Ostrobothnia Digital Clinic Experiment: Populated Statistical Analysis Plan for a Randomized Controlled Trial

Tapio Haaga, Alex Kivimäki, Mika Kortelainen, Oskari Nokso-Koivisto, Tanja Saxell, Meeri Seppä, Lauri Sääksvuori*

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*Haaga: Finnish Institute for Health and Welfare (THL). **Kivimäki:** University of Turku (UTU). **Kortelainen:** UTU and THL. **Nokso-Koivisto:** Aalto University (Aalto). **Saxell:** Aalto and VATT Institute for Economic Research. **Seppä:** Aalto. **Sääksvuori:** THL.

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Abstract

We used a large-scale randomized controlled trial (RCT) to examine the impacts of a publicly provided digital clinic that offers primary care services through digital channels. Our intervention granted access to a public digital clinic that provides chat-based primary care consultations via a mobile phone application and website, including care needs assessments, diagnoses, prescriptions, and follow-up care recommendations. The digital clinic was designed to supplement traditional public primary care (PPC) services, including in-person visits and phone consultations. The trial took place in Ostrobothnia, Finland, a healthcare district serving a population of 179,000 residents. We randomized access to the digital clinic at the household level, providing access to 50% of the households.

This populated statistical analysis plan (SAP) reports the results of our pre-registered analyses with minimal changes relative to our pre-analysis plan (PAP) and SAP. Using administrative individual level data, we find that the impact of using the digital clinic on the number of in-person PPC visits was not statistically different from zero. The point estimate is negative, suggesting that for 100 digital clinic contacts only five in-person visits were avoided (95% CI: -23 to $+13$). In contrast, the digital clinic reduced the number of other traditional PPC contacts, including telemedicine, professional-to-professional interactions, and care needs assessments: for 100 digital clinic contacts 37 other contacts in traditional PPC were avoided (95% CI: -60 to -14). These results indicate that other traditional PPC services are closer substitutes for digital services than in-person visits. Overall, access to the digital clinic increased the total number of PPC contacts by 2.4% (95% CI: $+0.4\%$ to $+4.4\%$). We do not find statistically significant effects on the use of specialized health care provided at hospitals, nor evidence of substitution from private or occupational health care to the public sector.

Keywords: digital healthcare, telemedicine, digital clinic, primary care, healthcare utilization, randomized controlled trial

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1 Introduction

This document is our populated statistical analysis plan for a randomized controlled trial (RCT) entitled *Ostrobothnia Digital Clinic Experiment*. Our complete analysis plan and experimental protocol consist of three separate documents written and made public in the following order:

1. A general-level pre-analysis plan (PAP) describing the main idea, study design, data sources, primary and secondary outcomes, and our statistical approach. The PAP was registered *before* initiating the trial. See our registration in the AEA RCT Registry (https://doi.org/10.1257/rct.15587-3.0) and on ClinicalTrials.gov (NCT06904469).

2. A statistical analysis plan (SAP), which maps the idea into code based on blinded/placebo data. Statistical codes were included at this step of the registration, illustrating in detail how we plan to construct our variables and analysis data. We registered the SAP *after* initiating the trial and observing aggregate statistics on the utilization of digital services, but *before* linking the treatment assignment and outcome data. This process can be verified by a third party. In the SAP, we include some additional analyses compared to the PAP.

3. A populated SAP, which replaces the tables and figures in the SAP with real data after the trial has ended.[1] This is the document you are currently reading. At this stage, only minimal deviations from the SAP were made.[2]

Finally, we will produce a research paper or series of papers, intended for publication in scholarly journals. The final research papers are distinct from the populated SAP as they may contain not only confirmatory analyses registered in the PAP and SAP but also post-blind and exploratory analyses conducted after linking the treatment indicator and outcome data. In our research papers, we will clearly differentiate between pre-registered analyses and post-blind analyses.

[1]Note that in contrast to the original plan, the current version of the populated SAP excludes for Outcomes Y3.3–Y3.4 data from December 2025 – January 2026, which we are still waiting to get access to. The final research paper will include the whole study period for all outcomes.

[2]See Footnote 1. In addition, some small changes and typo corrections were made to our SAP codes, and these changes appear documented as comments in the codes.

The structure of this populated SAP is as follows. Section 1 discusses the key motivation for our experiment, describes our key research questions, and summarizes the results of the pre-registered analyses. Section 2 describes the institutional context. Section 3 describes the intervention. Section 4 provides details on the extraction of the study population, our inclusion and exclusion criteria, and the randomization process. Section 5 presents our data sources and specifies our outcomes of interest. Section 6 presents our main statistical approach. Section 7 reports the results of the pre-registered analyses. Section 8 outlines potential extensions to the pre-registered analyses. Appendix A adds some relevant post-blind, non pre-registered material not included in the SAP. Appendix B complements evidence from the RCT with expert forecasts. Appendix C discusses the likelihood of interference in the context of our experiment and presents a back-of-the-envelope model to assess the Stable Unit Treatment Value Assumption (SUTVA) in our experimental context.

1.1 Study motivation

Digital services leverage technological solutions to deliver online services to consumers through digital platforms, such as mobile applications, websites, and marketplaces. As in other digital service industries, digital platforms in healthcare offer consumers fast and easy access to services, potentially shifting demand from in-person visits and phone hold queues to online platforms. This trend is largely driven by increased convenience and lower costs of online services (Dorsey & Topol, 2020). While there is a growing body of literature examining the effects of health information technologies (Goldfarb & Tucker, 2019), the impacts of digital services remains much less well understood. Importantly, digital healthcare services may not only substitute for traditional health care but also induce new demand and utilization that would not have occurred without the digital channel (Dahlgren et al., 2023; Ellegård et al., 2026). The convenience of digital care (no time spent traveling, shorter waiting times, extended opening hours, and access from any geographical location) can lower the barriers to access, potentially reducing underdiagnosis and undertreatment, or, conversely, exacerbating the utilization of medically low-value services.

Through random assignment of patients to a digital clinic, our objective is to contribute to a better understanding of the impacts of digital healthcare services. We conducted a large, region-wide RCT in Finland, where we randomly granted access to digital primary care services. Our intervention was implemented at the household level and provided digital medical services during a nine-month period to 50% of households in an administrative region with approximately 179,000 individuals.

The digital clinic provides chat-based primary care consultations through a mobile app and website, offering consumers fast access to healthcare professionals. Services include care needs assessments, diagnoses, follow-up recommendations, and prescriptions. Thus, the digital clinic supplements traditional primary care services, including in-person visits and telephone consultations. Consequently, a key question of scientific and policy relevance is whether and how digital clinics affect the utilization of primary care services.

1.2 Research questions

Our main research questions are the following:

A. What is the impact of having *access* to the digital clinic on the use of digital clinic services (*i.e.*, take-up)?

B. What is the impact of having *access* to the digital clinic on the utilization of traditional primary care services (intent-to-treat effect, ITT)?

C. **What is the impact of *using* the digital clinic on the utilization of traditional primary care services (average causal response, ACR)?**

D. What is the impact of having *access* to the public digital clinic on the overall utilization of public primary care services, including both the digital clinic and traditional primary care services (ITT)?

In our research, we prioritize the importance of research question C. Beyond its policy relevance, the ACR estimate accounts for expected non-compliance in using the digital clinic.[3] However, we list our research questions here in a sequential order from A to D, as we are unlikely to detect the impact of using the digital clinic on any downstream outcomes if the take-up of the digital clinic during the trial period is not large enough.

In evaluating outcomes related to the use of traditional PPC services, we chose the number of in-person visits as our primary outcome because they are more expensive to provide than traditional phone contacts and require face-to-face interaction. On the other hand, we expect that other contacts with traditional PPC, involving telemedicine (mainly phone calls), professional-to-professional interactions and care needs assessments, are likely to be a closer substitute for digital clinic contacts than in-person visits. Therefore, we use the number of other PPC contacts as our secondary outcome. Our tertiary outcome is the total number of PPC contacts, including both the digital clinic and traditional PPC services, that might increase after digital clinic access was granted.

Besides our main analysis on the impacts of the use of the public digital clinic on the use of *traditional public primary care*, we registered two other families of outcomes in the SAP.

First, we are interested in whether improved access to and the utilization of the public digital clinic can reduce reliance on more expensive *specialized healthcare at hospitals, such as referrals to hospitals, emergency department (ED) visits – either telemedicine or in-person – or outpatient hospital visits*. This could happen, for example, if the improved access to the public digital clinic enhances health by enabling earlier medical intervention and preventive care. We note that the power to detect differences in some of these outcomes is expected be lower than for our pre-registered outcomes restricting to primary care. These analyses will be informative about the digital clinic's role in optimizing the delivery and utilization of healthcare services.

Second, we will examine whether the use of the public digital clinic reduces *telemedicine contacts or in-person visits with private clinics or occupational healthcare*. In other words, we

[3]See Angrist and Hull (2023) for an illustration for the importance of accounting for non-compliance in pragmatic randomized trials.

examine whether the use of the public digital clinic has impacts on the use of services in other sectors that complement public primary care.

1.3 Results

We find that granting access to the digital clinic increases the number of digital clinic contacts[4] by 0.113 per person per year (95% CI: +0.108 to +0.119). There is, however, little evidence of substitution from in-person visits to the digital clinic. Specifically, the intent-to-treat (ITT) estimate on the impact of access to the digital clinic on the number of in-person visits is not statistically different from zero. The point estimate is negative, suggesting a decrease of 0.006 visits per year, or −0.7% compared to the control group mean (95% CI: −0.026 to +0.015, or −3.1% to +1.8%) after access was granted. The average causal response (ACR) estimate on the impact of using the digital clinic on the number of in-person visits in traditional PPC is not statistically different from zero either, suggesting that for 100 digital clinic contacts only five in-person visits are avoided (95% CI: −23 to +13).

We find evidence of moderate substitution from other traditional PPC contacts to the digital clinic. The ITT estimate on the impact of access to the digital clinic is statistically significant and negative, indicating a decrease of 0.042 in other PPC contacts per year, or −3.5% (95% CI: −0.067 to −0.016, or −5.6% to −1.3%). The ACR estimate on the impact of using the digital clinic is also statistically significant, indicating that for 100 digital clinic contacts 37 other contacts in traditional PPC were avoided (95% CI: −60 to −14). Overall, our results align with our expectation that other traditional PPC services, such as phone calls, are closer substitutes for digital services than in-person visits.

We also find that the digital clinic increased the total number of PPC contacts. The ITT estimate on the impact of granting access to the digital clinic is statistically significant, indicating an increase of 0.043 contacts per year, or +2.4% compared to the mean (95% CI: +0.007 to +0.080, or +0.4% to +4.4%). This finding is consistent with the interpretation that the digital

[4] All outcomes were defined in terms of the annualized number of days with contact (contact days).

clinic improved access to PPC by introducing an additional online contact channel with shorter waiting times.

We cannot rule out zero effects on the utilization of specialized healthcare at hospitals based on the 95% confidence intervals. Finally, our effect estimates on the utilization of other healthcare sectors, namely private and occupational healthcare, were negative, but not statistically different from zero. Therefore, our results suggest that granting access to the digital clinic in PPC does not induce major substitution from other sectors to the public sector.

2 Institutional background

2.1 Primary care in Finland

Finland has a decentralized, universal healthcare system that primarily relies on the public provision of health services. By law, the wellbeing service counties (21 in total) are responsible for organizing public health and social care services, including public primary care (PPC), for their residents. PPC services are characterized by gatekeeping, varying and sometimes long waiting times, and moderate copayments.[5] Primary care services provided by employer-sponsored occupational healthcare and the private sector complement the services provided by PPC, which is disproportionally important for low-income individuals, unemployed, and pensioners. Fast access and no copayments make occupational healthcare an attractive alternative to PPC for those who have access to it. There are also private clinics with fast access and no gatekeeping (even for specialists), albeit with fees much higher than in PPC.

2.2 Traditional public primary care

Traditionally, patients first contact a nurse by phone (hold queue or a call-back service) or by visiting a traditional PPC clinic. The nurse then conducts a care needs assessment, provides

[5]There are no co-payments for nurse visits. The maximum co-payment for general practitioner visits is 23€ for the first three visits annually.

potential self-care guidance, and, acting as a gatekeeper, books a phone consultation or an in-person appointment for a physician or other professionals if needed. Team-based models, based on collaboration and consultative interactions between physicians and nurses, are common in PPC, with the goal of optimizing healthcare delivery and addressing issues even during the first contact, reducing the need for follow-ups.

2.3 Public digital clinic

We study the launch of a public digital clinic by a wellbeing services county. Following the widespread adoption and use of digital clinics in occupational healthcare and the private sector, several wellbeing service counties (PPC providers) have launched their own digital clinics in 2020s as a remote, chat-based access channel to complement their traditional clinic-based service provision.[6] Digital clinics aim to provide patients fast access to healthcare professionals (here: nurses and physicians) through chat, available through a mobile application or website, with extended opening hours and waiting times measured in minutes. Compared with traditional clinics, digital clinics can reduce barriers to healthcare access through extended opening hours, shorter waiting times, and reduced travel time to a health clinic for an in-person visit. While digital clinic services are not suitable for all patients and health conditions, these services offer many patients fast, easy, and user-friendly access to primary care. The most common medical issues treated in digital clinics are cold symptoms, stomach problems (e.g., diarrhea, vomiting), gynecological problems, skin problems, allergies, eye infections, and mental health problems.

Figure 1 illustrates the use of a public digital clinic from the patient's perspective and the potential care paths after the digital clinic contact. Logging into the digital clinic via a mobile application or website requires verifying a person's identity through online banking credentials or a national authentication service for public services. Thereafter, the patient fills out a digital pre-visit questionnaire before a healthcare professional, often a nurse, performs a care needs

[6]Patients can choose to contact a public or private clinic (traditional or digital). In the private sector, patients can access digital clinic services through occupational healthcare (employed working-age population), voluntary private health insurance, or by paying the full out-of-pocket cost.

