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

Marianne Tenand* Pieter Bakx† Bram Wouterse‡

September 2023

Abstract

We assess the impact of an increase in the co-payments for nursing home care on care use and welfare. Nursing home residents often have to pay substantial user fees, which expose them to a financial risk. Co-payments may incentivize efficient care use, but empirical evidence is limited. We leverage a reform in the Netherlands that increased co-payments for a group of individuals, and implement a difference-in-differences approach. An increase in the monthly co-payment induces users to postpone permanent nursing home admissions. The resulting savings are only partially offset by increases in home care use. There are no overall effects on mortality nor on children's care use and income. While the change in the monthly co-payment is modest, average lifetime payments increase 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: I18; J14; D12; D61.

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1 Introduction

With population ageing, the demand for long-term care is on the rise, posing a societal challenge for its financing. In particular, 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] which can add up to tens of thousands of euros per year (Muir, 2017). They 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. Yet, it is not granted that co-payments actually reduce care use. It is often suggested that "no one wants to go to a nursing home"(Hitchcock, 2015). If an admission really is an option of last resort, 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; Takahashi, 2020), 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).

We investigate the effects of co-payments on nursing home care by exploiting a reform implemented in the Netherlands in 2013. This reform increased co-payments substantially for some individuals, while others were not affected. Dutch nursing

[1] Strictly speaking, 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 all types of cost sharing as *co-payments*.

home residents pay a monthly co-payment, which depends on their income and wealth. In 2013, the proportion of wealth that is taken into account in the computation of the co-payment increased. As many Dutch older people have little wealth and some exemptions apply, the reform left the price for an extra month in the nursing home unchanged for about two thirds of the population.

We estimate the price sensitivity of permanent nursing home care use by implementing a difference-in-differences (DiD) approach, comparing changes in nursing home admission rates across groups that are affected differently by the 2013 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 compute the change in co-payments induced by the reform. We identify not only its effects on nursing home use, but also on home care use, medical care costs, mortality, as well as on the health care and income of children.

We make two contributions to the existing literature. First, we provide estimates of the price sensitivity of the demand for nursing home care that has a clear empirical and theoretical interpretation. Prior research could not disentangle between simultaneous supply-side and demand-side responses to long-term care reimbursement rules (Grabowski & Gruber, 2007; Konetzka *et al.*, 2019), could not separate the effects of the price increase for nursing home care from the effect of more generous home care benefits (Kim & Lim, 2015), or studied the response of aggregate use of long-term care without separating home care from institutional care (Fu & Noguchi, 2019; Lin & Imanaka, 2020). The Dutch institutional setting enables us to isolate the demand-side response from supply-side adjustments, and to look at nursing home care separately from other health care.[2] Furthermore, we are able to

[2]A related strand of the literature studies the price sensitivity of *post-acute care* use (e.g. Hackmann & Pohl (2018)). This type of facilities is quantitatively very important in the U.S., but less so in many other countries (Bom *et al.*, 2023). Post-acute care corresponds to medical and supportive services provided to patients after a hospitalization, so that they can regain strength and autonomy and return home. In post-acute care, cost-sharing is used to incentivize quicker discharges. By contrast, nursing home care is intended as a mostly permanent residential setting for individuals with long-term care needs. There, incentives are geared to

rule out that a reduction in use following the co-payment increase arises because individuals are not able to access a nursing home anymore on financial grounds, which would cause an efficiency loss (Nyman, 1999). The design of the co-payment reform ensures that co-payments remain within the financial means of every potential user. We are thereby able to interpret a reduction in care use due to the co-payment increase as an efficiency gain, unlike previous studies.

Second, we examine a comprehensive set of outcomes, which is needed to assess the welfare effects the co-payment increase had from a societal perspective. Prior research mostly investigates the impact of co-payments on nursing home care use only (e.g. Grabowski & Gruber (2007) and Konetzka *et al.* (2019)). We also identify effects on home care, medical care, mortality and effects on potential informal caregivers. Focusing on care use leaves out important other effects. 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 out-of-pocket price of evidently lifesaving medication leads individuals to reduce their use. Cognitive constraints seem especially important in the case of long-term care: 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 consider how their choice affects public expenditures on medical care and home care, nor how it affects potential informal caregivers.

We find that individuals affected by the reform reduce the use of nursing home care 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 increase in the marginal price of nursing home care that most individuals experience (the median monthly price increase was €200). Users postpone nursing

postponing admissions.

home use by 0.8 day per 100-euro increase in the monthly price. Although there is suggestive evidence of a small increase in expenditures on home care, it is trumped by the savings generated by the reduction in nursing home use. Furthermore, there are no effects on medical care and mortality on average, nor negative effects on the outcomes of adult children - who are the main source of informal care for single older adults.

Finally, we assess the welfare gains and losses associated with the reform. Although the increase in the monthly co-payment is modest, total payments increase more substantially: average lifetime payments among potential users affected by the reform increase by €4,845. Because of the increased risk of having to pay substantially higher lifetime co-payments when needing care for many years, the reform leads to a sizable reduction in the value of insurance. The welfare loss associated with this risk likely outweighs the welfare gains from the reduction in moral hazard and government spending.

2 Long-term care in the Netherlands

2.1 Social insurance for long-term care

The Netherlands provides universal and comprehensive coverage of long-term care. During the study period (2009-2014), all long-term care except for domestic help was funded through a universal social insurance scheme (AWBZ) (Schut *et al.*, 2013). Extensive coverage results in the Netherlands being one of the top spenders on long-term care worldwide. In 2014, 5.3% of the 65+ population lived in an institution (OECD, 2020), one of the highest rates in the OECD. Privately funded alternatives were virtually absent during the study period (Tenand, Hussem & Bakx, 2021; Hussem *et al.*, 2020).

The use of long-term care is rationed in two ways. First, users pay co-payments (as will be explained in Section 2.2). Second, people have to be granted *eligibility*.

The eligibility assessment is performed by the independent assessment agency CIZ (*Centrum Indicatiestelling Zorg*) based on the applicant's needs. Assessors should not take into account other factors, such as the applicant's income or supply constraints (Tenand *et al.*, 2020) and applicants do not report their income or wealth during the process. The assessor decides on the eligibility for a care setting (home care or institutional care) and the types and intensity of care.

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).[3] To reduce the number of older individuals using institutional care, the rules for institutional care eligibility were made stricter over time and these restrictions were codified in 2013 and 2014. Specifically, assisted-living facility stays were no longer funded for new applications, while rules for nursing home care were unchanged.

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 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).

The eligibility assessment restricts access to those who meet the threshold for eligibility, but empirical evidence eligible those individuals still have room for choice

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

[4] CIZ assessors play no role in care provision after someone has been granted eligibility.

on whether and when they enter a nursing home (Bakx *et al.*, 2021; Diepstraten *et al.*, 2020). This suggests there is scope for moral hazard, whereby the societal costs of an individual's (immediate) admission would exceed its societal benefits.

For each resident, nursing homes are paid a per-diem rate that only depends on the patient's care package, and not on the user's co-payment. This means that nursing homes have no incentive to attract patients with specific socio-economic characteristics. In other words, paying a higher co-payment (or being richer) does not provide users with higher quality, a larger choice set, or lower waiting times.

2.2 The co-payment schedules

As the aggregate level, co-payments are limited in the Netherlands: in 2012, they covered 8% of total spending on long-term care (Schut *et al.*, 2013; Hashiguchi & Llena-Nozal, 2020). At the individual level, co-payments are a function of total income, which in the Netherlands is computed as the sum of household earnings and a fixed fraction of wealth (4%). Wealth is defined as any financial assets and real estate (excluding the net value of the own house) above an exemption threshold.

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. Potential users can estimate their co-payments using an official, detailed online simulator.[5] Co-payments are billed by CAK, a government agency distinct from CIZ.

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* 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-

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

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 €2,190 of per month (2013 value). 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.[6]

In 2011, the median annual co-payment for all-year nursing home residents was €7,635, equivalent to 56% of their available income; the median annual co-payment for home care was €185 (Bakx *et al.*, 2020).

2.3 The 2013 reform

In 2012, a reform was announced: an additional 8% of wealth was included in the computation of co-payments (i.e. on top of the 4% already included). This "wealth addition"[7] was part of a set of budgetary measures, which were based on an agreement reached between a number of political parties in April 2012, right after the fall of the government.[8] The agreement was sent to the Parliament on May 23rd, 2012.[9] and the reform was implemented on January 1st, 2013. As the reform was not part of the original government plans, anticipation effects before April 2012 are unlikely. Furthermore, the other measures included in the coalition agreement were unrelated to co-payments.

The reform increased co-payments for individuals with moderate to high levels of wealth and low to moderate levels of income. Figure 1 illustrates this by plotting the high-rate co-payment on nursing home care as a function of wealth at a

[6] In Tenand, Bakx & Wouterse (2021) (https://doi.org/10.25397/eur.16866442), we provide the detailed schedules.

[7] The reform is called the *vermogensinkomensbijtelling*, or VIB, in Dutch.

[8] See https://www.parlement.com/id/vj1ped4lzuz8/kabinetscrisis_2012_de_catshuiscrisis.

[9] See https://www.tweedekamer.nl/kamerstukken/detail?id=2012Z10576&did=2012D22281.

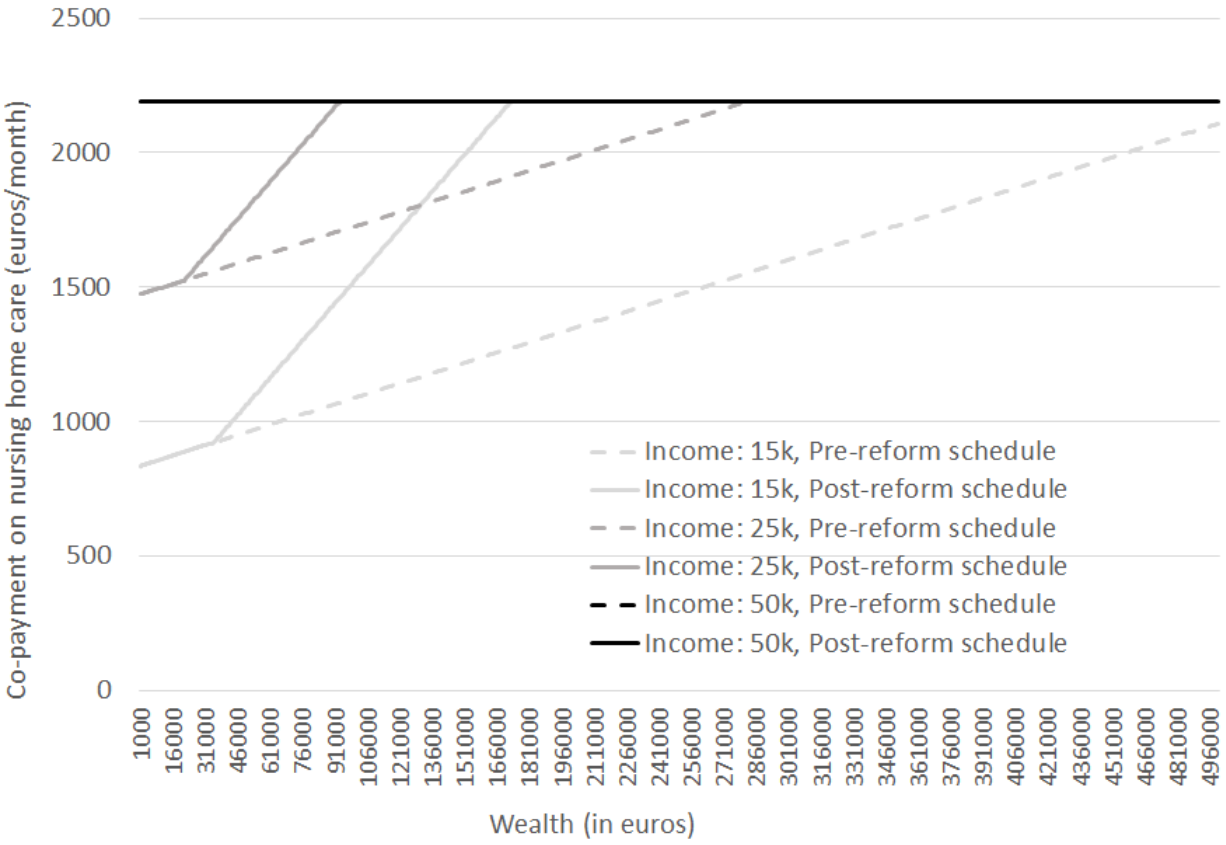
low, moderate and high levels of income, under the pre-reform and the post-reform schedules. Individuals with a wealth lower than the wealth-exemption threshold (€21,000 in 2013, the left-hand side of the x-axis in Figure 1) were not affected by the reform, whatever their income. Individuals whose income or wealth was so high that they already reached the co-payment cap prior to the reform were also not affected (this case is illustrated in Figure 1 by the highest income group, whose pre- and post-reform co-payments fully overlap). The individuals for whom the reform caused the higher increase in nursing home co-payments are those with low and moderate incomes and moderate to high levels of wealth.

