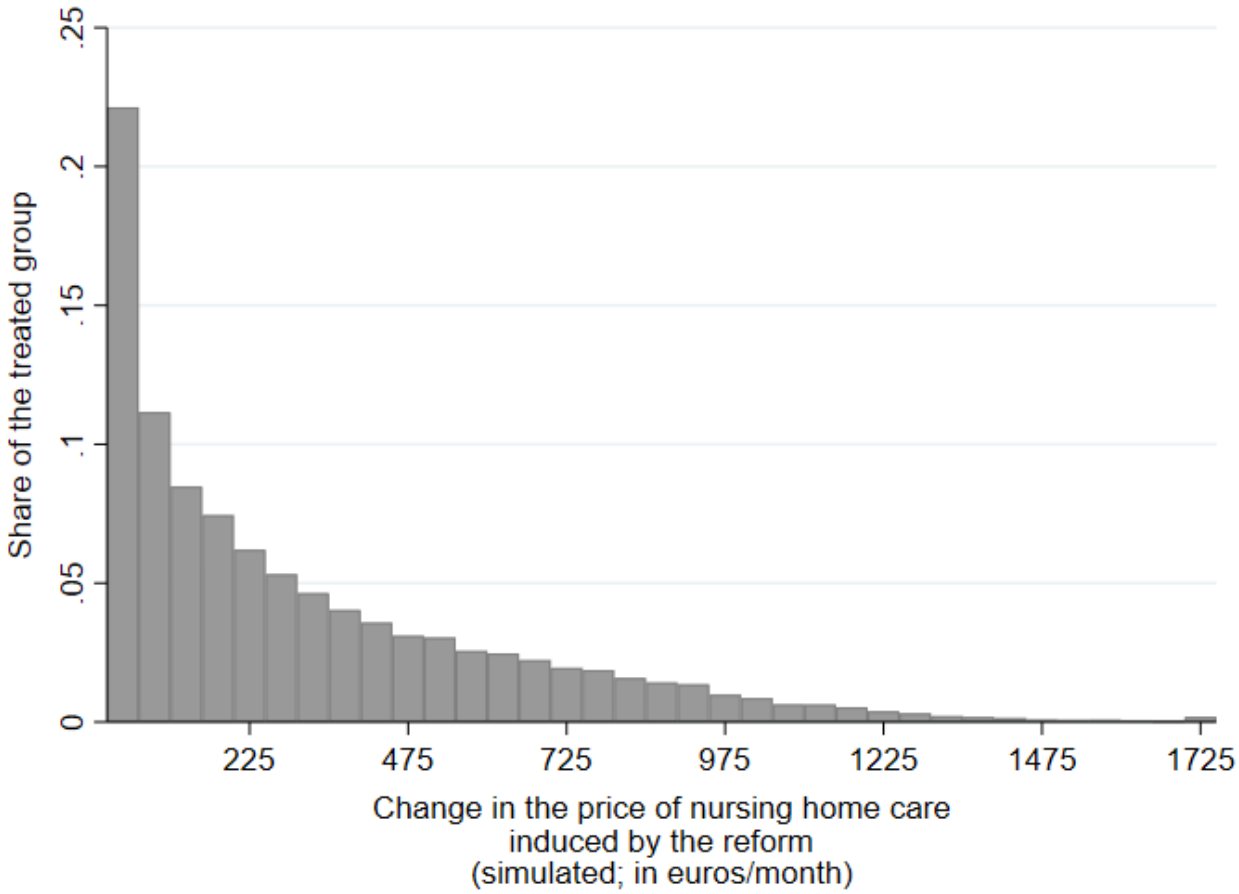
$$Treat_i = 1 \iff \Delta_i > 0 \tag{5}$$

5.3 Effect of the reform on nursing home price: descriptive statistics

The treated group consists of the 37% (N=29,380) of the study population. The remaining 63% of the population have income and wealth such that the co-payment is exactly the same under the pre-reform and the post-reform co-payment rules. They form the control group (N=50,179).

Within the treated group, there is considerable variation in the change in the price for nursing home care induced by the reform. Figure 3 plots the distribution of the change in the monthly price. The median increase induced by the reform is around €200 per month. For 25% of the treated group, the increase is less than €60 per month, while 5% of the treated group faces an increase of at least €970 per month. In relative terms, the increase was substantial for many individuals: the median increase is 18% within the treated group and the increase exceeds 100% for 2.5% of the treated group. Yet, 25% of the treated group face an increase of less than 6%.

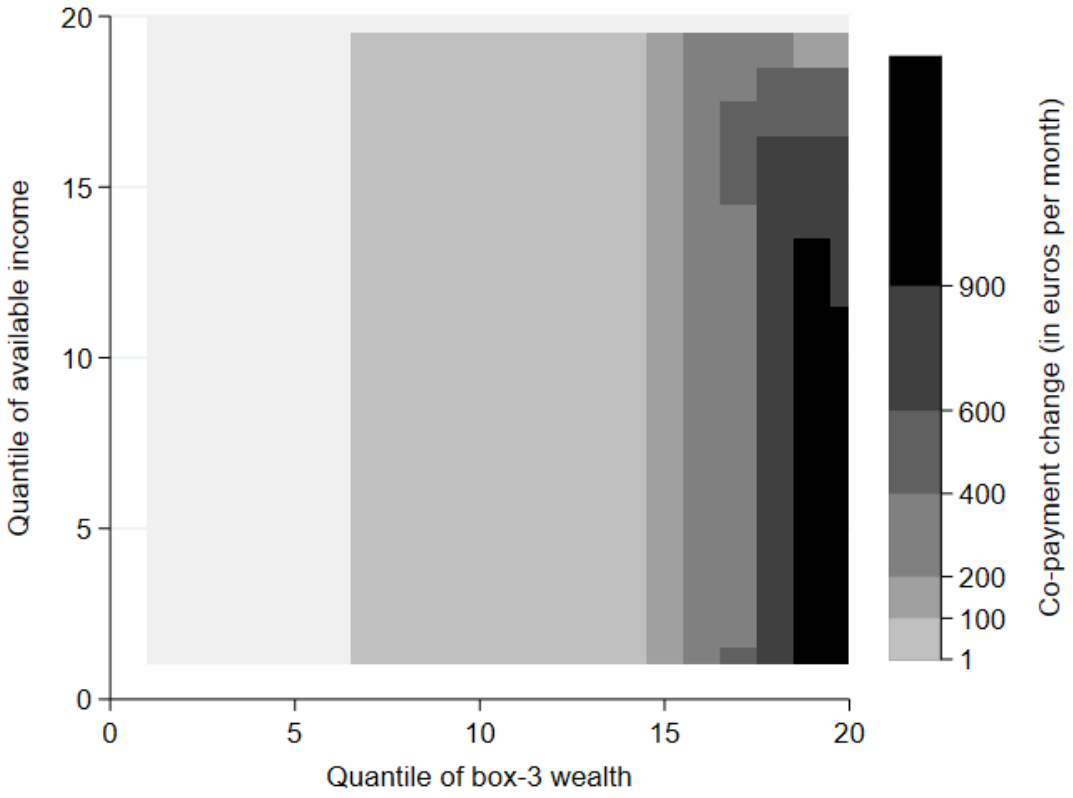
Figure 3: Change in the price for nursing home use induced by the reform within the treated group (euros per month).



STUDY POPULATION: Individuals aged 66+ who are single became eligible for nursing home care for the first time between 2009 and 2014 and whose price for nursing home care increases because of the co-payment reform (N=29,380).
NOTES: Authors' simulations. The bin width is €50. The values represent the change in the difference between co-payments for nursing home care and co-payments for an equivalent amount of home care.

Figure 4 displays the change in the average monthly co-payment for nursing home care (in euros) by income and wealth. Individuals with higher financial wealth experienced larger increases in the co-payment. Consistent with Figure 1, individuals with lower than median (financial) wealth are not affected by the reform. The largest increase in price was experienced by those in the top 10% of the wealth distribution with a low to average income. Being in the treated group thus hinges primarily upon one's wealth, but treatment intensity also correlates with income.

Figure 4: Simulated change in the price for nursing home care induced by the reform (euros per month), depending on income and wealth.



STUDY POPULATION: Individuals aged 66+ who are single and became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Box-3 wealth corresponds to wealth as taken into account in the co-payment schedule.
NOTES: Authors' simulations. The values represent the change in the difference between the high-rate co-payment for nursing home care and co-payments for a fixed amount of home care.

5.4 Difference-in-differences specification

We estimate the following difference-in-differences equation:

$$Y_i = \beta_0 + \beta_1 Post_i + \beta_2 Treat_i + \beta_3 Post_i \times Treat_i \times \Delta_i + X_i\theta + u_i \quad (6)$$

Drawing upon Duflo (2001), Equation (6) extends the basic difference-in-differences specification by including treatment intensity Δ_i. Y_i is the outcome for individual i who becomes eligible for nursing home care for the first time in a given month of a given year. $Post_i$ is a dummy equal to 1 if i becomes eligible after the reform was announced (April 2012).[23] $Treat_i$ is a dummy equal to 1 if the individual belongs to the group affected by the reform, X_i' is a vector of individual characteristics (at the time of first eligibility) and u_i an individual error term.[24]

Coefficient β_3 in Equation (6) captures the effect of a one hundred-euro change in the net out-of-pocket price for an additional month in the nursing home. When the outcome is Use, β_3 captures the price sensitivity of nursing home care use at the extensive margin; when the outcome is $Duration$, this coefficient reflects by how much the price of nursing home stays delays a nursing home admission.[25] We expect β_3 to be zero or negative: as treated individuals are exposed to an *increase* in the price of nursing home care, they should, if anything, lower their use of care compared to the control group.

All models are estimated by Ordinary Least Squares (OLS) and we report robust standard errors. Equation (6) assumes a linear effect of the price change on the outcomes.[26]

5.5 Identifying assumptions

The internal validity of the estimates relies on the unconfoundedness assumption: the control group provides a valid counterfactual for the evolution of nursing home care use in the treated group, were the reform not have been implemented (parallel

[23] We assume here that individuals are forward-looking: an individual who becomes eligible for nursing home care after the reform was announced can anticipate that the private price of nursing home care depends on the post-reform co-payment rules.

[24] In this setup, there is one observation per individual because of the definition of our study population and the definition of the outcomes. Hence, we do not include any time subscript for the month or year at which the individual becomes eligible.

[25] As explained in Section 3, this interpretation means that we assume that (i) residents do not exit the nursing home and (ii) there is no mortality effect of the reform. We test the latter assumption later on. Regarding the former, among individuals in our study population who had at least a stay in a nursing home within 12 months after becoming eligible, only 6.5% experience a lapse of more than 7 days between two recorded stays.

[26] In Section 7.3 we explore further the appropriate functional form of this relationship.

trend assumption - PTA).[27] As explained in Section 2, the rich and the poor in the Netherlands make use of the same social long-term insurance scheme and use the same nursing homes; quality differences are limited and the co-payments are the same for all providers.

Still, in the context of our study, we see three reasons why the PTA might be challenged. First, the composition of the control group relative to the composition of the treated group may change around the time of the reform. This might be due to determinants of nursing home care unrelated with the reform, e.g. changes in age composition or income and wealth across cohorts. Second, other policy changes affecting the control and treated groups differently may have occurred around the same time as the co-payment reform. Finally, the PTA could also be challenged if treatment status and intensity were endogenously determined. The magnitude of the co-payment change caused by the reform depends primarily on a specific part of an individual's wealth. The relevant wealth is that of 2 years before the admission, which limits the scope for such behaviors; but we cannot *a priori* rule out strategic and forward-looking individuals could anticipate their care needs and reallocate their assets to lower their financial wealth and, thus, their co-payments.

We deal with these concerns in four ways. First, we include a rich set of controls including gender-specific age categories, the care package that the individual is initially eligible for, whether the individual was eligible for a nursing home admission because of a psycho-geriatric condition, a somatic condition, or both. We also include the number of children alive (4 categories) and whether the individual has a daughter alive at the beginning of the year, to proxy for potential supply of informal care. Furthermore, we include a dummy for whether the individual is a home owner, wealth quintiles and 20 5-percent disposable income groups to capture potential wealth and income effects on nursing home use (apart from the price effect a higher co-payment may induce).[28] In addition, we control for which of the 32 long-term care provision regions the individual lives in. Finally, we include quarter-year fixed-effects, referring to the quarter in which the individual becomes eligible, that pick up any unobserved time shock in terms of nursing home demand or supply.[29]

Secondly, we examine pre-trends. Although parallel trends in nursing home

[27]The concerns about the impact of differential timing on difference-in-differences estimates as voiced by Borusyak & Jaravel (2017); Goodman-Bacon (2021) and others do not apply to this set-up because the reform is implemented at one point.

[28]One shortcoming of including income and wealth as controls is that they correlate strongly with treatment intensity and thus reduce the statistical precision of the estimates.

[29]We also checked the robustness of our estimates to the inclusion of health-care spending and home-care entitlements in the year prior to the eligibility decision. These variables can help to control for changes in the underlying health status in the prior calendar year. Estimates are not reported here but are extremely close to those from our preferred specification.

use between the control group and the treated group before the reform are not a sufficient nor a necessary condition for the PTA to hold (Roth, 2019), observing non-parallel pre-trends even after controlling for a rich set of control variables may help pinpointing a deviation from the PTA. For this, we perform an F-test on the coefficients from a dynamic DiD specification for the pre-reform quarters (presented in next subsection).

As a third step, we investigate the role of the other contemporary policy change that may also have affected nursing home care admission rates. Between 2011 and 2014, stricter eligibility criteria for admissions to elderly institutional care were implemented in order to decrease institutionalization rates among the Dutch older people. This measure resulted in a decreasing number of positive eligibility decisions during the study period. We show in Section 7 that this reform did not to interfere directly with the co-payment variation that we use, and thus does not to bias our results, to the extent that individuals subject to a higher co-payment increase did not become more (or less) likely to become eligible for nursing home care than individuals less affected by the co-payment reform.

In a fourth step, we examine the evolution of the wealth that is taken into account for co-payments prior to first eligibility. Results are shown in Section 7 and support the hypothesis that individuals did not react to the reform by decreasing their wealth position in the years preceding their first eligibility for nursing home care.

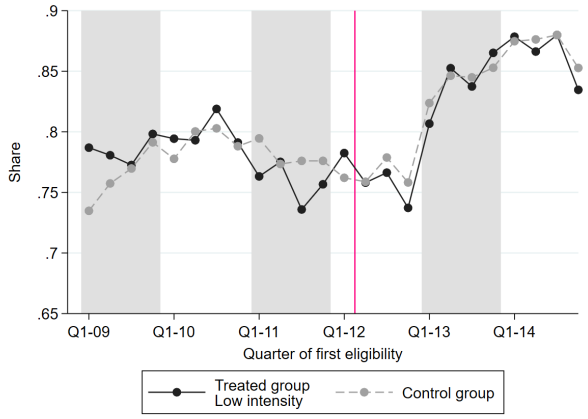
5.6 Raw trends

Figures 5 and 6 provide graphical evidence that trends in nursing home care use were the same for the treated and control groups. The four panels in Figure 5 plot nursing home care use by quarter of first eligibility for the control group and treated group, splitting the latter between (i) the subgroup subject to a low-intensity treatment (defined as a nursing home price increase lower than the conditional median increase) and (ii) the subgroup subject to a high-intensity treatment (increase higher than the conditional median increase). Panels A and B (resp. Panels C and D) display the probability of any nursing home care use (resp. the number of days spent in a nursing home).

