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Discussion paper

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Understanding Society

Cost-Sharing Design Matters: A Comparison of the Rebate and Deductible in Healthcare[1]

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Abstract

Since 2006, the Dutch population has faced two different cost-sharing schemes in health insurance for curative care: a mandatory rebate of 255 euros in 2006 and 2007, and since 2008 a mandatory deductible. Using administrative data for the entire Dutch population, we compare the effect of both cost-sharing schemes on healthcare consumption between 2006 and 2013. We use a regression discontinuity design which exploits the fact that persons younger than eighteen years old neither face a rebate nor a deductible. Our fixed effect estimate shows that for individuals around the age of eighteen, a one euro increase of the deductible reduces healthcare expenditures 18 eurocents more than a euro increase of the rebate. These results demonstrate that differences in the design of a cost-sharing scheme can lead to substantial different effects on total healthcare expenditure.

Keywords: deductible, rebate, cost-sharing, healthcare consumption, regression discontinuity design, panel data

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1. Introduction

Health insurance reduces a person's risk of unexpected high healthcare expenditures (Finkelstein et al., 2017). For risk-averse persons, this risk reduction raises utility. Also, health insurance prevents healthcare costs from becoming so high that an uninsured individual is not able to afford treatment. However, insurance can also lead to moral hazard. In the health economics literature, moral hazard refers to an increased use of medical care driven by the reduction in price of care as a result of health insurance (Zweifel and Manning, 2000). In many countries, policymakers have introduced demand side cost-sharing to reduce such moral hazard (see e.g. Paris et al., 2010). A wide range of cost-sharing instruments exist, including deductibles, co-payments, co-insurance rates, two-tier systems, rebates, and shifted deductibles. In addition, policymakers have to decide on the amount of cost-sharing and which groups or treatments to target. All these instruments have in common that they shift (part of) healthcare expenditures to users in order to incentivize them to reduce their healthcare use. However, different instruments may lead to different responses.

This paper sheds light on the effects of two different demand-side cost-sharing instruments that were in place in the Dutch curative healthcare sector between 2006 and 2013. In 2006 and 2007, the Dutch population faced a mandatory no-claims rebate of 255 euros as part of the government mandated basic insurance package. A no-claims rebate, from here on referred to as a rebate, implies that a person who spends y euros on healthcare in a given year receives a $255 - y$ euro reward at the end of the year as long as this amount is positive, i.e., if he or she makes no or few claims. The rebate was introduced in 2005 by the Dutch government as a consumer-friendly cost-sharing instrument (Holland et al., 2009). Cost-sharing was, and still is, a politically-controversial issue in the Netherlands, especially cost-sharing for low-income individuals with poor health status. The rebate was initially preferred over a deductible because it was perceived as an end-of-year bonus for low healthcare consumption during that year, instead of a penalty for consuming healthcare. Also, deductibles were seen as a financial hurdle for someone in need of care.

In 2008, however, a new government replaced the rebate with a mandatory deductible of 150 euros. A person pays the first 150 euros of his or her healthcare consumption out-of-pocket, while costs above 150 euros are covered by health insurance. The reasoning for abolishing the rebate in 2008 was threefold: preliminary evaluations showed only modest effects of the rebate on reducing healthcare consumption, it was (now) perceived as unfair that chronically ill individuals would never receive the end-of-year bonus, and the government could lower the health insurance premium paid by consumers by replacing the rebate with a deductible (Goudriaan et al., 2007;

Holland et al., 2009).[1] The mandatory deductible has been raised annually since 2008, reaching 350 euros in 2013. Importantly, both cost-sharing mechanisms only applied to individuals of 18 years and older; younger individuals were fully insured (no cost-sharing).

The aim of this paper is to compare the effect of a rebate and a deductible on healthcare consumption. We use a regression-discontinuity design that exploits the fact that persons under 18 years do not face a rebate or a deductible. We compare people younger than 18 who do not face a deductible or rebate with people older than 18 who do face a deductible or rebate and use time- and individual-fixed effects, to control for factors affecting healthcare consumption other than the rebate or deductible. We view this as a quasi-experimental set-up. Potential selection effects are largely absent because basic insurance is mandatory in the Netherlands, and the basic-insurance package is set by the government. For our estimations we use administrative data that cover the entire Dutch population.

We find that the reduction of healthcare consumption due to the mandatory deductible is significantly larger than the reduction due to the mandatory rebate of comparable size. A one euro increase of the deductible reduces healthcare expenditures by 18 eurocents more than a one euro increase in the rebate. Furthermore, we find that persons who live in an area with a low average-household income do not respond to the rebate, but do respond strongly to the deductible. These findings may appear puzzling as in a standard economic framework a deductible and a rebate induce the same budget constraint. We discuss three possible explanations for our findings: prospect theory, discounting, and liquidity constraints (see Section 6).

Our paper contributes a comparison of the effects of the rebate and deductible to the health economics literature. As far as we are aware, there is no literature available that makes this specific comparison, despite the extant body of literature on various cost-sharing mechanisms.[2] Stockley (2016) compares the effect of two types of cost-sharing that differ from ours: the deductible and co-payments. She finds that people are substantially more responsive to co-payments than to deductibles. The RAND Experiment compared the effect of multiple co-insurance rates, which differ in size and upper limit (Newhouse, 1993). Spending of persons with a 95 percent coinsurance rate (basically a deductible plan) was 31 percent lower than those without any cost-sharing, corresponding to a price elasticity of approximately -0.2. These reductions in healthcare consumption did not result in a worse health status, except for persons who already had a poor health status and low income. Recently, Brot-Goldberg et al. (2017)

[1] To finance the end-of-year rebate, each citizen has to pay a higher premium at the beginning of the year. For the deductible, the premium is lower at the beginning of the year because out-of-pocket payments have to be paid during the year.

[2] We do not review this large body of literature here. The interested reader is referred to Baicker and Goldman (2011) and McGuire (2012).

compared a situation where a firm switches from free healthcare insurance to insurance with a high deductible. They find that a high deductible reduces care expenditure. As in Newhouse (1993), they find that individuals reduce both high-value care as well as 'wasteful' care. In a standard rational framework, consumers would most likely cut the latter type of care. Brot-Goldberg et al. (2017) also show that even the sickest quartile in their sample reduces healthcare expenditure, even though they tend to exceed the deductible.

There is little literature on demand responses of a rebate in healthcare. The Netherlands seems to be the only country to have implemented a rebate in healthcare on a national level. Some small-scale experiments have been carried out in Switzerland and Germany, and some private health insurers implemented a rebate in Switzerland in the 1980s and 1990s (Zweifel, 1987). The Swiss rebate was different from the Dutch rebate because individuals could only voluntarily choose for a rebate if they had low healthcare expenditures for five consecutive years. Zweifel reports a decrease in healthcare consumption due to the rebate. An experiment with a rebate for corporate sickness funds in Germany in the 1990s led to rather small effects (Groenewegen and de Jong, 2004). Finally, Goudriaan et al. (2007) and Holland et al. (2009) use a survey to evaluate the effect of the mandatory rebate in the Netherlands and report that 3 to 4 percent of the respondents claimed to reduce their healthcare consumption because of the rebate.

There are several papers that study the effect of the deductible in the Dutch context, although none of them are of an experimental or quasi-experimental nature like in the United States. Need et al. (1992), van Tulder and Bruyns (1995), and van der Maat and de Jong (2010) show with survey data that the deductible reduces Dutch healthcare expenditure significantly. Esch et al. (2015) combine survey data with registration data from general practitioners (GPs) and claim data from healthcare insurers. They show that the share of persons that do not follow up their GP's referrals increases with the size of the deductible.[3]

We show that differences in the design of a cost-sharing instrument can lead to different effects. If the goal of policymakers is to reduce expenditure and to offer a low health insurance premium, a deductible is more suitable than a rebate. However, if policymakers are concerned that a deductible stops (low income) individuals from using high value care then a rebate is preferred.

The structure of this paper is as follows: the institutional setting of the Dutch healthcare system is described in Section 2. Further, in Section 3 we explain our administrative data set

[3]GP care in the Netherlands does not fall under the deductible. GPs serve as gatekeepers to other forms of healthcare, for example hospitals. The idea of the work of Esch et al. (2015) is that it is undesirable from a societal point of view if persons do not follow up on their GP's advice and referral because of the deductible. Therefore, they investigate how often this occurs.

and provide descriptive statistics. In Section 4, the regression discontinuity design comparing the effect of the rebate and the deductible is described. Then the results are presented and discussed in Section 5. Section 6 offers three mechanisms why a rebate and deductible can have different effects on expenditure. Section 7 goes over a number of robustness checks, and we conclude in Section 8.

2. Institutional setting

The main feature of the Dutch curative healthcare sector is managed competition, introduced through the Health Insurance Act ('Zorgverzekeringswet') in 2006. Each person who lives or works in the Netherlands is obliged to buy health insurance for a basic benefit package from a private health insurer (van de Ven and Schut, 2008). Insurers negotiate with healthcare providers about prices and may selectively contract care for their clients. Competition therefore takes place among healthcare insurers –for buyers of insurance– and among healthcare providers –for patients and contracts with healthcare insurers (van de Ven and Schut, 2008).

The basic benefit package is the same[4] for everyone and covers a wide range of curative health care, such as hospital care, GP care, and mental health care.[5] The government determines and changes the coverage of the basic benefit package (for a list of changes in coverage over the period of our analysis, see Appendix A).

If people want to insure care that is not covered under the basic benefit package, such as orthodontic care, cosmetic surgery, or alternative medicine, they can buy supplementary health insurance. Healthcare insurers offer supplementary insurance independently from the basic package and individuals do not have to buy basic and supplementary insurance from the same insurer. During our sample period, over 85 percent of the population purchased supplementary health insurance (Nederlandse Zorgautoriteit, 2014). Health insurers may not refuse anyone for the basic benefit package, for example because of pre-existing conditions. Instead, they must offer insurance for everyone at a community-rated premium. These features, as well as an extensive risk-equalization scheme, are in place to prevent cherry picking, selection, and other market failures (van de Ven and Schut, 2008; Nederlandse Zorgautoriteit, 2016).

All Dutch citizens, except children under 18 years old, pay for healthcare costs in three ways. The first is through an insurance premium, which persons pay directly to their health insurer. This annual premium is currently between 1000 to 1200 euros. Low-income groups

[4] There exist small differences in basic benefit packages, not in terms of coverage but provider networks can differ between insurers (Nederlandse Zorgautoriteit, 2014).

[5] Long-term care is not part of the Health Insurance Act and is outside the scope of this study.

receive an income dependent monthly subsidy to pay for their premium. The second component of the contribution is an income-dependent fee, which is levied on an individual basis by the tax collector (van de Ven and Schut, 2008). The Health Insurance Act establishes that these income-dependent fees should cover exactly 50 percent of total health expenditures in a year. The third component consists of demand side cost-sharing.

Cost sharing is mandatory for all Dutch citizens of 18 years or older, and starts on the first day of the month after a person's eighteenth birthday.[6] In 2006 and 2007, the rebate was 255 euros,[7] but in 2008, the government replaced the rebate with a deductible of 150 euros. Since then, the deductible has been raised every year; see table 1.[8]

Table 1: Mandatory rebate and deductible in the Netherlands for 2006-2013

type	rebate		deductible					
year	2006	2007	2008	2009	2010	2011	2012	2013
amount (in euros)	255	255	150	155	165	170	220	350

Almost all types of health services covered under the basic-benefit package apply to the rebate or deductible. Only maternal care, obstetric care, GP care, and other types of primary care are exempted (see Appendix B for an overview), to ensure accessibility to these types of care. In addition to the mandatory rebate or deductible, people can also choose a voluntary deductible (also in 2006 and 2007) of maximally 500 euros. Only about a tenth of the Dutch population chooses such a voluntary deductible.[9]

Reinsurance of the mandatory deductible is allowed under special circumstances, for example for seasonal workers, people with a low income, or students. Less than 1.5 percent of the population has this reinsurance (Nederlandse Zorgautoriteit, 2014).

[6]The size of the rebate or deductible for an 18-year old depends on how many months remain between his or her birthday and the end of the year.

[7]Cost-sharing before 2006 is not reported, because the curative healthcare sector was organized very differently before the Health Insurance Act of 2006. Insurance was mandatory for low and middle incomes and offered by public insurers. Persons with a high income could purchase insurance at a private healthcare insurer, but were not obliged to. Deductibles were voluntary and often chosen, as the reduction in premium was quite generous. The rebate was introduced in public health insurance in 2005, just before the large healthcare reform of 2006.

[8]The mandatory deductible was raised to 365 euros in 2014, 375 euros in 2015, 385 euros in 2016, and 385 euros in 2017 and 2018 as well. Table 1 shows deductibles for 2006 up to 2013, as we have data for these years.

[9]The percentage of insured individuals choosing a voluntary deductible has increased since 2006, from about 3 percent in 2006 to 8 percent in 2013.

3. Data

Our data are proprietary healthcare claims data from 2006 to 2013 and include all insured inhabitants in the Netherlands (roughly 17 million). The data originate from Vektis, a private organization that collects and maintains data on behalf of all healthcare insurers in the Netherlands.[10] In total, the data set consists of 133 million observations. The data include for each person his or her total annual healthcare expenditures. These total annual healthcare expenditures have been broken down into expenditures of 21 categories of healthcare, such as hospital care, GP care, mental health care, and dental care.[11] A common problem with claims data is that people with low healthcare expenditures do not claim their bills at their insurer, because they do not expect to receive any compensation in return or to exceed their deductible. This is, however, not a problem in our data, because healthcare providers have a strong incentive to report all costs to patients' health insurers directly. If they do not report the costs, they will not be reimbursed by the health insurer. Providers send –often electronically– their bills to the insurer, who will then bill the patient.

