Abolishing a GP Visit Copayment: Did It Affect GP Use?

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Abstract

We analyze whether abolishing a copayment of 14 euros for visits to primary care general practitioners (GP) in Helsinki, the capital of Finland, increased the number of GP visits for adults, especially among low-income individuals. Using a difference-in-differences (DD) design and combining several administrative registers from 2011 to 2014, we find that the abolition is associated with only a small increase in GP visits (+0.04 visits annually, or +4.4%, for the whole sample). The increase is driven by low-income adults (+0.06 visits, or +4.5%, at the bottom 40%). Given only one treated cluster and a finite number of comparison clusters, statistical significance of the estimates is inconclusive. Although the point estimates are rather robustly positive, the significance conclusions are sensitive to how we account for clustering.

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

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

Contents

1 Introduction

A large literature has found that out-of-pocket costs reduce the use of health care (Einav and Finkelstein, 2018). The evidence is based on randomized and quasi-experimental research designs that use different cost-sharing schemes, such as coinsurance rates, deductibles, copayments, and two-tier systems. Small or moderate copayments have potentially useful properties as a policy instrument. For patients, these prices are transparent and easy to understand before consumption. At the same time, their low level does not constitute a large financial risk to patients. Consequently, copayments are widely utilized in public healthcare systems in the Nordic countries. Denmark is currently the only Nordic country that does not charge a copayment for primary care GP visits.[1]

Lower copayments reduce financial risks faced by patients, but they also generate less revenue to the providers of care. The question is whether low copayments can still noticeably affect healthcare use. The effects of copayments on health care use may be diminished if there are other costs to the patient (e.g., waiting times) or if access to GPs is triaged by nurses. If small copayments nevertheless have observable utilization effects, it is of interest whether beneficial or low-value care or both are affected. Potential utilization effects are arguably disproportionately larger among low-income individuals, who are on average sicker and need more services. In such a case, a copayment-based cost-sharing scheme can have unintentional and undesirable effects on inequality.

Helsinki, the capital of Finland and the most populous municipality, abolished its general practitioner (GP) visit copayment of 14 euros in January 2013. The reason for this policy change was to reduce health inequality. Using a difference-in-differences (DD) design and the fact that other municipalities continued to charge GP visit copayments, we examine the effects of the abolition on public primary care GP use (our primary outcome), emergency department use (ED; potential offset effects), specialist consultations proxying a need for diagnosis, and social assistance use (a last-resort benefit that can be applied to

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[1]In Sweden, some regions do not charge the GP visit copayment.

cover healthcare costs). Our estimates are based on administrative register data from 2011 to 2014. The analysis focuses on whether low-income individuals are especially sensitive to cost sharing.

We find that the abolition is associated with a small increase in GP visits in Helsinki (+0.04 visits annually, or +4.4%, for the whole sample) after subtracting an increasing linear pre-trend difference. The overall estimates are driven by low-income individuals. The results show an increase of +0.06 visits (4.5%) at the bottom 40% of the income distribution and +0.02 visits (3.3%) at the top 40%. The effect size is larger in absolute terms for low-income groups, but such heterogeneity is not apparent in relative terms. Overall, the magnitude of the estimates is modest or small compared to monthly noise in the outcome. The effect sizes increase (decrease) if the pre-trend difference is assumed to slow down (accelerate) in the post-treatment periods. Consistent with the small effects for our primary outcome, we do not detect changes in our secondary outcomes.[2]

Although the point estimates are rather robustly positive, the significance conclusions are sensitive to how we account for clustering. For instance, the p-value for the estimate for all individuals (+0.04 visits annually or +4.4%) is 0.00, 0.01, 0.07, or 0.22 depending on the method. Inference is complicated due to the availability of only one treated cluster and a finite number of comparison clusters. Statistical significance of the estimates is consequently inconclusive.

Several studies from the Nordic countries have examined the effects of copayments on service use in primary care. In comparison, our estimates appear to be relatively modest or even small. We contribute to the literature by estimating the effects of a GP visit copayment abolition on GP use in the entire adult population (and not just at an age cut-off or in a narrow subgroup, such as adolescents). The distinction between an adoption and an abolition is relevant, as the effects of increased and decreased out-of-pocket costs may not be symmetric

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[2]Note, however, that we observe a reduction in dentist visits, which we interpret as a plausible placebo outcome. The reduction in dentist visits was unexpected and is of similar magnitude to the increase in GP visits.

(Hayen et al., 2021; Iizuka and Shigeoka, 2021; Remmerswaal et al., 2019). In addition, we contribute to the literature by examining whether low-income individuals are more sensitive to changes in cost sharing.

Three studies from Sweden and Norway employ a regression discontinuity (RD) design using the fact that adolescents under a given age are exempted from GP visit copayments of 10 to 18 euros (Johansson et al., 2019; Magnussen Landsem and Magnussen, 2018; Nilsson and Paul, 2018). In these studies, the number of GP visits decreases by 7-12% after the copayment is charged. Although the RD design has its own advantages for identification of the effects, it is worth noting that the RD estimates of these studies are by definition very local and do not generalize to the adult population. Furthermore, the patients decide when to contact the healthcare system and can thus try to benefit from the exemption just below the cutoff of payments. The RD estimates may thus capture not only the price effect, but also an intertemporal substitution effect. The short observation window in RD designs may amplify this problem. Also focusing on the adolescents, Beck Olsen and Melberg (2018) examine the effect of abolishing an 18-euro copayment from individuals aged from 12 to 15 in Norway on their GP use based on the synthetic control method. In essence, they match individuals aged 12 to 15 to other age cohorts based on pre-treatment fit in GP use. The estimates are large: GP use increases by 22% among women and by 14% among men.[3]

Besides the RD designs focusing on adolescents, there are Nordic studies employing DD designs that focus on adults or the entire population. Jakobsson and Svensson (2016b) find that a 33% increase (approximately 5 euros) in the GP visit copayment did not affect GP use in an 8-month follow-up in Sweden. They use a canonical 2x2 DD framework with two time periods and one treated and one comparison region. In another study, Jakobsson and Svensson (2016a) exploit regional and time-varying changes in GP visit copayment levels in Sweden to examine the link between copayment levels and GP use with two-way fixed-effects

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[3]For women, their preferred elastic net procedure gives positive weights to ages 9, 18, and 19. For men, ages 1, 9, 19, 16, and 17 receive positive weight.

(TWFE) regression models.[4] These two studies focus on modest changes in the intensity of the copayment, while we examine a copayment abolition. Recently, Haaga et al. (2022) find that the adoption of a copayment for curative primary care nurse visits of approximately 10 euros reduces curative nurse visits by 9% to 12% during a one-year follow-up in the adult population in Finland. They use a pre-registered staggered DD design.

Regarding the income-related heterogeneity of the results, Nilsson and Paul (2018), Johansson et al. (2019), and Haaga et al. (2022) find that patients at the lower end of the income distribution are more sensitive to copayments than the ones at the top of the income distribution. Our results are consistent with these findings.

2   Institutional Background

Finnish primary care services for the adult population are provided by three sectors. They target different population groups and differ by the level of copayments, waiting times and the strictness of gatekeeping. Public primary care charges small or moderate copayments (approximately 14 euros per GP visit). Waiting times for nonurgent health conditions can be long. There is also gatekeeping at the point of entry (nurses do triage and book appointments to GPs) and in accessing specialists (a referral from a GP is required). In contrast, private clinics offer fast access without gatekeeping and also to specialists, but out-of-pocket costs are many times higher. Curative occupational health care is available to many employees with fast access and without copayments. Usually, a designated healthcare professional must be contacted before booking an appointment. For these reasons, employed people often prefer occupational health care or private clinics to public primary care. Low-income individuals, the unemployed, and pensioners disproportionately rely on public primary care.

Municipalities organize public primary care on their own or in a voluntary

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[4]Although the methods applied in Jakobsson and Svensson (2016a) were viewed as appropriate at the time of dissemination of the results, recent advances in econometrics have shown that such TWFE models can produce misleading results in the presence of staggered timing and treatment effect heterogeneity (Goodman-Bacon, 2021; Sun and Abraham, 2021).

cooperation with other municipalities. Primary care areas may have one or more physical locations called health stations. Residents have a designated health station, determined by their address.[5] To initiate a first contact, patients call or visit their health station where they are triaged by nurses who book appointments to GPs based on need and urgency. The supply of GP appointment slots is relatively fixed in the short term.

Public primary care is financed by state transfers, municipal taxes, copayments, and municipal debt. The state guides copayment policies by setting the upper limits of copayments for most services. Before the abolition in January 2013, Helsinki charged a copayment of 13.80 euros for the first three GP visits annually. Minors, war veterans and war invalids were exempted. Copayments were not charged for preventive services. Our comparison municipalities had a similar per-visit copayment or an annual copayment, the amount of which was twice the per-visit copayment. Copayments continued to be charged in other services, such as dentist visits in public primary care and ED visits and specialist consultations at hospitals.

The presumption of the Helsinki health care services committee (6 March 2012) was that the abolition of the GP visit copayment would reduce health inequality by increasing primary care use at the lower end of the income distribution. In 2011, copayment revenues covered 7% of the operating costs of public primary care in Helsinki. Copayments for primary care appointments were 10% of the total copayment revenue. However, total invoice and collection costs divided by the share of copayment bills for GP visits was estimated to be up to 25-30% of revenue.

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[5]Since 2014, patients have had the right to choose their health station once a year, but these changes were rare.

3   Data

We combine four Finnish national-level administrative registers by using unique person IDs.[6] We extract GP visits and dentist visits in public primary care, emergency department (ED) visits, and specialist consultations in specialized healthcare (hospitals), monthly data on whether the individual received social assistance[7], and annual data on socioeconomic and sociodemographic characteristics of the individuals. Individuals and their visits are linked to policies using the municipality of residence.[8] We also use publicly available data on GP visit copayments in municipalities (Finnish Institute for Health and Welfare) and municipal characteristics (Statistics Finland, Sotkanet, and Social Insurance Institution of Finland).

Our analysis dataset is a person-month panel from 1/2011 to 12/2014.[9] We first exclude small municipalities with less than 30,000 residents in 2012 (36 municipalities remain in the sample).[10] Using the copayment data, we exclude municipalities whose policy is not observed in every period or whose policy changed from a per-visit copayment to an annual copayment (or *vice versa*) between 1/2013 and 12/2014. One municipality (Espoo) is also excluded, because it adopted exemptions for several low-income groups in 8/2011. These restrictions lead to a sample of 28 municipalities. Our sample individuals are those who were 25 years or older at the end of 2011 and who were observed to reside in the same sample municipality throughout 2011-2014 (the municipality of residence observed at the end of year). This leaves us with 380,000 people in Helsinki and 1.35 million people in the

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[6]Public primary care contacts are from the Register of Primary Health Care Visits, and specialized healthcare contacts are from the Care Register for Health Care. Social assistance data come from the Register of Social Assistance. All three registers are administered by the Finnish Institute for Health and Welfare (THL). Socioeconomic data are from Statistics Finland's FOLK modules "basic", "family", and "income".

[7]Social assistance is a last-resort benefit for those with low income and little wealth, which can be applied to pay for copayments of public healthcare.

[8]Not all visits are done in the municipality of residence. Acute care is provided for nonresidents as well. Since 2014, individuals have also had an annual option to actively choose other public primary care provider than the one determined by municipality of residence. However, these changes have been rare.

[9]The primary care data collection started in 2011, and the copayment for GP visits was increased by 10% in 1/2015 in comparison areas.

[10]Helsinki is by far the largest municipality in Finland with 600,000 residents in 2012. The population size restriction excludes small and rural municipalities where primary care demand and supply differ considerably from Helsinki.

27 comparison municipalities.

We have five outcomes in the study: GP visits in public primary care (the primary outcome), ED visits and specialist consultations in specialized healthcare, an indicator of living in a family where someone received social assistance (the secondary outcomes), and dentist visits in public primary care (the placebo outcome). An individual may have had more than one visit in a given service on the same day, but we treat these contacts as one. Regarding GP and dentist visits, we only include visits from Monday to Friday to reduce the share of acute visits outside of normal office hours, which have a different copayment level. Specialist consultations do not include repeated visits to treat the same health problem or outpatient contacts of those recently transferred from inpatient care to outpatient care.

The final sample sizes depend on the outcome. Some municipalities are excluded for data quality reasons. In a difference-in-differences design, missing health care contacts would bias the results if data quality issues are time-variant and correlate with treatment assignment. In our application, two types of changes in data quality are noteworthy. First, the national data collection on primary care contacts started in its current form in 2011. Not all areas transferred high-quality data to the national register at the beginning.[11] For instance, the number of GP visits was suspiciously low in Rovaniemi (698) in 2011 (Figure A1). Second, changes in electronic health record (EHR) systems, such as software updates or provider changes, may have led to drastic but mainly short reductions in observed health care use until transfer problems were fixed.

We detect and exclude municipalities with data quality issues from the analysis using the following algorithm: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, 2) mark an observation as invalid if its value is less than X% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations), and 3) exclude municipalities with abnormal observations. The threshold X varies by outcome and is guided on what

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[11]Either no data were transferred or the coding rate in some key variables used to extract outpatient GP visits was low.

we observe in the trends plots. We use 50% for GP visits (19 comparison municipalities remain), 30% for ED visits (23), 40% for specialist consultations (24), 55% for dentist visits (17), and 40% for the social assistance indicator (27). We show the evolution of the outcome in the sample municipalities and highlight the susceptible municipality-year observations that led us to exclude that municipality in Figure A1 for GP visits, Figure A2 for ED visits, Figure A3 for specialist consultations, Figure A4 for dentist visits, and Figure A5 for social assistance use. Our assessment is that the algorithm works well in detecting outliers. The sample municipalities for the main outcomes are illustrated in Figure A6. The background statistics for Helsinki, the 19 comparisons (for the GP visits outcome), and the remaining municipalities are in Table A1.

