Do co-payments affect nursing home entry? Quasi-experimental evidence using Dutch administrative data

Marianne Tenand* Pieter Bakx Bram Wouterse†

June 2020

Preliminary draft. Prepared for the 6th Ahepe Workshop. Please do not cite.

Abstract

Virtually all developed countries use co-payments to limit nursing home stays. However, as nursing home stays are generally a last resort, there may be less moral hazard than with other types of care. Hence, it is not clear whether the co-payments affect the use. This paper assesses the impact of the co-payments for permanent nursing home residents by exploiting quasi-experimental variation. A co-payment reform implemented in 2013 in the Netherlands led to a sizeable increase in the co-payments for nursing home care, but only for elderly with high wealth. We perform a difference-in-differences analysis on administrative data for the full Dutch 65+ population from 2010 to 2014 to test for an impact of the co-payment increase for this group on nursing home entry. We focus on single elderly who are eligible for a permanent nursing home admission for the first time and monitor their subsequent nursing home care use. Our results indicate that, on average, the increased co-payments did not lower the use of nursing home care. But the reform did reduce the probability of an admission for the elderly who were subject to relatively large increases (€1,500 per month) by 7 percentage points in the first 8 months after becoming eligible. Compared to a baseline nursing home use rate of 79%, these estimates imply that the co-payment increase induced by the reform led to an important reduction of nursing home admissions for those with high financial wealth and a moderate income. These results mean that, contrary to what is often conjectured, demand-side financial incentives do play a role in the timing of permanent nursing home stays, even in a country like the Netherlands that ensures financial accessibility of institutional care.

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

JEL codes: C24; D12; I18; J14.

*Corresponding author. Contact: tenand@eshpm.eur.nl

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

Access to and use of individual-level data

The results presented in this article are based on calculations by the authors using non-public microdata from Statistics Netherlands (CBS). Under certain conditions and a confidentiality agreement, these microdata are accessible for statistical and scientific research. For further information: microdata@cbs.nl. Data use and publication of the results are in compliance with the European privacy legislation (GDPR, 25 May 2018).

Acknowledgements

The authors are grateful to Raun van Oijen as well as to the participants of the 2020 Netspar International Pension Workshop and of the internal Health Economics seminar (Erasmus School of Economics, Rotterdam) for their valuable comments. They also thank Judith Bom for her input to the early stages of this research and Francisca Vargas Lopes for her contribution to the understanding of the co-payment schedule.

Funding

Access to the data was in part funded by the Network for Studies on Pensions, Aging and Retirement (Netspar) within the project "Optimal saving and insurance for old age: the role of public long-term care insurance" and by ODISSEI.

1 Introduction

Co-payments for nursing home care impose a considerable financial risk on the elderly. Most countries have some sort of public schemes to pay for nursing home care (Hashiguchi & Llena-Nozal, 2020). Even in the U.S., which is known for its limited long-term coverage, about half of the elderly who are admitted to a nursing home do not pay for it themselves (Hurd *et al.*, 2017). These programs are designed to protect the elderly form the substantial financial costs of nursing home use. In the Netherlands, 90% of elderly would be pushed into poverty if they had to pay the full cost of nursing home care out-of-pocket, but a public insurance ensures its financial accessibility (Hashiguchi & Llena-Nozal, 2020). However, public schemes generally do not cover the full costs; nursing home residents pay often substantial co-payments.[1](Colombo *et al.*, 2011). Co-payments can reach tens of thousands of euros per year (Muir, 2017). Although they often depend on on financial means, co-payments considerably limit the financial protection offered by the public programs: five percent of the elderly in the U.S. will have lifetime spending of at least $47,000 on co-payments for long-term care (Hurd *et al.*, 2017). Similarly, five percent of the Dutch elderly with a middle income pay 33,000 euros or more (Wouterse *et al.*, 2019).

It is unclear to what extent co-payments reduce nursing home use. The main motivation for co-payments is that they reduce unwarranted use of care (moral hazard) by shifting part of the marginal price to the user herself. [2] However, it is not obvious that users of nursing home care are price responsive. As "no one wants to go to a nursing home" (Hitchcock, 2015), it might only be an option of last resort that is used by the elderly with very severe disabilities, and thus there may be little moral hazard. Empirical research on moral hazard in nursing home care is limited (e.g. Grabowski & Gruber (2007); Hackmann & Pohl (2018); Konetzka *et al.* (2019)), focuses mostly on the U.S., with many studies considering the difference in use between elderly with and without long-term care insurance rather than the effect of exogenous changes in the out-of-pocket price of care.

In this article, we investigate the effects of co-payments on nursing home admissions, by studying a co-payment reform that took place in the Netherlands in 2013. It increased co-payments substantially for some individuals, while others were

[1]Co-payments are explicit user charges; programs may also feature other forms of cost-sharing, such as means-testing or partial subsidies.

[2]A second argument that is used for co-payments is that they reduce the welfare losses from public financing. However, these welfare losses do not stem from the insurance but from the (income-dependent) way that premiums or taxes are raised. These welfare losses can be resolved without introducing co-payments, at least in theory (e.g. by making premiums more actuarially fair or less progressive).

not affected. The Netherlands has universal, comprehensive social LTC insurance and private long-term care insurance is absent. Nursing home residents pay a co-payment. To ensure access to care for everyone, this co-payment depends on a user's income and financial wealth. In 2013, the proportion of financial wealth that is taken into account in the computation of the co-payment increased. As the first €25,000 of financial wealth are exempted from the calculation and many Dutch elderly have little financial wealth, the reform only affected the co-payments for nursing home care for only 35% of the 65+ singles.

We implement a difference-in-differences (DiD) approach to compare changes in nursing home admission rates across groups that were affected differently by the 2013 reform. We use administrative data on nursing home eligibility, use and co-payments for the entire 66+ Dutch population. We focus on the elderly who are eligible for nursing home care. In contrast to post-acute care, an admission for this type of care is intended to be permanent, and financial incentives are thus geared at postponing nursing home entry instead of discharge. We estimate the probability of nursing home entry and the duration of nursing home stays within the eight months after the day a person became eligible for nursing home care for the first time. To identify to which extent each person was affected by the reform, we combine individual-level income and wealth information with the co-payment schedule: the change in co-payments due to the reform was a function of a person's income and non-housing wealth.

While there are many studies on how financial incentives for patients affect health care use (e.g. (Brot-Goldberg *et al.*, 2017; Einav & Finkelstein, 2018), evidence on the price elasticity of demand for permanent nursing home care is relatively scarce, possibly because of limited availability of data on prices and co-payments (Konetzka *et al.*, 2019).[3] Kim & Lim (2015) show that nursing home users in South Korea are price sensitive, using a regression discontinuity design that exploit jumps in public LTC insurance benefits caused by eligibility cut-offs. The other studies are from the United States. Grabowski & Gruber (2007) rely on variation in the generosity of public, means-tested (Medicaid) coverage of nursing expenditures between and within states. Konetzka *et al.* (2019) study the impact of *private* LTC insurance, which reduces the out-of-pocket price, on permanent nursing home use, instrumenting insurance status by tax deductions. Both studies find that the elderly do not adjust their use of nursing home care to its out-of-pocket price. Finally, part of this literature focuses on post-acute and other short-term stays. Hackmann & Pohl

[3] A related strand of the literature has focused on testing for the price-sensitivity of *home care* demand and consistently shows a non-zero price elasticity (Pezzin *et al.*, 1996; Stabile *et al.*, 2006; Rapp *et al.*, 2011; Roquebert & Tenand, 2017; Non, 2017; Konetzka *et al.*, 2019).

(2018) exploit variation across nursing homes in the differential rate of privately-paid and Medicaid-paid stays. They find that individuals respond to lower cost-sharing on nursing home care by extending their stay instead of transitioning back to the community. By studying this outcome, Hackmann & Pohl (2018) capture the price-elasticity of nursing home care demand of a relatively healthy group of elderly, with moderate care needs and who are able to move in and out of a institutional care setting.

We contribute to this literature by providing the first evidence of a price response in *permanent* nursing home admissions for a European country with extensive public LTC insurance and a relatively high and homogeneous quality of nursing home care. Our estimate is arguably relevant for many developed countries because the reform we exploit – a change in the existing co-payment rate –is more similar to the reforms that other countries may consider than the highly-specific features that are exploited in studies on nursing home use in the US, which is one of the few countries where private insurance plays a role (Colombo *et al.*, 2011). Moreover, unlike the US studies we use detailed administrative data for the full Dutch population of elderly who are eligible for nursing home care rather than survey data, which improves the statistical power to detect small effects.

Our results indicate that, on average, the increased co-payments did not lower the use of nursing home care. But the reform did reduce the probability of an admission for the elderly who were subject to the relatively large increases (€1,500 per month) by 7 percentage points in the first 8 months after becoming eligible. Compared to a baseline nursing home use rate of 79%, these estimates imply that the co-payment increase induced by the reform led to an important reduction, or postponement, of nursing home admissions for those with high financial wealth and a moderate income. These results mean that, contrary to what is often conjectured, demand-side financial incentives do play a role in the timing of permanent nursing home stays, even in a country like the Netherlands that has comprehensive, universal coverage. Co-payments, even when relatively moderate compared to residents' financial means, may limit nursing home care use and foster ageing in place.

2 Institutional background

2.1 Long-term care in the Netherlands

Until 2014 all long-term care services (except for domestic help) were funded through a single social insurance scheme (AWBZ) (Schut *et al.*, 2013).[4] Social LTC insurance provided universal, comprehensive coverage, making the Netherlands one of the top spenders on LTC worldwide. With 5.3% of the 65+ population living in institutions in 2014 (OECD, 2020), it has one of highest shares of the elderly population living in a nursing home. Private alternatives are virtually absent.

One major demand-side constraint to nursing home use is that an individual needs to first become eligible for this type of care. An independent agency (CIZ - *Centrum Indicatiestelling Zorg*) decides about eligibility for insurance benefits. After a voluntary individual application, assessors classify eligible applicants based on their needs,[5] and decide about the care setting (home *versus* institutional care) and the type and intensity of care services that the applicant is eligible for. Someone who is eligible for nursing home care may choose to enter a nursing home, to receive an equivalent package of in-kind care services at her or his home instead or to receive LTC vouchers. However, someone who is eligible for home care can only choose to receive in-kind home care or LTC vouchers and cannot opt for a nursing home admission.

There are two types of permanent institutional care facilities: depending on their care needs, elderly may go either to an assisted-living facility or to a nursing home.[6] Until 2012, applicants who were eligible for institutional care were assigned a care package reflecting the severity and the types of needs that corresponds to 3 to 32 hours of nursing care, personal care and guidance per week. To reduce the number of elderly using institutional care, the rules for institutional care eligibility were made stricter in 2013 and further so in 2014. Specifically, assisted-living facilities were no longer funded for new applications, while rules for nursing home admissions were unchanged.[7] As reflected by Figure 1, the proportion of the 65+ who spend some time in an institutional elderly care facility paid by the LTC social insurance

[4]The Dutch LTC system was substantially reorganized in 2015. In what follows, we describe the pre-reform system.

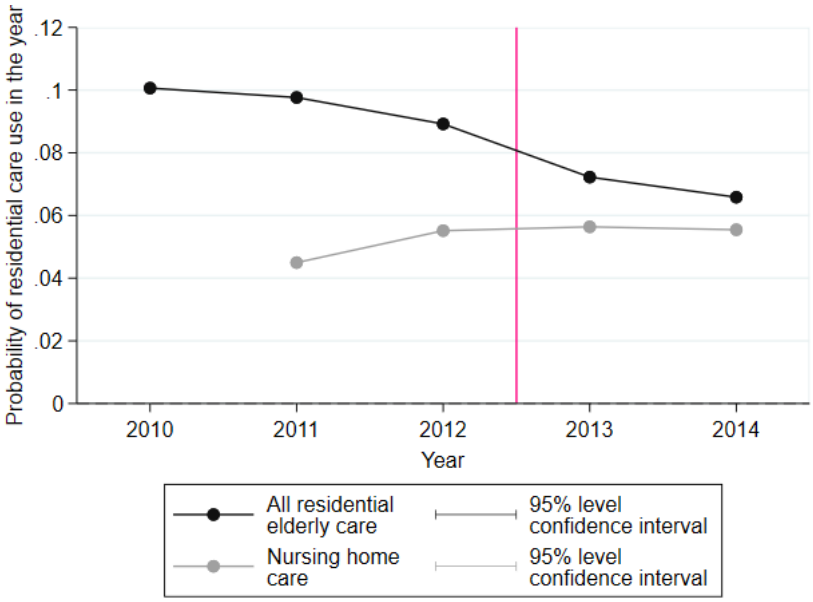
[5]Assessors should not take other considerations such as the applicant's income or supply constraints into account, as explained in Tenand *et al.* (2020*b*).

[6]In addition, they may go 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.

[7]In practice, this reform was merely a reflection of standing policy and had been phased-in progressively over a couple of years: in the years prior to 2013, the number of eligibility decisions for assisted-living facilities had been decreasing quickly.

(nursing home, assisted-living facility, post-acute care unit or hospice) markedly decreased between 2010 and 2014 (from 10% to less than 7%), while the share of the elderly population staying in a nursing home slightly *increased* over the same period.

Figure 1: Trends in institutional elderly care use in the 65+ population in the Netherlands (2010-2014).



NOTES: Authors' calculations. Confidence intervals are extremely narrow.

The contracting of LTC services is the responsibility of 32 regional purchasing offices; the independent assessment agency did not play any role in the provision of LTC services to eligible individuals.[8] These offices can only contract care up to the annual regional budget ceiling and this ceiling is virtually always reached in all regions, yet the number of patients waiting for nursing home care was virtually zero. There are, however, individuals who want to wait until there is a place at the nursing home of their preference. For each resident, nursing homes are paid a per-diem rate that is negotiated with the regional purchasing office that depends on the amount of care that the resident is eligible for.[9]

[8] see Schut *et al.* (2013) for details on the supply and contracting of nursing home care.
[9] The negotiated rates are on average only a few percent below with very little variation around the average.

2.2 The schedule for co-payments on nursing home care

In the Netherlands, LTC users are required to contribute to the costs of the care they receive via co-payments, which make for another potential demand-side constraint on LTC use. Co-payments are computed on a monthly basis in the case of nursing home care, and a four-week basis for home care. For LTC vouchers also, a co-payment is subtracted from their gross value. Co-payments are levied by the Central Administration Office of the LTC insurance scheme (*Centraal Administratie Kantoor*, or CAK). All co-payments are income-dependent, subject to minimum amounts and ceilings. Rebates for a range of personal circumstances (mainly household composition and age) applied. In the aggregate, co-payments are limited in the Netherlands: in 2012, they represented 8% of total spending on LTC (Schut *et al.*, 2013). We first explain the co-payment schedule for nursing home care and how it was in 2012. We then discuss the 2013 reform.

2.2.1 Co-payments until 2012

A resident is charged the same co-payment whichever nursing home she or he enters. Moreover, the co-payment does not depend on the type or intensity of the care received in the nursing home either. However, there exist two regimes of co-payments that nursing home users may be subject to either a *low-rate* co-payment or a *high-rate* co-payment, each coming with specific schedule and ceilings.

