Research Article

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Priced Out: Do Adolescents from Low-Income Families Respond More to Cost-Sharing in Primary Care?

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Abstract: We examine the heterogeneous effects of 14–21 euro copayments on primary care general practitioners (GPs) visits in Finland. Our study focuses on a triage-based appointment system in public primary care. Using an age-based regression discontinuity (RD) design and leveraging variation across Finnish municipalities in whether the copayment is charged, we analyze the effects at the 18th birthday, when previously exempted adolescents become subject to copayments. Using nationwide administrative data from 2011 to 2019, we find that GP visits decrease in the copayment municipalities by 4–5 %. The reductions are largest for the bottom 20 % of the equivalized family disposable income distribution: their GP use decreases by 0.08–0.10 annualized visits (7–10 %). Unexpectedly, the effects are also larger than average (albeit rather temporary) for the top 50 %, showing reductions of 6–8 %. Compared to earlier studies focusing on moderate copayments and different populations, our effect estimates are smaller, and the heterogeneity by income level is weaker.

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1 Introduction

Cost-sharing is used to finance healthcare services and allocate resources to those in need. In high-income countries, populations are rapidly aging, demand for primary care rising, and shortage for healthcare professionals growing. Policymakers are therefore seeking tools to mitigate healthcare spending without negatively affecting population health. There is compelling evidence that out-of-pocket costs reduce the demand for healthcare (Einav and Finkelstein 2018). However, increased cost-sharing can amplify inequality, as it constitutes a larger fraction of disposable income for low-income households. As a result, cost-sharing schemes may create a greater barrier to access for low-income people compared to the rest of the population, potentially leading to poor health outcomes if low-income patients are unable to consult a doctor for financial reasons. This concern is exacerbated by the fact that low-income individuals typically require more health services than the more affluent.

We analyze how copayments of 14–21 euros affect the general practitioner (GP) use in a public primary care system where appointments are allocated based on triage. To do so, we use an age-based regression discontinuity (RD) design exploiting the fact that individuals under the age of 18 are nationally exempted from GP visit copayments in Finland. We observe both municipalities charging the copayment (copayment municipalities) from adults and municipalities either exempting students from the copayment or charging no copayment at all (exemption municipalities).[1] Our main analysis is based on the age-based RD design at the copayment and exemption municipalities. The identification challenge with the RD analysis is that there are many changes that happen at the age 18 cutoff when person becomes legally adult. Therefore, we complement the RD analysis by evaluating the effects at placebo cutoffs (same age cutoff but in the exemption municipalities), which works as a test for confounders. Relatedly, we estimate models that subtract from the discontinuities in the copayment municipalities the discontinuities observed in the exemption municipalities (difference-in-discontinuities, RD-DID). Similar to the

1 Municipalities form primary care areas that set copayment policies within nationally-set constraints.

placebo RD analysis, this RD-DID setting allows us to account for other factors that potentially cause discontinuities at the 18th birthday (preferable for unbiased point estimation), but it has less statistical power (inferior for inference).

Using nationwide administrative data from 2011 to 2019, we focus on the potentially heterogeneous effects by income level.[2] We estimate the effects only for women as the identification assumptions are not plausible for men due to compulsory military conscription. The key policy question is whether the effects vary by income level even in an institutional setting characterized by moderate copayments, gatekeeping at the point of entry, waiting times for non-urgent care, and extensive social safety nets. Despite Finland's universal health care system, notable differences in health care consumption by income level persist, affecting health outcomes and widening socioeconomic inequalities (Kangas and Blomgren 2014). Examining these patterns can help identify system gaps, improve resource allocation, and address barriers faced by lower-income groups.

The question of whether copayments reduce health care use among young patients and disproportionately among long-income adolescents is of interest for several reasons. The utilization of appropriate primary health care services at a younger age is a key element of preventive care and can lead to substantial long-term health benefits (Starfield, Shi, and Macinko 2005). Notably, during adolescence and early adulthood individuals are highly receptive to new experiences, which can shape their long-term attitudes, behaviors, and preferences (Steinberg 2005). As a result, early health care engagement can foster a lasting connection to the health care system, promoting sustained access and utilization of these services over the course of life. Moreover, limiting the access of young women to health services may have dire long-term consequences on their mental health: anxiety has been increasing most notably among young women (OECD and Policies 2023), and the largest health differences between low-income women and either high-income women or men in general is the prevalence of mental health issues. These concerns are more pronounced when it comes to low-income women, but it is a concern regardless of the income level.

Our results show that GP visits decrease by 4–5 % in the copayment municipalities at the 18th birthday. These RD estimates are statistically significant, while the estimates in the comparison municipalities are close to zero and insignificant. The RD-DID estimates are consistent with the RD results, but due to their lower statistical power are statistically insignificant.[3] Regarding the magnitudes, the RD estimates

2 We use the equivalized family disposable income in the year when an individual turns 17.

3 The RD-DID estimates are by construction less precise. These estimates compare the difference between two RD estimates, rather than comparing a single RD estimate to a fixed value of zero without statistical uncertainty.

for all individuals vary, depending on the estimator, between − 0.04 and −0.05 annualized visits (−4.8 % to −5.8 %) in the copayment municipalities compared to the estimates of −0.00 to −0.01 visits (−0.3 % to −1.0 %) in the exemption municipalities. Regarding the potential income-related heterogeneity, we do find that the largest reductions in the RD-DID estimates are for the bottom 20 % of the income distribution. Their GP use decreases by 0.08–0.10 annualized visits (7–10 %). The estimates attenuate for the bottom 40 %. However, the estimates are also larger than average for the top 50 %, showing reductions (albeit rather temporary) of 0.05–0.06 visits (6–8 %). Thus, our results do not support the hypothesis that the effects are overwhelmingly concentrated at the lower end of the income distribution while the top end does not respond noticeably. The modest average effects and the lack of significant income-related heterogeneity in the effects partially alleviate popular policy worries about the unequal impacts of the copayment examined in this context.