4 Methods

We use a difference-in-differences (DD) design, comparing individuals in Helsinki to individuals living in a comparison group of municipalities that continued to charge the GP visit copayment. The key identifying assumption in DD designs is the parallel trends assumption (PTA): the outcomes of individuals in the treated and comparison municipalities would have evolved similarly in the absence of treatment. Figure 1 shows that there was an increasing trend in GP use in Helsinki relative to the comparison municipalities. The same pattern can also be observed separately at the bottom 40% and the top 40% of the income distribution (Figure A7). Helsinki is by far the largest municipality and in many aspects different from the other municipalities (Table A1), so the trend difference is not a complete surprise.

To account for the observed pre-trend differences, we make a modified PTA: we assume that the PTA holds after removing a linear pre-trend difference from the data (detrending). That is, there should be no time-variant confounders correlated with the treatment assignment once a linear pre-trend difference is eliminated. Specifically, we fit
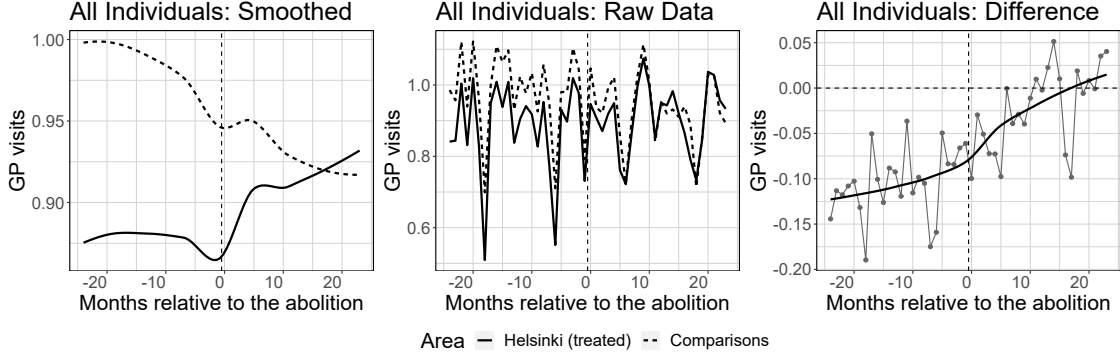
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Figure 1: Trends in GP Visits.

Notes: The outcome is the number of annualized GP visits per capita. We show 1) smoothed conditional means fitted with local linear regression, 2) the raw data, and 3) the difference in outcomes between Helsinki and the comparison areas. The sample is described in Section 3.

with OLS a linear trend difference (an intercept and a time trend) between Helsinki and the comparisons using only pre-treatment data. The estimated trend difference is then subtracted from the outcomes to construct a transformed outcome variable.[12] The key benefit of this approach is that we can examine the sensitivity of the results to the assumption on how the estimated pre-trend difference should be extrapolated to the post-treatment periods. Our baseline choice is to assume that the trend difference does not change in post-treatment periods, but one could instead assume that the trend difference slows down or accelerates by changing the slope of the estimated pre-trend difference for the post-treatment periods.

Figure 2 shows that the difference in GP use between Helsinki and comparison areas varies around zero in pre-treatment periods after removing the estimated linear trend difference.[13] There appears to be a modest increase in GP visits in Helsinki after the copayment abolition relative to the comparison municipalities. This increase is driven by

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[12]The idea of first estimating trend differences with only pre-treatment data and then removing them to create a transformed outcome has been earlier used at least by Bhuller et al. (2013) and Goodman-Bacon (2021). An alternative is to control for a linear pre-trend difference in one regression, as Bilinski and Hatfield (2020) suggest. That requires the inclusion of separate treatment indicators for each post-treatment period in order to fit the trends on pre-treatment data only. Average treatment effect is computed by averaging the time-specific treatment effects.

[13]There is some seasonal variation in GP use which appears to be especially low in Helsinki in June or July relative to comparison areas, plausibly explained by vacations.

the lower end of the income distribution. For estimation and statistical inference, we fit the
following regression model on the detrended data:

$$y_{imt} = \alpha + \beta_1 Post_t + \beta_2 Treat_m \times Post_t + \gamma_m + \varepsilon_{imt}. \tag{1}$$

Here, y is the outcome for individual i in municipality m at time t, α is an intercept, $Post$ is
an indicator for post-abolition periods, $Treat$ is an indicator for Helsinki (the treated area),
γ_m denote municipality fixed effects, and β_2 is the coefficient of interest.
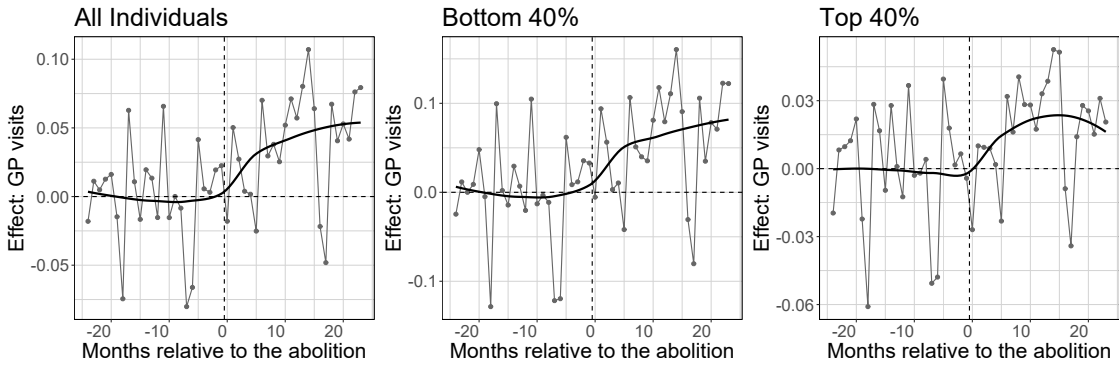

Figure 2: Trends in GP Visits after Removing a Linear Pre-Trend Difference.

Notes: We show the difference in outcomes between Helsinki and the comparison areas after subtracting a
linear pre-trend difference from the outcomes, estimated with OLS using only pre-abolition data. The plot
shows the raw difference and its smoothed conditional mean, fitted with local linear regression. We use the
distribution of equivalized family disposable income to extract the bottom 40% and the top 40%.

The setting is challenging inference-wise - see Roth et al. (2022) for a discussion
on recent econometrics studies on inference with few clusters in the DD context. The policy
is set at the municipal level, but we have only one treated cluster and a finite number
of smaller comparison clusters. Hagemann (2020) provide a rearrangement procedure to
conduct inference with one treated cluster and a finite number of control clusters. The
approach requires no cluster-specific heterogeneity in trends in untreated potential outcomes
so that any single untreated cluster could be used as a counterfactual. Unfortunately, this
assumption is implausible in our application even after removing a linear pre-trend difference
from each sample municipality (Table A2). Alternatively, we can increase the number of

clusters by using postal code areas. However, assuming that there are no correlations between postal code areas within municipalities is unrealistic. Furthermore, the postal code area is missing for 34% of the population in Helsinki and for 22% in comparison areas.[14] With no single right choice available, we use several methods for inference. We cluster using analytical formulas by 1) postal code area and 2) municipality. We also provide confidence intervals based on the 3) unrestricted (WCU) and 4) restricted (WCR) versions of the wild cluster bootstrap (Roodman et al., 2019), clustering at the municipality level. For the main results, we also show IID and robust (HC1) standard errors after ignoring the time series information by aggregating the data at the municipality-by-post-treatment-indicator level (Bertrand et al., 2004).[15]

We also estimate a triple differences (DDD) model on the detrended data and exploit municipal policy, time, and income information.[16] Our hypothesis is that the low-income individuals respond more strongly to the copayment abolition. If high-income individuals are virtually unaffected by the 14-euro copayment, DDD estimation is more robust than DD estimation as we control for both 1) changes in GP use that are same across municipalities among the more affected groups (e.g., an increase in short-term cyclical unemployment making the pool of unemployed healthier on average) and 2) changes in the GP use for all people living in Helsinki (e.g., changes in the supply of GP appointments). The model also allows us to directly test whether the estimated effect was larger at the lower end of the income distribution compared to the top end.

Specifically, we first estimate the linear pre-trend differences separately for the bottom 40% and the top 40% of the income distribution and subtract the estimated trends from the outcome data.[17] The identification assumption is a PTA (after detrending) but in

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[14]The postal code area is obtained by 1) reading all public primary care contacts between 2011 and 2014, 2) including only those person-by-postal-code rows which can be linked to the real 2015 postal codes, and 3) excluding those person-by-postal-code rows whose the person has multiple observed postal codes (18% of the individuals). For individuals with missing postal code, we use the municipality of residence as the cluster.

[15]Note that neither of these methods accounts for the uncertainty induced by estimating the linear pre-trend difference between Helsinki and the comparisons, which is removed from the data (detrending).

[16]Results obtained without detrending are shown as a comparison.

[17]Our *ad hoc* choice is to use these subgroups in statistical testing, but other definitions for the subgroups

ratios: the relative outcomes of the two income groups in Helsinki would follow the relative outcomes of the same groups in control municipalities in the absence of treatment (Olden and Møen, 2022). We estimate the following DD regression specification:

$$
\begin{aligned}
y_{igmt} = \alpha &+ \beta_1 Helsinki_m + \beta_2 Affected_g + \beta_3 Post_t + \beta_4 Helsinki_m \times Affected_g \\
&+ \beta_5 Helsinki_m \times Post_t + \beta_6 Affected_g \times Post_t \\
&+ \gamma Helsinki_m \times Affected_g \times Post_t + \varepsilon_{mgt}.
\end{aligned}
\tag{2}
$$

Here, y is the outcome for individual i in income group g in municipality m at time t, α is an intercept, $Post$ is an indicator for post-abolition periods, $Helsinki$ is an indicator for Helsinki (the treated area), $Affected$ is an indicator for the low-income group that is supposedly more affected by the copayment, and γ is the coefficient of interest.

To complement the main analyses, we exploit synthetic controls (SC) by Abadie et al. (2010). We use all pre-treatment outcome values as matching variables following the recommendation of Ferman et al. (2020). The method computes weights to construct a weighted average (synthetic control) of the available comparison units (donor pool) so that the weights optimize pre-treatment fit (mean square error; MSE) in the matching variables between the treated area and the SC. The weights are restricted to be positive and sum up to one to avoid extrapolation. We include only donors with at least 40,000 sample individuals.[18] Excluding small municipalities with greater variation in the outcome reduces the risk of overfitting. That is, to find purely by chance a synthetic control with a small pre-treatment MSE without it being a valid counterfactual (overfitting). We demean the data by subtracting the pre-treatment outcome mean from each municipality before computing the weights, leading to the demeaned SC estimator (Ferman and Pinto, 2021).[19] The demeaned SC estimator assumes that treatment assignment is not correlated with time-varying confounders. It is, under some conditions, superior to the DD estimator in

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could be equally appropriate.

[18]After this restriction, there are nine municipalities in the donor pool for the GP visit outcomes.

[19]Intuitively, we eliminate time-invariant unit fixed effects before matching.

terms of variance and bias (Ferman and Pinto, 2021). The results are estimated using the R package *Synth.*

5   Results

Figure 1 (the raw data) and Figure 2 (the detrended data; see Section 4) indicate that there appears to be a modest increase in GP visits in Helsinki after the abolition of copayments relative to comparison municipalities, and this increase is driven by the lower end of the income distribution. In this section, we provide the regression estimates.

5.1   Main Results

The DD results on GP visits using Specification 1 with the detrended data are reported in Table 1. The copayment abolition is associated with an increase in annualized GP visits: +0.04 (+4.4%) for the whole sample, +0.06 (+4.5%) for the bottom 40%, and +0.02 (+3.3%) for the top 40% of the income distribution. Significance conclusions are sensitive to the inference method. The estimate for all individuals is significant in four cases out of six at the 10% level (clustering by postal code and the WCR produce insignificant effects) and in three cases at the 5% level. The estimate for the bottom 40% is significant in five cases at the 10% level and in four cases at the 5% level. The estimate for the top 40% is insignificant at the 5% level in all cases.

      Sensitivity of the estimates. As the baseline, we assume that the observed linear pre-trend difference would have continued similarly in the post-abolition periods in the absence of the abolition. Figure A8 shows the sensitivity of the point estimates to multiplying the observed pre-treatment trend slope with different values and using the transformed slope for post-treatment periods. The estimates grow in size if we assume that the trend difference would have slowed down after the abolition. In contrast, the estimates attenuate if we instead assume that the trend difference would have accelerated. However, they are positive for all

Table 1: DD Estimates: GP Visits.

	All	Bottom 40%	Top 40%
Mean	0.868	1.306	0.513
Estimate	0.038	0.059	0.017
Change (%)	4.43%	4.51%	3.33%
SE (IID)	0.020 (p=0.072)	0.026 (p=0.033)	0.017 (p=0.323)
SE (HC1)	0.012 (p=0.005)	0.014 (p=0.001)	0.010 (p=0.099)
SE (CL: postal code)	0.032 (p=0.224)	0.032 (p=0.064)	0.036 (p=0.635)
SE (CL: municipality)	0.012 (p=0.004)	0.014 (p=0.000)	0.010 (p=0.090)
CI WCU	[0.012; 0.065]	[0.027; 0.091]	[-0.005; 0.039]
CI WCR	[-0.048; 0.124]	[-0.050; 0.177]	[-0.042; 0.077]
Individuals	1,365,486	541,431	555,529

Notes: We estimate Specification 1. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. Several methods are used for statistical significance testing. First, we show IID and robust (HC1) standard errors and corresponding p-values after ignoring the time series information by aggregating the data at the municipality-by-post-treatment-indicator level (Bertrand et al., 2004). Second, we use analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019), clustering by municipality. Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.

sensible multiplier values and remain so even if the slope of the pre-trend difference doubled. In Table A3, assuming no underlying trend difference in the post-abolition periods produces estimates of +0.06 (+6.9%) for the whole sample, +0.09 (+6.7%) for the bottom 40%, and +0.03 (+5.9%) for the top 40% of the income distribution. In contrast, assuming that the slope of the trend-difference accelerates by 50% in post-treatment periods yields smaller estimates: +0.03 (+3.2%) for the whole sample, +0.05 (+3.4%) for the bottom 40%, and +0.01 (+2.1%) for the top 40% of the income distribution.