In general, the low-rate co-payment applies during the first six months of a stay and for individuals who still have a partner living at home, while the high rate applies to all other residents. Here below we focus on describing the schedule for the high rate co-payment: as will be explained in Section 3, we posit that this is the regime that matters for individuals' decision regarding the timing of their nursing home admission.[10]

In case of of the high rate, the annual co-payment is equal to the 'contribution income'. 'Contribution income' is based on taxable income, which itself consists of household earning plus income derived from financial assets and real estate, excluding the net value of the own house (henceforth: wealth).[11] To calculate income derived out of wealth, the Tax Office uses a flat-rate approach: a fixed percentage of the stock of wealth is added to the annual household income. This share was equal to 4% until 2012. A part of wealth is excluded from this calculation (€21,000 for singles in 2012) so as to eliminate wealth taxation of those with very low assets. Af-

[10] More details on the low-rate co-payment regime and the situations in which it applies can be found in B.
[11] 'Box 3: sparen en beleggen' in Dutch.

ter further deductions are applied,[12] 25% of the remaining income is exempted. The (monthly) high-rate co-payment is then simply equal to (1/12 of) the 'contribution income'.

No minimum co-payment applies when the individual is subject to the high rate co-payment, but if the 'contribution income' is negative (in the case the individual economic resources are very low), the co-payment is set to 0. Furthermore, co-payments are capped on a monthly basis, such that no nursing home care resident would pay more than €2,250 of co-payment per month in 2014.

2.2.2 Magnitude of co-payments until 2012

In 2011, the median yearly co-payment for all-year nursing home residents amounted to €7,635, equivalent to 56% of their available income (Bakx *et al.*, 2020). For an individual receiving old-age social security benefits (i.e. a disposable income of circa €1,160 per month in 2010) with no taxable wealth, the monthly high-rate co-payment would not exceed €750 in 2012, amounting to 65% of available income. Although the absolute value of co-payments increases with income, the schedule is such that the share of income devoted to cost-sharing increases with income. It peaks at 72% for those with about €36,000 disposable income (Wouterse *et al.*, 2019) and decreases beyond that income level because of the cap.

Overall, cost-sharing on nursing home care was (and is still) limited in the Netherlands, especially when compared with other OECD countries (Hashiguchi & Llena-Nozal, 2020). Up until 2012, virtually no NH user had to pay more than their current income on LTC. Still, co-payments represent a sizable share of income for a large middle class, especially for individuals who end up staying several years in a nursing home.[13] It also implies that additional time spent in the nursing home comes at a non-zero out-of-pocket cost, even in the case of a long-duration stay.

[12] More information is provided in Appendix B.

[13] Among the 65+ who died in 2016, Bakx *et al.* (2020) report that 41% stayed in a nursing home in the 5 to 6 years prior to their death, paying about €23,000 of co-payments on average. Among the 8% of those who stayed permanently in a nursing home, the average co-payment paid reached €54,000.

2.3 The 2013 reform

2.3.1 A higher implicit tax rate of financial wealth

On January, 1st 2013 the co-payment schedule was reformed.[14] Specifically, the share of wealth included in the calculation of the maximum co-payment was increased: an additional 8% of wealth was added directly to the contribution income.

Denoting CP_{pre}^{high} the monthly high-rate co-payment up until 2012 and CP_{post}^{high} this co-payment after the 2013 reform:

$$CP_{pre}^{high} = min\Big(CP_{max}^{high}, max\big(0, (1/12) \times cont\_income\big)\Big)$$

$$CP_{post}^{high} = max\Big(CP_{max}^{high}, min\big(0,(1/12) \times (cont\_income + 8\%(wealth - exemptions)\big)\Big)$$

where CP_{max}^{high} is the monthly co-payment cap, and the individual's contribution income is:

$$cont\_income = 75\% \times \big(hh\_income + 4\%(wealth - exemptions) - rebates\big)$$

hh_income denotes household income, *wealth* is household wealth (as taken into account in the co-payment schedule), and *exemptions* and *rebates* encompass a range of exemptions and rebates based on personal circumstances.

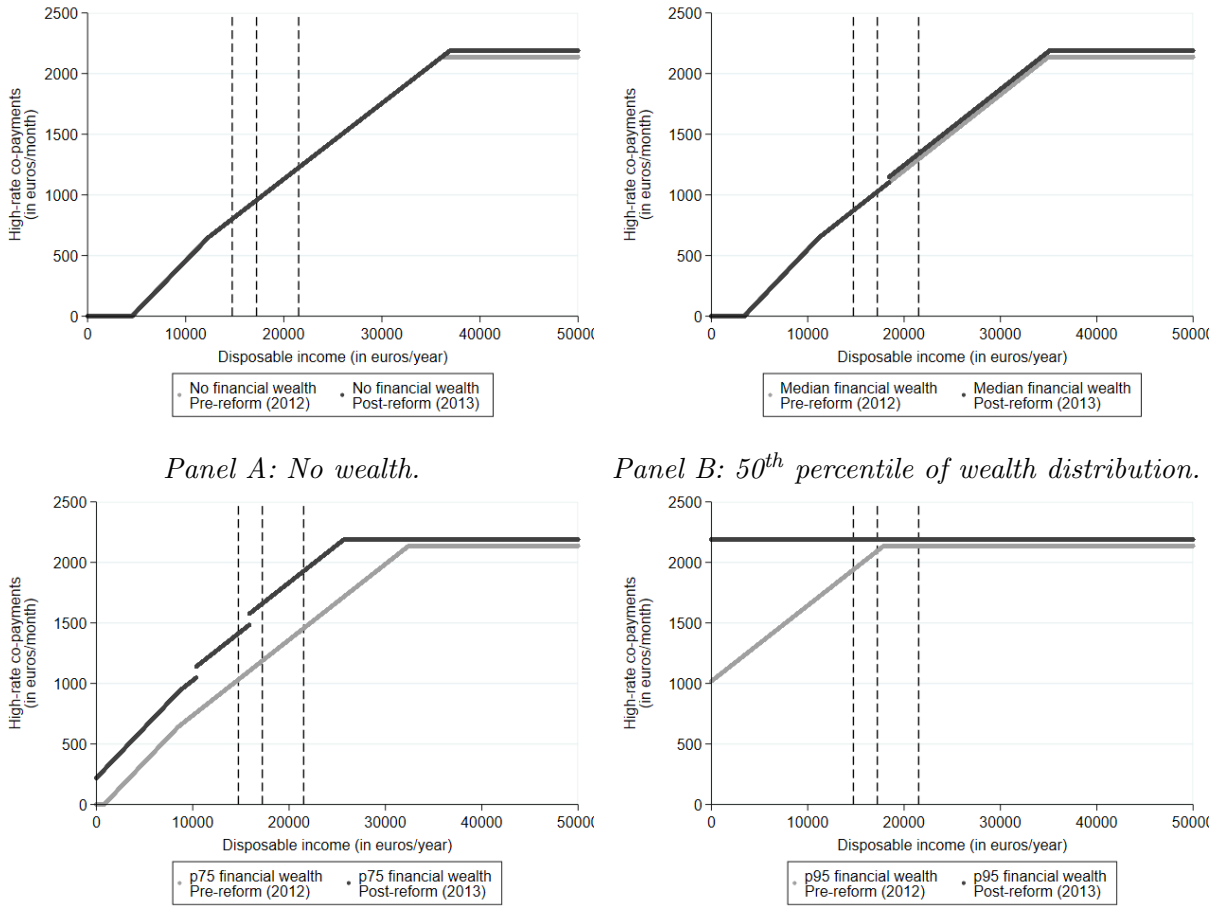
Put differently, the reform has increased the flat-rate taxation of wealth implicit in the cost-sharing computation, from $(75\% \times 4\%) = 3\%$ to $(75\% \times 4\% + 8\%) = 11\%$. No other change to the computation of co-payments was made simultaneously.

2.3.2 Who is expected to be affected by the reform?

The reform has increased cost-sharing on nursing home stays for the elderly with positive taxable wealth, but with sufficiently low income and wealth so that they would not hit the maximum co-payment already under the 2013 co-payment schedule or even under the 2012 schedule. Schematically, we predict that the low-income-high-wealth were those most affected by the reform. Figure 2 graphically shows how the reform is predicted to affect the high-rate co-payment to be paid for a month in the nursing home, depending on the income level and for four different levels of wealth.

[14] This reform was announced in April 2012 as part of a larger set of political decisions.

Figure 2: Co-payment schedule before and after the 2013 reform depending on income, by level of financial wealth.



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

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

NOTES: Authors' simulations. The grey (resp. black) curves are based on the co-payment rules and parameters of 2012 (resp. 2013). Panel B: schedule for an individual at the 50$^{\text{th}}$ percentile of the wealth distribution (€28,000). Panel C: schedule for an individual at the 50$^{\text{th}}$ percentile of the wealth distribution (€92,000). Panel D: schedule for an individual at the 95$^{\text{th}}$ percentile of the wealth distribution (€455,000). The dashed vertical lines indicate the 25$^{\text{th}}$, 50$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of the income distribution. The wealth and income distributions refer to the distribution of financial wealth (per capita) and the distribution of available income (per consumption unit) as reported in tax year 2010 in the 65+ Dutch population alive in 2012. The notches in the post-reform schedule (visible in Panels B and C) are due to rebates on taxable wealth that apply to individuals below some income thresholds and a certain wealth level. Before the 2013 reform, the effect of these rebates was smoothed due to the inclusion of taxable wealth was included in the contribution income.

Panel A of 2 shows that the reform had no effect on the co-payments to be paid by users with no wealth (the pre-reform and post-reform schedules are exactly the same). Similarly, even for individuals with median wealth, the reform hardly affected cost sharing on nursing home care, regardless of income (Panel B).

However, co-payments increased for individuals at the 75$^{\text{th}}$ wealth percentile (Panel C). This increase was the larger for those with a low income; the overall cap on the maximum co-payment limits the maximum co-payment only for those with a relatively high income (around €25,000 a year, while the third quartile of the available income distribution was only around €22,000). Among potential users in the 75$^{\text{th}}$ wealth percentile, those with a median income experienced an increase in the co-payment to be paid for a month in the nursing home of about €500 (from €1,200 to €1,700). By contrast, individuals in the 95$^{\text{th}}$ of the wealth distribution (Panel D) only experienced a limited increase in cost-sharing as for them, the cap on maximum co-payments is relevant. Those with high wealth and an income higher than the median income experienced no change in the co-payment to pay for additional time spent in the nursing home, as they already hit the cap under the 2012 rules. However, those with high wealth and low income experienced a marked increase in the monthly co-payment, of up to €500.

To sum up, the 2013 reform led to an increase in the out-of-pocket price of nursing home care only for a specific group, but among those affected the change in cost sharing was heterogeneous, and considerable for some. Descriptive evidence from Bakx *et al.* (2020) indicates that there was no differential trend in nursing home use across the income and wealth groups that were least *versus* most affected by the reform of the cost-sharing rules. Although this is suggestive of little to none behavioral reaction on average, the effect of the reform could be blurred by changes in the income and wealth group composition (in terms of health and functional status) or by the heterogeneity of the co-payment change to which individuals were exposed.

3 Theoretical background

3.1 Nursing home admission and discharge

The goal of our empirical analysis is to estimate the price-elasticity of nursing home care use. In order to define the most relevant empirical strategy, we must formulate the margins at which the elderly with care needs, jointly with their relatives, are likely to adjust their use of nursing home care following an increase in co-payments. We develop a simple model of nursing home entry and use. We use the model to discuss moral hazard in the context of nursing home care and to establish the effective private marginal price of care.

In contrast to most other types of care, using nursing home care means changing where you live. In contrast to rehabilitative care, for nursing home care this change is almost always permanent. Individuals move to a nursing home at the end of their life, because of health problems and restrictions in performing daily activities that are expected to grow more severe. Moving to a nursing home often also involves selling or stop renting one's house, and comes with organisational and psychological costs for individuals themselves and for their family members. Therefore, once individuals are in a nursing home, we can expect that they do not respond to changes in the out-of-pocket price of LTC (at least not within the range of price changes we observe, and given that co-payments fall within ability to pay).

By contrast, we do expect that the decision to *enter* a nursing home is sensitive to the price that users have to pay themselves. When an older person with health problems and functional limitations is still living at home, she or he has several options to consider: she can move to a nursing home, use formal home care or informal care. The individual and her family weigh off the benefits of entering the nursing home with the costs involved. Both the benefits and the costs contain monetary components (the financial costs of living at home and the cost of home care versus the costs of nursing home care) and non-monetary components (e.g. the (perceived) loss of quality of living conditions and loss of independence versus the health gains from living in a nursing home). Each day, there is the choice to go the the nursing home *that* day, or postpone entry by at least one more day. In the empirical analysis, we focus on the decision to enter a nursing home of individuals who become eligible for the first time for nursing home care: they are the ones for which the marginal price of nursing home care may influence the timing of nursing home entry.

3.2 A simple model of nursing home entry

Individuals who become eligible for nursing home use can enter a nursing home in each consecutive period $t = 1, .., T$ after eligibility. An individual will enter a nursing home when the *private* benefits from living in a nursing home are larger than *private* the costs. As we are interested in the effects of variation in the out-of-pocket price of nursing home care p_t, we capture the net difference between all other (monetary and non-monetary) costs and benefits in a single term u_t: the (monetarized) net utility of living in a nursing home in period t. For now, we can also assume that the private price is constant in each period: $p_t = p$. An individual will live in a nursing home in period t when:

$$u_t > p \tag{1}$$

Deterioration of health and functional decline tends to occur in a staggered way at old age (Verbrugge & Jette, 1994; Edjolo *et al.*, 2016). We therefore assume that the utility of being in a nursing home increases over the time since eligibility: $\frac{du}{dt} > 0$. Taken together with a constant out-of-pocket price per period, the increasing utility of nursing home use over time implies that once a person chooses to move to a nursing home in period t, she will stay there for the rest of her life. The timing of nursing home entry is based on the first period for which u_t outweighs p.

3.3 Moral hazard

Moral hazard occurs because the individual only takes the private costs of nursing home care - the out-of-pocket price p - into account (Pauly, 1968; Zeckhauser, 1970). In an insurance system, these private costs are lower than the total costs of nursing home care (denote them $c_t = c$). In some cases, namely when $u_t > p$ but $u_t < c$, an individual will consume care for which the benefits u_t are smaller than the total costs c. If u_t fully reflects all other relevant societal costs and benefits, such use of care is suboptimal from a societal perspective, as the total benefits do not outweigh the total costs. The size of the welfare loss depends on the difference between the private price and the total price, and on the distribution of the net utility u_t.

We can establish the presence of moral hazard by considering the effect of an increase in the private price on use (e.g average use of nursing home care in period t). Generally, when an increase of the private price leads to a decrease in use, this is seen as evidence of (ex-post) moral hazard. However, this does not necessarily imply a socially inefficient use of nursing home care, for two main reasons. First, insurance not only reduces the marginal private price of care: it also provides an

(implicit) income transfer to individuals in need of care, thereby giving access to nursing home care that they could possibly otherwise not afford (Nyman, 1999; Konetzka *et al.*, 2019; Bakx *et al.*, 2015). It could be the case that individuals reduce their care use, not because the benefits are lower than the costs, but because they can not afford the higher private price. If an increase in the private price leads to individuals abstaining form nursing home care use for which $u_t > c_t$, the reduction in care use is not socially optimal.

Second, individuals might not fully include all other societal costs and benefits in u_t. For instance, for elderly with sever disabilities living at home, the costs of home care and curative care can be substantial and sometimes equally large or larger than the costs of nursing home care. As these costs of care at home are also partially or fully insured, individuals also do not fully account for them.

In our empirical application, we deal with the two issues that complicate the welfare implications of moral hazard in two ways. First, the increase in co-payments that we exploit is wealth-dependent. In fact, the increase was meant to adjust the co-payments for individuals with considerable financial wealth in accordance with their ability to pay. It is therefore unlikely that financial constraints play an important role in the decision to use nursing home care by those affected by the reform. Second, we will consider a range of other outcomes (in this version of the paper: survival) that are important elements of the societal welfare function, but may not (or not fully) included in the individual decision.