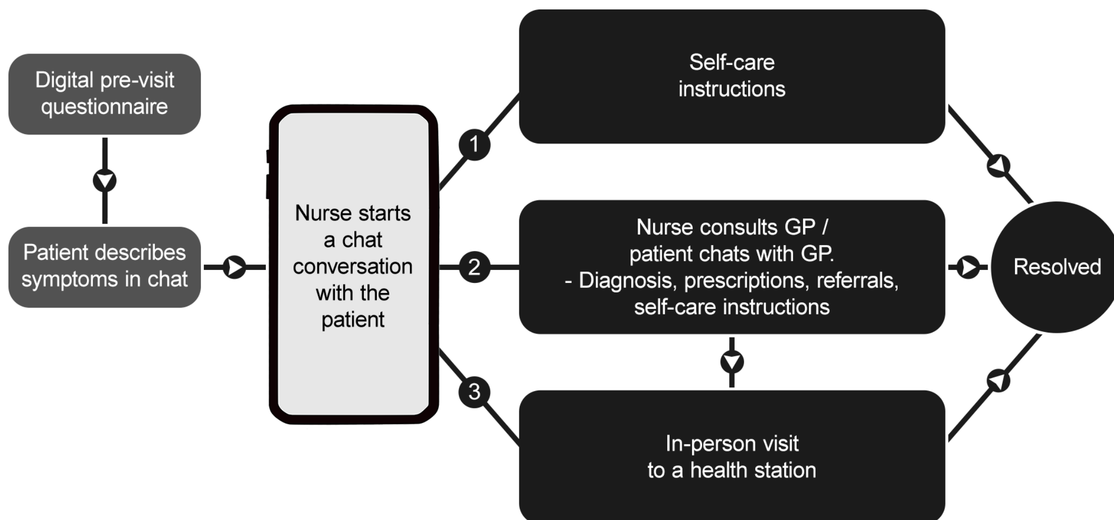

Figure 1: Potential care paths for a patient who experiences a care need and chooses to contact the public digital clinic instead of traditional public primary care by phone or by visiting a traditional PPC clinic in person.

assessment, asking follow-up questions via chat. In broad terms, the patient may i) receive instructions from the nurse for self-care and health monitoring, ii) be directed to a physician at the digital clinic (nurse-physician consultation or patient-physician chat), for diagnoses, prescriptions, or referrals (for lab tests or specialist visits), or iii) be directed to an in-person visit to an appropriate professional at the traditional PPC clinic.

From the provider's perspective, digital clinics allow healthcare professionals to manage several patients at the same time via chat, unlike in traditional clinics, where they manage one patient at a time in person or by phone. Consequently, compared with traditional clinics, digital clinics can save professionals' time as no time is wasted waiting for the next patient. The model also allows professionals to specialize in telemedicine and in the chat-based user interface. The task of asking routine questions is automated in the digital clinic via a pre-visit questionnaire. Having a large customer base with enough contacts outside typical office hours makes extended opening hours possible.

3 Intervention

The Wellbeing Services County of Ostrobothnia, a mid-sized administrative region in Western Finland (Figure 2), launched its *digital clinic platform*, a website and app for its digital services, on April 15, 2025. Over time, the digital clinic platform will include several different chat channels for various services. The main channel, the *digital clinic*, is a chat channel to contact primary care professionals. The initial contact will be with a nurse, after which the nurse has the opportunity to consult with a physician. Primary care patients with new health issues who choose to contact the digital clinic are expected to log in with strong identification for a care needs assessment and treatment. The digital clinic is open from 8 AM to 2 PM on Monday through Thursday and from 8 AM to 1 PM on Fridays.[7] Other chat channels that Ostrobothnia has launched, such as a chat for social services, a chat for rehabilitation, a chat for contraception services, a chat for mental health and addictions, and a chat for customer service, do not require strong identification, and are not intended to serve as a substitute for the digital clinic.

3.1 Access to the digital clinic

Our intervention randomized access to the digital clinic for a nine-month period, starting on April 15. The randomization assigned households to two groups that either have access (*the treatment group*) or do not have access (*the control group*) to the newly launched digital clinic. Individuals in the treatment group had access to the digital clinic immediately after its opening. The entire population residing in the region received access to the digital clinic after the nine-month trial period. The trial did not affect access to other available alternatives for contacting primary care, such as traditional PPC, occupational healthcare, or private clinics.

Individuals in the treatment group could access the digital clinic by logging into the clinic via a mobile application or website, using strong identification and personal identity number. We expected that some individuals belonging to the control group may try to log in to the digital clinic.

[7]The telephone service, a potential substitute, is is open from 8 AM to 3 PM on Monday through Thursday and from 8 AM to 2 PM on Fridays.

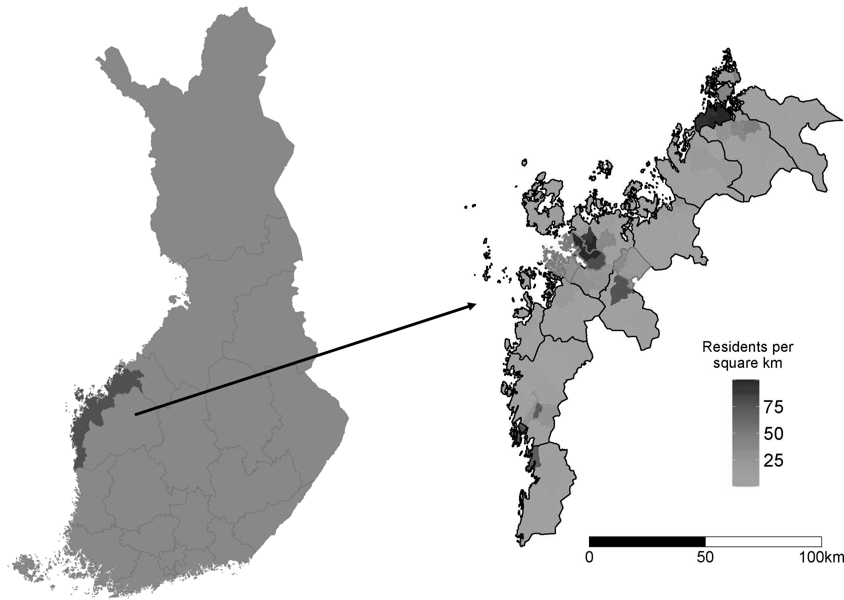

Figure 2: The Wellbeing Services County of Ostrobothnia is located in Western Finland.

Notes: By law, the Wellbeing Services County of Ostrobothnia is responsible for organizing public health, social, and rescue services, including public primary care (PPC), for its residents. Ostrobothnia has the highest share of Swedish speakers in Finland, with a Swedish-speaking majority in many municipalities. Unlike most of Finland, which is Finnish-dominated, Ostrobothnia has a strong bilingual culture. Unlike the heavily urbanized capital region, Ostrobothnia has a balanced mix of urban centers and strong rural communities. The region also has strong religious traditions, which have contributed to socially conservative values in some areas.

In this case, their access was automatically denied based on their identity number. Moreover, the digital clinic platform displayed a short message notifying them that their access to the platform is currently blocked because the digital clinic is being tested with a subset of the population, but that they will ultimately get access to the digital clinic after the test period.

3.2 Communication with the treatment group

The intervention (access to the digital clinic) was accompanied by an information campaign targeting all households in the treatment group. The primary communication channel with the treatment group was through mailed letters. These letters informed recipients about their option to use the digital clinic during the trial period and provided instructions on what the digital clinic is,

and how to use it, as well as the rationale for granting access initially only to a randomly-selected subgroup of the population. These letters were sent on the first week of May, 2025.

These information letters were sent to all households belonging to the treatment group. We sent one letter per household and randomized the recipient within the household so that all household members over the age of eighteen had the same probability of receiving the letter. In a small sample of households consisting only of minors, all individuals aged 15 to 18 had an equal probability of receiving the letter. There were no information letters addressed to individuals under the age of fifteen.

3.3 Population-wide information during the study period

The scale of the digital clinic launch and the significant changes in available healthcare services were expected to generate public discussion and interest in the reform. In response, the Wellbeing Services County of Ostrobothnia issued a press release about the digital clinic launch, its staggered implementation, and the associated informational letters to the treatment group on April 11, 2025, shortly before the launch of the digital clinic.

The launch of the trial received moderate media attention. YLE, one of the largest media outlets in Finland, wrote a brief article about it on April 11. In May 2025, the trial was mentioned in a local news paper *Vaasa-Pohjanmaa* (May 27th) and in the Finnish doctors' association's own publication *Lääkärilehti* (May 27th). In addition, the Ministry of Social Affairs and Health published an article on the trial on their website (May 28th). In June, a local newspaper *Vasabladet* reported about the launch of the trial (June 2nd).

In Fall 2025, Ostrobothnia also conducted the following information campaings: 1) Paid advertisement on social media on their digital platform (the app) and the digital clinic (the chat). 2) Digital messages to parents of schoolchildren informing about the digital platform and the digital clinic. 3) An information letter to all households in early October 2025, advertising seasonal influenza vaccinations, and informing about the services of the wellbeing services county, as well

as the digital platform and the digital clinic. 4) A press release on the early experiences with the digital clinic: the utilization rates and experiences of patients and healthcare professionals.

4 Study design

4.1 Target and study population

We extracted individuals whose municipality of residence was within the Ostrobothnia region on March 14, 2025 (target population). Our inclusion criteria required individuals to be alive at the time of extraction and to have a registered permanent address.[8] We additionally excluded individuals residing in the city of Kristinestad, as PPC services in this municipality are outsourced to a private provider. Finally, we aimed to exclude individuals residing in institutional care homes. We defined institutional care homes as residences where more than two individuals aged over 80 years lived or where more than four individuals over 60 years lived. We identified no other scientific or ethical reasons to exclude any other individuals who met the inclusion criteria from randomization. See Figure 3 for our target population and sample sizes.

However, in our analyses, we will restrict the sample to individuals aged 0 to 70 to have more statistical power. Moreover, we exclude from the analysis, but not from the randomization, those individuals who are observed in the Finnish Population Information System (study population) but not in the Statistics Finland datasets (background covariates used in analysis) – see Section 5. The number of such individuals was small.

4.2 Randomization

We randomized treatment at the household level based on permanent addresses, ensuring that all members of a household were assigned to the same treatment group. Households were stratified

[8]The permanent address was missing for 1% of the population. Age, gender, or language did not appear to be correlated with the address being missing. The permanent address is not recorded for individuals with a protection order (approximately 0.2% of the population nationally). The protection order is a legal measure in Finland that restricts the disclosure of an individual's address and other personal information in official registries to protect their safety and privacy.

by size to maintain balance across different household compositions. Within each stratum, we randomly assigned 50% of the households to the treatment group (a 1:1 ratio). Specifically, for each household ID cluster, we generated a random floating-point number and sorted the clusters by this value within each household size group. Households in the top 50% of these sorted values were assigned to the treatment group.[9]

Moreover, we randomized one recipient of the information letter (see Section 3.2) per treated household as follows: All household members over the age of eighteen had the same probability of receiving the letter. In a small sample of households consisting only of minors, all individuals aged 15 to 18 had an equal probability of receiving the letter. The randomization code, like all other code, is available in the Github repository of this project.[10]

5 Data and outcomes

5.1 Data sources

This study uses multiple Finnish administrative data sources containing individual-level data. The datasets are merged via pseudonymized person identifiers (IDs). Figure 3 summarizes different steps in our research design, including target population construction, randomization, and the construction of the analysis data and study population. We use the following data sources:

- Finnish Population Information System maintained by the Digital and Population Data Services Agency. This dataset was used to extract the target population on March 14, 2025.
- Register of Primary Care Visits maintained by the Finnish Institute for Health and Welfare. This dataset contains contacts with public primary care, private outpatient care, and occupational healthcare. We use data from 4/2024–1/2026.

[9] The actual proportion of treated households and individuals may not be exactly 50%. If the remainder when dividing the stratum size by 2 was not zero, we randomly varied between using the floor and ceiling function within each stratum to select the number of treated units. This approach ensures that approximately half of the units are in the treatment group.

[10] https://github.com/SoteDataLab/ostrobothnia_digi_rct

- Full population data on the socioeconomic and sociodemographic characteristics of Finnish residents, maintained by Statistics Finland (FOLK population data and INFRA location data) from 2024. *Note: At the time of writing, the latest data is available for year 2023. Data for the year 2024 is expected to be released in spring 2026. Due to this release lag, we use the data for year 2023 for the populated SAP, and the data for the year 2024 for the final research paper(s). The choice of the FOLK statistical data year affects the sample sizes of the analysis data. For example, restricting the analysis to the year 2023 data excludes from the analysis sample individuals born between January 1, 2024, and March 14, 2025.*

- Hospitalizations and contacts with specialized health care from the Care Register for Health Care, maintained by the Finnish Institute for Health and Welfare. We use these data from 4/2024 to 1/2026.

- Reimbursements for private doctor visits from the Finnish Social Insurance Institution. We use these data from 4/2024 to 12/2025. *Note: we will include data from January 1st–14th, 2026, as specified in the PAP, for the final research paper(s) once we receive access to this batch of data.*

- Entitlements to prescription drug reimbursements at a higher rate of special reimbursement from the Finnish Social Insurance Institution from 2024.

- Moreover, our data set includes self-collected data on the location (address) of traditional PPC clinics in September 2023 from the websites of PPC providers.

Access to the data. The research data are governed by their owners listed above. Access to the healthcare data can be obtained by sending a request to the Finnish Social and Health Data Permit Authority, Findata (https://findata.fi/en/). Access to demographic administrative data can be obtained by sending a request to Statistics Finland (https://www.stat.fi/en).

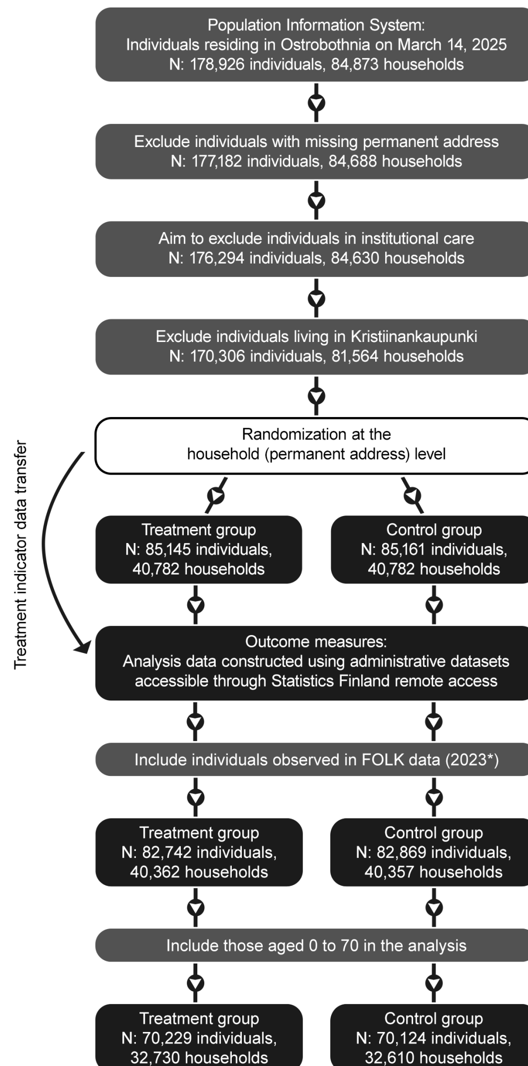

Figure 3: Target Population, Randomization, and Data Construction

Notes: For the final research paper, we will use the Statistics Finland full population data (FOLK) from 2024 as pre-registered (*). The FOLK data from 2024 are expected to be released in Spring 2026. For now, we use the FOLK year 2023 in this populated SAP. This has implications on sample sizes. For example, restricting the analysis to the FOLK statistical year 2023 would exclude from the analysis sample individuals born between January 1, 2024, and March 14, 2025, as they are not observed in the FOLK data for 2023.

