Optimizing Cervical Cancer Screening: Investigating the Implementation and Impact of Home-Based HPV Self-Sampling in the United States

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Abstract

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Cervical cancer (CC) screening and human papillomavirus (HPV) testing programs have been successful in reducing CC incidence and mortality in the United States. However, screening adherence has declined over the past 20 years, in part due to knowledge gaps, stigma, and inaccessibility driven by structural determinants of health. This has established the need for alternative screening strategies, such as home-based HPV self-sampling (HPV-SS). HPV-SS is nascent in the US but gaining traction, having received recent approvals from the Food and Drug Administration. Additionally, policies like the June 2022, *Dobbs v. Jackson Women's Health Organization* (*Dobbs*) Supreme Court decision targeted reproductive care access among those eligible for screening and may be spilling over and influencing CC screening uptake in ways not yet evident. As HPV-SS and a shifting policy environment shape screening in the US, this dissertation aims to understand this evolving landscape and the broader implications through three distinct but related Chapters.

Chapter 1 used data from STEP (Self-Testing options in the Era for Primary HPV screening for CC), a pragmatic, randomized trial within Kaiser Permanente Washington of ~31,000 participants to assess the effectiveness of mailed, home-based HPV-SS for increasing CC screening uptake compared to usual care and educational resources

across a range of screening histories (adherent, overdue, unknown). We used binomial and multinomial regression models with generalized estimating equations to investigate if the effectiveness of offering home-based HPV-SS differed by residential neighborhood socioeconomic status (nSES), operationalized as Social Vulnerability Index (SVI) and affluence. This work involved the deterministic linkage of electronic health records with publicly available census tract nSES data. We demonstrated that mailing home-based HPV-SS kits increased screening completion overall, but effects were lower among those living in low nSES tracts. Among overdue participants, who are of highest priority, the directly mailed HPV-SS intervention increased screening completion by 11.9% (95%CI: 6.2, 17.7) for those in low vs. 17.9% (95%CI: 15.0, 20.8) in high nSES SVI areas. Individuals in low nSES areas were less likely to choose self-sampling over in-clinic screening.

Chapter 2 also used data from STEP, but restricted to those with a history of screening adherence (~13,000). In this chapter, we used modified Poisson regression and propensity score stratification for confounding to investigate if offering and choosing to use HPV-SS was associated with subsequent flu vaccine uptake, an important and annually recommended preventive service. There were no differences in flu vaccine uptake comparing those randomized to receive mailed HPV-SS kits compared to usual care screening procedures or educational materials (relative risk (RR)=1.01; 95%CI: 0.96, 1.07), and no differences in vaccine uptake comparing those who utilized HPV-SS compared to those who in-clinic screened (RR=0.99; 95%CI: 0.88, 1.10). However, those who did not complete any screening were less likely to receive a flu vaccine

compared to those who did (self-sample or in-clinic), and were characteristically different regarding past healthcare behaviors.

Data for Chapter 3 came from the Behavioral Risk Factor Surveillance System (BRFSS), a national telephone survey of US residents. With data from ~41,000 respondents, we employed a difference-in-differences (DID) design to investigate if abortion bans enabled by the *Dobbs* decision were associated with changing trends in CC screening in states that did vs. did not restrict abortion, as well as a difference-in-difference-in-differences (DDD) design to investigate if this relationship was stronger in populations with historically less access to screening. DID and DDD were fit both unadjusted and adjusted as linear regression models, directly estimating prevalence differences, with state and year fixed effects. All states in our analysis experienced an increase in CC screening from 2022 to 2024. However, while nonsignificant, there was preliminary evidence that *Dobbs*-enabled abortion bans influenced CC screening adherence prevalence over this period (adjusted DID= -2.20%; 95%CI: -7.02, 2.62), and that *Dobbs* may be influencing some populations' screening more than others (adjusted DDD for public vs. private insurance= -4.8%; 95%CI: -16.0, 6.3).

This dissertation demonstrated that HPV-SS may be less effective for improving screening rates in low nSES areas, which has implications for existing CC screening disparities. However, HPV-SS is also unlikely to influence flu vaccination, providing evidence that this novel screening tool may not have unintended consequences on uptake of other care. Additionally, *Dobbs* could be contributing to the already challenging screening situation in the US, and may reveal populations in greatest need of targeted interventions, such as HPV-SS. This dissertation provides valuable context

for healthcare officials and stakeholders looking to make evidence-based decisions around the implementation and utility of HPV-SS in the US, with implications for increasing CC screening uptake, addressing disparities, and improving care delivery, considering individual, community, and policy-level factors.

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Conceptual Framework

This dissertation covers a series of research questions all related to CC screening, how it is accessed, and how a range of factors impact access across populations. Our ability to access and use CC screening, particularly in the United States, is influenced by many factors that exist at many levels. As posited and acknowledged by the social ecological model (SEM), individuals, and the health choices we make, are not made in isolation. The SEM recognizes that individuals are "embedded within larger social systems,"[1] that all health behaviors exist within multilevel, integrated layers of influence, and that no single level or layer of influence can sufficiently explain an individual's health or healthcare behaviors.[1–5]

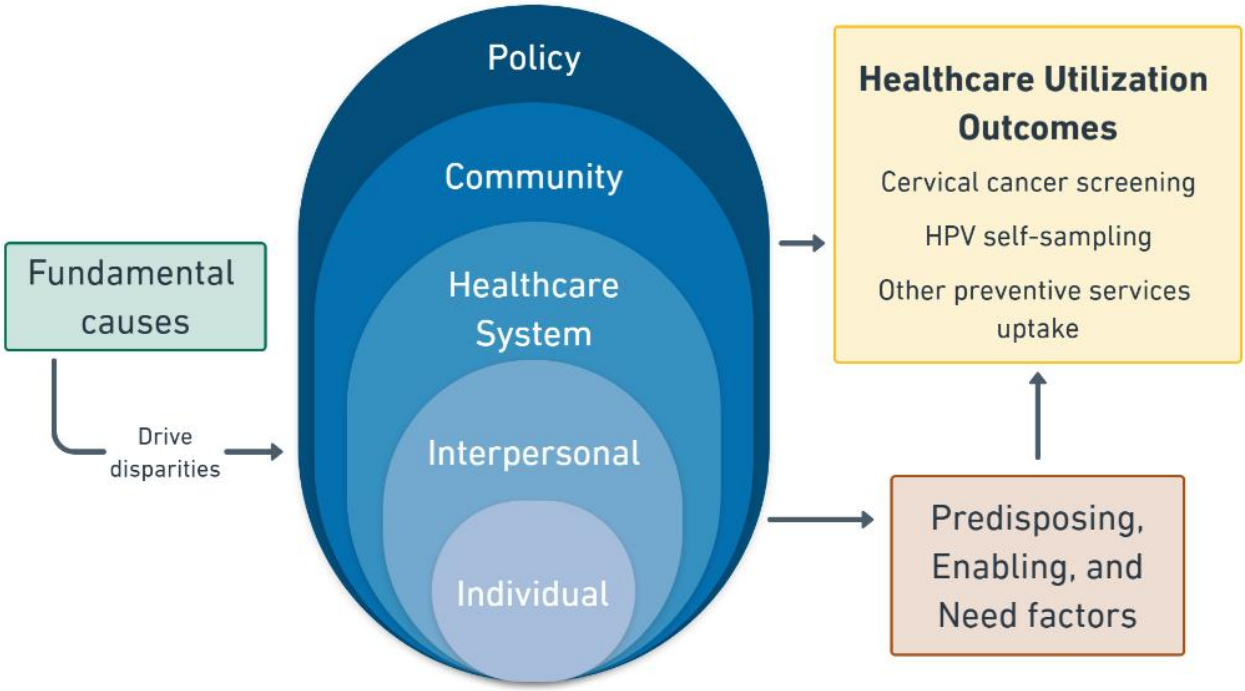
Below, in Figure 0.1, I have created a conceptual framework for my own dissertation that merges three established conceptual frameworks: Fundamental Cause Theory (green),[6,7] SEM of health (blue),[2,3] and Andersen's Behavioral Model of Health Services Use (orange).[8,9] Fundamental causes (upstream social conditions such as racism and socioeconomic status) drive disparities that exist and operate on all levels of the SEM: policy, community, healthcare, interpersonal, and individual. These factors shape healthcare utilization, such as cancer screening, by influencing one's predisposition to seek care, the available resources enabling utilization of care, and perceived need for care. These, along with all SEM factors, affect the uptake of outcomes used in this dissertation (yellow). This framework informed key variables of interest throughout this dissertation, and also aided in the interpretation and framing of my results.

The present dissertation, taken together with this proposed conceptual framework, makes it clear that no single study of HPV self-sampling (HPV-SS) or CC screening access is sufficient to capture and understand the evolving landscape of CC screening in the United States. In Chapter 1, we investigate how the effectiveness of HPV-SS for increasing screening uptake may be modified by *community-level factors*, specifically nSES, and if these factors may influence a population's likelihood of utilizing this novel CC screening modality. In Chapter 2, we investigate how this novel home-based screening modality introduced at the *healthcare system-level* might influence the uptake of other, established, and essential preventive services, specifically flu vaccination, and how those who used HPV-SS might differ in their *individual-level* characteristics compared to others. In Chapter 3, we are investigating how a *policy-level factor,* specifically the *Dobbs* decision which allowed abortion policies to vary at the state-level, may be influencing CC screening trends in states that did vs. did not ban abortion, and how *Dobbs'* influence on these trends may even differ by *community- and individual-level factors*.

Notably, none of these chapters explicitly investigate or analytically incorporate factors across all levels of the SEM. For example, in Chapter 1, we were not able to investigate if *individual-level* SES modifies the effectiveness of HPV-SS for screening uptake in the same ways as nSES, or in Chapter 3, we did not have information on how specific *healthcare systems* within abortion banned states might be working to improve CC screening rates in the post-*Dobbs* era. This reminds us that every individual analysis is necessarily partial, and that returning to our conceptual framework for interpreting and contextualizing our findings in the broader systems that exist is essential. If we are to

improve CC screening access and CC outcomes more broadly in an equitable capacity in the United States, multidimensional and intersectional approaches and frameworks must drive this work. No singular intervention, however well-designed, can overcome the systems that drive these disparities.

Figure 0.1. Dissertation conceptual framework



Chapter 1: Neighborhood socioeconomic status and mailed human papillomavirus (HPV) self-sampling kit effectiveness across an integrated healthcare system in the US

Abstract

Background: Mailed HPV self-sampling increases cervical cancer screening completion, but its impact across neighborhood socioeconomic status (nSES) remains unknown.

Methods: We used data from STEP, a pragmatic trial within Kaiser Permanente Washington, including participants with varied screening histories (adherent [N=13,025]; overdue [N=8,282]; unknown [N=9,929]). Participants were randomized to usual care, education, direct-mail self-sampling, or opt-in self-sampling, with usual care and education combined as the reference exposure. We linked participant data with census tract-level nSES measures (Social Vulnerability Index [SVI] and neighborhood affluence), each categorized as high (top 75%) or low (bottom 25%). Outcomes were screening completion and modality (in-clinic/self-sampling/none). We fit binomial models estimating risk differences by nSES and multinomial models investigating chosen modality, incorporating generalized estimating equations for clustering.

Results: Across screening histories and nSES measures, low nSES prevalence ranged from 13.3-24.0%. Direct-mail significantly increased screening completion, but effects were lower in low nSES areas. Among overdue participants, direct-mail increased screening completion by 11.9% (95%CI=6.2-17.7) for those in low vs. 17.9% (95%CI=15.0-20.8) in high nSES SVI areas; a smaller difference between levels was seen for affluence. Results were similar among adherent participants: direct-mail increased completion by 9.5% (95%CI=3.5-15.6) in low vs. 15.9% (95%CI=12.9-19.0) in high nSES SVI areas. Individuals in low nSES areas were less likely to choose self-sampling over in-clinic screening.

Conclusions: While mailed self-sampling kits improve screening rates, their impact may be lower in low nSES neighborhoods. As self-sampling becomes widely available, addressing these nSES-based differences in preference and uptake is important to prevent widening screening disparities.

Introduction

Cervical cancer (CC) outcomes have improved substantially over the past 50 years in the US, largely attributable to CC screening.[10–12] Unfortunately, adherence to guideline-recommended screening has dropped and declines in CC incidence have halted.[13–17] Screening nonadherence has increased from 14% in 2005 to 23% in 2019,[14] and further to 27% in 2021.[18] Screening barriers include lack of awareness, stigma, transportation, scheduling, and competing obligations.[14,19–24] The COVID-19 pandemic contributed to declines in preventive services use, including CC screening.[25–28] Novel tools that promote uptake and mitigate barriers are needed.

In 2018, the US Preventive Services Task Force (USPSTF) recommended primary human papillomavirus (HPV) screening as an option for those aged 30-65 years.[29] Prior studies show that HPV testing is more sensitive than cervical cytology (clinician-performed Papanicolaou (Pap) testing) for detecting cervical pre-cancers, with comparable effectiveness across self-collected and clinician-collected samples.[30–33] USPSTF 2018 guidelines paved the way for HPV self-sampling (HPV-SS) as an option, where patients could collect their own samples at home without a pelvic exam. Further, USPSTF, the American Cancer Society, and Health Resources and Services Administration all released recent guidance including HPV-SS as a recognized option.[34–36] HPV-SS is cost-effective, and improves screening participation across many populations.[31,37–47]

Neighborhood socioeconomic conditions contribute to well-documented CC screening disparities in the US.[3,5,48–53] Area-level social vulnerability (measured by Social Vulnerability Index [SVI]) and poverty are associated with lower screening

uptake,[48–50] and rates are lower in rural compared to urban communities.[54,55] Those living in high deprivation areas are also more likely to receive later stage CC diagnoses, with disparities in prognosis and survival.[56,57] There is some research, primarily from non-US studies, investigating if HPV-SS uptake may differ by area-level socioeconomic conditions. Work in Australia and England has demonstrated that HPV-SS uptake may be greatest and more effective for improving screening rates in more disadvantaged areas.[58–60]

Evaluating whether mailed HPV-SS effectiveness varies across neighborhood socioeconomic status (nSES) could inform if this patient-centered healthcare option[61,62] might narrow screening disparities, and would be relevant for healthcare systems considering HPV-SS to target priority populations. This is particularly important in the US as access to HPV-SS is likely to expand. We investigated this question through a secondary analysis of a large, pragmatic trial. This is the first study to empirically investigate potential nSES effect modification of HPV-SS in a US population with a range of screening histories.

Methods

Design and Population

Data came from STEP ("Self-Testing options in the Era for Primary HPV screening for CC"), a pragmatic, parallel, single-blind, randomized clinical trial embedded within Kaiser Permanente Washington (KPWA), an integrated health system in Washington State.[63] STEP was designed to understand the effectiveness of HPV-SS to increase CC screening in routine clinical practice.[64] The KPWA institutional review

board approved STEP, and allowed enrollment under a waiver of consent to reduce participation bias. Details were previously described.[63,65]

Eligible individuals were identified using electronic medical records (EMR) and administrative claims. STEP continually randomized KPWA members from November 2020 through January 2022 with CC screening histories stratified as: 1) due: previously screening compliant and due in ≤3 months, 2) overdue: completed Pap with HPV co-testing >5.25 years ago, Pap testing alone >3.25 years ago, or no screening with continuous KPWA enrollment ≥3.25 years, and 3) unknown: KPWA enrollment between ≥6 months and <3.25 years with no recorded screening.

Within each stratum, participants were randomized to receive one of three or four trial arms: 1) usual care reminders to complete in-clinic screening ('usual care'), 2) usual care + mailed, screening educational materials ('education'), 3) usual care + education + HPV-SS kit mailed directly to their home ('direct-mail'), or 4) usual care + education + insert with information on how to request a kit be mailed directly to their home ('opt-in'). Participants in direct-mail and opt-in arms received return mail boxes with prepaid postage addressed to the KPWA central laboratory. If participants had not returned their kits within 21 days, the research team followed KPWA protocols, calling up to three times over seven days and leaving up to two messages, and offering a replacement if the participant indicated that the kit was never delivered or lost.[65] Overdue participants were not randomized to opt-in as it is inferior to direct-mail for increasing screening in this population,[38] and unknown participants were not randomized to direct-mail to reduce overscreening risk (Figure 1.1).[65]

STEP inclusion criteria were female sex, 30-64 years of age, having an intact cervix, and having KPWA insurance and a KPWA primary care physician. Participants were only considered for randomization into STEP if they were eligible for routine screening and due or overdue. Individuals were not randomized if they had a recent positive CC screen, were participating in other KPWA HPV-SS studies, were pregnant, required language interpretation, as kit instructions and study materials were only available in English, or opted out of research. Additionally, for this secondary analysis, participants were excluded if they did not have a successfully geocoded home address, or if their tract was missing modifier information.

Exposure

To understand whether STEP results were modified by nSES, our exposure was randomization arm. As usual care and education arms results in the primary study were very similar,[45] we collapsed these groups to improve power. Our final exposure was randomization arm, categorized as direct-mail vs. opt-in vs. usual care and education combined (UC/EDU).

Outcomes

Our primary outcome was consistent with the main trial: binary CC screening completion within six months of randomization.[63] A positive outcome comprised in-clinic screening; HPV-SS kit return with negative results or positive for HPV-16/HPV-18; or kit return with other high-risk HPV+ or unsatisfactory results followed by in-clinic Pap testing. Those randomized to UC/EDU solely had the option to in-clinic screen (Figure 1.1). Among direct-mail and opt-in, we investigated a secondary, three-level CC

screening modality outcome: HPV-SS, in-clinic screening only, or no screening completed within six months.

Individual-level characteristics

Individual-level characteristics at randomization were obtained through the EMR: age group, body mass index (BMI), years enrolled in KPWA at randomization, duration screening overdue, tobacco use, Charlson Comorbidity Index,[66] self-reported race and ethnicity, and travel time to primary care clinic. Travel time was based on distance from participants' residential census tract centroid to primary care clinic. These were collected as they are associated with CC screening utilization.[67]

Neighborhood socioeconomic status effect modifiers

We assessed two census tract-level nSES effect modifiers using publicly available data sources: state-based SVI and neighborhood affluence. For this analysis, we used 2020 census tract boundaries as a proxy for neighborhoods. These administratively defined areas are widely used in health research to capture neighborhood-level effects and designed to be socioeconomically homogeneous.[68–70]

State-based SVI (hereafter ‘SVI’), from the Centers for Disease Control and Prevention Agency for Toxic Substances and Disease Registry, measures “potential negative effects on communities caused by external stressors on human health.”[71,72] SVI is calculated as the sum of 16 area-level factors across four domains (SES, household characteristics, racial composition, built environment) and is then transformed into a percentile and ranked. State-based solely compares tracts within a state, making it more locally relevant. Neighborhood affluence (hereinafter ‘affluence’),

from University of Michigan's National Neighborhood Data Archive, captures a construct beyond the absence of disadvantage, as it is "associated with higher levels of social control and leverage over local institutions that can foster social environments that facilitate health".[73,74] Affluence is calculated as the average of three percentages: adults with a bachelor's degree or higher, adults in professional/managerial jobs, and households earning ≥$125,000 annually (Table 1.1).

Each nSES modifier was converted into a binary variable, comparing the "lowest" quartile (bottom 25% of tracts) to the "highest" three quartiles (top 75% of tracts) in Washington. This cut-off was selected to ensure adequate statistical power, as the study population was concentrated in higher nSES areas, while still maintaining a meaningful threshold. Individual-level STEP and modifier data were deterministically linked at the 2020 census tract level to the geocoded KPWA billing address on file, assumed to represent the residential address. Participants were classified as living in low or high nSES tracts for each modifier independently, and could be classified differently across them.

Statistical Analysis

We fit a series of binomial regression models with our primary outcome regressed on arm and binary effect modifier, and an interaction term between the two, to estimate the effect of HPV-SS interventions by nSES. Models were fit with an identity link and binomial distribution to directly estimate risk differences (RDs). Separate models were fit for each effect modifier and stratified by CC screening history consistent with the main STEP analysis using an intention-to-treat approach.[63,75] Models were fit using generalized estimating equations (GEEs) with a working independence correlation

structure and robust sandwich standard errors to obtain 95% confidence intervals (95%CI) accounting for tract-level clustering. Models were unadjusted, as stratifying participants based on nSES does not impact randomization. However, for the overdue group, we adjusted for duration screening overdue (<3 years, ≥3 years, no prior screen).

We performed a series of secondary analyses. The models above were fit among those living in King County, Washington to assess whether results differed in a smaller region with greater healthcare concentration and more homogeneous environmental characteristics. King County encompasses the greater Seattle metro area, has a population of 2,269,675, median household income of $99,200, and is 54% Non-Hispanic white, 20% Asian, 11% Hispanic, and 7% Black.[76] STEP spanned the COVID-19 pandemic, during which vaccine rollout may have impacted healthcare access and willingness to seek in-person care. Participants were stratified by randomization date: on or before April 30, 2021, vs. after, representing the pre- vs. post-COVID-19 vaccine eras, respectively, with models fit separately for the two periods.

Finally, we carried out an analysis restricted to those offered HPV-SS (direct-mail and opt-in). For each modifier, we calculated screening modality outcome distribution (HPV-SS, in-clinic, none) for both modifiers and for high and low nSES. We fit unadjusted multinomial logistic regression models with three-level screening modality as the outcome (in-clinic as reference) and nSES as the predictor (high nSES as reference), using GEEs for clustering.[77] These models allowed us to estimate if those living in low nSES areas, compared to high, were more or less likely to select HPV-SS or no screening over in-clinic screening.

Analyses were performed using SAS statistical software version 9.4 (SAS Institute). Visualizations were created using R version 4.4.1 (R Foundation).

Results

Study population

Over 99.5% of original STEP participants were included (adherent [n=13,025]; overdue [n=8,282]; unknown [n=9,929]). Among the adherent, 17.9% and 13.3% were categorized as low nSES by SVI and affluence, respectively. A higher proportion of overdue (19.9% and 15.6%) and unknown (24.0% and 18.1%) participants were categorized as low nSES. Continuous nSES modifier distributions were similar across screening histories (Supplemental Figure 1.1).

Characteristics were similar between STEP arms within nSES SVI levels for all screening histories, consistent with randomization (Supplemental Tables 1.1A-1.1C). However, differences emerged when comparing characteristics between nSES SVI levels, consistent with the non-randomized nature of nSES. A greater proportion of low nSES participants identified as non-White. A smaller proportion of low nSES participants had BMI ≤24.9 kg/m², and, among the adherent and unknown, a greater proportion had ≥1 comorbidity. A greater proportion of low nSES participants lived <10 minutes from their KPWA primary care clinic. Affluence results were similar (Supplemental Tables 1.2A-1.2C). However, a greater proportion of low nSES affluence participants identified as non-Asian, and travel time differences were less pronounced, likely reflecting how modifiers are calculated.

Screening completion by population, STEP arm, and nSES level

Screening completion varied by screening history, trial arm, and nSES (Figure 1.2). Within each screening history and trial arm, completion was lower among those in low nSES areas vs. high. For example, among the adherent for SVI, 52.7% of direct-mail low nSES participants completed screening vs. 64.2% of high nSES. This held true for opt-in and UC/EDU, and across screening histories, though differences were less pronounced among the overdue and unknown.

Effect modification by nSES

Direct-mailing HPV-SS kits significantly increased screening completion in both the adherent and overdue populations and across nSES levels compared to UC/EDU (Figure 1.3). However, intervention point estimates (RDs) were non-statistically significantly lower among those with low nSES compared to high nSES. Among the adherent, direct-mail increased screening completion by 9.5% (95%CI: 3.5-15.6) for those in low and 15.9% (95%CI: 12.9-19.0) for those in high nSES SVI areas (interaction p-value=0.063). In this same population but for affluence, direct-mail increased screening completion by 10.2% (95%CI: 3.0-17.5) in low nSES and 15.4% (95%CI: 12.4-18.3) in high nSES areas (p=0.197). The same trend was seen among the overdue, with the largest difference for SVI: direct-mail increased screening completion by 11.9% (95%CI: 6.2-17.7) in low nSES and 17.9% (95%CI: 15.0-20.8) in high nSES areas (p=0.069). Similar results were seen in secondary analyses, with differences that were stronger and statistically significant in some cases (Supplemental Figure 1.2). When restricted to King County, direct-mail was less than half as effective for those in low nSES areas vs. high among the overdue. For SVI, direct-mail increased screening

completion by 8.7% (95%CI: 1.4-16.0) for those in low vs. 20.4% (95%CI: 15.6-25.2) in high nSES areas (p=0.008).

Contrasting direct-mail, opt-in intervention RDs were closer to the null across nSES levels, consistent with primary STEP results (Figure 1.3).[45] Minimal differences for opt-in were seen among the adherent when comparing nSES levels. However, among the unknown, the opt-in intervention was less effective overall for those in low nSES areas. In fact, low nSES RDs were below zero for SVI and affluence, indicating that the opt-in intervention was less effective than UC/EDU, though both 95%CIs crossed the null. Among the unknown for SVI, opt-in decreased completion by -1.9% (95%CI: -5.0-1.1) for low nSES but increased completion by 2.5% (95%CI: 0.7-4.3) for high nSES areas (p=0.014). Similar results were seen for affluence (RD=-1.3, 95%CI: -4.8-2.2 for low nSES vs. RD=2.1%, 95%CI: 0.4-3.8 for high nSES; p=0.091). Secondary analyses were consistent (Supplemental Figure 1.2).

Distribution of screening modality and multinomial regression

Overall, the distribution of CC screening modality chosen (HPV-SS, in-clinic, no screening) among participants given the opportunity to self-sample varied by nSES (Figure 1.4). Compared to high nSES areas, participants in low nSES areas were less likely to utilize HPV-SS vs. in-clinic screening across modifiers and screening histories. Simultaneously, participants in low nSES areas were more likely to not complete any screening vs. in-clinic screening. For example, among the adherent direct-mail for SVI, 32.6%, 20.1%, and 47.3% of low nSES participants self-sampled, in-clinic screened, and did not screen, respectively, compared to 42.3%, 21.9% and 35.8% for high nSES. Multinomial regression models demonstrated that these patterns held across

populations and both modifiers, but that the majority of these associations (odds ratios) were not statistically significant (Figure 1.5).

Discussion

Mailed, home-based HPV-SS increases CC screening completion, but its effectiveness across subpopulations is less understood. In this secondary analysis of a pragmatic trial, we observed some evidence suggesting that mailed, home-based HPV-SS interventions for increasing screening completion may be less effective among those living in low nSES areas. Additionally, CC screening modality choice varied, with those in low nSES areas less likely to utilize self-sampling compared to in-clinic screening. These results could inform healthcare systems' implementation planning and make HPV-SS access and uptake as equitable as possible, particularly if deploying an HPV-SS outreach approach to priority populations.

There is a vast literature documenting CC outcome disparities by nSES in the US.[48–56] In a national 2018 analysis, rates of USPSTF-recommended screening were significantly worse in counties in the lowest two SVI quintiles compared to the highest.[48] HPV vaccination-series rates among 11-18 year-olds are lowest in areas with greatest deprivation, and late-stage CC diagnosis and mortality risk are higher for those in low nSES areas.[51,52,56] While HPV-SS may address a number of barriers that exist for in-clinic, provider-collected screening, present results indicate that the underlying systems driving persistent screening disparities may be perpetuated even with a novel tool like HPV-SS.[7] And while prior work has established that HPV-SS is feasible and acceptable in under-screened populations and those who face barriers to screening, this does not necessarily mean that HPV-SS will make access equitable if HPV-SS is more effective

for increasing screening in populations with greater privilege.[78–82] As HPV-SS becomes increasingly available in the US, monitoring will be crucial to ensure disparities are not exacerbated.

