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

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Abstract

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

Keywords: Cost-sharing, copayment, out-of-pocket costs, healthcare use, primary care, general practitioner, regression discontinuity, difference-in-discontinuities
JEL codes: I18, I14, I13, H42, I11

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1 Introduction

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

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

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

cause discontinuities at the 18th birthday (preferable for unbiased point estimation), but it has less statistical power (inferior for inference).

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

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

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

Our results show that GP visits decrease by 4–5% in the copayment municipalities at the 18th birthday. These RD estimates are statistically significant, while the estimates in the comparison municipalities are close to zero and insignificant. The RD-DID estimates are consistent with the RD results, but due to their lower statistical power are statistically insignificant.[3] Regarding the magnitudes, the RD estimates for all individuals vary, depending on the estimator, between -0.04 and -0.05 annualized visits (-4.8% to -5.8%) in the copayment municipalities compared to the estimates of -0.00 to -0.01 visits (-0.3% to -1.0%) in the exemption municipalities. Regarding the potential income-related heterogeneity, we do find that the largest reductions in the RD-DID estimates are for the bottom 20% of the income distribution. Their GP use decreases by 0.08–0.10 annualized visits (7–10%). The estimates attenuate for the bottom 40%. However, the estimates are also larger than average for the top 50%, showing reductions (albeit rather temporary) of 0.05–0.06 visits (6–8%). Thus, our results do not support the hypothesis that the effects are overwhelmingly concentrated at the lower end of the income distribution while the top end does not respond noticeably. The modest average effects and the lack of significant income-related heterogeneity in the effects partially alleviate popular policy worries about the unequal impacts of the copayment examined in this context.

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

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

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

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 empirical studies from other Nordic countries that examine the effects of copayments on public primary care GP use at age thresholds 7, 16, and 20 in Sweden and Norway (Johansson et al., 2019; Magnussen Landsem and Magnussen, 2018; Nilsson and Paul, 2018).[5] Similarly to Finland, Sweden has a primary care system where access to GP appointments is based on triage. This is a crucial aspect of the institutions: gatekeeping should disproportionately reduce GP appointments that are of low value medically, and thus, the effects of the copayment on GP use should be smaller.

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

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

[6]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, and Vargas Lopes et al. (2022) study the effect of an increased deductible on mental health care use.

accounts for the possibility of confounding from other events at the milestone 18th birthday.

We contribute to the literature also both data-wise and by using more state-of-the-art estimation procedures than those previously applied in this specific context. First, we combine recent data from 2011–2019 with a design that also has comparison municipalities which either exempt students or charge no copayment at all. This setting stands in contrast to Nilsson and Paul (2018), who use older data from 1999–2006, as well as 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 essential because the composition of patients has most likely changed over time as the widespread adoption of internet and online symptom checkers have made self-diagnosing much easier, and some tasks have been shifted from GPs to nurses. Importantly, the availability of comparison municipalities allows us to account for discontinuities in the potential outcomes that are unrelated to the copayment policy, as Nilsson and Paul (2018) do. The age cutoffs of 20 in Johansson et al. (2019) and 16 in Magnussen Landsem and Magnussen (2018) are likely less problematic than our cutoff of 18 with respect to other factors creating discontinuities in the potential outcomes. Therefore, the comparison municipalities and RD-DID models have greater value in our application. Second, because our administrative data contain both exact birth dates and daily visits, which is more granular than in the earlier studies, we can use appropriate optimal bandwidths, and bias-corrected and robust inference methods (Calonico et al., 2014). We also account for a discrete running variable (Kolesár and Rothe, 2018).

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

2 Institutional Background

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

Moreover, school and student healthcare is organized by municipalities and provides preventive services concerning mental, sexual and dental health, and substance abuse. It varies across municipalities whether curative GP appointments are available and whether there is a copayment. Finally, occupational healthcare provides for many employees a fast access to curative services without charging copayments, but there is some gatekeeping.

Municipalities organize publicly funded primary care and run a health center (with potentially many operating locations) on their own, in voluntary cooperation with others, or outsource the services. Services are financed by municipal taxes, state transfers, copayments, and borrowing. The state sets both the maximum copayments and which services are offered

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

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

free of charge. The maximum GP visit copayment increased from 13.70 euros to 20.60 euros in 2011–2019.[9] Municipalities set their policies given these constraints. Individuals under 18 years of age are nationally exempted from GP visit copayments. At least three municipalities (Espoo, Turku, and Tuusula) also exempt students with a student status certificate, available free of charge from the school chancellery. The capital Helsinki is the only municipality not charging copayments for GP visits. It abolished the copayment in 2013. In 2018, over two thirds of the population lived in municipalities charging the copayment for the first three visits annually and 15% in municipalities charging an annual copayment that is in most cases two times the per-visit copayment (Haaga, 2019). Social assistance is a means-tested last-resort benefit for those households with the lowest incomes and wealth, and unfortunately underused. Social assistance can also cover out-of-pocket costs for public health care and prescription drugs.

3 Data

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

We searched whether students over 18 years of age pay copayments for curative GP visits by observing the websites of the 30 largest municipalities. The policy was clearly and explicitly stated in 23 cases. 19 municipalities charged copayments in early 2020, 3 (Espoo, Turku, and Tuusula) exempted individuals showing a student status certificate, and 1 (Helsinki) charged

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

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

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

no copayment at all. We assume that the policies were the same over the study period.[12] The copayment municipalities contain both municipalities offering the annual copayment option and municipalities charging a copayment for the first three visits annually. There is also variation in copayment levels across municipalities (in 2016–2019) and over time (the maximum copayment increased from 13.70 euros to 20.90 euros in 2011–2019).

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

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

4 Methods

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

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

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

(Figure A1 and Figure A2), challenging the RD design. Every male takes part in a call-up during the year they turn 18. They have nurse and GP appointments before the call-up, often in Spring. Women can serve voluntarily, but it is rare: no more than 5% of the birth cohort applied for service in a record year of 2018. Women do have a GP-led health check during the first or the second year of their upper secondary education, beginning at the age of 15 or 16.

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

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

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

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 municipalities (a clear discontinuity in policy) the observed discontinuities in the exemption municipalities (either no copayment or an exemption for students). However, our main analysis is based on RD models due to their higher statistical power. For clarity, we report the effects separately in the copayment and exemption municipalities to allow for an easy comparison between the estimates.

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

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

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

policies.

RD estimates. As mentioned above, we estimate RD models separately in the copayment and exemption municipalities. Methods based on the nonparametric local polynomial approach allow us to use data-driven bandwidth selection and inference that accounts for misspecification or smoothing bias (Cattaneo 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 et al., 2014) using the R package *rdrobust*.[15] To construct confidence intervals, the smoothing bias is removed by first estimating it using another local regression – one order higher. Valid inference must account for both regressions. Ultimately, the confidence intervals are rescaled and recentered around the bias-corrected point estimate.

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

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

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

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

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

following regression specification:

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

where α is an intercept, g and t denote policy group (copayment and exemption) and day. Age is in days, $Threshold$ is the cutoff, $Adult$ is a dummy for being over 18, and $Copay$ is a dummy for copayment municipalities. The parameter δ is the coefficient of interest.

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

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

5 Results

5.1 Main Results

When interpreting the results, we should keep in mind that they concern only young women. The RD plots are presented in Figure 1. In copayment municipalities, there appears to be a drop of 0.05 annualized visits per capita for all individuals at the cutoff and a larger reduction for the lower end of the income distribution. A somewhat similar decrease seems to be present also for the top 50%, but the effect is less obvious as the data are noisier and the effect arises solely from observation very close to the cutoff. In the exemption municipalities, there are no clear indications of substantial discontinuities. Figure A6 shows the same RD plots but uses data only from within the optimal bandwidth (Calonico et al., 2014) and fits linear splines instead of fourth-order polynomials. Also this figure indicates a robust effect for bottom income groups but a more sensitive and less persistent effect for the top 50% group.

The main RD results based on local polynomial methods are presented in Table 1. For inference and precision, we prefer the local polynomial RD estimates that are reported separately in the copayment and exemption municipalities. These results are depicted graphically in Figure A6 as RD plots. In copayment municipalities, turning 18 is associated with a reduction in GP use: -0.05 annualized visits (-5.7%) for all individuals. The corresponding estimate is close to zero in exemption municipalities: -0.01 annualized visits (-0.5%). In both policy areas, the reductions are largest at the bottom 20% of the income distribution. For them, GP visits decrease by -0.12 visits (-10.8%) in the copayment municipalities and by -0.03 visits (-2.9%) in the exemption municipalities. For the bottom 40%, GP visits decrease by -0.07 visits (-6.7%) in the copayment municipalities and by -0.03 visit (-2.5%) in the exemption municipalities. For the top 50%, we estimate effects of -0.07 annualized visits (-8.9%) in the copayment municipalities and $+0.02$ annualized visits ($+2.2\%$) in the exemption municipalities.