3.4 The effective price of nursing home use

In contrast to what we assumed above, the out-of-pocket price of nursing home use (the co-payment) is not constant over time, but depends on the duration of use. In the Dutch system, there are in fact two regimes: the first six months after nursing home entry, residents pay a low price $p_t = p_{low}$, and after that they pay a high price $p_t = p_{high}$. Such a regime, where the private price increases after a certain amount of time, is not uncommon. In the U.S. for instance, the first hundred days of nursing home care are covered by Medicare, after which individuals have to pay for this care themselves[15].

The fact that the out-of-pocket price depends on the length of stay means that the effective marginal price of care is not equal to the low price that has to be paid at nursing home entry (the spot price), but includes the expected additional payment under the high price regime. This can be seen by considering the marginal decision problem of an individual to enter the nursing home in month t^* instead of $t^* + 1$.

[15]Unless they qualify for means-tested Medicaid or have a private long-term care insurance.

We thus simplify our model a bit by assuming that the individual makes a decision each month instead of every day. The additional net utility that can be gained by entering the nursing home in month t^* instead of $t^* + 1$ is equal to $u(t^*)$. The individual has to weigh these benefits against the additional expected costs. Let s_t^{t+a} be the subjective probability of surviving from period t to $t + a$. This probability is the sum-product of the one-period survival probabilities s_j: $s_t^{t+a} = \prod_{j=t}^{t+a} s_j$. Now, we can formulate the total expected costs $\mathrm{E}(P_{t^*})$ of entering a nursing home in period t^* as follows:

$$\mathrm{E}(P_{t^*}) = \sum_{j=t^*}^{T} s_{t^*}^j p_j = \sum_{j=t^*}^{t^*+5} s_{t^*}^j p_{low} + \sum_{j=t^*+6}^{T} s_{t^*}^j p_{high}. \tag{2}$$

The expected costs (at time t^*) of entering the nursing home in period $t^* + 1$ is given by:

$$\mathrm{E}(P_{t^*+1}) = \sum_{j=t^*+1}^{t^*+6} s_{t^*}^j p_{low} + \sum_{j=t^*+7}^{T} s_{t^*}^j p_{high}. \tag{3}$$

The effective marginal price of entering the nursing home in t^* instead of $t^* + 1$ is thus given by:

$$\mathrm{E}(P_{t^*}) - \mathrm{E}(P_{t^*+1}) = p_{low} - s_{t^*}^{t+6}(p_{high} - p_{low}), \tag{4}$$

where we note that $s_{t^*}^{t^*} = 1$. An individual will enter the nursing home in period t^* instead of $t^* + 1$ if:

$$u_{t^*} > p_{low} - s_{t^*}^{t+6}(p_{low} - p_{high}). \tag{5}$$

The *effective* price is thus not the *spot* price paid in the first month p_{low}, but depends on the *expected* additional time spent under the high price regime. If survival up to the first additional month spent in the high price regime is high ($s_{t^*}^{t+6}$ is (almost) 1), the effective price is (close to) p_{high}.

4 Data and sample

4.1 Data

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

We link these individual-level data to background information from other administrative data sets using pseudonymyzed individual and household identifiers. First, we link data from CIZ on *eligibility* for institutional care and home care. Each eligibility decision specifies the type of care to be received, the care intensity and the patient's condition motivating LTC receipt. Second, we add data from the Tax Office on household income and wealth as of two years before: as we explain below, we use this information to simulate the magnitude of cost sharing on nursing home care at the individual level.[16] Third, we link information on age, date of death (if applicable), gender, marital status, household composition, number of children alive and municipality of residence, all taken from population registers (*Basisregistratie Personen*, or BRP). Fourth, we add claims data from the mandatory health insurance, which pays for the majority of spending on medical care in the Netherlands, collected by the federation of health insurers (Vektis). In addition, we retrieve the co-payments paid by nursing home residents from the CAK registers, for the years 2011-2014.[17]

4.2 Study population

We select individuals who are at least 66 years old. In this way, our study population has reached the normal retirement age (*AOW-leeftijd*),[18]. We focus on individuals who become eligible for a permanent nursing home stay for a somatic or psycho-geriatric condition for the first time. That is, we drop individuals who were eligible for nursing home care in the preceding months and calendar year. Moreover, we drop individuals who become eligible for other types of institutional care: assisted-living facilities, post-acute rehabilitation centers and hospices using

[16] The computation of co-payments in year t is based on income and wealth reported in $t-2$.

[17] In 2012-2013, the co-payment data cover (virtually) the entire population; for 2011 the co-payments are missing for individuals who died in that year. In addition, co-payment data for 2014 — and for years prior to 2011 — could not be retrieved

[18] Rebates for LTC co-payments that users are entitled to depend on whether they have reached the normal retirement age

the information on eligibility decisions. 86,430 individuals comply with these criteria.[19] We focus on individuals who became first-time eligible for nursing home care between 2010 and 2014, as reliable information on eligibility is not available before 2010[20], and the LTC system was substantially reformed in 2015.[21]

We then discard the few individuals (1.1%) with outlying values for income and wealth or inconsistent information on co-payment (e.g. values in excess of the co-payment cap). We end up with a sample of 82,972 individuals.

4.3 Outcomes

We define two outcomes. First, nursing home use $Use^k_{im(t)}$ is a dummy equal to 1 if individual i who has become eligible for the first time for a nursing home admission in month m of year t has used any nursing home care within k months after the date of first eligibility. Second, $Duration^k_{m(t)}$ equals the number of days spent in a nursing home within the same period. It is a continuous and bounded variable.[22] [23]

Setting the period k involves a trade-off. On the one hand, we want to track nursing home admission decisions over a longer time to capture the full extent of a behavioral response to a co-payment increase. Moving into a NH may take time because of demand-related and supply-related factors: less than 30% of those who become eligible for a nursing home admission had entered a nursing home within 30 days, but this figure exceeds 3/4 after 8 months. On the other hand, tracking nursing home use over a longer period means a larger risk that the outcome measures are contaminated by the reform for the sample of those who became eligible for nursing home care *right before* to the reform. For example, the probability that individuals who became eligible for nursing home care in (e.g.) October 2012 enter a nursing home within (e.g.) 12 months may be influenced by the change in the co-payment schedule implemented in January 2013. Indeed, for those who have not entered a nursing home yet by January 2013, the effective price of nursing home care that is taken into account when choosing whether to enter a nursing home in January

[19] To ensure that the health profile of selected individuals is relatively homogeneous, we also discard the elderly who have used care for the handicapped or long-term psychiatric care) in the same calendar year. This group is fairly small (2,942 individuals).

[20] Our sample starts with individuals who became eligible for nursing home care in February 2010 and were not already so in January 2010.

[21] For an overview of the 2015 reform, see Maarse & Jeurissen (2016).

[22] When computing these variables, we do not take into account the stays that have been made in a specialized institution other than a nursing home care (e.g. psychiatric hospitals, handicap centers, etc.).

[23] More precisely, our outcomes include any use of institutional elderly care, encompassing not only (i) nursing home care but also (ii) stays in assisted living facilities, (iii) hospice care and (iv) rehabilitative care. These 4 types of care can be distinguished in the data since 2011, but not in the 2010 data.

2013 or one month later (cf. Section 3) would be determined by the post-reform co-payment. Given that only a minority of individuals who have not entered a NH within 8 months after the first eligibility date ends up ever being admitted, we choose to set k to 8 months.[24]

5 Empirical strategy: a difference-in-differences approach

Nursing home admission rates have not been stable over time; therefore a simple before-after, event study type of analysis does not suffice to estimate the effect of the co-payment reform. Instead, we compare the change of nursing home use in the group for whom cost sharing on nursing home care was affected to the change in the use by the group of individuals who were not affected by the change in the maximum co-payments.

5.1 Definition of the treatment group and of the treatment intensity

As explained in Section 3, the effective marginal out-of-pocket price depends on the expected survival until the first month under the high-rate co-payment regime. As most individuals survive up to at least six month, we use the (monthly) high-rate co-payment as (a proxy for) the price individuals (and their families) take into account when deciding about the timing of their nursing home admission. The treatment group consists of individuals who's high co-payment increases as a result of the reform. To identify this group we calculate, for each individual, the high-rate co-payment *before* the reform (p_i^{pre}) and *after* the reform (p_i^{post}). We do this by applying the co-payment schedules explained in Section 2.3 to the information on the individual's income and wealth.

For individual i, the treatment intensity, denoted Δ_i, is the difference between the post-reform co-payment and the pre-reform co-payment:

$$\Delta_i = p_i^{post} - p_i^{pre} \tag{6}$$

[24] As the reform was announced in April 2012, we cannot rule out that the post-reform effective price of nursing home care enters the NH admission decision process for all the elderly who became eligible for nursing home care for the first time after that announcement. In the baseline analysis, we include individuals who have become eligible between April and December 2012, but we check the robustness of our findings to their exclusion.

Because of the design of the reform, Δ_i is non-negative (individuals either pay the same co-payment under both regimes, or a higher co-payment under the new regime).

In the main analysis, the treatment group consists of all individuals whose co-payment is higher with the post-reform schedule than with the pre-reform one:

$$Treated_i = 1 \iff \Delta_i > 0 \tag{7}$$

5.2 Difference-in-difference specifications

We first estimate the following difference-in-differences model:

$$Y_i = \beta_0 + \beta_1 Post_i + \beta_2 Treated_i + \beta_1 Post_i \times Treated_i + X_i'\theta + u_i \tag{8}$$

Y_i is the outcome (Use or $Duration$) for individual i who becomes eligible for nursing home care for the first time in a given month of a given year. By definition of our sample and outcomes of interest (nursing home admissions following first eligibility), an individual can only be observed once.[25] To make this point clear, we do not include any time subscript for the month or year at which the individual becomes eligible. $Post_i$ is a dummy equal to 1 if i becomes eligible for nursing home care for the first time after the reform was implemented (from January 2013 on), $Treated_i$ is a dummy equal to 1 if the individual belongs to the group affected by the reform, X_i' is a vector of individual characteristics, and u_i an individual error term.

Coefficient β_3 in Equation (8) captures the average effect of the reform, i.e. the population average of the change in nursing home use induced by the change in the co-payment schedule across all individuals affected by the reform. We expect β_3 to be zero or negative: as treated individuals are exposed to an *increase* in the marginal price of nursing home care, they should, if anything, lower their use of care compared to the control group.

The average effect of the reform masks heterogeneity in the response based on differences in treatment intensity: some individuals within the treatment group experience a small increase in their co-payment, while others experience an increase of more than €1,000 per month (see Figure 2). The effect on use can be expected to be bigger for the latter group than for the first. We take into account such heterogeneity in two ways.

[25] This implies that our identification strategy exploits cross-sectional variation in cost sharing induced by a reform.

First, we define separate treatment groups depending on the magnitude of the change in co-payments individuals were subject to. We divide individuals based on the treatment intensity into K X-euro bins, and estimate an average treatment effect for each of these groups. Or formally:[26]

$$y_i = \alpha_0 + \alpha_1 Post_i + \sum_{k=1}^{K} \alpha_2^k Treated_i^k + \sum_{k=1}^{K} \alpha_3^k Post_i \times Treated_i^k + X_i'\theta + \varepsilon_i. \quad (9)$$

In Equation (9), we expect the coefficients α^k to be decreasing in k: the higher the price change one group is exposed to, the lower its nursing home care use compared to the control group, following the reform.

Second, we estimate the treatment-effect as a linear function of the price change. This is done estimating the following equation:

$$y_i = \gamma_0 + \gamma_1 Post_t + \gamma_2 Treated_i + \gamma_3 Post_i \times (p_i^{post} - p_i^{pre})/100 + X_i'\theta + \mu_i \quad (10)$$

γ_3 then captures the effect of a one hundred-euro change on the outcome. As a sensitivity analysis, we also estimate the same specification using the log of the price change.

When the outcome is Use, the coefficient(s) of interest (either β_3, γ_3 or the α^ks) capture(s) the price sensitivity of NH care use (within a certain time) at the extensive margin; when the outcome is $Duration$, these coefficients reflect by how much the price of nursing home stays delays a nursing home admission.[27]

5.3 Identification assumptions

5.3.1 Unconfoundedness

The internal validity of the estimates relies on the unconfoundedness assumption: the composition of the control group on one hand, and of the treated group on the other hand, in terms of the determinants of nursing home care use, has not changed while the reform was implemented.

To ensure that this assumption holds, we include year fixed-effects, which pick up any unobserved time shock in terms of nursing home demand or supply. We furthermore control for age (in categories), gender, the amount of care that the individual is eligible for (from the first eligibility), whether the individual suffered

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

[27] This interpretation holds if we assume that (i) residents do not exit the nursing home and (ii) there is no mortality effect of the reform (which we test for later on).

from a psycho-geriatric condition, a somatic condition or both. We also include the number of children alive and whether the individual has a daughter which is alive at the beginning of the year. A higher number of children leads to a lower nursing home use at old age in the US, as was empirically shown by e.g. Wettstein & Zulkarnain (2019); possibly because having more children translates into a lower incidence of loneliness and depression, or higher informal care support — which may in turn delay a nursing home admission (e.g. Charles & Sevak (2005)). Furthermore, we include a dummy for whether the individual is a home owner: Rouwendal & Thomese (2013) show that Dutch home-owners are less likely to move to a nursing home than renters, possibly because the former incur higher emotional and transition costs when moving out of their residence.

Finally, we check the robustness of our estimates to the inclusion of health care spending in the previous calendar year[28] and home care entitlements [29] in the year prior to the year of first eligibility for nursing home. These variables can help to control for past changes in the underlying health status of the study population. Moreover, we control for disposable income to capture for possible income effects in nursing home use (independently from the price effect a higher co-payment may induce).Two potential shortcomings of including these variables as controls are that: (i) health care spending and home care entitlements may pick up preferences for formal care, which may play a role in the nursing home admission decision, while (ii) income correlates strongly with treatment intensity, and could thus undermine the statistical precision of the estimates.

5.3.2 Parallel trend assumption

Our DiD identification relies on the assumption that, absent the co-payment reform, the evolution of nursing home care use in the control and treated groups would have been the same over the 2010-2014 period. While this assumption is untestable, we believe it is likely to hold. Subsection 7.4 shows that the trends in nursing home use was similar for the control group and the treatment group if we exclude the period between the announcement of the reform and the implementation. Given the large number of observations (N¿77,000) and the nursing home admission rate (79% is admitted), it is unlikely that the finding that the difference is statistically

[28]Thus, we circumvent (a) potential endogeneity issues arising if nursing home care use and some health care use are substitutes or complements, and (b) potential measurement issues, as some health care for nursing home residents may be paid for by the nursing home and not billed to health care insurers.

[29]We control for *entitlements* for home care rather than*actual use*. The former has been shown to be equally distributed across income and wealth, while there is income-related inequality in the conversion of entitlements into actual use (Tenand *et al.*, 2020*a*).

insignificant is caused by a lack of statistical power (cf. Roth (2019)).

Furthermore, the parallel trend assumption is likely to hold because the other major reform during the study period did not affect differently the treatment group and the control group; thus it is plausible that the trends for these groups would indeed have moved parallel in the absence of the co-payment reform. Specifically, this other reform - a tightening of the criteria for admissions to assisted-living facilities, which resulted in a decreasing number of admissions that were approved during the study period - caused similar increases in all income groups and in all wealth groups in the shares of applications for residential care that were for nursing home care (cf. Appendix D). Moreover, as explained in Section 2, the rich and the poor in the Netherlands make use of the same LTC insurance scheme and use the same nursing homes; quality differences are limited and the co-payments are the same for all nursing homes.