Few other studies have directly investigated nSES and self-collected cancer screening strategies. These studies drew from populations different from STEP, and most were in countries with healthcare contexts that may be nongeneralizable to the US. An Australian direct-mail HPV-SS trial among never- and under-screened individuals found no evidence of nSES modification .[83] However, once HPV-SS was universally accessible, a population-level analyses in Victoria and all of Australia showed that uptake was higher in the most disadvantaged areas.[59,60] In Northern England, a pragmatic trial of non-responders found that HPV-SS effectiveness was greatest among those living in high deprivation areas, though screening completion was low (13% and 6% in HPV-SS and usual care groups, respectively) and a large proportion lived in highly deprived areas.[58] Looking to the colorectal cancer literature, a randomized trial within a San Francisco safety-net health system found that mailing a fecal immunochemical test kits increased screening rates similarly across nSES.[82]

Neighborhoods shape health outcomes through physical, social, and environmental factors, and their socioeconomic conditions can be measured in many ways.[84,85] In the present analysis, we used two composite variables representing nSES. While composite variables may complicate identification of which components drive population-level disparities, they are beneficial for capturing a broader underlying construct.[86] Our findings suggest a geographic paradox: low-nSES participants often lived closer to KPWA clinics yet had lower screening completion, indicating that physical

proximity to healthcare may not negate the barriers captured by SVI even with a home-based option. Future studies exploring area-level variation might consider narrowing in on specific measures, such as transit accessibility and healthcare facility concentration, and qualitatively exploring why non-screeners who live close to facilities are not drawn to HPV-SS. Additionally, if individuals in high-vulnerability neighborhoods face barriers to kit return, they likely face even steeper barriers to follow-up after a positive result. Interventions must ensure that HPV-SS programs are paired with robust patient navigation and coverage for follow-up to prevent widening downstream disparities.[87,88]

Our analysis suggested the possibility of area-level effect modification. However, HPV-SS interventions were administered at the individual-level, inviting participants to use self-collection to complete screening, with no comprehensive community-level outreach.[45] Cancer screening, and other health behaviors, are influenced by factors existing at multiple levels, collectively impacting wellbeing.[89] Factors such as housing instability or unreliable mail service in low nSES neighborhoods, as well as individual-level barriers such as health literacy and cultural beliefs, may have contributed to lower direct-mail effectiveness. This is why multi-level health interventions are often highly effective, and potentially why we observed evidence of nSES differences. Further, STEP only provided materials in English due to resource constraints. This may have influenced the generalizability of results and played a role in the differential uptake of screening and HPV-SS by nSES. However, in a real-world HPV-SS implementation at KPWA in 2023, kit materials were included in Spanish and 13 additional languages through a QR code, meaning future work could investigate how language accessibility affects uptake.[90] Additionally, HPV-SS in the US was novel during this period (2020-

2022). As HPV-SS access and knowledge become more ubiquitous, uptake patterns may shift across communities based on familiarity and context.

Our study had a number of strengths. STEP's pragmatic design and use of EMR from an integrated healthcare system, which allowed for linkage and highly complete data, likely increased generalizability.[45,64] The fact that KPWA members receive the majority of their care within KPWA enabled comprehensive capture of screening outcomes and minimized loss to follow-up. Unlike most HPV-SS trials focused on under-screened populations, our study is strengthened through enrollment of participants with diverse screening histories. Using multiple nSES measures allowed us to more comprehensively examine this relationship.

Our study also had limitations. Census tracts are administratively defined and may not capture one's neighborhood, either conceptually or as is most relevant for influencing HPV-SS uptake.[69,91] Our nSES binarization may have also influenced results. We did not have access to individual-level SES information, which could have allowed for a more nuanced investigation, as neighborhood environments do not affect everyone equally.[92] While Kaiser populations have been shown to be generally similar to the general population regarding demographic and health characteristics, STEP being embedded within KPWA might limit generalizability, as Kaiser's integrated nature may provide members with more reliable access to preventive care.[93] Additionally, all participants in this analysis were insured through Kaiser, which may further limit generalizability to uninsured population.

Conclusion

While directly mailing HPV-SS kits is effective for increasing overall screening completion in an integrated healthcare system, the effect may be lower in low nSES areas, and the preference for HPV-SS over provider-collected, in-clinic screening may also differ by nSES. Understanding if HPV-SS may contribute to, maintain, or lessen CC screening disparities will become increasingly relevant as this option is made widely available across the US. We encourage others to investigate nSES subgroup differences, and explore how HPV-SS interventions can be designed and implemented to address disparities.

Tables and Figures

Figure 1.1. STEP trial randomization and outcomes flowchart

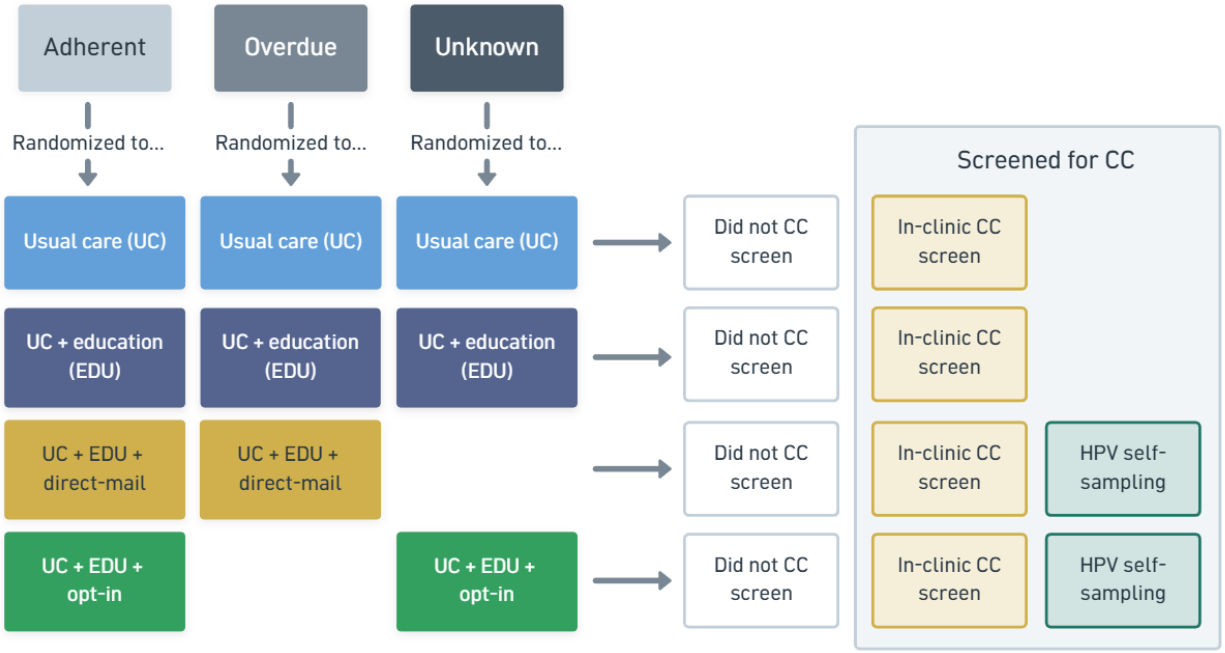


Table 1.1. Effect modifiers definitions and sources

Effect modifier	Definition	Source	Direction associated with lower area-level socioeconomic status
State-based Social Vulnerability Index (SVI)[71,72]	Sum of 16 scores related to area-level socioeconomic status, household characteristics, racial composition, and built environment, ranked by percentile within Washington state	2020 CDC/ATSDR	High
Neighborhood affluence[73,74]	Average of 1) percent of adults with at least a bachelor's degree, 2) percent of population employed in professional or managerial occupations, and 3) percent with annual income ≥$125,000	2020 NaNDA	Low

Abbreviations: CDC/ATSDR, Centers for Disease Control and Prevention Agency for Toxic Substances and Disease Registry; NaNDA, National Neighborhood Data Archive

Figure 1.2. Heatmap of cervical cancer screening completion by screening history population, STEP trial arm, and effect modifier stratum

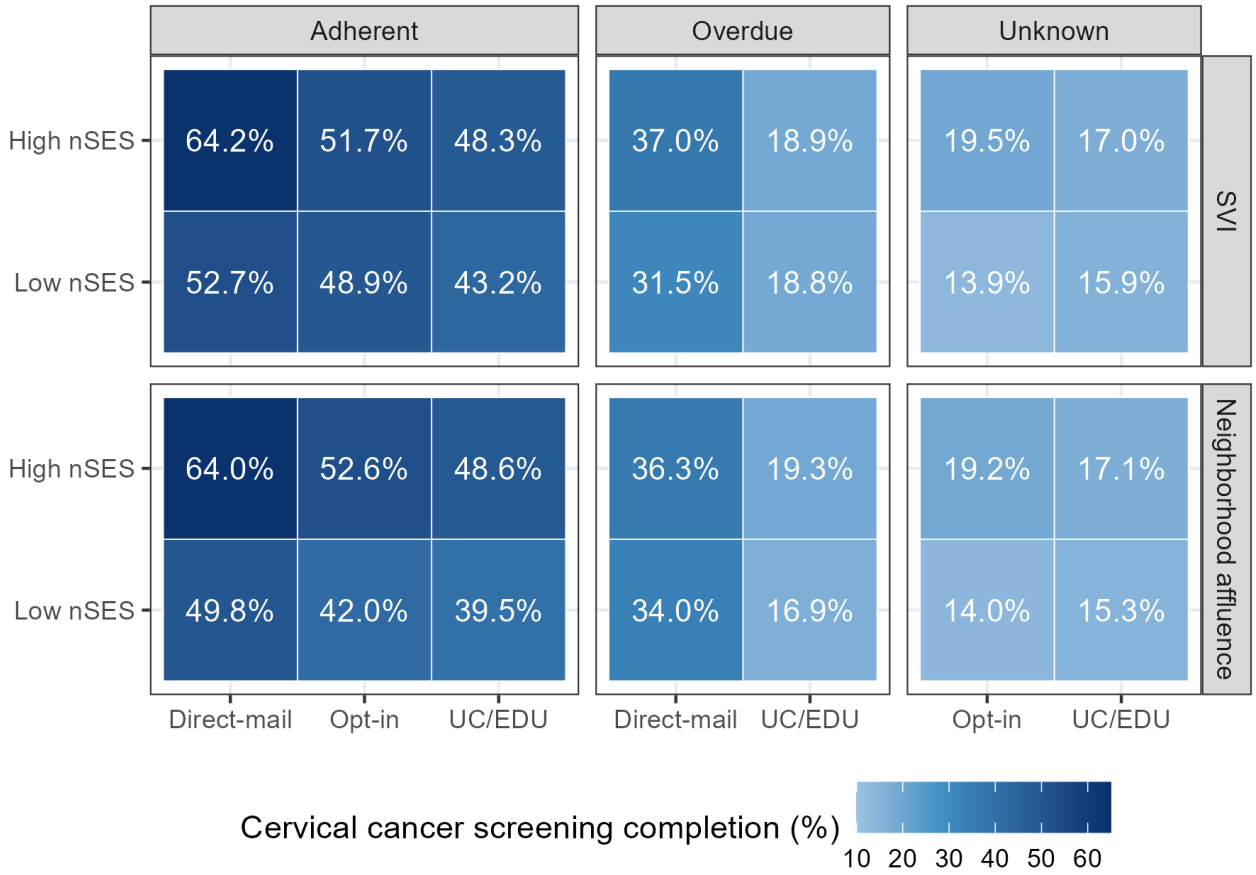


Figure 1.3. Effectiveness of direct-mail and opt-in intervention by screening history population, STEP trial arm, and effect modifier stratum. Interaction p-values presented for each screening history population, STEP trial arm, and modifier

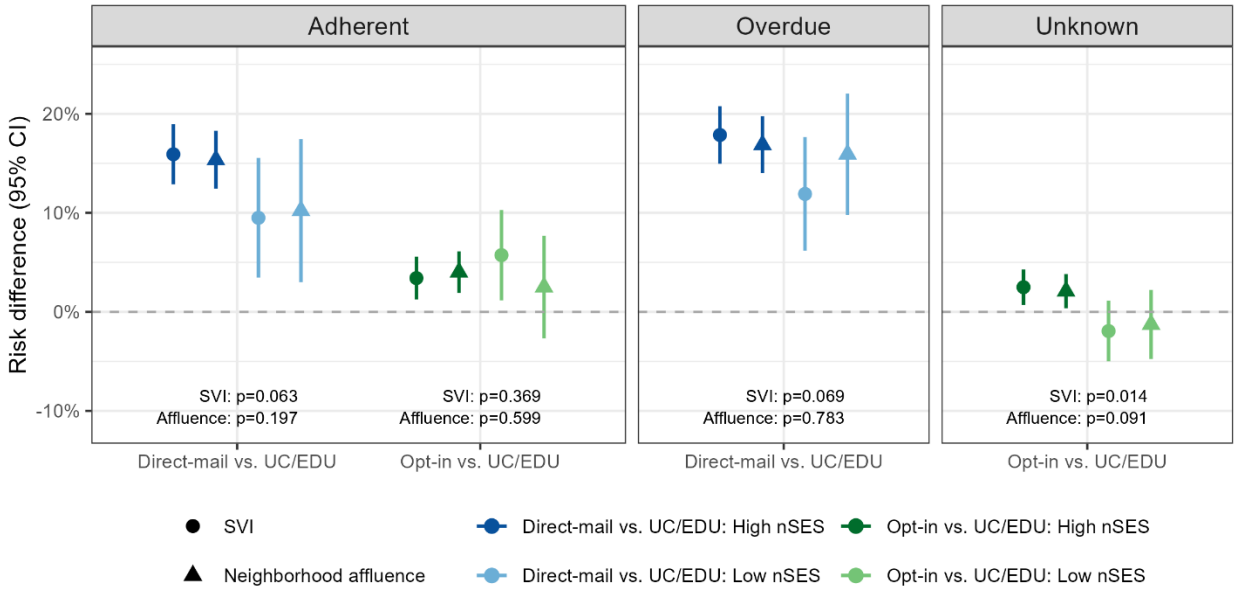


Figure 1.4. Distribution of cervical cancer screening modality selected among those able to utilize HPV self-sampling by effect modifier stratum

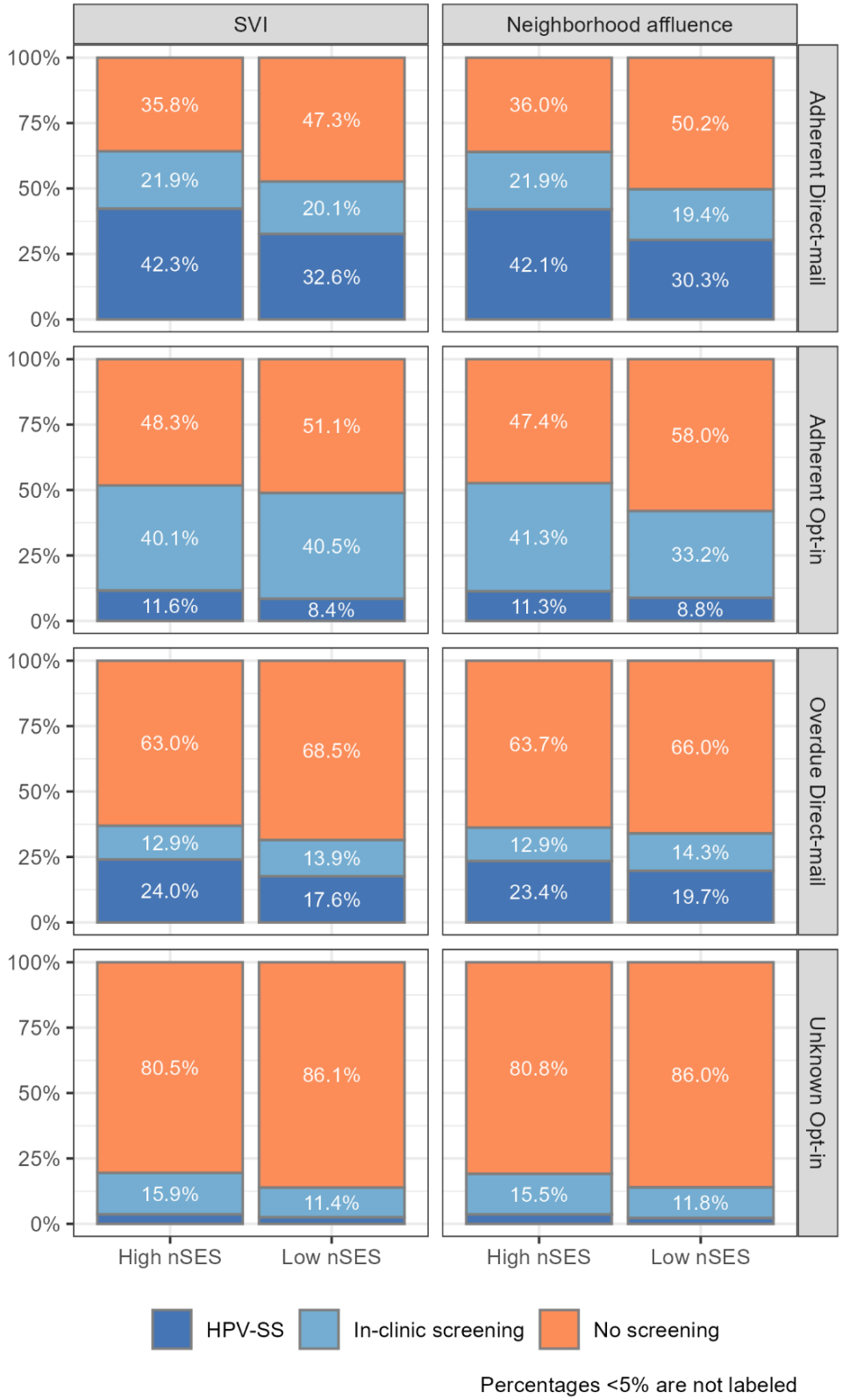
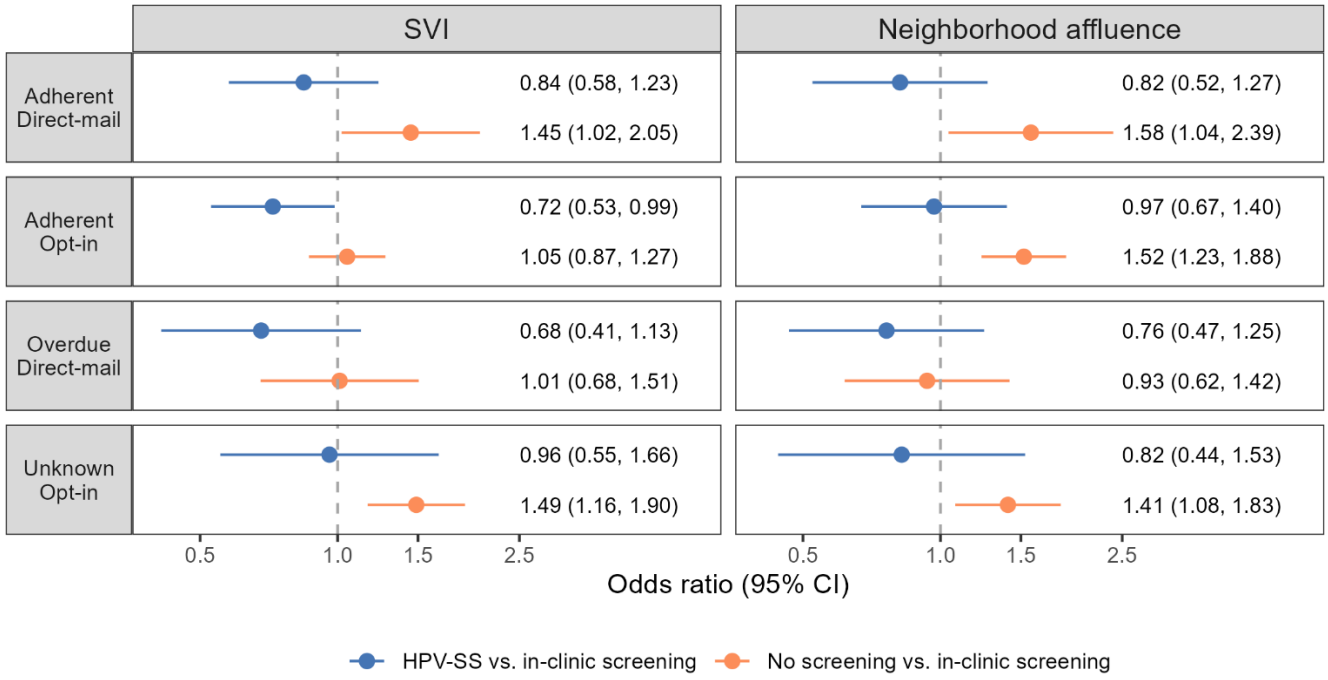


Figure 1.5. Multinomial regression model results comparing odds of HPV self-sampling and no screening completion to in-clinic screening for those in low vs. high nSES areas



Chapter 2: Impact of Home-Based Cervical Cancer Screening on Other Preventive Health Service Utilization: An Analysis of Flu Vaccine Uptake

Abstract

Introduction: Home-based HPV self-sampling (HPV-SS) is an emerging cervical cancer screening option in the US. Moving screening out of a healthcare setting may inadvertently reduce uptake of other in-person preventive services. This study evaluated if mailed, home-based HPV-SS influenced flu vaccine uptake among individuals with a history of cervical cancer screening adherence.

Methods: This was a secondary analysis of a pragmatic trial embedded in a US healthcare system. From 2020-2022, participants (N=12,924) were randomized to receive direct-mail self-sampling, opt-in self-sampling, usual care, or education (usual care and education (UC/EDU) combined as the referent group for analysis). Flu vaccination 1-year post-randomization was compared between arms, to understand if offering HPV-SS influenced vaccine uptake, then compared by screening modality utilized (self-sampled vs. in-clinic vs. no screening) within the direct-mail and opt-in arms. Modified Poisson models estimated risk ratios (RRs) and 95%CIs. Propensity score stratification was implemented to control for confounding.

Results: There were no differences in vaccine uptake comparing direct-mail (RR=1.01; 95%CI=0.96-1.07) or opt-in (RR=0.99; 95%CI=0.96-1.03) to UC/EDU. In the direct-mail within-arm analysis, there were no differences comparing those who self-sampled vs. in-clinic screened (RR=0.99; 95%CI=0.88-1.10). However, those who self-sampled (RR=1.29; 95%CI=1.13-1.48) or in-clinic screened (RR=1.44; 95%CI=1.23-1.67) vs. no screening were more likely to vaccinate. Opt-in results were consistent.

Conclusions: Among those with a history of screening adherence, offering home-based HPV-SS did not impact flu vaccination rates, and those who self-sampled were as likely

to vaccinate as in-clinic screeners. As HPV-SS availability expands, it is important to understand the impacts of home-based screening on uptake of other clinical screening and preventive services.

Introduction

Advancements in cervical cancer (CC) screening have reduced CC morbidity and mortality in the United States.[10–12] However, screening rates have been declining, pointing us towards novel strategies to encourage screening uptake, such as mailed, home-based human papillomavirus self-sampling (HPV-SS).[13,14,16] HPV-SS, which allows patients to screen from home with a vaginal swab, is as accurate as clinician-collected screening, effective for increasing screening uptake, and overcomes several barriers that exist for traditional, in-clinic screening.[30,38,46,63,94]

However, there is concern that shifting CC screening out of a healthcare setting could lead to missed connections with the healthcare system.[95] In-person interactions with providers give patients space to discuss other concerns, which may lead to unplanned conversations on the need for other preventive services. Alternatively, the opportunity to engage with non-clinic-based screening may provide an opening to connect with the healthcare system in a new way and motivate patients to complete other preventive services.

Home-based HPV-SS increases screening uptake in under- and never-screened populations who are at highest risk for CC, as well as among those with a history of screening adherence.[63,96–98] HPV-SS has been implemented in multiple non-US countries but is nascent in the US, with limited adoption in select health systems, such as Kaiser Permanente Washington (KPWA).[90,99] With recent US Food and Drug Administration (FDA) approvals of HPV-SS for use in healthcare settings (2024) and home-based HPV-SS (2025),[100,101] the availability of this patient-centered screening modality will likely expand. As healthcare systems broaden access across patient

populations, including to those already engaged in screening, it is important to consider whether doing so might influence uptake of other routinely recommended services, such as flu vaccines.

The present study explored if home-based, mailed HPV-SS influences subsequent flu vaccine uptake among individuals who were previously CC screening adherent using data from a large, pragmatic trial in a US integrated healthcare delivery system. This question has been explored among those overdue for screening using data from a pragmatic trial in the same health system.[46,102] However, these two populations have different healthcare utilization behaviors, and impacts may vary by previous screening engagement. Flu vaccination is recommended annually across all screening eligible populations regardless of age, health status, or healthcare history, and it protects against several severe outcomes.[103–105] Additionally, flu vaccination rates in the US have been declining; understanding if novel home-based screening approaches, like HPV-SS, might unintentionally contribute to this decline is relevant.[106]

Methods

Design and Population

Data for this secondary analysis came from STEP, a pragmatic, parallel, single-blind, randomized trial designed to understand if offering HPV-SS increased CC screening within KPWA, an integrated health system in Washington State.[63] Design details have been described previously.[65] The KPWA institutional review board approved STEP and allowed for enrollment under a waiver of consent to reduce participation bias.

Inclusion criteria were 30-64 years old, female sex, an intact cervix, KPWA insurance, and assignment to a KPWA primary care physician. Exclusion criteria were having a recent positive CC screen, participating in other KPWA HPV-SS studies, being pregnant, requiring language interpretation, or opting out of research. Eligible individuals were identified using KPWA electronic medical records (EMR) and administrative claims and randomized from November 2020-January 2022. STEP included members who were previously screening adherent and now due (within ≤3 months), overdue for screening, and with unknown screening history. The present analysis solely included the adherent group.

Adherent participants were randomized to: 1) usual care outreach consisting of standard preventive care outreach and provider notifications to complete in-clinic screening ('usual care'), 2) usual care+mailed, screening educational materials ('education'), 3) usual care+education+HPV-SS kit mailed directly to their home ('direct-mail'), or 4) usual care+education+insert with information on how to request a kit be mailed to their home ('opt-in'). The primary STEP outcome was CC screening completion within 6 months of randomization: in-clinic screening, kit return with negative or HPV-16/HPV-18-positive results, or kit return with other high-risk HPV-positive or unsatisfactory results followed by in-clinic Papanicolaou testing.

Outcome and Exposures

Our outcome in the present analysis was flu vaccine uptake within one year after randomization, identified using EMR and claims data. We had two exposures of interest. The first was randomization arm to understand how offering home-based HPV-SS influenced flu vaccine uptake ('between-arm' analysis). Usual care and education were

combined (UC/EDU); direct-mail and opt-in arms remained disaggregated. The second was screening approach selected within 6 months post-randomization among those given the option to self-sample (direct-mail and opt-in) to understand how utilizing home-based HPV-SS influenced flu vaccine uptake ('within-arm' analysis). Exposure categories were HPV-SS vs. in-clinic vs. no screening completed (Figure 2.1).

Covariates

Covariates collected from EMR at randomization included patient age, self-reported race and ethnicity, travel time to primary care clinic, years enrolled in KPWA, residential census tract Social Vulnerability Index (SVI),[72] Charlson comorbidity index (CCI),[66] tobacco use, body mass index (BMI), primary care provider or obstetrician-gynecologist visit 1 year pre-randomization, any in-person clinic visit 1 year pre-randomization, viewed a MyChart message 1 year pre-randomization, sent a MyChart message 1 year pre-randomization, number of days with a MyChart login 1 year pre-randomization, and any flu vaccine receipt 10 years pre-randomization.

