Patient Cost-Sharing, Socioeconomic Status, and Children's Health Care Utilization*

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Abstract

This paper estimates the effect of cost-sharing on the demand for children's and adolescents' use of medical care. We use a large population-wide registry dataset including detailed information on contacts with the health care system as well as family income. Two different estimation strategies are used: regression discontinuity design exploiting age thresholds above which fees are charged, and difference-in-differences models exploiting policy changes. We also estimate combined regression discontinuity difference-in-differences models that take into account discontinuities around age thresholds caused by factors other than cost-sharing. We find that when care is free of charge, individuals increase their number of visits by 5-10 percent. Effects are similar in middle childhood and adolescence, and are driven by those from low-income families. The differences across income groups cannot be explained by other factors that correlate with income, such as maternal education.

Keywords: Cost-sharing, Health care utilization, Children, Income-health gradient, Difference-in-differences, Regression discontinuity

JEL Classification: I13, I14, I18

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1 Introduction

A growing literature shows that health in childhood has strong long-term impacts on both socioeconomic status (SES) and health in adulthood (e.g., Case et al. 2005; Smith 2009; Lundborg et al. 2014a). Consistent with this, many government-funded health insurance programs, such as Medicaid and CHIP in the U.S., exempt children from most cost sharing requirements, thus increasing incentives to seek necessary care. However, there is little evidence on if, or the extent to which, the price of health care actually poses a barrier to utilization among young individuals.

If parental SES influences child health outcomes, health could also be one important channel governing the intergenerational transmission of SES (Currie 2009). A large literature documents that parental SES is positively correlated with child health outcomes, and work including Milligan and Stabile (2011), Lundborg et al. (2014b) and Kuehnle (2014) provides evidence that the relationship is causal. This income gradient in child health is not only driven by higher arrival rates of health shocks among poor children due to, for instance, inappropriate nutrition (the so-called prevalence effect), but also reflects a more adverse response to health shocks once they are present (Case et al. 2002; Currie and Stabile 2003; Reinhold and Jürges 2012). This so-called severity effect might result from under-use of medical care, perhaps because children in poor families fail to adhere to therapy, but also because cost-sharing deters them from seeking care in the first place.

In this paper, we study if cost-sharing that comes in the form of copayments (per-visit fees) influences children's and adolescents' health care utilization. We also investigate whether the response to cost-sharing varies with parental SES, such as family income, or with health.

We exploit age thresholds below which copayments are not charged, and estimate regression discontinuity (RD) models. The thresholds were either at age 7 or age 20, depending on the time period of study. As a complement, we exploit reforms that changed the threshold – first from 20 to 7 in 1999, and then back from 7 to 20 in 2002. We exploit these reforms both to estimate combined regression RD-DiD models, and to estimate standard DiD models, with control groups consisting either of 6-year-olds or 20-year-olds. While the standard DiD estimation relies on the assumption that unobserved factors in the treatment and control groups develop in parallel, it allows us to estimate effects for individuals in the whole treatment group, and not only around the thresholds. The combined RD-DiD model relies on the assumption that any discontinuities other than those not related to cost-sharing at the age threshold are the same in the treatment and control period. Our analyses make use of a large full-population administrative dataset, which in an unprecedented way merges information on health care visits with socioeconomic information on individuals.

Our results show that when health care is free of charge, doctor visits increase by 5-10 percent. The estimates from the DiD models are similar for younger and older individuals (children aged 6-12 and adolescents aged 13-19), and they are also similar to the ones based on the RD (or RD-DiD) design. The finding that cost-sharing substantially impacts the health care utilization also of children and adolescents is our first important contribution to the literature.

As the second major contribution of the paper, we show that the response is driven by low-income individuals. This difference across income groups cannot be explained by family status, maternal education, or mothers that stay at home. Very few previous studies have been able to examine heterogeneous responses by income credibly, not even among adults (Baicker and Goldman 2011). We improve on previous studies because, in contrast to them, we are able to observe income at the individual level and we study the whole population rather than selected subgroups.

The rest of the paper is organized as follows. Section 2 provides an overview of previous studies estimating the price sensitivity of health care demand. Section 3 gives information on the institutional setting, on the data we use, and on our econometric approach. In Section 4, we show and discuss our results. Section 5 concludes.

2 Previous work

While there are numerous empirical studies that estimate how cost-sharing affects the demand for health care among adults,[1] only little evidence exists for children. One of the most credible estimates comes from the Rand Health Insurance Experiment (hereafter Rand HIE; Newhouse and the Insurance Experiment Group 1993). Conducted in the 1970s, the Rand HIE randomly assigned families to health insurance plans with different levels of cost-sharing. Results were translated into a widely cited overall price elasticity of -0.2, a response that was found to be about the same for adults and children (Keeler and Rolph 1988).

Subsequently to the Rand HIE, a number of studies have used policy changes to estimate how health care demand responds to cost-sharing. For example, several studies have considered how expansion of public health insurance affected utilization rates (Finkelstein 2007; Dafny and Gruber 2005; Kolstad and Kowalski 2012; Kondo and Shigeoka 2013).[2] Among studies that consider changes in cost sharing in the form of copayments, both Cherkin et al. (1989) and Selby et al. (1996) found that patients seek less care when copayments are introduced, and the pattern of the response was found to be similar for children and adults. However, these studies, as well as the Rand HIE, suffered from the problem that

1. For reviews of this literature, see Chandra et al. (2007), Swartz (2010), or Baicker and Goldman (2011).

2. Two randomized experiments have also been carried out more recently. Michalopoulos et al. (2011) studied the Accelerated Benefits Demonstration, which provided medical benefits to Social Security Disability Insurance beneficiaries immediately rather than after a 24month waiting time. Finkelstein et al. (2012) studied the Oregon Health Insurance Experiment which allowed a group of uninsured low-income adults to apply for Medicaid. Moreover, a few studies (Card et al. 2008; Anderson et al. 2012, 2014; Shigeoka 2014; Han et al. 2016) have exploited age thresholds in health insurance coverage.

changes in cost-sharing affected whole families. If parents face no simultaneous increases in cost-sharing and the family budget constraint is not tightened as much, cost-sharing might have a different effect, if any, on children.

The evidence on whether children's response to cost-sharing varies by parental characteristics, such as family income, is even scarcer. In the Rand HIE, poor children appeared to respond more strongly than non-poor children, but this difference could be shown to be statistically significant only for a subgroup of contacts related to trauma and accidents (Lohr et al. 1986). The Rand HIE suffered from small sample sizes at this level of analysis and from the fact that poor families had a lower out-of-pocket-cap and would often enjoy free care for a considerable part of the year.

Some non-experimental work has explored income heterogeneity among adults. Findings have been mixed. These studies either had to proxy individual income by regional indicators based on, for instance, ZIP code (Cherkin et al. 1992; Selby et al. 1996; Hsu et al. 2006; Kolstad and Kowalski 2012) or focused on programs targeted only at the poor, so that comparisons with non-poor individuals must rely on estimates from other contexts (Chandra et al. 2014).[3] Surveying the literature, Baicker and Goldman (2011), conclude that "while there is a lot of speculation that the poor have more elastic demand, there is little evidence..." (p. 58). In our paper, we add a credible estimate of income heterogeneity to the literature for two reasons: First, we use administrative data with precise income information at the individual level. Second, because public health insurance in Sweden covers the whole population, we can make comparisons across all income groups.

3 Data and method

3.1 Institutional setting

Health care in Sweden is provided at the county level. This paper focuses on Scania (*Skåne*), a county with approximately one million inhabitants. Scania is similar to the rest of Sweden with regards to its organization of health care, but also along other dimensions, such as degree of urbanization and incomes.

Public health insurance in Sweden is universal, i.e., all residents are entitled to publicly funded health care. Supplemental private health insurance is available, but uncommon.[4] Primary care is mostly provided by health care centers, which offer all types of ambulatory treatment. Hospitals provide outpatient care by specialists and supply inpatient care, which involves at least one overnight stay. Most health care providers are public, but there are also

3. In the only non-experimental paper on children, Han et al. (2016) used insured income as a proxy for household income, and found mixed evidence of differential responses.

4. In the year 2000, only 1.1 percent of the Swedish population had supplemental private health insurance (Finansdepartementet 2008). Private health insurance provides shorter waiting times, but only at private health care providers.

privately run providers that work under public contract. Hospitals are almost all public.

When individuals wish to visit a health care provider due to a new health problem, they first call their local health care center. Sweden applies a telephone triage system, which means that appointments cannot be made by the patient alone, and the nurse answering the phone acts as a gatekeeper. In addition to scheduling and trying to keep away individuals who are in less need of care, the gatekeeper can provide advice. The gatekeeper is only able to make appointments with the local health care center, and if the individual needs to see a specialist, they will need to visit a GP at their health care center and obtain a referral. Re-visits due to the same health problem are often scheduled by the patient directly with the doctor.

Cost-sharing comes in the form of copayments that are charged for several medical services, such as visits to a doctor. Copayment levels are determined by the Scania Regional Council. Due to the universal coverage of public health care and to the low rate of private insurance, everyone who lives in Scania is essentially exposed to the same copayment structure. Copayments are collected by health care centers and hospitals, but are transferred to the county administration, implying that incentives to provide care are unrelated to the level of copayments. The contribution of copayments to health care funding is small. Financing is mainly via locally levied income taxes and, to a smaller degree, central government subsidies.

Since the detailed regulation of the healthcare sector is in the hands of the regional council, copayment schedules may vary depending on the political majority. When the Region of Scania was established following the merger of two smaller counties and one city in January 1999, copayments were set to be charged from age 20, whereas those below 20 had free access to care. Following the first election to the county council, however, a centre-right coalition supported by a populist party came into power. In June 1999 it was decided that instead of being charged from age 20, individuals would be charged copayments from the age of 7, starting from July the same year.

As it turned out, the new policy regime would not last very long. In the end of October 2001, the left-wing opposition in the Scania Regional Council put forward the proposal to abolish copayments for individuals aged 19 and below, thus reversing the policy change of 1999. To the surprise of everyone, the proposal was accepted, as two members of the center-right majority accidentally pressed the wrong button (Hanson et al. 2001).[5] This reform came into effect in January 2002. In addition to abolishing copayments for children and adolescents, a few minor changes to the fee structure were implemented at the same time. These included free nurse visits in psychiatric care for individuals above age 18, provi-

5. The incident serves as an example of failed tactical voting. The left-wing minority had proposed to abolish copayments for individuals up to age 19 whereas the populist party had proposed to abolish copayments for those up to age 12. The larger center-right parties wanted to keep the status quo. In the first round of votes, the proposal to abolish fees for those up to 19 defeated the proposal to abolish fees for those up to 12 as two members of the larger center-right parties accidentally voted for the left-wing proposal. The larger center-right parties were then defeated in the second round as the populist party abstained from voting.

sion of free contraceptives for individuals aged 20 and below, and abolishing the practice of, first, not charging individuals that had to wait more than 30 minutes beyond the appointment time for a scheduled acute visit and, second, not charging individuals for outpatient visits that lead to immediate hospitalization (Regionfullmäktige Skåne 2001b). Finally, in January 2002 a family doctor system was introduced, allowing families to sign up for a preferred doctor at their health care center. It is not inconceivable that this somehow affected individuals' preferences for health care utilization; however, it is unclear why the impact would be different for the specific age group of our interest.

The first reform had a positive impact on the Scania budget due to lower utilization and the inflow of copayments, while the second reform had the opposite effect. We estimate the annual fiscal impact of the reforms to be about 70-100 million SEK (roughly 10 million USD). This accounted for less than 1 percent of overall spending on health care in Scania in 2001 (Regionfullmäktige Skåne 2001a). Scania ran budget deficits between 1999 and 2002 (ranging between 440 and 1748 million SEK), which were largely driven by large-scale investments in the health care sector (Regionfullmäktige Skåne 2002a, 2003b). With taxes left unchanged between 1999 and 2002, the deficits were financed through additional debt. To reduce the growing budget deficit, the Scania-level component of the income tax was eventually raised by 1 percentage point to 10.39 percent in 2003 (Regionfullmäktige Skåne 2002b), and in 2004, large cutbacks were implemented (Regionfullmäktige Skåne 2003a).

