Reminders, Cost Sharing, and Healthcare Use

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Abstract

This populated pre-analysis plan presents all pre-specified analyses of a large-scale randomized controlled trial that examines the effects of an information intervention on health care use in Finland. Informational letters reminded individuals about the importance of early detection and treatment of health conditions. Furthermore, there were treatment arms that also informed about the recent abolition of copayments for curative primary care nurse visits. Our primary objectives are twofold. First, we examine whether our intervention induces an exogenous increase in public primary care visits in Finland, a healthcare system characterized by strict gatekeeping. Second, we estimate how information on the abolition of nurse visit copayments affects the consumption of medical care. Our intent-to-treat estimates show null results. Neither the letters in general nor the copayment information affect public primary care use.

Keywords: Reminder, nudge, information nudge, cost sharing, copayment, out-of-pocket costs, health care use, primary care, randomized controlled trial
JEL codes: H42, I11, I13, I14, I18

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Contents

1 Introduction

The rapid aging of the western world puts pressure on health care systems through increased service demand, higher healthcare costs, and ever smaller cohorts participating in the labor market. Several policy instruments are used for cost containment. Both gatekeeping and consumer cost-sharing ideally reduce unnecessary treatment. Conventionally, the term gatekeeping means that access to specialist care is authorized by primary care general practitioners (GPs) by referrals (Greenfield et al., 2016). Some countries, such as Sweden and Finland, even have two-tier gatekeeping: patients are triaged by nurses before they potentially see a GP. Higher out-of-pocket costs have been shown to reduce the use of healthcare services (Einav and Finkelstein, 2018).

Using informational letters sent to randomly chosen households, we aim to contribute to the literature of both gatekeeping and out-of-pocket costs as cost containment tools. First, we examine whether our information intervention induces an exogenous increase in public primary care visits in Finland. If there were this first stage, the induced use of services could be exploited to learn about the value of primary care at the margin for these patients. Notably, if our intervention leads to more primary care visits (patients are triaged by nurses at the point of entry) and to more prescriptions and referrals (based on the doctor's assessment), it would be a sign that there are people with real care needs who could plausibly be treated in another institutional context. Second, we estimate how information on a recent copayment policy change affects primary care consumption. The copayment for curative primary care nurse visits - varying from 10 to 21 euros per visit in our target areas - was abolished nationally only three and a half months before the beginning of our trial.

Three types of informational letters are sent in our randomized controlled trial. The base treatment variant reminds individuals about the importance of early detection and treatment of health conditions, especially in the context of the missed non-Covid health care during the pandemic. The other two letters are copayment treatment variants that additionally inform individuals about the recent abolition of copayments for curative primary care nurse visits. This populated pre-analysis (PAP) plan presents all analyses that were specified in our detailed PAP. Pre-registered RCTs that use a PAP exhibit less p-hacking than pre-registered RCTs without one (Brodeur et al., 2022).[1]

In spirit, we follow Goldin et al. (2020) who show that mailed informational letters about a tax penalty for lacking health insurance coverage actually increased health insurance enrollment and that the increased health insurance coverage reduced mortality. Their key methodological contribution is to illustrate how an ethical, easily implementable, and relatively inexpensive outreach program can be used to study not only the intent-to-treat (ITT) effects of the intervention, but also the effects of the behavior the experiment induces. Our informational letters represent one attempt to generate an exogenous source of variation in primary care use. It is related to a large array of literature in medicine and social science that exploit simple informational letters, emails, or messages to encourage some type of behavior, e.g., to get vaccinated, enroll in health insurance, pay taxes on time, apply for social benefits, or conserve energy.

We also build on and contribute to the literature that has examined how cost sharing (Newhouse and the Insurance Experiment Group, 1993; Finkelstein et al., 2012) and price transparency (Kling et al., 2012; Lieber, 2017) affect the use of health care services. Cost sharing decreases the demand for health care even in the Nordic context where out-of-pocket costs are moderate or small, pricing is transparent (a copayment per visit), and inequality is low (Johansson et al., 2019; Magnussen Landsem and Magnussen, 2018; Nilsson and Paul, 2018). Instead of directly randomizing different financial incentive schemes to patients, our study randomly provides information about a recent policy change that abolished the copayment for curative primary care nurse visits. As our trial occurs shortly after the policy change, it is a plausible assumption that many patients are not aware of the reform.

The objectives of policies may not be achieved if the general population is unaware of the reform. Still, the overwhelming majority of impact evaluation literature in economics

[1] Brodeur et al. (2022) define the PAP as some form of a write-up document. In contrast, our PAP includes a placebo report and statistical programs and is thus much more detailed.

typically focuses on reporting the ITT effects of the initial policy change and bypasses the question of how large the treatment effect could be after informing individuals with outreach campaigns. Our study empirically contributes towards the aim of understanding how informing citizenry about policy changes can affect the achievement of policy objectives, complementing the quasi-experimental evaluation of the ITT effects of the copayment abolition and its earlier staggered adoption on primary care use (Haaga et al., 2022).

Our main findings are the following: Neither the letters in aggregate nor informing about the abolition of the nurse visit copayment have any observable effect on public primary care use in a 6-month follow-up. Neither do we find any short-term effects when assessing the effects of the letters by week. We find no evidence to support our hypothesis that the lower end of the income distribution would respond more to the copayment information than high-income individuals. Data-driven machine learning methods do not find heterogeneity in the effects. Unfortunately, we do not reliably observe the number of first contacts to primary care. Consequently, we cannot infer whether the absence of any utilization effects is a result of zero effects on the number of first contacts or because the (potentially) increased first contacts do not lead to extra appointments when the patients are triaged by nurses.

2 Institutional Background

Three sectors provide primary care services in Finland. Public primary care covers the whole population and is characterized by gatekeeping, varying wait times for nonurgent care and modest copayments. Nurses conduct triage and book appointments to primary care, and a referral is required to visit a specialist. Occupational curative healthcare is available for many employees free of charge at the point of use and with fast access. A designated occupational healthcare professional must often be consulted before booking an appointment. Private outpatient care is available with a short notice and patients can directly go to a specialist, but the out-of-pocket costs are much higher than in public primary care or occupational healthcare. The pensioners, the unemployed, and the low-income individuals disproportionately rely on public primary care while the employed and the high-income individuals often self-select into occupational healthcare or private outpatient care (Blomgren and Virta, 2020).

Public primary care is organized by primary care areas which cover a single municipality or a group of municipalities. The state has a major coordinating and regulatory role in setting out which services must be offered and what the maximum copayments are. Primary care areas independently set their copayment policies within the natiotally set limits, and decide how the services are provided. The financing comes from transfers from the state, municipal taxation, copayments, and municipal bonds.

The supply of public primary care is relatively rigid and capacity constrained for several reasons. First, medical school cohorts are fixed in size which do not appear to adjust to high wages nor to a consistent excess demand for labor in the public sector at the current wage level. Second, few areas seem to have fiscal room or be willing to considerably increase the resources of public primary care. Third, the Covid-19 pandemic has further reduced the capacity of the system to treat non-Covid patients as labor has been allocated to test, trace, and treat Covid patients.

In July 2021, the state reformed a law that sets out the services for which copayments can be charged and the services that must be offered free of charge. The motivation was to reduce barriers to access and, consequently, health inequality. A key change was to set primary care nurse visits to be offered free of charge. In the Finnish primary care system, curative nurse visits have a large role. In these visits, nurses monitor and treat individuals with chronic conditions or infectious diseases and act as care coordinators in collaboration with other healthcare professionals. By the time of the reform, a clear majority of municipalities charged a copayment for curative nurse visits varying between 10 and 20 euros per visits and covering a majority of the population. To compensate for reduced revenue, the state increased transfers to municipalities. Our trial areas contain three regional public primary care areas: Kymenlaakso, Päijät-Häme, and South Karelia. In Kymenlaakso and South Karelia, the per-visit copayments before the abolition were 10.00 and 11.40 euros respectively, paid only for the first three visits annually. In Päijät-Häme, patients could choose between paying an annual copayment of 41.20 euros or paying a per-visit copayment of 20.60 euros for each visit. Each area announced the nurse visit copayment abolition and the other copayment changes at the end of June on their websites.

For the patient, contacting public primary care is rather simple. The provider of primary care services is determined by the municipality of residence.[2] The first contact can be taken via a phone call or by visiting a health station. Then, nurses conduct triage and potentially book an appointment. There are also several institutions that protect individuals from healthcare costs. There is an annual out-of-pocket cap of 692 euros in 2022 for public healthcare services. Social assistance, which is a means-tested last-resort benefit for those with low income and little wealth, can be applied to cover out-of-pocket costs of public health care services.

3 Methods and Data

3.1 Experimental Design

Intervention Arms. Individuals in the control group receive no informational letter (T0 or control). The study includes three active treatment arms (T1-T3), letters varying by their content. The base treatment variant (T1) starts as follows:

Many non-Covid health care contacts have been missed during the Covid-19 pandemic in Finland. If care is not sought at the right time, there is a risk of further deterioration of health. Diagnosing and treating diseases may be delayed. Chronic conditions

[2]Since 2014, people have been able to actively choose a health station from another primary care area and this choice can be revised annually, but the changes have been rather rare.

may worsen. With this letter, we want to remind you and your household members that you can contact your local health center to treat potential health problems.

Thereafter, the letter provides contact information of the local health center and information about the outreach program: all three reminders are part of an information campaign by the Finnish Institute for Health and Welfare (THL) aimed at people aged 55 or more. The addresses are extracted from the Finnish Population Register. Only one reminder is sent per household for economical and environmental reasons. Finally, the reminder requests the recipient to inform other household members as well. The reminders are in both Finnish and Swedish, the official languages of Finland. Individuals residing in the study regions are not aware that they are being studied: the reminders have no references to the experimental setting.

Our copayment treatment variants (T2 and T3) are otherwise similar to the base reminder except for that they additionally contain information about copayments for primary care visits. They both add the following sentences:

We also want to inform you about the reformed Act on Client Charges in Healthcare and Social Welfare which has affected primary care copayments. Due to the new law, all primary care nurse visits have become free of charge in Finland from July 1st, 2021.

The difference between arms T2 and T3 is that the latter also mentions the level of GP visit copayments in public primary care and that they remain unchanged. English translations for interventions in South Karelia are in Figure A1 (T1), Figure A2 (T2), and Figure A3 (T3).

Study Population. The trial is conducted in three regional primary care areas (Kymenlaakso, Päijät-Häme, and South Karelia) covering 25 municipalities and approximately 480,000 residents (out of 5.5 million Finns in 2019). They are geographically large and diverse regional primary care areas that before the national abolition charged a copayment for curative nurse visits and that contain both cities and rural areas. We restrict to a small number of primary care areas for the ease of implementation as we wanted to inform and consult the management of the primary care areas in advance. For the randomization, we include individuals 1) born in 1966 or earlier (aged 55 or above at the end of 2021) in households with 2) a permanent address in one of the 25 target municipalities on September 15h, 2021 and 3) at maximum three individuals born in 1966 or earlier (we want to exclude nursing home residents).[3] These restrictions leave us with 198,657 residents in 142,194 households.

Randomization. Randomization takes place at the household level separately in each municipality (a stratified RCT). Within each of the 25 target municipalities, 2/3 of the eligible households, defined by unique apartment IDs, are randomized to the control group (T0) while the remaining 1/3 of the eligible households are randomly assigned across three equal-sized active treatment arms (T1-T3). If there are more than one eligible individual in the household, the recipient of a reminder is randomized (only one reminder is sent per household). Similarly, one individual is randomly selected from control households. Overall, we send 47,398 reminders. The size of the experiment is constrained by the budget available. The reminders are sent in four waves over four weeks - four equal-sized waves are randomized within each municipality - as we wanted to avoid potentially congesting the public primary care providers by a large amount of extra calls in a short period of time.