Statistical analysis

We calculated descriptive statistics for the overall population and each trial arm. We calculated outcome prevalences by arm and screening approach for between- and within-arm analyses, respectively.

Different analytical approaches were taken for between- and within-arm analyses. For between-arm, as randomization was preserved, we fit unadjusted modified Poisson regression models to obtain risk ratios (RRs) and 95% confidence

intervals (95%CI) with binary flu vaccination as the outcome and STEP arm (direct-mail vs. opt-in vs. UC/EDU) as the exposure.[107]

For within-arm, we fit modified Poisson regression models with outcome regressed on three-level screening modality selected (HPV-SS use vs. in-clinic screening vs. no screening), unadjusted, and then adjusted for the covariates described above except for self-reported race and ethnicity. All variables were binary or categorical with the exception of age and days with a MyChart login, which were continuous (Table 2.1).

Further, as randomization was no longer preserved in the within-arm analysis, we implemented a propensity score stratification approach.[108–110] Many factors influence an individual's healthcare choices, and in our study, patients choosing HPV-SS may be characteristically different from those choosing in-clinic or no screening. While those who were offered HPV-SS were randomly selected, we were not able to control who actually utilized each approach, introducing confounding.[111,112] The propensity score is defined as the predicted and "conditional probability that a subject will be 'treated' based on an observed group of covariates,"[112] and provides a one-dimensional summary of confounders, allowing us to balance across multiple covariates using a single value ranging from 0 to 1.[113] To estimate propensity scores, 20 candidate logistic regression models for each comparison (HPV-SS vs. in-clinic screening; HPV-SS vs. none; in-clinic vs. none), and for direct-mail and opt-in separately, were fit as a function of covariates to determine the conditional probability that the patient selected the screening approach ('treatment') of interest.[110,111,114,115] Selection of covariates associated with both CC screening and flu vaccine receipt was informed by the literature (Supplemental Table

2.1).[116–121] Participants were stratified into quintiles based on propensity scores.[108,110] Within each quintile, we visually assessed propensity score balance between exposure groups by plotting score distributions.[108] We assessed balance through absolute standardized differences plots for covariates of interest within each quintile to select final models. We also plotted standardized differences comparing exposure groups for the entire population to understand covariate balance prior to stratification. We fit modified Poisson models to obtain RRs and 95%CIs for flu vaccine uptake comparing screening modalities within each quintile. RRs from these five models were combined through weighting by proportion in the subclass to estimate the average treatment effect.[122] We implemented complete case analysis for conventionally adjusted and propensity score quintile models as missing data was minimal (91.2% to 96.8% of participants included in propensity score models).

Secondary between- and within-arm analyses were performed. We stratified models by randomization month to explore variation by flu season proximity (March-August vs. September-November vs. December-February). We also stratified by randomization date (on or before April 30, 2021, vs. after) representing the pre- vs. post-COVID-19 vaccine eras, as willingness to enter healthcare settings to receive in-person services may have been impacted by vaccine rollout.

We used SAS version 9.4 (SAS Institute) and R version 4.4.1 (R Foundation) for this analysis.

Results

Study population

Analyses included 7,526, 1,451, and 3,947 (N=12,924) participants in the UC/EDU, direct-mail, and opt-in arms, respectively. Baseline characteristics were similar across randomization arms (Table 2.1). The median age overall was 46.5 years. The majority were white (73.5%), lived within 20 minutes of their KPWA clinic (69.3%), never used tobacco (71.9%), attended a primary care or obstetrics-gynecology visit the year before randomization (66.8%), and had received flu vaccines in the past (85.1%). Missing data was minimal. Of the variables included in models, two had missing data (BMI: 4.9% missing, tobacco use: 4.7% missing). Four participants missing travel time information were dropped entirely.

Exposure and outcome distributions

Figures 2.2A and 2.2B show exposure and outcome distributions, respectively, for between- and within-arm analyses. For within-arm direct-mail, 20.7%, 41.1%, and 38.1% utilized in-clinic screening, HPV-SS, and no screening, respectively. The opt-in distribution differed, with larger proportions selecting in-clinic (40.2%) and no screening (48.8%), and a smaller proportion self-sampling (11.0%). Flu vaccine distributions were very similar across arms in the between-arm analysis, ranging from 52.1% to 53.2%. For within-arm, flu vaccination was lowest among those who did not complete any CC screening. For direct-mail, 40.1% of non-screeners received a vaccination, compared with 61.8% of in-clinic screeners and 61.0% of self-samplers. Distributions were similar for opt-in.

Between-arm analysis

Results for the between-arm analysis are in the top panel of Figure 2.3. Overall, there were no differences in flu vaccine uptake comparing the direct-mail (RR=1.01; 95%CI=0.96-1.07) or opt-in (RR=0.99; 95%CI=0.96-1.03) arms vs. UC/EDU, indicating that offering mailed HPV-SS did not influence flu vaccine uptake.

Within-arm analysis

Variables included in propensity score models are in Supplemental Table 2.1, and distribution of propensity scores by screening modality and quintile are in Supplemental Figure 2.1. Propensity score distributions comparing modalities within each quintile were nearly identical for quintiles two, three, and four; distribution tails varied more so in quintiles one and five, and the range was wider.

Absolute standardized differences for covariates of interest comparing CC screening modality selected (HPV-SS vs. in-clinic; HPV-SS vs. no screening; in-clinic vs. no screening) for the entire within-arm analytical populations are shown in Figure 2.4. Generally, those who self-sampled were similar to those who screened in-clinic; however, for both direct-mail and opt-in, those who self-sampled were older and less likely to currently use tobacco. Differences emerged when comparing those who screened using either method vs. did not screen, particularly for past healthcare utilization. Those who self-sampled or in-clinic screened were more likely to have viewed or sent a message in MyChart, attended an in-person clinic visit the year before randomization, and received a flu vaccine previously compared to those who did not screen. Absolute standardized differences for covariates stratified by propensity score quintile are in Supplemental Figure 2.2.

Results of the unadjusted, adjusted, and propensity score quintile adjusted models for the within-arm analysis are presented in the bottom panel of Figure 2.3. For direct-mail, there were no differences in flu vaccine uptake comparing those who self-sampled vs. completed in-clinic CC screening, indicating that utilizing HPV-SS is not associated with flu vaccine uptake. Results from adjusted (RR=0.95; 95%CI=0.87-1.05) and propensity score quintile (RR=0.99; 95%CI=0.88-1.10) models were similar. Those who self-sampled were more likely to receive a flu vaccine within a year of randomization vs. non-screeners; results from adjusted (RR=1.27; 95%CI=1.14-1.42) and propensity score models (RR=1.29; 95%CI=1.13-1.48) were again similar. Those who in-clinic screened were also more likely to receive a flu vaccine vs. non-screeners (adjusted: RR=1.33; 95%CI=1.18-1.51, propensity score: RR=1.44; 95%CI=1.23-1.67). Results for the opt-in population were consistent: there was no difference in vaccine uptake comparing those who self-sampled vs. in-clinic screened (propensity score: RR=1.02; 95%CI=0.93-1.11), and those who self-sampled (propensity score: RR=1.34; 95%CI=1.17-1.53) and in-clinic screened (propensity score: RR=1.36; 95%CI=1.27-1.47) were more likely to receive a flu vaccine vs. non-screeners.

Secondary analyses

Secondary between- and within-arm analyses results were consistent with primary analyses (Supplemental Figure 2.3). Neither offering nor utilizing HPV-SS vs. in-clinic screening were associated with flu vaccination, while both self-sampling and in-clinic screening were each positively associated with vaccination vs. no screening across all time periods.

Discussion

Home-based HPV-SS successfully increases CC screening completion across a range of populations, but there is concern that moving screening out of a clinical setting may inadvertently impact the uptake of other routinely recommended health services. Leveraging data from a pragmatic trial in an integrated US-based healthcare system, we observed that offering and utilizing HPV-SS compared to in-clinic screening did not influence subsequent flu vaccine uptake in a population with a history of screening adherence. This study provides early evidence that in this population, choosing home-based HPV-SS is unlikely to alter, positively or negatively, rates of flu vaccination, an important and annually recommended preventive service.

We encourage researchers to investigate these questions in other populations and healthcare systems, especially given recent HPV-SS FDA approvals, and acceptance among individuals and US guideline-generating organizations.[61,123–125,34] Studies examining the relationship between home-based HPV-SS and subsequent care utilization are sparse. In another secondary analysis of a KPWA pragmatic trial to assess HPV-SS effectiveness vs. usual care for screening uptake (HOME), our study team found no significant between-arm difference in subsequent uptake of flu vaccination (odds ratio=0.99).[102] In the within-arm analysis, contrasting our present STEP results, vaccination uptake was actually higher among those who completed in-clinic screening vs. HPV-SS (odds ratio=1.23). Reasons for this diverging finding may be that the prior study only included those overdue for screening, the HPV-SS intervention differed from STEP (those in HOME were instructed to still come into the clinic for screening even if they self-sampled with a negative result), and their within-arm

estimate was unadjusted, potentially introducing residual confounding by healthcare engagement history.[46,102] Additionally, timing of these trials may have impacted findings. STEP occurred during the COVID-19 pandemic (randomization November 2020-January 2022), which influenced preventive services uptake in the US.[26,126] The distribution of months between the trials also differed and may have contributed to divergent findings: HOME's randomization (February 2014-August 2016) spanned three summers and two flu seasons, while STEP only spanned one of each.

We assessed two HPV-SS interventions: direct-mail and opt-in. These present meaningfully different barriers for CC screening, with direct-mail removing many logistics entirely by sending a kit to a patient's home vs. opt-in requiring a patient to actively request a kit. However, neither influenced flu vaccine uptake in this analysis. This is notable given that direct-mail was superior to opt-in for increasing screening uptake in the primary STEP analysis and is more cost-effective among the adherent.[37,63] As healthcare systems adopt HPV-SS and determine implementation strategies, future studies will be well-positioned to examine if these patterns hold more broadly.

In our study, those who chose in-clinic screening and HPV-SS were very similar on most measured characteristics, while those who did not screen differed, particularly in terms of previous healthcare engagement. The literature provides evidence of an association between cancer screening and flu vaccine uptake across a range of populations. This may partially reflect that individuals already inclined to engage with the healthcare system are more likely to complete multiple preventive services.[127–131] A 2016 US-based study demonstrated that those with a history of ever breast, prostate, or colorectal cancer screening were more likely to have received a flu vaccine in the last

year.[127] A similar study using New York State 2022 survey data demonstrated that CC screening adherence was positively associated with receiving flu vaccination and having completed a recent check-up after controlling for a number of healthcare access variables, indicating that those who complete CC screening are likely more willing to interact with the medical system.[128] A 2017 analysis of Kaiser Permanente Southern California 21-year-old females found that receiving a flu vaccine in the past year was positively associated with CC screening initiation.[129] As demonstrated by the present analysis and previous work, health engagement indicators are associated and reinforce each other, and those not engaged in CC screening remain at highest risk for not receiving other important services.

Flu vaccination was selected as our outcome as it is recommended annually, and protects against a range of severe outcomes.[103–105] Flu vaccination prevalence in our KPWA population was similar though slightly higher than the overall US population. National vaccination rates among US adults were 50.2%, 49.4%, and 46.9% during the 2020-21, 2021-22, and 202-23 flu seasons, respectively.[106] STEP was launched and ongoing during the COVID-19 pandemic, and the relationship between flu vaccine uptake and cancer screening may have been unique to this period.[25,26] Additionally, the location where individuals receive flu vaccines may influence the relationship between HPV-SS and vaccine uptake. Those who obtain their vaccines in settings such as pharmacies or workplaces, venues inherently decoupled from cancer screening, may be equally likely to vaccinate regardless of whether they screen in-clinic or self-sample. However, a substantial proportion of US adults receive their flu vaccines in settings that

also provide cancer screenings (doctor's offices: 23.4%, clinics/health centers: 9.5%, hospitals: 7.6%).[132]

The context of HPV-SS is changing rapidly in the US. The FDA approved HPV-SS for use in healthcare settings in 2024 and home-based HPV-SS in 2025, and the Health Resources and Services Administration updated guidelines to include language requiring most insurance plans to cover this option beginning 2027.[100,101,133,134] KPWA adopted in-clinic and home-based HPV-SS as standard care after the success of the STEP trial.[90] Different HPV-SS distribution strategies may have varying impacts on subsequent preventive services; these relationships should be explored as health systems expand HPV-SS. Further, future works should investigate how HPV-SS may influence uptake of other recommended cancer screenings (e.g., breast and colorectal). as the impact of offering or using HPV-SS may depend on the preventive service.[67]

In this analysis, there were specific considerations around controlling for confounding in the within arm analysis due to institutional review board requirements to suppress small cells (often called 'minimum cell size requirements'). Specifically, STEP data were only allowed to be analyzed in aggregate due to human subjects restrictions on obtaining individual level data, meaning that no combination of variables could represent fewer than five participants. While these rules are important for protecting participant confidentiality, they present a challenge in the presence of confounding, as requesting data on even a few confounders can quickly lead to cell sizes of less than five. And while aggregating confounder categories or omitting variables all together to abide by these rules is a solution, this likely leads to residual confounding that would be difficult to quantify. In this analysis, we employed the creative and novel implementation

of propensity scores stratification, where we fit, as mentioned in the Methods, 20 candidate propensity score logistic regression models, modeling the predicted probability of choosing a screening modality conditional on covariates, for each comparison of interest.[110,111] However, to stay compliant with data suppression policies, KPWA's analyst provided only aggregated data with information on outcome (flu vaccine uptake, yes/no), exposure status (CC screening modality chosen), and propensity score quintile (1 through 5) for each candidate models, protecting against any groupings of less than five participants while still obtaining collective information on confounders. The aggregated nature of the propensity score stratification method naturally allowed for compliance with the institutional review board's policy. If we had used an alternative propensity score method, such as matching or controlling for the propensity score as a continuous covariate, this would have required individual-level propensity score data, as with a large enough number of confounders, each participant should theoretically have their own unique predicted probability of being exposed. Additionally, receiving aggregated propensity score information allowed us to explicitly investigate and compare the propensity score distribution within quintiles between the exposed and unexposed by exporting summary statistics such as the minimum, maximum, and median (Supplemental Figure 2.1), and logically led to the calculation of standardized differences,[108,109] which allowed us to quantitatively investigate confounder balance between the exposed and unexposed in the entire study population (Figure 2.4), and within quintiles (Supplemental Figure 2.2). The consistent results between the conventionally adjusted and propensity score quintile models provided reassurance of this as a valid and novel methodological approach in the setting of data suppression.

Our study had several strengths. Use of EMR data and STEP's pragmatic design in an integrated healthcare system allowed for data linkage, highly complete data, and use of non-self-report measures for screening and vaccination, avoiding recall and social desirability biases.[63,64] The use and agreement across multiple methods to control confounding strengthens confidence in our findings. Additionally, including only adherent participants, as opposed to all screening histories, increased our study's internal validity.

Our study also had limitations. There were variables related to CC screening (family history of cancer, individual-level socioeconomic position) that we were unable to control for, as they were not readily available in the EMR. In the within-arm analysis, we were unable to establish temporality between screening approach within 6 months and flu vaccine receipt within 12 months due to data access restrictions on dates, meaning that a participant could have received their flu vaccine prior to CC screening. This could have led to temporal bias and potentially reverse causation, though this was not a concern in our between-arm analysis. While excluding these participants may seem like a solution, doing so would differentially remove those with positive exposure and outcome status, introducing selection bias through analytical choices. Finally, while KPWA patients have been shown to be demographically and clinically similar to the Washington State population, our population's screening adherence history, and the fact that KPWA patients receive the majority of their care through a single, coordinated health system that reduces access barriers, may ultimately limit generalizability.[93]

Conclusion

In this analysis of a pragmatic trial, offering patients with a history of CC screening adherence mailed HPV-SS was not associated with subsequent flu vaccination. Further, those who self-sampled were just as likely to receive their flu vaccination compared to those who in-clinic screened. Understanding if offering and using HPV-SS could influence uptake of other routinely recommended services will become increasingly important as HPV-SS programs expand in the US. We encourage others to investigate if these relationships hold across other preventive services, such as breast and colorectal cancer screenings, and different populations.

Tables and Figures

Figure 2.1. Flow chart of STEP trial, study exposures, and study outcome. STEP participants were randomized to the usual care, education (arms combined for this analysis; UC/EDU), direct-mail, or opt-in interventions. Flu vaccine uptake within one year of randomization into STEP was compared between the arms for the between-arm analysis. Participants in the direct-mail and opt-in arms then had the option to complete cervical cancer screening through HPV self-sampling kit return, traditional in-clinic screening, or not completing screening at all. Flu vaccine uptake within one year of randomization into STEP was also compared between screening modalities for the within-arm analysis. Only the opt-in and direct-mail arms were analyzed in the within-arm analysis as those in UC/EDU did not have the option to utilize an HPV self-sampling kit

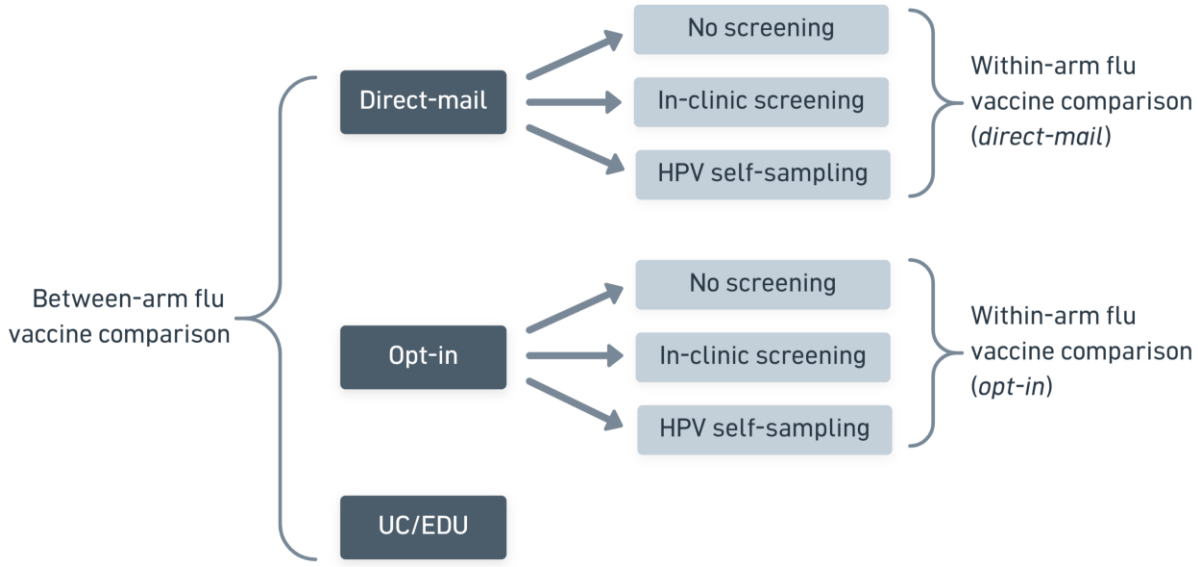


Table 2.1. Baseline characteristics of adherent STEP population by randomization arm, mean [SD] or N (%)

	STEP randomization arm			
	UC/EDU N = 7526	Direct-mail N = 1451	Opt-in N = 3947	Overall N = 12924
Age at randomization, years	46.4 [10.4]	47.1 [10.0]	46.3 [10.4]	46.5 [10.4]
Race				
Asian	863 (12.0)	175 (12.6)	482 (12.8)	1520 (12.4)
Black	363 (5.1)	74 (5.3)	163 (4.3)	600 (4.9)
More than one race or another race	547 (7.6)	88 (6.4)	286 (7.6)	921 (7.5)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	144 (2.0)	24 (1.7)	56 (1.5)	224 (1.8)
White	5247 (73.2)	1023 (73.9)	2769 (73.7)	9039 (73.5)
Ethnicity				
Hispanic	497 (7.1)	83 (6.1)	266 (7.2)	846 (7.1)
Non-Hispanic	6468 (92.9)	1273 (93.9)	3404 (92.8)	11145 (92.9)
Travel time to clinic, minutes				
<10	2424 (32.2)	481 (33.1)	1316 (33.3)	4221 (32.7)
10 to <20	2793 (37.1)	522 (36.0)	1423 (36.1)	4738 (36.7)
20 to <30	1151 (15.3)	229 (15.8)	596 (15.1)	1976 (15.3)
≥30	1158 (15.4)	219 (15.1)	612 (15.5)	1989 (15.4)
Length enrolled in KPWA, years				
<3.25	2123 (28.2)	423 (29.2)	1143 (29.0)	3689 (28.5)
3.25 to <5	989 (13.1)	229 (15.8)	537 (13.6)	1755 (13.6)
5 to <10	2083 (27.7)	311 (21.4)	1035 (26.2)	3429 (26.5)
≥10	2331 (31.0)	488 (33.6)	1232 (31.2)	4051 (31.3)
Census tract Social Vulnerability Index				
0-0.25 (least vulnerable)	2477 (32.9)	461 (31.8)	1246 (31.6)	4184 (32.4)
0.26-0.50	2221 (29.5)	429 (29.6)	1212 (30.7)	3862 (29.9)
0.51-0.75	1814 (24.1)	363 (25.0)	953 (24.1)	3130 (24.2)
0.76-1 (most vulnerable)	1014 (13.5)	198 (13.6)	536 (13.6)	1748 (13.5)
Charlson comorbidity index				
0	6271 (83.3)	1184 (81.6)	3293 (83.4)	10748 (83.2)
≥1	1255 (16.7)	267 (18.4)	654 (16.6)	2176 (16.8)

Note: missing/unknown categories were excluded from this table as missing data were minimal. Missingness was 4.8% for race, 7.2% for ethnicity, 4.7% for tobacco use, and 4.9% for body mass index; all other variables did not have missing data.

Table 2.1 (continued). Baseline characteristics of adherent STEP population by randomization arm, mean [SD] or N (%)

	STEP randomization arm			
	UC/EDU N = 7526	Direct-mail N = 1451	Opt-in N = 3947	Overall N = 12924
Current	440 (6.1)	89 (6.4)	209 (5.6)	738 (6.0)
Former	1565 (21.8)	289 (20.8)	873 (23.2)	2727 (22.1)
Body mass index, kg/m²				
≤24.9	2358 (32.9)	464 (33.7)	1221 (32.5)	4043 (32.9)
25-29.9	1987 (27.7)	377 (27.4)	1084 (28.9)	3448 (28.0)
30-34.9	1328 (18.5)	250 (18.1)	681 (18.2)	2259 (18.4)
35-39.9	749 (10.5)	144 (10.4)	376 (10.0)	1269 (10.3)
≥40	743 (10.4)	143 (10.4)	390 (10.4)	1276 (10.4)
Primary care or obstetrician-gynecologist visit 1 year pre-randomization	4993 (66.3)	980 (67.5)	2660 (67.4)	8633 (66.8)
Any in-person clinic visit 1 year pre-randomization	6657 (88.5)	1291 (89.0)	3521 (89.2)	11469 (88.7)
Viewed MyChart message 1 year pre-randomization	6290 (83.6)	1196 (82.4)	3307 (83.8)	10793 (83.5)
Sent MyChart message 1 year pre-randomization	5780 (76.8)	1090 (75.1)	3037 (76.9)	9907 (76.7)
Number of days with MyChart login 1 year pre-randomization	31.5 [33.4]	31.0 [33.2]	31.8 [33.5]	31.5 [33.4]
Flu vaccine receipt 10 years pre-randomization	6398 (85.0)	1229 (84.7)	3370 (85.4)	10997 (85.1)

Note: missing/unknown categories were excluded from this table as missing data were minimal. Missingness was 4.8% for race, 7.2% for ethnicity, 4.7% for tobacco use, and 4.9% for body mass index; all other variables did not have missing data.