Besides the reforms in 1999 and 2002, other modifications were made to the copayment schedule during our study period of January 1999 to December 2006, as can be seen in Table 1, which summarizes the schedule for outpatient copayments between 1999 and 2006. The copayments were generally the same for all individuals above a certain threshold, whereas those below the threshold (below 7 or below 20, depending on the time period) were exempt from fees. We distinguish between visits to doctors and non-doctors, as there were pronounced differences in copayments for these two categories of caregivers. As for doctor visits, the copayment initially amounted to 100 SEK (approximately $10 in 2001) for seeing a general practitioner (GP) at a health care center as well as for seeing a specialist at a hospital after referral. This was later (in July 2003) increased to 150 SEK. For visits to a specialist without a referral from a GP (for example, a revisit to a specialist), visits during out-of-office hours or visits to an emergency department, individuals were charged 200 SEK before July 2003 and 300 SEK thereafter. Regarding non-doctors, visiting a nurse outside primary care or visiting certain other types of health care professionals, such as physical therapists, psychologists or dietitians, was charged 0 SEK, 60 SEK, or 80 SEK, depending on the time period. Several specific services, such as vaccinations and prescriptions without contact with the doctor, were charged with a service-specific fee, whereas other services, including 24-hour-revisits, rehabilitation for disabled individuals and treatment of infectious diseases were exempt from copayments. Nurse visits in primary care were free of charge, which meant that most nurse visits were not charged.(Regionfullmäktige Skåne

Table 1: Outpatient fee structure in Scania between 1999 and 2006

Caregivers:

Doctors *(Jan 99-Jun 03 / Jul 03-Dec 06)*	SEK	Non-Doctors[d] *(Jan-Dec 99 / Jan 00-Dec 03 / Jan 04-Dec 06)*	SEK
General practitioner (GP)[a]	100/150	Nurses	
Specialists		in primary care	0
with referral	100/150	in specialist care	60/80/100
without referral (incl. revisits)	200/300	Other health care professionals (e.g., physical therapists, psychologists, dietitians)	60/80/100
Acute visit during out-of-office hours[b]	200/300		
Emergency department	200/300		
Medical service (e.g., x-ray, ultrasound)[c]	0		

Specific services (amongst others):	SEK
Prescription only	50
Vaccination (excl. cost of vaccin)	120 (from 2002: 150)
Gynecological health check-up by midwife	60 (from 2004: 120)
Mammography	120
Blood pressure control	200

Exemptions (amongst others):

→ Age-related
- All care for children aged 6 years and below
- All care for children between 7 and 19 years from January to June 1999 and after January 2002
- Psychiatric care for individuals under 18 years
- Speech therapy for individuals under 16 years
- Health care at schools (mostly counselling and highly recommended vaccinations)

→ Timing-related
- 24-hour-revisits at the same provider for the same condition
- After waiting for more than 3 months since diagnosis (so-called care guarantee)
- After waiting for more than 30 minutes since the scheduled time of a visit (from 2002: not for acute visits)
- Revisits within 7 days of patients with respiratory infection and when the doctor refrained from antibiotic treatment

→ Disease-related
- Rehabilitation for individuals with disabilities
- Dialysis treatment
- Forensic and compulsory psychiatric care

→ Urgency-related
- Treatment of infectious diseases
- Acute treatment of alcohol and drug abuse
- Acute referrals that require immediate medical assessment
- Outpatient visits leading to immediate hospitalization (only until 2001)

→ Other
- Birth control
- Contact by telephone/letter
- Trial and adaptation of technical utilities
- Research/drug testing

Notes: [a]Also includes psychiatrists in basic psychiatry. [b]5p.m.-8a.m., weekends and public holidays. [c]Performed by a doctor different from the one treating the patient. [d]Performed independently, i.e., not given directly after and connected with a doctor visit. 100 SEK (=Swedish krona) ≈ $10.

2003d, 2003c).

Throughout, there was an out-of-pocket cap on copayments implying that an individual paying an amount of 900 SEK within a twelve-month period became eligible for a "free card" that granted free outpatient care until the end of that period. The out-of-pocket cap applied jointly to all children under 18 years who lived in the same household. For inpatient care, children were not charged at all (individuals above the age of 24 – or 20 from July 2003 – were charged a fee of 80 SEK per day.).

3.2 Data and sample

Our dataset contains the universe of contacts with the medical sector in Scania.[6] We observe the copayment that the patient was charged, and we observe all diagnoses that the patient was given.[7] For outpatient care, we can identify the specific caregiver as either doctor or non-doctor. We restrict attention to actual visits and ignore contacts via letter, telephone etc. We also exclude preventive visits from our main analysis.[8]

We link the health care data to another administrative dataset, obtained from Statistics Sweden. This dataset covers all persons born between 1940 and 1985, who were registered in Sweden as of December 31, 1985, as well as their parents, and all their children. The data contain annual information on income by type, as well as data on education, marital status, place of residence, and many other variables for all individuals aged 16 years and above. We use these data for two main purposes: First, we define the sample of Scania inhabitants. For children under 16 years who do not have their own entry on residence, we utilize information on the parents who are linked to the child. Second, we use the dataset to analyze whether certain subgroups of the population (defined by family characteristics) responded differentially to the reform. Finally, we obtain year and month (but not exact date) of birth for all individuals. As noted, we only study outcomes between 1999 and 2006. While data on health care visits are also available for 2007 and 2008, we exclude these years since we do not have data on socioeconomics and place of residence for these years.

For an RD setup, there emerge two age thresholds that we can exploit. First, care was free for individuals aged 19 and younger before the first reform and after the second reform, creating a potential discontinuity in utilization at age 20. Between the two reforms, children aged 6 years and younger were not charged, creating a potential discontinuity in utilization at 7.

Potentially, there could be factors other than copayments which influence utilization

6. The only noticeable exceptions are visits to the dentist.

7. Diagnoses are entered using the ICD classification system.

8. We disregard preventive visits since age-dependent check-ups exhibit irregular and pronounced spikes, most notably related to the pre-school check-up at age 5. Other preventive visits include vaccinations, allergy tests, contacts with a welfare officer and contraception. Most of these visits are free of charge, while others (such as vaccinations) are charged independently of age and not affected by the reforms. We therefore expect no changes in preventive visits around the two age thresholds. Indeed, we obtain estimates that are small and insignificant from zero.

and which also change abruptly at the age thresholds that we exploit. To account for this, we use a combined RD and DiD setup. In this approach, which arguably delivers our most credible estimates, RD design is applied on a time interval with more than one policy regime, such as the entire time period 1999-2006. We allow for different jumps at the age threshold during the two regimes, and the treatment effect is represented by the difference between these jumps.

We have 2.5 years of observations for the 6/7 threshold and 5.5 years for the 19/20 threshold (0.5 years before the first reform and 5 years after the second). With a bandwidth of 12 months below and above the threshold, we have approximately 0.75 million observations of 56,215 individuals in the case of the 6/7 threshold and 1.46 million observations of 110,679 individuals in the case of the 19/20 threshold.

Finally, we employ a standard DiD model, which allows us to estimate effects for the entire treatment group of 7-19-year olds. As control groups we choose either 6-year-olds, who were exempt from fees during the whole study period, or 20-year-olds, who had to pay throughout. For better comparability with control subjects, we split the treatment group so that 7-12-year-olds are contrasted with 6-year-olds and 13-19-year olds contrasted with 20-year-olds. This gives 280,615 individuals who are observed for 61.7 months on average. Of all the individuals who are in the sample for at least two years, only 3 percent had no contact at all with the health care system.

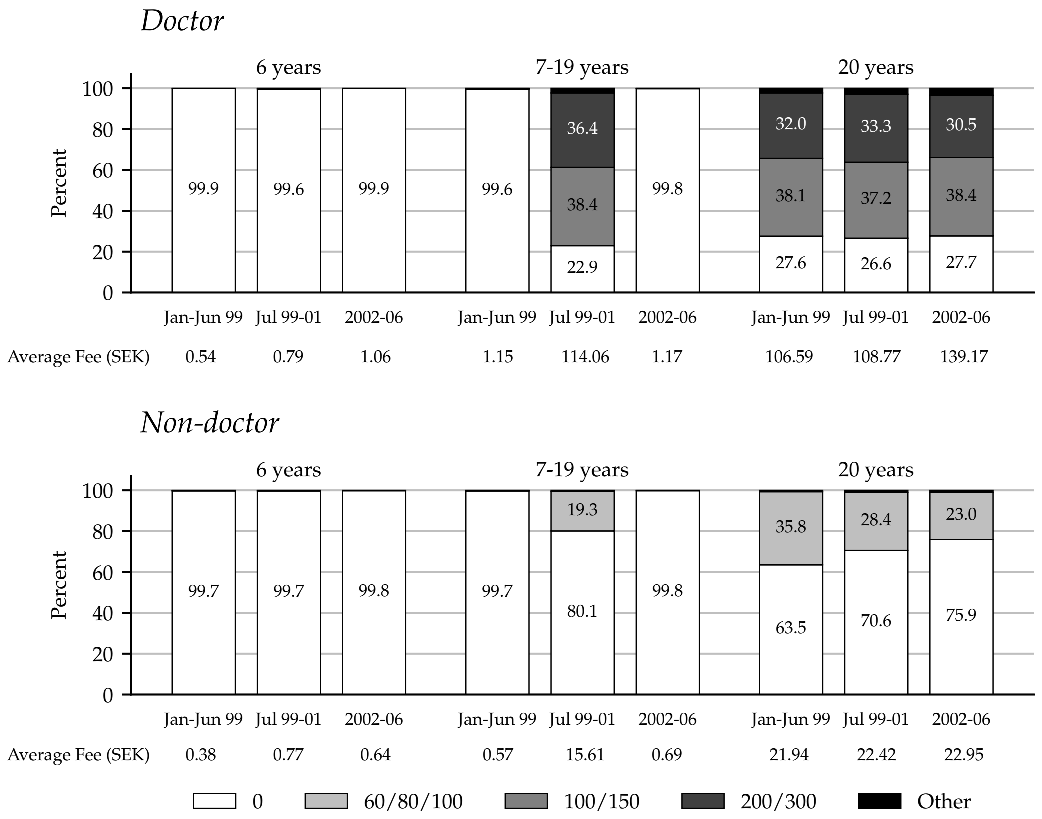
3.3 Fees charged

Displaying copayments charged over the time period of study, Figure 1 indicates that the reforms had strong impacts on children's exposure to cost-sharing. Before the first and after the second reform, 7-19-year-olds were essentially never charged when visiting a health care provider. This is also the case for 6-year-olds throughout. Between the first and second reforms, however, 7-19-year-olds were charged, and the distribution of fees payed is similar to the one that 20-year-olds have throughout the time period.

In most cases, visits to (outpatient) non-doctors were not charged for any time period and for any age group. In particular, only 19 percent of all visits to non-doctors among 7-19-year-olds were charged in between the two reforms, when copayments applied. These low numbers can partially be explained by the eligibility for a free card. However, most individuals did not consume enough health care to become eligible for the free card and the prevalence of the free card is rather low; between July 1999 and December 2001, the share of visits paid with a free card was only 12 percent in the treatment group. Thus, free visits mostly reflect the large number of exemptions.[9] In contrast, less than a one quarter of

9. In principle, it would be possible to exclude fee-exempt visits from the analysis. We prefer to retain all visits in the sample for two reasons: First, the data do not allow for a clear-cut identification of fee-exempt visits. Second, the reforms might also have affected visits that were free of charge already before. For example, it is conceivable that the second reform would have increased the demand for (previously free) psychiatric care because such care typically follows after a referral from a (previously costly, but now free) GP visit.

Figure 1: Distribution of actually paid fees (in SEK) by caregiver, age group and year



Notes: Percentage shares of actually paid fees by caregiver in the treatment group (7-19 years) and in the control groups (3 and 20 years) around the two policy changes of July 1999 and January 2002. Only non-preventive in-person visits plus contacts related to prescriptions. 100 SEK (=Swedish krona) $\approx$ \$10.

all doctor visits were free of charge in between the two reforms. Given this, we expect the demand for doctor visits to respond more to the reform than the demand for other visits.

3.4 Method

Visiting a health care provider is a decision that is likely to depend on several factors, including health, information, habits, and costs. Our empirical approaches rely on arguably credible assumptions regarding the invariance of other factors, either that these other factors do not change abruptly at a certain age threshold, or that they develop similarly for similar groups. While we believe that these approaches enable us to isolate individuals' average responses to the copayments, mechanisms may still be complex, both because they depend on individual characteristics and because prices vary endogenously over the year due to the 12-month cap on aggregate copayments. Regarding differential responses across individuals, these could arise due to differences in resources, but also in knowledge or preferences. In particular for resources, a wealthy patient (or parents of a patient) may decide to consume closer to the "optimal" level of care (the level where marginal utility is virtually zero), whereas a poor may have to cut back more when prices are higher. Further, due to habits, fear of doctors, or exceptionally good health, some individuals may not consume any care regardless of how cheap it is. Thus, responses may be different along the extensive and the intensive margin. Such differences may also arise since the first visit to a doctor is usually initiated by the patient, whereas re-visits may be initiated by the doctor. This mechanism would imply a weaker response to copayments along the intensive margin, at least for re-visits due to the same health problem.

Our analysis proceeds as follows. Based on the patient records, we calculate each individual's number of visits in each month during the study period. In an RD framework, we estimate a local linear regression that allows for different slopes below and above the threshold:

$$(1)\, Y_{it} = \alpha + \beta_1 \mathit{(Age\text{-}Threshold)}_{it} + \gamma \mathit{Free\ Care}_{it} + \beta_2 \mathit{(Age\text{-}Threshold)}_{it} \times \mathit{Free\ Care}_{it} + \varepsilon_{it}$$

Y_{it} is the number of visits of individual i in month t. *Age* is age in months and *Threshold* is the age threshold of interest (7 or 20 years). $\mathit{Free\ Care}_{it}$ is equal to one if *Age* < *Threshold* and zero otherwise. Since we consider deviations of *Age* from the *Threshold*, the coefficient γ directly estimates the treatment effect of enjoying free care. Observations right at the threshold are disregarded, as we observe the month but not the exact date of birth.

We choose a bandwidth of 12 months below and above the threshold, and this is for two reasons. First, we want to avoid the influence of the free card, which would generate non-linearities in the average price faced by individuals beyond 12 months after the age threshold. Second, a bandwidth of 12 months is close to optimal for the 6/7 threshold according to the selection procedure proposed by Calonico et al. (2014). The optimal bandwidth for the 19/20 threshold is larger, but here we conservatively opt for 12 months to be consistent with the 6/7 threshold. Standard errors are clustered at the individual level.