The reform also led to an increase in the co-payment cap for home care Non (2017), as it depends on the share of wealth taken into account. The increases in co-payments on home care are small compared with those for nursing home care.[10]

Although the reform increased co-payments substantially for parts of the population, the design of the schedule ensured that nursing home care remained within individuals' financial means.

[10]In Section 3.4 we explain how we compute the change in the financial disincentive to enter a nursing home admission, based on the changes in the co-payments on nursing home care and home care.

Figure 1: Effect of the reform on the co-payment for nursing home care by income and wealth



NOTES: Authors' simulations of the high-rate co-payment under the pre-reform and the post-reform schedules (e.g. without and with the wealth addition). 'k' stands for thousands euros. The other parameters of the co-payment schedule (e.g. minimum and maximum co-payments) are set to their 2013 values. Income and wealth in 2013 euros. Income is defined as net total annual income. Wealth is defined as financial assets and real estate excluding own residence (see above). The 5^{th} percentile (resp. the 75^{th} and the 95^{th} percentiles) of the wealth distribution equals 28k euros (resp. 92 and 455k euros).

3 Data and sample

3.1 Administrative data

We use individual-level data from administrative registers, which are linked using pseudonymized individual and household identifiers. First, we use data on eligibility decisions from CIZ that specify when someone is eligible and for which type of care. Second, we use data from CAK on long-term care use. For each institutional stay, 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 also know whether and how much home care was used. Third, we retrieve age, date of death, gender, marital status, household composition, children who are alive, and municipality of residence from the population register. Fourth, we link data from the Tax Office on household income and wealth, and employment. Fifth, we add claims data from the mandatory social health insurance scheme, which pays for the majority of spending on medical care in the Netherlands. Finally, we retrieve data on the income and medical care expenditures of the children of individuals in the study population.

3.2 Study population

We use four criteria to define the study population. First, we focus on older adults, which we define as individuals who are at least 66 years old, i.e. who have reached the statutory retirement age.[11] Second, we focus on individuals who become eligible for a nursing home stay for a somatic or psycho-geriatric condition, between January 2009 and December 2014.[12] Third, we select only those who become eligible *for the first time*.[13] Individuals are included if they were not beforehand eligible for

[11] Co-payment rules depend on whether the user has reached the statutory retirement age.

[12] Information on eligibility is not available prior to 2009. We also do not to extend the analysis to more recent years as the long-term care system was substantially reformed in 2015.

[13] These first-time eligible represent 1% of the 66+ population in a given year.

any institutional care (nursing home care, assisted-living facilities, post-acute rehabilitation centers and hospices) during the period between 2009 and 2014. 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 more complex for them. In 2013, 63% of older adults admitted to a nursing home for the first time were singles. The study population counts 79,559 individuals.[14]

3.3 Outcome variables

We define two sets of outcomes. The first set consists of two measures of nursing home use: the dummy variable Use_{it} indicates whether an individual i has used any nursing home care within the 12 months following the day of their first eligibility at time (month) t. The variable $Duration_{it}$ is the number of days spent in a nursing home within 12 months after t.[15] We choose a reference period of 12 months because the chance of a first-time admission becomes very small 12 months after eligibility.[16].

The second set consists of outcomes measuring the broader impact of co-payments on social welfare. These include 2-year mortality as well as expenditure on medical care and home care in the first two consecutive *calendar years* after eligibility.[17] Moreover, we include a number of outcomes for the most likely potential caregivers: the children of older individuals. We include these children's primary income and medical spending (as a proxy for their health) in the first calendar year after their parent becomes eligible for nursing home care.

[14] Appendix B.4 provides further details on sample selection.

[15] When computing these variables, we exclude the stays in specialized institutions such as the psychiatric hospitals and the centers for the mentally handicapped. 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 only about 7% of admissions. Therefore, for simplicity, we refer to our outcomes as capturing *nursing home* care use.

[16] See Figure 2 further below.

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

3.4 Calculating the price for nursing home care

Finally, for each individual we calculate the co-payment change induced by the reform. We do so by combining the co-payment rules, as explained in Section 2.2, with the administrative data on the individual's income and wealth.

We define the *net price for nursing home care* for individual i, p_i, as the difference for i between the co-payments for an extra month in a nursing home, and the monthly co-payment for home care. Eligible individual i will enter a nursing home in a given month if the utility they derive from the stay exceeds its net costs. The latter are all the lower as the out-of-pocket costs incurred when staying at home and receiving home care are high.[18]

The 2013 reform changed p_i by increasing the co-payments on both nursing home care and home care, to an extent that depended on the wealth and income of individual i. We compute the *change* in the net price induced by the reform, Δ_i, as the difference between the net price under the post-reform co-payment rules (p_i^{post}) and the net price under the pre-reform rules (p_i^{pre}).

$$\Delta_i = p_i^{post} - p_i^{pre} \tag{1}$$

$$= (p_i^{NH,post} - p_i^{HC,post}) - (p_i^{NH,pre} - p_i^{HC,pre}) \tag{2}$$

where $p_i^{NH,pre}$ and $p_i^{NH,post}$ correspond to the co-payment for an extra month in the nursing home, under the pre- and post-reform schedule respectively, while $p_i^{HC,pre}$ and $p_i^{HC,post}$ correspond to the co-payment for an extra month with home care, under the pre- and post-reform schedule respectively.

When computing $p_i^{NH,pre}$ and $p_i^{NH,post}$, we refer to the monthly *high-rate* co-payment on nursing home care. Because the co-payment in the first six months of use is lower than the one after that period, the *effective* marginal price is different

[18] Appendix C sketches a model of nursing home admissions and provides support to the hypothesis that this price definition reflects the financial incentives that are relevant for the individual's decision to enter a nursing home in a given month.

from the *spot* price. For an individual deciding between going to a nursing home now or in the next month, the difference in price depends on whether delaying a nursing home entry reduces the amount of time spent under the high-rate co-payment regime or the low-rate one (see Appendix C). As most individuals survive up to at least six months following first admission (see Table I), we consider the *high-rate* co-payment to be the effective price.

For home care, the monthly co-payment charged to the user depends on the care volume. To compute $p_i^{HC,pre}$ and $p_i^{HC,post}$, we refer to the care package the individual is entitled to receive in the nursing home. We retrieve the number of hours of personal care, nursing care, and guidance that the care package contains, and derive the co-payment that would be associated with these hours.[19]

3.5 Descriptive statistics on the outcomes and individual characteristics

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 owns their main residence. 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% have 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 individuals 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.

[19]The care packages are described in Appendix A. The results are robust to assuming that individuals choose between a nursing home stay and staying at home with a *lower*, rather than equivalent, volume of home care. See Appendix D.3.

About 80% of those who become eligible end up entering a nursing home within a year. The unconditional number of days spent in a nursing home within the first year is 164 days, with a standard deviation of 139 days. Yet only a small share of newly eligible enter a nursing home immediately after the eligibility decision. Figure 2 shows the timing of a nursing home admission. 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. The figure suggests that most of the admissions take place within one year after the eligibility decision.

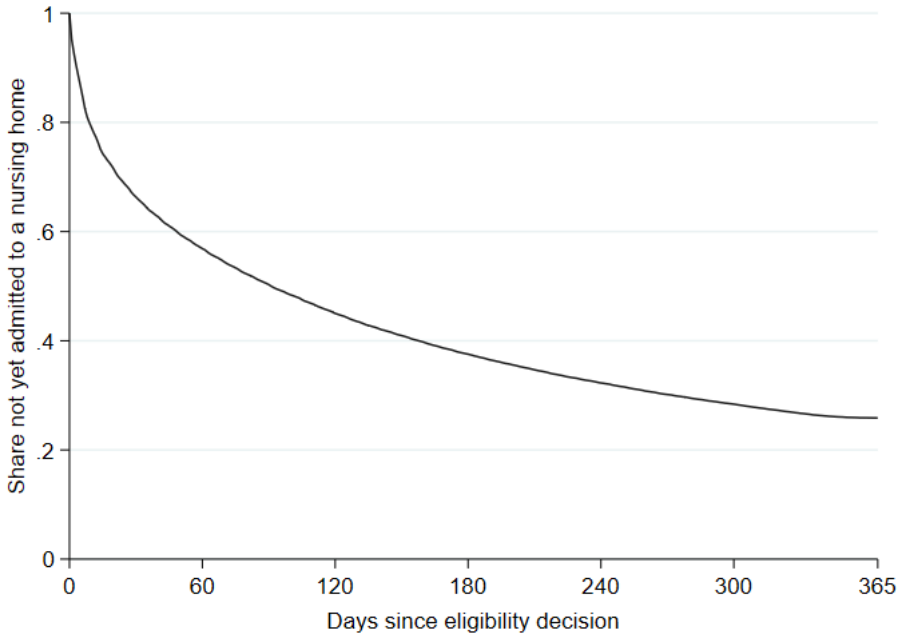
Table I: Descriptive statistics: Study population of first-time eligible.

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]	41,250	36,986
Home care expenditures[a]	14,436	19,016
Medical care expenditures[a]	15,565	21,263
Hospital care expenditures[a]	7,932	12,483
Total care expenditures[a]	71,251	41,381
Children's median medical care expenditures[b]	2355	5528
Children's median income[c]	32533	35247
Prices for home care and nursing home care		
Monthly price for nursing home care[d]	1,058.6	369.6
Monhtly price for home care[d]	105.7	93.7
Reform-induced change in the net monthly price for nursing home care	116.9	93.7
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	19,238	9,750
Total wealth (net)	142,654	355,250
Gender: woman	0.779	
Age: 66-74	0.080	
Age: 75-79	0.126	
Age: 80-84	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.
[d]: Co-payments for an additional month of nursing home care or home care, computed based on the pre-reform co-payment rules, in 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.

3.6 Descriptive statistics on the price for nursing home care

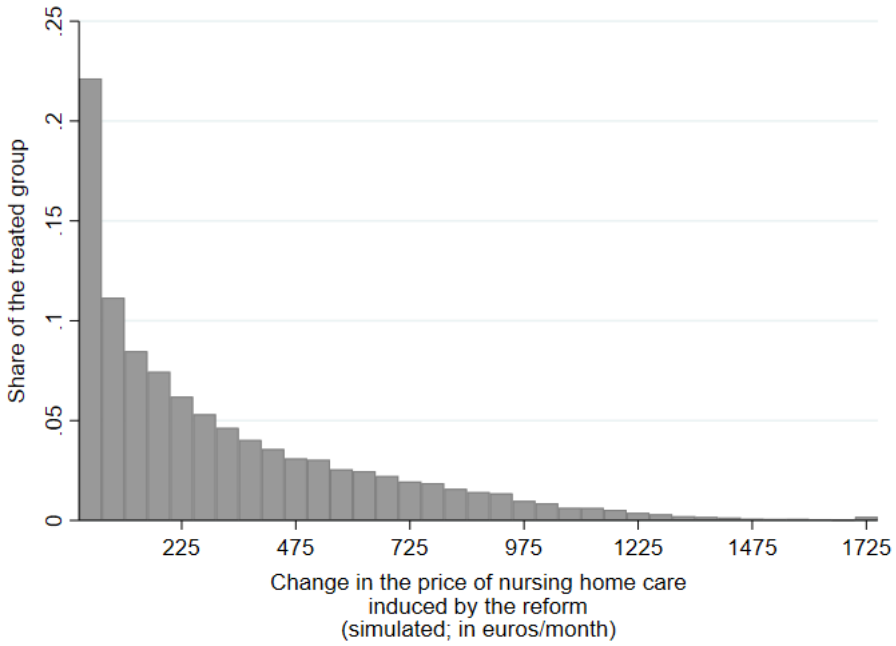
As shown in Table I, the average co-payment for an extra month in the nursing home is much higher than the co-payment for an equivalent package of home care (€1,058 against €105).

The co-payment reform translated into an increase in the net price for nursing home care (as defined in Equation (1)) of €117 on average. However, 63% of the study population (N=50,179) have income and wealth such that the net price is exactly the same under the pre-reform and the post-reform co-payment rules. For the remaining 37% (N=29,380), the co-payment reform means an increase in the nursing home price.

Within the latter group, there is considerable variation in the price increase. Figure 3 plots the distribution of the change in the monthly price. The median

increase induced by the reform amounts to around €200 per month. For 25% of the group affected by the price increase, the increase is less than €60 per month, while 5% face an increase of at least €970 per month. In relative terms, the increase was substantial for many individuals: the median increase is 20% and the increase exceeds 170% for 2.5% of them. Yet, 25% 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 net 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.

The largest increase in price was experienced by those in the top 10% of the wealth distribution with a low to average income (the increase across wealth and income groups is displayed on Figure D.1, in Appendix D.1). Being exposed to an increase in the nursing home price thus depends primarily on one's wealth, but treatment intensity also correlates with income. Individuals with lower than median wealth are not affected by the reform.

4 Empirical strategy

4.1 A difference-in-differences approach

To assess the causal effect of the increase in co-payments on nursing home care, we implement a difference-in-differences approach. A simple before-after comparison of co-payments and nursing home care use would provide a biased estimate 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). We take advantage of the fact that the co-payment reform led to an increase in the net price for nursing home care only for some individuals, and that the price change differed across affected individuals. We then compare the change in nursing home use between individuals who were (more) affected by co-payment reform, and those who were not.