6 Descriptive statistics and co-payment simulation

6.1 Descriptive statistics

Table I (p. 24) provides general descriptive statistics on the sample. Almost 8 out of 10 of the elderly who became eligible for nursing home care between 2010 and 2014 ended up entering a nursing home within the 8 months following their first indication for such care, with an average unconditional duration of stays of 122 days. The average co-payment paid in the calendar year of first eligibility is about €1,800.

The majority of individuals are females, aged between 80 and 95 years-old. Over three-quarters of the sample have a somatic condition upon first eligibility, and 35%pt have also a psycho-geriatric condition. Care packages ('ZZP') 7 and 8, which indicate severe care needs, are extremely uncommon: most elderly become eligible for nursing home care before reaching such a deteriorated health and functional status, and typically receive an indication for care packages 4 to 6. ZZP5, which corresponds to patients specifically with symptoms of dementia, represents a third of the sample. A third combines a psychosomatic condition and a psychogeriatric condition. Almost three quarters of the sample dies within the 4 calendar years following their first eligibility for nursing home care. The large standard deviation in both medical care spending incurred and the value of home care the individuals were eligible for reflects the high heterogeneity in health status.

Table I: General descriptive statistics: Study population.

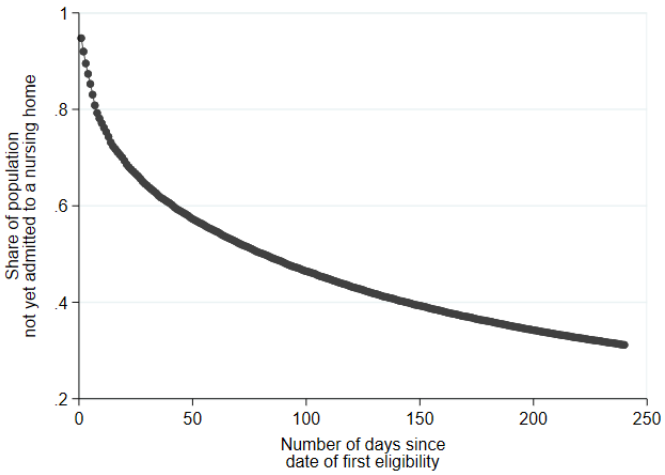
Variables	Mean	Standard-deviation
Year of first eligibility: 2010	0.144	0.351
Year of first eligibility: 2011	0.207	0.405
Year of first eligibility: 2012	0.176	0.381
Year of first eligibility: 2013	0.215	0.411
Year of first eligibility: 2014	0.257	0.437
Any nursing home care use	0.792	0.001
Days spent in a nursing home	121.817	0.343
Co-payment paid	1831.398	3363.119
Available income	19130.155	9548.834
Financial wealth	71366.286	228585.846
Home owner	0.249	0.433
Woman	0.781	0.414
Age: 66-74	0.000	0.273
Age: 75-79	0.119	0.324
Age: 80-74	0.243	0.429
Age: 85-89	0.301	0.459
Age: 90-94	0.197	0.398
Age: 95+	0.000	0.237
Child: 0	0.205	0.404
Child: 1	0.152	0.359
Children: 2	0.257	0.437
Children: 3+	0.386	0.487
Has at least a girl	0.624	0.484
ZZP 4	0.352	0.478
ZZP 5	0.349	0.476
ZZP 6	0.259	0.438
ZZP 7	0.000	0.169
ZZP 8	0.000	0.103
Somatic condition	0.786	0.410
Psychogeriatric condition	0.555	0.497
Both a somatic and a psychogeriatric condition	0.341	0.474
Death within 4 calendar years	0.725	0.447
Spending on GP care	304.901	244.303
Spending on pharmacy	1053.636	1677.905
Spending on auxiliary care	510.533	1087.936
Total health care spending	6658.494	13012.784
Value of eligible home care	17265.789	28817.312
N	82972	

SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).

NOTES: 'Co-payment paid' indicates the co-payments actually paid for nursing home care in the calendar year; in current euros. Income: annual, of two years before, in current euros. Wealth: on two years before, in current euros. Medical care spending and value of eligible home care: of previous calendar year, in current euros. 'ZZP' indicates the care package upon first eligibility.

Although a majority of the elderly who become eligible for a nursing home stay end up using institutional elderly care, only a small minority enters a nursing home right after the eligibility decision is issued. Figure 3 indicates the hazard rate of nursing admission. After 10 days, only 12% of the study population has been admitted to a nursing home (or has died). This proportion increases to 36% after 30 days, but only slowly increases to more than 70% eight months after first eligibility. Such a pattern is compatible with individuals and their families deciding upon the timing of a nursing home entry - although it might also reflect constraints on the supply-side.

Figure 3: Timing of first nursing home admission following the day of first eligibility.



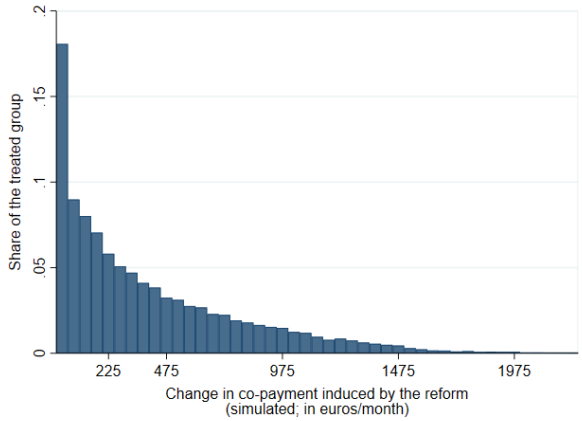
SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).

6.2 Change in cost-sharing due to the reform and treatment status

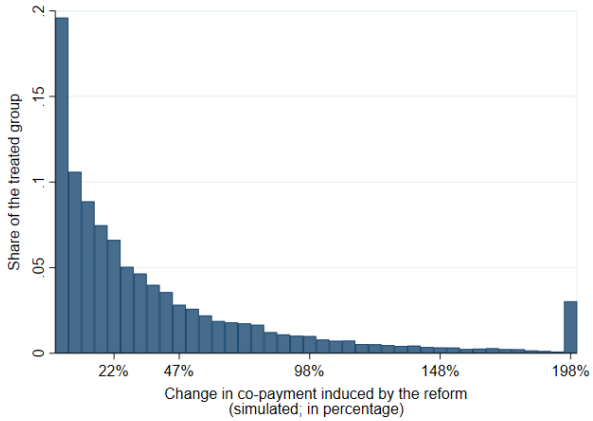
We first present descriptive statistics on the change in cost sharing induced by the reform. Our simulations indicate that 35% of the individuals in the study population would experience an increase in the high-rate co-payment to be paid for a nursing home stay, given their income and wealth. They form the treated group (N=29,065). The remaining 65% of the population have income and wealth such that cost sharing on nursing home care is exactly the same under the pre-reform and the post-reform schedules. They form the control group (N=53,907).

Within the treated group, the change in cost sharing on nursing home care induced by the reform is highly heterogeneous. Figure 4 plots the distributions of the change in the co-payment to be paid for an additional month in the nursing home, in euros per month (Panel A) and relative terms (Panel B). The median increase in co-payment induced by the reform is of 22% (or €270/month) within the treated group. 10% of the treated group is subject an increase in the high-rate co-payment to pay for a nursing home stay of 2% (or €21/month) at most, but for 10% of the treated group the increase exceeds 100% (€985), and reaches 300% (€1,636) for the 1% of individuals most affected by the change in the schedule.

Figure 4: Distribution of the change in the co-payment to be paid on nursing home use induced by the reform, within the treated group.



Panel A: Absolute change (in euros per month).

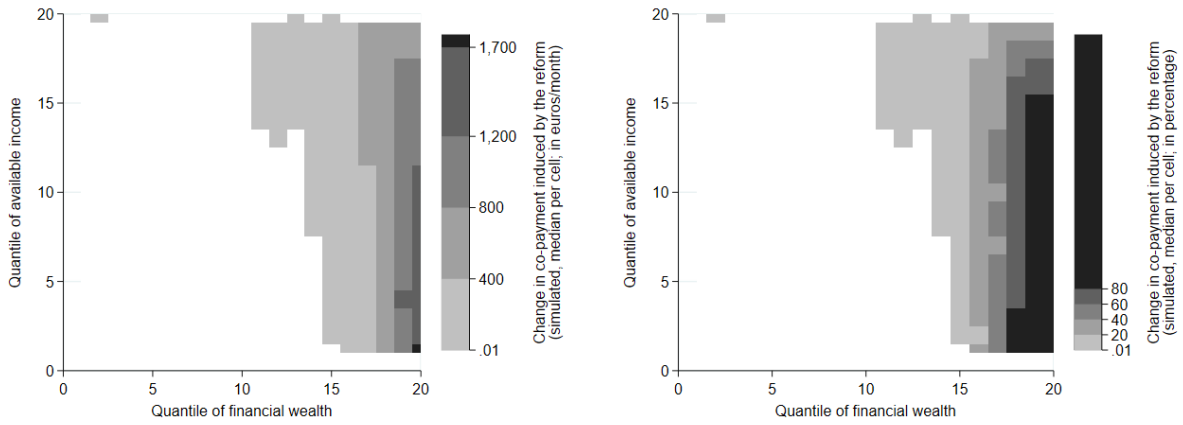


Panel B: Relative change (in percentages).

SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 and whose co-payment on nursing home care would increase with the 2013 reform (N=29,065).
NOTES: Authors' simulations.

Figure 5 displays the change in the average co-payment to be paid for a month spent in the nursing home (in euros), in absolute terms (Panel A) and relative terms (Panel B), depending on income and wealth. Larger increases in (counterfactual) co-payment were unambiguously experienced by the individuals with higher financial wealth. Consistent with Figure 2, individuals with lower than median financial wealth are predicted not to be affected by the 2013 reform. The largest increase in cost sharing on nursing home care was experienced by those in the top 10% of the wealth distribution but low to average income. Assignment into the treated group thus hinges primarily upon one's wealth, but treatment intensity correlates also with income.

Figure 5: Simulated change in co-payment on nursing home care induced by the reform, depending on income and wealth.



Panel A: Absolute change (in euros per month).

Panel B: Relative change (in percentages).

SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972). Financial wealth corresponds to wealth as taken into account in the co-payment schedule.
NOTES: Authors' simulations.

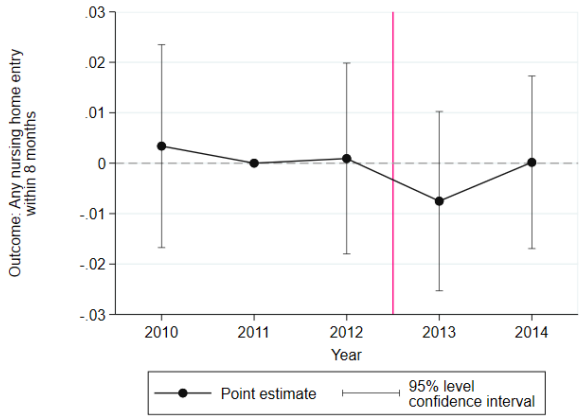
6.3 Descriptive statistics: control *versus* treatment groups

6.4 Raw difference-in-differences

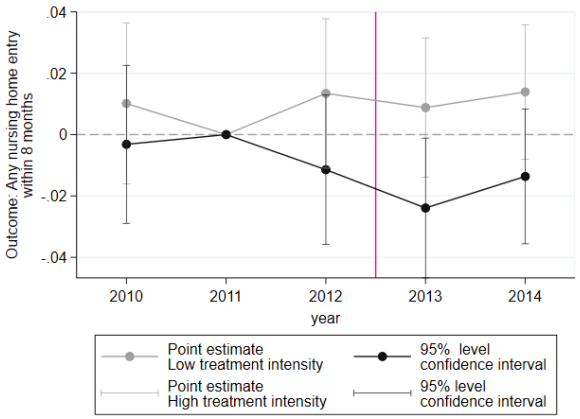
Before getting to the DiD estimations, we present graphical evidence on the evolution of nursing home use in the treated group, relative to the evolution in the control group. Figure 6 plots the point estimates from a very simple DiD equation, in which the probability to have been admitted to a nursing home within 8 months following first eligibility is regressed on year dummies, the treatment status and interaction terms between year and treatment status. 2011 is chosen as the reference year. In Panel A, each dot corresponds to the difference in average nursing home use in the treated group and average nursing home use in the control group (after controlling for year-specific effects), minus this difference in 2011. A point estimate different from zero would indicate that the evolution of nursing home use has differed in the treated and control groups, relative to 2011. In Panel B, we further distinguish between the group of individuals who would experience a high increase in cost sharing on nursing home care with the 2013 reform, and those who would experience a moderate increase.

Panel C from Figure 6 suggests that the trend in nursing home use, both prior to the reform and after the reform, has been similar in the treated and the control groups: in particular, individuals for whom the 2013 reform meant an increase in cost sharing on nursing home care did not have a lower probability of nursing home admission than those who were unaffected by the reform, following its implementation. However, Panel D suggests that the evolution of nursing home use after 2011 differed between those who were subject to a moderate increase in their co-payment and those who were exposed to a high increase in cost sharing. Compared with 2011, the nursing home use of the latter group somewhat decreased relative to what the control group experienced. The point estimates suggest that the divergence in trends started already in 2012, when the reform was announced. None of these raw differences are statistically significant however, and confidence intervals are relatively large (around 4 percentage points).

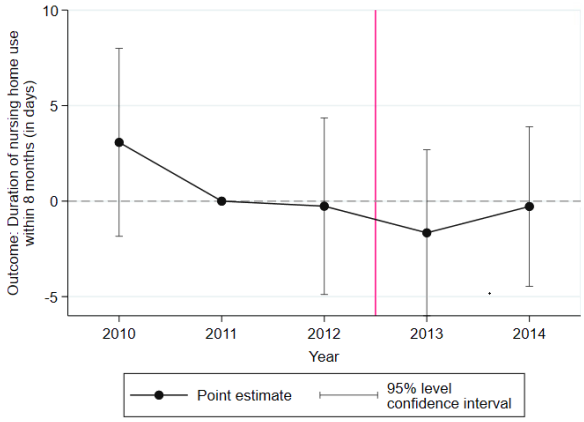
Figure 6: Use of nursing home care in the 8 months following first eligibility: difference between the treatment group and the control group.



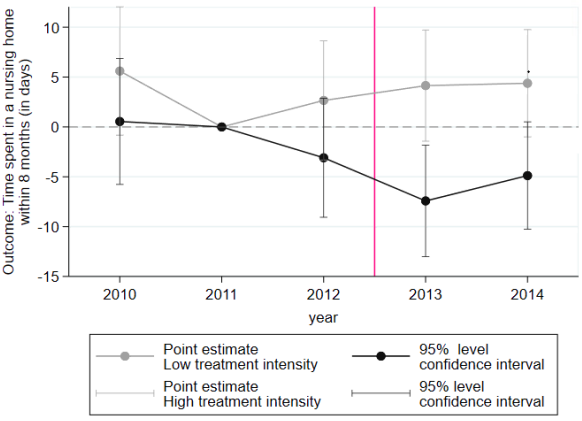
Panel A: by treatment status. Outcome: Nursing home use.



Panel B: depending on treatment intensity. Outcome: Nursing home use.



Panel C: by treatment status. Outcome: Time spent in the nursing home.



Panel D: depending on treatment intensity. Outcome: Time spent in the nursing home.

SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).
NOTES: Panels A and C: difference between the mean nursing home use in the treated group and the mean nursing home use in the control group in year t, minus the same difference for year 2011. Panels B and D: the black (resp. grey) series plots the difference between the mean nursing home use among individuals who would be subject to the a low (resp. high) change in co-payment due to the 2013 reform and the mean nursing home use in the control group in year t, minus the same difference for year 2011. A low (resp. high) change is defined as a change lower (resp. higher) than the median change estimated for the study population (+€270/month).

7 Difference-in-differences estimations

7.1 Baseline estimates

We now turn to the results from the DiD regression analyses. Table II provides the estimates of the main coefficients of interest, i.e. of β_1, β_2 and β_3 from Equation (8) (columns (1) to (4)) and γ_1, γ_2 and γ_3 from Equation (10) (columns (5) to (8)). Panel A does this taking 'Any nursing home care use' as the outcome of interest, and Panel B presents the result for the outcome 'Use duration'. Columns (1) and (5) include no control but year dummies; columns (2) and (6) additionally control for socio-demographic covariates and dummies for the LTC purchasing region the individual lives in; columns (3) and (7) further include dummies for one's position in the income distribution. Finally, columns (4) and (8) also control for medical care spending and the value of home care services the individual was eligible for in the calendar year preceding the year in which s/he became eligible for a nursing home admission. We derive robust standard errors.The estimates for the control variables are presented in Appendix C (p. 53 and 55).

In Panel A, the estimate for $Post \times Treated$ in columns (1) to (4) (i.e coefficient β_3) indicates whether nursing home use differs between the treated and the control groups following the reform. When we do not include any controls (column (1)), the treated group is predicted to have a 0.459 percentage point lower probability of any nursing home admission than the control group. This point estimate increases (in absolute value) to 0.6-0.7 percentage point when we include control variables (columns (2) to (4)). However the point estimate is never statistically significant, despite being practically so. Limited statistical precision may come from the fact that treatment intensity varies substantially.

Still in Panel A, the estimate for $Post \times Treated$ (γ_3) in columns (5) to (8) indicates whether individuals who are subject to a higher increase in the price of nursing homes due to the 2013 reform experience lower nursing home admission rates than the untreated group, following the reform. Column (5) indicates that a €100 increase in the co-payment to be paid for an additional month in the nursing home decreases the probability to be admitted to a nursing home within the 8 months following first eligible by 0.222 percentage points (column (5)). When controlling for one's position in the income distribution, the point estimate increases (in absolute value) to 0.384 percentage points (column (7)). The inclusion of medical care spending and home care eligibility in the previous year makes little difference for the treatment estimate (column (8)). In all specifications, the effect of the change in co-payment is statistically significantly different from 0.

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

Regressor:	Treatment dummy				Treatment intensity (co-payment change)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A	Outcome: **Any nursing home use**							
Post	0.1090***	0.0821***	0.0831***	0.0811***	0.110***	0.0829***	0.0864***	0.0842***
	(21.29)	(16.46)	(16.60)	(16.16)	(22.97)	(17.65)	(18.36)	(17.84)
Treated	-0.0174***	-0.00144	0.000206	0.000122	-0.0153***	-0.000551	0.00457	0.00418
	(-3.96)	(-0.33)	(0.05)	(0.03)	(-4.58)	(-0.17)	(1.29)	(1.18)
Post × Treated	-0.00459	-0.00699	-0.00661	-0.00703				
	(-0.78)	(-1.23)	(-1.16)	(-1.23)				
Post × Treated ×Δ					-0.00222**	-0.00228***	-0.00384***	-0.00379***
					(-3.15)	(-3.34)	(-5.58)	(-5.51)
Constant	0.744***	0.602***	0.641***	0.639***	0.744***	0.601***	0.642***	0.640***
	(175.20)	(40.79)	(40.83)	(40.52)	(179.22)	(40.81)	(40.93)	(40.63)
R^2	0.011	0.081	0.087	0.090	0.011	0.082	0.087	0.090
Panel B	Outcome: **Time spent in a nursing home**							
Post	29.64***	25.11***	26.69***	25.94***	29.48***	25.04***	27.09***	26.27***
	(24.64)	(22.30)	(23.58)	(22.88)	(26.32)	(23.79)	(25.61)	(24.78)
Treated	-3.705***	-1.830	0.0518	0.0817	-3.927***	-1.953*	0.546	0.481
	(-3.69)	(-1.92)	(0.05)	(0.08)	(-4.94)	(-2.57)	(0.67)	(0.59)
Post × Treated	-1.658	-1.271	-2.237	-2.359				
	(-1.16)	(-0.96)	(-1.68)	(-1.78)				
Post × Treated ×Δ					-0.297	-0.263	-0.807***	-0.789***
					(-1.70)	(-1.63)	(-4.94)	(-4.85)
Constant	104.2***	74.34***	94.93***	93.76***	104.3***	74.33***	95.23***	94.07***
	(112.80)	(24.15)	(28.19)	(27.75)	(115.59)	(24.18)	(28.32)	(27.87)
R^2	0.012	0.158	0.165	0.170	0.012	0.158	0.165	0.170
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies for LTC purchasing region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Dummies for vintile of available income	No	No	Yes	Yes	No	No	Yes	Yes
Medical care & home care	No	No	No	Yes	No	No	No	Yes
N	82972	82972	82972	82972	82972	82972	82972	82972

NOTES: t-statistics in parentheses, based on robust standard errors; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from 2010-2014, sample of 66+ single first-time nursing home eligible. The outcomes are defined as: (a) any nursing home use (admission) in the 8 months following the day of first eligibility, and (b) the number of days spent in a nursing home in the 8 months following the day of first eligibility. . For Specifications (5) to (8), the regressor of interest is an interaction between the dummies for being treated and becoming eligible for nurisng home care after the reform 2013 and the magnitude of the change in co-payments induced by the reform (Δ), expressed in hundred euros per month. Specifications (3), (4), (7) and (8) include dummies for the 'vintile' of available income, i.e. the position of the individual in the distribution of available income in the study population, from vintile 1 (5% lowest income) to vintile 20 (5% highest income). Specifications (4) and (8) include spending on medical care under the Health Insurance Act and the monetary value of home care the individual was eligible for in the previous calendar year.

Similarly, columns (1) to (4) of Panel B in Table II indicate that individuals in the treated group spend less time in a nursing home following their first eligibility than the control group, following the reform. The magnitude of the effect is relatively small (between -1.271 and -2.359 days depending on which controls are included) and never statistically significant. By contrast, the estimates in columns (7) to (8) indicate a statistically significant price sensitivity of the time spent in a nursing home. Assuming no admission ends up with a discharge back to the community, we interpret this as evidence we find that a €100-increase in effective price of nursing home care postpones an admission by about 0.8 day.[30]

If assignment to treatment in the study population were random, or as good as so, the inclusion of control variables would not affect the point estimates. In the DiD estimations reported in both Panels A and B of Table II, the inclusion of controls do slightly improve the precision of the estimates for β_3 and γ_3. Furthermore, controlling for income somewhat affects the estimates. This suggests that the income composition of the treated group has evolved differently from the income composition of the control group following the reform. Failing to account for this composition change biases the estimates of the price sensitivity of nursing home use, because income is a practically significant determinant of nursing home use. Consistent with the findings from Tenand *et al.* (2020*b*), our estimations reveal a negative income gradient in nursing home admissions: individuals higher up in the income distribution are less likely to enter a nursing home following eligibility for such care.[31]

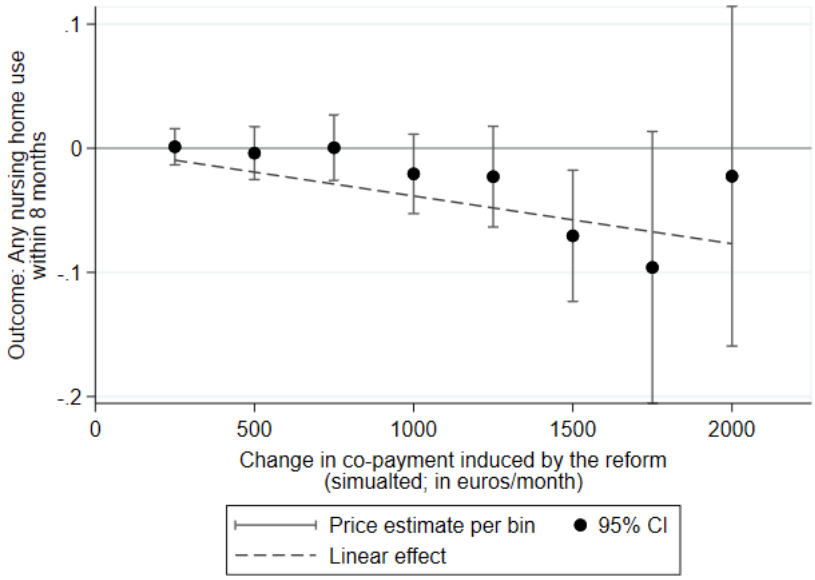
Equation (10) (whose estimates are reported in columns (5) to (8)) takes into account the heterogeneity in the magnitude of the reform by assuming a linear price effect: any additional euro increase in the co-payment to be paid is expected to yield the same effect on nursing home use. We tested the validity of this assumption by plotting the linear estimate against the estimate of the effect of the reform for individuals who are subject to varying levels of co-payment increase, as proposed in Equation (9) (page 21). Figure 7 displays the set of coefficients α_3^k, i.e. the effects of the increase in the price of nursing home care on the probability of a nursing home admission for different 'bins', each bin grouping together individuals who experience a similar change in their co-payment (within a €250 range). The linear price specification seems to slightly over-estimate the price sensitivity (in absolute value) of individuals who were subject to a change in co-payment lower than

[30] Due to data limitations for 2010, our measure of nursing home care use includes stays in institutional care facilities other than nursing homes. Data from 2011-2014 indicate yet that, in our sample, only 7% of the first admission following eligibility are in a facility other than a nursing home.

[31] Results available on request.

€750 per month, but it offers a good estimate of the price sensitivity of individuals who experience a higher increase in cost sharing (of between €1,000 and 1,750 per month). The estimate of the treatment effect (price sensitivity) for those who were subject to a change in co-payment between €1,750 and 2,000 per month is smaller (in absolute value) than what is suggested by the linear effect estimate, but the effect is extremely imprecisely estimated, due to the small number of individuals of this group. Overall, the linear price specification offers a reasonable approximation of the functional form of the price sensitivity of nursing home use.[32]

Figure 7: Price sensitivity by treatment intensity: difference-in-differences estimates across bins and linear effect of co-payment change.



SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).
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 8 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 co-payment on nursing home care induced by the reform. The first bin from the right groups individuals subject to a positive increase in co-payment up to €250/month. The second bin groups individual subject to an increase higher than €250/month and up to €500/month etc.

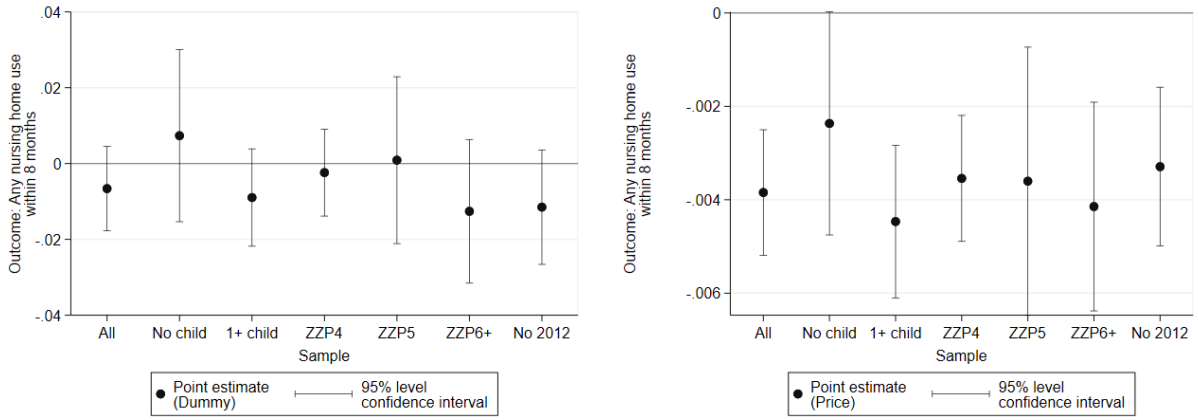
[32] In Appendix C.2, we also test a linear relative price effect specification, which offers a lower fit with the bin-based estimates.

7.2 Heterogeneous effects

We conduct a heterogeneity analysis to test whether the price sensitivity differs across care need levels and depending on the number and gender of children alive. We hypothesize that price sensitivity is lower for more severely disabled individuals. In particular, we assume that the disablement process induces a shift in individual preferences, lowering the degree of substitutability between long-term care services and other consumption, or even between nursing home care - whose marginal utility increases as functional status worsens - and home care. If this assumption is correct, then more severely disabled individuals should be less prone to adjusting their use of nursing home care to a change in its marginal price than individuals with moderate disability. Our hypothesis is that adult children, and daughters in particular, are more likely to provide informal care, which may then delay a nursing home admission (e.g. Charles & Sevak (2005)).

To test these hypotheses, we replicate the baseline estimations on different subsamples. Figure 8 displays the estimates for either the average impact of the reform (Panel A, corresponding to Equation (8)), or a linear price effect (Panel B, corresponding to Equation (10)), on any nursing home care use. The first estimate starting from the left-hand side in Panel A (resp. Panel B) corresponds to the baseline estimate reported above in column (3) (resp. column (7)) of Table II). Panel B suggests that the decision to enter a nursing home is more price-sensitive for the elderly who have children alive, consistent with our hypothesis that children may make aging-in-place a more feasible, or attractive, alternative to institutional care. However, statistical precision is too low to conclude that the elderly with potential informal caregivers are more price-sensitive. When comparing price sensitivity of nursing home care use across the profiles and levels of care needs (ZZP4, ZZP5 and ZZP6+), we find no evidence that those with less severe care needs when they become eligible for nursing home care are more price-sensitive in their decision of entering a nursing home.

Figure 8: Heterogeneity of effects: Estimates by care needs and number of children children, for any nursing home care use.



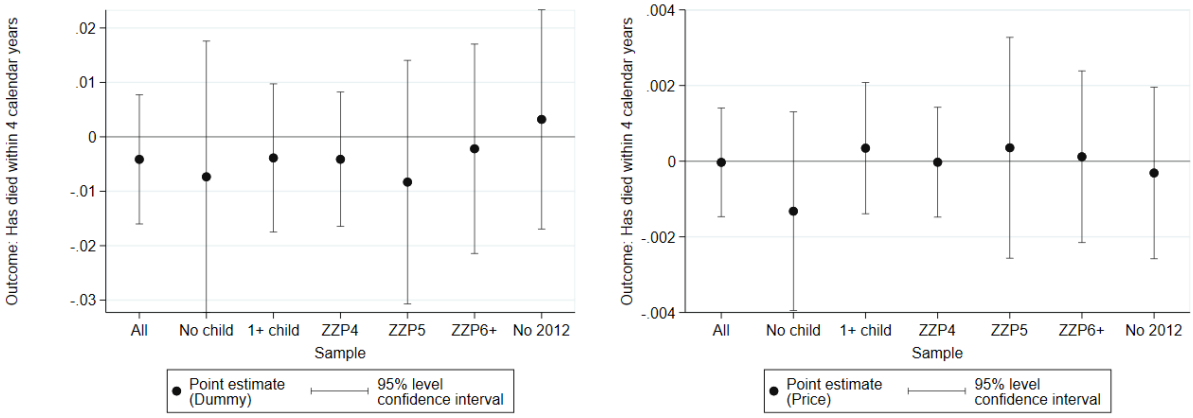
Panel A: Treatment effect. *Panel B: Linear price effect.*

SAMPLES: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972). 'No child': individuals with no children alive. '1+ child': individuals with children alive. 'ZZP4': individuals with moderate care needs; 'ZZP5': individuals with cognitive disorders; 'ZZP6+': individuals with severe to very severe care needs. 'No 2012': excludes individuals who become eligible for nursing home care between April and December 2012.
NOTES: Each dot corresponds to an estimate of coefficient β_3 (Panel A) or γ_3 (Panel B) derived on a different sample. Controls include year effects, dummies for LTC purchasing region, socio-demographic characteristics and income controls (cf. columns (3) and (7) of Tables II). 95% confidence interval, based on robust standard errors.