In addition to healthcare expenditures, our data include several characteristics of individuals such as gender, four digit zip code, and age. Age is given in years and reported for December 31st in a particular year.[12] We obtain two binary variables from the Dutch risk equalization that indicate whether an individual has a chronic disease or is a chronic user of medication (van de Ven and Schut, 2008). DCG is short for diagnosis cost group ('diagnosekostengroep') and indicates whether a person had high healthcare costs in the previous years. PCG is an abbreviation of pharmaceutical cost group ('farmaciekostengroep') and indicates whether a person is a chronic user of medication. Lastly, we know the level of an individual's voluntary deductible in each year.

Using the four digit zip code in our data, we can link additional information on moving and household income. First, we identify movers based on zip codes: a persons is considered to have moved if he or she changed his or her zip code. Secondly, we attach information on the average standardized disposable household income in a zip code area, using publicly available data from Statistics Netherlands.[13] Based on the full, uncleaned sample, we constructed disposable income quintiles.

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[10] The data are pseudonymized and not publicly available.

[11] Appendix B includes a list of all 21 categories.

[12] A person who turns 18 on November 1st in 2008 is classified as 18 years old in 2008 in our data, even though he or she was 17 for 10 months that year.

[13] Average standardized disposable household income is gross household income minus taxes and premiums for public insurance policies. This income measure is standardized for differences in size and composition of households.

We clean our data set by excluding persons with a missing (pseudonymized) social security number, an invalid zip code, or a missing or invalid health insurance registration period.[14] We exclude observations with other administrative errors: individuals with negative healthcare expenditures and individuals with errors in their age pattern over time. In total, we remove 2,834,720 observations from our data which corresponds to 2 percent of the total number of observations.

Table 2 reports descriptive statistics of our data after cleaning. The data consist of 130 million observations over eight years, of which 49 percent is male[15] and 5 percent of the population has opted for a voluntary deductible between 2006 and 2013. On average, 6 percent of the population is classified in the risk equalization as having a chronic disease and 22 percent a chronic user of medication.[16] In total, 7 percent of the people in our sample moved. The mean household income quintile is 3.1.[17] On average, a person in our data has 1920 euros of healthcare expenditure. The standard deviation is large, because the distribution of healthcare expenditure is highly skewed. The majority of persons in our data has no or very little healthcare expenditures, while a small number of individuals has very high expenditures. Expenditures also differ substantially per healthcare category: the average expenditures are highest for hospital care, with 1101 euros per person, and lowest for physiotherapy, with an average of 28 euros per person.

In our empirical analysis of the effect of cost-sharing on healthcare expenditure, we exploit that cost-sharing –whether through a deductible or a rebate– only applies to persons of 18 years and older. With a regression discontinuity approach, we compare persons above and below 18 years old in a given year.[18] We do not use the full sample, as described in Table 2: for our design we focus on young adults around the age of 18, when the rebate and deductible kicks in.[19] We select all persons between the age of 15 and 21, but exclude people aged 18.[20] As our data do not contain the exact date of birth, we cannot distinguish someone who becomes 18 at January 1st from someone who becomes 18 on December 31st (in the same year). Formally,

[14] The registration period is usually one year, because health insurance is compulsory and an individual can only switch in January of a given year. In some cases, an observation can have a shorter registration period if the enrollee emigrates or dies. We exclude persons with a registration period of more than one year.

[15] The binary variable male equals 1 for male and 0 for females.

[16] The definition of diagnosis cost-related group has changed in the period of our data. The definition expanded, which increased the share of persons with a DCG.

[17] That is, after cleaning the mean quintile is not exactly $(5+1)/2 = 3$.

[18] See Section 4.

[19] Throughout this paper we use different selections of the data, for example for different robustness analyses. Here we describe the selected sample for our baseline specification.

[20] Other papers that exclude individuals at the discontinuity are, for example those of Leuven and Oosterbeek (2004) and Ferreira (2010).

the deductible "kicks in" in the month after a person's 18th birthday, which means that the former faces the deductible for almost the whole year while the latter does not have a deductible at all in this year. Excluding 18 year olds has the additional advantage that anticipation or substitution effects are likely to be small in our analysis. Anticipation effects are likely to occur just before someone turns 18. For example, if your birthday is May 15th, you may go to the dentist in April to avoid paying a deductible.[21]

As we are interested in the effects of the *mandatory* rebate and deductible, we exclude all individuals who choose a voluntary deductible at any point between 2006 and 2013. These individuals are taken from the sample, to rule out effects of the voluntary deductible, such as selection and moral hazard.[22]

For the baseline specification, we take out all persons with mental health care expenditures in one or more years between 2008 and 2013. Mental care was not part of the Health Insurance Act until 2008, thus the data contain no mental health expenditures for 2006 and 2007. It is therefore impossible to estimate the effect of the rebate on mental health care expenditure in 2006 and 2007 and make comparisons with the deductible in the years after.

In our baseline specification, we use persons between 19 and 21 years old as the treatment group and between 15 and 17 years old as the control group.[23] This controls for other things that change over time, beside the level of the rebate and deductible. Think, for instance, of changes in coverage of the basic package. However, changes in the coverage of a particular healthcare category can affect our results, if they do not apply in the same way to the treatment and control group. Dental care coverage changed between the two groups between 2008 and 2011 (see Appendix A). As expenditures on dental care are low but common –compared with mental care– we do not remove people that use dental care as we do with people using mental care.[24] Instead, we delete expenditure on dental care from our dependent variable: healthcare expenditure under the deductible. Although there can be an interaction effect between dental care expenditure and the deductible (e.g. people "filling up" their deductible or rebate on dental

[21] Anticipation effects are further addressed in Section 7.

[22] The voluntary deductible is mostly chosen by persons with low healthcare expenditures, because it is profitable for them (Douven et al., 2016). They therefore differ from people who do not choose a voluntary deductible. Suppose we would only delete observations of people choosing a voluntary deductible in the *year* that they choose a voluntary deductible. That implies that we would delete for example a 19 year old in 2012, but keep observations of this same person in previous years (ages 15-17 without a voluntary or mandatory deductible). Health expenditures are likely to be already low at 15, 16, or 17 years old due to selection effects, which would bias our results. To avoid this selection bias, we delete all observations of this individual.

[23] The choice of 15-17 year olds as the control group is discussed further in Sections 4 and 7

[24] Dental care is so common that excluding people with dental care expenditures would leave almost no observations.

health care expenditure), this effect is likely to be small as the level of dental care expenditure is low (see Table 2).

To sum up, our baseline sample includes young adults aged 15-21 (but not 18), who had no mental health care expenditures between 2008 and 2013 nor a voluntary deductible between 2006 and 2013. The main dependent variable in our analyses is total healthcare expenditure for healthcare categories under the rebate or deductible, but without dental healthcare costs. We will henceforth refer to this dependent variable as healthcare expenditure with cost-sharing.

Table 3 gives a summary of expenditures and characteristics of our baseline sample, divided into 15-17 year olds (the control group) and 19-21 year olds (the treatment group). Overall, we see that healthcare expenditure in all categories is much lower for the baseline sample than the full sample, as described in Table 2. This makes sense because our baseline sample is young and healthy: only 1 percent of the sample is a chronic user of healthcare and between 2 and 3 percent are chronic users of medication. For both groups we have more than 3 million observations. Most characteristics are similar across the two groups: only average healthcare expenditure and the share of movers differ significantly. Mean healthcare expenditures are 532 euros and 565 euros for the treatment and control group, respectively. Fifteen percent of the 19 to 21 year olds in our sample moved in our study period, whereas only 4 percent moved of the 15 to 17 year olds. This difference may reflect the fact that many 19 to 21 year olds are studying and therefore move out of their parental home. As a move may affect income, life style, and health, it is an important factor to address in our empirical strategy. We come back to this below. We also observe a relatively large difference for physiotherapy (37 euros versus 7 euros), which suggests an effect of cost-sharing.

Figures 1 and 2 demonstrate the descriptive evidence of our two main findings: cost-sharing reduces average healthcare expenditures, while the deductible reduces healthcare expenditures more than the rebate. Figure 1 shows mean healthcare expenditures for each age in the baseline sample and for each year that the rebate (2006 and 2007) and the deductible (2008 to 2013) were in place. Mean healthcare expenditure increases with age[25], but drops for 19 year olds.[26] We argue that this drop in expenditure is the result of the deductible or rebate as 17 year olds face no cost-sharing. The difference between the 17 and 19 year olds is larger for years with the deductible (2008-2013) than years with the rebate (2006, 2007).[27]

Figure 2 is identical to Figure 1, but highlights years 2007 and 2012 and shows mean healthcare expenditure for 18 year olds in those years. As expected, mean healthcare expenditure is

[25]This increasing trend is consistent with Alemayehu and Warner (2004).

[26]Note that because healthcare expenditure is increasing in age, average expenditure for the 15-17 group does *not* exceed the average expenditure for the 19-21 group in Table 3.

[27]See also Table 4 for the mean healthcare expenditures per year for 17 and 19 year olds.

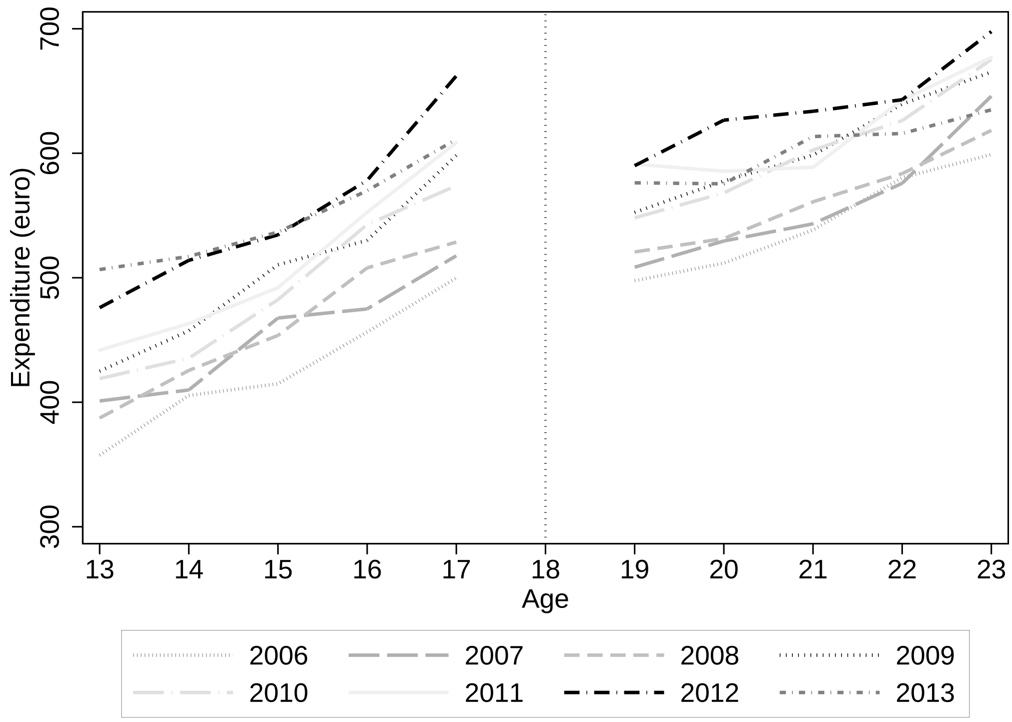

Figure 1: Mean healthcare expenditures by year and age

roughly in the middle of healthcare expenditure at 17 and 19: some of these '18 year olds' actually turn 18 in January, and thus face the rebate or the deductible for almost the whole calendar year, whereas others turn 18 in December and can consume healthcare for free during the same calendar year. The comparison of the years 2007 and 2012 is interesting because the level of the rebate and the deductible are similar (255 and 220 euros respectively), yet the difference in healthcare expenditures between 17 and 19 year olds is significantly bigger in 2012 than in 2007.[28] This suggests that the deductible has a larger impact on healthcare expenditure than the rebate.

4. Empirical strategy

In this section, we describe our empirical strategy. Results and robustness checks are presented in Sections 5 and 7.

To estimate causal effects of the rebate and the deductible on healthcare expenditure, we

[28]Figure 2 displays standard errors of the mean expenditures, which are different from standard deviations as reported in, for example Tables 2, 3, and 4.

