

Digital Access to Healthcare Services and Healthcare Utilization: A Quasi-Experiment

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Abstract

Objectives. This paper assesses whether facilitating the digital access to healthcare services impacts healthcare utilization. We exploit the introduction of a user-friendly web portal allowing women aged 25-65 to manage online their appointments in the public cervical cancer screening program carried out by a North-Eastern Italian Local Health Unit (LHU) in November 2019. We report quasi-experimental evidence on how this intervention affected both the program participation and the ability of the LHU to collect information on women's screening behaviour outside the program.

Methods. We use administrative data from the LHU and a difference-in-differences design to compare the changes in the outcome measures across the current and the previous invitation rounds for women in the control group (n=768) – who had no access to the web portal and could reschedule the appointments only through phone calls to the LHU – and in the treated group (n=870) – who could reschedule their appointments either by phone or through the web portal.

Results. Invited women do manage their appointments online, implying that the web portal reduces the number of phone calls to reschedule the appointments. The web portal also makes women more likely to report previous screenings and the reasons for cancelling their current appointments. However, giving access to the web portal decreases the probability of attending the screening by roughly 15 percentage points, and around two thirds of the reduction in participation is due to a higher likelihood of unjustified no-shows.

Conclusions. Digital innovations can undoubtedly increase the organizational efficiency of national screening programs. However, due to behavioural biases such as limited attention and procrastination, these interventions may backfire, discouraging women's screening. Pairing the web portal with frequent reminders sent to invited women may represent a viable solution to boost participation.¹

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Introduction

The digital transformation of services and the introduction of information and communication technologies (ICT) have considerably influenced healthcare provision¹ and a growing body of scientific literature tries to assess the impact of ICT innovations on the effectiveness, accessibility, and resilience of the healthcare systems.²⁻⁴

We contribute to this literature by reporting quasi-experimental evidence from a North-Eastern Italian local health unit (LHU) that introduced a user-friendly web portal allowing women enrolled in the national cervical cancer screening program to autonomously modify (either reschedule or cancel) their pre-assigned screening appointments. We employ administrative data from the LHU and a difference-in-differences design to compare the changes in several screening outcomes across the current and the previous invitation rounds for women in the control group – who had no access to the portal and could reschedule the appointment only through a phone call to the LHU – and in the treated group – who could reschedule their appointment either by phone or through the portal.

We assess the effects of the introduction of the web portal on three distinct outcomes. First, we study whether the web portal was effectively used by women to reschedule appointments. This is not a trivial question, given that the uptake of digital services is rather low, and governments struggle to make citizens use these services⁵. For example, according to Eurostat, only 55% of the EU population used online interfaces for interacting with the public authorities in 2019.

Second, we investigate whether the introduction of the web portal affects participation in the screening program. Providing women with an alternative digital solution to reschedule the date of the screening slot represents a nudge that can weaken the behavioural obstacles to cancer screening.⁶ For example, phone-averse women^{7,8} who would like to reschedule the pre-specified screening slot may be reluctant to call the phone service of the LHU and, therefore, may eventually postpone the screening opportunity, with negative effects for cancer prevention and higher costs for the healthcare system^{9,10}. Nonetheless, if given the opportunity to reschedule the appointments online, phone averse women may benefit from this chance and attend the screening.

Third, by using data on the reasons why women cancel their appointments, we assess whether the introduction of the portal increases the ability of the LHU to collect information on women's screening behaviour outside the public screening program.

We depart from existing studies in two respects. First, previous contributions investigating the impact of proactive web services on healthcare utilization and health outcomes generally compare program participation and health outcomes of users and non-users in setups where the proactive system is already in place for everyone to use it, and exploit the observed variation in the probability of *using the*

portal.^{11,12} In our setting, *the availability of the portal* is exogenously manipulated, which allows us to establish a cleaner causal relationship between the availability of a proactive web portal and the behaviour of eligible patients, and to properly address the issue of self-selection of users. Second, while most of the literature analyses the effects of non-proactive ICT innovations in screening programs (including platforms that are actively used by the program administrations for better management practices,^{13,14} computerized systems that send reminders to patients,¹⁵⁻¹⁷ interactive webpages providing information to the patients^{15,18}), we assess the impact of a newly introduced digital technology *in which women enrolled in the screening program have a proactive role* and can autonomously reschedule their appointment.

1. Methods

1.1 The institutional setting

In Italy, the implementation of public cervical screening programs has been recommended since 1996^{19,20} and included in the Ministry of Health's "Essential Health Interventions" list since 2001. The management of the national cancer screening program is handled by the 20 Italian Regional health authorities following national guidelines. Regions are further organised in LHUs that cover smaller geographical areas (akin to provinces) and take care of the local implementation of the program by delivering invitations, handling appointments, and managing the screening operations within dedicated screening centres. Women are automatically linked to screening centres on the basis of their municipality of residence.