We then turn to analyses exploiting the policy changes. First, the two changes are exploited to increase the credibility of our RD model, as we can now account for other (time-invariant) jumps taking place at the age threshold of interest. Intuitively, we subtract from our RD estimates any jumps around the age threshold during periods when copayments did not change around the same threshold (control periods). This RD-DiD model is estimated as follows:

$$\begin{aligned}
Y_{it} =& \alpha + \beta_1 \mathit{(Age\text{-}Threshold)}_{it} + \gamma \mathit{Free\ Care}_{it} + \beta_2 \mathit{(Age\text{-}Threshold)}_{it} \times \mathit{Free\ Care}_{it} + \\
(2)\qquad & \kappa \mathit{Treatment}_t + \beta_3 \mathit{Treatment}_t \times \mathit{(Age\text{-}Threshold)}_{it} + \delta \mathit{Treatment}_t \times \mathit{Free\ Care}_{it} + \\
& \beta_4 \mathit{Treatment}_t \times \mathit{(Age\text{-}Threshold)}_{it} \times \mathit{Free\ Care}_{it} + \varepsilon_{it}
\end{aligned}$$

$\mathit{Treatment}_t$ indicates periods in which copayments were charged for individuals above the given age threshold and *Free Care*$_{it}$ continues to be an indicator for being below this age threshold. γ thus captures jumps at the thresholds not generated by the copayment requirements (control periods), while the coefficient of interest is δ, which captures the additional jumps induced by the change in copayments during treatment periods.

We then turn to our standard DiD analysis. Under the assumption that treatment and control groups exhibit parallel trends, we can here make inferences not only around specific thresholds, but for the entire group of individuals affected by the reforms. The following equation is estimated:

$$Y_{it} = \alpha + \beta_1 \textit{Treated Jan-Jun 1999}_{it} + \beta_2 \textit{Treated 2002-2006}_{it} + \lambda_{it} + \delta_t + \varepsilon_{it} \tag{3}$$

Y_{it} is the outcome of individual i in month t. *Treated Jan-Jun 1999*$_{it}$ and *Treated 2002-2006*$_{it}$ are separate indicators for the treatment group (ages 7 to 19 years) for the two time periods where copayments where not charged. β_1 and β_2 are thus the treatment effects of enjoying free care. λ_{it} and δ_t are age (in months) fixed effects and month fixed effects. To prevent different degrees of seasonal variation in Y_{it} to affect our estimate of β_1, we seasonally adjust Y_{it} separately for treated and controls before estimating the above equation.[10] ε_{it} is an error term which captures other determinants of medical visits. To account for both serial correlation of utilization within individuals and correlation within cohorts, all DiD specifications cluster standard errors at the birth year $\times$ quarter level.

In some regressions, we investigate the effect on having any visit during the whole year the individual was at a given age a (7 years, say). We then define the outcome Y_{ia} to be a binary indicator for individual i having at least one visit at age a. We focus on the second reform, since we do not observe individuals for more than one year before the first reform. We restrict attention on individuals that spend the whole year of age either before or after the reform. Moreover, we redefine λ_a as fixed effects for age (in years) and δ_i as fixed effects for the month in which age a is reached. As before, *Treated 2002-2006*$_{ia}$ is an indicator for reaching ages 7 to 19 years between 2002 and 2006.[11]

10. Younger children exhibit larger seasonal variation as can be seen in Figure 3. We use the years 2000, 2004, 2005, 2006 as our reference period for seasonal adjustment. No major reforms of copayments occurred just before or after these years, so that seasonal patterns within these years are unlikely to be affected by potential intertemporal substitution induced by the reforms.

11. As an alternative to the specification in (3), we have aggregated observations by outcome month and month of birth, and used the logarithm of average visits as the outcome. As shown in the Online Appendix, Table C.2, the result is similar to that of our main specification. Results are also similar when adding further control variables, or when applying a negative binomial model (NB2, see Cameron and Trivedi (2013)). We have also have tried models with additional controls when looking at the probability of having at least one visit per year, as well as logit and probit models, all with similar results.

4 Results

4.1 Main analysis: Effects around age thresholds

In Figure 2, we plot the pattern of outpatient doctor visits around the thresholds that we exploit for the RD estimations.[12] We plot average visits around both the 6/7 and the 19/20 threshold for each of the three time periods that we study: before the first reform in July 1999, between the two reforms, and after the second reform in January 2002. Indeed, three of our six graphs can be viewed as placebo checks, whereas the other three indicate the (potential) effects of copayments on utilization.

We find clear evidence that doctor visits reduce when copayments begin to be charged. Comparing 19- and 20-year-olds in the first half of 1999, for example, we find that average visits drops from around 2.2 to somewhat above 2.0 visits per year. Similar drops can be seen when comparing 19- and 20-year-olds in the third period – or when comparing 6- and 7-year-olds in the second period. Notably, there is some evidence of adjustment behavior, so that when copayments are charged from age 20, individuals substitute visits from the month(s) right after turning 20 to the month(s) right before turning 20.

The control thresholds provide no evidence of any jumps, as the developments appear continuous around the thresholds. This evidence strongly suggests that the jumps documented represent effects of copayment exposure, rather than other factors that affect different age groups differentially. To more formally examine the effects of copayments on utilization around the 6/7 and 19/20 thresholds, we next turn to our RD regressions, focusing on outpatient doctor visits.

In Table 2, the first panel reports results from RD regressions around thresholds where effects would be expected (and where effects were detected in the graphs). The estimates are relatively homogeneous (not statistically different) and suggest that free care is associated with around 0.2 more visits per year. The implied percentage effects are 13.5 percent during the first period and around 9 percent in the two later periods.

In Panel B, we report results from our control regressions. The estimates are close to zero and statistically insignificant. There is no evidence of other relevant changes taking place around the thresholds of study.

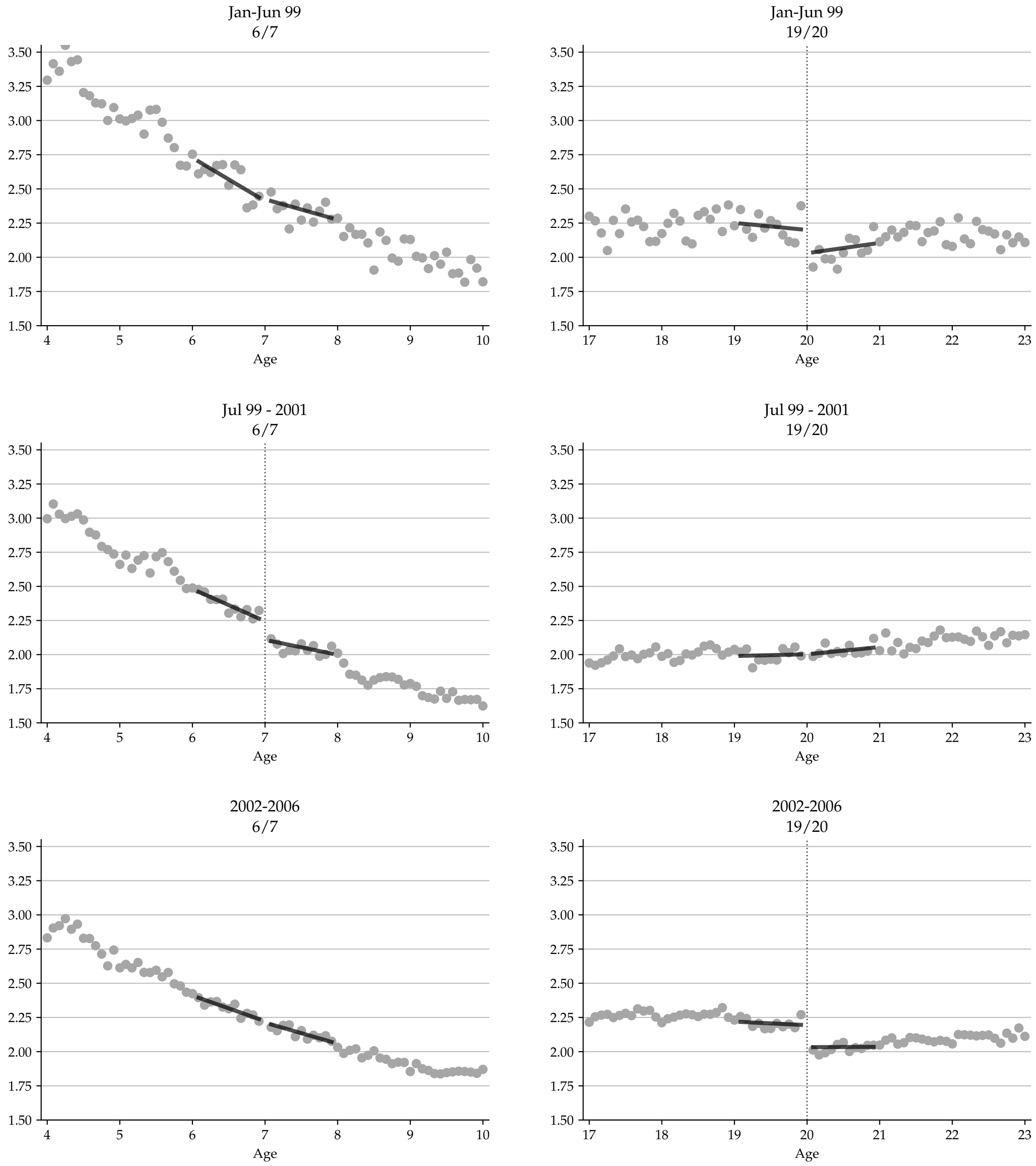
Although no jumps were detected at the control thresholds, it cannot be ruled out that small discontinuities are present. Next, in Panel C, we thus use a combined RD and DiD approach, where the baseline and control thresholds are exploited simultaneously, allowing for even more credible estimates. Estimates for the interactions between being below the threshold and being in the treatment period are reported in the table.[13] Indeed, results

12. We return to other types of visits in Section 4.3.

13. The model in column 1 includes the first two policy periods (1999-2001) whereas the one in the third column includes the two last (July 1999-2006). The mid column includes the entire interval from 1999 to 2006, since both the first and third period are relevant controls.

are very similar to the main ones in Panel A. This is not unexpected given the small estimates for the control thresholds. Noticeably, however, the standard errors become some-

Figure 2: Doctor Visits Around Thresholds



Notes: Annualized average number of doctor visits by age. Dots represent months. "6/7" indicates that the threshold is the month in which individuals become 7 years old; analogously for "19/20". Vertical dotted lines indicate that copayments stopped being charged to the right of the age threshold in the given time period. Dark lines are from fitted RD models according to the specification described in Section 3.4.

Table 2: RD Effects on Doctor Visits

	Jan-Jun 99	Jul 99-2001	2002-2006
	A. Baseline Effects		
	19/20	6/7	19/20
Free care	0.26**	0.17**	0.20**
	(0.09)	(0.03)	(0.02)
Mean Above	1.94	2.07	2.00
% Change	13.5	8.2	9.9
N	135,131	745,987	1,320,973
	B. Control Thresholds		
	6/7	19/20	6/7
Free care	0.02	-0.00	0.02
	(0.07)	(0.03)	(0.02)
% Change	1.0	-0.2	0.9
N	159,839	660,685	1,226,670
	C. RD-DiD		
	19/20	6/7	19/20
Free care	0.27**	0.15**	0.20**
	(0.10)	(0.04)	(0.04)
% Change	13.8	7.2	10.2
N	795,816	2,132,496	1,981,658
	D. Donut RD-DiD (2 months omitted above and below threshold)		
	19/20	6/7	19/20
Free care	0.28*	0.17**	0.20**
	(0.14)	(0.06)	(0.06)
% Change	14.8	8.2	10.0
N	663,406	1,777,101	1,654,463

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. 'Free care' is equal to one if an individual is below the threshold. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Coefficients are scaled up by 12 to represent annual figures. Means are estimated just above the threshold and scaled up to annual figures. Standard errors clustered at the person level are shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

what larger, but effects are still strongly significant.[14]

Finally, in Panel D, we account for the possibility of adjustment effects around the thresholds. As noted, individuals may substitute visits over time and make more doctor visits shortly before copayments begin to be charged, and then make fewer visits after-

14. In Table C.1 in the Online Appendix, we provide placebo checks, using different thresholds ranging from 3/4 to 22/23. In a few cases, RD estimates suggest that effects are significant at thresholds other than the ones where copayments begin to be charged. However, when applying RD-DiD, only one out of 18 placebo estimates is statistically significant. Moreover, the effect sizes are clearly the largest at the thresholds with the expected jumps.

wards. We here exclude two months below and above the threshold in order to avoid such concerns. As can be seen, the results are virtually the same.

To compare our findings with other estimates in the existing literature, we follow Brot-Goldberg et al. (2017) and convert the treatment effects into semi-arc elasticities.[15] We obtain values of -0.88 for the 19/20 threshold and -0.55 for the 6/7 threshold. These numbers are considerably lower than the semi-arc elasticities that Brot-Goldberg et al. (2017) report for the Rand HIE, which lie between -2.11 and -2.26, but similar to the estimates by Brot-Goldberg et al. (2017), based on a recent dataset of employees and their dependents. This number gives a simple measure of how much patient in our setting respond to copayments, although, as noted by Aron-Dine et al. (2013), the price responsiveness to a health insurance plan may not be captured by a single number, as prices are nonlinear. Nevertheless, the above number is calculated in the same way as in Brot-Goldberg et al. (2017) and in line with their paper we will carry out an analysis of whether patients respond to future prices.