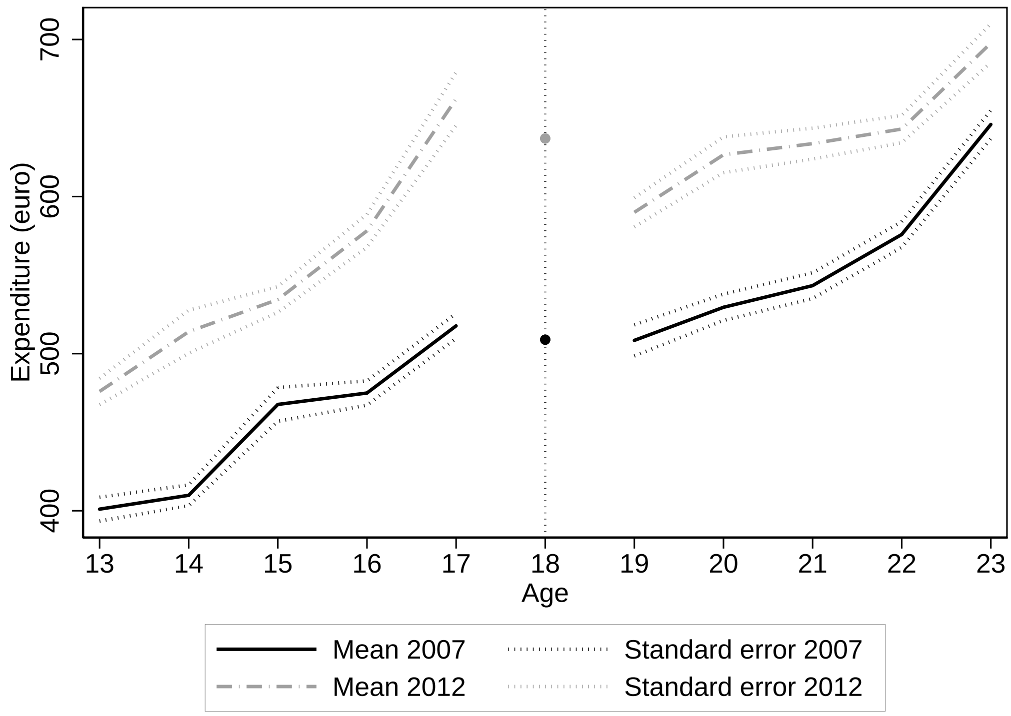

Figure 2: Mean and standard errors of healthcare expenditures by age for 2007 and 2012.

use a regression discontinuity design which exploits that cost-sharing in the Netherlands only applies to persons above 18 years old. This cut-off point is a sharp discontinuity that splits our sample into a treatment group, persons above 18, and control group, persons below 18 (Lee and Lemieux, 2010). Age in years is the 'assignment' or 'running' variable: by knowing a person's age, one knows whether he or she is in the treatment or control group. Or expressed differently:

$$T = \begin{cases} 1 & \text{if } age > 18 \\ 0 & \text{if } age < 18 \end{cases}$$

where T is a binary variable indicating whether an individual receives the treatment or not.

An important assumption for estimating causal effects with a regression discontinuity design is that individuals cannot manipulate or influence the assignment variable (Lee and Lemieux, 2010), which clearly holds in our case as individuals cannot manipulate their age in our data as it is based on official records.

Another requirement in a standard regression discontinuity design is that all factors determining the dependent variable, i.e., healthcare expenditures with cost-sharing, must evolve smoothly, except for the treatment variable. If this condition is satisfied, then a discontinuity or jump in the dependent variable can be ascribed to the treatment. This assumption does not necessarily hold in our design: several things change when a person turns 18, which may affect healthcare expenditure. To illustrate, the legal age for consumption of strong liquor and to drive a car is 18 during the period of our study.[29] This may cause an increase in healthcare expenditure due to (excessive) alcohol consumption and/or car accidents for people above 18. Also, 18 year olds are more likely to move out from their parental home to live on their own and/or to go to university. Moving out may reduce healthcare expenditures as 18 year olds may start paying their own bills after moving out.

However, this form of the assumption is not necessary for our analysis. Even if discontinuities exist around 18 (other than the deductible or rebate), this does not invalidate our design as we focus on the *change* from a rebate to a deductible and compare the regression discontinuity estimates over time. Discontinuities at 18 may exist but do not influence our results as long as these discontinuities stay constant over time. In Section 7.2 we show that this assumption is plausible for relevant factors that may be discontinuous at 18 and affect health expenditure.

Equation (1) formulates our regression discontinuity model. We consider individuals i in periods $t \in \{2006, 2007, 2008, 2009, 2010, 2011, 2012, 2013\}$ where $age_{it} \in \{15, 16, 17, 19, 20, 21\}$. That is, we use a bandwidth of three age-years before the cut-off point, from 15 to 17, and three years after, from 19 to 21.[30]

$$y_{it} = \alpha_t + \alpha_i + \beta \tilde{age}_{it} + \tau_t T_{it} + \beta' T_{it} \tilde{age}_{it} + \epsilon_{it} \tag{1}$$

Here, y_{it} denotes healthcare expenditure with cost-sharing of individual i in period t (Appendix B specifies the cost categories included in y_{it}), $T_{it} = 1$ if $age_{it} \in \{19, 20, 21\}$ and zero otherwise. $\tilde{age}_{it} = age_{it} - 18$: we center age around 18 for a straightforward interpretation of the coefficients. τ_t is the treatment effect and ϵ_{it} the error term.

There are many papers that use age as the running variable in a regression discontinuity design.[31] Our running variable, age, is only available on a yearly basis. As recommended by Lee and Card (2008) for such data, we use a parametric regression discontinuity design with a discrete running variable in years.[32]

[29] The legal age for consuming wine and beer was 16 in our study, but raised to 18 years in 2014.

[30] We have also explored another approach, as described by Dalton (2014). Dalton focuses on individuals who are price sensitive at the margin, i.e., their healthcare expenditure is just below or just above the deductible. These people would benefit from reducing their health consumption. Therefore, a local effect can be found near the threshold of the deductible: a 'bump' in the distribution of healthcare costs just before the threshold and a drop just after. In our data, however, no such effect is present. See Figures 6 and 7 in Appendix C.

[31] See for example: Behaghel et al. (2008), Card et al. (2008), Carpenter and Dobkin (2009), Leuven and Oosterbeek (2004), Ferreira (2010), Lemieux and Milligan (2008), Edmonds et al. (2005), and Bargain and Doorley (2011).

[32] Of the papers mentioned, only Carpenter and Dobkin (2009) and Bargain and Doorley (2011) knew the exact date of birth and hence age in days.

Our empirical strategy expands upon the standard regression discontinuity analysis in several ways. First, whereas standard regression discontinuity designs estimate a single equation, we pool eight equations: one for each year. Pooling increases the number of observations and enables us to include unobserved individual characteristics (α_i) that remain constant over time, such as health status. Further, pooling makes it possible to control for annual variation (α_t), such as changes in the coverage of the basic package.

Our baseline specification allows the β coefficients to vary before and after the threshold (i.e. β and β'), but assumes they are constant over time (i.e. not β_t). This assumption is valid, because β and β' are not significantly different for different values of t when we do include them in equation (1).[33]

The disadvantage of equation (1) is that the estimated coefficients τ_t are not intuitively interpretable: the coefficients are not adjusted for the size of the rebate or deductible. The model also yields eight coefficients: two for the rebate and six for the deductible. To get one coefficient for each form of cost-sharing, we also estimate equation (2) with a linear approximation of the relation between r_t, d_t and expenditure:

$$y_{it} = \alpha_t + \alpha_i + \beta \tilde{age}_{it} + \beta' T_{it} \tilde{age}_{it} + \gamma r_t R_{it} + \delta d_t D_{it} + \epsilon_{it} \tag{2}$$

Equation (2) is identical to (1), except for $\gamma r_t R_{it}$ and $\delta d_t D_{it}$. γ is the treatment effect of the rebate and δ for the deductible, and r_t and d_t denote the size of the rebate and deductible in year t, respectively.[34] $R_{it} = 1$ if $age_{it} \in \{19, 20, 21\}$ and $t \in \{2006, 2007\}$ and zero otherwise. Similarly, $D_{it} = 1$ if $age_{it} \in \{19, 20, 21\}$ and $t \in \{2008, 2009, 2010, 2011, 2012, 2013\}$.

Although not shown explicitly in equation (1), we allow for different age and year effects for men and women, as the trend in health expenditure over age is different for (young) men and women. Women, for example, are likely to start using birth control or become pregnant when they are between 15 and 21 years old. As we are interested in the average effect (women and men combined), τ_t is estimated for men and women together.[35]

Standard errors are clustered at the individual level to correct for correlation (Lee and Lemieux, 2010). Finally, we also estimate the model without individual fixed effects (pooled ordinary least squares) to assess the importance of unobserved individual characteristics:

[33] The hypothesis that β_t's (β'_t) are the same over time, after estimating equation (1) with β_t and β'_t instead of β and β', cannot be rejected at a 1 percent significance level. We find a p-value of 0.649 (0.982).

[34] Note that r_t is constant in 2006 and 2007.

[35] Hence the full specification of our model is: $y_{it} = \alpha_t + \delta_t * male + \alpha_i + \beta \tilde{age}_{it} + \zeta \tilde{age}_{it} * male + \tau_t T_{it} + \beta' T_{it} \tilde{age}_{it} + \zeta' T_{it} \tilde{age}_{it} * male + \epsilon_{it}$ where $male$ denotes a binary variable for male (1 is male, 0 is female). As equation (1) is easier to read, we refer to that equation as our model in this paper. Unless otherwise stated, we estimate different α_t, β, β' for men and women.

$$y_{it} = \alpha_t + \beta a\tilde{g}e_{it} + \tau_t T_{it} + \beta' T_{it} a\tilde{g}e_{it} + \epsilon_{it} \tag{3}$$

5. Results

The estimated coefficients of equations (1) and (3) for the baseline specification are reported in Table 5, in the second and first column, respectively. The estimated coefficients of τ_{2006} are -68.1 and -54.3 for fixed effects and ordinary least squares, respectively. To illustrate the economic significance of these estimates, a coefficient of -68.1 means that the rebate of 255 euros reduced healthcare expenditure on average by 68.1 euros per person. The estimated coefficients for the fixed effects specification are displayed in figure 3, which shows that all estimated coefficients are statistically significant from zero. For the years 2006-2011, the coefficients have roughly the same size, but in 2012 and 2013 they increase in absolute value as the size of the deductible increases.

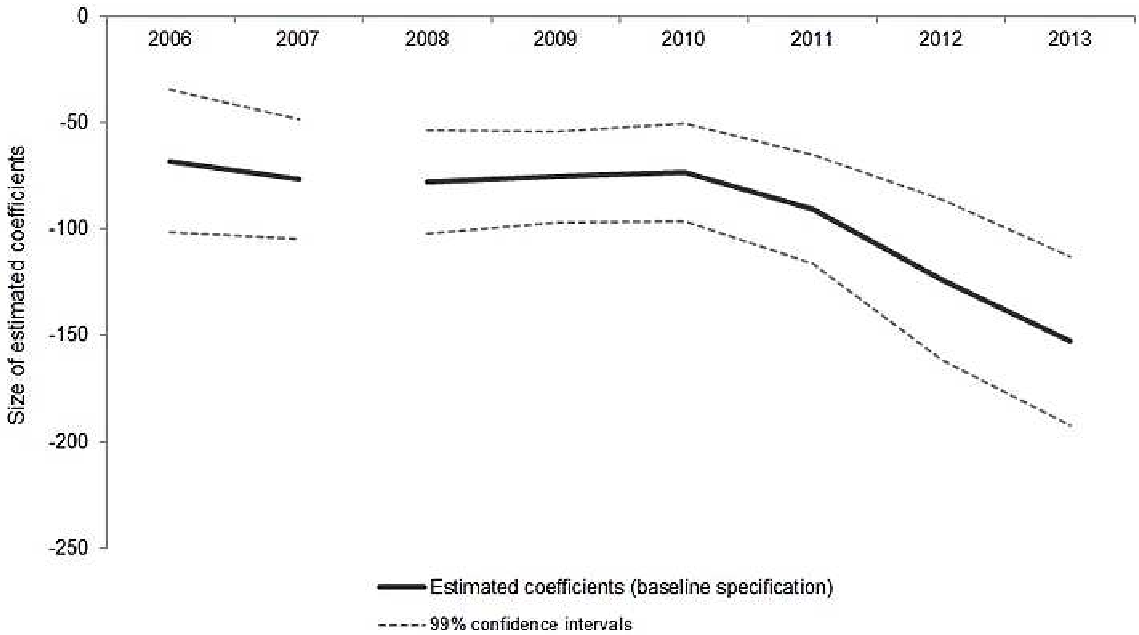

Figure 3: Estimated τ_t coefficients and their 99% confidence intervals for 2006-2013 using equation (1)

As we compare the effect of the rebate and the deductible, we focus on the *difference* between the estimated coefficients τ_t between different years. Table 6 lists all differences between the coefficients of the rebate and deductible as reported in Table 5 with fixed effects. Again, we see that the coefficients do not differ much for the rebate and deductible up to 2011. An F-test shows that up to 2011 the estimated coefficients did not differ significantly from each other, using a 1 percent significance level.

However, to assess whether the effect on healthcare usage of a deductible differs from a rebate, we also have to account for the level of the deductible and rebate. Even though the estimated coefficients do not differ significantly between 2006 and 2011, the level of the deductible between 2008 and 2011 (150 to 170 euros) was considerably lower than that of the rebate (255 euros). The size of the deductible in 2012 (220 euros) is relatively close to the size of the rebate in 2006 and 2007 (255 euros). τ_{2012} is -123.8 for fixed effects. Comparing this with τ_{2006} and τ_{2007}, we find that the effect of a deductible is on average 50 euros higher (in absolute value). The differences of τ_{2006} and τ_{2007} with τ_{2012} are statistically significant at a 5 percent significance level. The differences with the deductible in 2013 are even larger, however the size of the deductible was also larger in 2013 than the rebate in 2006 and 2007.

