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Research Article

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Does Abolishing a Copayment Increase Doctor Visits? A Comparative Case Study

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Abstract: Insurance coverage increases health care consumption, but less is known whether moderate copayments affect adults' primary care utilization in a system characterized by gatekeeping. We analyze whether abolishing a 14-euro copayment for visits to general practitioners (GP) in Helsinki, the capital of Finland, increased the number of GP visits among adults and 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 all adults). The increase is driven by low-income adults (+0.06 visits, or +4.5 %, at the bottom 40 %). Although the point estimates are rather robustly positive, the conclusions regarding the statistical significance are sensitive to how we account for clustering in a setting characterized by only one treated cluster and a finite number of comparison clusters.

Keywords: cost-sharing; copayment; out-of-pocket costs; primary care use; general practitioner; difference-in-differences

JEL Classification: I18; I14; I13; H42; I11

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1 Introduction

Out-of-pocket costs reduce health care utilization (Einav and Finkelstein 2018). Most of the literature is based on variation in insurance coverage, but studies have also exploited variation in cost-sharing schemes, such as coinsurance rates, deductibles, and copayments. Copayments have potentially useful features as a policy instrument. They are transparent and understandable for patients, easy to bill, and they can yield fiscal revenue. Their level is usually low, which mitigates financial risks to patients but generates less revenue. Copayments are widely utilized in tax-funded public healthcare systems, including the Nordic countries.[1] A central policy concern is that fixed copayments may create a greater barrier to access for low-income patients, who are on average sicker and need more services.

We examine whether low copayments affect primary care general practitioner (GP) use in a system where patients are triaged by nurses at the entry (gatekeeping) and waiting times vary for non-urgent visits. Our interest is in the potential heterogeneity of the effects by income level. The capital Helsinki, Finland's most populous city, abolished its GP visit copayment of 14 euros in January 2013 to reduce health inequality. Using a difference-in-differences (DD) design and the fact that other municipalities continued to charge the copayment, we examine the effects of the abolition on public primary care GP use (our primary outcome), emergency department (ED) visits, specialist consultations, and social assistance use (a last-resort benefit that also covers healthcare costs) based on administrative register data from 2011 to 2014. The synthetic control method complements the DD results.

We find that the abolition is associated with a small increase in GP visits in Helsinki (+0.04 visits annually, or +4.4 %, for all sample adults) after subtracting an increasing linear pre-trend difference. The overall estimates are driven by low-income individuals, showing 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 less clear or unobservable in relative terms. The effect sizes increase (decrease) if the increasing pre-trend difference is assumed to slow down (accelerate) in the post-treatment periods. Overall, the quantitative magnitude of the estimates is modest or small. Consistent with the small effects for our primary outcome, we do not find significant effects for our secondary outcomes.[2]

Statistical inference is challenging in our setting due to the availability of only one treated cluster and a finite number of comparison clusters. Our inference

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1 Denmark is currently the only Nordic country that does not charge a copayment for primary care general practitioner (GP) visits. In Sweden, some regions do not charge the GP visit copayment.

2 However, we observe an unexpected reduction in dentist visits, a potential placebo outcome, of similar magnitude to the increase in GP visits.

results are inconclusive without strict and seemingly implausible assumptions. Although the point estimates are rather robustly positive, the conclusions on statistical significance 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 (Table 1).

Previously, five studies have examined the impacts of GP visit copayments of 10–18 euros on GP use in the Nordic countries (Beck Olsen and Melberg 2018; Haaga et al. 2023a; Johansson, Jakobsson, and Svensson 2019; Magnussen Landsem and Magnussen 2018; Nilsson and Paul 2018). They all focus on children or adolescents and exploit the fact that adolescents under a given age are exempted from copayments. Four of the studies employ an age-based regression discontinuity (RD) design, while Beck Olsen and Melberg (2018) construct a synthetic control for individuals aged 12–15 from other age cohorts. In the RD studies, the number of GP visits decreases by 4–12 % after the copayment is charged. Beck Olsen and Melberg (2018) report large estimates for free care: +22 % for women and +14 % for men.

Our study relates to these studies, but we examine the effects for the whole adult population and not only for those aged 12–15 or for adolescents at a specific birthday. Moreover, we study the impacts of a copayment abolition (i.e. a policy change) instead of individuals aging out of an exemption. As age-based policy rules are foreseeable, such schemes may be more sensitive to the strategic behavior of individuals who decide when to contact primary care. Besides the above-mentioned studies focusing on adolescents, Jakobsson and Svensson (2016) report that a 33 % increase (circa 5 euros) in the GP visit copayment did not affect the total number of GP visits in an 8-month follow-up in Sweden. While their exposure is a change in the intensity of the copayment, we examine a copayment abolition. The distinction between an adoption and an abolition is also relevant, as the effects of increased and decreased out-of-pocket costs may not be symmetric (Hayen, Klein, and Salm 2021; Iizuka and Shigeoka 2023; Remmerswaal et al. 2019).

Furthermore, we contribute to the understanding of whether low-income individuals respond more strongly to changes in cost-sharing. Previously, Nilsson and Paul (2018) and Johansson, Jakobsson, and Svensson (2019) have found that patients at the lower end of the income distribution are more sensitive to copayments than high-income individuals in both absolute (the number of visits) and relative (compared to baseline utilization) terms, while Haaga et al. (2023b) report heterogeneity but only in absolute terms. In Haaga et al. (2023a), the evidence for heterogeneity by income is weaker.

Section 2 introduces the context and Section 3 the data. Section 4 presents our empirical approach and Section 5 the results. Section 6 concludes. Our Appendix contains additional figures and tables.

2 Institutional Background

Primary care for Finnish adults is provided by three sectors, targeting different subpopulations and differing by the level of out-of-pocket costs, waiting times and the strictness of gatekeeping. Public primary care charges copayments of about 14 euros for GP visits. Waiting times for non-urgent conditions can be long. There is also gatekeeping at the point of entry and in accessing specialists. Patients are triaged by nurses when they call or visit their health station, determined by their address. Pensioners, low-income individuals, and the unemployed disproportionately rely on public primary care. In contrast, employed people often prefer occupational healthcare, which is free of charge, or private clinics over public primary care due to faster access and less or no gatekeeping.

Municipalities organize public primary care on their own or in cooperation with other municipalities. The services are financed by state transfers, municipal taxes, copayments (a minor share), and borrowing. The state sets the upper limits for copayments and determines which services and groups are nationally exempted. Helsinki charged a copayment of 13.80 euros for the first three GP visits annually before abolishing it in January 2013. Its health care services committee assumed (6 March 2012) that the copayment abolition would reduce health inequality.[3] The committee also noted that many patients of public primary care are pensioners or unemployed and thus not entitled to occupational healthcare that is free of charge. Minors, war veterans and war invalids were exempted already before the abolition, and copayments were not charged for preventive services, such as health checks. Our comparison municipalities had a similar per-visit copayment or an annual copayment which was twice the amount of 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.

3 Data

We combine Finnish national-level administrative registers to construct five outcomes:[4] GP visits in public primary care (the primary outcome), ED visits and

3 The mechanism was not stated explicitly, but we assume the aim was to increase primary care use at the lower end of the income distribution.

4 Public primary care contacts are from the Register of Primary Health Care Visits, specialized healthcare contacts from the Care Register for Health Care, and social assistance data 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).

specialist consultations at hospitals, an indicator of belonging to a family where someone received social assistance[5] (the secondary outcomes), and dentist visits in public primary care (a plausible placebo). We also use publicly available data on municipal copayment policies (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 2011 to 2014. Individuals and their visits are linked to policies using the municipality of residence.[6] We first exclude small municipalities with less than 30,000 residents in 2012, with 36 municipalities remaining.[7] We exclude municipalities with missing copayment policy or which changed from a per-visit copayment to an annual copayment (or *vice versa*) in 2013–2014. One municipality (Espoo) is 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. This leaves us with 380,000 people in Helsinki and 1.35 million people in the 27 comparison areas.

An individual may have had multiple contacts on a given day, but we treat these as one visit. We only include GP and dentist visits from Monday to Friday to reduce the share of acute visits outside of normal office hours, which have a different copayment. Specialist consultations do not include repeated visits to treat the same health problem.

The final sample sizes vary across outcomes as some municipalities are excluded for data quality reasons. In a DD design, missing visits correlated with the treatment would bias the results. Two types of data quality concerns are noteworthy here. First, the register on primary care contacts started in its current form in 2011. Not all areas transferred high-quality data to the register at the beginning.[8] Second, changes in municipal IT systems may have led to drastic but brief reductions in observed contacts.

We detect and exclude municipalities with data quality concerns as follows: (1) compute a distribution of mean contacts by permutationally excluding every

5 Social assistance is a last-resort benefit for those with low income and little wealth to cover basic living expenses.

6 Only since 2014 have individuals had an option to choose another public primary care provider than the one determined by municipality of residence. However, these changes have been rare.

7 Helsinki is by far the largest municipality in Finland with 600,000 residents in 2012. We exclude small and rural municipalities where primary care demand and supply may differ considerably from Helsinki.