The RD results are all statistically different from zero in the copayment municipalities, but closer to zero and insignificant in the exemption municipalities. The largest p-value in the

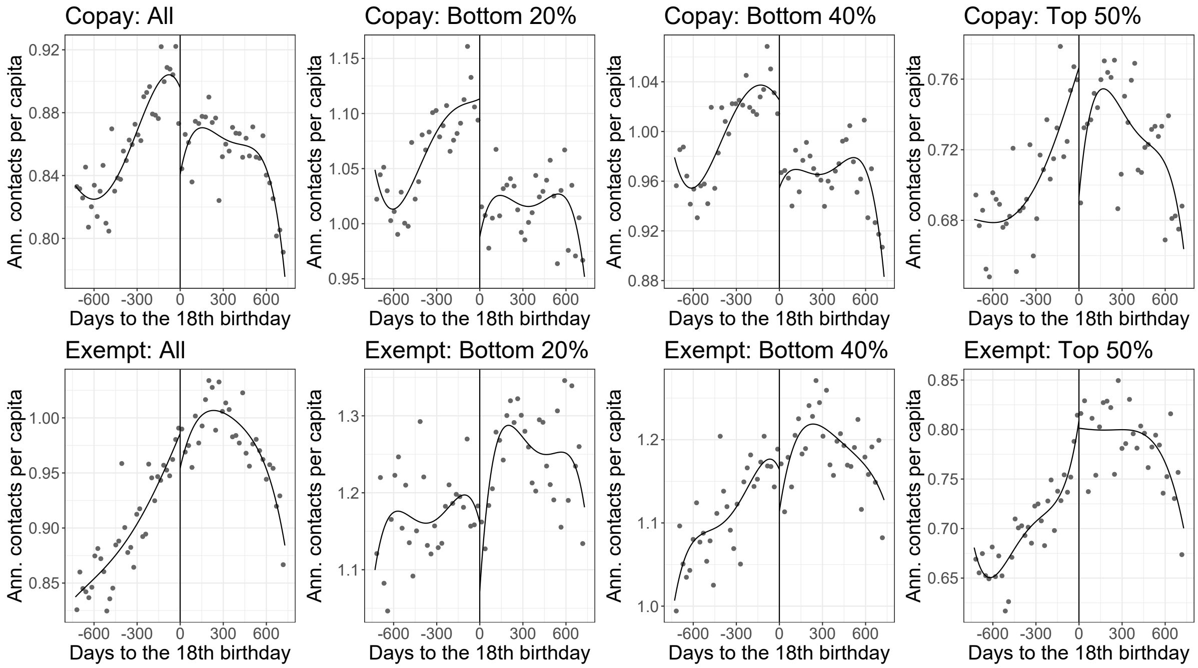

Figure 1: RD Plots.

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

copayment municipalities is 0.02, estimated for the top 50%. In the exemption municipalities, the estimate for all individuals is the most precise estimate and close to zero (-0.5%). It thus appears a reasonable assumption for inference that the confounding discontinuities are zero at the cutoff.

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

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

annualized visits (-4.6%). Interestingly, the estimate for the top 50% is also above average: -0.06 annualized visits (-8.1%).

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

5.2 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.[19] Figure A7 contains the results for the RD setting and Figure A8 for the RD-DID setting. The RD results show that the largest reduction in GP use in the copayment municipalities for each subgroup is induced by the real cutoff. However, in the exemption municipalities the real RD estimates do not stand out from the placebo estimates. In contrast,

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

Table 1: Main Results.

A. 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)
B. 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)
C. 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

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

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

6 Comparison of the Estimates

We find that GP visits decrease by 4–5% for women in the copayment municipalities. These are moderate effects compared to the earlier Nordic studies that estimate the effects of copayments on GP use for adolescents using age-based RD designs. For instance, Johansson 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

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

GP visits was 10–15 euros. For Norway, Magnussen Landsem and Magnussen (2018) report that a copayment of 17–18 euros reduces GP visits by 10% for all individuals.

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

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

A plausible explanation for the small effects is the combination of gatekeeping and waiting times for non-urgent care that in essence works as a rationing device, moderating the effects of out-of-pocket costs. Although waiting times and gatekeeping are by no means unique to Finland, gatekeeping may be stricter and waiting times longer in Finland. Consistent with this, a study examining the abolition of a 14-euro copayment for GP visits in Helsinki, the Finnish capital, reports a semi-arc elasticity of -0.26 for adults based on difference-in-differences methods

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

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

(Haaga et al., 2024). That figure is surprisingly close to our elasticity estimate, although the context is different (intervention, design, and sample). Among the alternative explanations, our shorter baseline bandwidth or our use of the 3-day donut hole are unlikely to explain the smaller magnitude of the effects, because our RD-DID estimates do not increase when using a larger bandwidth (Figure A9) and they are rather robust to not using the 3-day donut hole (Table A1 vs. Table A4).

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

7 Conclusion

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

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

that the intensity of other barriers to access, such as waiting times and gatekeeping, may at least partially explain the findings. Future research from different institutional contexts may help in understanding how institutional features moderate the effects of copayments.

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

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

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

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

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A Online Appendix

A.1 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 in 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 A5, and the local randomization RD results are in Table A6. 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 municipalities with a discontinuity in copayments. The corresponding estimates in our comparison municipalities are close to zero (-0.3% to -1.0%).

The reduction in GP use in the copayment municipalities 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 municipalities are -0.01 to -0.03 visits (-1.2% to -2.9%). The estimates attenuate for the bottom 40% in the copayment municipalities: 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 municipalities and between $+0.01$ and $+0.02$ ($+1.2\%$ to $+2.2\%$) in the exemption municipalities.

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

Results: additional robustness checks. For the local polynomial methods, we illustrate sensitivity to the bandwidth choice (Figure A12), specification (Figure A13), and to not using the 3-day donut hole (Table A7). 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 municipalities and -0.03 annualized visits (-3.1%) in the exemption municipalities.

Figure A14 shows that the local randomization RD estimates are relatively stable when using a bandwidth of 25–45 days, but with 15–20 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 A15 shows the RD plots. The local polynomial results are in Table A8, the RDHonest results in Table A9, and the local randomization results in Table A10. The results are in line with the main findings. The estimates for all individuals in the copayment municipalities 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 municipalities. In absolute terms, the reductions in the copayment municipalities 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 municipalities vary between $+0.01$ and $+0.04$ visits ($+0.6\%$ to $+4.0\%$). Figure A16 shows the sensitivity of the estimates to the bandwidth choice.

Next, we focus on Alternative Sample 2. Figure A17 shows the RD plots. The local polynomial results are in Table A11, the RDHonest results in Table A12, and the local randomization results in Table A13. 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 municipalities, meaning that the estimated reductions attenuate in the copayment municipalities. The local polynomial results of Table A11 remain negative in the copayment municipalities. 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 municipalities. However, the finding in the local randomization RD-DID analysis of GP visits decreasing more in the copayment municipalities at the 18th birthday is now driven more by a positive point estimate in the exemption municipalities than a negative point estimate in the copayment municipalities. Although this observation is surprising, we are inclined to put more weight to the RD-DID estimates than to the individual RD estimates. Figure A18 shows the sensitivity of the estimates to the bandwidth choice.

Results: additional outcomes. Figure A19 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.2 RD-DID Robustness Checks and Supplementary Analyses

RD-DID: alternative samples. Table A1 shows the sensitivity of the main RD-DID estimates to the 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 year's end when aged 17 and year's end when aged 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 A9 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 (in all but one case).

RD-DID: local randomization approach. We also use the local randomization approach (Cattaneo et al., 2018) in the RD-DID framework. Specifically, we estimate the difference in discontinuities between the copayment municipalities and the exemption municipalities in a narrow 30-day bandwidth. We only include individuals who are observed throughout the whole window with no changes in copayment status. 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 approach 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. Robust standard errors are reported.

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 estimates for income-based subgroups are noisier, but negative in each case. Figure A10 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 municipalities to Helsinki. Helsinki is the only municipality that does not charge the GP visit copayment (since 2013) from anyone. This policy can represent a clearer absence of any policy discontinuity to adolescents than the exemption for students in the exemption municipalities. 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 A11.