The intervention analysed in this study was carried out by a LHU located in North-Eastern Italy. For privacy reasons, we have to anonymise all the information on the LHU. The cervical cancer screening program has been in place in this LHU since 1996, has population-level coverage and targets women aged 25 to 64. The actual coverage is above 80 percent, and the average screening take-up is around 65 percent once valid reasons for exclusion (recent test undertaken outside the program, pregnancy status, health conditions) are considered. Women aged 25-29 are offered to take a PAP test every three years while women aged 30-65 are offered to take a HPV test every five years.

Within the program, women receive closed-date invitations at regular intervals by regular mail. Invitations are scheduled 3 years after a missed invitation, 3 years after a PAP test, 5 years after a HPV test, and 1.5 years after a cancelled invitation for pregnancy. Screening slots are allocated to women on a monthly basis, and the available time slots are randomly allocated among women eligible to screen within a given month. A text message reminds women about their appointment a few days before the scheduled date.

1.2 The invitation letters and the quasi-experimental manipulation

The intervention targets women invited for a cervical cancer screening within the program between November 2019 and January 2020. This population was divided in two groups:

- i)* the “phone only” control group has access to the phone service only to manage the appointments. The web portal is not accessible to this group, and neither is this group formally aware of the existence of the web portal.
- ii)* the “web or phone” treatment group has access to both the phone service and the web portal. The invitation letter provides all the information needed to access and use the portal as well as the login credentials of the recipients. Some information about the portal, such as the potential advantages to users in terms of flexibility and autonomy in managing their appointments, is printed on the back of the invitation letter.

The LHU has several different screening centres, four of which are involved in this program. Letters for each screening centre are prepared and dispatched in separate batches. The solution adopted to generate variability in treatment status was to dispatch different letters to women affiliated with different screening centres (all females in a screening centre received the same letter). Screening centres were allocated to different treatment groups in such a way that trends in screening take-up across the two groups for the periods November 2018-January 2019 vs. November 2017-January 2018 were comparable. The treatment received by each of the groups was then randomly assigned. The resulting allocation is depicted in Online Appendix 1, Table A1.1, while the invitation letter is reported in Online Appendix 2.

1.3 Data

The data come from the administrative archives of the LHU and were anonymised prior to the analysis. For each woman invited during the intervention, we collected information on the month and year of birth, the screening centre of reference (and hence treatment status), the date of the screening invitation, the type of test proposed (PAP or HPV), and the outcome of the invitation. This is coded as follows:

- Screened: the invited woman participates in the program.
- No-show: the invited woman does not participate in the program and does not inform the LHU about her absence. Importantly, the screening program had to be stopped on March 1st, 2020, because of the COVID-19 pandemic, and all women who did not take part in the program or did not communicate their absence by that date are recorded as no-shows. As a result, during the current round we also monitor re-scheduling for a shorter time-period than in previous rounds. Fortunately, this is not a concern empirically, as in previous rounds only 19 subjects in total moved the appointment later than the end of the monitoring period during the current round. In addition, the distributions of re-scheduling times in the previous and current round overlap largely.

- Cancelled slot: the invited woman informs the LHM that she will not participate. Cancellation can be communicated over the phone in the control group and via the web portal or over the phone in the treatment group. The appointment can be cancelled for the following reasons: *i*) having already undertaken a PAP or HPV test in the last three years in a private facility or within the public sector following a GP or gynaecologist prescription; *ii*) pregnancy; *iii*) other health reasons; *iv*) general willingness to drop-out from the screening programme without any reason. In case of cancellation, women are asked to indicate the reasons for cancelling and, eventually, to provide other relevant information (such as the date of the external examination and associated gynaecologist prescription). If the reason for cancellations are the recent tests carried out privately, women are also requested to report the dates of these tests so that the next invitations can be correctly scheduled.

In addition, we know whether the appointments were moved or cancelled and whether the rescheduling or cancellation took place via phone or the portal.

We also gained access to the screening history of women involved in the intervention. For each previous invitation, we know the invitation year, the type of the test proposed (PAP or HPV), the outcome of the invitation (coded as above) and whether the appointment was handled (moved or cancelled) by women. As described in Online Appendix 3, where we describe our sample selection criteria, we limit the sample to the current and up to two previous invitations for each woman.

The initial sample consists of 5,642 invitations for 1,659 women aged 25-65 residing in the 20 municipalities related with the four screening centres of our partner LHM. The final sample is composed of 4,003 invitations for 1,638 women. In total, 1,005 women (61% of the sample) are observed 3 times, 355 (22%) are observed twice, and 278 (17%) are only observed once. Please refer to Online Appendix 3 for details on sample selection.

Table 1 reports descriptive statistics for the final sample. Average age is close to 39 years, 53.9 percent of observations belong to the treatment group and 46.1 to the control. In terms of outcomes, 39.7 percent of all appointments have been handled and roughly 90 percent of the handling took place over the phone. Close to 44 percent of appointments ended in a completed screening, 38 percent are no-shows, and 18 percent were cancelled. Roughly 3 out 4 cancellations took place because of a recent test, and 1 in 4 for other reasons. In addition, for invitations beyond the first one observed, we also reconstruct the outcome of the previous invitation, as this determines the spacing between invitations.

