Do Low-Income Households Respond More to Cost Sharing in Primary Care? Evidence from Regression Discontinuity Design

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Abstract

We examine the heterogeneous effects of copayments of 14 to 21 euros on primary care general practitioner (GP) use in a setting in which access to appointments is based on triage. Using an age-based regression discontinuity (RD) design and variation across areas in whether the copayment is charged (RD-DID), we analyze the effects at the 18th birthday, when previously exempted individuals become subject to copayments. Based on Finnish administrative data from 2011-2019, we find that GP visits decrease by 4-5% in the copayment areas relative to the comparison areas. The reductions are largest 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 effects are also larger than average for the top 50%, showing reductions of 0.05-0.06 visits (6-8%).

Keywords: Cost sharing, copayments, out-of-pocket costs, healthcare use, primary care, general practitioner, regression discontinuity, difference-in-discontinuities

JEL codes: H42, I11, I13, I14, I18

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Contents

1 Introduction

Cost sharing is used to finance healthcare services and allocate resources to those in need. Moreover, out-of-pocket costs reduce the demand for healthcare (Einav and Finkelstein, 2018). In developed countries, populations are rapidly aging, demand for primary care rising, and shortage for healthcare professionals growing. Policymakers are consequently seeking ways to mitigate healthcare spending without negatively affecting population health. Increased out-of-pocket costs potentially amplify inequality. If the out-of-pocket costs are independent of income, they constitute a larger fraction of disposable income for low-income households. Cost sharing policies may thus create a greater barrier to access for low-income persons than for the rest of the population. This could lead to detrimental health effects for the population group most in need of health services. Notably, even if the missed care had no detectable health effects, subjective utility losses are likely if low-income patients are not able to consult a doctor for financial reasons.

We analyze how copayments of 14 to 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 exempted from GP visit copayments in Finland. We observe both areas charging the copayment (copayment areas) and areas either exempting students from the copayment or charging no copayment at all (exemption areas). This allows us to account for other factors that potentially cause discontinuities at the 18th birthday by estimating models that subtract from the discontinuities in the copayment areas the discontinuities in the exemption areas (difference-in-discontinuities, RD-DID). Using nationwide administrative data 2011-2019, our focus is on the potentially heterogeneous effects by income level.[1] We estimate the effects for women only as the identification assumptions are not plausible for men due to compulsory military conscription. The key policy question is whether heterogeneous effects are observable even in the institutional

[1] The measure is the equivalized family disposable income in the year when an individual turns 17.

setting characterized by moderate copayments, gatekeeping at the point of entry, waiting times for non-urgent care, and extensive social safety nets.

We find that GP visits decrease by 4-5% in the copayment areas relative to the comparison areas at the 18th birthday. These RD-DID estimates are driven by statistically significant reductions in the copayment areas, while the estimates in the comparison areas are close to zero and insignificant.[2] For instance, the RD estimates for all individuals vary, depending on the estimator, between -0.04 and -0.05 annualized visits (-4.8% to -5.8%) in the copayment areas compared to estimates of -0.00 to -0.01 (-0.3% to -1.0%) in the exemption areas. 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 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 small average effects and a lack of large and clear income-related heterogeneity in the effects alleviate worries about the potentially unequal impacts of out-of-pocket costs.

RD designs based on age cutoffs are popular in the quasi-experimental literature examining the effects of cost sharing on service use.[3] Prominent studies by Card et al. (2008), Shigeoka (2014), and Fukushima et al. (2016) exploit changes 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

[2]The RD-DID estimates are by construction less precise. They compare the difference of two RD estimates instead of one RD estimate to a fixed zero with no statistical uncertainty. The RD-DID estimates are no longer significant.

[3]Besides the age-based RD designs, there are studies that exploit income-based discontinuities in cost-sharing policies (Chandra et al., 2014; Serna, 2021).

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 et al. (2020) focus on copayment exemptions that expire at the 3rd birthday in Taiwan.

Our analysis is most closely related to the studies from other Nordic countries that examine the effects of moderate copayments on public primary care GP use at age thresholds 7, 16, and 20 in Sweden and Norway (Johansson et al., 2019; Magnussen Landsem and Magnussen, 2018; Nilsson and Paul, 2018). Similarly to Finland, Sweden has a primary care system where access to GP appointments is based on triage. This is an important aspect of the institutions: gatekeeping should disproportionately reduce GP appointments that are of low value medically.

We contribute to the literature data-wise and methodologically. First, we combine recent data from 2011-2019 with a design that also has comparison areas which either exempt individuals showing a student ID or charge no GP visit copayment at all. This setting stands in contrast to Nilsson and Paul (2018), who use data from 1999-2006, and to Johansson et al. (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 important because the composition of GP 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 reallocated from GPs to nurses. Crucially, the availability of comparison areas allows us to account for discontinuities in the potential outcomes that are unrelated to the copayment policy, as in Nilsson and Paul (2018).[4]

Second, in contrast to the conventional approach of using an *ad hoc* fixed bandwidth in RD designs and ignoring the smoothing bias inherent in approximating regression functions with low-order polynomials, our complementary RD analysis uses methods that allow for

[4] The age cutoffs of 20 in Johansson et al. (2019) and 16 in Magnussen Landsem and Magnussen (2018) are probably less problematic than our cutoff at the 18 birthday with respect to other factors creating discontinuities in the potential outcomes. Therefore, the value of RD-DID models is greater in our application.

data-driven bandwidth selection leading to mean-square-error-optimal point estimation and inference that is valid in the presence of smoothing bias and/or a discrete running variable (Calonico et al., 2014; Kolesár and Rothe, 2018). This is possible, because we observe exact birth dates and daily health care use, which is more granular than in the earlier studies.

We also contribute to the broader cost-sharing literature by analyzing whether low-income households are more responsive to out-of-pocket costs. By the start of the 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 studies mentioned above, Nilsson and Paul (2018), Johansson et al. (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. Johansson et al. (2019) examine the effect of a copayment on GP use, Nilsson and Paul (2018) report the effect of a copayment on outpatient doctor visits[5], and Vargas Lopes et al. (2022) study the effect of an increased deductible on mental health care use. Besides these RD studies, Haaga et al. (2022b) use a pre-registered and pre-specified staggered difference-in-differences design and find that the effects of a nurse visit copayment on primary care nurse use in the adult population were higher in absolute terms, but not necessarily in relative terms, at the lower end of the income distribution.

2 Institutional Background

Several sectors provide primary care in Finland. Every citizen is entitled to public primary care, which is characterized by small or moderate copayments, gatekeeping, and waiting times for non-urgent conditions. Private clinics offer fast and direct access to specialists without a referral, but the out-of-pocket costs are much higher.[6] Occupational healthcare provides for many employees a fast access to curative services without charging copayments,

[5] Heterogeneity is large in specialist visits, but less obvious in GP visits.

[6] In January 2020, a GP visit of 20 minutes cost 86.70 euros at the largest private healthcare service company (by revenue). The reimbursement was 9.00 euros for visits of this length. The maximum copayment for a GP visit at publicly funded health centers was 20.60 euros.

but there is some gatekeeping. This often means contacting a designated professional before being allowed to book an appointment. School and student healthcare is organized by municipalities, providing preventive services concerning health checks, mental health, substance abuse, sexual health, dental health, and laboratory tests. It varies between municipalities whether GP appointments are available in student healthcare and, if available, whether there is a copayment.

Around the 18th birthday, public primary care and private clinics are the main providers of curative GP visits. Based on the statistics of the Social Insurance Institution of Finland, 23% of those aged 15 to 19 years and 20% of those aged 20 to 24 years received reimbursements for private doctor visits in 2019, including both GPs and specialists. Voluntary private health insurance to pay for private fees is also popular, especially in families with children. In 2016, almost half of the families with children and two adults had a private insurance plan, 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.

Municipalities organize publicly funded primary care and run a health center on their own, in voluntary cooperation with others, or outsource the services. The number of operating locations called health stations varies between health centers. Copayments are one channel to finance the services in addition to municipal taxes, municipal bonds, and state transfers. 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 between 2011 and 2019. Municipalities set their policies given these constraints. Individuals under 18 years of age are nationally exempted from public primary care 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 already in January 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 (in most cases two times the per-visit copayment) (Haaga, 2019).

For GP appointments, patients contact their designated health station that is often geographically the closest. Nurses do triage and make bookings. The supply of appointments is relatively fixed in the short run. The number of medical students per cohort in Finnish universities is fixed, and the language barrier for foreign GPs exists. 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 varied between 3.5% and 6.5% in 2011-2018 in Finland (THL, 2020). In addition, long waiting times for non-urgent care are a long-standing situation, although waiting times vary even within municipalities.

3 Data and Methods

3.1 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' sociodemographic and socioeconomic characteristics.[7] We use data from 2011 to 2019.[8] The data collection on primary care contacts started in 2011, and we restrict the analysis to the pre-COVID-19 pandemic times. We also use publicly available data on municipal mergers from 2001-2020 (Association of Finnish Municipalities).

We searched whether students over 18 years of age pay copayments for curative GP visits in public primary care by observing the websites of the 30 largest municipalities. In 23 cases, the policy was clearly and explicitly stated. 19 municipalities charged copayments, 3 (Espoo, Turku, and Tuusula) exempted individuals showing a student status certificate,

[7]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.

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

and 1 (Helsinki) charged no copayment at all. We only observe the policy of the early 2020 and assume that the policies were the same over the study period.[9] The copayment municipalities contain both areas offering the annual copayment option and areas 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 between 2011 and 2019).

Section A.1 in the Online Appendix and the replication codes provide 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 3.2).

3.2 Methods

Identifying assumptions. The key RD assumption is that the average potential outcomes are continuous functions of the running variable at the cutoff (Hahn et al., 2001). After turning 18, Finns can obtain a driver's license and legally buy and consume alcoholic drinks containing at most 22% alcohol by volume. Increases in driving accidents and alcohol misuse are more likely visible in emergency department visits than in our primary care data. The health certificate to apply for a driver's license is given after a health check at the 8th or the 9th grade, two to three years before the 18th birthday. The ability to drive a car can reduce travel times and thus indirect costs of access. A potential increase in GP use is likely small given that our sample contains large municipalities with public transport available and that most adolescents live with their parents who probably have a driver's license.

We focus only 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 (Figure A3 and Figure A4), invalidating the RD design. Every male takes part in a call-up during the year they turn 18, organized annually from August to December. Before

[9]We know that the exemptions were in place in Turku and Tuusula during the whole study period, and Espoo's municipal board 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.

the call-up, they have nurse and GP appointments organized by municipalities and often occurring in Spring. Women can serve voluntarily, but it is rare. In 2018, a record number of 1500 women applied for voluntary military service - no more than 5% of the birth cohort. All women do have a health check by a GP during the first or the second year of their upper secondary education, beginning at the age of 15 or 16.

Years 17 to 19 are also characterized by two transitions for many individuals: from school to work (mainly after vocational education) and moving away from parents. There are institutions that incentivize these transitions at the 18th birthday, discussed in Section A.2. We believe that the end of classroom learning at the end of upper secondary education is a natural transition point. The dates when the classroom learning ends are not related to our running variable. Consequently, we expect that the potential discontinuities in school-to-work transitions and between-municipality migration are small. However, the same argument probably does not apply to within-municipality moves.

Based on the data, 16% of those who lived with their parents on December 31st when aged 17 moved away by the end of the next calendar year, using a change in family relationship status (previously a child) as a proxy. 63% of these moves were within the same municipality. Moves are more common in areas charging the copayment than in areas that exempted students or charged no copayments for GP visits. 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 A5 examines the potential discontinuities at the 18th birthday for those born at the turn of the year.[10] Both the total number of GP visits in the study period and equivalized disposable income appear to be continuous at the cutoff in both policy groups. We would expect to observe a discontinuity if those born at the turn of the year move discontinuously towards our sample areas from the rest of the country at the 18th birthday and if they are on average different from the population average. However, we observe a

[10]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 $t \in \{-365, 0, 365\}$.

small reduction at the 18th birthday in the share of those living with their parents in both the copayment and exemption areas and potentially a small increase in the share being unemployed. Both observations are in line with the economic incentives.

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 et al., 2016) that subtract from the discontinuities in the copayment areas (a clear discontinuity in policy) the observed discontinuities in the exemption areas (either no copayment or an exemption for students). In complementary RD analyses, we report the effects separately in the copayment and exemption areas 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 does not mechanically affect sample sizes in a 180-day bandwidth or create discontinuities at the 18th birthday, but persons with within-policy-area moves are still included. Alternative Sample 2 restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18. The corresponding population sizes in age cells are shown in Figure A6. We first report the effects for the main analysis sample before examining whether the results are robust to the sample definition.

Income groups of interest. 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 on December 31st.[11] These are *ad hoc* choices. Other choices could be equally well justified. We note that the bottom 20%

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

includes many adolescents who have moved away from their parents.

Donut hole. Unless otherwise stated, we exclude observations within the 3-day bandwidth (donut hole RD). We do this, because we observe that GP use is noticeably lower on the 18th birthday and the following two days both in copayment and exemption areas (Figure A7). The pattern is similar at both ends of the income distribution. These features make it unlikely that the pattern is related to copayment policies.

Main RD-DID estimates. We use local linear regression parametrically in a 180-day bandwidth (approximately 6 months) with the uniform kernel, weighting by age cell size.[12] The data are aggregated at the age-cell-by-policy-area level. Confidence intervals are based on robust standard errors. We use the following regression specification:

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

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

Main RD estimates. To complement the local polynomial RD-DID estimates, we also 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 (for a practical guide, see Cattaneo et al., 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

[12] In the RD estimation, which we describe shortly, our MSE-optimal bandwidths depend on data and allow for a different bandwidth below and above the cutoff (Calonico et al., 2014). For the main RD results, the optimal left bandwidth varies between 182 and 247 days in the copayment areas and between 169 and 270 days in the exemption areas (Table 1). The optimal right bandwidth varies between 143 and 176 days in the copayment areas and between 174 and 211 days in the exemption areas.

et al., 2014) using the R package *rdrobust*. To construct valid confidence intervals, the smoothing bias is removed by first estimating it using another local regression - one order higher. Valid inference has to account for both regressions. The procedure of Calonico et al. (2014) yields confidence intervals that are both 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.[13] 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. Given a discrete running variable, the effective sample size for local polynomial methods is the number of mass points (Cattaneo et al., 2018). Hence, we aggregate the data at the running variable level and weight the regressions by cell population size multiplied by the kernel.