A.3 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. Our baseline sample includes approximately 86% of all the individuals who turned 18 during 2011–2019, which we think is a good coverage. 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 year's end and has positive equivalized family 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 year's end. We then restrict to those 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 year's end when being 16 (17) years old (approximately 5% of sample living in the eligible municipalities).

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 year's end. 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 year's end.

The corresponding population sizes in age cells are shown in Figure A4.

Data quality issues. We exclude some person-year observations due to quality issues in the primary care data: there are periods for some municipalities 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 A20. 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.

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.

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.4 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 for 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.5 Additional Figures and Tables

Table A1: RD-DID Results.

A. Main Sample (also in Table 1).	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. 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 municipalities to the exemption municipalities and Helsinki. Bottom 20% and 40% and top 50% are based on the distribution of equivalized family disposable income, measured for the year when the individual was 17 years old at year's end. Level is the fitted mean for the copayment municipalities 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 year's end when aged 17 and year's end when aged 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 family disposable income, measured for the year when the individual was 17 years old at year's end. Level is the fitted mean for the copayment municipalities 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 year's end when aged 17 and year's end when aged 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 family disposable income, measured for the year when the individual was 17 years old at year's end. Level is the fitted mean for the copayment municipalities 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 year's end when aged 17 and year's end when aged 18.

Table A4: RD-DID Results without the Donut Hole.

A. Main Sample (also in Table 1).				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.879	1.059	0.985	0.767
RD-DID estimate	−0.049	−0.083	−0.045	−0.073
Change, %	−5.628	−7.822	−4.585	−9.557
Std. error	0.032	0.059	0.045	0.042
P-value	0.117	0.162	0.316	0.083
Individuals	93,008	24,989	44,333	39,411
B. Alternative Sample 1.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.824	1.016	0.937	0.719
RD-DID estimate	−0.035	−0.080	−0.033	−0.047
Change, %	−4.253	−7.885	−3.542	−6.554
Std. error	0.031	0.063	0.046	0.042
P-value	0.260	0.203	0.470	0.264
Individuals	78,157	18,647	35,044	35,225
C. Alternative Sample 2.				
	All	Bottom 20%	Bottom 40%	Top 50%
Level	0.792	0.965	0.911	0.709
RD-DID estimate	−0.049	−0.096	−0.058	−0.075
Change, %	−6.162	−9.965	−6.319	−10.638
Std. error	0.032	0.070	0.048	0.042
P-value	0.132	0.168	0.233	0.073
Individuals	76,937	17,716	32,950	36,230

Notes: The estimates are based on Model 1, a 180-day bandwidth and the uniform kernel. Here, we use all the data unlike in main analysis in which we use a 3-day donut hole. 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 the exemption municipalities and Helsinki. Bottom 20% and 40% and top 50% are based on the distribution of equivalized family disposable income, measured for the year when the individual was 17 years old at year's end. Level is the fitted mean for the copayment municipalities 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 year's end when aged 17 and year's end when aged 18.

Table A5: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A6: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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 A7: 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): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A8: 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): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A9: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A10: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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 A11: 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): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A12: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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. The CIs are based on the 95% confidence level.

Table A13: 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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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.

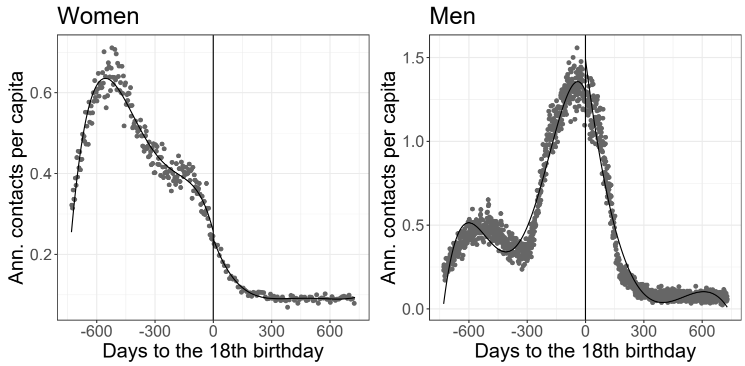

Figure A1: Conscript Health Checks for Men Invalidate Identification.

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).

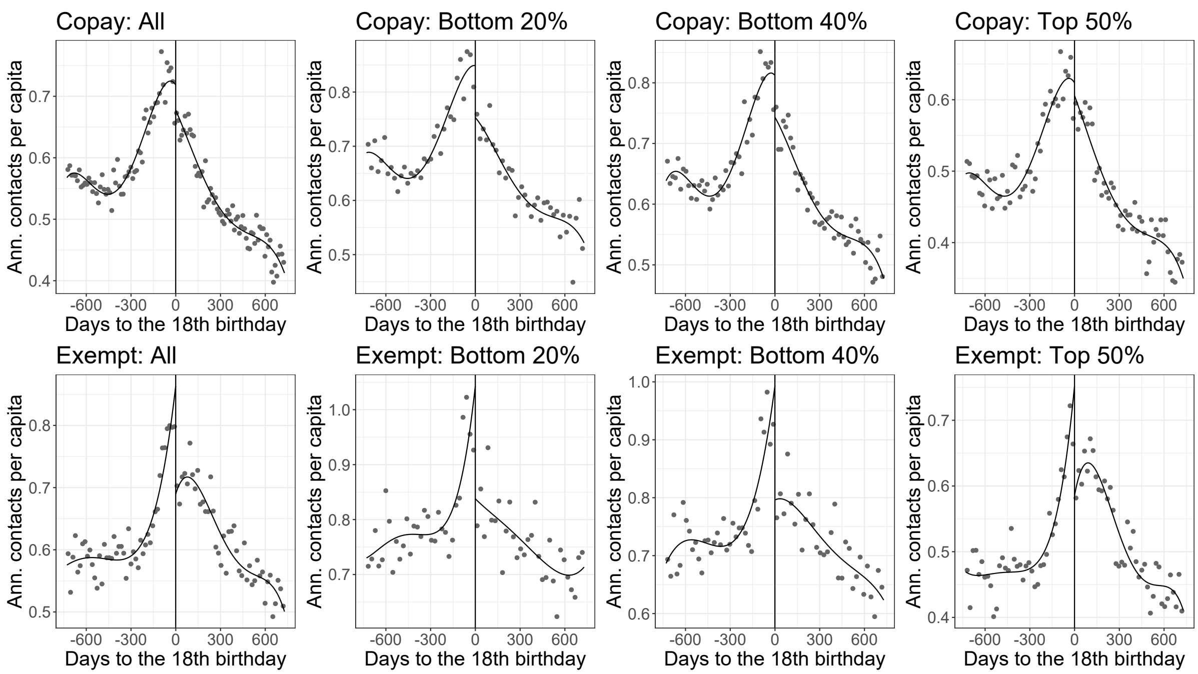

Figure A2: RD Plots for Men: GP Visits.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The number of evenly spaced bins is selected in a data-driven way to mimic the underlying variability of the raw data while still smoothing the scatterplot of the data (Calonico et al., 2015).

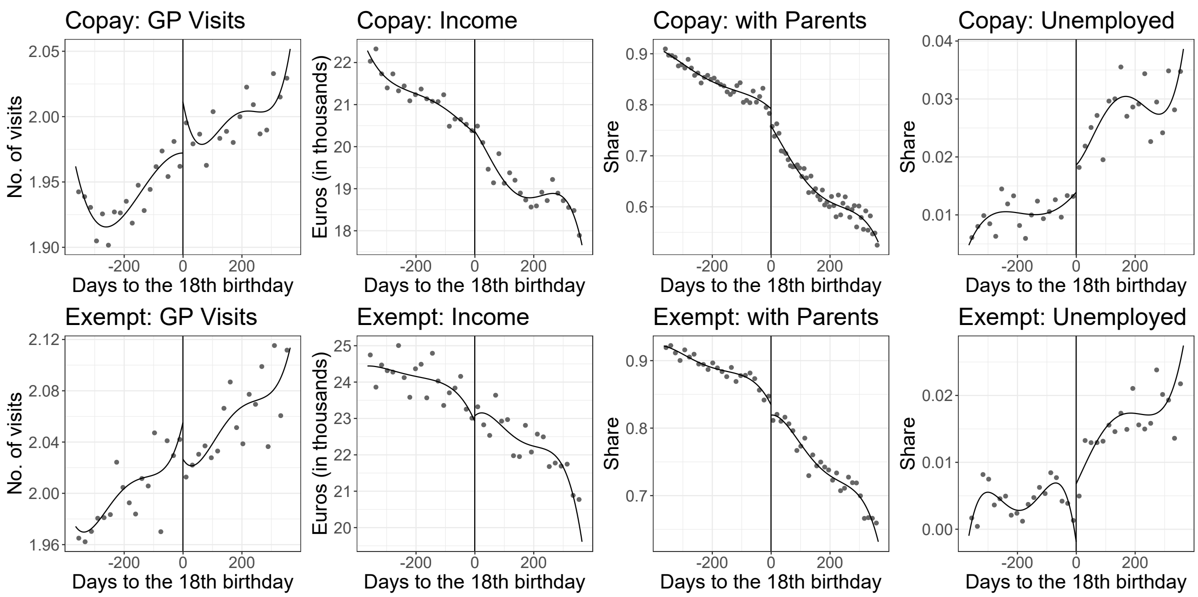

Figure A3: Does Moving away from Parents Threaten Identification?