We stratify by municipality to make sure that 1/3 of the eligible households in each municipality receive a reminder. The use of primary care services may vary considerably across municipalities for reasons both on the demand side (some areas have healthier residents) and the supply side (most municipalities have a health station, and resources may vary at the health station level in larger primary care areas).

Implementation. Our team conducted the randomization, and an external firm mailed the letters. As there are three different types of reminders tailored for three regions, there are nine different reminders in total. They were sent in four equal-sized waves over four weeks (October 13th, October 20th, October 27th, and November 3rd, 2021). The research

[3]We focus on the elderly population because of their higher healthcare use and larger dependency on public primary care compared to the working age population.

group did not have direct contact with the treated individuals, but a couple of them called the Finnish Institute for Health and Welfare to ask for extra information using a phone number provided in the informational letters.

3.2 Data

The study population and their IDs, postal information, and apartment IDs are extracted from the Finnish Population Information System. These data are combined with several administrative registers via unique person IDs. Curative nurse and GP visits in public primary care and private outpatient doctor visits are extracted from the Register of Primary Health Care Visits and referrals to specialized healthcare from the Care Register for Health Care, both registers administered by the Finnish Institute for Health and Welfare (THL). Prescriptions are from the Finnish National Prescription Center (Kanta), administered by the Social Insurance Institution of Finland. This populated PAP uses data from 1/2021-5/2022. Sociodemographic and socioeconomic information from the end 2020 come from Statistics Finland's FOLK modules "basic information", "family", and "income". Access to these registers can be applied for through Findata and Statistics Finland. We also use two publicly available registers called TOPI and SOTE on the characteristics of healthcare providers, both administered by THL.

We provide a detailed discussion on how we clean and construct the analysis data in Section A.1 in the Online Appendix and provide the replication codes as a supplement.

Our primary contact types of interest are curative nurse and GP visits in public primary care. This is motivated by the fact that the nurse visit copayment abolition directly affected curative nurse visits in public primary care and that our letters provide contact information of local public primary care services. Our secondary interest is in prescriptions written by public sector organizations and referrals to specialized healthcare written by public health centers, because these outcomes should proxy professional-assessed need for diagnosis and treatment. We plot the evolution of these contacts per capita by primary care area in Figure 1. We also look for spillovers to private outpatient care by examining the effects of the reminders on private outpatient doctor visits, prescriptions written by private sector units, and referrals to specialized healthcare written by private clinics. The trends by primary care area are shown in Figure 2.

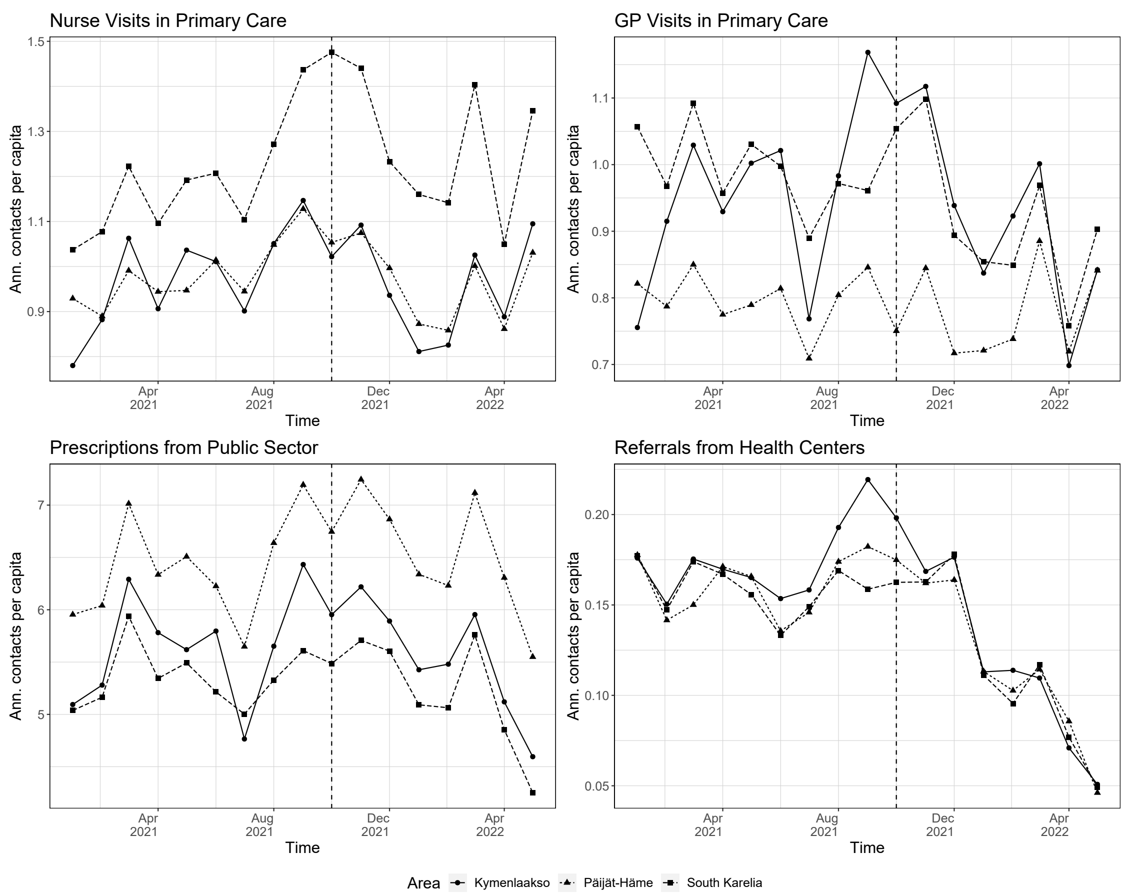

Figure 1: Public Primary Care: Trends in Annualized Number of Contacts per Capita.

Notes: The plots show the evolution in public primary care contacts between 1/2021 and 5/2022 in our trial regions. The start of the trial in 10/2021 is by dashed vertical line. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

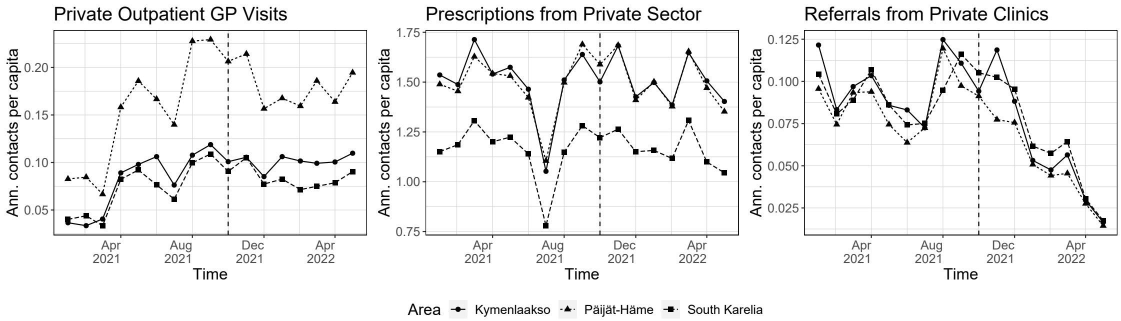

Figure 2: Private Outpatient Care: Trends in Annualized Number of Contacts per Capita.

Notes: The plots show the evolution in private outpatient care contacts between 1/2021 and 5/2022 in our trial regions. The start of the trial in 10/2021 is by dashed vertical line. Prescriptions are written by private sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include prescriptions written in public primary care, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by private outpatient care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

3.3 Descriptive Statistics

Table 1 reports the observed means for the control group (T0), the base reminder group (T1) and the two copayment reminder groups (T2 and T3). The table includes pre-trial values of health care use by service (our outcomes) and socioeconomic and sociodemographic covariates that we select assuming that they are predictive of health care use or treatment effect size or both. Standardized mean differences and p-values from the test of equality in means and the F-test are reported in Table A1 and Table A2. These tests are conducted to assess covariate balance and whether there is need for covariate adjustment in robustness checks. The tests are conducted using OLS with heteroskedasticity-robust standard errors. From regression estimation, we exclude covariates that were highly collinear, defined as having a correlation of more than 0.4 in absolute value.[4] We report p-values from F-tests in three cases: 1) including all individuals and covariates of Table A1, 2) including only

[4]The excluded variables are the following (the other covariate that we keep of the collinear pair is in parentheses): unemployed for at least 197 days (unemployed for at least one day), disposable income (equivalised disposable income), being widowed (age), being a pensioner (age and unemployment), has Finnish background (Finnish as a native language), and prescriptions from public sector (GP visits in public primary care). There was a typo in the PAP: the text says the correlation boundary is 0.5, but the results were computed with 0.4 as the boundary.

those with disposable income and equivalised family disposable income below 500,000 euros and covariates of Table A1, and 3) including all individuals but only the covariates on prior health care use in Table 1.

Table A1 examines the balance in the following comparisons: comparing treated individuals (T1+T2+T3) to the controls (T0), the copayment reminder group (T2+T3) to the base reminder group (T1), and the group that received extra information on GP visit copayments (T3) to the group that received only information on the abolition of the nurse visit copayment (T2). The p-values are large especially when only prior health care use is considered, but the inclusion of other controls makes the corresponding F-tests significant at the 10% level. The comparison of T2 vs. T3 produces the smallest F-test p-values. When testing the equality of means by covariate, six of the 51 differences are significant at the 5% level (three comparisons and 17 covariates). Table A2 compares the balance between the control group (T0) and each treatment group (T1, T2, and T3) separately.

3.4 Statistical Methods

Main Analyses. We report the intent-to-treat effects of the informational letters on annualized health care contacts in a six-month follow-up starting from October 13th, 2021, when the first wave of reminders was sent. We also report in the Online Appendix the effects on the indicator of having any contacts in the follow-up. The main comparisons that we report throughout the study are the following: comparing treated individuals (T1+T2+T3) to the controls (T0), and the copayment letter group (T2+T3) to the base letter group (T1). Following Athey and Imbens (2017), we analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level.[5] That is, we first compute stratum-specific ATEs as a difference in means between the treated and control individuals. Stratum-specific variance estimates are computed by summing outcome

[5]We could also stratify by municipality-by-wave, but the randomized wave variable is only weakly correlated with the potential outcomes. Still, the correlations are probably not exactly zero as the fourth wave had three weeks less post-treatment time compared to the first wave with the six-month follow-up.

Table 1: Covariate Balance: Means.