1.4 Empirical approach

We use a difference-in-differences (DiD) design. We identify treatment effects comparing the observed trends in the outcomes over invitations and across the treatment and the control groups. Identification of the effect of the web portal rests upon the assumption that, after the introduction of the portal, the treatment and the control groups would have followed the same trend in the outcomes had the web

portal not been introduced. We test this assumption using data for invitations dating back to the period before the introduction of the web portal. The identification strategy also requires that no shocks separately affect either the treatment or the control group during the intervention. The strong control over the institutional setup corroborates the validity of these assumptions. The program is implemented in a geographic area with a homogenous population, and the management of the program falls within a single LHU, that applies the same screening protocols throughout its territory.

Formally, we estimate the following model with Ordinary Least Squares (OLS):

$$Y_{igt} = \beta \text{WebOrPhone}_g \times \text{Post}_t + \phi_t + \text{WebOrPhone}_g + X'_{igt}\delta_t + \varepsilon_{igt} \quad (1)$$

In Equation (1), subscripts i , g and t stand for individual, treatment group and invitation, and Y is a vector of invitation outcomes. t equals 0 for the current (post-intervention) invitation, and -1 or -2 for the first- and second-to-last invitations, respectively. Post_t is a dummy variable that equals 1 if $t = 0$ and 0 otherwise. WebOrPhone_g is a dummy variable that equals 1 for women in the treatment group and 0 otherwise. The main coefficient of interest is β , that identifies the DiD treatment effect. In addition, ϕ_t are invitation round fixed effects, and X_{igt} is a vector of invitation-specific controls that includes age at invitation dummies, a dummy for test type (PAP vs. HPV), and previous invitation test type-by-outcome dummies, that determine the spacing between invitations. To capture potential trends in screening behaviour by different demographic groups, we allow the coefficients δ_t to vary by round. Finally, ε_{igt} is the error term. We cluster the standard errors by individual.

We test for parallel trends by estimating the following event study specification using OLS:

$$Y_{igt} = \sum_{\tau=-2,0} \beta_{\tau} \times 1(t = \tau) \times \text{WebOrPhone}_g + \phi_t + \text{WebOrPhone}_g + X'_{igt}\delta_t + \varepsilon_{igt} \quad (2)$$

Equation (2) is analogous to Equation (1) but includes the parameter β_{-2} , that identifies the lagged placebo treatment effect given by the comparison between the treatment and control groups between the $t=-2$ and $t=-1$ pre-intervention periods. Under the parallel-trends assumption, this coefficient should be equal to zero for all outcomes.

2. Results.

First, Column (1) of Table 2 shows that the treatment had no significant impact on the probability that an appointment was handled (cancelled or moved). However, Column (2) shows that the (unconditional) probability of handling the appointment by phone decreases starkly, by roughly 18 percentage points (pp) or by 50% of the control group mean, implying that eligible women who need to move or cancel their slot do take advantage of the possibility to do so online. From a descriptive perspective, women in the “web or phone” group who prefer the portal to the phone are younger on average (36 vs 40 years old).

Second, results on the outcomes of screening appointments are reported in Columns (3) to (5) of Table 2. Access to the portal reduced screening by 16pp or roughly 30% of the control group mean. A big part of this result – around 10pp – is explained by the increased likelihood of no-shows. The remainder – 6pp – is explained by the increased likelihood of appointment cancellations.

Treatment effects on cancellations deserve further investigation. To begin with, we report treatment effects on the (unconditional) probability of cancelling a slot because the women had already undertaken a test over the last three years or because of other reasons (listed in subsection 2.3) in Columns (6) and (7) of Table 2, respectively. The higher rate of cancellations detected for the treatment groups is because of a higher likelihood of reporting tests carried out over the previous year, and not because of other reasons. Considering that women in the treatment and control groups are comparable in terms of their screening behaviour or health outcomes, this difference is likely due to changes in the probability of *reporting* a recent test, as this is explicitly mentioned as an option for cancellation in the web portal.

In addition, when they communicate recent tests, women are also asked to report the date of the test. Columns (8) and (9) of Table 2 illustrate that access to the portal increased the (unconditional) likelihood that women report this information by 4.4pp. Considering that the control group mean is 1.9pp, this is a very large effect in relative terms. A potential mechanism behind this finding could be the availability of more time to search for the latest appointment date if women cancel the appointments via the portal compared to cancelling over the phone, with an operator waiting for inputs.

Finally, Columns (10) and (11) of Table 2 report insignificant treatment effects on the unconditional likelihood of cancellations with or without a reported date handled over the phone, confirming that the positive effect detected on overall cancellations with a reported date of recent screening is due to changes undertaken via the portal.

