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Co-payments in long-term home care: do they affect the use of care?

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Co-payments in long-term home care: do they affect the use of care?*

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Abstract

We study the effect of a policy change in co-payments on the use of long-term home care in The Netherlands. The change increased co-payments for persons with considerable household financial assets. For identification we apply a difference-in-difference analysis with matched treatment and control groups. We find a significant effect at the extensive margin: persons affected by the policy change are less likely to take up care. The magnitude of the response increases over time, corresponding with an average annual price elasticity of demand of -0.14 and a price elasticity of -0.26 for episodes of care one year after the change. We do not find a significant effect at the intensive margin: persons who take up care do not use less hours of care.

Keywords: long-term care, home care, co-payment, difference-in-difference, matching, elasticity

JEL classification: I12, I13, C23, D12, H51

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1 Introduction

In many countries an aging population is leading to an increasing demand for long-term home care. A recent projection from the EU (Economic Policy Committee-European Commission (2015)) suggests that the number of dependent people receiving publicly financed home care will grow substantially between 2013 and 2060: the EU average growth is projected to be between 69% and 143%, depending on the scenario used. In the EU, The Netherlands stands out as one of the countries with high current spending on long-term care with a substantial projected growth of users of home care. This will cause considerable pressure on the government budget.

To reduce the financial burden, all countries with publicly provided home care have implemented some form of user cost sharing (see Colombo et al. (2011)). In the Netherlands this takes the form of a co-payment, the amount of which is dependent on a persons income, financial assets, age and household composition. Thus, the price the consumer pays for home care is equal to the co-payment. One reason for having co-payments is to share the costs of care between the user and the public payer. However, there might also be a behavioral reaction to co-payments as they may alter the demand for home care. Co-payments may increase the efficiency of demand by ameliorating overuse, however they may also exaggerate underuse (see e.g. Baicker et al. (2015)).

Although there is a large literature on co-payments in health insurance, most of this literature focuses on curative care (see e.g. the RAND experiment, Newhouse (1993), the Oregon experiment, Baicker et al. (2013), and McGuire (2011)). There are only a few papers that study demand responses to changes in co-payments in long-term home care. There are several reasons why co-payments in home care may differ from curative care. First, curative care often concerns single treatments that occur in a short period of time, while long-term home care is provided regularly, such as every week some hours of personal or nursing care. Users of home care might therefore make more informed decisions as they are regularly confronted with care. Also on the payment side they may be better informed as they have to regularly pay a co-payment amount. A change in copayments, as we investigate in this paper, may therefore be more visible to users. Second, the type of care differs. Home care users may be more accustomed to a certain level of care, and immediately notice a lack of care as this impairs their ability to perform ordinary tasks that pertain to Activities of Daily Living (ADL). Third, substitution effects may play a larger role than in curative care. Users might have the possibility to consider outside options, such as informal care or privately organized home care.

We estimate the effect of co-payments on the use of home care, using an exogenous policy change in The Netherlands that was implemented in January 2013. The policy change increased co-payments linearly in financial assets for persons with assets above a threshold, resulting in an average increase of co-payments of about 42%. Persons with financial assets below the threshold were not affected by the policy change. We use data of 26 four weeks periods in 2012 and 2013 and apply a difference-in-difference analysis with matched treatment and control groups. We find a significant effect at the extensive margin: persons affected by the shock are less likely to take up care. The magnitude of

the response increased over time. We find no effect for the first episodes in 2013 but in the last episodes of 2013 persons affected by the shock took up about 11% less care compared to persons not affected by the shock. The average effect over 2013 is 5.7%. These effects correspond with a price elasticity of demand of -0.14, averaged over all episodes in 2013, and a price elasticity of -0.26 for the last episodes in 2013. Price elasticities are lower for persons who are more heavily affected by the policy change.

Our paper adds to the literature in several ways. First, we study the reaction to an exogenous change in co-payments. Most studies on user cost sharing in home care only consider the effect of eligibility for or participation in a publicly financed system (Coughlin et al. (1992), Ettner (1994), Fontaine (2012), Pezzin et al. (1996), Rapp et al. (2011)) or they only consider the average subsidy size at a regional level (Stabile et al. (2006), Arrighi et al. (2015)). Our panel data set allows us to study effects at the personal level, as we have individual data on the need for care in hours, the use of care and the size of the co-payment for all Dutch users of home care.

Second, because our data covers 13 four-week periods before the policy change and 13 four-week periods after the change, we are able to estimate a difference-in-difference model with individual level fixed effects. Here we exploit the fact that some persons are more heavily affected by the policy change than others and that some persons are not affected by the change at all. The only other study we could find on co-payments in home care that analyzes the effect of the size of the co-payment, Roquebert and Tenand (2017), instead relies on local price differences in France to estimate a price-elasticity of demand. Moreover, the 26 periods allows us to study short and longer term effects of the policy change.

Third, our data allows us to study both the uptake of home care (the extensive margin) and the use of home care given uptake (the intensive margin). We find an effect on the extensive but not the intensive margin, which resembles findings of the RAND experiment (Newhouse (1993)). In the RAND experiment, a higher co-payment reduced the number of episodes of care (seeking care or not) but not the care within an episode. However, our study contrasts with Roquebert and Tenand (2017), who study only the intensive margin and report a significant effect. One possible explanation might be the difference in co-payment system between France and the Netherlands. In the Netherlands, users of home care pay a fixed amount per hour of care, up to a co-payment cap. After a person hits the co-payment cap, any additional care use is free of charge.¹ In France, users of home care also pay a (subsidized) hourly fee up to a maximum, but if they want to use more care, they will have to pay the market price on the additional hours of care.

Fourth, as we have detailed information on 135.000 individuals, we can add several interactions in our model. We show that the effect on the extensive margin increases over time which suggests that persons may learn over time. Also, the effect on the extensive margin is stronger for persons who experience a larger shock in co-payments.

¹There is a limit to the 'free' care, as every person gets a needs assessment which states to how much care (s)he is entitled. In practice, however, the entitlement is almost never binding, see Bakx et al. (2016).

The effect on the uptake of care is stronger for persons in a multiple person household, which suggests that outside options, in the form of an informal caregiver, play a role.

In the remainder of this paper, we first describe the Dutch system of home care in 2012 and 2013. Section 3 presents the empirical strategy and section 4 gives a description of the data we use. In section 5 we present the main results of the paper and in section 6 we extend the analysis by including size of the co-payment shock in the model. Section 7 presents the results on different household types. In section 8 we discuss some alternative reactions to the policy change and section 8 concludes.

2 The Dutch system of home care in 2012-2013

The Netherlands has a system of long-term care that is separate from the curative care. For long-term care there is a mandatory public insurance scheme for the total population which covers the costs of both home care (personal care, nursing care and guidance) and intramural care (mainly nursing homes). The public insurance has an income-dependent co-payment. Separate from the long-term care insurance, the municipalities are responsible for house cleaning services for people with disabilities. Our research focuses on the home care under the compulsory long-term care insurance, but as the co-payments for house cleaning are interwoven with the co-payments in long-term care we have to take the use of house cleaning services into account as well.

A person with a need for home care obtains an assessment by the independent Dutch assessment office CIZ. Based on the ADL limitations of the client, CIZ decides which, and how much, care the client is entitled to. After obtaining an assessment the client can choose between several competing care providers that are directly paid by a governmental institution.² The use of publicly paid care cannot exceed the needs assessment. A person who is in need of house cleaning services has to contact her municipality. The municipality decides whether the person is entitled to house cleaning services and determines the amount needed.

The co-payment for home care in 2012 was determined as follows. A year is divided in 13 periods of four weeks. For each period the total use of care in hours, including the use of house cleaning services, is computed at the household level. The co-payment is $\in 13.40$ per hour of care used up to a maximum (the co-payment cap). The $\in 13.40$ per hour is the same for everyone, and amounts to about 18% to 25% of the true cost of an hour of home care.

The co-payment cap is household-specific. For each period, the cap on co-payments is 1.15% of the gross yearly household income minus an exemption which is dependent on household composition and age. For households with a low income, there is a floor in the co-payment cap. The computation of the co-payment cap is based on a two-year lagged income, hence the 2012 cap is based on the 2010 income and the 2013 cap is based on the 2011 income.