RD designs based on age cutoffs are popular in the quasi-experimental literature examining the effects of cost-sharing on healthcare use.[4] Prominent studies by Card, Dobkin, and Maestas (2008), Shigeoka (2014), and Fukushima et al. (2016) use variation in out-of-pocket costs stemming from changes in insurance status or coinsurance level and examine the effects for the elderly at age cutoffs 65 and 70. Recently, most studies in this literature have focused on adolescents for whom many countries provide exemptions. Vargas Lopes et al. (2022) examine the effects of an increase in the annual deductible on mental health care use at the 18th birthday in the Netherlands. Also in the Netherlands, Remmerswaal et al. (2019) estimate the impacts of out-of-pocket costs on healthcare spending at the 18th birthday under two different cost-sharing schemes (a deductible and a rebate). Han, Lien, and Yang (2020) focus on copayment exemptions that expire at the 3rd birthday in Taiwan.

Our analysis is most closely related to the empirical studies from other Nordic countries that examine the effects of copayments on public primary care GP use at age thresholds 7, 16, and 20 in Sweden and Norway (Johansson, Jakobsson, and Svensson 2019; Magnussen Landsem and Magnussen 2018; Nilsson and Paul 2018).[5] Similarly to Finland, Sweden has a primary care system where access to GP appointments is based on triage. This is a crucial aspect of the institutions: gatekeeping

4 Besides the age-based RD designs, there are studies that exploit income-based discontinuities in cost-sharing policies (Chandra, Gruber, and McKnight 2014; Serna 2021).

5 In Finland, Norway and Sweden, individuals typically face copayments for the first time for primary care when they turn 16, 18 or 20. While healthcare systems are quite similar in different Nordic countries, there are also important differences. Perhaps most importantly, Finland's reliance on the private sector and occupational healthcare to supplement the public system in primary care stands out compared to its Nordic neighbors.

should disproportionately reduce GP appointments that are of low value medically, and thus, the effects of the copayment on GP use should be smaller.

We contribute to the broader cost-sharing literature by analyzing in detail whether low-income adolescents are more responsive to out-of-pocket costs. In the early 2010s, the evidence for higher price sensitivity among the low-income households was "suggestive" but "less than fully reliable" (Baicker and Goldman 2011). Of the above-mentioned studies, Nilsson and Paul (2018), Johansson, Jakobsson, and Svensson (2019), and Vargas Lopes et al. (2022) use income as a key stratifying dimension, all finding that low-income individuals are more affected both in absolute and relative terms.[6] More recently, Haaga et al. (2024) studied the effect of abolishing a 14-euro copayment for visits to GP in Helsinki, the capital of Finland. They find that the abolition was associated with only a small increase in GP visits (+0.04 visits annually), but the increase was driven by low-income adults. Unlike Haaga et al. (2024), who analyzed a change policy in Helsinki affecting all adults, our study focuses on an age-based cutoff. We complement their findings by examining a larger geographic area, including more rural municipalities, even if a more narrow age group. Also the identification strategy differs. While our paper is the first Finnish study using RD design in this context, it is also one of the first age-based RD studies in the copayment literature that accounts for the possibility of confounding from other events at the milestone 18th birthday.

We contribute to the literature also both data-wise and by using more state-of-the-art estimation procedures than those previously applied in this specific context. First, we combine recent data from 2011 to 2019 with a design that also has comparison municipalities which either exempt students or charge no copayment at all. This setting stands in contrast to Nilsson and Paul (2018), who use older data from 1999 to 2006, as well as Johansson, Jakobsson, and Svensson (2019) and Magnussen Landsem and Magnussen (2018), who use more recent data but in a setting where the copayment affects the whole study population. Using up-to-date data is essential because the composition of patients has most likely changed over time as the widespread adoption of internet and online symptom checkers have made self-diagnosing much easier, and some tasks have been shifted from GPs to nurses. Importantly, the availability of comparison municipalities allows us to account for discontinuities in the potential outcomes that are unrelated to the copayment policy, as Nilsson and Paul (2018) do. The age cutoffs of 20 in Johansson, Jakobsson, and Svensson (2019) and 16 in Magnussen Landsem and Magnussen (2018) are likely

6 Johansson, Jakobsson, and Svensson (2019) examine the effect of a copayment on GP use, Nilsson and Paul (2018) report the effect of a copayment on outpatient doctor visits, and Vargas Lopes et al. (2022) study the effect of an increased deductible on mental health care use.

less problematic than our cutoff of 18 with respect to other factors creating discontinuities in the potential outcomes. Therefore, the comparison municipalities and RD-DID models have greater value in our application. Second, because our administrative data contain both exact birth dates and daily visits, which is more granular than in the earlier studies, we can use appropriate optimal bandwidths, and bias-corrected and robust inference methods (Calonico, Cattaneo, and Titiunik 2014). We also account for a discrete running variable (Kolesár and Rothe 2018).