4.2 Effects of the 1999 and 2002 reforms

We here provide graphs showing the patterns of visits between 1999 and 2006, along with DiD regression results, exploiting the policy changes of July 1999 and January 2002. Again, our focus is on outpatient doctor visits, since this is where the reforms had their main impacts on copayments, and we return to other types of visits in Section 4.3. By assessing the entire period between 1999 and 2006, we produce evidence on whether non-parallel trends may be an issue, and on whether impacts of the reforms came immediately or only with some delay. A delayed effect could arise because it takes time for individuals to become knowledgeable about the reform and because visits are sometimes scheduled long in advance, such as before the reform had been implemented or even announced. For outpatient care, the latter should be a small concern, however, as visits are rarely scheduled several months in advance.

In Figure 3 we display average doctor visits over the time period 1999-2006, where averages are created over six-month intervals. The first panel shows averages among 6-year-olds (the young control group) and 7-year-olds (the youngest individuals in the treatment group). Indeed, trends are rather parallel, and there appears to be evidence of reform impacts. This is seen more clearly in the panel below, which shows the differences between the 6- and 7-year-olds. In particular, the relative number of visits among 7-year-olds reduces after the reform in 1999, and then increases following the second reform in 2002. This second change operates with some delay, however, and only sets in after half a year, perhaps taking

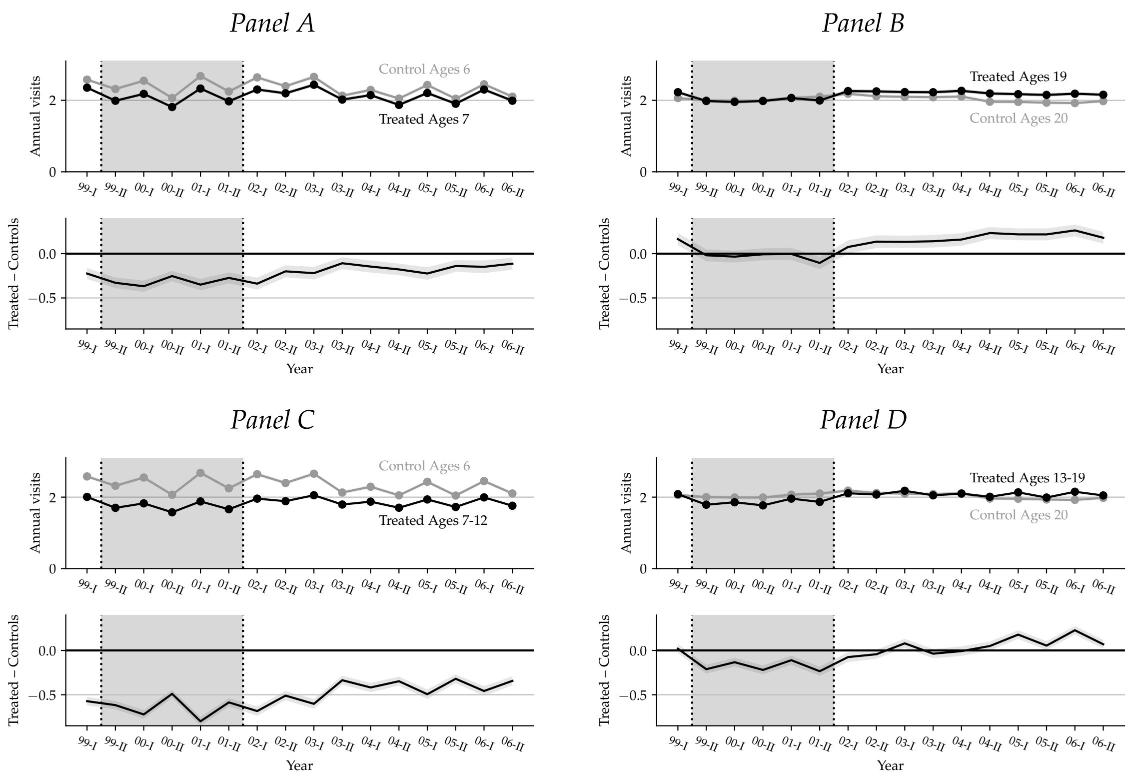
15. When the price change starts or ends with a zero price, standard arc-elasticities become meaningless, since they only depend on the quantity change. The semi-arc elasticity is defined as $[2(q_1 - q_0)/(q_0 + q_1)]/(p_1 - p_0)$, where q_0 and q_1 are the number of visits below and above the age threshold, respectively. Here, p_0 and p_1 refer to the average share of patient copayments in the total cost of a visit (measured as provider reimbursements) below and above the age threshold, respectively. We find that p_1 amounts to 11-13%, while p_0 is practically zero.

even longer to be realized at full. As the second reform was implemented unexpectedly, it reasonable that it took some time before individuals became knowledgeable about it. After the full effect has set in, however, the difference between treated and control individuals is rather stable.

In the first panel to the right, we display average visits among 19-year-olds (the oldest ones in the treatment group) and 20-year-olds (the old control group). Differences are shown below. Again, differences are quite stable within periods with constant policy regimes, whereas policy changes are associated with changes in the expected directions. The response to the second reform appears somewhat gradual.

The remaining panels are based on comparisons of 6-year-olds with the broader age group of 7-12-year-olds, and of 20-year-olds with the broader age group of 13-19-year-olds. We again see the expected reform impacts and a stable difference in the period between the first and second reform. After the second reform, the increase among the treated is more gradual, perhaps reflecting an adjustment process, although differential trends cannot be ruled out.

Figure 3: Doctor Visits between 1999 and 2006



Notes: In each panel, the upper graph shows the average annualized number of doctor visits by half-year over time, separately for treated and control ages. The lower graph shows the difference between treated and controls ages.

Table 3: DiD Effects of Free Care on the Number of Doctor Visits

Controls	6 Years	20 Years	6 Years	20 Years
Treated	7 Years	19 Years	7-12 Years	13-19 Years
A. Number of Visits: Overall				
Jan-Jun 1999	0.12***	0.20***	0.15***	0.15***
	(0.03)	(0.04)	(0.03)	(0.03)
2002-2006	0.14***	0.21***	0.22***	0.23***
	(0.02)	(0.03)	(0.02)	(0.03)
% Jan-Jun 1999	5.6	9.8	8.7	7.8
% 2002-2006	6.3	10.3	12.2	11.9
N	2,128,563	2,116,806	8,146,460	9,172,255
B. Number of Visits: By Half-Year				
1999-H1	0.11***	0.19***	0.10***	0.15***
	(0.03)	(0.06)	(0.03)	(0.04)
1999-H2	-0.04	0.00	-0.09***	0.00
	(0.03)	(0.05)	(0.03)	(0.04)
2001-H1	-0.01	0.02	-0.11***	0.03
	(0.04)	(0.06)	(0.04)	(0.04)
2001-H2	0.01	-0.08	-0.05	-0.01
	(0.04)	(0.05)	(0.05)	(0.04)
2002-H1	-0.00	0.10*	0.01	0.07*
	(0.04)	(0.05)	(0.03)	(0.04)
2002-H2	0.08*	0.16***	0.03	0.18***
	(0.04)	(0.05)	(0.03)	(0.04)
2003-H1	0.11***	0.16***	0.09***	0.22***
	(0.03)	(0.05)	(0.02)	(0.04)
2003-H2	0.18***	0.16**	0.21***	0.18***
	(0.05)	(0.07)	(0.05)	(0.06)
2004	0.15***	0.22***	0.24***	0.20***
	(0.03)	(0.06)	(0.03)	(0.05)
2005	0.13***	0.24***	0.21***	0.30***
	(0.04)	(0.05)	(0.03)	(0.04)
2006	0.18***	0.24***	0.22***	0.32***
	(0.03)	(0.06)	(0.03)	(0.05)
N	2,128,563	2,116,806	8,146,460	9,172,255
C. Probability of Any Visit				
2002-2006 (p.p. change)	0.52	2.20***	2.39***	3.56***
	(0.41)	(0.52)	(0.49)	(0.47)
2000 Mean (%)	69.7	64.0	62.8	62.7
N	119,654	118,279	467,548	532,378

Notes: Each column in each panel shows the treatment effect from a separate DiD regression. In Panel A and B, the dependent variable is the monthly number of doctor visits. Coefficients are scaled up by 12 to represent annual numbers. Means are scaled up to annual figures. Visits are seasonally adjusted separately for treated and control ages. Regressions control for age in months and month fixed effects. In Panel C, the dependent variable is an indicator for having any visit during the whole year the individual was at the given age. Coefficients are scaled up by 100 to represent percentage points. Regressions controls include fixed effects for age in years and the month in which this age was reached. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

We then turn to our regression results that are based on the DiD specification. Table 3 presents results from a number of different specifications, where the columns correspond to different treatment and control groups as in Figure 3. In the first panel, we separately report effects of the first and second reform. In a comparison of 6- and 7-year-olds, we find increases in visits by 0.12 and 0.14 per year when no copayments are charged, corresponding to about 6 percent. The effects are somewhat larger when comparing 19- and 20-year-olds. We here find increases by 0.20 and 0.21 visits per year, corresponding to about 10 percent. The effects are very symmetric irrespectively of whether we study to introduction of copayments resulting from reform 1, or the abolition of copayments resulting from reform 2.

Columns further to the right display results from models including treated individuals in broader age ranges. Results are roughly in line with those based only on treated individuals with a one-year age difference from control individuals, but some estimates are larger. Indeed, non-parallel trends may be a larger concern when considering treatment and control individuals that are less similar in age. Therefore, these results should be interpreted with more caution, but it seems clear that effects are at least not smaller than for 7- or 19-year-olds.

The results are also roughly in line with our findings based on RD, but the latter are if anything larger. A larger effect from the RD would make sense, since individuals that are right above the threshold where copayments are charged would never have accumulated copayments so that they are eligible for the free card. As a consequence, the average copayment that the individual is exposed to is higher, and the response stronger.

In the second panel of Table 3, we conduct an analysis by half-year. The year 2000 is omitted, and the years 2004, 2005, and 2006 are kept as full years to enable estimation of the treatment-group-specific seasonal effects. In line with the descriptive evidence in Figure 3, the reform impact is only seen from the second half of 2002 among 7-year-olds. However, among 19-year-olds, there is a statistically significant effect already in the first half of 2002. The difference in the response is barely statistically significant, however, and it is in line with the somewhat larger response that we see among 19-year-olds in general.

Finally, Panel C examines the probability of having at least one visit during the year.[16] Arguably, individuals who underutilize healthcare may not see a doctor during the entire year, and the reform may be viewed as more successful if having an impact on the extensive margin. We find effects on the extensive margin in most of our models, although not when comparing 6- and 7-year-olds. However, the effect is relatively small. For 19-year-olds, for example, the effect is only 2.2 percentage points. Such an effect, of 0.02 visits per person and year, only corresponds to about 10 percent of the overall effect of 0.2 visits per person and year, implying that most of the reform impact takes place on the intensive margin. Thus, while the reform appears to have made some adolescents see a doctor at least once, the overall result is mostly driven by individuals who would have gone to the doctor in any case, raising the possibility of overutilization.

16. Since we do not have an entire year of observations before reform 1, we here only exploit reform 2. Moreover, we only use the DiD model when considering yearly outcomes, since an RD analysis of changes around a threshold requires more high-frequency data.

A potential threat to identification in a DiD framework arises from congestion. Additional demand triggered by free visits may have increased waiting times for everyone. If, in particular, individuals in the control groups were deterred from using health care when it was free for the treated individuals, treatment effects may have been overestimated. But note that the treated would also not increase their visits as much in response to free care in the presence of congestion, potentially cancelling out the bias. Moreover, there is no indication that patients in the control groups changed their utilization after the reform. Also, while the treatment group represented 16.5 percent of the population of Scania, it only accounted for 9 percent of doctor visits. Our estimates imply that the reforms affected overall visits by less than one percent, a number lying well within the range of year-to-year fluctuations.

4.3 Types of visits

So far, we have focused on outpatient doctor visits, as the reforms mostly affected copayments for these. In this section, we make a more careful analysis of the reforms' impacts by type of visit and caregiver. In addition to doctor visits, we consider non-doctor visits. Some of these were charged, with the same age thresholds applying as for doctor visits. We also consider visits to inpatient care, although these were free of charge throughout. However, effects could still arise if copayments make patients forgo treatment in outpatient care. Such a mechanism has been shown to be at work for populations of elderly (Chandra et al. 2010; Trivedi et al. 2010), but not by the Rand HIE for the non-elderly. Graphs showing non-doctor and inpatient visits around the age thresholds are shown in Figures B.1 and B.2 in the Online Appendix, and graphs showing the developments of these visits over time are provided in Figures Figures B.3 and B.4 in the Online Appendix. In general there appear to be no clear effects, with the exception of non-doctor visits among teenagers.

We then provide regression results based on both RD and DiD.[17] First of all, we split outpatient doctor visits into GP and specialist visits. Results are shown in column (2) and (3) of Table 4. On the one hand, specialist visits may be less price sensitive than GP visits, since visits to specialists normally come after the referral from a GP, who makes his decision based on the patient's health status rather than on the copayment. On the other hand, fees for specialist visits were twice as high as those for GP visits. When using individuals aged 6 and 7, or when using the entire sample, we find evidence that specialist visits would be less price sensitive. Among the 19- and 20-year-olds, the estimates are about the same, however. At a minimum, our results seem to imply an important role for the referring doctor, as the much higher copayment for specialist visits does not induce a stronger impact on visits.

17. When exploiting the 19/20 threshold for an RD design we here, as well as in subsequent analyses, pool data from the first and third period, when the same age threshold applied.