8 For instance, the number of GP visits was suspiciously low in Rovaniemi (698) in 2011 in Figure A1.

combination of four consecutive months, (2) mark an observation as invalid if its value is less than X % of the largest observed mean (July is not considered due to holidays), and (3) exclude municipalities with invalid observations. The threshold X varies by outcome based on what we think works well in detecting outliers. 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 outcomes and highlight the detected municipality-year observations 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. 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 Empirical Approach

We use a difference-in-differences (DD) design, comparing individuals in Helsinki to individuals living in comparison municipalities that continued to charge the GP visit copayment. The key identifying assumption 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 an increasing pre-trend in GP visits in Helsinki relative to the comparison units. The same pattern also exists separately at the bottom 40 % and the top 40 % of the income distribution (Figure A7). Helsinki is by far the largest and most urban municipality and in many ways an outlier (Table A1), so the trend difference is not a complete surprise.

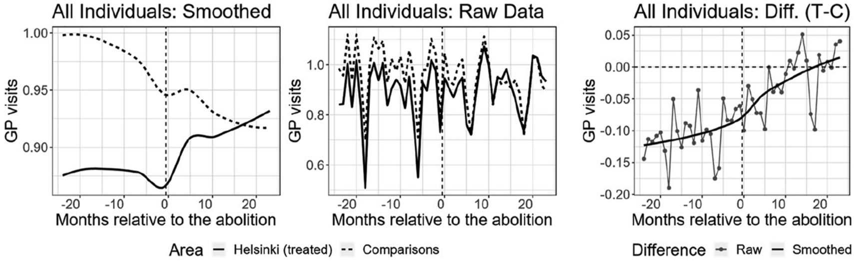

Figure 1: Trends in GP visits. *Notes*: The outcome is the number of annualized GP visits per capita. We show (1) smoothed conditional means fitted with local linear regression, (2) the raw data, and (3) the difference in outcomes between Helsinki and the comparison areas. The sample is described in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

We make a modified PTA to account for diverging pre-trends: we assume that the PTA holds after subtracting a linear pre-trend difference from the data (detrending). That is, there should be no time-variant confounders once a linear pre-trend difference is eliminated. Specifically, we fit a linear trend difference in time with OLS 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.[9] Our baseline assumption is that the trend difference does not change in post-treatment periods. Still, we examine the sensitivity of the results to the trend difference slowing down or accelerating by changing the slope of the estimated pre-trend difference for the post-treatment periods.

Figure 2 shows that the difference in GP visits between Helsinki and the comparisons varied around zero in pre-treatment periods after removing the estimated linear trend difference.[10] GP visits increased modestly in Helsinki after the copayment abolition relative to the comparison municipalities. This increase is driven by the lower end of the income distribution. We fit the following regression model using the detrended data:

$$y_{imt} = \alpha + \beta_1 \,\mathrm{Post}_t + \beta_2 \,\mathrm{Treat}_m \times \mathrm{Post}_t + \gamma_m + \varepsilon_{imt} \tag{1}$$

where 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.

4.1 Inference

The setting is challenging inference-wise; see Roth et al. (2023) for a discussion on advances in econometrics for the DD context with a small number of clusters. In principle, it is advisable to cluster standard errors at the municipality level, given that treatment assignment occurs at that specific level (Abadie et al. 2023). However, we have only one treated cluster and a finite number of smaller comparison clusters. Thus, the conventional asymptotic methods for estimating clustered standard errors are not applicable. Hagemann (2020) provides a rearrangement procedure to conduct inference in a setting like ours, but the approach requires no cluster-specific heterogeneity in trends in untreated potential outcomes so that any single

9 The same idea has been earlier used by Bhuller et al. (2013) and Goodman-Bacon (2021). An alternative is to control for a linear pre-trend difference in one regression (Bilinski and Hatfield 2020).

10 GP use appears to be low in Helsinki in June or July relative to the comparisons, explained by holidays.

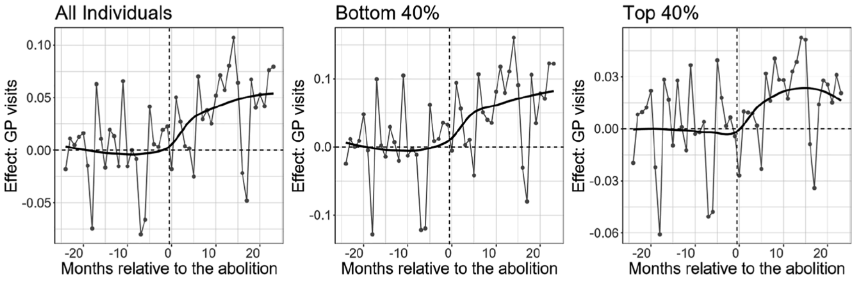

Figure 2: Trends in GP visits after removing a linear pre-trend difference. *Notes*: We show the difference in outcomes between Helsinki and the comparison areas after subtracting a linear pre-trend difference from the outcomes, estimated with OLS using only pre-abolition data. The plot shows the raw 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 %.

untreated cluster could be used as a counterfactual. This strict assumption does not seem to be valid in our application even after removing a linear pre-trend difference from each sample municipality (Table A2). Alternatively, we could increase the number of clusters by using postal code areas, but that is not the level of the treatment assignment, and the postal code area is often missing.[11]

With no ideal 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, Duflo, and Mullainathan 2004).[12]

11 The postal code area is obtained by (1) reading all public primary care contacts from 2011 to 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 where the person has multiple observed postal codes (18 % of the individuals). The postal code area information is missing for 34 % of the population in Helsinki and for 22 % in the comparison areas. For these individuals, we use the municipality of residence as the cluster.

12 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).

4.2 Effect Heterogeneity by Income Level

We estimate a triple difference (DDD) model to test whether low-income individuals respond more strongly to the copayment abolition, comparing the outcomes of the bottom 40 % of the income distribution to that at the top 40 % in both Helsinki and comparison areas. As the baseline, 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. The PTA after detrending is now assumed in ratios concerning the relative outcomes of the income groups (Olden and Møen 2022). We estimate the following specification:

$$\begin{aligned} y_{igmt} = \alpha &+ \beta_1 \text{Helsinki}_m + \beta_2 \text{Affected}_g + \beta_3 \text{Post}_t + \beta_4 \text{Helsinki}_m \\ &\times \text{Affected}_g + \beta_5 \text{Helsinki}_m \times \text{Post}_t + \beta_6 \text{Affected}_g \times \text{Post}_t \\ &+ \gamma \text{Helsinki}_m \times \text{Affected}_g \times \text{Post}_t + \varepsilon_{igmt} \end{aligned} \tag{2}$$

where 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 bottom 40 % of the income distribution, and γ is the coefficient of interest.

4.3 Complementary Synthetic Control (SC) Analysis

We use the demeaned SC estimator (Ferman and Pinto 2021) by subtracting the pretreatment outcome mean from each municipality before computing the weights for available comparison units that optimize pre-treatment fit (MSE) in the matching variables between the treated area and the SC, assuming no time-varying confounders. The matching variables include all pre-treatment outcome values (Ferman, Pinto, and Possebom 2020). The weights are restricted to be positive and sum up to one to avoid extrapolation. To reduce the risk of overfitting, we only include donors with at least 40,000 sample individuals, resulting in 9 donors for the GP visits outcome.

5 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

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, Duflo, and Mullainathan 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.

is significant in four cases out of six at the 10 % level 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.

5.1 Sensitivity of the Estimates

The effect estimates grow (attenuate) if we assume that the trend difference would have slowed down (accelerated) after the abolition (Figure A8). Here, we multiply the estimated pre-treatment trend slope with different values and use the transformed slope for post-treatment periods. Our estimates are positive for all sensible multipliers and remain so even if the slope of the pre-trend difference doubled. 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 (Table A3).

Similarly, 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 as proposed by Rambachan and Roth (2023), varying how much the slope of the trend difference is allowed to deviate from linearity between consecutive periods (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 %. Once the exact linearity is relaxed, the confidence intervals widen considerably and contain larger negative values.

Figure A10 shows the robustness of the results to small changes in the comparison group. Based on the leave-one-out results, the estimates grow noticeably if either Vantaa (92) or Kouvola (286) are excluded and decrease if either Turku (853) or Joensuu (167) are excluded.[13] The estimates for all individuals vary between +0.02 (+2.6 %) and +0.05 (+6.2 %). The leave-two-out estimates for all individuals remain positive, while a couple of leave-three-out combinations out of 969 produce negative point estimates. Table A4 presents the results from using a monthly indicator of having any GP visits as the outcome and from replacing municipality fixed effects with postal code area fixed effects. The main findings are robust.

5.2 Effect Heterogeneity by Income Level

Figure 3 shows the effects by income decile: they are larger in absolute terms for low-income individuals, but such a pattern is not observable in relative terms (% changes). The DDD estimates are reported in Table 2. GP use increased by +0.04 (+3.2 %) annualized visits at the bottom 40 % relative to the top 40 % when the pre-trend difference is extrapolated to the post treatment periods (1.0 × slope; our baseline). Alternatively, assuming that there was no underlying trend difference in the post-abolition periods (0 × slope) yields a somewhat larger estimate: +0.06 (+4.4 %) visits. Similarly, assuming that the trend difference accelerated in the post-abolition periods (1.5 × 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 rejects at the 10 % level, but the WCR bootstrap does not reject in any case after detrending at the 5 % level.