Importantly, the identification strategy relies on the parallel-trends assumption. Figure 1 reports the estimates of the placebo tests and treatment effects estimated from Equation (2). The figure shows that – for all outcomes – the research design delivers parallel trends for the pre-intervention period, as the coefficients for the lagged treatment effect at time $t = -2$ are close to zero and insignificant.

In Table A4.1 in Online Appendix 4 we report the results of a battery of robustness checks. Specifically, our results are robust to: *i*) adjusting inference for multiple testing; *ii*) dropping all invitation-specific controls and including only invitation round and treatment group fixed effects; *iii*) introducing screening centre fixed effects instead of a single “treatment group” dummy; *iv*) dropping 365 women who changed municipality of residence across invitation rounds or who reside in three municipalities that changed screening centre of affiliation across rounds.

Lastly, in Table A4.2 in Online Appendix 4, we report the split-sample effects for women aged in 2019 below and above 40, the median age value in the sample. The overall pattern of effects is comparable across the two samples. This is not very surprising, considering that all women in the final sample are of working age, and thus familiar with the use of web services. Still, there are two notable differences. First, the effect on no-shows is predominantly due to younger women. Second, the effect on cancellation is mostly coming from senior women.

3. Discussion and limitations

We document both positive and backfiring effects of introducing a web portal that allows eligible women to modify their screening slots. On a positive note, the portal reduces the organizational burden borne by the LHU since the number of phone calls to reschedule appointments substantially drops. The portal also increases the organizational aspects of the program, since women become more likely to report previous screenings and the reasons for cancelling the appointments. This information is crucial for the correct scheduling of the invitations in the future.

On a negative note, giving access to the web portal decreases the probability of attending the screening by roughly 15pp, and around two thirds of this reduction is because of unjustified no-shows. Most likely, the portal triggers procrastination²²⁻²⁴ when rescheduling appointments, and eventually these women do not screen.

In the “phone only” condition, phone-averse women (who avoid making phone calls) would most likely prefer screening on the assigned date to rescheduling their appointment via phone. Thus, the original invitation date is salient/important for these women, given the absence of alternative dates. In the “web or phone” condition, there are fewer psychological barriers to reschedule the appointment, since phone averse women can do this online. Consequently, the original invitation date loses its salience in the minds of phone averse women, as they can easily reschedule the appointment to many alternative dates through the portal. Most likely, women keep procrastinating given the possibility to reschedule online and eventually either forget about the appointment due to limited attention²⁵ or cannot make it on the original date because of competing obligations even if reminded through a text message few days before. The positive relationship between portal availability and women’s tendency to procrastinate is confirmed by the effect of portal availability on moved appointments, that is negative and equal to -6.4pp (p-value = 0.023). Moreover, the larger treatment effect on the share of no-shows among younger women is also in line with this hypothesised mechanism, as there is evidence that procrastination is more common at young ages.^{26,27}

To help solving the problem of procrastination, the LHU may decide to send frequent and early notifications²⁸ to the invited women – on top of the SMS few days before the appointment – reminding them to reschedule the appointments. Reminders represent one of the most popular and effective

interventions to steer individuals in a certain direction²⁹ and their positive impact is well-documented in health decisions.³⁰

Our study is not exempt from limitations. First, we can only monitor screening outcomes for the current invitation round, but it might be interesting to understand whether in the long run women adapt to the presence of the portal. Similarly, we have no data on women's health outcomes. Second, while our administrative data have population coverage and minimize the potential for measurement error in treatment status and outcomes, they provide limited information on women's background. This may be useful both for additional controls in the model and for heterogeneity analysis. For example, we would have liked to investigate the heterogeneous responses to the portal introduction by employment and family status. Similarly, the availability of the time of the appointments could have allowed us to understand how different subpopulations (e.g., employed vs. not employed women) move appointments, thereby helping to finetune the scheduling of the appointments.