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

$$Treat_i = 1 \iff \Delta_i > 0 \tag{3}$$

Δ_i corresponds to the *treatment intensity*, which we express in hundred euros per month.

4.2 Econometric specification

We estimate the following difference-in-differences equation:

$$Y_i = \beta_0 + \beta_1 Post_i + \beta_2 Treat_i + \beta_3 Post_i \times Treat_i \times \Delta_i + X_i\theta + u_i \tag{4}$$

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).[20] Equation (4) extends the basic difference-in-differences specification by including treatment intensity Δ_i (Duflo, 2001).[21] X_i' is a vector of individual characteristics (at the time of first eligibility) and u_i an individual error term. The model is estimated using Ordinary Least Squares (OLS) and we report robust standard errors.

The specification includes one observation per individual, such that identification comes from inter-individual variation in the timing of first eligibility, and in income and wealth. The primary outcomes being defined as cumulative care use in the first 12 months after an individual's first eligibility, we capture the total effect of the co-payment increase on nursing home admissions,[22] which we take it to be the most relevant treatment effect to analyze the welfare effects of the reform and its policy implications Callaway & Sant'Anna (2021). Further, we aggregate the treatment effect over calendar time to increase statistical power.

The coefficient β_3 captures the price sensitivity of nursing home care use, assuming a linear effect of the price change on the outcomes.[23] β_3 reads as the effect of a €100 change in the net price for an additional month in the nursing home. When the outcome is Use, β_3 captures the price sensitivity at the extensive margin; when the outcome is $Duration$, this coefficient reflects by how many days the increase in the price of nursing home care *delays* a nursing home admission. This interpretation rests on the assumptions that (i) residents do not exit the nursing home and (ii) there is no mortality effect of the reform (see Appendix C). We check the latter assumption later on. Regarding the former, among individuals in our

[20] Consistent with the definition of the relevant price, 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 price of nursing home care depends on the post-reform co-payment rules.

[21] Equation (4) does not contain a separate Δ_i term. Instead, we choose for a more flexible specification by controlling for income quintiles and four wealth groups: within the study population, Δ_i is a function of only income and wealth, and income and wealth may have an influence on the baseline use of nursing home care beyond their impact through the copayment schedule.

[22] The effect is aggregated over a period of 12 months following eligibility, within which admissions and postponements thereof are expected to take place, as explained in Section 3. In a robustness check (Section 6), we dis-aggregate the effect by event time.

[23] In Appendix E.4 we explore the appropriate functional form of this relationship further.

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, which implies that discharges back to the community are rare.

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 with the control group, all the more as they are exposed to a higher price increase.

4.3 Identifying assumptions

A crucial identifying assumption is that the control group provides a valid counterfactual for the evolution of nursing home care use in the treated group, would the reform not have been implemented (the parallel trend assumption, or PTA). 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. This makes the PTA likely to hold.

Still, in the context of our study, we see two 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 because of trends in determinants of nursing home care unrelated to the reform, e.g. changes in age composition or income and wealth across cohorts. We address this concern by including a rich set of covariates. Those include interactions between gender and 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; a dummy for whether the individual is a home owner; wealth quintiles and twenty 5-percent disposable

income groups to capture potential wealth and income effects on nursing home use. 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.[24]

A second potential threat to the PTA is that other policy changes affecting the control and treated groups differently may have occurred around the same time as the co-payment reform. In order to rule out this concern, we do two things. First, we examine pre-trends. Although parallel trends in nursing home 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 may help pinpointing a deviation from the PTA. Second, we examine the only other relevant policy change happening in the study period. In 2013 and 2014, stricter eligibility criteria for admissions to elderly institutional care were implemented in order to decrease institutionalization rates among the Dutch older people. In the following section we present evidence suggesting that this reform does not interact with the co-payment reform and examine the pre-trends .

4.4 Parallel pre-trends and effect of the reform: graphical evidence

Figure 4 displays the trends in nursing home care use. The vertical line indicates the year in which the reform was announced (2012). The outcome in Panels A and B is the probability of nursing home care use, while it is the duration of nursing home care use in Panels C and D. The black series displays nursing home care use in the control group; the grey series displays nursing home care use in a subset of the

[24]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 extremely close to those from the main specification.

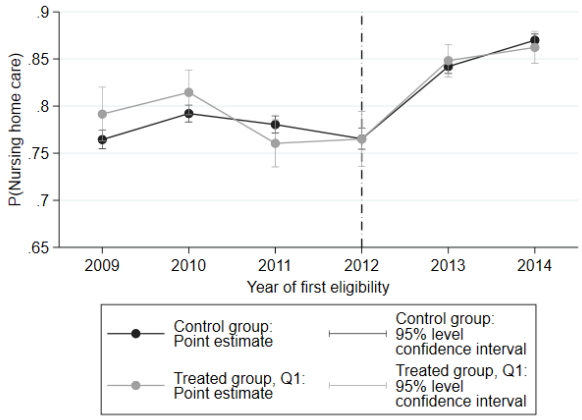
treated group. As our identification leverages the magnitude of the price increase (treatment intensity), we display pre- and post-trends across different levels of the price change. Therefore, we split the treated group in four quartiles of treatment intensity. Panels A and C of Figure 4 display nursing home care use among the 25% treated individuals with the lowest increase in their nursing home care price, while Panels B and D display nursing home care use among the 25% treated individuals with the highest price increase. In Appendix D.2, Figures D.2 and D.3 present the comparison between the control group and all four quartiles of treatment intensity.

Parallel pre-trends

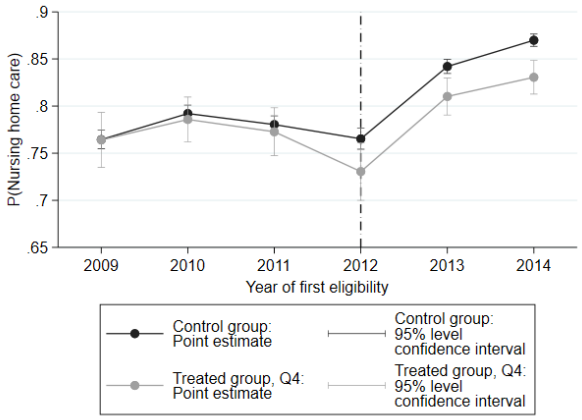
Focusing on the years prior the reform (2009 to 2011), Figure 4 shows that, before the reform, the trends in nursing home care use, and also their levels, were similar in the treated and control groups. On top of the visual inspection, we statistically test for the existence of differential pre-trends. We fit a dynamic version of Equation (4), in which we interact each quarter with the price change. The omitted quarter is the first quarter of 2012 (the last quarter before the reform). If, before this date, trends in care use are the same whatever the magnitude of the price increase, then we expect all coefficients of the quarter-price interactions prior to the reform to be zero. We therefore implement a Fisher test for the joint significance of pre-reform coefficients. The results are presented in Table D.I in Appendix D.2. They show no evidence of diverging pre-trends.[25]

[25]The p-values are 0.31 (when the outcome is probability of use) and 0.30 (duration of use), such that we cannot reject the null hypothesis that the coefficients are all zero. In addition, we run the same tests for the other outcomes and a simple, binary DiD specification; the results (Tables D.I to D.IV) do not show evidence of diverging trends either.

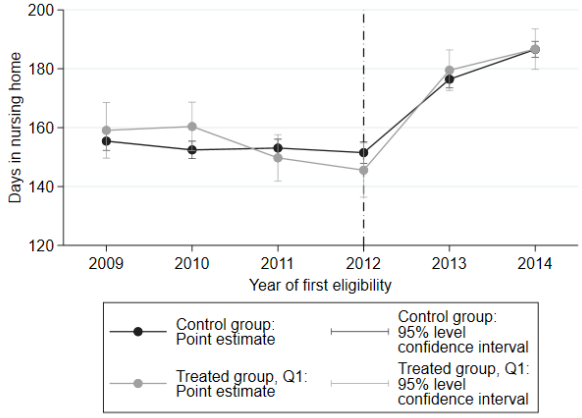
Figure 4: Use of nursing home care, depending on treatment intensity, by year of first eligibility.



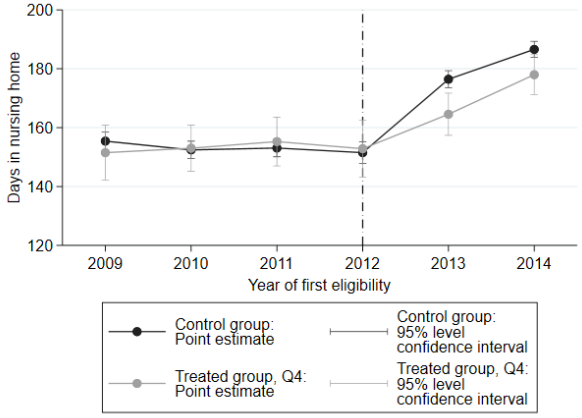
Panel A: control group versus 1^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel B: control group versus 4^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel C: control group versus 1^{th} quartile of the treated group.
Outcome: days spent in a nursing home.



Panel D: control group versus 4^{th} quartile of the treated group.
Outcome: days spent in a nursing home.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Nursing home care use within the 12 months following the individual's first eligibility for nursing home care.
NOTES: Within the treated group, quartiles are defined with respect to treatment intensity: quartile 1 (resp. 4) groups the 25% individuals who experienced the smallest (resp. largest) change in the nursing home care price due to the reform. The reform was announced in Q2-2012. In the graphs, year 2012 excludes observations from Q1-2012. In Appendix D.2, Figures D.2 and D.3 present the comparison between the control group and all four quartiles of treatment intensity.

Trends following the reform

Turning to the years following the reform, Panels A and C of Figure 4 show that nursing home care use has followed similar trends in the control group and among individuals with a very low (i.e. less than €60 per month) increase in the price for nursing home care. By contrast, the probability of nursing home care use and its duration have increased less in the control group than for individuals who have experienced the highest price increase with the reform (Panels B and D). These patterns are consistent with an increase in the nursing home price leading to a decrease in use. The results for the second and third quartiles of the treated group (in Appendix D.2) show a similar pattern: differences in nursing home use across the control and treated individuals following the reform are visible in quartile 3 of the treated group (in which individuals experience a price increase higher than €200 euros/month), but not in quartile 2.

Upward trend in use and tightening of eligibility criteria

Figure 4 also shows an upward trend in nursing home care use, which is taking place only starting in 2013, and is visible both in the treated and control groups. This trend is likely related to the tightening of the eligibility criteria for nursing home care: if the severity of disability among those who become eligible increases, their care use is expected to increase as well.

We see one reason why the tightening of the eligibility criteria could confound the identification of the effect of the co-payment increase. Richer individuals - those exposed to a higher price increase - may have retained better access to nursing home care despite the tightening of eligibility criteria (by being able to make the case that they qualify for such care) than those, poorer, in the control group. We would expect such individuals to be entitled to care package 4, which in 2013 became the new threshold to become eligible for nursing home care. If this happens, individuals from a higher socio-economic background who are eligible for nursing home care would

tend to have on average lower (unobserved) care needs than those from a lower socio-economic background in 2013 and 2014. Such a pattern could then partly explain why those exposed to a higher price increase have a lower nursing home care following the co-payment reform, than individuals not subject to a change in the nursing home price.

We conduct two checks to dismiss this concern. First, we replicate Figure 4 excluding individuals who become eligible for care package 4 (see Figure D.5, Appendix D.2). As this is the lowest care package still available after the tightening of the eligibility criteria, we expect any effects of this tightening on the composition of the group of nursing home users to concentrate there. When excluding care package 4, the upward trend in nursing home care use indeed almost completely disappears. We do still find that, following the reform, those with a higher price change have lower nursing home care than individuals in the control group.

Second, we show that individuals subject to a higher increase in the price for nursing home care are not more (nor less) likely to become eligible for nursing home care than individuals less affected by the price change, following the reform (see Section 6). These patterns make it unlikely that the tightening of eligibility criteria has led to differences between the control and treatment groups in our baseline sample of eligible individuals. It is therefore not expected to confound our identification strategy.

5 Main results

5.1 The impact on nursing home admissions

Table II presents the estimates of the effects of the nursing home care price increase on the probability of any use (Column 1) and the number of days spent in a nursing home (Column 2) within the 12 months following first eligibility. A €100 increase in the net price decreases the probability of entering a nursing home by 0.3

percentage point (β_3 in Column 1 of Table II). The unconditional number of days spent in a nursing home decreases by a little less than one day (0.821 day, Column 2).

Consistent with the graphical evidence, the coefficient *Post* is positive, reflecting the upward trend in nursing home admissions conditional on eligibility over the study period. The coefficient *Treat* is not statistically significant: nursing home use prior to the reform was similar in the control and treated groups.

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

Outcome:	P(nursing home use)	Days in nursing home
	(1)	(2)
Post	0.069***	28.84***
	(0.010)	(3.349)
Treat	0.007	1.201
	(0.004)	(1.552)
Post.Treat.Δ	-0.003***	-0.821**
	(0.001)	(0.323)
Control variables	Yes	Yes
Quarter fixed effects	Yes	Yes
Observations	79,559	79,559
R^2	0.060	0.053

NOTES: Robust standard errors in brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.