Table 4: Effects by Type of Caregiver

	Doctor					
	Overall	GP	Specialist	Out-of-office Hours + ED	Non-doctor	Inpatient Care
A. RD	(1)	(2)	(3)	(4)	(5)	(6)
6/7 Years	0.17**	0.11**	0.06**	0.04*	-0.01	-0.00
	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.00)
Mean	*2.07*	*1.10*	*0.95*	*0.51*	*0.89*	*0.04*
%	*8.2*	*10.1*	*6.0*	*7.7*	*-1.3*	*-1.9*
19/20 Years	0.20**	0.10**	0.09**	0.04**	0.19**	0.00
	(0.02)	(0.01)	(0.02)	(0.01)	(0.04)	(0.00)
Mean	*1.99*	*0.89*	*0.99*	*0.55*	*1.39*	*0.07*
%	*10.3*	*11.4*	*8.7*	*7.2*	*13.9*	*2.1*
B. RD-DiD						
6/7 Years	0.15**	0.08**	0.06*	0.02	-0.01	-0.00
	(0.04)	(0.03)	(0.03)	(0.02)	(0.04)	(0.00)
%	*7.2*	*7.7*	*6.6*	*3.1*	*-0.9*	*-8.2*
Mean	*2.07*	*1.10*	*0.95*	*0.51*	*0.89*	*0.04*
19/20 Years	0.21**	0.10**	0.09**	0.06**	0.18**	0.01
	(0.04)	(0.03)	(0.03)	(0.02)	(0.06)	(0.01)
%	*10.5*	*11.2*	*9.3*	*11.6*	*12.9*	*10.6*
Mean	*1.99*	*0.89*	*0.99*	*0.55*	*1.39*	*0.07*
C. DiD - Number of Visits						
7-12 Years	0.21**	0.17**	0.03**	0.08**	0.00	-0.00
	(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.00)
%	*11.8*	*18.2*	*4.4*	*18.5*	*0.2*	*-7.8*
13-19 Years	0.22**	0.10**	0.12**	0.07**	0.16**	0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.00)
%	*11.5*	*10.6*	*14.3*	*14.4*	*13.6*	*13.3*
D. DiD - Any Visit						
7 Years	0.52	1.81**	-0.06	1.29**	-1.41**	0.34*
	(0.41)	(0.44)	(0.34)	(0.23)	(0.50)	(0.15)
%	*0.7*	*3.4*	*-0.1*	*4.8*	*-4.8*	*11.3*
7-12 Years	2.39**	3.28**	1.08**	2.31**	-0.24	0.22
	(0.49)	(0.43)	(0.40)	(0.35)	(0.48)	(0.17)
%	*3.8*	*7.0*	*3.0*	*10.1*	*-1.0*	*7.9*
19 Years	2.20**	1.41**	1.88**	1.67**	2.51**	0.22
	(0.52)	(0.48)	(0.59)	(0.47)	(0.64)	(0.15)
%	*3.4*	*2.9*	*5.1*	*6.2*	*9.5*	*4.4*
13-19 Years	3.56**	2.38**	2.93**	2.16**	0.84	0.48*
	(0.47)	(0.54)	(0.64)	(0.51)	(0.55)	(0.20)
%	*5.7*	*5.1*	*8.2*	*9.1*	*3.4*	*11.8*

Continued on next page

Notes: Each column in each panel shows the treatment effect from a separate estimation. In Panel A-C, the dependent variable is the monthly number of visits. Coefficients are scaled up by 12 to represent annual figures. In Panel A and B, '6/7 Years' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20 Years'. '19/20 Years' aggregates the time periods Jan-Jun 1999 and 2002-2006. 'Free care' is equal to one if an individual is below the threshold. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Means are estimated just above the threshold and scaled up to annual figures. Standard errors clustered at the person level are shown in parentheses. In Panel C, visits are seasonally adjusted separately for treated and control ages. Regressions control for age in months and month fixed effects. In Panel D, the dependent variable is an indicator for having any visit during the whole year the individual was at the given age. Coefficients are scaled up by 100 to represent percentage points. Regressions controls include fixed effects for age in years and the month in which this age was reached. Standard errors are clustered at the birth year $\times$ birth quarter level and shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

Our data also allow us to identify visits to emergency visits, and visits to doctors out of office hours (which we combine since visits out of office hours would only be granted in cases of emergent need for care). In both cases, visits reflect potentially more severe health problems, although, as for specialist visits, the associated copayment was higher. Our results (column 4 of Table 4) constantly suggest a relatively small, but positive and statistically significant, effect on these types of visits.

In column (5) we then consider effects on non-doctor visits, such as nurse visits. We find effects when using older individuals, suggesting that responses may vary depending on treatments received at different ages.[18]

Finally, we consider inpatient visits. While the estimates for total visits are insignificant, we find evidence of positive effects for inpatient stays on the extensive margin. This contrasts with earlier evidence on elderly and suggests that more health problems may get detected when young individuals see a doctor more often, thus leading to an increase in hospitalizations.

4.4 Visits by income

In this section, we examine if the effects on doctor visits differ by income, as measured by income available to the mother.[19] We exclude children whom we identify to live separate from the mother. As Table C.3 in the Online Appendix verifies, our baseline estimates change only little as a consequence of this restriction.

In Table 5, we compare effects in the lowest income quartile with those in the highest

18. These effects might partially be explained by a reform in 2004 that exempted children under 20 years from paying a fee for medical aids such as insoles and stabilizing bandages. This reform likely also affected complementary visits with non-doctors (for trial and adaption). Consistent with this hypothesis, we observe that the effects are concentrated in the years 2004-2006.

19. In this section, we remove the year 2003 from the regressions, because income data is unavailable for this year. Our measure of income available to the mother includes the income of a partner that lives in the same household, provided that the mother and the partner are married or in a registered partnership, or have common children. It also includes potential child support payments by the biological father.

income quartile, where income quartile is calculated conditional on year and age of the child. We provide results based on RD and RD-DiD, but also based on a simple DiD, where effects are interacted with time period. We study effects on overall outpatient doctor visits, and also split these into GP and specialist visits. Results for treated individuals all the way up to age 19 are included, although we note that those above age 18 may be less dependent on parental income.

The evidence clearly points in the direction of smaller effects in higher income households and suggests that much of the overall results are driven by low-income children. According to a simple RD model, for example, low-income individuals increase their outpatient doctor visits by 23 percent when no copayments are charged, corresponding 11 to 12 perenct. In contrast, the effect among high-income individuals is clearly smaller and statistically insignificant. The difference between those from high-income and low-income families is particularly salient when looking at specialist visits. The conclusions are very similar when applying RD-DiD. In a simple DiD model, we also find similar differences between low- and high-income individuals when considering the years around the reforms, but effects become more similar across high- and low-income individuals later on, possibly an artefact of non-parallel trends.[20]

Table 5: Effect on Doctor Visits by Income

	Overall		GP		Specialist	
Income group	1st Quartile	4th Quartile	1st Quartile	4th Quartile	1st Quartile	4th Quartile
A. RD	(1)	(2)	(3)	(4)	(5)	(6)
6/7 Years	0.23***	0.09	0.08*	0.09**	0.14***	0.00
	(0.07)	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)
p-value	-	0.14	-	0.90	-	0.02
%	*10.6*	*4.5*	*6.9*	*8.6*	*15.2*	*0.3*
19/20 Years	0.23***	0.03	0.07*	0.04	0.15***	-0.01
	(0.07)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)
p-value	-	0.03	-	0.57	-	0.01
%	*12.3*	*1.8*	*7.9*	*4.4*	*17.5*	*-1.2*
B. RD-DiD						
6/7 Years	0.27***	0.01	0.08	0.03	0.18***	-0.02
	(0.09)	(0.08)	(0.06)	(0.05)	(0.06)	(0.06)
p-value	-	0.04	-	0.51	-	0.02
%	*12.6*	*0.7*	*7.1*	*2.8*	*18.9*	*-2.2*
19/20 Years	0.18*	0.11	0.03	0.01	0.13*	0.09
	(0.11)	(0.11)	(0.07)	(0.06)	(0.07)	(0.08)
p-value	-	0.67	-	0.81	-	0.68
%	*9.7*	*6.2*	*3.3*	*0.7*	*15.2*	*9.4*

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20. Differences across income group are of interest per se since these relates to inequality, but it is also interesting to see how effects vary depending on resources, where the amount of resources available to the individual depends on the size of the family. To account for differences in family size, one can use the OECD equivalised disposable income instead of raw income. Results, displayed in Table C.4 in the Online Appendix, are similar to the ones above.

C. DiD - 7-12 Years						
Jan-Jun 1999	0.26***	0.10**	0.14***	0.12***	0.13**	-0.01
	(0.09)	(0.05)	(0.05)	(0.03)	(0.06)	(0.04)
2002	0.20***	0.03	0.11***	0.05*	0.08**	-0.01
	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)
2004	0.42***	0.31***	0.24***	0.21***	0.16***	0.09**
	(0.06)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)
2005	0.33***	0.28***	0.27***	0.19***	0.03	0.09*
	(0.05)	(0.06)	(0.03)	(0.03)	(0.04)	(0.05)
2006	0.32***	0.33***	0.19***	0.22***	0.08*	0.10**
	(0.06)	(0.06)	(0.03)	(0.03)	(0.05)	(0.05)
D. DiD - 13-19 Years						
Jan-Jun 1999	0.22**	0.19**	0.16***	0.08*	0.12*	0.10
	(0.11)	(0.08)	(0.03)	(0.04)	(0.07)	(0.06)
2002	0.26***	0.03	0.12***	-0.03	0.15**	0.11**
	(0.07)	(0.08)	(0.02)	(0.06)	(0.06)	(0.05)
2004	0.26***	0.15**	0.08**	0.03	0.15***	0.13**
	(0.06)	(0.06)	(0.03)	(0.04)	(0.04)	(0.05)
2005	0.31***	0.33***	0.13***	0.10**	0.18***	0.22***
	(0.06)	(0.07)	(0.04)	(0.04)	(0.05)	(0.05)
2006	0.29***	0.27***	0.11***	0.02	0.17***	0.22***
	(0.08)	(0.09)	(0.04)	(0.04)	(0.05)	(0.06)

Notes: Each column in each panel shows the treatment effect from a separate estimation. In Panel A and B, the dependent variable is the monthly number of doctor visits. '6/7 Years' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20 Years'. '19/20 Years' aggregates the time periods Jan-Jun 1999 and 2002-2006. 'Free care' is equal to one if an individual is below the threshold. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Coefficients are scaled up by 12 to represent annual figures. Standard errors clustered at the person level are shown in parentheses. In Panels C and D, the dependent variable is the monthly number of doctor visits. Coefficients are scaled up by 12 to represent annual figures. Visits are seasonally adjusted separately for treated and control ages. Regressions control for age in months and month fixed effects. P-values are from t-tests of differences in free care effects in comparison with "1st Quartile". *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively.

The finding that children from low-income families respond more strongly to copayments might be driven by differences in education or other factors that correlate with income. To explore this, we now also stratify by other characteristics, and conduct an RD and RD-DiD analysis. Results for 6-7-year-olds are presented in Table 6. First, in Panel A, we stratify by maternal (college) education. Theoretically, education could affect the response to our reform in either direction. More highly educated mothers may be assumed to appreciate the value of health care better, especially with respect to children, and therefore react less strongly to financial incentives. Indeed, Goldman and Smith (2002) showed that more educated patients are more likely to adhere to the therapy of chronic conditions. Similarly, Cutler and Lleras-Muney (2006) argue that more education leads to different thinking and decision-making patterns in general. More highly educated mothers may also have healthier children Lundborg et al. (2014b), which may lead to a different response. As can be seen, however, education does not have an independent effect on the response to the reform, and both mothers with and without college education respond more when they are low-income.

Second, single mothers are disproportionately often low-income. Since single mothers might also differ in other dimensions, such as in preferences and behaviors, in Panel B we gauge whether the differential effect prevails if we keep family type fixed. We find that the difference across income groups is still present if we condition on whether the mother is in a partnership.

Some mothers may have low income because they stay at home rather than work, for example because they cannot not find a job. Staying at home implies a low opportunity cost of time. Together with the monetary cost in the form of copayments, the cost of time affects the decision of taking a child to a doctor. However, when we split the sample by whether a mother works in Panel C (defined to be true if she has nonzero earnings), we find that the response is similar regardless of whether the mother works, and the response varies by income in a similar way for working and non-working mothers. Summing up, these results appear to reflect differential responses by resources rather than other factors correlated with income.