Variable	Control T0 N=94,796	Base T1 N=15,800	Copayment 1 T2 N=15,800	Copayment 2 T3 N=15,798
A. Prior health care use				
Primary care nurse visits	1.022	0.998	1.006	1.044
Primary care GP visits	0.919	0.920	0.915	0.917
Private outpatient doctor visits	0.085	0.083	0.079	0.083
Prescriptions from public sector	6.042	5.995	5.999	6.013
Prescriptions from private sector	1.419	1.441	1.378	1.405
Referrals from health centers	0.162	0.168	0.171	0.154
Referrals from private clinics	0.087	0.090	0.084	0.089
B. Sociodemographic covariates				
Age	68.943	69.039	68.916	69.016
Is male	44.31%	44.74%	44.62%	44.06%
Has Finnish background	96.67%	96.47%	96.61%	96.79%
Native language Finnish	96.46%	96.29%	96.22%	96.62%
In relationship	40.61%	40.04%	40.48%	41.46%
Widowed	17.62%	18.52%	18.08%	17.79%
Children living at home	9.28%	9.65%	9.75%	9.03%
C. Socioeconomic covariates				
Living in an apartment	40.14%	39.40%	39.56%	39.50%
Degree from tertiary education	11.80%	11.55%	11.40%	11.28%
Pensioner	65.43%	65.35%	65.02%	65.56%
Disposable income	23.051	22.786	23.016	23.116
Equivalized disposable income	26.350	26.069	26.264	26.454
Unemployed for at least one day	9.87%	9.87%	10.20%	9.80%
Unemployd for at least 197 days	4.36%	4.61%	4.53%	4.26%
Received social assistance	3.97%	3.87%	3.82%	3.53%
Received sickness allowance	2.58%	2.72%	2.55%	2.92%

Notes: We report means for continuous covariates and shares as percentages for binary covariates. Health care use is measured by the annualized number of contacts in 1/2021-6/2021, prior to the law reform in 7/2021 and our trial starting in 10/2021. Covariates in Panel B and Panel C are measured at the end of 2020, and their sample sizes are slightly lower than reported in the table due to missing values in 0.1% of the rows. Income is measured in thousands.

variance in a given group divided by the group's sample size over the two groups. The overall ATE (variance) is estimated by averaging the within-stratum estimates weighted by the stratum share (the square of stratum share).

Supplementary Analyses. We conduct robustness checks that aim to increase balance and/or precision. Specifically, we 1) include the covariates shown in Table A1 linearly in the regression formula (inclusion of controls), 2) subtract for each individual the annualized pre-treatment health care use of a given service in 1/2021-6/2021 from the annualized post-treatment health care use of the same service (RCT-DID), and 3) conduct subclassification matching. In the first two cases, we estimate the effects using OLS regression with heteroskedasticity-robust standard errors, including an indicator for the treated group and municipality fixed effects. When using propensity-score based subclassification matching, we use 10,000 subclasses, the propensity scores being estimated with logistic regression and the covariates from Table A1.[6] The target estimand is the average treatment effect (ATE). Matching is conducted with the R package *MatchIt* (Ho et al., 2011). The final results are estimated using OLS with heteroskedasticity-robust standard errors, observations weighted by matching weights.

Heterogeneity Analyses. We conduct pre-registered tests on the form of treatment effect heterogeneity and as a complement use generic machine learning inference (Chernozhukov et al., 2020) to conduct data-driven analyses. There is a clear trade-off between these two approaches: the former makes *ex ante* predictions where the heterogeneity may be and, thus, attains a larger power, while the latter makes no assumptions on where the heterogeneity is, leading to a loss of power (Chernozhukov et al., 2020). As in the main analysis, we use a six-month follow-up. With the pre-registered tests, we focus on curative nurse and GP visits in public primary care and use a stratified OLS regression model:

$$y_i = \alpha + \beta_1 Treat_i + \beta_2 Group_i + \beta_3 Treat_i \times Group_i + \varepsilon_i. \tag{1}$$

[6] Only 5,000 for the analysis of within-household spillovers.

Suppose *Group* is an indicator for being in the top 50% of the income distribution, and $Treat$ is an indicator for being in the treated group. Then, the intercept α shows the mean in the control group among the bottom 50%. Standard errors are robust to heteroskedasticity. The parameter of interest is β_3 as it shows the difference between the treatment effects in the two treated groups.

The key concept in heterogeneity analysis is the conditional average treatment effect (CATE) function $s_0(Z) = E[Y|T = 1, Z] - E[Y|T = 0, Z]$ where Z contains the covariates, Y is the outcome, and T denotes treatment status. Generic machine learning inference by Chernozhukov et al. (2020) proposes strategies for estimation and inference on *key features* of $s_0(Z)$. We focus on the Best Linear Predictor (BLP) of $s_0(Z)$ on the ML proxy predictor $S(Z)$. BLP answers the question of whether there is evidence of treatment effect heterogeneity based on observables.

The ML proxy predictor $S(Z)$ is estimated in the following steps. First, we split the data into two equal-sized parts: an auxiliary sample and a main sample. Data splitting is used to avoid overfitting. Second, we predict $E[Y|T = 1, Z]$ and $E[Y|T = 0, Z]$ *in the auxiliary sample* and store the fitted models. Third, we predict Y(1) and Y(0) - the potential outcomes - for all individuals *in the main sample* using the models fitted in the auxiliary sample. Then, we compute their difference, resulting in an estimate for $S(Z)$ that is our ML proxy predictor for $s_0(Z)$. We repeat these steps multiple times (here: 50) to account for the splitting uncertainty: the reported point estimate is the median of the estimated key features. Similarly, the reported lower and upper bounds of confidence intervals and p-values are medians, and the nominal confidence level and p-values are adjusted. Heteroskedasticity-robust variance-covariance matrix is used.

We implement the generic machine learning analysis with the R package *GenericML* (Welz et al., 2021) to examine heterogeneity in the comparison of controls (T0) and the reminder group (T1+T2+T3), using covariates of Table 1 in addition to three primary care area dummies and two indicators for having a prescription for diabetes or hypertension drugs.[7] Here, we examine effects on the indicator of having any visits in the follow-up instead of on the annualized number of visits.[8] We use four learners: two random forest (Breiman, 2001) and XGBoost (Chen and Guestrin, 2016) learners using both tuned and default hyperparameters of R packages *ranger* and *xgboost*. The details on hyperparameter tuning are in Section A.2.

4 Results

4.1 Main Results

The main results are in Table 2. The estimates for the effects on curative nurse and GP visits - our primary outcomes - are insignificant and very close to zero, the largest relative change being only 0.7%. The results for having any such contacts are in Table A3. We conclude that neither receiving a reminder in general nor informing about the nurse visit copayment abolition has any observable effect on public primary care use. Given that there is no effect on GP visits, we would expect null effects on drug prescriptions and referrals. Indeed, the estimates for prescriptions are very close to zero. The estimates for referrals are slightly larger in relative terms, but likely caused by more noise.[9]

Table A4 shows the results when comparing T3 to T2, assessing the effects of adding information on GP visit copayments besides the information on the nurse visit copayment abolition. There is more noise due to smaller sample sizes and all the estimates are insignificant. Taking the point estimates at face value, informing about the relative price appears to increase nurse use and decrease GP use. However, our conclusion is that there is

[7]Diabetes: ATC A10 for drugs used in diabetes. Hypertension: ATC C02 for antihypertensives, C03 for diuretics, C07 for beta blocking agents, C08 for calcium channel blockers, and C09 for agents acting on the renin-angiotensin system.

[8]This is done so that the tree-based ML methods we use do not have to learn to detect high-users - a small group with disproportionately large primary care use for whom we expect small effects.

[9]We do not currently observe all referrals. The data were extracted in June 2022, and we only observe those referrals that have led to actualized visits. Most individuals getting a referral in May are still on the waiting list in June and are thus not observed.

Table 2: Public Primary Care: Annualized Number of Contacts.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	1.054	0.891	6.258	0.138
Estimate	0.001	0.003	-0.002	-0.003
Std. error	0.020	0.010	0.047	0.003
Confidence interval	[-0.038, 0.040]	[-0.017, 0.022]	[-0.094, 0.091]	[-0.010, 0.003]
Change (%)	0.099	0.314	-0.028	-2.523
Std. mean difference	0.000	0.002	0.000	-0.006
N: treated	47,398	47,398	47,398	47,398
N: reference group	94,796	94,796	94,796	94,796
B. Base vs. copayment reminder				
Reference group mean	1.050	0.895	6.254	0.135
Estimate	0.007	-0.002	0.002	-0.001
Std. error	0.033	0.017	0.082	0.005
Confidence interval	[-0.058, 0.072]	[-0.036, 0.032]	[-0.158, 0.162]	[-0.012, 0.009]
Change (%)	0.687	-0.197	0.039	-0.832
Std. mean difference	0.002	-0.001	0.000	-0.002
N: treated	31,598	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800	15,800

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

no evidence of such effects given that statistical uncertainty is high and we do not observe any effects in the abovementioned primary comparisons.

Figure A4 (annualized contacts) and Figure A5 (has any contact) show the estimates as a function of the follow-up length. In terms of annualized contacts, shorter follow-ups appear to produce estimates that are farther from zero. This is likely explained by more noise and not by some short-term effects. This interpretation is supported by Figure A6 which shows the number of accumulated curative nurse and GP visits per capita comparing the letter group (T1+T2+T3) to the comparisons (T0). Essentially, visits accumulate similarly in both groups before and after the trial. Even if there were some (very small) increase in visits in the treatment group in a short 6-week period after the start of the trial, such a pattern is not by any means an outlier compared to the pre-trial evolution in contacts.

We assess the robustness of the main results to exploiting controls in three ways - the details are reported in Section 3.4. First, we include controls linearly in the OLS formula. Second, we subtract pre-trial service use from the post-trial outcome. Third, we use subclassification matching. The conclusions do not change: the trial had no effect on public primary care use. The estimate of the effect of copayment information (T1 vs. T2+T3) on nurse visits is somewhat noisy, but it is insignificant and relatively close to zero. These tables and a plot on covariance balance after matching are available in the replication codes folder.

We also examine robustness to excluding high users of health care services, a small group of individuals constituting a disproportionately large service use, in order to reduce the chance that our estimates are driven by a few outliers. We also hypothesize that the treatment effects for this group are smaller than for the general population as high-users already use services and are familiar with the copayments. To define the high-users, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up using the whole trial sample. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user. With this definition, 4.4% of the trial population are high-users. The results excluding the high-users on annualized visits are in Table A5 and on having any visits in Table A6 - the point estimates are close to zero and insignificant.

4.2 Spillovers to the Use of Private Services

Even though the reminders provide contact information of only public primary care, the trial may have spillover effects to private outpatient care as well. The results on the annualized number of contacts in private outpatient care are in Table 3. We find a statistically significant but economically insignificant reduction in private doctor visits among the letter recipients (T1+T2+T3). The magnitude of this effect is small in absolute terms, but rather large in relative terms (-6%) due to low baseline private doctor use. The effect is driven by the copayment letter group (T2+T3). We find it unlikely that people who use private clinics with much higher copayments than in public primary care would respond to the copayment information but not to the base treatment variant. Furthermore, the estimates somewhat attenuate when we exploit controls. These result tables are not presented in the study, but are available in the replication codes folder. The corresponding results on having any such contacts are in Table A7.

4.3 Spillovers within Households

In the trial, we send only one reminder per household and the recipient is chosen randomly. Consistent to the main results, we do not find signs of spillover effects within the household. Specifically, we take from the trial population households with at least two target individuals and exclude those individuals who belonged to the sample in main analysis (those who received the reminder personally and those in control households who were randomly picked for main analysis). The results on curative public primary care nurse and GP visits are in

Table 3: Private Outpatient Care: Annualized Number of Contacts.

	Doctor visits	Prescriptions	Referrals
A. No vs. any reminder			
Reference group mean	0.119	1.425	0.064
Estimate	-0.007	-0.016	0.003
Std. error	0.003	0.020	0.002
Confidence interval	[-0.013, -0.001]	[-0.056, 0.024]	[-0.001, 0.007]
Change (%)	-5.935	-1.144	5.253
Std. mean difference	-0.012	-0.004	0.009
N: treated	47,398	47,398	47,398
N: reference group	94,796	94,796	94,796
B. Base vs. copayment reminder			
Reference group mean	0.117	1.454	0.065
Estimate	-0.007	-0.068	0.003
Std. error	0.006	0.036	0.004
Confidence interval	[-0.018, 0.004]	[-0.138, 0.002]	[-0.004, 0.010]
Change (%)	-5.642	-4.693	4.366
Std. mean difference	-0.011	-0.019	0.008
N: treated	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Prescriptions are written by private sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include prescriptions written in public primary care, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by private outpatient care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A8. The result tables from exploiting controls are not presented in the study, but are available in the replication codes folder.