We also report bounds-based confidence sets at the 10% level as proposed by Rambachan and Roth (2022), varying how much the slope of the trend difference is allowed to deviate from linearity between consecutive periods. First, we estimate a population-weighted event study specification that includes dynamic treatment indicators for Helsinki, normalized at time $t = -1$, and municipality and time fixed effects. We then use the "second derivative" smoothness restriction ($\Delta^{SD}(M)$) and construct fixed length confidence intervals (FLCIs) for the average of the estimated post-treatment effects using the R package *HonestDiD*. M represents how much the slope can deviate from linearity between consecutive periods. $M = 0$ means that exact linearity is assumed. Our remaining M values are derived from multiplying the estimated slope of the linear pre-trend difference by 0.05, 0.10, or 0.15. The results are reported in Figure A9. We do not reject the null of no effects at the 10% level even if exact linearity is assumed. The confidence interval for all individuals is from -1% to $+10\%$.[20] The confidence intervals widen considerably and start to contain large negative values as well once we relax the assumption of exact linearity.

In Figure A10, we check the robustness of the results to small changes in the comparison group. Specifically, we exclude each X-municipality combination, $X \in \{1, 2, 3\}$, from the comparisons. Based on the leave-one-out results, the estimates grow in size noticeably if either Vantaa (92) or Kouvola (286) are excluded and decrease if either Turku (853) or Joensuu (167) are excluded.[21] The estimates for all individuals vary between

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[20]The variance-covariance matrix of the event-study specifications is based on the IID assumption.

[21]We consider Vantaa to be an important comparison area as it belongs to the Helsinki metropolitan area

+0.02 (+2.6%) and +0.05 (6.2%). The leave-two-out estimates for all individuals remain positive. However, a couple of combinations out of 969 produce negative point estimates in the leave-three-out estimation.

Table A4 presents the results from using a monthly indicator of having any GP visits as the outcome instead of the number of GP visits. The main findings are robust. Furthermore, the table contains the results from replacing municipality fixed effects in Specification 1 with postal code area fixed effects, which slightly attenuates the estimates.

Effect heterogeneity by income level. Figure 3 shows the effects by income decile. In absolute terms (the number of visits), there is a clear pattern that estimates are larger for low-income individuals. Estimates are significant at the 5% level for Decile 1 and Decile 3 when standard errors are clustered by postal code area. However, such a pattern is not observable in relative terms (the estimate as a percentage change).
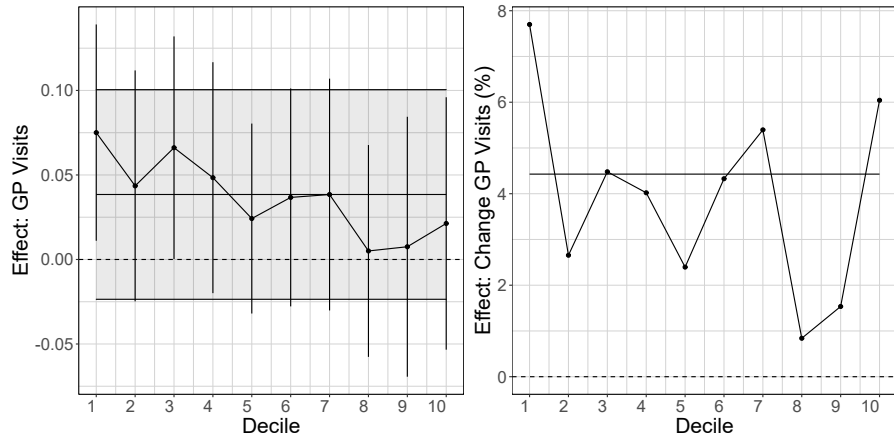
Figure 3: DD Estimates by Income Decile.

Notes: We estimate Specification 1 and cluster standard errors by postal code area. The effects are shown by income deciles (equivalized family disposable income). On the left, the gray block centered at the black horizontal line shows the estimate and its confidence interval for the whole sample. On the right, we map the point estimates to percentage changes by dividing the estimate by mean GP use in Helsinki in 2012 and multiply by 100. Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods.

and is large and urban. Turku as a large city is similarly an important comparison.

The DDD estimates are reported in Table 2. Our preferred estimate is based on the assumption that the pre-trend difference can be extrapolated to the post treatment periods (1.0 x slope): the number of annualized visits increases by +0.04 (+3.2%) visits at the bottom 40% relative to the top 40%. Assuming that there was no underlying trend difference in the post-abolition periods (0 x slope) yields a somewhat larger estimate: +0.06 (+4.4%) visits. In contrast, assuming that the trend difference accelerated in the post-abolition periods (1.5 x slope) leads to a smaller estimate: +0.03 (+2.6%) visits. Detrending attenuates the estimates: the DDD estimate is +0.07 (+5.7%) on the raw data. The estimates are robust to changing the outcome from the number of GP visits to a monthly indicator of having any GP visits. Clustering analytically and the WCU bootstrap reject at the 10% level, but the WCR bootstrap does not reject in any case after detrending at the 5% level.

5.2   Supplementary Analyses

Synthetic controls results. Figure 4 report the SC results on GP use. Implementation details are reported in Section 4. When analyzing the effects on GP use in the whole population, our synthetic Helsinki follows Helsinki in terms of GP use sufficiently well prior to the abolition. The gaps vary between +0.05 and −0.05 annualized visits. There may be a small increasing pre-trend in GP use in Helsinki relative to the SC. For this reason, we report both the raw results and detrended results. The latter are computed by subtracting a linear pre-trend difference from the raw gaps. The trend difference is estimated with OLS using only pre-treatment data. We prefer the detrended estimates that assume that there is no correlation between the treatment assignment and time-varying confounders once a linear pre-trend difference is removed.[22]

The detrended SC estimate for all individuals is +0.037 (+4.3%), essentially the same as the corresponding regression DD estimate of Table 1. The estimate without detrending is almost twice as large: +0.070 (+8.0%). In the middle, the outcome is the

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[22]In contrast, the raw estimates assume that there is no correlation between the treatment assignment and time-varying confounders at all.

Table 2: DDD Estimates: GP Visits.

A. Outcome: the number of GP visits

	No detrending	0 x slope	1.0 x slope	1.5 x slope
Mean	1.306	1.306	1.306	1.306
Estimate	0.074	0.057	0.042	0.034
Change (%)	5.68%	4.39%	3.20%	2.61%
SE (postal code)	0.018 (p=0.000)	0.018 (p=0.002)	0.018 (p=0.024)	0.018 (p=0.066)
SE (municipality)	0.009 (p=0.000)	0.009 (p=0.000)	0.009 (p=0.000)	0.009 (p=0.001)
CI WCU	[0.054; 0.094]	[0.038; 0.077]	[0.022; 0.062]	[0.014; 0.054]
CI WCR	[0.015; 0.134]	[-0.002; 0.117]	[-0.018; 0.101]	[-0.025; 0.094]

B. Outcome: the indicator of having any GP visits

	No detrending	0 x slope	1.0 x slope	1.5 x slope
Mean	9.243	9.243	9.243	9.243
Estimate	0.443	0.369	0.301	0.267
Change (%)	4.79%	3.99%	3.26%	2.89%
SE (postal code)	0.128 (p=0.001)	0.128 (p=0.004)	0.128 (p=0.019)	0.128 (p=0.037)
SE (municipality)	0.061 (p=0.000)	0.061 (p=0.000)	0.061 (p=0.000)	0.061 (p=0.000)
CI WCU	[0.302; 0.583]	[0.229; 0.509]	[0.161; 0.441]	[0.127; 0.407]
CI WCR	[0.025; 0.866]	[-0.049; 0.793]	[-0.117; 0.725]	[-0.151; 0.691]

Notes: We estimate Specification 2. The pre-abolition mean is computed at the bottom 40% of the income distribution in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019), clustering by municipality. In the first column, we use raw data without detrending. Otherwise, we remove a linear pre-trend difference from the data before estimation: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The multiplier of the slope of the linear trend difference is varied for the post-abolition periods in columns (0, the baseline 1.0, and 1.5). If the multiplier is larger (smaller) than 1, the trend difference is expected to accelerate (slow down) in post-abolition periods. Sample size is 1,096,960 individuals.

difference in GP use between the bottom 40% and the top 40% of the income distribution. The detrended results suggest that GP use increased in absolute terms (levels) by +0.054 annualized visits (+6.8%) in the bottom 40% compared to the top 40%. The corresponding estimate without detrending is higher: +0.079 annualized visits (+10.0%). On the right, the outcome is the ratio of GP use between the two income groups. The point estimates are small and close to zero, suggesting that there is no observable heterogeneity in relative terms. The point estimates are robust to a leave-two-out analysis in which we report the average results after permutatively excluding each two-donor pair from the donor pool (Figure A11).
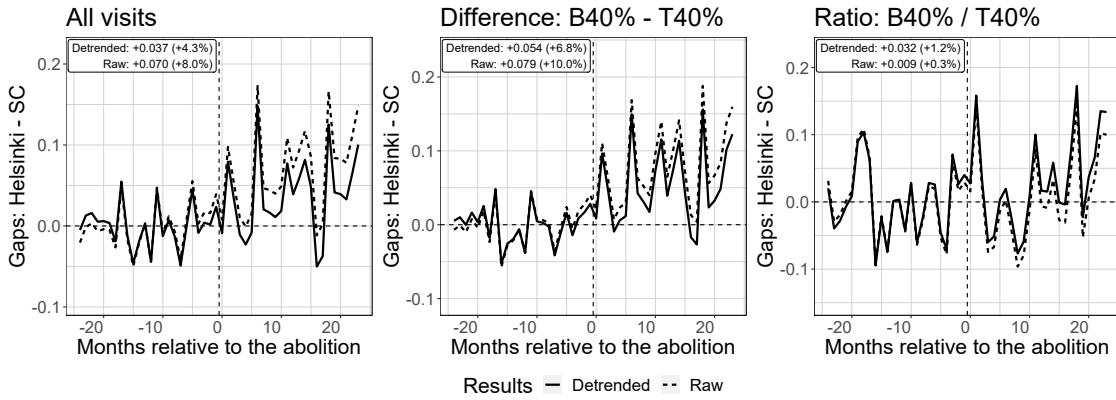

Figure 4: Synthetic Control Estimates: GP Visits.

Notes: The plots show the difference in outcomes between Helsinki and its synthetic control (gaps). We include in the donor pool municipalities with more than 40,000 sample individuals. Pre-treatment lags are used as matching variables. The SC weights are reported in Table A5. We subtract from each municipality its pre-treatment outcome mean (demeaning) before estimation. B40% and T40% refer to the bottom 40% and the top 40% of the equivalized disposable income distribution. The detrended results show the gaps after subtracting a linear pre-trend difference. In the top left corner, we show aggregated treatment effect estimates from averaging all post-treatment gaps. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean.

Secondary outcomes. If the effect of the copayment abolition on GP use were large enough, we would expect a reduction in ED visits, an increase in specialist consultations, and a decrease in the probability of receiving social assistance. However, the estimates on GP visits are small, and it is unlikely that we find any effects on our secondary outcomes. The scarcity of GP appointment slots and waiting times drive some patients to seek care from ED

departments. The abolition of the GP visit copayment made public primary care GP visits more attractive relative to ED visits for which copayments continued to be charged. The effect on specialist consultations depends on what kind of patients increase their visits as a response to the copayment abolition. If there previously were missed medically valuable care, we would expect an increase in referrals to specialist consultations. Finally, the copayment abolition is a financial improvement for a large fraction of low income individuals, plausibly reducing the need to apply for social assistance (to a small extent).

Figure A12 shows the trends in raw outcomes in Helsinki and the comparison areas. In ED visits and specialist consultations, the utilization is lower in Helsinki, but there are few signs of a similar pre-trend difference as in GP visits. For social assistance use, however, there is a clear increasing trend in Helsinki. The slope of the pre-trend difference is larger before the abolition, and even our detrending is unlikely to lead to robust estimates.[23] Our interpretation is that there are no observable level effects of the copayment abolition on the use of social assistance.

Figure A13 shows the difference in ED visits and specialist consultations between Helsinki and the comparison areas after removing the estimated linear trend difference. Nothing striking seems to happen after the abolition. There may be an increase (and not a decrease) in ED visits at the bottom 40% of the income distribution. In absolute terms, the change is small compared to the observed increase in GP visits. The corresponding regression results are in Table A6. The increase in ED visits at the bottom 40% of the income distribution translates to a +3.0% increase, but it is insignificant. The other estimates are close to zero and insignificant.

Placebo outcome: dentist visits. There were no major changes in copayments for dentist visits during our study period and there should be little if any substitution between GP and dentist appointments. Consequently, dentist visits are a potential placebo outcome.

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[23]If we used all available 12 pre-treatment months and extrapolated the observed pre-trend, we would conclude that the probability of receiving social assistance decreases after January 2013. If, however, we used only 8 pre-treatment months for pre-trend fitting, we would conclude that the probability of receiving social assistance increases after the abolition.

Figure A14 shows the trends in dentist visits in Helsinki and the comparison municipalities by income group, and Figure A15 shows the difference in outcomes after removing a linear pre-trend difference from the outcome data. Interestingly, dentist use appears to decrease in Helsinki after the GP visit copayment abolition relative to the comparison municipalities. We note that the change appears to be the result of a reduction in dentist use in Helsinki, while in the comparison municipalities the use did increase somewhat in 2013-2014.[24] Second, dentist use in Helsinki appears to have at least partially recovered since April 2014 relative to the comparison municipalities. In contrast, such convergence is not observed in GP visits, which remain at a higher level in Helsinki.

The corresponding DD estimates are given in Table A7 and the DDD estimates in Table A8. The DD estimates on dentist visits are of similar magnitude than the estimates on GP visits, but with a different sign. Due to the lower baseline level of dentist use, the estimates are farther from zero in relative terms than the estimates on GP visits. The DDD estimates show that it is the lower end of the income distribution whose dentist use decreased more in Helsinki after the GP visit copayment abolition.