5.2 Data cleaning and preparation

This populated SAP is accompanied by detailed R code on the construction of our variables, analysis data, and the study population (link: https://github.com/SoteDataLab/ostrobothnia_digi_

rct). The codes define how variables are constructed, missing data are handled, and transformations or aggregations are conducted. Note that:

- If an individual from the target population, extracted from the Population Information System, is not observed in Statistics Finland datasets, which is defined as not having a municipality of residence (missing data) at the end of 2024 for the final paper(s) (and 2023 for this populated SAP due to data release lag), we will not use that individual in the study population in the analyses because we do not have covariates for the individual. *Note: The Statistics Finland datasets are expected to become fully available for the year 2024 in spring 2026. Due to this release lag, we use the statistical year 2023 for the populated SAP, and the statistical year 2024 for the final research paper(s). The choice of the FOLK statistical year affects the sample sizes of the analysis data. For example, restricting the analysis to the statistical year 2023 excludes from the analysis sample individuals born between January 1, 2024, and March 14, 2025.*

- Some analyses in the final research paper will be post-blind, *i.e.*, implemented after unblinding the data by linking treatment indicators and outcomes. In these cases, the data construction choices will also be post-blind.

- There may be outliers, *e.g.*, individuals with suspiciously high health care utilization, leading to suspicion about duplicate values in the data (one underlying contact could be linked to several rows in the data). By defining health care contacts as the number of days with any contact (see Section 5.3) should partially alleviate the risk that outliers pose to the estimates. Our plan is to include potential outliers in the main analysis, but we may conduct a robustness check to examine whether the estimates change as a result when excluding individuals with suspiciously high health care utilization from the analysis.

5.3 Outcomes Y1.X and D.X: the utilization of public primary care

In this subsection, we list the outcomes registered earlier in the PAP. They were restricted to curative outpatient contacts in the public primary care.[11] The definitions of outcome variables are based on three additional variables: health care provision unit's identifier, contact type (telemedicine or traditional), and the profession (nurse or physician) of the provider.

As stated in Section 1, our main interest is in estimating the impact of *using* the public digital clinic on the use of traditional PPC services. Accordingly, we need to estimate impacts not only on the utilization of traditional PPC (ultimate outcome of interest) but also on the utilization of the digital clinic (take-up). We measure the annualized number of digital clinic contacts in PPC (D. digital clinic utilization; take-up) and the annualized number of contacts to traditional PPC clinics (Y. traditional PPC utilization; reduced form).[12] We count all contacts received during the same day as one contact or visit.

Our primary outcomes include the following types of contacts:

- **in-person visits in PPC (Y1.1)**. Our main research question is whether the use of digital clinics can reduce contacts with traditional PPC, including in-person visits and phone contacts. Of these contacts, we chose in-person visits as our primary outcome. In-person visits are more expensive to provide than phone contacts and require face-to-face interaction. At the same time, we expect that other contacts with traditional PPC, involving telemedicine (mainly phone calls) and professional-to-professional interactions, are a closer substitute for digital clinic contacts than in-person visits. *This outcome includes in-person visits to nurses and physicians in traditional PPC clinics.*

- **the number of public digital clinic contacts (D.1)**. This outcome is required for estimating the impact of *using* the public digital clinic on the utilization of traditional PPC. *This outcome*

[11] Restricting to curative contacts should exclude preventive contacts, such as seasonal influenza vaccinations. Restricting service type to outpatient care should exclude visits to other service types, such as school and student healthcare, dental care, or occupational healthcare.

[12] We compute the annualized number of contacts during our 9-month follow-up period by dividing the total number of contacts by 9 and multiplying it by 12.

includes care needs assessments, remote appointments to nurses and physicians (via chat), and professional-to-professional interactions between nurses and physicians in digital PPC clinics.

Our secondary outcome can help provide a more nuanced picture of whether the use of the digital clinic can reduce pressure on traditional PPC as compared to focusing strictly on in-person visits, which represent only a minority of contacts in traditional PPC. It includes all traditional PPC contacts except in-person visits:

- **other contacts with traditional PPC (Y1.2)**. We expect that the substitution rate between digital clinics and traditional PPC is higher with respect to this outcome, compared to in-person visits (Y1.1). Other traditional PPC contacts involve remote contacts (*e.g.*, phone calls between professionals and patients and professional-to-professional interactions) and are likely to be a closer substitute for digital clinic contacts than in-person visits. *This outcome includes care needs assessments, remote appointments to nurses and physicians, and professional-to-professional interactions between nurses and physicians in traditional PPC clinics.*[13]

Finally, we have two tertiary outcomes:

- **the total number of PPC contacts (Y1.3)**. We expect that the digital clinic access will increase the total number of contacts to PPC, including the digital clinic and the traditional PPC. The question is: by how much? *This outcome includes in-person visits to nurses and physicians, care needs assessments, remote appointments to nurses and physicians, and professional-to-professional interactions between nurses and physicians in digital and traditional PPC clinics.*

- **an indicator for having any public digital clinic contact during the follow-up (D.2)**. The purpose of adding this outcomes is to allow interested readers to construct the Local

[13] While our institutional knowledge suggests that nurses' care needs assessments and professional-to-professional interactions in traditional PPC are often done remotely on rather than in-person with the patient being present, our data does not distinguish between these contact types for these outcomes.

Average Treatment Effect (LATE) parameter by dividing reduced-form estimates on Y1.1 and Y1.2 by D.2. In other words, this outcome is an alternative approach for estimating the impact of *using* the public digital clinic on the utilization of traditional PPC. *This outcome includes care needs assessments, remote appointments to nurses and physicians (via chat), and professional-to-professional interactions between nurses and physicians in digital PPC clinics.*

5.4 Outcomes Y2.X: the utilization of specialized healthcare at hospitals

Our second family of outcomes includes variables related to hospital utilization. There are four outcomes in this domain.

- **The number of referrals to hospitals (Y2.1)** *This outcome includes the total number of referrals from public primary care to hospitals*.
- **The number of in-person ED contacts (Y2.2)** If the digital clinic access has an effect on the number of emergency department visits, we would expect it to be stronger for telemedicine contacts. Therefore we divide emergency department visits into two outcomes: in-person and other. *This outcome includes all in-person emergency department visits.*
- **The number of other ED contacts (Y2.3)** *This outcome includes all remote contact to emergency department as well as professional-to-professional interactions at the emergency departments.*
- **The number of outpatient hospital visits (Y2.4)** We focus on out-patient visits, as we do not expect to see any effect on longer hospital stays. *This variable includes all in-person out-patient contacts to hospitals in public sector.*

5.5 Outcomes Y3.X: impacts on the utilization of private clinics and occupational healthcare

Our third family of outcomes focuses on the potential impacts of the use of the public digital clinic on the use of other sectors, namely private clinics and occupational healthcare. We have four outcomes in this domain:

- **in-person visits in occupational healthcare (Y3.1)**. *This outcome includes in-person visits to nurses and physicians in occupational health care.*
- **other contacts with occupational healthcare (Y3.2)**. Occupational clinics are free-of-charge for patients at the point of use. For this reason, we assume that the public digital clinic utilization is hardly a substitute for telemedicine in occupational health care. *This outcome includes telemedicine contacts (chat, video, and calls) to nurses and physicians in occupational health care.* Due to data limitations, we cannot separate digital contacts from other telemedicine contacts.
- **in-person visits in private healthcare (Y3.3)**. *This outcome includes in-person visits to physicians in private clinics that are reimbursed by the Finnish Social Insurance Institution.*
- **other contacts with private healthcare (Y3.4)**. This outcome includes, among others, contacts with private digital clinics that are reimbursed by the Finnish Social Insurance Institution, which we think are the closest substitute for the public digital clinic. For instance, parents who earlier contacted a private digital clinic for their ill children may start to use the public digital clinic (lower out-of-pocket costs) once it is launched. *This outcome includes reimbursed telemedicine contacts (chat, video, and calls) to physicians in private clinics.* Due to data limitations, we cannot separate digital contacts from other telemedicine contacts.

6 Estimation and inference

To answer our research questions specified in Section 1.2, we estimate two statistical models. Our first model uses Ordinary Least Squares (OLS) to estimate the impacts of *access* to the public digital clinic. We estimates the impacts of digital clinic *access* on outcomes related to i) the utilization of the public digital clinic (D.1 and D.2; *take-up*), ii) the utilization of traditional PPC services (Y1.1 and Y1.2), and iii) the total utilization of PPC (Y1.3), including traditional and digital services. We estimate the following model for individual i:

$$Y_i = \beta_0 + \beta_1 1(Treatment)_i + \beta_2 X_i + \varepsilon_i, \quad (1)$$

where Y_i is the outcome of interest and $1(Treatment)_i$ is an indicator variable equal to one if the individual belongs to a household randomly assigned to the treatment group and zero if the individual belongs to a household randomized to the control group. X_i is a vector of control variables, defined at the end of this subsection. β_0 is an intercept, and ε_i is the error term. Our parameter of interest is β_1, which measures the causal effect of *access* to the public digital clinic on the corresponding outcome (D.1–D.2, Y1.1–Y1.3).

Our primary objective is to estimate the impact of *using* the public digital clinic on downstream outcomes. For this purpose, we account for non-compliance in the RCT, unlike in the estimation of ITT effects using Model 1. This non-compliance occurs because not all individuals in the treatment group are expected to use the digital clinic.[14] Due to this non-compliance, the impact of *using* the digital clinic can be expected to be different from the impact of *having access to* the clinic (ITT).

Our second model estimates the impacts of *using* the public digital clinic (D.1) on the utilization of traditional PPC services (Y1.1 and Y1.2), using Two-Stage Least Squares (2SLS) and the random assignment into the treatment group as an instrumental variable for the utilization

[14] Our experiment has one-sided non-compliance. All individuals in the treatment group cannot be expected to use digital health care services during the follow-up period, but none of the individuals in the control group are by construction able to use the digital clinic during the follow-up period.

of the public digital clinic.[15] Specifically, our parameter of interest is *the average causal response (ACR)*, which is defined as a generalization of the Local Average Treatment Effect (LATE) to settings where the treatment variable is multi-valued rather than binary (Angrist & Imbens, 1995). It represents the expected causal effect of a unit increase in the treatment variable (the number of digital clinic contacts) for individuals whose treatment status (digital clinic use) is influenced by the instrument (randomization).[16]

We estimate for individual i the following model using 2SLS:

$$\begin{aligned} D_i &= \alpha_0 + \alpha_1 1(Treatment)_i + \alpha_2 X_i + \varepsilon_i, \\ Y_i &= \pi_0 + \pi_1 \widehat{D}_i + \pi_2 X_i + \zeta_i, \end{aligned} \tag{2}$$

where $1(Treatment)_i$ is an indicator variable equal to one if the individual belongs to a household randomly assigned to the treatment group and zero if the individual belongs to a household randomized to the control group; D_i is the number of public digital clinic contacts (D.1) and $\widehat{D}$ is its predicted value based on the first equation, and Y_i is the outcome of interest (Y1.1–Y1.2). Moreover, X_i is a vector of control variables, defined at the end of this subsection. α_0 and π_0 are intercepts, and ε_i and ζ_i are the error terms. The parameter of interest, π_1, is the estimated average causal response (ACR).We use outcome D.1 as the take-up outcome in the 2SLS regressions.

Covariates: We include the following fixed effects as covariates (X_i) in all regressions (OLS and 2SLS): the previous number of in-person visits in PPC (Y1.1), an indicator variable for having at least one previous contact with occupational healthcare during 12 months preceding the trial, age (in years), gender, municipality, income percentile, language, distance quartile to the nearest

[15] See Angrist and Imbens (1995), Angrist et al. (1996), and Imbens and Angrist (1994) for the econometric and statistical background of using 2SLS estimation in randomized controlled trials.

[16] Formally, the ACR is a weighted average, over all values of d (potential intensity of the treatment), of the effect of increasing treatment from $d-1$ to d among switchers whose treatment status goes from strictly below to above d over time (Angrist & Imbens, 1995). In our application, we interpret it to measure the degree of substitution between the public digital clinic and traditional PPC for compliers who consult the digital clinic more only because they were offered access to it.

traditional PPC clinic, and indicators for having a common chronic disease and multimorbidity.[17] These covariates are expected to be uncorrelated with the treatment indicator, but can substantially improve the precision of our estimates.

Standard errors: Standard errors are clustered at the permanent address level, which corresponds to the level of randomization.

Multiple hypothesis testing: In the PAP and in the SAP, we did not pre-specify a plan to adjust for multiple comparisons in our main results table, Table 3, but we specify a hierarchy of outcomes (primary, secondary, tertiary) in Section 5.3. Consequently, we report p-values only for the primary outcome (the ACR and ITT effects on the number of in-person visits in PPC, Y1.1) and report 95% confidence intervals without p-values for all secondary and tertiary outcomes. The confidence intervals for secondary and tertiary outcomes will not be adjusted for multiple comparisons, suggesting that inferences drawn from these outcomes may therefore not be reproducible. However, for the other results tables registered in the SAP (Table 4 and Table 5), instead of p-values, we report sharpened q-values proposed by Benjamini and Yekutieli (2001) within each family of outcomes (Y2.X and Y3.X). The confidence intervals remain unadjusted.

7 Results

Table 1: Characteristics of the Residents at Baseline. This table presents the means, standard deviations (SD), percentage differences in means (difference %), and standardized mean differences (SMD) of several baseline covariates for the treated and control individuals in Ostrobothnia, as well as means for the populations of Ostrobothnia and Finland.[18]

[17]The common diseases covered here include special reimbursement rights for cardiovascular diseases, diabetes, respiratory diseases, rheumatic diseases, cancer, neurological diseases, and severe mental health disorders. Multimorbid individuals are defined as those with special reimbursement rights for at least two out of three: cardiovascular diseases, respiratory diseases, and diabetes.

[18]The standardized mean difference is the difference in means divided by standard deviation.

Table 1: Characteristics and Means Comparisons of Residents at Baseline.