13 We consider Vantaa to be an important comparison area as it belongs to the Helsinki metropolitan area and is large and urban. Turku as a large city is similarly an important comparison.

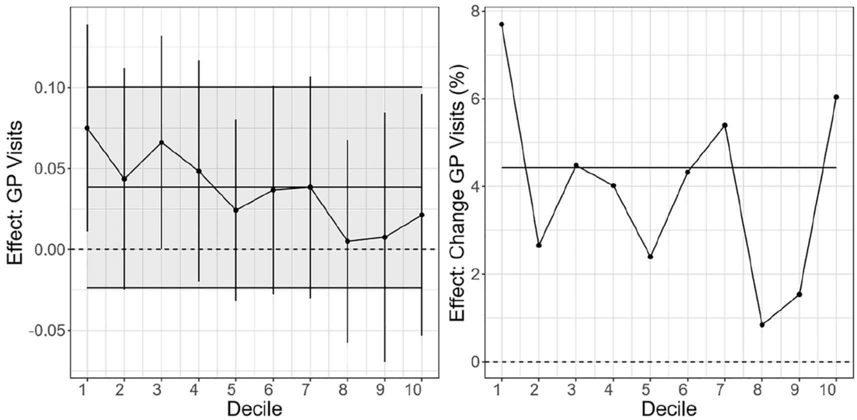

Figure 3: DD estimates by income decile. *Notes*: We estimate specification 1 and cluster standard errors by postal code area. The effects are shown by income deciles (equivalized family disposable income). On the left, the gray block centered at the black horizontal line shows the estimate and its confidence 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.

6 Supplementary Analyses

6.1 Synthetic Control Results

Figure 4 shows that there may be a small increasing pre-trend in GP visits for Helsinki compared to our synthetic control. We report both raw and detrended results, preferring the latter. The detrended results are computed by subtracting a linear pre-trend difference, estimated with OLS using the pre-treatment data, from the raw gaps. The detrended SC estimate for all individuals is +0.037 annualized visits (+4.3 %), essentially the same as the corresponding DD estimate of Table 1. The estimate without detrending is larger: +0.070 (+8.0 %). Figure 4 also shows the SC results on the difference and the ratio of GP visits between the bottom 40 % and the top 40 % of the income distribution. GP use increased in absolute terms by +0.054 annualized visits (+6.8 %) in the bottom 40 % compared to the top 40 % based on detrending (without detrending: +0.079 visits, or +10.0 %). In contrast, the point estimates are close to zero in relative terms. The SC results 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).

Table 2: DDD estimates: GP visits.

	No detrending	$0 \times$ slope	$1.0 \times$ slope	$1.5 \times$ slope
A. Outcome: the number of GP visits				
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				
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.

6.2 Secondary Outcomes

Given the small effects on GP visits, we expect null or small effects on our secondary outcomes. Figure A12 shows the trends in raw outcomes in Helsinki and the comparison areas. For social assistance use, there is a clear increasing trend in Helsinki. The slope of the pre-trend difference was larger before the abolition, but we think that few conclusions can be made regarding the use of social assistance. Figure A13 shows the detrended difference in ED visits and specialist consultations between

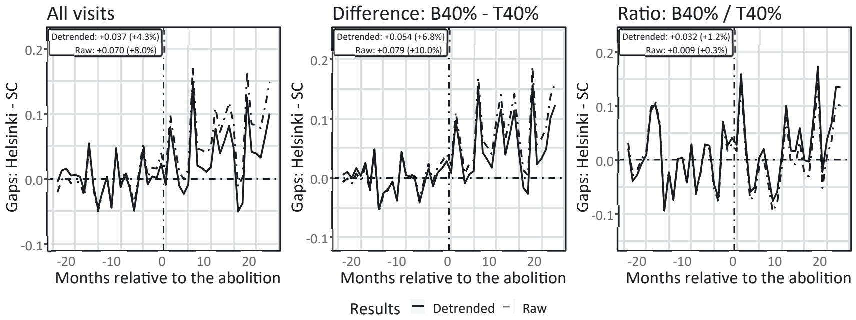

Figure 4: Synthetic control estimates: GP visits. *Notes*: The plots show the difference in outcomes between Helsinki and its synthetic control (gaps). The donor pool contains municipalities with more than 40,000 sample individuals. Pre-treatment lags are used as matching variables. The SC weights are 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.

Helsinki and the comparison areas. Nothing striking seems to happen after the abolition. In absolute terms, the changes are 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.

6.3 Placebo Outcome: Dentist Visits

Interestingly, dentist visits decreased in Helsinki after the GP visit copayment abolition relative to the comparison areas (Figures A14 and A15). This results from a reduction in dentist use in Helsinki, which has partially recovered since April 2014 relative to the comparison areas. The DD estimates on dentist visits in Table A7 are of similar magnitude in absolute terms than the estimates on GP visits, but with a different sign. The DDD estimates in Table A8 show that it is the lower end of the income distribution whose dentist use decreased more in Helsinki after the GP visit copayment abolition. We searched for possible explanations for the observed reduction in dentist use but did not find convincing candidates.[14] As dentist visits

14 A partial explanation is that Helsinki reduced the supply of vouchers for private dentist visits from July 2013 to the end of the year for budgetary reasons (Helsinki's social and healthcare services committee, September 17th, 2013). There were 7600 voucher visits in the first half of 2013, were *ex ante* a plausible placebo, detecting effects (and not a precise null) creates some doubts abouts our main results on GP visits.

6.4 Time Placebo

We estimate the effects of a placebo intervention using only pre-abolition data from 2011 to 2012 and proceed as if Helsinki abolished the copayment in January 2012 (Table A9). All other aspects of data processing and analysis remain fixed. The placebo estimates are negative (not positive) and closer to zero than the policy estimates of Table 1.

7 Conclusions

Overall, our finding that smaller out-of-pocket costs increase primary care use is in accordance with the earlier literature, but the magnitude of our estimates is modest. Our main estimate of the effect of the copayment abolition on the number of annualized GP visits for all individuals (+0.038 visits, or +4.4 %) maps to a semi-arc elasticity of 0.26.[15] For comparison, Nilsson and Paul (2018) report semi-arc elasticities of 0.88 at the 20th birthday and 0.55 at the 7th birthday for doctor visit copayments in Sweden. Moreover, Johansson, Jakobsson, and Svensson (2019) report a semi-arc elasticity of 1.11 at the 20th birthday in Sweden for GP visit copayments.[16] Regarding the heterogeneity of the effects by income level, we find some evidence to support the hypothesis of low-income individuals responding more strongly to copayments. However, this heterogeneity is only present in absolute terms (the number of visits) but not in relative terms (compared to the baseline). Our standard errors are sensitive to how we account for clustering. Depending on the inference method, confidence intervals can be rather narrow, or they can include large point estimates as well. We advise against strong conclusions on statistical significance.

The small effect sizes may be explained by several factors: gatekeeping at the entry, supply that is relatively insensitive to changes in demand, and by the fact

converting to 0.025 annualized visits per capita. However, the estimated reduction in 7/2013–3/2014 was much larger.

15 The semi-arc elasticity captures the change in GP visits, normalized by the baseline, divided by the price change (Brot-Goldberg et al. 2017): $((q1 - q0) / (q1 + q0)) / ((p1 - p0) / 2) = ((0.868 + 0.038 - 0.868) / (0.868 + 0.038 + 0.868)) / ((0 - 13.8/83) / 2)$. As in Nilsson and Paul (2018), 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).

16 We computed the elasticity from the estimates for all individuals in Table 1 in Johansson, Jakobsson, and Svensson (2019), using a copayment of SEK 100 and the total cost of SEK 1500 per visit, a figure appearing in the study.

that the copayment was charged only for the first three visits annually before the abolition. Our analysis does not account for some visits consequently having been free of charge. Future studies should formally test whether Finnish adults respond to the spot price change after the third GP or nurse visit of the calendar year. More specifically, such studies could test for discontinuities in the frequency of visiting primary care after the third visit of the year, also exploiting the staggered adoption of the nurse visit copayment in the 2010s and the abolition of the GP visit copayment in Helsinki in 2013. As another potential explanation for small effects, the effects of increased and decreased cost-sharing may not be symmetric. Indeed, some recent studies have concluded that framing the change as a loss may have larger effects than framing it as a gain (Hayen, Klein, and Salm 2021; Iizuka and Shigeoka 2023; Remmerswaal et al. 2019).

There are several limitations. Helsinki is in many ways unique in Finland, which can explain the observed pre-trend differences. Our point estimation relies on the detrended PTA, and statistical inference is complicated in the presence of only one treated cluster and a finite number of comparison clusters. Conclusions regarding the statistical significance are sensitive to how we account for clustering. Interestingly, the number of dentist visits (a potential placebo) decreased in Helsinki after the GP visit copayment abolition by a similar magnitude as GP use increased.