 $^{^{2}}$ A client can also opt for a cash benefit and buy home care at the private market. About 7% of the home care entitled clients chooses this option. We exclude recipients of a cash benefit from our analysis as we do not have sufficient data on them.

Household type	age	co-payment cap
-	< 18	0
single	\geq 18 and < 65	$\max \left[{ { \in } 18;1.15\% } \text{ of income minus } { { \in } 246 } \right]$
single	≥ 65	$\max\left[\in 18; 1.15\% \text{ of income minus} \in 167 \right]$
multiple	Youngest partner ≥ 18 and < 65	$\max\left[{\textcircled{\in}25.80}; 1.15\% \text{ of income minus }{\textcircled{\in}301}\right]$
$\operatorname{multiple}$	both ≥ 65	$\max\left[{ \in } 25.80 ; 1.15\% \text{ of income minus } { \in } 232 ight]$

Table 1: Computation of co-payment cap (per four week period) in 2012

In 2012, the relevant income measure is defined as all income from work, benefits and pensions plus 4% of the taxable financial assets of the household (measured on January 1 2010). The taxable financial assets are all assets in bank accounts, stocks and bonds minus an exemption of $\in 21,139$ per person. Important for our research is that the percentage of taxable financial assets in the formula changes in 2013 from 4% to 12%.

Table 1 gives an overview of the computation of the cap in 2012. Take for example a man older than 65 who lives alone. Suppose in 2010 he had a gross yearly income from pensions and benefits of $\leq 30,000$ and $\leq 20,000$ in taxable financial assets. The co-payment is therefore based on an income of $\leq 30,000 + 0.04 \times \leq 20,000 = \leq 30,800$. In each care period, his co-payment cap is the maximum of ≤ 18 and 1.15% of $\leq 30,800$ minus an exemption of $\leq 167 = \leq 187.20$. Hence, the person has to pay ≤ 13.40 per hour up to a maximum of ≤ 187.20 . If he would use two hours of care per week, the total bill over a four-week period would be $8 \times \leq 13.40 = \leq 107.20$. If he would use four hours of care per week, his care use would exceed his maximum co-payment and he would get a four-weekly bill of ≤ 187.20 . Note that if the person would have had a very low income in 2010, e.g. $\leq 15,000$ with no taxable financial assets, the co-payment cap would be ≤ 18 . In that case, the person would only have to pay the first 1.3 hours of care use in a four-week period.

In 2013 the structure of the co-payment system was the same as in 2012, except that the percentage of 4% of the taxable financial assets changed to 12%. The change took effect in January 2013³ and implies that for all persons with taxable financial assets the co-payment cap increased. ⁴

To see the effect of the change in the way financial assets were included in the copayment calculations, take once again the single elderly person with gross yearly income from pensions and benefits of $\in 30,000$ and $\in 20,000$ in taxable financial assets. The copayment would now be based on an income of $\in 30,000 + 0.12 \times \in 20,000 = \in 32,400$. Ignoring the inflation correction, the co-payment cap becomes the maximum of $\in 18$ and

 $^{^{3}}$ The official proposal for change was filed on March 16 2012 by the lower house and was validated by the senate on October 23 2012.

⁴Apart from this change, the amounts in table 1 were corrected for inflation. This is a minor change which (with constant income) affected every care user in a similar way and increased the cap by $\in 2$ to $\in 4$.

1.15% of $\in 32,400$ minus $\in 167 = \in 205.60$. Hence, the cap increases by $\in 18.40$.

Note that the increase is linear in the amount of taxable financial assets; persons with large assets were more strongly affected by the change. Also, note that only persons with a relatively large use of care noticed the change. In our example, if the person would use two hours of care per week, the co-payment would still be ≤ 107.20 . Because his use of care is below the co-payment cap, an increase in the cap does not change the billed amount. However, if the person would use four hours of care per week, the co-payment would increase to ≤ 205.60 , and the person would directly notice the change.

3 Empirical strategy

3.1 Affected and non-affected groups

To assess the effect of co-payments on the use of home care, we exploit the exogenous change in the co-payment cap in 2013. In the years 2012 and 2013 this was the only change in the long-term home care system, hence we can use a difference-in-difference analysis to isolate the effect.

In our main analysis, we use three mutually exclusive groups, which are defined in such a way that over time a person cannot switch between groups:

- 1. Persons with no taxable financial assets in 2011 (control group).
- 2. Persons with taxable financial assets in 2011 whose average use of care in 2012⁵ is above their co-payment cap in 2012 (directly affected group).
- 3. Persons with taxable financial assets in 2011 whose average use of care in 2012 is below their co-payment cap in 2012 (indirectly affected group).

The first group is the control group which is not at all affected by the change. This group is the baseline against which all effects are measured. The second group is our main treatment group. As the persons in this group have taxable financial assets, their maximum co-payment increases. Moreover, since their use of care in 2012 (expressed in euros by multiplying the hours of care by 13.40) is above the co-payment cap in 2012, the increase in co-payment cap will, with constant use of care, translate in a higher payable amount. This might lead them to lower the uptake or use of care.

The third group is affected by the policy change in the sense that their co-payment cap increases.⁶ With a constant use of care, this will however not lead to a higher payable amount, as the use of care (expressed in euros) is below the 2012 co-payment cap. Hence, we do not expect that those persons lower their use of care in response to the policy change. It is however possible that this group will be reluctant to increase their use of care.

⁵The average use of care of person i in 2012 is based on the periods in 2012 in which person i had an assessed need for care. If the assessment is e.g. obtained in July 2012, the average is based on the use of care in July-December 2012. The average use of care includes the use of house cleaning services.

⁶The co-payment cap is mentioned on the four-weekly bill, so clients can observe the changes if they compare bills from 2012 and 2013.

Figure 1: Affected and non-affected groups: location relative to the copayment cap for a given household composition, age and income.



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Figure 1 displays the three different groups for a given level of income from work, benefits and pensions and a given age and household composition. The red line indicates the co-payment cap. Financial assets below a certain threshold are not taxable, hence the co-payment cap is constant for low financial assets. All persons with financial assets below the threshold belong to the control group, independent of their care use. Above the threshold, the co-payment cap increases linearly in financial assets. In 2013 the cap increases stronger in financial assets than in 2012. The directly and indirectly affected groups are defined by their care use in 2012 relative to their co-payment cap in 2012.

If the income from work, benefits and pensions increases, the layout of the figure does not change, but the red line shifts upwards. Similarly, a shift in age group or household composition will shift the red line. Therefore, the affected groups are not only defined by financial assets and care use. For example, the single elderly person with taxable financial assets of 20,000 euros, a care use of 16 hours and a gross yearly income of 30,000 euros would fall in the directly affected group. If his income would be 35,000 euros he would fall in the indirectly affected group.

3.2 Main econometric specification

We analyze both the effect on uptake of care (extensive margin) and the effect on use of care in hours (intensive margin). Denote by i all persons with an assessed need for home care. Let t denote the four-week periods, starting with the first four-week period in 2012 and ending with the last period in 2013. We first estimate the following basic fixed effects logit model to analyze the extensive margin:

$$Y_{it} = 1 \text{ if } U_{it}^* \ge 0 \tag{1}$$

$$Y_{it} = 0 \text{ if } U_{it}^* < 0$$
 (2)

$$U_{it}^* = \alpha_i + \delta_t + \beta_2 X_{i2} Z_{2013} + \beta_3 X_{i3} Z_{2013} + \epsilon_{it}$$
(3)

with U_{it}^* a latent unobserved variable, $\epsilon_{it} \sim \text{Type II Extreme Value and } Y_{it}$ a dummy indicating the uptake of care. α_i and δ_t are fixed individual and time effects, X_{ik} is a dummy that indicates whether client *i* belongs to group *k* and Z_{2013} is a dummy for year 2013.

Next we estimate the following linear fixed effects panel model for person-period combinations in which the use of care in hours is non-zero to analyze the intensive margin:

$$H_{it} = \alpha_i + \delta_t + \beta_2 X_{i2} Z_{2013} + \beta_3 X_{i3} Z_{2013} + \epsilon_{it} \tag{4}$$

where H_{it} is the amount of care client *i* used in period *t* as percentage of the assessed need of care of client *i*.⁷ House cleaning is not included in H_{it} as the assessed need

⁷We also estimated the model using the amount of care in hours as dependent variable instead of H_{it} . This does not significantly change the results.

for house cleaning is unknown.⁸ Note that the logit and linear model are estimated independently.⁹

As a second step in our analysis, we break down the main effects $\beta_k X_{ik} Z_{2013}$ into separate period effects. That is, we replace Z_{2013} by period dummies. This allows us to investigate whether there is a transitional period in which persons start adjusting their use of care. Even though the change in co-payment cap was announced several months in advance and was widely known, it might be that some care users only realized the extent of the change after receiving the bill over the first period of 2013.