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 family 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).

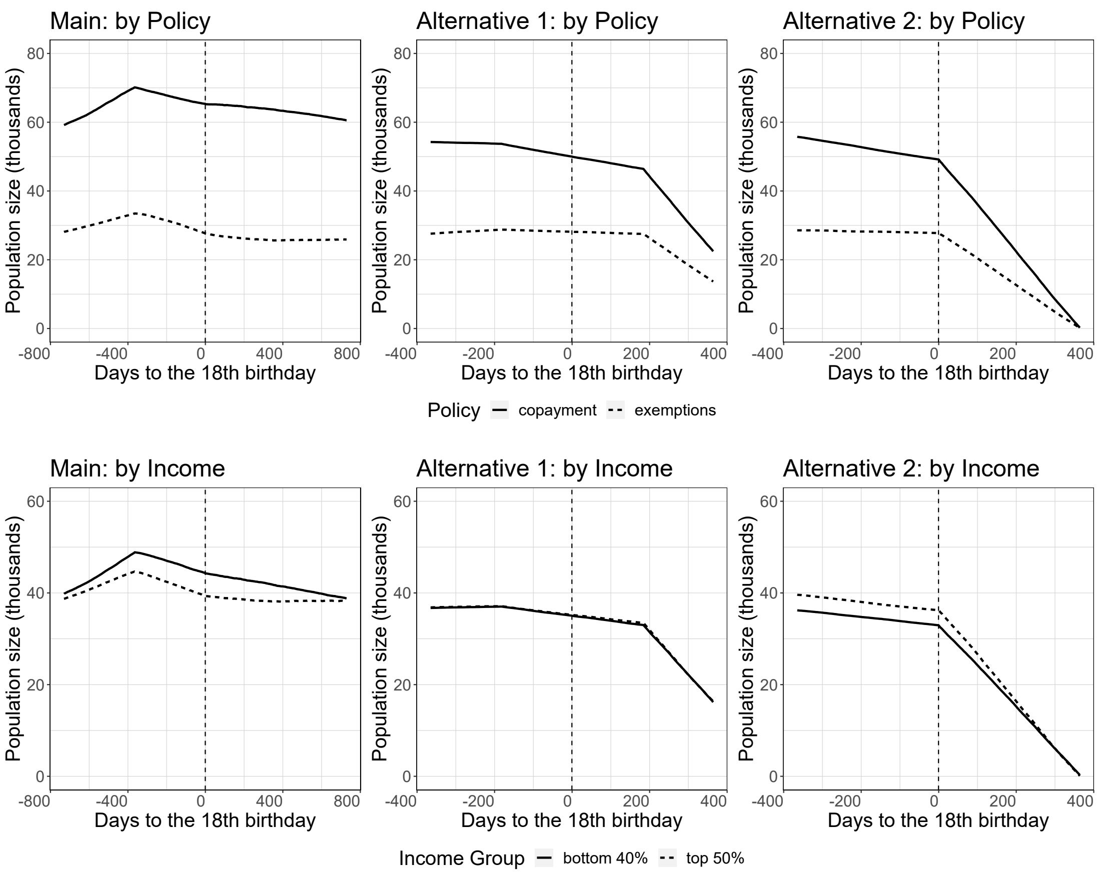

Figure A4: Age Cell Population Sizes.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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 year's end when aged 17 and year's end when aged 18.

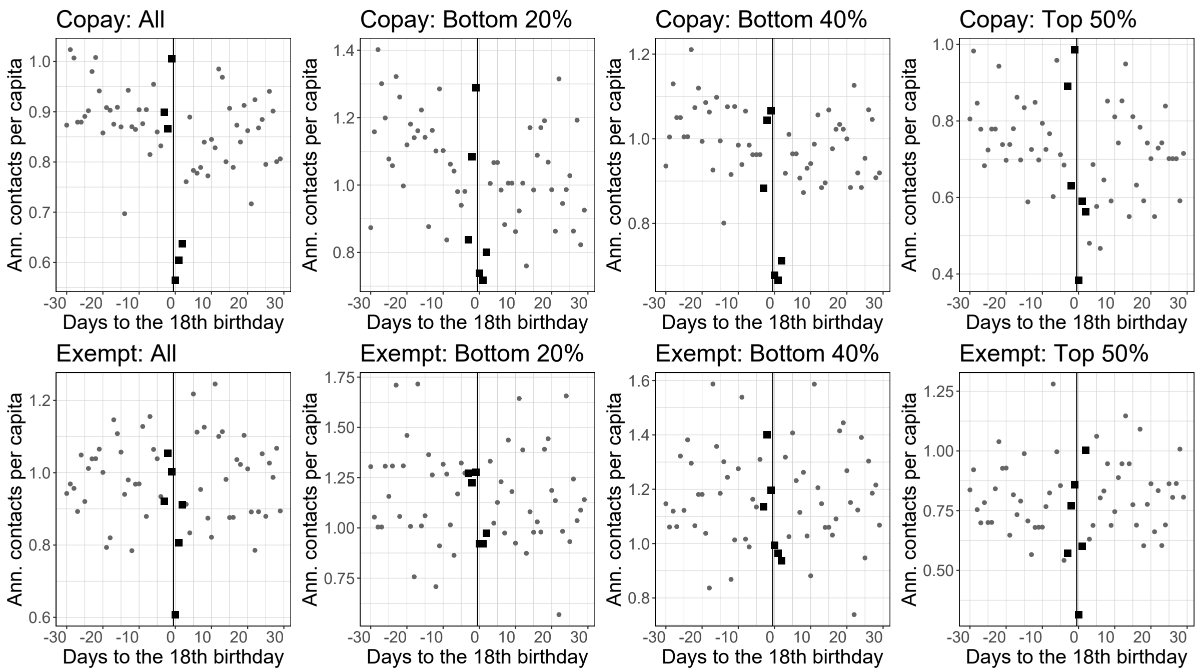

Figure A5: Age Cell Means of GP Use.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. The sample only includes women. Dots and squares both present the daily means. Squares indicate observations within the 3-day donut whole that are excluded from the analysis.

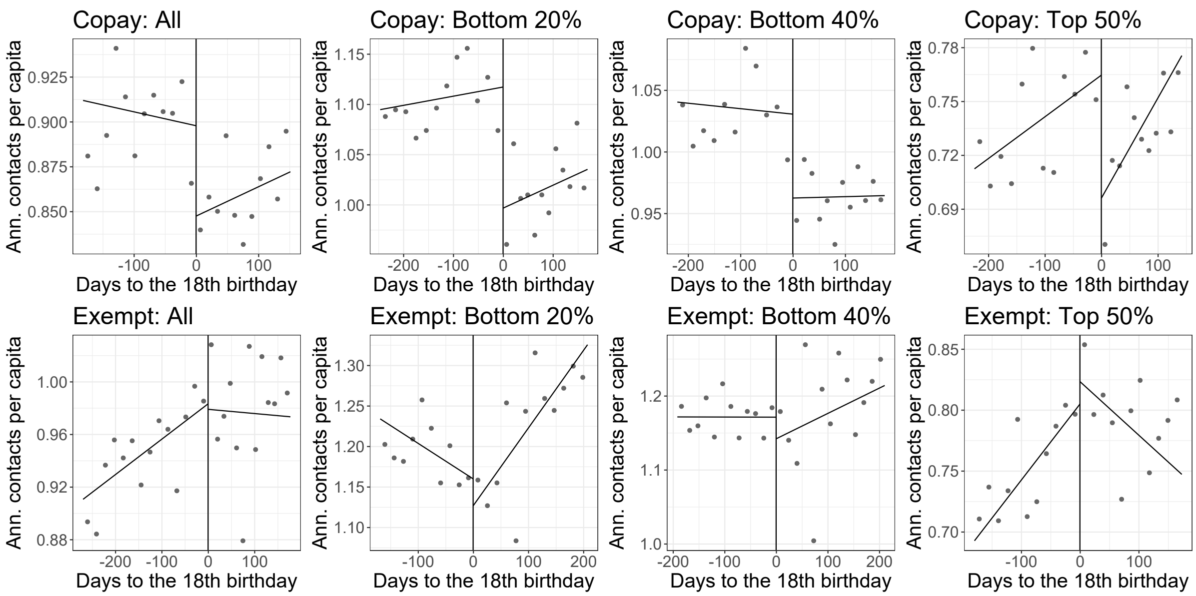

Figure A6: Main RD Results.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. Table 1 presents the corresponding point estimates, confidence intervals, and sample sizes.