Figure 2.2A. Exposure distribution for the between- and within-arm analyses

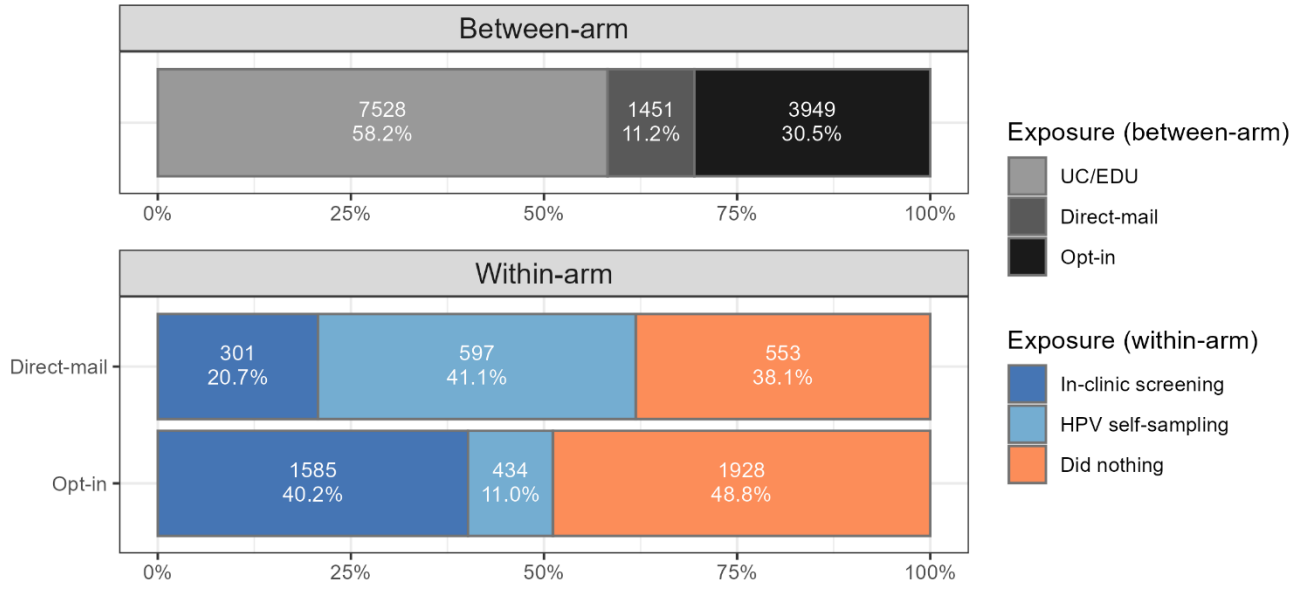


Figure 2.2B. Outcome distribution for the between- and within-arm analyses

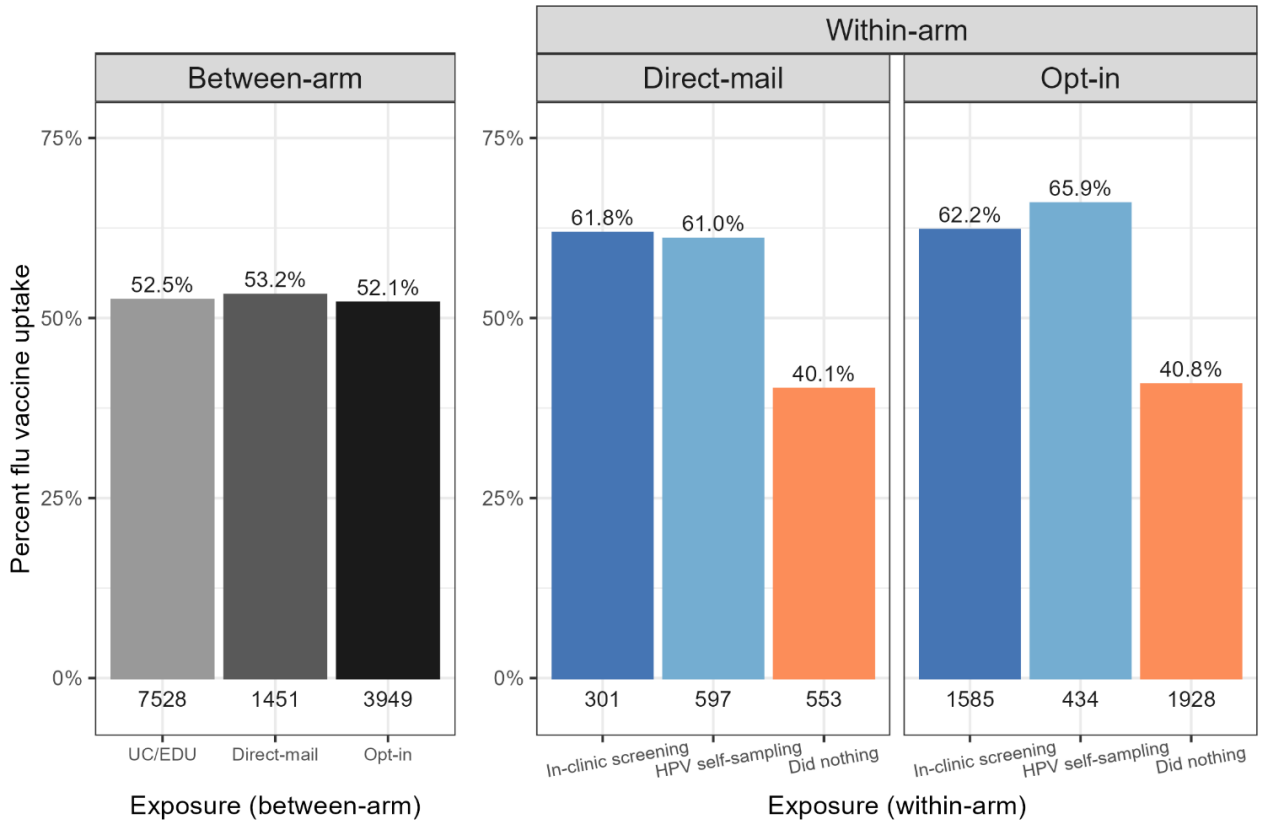


Figure 2.3. Risk ratios for flu vaccine uptake for between- and within-arm analyses

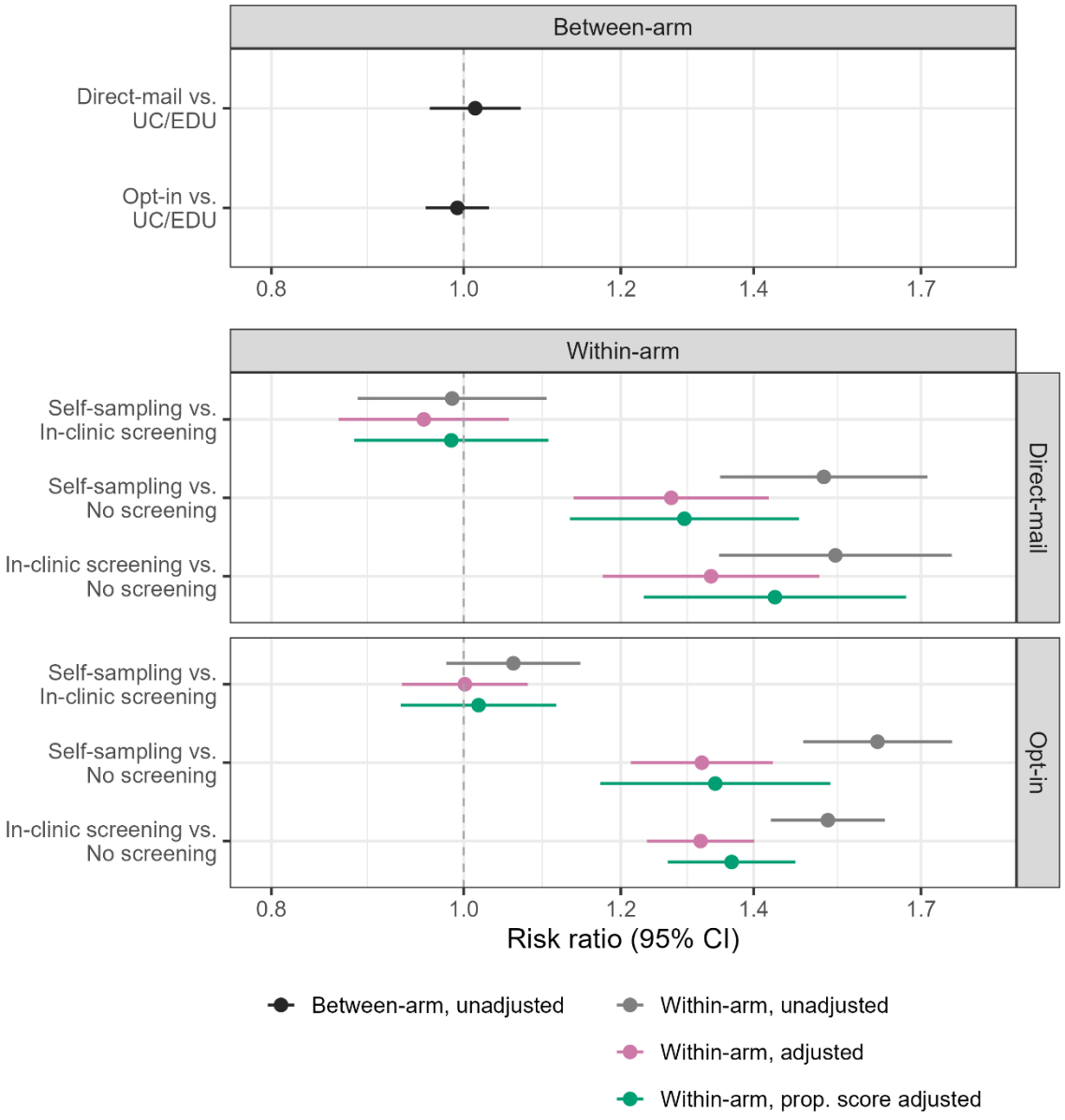
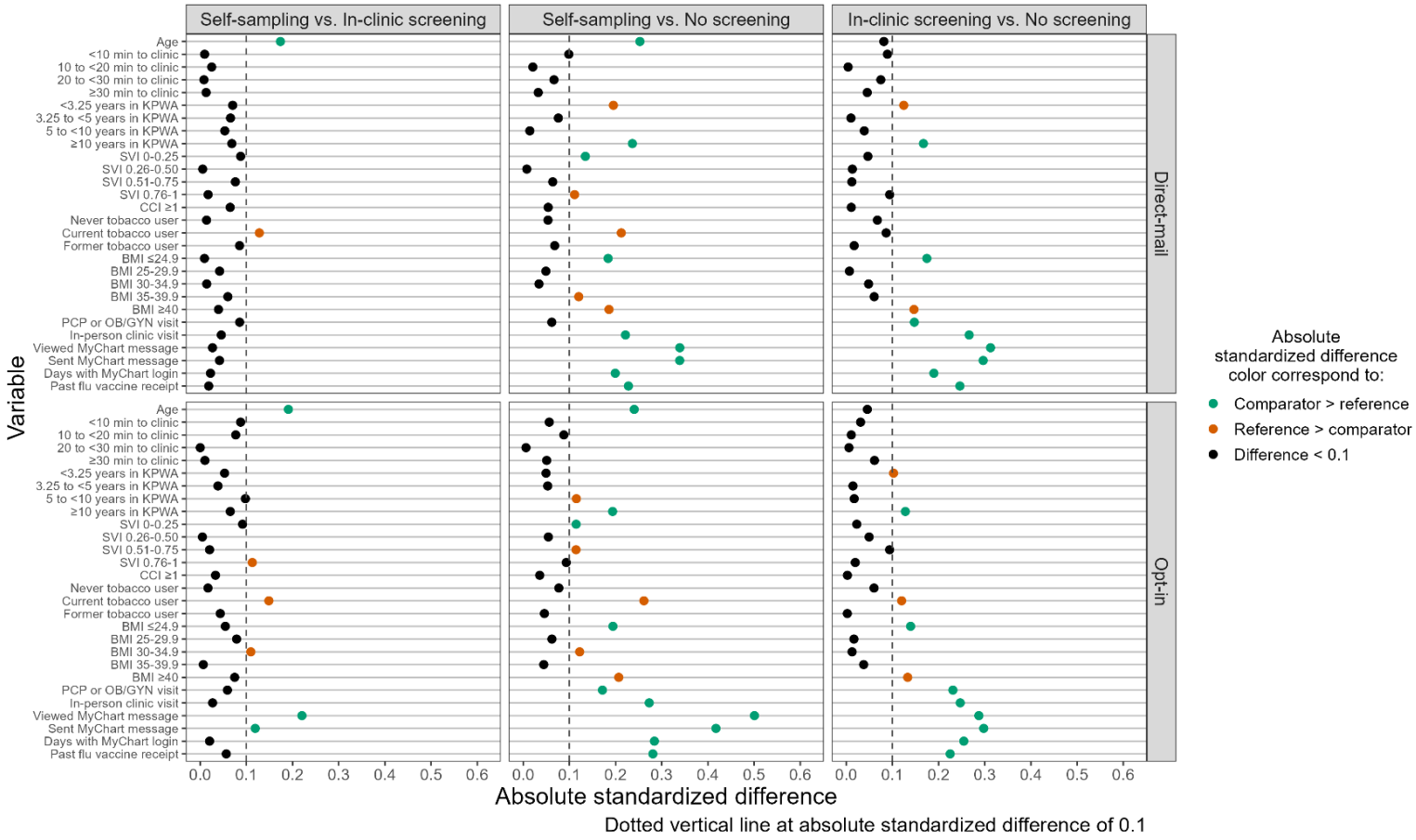


Figure 2.4. Absolute standardized differences of covariates for each within-arm analysis comparison for the entire population, for both direct-mail and opt-in arms. Green dots indicate that the comparison group had a greater or higher value of the corresponding covariate compared to the reference group; red dots indicate the opposite



Chapter 3: Potential for spillover: an analysis investigating cervical cancer screening after the *Dobbs v. Jackson Women's Health Organization* US Supreme Court Decision

Abstract

Background: The *Dobbs v. Jackson Women's Health Organization* (*Dobbs*) Supreme Court decision created a patchwork of abortion policies in the US, with impacts widely documented. However, *Dobbs'* influence on non-abortion and non-reproductive outcomes, such as cervical cancer screening (CC), has not been thoroughly explored.

Methods: We used data from the Behavioral Risk Factor Surveillance System (BRFSS), a national, serial cross-sectional survey of US residents (N=41,410). We analyzed trends in the prevalence of CC screening adherence from 2022 to 2024 in states that did (n=6) versus did not (n=25) ban abortion after *Dobbs* using a difference-in-differences (DID) design, and assessed whether this relationship differed in populations with historically lower access to screening using a difference-in-difference-in-differences (DDD) design. All models were fit unadjusted and adjusted, with state and year fixed effects, and a linear specification to directly estimate absolute adherence prevalence differences.

Results: The prevalence of CC screening adherence increased across all states in our analysis over the study period. States that banned abortion post-*Dobbs* experienced a lesser increase compared to states where abortion remained accessible, though results were nonsignificant ($DID_{adjusted}$ = -2.2%; 95%CI: -7.0, 2.6). Abortion bans may have a disproportionate influence on those with less education ($DDD_{adjusted}$ = -5.5%; 95%CI: -14.9, 3.8), public health insurance ($DDD_{adjusted}$ = -4.8; 95%CI: -16.0, 6.3), and certain racial/ethnic groups such as Hispanic populations ($DDD_{adjusted}$ = -10.5%; 95%CI: -28.2, 7.1), though results were again nonsignificant.

Conclusions: While we did not find significant effects, there was early evidence of a negative association between post-*Dobbs* abortion bans and screening adherence trends, and this association may vary by population. These results should be interpreted as preliminary. This study contributes to the growing evidence around the far-reaching effects of *Dobbs* beyond abortion and reproductive outcomes and underscores the need for research that investigates CC screening over additional years post-*Dobbs*.

Introduction

On June 24, 2022, the US Supreme Court ruled on *Dobbs v. Jackson Women's Health Organization*, overturning *Roe v. Wade* and *Planned Parenthood v. Casey* and eliminating the constitutional right to abortion.[135] Prior to *Dobbs*, abortion was considered legally accessible until fetal viability across the country, though access varied. *Dobbs* was not an outright national abortion ban. Rather, it returned the regulation of abortion to individual states, allowing multiple states to implement severe restrictions access, resulting in a state-by-state patchwork of abortion policies.

The early impacts of *Dobbs* have been well-documented.[136–143] Studies have shown higher than expected infant mortality in states that adopted abortion bans,[136] increases in tubal sterilization procedures across the US but greater increases in banned states,[137] increases in estimated travel times to abortion facilities,[138] significant decreases in medical residency applications to programs in abortion-restricted states,[139] and moderate but significant decreases in obstetrician-gynecologist (OBGYN) practitioners in the most restrictive states.[140]

However, the impacts of *Dobbs* on non-abortion and non-reproductive outcomes have not been as thoroughly investigated. Abortion trigger laws (anticipatory bans that took effect upon passage of *Dobbs*) were associated with increases in self-reported anxiety and depression symptoms among trigger-law state residents, with greatest increases observed among females 18 to 45 years.[144] Post-*Dobbs* intimate partner violence Google search trends have also been investigated.[145]

These studies raise concern that there may be additional, wider-reaching impacts of this court case, demonstrating a need to explore potential ‘spillover’ effects of *Dobbs*,

as it may be leading to diminished use of other essential care services, such as cervical cancer (CC) screening. Such policies in the US often impact populations that face systemic barriers and historically less access to healthcare utilization, such as those living in rural areas, low-income individuals, and marginalized racial and ethnic groups.[146–149] While *Dobbs* did not directly target CC screening, resulting legislation could influence services through other mechanisms, such as provider shortages, increased stigma and fear, or general confusion around sexual-health-adjacent care among patients.[149] This is of particular concern in light of declining screening rates, which have fallen steadily over the past 20 years. Up-to-date screening prevalence estimates were approximately 74% as of 2021, though figures vary across sources.[150–153] There is also considerable overlap between states with abortion restrictions and higher cancer mortality among reproductive age adults, making investigating this relationship even more pertinent.[149]

This study investigated if abortion bans enabled by *Dobbs* impacted CC screening patterns in states where abortion access did versus did not change, and if this relationship differed in populations with historically less access to screening. Assessing if abortion bans influence CC screening uptake provides an opportunity to proactively identify populations at risk of falling behind on screening, and may provide additional support for abortion protections.

Methods

We used Behavioral Risk Factor Surveillance System (BRFSS) data, a population-based, self-report, telephone survey conducted at the state-level but organized and standardized through the US Centers for Disease Control and Prevention

(CDC).[154] BRFSS collects data on a range of topics to understand the health of US residents, including chronic diseases, health behaviors, and preventive services use. We incorporated BRFSS survey weights into the present analysis. The CDC computes weights to account for noncoverage and nonresponse, allowing analyses to be representative of all adults in each state. The “Breast and Cervical Cancer Screening” Core Section is collected across all states during even-numbered calendar years. States may include this section during odd-numbered years, though this is uncommon.[155] Our main analysis used 2022 and 2024 data, and assessments of analytical validity threats incorporated 2018 and 2020. We included respondents who identified as female, reported no history of hysterectomy, were not currently pregnant, and were 30-49 years of age. For sex ascertainment, in 2022, BRFSS participants were asked “What was your sex at birth? Was it male or female?”; in 2024, participants were instead asked “Are you male or female?” We excluded respondents who were missing information on all CC screening questions.

We restricted to respondents aged 30-49 as this reproductive age group is most likely to be affected by abortion restrictions, and those under 30 were excluded due to evolving screening guidelines that vary by recommending organization for ages 21-29.[36,156] However, those 25-29 were included in a sensitivity analysis. We excluded respondents who reported a history of cervical or uterine cancer, as this population may be on a different screening schedule, and respondents who reported a history of two or more cancers, as BRFSS solely collects information on most recent cancer type.

Our outcome was CC screening adherence based on US Preventive Services Task Force (USPSTF) 2018 guidelines for average-risk individuals,[156] derived from a

series of four BRFSS questions (Supplemental Tables 3.4 and 3.5). Participants were asked if they had ever been screened for CC, how long it had been since their last screening, and if at their most recent screening, they received a Pap test and/or a human papillomavirus (HPV) test. For our primary outcome definition, participants were considered adherent if they reported receiving any CC screening within the past three years, or receiving an HPV test (alone or as co-test with Pap) within the past three to five years. Participants were considered nonadherent if they reported never been screened, last screening five or more years ago, receiving a Pap test alone within the past three to five years, or uncertainty about whether they had ever been screened, timing of last screening, or screening modality received within the past three to five years. We conducted sensitivity analyses with two additional outcome definitions. Our secondary outcome defined participants the same as the primary, but excluded those who reported uncertainty around ever screening, timing of last screening, or screening modality received within the past three to five years. For our tertiary outcome, participants were considered adherent if they reported any screening in the past five years, and nonadherent otherwise.

We defined our exposure as residence in a state where abortion went from accessible and legal at or beyond fetal viability to fully banned, defined as banned at 6 weeks of pregnancy or any gestational age, within two months of the *Dobbs* decision (i.e., by August 25, 2022). Control states were those where abortion remained accessible and steady pre- and post-*Dobbs*, defined as legal until at least 24 weeks or at or beyond fetal viability. We excluded states if they expanded Medicaid after January 2019 to limit the risk of confounding (Supplemental Figure 3.4). Medicaid expansion is

associated with increased uptake of CC screening and gynecologic cancer care outcomes, which could lead us to misattribute a change in screening due to Medicaid expansion to *Dobbs*.[157–159] For example, while Oklahoma and South Dakota both had trigger abortion bans go into effect immediately upon the release of *Dobbs*, they expanded Medicaid in July 2021 and July 2023, respectively, and were thus excluded from the main analysis.[160,161] This 2019 cut-off ensured minimum 3 years between expansion and our pre-*Dobbs* period (January 2022). We also excluded states if abortion access fluctuated throughout the study period, or if the abortion ban implemented post-*Dobbs* was not defined as banned at 6 weeks of pregnancy or any gestational age to maintain a consistent exposure definition. For example, Florida implemented a 15-week abortion ban post-*Dobbs*, but in April 2024, a State Supreme Court decision allowed for the implementation and enforcement of a more restrictive 6-week ban, taking effect May 2024, thus excluding Florida from our main analysis.[161] In another example, Iowa attempted to reimplement a 6-week abortion ban in 2023, which was quickly blocked by courts; however, the ban successfully went into effect on July 29, 2024, during our post-*Dobbs* study period.[162] Our final main exposure definition included 25 abortion accessible states (AK, CA, CO, CT, DC, DE, HI, IL, ME, MD, MA, MI, MN, MT, NV, NH, NJ, NM, NY, OR, PA, RI, VT, VA, WA), and 6 abortion banned states (AL, AR, GA, KY, LA, MS) (Figure 3.1). Tennessee would have been considered a banned state, but did not collect sufficient data to be included in the 2024 BRFSS public dataset.[163] State inclusion/exclusion criteria were varied methodically for a series of sensitivity analyses (Supplemental Figure 3.1).

In our main analysis, we defined the pre-*Dobbs* period as January 1, 2022, through June 23, 2022 (*Dobbs* released the following day). We then implemented a "washout period" from June 24, 2022, through the remainder of 2022 BRFSS. We defined post-*Dobbs* as January 1, 2024, through the remainder of the 2024 BRFSS data collection period (Figure 3.2). We conducted two washout period sensitivity analyses. The first shifted the washout period to begin on May 2, 2022, the date a draft of the *Dobbs* decision was leaked.[164] The second removed the washout period altogether, having the post-*Dobbs* period begin on June 24, 2022.

We included individual characteristics from BRFSS, specifically age, race and ethnicity, annual household income, educational attainment, healthcare coverage, and urban/rural county status (categories described in Table 3.1).

Statistical analysis

We assessed weighted characteristics of the study population stratified by state abortion access status in the pre- and post-*Dobbs* period. We also evaluated weighted CC screening adherence from 2018-2024 stratified by abortion access status and state to visualize heterogeneity.

In our main analysis, we evaluated the association between *Dobbs*-enabled abortion bans and CC screening adherence using a difference-in-differences (DID) approach.[165–167] This design involves comparing changes in an outcome before and after an event in a 'treatment' group (those living in a state that implemented an abortion ban) to changes in an outcome before and after that same event in a 'control' group (those living in a state where abortion remained accessible), where it is assumed that

both groups would have followed similar trajectories in the absence of the event, commonly called the parallel trends assumption.[167,168] To directly estimate prevalence differences, we fit a linear regression model with CC screening adherence as our outcome, with a fixed effect for state, a fixed effect indicator for pre- (January 1, 2022, through June 23, 2022) vs. post-*Dobbs* (January 1, 2024 through remainder of 2024 BRFSS data collection) period, and an interaction term between post-*Dobbs* indicator and binary abortion access status, which provided the DID estimate. All models in this analysis included state fixed effects to account for time-invariant differences between states (i.e., time-invariant group attributes such as pre-existing cancer screening infrastructure, political environment), and year fixed effects to account for time-varying factors that impact all states (i.e., group-invariant time attributes such as COVID impacts on healthcare). We fit an unadjusted model, and an adjusted model controlling for age (5-year intervals), insurance type (private, public, none), and county metropolitan status (urban, rural), as well as an additional adjusted model further controlling for education level (did not graduate high school, graduated high school, attended college/technical school, graduated from college/technical school) and annual household income (<$25k, $25k to <$50k, $50k to <$75k, ≥$75k).

A series of validity checks were performed for the main analysis. We performed a series of prior trends tests to examine if banned and accessible states had differing screening trends in the period leading up to *Dobbs.*[168] While these tests do not allow us to directly prove or disprove the parallel trends assumption, they can be useful to understand if this assumption is plausible. Using pre-*Dobbs* BRFSS data (2018, 2020, and 2022 before June 24, 2022), we again fit a linear regression model with screening

adherence as our outcome, state and year fixed effects, and included an interaction term between year and binary post-*Dobbs* abortion access status (banned vs. accessible). We used a quasi-likelihood ratio test to assess if the interaction term differed significantly from zero.[169] We fit prior trends tests with the year operationalized as a 3-level categorical (2022 vs. 2020 vs. 2018) and then a continuous variable, and then unadjusted and adjusted. We also performed placebo tests where we fit a linear regression with the same specifications as our main DID model using pre-*Dobbs* data, but artificially backdated the policy date (a "fake" treatment date).[168] We fit placebo tests with 2018 and 2020 BRFSS data, imagining *Dobbs* was passed in 2019, and then again with 2020 and pre-*Dobbs* 2022 data, imagining *Dobbs* was passed in 2021. We again fit these models unadjusted and adjusted, and used quasi-likelihood ratio tests to assess interaction term significance.[169]

A series of sensitivity DID analyses were performed. We repeated the main DID analysis as described but varied state exposure status based on abortion access nuances and Medicaid expansion timing (Supplemental Figure 3.1). We also varied our washout period definition, first shifting washout period to begin May 2, 2022, based on the leaked *Dobbs* decision, and second removing the washout period altogether. We also ran a sensitivity analysis where we decreased our minimum age inclusion to 25 years. Finally, we reran models using our secondary and tertiary outcome definitions.

We also conducted a series of difference-in-difference-in-differences (DDD) secondary analyses to highlight potential heterogeneity in *Dobbs'* influence.[170] We implemented this approach to examine if the relationship between abortion bans and screening adherence in banned vs. accessible states varied across subpopulations,

aiming to understand if the association between abortion access restrictions and screening was more pronounced among groups with historically lower screening rates and greater systemic barriers to healthcare.[148,171,172] We investigated this possibility across five individual-level characteristics and categorized as follows: age (30-39; 40-49 years), race/ethnicity (white, non-Hispanic (NH); American Indian/Alaska Native (AI/AN), multiracial, or other race, NH; Asian or Native Hawaiian and Pacific Islander (NHPI), NH; Black, NH; Hispanic, any race), county metropolitan status (urban; rural), education level (completed college/technical school; did not complete), and health insurance (private; public; none). BRFSS-weighted CC screening prevalence estimates were calculated and plotted for pre-Dobbs 2022 and 2024 in banned and accessible states for each of these aforementioned populations (Supplemental Figures 3.5A-3.5E). DID models comparing screening before and after *Dobbs* in banned vs. accessible states, with the specifications described above, were fit for each subpopulation of interest (for example, DID model fit among those 30-39 years and then for those 40-49 years). DDD models were fit with the same specifications as DID models but included a triple interaction term between post-*Dobbs* indicator, binary abortion access status, and the categorical variables mentioned above, providing our DDD estimate, and were fit unadjusted and adjusted.

We used R version 4.4.1 (R Foundation) for this analysis.

Results

Our main analysis included data from N=41,410 2022 and 2024 BRFSS participants from 31 states (including DC), with N=36,695 (88.6%) in 25 abortion accessible and N=4,715 (11.4%) in 6 banned states. Banned states were concentrated

in the southeast US (Figure 3.1). All subsequent percentages are weighted. Participant characteristics are presented in Table 3.1. In banned compared to accessible states, a larger percentage identified as non-Hispanic Black or Hispanic of any race, reported annual household incomes <$50,000, did not attend or graduate from college/technical school, did not have health insurance, and resided in rural counties.

Figure 3.3 shows CC screening adherence prevalence trends for our population from 2018 through 2024 (only pre-*Dobbs* 2022 and 2024 data used in main analysis) as well as main DID analysis results. For both banned and accessible states, screening adherence was highest in 2018 (86.5% accessible; 85.8% banned). Adherence dropped from 2020 to 2022 (49.5% accessible; 51.4% banned), likely due to a change in BRFSS CC screening questions between these waves and changes in preventive care behavior during the COVID-19 pandemic.[152,173] Adherence then rose in 2024 for both groups (72.5% accessible; 73.3% banned), suggesting a likely rebound from the effects of the COVID-19 pandemic across the US. While there was heterogeneity in adherence prevalence across states, trends were similar (Supplemental Figure 3.2).

In the main, unadjusted DID analysis, abortion bans following *Dobbs* were associated with 0.71% lower increase in CC screening adherence compared to states that maintained abortion access, though this was not statistically significant (95%CI: -5.37, 3.95). Adjusted DID estimates analyses provided similar inference, but were further from the null. In minimally and fully adjusted models, abortion bans were associated with 1.43% (95%CI: -5.98, 3.11) and 2.20% (95%CI: -7.02, 2.62) lower increases in adherence prevalence, respectively (Figure 3.3).

Prior trends tests, which we used to evaluate the plausibility of the parallel trends assumption, are presented in Supplemental Table 3.1. Prior trends tests did not demonstrate statistically significant evidence that pre-*Dobbs* trends in screening adherence differed by state abortion status, regardless of how year was parameterized. Placebo tests results also did not demonstrate evidence that our selected fake treatment dates were significantly associated with screening adherence prevalence (Supplemental Table 3.2).

DID results were generally robust to sensitivity analyses. The most notable shifts were for the secondary and tertiary outcome definition sensitivity analyses. For the secondary definition, unadjusted and fully adjusted DID estimates were 0.47 (95%CI: -4.31, 5.26) and -1.22 (95%CI: -6.06, 3.62), respectively (Supplemental Figure 3.3).

DDD results are presented in Figure 3.4. We did not find evidence that the relationship between *Dobbs*-enabled abortion bans and CC screening adherence prevalence from 2022 to 2024 differed significantly by population. However, results suggest that bans may have had slightly more influence on certain groups. For example, the adjusted DDD estimate comparing those with public vs. private health insurance was -4.8 (95%CI: -16.0, 6.3), suggesting that the difference in screening adherence in prevalence banned vs. accessible states between 2022 and 2024 was more pronounced among those with public insurance. The largest adjusted DDD estimates were for AI/AN, multiracial, or other race, NH vs. white, NH (-13.2; 95%CI: -36.9, 10.4), followed by Hispanic, any race vs. white, NH (-10.5; 95%CI: -28.2, 7.1). Full DDD results are presented in Supplemental Table 3.3 (complemented by Supplemental Figures 3.5A-3.5E).