Comparing the main RD-DID and RD results. We view these two approaches as complementary and equally important. For point-estimation and unbiased estimates, we prefer the RD-DID results because accounting for the potential discontinuities in the comparison areas likely reduces the bias of the effect estimates. For inference and precision, we prefer the RD results in the copayment areas 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 RD estimates in the comparison areas 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.

Supplementary RD-DID results: the local randomization approach. In

[13]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.

the RD-DID framework, we also use the local randomization approach (Cattaneo et al., 2018).[14] Specifically, we estimate the difference in discontinuities between the copayment areas and the exemption areas in a narrow 30-day bandwidth. We only include individuals who are observed throughout the whole window and who have unique values for copayment policy in the window. The bandwidth is our *ad hoc* choice. The estimands are by construction different compared to the local polynomial approach (at the cutoff vs. in a narrow neighborhood). In RD settings, the method requires a strict assumption that in the small neighborhood i) placement above or below the cutoff is as if randomly assigned, and ii) the potential outcomes are unrelated to the running variable. Our data are at the person-by-adult-dummy level. We use the following regression specification:

$$y_{it} = \alpha + \beta_1 Copay_i + \beta_2 Adult_t + \delta Copay_i \times Adult_t + \varepsilon_{it}. \tag{2}$$

Here, α is an intercept, i and t denote individual and day. *Copay* is a dummy for copayment municipalities, and *Adult* is a dummy for being over 18. The parameter δ is the coefficient of interest: it captures the difference in discontinuities at the 18th birthday between the copayment and comparison areas. Robust standard errors are reported.

4 Results

4.1 Main Results

The RD plots are in Figure 1. In copayment municipalities, there appears to be a drop of 0.05 annualized visits per resident for all individuals at the cutoff and a larger reduction for the lower end of the income distribution. A similar decrease is not obvious for the top 50% for whom the data are noisier. In exemption municipalities, there are no clear indications of significant discontinuities. Figure A8 shows the same RD plots but uses data only from within

[14] The local randomization approach is also used in the RD framework, but the corresponding details and results are presented in the Online Appendix.

the optimal bandwidth (Calonico et al., 2014) and fits linear splines instead of fourth-order polynomials.

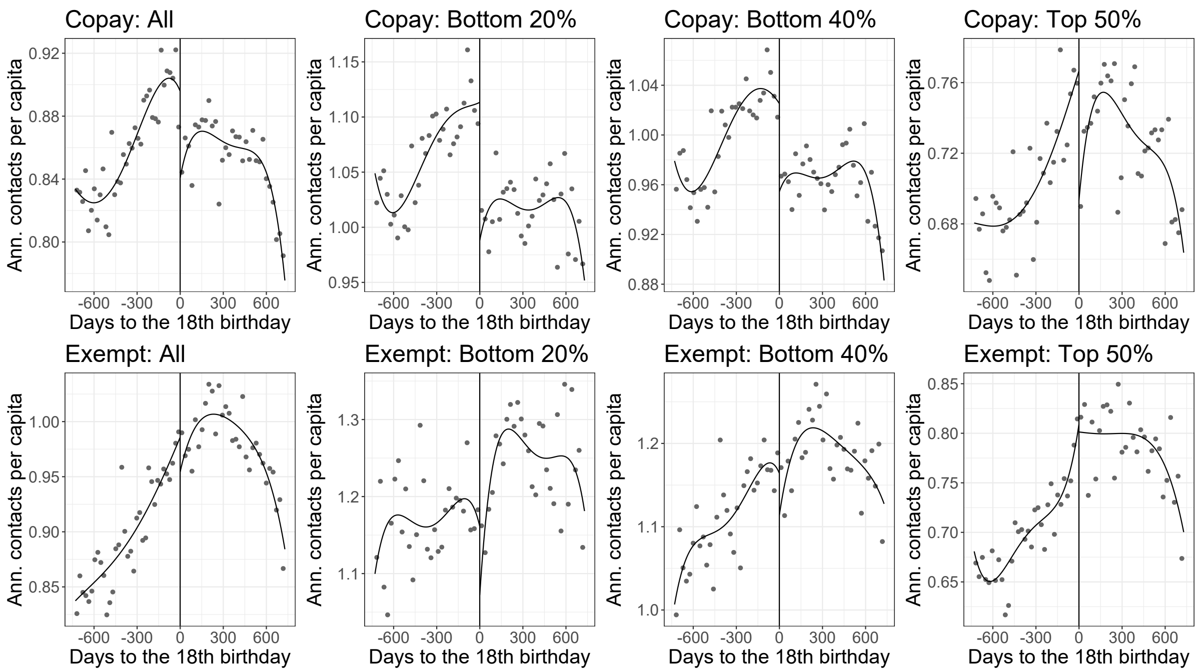

Figure 1: RD Plots.

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 household disposable income, measured for the year when the individual was 17 years old on December 31st. 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 et al., 2015). The sample only includes women.

The main results based on local polynomial methods are in Table 1. For point estimation and unbiased estimates, we prefer RD-DID results estimated with Model 1. They show that turning 18 is associated with a larger reduction in GP use in the copayment areas than in the exemption areas. The 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%). The estimate for the top 50% is above average: -0.06 annualized visits (-8.1%).

Table 1: Main Results.

A. RD-DID Results.				
	All	Bottom 20%	Bottom 40%	Top 50%
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
B. RD Results in Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
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)
C. RD Results in Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
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)

Notes: 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. 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). Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. For RD-DID results, level is the fitted mean for the copayment areas 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.

For inference and precision, we prefer the local polynomial RD estimates that are reported separately in the copayment and exemption areas. These results are depicted graphically in Figure A8 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 areas and by −0.03 visits (−2.9%) in the exemption areas. For the bottom 40%, GP visits decrease by −0.07 visits (−6.7%) in the copayment areas and by −0.03 visit (−2.5%) in the exemption areas. For the top 50%, we estimate effects of −0.07 annualized visits (−8.9%) in the copayment areas and +0.02 annualized visits (+2.2%) in the exemption areas.

The RD results are all statistically different from zero in the copayment areas, but closer to zero and insignificant in the exemption areas. The largest p-value in the copayment areas is 0.02, estimated for the top 50%. In the exemption areas, 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. In contrast, the RD-DID models are by construction more conservative with respect to statistical inference. They essentially compare the RD estimate in the copayment areas to the RD estimate in the exemption areas that is close to zero but estimated with noise.[15]

Both the RD-DID results and the RD estimates in the copayment areas 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 - our data do not overwhelmingly support the hypothesis that the average effects are mostly driven by the poor and individuals

[15] The sample sizes in the exemption areas are less than half of the sample sizes in the copayment areas. All the RD-DID estimates are statistically insignificantly different from zero and from each other. The p-value for all individuals is 0.14.

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.

4.2 Robustness Checks and Supplementary Analyses

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.[16] Figure A9 contains the results for the RD setting and Figure A10 for the RD-DID setting. The RD results show that the largest reduction in GP use in the copayment areas for each subgroup is induced by the real cutoff. However, in the exemption areas 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.

RD results. Due to space constraints, this section discusses in detail only the robustness checks for the RD-DID setting. The RD-DID estimates are plausibly less biased but also less precise compared to the RD estimates. We view both approaches as equally important. An extensive set of robustness checks for the RD setting are reported and discussed in Section A.3 in the Online Appendix. To summarize these results, the magnitude of the estimates and conclusions on statistical significance are robust to using two alternative RD estimators. Although the magnitude of the individual local polynomial RD estimates appears to be sensitive to the bandwidth choice, the differences in RD estimates between the two policy areas are more robust. That is, the estimates tend to move in the same direction, and the RD-DID estimates should be rather robust. The RD results in Alternative Sample

[16] The former restriction ensures that the discontinuity at the real cutoff does not bias any of the placebo runs.

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 areas, meaning that the estimated reductions attenuate in the copayment areas.

RD-DID: alternative samples. Table A1 shows the sensitivity of the main RD-DID estimates to the sample. Unlike our main analysis sample, Alternative Sample 1 only includes persons who are observed to reside in the same policy area (either copayment or exemption) at least six months before and after the 18th birthday. Alternative Sample 2 restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18. In Alternative Sample 1, the estimate for all individuals attenuates to −0.02 annualized visits (−2.8%), driven by a smaller reduction at the top 50% of the income distribution. However, the estimates for the lower end of the income distribution are robust. In Alternative Sample 2, we estimate a reduction of −0.04 annualized visits (−4.8%) for all individuals. For the lower end of the income distribution, the estimated reductions are somewhat larger than in the main analysis sample.

RD-DID: bandwidth choice. Figure A11 shows the sensitivity of the RD-DID results to the bandwidth choice against the baseline of 180 days. In the main analysis sample, the estimate for all individuals varies approximately between −0.03 and −0.05 visits, the estimate for the bottom 20% between −0.05 and −0.10 visits, the estimate for the bottom 40% between −0.01 and −0.05 visits, and the estimate for the top 50% between −0.05 and −0.10 visits. The alternative samples show greater variation, but the point estimates for all subgroups are still negative.[17]

RD-DID: local randomization approach. Table A2 shows the RD-DID results in a narrow 30-day bandwidth for the main analysis sample and our two alternative samples. The estimates for all individuals vary between −0.04 and −0.05 (−5.4% to −5.7%). The

[17] Of the many point estimates, only the estimate for the bottom 40% in Alternative Sample 2 with a 130-day bandwidth is positive.

estimates for income-based subgroups are more noisy, but negative in each case. Figure A12 shows the sensitivity of the estimates to the bandwidth choice.

RD-DID: Helsinki, a clean placebo area. Table A3 contains the RD-DID estimates comparing the copayment areas to Helsinki. Helsinki is the only municipality that does not charge the GP visit copayment (since 2013) from anyone. This policy can represent a more clearer absence of any policy discontinuity to adolescents than the exemption for students in the exemption areas. In the main analysis sample, the estimate for all individuals is -0.03 annualized visits (-3.6%), somewhat closer to zero than the baseline results. The largest effect in absolute terms is found for the bottom 20%: -0.08 annualized visits (-7.0%). The estimate for the bottom 40% is -0.01 annualized visits (-0.7%). The estimate for the top 50% is above average: -0.05 annualized visits (-6.4%). The reductions attenuate in Alternative Sample 1 but increase in Alternative Sample 2, varying between -0.02 and -0.05 visits (-2.4% to -5.6%) for all individuals. The RD plots for Helsinki are in Figure A13.

5 Comparison of the Estimates

We find that GP visits decrease by 4-5% for women in the copayment areas relative to the comparison areas. These are small 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 et al. (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 to 15 euros. For Norway, Magnussen Landsem and Magnussen (2018) report that a copayment of approximately 17 to 18 euros reduces GP visits by 10% for all individuals.

For comparison, we convert our main 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 nurse visits, normalized by the baseline, divided by the price change (Brot-Goldberg et al., 2017): -0.27.[18] In contrast, the estimates of Johansson et al. (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.[19] 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 quantitative size of our estimates is even smaller when we take into account our sample population: women who are observed to have at least one GP visit in public primary care in 2011-2019. Regarding sex, 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 et al., 2019; Vargas Lopes et al., 2022). The latter restriction disproportionately excludes individuals from high-income families, who are on average more likely to rely on private services that have higher out-of-pocket costs but provide fast access without gatekeeping. Their sensitivity to moderate copayments is plausibly lower than for the general population.

To explain our small estimates, a plausible explanation is the combination of gatekeeping and waiting times for non-urgent care that in essence works as a rationing mechanism, moderating the effects of out-of-pocket costs. Although waiting times and gatekeeping are by no means unique to Finland, gatekeeping in the Finnish health care may be stricter and waiting times longer. 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

[18] 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).

[19] We computed the elasticities based on the estimates of Table 1 in Johansson et al. (2019), assuming that the total cost is SEK 1500 per visit (the figure is used in the study).

et al., 2022a). The elasticity estimated in our study for women at the 18th birthday (-0.27) with the same parameter value for the total cost of the visit (83 euros) is notably close to the estimate in Haaga et al. (2022a).

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 the poor 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 et al. (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.

6 Conclusion

We find smaller effects of copayments on GP use than in the earlier Nordic studies. This is despite the similarity in the empirical approach, the source of exogenous variation based on age cutoffs, the population under study, and the fact that the Nordic countries share notably similar institutions.[20] The point estimates are at face value largest among the bottom 20% of the income distribution, but also larger than average for the top 50%. Overall, our data do not support the hypothesis that the effects are mostly driven by low-income persons while the individuals from high-income households hardly respond to copayments.

[20]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.

The small average effects and a lack of large and clear income-related heterogeneity in the effects alleviate worries about the potentially unequal impacts of out-of-pocket costs. In our setting, income-independent moderate copayments do not appear to lead to a large increase in inequality in GP use.

Our analysis has some limitations. First, we are not able to distinguish why the estimated effects are small. We suspect that the intensity of other factors limiting access to primary care may decrease the relative importance of copayments in affecting the number of primary care visits in Finland. These institutions include gatekeeping conducted by nurses and waiting times for non-urgent care. Moreover, it has been argued that the Finnish public primary care is under-funded, also limiting accessibility. Second, the RD estimates are by definition local to the cutoff, and the 18th birthday is in some important ways special. The time window is characterized by major transitions in life for many adolescents. This reduces the external validity of the estimates for other age groups. Third, our evidence for the incentive effects does not reveal whether appropriate or wasteful healthcare care is being missed. However, gatekeeping in the system aims to reduce visits with the lowest medical value. Fourth, our study does not reveal how copayments affect health and, consequently, health inequalities. Examining the health effects is challenging also using alternative empirical designs. A notable exception in the literature is Chandra et al. (2021) who find that an increase in cost sharing in Medicare, a prescription drug benefit program in the USA, leads to fewer medications taken and higher mortality among the affected patient population. Health effects remain an important topic for future studies.