An increase in the price for nursing home care has a larger impact on nursing home care use for some groups than for others, as shown in Table III, Columns 1 and 2. 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 is the group that postpones an admission most. They spend 1.861 fewer days in the nursing home for every 100-euro increase – this effect is more than twice as large as the full-study population effect (-0.821 days). This finding suggests that the price sensitivity is higher for less severely impaired individuals, possibly because

preferences for nursing home care versus living at home depend on the severity of functional limitations. 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 at the 10% level.

The magnitude of the effects are similar for people with and without children, for people whose children all work and for those who have at least one child who does not work. These findings are unexpected because adult children are the main source of informal care for single elderly. In the presence of informal care, the trade-off between entering a nursing home and staying longer at home should be more sensitive to the net price of nursing home care, if the combination of formal home care and informal care is a better substitute to nursing home care than home care alone. Statistical power may however be insufficient to detect this difference.

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	-0.003***	-0.821***	0.002*	79,559
	(0.001)	(0.316)	(0.001)	
Men	-0.003*	-1.376**	0.004*	17,608
	(0.002)	(0.648)	(0.002)	
Women	-0.003***	-0.713**	0.001	61951
	(0.001)	(0.363)	(0.001)	
Has children: No	-0.003*	-0.766	0.005**	16,101
	(0.002)	(0.608)	(0.002)	
Has children: Yes	-0.003***	-0.874**	0.002	63,458
	(0.001)	(0.376)	(0.001)	
All children work: No	-0.004**	-0.849*	0.001	38,086
	(0.001)	(0.506	(0.002)	
All children work: Yes	-0.003*	-0.890	0.004**	25,372
	(0.002)	(0.563)	(0.002)	
Care package 4, dementia	-0.004	-0.649	0.005**	17,010
	(0.002)	(0.682)	(0.002)	
Care package 4, no dementia	-0.003	-1.861**	0.001	13,327
	(0.002)	(0.773)	(0.003)	
Care package 5	-0.003*	-0.696	0.003	27,761
	(0.001)	(0.528)	(0.002)	
Care package 6 to 8	-0.003**	-0.499	-0.001	21,461
	(0.001)	(0.630)	(0.002)	

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

5.2 Mortality effects

We do not find compelling evidence for an effect on 2-year mortality on average. As shown in Table III (p. 30, Column 3), the point estimate of the treatment effect is 0.002, which means that a 100-euro increase in the co-payment increases the mortality probability by 0.2 percentage point. This is small relative to the baseline mortality (50% of individuals die within two years). The estimate is also not statistically significant at the 5% level.

We do however find a significant effect on the mortality probability for people with dementia who are eligible for the lowest level of nursing home care (+0.5 percentage point). This heterogeneity is not related to a larger change in care use, as the treatment effects on use are similar to the other groups (Column 1 of Table III). Hence, these results suggest that the mortality impact of postponing a nursing home admission is different for these 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 percentage 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 adverse health effects of postponing a nursing home admission.

5.3 Effects on 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.[26] 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 net price of nursing home care. This is 0.4% of the average health care expenditures in the year of first eligibility and the year thereafter (€71,251). This decrease is almost entirely driven by the decrease in nursing home expenditures, by €-299.4.

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).

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

[26]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.

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 an average of 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 hour (or about 0.8 hour for a day more out of the nursing home) within the 12 months following eligibility.[27]

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.

5.4 Effects on potential informal caregivers' income and medical care spending

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. We interpret this result as evidence that a postponement of nursing home admissions has had no 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

[27] These additional results on home care hours are presented in Appendix D.3.

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 brackets; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.

6 Robustness checks

We perform a number of robustness checks. First, we use the difference-in-differences model from Equation (4) to analyze whether the reform had an effect on the probability of becoming eligible for nursing home use. The reform might not only have affected use *conditional* on eligibility, which is our main outcome, but also the propensity of individuals to apply for eligibility if they become less likely to plan to enter a nursing home. Our results (in Appendix E.1) show a precisely estimated zero effect on eligibility, which means that the effect of the change in the private price of nursing home care runs entirely through conditional use.

Second, there may be concerns that the reform led to strategic reallocation of wealth. After the reform, it became more financially attractive to deplete or reallo-

cate wealth holdings to avoid high co-payments. Because the government uses a two year lag for the wealth included in the co-payment calculation, and we only consider the first few years after the reform, we do not expect any such effects during this study period. Nonetheless, we test this in Appendix E.2: we look at the differences in wealth between a group of first-time eligible and comparable individuals who do not become eligible for nursing home care, before and after the reform. If the reform lead individuals to lower their wealth so as to decrease their co-payments, the difference across the two groups should increase over time. We find evidence that this is not the case.

Third, 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. These are averages over the individual decisions to enter a nursing home, or postpone admission in each of these 12 months. As an alternative, we estimate separate regressions for each month since eligibility for both nursing home entrance (the monthly 'hazard rate') and the probability of being in a nursing home (the 'cumulative hazard'). The results (provided in Appendix E.3) provide additional insights in the time dynamics of the effects of the reform, with the largest impact being the decrease in the probability of nursing home entry in the second month after eligibility.

Fourth, we replace the linear price effect we impose in our main estimation equation with a more flexible one, using six 200-euro bins for the change in price. Based on this flexible specification, we conclude that the linear price effect is a relatively good proxy of the true functional form (see Appendix E.4). Furthermore, in Appendix , we fit a similar specification using bins based on the change in the *relative* price rather than the change in the *absolute* price. The results indicate that the latter offers a better fit to the data.

7 Welfare effects of the reform

The co-payment reform leads to a decrease in nursing home care use and public spending, which, in the absence of negative effects on health or informal caregivers, represents welfare gains. However, the reform increases financial risk for potential users, i.e. the probability to face high(er) out-of-pocket expenditures on long-term care in old age. Although the increase in the monthly co-payment is relatively small compared to individuals' financial resources, it may lead to a substantial financial risk, because there is a chance of needing nursing home care, and thus paying the co-payments, for several years.

This section provides an estimate of the increase in financial risk due to the reform and the welfare loss that this risk induces. Then, we compare this loss to the welfare gains due to lower moral hazard and lower costs of public spending. We follow an approach similar to those of Feldstein & Gruber (1995); Finkelstein, Amy and McKnight, Robin (2008); Engelhardt & Gruber (2011); Barcellos & Jacobson (2015); Shigeoka (2014).[28] Note that we focus on the *efficiency* aspects, and leave aside any welfare change that may arise if the co-payment reform leads to a more (or less) equitable financing of long-term care.

7.1 An increase in financial risk

Because the financial costs of co-payment are concentrated among the individuals who use care for several years, we need to identify the effects of the reform on the distribution of *lifetime* co-payments to understand the impact of the reform on the financial risk. We estimate the impact of the reform on lifetime co-payments for a 70-year old, who has such an income and wealth that *if* they would become eligible for care, they 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

[28] Details can be found in Appendix F.

(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 VI shows the distribution of lifetime co-payments for one of these groups: individuals with a disposable income of €19.900 and a financial wealth of €101.200, which are the averages of individuals who are in the 2nd income quartile and 3rd wealth quartile. For this particular group, the reform shifts the distribution of co-payments substantially to the right. The average value increases from €11,556 to €17,408. For the 5 percent highest users of nursing home care within this group, co-payments increase from €68,924 or more, to €104,491 or more.

Table VI: The distribution of lifetime co-payments for individuals with €19.900 of disposable income 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 average values for individuals who are in the 2nd income quartile and 3rd wealth quartile in 2013.

To express the welfare effect of the increased financial risk in monetary terms, 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. Table VII shows the effect of the reform on average lifetime co-payments and the associated risk-premium under different values of risk-aversion. For example, when using a risk-aversion parameter of 5,[29] we find that, among those potentially affected by the reform, the reform decreases 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).

Table VII: 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 (euros)	Average payment (%)	Risk premium (euros)	Risk premium (%)
3	4,845	97.1%	1,979	39.7%
5	4,845	98.4%	3,521	71.5%
7	4,845	97.7%	6,002	121.0%

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: Payments are defined as the average changes in co-payments. Variations in payments and in the risk premium are expressed in euros and in percentages. Percentages refer to the percentage of total pre-reform welfare (measured by the certainty equivalent consumption). The values are weighted averages across income and wealth groups.

7.2 Relative magnitude of welfare gains and losses

Based on our econometric estimates, we infer that the reduction in moral hazard is quite small (Appendix F): the reform reduces total public spending on care by €193 per potentially affected person. How much the reduction in public spending (consisting of the reduction in public costs due to less use of care, and of the transfer of costs from the government to the users) is valued, in terms of welfare, and so how it compares with the decrease in the value of insurance computed above, depends on the marginal costs of public funds (MCF). A higher MCF implies a higher welfare gain from reducing public spending.

Because of the uncertainty regarding the MCF, and regarding the risk-aversion parameter (which matters for the value of the financial risk), we compute the net

[29] 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)).

welfare effects under different values for these two parameters (summarized in Figure F.4 in Appendix F). For a risk-aversion parameter of 5, the welfare loss is equal to €-3,328 per potentially affected person if we assume a MCF of 1 (that is, the transfer of costs from the government to users is neutral). The welfare effect would still be negative (€-809) for a MCF of 1.5, a seemingly upper bound for the Netherlands.

While the precise figures are subject to uncertainty, our calculations suggest that the welfare gains due to less moral hazard and the transfer of costs to users are unlikely to outweigh the welfare loss associated with the increase in financial risk. In other words, the cumulative feature of co-payments makes it unlikely that the co-payment reform has generated welfare gains on balance.

8 Conclusion

To what extent do older people postpone a nursing home admission when exposed to higher co-payments? And what are the welfare effects of a co-payment increase? This paper provides empirical evidence on these questions. We leverage a reform of co-payment rules in the Netherlands, and combine detailed information on these rules with individual administrative data on income and wealth. Thereby we retrieve for each older adult the increase in the price of nursing home care induced by the reform, and assess its impact on care use and other relevant outcomes.

We find that single individuals eligible for nursing home care who are affected by the reform reduce their nursing home use. 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 a nursing home 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 decreases by 5 percentage point. We do not find evidence of externalities or negative health effects on average. However, we find suggestive evidence that the co-payment might lead

to an increase in mortality for individuals with cognitive problems or no potential informal care support.

Our overall conclusion is that the demand for nursing home care is relatively but not completely inelastic. When generalizing the results to other settings, it is relevant to identify which individuals are at the margin of nursing home use (Bakx *et al.*, 2021), which depends on the specific institutional setting. One the one hand, because of the availability of high-quality home care in the Netherlands, we might expect a high price response, as even individuals with relatively poor health might be able to postpone an admission. On the other hand, one might think that the presence of an independent needs assessment eliminates the scope for moral hazard. Eligibility criteria are not unique to the Netherlands and exist in most long-term care systems. Such criteria may limit moral hazard by defining a minimum threshold for care use. However, such thresholds are generally not perfect, and unwarranted use above the threshold might still exists. Further, needs assessors operate as imperfect agents, and may be motivated to place applicants on higher thresholds. Previous research provides evidence of this happening in long-term care systems.[30] Co-payments may then incentivize those who do qualify for nursing home care but have relatively lower benefits attached to it, to delay an admission.

Further, we find that the additional financial risk for older individuals generated by the reform is considerable. The reform shifts lifetime payments from the government to the potential users affected by the reform by €4,845 on average, which translates into a reduction of the value of insurance of several thousands euros under plausible values of individuals' risk aversion. Because of the uneven lifetime distribution of nursing home use, the increase in financial risk for potential users is substantial. The public cost saving effect from the reduction of care (of €345 per affected person) is small in comparison. From a societal perspective, the welfare

[30] Hernández-Pizarro *et al.* (2020) show that assessors in the Spanish long-term care system make pro-social decisions. For the Netherlands, (Bakx *et al.*, 2021) show that the number of hours for which applicants were eligible was not binding for more than 90% of the users, and Diepstraten *et al.* (2020) find that the physical accessibility of one's house (i.e. presence of stairs) affects the timing of a nursing home admission.

loss caused by the increased financial risk likely outweighs the welfare gains from the reduction in care use and reduced government spending.[31]

Our findings have three policy implications. First, 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; 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.

Second, a reduction in use can be achieved by relatively small marginal co-payments, which do not exceed users' financial means, without negative health effects on average. 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).

Finally, however, even relatively low and affordable marginal changes in co-payments come at the cost of imposing a financial risk on potential users of nursing home care. The welfare effect of the increased financial risk is likely larger than the cost savings achieved by the reform. 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 upon Arrow's theorem on insurance deductibles (Arrow, 1963), 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 disability, when individuals are still

[31] Additional welfare gains may arise from a more equitable distribution of co-payments, but the equity effects of the reform lay outside the scope of our study. Moreover, there are other more efficient ways of achieving equitable financing of LTC, for instance through the design of the insurance premium.

price responsive, and full insurance in later, more severe stages of disability.