Our findings suggest that the abolition of a 14-euro copayment did not lead to a large increase in GP use. Unfortunately, we do not have data on waiting times. The copayment abolition was a clear improvement in terms of reduced costs for low-income individuals, including the unemployed and pensioners, who disproportionately rely on public primary care. In this sense, the policy reduced inequality in barriers to access. However, the policy most likely did not greatly reduce health inequalities, because the first-order effects on service use were so moderate.

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Supplementary Material: This article contains supplementary material (https://doi.org/10.1515/bejeap-2023-0056).

A Online Appendix: Additional Figures and Tables

Table A1: Background Statistics by Municipality Group.

Municipalities	Helsinki 1	Comparisons 19	The Rest 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.

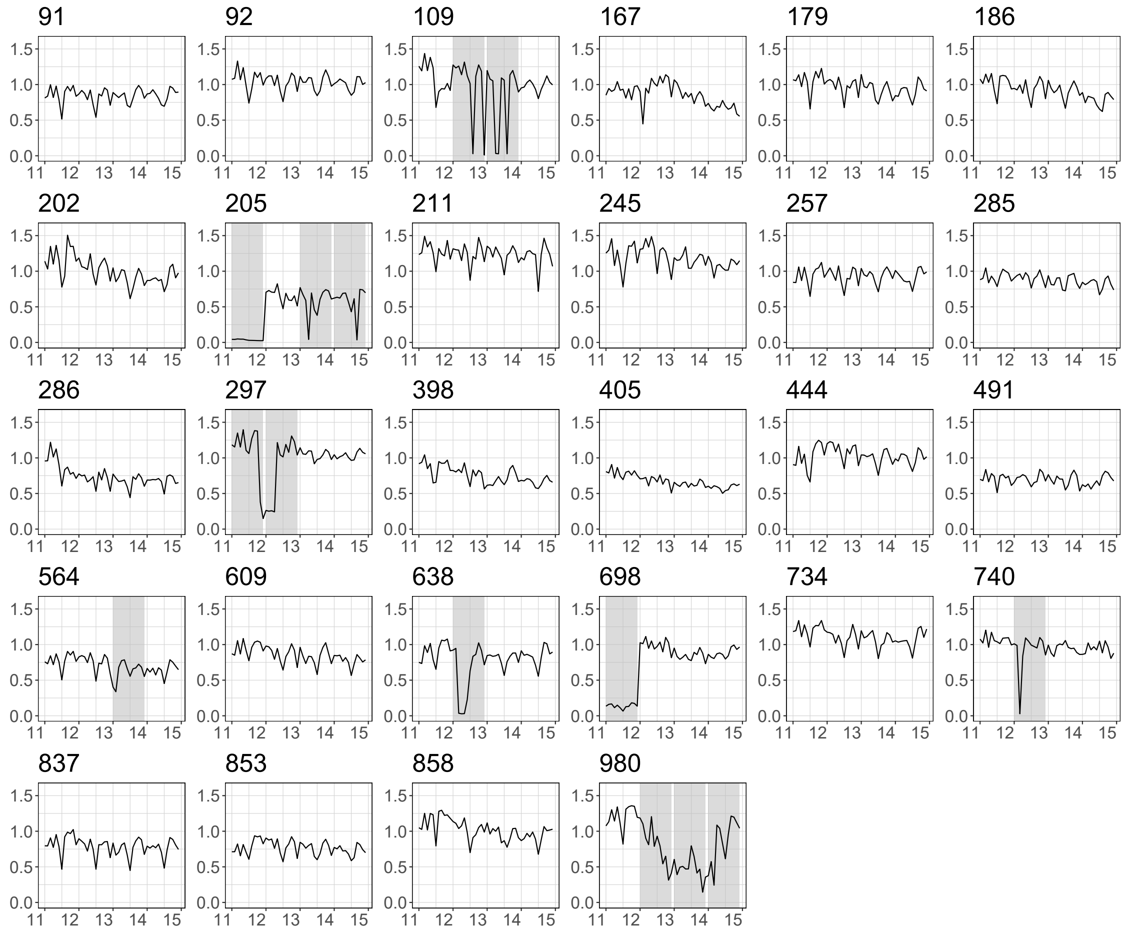

Figure A1: The Evolution of GP Visits by Sample Municipality.

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).

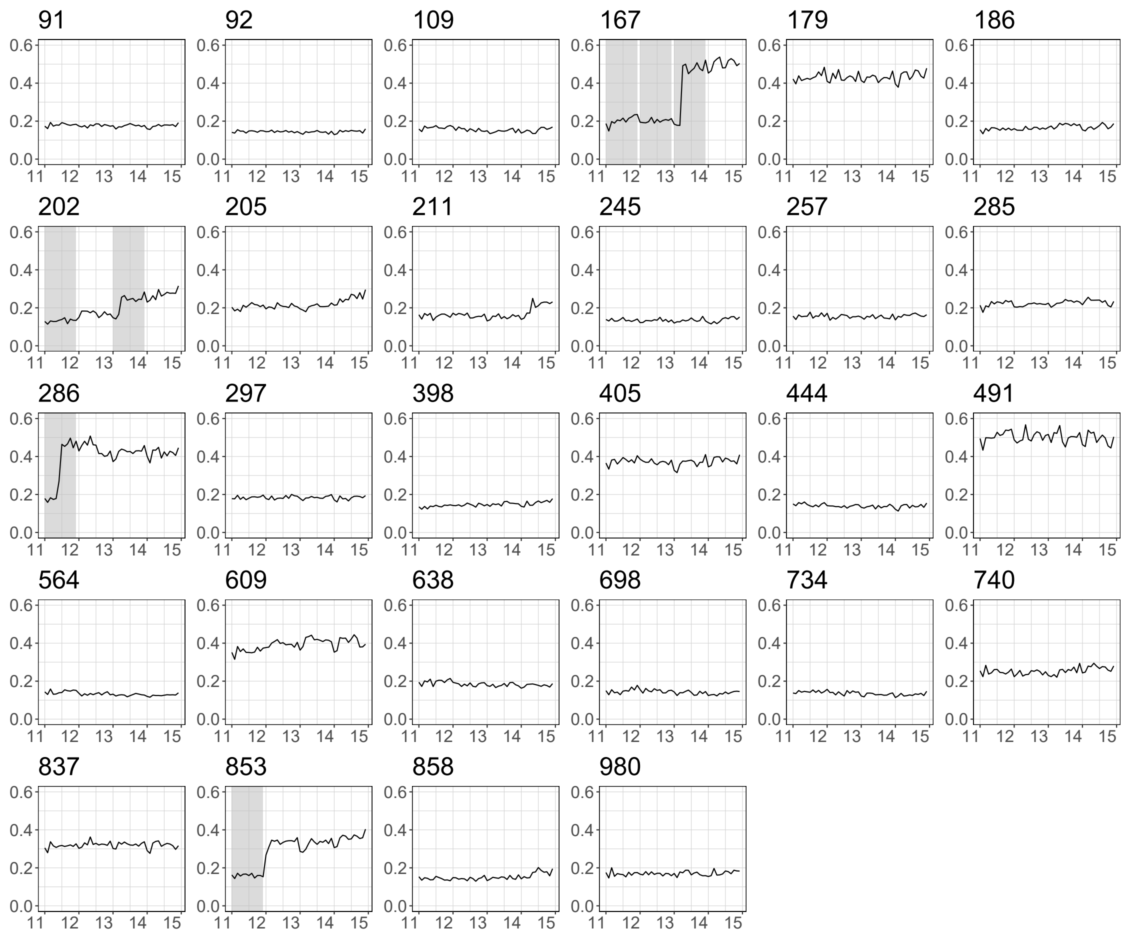

Figure A2: The Evolution of ED Visits by Sample Municipality.

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).