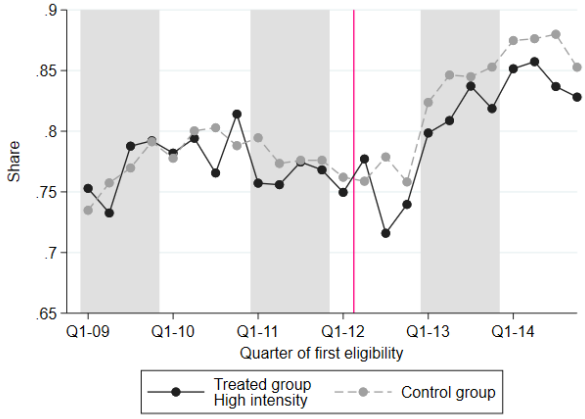
Figure 5 show a clear upward trend in the probability of nursing home care use and the duration of the stay after 2012. As explained above, these trends are likely related to the other major reform that tightened the eligibility criteria for nursing home care. That is, a higher eligibility threshold means an increase in the severity of health problems and functional limitations of the newly eligible and thus a shift

in the costs and benefits of postponing a nursing home admission by a week or a month (cf. Section 3). For our identification strategy it is important to note that the pre-reform trends and even the levels, including the major increases, are virtually identical for the treatment and control groups,[30] suggesting that there were no initial differences in use between both groups and that the other reform had a similar impact on both groups. Figure 5 shows that this is indeed the case.

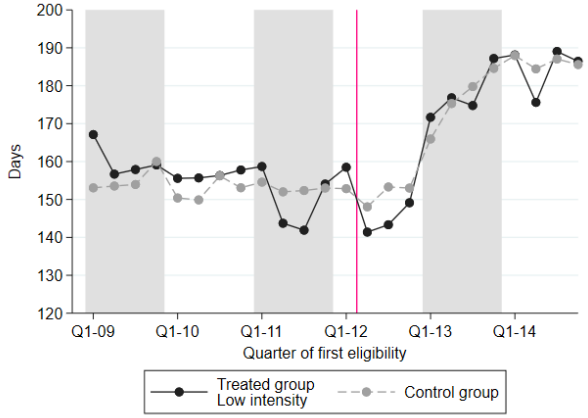
Figure 5: Use of nursing home care in the 12 months following first eligibility, depending on treatment intensity, by quarter of first eligibility.



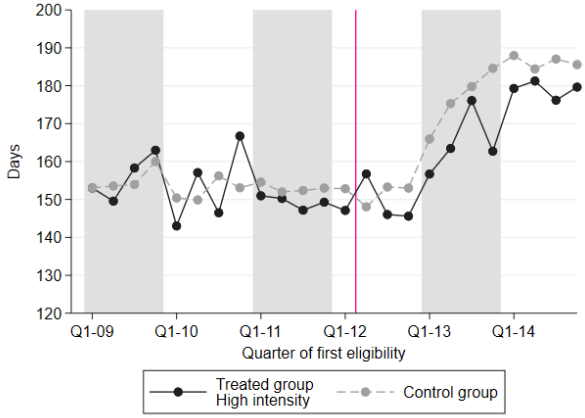
Panel A: control group versus low-intensity treated group. Outcome: probability of nursing home use.



Panel B: control group versus high-intensity treated group. Outcome: probability of nursing home use.



Panel C: control group versus low-intensity treated group. Outcome: days of nursing home use.



Panel D: control group versus high-intensity treated group. Outcome: days of nursing home use.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

[30] As argued in Roth (2019); Kahn-Lang & Lang (2019), the fact that the control and the treated groups show parallel pre-trends is a more compelling indication that the two groups experience similar determinants of the outcomes and shocks when large variations in the outcome are observed.

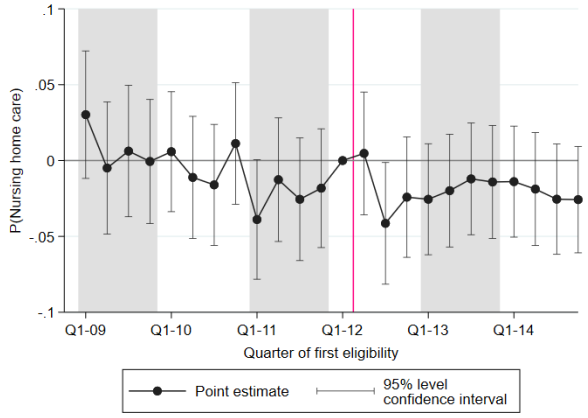
In addition, we provide statistical evidence that the evolution of nursing home care use did not differ in the treated and control groups before the reform. Figure 6 shows the point estimates from dynamic DiD analyses, in which the outcome is regressed on quarter dummies, the treatment status and interaction terms between quarter and treatment status. The first quarter of 2012 (i.e. the last quarter before the reform is announced) is chosen as the reference period. Both panels show that the change in the difference in average nursing home use between the treated group and the control group compared to the difference in the first quarter of 2012 is not significant in any of the pre-reform periods. Nor is there a clear pattern in the trend. A joint test of statistical significance (Fisher test) on the coefficients for the pre-treatment periods confirms that we cannot reject the null hypothesis that the coefficients are all zero: the p-values are 0.31 (probability of use), 0.30 (duration of use).[31]

Panels A and C of Figure 5 show that *after* the announcement of the co-payment reform nursing home care use have stayed fairly similar in the control group and among those who are subject to a limited increase in the nursing home price. By contrast, nursing home care use has been substantially lower for individuals subject to a substantial increase in the nursing home price compared with the control group (Panels B and D). These statistics point towards an effect of the co-payment increase, which we provide evidence for in next section.[32]

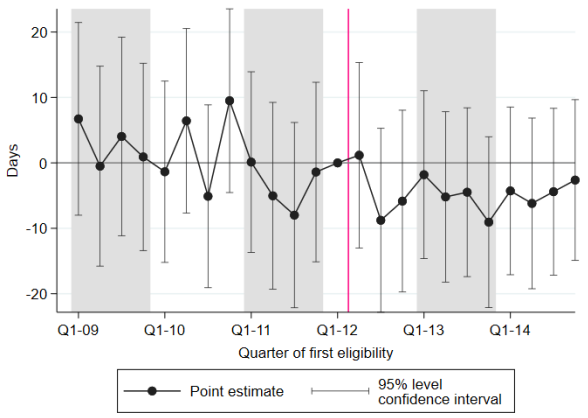
[31] As shown in Appendix D.2, the trends after controlling for covariates are largely identical. We also implement a Fisher test for the joint significance of pre-reform interaction terms between each quarter and the price change. This amounts to examining the pre-trends for our preferred specification (Equation 6). These tests are implemented for all the outcomes we consider in the analysis (nursing home care use, mortality, health care expenditures, children's outcomes). Results are presented in Table D.II to D.V, in Appendix D.2. They show no evidence of diverging pre-trends.

[32] Figure **??** confirms that nursing home care use was lower after the reform in the treated group than in the control group, relative to the difference in use just prior to the reform. Virtually all point estimates are negative, although non statistically significantly different from 0 at the 5% level. Pooling quarters together and taking explicitly into account the magnitude of the price increase will help gaining statistical power.

Figure 6: Use of nursing home care in the 12 months following first eligibility: difference between the treatment group and the control group by quarter of first eligibility.



Panel A: difference between treated and control group, compared with this difference in Q1-2021. Outcome: probability of nursing home use.



Panel B: difference between treated and control group, compared with this difference in Q1-2021. Outcome: days of nursing home use.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

NOTES: The panels provide the estimates from a dynamic DiD specification at the quarter level without covariates: each dot shows the estimate of the difference in the outcome between the treated and the control groups, compared with that same difference in the first quarter of 2012.

6 Main results

6.1 The impact on nursing home admissions

Table II presents the estimate of the effect of the nursing home care price increase on the probability of any use (Columns (1) and (3)) and the number of days spent in a nursing home (Columns (2) and (4)) within the 12 months following first eligibility. The coefficient for the interaction between the treatment dummy and the dummy indicating that the individuals became eligible for nursing home care after the reform was announced ($Post.Treat$) from a basic difference-in-differences estimation provides the average treatment effect of the reform among the treated. The probability of a nursing home care admissions decreased by 1.15 percentage points after the reform for those who were exposed to an increase in the price of nursing home care compared to the control group. Furthermore, the treated group spent 4.426 days less in a nursing home, over the 12 months following eligibility - to be compared with an average conditional time spent in the nursing home higher than 28 months.

Table II: Difference-in-differences regression: baseline results.

Specification:	—— *Binary* ——		—— *Price* ——	
Outcome:	P(nursing home use)	Days in nursing home	P(nursing home use)	Days in nursing home
	(1)	(2)	(3)	(4)
Post	0.0694***	29.35***	0.0692***	28.84***
	(0.0100)	(3.400)	(0.00989)	(3.349)
Treat	0.0100*	2.710	0.00702	1.201
	(0.00553)	(1.833)	(0.00446)	(1.552)
Post.Treat	-0.0115**	-4.426**		
	(0.00577)	(2.000)		
Post.Treat.Δ			-0.00299***	-0.821**
			(0.00093)	(0.323)
Control variables	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	79559	79559	79559	79559
R^2	0.060	0.053	0.060	0.053

NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Study population: 66+ single first-time eligible for nursing home care between 2009 and 2014. Δ stands for a €100 increase in the net monthly price for nursing home care induced by the reform.

The price specification shows that a €100 increase in the co-payment is predicted to decrease the probability of entering a nursing home by 0.299%-pt (β_3 in Column (3) of Table II). The unconditional number of days spent in a nursing home decreases by 0.821 day (β_3 in Column (4)). We interpret this as evidence that a €100-increase

in the effective price for a month in the nursing home postpones an admission by slightly less than one day. Furthermore, the coefficient *Post* is positive: it reveals an upward trend in nursing home admissions conditional on eligibility over the study period, which is consistent with an increase in the severity of health problems and functional limitations at the moment of a positive eligibility decision caused by a higher eligibility threshold. The magnitude of the coefficient $Treat$ is close to 0: nursing home use prior to the reform was similar in the control and treated groups.

6.2 Heterogeneous effects

An increase in the price for nursing home care has a larger impact on nursing home care use for some groups than for others (Table III). While the differences in the probability of any admission in the first 12 months are limited, the differences among subgroups in the effect on the number of days in the nursing home are larger. In particular, those who are eligible for the lowest amount of care (Care Package 4) because of somatic health problems spent 1.861 fewer days in the nursing home for every 100-euro increase. That is, this group postpones the admission most. This decrease is more than twice as large as the full-study population estimate (0.821 days) and is in line with the hypothesis that the price sensitivity is higher for less severely disabled individuals because functional limitations induce a shift in individual preferences for nursing home care versus living at home. Moreover, men decrease their use twice as much as women in response to a 100-euro increase: -1.38 versus -0.71 days, but the difference is not statistically significantly different from 0.

Adult children, and daughters in particular, are likely to provide informal care, which - if combined with formal care - may increase the net utility associated with staying at home. However, we do not find differences between people with and without children, the number of children, whether at least one child is a daughter or by the employment status of their children.

Table III: Heterogeneity of effects: difference-in-differences estimates of co-payment increase on nursing home care use, by gender, characteristics of children and care needs.

Subpopulation:	*Outcome*: P(nursing home use)	Days in Nursing home	2-year mortality	N
	(1)	(2)	(3)	(4)
All	-0.0029	-0.8207	0.0020	79,559
	(0.0008)	(0.3160)	(0.0010)	
Men	-0.0031	-1.3761	0.0040	17,608
	(0.0017)	(0.6476)	(0.0022)	
Women	-0.0031	-0.7129	0.0014	61951
	(0.0010)	(0.3625)	(0.0012)	
Has children: No	-0.0028	-0.7663	0.0049	16,101
	(0.0016)	(0.6084)	(0.0020)	
Has children: Yes	-0.0032	-0.8737	0.0018	63,458
	(0.0010)	(0.3759)	(0.0012)	
All children work: No	-0.0036	-0.8490	0.0005	38,086
	(0.0014)	(0.5059	(0.0017)	
All children work: Yes	-0.0027	-0.8894	0.0042	25,372
	(0.0016)	(0.5632)	(0.0019)	
Care package 4, dementia	-0.0038	-0.6490	0.0049	17,010
	(0.0023)	(0.6820)	(0.0022)	
Care package 4, no dementia	-0.0032	-1.8609	0.0005	13,327
	(0.0023)	(0.7728)	(0.0026)	
Care package 5	-0.0026	-0.6963	0.0028	27,761
	(0.0014)	(0.5280)	(0.0018)	
Care package 6 to 8	-0.0027	-0.4992	-0.0005	21,461
	(0.0013)	(0.6299)	(0.0021)	

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

NOTES: Robust standard errors in brackets. Care package 4, dementia: individuals with moderate care needs and dementia upon eligibility; Care package 4, no dementia: individuals with moderate care needs and no dementia at eligibility; Care package 5: individuals with need of care tailored for people with dementia; Care package 6 to 8: individuals with severe to very severe care needs.

6.3 Mortality

The decrease in nursing home use is evidence of ex-post moral hazard in nursing home care but does not necessarily imply that the Dutch co-payment reform was welfare-improving, as explained in Section 1. Hence, we study three sets of additional outcomes: mortality, expenditures on medical care and home care, and spillover effects on potential informal caregivers, namely adult children.

Starting with mortality: for the treated group as a whole, we do not find strong evidence for an effect on 2-year mortality. As shown in Table III (p. 31), the point estimate is 0.002, which means that a 100-euro increase in the co-payment increases the mortality rate by 0.2%-point. This is small relative to the two-year baseline mortality rate of 0.5 ((0.4%). The estimate is also not statistically significant at the 5% level (although it is at the 10%-level).

The increase in co-payment causes a larger increase in the mortality rate for people with dementia who are eligible for the lowest amount of home care (+0.49%-point mortality); the point estimate also suggests an increase of 0.28%-point in the 2-year mortality for individuals with care package 5 (with marked dementia), statistically significantly different from 0 at the 5% level. This heterogeneity is not related to a larger change in care use as this group did not change their nursing home admission rate and duration of the stay more than the study population did on average. Hence, these results suggest that the mortality impact of postponing a nursing home admission is different for individuals with dementia compared with the average person in the study population.

For individuals with no child or with all their children working, a €100 increase in co-payment leads to a +0.4%-point probability of death within 2 years, while the point estimate is close to zero for individuals with at least a child who does not work. Recall that we find a similar decrease in nursing home care use for both groups. We interpret these results as evidence that the presence of potential informal caregivers may protect against an adverse health response to a postponement of a nursing home admission.

6.4 Health care expenditures

We find no evidence that the decrease in nursing home use induced by higher co-payments caused other adverse health effects or spillovers to other types of health care that undo the savings on nursing home expenditures. Total health care expenditures consist of expenditures incurred in the year of first eligibility for nursing home care and the following one, on nursing home care, home care and medical care.[33] The latter category includes, among other things, hospital care, GP care, and medication. As indicated in Table IV, total health care expenditures decrease by €306.7 because of a €100 increase in the co-payment for nursing home care in the year of first eligibility and the year thereafter. This is 0.4% of the average health care expenditures (€71,251). This decrease is almost entirely driven by the decrease in nursing home expenditure, by €-299.4.

The impact on home-care expenditures amounts to €51 and is not statically significant at the 10% level. Descriptive statistics show that 84% of the study population receive some home care in the 3 months prior to becoming eligible for nursing home care and 72% in the 12 months following eligibility. In both cases, home care users receive 1.1 hour of home care per day. The estimates are consistent with these descriptives: a €100 decrease in the price of nursing home care leads to postponing an admission by 0.8 day and increasing the volume of home care received by 0.6 hours (or about 0.8 hour for a day more out of the nursing home) within the 12 months following eligibility.[34]

There is no evidence that higher co-payments lead to a change in hospital-care expenditures and total medical-care expenditures in year of first eligibility and following one (Table IV). Neither estimate is statistically significant. Moreover, the estimates are very small (-€58 and -€43, respectively), both in absolute terms and when compared with the average medical care spending in the years of first eligibility and the next one (€7,931 for hospital care and €15,565 for total medical care). The effect on medical care spending may mean that there is no health effect and no substitution of medical care for nursing home care, or that these cancel out. All in all, the savings on nursing home expenditures caused by the response to the co-payment reform are not offset by higher expenditures on medical care or home care.