Table 5 shows that the effect of age on healthcare expenditure is positive and statistically significant. However, the magnitudes are different for men and women and before and after the discontinuity. Looking at the ordinary least squares specification, health expenditure of women under 18 increases on average with 48 euros if age increases by one year. This is lower for men under 18: $48.00 - 10.60 = 37.40$ euros. For women of 19 years or older, the slope is similar as for the below 18s: $48.0 + 1.9 \approx 48.0$. This is not the case for men over 18, as health expenditure increases by only 3 euros for an increase in age by one year: $48.0 - 10.6 + 1.9 - 36.3 = 3.0$. With individual fixed effects, the age relation is different from ordinary least squares. Introducing fixed effects changes the interpretation of the coefficients for age and hence their magnitude.

To correct for the size of the rebate and the deductible, we estimate equation (2), where we use a linear approximation to find the effect of the level of the deductible and rebate. The results are reported in Table 7. The estimated coefficient δ is larger (in absolute value) than γ in both the ordinary least squared specification and the fixed effects specification: the deductible reduces healthcare expenditure more than the rebate per euro of cost-sharing. The differences between these coefficients are statistically significant at a 1 percent significance level.[36] A one euro increase of the rebate leads on average to a reduction of healthcare expenditures of 26 eurocents. For a one euro increase of the deductible, this reduction is 44 eurocents: 18 eurocents larger, almost twice the reduction of the rebate.

As γ, δ allow for one-dimensional summaries of the two, respectively six effects in Table 5, they make it easier to compare effects across groups. Hence, we estimate equation (2) for multiple groups: men and women, persons living in a zip code with the highest and lowest

[36]The p-values of the F-tests are 0.000 for both the ordinary least squares and fixed effects specifications.

household income quintile. Table 8 shows the estimated coefficients γ for the rebate and δ for the deductible for these groups. For both men and women, we see that δ is higher than γ. These differences are statistically significant.[37] The coefficients for women are slightly higher than for men, but so is their mean healthcare expenditure.

We also compare between individuals who live in high or low income areas. People with a low income may forgo healthcare consumption because they cannot afford it in case of a deductible. This effect is likely to be smaller for persons living in a high income area. We find evidence for this hypothesis. The estimated coefficients of the rebate γ are -0.11 and -0.13, and they are not statistically significant at a 1 or 5 percent significance level for persons in the lowest income quintile. This suggests that they do not respond strongly to the rebate.

However, they do respond to the deductible, as the coefficients for the deductible are both significantly larger (-0.57 and -0.41).[38] Persons in the highest quintile do not respond in a significantly different way to the rebate and the deductible.[39,40]

To conclude: the deductible is more effective in reducing healthcare expenditures than the rebate. We observe this for both men and women. Persons living in low income areas do not respond strongly to a rebate but do respond to a deductible. The next section explores a number of reasons that may explain the differential response of individuals to a rebate and a deductible.

6. Mechanisms

We first present a standard model where a deductible and rebate lead to the same choices by the consumer. Then we present a number of deviations from the standard model explaining why a deductible reduces healthcare expenditure more than a rebate. The main goal of the paper is to document the different effects of deductibles and rebates on healthcare expenditure. We do not aim to –and with our data we cannot– pinpoint which of the explanations below is "correct".

[37] The p-values of the F-tests are 0.000 for the ordinary least squares estimates for both men and women. For the fixed effects estimations we find p-values of 0.0015 and 0.0014 for men and women, respectively.

[38] The difference between γ and δ of the ordinary least squares and fixed effects estimations are significant for the lowest household income quintile: the p-values of the F-tests are 0.0000 and 0.0008, respectively.

[39] The p-values of the F-tests are 0.2501 and 0.2344 for ordinary least squares and fixed effects, respectively.

[40] For these analyses, we used average *standardized* household income quintiles. Results based on non standardized household income quintiles are similar. For example, for the ordinary least squares estimates we find for quintile 1, γ is -0.11 and δ -0.57 (significant at a 1 percent significance level). For quintile 5, γ is -0.26 and δ -0.34 (both significant at a 1 percent significance level).

6.1. Standard rational framework

With a standard utility function and no imperfections in consumer behavior or on the insurance market, consumer behavior should be the same under a deductible and a rebate. This can be seen as follows. Consider a consumer with initial wealth w who has bought insurance with a deductible D at a premium equal to σ. For ease of exposition, we assume that financial utility u (e.g. utility derived from consuming other goods) and health utility v are additively separable. Hence, overall utility is of the form $u + v$. We assume that the patient is offered only one treatment per period with utility v which is drawn from some distribution defined on $\mathbb{R}_+$. The patient decides whether to be treated or not.

Assume that the patient is offered a treatment with value v at a cost equal to $y \in [0, D]$.[41] She decides to undergo the treatment if

$$u(w - \sigma - y) + v \geq u(w - \sigma). \tag{4}$$

Now consider the same consumer buying health insurance with a rebate D. That is, if her healthcare expenditures y are below D, she receives a bonus at the end of the period equal to $D - y$. For the insurance market to be able to finance this bonus, the premium σ needs to be increased with D, assuming that the rebate leads to the same healthcare expenditure as the deductible, as indeed it will in this model. Further, assume that all these payments are close enough together in time that no discounting is needed. Then the consumer accepts the treatment if

$$u(w - (\sigma + D) + D - y) + v \geq u(w - (\sigma + D) + D). \tag{5}$$

Clearly, the trade offs are the same under the deductible and the rebate. Hence, a treatment is accepted under the rebate if and only if it is accepted under the deductible. According to this model, healthcare expenditures are the same under the rebate and the deductible.

6.2. Prospect theory

As a first explanation of the difference between healthcare expenditure under rebate and deductible, we discuss Kahneman and Tversky (1979), Johnson et al. (1993), and Thaler (1999). We present a simple model in this vein to show that treatments exist that are consumed if a person faces a rebate but not in case of a deductible. Accordingly, healthcare expenditure will be lower with a deductible than a rebate of the same magnitude D.

Figure 4 plots the value function of financial utility under prospect theory, $u(.)$. In a system with deductible D, if a patient decides to accept a treatment with value v and costs $y \in \langle 0, D \rangle$,

[41] For costs $y > D$, the expressions below are valid with $y = D$, both for the deductible and the rebate case.

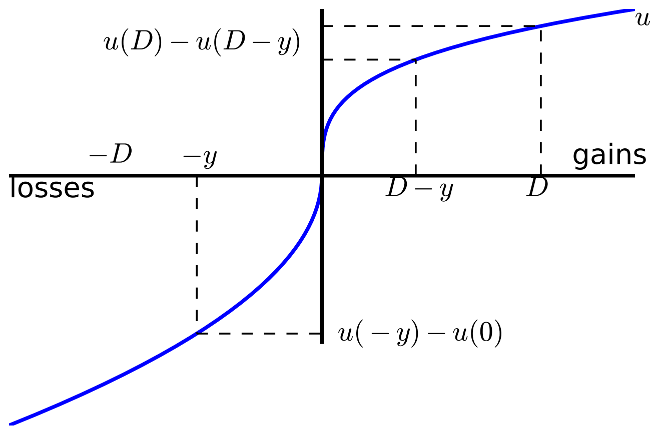

Figure 4: Value function $u(.)$ of an individual facing either deductible or rebate of D (based on Kahneman and Tversky (1979)).

then he or she will pay y. This can be considered as a financial loss compared to the status quo of not paying for treatment, which is depicted in the left half of the quadrant in Figure 4.[42] Costs y of the treatment lead to value $u(-y) < 0$. Therefore, a patient will only choose to be treated if $u(-y) + v > u(0)$ or equivalently, $v > u(0) - u(-y)$.

If the same patient decides to accept the treatment when a rebate is in place, this implies that he or she will not receive a rebate/bonus D at the end of the year, but $D - y$. In other words, the financial gain at the end of the year is reduced from D to $D - y$. Gains are depicted in the right half of the quadrant in the figure. Hence, by choosing the treatment, value from the financial gain is reduced by $u(D) - U(D - y)$. Consequently, the patient accepts treatment only if treatment utility $v > u(D) - u(D - y)$.

The idea of prospect theory is that value is concave in gains and convex in losses (compared to the status quo), as in Figure 4. The figure shows that $u(0) - u(-y) > u(D) - u(D - y)$. Therefore, there will be treatments that a patient chooses if she faces a rebate but not under a deductible. These are treatments generating utility $v \in \langle u(D) - u(D - y), u(0) - u(-y)\rangle$. Losses (or expenditure under the deductible) have a bigger impact than gains (or expenditure under the rebate). Further, as the value function is steeper for losses than for gains, we find a similar effect for treatments with costs $y \geq D$. To be precise, the properties of Figure 4 that we need for this result are: (i) the reference/status quo point is a patient who has paid his or her

[42]To be precise, the status quo in this example is a person who has paid his or her insurance premium and who has not used any health care yet. An out-of-pocket expense due to the deductible is therefore a financial loss.

premium and who has not used healthcare yet, (ii) financial utility is concave in financial gains and convex in losses, and (iii) financial utility is steeper for losses than for gains (Kahneman and Tversky, 1979, pp. 279).

6.3. Discounting

If there is discounting, an intuitive effect is caused by the difference in timing between rebate and deductible. With a deductible, the out-of-pocket payment is made when the treatment is received. Hence, the comparison between expenditure and health benefit is the one in equation (4). With a rebate, the bonus is received at the end of this period (beginning of next period). Let r denote the discount rate used by insurers who need to reserve $D/(1+r)$ when receiving the premium to be able to pay a bonus D at the end of the period. We denote the discount rate used by the agent by r_a. Then, the comparison for the rebate becomes:

$$u(w-(\sigma+\frac{D}{1+r})+\frac{D-y}{1+r_a})+v \geq u(w-(\sigma+\frac{D}{1+r})+\frac{D}{1+r_a}). \tag{6}$$

First consider the case where $r_a = r$. Then the D-terms drop out in this comparison. The effect that we are left with is that the price of the out-of-pocket payment equals $1/(1+r) < 1$ with a rebate. As the price falls, people consume more healthcare. This is consistent with the results we find above.

An interpretation of $r_a > r$ is the finding by Brot-Goldberg et al. (2017) that people are myopic and do not respond to end-of-year prices in healthcare but rather to spot prices. With $r_a = +\infty$, people do not respond to end-of-year prices at all. In case of the rebate, people then face a spot price of zero, but with a deductible a spot price of 1. Aron-Dine et al. (2015) and Einav et al. (2015), however, show that people are partially myopic ($r_a > 0$ but finite). One reason for such myopia is that people do not know or are not aware that under a rebate, healthcare expenditure now can affect their income in the next period. With the deductible, the connection between treatment and payment is more clearly linked and people take this into account. Again, this is consistent with our findings above.

6.4. Liquidity constraints

With discounting, as in (6) with $r = r_a$, agents can move their money "freely" between periods. This is no longer true if agents face liquidity constraints. Again assume that with the deductible all expenditures are done in the same period (with the same liquidity constraint), while with a rebate the premium is paid in this period and the rebate is received in the next period. Liquidity constraints in this form need not create a difference between expenditure under the rebate and deductible. Under the rebate people pay an additional D for their insurance premium. If they are prudent enough to set the same amount D aside under a deductible, they can spend the same on healthcare under both systems.

However, if they are not this prudent and spend more on other (consumption) goods under the deductible in the first period, they can run into liquidity problems. When they fall ill, they do not have the resources to pay the out-of-pocket payment with the deductible in this period. In contrast, with the rebate the out-of-pocket payment has already been sunk in terms of a higher premium σ. The effect of a reduced bonus only comes in the next period. Hence, the treatment can be accepted in this period.

As such, a combination of liquidity constraints and a lack of prudence leads to the prediction that healthcare expenditure is higher under a rebate than under a deductible. Moreover, one expects this effect to be stronger for lower income households as liquidity constraints are more likely to be binding for them. Indeed, this is what we find in Table 8. For the low income quintiles, the difference between the effects of a rebate and a deductible is bigger than for high income quintiles.

We cannot pinpoint which of the mechanisms above contributes most to the differences that we find, but it is worth pointing out the different welfare and policy implications. In a model based on prospect theory, the potential health loss due to a deductible instead of rebate is limited. Indeed, the difference between the two is observed for treatments with value below $u(0) - u(-y)$; there is an upperbound on the potential loss. With discounting where $r_a > r$, the policy issue is actually over-consumption as this discounting reduces the price of treatment. In both these cases, a policy maker can turn to a deductible if the goal is to reduce healthcare expenditure, without much concern that people may forgo high value treatments. This is different in the case of liquidity constraints. In the latter model, people may run out of money to pay the deductible for a high-value treatment. In this case, the deductible will reduce healthcare expenditure more than the rebate but with the risk of low income people going without high value treatments.

7. Robustness analyses

Several assumptions are made for our empirical strategy. In this section, we test how sensitive our results are to these assumptions. On the whole, we find that our conclusion –a deductible is more effective in reducing healthcare expenditure than a rebate of similar size– is robust.

