Reminders, Cost Sharing, and Health Care Use

Tapio Haaga, Lauri Sääksvuori, and Jussi Tervola*

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Abstract

This pre-analysis plan is a time placebo analysis of a large-scale randomized controlled trial that examines the effects of an information intervention on health care use in Finland. Our letter intervention reminded individuals about the importance of early detection and treatment of health conditions. Moreover, there were treatment arms that additionally informed about the recent abolition of copayments for curative primary care nurse visits. Our primary objectives are twofold. First, we aim to examine how an exogenous (hypothesized) increase in demand for primary care affects the use of health care services in a health system characterized by strict gatekeeping. Using rich administrative data, we do not only examine how increased demand affects public primary care use (curative nurse visits and GP visits) but also prescriptions and referrals to specialist consultations. Second, we estimate how price information about nurse visit copayments (zero after the abolition) affects the consumption of medical care. Finally, we examine the potential heterogeneity of treatment effects using pre-registered heterogeneity tests and data-driven machine learning methods.

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

***Haaga:** Finnish Institute for Health and Welfare (THL), University of Turku (email: tapio.haaga@thl.fi). **Sääksvuori:** Finnish Institute for Health and Welfare (THL) (email: lauri.saaksvuori@thl.fi). **Tervola:** Finnish Institute for Health and Welfare (THL) (email: jussi.tervola@thl.fi). **Acknowledgements:** We thank Mika Kortelainen for comments and suggestions. This work is supported by Finnish Ministry of Social Affairs and Health. **Trial registration:** AEA RCT registry (AAEARCTR-0008285). **Ethical assessment:** Finnish Institute for Health and Welfare Institutional Review Board (THL/3181/6.02.01/2021). **Replication codes:** `https://github.com/tapiohaa/ASMA4`.

Contents

1 Introduction

We conducted a randomized controlled trial to examine the effects of mailed reminders on health care use in Finland. The information intervention reminded individuals about the importance of early detection and treatment of health conditions. Moreover, there were treatment arms that additionally informed individuals about the recent abolition of copayments for curative primary care nurse visits. We motivate the intervention by the strong belief that missed non-Covid-19 health care use has been accumulating during the pandemic.

Our primary objectives are twofold. First, we aim to examine how an exogenous (hypothesized) increase in demand for primary care affects the use of health care services in a health system characterized by strict gatekeeping. Using rich administrative data, we do not only examine how increased demand affects public primary care use (curative nurse visits and GP visits) but also prescriptions and referrals to specialist consultations, proxying professional-assessed need for diagnosis and treatment. Second, we estimate how price information about nurse visits affects the consumption of medical care. Finally, we examine the potential heterogeneity of treatment effects using pre-registered heterogeneity tests and data-driven machine learning methods.

Our paper builds on and contributes to the literature that has examined how cost sharing (Newhouse and the Insurance Experiment Group, 1993; Finkelstein et al., 2012) and price transparency (Whaley et al., 2014; Lieber, 2017) affect the use of health care services. However, in contrast to directly randomizing different financial incentive schemes to patients, our study intends to generate an exogenous change in demand for primary care using mailed reminders. 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. In this sense, our study is related to the question of the optimal allocation of resources between primary and specialized healthcare in nationalized healthcare systems characterized by strict gatekeeping.

We test whether information about the abolition of the nurse visit copayment increases public primary care use. The copayment for curative primary care nurse visits was abolished nationally three and a half months before the beginning of our trial to reduce barriers to health care and diminish health inequality. The copayments for nurse visits varied from 10 to 21 euros per visit in our target areas. Previous literature has shown that out-of-pocket costs decrease demand for health care (Einav and Finkelstein, 2018). This finding has also been made in the Nordic context where out-of-pocket costs are moderate or small, pricing is transparent (a copayment per visit), and inequality low (Johansson et al., 2019; Magnussen Landsem and Magnussen, 2018; Nilsson and Paul, 2018). For price effects to exist, some people need to have perceptions about the prices. However, we hypothesize that there is also a considerable fraction of individuals who do not have correct information, potentially affecting their health care use. Incomplete information has earlier been shown to lead to underutilization of social benefits (Engström et al., 2019; Matikka and Paukkeri, 2019) and higher drug costs (Kling et al., 2012).

The objectives of well-intending policies, such as to lower the barrier to access through abolishing copayments, may not be achieved if the general population is not aware of the reform. Still, the overwhelming majority of impact evaluation literature in economics typically focuses on reporting the results of the initial policy change (intent-to-treat) and bypasses the question of how large the treatment effect could be after informing individuals through outreach campaigns. Our study empirically contributes towards the aim of understanding how informing citizenry about policy changes can affect the achievement of policy objectives. Overall, this paper complements the (planned) quasi-experimental evaluation of the effects of the copayment abolition and its earlier staggered adoption on primary care use (Haaga et al., 2022).

This pre-analysis plan (PAP) (or a placebo report) was written after randomization and sending the reminders but before having access to data for years 2021-2022 when the trial occurred. We wrote the statistical programs as if the reminders were sent two years earlier, in October-November 2019, to conduct a placebo study, and report the corresponding placebo results in this PAP, representing the analyses we plan to conduct and how we plan to report them. Statistical programs are provided as a supplement. Our main motivation to register this detailed PAP as a supplement to the original general-level pre-trial registration (AEA RCT Registry: AAEARCTR-0008285) is to be able to make the potentially influential data preparation and analysis choices and write the corresponding statistical programs without getting any feedback from the real outcome data, thereby increasing research transparency and credibility.

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.

For the patient, contacting public primary care is rather simple. The provider of primary care services is determined by the municipality of residence[1]. 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 reminder (T0 or control). The study includes three active treatment arms (T1-T3), written reminders varying by their content. The base reminder (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 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 reminder provides contact information of the local health center and information about the letter campaign: 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 were extracted from the Finnish Population Register. Only one reminder was sent to a household for economical and environmental reasons. Finally, the reminder encourages to inform other household members as well. The reminders are in both

[1]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.

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 reminders (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 treatment arm also tells the level of GP visit copayments in public primary care and that they remain unchanged. We show the original reminder letters for interventions T1 and T2 in figures A1 and A2 (English translations to follow).

Study Population. The trial was 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). We selected these areas because they are geographically large and diverse regional primary care areas that charged a copayment for curative nurse visits before the national abolition in July 2021 and that contain both cities and rural areas. We restricted 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 included 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). These restrictions leave us with 198,657 residents in 142,194 households.

Randomization. Randomization took 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, were randomized to the control group (T0) while the remaining 1/3 of the eligible households were randomly split into three equally-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 sent 47,398 reminders. The size of the experiment was constrained by the budget available. The reminders were sent in four waves over four weeks - four equally-sized waves were 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 stratified 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). Stratification also enables us to provide better localized estimates for local policy advise.

Implementation. The addresses were extracted from the Finnish Population Information System. Our team conducted the randomization. We hired an external firm to send the reminders. As there are three different types of reminders tailored for three regions, there are nine different reminders in total. They were sent via regular post to treated individuals in four waves of equal size over four weeks (October 13th, October 20th, October 27th, and November 3rd in 2021). The follow-up time of six months starts from October 13th, 2021. The research group did not have direct contact with the treated individuals.

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 linked to several administrative registries 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 registries 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. We use these datasets from 2019-2020 in this PAP, and will use data from 2021-2022 in the final report (the trial started in October 2021). Sociodemographic and socioeconomic information from the end 2019 come from Statistics Finland's FOLK modules "basic information", "family", and "income". In the final report, we will use these data from the end of 2021. Access to these registries can be applied for through Findata and Statistics Finland. We also use two publicly available registries 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.