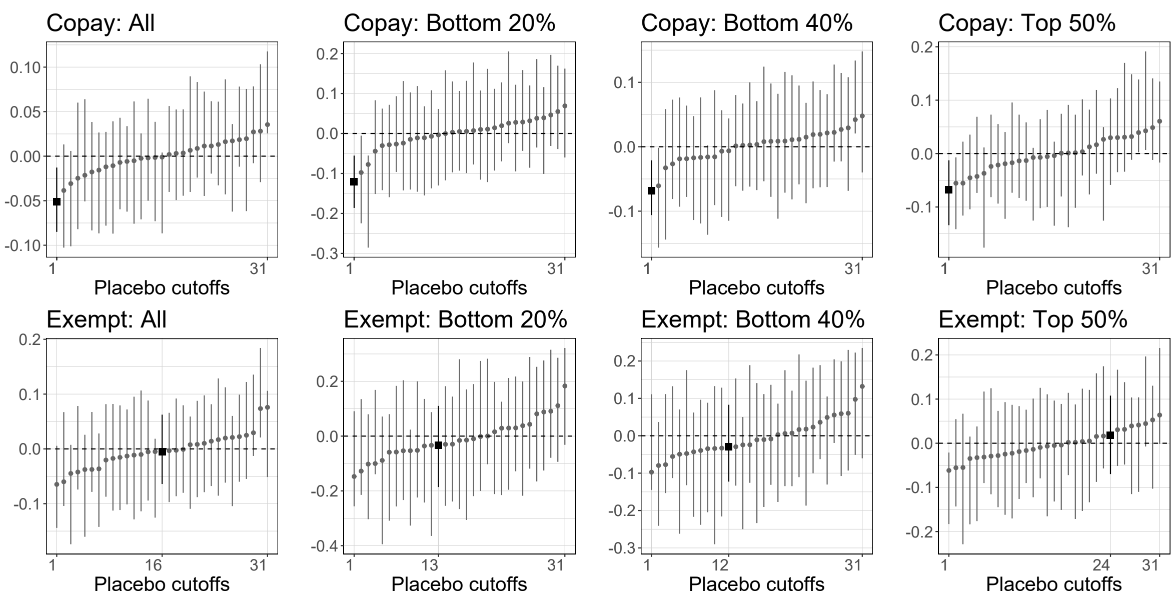

Figure A7: RD Results, Placebo Cutoffs.

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 use robust bias corrected inference (Calonico et al., 2014): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. The CIs are based on the 95% confidence level.

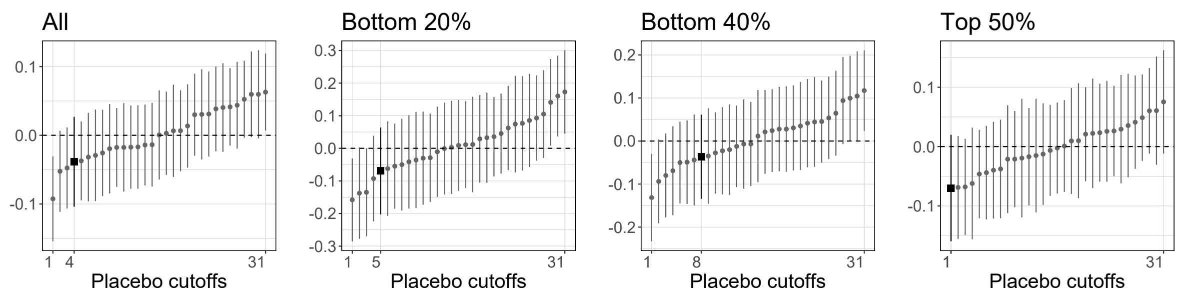

Figure A8: RD-DID Results, Placebo Cutoffs.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. The CIs are based on the 95% confidence level.

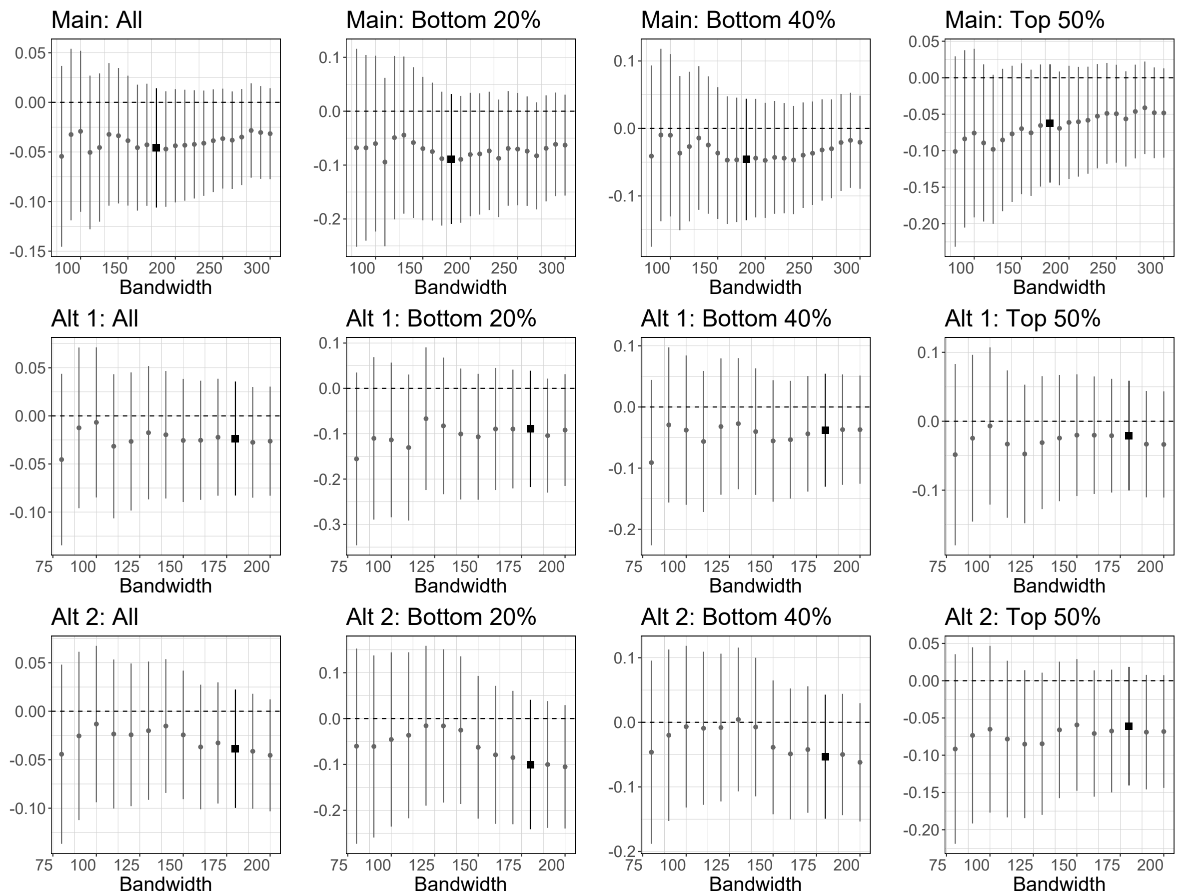

Figure A9: RD-DID Results, Sensitivity to Bandwidth Size.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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 year's end when aged 17 and year's end when aged 18. The CIs are based on the 95% confidence level.