Table 6: Effects on Doctor Visits for Income Interactions – 6/7 – Jul 1999-2001

	Has No College		Has College	
Panel A	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.26**	0.13	0.11	0.05
	(0.07)	(0.09)	(0.14)	(0.09)
Mean Above	2.19	2.07	1.95	1.93
N	139,038	92,658	33,590	80,391
RD-DiD	0.27**	0.08	0.25	-0.06
	(0.10)	(0.12)	(0.19)	(0.12)
Mean Above	2.20	2.08	1.94	1.94
N	325,461	210,581	89,641	205,493
	In Partnership		Single	
Panel B	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.23*	0.09	0.22**	-0.04
	(0.11)	(0.06)	(0.08)	(0.91)
Mean Above	2.06	2.01	2.17	1.62
N	58,801	172,868	115,127	852
RD-DiD	0.24	0.02	0.29**	-0.43
	(0.15)	(0.08)	(0.11)	(1.10)
Mean Above	2.07	2.02	2.18	1.62
N	137,576	414,375	280,214	2,979
	Stays At Home		Works	
Panel C	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.33*	0.16	0.20**	0.09
	(0.14)	(0.30)	(0.07)	(0.06)
Mean Above	1.95	2.04	2.18	2.01
N	34,829	7,337	139,099	166,383
RD-DiD	0.43*	0.21	0.23*	0.01
	(0.20)	(0.46)	(0.10)	(0.09)
Mean Above	1.99	2.19	2.18	2.01
N	82,178	16,382	335,612	400,972

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. "In Partnership" is defined as being married or cohabiting. "Stays At Home" is defined as having zero earnings."Has No College" is defined as having only elementary or secondary education. 'Free care' is equal to one if an individual is below the threshold. Coefficients are scaled up by 12 to represent annual figures. Means are estimated just above the threshold and scaled up to annual figures. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Standard errors clustered at the person level are shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

In Table C.5 in the Online Appendix, we conduct the same analysis, but instead based on 19-20-year-olds. This analysis is somewhat more problematic, as 19-20-year-olds may not depend on their parents financially. When we stratify by whether mothers are single, we only find a statistically significant effect when mothers are single and household income is low. However, effects estimates are of similar size when mothers are in partnership and income is low, and when mothers are single and household income is high. Only when mothers are in a partnership and household income is high, is the effect clearly smaller. When we stratify by whether the mother stays at home or whether she has college education, RD results point in the same direction as the ones for 6-7-year-olds, but the RD-DiD results are more mixed, and always statistically insignificant due to large standard errors.

In Figure 4, we illustrate the time patterns of doctor visits in different income quartiles. Consistent with the widely documented income-health gradient – children are healthier in richer families – the number of doctor visits among 7-to-19-year-olds decreases monotonically with parental income in the first half of 1999 as well as in 2002 and subsequent years, when care was free of charge. In 2004, visits drop in all groups due to substantial cutbacks to the Scania health care sector (Regionfullmäktige Skåne 2003a), but the ranking remains the same. In contrast, in the period between the two reforms, when fees were charged, the number of doctor visits was very similar across the first, second, and third income quartiles, and the poorest did not have the highest utilization. During this period, children from the poorest quartile appear to have underutilized care, at least relative to children from richer families.

4.5 Effects by health status

In Table 7, we estimate treatment effects separately for sickly and non-sickly individuals. We equate "sickly" with having a certain chronic condition or to rank high in terms of health care use. Since the sickly see a doctor more often, copayments impose a larger financial burden on them, possibly leading to a larger response. Effects on health may then become likely. Note, however, that some of the sickly exceed the out-of-pocket cap before the reform, which should have the effect of mitigating the response to the reform.

Table 7: Effects by Health Status

	Jul 99 - 2001 6/7		2002-2006 19/20	
	Non-Sickly	Sickly	Non-Sickly	Sickly
Asthma	0.13**	0.47*	0.19**	0.26
	(0.04)	(0.22)	(0.04)	(0.28)
Mean Above	2.01	3.52	1.89	3.26
p-value	-	0.06	-	0.41
Low Income	0.19*	0.34	0.25**	2.50**

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	(0.08)	(0.51)	(0.10)	(0.77)
Mean Above	2.05	3.95	1.73	2.61
p-value	-	0.38	-	0.00
High Income	0.08	0.22	0.07	-0.56
	(0.07)	(0.40)	(0.09)	(0.55)
Mean Above	1.98	3.18	1.74	2.69
p-value	-	0.36	-	-
Chronic diseases of tonsils and adenoids	0.14**	0.75	0.20**	0.20
	(0.04)	(0.39)	(0.04)	(0.44)
Mean Above	2.08	2.71	1.93	2.72
p-value	-	0.06	-	0.49
Low Income	0.20**	-0.10	0.30**	2.88*
	(0.08)	(0.78)	(0.10)	(1.11)
Mean Above	2.13	4.03	1.76	1.52
p-value	-	-	-	0.01
High Income	0.08	0.49	0.06	-1.04
	(0.07)	(1.24)	(0.09)	(0.71)
Mean Above	2.03	2.74	1.77	2.57
p-value	-	0.37	-	-
Obesity	0.14**	-0.00	0.19**	1.83*
	(0.04)	(0.52)	(0.04)	(0.83)
Mean Above	2.06	4.00	1.93	2.98
p-value	-	-	-	0.02
Low Income	0.19*	0.95	0.30**	3.71
	(0.08)	(0.81)	(0.10)	(1.95)
Mean Above	2.13	3.32	1.76	1.60
p-value	-	0.17	-	0.04
High Income	0.09	-5.21	0.05	-0.10
	(0.07)	(4.14)	(0.09)	(1.89)
Mean Above	2.01	9.59	1.77	4.02
p-value	-	-	-	-
Any Chronic Condition	0.11**	0.44**	0.19**	0.32*
	(0.04)	(0.15)	(0.04)	(0.16)
Mean Above	1.94	3.47	1.82	3.23
p-value	-	0.02	-	0.21
Low Income	0.15*	0.53	0.27**	1.01*
	(0.08)	(0.33)	(0.10)	(0.43)
Mean Above	1.97	3.71	1.61	3.24
p-value	-	0.14	-	0.05
High Income	0.08	0.07	0.09	-0.43
	(0.07)	(0.36)	(0.09)	(0.32)
Mean Above	1.89	3.57	1.68	2.85
p-value	-	-	-	-
Use	0.11**	0.35**	0.17**	0.32**
	(0.03)	(0.12)	(0.04)	(0.12)
Mean Above	1.71	3.85	1.53	3.72
p-value	-	0.03	-	0.11
Low Income	0.12	0.60*	0.20*	0.98**
	(0.08)	(0.26)	(0.09)	(0.31)
Mean Above	1.75	3.88	1.41	3.34

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p-value	-	0.03	-	0.01
High Income	0.06	0.21	0.02	0.21
	(0.07)	(0.26)	(0.09)	(0.30)
Mean Above	1.67	3.94	1.51	3.18
p-value	-	0.29	-	0.27

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. The reported coefficient belongs to an indicator for being below the threshold. Coefficients are scaled up by 12 to represent annual figures. Means are estimated just above the threshold and scaled up to annual figures. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Standard errors clustered at the person level are shown in parentheses. Individuals are defined as suffering from a chronic condition if they were diagnosed with it at least once during the age 9 or 10 for the 6/7 threshold and during the age 16 or 17 for the 19/20 threshold. 'Sickly' indicates having the condition. See Section A in the Online Appendix for how we define diagnoses using ICD-10 codes. Use is measured as the average number of monthly doctor visits at ages 9-10 and 16-17, respectively, and ranked among individuals of the same birth year. 'Sickly' indicates being in the top 20% of the doctor visits distribution. For the 6/7 threshold the included years are 2000-2001 and for the 19/20 threshold the included years are 2005-2006. P-values are from a one-sided t-test with the null hypothesis that the effect on the 'Sickly' is smaller than or equal to that on the 'Non-Sickly'. * and ** denote significance at the 5 and 1 percent level, respectively.

We define having a certain condition as being diagnosed with it at least once at age 10 or 11 for the 6/7 threshold and at age 16 or 17 for the 19/20 threshold.[21] We present results for the three most frequent conditions as well as for having any condition. We also rank individuals by their visits to a doctor at ages 10-11 and 18-19, respectively. We here define individuals in the top 20% of the distribution (by birth year) as "sickly" and the remainder as "non-sickly". There is a tendency for individuals who suffer from a chronic condition or had high use to respond significantly more strongly if they come from a low-income family, especially around the 19/20 threshold. Regardless of health status, high-income individuals appear to be protected from cost-sharing, while sickly low-income individuals appear to be most vulnerable.[22]

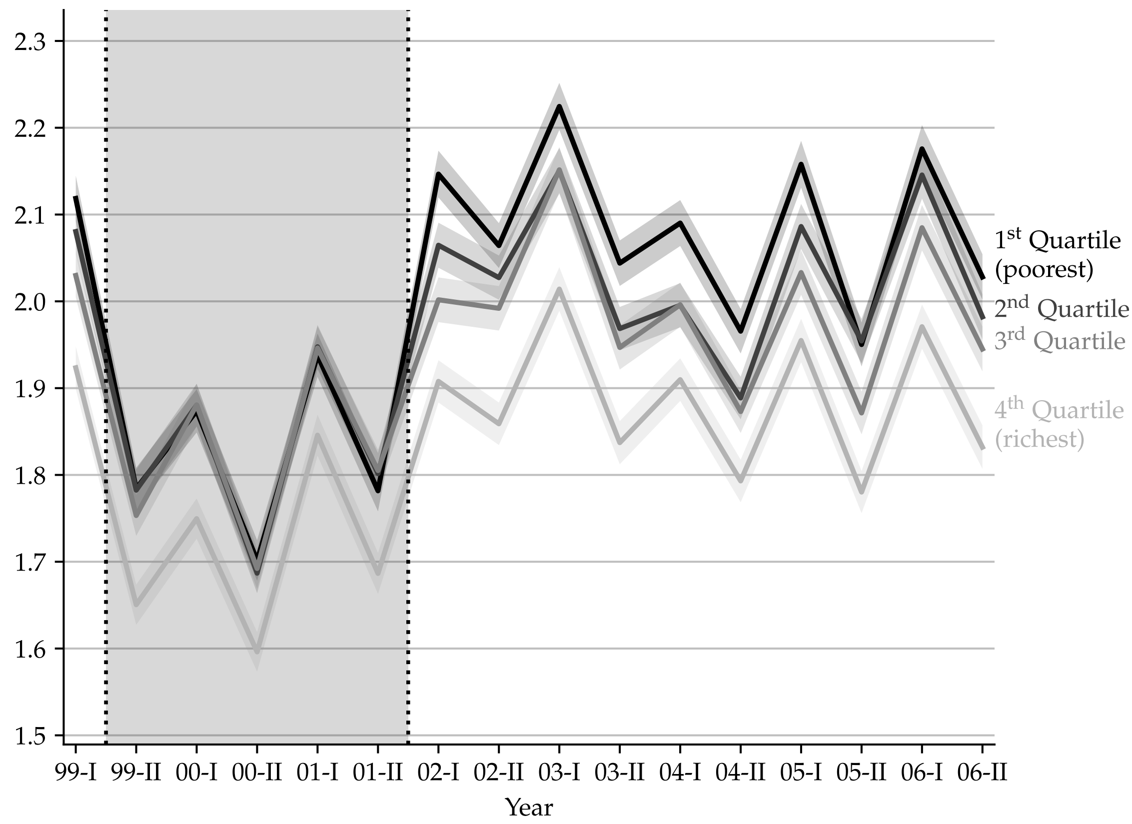
5 Conclusion

In this paper, we exploited a policy change in Sweden to study how copayments affect children's and adolescents' usage of health care. Despite the importance of health in young ages, very little evidence exists on the health care demand of this particular group. Our

21. See Section A in the Online Appendix for more details about how we measure health status. Because the probability of being diagnosed varies with age and year, we only present RD results, which unlike the DiD do not rest on the common trends assumption.

22. We have also tested whether visits associated with certain diagnoses respond differently. For example, one would be worried if individuals saw a doctor more often in connection with cancer after the reform. However, the number of visits with a particular diagnosis is quite low, even if we aggregate diagnoses. As a consequence, effects are imprecisely estimated and almost always not significantly different from the baseline effects.

Figure 4: Doctor Visits by Income Quartiles Over Time



Notes: Average number of doctor visits in the treatment group (7-19 years) by income quartile over time. Shaded areas are 95%-confidence bands. Missing income information for 2003 has been imputed from 2002 if available.

study made use of two alternative identification strategies, both with well-knows strengths and weaknesses: RD and DiD design, with additional credibility obtained by the use of an RD-DiD design. Results based on the different models largely turned out similar, lending credibility to our estimates.

We find that charging a small fee for medical care reduces doctor visits by 5-10 percent, a response that is similar to previous evidence on copayments based on adults or all ages (Selby et al. 1996; Cherkin et al. 1989). In the study the Selby et al. (1996), copayments of about 85 SEK in year 2001 prices were introduced, which is similar to our setting. The copayments studied by Cherkin et al. (1989) were three to four times larger, and only applied to emergency care. In our study, we find little evidence of effects on emergency department visits, but it is possible that overutilization of emergency care was a larger problem in the setting of Selby et al. (1996), where other visits were costlier. It is less straightforward to compare our results with the RAND HIE since that was based on coinsurance rates rather than copayments. When expressed as semi-arc elasticities, our results are smaller, however. For non-doctor and inpatient visits, where our reform had little or no impact on the average fee charged, results generally suggest no impact, contrasting some studies on elderly (ChandraEtAl2010, TrivediEtAl2010) which found evidence of spill-over effects.

One distinguishing feature of our setting is the existence of a telephone triage system, where individuals need to call a gatekeeping nurse at the health care center and are only provided an appointment if deemed necessary by the gatekeeper. Presumably in this context, visits would be reflective of "need" to a larger extent than elsewhere. In light of this, the 5-10 percent response to copayments may be viewed as quite substantial.

In addition to establishing the overall response to copayments among children and adolescents, our most important contribution is showing that responses vary by income. Virtually no previous studies of health care demand have been able to credibly explore differential responses by income, not even among adults. We find that the overall response is driven by individuals of lower income, as those in the highest income quartile respond very little – if at all – to the copayments.