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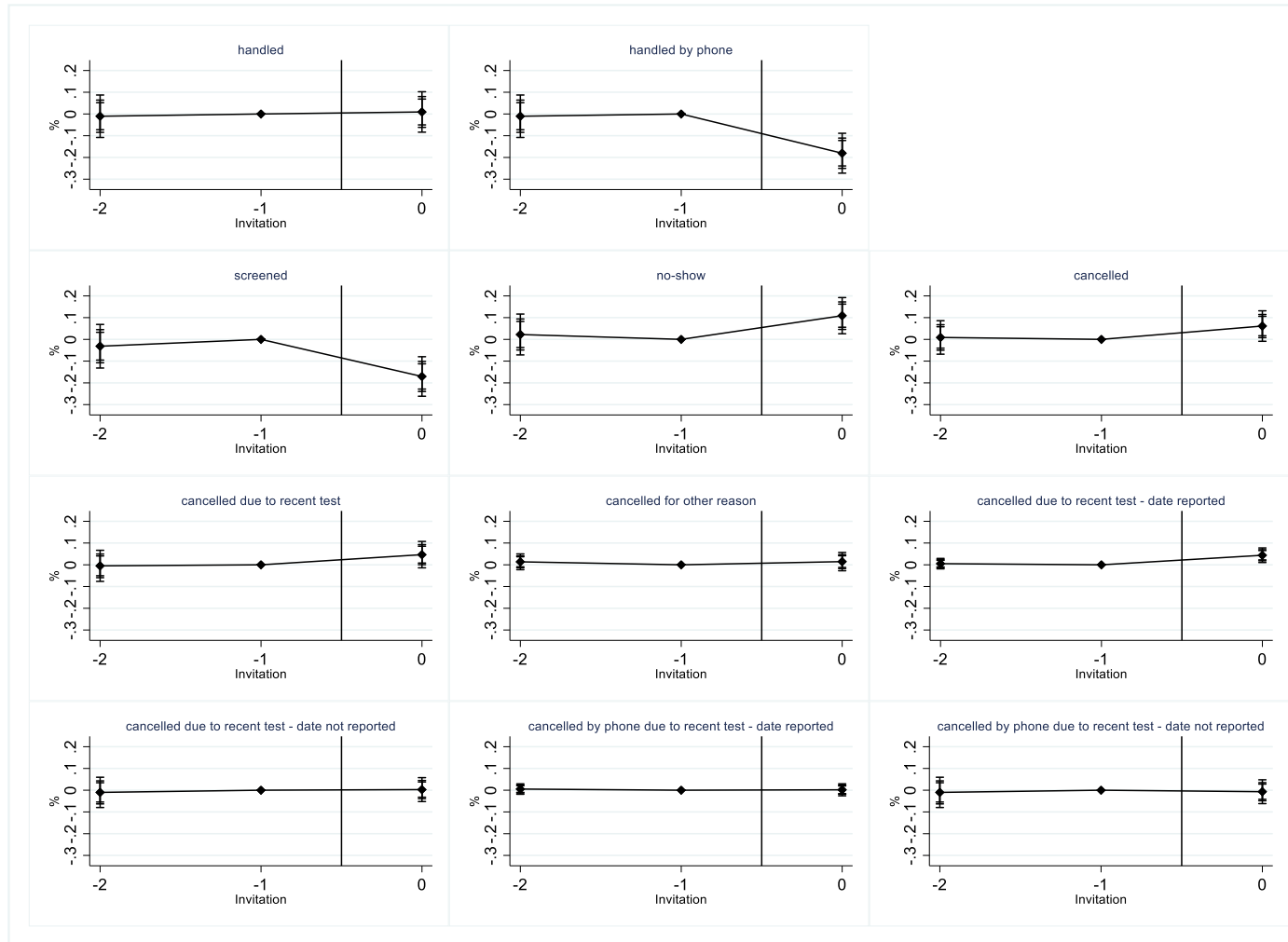
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Figures and Tables

Figure 1. The effects of the web portal access on screening outcomes



Notes: The Figure reports the effects of the “web or phone” treatment vs. the “phone only” control group, as estimated from Equation (2), together with their 90, 95 and 99% confidence intervals. The outcome considered in each panel is reported in the heading.

Table 1. Descriptive statistics

	Mean	Std. Dev.
Age	39.121	9.074
<i>Treatment group:</i>		
Phone only	0.461	0.499
Web or phone	0.539	0.499
<i>Invitation outcomes:</i>		
Appointment handled	0.397	0.489
Appointment handled by phone	0.359	0.479
Screened	0.438	0.496
No-show	0.378	0.485
Cancelled	0.184	0.387
Cancelled due to recent test	0.142	0.349
Cancelled for other reason	0.041	0.199
Cancelled due to recent test – date reported	0.022	0.147
Cancelled due to recent test – date not reported	0.120	0.325
Cancelled by phone due to recent test – date reported	0.104	0.118
Cancelled by phone due to recent test – date not reported	0.119	0.323
Pap (vs. HPV) test	0.156	0.363
<i>Previous outcome by test type:</i>		
First test recorded, no previous outcome	0.409	0.492
PAP test, attended	0.024	0.157
PAP test, not attended	0.061	0.240
HPV test, attended	0.222	0.416
HPV test, not attended	0.270	0.444
Pregnant, not attended	0.011	0.105

Notes: the sample only includes invitations for women observed in the age range 25-58. A maximum of four invitations for each woman is considered. The sample includes 4,003 invitation-women observation for 1,638 women.