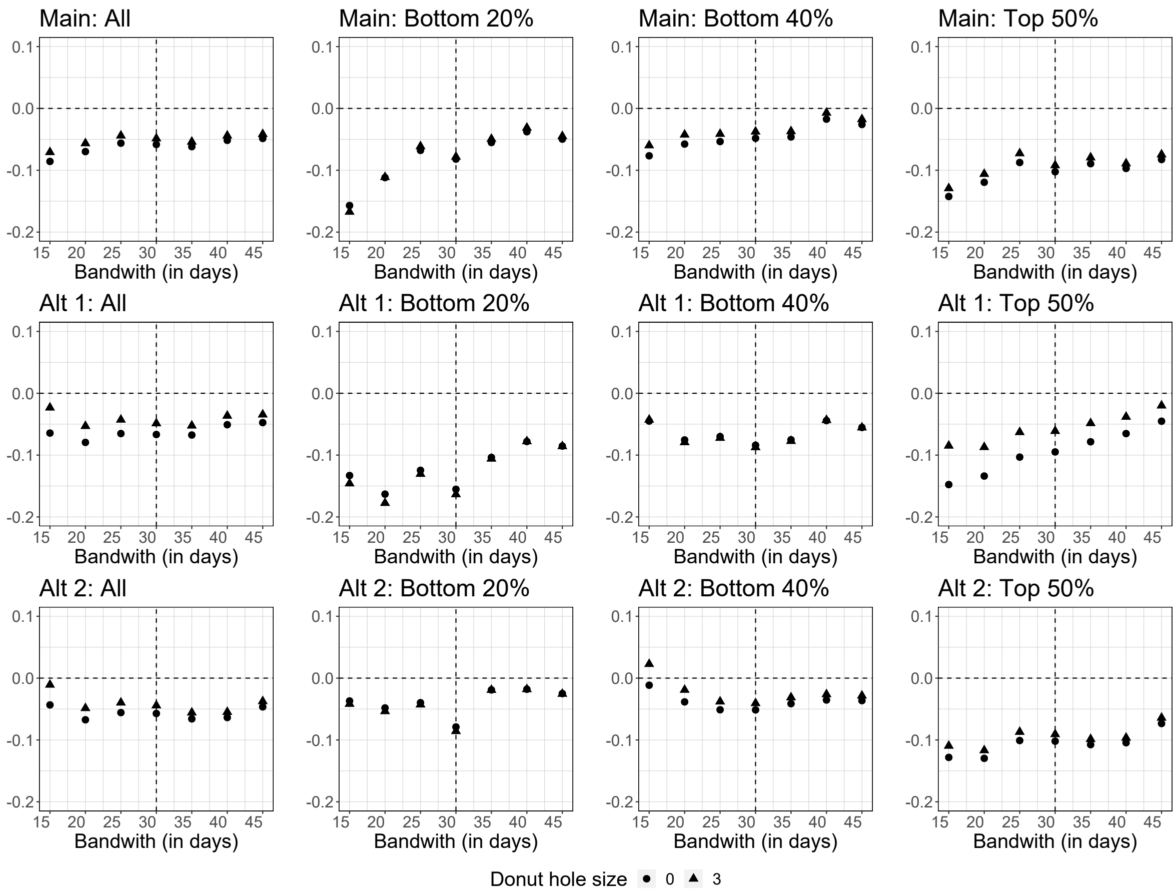

Figure A10: Local Randomization RD-DID Results, Sensitivity to Bandwidth and Donut Hole.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. 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 year's end when aged 17 and year's end when aged 18. The sample only includes women.

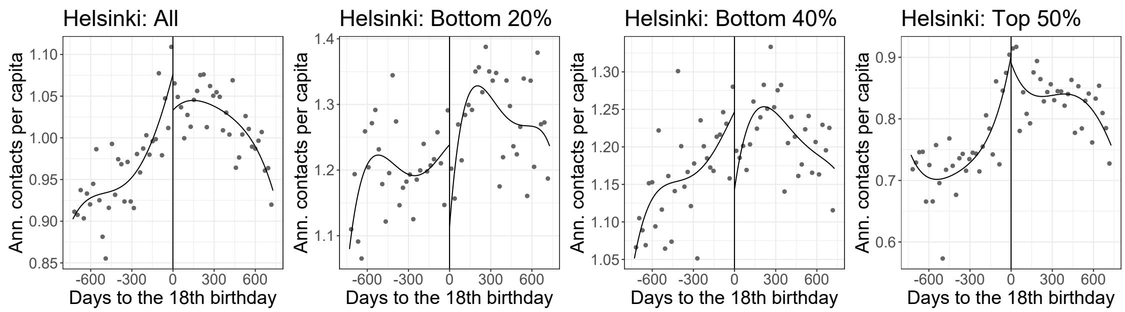

Figure A11: RD Plots in Helsinki.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The 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).

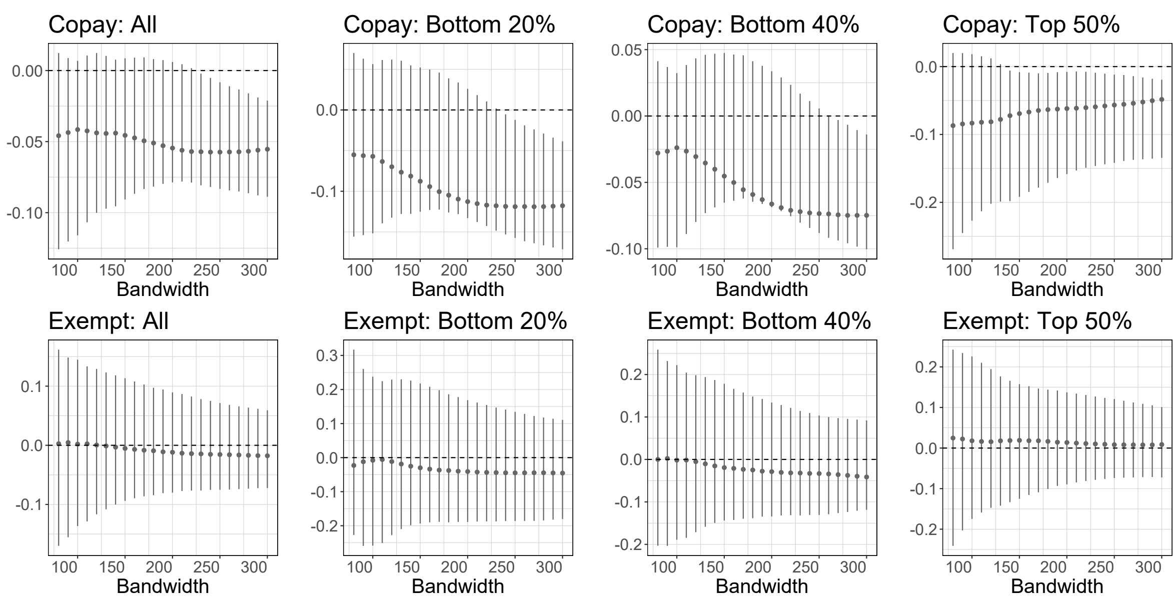

Figure A12: RD Results, Sensitivity to Bandwidth Size.

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 use robust bias corrected inference (Calonico et al., 2014): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. The CIs are based on the 95% confidence level.

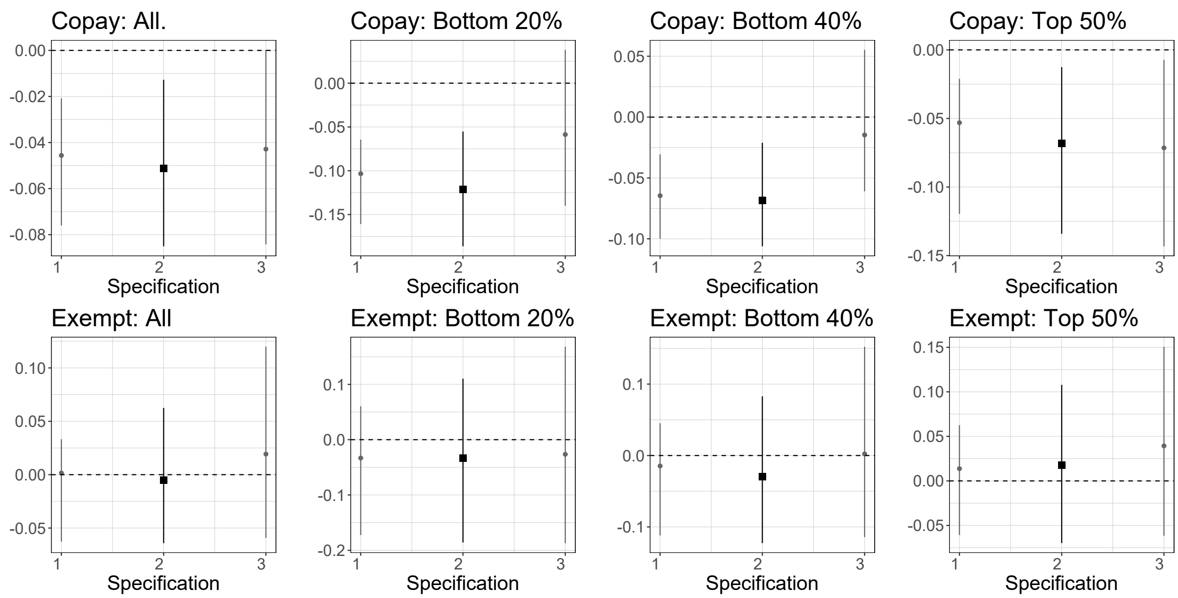

Figure A13: RD Results, Sensitivity to Specification.