Section 2 introduces the institutional background and Section 3 the data. Section 4 presents our empirical approach. Section 5 reports the results, and Section 6 compares them to the earlier literature. Section 7 concludes. Our Supplementary Material contains additional supplementary analyses and a description on data construction.

2 Institutional Background

Publicly funded health centers and private clinics are the main providers of curative GP visits for Finnish adolescents around the 18th birthday. There is an income gradient in the selection into these two sectors.[7] Public primary care and private clinics compete for GPs whose unemployment is very low or nonexistent. The share of unfilled vacancies in public health centers has been 3.5–6.5 % in 2011–2018 (THL 2020). Public primary care is characterized by moderate copayments, gatekeeping, and waiting times. For GP appointments, patients contact their designated health station that is often geographically the closest. Nurses do triage and make bookings. Private clinics offer fast access to specialists without gatekeeping or a referral, but out-of-pocket costs are much higher.[8] In 2016, almost half of the families with children and two adults had a voluntary private insurance plan (e.g. to pay for private fees), while only 6 % of households with individuals above 64 years of age had one. The higher the income, the more common it is to have a private insurance plan (Kajantie 2019). The share of the insurance contracts expiring at the 18th birthday is unclear.

Moreover, school and student healthcare is organized by municipalities and provides preventive services concerning mental, sexual and dental health, and substance abuse. It varies across municipalities whether curative GP appointments

7 In our data, family equivalized disposable income was lower for those who had a public primary care visit in the year in which they turned 17 or 18, the mean (median) being 22,4 k (21,0 k) euros, compared to those who did not, the mean (median) being 29,5 k (25,0 k) euros.

8 In January 2020, a GP visit of 20 min cost 86.70 euros at the largest private healthcare service company. The reimbursement was 9.00 euros for visits of this length. The maximum copayment for a GP visit at publicly funded primary care was 20.60 euros.

are available and whether there is a copayment. Finally, occupational healthcare provides for many employees a fast access to curative services without charging copayments, but there is some gatekeeping.

Municipalities organize publicly funded primary care and run a health center (with potentially many operating locations) on their own, in voluntary cooperation with others, or outsource the services. Services are financed by municipal taxes, state transfers, copayments, and borrowing. The state sets both the maximum copayments and which services are offered free of charge. The maximum GP visit copayment increased from 13.70 euros to 20.60 euros in 2011–2019.[9] Municipalities set their policies given these constraints. Individuals under 18 years of age are nationally exempted from GP visit copayments. At least three municipalities (Espoo, Turku, and Tuusula) also exempt students with a student status certificate, available free of charge from the school chancellery. The capital Helsinki is the only municipality not charging copayments for GP visits. It abolished the copayment in 2013. In 2018, over two thirds of the population lived in municipalities charging the copayment for the first three visits annually and 15 % in municipalities charging an annual copayment that is in most cases two times the per-visit copayment (Haaga 2019). Social assistance is a means-tested last-resort benefit for those households with the lowest incomes and wealth, and unfortunately underused. Social assistance can also cover out-of-pocket costs for public health care and prescription drugs.

3 Data

We combine four national individual-level Finnish administrative registers containing public primary care contacts, specialized healthcare contacts, data on social assistance recipients, and the individuals' socioeconomic characteristics.[10] We use data from 2011 to 2019.[11] The data collection on primary care contacts started in 2011, and we restrict the analysis to the pre-pandemic (COVID-19) times. We also use publicly available data on municipal mergers from the Association of Finnish Municipalities.

9 The at-risk-of-poverty threshold was 1,190 euros per month for single-person households in 2016.

10 The names of the registers are the following: Register of Primary Health Care Visits, Care Register for Health Care, Register of Social Assistance, and FOLK (modules: Basic, Income, and Family). The first three are administered by the Finnish Institute for Health and Welfare and the fourth by Statistics Finland.

11 There are two exceptions. We use social assistance data from 2012 to 2018 and socioeconomic data from 2011 to 2020.

We searched whether students over 18 years of age pay copayments for curative GP visits by observing the websites of the 30 largest municipalities. The policy was clearly and explicitly stated in 23 cases. 19 municipalities charged copayments in early 2020, 3 (Espoo, Turku, and Tuusula) exempted individuals showing a student status certificate, and 1 (Helsinki) charged no copayment at all. We assume that the policies were the same over the study period.[12] The copayment municipalities contain both municipalities offering the annual copayment option and municipalities charging a copayment for the first three visits annually. There is also variation in copayment levels across municipalities (in 2016–2019) and over time (the maximum copayment increased from 13.70 euros to 20.90 euros in 2011–2019).

Section A.3 in the Supplementary Material provides the details of data extraction and construction. To summarize, we create person-date panels, which we use to construct estimator-specific analysis datasets (see Section 4).

Throughout analyses, we estimate the effects separately in the following subgroups based on the distribution of family equivalized disposable income: all individuals, bottom 20 %, bottom 40 %, and the top 50 %. Income is measured for the year when the individual was 17 years old at year's end.[13] The bottom 20 % includes many adolescents who have recently moved away from their parents. To provide a sense of the average incomes in these four income groups, the mean equalized family disposable income was € 22,4 k for those women in our sample municipalities who turned 17 and had a primary care visit during a year in which they turned 17 or 18. Considering the same income distribution, the mean income was € 9,1 k for the bottom 20 %, € 13,1 k for the bottom 40 %, and € 34,5 k for the top 50 %.