4.4 Heterogeneity Analysis

The trial did not affect public primary care use on aggregate. In this section, we assess whether there are subgroups that were affected, focusing on curative nurse and GP visits in public primary care. We have five pre-registered heterogeneity tests. When comparing the control group to the letter group (T0 vs. T1+T2+T3), we stratify the population into two groups by i) the median equivalised family disposable income, ii) the median age, iii) the indicator of having any curative public primary care visits (either nurse or GP visits) prior to the trial, and iv) the indicator of residing in a municipality that has outsourced the public primary care services (Lahti, Iitti, and Kärkölä). Stratifying by income helps us to assess whether the trial induces more contacts among the lower end of the income distribution where health is on average worse and additional health care use may be more beneficial at the margin. The top half of the age distribution are pensioners by a clear majority who rely on public primary care and have no access to occupational healthcare. Those who have pre-treatment visits in public primary care are on average sicker and rely more on public primary care than those who do not need or use public services. The estimation is conducted with Model 1.

The results on annualized visits are in Table 4. The effect on nurse visits is larger for those who had any curative public primary care visits prior to the trial (statistically significant). Other estimates are insignificant. The results on having any visits in Table A9 are all insignificant. As a robustness check, we estimate the results on annualized visits also after excluding the high-users (see the definition in Section 4.1) who are supposedly less sensitive to our intervention. These results are in Table A10. The effect on nurse visits remains larger for those who had any curative public primary care visits prior to the trial.

When comparing the base reminder group (T1) to the copayment reminder group

Table 4: Heterogeneity Tests, T0 vs. T1+T2+T3, Annualized Visits.

	Income ≥ median	Age ≥ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	1.244 [0.018]	0.717 [0.013]	0.525 [0.009]	1.141 [0.014]
TREAT	-0.008 [0.032]	-0.021 [0.023]	-0.040 [0.014]	0.018 [0.025]
GROUP	-0.377 [0.023]	0.646 [0.023]	1.396 [0.027]	-0.329 [0.024]
TREAT:GROUP	0.015 [0.040]	0.040 [0.039]	0.110 [0.049]	-0.067 [0.039]
P-value	0.709	0.308	0.024	0.088
Change (G=0)	-0.60%	-2.92%	-7.68%	1.62%
Change (G=1)	0.85%	1.40%	3.61%	-5.99%
B. GP visits				
Intercept	1.041 [0.009]	0.646 [0.007]	0.514 [0.005]	0.956 [0.007]
TREAT	0.002 [0.016]	0.006 [0.013]	0.007 [0.009]	0.000 [0.012]
GROUP	-0.298 [0.012]	0.469 [0.012]	0.994 [0.013]	-0.248 [0.012]
TREAT:GROUP	0.000 [0.020]	-0.007 [0.020]	-0.010 [0.023]	0.012 [0.021]
P-value	0.988	0.730	0.654	0.586
Change (G=0)	0.23%	0.99%	1.45%	0.04%
Change (G=1)	0.36%	-0.05%	-0.18%	1.71%

Notes: We compare the controls (T0) to the reminder group (T1+T2+T3), the former being the reference group. We use Model 1 and focus on curative nurse and GP visits in public primary care. Heteroskedasticity-robust standard errors in square brackets. "Pre-trial visits" is an indicator for having any curative nurse or GP visits in 1/2021-6/2021, before the law change and our trial. The other "GROUP" variables are indicators for having age or equalized family disposable income above the median and for residing in a municipality where the public primary care is outsourced (Lahti, Iitti, and Kärkölä). The follow-up is six months. P-value is reported for the term "TREAT:GROUP". The percentage changes show the CATEs relative to the untreated observations in a given subgroup.

(T2+T3), we stratify the population into two groups by the median equivalised family disposable income. The hypothesis is that the bottom end of the income distribution is more sensitive to the price information. Again, the estimation is conducted with Model 1. The results are in Table 5. In contrast to our hypothesis, there is no evidence that low-income individuals increased their nurse use after receiving information about the copayment abolition.

Figure A7 plots the effects of the copayment information (T1 vs. T2+T3) by quintiles of the equalized family disposable income, providing more flexibility compared to the analysis above. The regression model contains an indicator for the treated individuals and municipality fixed effects. Standard errors are robust to heteroskedasticity. We also map the point estimates and their confidence intervals to percentage changes by dividing the estimate by the control group mean and multiplying by 100. Similarly, Figure A8 plots the CATEs by primary care area.

Next, we report the results of the generic machine learning inference analysis that examines whether there is evidence of treatment effect heterogeneity. For this task, Chernozhukov et al. (2020) propose to estimate the following regression

$$Y = \alpha + \beta_0 B(Z) + \beta_1(D - p(Z)) + \beta_2(D - p(Z))(S(Z) - \bar{S}) + \varepsilon \tag{2}$$

weighted by $w(Z) = \{p(Z)(1 - p(Z))\}^{-1}$ where D and Z are the treatment indicator and observables, $B(Z)$ is the predicted baseline conditional average $E[Y|D = 0, Z]$, $p(Z)$ is the propensity score equal to 1/3 for everyone, and $S(Z)$ is the ML proxy predictor. Chernozhukov et al. (2020) show that the best linear predictor of CATE $s_0(Z)$ using the ML proxy predictor $S(Z)$ can be identified from the above regression: $BLP[s_0(Z)|S(Z)] = \beta_1 + \beta_2(S(Z) - \bar{S})$. Furthermore, $\beta_1 = E[s_0(Z)]$ and $\beta_2 = Cov(s_0(Z), S(Z))/Var(S(Z))$. The main implication is that if we can reject the null that $\beta_2 = 0$, we would conclude that 1) there is heterogeneity in $s_0(Z)$ and 2) $S(Z)$ is a relevant predictor for it.

Table 5: Heterogeneity Tests, T1 vs. T2+T3, Income above Median.

	Annualized visits		Any visits
	All	No high-users	All
A. Nurse visits			
Intercept	1.271 [0.043]	0.862 [0.020]	0.291 [0.005]
TREAT	-0.050 [0.054]	-0.024 [0.024]	-0.006 [0.006]
GROUP	-0.440 [0.052]	-0.236 [0.026]	-0.069 [0.007]
TREAT:GROUP	0.116 [0.067]	0.054 [0.032]	0.014 [0.008]
P-value	0.081	0.087	0.089
Change (G=0)	-3.95%	-2.76%	-1.98%
Change (G=1)	7.96%	4.89%	3.92%
B. GP visits			
Intercept	1.038 [0.022]	0.844 [0.017]	0.321 [0.005]
TREAT	0.010 [0.027]	0.000 [0.021]	0.001 [0.006]
GROUP	-0.284 [0.028]	-0.197 [0.023]	-0.071 [0.007]
TREAT:GROUP	-0.021 [0.035]	-0.022 [0.028]	-0.011 [0.009]
P-value	0.548	0.424	0.195
Change (G=0)	0.94%	0.05%	0.35%
Change (G=1)	-1.48%	-3.35%	-4.09%

Notes: We compare the base reminder group (T1) to the copayment reminder group (T2+T3), the former being the reference group. To define the high-users, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user. We use Model 1 and focus on curative nurse and GP visits in public primary care. Heteroskedasticity-robust standard errors in square brackets. The "GROUP" variable is an indicator for having equalized family disposable income above the median. The follow-up is six months. P-value is reported for the term "TREAT:GROUP". The percentage changes show the CATEs relative to the untreated observations in a given subgroup.

The BLP results are in Table 6. The β_1 estimates ($E[s_0(Z)]$) are of similar magnitude than the ATE estimates in Table A3.[10] All the estimates on heterogeneity loadings (β_2) are close to zero and insignificant, and many of them are even negative. Thus, we do not find any evidence of treatment effect heterogeneity. Negative heterogeneity loading estimates imply that the covariance of the CATE ($s_0(Z)$) and the proxy predictor ($S(Z)$) is negative. Basically, we do not estimate large CATEs in groups where we predict large effects based on the proxy predictor. This can be explained by a lack of heterogeneity (based on observables) or by a bad prediction model or both. We compared the predictive accuracy of our tuned random forest and XGBoost models to simply predicting for each individual the sample average and found that the tree-based learners are superior, the tuned XGBoost being the best (see Table A11).[11]

5 Discussion

We conduct a randomized informational outreach program to encourage individuals to seek care if they have health problems and to inform them about the abolition of a copayment for curative primary care nurse visits. We observe null results. Neither the letters in aggregate nor the copayment information have any observable effect on public primary care use in a 6-month follow-up or in a more granular analysis at the week level. We find no evidence to support our hypothesis that the lower end of the income distribution would respond more to the copayment information than the high-income individuals. Data-driven machine learning methods do not find heterogeneity in the effects.

Unfortunately, we do not reliably observe the number of first contacts to primary care. Consequently, we cannot infer whether the absence of any utilization effects is a result of zero effects on the number of first contacts or because the (potentially) increased first contacts do not lead to extra appointments when the patients are triaged by nurses.

[10]The BLP results are not multiplied by 100 whereas the estimates in Table A3 are.

[11]Specifically, we predict $E[Y|T=1,Z]$ and $E[Y|T=0,Z]$, use 50 random splits to equally-sized training and holdout sets, and report the median root mean squared error (RMSE) over the splits.

Table 6: Best Linear Predictor (BLP) of CATE on the ML Proxy Predictor.

	Nurse visits		GP visits	
	ATE (β_1)	HET (β_2)	ATE (β_1)	HET (β_2)
A. Random forest (default)				
Estimate	0.001	0.000	0.003	0.023
CI	(-0.005, 0.008)	(-0.079, 0.079)	(-0.004, 0.009)	(-0.056, 0.102)
P-value	[0.626]	[0.972]	[0.401]	[0.475]
B. Random forest (tuned)				
Estimate	0.001	-0.005	0.003	0.021
CI	(-0.005, 0.008)	(-0.090, 0.080)	(-0.004, 0.009)	(-0.065, 0.107)
P-value	[0.674]	[0.898]	[0.390]	[0.586]
C. XGBoost (default)				
Estimate	0.001	-0.033	0.002	-0.134
CI	(-0.005, 0.007)	(-0.346, 0.289)	(-0.005, 0.009)	(-0.465, 0.195)
P-value	[0.772]	[0.829]	[0.520]	[0.432]
D. XGBoost (tuned)				
Estimate	0.001	-0.041	0.002	-0.062
CI	(-0.005, 0.007)	(-0.233, 0.152)	(-0.005, 0.009)	(-0.234, 0.110)
P-value	[0.753]	[0.680]	[0.467]	[0.479]

Notes: The results are from Model 2, and we focus on curative nurse and GP visits in public primary care. We compare treated invidivuals (T1+T2+T3) to the controls (T0). Outcome is an indicator of having any visits in the follow-up. We report medians over 50 splits, 90% confidence intervals in parenthesis, and respective p-values in brackets. Rejecting the null that $\beta_2 = 0$ would mean that 1) there is heterogeneity in CATE and 2) the proxy predictor $S(Z)$ is a relevant predictor for CATE. The details on the tuned learners are in Section A.2. Observables include the covariates of Table 1, three primary care area dummies and two indicators for having a prescription for diabetes or hypertension drugs.