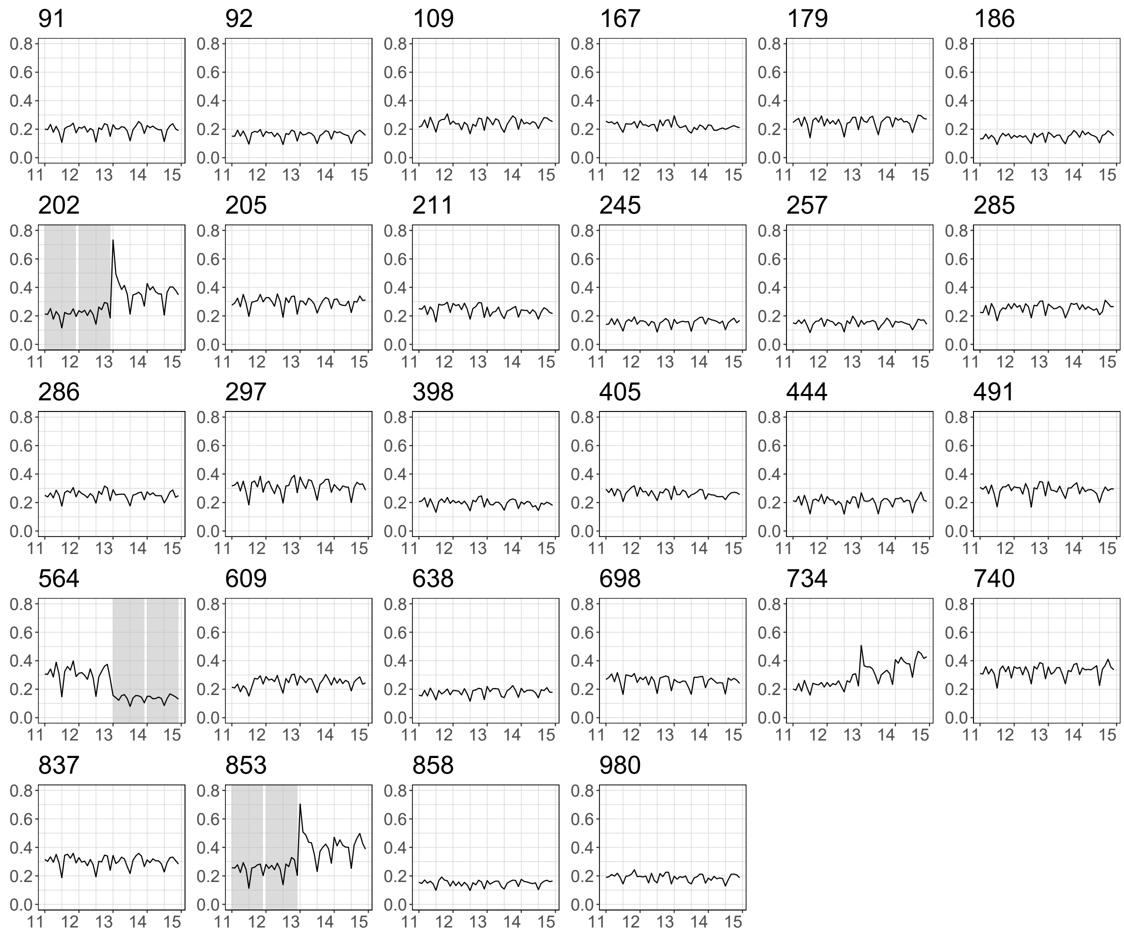

Figure A3: The Evolution of Specialist Consultations by Sample Municipality.

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).

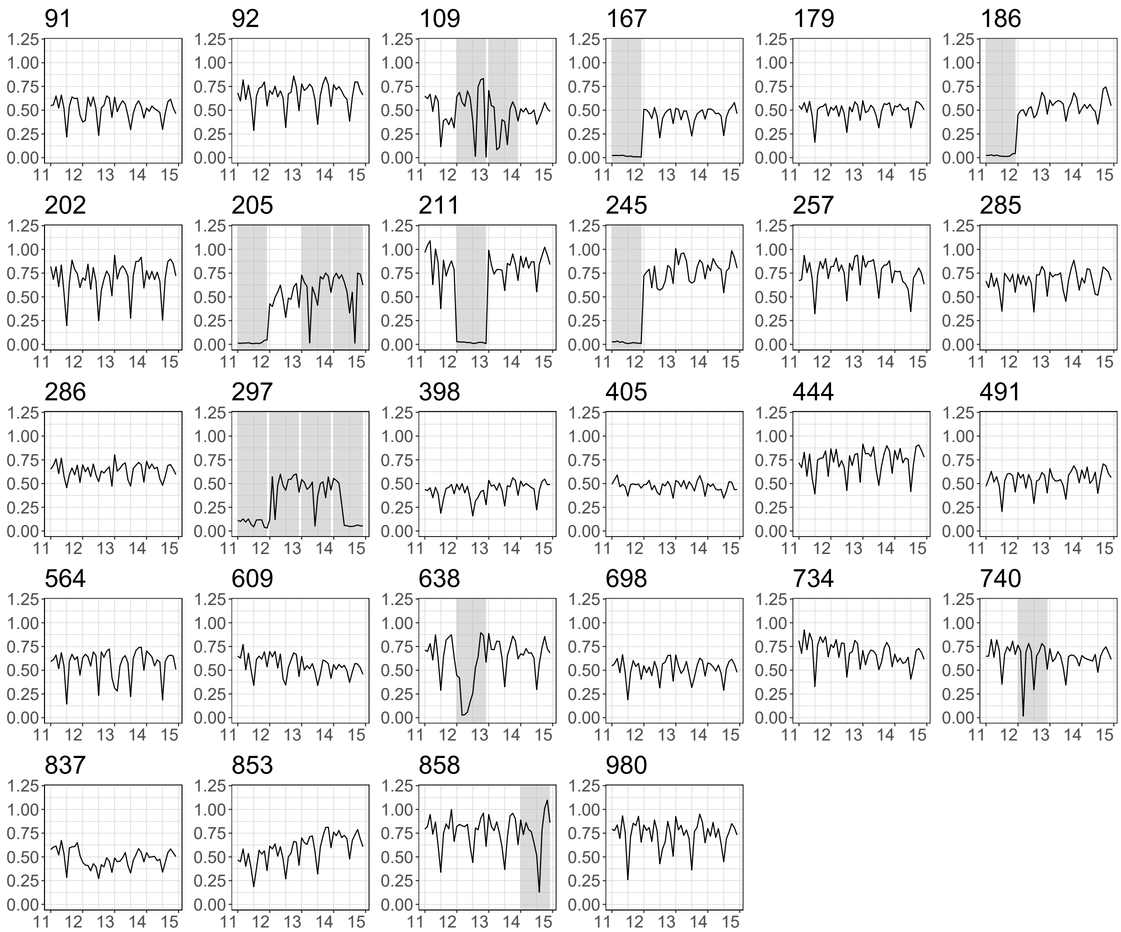

Figure A4: The Evolution of Dentist Visits by Sample Municipality.

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).

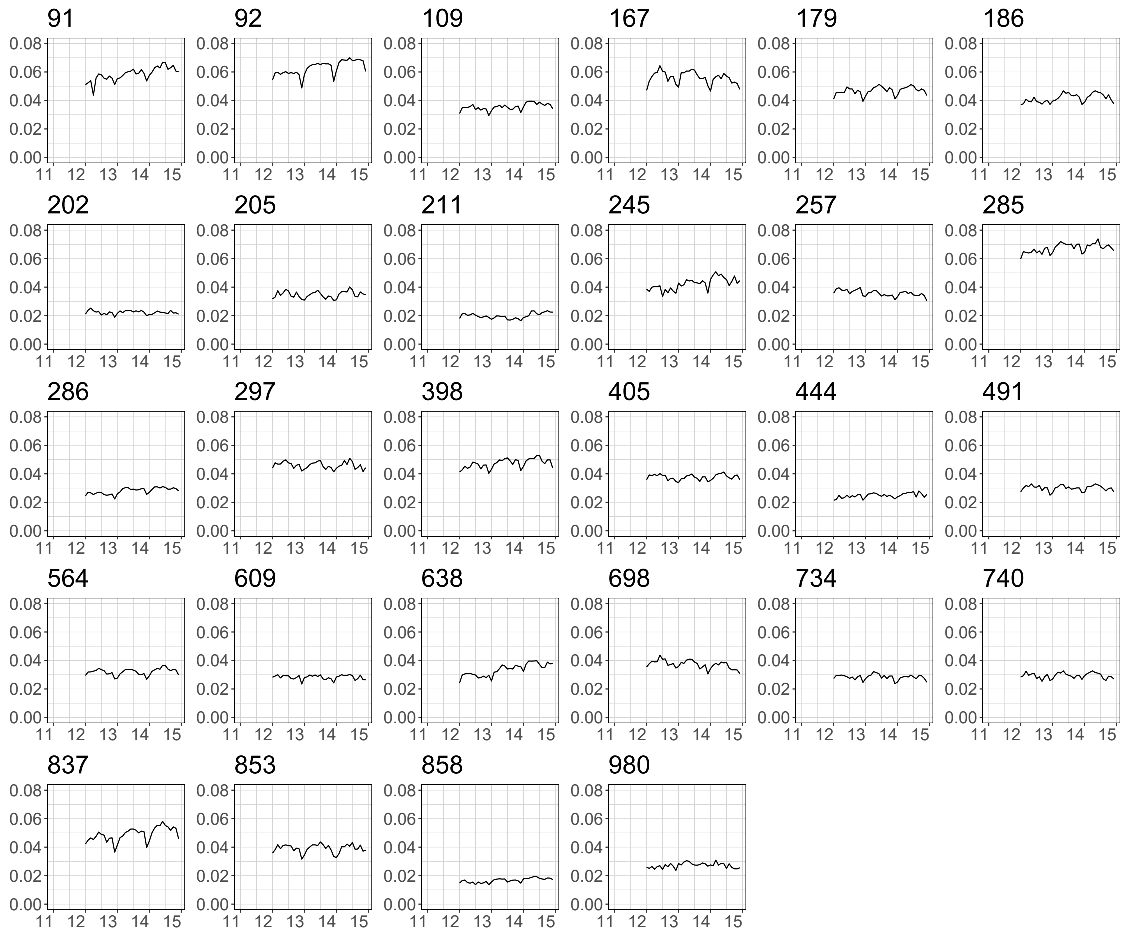

Figure A5: The Evolution of Social Assistance Use by Sample Municipality.