4 Methods

4.1 Identifying Assumptions

The key identification assumption in RD designs is that the expected potential outcomes are continuous functions of the running variable at the cutoff (Hahn, Todd, and van der Klaauw 2001). We focus on women and exclude men from the analyses because health checks before conscription and corresponding primary care use concentrate around the 18th birthday for men (Figures A1 and A2), challenging the

12 Exemptions were in place in Turku and Tuusula over the whole study period. Espoo made the exemption decision in August 2011. Helsinki abolished the GP visit copayment in January 2013. These exemptions are rare, and unlike nominal copayment levels, they are not adjusted regularly.

13 A time-varying measure for income would mechanically decrease for most adolescents after they move away from their parents.

RD design. Every male takes part in a call-up during the year they turn 18. They have nurse and GP appointments before the call-up, often in Spring. Women can serve voluntarily, but it is rare: no more than 5 % of the birth cohort applied for service in a record year of 2018. Women do have a GP-led health check during the first or the second year of their upper secondary education, beginning at the age of 15 or 16.

After turning 18, Finns can obtain a driver's license and legally buy and consume alcoholic drinks containing at most 22 % alcohol by volume. However, increases in accidents and alcohol misuse are most likely visible in emergency departments instead of primary care. A suitable health certificate for a driver's license is given at a health check at the 8th or the 9th grade, two to three years before the 18th birthday. Driving can reduce travel times and thus indirect costs of access. The potential bias is likely small as our sample contains large municipalities with public transport available and most adolescents live with their parents who probably have a driver's license.

Years 17–19 are characterized by two transitions for many individuals: moving away from parents and from school to work (mainly after vocational education). There are institutions that incentivize these transitions at the 18th birthday, discussed in Section A.4. We expect that the potential discontinuities in school-to-work transitions and between-municipality migration are small. The end of classroom learning at the end of upper secondary education is a natural transition point and unrelated to our running variable. However, the same argument does not apply to most within-municipality moves. Based on the data, 16 % of those who lived with their parents at year's end when aged 17 moved away by the end of the next calendar year, proxied by a change in family relationship status. 63 % of these moves were within the same municipality. Moves are more common in municipalities charging the copayment than in our comparison municipalities. There also appears to be selection in moving: those whose policy area (copayment, exemption, or not in sample) changes have an average a lower mean income decile and higher public primary care use.

Figure A3 examines the potential discontinuities at the 18th birthday for those born at the turn of the year.[14] Both the number of GP visits in the study period and equivalized disposable income appear to be continuous at the cutoff in both policy groups. As hypothesized, there may be a small reduction at the 18th birthday in the share of those living with their parents in both the copayment and exemption municipalities and potentially a small increase in the share being unemployed.

14 Those born on January 1st are observed in age cells $t \in \{-366, -1, 364\}$ and those born on December 31st are in age cells $t \in \{-365, 0, 365\}$.

4.2 Accounting for Potential Biases from Moves

To account for other factors that affect potential outcomes discontinuously at the 18th birthday but similarly in both policy areas, we estimate difference-in-discontinuities (RD-DID) models (Grembi, Nannicini, and Troiano 2016) that subtract from the discontinuities in the copayment municipalities (a clear discontinuity in policy) the observed discontinuities in the exemption municipalities (either no copayment or an exemption for students). However, our main analysis is based on RD models due to their higher statistical power. For clarity, we report the effects separately in the copayment and exemption municipalities to allow for an easy comparison between the estimates.

Our second approach to account for the potential bias is to restrict to subsamples with less or no moves at the cutoff. In Alternative Sample 1, there are only individuals who are observed to reside in the same policy area (either copayment or exemption) 6 months before and after the 18th birthday. Thus, between-policy-area migration should not cause concern. Alternative Sample 2 is restricted to those individuals for whom we observe no change in policy area or in family relationship status (a proxy for moving away from parents) between year's end when aged 17 and year's end when aged 18. Figure A4 shows the corresponding population sizes by age cell. We first report the effects for the main analysis sample before examining whether the results are robust to the sample definition.

4.3 Donut Hole

Unless otherwise stated, we exclude observations within the 3-day bandwidth (donut hole), because we observed that GP use is noticeably lower on the 18th birthday and the following two days both in copayment and exemption municipalities (Figure A5). The pattern is similar at both ends of the income distribution and likely unrelated to copayment policies.

4.4 RD Estimates

As mentioned above, we estimate RD models separately in the copayment and exemption municipalities. Methods based on the nonparametric local polynomial approach allow us to use data-driven bandwidth selection and inference that accounts for misspecification or smoothing bias (Cattaneo, Idrobo, and Titiunik 2020). Reducing the bandwidth size can reduce the smoothing bias, but smaller sample sizes increase variance. To address this trade-off, we use a data-driven bandwidth selector that leads to an MSE-optimal point estimator (Calonico, Cattaneo,

and Titiunik 2014) using the R package *rdrobust*.[15] To construct confidence intervals, the smoothing bias is removed by first estimating it using another local regression – one order higher. Valid inference must account for both regressions. Ultimately, the confidence intervals are rescaled and recentered around the bias-corrected point estimate.

Specifically, we use the local-linear point estimator with an MSE-optimal bandwidth selector allowing for different bandwidths below and above the cutoff.[16] The smoothing bias is estimated with local-quadratic regression. We use the triangular kernel function that symmetrically and linearly decreases the weights once we move away from the cutoff. Our running variable is discrete (days relative to the cutoff), and the effective sample size is the number of mass points (Cattaneo, Idrobo, and Titiunik 2018). Hence, we aggregate the data at the running variable level and weight the regressions by cell population size multiplied by the kernel.