Earlier literature has shown that informational letters can in some circumstances affect health behavior in the Finnish public primary healthcare. Sääksvuori et al. (2022) find that reminding individuals to get vaccinated against influenza has a substantial positive effect on vaccination coverage in the elderly population. There are, however, several differences between influenza vaccinations and public primary care visits that may explain the difference in findings. Influenza vaccinations are available free-of-charge for all and as a walk-in service. The expected benefit - a reduced risk of infection and severe disease - is also positive for most. In contrast, access to public primary care visits is constrained by gatekeeping and wait times. If one feels well, both the probability of getting an appointment and the expected benefit of it conditional on getting one may seem small for an individual.

We hypothesized that the observed reduction in non-Covid healthcare use during the pandemic would have led to health problems accumulating and thereby creating an environment where our base treatment variant could have utilization effects. At the same time, the wave of Covid cases and hospitalizations of late 2021 and early 2022 in Finland probably also attenuated the potential effects of our trial by reducing the supply of public primary care for non-Covid patients. The effects of the copayment information were probably attenuated somewhat by the fact that the trial did not start instantly when the law change came into effect. In those three and a half months between the law change and the trial, many patients had curative nurse visits and thus learned that they were free-of-charge.

Besides treatment, health promotion and disease prevention are important functions of primary care. Our results suggest that reminding individuals to treat health problems early on may not be effective in inducing extra appointments in a system characterized by two-tier gatekeeping. Instead, a program like the voluntary health checks provided free-of-charge for the unemployed in Finland could have larger ITT effects in a similar informational intervention to induce exogenous variation in primary care use because access is not essentially restricted to those who exhibit clear symptoms. A program focusing on health checks could also work better in finding individuals who exhibit no symptoms but are at risk of developing a costly disease (e.g., those with high blood pressure or blood sugar).

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A Online Appendix

A.1 Constructing our Analysis Data

Sociodemographic and Socioeconomic Data. These data come from FOLK modules ”basic information”, ”income”, and ”family” from Statistics Finland. The covariates are measured at the end of each year, and some of them represent annual information (like the disposable income or the number of days unemployed). We use data from 2020 in this populated PAP. The subset of covariates selected for analyses are in Panel B and Panel C in Table 1. We do not observe these covariates for 0.07% of the trial population, explained by the fact that the trial population was extracted in September 2021, while the FOLK data only includes those individuals who had a permanent residence in Finland at the end of 2020. In addition, we set the equivalized family disposable income to be missing if it is observed to be exactly zero (0.1% of the rows). We exclude individuals with any missing values in covariates (0.2%) from those supplementary analyses that exploit covariates. Exact sample sizes are in corresponding results tables.

Primary Care Visits. Visits to public primary care and private outpatient care are extracted from the Register of Primary Health Care Visits. We compute the number of annualized primary care visits in trial areas for the trial population. We use data from 1/2021-5/2022 in this populated PAP. We begin by extracting those contacts where the patient ID and the visit start date are observed, the contact is a physical visit in outpatient care, and information on potential cancellation is missing. Then, we restrict to curative nurse and doctor visits. The coding rates of both the preventive/curative categorical and the profession class are high during the study period. Restricting to curative contacts is important as the national abolition of the nurse visit copayment effectively affected only curative nurse visits. Preventive nurse visits containing, e.g., vaccinations and public health nurse visits were free of charge already before the policy change.

Since 2019, the register has been collecting data from private outpatient care as well. This has two implications for data construction and analysis: we need to separate publicly-funded primary care visits from private outpatient visits, and we can also examine potential spillovers to private outpatient care.

To extract visits to public primary care, we link each visit via unit and organization IDs to the publicly available registers called TOPI and SOTE which contain information about healthcare providers. Both registers are continuously updated. In this populated PAP, we have two cross-sections of TOPI from 2021 and 2022 and one cross-section of SOTE from early 2022. Thus, TOPI is linked to the visit data at the unit-ID-by-year level. In our data, all visits contain a TOPI code, but the linking does not work for 4% of the rows. 0.06% of the rows have missing SOTE code, but the linking does not work for 0.1% of the rows.

Each unit in TOPI has one or more service area codes. We extract those units that are located in one of the trial municipalities and that have a service area code related to health centers (120, 121, 122). From the SOTE register, we extract units whose organization names map to the regional public primary care providers (see replication codes to observe the names). That is, public primary care visits are defined to be those where the provider 1) has a TOPI code referring to health centers and is located in the trial area (by TOPI) *or* 2) is part of the regional public primary care areas (by SOTE). Private outpatient visits are defined to be those where the provider 1) is located in the trial area (by TOPI) and does not have a TOPI code referring to health centers *and* 2) is located in the trial area (by SOTE) and does not belong to the regional public primary care area (by SOTE).

Since 2014, individuals have had the freedom of choosing their public primary care provider once a year, but actively choosing another provider than location-based default has been uncommon. There are some individuals who reside in our trial areas and are in the trial population, but who have actively chosen another public primary care provider outside our trial areas. We cannot distinguish these individuals from the data, and we do exclude the visits made by these individuals outside our target areas, slightly diminishing power.

Once we have separated nurse visits and GP visits in public primary care and private outpatient doctor visits, we sum contacts by date and service and create an indicator of the person having any such visits on the given date. That is, if the person has more than one nurse visit on a given date, we treat these events as one visit.

First contacts to primary care would have been an important outcome in this study. Our hypothesis was that the reminders increase first contacts to primary care, and some (but not all) of these first contacts lead to nurse and GP appointments. This is due to gatekeeping in the form of triage done by nurses who book appointments. Unfortunately, the corresponding coding rates are low in our trial regions. Consequently, we do not use first contacts as an outcome.

Prescriptions. The data on prescriptions come from Kanta Prescription Center, administered by the Social Insurance Institution of Finland. We compute the number of annualized prescriptions for the trial population, but we cannot restrict to prescriptions written in our trial areas. We extract unique prescriptions and aggregate them to the ID-by-date-by-sector level where the sector refers to the unit where the prescription was written (either public or private). We aim to separate prescriptions written by public primary care and private outpatient care and do this with the sector variable. This should work well in Kymenlaakso and South Karelia where public primary care is provided by public-sector organizations. However, the Päijät-Häme primary-care area outsourced primary care services in three of its ten member municipalities to a joint venture of the primary care area and a private firm in 1/2021. Since then, prescriptions written in public primary care of these three municipalities should be observed as private sector prescriptions in our data.

Specifically, we define public sector prescriptions in the following way: we extract public sector prescriptions unless the individual lives in one of the three municipalities whose primary care services are outsourced to a private firm. In the latter case, we include all prescriptions. Consequently, in the three municipalities we include not only prescriptions written in public primary care but also prescriptions written in private outpatient care.

Similarly, the private sector prescriptions we talk about in the study contain in the three outsourcing municipalities prescriptions written also in public primary care.

Referrals to Specialized Healthcare. Referrals are extracted from the Care Register for Health Care. We compute the number of annualized referrals for the trial population, but we cannot restrict to referrals written in our trial areas. We extract the events where the person ID and the date of arrival of the referral are observed. Then, we take unique ID-by-arrival-date-by-referring-organization-type observations where referring organization types include health centers (public primary care) and private outpatient care. The coding rate with respect to the organization type is high. When writing this populated PAP, we had only access to those referrals that had led to an actualized visit. This leads us to miss those referrals where the patient is in the waiting list. If we receive a new batch of data in the future, the problem will be addressed.

A.2 Tuning Random Forest and XGBoost

The data-driven analysis of treatment effect heterogeneity examines the comparison of the control group (T0) to the reminder group (T1+T2+T3). The sample of individuals for tuning are those in the reminder group who have no missing values in covariates. The R package *GenericML* does not currently allow us to use different models or hyperparameters to predict $E[Y|T=1,Z]$ and $E[Y|T=0,Z]$. Therefore, we decided to tune the models among the treated individuals who constitute 1/3 of the total trial population. Two thirds of these individuals are in the training set while the rest are in the holdout set.

We tune random forest and XGBoost learners for a regression problem and use the root mean squared error as the optimization metric. With respect to the random forest, we tune the number of trees (from 80 to 140) and the number of available variables (from 4 to 6) at each split. We use a histogram-based version of the XGBoost algorithm that grows the trees leaf-wise with a learning rate of 0.1. We tune the number of boosting iterations (from 50 to 120) and the maximum number of nodes to be added at each iteration (from 5 to 15).

We use grid search with grid resolution 10, but restrict the budget available for tuning by restricting the number of evaluations (30 for random forest and 60 for XGBoost).

A.3 Additional Figures and Tables

Table A1: Balance Tests: Standardized Mean Differences, P-values, and F-Tests, 1.

Variable	T0 vs. T1+T2+T3	T1 vs. T2+T3	T2 vs. T3
A. Prior health care use			
Primary care nurse visits	-0.002 (0.750)	0.008 (0.414)	0.011 (0.340)
Primary care GP visits	-0.001 (0.876)	-0.002 (0.838)	0.001 (0.932)
Private outpatient doctor visits	-0.007 (0.214)	-0.003 (0.771)	0.007 (0.519)
Prescriptions from private sector	-0.003 (0.587)	-0.014 (0.159)	0.007 (0.509)
Referrals from health centers	0.004 (0.517)	-0.010 (0.317)	-0.027 (0.015)
Referrals from private clinics	0.002 (0.731)	-0.007 (0.478)	0.012 (0.274)
B. Sociodemographic covariates			
Age	0.005 (0.395)	-0.007 (0.453)	0.010 (0.370)
Is male	0.003 (0.562)	-0.008 (0.408)	-0.011 (0.323)
Native language Finnish	-0.004 (0.471)	0.007 (0.471)	0.022 (0.056)
In relationship	0.001 (0.851)	0.019 (0.053)	0.020 (0.077)
Children living at home	0.007 (0.236)	-0.009 (0.360)	-0.025 (0.029)
C. Socioeconomic covariates			
Living in an apartment	-0.013 (0.017)	0.002 (0.798)	-0.001 (0.916)
Degree from tertiary education	-0.012 (0.031)	-0.006 (0.506)	-0.004 (0.731)
Equivalized disposable income	-0.005 (0.374)	0.018 (0.062)	0.011 (0.333)
Unemployed for at least one day	0.003 (0.614)	0.004 (0.663)	-0.014 (0.228)
Received social assistance	-0.012 (0.032)	-0.010 (0.295)	-0.015 (0.177)
Received sickness allowance	0.009 (0.113)	0.001 (0.929)	0.023 (0.042)
Joint F-tests (p-values):			
All individuals and covariates	0.071	0.359	0.022
Exclude the high-income folks	0.075	0.469	0.015
Only prior healthcare use	0.853	0.731	0.181

Notes: We report standardized mean differences and p-values (in parentheses) of a test of equality, estimated using OLS with heteroskedasticity-robust standard errors. The reference group is the group mentioned first in column names. F-tests are estimated in three cases: 1) with all individuals, 2) with only those with disposable income and equivalized family disposable income below 500,000 euros, and 3) using only the covariates on prior health care use in Table 1. Health care use is measured by the annualized number of contacts in 1/2021-6/2021, prior to the law reform in 7/2021 and our trial starting in 10/2021. Covariates in panels B and C are measured at the end of 2020.

Table A2: Balance Tests: Standardized Mean Differences, P-values, and F-Tests, 2.