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A Online Appendix

A.1 The Construction of the Analysis Data

Note: Read this section in chronological order.

Main Analysis Sample. We take all individuals who had at least one outpatient GP visit in public primary care in 2011-2019. Then, we expand the data to include years 2011-2019 for each individual, keeping only rows where the person lives in a sample municipality at the end of that year and has positive family equivalized disposable income. We exclude such person-year observations where the individual lives in an exemption municipality (Espoo, Turku, and Tuusula) but is not a student at the end of year and thus has no student status certificate required for the exemption.

Alternative Samples. Our alternative samples focus on a subset of individuals with less moves away from parents than in the main analysis sample. The purpose is to account for the bias induced by a potential discontinuity in moving away from parents at the 18th birthday. Similarly to the main analysis sample, we first take all individuals who had at least one outpatient GP visit in public primary care in 2011-2019. Then, we expand the data to include years 2011-2020 for each individual.

Alternative Sample 1 is constructed as follows. For those born between January 1st and June 30th (July 1st and December 31st), we include those person-year observations in which the individual is aged 16 to 18 (17 to 19) at the end of the calendar year. We then restrict to individuals who are observed with three person-year observations. This procedure ensures that all eligible individuals are observed for at least six months before and six months after the 18th birthday. We include only persons who are observed to reside in the same policy area (either copayment or exemption). Thus, between-policy-area migration does not mechanically affect sample sizes in a 180-day bandwidth or create discontinuities at the 18th birthday, but persons with within-policy-area moves are still included. We exclude persons living in a municipality exempting students from the GP visit copayment and born between

January 1st and June 30th (July 1st and December 31st) who were not students at the end of year when being 16 (17) years old (approximately 5% of sample living in the eligible areas).

Alternative Sample 2 aims to exclude all moves away from parents at the cutoff, also those within the same municipality. First, we keep those person-year observations in which the person is 17 or 18 years old at the end of the calendar year. We require that the person is observed in both years and that neither policy area nor family relationship status (a proxy for moving away from parents) changes between the two observations. To exclude non-students, we only include those persons in the municipalities exempting students who were students when being 17 years old at the end of year.

The corresponding population sizes in age cells are shown in Figure A6.

Data quality issues. We exclude some person-year observations due to quality issues in the primary care data: there are periods for some areas in which the reported number of GP visits per capita is zero or very low, caused by problems in transferring data from health centers' IT systems to the national register. Of the 23 sample municipalities, six appear to have these issues. We detected these municipalities in the following way: we first computed a distribution of mean contacts by permutationally dropping every combination of four consecutive months. We then marked an observation to be invalid if its value is less than 40% of the largest observed mean (July was not considered because the GP supply is considerably reduced due to vacations). We plot the number of annualized GP visits per resident in these municipalities in Figure A1. Municipality-year pairs to be excluded from the analysis data are highlighted in gray. We choose to keep one municipality that is picked up by the above algorithm but that does not appear to have similar problems than the other municipalities.

We exclude only one municipality-year observation due to quality issues in the referral data (Rauma due to a considerable spike in referrals in December 2011). Figure A2 plots the number of annualized referrals per capita by sample municipality. Note that there are suspicious increases in the level of referrals in many municipalities over the study period.

We believe that the absolute number of referrals is not reliable in many municipalities due to variation in coding rates of the referral arrival date. However, the issue should not be related to our running variable in any way and consequently should only reduce the effective sample size and increase variability in the data but not bias our estimates.

ID-date panels. We expand the ID-year data to the ID-date level. The bandwidth is 730 days for the main analysis data and 365 days for our alternative datasets. The analysis datasets are based on this ID-date panel.

GP visits in public primary care. We extract uncancelled outpatient GP visits with unique person IDs and observed birth dates. Visits are linked to municipalities based on the patients' municipality of residence at the end of year using the 2020 municipal boundaries. Visits on weekends are excluded to reduce the potential bias from changes in whether emergency care GP visits are coded to our data, as some health centers and hospitals have created joint emergency departments during the study period. There are also some duplicate GP visits with the same date and time - we take only unique id-date observations.

Referrals to specialist care. We extract referrals written in public primary care with unique person IDs from the universe of specialized health care contacts. Of the referrals, we include only unique person-date pairs (only one referral per day). As already mentioned above, we believe that there are missing values in the referral arrival date column in the raw data, leading to missing referrals. This, however, should not be related to our running variable and is thus not a source of bias.

Data on social assistance recipients. Social assistance is applied for at the household level, but we construct person-month indicators for whether the person lived in a family where someone received social assistance in that month.

A.2 Important Transitions around the 18th Birthday

With respect to the school-to-work transitions, the Labor Code does distinguish between underage and adult workers, but the effects on hiring from this alone should not be that dramatic. Getting a driver's license may be a valuable skill in some jobs, and the number of potential jobs also increases because of a larger local labor market. Those aged 18 years or more are eligible to apply for the labor market subsidy (an unemployment benefit for labor market entrants) without the need to participate in active labor market programs which is required from the minors.

The social insurance institutions appear to incentivize moving away from parents at the 18th birthday. Before August 2017, high parental income reduced the study grant for those aged 19 years or less no matter whether the individuals lived on their own or with their parents, but in the supplementary housing benefit for students the same cutoff was at the 18th birthday. Since then, the benefit system for students has been reformed multiple times. The labor market subsidy (more generous for those aged 18 or more) depends on parental income if the individual lives with parents, but parental income does not affect the benefit for those living away from parents. From the parents' perspective, the child maintenance liability ends at the 18th birthday. If the parent is unemployed, the unemployment benefits decrease at maximum by 110 euros per month at the child's 18th birthday.

A.3 RD Robustness Checks and Supplementary Analyses

Methodology: RDHonest. In the main RD analysis, we proceed as if the running variable (days relative to the 18th birthday) were continuous and do not account for the effect of using a 3-day donut hole on inference. The inference proposed by Kolesár and Rothe (2018) circumvents these problems. Essentially, the method bounds the magnitude of the second derivative of the conditional expectation function by a smoothness constant K using a Hölder class to impose smoothness. We use the rule of thumb proposed by Armstrong and Kolesár (2020) to select the parameter in a data-driven way. Heuristically, the additional assumption is that the local smoothness of the regression function should be no smaller than the smoothness of its global polynomial approximation. We implement estimation and inference using the R package *RDHonest*. Similarly to the main RD analysis, we estimate the effects using data aggregated at the age cell (day) level, weight by population size, fit linear splines, use the triangular kernel, and allow for different bandwidths below and above the cutoff. The optimality criterion for the bandwidth is finite-sample MSE. Standard errors are estimated using the nearest neighbor method.

Methodology: the local randomization approach. We use local randomization methods (Cattaneo et al., 2018) in a narrow 30-day bandwidth around the cutoff as a complement to the local polynomial analysis at the cutoff. The estimands are by construction different (at the cutoff vs. in a narrow neighborhood). The method requires a strict assumption that in the small neighborhood i) placement above or below the cutoff is as if randomly assigned, and ii) the potential outcomes are unrelated to the running variable. We use Neyman inference for large samples. Regarding the analysis data, we only include individuals who are observed throughout the whole window and who have unique values for copayment policy in the window. Finally, we sum up GP visits at the individual level below and above the cutoff.

Results: alternative estimators. The local polynomial RDHonest estimates are presented in Table A4, and the local randomization RD results are in Table A5. Summarizing these results and our baseline RD results of Table 1, we find that GP visits decrease by −0.04 to −0.05 visits (−4.8% to −5.8%) after the 18th birthday in areas with a discontinuity in copayments. The corresponding estimates in our comparison areas are close to zero (−0.3% to −1.0%).

The reduction in GP use in the copayment areas is largest at the bottom 20% of the income distribution, varying between −0.08 and −0.12 annualized visits (−7.2% to −10.8%). The corresponding estimates in the exemption areas are −0.01 to −0.03 visits (−1.2% to −2.9%). The estimates attenuate for the bottom 40% in the copayment areas: they are from −0.03 to −0.07 annualized visits (−2.9% to −6.7%) compared to the exemption-area estimates of −0.01 to −0.03 visits (−0.9% to −2.5%). Finally, the estimates for the top 50% vary between −0.07 and −0.08 (−8.9% to −10.6%) in the copayment areas and between +0.01 and +0.02 (+1.2% to +2.2%) in the exemption areas.

The estimates in the copayment areas are statistically significant in ten cases out of twelve, the bottom 40% producing two insignificant estimates. However, the estimates in the exemption areas are always insignificant.

Results: additional robustness checks. For the local polynomial methods, we illustrate sensitivity to the bandwidth choice (Figure A14), specification (Figure A15), and to not using the 3-day donut hole (Table A6). Although the magnitude of the individual RD estimates appear to be sensitive to the bandwidth choice, the differences in RD estimates between the two policy areas are more robust. Regarding the specification, the main findings are robust to using a difference-in-means model, but the estimates for the lower end of the income distribution in copayment municipalities attenuate and are no longer significant when using a local-quadratic model. Reductions in GP visits grow larger in both policy areas without the 3-day donut hole, as expected. The estimate for all individuals is −0.10 annualized visits (−10.7%) in the copayment areas and −0.03 annualized visits (−3.1%) in the exemption areas.

Figure A16 shows that the local randomization RD estimates are relatively stable when using a bandwidth of 25 to 45 days, but with 15 or 20 days they are close to zero and even positive in the exemption municipalities. The figure also shows the point estimates from not using the 3-day donut hole.

Results: alternative samples. We start with Alternative Sample 1. Figure A17 shows the RD plots. The local polynomial results are in Table A7, the RDHonest results in Table A8, and the local randomization results in Table A9. The results are in line with the main findings. The estimates for all individuals in the copayment areas vary between −0.04 and −0.06 annualized visits (−4.4% to −7.0%) compared to estimates of −0.01 to −0.02 visits (−1.1% to −1.7%) in the exemption areas. In absolute terms, the reductions in the copayment areas remain largest at the bottom 20% of the income distribution: from −0.09 to −0.13 annualized visits (−8.8% to −11.9%). The corresponding estimates in the exemption areas vary between +0.01 and +0.04 visits (+0.6% to +4.0%). Figure A18 shows the sensitivity of the estimates to the bandwidth choice.

Next, we focus on Alternative Sample 2. Figure A19 shows the RD plots. The local polynomial results are in Table A10, the RDHonest results in Table A11, and the local randomization results in Table A12. As we observed in Table A1, the main RD-DID findings are in line with the RD-DID results in Alternative Sample 2. However, the pattern of individual RD estimates changes compared to what we have observed earlier. They grow in size in both the copayment and exemption areas, meaning that the estimated reductions attenuate in the copayment areas. The local polynomial results of Table A10 remain negative in the copayment areas. The estimate for all individuals is −0.02 visits (−2.9%) compared to the corresponding estimate of +0.01 visits (+0.9%) in the exemption areas. However, the finding in the local randomization RD-DID analysis of GP visits decreasing more in the copayment areas at the 18th birthday is now driven more by a positive point estimate in the exemption areas than a negative point estimate in the copayment areas. Although this observation is surprising, we are inclined to put more weight to the RD-DID estimates than to the individual RD estimates. Figure A20 shows the sensitivity of the estimates to the bandwidth choice.

Results: additional outcomes. A plausible proxy for GP-assessed needs for diagnosis and treatment is how referrals to specialist care evolve around the 18th birthday. The RD plots on referrals are in Figure A21. There may be a small decrease in the outcome in the copayment areas, but the data for the exemption areas are noisy. Few conclusions would be reached in the RD-DID framework. Figure A22 shows the RD plots for the probability of receiving social assistance. The probability increases considerably in both areas as the child maintenance liability ends. The increases are much higher than any plausible effect of copayments.

A.4 Additional Figures and Tables

Table A1: RD-DID Results.

	All	Bottom 20%	Bottom 40%	Top 50%
A. Main Sample (also in Table 1).				
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
B. Alternative Sample 1.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.828	1.037	0.956	0.707
RD-DID estimate	−0.024	−0.089	−0.038	−0.021
Change, %	−2.844	−8.615	−3.983	−2.941
Std. error	0.030	0.065	0.047	0.041
P-value	0.437	0.173	0.420	0.609
Individuals	78,223	18,665	35,078	35,254
C. Alternative Sample 2.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.801	0.986	0.927	0.712
RD-DID estimate	−0.039	−0.100	−0.053	−0.061
Change, %	−4.825	−10.158	−5.741	−8.578
Std. error	0.031	0.072	0.049	0.041
P-value	0.215	0.164	0.278	0.133
Individuals	76,997	17,735	32,981	36,257

Notes: The 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. The sample only includes women. We compare the copayment areas to the exemption areas and Helsinki. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean for the copayment areas just above the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals just below the cutoff. In Alternative Sample 1, there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 restricts to persons for whom we observe no change in policy area or in family relationship status (a proxy for moving away from parents) between December 31st of the year when turning 17 and December 31st of the year when turning 18.

Table A2: Local Randomization RD-DID Results.