One concern with the specification above is that the control and treatment groups might not be comparable. Persons in the directly and indirectly affected groups have higher financial assets, and financial assets might also be correlated with other characteristics, e.g. income, age and household composition. Moreover, the directly affected group by construction consists of persons with a relatively high use of care, while the indirectly affected group by construction consists of persons with a relatively low use of care. The individual level fixed effects do control for individual differences in the *level* of care use, but they do not correct for potential differences in development over time.

To alleviate this concern, we re-estimate the models using propensity score matching. For every person in the directly affected group and for every person in the indirectly affected group, we find a matching person in the control group, based on available personal characteristics (e.g. income and age) and the care use in 2012. Persons in the control group that do not match with someone in the affected groups are removed. The resulting control groups closely resemble the treatment groups in terms of observables and care use in 2012. We re-estimate the models based on these control groups. Note that in this procedure we construct two control groups, one for the directly affected group and one for the indirectly affected group. For each combination of treatment and control group we estimate a separate model, which leaves out X_{i3} for the directly affected group and X_{i2} for the indirectly affected group.

The models above only include a dummy variable for group membership, and hence only estimate an average effect for each treatment group. However, the size of the shock in co-payments varies between individuals as it depends on the amount of financial assets. We expect that the reaction of clients is stronger when the shock in co-payments is larger. Also, the variation in shock sizes allows us to estimate elasticities. We explore this in detail in a separate section.

Finally, we also estimate the models on a subsample of single person households and a subsample of multiple person households to determine whether the effects are different for persons who have a potential informal caregiver (like a spouse) in the household.

⁸When we re-estimate the models using only persons who do not use house cleaning services, the results become slightly stronger than for the complete sample.

⁹It would be possible to combine the two models in a Tobit I model, but this requires the assumption that both models have identical parameters. Our results on the separate models show that this assumption is not valid on our data. A Tobit type II model circumvents the requirement of identical parameters, but assumes that there is a variable which is expected to have a significant effect in one of the models and no effect in the other model. We have not been able to find such a variable. For those reasons, we decided to estimate the logit and panel model independently from each other.

Table 2: Variables in the data set.

Variable	Description
Person id	Identifier of the person
Period	Identifier of the four-week period
Need for personal care	Assessed need for personal care ^a in minutes per period
Need for nursing care	Assessed need for nursing care ^b in minutes per period
Need for individual guidance	Assessed need for individual guidance ^c in minutes per period
Need for group guidance	Assessed need for group guidance ^d in half-days per period
Use of personal care	Minutes of personal care used in the period
Use of nursing care	Minutes of nursing care used in the period
Use of individual guidance	Minutes of individual guidance used in the period
Use of group guidance	Number of half-days of group guidance used in the period
Use of home cleaning	Minutes of home cleaning services used in the period
Income	Yearly gross household income
Financial assets	Household financial assets
Household composition	Dummy indicating a single-person or multiple-person household
Age	Age of the person
Partner age	Age of the partner of the person (if multiple-person household)

^a Personal care is e.g. help with washing and dressing.

^b Nursing care is e.g. help with injections and dressing wounds.

^c Individual guidance is e.g. help with administration and planning.

^d Group guidance is e.g. daytime activities (group wise) in a care center. The activities are meant to stimulate the client and alleviate the burden of informal caregivers.

4 Data and descriptives

We use individual level data provided by Statistics Netherlands. The data includes all persons who have a need for home care as assessed by the CIZ. We use all four-week care periods in 2012 and 2013, hence the data includes 26 periods in total.¹⁰ Table 2 gives an overview of the variables in the dataset.

From the raw data we delete those persons whose need for care ends before January 1 2013 or starts after December 31 2012, as those persons cannot be classified as affected or non-affected by the policy change. Also, we remove several groups from our data which might bias the results. Appendix A gives an overview of the data cleaning process.

The resulting data is a panel data set with 135,603 persons¹¹ and 26 periods. The total number of observations is 2,675,984. The panel is unbalanced; in total 24.1% of

 $^{^{10}}$ Statistics Netherlands also provided data over 2014, but as there was another major policy change in June 2014, we did not use this data.

¹¹The co-payment is based on the total care use of a household. In 96% of the households in the data only one person is in need of care and hence the co-payment is based on the care use of that person. In the cases where multiple persons in a household are in need of care, we sum the care use of those persons, and keep only one observation. In the remainder of the paper we will use the term 'person' or 'client' even though this might refer to the summed care use of multiple persons in a household.

the person-period combinations is missing. The reason is that persons only enter the panel when they get an assessment of need for home care, and exit the panel when their assessment expires without renewal¹², when they move to a nursing home or when they pass away. Also, there are several persons who have missing data in one or multiple intermediate periods. This happens e.g. when their needs assessment changes or the person moves.

Of the variables in table 2, the household composition does not change over time. Income and financial assets are measured at a yearly level, but the correlation over the years is quite strong. The need and use of care are measured at the level of four-week periods that correspond to the billing periods. However, the need for care does not change much over time.

Table 3 provides descriptive statistics for the three different groups. Most persons belong to the control group. The smallest group is the indirectly affected group. However, this group is still large enough for econometric inference.

In general, the persons in our data are elderly, and many live alone. The use of care is low relative to the assessed need for care¹³. This is in line with Bakx et al. (2016), who discuss this gap between assessed need and actual use in more detail.

Based on income, financial assets, household composition and age, we reconstructed the co-payment caps in 2012 and 2013. Although we do not have the co-payment cap available in our data, we can make an accurate reconstruction. The governmental institution (CAK) that bills the co-payments uses the formulas mentioned in section 2 and Statistics Netherlands provided us with the same data on income, financial assets and use of care as CAK uses in their computation. Note that indeed the groups with financial assets experienced a sizable increase in the co-payment cap. For the directly affected group, the co-payment cap on average increases from $\in 105$ to $\in 159$ per four-week period and for the indirectly affected group the cap increases from $\in 273$ to $\in 374$ per four-week period.

Also, for each observation in 2012 we determined the billed co-payment by taking the minimum of the care use and the co-payment cap. As expected, in the directly affected group the average billed co-payment of $\in 103$ is close to the 2012 average cap of $\in 105$. In the indirectly affected group, with average care use below the co-payment cap, the average billed co-payment of $\in 84$ is indeed much lower than the cap of $\in 273$. In the control group, the billed co-payment of $\in 47$ is slightly below the cap of $\in 62$.

In the row 'billed co-payment 2013' we report the average billed co-payment based on the 2013 co-payment cap, but with a care use equal to the average care use in 2012. Hence, this row does not include any behavioral effects yet. For the control group, the billed co-payment increases slightly from $\in 47$ to $\in 50$ due to an inflation correction. The indirectly affected group experiences a similar small increase from $\in 84$ to $\in 87$. For the directly affected group, the billed co-payment however increases sharply, from $\in 103$ to

 $^{^{12}}$ If the health status of the client has not changed, renewal of the assessment is a simple formality which is always granted.

¹³The need for care is assessed in a range, e.g. 2 to 4 hours of personal care per week. The need for care reported in table 3 is based on the maximum of the range. If we use the minimum, the average use of care is still below the assessed need for care.

	Group 1:	Group 2:	Group 3:
	Control	Directly affected	Indirectly affected
	group	group	group
Persons	96,822	24,418	$14,\!363$
Total observations	1,921,300	492,775	$261,\!909$
Age in years	80.1	82.9	81.6
Single-person household	74.7%	87.0%	65.8%
Mean income	€21,632	€23,902	€38,893
Mean taxable financial assets	€0	€58,088	€107,833
Median taxable financial assets	€0	€25,676	€53,650
Co-payment cap 2012	€62	€105	€273
Co-payment cap 2013	€65	€159	€374
Billed co-payment 2012	€47	€103	€84
Billed co-payment 2013	€50	€146	€87
Uptake of cleaning services ^a	65%	73%	18%
Use of cleaning services ^b	807	811	586
Need for personal care ^c	1534	1709	1122
Need for nursing ^c	890	855	773
Need for individual guidance ^c	891	928	761
Need for group guidance ^d	20.0	20.5	17.9
Uptake of care 2012 ^e	81%	92%	57%
Uptake of care 2013 ^e	80%	90%	59%
Use of care $2012^{\rm f}$	57%	61%	43%
Use of care $2013^{\rm f}$	58%	61%	47%

 Table 3: Descriptive statistics

^a Percentage of observations with a non-zero use of home cleaning services.