	Treated N:	Control N:	Treated - Control	Ostrobothnia N:	Finland N:
	Mean [SD]	Mean [SD]	Diff. (%) [SMD]	Mean	Mean
A. Prior health care use					
PPC: in-person visits (days)	0.901 [2.295]	0.908 [2.394]	-0.791 [-0.003]	0.904	0.892
PPC: other contacts (days)	1.094 [2.379]	1.095 [2.424]	-0.075 [0.000]	1.095	1.507
Private HC: in-person visits (days)	0.255 [0.751]	0.258 [0.819]	-0.884 [-0.003]	0.257	0.486
Private HC: other contacts (days)	0.056 [0.370]	0.057 [0.367]	-1.988 [-0.003]	0.057	0.107
Occup. HC: in-person visits (days)	0.504 [1.480]	0.508 [1.488]	-0.763 [-0.003]	0.506	0.728
Occup. HC: other contacts (days)	0.516 [1.594]	0.521 [1.625]	-0.847 [-0.003]	0.519	0.712
B. Sociodemographic covariates					
Age (in years)	35.102 [19.995]	35.148 [20.001]	-0.130 [-0.002]	35.125	36.594
Is female (share)	0.482 [0.500]	0.482 [0.500]	-0.105 [-0.001]	0.482	0.492
Language: Finnish (share)	0.398 [0.490]	0.404 [0.491]	-1.509 [-0.012]	0.401	0.835
Language: Swedish (share)	0.491 [0.500]	0.485 [0.500]	1.396 [0.014]	0.488	0.049
Relationship or widowed (share)	0.353 [0.478]	0.356 [0.479]	-0.949 [-0.007]	0.354	0.326
Living in a city (share)	0.642 [0.479]	0.640 [0.480]	0.396 [0.005]	0.641	0.746
Dist. to nearest trad. PPC clinic (km)	4.304 [4.232]	4.297 [4.320]	0.173 [0.002]	4.300	3.397
Tertiary education (share)	0.280 [0.449]	0.283 [0.450]	-0.900 [-0.006]	0.281	0.291
Pensioner (share)	0.100 [0.300]	0.099 [0.299]	1.275 [0.004]	0.100	0.118
Employed (share)	0.577 [0.494]	0.575 [0.494]	0.237 [0.003]	0.576	0.575
Income (thousands of euros)	32.464 [29.907]	32.256 [28.928]	0.646 [0.007]	32.360	33.302
C. Morbidities					
Common chronic disease (share)	0.139 [0.346]	0.139 [0.346]	0.025 [0.000]	0.139	0.162
Has multimorbidity (share)	0.017 [0.130]	0.017 [0.130]	1.014 [0.001]	0.017	0.021
N	70,229	70,124		140,353	4,707,791

Notes: The table presents the means, standard deviations (SD), percentage differences in means (difference %), and standardized mean differences (SMD) of several covariates for the treated and control individuals, as well as means and SDs for the total population of Ostrobothnia and Finland. The analysis sample is restricted to those aged 0–70. In Panel A, health care use is measured in 12 months preceding the trial and represent annualized health care utilization. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in public primary care (PPC) include telemedicine, professional-to-professional interactions, and care needs assessments. For private clinics, we include reimbursed physician contacts. For occupational health care, we include curative contacts conducted by nurses or doctors. Covariates in Panel B are measured at the end of 2023. Living in a city is defined based on the city–countryside classification. Income is defined as the equivalent family disposable income. Distance to the neareast traditional PPC clinic is a straight-line distance. The list of traditional PPC clinics was collected in late 2023. In Panel C, morbidity is defined based on special reimbursement rights in 2024. The common diseases covered here include special reimbursement rights for cardiovascular diseases, diabetes, respiratory diseases, rheumatic diseases, cancer, neurological diseases, and severe mental health disorders. Multimorbid individuals are defined as those with special reimbursement rights for at least two out of three: cardiovascular diseases, respiratory diseases, and diabetes.

Annualized primary health care utilization is similar for treated and control individuals at baseline, measured 12 months before the trial. On average, both groups have approximately one in-person visit and one other contact in public primary care per year. The use of private and occupational health care services is less common (0.3–0.5 in-person visits and 0.06–0.5 other contacts on average) than the use of public primary care services (0.9 in-person visits and 1.1 other contacts on average), with minimal differences (less than 2 percent) between treated and control individuals.

Socioeconomic characteristics and measures of health status at baseline are also similar between the two groups. The mean age is 35 years, and 48 percent of individuals are female. Forty percent report Finnish and nearly half report Swedish as their primary language. Approximately 35 percent are in a relationship or widowed, 64 percent live in a city, and 28 percent have tertiary education. Ten percent of treated and control individuals are pensioners and 58 percent are employed, with a mean annual income of approximately 32,000 euros. The mean distance to the nearest public primary care clinic is four kilometers for treated and control individuals. In terms of health status, 14 percent have a common chronic disease (e.g., cardiovascular disease, respiratory disease, or diabetes), and 2 percent have multiple chronic conditions. Overall, the means of the outcomes and covariates at baseline are balanced between treated and control individuals, indicating that the randomization generated comparable groups.

There are some differences in the means of the outcomes and covariates between Finland as a whole and Ostrobothnia, where the trial took place. In terms of health care utilization, the number of other than in-person contacts is somewhat lower in Ostrobothnia than in the whole country (0.06–1.1 versus 0.1–1.5 contacts per year, depending on the sector). A larger share of individuals in Ostrobothnia are Swedish-speaking (49 versus 5 percent). In addition, a smaller proportion of individuals live in a city (64 versus 75 percent), and the mean distance to the nearest public primary care clinic is slightly longer in Ostrobothnia (four versus three kilometers). By contrast, the means of other socioeconomic characteristics, including age, sex, relationship status, education, pensioner status, employment status, and income, are quite similar across the two

regions. A somewhat smaller share of individuals in Ostrobothnia than in Finland have a common chronic condition (14 versus 16 percent), while the share with multimorbidity is two percent in both regions.

Table 2: Characteristics of Public Digital Clinic Users vs. Traditional PPC Clinic Users in the Treatment Group in Ostrobothnia. This table presents the means, standard deviations (SD), percentage differences in means (difference %), and standardized mean differences (SMD) of several baseline covariates for the users of public digital clinics and for the users of traditional PPC services among treated individuals, as well as means and SDs for the total population of Ostrobothnia.

We hypothesize that the digital clinic users differ noticeably from the general population and from the population that use traditional PPC services. This is indeed what we find. On average, users of public digital clinics have lower baseline health care utilization in the public sector but higher prior utilization in the private sector and in occupational health care compared with users of traditional PPC services. For example, within PPC, the mean number of in-person visits (days) at baseline is 1.5 per year for digital clinic users and 1.7 for users of traditional PPC clinics, with a difference of 11 percent. Compared with the total treated population of Ostrobothnia, digital clinic users have more prior health care use in PPC and in the private sector but somewhat less prior health care use in occupational health care at baseline.

Digital clinic users also differ from users of traditional PPC services in terms of their sociodemographic characteristics and health status at baseline. They are younger on average (34 versus 38 years), more likely to be female (66 versus 54 percent), and more likely to have Finnish as their main language (42 versus 37 percent). They are less likely to be in a relationship or widowed (36 versus 38 percent), more likely to have tertiary education (36 versus 24 percent) and to be employed (56 versus 49 percent), and less likely to be pensioners (11 versus 18 percent), which is expected given their younger mean age. In addition, digital clinic users are more likely to live in a city (69 versus 57 percent), but the average distance to the nearest PPC clinic is similar for both groups. They also have higher average income (approximately 32,500 euros versus 31,300

Table 2: Characteristics of Public Digital Clinic Users vs. Traditional PPC Clinic Users in the Treatment Group in Ostrobothnia.

Clients of:	digital clinics N:	trad. PPC clinics N:	Digi - trad. clinics	Treated N:
	Mean [SD]	Mean [SD]	Diff. (%) [SMD]	Mean
A. Prior health care use				
PPC: in-person visits (days)	1.506 [2.547]	1.700 [3.283]	-11.419 [-0.066]	0.901
PPC: other contacts (days)	2.075 [3.069]	2.077 [3.277]	-0.083 [-0.001]	1.094
Private HC: in-person visits (days)	0.371 [0.935]	0.282 [0.785]	31.781 [0.104]	0.255
Private HC: other contacts (days)	0.092 [0.513]	0.053 [0.366]	72.456 [0.086]	0.056
Occup. HC: in-person visits (days)	0.400 [1.372]	0.339 [1.269]	18.074 [0.046]	0.504
Occup. HC: other contacts (days)	0.437 [1.619]	0.340 [1.404]	28.676 [0.064]	0.516
B. Sociodemographic covariates				
Age (in years)	34.059 [20.732]	37.773 [21.892]	-9.830 [-0.174]	35.102
Is female (share)	0.652 [0.476]	0.538 [0.499]	21.191 [0.234]	0.482
Language: Finnish (share)	0.415 [0.493]	0.367 [0.482]	13.086 [0.099]	0.398
Language: Swedish (share)	0.539 [0.499]	0.518 [0.500]	4.079 [0.042]	0.491
Relationship or widowed (share)	0.362 [0.481]	0.378 [0.485]	-4.053 [-0.032]	0.353
Living in a city (share)	0.690 [0.462]	0.567 [0.495]	21.729 [0.257]	0.642
Dist. to nearest trad. PPC clinic (km)	4.438 [4.443]	4.435 [4.330]	0.074 [0.001]	4.304
Tertiary education (share)	0.358 [0.480]	0.241 [0.428]	48.728 [0.258]	0.280
Pensioner (share)	0.108 [0.310]	0.184 [0.387]	-41.312 [-0.216]	0.100
Employed (share)	0.559 [0.497]	0.492 [0.500]	13.647 [0.135]	0.577
Income (thousands of euros)	32.527 [17.120]	31.320 [36.021]	3.854 [0.043]	32.464
C. Morbidities				
Common chronic disease (share)	0.174 [0.380]	0.227 [0.419]	-23.209 [-0.132]	0.139
Has multimorbidity (share)	0.019 [0.136]	0.038 [0.192]	-50.990 [-0.118]	0.017
N	3,933	23,730		70,229

Notes: The table presents the means, standard deviations (SD), percentage differences in means (difference %), and standardized mean differences (SMD) of several covariates for the users of public digital clinics versus the users of traditional PPC clinics among the treated individuals. The two groups are constructed as follows: 1) those in the treatment group with at least one digital clinic contact in PPC are defined as users of the digital clinic, and 2) those in the treatment group with at least one contact in traditional PPC but zero digital clinic contacts are defined as the users of traditional PPC clinics. The table also contains means for the whole treatment group in Ostrobothnia. The analysis sample is restricted to those aged 0–70. In Panel A, health care use is measured in 12 months preceding the trial, representing annualized utilization. Health care contacts are defined in terms of contact dates: individuals get value 1 if they have any eligible contact on the given day. Other contacts in public primary care (PPC) include telemedicine, professional-to-professional interactions, and care needs assessments. For private clinics, we include reimbursed physician contacts. For occupational health care, we include curative contacts conducted by nurses or doctors. Covariates in Panel B are measured at the end of 2023. Living in a city is defined based on the city–countryside classification. Income is defined as the equivalized family disposable income. Distance to the neareast traditional PPC clinic is a straigth-line distance. The list of traditional PPC clinics was collected in late 2023. In Panel C, morbidity is defined based on special reimbursement rights in 2024. The common diseases covered here include special reimbursement rights for cardiovascular diseases, diabetes, respiratory diseases, rheumatic diseases, cancer, neurological diseases, and severe mental health disorders. Multimorbid individuals are defined as those with special reimbursement rights for at least two out of three: cardiovascular diseases, respiratory diseases, and diabetes.

euros per year, a difference of about 4 percent). Finally, digital clinic users have a lower prevalence of chronic diseases (17 versus 23 percent) and are less likely to have multimorbidity (2 versus 4 percent) at baseline.

Table 3: Effects of Access and Utilization of Public Digital Clinic on the Utilization of Traditional Public Primary Care. This table reports the results of our pre-registered confirmatory analyses using the individual-level data. In Panel A, we report the impact of having *access* to the public digital clinic on the use of the public digital clinic (take-up). In Panel B, we report the impact of having *access* to the public digital clinic on the use of traditional PPC services and the total use of PPC (intent-to-treat effect, ITT). In Panel C, we report the impact of *using* the public digital clinic on the use of traditional public primary care services (average causal response, ACR). Here, we use outcome D.1 (the total utilization of the digital clinic in PPC) as the take-up outcome in the 2SLS regressions (see Section 5.3).

We find that granting access to the digital clinic increases the use of digital clinic services (Panel A). Specifically, the number of digital clinic contacts increases by 0.11 per year (95% CI: 0.108 to 0.119). Similarly, the probability of having at least one digital clinic contact during the 9-month trial increases by 5 percentage points. The control group means are close to zero, which is expected since these individuals do not have access to the digital clinic.[19] This level of digital clinic utilization (0.11 contacts per year) is rather modest compared to the baseline of 0.83 in-person visits per year in the control group. Consequently, the launch of the digital clinic should have only modest short-term effects on the utilization of traditional services at the system level.

While the use of digital services increases after access is granted, there is little change in the number of in-person visits and we cannot rule out a zero effect based on the 95 percent confidence interval (CI). The intent-to-treat (ITT) point estimate indicates a reduction of 0.006, or -0.7%, in-person visits per year (95% CI: -3.1% to $+1.8\%$). The CI of the coefficient estimate for the average causal response (ACR) from the 2SLS regression (Panel C) indicates that we can

[19]The control group mean for digital clinic visits is not exactly zero because the data reflect manual entries made by health care personnel in the patient record system.

Table 3: Effects of Access and Use of Public Digital Clinic on Use of Traditional Public Primary Care.

	Digital clinic contacts (D.1)	Any digital clinic contact (D.2)	
A. Impact of *access* on the use of the digital clinics			
Effect	0.113	0.048	
Control group mean	0.012	0.008	
SE	0.003	0.001	
CI	[0.108,0.119]	[0.046,0.050]	
	In-person visits (Y1.1)	Other traditional contacts (Y1.2)	Primary care contacts in total (Y1.3)
B. Impact of *access* on the use of traditional primary care and primary care in total			
Effect	-0.006	-0.042	0.043
Control group mean	0.828	1.198	1.828
SE	0.010	0.013	0.019
CI	[-0.026, 0.015]	[-0.067,-0.016]	[0.007, 0.080]
Relative effect (percent)	-0.682	-3.481	2.359
Relative CI (percent)	[-3.144, 1.779]	[-5.612,-1.349]	[0.357, 4.361]
	In-person visits (Y1.1)	Other traditional contacts (Y1.2)	
C. Impact of *using* digital clinic (D.1) on the use of traditional primary care			
Effect	-0.050	-0.369	
SE	0.092	0.117	
CI	[-0.230, 0.130]	[-0.597,-0.140]	
N	70,229 (treated)	70,124 (control)	

Notes: The table contains our baseline intention-to-treat (ITT) results for the impact of access to the digital clinic and our baseline average causal response (ACR) results for the impact of the use of the digital clinic, using a 9-month follow-up. The analysis sample is restricted to those aged 0–70. We estimate the impacts on both the annualized number of digital clinic contacts (DCT utilization; take-up) in Panel A and the annualized number of contacts to traditional PPC (traditional PPC utilization; reduced form) or the annualized number of total contacts to PPC (ITT) in Panel B. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in traditional PPC include telemedicine (mainly calls), professional-to-professional interactions, and care needs assessments. Total contacts include digital clinic contacts, as well as in-person and other contacts with traditional PPC. Estimators: OLS with Model 1 in Panel A (take-up) and in Panel B (reduced-form), and 2SLS with Model 2 in Panel C (ACR). Covariates: fixed effects for the previous number of in-person visits in PPC (Y1.1), an indicator variable for having at least one previous contact with occupational healthcare during 12 months preceding the trial, age (in years), gender, municipality, income percentile, language, distance quartile to the nearest traditional PPC clinic, and indicators for having a common chronic disease and multimorbidity. Standard errors are clustered at the permanent address level (the level of randomization). 95% confidence intervals (CI) are reported. Relative effects are calculated by dividing effect estimates by control group means and multiplying by 100. Outcome D.1 (the total utilization of the digital clinic in PPC) is used as the take-up outcome in the 2SLS estimation. The p-values for the primary outcome Y1.1: ACR 0.587, ITT 0.587.

rule out reductions larger than 23 in-person visits for 100 generated digital clinic visits. These findings indicate that there is little substitution from in-person visits (days) to digital services.