Table 2. The effects of the web portal access on screening outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent Variable:	Handled	Handled by phone	Screened	No-show	Cancelled	Cancelled due to recent test	Cancelled for other reason	Cancelled due to recent test – date known	Cancelled due to recent test – date unknown	Cancelled by phone due to recent test – date known	Cancelled by phone due to recent test – date unknown
Web or phone vs. Phone only treatment effect	0.014 (0.033)	-0.176*** (0.032)	-0.156*** (0.031)	0.099*** (0.029)	0.057** (0.025)	0.049** (0.022)	0.008 (0.014)	0.042*** (0.011)	0.007 (0.020)	-0.001 (0.009)	-0.002 (0.020)
Observations	4,003	4,003	4,003	4,003	4,003	4,003	4,003	4,003	4,003	4,003	4,003
Individuals	1,638	1,638	1,638	1,638	1,638	1,638	1,638	1,638	1,638	1,638	1,638
Mean 'phone only' at $t=0$	0.379	0.379	0.503	0.331	0.166	0.111	0.0548	0.0194	0.0919	0.0194	0.0919

Notes: The Table reports the OLS estimates of parameters β in Equation (1), that identifies the effect on screening outcomes of the “web or phone” treatment vs. the “phone only” control group. Each column is for a different outcome, and outcomes are reported in columns’ headings. All regression models include invitation round fixed effects, treatment group fixed effects, age-by-invitation round dummies, test type (PAP vs. HPV)-by-round dummies and previous invitation test type-by-outcome-by-round dummies. Standard errors clustered by individual are reported in parenthesis. ***: $p < 0.01$; **: $p < 0.01$, *: $p < 0.1$.

Online Appendix

1. Additional results

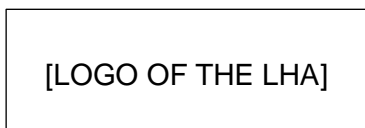
Table A1.1. The assignment of screening centres to treatment groups.

Screening centre	Affiliated municipalities	Invitations from 01/11/2018 to 31/01/2019		Invitations from 01/11/2017 to 31/01/2018	
		Invitations	Screening take-up rate	Invitations	Screening take-up rate
1. “Phone only” group					
Screening centre 1	5	1174	55.9	1289	63.8
Screening centre 2	6	584	65.1	728	52.7
Total		1758	58.96%	2017	59.79%
2. “Web or phone” group					
Screening centre 3	7	1103	66.5	1143	64.2
Screening centre 4	2	522	54.4	569	60.3
Total		1625	62.61%	1712	62.90%

2. The invitation letter.

All the identifiable information has been removed and replaced by general indications in brackets. The only difference between the invitation letter in “web or phone” and “phone only” was that, in the latter, no reference to the web portal was included. Moreover, the letter sent to women in the “phone only” group did not include the additional information on the web portal printed on the back side. The invitation letter was originally written in Italian.

Front side.



[DENOMINATION OF THE LHU]
Department of Prevention
Cytological Screening Program

[PLACE],

Dear Madam,

We invite you to participate in the national screening program for the **prevention of the cervical cancer**, organized by [DENOMINATION OF THE LHU], in adherence with the guidelines of the Region [REGION OF THE LHU].

The exam will be conducted by specialized healthcare personnel and will consist of a single sample collection for both the **HPV test** and the **PAP smear**. The latter will be considered only in case of a positive result from the HPV test. The sample collection for these tests is simple, painless and takes only few minutes. We scheduled the following appointment for you:

[DATE OF THE APPOINTMENT] [TIME OF THE APPOINTMENT] [ADDRESS OF THE APPOINTMENT]

The exam is **free of charge** and does not require any prescription from your GP. On your request, we will provide a certificate of attendance to justify your absence from work. We will send the outcome of the exam directly at home by mail.

If you have attended a Pap test or a HPV test in the last three years outside of the screening program, it is not necessary to undertake the proposed exam and we kindly ask you to cancel the appointment.

To change the appointment, to cancel it or for any other information you can access the “[NAME OF THE SCREENING WEB PORTAL]” of the Region [REGION OF THE LHU] ([WEBSITE OF THE SCREENING WEB PORTAL]) by following the indications that you find on the back of this letter and by using your Tax Code and the following password, **or by contacting the screening secretary** at the phone number [PHONE NUMBER OF THE LHU] from 10.30 to 13.30, from Monday to Friday.

Tax Code: XYZBCA65C30F123P

Password: AbC_\$1234

Remember:

- Bring this letter with you, together with your health insurance card and your ID card;
- Only undertake the examination at least three days after the end of the menstrual cycle and if you do not have blood losses;
- Do not have sexual intercourses, even protected, and avoid undertaking vaginal ultrasounds or gynecological examinations in the two days before the test;
- Do not use vaginal suppositories, creams or douches in the three days before the test.

We trust in your participation, and we send sincere greetings.

The Director of the Department of Prevention
Dr. XYZ

Registered Office [ADDRESS OF THE LHU]; [WEBSITE OF THE LHU]; [CERTIFIED EMAIL OF THE LHU]; [FISCAL CODE OF THE LHU]

Back side. Additional information on the web portal (web & phone group only)

[LOGO OF THE
REGION]

ONCOLOGICAL SCREENING PROGRAM FOR
PREVENTION OF THE CERVICAL CANCER

[LOGO OF THE LHA]

MANAGING THE APPOINTMENT ONLINE TO FREE UP YOUR OWN TIME



Dear Madam,

through the web portal dedicated to the oncological screening program of the Region [REGION OF THE LHU] “[NAME OF THE SCREENING WEB PORTAL]”, it is possible to manage your appointment to the screening test (the PAP smear or the HPV test) autonomously.