A relevant question is, how much should the main results (the estimates on GP visits) be weighted down as a consequence of observing a decrease in dentist visits in Helsinki after the GP visit copayment abolition instead of a precise null. We searched for possible explanations for the observed reduction in dentist use, but did not find a convincing candidate for explanation.[25]

Time placebo. Instead of taking a two-year window around the actual policy change in January 2013, we estimate the effects of a placebo intervention using only

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[24]This stands in contrast with the estimates that show an increase in GP visits. In that case, the GP use was decreasing in the comparison municipalities and somewhat increased in Helsinki in post-abolition periods.

[25]Some of the reduction may be explained by the fact that the public primary care in Helsinki reduced the supply of vouchers for dentist visits that the patients could use to get the same service from the private sector, these visits being observed in our data. The supply was reduced from July 2013 to the end of the year due to budgetary reasons (Helsinki's social and healthcare services committee, September 17th, 2013). In the first half of 2013, there were 7,600 voucher dentist visits, converting to 0.025 annualized visits per capita. The observed reduction between July 2013 and March 2014 is, however, much larger.

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pre-abolition data over the period 2011-2012 and proceed as if Helsinki abolished the copayment in January 2012. Other aspects of data processing and analysis remain fixed, including the detrending.[26] The placebo results are reported in Table A9. They are negative and closer to zero than the policy estimates in Table 1. The placebo estimate for the whole sample is a decrease of -0.02 annualized visits (-2.1%) while the real estimate is an increase of $+0.04$ ($+4.4\%$) visits.

6 Discussion

In comparison to earlier research (summarized in Section 1), our estimates are relatively modest if not small in terms of quantitative size. The main estimate for all individuals ($+0.038$ or $+4.4\%$) maps to a semi-arc elasticity of -0.26.[27] For comparison, Nilsson and Paul (2018) find semi-arc elasticities of -0.88 (at the 20th birthday) and -0.55 (at the 7th birthday) for GP visit copayments in Sweden. Moreover, Johansson et al. (2019) report a semi-arc elasticity of -1.11 by focusing on the effect of GP visit copayments at the 20th birthday in Sweden.[28] In Finland, Haaga et al. (2022) provide two semi-arc elasticity estimates in a study assessing the effects of a nurse visit copayment introduction in the entire adult population: -0.41 (their baseline) and -1.24 (their upper bound). Taking the estimates and their standard errors of Table 1 for all individuals at face value and depending on the inference method, we can rule out effects larger than $+0.06$ (twice), $+0.07$, $+0.08$, $+0.10$, or $+0.12$ ($+7.1\%$, $+7.5\%$, $+8.9\%$, $+11.6\%$, or 14.2%).

The effects of increased and decreased cost sharing on health care use may not be symmetric. Some recent studies have concluded that framing the change as a loss may

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[26]The linear pre-trend difference is now estimated using data from 2011.

[27]The semi-arc elasticity captures the change in nurse visits, normalized by the baseline, divided by the price change (Brot-Goldberg et al., 2017): $\frac{(q_1-q_0)/(q_1+q_0)}{(p_1-p_0)/2} = \frac{(0.868+0.038-0.868)/(0.868+0.038+0.868)}{(0-13.8/83)/2}$. As Nilsson and Paul (2018) and Haaga et al. (2022), our price is the share of the out-of-pocket costs of the total cost of the visit. The average production cost of a GP visit was 83 euros in 2017 (Mäklin and Kokko, 2020).

[28]We computed the elasticity by taking the estimates for all individuals from Table 1 in Johansson et al. (2019), using a copayment of SEK 100 and assuming that the total cost is SEK 1500 per visit.

have larger effects than framing it as a gain (Hayen et al., 2021; Iizuka and Shigeoka, 2021; Remmerswaal et al., 2019). Furthermore, Finnish gatekeeping institutions at the point of entry may cause the effect on GP appointments (observable to an analyst) to be lower than the effect on first contacts to primary care (not observable to an analyst). This could attenuate the estimates compared to a system with no or less gatekeeping. Overall, our findings that smaller out-of-pocket costs increase primary care use and that the low-income individuals are more sensitive to cost sharing are in accordance with the earlier evidence.

The economic significance of our estimates can be illustrated based on a simple back-of-the-envelope calculation. Taking the effect estimate of $+0.038$ ($+4.43\%$) annualized visits per capita and the average production cost of 83 euros per GP appointment in 2017 (Mäklin and Kokko, 2020) at face value and assuming that the marginal cost is equal to the average cost, the short-term extra costs to Helsinki due to the increase in GP use were 3 euros per capita in our sample population. In contrast, the loss of copayment revenue could have been in the magnitude of 8 euros per capita if we do not account for billing and collection costs nor for an increased use of social assistance.[29] Thus, the short-term fiscal effects of the observed behavioral change seem to have been small. Consistent with earlier research from other contexts, we find some evidence that low-income individuals are more responsive to out-of-pocket costs (at least in absolute terms).

To evaluate the credibility of our findings, it is important to highlight that Helsinki is in many ways a unique area in Finland. The parallel trends assumption seems plausible only after removing a linear pre-trend difference from the outcome data. The effect sizes are modest compared to the monthly noise in the outcome. The key limitation of our research design is that statistical inference is complicated due to the presence of one treated

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[29]The copayment revenue from primary care visits was 4.2 million euros in Helsinki in 2011 (Helsinki's committee for health services, March 6th, 2012), and we assume that 3.4 million euros (81%) came from GP visit copayments, the rest coming from fines for uncancelled missed visits and charges for health certificates. Of this sum, we assume that 3.0 million (89%) came from individuals satisfying our sample restrictions ($N = 380,000$). Alternatively, the 8-euro figure can be reached by assuming that the actually collected copayment revenue was 70% of the theoretical maximum (the number of annualized visits multiplied by the nominal copayment).

cluster and a finite number of comparison clusters. Although our point estimates are rather robustly positive, the significance conclusions are sensitive to how we account for clustering. Interestingly, the number of dentist visits (a plausible placebo outcome) decreases in Helsinki after the GP visit copayment abolition by approximately the same magnitude as GP use increases relative to the comparison areas.

CRediT author statement: **Haaga:** Conceptualization, Formal analysis, Writing - Original Draft, Writing - Review & Editing. **Böckerman:** Conceptualization, Writing - Review & Editing, Supervision. **Kortelainen:** Conceptualization, Writing - Review & Editing, Supervision. **Tukiainen:** Conceptualization, Writing - Review & Editing, Supervision.

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A Online Appendix

A.1 Additional Figures and Tables

Table A1: Background Statistics by Municipality Group.

	Helsinki	Comparisons	The Rest
Municipalities	1	19	274
A. Health care use			
Primary care GP visits	0.83	0.89	0.99
Emergency department visits	0.18	0.27	0.19
Specialist consultations	0.20	0.23	0.25
Private doctor visits	0.90	0.78	0.58
Medicine reimbursements	68.2%	71.5%	71.5%
B. Sociodemographic and socioeconomic characteristics			
Population mean	603,968	84,027	11,670
Pensioners	19.5%	23.0%	25.7%
Students	7.7%	8.2%	7.3%
Employment rate	71.7%	68.5%	69.5%
Tertiary education	38.6%	29.7%	26.4%
Social assistance (euros)	227.70	145.45	100.43
Rental households	47.3%	34.2%	24.4%
Urbanization rate	99.9%	93.6%	77.5%

Notes: The comparison municipalities depend on the outcome as described in Section 3, here we use GP visits. The data are from 2012 and contain aggregated registry data and publicly available municipal-level data from Statistics Finland, Sotkanet, and the Social Insurance institution.

Table A2: Time Effects after Detrending.

| | All | | Bottom 40% | | Top 40% | |
Order	Area	Estimate	Area	Estimate	Area	Estimate
1	286	0.248	286	0.283	286	0.195
2	734	0.131	734	0.167	92	0.106
3	92	0.094	405	0.118	398	0.083
4	405	0.088	858	0.116	734	0.081
5	179	0.060	609	0.055	211	0.081
6	609	0.054	92	0.050	405	0.069
7	**91**	**0.053**	**91**	**0.030**	837	0.063
8	186	0.050	179	0.023	**91**	**0.062**
9	398	0.044	837	0.002	186	0.060
10	211	0.036	186	−0.006	179	0.057
11	837	0.034	211	−0.008	257	0.043
12	858	0.033	398	−0.011	202	0.041
13	202	−0.010	491	−0.061	609	0.036
14	257	−0.010	257	−0.089	285	0.017
15	491	−0.032	285	−0.117	245	0.002
16	285	−0.058	202	−0.162	858	−0.002
17	245	−0.073	853	−0.208	491	−0.007
18	853	−0.112	245	−0.219	853	−0.013
19	444	−0.214	444	−0.301	444	−0.135
20	167	−0.321	167	−0.415	167	−0.194

Notes: We first detrend the data by estimating and subtracting a linear pre-trend difference from each municipality (labeled as area in the table). Then, we regress for each municipality the detrended outcome on an indicator for post-treatment periods and an intercept. The table reports coefficients for the time effects. The results show that the time effects can be large in absolute value in single comparison municipalities. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.

Table A3: DD Estimates: GP Visits, and Sensitivity to the Parallel Trends Assumption.

A. No trend difference after the abolition (0 x the estimated slope)

	All	Bottom 40%	Top 40%
Mean	0.868	1.306	0.513
Estimate	0.060	0.088	0.030
Change (%)	6.89%	6.70%	5.89%
SE (postal code)	0.032 (p=0.059)	0.032 (p=0.006)	0.036 (p=0.400)
SE (municipality)	0.012 (p=0.000)	0.014 (p=0.000)	0.010 (p=0.005)
CI WCU	[0.034; 0.086]	[0.056; 0.119]	[0.008; 0.052]
CI WCR	[-0.027; 0.145]	[-0.021; 0.206]	[-0.029; 0.090]

B. Trend difference slows down after the abolition (0.5 x the estimated slope)

	All	Bottom 40%	Top 40%
Mean	0.868	1.306	0.513
Estimate	0.049	0.073	0.024
Change (%)	5.66%	5.61%	4.61%
SE (postal code)	0.032 (p=0.121)	0.032 (p=0.021)	0.036 (p=0.511)
SE (municipality)	0.012 (p=0.000)	0.014 (p=0.000)	0.010 (p=0.023)
CI WCU	[0.023; 0.075]	[0.042; 0.105]	[0.002; 0.045]
CI WCR	[-0.038; 0.134]	[-0.036; 0.191]	[-0.036; 0.084]

C. Trend difference accelerates after the abolition (1.5 x the estimated slope)

	All	Bottom 40%	Top 40%
Mean	0.868	1.306	0.513
Estimate	0.028	0.045	0.010
Change (%)	3.20%	3.41%	2.05%
SE (postal code)	0.032 (p=0.380)	0.032 (p=0.160)	0.036 (p=0.770)
SE (municipality)	0.012 (p=0.027)	0.014 (p=0.004)	0.010 (p=0.286)
CI WCU	[0.002; 0.054]	[0.013; 0.076]	[-0.011; 0.032]
CI WCR	[-0.059; 0.113]	[-0.064; 0.163]	[-0.049; 0.071]

Notes: We estimate Specification 1. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019), clustering by municipality. Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The multiplier of the slope of the linear trend difference is varied for the post-abolition periods (0, 0.5, and 1.5). Bottom 40% and top 40% are based on the equivalized family disposable income distribution. Sample sizes: 1,365,486 individuals in the whole sample, 541,431 at the bottom 40%, and 555,529 at the top 40%.

Table A4: DD Estimates: GP Visits, Robustness Checks.

A. Postal code area fixed effects

	All	Bottom 40%	Top 40%
Mean	0.868	1.306	0.513
Estimate	0.037	0.053	0.016
Change (%)	4.23%	4.09%	3.19%
SE (postal code)	0.031 (p=0.239)	0.031 (p=0.084)	0.036 (p=0.646)
SE (municipality)	0.011 (p=0.004)	0.014 (p=0.001)	0.010 (p=0.102)
CI WCU	[0.012; 0.065]	[0.027; 0.091]	[-0.005; 0.039]
CI WCR	[-0.048; 0.124]	[-0.050; 0.177]	[-0.042; 0.077]

B. Has any GP visits + municipality fixed effects

	All	Bottom 40%	Top 40%
Mean	6.239	9.243	3.783
Estimate	0.303	0.450	0.149
Change (%)	4.86%	4.86%	3.93%
SE (postal code)	0.228 (p=0.183)	0.213 (p=0.035)	0.267 (p=0.578)
SE (municipality)	0.079 (p=0.001)	0.094 (p=0.000)	0.064 (p=0.032)
CI WCU	[0.121; 0.485]	[0.232; 0.667]	[0.002; 0.295]
CI WCR	[-0.272; 0.885]	[-0.282; 1.242]	[-0.238; 0.548]

C. Has any GP visits + postal code area fixed effects

	All	Bottom 40%	Top 40%
Mean	6.239	9.243	3.783
Estimate	0.291	0.413	0.144
Change (%)	4.67%	4.47%	3.80%
SE (postal code)	0.224 (p=0.194)	0.207 (p=0.046)	0.265 (p=0.588)
SE (municipality)	0.077 (p=0.001)	0.093 (p=0.000)	0.064 (p=0.036)
CI WCU	[0.121; 0.485]	[0.232; 0.667]	[0.002; 0.295]
CI WCR	[-0.272; 0.885]	[-0.282; 1.242]	[-0.238; 0.548]

Notes: We estimate Specification 1. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019), clustering by municipality. Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution. In Panel B and Panel C, we use the monthly indicator of having any GP visits as the outcome. Sample sizes: 1,365,486 individuals in the whole sample, 541,431 at the bottom 40%, and 555,529 at the top 40%.

Table A5: Synthetic Control Weights.

	All	Difference: B40% - T40%	Ratio: B40% / T40%
Vantaa	0.164	0	0.175
Joensuu	0.035	0.014	0.204
Jyväskylä	0	0.177	0.091
Kouvola	0.080	0.149	0
Lahti	0.046	0.014	0.017
Lappeenranta	0	0	0
Pori	0	0	0.133
Tampere	0.448	0.276	0.099
Turku	0.227	0.370	0.281

Notes: The table shows the synthetic control weights for our donor pool municipalities, the weights depending on outcome and visit type. We include in the donor pool municipalities with more than 40,000 sample individuals. Pre-treatment lags are used as matching variables. We subtract from each municipality its pre-treatment outcome mean (demeaning) before estimation. "All" = all individuals and all visits. "Difference" = the difference between the bottom 40% and the top 40% of the equivalized disposable income distribution in visits per capita. "Ratio" = the ratio between the bottom 40% and the top 40% in visits per capita.