[33] For this analysis, we exclude individuals who become eligible for home care in 2014: because of a reform taking place in 2015, we cannot track the use of home care beyond 2014 in a consistent way.

[34] These additional results on home-care hours are presented in Appendix D.1.

Table IV: Impact of the price of nursing home care on medical care and long-term care expenditures.

Outcomes:	Nursing home care costs	Home care costs	Medical care costs	Hospital care costs	Total care costs
	(1)	(2)	(3)	(4)	(5)
Post	-20355.6***	5531.0***	9399.2***	2112.4***	-5425.3***
	(1000.6)	(441.3)	(507.0)	(301.9)	(1099.2)
Treat	772.4*	-335.4	152.4	89.73	589.5
	(457.1)	(250.8)	(256.4)	(144.0)	(523.9)
Post.Treat.Δ	-299.4***	50.80	-58.06	-43.52	-306.7***
	(103.2)	(53.16)	(54.81)	(31.87)	(115.1)
Control variables	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	60610	60610	60610	60610	60610
R^2	0.120	0.042	0.101	0.112	0.063

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2013, and who were found in the health insurance claim data in the year of their first eligibility (N=60,610).

NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Δ stands for a €100 increase in the net monthly price for nursing home care induced by the reform. Expenditures incurred in the year of first eligibility and in the following one. Medical care expenditures are expressed in 2013 euros based on CBS Consumer Price Index. nursing home care expenditures are computed multiplying the days spent in a nursing home with a given care package by the day tariff associated with this care package in 2013. Home-care expenditures are computed multiplying the number of hours of home care of a certain type received by the associated hourly tariff in 2013. Expenditures include public and private expenditures (including co-payments on long-term care and the mandatory deductible for medical care). Expenditures on nursing home care also include expenditures on other institutional elderly care (rehabilitative and palliative care, i.e. care packages 9 and 10).

6.5 Effects on potential informal caregivers

For the sub-population who have children, we find no effect of an increase in medical care expenditures of the children in the calendar year after the parent became eligible for a nursing home admission. This means that there is no evidence that any increase in informal care has had a negative impact on the health of this main group of potential informal caregivers. Similarly, there is no evidence of an effect on the primary income of the children. The latter finding is in line with previous research documenting that, in the Netherlands, a parental hospitalization has no effect on the labor market outcomes of the children (Rellstab *et al.*, 2020).

Table V: Impact of the price of nursing home care on children's medical care spending and income.

Outcome:	Children's medical care spending	Children's income
	(1)	(2)
Post	115.2	3031
	(153.5)	(826)
Treat	-121.1*	1235***
	(66.26)	(412)
Post.Treat.Δ	-18.88	73.45
	(13.16)	(114.0)
Control variables	Yes	Yes
Quarter fixed effects	Yes	Yes
Observations	62,900	62,830
R^2	0.007	0.143

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014, with at least a child alive.
NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Δ stands for a €100 increase in the net monthly price for nursing home care induced by the reform. Outcomes are defined as the average across all children who are alive, in current euros.

7 Robustness checks

7.1 No effect on the probability of becoming eligible for nursing home care

By focusing on the individuals who become eligible for nursing home care, we assume that the reform did not affect the probability to apply for or be granted eligibility or that it coincided with an external change in this probability. Such a change could happen for two reasons. First, the eligibility assessment process may have changed in terms of assessments and eligibility decisions. Specifically, during the study period the eligibility for assisted-living facilities was phased out. Although

the independent assessment should in theory prevent this, the phasing out of this type of institutional care might have led to upcoding, increasing the group being eligible for (more intensive) nursing home care. As individuals with low income and wealth might be less able to substitute institutional care and home care than the rich (Tenand *et al.*, 2020), this upcoding could potentially affect the treated and the control group differently. Second, those who are subject to an increase in the nursing home price may become less inclined to apply for eligibility for nursing home care. To check these two channels formally, we implement a DiD approach which is similar to the baseline analysis but has eligibility as the outcome.

In this analysis, the outcome is $Elig_{it}$, an indicator for becoming eligible for nursing home care at time t:

$$Elig_{it} = \gamma_0 + \gamma_1 Treat_i + \gamma_2 Post_{it} + \gamma_3 Post_{it} \times Treat_i \times (p_i^{post} - p_i^{pre})/100 + \mu_t + X_{it}\delta + \epsilon_{it} \quad (7)$$

We estimate Equation (7) on the 66+ population of singles who are not yet eligible for nursing home care before time t.[35] The average share of individuals becoming eligible for nursing home care was 0.01 per year over the study period. We control for age (in categories), gender and their interaction, as well for home ownership, wealth quintile and disposable income ventile. Standard errors are clustered at the individual level.

A €100 increase in the nursing home price is associated with a (precisely estimated) zero decrease in the probability of becoming eligible for nursing home care (Table VI): the co-payment increase did not deter individuals from applying. This finding justifies the focus on the study population of individuals who are eligible for nursing home care.

[35] The panel is unbalanced: as soon as an individual i becomes eligible, they are dropped from the sample in $t+1$.

Table VI: Effect of the price of nursing home care on the probability to become eligible for nursing home care: difference-in-differences estimates.

Outcome:	P(become eligible for NH)
Post	0.000
	(0.000)
Treat	-0.001***
	(0.000)
Post.Treat.Δ	0.000
	(0.000)
Controls (age, gender)	Yes
Quarter fixed effects	Yes
Observations	39,651,698
R^2	0.001

Study population: 66+ and single in the years 2009 to 2014, not eligible for nursing home care at least up until the month of observation.

NOTES: Regression at the monthly level. Standard errors in parentheses, clustered at the individual level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.Δ stands for a €100 increase in the monthly price for nursing home care induced by the reform.

7.2 No evidence of strategic wealth reallocation

Treatment status and intensity depend on individual income and wealth. One concern might be that, in response to the reform, individuals strategically decreased their wealth to avoid the higher co-payments. Co-payments are computed based on the income and wealth of 2 years earlier, which limits the scope for such a behavior. Still, some individuals might be aware of having an increased risk of nursing home use in the years prior to actual admission and decrease their wealth as a precaution. In particular, transfers to children can decrease one's wealth. As the reform was announced in 2012, individuals anticipating they could require nursing home care in upcoming years might have become (more) likely to strategically change their wealth position.

To check whether individuals spent down their financial wealth in reaction to the reform, we use a timing-of-events approach. We estimate the wealth trajectory in the three years prior to becoming eligible and compare this trajectory for individuals becoming eligible prior to the reform to that of individuals becoming eligible after the reform. To identify calendar-year effects, we include individuals who are single and aged 66+ and whose income and wealth are such that they would be exposed to an increase in the price of nursing home care with the reform, but do not become eligible during the observation period.[36]

We use annual wealth observations for the years 2007 to 2013. The sample consists of individuals becoming eligible in the years 2009 to 2014 and a control group of individuals who do not become eligible. For individuals who become eligible, we only include the last three years prior to the year of first eligibility. We estimate the following model:

$$W_{it} = \alpha_0 + \alpha_1 Elig_i + \alpha_2 Epost_i + \sum_{\tau=-2}^{-1} [\beta\tau 1_{it}^{\tau} + \gamma_\tau 1_{it}^{\tau} \times Epost_i] + \xi_t + X_{it}\delta + u_{it} \quad (8)$$

where W_{it} is wealth (taken into account for the computation of co-payments) of individual i in year t. $Elig_i$ is a dummy indicating that an individual i has become eligible for nursing home care between 2009 and 2014. $Epost_i$ indicates that the individual becomes eligible after the reform.[37] 1_{it}^{τ} is an indicator equal

[36] We do not use the difference-in-differences design in this case, because the individuals not affected by the co-payment reform have, by construction, different wealth and income levels (and possibly trajectories) than those affected by the reform. Instead, we limit the sample to individuals who are affected by the reform. To define one individual's treatment status and intensity, we refer to her income and wealth in the year prior to first eligibility for individuals who become eligible for nursing home care, and to income and wealth in the last year of observation for those who do not become eligible.

[37] The reform was announced in April 2012. As wealth is measured by calendar year, we discard individuals

to 1 if individual i observed in year t is τ years away from becoming eligible. ξ_t are calendar-year fixed effects. The parameters of interest are γ_τ, $\tau = -2, -1$: the effect of the reform on the wealth trajectory in the last two years before becoming eligible.

Table VII shows the parameters of interest for a number of specifications and samples. Panel A (resp. B) shows the results when the outcome is defined as the level (resp. log) of wealth. Column (1) shows the results for the entire sample, while Column (2) focuses on individuals who were exposed to an increase in the price of nursing home care higher than the median increase (€200/month), whom we expect to react more to the reform than individuals who would be exposed to a lower price increase if they leave their wealth level unchanged.

Table VII shows no evidence of strategic reallocation in reaction to the reform: in both samples, the estimates of interaction terms $1^1 \times Epost$ and $1^2 \times Epost$ are not only insignificant at the 10% level, but they are also economically small. Average wealth among the eligible amounts to €96,200 and reaches €145,800 among those exposed to a high price increase. The largest estimate (in Column (2), Panel B) indicates that, among those who would have the most to gain from depleting their wealth, the difference in wealth between 3 and 1 years before first eligibility is 3.6% *higher* - rather than lower because of reallocation - for those who become eligible after the reform than for individuals who become eligible before the reform - but the estimate is imprecisely estimated and not statistically different from 0 at conventional levels.[38]

who become eligible for nursing home care in 2012, and compare individuals who become eligible in 2009-2011 to those who become eligible in 2013-2014.

[38]The estimates of the terms 1^1 and 1^2 are generally small and statistically insignificant at the 10% level, which means that before the reform there was no pattern of wealth depletion in the years preceding first eligibility either. If anything, individuals exposed to a high price tended to improve their wealth position prior to an eligibility (Column (2), Panel B suggests a 3.85% higher wealth in the year prior to eligibility, compared with 3 years before).

Table VII: Robustness check: Wealth evolution before first nursing home care eligibility, before *versus* after the reform.

Sample	All	High price
	(1)	(2)
	Panel A - Outcome: Wealth (level)	
Elig	2046.9**	8863.2***
	(856.3)	(1336.0)
Epost	-2041.5*	-5161.5***
	(1196.3)	(1831.4)
1^3	*Ref.*	*Ref.*
1^2	-42.50	803.7
	(1190.7)	(1859.2)
1^1	-434.4	1473.3
	(1195.5)	(1866.7)
$1^3 \times Epost$	*Ref.*	*Ref.*
$1^2 \times Epost$	32.68	-1299.5
	(1665.0)	(2550.2)
$1^1 \times Epost$	-1119.8	-2868.9
	(1675.1)	(2564.7)
Controls	Yes	Yes
N	2701168	1463388
R^2	0.043	0.027
	Panel B - Outcome: Wealth (log)	
Elig	0.0686***	0.145***
	(0.00919)	(0.0118)
Epost	-0.0861***	-0.126***
	(0.0128)	(0.0162)
1^3	*Ref.*	*Ref.*
1^2	0.00379	0.00988
	(0.0128)	(0.0164)
1^1	0.00437	0.0385**
	(0.0128)	(0.0165)
$1^3 \times Epost$	*Ref.*	*Ref.*
$1^2 \times Epost$	-0.000855	-0.00957
	(0.0179)	(0.0225)
$1^1 \times Epost$	-0.00904	-0.0356
	(0.0179)	(0.0226)
Controls	Yes	Yes
N	2668608	1450773
R^2	0.050	0.035

Individuals 66+, singles, between 2009 and 2014 (excluding individuals who become first time eligible for nursing home care in 2012). The 'high price' sample in Column (2) corresponds to individuals exposed to an increase in the price of nursing home care higher than 200 euros per month.
NOTES: Standard errors in parentheses, clustered at the individual level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls include gender, age categories and their interaction, income vintiles and year fixed effects. Wealth taken into account for the computation of co-payments, in current euros.

7.3 Alternative specifications

Month-by-month analysis of nursing home entry

The baseline specification estimates the change in the number of days of use and the probability of any use within the first 12 months after eligibility. As explained in Section 3, these are averages over the individual decisions to enter a nursing home or postpone use in each of these 12 months. To get a better idea of the underlying time dynamics and to assess whether the effects on use within the first 12 months after eligibility capture all effects on use, we introduce a different, monthly, specification.

We construct a panel tracking nursing home admissions and stays of the individuals who became first-time eligible for nursing home care on a monthly basis between January 2009 and December 2014 (i.e. the baseline study population). The panel is unbalanced because individuals are dropped in the month following their death. The number of observations per individual is then 12 or lower. Appendix D.4 provides descriptives statistics for this sample.

We consider two outcomes. First, we introduce a panel specification taking as the outcome *any nursing home care use* in each monthly period τ since eligibility. We define NH_{it}^{τ} as a dummy equal to 1 if individual i observed at month t (ranging between January 2009 and November 2015) has stayed in a nursing home in this month, corresponding to month τ elapsed since first eligibility. For each of $\tau = 1, ..., 12$, we estimate the following regression model:

$$NH_{it}^{\tau} = \beta_0^{\tau} + \beta_1^{\tau} Post_{it} + \beta_2^{\tau} Treat_{it} + \beta_3^{\tau} Treat_{it} \times Post_{it} \times \Delta_i + X_{it}\theta^{\tau} + v_{it}, \quad (9)$$

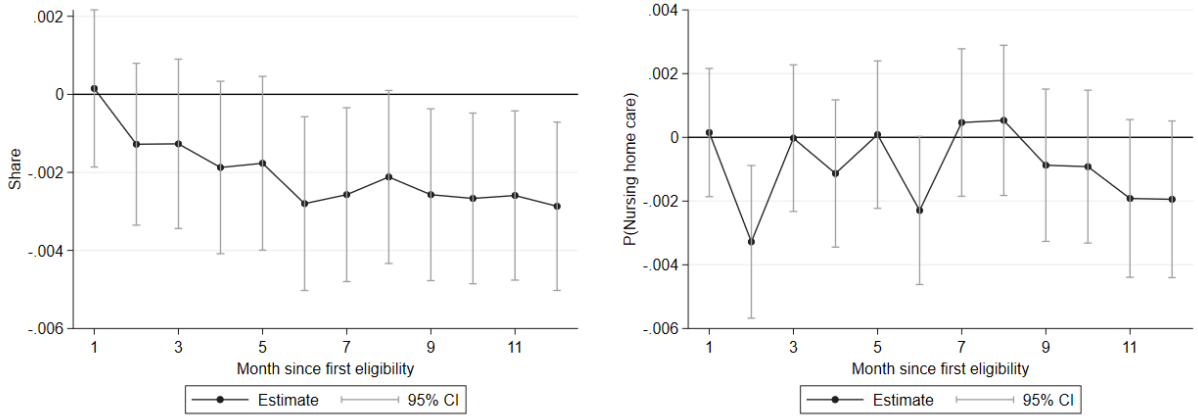
where treatment status and pre- and post-periods are defined as before. As individuals generally stay in a nursing home once they have entered, the coefficients β_3^{τ} capture the effect of the reform on the cumulative hazard of nursing home entry. [39] The specification also includes quarter fixed effects. We estimate the set of Equations (9) by OLS with clustered standard errors.

Second, we use the same empirical specification with another outcome: nursing home *admission*. The outcome variable NA_{it}^{τ} is a dummy which is 1 if individual i *enters* the nursing home in month t, and month t is equal to τ months since first eligibility. Whereas the model for NH_{it}^{τ} captures the impact of the reform on the *cumulative* hazard, the one for NA_{it}^{τ} captures the effect of the reform on the hazard *rate*: the probability of entering a nursing in τ months since eligibility.