7.3 Mortality

The empirical analysis so far does not allow to conclude whether the decrease in nursing home care admissions induced by the reform was welfare-decreasing. To shed light on some of the utility costs associated with lower nursing home use, we assess the impact of co-payments on the probability of death within 4 calendar years following the day of first eligibility for nursing home care, using the same DiD approach as previously. The estimates are reported in Figure 9 for the entire sample as well as for several groups. Note that the 4-year mortality rate is over 72% for the study population. Panel B suggests that additional hundred euro of co-payments do not make any difference for mortality, with point estimates extremely close to 0.

Figure 9: Impact of co-payments on mortality: difference-in-differences estimates.



Panel A: Treatment effect. *Panel B: Linear price effect.*

Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972). 'No child': individuals with no children alive. '1+ child': individuals with children alive. 'ZZP4': individuals with moderate care needs; 'ZZP5': individuals with cognitive disorders; 'ZZP6+': individuals with severe to very severe care needs. 'No 2012': excludes individuals who become eligible for nursing home care between April and December 2012.
NOTES: Each dot corresponds to an estimate of coefficient β_3 (Panel A) or γ_3 (Panel B) derived on a different sample. Outcome is defined as having died within 4 calendar years following first eligibility for nursing home care. Controls include year effects, dummies for LTC purchasing region, socio-demographic characteristics and income controls (cf. columns (3) and (7) of Tables II). 95% confidence interval, based on robust standard errors.

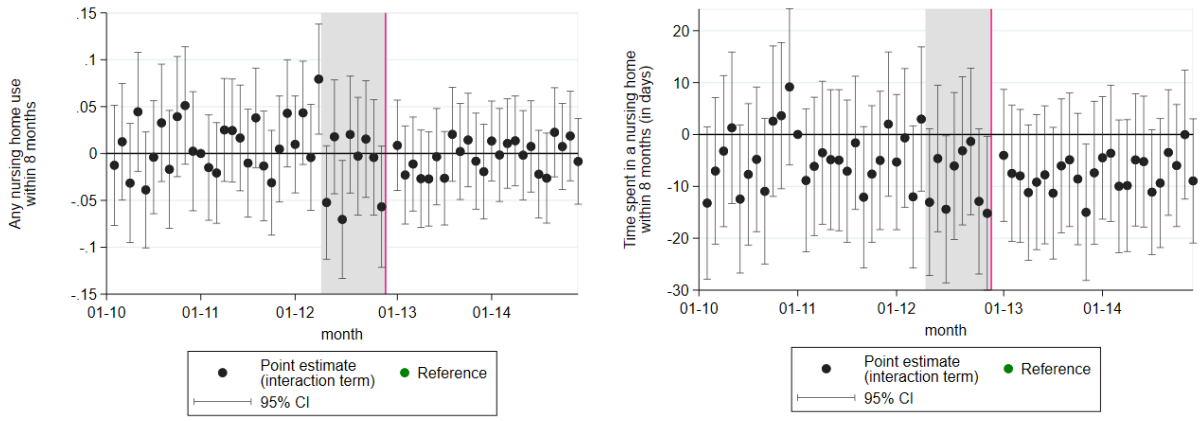
7.4 Robustness checks and directions for further analyses

7.4.1 Assessments of pre-trends at a monthly level and scope for anticipation effects

We discuss and assess the robustness of our estimates in several ways. First, we check further whether there is any sign that the treated and the control groups had diverging trends in terms of nursing home care use before the co-payment reform took place. Our data do not allow to get back before 2010, such that we only have 3 years of observation prior to the reform. However, eligibility decisions and use of nursing home care are recorded at the daily level in the data. In choosing the unit of observation for the regression analysis (e.g. day, month or year), we trade off between statistical precision (lower for a more disaggregate unit of observation) with ability to observe pre-reform and post-reform trends. We conduct the baseline analysis at the year level to ensure that minimal detectable effects are not excessively large; however, we also replicate the DiD estimations at the monthly level to check that we observe similar patterns. Figure 10 displays the estimate of how nursing

home care use has differed between the treated and the control groups, relative to how the outcome differed in January 2011 (chosen as the reference month).[33] We also implement a Fisher test to test for the joint significance of the pre-reform coefficients, the results of which are displayed in Table III.

Figure 10: Check on pre-trends: difference-in-differences estimates at a monthly level.



Panel A: Any nursing home care use.

Panel B: Time spent in a nursing home (in days).

SAMPLES: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).
NOTES: Confidence intervals at the 95% level, based on robust standard errors. January 2011 is chosen as the reference month. As information on eligibility for nursing home care is not available prior to January 2010, the observation period starts with February 2010. The grey-shaded area corresponds to the pre-reform months in which the reform was already known.

Row (1) of Table III indicates that we reject the null hypothesis that all pre-reform coefficients are equal to 0 when the outcome is nursing home use, but not for time spent in a nursing home if we refer to a significance level of 10% ($p > 0.10$). However, Panel A from Figure 10 suggests that there is no clear direction in which nursing home use would diverge between the two groups: the point estimates jump from negative to positive values. In addition, both Panels A and B reveal that point estimates are (individually) statistically different from zero only in the period from April to December 2012. Given that the reform was announced in April 2012, we conjecture that anticipation effects might create a divergence in nursing home care use already prior to the implementation of the reform, and confound the estimate of its effect. We therefore replicate the test of joint significance of the month-treatment interaction term for the pre-reform period, excluding April to December

[33] We estimate an equation similar to Equation (8), in which we replace dummy $Post$ by a full set of month dummies ($Month_m$) and the interaction term $Post \times Treated$ by a full set of interaction terms between treatment status and month dummies ($Month_m \times Treated$).

2012. Row (2) of Table III indicates that it becomes impossible to reject that pre-trends are similar. This is reassuring, although the reduction of sample size may underpower the test. Furthermore, we test the robustness of our DiD estimates to the exclusion of individuals who become eligible for nursing home care between April and December 2012. As shown on Figure 8 (p. 35), the point estimates are very similar to the baseline estimates.

Table III: Check on pre-trends: p-value of test of joint significance.

Outcome:	Any nursing home care use	Time spent in a nursing home	N
(1) Entire period	0.00	0.25	82,972
(2) Excluding April-December 2012	0.29	0.50	77,224

SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).
NOTES: p-value of Fisher test of joint significance of coefficients for $Treated \times Month_m$ for the months prior to the reform. Reference month: January 2011. Controls include year effects, dummies for LTC purchasing region, socio-demographic characteristics and income controls (cf. columns (3) and (7) of Tables VI). Robust standard errors.

7.4.2 Quality of co-payment simulations

The internal validity of our estimates hinges upon the quality of our simulations of the high-rate co-payment, under the pre-reform and the post-reform schedules. Two elements make us confident in our ability to approximate the price increase due to the reform in a satisfactory way. First, co-payments and their increase with the 2013 reform critically depend on a specific tax concept of wealth, which we observe in the administrative data. Second, we use individual-level data on the co-payments actually paid in 2012 and 2013 to assess the extent to which they match the co-payments we simulate. We use an external sample consisting of elderly who spent the entire years of 2012 and 2013 in a institutional elderly care facility and who were subject to the high-rate co-payment for this entire period. For this group, the co-payments paid in a year are simply equal to 12 times the monthly price of nursing home care; furthermore, we can not only compare the co-payment actually paid by a given individual in 2012 (resp. 2013) with the co-payment we simulate based on the pre-reform (resp. post-reform) schedule, but also compare the *change* of actual co-payments between 2012 and 2013 with the co-payment change we predict based on our simulations. We are thus able to assess the quality of our simulation of the 2013 reform.

In the sample of all-year users, simulated co-payments exceed the co-payments actually paid by only 2% on average. For a majority of individuals, the difference

between simulated and actual co-payments is only a few percent. However, for some individuals, the gap is extremely large. On addition, we tend to over-estimate co-payments for individuals who earn an income around the minimum pension benefit.[34] We conjecture that simulation errors mainly arise because we do not observe taxable income nor the income tax paid - which both enter the co-payment schedule -, but proxy it with available income. On average, this seems to be a good approximation, but for some individuals, who must fall under specific tax rules or be able to claim some tax rebates, it results in lower-quality simulations. One limitation of our simulation approach is that we have to focus on singles, as co-payment rules for couples are extremely complex.

7.4.3 Change in the probability to become eligible for nursing home care

By focusing on the individuals who have received an indication for nursing home care, we implicitly assume that the reform has not affected the *probability to be granted a nursing home indication*, nor has coincided with an external change in this probability. Such a change could happen for two reasons: first, the assessors of the central agency in charge of granting eligibility for long-term care (CIZ) may have changed their practices in terms of assessments and eligibility decisions; second, the elderly who are subject to an increase in their co-payment may become less likely to apply for a needs assessment by CIZ, as the out-of-pocket price of nursing home care increases.

Regarding the first channel, the organization and functioning of needs assessment make it unlikely: given the universal nature of the social insurance system and the fact that individual income and wealth are not taken into account during the needs assessment process, we do not expect that the co-payment reform would have induced the CIZ assessors to become relatively more lenient towards individuals exposed to an increase in their cost sharing. By contrast, we cannot rule out the second channel. Our current analysis focuses on the price sensitivity of nursing home admission *conditional on eligibility* and thus ignores the second margin of behavioral reaction - the price-sensitivity of the claiming of a nursing home admission. Not only is this second margin relevant to document, but also ignoring it could potentially bias our estimates, if more price-elastic individuals - or their families - are relatively less likely to apply to CIZ in order to be granted a nursing home admission after the reform than before.

[34] In a next version, we will derive more detailed figures.

7.4.4 Modelling of substitute care options

So far, our approach does not take into account the fact that the 2013 reform also led to an increase in the co-payments levied on home care use. In microeconomic terms, our identification strategy ignores the change in price of a substitute for nursing home care. We focus on individuals who become eligible for nursing home care strictly speaking; according to the Dutch needs assessment agency, an individual with a ZZP package 4 (the threshold to become eligible for nursing home care) has a functional status and health such that she or he requires 13 hours of home care per week, including 5.5 hours of skilled nursing care (College voor Zorgverzekeringen, 2012). Although possible, it is unlikely that individuals with such relatively high care needs would receive only informal care in the case they do not enter a nursing home and stay at their home. A richer modelling would bring into the picture the trade-off between nursing home care and home care services, by looking at how the relative price of these two types of care has changed with the 2013 reform.Previous empirical research (Non, 2017) exploits the same reform of co-payments in the Netherlands in 2013 as we do: this reform also led to an increase in the out-of-pocket price of home care services for individuals with positive taxable wealth. The price increase reduces the probability to use any home care. However, there was no change in the number of hours of home care received conditional on use.

8 Discussion

Nursing home residents pay for part of the costs of their stay in virtually all countries, but there is limited evidence to what extent such co-payments affect the decision to move to a nursing home. Our study is one of the first to provide quasi-experimental evidence that users of permanent nursing home care are responsive to changes in co-payments, even when relatively moderate in comparison to residents' financial means.We exploit exogenous variation in the price of nursing home care induced by a reform of the co-payment rules in the Netherlands that increased the co-payments for 35% of the elderly (those with moderate income and high wealth), while others (those with low wealth or a very high income) were unaffected. 25 percent experienced an increase in the co-payment to be paid for an additional month in a nursing home of €600 or more, meaning that the increases in co-payments were substantial. Yet the income- and wealth-dependent design of the Dutch co-payment system ensures that co-payments should still be within an individual's financial means.

The reform on average did not decrease the probability or the timing of nursing home use within the first 8 months after eligibility. However, those who experienced a larger increase in their co-payments decreased their use more: the elderly who were subject to a co-payment increase of around €1,500 per month have a 7 percentage point lower probability of a nursing home admission within 8 months than the elderly who were not affected by the co-payment reform. The elderly who faced a co-payment increase between €750 and €1,250 per month postponed their nursing home admission within the first 8 months by 10 days on average .

The results show that co-payments can indeed contribute to restricting (unwarranted) use of nursing home care. They also show that even individuals who are eligible for nursing home care, and for whom an independent assessor has determined that around the clock care and supervision are needed, have some discretion in choosing whether or when to use care. Although this stands in contrast to what is sometimes believed, this is in line with prior research that finds that the timing of nursing home care use is indeed not only determined by health status but also by other financial- and non-financial factors (e.g. Tenand *et al.* (2020*b*); Diepstraten *et al.* (2020)).

Our study complements earlier empirical research from the U.S. and South Korea that looks at the effects of insurance status or different levels of cost-sharing on nursing home use. These studies have found mixed results, possibly related to the low power of some of these studies or differences in quality of the nursing home care investigated. While in the US a nursing home resident's co-payments are related to the payment received by the nursing home, in the Netherlands they are not. The 2013 co-payment had therefore no effect on financial incentives on the provider's side, and we can identify the effect of demand-side financial incentives on nursing home use. Most studies in this literature, with the notable exception of Hackmann & Pohl (2018), capture both demand-side and supply-side adjustments, which have different policy implications.

Two aspects of the context are important when relating our findings to other institutional care settings. First, we consider nursing home care use. A lot of previous work has focused on post-acute care (or rehabilitative care, skilled nursing facilities), which has very different dynamics: because users of this care are expected to return home (see e.g. Hackmann & Pohl (2018)), financial incentive are primarily geared at shortening stays. For nursing home care, residents are expected to stay for the rest of their life, and incentives are geared towards delaying entry. Our results thus pertain to 'ageing in place', i.e. the policy efforts to stimulate and facilitate living at home as long as possible. Second, the Netherlands has extensive

public provision of both home care and nursing home care. This context affects which elderly are on the margin of nursing home use (Bakx *et al.*, 2018). On the one hand, we can expect that elderly who consider a nursing home admission have a relatively poor health status (or else they would choose for publicly-subsidized, comprehensive home care). On the other hand, the availability of high quality home care might also be one of the very reasons that even the elderly with relatively poor health are able to postpone an admission.

The empirical analysis so far does not provide a final conclusion whether the reduction in nursing home care use induced by the co-payment reform, and by co-payment increases more generally, is welfare improving. This would be the case if the societal costs of the reduction in care use induced by the reform are larger than the benefits. Given the design of the Dutch co-payment schedule and the features of the 2013 reform, which both aim at ensuring that the elderly contribute based on their financial means, it is unlikely that the reduction in care use is due to some elderly being no longer able to afford care. It is, however, possible that elderly are not aware of, or do not fully include all other costs and benefits. For example, elderly do not pay the full costs of the care they can receive at home; in addition, they and their families might over- or underestimate the health benefits that can be gained from a nursing home admission. To obtain a more complete picture of the welfare effects of the reform, we plan to assess its impact on some of the major costs and benefits (home care use, health) in a next version of the paper. As a first attempt in this direction, we already estimated the effects on mortality and found no effect.