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).

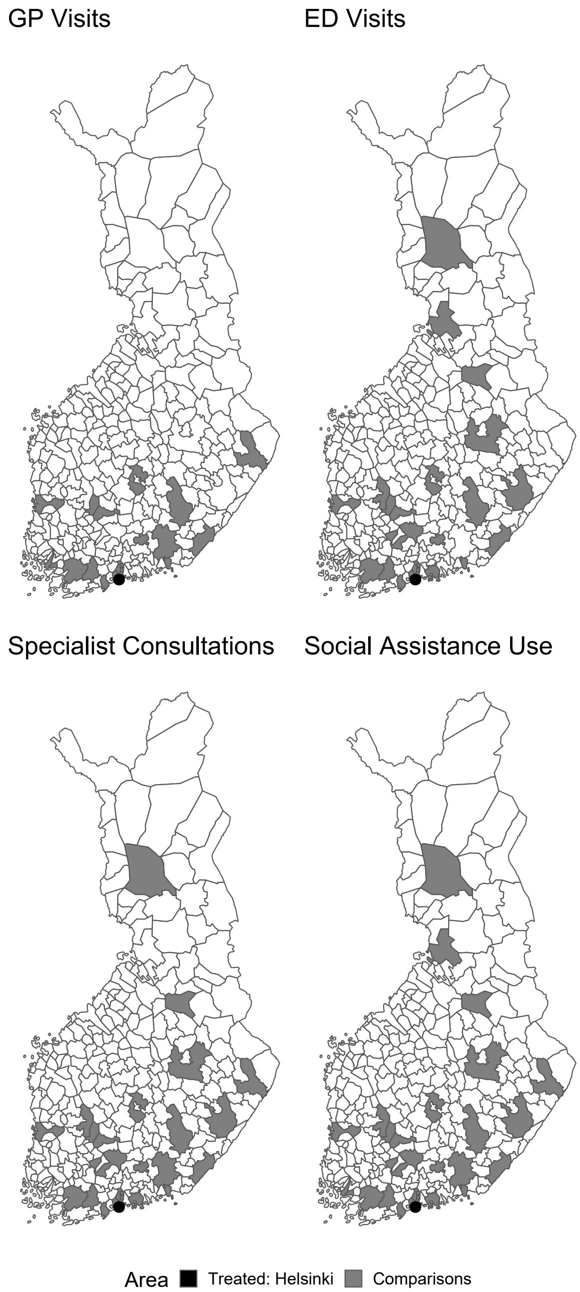

Figure A6: Sample Municipalities on the Map.

Notes: The plot illustrates where our sample municipalities, that depend on the outcome, locate. See Section 3 on how the sample municipalities were chosen.

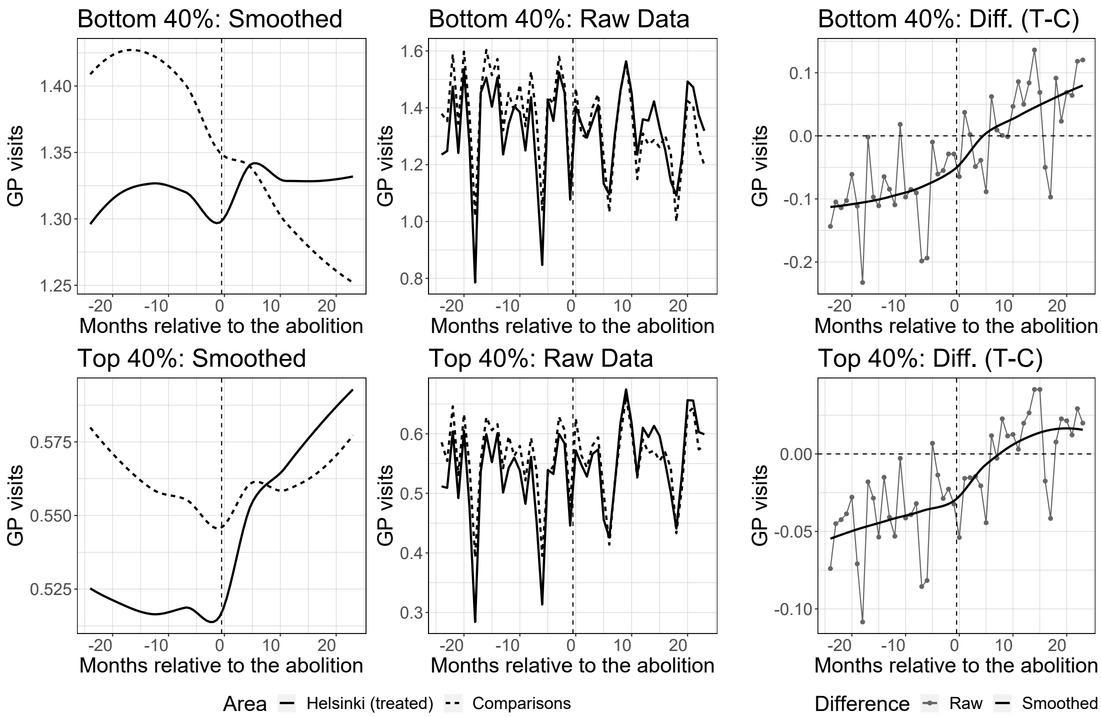

Figure A7: Trends in GP Visits by Income Group.

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%.

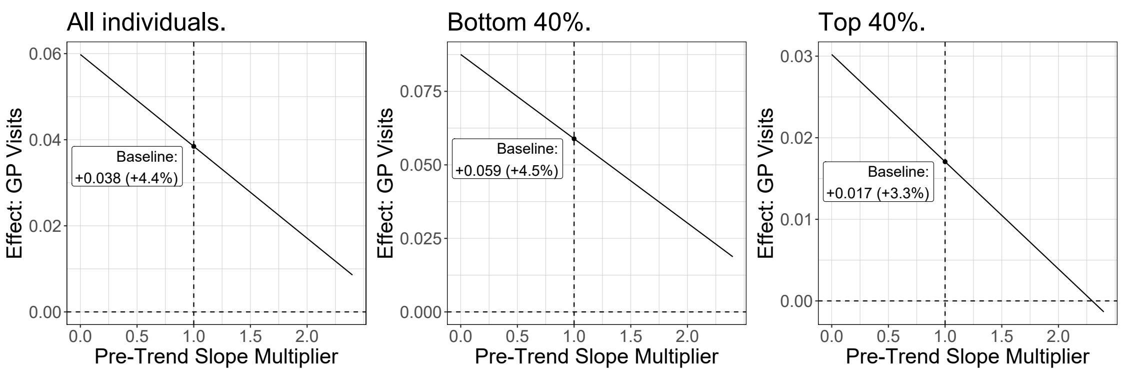

Figure A8: DD Estimates: GP Visits, and Sensitivity to the Parallel Trends Assumption.

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.

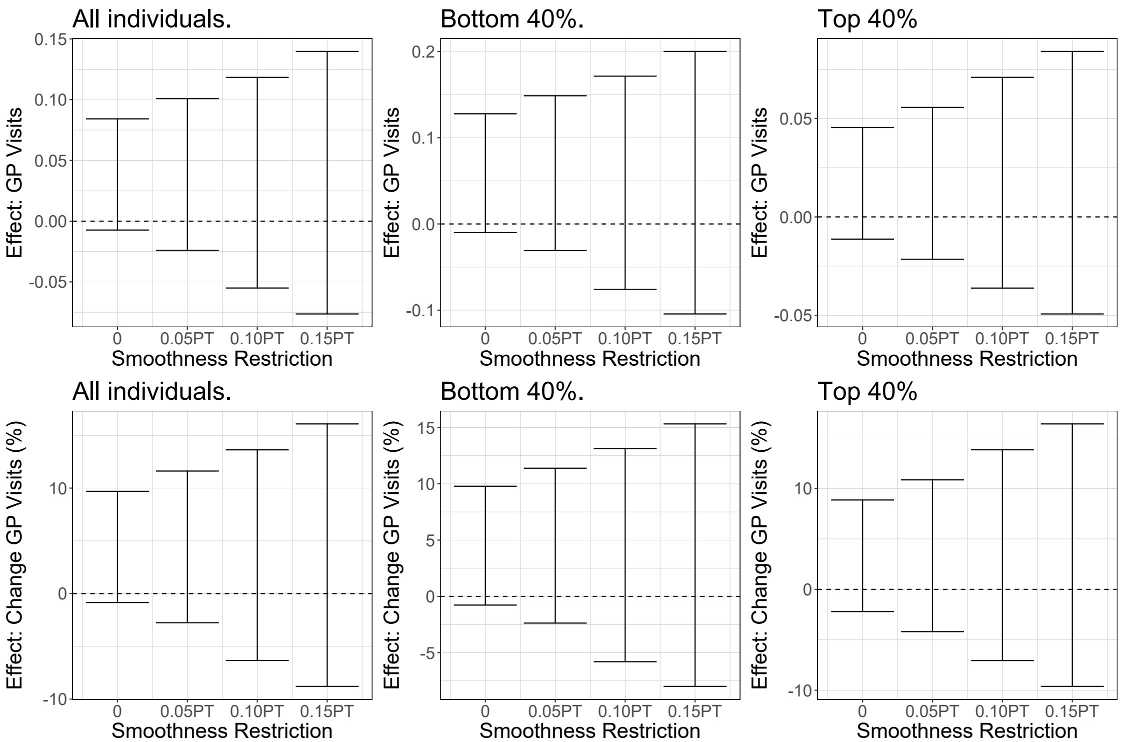

Figure A9: DD Estimates: GP Visits, and Bounding Pre-Trends.