In the main text, we report the effects of the reminders 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 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 reminders provided 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.

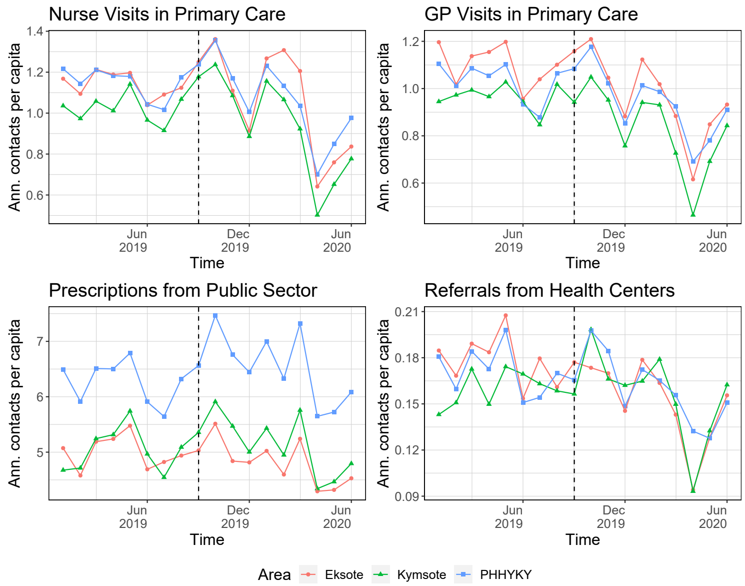

Figure 1: Public Primary Care: Trends in Annualized Number of Contacts per Capita.

Notes: The plots show the evolution in public primary care contacts in 2019-2020 in Kymenlaakso (Kymsote), Päijät-Häme (PHHYKY), and South Karelia (Eksote). The start of the placebo trial in 10/2019 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.

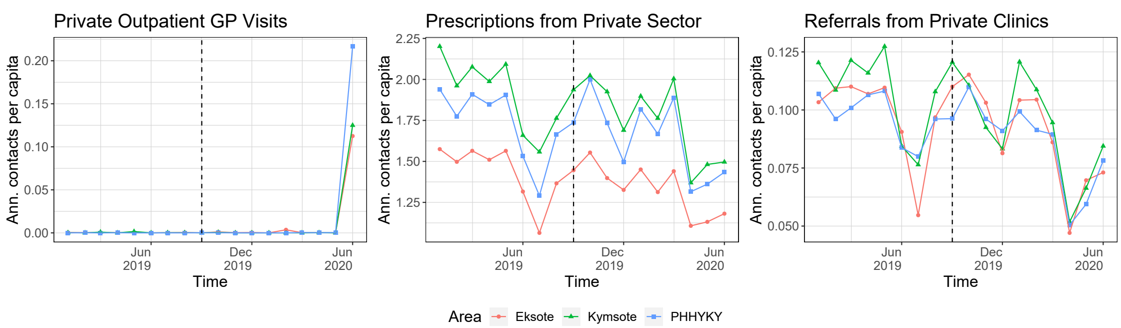

Figure 2: Private Outpatient Care: Trends in Annualized Number of Contacts per Capita.

Notes: The plots show the evolution in private outpatient care contacts in 2019-2020 in Kymenlaakso (Kymsote), Päijät-Häme (PHHYKY), and South Karelia (Eksote). The start of the placebo trial in 10/2019 is by dashed vertical line. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas. 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.

3.3 Descriptive Statistics

Table 1 reports the observed means for the controls (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 selected 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 tables A1 and A2. These tests were conducted to assess covariate balance and whether there is need for covariate adjustment in robustness checks. The tests were conducted using OLS with heteroskedasticity-robust standard errors. Before regression estimation, we excluded covariates that were highly collinear, defined as having a correlation of more than 0.5 in absolute value[2]. We report p-values from F-tests in three cases: 1) including all individuals and covariates of Table A1, 2) including only 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 leading 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). All F-statistics are insignificant. The p-values are large especially when only leading health care use is considered. The comparison of T1 vs. T2+T3 produces the smallest F-test p-values. When testing the equality of means by covariate, four 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,

[2]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), has Finnish background (Finnish as a native language), and prescriptions from public sector (GP visits in public primary care).

and T3) separately. The F-test p-values are large when comparing T0 to T2 or T0 to T3, but the F-statistic is significant if we compare T0 to T1 and use the 17 covariates appearing in the table.

3.4 Statistical Methods

Main Analysis. The main comparisons that we report throughout the paper are the following: comparing treated individuals (T1+T2+T3) to the controls (T0), and the copayment reminder group (T2+T3) to the base reminder group (T1). The follow-up of six months begins from the start of our (placebo) trial, October 13th, 2019. 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.[3] 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 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). The main text reports changes in the annualized contacts per capita, but we also report in the Online Appendix changes in the indicator of having any contacts.

Supplementary Analysis. We do not use covariates in the main analysis. However, 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 1/2019-6/2019 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

[3] 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. Leading health care use				
Primary care nurse visits	1.129	1.101	1.153	1.151
Primary care GP visits	1.057	1.050	1.055	1.060
Private outpatient doctor visits	-	-	-	-
Prescriptions from public sector	5.744	5.753	5.712	5.668
Prescriptions from private sector	1.750	1.794	1.754	1.688
Referrals from health centers	0.173	0.176	0.172	0.173
Referrals from private clinics	0.103	0.107	0.100	0.102
B. Sociodemographic covariates				
Age	67.946	68.038	67.924	68.022
Is male	44.31%	44.75%	44.58%	44.05%
Has Finnish background	96.69%	96.48%	96.66%	96.84%
Native language Finnish	96.48%	96.30%	96.27%	96.67%
In relationship	41.83%	41.29%	41.67%	42.76%
Widowed	16.48%	17.39%	16.92%	16.57%
Children living at home	10.55%	11.03%	11.08%	10.30%
C. Socioeconomic covariates				
Living in an apartment	39.75%	39.01%	39.10%	39.00%
Degree from tertiary education	11.77%	11.48%	11.38%	11.23%
Pensioner	62.82%	62.65%	62.42%	63.12%
Disposable income	23.190	22.805	23.170	23.160
Equivalized disposable income	26.546	26.261	26.439	26.611
Unemployed for at least one day	7.55%	8.07%	7.83%	7.42%
Unemployd for at least 197 days	3.89%	3.79%	4.03%	3.87%
Received social assistance	4.23%	4.40%	4.29%	3.92%
Received sickness allowance	2.79%	2.97%	2.88%	2.63%

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 1/2019-6/2019, prior to the (placebo) law reform in 7/2019 and our (placebo) trial starting in 10/2019. Covariates in panels B and C are measured at the end of 2019, and their sample sizes are slightly lower than reported in the table due to missing values in 0.3% of the rows. Income is measured in thousands. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas.

standard errors, including an indicator for the treated group and municipality fixed effects. When using propensity-score based subclassification matching, we use 10,000[4] subclasses, the propensity scores being estimated with logistic regression and the covariates from Table A1. 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 Analysis. 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:

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

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 $s_0(Z)$. These features include:

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

- Best Linear Predictor (BLP) of $s_0(Z)$ on the ML proxy predictor $S(Z)$. The proxy predictor is estimated in the steps described below, and it may be biased and inconsistent for $s_0(Z)$. BLP answers the question of whether there is evidence of treatment effect heterogeneity based on observables.