Discussion

The detrimental effects of the *Dobbs* decision on health and care access in the US have been widely documented, but the vast majority of this work has focused on abortion-, maternal-, or pregnancy-adjacent outcomes. The present work expands the literature by investigating the potential consequences of abortion bans on CC screening adherence. Using national survey data, we found that states across the US experienced an increase in screening adherence prevalence from 2022 to 2024. However, our findings also suggest that states which enacted severe and immediate abortion bans may have experienced a lesser increase in adherence and that populations which face systemic barriers to accessing screening may be more acutely impacted, though results were nonsignificant. This work should be considered early and preliminary, and we encourage other investigators to explore this relationship using other data sources, methods, and designs.

There are a number of reasons why we may not have found clear evidence of *Dobbs'* impact. CC screening is recommended every 3-5 years for the majority of the population based on USPSTF guidelines;[156] this observation window which only extended two years post-*Dobbs* may have been too short to detect a meaningful change. Future work might consider incorporating 2026 BRFSS data once available, or looking to alternative sources, such as MarketScan or claims data within healthcare systems, to implement other quasi-experimental designs, such as interrupted time-series or synthetic control. Additionally, CC screening in BRFSS is ascertained through a series of self-reported questions, which may overestimate adherence and be differential by state abortion status.[174] Further, our main analysis restricted to those over

30 years; analyses investigating those 21-29 years might detect more pronounced results, as this population accounts for over half of abortions.[175] In fact, our sensitivity analysis incorporating those between 25-29 years did demonstrate a stronger, though nonsignificant, DID effect compared to the main analysis. USPSTF recommends initiating CC screening at age 21, but publicly available BRFSS data aggregates ages 18-24, preventing us from investigating this population. These highlight opportunities for future work to more precisely evaluate *Dobbs'* impact on CC screening.

While no other studies have empirically examined the influence of *Dobbs* on cancer screening uptake, other studies investigating the influence on abortion restrictions on a range of outcomes provide context. Abraha et al. examined the relationship between post-*Dobbs* abortion bans and pregnancy-associated mortality using synthetic control methods. While all states experienced a decline post-*Dobbs*, pregnancy-associated mortality declined less in states that banned abortion, though this was nonsignificant.[176] Authors attributed these results partially to their post-ban period being short (through end of 2023). This study and our own demonstrate that disentangling the effects of abortion restrictions from the COVID-19 pandemic is challenging, as all states were experiencing a post-COVID rebound in health outcomes around the time of the *Dobbs* decision. A recent study from Ganguly et al. demonstrated a significant decrease in residency applications in abortion-restricted states with strongest effects in abortion-related specialties which perform CC screening (obstetrics/gynecology, family medicine, internal medicine).[139] Healthcare workforce changes could be a plausible mechanism linking *Dobbs* to screening adherence in abortion-restricted states. In the context of previous policies aiming to restrict abortion

through restriction of public funds and clinic closures, Ellison et al. demonstrated that among lower-income women aged 18-45 years in Ohio, each additional 10 minutes of driving to a family planning clinic was associated with 6.5 percentage point decrease in CC screening, though this result was nonsignificant.[177] Clinic closures post-*Dobbs* have been concentrated in states with total bans, aligning with the directionality of our results.[178]

We observed some, though preliminary and imprecise, evidence that *Dobbs* may influence CC screening differentially across populations. Decades of US-based research have demonstrated that abortion bans, and health policies more broadly, often impact the most marginalized populations that already face barriers to accessing essential healthcare services, perpetuating inequities.[146] In a DID study of female Medicaid enrollees, Baser et al. found that only those in the lowest socioeconomic status tertile, based on zip code, experienced an increase in postpartum depression rates pre- to post-*Dobbs*.[147] Studies investigating the influence of Medicaid expansion under the Affordable Care Act (ACA) provide evidence that generous health policies can also more positively impact marginalized groups. Using DDD methods to investigate Medicaid expansion's impact on postpartum insurance access, Boggs et al. demonstrated that while coverage increased across the entire study population, rural areas experienced the greatest increase when comparing states that did versus did not expand.[171] Similar results have been shown in the general population.[148] Taken together with existing CC screening disparities, the present study underscores the need for continued surveillance of trends, and development of targeted interventions to prevent the widening of disparities.[14]

State-level post-*Dobbs* abortion access status is complex, and has been operationalized in a range of ways, differing depending on study outcome, design, and goals. In an analysis investigating changes in obstetrics-related 9-1-1 activations within 3 months of *Dobbs*, Powell et al. classified states into 3 abortion policy groups (protective, mixed, and restrictive), explaining that due to the acute nature of the outcome, any gradient in abortion restrictiveness would be important to explore.[179] Other work has defined abortion status as complete or 6-week bans as exposed to study pregnancy-associated mortality,[176] and presence of any new abortion restrictions between June and October 2022 as exposed to study medical residency application trends.[139] In our main analysis, we took a fairly stringent approach, classifying states as banned only if they implemented a complete or 6-week abortion ban within two months of *Dobbs* that remained through the study period (end of 2024) in an attempt to maintain exposure consistency, especially important in quasi-experimental designs. We also excluded states if they expanded Medicaid within three years of *Dobbs*. While this unfortunately decreased our sample size, we felt it was necessary to ensure we were not capturing and thus misattributing changes in screening that were actually a result of state-level Medicaid expansion, enhancing the internal validity of this work.[158,159] However, of note, our results and inferences were robust to varying state exposure status in sensitivity analyses, providing reassurance.

This study had a number of strengths. BRFSS provided access to US-wide information on CC screening adherence over a number of years. As this is a population-based weighted survey, results are more representative of the general US population than if we had used data from a convenience sample or a healthcare-based data

source. Additionally, restricting our main analysis period to 2022/2024 minimized the introduction of time-varying, group-varying confounding, as it is less likely that there would have been considerable state-level demographic or political changes (beyond abortion bans) in this short timeframe. Finally, the use of DID, as opposed to an uncontrolled pre-post design, intentional exposure definitions, and implementation of sensitivity analyses strengthened our findings.

This study also had limitations. Because BRFSS is a serial cross-sectional survey, 2022 and 2024 populations are not the same respondents, preventing individual-level assessment of screening changes over time. Further, BRFSS changed CC screening adherence questions from 2020 to 2022 , limiting our ability to establish a pre-*Dobbs* screening trend which could have strengthened our DID design (Supplemental Tables 3.4 and 3.5).[173] BRFSS also changed how it ascertained sex from 2022 to 2024, likely leading to misclassification and exclusion of nonbinary and trans individuals, making our results less generalizable to these populations. This is particularly concerning as nonbinary and trans individuals face significant barriers to CC screening.[180] We were unable to control for a number of unmeasured individual-level healthcare access variables, as well as unmeasured state- or local-level time-varying factors, such as local public health department initiatives to increase cancer screening awareness. Finally, there are considerations around exposure consistency; for example, we assumed that abortion bans in all banned states are the same ‘version’, with consistent impacts on screening adherence.

Conclusion

This work contributes to the growing evidence around the far-reaching effects of the *Dobbs* decision beyond abortion, reproductive, and pregnancy-related outcomes. Using a DID design with national survey data, we found that abortion bans post-*Dobbs* may be associated with declining CC screening adherence, and that this association may vary by population, though these results should be interpreted as preliminary. These findings underscore the need for future research that both investigates CC screening over additional years post-*Dobbs* alongside the potential 'spillover' effects on other preventive services critical to population health.

Tables and Figures

Table 3.1. Participant characteristics by post-Dobbs state-level abortion access status and BRFSS survey year, presented as N (%). N values are unweighted, and percentages are BRFSS-weighted for the respective survey year

	2022 (N = 11,365)		2024 (N = 30,045)	
	Accessible states N = 10,048	Banned states N = 1,317	Accessible states N = 26,647	Banned states N = 3,398
Age group, years				
30-34	2325 (31.1)	332 (32.1)	6138 (28.5)	819 (31.1)
35-39	2568 (22.6)	380 (27.0)	6867 (24.1)	876 (24.1)
40-44	2707 (26.8)	345 (25.5)	7305 (27.3)	884 (26.2)
45-49	2448 (19.5)	260 (15.4)	6337 (20.1)	819 (18.7)
Race/ethnicity				
AI/AN only, NH	173 (1.1)	11 (0.4)	405 (0.9)	37 (0.9)
Asian only, NH	583 (8.9)	26 (2.9)	1310 (11.4)	51 (2.3)
Black only, NH	859 (11.2)	354 (30.4)	2311 (10.3)	864 (30.0)
Hispanic, any race	1358 (16.2)	77 (8.3)	4777 (23.6)	267 (11.7)
Multiracial or other race, NH	340 (3.5)	21 (2.1)	1160 (4.2)	99 (3.6)
NHPI only, NH	79 (0.6)	4 (0.6)	148 (0.4)	8 (0.5)
White only, NH	6414 (56.2)	797 (52.5)	16121 (47.5)	2038 (50.0)
Don’t know/not sure/refused	242 (2.3)	27 (2.7)	415 (1.6)	34 (1.0)
Household income group, $				
<25k	979 (10.3)	205 (16.3)	3214 (12.7)	559 (16.6)
25k to <50k	1608 (17.5)	297 (23.7)	4483 (17.2)	745 (22.7)
50k to <75k	1228 (12.9)	182 (13.6)	3110 (9.9)	467 (13.2)
75k to <100k	1351 (12.4)	150 (11.4)	3140 (11.6)	385 (10.0)
100k to <150k	1675 (15.1)	173 (11.3)	4148 (13.8)	484 (13.7)
150k or more	1916 (17.3)	127 (10.5)	5639 (21.6)	435 (13.5)
Don't know/not sure/refused	1291 (14.5)	183 (13.1)	2913 (13.2)	323 (10.2)
Educational attainment				
Did not graduate HS	484 (9.4)	70 (9.9)	1678 (10.8)	204 (10.6)
Graduated HS	1508 (18.2)	261 (22.4)	4502 (18.1)	706 (22.8)
Attended college or technical school	2327 (27.8)	375 (32.6)	5749 (25.0)	960 (31.9)
Graduated from college or technical school	5702 (44.5)	610 (35)	14615 (45.5)	1519 (34.6)
Don't know/not sure/missing	27 (0.2)	1 (0.0)	103 (0.5)	9 (0.2)
Healthcare coverage primary source				
Private	6816 (65.1)	781 (59.5)	16469 (59.3)	2007 (56.3)
Public	2531 (26.6)	397 (28.4)	7386 (29.5)	1018 (30.1)
No coverage	522 (5.8)	116 (10.1)	2094 (8.3)	312 (11.9)
Don't know/not sure/refused	179 (2.4)	23 (2.0)	698 (2.9)	61 (1.8)
County metropolitan status				
Urban	9204 (96.2)	1083 (87.3)	24451 (96.7)	2800 (88.0)
Rural	841 (3.8)	234 (12.7)	2189 (3.3)	598 (12.0)
Not defined/missing	3 (0.0)	0 (0.0)	7 (0.0)	0 (0.0)

Abbreviations: AI/AN, American Indian Alaska Native; HS, high school; NH, Non-Hispanic; NHPI, Native Hawaiian Pacific Islander

Figure 3.1. Main analysis state exposure (abortion access post-*Dobbs*) classifications

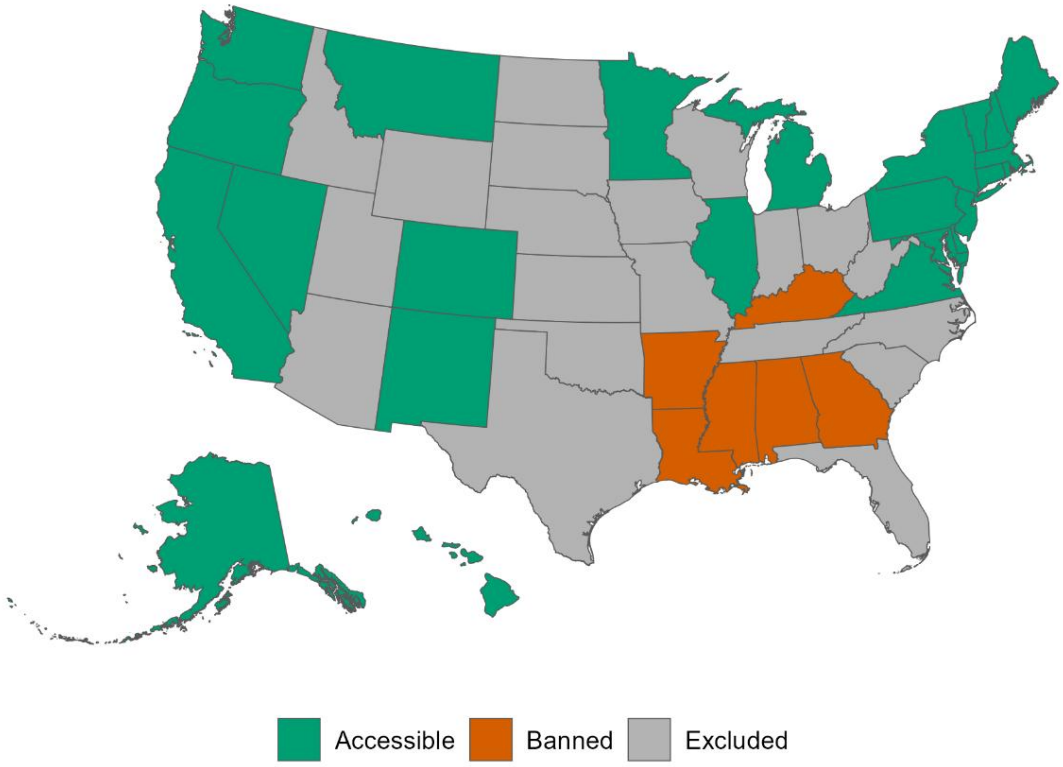


Figure 3.2. Study timeline for difference-in-differences (DID) and difference-in-difference-in-differences (DDD) design for main analysis

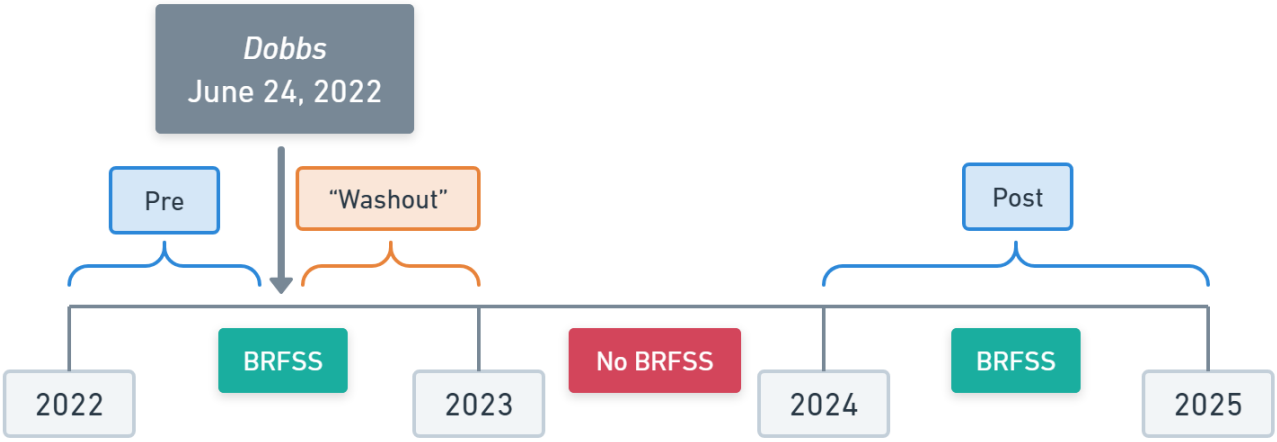
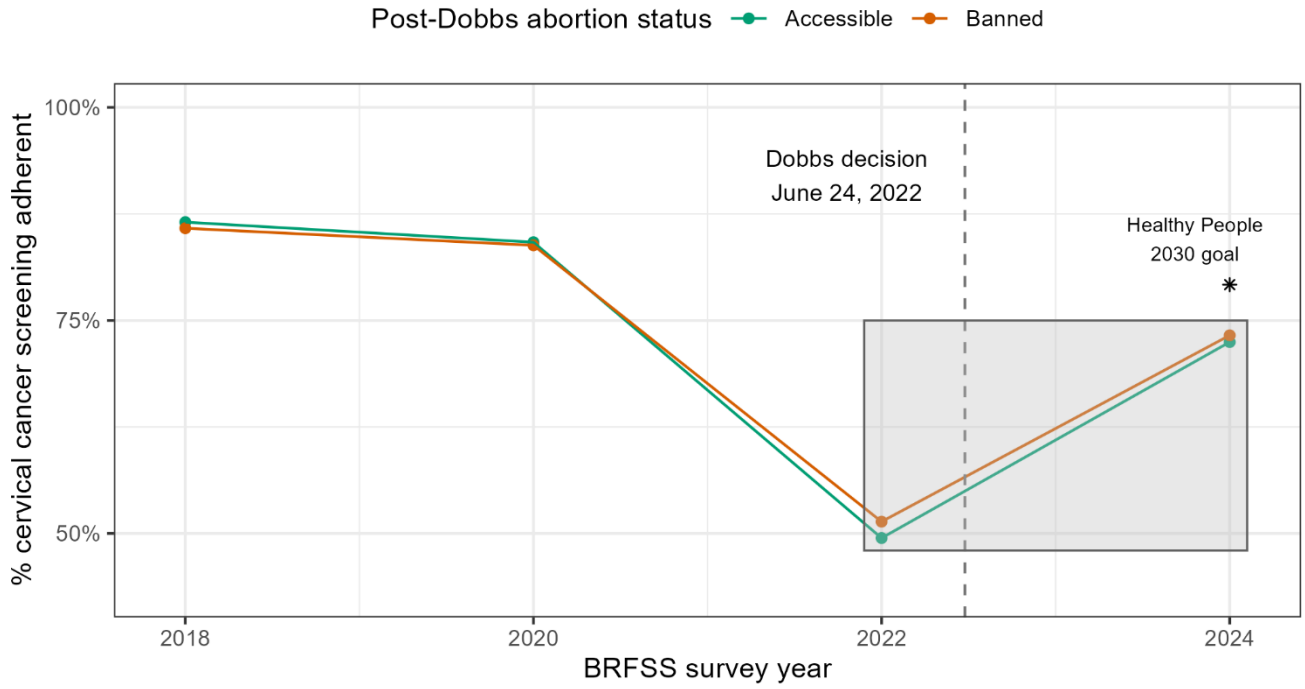


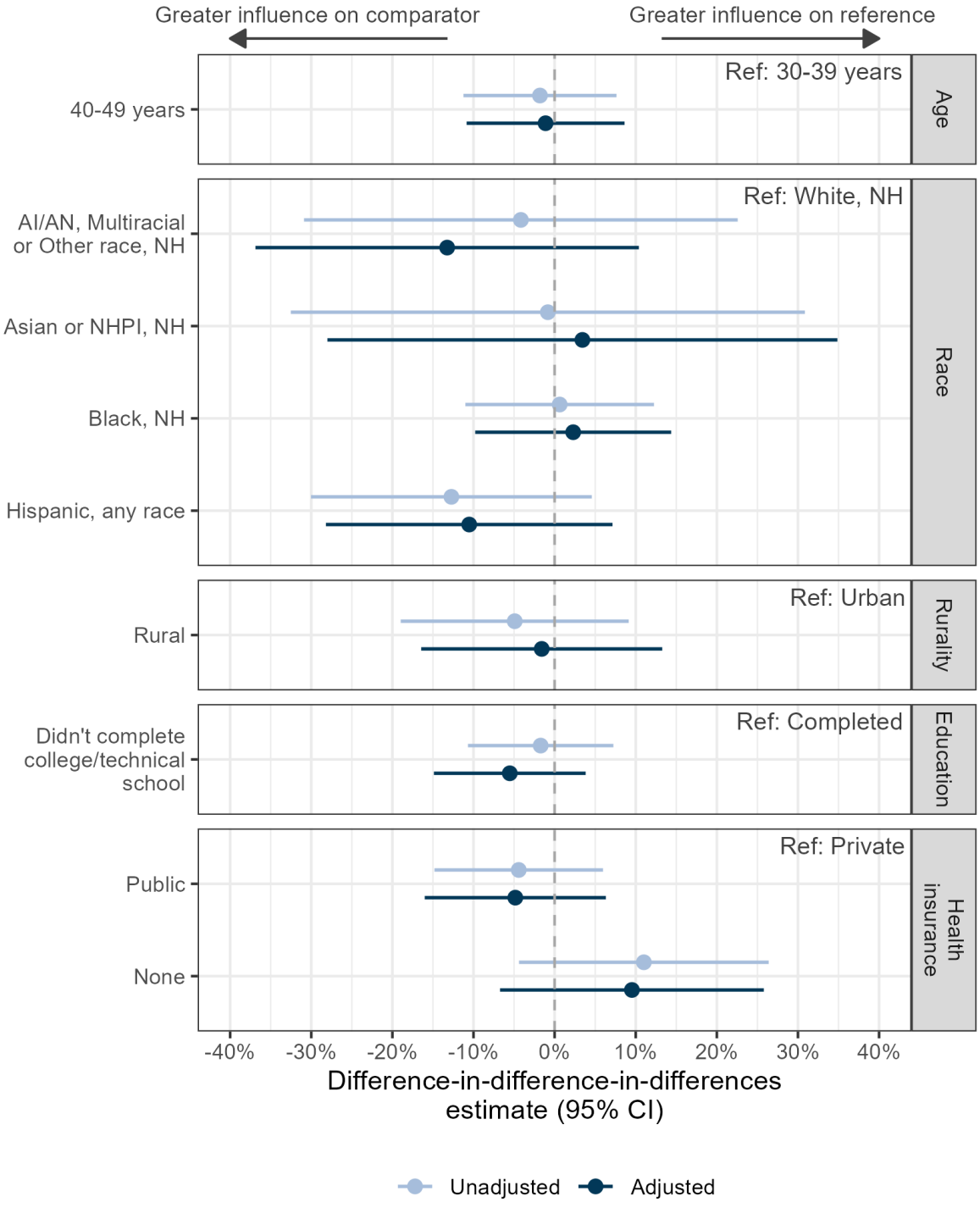
Figure 3.3. BRFSS-weighted cervical cancer screening adherence prevalence by post-Dobbs abortion access status for main analysis. The gray box indicates the time period of interest for the difference-in-differences (DID) analysis. Main DID model results are presented in the table. The dashed vertical line marks the *Dobbs* decision on June 24, 2022. Healthy People 2030 target goal of 79.2% "proportion of females who get screened for cervical cancer" included for reference[181]



Model type	Main analysis DID estimates (95%CI)
Unadjusted	-0.71 (-5.37, 3.95)
Adjusted, minimal	-1.43 (-5.98, 3.11)
Adjusted, full	-2.20 (-7.02, 2.62)

Only participants who completed their BRFSS survey prior to June 24, 2022 included in the 2022 BRFSS year data

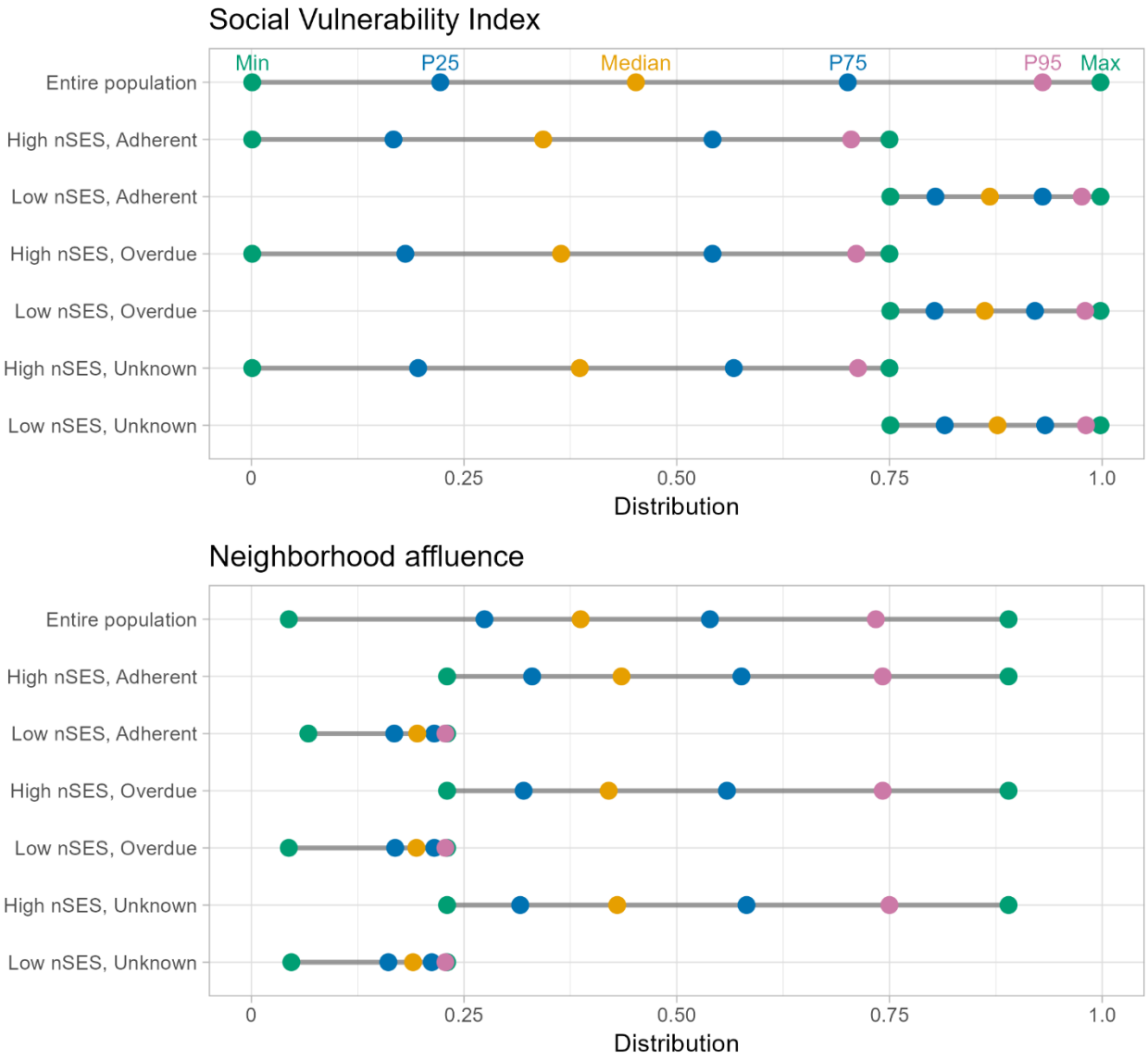
Figure 3.4. Difference-in-difference-in-differences (DDD) analysis results. DDD estimates and corresponding 95%CIs below are the triple-interaction term included in DDD models. Adjusted models controlled for age, health insurance status, county metropolitan status, education level, and annual household income, with the DDD characteristic of interest withheld (e.g., age was not adjusted for in the age DDD models, but the four other variables listed were)



Supplementary Materials

Chapter 1 Supplementary Tables and Figures

Supplemental Figure 1.1. Continuous distribution of neighborhood socioeconomic status (nSES) effect modifiers for the entire STEP study population, and stratified by screening history and effect modifier stratum. Dots represent minimum, 25th percentile, median, 75th percentile, 95th percentile, and maximum of the continuous effect modifier within the corresponding group. Higher Social Vulnerability Index represents lower nSES, while lower neighborhood affluence represents lower nSES