7.1. Functional forms and bandwidths

In Section 5, we assume that expenditure is linear in age allowing for a different slope before (β) and after (β') the discontinuity. We test for six other functional forms: linear, quadratic, and cubic, as well as these three forms including an interaction allowing for a different slope before and after the discontinuity. To illustrate, we show below the equations for the linear and quadratic functional forms (our baseline specification from Section 4 is equation (8)):[43]

$$y_{it} = \alpha_t + \alpha_i + \tau_t T_{it} + \beta \tilde{age}_{it} + \epsilon_{it} \tag{7}$$

$$y_{it} = \alpha_t + \alpha_i + \tau_t T_{it} + \beta \tilde{age}_{it} + \beta' T_{it} \tilde{age}_{it} + \epsilon_{it} \tag{8}$$

$$y_{it} = \alpha_t + \alpha_i + \tau_t T_{it} + \beta \tilde{age}_{it} + \gamma \tilde{age}_{it}^2 + \epsilon_{it} \tag{9}$$

$$y_{it} = \alpha_t + \alpha_i + \tau_t T_{it} + \beta \tilde{age}_{it} + \gamma \tilde{age}_{it}^2 + \beta' T_{it} \tilde{age}_{it} + \gamma' T_{it} \tilde{age}_{it}^2 + \epsilon_{it} \tag{10}$$

To identify the best functional form, we follow Jacob et al. (2012) and Lee and Lemieux (2010) and estimate the simplest functional form –a linear specification– up to the most flexible form. Jacob et al. (2012) and Lee and Lemieux (2010) argue that each functional form must be estimated twice: once 'normally', as described above (restricted model), and once including age dummies (unrestricted model). If the latter model significantly improves the former, then this means that the restricted model is too limited and a more flexible model is baseline. This process must be repeated until the unrestricted model is no longer better than the restricted model in the sense that an F-test, which tests whether the age dummies are jointly significant, is no longer significant (Jacob et al., 2012). Table 9 shows the results for OLS estimations for six different functional forms and a three- and five-year bandwidth.[44] The results in bold indicate the best specification given a bandwidth.

The linear specification with an interaction term is the best functional form, so we chose this functional form as our baseline specification.[45] Table 9 also shows that choosing a functional form with a higher polynomial would not alter the results because the coefficients are similar for the different functional forms. Although a cubic form with an interaction gives slightly higher coefficients, this does not change our outcome since we compare the differences between years. Also, with a cubic form, the deductible reduces healthcare consumption more than the rebate.

Given the small bandwidth, it may not be surprising that a linear specification performs best. Jacob et al. (2012) advise running an extra regression discontinuity analysis with a different bandwidth, to test how sensitive the functional form is to data points further away from the discontinuity. We therefore conducted an additional regression discontinuity analysis extending the interval to five age-years before and after the discontinuity. The results are also presented in Table 9. With the wider bandwidth, a quadratic form with an interaction now performs

[43] Table 9 presents the results of the different functional forms. We also tested the quartic and quintic forms but do not report these as they do not improve the specification.

[44] Appendix D shows the results of the same estimations, but including individual fixed effects.

[45] For a three-year bandwidth, the F-test resulted in a linear model with an interaction in a p-value of 0.061 for OLS and 0.593 for fixed effects.

best using the selection procedure described above.[46] Given this bandwidth, the τs for the years 2006-2011 are approximately the same size while the deductible in these years is smaller than the rebate. For the best specification with a five-year bandwidth (column (4)), the treatment effect in 2012 is bigger than in 2006 and 2007, while the levels of rebate and deductible are comparable in these years. Overall, the coefficients are quite stable throughout different bandwidths and functional forms. Our conclusion that the deducible has a bigger effect on consumption than a rebate of a similar magnitude is robust with respect to our bandwidth choice.

7.2. Balancing tests

An important assumption for our empirical strategy is that discontinuities at 18, other than the rebate or deductible, may occur as long as they are constant over time (see Section 4). Below we show important potential causes of discontinuities at 18, revealing that they are indeed constant over time, and therefore do not invalidate our design.

The first potential factor is the share of persons moving at 18. Many students in the Netherlands graduate from secondary education ('voortgezet onderwijs') at 18 and continue their education at university. Within this group, a relatively large share moves out of their parental home. Moving out often coincides with changes in income and life style, which may affect healthcare expenditure. Our data show a small jump in the number of people who move at 18, compared with 17. However, this jump is constant over time and will therefore not affect our results. To prove this with our data, we exclude all people from our sample who at one point moved between 2006 and 2013 and reestimate our model. The results are reported in table 10. The estimated coefficients are comparable with Table 5. Again, effects in 2012 are clearly higher than in 2006 and 2007. Our data do not specify whether a person actually moved out of his or her parental house, or perhaps simply moved with his or her parents. Statistics Netherlands does offer this distinction in population data.[47] Table 11 shows the share of individuals who moved out of their parental house by age and by year. We see the same pattern: the share of individuals who no longer live with their parents sharply increases at 18, compared with 17. This difference is constant over time.

The data from Statistics Netherlands also includes information on education levels of individuals between 15 and 21 years old over time.[48] Table 12 reports by school year the share of individuals at each level of education: secondary education, vocational education, university of

[46]For a five-year bandwidth, the F-test resulted in a quadratic model with an interaction in a p-value of 0.076 for OLS and 0.710 for fixed effects.

[47]We can use and compare these data with our own data, because they are both based on the whole Dutch population.

[48]Unfortunately, we do not have information on education level in our own data set.

applied sciences and university.[49] We do not observe any sharp changes in education levels over time.

7.3. Anticipatory effects

The identification strategy in this paper exploits that persons below the age of 18 do not face the rebate or deductible and therefore can be used as a control group. However, persons at the age of 17 may already react to the rebate and deductible by consuming more healthcare consumption before they turn 18, when it is still free. Anticipatory behavior or timing of healthcare consumption in response to cost-sharing is established and can be substantial (see for example Einav et al. (2015) for Medicare Part D, and Cabral (2013) for dental care in the United States).

We expect that anticipatory behavior will be at its highest in the year that persons turn 18. This is one of the reasons why in our baseline specification we remove from our data persons who turn 18 in a given year. As an additional robustness check, we also exclude persons who turn 17 in a given year. Tables 13 and 14 show the results of estimations of equations (1) and (2) in which 17 and 18 year olds are excluded from the sample. The differences between the rebate in 2006 and 2007 and the deductible are still visible in Table 13, though sometimes smaller. When we account for the size of the rebate and the deductible in Table 14, as above δ is significantly higher than γ (in absolute value).[50] Anticipatory effects at 17 thus do not explain the different effects between a rebate and deductible.

7.4. Users of mental healthcare

As discussed in Section 3, in our baseline specification we exclude all individuals who have used any mental healthcare between 2008 and 2013. To test the effect of this selection on our results, we reestimate our baseline regression including these individuals, but excluding their mental healthcare expenditures. Recall from Section 3 and Appendix A that coverage of mental healthcare in the basic package has varied substantially over time. Hence, comparing healthcare expenditures including mental care expenditures over time gives untrustworthy results.

The results are reported in Table 15. The fixed effects coefficients are fairly robust: results are comparable with Table 5. The individual fixed effects pick up differences in net-rebate or net-deductible after mental care expenditures have been accounted for. The ordinary least

[49] In Dutch secondary education is ‘voorgezet onderwijs,’vocational education is ‘middelbaar beroepsonderwijs,’ university of applied sciences is ‘hoger beroepsonderwijs,’ and university is ‘wetenschappelijk onderwijs.’ These education levels do not overlap, e.g. university of applied sciences and university are two different levels.

[50] The p-values of the F-tests are 0.000 and 0.001 for ordinary least squares and fixed effects, respectively.

squares estimates are more strongly affected by introducing users of mental care. In each case, however, the effect in 2012 is larger than the rebate effect in 2006 and 2007.

7.5. Price level effects

In our analysis, we estimate the effects of the rebate and deductible over the years 2006-2013. Treatment prices may vary over these years. To illustrate, prices fall with the introduction of generic drugs and prices tend to rise when an old treatment is replaced by newly developed drugs. In principle, time fixed effects will correct for price changes. To check if somehow price changes still bias the estimated treatment effects τ_t over the years, we perform the following test. We scale each year's health expenditures with the ratio of average healthcare expenditure for 17 year olds in that year and the average healthcare expenditure of 17 year olds for all years (unfortunately we do not have treatment prices in our data and cannot perfectly adjust them). To illustrate, the average health expenditures of 17 year olds in 2006 are 0.8 times the average healthcare expenditures of 17 year olds between 2006 and 2013. Assuming that treatment quantities did not change (much) for 17 year olds over the years, prices were relatively low in 2006. The idea is that the development of expenditures for 17 year olds is not affected by the development of rebate and deductible and represents a "truer" price effect. We divide the healthcare expenditures for all age groups in 2006 by 0.8 and similarly for other years.

Table 16 shows the results of the regression discontinuity estimation with these rescaled variables. The differences between the coefficients of the rebate and the deductible remain. In particular, the difference between the rebate in 2006 and 2007 and the deductible in 2012 persists.

7.6. Rounding the running variable

Dong (2015) argues that rounding of the running variable, age in our case, can bias results. A regression discontinuity design with age in days can give different results from a regression discontinuity design with age rounded to years, especially if it is not known whether a person's age was rounded upwards or downwards. In our data, we know that age is rounded upwards: a person's age in a given year is his or her age on December 31st of that year. Therefore, we know that if a bias occurs, this bias will be in the same direction for all ages and years and will not affect regression discontinuity estimates much. In our baseline specification, we mitigate the effect of rounding by excluding all persons aged 18. Therefore, we never incorrectly classify a 17 year old as 18. Dong offers a way to correct for potential bias, assuming one has some information regarding the distribution of the rounding error. We assume –like Dong– that this distribution is uniform. With the fixed effects estimation the results are $\tau_{2006} = -100.05, \tau_{2007} = -108.15$, and $\tau_{2012} = -155.75$. Hence the correction enlarges the coefficients, but the relative differences remain the same: the effect of the deductible in 2012 remains about 50 euros larger than the effect of the rebate in 2006 and 2007.

7.7. Clustering of standard errors

Some studies (e.g. Lee and Card (2008), Card et al. (2008), and Ferreira (2010)) suggest that standard errors must be clustered by age when conducting a regression discontinuity design with age as the running variable. Standard errors in our study are clustered by individual because we use a panel and individual fixed effects. If we cluster the standard errors by age, we find that the standard errors become much smaller compared with the standard errors clustered by individual. Hence, the differences between τ_{2006} and τ_{2012} and between τ_{2007} and τ_{2012} remain statistically significant. We also clustered the standard errors by age and cohort simultaneously and individual and age simultaneously. All standard errors were smaller than just clustering by individual, as reported in Table 17.

7.8. A more flexible specification for age over time

In our specification (1), we assume that β and β' are constant over time as we do not expect the effect of age on expenditure to vary over time.[51] However, we can allow for β_t and β'_t. In the ordinary least squares regression we then find $\tau_{2006} = -66.9(19.41), \tau_{2007} = -65.1(19.05)$, and $\tau_{2012} = -146.4(27.01)$, with standard errors in brackets. Hence, all coefficients are significant and the effect in 2012 is bigger than in 2006 and 2007 when deductible and rebate are of comparable size. With the fixed effects estimation, the results are $\tau_{2006} = -77.4(31.83), \tau_{2007} = -58.0(27.02)$, and $\tau_{2012} = -149.0(43.33)$. Hence, the main result is not affected when we allow β and β' to vary with calendar time.

7.9. Fictional discontinuities

A regression discontinuity design should only measure an effect on healthcare expenditure at 18 with the switch from no-treatment to treatment group. If our specified model (1) works properly it should therefore only pick up an effect at the actual discontinuity at 18, not at other ages. We ran our model numerous times assuming fake or placebo discontinuities, at other ages than 18.[52] For example, in Table 18 we show the estimation results when we assumed a

[51] Recall that we also tested whether β and β' change significantly over time: this was not the case. See Section 4.

[52] To clarify, the analyses in this section are solely a test of the specification of our model. We do not conduct a real placebo test in the sense that we used a control group.

discontinuity at age 24. Of course, we know that there was no discontinuity at that age, and, consequently, our model should not pick up any effects. In contrast to our results in the paper, we find only small coefficients with positive as well as negative signs. Moreover, except for 2008, all coefficients are insignificant for the fixed effects estimation. In Figure 5, the τ_t coefficients are graphically shown for multiple fictional discontinuities at ages 10 to 50 and for all years.[53] The figure shows variation in the estimated coefficients for the placebo tests. The coefficients of the real discontinuity, at 18, are clearly distinguishable from the placebo discontinuities. These results suggest that our estimates in the paper are strongly related to the discontinuity of the deductible or rebate at age 18.