Previous research for several countries has shown that there is a positive gradient in child health with respect to parental income. Our finding of differential effects of cost-sharing by income could provide one explanation for the presence of this gradient. We leave it for future studies to examine if such long-term health effects may be present.

The findings of this study pertain to Sweden, one of the countries with the smallest income inequalities and the most affordable health care to begin with. Future studies need to examine effects by income in countries where inequalities are higher and where the poor face greater financial obstacles to health care. In particular, a larger degree of income inequality would likely lead to even larger differences in responses across income groups.

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Online Appendix

A Measurement of health status

In Section 4.5, we present RD regressions after stratifying individuals by health status. We use two different ways of measuring health status. First, we define an individual as sickly if he or she was diagnosed with a certain set of diagnoses (see Table A.1 for how we define diagnoses using ICD-10 codes). Second, we rank individuals according to their health care utilization and define those as sickly who are in the top 20 percent of the distribution. To measure health status accurately, we consider a time span of two years for each individual. These two years are chosen so that the age when we measure health is fixed across individuals. We make sure that we do not measure health on the same age that we include an individual in the regression.

Specifically, when exploiting the 6/7 threshold, we measure health at ages 9 to 10 years. Given a bandwidth of 12 months above and below 7 years, individuals will be between 6 and 8 years when included in the regression, so there is no overlap with the time we measure health status. Moreover, we only include the years 2000 and 2001 in the regressions. As a result, the oldest individuals turned 8 years in January 2000 and they thus turned 9 in January 2001. The youngest ones, on the other hand, turned 6 in December 2001 and 9 in December 2004.

It may be pointed out that the older individuals in this sample are exposed to fees during some part of the time period when their health is being measured. In particular, the very oldest ones are exposed to fees for 12 months when we measure their health status from age 9 to 10. This time of exposure then decreases linearly with birth time. Only those who are 7 years or younger in January 2000 are never exposed to fees when health status is measured, as they turn 9 in January 2002. This feature could potentially influence the slope of our RD line to the right of the threshold. For each month to the right of the threshold, a larger share of individuals have their health status measured at a time when fees were in place. Importantly, however, only the slope to the right of the threshold should be different, but not the level. Since we allow for different slopes above and below the threshold, our RD estimate remains valid.

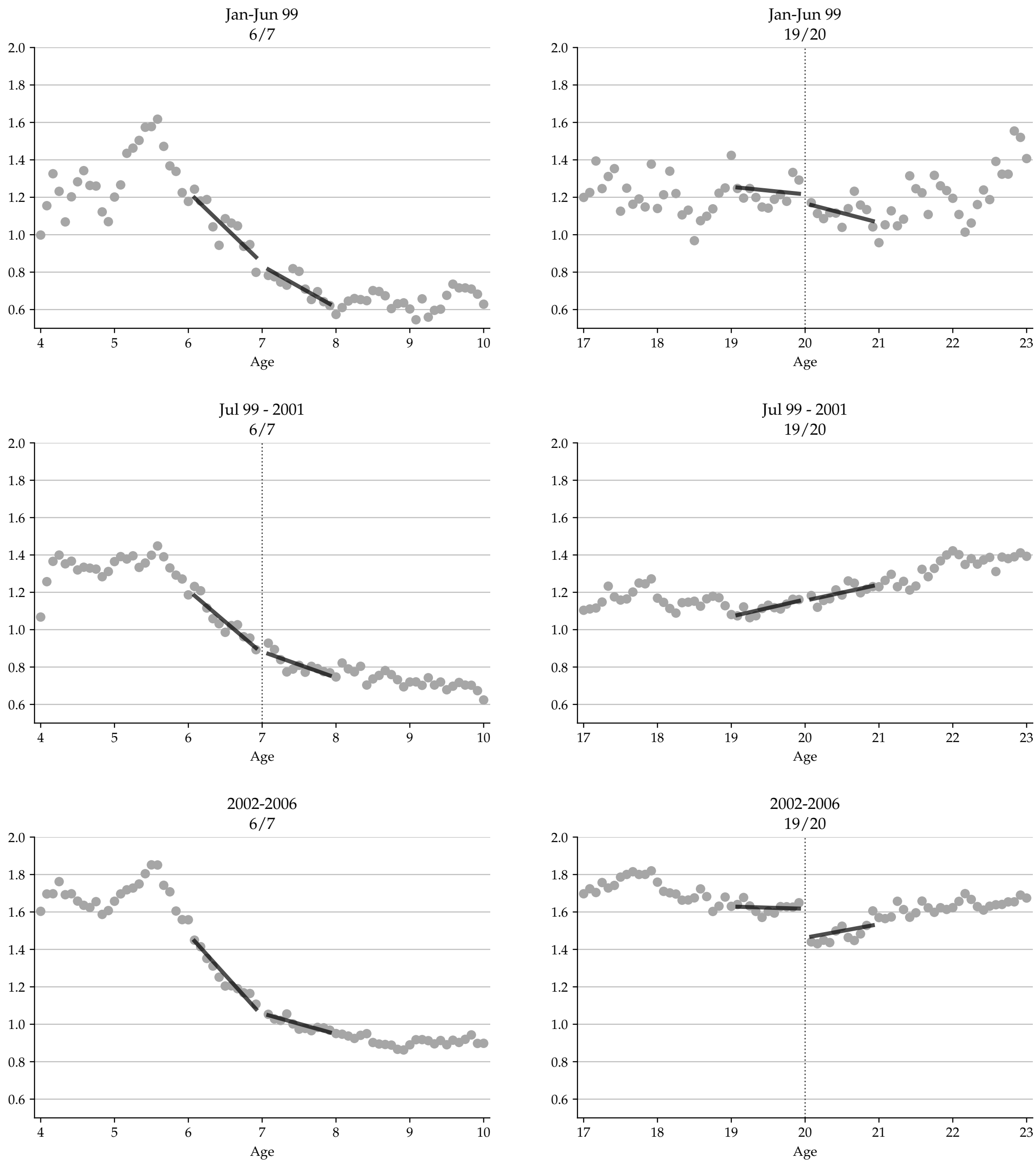
Analogously, for the 19/20 threshold, we measure health status at ages 16 and 17 years and include the years 2005 and 2006 in the regression. Given a bandwidth of 12 months above and below the threshold, the oldest individuals will turn 21 in January 2005 and thus turn 16 in January 2000. They will have 24 months of exposure to fees when their health status is being measured. The youngest individuals turn 19 years in December 2006 and 16 in December 2003.

Table A.1: Overview of chronic conditions and associated ICD-10 codes

Chronic Condition	*ICD-10 codes*
Neoplasms of uncertain or unknown behaviour	D37-D48
Diabetes mellitus	E10-E14
Obesity and other hyperalimentation	E65-E68
Metabolic disorders (incl. cystic fibrosis - E84)	E70-E90
Epilepsy	G40
Cerebral palsy and other paralytic syndromes	G80-G83
Visual disturbances (amblyopia)	H53
Chronic diseases of tonsils and adenoids	J35
Asthma	J45
Inflammatory polyarthropathies (rheumatoid arthritis) (incl. juvenile arthritis - M08)	M05-M10
Internal derangement of knee	M23
Deforming dorsopathies (Kyphosis, lordosis, scoliosis, spinal osteochondrosis)	M40-M42
Congenital malformations of eye, ear, face and neck (craniofacial anomalies)	Q10-Q16
Congenital malformations of the circulatory system (o.a. transposition of great vessels)	Q20-Q28
Sequelae of injuries, of poisoning and of other consequences of external causes	T90-T98

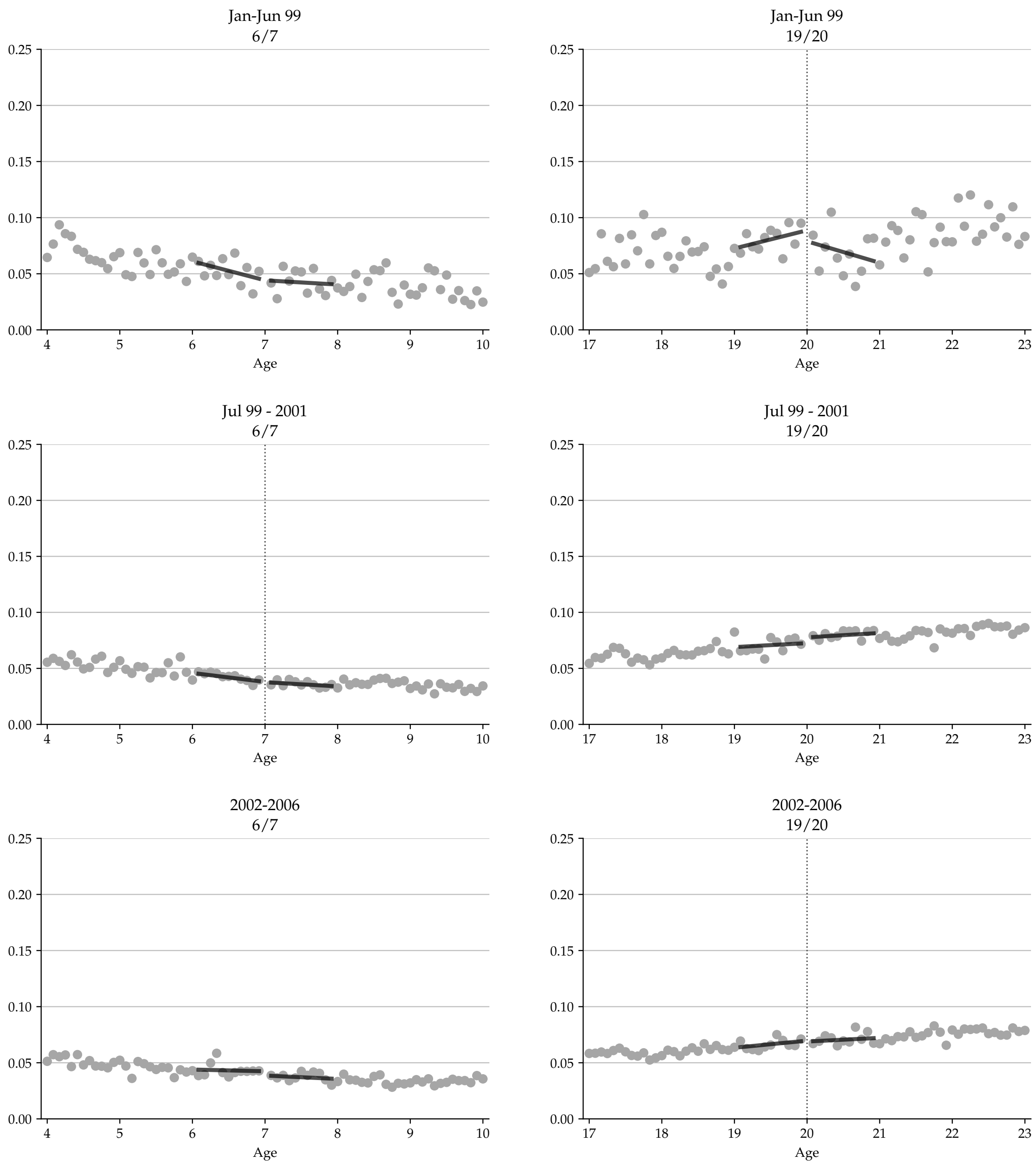
B Figures

Figure B.1: Non-doctor Visits Around Thresholds



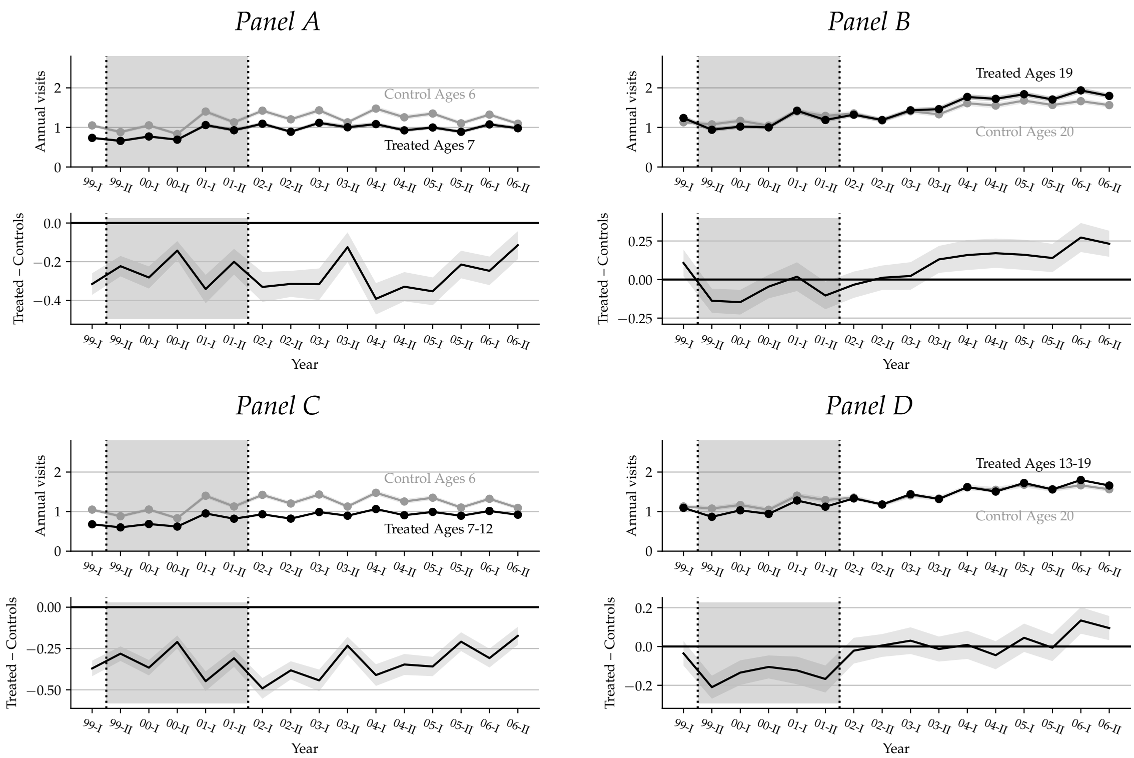
Notes: Annualized average number of non-doctor visits by age. Dots represent months. "6/7" indicates that the threshold is the month in which individuals become 7 years old; analogously for "19/20". Vertical dotted lines indicate that copayments stopped being charged to the right of the age threshold in the given time period. Dark lines are from fitted RD models according to the specification described in Section 3.4.