Supplemental Table 1.1A. Baseline characteristics of adherent participants by STEP randomization arm and social vulnerability index (SVI) level, N (%)

	Low SVI (high nSES)			High SVI (low nSES)		
	UC/EDU N = 6290	Opt-in N = 3213	Direct-Mail N = 1194	UC/EDU N = 1313	Opt-in N = 736	Direct-Mail N = 279
Age at randomization, years	6290	3213	1194	1313	736	279
30-34	962 (15.3)	526 (16.4)	146 (12.2)	234 (17.8)	137 (18.6)	52 (18.6)
35-39	967 (15.4)	450 (14.0)	158 (13.2)	208 (15.8)	115 (15.6)	36 (12.9)
40-44	870 (13.8)	459 (14.3)	172 (14.4)	206 (15.7)	113 (15.4)	37 (13.3)
45-49	830 (13.2)	450 (14.0)	186 (15.6)	183 (13.9)	97 (13.2)	50 (17.9)
50-54	820 (13.0)	402 (12.5)	192 (16.1)	161 (12.3)	95 (12.9)	24 (8.6)
55-59	885 (14.1)	456 (14.2)	179 (15.0)	187 (14.2)	90 (12.2)	38 (13.6)
60-64	956 (15.2)	470 (14.6)	161 (13.5)	134 (10.2)	89 (12.1)	42 (15.1)
Race	5907	3054	1117	1232	697	258
Asian	682 (11.4)	384 (12.6)	142 (12.5)	180 (14.6)	98 (14.0)	33 (12.4)
Black	216 (3.6)	105 (3.4)	39 (3.4)	147 (11.9)	58 (8.3)	34 (12.7)
More than one race or another race	417 (6.9)	216 (7.1)	58 (5.1)	128 (10.3)	70 (10.0)	29 (10.9)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	100 (1.7)	39 (1.3)	17 (1.5)	45 (3.6)	18 (2.6)	7 (2.6)
White	4492 (74.8)	2310 (75.5)	861 (75.6)	732 (59.2)	453 (64.8)	155 (58.1)
Ethnicity	5743	2983	1095	1198	682	252
Hispanic	392 (6.7)	206 (6.9)	59 (5.3)	104 (8.6)	60 (8.8)	23 (8.8)
Non-Hispanic	5351 (91.6)	2777 (92.9)	1036 (92.7)	1094 (90.9)	622 (90.9)	229 (87.7)
BMI, kg/m²	5900	3041	1110	1245	706	259
≤24.9	2035 (33.9)	1021 (33.5)	397 (35.1)	315 (25.2)	198 (28.0)	65 (24.3)
25-29.9	1674 (27.9)	889 (29.2)	300 (26.5)	310 (24.8)	193 (27.3)	76 (28.4)
30-34.9	1040 (17.3)	543 (17.8)	193 (17.0)	285 (22.8)	137 (19.4)	53 (19.8)
35-39.9	585 (9.8)	300 (9.8)	109 (9.6)	161 (12.9)	76 (10.7)	34 (12.7)
≥40	566 (9.4)	288 (9.5)	111 (9.8)	174 (13.9)	102 (14.4)	31 (11.6)
Tobacco use	5907	3041	1119	1243	710	261
Never	4264 (72.2)	2185 (71.9)	822 (73.5)	891 (71.7)	483 (68.0)	181 (69.3)
Current or former	1643 (27.8)	856 (28.1)	297 (26.5)	352 (28.3)	227 (32.0)	80 (30.7)
Length enrolled on health plan, years	5907	3041	1119	1243	710	261
<3.25	1793 (28.5)	923 (28.7)	371 (31.1)	424 (32.3)	227 (30.8)	80 (28.7)
3.25 to <5	817 (13.0)	421 (13.1)	186 (15.6)	170 (12.9)	113 (15.4)	42 (15.1)
5 to <10	1718 (27.3)	830 (25.8)	247 (20.7)	356 (27.1)	203 (27.6)	61 (21.9)
≥10	1962 (31.2)	1039 (32.3)	390 (32.7)	363 (27.6)	193 (26.2)	96 (34.4)
Travel time to clinic, minutes	6191	3207	1172	1308	734	270
<10	1903 (30.7)	1040 (32.4)	380 (32.4)	518 (39.6)	276 (37.6)	101 (37.4)
10 to <20	2381 (38.5)	1181 (36.8)	439 (37.5)	411 (31.4)	241 (32.8)	81 (30.0)
20 to <30	979 (15.8)	513 (16.0)	188 (16.0)	170 (13.0)	83 (11.3)	40 (14.8)
≥30	928 (15.0)	473 (14.7)	165 (14.1)	209 (16.0)	134 (18.3)	48 (17.8)
Charlson comorbidity index	6192	3208	1172	1308	734	270
0	5203 (82.7)	2706 (84.2)	971 (81.3)	1043 (79.4)	582 (79.1)	205 (73.5)
≥1	989 (15.7)	502 (15.6)	201 (16.8)	265 (20.2)	152 (20.7)	65 (23.3)

Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Table 1.1B. Baseline characteristics of overdue participants by trial arm and social vulnerability index (SVI) level, N (%)

	Low SVI (high nSES)		High SVI (low nSES)	
	UC/EDU N = 5496	Direct-Mail N = 1136	UC/EDU N = 1377	Direct-Mail N = 273
Age at randomization, years	5496	1136	1377	273
30-34	752 (13.7)	149 (13.1)	227 (16.5)	38 (13.9)
35-39	711 (12.9)	131 (11.5)	187 (13.6)	39 (14.3)
40-44	736 (13.4)	153 (13.5)	207 (15.0)	37 (13.6)
45-49	764 (13.9)	141 (12.4)	191 (13.9)	40 (14.7)
50-54	833 (15.2)	182 (16.0)	195 (14.2)	50 (18.3)
55-59	858 (15.6)	203 (17.9)	184 (13.4)	41 (15.0)
60-64	842 (15.3)	177 (15.6)	186 (13.5)	28 (10.3)
Race	4708	984	1168	222
Asian	513 (10.9)	118 (11.8)	152 (13.0)	33 (14.7)
Black	196 (4.2)	35 (3.5)	114 (9.7)	24 (10.7)
More than one race or another race	321 (6.8)	76 (7.6)	104 (8.9)	24 (10.7)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	104 (2.2)	15 (1.5)	36 (3.1)	7 (3.1)
White	3574 (75.7)	740 (74.2)	762 (65.1)	134 (59.6)
Ethnicity	4292	888	1058	212
Hispanic	254 (5.9)	56 (6.2)	76 (7.2)	15 (7.0)
Non-Hispanic	4038 (93.8)	832 (92.3)	982 (92.6)	197 (91.6)
BMI, kg/m²	3629	754	931	198
<24.9	1016 (27.9)	204 (26.6)	215 (23.0)	37 (18.4)
25-29.9	931 (25.6)	191 (24.9)	228 (24.4)	54 (26.9)
30-34.9	732 (20.1)	160 (20.9)	190 (20.3)	44 (21.9)
35-39.9	460 (12.6)	100 (13.0)	131 (14.0)	25 (12.4)
≥40	490 (13.5)	99 (12.9)	167 (17.9)	38 (18.9)
Tobacco use	3802	775	954	199
Never	2583 (67.9)	514 (66.3)	609 (63.8)	129 (64.8)
Current or former	1219 (32.1)	261 (33.7)	345 (36.2)	70 (35.2)
Length enrolled on health plan, years	5496	1136	1377	273
<3.25	1742 (31.7)	390 (34.3)	462 (33.6)	102 (37.4)
3.25 to <5	1353 (24.6)	270 (23.8)	326 (23.7)	61 (22.3)
5 to <10	1305 (23.7)	244 (21.5)	357 (25.9)	67 (24.5)
≥10	1096 (19.9)	232 (20.4)	232 (16.8)	43 (15.8)
Length of time overdue, years	5496	1136	1377	273
<3	2252 (41.0)	455 (40.1)	554 (40.2)	116 (42.5)
≥3	1602 (29.1)	359 (31.6)	365 (26.5)	83 (30.4)
No prior screen	1642 (29.9)	322 (28.3)	458 (33.3)	74 (27.1)
Travel time to clinic, minutes	5482	1123	1374	270
<10	1650 (30.1)	351 (31.3)	551 (40.1)	104 (38.5)
10 to <20	2094 (38.2)	440 (39.2)	459 (33.4)	85 (31.5)
20 to <30	875 (16.0)	154 (13.7)	169 (12.3)	39 (14.4)
≥30	863 (15.7)	178 (15.9)	195 (14.2)	42 (15.6)
Charlson comorbidity index	5483	1123	1374	270
0	4856 (88.4)	979 (86.2)	1181 (85.8)	228 (83.5)
≥1	1642 (29.9)	322 (28.3)	458 (33.3)	74 (27.1)

Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Table 1.1C. Baseline characteristics of unknown participants by trial arm and social vulnerability index (SVI) level, N (%)

	Low SVI (high nSES)		High SVI (low nSES)	
	UC/EDU N = 4848	Opt-in N = 2696	UC/EDU N = 1588	Opt-in N = 797
Age at randomization, years	4848	2696	1588	797
30-34	1177 (24.3)	673 (25.0)	422 (26.6)	198 (24.8)
35-39	868 (17.9)	465 (17.2)	254 (16.0)	143 (17.9)
40-44	659 (13.6)	369 (13.7)	212 (13.4)	121 (15.2)
45-49	563 (11.6)	320 (11.9)	202 (12.7)	99 (12.4)
50-54	579 (11.9)	309 (11.5)	191 (12.0)	89 (11.2)
55-59	531 (11.0)	301 (11.2)	180 (11.3)	74 (9.3)
60-64	471 (9.7)	259 (9.6)	127 (8.0)	73 (9.2)
Race	2904	1658	901	473
Asian	510 (17.4)	294 (17.7)	178 (19.6)	84 (17.7)
Black	170 (5.8)	73 (4.4)	120 (13.2)	79 (16.7)
More than one race or another race	220 (7.5)	111 (6.7)	87 (9.6)	36 (7.6)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	75 (2.6)	35 (2.1)	38 (4.2)	18 (3.8)
White	1929 (65.9)	1145 (69.0)	478 (52.5)	256 (54.0)
Ethnicity	1589	921	520	271
Hispanic	215 (13.3)	116 (12.6)	88 (16.6)	38 (14.0)
Non-Hispanic	1374 (85.3)	805 (87.3)	432 (81.7)	233 (85.7)
BMI, kg/m²	2047	1188	662	340
<24.9	620 (30.0)	366 (30.8)	167 (24.9)	69 (20.2)
25-29.9	547 (26.4)	294 (24.7)	156 (23.2)	82 (24.0)
30-34.9	401 (19.4)	226 (19.0)	123 (18.3)	77 (22.6)
35-39.9	218 (10.5)	148 (12.4)	106 (15.8)	52 (15.2)
≥40	261 (12.6)	154 (13.0)	110 (16.4)	60 (17.6)
Tobacco use	2259	1315	721	369
Never	1632 (72.2)	914 (69.5)	470 (65.2)	259 (70.2)
Current or former	627 (27.8)	401 (30.5)	251 (34.8)	110 (29.8)
Travel time to clinic, minutes	4826	2694	1578	796
<10	1771 (36.7)	973 (36.1)	761 (48.2)	358 (45.0)
10 to <20	1741 (36.1)	978 (36.3)	521 (33.0)	276 (34.7)
20 to <30	665 (13.8)	376 (14.0)	136 (8.6)	73 (9.2)
≥30	649 (13.4)	367 (13.6)	160 (10.1)	89 (11.2)
Charlson comorbidity index	4826	2695	1579	796
0	4481 (92.4)	2490 (92.4)	1419 (89.4)	724 (90.8)
≥1	345 (7.1)	205 (7.6)	160 (10.1)	72 (9.0)

Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Table 1.2A. Baseline characteristics of adherent participants by STEP randomization arm and neighborhood affluence level, N (%)

	High Affluence (high nSES)			Low Affluence (low SES)		
	UC/EDU N = 6601	Opt-in N = 3416	Direct-Mail N = 1272	UC/EDU N = 1002	Opt-in N = 533	Direct-Mail N = 201
Age at randomization, years	6601	3416	1272	1002	533	201
30-34	1020 (15.5)	569 (16.7)	164 (12.9)	176 (17.6)	94 (17.6)	34 (16.9)
35-39	1021 (15.5)	498 (14.6)	169 (13.3)	154 (15.4)	67 (12.6)	25 (12.4)
40-44	929 (14.1)	495 (14.5)	185 (14.5)	147 (14.7)	77 (14.4)	24 (11.9)
45-49	877 (13.3)	469 (13.7)	202 (15.9)	136 (13.6)	78 (14.6)	34 (16.9)
50-54	856 (13.0)	427 (12.5)	194 (15.3)	125 (12.5)	70 (13.1)	22 (10.9)
55-59	931 (14.1)	482 (14.1)	186 (14.6)	141 (14.1)	64 (12.0)	31 (15.4)
60-64	967 (14.6)	476 (13.9)	172 (13.5)	123 (12.3)	83 (15.6)	31 (15.4)
Race	6200	3245	1194	939	506	181
Asian	764 (12.1)	433 (13.3)	159 (13.0)	98 (10.3)	49 (9.7)	16 (8.6)
Black	275 (4.4)	134 (4.1)	53 (4.3)	88 (9.3)	29 (5.7)	20 (10.7)
More than one race or another race	474 (7.5)	234 (7.2)	74 (6.1)	71 (7.5)	52 (10.3)	13 (7.0)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	107 (1.7)	40 (1.2)	17 (1.4)	38 (4.0)	17 (3.4)	7 (3.7)
White	4580 (72.8)	2404 (73.9)	891 (73.1)	644 (67.9)	359 (70.9)	125 (66.8)
Ethnicity	6025	3170	1171	916	495	176
Hispanic	425 (6.9)	215 (6.8)	70 (5.9)	71 (7.7)	51 (10.3)	12 (6.6)
Non-Hispanic	5600 (91.5)	2955 (93.0)	1101 (92.1)	845 (91.4)	444 (89.7)	164 (90.1)
BMI, kg/m²	6202	3236	1185	943	511	184
≤24.9	2155 (34.2)	1093 (33.7)	425 (35.1)	195 (20.5)	126 (24.7)	37 (19.5)
25-29.9	1764 (28.0)	955 (29.4)	323 (26.7)	220 (23.1)	127 (24.9)	53 (27.9)
30-34.9	1094 (17.4)	569 (17.5)	207 (17.1)	231 (24.3)	111 (21.7)	39 (20.5)
35-39.9	599 (9.5)	312 (9.6)	113 (9.3)	147 (15.4)	64 (12.5)	30 (15.8)
≥40	590 (9.4)	307 (9.5)	117 (9.7)	150 (15.8)	83 (16.2)	25 (13.2)
Tobacco use	6208	3238	1195	942	513	185
Never	4521 (72.8)	2358 (72.8)	885 (74.1)	634 (67.3)	310 (60.4)	118 (63.8)
Current or former	1687 (27.2)	880 (27.2)	310 (25.9)	308 (32.7)	203 (39.6)	67 (36.2)
Length enrolled on health plan, years	6601	3416	1272	1002	533	201
<3.25	1901 (28.8)	1000 (29.3)	387 (30.4)	316 (31.5)	150 (28.1)	64 (31.8)
3.25 to <5	877 (13.3)	461 (13.5)	199 (15.6)	110 (11.0)	73 (13.7)	29 (14.4)
5 to <10	1788 (27.1)	885 (25.9)	269 (21.1)	286 (28.5)	148 (27.8)	39 (19.4)
≥10	2035 (30.8)	1070 (31.3)	417 (32.8)	290 (28.9)	162 (30.4)	69 (34.3)
Travel time to clinic, minutes	6506	3409	1247	993	532	195
<10	2077 (31.9)	1134 (33.3)	409 (32.8)	344 (34.6)	182 (34.2)	72 (36.9)
10 to <20	2472 (38.0)	1280 (37.5)	466 (37.4)	320 (32.2)	142 (26.7)	54 (27.7)
20 to <30	1023 (15.7)	520 (15.3)	196 (15.7)	126 (12.7)	76 (14.3)	32 (16.4)
≥30	934 (14.4)	475 (13.9)	176 (14.1)	203 (20.4)	132 (24.8)	37 (19.0)
Charlson comorbidity index	6507	3409	1247	993	533	195
0	5471 (82.9)	2885 (84.5)	1030 (81.0)	775 (77.3)	403 (75.6)	146 (72.6)
≥1	1036 (15.7)	524 (15.3)	217 (17.1)	218 (21.8)	130 (24.4)	49 (24.4)

Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Table 1.2B. Baseline characteristics of overdue participants by trial arm and neighborhood affluence level, N (%)

	High Affluence (high nSES)		Low Affluence (low SES)	
	UC/EDU N = 5817	Direct-Mail N = 1171	UC/EDU N = 1056	Direct-Mail N = 238
Age at randomization, years	5817	1171	1056	238
30-34	818 (14.1)	152 (13.0)	161 (15.2)	35 (14.7)
35-39	757 (13.0)	140 (12.0)	141 (13.4)	30 (12.6)
40-44	788 (13.5)	153 (13.1)	155 (14.7)	37 (15.5)
45-49	796 (13.7)	153 (13.1)	159 (15.1)	28 (11.8)
50-54	876 (15.1)	184 (15.7)	152 (14.4)	48 (20.2)
55-59	896 (15.4)	207 (17.7)	146 (13.8)	37 (15.5)
60-64	886 (15.2)	182 (15.5)	142 (13.4)	23 (9.7)
Race	4967	1012	909	194
Asian	594 (11.9)	133 (13.0)	71 (7.8)	18 (9.2)
Black	238 (4.8)	47 (4.6)	72 (7.9)	12 (6.1)
More than one race or another race	342 (6.9)	87 (8.5)	83 (9.1)	13 (6.6)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	109 (2.2)	17 (1.7)	31 (3.4)	5 (2.6)
White	3684 (74.0)	728 (71.0)	652 (71.6)	146 (74.5)
Ethnicity	4525	921	825	179
Hispanic	276 (6.1)	60 (6.4)	54 (6.5)	11 (6.1)
Non-Hispanic	4249 (93.6)	861 (92.1)	771 (93.2)	168 (92.8)
BMI, kg/m²	3845	792	715	160
<24.9	1094 (28.3)	210 (26.1)	137 (19.1)	31 (19.1)
25-29.9	990 (25.7)	206 (25.6)	169 (23.6)	39 (24.1)
30-34.9	770 (20.0)	173 (21.5)	152 (21.2)	31 (19.1)
35-39.9	478 (12.4)	100 (12.4)	113 (15.8)	25 (15.4)
≥40	513 (13.3)	103 (12.8)	144 (20.1)	34 (21.0)
Tobacco use	4010	812	746	162
Never	2754 (68.7)	547 (67.4)	438 (58.7)	96 (59.3)
Current or former	1256 (31.3)	265 (32.6)	308 (41.3)	66 (40.7)
Length enrolled on health plan, years	5817	1171	1056	238
<3.25	1847 (31.8)	402 (34.3)	357 (33.8)	90 (37.8)
3.25 to <5	1430 (24.6)	275 (23.5)	249 (23.6)	56 (23.5)
5 to <10	1398 (24.0)	252 (21.5)	264 (25.0)	59 (24.8)
≥10	1142 (19.6)	242 (20.7)	186 (17.6)	33 (13.9)
Length of time overdue, years	5817	1171	1056	238
<3	2383 (41.0)	475 (40.6)	423 (40.1)	96 (40.3)
≥3	1668 (28.7)	367 (31.3)	299 (28.3)	75 (31.5)
No prior screen	1766 (30.4)	329 (28.1)	334 (31.6)	67 (28.2)
Travel time to clinic, minutes	5802	1157	1054	236
<10	1826 (31.5)	373 (32.2)	375 (35.6)	82 (34.7)
10 to <20	2227 (38.4)	452 (39.1)	326 (30.9)	73 (30.9)
20 to <30	894 (15.4)	163 (14.1)	150 (14.2)	30 (12.7)
≥30	855 (14.7)	169 (14.6)	203 (19.3)	51 (21.6)
Charlson comorbidity index	5803	1157	1054	236
0	5153 (88.6)	1002 (85.6)	884 (83.7)	205 (86.1)
≥1	650 (11.2)	155 (13.2)	170 (16.1)	31 (13.0)

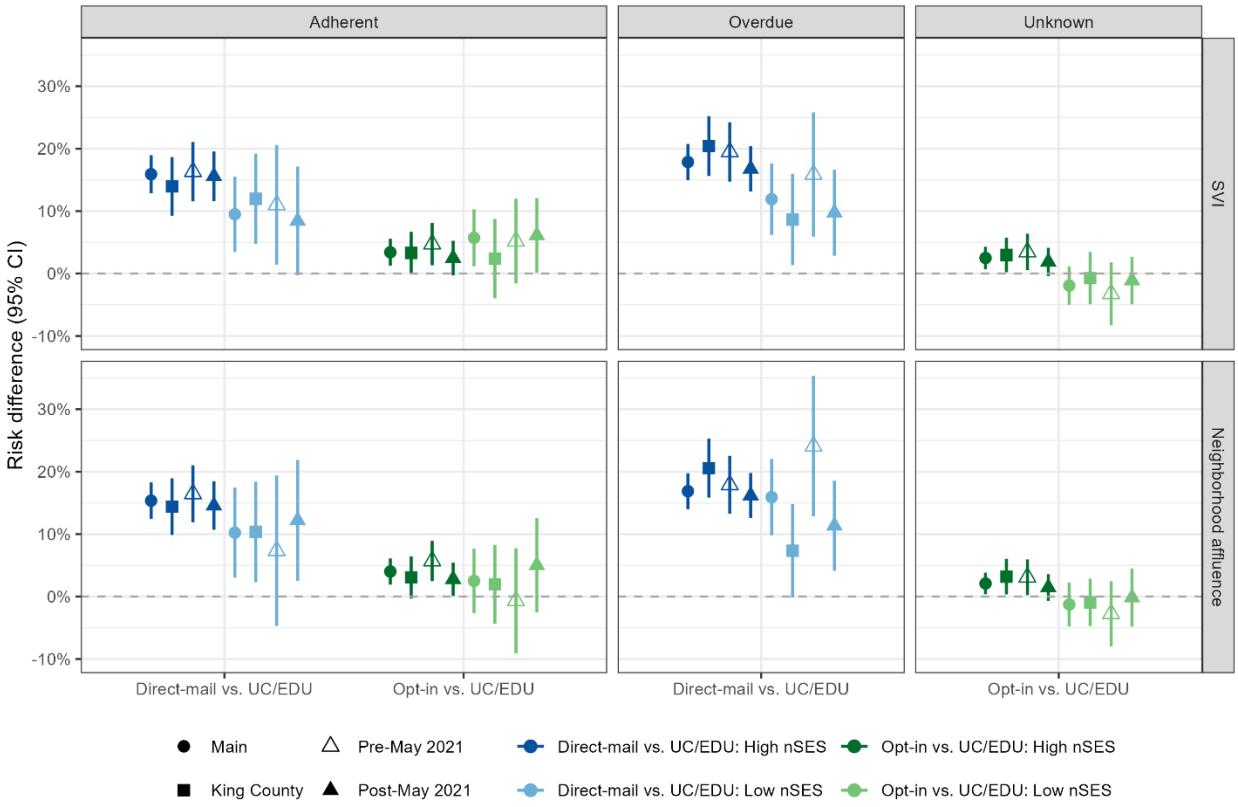
Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Table 1.2C. Baseline characteristics of unknown participants by trial arm and neighborhood affluence level, N (%)

	High Affluence (high nSES)		Low Affluence (low SES)	
	UC/EDU N = 5258	Opt-in N = 2872	UC/EDU N = 1178	Opt-in N = 621
Age at randomization, years	5258	2872	1178	621
30-34	1299 (24.7)	732 (25.5)	300 (25.5)	139 (22.4)
35-39	909 (17.3)	490 (17.1)	213 (18.1)	118 (19.0)
40-44	718 (13.7)	394 (13.7)	153 (13.0)	96 (15.5)
45-49	622 (11.8)	347 (12.1)	143 (12.1)	72 (11.6)
50-54	636 (12.1)	322 (11.2)	134 (11.4)	76 (12.2)
55-59	583 (11.1)	309 (10.8)	128 (10.9)	66 (10.6)
60-64	491 (9.3)	278 (9.7)	107 (9.1)	54 (8.7)
Race	3130	1738	675	393
Asian	620 (19.7)	337 (19.4)	68 (10.0)	41 (10.4)
Black	213 (6.8)	105 (6.0)	77 (11.3)	47 (12.0)
More than one race or another race	236 (7.5)	121 (7.0)	71 (10.4)	26 (6.6)
Native American/Alaska Native or Native Hawaiian/Pacific Islander	80 (2.5)	36 (2.1)	33 (4.8)	17 (4.3)
White	1981 (62.8)	1139 (65.5)	426 (62.6)	262 (66.7)
Ethnicity	1708	957	401	235
Hispanic	241 (13.9)	127 (13.2)	62 (15.2)	27 (11.5)
Non-Hispanic	1467 (84.7)	830 (86.5)	339 (83.3)	208 (88.5)
BMI, kg/m²	2182	1234	527	294
<24.9	677 (30.7)	392 (31.7)	110 (20.6)	43 (14.6)
25-29.9	585 (26.5)	304 (24.6)	118 (22.1)	72 (24.5)
30-34.9	423 (19.2)	229 (18.5)	101 (18.9)	74 (25.2)
35-39.9	233 (10.6)	151 (12.2)	91 (17.1)	49 (16.7)
≥40	264 (12.0)	158 (12.8)	107 (20.1)	56 (19.0)
Tobacco use	2409	1363	571	321
Never	1777 (73.8)	980 (71.9)	325 (56.9)	193 (60.1)
Current or former	632 (26.2)	383 (28.1)	246 (43.1)	128 (39.9)
Travel time to clinic, minutes	5232	2869	1172	621
<10	2055 (39.3)	1088 (37.9)	477 (40.7)	243 (39.1)
10 to <20	1868 (35.7)	1045 (36.4)	394 (33.6)	209 (33.7)
20 to <30	678 (13.0)	375 (13.1)	123 (10.5)	74 (11.9)
≥30	631 (12.1)	361 (12.6)	178 (15.2)	95 (15.3)
Charlson comorbidity index	5233	2870	1172	621
0	4864 (92.5)	2660 (92.6)	1036 (87.9)	554 (89.2)
≥1	369 (7.0)	210 (7.3)	136 (11.5)	67 (10.8)

Abbreviations: SVI, Social Vulnerability Index; nSES, neighborhood socioeconomic status; UC/EDU, usual care + education; BMI, body mass index

Supplemental Figure 1.2. Effectiveness of direct-mail and opt-in intervention by screening history and effect modifier stratum, including all secondary analyses