Variable	T0 vs. T1	T0 vs. T2	T0 vs. T3
A. Prior health care use			
Primary care nurse visits	-0.007 (0.394)	-0.005 (0.557)	0.006 (0.506)
Primary care GP visits	0.000 (0.958)	-0.002 (0.812)	-0.001 (0.902)
Private outpatient doctor visits	-0.005 (0.547)	-0.012 (0.162)	-0.004 (0.624)
Prescriptions from private sector	0.006 (0.479)	-0.011 (0.186)	-0.004 (0.646)
Referrals from health centers	0.010 (0.238)	0.014 (0.112)	-0.014 (0.107)
Referrals from private clinics	0.007 (0.450)	-0.007 (0.441)	0.006 (0.508)
B. Sociodemographic covariates			
Age	0.010 (0.263)	-0.003 (0.754)	0.007 (0.388)
Is male	0.009 (0.315)	0.006 (0.475)	-0.005 (0.562)
Native language Finnish	-0.009 (0.312)	-0.012 (0.156)	0.009 (0.280)
In relationship	-0.012 (0.180)	-0.003 (0.758)	0.017 (0.045)
Children living at home	0.013 (0.145)	0.016 (0.067)	-0.009 (0.308)
C. Socioeconomic covariates			
Living in an apartment	-0.015 (0.080)	-0.012 (0.164)	-0.013 (0.125)
Degree from tertiary education	-0.008 (0.363)	-0.012 (0.149)	-0.016 (0.058)
Equivalized disposable income	-0.016 (0.039)	-0.005 (0.584)	0.006 (0.478)
Unemployed for at least one day	0.000 (0.999)	0.011 (0.203)	-0.003 (0.765)
Received social assistance	-0.005 (0.542)	-0.008 (0.352)	-0.023 (0.006)
Received sickness allowance	0.008 (0.334)	-0.002 (0.780)	0.020 (0.020)
Joint F-tests (p-values):			
All individuals and covariates	0.104	0.291	0.017
Exclude the high-income folks	0.208	0.295	0.009
Only prior healthcare use	0.683	0.388	0.722

Notes: We report standardized mean differences and p-values (in parentheses) of a test of equality, estimated using OLS with heteroskedasticity-robust standard errors. The reference group is the group mentioned first in column names. F-tests are estimated in three cases: 1) with all individuals, 2) with only those with disposable income and equivalized family disposable income below 500,000 euros, and 3) using only the covariates on prior health care use in Table 1. Health care use is measured by the annualized number of contacts in 1/2021-6/2021, prior to the law reform in 7/2021 and our trial starting in 10/2021. Covariates in panels B and C are measured at the end of 2020.

Table A3: Public Primary Care: Has Any Contact.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	25.590	28.039	66.236	6.340
Estimate	0.091	0.192	0.535	-0.200
Std. error	0.243	0.251	0.264	0.136
Confidence interval	[-0.386, 0.567]	[-0.300, 0.684]	[0.018, 1.052]	[-0.466, 0.065]
Change (%)	0.355	0.685	0.807	-3.161
Std. mean difference	0.002	0.004	0.011	-0.008
N: treated	47,398	47,398	47,398	47,398
N: reference group	94,796	94,796	94,796	94,796
B. Base vs. copayment reminder				
Reference group mean	25.608	28.557	66.804	6.234
Estimate	0.110	-0.487	-0.049	-0.142
Std. error	0.421	0.436	0.456	0.235
Confidence interval	[-0.715, 0.934]	[-1.341, 0.367]	[-0.943, 0.845]	[-0.602, 0.318]
Change (%)	0.429	-1.705	-0.074	-2.273
Std. mean difference	0.003	-0.011	-0.001	-0.006
N: treated	31,598	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800	15,800

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A4: Public Primary Care: T2 vs. T3.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No. of annualized contacts				
Reference group mean	1.045	0.911	6.266	0.132
Estimate	0.025	-0.037	-0.018	0.004
Std. error	0.042	0.020	0.094	0.006
Confidence interval	[-0.056, 0.107]	[-0.077, 0.003]	[-0.202, 0.165]	[-0.008, 0.017]
Change (%)	2.406	-4.075	-0.294	3.273
Std. mean difference	0.007	-0.020	-0.002	0.008
N: treated	15,798	15,798	15,798	15,798
N: reference group	15,800	15,800	15,800	15,800
B. Has any contact				
Reference group mean	25.589	28.316	66.873	5.937
Estimate	0.258	-0.497	-0.239	0.311
Std. error	0.487	0.502	0.527	0.269
Confidence interval	[-0.696, 1.213]	[-1.481, 0.487]	[-1.273, 0.794]	[-0.216, 0.838]
Change (%)	1.010	-1.754	-0.358	5.235
Std. mean difference	0.006	-0.011	-0.005	0.013
N: treated	15,798	15,798	15,798	15,798
N: reference group	15,800	15,800	15,800	15,800

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is T2. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A5: Public Primary Care: Annualized Number of Contacts, Excluding High-Users.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	0.734	0.734	5.517	0.121
Estimate	0.012	0.003	0.012	-0.002
Std. error	0.009	0.008	0.039	0.003
Confidence interval	[-0.006, 0.030]	[-0.013, 0.018]	[-0.065, 0.088]	[-0.008, 0.003]
Change (%)	1.621	0.404	0.210	-2.031
Std. mean difference	0.008	0.002	0.002	-0.005
N: treated	45,307	45,307	45,307	45,307
N: reference group	90,505	90,505	90,505	90,505
B. Base vs. copayment reminder				
Reference group mean	0.744	0.745	5.508	0.121
Estimate	0.002	-0.012	0.027	-0.004
Std. error	0.016	0.014	0.068	0.005
Confidence interval	[-0.029, 0.033]	[-0.039, 0.015]	[-0.106, 0.160]	[-0.014, 0.006]
Change (%)	0.231	-1.631	0.493	-3.190
Std. mean difference	0.001	-0.009	0.004	-0.008
N: treated	30,232	30,232	30,232	30,232
N: reference group	15,075	15,075	15,075	15,075

Notes: To define the high-users, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user. We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A6: Public Primary Care: Has Any Contact, Excluding High-Users.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	23.814	26.418	65.041	5.667
Estimate	0.173	0.218	0.611	-0.118
Std. error	0.243	0.253	0.272	0.132
Confidence interval	[-0.303, 0.649]	[-0.277, 0.713]	[0.077, 1.144]	[-0.377, 0.141]
Change (%)	0.727	0.826	0.939	-2.086
Std. mean difference	0.004	0.005	0.013	-0.005
N: treated	45,307	45,307	45,307	45,307
N: reference group	90,505	90,505	90,505	90,505
B. Base vs. copayment reminder				
Reference group mean	23.900	26.992	65.645	5.718
Estimate	0.118	-0.542	-0.025	-0.256
Std. error	0.421	0.439	0.471	0.230
Confidence interval	[-0.707, 0.943]	[-1.402, 0.317]	[-0.948, 0.898]	[-0.707, 0.195]
Change (%)	0.495	-2.010	-0.038	-4.476
Std. mean difference	0.003	-0.012	0.000	-0.011
N: treated	30,232	30,232	30,232	30,232
N: reference group	15,075	15,075	15,075	15,075

Notes: To define the high-users, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user. We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care. Prescriptions are written by public sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include private sector prescriptions, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by public primary care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A7: Private Outpatient Care: Has Any Contact.

	Doctor visits	Prescriptions	Referrals
A. No vs. any reminder			
Reference group mean	4.720	23.680	3.092
Estimate	-0.177	-0.101	0.172
Std. error	0.117	0.238	0.099
Confidence interval	[-0.406, 0.052]	[-0.567, 0.365]	[-0.022, 0.366]
Change (%)	-3.753	-0.428	5.562
Std. mean difference	-0.008	-0.002	0.010
N: treated	47,398	47,398	47,398
N: reference group	94,796	94,796	94,796
B. Base vs. copayment reminder			
Reference group mean	4.614	23.759	3.203
Estimate	-0.108	-0.272	0.092
Std. error	0.202	0.412	0.172
Confidence interval	[-0.503, 0.288]	[-1.080, 0.536]	[-0.246, 0.430]
Change (%)	-2.333	-1.144	2.872
Std. mean difference	-0.005	-0.006	0.005
N: treated	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. Prescriptions are written by private sector providers. Note that for three municipalities in the Päijät-Häme primary care area, we also include prescriptions written in public primary care, because their primary care provider is a private corporation. Referrals to specialized healthcare are written by private outpatient care. We only observe referrals that have led to an actual visit or procedure by the end of May 2022.

Table A8: Household Spillovers: Public Primary Care Use.

	No. of annualized visits		Has any visit	
	Nurse visits	GP visits	Nurse visits	GP visits
A. No vs. any reminder				
Reference group mean	1.026	0.873	26.122	27.780
Estimate	-0.014	0.021	-0.176	0.629
Std. error	0.027	0.016	0.387	0.398
Confidence interval	[-0.066, 0.038]	[-0.010, 0.051]	[-0.936, 0.583]	[-0.151, 1.409]
Change (%)	-1.399	2.365	-0.675	2.265
Std. mean difference	-0.005	0.011	-0.004	0.014
N: treated	18,947	18,947	18,947	18,947
N: reference group	37,516	37,516	37,516	37,516
B. Base vs. copayment reminder				
Reference group mean	0.994	0.881	25.751	27.971
Estimate	0.019	0.012	0.176	0.483
Std. error	0.046	0.027	0.671	0.691
Confidence interval	[-0.072, 0.111]	[-0.042, 0.065]	[-1.140, 1.491]	[-0.872, 1.838]
Change (%)	1.957	1.320	0.682	1.725
Std. mean difference	0.009	0.010	0.006	0.015
N: treated	12,687	12,687	12,687	12,687
N: reference group	6,260	6,260	6,260	6,260

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up is six months. The reference group is the group mentioned first in panel titles. The sample includes those sample individuals from letter households that did not personally receive the letter and their randomized counterparts in the control group. Nurse and GP visits are curative visits in public primary care.

Table A9: Heterogeneity Tests, T0 vs. T1+T2+T3, Has Any Visit.

	Income ≥ median	Age ≥ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	0.289 [0.002]	0.186 [0.002]	0.158 [0.002]	0.278 [0.002]
TREAT	-0.002 [0.004]	-0.000 [0.003]	-0.002 [0.003]	0.002 [0.003]
GROUP	-0.065 [0.003]	0.133 [0.003]	0.258 [0.003]	-0.084 [0.003]
TREAT:GROUP	0.005 [0.005]	0.002 [0.005]	0.007 [0.005]	-0.003 [0.005]
P-value	0.305	0.634	0.185	0.553
Change (G=0)	-0.59%	-0.22%	-1.00%	0.65%
Change (G=1)	1.49%	0.59%	1.28%	-0.68%
B. GP visits				
Intercept	0.321 [0.002]	0.210 [0.002]	0.182 [0.002]	0.298 [0.002]
TREAT	0.000 [0.004]	0.003 [0.003]	0.003 [0.003]	0.002 [0.003]
GROUP	-0.081 [0.003]	0.135 [0.003]	0.260 [0.003]	-0.067 [0.003]
TREAT:GROUP	0.003 [0.005]	-0.002 [0.005]	-0.003 [0.005]	0.001 [0.006]
P-value	0.584	0.676	0.552	0.851
Change (G=0)	0.15%	1.43%	1.84%	0.62%
Change (G=1)	1.35%	0.27%	0.04%	1.25%

Notes: We compare the controls (T0) to the reminder group (T1+T2+T3), the former being the reference group. We use Model 1 and focus on curative nurse and GP visits in public primary care. Heteroskedasticity-robust standard errors in square brackets. ”Pre-trial visits” is an indicator for having any curative nurse or GP visits in 1/2021-6/2021, before the law change and our trial. The other ”GROUP” variables are indicators for having age or equalized family disposable income above the median and for residing in a municipality where the public primary care is outsourced (Lahti, Iitti, and Kärkölä). The follow-up is six months. P-value is reported for the term ”TREAT:GROUP”. The percentage changes show the CATEs relative to the untreated observations in a given subgroup.