[39] Note that the treatment status ($Treat_{it}$) may vary across time for an individual: if observations come from two subsequent calendar years and if their income and wealth were not the same two years before eligibility and one year before, they might can go from treated to untreated or the other way around.

Figure 7 shows the estimates of β_3^τ for both outcomes. It reveals three results. First, the probability to enter a NH in the first month of eligibility is not affected by the reform (Panels A and B). This could be explained by the fact that individuals who are admitted within a few days or weeks following their first eligibility have very severe needs or an emergency admission, and are thus very price-inelastic. Second, for months 2 to 12 following eligibility, the effect on the cumulative hazard is relatively stable, lying between -0.3 and -0.2 percentage point per €100 increase of the monthly price (Panel A). Finally, the largest effect on the monthly hazard is observed in the second month (Panel B), when the probability of an admission in the month decreases by 0.4 percentage point by every additional €100 increase in the price to be paid for an extra month in the nursing month. For the other months, the estimates are not statistically significantly different from 0 at the 5% level. Based on these results, we are fairly confident that our main analysis, focusing on overall effect in use within the first 12 months after eligibility, captures the majority of the behavioral response.

Figure 7: Impact of the price of nursing home care use and admission, by treatment status and month since first eligibility



Panel A: probability of nursing home care use (cumulative hazard).

Panel B: probability of a nursing home admission (hazard).

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).
NOTES: Robust confidence intervals at the 95% level are displayed.

Functional form of the price effect

The main specification (Equation (6)) assumes a linear price effect: every additional euro increase in the co-payment to be paid is expected to yield the same effect on nursing home use. As an alternative, we test a more flexible specification: we divide individuals based on the treatment intensity into six 200-euro bins, and estimate an average treatment effect for each of these groups.[40] Or formally:

$$y_i = \alpha_0 + \alpha_1 Post_i + \sum_{k=1}^{K} \alpha_2^k Treated_i^k + \sum_{k=1}^{K} \alpha_3^k Post_i \times Treated_i^k + X_i\theta + \varepsilon_i. \quad (10)$$

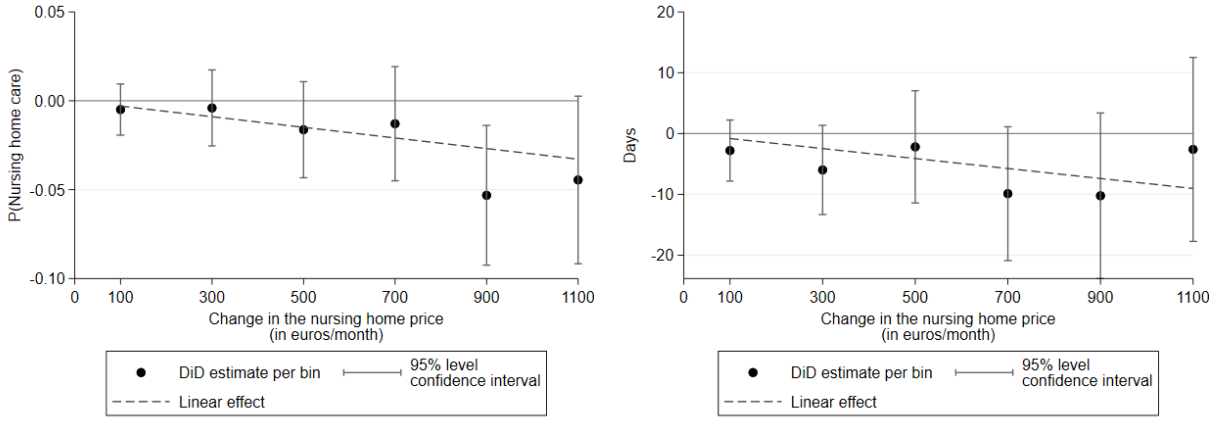
We expect that the coefficients α^k are more negative when k increases: a larger price change leads to a larger decrease in nursing home care use compared to the control group.

Indeed, the estimates of α_3^k become more negative (Figure 8). Furthermore, the linear price effect appears to be a reasonable approximation of the true functional form, although the baseline specification slightly under-estimates the price sensitivity of individuals whose co-payments increase by more than €600 per month.[41]

[40] This specification is analogous to a difference-in-differences approach applied to a randomized control trial with several treatment arms.

[41] In Appendix D.3, we also test a linear relative price-effect specification (including one measuring treatment intensity using the log-price of nursing home care). The bin-based estimates for the log-price specification show a worse fit to the data.

Figure 8: Price sensitivity by treatment intensity: difference-in-differences estimates across bins and linear effect of the increase in the nursing home price.



Panel A: probability of nursing home use. *Panel B: days of nursing home use.*

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

NOTES: Robust confidence intervals at the 95% level are displayed. The outcome is defined as the number of days spent in a nursing home in the 12 months following the day of first eligibility. A dot corresponds to the DiD estimate for individuals in the corresponding bin. Individuals in the treated group are grouped into bins based on the change in the monthly net price for nursing home care induced by the reform. The first bin from the right groups individuals who are subject to a positive increase in co-payment up to €200/month. The second bin groups individuals who are subject to an increase higher than €200/month and up to €400/month etc. The fitted line shows the effect of a €100 increase in the monthly price for nursing home care induced by the reform.

8 An efficient reform?

The co-payment reform implemented in the Netherlands led to a decrease in nursing home care use and public spending. To gain insight into whether the reform was desirable from a societal perspective, we assess whether the reduction in ex-post moral hazard and the costs of public spending outweigh the increase in financial risk for potential care users. Although the increases in the monthly co-payment were relatively small compared to financial resources, they might still have led to substantial financial risk because of the small but relevant probability of needing nursing home care for many years in a row.

We first provide an estimate of the increase in the lifetime financial risk due to the reform and the size of the welfare loss that this risk induces. Second, we compare this welfare loss to the welfare gains due to lower moral hazard and lower costs of public spending. The details of the analysis can be found in Appendix E.

8.1 Effects on financial risk

Because the financial costs of co-payment are, to a large extent, concentrated among the individuals who use care for many years, we need to identify the effects of the reform on *lifetime* co-payments. We do this by calculating the distribution of lifetime payments for a 70-year old, who has such an income and wealth that *if* he would become eligible for care he would be affected by the reform. The effects of the reform on financial risk thus not only pertain to individuals who actually end up needing care (the group of eligible individuals we have been studying thus far), but to the entire group of *potentially* affected.

To estimate the post-reform lifetime distribution of nursing home care for this group, we use data on survival and new admissions over the years 2013 to 2019 by age. Using a lifetable approach, this provides the lifetime probability distribution of care use: the probabilities q_x that a 70-year old will use x months of care during the rest of their life. Using the pre- and post-reform co-payment rules, we can then calculate the probability distribution of life-time co-payments before and after the reform. We do this for 16 different combinations of income and wealth, based on the quartiles of the income- and wealth-distribution of all individuals potentially affected by the reform.

Table VIII shows the distribution of lifetime co-payments for one of these groups: individuals with a disposable income of €19.900 and €101.200 of financial wealth, which are the averages of individuals who are in the 2nd income quartile and 3rd wealth quartile. For this particular group, the reform shifted the distribution of

co-payments substantially to the right. The average increased from €11,556 to €17,408. For the 5 percent biggest users of nursing home care within this group, co-payments increased from €68,924 or more to €104,491 or more.

Table VIII: The distribution of lifetime co-payments for individuals with a disposable income of €19.900 and €101.200 of financial wealth

	Average	75th pctile	90th pctile	95th pctile
pre-reform	11556	14817	47486	68924
post-reform	17408	22091	71842	104491

NOTES: Co-payments in euros. The income and wealth levels are based on the averages of individuals who are in the 2nd income quartile and 3rd wealth quartile.

To estimate the welfare effect of this increase in financial risk, we use a one-period constant relative risk aversion utility model. The model captures the welfare effects of the reform on risk through the risk-premium: the price individuals are willing to pay - on top of the increase in average lifetime co-payments - to fully insure themselves against the increase in co-payments. We calculate the risk premium for each of the income- and wealth-groups and then take a weighted average (based on the population size of each group). For example, when using a risk-aversion parameter of 5,[42] we find that, among those potentially affected by the reform, the reform decreased the lifetime value of insurance by €3,521. This is equal to 0.7 percent of this group's pre-reform lifetime welfare (measured as certainty equivalent consumption). A higher (lower) risk aversion would result in a higher (lower) decrease in the lifetime value of insurance.

8.2 Relative magnitude of costs and benefits

We compare the welfare loss due to the increased financial risk of potential care users to the welfare gains stemming from a reduction in moral hazard and in public spending.

The reduction in moral hazard is computed assuming that the entire reduction in the use of publicly financed care is a welfare-improving reduction in moral hazard.[43] As explained in Appendix E, we estimate this gain to be of €246 per potentially affected person. The second effect, the reduction in publicly financed care costs, is computed as the sum of i) the reduction in care costs net of co-payments and ii) the shift in costs from the government to the users due to the higher co-payments. To

[42]This value falls in the range of values for a CRRA parameter calibrated in life-cycle models of savings and medical expenditures (see e.g. De Nardi *et al.* (2010); Wouterse *et al.* (2020)).

[43]The implicit, simplifying assumption here is that the consumption value of the care foregone due to the higher co-payment is equal to the pre-reform private price. See Appendix E.

capture the marginal costs associated with raising public revenues through taxes or social insurance premiums, we value the reduction in government spending at values ranging between 1.0 and 1.5 euro for each euro of reduction in public spending on care (see Appendix E for a justification). Third, we include the welfare effects of increased financial risk that we estimated in the previous subsection as a societal cost.

Because of the uncertainty regarding the underlying parameters, Figure E.1 graphically presents the net welfare effects (per potentially affected person) of the reform across a range of reasonable values for both risk-aversion and the marginal costs of public funds. The upper-left triangle (in red) displays combinations of risk aversion and the marginal costs of funds resulting in a welfare loss. The lower-right triangle (in green) displays the combinations resulting in a welfare gain. The diagonal (in blue) shows the break even points, i.e. the combinations of risk aversion and MCF such that the reform was likely neutral in terms of its welfare impact.

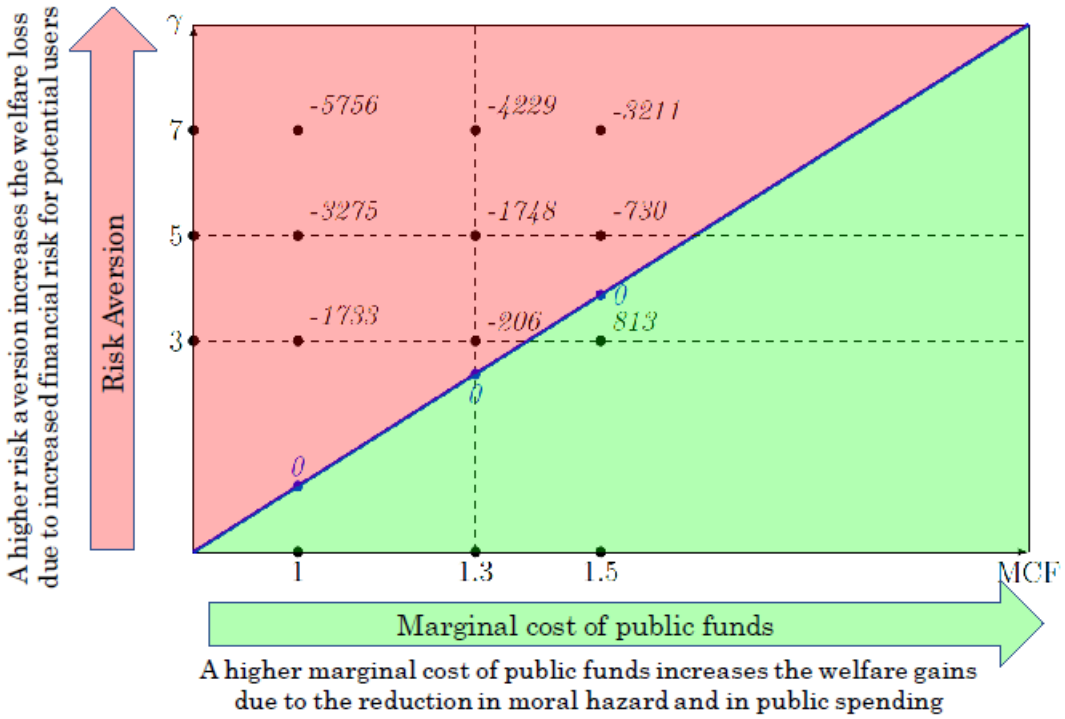
Albeit rough estimates, the numbers displayed on Figure E.1 confirm what we can sense by comparing the average reduction in total publicly financed health care costs to the increased financial risk in Table VIII: the relatively small reduction in publicly financed care costs comes at the cost of a sizable increase in financial risk. Only if individuals have a low risk aversion will the welfare value of this financial risk be limited; and only if the marginal cost of public funds is very high will the decrease in public savings result in a welfare gain.

Assuming a fairly low value of 3 for γ and a MCF equal to 1.3, we estimate that the reform decreased welfare by €206. Only if the distortionary effects of levying public revenues are higher can we conclude to a likely welfare gain from the reform. If individuals happen to be more risk averse, only a fairly higher MCF (likely in excess of what it is plausible in the context of our study) would enable welfare gains.[44]

As we do not observe all relevant elements of the social welfare function (such as the effects on well-being and societal inequality aversion) and rely on an number of assumptions, the results should be viewed as indicative. However, we believe that it is unlikely that the result would be more positive if we were able to take into account the well-being effects of a (postponed) nursing home admission. The effects of an admission on well-being should be negative and sizable to offset the high cost of increased financial risk. Research on the effect of a nursing home admission on loneliness and mental health does not find such an effect (Bom, 2021).

[44]Table E.III in Appendix E displays the value of MCF such that the reform would be neutral from a welfare perspective, for different values of risk aversion.

Figure 9: Welfare effects of the reform depending on the value of the reduction in public spending and the increase in financial risk.



NOTES: γ stands for the risk aversion parameter, MCF for the marginal cost of public funds. The numbers displayed in cursive on the figure shows the net welfare effect induced by the reform, for a given γ and MCF values, in euros per affected person.

9 Discussion

9.1 Summary of findings

Nursing home residents pay for a part of the costs of their stay in virtually all countries. Yet, there is little evidence on how these co-payments affect the decision to move to a nursing home. Moreover, it is unknown if there are spillovers to health, other health care expenditures, and informal caregivers, and what their welfare implications are. We show that potential users of permanent nursing home care are responsive to changes in co-payments by analyzing the impact of a reform of the co-payment rules in the Netherlands. This reform increased the co-payments for one third of the older single-living people (those with a moderate income and high wealth), while others (those with low wealth or a very high income) were unaffected.

We find that singles eligible for nursing home care who were affected by the reform reduced their nursing home use by 4.4 days on average. Individuals who were subjected to a larger increase in the price of nursing home care decreased their

use more. In absolute terms, the impact of the reform on use was small: in 2014 it led to a decrease of use equivalent to about 100 less nursing home beds,[45] on a total number of beds of 139 thousand CBS (2021*b*). Compared to the change in the marginal price however, the effects are economically significant: a €100 increase in the monthly price reduces the probability of a nursing home admission by 0.3 percentage point, and causes a drop in the unconditional number of days spent in residential elderly care by 0.8 day. For the group most affected by the reform (experiencing a price increase of €800 or more per month), the probability to use any nursing home care decreased by 5 percentage points.