How could the Netherlands - and other countries - remodel co-payments so as to improve efficiency of their long-term care system? Because the effective price of permanent nursing home care is not the spot price at entry, but the (expected) price of the last month of care used, levying co-payment in the very first months of use only is likely to be ineffective. 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 along these lines have been proposed in the U.S. (Cohen & Butler, 2021) and in the UK (Dilnot, 2011)[32]. 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.

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

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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 25th, 2018).

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Part I

Appendices (Online Material)

A Additional information on the institutional context

A.1 The co-payment schedules in the Netherlands

We retrieved information on the co-payment schedule from public CAK documentation and legislative work around the 2013 reform with the help from a policy advisor from CAK.

Detailed information on the computation of co-payments for long-term care in the Netherlands, which we have used for the purpose of the present study, have been made available in the following online material: Tenand, Bakx & Wouterse (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 indicates which type of care and how much an individual is entitled to receive, depending on their care package.

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.

B Data sources and sample selection

B.1 Overview of the microdata used

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

Table B.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

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).[33]

The linkage of individuals to their legal parents through KINDOUDERTAB is most reliable for individuals born since 1966. For our analysis, this implies that measurement errors on the characteristics of children are more likely to occur for the older cohorts in our sample.

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

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.[34]

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); van Eijkel *et al.* (2022).

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

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

B.4 Further details on sample selection

Here below we provide additional details on sample selection.

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 re-coded 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.

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.

C A model of nursing home admissions

To guide the empirical analysis and aid the interpretation of results, we introduce a theoretical model of nursing home admission. 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.[35] 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:

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

The type of care we investigate in this paper is permanent nursing home care: once individuals move to a nursing home, they mostly stay there until they die.[36] This means that the relevant marginal decision for an individual is whether to enter a nursing home now, or extend the period living at home by at least one month.

C.1 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. 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 3.4 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

[35] We abstract 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.

[36] We assume this is the case when we derive the effective 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, something we seldom 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.

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:

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

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.

C.2 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:

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

where $I[.]$ is an indicator function. Furthermore, we assume that changes in nursing home admission do not affect survival (which we test empirically). Hence, we can interpret Equation (C.3) as the average delay in nursing home admission 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 admission 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.[37]

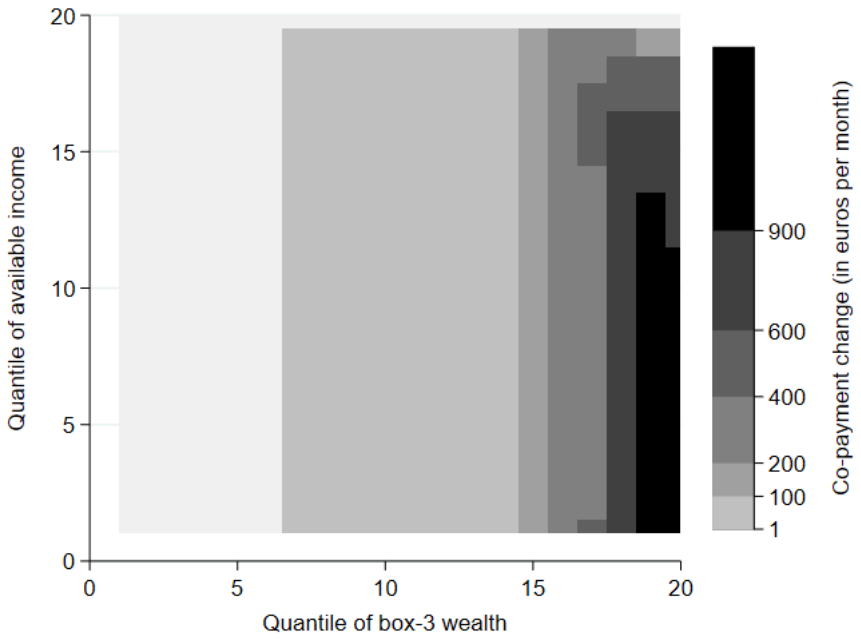
[37] As the low and high co-payments are strongly correlated, using the high price sufficiently captures the heterogeneity in treatment within the population. However, not being able to distinguish between responses to the spot price and the effective price means that we have to be careful in interpreting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

D Additional results

D.1 Change in co-payments, by income and wealth level

Figure D.1 displays the change in the average monthly co-payment for nursing home care (in euros) by income and wealth. Individuals with higher wealth experienced larger increases in the co-payment. Consistent with Figure 1, individuals with lower than median 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 D.1: Simulated change in the price for nursing home care induced by the reform (euros per month), depending on income and wealth.

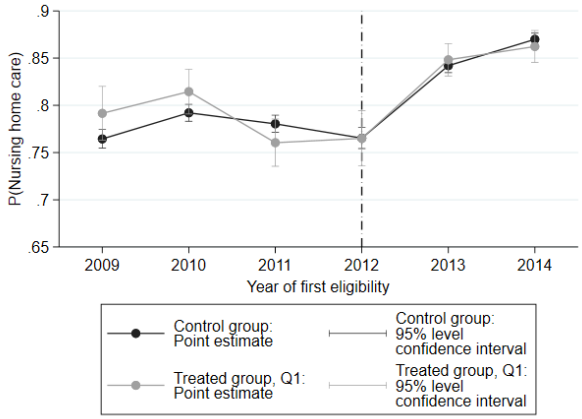


STUDY POPULATION: Individuals aged 66+ who are single and became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Box-3 wealth corresponds to wealth as taken into account in the co-payment schedule.
NOTES: Authors' simulations. The values represent the change in the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

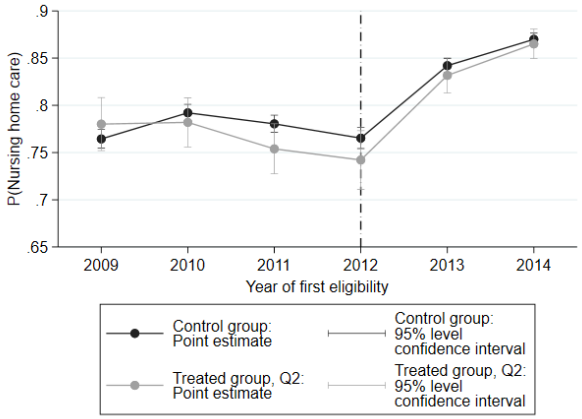
D.2 Inspection of pre-trends

Trends in nursing home care use and mortality by treatment status: graphical evidence

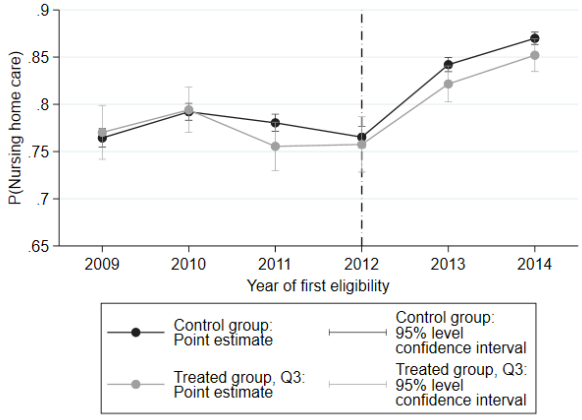
Figure D.2: Probability of use of nursing home care in the 12 months following first eligibility, depending on treatment intensity, by year.



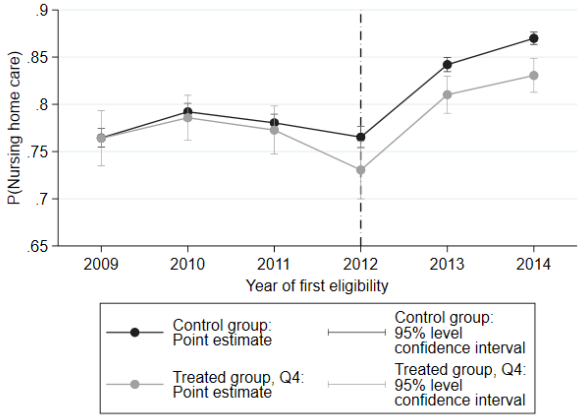
Panel A: control group versus 1^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel B: control group versus 2^{th} quartile of the treated group.
Outcome: probability of nursing home use.



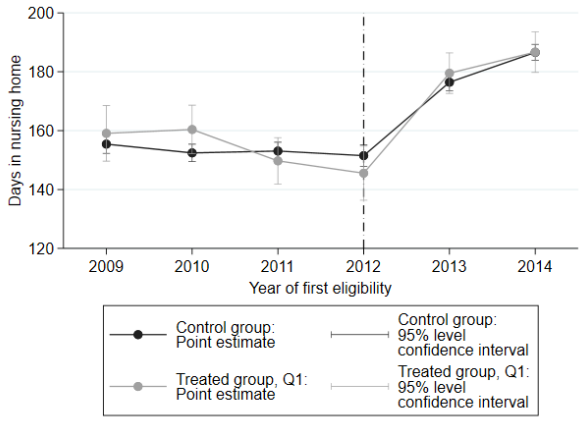
Panel C: control group versus 3^{th} quartile of the treated group.
Outcome: probability of nursing home use.



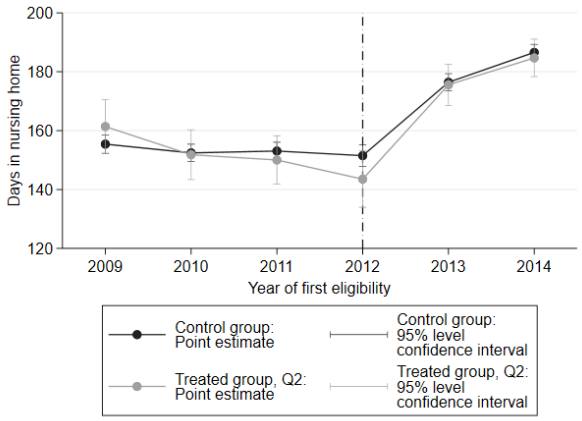
Panel D: control group versus 4^{th} quartile of the treated group.
Outcome: probability of nursing home use.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Within the treated group, quartiles are defined with respect to treatment intensity: quartile 1 (resp. 4) groups the 25% individuals who experienced the smallest (resp. largest) change in the nursing home care price due to the reform. The reform was announced in Q2-2012. On the graphs, year 2012 excludes observations from Q1-2012.

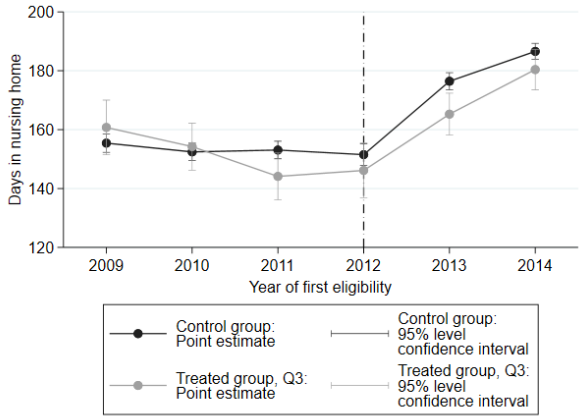
Figure D.3: Duration of nursing home care use in the 12 months following first eligibility, depending on treatment intensity, by year.



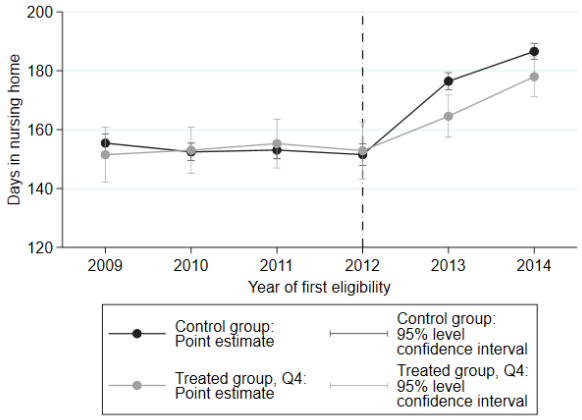
Panel A: control group versus 1^{th} quartile of the treated group. Outcome: number of days in a nursing home.



Panel B: control group versus 2^{th} quartile of the treated group. Outcome: number of days in a nursing home.



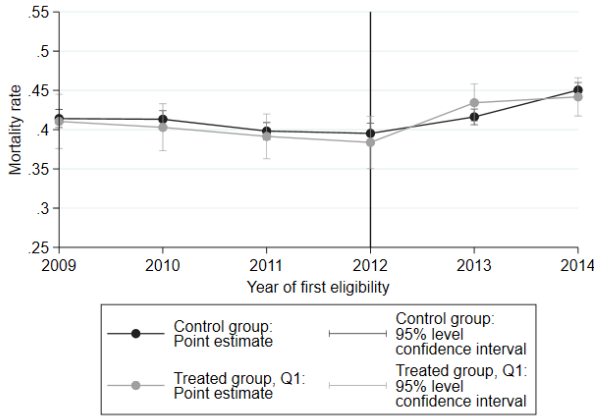
Panel C: control group versus 3^{th} quartile of the treated group. Outcome: number of days in a nursing home.