Table A10: Heterogeneity Tests, T0 vs. T1+T2+T3, Annualized Visits Excluding High-Users.

	Income ≥ median	Age ≥ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	0.833 [0.008]	0.508 [0.006]	0.422 [0.005]	0.809 [0.006]
TREAT	0.015 [0.014]	0.001 [0.011]	-0.006 [0.008]	0.014 [0.011]
GROUP	-0.196 [0.010]	0.435 [0.010]	0.850 [0.012]	-0.288 [0.011]
TREAT:GROUP	-0.006 [0.018]	0.021 [0.018]	0.051 [0.021]	-0.006 [0.019]
P-value	0.753	0.233	0.016	0.769
Change (G=0)	1.75%	0.20%	-1.45%	1.69%
Change (G=1)	1.38%	2.37%	3.49%	1.56%
B. GP visits				
Intercept	0.840 [0.007]	0.539 [0.006]	0.464 [0.005]	0.785 [0.006]
TREAT	0.005 [0.012]	0.013 [0.010]	0.002 [0.008]	-0.001 [0.010]
GROUP	-0.209 [0.009]	0.377 [0.009]	0.738 [0.010]	-0.193 [0.010]
TREAT:GROUP	-0.006 [0.016]	-0.019 [0.016]	0.004 [0.018]	0.015 [0.017]
P-value	0.729	0.237	0.843	0.390
Change (G=0)	0.64%	2.38%	0.45%	-0.07%
Change (G=1)	-0.02%	-0.63%	0.47%	2.41%

Notes: We compare the controls (T0) to the reminder group (T1+T2+T3), the former being the reference group. To define the high-users, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user. We use Model 1 and focus on curative nurse and GP visits in public primary care. Heteroskedasticity-robust standard errors in square brackets. "Pre-trial visits" is an indicator for having any curative nurse or GP visits in 1/2021-6/2021, before the law change and our trial. The other "GROUP" variables are indicators for having age or equalized family disposable income above the median and for residing in a municipality where the public primary care is outsourced (Lahti, Iitti, and Kärkölä). The follow-up is six months. P-value is reported for the term "TREAT:GROUP". The percentage changes show the CATEs relative to the untreated observations in a given subgroup.

Table A11: Assessing Predictive Performance.

	RMSE	
	Nurse visits	GP visits
A. T1+T2+T2		
Use outcome mean	0.437	0.450
Random forest (tuned)	0.407	0.425
XGBoost (tuned)	0.403	0.422
B. T0		
Use outcome mean	0.437	0.449
Random forest (tuned)	0.407	0.422
XGBoost (tuned)	0.404	0.420

Notes: Outcome is an indicator of having any visits in the follow-up, focusing on curative nurse and GP visits in public primary care. We predict $E[Y|T=1,Z]$ and $E[Y|T=1,Z]$, use 50 random splits to equally-sized training and holdout sets, and report the median root mean squared error (RMSE) over the splits. The details on the tuned learners are in Section A.2. Observables include the covariates of Table 1, three primary care area dummies and two indicators for having a prescription for diabetes or hypertension drugs.

Terveyden ja hyvinvoinnin laitos

Informational letter

5.10.2021
1(1)

Dear recipient

Many non-Covid health care contacts have been missed during the Covid-19 pandemic in Finland. If care is not sought at the right time, there is a risk of further deterioration of health. Diagnosing and treating diseases may be delayed. Chronic conditions may worsen.

With this letter, we want to remind you and your household members that you can contact your local health center to treat potential health problems.

The public primary care provider of your area of residence is South Karelia Social and Health Care District (Eksote). If you feel the need to treat health problems, you can contact a healthcare professional by calling Eksote's telephone service 05 352 7260 or by using electronic health services (www.hyvis.fi or www.omaolo.fi). The telephone service is open 7-16 on weekdays, but in the near future the service will be open 7-20. The need for an appointment is assessed by the professional.

The letter is part of an informational outreach campaign by Finnish Institute for Health and Welfare (THL) targeted for those aged 55 or more. THL is not responsible for providing health care services nor booking appointments. Extra information about the letter can be received by calling THL: 029 524 6185 (9-16 on weekdays).

The recipients were chosen based on birth year extracted from the Population Information System. If your household has more than one individual aged 55 or more, we randomized the recipient for economical and environmental reasons. Please share the content of the letter with your household members.

www.thl.fi

Terveyden ja hyvinvoinnin laitos • Institutet för hälsa och välfärd • Finnish Institute for Health and Welfare
Mannerheimintie 166, Helsinki, Finland PL/PB/P.O. Box 30, FI-00271 Helsinki, puh/tel +358 29 524 6000
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Figure A1: Base Letter (T1) in South Karelia.

Terveyden ja
hyvinvoinnin laitos

Informational letter

5.10.2021
1(1)

Dear recipient

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With this letter, we want to remind you and your household members that you can contact your local health center to treat potential health problems. We also want to inform you about the reformed Act on Client Charges in Healthcare and Social Welfare which has affected primary care copayments. **Due to the new law, all primary care nurse visits have become free of charge in Finland from July 1st, 2021.**

The public primary care provider of your area of residence is South Karelia Social and Health Care District (Eksote). If you feel the need to treat health problems, you can contact a healthcare professional by calling Eksote's telephone service 05 352 7260 or by using electronic health services (www.hyvis.fi or www.omaolo.fi). The telephone service is open 7-16 on weekdays, but in the near future the service will be open 7-20. The need for an appointment is then assessed by the professional.

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Figure A2: Copayment Treatment Variant No. 1 (T2) in South Karelia.

Terveyden ja
hyvinvoinnin laitos

Informational letter

5.10.2021
1(1)

Dear recipient

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With this letter, we want to remind you and your household members that you can contact your local health center to treat potential health problems. We also want to inform you about the reformed Act on Client Charges in Healthcare and Social Welfare which has affected primary care copayments. **Due to the new law, all primary care nurse visits have become free of charge in Finland from July 1st, 2021.** Primary care GP visit copayments remain unchanged: 20.60 euros per visit (or, alternatively, an annual copayment of 41.20 euros).

The public primary care provider of your area of residence is South Karelia Social and Health Care District (Eksote). If you feel the need to treat health problems, you can contact a healthcare professional by calling Eksote's telephone service 05 352 7260 or by using electronic health services (www.hyvis.fi or www.omaolo.fi). The telephone service is open 7-16 on weekdays, but in the near future the service will be open 7-20. The need for an appointment is then assessed by the professional.

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Figure A3: Copayment Treatment Variant No. 2 (T3) in South Karelia.

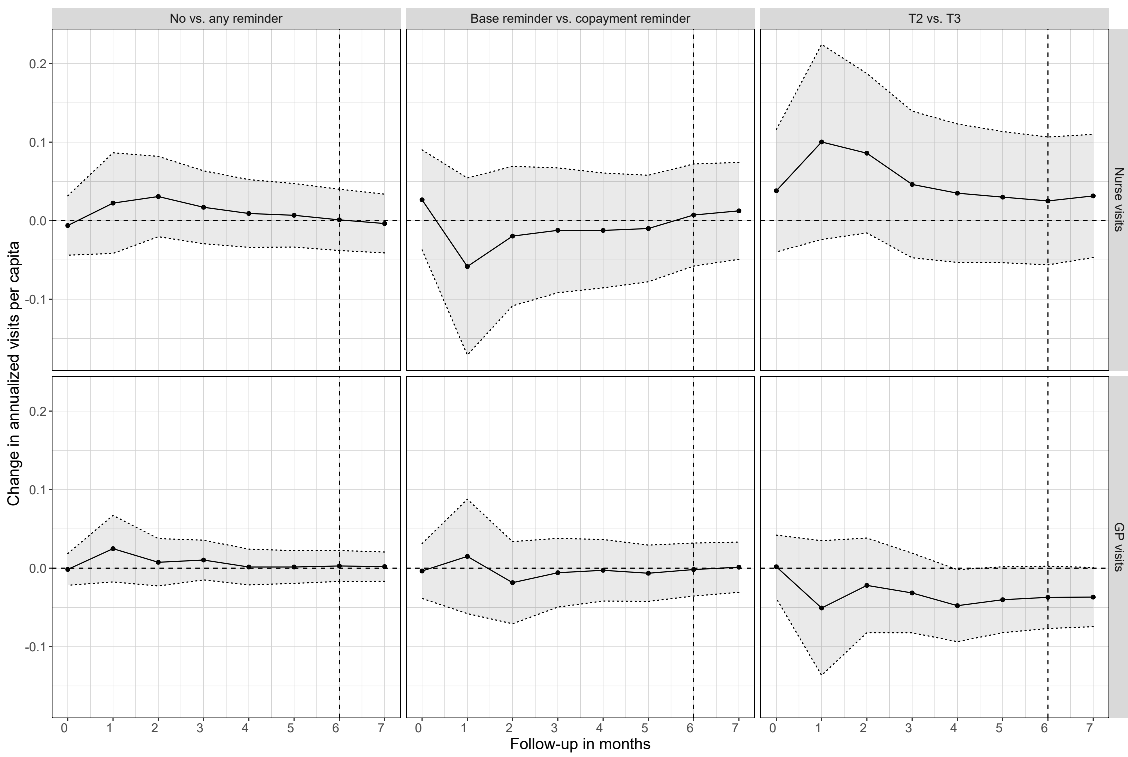

Figure A4: Public Primary Care: Annualized Number of Contacts, Varying Follow-up Length.

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up of 0 months refers to the period of 1/2021-6/2021. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care.

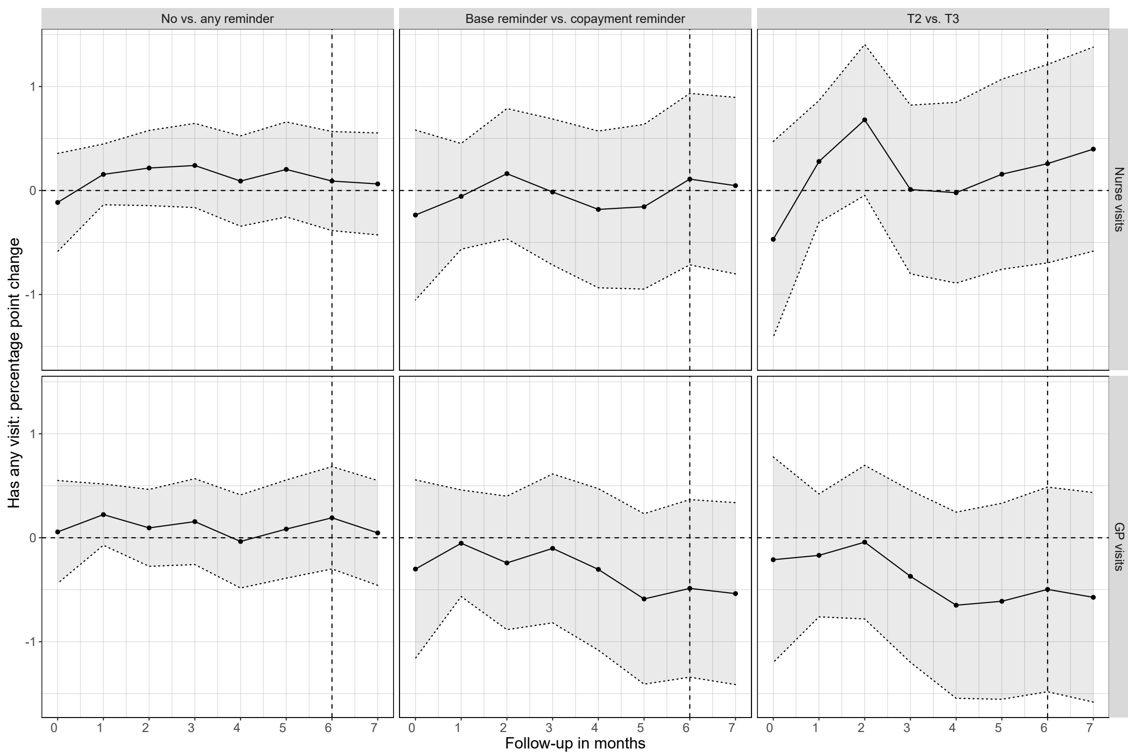

Figure A5: Public Primary Care: Has Any Contact, Varying Follow-up Length

Notes: We analyze the trial using Neyman's repeated sampling approach taking into account the stratified randomization at the municipal level, following (Athey and Imbens, 2017). The follow-up of 0 months refers to the period of 1/2021-6/2021. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care.