We do not find evidence of externalities or negative health effects on average, among an extensive set of outcomes, although we could not estimate the impact on well-being. The savings from postponing a nursing home admission are not fully offset by higher expenditures on medical care or home care or by increased medical care expenditures for the children (potential informal caregivers), so the co-payment increase indeed lowers total expenditures. Furthermore, there is no strong evidence that higher co-payments increase the 2-year mortality rate in the study population. Combined with the finding that medical care use did not increase, this suggests that the lower nursing home care use induced by the co-payment increase (at least within the range we observe) does not, on average, come at the cost of a health deterioration. However, the co-payment might have led to an increase in mortality in some groups. In specific, individuals with cognitive problems or no potential informal care support may have responded to the reform in a way that was detrimental to their health.

The additional financial risk for older individuals is considerable. The reform shifted lifetime payments from the government to the potential users affected by the reform by €4,845 on average. The public cost saving effect from the reduction of care (€440 per eligible individual) is small in comparison. This shift is a mere redistribution of costs, of which only the associated reduction in the distorting effects of public financing could be included as a positive welfare gain (Appendix E.2). However, due to the uneven lifetime distribution of nursing home use, this average transfer is surrounded by large uncertainty. The increase in financial risk for the potential users affected by the reform is substantial and the associated welfare loss is likely to outweigh the welfare gains from the reduction in care use and reduced government spending.

When generalizing the results to other settings, one needs to keep in mind that

[45] Computed using the number of individuals who became eligible in 2014 (27,000), times 1/3 (proportion of individuals affected by the co-payment reform), times 4.4 days less spent in the nursing home.

in the Netherlands, public long-term care insurance pays for both home care and nursing home care. This affects which individuals are at the margin of nursing home use (Bakx *et al.*, 2021). On the one hand, it means that older people who consider a nursing home admission have a relatively poor health status (or else they would only be eligible for publicly-subsidized, comprehensive home care) and are therefore less responsive to price changes. On the other hand, the availability of high-quality home care might also be one of the reasons that even the individuals with relatively poor health are able to postpone an admission and thus have a choice.

9.2 Policy implications

Our finding that users of nursing home care are responsive to co-payments is important for policy makers for at least four reasons. First, our results show that co-payments can reduce the use of publicly financed nursing home care, even among those with high needs. Our study population consists of individuals who are eligible for nursing home care, and for whom an independent assessor has determined that around the clock care and supervision are needed. Even these individuals turn out to have some discretion in choosing whether or when to use care. Although this stands in contrast to what is sometimes believed, this is in line with prior research that finds that the timing of nursing home care use is not only determined by one's health status but also by other - financial and non-financial - factors (e.g. Tenand *et al.* (2020); Diepstraten *et al.* (2020)).

Second, our results reveal that the reduction in use can be achieved by relatively moderate marginal co-payments, which do not exceed users' financial means. This might motivate the implementation of co-payments based on individual's ability to pay in other countries as well. Income- and wealth-dependent co-payment schemes like the Dutch one may be a more efficient alternative to often-used, more drastic cost-sharing schemes such as means testing, that put a much higher financial burden and ex-ante risk on the individual user (Wouterse *et al.*, 2021).

Third, besides its effect on use, the reform shifted a substantial part of the costs of nursing home care from the government to the users itself. Although one can wonder whether co-payments are the most efficient way to reduce the governments' costs of funds, in practice reductions in the costs of public spending are an important reason for policy makers to introduce co-payments. The reform achieved such a shift without any considerable negative effects on health.

Finally, however, even relatively low and affordable marginal changes in co-payments come at the cost of imposing a financial risk on all potential users of nursing home care. The welfare effect of the increased financial risk seems larger

than the cost savings achieved by the reform.[46] This finding highlights the difficulty of targeting co-payments at the decision of entering a nursing home, while at the same time limiting the impact of co-payments on those individuals who end up needing care for a long time. Building on Arrow's theorem on insurance deductibles, Blomqvist (1997); Drèze & Schokkaert (2013) and Klimaviciute & Pestieau (2020) have shown that efficient insurance for long-term care should have high co-insurance rates in the initial stages of disablement. A high effective private price for individuals with relatively low disability levels can reduce moral hazard, as these individuals are likely the most responsive to the price of care. Full insurance for individuals who are severely disabled and have been using care for a long time leads to little moral hazard, as these individuals seemingly have a low price elasticity (they are unlikely to go back home), while the value of insurance is large.

How could the Netherlands - and other countries - remodel co-payments so as to improve efficiency of their long-term care system? A major complication is that the intended behavioral response is postponing care, or equivalently reducing the total number of months of care used. This implies that the effective price is not the spot price at entry, but the (expected) price of the last month of care used. To be effective, co-payments can thus not be levied in the first few months of use only. A way to achieve both a price incentive for efficient use and risk protection for long-term users might be to introduce a cap on lifetime co-payment or offering full insurance for individuals who spent more than a certain number of years with severe limitations. Policies like this have been proposed in the U.S. (Cohen & Butler, 2021) and in the UK (Dilnot, 2011)[47]. The size of the cap should then be chosen in such a way that it reduces the tail risk associated with co-payments, but leaves a sufficiently large effective price for those at the margin of entering a nursing home.

[46]Note that our welfare assessment focuses on efficiency aspects. The equal treatment of different types of wealth was another reason for the reform and higher equity in financing may have contributed positively to societal welfare. Prior to the reform, individuals who held most of their lifetime wealth in financial assets paid a substantially smaller co-payment than individuals with comparable levels of pension income (Wouterse *et al.*, 2020). More generally, equity concerns might have played a role, as the reform was targeted (mostly) at individuals with substantial wealth. Wouterse *et al.* (2021) illustrate the potential positive equity implications by showing that if the additional funds from an increase in the wealth contribution would have been used to lower the income contribution, the overall welfare effects of risk would actually have been positive: the value of the reduction in risk for the low and middle income groups would then have out-weighted the value of the increased risk for those affected by the reform. In practice, the additional revenues were added to the overall government funds, and the distributional effects of the reform on those not directly affected by it are negligible.

[47]In September 2021, the UK government announced it would cap lifetime co-payments on social care at £86,000 (Institute for Fiscal Studies, 2021).

References

Aron-Dine, A., Einav, L. & Finkelstein, A. (2013), 'The RAND health insurance experiment, three decades later', *Journal of Economic Perspectives* **27**(1), 197–222.

Baicker, K., Mullainathan, S. & Schwartzstein, J. (2015), 'Behavioral hazard in health insurance', *The Quarterly Journal of Economics* **130**(4), 1623–1667.

Bakx, P., Bom, J., Tenand, M. & Wouterse, B. (2020), Eigen bijdrage aan ouderenzorg, NETSPAR Design Paper, Network for the Studies of Pensions, Ageing and Retirement.

Bakx, P., Chernichovsky, D., Paolucci, F., Schokkaert, E., Trottmann, M., Wasem, J. & Schut, F. (2015), 'Demand-side strategies to deal with moral hazard in public insurance for long-term care', *Journal of Health Services Research & Policy* **20**(3), 170–176.

Bakx, P., Douven, R. & Schut, F. T. (2021), 'Does independent needs assessment limit use of publicly financed long-term care?', *Health Policy* **125**(1), 41–46.

Blomqvist, Å. (1997), 'Optimal non-linear health insurance', *Journal of Health Economics* **16**(3), 303–321.

Bom, J. (2021), Formal and Informal Long-Term Care in an Ageing Society, Doctoral thesis, Erasmus University Rotterdam, the Netherlands.

Borusyak, K. & Jaravel, X. (2017), Revisiting event study designs, with an application to the estimation of the marginal propensity to consume, Working paper, Harvard University.

Brot-Goldberg, Z. C., Chandra, A., Handel, B. R. & Kolstad, J. T. (2017), 'What does a deductible do? the impact of cost-sharing on health care prices, quantities, and spending dynamics', *The Quarterly Journal of Economics* **132**(3), 1261–1318.

CBS (2021*a*), *Monitor Langdurige Zorg.Stat. Zorg in natura; uitgaven/volume op basis van declaraties, 2010-2019 Gewijzigd op: 25 september 2020* , Statistics Netherlands. Accessed on October 19, 2021.
URL: *https://mlzopendata.cbs.nl/#/MLZ/nl/dataset/40025NED/table?ts=1634664908283*

CBS (2021*b*), *Monitor Langdurige Zorg.Stat. Zorgzwaartepakket; uitgaven/volume o.b.v. nacalculatie afspraken, 2010-'17*, Statistics Netherlands. Accessed on October 19, 2021.
URL: *https://mlzopendata.cbs.nl/#/MLZ/nl/dataset/40030NED/table?ts=1634664827787*

Chandra, A., Coile, C. & Mommaerts, C. (2020), What can economics say about alzheimer's disease?, NBER Working Paper 27760, National Bureau of Economic Research.

Chandra, A., Flack, E. & Obermeyer, Z. (2021), The health costs of cost-sharing, NBER Working Paper 28439, National Bureau of Economic Research.

Cohen, M. & Butler, S. (2021), 'The middle ground for fixing long-term care costs: The WISH Act'.

College voor Zorgverzekeringen (2013), Wachtlijstonderzoek awbz. factoren die van invloed zijn op de betrouwbaarheid van wachtlijstinformatie, , Diemen.

Colombo, F., Llena-Nozal, A., Mercier, J. & Tjadens, F. (2011), *Help wanted? Providing and paying for long-term care*, OECD Health Policy Studies, OECD Publishing edn, OECD, Paris.

De Nardi, M., French, E. & Jones, J. B. (2010), 'Why do the elderly save? The role of medical expenses', *Journal of Political Economy* **118**(1), 39–75.

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

Dilnot, A. (2011), *Fairer care funding: The report of the Commission on Funding of Care and Support*, Vol. 1, The Stationery Office.

Drèze, J. H. & Schokkaert, E. (2013), 'Arrow's theorem of the deductible: moral hazard and stop-loss in health insurance', *Journal of Risk and Uncertainty* **47**(2), 147–163.

Duflo, E. (2001), 'Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment', *American Economic Review* **91**(4), 795–813.

Einav, L. & Finkelstein, A. (2018), 'Moral hazard in health insurance: What we know and how we know it', *Journal of the European Economic Association* **16**(4), 957–982.

Finkelstein, A. (2014), *Moral hazard in health insurance*, Columbia University Press.

Fu, R. & Noguchi, H. (2019), 'Moral hazard under zero price policy: Evidence from japanese long-term care claims data', *The European Journal of Health Economics* **20**(6), 785–799.

Goodman-Bacon, A. (2021), 'Difference-in-differences with variation in treatment timing', *Journal of Econometrics* .

Grabowski, D. C. & Gruber, J. (2007), 'Moral hazard in nursing home use', *Journal of Health Economics* **26**(3), 560–577.

Hackmann, M. B. & Pohl, R. V. (2018), Patient vs. provider incentives in long term care, NBER Working Paper 25178, National Bureau of Economic Research.

Hashiguchi, T. C. O. & Llena-Nozal, A. (2020), The effectiveness of social protection for long-term care in old age: Is social protection reducing the risk of poverty associated with care needs?, OECD Health Working Papers 117.

Hitchcock, K. (2015), *Quarterly Essay 57: Dear Life: On caring for the elderly*, Vol. 57, Black Inc.

Hurd, M. D., Michaud, P. C. & Rohwedder, S. (2017), 'Distribution of lifetime nursing home use and of out of pocket spending', *PNAS* **114**(37), 9838–9842.

Hussem, A., Tenand, M. & Bakx, P. (2020), Publieke middelen voor particuliere ouderenzorg, Design Paper, Netspar Industry Series 143, Network for Studies on Pensions, Aging and Retirement.

Institute for Fiscal Studies (2021), *An initial response to the Prime Minister's announcement on health, social care and National Insurance*, Institute for Fiscal Studies. Press Release. Consulted on October 15th 2021. Authors: Paul Johnson, Carl Emmerson, Helen Miller, David Phillips, George Stoye, Isaac Delestre, Isabel Stockton, Kate Ogden, Robert Joyce, Stuart Adam, Tom Waters, Max Warner and Ben Zaranko.
URL: *https://ifs.org.uk/publications/15597*

Jacobs, B. (2015), *De prijs van gelijkheid.*, Amsterdam.

Jacobs, B. (2018), 'The marginal cost of public funds is one at the optimal tax system', *International Tax and Public Finance* **25**(4), 883–912.

Kahn-Lang, A. & Lang, K. (2019), 'The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications', *Journal of Business & Economic Statistics* pp. 1–14.

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

Klimaviciute, J. & Pestieau, P. (2020), 'Insurance with a deductible: a way out of the long term care insurance puzzle', *Journal of Economics* **130**, 297–307.

Konetzka, R. T., He, D., Dong, J. & Nyman, J. A. (2019), 'Moral hazard and long-term care insurance', *The Geneva Papers on Risk and Insurance-Issues and Practice* **44**(2), 231–251.

Lin, H.-R. & Imanaka, Y. (2020), 'Effects of copayment in long-term care insurance on long-term care and medical care expenditure', *Journal of the American Medical Directors Association* **21**(5), 640–646.

Maarse, J. A. M. H. & Jeurissen, P. P. P. (2016), 'The policy and politics of the 2015 long-term care reform in the Netherlands', *Health Policy* **120**(3), 241–245.

Muir, T. (2017), Measuring social protection for long-term care, OECD Health Working Papers, Organisation for Economic Co-operation and Development, Paris.

Non, M. (2017), Co-payments in long-term home care: do they affect the use of care?, CPB Discussion Paper 363, CPB Netherlands Bureau for Economic Policy Analysis.

Nyman, J. A. (1999), 'The value of health insurance: the access motive', *Journal of Health Economics* **18**(2), 141–152.

OECD (2020), *OECD.Stat*, Organisation for Economic Co-operation and Development. **URL:** *https://stats.oecd.org/Index.aspx?DataSetCode=HEALTH_LTCR*

Pezzin, L. E., Kemper, P. & Reschovsky, J. (1996), 'Does publicly provided home care substitute for family care? Experimental evidence with endogenous living arrangements', *The Journal of Human Resources* **31**(3), 650–676.

Rapp, T., Grand, A., Cantet, C., Andrieu, S., Coley, N., Portet, F. & Vellas, B. (2011), 'Public financial support receipt and non–medical resource utilization in Alzheimer's disease – results from the PLASA study', *Social Science & Medicine* **72**(8), 1310–1316.

Rellstab, S., Bakx, P., Garcia-Gomez, P. & Van Doorslaer, E. (2020), 'The kids are alright-labour market effects of unexpected parental hospitalisations in the netherlands', *Journal of Health Economics* **69**, 102275.

Roquebert, Q. & Tenand, M. (2017), 'Pay less, consume more? The price elasticity of home care for the disabled elderly in France', *Health Economics* **26**(79), 1162–1174.

Roth, J. (2019), Pre-test with caution: Event-study estimates after testing for parallel trends, Working Paper, Harvard University.

Schut, F., Sorbe, S. & Hoj, J. (2013), Health Care Reform and Long-Term Care in the Netherlands, OECD Economics Department Working Paper 1010, OECD.

Shigeoka, H. (2014), 'The effect of patient cost sharing on utilization, health, and risk protection', *American Economic Review* **104**(7), 2152–84.

Stabile, M., Laporte, A. & Coyte, P. C. (2006), 'Household responses to public home care programs', *Journal of Health Economics* **25**(4), 674–701.

Takahashi, M. (2020), Behavioral Effects of Insurance Coverage and Health Consequences: Evidence from Long-Term Care, SSRN Working Paper 3352257, Social Science Research Network.

Tenand, M., Bakx, P. & Van Doorslaer, E. (2020), 'Equal long-term care for equal needs with universal and comprehensive coverage? an assessment using dutch administrative data', *Health Economics* **29**(4), 435–451.

Tenand, M., Bakx, P. & Wouterse, B. (2021), Co-payments in the Dutch long-term care system: schedule and computation with Statistics Netherlands individual-level data, Online Resource, Erasmus University Rotterdam Data Repository.
URL: *https://doi.org/10.25397/eur.16866442*

van Eijkel, R. & Kattenberg, M. (2018), Competition and pricing behavior in long term care markets: Evidence from the Market for Assistance in Daily Housekeeping Activities, CPB Discussion Paper 373, CPB Netherlands Bureau for Economic Policy Analysis.