^b Use of home cleaning services in minutes per four-week period, conditional on using home cleaning services.

^c Upper bound of need for care in minutes per four-week period, conditional on having an assessed need for this specific type of care.

^d Upper bound of need for care in half-days per four-week period, conditional on having an assessed need for group guidance.

^e Percentage of observations having a non-zero use of care, conditional on having an assessed need for care.

 $^{\rm f}\,$ Use of care (excluding home cleaning services) as percentage of the need for care, conditional on using a positive amount of care.



Figure 2: Development of average uptake of care over time

€146.

As expected, table 3 shows several differences between the groups. By definition, the directly affected group consists of persons with taxable financial assets and an average care use (in 2012) above the co-payment cap. This is reflected in a relatively high uptake and use of care. Also, the need for care in general is higher than for the control group. The mean income is slightly higher as well, as is the percentage of single-person households and the age in years. The indirectly affected group, defined by having taxable financial assets and a care use below the co-payment cap, also differs from the control group. As expected, this group has a lower uptake and use of care, and a lower need for care. Moreover, the persons in this group have high taxable financial assets and a high income. This would give them a high co-payment cap, increasing the probability of having a care use below the cap.

Comparing 2012 and 2013, note that the uptake of care decreases slightly for the directly affected group and increases slightly for the indirectly affected group. The use of care does not seem to change much between 2012 and 2013, except for the indirectly affected group, which shows an increase in use of care.

Figures 2 and 3 show the average uptake of care and use of care per four-week period. The uptake is defined as the percentage of persons with a need for care who indeed have a nonzero use of care. The use of care is measured as percentage of the need that is used, conditional on a nonzero use of care.

First consider the uptake of care. In 2012, all three groups follow roughly the same pattern, except that the levels of uptake differ per group. Starting in the first period of 2013 (period 14), the uptake of care decreases slightly for the directly affected group,



Figure 3: Development of average use of care over time

while increasing for the control group and indirectly affected group. The increase in uptake by the control group can be explained by the construction of the data set. Recall that only persons are included who have a need for care in (part of) 2012 and who continue to have a need for care in (part of) 2013. This means that in 2012 over time persons who obtain a need for care assessment are entering the dataset, but persons who pass away or move to a nursing home during 2012 are not included in the data at all. As persons who recently obtained a need for care assessment tend to have a lower uptake of care than persons who are about to move to a nursing home or pass away, the uptake of care decreases slightly in 2012. Starting from January 2013, persons with a newly obtained assessment are not included in the data anymore. As the remaining persons in the data grow older and frailer over time, the uptake of care gradually increases.

As the directly and indirectly affected groups also consist of persons who already had a need for care in 2012, one might expect a similar increase in uptake, but instead there is a slight decrease in the directly affected group. This suggests that those persons indeed react to the shock in co-payments. In the indirectly affected group, we observe a relatively strong increase in uptake. This is not as we expected, since this group experienced a large increase in the co-payment cap. One might expect this group to be reluctant in taking up care as the resulting bill is quite a bit higher than in 2012.

When looking at the use of care (figure 3), we see some seasonal effects. In periods 12 and 26, which are December 2012 and 2013, there is a marked drop in the use of care. This probably is a supply-effect as many care-workers have some time off during the Christmas holidays. There also seems to be a slightly higher use of care in the winter months and during the summer holidays (when informal caregivers might be away).

The control group and the directly affected group show similar seasonal effects and also seem to show a similar development over time. This suggests that persons who experienced an increase in the co-payment cap do not respond by decreasing their use of care. The use of care increases quite strongly for the indirectly affected group. Again, this is an unexpected pattern. On close inspection, the indirectly affected group seems to show a slightly stronger increase in the use of care in the final periods of 2012 than the control group. This casts some doubts on the appropriateness of the control group and is one of the reasons we will resort to a matching procedure.

5 Regression results

5.1 Basic fixed effects models

We first estimate the models using the full control group. The results of this can be found in appendix B. Underlying this analysis is a common trend assumption. That is, we assume that in 2012 the treatment and control group develop in the same way, and if the policy change would not have taken place, the groups would also show the same development in 2013. We perform two different tests of this assumption.

First, we add pre-treatment time and treatment group interaction dummies to the model. That is, for each treatment group we add dummies for periods 1 till 12 in 2012 (period 13 is used as baseline). If the common trend assumption would hold, those dummies should be insignificant.

Second, we perform a placebo test where we pretend that the policy change would not have taken place in January 2013, but in July 2012. Recall that we have data on all 13 periods in 2012. Now we define the average use of care in 2012 as the average use in periods 1 till 6. Next, we redefine group membership based on the new definition of average use of care and we follow these groups over time. As the real policy change takes effect in period 14, we would expect that the effects in periods 7 till 13 are nonsignificant, and that the affected groups only differ from the control group from period 14 onwards.

Detailed results of both test can be found in appendix B. In general, we find several indications that the common trend assumption might be violated when using the full control group. Hence, we will continue with a matching procedure to control for observable differences between the groups.

5.2 Matching

For each of the treatment groups, we construct a matching control group, using propensity score matching with replacement. Hence, we construct two control groups; one matching the directly affected group and one matching the indirectly affected group.

Persons are matched based on their characteristics in the period in which they first enter the panel. In subsequent periods, we keep the same match. As characteristics we include age, household type, income, need for care, use of house cleaning and the 2012 average use of care. In appendices C and D we present the results of the matching regression and descriptive statistics of the resulting matched samples. As we use matching



Figure 4: Development of uptake and use of care over time, directly affected group and matched control group

with replacement, some of the persons in the control group are matched to multiple persons in the treatment group. In the subsequent analysis, those persons are weighted according to the number of matches they represent.

Figures 4 and 5 show the development over time of the uptake of care and use of care when the treatment groups are compared to their matched control group. The scale of the figures is kept equal to the scale in figures 2 and 3. From the figures, it is clear that after matching the directly and indirectly affected groups are more similar to their control group, compared to the figures without matching.

For the directly affected group (figure 4) the uptake of care decreases in 2013, while for the control group the uptake stays roughly constant. The use of care does not seem to differ much between the directly affected group and control group. For the indirectly affected group (figure 5), note that the matched control group shows a marked increase in both the uptake of care and the use of care. Hence, the increase that the indirectly affected group shows is most likely related to the personal characteristics that are also used in the matching procedure. From figure 5 we see that both the uptake and use of care of the indirectly affected group increases at slower pace than the control group from period 14 onwards.

The first row in table 4 presents the results of the fixed effects logit and linear models in equations (1) - (4) for the directly affected group and its matching control group. The logit regression indicates that in the directly affected group the probability of takeup decreases with on average 5.7 percentage points in 2013 compared to the control group. This is a significant effect. The use of care given takeup does not significantly differ between the directly affected group and its control group. The second row in table 4 gives the results for the indirectly affected group. Again, the logit specification gives a negative significant average effect, albeit limited in size (1.8 percentage points). The linear specification also gives a negative effect, which is significant. However, given that the co-payment cap on average increases with 37%, from \in 273 to \in 374, the estimated effect of -0.95% lower use of care is small in economic terms.

Figure 6 shows the development of the marginal effects over time for the logit regres-



(a) uptake of care

(b) use of care

Figure 5: Development of uptake and use of care over time, indirectly affected group and matched control group

	logit specification / extensive margin		logit specification / linear specification extensive margin intensive		ification / margin
	marginal				
	$\operatorname{coefficient}$	t-value	effect	coefficient	t-value
Directly affected group	-0.23	-8.98	-5.7%	0.05	0.68
Indirectly affected group	-0.07	-3.15	-1.8%	-0.95	-6.70

Table 4: Regression results with matched control groups.

sion, using the matched control groups. Similarly, figure 7 shows the development of the coefficients from the linear specification, using matched control groups. For the directly affected group, the marginal effect becomes negative and significant after period 17. This pattern can be explained by the way the co-payment is billed. The bill is usually sent a few weeks after the care period closed, hence until period 16 the affected care-users do not yet exactly know the extent of the change. On top of that, it might take time to make other care arrangements. For the directly affected group the coefficient of the linear specification revolves around zero and is insignificant.