A. Main Sample.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.894	1.101	1.008	0.771
RD-DID estimate	−0.049	−0.078	−0.038	−0.092
Change, %	−5.439	−7.106	−3.726	−11.936
Std. error	0.041	0.087	0.065	0.057
P-value	0.238	0.367	0.566	0.105
Individuals	87,890	23,555	41,825	37,268
B. Alternative Sample 1.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.849	1.059	0.971	0.732
RD-DID estimate	−0.049	−0.163	−0.087	−0.061
Change, %	−5.736	−15.434	−9.005	−8.344
Std. error	0.041	0.092	0.068	0.055
P-value	0.235	0.076	0.196	0.270
Individuals	76,110	18,123	34,105	34,305
C. Alternative Sample 2.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.777	0.925	0.863	0.698
RD-DID estimate	−0.044	−0.086	−0.041	−0.091
Change, %	−5.700	−9.283	−4.709	−13.014
Std. error	0.042	0.095	0.070	0.056
P-value	0.292	0.366	0.564	0.105
Individuals	67,464	15,635	28,870	31,823

Notes: The estimates are based on Model 2 using a 30-day bandwidth and robust standard errors. Treatment municipalities are the copayment municipalities. Comparison municipalities are the exemption municipalities and Helsinki. The model is estimated separately for income groups. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean for the copayment areas below the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals below the cutoff. The sample only includes women. In Alternative Sample 1, there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18.

Table A3: RD-DID Results, Helsinki as the Comparison Area.

A. Main Sample.

	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.878	1.068	0.964	0.755
RD-DID estimate	−0.032	−0.075	−0.006	−0.049
Change, %	−3.646	−7.039	−0.670	−6.443
Std. error	0.036	0.072	0.053	0.053
P-value	0.371	0.294	0.903	0.357
Individuals	82,208	22,468	39,804	34,083

B. Alternative Sample 1.

	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.824	0.986	0.903	0.720
RD-DID estimate	−0.020	−0.039	0.015	−0.034
Change, %	−2.405	−3.916	1.624	−4.696
Std. error	0.038	0.081	0.059	0.056
P-value	0.599	0.634	0.802	0.544
Individuals	64,936	16,135	30,085	28,109

C. Alternative Sample 2.

	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.807	0.979	0.884	0.737
RD-DID estimate	−0.045	−0.093	−0.010	−0.087
Change, %	−5.592	−9.526	−1.133	−11.788
Std. error	0.039	0.082	0.060	0.059
P-value	0.248	0.256	0.868	0.138
Individuals	63,313	15,235	28,089	28,674

Notes: The 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. The sample only includes women. We compare the copayment municipalities to Helsinki. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean for the copayment areas just above the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals just below the cutoff. In Alternative Sample 1, there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18.

Table A4: RD Results, RDHonest.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.898	1.089	1.014	0.766
RD estimate	-0.043	-0.079	-0.030	-0.077
Std. error	0.019	0.034	0.023	0.031
Change (%)	-4.766	-7.240	-2.923	-10.074
CI	[-0.09, -0.00]	[-0.16, -0.00]	[-0.08, 0.02]	[-0.14, -0.01]
Individuals	65,367	17,859	31,850	26,653
Bandwidth	(117, 118)	(135, 138)	(119, 120)	(127, 133)
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.984	1.152	1.168	0.804
RD estimate	-0.010	-0.013	-0.021	0.016
Std. error	0.030	0.081	0.050	0.043
Change (%)	-0.979	-1.156	-1.793	1.993
CI	[-0.07, 0.05]	[-0.19, 0.16]	[-0.13, 0.09]	[-0.08, 0.11]
Individuals	27,746	7,171	12,540	12,803
Bandwidth	(169, 174)	(115, 121)	(158, 160)	(148, 157)

Notes: We implement the RDHonest method proposed by Kolesár and Rothe (2018). We use data aggregated at the age cell (day) level, weight by population, fit linear splines, use the triangular kernel, and allow for different bandwidths below and above the cutoff. The sample only includes women. The optimality criterion for the bandwidth is finite-sample MSE. Standard errors are estimated with the nearest neighbor method. To select the smoothness constant in a data-driven way, we apply the rule of thumb proposed by Armstrong and Kolesár (2020). Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A5: Local Randomization RD Results.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.894	1.101	1.008	0.771
RD estimate	−0.052	−0.099	−0.048	−0.082
Change, %	−5.794	−8.973	−4.775	−10.599
P value	0.014	0.025	0.133	0.008
Individuals	62,367	16,975	30,322	25,470
Visits	8,011	2,640	4,416	2,752
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.999	1.167	1.181	0.826
RD estimate	−0.003	−0.021	−0.011	0.010
Change, %	−0.318	−1.761	−0.896	1.248
P value	0.928	0.783	0.853	0.829
Individuals	25,523	6,580	11,503	11,798
Visits	3,766	1,126	2,001	1,451

Notes: We use a 30-day bandwidth and Neyman large sample inference. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the mean below the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals and the number of GP visits in the bandwidth. The sample only includes women.

Table A6: RD Results without the Donut Hole.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.904	1.115	1.028	0.768
RD estimate	-0.096	-0.145	-0.097	-0.105
Change, %	-10.662	-13.012	-9.462	-13.702
P-value	0.000	0.000	0.000	0.000
CI	[-0.14, -0.06]	[-0.21, -0.08]	[-0.14, -0.05]	[-0.18, -0.05]
Individuals	65,337	17,842	31,829	26,642
Bandwidth	(215, 109)	(270, 147)	(223, 144)	(287, 115)
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.984	1.172	1.179	0.797
RD estimate	-0.030	-0.063	-0.052	-0.007
Change, %	-3.094	-5.413	-4.391	-0.863
P-value	0.202	0.306	0.280	0.757
CI	[-0.09, 0.02]	[-0.20, 0.06]	[-0.14, 0.04]	[-0.09, 0.07]
Individuals	27,671	7,147	12,504	12,769
Bandwidth	(271, 242)	(183, 197)	(209, 237)	(192, 261)

Notes: We use 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. The sample only includes women. We use robust bias corrected inference (Calonico et al., 2014). Here, we use all the data unlike in main analysis in which we use a 3-day donut hole. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A7: Alternative Sample 1: RD Results.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.835	1.027	0.957	0.716
RD estimate	-0.037	-0.091	-0.047	-0.034
Change, %	-4.428	-8.818	-4.901	-4.723
P-value	0.065	0.205	0.234	0.110
CI	[-0.10, 0.00]	[-0.16, 0.03]	[-0.09, 0.02]	[-0.16, 0.02]
Individuals	50,072	12,386	23,321	21,325
Bandwidth	(177, 177)	(177, 177)	(177, 177)	(177, 177)
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.978	1.111	1.150	0.822
RD estimate	-0.012	0.007	-0.001	-0.003
Change, %	-1.219	0.622	-0.058	-0.325
P-value	0.739	0.517	0.761	0.952
CI	[-0.09, 0.07]	[-0.12, 0.24]	[-0.12, 0.16]	[-0.12, 0.11]
Individuals	28,151	6,279	11,757	13,929
Bandwidth	(177, 177)	(177, 177)	(177, 177)	(177, 177)

Notes: In contrast to the main analysis, in Alternative Sample 1 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. We use the local-linear point estimator with a fixed 180-day bandwidth, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We use robust bias corrected inference (Calonico et al., 2014). Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A8: Alternative Sample 1: RD Results, RDHonest.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.840	1.029	0.961	0.709
RD estimate	-0.057	-0.099	-0.050	-0.077
Std. error	0.026	0.056	0.030	0.049
Change (%)	-6.745	-9.607	-5.199	-10.924
CI	[-0.12, 0.00]	[-0.23, 0.04]	[-0.13, 0.03]	[-0.18, 0.03]
Individuals	50,072	12,386	23,321	21,325
Bandwidth	(85, 86)	(61, 61)	(57, 60)	(75, 81)
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.991	1.057	1.156	0.847
RD estimate	-0.017	0.042	0.007	0.006
Std. error	0.041	0.081	0.069	0.059
Change (%)	-1.747	4.012	0.637	0.763
CI	[-0.11, 0.07]	[-0.14, 0.23]	[-0.15, 0.16]	[-0.12, 0.14]
Individuals	28,151	6,279	11,757	13,929
Bandwidth	(87, 85)	(102, 107)	(85, 88)	(84, 85)

Notes: In contrast to the main analysis, in Alternative Sample 1 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. We implement the RDHonest method proposed by Kolesár and Rothe (2018). We use data aggregated at the age cell (day) level, weight by population, fit linear splines, use the triangular kernel, and allow for different bandwidths below and above the cutoff. The sample only includes women. The optimality criterion for the bandwidth is finite-sample MSE. Standard errors are estimated with the nearest neighbor method. To select the smoothness constant in a data-driven way, we apply the rule of thumb proposed by Armstrong and Kolesár (2020). Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A9: Alternative Sample 1: Local Randomization RD Results.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.849	1.059	0.971	0.732
RD estimate	−0.060	−0.126	−0.068	−0.065
Change, %	−7.019	−11.852	−7.047	−8.889
P value	0.011	0.014	0.060	0.050
Individuals	48,799	12,064	22,729	20,766
Visits	5,911	1,778	3,149	2,150
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.983	1.116	1.162	0.827
RD estimate	−0.011	0.038	0.019	−0.004
Change, %	−1.108	3.400	1.636	−0.483
P value	0.747	0.621	0.739	0.928
Individuals	27,311	6,059	11,376	13,539
Visits	3,948	1,017	1,972	1,652

Notes: In contrast to the main analysis, in Alternative Sample 1 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. We use a 30-day bandwidth and Neyman large sample inference. The sample only includes women. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the mean below the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals and the number of GP visits in the bandwidth.

Table A10: Alternative Sample 2: RD Results.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.784	0.940	0.889	0.693
RD estimate	-0.022	-0.042	-0.009	-0.052
Change, %	-2.867	-4.517	-1.024	-7.532
P-value	0.521	0.423	0.401	0.063
CI	[-0.07, 0.03]	[-0.07, 0.16]	[-0.04, 0.11]	[-0.18, 0.00]
Individuals	49,232	11,701	21,811	22,115
Bandwidth	(177, 177)	(177, 177)	(177, 177)	(177, 177)
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.907	1.020	1.058	0.768
RD estimate	0.008	0.012	0.021	0.023
Change, %	0.874	1.199	1.942	3.049
P-value	0.658	0.531	0.537	0.624
CI	[-0.07, 0.11]	[-0.14, 0.27]	[-0.10, 0.19]	[-0.08, 0.14]
Individuals	27,765	6,034	11,170	14,142
Bandwidth	(177, 177)	(177, 177)	(177, 177)	(177, 177)

Notes: In contrast to the main analysis, in Alternative Sample 2 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday with no change in family relationship status (e.g., a child). We use the local-linear point estimator with a fixed 180-day bandwidth, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We use robust bias corrected inference (Calonico et al., 2014). Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A11: Alternative Sample 2: RD Results, RDHonest.

A. Copayment Municipalities.

	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.745	0.879	0.813	0.699
RD estimate	-0.003	0.092	0.047	-0.074
Std. error	0.035	0.065	0.048	0.043
Change (%)	-0.385	10.469	5.777	-10.592
CI	[-0.09, 0.08]	[-0.07, 0.26]	[-0.07, 0.17]	[-0.17, 0.02]
Individuals	49,232	11,701	21,811	22,115
Bandwidth	(44, 47)	(50, 54)	(45, 52)	(91, 96)

B. Exemption Municipalities.

	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.903	1.013	1.029	0.751
RD estimate	-0.002	0.102	0.060	0.040
Std. error	0.086	0.125	0.121	0.095
Change (%)	-0.251	10.032	5.849	5.334
CI	[-0.19, 0.19]	[-0.19, 0.39]	[-0.21, 0.33]	[-0.17, 0.25]
Individuals	27,765	6,034	11,170	14,142
Bandwidth	(37, 37)	(55, 63)	(42, 44)	(42, 46)

Notes: In contrast to the main analysis, in Alternative Sample 2 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday with no change in family relationship status (e.g., a child). We implement the RDHonest method proposed by Kolesár and Rothe (2018). We use data aggregated at the age cell (day) level, weight by population, fit linear splines, use the triangular kernel, and allow for different bandwidths below and above the cutoff. The sample only includes women. The optimality criterion for the bandwidth is finite-sample MSE. Standard errors are estimated with the nearest neighbor method. To select the smoothness constant in a data-driven way, we apply the rule of thumb proposed by Armstrong and Kolesár (2020). Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the fitted mean just below the cutoff, and percentage change compares the estimate to this level. For sample sizes, 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.

Table A12: Alternative Sample 2: Local Randomization RD Results.

A. Copayment Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.777	0.925	0.863	0.698
RD estimate	−0.003	−0.007	0.026	−0.050
Change, %	−0.404	−0.706	3.028	−7.107
P value	0.896	0.902	0.491	0.143
Individuals	43,140	10,351	19,148	19,358
Visits	4,946	1,411	2,481	1,927
B. Exemption Municipalities.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.888	1.003	1.039	0.756
RD estimate	0.041	0.079	0.067	0.041
Change, %	4.634	7.908	6.426	5.452
P value	0.233	0.314	0.260	0.356
Individuals	24,324	5,284	9,722	12,465
Visits	3,268	815	1,542	1,432

Notes: In contrast to the main analysis, in Alternative Sample 2 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday with no change in family relationship status (e.g., a child). We use a 30-day bandwidth and Neyman large sample inference. The sample only includes women. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Level is the mean below the cutoff, and percentage change compares the estimate to this level. For sample sizes, we report the number of individuals and the number of GP visits in the bandwidth.