4.5 RD-DID Estimates

In the complementary RD-DID analysis, we use local linear regression in an *ad hoc* 180-day bandwidth (approximately 6 months) with the uniform kernel, weighting by age cell size. The data are aggregated at the age-cell-by-policy-area level (sample size: 2× bandwidth). Confidence intervals are based on robust standard errors.[17] We use the following regression specification:

$$\begin{aligned} y_{gt} = {} & \alpha + \beta_1 \left(Age - Threshold\right)_{gt} + \gamma Adult_{gt} + \beta_2 \left(Age - Threshold\right)_{gt} \times Adult_{gt} \\ & + \kappa Copay_g + \beta_3 Copay_g \times \left(Age - Threshold\right)_{gt} + \delta Copay_g \times Adult_{gt} \\ & + \beta_4 Copay_g \times \left(Age - Threshold\right)_{gt} \times Adult_{gt} + \varepsilon_{gt} \end{aligned} \tag{1}$$

where α is an intercept, g and t denote policy group (copayment and exemption) and day. *Age* is in days, *Threshold* is the cutoff, *Adult* is a dummy for being over

15 This data-driven (MSE-optimal) approach allows for a different bandwidth below and above the cutoff (Calonico, Cattaneo, and Titiunik 2014). For the main RD results, the optimal left bandwidth is 182–247 days in the copayment municipalities and 169–270 days in the exemption municipalities (Table 1). The optimal right bandwidth is 143–176 days in the copayment municipalities and 174–211 days in the exemption municipalities.

16 The local polynomial results for Alternative Sample 1 and Alternative Sample 2 are an exception. For these subsamples, we prefer a fixed bandwidth of 180 days to have more data for estimation. Still, we show the sensitivity of the results to different bandwidth choices.

17 The effective sample size in RD designs with a discrete running variable is the number of mass points (Cattaneo, Idrobo, and Titiunik 2018). This motives the aggregation of our analysis dataset and the use of robust SEs.

18, and *Copay* is a dummy for copayment municipalities. The parameter δ is the coefficient of interest.

4.6 Comparing the Main RD and RD-DID Results

We view the RD-DID approach to be complementary for our main RD analyses. For robustness, it is important to show also the RD-DID results, because accounting for the potential discontinuities in the comparison municipalities likely reduces the bias of the effect estimates. However, for inference and precision, we prefer the RD results in the copayment municipalities as we think that inference is unnecessarily conservative in the RD-DID models that are by construction less precise. RD-DID models essentially compare two RD estimates that have their own standard errors, while RD models compare one RD estimate with a fixed zero that has no statistical uncertainty. If the placebo RD estimates in the comparison municipalities are close to zero (as is mostly the case), it may be reasonable to simply assume that there are no confounding discontinuities at the 18th birthday and analyze the data with RD models.

Addressing the trade-off between precision in RD analysis and more reliable identification in RD-DID analysis is case-specific. In general in age cutoff designs, the loss of power from RD-DID may not be worth the cost given that not all age cutoffs are suspect to confounders. However, in our case we cannot *a priori* rule out that age cutoff at 18 would not be susceptible to confounders. This would make the RD-DID preferable for unbiased point estimation in our specific case. On the other hand, in our view, a good compromise between precision and identification can be achieved by running a separate RD analysis on both the treatment group and the placebo control group. The first achieves precision in the treatment effect of interest and the second tests for confounders.

5 Results

5.1 Main Results

When interpreting the results, we should keep in mind that they concern only young women. The RD plots are presented in Figure 1. In copayment municipalities, there appears to be a drop of 0.05 annualized visits per capita for all individuals at the cutoff and a larger reduction for the lower end of the income distribution. A somewhat similar decrease seems to be present also for the top 50 %, but the effect is less obvious as the data are noisier and the effect arises solely from observation very close to the cutoff. In the exemption municipalities, there are no clear indications of

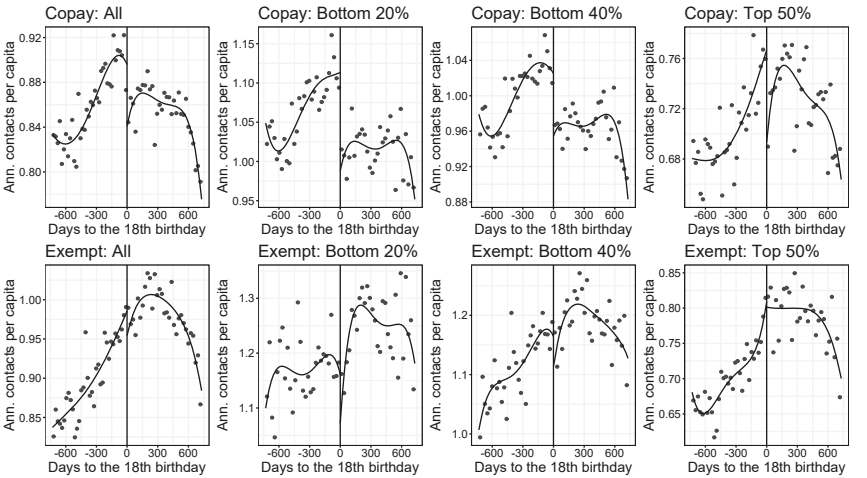

Figure 1: RD plots. *Notes:* Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both municipalities where students over 18 years of age are exempted if they show a student status certificate and Helsinki, where no copayment is charged. Bottom 20 % and 40 % and top 50 % are based on the distribution of equivalized family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The number of evenly spaced bins is selected in a data-driven way to mimic the underlying variability of the raw data while still smoothing the scatterplot of the data (Calonico, Cattaneo, and Titiunik 2015). The sample only includes women.

substantial discontinuities. Figure A6 shows the same RD plots but uses data only from within the optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014) and fits linear splines instead of fourth-order polynomials. Also this figure indicates a robust effect for bottom income groups but a more sensitive and less persistent effect for the top 50 % group.