For the indirectly affected group, the logit regression gives a slightly decreasing marginal effect, which becomes significant around period 11. The coefficient of the linear specification is non-significant for the first three periods, decreases after that, but seems to increase again at the end of the year. To interpret those results, recall that the coefficients measure the development of the treatment group *relative to the control group*. In the case of the indirectly affected group, the matched control group shows an upward trend in uptake and use of care. The negative coefficients imply that the growth in the use of care is less strong for the indirectly affected group than for its matched control group.



Figure 6: Marginal effects from the logit specification (extensive margin) using matched control groups, with 99% confidence interval.



Figure 7: Coefficients of the linear specification (intensive margin) using matched control groups, with 99% confidence interval.



Figure 8: First test of the common trend assumption using matched control groups. Marginal effects from the logit specification (extensive margin), with 99% confidence interval.



Figure 9: First test of the common trend assumption using matched control groups. Coefficients of the linear specification (intensive margin), with 99% confidence interval.

5.3 Common trend assumption

We use the same two tests as before to test the common trend assumption. The results of the first test, adding dummies for periods 1 till 12, are presented in figures 8 and 9. In none of the figures the added dummies are significant, except for period 6 in the logarithmic specification for the indirectly affected group. As we are testing in total 48 (four times 12) coefficients, significance of a single coefficient falls within the type I error margin. Hence, we conclude from this test that the common trend assumption cannot be rejected.

The results of the second test are presented in figures 10 and 11. Recall that the second test pretends that the policy change took place at the start of period 7 and follows the groups from that point onwards. As can be seen in the figures, for both models and both groups the coefficients are nonsignificant for period 7 till 13 (the real period of the policy change). Hence, from this test we also conclude that the common trend assumption cannot be rejected.



Figure 10: Second test of the common trend assumption using matched control groups. Marginal effects from the logit specification (extensive margin), with 99% confidence interval.



Figure 11: Second test of the common trend assumption using matched control groups. Coefficients of the linear specification (intensive margin), with 99% confidence interval.

6 Price effects

In the previous section we estimated that the probability of using care decreases by 5.7 percentage points on average for the directly affected group. Given that the billed co-payment increases with 42% on average, this gives a rough estimate of the elasticity of -0.14. When instead we use the estimated effect in the final periods of 2013 (approximately 11 percentage points), the elasticity would amount to -0.26. Admittedly, these are rough back-of-the-envelope calculations. The data provides us with the exact increase in co-payments on an individual level, and as this increase varies widely between individuals we will analyze the price effect in more detail in this section.

We focus on calculating the price effects for the extensive margin of the directly affected group.¹⁴ In this context, we define an elasticity as the percentage point change in the probability of taking up care in response to a one percent increase in the maximum co-payment. Alternatively, we could analyze the response with respect to an increase in the billed co-payment. We decided to use the maximum co-payment as this is the variable that is directly influenced by the policy change. For the directly affected group the co-payment cap and the billed co-payment are strongly correlated (correlation of 0.91). Using the billed co-payment instead of the co-payment cap indeed does not notably change the results. In the remainder of this section, we refer to the change in maximum co-payment as the "price change".¹⁵

For the directly affected group, the distribution of the percentage change in copayment cap has a long tail to the right. The quartiles of the distribution are 7.5%, 29.2% and 90.3%. To get a first insight in how the response to the policy change depends on the price change, we divide the directly affected group in four quarters. For each quarter we re-estimate the logit model from the previous section. The results are presented in figure 12. The first quarter, with an increase in price below 7.5%, does not significantly respond to the price change. The second quarter, with an increase in price between 7.5% and 29.2%, does show a significant decrease in the probability of takeup of care. In the last two periods of 2013, the effect is approximately 10 percentage points. The third quarter, with an increase in price between 29.2% and 90.3% also significantly decreases their takeup. The effect is between 12 and 15 percentage points in the final periods of 2013. The last quarter, with a price increase of more than 90.3%, as well shows a significant decrease in takeup, this time between approximately 13 and 17 percentage

¹⁴We repeated the analysis for the intensive margin, but this did not give any significant results for the directly affected group, in accordance with the results in the previous section. We also repeated the analysis for the indirectly affected group, leading to mixed results. For both the intensive and the extensive margin, the analysis showed only a weak relationship between the size of the shock in co-payments and the use of care.

¹⁵Note that apart from the policy change, the co-payment cap also changed slightly because of an inflation correction and because in 2013 the co-payment cap is based on the 2011 income, while in 2012 the cap is based on the 2010 income. As all treatment and control groups are equally affected by this, it did not confound our results in the previous section. However, to calculate elasticities we need to relate the change in behavior of the treatment group as compared to the control group to the price shock that caused this change. Hence, in our calculations we only use the part of the price shock that is caused by the policy change.



Figure 12: Marginal effects for four subgroups with different price changes, with 99% confidence interval.

points in the final periods of 2013.

Note that the analysis above suggests a non-linear response to the price shock. The estimated effect differs quite strongly between the first and second group, while it seems to level off in the third and fourth group. This is especially salient as the price shock increases exponentially over the groups.

To get some insight in the elasticities, we use the logit model from the previous section, but add an interaction between the 2013 periods and the percentage shock in price. Hence, we replace equation (3) by

$$U_{it}^* = \alpha_i + \delta_t + \sum_{j=14}^{26} \beta_{1j} X_i Z_j + \sum_{j=14}^{26} \beta_{2j} X_i Z_j P_i + \epsilon_{it}$$
(5)

Here, X_i is a dummy indicating whether person *i* belongs to the treatment group, the Z_j are period dummies and P_i is the percentage change in price that person *i* experienced. The marginal effect of $X_i Z_j P_i$ is our estimate of the elasticity.¹⁶

¹⁶The effect that we calculate is an average marginal effect. The logit model assumes that the probability of using care follows a logistic distribution, where the effect of a one percentage point increase in P_i depends on the value of P_i itself. We also estimated a linear model, which does not suffer from this problem, and found somewhat smaller elasticities. The main conclusions about significance and nonlinearity of the effect are the same as in a logit model.



(a) Full model including constant effects (b) Model without constant effects

Figure 13: Estimates of the elasticity, with 99% confidence interval.

As the results above indicate a nonlinear response to the price shock, we also estimate a version of equation 5 where the change in price P_i is replaced by the log change in price $\ln P_i$. If the price effect indeed levels off for higher price changes, using the log change will give a better model fit. Note that in this model the marginal effect of β_{2j} cannot be directly interpreted as an elasticity.

Before estimating the model, we remove the 5% observations in the treatment group with the highest percentage shock in price. Those observations have an increase in price of at least 276.3% and have a disproportionate influence on the estimates.

The left panel in Figure 13 presents the estimated elasticity from equation (5). The estimated β_{2j} , and hence the elasticities, are not or only just significant. The constant effects for the treatment group, β_{1j} , are however highly significant (not reported here). This might indicate that there is no price effect, but it might also be the result of a non-linear price effect. Note that the significant β_{1j} imply that persons with a very small price change respond quite strongly to this change. When we estimate the model with β_{1j} fixed to zero, the estimated elasticities are significant in the final periods of 2013 (see the right panel in figure 13).

Next, we estimate the model replacing P_i by $\ln P_i$. The left panel in figure 14 presents the estimated marginal effects of β_{2j} . Now, the effects are significant in seven of the 13 periods, even though the term $\sum_{j=14}^{26} \beta_{1j} X_i Z_j$ is included in the model. The estimated β_{1j} are not significant. When β_{1j} is fixed to zero, the estimated marginal effects of β_{2j} are similar in size to the estimates when the term $\sum_{j=14}^{26} \beta_{1j} X_i Z_j$ is included, but the standard errors are much smaller. This gives a higher significance (see the right panel in figure 14).