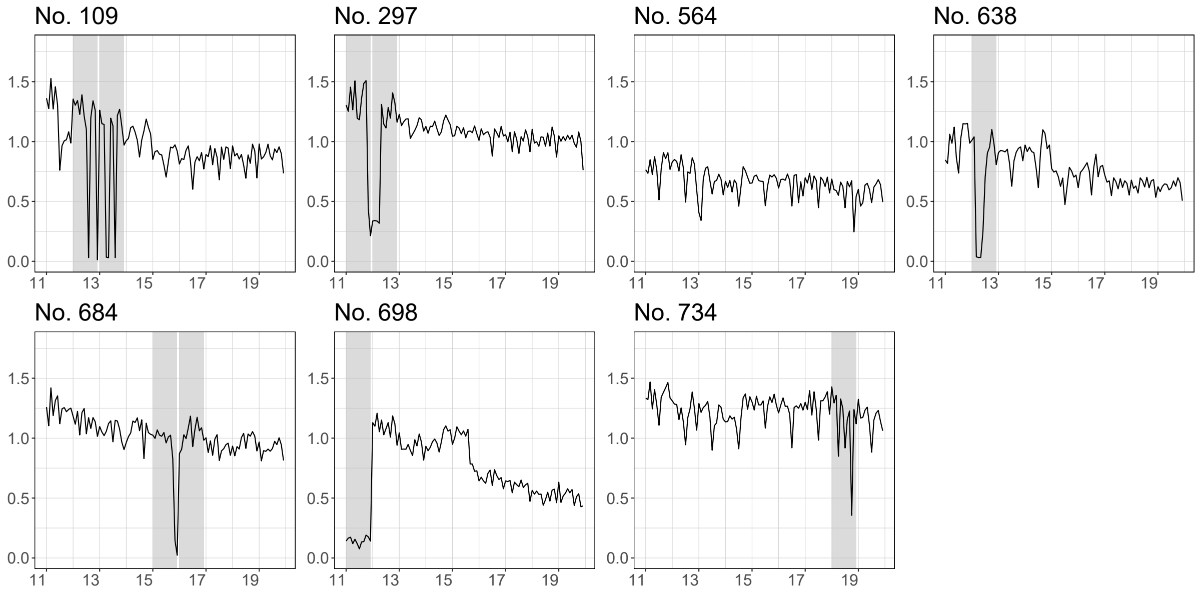

Figure A1: Visit Data Quality.

Notes: Municipality-year pairs that we exclude from analysis are highlighted in gray. We first computed a distribution of mean contacts by permutationally dropping every combination of four consequtive months. We then marked an observation to be invalid if its value was less than 40% of the largest observed mean (July as a holiday month was not considered).

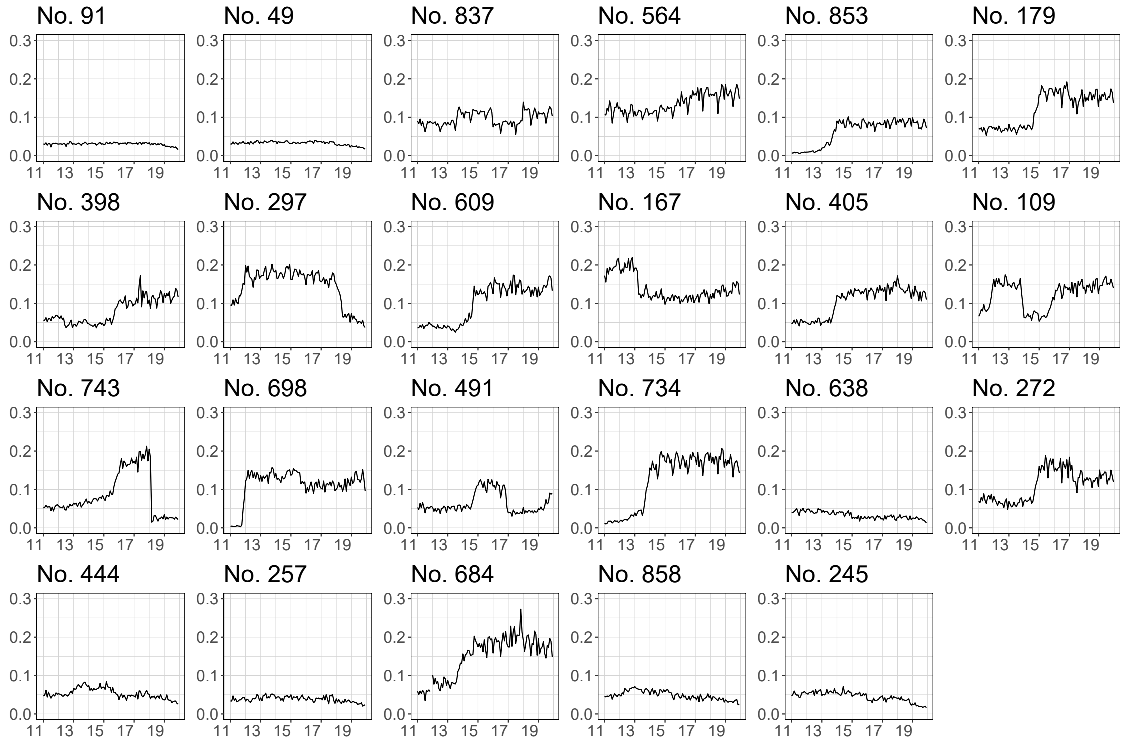

Figure A2: Trends in Referrals to Specialist Care.

Notes: Some observed patterns create doubts on whether the absolute number of referrals is reliable. However, the data issues should not be related to our running variable. We exclude only one municipality-year pair from the analysis: Rauma in 2011 due to a considerable spike in December 2011.

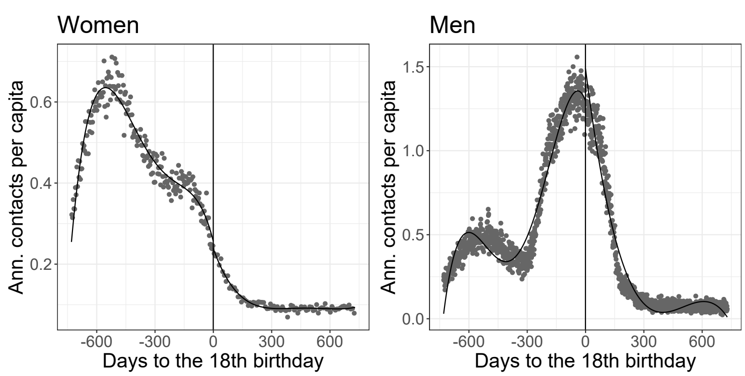

Figure A3: Conscript Health Checks for Men Invalidate Identification.

Notes: Health checks contain both conscript and student health checks from 2011-2018. 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 et al., 2015).

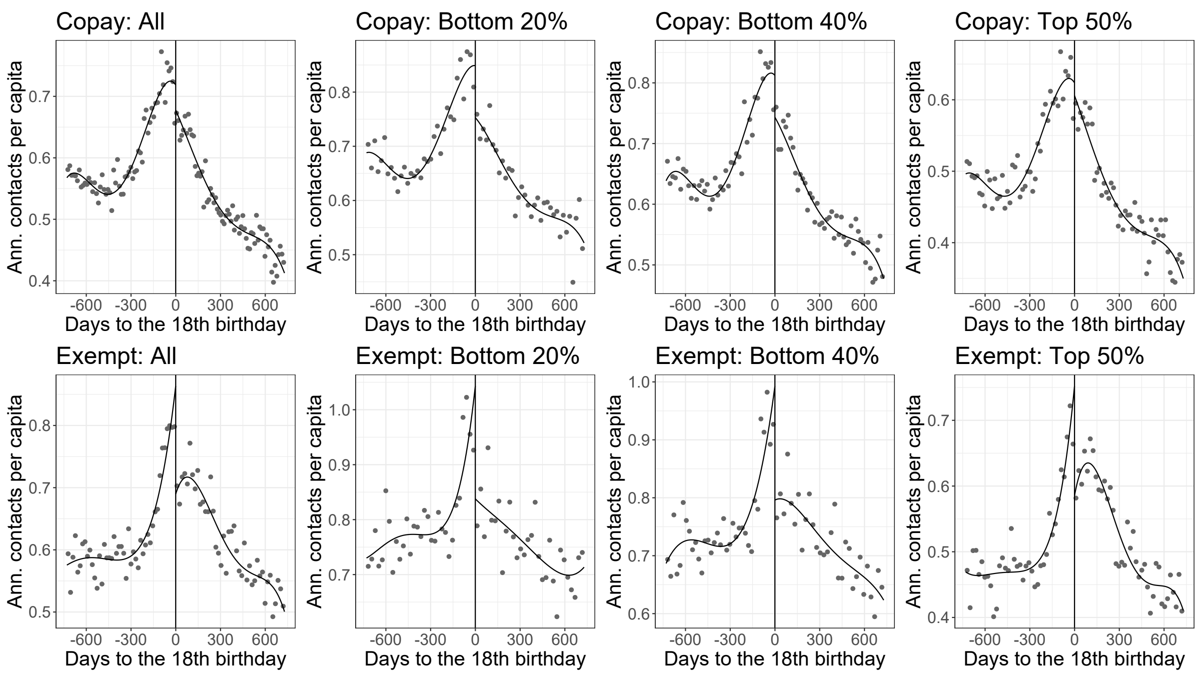

Figure A4: RD Plots for Men: GP Visits.

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 household disposable income, measured for the year when the individual was 17 years old on December 31st. 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 et al., 2015).

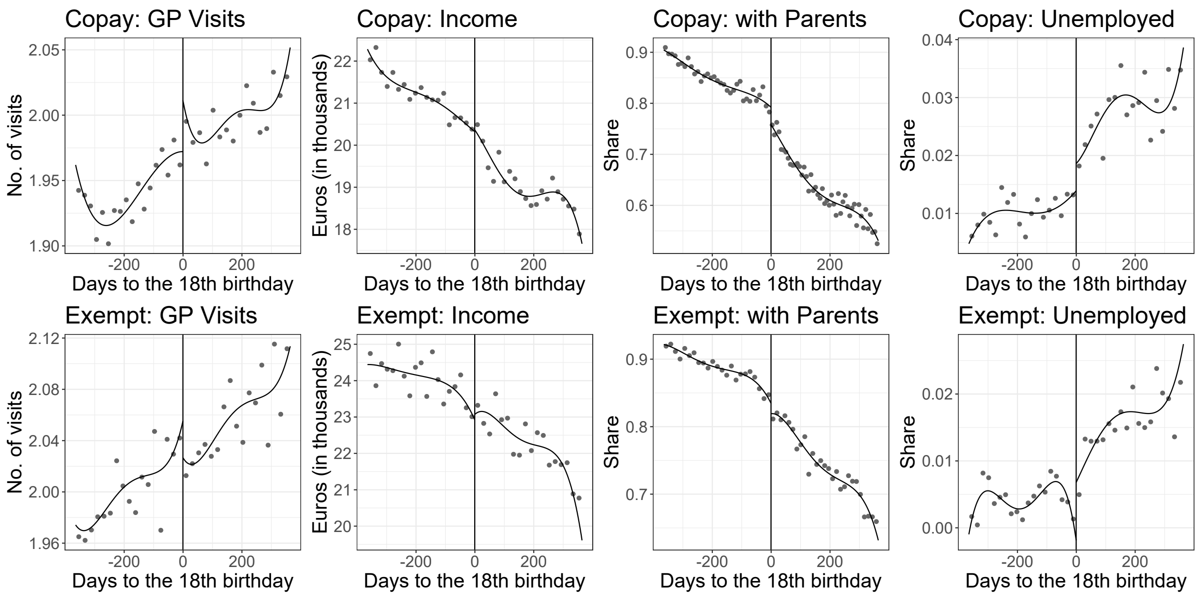

Figure A5: Does Moving away from Parents Threaten Identification?

Notes: We examine the potential discontinuities at the 18th birthday for those born at the turn of the year. The sample only includes women. 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 $t \in \{-365, 0, 365\}$. 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. GP visits represent the number of total GP visits observable in the data in 2011-2019. Income is equivalized disposable income, measured annually. "With parents" refers to the share of those who live with their parents on December 31st. Unemployment status is measured on December 31st. 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 et al., 2015).

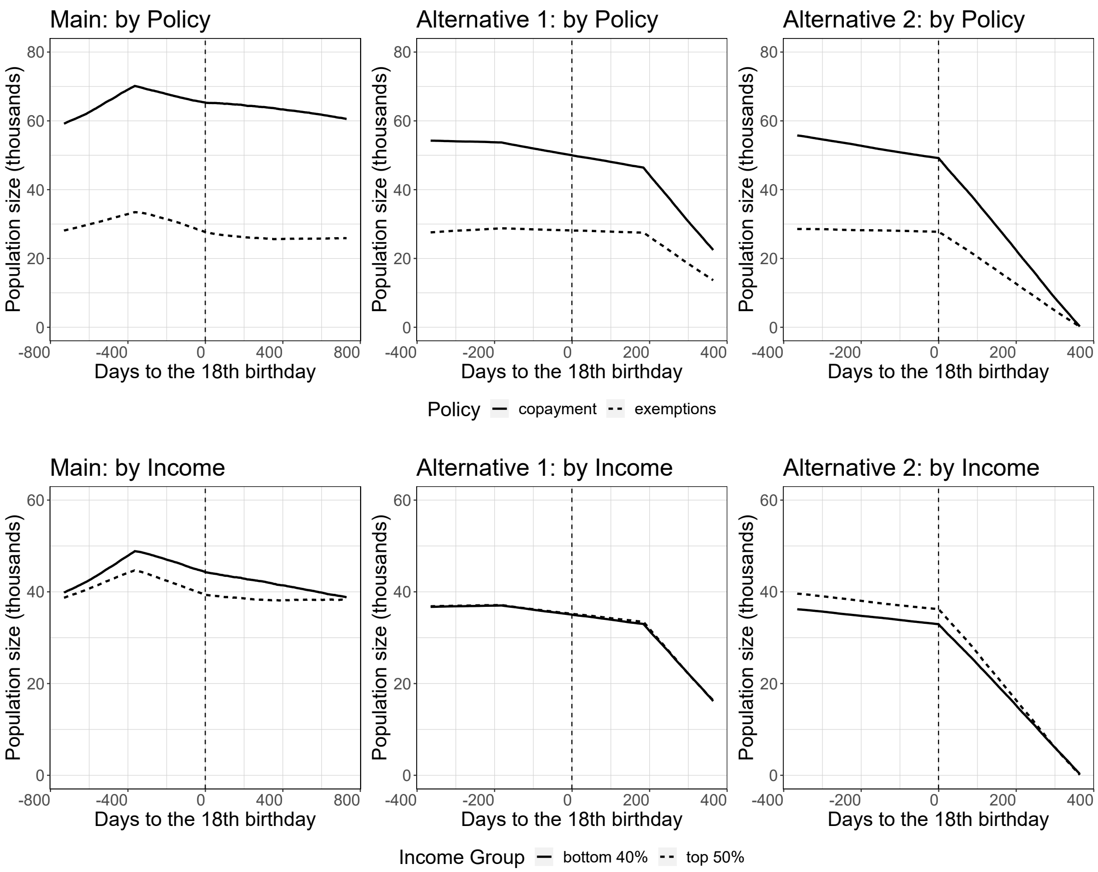

Figure A6: Age Cell Population Sizes.