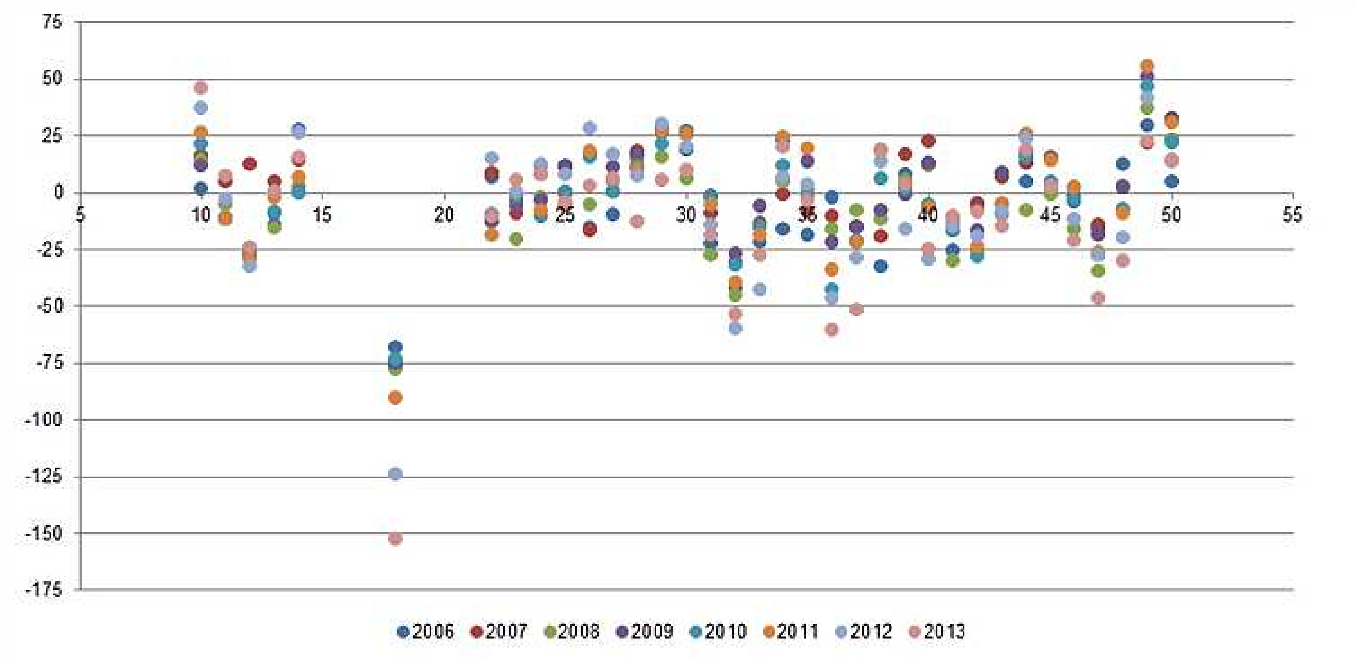

Figure 5: Estimated treatment effects τ_t for placebo discontinuities at ages 10 - 50

[53]We did not include placebo discontinuities in the bandwidth of our baseline specification, from 15 to 21, as they may pick up the effect at 18.

8. Concluding remarks

In this study, we compare the effect of the rebate (in 2006 and 2007) and the deductible (from 2008 to 2013) on healthcare spending of 18 year olds in the Netherlands. Our main result is that people respond in significantly different ways to a rebate than to a deductible: a one euro increase of a rebate reduces healthcare expenditures by 18 cents less than a one euro increase of a deductible.

Three possible explanations for these results are discussed in this paper: prospect theory, discounting, and liquidity constraints. With our data and analyses we cannot determine which of these explanations contributes most to the differences we find. However, our comparisons of persons living in an area with the lowest and highest average household income quintile suggest that liquidity constraints may cause the results. Persons with a high income (no liquidity problems expected) do not respond in significantly different ways to the rebate or deductible, whereas persons with a low income do. The latter do not respond strongly to the rebate but do respond strongly to the deductible.

Our empirical strategy relies on a discontinuity at the age of 18. Hence, our estimated effects are local and apply to 18 year olds. They may not be generalized to other ages or the whole population. However, the internal validity of our findings is strong. We apply a regression discontinuity design to infer causal effects and use a dataset that encompasses the whole Dutch population. The results are also robust to multiple specifications of the model and not driven by anticipatory or price level effects.

This study, together with the work of Stockley (2016) and Newhouse (1993), is important for policy making in healthcare as it compares different cost-sharing designs. Small differences in the design of cost-sharing schemes lead to significantly different effects on healthcare expenditures. Unfortunately we cannot determine the optimal form of cost-sharing as we cannot measure the effects of the rebate and deductible on welfare, health status, or quantity of care, nor can we assess whether people reduce wasteful or valuable care. However, our results suggest that if policymakers' priority is to reduce expenditure and to offer a low health insurance premium, then a deductible is more effective than a rebate. Yet policymakers may favor a rebate if they are concerned that a deductible discourages (low income) individuals from using necessary care.

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Tables

Table 2: Descriptive statistics of full sample after cleaning

	Mean	Minimum	Maximum
Total healthcare expenditure (euro)	1920	0	2253745
	(6346)		
GP care (euro)	70	0	39844
	(107)		
Hospital care (euro)	1101	0	2234379
	(4838)		
Physiotherapy (euro)	28	0	34796
	(208)		
Pharmaceutical care (euro)	299	0	728415
	(1381)		
Mental health care (euro)	180	0	1217864
	(2831)		
Dental care (euro)	40	0	28002
	(221)		
Other care (euro)	151	0	951926
	(907)		
Age (years)	41	0	115
Male (%)	0.49	0	1
Voluntary deductible (%)	0.05	0	1
Diagnosis cost-related group (%)	0.06	0	1
Pharmaceutical cost-related group (%)	0.22	0	1
Movers (%)	0.07	0	1
Household income quintile	3.09	1	5
Number of observations	130,225,484		

Notes: Standard deviations are reported between parentheses. The category 'other care' includes costs of paramedical care, medical aids, transportation costs of patients, care that is provided over the Dutch borders, geriatric revalidation, and other healthcare costs that do not apply to any of the cost categories listed in Appendix B. Household income is the average standardized disposable household income.

Table 3: Descriptive statistics of baseline sample

	15 to 17 years	19 to 21 years
Healthcare expenditure with cost-sharing (euro)	532	565
	(3557)	(3262)
Of which:		
Hospital care (euro)	362	427
	(3077)	(2900)
Physiotherapy (euro)	37	7
	(148)	(104)
Pharmaceutical care (euro)	89	95
	(1278)	(1016)
Other care (euro)	52	45
	(635)	(473)
Age (years)	16	20
Male (%)	0.51	0.51
Diagnosis cost-related group (%)	0.01	0.01
Pharmaceutical cost-related group (%)	0.02	0.03
Movers (%)	0.04	0.15
Household income quintile	3.19	2.93
Number of observations	3,335,061	3,218,518

Notes: Standard deviations are reported between parentheses. The mean values are calculated from 2006 to 2013. Healthcare expenditure with cost-sharing excludes users of mental care between 2008 and 2013, dental healthcare costs, and individuals with a voluntary deductible. Household income is the average standardized disposable household income. This is the sample of our baseline specification.

Table 4: Mean healthcare expenditure with cost-sharing (in euros) for 17 and 19 year olds

	17	19
2006	500	498
	(3333)	(3597)
2007	518	508
	(2845)	(3562)
2008	529	521
	(2920)	(3355)
2009	598	552
	(3763)	(2680)
2010	574	548
	(2774)	(2790)
2011	609	591
	(3015)	(3266)
2012	662	590
	(6140)	(3353)
2013	611	576
	(4349)	(3414)

Notes: Standard deviations are reported between parentheses. We only report expenditures that apply to the rebate or deductible and exclude dental costs. Persons with any mental care between 2006 and 2013 in our age bandwidth have also been excluded from the sample. Finally, individuals with a voluntary deductible have been excluded.

Table 5: Regression discontinuity results of baseline specification

	(1)	(2)
τ_t (Rebate)		
2006	-54.3***	-68.1***
	(8.7)	(13.1)
2007	-71.7***	-76.2***
	(8.9)	(10.9)
τ_t (Deductible)		
2008	-81.7***	-77.8***
	(8.7)	(9.4)
2009	-95.2***	-75.4***
	(8.2)	(8.3)
2010	-90.8***	-73.3***
	(7.9)	(8.8)
2011	-98.4***	-90.5***
	(8.4)	(10.0)
2012	-118.5***	-123.8***
	(11.1)	(14.7)
2013	-135.2***	-152.4***
	(9.7)	(15.4)
Age centered	48.0***	58.4***
	(2.3)	(22.2)
Age centered * male	-10.6***	-52.3*
	(2.3)	(31.4)
Age centered * treatment	1.9	-10.0**
	(3.3)	(4.5)
Age centered * treatment * male	-36.3***	-14.3**
	(4.2)	(5.9)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	6,787,082	6,787,082
R-squared	0.000	0.687

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects (see equation (1)). The dependent variable y_{it} is healthcare expenditures with cost-sharing (excluding dental care).

Table 6: Differences between estimated τ_t with τ_{2006} and τ_{2007} from Table 5 column (2)

	τ_{2006}	τ_{2007}
Difference with:		
τ_{2006}	-	8.1
τ_{2007}	-8.1	-
τ_{2008}	-9.7	-1.6
τ_{2009}	-7.3	0.8
τ_{2010}	-5.2	2.9
τ_{2011}	-22.4	-14.3
τ_{2012}	-55.7***	-47.6**
τ_{2013}	-84.3***	-76.2***

Notes: This Table reports the differences between the τ coefficients that we presented in Table 5. In the first two columns, we show the difference between τ_{2006} and the τ values of the other years. The last column shows the difference with τ_{2007}. The coefficients are estimated with individual fixed effects. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively.

Table 7: Results of regression discontinuity estimations of rebate (γ) and deductible (δ) in equation (2)

	(1)	(2)
γ (Rebate)	-0.17***	-0.26***
	(-0.03)	(-0.03)
δ (Deductible)	-0.40***	-0.44***
	(-0.03)	(-0.03)
Age centered	43.42***	56.61**
	(-2.16)	(-22.37)
Age centered * male	-10.55***	-52.39*
	(-2.32)	-31.37
Age centered * treatment	1.9	-10.31**
	(-3.34)	(-4.23)
Age centered * treatment * male	-36.4***	-14.3**
	(-4.21)	(-5.92)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	6,787,082	6,787,082
R-squared	0.000	0.687

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects (see equation (2)). The dependent variable y_{it} is the same as in Table 5.

Table 8: Estimated coefficients γ and δ for multiple groups

	Estimated γ (Rebate)		Estimated δ (Deductible)		Mean	Observations
	(1)	(2)	(3)	(4)	expenditure	
Baseline	-0.17***	-0.26***	-0.40***	-0.44***	559	6,787,082
	(0.03)	(0.03)	(0.03)	(0.03)		
Men	-0.14***	-0.21***	-0.35***	-0.40***	518	3,473,759
	(0.05)	(0.05)	(0.04)	(0.05)		
Women	-0.22***	-0.32***	-0.45***	-0.49***	601	3,313,323
	(0.04)	(0.04)	(0.04)	(0.05)		
Household income:						
Quintile 1	-0.11	-0.13*	-0.57***	-0.41***	555	1,276,528
	(0.07)	(0.08)	(0.08)	(0.09)		
Quintile 2	-0.09	-0.27***	-0.37***	-0.39***	563	1,259,121
	(0.07)	(0.07)	(0.07)	(0.07)		
Quintile 3	-0.21***	-0.25***	-0.34***	-0.46***	564	1,368,067
	(0.06)	(0.06)	(0.07)	(0.07)		
Quintile 4	-0.23***	-0.25***	-0.35***	-0.40***	558	1,414,671
	(0.06)	(0.07)	(0.06)	(0.07)		
Quintile 5	-0.26***	-0.38***	-0.34***	-0.48***	564	1,384,918
	(0.07)	(0.07)	(0.06)	(0.07)		

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1,3) are estimated with ordinary least squares, and (2,4) include individual fixed effects. The dependent variable y_{it} is the same as in table 5. The Table shows the estimated coefficients of γ and δ. The estimated coefficients of the other variables are available upon request.

Table 9: Regression discontinuity results for two bandwidths and six functional forms

Year	Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)
2006	3 years	-54.0***	**-54.3*** **	-54.0***	-56.0***	-55.3***	-70.1***
		(8.8)	(8.7)	(8.7)	(15.9)	(12.9)	(16.0)
	5 years	-68.9***	-68.7***	-68.5***	**-71.5*** **	-70.9***	-78.3***
		(6.6)	(6.6)	(6.6)	(9.8)	(8.5)	(17.5)
2007	3 years	-72.6***	**-71.7*** **	-71.8***	-72.9***	-73.1***	-86.9***
		(8.9)	(8.9)	(8.9)	(15.5)	(12.6)	(15.8)
	5 years	-81.7***	-80.4***	-80.6***	**-83.0*** **	-83.1***	-89.6***
		(6.4)	(6.4)	(6.4)	(9.5)	(8.2)	(17.2)
2008	3 years	-82.5***	**-81.7*** **	-81.9***	-83.0***	-83.1***	-97.0***
		(8.7)	(8.7)	(8.7)	(15.6)	(12.7)	(16.0)
	5 years	-85.1***	-84.1***	-84.3***	**-86.6*** **	-86.7***	-93.2***
		(6.3)	(6.3)	(6.3)	(9.2)	(8.0)	(17.0)
2009	3 years	-95.6***	**-95.2*** **	-95.2***	-96.4***	-96.5***	-110.4***
		(8.2)	(8.2)	(8.2)	(15.3)	(12.3)	(15.8)
	5 years	-83.2***	-82.8***	-82.9***	**-85.2*** **	-85.3***	-91.9***
		(6.2)	(6.2)	(6.2)	(9.0)	(7.8)	(17.1)
2010	3 years	-91.1***	**-90.8*** **	-90.9***	-92.0***	-92.1***	-106.0***
		(7.9)	(7.9)	(7.9)	(15.1)	(12.1)	(15.8)
	5 years	-70.9***	-70.7***	-70.7***	**-73.2*** **	-73.1***	-79.8***
		(6.2)	(6.2)	(6.2)	(9.1)	(7.8)	(16.9)
2011	3 years	-98.5***	**-98.4*** **	-98.4***	-99.6***	-99.7***	-113.6***
		(8.4)	(8.4)	(8.4)	(15.2)	(12.3)	(15.8)
	5 years	-78.8***	-78.6***	-78.5***	**-81.1*** **	-80.9***	-87.8***
		(6.3)	(6.3)	(6.3)	(9.4)	(8.1)	(16.8)
2012	3 years	-118.8***	**-118.5*** **	-118.6***	-119.9***	-119.9***	-133.9***
		(11.1)	(11.1)	(11.1)	(17.0)	(14.4)	(18.2)
	5 years	-98.6***	-98.3***	-98.3***	**-100.8*** **	-100.6***	-107.5***
		(8.0)	(8.0)	(8.0)	(10.6)	(9.5)	(18.3)
2013	3 years	-135.3***	**-135.2*** **	-135.2***	-136.4***	-136.5***	-150.4***
		(9.7)	(9.7)	(9.7)	(16.3)	(13.4)	(17.0)
	5 years	-131.6***	-131.1***	-131.1***	**-133.6*** **	-133.5***	-140.3***
		(7.3)	(7.3)	(7.3)	(10.2)	(9.0)	(17.6)

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. The dependent variable y_{it} is the same as in Table 5. Ordinary least squares estimations are performed for two bandwidths (15 to 21 year olds and 13 to 23 year olds) and six functional forms. Estimations in bold indicate the best specification. For a three-year bandwidth, the F-test, testing for the best functional form, showed the linear model with an interaction to be the best model, with a p-value of 0.061. For a five-year bandwidth, the quadratic model with an interaction, with a p-value of 0.076. Model (1) is a linear specification, (2) is linear with interactions, (3) and (4) are quadratic specifications without and with interactions, respectively, and (5) and (6) are a cubic model and a cubic model with interactions, respectively. Quartic and quintic models were also estimated but they did not improve the specification. Specifications that include an interaction allow for a different slope before and after the discontinuity.