By contrast, we find that access to the digital clinic reduces the number of other traditional PPC contacts (e.g., phone calls) by 0.04 per year, or 3.5 percent relative to the control group mean (95% CI: -5.6% to -1.3%). The coefficient estimate for the ACR from the 2SLS regression (Panel C) indicates that for 100 digital contacts, 37 other traditional PPC contacts are avoided (95% CI: -60 to -14).[20] Overall, our results show that other traditional PPC services are closer substitutes for digital services than in-person visits.

Because the digital clinic improves access to health care, it may generate new demand rather than merely substitute for traditional PPC services. Consistent with this interpretation, we find that the total number of primary care contacts (daily visits) increases by 0.04 per year, or 2.4 percent relative to the mean (95% CI: $+0.4\%$ to $+4.4\%$). As a daily visit is counted once even if contacts from multiple channels occur on the same day (e.g., a digital and an in-person contact), this measure does not fully capture changes in total PPC utilization. Using the coefficient estimates for different contact channels (digital, in-person, other), we can, however, approximate that $100 \times [0.113 - (0.006 + 0.042)]/0.113 \approx 58$ percent of the increase in digital clinic contacts represents new demand for PPC services and 42 percent represents substitution from traditional PPC services.

Table 4: Effects of Access and Utilization of Public Digital Clinic on the Utilization of Hospital Visits. This table reports the results on the use of specialized healthcare at hospitals, including the number referrals to hospitals, the number of ED contacts (in-person and other) and the number of outpatient hospital visists annually. The digital clinic may reduce hospitalizations by improving timely access to PPC, enabling earlier diagnosis and management of health conditions and better health outcomes. At the same time, improved access may increase hospital referrals from PPC.

[20] The coefficient estimate for the ACR is the same as the estimated Wald ratio defined as the ITT estimate for the other traditional contacts divided by the ITT estimate for the take-up, 0.042/0.113 $\approx$ 0.37.

Overall, we cannot rule out zero effects on hospital utilization outcomes based on the 95% CIs. The ITT point estimates of the impact of access to the digital clinic are small in magnitude in absolute terms, ranging from -0.015 to +0.002 annualized visits depending on the outcome. However, we note that the baseline means for referrals (Y2.1) and other ED contacts than in-person visits (Y2.3) are very low, which leads to large but imprecise, statistically insignificant point estimates in relative terms for these outcomes (increases of 6 to 7 percent).[21]

The ACR point estimates of the impact of using the digital clinic are also small in magnitude and not statistically significant. The ACR point estimates suggest that 100 public digital contacts lead to one additional referral (95% CI: -1 to $+3$), four fewer in-person ED contacts (95% CI: -14 to $+5$), one additional other ED contacts (95% CI: -1 to $+3$), and 13 fewer outpatient hospital visits (95% CI: -49 to $+22$) annually.

Table 5: Effects of Access and Utilization of Public Digital Clinic on the Utilization of Other Healthcare Sectors. This table reports the results on the use of health care services in other sectors, including private clinics and occupational healthcare. Because the digital clinic improved access to PPC services, it may have induced substitution from other sectors to the public sector. However, we find no statistically significant effects on the use of in-person or other (i.e., telemedicine) visits in these other sectors, and the confidence intervals do not allow us to rule out zero effects. The ITT point estimates for the impact of access to the digital clinic are negative and small in magnitude, suggesting a decrease of 0.001 to 0.010 annualized visits (-0.8% to -2.1% relative to the baseline), depending on the outcome.

The ACR point estimates for the impact of using the digital clinic are also not statistically significant. For occupational healthcare, the (statistically insignificant) ACR point estimates indicate that for 100 public digital contacts, seven in-person (95% CI: -20 to $+7$) and nine other visits (95% CI: -23 to $+5$) are avoided. For private healthcare, the corresponding ACR

[21] When comparing our register data with earlier years or with the official statistics released by the Wellbeing Services County of Ostrobothnia, we suspect that the number of referrals and ED contacts in our register data are smaller than in reality. Such data quality issues should be balanced across the treatment and control groups. However, missing data can lead to measurement error and make the estimates less precise.

Table 4: Effects of Access and Utilization of Public Digital Clinic on Utilization of Hospitals.

	Referrals to hospitals (Y2.1)	ED contacts		Outpatient hospital visits (Y2.4)
		in-person (Y2.2)	other (Y2.3)	
A. Impact of *access* on the use of hospitals				
Effect	0.002	-0.005	0.001	-0.015
Control group mean	0.022	0.280	0.018	0.895
SE	0.001	0.005	0.001	0.020
CI	[-0.001,0.004]	[-0.015,0.006]	[-0.001,0.003]	[-0.055,0.025]
Relative effect (percent)	6.948	-1.724	6.089	-1.654
Relative CI (percent)	[-2.735,16.631]	[-5.494, 2.047]	[-4.954,17.131]	[-6.140, 2.831]
Sharpened q-values	0.887	0.887	0.887	0.887
B. Impact of *using* digital clinic (D.1) on the use of hospitals				
Effect	0.013	-0.043	0.010	-0.131
SE	0.010	0.048	0.009	0.181
CI	[-0.005,0.032]	[-0.136,0.051]	[-0.008,0.027]	[-0.486,0.224]
Sharpened q-values	0.887	0.887	0.887	0.887
N	70,229 (treated)	70,124 (control)		

Notes: The table contains results on the utilization of specialized health care at hospitals: intention-to-treat (ITT) results for the impact of access to the digital clinic and average causal response (ACR) results for the impact of the use of the digital clinic, using a 9-month follow-up. The analysis sample is restricted to those aged 0–70. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in emergency department (ED) include telemedicine (mainly calls) and professional-to-professional interactions. Estimators: OLS with Model 1 in Panel A (reduced-form), and 2SLS with Model 2 in Panel B (ACR). Covariates: fixed effects for the previous number of in-person visits in PPC (Y1.1), an indicator variable for having at least one previous contact with occupational healthcare during 12 months preceding the trial, age (in years), gender, municipality, income percentile, language, distance quartile to the nearest traditional PPC clinic, and indicators for having a common chronic disease and multimorbidity. Standard errors are clustered at the permanent address level (the level of randomization). 95% confidence intervals (CI) are reported. Relative effects are calculated by dividing effect estimates by control group means and multiplying by 100. Sharpened q-values are reported to account for multiple hypothesis testing (Benjamini & Yekutieli, 2001). Outcome D.1 (the total utilization of the digital clinic in PPC) is used as the take-up outcome in the 2SLS estimation.

point estimates appear closer to zero, indicating that for 100 public digital clinic contacts, two in-person (95% CI: -9 to $+6$) and one other visits (95% CI: -5 to $+3$) are avoided.

Table 5: Effects of Access and Utilization of Public Digital Clinic on the Utilization of Other Healthcare Sectors.

	Occupational HC		Private HC	
	in-person (Y3.1)	other (Y3.2)	in-person (Y3.3)	other (Y3.4)
A. Impact of *access* on the use of other healthcare sectors				
Effect	-0.008	-0.010	-0.002	-0.001
Control group mean	0.525	0.496	0.210	0.052
SE	0.008	0.008	0.004	0.002
CI	[-0.023,0.007]	[-0.026,0.005]	[-0.010,0.006]	[-0.005,0.003]
Relative effect (percent)	-1.459	-2.085	-0.791	-1.732
Relative CI (percent)	[-4.312,1.395]	[-5.224,1.055]	[-4.552,2.970]	[-10.045,6.582]
Sharpened q-values	1.000	1.000	1.000	1.000
B. Impact of *using* digital clinic (D.1) on the use of other healthcare sectors				
Effect	-0.068	-0.092	-0.015	-0.008
SE	0.068	0.070	0.036	0.019
CI	[-0.200,0.065]	[-0.229,0.046]	[-0.085,0.055]	[-0.046,0.030]
Sharpened q-values	1.000	1.000	1.000	1.000
N	70,229 (treated)	70,124 (control)		

Notes: The table contains results on the utilization of other sectors of health care, occupational and private: intention-to-treat (ITT) results for the impact of access to the digital clinic and average causal response (ACR) results for the impact of the use of the digital clinic, using a 9-month follow-up. The analysis sample is restricted to those aged 0–70. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in occupational and private health care (HC) include telemedicine (calls, video calls, and chat). Estimators: OLS with Model 1 in Panel A (reduced-form), and 2SLS with Model 2 in Panel B (ACR). Covariates: fixed effects for the previous number of in-person visits in PPC (Y1.1), an indicator variable for having at least one previous contact with occupational healthcare during 12 months preceding the trial, age (in years), gender, municipality, income percentile, language, distance quartile to the nearest traditional PPC clinic, and indicators for having a common chronic disease and multimorbidity. Standard errors are clustered at the permanent address level (the level of randomization). 95% confidence intervals (CI) are reported. Relative effects are calculated by dividing effect estimates by control group means and multiplying by 100. Sharpened q-values are reported to account for multiple hypothesis testing (Benjamini & Yekutieli, 2001). Outcome D.1 (the total utilization of the digital clinic in PPC) is used as the take-up outcome in the 2SLS estimation.

8 Complementary analyses and next steps

For the final research paper or a set of papers, we may add more analyses compared to the SAP and PAP. Adding these analyses does not change our key research questions as stated in Section 1. We will label any post-blind analyses explicitly in the final research paper(s). We will also explicitly state that the results of the post-blind analyses are in nature more hypothesis-generating and hypothesis-confirming relative to the main pre-registered analyses.

We are considering additional analyses that could further deepen the interpretation of the results and clarify the mechanisms underlying the observed effects. First, given the low substitution between digital and in-person visits, it would be useful to explore whether these services are complements rather than substitutes, for example, by examining whether digital contacts are followed by subsequent in-person visits. Second, we plan to analyze changes in observable characteristics of patients conditional on having a PPC contact (digital, in-person, other), which would help assess potential selection patterns and changes in patient composition across different contact channels. Third, a dynamic analysis of treatment effects over time, such as an event study, could reveal how outcomes evolve and whether the impacts strengthen over time. Fourth, exploring heterogeneity in treatment effects by demographic characteristics or pre-existing health status would help identify which populations drive the results. Fifth, redefining PPC utilization in terms of total contacts across different contact channels within a day could provide a more comprehensive picture of PPC utilization and complement our main analyses on the effects of the digital clinic on PPC utilization in terms of daily visits. Sixth, examining waiting times would offer insights into improved access and potential efficiency gains from digital services. Finally, a more granular breakdown of “other traditional PPC contacts” would help identify which types of contacts, such as phone calls, care needs assessments, or professional-to-professional interactions, are driving the observed reductions in the outcome, while also informing interpretation in terms of the effects of the digital clinic on care pathways and triage.

We are also considering extending the analyses with the aim of answering some of the following questions:

- Does the utilization of the public digital clinic increase the number of prescription initiations of certain drugs or referrals to medical examinations (a potential benefit/cost of improved access)?
- Does the utilization of the public digital clinic affect continuity of care?

Precision of the estimates: Based on the simulations documented earlier in the PAP, we anticipate that incorporating pre-exposure data with fixed effects estimates will effectively reduce variance in our estimations. However, we also may assess the robustness of our estimation strategy by exploring machine learning-based tools for flexible covariate adjustment in experimental data (see, *e.g.*, List et al., 2024).

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Appendix A: Extra post-blind material

In this section, we present extra materials not included in the statistical analysis plan (SAP) but which we plan to include in the final research paper(s).

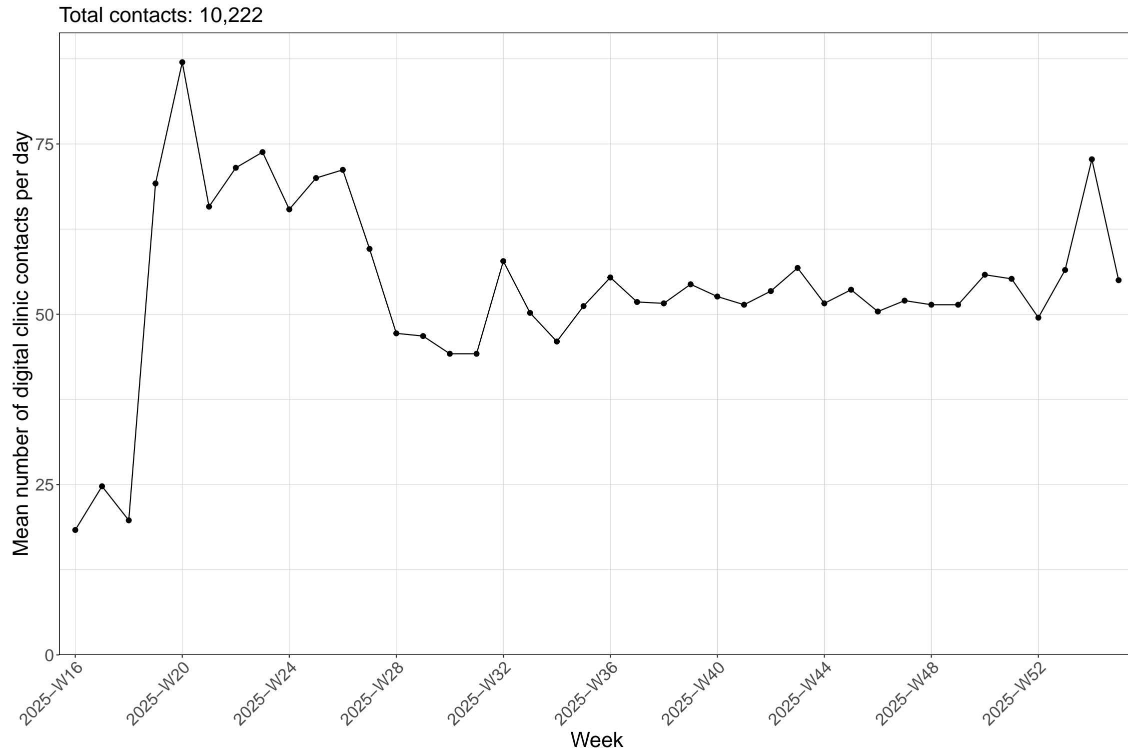

Figure A1: Mean Daily Digital Clinic Contacts by Week.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure presents the mean number of digital clinic contacts accross observed days within an ISO week during out trial period between April 15, 2025, to January 14, 2026. The data are from the digital clinic platform. The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70.