- Sorted Group Average Treatment Effects (GATES) are ATEs estimated within heterogeneity groups that are defined by the proxy predictor $S(Z)$.

- Classification Analysis (CLAN) examines the balance in covariates between the least and most affected units, the groups being defined by the proxy predictor $S(Z)$.

The ML proxy predictor $S(Z)$ is estimated in the following steps. First, we split the data into two equally-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[5]. Here, we examine effects on the indicator of having any visits in the follow-up

[5]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.

instead of on the annualized number of visits[6]. 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 Placebo Results

4.1 Main Results

The main results with a six-month follow-up on the annualized number of contacts in public primary care are in Table 2. Overall, the point estimates are close to zero and insignificant. An exception is the estimate on GP visits when comparing controls (T0) to the reminder group (T1+T2+T3). The estimates on GP visits are largest in absolute value in terms of percentage changes (up to 2.5%) and standardized mean differences (up to 0.013) and seem to be driven by the base reminder group (T1). The results on having any such contacts are in Table A3. 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. All the estimates are insignificant and mostly close to zero. An exception are the estimates on referrals which are large in terms of percentage changes (up to 7.6%) and standardized mean differences (up to 0.020). Figures A3 (annualized contacts) and A4 (has any contact) show the estimates as a function of the follow-up length.

Next, we assess the robustness of the main results to exploiting controls. We do this in three ways - the details are reported in Section 3.4. First, we include controls linearly in the OLS formula. The point estimates are robust, and our significance conclusion changes in only one case out of 16 - the estimate on having any GP visit becomes significant when comparing controls (T0) to the reminder group (T1+T2+T3). Second, we subtract pre-trial

[6]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.

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

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	1.101	0.945	5.932	0.164
Estimate	-0.010	-0.024	-0.023	-0.001
Std. error	0.020	0.010	0.045	0.003
Confidence interval	[-0.049, 0.030]	[-0.044, -0.003]	[-0.110, 0.065]	[-0.007, 0.006]
Change (%)	-0.865	-2.512	-0.379	-0.373
Std. mean difference	-0.003	-0.013	-0.003	-0.001
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	1.078	0.907	5.958	0.164
Estimate	0.020	0.022	-0.073	-0.001
Std. error	0.034	0.018	0.077	0.006
Confidence interval	[-0.046, 0.086]	[-0.013, 0.057]	[-0.224, 0.078]	[-0.013, 0.010]
Change (%)	1.894	2.455	-1.226	-0.914
Std. mean difference	0.006	0.012	-0.009	-0.002
N: treated	31,598	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800	15,800

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

service use from the post-trial outcome. The point estimates are somewhat more sensitive to this than to the inclusion of controls linearly, and all estimates are now insignificant. Third, we use subclassification matching: the estimate on the number of GP visits is no longer significant. In the Online Appendix, we show the results with subclassification matching on annualized contacts in Table A5 and on having any contacts in Table A6. The rest of the six tables (three variations and two outcome types) are available in the replication codes folder. Figure A5 shows the covariate balance after subclassification matching.

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 to treat health problems 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, 4.7% of the trial population are high-users. The placebo results excluding the high-users on annualized visits are in Table A7 and on having any visits in Table A8 - the point estimates on the number of nurse and GP visits attenuate towards zero, but otherwise they do not differ noticeably from the main estimates.

4.2 Spillovers to the Use of Private Services

Even though the reminders provide only contact information of 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. Note that the estimates on private outpatient doctor visits are invalid because these events have been transferred to the national registry only since 6/2020 in our trial areas. In the two other outcomes, the point estimates are insignificant and close to zero. An exception is the estimate on referrals, which is insignificant, but constitutes a 6% change. This estimate is rather robust to the inclusion of controls linearly or matching, but attenuates towards zero if RCT-DID is used.[7] The corresponding results on having any such contacts are in Table A9.

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

	Doctor visits	Prescriptions	Referrals
A. No vs. any reminder			
Reference group mean	-	1.635	0.093
Estimate	-	-0.003	-0.001
Std. error	-	0.023	0.002
Confidence interval	-	[-0.047, 0.042]	[-0.006, 0.004]
Change (%)	-	-0.167	-1.360
Std. mean difference	-	-0.001	-0.003
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	-	1.629	0.096
Estimate	-	0.004	-0.006
Std. error	-	0.039	0.004
Confidence interval	-	[-0.072, 0.080]	[-0.014, 0.003]
Change (%)	-	0.231	-6.026
Std. mean difference	-	0.001	-0.013
N: treated	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas.

[7] These result tables are not presented in the paper, but are available in the replication codes folder.

4.3 Spillovers within the Household

In the trial, we send only one reminder per household and the recipient is chosen randomly. Conditional on observing robust effects for those who received the reminder, we examine and report whether there are signs of spillover effects within the household. To do so, 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)[8]. The results on curative public primary care nurse and GP visits are in Table A10. The estimates on the number of nurse visits are large in absolute value and significant when comparing the controls (T0) to the reminder group (T1+T2+T3): up to a 5.3% change in percentage terms (up to 0.017 in standardized mean differences). However, exploiting controls in any of the three ways described in Section 3.4 attenuates the estimates on the number of nurse visits towards zero.[9]

4.4 Heterogeneity Analysis

In this section, we focus on curative nurse and GP visits in public primary care. We have five pre-registered tests on the form of treatment effect heterogeneity. When comparing the control group to the reminder 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 an 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

[8] We are planning to change the sample definition for the household spillover analysis. Currently, the leading candidate in our discussions is the following: restrict to households of two or more target persons and include in control households (T0) the individual that was included in main analyses (randomized) and randomize one individual from each treated household out of persons that did not personally receive a letter.

[9] These result tables are not presented in the paper. Conditional on observing robust effects for those who received the reminder, we provide the spillover result tables in the replication codes folder.

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 by a clear majority pensioners 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 and on having any visits in Table A11. As a robustness check, we estimate the results on annualized visits also after excluding high-users (see the definition in Section 4.1) who are supposedly less sensitive to our intervention. These results are in Table A12.

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

Figure A6 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 A7 plots the CATEs by primary care area.

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

	Income $\geq$ median	Age $\geq$ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	1.350 [0.018]	0.701 [0.013]	0.488 [0.008]	1.137 [0.014]
TREAT	-0.009 [0.032]	-0.032 [0.022]	-0.019 [0.014]	-0.000 [0.024]
GROUP	-0.492 [0.023]	0.769 [0.023]	1.501 [0.026]	-0.126 [0.025]
TREAT:GROUP	-0.004 [0.040]	0.041 [0.039]	0.024 [0.046]	-0.036 [0.043]
P-value	0.928	0.295	0.606	0.403
Change (G=0)	-0.63%	-4.56%	-3.96%	-0.00%
Change (G=1)	-1.42%	0.61%	0.21%	-3.53%
B. GP visits				
Intercept	1.152 [0.010]	0.666 [0.008]	0.490 [0.005]	0.943 [0.007]
TREAT	-0.031 [0.016]	-0.020 [0.013]	-0.016 [0.009]	-0.022 [0.012]
GROUP	-0.408 [0.012]	0.536 [0.012]	1.114 [0.013]	0.018 [0.014]
TREAT:GROUP	0.013 [0.021]	-0.008 [0.021]	-0.019 [0.022]	-0.005 [0.024]
P-value	0.531	0.686	0.386	0.832
Change (G=0)	-2.67%	-2.97%	-3.20%	-2.34%
Change (G=1)	-2.38%	-2.34%	-2.19%	-2.82%

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 1/2019-6/2019, before the (placebo) 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 5: Heterogeneity Tests, T1 vs. T2+T3, Income Above Median.