Chapter 2 Supplementary Tables and Figures

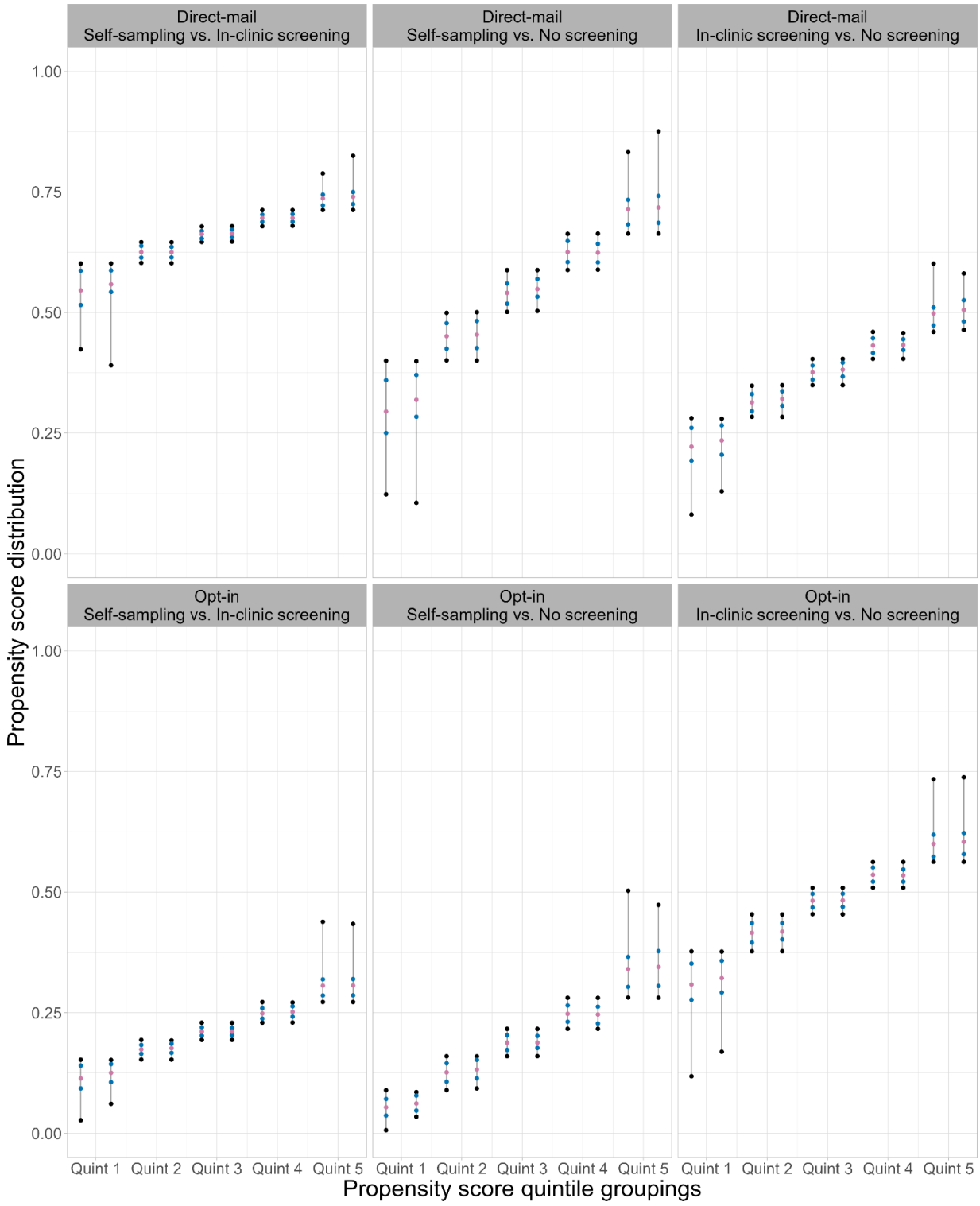
Supplemental Table 2.1. Variables included in final propensity score models for STEP trial arm and each comparison

STEP arm	Exposure comparison	Variables included in propensity score model
Direct-mail	Self-sampling vs. in-clinic screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization (binary, yes/no) • interaction between age and CCI
Direct-mail	Self-sampling vs. No screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • any in-person clinic visit 1 year pre-randomization (binary, yes/no) • sent a MyChart message 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization (binary, yes/no) • interaction between tobacco use and PCP or OGBYN visit 1 year pre-randomization
Direct-mail	In-clinic screening vs. No screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • any in-person clinic visit 1 year pre-randomization (binary, yes/no) • sent a MyChart message 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization (binary, yes/no) • interaction between SVI and receipt of flu vaccine 10 years pre-randomization

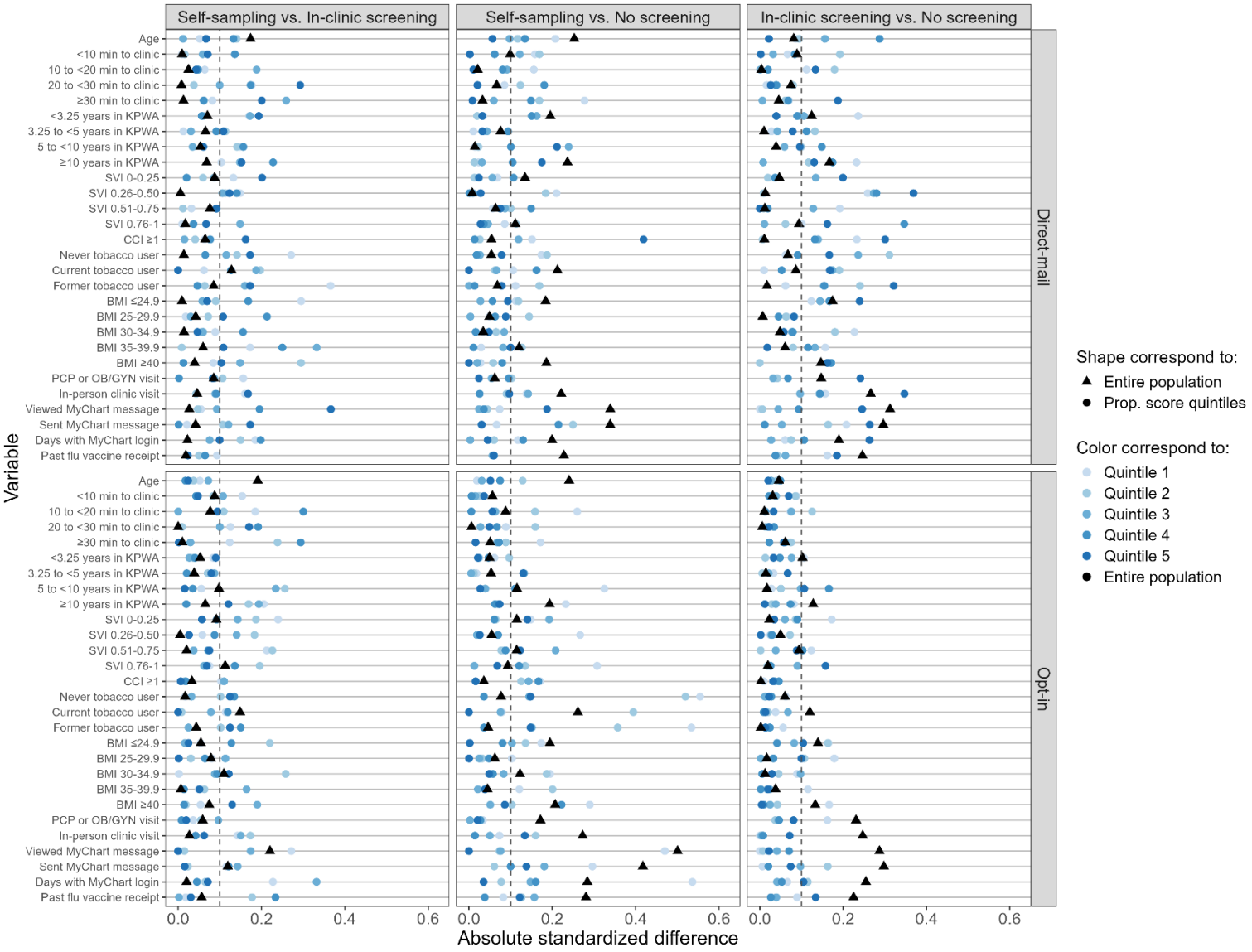
Opt-in	Self-sampling vs. in-clinic screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • any in-person clinic visit 1 year pre-randomization (binary, yes/no) • sent a MyChart message 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization (binary, yes/no)
Opt-in	Self-sampling vs. No screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization s(binary, yes/no)
Opt-in	In-clinic screening vs. No screening	• age (continuous) • SVI (categorical, 4 levels) • travel time to primary care clinic (categorical, 4 levels) • years enrolled in KPWA (categorical, 4 levels) • PCP or OGBYN visit 1 year pre-randomization (binary, yes/no) • any in-person clinic visit 1 year pre-randomization (binary, yes/no) • sent a MyChart message 1 year pre-randomization (binary, yes/no) • viewed a MyChart message 1 year pre-randomization (binary, yes/no) • number of days with a MyChart login 1 year pre-randomization (continuous) • CCI (binary, 0/≥1) • tobacco use (categorical, 3 levels) • BMI (categorical, 5 levels) • receipt of flu vaccine 10 years pre-randomization (binary, yes/no) • interaction between tobacco use and PCP or OGBYN visit 1 year pre-randomization

Abbreviations: BMI, body mass index; CCI, Charlson comorbidity index; KPWA, Kaiser Permanente Washington; OBGYN, obstetrician-gynecologist; PCP, primary care provider; SVI, Social Vulnerability Index

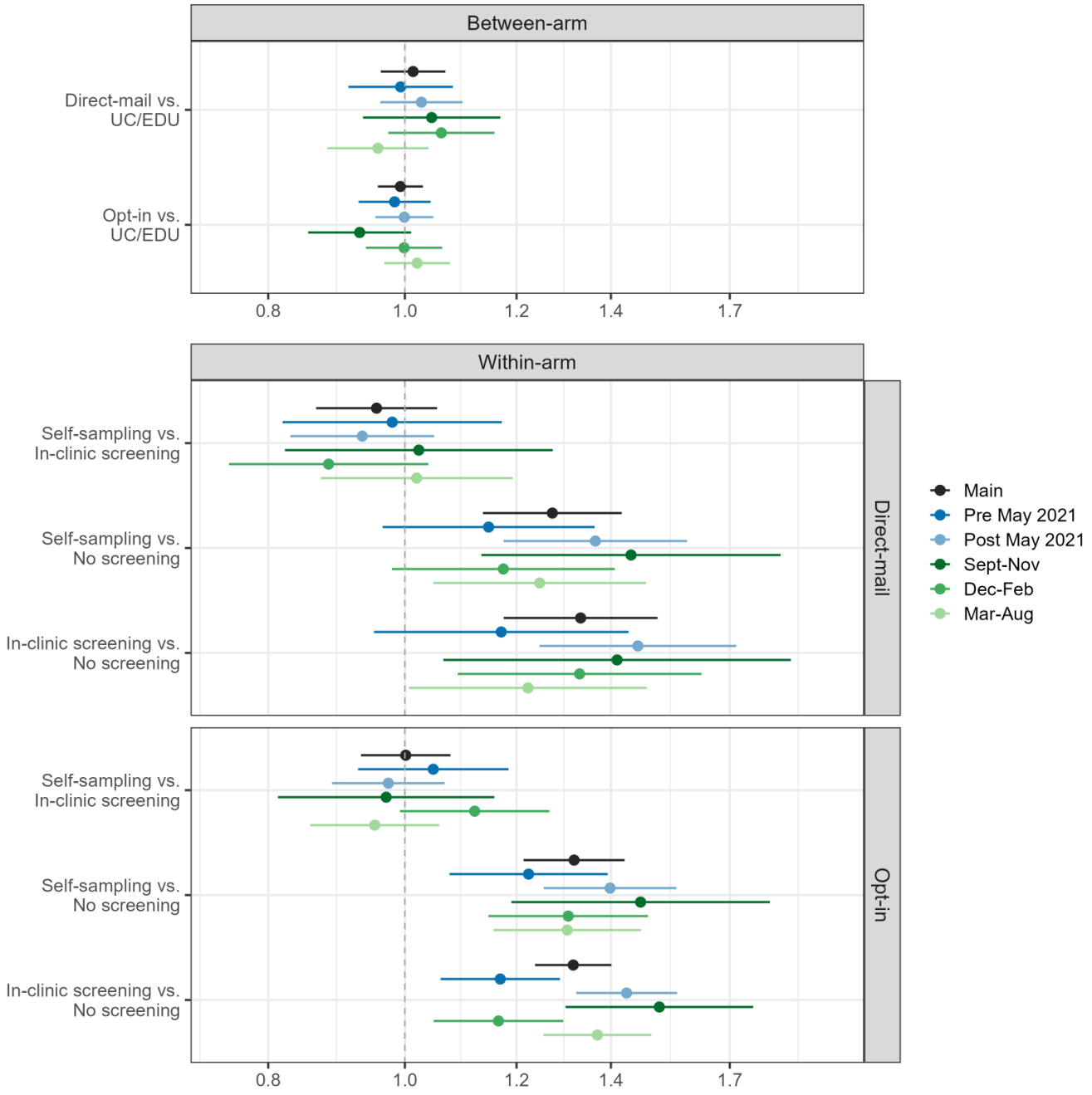
Supplemental Figure 2.1. Distribution of propensity scores stratified by propensity score quintile and CC screening modality selected for final propensity score models. Pink dots represent the mean, blue dots represent the 25th and 75th percentiles, and black dots represent the minimum and maximum



Supplemental Figure 2.2. Absolute standardized differences for the entire population (triangles) and stratified by propensity score quintile (circles) comparing CC screening modality selected for all covariates of interest. Dashed horizontal line are at absolute standardized difference of 0.1



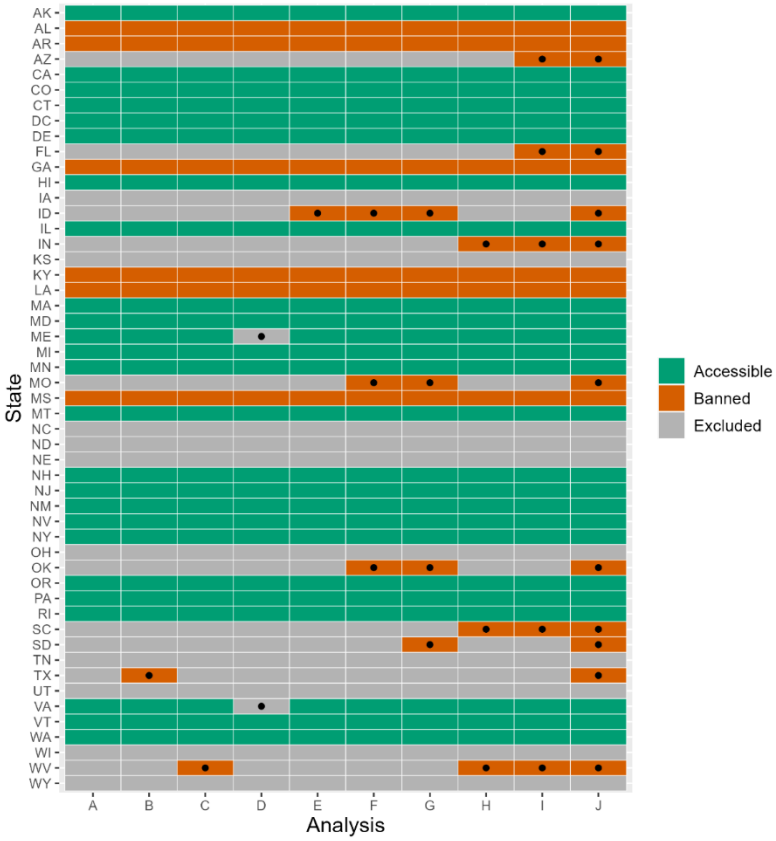
Supplemental Figure 2.3. Risk ratios for flu vaccine uptake for secondary between- and within-arm analyses. Between-arm results were unadjusted, and within-arm results were adjusted for age, travel time to primary care clinic, years enrolled in KPWA, census tract SVI, CCI, tobacco use, BMI, primary care provider or obstetrician-gynecologist visit 1 year pre-randomization, any in-person clinic visit 1 year pre-randomization, viewed a MyChart message 1 year pre-randomization, sent a MyChart message 1 year pre-randomization, number of days with a MyChart login 1 year pre-randomization, and any flu vaccine receipt 10 years pre-randomization



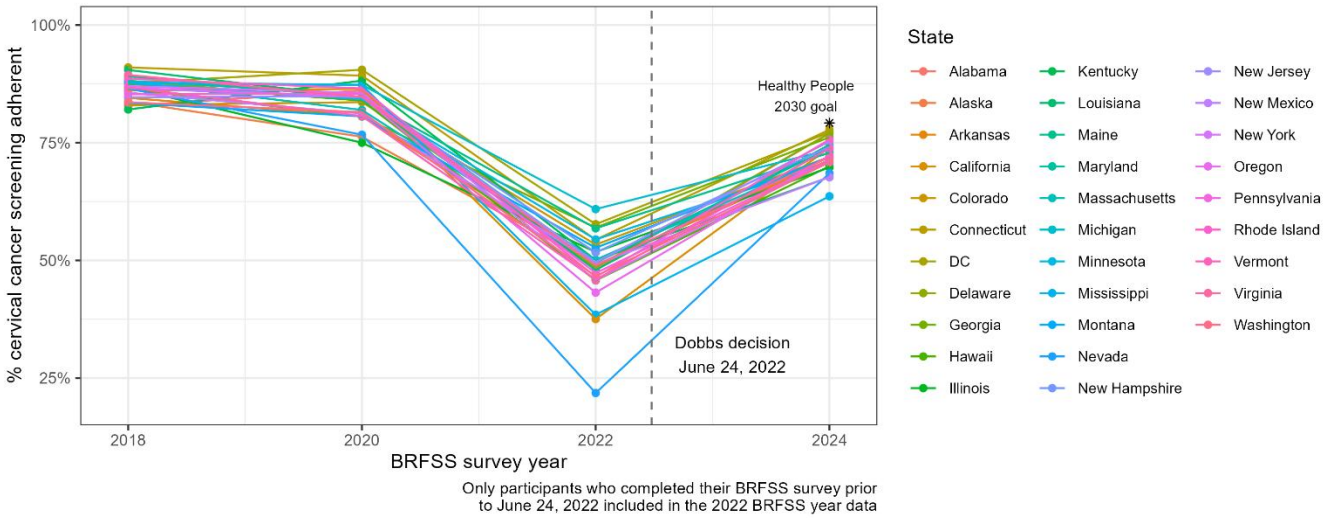
Between-arm results were unadjusted;
Within-arm results were adjusted for variables described in figure caption

Chapter 3 Supplementary Tables and Figures

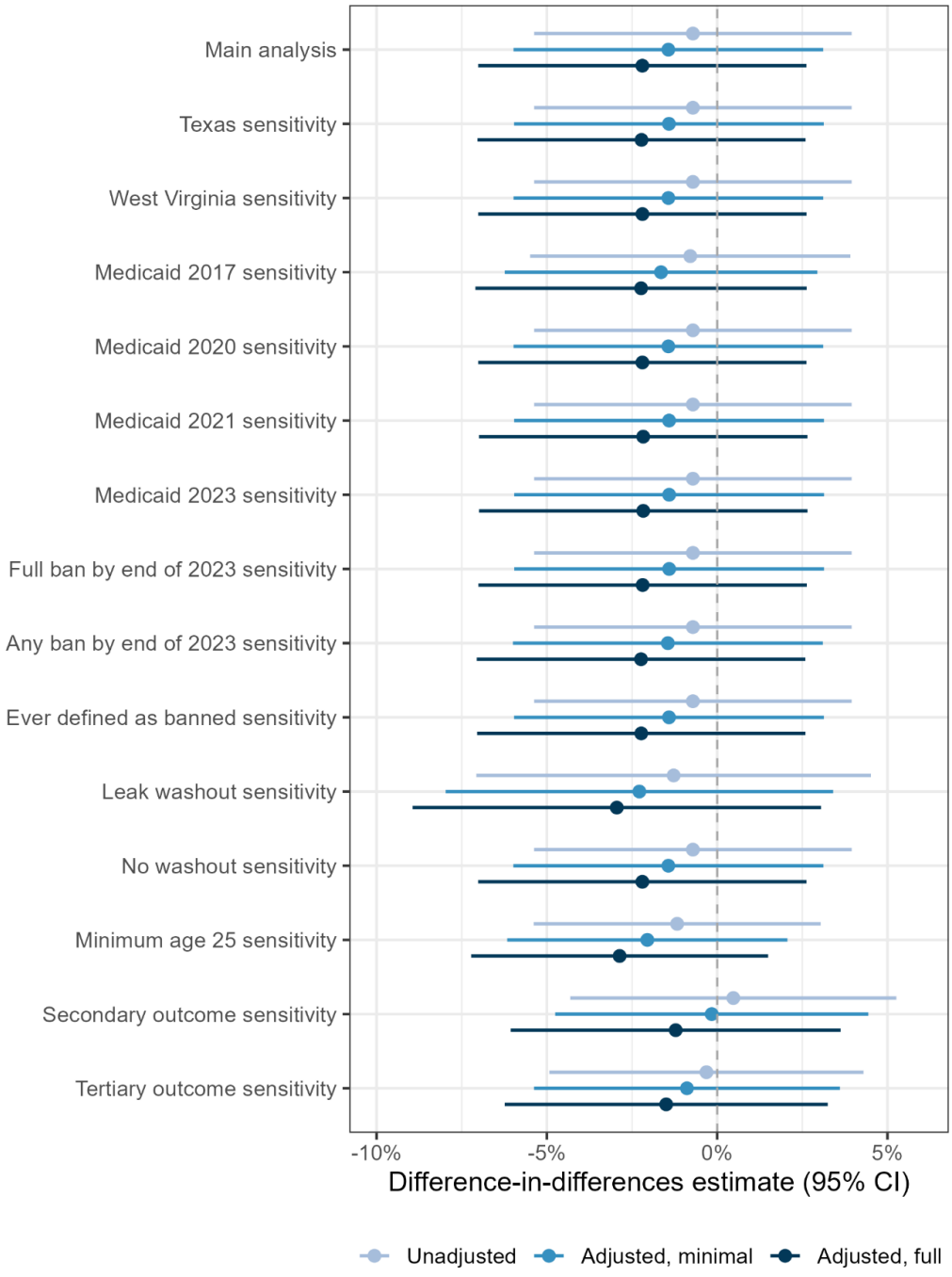
Supplemental Figure 3.1. State abortion policy classifications. Analysis A is the main analysis (corresponding to Figure A in manuscript); B-J are sensitivity analyses. States are classified as abortion accessible, abortion banned, or excluded from each analysis. For analysis A, accessible states were those where abortion was fully accessible for the duration of the study period, banned states were those where abortion went from accessible to banned from the pre- to post-*Dobbs* period, and excluded states were those 1) that expanded Medicaid after January 2019, 2) with abortion restrictions that fluctuated during the study period, or 3) where abortion was not 'fully' banned (remained accessible somewhere between 6 weeks and fetal viability). B included Texas, due to Senate Bill 8, which went into effect on September 1, 2021. C included West Virginia as banned, as a total abortion ban went into effect on September 16, 2022, slightly past our two-month trigger ban criteria. D excluded states that expanded Medicaid any time after January 2017 for a more stringent Medicaid implementation requirement. E allowed for the inclusion of states that expanded Medicaid in 2020, F allowed for the inclusion of states that expanded in 2021, and G allowed for the inclusion of states that expanded in 2023. H included states that implemented a full abortion ban between *Dobbs* and the end of 2023. Analysis I included states that implemented any abortion ban (full ban or before 22 weeks) between *Dobbs* and the end of 2023. J allowed states that were ever defined as abortion banned in this study to be classified as "banned". Dots highlight classification differences from the main analysis (column A)



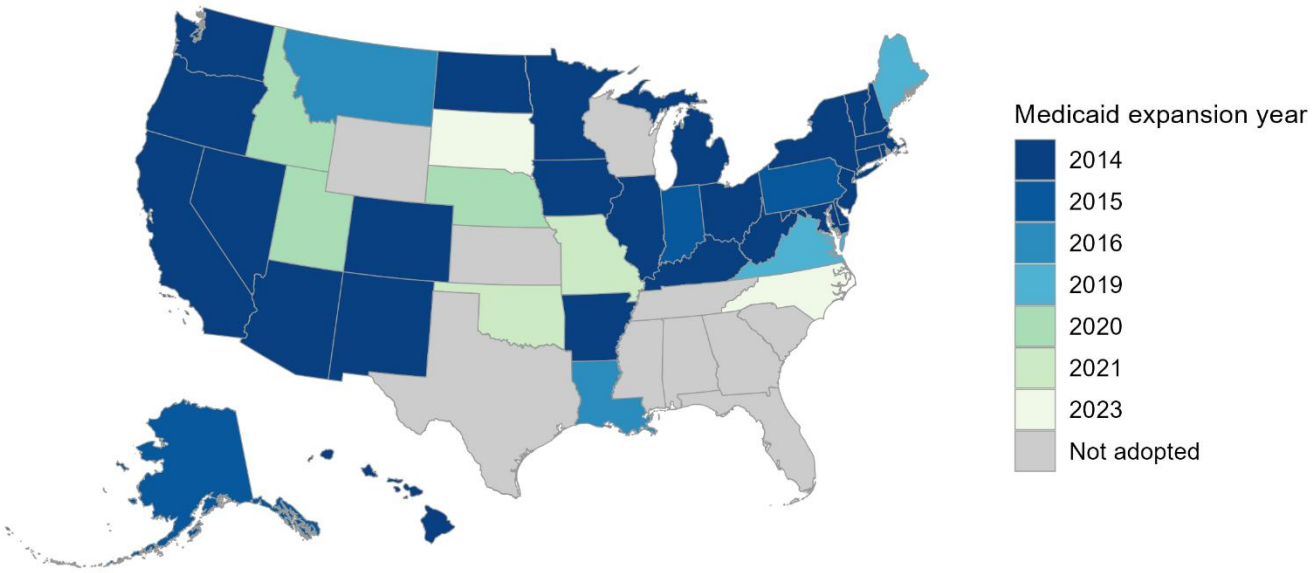
Supplemental Figure 3.2. BRFSS-weighted cervical cancer screening adherence prevalence for main analysis stratified by state. Healthy People 2030 target goal of 79.2% “proportion of females who get screened for cervical cancer” included for reference.[181] The blue line that reaches the lowest point in 2022 is Nevada