The model using $\ln P_i$ (and including the constant effects) has a log-likelihood of -65,051.954 while the model based on P_i (and including the constant effects) has a log-likelihood of -65,071.441. Based on this, the model assuming a nonlinear relationship has a better fit and is to be preferred over the model based on P_i .

From the results above we conclude that there is evidence of a nonlinear price effect. Note that taking the log of the price change implies that the elasticity estimate becomes smaller when the price increase is larger. E.g., if we measure the elasticity by comparing



(a) Full model including constant effects

(b) Model without constant effects

Figure 14: Marginal effects of the log change in price, with 99% confidence interval.



Figure 15: Elasticity estimates from the model using $\ln P_{ij}$.

a price increase of 6% with a price increase of 5%, the resulting estimate will be larger than if we measure the elasticity by comparing a price increase of 40% with an increase of 39%. Figure 15 shows the resulting elasticity estimates when using the model that takes the log of the price changes.¹⁷

We conclude this section with a note of caution. Even though the elasticity estimates in figure 15 are based on a model which has a better fit than the model including P_i in a linear fashion, we did not explore other possible nonlinear models. Especially for small price changes the exact model specification might have a large influence on the elasticity estimates. It is outside the scope of this paper to investigate this in depth. For now, we refrain from making strong statements about the 'true' price elasticity, except that there is evidence that the elasticity is nonlinear in the price change.

	logit specification / extensive margin		linear specification / intensive margin		
			marginal		
	coefficient	t-value	effect	coefficient	t-value
Directly affected group	-0.21	-7.50	-5.2%	0.03	0.47
Indirectly affected group	-0.11	-3.75	-2.8%	-1.58	-9.27
	(a) Single person household		ousehold		
	logit specification /		1		
	logit s	pecificati	ion / 🛛 🛛	linear spec	ification /
	logit s exter	pecificati 1sive mar	ion / rgin	linear spec intensive	ification / margin
	logit s exter	pecificati isive mar	ion / rgin marginal	linear spec intensive	ification / margin
	logit s exter	pecificati nsive mar t-value	ion / rgin marginal effect	linear spec intensive coefficient	ification / margin t-value
Directly affected group	logit s exter coefficient -0.48	pecificati nsive man t-value -7.28	ion / rgin marginal effect -11.7%	linear spec intensive coefficient 0.40	ification / margin t-value 1.98
Directly affected group Indirectly affected group	logit s exter coefficient -0.48 -0.08	pecificati nsive man <u>t-value</u> -7.28 -2.09	ion / ·gin marginal effect -11.7% -1.8%	linear spec intensive coefficient 0.40 0.23	ification / margin t-value 1.98 0.89

(b) Multiple person household

Table 5: Regression results (based on matched control groups) for a subsample of single person households and a subsample of multiple person households.

7 Results per household type

In this section, we explore whether the effects depend on the household composition. We hypothesize that multiple person households react stronger to the shock in co-payments as they might have a potential informal caregiver (e.g. a spouse) in the household. For the sake of brevity, we only consider the model with a single dummy for 2013.¹⁸ Moreover, we leave out potential price effects. Simple statistics show that single and multiple person households do not differ greatly in the amount of taxable financial assets.¹⁹

Table 5 shows the regression results for two subsamples: a sample with single person households and a sample with multiple person households. When we consider the directly affected group, it is clear that the effects in the logit specification (extensive margin) are much stronger for multiple person households. Single person households on average decrease their uptake of care by 5.2 percentage points compared to a control group of single person households, while multiple person households on average show a decrease of 11.7 percentage points compared to a control group of multiple person households. Hence, multiple person households are less willing to take up care in response to the policy shock than single person households.

For the indirectly affected group, the results on the logit specification are reversed: the

¹⁷To construct the figure, we calculated $-2.9 \times (\ln(P+1) - \ln(P))$ for several values of P. Here, P is the percentage price change, hence $(\ln(P+1) - \ln(P))$ is the change in $\ln P$ if P increases by one percentage point. The constant, -2.9, is the marginal effect of β_{2j} in period 26 in the model including constant effects.

¹⁸Adding period-dummies does not change the conclusions. Results are available on request.

¹⁹In the directly affected group, single person households on average experience an increase in the co-payment cap of $\in 68$. For multiple person households this is $\in 82$.

effect is slightly stronger in single person households. The difference between single and multiple person households is however small, and not significant. A possible explanation for this is that the group of indirectly affected persons already uses as much informal care as possible in 2012. This is suggested by the fact that they have a low use of care compared to their need for care in 2012. Also, table 3 shows that the indirectly affected group has a relatively low percentage of single person households. If indeed the indirectly affected group relies strongly on informal care in 2012, this would make it hard to increase the use of informal care in 2013, both for single and for multiple person households.

In the linear specification, most results are not significant, and very small in economic terms. Only the single person households in the indirectly affected group significantly decrease their use of care compared to the control group. With -1.58 percentage points, the effect is however limited.

8 Alternative responses to the policy change

The analysis has thus far concentrated on the effect of the policy change on the uptake and use of care. However, affected care users might respond in two other possible ways.

In the first place, care users might decide to apply for a cash handout instead of receiving care in kind. The co-payment system for the cash handouts differs slightly from the co-payments for care in kind, and might in some cases be more attractive. Moreover, clients can use the cash handouts to pay informal caregivers (e.g. a spouse or children).

In the analysis above we excluded persons with a cash handout, as the use of care of those persons is not centrally registered. Hence, persons who switch from care in kind to cash handouts during 2013 will most likely not affect our results on the use of care.

Moreover, we tracked the cash handouts over time, and found that fewer than 0.5% of the persons in our data switch from care in kind to a cash handout during 2012 and 2013. Also, the switching levels are not significantly different between the treatment and control groups. We conclude that it is very unlikely that persons switch to cash handouts as a response to the increase in co-payments.

Second, care users might respond by reducing their financial assets, e.g. by giving part of it to their children. The standard calculation of the co-payment cap uses the two-year lag of the financial assets, hence this reaction will not immediately have effect on the billed amount. However, if the income and financial assets of 2013 are much lower than the income and financial assets in 2011, a person might ask for a revision of the cap based on the 2013 data. In our analysis, we excluded all persons who are eligible for such a revision, so they will not affect the results.

In a separate analysis, we examined the development of financial assets over time from 2010 to 2014. Here, we compared the persons who were directly or indirectly affected by the co-payment increase (including the persons with a large shock in income or financial assets) with the group of all persons older than 65. Most of those persons do not have a need for care and hence were not affected by the policy change. We found all groups have decreasing financial assets from January 2013, probably due to the recession at that

time. In the groups that were directly or indirectly affected by the co-payment change, the decrease in financial assets is slightly larger than in the general group of persons older than 65. The total decrease in financial assets is however limited and hence it is unlikely that many care users transferred their financial assets to others in order to evade the increase in co-payments.

9 Discussion

We use a policy change in the Netherlands to study the response to an increase in copayments in long-term home care. For identification, we apply a difference-in-difference analysis with matched treatment and control groups. We find that the group that is directly affected by the increase in co-payments significantly decreases the uptake of care compared to the matching control group. The effect increases over time; it is insignificant in the first months after the policy change, but corresponds to an elasticity of -0.26 for episodes of care one year after the change. Averaged over the full year, the estimated effect corresponds to a price elasticity of -0.14. A group of care users who are only indirectly affected by the policy change also decrease their uptake of care, but to a lesser extent than the directly affected group. Moreover, we find that the effect increases in the size of the shock in co-payments and is also stronger for multiple person households.

We do not find an effect on the hours of care used. This resembles findings of the RAND experiment (Newhouse (1993)), that considered the effect of co-payments on the use of curative care. In the RAND experiment, a higher co-payment reduced the number of episodes of care (seeking care or not) but not the amount of care within an episode.

Our study is one of the few that examines the effect of co-payments on the use of home care. Most of the literature on co-payments studies curative care. There are several reasons why the effect of co-payments in home care might differ from curative care. First, users of home care are regularly confronted with a co-payment, which enables them to make more informed decisions. We indeed find a learning effect after the policy change; it does take several care episodes before care users respond significantly. Second, outside options might play a larger role in home care compared to curative care. We find this as well, as persons in multiple person households, with presumably better access to informal care, react stronger to the increase in co-payments. On the other hand, users of home care might be unwilling to reduce their care use as it directly affects their activities of daily living. However, according to our results this effect either does not exist, or is weaker than the previously mentioned effects.