The main RD results based on local polynomial methods are presented in Table 1. For inference and precision, we prefer the local polynomial RD estimates that are reported separately in the copayment and exemption municipalities. These results are depicted graphically in Figure A6 as RD plots. In copayment municipalities, turning 18 is associated with a reduction in GP use: −0.05 annualized visits (−5.7 %) for all individuals. The corresponding estimate is close to zero in exemption municipalities: −0.01 annualized visits (−0.5 %). In both policy areas, the reductions are largest at the bottom 20 % of the income distribution. For them, GP visits decrease by −0.12 visits (−10.8 %) in the copayment municipalities and by −0.03 visits (−2.9 %) in the exemption municipalities. For the bottom 40 %, GP visits decrease by −0.07 visits (−6.7 %) in the copayment municipalities and by

Table 1: Main results.

	All	Bottom 20 %	Bottom 40 %	Top 50 %
A. RD results in copayment municipalities.				
Level	0.898	1.118	1.031	0.765
RD estimate	−0.051	−0.121	−0.069	−0.068
Change, %	−5.704	−10.831	−6.655	−8.898
P-value	0.008	0.000	0.003	0.018
CI	[−0.09, −0.01]	[−0.19, −0.06]	[−0.11, −0.02]	[−0.13, −0.01]
Individuals	65,367	17,859	31,850	26,653
Bandwidth	(182, 152)	(247, 170)	(221, 176)	(225, 143)
B. RD results in exemption municipalities.				
Level	0.983	1.161	1.171	0.805
RD estimate	−0.005	−0.034	−0.029	0.018
Change, %	−0.499	−2.894	−2.517	2.235
P-value	0.980	0.616	0.705	0.675
CI	[−0.06, 0.06]	[−0.19, 0.11]	[−0.12, 0.08]	[−0.07, 0.11]
Individuals	27,746	7,171	12,540	12,803
Bandwidth	(270, 178)	(169, 208)	(192, 211)	(180, 174)
C. RD-DID results.				
Level	0.891	1.081	1.004	0.769
RD-DID estimate	−0.046	−0.089	−0.046	−0.063
Change, %	−5.147	−8.206	−4.585	−8.149
Std. error	0.031	0.062	0.046	0.041
P-value	0.135	0.150	0.317	0.130
Individuals	93,113	25,030	44,390	39,456

Notes: The RD estimates are based on the local-linear point estimator with an MSE-optimal bandwidth selector allowing for different bandwidths below and above the cutoff, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. We use robust bias corrected inference (Calonico et al. 2014): CIs may be rescaled and recentered. Bottom 20 % and 40 % and top 50 % are based on the distribution of equivalized family disposable income, measured for the year when the individual was 17 years old at year's end. The RD-DID estimates are based on Model 1, a 180-day bandwidth and the uniform kernel. We aggregate the data at the policy-group-by-relative-time level before estimation, use population size weights, and report robust standard errors. We compare the copayment municipalities to the exemption municipalities and Helsinki. For RD-DID results, level is the fitted mean for the copayment municipalities at the cutoff. For RD results, level is the fitted mean just below the cutoff. We report the number of individuals in the age cell just below the cutoff and the chosen bandwidths in days below and above the cutoff. The sample only includes women. The CIs are based on the 95 % confidence level.

−0.03 visit (−2.5 %) in the exemption municipalities. For the top 50 %, we estimate effects of −0.07 annualized visits (−8.9 %) in the copayment municipalities and +0.02 annualized visits (+2.2 %) in the exemption municipalities.

The RD results are all statistically different from zero in the copayment municipalities, but closer to zero and insignificant in the exemption municipalities. The largest *p*-value in the copayment municipalities is 0.02, estimated for the top 50 %. In the exemption municipalities, the estimate for all individuals is the most precise estimate and close to zero (−0.5 %). It thus appears a reasonable assumption for inference that the confounding discontinuities are zero at the cutoff.

The RD-DID models (Model 1), which we prefer for unbiased point estimation, are by construction more conservative with respect to statistical inference. The models essentially compare the RD estimate in the copayment municipalities to the RD estimate in the exemption municipalities that is close to zero but estimated with noise.[18] The RD-DID estimate for all individuals is −0.05 annualized visits (−5.1 %). The largest effect in absolute terms is found for the bottom 20 %: −0.09 annualized visits (−8.2 %). The estimate for the bottom 40 % is −0.05 annualized visits (−4.6 %). Interestingly, the estimate for the top 50 % is also above average: −0.06 annualized visits (−8.1 %).