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 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, 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.

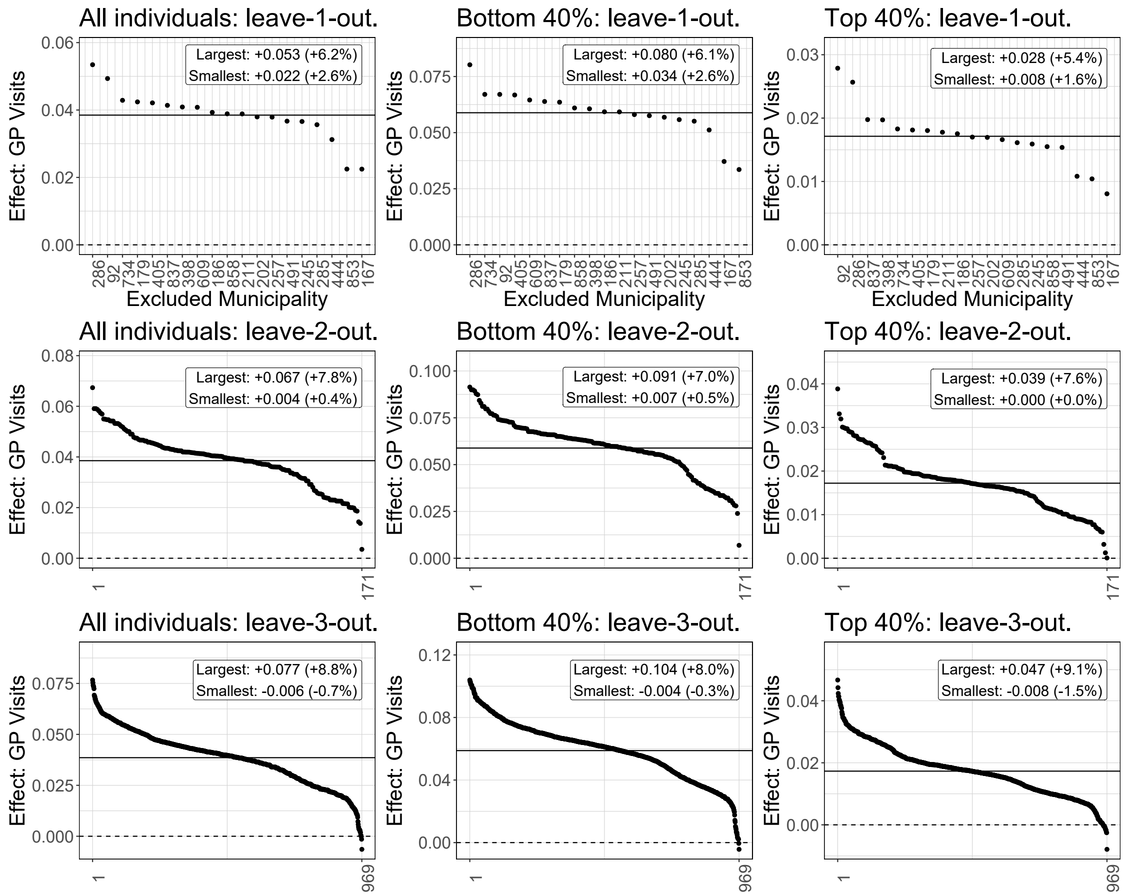

Figure A10: DD Estimates: GP Visits, and Leave-X-out Estimation.

Notes: We exclude each X-municipality combination, $X \in \{1,2,3\}$, from the comparison group 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.

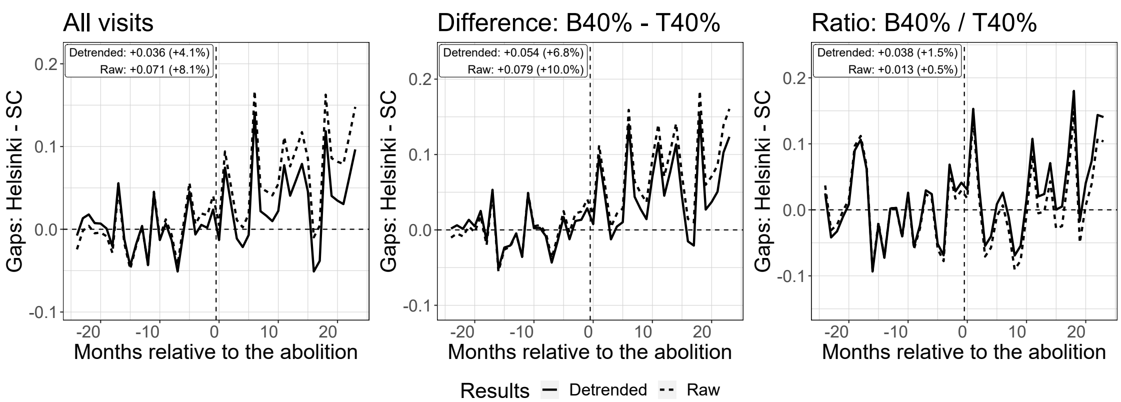

Figure A11: Synthetic Control Estimates: GP Visits, Leave-Two-Out Estimation.

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). The donor pool contains 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.

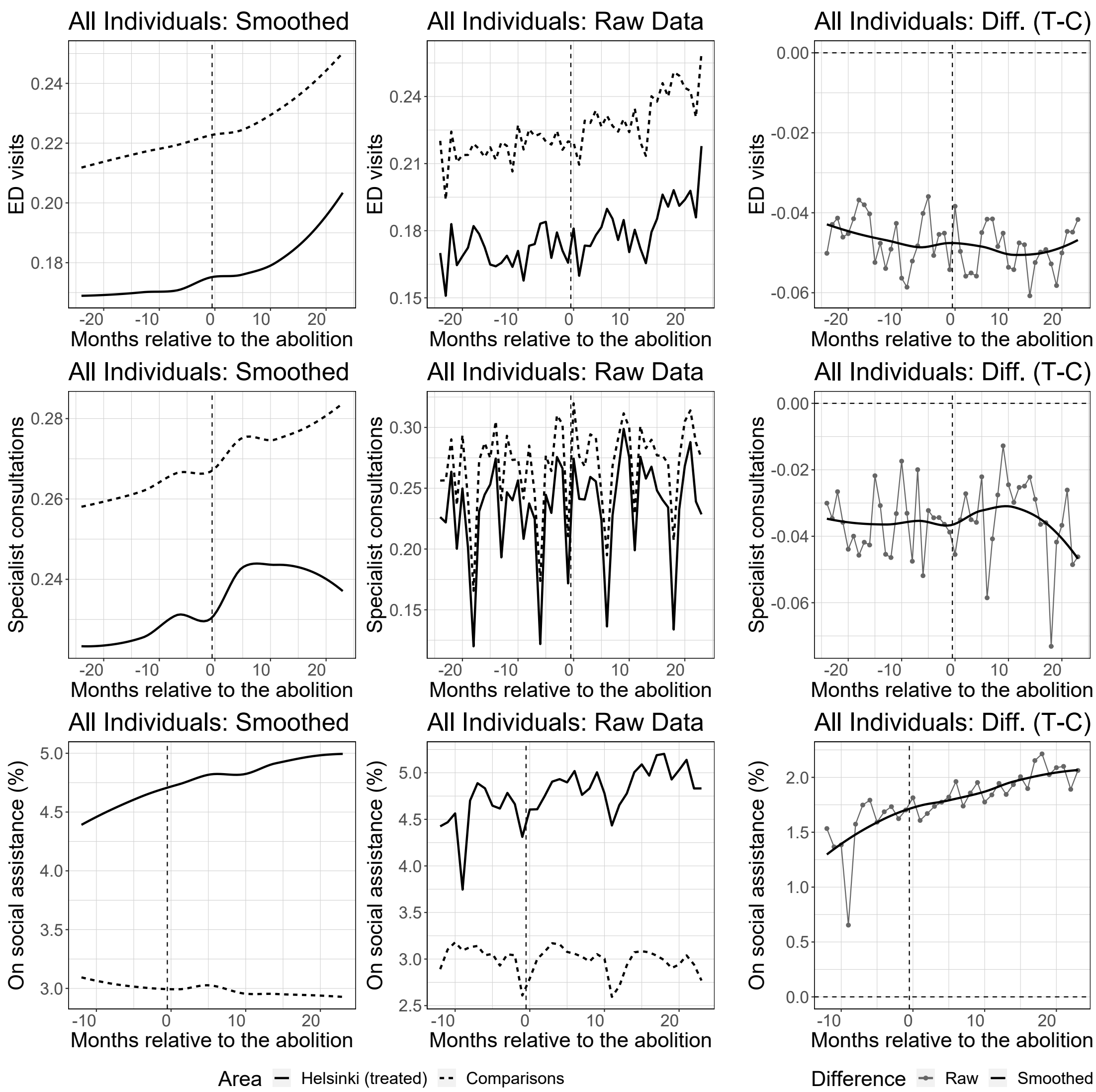

Figure A12: Trends in ED Visits, Specialist Consultations, and Social Assistance Use.