To access to the web portal, visit the following website: [WEBSITE OF THE SCREENING WEB PORTAL] (even from your smartphone) and enter your Tax Code and the personal password included in the invitation letter.

TWO MINUTES FOR YOURSELF

By using the [NAME OF THE SCREENING WEB PORTAL] you can easily confirm, modify, or cancel the appointment reserved for you. Your appointment will be managed:

- **quickly**, saving the precious time of the telephone waiting
- **freely**, being the web portal accessible at any time
- **flexibly**, giving you the possibility, if needed, to modify the appointment many times, by visualizing the slot availability on the online calendar

If you will not come to the appointment because you have already undertaken an examination privately, it is important to enter the date of the test. This will allow us **to contact you again in future** with optimal timing to offer a new opportunity to participate in the screening program.

If you **do not want to undertake the proposed screening test**, for instance because you already undertake regular tests with your doctor, it will only take few seconds **to cancel your appointment**.

Your collaboration is important!

3. Sample selection criteria

This Appendix describes our sample selection criteria

The initial sample consists of 5,642 invitations for 1,659 women aged 25-65 residing in the 20 municipalities related with the four screening centres of our partner LHU where the intervention took place.

Since we use a difference-in-differences design, to estimate treatment effects and test for parallel trends in the pre-intervention period we need at least three invitation rounds – the current one and two pre-intervention ones.²¹ The number of pre-intervention invitations observed for each woman depends on age – since the program invites only women aged 25-64 – and the timing of arrival within the boundaries of the LHU – as we only observe invitations from our partner LHU. In the final sample, we observe two or more pre-intervention invitations for roughly 75% of invitees, and 3 or more for only 50%. Hence, data for the third-to-last invitation (or previous ones) are only available for a selected share of the target population.

In addition, given that the program invites women aged between 25 and 64 and that we only know the screening history of women invited within the intervention, carried out between November 2019 and January 2020, the support on age that we observe for previous invitations is not full. We do not observe women older than 61, 58, and 53 in the first-, second-, and third-to-last invitation, respectively. This happens because these older women were not any longer eligible for screening in 2019 and hence are not part of the sample.

In order to minimize the loss of observations and of common support for age that would be related with selecting a longer pre-intervention period, we limit the analysis to the current invitation and up to two pre-intervention ones, and to the common age range 25-58. This leads us to retain 4,167 invitations for 1,649 women. We are also forced to drop close to 100 invitations of women in the control group invited during the intervention period whose appointments had to be cancelled and postponed because the health professional in charge of doing the test was sick on three screening sessions, as well as roughly 60 invitations for whom the outcome is missing.

The final sample is composed of 4,003 invitations for 1,638 women. In total, 1,005 women (61% of the sample) are observed 3 times, 355 (22%) are observed twice, and 278 (17%) are only observed once.

4. Robustness tests and Heterogeneity analysis

In Table A4.1 we show how the estimates in Table 2 of the manuscript change when we:

- i)* Adjust the significance of the results for the problem of multiple testing using the stepdown method proposed by Romano and Wolf, 2005 (see Panel A). The significance of all treatment effects is unaltered.
- ii)* Drop all invitation-specific controls and include only invitation round and treatment group fixed effects (see Panel B). The magnitude of the effects becomes smaller, but their signs and significance are unaltered. This result underlines the importance of including covariates and allowing for time-varying effects. Whilst we wish we had the possibility to verify that our results are robust to the inclusion of more controls – including education, income, HPV vaccination status, and other risk factors such as sexual habits and smoking – these variables are not observed in the administrative data that we use for our analysis. Nevertheless, we hasten to stress that the random assignment of groups to treatment status and the evidence on the parallel trends assumption supports the internal validity of the design.
- iii)* Introduce screening centre fixed effects instead of a single “treatment group” dummy, since assignment to treatment was at the centre level, and allow for clustering of the error term at the same level (in Panel C). Since there are only 4 screening centres, we use the wild bootstrap for inference and use the 6-point bootstrap weight distribution proposed by Webb (2014), instead of the standard Rademacher weights. The magnitude and significance of all effects is confirmed, except for the one on no-shows, whose significance shrinks marginally.
- iv)* Drop 365 women who changed municipality of residence across invitation rounds or who reside in three municipalities that changed screening centre of affiliation across rounds (in Panel D). Results are again unchanged.

Table A4.2 reports instead the heterogeneous effects by women’s age.