Figure A1: Mean Daily Digital Clinic Contacts by Week (post-blind). This figure presents the weekly means of the number of daily digital clinic contacts accross observed days. The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70. In total, there were 10,222 digital clinic contacts during our trial period recorded by the digital clinic platform.[22] The number of contacts was low (less than 25 contacts per day) in the first three

[22]Note that the aggregated digital clinic platform data are different from the electronic health records microdata that we use to construct our outcomes.

weeks of the trial and increased only after we mailed informational letters to the treatment group in Week 19, 2025. The utilization was at its highest in May–June 2025 (more than 65 contacts per day). In July 2025, the utilization was relatively low (less than 50 contacts per day) due to the holiday season. After the summer of 2025, the utilization was relatively stable, a little over 50 contacts per day, but below the top months of May–June 2025.

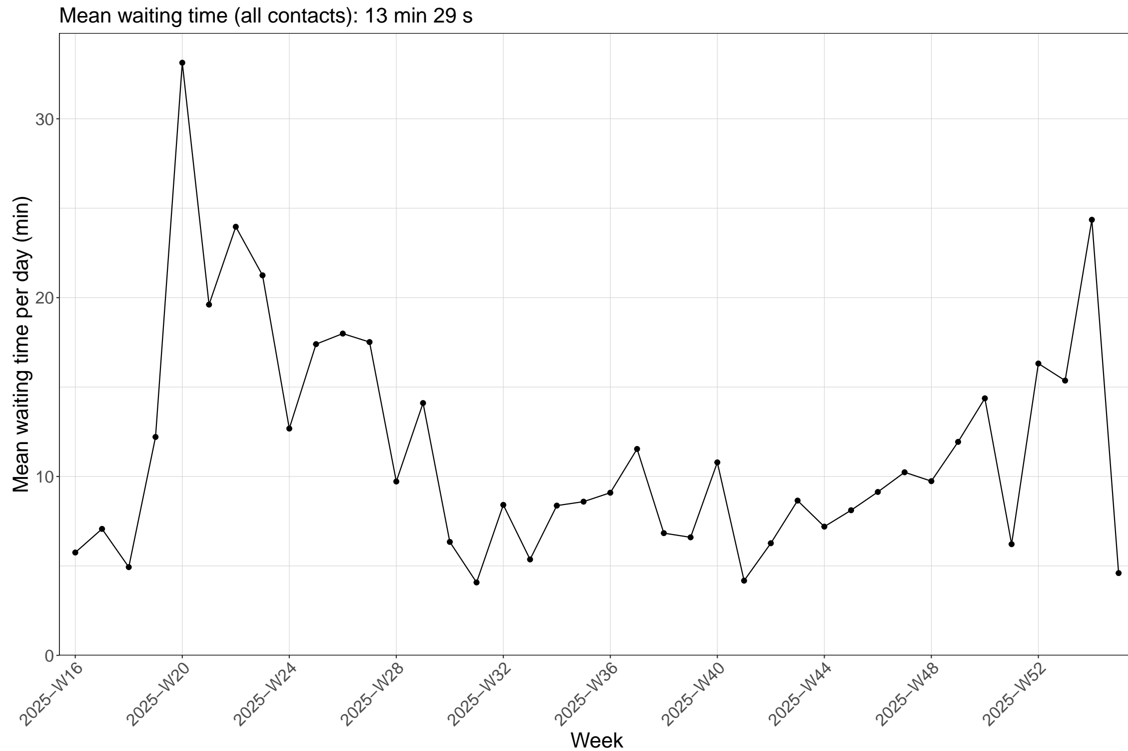

Figure A2: Mean Daily Waiting Time in the Digital Clinic by Week.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure presents the mean waiting time in the digital clinic accross observed days within an ISO week during out trial period between April 15, 2025, to January 14, 2026. The data are from the digital clinic platform. The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70.

Figure A2: Mean Daily Waiting Time in the Digital Clinic by Week (post-blind). This figure presents the weekly means of the waiting time in the digital clinic accross observed days. The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70. The mean waiting time accross all contacts was 13 minutes and 29 seconds, recorded

by the digital clinic platform. We observe that the weekly mean waiting time was positively correlated with the number of digital clinic contacts (see Figure A1). The waiting times were at their highest in May–June 2025 when the utilization of the digital clinic was its highest, with the waiting times increasing again at the end of the trial.

In contrast, the waiting times in the traditional telephone service for care needs assessment were somewhat longer for urgent matters and much longer for non-urgent issues – the patients themselves choose on the phone whether their issue is urgent or not. In a five-month period (6, 8–9, 11–12 / 2025), the mean waiting time varied between 7 min 21 s and 19 min 27s in the digital clinic, between 11 min 26 s and 26 min 19 s in the urgent calls, and between 2h 19 min and 23 h 7 min in the non-urgent calls.[23]

Moreover, the average duration of the chat exchanges, from the time a professional joined the conversation to the time the exchange was closed, was 34 minutes and 38 seconds.

Figure A3: The Share of 15 Symptom Groups Based on Digital Pre-Visit Questionnare (post-blind). This figure presents the shares of different symptom groups on the digital clinic based on a standardized digital pre-visit questionnaire that precedes chat exchanges between professionals and patients on the digital clinic. The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70. In total, there were 6,065 filled pre-visit questionnaires between May 2025 to January 2026 with November 2025 missing.[24]

The underlying data had 583 different symptom start points which we aggregated into 15 coarser symptom groups with the help of Claude, an LLM tool. Based on these data, respiratory infections and symptoms are by far the most common symptom group on the digital clinic (26% of the patients report these symptoms), followed by skin diseases and allergies (12%), musculoskeletal symptoms (11%), abdominal and digestive symptoms (10%), and urinary and

[23]Source: https://pohjanmaanhyvinvointi.fi/palvelumme/paivystys-ja-hoitoon-hakeutuminen/hoidon-tarpeen-arviointi/, Wayback Machine.

[24]The digital clinic platform recorded approximately 1,140 monthly contacts, while the pre-visit questionnaire recorded approximately 760 monthly contacts.

genital symptoms (8%). The least common of the 15 symptom groups is symptoms of chronic diseases (1%).

Figure A4: Survey Responses of Digital Clinic Professionals (post-blind). This figure presents survey responses of digital clinic professionals on whether they thought that digital clinic contacts were beneficial for the patient and whether they reduced the client's need to call or visit public primary care in person. The digital clinic platform asked these questions after every digital clinic contact (N = 10,222), and in 62% of the cases the professional answered (N = 6,371). Those professionals who responded were very positive: 96% of the respondents reported that they thought that the client benefited from the digital clinic contact. 85% of the respondents reported that they believed that the digital clinic contact reduced the client's need to call or visit in person in traditional PPC. Even in an overpessimistic scenario in which all nonrespondents would have answered ”no”, 60% of the professionals would have believed in the client benefiting and 53% would have believed that the digital clinic contact reduced the need to call or visit in person.

Figures A5–A7: The Effects of Access to Public Digital Clinic On Outcomes Over Time (post-blind). To illustrate how outcomes evolve over time after digital clinic access was granted, we plot the differences in the (cumulative) outcomes between treated and control individuals over time.[25] These results complement our main regressions and ITT results using pooled data over the entire trial period. Figure A5 shows that digital clinic contacts begin to increase approximately 25 days after access is granted (the corresponding main regression results are in Table 3). The results in Figure A6 show little effect on hospital utilization, although outpatient visits start to decline approximately 100 days after access was granted (the corresponding main regression results are in Table 4). Finally, the results in Figure A7 shows little evidence of substitution from the private sector and occupational health care to the public sector over time (the corresponding main regression results are in Table 5).

[25]Note that in these figures the outcomes are not annualized as in the main regression results.

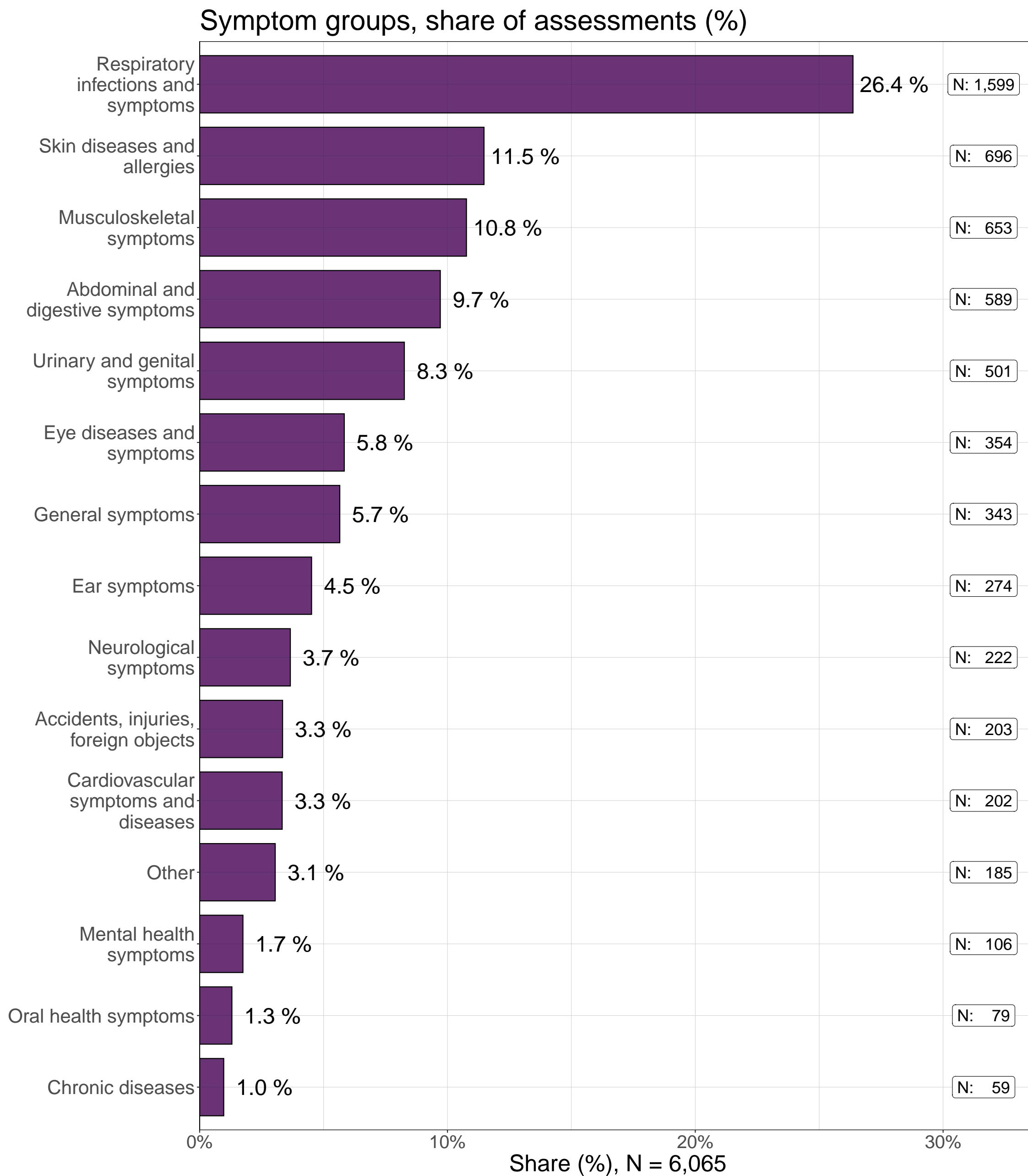

Figure A3: The Share of 15 Symptom Groups on the Digital Clinic Based on a Digital Pre-Visit Questionnaire.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure presents the shares of different symptom groups on the digital clinic based on a standardized digital pre-visit questionnaire that precedes chat exchanges between professionals and patients on the digital clinic. The data are from 5/2025 to 1/2026 (11/2025 missing, however). The underlying data are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70. The underlying data have 583 different symptom start points which we aggregated into 15 coarser symptom groups with the help of Claude, an LLM tool.

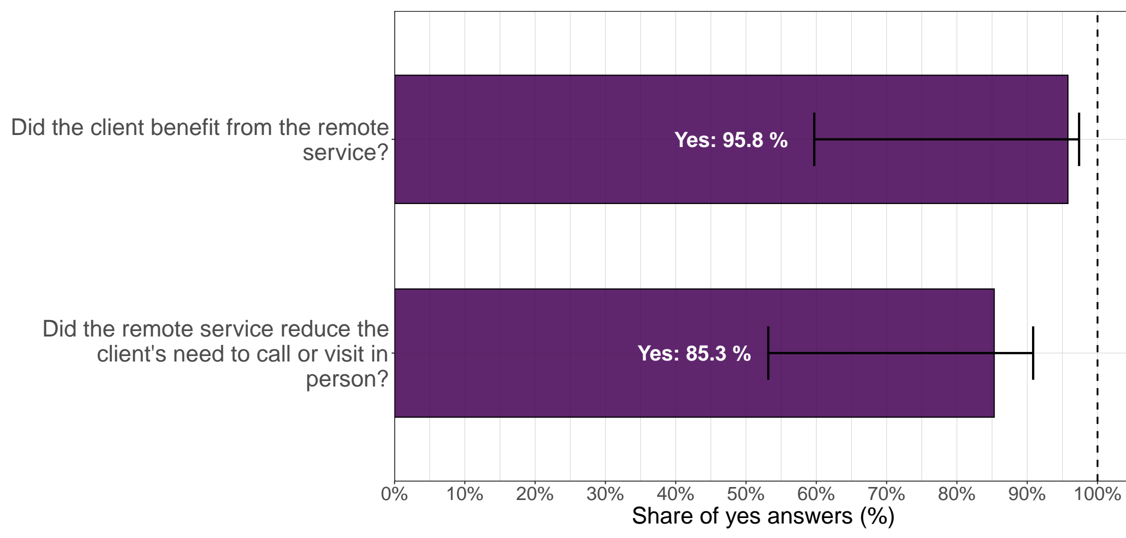

Figure A4: Survey Responses of Digital Clinic Professionals.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure presents survey responses of digital clinic professionals on whether they thought that digital clinic contacts were beneficial for the patient and whether they reduced the client's need to call or visit public primary care in person. The digital clinic platform asked these questions after every digital clinic contact (N = 10,222), and in 62% of the cases the professional answered (N = 6,371). The figure plots the shares of "yes" answers as percentages. The black error bars report bounds for two extreme scenarios: 1) all nonrespondents would have answered "no" (an overpessimistic scenario), and 2) all nonrespondents would have answered "yes" (an overoptimistic scenario). The underlying data from April 15, 2025, to January 14, 2026, are aggregated and include all patients, not just our analysis sample of those aged from 0 to 70.