Table A6: DD Estimates: ED Visits and Specialist Consultations.

A. ED Visits

	All	Bottom 40%	Top 40%
Mean	0.172	0.227	0.131
Estimate	0.003	0.007	-0.001
Change (%)	1.48%	3.02%	-1.08%
SE (postal code)	0.006 (p=0.662)	0.005 (p=0.157)	0.007 (p=0.845)
SE (municipality)	0.004 (p=0.534)	0.006 (p=0.272)	0.002 (p=0.570)
CI WCU	[-0.006; 0.011]	[-0.006; 0.020]	[-0.007; 0.004]
CI WCR	[-0.027; 0.036]	[-0.040; 0.062]	[-0.018; 0.015]
Individuals	1,491,828	586,151	608,406

B. Specialist Consultations

	All	Bottom 40%	Top 40%
Mean	0.227	0.262	0.195
Estimate	0.001	0.000	0.002
Change (%)	0.58%	-0.15%	0.93%
SE (postal code)	0.006 (p=0.823)	0.005 (p=0.937)	0.008 (p=0.813)
SE (municipality)	0.006 (p=0.823)	0.008 (p=0.961)	0.004 (p=0.688)
CI WCU	[-0.010; 0.013]	[-0.017; 0.016]	[-0.007; 0.011]
CI WCR	[-0.038; 0.041]	[-0.063; 0.063]	[-0.025; 0.029]
Individuals	1,485,103	590,254	598,624

Notes: We estimate Specification 1. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019). Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.

Table A7: DD Estimates: Dentist Visits.

	All	Bottom 40%	Top 40%
Mean	0.449	0.598	0.314
Estimate	-0.034	-0.054	-0.020
Change (%)	-7.65%	-9.04%	-6.33%
SE (postal code)	0.007 (p=0.000)	0.008 (p=0.000)	0.006 (p=0.002)
SE (municipality)	0.016 (p=0.050)	0.021 (p=0.021)	0.012 (p=0.111)
CI WCU	[-0.067; -0.002]	[-0.095; -0.013]	[-0.046; 0.006]
CI WCR	[-0.171; 0.059]	[-0.263; 0.081]	[-0.111; 0.041]
Individuals	1,403,089	560,158	565,169

Notes: We estimate Specification 1. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019). Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.

Table A8: DDD Estimates: Dentist Visits.

A. Outcome: the number of dentist visits

	No detrending	0 x slope	1.0 x slope	1.5 x slope
Mean	0.598	0.598	0.598	0.598
Estimate	-0.017	-0.026	-0.034	-0.038
Change (%)	-2.92%	-4.38%	-5.71%	-6.38%
SE (postal code)	0.006 (p=0.006)	0.006 (p=0.000)	0.006 (p=0.000)	0.006 (p=0.000)
SE (municipality)	0.011 (p=0.117)	0.011 (p=0.024)	0.011 (p=0.005)	0.011 (p=0.002)
CI WCU	[-0.038; 0.003]	[-0.047; -0.005]	[-0.055; -0.013]	[-0.059; -0.017]
CI WCR	[-0.106; 0.045]	[-0.115; 0.037]	[-0.123; 0.029]	[-0.127; 0.025]

B. Outcome: the indicator of having any dentist visits

	No detrending	0 x slope	1.0 x slope	1.5 x slope
Mean	3.844	3.844	3.844	3.844
Estimate	-0.047	-0.110	-0.167	-0.195
Change (%)	-1.24%	-2.85%	-4.34%	-5.08%
SE (postal code)	0.041 (p=0.243)	0.041 (p=0.007)	0.041 (p=0.000)	0.041 (p=0.000)
SE (municipality)	0.059 (p=0.431)	0.059 (p=0.080)	0.059 (p=0.011)	0.059 (p=0.004)
CI WCU	[-0.167; 0.072]	[-0.229; 0.010]	[-0.286; -0.047]	[-0.315; -0.076]
CI WCR	[-0.532; 0.318]	[-0.594; 0.255]	[-0.651; 0.198]	[-0.680; 0.170]

Notes: We estimate Specification 2. The pre-abolition mean is computed at the bottom 40% of the income distribution in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019). In the first column, we use raw data without detrending. Otherwise, we remove a linear pre-trend difference from the data before estimation: we compute outcome means over time by policy group and calculate their difference using only pre-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The multiplier of the slope of the linear trend difference is varied for the post-abolition periods in columns (0, the baseline 1.0, and 1.5). If the multiplier is larger (smaller) than 1, the trend difference is expected to accelerate (slow down) in post-abolition periods. Sample size is 1,125,327 individuals.

Table A9: Time Placebo DD Estimates: GP Visits.

	All	Bottom 40%	Top 40%
Mean	0.885	1.324	0.523
Estimate	-0.019	-0.022	-0.014
Change (%)	-2.14%	-1.70%	-2.61%
SE (postal code)	0.010 (p=0.053)	0.016 (p=0.171)	0.006 (p=0.028)
SE (municipality)	0.016 (p=0.255)	0.024 (p=0.368)	0.009 (p=0.139)
CI WCU	[-0.054; 0.016]	[-0.079; 0.034]	[-0.032; 0.005]
CI WCR	[-0.127; 0.088]	[-0.216; 0.162]	[-0.061; 0.034]
Individuals	1,365,486	541,431	555,529

Notes: We estimate the effects of a placebo intervention using pre-abolition data from 2011-2012 and proceed as if Helsinki abolished the copayment in January 2012. We estimate Specification 1. The pre-placebo-abolition mean is computed in Helsinki for 2011, and the change in percentage terms compares the estimate to this mean. For statistical significance, we report standard errors and corresponding p-values using analytical formulas and cluster by postal code area and by municipality. We also provide confidence intervals from the unrestricted (WCU) and restricted (WCR) wild cluster bootstrap (Roodman et al., 2019). Before estimation, we remove a linear pre-trend difference from the data: we compute outcome means over time by policy group and calculate their difference using only pre-placebo-treatment data, then fit a linear trend difference with ordinary least squares (OLS), and finally subtract the estimated linear pre-trend difference from the outcome data. The observed pre-trend difference is assumed to extrapolate to the post-abolition periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.
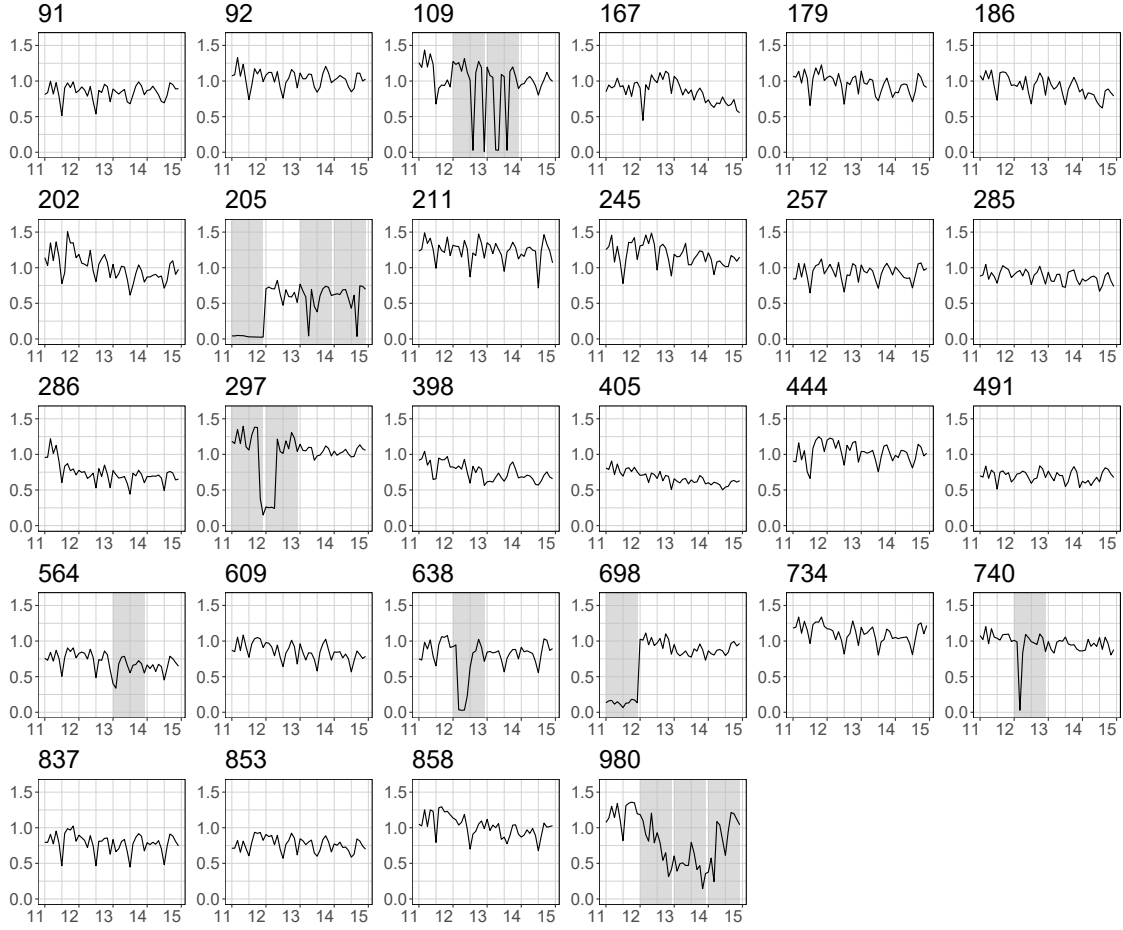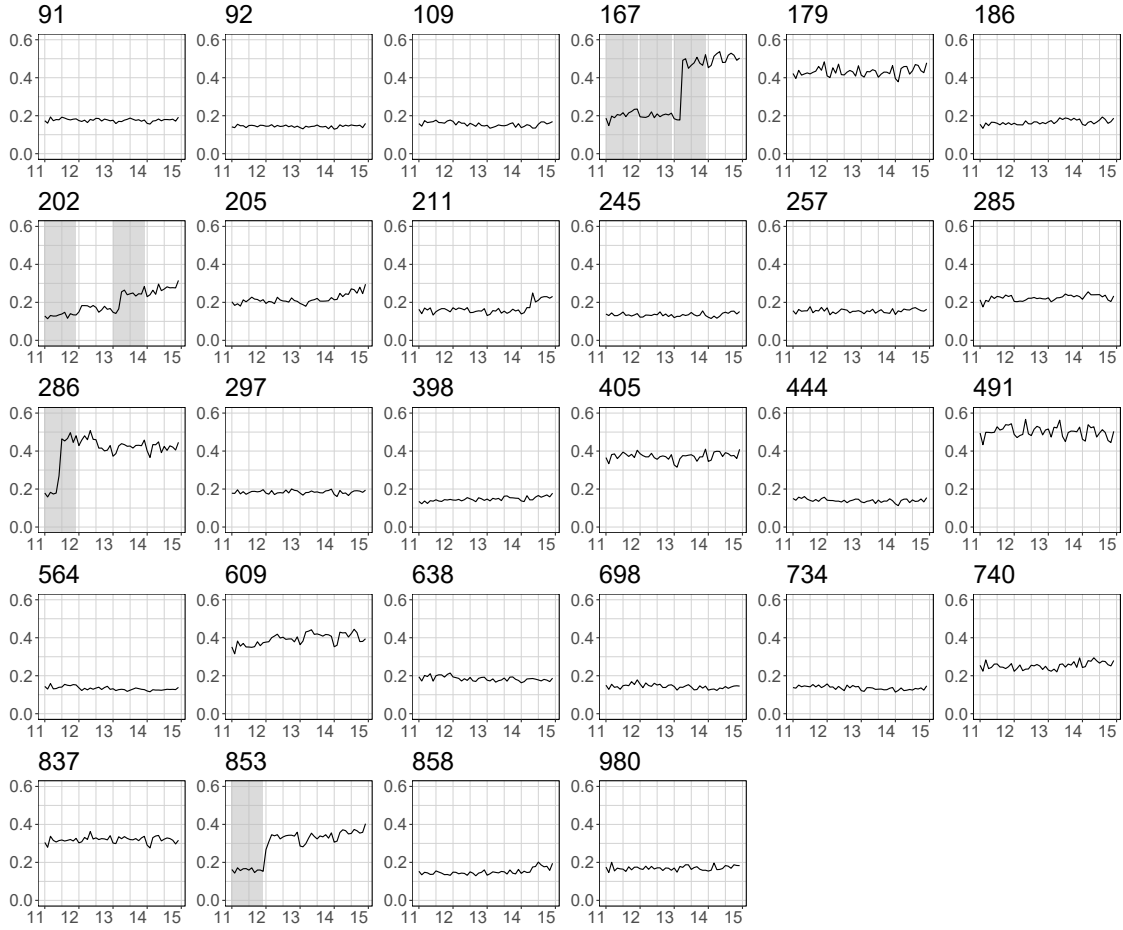
Figure A1: The Evolution of GP Visits by Sample Municipality.