	Annualized visits		Any visits
	All	No high-users	All
A. Nurse visits			
Intercept	1.340 [0.045]	0.907 [0.020]	0.304 [0.005]
TREAT	-0.001 [0.056]	0.004 [0.025]	0.008 [0.006]
GROUP	-0.517 [0.054]	-0.286 [0.026]	-0.083 [0.007]
TREAT:GROUP	0.041 [0.068]	-0.001 [0.032]	-0.012 [0.009]
P-value	0.546	0.973	0.148
Change (G=0)	-0.05%	0.45%	2.67%
Change (G=1)	4.87%	0.48%	-1.91%
B. GP visits			
Intercept	1.088 [0.022]	0.892 [0.017]	0.335 [0.005]
TREAT	0.051 [0.028]	0.020 [0.021]	0.010 [0.007]
GROUP	-0.357 [0.029]	-0.290 [0.023]	-0.101 [0.007]
TREAT:GROUP	-0.058 [0.036]	-0.012 [0.028]	-0.009 [0.009]
P-value	0.102	0.662	0.304
Change (G=0)	4.71%	2.19%	3.12%
Change (G=1)	-0.99%	1.22%	0.62%

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

Next, we report the results of the generic machine learning inference analysis. We start by examining 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.

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. All the estimates on heterogeneity loadings (β_2) are close to zero and insignificant, and many of them are even negative (but close to zero). 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 A13)[10].

If we do not observe any evidence of treatment effect heterogeneity with the real

[10]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.

trial data, we would stop the heterogeneity analysis here. If, however, we observed that the β_2 estimates (heterogeneity loading) are positive and not close to zero (even if not statistically significant), we would continue to analyze Sorted Group Average Treatment Effects (GATES) and average characteristics of the least affected and most affected groups (Classification Analysis; CLAN) as proposed by Chernozhukov et al. (2020).

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	-0.003	-0.016	-0.004	-0.012
CI	(-0.009, 0.003)	(-0.100, 0.069)	(-0.010, 0.003)	(-0.094, 0.071)
P-value	[0.379]	[0.716]	[0.261]	[0.778]
B. Random forest (tuned)				
Estimate	-0.003	-0.024	-0.004	-0.012
CI	(-0.009, 0.004)	(-0.114, 0.066)	(-0.010, 0.003)	(-0.101, 0.078)
P-value	[0.387]	[0.604]	[0.276]	[0.784]
C. XGBoost (default)				
Estimate	-0.003	-0.029	-0.004	0.092
CI	(-0.009, 0.003)	(-0.357, 0.300)	(-0.010, 0.003)	(-0.230, 0.421)
P-value	[0.381]	[0.871]	[0.261]	[0.564]
D. XGBoost (tuned)				
Estimate	-0.003	-0.019	-0.004	0.096
CI	(-0.009, 0.003)	(-0.228, 0.182)	(-0.010, 0.003)	(-0.098, 0.282)
P-value	[0.379]	[0.849]	[0.252]	[0.323]

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

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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 2019 in this PAP, but the final report will use data from 2021. The subset of covariates selected for analyses are in panels B and C in Table 1. We cannot find these covariates for 0.2% of the trial population, partly 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 2019. 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 (0.3%) 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 2019-2020 in this PAP, and will use data from 2021-2022 in the final report. 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. We verified that 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 registry has been collecting data from private outpatient care as well. However, we observe private outpatient visits in our trial areas only from June 2020 (Figure 2). 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 TOPI and SOTE registries which contain information about healthcare providers. Both registries are continuously updated. In this PAP, we have two cross-sections of TOPI from 2019 and 2020 and one cross-section of SOTE from early 2020. Thus, TOPI is linked to the visit data at the unit-ID-by-year level. With the data from 2019-2020, all visits contain a TOPI code, but the linking does not work for 4% of the rows. 0.04% of the rows have missing SOTE code, but the linking does not work for 1% of the rows. In the final report, we will use more recent cross-sections of both registries.

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 registry, 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 be an important outcome in this study. We hypothesize that the reminders will 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. Ideally, we would use both first contacts and appointments as outcomes. The Register of Primary Health Care Visits contains a variable for the date of the first contact. We extract all rows with a person ID and the date of the first contact, and include only those rows 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). This is done to extract first contacts to *public* primary care.

Unfortunately, the coding rates with respect to the date of the first contact are low in our trial regions and noticeably lower than the national average. Of the three trial regions, the coding rate is highest in Kymenlaakso (85% of the rows have missing value) and lowest in Päijät-Häme (97% of the rows have missing value) when we focus on the sample of actually occurred curative public primary care nurse and GP visits. When writing this PAP, we had access to data that was extracted based on the visit start date being in a given time window. For the final report, we will also acquire the data rows where the date of the first contact is observed but the contact start date is missing. It is possible (although unlikely) that this change would result in a much larger and more representative sample of first contacts to primary care. We will later re-examine the data quality with this respect and decide whether we can include first contacts to primary care as an outcome in the study, once we have access to the updated data.

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 use data from 2019-2020 in this PAP, and will use data from 2021-2022 in the final report. We extract unique prescriptions (not cancellations nor edits) 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. The PAP uses data from 2019-2020, and we will use data from 2021-2022 in the final report. When writing this PAP, we had only access to those events where the contact start date is observed.

For the final report, we will also include patients who are in a wait list (those who have a referral processed but who have not yet had the visit). 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 observed to be high.

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. Leading health care use			
Primary care nurse visits	0.002 (0.780)	0.015 (0.109)	-0.001 (0.953)
Primary care GP visits	-0.001 (0.843)	0.004 (0.688)	0.002 (0.825)
Private outpatient doctor visits	-	-	-
Prescriptions from private sector	-0.001 (0.831)	-0.018 (0.068)	-0.016 (0.146)
Referrals from health centers	0.000 (0.933)	-0.006 (0.531)	0.002 (0.840)
Referrals from private clinics	-0.000 (0.974)	-0.013 (0.194)	0.005 (0.643)
B. Sociodemographic covariates			
Age	0.005 (0.386)	-0.007 (0.501)	0.010 (0.378)
Is male	0.003 (0.587)	-0.009 (0.369)	-0.011 (0.341)
Native language Finnish	-0.004 (0.531)	0.009 (0.366)	0.021 (0.057)
In relationship	0.002 (0.763)	0.019 (0.054)	0.022 (0.050)
Children living at home	0.008 (0.139)	-0.011 (0.259)	-0.025 (0.025)
C. Socioeconomic covariates			
Living in an apartment	-0.015 (0.010)	0.001 (0.937)	-0.002 (0.849)
Degree from tertiary education	-0.013 (0.022)	-0.006 (0.565)	-0.005 (0.656)
Equivalized disposable income	-0.006 (0.291)	0.015 (0.110)	0.010 (0.397)
Unemployed for at least one day	0.008 (0.143)	-0.016 (0.096)	-0.015 (0.170)
Received social assistance	-0.001 (0.816)	-0.015 (0.136)	-0.019 (0.092)
Received sickness allowance	0.002 (0.693)	-0.013 (0.181)	-0.015 (0.177)
Joint F-tests (p-values):			
All individuals and covariates	0.182	0.078	0.230
Exclude the high-income folks	0.182	0.111	0.162
Only prior health care use	0.997	0.253	0.885

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 leading health care use in Table 1. Health care use is measured by the annualized number of contacts in 1/2019-6/2019, prior to the (placebo) law reform in 7/2019 and our (placebo) trial starting in 10/2019. Covariates in panels B and C are measured at the end of 2019. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas.