Wouterse, B., Hussem, A. & Aalbers, R. (2020), Betere risicospreiding van eigen bijdragen in de verpleeghuiszorg, Policy brief, CPB, The Hague.

Wouterse, B., Hussem, A. & Wong, A. (2021), 'The welfare effects of co-payments in long term care', *Journal of Risk and Insurance* .

Online Resources

Additional information on the schedule of co-payments and their computation with individual-level data from Statistics Netherlands is available in Tenand *et al.* (2021) (https://doi.org/10.25397/eur.16866442).

A Additional information on the institutional context

A.1 The co-payment schedules in the Netherlands

For detailed information on the computation of co-payments for long-term care in the Netherlands, we refer the reader to Tenand *et al.* (2021) (link).

A.2 Care packages in nursing homes

When notified a positive eligibility decision for a nursing home admission, individuals are assigned a care package (or ZZP), with a number between 4 and 8. Each care package includes a specific combination of personal care, nursing care and guidance. The packages have remained the same throughout the study period.

When eligible for nursing home care, individuals can choose to stay at home and receive an equivalent package of home care instead of care within the nursing home. In our analysis, we combine individual information on the care package upon first eligibility and the official grid describing the content of each package to derive the number of hours of home care that an individual would receive at home. We use this information to construct the net price of nursing home care, as the difference between the co-payment to be paid for a month of nursing home care and the co-payment to be paid for a month of equivalent home care.

Table A.I: Profiles of care packages (ZZP).

ZZP	Description	Recommended hours of:			Total hours
		Personal care	Nursing care	Guidance	
4	Institutional living with intensive guidance and comprehensive personal care	5.5	1.5	5.5	13
5	Protected living with intensive care for patients with dementia	5.5	5.5	8.5	19.5
6	Protected living with intensive personal care and nursing care	8.5	5.5	5.5	19.5
7	Protected living with very intensive care, with an emphasis on guidance	8.5	5.5	11.5	25.5
8	Protected living with very intensive care, with an emphasis on nursing care	11.5	5.5	11.5	28.5

SOURCE: College voor Zorgverzekeringen (2012), Zelf uw zorg inkopen in 8 stappen 2012. https://www.hetvenster.com/Content/DownloadDocs/2012/pgb%20brochure,%20zelf%20zorg%20inkopen,%202012.pdf288.
NOTES: Hours are expressed per week.

B Data sources

The data used in this study are individual-level or household-level data provided by Statistics Netherlands (CBS). There are accessible via a remote access environment in a set of different datasets. In a dataset, each individual is identified by a unique number (which has been pseudomyzed). The linkage of the different datasets is performed using the individual identifier, and is thus exact.

B.1 Overview of the microdata used

Table C.I provides the list of the microdata used in this research.

Table C.I: Datasets used (as listed in CBS microdata catalogue)

Content	Name of dataset	Source
Eligibility for nursing home care and home care	INDICAWBZTAB	CIZ
Use of nursing home care	ZORGMVTAB	CAK
	GEBWLZTAB	
Use of home care	GEBZZVTAB	CAK
	ZVWZORGKOSTEN	Vektis
Health care expenditures	ZVWZORGKOSTEN	Vektis
Death	GBAPOVERLIJDENTAB	Death records
Date of birth and gender	GBAPERSOONTAB	Population registers
Address	GBAADRESOBJECTBUS	Population registers
	VSLGWB2019TAB	Population registers
Household income & tax household composition	Integraal Huishoudens Inkomen IHI	Tax Office and CBS
Household wealth	VEHTAB	Tax Office and CBS
Linkage parent-child	KINDOUDERTAB	Population registers
Co-payments	E3_EB_EIGEN_BIJDRAGES_v3.2	CAK & VWS

In addition, in order to link each individual to their tax household and household income, we use the bridge table RINPERSOONKERN (one for each year), which link individual pseudomyzed identifiers and household identifiers. Similarly, we use the table of correspondence KOPPELTABELVEHTAB to link the wealth variables at the individual level.

The dataset that contains the co-payment information was compiled by Statistics Netherlands using data from CAK, initially at the request of the Ministry of Health (VWS).[48]

The linkage of individuals to their legal parents through KINDOUDERTAB is most reliable for individuals born since 1966. For our analysis, this implies that

[48] In the remote access environment from CBS/Statistics Netherlands, it can be found under G:\Maatwerk.

measurement errors on the characteristics of children are more likely to occur for the older cohorts in our sample.

B.2 Additional data used

To link each municipality to one of the LTC purchasing regions, we also used the table of correspondence 'GIN - Gebieden in Nederland' (2013-V1 and 2014-V1). For years 2010 to 2012, we refer to the grouping of municipalities into LTC regions as they were defined in 2013.

To compute nursing home care expenditures, we combine the time spent in nursing homes with a given care package (ZZP) (retrieved from the microdata, cf. supra) with the daily cost of a stay for residents with this care package. In the Netherlands, the daily tariff paid to nursing homes is set at the national level by the National Health Care Authority (NZa) and varies with across care packages. There exist two tariffs per care package: one for stays (or days) including therapy (*behandeling*) and one for days that do not include therapy. CBS microdata allow us to retrieve the care package of each stay, but not whether the resident receives therapy. Therefore, instead of referring to the national tariffs set by the Health Care Authority, we use the average daily cost incurred in a given year. We compute this cost as the ratio of spending to the number of days of care use, available for each care package. We refer to the figures of 2012, as provided by CBS (2021*b*). For nursing home care strictly speaking, the average daily cost varies between €167 and €319 in 2019.[49]

To compute home care expenditures, we compute the volume of hours of home care receive (retrieved in the microdata) with the average hourly expenditure, for each type of care (personal care, nursing care and guidance for AWBZ-funded home care, and domestic help for WMO-funded home care). For AWBZ-funded home care, we proxy this hourly expenditure by dividing total expenditures incurred in year on a given type of care by the number of hours of such care that have been provided CBS (2021*a*). For WMO-funded care, we refer instead to the average price charged by providers, as computed by van Eijkel & Kattenberg (2018).

B.3 Income and wealth definitions

The simulation of co-payments relies on individual-level information on income and wealth. For information on which concepts of income and wealth are used for

[49] As expected, for each the average daily cost falls between the national tariff without therapy and the tariff including therapy.

the computation of co-payments and which variables from the administrative data we use to capture these concepts, see Tenand *et al.* (2021) (link).

C Further details on sample selection

Here below we provide additional details on sample selection.

C.1 First-time eligible for nursing home care

In our baseline analysis, we monitor nursing home admissions for older individuals who are singles, after they became eligible for nursing home care for the first time.

We define eligibility for nursing home care as the day at which the individual becomes eligible for institutional care with a care package (ZZP) 4 to 8 and given a psychogeriatric or somatic condition, as assessed during the needs assessment. A first-time eligibility is such that the individual should not have become eligible for any institutional care beforehand, at any point between 2009 and 2014.

In the source data, eligibility spells are recorded within a calendar year, such that an individual who would be eligible for a nursing home stay e.g. between March 2011 and July 2012 will have two spells recorded: one from March 2011 to 31/12/2011 and one from 01/01/2012 to July 2012. Given that we have no data on eligibility spells prior to 2009, for spells that are recorded as starting on 01/01/2009 it is *a priori* impossible to know whether eligibility started exactly that day or whether eligibility was carried forward from the previous calendar year. Inspecting the data, we observe a spike in the number of eligibility spells starting on 01/01/2009. We thus assume that all eligibility spells starting on 01/01/2009 are eligibility spells carried forward from year 2008 and thus never indicate a new eligibility.

In addition, we use the records of institutional care use to check whether individuals used institutional care between 2009 and 2014 before their first eligibility spell. While in theory this should not happen, the data reveal that this can be the case. We distinguish between two cases: (i) when the earliest recorded admission preceded the start of first eligibility by 30 days or less, we recoded the beginning of the first eligibility spell using the admission date; (ii) otherwise, we discard the individual from the sample of first-time eligible.

We keep only individuals who were 66 years or older in the year of first eligibility, which correspond to the statutory retirement age at the end of the study period. Rebates for long-term care co-payments that users are entitled to do depend on whether they have reached the statutory retirement age. For the purpose of this study, an individual who becomes eligible for nursing home care in year Y is considered to be single if she was recorded as a one-person household in the tax records of two years before first eligibility. We apply this selection because the computation

of co-payments depends on whether the care recipient has a tax partner or not. As the schedule of co-payments for individuals with a partner is complex, we have left this case aside. A minority of old age individuals live without a partner but other adults or children. We also drop these individuals from the study population, as it is not clear how the household wealth recorded in the microdata is split across household members and taken into account for the computation of co-payments.

In addition, we discard the few individuals with missing information. Individuals with no tax record two years before they became eligible for nursing home care are dropped because it is not possible to know whether they are singles nor to compute their co-payments. Individuals with missing information on their address are dropped as well.

Finally, we exclude the small number of individuals (N=42) for whom the co-payment on nursing home care increased less than the co-payment on home care. The study population is made of 79,559 individuals.

C.2 Individuals not yet eligible for institutional care

As a robustness check, we assess whether individuals in the treated group (alternatively, those with higher treatment intensity) have a lower chance to become eligible for nursing home care following the reform. For this analysis, we select for each month of the time frame 2009-2014 individuals who were 66 or older, and were a single person household in the tax records of two years before. In each month, we further drop individuals who became eligible for institutional care before that month. As for the baseline analysis, all individuals who are eligible for institutional care on January 1st, 2009 are discarded from the sample, as we have no way to retrieve previous eligibility.

D Robustness checks and additional results

D.1 Results on home care use: additional results

In the results presented in Table IV, we assess the impact of the price increase on the care expenditures in the year of first eligibility and the following one, because health insurance claims are available at the yearly level. Information on the hours of care received per 4-week period is however available between 2009 and 2014. We use these data to assess the use of home care in the months prior to and following eligibility and estimate the impact of the nursing home price increase on the probability and volume of home care receipt in the 12 months following eligibility.

For these analyses, we exclude individuals who become eligible in 2014 (as we cannot track their volume of home care use in 2015, when only expenditures are available) and individuals who become eligible in January or February 2009 (as we do not observe home care prior to 2009).

Effect of the nursing home price increase on care use within 12 months following eligibility

Table D.I: The effect of the nursing home price increase on nursing home care and home care use within 12 months following first eligibility for nursing home care.

Outcome:	Days in a nursing home	Any home care use	Hours of home care use
$Post.Treat.\Delta$	-0.763*	0.000521	0.619
	(0.405)	(0.00130)	(0.651)
Controls	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
N	60845	60845	60845

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between March 2009 and 2013 (N=60,845).
NOTES: Estimates from a difference-in-differences regression. Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Δ stands for a €100 increase in the net monthly price for nursing home care induced by the reform. .

Effect of the nursing home price increase on care use prior to eligibility

We also document an absence of effect of this increase on home care use in the 3 months prior to eligibility (detailed results available on demand). We interpret this estimation as a placebo test: if the treated and control groups are comparable in terms of the underlying determinants of nursing home care use, we should find no difference in their prior home care use between before and after the reform.

A lower equivalent to nursing home care?

The descriptives and estimates suggest that individuals are able to stay at home with less home care support than their care package would suggest: for example, an individual with care package 4 is suggested to require 13 hours of care per week (cf. Appendix A.2). We have defined the change in the price of nursing home care taking into account the increase in the co-payment on home care, assuming individuals would consume the hours indicated by their care package. If individuals do the trade-off taking into account less hours of home care than we do, this could distort our measure of treatment intensity.

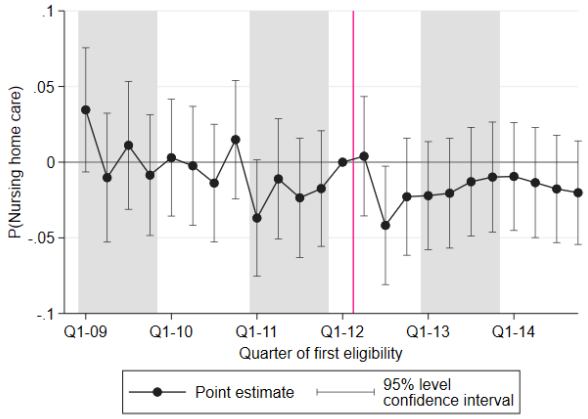
To assess the robustness of our results, we re-assess the effect of the increase in the price of nursing home on nursing home care use, assuming that individuals consider using half of the (formal) care volume they would receive in the nursing home if they stay at home instead. The results are qualitatively the same and the point estimates are actually virtually the same (detailed results available on demand). This can be explained by the facts that: (i) for most individuals, the co-payment on home care is set by the cap (the volume of care then does not weigh on the co-payment; cf. formulas and graphs in the Companion Document), and (ii) co-payments on home care are much lower than co-payments on nursing home care (even for an equivalent care package), such that the reform-induced change in the co-payment on home care is generally second order compared with the change in co-payment on nursing home care, whatever the assumptions on the volume of home care used.

D.2 Inspection of pre-trends

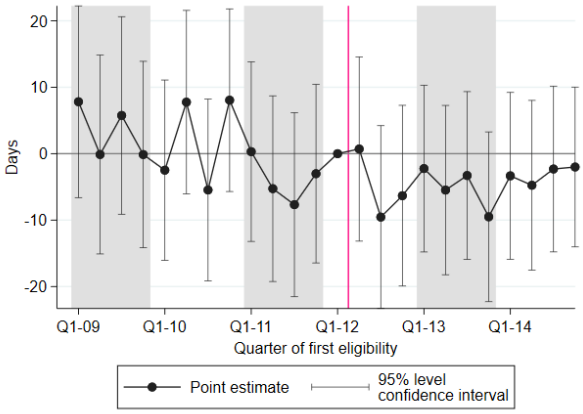
Pre-trends for nursing home care use and mortality

As discussed in the main text, one critical condition for the internal validity of the difference-in-differences estimate is that the parallel trend assumption is valid. In order to provide support to this assumption, we inspect pre-trends, i.e. how the trends in the outcomes compare before the reform kicks in across the treated and control groups. In Section 6, we provided a visual representation of the trends in nursing home care use across the treated and control groups (Figure 6, Panels C and D). It shows that in none of the quarters prior to the reform nursing home care use the difference between the treatment and the control group was statistically different (at the 5% level) than what it was just before the reform is announced (reference quarter: Q1-2012). Figure D.1 confirms that when including the covariates the results remain the same.

Figure D.1: Pre-trends for nursing home care use and mortality



Panel A: difference between treated and control group.
Outcome: probability of nursing home use.



Panel B: difference between treated and control group.
Outcome: days of nursing home use.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).
NOTES: Estimates from a dynamic DiD specification at the quarter level with covariates: each dot shows the estimate of the adjusted difference in the outcome between the treated and the control group, compared with that same difference in the first quarter of 2012.

Here below, we provide an additional statistical test to bring support to the assumption that individuals who are exposed to a (higher) price change had a different trend in nursing home care use already before the reform kicked in. We re-estimate the dynamic difference-in-differences model that underlies Figure 6 (all quarters being interacted with the treatment and the post period dummies) and

test the joint significance of the interaction terms for all quarters in the pre-reform period (but Q1-2012, which is the reference period), through a Fisher test. The p-value from this test is presented in Table D.II, row 'Binary specification', for 3 outcomes: any nursing home care use, unconditional time spent in the nursing home and 2-year mortality. Whether we include control variables or not, for all three outcomes, we find p-values largely in excess of the conventional threshold of 0.10, meaning that even at the 10% level we cannot reject that all pre-reform interaction terms are jointly null.