We use unique and very detailed individual level data. Moreover, the policy change provides us with a sharp shock which enables a strong research design. However, our study also comes with some limitations. First, the persons affected by the policy change have sizable financial assets and in general belong to the more wealthy part of the population. We use matching to construct a similar control group. Still, the effect might be different if less wealthy users of home care would experience an increase in co-payments. On one hand, those persons might be more price sensitive. On the other hand, they might have less outside options than richer persons. Second, the policy change only affected the co-payment cap, and left the price per hour unchanged. A change in the hourly fee might have different effects. Third, the results are for an *increase* in co-payment. The response might not be symmetric if there would be a decrease in co-payments. Finally, we focused on persons who already have a need for care at the time of the policy change. New clients who obtain their need for care assessment in 2013 might respond in a different way to the increased co-payments, as they have no reference point in 2012.

In the future, more research on co-payments in home care is needed to address the limitations mentioned above. It would be interesting to see whether our results are robust to changes in the affected population, or changes in the price per hour instead of the co-payment cap. Another avenue for future research would be to study the persons who stop using care. Our data does not allow us to see what outside options they used, if any. For policy purpose, it is however important to understand whether the persons who avoid the public system manage to fulfill their need for care in a different way, or whether they go without any care.

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Appendices

A Construction of the final data set

Table 6 details the construction of our data set. Our initial data consists of all persons who have a need for home care as assessed by the CIZ. First, we remove persons who decided to get a cash handout instead of using care in kind. For persons who get a cash handout data on the use of care is not available, hence we cannot include them in our analysis.

Second, we remove persons who were younger than 60 on December 31 2011. Those persons often get home care because of a lifelong mental or physical disability, and their pattern of care use is slightly different from the care use of persons with age-related ADL's. As persons with a lifelong mental or physical disability in general have low income and financial assets, the majority falls in group 1 (not affected by the policy change). This might bias our analysis.

Third, we remove persons who have a partner with an assessed need for intramural care. In cases where one partner is institutionalized and the other partner uses home care, the household can under certain circumstances apply for a cancellation of the copayment for home care. We do not observe who applied for this, hence we remove all potential applicants.

Fourth, we remove periods during which a person has a change in personal circumstances and we remove persons whose household composition changes in 2012 or 2013. More specific, we remove period t of person i when person i gets a first-time needs assessment during period t, when person i gets a re-assessment of needs during period t or when person i moves during period t. A new needs assessment during a period complicates our calculation of the entitlement of care. Moreover, there might be a short period of adjustment when the care provider cannot immediately increase the amount of care delivered. When a person moves, it might be that he or she has to change the care provider, which might lead to a short interruption of care. Finally, when the household composition changes, the co-payment is re-calculated. For most persons, the co-payment is based on the income in 2010 (for the 2012 co-payment) or 2011 (for the 2013 co-payment). When the household composition changes, the most recent income is used. Also, a change in household composition might be related to a change in available informal care. For those reasons, we only keep the persons with a constant household composition in 2012 and 2013.

Fifth, our data contains the household income and financial assets for both partners. Theoretically, these household level variables should be equal for both household members. We remove those persons for whom the partner has a different reported household income or financial assets. Also, the use of house cleaning services is limited to one person in the household. We delete those persons who belong to a household where both partners have a reported use of house cleaning services.

Sixth, we remove persons who received an unlimited needs assessment. This is a special class for persons with severe limitations. As the needs assessment in hours is part

Step		Observations	Persons
	Initial data set	$8,\!451,\!371$	644,441
1.	Remove if cash handouts	$7,\!466,\!477$	$600,\!464$
2.	Remove if age below 60	$5,\!421,\!508$	$462,\!811$
3.	Remove if partner with intramural care	$5,\!346,\!437$	$457,\!452$
4.	Remove if shock in personal circumstances	$4,\!469,\!338$	$395,\!579$
5.	Remove if inconsistencies between partners	$4,\!462,\!106$	394,763
6.	Remove if unlimited needs	$4,\!415,\!178$	$388,\!526$
7.	Remove if missing income or taxable financial assets	$4,\!395,\!598$	$386,\!453$
8.	Remove outliers	$4,\!311,\!124$	$377,\!924$
9.	Remove persons with a large shock in income	$3,\!481,\!092$	$307,\!664$
10.	Remove persons with no needs in 2012	$3,\!128,\!228$	$228,\!018$
11.	Remove persons with no needs in 2013	$2,\!675,\!984$	$135,\!603$

Table 6: Construction of the final data set

of our explanatory variable, we cannot analyze those with a boundless entitlement.

Seventh, for some persons data on income or taxable financial assets is missing. As we need this information to compute the co-payment, we remove those persons. In step 8 we remove several outliers in the use of care and we winsorize the income and financial assets data by removing the top 1% incomes and financial assets.

In step 9, we remove persons who had a large decrease in income or taxable financial assets between 2010 and 2013. The co-payment in 2012 is based on the income and financial assets in 2010, but if a person can prove that his 2012 income (including 4% taxable financial assets) is at least \in 1.816 lower than in 2010, the person can apply for a re-calculation of the co-payment based on 2012 income. The same holds for the co-payment in 2013 which is based on 2011 income. We do not observe these recalculations and therefore remove everyone who might successfully apply for it. Moreover, note that persons might respond to the policy change by e.g. donating some of their financial assets to their children. Because we remove those persons, the effect we estimate is a pure response in use of care.

In the last two steps we remove the persons who got a first needs assessment in 2013 (and hence have no assessed need for care in 2012) and we remove the persons whose need for care ended before December 31 2012.

	logit specification / extensive margin		linear spec intensive	ification / margin	
			marginal		
	coefficient	t-value	effect	coefficient	t-value
Directly affected group	-0.26	-12.83	-6.5%	-0.14	-2.43
Indirectly affected group	0.21	11.56	5.1%	1.73	18.42

Table 7: Basic regression results

B Estimation results without matching

Table 7 presents the results of the fixed effects logit and linear models in equations (1) - (4). Only the effects of interest (β_2 and β_3) are reported.

The logit regression indicates that after the policy change the directly affected group has a significantly decreased probability of taking up care compared to the control group, while the indirectly affected group has a significantly increased probability of taking up care. More specific, for persons in the directly affected group, the probability of taking up care decreases by on average 6.5% in 2013 compared to the group with no financial assets. For persons in the indirectly affected group the probability of taking up care increases by 5.1 percentage points, compared to the control group. These results match the pattern that is visible in figure ??.

The linear specification shows that after the policy change the directly affected group has an average decrease of 0.14 percentage points in care use relative to the needs assessment. Although the sign of the coefficient is as expected, it is only significant at the 5% level, despite the large sample size. Moreover, the economic significance is very small, given that the billed co-payment on average increases with 42%, from \in 103 to \in 146. The indirectly affected group shows a significant increase of 1.73 percentage points in use of care. Again, the results match with figure ??.

Figure 16 displays the marginal effects and corresponding 99% confidence intervals when period dummies are used instead of year dummies in the logit specification. That is, for each of the 13 periods in 2013 a separate coefficient is estimated. The figure displays a decreasing time trend for the directly affected group. In the first three periods, there is no significant difference between this group and the control group. From period 17 onwards, the directly affected group shows a significantly lower uptake of care compared to the control group. This effect becomes stronger over time. This pattern can be explained by the way the co-payment is billed. The bill is usually sent a few weeks after the care period closed, hence until period 16 the affected care-users do not yet exactly know the extent of the change. On top of that, it might take time to make other care arrangements.

Panel (b) of figure 16 shows the results for the indirectly affected group. As in table 7, for this group the probability of take up of care increases relative to the control group. The effect is not significant in the first two periods, but after that becomes significantly positive. The quick positive response (there already is a significant and fairly strong increase in uptake in the third period of 2013) suggests that the increase in uptake is not



(a) directly affected group

(b) indirectly affected group

Figure 16: Marginal effects from the logit specification (extensive margin), with 99% confidence interval.



Figure 17: Coefficients of the linear specification (intensive margin), with 99% confidence interval.

related to the shock in co-payments, but instead is caused by other differences between the control group and the indirectly affected group.