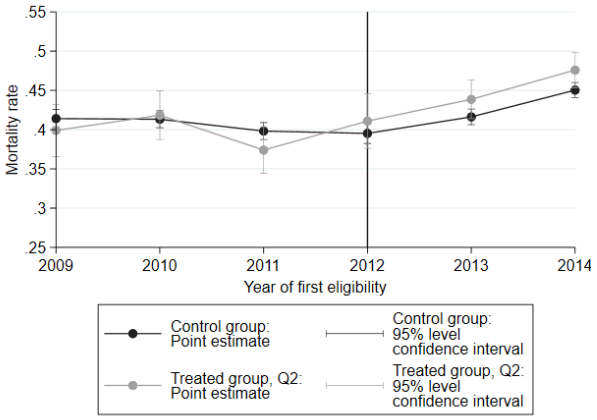
Panel D: control group versus 4^{th} quartile of the treated group. Outcome: number of days in a nursing home.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Within the treated group, quartiles are defined with respect to treatment intensity: quartile 1 (resp. 4) groups the 25% individuals who experienced the smallest (resp. largest) change in the nursing home care price due to the reform. The reform was announced in Q2-2012. On the graphs, year 2012 excludes observations from Q1-2012.

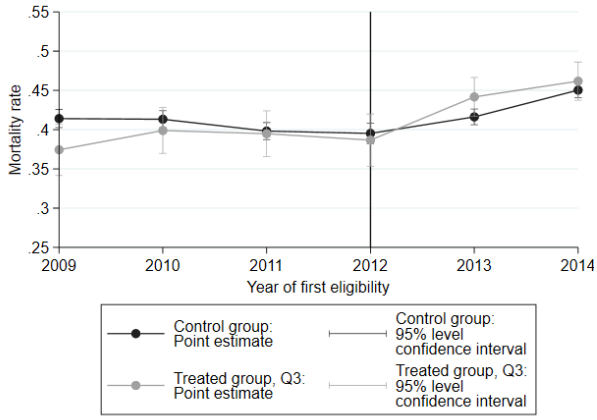
Figure D.4: Mortality in the 24 months following first eligibility, depending on treatment intensity, by year.



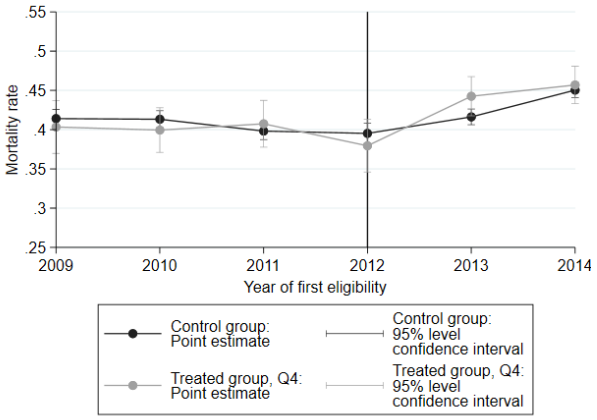
Panel A: control group versus 1^{th} quartile of the treated group. Outcome: 2-year mortality.



Panel B: control group versus 2^{th} quartile of the treated group. Outcome: 2-year mortality.



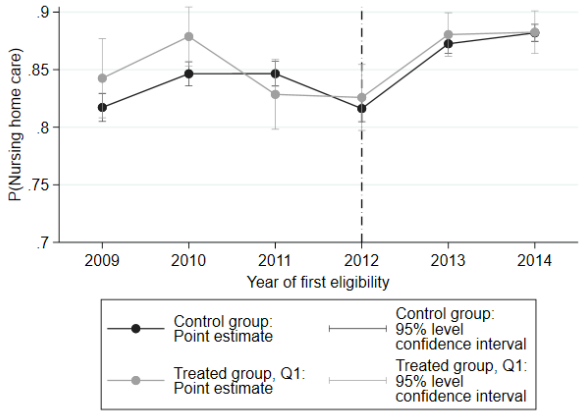
Panel C: control group versus 3^{th} quartile of the treated group. Outcome: 2-year mortality.



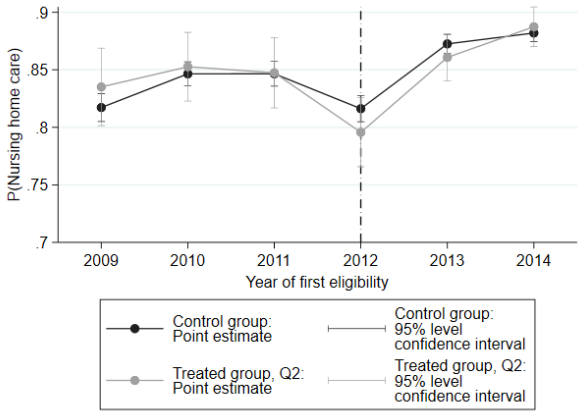
Panel D: control group versus 4^{th} quartile of the treated group. Outcome: 2-year mortality.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). Within the treated group, quartiles are defined with respect to treatment intensity: quartile 1 (resp. 4) groups the 25% individuals who experienced the smallest (resp. largest) change in the nursing home care price due to the reform. The reform was announced in Q2-2012. On the graphs, year 2012 excludes observations from Q1-2012.

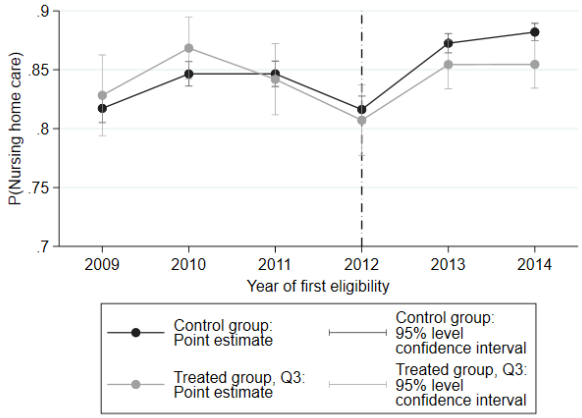
Figure D.5: Probability of use of nursing home care in the 12 months following first eligibility, depending on treatment intensity, by year, excluding individuals who become eligible for nursing home care with care package 4.



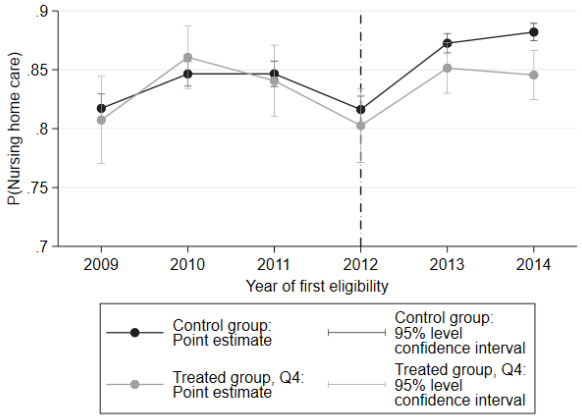
Panel A: control group versus 1^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel B: control group versus 2^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel C: control group versus 3^{th} quartile of the treated group.
Outcome: probability of nursing home use.



Panel D: control group versus 4^{th} quartile of the treated group.
Outcome: probability of nursing home use.

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 with care packages 5 to 8. Within the treated group, quartiles are defined with respect to treatment intensity: quartile 1 (resp. 4) groups the 25% individuals who experienced the smallest (resp. largest) change in the nursing home care price due to the reform. The reform was announced in Q2-2012. On the graphs, year 2012 excludes observations from Q1-2012.

Pre-trends in nursing home care use and mortality by treatment status: tests of joint significance

This Appendix provides the results of the statistical tests on the trends in the outcomes in the pre-reform period.

We estimate the dynamic difference-in-differences model, whereby all quarters are interacted with the treatment and the post period dummies. We then test the joint significance of the interaction terms for all quarters in the pre-reform period (except for Q1-2012, which is the reference period; the interaction term is equal to 0 by construction), through a Fisher test.

For nursing home care use, the p-value from this test is presented in Table D.I, 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, p-values largely that exceed 0.10, meaning that even at the 10% level we cannot reject that all pre-reform interaction terms are jointly null.

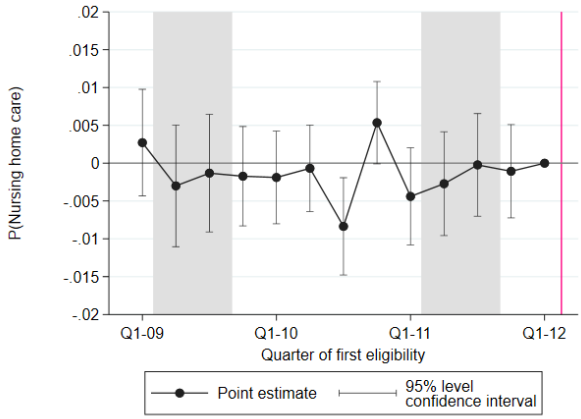
We also run the F-tests on a dynamic DiD specification that extends upon Equation (4) (our baseline specification): we now interact the change in the price for nursing home with the treatment and post-reform period dummies, and test for the joint significance of these interaction terms. The p-values 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. For this specification, we also display the estimated coefficients graphically, in Figure D.6. Apart from statistically a negative coefficient in Q2-2010, caught up by a positive coefficient in Q3-2010, all coefficients are practically small and non statistically significant at the 5% level. Again there is no evidence of differential trends across individuals experiencing lower or higher increase in the price of nursing home care due to the reform.

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

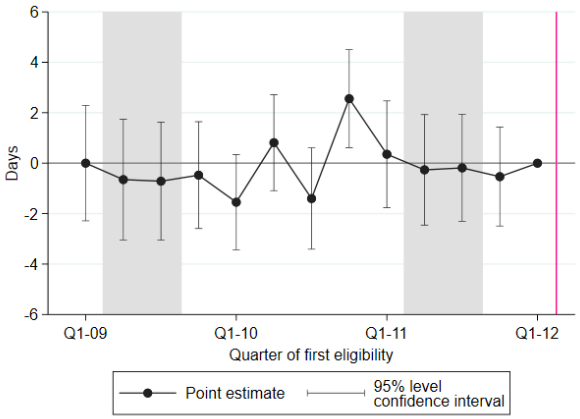
Outcomes	P(nursing home use)		#days spent in a NH		2-year mortality	
Binary specification (treated vs control groups)						
p-value	0.3078	0.2392	0.4872	0.3386	0.7026	0.7259
Price specification (treatment intensity)						
p-value	0.2658	0.3107	0.3407	0.2987	0.2765	0.6250
Covariates	No	Yes	No	Yes	No	Yes

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'.

Figure D.6: Pre-trends in nursing home care use: estimated quarter-price increase interaction terms.



Panel A - Outcome: probability of nursing home use.



Panel B - Outcome: Duration of nursing home care use .

STUDY POPULATION: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2009 and 2014 (N=79,559). OLS estimates of the interactions terms between quarter dummies and a €100 price increase. Reference quarter is Q1-2012.

Pre-trends for other outcomes

Table D.II: 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.III: 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.IV: 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 Additional results on home care use

In the analysis presented in Table IV, we assess the impact of the price increase on the care expenditures in the year of first eligibility and the year thereafter, 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 two months prior to and following eligibility.

For these analyses, we exclude 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 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 long-term care use, we should find no difference in their prior home care use between before and after the reform.

A cheaper equivalent to nursing home care?

We have defined the net price of nursing home care as the difference between the (monthly) co-payment on nursing home care and the co-payment on home care. To compute the latter, we refer to the volume of care each individual is expected to receive based on their care package, as explained in Section 3.4.

The descriptive statistics and estimates suggest that individuals 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). If individuals do the trade-off taking into account less hours of home care than we assume in our baseline computation of the net price, 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 Tenand, Bakx & Wouterse (2021)), 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.4 Alternative specification: binary treatment definition

Table D.V presents the estimate of the effect of the nursing home care price increase on the probability of any use (Column 1) and the number of days spent in a nursing home (Column 2) within the 12 months following first eligibility using a basic difference-in-differences model, which compares the outcome evolution in the treated group and in the control group.

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 ($Post.Treat$) provides the average treatment effect of the reform among the treated. The probability of a nursing home care admissions decreased by 1.2 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 unconditional duration spent in a nursing home of 166 days).

Table D.V: Difference-in-differences regression: baseline results.

Outcome:	P(nursing home use)	Days in nursing home
	(1)	(2)
Post	0.0694***	29.35***
	(0.0100)	(3.400)
Treat	0.0100*	2.710
	(0.00553)	(1.833)
Post.Treat	-0.0115**	-4.426**
	(0.00577)	(2.000)
Control variables	Yes	Yes
Quarter fixed effects	Yes	Yes
Observations	79559	79559
R^2	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.

Similarly, Table D.VI provides estimates from a binary difference-in-differences estimation of the average effect of the reform on nursing home care costs, home care costs, medical care costs and total care costs. These estimates are used when assessing the welfare effects of the reform (Appendix F).