Notes: The plots show the evolution of annualized GP visits in the total population in Helsinki (municipality number 91) and in the 27 potential comparison municipalities. Municipality-year observations having susceptible values of health care contacts are highlighted by gray. These municipalities are excluded from the analysis sample. They were identified as follows: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, and 2) mark an observation to be invalid if its value is less than 50% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations).
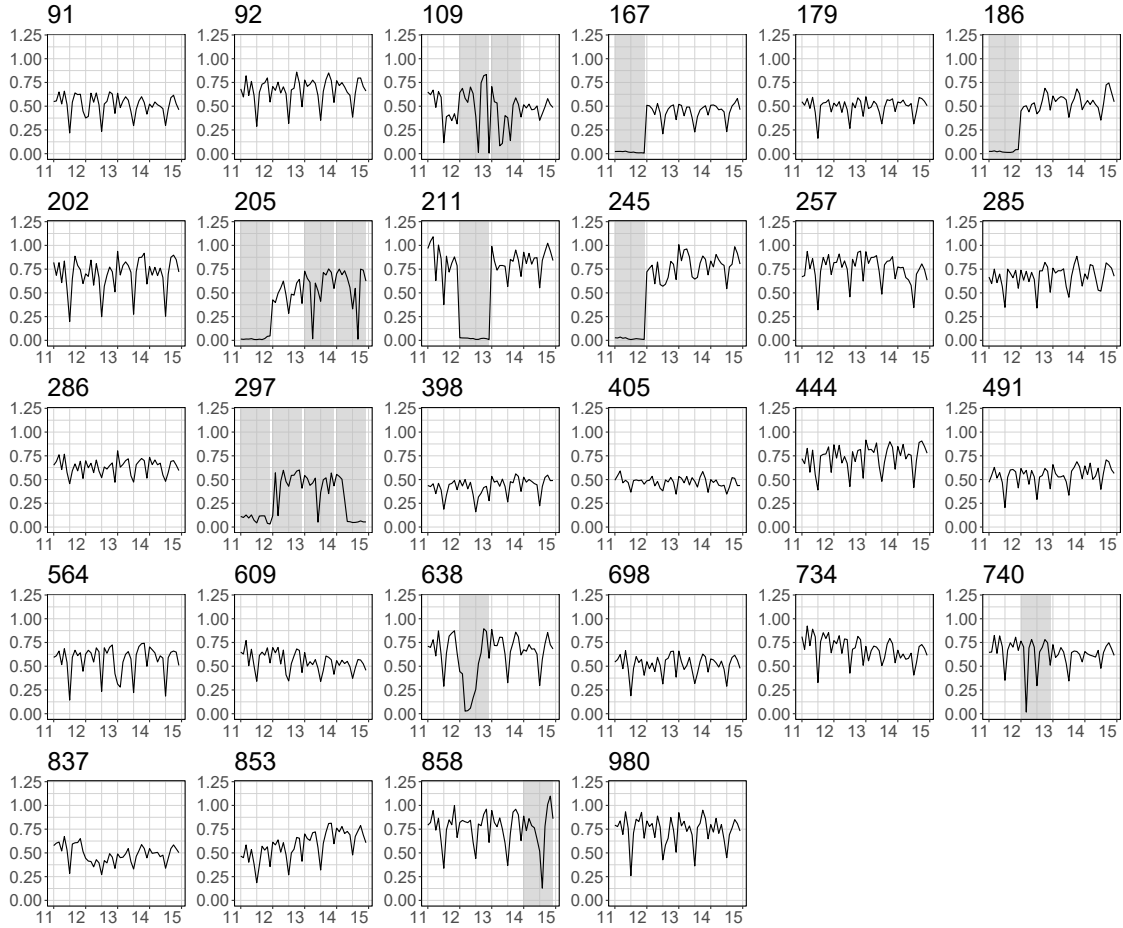
Figure A2: The Evolution of ED Visits by Sample Municipality.

Notes: The plots show the evolution of annualized ED visits in the total population in Helsinki (municipality number 91) and in the 27 potential comparison municipalities. Municipality-year observations having susceptible values of health care contacts are highlighted by gray. These municipalities are excluded from the analysis sample. They were identified as follows: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, and 2) mark an observation to be invalid if its value is less than 30% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations).
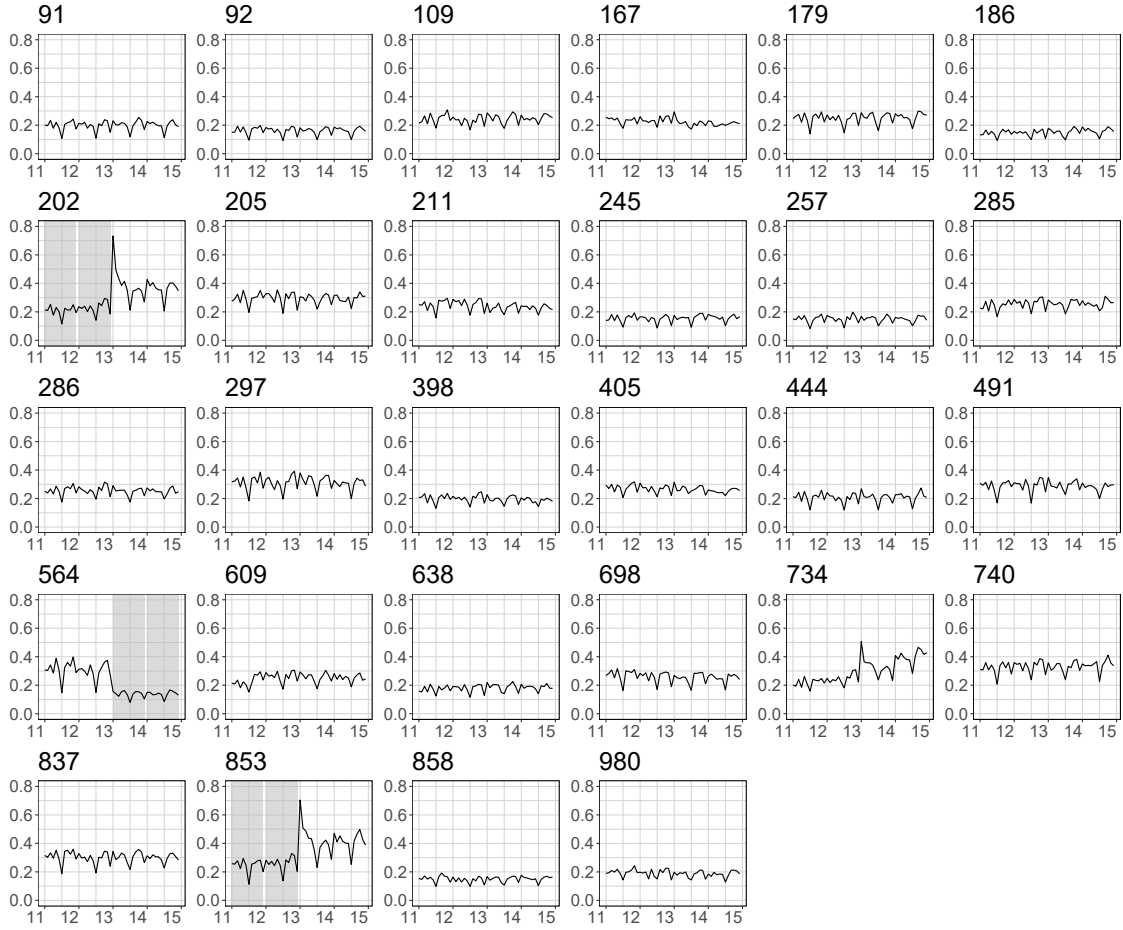
Figure A3: The Evolution of Specialist Consultations by Sample Municipality.

Notes: The plots show the evolution of annualized specialist consultations in the total population in Helsinki (municipality number 91) and in the 27 potential comparison municipalities. Municipality-year observations having susceptible values of health care contacts are highlighted by gray. These municipalities are excluded from the analysis sample. They were identified as follows: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, and 2) mark an observation to be invalid if its value is less than 40% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations).

Figure A4: The Evolution of Dentist Visits by Sample Municipality.

Notes: The plots show the evolution of annualized dentist visits in the total population in Helsinki (municipality number 91) and in the 27 potential comparison municipalities. Municipality-year observations having susceptible values of health care contacts are highlighted by gray. These municipalities are excluded from the analysis sample. They were identified as follows: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, and 2) mark an observation to be invalid if its value is less than 55% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations).
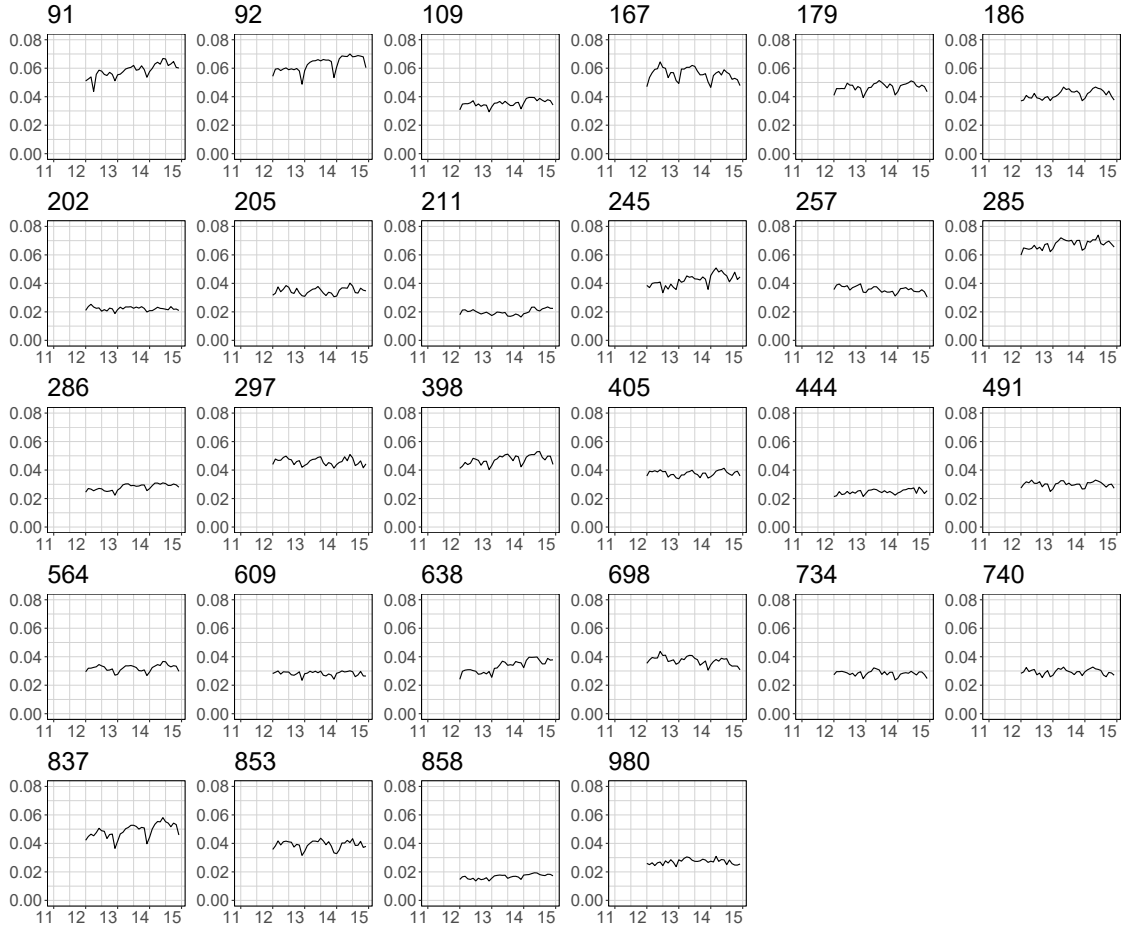
Figure A5: The Evolution of Social Assistance Use by Sample Municipality.

Notes: The plots show the probability of living in a family in which someone received social assistance in the total population in Helsinki (municipality number 91) and in the 27 potential comparison municipalities. Municipality-year observations having susceptible values of health care contacts are highlighted by gray. These municipalities are excluded from the analysis sample. They were identified as follows: 1) compute a distribution of mean contacts by permutationally dropping every combination of four consecutive months, and 2) mark an observation to be invalid if its value is less than 40% of the largest observed mean (July was not considered because the health care supply is considerably reduced due to vacations).

GP Visits ED Visits

Specialist Consultations Social Assistance Use

Area ■ Treated: Helsinki ■ Comparisons
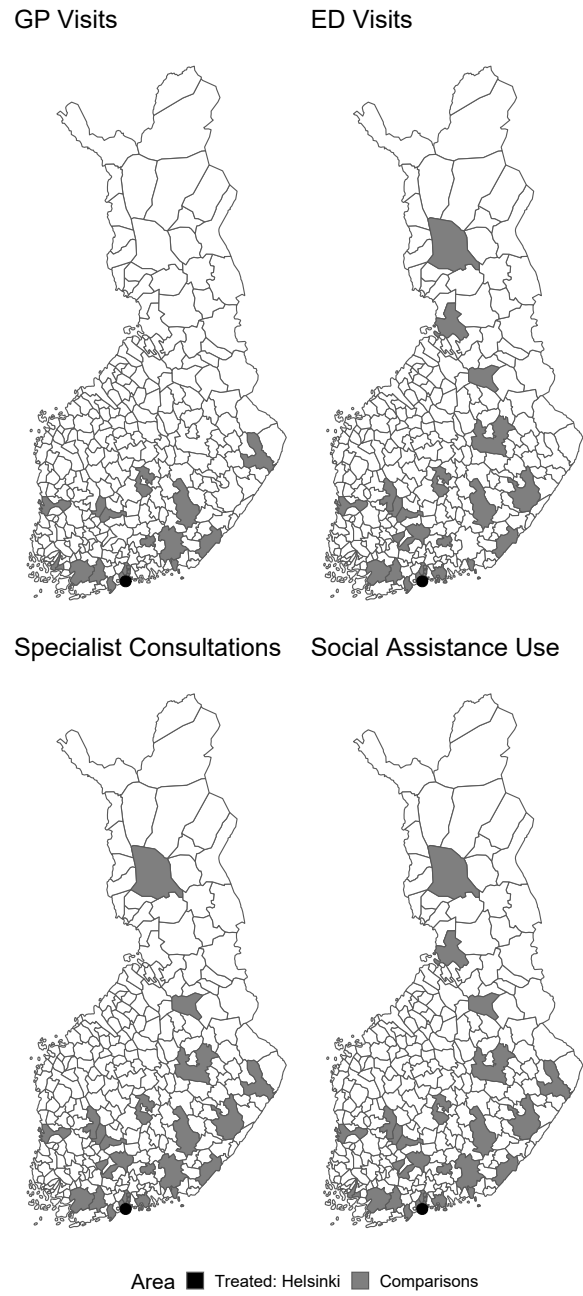
Figure A6: Sample Municipalities on the Map.