As for the linear specification, the results are presented in figure 17. The directly affected group does not follow a clear pattern over time, and in most periods the coefficient is not significant. The indirectly affected group does have significant coefficients, which increase over time. As in table 7, the use of care increases after the policy change. It is striking that the coefficient of the first period in 2013 is already relatively large and strongly significant. There does not seem to be an adjustment period. As before, this suggests that other effects might play a role.

As a first test of the common trend assumption, we add pre-treatment time and treatment group interaction dummies to the model. Figure 18 gives the results for the logarithmic specification. For both the directly and the indirectly affected group the common trend assumption does not seem to hold. In the directly affected group, the logarithmic specification gives significantly negative coefficients in the first five periods of 2012. For the indirectly affected group, the first three periods of 2012 give a significantly positive coefficient.



Figure 18: First test of the common trend assumption. Coefficients of the logit specification (extensive margin), with 99% confidence interval.



Figure 19: First test of the common trend assumption. Coefficients of the linear specification (intensive margin), with 99% confidence interval.

In figure 19 we present the results for the linear specification. The directly affected group has significant coefficients in the first four periods of 2012, again suggesting a violation of the common trend assumption. For the indirectly affected group, there are no significant coefficients in 2012, hence the common trend assumption seems to hold. There is however still an unexplained jump in the coefficient in periods 14 and 15.

Second, we test the common trend assumption with a placebo test. In figure 20 we show the coefficients of the logarithmic specification. For the directly affected group, those become significant from period 10 onwards. For the indirectly affected group, the coefficients are significant from period 12 onwards. Hence, for both groups the test rejects the common trend assumption, as the groups already significantly differ from the control group in periods before the policy change takes place.

Figure 21 gives the results for the linear specification. In the directly affected group, the coefficients are not significant, except for period 22. Hence, in this case we cannot reject the common trend assumption. However, for the indirectly affected group, we again need to reject this assumption. There is a significant positive effect already in



Figure 20: Second test of the common trend assumption. Coefficients of the logit specification (extensive margin), with 99% confidence interval.



Figure 21: Second test of the common trend assumption. Coefficients of the linear specification (intensive margin), with 99% confidence interval.

period 7, and this effect only gets stronger over time.

Variable	coefficient	t-value
Age in years	0.037	35.56
Household type (1=single, 2=multi-person)	-1.345	-54.91
Household income	0.0000641	62.26
Need for personal care	-0.0000474	-4.45
Need for nursing	-0.000168	-9.58
Need for individual guidance	0.0000116	0.41
Need for group guidance	-0.0035094	-3.02
Use of house cleaning	-0.0000891	-4.33
Average 2012 use of care	0.0011554	21.94

Table 8: Logit regression on membership of control or directly affected group.

C Matched control group for the directly affected group

In this section we present statistics for the matched control group that is constructed for the directly affected group. That is, the control group that results after matching each person in the directly affected group with a corresponding person in the control group (with replacement).

Table 8 presents the results of a logit regression that relates the membership of either the control (0) or directly affected (1) group to several characteristics. The coefficients largely follow the statistics in table 3: the probability of belonging to the directly affected group increases in age, income and average use of care. Single person households have a larger probability of belonging to the directly affected group. Surprisingly, signs of the coefficients for the need for care and use of house cleaning differ from the statistics in table 3. This only occurs after controlling for the other background variables.

The regression in table 8 is used to construct a matching control group, by matching persons with similar values in the regression equation. This gives a control group of 18.947 persons, of which some have a double weight as the matching is performed with replacement. Table 9 gives some (weighted) descriptives of the matched control group. The matched control group is indeed similar to the directly affected group.

	Group 1:	Group 2:
	Matched control	Directly affected
	group	group
Persons	18,947	24,418
Total observations	384,306	492,775
Age in years	83.3	82.9
Single-person household	87.2%	87.0%
Mean income	€23,292	€23,902
Co-payment cap 2012	€89	€105
Co-payment cap 2013	€92	€159
Billed co-payment 2012	€64	€103
Billed co-payment 2013	€66	€146
Uptake of cleaning services ^a	71%	73%
Use of cleaning services ^b	844	811
Need for personal care ^c	1755	1709
Need for nursing ^c	884	855
Need for individual guidance ^c	907	928
Need for group guidance ^d	20.7	20.5
Uptake of care 2012 ^e	87%	92%
Uptake of care 2013^{e}	86%	90%
Use of care $2012^{\rm f}$	62%	61%
Use of care $2013^{\rm f}$	62%	61%

Table 9: Descriptive statistics directly affected group and its matched control group

^a Percentage of observations with a non-zero use of home cleaning services.

^b Use of home cleaning services in minutes per four-week period, conditional on using home cleaning services.

^d Upper bound of need for care in half-days per four-week period, conditional on having an assessed need for group guidance.

 $^{\rm e}\,$ Percentage of observations having a non-zero use of care, conditional on having an assessed need for care.

 $^{\rm f}$ Use of care (excluding home cleaning services) as percentage of the need for care, conditional on using a positive amount of care.

^c Upper bound of need for care in minutes per four-week period, conditional on having an assessed need for this specific type of care.

Variable	coefficient	t-value
Age in years	0.056	32.90
Household type (1=single, 2=multi-person)	-1.690	-53.76
Household income	0.0001406	110.08
Need for personal care	-0.0000024	-0.14
Need for nursing	-0.000041	-1.39
Need for individual guidance	0.0000656	1.10
Need for group guidance	0.0029095	1.26
Use of house cleaning	-0.0007216	-13.42
Average 2012 use of care	-0.0089913	-57.85

Table 10: Logit regression on membership of control or indirectly affected group.

D Matched control group for the indirectly affected group

In this section we present statistics for the matched control group that is constructed for the indirectly affected group. That is, the control group that results after matching each person in the indirectly affected group with a corresponding person in the control group (with replacement). Note that the resulting matched control group is different from the matched control group in Appendix C, as it is specifically constructed to match the indirectly affected group.

Table 10 presents the results of a logit regression that relates the membership of the control (0) or indirectly affected group (1) to several characteristics. The coefficients again are largely as expected: the membership of the indirectly affected group increases in age and income, and decreases in use of house cleaning and use of care. The need for care is not significant, which is surprising given that in the raw data the control group and indirectly affected group seem to differ quite a bit on the need for care. Apparently, correcting for the other characteristics also accounts for the differences in need for care. Also surprising is that the sign for the household type is opposite from the raw data: according to the regression the probability of belonging to the indirectly affected group increases for single-person households, after correcting for the other variables. The household composition is most likely positively related to the household income.

In table 11 some descriptive statistics are presented for the indirectly affected group and its matching control group. Note that the control group is only about half the size of the indirectly affected group; many persons in the control group are matched to multiple persons in the treatment group. The treatment group is a fairly specific group, with many multiple-person households and a high income. Apparently, there is only a limited amount of persons in the control group that matches these characteristics. After matching, the groups are indeed fairly similar on their (weighted) characteristics.

	Group 1:	Group 3:
	Control	Indirectly affected
	group	group
Persons	7,320	$14,\!363$
Total observations	$133,\!226$	$261,\!909$
Age in years	80.6	81.6
Single-person household	59.9%	65.8%
Mean income	€38,146	€38,893
Co-payment cap 2012	€236	€273
Co-payment cap 2013	€237	€374
Billed co-payment 2012	€54	€84
Billed co-payment 2013	€58	€87
Uptake of cleaning services ^a	11%	18%
Use of cleaning services ^b	561	586
Need for personal care ^c	1127	1122
Need for nursing ^c	769	773
Need for individual guidance ^c	773	761
Need for group guidance ^d	17.4	17.9
Uptake of care 2012^{e}	49%	57%
Uptake of care 2013^{e}	51%	59%
Use of care $2012^{\rm f}$	42%	43%
Use of care 2013 ^f	47%	47%

Table 11: Descriptive statistics indirectly affected group and its matched control group

 $^{\rm a}\,$ Percentage of observations with a non-zero use of home cleaning services.

^b Use of home cleaning services in minutes per four-week period, conditional on using home cleaning services.

^c Upper bound of need for care in minutes per four-week period, conditional on having an assessed need for this specific type of care.

^d Upper bound of need for care in half-days per four-week period, conditional on having an assessed need for group guidance.

^e Percentage of observations having a non-zero use of care, conditional on having an assessed need for care.

^f Use of care (excluding home cleaning services) as percentage of the need for care, conditional on using a positive amount of care.

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