The finding that users of nursing home care are responsive to co-payments is important for policy makers. Across most of the developed world, governments have put forms of public provision or financing of nursing home care into place. Generally, these public schemes do involve substantial cost-sharing, that shifts a considerable part of the financial burden onto care users themselves. Our results at least partly motivate the adoption of cost-sharing by showing that it is indeed an effective way to reduce the use of publicly financed care. Moreover, the fact that reduction in use can be achieved by relatively moderate co-payments, which do not exceed users' financial means, might motivate the implementation of explicit co-payments based on individual's ability to pay. Income- and wealth-dependent co-payment schemes like the Dutch one may be an efficient alternative to the often used, more drastic cost-sharing schemes such as means-testing, that put a much higher financial burden on the individual user (Wouterse *et al.*, 2019).

References

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

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

Bakx, P., Wouterse, B., van Doorslaer, E. et Wong, A. (2018), Better off at home? effects of a nursing home admission on costs, hospitalizations and survival, Tinbergen Institute Discussion Paper 2018-060/V.

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

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

College voor Zorgverzekeringen (2012), *Zelf uw zorg inkopen in 8 stappen 2012*.
URL: *https://www.hetvenster.com/Content/DownloadDocs/2012/pgb%20brochure,%20zelf%20zorg%20inkopen,%202012.pdf*

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

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

Edjolo, A., Proust-Lima, C., Delva, F., Dartigues, J.-F. et Pérès, K. (2016), 'Natural history of dependency in the elderly: a 24-year population-based study using a longitudinal item response theory model', *American Journal of Epidemiology* **183**(4), 277–285.

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

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

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

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

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

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

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

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

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

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

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

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

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

Pauly, M. V. (1968), 'The economics of moral hazard: comment', *The American Economic Review* **58**(3), 531–537.

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

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

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

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

Rouwendal, J. et Thomese, F. (2013), 'Homeownership and long-term care', *Housing Studies* **28**(5), 746–763.

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

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

Tenand, M., Bakx, P. et Van Doorslaer, E. (2020*a*), Eligibility or use? horizontal inequity in home care receipt in the netherlands, Working paper.

Tenand, M., Bakx, P. et van Doorslaer, E. (2020*b*), 'Equal long-term care for equal needs with universal and comprehensive coverage? An assessment using Dutch administrative data?', *Health Economics* . Published online 20 January 2020.

Verbrugge, L. M. et Jette, A. M. (1994), 'The disablement process'.

Wettstein, G. et Zulkarnain, A. (2019), Will fewer children boost demand for formal caregiving?, CRR Working Paper, Center for Retirement Research at Boston College.

Wouterse, B., Hussem, A. et Wong, A. (2019), The welfare effects of co-payments in long term care, Discussion Paper 394, CPB Netherlands Bureau for Economic Policy Analysis.

Zeckhauser, R. (1970), 'Medical insurance: A case study of the tradeoff between risk spreading and appropriate incentives', *Journal of Economic Theory* **2**(1), 10–26.

A Data sources

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

A.1 Overview of the data used

Table IV provides the list of the microdata used in this research.

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 Social Affairs (VWS).[35]

In addition, in order to link each individual to their tax household and household income, we use the table of correspondence 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.

To link each municipality to one of the CIZ regional offices and 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 it stood in 2013.

The linkage of individuals to their legal parents 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.

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

Table IV: Datasets used (as recorded in CBS microdata catalogue)

Content	Name of dataset	Source
Eligibility for nursing home care and home care	INDICAWBZTAB2009 (V1) INDICAWBZTAB2010 (V1) INDICAWBZTAB2011 (V1) INDICAWBZTAB2012 (V1) INDICAWBZTAB2013 (V1) INDICAWBZTAB2014 (V1)	CIZ
Use of nursing home care	ZORGMVTAB2010 (V1) ZORGMVTAB2011 (V2) ZORGMVTAB2012 (V1) ZORGMVTAB2013 (V1) ZORGMVTAB2014 (V1) GEBWLZTAB2015 (V1)	CAK
Death	GBAPOVERLIJDENTAB2018 (V1)	Death records
Date of birth and gender	GBAPERSOONTAB2019 (V1)	Population registers
Health care spending	ZVWZORGKOSTEN2009 (v1) ZVWZORGKOSTEN2010 (v1) ZVWZORGKOSTEN2011 (v1) ZVWZORGKOSTEN2012 (v1) ZVWZORGKOSTEN2013 (v1) ZVWZORGKOSTEN2014 (v1)	Vektis
Address	GBAADRESOBJECTBUS2019 (v1)	Population registers
	VSLGWB2019TAB03 (v1)	Population registers
Household income & tax household composition	Integraal Huishoudens Inkomen IHI 2008 (V3) IHI 2009 (V3) IHI 2010 (V3) IHI 2011 (V2) IHI 2012 (V2)	Tax Office and CBS
Individual income	Integraal Persoonlijk Inkomen IPI 2008 (V3) IPI 2009 (V2) IPI 2010 (V3) IPI 2011 (V2) IPI 2012 (V2)	Tax Office and CBS
Household wealth	VEHTAB2008 (V1) VEHTAB2009 (V1) VEHTAB2010 (V1) VEHTAB2011 (V1) VEHTAB2012 (V1)	Tax Office and CBS
Linkage parent-child	KINDEROUDERTAB2018 (v1)	Population registers
Co-payments		CAK

A.2 Income and wealth definitions

Income and wealth datasets

In our analysis, we use household income and household wealth data. In order to link each individual to her or his relevant income and wealth information, we rely on a conversion table that links together individual and household unique identifiers (cf. *supra*).

For the computation of co-payments levied in year Y, CAK uses information on income from two years before, Y-2. In addition, the amount of box 3-wealth included in the computation of co-payments is wealth as of January, 1st of year Y-2.

The wealth dataset 'VEHTAB' of year Y provides wealth as of December, 31st of year Y-1. When interested in (the co-payments levied in) year Y, we therefore link VEHTAB of year Y-2, which provides wealth as of December, 31st of year Y-3, assuming it proxies wealth as of January, 1st of year Y-2.

The income datasets come from the income data series 'IHI'. IHI data of year Y provide the income earned in year Y, and the tax household composition reported that same year. Therefore, when interested in the co-payments paid in year Y, we use IHI from Y-2.

Income and wealth variables

In progress!

B The co-payment schedule on nursing home care in the Netherlands: additional information

B.1 When are the high-rate and the low-rate regimes applicable?

The high-rate is the default regime for cost-sharing on nursing home care. However, there are a number of situations in which the low-rate applies.

The low-rate co-payment is charged under the following conditions: (i) the nursing home resident is married (or assimilated) to a partner still residing in the community; (ii) the resident financially supports her or his children, has an entitlement to child benefits or has children who receive student support; (iii) the first 6 months of the first permanent admission to a nursing home; (iv) for a temporary admission (if the resident has a partner, then the partner should be in the community or have a temporary admission too); (v) the resident receives home care in-kind or LTC vouchers instead of entering a nursing home. In addition, for partners who are both in a residential care setting, the high-rate co-payment is charged to one of them but waved for the second one.

B.2 Schedule for the high-rate co-payment

In case of of the high rate, the maximum annual co-payment is equal to the 'contribution income'.

'Contribution income' is based on taxable income, which itself consists of household earning plus income derived from financial assets and real estate, excluding the net value of the own house (henceforth: wealth).[36]

To calculate income derived out of wealth, the Tax Office uses a flat-rate approach: a fixed percentage of the stock of wealth is added to the annual household income. This share is equal to 4%. A part of wealth is excluded from this calculation (€21,000 for singles in 2012) so as to eliminate wealth taxation of those with very low assets. To protect elderly with low pension incomes, people having reached the statutory retirement age (roughly speaking, those 65 or older) may benefit from an additional income-dependent exemption on the wealth taken into account in the computation of taxable income. This exemption could add up to an additional €20,000 for singles with a low income.

Then, to derive the 'contribution income' for the high-rate co-payment, the

[36] 'Box 3: sparen en beleggen' in Dutch.

health insurance premiums paid and an allowance for pocket money and clothing (*Zak- en kleedgeld*, around €3,000 per year for singles) are deducted from the taxable income.

Finally, 25% of the remaining income is exempted. The monthly high-rate co-payment is then simply equal to 1/12 of the 'contribution income'.

B.3 Schedule for the low-rate co-payment

The low-rate co-payment is equal to 12.5% of a 'contribution income' defined roughly as the contribution income for the high rate co-payment, although the definition for the former allows for a more limited set of exemptions and rebates. For individuals who have opted for LTC vouchers instead of in-kind nursing home care, the low-rate co-payment is further reduced by a fixed rebate.

Individuals with an income below a certain threshold are exempt from co-payments if they fall under the low-rate regime. Furthermore, the co-payment charged every month cannot exceed a certain amount. The co-payment cap under the low-rate regime is substantially lower than the cap under the high-rate regime (€820 against €2,250 in 2014).

With the 2013 reform, in the low-rate regime the co-payment became, in rough terms, equal to: $(1/12) \times 12.5\% \times$ (taxable income + 8% of wealth).

B.4 Co-payment minimums and caps over the study period

The values for the minimum and maximum co-payments, as well as the values of the various rebates, are indexed each calendar year. Table V displays the minimum and maximum values of the monthly co-payments for years 2011 to 2014.

B.5 Quality of the co-payment simulations

In progress!

Table V: Minimum and maximum values for co-payments (2011 to 2014)

	2011	2012	2013	2014
High-rate copayment				
Minimum	0	0	0	0
Evolution relative to previous year	+0%	+0%	+0%	+0%
Maximum	2,097.40	2,136.40	2,189.20	2,248.60
Evolution relative to previous year	+0.8%	+1.9%	+2.5%	+2.7%
Low-rate copayment				
Minimum	145.60	148.20	152.00	156.00
Evolution relative to previous year	+0.8%	+1.8%	+2.6%	+2.6%
Maximum	764.40	778.60	797.80	819.40
Evolution relative to previous year	+0.8%	+1.9%	+2.5%	+2.7%

NOTES: The values of co-payments are expressed in current euros per month.

C Robustness checks and additional results

C.1 Baseline DiD estimates: full tables of results

Table VI: Difference-in-differences regression results: Any nursing home use (1/2).

Outcome:	Any nursing home use.							
Regressor:		Treatment dummy			Treatment intensity (co-payment change)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.109***	0.0821***	0.0831***	0.0811***	0.110***	0.0829***	0.0864***	0.0842***
	(21.29)	(16.46)	(16.60)	(16.16)	(22.97)	(17.65)	(18.36)	(17.84)
Treated	-0.0174***	-0.00144	0.000206	0.000122	-0.0153***	-0.000551	0.00457	0.00418
	(-3.96)	(-0.33)	(0.05)	(0.03)	(-4.58)	(-0.17)	(1.29)	(1.18)
Post × Treated	-0.00459	-0.00699	-0.00661	-0.00703				
	(-0.78)	(-1.23)	(-1.16)	(-1.23)				
Post × Treated ×Δ					-0.00222**	-0.00228***	-0.00384***	-0.00379***
					(-3.15)	(-3.34)	(-5.58)	(-5.51)
Woman		-0.0132***	-0.0194***	-0.0172***		-0.0131***	-0.0194***	-0.0173***
		(-4.01)	(-5.87)	(-5.24)		(-3.98)	(-5.88)	(-5.24)
Age category: 66-74 y.o.		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Age category: 75-79 y.o.		-0.00153	0.000264	0.00212		-0.00151	0.000469	0.00230
		(-0.25)	(0.04)	(0.35)		(-0.25)	(0.08)	(0.38)
Age category: 80-84 y.o.		-0.0101	-0.00604	-0.00293		-0.00999	-0.00566	-0.00257
		(-1.85)	(-1.11)	(-0.54)		(-1.83)	(-1.04)	(-0.47)
Age category: 85-89 y.o.		0.00124	0.00702	0.0117*		0.00137	0.00756	0.0122*
		(0.23)	(1.32)	(2.19)		(0.26)	(1.42)	(2.29)
Age category: 90-94 y.o.		0.0153**	0.0220***	0.0282***		0.0155**	0.0225***	0.0287***
		(2.76)	(3.96)	(5.02)		(2.78)	(4.06)	(5.11)
Age category: 95+ y.o.		0.0296***	0.0361***	0.0445***		0.0297***	0.0366***	0.0450***
		(4.29)	(5.25)	(6.42)		(4.31)	(5.33)	(6.49)
Children: 0		0.0219***	0.0232***	0.0213***		0.0225***	0.0243***	0.0223***
		(4.24)	(4.51)	(4.14)		(4.36)	(4.72)	(4.34)
Children: 1		0.0145**	0.0134**	0.0124**		0.0145**	0.0134**	0.0125**
		(3.27)	(3.04)	(2.82)		(3.28)	(3.05)	(2.83)
Children: 2		0.0134***	0.0143***	0.0135***		0.0133***	0.0143***	0.0135***
		(3.77)	(4.05)	(3.81)		(3.77)	(4.06)	(3.82)
Children: 3		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Has a daughter		-0.00834*	-0.00925*	-0.00863*		-0.00832*	-0.00924*	-0.00863*
		(-2.12)	(-2.35)	(-2.20)		(-2.11)	(-2.35)	(-2.20)
Homeowner		-0.0647***	-0.0426***	-0.0427***		-0.0642***	-0.0412***	-0.0413***
		(-18.72)	(-11.90)	(-11.93)		(-18.53)	(-11.47)	(-11.50)
Care needs (ZZP 4)		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Care needs (ZZP 5)		0.152***	0.152***	0.157***		0.152***	0.152***	0.157***
		(34.88)	(34.91)	(36.08)		(34.90)	(34.92)	(36.09)
Care needs (ZZP 6)		0.158***	0.159***	0.159***		0.158***	0.159***	0.159***
		(42.03)	(42.30)	(42.29)		(42.03)	(42.31)	(42.30)
Care needs (ZZP 7)		0.236***	0.233***	0.233***		0.237***	0.233***	0.233***
		(36.15)	(35.79)	(35.88)		(36.21)	(35.86)	(35.94)
Care needs (ZZP 8)		0.134***	0.135***	0.136***		0.134***	0.135***	0.136***
		(11.45)	(11.49)	(11.73)		(11.47)	(11.51)	(11.75)

NOTES: Table continues on the following page.

Table VII: Difference-in-differences regression results: Any nursing home use (2/2).

Outcome:	Any nursing home use.							
Regressor:		Treatment dummy			Treatment intensity (co-payment change)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cont'ed from previous page								
Psychogeriatric condition		-0.0960***	-0.0938***	-0.0911***		-0.0960***	-0.0938***	-0.0910***
		(-18.02)	(-17.66)	(-17.10)		(-18.02)	(-17.66)	(-17.09)
Both psychogeriatric and somatic condition		-0.00855*	-0.00878*	-0.00631		-0.00863*	-0.00890*	-0.00643
		(-2.11)	(-2.18)	(-1.56)		(-2.13)	(-2.21)	(-1.59)
Death in the civil year		-0.00228	-0.00284	-0.00359		-0.00223	-0.00278	-0.00354
		(-0.67)	(-0.84)	(-1.06)		(-0.66)	(-0.82)	(-1.04)
Spending on GP care				-0.0000155**				-0.0000151*
				(-2.58)				(-2.52)
Spending on pharmacy				-0.000000326				-0.000000337
				(-0.43)				(-0.44)
Spending on auxiliary care				0.00000174				0.00000171
				(1.03)				(1.02)
Total spending on medical care				0.000000678***				0.000000677***
				(7.10)				(7.09)
Value of eligibility for home care				-				-
				0.000000685***				0.000000684***
				(-13.02)				(-13.00)
Constant	0.744***	0.602***	0.641***	0.639***	0.744***	0.601***	0.642***	0.640***
	(175.20)	(40.79)	(40.83)	(40.52)	(179.22)	(40.81)	(40.93)	(40.63)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies for LTC purchasing region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies for vintile of available income	No	No	Yes	Yes	No	No	Yes	Yes
N	82972	82972	82972	82972	82972	82972	82972	82972
R^2	0.011	0.081	0.087	0.090	0.011	0.082	0.087	0.090

NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from 2010-2014, sample of 66+ single first-time nursing home eligible. The outcome is defined as any nursing home use (admission) in the 8 months following the day of first eligibility. For Specifications (5) to (8), the regressor of interest is an interaction between the dummies for being treated and becoming eligible for nurisng home care after the reform 2013 and the magnitude of the change in co-payments induced by the reform (Δ). Specifications (3), (4), (7) and (8) include dummies for the 'vintile' of available income, i.e. the position of the individual in the distribution of available income in the study population, from vintile 1 (5% lowest income) to vintile 20 (5% highest income). Specifications (4) and (8) include spending on medical care under the Health Insurance Act and the monetary value of home care the individual was eligible for in the previous calendar year.