Table 10: Regression discontinuity results without individuals who moved

	(1)	(2)
τ_t (Rebate)		
2006	-50.0***	-63.8***
	(9.0)	(14.5)
2007	-73.8***	-76.0***
	(9.5)	(12.3)
τ_t (Deductible)		
2008	-76.9***	-74.0***
	(9.4)	(10.5)
2009	-92.5***	-73.5***
	(8.7)	(9.2)
2010	-88.2***	-71.7***
	(8.4)	(9.8)
2011	-90.1***	-89.4***
	(8.9)	(11.2)
2012	-110.0***	-122.8***
	(11.9)	(16.2)
2013	-125.3***	-150.7***
	(10.5)	(16.9)
Age centered	48.1***	53.7
	(2.4)	(55.4)
Age centered * male	-9.5***	-60.4*
	(2.4)	(35.9)
Age centered * treatment	-1.9	-15.1***
	(3.6)	(5.1)
Age centered * treatment * male	-35.3***	-9.5
	(4.4)	(6.6)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	6,212,864	6,212,864
R-squared	0.000	0.697

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects. The dependent variable y_{it} is the same as in Table 5.

Table 11: Share of individuals no longer living with their parents by year and age

	2008	2009	2010	2011	2012	2013
15 years old	0.02	0.02	0.02	0.02	0.02	0.02
16 years old	0.03	0.03	0.03	0.03	0.03	0.03
17 years old	0.04	0.04	0.04	0.04	0.04	0.04
18 years old	0.15	0.14	0.14	0.14	0.13	0.13
19 years old	0.25	0.25	0.25	0.24	0.24	0.23
20 years old	0.34	0.35	0.35	0.34	0.33	0.32
21 years old	0.43	0.43	0.43	0.43	0.41	0.41

Note: These data were taken from StatLine, the open data source of Statistics Netherlands, on March 30, 2017. The shares are based on the entire Dutch population. The original data show the number of individuals in the Netherlands by age and year in total, and of that group those who still live at home with their parents. We took the ratio of these two and inverted it, to reflect the share of individuals, by age and year, who do not live with their parents anymore. The data reflect the number of individuals on January 1st of each year.

Table 12: Share of individuals per education level by year and age

	school	age in years						
	year	15	16	17	18	19	20	21
	2007-2008	1.00	0.78	0.42	0.13	0.02	0.00	0.00
	2008-2009	0.99	0.78	0.42	0.13	0.02	0.00	0.00
	2009-2010	0.99	0.78	0.43	0.13	0.02	0.00	0.00
secondary education	2010-2011	0.96	0.78	0.43	0.14	0.02	0.00	0.00
	2011-2012	0.96	0.76	0.44	0.14	0.02	0.00	0.00
	2012-2013	0.97	0.77	0.43	0.14	0.02	0.00	0.00
	2013-2014	1.00	0.76	0.43	0.14	0.02	0.00	0.00
	2007-2008	0.00	0.22	0.49	0.56	0.53	0.42	0.30
	2008-2009	0.00	0.22	0.48	0.56	0.51	0.40	0.29
	2009-2010	0.00	0.21	0.48	0.54	0.51	0.40	0.29
vocational education	2010-2011	0.00	0.21	0.47	0.54	0.50	0.39	0.29
	2011-2012	0.00	0.20	0.46	0.54	0.49	0.39	0.28
	2012-2013	0.00	0.20	0.46	0.54	0.49	0.38	0.28
	2013-2014	0.00	0.21	0.46	0.53	0.48	0.37	0.28
	2007-2008	0.00	0.00	0.07	0.18	0.27	0.38	0.46
	2008-2009	0.00	0.00	0.07	0.18	0.27	0.38	0.46
	2009-2010	0.00	0.00	0.07	0.18	0.28	0.38	0.46
university of applied sciences	2010-2011	0.00	0.00	0.07	0.18	0.28	0.39	0.46
	2011-2012	0.00	0.00	0.07	0.18	0.29	0.39	0.46
	2012-2013	0.00	0.00	0.07	0.18	0.29	0.39	0.47
	2013-2014	0.00	0.00	0.07	0.19	0.30	0.40	0.47
	2007-2008	0.00	0.00	0.00	0.09	0.16	0.19	0.23
	2008-2009	0.00	0.00	0.00	0.09	0.16	0.20	0.24
	2009-2010	0.00	0.00	0.00	0.09	0.17	0.20	0.24
university	2010-2011	0.00	0.00	0.00	0.09	0.17	0.21	0.24
	2011-2012	0.00	0.00	0.00	0.09	0.17	0.21	0.25
	2012-2013	0.00	0.00	0.00	0.09	0.17	0.21	0.25
	2013-2014	0.00	0.00	0.00	0.10	0.18	0.22	0.25

Note: These data were taken from StatLine, the open data source of Statistics Netherlands, on April 2nd 2017. The shares are based on the entire Dutch population.

Table 13: Regression discontinuity results without 17 year olds

	(1)	(2)
τ_t (Rebate)		
2006	-40.8***	-63.5***
	(11.5)	(19.5)
2007	-65.1***	-71.4***
	(11.8)	(17.3)
τ_t (Deductible)		
2008	-74.5***	-73.3***
	(11.8)	(16.1)
2009	-79.9***	-63.8***
	(10.8)	(13.4)
2010	-82.5***	-61.1***
	(11.1)	(12.9)
2011	-81.3***	-79.5***
	(11.4)	(14.5)
2012	-97.0***	-104.1***
	(13.2)	(18.3)
2013	-128.8***	-140.0***
	(12.0)	(22.0)
Age centered	42.9***	55.5**
	(3.5)	(25.6)
Age centered * male	-9.6***	-58.3
	(2.5)	(43.5)
Age centered * treatment	8.3*	-3.6
	(4.2)	(6.5)
Age centered * treatment * male	-39.8***	-19.5***
	(4.5)	(6.9)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	5,681,632	5,681,632
R-squared	0.000	0.735

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, (2) includes individual fixed effects (see equation1). The dependent variable y_{it} is the same as in Table 5. Persons aged 17 were excluded from the analysis.

Table 14: Results of regression discontinuity estimations with one coefficient for the rebate and the deductible without 17 year olds

	(1)	(2)
γ (Rebate)	-0.12***	-0.23***
	(-0.04)	(-0.04)
δ (Deductible)	-0.33***	-0.38***
	(-0.04)	(-0.05)
Age centered	35.18***	52.61**
	(-2.96)	(-25.73)
Age centered * male	-9.50***	-58,51
	(-2.47)	(-43.52)
Age centered * treatment	14.41***	-1,87
	(-3.83)	(-4.93)
Age centered * treatment * male	-39.91***	-19.54***
	(-4.54)	(-6.91)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	5,681,632	5,681,632
R-squared	0	0.735

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects (see equation 2). The dependent variable y_{it} is the same as in Table 5. Persons aged 17 were excluded from the analysis.

Table 15: Regression discontinuity results including individuals who have used mental healthcare

	(1)	(2)
τ_t (Rebate)		
2006	-80.2***	-61.0***
	(8.6)	(13.3)
2007	-88.4***	-69.6***
	(8.8)	(11.5)
τ_t (Deductible)		
2008	-90.6***	-76.1***
	(8.9)	(10.0)
2009	-94.9***	-75.1***
	(8.4)	(8.7)
2010	-91.8***	-83.3***
	(7.9)	(9.2)
2011	-81.7***	-95.3***
	(8.2)	(10.1)
2012	-96.0***	-130.5***
	(10.6)	(14.0)
2013	-112.6***	-165.7***
	(9.7)	(15.0)
Age centered	55.3***	62.9***
	(2.3)	(21.0)
Age centered * male	-13.8***	-63.2**
	(2.3)	(28.3)
Age centered * treatment	-12.4***	-15.1***
	(3.3)	(4.8)
Age centered * treatment * male	-34.4***	-9.1
	(4.2)	(6.0)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	7,920,076	7,920,076
R-squared	0.001	0.667

Notes: The analyses are the same as Table 5, but include individuals who used some mental healthcare between 2006 and 2013 (mental healthcare costs have been excluded, but the individuals are kept in the sample). The dependent variable y_{it} is healthcare expenditure with cost-sharing (excluding dental and mental care). Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects.

Table 16: Regression discontinuity results with a correction for a general price effect

	(1)	(2)
τ_t (Rebate)		
2006	-43.3***	-72.9***
	(9.5)	(14.2)
2007	-65.5***	-81.9***
	(9.5)	(11.8)
τ_t (Deductible)		
2008	-77.9***	-81.5***
	(9.2)	(10.1)
2009	-97.5***	-81.5***
	(7.9)	(8.4)
2010	-91.3***	-73.5***
	(7.9)	(8.7)
2011	-101.8***	-87.0***
	(8.1)	(9.4)
2012	-122.5***	-114.0***
	(10.0)	(13.3)
2013	-136.5***	-134.5***
	(9.3)	(14.4)
Age centered	46.6***	53.5**
	(2.3)	(22.4)
Age centered * male	-8.3***	-50.2*
	(2.4)	(29.5)
Age centered * treatment	5.6*	-15.6***
	(3.4)	(4.5)
Age centered * treatment * male	-41.4***	-11.9**
	(4.5)	(5.8)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	6,787,082	6,787,082
R-squared	0.000	0.690

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares, and (2) includes individual fixed effects. The dependent variable y_{it} is the same as in Table 5 but now corrected for prices.

Table 17: Standard errors for different clusters

	(1)	(2)	(3)	(4)
τ_t (Rebate)				
2006	13.1	3.9	7.1	10.7
2007	10.9	5.3	5.9	9.3
τ_t (Deductible)				
2008	9.4	6.1	5.8	8.4
2009	8.3	6.6	6.7	7.8
2010	8.8	4.9	6.4	8.0
2011	10.0	5.8	6.8	9.2
2012	14.7	8.6	9.4	12.0
2013	15.4	5.4	9.6	12.2

Notes: The analyses that were conducted for this Table are the same as table 5, column (2), but vary in the way we clustered the standard errors. For all four columns, the coefficients of τ_t are the same as in column (2) in Table 5. All analyses include fixed effects and the dependent variable y_{it} is the same as in Table 5. (1) is estimated with standard errors clustered by individuals, (2) clustered by age, (3) clustered by age cohort (age x birth year) and (4) clustered by individual x age.

Table 18: Regression discontinuity results for fictional discontinuity at 24

	(1)	(2)
τ_t (Rebate)		
2006	18.9**	11.9
	(8.6)	(14.2)
2007	3.6	-7.8
	(9.1)	(12.3)
τ_t (Deductible)		
2008	-18.3**	-23.4**
	(8.1)	(10.0)
2009	1.5	-3.2
	(8.0)	(9.4)
2010	-8.0	-10.2
	(8.2)	(9.9)
2011	-8.8	-7.9
	(9.4)	(12.4)
2012	11.3	12.2
	(10.2)	(14.1)
2013	-0.2	8.2
	(9.9)	(16.1)
Age centered	14.9***	47.5
	(2.2)	(65.4)
Age centered * male	2.9	-47.3
	(2.4)	(38.5)
Age centered * treatment	92.3***	51.5***
	(3.3)	(5.2)
Age centered * treatment * male	-125.1***	-45.4***
	(4.3)	(6.2)
Year dummies	Yes	Yes
Individual fixed effects	No	Yes
Constant	Yes	Yes
Observations	6.673.554	6.673.554
R-squared	0.003	0.656

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two-sided test at the .10, .05, and .01 levels, respectively. (1) is estimated with ordinary least squares and (2) includes individual fixed effects. The analyses are performed for individuals aged 21 to 27. τ_t is a fictional discontinuity placed at 24 years old. The dependent variable y_{it} is the same as in Table 5.