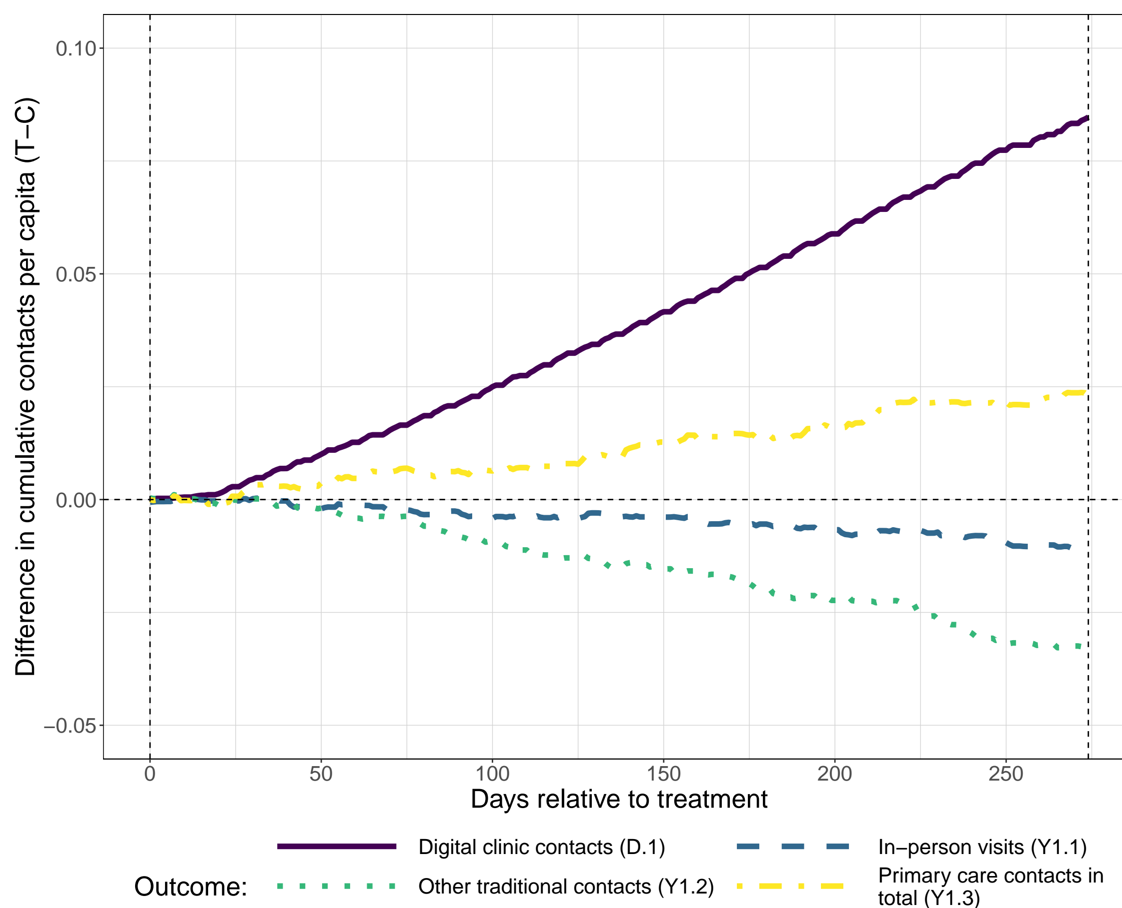

Figure A5: Effects of Access to Public Digital Clinic on Use of Traditional Public Primary Care.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure plots the cumulative difference in outcomes between the treatment group and the control group, focusing on the utilization of traditional public primary care (PPC). Positive differences indicate that the treatment group had more cumulative contacts. Essentially, the figure shows intention-to-treat (ITT) results for the impact of access to the digital clinic, using a 9-month follow-up. The corresponding regression results are in Table 3. Note that in this figure the outcomes are not annualized as in the main regression results. The analysis sample is restricted to those aged 0–70. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in traditional PPC include telemedicine (mainly calls), professional-to-professional interactions, and care needs assessments. Total contacts include digital clinic contacts, as well as in-person and other contacts with traditional PPC.

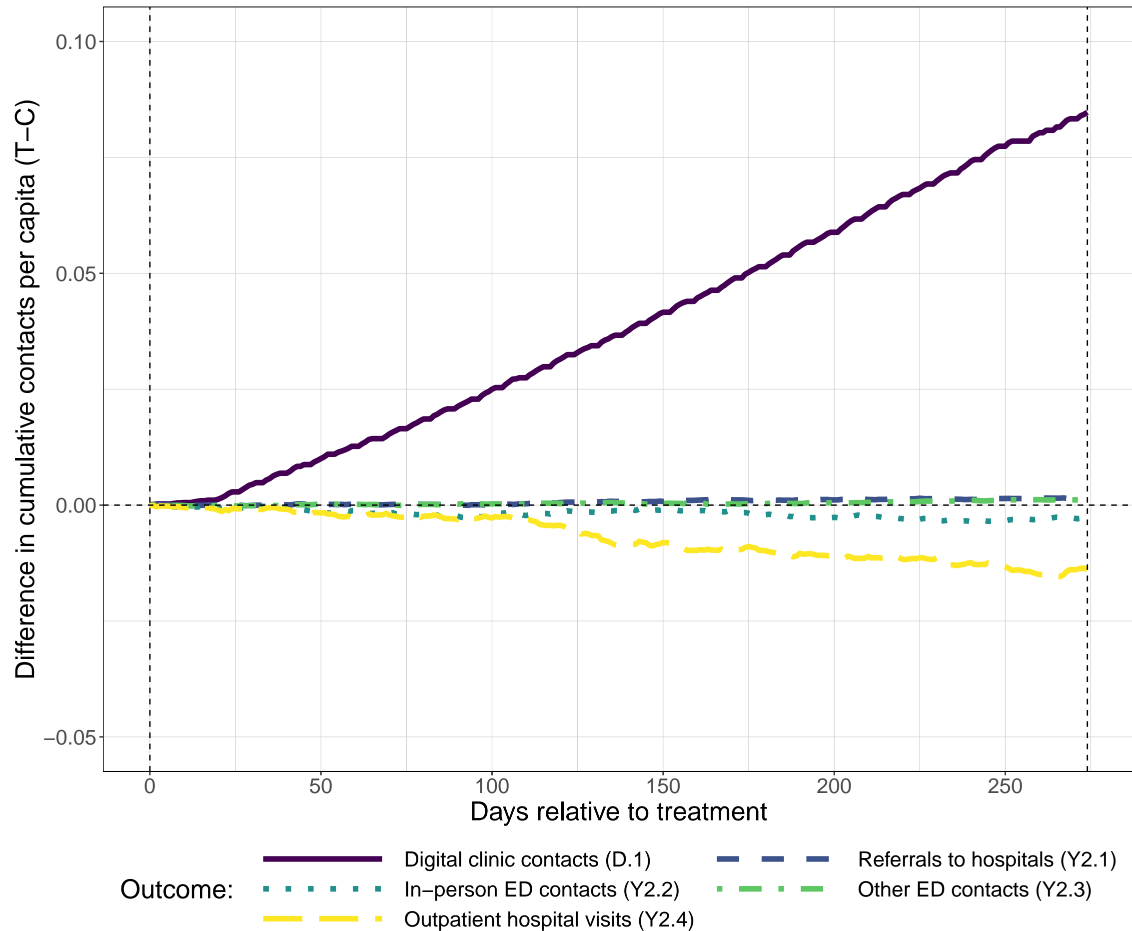

Figure A6: Effects of Access to Public Digital Clinic on Utilization of Hospitals.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure plots the cumulative difference in outcomes between the treatment group and the control group, focusing on the utilization of specialized health care at hospitals. Positive differences indicate that the treatment group had more cumulative contacts. Essentially, the figure shows intention-to-treat (ITT) results for the impact of access to the digital clinic, using a 9-month follow-up. The corresponding regression results are in Table 4. Note that in this figure the outcomes are not annualized as in the main regression results. The analysis sample is restricted to those aged 0–70. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in emergency department (ED) include telemedicine (mainly calls) and professional-to-professional interactions.

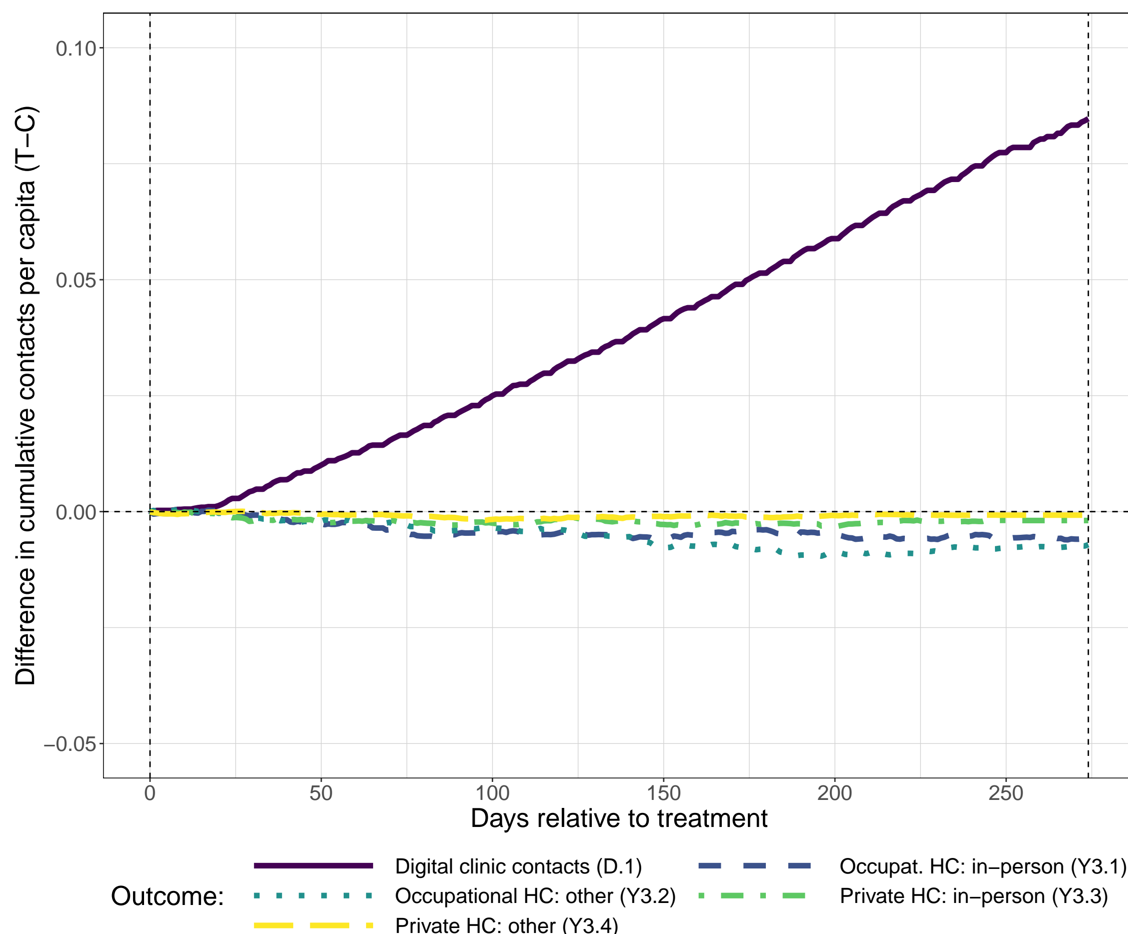

Figure A7: Effects of Access to Public Digital Clinic on the Utilization of Other Healthcare Sectors.

Notes: This **post-blind** analysis was added after registering the statistical analysis plan. The figure plots the cumulative difference in outcomes between the treatment group and the control group, focusing on the utilization of other sectors of health care, occupational and private. Positive differences indicate that the treatment group had more cumulative contacts. Essentially, the figure shows intention-to-treat (ITT) results for the impact of access to the digital clinic, using a 9-month follow-up. The corresponding regression results are in Table 5. Note that in this figure the outcomes are not annualized as in the main regression results. The analysis sample is restricted to those aged 0–70. Health care contacts are here defined in terms of contact dates: individuals get value 1 if they have any relevant contact on the given day. Other contacts in occupational and private health care (HC) include telemedicine (calls, video calls, and chat).

Appendix B: Expert forecasts

Survey design

Following DellaVigna and Pope (2018a, 2018b), we complement evidence from our RCT with expert forecasts. Our aim was to collect experts' priors about the impact of digital healthcare services (digital clinics) and to examine differences in impact forecasts across occupational groups.

To collect expert forecasts, we created an online survey using Qualtrics and invited participants to report their forecasts about i.) the take-up of digital clinic services (Research Question A) and ii.) the substitution rate between digital and in-person healthcare services (Research Question C | Outcome Y.1.1). At the beginning of the survey, participants read a brief description of the RCT, summarizing the institutional background and treatment variation. The participants were then informed of the opportunity to win a prize based on the accuracy of their forecasts. Following the incentive structure used by DellaVigna and Pope (2018b), we paid two randomly selected participants who completed the survey. These individuals received €$500-(Prediction\,error)^2/20$, where the prediction error denotes the absolute difference between the prediction and observed point estimate. The payment structure incentivizes participants to reveal their true beliefs about the study's key outcome variables. The opportunity to win large prizes (up to €500) was chosen to motivate high-earning experts to participate in the study and to truthfully reveal their beliefs about the effectiveness of digital health services.

We collected the two key forecasts using sliders controllable by finger (smartphone interface) or mouse (computer interface). The sliders were running from 0 to 100 percent. The forecasting tasks were followed by a concise questionnaire covering participants' background characteristics, including questions, for example, about their current occupation (e.g., nurse, physician, executive) and experience in providing digital healthcare services to patients. Our survey design and implementation were approved by the Institutional Review Board of the Finnish Institute for Health and Welfare through an evaluation process that was separate from that of the randomized controlled trial (Decision Number: THL/5935/6.02.01/2024)

Table B1: Descriptive Statistics for Experts Participating in the Forecasting Study.

Variable	Category	N (%)
Profession	Practical nurse	4 (1.3)
	Registered nurse	40 (13.1)
	Physician (GP)	144 (47.2)
	Physician (specialist)	17 (5.6)
	Healthcare executive	51 (16.7)
	Other profession	49 (16.1)
Sector of employment	Public	242 (79.3)
	Private	20 (6.6)
	Other (e.g., university, non-profit org.,...)	43 (14.1)
Age	20 - 29	12 (4.0)
	30 - 39	107 (35.8)
	40 - 49	102 (34.1)
	50 - 59	60 (20.1)
	60 or above	18 (6.0)
Experience in digital services	None	97 (31.8)
	0 - 1 years	67 (22.0)
	1 - 10 years	121 (39.7)
	More than 10 years	20 (6.6)

Notes: This table presents descriptive statistics for the experts participating in the forecasting study. Experts were recruited through professional associations, healthcare providers, and professional events. Forecasts and self-reported demographic information were collected through the Qualtrics online survey platform. The survey was administered in Finnish and Swedish, the two official languages of Finland. Expert forecasts were collected between September 9, 2025 and December 31, 2025.