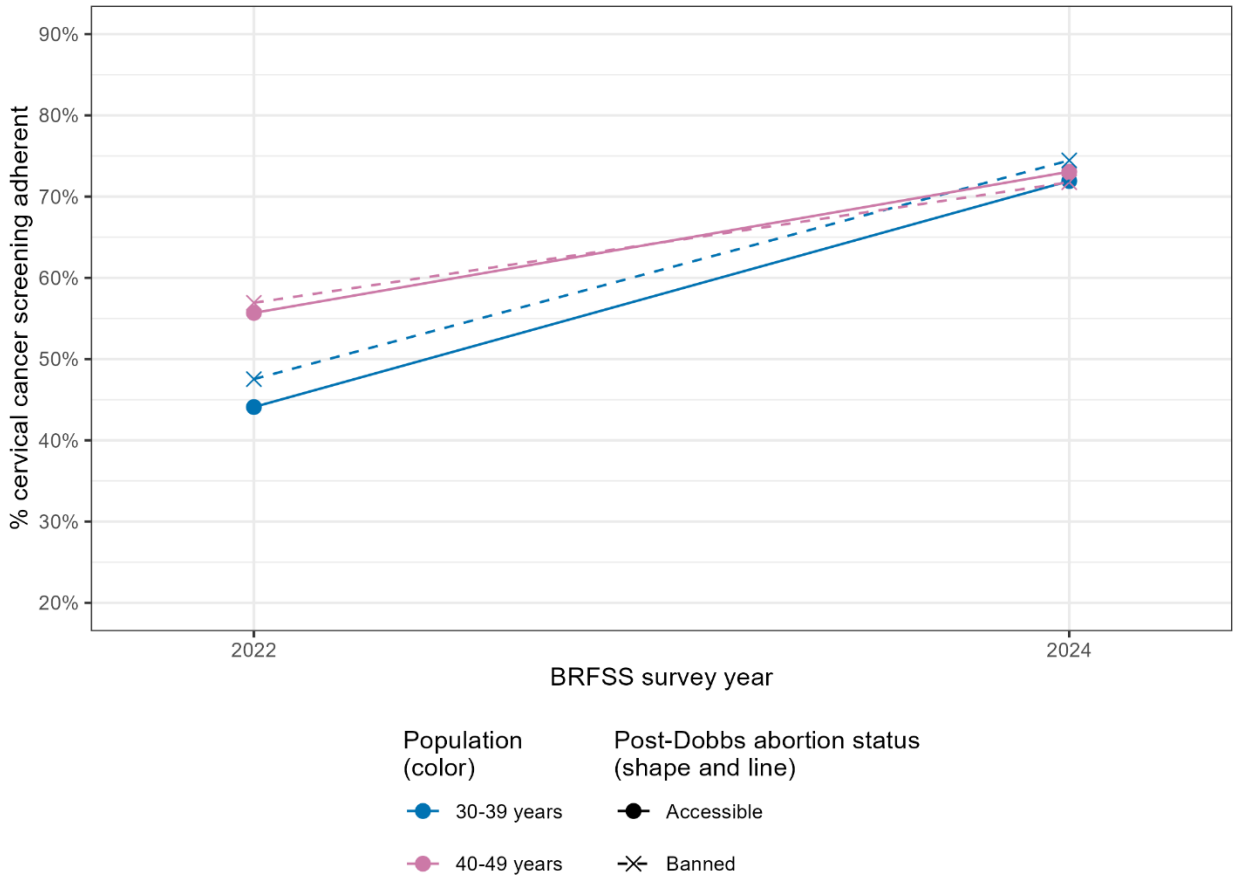
Supplemental Figure 3.3. Difference-in-differences (DID) model results. Minimally adjusted models were adjusted for age (5-year intervals), insurance type (private, public, none), and county metropolitan status (urban, rural). Fully adjusted models were further adjusted for education level (did not graduate high school, graduated high school, attended college/technical school, graduated from college/technical school) and annual household income (<$25k, $25k to <$50k, $50k to <$75k, ≥$75k)



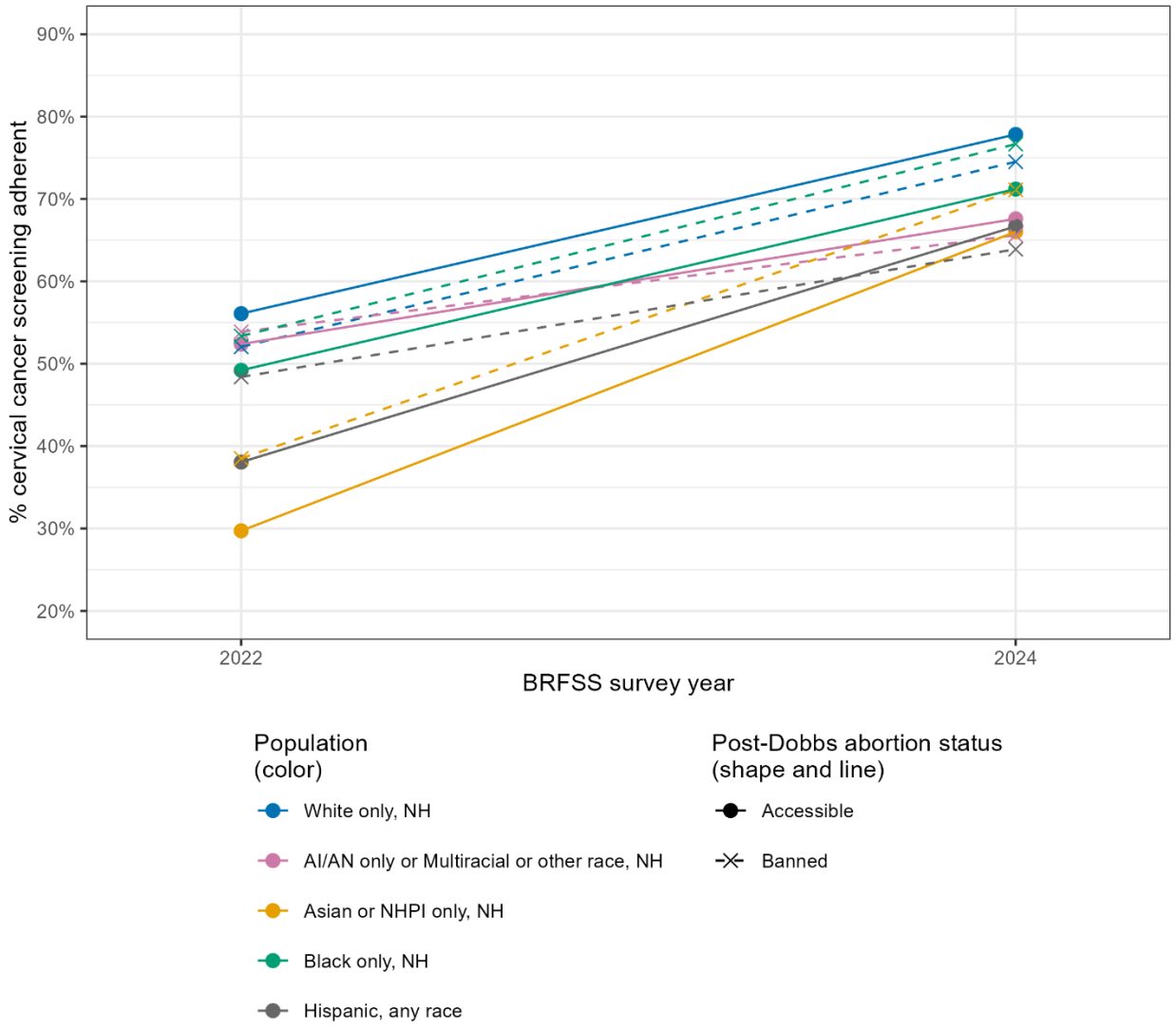
Supplemental Figure 3.4. Medicaid expansion status in the US



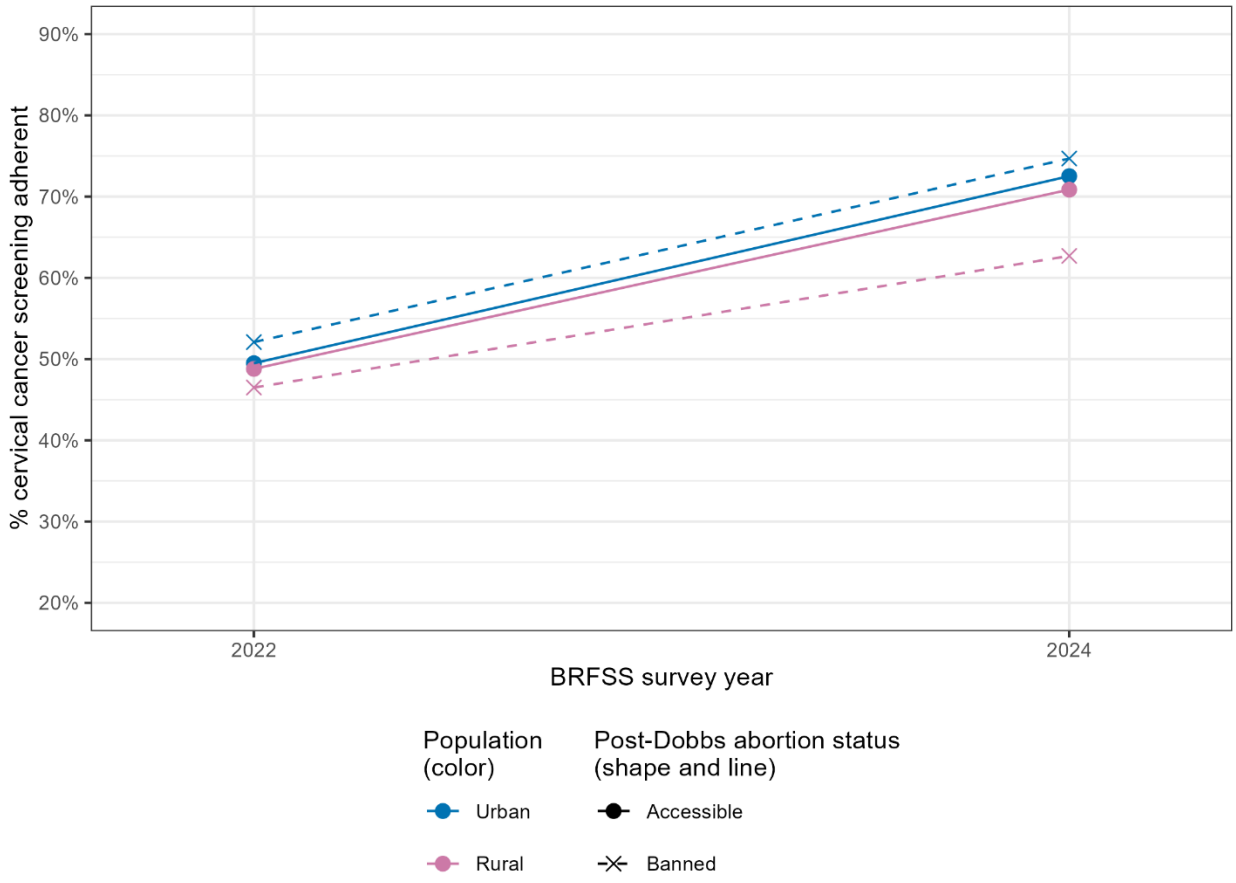
Supplemental Figure 3.5A. BRFSS-weighted cervical cancer screening adherence prevalence by post-*Dobbs* abortion access status and further stratified by age categorization



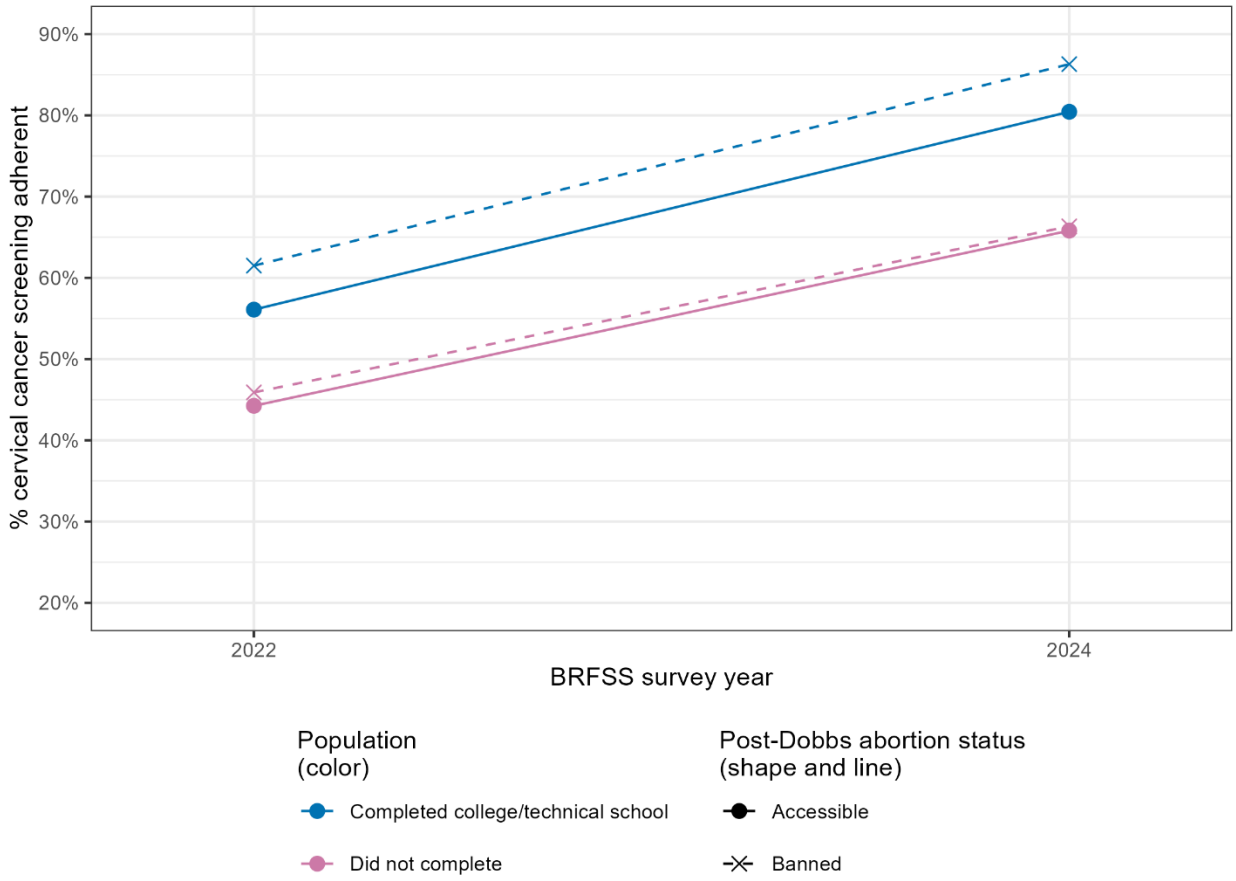
Supplemental Figure 3.5B. BRFSS-weighted cervical cancer screening adherence prevalence by post-*Dobbs* abortion access status and further stratified by race categorization



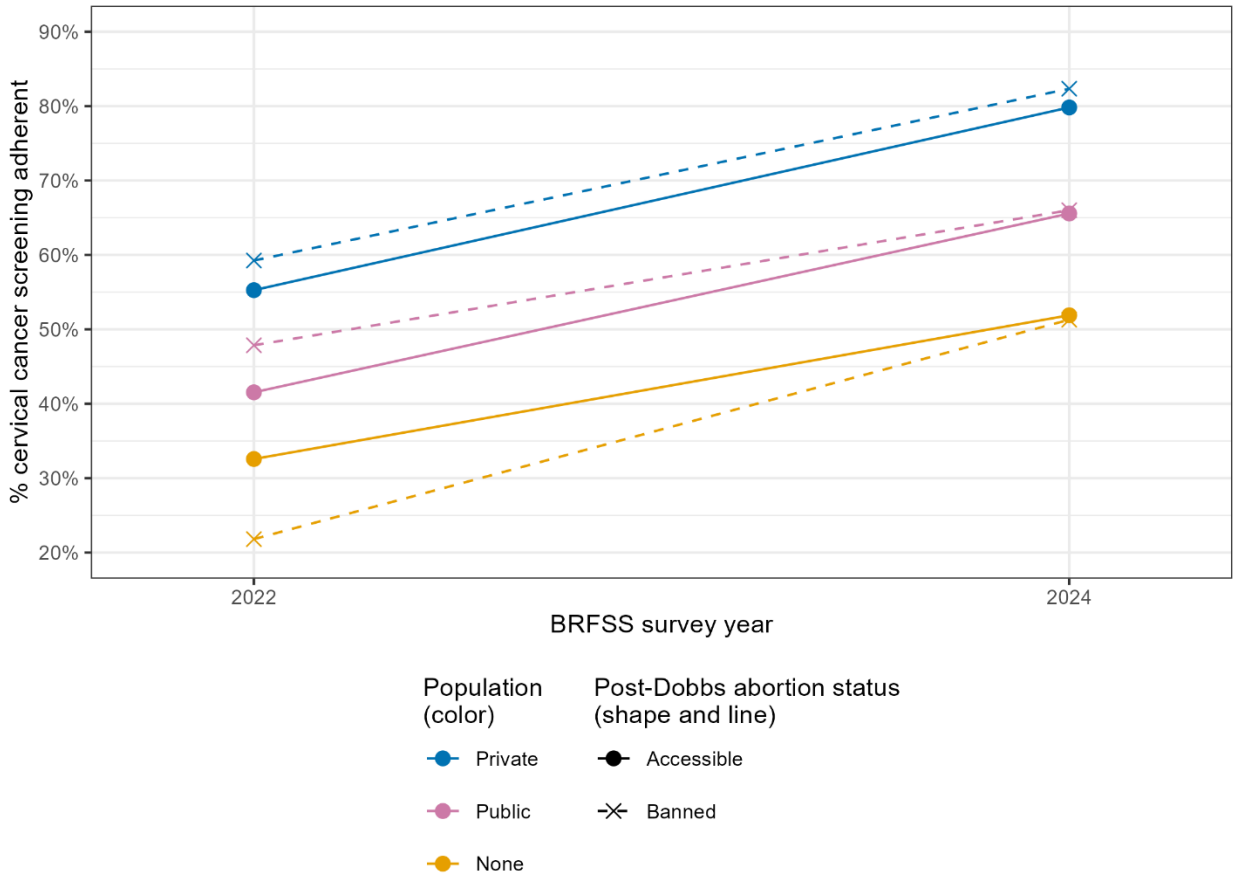
Supplemental Figure 3.5C. BRFSS-weighted cervical cancer screening adherence prevalence by post-*Dobbs* abortion access status and further stratified by county metropolitan status



Supplemental Figure 3.5D. BRFSS-weighted cervical cancer screening adherence prevalence by post-*Dobbs* abortion access status and further stratified by education level



Supplemental Figure 3.5E. BRFSS-weighted cervical cancer screening adherence prevalence by post-Dobbs abortion access status and further stratified by health insurance status



Supplemental Table 3.1. Results of prior trends tests for main difference-in-differences (DID) analysis. These models solely used pre-*Dobbs* data, and were fit as linear regression models with cervical cancer screening adherence as the outcome, with state and year fixed effects, and an interaction term between year (defined first categorically then continuously) and binary post-*Dobbs* abortion access status (banned vs. accessible). The parameters and associated significance figures included below are for interaction terms. Quasi-likelihood ratio tests were used to obtain p-values and assess if the interaction term differed significantly from zero. Adjusted models controlled for age, county metropolitan status, education level, and annual household income; insurance status was not controlled for as very few states collected this information in the 2018 and 2020 BRFSS waves

Model type	Parameter	Estimate (95%CI)	p-value
Categorical year, unadjusted	2020 vs. 2018 * post-*Dobbs* status	0.35 (-2.54, 3.25)	0.47
	2022 vs. 2018 * post-*Dobbs* status	2.58 (-1.79, 6.95)	
Categorical year, adjusted	2020 vs. 2018 * post-*Dobbs* status	0.76 (-2.22, 3.75)	0.37
	2022 vs. 2018 * post-*Dobbs* status	3.20 (-1.42, 7.82)	
Continuous year, unadjusted	Continuous year * post-*Dobbs* status	0.04 (-0.95, 1.03)	0.93
Continuous year, adjusted	Continuous year * post-*Dobbs* status	0.20 (-0.83, 1.24)	0.69

Supplemental Table 3.2. Results of placebo tests for main difference-in-differences (DID) analysis. These models solely used post-*Dobbs* data, and were fit as linear regression models with cervical cancer screening adherence as the outcome, with state and year fixed effects, and an interaction term between a binary indicator for year (specified below) and binary post-*Dobbs* abortion access status (banned vs. accessible). The parameters and associated significance figures included below are for interaction terms. Quasi-likelihood ratio tests were used to obtain p-values and assess if the interaction term differed significantly from zero. Adjusted models controlled for age, county metropolitan status, education level, and annual household income; insurance status was not controlled for as very few states collected this information in the 2018 and 2020 BRFSS waves

Model type	Parameter	Estimate (95%CI)	p-value
2019 as fake treatment, unadjusted	2020 vs. 2018 * post-*Dobbs* status	0.32 (-2.57, 3.21)	0.83
2019 as fake treatment, adjusted	2020 vs. 2018 * post-*Dobbs* status	0.73 (-2.25, 3.72)	0.63
2021 as fake treatment, unadjusted	2022 vs. 2020 * post-*Dobbs* status	2.14 (-2.56, 6.84)	0.37
2021 as fake treatment, adjusted	2022 vs. 2020 * post-*Dobbs* status	2.37 (-2.58, 7.32)	0.35

Supplemental Table 3.3. Difference-in-difference-in-differences (DDD) analysis results. Cervical cancer screening adherence prevalences for abortion accessible and banned states are also presented, with prevalence differences from the pre- (2022) to post-*Dobbs* (2024) period calculated. Difference-in-differences (DID) models were fit within each subpopulation of interest, and then DDD models were fit on the entire population to compare changes in cervical cancer screening adherence prevalence from pre- to post-*Dobbs* in abortion banned vs. accessible states. DID models were adjusted for age (5-year intervals), insurance type (private, public, none), county metropolitan status (urban, rural), education level (did not graduate high school, graduated high school, attended college/technical school, graduated from college/technical school) and annual household income (<$25k, $25k to <$50k, $50k to <$75k, ≥$75k). DDD models were also adjusted for age, health insurance status, county metropolitan status, education level, and annual household income, with the DDD characteristic of interest withheld (e.g., age was not adjusted for in the age DDD models, but the four other variables listed were). Results are BRFSS-weighted

	Abortion accessible states			Abortion banned states						
							DID estimates		DDD estimates	
	Pre-*Dobbs*, %	Post-*Dobbs*, %	Prevalence difference, % (95% CI)	Pre-*Dobbs*, %	Post-*Dobbs*, %	Prevalence difference, % (95% CI)	Unadjusted (95%CI)	Adjusted (95%CI)	Unadjusted (95%CI)	Adjusted (95%CI)
Age										
30-39 years	55.7	73.1	17.4 (14.2, 20.6)	56.9	71.8	14.8 (8.5, 21.2)	-2.7 (-9.8, 4.4)	-3.6 (-10.7, 3.5)	REF	REF
40-49 years	44.1	71.9	27.8 (25.0, 30.7)	47.5	74.5	26.9 (21.3, 32.5)	-0.1 (-6.3, 6.0)	-1.7 (-8.2, 4.7)	-1.8 (-11.2, 7.6)	-1.1 (-10.8, 8.6)
Race/ethnicity										
White, NH	56.1	77.8	21.8 (19.1, 24.4)	52.1	74.5	22.5 (17.3, 27.6)	0.9 (-4.9, 6.6)	-1.0 (-7.0, 4.9)	REF	REF
AI/AN, multiracial, or other race, NH	52.4	67.6	15.2 (5.9, 24.5)	53.9	65.6	11.7 (-14.8, 38.2)	-3.5 (-27.9, 20.9)	-11.9 (-37.7, 13.8)	-4.2 (-30.9, 22.6)	-13.2 (-36.9, 10.4)
Asian or NHPI, NH	29.7	66.0	36.3 (28.7, 43.9)	38.5	71.1	32.6 (-1.9, 67.1)	2.6 (-33.1, 38.3)	3.3 (-25.4, 31.9)	-0.8 (-32.5, 30.9)	3.4 (-28.0, 34.9)
Black, NH	49.2	71.2	22.0 (16.0, 28.0)	53.4	76.7	23.3 (15.0, 31.6)	2.3 (-7.6, 12.2)	1.1 (-9.0, 11.3)	0.6 (-11.0, 12.3)	2.3 (-9.8, 14.4)
Hispanic, any race	38.1	66.7	28.6 (23.5, 33.7)	48.4	63.9	15.5 (-0.3, 31.3)	-9.8 (-26.8, 7.1)	-9.5 (-27.1, 8.2)	-12.7 (-30.0, 4.6)	-10.5 (-28.2, 7.1)
Rurality										
Urban	49.5	72.5	23.0 (20.8, 25.2)	52.1	74.7	22.6 (18.0, 27.2)	-0.2 (-5.2, 4.8)	-1.9 (-7.1, 3.3)	REF	REF
Rural	48.8	70.9	22.1 (13.1, 31.0)	46.5	62.7	16.2 (6.1, 26.3)	-4.4 (-17.8, 9.0)	-4.5 (-18.7, 9.8)	-4.9 (-19.0, 9.1)	-1.6 (-16.4, 13.3)
Education										
Completed college/technical school	56.1	80.4	24.4 (21.6, 27.1)	61.5	86.3	24.8 (19.4, 30.2)	0.4 (-5.6, 6.4)	0.7 (-5.6, 6.9)	REF	REF
Did not complete	44.2	65.8	21.6 (18.4, 24.7)	45.9	66.3	20.4 (14.7, 26.2)	-0.4 (-6.9, 6.0)	-3.9 (-10.7, 2.9)	-1.7 (-10.7, 7.2)	-5.5 (-14.9, 3.8)
Insurance										
Private	55.3	79.8	24.6 (22.0, 27.2)	59.2	82.3	23.1 (17.9, 28.3)	-1.0 (-6.7, 4.7)	-1.8 (-7.7, 4.1)	REF	REF
Public	41.5	65.6	24.0 (19.9, 28.2)	47.9	66.0	18.2 (10.3, 26.0)	-5.3 (-14.1, 3.4)	-6.5 (-16.0, 3.0)	-4.4 (-14.8, 6.0)	-4.8 (-16.0, 6.3)
None	32.6	51.9	19.3 (11.0, 27.7)	21.8	51.3	29.5 (17.6, 41.4)	10.6 (-4.2, 25.3)	8.9 (-6.8, 24.6)	11.0 (-4.4, 26.4)	9.5 (-6.7, 25.8)

Abbreviations: AI/AN, American Indian Alaska Native; DDD, difference-in-difference-in-differences; DID, difference-in-differences; NH, Non-Hispanic; NHPI, Native Hawaiian Pacific Islander

Supplemental Table 3.4. Behavioral Risk Factor Surveillance System (BRFSS) cervical cancer screening questions for 2022 and 2024. These questions were used to create the cervical cancer screening adherence outcome for the main analysis. Note that there was a change in how BRFSS asked respondents about cervical cancer screening history from 2020 to 2022

Variable	Question	Responses and skip pattern
CERVSCRN	Have you ever had a cervical cancer screening test?	• Yes • No (*skip remaining questions*) • Don't know/Not Sure (*skip remaining questions*) • Refused (*skip remaining questions*)
CRVCLCNC	How long has it been since you had your last cervical cancer screening test?	• Within the past year (anytime <12 months ago) • Within the past 2 years (1 year but <2 years ago) • Within the past 3 years (2 years but <3 years ago) • Within the past 5 years (3 years but <5 years ago) • 5 or more years ago • Don't know/Not sure • Refused
CRVCLPAP	At your most recent cervical cancer screening, did you have a Pap test?	• Yes • No • Don't know/Not sure • Refused
CRVCLHPV	At your most recent cervical cancer screening, did you have an HPV test?	• Yes • No • Don't know/Not sure • Refused

Supplemental Table 3.5. Behavioral Risk Factor Surveillance System (BRFSS) cervical cancer screening questions for 2018 and 2020. These questions were used to create the cervical cancer screening adherence outcome for validity tests (prior trends and placebo tests), and for visualizing screening adherence trends over time before the *Dobbs* decision. Note that there was a change in how BRFSS asked respondents about cervical cancer screening history from 2020 to 2022

Variable	Question	Responses and skip pattern
HADPAP2	Have you ever had a Pap test?	• Yes • No (*skip to HPVTEST*) • Don't know/not sure (*skip to HPVTEST*) • Refused (*skip to HPVTEST*)
LASTPAP2	How long has it been since you had your last Pap test?	• Within the past year (anytime <12 months ago) • Within the past 2 years (1 year but <2 years ago) • Within the past 3 years (2 years but <3 years ago) • Within the past 5 years (3 years but <5 years ago) • 5 or more years ago • Don't know/Not sure • Refused
HPVTEST	An HPV test is sometimes given with the Pap test for cervical cancer screening. Have you ever had an HPV test?	• Yes • No (*skip remaining questions*) • Don't know/not sure (*skip remaining questions*) • Refused (*skip remaining questions*)
HPLSTTST	How long has it been since you had your last HPV test?	• Within the past year (anytime <12 months ago) • Within the past 2 years (1 year but <2 years ago) • Within the past 3 years (2 years but <3 years ago) • Within the past 5 years (3 years but <5 years ago) • 5 or more years ago • Don't know/Not sure • Refused

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