Both the RD-DID results and the RD estimates in the copayment municipalities show that the largest reductions in GP use are at face value for the bottom 20 % of the income distribution. Other noticeable patterns are the clear attenuation of the estimates at the bottom 40 % and the estimates for the top 50 % that are above average. Considering all evidence – both these baseline results and robustness checks reported below – our data do not overwhelmingly support the conclusion that the average effects are mostly driven by the poor and individuals from high-income households do not respond to the copayment. Neither do our results provide evidence of a clear difference in the magnitude of the estimates between the bottom 20 % and the top 50 % of the income distribution. In relative terms, we do not find any difference between these income groups. On the other hand, the effect estimates for the top 50 % are less persistent and driven by a smaller number of observations, indicating that they are perhaps more sensitive than results for the bottom of the income distribution. Moreover, family disposable income, which we use and which essentially is parental income for those minors who live with their parents, may differ from an individual's own income in adulthood. Therefore, our results should not be taken as a challenge against the evidence of the existence of the income gradient in responses to copayments.

18 The sample sizes in the exemption municipalities are less than half of the sample sizes in the copayment municipalities. All the RD-DID estimates are statistically insignificant. The *p*-value for all individuals is 0.14.

5.2 Robustness Checks and Supplementary Analyses

5.2.1 Placebo Cutoffs

As a falsification test, we estimate the effects at all placebo cutoffs occurring every 30 days for which we have a 150-day bandwidth with the restrictions that (1) we only use data from one side of the real cutoff in a given run and (2) we consider data from 730 days before and after the real cutoff.[19] Figure A7 contains the results for the RD setting and Figure A8 for the RD-DID setting. The RD results show that the largest reduction in GP use in the copayment municipalities for each subgroup is induced by the real cutoff. However, in the exemption municipalities the real RD estimates do not stand out from the placebo estimates. In contrast, the RD-DID estimates based on the real cutoff are not that extreme compared to a distribution of placebo effects estimated at different placebo cutoffs. For instance, the real estimate constitutes the fourth largest reduction out of 31 estimates for all individuals.

5.2.2 RD Results

This section discusses shortly the robustness checks for the RD setting, but an extensive set of robustness checks for the RD setting is reported in Section A.1. In addition, we report separate set of robustness checks for the RD-DID setting in Section A.2. To summarize these additional robustness results, the magnitude of the estimates and conclusions on statistical significance are robust to using two alternative RD estimators (RDHonest and the local randomization approach). Although the magnitude of the individual local polynomial RD estimates can be sensitive to the bandwidth choice, the differences in RD estimates between the two policy areas are more robust. That is, the RD-DID estimates should be rather robust. The RD results in Alternative Sample 1 are in line with the main findings. However, in Alternative Sample 2 the pattern of individual RD estimates changes. They grow in size in both the copayment and exemption municipalities, meaning that the estimated reductions attenuate in the copayment municipalities.[20]

19 Consequently, the discontinuity at the real cutoff does not bias any of the placebo runs.

20 The smaller estimates for Alternative Sample 2 suggest that recently moved individuals, who are likely more economically vulnerable, may respond more strongly to copayment changes. This aligns with the idea of a potential gradient in responsiveness based on economic vulnerability.

6 Comparison of the Estimates

We find that GP visits decrease by 4–5 % for women in the copayment municipalities. These are moderate effects compared to the earlier Nordic studies that estimate the effects of copayments on GP use for adolescents using age-based RD designs. For instance, Johansson, Jakobsson, and Svensson (2019) estimate a 7 % reduction in primary care GP visits for all individuals and a 9 % reduction for women at the 20th birthday in Sweden, the copayment being approximately 10 euros. Also using Swedish data, Nilsson and Paul (2018) find that both all doctor visits and GP visits decrease for all individuals by approximately 10 % at the 20th birthday and slightly less at the 7th birthday. The copayment for GP visits was 10–15 euros. For Norway, Magnussen Landsem and Magnussen (2018) report that a copayment of 17–18 euros reduces GP visits by 10 % for all individuals.

For a back-of-the-envelope comparison, we convert our main RD-DID estimates for all individuals (Table 1: level 0.891 and RD-DID estimate −0.046) to the semi-arc elasticity that represents the change in GP visits, normalized by the baseline, divided by the price change (Brot-Goldberg et al. 2017): −0.27.[21] In contrast, the estimates of Johansson, Jakobsson, and Svensson (2019) map to semi-arc elasticities of −1.11 for GP visits for all individuals and −1.45 for women at the 20th birthday in Sweden.[22] Nilsson and Paul (2018) report a semi-arc elasticity of −0.88 for doctor visits at the 20th birthday for both men and women. The corresponding estimate is somewhat lower at the 7th birthday: −0.55.

The magnitude of our estimates appears even smaller when considering our sample population: women who are observed to have at least one GP visit in public primary care in 2011–2019. There are suggestive findings in the literature that adolescent women are more responsive to out-of-pocket costs than men (Beck Olsen and Melberg 2018; Johansson, Jakobsson, and Svensson 2019; Vargas Lopes et al. 2022). The latter sample restriction disproportionately excludes individuals from high-income families, who are on average more likely to use private services and whose sensitivity to copayments is plausibly lower than the average.

A plausible explanation for the small effects is the combination of gatekeeping and waiting times for non-urgent care that in essence works as a rationing

21 The formula: $\frac{(q_1-q_0)/(q_1+q_0)}{(p_1-p_0)/2} = \frac{(0.891-0.046-0.891)/(0.891-0.046+0.891)}{(16/83-0)/2}$. Similarly to Nilsson and Paul (2018), our price measure is the share of the out-of-pocket costs of the total cost of the visit. The mean in the maximum per-visit copayment in 2011–2019 was 17.2 euros, but we use a smaller figure of 16 euros, because some municipalities charged a smaller copayment in 2016–2019. The average production cost of a GP visit was 83 euros in 2017 (Mäklin and Kokko 2020).