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

Variable	T0 vs. T1	T0 vs. T2	T0 vs. T3
A. Leading health care use			
Primary care nurse visits	-0.009 (0.275)	0.007 (0.440)	0.006 (0.513)
Primary care GP visits	-0.004 (0.663)	-0.001 (0.900)	0.001 (0.869)
Private outpatient doctor visits	-	-	-
Prescriptions from private sector	0.011 (0.218)	0.001 (0.914)	-0.015 (0.069)
Referrals from health centers	0.005 (0.599)	-0.003 (0.750)	-0.000 (0.959)
Referrals from private clinics	0.008 (0.339)	-0.007 (0.399)	-0.002 (0.828)
B. Sociodemographic covariates			
Age	0.009 (0.283)	-0.002 (0.792)	0.008 (0.371)
Is male	0.009 (0.301)	0.006 (0.523)	-0.005 (0.544)
Native language Finnish	-0.009 (0.278)	-0.011 (0.200)	0.010 (0.227)
In relationship	-0.011 (0.207)	-0.003 (0.718)	0.019 (0.027)
Children living at home	0.016 (0.070)	0.017 (0.048)	-0.008 (0.346)
C. Socioeconomic covariates			
Living in an apartment	-0.015 (0.079)	-0.013 (0.123)	-0.015 (0.073)
Degree from tertiary education	-0.009 (0.288)	-0.012 (0.153)	-0.017 (0.044)
Equivalized disposable income	-0.016 (0.050)	-0.006 (0.514)	0.003 (0.667)
Unemployed for at least one day	0.019 (0.028)	0.010 (0.226)	-0.005 (0.561)
Received social assistance	0.008 (0.332)	0.003 (0.715)	-0.016 (0.062)
Received sickness allowance	0.011 (0.209)	0.005 (0.539)	-0.010 (0.245)
Joint F-tests (p-values):			
All individuals and covariates	0.014	0.650	0.197
Exclude the high-income folks	0.025	0.605	0.140
Only prior health care use	0.776	0.964	0.610

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 leading health care use in Table 1. Health care use is measured by the annualized number of contacts in 1/2019-6/2019, prior to the (placebo) law reform in 7/2019 and our (placebo) trial starting in 10/2019. Covariates in panels B and C are measured at the end of 2019. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas.

Table A3: Public Primary Care: Has Any Contact.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	26.493	29.205	64.388	7.418
Estimate	-0.160	-0.454	0.596	-0.021
Std. error	0.247	0.253	0.266	0.147
Confidence interval	[-0.644, 0.323]	[-0.951, 0.042]	[0.074, 1.117]	[-0.309, 0.267]
Change (%)	-0.605	-1.556	0.925	-0.284
Std. mean difference	-0.004	-0.010	0.012	-0.001
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	26.196	28.361	65.082	7.342
Estimate	0.205	0.585	-0.147	0.083
Std. error	0.427	0.437	0.460	0.254
Confidence interval	[-0.631, 1.041]	[-0.272, 1.443]	[-1.049, 0.755]	[-0.415, 0.582]
Change (%)	0.781	2.064	-0.226	1.133
Std. mean difference	0.005	0.013	-0.003	0.003
N: treated	31,598	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800	15,800

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

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

	Nurse visits	GP visits	Prescriptions	Referrals
A. No. of annualized contacts				
Reference group mean	1.109	0.920	5.891	0.157
Estimate	-0.020	0.018	-0.012	0.012
Std. error	0.041	0.021	0.088	0.007
Confidence interval	[-0.101, 0.060]	[-0.022, 0.059]	[-0.185, 0.161]	[-0.001, 0.025]
Change (%)	-1.828	1.971	-0.204	7.610
Std. mean difference	-0.006	0.010	-0.002	0.020
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	26.367	28.759	64.601	7.165
Estimate	0.068	0.371	0.665	0.520
Std. error	0.493	0.507	0.532	0.295
Confidence interval	[-0.899, 1.034]	[-0.622, 1.365]	[-0.378, 1.708]	[-0.057, 1.098]
Change (%)	0.258	1.291	1.030	7.261
Std. mean difference	0.002	0.008	0.014	0.020
N: treated	15,798	15,798	15,798	15,798
N: reference group	15,800	15,800	15,800	15,800

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

Table A5: Public Primary Care: Annualized Number of Contacts, Matching.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	1.104	0.947	5.945	0.164
Estimate	−0.003	−0.019	0.010	−0.002
Std. error	0.023	0.011	0.050	0.004
P-value	0.882	0.104	0.847	0.667
Change (%)	−0.310	−1.966	0.161	−0.962
N: treated	47,240	47,240	47,240	47,240
N: reference group	94,503	94,503	94,503	94,503
B. Base vs. copayment reminder				
Reference group mean	1.081	0.909	5.977	0.164
Estimate	−0.001	0.024	−0.071	0.003
Std. error	0.038	0.020	0.086	0.006
P-value	0.972	0.220	0.408	0.595
Change (%)	−0.122	2.667	−1.183	2.092
N: treated	31,493	31,493	31,493	31,493
N: reference group	15,747	15,747	15,747	15,747

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. We use propensity-score based subclassification with 10,000 subclasses, the propensity scores being estimated with logistic regression and the covariates in Table A1. The target estimand is the average treatment effect (ATE). The matching is conducted with the R package *MatchIt* (Ho et al., 2011). The final results are estimated with linear regression with heteroskedasticity-robust standard errors, observations weighted by matching weights. 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.

Table A6: Public Primary Care: Has Any Contact, Matching.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	26.557	29.269	64.533	7.434
Estimate	−0.120	−0.331	0.574	−0.053
Std. error	0.269	0.277	0.290	0.158
P-value	0.655	0.232	0.048	0.736
Change (%)	−0.451	−1.131	0.889	−0.719
N: treated	47,240	47,240	47,240	47,240
N: reference group	94,503	94,503	94,503	94,503
B. Base vs. copayment reminder				
Reference group mean	26.272	28.431	65.270	7.354
Estimate	0.432	0.761	−0.355	0.306
Std. error	0.471	0.486	0.509	0.277
P-value	0.359	0.117	0.486	0.269
Change (%)	1.645	2.678	−0.543	4.166
N: treated	31,493	31,493	31,493	31,493
N: reference group	15,747	15,747	15,747	15,747

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. We use propensity-score based subclassification with 10,000 subclasses, the propensity scores being estimated with logistic regression and the covariates in Table A1. The target estimand is the average treatment effect (ATE). The matching is conducted with the R package *MatchIt* (Ho et al., 2011). The final results are estimated with linear regression with heteroskedasticity-robust standard errors, observations weighted by matching weights. 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.