Figure B.2: Inpatient Visits Around thresholds



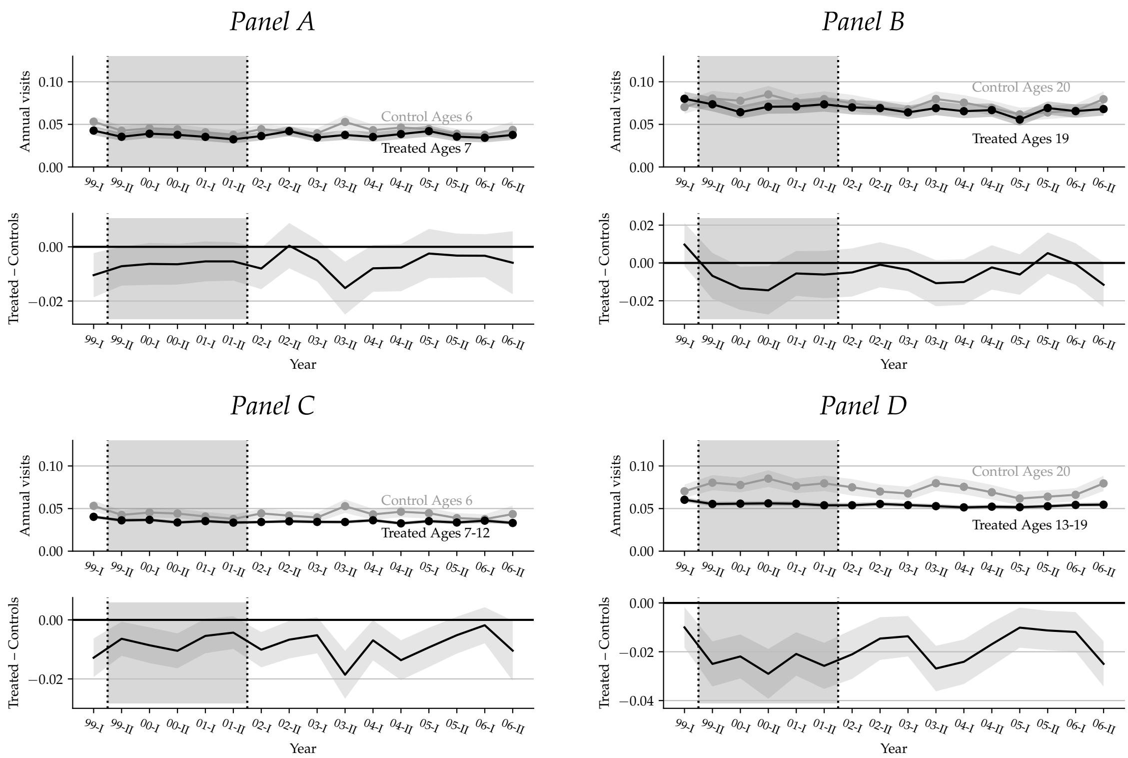
Notes: Annualized average number of inpatient visits by age. Dots represent months. "6/7" indicates that the threshold is the month in which individuals become 7 years old; analogously for "19/20". Vertical dotted lines indicate that copayments stopped being charged to the right of the age threshold in the given time period. Dark lines are from fitted RD models according to the specification described in Section 3.4.

Figure B.3: Non-doctor Visits between 1999 and 2006



Notes: In each panel, the upper graph shows the average annualized number of non-doctor visits by half-year over time, separately for treated and control ages. The lower graph shows the difference between treated and controls ages.

Figure B.4: Inpatient Visits between 1999 and 2006



Notes: In each panel, the upper graph shows the average annualized number of inpatient visits by half-year over time, separately for treated and control ages. The lower graph shows the difference between treated and controls ages.

C Tables

Table C.1: Effects on Doctor Visits at Placebo Age Thresholds

	Jan-Jun 1999						
	16-17	17-18	18-19	19-20	20-21	21-22	22-23
RD	-0.09	-0.06	0.07	0.26**	-0.10	-0.03	-0.05
	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.11)	(0.10)
N	134,252	134,571	135,355	135,131	135,423	137,855	143,949
RD-DiD	-0.10	-0.13	-0.00	0.27**	-0.11	-0.06	-0.01
	(0.09)	(0.09)	(0.10)	(0.10)	(0.11)	(0.11)	(0.11)
N	803,154	799,483	796,210	795,816	804,387	817,005	834,025
	Jul 1999-2001						
	3-4	4-5	5-6	6-7	7-8	8-9	9-10
RD	-0.23**	-0.02	0.08*	0.17**	0.12**	0.03	0.00
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
N	617,609	656,711	701,552	745,987	783,803	812,096	817,992
RD-DiD	-0.10*	-0.02	0.03	0.15**	0.07	-0.00	0.03
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
N	1,950,839	1,991,093	2,052,445	2,132,496	2,229,357	2,323,573	2,400,736
	2002-2006						
	16-17	17-18	18-19	19-20	20-21	21-22	22-23
RD	-0.06**	0.04	0.07**	0.20**	-0.06*	-0.03	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
N	1,501,880	1,442,183	1,371,854	1,320,973	1,308,325	1,323,418	1,348,257
RD-DiD	-0.07	-0.03	-0.00	0.20**	-0.08	-0.05	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
N	2,170,782	2,107,095	2,032,709	1,981,658	1,977,289	2,002,568	2,038,333

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. 'X-Y' indicates that the (pseudo-)threshold is the month in which individuals turned from X to Y years old. 'Free care' is equal to one if an individual is below the threshold. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Coefficients are scaled up by 12 to represent annual figures. Standard errors clustered at the person level are shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.2: Robustness to Choice of Controls and Functional Form

	Number of Doctor Visits			
	Baseline (1)	Log Visits (2)	With Controls (3)	Neg. Bin. (4)
Treated 7 Years, Controls 6 Years				
% Jan-Jun 99	5.60	5.77	6.39	5.10
% 2002-2006	6.34	6.40	6.19	6.04
N	2,128,563	2,304	1,879,197	2,128,563
Treated 19 Years, Controls 20 Years				
% Jan-Jun 99	9.83	10.09	10.67	9.28
% 2002-2006	10.33	10.34	10.49	9.97
N	2,116,806	2,304	1,375,152	2,116,806
	Likelihood of at least one doctor visit			
	Baseline (1)	With Controls (2)	Probit (3)	Logit (4)
Treated 7 Years, Controls 6 Years				
p.p. change	0.52	0.81	0.53	0.53
N	119,654	106,226	119,654	119,654
Treated 19 Years, Controls 20 Years				
p.p. change	2.20	1.89	2.19	2.20
N	118,279	86,609	118,279	118,279

Notes: Each column in each panel shows the treatment effect from a separate DiD regression. In the upper part, the dependent variable is the number of doctor visits per month. Column 1 reproduces the estimate from our baseline specification. Column 2 reports results from an log regression that group individuals by age (in months) and weights groups by size. The estimated coefficient can directly be interpreted as a percentage effect. Column 3 is the same as Column 2, except that we additionally control for municipality of residence, gender as well as maternal information on age, marital status, country of origin, unemployment benefits, employment status, education and income. Column 4 gives estimates from a negative binomial count data model that flexibly allows for overdispersion. In the lower part, the dependent variable is an indicator for whether an individual had any doctor visit in the current year. Once again, column 1 reproduces the baseline specification. Column 2 reports results from an individual-level-regression with the same additional controls as in column 3 in the top panel. Columns 3 and 4 report marginal effects from a probit and logit model, respectively.Regressions controls include fixed effects for age in months and month (upper panel) or age in years and the month in which this age was reached (lower panel).

Table C.3: RD Effects - With Sample Restricted to Individuals Living At Least With Mother

	A. RD	
	6/7	19/20
Free care	0.18**	0.18**
	(0.03)	(0.03)
Mean Above	2.09	1.82
% Change	8.6	9.9
N	669,531	769,893
	B. RD-DiD	
	6/7	19/20
Free care	0.16**	0.17**
	(0.04)	(0.05)
% Change	7.9	9.5
N	1,670,376	1,188,839

Notes: Each column in each panel shows the treatment effect from a separate estimation. 'Before Reform' includes the time period between July 1999 and 2001. 'After Reform' includes the years 2002-2006. The dependent variable is the monthly number of doctor visits. '6/7' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20'. '19/20' aggregates the time periods Jan-Jun 1999 and 2002-2006. 'Free care' is equal to one if an individual is below the threshold. Coefficients are scaled up by 12 to represent annual figures. Means are estimated just above the threshold and scaled up to annual figures. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Standard errors clustered at the person level are shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.

Table C.4: Effects on Doctor Visits by OECD Equivalised Disposable Income

	Overall		GP		Specialist	
Income group	1st Quartile	4th Quartile	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	(1)	(2)	(3)	(4)	(5)	(6)
6/7 Years	0.21***	0.13**	0.10**	0.11**	0.12***	0.01
	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)	(0.05)
p-value	-	0.41	-	0.84	-	0.08
%	*10.4*	*6.3*	*8.6*	*10.3*	*13.8*	*1.1*
19/20 Years	0.27***	0.05	0.10***	0.03	0.16***	0.01
	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)
p-value	-	0.01	-	0.21	-	0.02
%	*16.0*	*2.8*	*13.0*	*4.2*	*19.8*	*0.5*
RD-DiD						
6/7 Years	0.21**	0.01	0.07	0.06	0.13**	-0.05
	(0.09)	(0.09)	(0.06)	(0.06)	(0.06)	(0.06)
p-value	-	0.10	-	0.96	-	0.03
%	*10.5*	*0.5*	*6.1*	*6.1*	*15.0*	*-5.1*
19/20 Years	0.16	0.09	0.03	0.02	0.10	0.07
	(0.11)	(0.11)	(0.06)	(0.06)	(0.07)	(0.08)
p-value	-	0.65	-	0.89	-	0.77
%	*9.5*	*5.0*	*3.6*	*1.9*	*12.6*	*7.1*

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. '6/7 Years' indicates that the threshold is the month in which individuals become 7 years old; analogously for '19/20 Years'. '19/20 Years' aggregates the time periods Jan-Jun 1999 and 2002-2006. 'Free care' is equal to one if an individual is below the threshold. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Coefficients are scaled up by 12 to represent annual figures. Standard errors clustered at the person level are shown in parentheses. P-values are from t-tests of differences in free care effects in comparison with "Low" (1st Quartile).

Table C.5: Effects on Doctor Visits for Income Interactions – 19/20 – Jan-Jun 99 + 2002-2006

	Has No College		Has College	
Panel A	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.26**	-0.01	0.18	0.09
	(0.08)	(0.08)	(0.14)	(0.09)
Mean Above	1.89	1.88	1.75	1.78
N	150,113	107,263	40,713	83,313
RD-DiD	0.15	0.00	0.35	0.25
	(0.13)	(0.16)	(0.23)	(0.16)
Mean Above	1.89	1.88	1.76	1.78
N	230,261	164,161	61,572	126,833
	In Partnership		**Single**	
Panel B	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.18	0.03	0.25**	0.32
	(0.12)	(0.06)	(0.08)	(0.39)
Mean Above	1.78	1.83	1.89	1.75
N	56,168	188,114	136,416	4,214
RD-DiD	-0.08	0.12	0.30*	-0.15
	(0.19)	(0.11)	(0.13)	(0.72)
Mean Above	1.78	1.83	1.89	1.74
N	91,058	290,893	206,299	6,155
	Stays At Home		**Works**	
Panel C	1st Quartile	4th Quartile	1st Quartile	4th Quartile
RD	0.26	0.01	0.22**	0.03
	(0.16)	(0.36)	(0.07)	(0.06)
Mean Above	2.12	1.96	1.79	1.83
N	40,160	6,749	152,424	185,579
RD-DiD	0.15	0.18	0.19	0.11
	(0.26)	(0.55)	(0.12)	(0.11)
Mean Above	2.12	1.96	1.79	1.83
N	61,323	10,537	236,042	286,511

Notes: Each column in each panel shows the treatment effect from a separate estimation. The dependent variable is the monthly number of doctor visits. "In Partnership" is defined as being married or cohabiting. "Stays At Home" is defined as having zero earnings."Has No College" is defined as having only elementary or secondary education. 'Free care' is equal to one if an individual is below the threshold. Coefficients are scaled up by 12 to represent annual figures. Means are estimated just above the threshold and scaled up to annual figures. Using a bandwidth of 12 months, we estimate local linear regressions that allow for varying slopes on either side of the threshold. Standard errors clustered at the person level are shown in parentheses. * and ** denote significance at the 5 and 1 percent level, respectively.