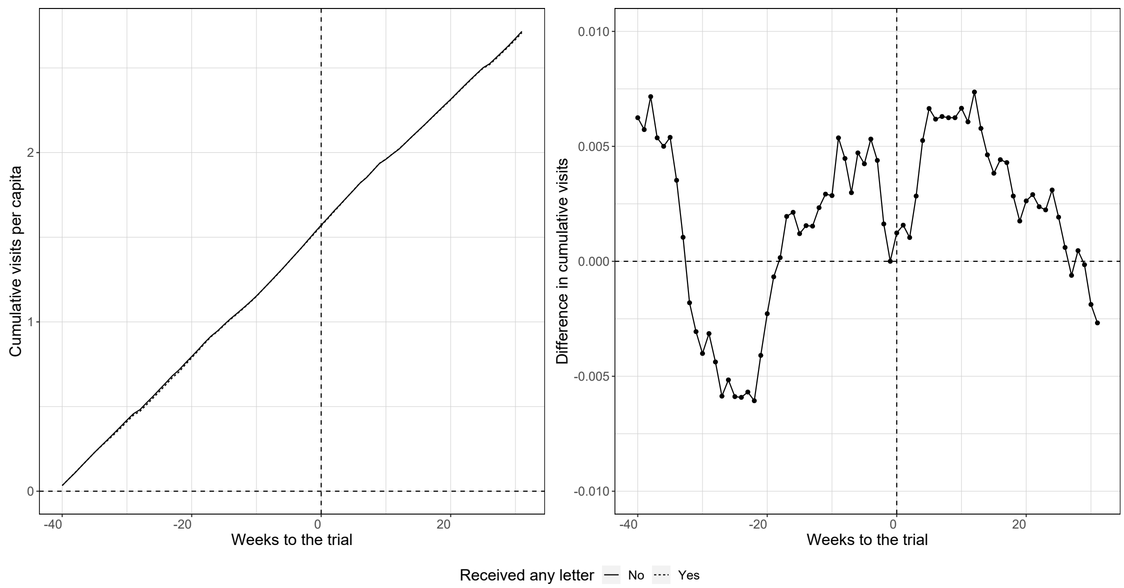

Figure A6: Public Primary Care: the Accumulation of Nurse and GP Visits over Time.

Notes: **This analysis was not pre-registered.** On the left, we plot the number of cumulative curative nurse and GP visits per capita by treatment group (T0 vs. T1+T2+T3), the reference period being the week -40 before the start of the trial. On the right, we zoom to the corresponding difference between the letter group and the comparisons. Negative values imply that the cumulative primary care use at a given point in time was larger in the comparison group.

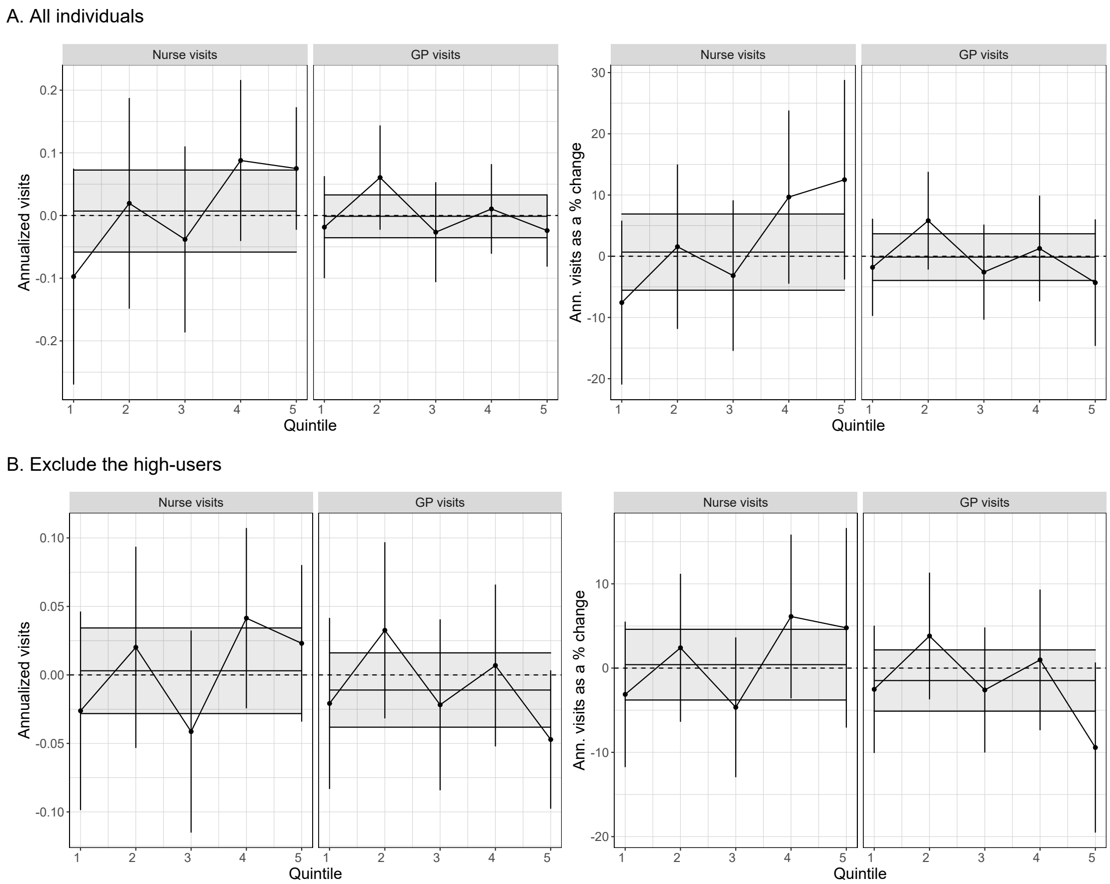

Figure A7: Public Primary Care: Effects of Copayment Information by Income Quintiles.

Notes: We compare the base reminder group (T1) to the copayment reminder group (T2+T3). The regression model contains an indicator for the treated individuals and municipality fixed effects. Standard errors are robust to heteroskedasticity. We also map the point estimates and their confidence intervals to percentage changes by dividing the estimate by the control group mean and multiplying by 100. The follow-up is six months. Nurse and GP visits are curative outpatient visits to public primary care. The grey block, centered at the black horizontal line, shows the ATE estimate and its confidence interval. Quintiles are derived from the distribution of equivalised family disposable income. In Panel B, the high-users are excluded. To define them, we first compute the 99th percentiles with respect to the number of 1) curative nurse and 2) GP visits in public primary care, and 3) public and 4) private sector prescriptions in the 6-month follow-up. If an individual's health care use is at least the 99th percentile score in one or more of these four dimensions, we treat the person as a high-user.

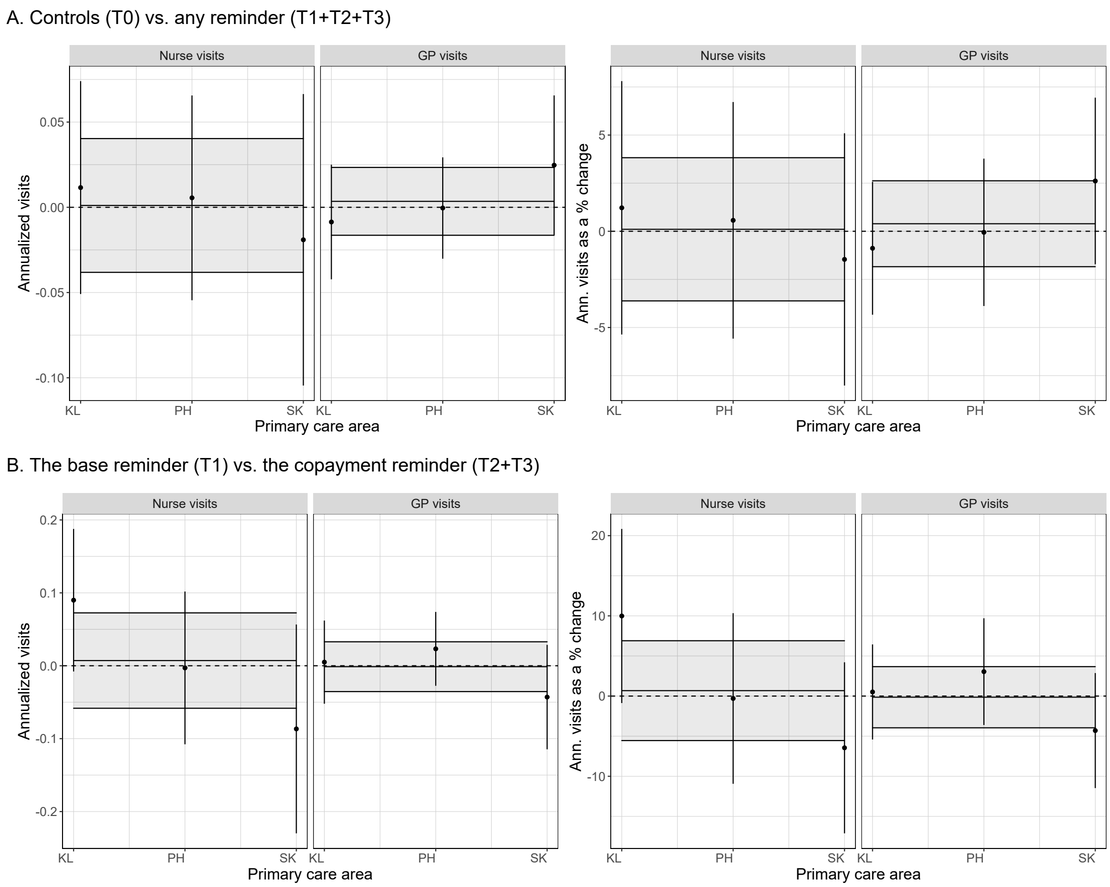

Figure A8: Public Primary Care: CATEs by Primary Care Area.

Notes: The reference group is the group mentioned first in panel titles. The regression model contains an indicator for the treated individuals and municipality fixed effects. Standard errors are robust to heteroskedasticity. We also map the point estimates and their confidence intervals to percentage changes by dividing the estimate by the control group mean and multiplying by 100. The follow-up is six months. Nurse and GP visits are curative outpatient visits to public primary care. The grey block, centered at the black horizontal line, shows the ATE estimate and its confidence interval.