Notes: We count the age cell population sizes by municipal policy or by income groups for women in our analysis data. Bottom 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. In Alternative Sample 1, there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18.

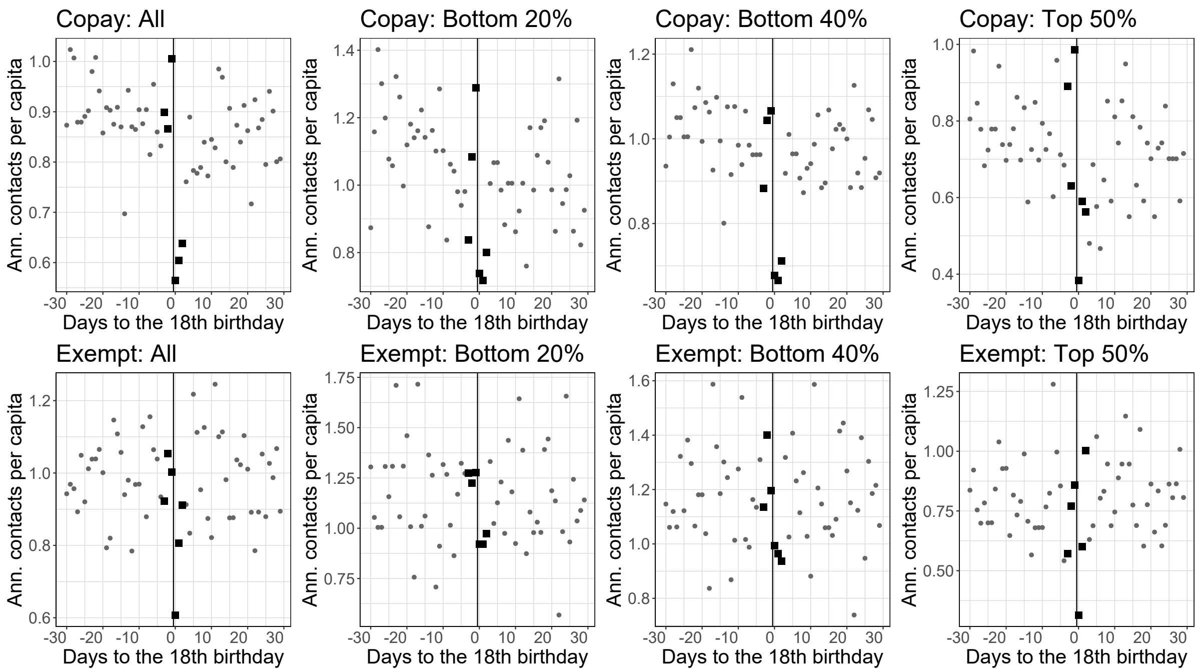

Figure A7: Age Cell Means of GP Use.

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 household disposable income, measured for the year when the individual was 17 years old on December 31st. The sample only includes women.

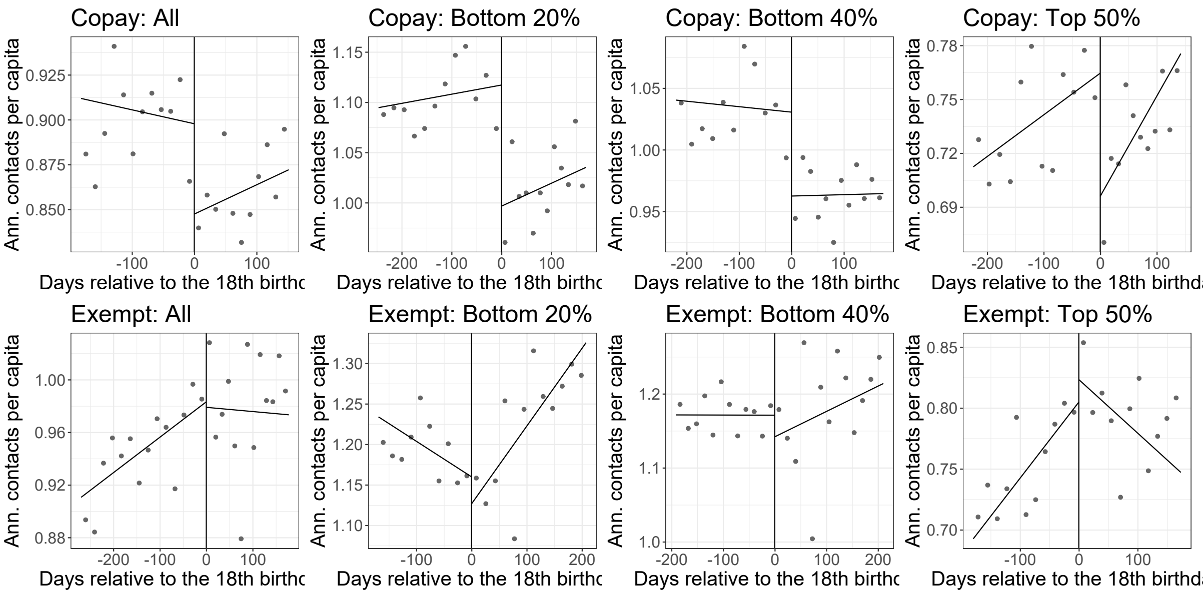

Figure A8: Main RD Results.

Notes: We use 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 (Calonico et al., 2014). The sample only includes women. 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 et al., 2015). Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Table 1 presents the corresponding point estimates, confidence intervals, and sample sizes.

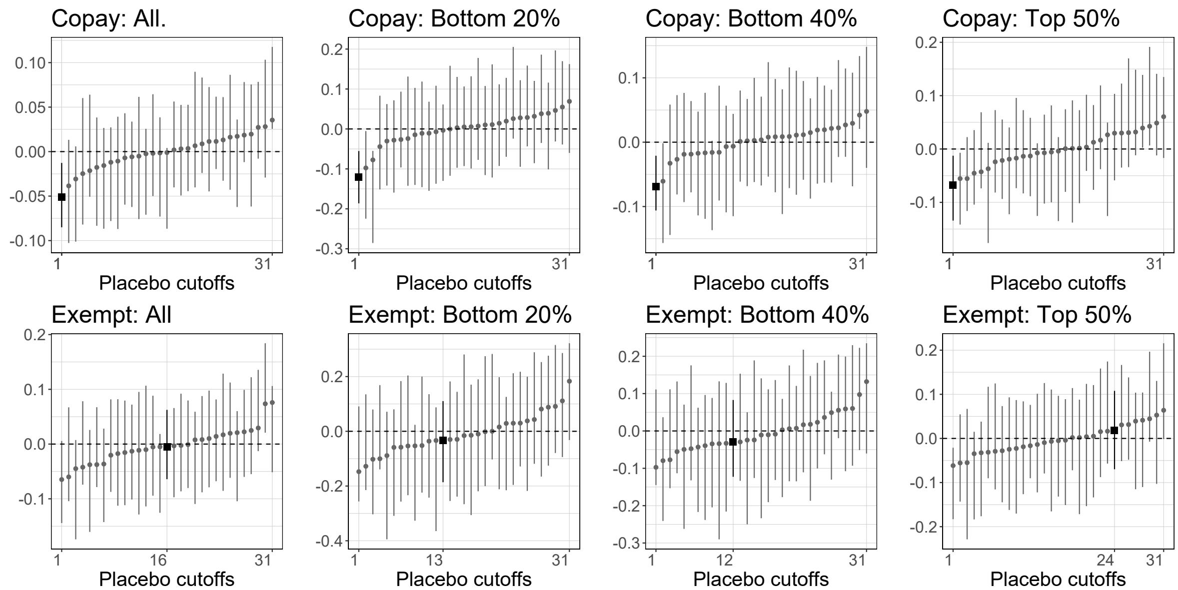

Figure A9: RD Results, Placebo Cutoffs.

Notes: We use the local-linear point estimator with a fixed 150-day bandwidth, the triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We report robust bias corrected confidence intervals (Calonico et al., 2014). The estimate based on the real cutoff is shown in black. We estimate the effects for 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. Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st.

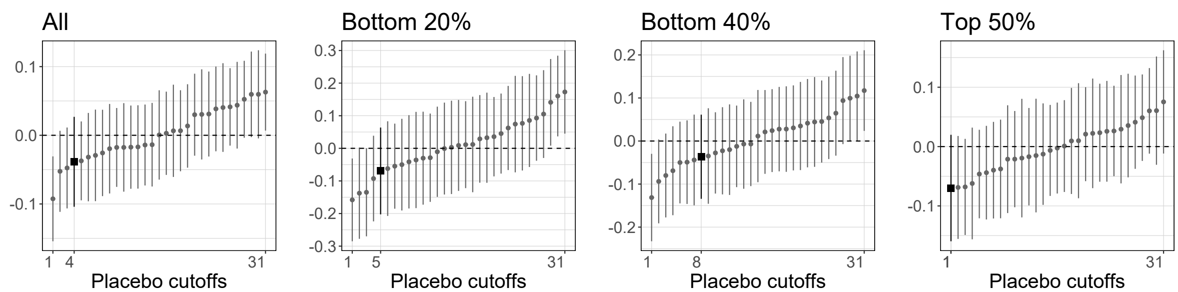

Figure A10: RD-DID Results, Placebo Cutoffs.

Notes: The estimates are based on Model 1 with 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. The sample only includes women. The estimate based on the real cutoff is shown in black. We estimate the effects for 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. Treatment municipalities are the copayment municipalities. Comparison municipalities are the exemption municipalities. The model is estimated separately for income groups. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st.

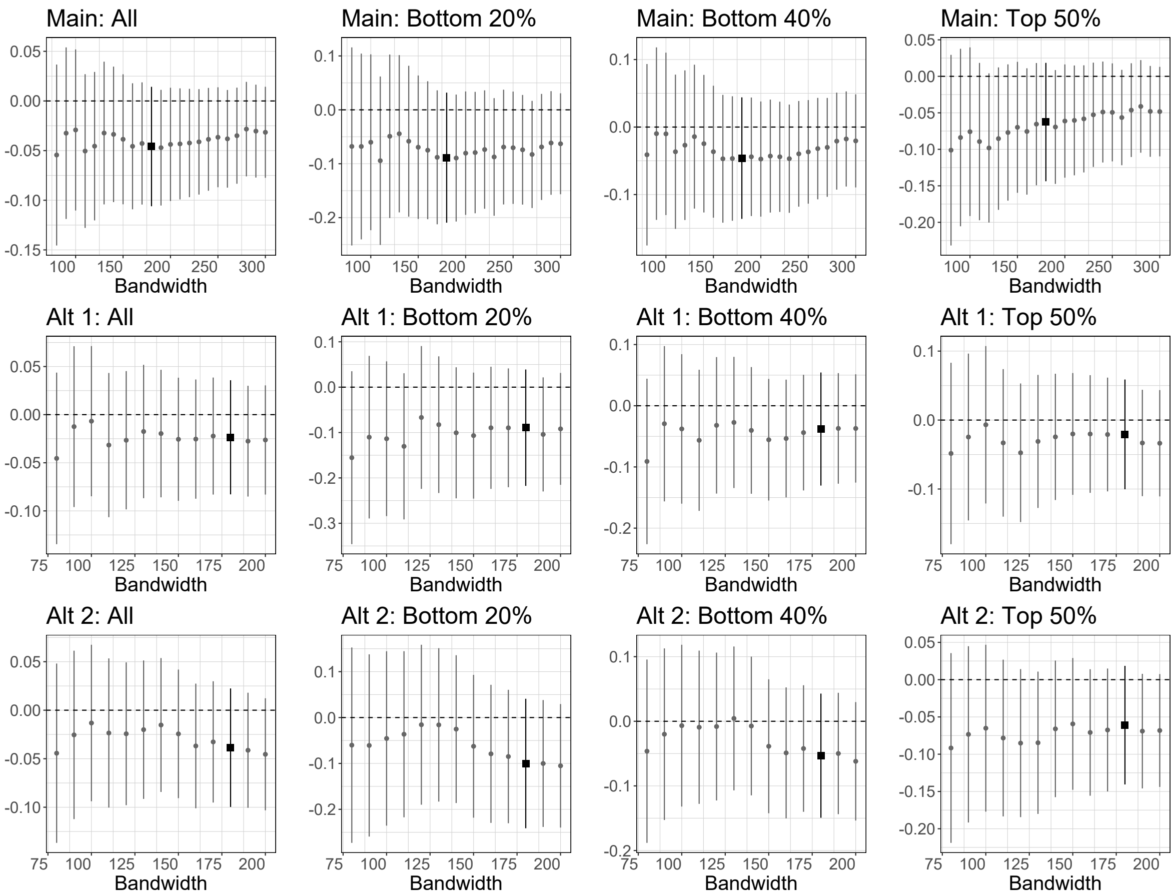

Figure A11: RD-DID Results, Sensitivity to Bandwidth Size.

Notes: The estimates are based on Model 1 using 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. The sample only includes women. Treatment municipalities are the copayment municipalities. Comparison municipalities are the exemption municipalities and Helsinki. The model is estimated separately for income groups. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. In Alternative Sample 1 (Alt 1), there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 (Alt 2) restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18.