Table A7: Public Primary Care: Annualized Number of Contacts, Excluding High-Users.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	0.765	0.769	5.183	0.141
Estimate	0.000	-0.014	0.024	0.000
Std. error	0.009	0.008	0.037	0.003
Confidence interval	[-0.018, 0.018]	[-0.030, 0.001]	[-0.049, 0.097]	[-0.006, 0.006]
Change (%)	-0.001	-1.870	0.461	-0.112
Std. mean difference	0.000	-0.011	0.004	0.000
N: treated	45,107	45,107	45,107	45,107
N: reference group	90,103	90,103	90,103	90,103
B. Base vs. copayment reminder				
Reference group mean	0.762	0.745	5.263	0.141
Estimate	0.003	0.012	-0.085	-0.001
Std. error	0.016	0.014	0.064	0.006
Confidence interval	[-0.028, 0.035]	[-0.015, 0.040]	[-0.211, 0.041]	[-0.012, 0.010]
Change (%)	0.447	1.675	-1.621	-0.420
Std. mean difference	0.002	0.009	-0.013	-0.001
N: treated	30,045	30,045	30,045	30,045
N: reference group	15,062	15,062	15,062	15,062

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

Table A8: Public Primary Care: Has Any Contact, Excluding High-Users.

	Nurse visits	GP visits	Prescriptions	Referrals
A. No vs. any reminder				
Reference group mean	24.631	27.429	63.094	6.547
Estimate	-0.041	-0.408	0.682	-0.019
Std. error	0.247	0.255	0.275	0.142
Confidence interval	[-0.525, 0.444]	[-0.908, 0.093]	[0.142, 1.221]	[-0.298, 0.260]
Change (%)	-0.165	-1.487	1.081	-0.287
Std. mean difference	-0.001	-0.009	0.014	-0.001
N: treated	45,107	45,107	45,107	45,107
N: reference group	90,103	90,103	90,103	90,103
B. Base vs. copayment reminder				
Reference group mean	24.446	26.656	64.002	6.467
Estimate	0.188	0.510	-0.361	0.089
Std. error	0.427	0.440	0.475	0.246
Confidence interval	[-0.650, 1.026]	[-0.353, 1.373]	[-1.292, 0.570]	[-0.393, 0.571]
Change (%)	0.769	1.913	-0.564	1.381
Std. mean difference	0.004	0.012	-0.007	0.004
N: treated	30,045	30,045	30,045	30,045
N: reference group	15,062	15,062	15,062	15,062

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

Table A9: Private Outpatient Care: Has Any Contact.

	Doctor visits	Prescriptions	Referrals
A. No vs. any reminder			
Reference group mean	-	26.085	4.433
Estimate	-	-0.228	-0.072
Std. error	-	0.245	0.115
Confidence interval	-	[-0.709, 0.253]	[-0.297, 0.154]
Change (%)	-	-0.873	-1.617
Std. mean difference	-	-0.005	-0.003
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	-	26.000	4.506
Estimate	-	-0.215	-0.218
Std. error	-	0.425	0.200
Confidence interval	-	[-1.047, 0.618]	[-0.611, 0.175]
Change (%)	-	-0.825	-4.845
Std. mean difference	-	-0.005	-0.011
N: treated	31,598	31,598	31,598
N: reference group	15,800	15,800	15,800

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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. Private outpatient doctor visits have been transferred to the national registry since 6/2020 in our target areas.

Table A10: 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	1.052	0.891	26.311	28.383
Estimate	-0.053	-0.001	-0.698	-0.448
Std. error	0.027	0.016	0.388	0.398
Confidence interval	[-0.107, 0.000]	[-0.032, 0.030]	[-1.458, 0.063]	[-1.229, 0.332]
Change (%)	-5.078	-0.074	-2.652	-1.579
Std. mean difference	-0.017	-0.001	-0.016	-0.010
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	1.030	0.878	25.240	28.163
Estimate	-0.054	0.013	0.456	-0.441
Std. error	0.048	0.027	0.670	0.691
Confidence interval	[-0.149, 0.040]	[-0.040, 0.066]	[-0.856, 1.769]	[-1.795, 0.913]
Change (%)	-5.284	1.490	1.807	-1.566
Std. mean difference	-0.016	0.010	0.012	-0.008
N: treated	12,687	12,687	12,687	12,687
N: reference group	6,260	6,260	6,260	6,260

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 A11: Heterogeneity Tests, T0 vs. T1+T2+T3, Has Any Visit.

	Income $\geq$ median	Age $\geq$ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	0.311 [0.002]	0.183 [0.002]	0.150 [0.002]	0.276 [0.002]
TREAT	-0.001 [0.004]	-0.005 [0.003]	-0.002 [0.003]	-0.000 [0.003]
GROUP	-0.090 [0.003]	0.157 [0.003]	0.282 [0.003]	-0.039 [0.003]
TREAT:GROUP	-0.001 [0.005]	0.006 [0.005]	-0.000 [0.005]	-0.004 [0.006]
P-value	0.782	0.203	0.989	0.476
Change (G=0)	-0.33%	-2.72%	-1.04%	-0.18%
Change (G=1)	-1.09%	0.35%	-0.38%	-1.87%
B. GP visits				
Intercept	0.347 [0.002]	0.211 [0.002]	0.173 [0.002]	0.291 [0.002]
TREAT	-0.006 [0.004]	-0.004 [0.003]	-0.004 [0.003]	-0.003 [0.003]
GROUP	-0.109 [0.003]	0.157 [0.003]	0.292 [0.003]	0.005 [0.003]
TREAT:GROUP	0.002 [0.005]	-0.000 [0.005]	-0.000 [0.005]	-0.004 [0.006]
P-value	0.687	0.965	0.946	0.496
Change (G=0)	-1.62%	-2.14%	-2.49%	-1.15%
Change (G=1)	-1.51%	-1.28%	-1.00%	-2.47%

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 1/2019-6/2019, before the (placebo) 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 A12: Heterogeneity Tests, T0 vs. T1+T2+T3, Annualized Visits Excluding High-Users.

	Income ≥ median	Age ≥ median	Pre-trial visits	Outsourced
A. Nurse visits				
Intercept	0.906 [0.008]	0.502 [0.006]	0.399 [0.005]	0.799 [0.006]
TREAT	0.003 [0.014]	-0.013 [0.011]	-0.003 [0.008]	0.003 [0.011]
GROUP	-0.279 [0.011]	0.509 [0.010]	0.925 [0.012]	-0.127 [0.012]
TREAT:GROUP	-0.008 [0.019]	0.023 [0.018]	0.004 [0.020]	-0.010 [0.021]
P-value	0.658	0.197	0.835	0.626
Change (G=0)	0.32%	-2.67%	-0.66%	0.31%
Change (G=1)	-0.84%	0.99%	0.12%	-1.12%
B. GP visits				
Intercept	0.920 [0.007]	0.540 [0.006]	0.441 [0.005]	0.764 [0.005]
TREAT	-0.015 [0.012]	-0.013 [0.010]	-0.014 [0.008]	-0.014 [0.009]
GROUP	-0.298 [0.009]	0.442 [0.009]	0.830 [0.010]	0.023 [0.011]
TREAT:GROUP	-0.001 [0.016]	-0.003 [0.016]	-0.003 [0.017]	0.001 [0.019]
P-value	0.943	0.840	0.843	0.952
Change (G=0)	-1.61%	-2.49%	-3.08%	-1.90%
Change (G=1)	-2.56%	-1.70%	-1.34%	-1.70%

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 1/2019-6/2019, before the (placebo) 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 A13: Assessing Predictive Performance.

	RMSE	
	Nurse visits	GP visits
A. T1+T2+T2		
Use outcome mean	0.441	0.453
Random forest (tuned)	0.406	0.421
XGBoost (tuned)	0.403	0.418
B. T0		
Use outcome mean	0.442	0.455
Random forest (tuned)	0.407	0.422
XGBoost (tuned)	0.404	0.419

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.