A. Overview of policy changes between 2006 and 2013

Year	Policy change
2006	Introduction of managed competition 'Health Insurance Act' (Zvw)
2006	Agreement to curb pharmaceutical costs (extension of 'preferentiebeleid geneesmiddelen')
2006	**Introduction of rebate of 255 euros**
2007	Abdominoplasty (for severe cases) is included in basic package
2007	Psychotherapy (for severe cases) is included in basic package
2007	First IVF treatment (of maximum 3) is included in basic package
2008	**Introduction of mental healthcare in 'Health Insurance Act' (Zvw)**
2008	Contraceptives are included in the basic package
2008	**Limited dental care for 18 to 22 year olds included in basic package. The deductible does not apply to dental care.**
2008	Five hours of extra maternity care are included in basic package
2008	The first 8 sessions of psychological counseling are included in the basic package plus co-payment of 10 euros per session
2008	**Introduction deductible of 150 euros**
2009	Chairs to help a person stand up ('sta op stoelen'), strollers, and anti-allergen matress covers removed from the basic benefit package
2009	Reimbursement for statins limited
2009	Sleeping pills and tranquilizers removed from the basic package
2009	Severe dyslexia diagnostics and treatment for 6 and 7 year olds included in basic package
2009	**Increase of deductible to 155 euros**
2010	Introduction of diagnosis treatment combinations (DBCs)
2010	Acetylcysteine removed from basic package
2010	Lowering of registration fee for general practitioner
2010	Severe dyslexia diagnostics and treatment for 9 year olds included in basic package
2010	More precise requirements about reimbursement of IVF treatments
2010	Maximal reimbursement of wigs increases from 294 euros to 374 euros
2010	MRA machine is reimbursed in specific cases
2010	Reimbursement of devices to ease breathing in specific cases included

2010	Anti-snoring device ('snurkbeugel') included in basic package for specific cases
2010	**Increase of deductible to 165 euros**
2011	Contraceptives for individuals aged over 21 years removed from the basic benefit package
2011	**Dental care for 18 to 21 year olds removed**
2011	Stricter indication for anti-depressants
2011	Physiotherapy limited: patient must pay for first 12 sessions (it used to be the first 8 sessions)
2011	Physical therapy for urine incontinence included in basic package
2011	Uncomplicated dental extraction by dental surgeon removed from basic package
2011	Quit smoking treatments included in basic package
2011	**Increase of deductible to 170 euros**
2012	**Additional deductible for specialist mental healthcare introduced**
2012	Gastricacid blockers removed from basic package
2012	Physiotherapy (first 20 sessions) removed from basic package
2012	Treatments to quit smoking removed from basic package
2012	Dietary advice removed from basic package
2012	Treatment of adjustment disorders (mental healthcare) removed
2012	Primary psychological care reduced from 8 to 5 sessions
2012	**Increase of deductible to 220 euros**
2013	Paracetamol-codeine combination medication removed
2013	Co-payment of 25 percent for hearing aids introduced to replace the fixed fee of 500 euros
2013	Co-payment of 7.50 euros per day for 'hotel' costs in hospital or other overnight stay
2013	Simple walking aids removed from basic package
2013	Repositioning helmet for babies removed from basic package
2013	Treatments to quit smoking included
2013	**Co-payments for specialist mental healthcare abolished**
2013	IVF treatment for women aged 43 years and over removed from basic package
2013	Geriatric rehabilitation care switched from Exceptional Medical Expenses Act (AWBZ) to Health Insurance Act (Zvw)
2013	**Increase of the deductible to 350 euros**

Notes: This list is an adaptation of Kroneman and Jong (2015). We have emphasized those policy changes that we think are important to our study.

B. List of healthcare expenditure categories

Type of costs	Apply to deductible or rebate	Included in y_{it}
GP registration		
GP visits		
Other costs of GP care		
Pharmaceutical care	X	X
Dental care	X	
Obstetrical care		
Hospital care	X	X
Physiotherapy	X	X
Paramedical care	X	X
Medical aids	X	X
Transportation for persons lying down	X	X
Transportation for seated persons	X	X
Maternity care		
Care that is delivered over the Dutch borders	X	X
Primary healthcare support		
Primary mental healthcare support		
Mental healthcare with (overnight) stay	X	
Mental healthcare without (overnight) stay:		
- at institutions	X	
- by self-employed providers	X	
Other mental healthcare costs	X	
Geriatric revalidation	X	X
Other costs	X	X

Notes: Cost categories marked with X in the second column apply to the rebate or deductible. The other cost categories are exempted from these cost-sharing instruments. y_{it} in the third column refers to the dependent variable in our baseline specification. See equation (1) in Section 4. The cost categories marked with an 'X' in the third column are included in y_{it} for the analyses in, for example, Table 5.

C. Density functions expenditures

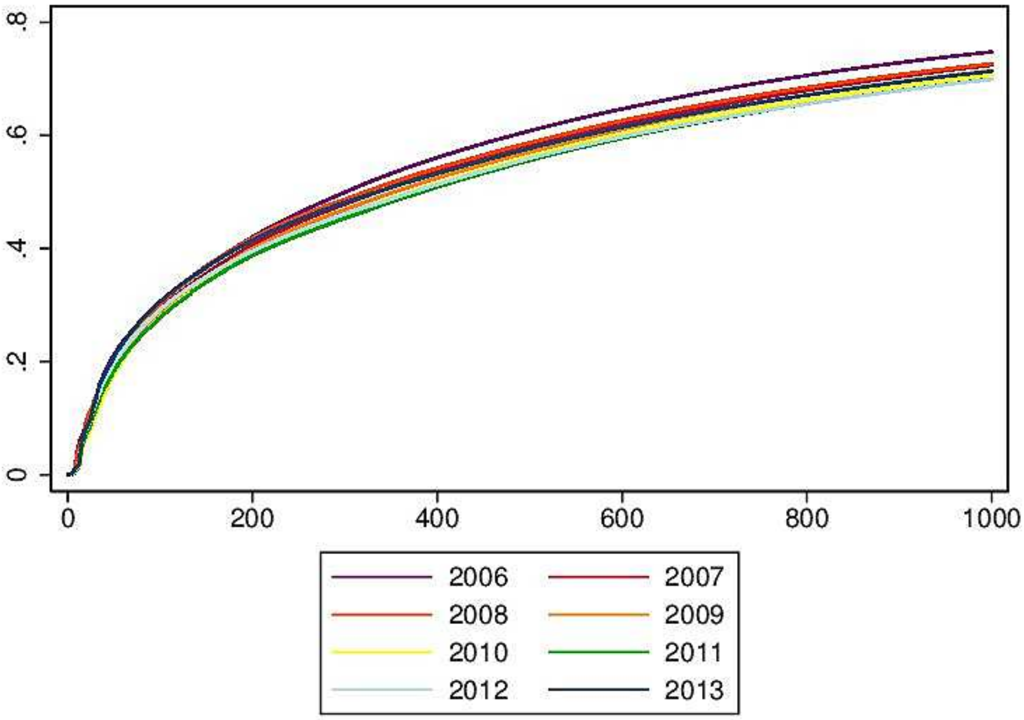

Figure 6: Cumulative density distribution of healthcare expenditure with cost-sharing (y) of 18-65 year olds between 0 and 1000 euros for years 2006-2013

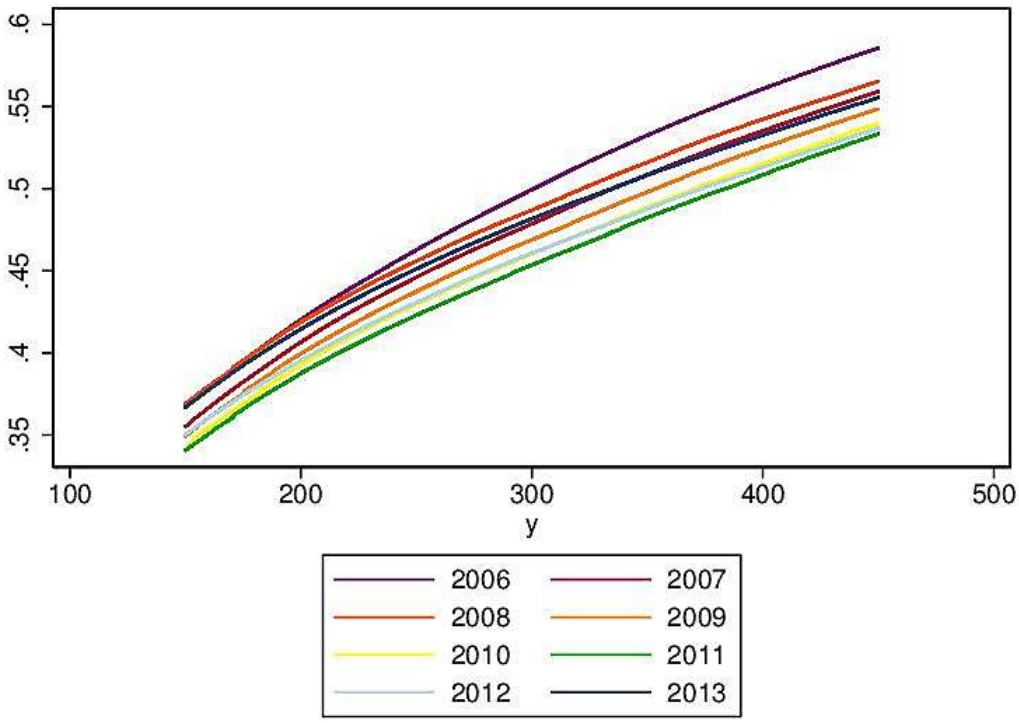

Figure 7: Cumulative density distribution of healthcare expenditure with cost-sharing (y) of 18-65 year olds between 150 and 450 euros for years 2006-2013

D. Regression discontinuity results for two bandwidths and six functional forms (FE)

Year	Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)
2006	3 years	-39.0***	**-68.1*****	-65.6***	-67.3***	-66.5***	-74.3***
		(11.0)	(13.1)	(13.1)	(19.8)	(16.8)	(24.7)
	5 years	-51.1***	-82.2***	-66.7***	**-69.6*****	-66.3***	-84.5***
		(7.2)	(9.6)	(9.7)	(12.3)	(11.2)	(20.0)
2007	3 years	-54.9***	**-76.2*****	-74.3***	-76.2***	-75.2***	-83.2***
		(9.8)	(10.9)	(10.9)	(17.9)	(14.7)	(23.2)
	5 years	-61.9***	-87.1***	-74.3***	**-78.2*****	-74.1***	-93.4***
		(6.7)	(8.1)	(8.1)	(10.6)	(9.4)	(19.0)
2008	3 years	-65.2***	**-77.8*****	-76.8***	-77.7***	-77.7***	-84.7***
		(9.0)	(9.4)	(9.4)	(16.8)	(13.5)	(22.7)
	5 years	-64.6***	-81.0***	-72.8***	**-75.1*****	-72.6***	-90.1***
		(6.5)	(6.8)	(6.8)	(9.3)	(8.0)	(18.1)
2009	3 years	-70.4***	**-75.4*****	-75.0***	-76.1***	-75.9***	-83.1***
		(8.4)	(8.3)	(8.3)	(16.4)	(12.9)	(22.1)
	5 years	-74.0***	-80.2***	-77.1***	**-77.5*****	-76.9***	-92.4***
		(6.1)	(6.0)	(6.0)	(8.7)	(7.3)	(18.4)
2010	3 years	-76.9***	**-73.3*****	-73.6***	-74.4***	-74.5***	-81.4***
		(8.7)	(8.8)	(8.8)	(16.6)	(13.2)	(22.2)
	5 years	-78.0***	-73.2***	-75.6***	**-73.8*****	-75.4***	-88.8***
		(6.0)	(6.2)	(6.3)	(9.2)	(7.8)	(18.4)
2011	3 years	-102.4***	**-90.5*****	-91.5***	-92.3***	-92.3***	-99.3***
		(9.2)	(10.0)	(10.0)	(17.8)	(14.5)	(23.2)
	5 years	-96.3***	-80.9***	-88.7***	**-84.8*****	-88.4***	-99.8***
		(6.1)	(7.2)	(7.4)	(10.6)	(9.3)	(18.8)
2012	3 years	-144.9***	**-123.8*****	-125.6***	-125.7***	-126.5***	-132.6***
		(13.8)	(14.7)	(14.9)	(21.9)	(19.0)	(27.3)
	5 years	-126.4***	-102.0***	-114.4**	**-108.8*****	-114.0***	-123.8***
		(8.6)	(10.1)	(10.4)	(13.3)	(12.1)	(21.2)
2013	3 years	-181.2***	**-152.4*****	-154.8***	-155.0***	-155.7***	-161.9***
		(13.8)	(15.4)	(15.6)	(22.7)	(19.8)	(27.8)
	5 years	-161.4***	-130.7***	-146.0***	**-141.2*****	-145.5***	-156.2***
		(8.4)	(10.9)	(11.3)	(13.8)	(12.7)	(21.2)

Notes: Standard errors are reported between parentheses and clustered at the individual level. *, **, and *** indicate significance based on a two sided test at the .10, .05, and .01 levels, respectively. The dependent variable y_{it} is the same as in Table 5. Fixed effects estimations are performed for three bandwidths (15 to 21 year olds and 13 to 23 year olds) and six functional forms. Estimations in bold indicate the best specification. For a three-year bandwidth, the F-test, testing for the best functional form, showed the linear model with an interaction to be the best model, with a p-value of 0.593. For a five-year bandwidth, the quadratic model with an interaction, with a p-value of 0.710. Model (1) is a linear specification, and (2) is linear with interactions, (3) and (4) are quadratic specifications without and with interactions, respectively, and (5) and (6) are a cubic model and a cubic model with interactions, respectively. Quartic and quintic models were also estimated but they did not improve the specification. Specifications that include an interaction allow for a different slope before and after the discontinuity.