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): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. Base specification (local linear) is shown in black. The CIs are based on the 95% confidence level.

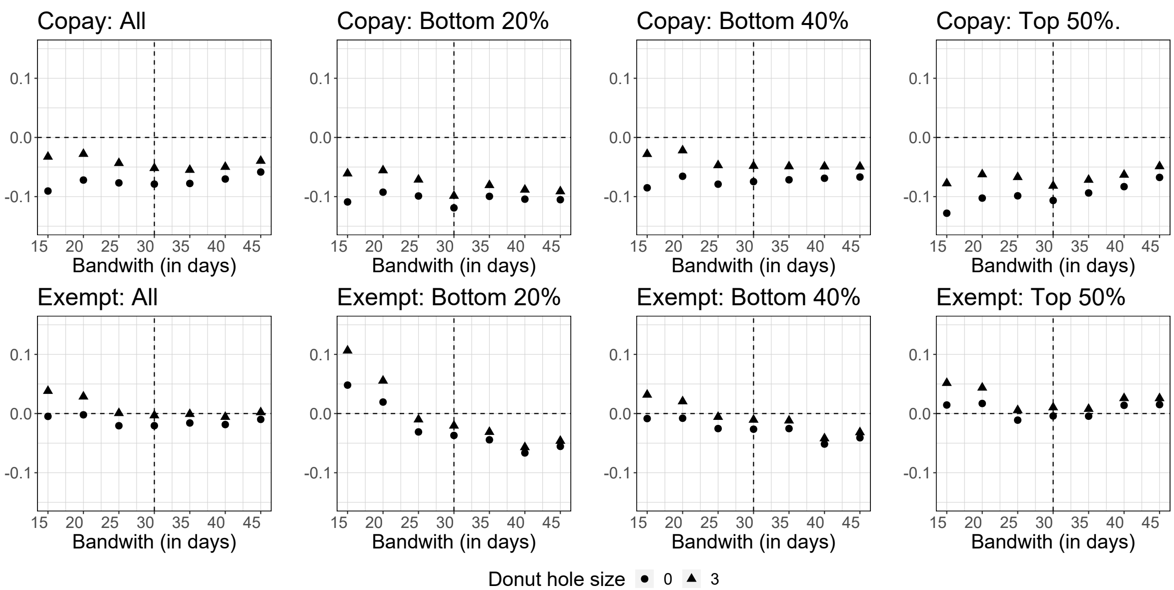

Figure A14: Local Randomization RD Results, Sensitivity to Bandwidth and Donut Hole.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. The sample only includes women.

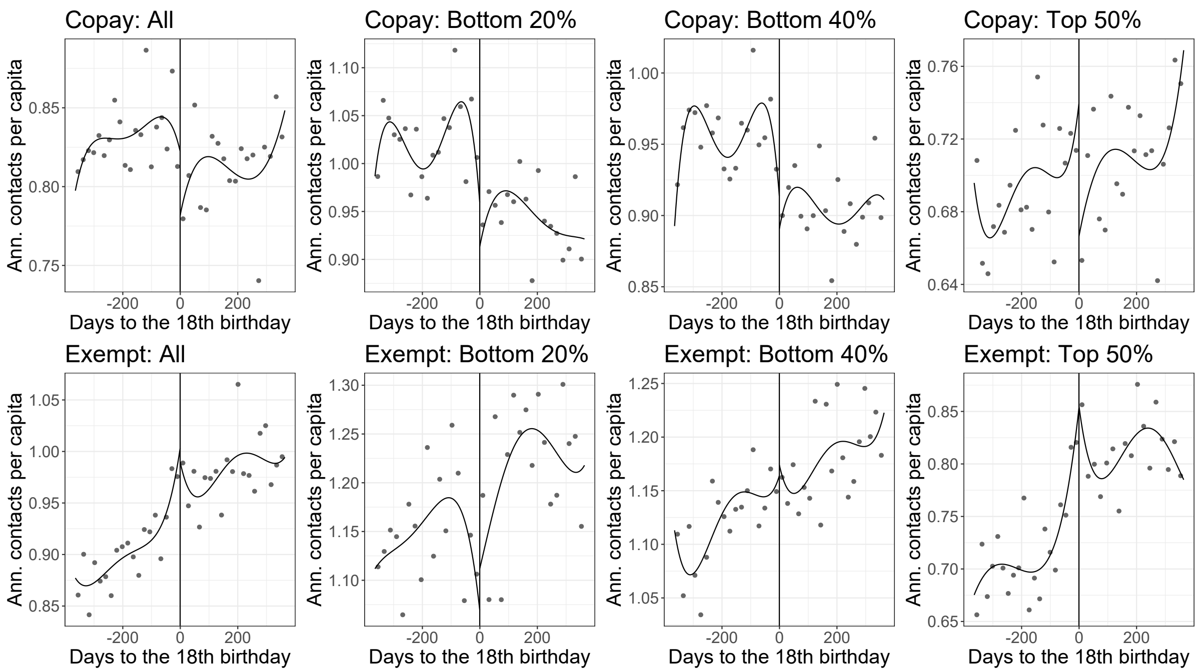

Figure A15: Alternative Sample 1: RD Plots.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The 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).

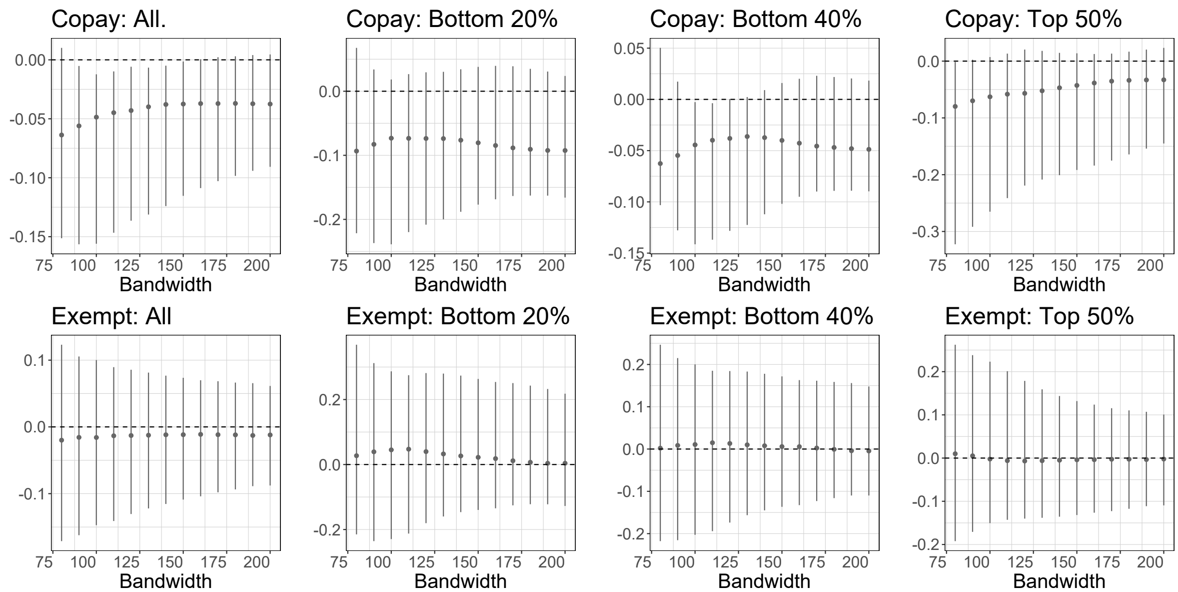

Figure A16: Alternative Sample 1: RD Results, Sensitivity to Bandwidth Size.

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 use robust bias corrected inference (Calonico et al., 2014): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. The CIs are based on the 95% confidence level.