22 We computed the elasticities based on the estimates of Table 1 in Johansson, Jakobsson, and Svensson (2019), assuming that the total cost is SEK 1500 per visit (the figure is used in the study).

device, moderating the effects of out-of-pocket costs. Although waiting times and gatekeeping are by no means unique to Finland, gatekeeping may be stricter and waiting times longer in Finland. Consistent with this, a study examining the abolition of a 14-euro copayment for GP visits in Helsinki, the Finnish capital, reports a semi-arc elasticity of −0.26 for adults based on difference-in-differences methods (Haaga et al. 2024). That figure is surprisingly close to our elasticity estimate, although the context is different (intervention, design, and sample). Among the alternative explanations, our shorter baseline bandwidth or our use of the 3-day donut hole are unlikely to explain the smaller magnitude of the effects, because our RD-DID estimates do not increase when using a larger bandwidth (Figure A9) and they are rather robust to not using the 3-day donut hole (Table A1 vs. Table A4).

Regarding the heterogeneity of the results, we find that the largest reductions in GP visits are for the bottom 20 % of the income distribution, i.e. their GP use decreases by 0.08–0.10 annualized visits (7–10 %). However, the results do not overwhelmingly support the hypothesis that the average effects are mostly driven by low-income individuals and individuals from high-income households do not respond to the copayment. In fact, the RD-DID estimates are higher than average for the top 50 %, showing reductions of 0.05–0.06 visits (6–8 %). Neither do our RD results in the copayment municipalities provide evidence of a clear difference in the magnitude of the estimates between these income groups, considering both baseline and supplementary results. In relative terms, we do not find any difference between these income groups. In contrast, there is much more support for the income hypothesis in the earlier literature. Nilsson and Paul (2018), Johansson, Jakobsson, and Svensson (2019), and Vargas Lopes et al. (2022) use income as the key stratifying dimension and find that low-income individuals are more affected both in absolute and relative terms.

7 Conclusions

We find that GP visits decrease by 4–5 % when adolescents start facing copayments for the first time for GP use at the 18th birthday. The effects appear smaller than in earlier studies from other Nordic countries, despite the similarity in the empirical approaches, the use of age cutoffs for exogenous variation, the population under study, and the fact that the Nordic countries have notably similar institutions.[23] Our study cannot identify which mechanism causes the difference, but we argue that the intensity of other barriers to access, such as waiting times and gatekeeping, may

23 Here, we refer to the RD design, the policy of exempting individuals under a given age from a moderate copayment, and the focus on adolescents and young adults.

at least partially explain the findings. Future research from different institutional contexts may help in understanding how institutional features moderate the effects of copayments.

Our point estimates are at face value largest at the bottom 20 % of the income distribution, but also larger than average for the top 50 %. Overall, our data do not support the conclusion that the effects are mostly driven by low-income people while high-income individuals only slightly respond to copayments. The small average effects and a lack of large and clear income-related heterogeneity in the effects alleviate popular policy worries about the unequal impacts of the studied copayment. In our setting, moderate copayments do not appear to lead to a large increase in inequality in GP use. However, we do not have data on which type of visits have been displaced in each group, and thus, one should be cautious with this interpretation.

As a limitation, we do not observe the triage score (there is no triage score) or a direct measure of health status during the visits in our data. As a result, we are not able to test for continuity in health risk at the age cutoff. However, since our focus is on young adults, they represent a relatively homogeneous group with generally good average health. Second, the RD estimates are by definition local to the cutoff, and the 18th birthday is in some important ways special. This reduces the external validity of the estimates for other age groups. Third, our findings for the incentive effects do not reveal whether appropriate or wasteful healthcare care is being missed or whether there are effects on health. These questions should be the focus of future research.

Finally, the absence of data on private healthcare visits presents a potential limitation for our study. Given that a significant portion of Finnish families have private health insurance, which is more prevalent among higher-income households, this may allow wealthier adolescents to access private care options outside the public system. This selection into private insurance could compress the income distribution within our sample, potentially obscuring sharper income-related gradients seen in countries with universal public coverage, such as Norway and Sweden. The observed increase in social assistance uptake at age 18 could also play a role in moderating the effect of copayments in our setting. As recipients of social assistance are typically exempt from healthcare copayments, this may reduce the financial barrier for low-income individuals to access primary care in Finland. Overall, this exemption policy might contribute to the smaller effect observed in our study compared to Sweden and Norway, where similar exemptions or incentives to apply for social assistance may be less common or structured differently.

The results of this study have important policy implications, especially regarding the effects of cost-sharing in primary healthcare services. The lack of strong income-related heterogeneity in response to copayments suggests that the effect of

copayments extends across income groups, challenging the assumption that only low-income individuals alter their behavior due to cost-sharing. This suggests that a uniform application of copayments might not be the most suitable approach for cost-sharing in a public healthcare system. Policymakers might explore alternative strategies, such as income-adjusted copayments or exemptions for specific age groups, to balance healthcare access with budgetary constraints without disproportionately affecting the youth. Future research could further examine which healthcare needs are most affected by these policies to refine strategies aimed at minimizing adverse impacts on health equity and access.

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