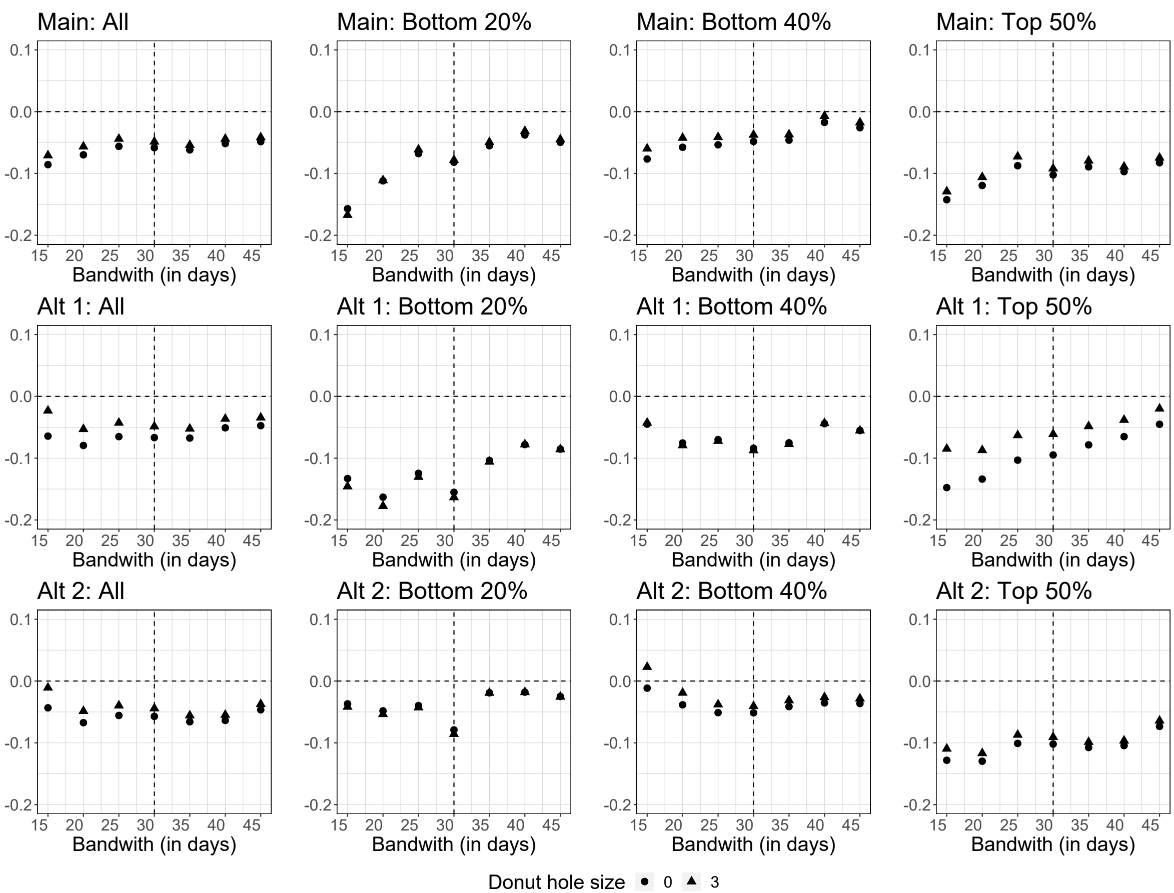

Figure A12: Local Randomization RD-DID Results, Sensitivity to Bandwidth and Donut Hole.

Notes: The estimates are based on Model 2 using a 30-day bandwidth as the baseline. Treatment municipalities are the copayment municipalities. Comparison municipalities are the exemption municipalities and Helsinki. The model is estimated separately for income groups. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. In Alternative Sample 1 (Alt 1), there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. Alternative Sample 2 (Alt 2) restricts 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 December 31st of the year when turning 17 and December 31st of the year when turning 18. The sample only includes women.

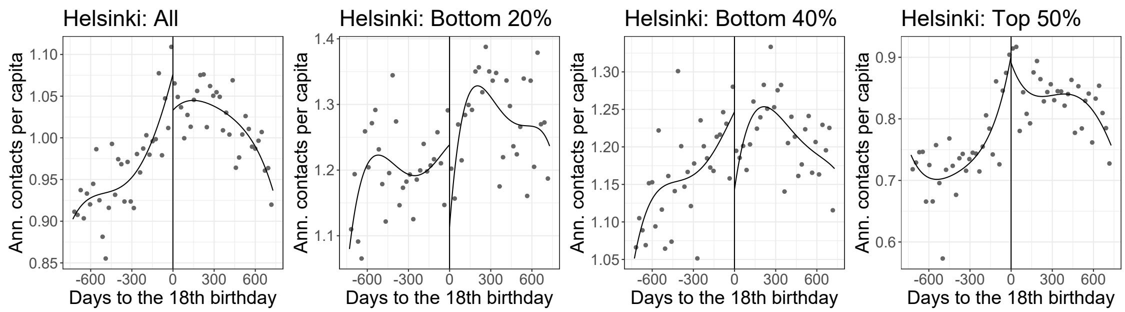

Figure A13: RD Plots in Helsinki.

Notes: In Helsinki, no GP visit copayment was charged in 2013-2019. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The sample only includes women. 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 et al., 2015).

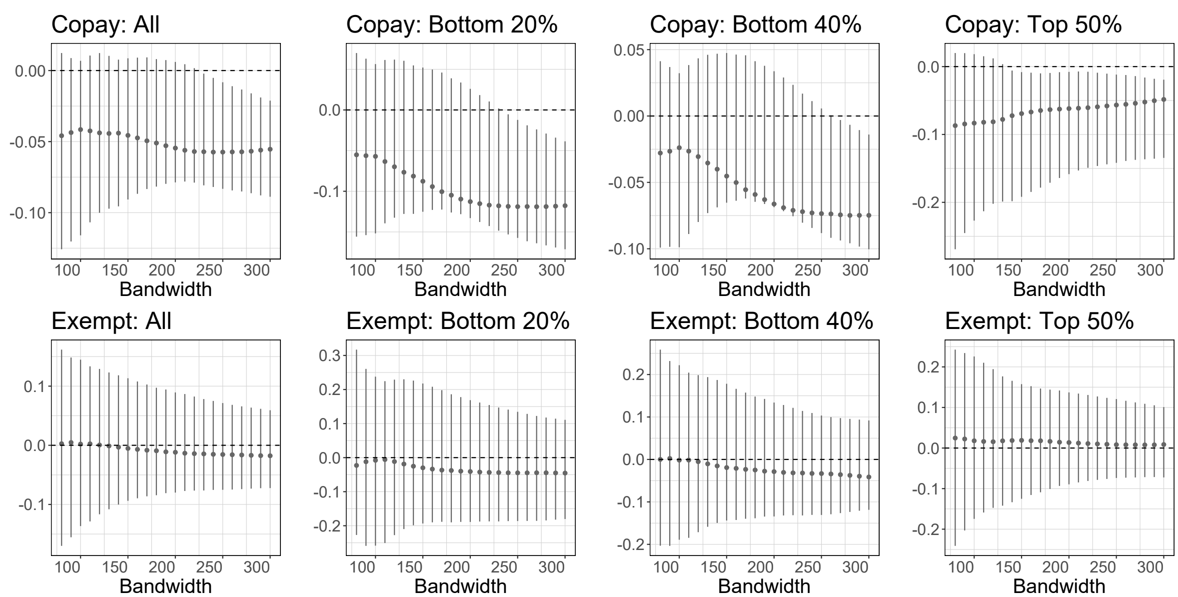

Figure A14: RD Results, Sensitivity to Bandwidth Size.

Notes: We use the local-linear point estimator with the same bandwidth below and above the cutoff, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We report robust bias corrected confidence intervals (Calonico et al., 2014). Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st.

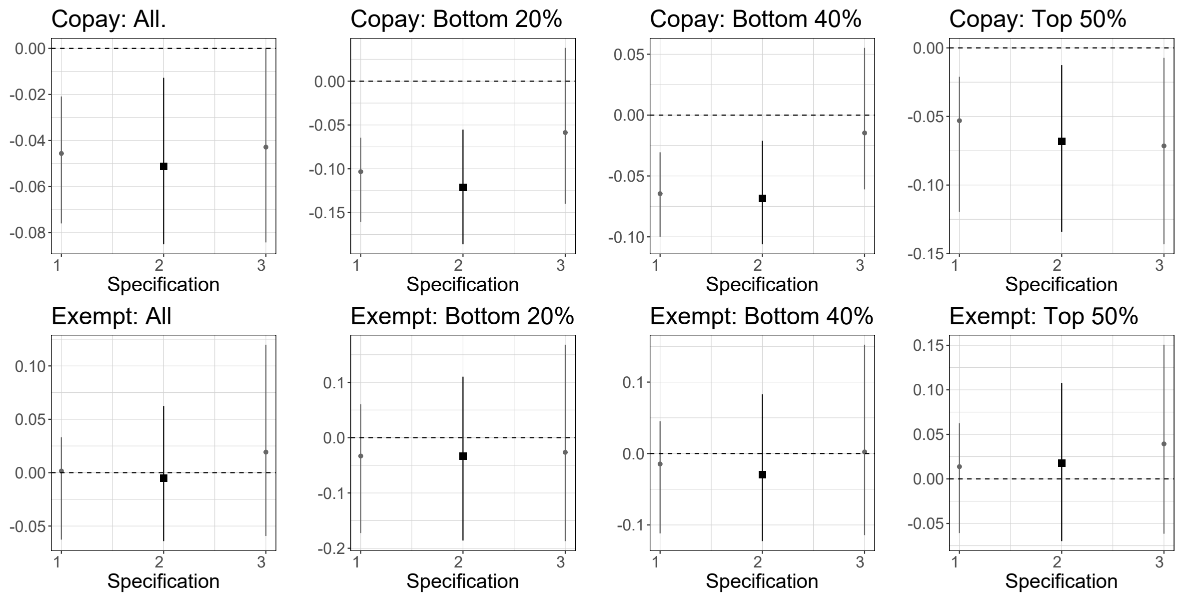

Figure A15: RD Results, Sensitivity to Specification.

Notes: As the baseline, we use 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. The sample only includes women. We use robust bias corrected inference (Calonico et al., 2014). Specifications from 1 to 3: difference-in-means, local linear, and local quadratic. Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. Base specification (local linear) is shown in black.

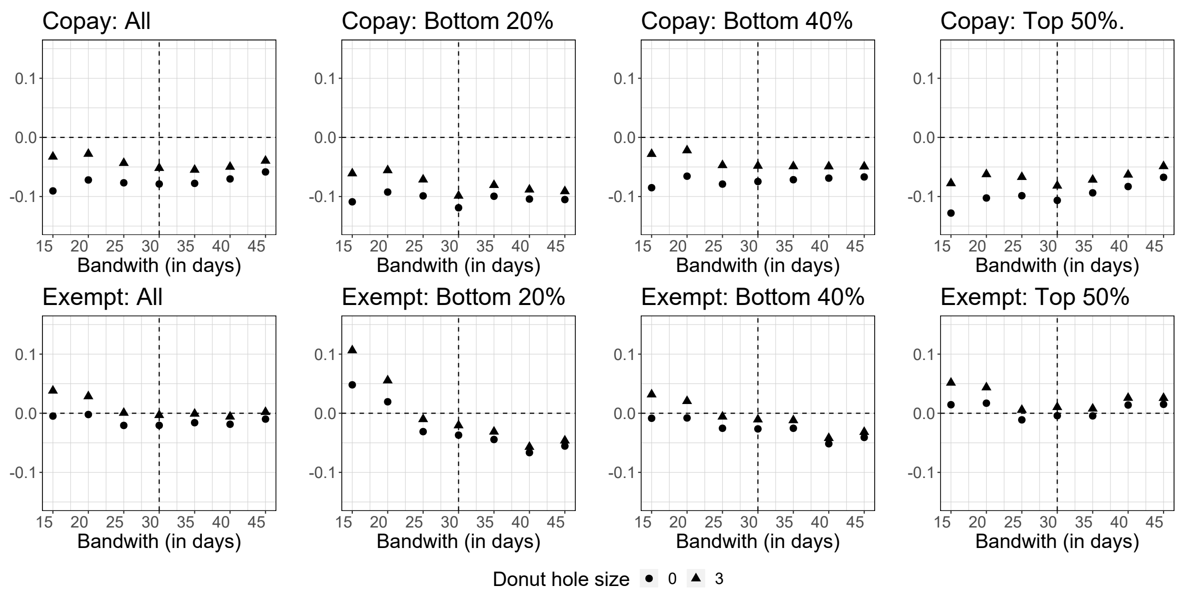

Figure A16: Local Randomization RD Results, Sensitivity to Bandwidth and Donut Hole.

Notes: As the baseline, we use a 30-day bandwidth with a 3-day donut hole. Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st. The sample only includes women.

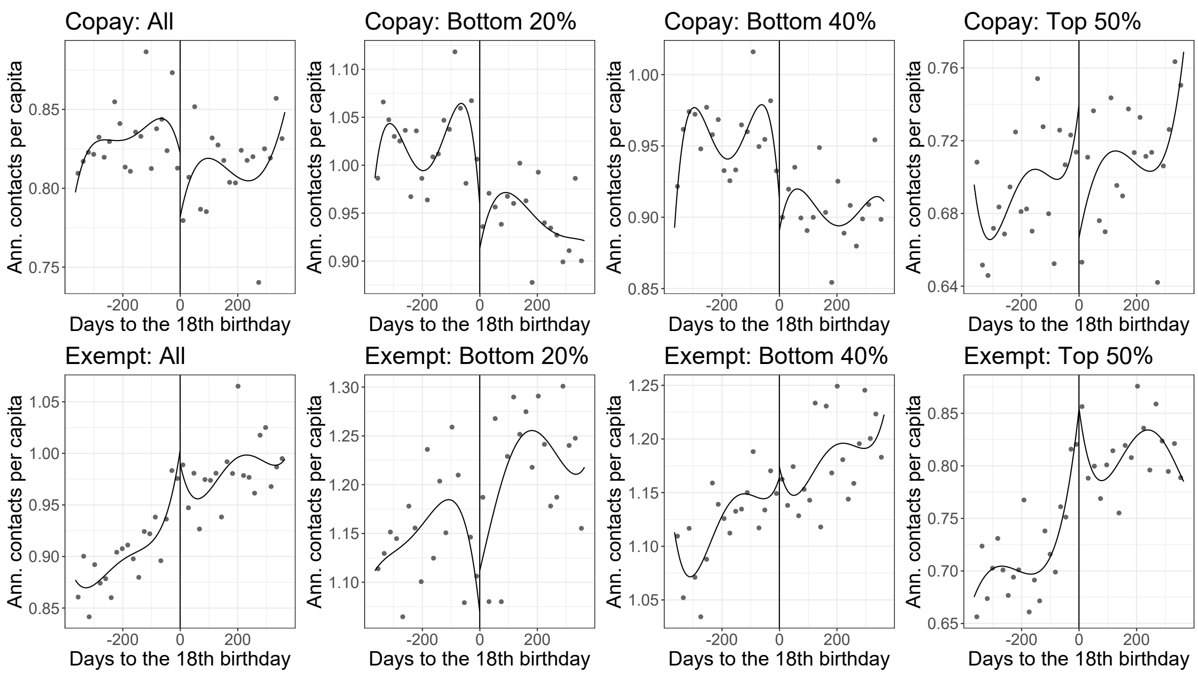

Figure A17: Alternative Sample 1: RD Plots.