Sample of experts

To collect the expert forecasts, we formed a list of relevant professional associations, healthcare providers, and professional events regularly attended by relevant experts. To obtain responses from these professional groups, we posted research invitations on the organizations' intranets and invited participants from various events where we were invited to present our ongoing research. Our sampling strategy can be described as a non-probability sampling mostly drawn from a broad and heterogeneous population of healthcare executives and medical experts working in the field. We informed all potential respondents that participation in the study was voluntary and that the online form could be completed anonymously. Finally, participants were assured that all data

would be analyzed and reported in a manner that does not allow individual identification. The online platform used to collect predictions was open from September 9, 2025 until December 31, 2025.

We obtained forecasts and answers to background questions from 305 individuals, as shown in Table B1. Out of the 305 participants, 161 were practicing physicians (144 GPs and 17 medical specialists) and 51 identified themselves primarily as healthcare managers or executives. The vast majority of participants (242) were employed in the public sector and had prior experience in providing or managing digital health care services.

Results

We highlight two key findings. First, Figure B1 shows substantial differences in predicted substitution between in-person and digital clinic visits across professional groups. Notably, general practicians (GPs) predicted on average a substitution rate of 30.0% (95% CI: 27.0–32.9), whereas individuals primarily identifying as healthcare executives expected a substitution rate of 50.7% (95% CI: 44.8–56.6). We can reject the null hypothesis of no difference in mean predictions between the GPs and healthcare executives (*t-test:* $t = -6.806$, $p < 0.001$).

Second, Figure B2 shows that the experts on average overestimated the substitution rate between in-person and digital clinic visits. The average predicted treatment effect among survey participants is 39.0% (red vertical line), whereas the point estimate of the substitution rate in the trial is 5.0% (Figure B2). The gray region around the point estimate (5.0%) represents the 95% confidence interval for the estimated treatment effect, accounting for the fact that the point estimate is a noisy estimate of the average causal response (ACR). The 95% CI lies clearly below the mean prediction, meaning that we can reject the equality between the average prediction and the actual treatment effect.

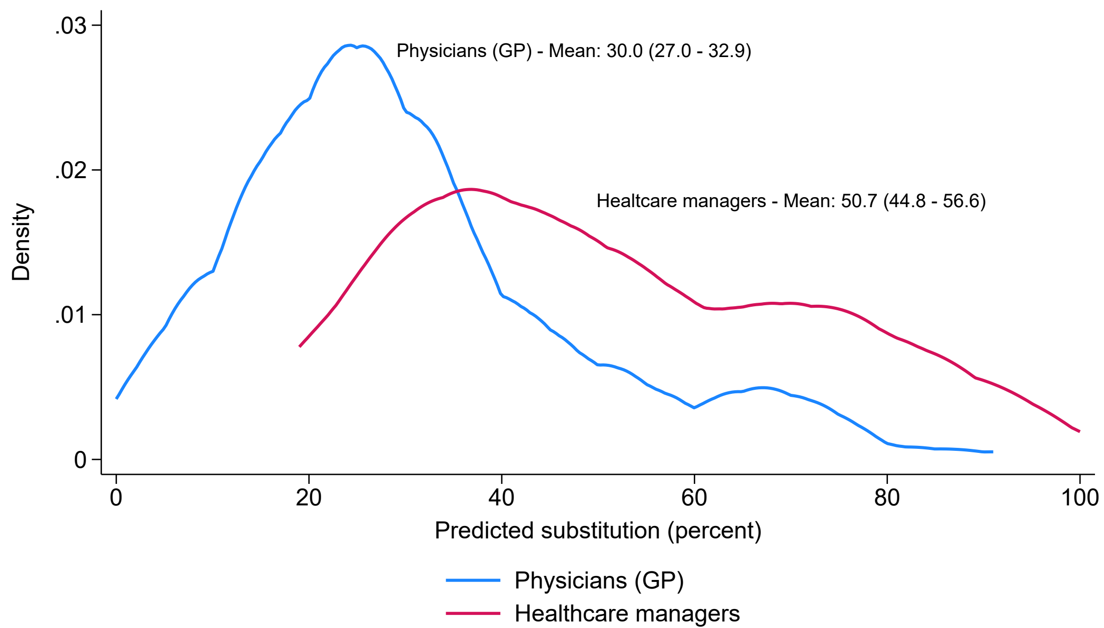

Figure B1: Forecasts by GPs and healthcare executives

Notes: This figure presents data from the expert survey collected for this study. The blue density function shows expert predictions of the treatment effect (substitution rate) by 144 general practitioners (GPs). The red density function shows predictions of the treatment effect by 51 healthcare executives. Means and 95% CIs are reported in the figure. Using the expert survey data, we can reject the null hypothesis of no difference in mean predictions between the GPs and healthcare executives (*t-test:* $t = -6.806$, $p < 0.001$).

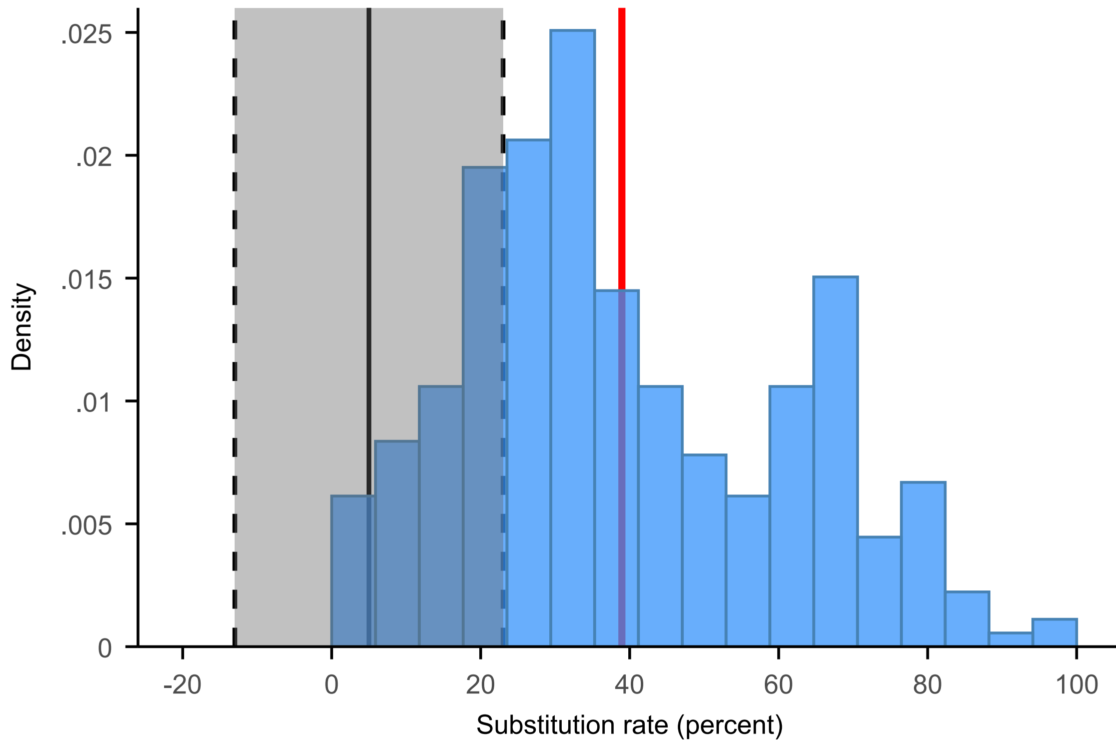

Figure B2: Predicted and estimated substitution between in-person and digital visits.

Notes: This figure presents data from the expert survey collected for this study. The histogram shows expert predictions of the treatment effect (substitution rate) by all 305 survey participants. The red vertical line shows the average prediction (39.0%) The black solid line indicates the actual treatment effect point estimate (5.0%). The gray region represents the 95% CI around the estimated treatment effect, accounting for the fact that the point estimate is a noisy estimate of an underlying average causal response (ACR).

Appendix C: Are there spillovers to traditional PPC?

There may be both a direct and an indirect impact of the experiment on access to traditional PPC. The direct effect is negative and caused by moving labor to the digital clinic (nurses and physicians, mostly nurses) from traditional PPC. The positive indirect effect potentially exists because digital clinic contacts (at least partially) substitute for traditional PPC. We expect that the spillover on access is largest for the care needs assessment telephone service and smaller for physician contacts due to institutional factors (gatekeeping by nurses at both digital clinics and traditional PPC).[26] The telephone service for the care needs assessment is centralized in Ostrobothnia (as the digital clinic will be) while other services in traditional PPC, such as in-person visits, are not as they are produced by traditional PPC clinics. The fact that the care needs assessment telephone service is centralized creates challenges for potential random saturation designs.

Consider the potential bias from spillovers via the following back-of-the-envelope model. Before the trial, the utilization of traditional PPC is the same (Y_0) for the treated and the control group due to the randomized treatment assignment. During the trial, suppose that the equilibrium is reached in three steps. In the first step, access to traditional PPC is reduced because some labor is transferred to the digital clinic. This reduces the use of traditional PPC by X units in both the treatment and control groups, with utilization being $Y_0 - X$ in both groups. In the second step, the treatment group has D digital clinic contacts (the control group 0). The substitution rate is α so that the utilization of traditional PPC is $Y_0 - X$ for the controls and becomes $Y_0 - X - \alpha D$ for the treated. In the third step, αD contacts to traditional PPC that became available because the treatment group did not consume them due to digital clinic contacts are allocated between the treatment and the control groups, increasing the use of traditional PPC. Ultimately, the utilization of traditional PPC is $Y_0 - X + \beta\alpha D$ for the controls and $Y_0 - X - \alpha D + (1-\beta)\alpha D$ for the treated.

[26] For reference, the digital clinic of the Wellbeing Services County of Pirkanmaa produced 31,000 contacts during its first two months. During the first month, there were 80,900 calls to the care needs assessment telephone service, which was 8,300 fewer calls than in the previous month. Source: https://www.pirha.fi/w/digiklinikalle-rekisteroitynyt-jo-yli-45-000-kayttajaa-pirkanmaalla, accessed on July 24th 2024.

Thus, the difference is $-2\beta\alpha D \in [-2\alpha D, 0]$ for $\beta \in [0,1]$. From this expression, we can see that it is the coefficient β which can make the estimated effect differ from the true effect ($-\alpha D$).

The back-of-the-envelope model suggests that there are two cases in which the estimated effect is unbiased: either 1) the substitution rate is zero ($\alpha = 0$) or 2) the potential spillover affects the potential outcomes of the treatment and the control group similarly ($\beta = 0.5$). In other words, the αD contacts with traditional PPC that became available because the treatment group did not consume them due to digital clinic contacts should be allocated equally between the treatment and the control groups. If these contacts are disproportionately allocated to the control (treatment) group, we would overestimate (underestimate) the substitution rate.

We believe that $\beta > 0.5$ and that our empirical approach overestimates the substitution rate. The logic is as follows. Suppose that 1) all health shocks can be ordered based on their severity, 2) the gatekeeping system leads to a situation where only the most severe (but not all) health shocks lead to a PPC contact, and 3) the distribution of health shock severity is uniform (unrealistic but assumed for simplicity) and the same for the treated and controls (in other words, no health effects from access to the digital clinic). Then, the difference in PPC use (including both the traditional PPC and digital clinics) between the treated and the controls would be $(1-\alpha)D$ at the second step of the above-described back-of-the-envelope model, before the αD contacts with traditional PPC that became available because the treatment group did not consume them due to digital clinic contacts are reallocated. If $\alpha \in]0, 0.5]$, then $(1-\alpha)D \geq \alpha D$, implying that the control group has αD or more untreated health shocks that are more severe than all the untreated health shocks in the treatment group, leading to $\beta = 1$. If $\alpha = 1$, then the utilization of PPC is the same for the treated and the controls ($Y_0 - X$), implying that $\beta = 0.5$ because the distribution of the untreated health shocks is the same for the treated and the controls. Finally, if $\alpha \in]0.5, 1[$, then $(1-\alpha)D \in]0, 0.5D[$ while $\alpha D \in]0.5D, D[$, implying that the majority of the contacts with traditional PPC that became available because the treatment group did not consume them due to digital clinic contacts are reallocated to the control group, with $\beta \in]0.5, 1[$. In fact, $\beta = \frac{1}{2\alpha}$

decreases for $\alpha \in]0.5,1[$.[27] Under the assumptions listed above, the previous results suggest that for $\alpha \in]0,0.5]$ our estimator would overestimate the substitution rate by a factor of 2 while for $\alpha \in]0.5,1[$ we would always estimate a substitution rate of 1.

The main limitation of the proposed experiment and the related analyses is that they do not take into account the potential spillover effects of the experiment on access to traditional PPC. How serious is this limitation? The answer is ultimately subjective, but we are not overly worried about the potential spillovers. Importantly, it would be unrealistic to assume that the αD contacts with traditional PPC that were not consumed by the treatment group due to the digital clinic would all be reallocated to the treatment and the control group (traditional PPC is not that supply-constrained). A more realistic model would thus contain an additional parameter $\gamma \in (0,1)$ in the expressions for the utilization of traditional PPC, which would become $Y_0 - X + \beta\gamma\alpha D$ for the controls and $Y_0 - X - \alpha D + (1-\beta)\gamma\alpha D$ for the treated. Thus, the difference is $-2\beta\gamma\alpha D - (1-\gamma)\alpha D$ instead of the earlier $-2\beta\alpha D$. From these expressions, it can easily be verified using inequalities that the bias is smaller once $\gamma \in (0,1)$ is included in the model.

Second, it would be unrealistic to assume that the newly available traditional PPC contacts are allocated entirely based on health shock severity. In other words, it would be unrealistic to assume that the newly available traditional PPC contacts are allocated entirely to one of the two treatment groups ($\beta = 1$ or $\beta = 0$), limiting the size of the potential bias. For example, $\beta = \frac{2}{3}$ ($\beta = \frac{3}{5}$) [$\beta = \frac{4}{7}$] would lead us to overestimate the substitution rate only by a factor of 1.33 (1.2) [1.14].

One way to measure changes in access to traditional PPC is to track the mean and maximum response time of the call-back service for care needs assessment. The Wellbeing Services County of Ostrobothnia follows these indicators regularly, and they should be also followed regularly throughout the trial. However, a simple before-and-after analysis of response times can be highly sensitive to time effects. Alternatively, we could compare the evolution in the outcomes before and after the experiment between the control group in Ostrobothnia and

[27] $\beta = \frac{(1-\alpha)D + [\alpha D - (1-\alpha)D]/2}{\alpha D} = \frac{1}{2\alpha}$ for $\alpha \in]0.5,1[$

individuals residing in other wellbeing services counties in a difference-in-differences (DID) framework. However, we believe that the signal would be modest relative to noise in this analysis (lack of power).[28]

[28]This hypothesis is based on the findings of two earlier studies that examine the impacts of copayment changes on the utilization of traditional PPC in Finland, both based on a DID approach and having a much larger population in analysis than we would have for testing spillovers (Haaga et al., 2024a; Haaga et al., 2024b).