We also run the F-tests on a dynamic DiD specification that matches Equation (**??**) (our baseline specification): we now interact the change in the price for nursing home with the treatment and post-reform period dummies, and test for the joint significance of these interaction terms. The p-values for this test are shown in Table , row 'Price specification'. Again, there is no statistical indication that trends in nursing home care have evolved differently for people affected by lower or higher changes in co-payments.

Table D.II: Inspection of pre-trends for nursing home care use and mortality: results from a test of joint significance

Outcomes	P(nursing home use)		#days spent in a NH		2-year mortality	
Binary specification (treated vs control groups)						
p-value	0.3078	0.2392	0.4872	0.3386	0.7026	0.7259
Price specification (treatment intensity)						
p-value	0.2658	0.3107	0.3407	0.2987	0.2765	0.6250
Covariates	No	Yes	No	Yes	No	Yes

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).
NOTES: P-values from a Fisher test of joint significance on the interaction terms for the pre-reform quarters in a dynamic difference-in-differences estimation. H0: 'All coefficients are jointly null'.

Pre-trends for other outcomes

Table D.III: Inspection of pre-trends for health care expenditures: results from a test of joint significance

Outcomes	Nursing home stay expenditures	Home care expenditures	Medical care expenditures	Total care expenditures
Binary specification (treated vs control groups)				
p-value	0.5080	0.4613	0.0805	0.7399
Price specification (treatment intensity)				
p-value	0.5465	0.6209	0.1366	0.39710

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).
NOTES: P-values from a Fisher test of joint significance on the interaction terms for the pre-reform quarters in a dynamic difference-in-differences estimation. H0: 'All coefficients are jointly null'. Regressions include covariates.

Table D.IV: Inspection of pre-trends for children's income and medical care expenditures: results from a test of joint significance

Outcomes	Children's income	Children's medical care expenditures
Price specification (treatment intensity)		
p-value	0.1801	0.9101
N	62,830	62,900

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014, with at least a child alive in the year following eligibility and whose income or medical care expenditures can be retrieved.
NOTES: P-values from a Fisher test of joint significance on the interaction terms for the pre-reform quarters in a dynamic difference-in-differences estimation. H0: 'All coefficients are jointly null'. Regressions include covariates.

Table D.V: Inspection of pre-trends for the probability of becoming eligible for nursing home care: results from a test of joint significance

Outcomes	P(become eligible)
Binary specification (treated vs control groups)	
p-value	0.0000
Price specification (treatment intensity)	
p-value	0.6403

STUDY POPULATION: Individuals 66+, singles, not previously eligible for nursing home care, observed monthly between 2009 and 2014 (N=39,651,698).
NOTES: P-values from a Fisher test of joint significance on the interaction terms for the pre-reform quarters in a dynamic difference-in-differences estimation. H0: 'All coefficients are jointly null'. Regressions include covariates.

D.3 Empirical specification with relative price change

In the baseline analysis, we have estimated the price sensitivity of nursing home care use using a DiD approach and positing a linear effect of the price change in absolute terms (cf. Equation (**??**)).

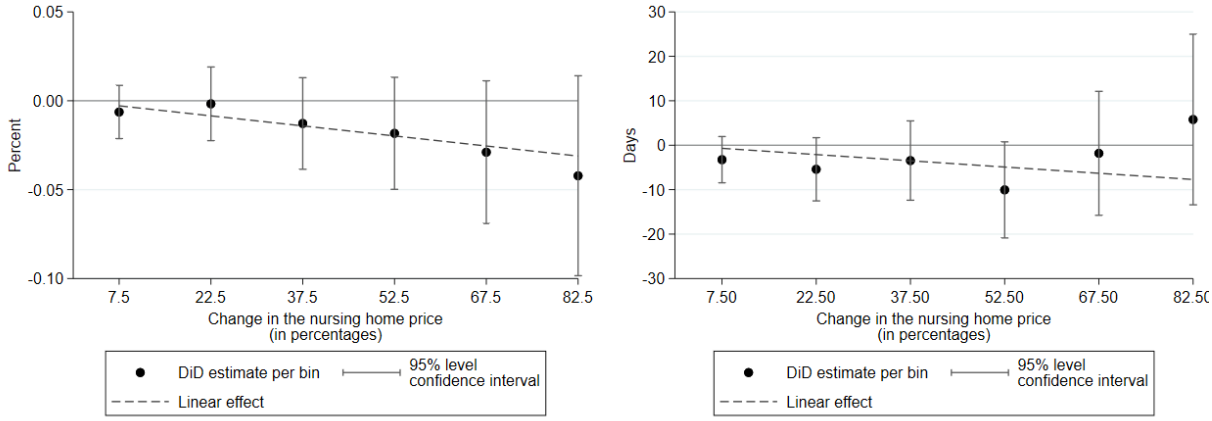
Alternatively, we can include the relative price change, in the following way:

$$Y_i = \gamma_0 + \gamma_1 Post_t + \gamma_2 Treat_i + \gamma_3 Post_i \times Treat_i \times (\ln(p_i^{post}) - \ln(p_i^{pre})) + X_i'\theta + \mu_i \quad (11)$$

γ_3 captures the effect of a one percent change in the price of nursing home stays on the percentage-**point** change in the probability of any nursing home admission, or on the unconditional number of days spent in a nursing home within 12 months following first eligibility.

Figure D.2 shows the estimate of γ_3 in Equation (11). The fit of the log-linear specification when the outcome is any nursing home care admission within 12 months is high: whatever the magnitude of the relative price change, a 1% increase in the price is predicted to decrease the probability of a nursing home admission by 0.038%-pt. It also shows that the log-linear price specification offers a poorer fit with the data when the outcome is the time spent in the nursing home, especially for individuals who experienced a high increase in the nursing home price.

Figure D.2: Price sensitivity by treatment intensity: difference-in-differences estimates across bins and linear effect of co-payment change.



Panel A: probability of nursing home use. *Panel B: days of nursing home use.*

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

NOTES: Robust confidence intervals at the 95% level are displayed. The outcome is defined as the number of days spent in a nursing home in the 12 months following the day of first eligibility. A dot corresponds to the DiD estimate for individuals in the corresponding bin. Individuals in the treated group are grouped into bins based on the relative change in the price of nursing home care induced by the reform. The first bin from the right groups individuals subject to an increase in the price of nursing home care lower than €15% (7.5 is the midpoint). The second bin groups individual subject to an increase ranging between 15% and 30% (mid-point is 22.5) etc. The fitted line shows the effect of a 1% increase in the monthly price for nursing home care induced by the reform.

D.4 Alternative specification: monthly decision to enter a nursing home

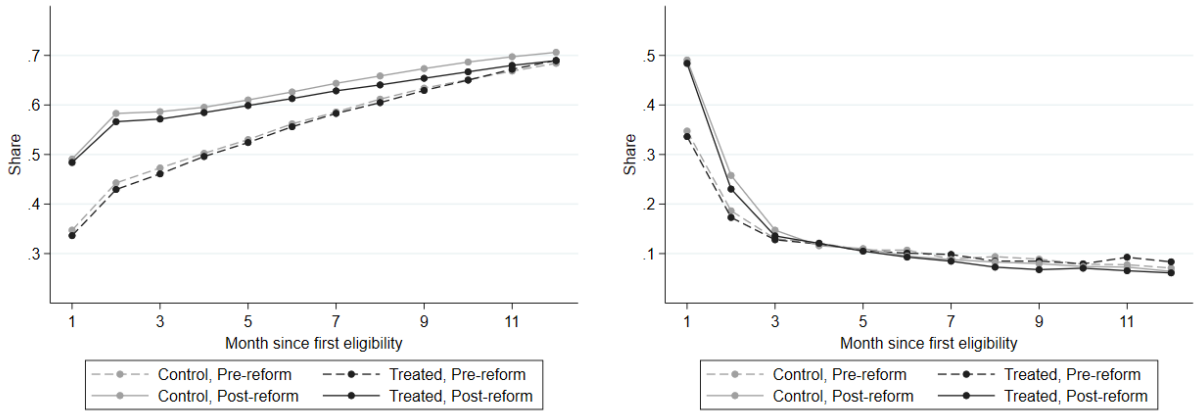
In Section 7.3, we provide estimates of the effect of the price of nursing home care on use for each of the 12 months following first eligibility. In this Appendix, we provide descriptive statistics, showing the probability of staying in a nursing home by month, depending on treatment status and whether the reform was already announced in the month being considered (Figure D.3, Panel A).

Three main patterns can be observed: first, for all four series, the probability of nursing home care use increases as time elapses since first eligibility (consistent with the fact that admissions are rarely followed by a discharge). Second, the probability of nursing home care use is higher in the post-reform period that in the pre-reform period in the 10 months following eligibility, for both the treatment and control groups (this probability converges to 68-70% after 11 months, similarly for the pre- and post-reform periods). The level difference between the two periods could be explained by the tightening of the conditions to become eligible for nursing home care that occurred during the study period, as discussed in the main text: such that the average individual who become eligible for care in the post-reform period has a worse condition (triggering earlier admission) than the average individual who become eligible for nursing home care in the pre-reform period.

The third pattern we observe is the one we are mostly interested in, as it relates to the effect of the reform: while in the pre-reform period the probability of nursing home care use for treated observations closely matches that of control observations, in the post-reform period we observe instead a lower probability of care use for the treated group than for the control group (except in the month of first eligibility).

In a similar fashion, Panel B of Figure D.3 displays the probability of a nursing home *admission* (i.e. the beginning of a stay) in the month. For all four series, the probability of an admission sharply decreases after two months following the date of first eligibility. The proportion of individuals admitted in the first and second months following eligibility is higher after the reform than before on average, again consistent with the fact that the pool of first-time eligible has on average a more deteriorated condition in 2012-2014 than in the previous years. Differences across the series that would inform of the effect of the reform are yet harder to see visually.

Figure D.3: Descriptive statistics: nursing home care use and admission, by treatment status and month since first eligibility, before and after the reform.



Panel A: probability of nursing home care use.

Panel B: probability of a nursing home admission.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559).

D.5 Quality of the co-payment simulations

We use individual-level data on the co-payments actually paid in 2012 and 2013 to assess the extent to which they match the co-payments we simulate. We use an external sample consisting of the 66+ who spent the entire years of 2012 and 2013 in an institutional elderly care facility and who were subject to the high-rate co-payment for this entire period. For this group, the co-payments paid in a year are expected to equal 12 times the monthly price of nursing home care. This provides a way to gauge the quality of our simulation of the co-payment reform. Note that in this assessment we focus on co-payments for nursing home care and leave co-payments on home care aside, because the latter have straightforward rules.

In the external sample, the simulated co-payments for nursing home care exceed the co-payment actually paid by only 3% for the median individual, and the median difference in the co-payment *change* between 2012 and 2013 is of 2%. For a majority of individuals, the difference between simulated and actual co-payments is only a few percent. However, for some individuals, the gap is extremely large. For 10% of the external sample, the simulated monthly change in price falls short of the observed change by at least €129 and for 10% it exceeds €161.

It should be noted that specific rules apply to 'long-stayers' in nursing homes (e.g. the treatment of own housing wealth), which could per se generate a substantial discrepancy between the co-payments we simulate (using rules that apply to individuals in the first two years of a stay) and the co-payments paid in the external sample. Still, we conjecture that simulation errors can well arise for our study population, mainly because we do not observe the income tax paid but proxy it by the difference between gross income and available income. This is a good approximation for most individuals, but for those who must fall under specific tax rules and are able to claim unobserved tax rebates, it may result in noisy simulations.

E Assessing the efficiency gains and costs from the reform

The assess the overall welfare effects of the co-payment change,[50] we consider the following elements:[51] the efficiency gains due to a reduction in moral hazard (A), a transfer in costs from the government to the individual user (B), and the welfare loss due to additional risk for older individuals in the treatment group, which arises because of the higher co-payments (C):

$$\begin{aligned} Net\ Benefit &= Benefits - Costs && (12)\\ &= MCF \times (A+B) - (B+C) && (13)\\ &= MCF \times A + (MCF-1) \times B - C, && (14) \end{aligned}$$

with MCF is the marginal costs of raising government funds.[52]

E.1 Moral hazard

Figure E.1 provides a stylized example of the effect of an increase in the private price of nursing home care on demand. Before the reform, individuals pay a private price p_0 which is lower than the societal price p_s. Consequently, demand (q_0) is higher than societally optimal (q_s). A reform like the co-payment reform we study increases the private price of care to p_1 and moves care use (q_1) towards the societal optimum. The net welfare effect associated with moral hazard is the reduction in use times the full (societal) price (areas 1+2+3+4) minus the value the foregone care had to the user (the loss in consumer surplus: areas 3+4).

Because area 3 will be very small in practice, we ignore it and assume that the value of the foregone care for the user is equal to the private price they had to pay for this care prior to the reform. This means that the increase in welfare due to the reduction in moral hazard (A) is equal to the decrease in government spending due to the decrease in use: $(p_s - p_0) * (q_1 - q_0)$.

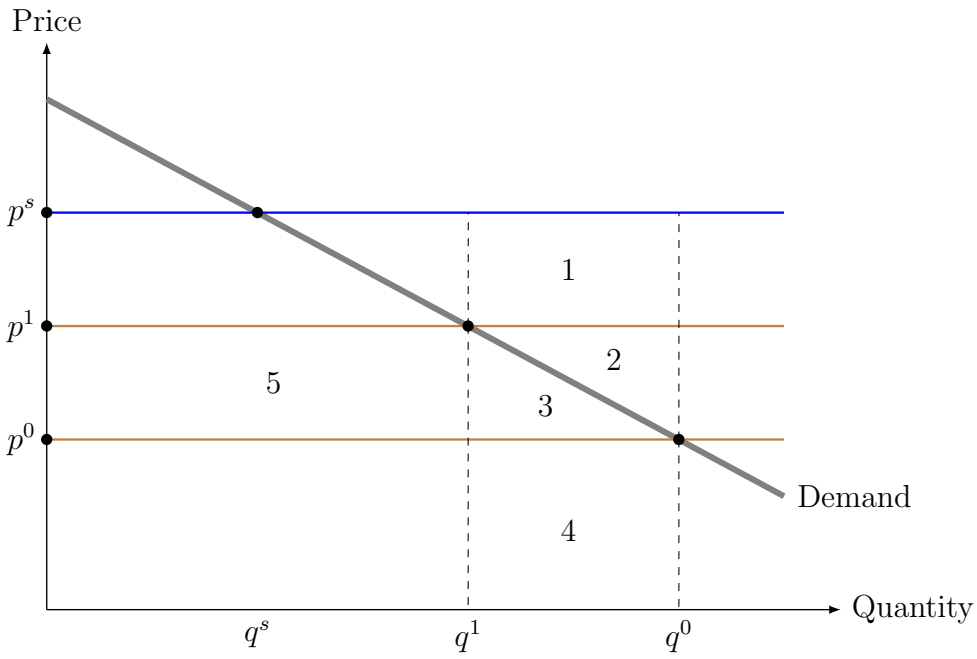
The decrease in government spending equals the public savings induced by the reduced use of nursing home care minus any additional public spending on other types of care used in response to this reduction. Table E.I provides the average

[50] Here we consider only efficiency aspects: possible welfare gains (losses) arising if the distribution of co-payments across wealth groups is more (less) in line with societal preferences are ignored.

[51] We draw inspiration in Shigeoka (2014), Online Appendix.

[52] When public spending decreases by €1, the net gain for society is valued (MCF-1) euro.

Figure E.1: Ex post welfare gains and losses from a change in the consumer price of care.



treatment effect among the treated (ATT) of the co-payment reform on medical care and long-term care expenditures.[53]. The reform decreased total care costs by €485. Net of co-payments[54] this represents a reduction of €440 in public spending per individual affected by the reform.

[53]By contrast with Table presented in Section 6, we provide the average effect of the co-payment increase rather than the impact of a €100 increase in the price of nursing home care.