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Hyvä vastaanottaja

Koronaviruspandemian aikana moni hoitokäynti on jäänyt Suomessa toteutumatta. Jos hoitoa terveysongelmiin ei haeta oikeaan aikaan, riskinä on, että terveys heikkenee entisestään. Sairauksien tunnistaminen ja hoidon aloittaminen voivat viivästyä. Pitkäaikaissairailla sairauden hoitotasapaino voi heiketä.

Haluamme tämän kirjeen avulla muistuttaa sinua ja kotitaloutesi muita henkilöitä, että mahdollisten terveysongelmien hoitamiseksi voit ottaa yhteyttä oman alueesi perusterveydenhuoltoon.

Alueesi perusterveydenhuollon järjestäjä on Etelä-Karjalan sosiaali- ja terveyspiiri (Eksote). Jos koet tarvetta terveysongelmien hoitoon, voit ottaa yhteyttä terveydenhuoltoalan ammattilaiseen soittamalla Eksoten laajentuneeseen puhelinpalveluun 05 352 7260 tai käyttämällä etäpalvelua (www.hyvis.fi tai www.omaolo.fi). Puhelinpalvelu on avoinna arkisin klo 7-16, mutta lähitulevaisuudessa aukiolo laajenee välille 7-20. Hoitokäynnin tarve arvioidaan yhteydenoton yhteydessä.

Kirje on osa THL:n toteuttamaa yli 55-vuotiaita henkilöitä koskevaa tiedotuskampanjaa. THL ei vastaa terveyspalveluiden tuottamisesta tai ajanvarauksesta alueellasi. Lisätietoja kirjeeseen liittyen on mahdollista saada THL:ltä puhelinnumerosta: 029 524 6185 (arkisin klo 9–16).

Kirjeen vastaanottajat on valittu väestötietojärjestelmästä saadun syntymäpäivän perusteella. Mikäli samassa kotitaloudessa asuu useita yli 55-vuotiaista henkilöitä, kirje on lähetetty taloudellisista syistä ja ympäristösyistä vain yhdelle satunnaisesti valitulle samassa kotitaloudessa asuvalle henkilöille. Välitäthän tietoa myös kotitalouden muille jäsenille.

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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: The Base Reminder (T1) in South Karelia.

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Hyvä vastaanottaja

Koronaviruspandemian aikana moni hoitokäynti on jäänyt Suomessa toteutumatta. Jos hoitoa terveysongelmiin ei haeta oikeaan aikaan, riskinä on, että terveys heikkenee entisestään. Sairauksien tunnistaminen ja hoidon aloittaminen voivat viivästyä. Pitkäaikaissairailla sairauden hoitotasapaino voi heiketä.

Haluamme tämän kirjeen avulla muistuttaa sinua ja kotitaloutesi muita henkilöitä, että mahdollisten terveysongelmien hoitamiseksi voit ottaa yhteyttä oman alueesi perusterveydenhuoltoon. Lisäksi haluamme tiedottaa uudesta sosiaali- ja terveydenhuollon asiakasmaksulaista, joka on muuttanut perusterveydenhuollosta perittäviä maksuja. **Uuden lain myötä kaikki sairaanhoitajien vastaanottokäynnit ovat muuttuneet maksuttomiksi koko Suomessa 1.7.2021 alkaen.**

Alueesi perusterveydenhuollon järjestäjä on Etelä-Karjalan sosiaali- ja terveyspiiri (Eksote). Jos koet tarvetta terveysongelmien hoitoon, voit ottaa yhteyttä terveydenhuoltoalan ammattilaiseen soittamalla Eksoten laajentuneeseen puhelinpalveluun 05 352 7260 tai käyttämällä etäpalvelua (www.hyvis.fi tai www.omaolo.fi). Puhelinpalvelu on avoinna arkisin klo 7-16, mutta lähitulevaisuudessa aukiolo laajenee välille 7-20. Hoitokäynnin tarve arvioidaan yhteydenoton yhteydessä.

Kirje on osa THL:n toteuttamaa yli 55-vuotiaita henkilöitä koskevaa tiedotuskampanjaa. THL ei vastaa terveyspalveluiden tuottamisesta tai ajanvarauksesta alueellasi. Lisätietoja kirjeeseen liittyen on mahdollista saada THL:ltä puhelinnumerosta: 029 524 6185 (arkisin klo 9–16).

Kirjeen vastaanottajat on valittu väestötietojärjestelmästä saadun syntymäpäivän perusteella. Mikäli samassa kotitaloudessa asuu useita yli 55-vuotiaista henkilöitä, kirje on lähetetty taloudellisista syistä ja ympäristösyistä vain yhdelle satunnaisesti valitulle samassa kotitaloudessa asuvalle henkilöille. Välitäthän tietoa myös kotitalouden muille jäsenille.

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Figure A2: A Copayment Reminder (T2) in South Karelia.

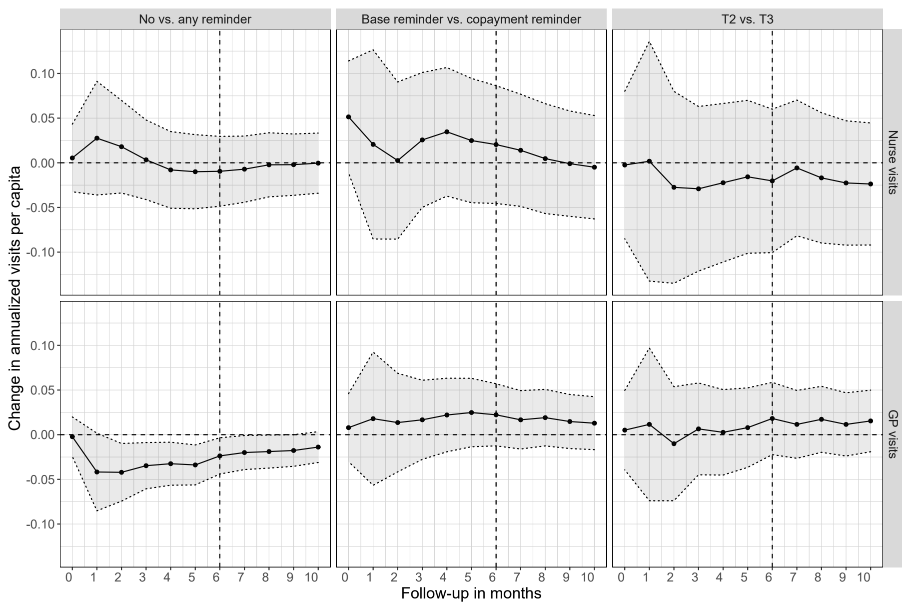

Figure A3: Public Primary Care: Annualized Number of Contacts, Varying Follow-up Length.

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 1/2019-6/2019. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care.