Notes: The plot illustrates where our sample municipalities, that depend on the outcome, locate. See Section 3 on how the sample municipalities were chosen.
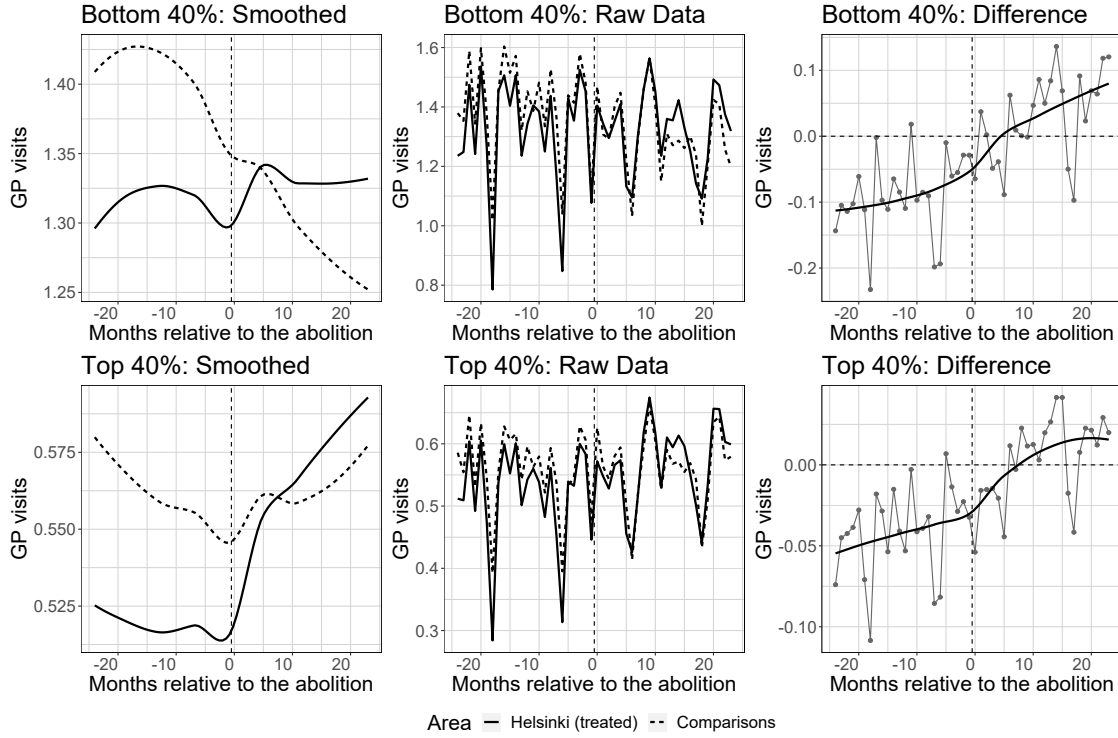
Figure A7: Trends in GP Visits by Income Group.

Notes: The outcome is the number of annualized GP visits per capita. We show 1) smoothed conditional means fitted with local linear regression, 2) the raw data, and 3) the difference in outcomes between Helsinki and the comparison areas. The sample is described in Section 3. We use the distribution of equivalized family disposable income to extract the bottom 40% and the top 40%.
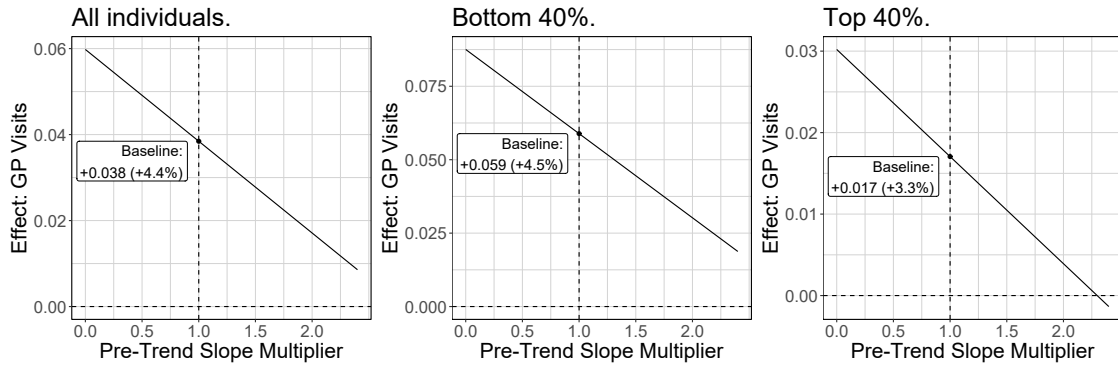
Figure A8: DD Estimates: GP Visits, and Sensitivity to the Parallel Trends Assumption.

Notes: We estimate Specification 1 but with data aggregated at the municipality level and weighted by population size. The effects represent the estimated change in the number of annualized GP visits in a two-year follow-up. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. Before effect estimation, we remove a linear pre-trend difference from the data by estimating it on the pre-abolition data. Then, we transform the outcome variable by subtracting the estimated trend difference. The figure shows the sensitivity of the estimates to assumptions on how the trend difference would have evolved in post-treatment periods. Specifically, we use different multipliers of the trend difference for post-treatment periods. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.
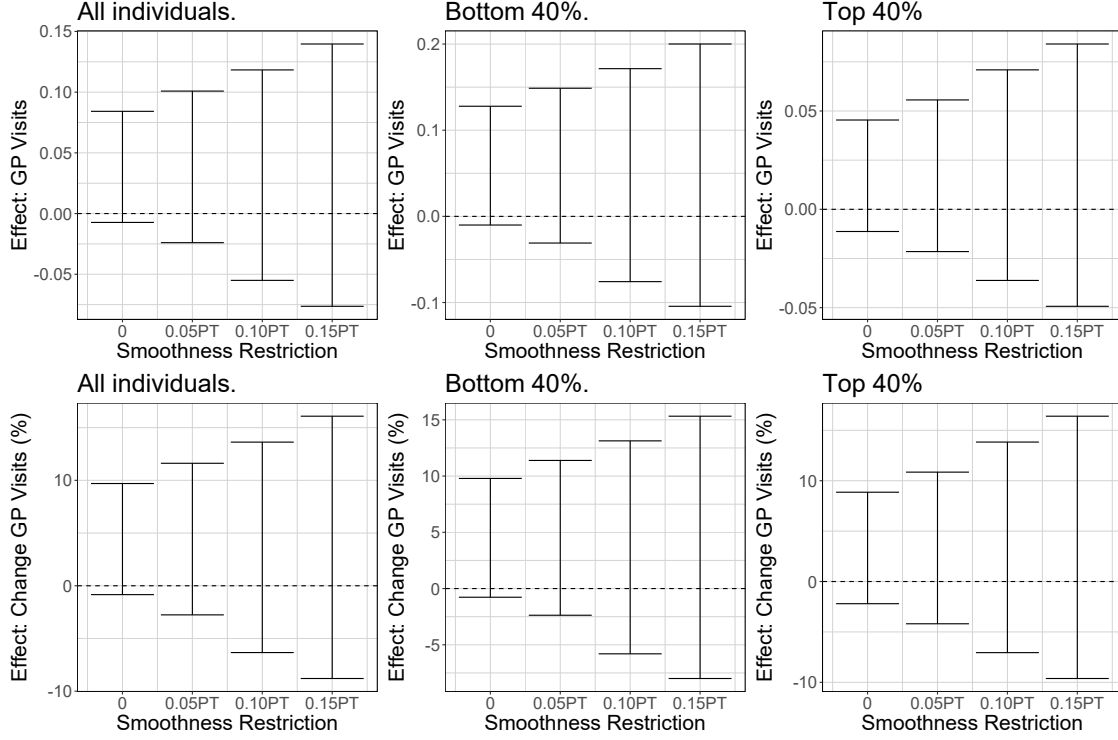
Figure A9: DD Estimates: GP Visits, and Bounding Pre-Trends.

Notes: We apply the method proposed by Rambachan and Roth (2022), estimated with the R package *HonestDiD*, to construct confidence sets by bounding pre-trends. The 10% significance level is used. First, we estimate a population-weighted event study specification that includes dynamic treatment indicators for Helsinki, normalized at time $t = -1$, and municipality and time fixed effects. The IID assumption is used for the variance-covariance matrix. The data are at the municipality-by-month level. We then use the "second derivative" smoothness restriction ($\Delta^{SD}(M)$) and construct fixed length confidence intervals (FLCIs) for the average of the estimated post-treatment effects. M represents how much the slope can deviate from linearity between consecutive periods. $M = 0$ means that exact linearity is assumed. Our remaining M values are derived from multiplying the estimated slope of the linear pre-trend difference by, e.g., 0.10 (0.10PT). The effects represent the estimated change in the number of annualized GP visits in a two-year follow-up. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.
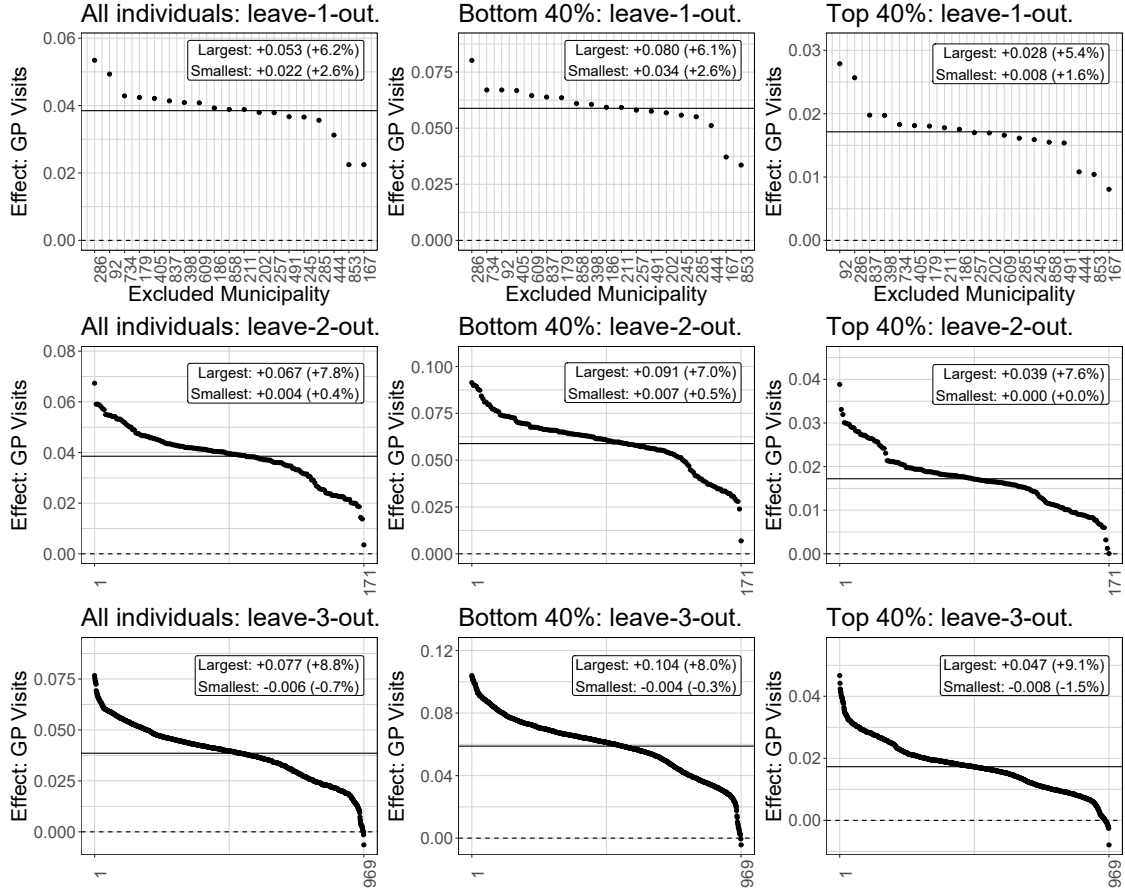
Figure A10: DD Estimates: GP Visits, and Leave-X-out Estimation.

Notes: We exclude each X-municipality combination ($X \in \{1, 2, 3\}$) from the comparison permutatively and estimate Specification 1 but with data aggregated at the municipality level and weighted by population size. The effects represent the estimated change in the number of annualized GP visits in a two-year follow-up. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean. Before effect estimation, we remove a linear pre-trend difference from the data by estimating it on the pre-abolition data. Then, we transform the outcome variable by subtracting the estimated trend difference. Bottom 40% and top 40% are based on the equivalized family disposable income distribution.
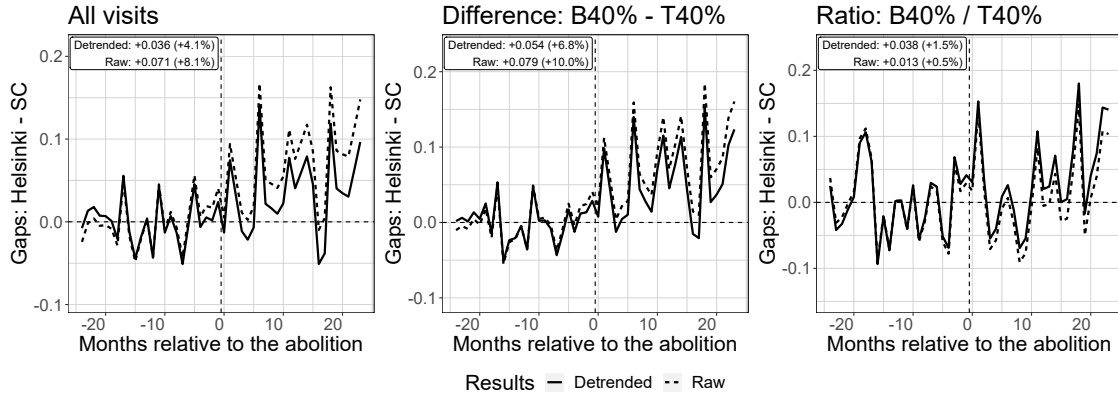
All visits

Difference: B40% - T40%

Ratio: B40% / T40%

Figure A11: Synthetic Control Estimates: GP Visits, Leave-Two-Out Estimation.