Table A4.1. Robustness checks on the effects of the web portal access on screening outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent Variable:	Handled	Handled by phone	Screened	No-show	Cancelled	Cancelled due to recent test	Cancelled for other reason	Cancelled due to recent test – date known	Cancelled due to recent test – date unknown	Cancelled by phone due to recent test – date known	Cancelled by phone due to recent test – date unknown
<u>Panel A. Romano and Wolf, 2005, stepwise resampled p-values robust to multiple hypothesis testing and clustering by individual are reported in brackets.</u>											
Web or phone vs. Phone only treatment effect	0.014 [0.929]	-0.176*** [0.001]	-0.156*** [0.001]	0.099*** [0.001]	0.057** [0.010]	0.049** [0.010]	0.008 [0.895]	0.042*** [0.001]	0.007 [0.929]	-0.001 [0.985]	-0.002 [0.985]
<u>Panel B. Without invitation-specific controls</u>											
Web or phone vs. Phone only treatment effect	0.025 (0.031)	-0.162*** (0.030)	-0.090*** (0.027)	0.045* (0.026)	0.045* (0.024)	0.032 (0.020)	0.012 (0.014)	0.034*** (0.011)	-0.002 (0.019)	-0.004 (0.008)	-0.010 (0.019)
<u>Panel C. With screening centre fixed effects instead of a treatment group dummy. Wild bootstrap p-values adjusting for clustering by screening centre are reported in brackets.</u>											
Web or phone vs. Phone only treatment effect	0.012 [0.678]	-0.178*** [0.028]	-0.158* [0.058]	0.102 [0.183]	0.057* [0.079]	0.049** [0.044]	0.008 [0.587]	0.041* [0.090]	0.008 [0.471]	-0.001 [0.869]	-0.001 [0.897]
<u>Panel D. Dropping women who change residence across invitation rounds or who reside in three municipalities that screening centre of affiliation across rounds</u>											
Web or phone vs. Phone only treatment effect	0.013 (0.037)	-0.181*** (0.036)	-0.161*** (0.034)	0.083*** (0.031)	0.077*** (0.029)	0.061** (0.026)	0.016 (0.017)	0.045*** (0.013)	0.016 (0.023)	0.000 (0.010)	0.005 (0.023)
Mean 'phone only' at $t=0$	0.379	0.379	0.503	0.331	0.166	0.111	0.0548	0.0194	0.0919	0.0194	0.0919

Notes: The Table reports the OLS estimates of parameters β in Equation (1), that identifies the effect on screening outcomes of the “web or phone” treatment vs. the “phone only” control group. Each column is for a different outcome, and outcomes are reported in columns’ headings. Observations: 4,003 (3,096 in Panel D). Individuals: 1,638 (1,273 in Panel D). Screening centres: 4. In Panel A, all regression models include invitation round fixed effects, treatment group fixed effects, age-by-invitation round dummies, test type (PAP vs. HPV)-by-round dummies and previous invitation test type-by-outcome-by-round dummies. In Panel B we only include invitation round fixed effects, treatment group fixed effects. In Panel C we include screening centre fixed effects instead of a treatment group dummy. Standard errors are clustered by individual in Panels A and B and by screening centre in Panel C. We used 1000 replications for the Romano and Wolf, 2005, stepwise resampling method in Panel A as well as for the wild bootstrap in Panel C. In Panel C the number of clusters is below 11. As a result, we use the 6-point bootstrap weight distribution proposed by Webb, 2014, instead of the standard Rademacher weights. ***: $p < 0.01$; **: $p < 0.01$, *: $p < 0.1$.

Table A4.2. The effects of the web portal access on screening outcomes by age in 2019 above or below median (40 years)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent Variable:	Handled	Handled by phone	Screened	No-show	Cancelled	Cancelled due to recent test	Cancelled for other reason	Cancelled due to recent test – date known	Cancelled due to recent test – date unknown	Cancelled by phone due to recent test – date known	Cancelled by phone due to recent test – date unknown
<u>Panel A. Junior women, age<40.</u>											
Web or phone vs. Phone only treatment effect	-0.021 (0.049)	-0.261*** (0.047)	-0.203*** (0.046)	0.160*** (0.044)	0.043 (0.036)	0.039 (0.030)	0.004 (0.025)	0.049*** (0.017)	-0.010 (0.026)	-0.001 (0.013)	-0.021 (0.026)
Observations	1,708	1,708	1,708	1,708	1,708	1,708	1,708	1,708	1,708	1,708	1,708
Individuals	776	776	776	776	776	776	776	776	776	776	776
Mean 'phone only' at t=0	0.398	0.398	0.478	0.334	0.188	0.102	0.0860	0.0223	0.0796	0.0223	0.0796
<u>Panel B. Senior women, age>40.</u>											
Web or phone vs. Phone only treatment effect	0.035 (0.044)	-0.106** (0.043)	-0.122*** (0.041)	0.050 (0.037)	0.072** (0.034)	0.054* (0.031)	0.018 (0.016)	0.035** (0.015)	0.019 (0.029)	0.001 (0.012)	0.011 (0.028)
Observations	2,295	2,295	2,295	2,295	2,295	2,295	2,295	2,295	2,295	2,295	2,295
Individuals	862	862	862	862	862	862	862	862	862	862	862
Mean 'phone only' at t=0	0.359	0.359	0.529	0.327	0.144	0.121	0.0229	0.0163	0.105	0.0163	0.105

Notes: see Table 2 in the main text.