Table D.VI: 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 Robustness analyses

E.1 The probability of becoming eligible for nursing home care

In this Section, we assess the potential impact on the reform on eligibility. In this analysis, the outcome is $Elig_{it}$, an indicator for becoming eligible for nursing home care at time t:

$$Elig_{it} = \gamma_0 + \gamma_1 Treat_i + \gamma_2 Post_{it} + \gamma_3 Post_{it} \times Treat_i \times (p_i^{post} - p_i^{pre})/100 + \mu_t + X_{it}\delta + \epsilon_{it} \tag{E.1}$$

We estimate Equation (E.1) on the 66+ population of singles who are not yet eligible for nursing home care before time t.[38] 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 vingtile. 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 E.I): the co-payment increase does not deter individuals from applying. This finding justifies our focus on the population of individuals who are eligible for nursing home care.

In Section 4.4, we argue that this zero effect is also evidence for the tightening of eligibility criteria not confounding the co-payment increase. A zero effect could however result from two opposite effects: richer individuals (i.e. those who tend to pay a higher nursing home price following the co-payment reform) could game the eligibility threshold and become more likely to become eligible for nursing home care; but they could react to the price increase by becoming less likely to apply for eligibility. We believe this is unlikely to explain the zero effect on eligibility we find, because (i) it would require a very specific economic gradient in the ability to go round the eligibility criteria, mirroring the income and wealth gradient in the price increase, and (ii) the effect is very precisely estimated.

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

Table E.I: 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.

E.2 Strategic wealth reallocation

To check whether individuals spent down their 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.[39]

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:

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

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.[40] 1_{it}^τ is an indicator equal 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 E.II 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

[39] 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.

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

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 E.II: 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

SAMPLES: 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.

Table E.II 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, 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.[41]

[41] 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).

E.3 Month-by-month analysis of nursing home entry

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 likely 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.

We consider two outcomes. First, we we look at *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:

$$NH_{it}^{\tau} = \beta_0^{\tau} + \beta_1^{\tau} Post_{it} + \beta_2^{\tau} Treat_{it} + \beta_3^{\tau} Treat_{it} \times Post_{it} \times \Delta_i + X_{it}\theta^{\tau} + v_{it}, \quad \text{(E.3)}$$

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. [42] The specification also includes quarter fixed effects. We estimate the set of Equations (E.3) 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.

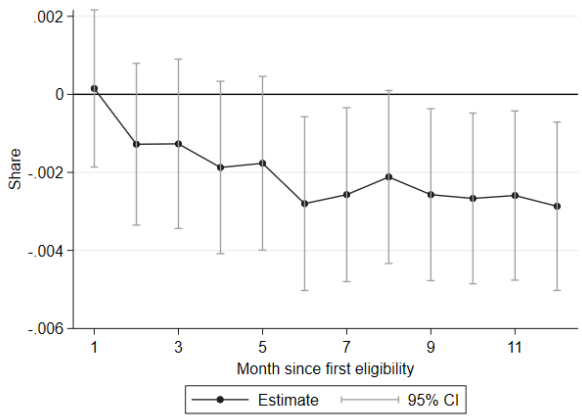
Figure E.1 shows the estimates of β_3^{τ} for both outcomes. It reveals three results. First, the probability to enter a NH in the first month of eligibility is not affected by the reform (Panels A and B). This could be explained by the fact that individuals who are admitted within a few days or weeks following their first eligibility have very severe needs or an emergency admission; they are thus expected to be highly price-inelastic. Second, for months 2 to 12 following eligibility, the effect on the

[42] Note that 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.

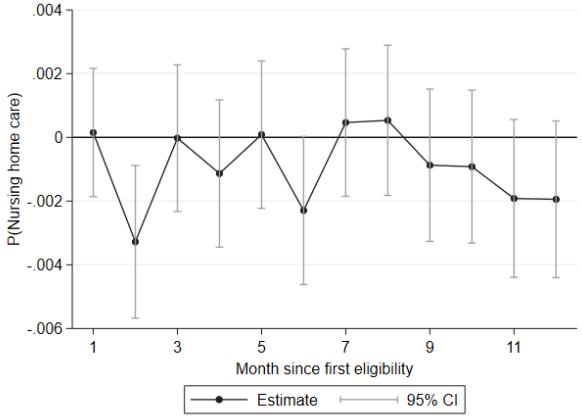
cumulative hazard is relatively stable, lying between -0.3 and -0.2 percentage point per €100 increase of the monthly price (Panel A). Finally, the largest effect on the monthly hazard is observed in the second month (Panel B), when the probability of an admission in the month decreases by 0.4 percentage 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 E.1: 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.

E.4 Flexible price effect

The main specification, in Equation (4), 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.[43] Or formally:

$$y_i = \alpha_0 + \alpha_1 Post_i + \sum_{k=1}^{K} \alpha_2^k Treated_i^k + \sum_{k=1}^{K} \alpha_3^k Post_i \times Treated_i^k + X_i\theta + \varepsilon_i. \quad \text{(E.4)}$$

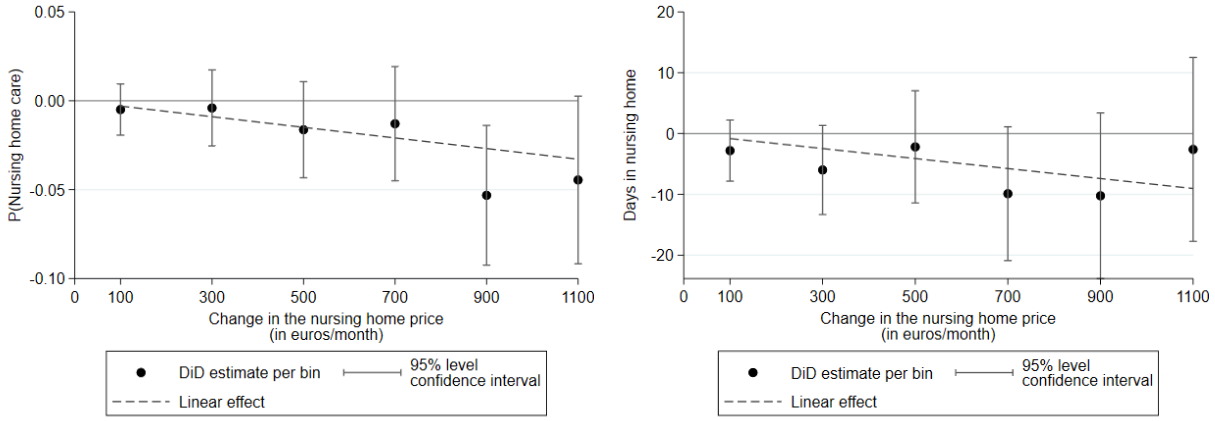
We expect coefficients α^k to be more negative when k increases: a larger price change leads to a larger decrease in nursing home care use compared to the control group.

Indeed, the estimates of α_3^k become more negative as k increases (Figure E.2). 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.

In Appendix E.5 (below), we also test a specification that assumes instead a linear relative price-effect.

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

Figure E.2: 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.

E.5 Relative price effect (log-price specification)

In the baseline analysis, we estimate the price sensitivity of nursing home care use using a DiD approach and positing a linear effect of the price change in absolute terms.

Alternatively, we can include the relative price change. We approximate it by the difference in logarithms, and estimate the following specification:

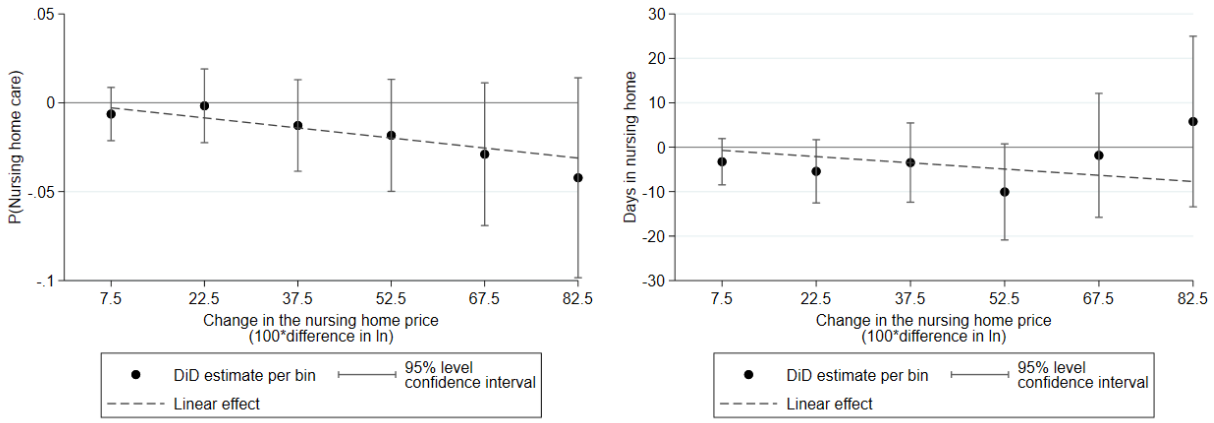
$$Y_i = \gamma_0 + \gamma_1 Post_t + \gamma_2 Treat_i + \gamma_3 Post_i \times Treat_i \times (\ln(p_i^{post}) - \ln(p_i^{pre})) + X_i'\theta + \mu_i \quad \text{(E.5)}$$

γ_3 captures the effect of (approximately) 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 E.3 shows the estimate of γ_3 in Equation (E.5), represented by the dashed line. The fit of the linear-log 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 percentage point.

Comparison of goodness-of-fit statistics also indicates that the log-linear price specification offers a poorer fit to the data than the baseline specification, when the outcome is the time spent in the nursing home. Figure E.3 of Panel B suggests this is the case especially for individuals who experienced a high increase in the nursing home price.

Figure E.3: Price sensitivity by treatment intensity: difference-in-differences estimates across bins and log-price 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 such that the difference in $ln(price)$ before and after the reform is lower than 15% (7.5 is the midpoint). The fitted line shows the effect of an (approximately) 1% increase in the monthly price for nursing home care induced by the reform.

F Assessing the welfare gains of the reform

The assess the overall welfare effects of the co-payment change, we consider the following elements:[44] the gains due to a reduction in moral hazard (A_1), a possible loss arising because of behavioral hazard (A_2), a transfer in costs from the government to the individual user (B), and the welfare loss due to additional risk for older individuals whose income and wealth are such that their net price for nursing home care increased following the reform (C). Note that we consider only efficiency aspects, and leave equity considerations aside: possible welfare gains (losses) arising if the distribution of co-payments across wealth groups is more (less) in line with societal preferences are ignored.

F.1 Moral hazard in nursing home care

Figure F.4 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).

In practice, area 3 will be very small: in the Dutch context $p^1 - p^0$ is small compared with $p^s - p^1$. Therefore, we ignore area 3 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_1) is computed as the decrease in government spending due to the decrease in care use: $(p_s - p_0) * (q_1 - q_0)$.

F.2 Net reduction in government spending

To incorporate behavioral hazard, we take into account the offsetting effects of the reduction in nursing home care use in terms of other health care spending and health. We ignore the latter because we have found no evidence of a mortality effect on average. Therefore, we capture the reduction in public spending (A) as the reduction in public spending on nursing home care minus the reduction in public spending on other health care ($A = A_1 - A_2$).

[44] We adopt an approach similar to those of Feldstein & Gruber (1995); Barcellos & Jacobson (2015); Shigeoka (2014); Finkelstein, Amy and McKnight, Robin (2008); Engelhardt & Gruber (2011).

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

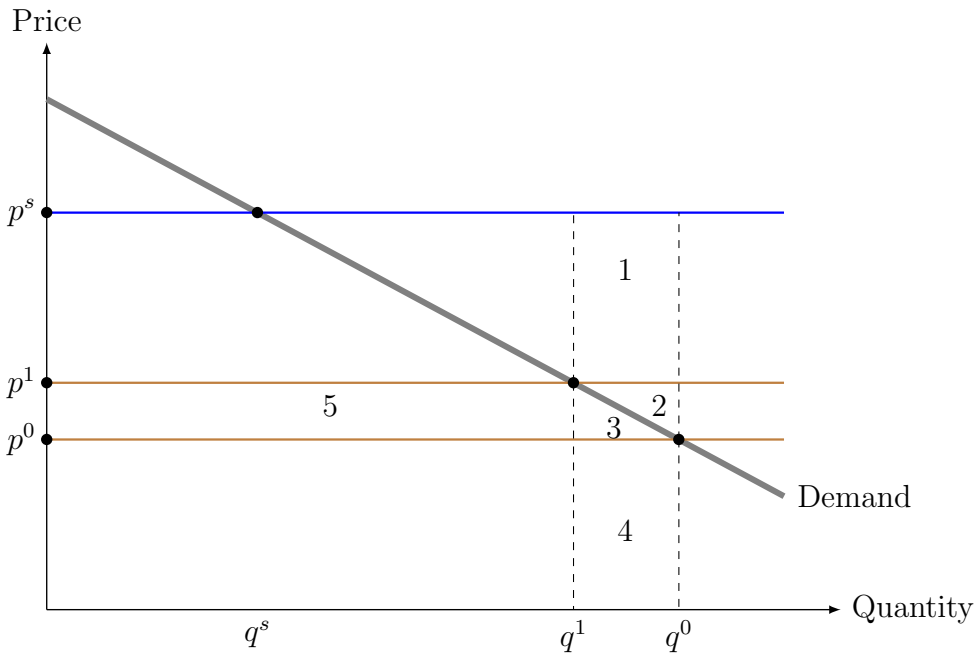


Table D.VI in Appendix D.4 provides the average treatment effect among the treated (ATT) of the co-payment reform on medical care and long-term care expenditures. These expenditures include both public and private expenditures. The reform decreased total care expenditures by €485. Net of co-payments, this represents a reduction of €345 in public spending per individual affected by the reform. To get to this value, we ignore cost sharing on medical care.[45] We calculate the decrease in private spending as the pre-reform average difference between daily co-payments on nursing home care and co-payments on home care, times the decrease in the number of days spent in a nursing home induced by the reform.[46] [47]

[45]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 (of €350 per year in 2013).