Notes: We permutatively exclude all two-donor combinations from the donor pool (leave-two-out), estimate the synthetic control, and average the results. The plots show the difference in outcomes between Helsinki and its synthetic control (gaps). We include in the donor pool municipalities with more than 40,000 sample individuals. Pre-treatment lags are used as matching variables. We subtract from each municipality its pre-treatment outcome mean (demeaning) before estimation. B40% and T40% refer to the bottom 40% and the top 40% of the equivalized disposable income distribution. The detrended results show the gaps after subtracting a linear pre-trend difference. In the top left corner, we show aggregated treatment effect estimates from averaging all post-treatment gaps. The pre-abolition mean is computed in Helsinki for 2012, and the change in percentage terms compares the estimate to this mean.
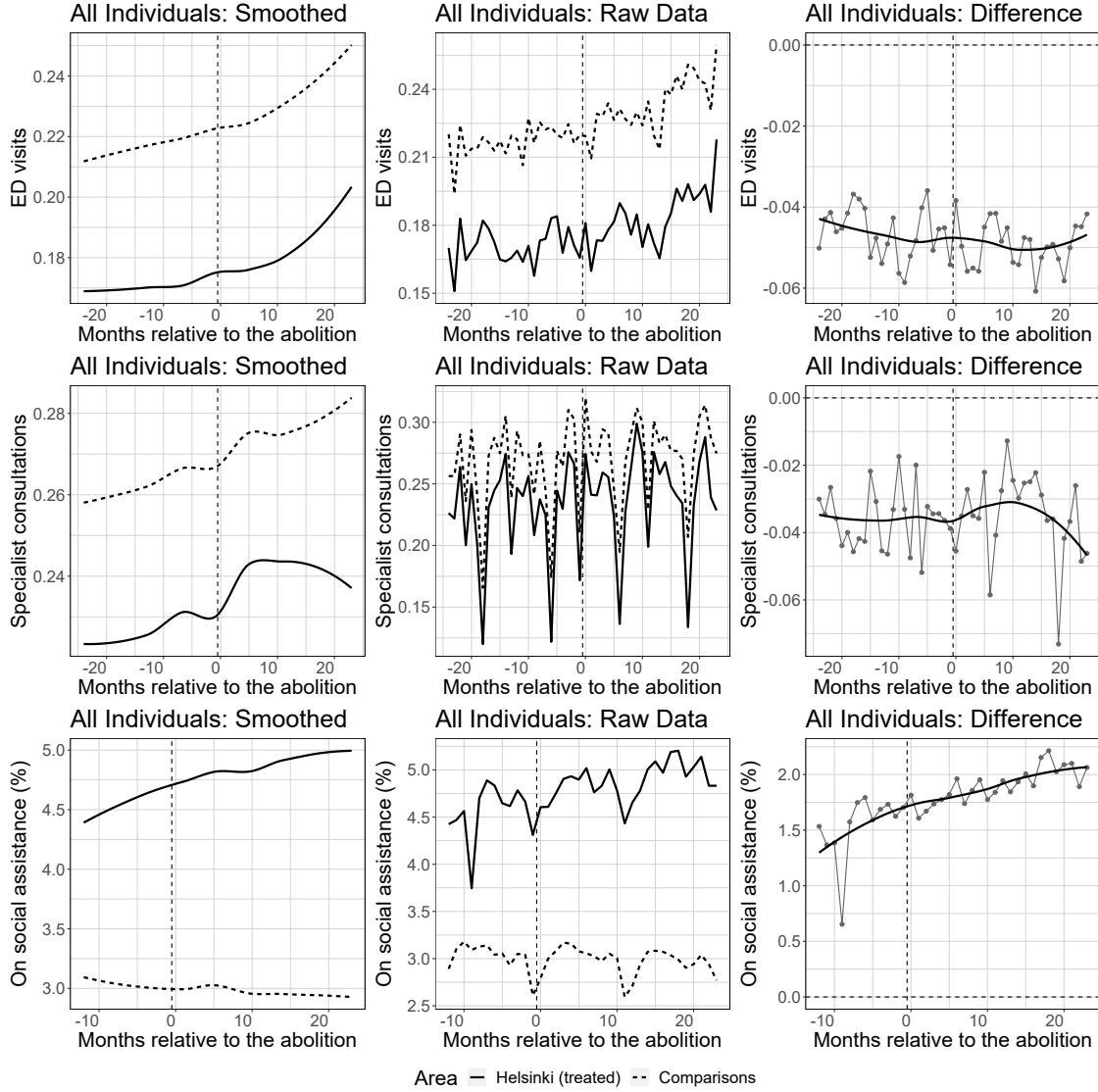
Figure A12: Trends in ED Visits, Specialist Consultations, and Social Assistance Use.

Notes: The outcomes are the number of annualized ED visits and specialist consultations per capita, and the probability of living in a family in which someone received social assistance. We show 1) smoothed conditional means fitted with local linear regression, 2) the raw data, and 3) the difference in outcomes between Helsinki and the comparison areas. The sample is described in Section 3.
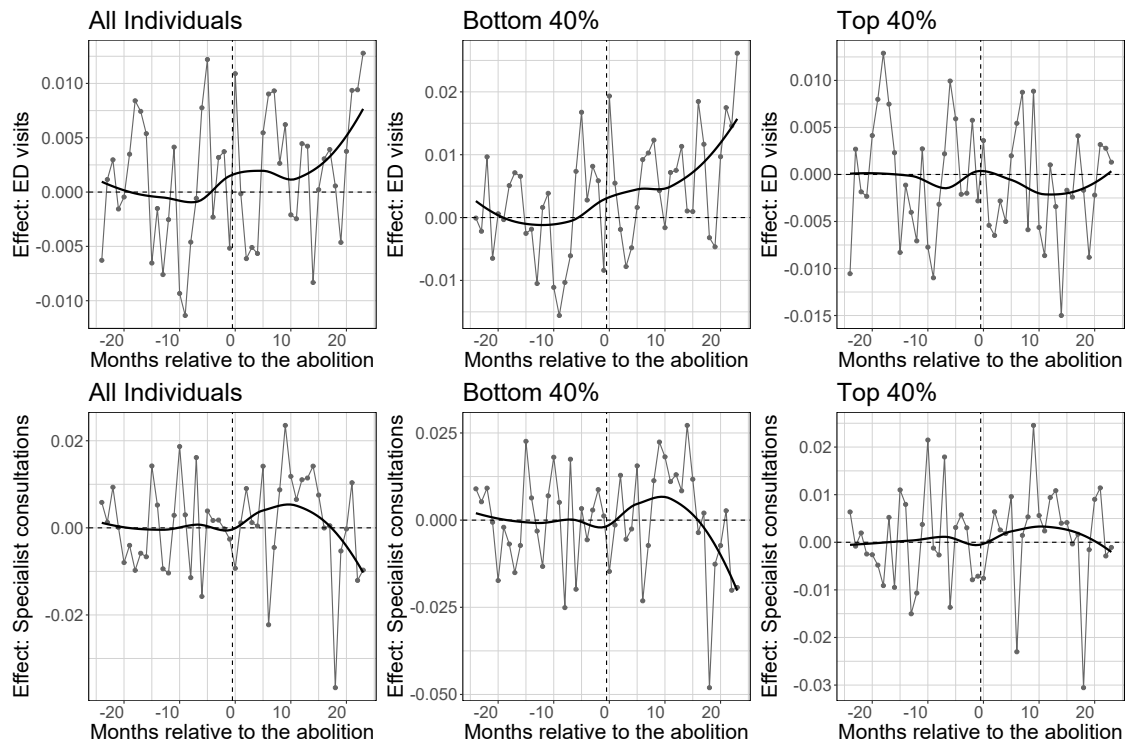
Figure A13: Trends in ED Visits and Specialist Consultations after Removing a Linear Pre-Trend Difference.

Notes: We show the difference in outcomes between Helsinki and the comparison areas after subtracting a linear pre-trend difference from the outcomes, estimated with OLS using only pre-abolition data. The plot shows the raw difference and its smoothed conditional mean, fitted with local linear regression. We use the distribution of equivalized family disposable income to extract the bottom 40% and the top 40%.
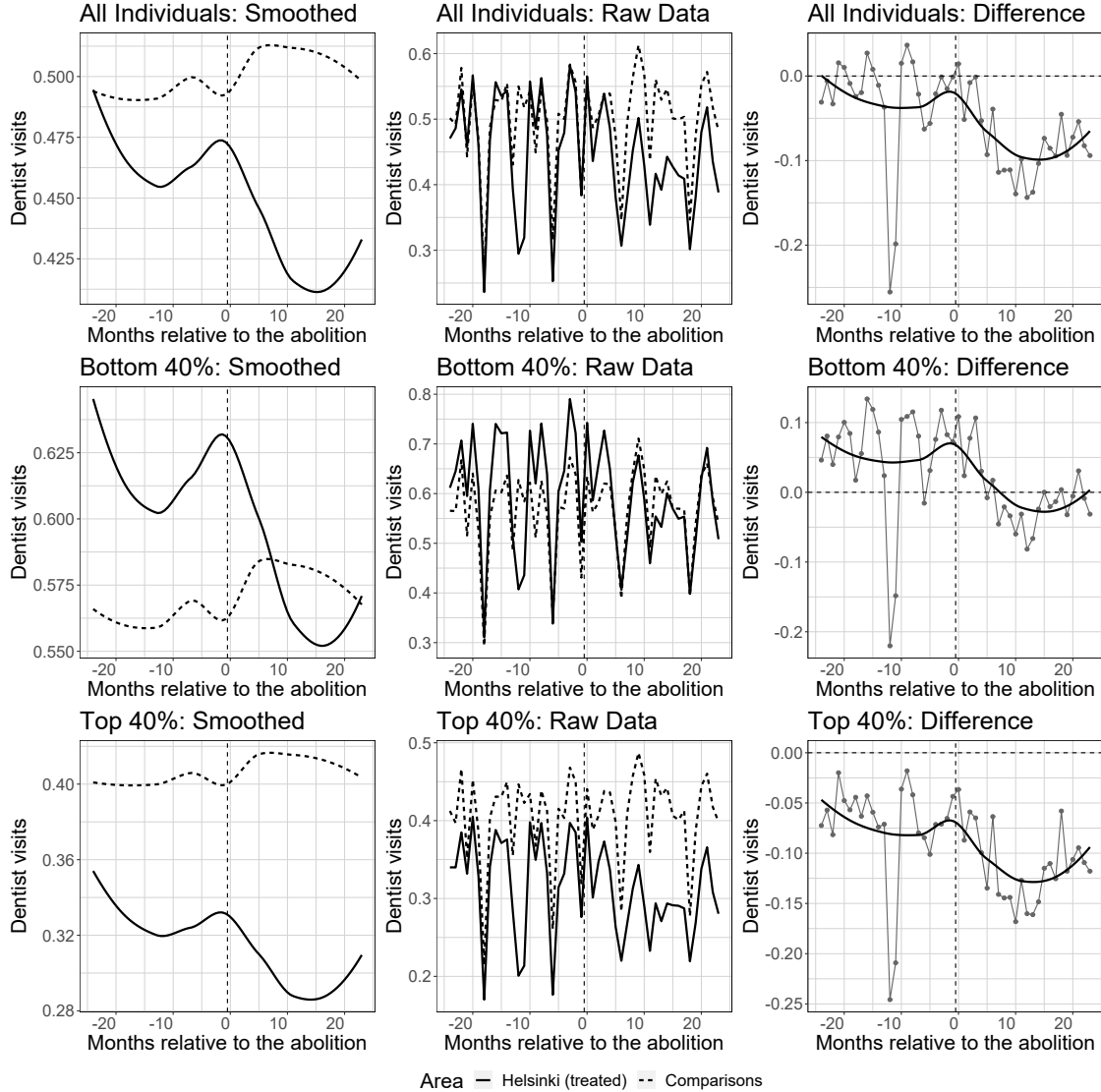
Figure A14: Trends in Dentist Visits.

Notes: The outcome is the number of annualized dentist visits per capita. We show 1) smoothed conditional means fitted with local linear regression, 2) the raw data, and 3) the difference in outcomes between Helsinki and the comparison areas. The sample is described in Section 3. We use the distribution of equivalized family disposable income to extract the bottom 40% and the top 40%.
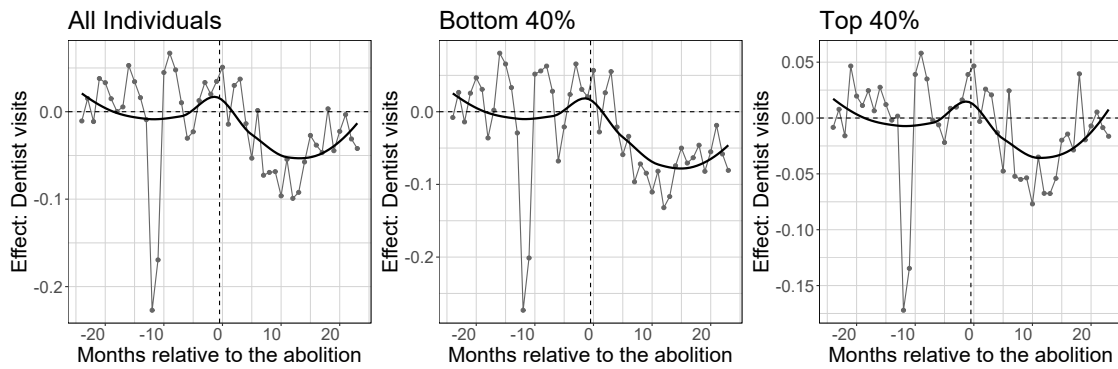
Figure A15: Trends in Dentist Visits after Removing a Linear Pre-Trend Difference.

Notes: We show the difference in dentist visits between Helsinki and the comparison areas after subtracting a linear pre-trend difference from the outcomes, estimated with OLS using only pre-abolition data. The plot shows the raw difference and its smoothed conditional mean, fitted with local linear regression. We use the distribution of equivalized family disposable income to extract the bottom 40% and the top 40%.