[54]The average pre-reform difference between the monthly co-payments for nursing home care and for home care is €1033. Combined with the average reduction in nursing home use of 4.4 days, this gives a net co-payment of €46. The effects on the deductible for medical care, which is the form assumed by cost-sharing in the Dutch mandatory health insurance, can be ignored, as almost all older people becoming eligible for nursing home care exceed the deductible threshold (€350 per year in 2013).

Table E.I: Impact of the reform on medical care and long-term care expenditures.

Outcomes:	Nursing home care costs	Home care costs	Medical care costs	Hospital care costs	Total care costs
	(1)	(2)	(3)	(4)	(5)
Post	-20219.3***	5318.9***	9272.2***	2039.3***	-5628.2***
	(1017.2)	(448.7)	(519.7)	(313.4)	(1120.1)
Treat	1103.4**	-601.6**	46.14	34.61	548.0
	(513.5)	(283.4)	(288.2)	(160.3)	(587.5)
Post.Treat	-1385.6**	750.5**	149.7	51.93	-485.4
	(602.2)	(313.8)	(344.7)	(204.2)	(688.1)
Control variables	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	60610	60610	60610	60610	60610
R^2	0.120	0.042	0.101	0.112	0.063

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2013, and who were found in the health insurance claims in the year of their first eligibility (N=60,610).

NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Expenditures incurred in the year of first eligibility and in the following one. Medical care expenditures are expressed in 2013 euros based on CBS Consumer Price Index. Nursing home care expenditures are computed multiplying the days spent in a nursing home with a given care package by the day tariff associated with this care package in 2013. Home care expenditures are computed multiplying the number of hours of home care of a certain type received by the associated hourly tariff in 2013. Expenditures include public and private expenditures (including co-payments on long-term care and the mandatory deductible for medical care). Expenditures on nursing home care also include expenditures on other institutional elderly care (rehabilitative and palliative care, i.e. care packages 9 and 10).

E.2 Government spending

The reduction in government spending is a combination of the effect of the reform on use (which decreases spending by areas 1+2+3) and the mechanical transfer of payments from the government to the individual because of the higher co-payment (area 5). In our case, referring to Equation (12), the first effect is equal to A. We calculate B, the transfer to the users, by estimating the impact of the reform on average lifetime payments in Section E.3.

We are agnostic about the welfare effects of shifting costs from the government to users. However, we do include the welfare gain due to the reduction in costs associated with the (labor market) distortions of income-dependent taxes and social insurance premiums, which finance the Dutch long-term care insurance. It is disputed whether this gain should be included though, as the costs of public financing stem from the desire of the government to redistribute income through the tax system, which, at least in theory, is unrelated to the design of the co-payment system.[55] In practice, however, governments do seem to see the reduction in public spending as one of the main reasons to introduce co-payments. We therefore include the reduction of the costs of public funding in our analysis. This at least provides an idea of whether such costs are sufficiently high to justify the efficiency loss induced by the co-payment increase in the form of additional risk induced by the co-payment reform (see Section E.4).

We will use a range of values for the MCF. Jacobs (2015) provides a range of estimates of the MCF for the Netherlands based on the elasticity of labor supply and concludes that the MCF is at least 1.4 and most likely around 1.5. We also include a value of 1.3 below this range because the co-payment reform also has distortionary effects itself: the increased wealth-dependence is as an implicit tax on wealth, which may also distort labor supply and saving decisions, although likely to a lesser extent than a tax directly levied on labor income.[56]

E.3 Cost transfer to care users

To estimate the transfer of costs to the individual, we simulate the change in expected lifetime co-payments at age 70 among all older individuals with an income- and wealth-level such that they would be affected by the co-payment reform.

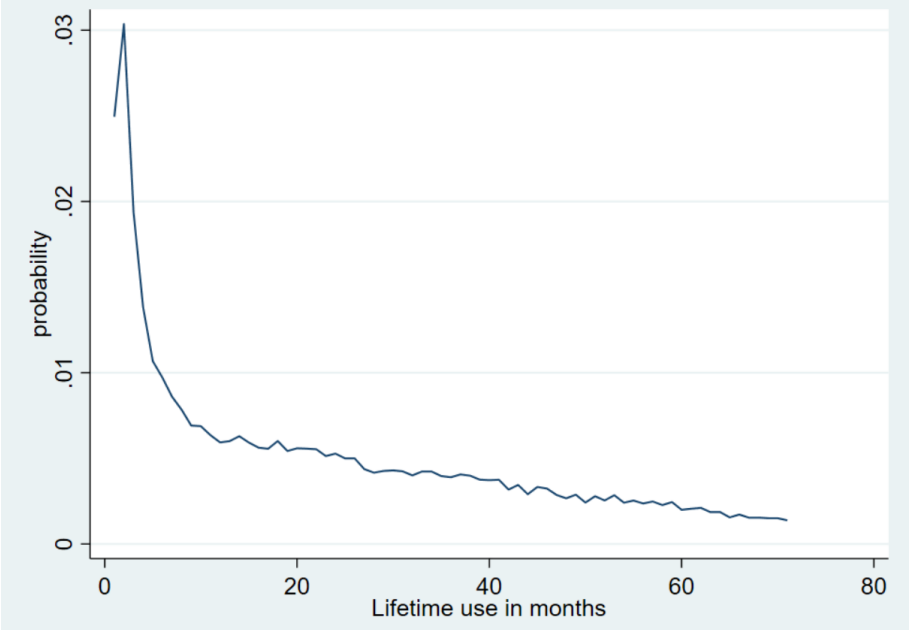
[55] Also, as the government would balance the distortionary effects of redistributive taxes with the societal benefits of income redistribution, one could thus assume that these costs are (exactly) counterbalanced by the societal benefits of redistribution (see Jacobs (2018)).

[56] A value of 1.3 for the marginal cost of public funds is also used by Shigeoka (2014), in an analysis of the welfare effect of patient cost sharing in the U.S.

To estimate the post-reform lifetime distribution of co-payments, we use data on all new admissions in 2013 by age. We limit the sample to individuals potentially affected by the reform, given their income and wealth. We can follow nursing home use until 2019, which means we can obtain the (right-censored) distribution of nursing home use in months by age of admission. To transform these into lifetime probabilities, we multiply the distribution at each age a ($71 < a < 95$) by the likelihood that a 70-year-old survives until that age without being admitted to a nursing home (based on the 2013 annual survival probabilities of the not-admitted population).

Figure E.2 provides the resulting lifetime distribution of months of nursing home use. We now have a probability distribution of the likelihood of lifetime nursing home use H, with $\mathrm{P}(H = h)$ is the probability of h months of nursing home care use (with h ranging from 0 to 72).

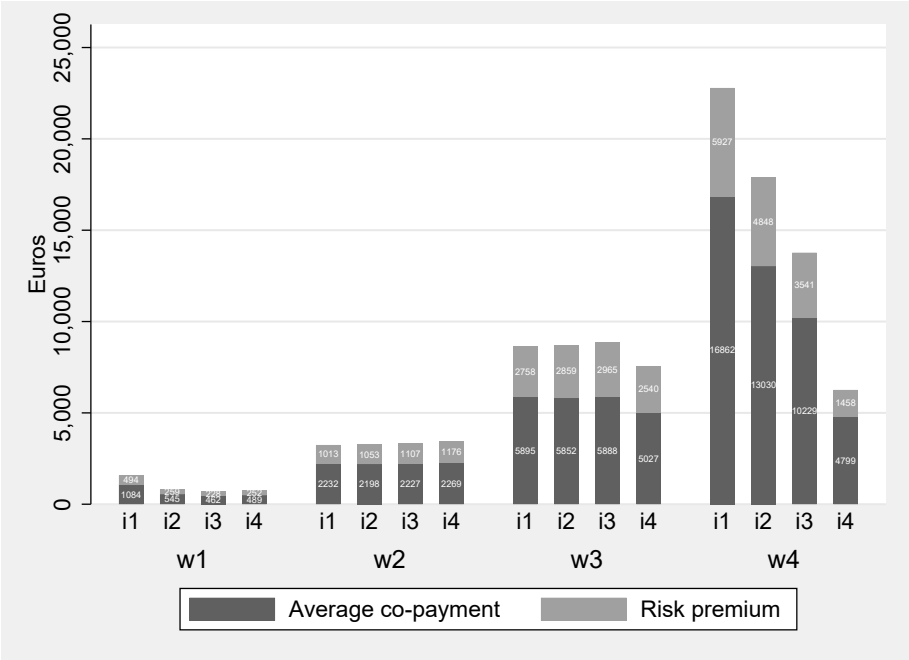
Figure E.2: Probability distribution of lifetime nursing home use in months (h).



Using the pre- and post-reform co-payment rules, we can then simulate the probability distribution of life-time co-payments. The impact of the reform differs greatly among individuals. We therefore simulate the co-payments for 16 combinations of income and wealth. We divide the potentially affected population into income and wealth quartiles, and then estimate the average income and wealth within each combination of income- and wealth-group. These group specific averages are the inputs for each simulation. Afterwards, we take the weighted average (based on the size of each income- and wealth-group) as the overall estimate.

Table VIII (in Section 8) shows the simulated distribution of pre- and post-reform lifetime co-payments for one of the 16 groups. Figure E.3 below shows the change in average co-payments across all groups (dark bars). The first column of Table E.III provides the weighted average: the reform increased lifetime co-payments among the potentially affected older adults by €4,845 (per person).

Figure E.3: The reduction in welfare (average co-payment increase + risk premium) due to the co-payment reform, for each income- and wealth-group affected by the reform



SAMPLE: Individuals who are 70 or older in 2013, single, with income and wealth such that they are potentially affected by the co-payment reform.
NOTES: Each group consists of individuals who are in a particular income quartile (i1,..,i4) and wealth quartile (w1,..,w4). 'Average co-payment' corresponds to the group-average increase in lifetime co-payments induced by the reform. 'Risk premium' corresponds to the increase in the group-average risk premium induced by the reform, assuming for this graph a risk-aversion parameter equal to 5 (see Section E.4).

E.4 Financial risk and the risk premium

The higher co-payments increase the financial risk for older individuals. To quantify the welfare effects associated with this risk, we use a standard expected utility framework. We treat the entire remaining life after 70 as a single period. The utility of consumption C is given by a standard constant relative risk aversion (CRRA) utility function:

$$u(C) = \frac{C^{1-\gamma}}{1-\gamma}. \tag{15}$$

The budget constraint is

$$Y = C + O, \tag{16}$$

where O is the lifetime amount spent on co-payments and Y is lifetime wealth, consisting of initial wealth at 70 and expected lifetime pension income. O can be calculated based on the number of months of nursing home use and the co-payment rules. We can do this, like in the previous section, using the pre-reform rules, $O^{pre}(h)$, or using the post-reform rules $O^{post}(h)$.

Expected lifetime utilities $\mathrm{E}(U)$ pre- and post-reform are equal to

$$\mathrm{E}(U^{pre}) = \sum_{h=0}^{72} u(C - O^{pre}(h))p(h) \tag{17}$$

$$\mathrm{E}(U^{post}) = \sum_{h=0}^{72} u(C - O^{post}(h))p(h). \tag{18}$$

To calculate the risk premium, we can transform the expected utilities into the certainty equivalent consumption (CEC), defined as the amount of certain consumption that provides the same expected lifetime utility as the actual uncertain consumption:

$$CEC = u^{-1}\left(\mathrm{E}(U)\right). \tag{19}$$

The welfare effect of the co-payment change for individuals in the treated group is then given by $CEC^{post} - CEC^{pre}$. This effect consists of two elements (see Wouterse *et al.* (2021)):

$$CEC^{post} - CEC^{pre} = \underbrace{\hat{O}^{pre} - \hat{O}^{post}}_{Change\ in\ average\ payment} + \underbrace{(CEC^{post} - CEC^{pre}) - (\hat{O}^{pre} - \hat{O}^{post})}_{Change\ in\ risk\ premium}. \tag{20}$$

The first element in this equation measures the change in the *average* lifetime co-payments. This is the transfer in average costs from the government to the individual (B), which we discussed and estimated above. The second element measures the rest of the welfare effect, which arises because of the changes in financial risk. We call this element the 'risk premium' as it is equal to the maximum additional amount, on top of the average payment, an individual in the treated group would be willing to pay to stay in the pre-reform co-payment scheme and thereby benefits from a higher financial protection.

Figure E.3 (displayed in the previous section) shows the risk-premium across income- and wealth-groups (assuming $\gamma = 5$). The effects are largest among the group with high wealth and low income, who are confronted with the highest increase

in co-payments due to the reform. Table E.II shows the average effect on the risk-premium across groups for different values of γ. Using $\gamma = 5$, we find that the reform decreased the lifetime value of insurance by €3,521. This is equivalent to 0.7 percent of total pre-reform welfare (CEC^{pre}).[57]

Table E.II: The effect of the co-payment reform on average co-payments and the risk premium for different values of risk-aversion (γ).

γ	Average payment	Average payment (%)	Risk premium	Risk premium (%)
3	4,845	0.971	1,979	0.397
5	4,845	0.984	3,521	0.715
7	4,845	0.977	6,002	1.210

SAMPLE: Individuals who are 70 or older in 2013, single, with income and wealth such that they are potentially affected by the co-payment reform.
NOTES: The values are weighted averages across income and wealth groups. The changes in co-payments and in the risk premium are expressed in euros and as a percentage of total pre-reform welfare.

E.5 Summing up costs and benefits

Before we can sum up all costs and benefits, we have to realize that the effects on use were estimated for individuals who are eligible for care only, while our estimates of the transfer and financial risk pertain to all older individuals with an income- and wealth-level such that they are potentially affected by the reform. To relate the moral hazard effects to the others, we multiply our estimate by the lifetime probability of becoming eligible, which is 0.56.[58]

We then have:

- (A) a reduction in moral hazard of €246, which we multiply by the MCF as they represent a reduction in government spending;
- (B) a mechanical transfer for the government of €4,845, which we value at $4845 \times (MCF - 1)$;
- (C) A decrease in welfare due to additional financial risk, ranging between €1,979 and €6,002 depending on the value of γ.

Table E.III shows the total welfare gain under different assumption about γ and the MCF. The baseline case we report in the main text is $\gamma = 5$ and $MCF = 1.3$. In this case, the total welfare loss is €3,257. A lower value of the risk premium decreases the welfare loss (to €206 in case of the $MCF = 1.3$). Whether or not the decrease in the marginal costs of fund for the government is included matters

[57] Note that pre-reform welfare depends on risk aversion.
[58] Proxied by the lifetime probability of using nursing home care.

a lot for the overall welfare effects, as their size is (potentially) much larger than that of the reduction in moral hazard. The last three rows of the table report the value of the MCF that would result in the total welfare effects to be zero instead of negative. Only in case of a risk-aversion parameter as low as 3 is the required MCF within the range reported for the Netherlands.

Table E.III: The effect of the co-payment reform on welfare, for different values of risk aversion and marginal cost of public funds.

γ	MCF	Risk premium	Moral hazard	Shift in costs	Total
3	1	1,979	246	4,845	-1,733
5	1	3,521	246	4,845	-3,275
7	1	6,002	246	4,845	-5,756
3	1.3	1,979	246	4,845	-206
5	1.3	3,521	246	4,845	-1,748
7	1.3	6,002	246	4,845	-4,229
3	1.34	1,979	246	4,845	0
5	1.64	3,521	246	4,845	0
7	2.13	6,002	246	4,845	0

SAMPLE: Individuals who are 70 or older in 2013, single, with income and wealth such that they are potentially affected by the co-payment reform.
NOTES: Total welfare gain induced by the reform, per affected individuals, in euros.

Erasmus University Rotterdam
Erasmus Centre for Health Economics Rotterdam
Burgemeester Oudlaan 50
3062 PA Rotterdam, The Netherlands
T +31 10 408 8555
E escher@eur.nl
W www.eur.nl/escher