[46]The average pre-reform difference between the monthly co-payments for nursing home care and for home care is €953, as can be retrieved from Table I on p. 16. Combined with the average reduction in nursing home use of 4.4 days reported in Table D.V in Appendix D.4, this gives a reduction in co-payments on nursing home care, net of the reduction in the co-payment on home care, of $(953/30) \times 4.4 = e140$. The difference between the reduction in total care expenditures and the reduction in co-payments is then equal to: €485 - €140 = €345.

[47]We omit the part of the decrease in private spending that corresponds to the increase in the daily co-payment times the post-reform number of days spent in the nursing home, because this is a transfer of costs from the government to private individuals that will be valued separately, as detailed in the next section.

F.3 Valuing the decrease in public spending and the transfer costs to users

The reduction in public spending is a combination of the effect of the reform on care use (which decreases spending by areas 1+2+3) and the mechanical transfer of costs from the government to private individuals because of the higher co-payments (area 5). The first effect is equal to A. We calculate B, the transfer to the users, in the following Section F.4.

Our analysis focuses on efficiency gains and losses and is agnostic about the welfare effects of the redistribution of resources from (a group of) private individuals towards the government. However, we take into account the fact that a decrease in public spending means a reduction in the (labor market) distortions of income-dependent taxes and social insurance premiums, which finance the Dutch long-term care insurance. This means that the welfare gain associated with the decrease in public spending is larger than the decrease in public spending itself, all the more as the societal cost of levying public revenues at the margin (i.e. the Marginal Cost of Public Funds, MCF) is high. Formally, the welfare gain associated with the reduction in public spending is valued at $MCF \times A$, while the transfer of costs from the government to the users is valued at $(1 - MCF) \times B$. Would MCF be equal to 1, the transfer of costs would be neutral in terms of efficiency.

It is disputed whether this additional effect should be included in welfare analyses 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.[48] 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 the associated gains 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 F.5).

We will consider 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, which is 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

[48] 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)).

to a lesser extent than a tax directly levied on labor income.[49]

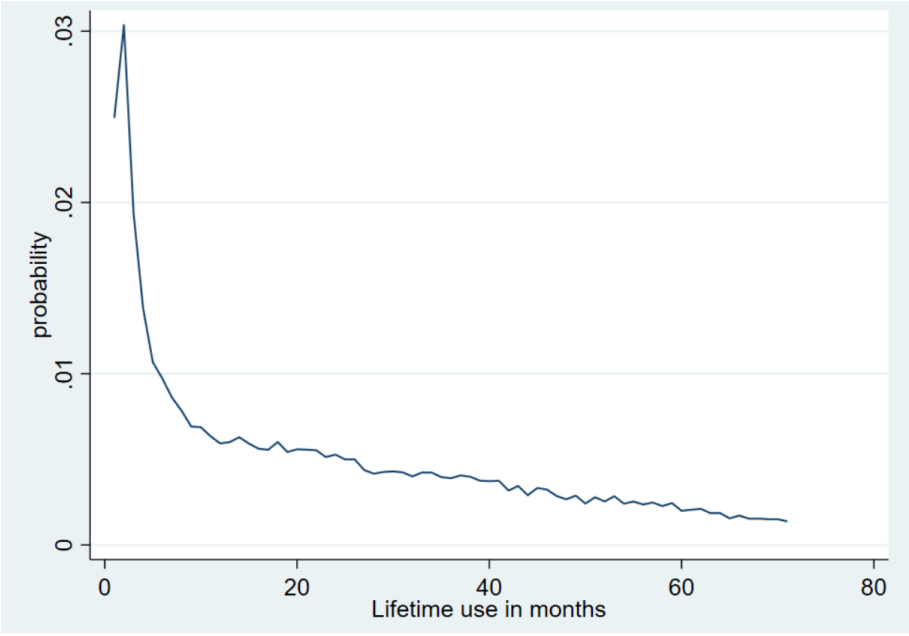
F.4 Cost transfer to care users

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.

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

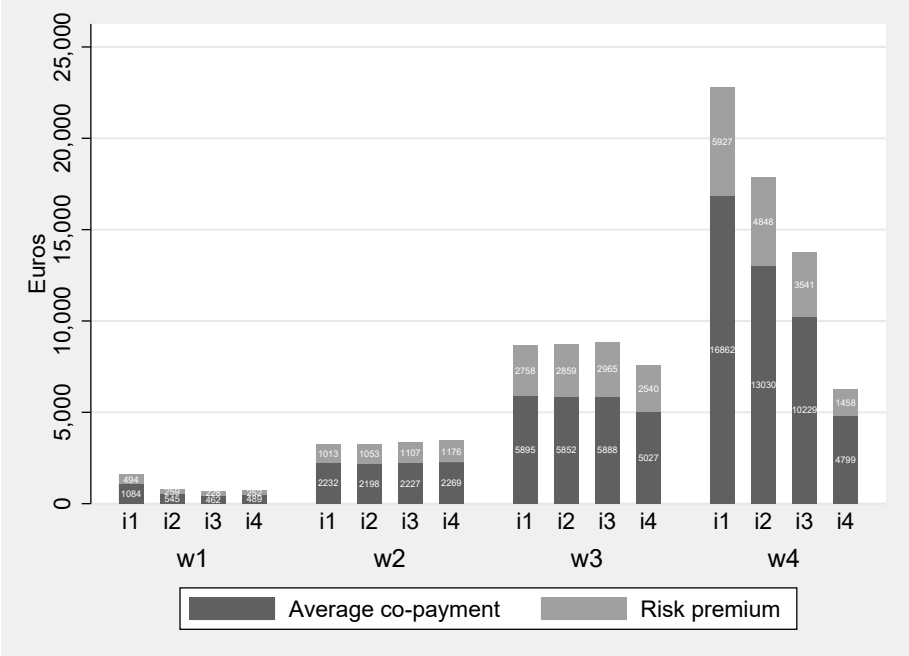


[49] A value of 1.3 for the marginal cost of public funds is also used by Shigeoka (2014), in an analysis of the welfare effect of patient cost sharing in the U.S.

Using the pre- and post-reform co-payment rules, we can then simulate the probability distribution of life-time co-payments and its change due to the reform. Exposure to the co-payment 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 VI (in Section 7) shows the simulated distribution of pre- and post-reform lifetime co-payments for one of the 16 groups. Figure F.3 below shows the change in average co-payments across all groups (dark bars). The weighted average is reported in Table F.I: the reform increased lifetime co-payments among the potentially affected older adults by €4,845 (per person).

Figure F.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 F.5).

$$
\begin{aligned}
Net\,Benefit &= Benefits - Costs && \text{(F.1)}\\
&= MCF \times (A+B) - (B+C) && \text{(F.2)}\\
&= MCF \times A + (MCF-1) \times B - C, && \text{(F.3)}
\end{aligned}
$$

with MCF is the marginal costs of raising government funds.[50]

F.5 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:

$$u(C) = \frac{C^{1-\gamma}}{1-\gamma}. \tag{F.4}$$

The budget constraint is

$$Y = C + O, \tag{F.5}$$

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

$$\mathrm{E}(U^{pre}) = \sum_{h=0}^{72} u(C - O^{pre}(h))p(h) \tag{F.6}$$

$$\mathrm{E}(U^{post}) = \sum_{h=0}^{72} u(C - O^{post}(h))p(h). \tag{F.7}$$

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:

$$CEC = u^{-1}\left(\mathrm{E}(U)\right). \tag{F.8}$$

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

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)):

$$CEC^{post} - CEC^{pre} = \underbrace{\hat{O}^{pre} - \hat{O}^{post}}_{Change\ in\ average\ payment} + \underbrace{(CEC^{post} - CEC^{pre}) - (\hat{O}^{pre} - \hat{O}^{post})}_{Change\ in\ risk\ premium}. \tag{F.9}$$

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.

On Figure F.3 (displayed in the previous section), the grey bars show 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 VII 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}).[51]

F.6 Summing up losses and gains

Finally, we turn to summing up the welfare gains and losses, as follows:

$$\begin{aligned} Net\ gain &= Gains - Losses && \text{(F.10)} \\ &= MCF \times A + (MCF - 1) \times B - C && \text{(F.11)} \\ &= MCF \times (A + B) - (B + C) && \text{(F.12)} \end{aligned}$$

Beforehand, we have to re-scale component A, the reduction in moral hazard. Indeed the effects on care use were estimated among individuals who are eligible for care, while our estimates of the cost transfer and of the 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 effect to the other effects, we multiply our estimate by the lifetime probability of becoming eligible, which is

[51] Note that pre-reform welfare depends on risk aversion.

0.56.[52]

We then have:

- A: a reduction in moral hazard of €193 (=€345 $\times$ 0.56), 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 $€4,845 \times (MCF - 1)$;
- C: a decrease in welfare due to additional financial risk, ranging between €1,979 and €6,002 depending on the value of γ.

Table F.I shows the total welfare gain under different assumption about γ and the MCF. For example, with $\gamma = 5$ and $MCF = 1$, the total welfare loss is €3,328. A lower value of the risk aversion parameter ($\gamma = 3$) decreases the welfare loss (to €1,786, in the case of $MCF = 1$). Whether or not the decrease in the marginal costs of fund for the government is included matters 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.

Figure F.4 offers a graphical illustration of how the net welfare effect leans towards a positive (resp. negative) value, depending on how high risk aversion and MCF are. The higher the risk aversion, or the lower the marginal value of public fund, the higher the chance that the co-payment increase resulted in a welfare loss.

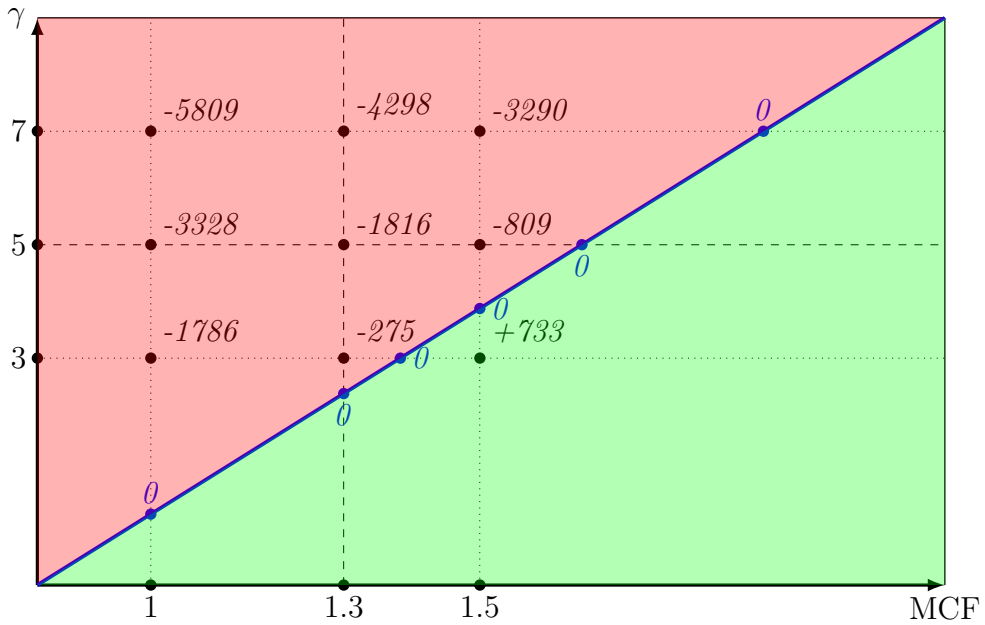
[52] Proxied by the lifetime probability of using nursing home care.

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

Shift in costs	γ	Risk premium	Reduction in public spending	MCF	Total effect on welfare
(1)	(2)	(3)	(4)	(5)	(6)
4,845	3	1,979	193	1	-1,786
4,845	5	3,521	193	1	-3,328
4,845	7	6,002	193	1	-5,809
4,845	3	1,979	193	1.3	-275
4,845	5	3,521	193	1.3	-1,817
4,845	7	6,002	193	1.3	-4,298
4,845	3	1,979	193	1.35	0
4,845	5	3,521	193	1.66	0
4,845	7	6,002	193	2.15	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: (6) Total welfare gain induced by the reform, per affected individuals, in euros. The risk premium in (3) depends on the value of γ in (2). The shift in costs in (1) and the reduction in public spending in (4) are valued using the MCF in (5).

Figure F.4: Lifetime welfare effects of the reform depending on the value of the reduction in public spending and the increase in financial risk, per person.



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 person. See Appendix F for calculations

References

The references cited in the Appendices can be found in the List of References after the main text.

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