Notes: In contrast to the main analysis, in Alternative Sample 1 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. 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 household disposable income, measured for the year when the individual was 17 years old on December 31st. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The sample only includes women. 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 et al., 2015).

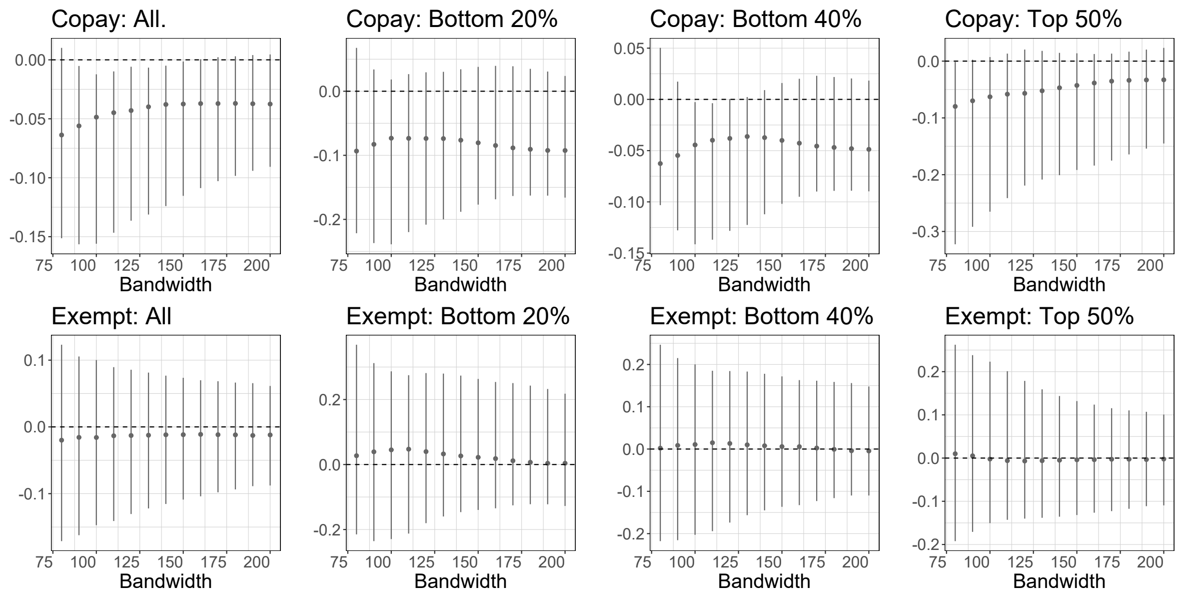

Figure A18: Alternative Sample 1: RD Results, Sensitivity to Bandwidth Size.

Notes: In contrast to the main analysis, in Alternative Sample 1 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday. We use the local-linear point estimator with the same bandwidth below and above the cutoff, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We report robust bias corrected confidence intervals (Calonico et al., 2014). Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st.

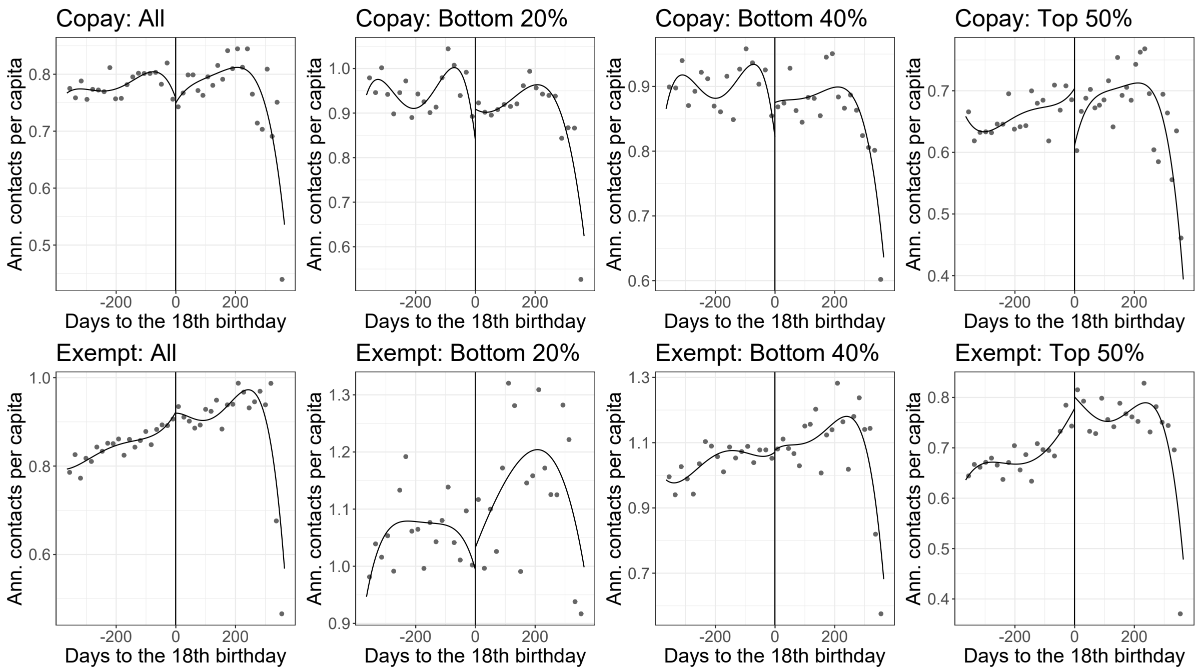

Figure A19: Alternative Sample 2: RD Plots.

Notes: In contrast to the main analysis, in Alternative Sample 2 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday with no change in family relationship status (e.g., a child). 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 household disposable income, measured for the year when the individual was 17 years old on December 31st. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The sample only includes women. 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 et al., 2015).

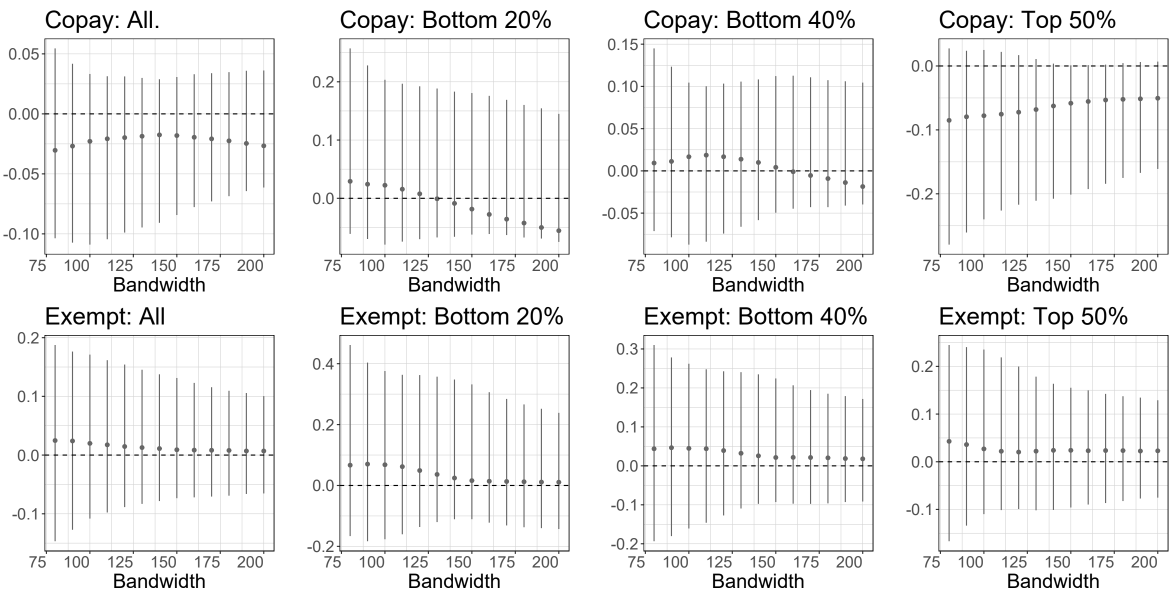

Figure A20: Alternative Sample 2: RD Results, Sensitivity to Bandwidth Size.

Notes: In contrast to the main analysis, in Alternative Sample 2 there are only individuals who are observed to reside in the same policy area 6 months before and after the 18th birthday with no change in family relationship status (e.g., a child). We use the local-linear point estimator with the same bandwidth below and above the cutoff, triangular kernel multiplied by age cell population size weights, and data aggregated at the running variable level. The sample only includes women. We report robust bias corrected confidence intervals (Calonico et al., 2014). Copay and exempt refer to copayment and exemption municipalities. Exemption municipalities contain both 1) municipalities where students over 18 years of age are exempted if they show a student status certificate and 2) Helsinki, where no copayment was charged. Bottom 20% and 40% and top 50% are based on the distribution of equivalized household disposable income, measured for the year when the individual was 17 years old on December 31st.

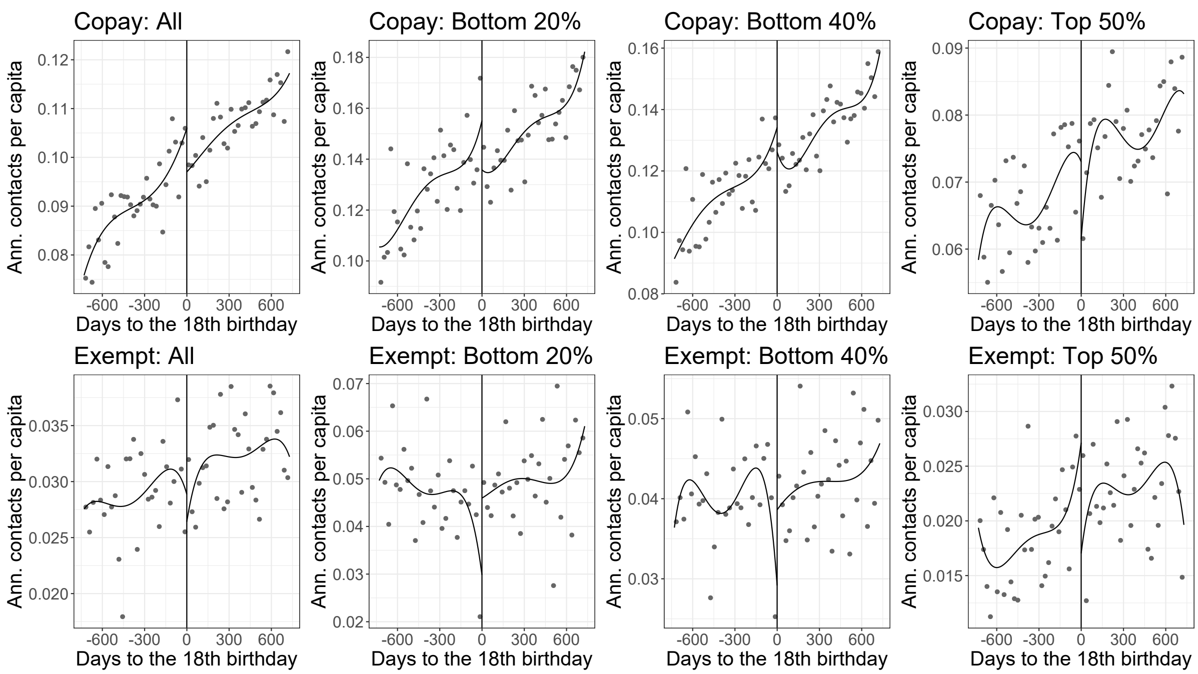

Figure A21: RD Plots: Referrals to Specialist Care.

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 household disposable income, measured for the year when the individual was 17 years old on December 31st. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The sample only includes women. 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 et al., 2015).

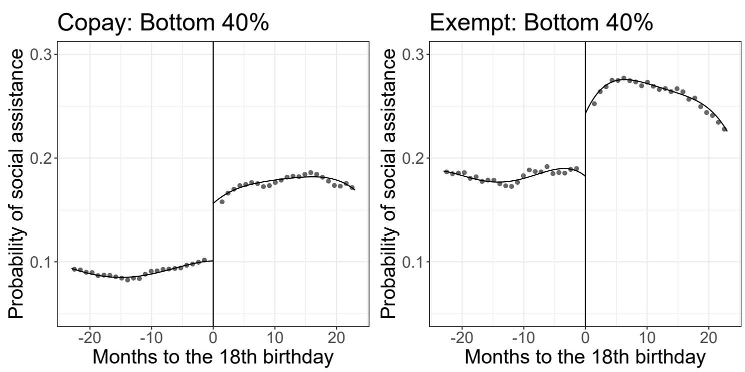

Figure A22: RD Plots: Social Assistance Use.

Notes: Outcome is in indicator of living in a family where someone received social assistance. 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 household disposable income, measured for the year when the individual was 17 years old on December 31st. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The sample only includes women. 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 et al., 2015).