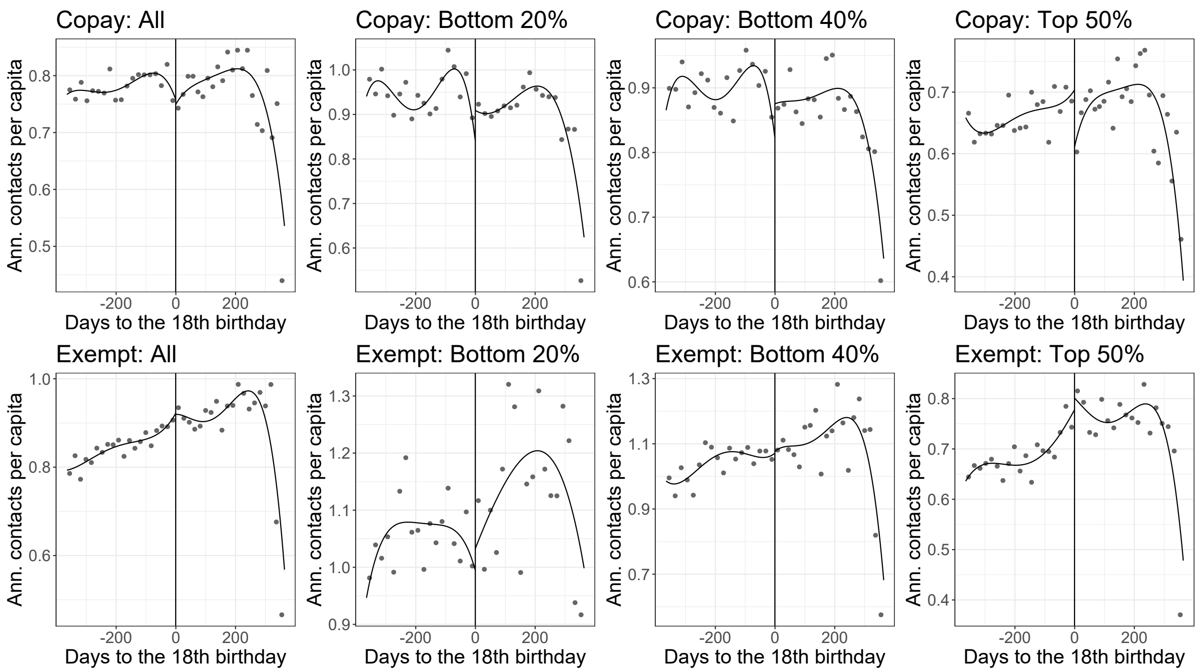

Figure A17: Alternative Sample 2: RD Plots.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The 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).

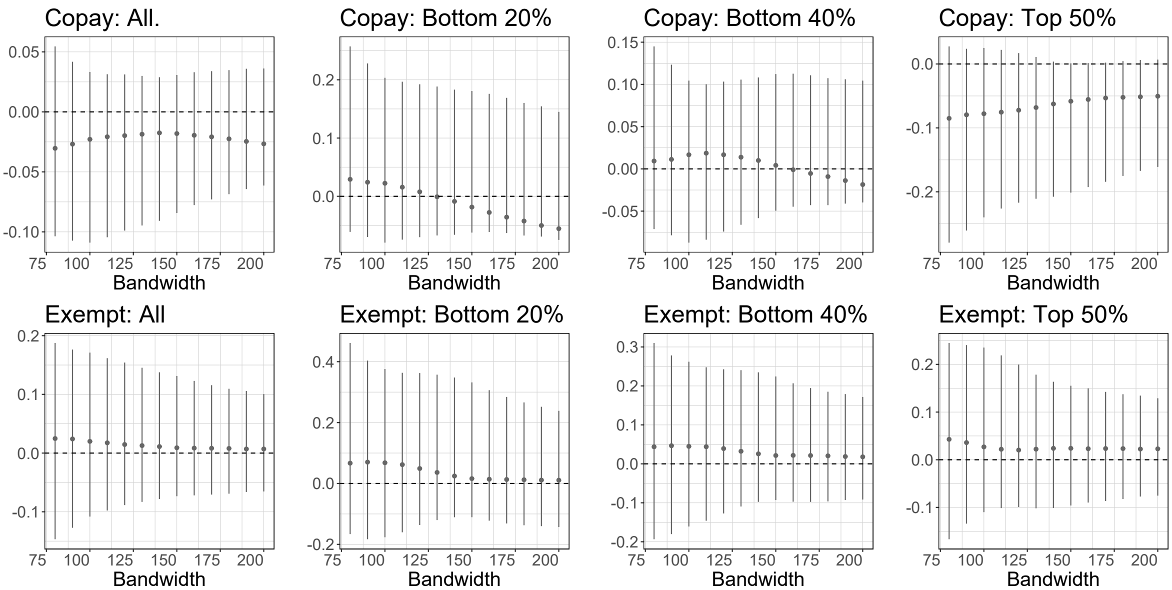

Figure A18: Alternative Sample 2: RD Results, Sensitivity to Bandwidth Size.

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 use robust bias corrected inference (Calonico et al., 2014): CIs may be rescaled and recentered. 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 family disposable income, measured for the year when the individual was 17 years old at year's end. The CIs are based on the 95% confidence level.

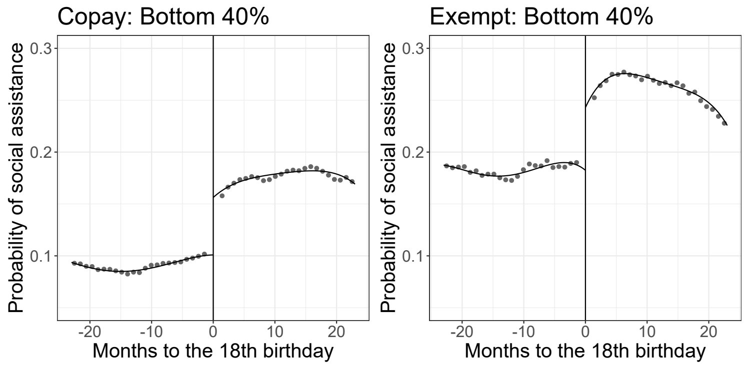

Figure A19: RD Plots: Social Assistance Use.

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 family disposable income, measured for the year when the individual was 17 years old at year's end. We use age cell population sizes as weights, the uniform kernel, and fourth-order polynomial approximations. The 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).

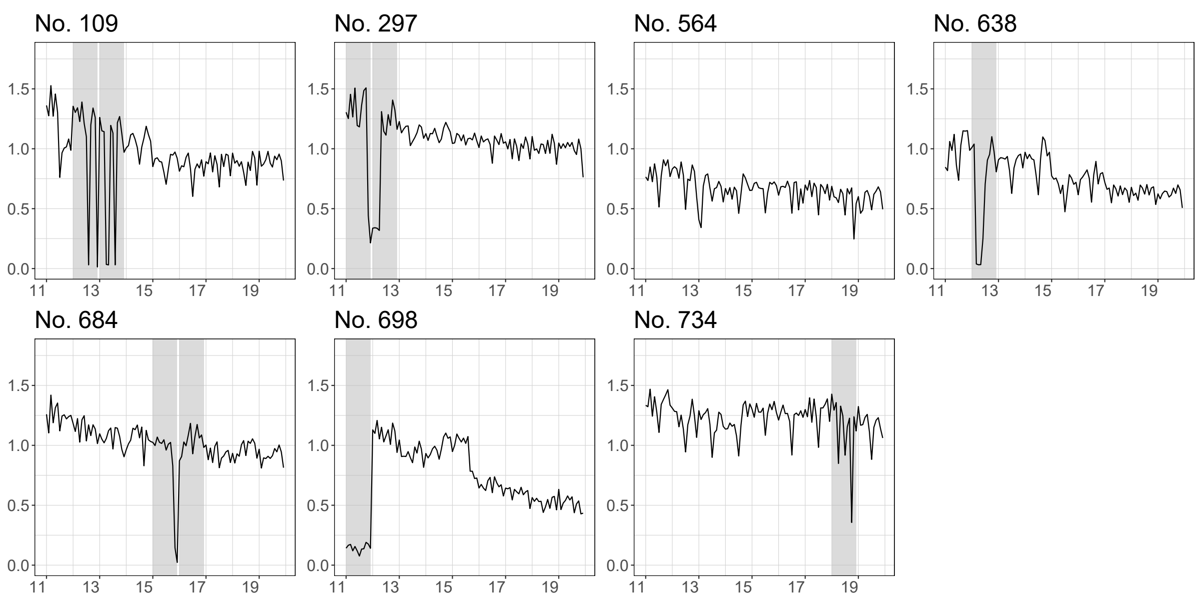

Figure A20: Visit Data Quality.

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).