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.

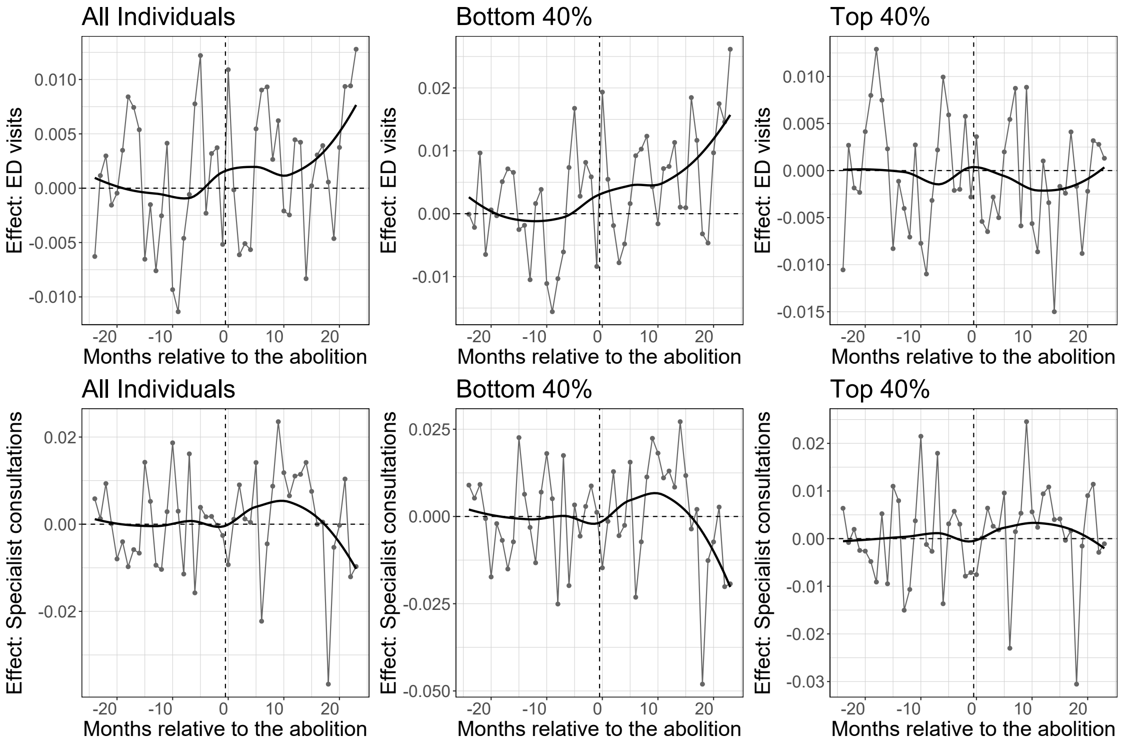

Figure A13: Trends in ED Visits and Specialist Consultations after Removing a Linear Pre-Trend Difference.

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%.

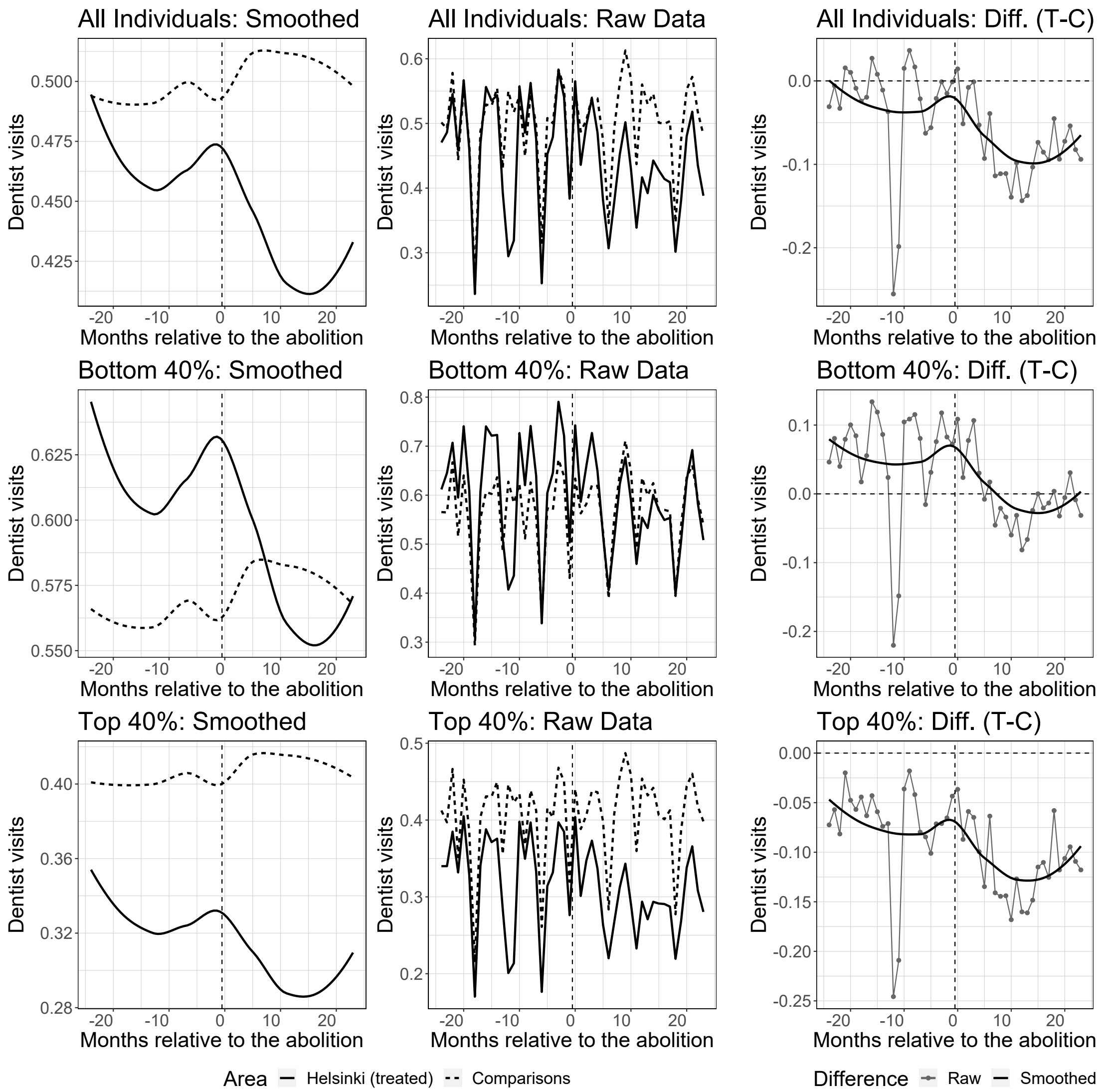

Figure A14: Trends in Dentist Visits.

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%.

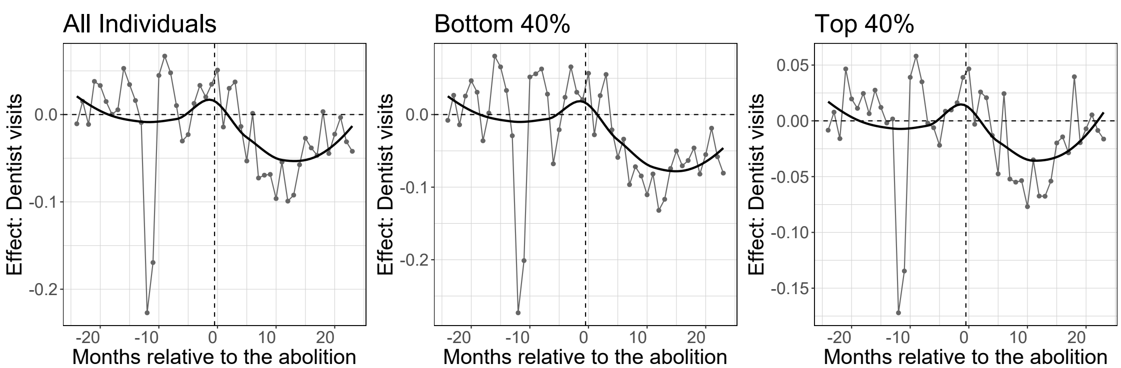

Figure A15: Trends in Dentist Visits after Removing a Linear Pre-Trend Difference.

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%.