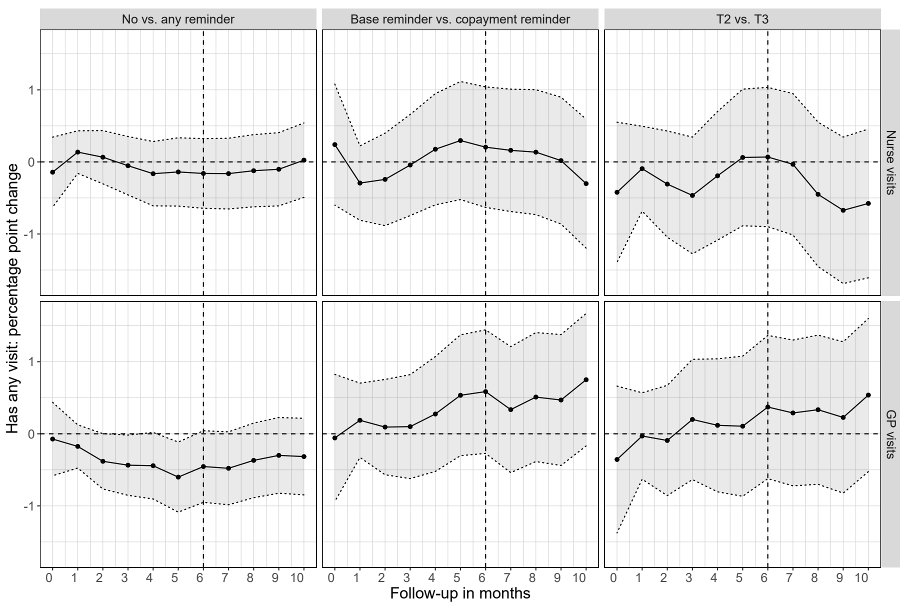

Figure A4: Public Primary Care: Has Any Contact, Varying Follow-up Length

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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 1/2019-6/2019. The reference group is the group mentioned first in panel titles. Nurse and GP visits are curative outpatient visits to public primary care.

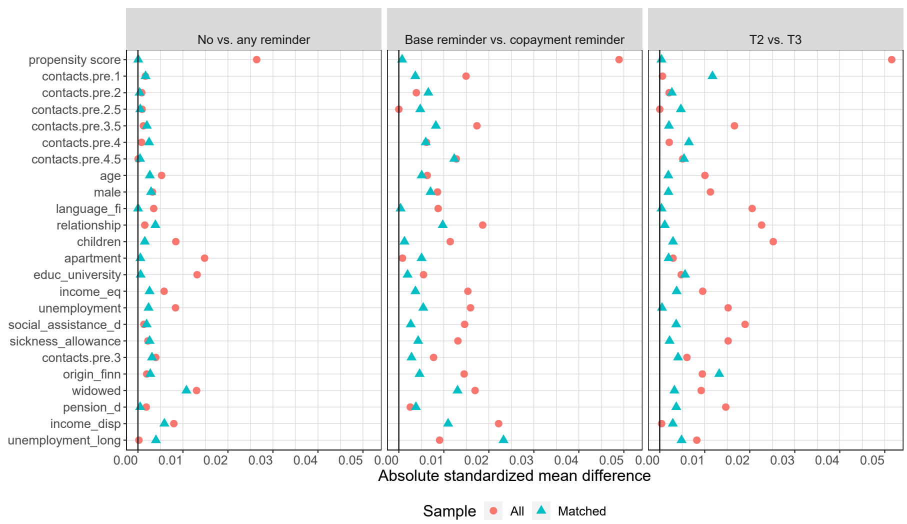

Figure A5: Covariate Balance after Subclassification Matching.

Notes: We use propensity-score based subclassification with 10,000 subclasses, the propensity scores being estimated with logistic regression and the covariates in Table A1 (in the same order). The plot compares balance with respect to variables that are in Table 1, a larger set than in Table A1. The target estimand is the average treatment effect (ATE). The matching is conducted with the R package *MatchIt* (Ho et al., 2011).

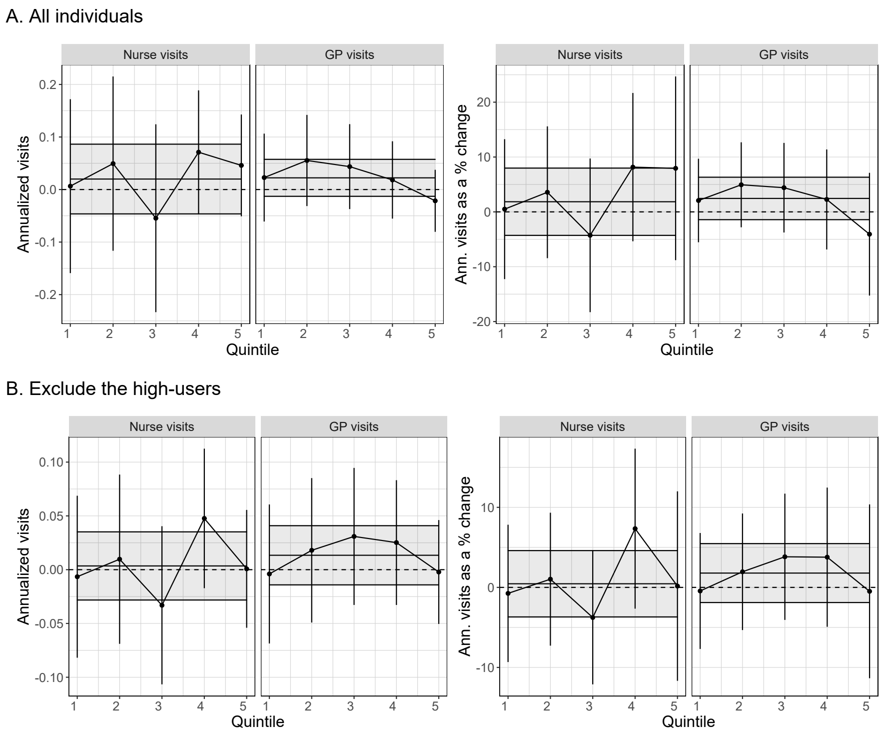

Figure A6: Public Primary Care: the Effects of Copayment Information by Income Quintiles.

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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, individuals having at least four curative nurse or GP visits in a six month window prior to the trial and the policy change (”high-users”) are excluded.

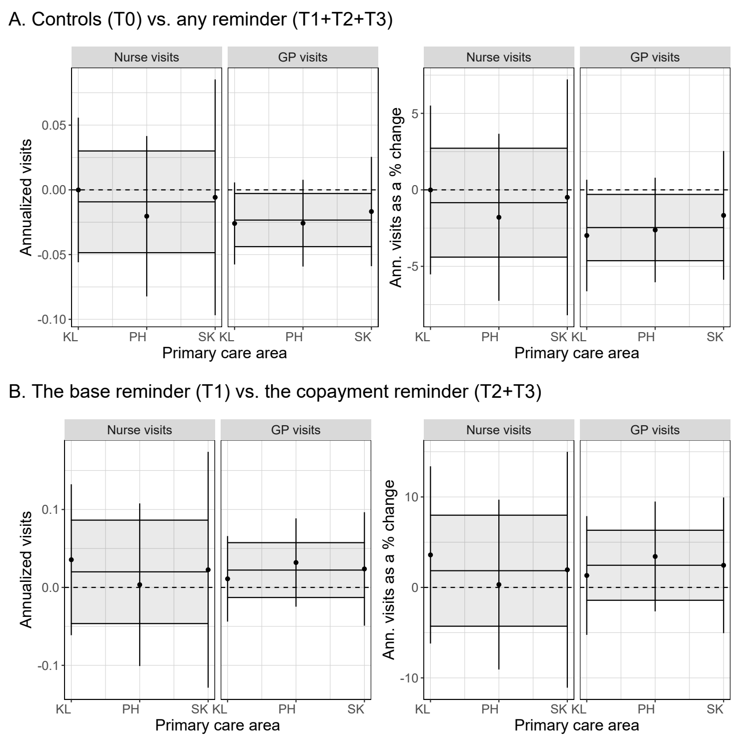

Figure A7: Public Primary Care: the CATEs by Primary Care Area.

Notes: These are placebo results that are based on a placebo trial starting in 10/2019. 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.