Table VIII: Difference-in-differences regression results: Time spent in a nursing home.

Outcome:	Time spent in a nursing home (in days).							
Regressor:		Treatment dummy			Treatment intensity (co-payment change)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	29.64***	25.11***	26.69***	25.94***	29.48***	25.04***	27.09***	26.27***
	(24.64)	(22.30)	(23.58)	(22.88)	(26.32)	(23.79)	(25.61)	(24.78)
Treated	-3.705***	-1.830	0.0518	0.0817	-3.927***	-1.953*	0.546	0.481
	(-3.69)	(-1.92)	(0.05)	(0.08)	(-4.94)	(-2.57)	(0.67)	(0.59)
Post × Treated	-1.658	-1.271	-2.237	-2.359				
	(-1.16)	(-0.96)	(-1.68)	(-1.78)				
Post × Treated ×Δ					-0.297	-0.263	-0.807***	-0.789***
					(-1.70)	(-1.63)	(-4.94)	(-4.85)
Woman		-2.777***	-4.323***	-3.691***		-2.765***	-4.323***	-3.693***
		(-3.60)	(-5.58)	(-4.77)		(-3.58)	(-5.58)	(-4.77)
Age category: 66-74 y.o.		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Age category: 74-79 y.o.		0.316	1.164	1.868		0.314	1.197	1.896
		(0.22)	(0.81)	(1.30)		(0.22)	(0.83)	(1.32)
Age category: 80-84 y.o.		1.187	2.785*	3.946**		1.191	2.854*	4.008**
		(0.91)	(2.14)	(3.02)		(0.91)	(2.19)	(3.07)
Age category: 85-89 y.o.		5.329***	7.339***	9.010***		5.336***	7.436***	9.098***
		(4.17)	(5.75)	(7.01)		(4.17)	(5.82)	(7.08)
Age category: 90-94 y.o.		12.43***	14.49***	16.62***		12.43***	14.58***	16.71***
		(9.25)	(10.79)	(12.26)		(9.25)	(10.87)	(12.33)
Age category: 95+ y.o.		22.08***	23.87***	26.69***		22.08***	23.97***	26.79***
		(12.93)	(14.04)	(15.60)		(12.93)	(14.10)	(15.65)
Children: no		9.160***	9.086***	8.486***		9.230***	9.308***	8.704***
		(7.52)	(7.48)	(7.01)		(7.57)	(7.66)	(7.18)
Children: 1		3.265**	3.014**	2.765**		3.272**	3.026**	2.777**
		(3.16)	(2.93)	(2.69)		(3.17)	(2.94)	(2.70)
Children: 2		2.365**	2.747***	2.526**		2.363**	2.752***	2.531**
		(2.88)	(3.35)	(3.09)		(2.87)	(3.36)	(3.10)
Children: 3+		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Any daughter		-1.396	-1.597	-1.437		-1.394	-1.595	-1.435
		(-1.51)	(-1.73)	(-1.56)		(-1.51)	(-1.73)	(-1.56)
Homeowner		-18.39***	-13.75***	-13.72***		-18.33***	-13.44***	-13.42***
		(-23.86)	(-17.02)	(-17.02)		(-23.74)	(-16.60)	(-16.60)
Care needs (ZZP): 4		*Ref.*	*Ref.*	*Ref.*		*Ref.*	*Ref.*	*Ref.*
Care needs (ZZP): 5		42.36***	41.91***	43.56***		42.36***	41.92***	43.56***
		(45.61)	(45.25)	(46.96)		(45.62)	(45.26)	(46.97)
Care needs (ZZP): 6		26.13***	26.14***	25.99***		26.13***	26.14***	25.99***
		(27.22)	(27.36)	(27.14)		(27.22)	(27.36)	(27.15)
Care needs (ZZP): 7		77.17***	75.07***	74.77***		77.20***	75.12***	74.81***
		(43.64)	(42.78)	(42.82)		(43.66)	(42.84)	(42.88)
Care needs (ZZP): 8		29.35***	28.69***	28.35***		29.38***	28.75***	28.41***
		(10.34)	(10.16)	(10.07)		(10.35)	(10.19)	(10.10)
1.condition_2		-11.45***	-10.92***	-9.772***		-11.45***	-10.91***	-9.760***
		(-9.84)	(-9.42)	(-8.41)		(-9.84)	(-9.41)	(-8.40)

NOTES: Table continues on the following page.

Table IX: Difference-in-differences regression results: Time spent in a nursing home (2/2).

Outcome:	Time spent in a nursing home (in days).							
Regressor:		Treatment dummy			Treatment intensity (co-payment change)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cont'ed from previous page								
Psychogeriatric condition		0	0	0		0	0	0
		(.)	(.)	(.)		(.)	(.)	(.)
Both psychogeriatric and somatic condition		-4.991***	-4.937***	-4.211***		-5.000***	-4.962***	-4.237***
		(-5.56)	(-5.52)	(-4.72)		(-5.57)	(-5.55)	(-4.75)
Dies the year		-83.88***	-83.98***	-84.32***		-83.88***	-83.97***	-84.31***
		(-115.27)	(-115.65)	(-116.00)		(-115.26)	(-115.64)	(-115.99)
Spending on GP care				-0.00552***				-0.00546***
				(-4.15)				(-4.11)
Spending on pharmacy				-0.000212				-0.000214
				(-1.24)				(-1.25)
Spending on auxiliary care				0.00164***				0.00164***
				(4.25)				(4.23)
Total medical care spending				0.000250***				0.000250***
				(9.94)				(9.94)
Value of eligibility for home care				-0.000216***				-0.000216***
				(-18.34)				(-18.31)
Constant	104.2***	74.34***	94.93***	93.76***	104.3***	74.33***	95.23***	94.07***
	(112.80)	(24.15)	(28.19)	(27.75)	(115.59)	(24.18)	(28.32)	(27.87)
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies for LTC purchasing region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies for vintile of available income	No	No	Yes	Yes	No	No	Yes	Yes
Observations	82972	82972	82972	82972	82972	82972	82972	82972
R^2	0.012	0.158	0.165	0.170	0.012	0.158	0.165	0.170

NOTES: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from 2010-2014, sample of 66+ single first-time nursing home eligible. The outcome is defined as the number of days spent in a nursing home in the 8 months following the day of first eligibility. For Specifications (5) to (8), the regressor of interest is an interaction between the dummies for being treated and becoming eligible for nurisng home care after the reform 2013 and the magnitude of the change in co-payments induced by the reform (Δ). Specifications (3), (4), (7) and (8) include dummies for the 'vintile' of available income, i.e. the position of the individual in the distribution of available income in the study population, from vintile 1 (5% lowest income) to vintile 20 (5% highest income). Specifications (4) and (8) include spending on medical care under the Health Insurance Act and the monetary value of home care the individual was eligible for in the previous calendar year.

C.2 Empirical specification with relative price change

In the baseline analysis, we have estimated the price sensitivity of nursing home care use using a DiD approach and positing a linear effect of the price change in absolute terms (cf. Equation (10).

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

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

γ_3 captures the effect of a one percent change in the price of nursing home stays on the percentage-**point** change in the probability.

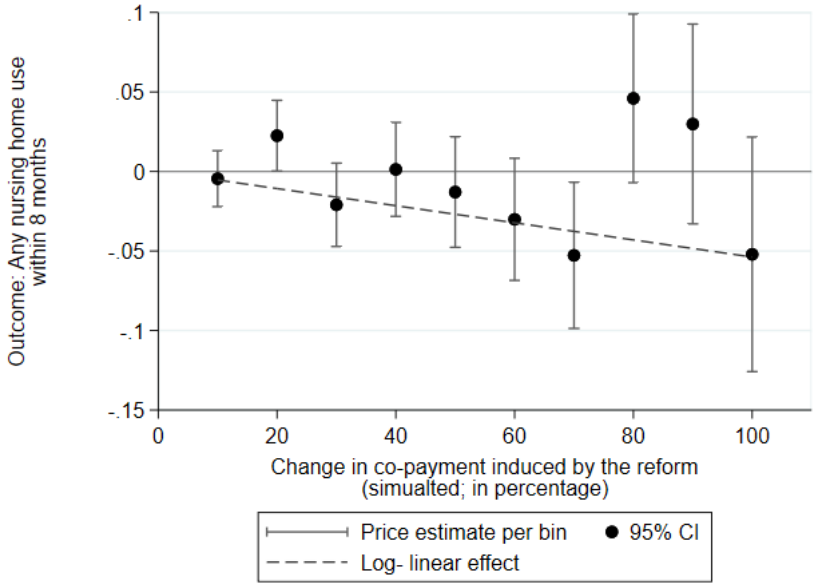
In case of number of days spent in the nursing home, we estimate:

$$\ln(Duration_i) = \gamma_0 + \gamma_1 Post_t + \gamma_2 Treated_i + \gamma_3 Post_t \times (\ln(p_i^{post}) - \ln(p_i^{pre})) + X_i'\theta + \mu_i \quad (12)$$

γ_3 now captures the effect of a one percent change in price on the percentage change in the number of care days used. As such, γ_3 can be interpreted as the price elasticity of nursing home duration.

Figure 11 shows the estimate of γ_3 in Equation (11).

Figure 11: Price sensitivity by treatment intensity: difference-in-differences estimates across bins and linear effect of co-payment change.



SAMPLE: Individuals 66+, singles, who became eligible for nursing home care for the first time between 2010 and 2014 (N=82,972).
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 8 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 co-payment on nursing home care induced by the reform. The first bin from the right groups individuals subject to a positive increase in co-payment up to 10%. The second bin groups individual subject to an increase higher than 10% and up to 20% etc.

D Tightening of eligibility conditions for residential elderly care and potential differential upcoding

D.1 Profiles of nursing home residents

In progress!

Officially, the profiles have remained the same throughout the study period.

Table X: Profiles of care packages (ZZP).

ZZP	Description	Recommended hours of:		
		Nursing care	Personal care	Guidance
4	Institutional living with intensive guidance and comprehensive personal care			
5	Protected living with intensive care for dementia			
6	Protected living with intensive personal care and nursing care			
7	Protected living with very intensive care, with an emphasis on guidance			
8	Protected living with very intensive care, with an emphasis on nursing care			

D.2 Distribution of new indications for residential elderly care, by income and wealth

Tables Tables XI to XV show that the share of elderly who were eligible for each of the four types of institutional care changed over time during the study period. This is because during the study period there were fewer and fewer approved applications for eligibility for assisted-living facilities (care packages 1-3). Care packages were officially phased out in 2013, care package 3 in 2014 but as the data show, fewer and fewer new applicants were eligible for care packages 1-2 in the years prior to these reforms and hence the share of eligibility decisions for packages 4-8 (nursing home care) and package 10 (hospice care) went up. The five panels show that the same trend occurred for all income groups.

Notes: All figures reported in Tables XI to XV are percentages. Care packages 1-3 are for assisted-living facilities, 4-8 for nursing homes, and 10 for hospice care. 65+ population only. Applications for care package 9 (post-acute care) not included.

Table XI: Panel A: income below 10000 euro (n = 17002)

Care package	2009	2010	2011	2012	2013	2014
1	7.9	3.7	2.7	3.7	0.0	0.0
2	19.3	17.7	14.9	11.9	0.3	0.1
3	11.1	17.0	20.0	22.9	20.2	6.2
4	15.0	15.2	18.2	16.9	17.9	15.9
5	24.7	24.3	24.2	24.6	27.8	39.0
6	12.2	11.7	9.8	10.0	20.6	23.1
7	2.8	2.7	1.9	3.0	3.5	3.7
8	1.0	1.0	0.9	0.7	1.6	1.6
10	6.2	6.7	7.5	6.4	8.1	10.5
Total	100%	100%	100%	100%	100%	100%

Table XII: Panel B: income between 10000 and 15000 (n = 112902)

Care package	2009	2010	2011	2012	2013	2014
1	7.9	3.7	2.7	3.7	0.0	0.0
2	19.3	17.7	14.9	11.9	0.3	0.1
3	11.1	17.0	20.0	22.9	20.2	6.2
4	15.0	15.2	18.2	16.9	17.9	15.9
5	24.7	24.3	24.2	24.6	27.8	39.0
6	12.2	11.7	9.8	10.0	20.6	23.1
7	2.8	2.7	1.9	3.0	3.5	3.7
8	1.0	1.0	0.9	0.7	1.6	1.6
10	6.2	6.7	7.5	6.4	8.1	10.5
Total	100%	100%	100%	100%	100%	100%

Table XIII: Panel C: income between 15000 and 20000 (n = 107064)

Care package	2009	2010	2011	2012	2013	2014
1	6.7	3.1	1.6	2.6	0.0	0.0
2	24.1	22.4	16.4	10.4	0.1	0.1
3	14.0	22.1	28.4	31.0	26.7	8.4
4	17.6	17.1	19.8	18.8	18.7	20.7
5	18.9	16.6	17.7	19.7	23.6	35.4
6	10.1	10.8	8.3	9.7	20.2	22.5
7	1.8	1.1	1.0	1.5	2.1	2.1
8	0.6	0.5	0.5	0.5	0.7	0.8
10	6.4	6.4	6.3	5.9	8.0	10.2
Total	100%	100%	100%	100%	100%	100%

Table XIV: Panel D: income between 20000 and 25000 (n= 37471)

Care package	2009	2010	2011	2012	2013	2014
1	6.5	2.8	2.0	2.5	0.0	0.0
2	22.7	22.3	15.7	9.7	0.1	0.1
3	13.6	21.0	27.8	30.8	25.6	8.6
4	17.0	16.2	18.9	17.9	17.4	19.6
5	19.3	18.2	19.4	21.3	25.6	35.6
6	11.4	11.2	8.1	9.4	20.4	21.7
7	2.3	1.4	1.2	1.8	2.2	2.3
8	0.7	0.6	0.5	0.7	1.0	1.1
10	6.6	6.5	6.4	5.9	7.5	11.1
Total	100%	100%	100%	100%	100%	100%

Table XV: Panel E: income of 25000 euro or higher (n= 34310)

Care package	2009	2010	2011	2012	2013	2014
1	5.0	2.5	1.8	2.7	0.0	0.0
2	20.9	21.5	16.2	9.8	0.1	0.0
3	13.9	20.3	26.8	30.1	25.8	8.2
4	17.2	16.7	18.0	18.8	17.3	20.4
5	22.4	20.3	21.1	22.1	27.3	37.7
6	11.4	10.2	8.4	8.3	18.9	20.5
7	2.2	1.4	1.3	1.5	2.1	2.3
8	0.9	0.5	0.6	0.5	0.8	1.0
10	6.3	6.7	5.8	6.3	7.9	9.9
Total	100%	100%	100%	100%	100%	100%